‘Did You Catch the Game Last Night?’ Peer Group Effects in Sell-Side Analyst Forecasts

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Abstract

In this paper, we identify a source of peer group influence that is plausibly orthogonal to information provision, yet nonetheless affects economic decision-making: the shock to an equity analyst of their undergraduate college football team winning the NCAA Championship Game. We find that analysts’ forecasts respond positively to their undergraduate school’s football team winning the NCAA final. We then show that the shock of ‘winning’ spreads within an analyst’s brokerage, positively influencing the forecasts of their colleagues. Brokerages where the degree of this diffusion is greater have lower female representation in their analyst teams, as well as lower ESG scores.

Keywords: social networks, connections, sentiment, analyst forecasts

JEL Codes: G14, G24, G34, G41

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1 Introduction

An ever growing body of research suggests that interactions between peers have meaningful implications for financial decision making. In early work, Madrian and Shea (2001) and Duflo and Saez (2003) show that an individual's decision to participate in certain employer-sponsored retirement plans is affected by the choices of their co-workers. Similarly, Hong et al. (2004) find that investors find the stock market more attractive when a larger number of their peers participate. Bailey et al. (2018a,b, 2021) proxy for peer groups through data from Facebook and tease out the relevance of peer effects in the housing market and product adoption. The relevance of selective exposure to peers (echo chambers) even among sophisticated investors is the subject of a recent study by Cookson et al. (2023).

Despite the overwhelming evidence regarding the relevance of peer effects, ambiguity remains around the exact mechanisms at play. On the one hand, individuals draw on their social networks to obtain additional, meaningful information to base their beliefs on (Bailey et al., 2021; Cookson et al., 2023; Fischer, 2022). On the other hand, peers may influence the sentiment and emotional aspect of an individual's worldview, which can in turn shape their behavior.[1] In a similar vein, the issue of homophily is a pervasive concern in the estimation of peer effects (Angrist, 2014).

Differentiating between these mechanisms is difficult, as it is often unclear whether individuals are able to parse out spurious information when drawing on their social network, and it is rare to see an instrument employed when assessing peer effects. While Bailey et al. (2018a,b) suggest that households react to the experiences of their peers, it remains an open question whether doing so is rational. In their setting, the US housing market, one can

[1]There exists a vast literature in psychology on peer influence (in adolescents). Brechwald and Prinstein (2011) provides an excellent summary.

argue that the experiences of peers is a meaningful source of information as long as there are common factors across regions. Furthermore, while households are generally considered unsophisticated investors, the question of how professionals draw on information from their peers remains an open one.

Several papers have considered how professionals react to information. Similar to our setting, Kempf and Tsoutsoura (2021) consider analysts as financial professionals, and attempt to identify how they incorporate outside information into their decision making. Specifically, they analyze the reaction of credit analysts to changes in the 'color' of the white house depending on partisanship. However, once again it is difficult to argue that these responses are spurious, as Democrats will likely expect the policies of a Democratic president to be economically more beneficial.

Drawing on the existing literature on the non-trivial impact of sports results on decision making (Edmans et al., 2007; Eren and Mocan, 2018), we exploit a novel source of plausibly exogenous variation in equity analyst forecasts that is well-suited to analyzing the sentiment component of local diffusion: the shock to an analyst's sentiment resulting from their college team winning/losing the NCAA National Championship Football game.

We choose this setting for two reasons. Firstly, equity analysts operate in a high stakes setting, with numerous papers demonstrating the significant implications that their forecasts can have on firm and stock market behavior.[2] Yet evidence is widespread that their forecasts

[2]Beginning with early work on the informational content of analyst forecasts (Elton and Gruber, 1972; Fried and Givoly, 1982), several papers have documented profitable trading strategies that are based on forecasts and their revisions (Elton et al., 1981; Givoly and Lakonishok, 1979, 1980; Griffin, 1976; Imhoff and Lobo, 1984). More recent work has examined stock market reactions to forecast revisions (Frankel et al., 2006; Gleason and Lee, 2003), the overweighting of forecasts by investors (So, 2013), the impact of analyst coverage on crash risk (Kim et al., 2018), and firms' earnings management responses to forecasts (Almeida et al., 2016; Bhojraj et al., 2009; Shore, 2023; Terry, 2015).

are subject to influence from plausibly arbitrary sources.[3] As such, it is not far fetched to claim that college football results influence an analyst directly, and that these shocks could have economic significance. Secondly, what makes these shocks useful for our purposes is the precise and individual-specific nature of their sentimental impact. To illustrate with an example, consider an analyst who attended LSU, whose colleagues are Boston College graduates. In this case, the shock of LSU winning the championship is plausibly isolated to the focal analyst and doesn't affect their coworkers other than through spillovers. It is then possible to assess whether the shock of 'winning' influences the forecast behavior of the analyst's *peers*; i.e. the other analysts that work in their office.

To perform this analysis, we require data on analysts' college attendance. This information is not readily accessible in standard databases of analyst forecasts, such as IBES, prompting us to collect this information from a variety of sources. Using a procedure we describe in Section 2, we combine three different datasets containing analyst-level attributes (Bloomberg, CapitalIQ, and LinkedIn). Of the 57,749 individual analysts in IBES, we are able to link 7,481 analysts to schooling information. We are therefore working with a sample that is more than three times larger than that in Cohen et al. (2010). To the best of our knowledge, our data is the most comprehensive on analyst education currently assembled.

As more prominent individuals are naturally easier to identify in our education data, the matched sample consists of analysts covering more, larger (by asset size), and more profitable firms. These individuals furthermore release more forecasts and tend to be longer tenured than their peers. While this is not surprising, it does prompt us to restrict the control group to other matched analysts, alleviating concerns that our results are driven by ex-ante

[3] Factors that have been shown to influence analyst forecasts range from herding of forecasts (Clement and Tse, 2005; Trueman, 1994); analysts' career concerns creating forecast bias (Bradley et al., 2022; Harford et al., 2019; Hong and Kubik, 2003); and exposure to terrorist shocks and natural disasters leading to greater pessimism in analyst earnings forecasts (Cuculiza et al., 2021; Kong et al., 2021).

differences in the sample composition.

With this sample in place, we turn to estimating the treatment effects on individuals and the magnitude of spillovers on their colleagues. We find two significant results. Firstly, we document that the shock of a college winning the NCAA championship game does indeed influence the forecasts of an analyst who is a graduate. Our estimates suggest that 'winners' post forecasts roughly 0.12 standard deviations higher, significant at the 5% level.[4] Furthermore, we provide evidence that this shock permeates to analysts who work at the same brokerage as these 'winners.' Our estimates suggest that, if 10% of an analyst's colleagues were 'winners,' a 'non-winner' analyst posts forecasts roughly 0.04 standard deviations higher, significant at the 5% level. Critically, our findings hold controlling for analyst-firm fixed effects. Including these fixed effects allows us to alleviate concerns surrounding selection effects in analyst-firm relationships, as the estimated impact of victory/loss is measured *relative to the analyst-firm history*, rather than across or between analysts and firms. We further control for a host of firm-level fundamentals following So (2013).

