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THREE ESSAYS ON ANALYST RESEARCH AND INVESTMENT

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To my husband, my child, and my parents

for their endless love and support.

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SUMMARY

This thesis investigates the behavior of investors and analysts participating in the financial market. In the first essay, we examine the effects of local stock returns on antidepressant usage using the Truven Health MarketScan® individual prescription drug data. There are three main findings. First, a one standard deviation decrease in local stock return increases local investors' antidepressant usage by approximately 0.42 percent (an economic cost of approximately 19 million dollars) in the subsequent weeks than what would have been in the absence of the decrease in stock return; in contrast, a stock price increase has no impact on antidepressant usage. Second, the effect of stock returns on antidepressant usage depends on an array of local socioeconomic characteristics including demographic structure, religiosity, political affiliation, political ideology, and personality traits. Third, local stock return fluctuations have significant effects on certain illnesses including insomnia, peptic ulcers, abdominal pains, and substance abuse which often result from depression. The results are consistent across a variety of robustness checks.

In the second essay, we examine the behavior of analysts. In July 2009, the Global Research Settlement (GRS), which was implemented to mitigate the conflicts of interests between analysts and investors, expired. The GRS mandated that sanctioned banks contract with Independent Research Firms (IRFs) to make independent research available to the banks' customers. We find that after the GRS expiration, the probability that analysts employed at sanctioned banks to issue positive recommendations increased by 3.3 percent compared to a control group. Our findings show that, after the GRS expiration, sanctioned banks might have become more optimistic and conflicts of interests seem to again threatened the credibility of the

research by sanctioned banks. Our paper calls into question the SEC's decision not codify to the GRS into permanent rules.

In the third essay, we examine the performance of analysts from Independent Research Firms (IRFs) and investment banks that cover firms in the financial sector. In particular, we evaluate six aspects of analyst performance: recommendation optimism, recommendation informativeness, earnings forecast optimism, earnings forecast accuracy, target price forecast optimism, and target price forecast accuracy. Using a sample of analyst recommendations and forecasts from 1994 to 2013, we document two important findings. First, compared to investment bank analysts, IRF analysts generally provide less biased, more informative, and more accurate recommendations and forecasts when covering firms in the financial sector. The only exception is the market reaction to pessimistic recommendations. The pessimistic recommendations issued by investment banks outperform those by IRFs. Second, conflicts of interests appear to play a significant role when investment bank analysts cover other bulge bracket investment banks. These results suggest that compared to their counterparts from IRFs, investment bank analysts are susceptible to institutional pressure related to underwriting business when covering firms in the financial sector.

Chapter 1

How Does the Stock Market Impact Investor Sentiment? —Evidence from Antidepressant Usage

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1.1 Introduction

Sentiment is a human response to internal or external events, which results in both physical and psychological changes in our behavior. Studies in the growing literature pertaining to sentiment and the stock market often focus on how investor sentiment affects stock prices.[1] The literature shows that people with pessimistic expectations about the future avoid buying risky assets such as stocks. However, there is little research on how stock market fluctuations affect investors' psychological health. The only previous study that uses non-survey data to investigate how stock returns affect people's sentiment is Engelberg and Parsons (2016). They examine the link between daily stock returns and hospital admissions related to mental illness.

We believe that the effects of stock market fluctuations on investor sentiment is an important question for two reasons: first, as depression could cause other illnesses, it is important to understand and quantify how movements in the stock market affect investor mental and physical health. This provides a more complete picture of the social and economic effects caused by market volatility. The consequences of a stock market crash might extend far beyond the lost wealth in stock assets. Second, our results support the finding that a

[1] Investor sentiment is the attitude of financial market participants toward a particular financial asset or the overall financial market. There are several approaches to measure investor sentiment. According to the first approach, investor sentiment is extracted from the financial market itself. Baker and Wurgler (2006, 2007) use a macroeconomic approach to measure sentiment and discern waves of sentiment; they find that investor sentiment has larger effects on the valuation of stocks difficult to arbitrage. In the second approach, investor sentiment depends on content from newspaper and other media sources and researchers use text mining technique to analyze them. Tetlock (2007) presents evidence using content from a Wall Street Journal column and finds that media pessimism influences stock prices. The third approach proxies investor sentiment using survey results (e.g. AAII Investor Sentiment Survey etc.). Puri and Robinson (2007) find that people with positive beliefs about future tend to retire later and save more. The fourth and last approach uses non-economic factors (e.g. health condition, internet search behavior, aviation disasters etc.) to proxy for sentiment. Engelberg and Parsons (2016) find that a decrease in the stock market return causes an increase in the number of hospital admissions.

shock to people's future consumption impacts people's current utility.[2]

This paper uses the Truven Health MarketScan Claims and Encounters Database (MarketScan), a national dataset of prescription drug claims, to answer the following question: how do local stock market returns affect local investors' antidepressant usage? We provide three major answers. First, we find that a one standard deviation decrease in local stock return increases subsequent local antidepressant usage by approximately 0.42 percent. In contrast, increases in the local stock index do not seem to impact the usage of antidepressants. Second, we find that the stock market effect on antidepressant usage varies across an array of local socioeconomics characteristics. More specifically, we find that people who: a) feel less productive, b) are more worried about money, c) receive less community recognition, d) exercise less frequently, and e) are less religious experience a larger increase in antidepressant usage when the stocks decline. A lower score on agreeableness and a higher score on neuroticism and extraversion reinforce the negative sentiment when the stock market declines. The effects of stock returns on sentiment are stronger for individuals aged 45-65 than those aged 35-45. Third, we find that local stock fluctuations also have significant effects on certain physical illnesses that often result from depression. These illnesses include insomnia, peptic ulcers, abdominal pains, substance abuse, and myocardial infarction.

Our results are robust across a variety of specifications. They are insensitive to: a) variations in insurance plans and physician types, and b) to the inclusion of local housing prices, local wages, and the local unemployment rate as control variables. As a test of internal validity, we also find that declines in stock returns are uncorrelated with the usage of antibiotics.

Our results are economically meaningful because antidepressants are among the most commonly prescribed medications in the United States. We find that a one standard deviation decrease in the two-week stock return results in 0.42 percent more prescriptions

[2]Utility functions are important in the field of economics and asset pricing. In the standard expected utility framework, current utility is determined only by current consumption. Anticipation of future shocks could still impact current utility, albeit only through the channel of current consumption. We use antidepressant usage to proxy for a person's utility, and we find that within 2 weeks after the stock shock, more people start taking antidepressants. It is unlikely, that within 2 weeks, the anticipation over a future consumption shock leads to a reduction in current consumption. Thus our results imply that investors experience disutility today because of the anticipation over a future consumption shock. We think this is an investor's rational reaction to an anticipated shock. Our results are consistent with the recursive utility function in Caplin and Leahy (2001).

of antidepressants filled (an economic cost of approximately 19 million dollars) than what would have been in the absence of the prior decrease in stock return. The consequences do not stop at the level of mental health; depression itself is a risk factor for suicide. Physical illnesses that are often a result of depression might cause long-term complications, which could cost the society much more than treating depression alone. We emphasize that one should take into account the increased medical expenses associated with increased incidence of depression and illnesses resulting from depression when trying to accurately quantify the loss related to stock crashes.

This paper contributes to the literature by providing a comprehensive examination of how stock market fluctuations affect investor's psychological health. McInerney et al. (2013), Cotti et al. (2014), and Schwandt (2014) use self-reported survey data (e.g., Health and Retirement Survey, Behavioral Risk Factor Surveillance System) to study the relationship between stock returns and self-reported depression.[3] Using the MarketScan data of prescription drug claims, this paper improves on past studies in the following ways. First, pharmaceutical claims are not subject to the common problems in the survey data (e.g. wording of questions, recall errors etc.).[4] Second, all classes of antidepressants require a prescription by physicians who could give an accurate and comprehensive assessment of the patients' mental health. Indeed, Cossman et al. (2010) and Chini et al. (2011) find that prescription drug usage is strongly associated with the disease prevalence. Third, the prescription data are of higher frequencies than the survey data,[5] which facilitates the discovery of the causality: a stock market decline results in more antidepressants prescribed in following weeks. Fourth, antidepressants are one of the most commonly prescribed drugs in the United States.[6] The introduction of affordable generic versions of antidepressants

[3]Huck (2015) uses crime rates to proxy for people's revealed utility and finds that stock returns impact people's utility.

[4]While all communications between a patient and a physician are protected by the law, the patient is more likely to be aware of mental health related stigma when talking to a survey interviewer. Bharadwaj et al. (2015) find that survey respondents are likely to underreport when asked about mental health conditions, but are less likely to underreport when asked about physical health conditions. In addition, wording of questions or interactions between the interviewer and interviewee could cause biased answers.

[5]The Health and Retirement Survey and the Behavioral Risk Factor Surveillance System Surveys are conducted bi-annually and annually, respectively.

[6]According to a CDC report, among Americans aged 12 and over, 11 percent of the population takes antidepressant medication. The report can be accessed at
http://www.cdc.gov/nchs/data/databriefs/db76.htm.

at lower prices improved the general public's access to these medications.[7] We believe that prescription drug claims of antidepressants could proxy sentiment better than self-reported measures.

Engelberg and Parsons (2016) use hospital admittance records of individual patients in California from 1983 to 2011, and find an inverse link between same day stock returns and hospital admissions. This study improves on Engelberg and Parsons' research in the following aspects. First, hospitalizations caused by mental illness are rare, while antidepressant usage is quite common. Antidepressant usage is 89 times more common than hospital admission caused by depression or anxiety disorders.[8] Generally speaking, patients are only admitted to inpatient stays when they are in critical conditions and might cause harm to themselves or others. Therefore, Engelberg and Parsons (2016) are likely to underestimate the overall effect of stock market declines on people's psychological health, because they only study hospitalized patients.[9] Second, Engelberg and Parsons (2016) only have medical records from California, whereas the MarketScan data cover the entire United States. Thus, we are able to explore a wide array of heterogeneity in the effect of stock returns on antidepressant usage due to differences in demographic structure, religiosity, political affiliation, political ideology, and personality traits. Third, given that the data include all insurance records of patients, we are able to identify patients suffering from various illnesses and examine the effect of stock returns on the illnesses resulting from depression (e.g. insomnia, substance abuse, etc.).

The remainder of the paper is organized as follows. Section 2 discusses the data and the estimation strategy. Section 3 reports the results of our main regressions. Section 4 investigates the heterogeneous effect of stock returns on antidepressants. Section 5 shows how stock returns affect an individual's physical health. Section 6 presents the results of our

[7]A 2008 report by the International Psychogeriatric Association finds that the price ranges from $1.89-$4.42 for a one-month supply of 8 types of generic antidepressants in the U.S.. Antidepressant usage does not vary by income status. A CDC report finds that there is no difference with respect to family income in the prevalence of antidepressant usage.

[8]Using the MarketScan 2005-2006 full sample, we find records of 22,253 individuals who were hospitalized with primary diagnosis of depression or anxiety disorder, whereas 2,002,217 individuals have at least one pharmacy claim for antidepressants in the same period. Thus, with antidepressants used commonly among Americans, we would have a large and representative dataset to work with.

[9]While Engelberg and Parsons (2016) find that a one standard deviation decrease in the local stock returns increases daily hospital admissions by about 0.18 percent, this study finds that a one standard deviation decrease in the local stock returns increases weekly antidepressant usage by about 0.42 percent.

falsification tests. Section 7 calculates the economic costs related to antidepressant usage. We conclude in Section 8.

1.2 Data and estimation strategy

1.2.1 Data

We combine data from several sources, including MarketScan, CRSP, COMPUSTAT and the Bureau of Labor Statistics, to construct the final data set for empirical analysis.

1.2.1.1 Stock and macroeconomic measures

Daily stock price data are from CRSP. The quarterly headquarter location information of publicly-traded firms is obtained from COMPUSTAT. We merge the stock price data with firms' headquarter location data and calculate the return of a value-weighted stock index consisting of companies headquartered in each state using the merged dataset.

We collect macroeconomic measures from multiple sources. We collect unemployment rates at the MSA-month level from the Local Area Unemployment Statistics of the Bureau of Labor Statistics. We collect MSA-quarter wage rates from the Quarterly Census of Employment and Wages of the Bureau of Labor Statistics. [10]

1.2.1.2 Medical claim data

The MarketScan database provides a national collection of inpatient, outpatient and pharmaceutical claims in the U.S. The detailed claim data come from over 200 large, self-insuring corporations and insurance carriers. This paper focuses on the 2005-2006 period. Due to the sensitive nature of health records, the individual-level data are de-identified, and each individual is assigned a unique patient ID. The patient ID can be used to link records across multiple data files and track patients over time, even when they move to a different state or switch their health plans. Patient demographic characteristics include the patient's geographical location (i.e., census region, state, and

[10] See US Department of Labor, Bureau of Labor Statistics, Local Area Unemployment Statistics website: http://www.bls.gov/lau/ and Quarterly Census of Employment and Wages website:
http://www.bls.gov/cew/ for detailed statistics.

MSA), age, gender, industry of employment, employment status (full-time versus part-time), and relationship to the beneficiary (employee versus spouse/children of the employee). We can group the type of health plans in the data into 8 categories: Basic/Major Medical, Comprehensive, Exclusive Provider Organization (EPO), Health Maintenance Organization (HMO), Non-Capitated Point-of-Service (Non-Capitated POS), Preferred Provider Organization (PPO), Capitated or Partially-Capitated Point-of-Service (Capitated or Partially-Capitated POS), and Consumer-Driven Health Plan (CDHP). The claims data include a continuum of settings (e.g., inpatient, outpatient, and pharmaceutical). To ensure the accuracy and reliability of the data, all claims have been paid and adjudicated. During the analysis, we eliminate duplicated claims and claims with negative payment and pharmaceutical claims with negative days of supply or quantity.

The full sample includes 13,344,000 individuals, which is approximately 4.5 percent of the U.S. population in 2005.[11] For our main analysis, we restrict the sample to individuals aged 35 to 64 years old and exclude those covered under "gatekeeper plans." We obtain a sample of 4.9 million individuals. We focus on individuals aged 35-64 because the census data shows that fewer people under 35 years old have stock holdings, and in the case that they do have stock holdings, they have much lower stock holdings than the older cohorts.[12] Thus, when the stock market declines, the financial loss is heavily concentrated among investors older than 35 who have more exposure to the stock market. Individuals aged 65 or older are not included in the data.[13] In the sample, 0.47 percent, 19.07 percent, and 15.89 percent of the individuals are covered under EPO, HMO, and POS plans ("gatekeeper plans"). People with "gatekeeper plans" are excluded because patients in these plans need to choose a Primary Care Physician ("the gatekeeper") to manage his/her health care needs. If the patient wants to visit a specialist, he/she need a referral from the "gatekeeper", and the referral needs to be approved by the insurance company before insurance covers the cost of the visit to a specialist. Walsh and Egdahl (1985) and Mechanic et al. (1995)

[11]The U.S. population was 295.5 million in 2005.

[12]The percentage of families having direct or indirect stock holdings is 52.5%, 60.4%, and 58.9% for age of the family head between 35-44 years old, 45-54 years old, and 55-64 years old. The corresponding median value among families with stock holdings by the age of the family head is $26,000, $45,000, and $78,000. For more detailed statistics on age and stock ownership, see http://www.census.gov/compendia/statab/2012/tables/12s1211.pdf.

[13]If an individual is 65 years old or above, his/her records are excluded from the MarketScan Commercial Claims and Encounters Database but are part of the MarketScan Medicare Supplemental Database.

argue that, to obtain psychiatric care, patients with "gatekeeper plans" have to accept the risk that their Primary Care Physician ("the gatekeeper") might label them as mentally ill. Because many "gatekeepers" work for an Employee Assistance Program, the risk of health information leakage is increased. Ojeda and McGuire (2006) find that depressed adults with "gatekeeper plans" are significantly less likely to obtain mental health care than adults with "non-gatekeeper plans". Perneger et al. (1995) show that individuals with "gatekeeper plans" have substantial unmet needs for psychiatric service. Another reason for excluding data of patients under HMO plans is that they are capitated plans. In capitated plans, physicians are pre-paid a fixed amount for each enrolled individual assigned to them each period, which minimizes the incentive for the physician office to keep rigorous claim records. Thus, the claim data associated with HMO plans are less accurate than data associated with other plans. The mean age in the sample is 49.77 years. Males make up 46.27 percent of the sample. Employed individuals are 63.49 percent of the sample. The remaining are either spouses or children. Of the employed individuals, 8.51 percent work in finance, insurance and real estate, and 75.37 percent are full-time workers. Later, we relax the insurance plan restriction and increase the sample size to 8.0 million for robustness checks.

In our sample period, namely 2005-06, 192,518 individuals (or 3.92 percent of our sample) had at least one pharmaceutical claim for antidepressants[14] and one outpatient visit claim indicating a primary or secondary diagnosis of depression or anxiety disorder.[15] We use International Classification of Diseases, Ninth Revision, Clinical Modification (ICD-9) to identify depression or anxiety disorder.[16] On average, each of these individuals filled 8.59 prescriptions for antidepressants in the 24-month period from 2005-2006, and each

[14]Records of pharmaceuticals dispensed at retail pharmacies, at specialty pharmacies, or by mail are all included in the database.

[15]Physicians do not usually provide diagnosis on pharmaceutical prescription. Thus, to link the pharmaceutical claims to the diagnosis, for each pharmaceutical claim, we search for related outpatient service claims filed within a time window around the pharmaceutical claim for the same individual.

[16]Claims were identified as any primary or secondary diagnosis for the following ICD-9 codes: 296.2 (Major depression, single episode), 296.3 (Major depression, recurrent episode), 300.0 (Anxiety disorder), 300.2 (Phobic disorders), 300.3 (Obsessive-compulsive disorder), 300.4 (Dysthymic disorder), 301.4 (Obsessive-compulsive personality disorder), 309.8 (Other specified adjustment reactions (PTSD)). See Wu et al. (2012a), Shrestha et al. (2013), Vlahiotis et al. (2011), and Wade et al, (2014) for justification of using this group of codes to identify depression and anxiety disorder. Among the disorders, major depression, single or recurrent episode (52.70 percent) is the most common diagnosis recorded, followed by anxiety disorder (27.79 percent) and dysthymic disorder (13.21 percent); the other disorders each make up less than 3 percent of the sample.

prescription includes, on average, 40.05 day supply of antidepressants. We use National Drug Classification code on the prescription claims to identify drugs within the antidepressant therapeutic class. The therapeutic class is classified using the Red Book system in MarketScan. Sertraline (Zoloft, Lustral, 13.9 percent) is the most frequently prescribed antidepressant in our sample. Other commonly prescribed antidepressants include Bupropion (Wellbutrin, 13.6 percent), Escitalopram (Lexapro, Cipralex, 13.4 percent), Fluoxetine (Prozac, 11.9 percent), Venlafaxine (Effexor, 10.8 percent), Paroxetine (Paxil, Seroxat, 9.6 percent), Citalopram (Celexa, 5.8 percent), Trazodone (Desyrel, 5.6 percent), Amitriptyline (Elavil, Endep, 5.3 percent).[17]

1.2.2 Estimation strategy

1.2.2.1 The econometric model

Following Engelberg and Parsons (2016), we use a linear regression model to estimate the effects of stock returns on investor's antidepressant usage.[18] Because antidepressant usage is also related to local economic conditions, seasonality, and unobserved geographical characteristics, we control for these factors in our regression. Specifically, we estimate the

[17] The name of the generic version is listed first, with name of the brand version in parentheses. The antidepressants are ranked by drug class, SSRIs (selective serotonin reuptake inhibitors) are prescribed in 55.2 percent of all cases, making them the most commonly used antidepressant classes. SSRIs are followed by SNRIs (serotonin-norepinephrine reuptake inhibitors, 14.8 percent), NRIs (norepinephrine reuptake inhibitors 13.6 percent), TCAs (tricyclic antidepressants 8.9 percent), and SARIs (serotonin antagonists and reuptake inhibitors, 5.6 percent). Antidepressants are not available over the counter. Pharmacies require patients to present a prescription from a physician before dispensing antidepressants.

[18] The basis of our regression is the home bias within U.S. phenomenon (Coval and Moskowitz (1999)). It is important to note that people might not be actively investing in stocks; however, their wealth is still affected by the stock market because of passive investments (e.g., 401(k), Employee Stock Ownership Plan). An employee stock ownership plan (ESOP) is an program that allows a company's workforce to have ownership interest in the company. According to the Employee Benefits Security Administration's (EBSA) Office of Policy and Research (OPR) at the Department of Labor, ESOPs have a total plan assets of $1.4 trillion. In addition, ESOPs cover 14 million participants in the U.S.. For more detailed statistics about ESOP, see http://www.nceo.org/articles/esops-by-the-numbers. Even for majority of people who do not have the option to participate in an ESOP, financial research still documents home bias for local stocks. Using the individual trading data of U.S. investors, Ivkovic and Weisbenner (2007) find that 17.1% of stock purchase involve firms where the headquarter is located within 50 miles from investor's household. Using the same data, Seasholes and Zhu (2010) find that 30% of a household's portfolio is invested in firms headquartered within a 250 mile radius of the household. However, if household would do not exhibit home bias, this fraction should be only 12%.

following model:

$$\log\left(Antidepressants_{m,t}\right) = \beta_0 + \beta_1 \cdot Market\_return_{s,(t-k,t)}$$
$$+\beta_2 \cdot MSA\_economic\_factors_{m,t} + MSA_m + Month_t + Year_t + \varepsilon_{m,t} \quad (1)$$

where the natural logarithm of $Antidepressants_{m,t}$ in MSA m during week t is specified as a function of $Market\_return_{s,(t-k,t)}$ and a vector of MSA-level macroeconomic characteristics $MSA\_economic\_factors_{m,t}$. $Market\_return_{s,(t-k,t)}$ is the market return from the closing index of week $t-k-1$ to the closing index of week t of a value-weighted index consisting of publicly-traded companies headquartered in state s. MSA_m is MSA fixed effects. $Month_t$ is monthly fixed effects.$Year_t$ is year fixed effect.

We use drug claim records to construct the dependent variable by aggregating the weekly count of prescriptions of antidepressants filled by patients with depression diagnosis in each MSA. The final data set includes more than 30,000 MSA-by-week observations. For our primary variable of interest, $Market\_return_{s,(t-k,t)}$, we use the $Market\_return_{s,(t-1,t)}$ which measures the cumulative two-week return in the state stock index as the percentage change from closing index Friday two weeks previously to the current Friday's closing index. Thus, in our main regressions, we allow returns up 2 weeks previous to influence the current week's antidepressant usage to account for the time new patients need to make an appointment in advance. The market return is standardized using the trailing one year standard deviation. β_1 , the coefficient of our primary variable of interest, measures the degree to which local stock variations affect antidepressant usage.

We include MSA fixed effects to control for time-invariant heterogeneity at the MSA level (unobserved differences in the prescription medicine utilization rate in general across MSAs, etc.). We control for monthly fixed effects for three reasons. First, seasonality may affect the rate of depression. Rosenthal et al. (1984) finds that Seasonal Affective Disorder (SAD) is a recurrent major depressive disorder that occurs more during the winter months when the hours of daylight are decreased. Second, we expect more health care utilization at the end of the calendar year. Employees can use a Flexible Spending Account (FSA) to set aside a portion of pre-tax earnings to pay for medical expenses or other qualified expenses. Individuals with an FSA must spend the money within the coverage period (most commonly defined as the calendar year). Thus, we expect more health care utilization at the end of the calendar year to deplete the outstanding balance in the FSA. Third, we expect less antidepressant usage in holiday months. Hartig et al. (2013) show that an increase in the number of vacationing workers results in a decline in the number of people with mental stress. Thus, monthly fixed effects account for seasonality of mental health and medical spending. Yearly fixed effects control for unobservable factors that affect antidepressant usage across years (e.g., long-term changes in health policies).

Our identification strategy assumes that stock returns are conditionally exogenous to people's decision to use antidepressants. Or put differently, stock returns and antidepressant usage should be uncorrelated conditional on observed characteristics except for the fact that stock market declines cause a decrease in people's wealth. There are threats to the validity of our identification strategy. If antidepressant usage is related to local macroeconomic indicators, our coefficient for local stock returns, β_1, may capture this indirect relationship between local macroeconomic indicators and antidepressant usage. We respond to this possibility by including controls for unemployment rate and local wage rate in each MSA and week to account for economic factors that might also affect antidepressant usage. The data in MarketScan are for individuals that are either employed or are the spouse/children of the individuals employed. Thus, effects on the covered employee's mental health through the local unemployment rate might be limited. However, we think there are at least two reasons why we should control for local unemployment rates. First, the covered employee might be stressed and fear future job loss when seeing his/her colleagues losing jobs. The expectation of future job stability might directly impact his/her current mental well-being, independent of current job status. Thus, we might observe that local unemployment rates affect the mental health of the employed population. Catalano et al. (1986) interview household principal wage earners and find that job insecurity increases the likelihood of seeking psychological help. Second, dependents (e.g., spouses) of covered employees could also suffer from unemployment.[19] In this case, the local unemployment rate directly influences mental health through the channel of the dependents. Compared to the local unemployment rate, MSA-by-quarter wage data measure local macroeconomic conditions more directly. It is reasonable to think that local wage rates, which affect instantaneous utility, have a larger

[19] Using data from Denmark, Browning and Heinesen (2012) find that job loss is followed by increased risk of suicide and suicide attempts.

effect on the mental health of the employed population than the local unemployment rates, which affect the expectation of future consumption.

1.2.2.2 The lag structure

In this section, we explain why we choose two weeks as the lag time for stock decline to affect antidepressant usage. According to medical literature, individuals experience the following stages after a shock in their stock wealth. First, individuals become aware of the change in the stock market. The time needed to learn about the market situation increases with age. Using our data, we find that older individuals respond to market returns more slowly than younger individuals. Second, symptoms of depression appears; this stage could take some time to develop.[20]

Third, after experiencing symptoms, patients make an appointment and wait to see a doctor if he/she decides to seek professional help. Due the complexity of the U.S. health care system, patients usually have to wait several days to see a physician. Forrest (2003) finds that the average appointment wait time to see a specialist is 8.4 days for those with "non-gatekeeper plans". Finally, the patient obtains antidepressants. Based on the medical literature, we think two weeks is a reasonable lag time between the onset of stock decline and prescription of antidepressants being filled.

1.3 Impact of stock returns on antidepressant usage

1.3.1 Main results

Table 2 presents our main results. We add control variables gradually to the equation. The specification in Column 1 is the most parsimonious. The estimated stock return effect is -1.350 with a t-statistic of -3.478, which implies that the lower local stock returns in the weeks t and $t-1$ result in an increase in local antidepressant usage in week t. Moving to the second column, adding year fixed effects only results in the coefficient β_1 to decrease slightly to -1.338 with a t-statistic of -3.449. When we include MSA fixed effects in the

[20] Although some people are aware of the stock market decline, their emotional well-being might only responds to the stock market decline gradually or not at all. It is likely that our empirical specification might not capture the change of health care usage of people whose emotional well-being responds slowly to the stock market decline.

third column, the coefficient β_1 is -0.420 with a t-statistic of -2.505. Further including local unemployment rate and local wage has limited impact on the estimate of β_1. In the most restrictive specification in Column 4, a one standard deviation decrease in local stock return increases local antidepressant usage by approximately 0.42 percent.

In Table 3, we allow a cumulative return from more than one week previous to influence the current week's antidepressant usage to account for the prolonged time that new patients need to make an appointment with a physician. Columns 1-3 of Table 3 test for the lagged relationship between stock returns and subsequent usage of antidepressants. While the dependent variable is the logarithm of antidepressant usage in MSA m and week t, we test four-, three- and two-week returns to examine whether there is a lagged response to shocks in local stock returns. We observe a strong relation between a decrease in cumulative returns in the market index in weeks t and $t-1$ and a subsequent increase in antidepressant usage in week t. However, we find the effect does not last for more than two weeks. In Column 4, we test for the instantaneous relationship between market returns and usage of antidepressants during the same week. Again, we observe a strong relation between a decrease in returns in the market index and an increase in antidepressant usage in the same week. In Columns 5-7, we test for a possible leading relationship between stock returns and the usage of antidepressants in the past. It is possible that taking antidepressants makes people less risk averse, and results in bullish market sentiment. It may also be possible that an increase in antidepressant usage is a proxy for worse physical and emotional health in general, which results in a downward pressure on future productivity of corporations, leading to subsequent stock market decline. When we allow future stock returns to influence the current week's antidepressant usage, antidepressant usage is not significantly associated with subsequent stock returns. The estimates for β_1 are all statistically insignificant at the 10 percent level.

Our results show a significant negative relationship between market returns in the previous week and the current week and the current week's antidepressant usage.

1.3.2 Robustness checks

In this section, we conduct a number of robustness tests to confirm that our results are not driven by potential confounding factors or the estimation specifications.

1.3.2.1 Alternative specifications for the dependent variable and the independent variable

Table 4 presents alternative specifications for both the dependent variable and the market return variable. In Columns 1-4, we subtract the average weekly usage for that MSA in the past 2, 3, 6, and 12 months from the dependent variable. By subtracting the average weekly usage in the past from the dependent variable, we adjust the dependent variable to control for a possible persistent trend of antidepressant usage. The market return variable is the same as Table 2. All estimates are statistically significant and range from -2.039 to -2.637.

In our main specification, we rescaled the market return by dividing its one year trailing standard deviation. Because stock market volatility changes over time, it is possible that a more volatile stock market in the past left investors numb to new stock shocks. In other words, investors might get used to stock fluctuations and become insensitive to the current stock returns. However, if the stock market in the past is peaceful, investors could be psychologically unprepared for new stock shocks and exhibit panic behavior. In Columns 5-7 of Table 4, we rescale the market returns by the trailing standard deviation of different periods: 2, 3, and 6 months. We find that scaling the market return by the volatility of different periods has little impact on the estimate of β_1. A one standard deviation decrease in local stock return increases local antidepressant usage by 0.38 percent to 0.43 percent.

1.3.2.2 Antidepressant usage for patients without depression diagnoses

We count one incident of antidepressant usage as the patients receiving depression diagnosis as the primary or secondary diagnosis. However, the majority of antidepressant prescriptions are not accompanied by an outpatient visit resulting in a depression diagnosis (Pagura et al.(2011) and Mojtabai and Olfson (2011)). In our main regression sample, there are 960,634 (or 19.54 percent of the sample) unique individuals who have at least one pharmacy claim for antidepressant in 2005-2006. However, only 192,518 (or 3.92 percent of the sample) unique individuals have at least one pharmacy claim for antidepressant as well as at least one medical claim of an outpatient visit with a primary or secondary diagnosis of depression or anxiety disorder. There are three explanations for the phenomenon that the majority of antidepressants are prescribed without psychiatric diagnosis. First, stigmas related to depression could explain the lack of diagnosis on some medical claims. It is likely that physicians avoid using the word depression on outpatient visit claims in an attempt to lower the emotional burden of the patient and thus improve the patient's medication adherence. Sirey et al. (2001) show that patients' stigma towards depression tend to show lower antidepressant adherence and suggested that even early in the treatment, physicians should pay attention to the attitude of patients toward the illness. Second, physicians might be reimbursed more for treating non-psychiatric conditions and hence choose not to assign psychiatric codes. Rost et al. (1994) show that, due to economic reasons, physicians routinely substitute another diagnostic code for depression. Third, the MarketScan database only provides content of the primary and secondary diagnosis listed on the outpatient visit claim. More diagnosis codes could be listed on the claim but were not recorded in the MarketScan database.

To capture all of the antidepressant usage, Column 2 of Table 5 extends the data to include all prescriptions of antidepressants filled, regardless of the diagnosis. The result indicates that stock returns also affect the antidepressant usage with any diagnosis: a one standard deviation of stock return decline results in a 0.16 percent increase in local antidepressant usage.

1.3.2.3 Antidepressant usage for patients covered by "gatekeeper plans"

In our main sample, we exclude individuals covered by "gatekeeper plans" (EPO, HMO, and POS plans) because: first, individuals covered by "gatekeeper plans" are significantly less likely to obtain mental health care than adults with other plans. Second, HMO plans, as capitated plans, are associated with less accurate medical records. In this robustness check, we include all antidepressants prescriptions associated with depression and anxiety disorder diagnosis regardless of the patient's insurance plan in our sample and re-run equation (1). Column 3 of Table 5 presents the result showing that stock returns also affect the antidepressant usage of individuals covered by all plans: a one standard deviation decrease in the local stock return increases local antidepressant usage by approximately 0.45 percent.

1.3.2.4 Antidepressant usage for patients with continuous insurance coverage

In this section, we exclude individuals who are not continuously enrolled in a health plan (45.87 percent of our sample). There are two reasons why we conduct this robustness check.

First, interruptions in health plan enrollment could be caused by other depression-triggering incidents such as unemployment, divorces, marriages, or death of a family member. Second, by excluding the new enrollees and those who drop out of a health plan, we are able to examine the change in psychological health of the same group over time. Column 3 of Table 5 presents the results when we re-run equation (1) using the continuously enrolled sample. The estimate is similar to our main results: a one standard deviation decrease in the local stock return increases local antidepressant usage by approximately 0.52 percent.

1.3.2.5 Antidepressant usage for patients diagnosed by primary care physicians

Both specialists and primary care physicians can prescribe antidepressant to patients.[21] Because specialists and primary care physicians might follow different criteria when prescribing antidepressants, we investigate the relationship between antidepressant prescriptions and stock returns in the subsample in which diagnoses were made by primary care doctors, who prescribe the majority of the antidepressant prescription in our sample. In Column 5 of Table 5, the dependent variable is the logarithm of the number of weekly antidepressant usage patients diagnosed by primary care physicians only. The estimation results remain consistent: a one standard deviation decrease in the local stock return increases the local usage of antidepressant prescribed by primary care physicians approximately 0.58 percent

1.3.2.6 Impact of stock returns on the antidepressant usage among the newly prescribed users

In this section, we examine the antidepressant usage among patients who are newly prescribed antidepressants. By examining the new patients, we can distinguish between onset of depression and reoccurring depression. In Column 6 of Table 5, we exclude refills of antidepressants. The dependent variable is the logarithm of the number of antidepressant prescription to patients who are newly prescribed antidepressants in each MSA and week. The independent variables are the same as Column 4 of Table 2. We find that investors are more likely to be prescribed antidepressants after stock declines: a one standard deviation

[21] MarketScan data have provider-level information; thus, we are able to distinguish between specialists and non-specialists (including primary care physicians). Patients seeing specialists include patients either treated at mental health facilities or by a psychiatrist.

decline in the local stock return increased local antidepressant usage of those who are newly initiated to antidepressants by 0.56 percent.

1.3.2.7 Impact of stock returns on the patient's starting dosage of antidepressants

In the Column 7 of Table 5, we use prescribed dosage of antidepressant as an alternative proxy for sentiment. More severe depression usually requires a higher dosage. Keller et al. (1982) suggest that physicians should be aware of recommended medications and doses for treating depression. We first define the daily dose of antidepressant for each prescription as the quantity of medication prescribed multiplied by the strength of medication divided by the days supplied. For the purpose of this robustness check, it is necessary to compare dosages between different antidepressants. We use the recommendations from the American Psychiatric Association (APA) Practice Guideline for the Treatment of Patients with Major Depressive Disorder to establish the equivalent dose for the most commonly used antidepressants.[22] We define the daily equivalent dosage as the daily dose of antidepressant for each patient calculated previously divided by the recommended starting dose from the APA Practice Guideline for each type of antidepressant.[23] Because specialists follow clinical guidelines more closely, they are more careful with prescribing antidepressants, and are able to prescribe more accurate dosages than primary care physicians (Kniesener et al. (2005)). For this regression only, we restrict our sample to patients treated by specialists. In addition, we exclude refills of antidepressants prescribed to the patients in this regression, because physicians tend to increase the dose for patients who did not respond to the initially prescribed dose (titration) (Wu et al. (2012b)). In this case, the dependent variable is the weekly mean of the daily equivalent dosage of antidepressants prescribed in each MSA and week. The independent variables are the same as Column 4 of Table 2. In Column 7 of Table 5, using the antidepressant dosage data to examine the relationship between the

[22]See APA Practice Guidelines, Page 34, Table 6 for the recommended dosage of medication. http://psychiatryonline.org/pb/assets/raw/sitewide/practice guidelines/guidelines/mdd.pdf.

[23]Although the MarketScan data indicate whether a prescription is for the generic version or the brand name version of an antidepressant, we do not distinguish between the brand name version or generic version of the same antidepressant. It is likely that the effects are similar. Vlahiotis et al. (2011) find no significant difference of discontinuation pattern of antidepressants between patients treated with a brand name version versus a generic version.

severity of depression and stock returns, we find patients are more likely to be treated with antidepressants at a higher starting dosage after days of large stock declines: a one standard deviation decline in the local stock return increased the starting dosage to 2.90 times the APA standard from a baseline of 2.18 times the APA standard. We believe it is best to be conservative, so for the robustness checks, we use a sample consisting of patients newly started on antidepressants who have no claims for any antidepressants 90 days (wash-out period) prior to the start of the first observed antidepressant prescription. The regression results are similar.

1.3.2.8 Impact of stock returns on number of psychotherapy procedures performed

Our main results show that declines in the stock market increase antidepressant usage. Given that depression and anxiety disorders are commonly treated with either antidepressants or/and more expensive psychotherapy, one could argue that the increase in antidepressant usage might not be a perfect proxy for the shift of sentiment in investors. Instead, it could simply represent the use of antidepressants by investors to substitute for the more expensive psychotherapy offered by psychologist when investors' wealth is declining. In addition, some physicians might not prescribe antidepressants at the first visit. Thus, examining the incidents of psychotherapy treatment is important. [24]

Psychotherapy could be very effective in helping a person improve. Blattman et al. (2015) find that cognitive behavioral therapy, one type of psychotherapy, helps criminally-engaged Liberian men to increase self-control. As a result, the local rate of crime and violence fell significantly. According to Spielmans et al. (2011), psychotherapy, which has fewer side effects, could be as effective as antidepressants in treating depression.

We examine cases in which patients with depression or anxiety disorder diagnosis receive psychotherapy (e.g., psychoanalysis, group psychotherapy, and family psychotherapy) and find that 193,596 individuals (of 3.94 percent of our main regression sample) received psychotherapy in an outpatient setting in 2005-2006. On average, each received 12.19 psychotherapy treatments in the 24 months. Among patients who received psychotherapy,

[24] Because the MarketScan data are from insurance carriers and large, self-insuring companies, individuals in the sample are all covered by insurance and only need to pay the copayment and deductible amount for psychotherapy treatments. Thus, the cost for the psychotherapy might be less of a concern in this case.

approximately 70 percent were diagnosed with depression as the primary diagnosis. The rest of the patients suffered from other disorders, such as anxiety disorders and post-traumatic stress disorder. We re-estimate our model using the number of psychotherapy procedures. The dependent variable is the logarithm of the weekly number of psychotherapy procedures administered in each MSA and week.[25] In the last Column of Table 5, we present the regression results using the number of psychotherapy procedures administered as an alternative proxy for the sentiment of investors. The results remain consistent with our main results: a one standard deviation decrease in the local stock return increases the number of psychotherapy procedures performed by approximately 0.21 percent.

1.3.2.9 Impact of stock returns on antidepressant usage of fulltime workers

One potential source of bias arises from the possibility that the mental health of some people in our sample is driven mainly by the threat of unemployment rather than stock returns. Because the data in MarketScan are from insurance carriers and large, self-insuring companies, the majority of individuals in the data are either employed or are the spouse/children of the individuals employed, for whom unemployment is less of a threat. However, 24.63 percent of individuals in our sample are either employed part-time only or are covered under the Consolidated Omnibus Budget Reconciliation Act of 1985 (COBRA) provision. COBRA is a law that mandates an insurance program offering some employees continued health insurance coverage after leaving employment. Since Haliassos and Bertaut (1995) find that people working in industries with high unemployment risk have substantially lower probability of owning stocks, we expect that the mental health of individuals working part-time or under the COBRA provision are driven mainly by the threat of unemployment and the pressure of finding a new job, whereas stock returns might be less of a concern for them. In Column 2 of Table 6, we limit our sample by including only individuals employed full-time and their spouse/children. The dependent variable is the logarithm of the weekly antidepressant usage of patients who are full-time workers or their spouse/children. The result is consistent with our main regression in Table 2.

[25] To identify the procedures, we use the CPT-4 (Current Procedural Terminology, 4th Edition), provided in the MarketScan outpatient service record. Each incident of psychotherapy is identified with Current Procedural Terminology (CPT) codes 90801 to 90861. In addition, the patient must be diagnosed with depression or anxiety disorder.