We assess the robustness of these results through several additional exercises. Firstly, adding a firm-analyst-month fixed effect ensures that our results are not driven by within-year variation of analyst forecasts. Given that the number of colleges that have won the NCAA championship game across our sample period (2000-2020) is relatively small (only 10 unique colleges in total), we furthermore perform a 'leave-one-out' exercise in which we systematically remove winning colleges to check whether any individual school is responsible for the effect. Reassuringly, we consistently find the same basic effect. Dropping small firms (below $200m in total assets), those in finance, insurance, and real estate sectors, as well

[4] In untabulated results, we fail to find evidence that these forecast responses result in lower accuracy of forecasts. Given that previous work has documented the endogenous relationship between forecasts and unearnings, this result is perhaps unsurprising. We discuss this endogeneity, and find firm-level results consistent with them in Section 6.1 of our paper.

as utilities, non-operating establishments, and industrial conglomerates doesn't materially change our results either.

We also assess whether the 'expectation' of victory/loss matters for the individual analyst result. It is natural to suppose that an unexpected loss, for instance, would be more upsetting than an expected one. We collect historical odds from betting markets for the NCAA finals in our sample, which are then interacted with the treatment variable in our difference-in-differences estimation. We find that whilst odds are unrelated to the impact for winners, their inclusion creates a statistically significant drop in forecasts for losers, with more unexpected losses inducing greater drops in subsequent forecasts that year. This result is therefore consistent with the idea that the 'expected' result matters for subsequent analyst sentiment.

Lastly, we conduct two placebo tests. In the first, we randomly assign a 'winner' indicator to analysts who we know were not in fact winners, followed by re-estimating the treatment effect. Repeating this exercise 2,000 times yields a distribution of the relevant coefficient that is centered around zero, with a mean and median of -.0005 and -0.0044 standard deviations respectively. 87.5% of the estimated coefficients are lower than in our initial analysis. In the second placebo test, we randomly assign a 'proportion' of winning analysts to a given brokerage in a given year that did not employ any 'winners,' followed by estimating our preferred regression specification. The proportions that we use for our this test are drawn from those observable in the data. The distribution of treatment effects after 2,000 samples is once again centered around zero. In total, 99.65% of the estimated placebo coefficients are lower than our original estimate.

Finally, to ensure that our brokerage spillover effect is driven by peer interaction and not proximity to a treated analyst's college town, we perform an exclusion exercise where we remove all US-based analysts operating outside of New York. New York has never had a college football team participating in the NCAA final, while accounting for 93.6% of all US-based analysts. Once again, we find a significant and positive spillover effect of winners to peers.

With these results in place, we proceed to investigate whether observable features of an analyst's peer group influence the nature of these spillovers. Given that organizational cultures can vary widely from company to company (Handy, 2007), it is probable that some brokerages are more likely to induce diffusion of the football sentiment shock to peer analysts than others.

To test this hypothesis, we estimate a brokerage-specific 'winner spillover' effect, which we will refer to as the 'Bro-ness.' The raw score we consequently refer to as 'Bro Score,' while the ranked measure we will call the 'Bro Rank.' We use the same specification as before, controlling for a firm-analyst fixed effect, a month-year fixed effect, and a host of controls.

Subsequently, we investigate whether these brokerage-specific effects correlate with observable features of the brokerages. Since men are nearly three times more likely to describe themselves as 'avid fans' of college football than women (37% of men vs. 14% of women),[5] we first assess whether brokerages that are more responsive to these kinds of sentiment shocks (i.e., have higher 'Bro Scores') differ in their share of female analysts. Consistent with our intuition, brokerages that respond more strongly to the sport sentiment shocks of peers have a significantly lower proportion of female analysts. Moving from the brokerage with the lowest response to the highest is associated with a fall in the total percentage of female analysts working at the brokerage of 6.24%. Given that the average female representation at a given brokerage across our sample is only 12.6%, this is a statistically and economically significant difference.

[5]This finding was reported in a recent (January 2023) poll of 2,201 American adults, published by Morning Consult, a global decision intelligence company.

Another notable feature of sports culture is a stereotype of unpleasant and/or problematic male behavior. With this in mind, we collect two sets of measures of corporate 'misbehavior', with a view to assessing whether our 'Bro Score' correlates with these measures. The first is MSCI/KLD Scores. As prominent measures of firm-level ESG performance, these scores are typically used in the literature to assess the degree of corporate social responsibility in firms.[6] Our second measure is the corporate culture scores developed in Li et al. (2021). These scores, constructed using machine learning techniques applied to quarterly earnings call data, identify the strengths of firm culture along five dimensions: (1) Integrity, (2) Teamwork, (3) Innovation, (4) Respect, and (5) Quality. We find mixed evidence that 'Bro Scores' correlate with ESG scores, though the overall picture suggests a negative relationship. By contrast, using the corporate culture scores from Li et al. (2021), we find that 'Bro Scores' are associated with higher degrees of 'Teamwork' and 'Respect'.

As a final exercise, we assess the firm and stock market implications of our sentiment shocks. Since the treatment of analysts is plausibly exogenous to the business environment of the firms they cover, we construct an instrument based on the share of winners covering a given firm. This instrument is then used in a standard instrumental variable (IV) setting to establish a plausibly causal relationship between forecasts and firm/stock market level objects. We consider two outcome variables: (1) firm-level earnings-per-share (EPS), and (2) abnormal stock returns, computed using a Carhart (1997) four-factor model.

Consistent with Shore (2023), we document a near one-to-one response of firm-level earnings to a consensus forecast shock induced by an analyst 'winning.' Secondly, and again consistent with Shore (2023), there does not seem to be any evidence that the forecast shock moves the market: a one standard deviation increase in the consensus forecast, driven

[6]See Gillan et al. (2021) and Hong and Shore (2022) for recent reviews of the Corporate Social Responsibility literature, where the use of MSCI/KLD scores is very widespread.

by the sentiment shock, leads to moves in abnormal returns by between -0.15% to -0.83% depending on specification, not statistically different from zero. We can rule out that these results are due to insufficient power in the first stage (F-stat of 194.96 to 290.37). These findings suggest that the market is able to parse out arbitrary variation from forecasts. That said, these findings are to be interpreted with caution due to the small number of winners in our sample.

Our paper contributes to several strands of literature. Firstly, we add to the growing literature on the importance of social networks in economic decision-making (Bailey et al., 2018a,b, 2021). We also document the significance of analyst-specific sentiment shocks to their forecasting behavior (Cuculiza et al., 2021; Edmans et al., 2007; Kempf and Tsoutsoura, 2021; Kong et al., 2021). Finally, we contribute to the literature that identifies how firms actively responds to analyst forecasts by managing their earnings (Almeida et al., 2016; Bhojraj et al., 2009; Shore, 2023; Terry, 2015).

The remainder of this paper is organized as follows: Section 2 describes the data collection process and presents summary statistics of our final matched dataset. The empirical approach for identifying analyst and brokerage-level responses to the NCAA championship sentiment shock is outlined in Section 3. Section 4 presents our main results. Subsequently, Section 5 provides details on the analysis of the variation in brokerage-specific responses to sentiment shocks and the underlying characteristics of those brokerages. Firm and stock market responses to our sentiment forecast shocks are assessed in Section 6. Our results are discussed in Section 7. Ultimately, Section 8 concludes.

2 Data

The construction of our novel dataset is a major contribution of this paper. In this section, we outline the different data sources we draw from and how they are combined to arrive at the final sample. Earnings forecasts from IBES, which include the initial and (abbreviated) last name of an analyst as well as a brokerage identifier are the starting point for the sample construction. After unmasking the brokerage names we substantiate the dataset with novel data on education, employment, and gender from Bloomberg, CapitalIQ, and LinkedIn. This process leads us to identify the education of 7,481 analysts, a sample that is more than three times larger than that of Cohen et al. (2010). We finalize the dataset by adding firm-level controls from Compustat and CRSP.