1.3.2.10 Controlling for local natural disasters and terrorist attack incidence

We are concerned that our results could be driven by natural disasters and terrorist attacks. These shocks could cause depression and post-traumatic stress disorder (PTSD). Nolen-Hoeksema and Morrow (1991) and Galea et al. (2002) show that more people suffer from PTSD and depression after the Loma Prieta earthquake and after the September 11 attacks. Galea et al. (2002) also find that people who live close to the location of an attack have a significantly higher prevalence of PTSD. Using the Disaster Declaration File by the Federal Emergency Management Agency (FEMA),[26] we first identify the weeks surrounding each terrorist attack and natural disaster and then generate state-week indicator variables for the attacks and disasters. In Column 3 of Table 6, including the indicator variables for terrorist attacks and natural disasters generates quantitatively and qualitatively similar results: a one standard deviation decrease in the local stock return increases the antidepressant usage by 0.43 percent.

1.3.2.11 Controlling for local housing prices

We are concerned that our results could be driven by the housing market instead of the stock market. Lin et al. (2013) find that declines in housing value cause an increase in antidepressant usage in the elderly population in the U.S. To address this potential threat to our main results, we include the local monthly Freddie Mac Housing Price index (FMHPI) as a control in Column 4 of Table 6. The FMHPI measures the average price changes in repeat sales or refinancing of single-family homes in 363 metropolises. The results remain consistent.

1.3.3 Non-linear relationship between stock returns and antidepressant usage

In this section, we examine the possibility of non-linearity of the relationship between stock returns and antidepressant usage. It is likely that a sharp stock drop leads to especially high stress levels among investors and that extreme stock gains result in a reduction in antidepressant usage. To investigate the non-linearity, we break the stock returns into

[26] The complete list of natural disasters and terrorist attacks is available at https://www.fema.gov/disasters/grid/year.

positive and negative returns and assign positive and negative returns indicator dummies. We also break the returns into quintiles and assign quintile indicator dummies. We estimate the following equation:

$$\log\left(Antidepressants_{m,t}\right) = \beta_0 + \beta_1 \cdot Market\_return_{s,(t-1,t)} \cdot Dummy\_return_{s,(t-1,t)}$$
$$+\beta_2 \cdot MSA\_economic\_factors_{m,t} + MSA_m + Month_t + Year_t + \varepsilon_{m,t} \tag{2}$$

where we interact $Market\_return_{s,(t-1,t)}$ with dummies for positive and negative market return, respectively, to examine whether people stop using antidepressants when stock prices go up. We find that the usage of antidepressant goes up when the stock return is negative. When the local two-week market return is negative, a one standard deviation decrease of stock return results in a 1.14 percent increase in antidepressant usage as shown in Column 3 of Table 7. In comparison, when stock prices go up, no reduction in antidepressant usage is observed. Next, $Dummy\_return_{s,(t-1,t)}$ breaks our variable of interest in the previous equation into quintiles. We interact the quintiles dummies with the market return variable. We find that only changes of market return in the bottom two quintiles affect usage of antidepressants. More specifically, when the local two-week market return is in the bottom quintiles, then a one standard deviation decrease of stock return increases antidepressant usage by 1.23 percent, as shown in Column 5 of Table 7.

1.4 The effect of stock returns on antidepressant usage depends on socioeconomic characteristics

1.4.1 Different age cohorts

Investors of different age cohorts might respond to stock returns differently. This section examines the heterogeneous effect of stock returns on the antidepressant usage of different age cohorts. Specifically, we answer two questions: first, which age cohort responds more to market returns: the older or younger age cohort? Second, how fast is the response? Medical literature suggests that elderly adults are at higher risk for depression. Using a sample of Mexican-origin individuals, Angel et al. (2003) show that the relationship among perceived economic stress and various health measures and cognitive indicators are stronger for elderly adults. Using U.S. mortality records, Hempstead and Phillips (2015) find that for people under 64, the suicide rate between 1999 and 2010 increased for the older cohorts but not for the younger cohorts. They also find that the most common suicide circumstances were related to job, financial, or legal problems. In the Andersen healthcare utilization model, Andersen (1995) identifies the three factors that contribute to the use of health services: predisposing, enabling, and need factors. Age is a predisposing factor. According to this model, an individual's age influences his/her use of health service (Andersen (1995)). As suggested above, we expect that the stock market may lead to a heterogeneous effect on the antidepressant usage of different age cohorts.

To test the impact of stock market declines on antidepressant usage among different age cohorts and whether some age cohorts react faster to stock market declines than other cohorts, we divide our sample into the following categories: 35 to 45, 45 to 55, and 55 to 65. Table 8 presents the results. The different age cohorts have varying sensitivity to stock market declines. The effect of stock returns is found to be significantly negative on antidepressant usage among the 45-55 and 55-65 cohorts, whereas the effect is insignificant for the younger cohort aged 35-45. The reaction time for the 45-55 cohort and the 55-65 cohort are different. The 45-55 cohort needs up to two weeks to react to the stock market decline, whereas the response time of the 55-65 cohort to stock market declines is up to 3 weeks. A one standard deviation decrease in the local two-week market return results in a 0.38 percent increase in antidepressant usage in the 45-55 cohort, whereas a one standard deviation decrease in the local three-week market return results in a 0.09 percent increase in antidepressant usage in the 55-65 cohort. The older cohort may be a "slow responder" to stocks market declines for two reasons. First, elderly people might read financial news less often and thus not be aware of the newest stock market prices. Statistics show that the median age of a Wall Street Journal reader is 45.4.[27] Second, elderly people might not recall the content of the news. Frieske and Park (1999) studied memory of news stories in young and older adults by presenting print, audio and TV news to them. They find that older adults recall a significantly lower proportion of the news content than their younger counterparts. Our results indicate that the effect is insignificant for the antidepressant usage of the 35-45 age cohort, which is not surprising because the youngest cohorts own less stock; thus, their

[27] The information on the average age of readers is available at http://www.megamediamarketing.com/demographics.html.

wealth is less affected by the stock market. Another possible reason is that most people in the 35-45 cohort are at least 20 years away from retirement and will continue receiving a salary from work until then, which is their main source of income, thereby resulting in lower level of stress when they face poor performing stock market.

1.4.2 Different level of stock ownership

We also explore whether the relationship between the stock returns and people's sentiment is different for MSAs with different levels of stock ownership. We use two proxies for stock ownership: first, the proportion of people in an MSA working in the finance industry. The industry of employment is given in MarketScan. Second, the local per capita dividend income, which consists of payments made by corporations headquartered both in the U.S. and abroad to U.S. residents.[28]

Table 9 presents the regression analysis that allows for heterogeneous response of antidepressant usage to stock returns across MSAs with different levels of stock ownership. In Column 1, we divide all MSAs into two groups based on the proportion of people working in the finance industry below or above the national median. We generate a high (low) stock ownership dummy which equals to 1 for all MSAs with the proportion of people working in the finance industry above (below) the national median, and 0 otherwise. We interact the stock returns with the two low/high indicators. In Column 2, we repeat the process for our second proxy for stock ownership: per capita dividend income. If people living in regions with high stock ownership on average are more likely to be affected by the stock returns, the effect on antidepressant usage should be stronger in those regions. This is exactly what we find in Columns 1-2 of Table 9. Stock returns only affect the antidepressant usage in regions where the stock ownership proxy is above national median. Individuals in that group experience a 0.268-0.38 percent increase in antidepressant usage when the local stock return declines by one standard deviation. There is no effect of the stock returns on the antidepressant usage of people with low stock ownership. However, given the data limitations, these two measures are not perfect proxies for stock ownership. Thus, the results should be interpreted with

[28]Dividends paid by mutual funds are not included in the calculation of Per Capita Dividend Income. Per Capita Dividend Income is from the Bureau of Economic Analysis(BEA), Regional Income Division. The 2005-2006 BEA Regional Data, Personal Income information can be accessed through: http://www.bea.gov/regional/.

caution.

1.4.3 Regional differences in subjective well-being, political affiliation, and religiosity within the U.S.

Researchers have studied how different cultures affect financial outcomes. For example, Ahern et al. (2012) find that national culture affects the volume of cross-border mergers. We hypothesize that the effects of stock returns on antidepressant usage is influenced by different cultures. To identify the differences across geographical areas, in this section, we use the Gallup State of the States poll to measure state level differences across several well-being, political, and religious measures. In the next section of our paper, we examine how differences in personality traits influence the effects of stock returns on antidepressant usage. The Gallup poll is commonly used in economic research. Kahneman and Deaton (2010) use the Gallup poll to study people's emotional well-being and life evaluation. The results of the Gallup State of the States poll are based on telephone interviews with a random sample of 177,000 adults above 18 years old, living in the 50 U.S. states and D.C. The detailed questions in the survey are listed in the appendix. We merge the MarketScan data with the Gallup State of the State poll results.

In Panel A and Panel B of Table 10, we explore how these regional well-being, political, and religious differences across states within the U.S. affect the stock market-sentiment relationship. We examine who worries the most about stock market declines by comparing the mental health sensitivity of different cohorts to stock returns. First, we examine how personal well-being affects antidepressant usage in response to stock returns. Griffin et al. (2002) find that lower control over one's own work increases the risk of developing depression and anxiety for both women and men. Musick and Wilson (2003) find that exposure to volunteer work lowers depression levels for the elderly population. Craft and Perna (2004) find that exercise can improve the symptoms of depression. We expect that productivity, work that helps the community, and frequent exercise mitigate the negative sentiment that people experience when the stock market declines. In particular, as suggested before, productive work could help people divert their attention from the stock market conditions, and more toward the positive aspects of life. Likewise, people could turn to community and friends for emotional support. In Columns 2-6 of Panel A, we divide states into two categories according to each measure of well-being: below or above the national median. We then interact the market return with low/high score dummies. We find that people: a) with worse overall well-being, b) who feel less productive, c) who are more worried about money, d) who receive less community recognition, and e) who exercise less frequently experience an increase in antidepressant usage when the stock market declines. Individuals belonging to these groups on average experience a 0.39 percent -0.46 percent increase in antidepressant usage when the local stock return declines by one standard deviation. There is no effect of stock returns on the antidepressant usage of people: a) with better overall well-being, b) who feel more productive, c) who are less worried about money, d) who receive more community recognition, and e) who exercise more frequently. People who are more worried about money tend to be depressed when facing losses in stock value. One implication from our study is that, for people whose self-esteem is linked closely with the value of stocks, it may be best to avoid risky investments because the emotional suffering caused by negative stock returns might be too large to bear. The grief experience might drive them to make poorer investment decisions in return. This panic behavior caused by grief experience could drive the whole stock market further down. In this aspect, our paper also contributes to the literature of human behavior and stock crashes (Shiller (1999)).

Second, we examine how religion affects the way people's antidepressant usage responds to stock returns. McCullough and Larson (1999) find that people with high levels of general religious involvement are at reduced risk for depression. He states that the reasons for a lower risk of depression among religious people include more social connections that result from attending church, the mental activity that comes with helping others, and coping strategies learnt to address negative aspects of life, including one's own mortality. We expect that the factors mentioned above help religious people address the financial stress. In Columns 7-8 of Panel A, we divide states into two categories according to their religious score: below or above the national median. The results show that that people in less religious states experience an increase in antidepressant usage when stock prices decline: a one standard deviation local stock decline results in a 0.30 percent increase in antidepressant usage. In comparison, we do not find the same effect for people living in more religious states. Our results are consistent with those of Callen and Fang (2012), who find that firms headquartered in more religious regions exhibit lower levels of stock price crash risk.

Finally, we look into how political affiliation and ideology affect how mental health responds to stock returns. With respect to Republicans/Democrats, we have no prior reason to believe that there is any difference in their level of happiness. With respect to Conservatives/Liberals, Okulicz-Kozaryn et al. (2014), using surveys conducted between 1970 and 2002 by the European Commission in 16 Western European countries, find that conservative individuals are happier than liberals. However, their results might not be extended to the U.S. In Columns 2-7 of Panel B, we do not find significant heterogeneity in antidepressant usage responsiveness to stock returns between individuals with different political affiliation; however, people in states with more conservative populations respond slightly differently to stock returns than their counterparts living in states with more liberal population. People living in states with higher levels of liberals experience a slight increase in antidepressant usage when the stock market declines. There is no effect of stock returns on the antidepressant usage of people living in states with higher level of conservatives.

1.4.4 Regional differences in personality traits within the U.S.

In this section, we use the five-factor model of personality to identify the cultural differences across geographical areas. The five-factor personality traits are neuroticism, extraversion, agreeableness, openness, and conscientiousness. The five-factor model describes human personality in general. The state-level personality traits data are from Rentfrow et al. (2013), who aggregate the survey responses of 1.5 million individuals. They find that personality traits tend to be similar for people residing within the same geographical area. We merge the MarketScan data with the state-level personality traits from Rentfrow et al. (2013).

In Table 11, we explore how these differences in personality traits across states affect the stock market-sentiment relationship. First, we examine how neuroticism affects antidepressant usage in response to stock returns. Neuroticism is the tendency to be emotionally sensitive and instable. Ormel et al. (2013) find that people with a high score of neuroticism exhibit low tolerance for stress and experience anxiety and depression often. We expect that neuroticism reinforces the negative sentiment when the stock market declines. In particular, as suggested before, people who score high on neuroticism could interpret minor frustrations as extreme difficulties. In Column 2 of Table 11, we divide states into two groups according to whether the measure of personality traits, neuroticism, falls below or above the national median. We generate a high (low) score dummy for neuroticism which equals to

1 for all state with the score for an above (below) the national median, and 0 otherwise. We then interact the market return with low/high score dummies. We find that people living in states with above median neuroticism score, on average, experience an increase in antidepressant usage when the stock market declines. Individuals living in states with above median neuroticism score, on average, experience a 0.79 percent increase in antidepressant usage when the local stock return declines by one standard deviation (Column 2 of Table 11). There is no effect of stock returns on the antidepressant usage of people living in states with low neuroticism score on average.

Second, we examine how extraversion affects the way people's respond to stock returns in terms of antidepressant usage. Extraversion is the tendency to be talkative, outgoing, and energetic. Weiss et al. (2009) find that high extraversion combined with high neuroticism results in the "overly emotional" style. This "overly emotional" style is associated with a higher risk for major depression. Thus, we expect that a high extraversion score reinforces the negative sentiment caused by financial stress. In Column 3 of Table 11, the result shows that that people in states with higher extraversion score on average experience an increase in antidepressant usage when stock prices decline: a one standard deviation local stock decline results in a 0.67 percent increase in antidepressant usage. We do not find the same effect for people living in states with lower extraversion score on average.

Third, we look into the way the agreeableness score affects how mental health responds to stock returns. Agreeableness is the tendency to be well-tempered, compassionate, and cooperative. Dunkley et al. (1997) find that for both men and women, low agreeableness is a predictor for self-blaming. In Column 4 of Table 11, we find that people in states with lower agreeableness score, on average, respond differently to stock returns than their counterparts living in states with higher agreeableness score on average. People living in states with lower agreeableness score, on average, experience an increase in antidepressant usage when the stock market declines: a one standard deviation local stock decline results in a 0.61 percent increase in antidepressant usage. There is no effect of stock returns on the antidepressant usage of people living in states with higher agreeableness score on average.

Finally, we examine how openness and conscientiousness affect how mental health responds to stock returns. Openness is the tendency to be inventive, imaginative and curious. Conscientiousness is the tendency to be organized, structured, and dependable. With respect to openness, Bienvenu et al. (2004) find that high openness to feelings is associated with slightly higher risk for Major Depression. With respect to conscientiousness, we have no prior reason to believe that it is a risk factor of depression. In Columns 5-6 of Table 11, we find people living in states with higher openness score, on average, show an increase in antidepressant usage when the stock market declines. There is no effect of stock returns on the antidepressant usage of people living in states with lower openness score on average. We do not find significant heterogeneous effect of stock returns on antidepressant usage in terms of different conscientiousness scores.

1.5 Depression as a risk factor for other illnesses

1.5.1 Illnesses resulting from depression treated in outpatient settings

In this section, we show that stock returns affect not only mental health by causing depression, but also physical health. In Table 12, we present the relationship between the number of outpatient diagnoses of insomnia, peptic ulcer and abdominal pain (irritable bowel syndrome) and stock returns. All three diseases are often results of depression.[29]

Insomnia, peptic ulcers, and abdominal pain are all common medical conditions. The National Sleep Foundation's 2002 Sleep in America poll shows that nearly 60 percent of adults in the U.S. experience symptoms of insomnia a few times a week.[30] Sung et al. (2009) find that the one-year prevalence rate of peptic ulcer is between 0.12 percent and 1.50 percent. Quigley et al.'s (2006) surveys of individuals establish the prevalence of abdominal cramping/pain. Their results show that the prevalence of abdominal cramping and pain is 24 percent in the U.S. In the MarketScan sample, 353,000 individuals are diagnosed with abdominal pain from 2005-06. On average, each of those individuals visits outpatient facilities 3.81 times because of the pain in the two-year period. Ohayon et al. (1998) and Patten (1999) show that insomnia and peptic ulcers, respectively, are strongly associated with depression.

[29] Because a primary care physician can successfully treat insomnia, peptic ulcer and abdominal pain and there is often no need to seek specialist to treat these conditions, we include individual in this regression regardless of the type of their insurance plans, including "gatekeeper plans".

[30] See WB&A Market Research: 2002 Sleep in America Poll for the detailed results of the survey.

The relationship between abdominal pain and depression is more complicated. The primary symptom of irritable bowel syndrome is abdominal pain. Because there is no imaging or lab test to diagnose irritable bowel syndrome, the syndrome is often misdiagnosed as general abdominal pain. Masand et al. (1995), Lydiard (2001), and Blanchard et al. (1990) find that irritable bowel syndrome is common in patients who suffer from depression. In addition to the fact that abdominal pain is associated with depression, there is another way that pain in general is related to depression: the fear of incurring the stigma of depression. Pelz and Merskey (1982) interviewed patients with problems related to pain. They find that interviewees on average test positive for mild depression only when the questionnaire avoids the term "depression". If, however, the questionnaire uses the term "depression", the interviewees test negative for depression. Since the difference in wording causes discrepancies between the two test results, Pelz and Merskey (1982) conjecture that the stigma surrounding depression causes patients' unwillingness to admit to symptoms in the interviews. Thus, it is entirely possible that some patients who visit doctors and complain about pain are actually experiencing depression symptoms; the stigma surrounding depression results in the biased diagnosis.

We define the dependent variable as the logarithm of the number of weekly incidences of insomnia, peptic ulcers, and abdominal pain in each MSA and week.[31] Table 12 presents the results for three types of comorbidity of depression. We find that people are more likely to have an outpatient visit with a diagnosis of insomnia and peptic ulcer within two weeks of experiencing negative stock returns and are more likely to have an outpatient visit with a diagnosis of abdominal pain within one week of experiencing negative stock returns. A local

[31]Each outpatient visit as a result of insomnia is identified with a primary or secondary diagnosis of ICD-9 code 307.42 (Sleep disorder, persistent), 307.41 (Insomnia, transient), 327.0 (Organic insomnia), 780.51 (Insomnia with sleep apnea, unspecified), or 780.52 (Insomnia, unspecified), whereas stomach ulcer and abdominal pain-related outpatient visits have a primary or secondary diagnosis of ICD-9 codes 531-534 (Gastric ulcer, Duodenal ulcer, Peptic ulcer (site unspecified), and Gastrojejunal ulcer) and ICD-9 codes 789.0 (Abdominal pain), respectively. For detailed explanation on how to use the MarketScan data and ICD-9 code to identify insomnia-related visits in outpatient settings, see Wiechers et al. (2014). Insomnia and peptic ulcer are usually treated with prescription medications; we therefore impose the restriction that the outpatient visit for insomnia and peptic ulcer must be accompanied with a prescription to be counted as an incidence in our regression. We do not impose the same restriction for visits related to abdominal pain because this condition is not usually treated with prescription medications. For example, irritable bowel syndrome is often treated with restricted diet and fiber supplements. See Thompson et al. (1999) for a detailed discussion of treatment recommendations for functional bowel disorders and functional abdominal pain.

two-week stock decline of one standard deviation results in a 0.24 percent and a 0.53 percent increases in insomnia and peptic ulcer diagnosis, respectively. A one standard deviation local weekly stock decline results in a 0.47 percent increase in contemporaneous diagnosis of abdominal pain. The faster response of the increase in abdominal pain incidence after stock market declines could be caused by the urgency of abdominal pain and timely access to care. This result shows a strong relationship between depression and illnesses often resulting from depression and stock returns. Since Kamstra et al. (2000) find that poor sleep is associated with lower subsequent stock returns using daylight saving time change, in this aspect, our paper contributes to the literature of sleep and stock returns.

1.5.2 Illnesses resulting from depression treated in inpatient settings

In this section, we investigate how stock returns relate to the incidence of more serious illnesses often resulting from depression, which physicians cannot treat in outpatient settings. The advantage of analyzing critical illnesses resulting from depression in inpatient settings is that patients with critical illnesses have lower price elasticity of demand and are unlikely to delay being hospitalized. Thus, inpatient data reflect the disease prevalence more accurately and timely. We use daily analysis because both substance overdose and myocardial infarction require emergency health treatment.

1.5.2.1 Impact of stock returns on substance abuse incidence

Substance abuse disorder is one of the most common psychiatric disorders in the U.S. For substance abuse patients, inpatient stay allows the patient to leave the old environment and focus on the treatment without temptations and distractions to use the substance. Depression is a risk factor for substance abuse. Grant et al. (2004) find overwhelming evidence that substance abuse disorders are associated with mood and anxiety disorders. The association between substance abuse and depression is consistent with the notion that patients suffering from depression are likely to rely on substances to "self-medicate." The substances might relieve depression symptoms temporarily and cause temporary feelings of euphoria according to the self-medication hypothesis (Markou et al. (1998)).

The MarketScan database covers care in hospital stays (inpatient service). For each hospital stay, MarketScan summarizes information related to the hospital stay and identifies a principal diagnosis for the stay, which is often the discharge diagnosis on the claim. Our dependent variable is the logarithm of the number of daily incidences of substance abuse in each state.[32] The independent variable is the daily observations of the value-weighted stock returns of firms headquartered in the same state. The return of stocks on a non-trading day is set to zero. In addition to the controls and fixed effects we used for previous regressions, we also include a day of the week fixed effect, because certain days of the week could have higher incidence rates of inpatient admission. In Table 13, we use inpatient admission data to examine the relationship between substance abuse and stock returns. We find that people are more likely to experience inpatient stays with a diagnosis of substance abuse after days of negative stock returns. A one standard deviation decline in the local four-, three-, and two-day stock return results in a 1.26 percent, a 0.76 percent, and a 0.87 percent increase in inpatient stays with a diagnosis of substance abuse. This result again shows a consistent relationship between depression and illnesses often resulting from depression and stock returns.

1.5.2.2 Impact of stock returns on circulatory disease incidence

In this section, we investigate the impact of stock returns on incidences of inpatient stays related to myocardial infarction and all circulatory diseases. Myocardial infarction is the leading cause of death in the U.S. Approximately half a million people die from the disease each year in the U.S.[33] Halaris (2009) shows that high comorbidity exists between depression and cardiovascular disease. People with depression are at significantly higher

[32]The regression results are similar when our dependent variable is the logarithm of the number of incidences of illness in each MSA. Each hospitalization as a result of substance abuse, identified with ICD-9 codes 292 (Drug psychoses), 304 (Drug dependence), or 305 (Nondependent abuse of drugs), is counted as one incidence. In this regression, we include individuals regardless of the type of their insurance plans because insurance plans are unlikely the reason that deters patients from hospitalization for two reasons. First, an inpatient stay has lower demand elasticity, and second, the Emergency Medical Treatment and Active Labor Act (EMTALA) passed in 1986 requires hospital Emergency Departments to provide an appropriate medical screening examination (MSE) to individuals seeking treatment regardless of citizenship, legal status, or ability to pay. See https://www.cms.gov/Regulations-and-Guidance/Legislation/EMTALA/ for more details of EMTALA.

[33]See American Heart Association Cardiovascular disease statistics, available at http://www.americanheart.org/presenter.jhtml?identifier=4478 (accessed March 2, 2009) for more detailed statistics related to the incidence of myocardial infarction in the U.S.

risk of developing cardiovascular disease. In another study, stress was a risk factor for cardiovascular disease: Fiuzat et al. (2010) find an increased in the number of cardiovascular deaths a few days after earthquakes. Sweet et al. (2013) find that higher debt is related to increased risk of high blood pressure. Not only do negative sentiment and stress contribute to the incidence of myocardial infarction, excitement and euphoria are also risk factors for developing infarction. Dimsdale (1977) finds that intense emotions cause sudden death through cardiovascular disease. Engel (1971) finds that some paradoxical situations, such as relief from a dangerous situation, pleasure, reunion, or even triumph, also cause sudden death. He hypothesizes that the overwhelming excitation triggers an exaggerated response in the human body that is conducive to myocardial infarction, particularly in people with pre-existing cardiovascular diseases. Using meta-analysis, Nawrot et al. (2011) find that both positive and negative emotions are triggers for myocardial infarction.

Our dependent variable is the logarithm of the number of daily incidences of myocardial infarction and circulatory disease in each state.[34] We include a variety of controls (local unemployment rate and average wage) and fixed effects (geographical and time fixed effects). Controlling for monthly fixed effect is of particular importance in this regression, because the incidence of myocardial infarction is higher during winter months (Fiuzat et al. 2010). Potential explanations offered include colder temperatures, increased level of air pollution, short daylight hours, more respiratory-related symptoms, and increased food intake. In Table 14, we include two stock return variables, one representing positive returns, the other negative returns. We show that a rise in the stock market might be harmful due to the intense emotional pleasure that comes with having more assets, which is too great to bear for people with preexisting cardiovascular conditions. After local stocks experience a positive return, the number of inpatient admissions affiliated with circulatory disease and myocardial infarction both increase. A positive local four-, three-, two-, and same-day market return results in a 1.20 percent, a 1.50 percent, a 1.62 percent, and a 1.91 percent increase in the number of inpatient stays with myocardial infarction diagnosis. When stock returns fall into negative territory, we do not observe the same phenomenon. Ma et al. (2011) and Schwartz et al. (2012) examine stock returns and the incidence of cardiac death using regional data.

[34]Hospitalization as a result of myocardial infarction is identified with a primary diagnosis of ICD-9 code 410 (Acute myocardial infarction), whereas all circulatory disease-related admissions have a primary diagnosis of ICD-9 codes 390 through 459 (Diseases of the circulatory system), which include myocardial infarction.

Ma et al. (2011) use data on coronary heart disease in Shanghai, China and the Shanghai Stock Exchange Composite Index, whereas Schwartz et al. (2012) use data on cardiac death in Los Angeles County and the Dow Jones Industrial Average Index. The only other study using U.S. nationwide data on cardiovascular events is by Fiuzat et al. (2010), who use data from the Duke Databank for Cardiovascular Disease. We extend their research by examining positive and negative returns separately.

Because people with preexisting cardiovascular diseases are prone to myocardial infarction due to excitement and the risk of myocardial infarction increases with old age (Graham et al. (1997)), it is important for elderly investors to understand what type of portfolio best suits them. Some elderly investors might benefit from very conservative investments. When the stock market fluctuates, people with preexisting conditions may experience devastating losses, like failing health and long-term complications after the acute phase of myocardial infarction.

1.6 Falsification tests

To verify that the effects of stock returns on investors' psychological health do not occur by chance, we test the validity of our results by performing two falsification tests. First, we consider whether stock market declines might cause increased drug usage in general. Thus, in the first falsification test, we test whether stock market declines are associated with increases in the usage of the antibiotics. In a CDC study, Hicks et al. (2013) find that antibiotics are commonly prescribed. Four out of five Americans received antibiotics prescriptions in 2010. The usage of antibiotics is likely to capture the effect of the stock returns on general drug usage for reasons not linked directly to the effects of stock returns on sentiment. Second, we consider whether the results are driven by the general economic situation around the world. Therefore, we examine the effect of foreign stock markets on the antidepressant usage of U.S. investors. The logic behind the second falsification test is that the market returns in foreign countries should not affect U.S. investors' psychological health because investors exhibit home bias in their stock holdings, and most do not directly invest in foreign stocks (Kang and Stulz (1997)).[35]

Table 15 presents the results for the two falsification tests. In Columns 1-3 of Table 14,

[35]Kang and Stulz (1997) find that the investments made by non-Japanese investors exhibit home bias.

the dependent variable is the natural logarithm of the number of antibiotic prescriptions. In our data, Amoxicillin (32.2 percent) is the most commonly prescribed antibiotic, followed by Azithromycin (19.9 percent), Fluconazole (5.7 percent), Doxycycline (5.5 percent), and Cephalexin (8.9 percent). We find that stock returns do not affect the usage of antibiotics. In Columns 4-12, we use the stock market returns of three major European countries: the U.K., Germany, and France, because the markets in these countries are comparable to the U.S. market in terms of maturity and efficiency. These countries are the three most developed European Countries according to the International Monetary Fund's World Economic Outlook for 2014. The results show no significant association between foreign stock returns and U.S. investors' antidepressant usage.

1.7 Economic cost

A critical question of any research on wealth shock and people's health is to identify the magnitude of economic cost that is associated with the deterioration of people's health caused by the market decline. In this section, we estimate the medical cost associated with the stock market decline. Our results in Column 3 of Table 3 indicate that a one standard deviation decrease in local stock return results in a 0.417 percent increase in antidepressant usage. During the 2005-2006 period, patients in the MarketScan sample spent $295,000,535 on coinsurance, copayment, and deductible related to antidepressant medications, and $136,153,681 on coinsurance, copayment, and deductible related to outpatient visits resulting a depression diagnosis. Thus, a one standard deviation decrease in local cumulative two-week stock returns results in an additional $13,629,962 (=0.417 percent×(295.5 million /13.3 million)× $295 million×0.5) spent on antidepressants filled in year 2005 and an additional $6,286,470 (=0.417 percent×(295.5 million /13.3 million)× $136 million ×0.5) spent on outpatient visits resulting a depression diagnosis in year 2005.[36]

However, the estimated economic cost of $19.8 million (=$13.6million + $6.2 million) is just the lower bound of the actual cost related to investor depression caused by the market decline in year 2005. First, the medical expenses in this section only include the fees charged to patients, but do not include fees charged to insurance carriers. Second, the medical expenses only include the fees occurred in outpatient settings to treat depression, but do not

[36]The U.S. population was 295.5 million in 2005. The MarketScan sample covers 13.3 million patients.

include the fees occurred in inpatient settings. Third, the medical expenses associated with the treatment for illnesses often resulting from depression (e.g. substance abuse, insomnia, etc.) are not taken into account. Fourth, economic costs associated with productivity loss and life loss of patients and the work-day loss of patients' families are not included in our calculation.

1.8 Conclusion

Our paper provides evidence that local stock returns are a relevant factor in determining investors' sentiment. Using the MarketScan database, we find that a one standard deviation decrease in the local stock return results in a 0.42 percent increase in local antidepressant usage. The complete insurance records of the diverse population in our sample allow us to examine the heterogeneous effect of stock returns on sentiment. We find that the effect is particularly strong for individuals between 45 and 55 years old and 55 and 65 years old, although individuals between 55 and 65 years have a slower response to stock market declines. This effect of stock returns on sentiment is reinforced by higher stock ownership. Local stock returns are also significantly related with certain physical illnesses, which are often results of depression, including insomnia, peptic ulcer, abdominal pain, substance abuse and myocardial infarction. These physical illnesses might cause long-term complications after the acute phase. The true consequences of stock market declines may go beyond the lost wealth in stock assets.

We examined how stock prices affect sentiment. This study contributes to the literature on the mental health consequences of changes in wealth, which includes studies using surveys (McInerney et al. (2013), Cotti et al. (2014), Schwandt (2014)) and studies using medical records (Engelberg and Parsons (2016), Lin et al. (2013)). Our study is also related to research on the impact of changes in wealth on interpersonal relationships (Dettling and Kearney (2014), Lovenheim and Mumford (2013), and Farnham et al. (2011)). Finally, our study is related to studies in neuroeconomics that use experiments to examine the brain's reaction to losses (Rick (2011)).

Our study adds to the literature in several ways. First, we use a commonly prescribed medication as a proxy for an individual's mental health and sentiment (antidepressants are prescribed to 19.54 percent of individuals in our sample). Second, given the diverse data, we are able to examine the heterogeneity of the effect of stock returns on an individual's sentiment. A limitation of our study is that there are potential sample selection biases. For example, the MarketScan dataset covers the healthcare claims of privately insured patients and their families only. Although the privately insured comprise the largest group of U.S. healthcare users (48 percent of the total U.S. population uses private insurance, 14 percent of the population is covered by Medicare, 17 percent is covered by Medicaid or other public insurances, and 5 percent of the U.S. population is not insured), we might not adequately capture trends in the antidepressant usage by Medicare, Medicaid or other public insurance users or the non-insured population. Also, all individuals in the MarketScan Commercial Claims and Encounters database are under 65. It would be interesting to study other populations if the data were available. At last, the MarketScan dataset we use covers insurance records of 2005-2006 only. Thus, we can only examine the short-term effects of stock returns on an individual's mental health. A fruitful area for future research is to explore whether stock returns cause long-term health consequences.

Table 1: Descriptive Statistics

This table summarizes the main variables. There are 4,917,091 non-gatekeeper insurance plan holders between 35 (included) and 64 (included) years of age in the MarketScan database from 2005 to 2006. Panel A summarizes demographic characteristics of the individuals in the sample. Full-time worker is the percentage of workers who are employed full-time in a MSA. Proportion of workers in finance industry is the percentage of workers in finance industry in a MSA. Panel B summarizes medical variables. Antidepressant count by MSA-week is the number of weekly prescription of antidepressants prescribed to individuals living in a MSA. Antidepressant count with diagnosis by MSA-week is the number of weekly prescription of antidepressants prescribed to individuals who are assigned a primary or secondary diagnosis of depression or anxiety disorder. Psychotherapy count with diagnosis by MSA-week is the number of weekly procedures of psychotherapy performed on individuals who are assigned a primary or secondary diagnosis of depression or anxiety disorder. Panel C summarize the stock weekly returns and the macroeconomic variables. Weekly state index return is the weekly, value-weighted return of stocks with firm headquarters in a state. One-Year Volatility of state stock return is the standard deviation of weekly returns over the past 52 weeks. Unemployment is the MSA-month unemployment rate. Average weekly wage is the MSA-quarter wage rate. Annual per capita dividend income is the state-level dividend income divided by the states population.

Regression Sample (n=4,917,091) **Variable**	**Mean**	**S.D.**	**Min**	**Max**
Panel A: Demographics Variables				
Age (Years)	49.77	8.23	35	64
Male (%)	46.27	49.86		
Full-time Worker (%)	75.37	43.08		
Proportion of Workers in Finance Industry (%)	8.51	27.89		
Panel B: Medical Variables				
Antidepressant Count by MSA-week	162.81	286.11	1	3645
Antidepressant Count with Diagnosis by MSA-week	39.54	75.13	1	904
Psychotherapy Count with Diagnosis by MSA-week	55.21	130.31	1	1885
Panel C: Stocks and Macroeconomic Variables				
Weekly State Index Return (Basis Points)	138.91	1975.24	-8577.52	59346.33
One-year Volatility of State Stock Index (Basis Points)	1281.74	1523.99	335.21	12553.04
Unemployment Rate (%)	5.33	1.92	2	25
Average Weekly Wage ($)	672.33	120.14	444	1840
Annual per Capita Dividend Income ($1,000)	2.19	0.71	0.94	6.11

Table 2: Local Market Return and Local Antidepressant Usage

This table presents the regression results of antidepressant usage on market returns. We include non-gatekeeper insurance plan holders between 35 (included) and 64 (included) years of age in the regression sample. Each observation is a MSA-week cell. The dependent variable is the logarithm of the number of antidepressant prescription to individuals living in a MSA during week t who are diagnosed with depression or anxiety disorder. The main independent variable is the cumulative two-week return (t-1,t) of a value-weighted index consisting of public companies headquartered in a state. The two-week return (t-1,t) is measured as the percentage change from the closing index Friday two weeks ago to this Fridays closing index. The return is scaled by trailing 1-year standard deviation of the state stock return. Each column represents a separate regression. Month fixed effects, year fixed effects, MSA fixed effects, controls for unemployment rate and average weekly wage are added gradually. In parentheses are robust standard errors clustered at MSA level. *, **, and *** represent significance at the 10%, 5%, and 1% levels, respectively.

	DEP: Log(Antidepressant with Diagnosis(t))			
	(1)	(2)	(3)	Baseline (4)
Market Return (t-1,t)	-1.350***	-1.338***	-0.420**	-0.417**
	(-3.478)	(-3.449)	(-2.505)	(-2.233)
Month Fixed Effects	YES	YES	YES	YES
Year Fixed Effects		YES	YES	YES
MSA Fixed Effects			YES	YES
Control: Unemployment Rate				YES
Control: Average Weekly Wage				YES
Observations	38,336	38,336	38,336	32,161
R-squared	0.000	0.000	0.926	0.912

Table 3: Local Market Return and Local Antidepressant Usage: Different Lag Structure

This table considers the effect between the market return from (t-3,t) through (t,t+3) on antidepressant usage during week t. The dependent variable is the same as in Table 2. The main independent variable in columns 1-3 is the cumulative return from week t-k to t of a value-weighted index consisting of public companies headquartered in a state. For example, the cumulative return in column 1 is the four-week return which is measured as the percentage change from the closing index Friday four weeks ago to this Fridays closing index. The main independent variable in column 4 is the return from week t of a value-weighted index consisting of public companies headquartered in a state. The main independent variable in columns 5-7 is the cumulative return from week t to t+k of a value-weighted index consisting of public companies headquartered in a state. For example, the cumulative return in column 5 is the two-week return from week t to t+1 which is measured as the percentage change from the closing index last Friday to next Fridays closing index. Thus, columns 1-3 test for the lagged relationship between market returns and subsequent usage of antidepressants, while column 4 tests for the instantaneous relationship between market returns and usage of antidepressants during the same week. Columns 5-7 test for the leading relationship between market returns and usage of antidepressants in the past. The return is scaled by trailing 1-year standard deviation of the state stock return. Each column represents a separate regression. All other control variables and fixed effects are the same as in column 4 of Table 2. In parentheses are robust standard errors clustered at MSA level. *, **, and *** represent significance at the 10%, 5%, and 1% levels, respectively.

	DEP: Log(Antidepressant with Diagnosis(t))						
	(t-3,t)	(t-2,t)	(t-1,t) Baseline	(t)	(t,t+1)	(t,t+2)	(t,t+3)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Market Return of Various Periods	-0.064	-0.261	-0.417**	-0.409**	-0.023	-0.192	0.153
	(-0.397)	(-1.400)	(-2.233)	(-2.176)	(-0.121)	(-1.036)	(0.896)
Month Fixed Effects	YES	YES	YES	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES	YES	YES	YES
MSA Fixed Effects	YES	YES	YES	YES	YES	YES	YES
Control: Unemployment Rate	YES	YES	YES	YES	YES	YES	YES
Control: Average Weekly Wage	YES	YES	YES	YES	YES	YES	YES
Observations	32,161	32,161	32,161	32,161	31,850	31,541	31,227
R-squared	0.912	0.912	0.912	0.912	0.912	0.912	0.912

Table 4: Alternative Specifications for Market Return and Antidepressant Usage

In this table, we include non-gatekeeper insurance plans holders between 35 (included) and 64 (included) years of age in the regression sample. The dependent variable in column 1 is the logarithm of the number of antidepressant prescription to individual living in a MSA during week t subtracting off the average weekly antidepressant usage in the MSA in the prior 2-month. The dependent variable in columns 2-4 subtracts off the average weekly antidepressant usage in the MSA in the prior 3-months, 6-months and 1-year. In columns 1-4, all independent variables, control variables, and fixed effects are the same as in Table 2 column 4. In columns 5, the main independent variable is the cumulative two-week return (t-1,t) of a value-weighted index consisting of public companies headquartered in a state. The two-week return (t-1,t) is measured as the percentage change from the closing index Friday two weeks ago to this Fridays closing index. The return is scaled by a trailing 2-month standard deviation of the state stock return. The market return variable in columns 6-8 is scaled by a rolling 3-months, 6-months and 1-year standard deviation of the state stock return. In columns 5-8, except for the market return variable, all other independent variables, control variables, fixed effects are the same as in Table 2 column 4. The dependent variable is the same as in Table 2. Each column represents a separate regression. In parentheses are robust standard errors clustered at MSA level. *, **, and *** represent significance at the 10%, 5%, and 1% levels, respectively.

	DEP: Log(Antidepressant with Diagnosis(t))							
	Antidepressant Subtracts off Past Rolling Average Use				Market Return Scaled by a Past Rolling S.D.			
	2 Months	3 Months	6 Months	1 Year	2 Months	3 Months	6 Months	1 Year Baseline
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Market Return (t-1,t)	-2.637***	-2.039**	-2.171**	-2.272**	-0.434**	-0.382**	-0.406**	-0.417**
	(-3.492)	(-2.454)	(-2.319)	(-2.013)	(-2.587)	(-2.187)	(-2.168)	(-2.233)
Month Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
MSA Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
Control: Unemployment Rate	YES	YES	YES	YES	YES	YES	YES	YES
Control: Average Weekly Wage	YES	YES	YES	YES	YES	YES	YES	YES
Observations	13,942	13,443	11,347	7,197	32,161	32,161	32,161	32,161
R-squared	0.315	0.313	0.314	0.401	0.912	0.912	0.912	0.912

Table 5: Robustness Checks: Diagnosis, Insurance Plan Type, Provider Type, Dosage, and Psychotherapy

Table 5 column 1 replicates Table 2 column 4. In Table 5 column 1, the dependent variable is the logarithm of the number of antidepressant prescription to individual living in a MSA during week t. In this table, except for column 3, we include non-gatekeeper insurance plans holders between 35 (included) and 64 (included) years of age in the regression sample. The dependent variable counts the weekly antidepressant usage of patients with any diagnosis, with any insurance plan, continuously enrolled in insurance plans, and diagnosed by primary care physicians only in columns 2-5, respectively. In column 6, the dependent variable is the weekly antidepressant usage of first-time patients. In column 7, the dependent variable is mean daily equivalent dosage of antidepressant prescription. The daily equivalent dosage is the daily dose of antidepressant for each patient divided by the recommended starting dose from APA Practice Guideline for each type of antidepressant. In column 8, the dependent variable is the logarithm of the number of psychotherapy procedure performed on individuals living in a MSA during week t. In this table, all independent variables, control variables, and fixed effects are the same as in Table 2 column 4. Each column represents a separate regression. In parentheses are robust standard errors clustered at MSA level. *, **, and *** represent significance at the 10%, 5%, and 1% levels, respectively.

	DEP: Log(Antidepressant(t))						**Mean Dosage(t)**	**Log (Therapy(t))**
	Baseline	ANY diagnosis	ANY plan	Continuously enrolled	Primary care	Excluding refills		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Market Return (t-1,t)	-0.417**	-0.157**	-0.450***	-0.520**	-0.581***	-0.557**	-0.715**	-0.212**
	(-2.233)	(-2.319)	(-2.600)	(-2.589)	(-2.601)	(-2.409)	(-2.373)	(-1.985)
Month Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
MSA Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
Control: Unemployment Rate	YES	YES	YES	YES	YES	YES	YES	YES
Control: Average Weekly Wage	YES	YES	YES	YES	YES	YES	YES	YES
Observations	32,161	33,480	32,675	30,865	29,457	29,649	26,715	31,720
R-squared	0.912	0.954	0.931	0.918	0.861	0.870	0.077	0.911

Table 6: Robustness Checks: Work Status, Disasters, and Housing Price

In this table, we include non-gatekeeper insurance plans holders between 35 (included) and 64 (included) years of age in the regression sample. Table 6 column 1 replicates Table 2 column4. In Table 6 column 1, the dependent variable is the logarithm of the number of antidepressant prescription to individual living in a MSA during week t. In column 2, the dependent variable counts the weekly antidepressant usage of patients who are full-time workers only, all independent variables, controls, and fixed effects are the same as in Table 2 column 4. In columns 3-4, we add a state-week dummy as indicator for the terrorists attacks and natural disasters and Freddie Mac Housing Price index as a state-month control, respectively. All other control variables, fixed effects, independent variables are the same as in Table 2 column 4. The dependent variable is the same as in Table 2. Each column represents a separate regression. In parentheses are robust standard errors clustered at MSA level. *, **, and *** represent significance at the 10%, 5%, and 1% levels, respectively.

	DEP: Log(Antidepressant with Diagnosis(t))			
	Baseline	Full-time Workers		
	(1)	(2)	(3)	(4)
Market Return (t-1,t)	-0.417**	-0.451**	-0.425**	-0.538**
	(-2.233)	(-2.178)	(-2.274)	(-2.008)
Month Fixed Effects	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES
MSA Fixed Effects	YES	YES	YES	YES
Control: Unemployment Rate	YES	YES	YES	YES
Control: Average Weekly Wage	YES	YES	YES	YES
Disaster Days Fixed Effects			YES	
Control: Local Housing Price Index				YES
Observations	32,161	31,225	32,161	17,872
R-squared	0.912	0.894	0.912	0.905

Table 7: Non-linear Relationship between Market Return and Antidepressant Usage

Table 7 column 1 replicates Table 2 column 4. Columns 2-3 break the market return variable, the main independent variable, into two parts, positive and negative returns. We interact the market return variable with its positive/negative indicator dummies. Column 4-5 break the market return variable, the main independent variable, into five parts (quintiles). We interact the market return variable with its quintile indicator dummies. All control variables and fixed effects are the same as in Table 2 column 4. The dependent variable is the same as in Table 2. Each column represents a separate regression. In parentheses are robust standard errors clustered at MSA level. *, **, and *** represent significance at the 10%, 5%, and 1% levels, respectively.

	DEP: Log(Antidepressant with Diagnosis(t))				
	Baseline				
	(1)	(2)	(3)	(4)	(5)
Market Return (t-1,t)	-0.417**				
	(-2.233)				
Market Return (t-1,t) * Negative return dummy		-1.102***	-1.136***		
		(-2.970)	(-2.596)		
Market Return (t-1,t) * Positive return dummy			0.068		
			(0.203)		
Market Return (t-1,t) * Bottom Quintile dummy				-0.933**	-1.225***
				(-2.498)	(-2.757)
Market Return (t-1,t) * Quintile 2 dummy					-2.366*
					(-1.879)
Market Return (t-1,t) * Quintile 3 dummy					-4.625
					(-1.250)
Market Return (t-1,t) * Quintile 4 dummy					1.487
					(1.383)
Market Return (t-1,t) * Top Quintile dummy					0.135
					(0.394)
Month Fixed Effects	YES	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES	YES
MSA Fixed Effects	YES	YES	YES	YES	YES
Control: Unemployment Rate	YES	YES	YES	YES	YES
Control: Average Weekly Wage	YES	YES	YES	YES	YES
Observations	32,161	32,161	32,161	32,161	32,161
R-squared	0.912	0.912	0.912	0.912	0.912

Table 8: Heterogeneous Effects between Different Age Cohorts

In this table, we include non-gatekeeper insurance plan holders in the regression sample. Table 8 columns 1-2 replicate Table 2 column 4 and Table 3 column 2, respectively. In Table 8 columns 1-2, the dependent variable is the logarithm of the number of antidepressant prescription to individual between 35 (included) and 64 (included) years of age living in a MSA during week t. The main independent variable is the cumulative two-week return (t-1,t) of a value-weighted index consisting of public companies headquartered in a state. The two-week return (t-1,t) is measured as the percentage change from the closing index Friday two weeks ago to this Fridays closing index. The three-week return (t-2,t) is measured as the percentage change from the closing index Friday three weeks ago to this Fridays closing index. The return is scaled by trailing 1-year standard deviation of the state stock return. In columns 3-4, columns 5-6, and columns 7-8, the dependent variable counts the weekly antidepressant usage among those aged 35-45, 45-55, and 55-65, respectively. All independent variables, control variables and fixed effects are the same as in Table 8 columns 1-2. Each column represents a separate regression. In parentheses are robust standard errors clustered at MSA level. *, **, and *** represent significance at the 10%, 5%, and 1% levels, respectively.

	DEP: Log(Antidepressant with Diagnosis(t))							
	Baseline		Age 35-45		Age 45-55		Age 55-65	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Market Return (t-1,t)	-0.417**		0.083		-0.382***		-0.242*	
	(-2.233)		(0.631)		(-2.915)		(-1.872)	
Market Return (t-2,t)		-0.261		-0.055		-0.056*		-0.091**
		(-1.400)		(-1.457)		(-1.663)		(-2.373)
Month Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
MSA Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
Control: Unemployment Rate	YES	YES	YES	YES	YES	YES	YES	YES
Control: Average Weekly Wage	YES	YES	YES	YES	YES	YES	YES	YES
Observations	32,161	32,161	28,376	28,376	28,947	28,947	27,047	27,047
R-squared	0.912	0.912	0.843	0.843	0.872	0.872	0.852	0.852

Table 9: Heterogeneous Effects between People with Varying Stock Ownership

In this table, we use two proxies for the stock ownership of individuals. 1) The proportion of people in a MSA working in the finance industry, and 2) local per capita dividend income. Table 9 column 1 breaks the variable proportion of people working in the finance industry into two parts, below or above the national median. We interact the main independent variable, the market return variable, with the lower/higher proportion of finance industry worker dummies. Column 2 breaks the local per capita dividend income into two parts, below or above the national median. We interact the market return variable with lower/higher per capita dividend income dummies. All control variables and fixed effects are the same as in Table 2 column 4. The dependent variable is the same as in Table 2. Each column represents a separate regression. In parentheses are robust standard errors clustered at MSA level. *, **, and *** represent significance at the 10%, 5%, and 1% levels, respectively.

	DEP: Log(Antidepressant with Diagnosis(t))	
	% in Finance Industry	Per Capita Dividend Income
	(1)	(2)
Market Return (t-1,t)	-0.209	-0.053
* Lower Proxy for Stock Ownership Dummy	(-1.225)	(-0.339)
Market Return (t-1,t)	-0.268**	-0.376**
* Higher Proxy for Stock Ownership Dummy	(-2.147)	(-2.304)
Month Fixed Effects	YES	YES
Year Fixed Effects	YES	YES
MSA Fixed Effects	YES	YES
Control: Unemployment Rate	YES	YES
Control: Average Weekly Wage	YES	YES
Observations	32,161	28,376
R-squared	0.912	0.843

Table 10: Heterogeneous Effects Across States with Well-being, Religious, Political Differences

In this table, we use Gallup State of the States poll to measure state level differences across several well-being, religious, and political measures. The measures in Panel A are Overall Well-Being, % Feel active and productive, % Worried about money, % Community recognition,% Exercise frequently,% Very religious, and % Nonreligious. The measures in Panel B are % Republican/Lean, % Democrat/Lean, Democratic advantage, % Conservative, % Liberal, and Conservative advantage. The detailed questions in the survey are listed in the appendix. For each measure, we break it into two parts, below or above the national median. We interact the main independent variable, the market return variable, with lower/higher score dummies. We repeat this for all measures. All control variables and fixed effects are the same as in Table 2 column 4. The dependent variable is the same as in Table 2. Each column represents a separate regression. In parentheses are robust standard errors clustered at MSA level. *, **, and *** represent significance at the 10%, 5%, and 1% levels, respectively.

Table 10: (Cont.)Heterogeneous Effects Across States with Well-being, Religious, Political Differences

	Panel A DEP: Log(Antidepressant with Diagnosis(t))							
	Baseline	**Well-being**					**Religion**	
		Overall	Active	Worry abt. Money	Community	Exercise	Very Religious	Non-religious
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Market Return (t-1,t)	-0.417**							
	(-2.233)							
Market Return (t-1,t)		-0.435***	-0.440***	-0.034	-0.386**	-0.409**	-0.253*	-0.157
* Lower Score Dummy		(-2.667)	(-2.640)	(-0.277)	(-2.292)	(-2.509)	(-1.827)	(-0.991)
Market Return (t-1,t)		-0.061	-0.107	-0.464***	-0.143	-0.118	-0.217	-0.302**
* Higher Score Dummy		(-0.448)	(-0.791)	(-2.593)	(-1.049)	(-0.858)	(-1.316)	(-2.162)
Month Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
MSA Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
Control: Unemployment Rate	YES	YES	YES	YES	YES	YES	YES	YES
Control: Average Weekly Wage	YES	YES	YES	YES	YES	YES	YES	YES
Observations	32,161	32,161	32,161	32,161	32,161	32,161	32,161	32,161
R-squared	0.912	0.912	0.912	0.912	0.912	0.912	0.912	0.912

Table 10: (Cont.)Heterogeneous Effects Across States with Well-being, Religious, Political Differences

	Panel B DEP: Log(Antidepressant with Diagnosis(t))						
	Baseline	**Politics**					
		% Republican	% Democrat	Democratic Adv.	% Conservative	% Liberal	Conservative Adv.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Market Return (t-1,t)	-0.417**						
	(-2.233)						
Market Return (t-1,t)		-0.209	-0.185	-0.215	-0.279**	-0.229	-0.264*
* Lower Score Dummy		(-1.484)	(-1.213)	(-1.351)	(-2.004)	(-1.461)	(-1.908)
Market Return (t-1,t)		-0.273*	-0.288*	-0.256*	-0.182	-0.244*	-0.200
* Higher Score Dummy		(-1.658)	(-1.934)	(-1.793)	(-1.093)	(-1.659)	(-1.184)
Month Fixed Effects	YES	YES	YES	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES	YES	YES	YES
MSA Fixed Effects	YES	YES	YES	YES	YES	YES	YES
Control: Unemployment Rate	YES	YES	YES	YES	YES	YES	YES
Control: Average Weekly Wage	YES	YES	YES	YES	YES	YES	YES
Observations	32,161	32,161	32,161	32,161	32,161	32,161	32,161
R-squared	0.912	0.912	0.912	0.912	0.912	0.912	0.912

Table 11: Heterogeneous Effects Across States with Personality Traits Differences

In this table, we use Five Factor model to measure the state level differences across several cultural factors. The factors are Neuroticism, Extraversion, Agreeableness, Openness, and Conscientiousness. The aggregate personality traits factors for each state are from Rentfrow et al. (2013). For each factor, we break it into two parts, below or above the national median. We interact the main independent variable, the market return variable, with lower/higher score dummies. We repeat this for all factors. All control variables and fixed effects are the same as in Table 2 column 4. The dependent variable is the same as in Table 2. Each column represents a separate regression. In parentheses are robust standard errors clustered at MSA level. *, **, and *** represent significance at the 10%, 5%, and 1% levels, respectively.

Table 11: (Cont.)Heterogeneous Effects Across States with Personality Traits Differences

		DEP: Log(Antidepressant with Diagnosis(t))				
	Baseline	**Five Factor Model of Personality Traits**				
		Neuroticism	Extraversion	Agreeableness	Openness	Conscientiousness
	(1)	(2)	(3)	(4)	(5)	(6)
Market Return (t-1,t)	-0.417**					
	(-2.233)					
Market Return (t-1,t)		-0.168	-0.095	-0.610**	-0.181	-0.512*
* Lower Score Dummy		(-0.732)	(-0.362)	(-2.190)	(-0.744)	(-1.836)
Market Return (t-1,t)		-0.790**	-0.671**	-0.249	-0.693**	-0.305
* Higher Score Dummy		(-2.506)	(-2.584)	(-1.004)	(-2.461)	(-1.219)
Month Fixed Effects	YES	YES	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES	YES	YES
MSA Fixed Effects	YES	YES	YES	YES	YES	YES
Control: Unemployment Rate	YES	YES	YES	YES	YES	YES
Control: Average Weekly Wage	YES	YES	YES	YES	YES	YES
Observations	32,161	32,161	32,161	32,161	32,161	32,161
R-squared	0.912	0.912	0.912	0.912	0.912	0.912

Table 12: Illnesses Resulting from Depression Treated in Outpatient Setting

In this table, we include non-gatekeeper insurance plan holders between 35 (included) and 64 (included) years of age in the regression sample. Table 11 column 1 examines the effect of market returns on the local antidepressant usage during the same week. Table 11 column 2 replicates Table 2 column 4. In Table 11 columns 1-2, the dependent variable is the logarithm of the number of antidepressant prescription to individual living in a MSA during week t. The main independent variable in column 1 is the weekly k return of a value-weighted index consisting of public companies headquartered in a state. The main independent variable in column 2 is the two-week return (t-1,t) of a value-weighted index consisting of public companies headquartered in a state, measured as the percentage change from the closing index Friday two weeks ago to this Fridays closing index. The return is scaled by trailing 1-year standard deviation of the state stock return. In columns 3-4, columns 5-6, and columns 7-8, the dependent variable counts the weekly diagnosis of Insomnia, peptic ulcer, and abdominal pain, respectively, of patients with any insurance plan. All independent variables, control variables and fixed effects are the same as in Table 11 columns 1-2. Each column represents a separate regression. In parentheses are robust standard errors clustered at MSA level. *, **, and *** represent significance at the 10%, 5%, and 1% levels, respectively.

	DEP: Log(Outpatient Incidents(t))							
	Antidepressant		**Insomnia**		**Peptic Ulcer**		**Abdominal Pain**	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Market Return (t)	-0.409**		-0.152		0.009		-0.466**	
	(-2.176)		(-0.715)		(0.017)		(-2.147)	
Market Return (t-1,t)		-0.417**		-0.239**		-0.533**		-0.192*
		(-2.233)		(-2.153)		(-1.968)		(-1.726)
Month Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
MSA Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
Control: Unemployment Rate	YES	YES	YES	YES	YES	YES	YES	YES
Control: Average Weekly Wage	YES	YES	YES	YES	YES	YES	YES	YES
Observations	32,161	32,161	30,512	30,512	6,792	6,792	31,809	31,809
R-squared	0.912	0.912	0.900	0.900	0.326	0.326	0.889	0.889

Table 13: Illnesses Resulting from Depression Treated in Inpatient Setting: The Case of Substance Abuse

This table measures the effect of the market return in days (t-3,t) through (t) on number of hospital admission with substance abuse diagnosis on day t. The dependent variable is the logarithm of the number of hospital admission with substance abuse diagnosis of individual living in a MSA on day t. The individual is above 35 years old. The main independent variable is the cumulative multiple day return (t-k,t) of a value-weighted index consisting of public companies headquartered in a state. For example, in column 1, the return is the one day return which is measured as the percentage change from yesterday's closing price to today's closing index; in column 2, the cumulative return is the two-day return which is measured as the percentage change from the closing index two days ago to todays closing index. The return is scaled by trailing 1-year standard deviation of the state stock return. We include a day of week fixed effects. In this table, all other fixed effects and control variables are the same as in Table 2 column 4. Each column represents a separate regression. In parentheses are robust standard errors clustered at MSA level. *, **, and *** represent significance at the 10%, 5%, and 1% levels, respectively.

	DEP: Log(No. of Hospital Admission with Substance Abuse Diagnosis(t))			
	(1)	(2)	(3)	(4)
Market Return (t)	-0.838			
	(-1.634)			
Market Return (t-1,t)		-0.867**		
		(-1.976)		
Market Return (t-2,t)			-0.756**	
			(-1.974)	
Market Return (t-3,t)				-1.257***
				(-3.407)
Day of Week Fixed Effects	YES	YES	YES	YES
Month Fixed Effects	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES
MSA Fixed Effects	YES	YES	YES	YES
Control: Unemployment Rate	YES	YES	YES	YES
Control: Average Weekly Wage	YES	YES	YES	YES
Observations	4,419	4,419	4,419	4,419
R-squared	0.258	0.258	0.258	0.260

Table 14: Illnesses Resulting from Depression Treated in Inpatient setting: The Case of Circulatory Disease and Myocardial Infarction

In this table, columns 1-4 and columns 5-8 measure the effect of the market return in days (t-3,t) through (t) on number of hospital admission with myocardial infarction and circulatory disease diagnosis, respectively, on day t. The dependent variable is the logarithm of the number of hospital admission with myocardial infarction or circulatory disease diagnosis of individual living in a MSA on day t. The individual is above 35 years old. The main independent variable is the cumulative multiple day return (t-k,t) of a value-weighted index consisting of public companies headquartered in a state. For example, in column 1 and 5, the return is the one day return which is measured as the percentage change from yesterday's closing price to today's closing index; in column 2 and 6, the cumulative return is the two-day return which is measured as the percentage change from the closing index two days ago to todays closing index. The return is scaled by trailing 1-year standard deviation of the state stock return. In all columns, we break the market return variable, the main independent variable, into two parts, positive and negative returns. We interact the market return variable with its positive/negative indicator dummies. We include a day of week fixed effects. In this table, all other fixed effects and control variables are the same as in Table 2 column 4. Each column represents a separate regression. In parentheses are robust standard errors clustered at MSA level. *, **, and *** represent significance at the 10%, 5%, and 1% levels, respectively.

Table 14: (Cont.)Illnesses Resulting from Depression Treated in Inpatient setting: The Case of Circulatory Disease and Myocardial Infarction

	DEP: Log(Number of Hospital Admission(t))							
	Myocardial Infarction				Circulatory System Disease			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Market Return (t) * Positive Return Dummy	1.914***				1.540**			
	(3.767)				(2.554)			
Market Return (t) * Negative Return Dummy	-0.749				0.013			
	(-1.165)				(0.017)			
Market Return (t-1,t) * Positive Return Dummy		1.615***				1.534***		
		(3.821)				(3.082)		
Market Return (t-1,t) * Negative Return Dummy		-1.066*				-0.926		
		(-1.891)				(-1.408)		
Market Return (t-2,t) * Positive Return Dummy			1.497***				1.048**	
			(3.809)				(2.320)	
Market Return (t-2,t) * Negative Return Dummy			-0.512				-1.138*	
			(-1.003)				(-1.914)	
Market Return (t-3,t) * Positive Return Dummy				1.197***				0.097
				(3.272)				(0.227)
Market Return (t-3,t) * Negative Return Dummy				-0.094				-0.476
				(-0.195)				(-0.841)
Day of Week Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
Month Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
MSA Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
Control: Unemployment Rate	YES	YES	YES	YES	YES	YES	YES	YES
Control: Average Weekly Wage	YES	YES	YES	YES	YES	YES	YES	YES
Observations	24,577	24,577	24,577	24,577	18,763	18,763	18,763	18,763
R-squared	0.750	0.750	0.750	0.750	0.607	0.607	0.607	0.607

Table 15: Falsification Tests

In this table, we include non-gatekeeper insurance plans holders between 35 (included) and 64 (included) years of age in the regression sample. In columns 1-3, the dependent variable is the logarithm of the number of antibiotics prescription to individual living in a MSA during week t; the main independent variable in is the one week (t), two-week (t-1,t), or three-week (t-2,t) return of a value-weighted index consisting of public companies headquartered in a state within U.S. In columns 4-12, the dependent variable is the logarithm of the number of antidepressant prescription to individual living in a MSA during week t. In columns 4-12, the main independent variable in is the one week (t), two-week (t-1,t), or three-week (t-2,t) return of U.K. FTSE100 index, German DAX index, and France CAC40 index. The return measures are scaled by its trailing 1-year standard deviation. Month fixed effects, year fixed effects, MSA fixed effects, controls for unemployment rate and average weekly wage are included. Each column represents a separate regression. In parentheses are robust standard errors clustered at MSA level. *, **, and *** represent significance at the 10%, 5%, and 1% levels, respectively.

	DEP: Log(Incidents(t))											
	Antibiotics			Antidepressant with Diagnosis								
				U.K. FTSE100			German DAX			France CAC 40		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Market Return (t)	0.002			0.053			-0.147			-0.013		
	(0.015)			(0.271)			(-0.706)			(-0.065)		
Market Return (t-1,t)		-0.021			0.070			-0.064			0.020	
		(-0.296)			(0.512)			(-0.412)			(0.123)	
Market Return (t-2,t)			0.001			0.088			-0.144			-0.015
			(0.038)			(0.740)			(-1.063)			(-0.107)
Month Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
MSA Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Control: Unemployment Rate	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Control: Average Weekly Wage	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	33,440	33,440	33,440	32,161	32,161	32,161	32,161	32,161	32,161	32,161	32,161	32,161
R-squared	0.955	0.955	0.955	0.912	0.912	0.912	0.912	0.912	0.912	0.912	0.912	0.912

Chapter 2

Are We Back to the Old Ways? Assessing the Impact of the Global Research Settlement Expiration.

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2.1 Introduction

In 2002-2003, numerous incidents involving analysts show that they compromise the integrity of research in order to generate underwriting business for their employers.[1] As a consequence, a number of regulations, including the new National Association Of Securities Dealers (NASD) Rule 2711 and the amended New York Stock Exchange (NYSE) Rule 472, were enacted to address and minimize the perceived conflicts of interests on the part of security analysts and the investors. On April 23rd, 2003, an agreement was reached between the Securities and Exchange Commission (SEC), the NYSE, the NASD, the New York Attorney General's Office (NYAG), and ten of the largest investment banks, the sanctioned banks, in the United States. This agreement, known as the Global Research Settlement (GRS), mandated that the ten of the largest investment banks, the sanctioned banks, to pay $432.5 million to contract with independent research.

On July 27, 2009, the GRS expired. Instead of codifying the GRS into permanent rules, the SEC proposed to a New York federal court that parts of the GRS be stripped away. Judge Pauley approved several changes and made funding independent research no longer mandatory for sanctioned banks.[2] Eliot Spitzer, former New York Attorney General, opposed the change and argued that the SEC should codify the GRS into rules that applied to everyone.[3] In a 2012 report to the Congressional Committees, the Government Accountability Office (GAO), an independent agency which provides to the United States Congress audit, evaluation and investigative services, also recommended that the SEC review the provisions of the GRS which are not currently codified into regulation and decide whether those provisions should be adopted.[4] The same report states that market participants and observers interviewed by GAO admitted that the GRS was effective in mitigating analysts'

[1]For the complete litigation release see Litigation Release No. 18115, April 28, 2003, which is available at http://www.sec.gov/litigation/litreleases/lr18115.htm.

[2]The judge also eased other restrictions involving the dealings between underwriters and analysts.

[3]Eliot Spitzen states "that was the moment when Wall Street should have been reformed and it seems no one did anything." See "SEC Didn't Expand Upon Stock-Abuse Settlement," *The Wall Street Journal*, March 19, 2010.

[4]See "Report to Congressional Committees: Additional Actions Could Improve Regulatory Oversight of Analyst Conflicts of Interest," January 2012. In the report, Government Accountability Office (GAO) alleges that "by not formally assessing whether codifying any of the Global Settlement's remaining terms provides an effective way of furthering investor protection, SEC may be missing an opportunity to provide the same level of protection for all investors."

conflicts of interests.[5]

This study evaluates the impact of the GRS expiration on analyst recommendations and their issuing institutions. To our knowledge, this study is the first to examine the effects of the GRS *expiration*.[6] Using a large sample of recommendations between January 2000 and April 2013, this study aims to provide answers to the following research questions. First, are sanctioned banks subject to conflicts of interests again after the GRS expiration? Second, have informativeness of analysts' recommendations changed after the GRS expiration? Third, what types of independent research firms (IRFs) are more likely to survive after the GRS expiration? To answer these questions, we first compare optimism of recommendations issued by different groups of analysts before and after August, 2009. Then, we examine the market reaction (informativeness) to the recommendations issued by analysts of different institutions before and after August, 2009. Lastly, we investigate the survival of different IRFs and examine factors affecting the survival probability of IRFs after the GRS expiration.

Our results can be summarized as follows. First, we document increase of optimistic recommendations issued by analysts at sanctioned banks after the GRS expiration. Multivariate regression analysis confirms our findings: analysts at sanctioned banks become more optimistic compared to the control sample after the GRS expiration. The results imply that sanctioned banks issue inflated recommendations in the hope of securing future underwriting business. By examining the optimism of recommendations issued by IRFs, we find that funded IRFs (IRFs that received funds from the sanctioned banks) act more optimistic compared to non-funded IRFs (IRFs that did not receive funds from the sanctioned banks) after the GRS expiration, which might imply that funded IRFs issue positively skewed rec-

[5] Examples cited by interviewees as to why the GRS is effective include: "securities research is more independent" (buy-side money manager) and "the regulatory reforms provide a compliance structure that requires broker-dealers to manage their analyst conflicts" (state securities regulators). See details in the Government Accountability Office "Report to Congressional Committees: Additional Actions Could Improve Regulatory Oversight of Analyst Conflicts of Interest," January, 2012.

[6] We argue that to determine whether GRS was effective and whether it is necessary to codify the GRS into permanent rules, examining the effect of GRS *expiration* is important. To a certain extent, examining the *expiration* of the Settlement is more important than examining the enactment of the Settlement, since the market condition and the behavior of market participants might have changed since the enactment. As Stiglitz (2009) states "...a great degree innovation has recently been directed at circumventing laws and regulations designed to ensure the efficiency, equity, and stability of the financial sector." If sanctioned banks learned to circumvent the GRS over time, then codifying the GRS into permanent rules would not serve its purpose. However, we show that sanctioned banks alter their behavior significantly following the GRS expiration. Thus we do not find evidence that the GRS could be circumvented.

ommendations in an effort to win renewed contract with the sanctioned banks.

Second, by examining the market reaction of recommendations, we find that after the GRS expiration, pessimistic recommendations issued by sanctioned banks cause significantly larger market reaction than those issued by the control group.[7] However, this distinction was not found for optimistic recommendations. The result indicates that situations at the covered firm are indeed quite unfavorable when pessimistic recommendations are issued. Among IRFs, optimistic recommendations by funded IRFs issued after the GRS expiration cause smaller market reaction than those issued by non-funded IRFs. It suggests that after the GRS expiration, the loss of the mandatory monetary support decreases research capacity at funded IRFs. Pessimistic recommendations by funded IRFs, however, cause significantly larger market reactions after the GRS expiration compared to those issued by non-funded IRFs. This result could be explained by the reluctance on the funded IRFs' side to issue pessimistic recommendations after the GRS expiration. When issued, such recommendations indicate quite unfavorable situations at the covered firm and induce larger market reaction.

Third, by examining the survival of IRFs, we find the number of IRFs increases after the enactment of the GRS and decreases after the GRS expiration, with the GRS expiration affecting the new IRFs (IRFs started after the GRS enactment) the most. Hazard ratio analysis shows that after the GRS expiration, larger broker size and more innovative recommendations contribute to longer survival time among IRFs. When we examine the survival of new IRFs only, we find that the broker size and analyst experience matter the most. Descriptive tests of characteristics and behavior of analysts employed at the funded IRFs and at the non-funded IRFs confirm this pattern. For funded IRFs, we find that their average broker size is significantly smaller after the GRS expiration, indicating that they lost significant amount of business and are forced to downsize. On the whole, we find evidence that the GRS expiration affects the market structure of the IRFs.

Our study contributes to the literature in two ways. Our study is the first to evaluate the changes brought by the GRS expiration. Sanctioned banks are again subject to the conflicts of interests and issue inflated recommendations after the GRS expiration as they did pre-GRS (i.e. before the GRS enactment). Our results thus call into question the SEC's decision

[7] I.e. Investment Bank/ Brokers (IBBs), Major Institutional Brokers (MIBs), Major Regional Brokers (MRBs), and Small Regional Brokers (SRBs) that are not sanctioned by the GRS. Section IV explains the reason for this choice of control group.

of stripping away parts of the GRS and not to codify the GRS into permanent rules. Second, the expiration of the GRS also means no mandate for sanctioned banks to use independent research and thus eliminates the need for many IRFs. Hazard ratio analysis show IRFs with larger broker size, more experienced analysts, and more innovative recommendations are more likely to survive. Thus, by altering industry landscape and consolidating the industry, a settlement that is aimed to restrict one type of financial institutions (sanctioned banks) might have an enormous impact on the structure and survival of other financial institutions.

The reminder of the paper is organized as follows. We document the changes in regulatory environment and summarize the literature in Section 2. We discuss the sample and the construction of variables in Section 3. Section 4 and Section 5 report the results of our statistical test of recommendation optimism and market reaction, respectively. Section 6 investigates factors that influence survival of IRFs after the GRS expiration. In Section 7, we present the robustness check of our paper. Policy implications are described in Section 8. We conclude in Section 9.

2.2 Regulation Background and Literature Review

2.2.1 Regulation Background

In 2002, a Wall Street Journal article reported that investment bankers maintain inappropriate influence over research analysts at Merrill Lynch. In the internal e-mails, Merrill Lynch analysts showed dissatisfaction with the performance of the stocks they had recommended to investors. The Attorney General of New York, Eliot Spitzer, thus began investigating investment banks on conflicts of interests issues. On April 28, 2003, an enforcement agreement was reached on between the SEC, the NASD, the NYSE, and ten of the United States' largest investment banks (sanctioned banks). The GRS applies directly to the sanctioned banks by requiring them to pay fines and penalties totaling roughly \$1.4 billion. Out of the \$1.4 billion, \$432.5 million are used to fund independent research. The sanctioned banks are required to contract with no fewer than three IRFs (i.e. "funded IRFs").[8] The funded

[8]See "Global Research Analyst Settlement Final Judgment Addendum," which is available at: http://www.sec.gov/litigation/litreleases/finaljudgadda.pdf.

IRFs must cover firms with market cap above \$150 million.[9] The independent research reports produced by funded IRFs are then provided to investors of the sanctioned banks. To maintain their independent status, all IRFs are not allowed to perform investment banking business or brokerage service.

The GRS expired on July 27, 2009 and SEC proposed to a New York federal court that parts of the GRS be stripped away. The U.S. District Judge Pauley approved the changes.[10] Thus the sanctioned banks are no longer forced to contract with IRFs and provide independent research to their investors. Certain provisions of the GRS remained. Then, in April 2012, the Jumpstart Our Business Startups (JOBS) Act relaxes the existing FINRA rule of 40 day of research quiet period for coverage by affiliated analysts immediately following the IPO of an emerging growth company (EGC). The JOBS Act also allows affiliated analysts to participate in communications with investment bankers and the management of the client company, if the client company is classified as an EGC.[11] According to the SEC, the JOBS Act has no impact on the remaining provisions of the GRS for the sanctioned banks.[12]

The IRFs are especially hard hit by the GRS expiration because it came at a time when the financial crisis was in full bloom and the future market looked bleak. Intense competitions among IRFs also contribute to the failure of many IRFs, since oversupply of independent research pushed down the prices each IRF can charge. After the GRS expiration, many IRFs cease to exist. The surviving IRFs began to hunt for new revenue channels:

(i) They began to increase small cap stock coverage. With only 29% of small caps have analyst coverage as of 2013, it seems to be a promising opportunity.[13]

(ii) They began to launch corporate access programs, by arranging meetings between clients and corporate executives for a fee.

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[9] For a detailed interpretation of the rules, see "Global Research Settlement: Staff Interpretive Responses," November 2, 2004, which could be accessed through: http://www.sec.gov/divisions/marketreg/mr-noaction/grs110204.htm.

[10] The approval of rule change is discussed in "WSJ: SEC Didn't Expand Upon Stock-Abuse Settlement," March 19, 2010.

[11] Dambra, Field, Gustafson, and Pisciotta (2016) find that analysts affected by the JOBS Act issue reports of lower quality compared to reports issued by other analysts. For more discussions on analyst behavior and JOBS Act, please refer to Corwin, Larocque, and Stegemoller (2016), and Wu, Wilson, and Wu (2015).

[12] For a detailed interpretation of the JOBS Act, see "Jumpstart Our Business Startups Act: Frequently Asked Questions About Research Analysts and Underwriters," August 22, 2012, which could be accessed through: https://www.sec.gov/divisions/marketreg/tmjobsact-researchanalystsfaq.htm.

[13] For example, in May, 2008, London Stock Exchange contract Argus Research, Independent International Investment Research Plc, and Pipal Research, three IRFs, to provide independent research on smaller caps.

(iii) They moved into financial advisor function, by offering advice and sharing profit in clients' portfolio.

(iv) They set target hedge funds as their potential customers by not limiting their service to valuation and research.

2.2.2 Literature Review

Various studies examine the relationship of analyst behavior and the GRS. Barber, Lehavy, and Trueman (2007) find that IRFs produce more informative buy recommendations than those produced by investment banks. In contrast, investment banks' hold and sell recommendations outperform those of IRFs, suggesting reluctance by investment banks to downgrade stocks whose prospects dimmed during the bear market of the early 2000s. Kadan, Madureira, Wang, and Zach (2009) document that following the GRS many banks have migrated from the traditional five-tier rating system to a three-tier system. They find that optimistic recommendations have become less frequent and more informative, whereas neutral and pessimistic recommendations have become more frequent and less informative, and the overall informativeness of recommendations has declined. The likelihood of issuing optimistic recommendations no longer depends on affiliation with the covered firm, although affiliated analysts are still reluctant to issue pessimistic recommendations. Our paper is also related to Clarke, Khorana, Patel, and Rau (2011), who find that analysts from various types of institutions issue fewer strong buys following the enactment of the GRS. While downgrades become more prevalent following the GRS, they are significantly less informative. They find IRFs set up after the GRS are of inferior quality; they issue more optimistic and less innovative recommendations that generate lower announcement period returns than independent firms existing prior to the GRS. Dubois, Fresard, and Dumontier (2014) expand on this theme and exploit the effect of the enactment of Market Abuse Directive (MAD), a directive that resembles the GRS in the European Community. They find MAD reduced optimistic investment advice across European countries. Buslepp, Casey, and Huston (2014) examine the performance of funded IRFs during the GRS period which ends on July 26th, 2009. They find that funded independent research is of lower quality than research by sanctioned banks and research by non-funded IRFs based on future performance of recommendations and the market reaction to the recommendations.

Our paper expands upon the above mentioned research by providing evidence on the impact of the GRS expiration. The sample periods end in June 2003, December 2004, and December, 2007 respectively in Barber, Lehavy, and Trueman (2007), in Kadan, Madureira, Wang, and Zach (2009), and in Clarke, Khorana, Patel, and Rau(2011). These three papers look into the effect of the GRS itself and not the GRS expiration. We are able to add to the results of Barber, Lehavy, and Trueman (2007) by showing that following the GRS expiration, sanctioned banks issue more optimistic recommendations. Pessimistic recommendations, if issued, cause significantly larger market reaction, implying that they their practices are threatened by conflicts of interests. Some of the pre-GRS conflict-of-interest practices have been discussed by Barber, Lehavy, and Trueman (2007). Our results complement the findings of of Kadan, Madureira, Wang, and Zach (2009) by showing a reversal of what was observed in their paper, namely an increase of optimism on the sanctioned banks' side after the expiration of the GRS. Our study complements that of Clarke, Khorana, Patel, and Rau (2011) by examining the survival of IRFs after the expiration of the GRS. We show larger broker size, more innovative recommendations, and more experienced analyst lead to longer survival time among IRFs. Our paper differs significantly from Buslepp, Casey, and Huston (2014). Indeed, it is likely that sanctioned banks provide better quality research due to their stronger research capacity as claimed in their paper. However we argue that the continuation of GRS was necessary to enable investors to seek a second opinion at no additional cost. The fear that investors will compare reports produced by analysts at the sanctioned banks and reports produced by analysts at the IRFs could induce sanctioned bank to issue less biased recommendations. A more detailed discussion of this issue is presented in the policy implication section of our paper.

2.3 Data and Variable Construction

The data for this study come from the IBES database and include all recommendations recorded from January 2000 through April 2013. The IBES database identifies the name of the firm covered, the analyst issuing the recommendation, the institution that the analyst worked for, the date and time of the recommendation issuance, and a rating based one a five-point scale (1=strong buy, 2=buy, 3=hold, 4=underperform, and 5=sell).[14] We obtain individual stock return data and market value-weighted index stock return data from the CRSP. As in Iskoz (2003) and Agrawal and Chen (2008), market reaction to recommendations are proxied using cumulative abnormal return (CAR) adjusted using Fama-French three factors model. We calculate the (-1,0) announcement period return surrounding recommendation days. We exclude institutions that issued less than 50 recommendations from 2000 to 2013 and we focus on U.S. firms. We use September, 2002 as cutoff for the enactment of the GRS following Kadan, Madureira, Wang and Zach (2009). We use July, 2009 as the expiration time of the GRS.[15]

We identify different types of institutions to examine the effects of the GRS expiration on the behavior of analyst at the institutions. Following Cowen, Groysberg, and Healy (2006), we utilize Nelson's Directory of Investment Research (NDIR) (1996 to 2006) to classify institutions into five major categories: Independent Research Firm (IRF), Investment Bank/ Broker (IBB), Major Institutional Broker (MIB), Major Regional Broker (MRB), and Small Regional Broker (SRB). IRFs are institutions engaged solely in research, while IBB, MIB, MRB and SRB (i.e., "non-IRFs") provide research and engage in investment banking business. For institutions missing in the NDIR and institutions which came into existence after 2006, we supplement the NDIR classification method with information from SEC[16] and online resources such as firm's websites to identify the types of institutions. We exclude institutions if we are not able to identify their types with confidence.