2.1 Forecast Data

In line with the existing literature on analysts' forecasts, we obtain the earnings-per-share (EPS) estimates for the current and following fiscal year from IBES. We start from the universe of sell-side analysts in IBES which compromises a total of 67,300 analysts. Since our shocks are analyst-specific, we remove forecasts that are attributable to research teams and desks. We identify and then eliminate entries with either multiple names in the name field (8,192), or those that contain names of research desks (1,359).[7] This leaves us with 57,749 unique analysts for whom we have a last name and initial.[8]

A roadblock in identifying individual analysts and brokerages in the data is related to a

[7]A research desk usually carries the name of an industry/region or a (truncated) form of 'research department.'

[8]According to Fang and Hope (2020) a large number of observations in IBES are attributable to teams despite only the name of the lead analyst being listed in the database. To the extent that we don't already address this concern by excluding observations from analyst teams, this should only bias our results downwards.

change made to the dataset in October 2018 when the vendor began anonymizing analyst and brokerage identifiers. In contrast to earlier studies (Cohen et al., 2010), we therefore need to first unmask the brokerage identifiers, followed by linking analysts to our remaining data. We do so following the methodology proposed by Gibbons et al. (2020). For each entry in the recommendation table ('RECDET'), we obtain the name of the analyst ('ANALYST'), the individual's identifier ('AMASKCD'), and the identifier of the brokerage the analyst is working at ('ESTIMID'). Since the brokerage identifiers are derived from institutions' names, we manually link the 94 most prevalent ESTIMIDs (i.e. largest brokerages) to the brokerage names. Analysts working at these entities are responsible for 46% of all forecasts.

2.2 Analyst-Level Attributes

Employment and educational background of analysts are obtained from three different sources: (1) Bloomberg, (2) CapitalIQ, and (3) LinkedIn. As the analyst names in IBES consist of the initial and last name, potential matches are identified based on an exact match of the (truncated) last name and an exact match of the initial. To zero in on valid matches, we require an overlap between the employers as derived from IBES and those reported in the outside dataset. Once we have obtained the (plausibly) complete name of an analyst, we use that information to specifically search for that individual across datasets.

It should be noted that neither of the three sources necessarily provides a complete record of an analyst's education.[9] In an attempt to alleviate these concerns, we construct plausibly complete educational histories for all of the matched analysts by drawing from data across all datasets. The names of all universities appearing in the datasets are disambiguated and assigned unique identifiers. We further obtain information on the degree and major earned

[9]Based on our investigations it is not uncommon for individuals to omit undergraduate education on LinkedIn, exclusively listing graduate degrees instead.

from the institution and, where available, the years of attendance.

Of the 57,749 individual analysts in IBES we are able to link 13,307 (23.04%) to at least one of our sources for career and education information. Since not every entry contains information on educational attainment, we end up with schooling information for 7,481 analysts. We are ultimately working with a sample that is more than three times larger than that in Cohen et al. (2010).[10] To the best of our knowledge, our data is the most comprehensive on analyst education compiled to date.

Table 1 presents summary statistics of the education data. To give a sense of the analysts in our matched sample we construct a series of dummy variables that are commonly used in the corporate governance literature. The dummy variables in Panel A of Table 1 take on values of one if the analyst has obtained a Bachelors/Masters/Doctoral degree respectively and zero otherwise. As mentioned previously, while we observe educational information for all analysts in this sample, we don't necessarily have a full record of the degrees that were obtained. Consequently, only 77.4% of individuals have a Bachelors. While we would expect all analysts to have completed undergraduate education, there are two reasons why we don't observe the attainment of a B.A. or equivalent for about one quarter of them. Firstly, some analysts report the attendance of a university without stating the terminal degree that was obtained, which biases down this number. Secondly, there are analysts who omit their undergraduate education and exclusively list the completion of an advanced degree (most commonly an MBA).

While more than half (59.9%) of the analysts in our sample have completed a graduate degree, only about one in twenty hold a doctoral degree. With respect to the universities attended, Panel B reports summary statistics for a series of dummy variables that are de-

[10]Cohen et al. (2010) end up with a sample of 1,820 unique analysts for whom they gather educational data from zoominfo.com among others.

Table 1: Analyst Education

This table presents the summary statistics of the analyst dataset. Data from Bloomberg, CapitalIQ, and LinkedIn are disambiguated and cross-verified where an individual analyst is found in more than one source. We construct a series of variables to quantify the educational background of the individuals in our sample. Panel A presents the summary statistics of dummy variables that capture the attainment of a Bachelors, Masters, or Doctoral degree. The variables presented in Panel B are related to university attendance, capturing location (US and UK dummies) and Ivy league membership (IvyLeague). Lastly, Panel C presents the summary statistics for the analyst gender and year of birth (since we are working with a panel dataset, the year of birth is more meaningful than age).

Statistic	**N**	**Mean**	**SD**	**Min**	**Max**
Panel A: Degree					
Bachelors	6,351	0.774	0.419	0	1
Masters	6,351	0.599	0.490	0	1
PhD	6,351	0.054	0.225	0	1
Panel B: University					
US school	6,181	0.536	0.499	0	1
UK school	6,181	0.163	0.369	0	1
IvyLeague	6,114	0.136	0.342	0	1
Panel C: Gender & Age					
Female	6,336	0.137	0.344	0	1
Year of Birth	764	1968	9.521	1929	1994

signed to capture the location of their education. 53.6% of individuals obtained at least one of their degrees from a university in the US. Universities in the United Kingdom are the second most frequently attended (16.3%). Somewhat surprisingly, one in four of the analysts that obtained at least one of their degrees from a US institution did so at an Ivy League university. As a point of comparison, among US board members the share of Ivy League educated individuals is about 19%.

Lastly, Panel C of Table 1 presents information on the age and gender of the analysts. Consistent with previous studies, women are underrepresented in the finance industry, with only about one in six (13.7%) analysts being female. While the coverage of the gender variable is very good, we only observe the year of birth for a very small number of the matched analysts. We therefore only report this information for exhibition purposes.

While our matching approach does not favor specific analysts conditional on those analysts being in one of the three data sources, we do expect the selection of analysts into these datasets to be non-random. In Table 2 we therefore compare the matched to the unmatched analysts at the analyst-year. Unsurprisingly, there are stark differences between the two groups. As more prominent individuals have stronger incentives to have accounts with Bloomberg/CapitalIQ/LinkedIn, the matched sample consists of analysts covering a larger number of bigger and more profitable firms. These individuals are furthermore more productive and longer tenured than the individuals in the unmatched sample. While this is not surprising, it does prompt us to restrict the control group to other matched analysts to alleviate concerns that our results are driven by ex-ante differences in the sample composition.

As the treatment variable will ultimately depend on university attendance, Table 3 presents the ten most common schools in our sample, as well as those that won/lost an NCAA final since 2000 (the start of our sample). We rank schools by the total number of unique analyst-school observations. Unsurprisingly, a large number of analysts attended Columbia University and New York University, two of the largest feeder schools for jobs on Wall Street. Outside the US, the universities of Oxford and Cambridge are the most attended schools. Naturally, fewer individuals attended the large football schools at any point in their life.

Another strength of our data is that we are able to observe the office location of most of the analysts in the matched sample throughout their careers. In Figure 1 we plot the

Table 2: Matched vs. Unmatched Analysts

This table presents a comparison between the matched and unmatched analysts. Our comparison is time-invariant, i.e., we collapse all time-series variables down to a single observation. As we rely on matching names and employment when identifying IBES analysts in our other sources (Bloomberg, CapitalIQ, LinkedIn), we are naturally biased towards those individuals that are more prominent, leading to the observable differences between the matched and unmatched parts of our sample.