We define affiliated banks as investment banks that acted as a lead advisor at any time over the three years prior to the release of the recommendation following Clarke, Khorana, Patel, and Rau (2011). Sanctioned banks are the ten investment banks sanctioned by the SEC (i.e. Bear Stearns, Credit Suisse First Boston, Deutsche Bank, Goldman Sachs, J.P. Morgan Chase, Lehman Brothers, Merrill Lynch, Morgan Stanley, Salomon Smith Barney,

[14]In this paper, we focus our research on recommendations rather than other information produced by institutions (i.e., earning forecast), since the GRS was designed to "restore investor confidence in analysts' work" and "...As a result of the Commission's examination findings, and given the serious concerns about the conflicts of interest analysts face that may taint or bias their *recommendations*..." as seen in the "Testimony Concerning Global Research Analyst Settlement," by William H. Donaldson, Chairman of SEC, before the Senate Committee on Banking, Housing and Urban Affairs, on May 7, 2003.

[15]See "Report to Congressional Committees: Additional Actions Could Improve Regulatory Oversight of Analyst Conflicts of Interest," January 2012.

[16]See "Historical Archive of FOIA Broker-Dealer Company Information Reports" for the classification of institutions, available at http://www.sec.gov/foia/docs/bd-archive.htm.

and UBS Warburg). An institution could be both a sanctioned bank and an affiliated bank. By examining affiliated banks and sanctioned banks separately, we can test whether or not analysts issue biased recommendations for related firms.[17] For IRFs, we categorize them into two exclusive subgroups: funded IRFs[18] and non-funded IRFs. Sanctioned banks are required by the GRS to contract with no fewer than three IRF. These IRFs are funded IRFs, the rest of IRFs are non-funded IRFs. We also divide IRFs by their start date following Clarke, Khorana, Patel, and Rau (2011): IRFs that came into existence before the GRS enactment are old IRFs, IRFs that started after the GRS enactment are new IRFs.

We are aware that institution/analyst characteristics could affect optimism of their recommendations and corresponding market reaction. We therefore define analyst experience as the number of years since an analyst's recommendation first appeared in the IBES database. Broker size is defined as the number of analysts the institution employed in the year of the recommendation issuance. We also define a binary variable to indicate the all star status of an analyst, which takes the value of one if the analyst has earned the Institutional Investor All-Star credential in the year of the recommendation issuance and zero otherwise.

Several studies have found association between innovative/ supported recommendation and the market reaction to those recommendations. Michaely and Womack (2006) find that recommendations that are issued concurrently with an earnings forecast produce larger price drifts than unsupported recommendations. We define supported recommendation as those accompanied by earning forecast release by the same analyst on the day prior to or the day of the release of the recommendation. In addition, we define innovative recommendation as recommendations accompanied by an one-year ahead EPS forecast revision, which is released on the same day or on the prior day of the release of a recommendation, and the one-year ahead EPS forecast revision is greater than (or less than) both the prevailing consensus of EPS forecast and the analyst's prior forecast.[19] The definition of innovative recommendation

[17] Michealy and Womack (1999) find that analysts show significant evidence of bias when recommending the companies their employer recently take public. Cliff (2007) examines the investment performance of stock recommendations made by analysts employed by lead underwriters relative to analysts independent of investment banking. He finds that both buy and hold recommendations from affiliated analysts underperform relative to stocks recommended by independent analysts.

[18] As identified from Buslepp, Casy, Huston (2013), "Did they get what they paid for? The Global Analyst Research Settlement and analyst research quality," working paper, Appendix B, page 26.

[19] Low innovation recommendations are those recommendations accompanied by a forecast that is between the analyst's own prior forecast and the prior consensus estimate. While constructing the prevailing consensus, we exclude forecasts issued by the institution the analyst works for. Earning forecast information are

in our study follows the study of Gleason and Lee (2003), who find that highly innovative forecast bring new information to the market and cause bigger price drifts.

We exclude some observations due to reshuffling caused by a transition process. The transition process, in which traditional five-tier rating system was substituted by a new three-tier or four-tier rating system, begins in 2002, with all ten sanctioned banks participating in the transition. Most institutions moved from the traditional five-tier system to a coarser three-tier or four-tier rating system over a short period of time. Kadan, Madureira, Wang and Zach (2009) find that institutions reshuffle outstanding recommendations which the majority of the reshuffled outstanding recommendations being downgrades from former "strong buy", "buy" or "hold" recommendations. They find the reshuffle of recommendations at the time of the transition does not seem to convey new information to the market. To exclude the reshuffle caused the transition between rating systems at each institution which opt for this change, we identify the last date in which each recommendation type appears in the IBES database for each institution, based on these dates, we infer the time periods during which the institution used a three-,four-, or five-tier rating system. We then exclude all recommendations issued by the institution doing its switch days.

Table 1 presents summary statistics on analysts at IRF, IBB, MIB, MRB, SRB, and MSB in our sample. Across the full sample period, we find that non-IRF analysts have significantly more experience than IRF analysts and are more likely to be employed at larger institutions. Non-IRF analysts are also significantly more likely to be all-star analysts. IRF analysts are more likely to support their recommendations with earnings forecasts and less likely to issue high innovative recommendations. This suggests that, in general, analysts employed by IRFs might have less knowledge of the firms covered and thus act as "followers" in the market. Among the non-IRFs, we find that the number of analysts an institution employs is the most for IBB, followed by MIB, MRB, MSB and SRB. This result gives us confidence about the accuracy of our identification method.

retrieved from IBES detailed history database.

2.4 Impact of GRS Expiration on Analyst Recommendations

2.4.1 Descriptive comparison between the proportion of optimistic recommendations issued by IRFs and non-IRFs

Numerous sources have documented that after the enactment of the GRS, sanctioned banks seemed to avoid issuing inflated recommendations.[20] Anecdotal evidence, however, shows that after GRS expiration, sanctioned banks, by issuing inflated recommendations, revert to pre-GRS practices. For example, in 2012, Massachusetts Secretary of the Commonwealth William Galvin stated Morgan Stanley's investment bankers maintained an improper influence over research analysts. In this section, we investigate whether after GRS expiration, sanctioned banks indeed revert to their pre-GRS practices. We hypothesize that, sanctioned banks issue more optimistic recommendation after the GRS expiration than during the GRS.

Table 2 summarizes the levels and changes of upgrades and downgrades issued by analysts at sanctioned banks and at non-sanctioned banks. We compare recommendations issued by analysts in the *Post-Reg* (the period following the GRS expiration: August 2009-April 2013) to the ones issued in *Reg* (the period between the enactment of the GRS and its expiration: September 2002-July 2009) and in *Pre-Reg* (the preceding period: January 2000-August 2002) periods. Panel A reports evidence on the distribution of upgrades recommendations. The GRS have an unambiguous impact on the proportion of upgrades — across the board, both types of analysts significantly reduce the proportion of upgrade recommendations. Before the regulatory changes, approximately 18.1% (23.9%) of recommendations issued by sanctioned bank (non-sanctioned bank) analysts were upgrades. After the regulatory changes, this percentage fell to approximately 12.5% (15.8%), suggesting that these firms make a deliberate decision to reduce the proportion of strong buy recommendations to avoid any appearance of conflicts of interests after the enactment of the GRS. Analysts at non-sanctioned banks also issue a higher fraction of upgrades in all the three periods: *Pre*, *Reg* and *Post*, than their counterparts employed by sanctioned banks. After the GRS

[20] Madureira (2004) find that sanctioned banks issue more downgrades in 2002-2003, which suggests that the GRS is productive in curbing effects of conflicts of interest. Guan, Lu, and Wong (2012) document a significant reduction in the optimistic recommendations issued by sanctioned banks in 2004–2007.

expiration, analysts at sanctioned bank (non-sanctioned bank) increase their upgrades by 2.50 percentage points (1.29 percentage points). This difference in the increase of proportion of upgrades implies that sanctioned banks, previously restricted by the GRS, now have a tendency to issue optimistic recommendations and might aim to act as future underwriter for the firm.

Table 2, Panels B report further evidence on frequency of recommendations. In general, we observe that sanctioned banks are becoming to be less pessimistic. Sanctioned banks are issuing significantly less downgrades in *Post* period compared to *Reg* period, a 2.04 percentage points decrease, while the proportion of downgrades issued by non-sanctioned banks decreases only by 0.72 percentage points in *Post* period.

2.4.2 How do analysts at sanctioned banks behave after the GRS expiration?

The descriptive analysis does not control for other factors that might affect the recommendations issued. It is clear that sanctioned banks status is not orthogonal to observable institution characteristics. Moreover, it is possible that firms covered by sanctioned banks and non-sanctioned banks are different. To incorporate these differences, we estimate the following logit regression:

$$\begin{aligned} \ln\left(\frac{P_{ijt}}{1-P_{ijt}}\right) &= \alpha + \beta_1 SB_j \times P\_GRS_t + \beta_2 SB_j + \beta_3 P\_GRS_t \\ &\quad + \beta_4 AB_j + \beta_5 Exp_j + \beta_6 AS_j + \beta_7 Log(BS_j) + \nu_i + \gamma_t + \varepsilon_{ijt} \end{aligned} \tag{1}$$

where P_{ijt} is the probability of issuing an upgrade or downgrade for a covered firm i by analyst j at time t. The dependent variable takes a value of one if the recommendation is an upgrade and zero otherwise in Columns (1), (3), and (5) and takes a value of one if the recommendations is a downgrade and zero otherwise in Columns (2), (4), and (6). For dependent variables, we focus on upgrades/downgrades rather on strong/buy and underperform/sell for two reasons: (1) there are unproportionally fewer underperform/sell than strong/buy,[21] while the number of upgrades and downgrades are fairly symmetrical, and (2) some institutions rate firms on a five-tier rating or four-tier system[22], while other institutions rate on a three-tier rating. Therefore, recommendation based on levels (i.e. strong/buy and underperform/sell) are not quite comparable. SB_j is a binary indicator that takes a value of one if the analyst works for one of the ten sanctioned banks in the GRS. P_GRS_t is a binary indicator that takes a value of one if the recommendation is issued after the GRS expiration. Firm and year fixed effects are included in all multivariate regressions to control for firm characteristics not varying over time and time trend in analyst behavior, respectively.[23] Robust standard errors are clustered at the firm level.

The sample period runs from January 2007 through April 2013. We choose the year 2007 as the start of our sample used in multivariate regression for the following reason: (1) in 2005, the Financial Services Authority (FSA) developed rules requiring greater disclosure of research service and products eligible to be paid with client commission. The rules came into effect beginning early 2006, with a transitional period ending June 2006; (2) similar to its UK counterpart, the SEC published guidelines regarding client commission and brokerage/research services in 2006.[24] In Columns (1) and (2) in Table 3, while we examine the behavior of analyst employed at sanctioned banks, we restrict our sample to include only recommendations issued by non-IRFs (i.e. IBBs, MIBs, MRBs and SRBs) as identified from Nelson's directory. IRFs, mostly small firm, might have fundamentally different structure compared to sanctioned banks.

Columns (1) and (2) in Table 3 present the marginal effects. The estimate of the interaction term represents the change that occurs in the difference in percentage of upgrades

[21]For example in the period between January, 2007 and April, 2013, we observe that Sanctioned banks issued 54165 strong buy/buy recommendations and 10373 underperform/sell recommendations, which roughly corresponds to a ratio of 5:1.

[22]After November 4th, 2002, Goldman Sachs switched to a three-tier system from a four-tier system, it no longer issues strong buy recommendations.

[23]The number of observations in Columns (1) and (2), (3) and (4), (5) and (6) are slightly different due to the fact that we use a conditional logit model to control for firm characteristics not varying over time. In stata, observations are dropped if all positive or all negative outcomes are encountered within group using conditional logit command.

[24]For the 2006 guidance, see "Commission Guidance Regarding Client Commission Practices Under Section 28(e) of the Securities Exchange Act of 1934," p.19, SEC, July 16, 2008. Also the 2006 guidance on commission may be particularly important because the last time a substantive guidance on commission use was issued was 21 years ago. In 1975, Congress enacted Section 28(e) of the Securities Exchange Act of 1934 and abolished fixed commission rates.

between the sanctioned bank and the control group (i.e. IBBs, MIBs, MRBs and SRBs that are not sanctioned by the GRS) before the GRS expiration compared to the difference after the GRS expiration. The estimate of β_1 indicates that after the GRS expired, sanctioned banks become more optimistic relative to the control sample: the probability that sanctioned banks issue an upgrade increase by 3.3 percentage point relative to the control group.

This result suggests that conflicts of interest cause sanctioned banks that are no longer restricted by the GRS after its expiration to issue inflated recommendations. This result is consistent with the anecdotal evidence from senate committee hearing which suggests that analysts are encouraged to write positively skewed reports in an effort to win underwriting business.[25] In addition, we control for broker size (BS_j), experience (Exp_j),[26] and a All-Star analysts dummy (AS_j).[27] We also control for AB_j, which is a binary indicator that takes a value of one if the analyst works for an investment bank that acted as a lead advisor at any time over the three years prior to the release of the recommendation.[28] We do not include $AB_j \times P\_GRS_t$ in our regression for the following reason: when the GRS expired, the U.S. District Judge Pauley approved the several changes, including that sanctioned banks are not required to fund IRFs after the GRS expired. However, the Judge declined to approve a proposed modification related to interaction between the research and investment banking arms of banks. According to the court, a firewall that forbids analysts and investment bankers from talking without a rules-compliance officer must continue to be present.[29]

[25] See "Testimony Pertaining to the Hearing: Examining the IPO Process: Is it Working for Ordinary Investors? Before The Senate Subcommittee on Securities, Insurance and Investment," Lise Buyer, June 20, 2012.

[26] For the selection of control variable, we refer to Sorescu and Subrahmanyam (2006), Clement (1999), Clarke, Khorana, Patel, and Rau (2011) and Kadan, Madureira, Wang, and Zach (2009). .

[27] Stickel (1992) find that all-star analysts produce more accurate earnings forecast compared to those produced by non-all-star analysts. .

[28] Michaely and Womack (1999) find that affiliated analysts, who work at banks which are underwriter for a firm, tend to be more optimistic than unaffiliated analysts. Kadan, Madureira, Wang, and Zach (2009) also find that before 2002, affiliated analysts are more reluctant to issue pessimistic recommendations than unaffiliated analysts.

[29] See United States District Court Southern District of New York Court Order 03 Civ. 2937, 2939, 2940, 2941, 2942, 2943, 2944, 2945, 2946, 2948, 6909, 6910 for Judge Pauley's full order.

2.4.3 How do analysts at IRFs behave after the GRS expiration?

We further investigate whether some IRFs have become biased following the expiration of the GRS. The expiration of the settlement meant no regulatory mandate to make independent research a requirement anymore, thus eliminates the need for many IRFs. Spokesmen at UBS and Goldman Sachs have clarified that their firms will not be renewing contracts to continue the IRF relationships. However Merrill Lynch planned to continue to provide independent research to their customers after July, 2009. Given the change in the industry circumstance, we are interested in the optimism of recommendations and the informativeness of recommendations issued by funded IRFs compared to those by non-funded IRFs after the GRS expiration. How do IRFs that were funded before the GRS expiration react to the new hope of contract renewal? After the GRS expiration, would funded IRFs craft their recommendation to be more similar to those issued by the sanctioned banks (i.e. more optimistic)?

To address these questions, we estimate the following logit regression:

$$\begin{aligned}\ln\left(\frac{P_{ijt}}{1-P_{ijt}}\right) &= \alpha + \beta_1 FI_j(OI_j) \times P\_GRS_t + \beta_2 FI_j + \beta_3 OI_j \\ &\quad + \beta_4 P\_GRS_t + \beta_5 Exp_j + \beta_6 AS_j + \beta_7 Log(BS_j) + \nu_i + \gamma_t + \varepsilon_{ijt}\end{aligned} \tag{2}$$

where the dependent variable is defined as before. FI_j is a binary indicator that takes a value of one if the recommendations issuer is a funded IRF and zero otherwise. OI_j is a binary indicator that takes a value of one if the recommendation issuer is an old IRF (i.e. independent research firm that came into existence before September, 2002) and zero otherwise. In Columns (3)-(6), we restrict our sample to recommendations issued by IRFs only and exclude recommendations issued by non-IRFs. Non-IRFs, mostly large banks, might have fundamentally different structure compared to IRFs, many of which are small boutiques started by former sell-side analysts. Columns (3) and (4) of Table 3 report the marginal effects. Here, we compared the recommendations issued by funded IRFs to those issued by non-funded IRFs. Because non-funded IRFs were, in general, not beneficiaries of the Settlement, the expiration of the Settlement will have little or no effect on these firms. The estimate of the interaction term, $FI_j \times P\_GRS_t$, is not statistically significant for upgrades. But for downgrades, the estimated β_1 is significant and negative. This suggests that after the GRS expiration, the probability of funded IRFs issuing a downgrade decreases by 6.4 percentage points compared to the non-funded IRFs. The result indicates that analysts at funded IRFs are less likely to issue pessimistic recommendations compared to analysts at non-funded IRFs after the GRS expired. Overall, we find evidence suggesting that funded IRFs cater their recommendations to the sanctioned bank's preferences in the hope of contract renewal.

We are also interested in the behavior of analysts at old IRFs and new IRFs after the GRS expiration. Clarke, Khorana, Patel, and Rau (2011) find that new IRFs issue more optimistic recommendations than old IRFs with a sample that ends in December, 2007. Would we find a similar trend using sample that begins in 2007 and covers the period after the GRS expiration? Columns (5) and (6) of Table 3 report the marginal effects. The estimated coefficient of $OI_j \times P\_GRS_t$, reveals that recommendations from old IRFs have become more optimistic following the GRS expiration compared to new IRFs. After the GRS expiration, the probability of old IRFs issuing an upgrade is 7.8 percentage points higher compared with the new IRFs. Anecdotal evidence supports our finding. During the recent financial crisis, some analysts who would like to issue objective hold/ underperform/ sell recommendations are forced to quit their sell-side job at large banks and started their own IRFs.[30]

2.5 Impact of GRS Expiration on Informativeness of Recommendations

2.5.1 Descriptive comparison between the market reaction to recommendations issued by funded IRFs, non-funded IRFs and sanctioned banks

In the previous section, we document that sanctioned banks and funded IRFs issue inflated recommendations after the GRS expiration. Without the GRS, the conflict of interest faced by analyst at sanctioned banks and funded IRFs become more pronounced. In this section,

[30]See "'Sell' For Research Renegades Becomes Business Off Wall Street," Bloomberg, October 9, 2009, for a discussion of the impact of sell-side analyst starting their own boutique.

we examine whether the market is misled by the inflated recommendations or does the market reaction indicate that the investors recognize conflicts faced by analysts after the GRS expiration?

Table 4 reports the average cumulative abnormal return during the announcement period for non-funded IRFs, funded IRFs and sanctioned banks. We calculate two-day cumulative abnormal return surrounding the recommendation release, which begins the day prior to the release till the end of the release day. The return is adjusted using Fama-French 3 factors model. Panels A and B of Table 4 present the market reaction to the strong buy/buy and hold/sell recommendations. During the *Reg* period, strong buy/buy recommendations of sanctioned banks significantly outperform those of the non-funded IRFs by 0.3%. This outperformance vanishes (0.00%) and become insignificant in the *Post* period. This result suggests that the gap of informativeness in recommendations issued by non-funded IRFs and sanctioned banks is growing smaller. Strong buy/buy recommendations by non-funded IRFs outperform funded IRFs significantly in both the *Reg* and the *Post* period by 0.5% and 1%. Panel B presents the hold/sell recommendation returns for each of the security firm categories. When examining hold/sell recommendation, non-funded IRFs again outperform funded IRFs by 1.5% and 0.9% in the *Reg and Post* period. For the *Reg* period, hold/sell recommendation by non-funded IRFs cause market reactions that are not significantly different from those by sanctioned banks. While in the *Post* period, downgrades by sanctioned banks significantly outperform both non-funded and funded IRFs by 1.4%, suggesting that the situation at the covered firm deteriorated enough when sanctioned banks choose to issue hold/sell recommendation.

Panels C and D of Table 4 report the market reaction to recommendation upgrades/downgrades. In both *Reg* and *Post* periods, for both upgrades and downgrades, we find that compared to those by funded IRFs, recommendations by non-funded IRFs almost always result in greater abnormal return.[31] We also observe that, in both *Reg* and *Post* periods, compared to recommendation upgrade and downgrade by funded IRFs and non-funded IRFs, those issued by sanctioned banks induce large daily abnormal return, possibly due to the strong research capability of sanctioned banks (Casey, 2013). Similar to results on strong buy/buy in panel A, in panel C we also observe that in the *Post* period,

[31] We therefore find confirming evidence that support the findings of Buslepp, Casey, Huston (2014), which shows research by funded IRFs is of lower quality than those by non-funded IRFs.

the gap of informativeness in recommendations issued by non-funded IRFs and sanctioned banks becomes smaller and non-funded IRFs are catching up. More specifically, during the *Reg* period, the market reaction to upgrades recommendations issued by sanctioned banks outperform those by non-funded IRFs by 1.5%. This difference decreased to 0.5% in the *Post-Reg* period, suggesting that non-funded IRFs are catching up. For downgrades, the gap of market reactions between sanctioned banks and IRFs became larger during *Post* period than during the *Reg* period, again suggesting that the situation at the covered firm worse enough when sanctioned banks, viewed to be prone to conflict of interest, choose to downgrade the firm.

2.5.2 Market reaction to recommendations issued by sanctioned banks after the GRS expiration

As in our previous section, we are concerned about confounding factors. Therefore, to access the informativeness of the recommendations issued by various institutions and whether the investors view recommendations issued by sanctioned banks as subject to conflict of interest before and after the GRS expiration, we estimate the following model:

$$\begin{aligned} R_{it(-1,0)} \;=\;& \alpha + \beta_1 SB_j \times P\_GRS_t + \beta_2 SB_j + \beta_3 P\_GRS_t + \beta_4 AB_j \\ & + \beta_5 Exp_j + \beta_6 AS_j + \beta_7 Log(BS_j) + \beta_8 R_{it(-22,-2)} + \beta_9 Inn_{ijt} + \nu_i + \gamma_t + \varepsilon_{ijt} \end{aligned} \tag{3}$$

where $R_{it(-1,0)}$, the dependent variable in Columns (1) and (2) of Table 5, is the two-day cumulative abnormal return surrounding the recommendation release. The return is adjusted using Fama-French 3 factors model. $R_{i,t(-22,-2)}$, prior month return, is the cumulative abnormal return from days -22 to -2. Inn_{ijt} is a binary indicator that takes a value of one if the recommendation is innovative.[32] All other variables are defined in section IV. In Column (1) of Table 5, we restrict our sample to upgrades, and in Column (2) of Table 5, to downgrades.

The coefficient of $SB_j \times P\_GRS_t$ shows that when sanctioned banks issue downgrades

[32] We define innovative recommendations as a recommendation that is accompanied by an one-year ahead EPS forecast revision on the same day or on the prior day of the release of the recommendation, and the one-year ahead EPS forecast revision is greater than (or less than) both the prevailing consensus of EPS forecast and the analyst's prior forecast.

after the GRS expiration, the market reaction is significantly negative and 0.9 percentage points larger in absolute terms than the market reaction of the control group (i.e. IBBs, MIBs, MRBs and SRBs that are not sanctioned by the GRS). However, when sanctioned banks issued upgrades after the GRS expiration, there is no significant market reaction compared to the control group. This result suggests that investors perceive that after the GRS expiration, analysts at sanctioned banks are pressured to publish favorable research report for firms that paid or will pay them to underwrite their stocks. Given this perceived tendency to issue inflated recommendations, favorable recommendations issued by sanctioned banks after the GRS expiration are view to convey limited amount of information, while pessimistic recommendations by sanctioned banks are viewed to convey important negative information that could not be hidden any longer.

2.5.3 Market reaction to recommendations issued by IRFs after the GRS expiration

In Section IV of our paper, we find that funded IRFs and old IRFs issue more inflated recommendations after the GRS expiration. Given this change in the optimism of recommendations, how will the informativeness of recommendations issued by various types of IRFs change? We estimate the following model:

$$\begin{aligned} R_{it(-1,0)} &= \alpha + \beta_1 FI_j(OI_j) \times P\_GRS_t + \beta_2 FI_j + \beta_3 OI_j + \beta_4 P\_GRS_t \\ &\quad + \beta_5 Exp_j + \beta_6 AS_j + \beta_7 Log(BS_j) + \beta_8 R_{it(-22,-2)} + \beta_9 Inn_{ijt} + \nu_i + \gamma_t + \varepsilon\circ_{ijt} \end{aligned} \tag{4}$$

where FI_j is a binary indicator that takes a value of one if the recommendations issuer is a funded IRF and zero otherwise. OI_j is a binary indicator that takes a value of one if the recommendation issuer is an old IRF (i.e. independent research firm that came into existence before September, 2002) and zero otherwise. Columns (3)-(6) of Table 5 present the results. The coefficients of $FI_j \times P\_GRS_t$ shows that after the GRS expiration, the informativeness of upgrades issued by funded IRFs drop significantly by 1.7 percentage points compared to non-funded IRFs, suggesting that lack of the monetary support required by the mandate harms funded IRFs' research capacity. However, downgrades issued by funded IRFs show surprising results: the coefficient of $FI_j \times P\_GRS_t$ shows that when funded IRFs issued downgrades after the GRS expiration, the market reaction is significantly negative and 3.1 percentage points larger in absolute terms than those issued by non-funded IRFs. This results should be viewed in the context of results from our previous section. In our previous section, we find that after the GRS expiration, analysts at funded IRFs cater their recommendations to the sanctioned bank's preferences in the hope of contract renewal. Given the reluctant of funded IRFs to downgrade firms, if, however, funded IRFs issue downgrades, it indicates the situation at the covered firm deteriorated enough that a downgrade is warranted.

The coefficients of the interaction term $OI_j \times P\_GRS_t$ in Columns (5) and (6) are statistically significant. When old IRFs issue upgrades after the GRS expiration, the market reaction is 2.1 percentage points less than the market reaction to the new IRF. For downgrades, the market reaction to recommendations by old IRFs is 2.7 percentage points less in absolute terms than to those by new IRFs. The results suggest that for both upgrades and downgrade, new IRFs are more informative than old IRFs after the GRS expiration. This result expands on results documented by Clarke, Khorana, Patel, and Rau (2011), who find that the recommendations of new IRFs generate lower announcement period returns with a sample ending in December, 2007. Consequently, their findings do not capture the competition after the GRS expiration which wipes out many IRFs and results in the survival of informative ones. It is important to note that the number of existing IRFs fluctuates in our sample. In 2001, we identify 10 firms as IRFs. In 2005, we have 41 firms as IRF. And in 2011, only 27 IRFs remain in our sample. Anecdotal evidence also show that supply of independent research far outstripped demand and only IRFs of best quality. We will discuss the factors of IRF survival in Section VI.

2.6 The GRS expiration affects survival of different types of IRFs

In this section, we investigate the survival of IRFs following the GRS expiration. Prior to the GRS enactment, there were 10 IRFs in 2001. A large number of IRFs were founded after the GRS enactment. For example, 41 IRFs existed in 2005. The increase in the number of IRFs indicates that some new IRFs might be founded to exploit the GRS for profits. Indeed, some of these new IRFs were contracted by the sanctioned banks.[33] The number of IRFs decreases slightly when it is close to the GRS expiration and decreases significantly after the GRS expiration. There are 37 IRFs in 2008 and the number dwindled to 27 three years later in 2011. We observe a greater change for the number of recommendations. In 2008, IRFs issued 4025 recommendations, this number decreases to 2117 three years later in 2011, while the number recommendations issued by non-IRFs stayed almost the same (21842 in 2008 vs. 18243 in 2011). When we look at funded IRFs, we observe an even greater change for the number of recommendations. The number of issued recommendations by funded IRFs decreased from 1003 in 2008 to 318 in 2011. This result clearly suggests that the IRF industry was hit hard by the GRS expiration: many IRFs did not survive more than two years after the GRS expiration. And those that survived issued fewer recommendations. Percentage of total research commission spent on IRFs shows the same trend. Various sources report that in 2001, independent analyst account for less than 10% of total research commission spent.[34] This number grow to 18% in 2007-2008 and drop again to only 11% in 2008-2009. Given the different trends funded IRFs, non-funded IRFs and non-IRFs experienced, we hypothesize that competition after 2009 wipes out many IRFs and results in the survival of high quality ones.

Table 6 shows the characteristics and behavior of analysts employed at the old IRFs and at the new IRFs. In Columns (1) and (2), we report descriptive statistics for the new and old IRFs during the period of September, 2002-July, 2009. Compared to old IRFs, new IRFs employ fewer analysts (4.3 analysts versus 31.7 analysts) and employ analysts who are slightly less experienced (4.0 years versus 4.4 years). Relative to their counterparts at old IRFs, analysts from the new IRFs are also less likely to issue strong buy recommendations (24.7% versus 45.7%) and less likely to issue high innovative recommendations (15.5% versus 18.3%), suggesting that the new IRFs act as "followers" in the markets in the September, 2002-July, 2009 period.

Columns (3) and (4) of Table 6 report descriptive statistics for the new and old IRFs

[33]For a list of funded IRFs, see Table II in the appendix. Source: Buslepp, W., Casey, R., and G. R. Huston, 2013, "Did they get what they paid for? The Global Analyst Research Settlement and analyst research quality," Working paper, Table Appendix B, page 26.

[34]See "Analyzing the Analysts: Hearings before the Subcommittee on Capital Markets, Insurance, and Government Sponsored Enterprises of the Committee of Financial Services of the U.S. House of Representatives." U.S. Congress. House. June 14, July 31, 2001, for the percentage of commission spent on investment banks and IRFs.

after August 2009. In this sample, we have 8 old IRFs and 26 new IRFs, a 20% decrease and a 35% decrease respectively from the September, 2002-July, 2009 period. After August 2009, we find that relative to their counterparts at old IRFs, analysts from the new IRFs exhibit some distinctive characteristics: analysts from the new IRFs are more experienced (6.3 years versus 5.9 years) and more likely to issue high innovative recommendations (22.8% versus 14.5%). Although the new IRFs still employ significantly fewer analysts than older IRFs (9.7 versus 46.1), the size of new IRFs doubled comparing to the period (9.7 versus 4.3). In the last two columns of Table 6, we further compare characteristics of new IRFs overtime. We find an interesting trend: new IRFs employ more experienced analysts (6.623 years vs. 4.021 years) and issue more innovative recommendations (22.8% vs. 15.5%) after August 2009 than in September, 2002-July, 2009. This suggests the GRS expiration affected the new IRFs the most: more than one third of the new IRFs cease to exist. However, those new IRFs that did survive after the GRS expiration are of high quality.

Table 7 shows the characteristics and behavior of analysts employed at funded IRFs and at non-funded.[35] In Columns (1) and (2), we report descriptive statistics for the funded IRFs and non-funded IRFs in the period September, 2002- July, 2009. Funded IRFs are significantly larger than non-funded IRFs (18.6 analysts versus 17.0 analysts). Relative to their counterparts at non-funded IRFs, analysts from the funded IRFs have more of experience (4.8 years versus 4.1 years), issue less strong buy recommendations (30.3% versus 34.8%) and more likely to issue high innovative recommendations (21.2% versus 15.4%), suggesting that funded IRFs act as "innovators" in the markets between September, 2002 and July, 2009.

Columns (3) and (4) of Table 7 report descriptive statistics for the funded IRFs and non-funded IRFs after August 2009. After August 2009, we find relative to their counterparts at non-funded IRFs, analysts from the funded IRFs still have more of experience (8.191 years versus 5.986 years), are more likely to issue high innovative recommendations (23.7% versus 16.9%). However, unlike in the September, 2002-July, 2009 period, funded IRFs are significantly smaller (13.8 analysts versus 32.9 analysts) and issued more strong buy recommendations (40.9% versus 38.1%)[36] after August 2009. On the whole, the result suggests the

[35] In the period September, 2002-July, 2009, we have 5 funded IRFs and 45 non-funded IRFs in our sample. In the period after August, 2009, we have 5 funded IRFs and 32 non-funded IRFs (a 29% decrease from the previous period) in our sample.

[36] We therefore provide confirming evidence that support the our findings in Columns (3) and (4) in Table

GRS expiration affect the market structure of the IRFs, funded IRFs lost significant amount of business and are force to downsize.

We estimate a hazard model for survival rate for an IRF. We use the hazard model for forecasting IRF failure for two reasons: (1) it controls for each IRF's period at risk, and (2) it incorporates explanatory variables that change over time (Shumway, 2001). The Cox proportional hazard model has the following form:

$$\lambda\left(t \mid X\right) = \lambda_0\left(t\right) e^{\left(\beta_1 X_1 + \ldots + \beta_p X_p\right)} \tag{5}$$

where $\lambda_0\left(t\right)$ is the underlying hazard function and $\lambda\left(t \mid X\right)$ is the hazard at time t for an IRF with time-variant explanatory variables X. Table 8 provides results from Cox proportional hazard model. In panel A, the sample is from 2008-2013. In Columns (1) and (2), all IRFs are included in the sample and the model is stratified by the *new_IRF*[37] variable. In Columns (3) and (4), only new IRFs are included in the sample. Columns (1) and (3) give the estimates of hazard ratios corresponding to each variable. Z statistictics are presented in Columns (2) and (4). We find for IRFs in general, a larger broker size and more innovative recommendations are important for survival. For every additional analyst employed by the IRF, the risk of IRF failure falls by 44.1%. For new IRFs, the broker size also matters for survival. Interestingly, in the sample of new IRFs, the survival rate is higher if the average analyst is more experienced. For every additional year of average analyst experience, the risk of new IRF failure falls by 28.4%. Anecdotal evidence, which shows market values analyst experience, corroborate with our findings.[38] The hazard model results show that as independent research is no longer mandatory, only the IRFs of best quality are able to survive after the GRS expiration.[39]In Panel B, we present the hazard results for the time period from 2000-2013. We find for IRFs in general, the ability to issue more recommendations is important for survival of IRFs. While the new IRFs survival longer if they issue more recommendations and hire more experienced analysts. The factors affecting the survival of IRFs are changing over time.

3.

[37] *new_IRF* equals one for those independent research firms that came into existence after the Global Settlement (September, 2002).

[38] See "Staying in the Game," The Wall Street Journal, May, 16, 2011, for a discussion on analyst experience.

[39] In addition, the expiration come at a time right after the financial crisis and the future market looked bleak, which could have contributed to the decline.

2.7 Robustness Checks

In previous sections, we examine the analyst recommendations issued before and after the expiration to evaluate the impact of the GRS expiration. However, as is always the case with studies not using randomized experiment, it is possible that confounding factors could explain the findings. We conduct a number of sensitivity analysis to confirm that our results are not driven by our estimation specification.

First, stock market conditions at the time of the issuance might bias our estimates. Womack (1996) and Jegadeesh, Kim, Krische, and Lee (2004) find that momentum affect analyst optimism. So we include the prior month return in equations (1) and (2) as control. The prior month return is the cumulative firm's market adjusted return from days -22 to -2. The results are similar to our main findings. Also, market reaction to recommendations could be affected by market wide economic conditions. We follow Clarke, Khorana, Patel, and Rau (2011) and Kadan, Madureira, Wang, and Zach (2009) and control for market volatility over the prior month. The reestimation of equations (3) and (4) yield similar results.

Second, our results could be driven by higher moments of our control variable. Mikhail, Walther, and Willis (1997) find that larger market reaction to strong buy recommendation is associated with analyst with more experience. Thus, we control for not only analyst experience, but also its higher moments. Including a quadratic term of experience in equations (1)-(4) produces similar quantitative and qualitative results.

Third, recommendations that are accompanied by earnings forecast might confound our findings. Michaely and Womack (2006) find that recommendations that are issued concurrently with an earnings forecast produce larger price drifts. We thus control for supported recommendation dummy in equations (3) and (4). The results remain consistent.

Fourth, our results may be unique to how we construct our market reaction proxy. In equations (3) and (4), our dependent variable is $R_{(-1,0)}$, the two-day CAR adjusted by Fama-French factors surrounding the recommendation release. When examining market reaction following recommendations, Kadan, Madureira, Wang, and Zach (2009) use the three-day period (-1, 1) surrounding the date of the recommendation issuance. We follow their specification and use (-1,1) as the announcement period in equations (3) and (4) and find that our conclusion is robust to different announcement period length. Clarke, Khorana, Patel, and Rau (2011) calculate the market reaction to recommendations as the market adjusted returns over (-1,0) window. They use the CRSP value-weighted index to proxy for market return. We use their measure of market reaction as the dependent variable in equations (3) and (4) and find similar results.

Finally, our results could be driven by different institutions that exist before and after the GRS expiration. We therefore limit our sample to include only recommendations issued by institution which are active both before and after the GRS expiration. The results are similar.

Overall, our multivariate results in equations (1)-(4) appear to be quite robust with respect to changes in independent variables, dependent variables, and sample selection.

2.8 Policy Implications

Our results show that after the expiration of the GRS, sanctioned banks appear to be more biased and conflicts of interests threatened their credibility of research more seriously. Thus, our study calls into question the SEC's decision not to codify the GRS into permanent rules. However, we are not arguing that IRFs have strong research capacities and issue recommendations associated with large market reaction. Indeed, it is likely that sanctioned banks have better access to corporate insiders and thus have more knowledge about the firms, and provide better quality research. Indeed, at least two academic studies have examined the quality of research provided by IRFs compared to the research by investment banks and reach different conclusions.[40] Instead, we are arguing that the double reporting system that was in place before 2009 may have cause sanctioned banks to behave differently before and after the GRS expiration. During 2003-2009, sanctioned banks were mandated to provide investors with a research report by IRFs along with the research report produced by the analyst at the sanctioned banks. It is possible that sanctioned banks issue more objective recommendations before the GRS expiration due to the fear that investors could compare reports produced by analysts at the sanctioned banks and the reports produced by analysts at IRFs and detect

[40] There are two view on the quality of the research provided by IRFs. According to the first, IRFs provide better quality research than investment banks. Barber, Lehavy, and Trueman (2007) find that recommendation upgrades by investment banks underperform those by IRFs, while recommendation downgrades by investment banks outperform those by IRFs. An alternative view is that IRFs provide low quality research. Buslepp, Casey, and Huston (2013) find that funded IRF provide research that is of lower quality than research by the sanctioned banks, mainly because IRFs analysts have less experience.

possible discrepancies. After the GRS expiration, investors are disadvantageous as their access to free independent research has become more restricted.

According to the GRS, to ensure that investors get access to objective investment research, each of the sanctioned banks is obligated to provide independent research to its clients. More specifically, the sanctioned banks must notify customers on the bank's website, on customers' account statements, and on the first page of research reports, that the independent research is available to the customer at no cost. Both Morgan Stanley and Citigroup failed to disclose the availability of independent third-party research in customer account statements per the requirements of the GRS for some period of time.[41] Financial Industry Regulatory Authority (FINRA) takes enforcement actions against both of these sanctioned banks for failing to adequately comply with disclosure requirements.

2.9 Conclusion

Our study directly evaluates the change brought by the GRS expiration. We document that following the GRS expiration, analysts employed at sanctioned banks and IRFs that received funds from the sanctioned banks issued positively skewed recommendations. In addition, the downgrades issued by sanctioned banks are viewed to convey more information after the GRS expiration, suggesting that situations are worse enough so that sanctioned banks choose to downgrade the firm. Overall, our findings show that conflicts of interests become a more serious problem and threatened the credibility of the research by sanctioned banks. Our paper calls into question the SEC's decision not to codify the GRS into permanent rules. Another implication of the expiration of the GRS is that it eliminates the need for many IRFs. By altering industry landscape and consolidating the industry, a settlement that is aimed to restrict one type of financial institutions (sanctioned banks) might have an enormous impact on the structure and survival of other financial institutions. Our paper suggests that exploring the unintended consequences of laws on all market participants is a promising avenue for future research.

[41] The funding to IRFs does not seem to contribute to Morgan Stanley's violation of disclosure. It appears that Morgan Stanley did pay IRFs for the research, but it did not disclose the availability of the research to its investors as the press release specifies that "...Morgan Stanley also did not disclose in approximately 127,600 monthly account statements sent to customers from August 2007 to February 2008 that it had available independent, third-party research.

Table 1: Descriptive Statistics

This table presents descriptive statistics for the sample of analysts recommendation. The sample period runs from January 2000 through April 2013. The variables are defined as: **Experience**: is the number of years since an analysts first earnings forecast on the I/B/E/S database. **All-star**: takes the value of one if the analyst was named an Institutional Investor All-Star in the year of the recommendation, and zero otherwise. **Broker Size**: is the number of analysts employed by the brokerage in the year of the recommendation release. **EPS Support**: takes the value of one if the analyst issued an one- or two-year ahead EPS forecast on the day or prior to the day of the release of a recommendation, and zero otherwise. **High innovation**: takes the value of one if there is an one-year ahead EPS forecast revision greater than (or less than) both the prevailing consensus of EPS forecast and the analyst's prior forecast on the day prior to or the day of the release of a recommendation, and zero otherwise. **Strong buy**: takes the value of one if the recommendation was a strong buy, and zero otherwise. Results are presented for Investment Bank/ Broker (**IBB**), Independent Research Firms (**IRF**), Major Institutional Broker (**MIB**), Major Regional Broker (**MRB**), Management Subsidiary of Broker (**MSB**) and Small Regional Broker (**SRB**). $***$, $**$, and $*$ indicate statistical significance at 1%, 5% and 10%.

			Panel A			
Variable	**IBB**	**IRF**	**MIB**	**MRB**	**MSB**	**SRB**
Experience	5.376	4.626	4.931	4.681	5.027	7.080
Allstar	0.100	0.000	0.072	0.000	0.000	0.000
Brokersize	71.280	22.360	26.330	21.950	18.000	7.997
EPS support	0.575	0.615	0.591	0.573	0.473	0.571
High innovation	0.173	0.167	0.175	0.167	0.108	0.190
Strong buy	0.188	0.372	0.216	0.231	0.054	0.222
Number of observations	253066	28650	22855	5478	74	2578

			Panel B	
Variable	**IRF**	**Non-IRF**	**Difference**	**T-stat**
Experience	4.626	5.342	0.716***	(28.49)
Allstar	0.000	0.095	0.095***	(54.67)
Brokersize	22.360	66.120	43.760***	(140.58)
EPS support	0.615	0.576	-0.039***	(-12.73)
High innovation	0.167	0.173	0.006***	(2.64)
Strong buy	0.372	0.191	-0.181***	(-72.53)
Number of observations	28650	284051		

Table 2: Optimism of Recommendations around the Settlement

The sample runs from January 2000 to April 2013. The sample includes recommendations issued by non-IRF analysts. IRF analysts are analysts at independent research firms as defined in Nelson's directory. All other analysts are defined as non-IRF. The variables are defined as: **Upgrade (Downgrade)**: is a positive (negative) change in the rating of a stock by the analyst. **Sanctioned Banks**: the recommendation issuer is one of the ten banks sanctioned in the Global Research Analyst Settlement. All other recommendation issuer in the sample are defined as **Non-sanctioned Banks**. **Pre**(-regulation) is the period between January, 2000 and August, 2002. **Reg**(regulation) is the period between September, 2002 and July, 2009. **Post**(-regulation) is the period between August, 2009 and April, 2013. T-statisticss in brackets below coefficient estimates are based on robust errors. $***$, $**$, and $*$ indicate statistical significance at 1%, 5% and 10%.

Table 2: (Cont.)Optimism of Recommendations around the Settlement

					Panel A: UPGRADE			
Type of institutions	**All**	**Pre**	**Reg**	**Post**	**Pre versus Reg**	**T-stat**	**Post versus Reg**	**T-stat**
Sanctioned Banks	0.181	0.125	0.189	0.214	-0.0642***	(-20.51)	0.0250***	(7.57)
Non-sanctioned Banks	0.239	0.158	0.256	0.269	-0.0979***	(-39.23)	0.0129***	(5.21)
Observations	283977	57811	162269	63897				
Difference tests								
Sanctioned versus non-sanctioned:	0.0576***	0.0332***	0.0669***	0.0548***				
T-stat	(35.29)	(10.65)	(30.78)	(14.60)				
					Panel B: DOWNGRADE			
Sanctioned Banks	0.243	0.215	0.255	0.234	-0.0402***	(-11.27)	-0.0204***	(-5.64)
Non-sanctioned Banks	0.293	0.230	0.311	0.304	-0.0811***	(-30.08)	-0.0072***	(-2.76)
Observations	283977	57811	162269	63897				
Difference tests								
Sanctioned versus non-sanctioned:	0.0498***	0.0154***	0.0563***	0.0695***				
T-stat	(28.21)	(4.18)	(24.06)	(17.86)				

Table 3: Logit Regression of Analyst Upgrades and Downgrades surrounding the Expiration of the Global Research Settlement

This table reports logit regression results of analyst recommendation behavior. The sample period runs from January 2007 through April 2013. In columns 1 and 2, we restrict our sample to include only recommendations issued by non-IRF, and in columns 3 to 6, we include only recommendations issued by IRFs. **IRF** : are independent research firms are identified from Nelson's directory. The dependent variable in columns 1, 3 and 5 (2, 4, and 6) is a dummy variable that takes the value of one if the recommendation is an upgrade (downgrade) and zero otherwise. **Upgrade (Downgrade)**: is a positive (negative) change in the rating of a stock by the analyst. Independent variables include the following: **Sanctioned Banks**: is an indicator variable equal to 1 if recommendation issuer is one of the ten banks sanctioned in the Global Research Analyst Settlement, and zero otherwise. **Funded IRF** is an indicator variable equal to 1 if the recommendation issuer is a firm that received penalty payment in exchange for independent research, and is identified from Buslepp, Casy, Huston (2013), and zero otherwise. **Old IRF** is an indicator variable equal to 1 if the recommendation issuer is an independent research firms that came into existence before the Global Settlement (September, 2002), and zero otherwise. **Affiliated**: is an indicator variable equal to 1 if the recommendation issuer is an investment banks that acted as a lead advisor at any time over the three years prior to the release of the recommendation, and zero otherwise. **Post**(-regulation) is an indicator variable equal to 1 if the recommendations is issued in the period between August, 2009 and April, 2013 and zero otherwise. **Experience**: is the number of years since an analysts first earnings forecast on the IBES database. **All-star**: is an indicator variable equal to 1 if the analyst was named an Institutional Investor All-Star in the year of the recommendation, and zero otherwise. **Log(brokersize)**: is the natural logarithm of number of analysts employed by the brokerage in the year of the recommendation release. Firm and year fixed effects are included in each regression. T(Z)-statisticss in brackets below coefficient estimates are based on robust errors. $***$, $**$, and $*$ indicate statistical significance at 1%, 5% and 10%.

Table 3: (Cont.)Logit Regression of Analyst Upgrades and Downgrades surrounding the Expiration of the Global Research Settlement

	Sample period: after 2006.					
	Non-IRF sample		IRF sample			
	Upgrade (1)	Downgrade (2)	Upgrade (3)	Downgrade (4)	Upgrade (5)	Downgrade (6)
Sanctioned banks*Post	0.033***	0.006				
	(5.080)	(0.950)				
Funded IRF*Post			-0.033	-0.064***		
			(-1.517)	(-3.229)		
Old IRF*Post					0.078***	0.027
					(4.144)	(1.527)
Sanctioned banks	-0.126***	-0.078***				
	(-23.846)	(-15.693)				
Affiliated banks	0.007	0.070***				
	(0.808)	(9.335)				
Funded IRF			0.007	0.054***	-0.001	0.031**
			(0.410)	(3.486)	(-0.100)	(2.329)
Old IRF			0.096***	0.024*	0.045**	0.003
			(6.702)	(1.737)	(2.469)	(0.188)
Post period	0.072***	-0.113***	0.117***	-0.167***	0.063**	-0.197***
	(8.327)	(-12.177)	(5.032)	(-7.284)	(2.459)	(-8.035)
Experience	0.011***	0.012***	0.007***	0.011***	0.007***	0.011***
	(37.073)	(45.652)	(6.238)	(10.246)	(6.367)	(10.273)
All-star	0.043***	0.019**				
	(4.841)	(2.091)				
Log(brokersize)	0.009***	-0.004**	-0.039***	-0.034***	-0.032***	-0.029***
	(4.545)	(-2.090)	(-7.856)	(-7.075)	(-6.668)	(-6.123)
Firm FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Observations	114,427	116,599	14,586	15,087	14,586	15,087

Table 4: Cumulative Abnormal Return to Strong Buy/Buy Recommendations, Hold/Sell Recommendations, Upgrades, and Downgrades Issued by Different Types of Institutions

This table reports the average cumulative abnormal return(**CAR**) and corresponding t-statistics during the announcement period (-1,0) for Non-funded IRF, Funded IRF and Sanctioned Banks. The return is adjusted using Fama-French 3 factors model. Panel A presents CAR for **strong buy/buy** recommendations (upgrades to buy or strong buy, or initiations/resumptions/reiterations with a buy or strong buy rating). Panel B presents CAR for **hold/sell** recommendations (downgrades to hold or sell, or initiations/resumptions/reiterations with a hold or sell rating. Panel C presents CAR for upgrades, and Panel D presents CAR for downgrades. **Upgrade (Downgrade)**: is a positive (negative) change in the rating of a stock by the analyst. Panel E presents CAR for Above Consensus recommendations, and Panel F presents CAR for Below Consensus recommendations. **Non-funded IRF** is a independent research firms that did not receive penalty payment in exchange for independent research. **Funded IRF** is a firm that received penalty payment in exchange for independent research, and is identified from Buslepp, Casy, Huston (2013). **Sanctioned Banks**: are the ten banks sanctioned in the Global Research Analyst Settlement. $***$, $**$, and $*$ indicate statistical significance at 1%, 5% and 10%.

Table 4: (Cont.)Cumulative Abnormal Return to Strong Buy/Buy Recommendations, Hold/Sell Recommendations, Upgrades, and Downgrades Issued by Different Types of Institutions

					Panel A: Strong buy/Buy			
Type of institutions	**All**	**Pre**	**Reg**	**Post**	**Pre versus Reg**	**T-stat**	**Post versus Reg**	**T-stat**
Non-funded IRF	0.012	-0.000	0.011	0.016	-0.0117***	(-4.88)	0.0045***	(3.09)
Funded IRF	0.006	0.005	0.006	0.006	-0.0010	(-0.31)	-0.0005	(-0.20)
Sanctioned Banks	0.010	0.000	0.014	0.016	-0.0140***	(-12.02)	0.0019*	(1.69)
Observations	44597	11504	23729	9364				
					Panel B: Sell/Underperform			
Non-funded IRF	-0.022	0.015	-0.025	-0.010	0.0394	(1.45)	0.0145**	(2.52)
Funded IRF	-0.010	-0.032	-0.010	0.001	-0.0230	(-1.41)	0.0101	(0.64)
Sanctioned Banks	-0.026	-0.060	-0.025	-0.024	-0.0350***	(-8.27)	0.0009	(0.39)
Observations	13217	574	10334	2309				

Table 4: (Cont.) Cumulative Abnormal Return to Strong Buy/Buy Recommendations, Hold/Sell Recommendations, Upgrades, and Downgrades Issued by Different Types of Institutions

					Panel C: Upgrade			
Type of institutions	**All**	**Pre**	**Reg**	**Post**	**Pre versus Reg**	**T-stat**	**Post versus Reg**	**T-stat**
Non-funded IRF	0.018	0.013	0.017	0.022	-0.0041	-0.67	0.0049**	2.08
Funded IRF	0.018	0.028	0.018	0.014	0.0095*	1.73	-0.0039	-1.01
Sanctioned Banks	0.031	0.034	0.032	0.027	0.0023	0.96	-0.0046**	-2.48
Observations	23931	2548	15973	5410				
					Panel D: Downgrade			
Non-funded IRF	-0.022	-0.027	-0.024	-0.015	-0.0031	-0.47	0.0098***	3.07
Funded IRF	-0.020	-0.039	-0.019	-0.012	-0.0201***	-3.16	0.0071	1.34
Sanctioned Banks	-0.037	-0.076	-0.030	-0.025	-0.0462***	-22.43	0.0044**	2.40
Observations	30888	4339	20616	5933				

Table 5: OLS Regression of Market Reaction to Analyst Recommendations Surrounding the Expiration of the Global Research Settlement

This table reports OLS regression results of market reaction to analyst recommendation. The sample period runs from January 2007 through April 2013. In columns 1 and 2, we restrict our sample to include only recommendations issued by non-IRF, and in columns 3 to 6, we include only recommendations issued by IRFs. **IRF** : are independent research firms are identified from Nelson's directory. In addition, in columns 1, 3 and 5 (2, 4, and 6), we restrict our sample to include only upgrades (downgrades). **Upgrade (Downgrade)**: is a positive (negative) change in the rating of a stock by the analyst. The dependent variable is the cumulative abnormal return(**CAR**) during the announcement period (-1,0). The return is adjusted using Fama-French 3 factors model. Independent variables include the following: **Sanctioned Banks**: is an indicator variable equal to 1 if recommendation issuer is one of the ten banks sanctioned in the Global Research Analyst Settlement, and zero otherwise. **Funded IRF**: is an indicator variable equal to 1 if the recommendation issuer is a firm that received penalty payment in exchange for independent research, and is identified from Buslepp, Casy, Huston (2013), and zero otherwise. **Old IRF**: is an indicator variable equal to 1 if the recommendation issuer is an independent research firms that came into existence before the Global Settlement (September, 2002), and zero otherwise. **Affiliated**: is an indicator variable equal to 1 if the recommendation issuer is an investment banks that acted as a lead advisor at any time over the three years prior to the release of the recommendation, and zero otherwise. **Post**(-regulation): is an indicator variable equal to 1 if the recommendations is issued in the period between August, 2009 and April, 2013 and zero otherwise. **Experience**: is the number of years since an analysts first earnings forecast on the IBES database. **All-star**: is an indicator variable equal to 1 if the analyst was named an Institutional Investor All-Star in the year of the recommendation, and zero otherwise. **Log(brokersize)**: is the natural logarithm of number of analysts employed by the brokerage in the year of the recommendation release. Firm and year fixed effects are included in each regression. **Prior month return**: is the cumulative abnormal return from days -22 to -2. The return is adjusted using Fama-French 3 factors model. T(Z)-statisticss in brackets below coefficient estimates are based on robust errors. $***$, $**$, and $*$ indicate statistical significance at 1%, 5% and 10%.

Table 5: (Cont.)OLS Regression of Market Reaction to Analyst Recommendations Surrounding the Expiration of the Global Research Settlement

	Sample period: after 2006. Dependent variable: CAR (-1,0)					
	Non-IRF sample		**IRF sample**			
	Upgrade (1)	Downgrade (2)	Upgrade (3)	Downgrade (4)	Upgrade (5)	Downgrade (6)
Sanctioned banks*Post	0.003	-0.009**				
	(0.998)	(-2.500)				
Funded IRF*Post			-0.017*	-0.031***		
			(-1.926)	(-2.588)		
Old IRF*Post					-0.021**	0.027**
					(-2.096)	(2.236)
Sanctioned banks	-0.000	0.012***				
	(-0.019)	(4.104)				
Affiliated	0.002	0.008				
	(0.560)	(1.618)				
Funded IRF			-0.006	0.015	-0.016**	0.004
			(-0.730)	(1.047)	(-2.182)	(0.329)
Old IRF			0.017*	0.006	0.029**	-0.013
			(1.691)	(0.581)	(2.161)	(-0.936)
Post-reg period	-0.013*	0.024***	0.021**	0.016	0.029***	-0.006
	(-1.891)	(5.716)	(2.042)	(1.138)	(2.603)	(-0.419)
Experience	0.000***	-0.000***	0.001**	-0.000	0.001*	-0.000
	(2.704)	(-2.588)	(2.049)	(-0.580)	(1.924)	(-0.553)
All-star	-0.004	-0.001				
	(-1.162)	(-0.261)				
Log(brokersize)	0.003***	-0.005***	-0.002	0.003	-0.002	0.007**
	(3.224)	(-5.216)	(-0.628)	(0.985)	(-0.801)	(2.014)
Prior month return	0.010	0.019*	-0.029	-0.006	-0.029	-0.006
	(0.703)	(1.790)	(-1.432)	(-0.406)	(-1.452)	(-0.385)
Innovative	0.003*	-0.030***	0.002	-0.025***	0.002	-0.024***
	(1.845)	(-15.763)	(0.520)	(-4.427)	(0.495)	(-4.291)
Firm FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Observations	27,419	31,176	5,008	5,645	5,008	5,645
R-squared	0.261	0.422	0.445	0.593	0.445	0.593

Table 6: Characteristics of Independent Research Firms Established Before versus After the Settlement

This table presents an analysis of analysts at independent research firms during the period January 2000- April 2013. Independent research firms are identified from Nelson's directory. **New IRFs** are those independent research firms that came into existence after the Global Settlement (September, 2002), while **Old IRFs** are independent research firms that were in existence prior to the settlement. We provide descriptive statistics on analysts employed by the New and Old IRFs for the sample period September, 2002- July, 2009 and sample period after August 2009 respectively. We also compares results across periods. The variables are defined as: **Experience** is the number of years since an analysts first earnings forecast on the I/B/E/S database. **Broker Size** is the number of analysts employed by the brokerage in the year of the recommendation release. **EPS Support** takes the value of one if the analyst issued an one- or two-year ahead EPS forecast on the day or prior to the day of the release of a recommendation, and zero otherwise. **High innovation** takes the value of one if there was an one-year ahead EPS forecast revision greater than (or less than) both the prevailing consensus of EPS forecast and the analyst's prior forecast on the day or prior to the day of the release of a recommendation, and zero otherwise. **Strong buy** takes the value of one if the recommendation was a strong buy, and zero otherwise. $***$, $**$, and $*$ indicate statistical significance at 1%, 5% and 10%.

Table 6: (Cont.)Characteristics of Independent Research Firms Established Before versus After the Settlement

	Sample period: 09/2002- 07/2009			Sample period: 08/2009			Across period comparison	
	Old IRF (1)	New IRF (2)	Difference (1) vs. (2)	Old IRF (3)	New IRF (4)	Difference (3) vs. (4)	(1) vs. (3)	(2) vs. (4)
Experience	4.373	4.021	-0.352***	5.873	6.623	0.750***	1.501***	2.602***
Brokersize	31.730	4.289	-26.940***	46.090	9.654	-36.436***	14.860***	5.365***
EPS support	0.653	0.515	-0.138***	0.721	0.715	-0.006	0.068***	0.200***
High innovation	0.183	0.155	-0.028***	0.145	0.228	0.083***	-0.038***	0.073***
Strong buy	0.457	0.247	-0.211***	0.552	0.142	-0.410***	0.093***	-0.105***
Number of observations	9770	9049		4542	3110			

Table 7: Characteristics of Independent Research Firms Receiving Funds In Exchange for Research versus Other Independent Research Firms

This table presents an analysis of analysts at independent research firms during the period January 2000- April 2013. Independent research firms are identified from Nelson's directory. **Funded IRFs** are firms that received penalty payment in exchange for independent research are identified from Buslepp, Casy, Huston (2013). **Non-funded IRF** are independent research firms that do not receive penalty payment in exchange for independent research. We provide descriptive statistics on analysts employed by the Funded and Non-funded IRFs for the sample period September, 2002- July, 2009 and sample period after August 2009 respectively. We also compares results across periods. The variables are defined as: **Experience** is the number of years since an analysts first earnings forecast on the I/B/E/S database. **Broker Size** is the number of analysts employed by the brokerage in the year of the recommendation release. **EPS Support** takes the value of one if the analyst issued an one- or two-year ahead EPS forecast on the day or prior to the day of the release of a recommendation, and zero otherwise. **High innovation** takes the value of one if there was an one-year ahead EPS forecast revision greater than (or less than) both the prevailing consensus of EPS forecast and the analyst's prior forecast on the day or prior to the day of the release of a recommendation, and zero otherwise. **Strong buy** takes the value of one if the recommendation was a strong buy, and zero otherwise. $***$, $**$, and $*$ indicate statistical significance at 1%, 5% and 10%.

Table 7: (Cont.)Characteristics of Independent Research Firms Receiving Funds In Exchange for Research versus Other Independent Research Firms

	Sample period: 09/2002- 07/2009			**Sample period: 08/2009**			**Across period comparison**	
	Funded IRF	Non-funded IRF	Difference	Funded IRF	Non-funded IRF	Difference		
	(1)	(2)	(1) vs. (2)	(3)	(4)	(3) vs. (4)	(1) vs. (3)	(2) vs. (4)
Experience	4.812	4.138	-0.674***	8.191	5.986	-2.204***	3.379***	1.848***
Brokersize	18.630	17.080	-1.554***	13.790	32.890	19.100***	-4.842***	15.820***
EPS support	0.581	0.557	-0.024***	0.658	0.713	0.055***	0.077***	0.156***
High innovation	0.212	0.154	-0.058***	0.237	0.169	-0.068***	0.024**	0.014***
Strong buy	0.303	0.348	0.045***	0.409	0.381	-0.027*	0.106***	0.034***
Number of observations	6687	15415		1390	7037			

Table 8: Survival Analysis of Independent Research Firms

This table provides results from Cox proportional hazards model for survival analyst of independent research firms **IRFs** during the period 2008- 2013 in panel A and during the period 2000-2013 in panel B. **IRFs** are identified from Nelson's directory. In columns 1 and 2, our sample include all IRFs and the model is stratified by the new IRF variable, in columns 3 and 4, our sample include only NEW IRFs. **New IRFs** are those independent research firms that came into existence after the Global Settlement (September, 2002). The explanatory variables are defined as: **Broker Size** is the number of analysts employed by the brokerage. **Experience** average years of experience for all analyst in the firm in a given year. **High innovation** percentage of high innovation recommendations issued in a given year. **Strong buy** percentage of strong buy recommendations issued in a given year. **Number of recommendations** is the number of recommendations issued by an IRF in a given year.

	Panel A: period 2008- 2013			
	IRF sample (Cox Stratified Model)		**New IRF sample (Cox Model)**	
Variable	**Haz. Ratio** (1)	**Z stat** (2)	**Haz. Ratio** (3)	**Z stat** (4)
Brokersize	0.559***	(-2.865)	0.081**	(-2.185)
Experience	0.991	(-0.109)	0.716**	(-2.072)
High innovation	0.006**	(-2.004)	0.006	(-1.471)
Strong buy	0.680	(-0.251)	273.405	(0.553)
Number of recommendations	0.923	(-0.630)	0.977	(-0.919)
Observations	109		75	
	Panel B: period 2000- 2013			
	IRF sample (Cox Stratified Model)		**New IRF sample (Cox Model)**	
Variable	**Haz. Ratio** (1)	**Z stat** (2)	**Haz. Ratio** (3)	**Z stat** (4)
Brokersize	0.901	(-1.155)	0.946	(-0.376)
Experience	0.939	(-0.798)	0.822*	(-1.857)
High innovation	0.338	(-0.696)	0.292	(-0.665)
Strong Buy	1.490	(0.235)	2.986	(0.492)
Number of recommendations	0.825**	(-2.406)	0.829*	(-1.676)
Observations	269		170	

Chapter 3
Analysts Coverage of the Financial Sector: Institutional Pressure and Forecast Accuracy

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3.1 Introduction

Recommendations and forecasts play an important role in shaping investors' beliefs. Investors can infer the fundamental value of a firm's assets from the recommendations and forecasts issued by analysts. Thus, analysts help to reduce the information asymmetry related to firms. The recent financial crisis has raised concerns about the objectivity of analyst recommendations and forecasts. Biased estimates of the health of a firm in the financial sector could potentially misguide investors and affect the efficiency and stability of financial markets. There has been extensive research focusing on the behavior of credit analysts and on the role of credit agencies covering the financial sector (Brunnermeier, 2009).[1] However, little is known about the behavior of equity analysts covering the financial sector.[2] When analysts are asked to cover other firms in the financial sector, can they still be objective? It is possible that the ability of investment bank analysts to provide investors with objective information about firms in the financial sector could be affected by conflicts of interests. The conflicts of interests stem from the fact the underwriting business provides a substantial portion of an investment bank's revenue and investment banks rely on each other to form syndications.

Numerous pieces of anecdotal evidence reported in the Wall Street Journal suggest that analysts covering firms in the financial sector are pressured to bias their research.[3] In addition, in at least five Wall Street Journal articles, it has been shown that investment banks employing pessimistic analysts have been retaliated against when the analysts issue negative reports for other firms in the financial sector. The ways of retaliation include cutting back on trading activity, loan activity, and investment banking business with the investment bank and excluding the investment bank that employs the analyst from asset-backed deals. Pessimistic analysts covering the financial sector also face personal retaliations, which include

[1] Brunnermeier (2009) argues that the models of many credit-rating agencies provided estimates that are too optimistic about structured finance products.

[2] Hereafter we will use "analysts" to refer to equity analysts unless otherwise noted.

[3] For a detailed discussion on the allegations, see the article titles "Incredible 'Buys': Many Companies Press Analysts to Steer Clear Of Negative Ratings," The Wall Street Journal, July 19, 1995, "Wall Street On The Run," Fortune, July 14, 2004, and "Bearish Call on Banks Lands Analyst in Doghouse," The Wall Street Journal, November 23, 1999, "Battle of the bank analysts," Institutional Investor, November 1, 2002, "The woman who called Wall Street's meltdown," Fortune, August 6, 2008, "It's 2009. Do You Know Where Your Bank Analyst Is?" The Wall Street Journal, March 26, 2009, and "Why Wall Street Can't Handle the Truth," The Wall Street Journal, November 8, 2011.

being barred from entering brokerage headquarters, being excluded from mailings, meetings and conference calls with management of the covered firm, being discouraged by the analyst's own supervisor, and, finally, being fired.[4] Some of the pessimistic analysts covering the financial sector have left big banks and joined Independent Research Firms (IRFs).

We examine two empirical questions in this paper. First, we test whether brokerage type affects the performance of recommendations and forecasts issued by the brokerages. We use both Nelson's Directory and the SDC Platinum Database by Thomson Reuters to classify brokerages as IRFs or investment banks. In our study, investment banks are defined as brokerages that engage both in research and in a material amount of equity underwriting business. Specifically, we examine whether, compared to investment bank analysts, IRF analysts offer less biased, more informative, and more accurate recommendations and forecasts when covering firms in the financial sector. Second, we investigate the reason for the worse performance of investment bank analysts when covering firms in the financial sector. We examine the performance of investment bank analysts across different types of firms being covered. We also examine whether the performance of investment bank analysts is especially worse when they cover bulge brackets.

Our empirical results show that analysts at investment banks provide less objective recommendations and forecasts for firms in the financial sector after controlling for a comprehensive set of variables. We find that analysts at IRFs tend to be less optimistic (more pessimistic) when covering firms in the financial sector. We also find that the recommendations by IRF analysts covering firms in the financial sector tend to be generally more informative and the forecasts by IRF analysts tend to be more accurate. The results related to forecast accuracy are especially convincing. Compared to recommendations, which could extend over a longer and unspecified horizon, it is relatively straightforward to check the performance of forecasts by comparing the forecasts with the realization of actual earnings. Opposite results are found when we restrict our sample to firms NOT in the financial sector. We also provide evidence showing that analysts at investment banks issue more biased recommendations and less accurate forecasts especially when they cover bulge brackets, namely, investment banks with a large market share. These results suggest that investment bank analysts are susceptible to institutional pressure related to the

[4] "Brokerage" refers to all financial institutions issuing recommendations or forecasts in IBES unless otherwise noted.

underwriting business. In summary, our results suggest that investment bank analysts issue rosier recommendations and forecasts than their counterparts at IRFs.

We contribute to the literature by focusing on whether a third party, namely, an IRF, provides better research when covering firms in the financial sector. Most previous studies show that compared to analysts at IRFs, investment bank analysts generally achieve better performance by issuing more influential recommendations and more accurate forecasts when covering all sectors. In other studies, the performance of IRF analysts is no better than that of their counterparts at investment banks. As for forecasts, Jacob, Rock, and Weber (2008) find that the forecasts issued by investment bank analysts are, on average, more accurate than forecasts made by other analysts. Gu and Xue (2008) find that the forecasts issued by IRF analysts are less accurate ex post. Dugar and Nathan (1995) find that forecasts made by investment bank analysts are associated with the same level of accuracy as those made by their counterparts at non-investment banks.

As for recommendations, the literature has also shown that investment bank analysts' upgrades are less informative in an earlier sample time period, while their upgrades and downgrades are more informative using a more recent sample period. Barber, Lehavy, and Trueman (2007), using a sample that runs from January 1996 through June 2003, find that recommendation upgrades (downgrades) by investment banks underperform (outperform) similar recommendations by IRFs. Clarke, Khorana, Patel, and Rau (2011), using a more recent sample that runs from November 2000 to December 2007, find that there is little difference in the informativeness of recommendations by investment bank analysts and IRF analysts prior to the Global Research Settlement. After the Global Research Settlement (GRS), recommendation upgrades by IRF analysts are significantly less informative than recommendation upgrades by other analysts. As for recommendation downgrades after the Settlement, IRF analysts again generate the least informative downgrades among all type of analysts. Using a sample that runs from 1996 to 2007, Casey (2013) finds that recommendation upgrades and downgrades issued by IRF analysts are significantly less informative, even when analyst ability, brokerage firm resources and portfolio complexity are controlled for. Clarke, Jayaraman, and Liu (2015), using post 2006 data, find that sanctioned investment banks revert to their old practices after the GRS expired in 2009.

However, the general findings on the performance of IRF analysts and investment bank analysts might not apply to the financial sector for the following reasons. IRF analysts could play an important role when covering firms in the financial sector, since they have fewer incentives to please other firms in the financial industry. Investment banks' incentive to please other firms in the financial sector could be one reason why the superior performance by investment bank analysts disappears. Especially when investment banks cover other bulge bracket investment banks, conflicts of interests may play a significant role and might affect the ability of analysts to provide investors with objective information.[5] In that respect, our research extends Ljungqvist, Marston, and Wilhelm (2009), who suggest that the relationship between investment banks in a syndicate might also create a potential conflict of interest problem for analysts. Since investment banks frequently work together on a deal ("syndicate") (Ljungqvist, Marston, and Wilhelm, 2006), there are opportunities to act as a "co-manager".[6] The investment bank chosen by the issuer as the "lead" (or "co-lead") underwriter(s) has some ability to influence the issuer regarding selecting additional investment banks ("co-managers") in syndicate. "Lead"(or "co-lead") underwriter(s) may reward this position to investment banks in a good relationship with the underwriter(s). Compared to IRF analysts, analysts at investment banks could have a stronger incentive to use optimism in order to help their employers maintain a good relationship with other investment banks and win a co-manager position in lucrative future deals. For analysts at investment banks, more accurate forecasts and more informative recommendations do not always translate to higher revenues. Lastly, our study also contributes to a growing body of literature that documents conflicts of interests faced by analysts and shows that analysts, even experienced ones, are more optimistic toward their brokerages' potential underwriting clients (issuers) (Lin and McNichols, 1998; Michaely and

[5] Bulge brackets are the largest investment banks that have significant market shares in the equity underwriting market.

[6] The equity underwriting business offers substantial revenues for investment banks. Ljungqvist, Marston, and Wilhelm (2009) find that as the proceeds raised by U.S. issuers increase, the lead underwriter capacity constraint might force it to share the fee revenue with other investment banks by forming a syndicate. In the equity underwriting market, the "lead" underwriter or "co-lead" underwriters (when there are multiple lead underwriters) controls the offering and leads a group of co-manager underwriters. All "lead" ("co-lead") and "co-manager" underwriters together form a syndicate. "Lead" ("co-lead") underwriter(s) are chosen by the issuer. Being a "co-manager" is still very important, as it could increase an investment bank's chances of becoming a "lead" underwriter in future deals, according to Ljungqvist, Marston, and Wilhelm (2009).

Womack, 1999; and Ljungqvist, Marston, and Wilhelm, 2006).[7]

The results of our study are potentially important to academics, investors, and regulators. In our paper, the biased recommendations and forecasts given to firms in the financial sector stem from the fact that investment bank analysts are under pressure to maintain a good relationship with other firms in the financial sector. Analysts at investment banks, despite the establishment of the "Chinese Wall", are potentially less likely to achieve the independent status of IRFs. Our paper also suggests a possible solution to the objectivity problem in the direction of regulatory and supervisory policies. The research by IRFs should be given more weight and used more often by regulators of the financial sector.

The remainder of the paper is organized as follows. Section 2 describes the sample and variable construction. Empirical results involving recommendations are presented in Section 3, while the results involving forecasts are presented in Section 4. Section 5 attempts to explain the reason for the inferior performance by investment bank analysts. We conclude our paper in section 6.

3.2 Data Sources and Variable Construction

This section describes the data and variables used to examine how brokerage types influence the performance of analysts covering firms in the financial sector.

3.2.1 Variables of Interest

3.2.1.1 Analyst Recommendations and Forecasts

The data on analyst behavior come from the Institutional Brokerage Estimation System (IBES). IBES records the names of the firms covered by analysts. We collect data on recommendations and forecasts issued by analysts for the sample period from January 1994 to December 2013 (although IBES provides information on analyst recommendations starting from 1993, 1994 is the first year in IBES when reasonably complete data on recommendations are available). We follow the convention in the literature and exclude observations without

[7] In addition, Cowen, Groysberg, and Healy (2006) show that analysts are under pressure to issue biased reports to help generate trades for the brokerage they work for. Kolasinski and Kothari (2008) find that analysts affiliated with the acquirer advisor publish biased reports for both the acquirer and the target in order to benefit the acquirer advisor.

industry information, level of recommendations, or earnings estimate information. We also exclude observations involving non-US firms issued by a team of analysts or by an anonymous analyst (Sonney, 2009). We exclude observations involving a team of analysts because it is problematic to account for their general and firm specific experience, number of stocks followed, and forecast frequency. In the forecast database, we focus on annual forecasts of firms' earnings. In addition, we truncate the data of earnings, where earnings estimates fall into the top or bottom 0.5% group. Lastly, we exclude reshuffled recommendations at the time when brokerages moved from a five-tier to a coarser three-tier or four-tier rating system (Kadan, Madureira, Wang, and Zach, 2009). Firms are determined to be in the financial sector and are thus retained in our sample if IBES indicates the firms as such or if the name of a firm appears either in Nelson's Directory or SDC as a financial institution. Our final sample contains 100,024 recommendations and 212,577 forecasts issued by 3,613 analysts covering the financial sector employed by 493 brokerages. With the data from the IBES, we construct the following variables to measure the optimism (pessimism) of recommendations and forecasts, the accuracy of forecasts, and the informativeness of recommendations:

Strong Buy/Buy and Hold/Underperform/Sell recommendations: IBES records each recommendation with a number ranging from 1 to 5 (1=Strong Buy, 5=Sell). We follow Barber, Lehavy, and Trueman (2007) and place strong buy and buy recommendations together into the same group and place hold, underperform, and sell recommendations into the other group. Thus, in our study, recommendations fall into one of the two groups.

Forecast Relative Optimism/Pessimism Score: Unlike recommendations, which tell us directly how optimistic (pessimistic) the analysts are towards a particular firm, forecasts have to be compared with a benchmark to measure their optimism (pessimism). Similar to Hong, Kubik, and Solomon (2000), Ljungqvist, Marston, and Wilhelm (2009), and Malmendier and Shanthikumar (2014), we use the distance between a forecast and the benchmark ("consensus") to measure its optimism (pessimism). We first calculate the consensus as the mean of all forecasts issued for each firm-period by all other analysts within the prior 12 month period. If we define the consensus as the median of all forecasts for each firm-period, our results do not change. We then assign all forecasts for each firm-period into two groups: forecasts that are greater than the consensus (the optimistic forecast group) and forecasts that are less than the consensus (the pessimistic forecast group). Lastly, we rank the forecasts within each forecast group from the most optimistic (pessimistic) forecast for a given firm-period to the forecast closest to the consensus. Following Hong and Kubik (2003), we calculate the relative optimism (pessimism) score as $100 - 100 * [(rank_{i,j,t} - 1)/(number\_of\_forecasts_{i,j,t} - 1)]$. A higher relative optimism (pessimism) score reflects a more optimistic (pessimistic) forecast. Similar to Cowen, Groysberg, and Healy (2006), we require that there be at least three outstanding forecasts issued by other analysts in the prior 12 month period for each firm-period; otherwise, the relative score variable is coded empty. This ordered relative score variable, by controlling firm and time factors, allows us to compare forecasts across different firms, analysts, and time periods.

Positive and Negative Forecast Error: Polls conducted by Institutional Investor imply that the ability to accurately forecast a firm's earnings is a valued skill. We capture each forecast's accuracy by calculating its error. To do that, we measure the difference between the forecast estimate and the earnings announced. To allow for comparison between firms, we scale this number using the beginning of quarter stock price from CRSP. $Standardized\_forecast\_error = 100 * (forecast\_EPS - actual\_EPS)/price$. Similar to the forecast relative score, we separate the standardized forecast error into two groups: a positive forecast error group, in which the forecast estimate is larger than the earnings announced, and a negative forecast error group, in which the forecast estimate is smaller than the earnings announced. We separate the forecast error into two variables, since both a very positive and a very negative forecast error point to inaccurate forecasts. We also refrain from taking the absolute value of the forecast error, since using the absolute forecast error as the dependent variable would be problematic for the regression by violating the underlying assumptions of OLS. Lastly, we follow Gu and Xue (2008) and remove outliers that fall into the top and bottom 1% of standardized forecast errors.

Market Reaction to Recommendations: Unlike forecast accuracy, which can be calculated by comparing the forecast estimate to the earnings announced, to measure the information content of recommendations, we need to look at their market reaction. The Cumulative Abnormal Return (CAR) surrounding the issuance of recommendations are downloaded from CRSP and Eventus. The CAR is determined using the Fama-French 3-factor model. Following Loh and Stulz (2011), we use the announcement return of the event window (0,1) surrounding the recommendation issuance. Day 0 is the day the recommendation is issued.[8]

3.2.1.2 The Definition of Independent Research Firms

In our study, the IRF group consists of a variety of brokerages that do not engage in a material amount of equity underwriting, and at the same time, they employ analysts who issue recommendations and forecasts. To accurately classify brokerage types, we follow the convention in the literature, specifically that of Cowen, Groysberg, and Healy (2006) and Jacob, Rock, and Weber (2008), who use both Nelson's Directory and SDC for the classification of brokerage types and who follow a multi-step approach. Similar to Barber, Lehavy, and Trueman (2007), when using SDC data, we only refer to the SDC equity underwriting database to classify brokerages; we do not utilize information appearing in the SDC debt deal or SDC merger and acquisition databases. Given the large magnitude of the equity underwriting market, it is unlikely that there are many brokerages that engage solely in debt or merger and acquisition deals and do not engage in equity deals. We obtain data of equity underwriting activities of brokerages both in the U.S. and abroad from SDC. The data we include in our sample cover equity offerings from January 1989 to December 2013. The sample includes records of IPO deals and seasoned offerings involving both public and private firms. In our SDC sample, we follow the convention in the literature and exclude observations with missing information on deal size, observations with missing identifiable underwriters, observations with missing SIC codes, and observations involving firms with a SIC code in the 6000-6999 (financial industry) or 9000-9999 (government agencies) range.[9] To link underwriters in SDC to brokerages that employ analysts in IBES, we use the IBES identification file. Sonney (2009) and Malmendier and Shanthikumar (2014) rely on the same technique to link the databases. In the SDC database, the subsidiary of a brokerage is sometimes listed as the underwriter of a deal. To ensure that we use the names and codes of the parent brokerage to link the two databases, we manually check each match.

To classify a firm as an IRF, we first manually match brokerages in IBES to institutions' names in Nelson's Directory. Brokerages listed either as Independent Research Firms or Research Firms in Nelson's Directory are initially classified as IRF; the remaining of

[8] According to Cliff (2004), recommendations are typically issued prior to market closing and often even in the morning.

[9] If the underwriter is listed as NONE-RETAINED, NON-UNDERWRITTER, NOT-AVAILABLE, NOTAPP, or DIRECTLY-PLACED, we assume the underwriter is not identifiable.

brokerages are classified as investment banks. Next, we examine the brokerages classified as IRFs during our preliminary classification stage and search their names in SDC. More specifically, we examine whether brokerages classified as IRFs are initially involved in any equity underwriting business and whether the equity underwriting business is material (equity market share in a given year larger than 0.1%; "co-lead" underwriters given partial credits). We are reassured about the accuracy of the information in Nelson's Directory, since we find only two brokerages that fit this description, and we reassign them as investment banks. Third, we look at all the brokerages classified as investment banks in the previous two stages and reassign those brokerages if their equity underwriting business is of a non-material amount (equity market share in a given year less than 0.1%). We reclassify these brokerages as IRFs, since they do not engage in a material amount of equity deal underwriting. Similar to Cliff (2004), we allow IRFs in our sample to engage in a non-material amount of equity underwriting business. In these infrequent instances, there is evidence that sometimes brokerages with a small investment banking arm participate only to receive share allocations for their customers. Similar to Barber, Lehavy, and Trueman (2007), we do not distinguish between brokerages issuing research while at the same time engaging in trading securities from brokerages engaging solely in research. Brokerages identified by our three stage procedure as IRFs could engage in trade commission generating activities. Through these three stages, we are confident that the IRFs in our sample do not engage in a material amount of equity underwriting business and do not receive a material amount of underwriting revenue.