	Matched			**Unmatched**			Δ
	N	Mean	S.D.	N	Mean	S.D.	
Individual	17,846	0.998	0.039	103,244	0.950	0.218	0.049***
Firms Covered (Total)	17,846	11.752	8.297	109,001	7.655	7.453	4.096***
Firms Covered (CRSP)	17,846	9.768	7.960	109,001	6.211	7.009	3.557***
Firms Covered (Comp)	17,846	9.757	7.956	109,001	6.205	7.003	3.552***
Forecasts (Total)	17,846	75.618	64.893	109,001	42.715	53.944	32.903***
Forecasts (Mean)	17,846	5.951	3.250	109,001	4.662	3.222	1.289***
Tenure	17,846	6.653	5.277	109,001	4.379	4.527	2.274***
Individual	17,846	0.998	0.039	103,244	0.950	0.218	0.049***
Size (Mean)	14,108	8.010	1.490	74,323	7.785	1.679	0.224***
EPS (Mean)	14,026	1.532	1.835	73,933	1.329	1.871	0.203***
NI (Mean)	14,111	657.625	973.640	74,338	556.761	965.117	100.863***
ROE (Mean)	14,055	0.006	0.084	73,611	0.003	0.090	0.003***

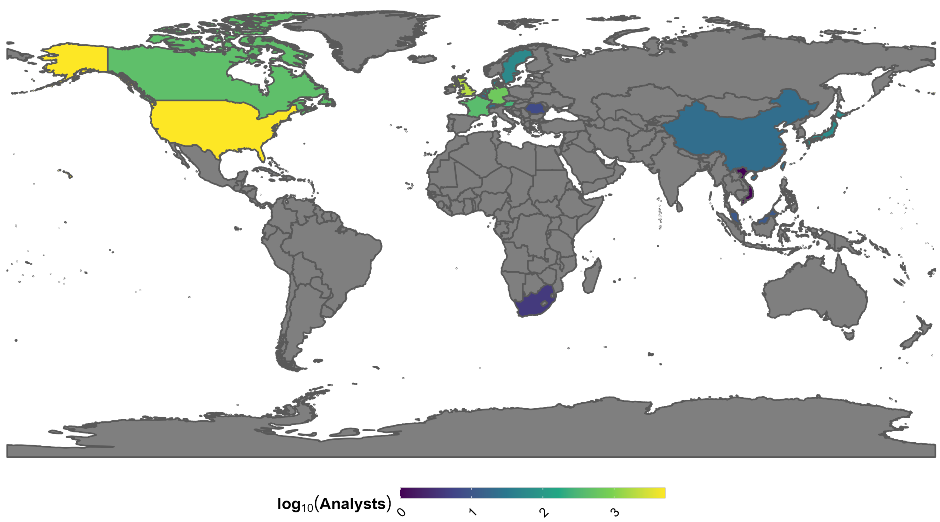
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

geographical distribution of analysts, inversely weighted by the number of positions held by an analyst. Unsurprisingly, the large (western) financial centers dominate the sample.

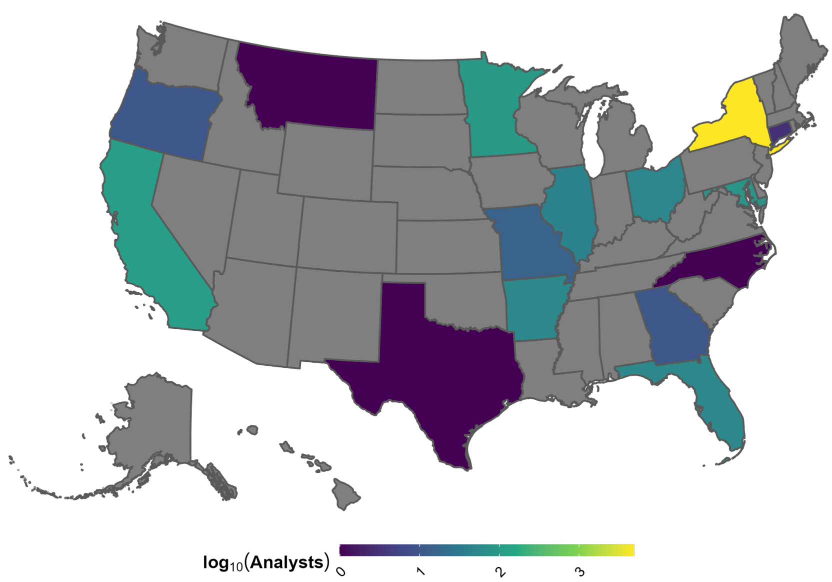
Lastly, we compare the three groups, winners, losers, control, within the matched sample with respect to the industries they cover to ensure that such differences are not driving our results. We do so by computing the share of firms covered by each analyst in a given year, where industries are defined based on one-digit SIC codes. Table 4 presents the relative distribution in coverage for the observations in the three subsamples. Our findings are consistent with no meaningful difference in industry coverage.

Figure 1: Distribution of Analysts

These figures plot the distribution of analysts by working place. Using detailed information on the office addresses of all individuals, we count the number of analysts in each location. We inversely weight by the number of different workplaces at the analyst level. Subsequently, we aggregate to the state level (for US locations, Panel b) and the country level (Panel a). Out of 9,512 analysts for whom we can observe locations, 5,183 are based in the US, of which 4,852 are working from an office in New York.



(a) World



(b) United States

Table 3: Most Attended Universities

This table presents the ten most attended schools in our sample of analyst education, as well as those that played in the NCAA Football Championship at any point in time between 2000 and 2022. For each university the number of different analysts that attended it at some point is computed. We disambiguate the raw university names and assign unique identifiers to each one. Observations therefore refer to a university (e.g. Columbia University) irrespective of the school attended (e.g. Columbia Business School).

Rank	**University/College**	**Analysts**
1	New York University	324
2	Columbia University in the City of New York	304
3	University of Pennsylvania	232
4	Harvard University	179
5	University of Oxford	177
6	University of Chicago	172
7	Cornell University	154
8	University of Cambridge	152
9	The London School of Economics and Political Science	140
10	University of Toronto	128
	⋮	
46	University of Texas	47
59	University of Southern California	41
67	University of Notre Dame	37
75	University of Florida	32
94	Virginia Tech	25
98	University of Miami	24
102	University of Georgia	24
119	The Ohio State University	20
180	University of Alabama	12
197	University of Oregon	11
230	Florida State University	9
251	Louisiana State University	8
278	Auburn University	7
283	Clemson University	7
286	University of Tennessee	6
322	University of Oklahoma	5
329	University of Nebraska	5

Table 4: Covariate Balance

This table presents a comparison in the industry coverage between the analysts in the treatment and control groups. For each analyst-year we compute industry coverage as the share of companies falling in each of the nine SIC industry ranges. The results when computing coverage based on total assets and market capitalization look similar.