3.2.2 Control Variables

Besides the IRF indicator variable, there are several other variables related to the observable characteristics of analysts, brokerages, firms covered, and timing of the research that could influence the performance of analysts. In this section, we discuss how we construct these control variables.

3.2.2.1 Analyst Characteristics

We construct several variables measuring analysts' general and specific experience, coverage breadth, and other characteristics that could affect their performance.

Analyst general experience: We use the length of an analyst's career as the proxy for analyst general experience. We calculate the career length as the number of years the analyst has been in the IBES database until a given year. Hong, Kubik, and Solomon (2000) find that the longer the analyst's career, the bolder his/her recommendations, while inexperienced analysts tend to herd, since they are penalized if they are bold and inaccurate. Also, analysts who are more accurate and more objective are more likely to remain employed and thus exhibit a longer tenure in our sample.

Analyst-Firm Experience: Following Clement and Tse (2005), we code the measure for analyst specific experience, analyst-firm experience, as the number of years the analyst has covered a given firm in the IBES database. Trueman (1994) predicts that analyst-firm experience is associated with bold forecasts, since most skilled analysts have no one else to mimic and therefore do not exhibit herding behavior. Mikhail, Walther, and Willis (1999) document that more accurate analysts are associated with longer analyst-firm experience. According to Cowen, Groysberg, and Healy (2006), analyst optimism could be associated with analyst-firm experience. The longer the analyst follows the firm, the more likely the analyst is acquainted with its management, and the more likely the analyst is to refrain from damaging this relationship.

Analyst Breadth: Following Firth, Lin, Liu, and Xuan (2013), we construct the variable analyst breadth, which is the number of firms for which the analyst issued at least one recommendation during a given year. It measures the breadth of analyst's coverage. Clement and Tse (2005) find that forecast boldness increases with the number of firms covered by the analyst. It can also measure the dilution of the analyst's time and attention, since analysts covering a large number of firms might not be able to allocate sufficient time to each firm.

All-Star: Ljungqvist, Marston, and Wilhelm (2006) find that all-star analysts are less likely to bend to pressure from investment bankers. We thus control for all-star status in our regression. Using the annual Institutional Investor All-America ranking, a ranking based on a survey of fund managers, we construct an all-star indicator following Clarke, Khorana, Patel, and Rau (2007). This indicator equals one if the analyst is awarded with the Institutional Investor All-America recognition in a given year, and zero otherwise.

Analyst Forecast Frequency: Following Clement and Tse (2005), we construct the analyst Forecast frequency variable, which is the number of forecasts the analyst issued for a given firm-period. Clement and Tse (2005) find that analysts who frequently issue forecasts are unlikely to herd. Jacob, Lys, and Neale (1999) find that forecast accuracy is positively associated with analyst forecast frequency. They argue that forecasts are costly for the analyst, since the analyst needs to spend time and resources to generate each forecast; thus, the frequency of forecasts can capture the analyst's efforts in a given firm.

3.2.2.2 Brokerage Characteristics

Using data from the IBES database, we first create the following three variables related to brokerage characteristics:

Brokerage Size: Larger brokerages are usually more established, they offer better access to data and training, and they attract more skilled analysts. Clement and Tse (2005) and Jacob, Lys, and Neale (1999) find that analysts at larger brokerages are less likely to issue a herding forecast, while analysts working at smaller brokerages who face a lack of resources are more prone to herd. They also find that analysts employed by larger brokerages issue more accurate forecasts. Stickel (1995) finds that the recommendations issued by analysts at larger brokerages are more informative. To measure brokerage size, we count the number of analysts working for the brokerage in the prior year and use it as a proxy for brokerage research environment and research resources.

Brokerage Age: We count the number of years the brokerage has been in the IBES database until the prior year and label this variable brokerage age.

Brokerage Breadth: We follow Cohen, Lou, and Malloy (2013) and use the number of firms covered by the brokerage in the prior year as a proxy for the brokerage's research breadth.

Using data from the SDC database, we then create the two remaining brokerage characteristics variables below:

Brokerage Reputation: The brokerage reputation variable measures the market share of each brokerage in the equity underwriting market. Similar to Malmendier and Shanthikumar (2014) and Ljungqvist, Marston, and Wilhelm (2009), when calculating the brokerage reputation variable, we only consider the amount of equity raised by the brokerage as "lead" underwriter. If the deals are "co-led" by multiple brokerages, equal partial credits are given to each "co-lead" brokerage. The aggregated equity deal amount of each brokerage in the prior year is then divided by the total equity market size in the prior year. The higher the share of the brokerage, the more reputable it is.

Brokerage Pressure: The brokerage pressure variable is similar in spirit to the fee pressure variable proposed by Ljungqvist, Marston, and Wilhelm (2006), which uses brokerage's underwriting fees earned from deals in the current year divided by the brokerage's prior-year fee income. Since in SDC, fee information is not available for many observations, we use proceeds instead of the fee income to construct this variable. In our paper, the brokerage pressure variable is calculated as brokerage's prior year, t-1, underwriting proceeds as lead underwriter divided by its underwriting proceeds in year t-2. In other words, the brokerage pressure variable is the percentage change in deal proceeds relative to the previous year. If deals are co-led by multiple brokerages, each brokerage is given an equal amount of partial credit. Ljungqvist, Marston, and Wilhelm (2006) find that if the brokerage's fee income rises in the past two years, there could be less pressure on an analyst to provide biased reports. Thus, rising fee income could be associated with less aggressive behavior on the brokerage's side. On the other hand, if the brokerage's fee income decreases in the past two years, the decreasing fee income might prompt the analyst to provide biased reports in an effort to attract new deals and to reverse the decline for the brokerage.

3.2.2.3 Characteristics of the Firms Being Covered

In this section, we describe how we construct the variables related to the characteristics of firms. By controlling for the variables in this section, we address the possibility that IRFs and investment banks cover completely different firms.

Analyst Following: As in Feng and McVay (2010) and Casey (2013), we control for the number of unique analysts following the firm in a given year.

Market Capitalization: We obtain market variables from CRSP. In CRSP, we exclude observations with negative stock prices. Using the beginning stock price and share outstanding per firm-year, we calculate the market capitalization (in thousand \$) of firms being covered. In the regression, we take the natural logarithm of the market capitalization variable.

Leverage: We obtain accounting variables from Compustat. For each firm-year, we download the end of the year total asset value and total long term debt value. We use the value measured at the end of the previous year as the measure for the beginning of the next calendar year. For Leverage, we refer to Casey (2013) and use the following equation: $Leverage = Long\_term\_debt\_total/Assets\_total$.

Earning Loss Indicator: Following Feng and McVay (2010), we construct the earning loss indicator, which equals one if the earnings announced is negative in a given year and zero otherwise.

Earning's Shock: we construct the earning's shock as the absolute change of earnings from the previous year, scaled by the beginning of the year stock price. $Earnings\_stock = 100 * [abs(actual\_EPS - last\_year\_EPS)]/price$.

3.2.2.4 Timing of Research Issuance

Horizon: We control for horizon, measured as the number of the days between the forecast issuance date and the date of the earnings announcement. We control for horizon in our regressions since the age of the forecast (horizon) is an important factor determining forecast accuracy (Clement, 1999). Older forecasts are less accurate than newer forecasts. Prior research also shows that analyst optimism declines when approaching the date of earnings announcement ("walk-down phenomenon") to enable the management to beat the forecast estimate by a slight margin. Note that for recommendations, the value of the horizon could not be determined; thus, we use the variables described below to control for the timing of recommendations.

Friday Issuance: DellaVigna and Pollet (2009) find that due to investor inattention before weekends, Friday announcements are followed by less immediate responses and more drifts. Thus, I construct the Friday issuance indicator, which equals one if the recommendation or forecast is issued on a Friday.

Proximity: When a recommendation elicits a larger market reaction, it could mean that this recommendation is very informative. However, the CAR surrounding the recommendation can also capture other confounding news about the firm. To ensure this is not the case, in regressions involving analyst recommendations, I control for the fact that some recommendations are issued within close proximity in terms of time to the earnings announcement. Proximity 3 days (5 days) equals one if the recommendation is issued within the three-day (five-day) window surrounding the announcement of earnings, and zero otherwise.

3.2.3 Descriptive Statistics

Panels A and B of Table 1 provide descriptive statistics for the recommendations and forecasts, respectively, in the IBES database given to firms in the financial sector. During the 1994–2013 period, our sample covers more than 100,000 recommendations and 210,000 forecasts issued by 493 brokerages on more than 2531 firms. In total, 48.86% of recommendations are strong buy/buy recommendations; the rest are hold/underperform/sell recommendations. The average Fama-French 3-factor model adjusted CAR (0,1) to strong/buy recommendations is 0.87%. For hold/underperform/sell recommendations, it is -1.19%. On average, 7.71 and 8.76 analysts follow a firm in the recommendation and forecast samples, respectively. Untruncated positive (negative) price-standardized forecast errors have a median of 0.5917 (-0.3387), with the mean quite affected by outliers.

3.3 Recommendation Optimism and Informativeness Tests and Results

3.3.1 Recommendation Optimism

3.3.1.1 Recommendation Optimism Research Design

In this section, we examine our first research question: whether analysts employed at IRFs issue less biased recommendations to firms in the financial sector. More specifically, since equity deals provide fees that are an important source of revenue for investment banks, does the incentive to maintain a friendly relationship with other firms in the financial sector prompt the investment bank analysts to issue biased recommendations to them? IRFs generally have no incentive to issue biased recommendations to other firms in the financial sector in return for being included in equity deals in the future. The following empirical test addresses this question.

We estimate the following logit regression model in which we relate the optimism (pessimism) of recommendations with the type of brokerage employing the analyst:

$$\ln\left(\frac{P_{i,F,t}}{1-P_{i,F,t}}\right) = \beta_0 + \beta_1 \cdot IRF_{B,t} + \beta_2 \cdot ANA\_CHA_{i,t} + \beta_2 \cdot BRO\_CHA_{B,t-1}$$
$$+ \beta_3 \cdot FIR\_CHA_{F,t} + \beta_4 \cdot TIM\_CHA_{F,t} + Firm_F + Year_t + \varepsilon_{i,F,t} \quad (1)$$

where in year t , analyst i works for brokerage B and issues a recommendation to firm F , a firm in the financial sector. We are interested in whether the probability of a hold/outperform/sell recommendation, $P_{i,F,t}$, is affected by the type of brokerage B . The unit of observation in this model is one single recommendation. Since recommendations are non-continuous from 1 to 5 on an ordinal scale, we aggregate the recommendations into two groups: strong buy/ buy or hold / underperform/ sell. The dependent variable in this model is an indictor variable for hold / underperform/ sell recommendations. If brokerage B is an IRF, then $IRF_{B,t}$ is coded to be equal to one, and zero otherwise. We use this IRF indicator variable we constructed in the data section as the independent variable of interest. To examine whether recommendation optimism (pessimism) varies by brokerage type, we also need to control for several measures associated with recommendation optimism identified by previous studies. They include an array of observable characteristics of the brokerage, $BRO\_CHA_{B,t-1}$, the analyst, $ANA\_CHA_{i,t}$, the firm covered, $FIR\_CHA_{F,t}$, and the timing of the issuance, $TIM\_CHA_{F,t}$, which we have described before as control variables. In addition, we include fixed effects for firm F , the firm in the financial sector that received the recommendation. If we find that firm F receives more optimistic recommendations from some brokerages and more pessimistic recommendations from other brokerages, then we might infer that some factors besides firm F 's fundamentals induce the difference. Year fixed effects are also included. All standard errors are robust standard errors. We test whether the type of brokerage predicts the optimism of recommendations issued by its analysts, conditional on observable characteristics and fixed effects. All else equal, we expect that analyst i , who is employed by IRF, is less likely to issue favorable recommendations (or more likely to issue unfavorable recommendations) for a firm in the financial sector.

3.3.1.2 Recommendation Optimism Empirical Results

Table 2 presents the findings for the above equation using measures of recommendation pessimism as the dependent variables. We estimate the results for above equation, separately using different sets of control variables in Columns (1)-(3). In Column (1), we only include control variables related to the observable characteristics of the brokerage and the analysts. In Column (2), in addition to the control variables in Column (1), we also include a set of variables related to the observable characteristics of the firm being covered. Finally, in Column (3), we add control variables related to the timing of the issuance. In Columns (1)-(3) of Table 2, the dependent variable is an indicator for hold/underperform/sell recommendations. The positive and significant coefficient for the IRF indicator indicates that brokerage B 's analyst i is more likely to issue hold/underperform/sell recommendations to firm F if brokerage B is an IRF. The estimates in Table 2 imply that when covering the financial sector, analysts at IRFs make less optimistic recommendations and more pessimistic recommendations than analysts at investment banks. In Column (3), the significant estimate for the IRF indicator of 0.017 implies that the likelihood of issuing a hold/underperform/sell recommendation is 50.8% for analysts at IRFs versus 49.1% for their counterparts at investment banks, causing a relative increase of 3.5% (=1.70%/49.10%).

Consistent with earlier findings, we find that analysts with longer tenure issue less biased recommendations. Also, analysts with more firm specific experience tend to be more optimistic. The significant and negative coefficients of the variable analyst-firm experience in Columns (1)-(3) imply that the longer the analyst follows a given firm, the more optimistic his/her recommendations.

Overall, the evidence in Table 2 is consistent with the interpretation that analysts at investment banks are more likely to refrain from issuing pessimistic recommendations to other firms in the financial sector, while analysts at IRFs issue less biased and more objective recommendations. The behavior of analysts at investment banks is likely driven by the investment bank's hope that it will maintain a good relationship with other firms in the financial industry.

3.3.2 Informativeness of Recommendations

3.3.2.1 Informativeness of Recommendations Research Design

Our initial set of analyses compares the optimism of recommendations issued by analysts at IRF and investment banks. Subsequently, we examine the informativeness of their recommendations.

As described in the data section, we measure the informativeness of recommendations by comparing the Cumulative Abnormal Return (CAR) in a two day window surrounding the recommendation issuance date, starting on the day of the issuance. The dependent variable in this section is the CAR (0,1) surrounding the recommendation issuance date. The CAR is adjusted using the Fama-French 3-factor model. Similar to the last section, we partition the recommendation sample into strong buy/buy and hold/underperform/ sell recommendations. We examine whether the recommendations made by IRFs are more informative than those issued by investment banks. The indicator variable IRF, takes the value of one if the brokerage engages in research and does not engage in a material amount of equity underwriting business. This independent variable of interest allows us to examine the incremental amount of information provided by IRF recommendations.

We adopt the following model:

$$\begin{aligned} CAR(0,1)_{i,F,t} = \beta_0 + \beta_1 \cdot IRF_{B,t} + \beta_2 \cdot ANA\_CHA_{i,t} + \beta_2 \cdot BRO\_CHA_{B,t-1} \\ + \beta_3 \cdot FIR\_CHA_{F,t} + \beta_4 \cdot TIM\_CHA_{F,t} + Firm_F + Year_t + \varepsilon_{i,F,t} \quad (2) \end{aligned}$$

where we regress $CAR\,(0,1)_{i,F,t}$ on the indicator $IRF_{B,t}$ and an array of observable characteristics of the brokerage, $BRO\_CHA_{B,t-1}$, the analyst, $ANA\_CHA_{i,t}$, the firm being covered, $FIR\_CHA_{F,t}$, and the timing of the issuance, $TIM\_CHA_{F,t}$, since these factors could also contribute to varying degrees of informativeness. We also control for firm and year fixed effects. We use the same set of control variables as in the last section.

3.3.2.2 Informativeness of Recommendations Empirical Results

Table 3 presents the results for equations (2) and compares the CAR (0,1) surrounding optimistic and pessimistic recommendations issued by IRFs to those issued by investment banks. The dependent variables in Columns (1)-(3) and (4)-(6) are

CAR (0,1) surrounding strong buy/buy recommendations and hold/underperform/sell recommendations, respectively. Columns (1), (2), (4), (5) present the base regression with a limited number of control variables, and Columns (3) and (6) show the results involving a more comprehensive set of control variables. The coefficients on the IRF indicator variable capture the differential informativeness of recommendations issued by IRF analysts and investment bank analysts. The positive and significant coefficients of the IRF indictor in Columns (1)-(3) are consistent with recommendations issued by IRF analysts to firms in the financial sector being more informative. Across Columns (1)-(3), the difference in market reaction to optimistic recommendations between those issued by IRFs and by investment banks is 19.6˜21.2 basis points. Columns (4)-(6) show an exception: the market finds pessimistic recommendations issued by investment banks to firms in the financial sector more informative.[10] The pessimistic recommendations by investment banks outperform those by IRFs by a significant and economically quite large 24.6˜29.4 basis points. In Column (6) of Table 3, the pessimistic recommendations of IRFs generate a CAR(0,1) of negative 119 basis points, this compares to a significantly negative CAR(0,1) of 148 basis points earned by those of investment banks. Given that investment bank analysts are usually more optimistic when covering firms in the financial sector, a pessimistic recommendation, which is costly to the investment bank analyst who issues it, is a strong and credible signal to the market that the fundamentals of the firm being covered are deteriorated such that even investment banks can no longer hide this situation. Thus the pessimistic recommendation issued by investment bank analysts causes a larger market reaction than a similar recommendation issued by its counterparts at IRFs.

For the control variables, the coefficients generally have the expected signs. We find that analyst-firm experience, brokerage age, and all-star indicator all contribute to more informative recommendations. I also find that analyst coverage breadth is associated with less informative recommendations, possibly pointing to a dilution of analysts' time and effort.

In summary, while analysts at IRFs issue more informative optimistic recommendations, analysts at investment banks issue more informative pessimistic recommendations.

[10]Note that the market reaction surrounding a pessimistic recommendation is generally negative; thus, a significant and negative coefficient points to a more informative pessimistic recommendation.

3.4 Forecast Optimism and Accuracy Tests and Results

3.4.1 Forecast Optimism

3.4.1.1 Forecast Optimism Research Design

In this section, we examine the differences in forecast optimism (pessimism) across brokerage types in multivariate settings to control for several factors associated with forecast optimism in prior research. It is likely that investment banking analysts are less likely to be objective not only in their recommendations but also in their forecasts.

The following multivariate model is used to examine the differences in forecast optimism across brokerage types:

$$\begin{aligned} Forecast\_Optimism/Pessimism_{i,F,t} &= \beta_0 + \beta_1 \cdot IRF_{B,t} + \beta_2 \cdot ANA\_CHA_{i,t} + \beta_2 \cdot BRO\_CHA_{B,t-1} \\ &+ \beta_3 \cdot FIR\_CHA_{F,t} + \beta_4 \cdot TIM\_CHA_{F,t} + Firm_F + Year_t + \varepsilon_{i,F,t} \quad (3) \end{aligned}$$

where the dependent variable is either the forecast relative optimism or pessimism score, $Forecast\_Optimism/Pessimism_{i,F,t}$, described in the data section. The IRF indicator $IRF_{B,t}$ equals one if the brokerage engages in research and does not engage in a material amount of equity underwriting. The indicator's coefficient measures the incremental optimism (pessimism) exhibited by the forecasts made by IRF analysts compared to those issued by investment bank analysts. If the IRF analysts' forecasts are, on average, not as optimistic as those issued by their counterparts at investment banks, the sign of the IRF indicator's coefficient will be negative for a regression in which the dependent variables are forecast relative optimism scores. When the dependent variable is the forecast relative pessimism score, we expect the IRF indicator to have a positive coefficient. Note that the forecast relative optimism (pessimism) score compares analyst i *'s* forecast to all outstanding forecasts for a given firm-period issued by other analysts in the last 12 months. Thus, this relative variable offers insights into the thought process of analyst i. Extreme values of the relative forecast scores indicate that the analyst knows in advance that such a forecast does not conform with the consensus and will likely attract the attention of the covered firm.[11]

Prior research documents that analyst forecast optimism is associated with several factors: horizon, analyst-firm experience (which we measure as the number of years the analyst has been following the given firm), and brokerage size (which is the number of analysts employed by the brokerage).

3.4.1.2 Forecast Optimism Empirical Results

In Columns (1)-(6) of Table 4, we examine whether there is any difference in forecast optimism for IRFs and investment banks. The dependent variable is the forecast relative optimism score in Columns (1)-(3) and the forecast relative pessimism score in Columns (4)-(6). Across Columns (1)-(3), the IRF indicator's coefficients are negative and significant, indicating that after controlling for a varying set of variables, IRF analysts remain relatively less optimistic than their counterparts at investment banks. For example, in Column (3), the IRF indicator's coefficient is -0.857 and is statistically significant. This estimate implies that on average, the forecast relative optimism score of IRF analysts is 0.857 points lower on a 0-100 scale than analysts at investment banks. These results are consistent with the incentive of investment banks to maintain a good relationship with other firms in the financial sector for the sake of future investment banking opportunities, which outweighs the investment bank analyst's concern to be objective. The coefficients for the IRF indicator across Columns (4)-(6) are positive but not significant, indicating that when examining all forecasts below the 12 month consensus, IRF analysts do not appear to be more pessimistic than their counterparts at investment banks.

For the set of control variables, we find that analyst-firm experience does affect forecast optimism, as the coefficient is significant and positive in Columns (1)-(3). This suggests that analysts who cover a firm for a longer time are more likely to rate it favorably relative to their peers, pointing to the stronger relationship formed between the analyst and the firm's

[11] Using the continuous forecast relative optimism (pessimism) score addresses the concern that pessimistic forecasts in our sample could actually be attempts by the analysts to please the firm covered. Prior research shows that forecast optimism declines slightly when approaching the date of earnings announcement to "lower the bar" for the firm's management. In these cases, the forecast ("the bar") is lowered just enough for the management to yield a positive surprise when announcing the earnings. Since we use a continuous variable that consists of information about the rank of a given forecast, the slightly pessimistic forecasts that are meant to "lower the bar" would not affect our results.

management over the analyst's tenure.

The results in this section provide evidence that IRF analysts tend to be less optimistic when covering firms in the financial sector than investment bank analysts.

3.4.2 Forecast Accuracy

3.4.2.1 Forecast Accuracy Research Design

In this section, we use forecast error as a measure of analyst forecast performance. We investigate whether IRF analysts issue more accurate forecasts to firms in the financial sector. We argue that the prior expectation typically associates investment bank analysts with more accurate forecasts, since investment banks are more resourceful and are able to equip their analysts with better access to information that is normally not available to IRF analysts. We examine whether this resource advantage outweighs investment bank analysts' incentive to please covered firms in the financial sector or vice versa. To do so, we test whether the forecast accuracy differs depending on the brokerage type, controlling for a set of comprehensive variables. If we find that investment bank analysts are relatively more accurate, it would be consistent with the hypothesis that investment bank resources explain the relative accuracy. If we find that IRF analysts are relatively more accurate, it would be consistent with the competing hypothesis that IRFs, which do not engage in a material amount of equity underwriting and are thus free from investment banking pressure, are able to issue relatively accurate forecast.

To test whether analysts employed by IRFs make more accurate forecasts relative to their counterparts at investment banks, we estimate a model that is similar to the equation in section 4.1.1.

$$\begin{aligned} Standardized\_forecast\_error_{i,F,t} &= \beta_0 + \beta_1 \cdot IRF_{B,t} + \beta_2 \cdot ANA\_CHA_{i,t} + \\ &+ \beta_2 \cdot BRO\_CHA_{B,t-1} + \beta_3 \cdot FIR\_CHA_{F,t} + \beta_4 \cdot TIM\_CHA_{F,t} + Firm_F + Year_t + \varepsilon_{i,F,t} \end{aligned} \tag{4}$$

where we replicate the previous equation now with $Standardized\_forecast\_error_{i,F,t}$ as the dependent variable. It is scaled by the beginning of quarter stock price to allow for comparison between firms. The dependent variable is either the positive forecast error, with the regression sample restricted to forecasts with estimates larger than the earnings announced, or the negative forecast error, with the sample restricted to forecasts with estimates less than the actual earnings number. To compare analysts employed by IRFs with those at investments banks, we include the IRF indicator $IRF_{B,t}$ in our regressions. This indicator variable takes the value of one if the forecast comes from an IRF analyst and zero otherwise. We estimate the model with firm and year fixed effects and control for a comprehensive set of factors shown in the literature to affect forecast accuracy. One of the controls we include is HORIZON, which measures the age of the forecast. Newer forecasts are likely to be more accurate since they include updated information. We include this control variable so that the forecast accuracy of either IRF or investment bank analysts is not driven by younger forecast age. We control for not only an array of brokerage and analyst characteristics, but also for characteristics of the covered firm, including leverage, natural logarithm of market capitalization, earnings shock and an indicator for earnings loss. This allows us to compare forecasts to different firms. In short, we use the same set of controls as in our previous section in which we examine forecast optimism.

3.4.2.2 Forecast Accuracy Empirical Results

In Table 5, we report results of the equation in section 4.2.1. In Columns (1)-(3), the dependent variable is the positive forecast errors. In Column (1), the coefficient of the IRF indicator is negative but not significant. However, as we add more control variables in Columns (2) and (3), the coefficient of the IRF indicator turns significant and negative, indicating that among forecasts with estimates larger than the earnings announced, analysts at IRFs are associated with higher forecast accuracy than analysts at investment banks. This result is consistent with the hypothesis that IRFs, by not engaging in a material amount of equity underwriting business, are able to provide more accurate forecasts. In Columns (4)-(6), the dependent variable is the negative forecast error. Among forecasts with estimates smaller than the earnings announced, we do not find that IRF analysts and investment banking analysts differ in terms of accuracy.

Perhaps unsurprisingly, among the control variables, we find that analyst covered breadth, measured by the number of stocks covered by a given analyst, has a positive and significant coefficient in Columns (1)-(3). This is consistent with the explanation that analysts are less accurate when their time and effort is diluted. HORIZON, the measure for the age of the forecast, has a positive coefficient in Column (3) and a negative coefficient in Column (6), with both coefficients being significant. This points to a larger absolute forecast error for older forecasts.

The results in Table 5 confirm the hypothesis that when covering firms in the financial sector, IRF analysts, free from investment banking pressure, outperform their counterparts at investment banks in terms of forecast accuracy.

3.5 Explaining the Inferior Performance by Investment Bank Analysts

3.5.1 Differences in Performance for Bulge Brackets and Non-Bulge Bracket Firms

In the previous sections, we find that compared to analysts at investment banks, analysts at IRFs issue more objective, more informative, and more accurate recommendations and forecasts. These results are very important to investors who read analysts reports. Yet it is also important to examine why the performance of investment bank analysts is poor when they cover firms in the financial sector. In this section, we hypothesize that there are at least two potential explanations for the superior performance of IRF analysts compared to investment bank analysts when covering the financial sector: (1) IRF analysts are better than investment bank analysts at networking and gathering new information. Given, first, that investment banks interact with each other when forming underwriting syndicates; and second, that investment banks could attract more informative analysts with better access to management using higher pay, this hypothesis is less credible; (2) the problem could be that analysts at investment banks have an incentive to please extremely large investment banks (bulge brackets) to pursue roles in future equity deals. Indeed, Ljungqvist, Marston, Wilhelm (2009) finds that both the issuer and the "co-lead" brokerages contribute to the choice of the "co-manager" brokerages in equity deals. They find that "co-lead" brokerages might influence the issuer's decision and steer the issuer to select a particular brokerage as the "co-manager". Note that by definition, investment banks in our study are brokerages that engage in a material amount of equity underwriting business. Thus, more revenue from equity underwriting is likely to be very attractive for investment banks compared to the cost associated with dishonest and biased research. Taking this into account, it is intuitive that investment banks may avoid issuing pessimistic recommendations or forecasts because they do not want to impair their relationship with bulge brackets, which have a say in the selection of "co-manager" for the next big equity underwriting deal. Thus, this second hypothesis would predict differences in recommendations and forecasts by firm type: bulge brackets are likely to receive biased recommendations and forecasts from other investment banks. If we do not observe that bulge brackets are treated differently by investment bank analysts, then this should be consistent with our first hypothesis.

To examine whether the poor performance of analysts at investments is driven by biased recommendations and forecasts issued to bulge brackets, we generate a sample that is made of all recommendations (forecasts) issued to investment banks. We use the reputation variable constructed in the data section, which measures market share, to distinguish between bulge brackets and other firms in the financial sector. We identify all firms in the financial sector receiving recommendations or forecasts and classify the firms based on their involvement in equity underwriting, using SDC. Firms whose amount of equity underwriting business accounts for more than a 10% market share in a given year are classified as a bulge bracket. The rest of the firms in the financial sector are classified as non-bulge bracket firms at this stage. Using the 10% criterion, we arrive at a set of 22 large investment banks and classify them as bulge brackets.[12] We supplement our SDC data with data from CRSP and Compustat. Firms with market capitalization in the 99th percentile are reclassified as bulge brackets. We use the following logit regression model for our multivariable regression:

$$\begin{aligned}\ln\left(\frac{P_{i,F,t}}{1-P_{i,F,t}}\right) &= \beta_0 + \beta_1 \cdot Bulge\_Bracket_{F,t} + \beta_2 \cdot IB\_analyst_{B,t} \\ &+ \beta_3 \cdot Bulge\_Bracket_{F,t} \cdot IB\_analyst_{B,t} + \beta_4 \cdot ANA\_CHA_{i,t} + \beta_5 \cdot BRO\_CHA_{B,t-1} \\ &+ \beta_6 \cdot FIR\_CHA_{F,t} + \beta_7 \cdot TIM\_CHA_{F,t} + Year_t + \varepsilon_{i,F,t} \quad (5)\end{aligned}$$

where the dependent variable is the probability of a hold/outperform/sell recommendation, $P_{i,F,t}$. The independent variables include an indicator variable for bulge

[12]The bulge brackets in our sample include Bank of America, Citigroup, Goldman Sachs, JP Morgan Chase, Morgan Stanley, UBS, and Wells Fargo, among others.

brackets, $Bulge\_Bracket_{F,t}$. The bulge bracket variable is set to one if the recommendation is issued to a bulge bracket. Other independent variables include $IB\_analyst_{B,t}$, an indicator variable that equals one if the recommendation is issued by an investment bank analyst. The independent variable of interest is the interaction variable between the two indicator variables. We expect the coefficient of the interaction to have a negative sign when the dependent variable is the indicator for hold/underperform/sell recommendations.

The coefficient for the interaction variable represents the incremental optimism (pessimism) exhibited by investment bank analysts while covering bulge brackets. We do not include firm fixed effects since doing so would absorb our bulge bracket variable. All other control variables and fixed effects are the same as the equation in section 3.1.1.

3.5.2 Empirical Results

The findings are reported in Table 6 for the recommendation optimism. In Columns (1) and (3), we control for brokerage reputation. However, there is concern for multicollinearity since we use the brokerage reputation variable to construct the bulge bracket variable. Thus, in Columns (2) and (4), we do not control for brokerage reputation. In Columns (3) and (4), we include issuing bank fixed effects instead of controlling for $IB\_analyst_{B,t}$. We find that analysts at investment banks are less likely to issue hold/underperform/sell recommendations to bulge brackets. In Columns (1)-(4), the bulge bracket variable has coefficients ranging from -0.069 to -0.078, all statistically significant at 1% level. In Column (2), for example, the coefficients imply that the probability of an investment bank analyst issuing a hold/underperform/sell recommendation to bulge brackets is 14% lower than to other firms in the financial industry. This suggests that investment banks are less likely to issue hold/underperform/sell recommendations to bulge brackets, which could damage their relationship with bulge brackets.

In Table 7, the dependent variable in Columns (1)-(4) is the CAR (0,1) to strong buy/ buy recommendations, while the dependent variable in Columns (5)-(8) is the CAR (0,1) to hold/underperform/sell recommendations. In Columns (2), (4), (6), and (8), we do not control for brokerage reputation because of multicollinearity concerns. In Columns (3), (4), (7), and (8), we include issuing bank fixed effects in our regressions instead of controlling for $IB\_analyst_{B,t}$. The coefficients in Columns (1)-(4) of Table 7 for the bulge bracket indicator are negative but insignificant. The coefficient in Columns (5)-(8) of Table 7 do not yield consistent or significant results. The insignificant results suggest that it is unlikely that the market and investors detected the bias exhibited in the recommendations issued by IB analysts covering bulge brackets.

In Table 8, we examine the optimism and pessimism of forecasts issued by investment banks to bulge brackets. Here, the dependent variable is the forecast relative optimism score in Columns (1)-(4) and the forecast relative pessimism score in Columns (5)-(8). However, we do not find that the forecasts received by bulge brackets differ significantly in terms of optimism (pessimism) from the forecasts received by non-bulge bracket firms in the financial sector. The lack of any significant difference in terms of forecast optimism could stem from the fact that forecasts, by nature, are less biased than recommendations. When considering the insignificant results in this table, we remind readers that analysts often produce two main quantifiable outputs about the covered firm's situation, namely, earnings forecasts and recommendations. Malmendier and Shanthikumar (2014) find that analysts have more incentive to twist their recommendations than their forecasts. A forecast is in the form of a continuous number, which is more difficult to interpret for the average investor and is more geared towards institutional investors, who are better at detecting biases. While a recommendation is scaled from 1-5 in the form of an integer (some brokerages issue coarser recommendations with only 3 levels instead of 5 levels) and is more geared towards individual investors who can more easily follow instructions like "buy" or "sell" and who can be easily misled by biased information. Thus our insignificant results in this table are not surprising.

In Table 9, we examine whether investment banks give bulge brackets inaccurate forecasts compared to the forecasts given to other firm in the financial sector. Here the model has overall better explanatory power. In Columns (1)-(4), the coefficient for the bulge bracket indicator is positive and significant at the 1% level, with the coefficient in Column (2) being an exception. In Columns (5)-(8), the coefficient for the bulge bracket indicator is negative and significant at the 5% level. Investment banks seem to perform worse when giving forecasts to bulge brackets compared to the forecasts given to other firms in the financial sector. This result is consistent with the prediction that investment banks want to please bulge brackets with biased research, which makes their research less valuable.

Overall, our results indicate that the more biased, less informative, and less accurate recommendations and forecasts by investment banks to firms in the financial sector is primarily driven by investment bank analysts' intention to please bulge brackets. Compared to recommendations and forecasts given by investment banks to other firms in the financial sector, the recommendations given to bulge brackets are significantly more optimistic and less pessimistic. The forecasts given to bulge brackets are significantly less accurate than forecasts given to other firms in the financial sector. These new results combined with our previous results on IRF analysts suggest that a brokerage's engagement in the equity underwriting business is detrimental to its objectivity when covering firms in the financial sector. IRFs, by not engaging in a material amount of equity underwriting, are less worried about straining their relationships with other firms in the financial sector and tend to blow the whistle more often. For investment banks, whose main source of revenue comes from underwriting and which would like to attract new underwriting business, the concern for maintaining a good relationship with other firms in the financial sector, especially the bulge brackets, outweighs the benefits associated with being unbiased.

3.6 Target Prices

Most analyst research reports contain three distinguishable measures: recommendations, earnings forecasts, and target price forecasts. While previous research finds that recommendations and earnings forecasts are often influential, recommendations and earnings forecasts also have certain disadvantages when transferring information to investors. Earnings forecasts are often issued shortly before the release of actual earnings and thus cover a relatively short period of time. Since most recommendations follow a discrete five-tier rating system, certain information might be lost when analysts issue recommendations that fall into one of the five tiers.

Compared to earnings forecasts and recommendations, target prices have the following advantages: 1) they explicitly tell investors about the analyst's beliefs regarding the firm's expected value; 2) compared to recommendations, the continuous nature of target prices is associated with less information loss before the information is transferred to investors. Recent research attempts to examine the incremental contributions of target price forecasts and whether analysts exhibit different abilities in making accurate target price forecasts. Brav and Lehavy (2003) and Asquith, Mikhail, and Au (2005) find that target price forecasts are incrementally informative. They show that earnings forecasts, recommendations, and target price forecasts each bring independent information to the market. Since target price forecasts do bring some incremental information to the market, they would then be relevant when we examine analysts' conflicts of interests and the potential biases exhibited by analysts.

In this section, we examine whether target price forecasts are more optimistic among investment bank analysts when they cover firms in the financial industry and whether they provide less accurate target price forecasts compared to their counterparts at IRF. The target price forecasts over the years 1994 through 2013 are from IBES. We restrict our sample to 12-month-ahead target price forecasts, which is the most commonly used target price forecast horizon. We follow Gleason, Johnson, and Li (2013) and remove extreme target prices to mitigate the influence of errors in the original data. We winsorize our target price sample at the 0.5% and 99.5% levels.

To examine the optimism of target price forecasts, we follow Brav and Lehavy (2003) and use the implied target price-based returns (TP/P), which is the ratio of target price announced by analysts (TP) to the current stock price (P). Following Bradshaw (2002), for the current stock price (P), we use the stock price just one day prior to the announcement of target prices. If the day prior to the announcement of the target price is a non-trading day, we use the price two days prior instead. The implied target price-based returns measure the belief of the analyst regarding the firm's expected return in 12 months. The greater the implied target price-based returns, the more attractive the stock looks to the analyst.

To access the accuracy of target price forecasts, we look at the long-term comovement of stock prices and target prices. More specifically, we follow Bradshaw, Brown, and Huang (2013) and construct the variable target price forecast error (TPERROR), which is (TP-P12)/P, where P12 is the stock price 12 months following the target price forecast announcement date. The closer the target price forecast error is to zero, the more accurate the target price prediction is. Since both a very positive and a very negative target price forecast error point to inaccurate target prices, we separate the target price forecast error into two variables, as we did in section 4 for earnings forecasts.

To examine whether IRF analysts and investment bank analysts exhibit differences in skills in forecasting target prices, we follow the regression setting for earnings forecasts in section 4. We include the IRF indicator and the same set of controls in this regression for target prices. The only exception is that we exclude the variable earnings shock and the indicator for earnings loss, since they are not applicable for the examination of target prices.

In Table 10, we report the results on the optimism of target price. The dependent variable is the implied target price-based returns, TP/P. In Columns (1)-(3), we add different sets of control variables. Through Columns (1)-(3), the coefficients of the IRF indicator are negative but only statistically significant at the 5%-10% level. The level of optimism exhibited by IRF analysts, as measured by the implied target price-based returns, appears to be less than the level of optimism exhibited by their counterparts at investment banks.

In Table 11, we report the results on the accuracy of target prices. In Columns (1)-(3), the dependent variable is the positive target price error. Among target prices with estimates greater than the stock price 12 months following the target price announcement, we do not find that IRF analysts and investment banking analysts differ in terms of accuracy. In Columns (4)-(6), the dependent variable is the negative target price error. In Column (4), the coefficient of the IRF indicator is positive but not significant. However, as we add more control variables in Columns (5) and (6), the coefficient of the IRF indicator turns to be significant at the 1% level, indicating that among target prices with estimates less than the stock price 12 months following the target price announcement, employment with IRF is associated with higher target price accuracy among analysts than employment with investment banks. This result is consistent with the hypothesis that IRFs, by not engaging in a material amount of equity underwriting, are able to provide more accurate target price forecasts. The results in Tables 10 and 11 echo earlier findings in our paper drawn from the recommendations sample and the earnings forecasts sample.

3.7 Conclusion

This paper investigates whether the recommendations and forecasts made by IRF analysts covering firms in the financial sector differ from those made by investment bank analysts. We evaluate several aspects of analyst performance: recommendation optimism and informativeness, earnings forecast optimism and accuracy, and target price forecast optimism and accuracy. We find that compared to investment bank analysts, IRF analysts offer less biased, more informative, and more accurate recommendations and forecasts when covering firms in the financial sector. We then make an effort to interpret the inferior performance of the investment bank analysts. Conflicts of interests appear to play a significant role when investment bank analysts cover other bulge bracket investment banks. This result suggests that investment bank analysts are susceptible to institutional pressure related to the underwriting business. The results of our study are potentially important not only to academics but also to regulators and investors. Our study has potential implications for policy debates on issues of objective research by analysts. We show that analysts at investment banks, despite the establishment of the "Chinese Wall", are less likely to achieve an independent status compared to IRFs. It might be beneficial for regulators to recognize the importance of IRF analysts covering firms in the financial sector.

Table 1: Descriptive Statistics

Table 1 provides descriptive statistics for the recommendations (Panel A) and forecasts (Panel B) issued to firms in the financial sector in the Institutional Brokerage Estimation System (IBES) database for the sample period from January 1994 to December 2013.