	Winners (N = 255)			**Control (N = 50,500)**			**Losers (N = 240)**	
Variable	μ	σ	$\mathbf{\Delta}_W$	μ	σ	$\mathbf{\Delta}_L$	μ	σ
SIC 1	0.050	0.183	-0.042***	0.091	0.256	-0.006	0.085	0.253
SIC 2	0.269	0.407	0.105***	0.163	0.330	0.053**	0.217	0.376
SIC 3	0.292	0.395	0.068***	0.225	0.358	0.013	0.237	0.365
SIC 4	0.075	0.207	-0.031**	0.106	0.271	-0.008	0.099	0.249
SIC 5	0.085	0.213	-0.003	0.087	0.236	-0.017	0.070	0.206
SIC 6	0.051	0.186	-0.090***	0.141	0.325	-0.023	0.118	0.303
SIC 7	0.149	0.283	-0.002	0.151	0.297	-0.004	0.148	0.296
SIC 8	0.028	0.116	-0.005	0.034	0.133	-0.007	0.027	0.107
SIC 9	0.001	0.013	-0.000	0.002	0.022	-0.001***	0.000	0.001

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

2.3 Firm-Level Data

We draw company fundamentals from Compustat and stock prices from CRSP, linked to EPS forecasts from IBES. Firm variables that have been shown to influence analysts forecasts are used as controls. Guided by So (2013) we construct the following control variables from firm fundamentals: earnings-per-share when earnings are positive and zero otherwise, a binary variable indicating negative earnings, negative and positive accruals per share,[11] the percent change in total assets, a binary variable indicating zero dividends, dividends per share, the book-to-market ratio defined as book value scaled by market value of equity, and the end of fiscal year share price.

[11] Accruals equal the change in current assets (Compustat item ACT) plus the change in debt in current liabilities (Compustat item DCL) minus the change in cash and short-term investments (Compustat item CHE) and minus the change in current liabilities (Compustat item CLI).

2.4 NCAA Results

We obtain data on college sports from Nielsen's Gracenote and the NCAA.[12] For each year from 2000 to 2022 we collect the teams that competed in the NCAA football finals which generally take place in January of a given year. Table 5 contains a list of all finalists in our sample and whether they won or lost the game.

Analysts that went to the winning (losing) team's institution will be referred to as 'winners' ('losers') throughout the remainder of this paper. In each year we therefore separate our analyst sample into three groups: (i) 'winners,' (ii) 'losers,' and (iii) control. It's crucial to notice that the group membership changes year over year, i.e. an analyst that is classified as treated in t_0 can, and likely will, be in the control group in t_1. We present an overview of the relative sizes of the treatment and control groups in Table 6.

2.5 Final Sample and Summary Statistics

Starting from the forecasts that are made throughout the year, we generate a monthly time-series for each analyst-firm pair. Figure 2 presents an example of how our monthly time-series of forecasts is constructed, based on Google's parent company Alphabet Inc. (NASDAQ:GOOG). In transforming the data, we need to address two separate issues. The first, and more relevant one, is the timing of our exogenous event in relation to the fiscal year end of most US corporations. As the NCAA finals take place in January, the treatment coincides with the earnings season. When working with the forecasts for the earnings release closest in time, this leads to a natural truncation of the time-series.

To make this point clearer, Figure 2a depicts the dispersion of EPS forecasts made for

[12] We access Gracenote through sports-reference.com, who supplement the raw data with additional information on teams. Data from the NCAA includes the location of all teams, their divisions, and a unique identifier that we use in matching datasets.

Table 5: NCAA Football Finalists

This table presents the NCAA football finalists since 1999. For each year, the winning and losing team are presented. The finals take place in January in a 'neutral' location.

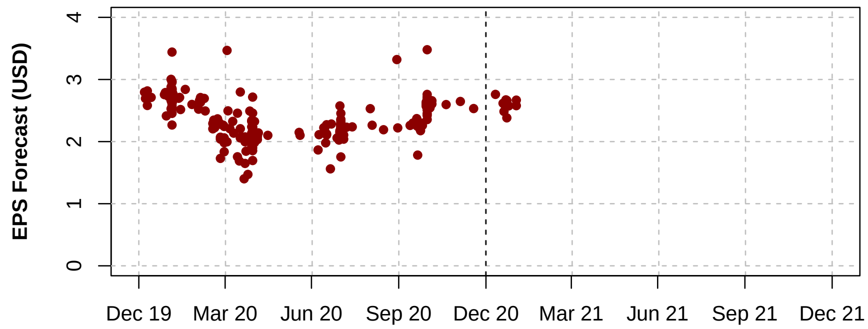
Year	**Winner**	**Loser**
2022	University of Georgia	University of Alabama
2021	University of Alabama	Ohio State University
2020	Louisiana State University	Clemson University
2019	Clemson University	University of Alabama
2018	University of Alabama	University of Georgia
2017	Clemson University	University of Alabama
2016	University of Alabama	Clemson University
2015	Ohio State University	University of Oregon
2014	Florida State University	Auburn University
2013	University of Alabama	Notre Dame University
2012	University of Alabama	Louisiana State University
2011	Auburn University	University of Oregon
2010	University of Alabama	University of Texas at Austin
2009	University of Florida	University of Oklahoma
2008	Louisiana State University	Ohio State University
2007	University of Florida	Ohio State University
2006	University of Texas at Austin	University of Southern California
2005	University of Southern California	University of Oklahoma
2004	Louisiana State University	University of Oklahoma
2003	Ohio State University	University of Miami
2002	University of Miami	University of Nebraska
2001	University of Oklahoma	Florida State University
2000	Florida State University	Virginia Tech
1999	University of Tennessee	Florida State University

Alphabet's 2020 fiscal year, which ended on December 31, 2020. As has been documented many times before, most forecast updates are clustered around the quarterly earnings releases. More importantly though, there are only very few forecasts made after the close of the fiscal year. This naturally renders the fiscal year 2020 ill-suited for testing the effect of a treatment in January 2021.

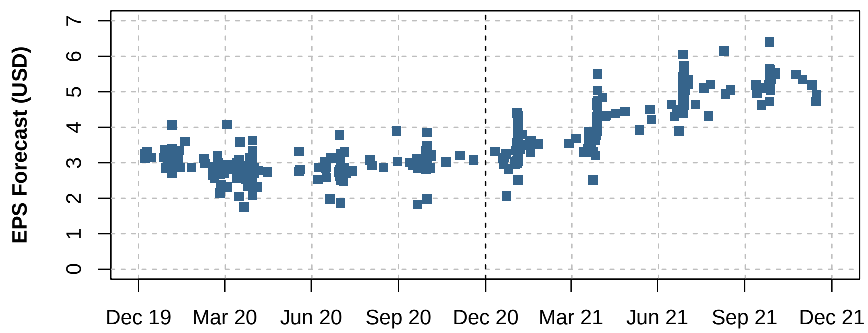
However, as most analysts release forecasts for several future periods, we instead construct our monthly time-series based on the forecasts for the 2021 fiscal year. As can be seen from

Figure 2: Forecast Revisions - Alphabet Inc. 2020/2021

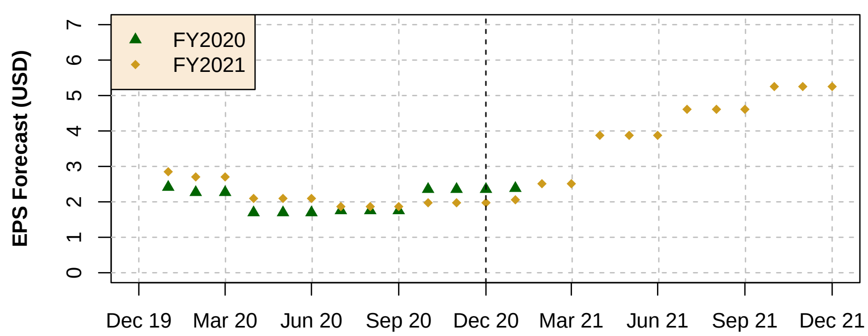
Panel a depicts the forecasts of all sell-side analysts in IBES for Alphabet Inc. (NASDAQ:GOOG) made in 2020 and 2021 for the fiscal year ending December 31st, 2020. Panel b depicts the same information for the fiscal year ending December 31st, 2021. Panel c contains the interpolated forecast time-series for one analyst in the sample (`analys` 114392). Across panels the dashed line represents the 2020 fiscal year end.



(a) FY2020 - All Analysts



(b) FY2021 - All Analysts



(c) Monthly Time-Series (Example)

Table 6: Treatment/Control Groups

This table provides an overview of the number of analysts that are treated in a given year. In our empirical section we exploit the fact that an individual analyst covers a range of companies to achieve identification, alleviating concerns regarding the comparatively small number of treated individuals. Furthermore, since there are differences between matched and unmatched analysts, we restrict the control group to those analysts that were matched to their educational background.

Year	**Winners**	**Losers**	**Control**	**Unmatched**
2000	0	2	517	4,779
2002	5	0	624	4,980
2003	2	5	633	4,820
2005	10	0	686	4,129
2006	5	10	682	4,085
2007	0	3	716	4,124
2008	2	2	719	4,012
2010	0	4	761	3,935
2011	0	2	794	4,045
2012	0	1	799	4,065
2013	0	8	828	4,552
2014	3	0	895	5,432
2015	1	2	923	5,771
2018	0	2	856	5,064
2021	0	1	729	4,525

Figure 2b, forecasts for FY2021 are available throughout the calendar years 2020 and 2021. This allows us to work with a significantly longer time-series. In cases where the fiscal year ends in a month other than December, we similarly work with the fiscal period giving us the most meaningful time-series of forecasts.

The second step in constructing the monthly time-series is a very intuitive one. As analysts update their forecasts at unpredictable frequencies, we construct our dataset from the most recent forecast by each analyst for a given company. Figure 2c depicts the time-series of EPS forecasts for Alphabet made by one of the analysts in our sample. The green triangles represent the time-series of forecasts pertaining to FY2020, underlining once more the necessity to work with the following fiscal period instead. The yellow diamonds are the time-series of the most recent EPS forecasts for FY2021 made by that analysts on the last day of a given month.

3 Empirical Strategy

In this section, we outline our empirical strategy for estimating analyst earnings forecast responses to college football sentiment shocks. Our approach involves a standard difference-in-differences framework, including analyst-firm fixed effects. Controlling for these fixed effects allows us to alleviate concerns surrounding selection effects in analyst-firm relationships. We begin by outlining how we estimate the analyst-level response to a direct shock of 'winning/losing.' We then discuss the construction of a brokerage specific treatment variable that measures the proportion of analysts affected by the college football shock. The construction of this variable allows us to measure within-brokerage spillovers, an advantage of our approach of identifying analyst-specific, rather than brokerage-wide shocks. Finally, we describe our approach for estimating firm and asset-pricing reactions to forecast shocks driven by the college football result.

3.1 College Sports

While sports events such as the NFL Super Bowl are known well beyond the borders of the US, the relevance of college sports is often not fully appreciated. Viewership of the 2023 NCAA football semi-finals and finals totaled more than 20mn per game. For comparison, the Super Bowl was viewed by 99mn individuals in 2022.

For our identification strategy to deliver a strong first stage it must be the case that having attended a college with a strong football culture leads the alumni to keep following the sport. Some of the schools in our sample are well known for investing heavily in their football programs, making it to the finals repeatedly over our sample period (see Table 5) and regularly having their graduates drafted into the NFL. Especially at these prominent schools (e.g., LSU), football is more than a pastime. Based on data compiled by Eren and Mocan (2018), the average attendance of Division I college football games was 45,000 in 2012 throughout the US. That said, it's not just the successful schools that draw fans from far and wide. In 1985 football dethroned baseball as the king of American sports. As most NFL franchises are located in large metro areas, there are vast parts of the country that are too far from the nearest venue to indulge in NFL games. As a natural consequence, college football had an equal rise in popularity.

College sports combine two core aspects of US culture, (1) alumni loyalty, and (2) enthusiasm for sports. It therefore doesn't come as a surprise that many graduates of schools with football programs develop a lifelong attachment to their *alma mater*'s team. Anecdotal evidence suggests that these ties remain strong even after having graduated and even moved to a different state.

3.2 Analyst Response

Our first goal is to show that winning/losing the NCAA Championship has a meaningful impact on analysts who attended the college that won/lost. To that end, we estimate the following regression specification:

$$\mathbb{E}_{i,j,t}[EPS_{j,y}] = \phi_{i,j} + \tau_t + \alpha_w w_{i,y} + \alpha_l l_{i,y} + \Gamma X_{j,y} + u_{i,j,t} \tag{1}$$

Where $\mathbb{E}_{i,j,t}[EPS_{j,y}]$ denotes the standardized earnings-per-share (EPS) forecast for firm j in year y, by analyst i at time (i.e. month-year) t.[13] $\phi_{i,j}$ is an analyst-firm fixed effect, τ_t is a month-year fixed effect, and $w_{i,y}/l_{i,y}$ is an indicator that takes a value of one if analyst i was a winner/loser in year y. X includes a host of covariates that have been shown to have predictive power for analysts' forecasts.[14] Our key parameters of interest are α_w and α_l. We interpret a positive/negative sign of these coefficients as evidence of a positive/negative shock to forecasts in the wake of victory/loss in the NCAA Championship College Football Final.

Identification is achieved through random variation in the identity of the school winning the NCAA final, even in the presence of endogenous matching between analysts and universities. Furthermore, by including analyst-firm fixed effects, we effectively compare an analyst's response relative to their own estimates in previous years. Nevertheless, the general concerns regarding DID estimated as outlined by Bertrand et al. (2004) remain to be addressed.

To assess whether the parallel trends assumption vital to DID estimators is violated, we estimate a dynamic specification of the following form:

$$\mathbb{E}_{i,j,t}[EPS_{j,y}] = \phi_{i,j} + \tau_t + \beta_{-5} \sum_{s \leq -5} D^s_{i,y} + \sum_{s \in [-4,-2]} \beta_s D^s_{i,y} + \sum_{s \in [0,10]} \beta_s D^s_{i,y} + \beta_{11} \sum_{s \geq 10} D^s_{i,y} + \epsilon_{i,j,t} \quad (2)$$

[13]As earnings-per-share forecasts are often negative, we cannot take logs. To avoid any issues related to scaling, we standardize all forecasts by subtracting the firm-analyst level average forecast and dividing by the firm-analyst standard deviation. Thus our coefficients can be interpreted as standard deviations, rather than USD amounts.

[14]Following So (2013), who in turn bases their analysis on Fama and French (2006), we include the following lagged firm characteristics from year $y-1$: earnings-per-share when earnings are positive and zero otherwise, a binary variable indicating negative earnings, negative and positive accruals per share, where accruals equal the change in current assets (Compustat item ACT) plus the change in debt in current liabilities (Compustat item DCL) minus the change in cash and short-term investments (Compustat item CHE) and minus the change in current liabilities (Compustat item CLI), the percent change in total assets, a binary variable indicating zero dividends, dividends per share, book-to-market defined as book value scaled by market value of equity, and end of fiscal year share price.

Going from (1) to (2), we replace the static treatment variables ($w_{i,y}$ and $l_{i,y}$) with a series of indicator variables $D^s_{i,y}$ for all months s. These indicators take values of one if the analyst was a winner/loser in year y and if the forecast is s months from the treatment event (i.e. the championship game). To allow for asymmetry in the contribution of the control variables we estimate separate specifications looking only at winners/losers, as well as including both treatment dummies simultaneously.