Regression Sample **Variable**	**Obs**	**Mean**	**S.D.**	**Min**	**Max**
Panel A: Recommendation Sample					
Strong Buy/Buy	100024	0.4886	0.4999	0.0000	1.0000
Hold/Underperform/Sell	100024	0.5114	0.4999	0.0000	1.0000
S. Buy/Buy CAR (0,1)	44367	0.8748	4.7188	-93.7207	142.7559
Hold/Underperform/Sell CAR (0,1)	47466	-1.1890	6.7638	-124.4806	144.4748
Brokerage Size	100024	58.5631	58.3125	1.0000	329.0000
Brokerage Pressure	100024	0.2006	2.7965	-0.9984	196.5909
Brokerage Reputation	100024	0.0167	0.0332	0.0000	0.1522
Brokerage Age	100024	8.8165	5.3003	1.0000	20.0000
Brokerage Breadth	100024	382.7085	318.3789	1.0000	1577.0000
Analyst Experience	100024	5.4128	3.9390	1.0000	20.0000
Analyst-Firm Experience	100024	2.1629	1.6172	1.0000	17.0000
Analyst Breadth	100024	14.1276	10.8888	1.0000	112.0000
Analyst Forecast Frequency	100024	1.7623	1.0851	1.0000	13.0000
Ln(Market Capitalization)	97319	14.2412	1.7924	6.0270	19.4275
Analyst Following	100024	7.7114	5.1848	1.0000	30.0000
Leverage	76704	0.1808	0.2070	0.0000	5.9451
CAR (-22,-2)	91834	-0.0039	0.1248	-2.4628	4.4596
Friday	100024	0.1739	0.3790	0.0000	1.0000
Proximity 5 days	100024	0.1854	0.3886	0.0000	1.0000
Proximity 3 days	100024	0.1407	0.3478	0.0000	1.0000

Table 1: (Cont.)Descriptive Statistics

Panel B: Forecast Sample					
Positive Forecast Error	104981	12.7838	208.2740	0.0000	41900.0000
Negative Forecast Error	95939	-1.8210	17.9468	-3730.0000	-0.0003
Forecast Relative Optimism Score	75275	50.0000	32.2247	0.0000	100.0000
Forecast Relative Pessimism Score	88948	50.0000	31.7914	0.0000	100.0000
Earning Loss	205954	0.0966	0.2954	0.0000	1.0000
Earning shock	185529	16.6264	692.8013	0.0000	159000.0000
Brokerage Size	212577	59.8256	58.2581	1.0000	329.0000
Brokerage Pressure	212577	0.1727	2.2096	-0.9984	196.5909
Brokerage Reputation	212577	0.0178	0.0341	0.0000	0.1522
Brokerage Age	212577	10.2979	5.4490	1.0000	20.0000
Brokerage Breadth	212577	376.1066	311.2088	1.0000	1577.0000
Analyst Experience	212577	6.2429	4.2233	1.0000	20.0000
Analyst-Firm Experience	212577	2.4607	1.8045	1.0000	17.0000
Analyst Breadth	212577	12.6890	9.0594	1.0000	112.0000
Analyst Forecast Frequency	212577	1.4822	0.8350	1.0000	13.0000
Ln(Market Capitalization)	207804	14.5960	1.8159	6.5965	19.4275
Analyst Following	212577	8.7604	5.7912	1.0000	33.0000
Leverage	169426	0.1350	0.1627	0.0000	1.6586
Friday	212577	0.1872	0.3900	0.0000	1.0000
Horizon	207053	193.2559	104.4844	-340.0000	2009.0000

Table 2: Optimism of Analyst Recommendations

Table 2 provides logit regression results testing whether the optimism of recommendations varies by brokerage type. The dependent variables are indicator variables for hold/ underperform/ sell recommendations in columns (1)-(3). The independent variables include IRF indicator, analyst general experience, analyst-firm specific experience, number of stocks covered by the analyst (analyst breadth), analyst forecast frequency, analyst all-star indicator, brokerage size, brokerage pressure, brokerage reputation, brokerage age, number of stocks covered by the brokerage (brokerage breadth), log(market capitalization of the firm being covered), number of analysts following the firm, firm leverage, cumulative abnormal return in the time window (-22,-2) before the issuance of recommendations, indictor for Friday announcements, proximity indictor which equals one if time between the date of recommendation issuance and the date of earnings announcement is less than five-days (three-days). Firm and year fixed effects are included. Each column represents a separate regression. Marginal effects are displayed. *, **, and *** represent significance at the 10%, 5% and 1% levels, respectively.

	DEP: Indicator variable for Hold/Underperform/Sell		
	(1)	(2)	(3)
IRF	0.027***	0.019***	0.017***
	(7.387)	(3.259)	(2.964)
Analyst Experience	-0.004***	-0.002***	-0.002***
	(-8.477)	(-3.313)	(-2.922)
Analyst-Firm Experience	0.009***	0.005***	0.004***
	(7.931)	(3.210)	(2.831)
Analyst Breadth	0.001***	0.000**	0.000**
	(4.383)	(2.458)	(2.028)
Analyst Forecast Frequency	-0.001	-0.003**	-0.004**
	(-0.599)	(-2.393)	(-2.545)
All Star	-0.017***	-0.013**	-0.013**
	(-3.147)	(-2.455)	(-2.504)
Brokerage Size	0.000	0.000	0.000
	(1.515)	(1.614)	(1.399)
Brokerage Pressure	0.000	0.000	0.000
	(0.557)	(1.039)	(1.030)
Brokerage Reputation	0.574***	0.409***	0.350***
	(10.130)	(3.464)	(3.113)
Brokerage Age	0.003***	0.001***	0.001***
	(7.093)	(2.955)	(2.605)
Brokerage Breadth	0.000**	0.000**	0.000**
	(1.966)	(2.060)	(2.211)

Table 2: (Cont.)Optimism of Analyst Recommendations

	(1)	(2)	(3)
Ln(Market Capitalization)		0.006**	0.007***
		(2.374)	(5.625)
Analyst Following		-0.000	0.000
		(-0.150)	(0.253)
Leverage		0.035*	0.031*
		(1.856)	(1.762)
CAR (-22,-2)			0.059***
			(3.095)
Friday			-0.003
			(-1.359)
Proximity 5 days			-0.003
			(-0.689)
Proximity 3 days			0.009*
			(1.809)
Year FE	YES	YES	YES
Firm FE	YES	YES	YES
Observations	98,557	75,002	71,060

Table 3: Market Reaction to Analyst Recommendation

Table 3 provides results testing whether the informativeness of recommendations varies by brokerage type. The dependent variables are cumulative abnormal return (CAR) in a two day window surrounding the issuance of strong buy/buy recommendations in columns (1)-(3) or hold/ underperform/ sell recommendations in columns (4)-(6), starting on the day of the issuance. The CAR is adjusted using the Fama-French 3-factor model. All independent variables, control variables and fixed effects are the same as in Table 2. Each column represents a separate regression. *, **, and *** represent significance at the 10%, 5% and 1% levels, respectively.

	DEP: Cumulative Abnormal Return (0,1) to Recommendations					
	Strong Buy/Buy			Hold/Underperform/Sell		
	(1)	(2)	(3)	(4)	(5)	(6)
IRF	0.196***	0.206***	0.212***	0.246**	0.294***	0.290***
	(2.800)	(2.790)	(2.883)	(2.406)	(2.696)	(2.661)
Analyst Experience	0.009	0.015	0.015	-0.017	-0.018	-0.017
	(0.870)	(1.399)	(1.407)	(-1.469)	(-1.467)	(-1.399)
Analyst-Firm Experience	0.071***	0.053***	0.046**	-0.099***	-0.103***	-0.091***
	(3.659)	(2.688)	(2.272)	(-4.217)	(-4.517)	(-4.020)
Analyst Breadth	-0.014***	-0.014***	-0.013***	0.012***	0.012***	0.010***
	(-6.004)	(-5.125)	(-4.991)	(3.584)	(3.302)	(2.997)
Analyst Forecast Frequency	0.081***	0.078**	0.071**	-0.048	-0.074**	-0.070**
	(3.082)	(2.564)	(2.315)	(-1.546)	(-2.167)	(-2.026)
All Star	0.248***	0.177*	0.165	-0.360***	-0.366***	-0.351**
	(2.923)	(1.671)	(1.555)	(-2.639)	(-2.689)	(-2.572)
Brokerage Size	0.005***	0.005***	0.005***	-0.008***	-0.006***	-0.007***
	(5.329)	(4.293)	(4.484)	(-6.204)	(-5.104)	(-5.270)
Brokerage Pressure	-0.003	-0.001	-0.000	-0.009*	-0.008*	-0.009*
	(-0.419)	(-0.115)	(-0.035)	(-1.818)	(-1.672)	(-1.868)
Brokerage Reputation	1.240	2.814**	3.151**	6.549***	7.234***	6.833***
	(1.051)	(2.293)	(2.561)	(4.280)	(4.507)	(4.330)
Brokerage Age	0.059***	0.056***	0.057***	-0.025***	-0.030***	-0.032***
	(6.911)	(6.370)	(6.482)	(-2.907)	(-3.446)	(-3.600)
Brokerage Breadth	-0.001***	-0.001***	-0.001***	0.001***	0.001***	0.001***
	(-3.551)	(-2.889)	(-3.106)	(3.852)	(3.304)	(3.444)

Table 3: (Cont.)Market Reaction to Analyst Recommendation

	(1)	(2)	(3)	(4)	(5)	(6)
Ln(Market Capitalization)		-0.565***	-0.604***		-0.485***	-0.467***
		(-5.421)	(-5.964)		(-2.952)	(-2.945)
Analyst Following		-0.026*	-0.026*		-0.101***	-0.099***
		(-1.730)	(-1.698)		(-2.919)	(-2.930)
Leverage		-0.123	-0.123		-1.878	-1.860
		(-0.323)	(-0.323)		(-1.469)	(-1.471)
CAR (-22,-2)			-1.700**			0.650
			(-2.563)			(0.648)
Friday			0.049			0.065
			(0.589)			(0.474)
Proximity 5 days			0.128			0.214
			(1.117)			(1.414)
Proximity 3 days			0.229*			-1.056***
			(1.649)			(-5.039)
Year FE	YES	YES	YES	YES	YES	YES
Firm FE	YES	YES	YES	YES	YES	YES
Observations	44,367	33,746	33,746	47,466	38,014	38,014
R-squared	0.107	0.122	0.124	0.138	0.154	0.157

Table 4: Optimism of Analyst Forecasts

In Table 4, we examine the differences in relative forecast optimism across brokerage types. The dependent variables are the relative forecast optimism score in columns (1)-(3) or the relative forecast pessimism score in columns (4)-(6). The score is constructed according to Hong and Stein (2003) by ranking the forecasts covering the same firm-period. We include horizon, which measures the number of the days between the forecast issuance date and the date of the actual earnings announcement, as a control variable. All other independent variables, control variables and fixed effects are the same as in Table 2. Each column represents a separate regression. *, **, and *** represent significance at the 10%, 5% and 1% levels, respectively.

	DEP: Relative Forecast Score (compared to forecasts issued in the last 12 months)					
	Relative Forecast Optimism Score			Relative Forecast Pessimism Score		
	(1)	(2)	(3)	(4)	(5)	(6)
IRF	-0.825**	-0.818*	-0.857**	0.001	0.027	0.017
	(-2.117)	(-1.903)	(-1.994)	(0.003)	(0.065)	(0.041)
Earning Loss	-0.167	-0.539	0.280	2.555***	2.307***	2.455***
	(-0.214)	(-0.534)	(0.277)	(5.831)	(4.103)	(4.456)
Earning shock	-0.001	0.001	0.004	0.001	0.031***	0.029***
	(-1.532)	(0.127)	(0.365)	(1.324)	(3.085)	(2.982)
Analyst Experience	0.058	0.083*	0.083*	-0.071*	-0.080**	-0.075*
	(1.451)	(1.953)	(1.959)	(-1.911)	(-1.983)	(-1.887)
Analyst-Firm Experience	0.386***	0.358***	0.418***	-0.016	-0.016	0.063
	(4.684)	(4.083)	(4.760)	(-0.214)	(-0.201)	(0.778)
Analyst Breadth	-0.005	0.003	0.004	0.004	0.000	-0.001
	(-0.287)	(0.168)	(0.218)	(0.260)	(0.023)	(-0.052)
Analyst Forecast Frequency	0.391**	0.204	0.266	0.261*	0.225	0.344**
	(2.478)	(1.152)	(1.503)	(1.910)	(1.464)	(2.261)
Brokerage Size	0.003	0.004	0.004	0.009*	0.009*	0.007
	(0.632)	(0.770)	(0.779)	(1.884)	(1.848)	(1.480)
Brokerage Pressure	0.104	0.081	0.076	0.020	0.038	0.036
	(1.542)	(0.989)	(0.930)	(0.470)	(0.886)	(0.814)
Brokerage Reputation	-13.220**	-8.540	-9.618	1.611	2.029	1.896
	(-2.394)	(-1.418)	(-1.600)	(0.308)	(0.350)	(0.330)
Brokerage Age	-0.064*	-0.064*	-0.061	0.012	0.032	0.048
	(-1.772)	(-1.682)	(-1.601)	(0.357)	(0.881)	(1.313)
Brokerage Breadth	0.000	0.000	0.000	-0.001	-0.001	-0.001
	(0.399)	(0.123)	(0.105)	(-1.323)	(-1.396)	(-0.955)

Table 4: (Cont.)Optimism of Analyst Forecasts

	(1)	(2)	(3)	(4)	(5)	(6)
Ln(Market Capitalization)		-0.734**	-0.471		1.386***	1.036***
		(-2.107)	(-1.351)		(4.115)	(3.106)
Analyst Following		0.053	0.073		0.084*	0.146***
		(1.057)	(1.452)		(1.680)	(2.911)
Leverage		0.370	0.756		0.214	0.115
		(0.153)	(0.314)		(0.102)	(0.055)
Friday			0.099			0.135
			(0.282)			(0.421)
Horizon			-0.020***			-0.046***
			(-12.765)			(-32.403)
Year FE	YES	YES	YES	YES	YES	YES
Firm FE	YES	YES	YES	YES	YES	YES
Observations	68,543	57,054	57,054	81,040	66,665	66,665
R-squared	0.002	0.002	0.005	0.002	0.002	0.019

Table 5: Analysts Forecast Accuracy

In Table 5, we investigate whether IRF analysts issue more accurate forecasts to firms in the financial sector. The dependent variables are the positive standardized forecast error in columns (1)-(3) or the negative standardized forecast error in columns (4)-(6). To construct the dependent variable, we measure the difference between the forecast estimate and the actual earnings per share number, and scale this number using the beginning of quarter stock price. We remove outliers which fall into the top and bottom 1% of price-scaled forecast errors. All independent variables, control variables and fixed effects are the same as in Table 4. Each column represents a separate regression. *, **, and *** represent significance at the 10%,5% and 1% levels, respectively.

	DEP: Standardized Forecast Error (scaled by beginning of quarter stock price)					
	Positive Standardized Forecast Error Forecast >Actual			Negative Standardized Forecast Error Forecast <Actual		
	(1)	(2)	(3)	(4)	(5)	(6)
IRF	-2.227	-0.488**	-0.468**	0.188	0.028	0.025
	(-1.451)	(-2.503)	(-2.406)	(1.500)	(0.540)	(0.482)
Earning Loss	37.210***	7.053***	6.848***	-1.443	-0.145	-0.302
	(15.112)	(3.661)	(3.539)	(-1.488)	(-0.269)	(-0.558)
Earning shock	0.084***	0.423***	0.423***	-0.111***	-0.139***	-0.138***
	(3.295)	(5.040)	(5.036)	(-7.265)	(-5.713)	(-5.716)
Analyst Experience	-0.230	-0.007	-0.010	0.010	0.006	0.007
	(-0.800)	(-0.554)	(-0.860)	(0.661)	(1.337)	(1.426)
Analyst-Firm Experience	1.529***	0.157***	0.121***	-0.081*	-0.025*	-0.013
	(3.575)	(3.998)	(3.025)	(-1.837)	(-1.919)	(-0.975)
Analyst Breadth	0.153***	0.022***	0.022***	0.004	-0.000	-0.000
	(2.630)	(3.521)	(3.548)	(1.207)	(-0.048)	(-0.007)
Analyst Forecast Frequency	-2.300***	0.029	0.009	-0.097	-0.006	0.002
	(-4.433)	(0.554)	(0.176)	(-1.502)	(-0.284)	(0.084)
Brokerage Size	0.011	-0.002	-0.002	0.004***	0.000	0.000
	(0.728)	(-1.209)	(-0.965)	(3.188)	(0.542)	(0.438)
Brokerage Pressure	-0.061*	0.003	0.004	-0.022	-0.003	-0.003
	(-1.795)	(0.258)	(0.332)	(-1.483)	(-0.820)	(-0.863)
Brokerage Reputation	-32.746**	-3.204	-2.581	3.490**	1.193*	1.079
	(-2.170)	(-1.562)	(-1.261)	(2.041)	(1.681)	(1.528)
Brokerage Age	0.189	0.002	-0.003	-0.016	0.001	0.003
	(0.965)	(0.142)	(-0.279)	(-1.293)	(0.185)	(0.436)
Brokerage Breadth	-0.004	0.001*	0.000	-0.000*	0.000	0.000
	(-1.172)	(1.719)	(1.444)	(-1.730)	(0.124)	(0.338)

Table 5: (Cont.)Analysts Forecast Accuracy

	(1)	(2)	(3)	(4)	(5)	(6)
Ln(Market Capitalization)		-0.109	-0.125		0.736***	0.728***
		(-0.221)	(-0.254)		(6.591)	(6.557)
Analyst Following		-0.143***	-0.143***		0.029***	0.028***
		(-6.213)	(-6.253)		(3.928)	(3.693)
Leverage		7.586***	7.552***		-0.180	-0.155
		(5.875)	(5.868)		(-0.771)	(-0.666)
Friday			-0.162**			0.023
			(-1.998)			(0.478)
Horizon			0.010***			-0.004***
			(21.125)			(-25.033)
Year FE	YES	YES	YES	YES	YES	YES
Firm FE	YES	YES	YES	YES	YES	YES
Observations	96,929	78,540	78,540	88,456	74,305	74,305
R-squared	0.350	0.645	0.647	0.643	0.499	0.503

Table 6: Heterogeneity: Optimism of Analyst Recommendations

Table 6 provides logit regression results testing whether the optimism of recommendations varies by the type of firm covered. The regression sample is made of all recommendations issued to investment banks for the sample period from January 1994 to December 2013. The dependent variables are indicators for hold/ underperform/ sell recommendations in columns (1)-(4). The independent variables include an indicator variable which equals one if the recommendation is issued by an investment bank analyst, an indicator variable which equals one if the firm being covered is a bulge bracket, and an interaction variable between the two indicator variables. We use the reputation variable, which measure the firm's share in equity underwriting market, which comes from SDC, to distinguish between bulge brackets and other firms in the financial sector. Firms whose amount of equity underwriting business compromises of more than 10% of the market in a given year is classified as a bulge bracket. We supplement our SDC data with data from CRSP and Compustat. Firms in the 99% percentile of market capitalization are reclassified as bulge brackets. Year fixed effects are included in every column, while issuing bank fixed effects are included in columns (3) and (4). All other independent variables, and control variables are the same as in Table 2. Each column represents a separate regression. Marginal effects are displayed. *, **, and *** represent significance at the 10%,5% and 1% levels, respectively.

	DEP: Indicator variable for Hold/Underperform/Sell			
	(1)	(2)	(3)	(4)
Bulge Bracket being covered * IB analyst	-0.069**	-0.074**	-0.078*	-0.078*
	(-2.363)	(-2.537)	(-1.889)	(-1.885)
Bulge Bracket being covered	-0.019	-0.017	-0.011	-0.011
	(-0.532)	(-0.477)	(-0.542)	(-0.544)
IB analyst	-0.063**	-0.022		
	(-2.376)	(-1.149)		
Brokerage Reputation	0.793**		0.119	
	(2.369)		(0.199)	
Analyst Experience	-0.010***	-0.010***	-0.008**	-0.008**
	(-3.987)	(-3.903)	(-2.272)	(-2.268)
Analyst-Firm Experience	0.014**	0.013**	0.006	0.006
	(2.055)	(2.059)	(1.059)	(1.061)
Analyst Breadth	-0.001	-0.001	0.000	0.000
	(-0.670)	(-0.719)	(0.417)	(0.416)
Analyst Forecast Frequency	0.026**	0.025**	0.022**	0.022**
	(2.490)	(2.462)	(2.113)	(2.119)
Allstar	-0.012	-0.010	-0.012	-0.012
	(-0.403)	(-0.320)	(-0.385)	(-0.373)
Brokerage Size	0.000	0.000	0.001	0.001
	(1.312)	(1.356)	(1.407)	(1.416)
Brokerage Pressure	0.008	0.008	0.002	0.002
	(1.079)	(0.967)	(0.536)	(0.547)

Table 6: (Cont.)Heterogeneity: Optimism of Analyst Recommendations

	(1)	(2)	(3)	(4)
Brokerage Age	0.005***	0.005***	0.034**	0.035***
	(2.693)	(2.898)	(2.527)	(2.930)
Brokerage Breadth	-0.000**	-0.000**	-0.000	-0.000
	(-2.295)	(-1.997)	(-0.914)	(-0.916)
Analyst Following	-0.003	-0.003	-0.003	-0.003
	(-1.427)	(-1.488)	(-1.486)	(-1.487)
Leverage	0.259**	0.261**	0.195***	0.195***
	(2.137)	(2.151)	(2.645)	(2.667)
CAR (-22,-2)	0.137	0.135	0.093	0.093
	(1.576)	(1.566)	(1.198)	(1.200)
Friday	-0.040**	-0.041**	-0.038*	-0.038*
	(-2.387)	(-2.417)	(-1.924)	(-1.915)
Proximity 5 days	-0.036	-0.037	-0.037	-0.037
	(-0.622)	(-0.627)	(-0.746)	(-0.747)
Proximity 3 days	0.056	0.055	0.065	0.065
	(0.864)	(0.847)	(1.244)	(1.245)
Year FE	YES	YES	YES	YES
Issuing Bank FE			YES	YES
Observations	3,579	3,579	3,498	3,498

Table 7: Heterogeneity: Market Reaction to Analyst Recommendation

Table 7 provides results testing whether the informativeness of recommendations varies by the type of firm covered. The regression sample is made of all recommendations issued to investment banks for the sample period from January 1994 to December 2013. The dependent variables are cumulative abnormal return (CAR) in a two day window surrounding the issuance of strong buy/buy recommendations in columns (1)-(4) or hold/ underperform/ sell recommendations in columns (5)-(8), starting on the day of the issuance. The CAR is adjusted using the Fama-French 3-factor model. All independent variables, control variables, and fixed effects are the same as in Table 6. Each column represents a separate regression. *, **, and *** represent significance at the 10%, 5% and 1% levels, respectively.

	DEP: Cumulative Abnormal Return (0,1) to							
	Strong Buy/Buy				Hold/Underperform/Sell			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Bulge Bracket being covered * IB analyst	-0.134	-0.134	-0.319	-0.316	0.089	0.043	-0.355	-0.355
	(-0.350)	(-0.350)	(-0.804)	(-0.795)	(0.212)	(0.103)	(-0.798)	(-0.800)
Bulge Bracket being covered	0.089	0.089	0.150	0.161	0.122	0.131	0.348	0.348
	(0.331)	(0.327)	(0.523)	(0.563)	(0.455)	(0.488)	(1.172)	(1.172)
IB analyst	-0.317	-0.316			-0.261	0.006		
	(-0.964)	(-1.011)			(-0.907)	(0.028)		
Brokerage Reputation	0.008		-25.464**		4.800		0.940	
	(0.002)		(-2.135)		(1.337)		(0.106)	
Analyst Experience	0.089***	0.089***	0.085*	0.082*	0.013	0.014	0.008	0.008
	(2.812)	(2.818)	(1.859)	(1.769)	(0.416)	(0.476)	(0.202)	(0.204)
Analyst-Firm Experience	-0.043	-0.043	-0.024	-0.032	-0.085*	-0.091*	-0.090	-0.090
	(-0.614)	(-0.617)	(-0.292)	(-0.397)	(-1.694)	(-1.826)	(-1.431)	(-1.431)
Analyst Breadth	-0.019**	-0.019**	-0.026**	-0.026**	0.007	0.007	0.008	0.008
	(-2.048)	(-2.067)	(-2.157)	(-2.139)	(0.811)	(0.748)	(0.669)	(0.667)
Analyst Forecast Frequency	0.150	0.150	0.165	0.174	-0.035	-0.037	-0.015	-0.015
	(1.545)	(1.521)	(1.498)	(1.569)	(-0.356)	(-0.374)	(-0.139)	(-0.141)
Allstar	0.770**	0.770**	1.047**	0.953*	-0.462	-0.470	-0.314	-0.312
	(2.075)	(2.150)	(2.091)	(1.946)	(-1.590)	(-1.614)	(-0.865)	(-0.867)
Brokerage Size	0.005	0.005	-0.002	-0.005	-0.004	-0.004	-0.001	-0.001
	(1.515)	(1.524)	(-0.250)	(-0.689)	(-1.243)	(-1.218)	(-0.200)	(-0.199)
Brokerage Pressure	0.128**	0.128*	0.129*	0.104	-0.001	-0.004	0.009	0.009
	(1.966)	(1.942)	(1.762)	(1.374)	(-0.010)	(-0.070)	(0.120)	(0.127)
Brokerage Age	-0.018	-0.018	0.707	0.405	-0.016	-0.012	0.018	0.029
	(-0.657)	(-0.650)	(1.442)	(1.521)	(-0.697)	(-0.531)	(0.079)	(0.143)
Brokerage Breadth	-0.000	-0.000	-0.001	-0.001	0.000	0.000	0.000	0.000
	(-0.772)	(-0.816)	(-0.490)	(-0.353)	(0.335)	(0.514)	(0.307)	(0.307)

Table 7: (Cont.)Heterogeneity: Market Reaction to Analyst Recommendation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Analyst Following	-0.039*	-0.039*	-0.025	-0.023	-0.027	-0.027	-0.017	-0.017
	(-1.881)	(-1.865)	(-1.133)	(-1.021)	(-1.193)	(-1.195)	(-0.648)	(-0.649)
Leverage	-0.075	-0.075	0.297	0.236	-0.237	-0.223	0.013	0.018
	(-0.106)	(-0.106)	(0.369)	(0.294)	(-0.391)	(-0.369)	(0.020)	(0.027)
CAR (-22,-2)	0.124	0.124	0.021	0.015	-1.097	-1.119	-1.003	-1.002
	(0.068)	(0.069)	(0.010)	(0.007)	(-0.834)	(-0.849)	(-0.757)	(-0.756)
Friday	-0.203	-0.203	-0.212	-0.213	0.107	0.103	0.025	0.025
	(-0.894)	(-0.896)	(-0.877)	(-0.881)	(0.490)	(0.467)	(0.102)	(0.101)
Proximity 5 days	0.383	0.383	0.239	0.231	-0.170	-0.165	0.057	0.057
	(0.808)	(0.811)	(0.457)	(0.441)	(-0.341)	(-0.332)	(0.114)	(0.113)
Proximity 3 days	-0.865	-0.865	-0.704	-0.699	0.094	0.084	-0.141	-0.140
	(-1.599)	(-1.600)	(-1.204)	(-1.191)	(0.167)	(0.150)	(-0.245)	(-0.244)
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Issuing Bank FE			YES	YES			YES	YES
Observations	1,739	1,739	1,739	1,739	1,840	1,840	1,840	1,840
R-squared	0.062	0.062	0.132	0.130	0.035	0.034	0.112	0.112

Table 8: Heterogeneity: Optimism of Analyst Forecasts

In table 8, we examine differences in relative forecast optimism across types of firm covered. The regression sample is made of all forecasts issued to investment banks for the sample period from January 1994 to December 2013. The dependent variables is the relative forecast optimism score in columns (1)-(4) or the relative forecast pessimism score in columns (5)-(8). The independent variables include an indicator variable which equals one if the recommendation is issued by an investment bank analyst, an indicator variable which equals one if the firm being covered is a bulge bracket, and an interaction variable between the two indicator variables. All other control variables are the same as in Table 4. Year fixed effects are included in every column, while issuing bank fixed effects are included in columns (5)-(8). Each column represents a separate regression. *, **, and *** represent significance at the 10%, 5% and 1% levels, respectively.

	DEP: Relative Forecast Score (compared to forecasts issued in the last 12 months)							
	Relative Forecast Optimism Score				Relative Forecast Pessimism Score			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Bulge Bracket being covered * IB analyst	0.305	0.262	-0.624	-0.676	2.940	2.793	3.024	2.861
	(0.155)	(0.134)	(-0.306)	(-0.332)	(1.534)	(1.458)	(1.495)	(1.419)
Bulge Bracket being covered	-1.018	-1.002	-0.484	-0.552	-1.175	-1.106	-2.411*	-2.411*
	(-0.732)	(-0.721)	(-0.338)	(-0.385)	(-0.911)	(-0.858)	(-1.825)	(-1.825)
IB analyst	0.055	0.410			-1.006	1.040		
	(0.032)	(0.325)			(-0.625)	(0.891)		
Brokerage Reputation	6.613		84.335		36.895*		82.977	
	(0.323)		(1.504)		(1.863)		(1.497)	
Earning Loss	1.117	1.109	2.124	2.057	-0.203	-0.115	-0.208	-0.161
	(0.329)	(0.326)	(0.606)	(0.588)	(-0.105)	(-0.059)	(-0.107)	(-0.083)
Earning shock	-0.109	-0.107	-0.082	-0.088	0.150	0.152	0.145	0.140
	(-0.965)	(-0.952)	(-0.700)	(-0.748)	(1.518)	(1.535)	(1.438)	(1.382)
Analyst Experience	0.008	0.008	0.095	0.107	0.139	0.123	0.107	0.116
	(0.058)	(0.053)	(0.560)	(0.631)	(1.008)	(0.893)	(0.643)	(0.696)
Analyst-Firm Experience	0.206	0.209	0.173	0.200	-0.186	-0.173	-0.132	-0.117
	(0.815)	(0.826)	(0.573)	(0.662)	(-0.798)	(-0.745)	(-0.490)	(-0.435)
Analyst Breadth	0.070	0.069	0.059	0.053	0.066	0.062	0.102	0.096
	(1.109)	(1.100)	(0.766)	(0.699)	(1.182)	(1.109)	(1.423)	(1.348)
Analyst Forecast Frequency	1.236*	1.228*	0.780	0.722	0.991**	0.981**	0.733	0.696
	(1.960)	(1.951)	(1.157)	(1.074)	(2.002)	(1.983)	(1.326)	(1.259)
Brokerage Size	-0.011	-0.011	-0.035	-0.026	-0.009	-0.010	0.028	0.036
	(-0.638)	(-0.636)	(-0.904)	(-0.673)	(-0.615)	(-0.650)	(0.856)	(1.091)
Brokerage Pressure	0.693*	0.685*	0.542	0.600	-0.737**	-0.735**	-0.620*	-0.575
	(1.732)	(1.713)	(1.252)	(1.395)	(-2.319)	(-2.306)	(-1.679)	(-1.562)
Brokerage Age	0.123	0.131	0.965	2.068	-0.132	-0.097	-0.131	1.378
	(0.975)	(1.057)	(0.117)	(0.273)	(-1.128)	(-0.838)	(-0.028)	(0.283)
Brokerage Breadth	-0.001	-0.001	0.001	0.001	-0.003	-0.002	-0.006	-0.006
	(-0.402)	(-0.350)	(0.261)	(0.168)	(-0.997)	(-0.733)	(-1.439)	(-1.545)

Table 8: (Cont.)Heterogeneity: Optimism of Analyst Forecasts

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Analyst Following	0.022	0.022	0.025	0.025	0.090	0.089	0.164	0.160
	(0.213)	(0.216)	(0.229)	(0.228)	(0.941)	(0.926)	(1.608)	(1.577)
Leverage	-0.745	-0.722	0.663	1.094	3.125	3.380	1.216	1.465
	(-0.131)	(-0.127)	(0.113)	(0.186)	(0.613)	(0.664)	(0.217)	(0.262)
Friday	-0.918	-0.918	-1.086	-1.067	-0.094	-0.102	-0.320	-0.361
	(-0.751)	(-0.751)	(-0.882)	(-0.868)	(-0.081)	(-0.088)	(-0.273)	(-0.309)
Horizon	-0.035***	-0.035***	-0.037***	-0.037***	-0.080***	-0.080***	-0.082***	-0.082***
	(-6.921)	(-6.933)	(-7.326)	(-7.341)	(-17.365)	(-17.345)	(-17.679)	(-17.652)
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Issuing Bank FE			YES	YES			YES	YES
Observations	4,557	4,557	4,557	4,557	4,928	4,928	4,928	4,928
R-squared	0.014	0.014	0.065	0.065	0.066	0.066	0.098	0.098

Table 9: Heterogeneity: Analysts Forecast Accuracy

In table 9, we investigate whether bulge brackets receive less accurate forecasts. The regression sample is made of all forecasts issued to investment banks for the sample period from January 1994 to December 2013. The dependent variables are the positive standardized forecast error in columns (1)-(4) or the negative standardized forecast error in columns (5)-(8). All independent variables, control variables and fixed effects are the same as in Table 8. Each column represents a separate regression. *, **, and *** represent significance at the 10%, 5% and 1% levels, respectively.

	DEP: Standardized Forecast Error(scaled by beginning of quarter stock price)							
	Positive Standardized Forecast Error Forecast >Actual				Negative Standardized Forecast Error Forecast <Actual			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Bulge Bracket being covered * IB analyst	0.292**	0.306**	0.335**	0.346**	-0.058	-0.056	-0.108	-0.103
	(2.232)	(2.320)	(2.408)	(2.511)	(-0.805)	(-0.765)	(-1.335)	(-1.264)
Bulge Bracket being covered	-0.600***	-0.605***	-0.657***	-0.655***	0.140***	0.139***	0.178***	0.181***
	(-5.977)	(-5.996)	(-6.736)	(-6.716)	(2.626)	(2.604)	(3.070)	(3.120)
IB analyst	-0.147	-0.274***			0.217***	0.194***		
	(-1.369)	(-2.586)			(3.246)	(3.157)		
Brokerage Reputation	-2.348*		-5.537		-0.412		-7.897***	
	(-1.743)		(-1.327)		(-0.521)		(-2.641)	
Earning Loss	3.843***	3.846***	3.593***	3.599***	1.366***	1.365***	1.513***	1.500***
	(11.370)	(11.386)	(11.386)	(11.400)	(4.162)	(4.162)	(4.558)	(4.533)
Earning shock	0.370***	0.369***	0.370***	0.370***	-0.191***	-0.191***	-0.192***	-0.191***
	(15.386)	(15.369)	(15.408)	(15.409)	(-16.039)	(-16.039)	(-15.821)	(-15.767)
Analyst Experience	-0.020	-0.018	-0.022	-0.022	0.015**	0.015**	0.027***	0.025***
	(-1.372)	(-1.298)	(-1.255)	(-1.248)	(1.998)	(1.993)	(3.043)	(2.863)
Analyst-Firm Experience	-0.014	-0.015	-0.026	-0.028	-0.008	-0.008	-0.004	-0.006
	(-0.639)	(-0.708)	(-1.112)	(-1.174)	(-0.681)	(-0.678)	(-0.242)	(-0.413)
Analyst Breadth	-0.002	-0.001	0.000	0.001	0.007***	0.007***	0.010***	0.011***
	(-0.395)	(-0.329)	(0.031)	(0.116)	(2.864)	(2.881)	(3.140)	(3.265)
Analyst Forecast Frequency	0.253***	0.255***	0.210***	0.214***	0.056*	0.056*	0.083***	0.086***
	(4.014)	(4.043)	(3.361)	(3.415)	(1.930)	(1.947)	(2.770)	(2.841)
Brokerage Size	0.004***	0.004***	-0.001	-0.002	-0.001	-0.001	-0.001	-0.002
	(2.799)	(2.811)	(-0.442)	(-0.643)	(-1.236)	(-1.224)	(-0.640)	(-1.126)
Brokerage Pressure	0.039***	0.039***	0.011	0.009	0.021**	0.021**	0.029**	0.022
	(2.604)	(2.682)	(0.563)	(0.445)	(2.088)	(2.147)	(2.089)	(1.565)
Brokerage Age	-0.024**	-0.027**	0.106	0.107	0.012*	0.011*	0.479***	0.285*
	(-2.004)	(-2.297)	(0.701)	(0.708)	(1.782)	(1.743)	(2.677)	(1.755)
Brokerage Breadth	-0.001***	-0.001***	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000
	(-2.874)	(-3.119)	(-0.606)	(-0.515)	(-0.193)	(-0.285)	(-0.426)	(-0.243)

Table 9: (Cont.)Heterogeneity: Analysts Forecast Accuracy

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Analyst Following	0.013	0.013	0.019*	0.020*	-0.012*	-0.012*	-0.017***	-0.017***
	(0.993)	(0.991)	(1.692)	(1.701)	(-1.912)	(-1.912)	(-2.670)	(-2.677)
Leverage	0.290	0.284	-0.315	-0.318	-0.656***	-0.659***	-0.689***	-0.744***
	(0.799)	(0.784)	(-0.732)	(-0.738)	(-2.744)	(-2.760)	(-2.769)	(-2.967)
Friday	0.132	0.132	0.077	0.077	0.021	0.021	0.024	0.027
	(1.171)	(1.173)	(0.788)	(0.795)	(0.400)	(0.406)	(0.456)	(0.508)
Horizon	0.008***	0.008***	0.009***	0.009***	-0.004***	-0.004***	-0.004***	-0.004***
	(21.034)	(21.060)	(21.384)	(21.419)	(-18.390)	(-18.405)	(-18.357)	(-18.338)
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Issuing Bank FE			YES	YES			YES	YES
Observations	5,525	5,525	5,525	5,525	4,966	4,966	4,966	4,966
R-squared	0.561	0.561	0.596	0.596	0.413	0.413	0.452	0.451

Table 10: Optimism of Target Price Estimates

Table 10 provides regression results testing whether the optimism of target price estimates vary by brokerage type. The dependent variables (TP/P) are the ratios of the announced 12 month target price (TP) to the stock price outstanding one day prior to the announcement (P). The independent variables include IRF indicator, analyst general experience, analyst-firm specific experience, number of stocks covered by the analyst (analyst breadth), analyst forecast frequency, analyst all-star indicator, brokerage size, brokerage pressure, brokerage reputation, brokerage age, number of stocks covered by the brokerage (brokerage breadth), log(market capitalization of the firm being covered), number of analysts following the firm, firm leverage, indictor for Friday announcements, proximity indictor which equals one if time between the date of price target issuance and the date of earnings announcement is less than five-days (three-days). Firm and year fixed effects are included. Each column represents a separate regression. *, **, and *** represent significance at the 10%, 5% and 1% levels, respectively.

	DEP: TP/P Ratio		
	(1)	(2)	(3)
IRF	-0.020**	-0.005*	-0.005*
	(-2.331)	(-1.876)	(-1.913)
Analyst Experience	0.003***	0.003***	0.003***
	(2.794)	(10.052)	(10.092)
Analyst-Firm Experience	0.008***	-0.001	-0.001
	(3.815)	(-1.179)	(-1.061)
Analyst Breadth	-0.000	-0.000	-0.000
	(-0.588)	(-1.261)	(-1.155)
Analyst Forecast Frequency	-0.022***	-0.009***	-0.009***
	(-7.034)	(-9.191)	(-9.267)
All Star	0.023*	0.004	0.004
	(1.813)	(0.946)	(0.962)
Brokerage Size	-0.001***	-0.000***	-0.000***
	(-4.731)	(-8.058)	(-8.106)
Brokerage Pressure	0.001**	-0.000	-0.000
	(1.964)	(-1.089)	(-1.181)
Brokerage Reputation	-0.489***	-0.178***	-0.182***
	(-3.876)	(-4.601)	(-4.721)
Brokerage Age	-0.000	0.000	0.000
	(-0.486)	(0.879)	(0.977)
Brokerage Breadth	0.000***	0.000***	0.000***
	(3.675)	(5.350)	(5.303)

Table 10: (Cont.)Optimism of Target Price Estimates

	(1)	(2)	(3)
Ln(Market Capitalization)		0.010***	0.010***
		(3.213)	(3.218)
Analyst Following		0.002***	0.002***
		(4.529)	(4.548)
Leverage		0.048***	0.048***
		(3.124)	(3.121)
Friday			0.004**
			(1.967)
Proximity 5 days			-0.011***
			(-2.852)
Proximity 3 days			0.002
			(0.432)
Year FE	YES	YES	YES
Firm FE	YES	YES	YES
Observations	109,647	91,913	91,913
R-squared	0.765	0.454	0.454

Table 11: Target Price Accuracy

In Table 11, we investigate whether IRF analysts issue more accurate target price estimates to firms in the financial sector. The dependent variables are the positive standardized target price error in columns (1)-(3) or the negative standardized target price in columns (4)-(6). To construct the dependent variable ((TP-P12)/P), we measure the difference between the 12 month target price estimate(TP) and the actual stock price 12 months following the target price announcement date (P12), and scale this number using the stock price outstanding one day prior to the announcement (P). We remove outliers which fall into the top and bottom 1% of price-scaled target price errors. All independent variables, control variables and fixed effects are the same as in Table 10. Each column represents a separate regression. *, **, and *** represent significance at the 10%, 5% and 1% levels, respectively.