3.3 Network Response

The central goal of our paper is to identify how shocks to analysts can spread to their social network. To this end, we consider one salient network to the analyst: their workplace. Specifically, we investigate whether working as an analyst at the same brokerage as a 'winner'/'loser' has any meaningful effect on the forecasts of a 'non-winner'/'non-loser.'

To assess spillovers between analysts within the same brokerage, we re-estimate the specification in (1) and (2), replacing the treatment variable with the within-brokerage proportion of 'winners'/'losers.' This altered specification reflects the notion that we expect the strength of peer effects to be increasing in the number of coworkers affected by the event. Let $\widehat{W}_{i,y}$ denote the proportion of 'winners' working at brokerage i in year y, and let $\widehat{L}_{i,y}$ denote the proportion of 'losers' working at brokerage i in year y. Then, the specification we estimate takes the following form:

$$\mathbb{E}_{i,j,t}[EPS_{j,y}] = \phi_{i,j} + \tau_t + \alpha_w w_{i,y} + \alpha_l l_{i,y} + \beta_w \widehat{W}_{i,y} + \beta_l \widehat{L}_{i,y} + \Gamma X_{j,y} + u_{i,j,t} \tag{3}$$

Where $\mathbb{E}_{i,j,t}[EPS_{j,y}]$ denotes the forecast of analyst i for firm j at time period (month-year) t, $w_{i,y}/l_{i,y}$ are indicator variables taking a value of one if analyst i is a winner/loser, and $X_{j,y}$

is a vector of firm-year level controls.

We interpret the coefficient of β_w and β_l in (3) as capturing a 'spillover' effect of winners/losers in the analyst workplace. If $\beta_w > 0$ for instance, we interpret this as evidence that the positive shock experienced by a 'winning' colleague spreads to their colleagues forecasting behavior.

4 Results

In this section we detail our main results. We begin by showing that an analyst's forecasts do indeed respond to the personal shock of 'winning' the NCAA National Championship game. We then show that this shock spreads to analysts that work alongside 'winners' at the same brokerage. Finally, we discuss our robustness checks.

4.1 Analyst Specific

We start by estimating (1) using the monthly time-series data we constructed as outlined in Section 2. The coefficient estimates are reported in Table 7. To assess the dynamics of this effect, we estimate the dynamic specification in (2) and plot the coefficients in Figure 3.

We find that 'winners' post forecasts that are roughly 0.12 standard deviations higher in the wake of the shock of winning. Note that this coefficient is estimated in the presence of a firm-analyst fixed effect, and hence reflects a change in the analyst's forecasts relative to their forecasts of the same firm in years when they are not winners. We find a negative point estimate for losers, though without statistical significance.

In untabulated results, we estimate the effect of winning/losing on an analyst's forecast accuracy. We fail to find evidence that the forecasts of treated individuals are systematically

Table 7: Static Estimation

This table presents the results of estimating equation (1). Here $w_{i,y}/l_{i,y}$ are indicators taking a value of one if analyst i was a 'winner'/'loser' in the NCAA Championship Final in year y. The controls are described in Section 3 and follow the specification in So (2013) for constructing characteristic forecasts. In all cases we control for a firm-analyst fixed effect and a 'month-year' fixed effect.

Dependent Variable:		$\mathbb{E}_{i,j,t}[EPS_{j,y}]$	
Model:	(1)	(2)	(3)
Variables			
$w_{i,y}$	0.1229***		0.1212***
	(0.0442)		(0.0465)
$l_{i,y}$		-0.0296	-0.0100
		(0.0526)	(0.0548)
Controls	Yes	Yes	Yes
Fixed-effects			
Firm-Analyst	Yes	Yes	Yes
Month-Year	Yes	Yes	Yes
Fit statistics			
Observations	3,072,433	3,072,433	3,072,433
R^2	0.94241	0.94241	0.94241

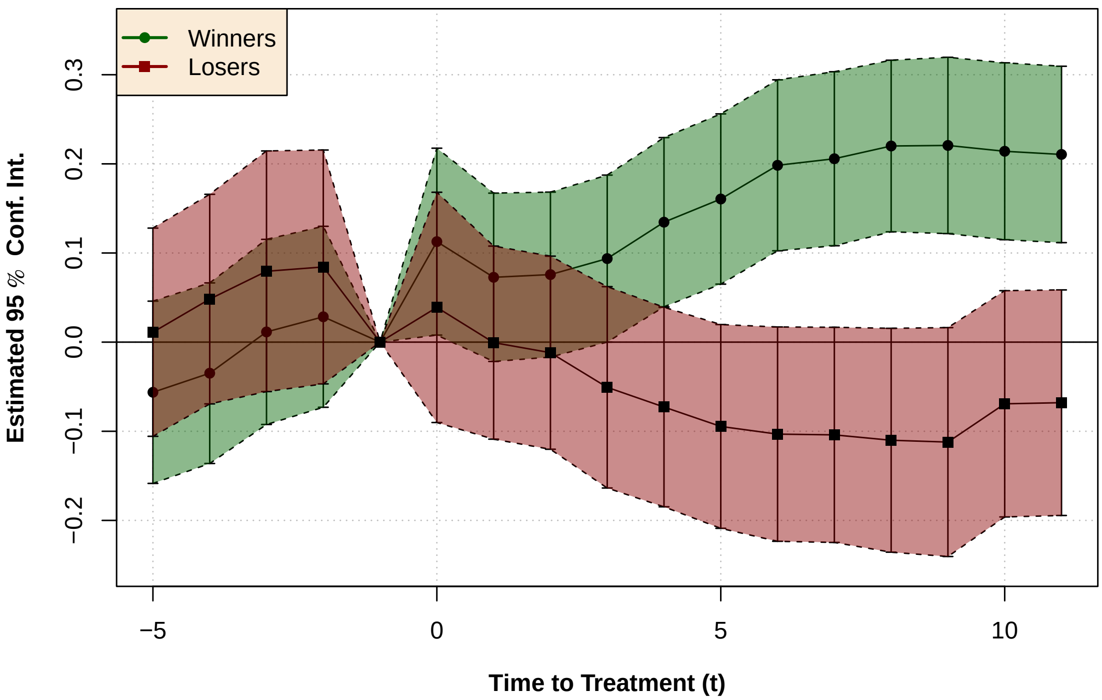
Clustered (Firm-Analyst) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

worse than those of their peers. While this may seem curious at first, this finding is consistent with Shore (2023) as well as our analogous findings reported in Section 6.1. Since firms respond to changes in the EPS target, there exists an endogeneity between forecasts and subsequent earnings. We discuss this endogeneity in more detail in Section 6.1.

As is visible in Figure 3 above, there is a clear parallel trend between 'winners' and 'losers,' which adds credibility to our identification strategy. Two additional features of Figure 3 are worth commenting on: the gradual widening of the forecasts, rather than immediate jumps, and the persistence of the shock over time.

Figure 3: Forecast - Dynamic

This figure plots the coefficients and 95% confidence intervals from our preferred difference-in-differences specification. We estimate the following regression $\mathbb{E}_{i,j,t}[EPS_{j,y}] = \phi_{i,j} + \tau_t + \beta_{-5}\sum_{s\leq -5} D^s_{i,y} + \sum_{s\in[-4,-2]} \beta_s D^s_{i,y} + \sum_{s\in[0,10]} \beta_s D^s_{i,y} + \beta_{11}\sum_{s\geq 10} D^s_{i,y} + \epsilon_{i,j,t}$ and extract the estimates $\widehat{\beta}_1$, which we then plot for two different definitions of treatment (a) 'winners' and (b) 'losers.' Standard errors are clustered at the analyst-level.