	DEP: Standardized Target Price Error (scaled by the stock price outstanding)					
	Positive Standardized Target Price Error TP >P12			Negative Standardized Target Price Error TP<P12		
	(1)	(2)	(3)	(4)	(5)	(6)
IRF	-0.006	-0.004	-0.004	0.018	0.017***	0.017***
	(-1.259)	(-0.744)	(-0.741)	(1.407)	(2.817)	(2.852)
Analyst Experience	-0.001***	-0.001**	-0.001**	-0.003*	-0.001**	-0.001**
	(-2.746)	(-2.209)	(-2.191)	(-1.780)	(-2.298)	(-2.323)
Analyst-Firm Experience	0.001	0.001	0.001	-0.005	-0.001	-0.001
	(0.922)	(1.037)	(1.067)	(-1.352)	(-1.297)	(-1.342)
Analyst Breadth	-0.000	-0.000	-0.000	0.000	-0.000	-0.000
	(-0.971)	(-1.176)	(-1.183)	(0.294)	(-0.241)	(-0.280)
Analyst Forecast Frequency	0.005**	0.007***	0.007***	0.011*	0.003	0.003
	(2.465)	(2.918)	(2.912)	(1.750)	(1.546)	(1.564)
All Star	-0.014*	-0.005	-0.005	-0.028	-0.020**	-0.020**
	(-1.675)	(-0.609)	(-0.590)	(-0.874)	(-2.142)	(-2.139)
Brokerage Size	0.000	0.000	0.000	0.000**	0.000***	0.000***
	(1.321)	(0.571)	(0.507)	(2.013)	(4.408)	(4.427)
Brokerage Pressure	-0.001**	-0.000	-0.000	0.001	0.001	0.001
	(-2.247)	(-1.169)	(-1.227)	(0.849)	(1.469)	(1.500)
Brokerage Reputation	0.115*	0.095	0.094	0.408**	0.130*	0.132*
	(1.706)	(1.355)	(1.335)	(2.357)	(1.850)	(1.882)
Brokerage Age	0.000	0.000	0.000	-0.000	-0.001**	-0.001***
	(0.564)	(0.555)	(0.595)	(-0.237)	(-2.572)	(-2.617)
Brokerage Breadth	-0.000	-0.000	-0.000	0.000	-0.000	-0.000
	(-1.000)	(-0.198)	(-0.180)	(0.453)	(-1.019)	(-0.983)

Table 11: (Cont.)Target Price Accuracy

	(1)	(2)	(3)	(4)	(5)	(6)
Ln(Market Capitalization)		-0.096***	-0.096***		-0.017	-0.017
		(-6.414)	(-6.419)		(-1.106)	(-1.106)
Analyst Following		0.002	0.002		-0.005***	-0.005***
		(1.004)	(1.006)		(-3.182)	(-3.187)
Leverage		0.081	0.082		-0.018	-0.018
		(1.482)	(1.491)		(-0.294)	(-0.299)
Friday			0.004			-0.007
			(1.002)			(-1.506)
Proximity 5 days			0.009*			0.010
			(1.736)			(1.626)
Proximity 3 days			-0.014**			-0.003
			(-2.548)			(-0.510)
Year FE	YES	YES	YES	YES	YES	YES
Firm FE	YES	YES	YES	YES	YES	YES
Observations	47,576	42,692	42,692	53,375	44,123	44,123
R-squared	0.371	0.395	0.396	0.860	0.505	0.505

Data Appendix For Chapter 1

Variables from the Gallup State of the States poll.

Well-Being measures:

Overall Well-Being The Gallup-Healthways Well-Being Index score is made up of five elements of well-being that are the core components of the best possible life: purpose, social, financial, community and physical
% Feel active and productive The percentage of state residents who report having felt active and productive every day in the last seven days
% Worried about money The percentage of state residents who report having worried about money in the last seven days
% Community recognition The percentage of state residents who report having received recognition for helping to improve their city or area in the past 12 months
% Exercise frequently The percentage of state residents who report exercising for at least 30 minutes three or more days per week

Religion related measures:

% Very religious The percentage of state residents who say religion is important in their lives and say they attend religious services weekly or nearly weekly
% Nonreligious The percentage of state residents who say religion is not important in their lives and say they seldom or never attend religious services

Politics related measures:

% Republican/Lean The percentage of state residents who identify as Republicans or who identify as independents but say they lean Republican
% Democrat/Lean The percentage of state residents who identify as Democrats or who identify as independents but say they lean Democratic
Democratic advantage The difference between the percentage of state residents identifying as Democrats or leaning Democratic and the percentage identifying as Republicans or leaning Republican
% Conservative The percentage of state residents who describe their political views as conservative
% Liberal The percentage of state residents who describe their political views as liberal

Conservative advantage The difference between the percentage of state residents describing their political views as conservative and the percentage describing their political views as liberal

Chapter 1 APPENDIX

Table 1: APPENDIX: Robustness Checks: MSAs on state borders and pharmaceutical stocks

In this table, we present results of additional robustness checks. In Appendix Table 1 column 1, we exclude MSAs on the border of two, three, or four states. In column 2, when generating the state stock index, we exclude pharmaceutical stocks (SICCD code equals to 2834 or NAICS code equals to 325412). All dependent variables, independent variables, controls, and fixed effects are the same as in Table 2 column 4. Each column represents a separate regression. In parentheses are robust standard errors clustered at MSA level. In parentheses are robust standard errors clustered at MSA level. *, **, and *** represent significance at the 10%, 5%, and 1% levels, respectively.

	DEP: Log(Antidepressant with Diagnosis(t))	
	Exclude MSAs on state borders	Exclude pharmaceutical stocks
	(1)	(2)
Market Return (t-1,t)	-0.403**	-0.419**
	(-1.979)	(-2.245)
Month Fixed Effects	YES	YES
Year Fixed Effects	YES	YES
MSA Fixed Effects	YES	YES
Control: Unemployment Rate	YES	YES
Control: Average Weekly Wage	YES	YES
Observations	28,292	32,161
R-squared	0.905	0.912

Table 2: APPENDIX: Nonlinear and Heterogeneous Effects between Different Age Cohorts

In this table, we include non-gatekeeper insurance plan holders in the regression sample. In Appendix Table 2 columns 1-2, the dependent variable is the logarithm of the number of antidepressant prescription to individual between 35 (included) and 44 (included) years of age living in a MSA during week t. The main independent variable is the cumulative two-week return (t-1,t) of a value-weighted index consisting of public companies headquartered in a state. The two-week return (t-1,t) is measured as the percentage change from the closing index Friday two weeks ago to this Fridays closing index. The three-week return (t-2,t) is measured as the percentage change from the closing index Friday three weeks ago to this Fridays closing index. The return is scaled by trailing 1-year standard deviation of the state stock return. In columns 3-4 and columns 5-6, the dependent variable counts the weekly antidepressant usage among those aged 45-55 and 55-65, respectively. Each column represents a separate regression. As independent variables, we interact the market return variable with its positive/negative indicator dummies. In parentheses are robust standard errors clustered at MSA level. In parentheses are robust standard errors clustered at MSA level. *, **, and *** represent significance at the 10%, 5%, and 1% levels, respectively.

	DEP: Log(Antidepressant with Diagnosis(t))					
	Age 35-45		**Age 45-55**		**Age 55-65**	
	(1)	(2)	(3)	(4)	(5)	(6)
Market Return (t-1,t) * Negative return dummy	0.039		-0.526***		-0.368**	
	(0.196)		(-2.805)		(-2.093)	
Market Return (t-1,t) * Positive return dummy	0.157		-0.130		-0.026	
	(0.620)		(-0.539)		(-0.099)	
Market Return (t-2,t) * Negative return dummy		-0.032		-0.049		-0.099**
		(-0.681)		(-1.013)		(-2.242)
Market Return (t-2,t) * Positive return dummy		-0.178		-0.094		-0.048
		(-1.451)		(-0.715)		(-0.361)
Month Fixed Effects	YES	YES	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES	YES	YES
MSA Fixed Effects	YES	YES	YES	YES	YES	YES
Control: Unemployment Rate	YES	YES	YES	YES	YES	YES
Control: Average Weekly Wage	YES	YES	YES	YES	YES	YES
Observations 28,376	28,376	28,947	28,947	27,047	27,047	
R-squared	0.843	0.843	0.872	0.872	0.852	0.852

Table 1: APPENDIX: Optimism of Recommendations around the Settlement

The sample runs from January 2000 to April 2013. The variables are defined as: **Strong buy**: recommendations in the "strong buy" category in IBES database. **Upgrade**: A positive change in the rating of a stock by the analyst. **Above consensus**: an recommendation issued on date t if the recommendation belongs to the top (bottom) quantile of all recommendations issued for the firm k in the prior year, excluding all other recommendations issued on date t and the analyst i's prior recommendations for firm k. **IRF** analysts: are analysts at independent research firms as defined in Nelson's directory. All other analysts are defined as **Non-IRF** analysts. **Pre**(-regulation) is the period between January, 2000 and August, 2002. **Reg**(regulation) is the period between September, 2002 and July, 2009. **Post**(-regulation) is the period between August, 2009 and April, 2013. T-statisticss in brackets below coefficient estimates are based on robust errors. $***$, $**$, and $*$ indicate statistical significance at 1%, 5% and 10%.

Table 1: (Cont.)APPENDIX: Optimism of Recommendations around the Settlement

					Panel A: STRONG BUY			
Type of institutions	**All**	**Pre**	**Reg**	**Post**	**Pre versus Reg**	**T-stat**	**Post versus Reg**	**T-stat**
IRF	0.372	0.495	0.358	0.379	0.138***	(11.64)	0.022***	(3.34)
Non-IRF	0.191	0.266	0.159	0.206	0.106***	(56.78)	0.047***	(26.55)
Observations	312701	59703	181101	71897				
Difference tests								
IRF versus non-IRF:	0.181***	0.230***	0.198***	0.173***				
T-stat	(72.53)	(21.73)	(67.98)	(35.25)				
					Panel B: UPGRADE			
IRF	0.254	0.106	0.281	0.224	-0.175***	(-16.26)	-0.057***	(-9.76)
Non-IRF	0.216	0.144	0.230	0.249	-0.086***	(-44.13)	0.019***	(9.58)
Observations	312701	59703	181101	71897				
Difference tests								
IRF versus non-IRF:	0.037***	-0.038***	0.051***	-0.025***				
T-stat	(14.50)	(-4.56)	(15.64)	(-4.94)				
					Panel C: ABOVE CONSENSUS			
IRF	0.278	0.232	0.288	0.264	-0.055***	(-4.55)	-0.023***	(-3.70)
Non-IRF	0.252	0.255	0.248	0.258	0.006***	(2.86)	0.010***	(4.74)
Observations	312701	59703	181101	71897				
Difference tests								
IRF versus non-IRF:	0.026***	-0.022*	0.039***	0.006				
T-stat	(9.37)	(-1.95)	(11.39)	(1.14)				

Table 2: APPENDIX: Cumulative Abnormal Return to Strong Buy/Buy and Hold/Sell by Different Types of Institutions

This table reports the average cumulative abnormal return(**CAR**) and corresponding t-statistics during the announcement period (-1,0) for for Investment Bank/ Broker (**IBB**), Major Institutional Broker (**MIB**), Major Regional Broker (**MRB**), Small Regional Broker (**SRB**), Management Subsidiary of Broker (**MSB**) and Independent Research Firms (**IRF**). The types of security firms are identified using Nelson's directory. The return is adjusted using Fama-French 3 factors model. Panel A presents CAR for **strong buy/buy** recommendations (upgrades to buy or strong buy, or initiations/resumptions/reiterations with a buy or strong buy rating). Panel B presents CAR for **hold/sell** recommendations (downgrades to hold or sell, or initiations/resumptions/reiterations with a hold or sell rating. Columns 1 report the average (**CAR**) and associated tstatistics, for the entire sample period, while Columns 2-3, 4-5 and 6-7 present the average (**CAR**) and associated tstatistics, for the period prior period between January, 2000 and August, 2002, the period between September, 2002 and July, 2009 and the period between August, 2009 and April, 2013. $***$, $**$, and $*$ indicate statistical significance at 1%, 5% and 10%.

Table 2: (Cont.)APPENDIX: Cumulative Abnormal Return to Strong Buy/Buy and Hold/Sell by Different Types of Institutions

				Panel A: STRONG BUY/BUY			
All	**All**	**Pre**		**Reg**		**Post**	
All	**CAR (%)**	**CAR (%)**	**T-stat**	**CAR (%)**	**T-stat**	**CAR (%)**	**T-stat**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
IBB	0.013	0.002	4.072	0.018	58.134	0.017	43.455
MIB	0.012	0.009	4.298	0.014	8.023	0.011	11.643
MRB	0.015	0.003	0.903	0.023	11.660	0.021	7.728
SRB	0.011	0.006	1.577	0.008	3.153	0.020	6.063
MSB	0.013	0.013					
IRF	0.012	0.002	0.615	0.0120	16.532	0.015	14.598
Observations	144412	37532		75105		31775	
Difference tests							
IRF-IBB		-0.001	(-0.26)	-0.006***	(-6.64)	-0.002*	(-1.89)
IRF-MIB		-0.007**	(-2.12)	-0.002	(-0.99)	0.004***	(2.97)
IRF-MRB		-0.001	(-0.28)	-0.011***	(-5.62)	-0.006*	(-1.88)
IRF-SRB		-0.004	(-0.73)	0.004	(1.60)	-0.006	(-1.57)
				Panel B: HOLD/SELL			
IBB	-0.021	-0.050	-44.265	-0.018	-50.969	-0.013	-21.663
MIB	-0.014	-0.028	-11.384	-0.013	-10.531	-0.008	-4.790
MRB	-0.023	-0.035	-6.223	-0.021	-7.320	-0.013	-2.492
SRB	-0.005	-0.017	-2.593	-0.004	-1.255	-0.003	-0.603
MSB	0.004	0.004					
IRF	-0.013	-0.020	-3.898	-0.013	-14.890	-0.010	-4.698
Observations	156413	21343		101308		33762	
Difference tests							
IRF-IBB		0.030***	(4.85)	0.004***	(4.16)	0.004**	(1.96)
IRF-MIB		0.009*	(1.65)	-0.000	(-0.25)	-0.002	(-0.82)
IRF-MRB		0.015**	(2.04)	0.008***	(2.87)	0.003	(0.55)
IRF-SRB		-0.003	(-0.24)	-0.009**	(-2.57)	-0.006	(-0.90)

Table 3: APPENDIX: Independent Research Providers

Source: Buslepp, W., Casey, R., and G. R. Huston, 2013, "Did they get what they paid for? The Global Analyst Research Settlement and analyst research quality," Working paper, table Appendix B, page 26. Firms in bold are available in I/B/E/S.

Bear Stearns	**BNY Jaywalk**
Credit Suisse First Boston	**Renaissance Capital** **BNY Jaywalk** Standard & Poor's
Goldman Sachs	Standard & Poor's Morningstar **Renaissance Capital**
J.P. Morgan Chase	Morningstar **Renaissance Capital** BOE Securities
Lehman Brothers	**BNY Jaywalk**
Merrill Lynch	Morningstar **BNY Jaywalk**
Morgan Stanley	Alpha Equity Research **Argus Research** **Buckingham Research Group** **Fulcrum Global Partners** IPOfinancial.com **Soleil Securities Group** Standard & Poor's **Zacks Investment Research**
Piper Jaffray	**Buckingham Research Group** Morningstar **Renaissance Capital** Standard & Poor's **Zacks Investment Research**
Smith Barney	**Argus Research** Morningstar **Renaissance Capital** Standard & Poor's Thomson Financial
UBS	**BNY Jaywalk**

Table 4: APPENDIX: GRS Provisions eliminated and remained in place

The sanctioned banks proposed that all provisions of the GRS to be eliminated, but the SEC believed that to retain certain provisions was in the public interest. Other provisions are now imposed by NASD and NYSE rules and are thus eliminated from the GRS. For the provisions eliminated but not covered by the new rules, the SEC and the sanctioned banks stated that elimination of these provisions would be consistent with the public interest. Source: U.S. SECURITIES AND EXCHANGE COMMISSION Litigation Release No. 21457, March 19th, 2010.

GRS provisions remained in place and approved by Judge Pauley:
The physical separation of research analysts and investment banking; A requirement that communications to the sales force be to have a reasonable basis; A requirement that Research analysts be able to express their views to a commitment committee on a proposed transaction outside the presence of investment banks working on the deal; A prohibition on investment banking input into company-specific coverage decisions and into research budget decisions A requirement that the Research oversight committees to ensure the integrity and independence of ratings, targets, and the overall quality of research; A prohibition on Communications between investment banking personnel and research analysts concerning a proposed transaction, unless a chaperone from the firms legal department is present.
GRS provisions no longer in effect:
An obligation that customers be provided with independent third-party research; A prohibitions against investment bankers' influence over research compensation and over evaluations of research personnel; A prohibitions against research participation in efforts to solicit investment banking business; A prohibitions against research participation in road shows; A prohibitions against investment bankers' direction to research to engage in marketing or selling efforts for investment banking deals.

Chapter 3 APPENDIX

Table 1: APPENDIX: Optimism of Analyst Recommendations Issued to Non-Financial Sector Firms

Appendix Table 1 provides logit regression results testing whether the optimism of recommendations varies by brokerage type. The sample includes recommendations issued to firms not in the financial sector in the Institutional Brokerage Estimation System (IBES) database for the sample period from January 1994 to December 2013. The dependent variables are indicator variables for hold/ underperform/ sell recommendations in columns (1)-(3). The independent variables include IRF indicator, analyst general experience, analyst-firm specific experience, number of stocks covered by the analyst (analyst breadth), analyst forecast frequency, analyst all-star indicator, brokerage size, brokerage pressure, brokerage reputation, brokerage age, number of stocks covered by the brokerage (brokerage breadth), log(market capitalization of the firm being covered), number of analysts following the firm, firm leverage, cumulative abnormal return in the time window (-22,-2) before the issuance of recommendations, indictor for Friday announcements, proximity indictor which equals one if time between the date of recommendation issuance and the date of earnings announcement is less than five-days (three-days). Firm and year fixed effects are included. Each column represents a separate regression. Marginal effects are displayed. *, **, and *** represent significance at the 10%, 5% and 1% levels, respectively.

	DEP: Indicator variable for Hold/Underperform/Sell		
	(1)	(2)	(3)
IRF	-0.036***	-0.043***	-0.041***
	(-10.390)	(-9.098)	(-8.449)
Analyst Experience	-0.003***	-0.004***	-0.003***
	(-8.444)	(-6.701)	(-5.541)
Analyst-Firm Experience	0.010***	0.010***	0.009***
	(11.688)	(9.281)	(7.642)
Analyst Breadth	0.002***	0.003***	0.002***
	(15.916)	(11.929)	(10.770)
Analyst Forecast Frequency	0.000	0.001	0.001
	(0.737)	(1.005)	(1.315)
All Star	-0.013***	-0.013***	-0.011**
	(-4.140)	(-2.934)	(-2.443)
Brokerage Size	-0.000	-0.000	-0.000***
	(-1.072)	(-1.037)	(-2.781)
Brokerage Pressure	-0.012***	-0.015***	-0.016***
	(-8.937)	(-8.273)	(-8.084)
Brokerage Reputation	0.456***	0.624***	0.666***
	(12.078)	(11.158)	(11.456)
Brokerage Age	0.003***	0.003***	0.004***
	(6.793)	(5.223)	(6.217)
Brokerage Breadth	-0.000	-0.000	-0.000
	(-1.315)	(-1.284)	(-0.174)

Table 1: (Cont.)APPENDIX: Optimism of Analyst Recommendations Issued to Non-Financial Sector Firms

	(1)	(2)	(3)
Ln(Market Capitalization)		-0.022***	-0.015***
		(-6.089)	(-3.582)
Analyst Following		0.005***	0.004***
		(8.674)	(7.616)
Leverage		-0.022	0.019
		(-1.517)	(1.214)
CAR (-22,-2)			0.027**
			(2.526)
Friday			0.001
			(0.231)
Proximity 5 days			-0.011
			(-1.488)
Proximity 3 days			0.036***
			(4.450)
Year FE	YES	YES	YES
Firm FE	YES	YES	YES
Observations	180,353	134,276	123,613

Table 2: APPENDIX: Market Reaction to Analyst Recommendation Issued to Non-Financial Sector Firms

Appendix Table 2 provides results testing whether informativeness of recommendations vary by brokerage type. The sample includes recommendations issued to firms not in the financial sector in the Institutional Brokerage Estimation System (IBES) database for the sample period from January 1994 to December 2013. The dependent variables are cumulative abnormal return (CAR) in a two day window surrounding the issuance of strong buy/buy recommendations in columns (1)-(3) or hold/ underperform/ sell recommendations in columns (4)-(6), starting on the day of the issuance. The CAR is adjusted using the Fama-French 3-factor model. All independent variables, control variables and fixed effects are the same as in Appendix Table 1. Each column represents a separate regression. *, **, and *** represent significance at the 10%, 5% and 1% levels, respectively.

	DEP: Cumulative Abnormal Return (0,1) to Recommendations					
	Strong Buy/Buy			Hold/Underperform/Sell		
	(1)	(2)	(3)	(4)	(5)	(6)
IRF	-0.251**	-0.171	-0.157	-0.018	-0.207	-0.231
	(-2.086)	(-1.219)	(-1.165)	(-0.128)	(-1.361)	(-1.523)
Analyst Experience	0.008	0.012	0.012	-0.046***	-0.051***	-0.048***
	(0.779)	(1.121)	(1.084)	(-3.855)	(-4.078)	(-3.892)
Analyst-Firm Experience	0.063***	0.073***	0.062***	-0.055**	-0.073***	-0.065***
	(3.161)	(3.389)	(2.812)	(-2.114)	(-2.957)	(-2.633)
Analyst Breadth	-0.013***	-0.015***	-0.014***	0.016***	0.015***	0.013***
	(-3.943)	(-3.715)	(-3.573)	(3.298)	(4.020)	(3.670)
Analyst Forecast Frequency	0.036***	0.034***	0.033***	-0.037	-0.027**	-0.029**
	(4.067)	(3.445)	(3.410)	(-1.364)	(-2.272)	(-2.408)
All Star	0.234***	0.231**	0.219**	-0.397***	-0.254**	-0.251**
	(2.945)	(2.414)	(2.290)	(-3.980)	(-2.219)	(-2.198)
Brokerage Size	0.009***	0.010***	0.009***	-0.012***	-0.013***	-0.012***
	(5.325)	(5.639)	(5.332)	(-6.897)	(-7.101)	(-6.803)
Brokerage Pressure	0.124*	0.131	0.129	-0.069	-0.088	-0.079
	(1.814)	(1.530)	(1.495)	(-0.695)	(-0.872)	(-0.786)
Brokerage Reputation	0.062	1.197	1.233	-0.304	0.938	0.534
	(0.057)	(0.945)	(0.967)	(-0.243)	(0.659)	(0.375)
Brokerage Age	0.039***	0.040***	0.044***	0.013	0.018	0.010
	(3.086)	(2.702)	(2.910)	(0.884)	(1.138)	(0.619)
Brokerage Breadth	-0.001***	-0.001***	-0.001***	0.001***	0.001***	0.001***
	(-5.353)	(-6.460)	(-6.106)	(6.312)	(6.403)	(6.130)

Table 2: (Cont.)APPENDIX: Market Reaction to Analyst Recommendation Issued to Non-Financial Sector Firms

	(1)	(2)	(3)	(4)	(5)	(6)
Ln(Market Capitalization)		-1.264***	-1.321***		-1.331***	-1.288***
		(-11.584)	(-9.228)		(-11.196)	(-10.507)
Analyst Following		0.020	0.022		-0.066***	-0.065***
		(1.383)	(1.472)		(-4.365)	(-4.329)
Leverage		0.711	0.697		-0.852*	-0.847*
		(0.981)	(0.957)		(-1.834)	(-1.831)
CAR (-22,-2)			-1.284			1.138**
			(-0.921)			(1.979)
Friday			-0.279***			-0.085
			(-3.028)			(-0.819)
Proximity 5 days			0.087			0.387**
			(0.644)			(2.308)
Proximity 3 days			0.806***			-1.619***
			(4.420)			(-7.708)
Year FE	YES	YES	YES			
Firm FE	YES	YES	YES			
Observations	80,976	61,614	61,613	82,939	64,944	64,944
R-squared	0.166	0.191	0.193	0.211	0.250	0.253

Table 3: APPENDIX: Optimism of Analyst Forecasts Issued to Non-Financial Sector Firms

In Appendix Table 3, we examine differences in relative forecast optimism across brokerage types. The sample includes earnings forecasts issued to firms not in the financial sector in the Institutional Brokerage Estimation System (IBES) database for the sample period from January 1994 to December 2013. The dependent variables are the relative forecast optimism score in columns (1)-(3) or the relative forecast pessimism score in columns (4)-(6). The score is constructed according to Hong and Stein (2003) by ranking the forecasts covering the same firm-period. We include horizon, which measures the number of the days between the forecast issuance date and the date of the actual earnings announcement, as a control variable. All other independent variables, control variables and fixed effects are the same as in Appendix Table 1. Each column represents a separate regression. *, **, and *** represent significance at the 10%, 5% and 1% levels, respectively.

	DEP: Relative Forecast Score (compared to forecasts issued in the last 12 months)					
	Relative Forecast Optimism Score			Relative Forecast Pessimism Score		
	(1)	(2)	(3)	(4)	(5)	(6)
IRF	-1.738***	-1.740***	-1.680***	-1.922***	-1.763***	-1.709***
	(-4.658)	(-4.225)	(-4.065)	(-5.653)	(-4.543)	(-4.378)
Earning Loss	-0.375	-0.082	0.468	1.031***	0.851**	1.293***
	(-0.873)	(-0.152)	(0.855)	(3.596)	(2.281)	(3.435)
Earning shock	-0.003	-0.005*	-0.005*	0.013***	0.037***	0.035***
	(-1.579)	(-1.676)	(-1.704)	(4.700)	(2.702)	(2.702)
Analyst Experience	-0.102***	-0.097***	-0.095***	0.006	0.020	0.033
	(-3.181)	(-2.758)	(-2.705)	(0.200)	(0.632)	(1.019)
Analyst-Firm Experience	0.302***	0.286***	0.335***	0.120**	0.144**	0.179***
	(4.534)	(3.939)	(4.586)	(2.065)	(2.270)	(2.778)
Analyst Breadth	0.009	0.018	0.016	-0.023**	-0.028**	-0.028**
	(0.758)	(1.268)	(1.146)	(-2.173)	(-2.198)	(-2.163)
Analyst Forecast Frequency	0.117**	0.129**	0.144**	0.225***	0.236***	0.249***
	(2.062)	(2.188)	(2.446)	(4.436)	(4.352)	(4.595)
Brokerage Size	0.015***	0.017***	0.016***	0.015***	0.014***	0.014***
	(3.622)	(3.623)	(3.383)	(3.741)	(2.833)	(2.982)
Brokerage Pressure	0.356***	0.389***	0.394***	0.197**	0.137	0.122
	(4.166)	(4.128)	(4.165)	(1.986)	(1.267)	(1.121)
Brokerage Reputation	5.153	6.276*	6.335*	5.864**	6.846**	6.190*
	(1.566)	(1.682)	(1.687)	(2.000)	(2.029)	(1.827)
Brokerage Age	-0.212***	-0.237***	-0.243***	-0.031	-0.064	-0.066
	(-5.193)	(-5.327)	(-5.422)	(-0.783)	(-1.454)	(-1.494)
Brokerage Breadth	0.000	0.001	0.001	-0.001	0.000	0.000
	(0.506)	(0.843)	(1.059)	(-1.010)	(0.115)	(0.015)

Table 3: (Cont.)APPENDIX: Optimism of Analyst Forecasts Issued to Non-Financial Sector Firms

	(1)	(2)	(3)	(4)	(5)	(6)
Ln(Market Capitalization)		-0.212	0.027		0.070	-0.013
		(-1.164)	(0.145)		(0.380)	(-0.071)
Analyst Following		0.041	0.073***		0.063**	0.091***
		(1.522)	(2.712)		(2.412)	(3.424)
Leverage		0.693	1.046		-0.845	-0.544
		(0.713)	(1.080)		(-0.935)	(-0.586)
Friday			0.125			0.368*
			(0.509)			(1.648)
Horizon			-0.022***			-0.034***
			(-11.430)			(-18.099)
Year FE	YES	YES	YES	YES	YES	YES
Firm FE	YES	YES	YES	YES	YES	YES
Observations	170,773	137,908	137,908	196,231	152,769	152,769
R-squared	0.027	0.029	0.033	0.025	0.028	0.038

Table 4: APPENDIX: Accuracy of Analysts Forecasts Issued to Non-Financial Sector Firms

In Appendix Table 4, we investigate whether IRF analysts issue more or less accurate forecasts to non-financial sector firms. The sample includes earnings forecasts issued to firms not in the financial sector in the Institutional Brokerage Estimation System (IBES) database for the sample period from January 1994 to December 2013. The dependent variables are the positive standardized forecast error in columns (1)-(3) or the negative standardized forecast error in columns (4)-(6). To construct the dependent variable, we measure the difference between the forecast estimate and the actual earnings per share number, and scale this number using the beginning of quarter stock price. We remove outliers which fall into the top and bottom 1% of price-scaled forecast errors. All independent variables, control variables and fixed effects are the same as in Appendix Table 3. Each column represents a separate regression. *, **, and *** represent significance at the 10%, 5% and 1% levels, respectively.

	DEP: Standardized Forecast Error (scaled by beginning of quarter stock price)					
	Positive Standardized Forecast Error Forecast >Actual			Negative Standardized Forecast Error Forecast <Actual		
	(1)	(2)	(3)	(4)	(5)	(6)
IRF	-0.027	0.460***	0.461***	-0.093	-0.098***	-0.096***
	(-0.113)	(2.956)	(2.963)	(-1.335)	(-2.860)	(-2.797)
Earning Loss	-0.943	-1.011	-1.218	-1.284***	0.992***	0.904***
	(-0.669)	(-0.916)	(-1.097)	(-4.617)	(4.314)	(3.930)
Earning shock	0.707***	0.743***	0.744***	-0.095***	-0.239***	-0.238***
	(9.116)	(6.477)	(6.494)	(-7.420)	(-8.336)	(-8.345)
Analyst Experience	0.017	0.007	0.004	0.003	-0.005	-0.004
	(1.002)	(0.836)	(0.448)	(0.496)	(-1.026)	(-0.765)
Analyst-Firm Experience	-0.038	-0.016	-0.038**	-0.013	-0.004	0.005
	(-1.128)	(-0.909)	(-2.137)	(-1.242)	(-0.520)	(0.747)
Analyst Breadth	-0.003	0.003	0.003	0.000	-0.001	-0.002
	(-0.560)	(1.040)	(1.186)	(0.199)	(-1.064)	(-1.243)
Analyst Forecast Frequency	0.174	-0.029	-0.028	-0.033***	0.001	0.004
	(1.229)	(-0.771)	(-0.758)	(-3.598)	(0.224)	(0.744)
Brokerage Size	-0.000	-0.001	-0.000	-0.001*	-0.002***	-0.002***
	(-0.101)	(-0.603)	(-0.399)	(-1.729)	(-3.175)	(-3.237)
Brokerage Pressure	-0.198	0.002	-0.003	0.020	0.007	0.007
	(-1.029)	(0.062)	(-0.083)	(0.726)	(0.410)	(0.446)
Brokerage Reputation	0.874	1.619**	1.593**	0.027	-0.259	-0.312
	(0.531)	(2.330)	(2.296)	(0.045)	(-0.688)	(-0.830)
Brokerage Age	0.004	0.033*	0.034*	-0.009	-0.007	-0.007
	(0.151)	(1.835)	(1.880)	(-1.206)	(-1.506)	(-1.620)
Brokerage Breadth	0.000	-0.000	-0.000	0.000	0.000***	0.000***
	(0.138)	(-0.092)	(-0.264)	(1.155)	(3.810)	(4.101)

Table 4: (Cont.)APPENDIX: Accuracy of Analysts Forecasts Issued to Non-Financial Sector Firms

	(1)	(2)	(3)	(4)	(5)	(6)
Ln(Market Capitalization)		0.626*	0.607*		0.354***	0.380***
		(1.886)	(1.836)		(5.179)	(5.550)
Analyst Following		-0.009	-0.014		0.002	-0.003
		(-0.944)	(-1.445)		(0.526)	(-0.803)
Leverage		-0.798*	-0.925**		-1.736***	-1.692***
		(-1.697)	(-1.968)		(-8.681)	(-8.504)
Friday			0.077			0.036
			(1.524)			(1.225)
Horizon			0.009***			-0.004***
			(18.048)			(-31.683)
Year FE	YES	YES	YES	YES	YES	YES
Firm FE	YES	YES	YES	YES	YES	YES
Observations	220,099	170,330	170,330	215,420	174,467	174,467
R-squared	0.928	0.860	0.861	0.706	0.554	0.557

Table 5: APPENDIX: Optimism of Target Price Estimates - Incremental Information

Appendix Table 5 provides regression results testing whether the optimism of target price estimates vary by brokerage type, conditioning on optimism level of earnings forecasts. The dependent variables (TP/P) are the ratios of the announced 12 month target price (TP) to the stock price outstanding one day prior to the announcement (P). The independent variables include IRF indicator, analyst general experience, analyst-firm specific experience, number of stocks covered by the analyst (analyst breadth), analyst forecast frequency, analyst all-star indicator, brokerage size, brokerage pressure, brokerage reputation, brokerage age, number of stocks covered by the brokerage (brokerage breadth), log(market capitalization of the firm being covered), number of analysts following the firm, firm leverage, indictor for Friday announcements, proximity indictor which equals one if time between the date of price target issuance and the date of earnings announcement is less than five-days (three-days). We control for the Relative Forecast Score of earnings forecast issued up to 90 days before the announcement date of the target price estimates by the analyst to the firm. Firm and year fixed effects are included. Each column represents a separate regression. *, **, and *** represent significance at the 10%, 5% and 1% levels, respectively.

	DEP: TP/P Ratio		
	(1)	(2)	(3)
IRF	-0.021	-0.012**	-0.012**
	(-0.935)	(-2.232)	(-2.254)
Analyst Experience	0.004***	0.002***	0.002***
	(3.011)	(3.909)	(3.921)
Analyst-Firm Experience	0.007	0.000	0.000
	(1.607)	(0.059)	(0.089)
Analyst Breadth	0.000	0.000	0.000
	(0.562)	(0.035)	(0.055)
Analyst Forecast Frequency	-0.022***	-0.008***	-0.008***
	(-3.156)	(-3.777)	(-3.800)
All Star	-0.057	-0.006	-0.007
	(-1.622)	(-0.610)	(-0.623)
Brokerage Size	-0.000	-0.000***	-0.000***
	(-1.398)	(-2.887)	(-2.875)
Brokerage Pressure	-0.001	-0.002	-0.002
	(-0.906)	(-1.433)	(-1.448)
Brokerage Reputation	-0.251	-0.236***	-0.242***
	(-0.967)	(-2.749)	(-2.820)
Brokerage Age	-0.001	0.001	0.001
	(-0.536)	(1.450)	(1.491)
Brokerage Breadth	0.000	0.000**	0.000**
	(0.933)	(2.207)	(2.168)

Table 5: (Cont.)APPENDIX: Optimism of Target Price Estimates - Incremental Information

	(1)	(2)	(3)
Ln(Market Capitalization)		0.022*	0.022*
		(1.807)	(1.806)
Analyst Following		0.002	0.002
		(1.111)	(1.114)
Leverage		0.067	0.067
		(1.374)	(1.374)
Friday			0.004
			(1.210)
Proximity 5 days			-0.008
			(-1.435)
Proximity 3 days			-0.002
			(-0.305)
Relative Forecast Score Control	YES	YES	YES
Year FE	YES	YES	YES
Firm FE	YES	YES	YES
Observations	65,018	55,450	55,450
R-squared	0.756	0.443	0.443

Table 6: APPENDIX: Target Price Accuracy - Incremental Information

In Appendix Table 6, we investigate whether IRF analysts issue more accurate target price estimates to firms in the financial sector, conditioning on accuracy level of earnings forecasts. The dependent variables are the positive standardized target price error in columns (1)-(3) or the negative standardized target price in columns (4)-(6). To construct the dependent variable ((TP-P12)/P), we measure the difference between the 12 months target price estimate (TP) and the actual stock price 12 months following the target price announcement date (P12), and scale this number using the stock price outstanding one day prior to the announcement (P). We remove outliers which fall into the top and bottom 1% of price-scaled target price errors. We control for the Standardized Forecast Error of earnings forecast issued up to 90 days before the announcement date of the target price estimates by the analyst to the firm. All other independent variables, control variables and fixed effects are the same as in Table 10. Each column represents a separate regression. *, **, and *** represent significance at the 10%, 5% and 1% levels, respectively.

	DEP: Standardized Target Price Error (scaled by the stock price outstanding)					
	Positive Standardized Target Price Error TP >P12			Negative Standardized Target Price Error TP<P12		
	(1)	(2)	(3)	(4)	(5)	(6)
IRF	-0.001	-0.001	-0.001	0.028	0.020***	0.020***
	(-0.201)	(-0.136)	(-0.129)	(1.642)	(2.851)	(2.885)
Analyst Experience	-0.001**	-0.001*	-0.001*	-0.004**	-0.001**	-0.001**
	(-2.272)	(-1.780)	(-1.768)	(-2.070)	(-2.335)	(-2.337)
Analyst-Firm Experience	0.001	0.001	0.001	-0.008	-0.001	-0.002
	(0.484)	(0.597)	(0.613)	(-1.524)	(-1.066)	(-1.118)
Analyst Breadth	-0.000	-0.000	-0.000	0.000	-0.000	-0.000
	(-0.858)	(-0.876)	(-0.886)	(0.156)	(-0.434)	(-0.464)
Analyst Forecast Frequency	0.005**	0.006**	0.006**	0.013	0.002	0.002
	(2.044)	(2.312)	(2.315)	(1.575)	(0.732)	(0.769)
All Star	0.002	0.015	0.015	0.001	-0.029**	-0.028**
	(0.156)	(1.233)	(1.238)	(0.040)	(-2.209)	(-2.199)
Brokerage Size	-0.000	-0.000	-0.000	0.000	0.000***	0.000***
	(-0.178)	(-0.611)	(-0.632)	(0.625)	(3.011)	(2.984)
Brokerage Pressure	0.000	0.001	0.001	0.000	0.001	0.001
	(0.316)	(0.828)	(0.792)	(0.350)	(1.257)	(1.255)
Brokerage Reputation	0.131	0.108	0.106	0.401*	0.115	0.119
	(1.509)	(1.245)	(1.223)	(1.866)	(1.299)	(1.350)
Brokerage Age	-0.000	0.000	0.000	-0.000	-0.001**	-0.001**
	(-0.071)	(0.168)	(0.206)	(-0.211)	(-2.038)	(-2.046)
Brokerage Breadth	-0.000	0.000	0.000	0.000	0.000	0.000
	(-0.228)	(0.224)	(0.232)	(1.117)	(0.291)	(0.345)

Table 6: (Cont.)APPENDIX: Target Price Accuracy - Incremental Information

	(1)	(2)	(3)	(4)	(5)	(6)
Ln(Market Capitalization)		-0.097***	-0.097***		-0.023*	-0.023*
		(-5.240)	(-5.243)		(-1.689)	(-1.685)
Analyst Following		0.001	0.001		-0.005***	-0.005***
		(0.318)	(0.321)		(-2.716)	(-2.715)
Leverage		0.034	0.034		0.021	0.021
		(0.556)	(0.564)		(0.347)	(0.341)
Friday			0.005			-0.003
			(1.175)			(-0.671)
Proximity 5 days			0.007			0.017**
			(1.047)			(2.316)
Proximity 3 days			-0.012*			-0.010
			(-1.736)			(-1.335)
Standardized Forecast Error Control	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
Firm FE	YES	YES	YES	YES	YES	YES
Observations	33,223	30,056	30,056	38,748	32,619	32,619
R-squared	0.378	0.402	0.403	0.836	0.492	0.492

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