With respect to the former, it is worth noting, as discussed in Section 2, that forecasts occur continuously throughout the year, though infrequently. Across the whole sample, the median number of forecasts per analyst-firm-year is 4, with a standard deviation of 2.8. Across 'winners,' the median number of forecasts per analyst per firm-year similarly is 4, with a standard deviation of 2.3. As such, it seems reasonable that some time may pass before 'winning' analysts actually post forecasts.

To test this possibility, we identify the proportion of winning analysts whose first forecast comes after the month of the football game (January, or $t = 0$). We find that 64.3% of winning analysts post their first forecast at least one month after the game ($t = 1$), 32.4% at least two months after, 26.5% at least three months after, and 15.8% at least four months after. A sizeable 13.2% of winning analysts do not post their first novel forecast for six months after the championship game.

Note that this feature of the forecast data also goes some way towards explaining the persistence of the effect, given that the impact of the college football shock may not appear until late in the year. That said, a natural and valid concern is to question the plausibility of a college football game influencing mood for such a lengthy period that it would affect forecasts some six months after the fact. Here we defer to previous work that illustrates just how seriously US adults treat college football (Eren and Mocan, 2018).

We also check whether winners are more likely to revise negatively than non-winners, and if they do revise negatively, then whether winners do so more aggressively than the control group. Given that we observe no meaningful revision in the wake of the positive forecast shock, we should find that winners are no more likely to revise negatively, and that their negative revisions are no larger than for non-winners. We find that whilst winners post negative revisions roughly 1.03 times more often than positive revisions, this is slightly less than for non-winners, who post negative revisions 1.08 times more often than positive revisions. Similarly, when a revision is downward, the average for winners is -0.37 standard deviations, whereas for non-winners the average revision is -0.44 standard deviations. Thus these mean comparisons are consistent with our finding of a persistent effect on forecasts stemming from the football shock.

4.2 Brokerage Spillovers

Having established that treated analysts respond in the expected direction, we move on to answering the question of whether there are spillovers between analysts in the same brokerage. We do so by adding the variables $\widehat{W}_{i,y}$ and $\widehat{L}_{i,y}$, that measure the proportion of analysts working in the same brokerage as analyst i who were 'winners' and 'losers' respectively.

Out of a total sample size of 469 brokerages, we find 34 (33) brokerages that have non-zero values for $\widehat{W}_{i,y}$ ($\widehat{L}_{i,y}$) in at least one year. In total, around 22.3% of our brokerage-year sample contains a non-zero value for $\widehat{W}_{i,y}$. Around 8.1% of our forecast observations contain non-zero values for $\widehat{W}_{i,y}$, and roughly 7.7% for $\widehat{L}_{i,y}$. Within the forecast observations with non-zero values, the average value of $\widehat{W}_{i,y}$ is 3.8%, with a standard deviation of 4.6%, and the average value of $\widehat{L}_{i,y}$ is 4.0% with a standard deviation of 5.6%.

The coefficient estimates from running regression (3) are reported in Table 8. We find that the presence of winners in the workplace has a statistically significant positive effect on the forecasts of their peers. While optimism is contagious, with coworkers raising their forecasts by 0.04 standard deviations for every 10% of their colleagues who are winners, the losers' pessimism doesn't spread within the brokerage.

As before, we separately assess the dynamics of this effect by estimating (2) with the treatment variable defined as above. The point estimates and confidence intervals are depicted in Figure 4. We observe a similar pattern as outlined in Section 4.1.

4.3 Robustness

To check for robustness, we perform five separate exercises. In the first, we control for a firm-analyst-*month* fixed effect in our first specification, seeking to alleviate concerns that our

Table 8: Contagion

This table reports coefficient estimates from the following regression $\mathbb{E}_{i,j,t}[EPS_{j,y}] = \phi_{i,j} + \tau_t + \alpha_w w_{i,y} + \alpha_l l_{i,y} + \beta_w \widehat{W}_{i,y} + \beta_l \widehat{L}_{i,y} + \Gamma X_{j,y} + u_{i,j,t}$. $w_{i,y}$ ($l_{i,y}$) is a dummy that takes on a value of one for an analyst that is a winner (loser) of the college championship game. $\widehat{W}_{i,y}$ ($\widehat{L}_{i,y}$) represents the proportion of winners (losers) that analyst i works alongside in year y. Standard errors are clustered at the firm-analyst-level.

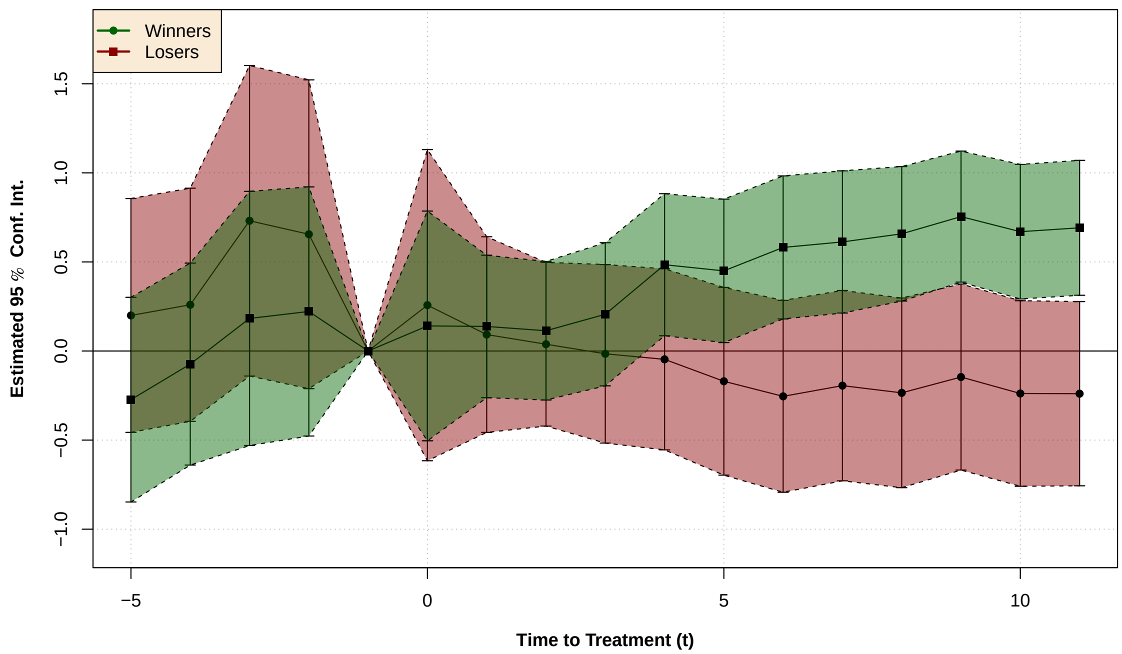
Dependent Variable:		$\mathbb{E}_{i,j,t}[EPS_{j,y}]$	
Model:	(1)	(2)	(3)
Variables			
$w_{i,y}$	0.0385		0.0368
	(0.0399)		(0.0422)
$\hat{W}_{i,y}$	0.4037**		0.4115**
	(0.1624)		(0.1636)
$l_{i,y}$		-0.0223	-0.0133
		(0.0564)	(0.0585)
$\hat{L}_{i,y}$		0.0804	0.1004
		(0.2023)	(0.2040)
Controls	Yes	Yes	Yes
Fixed-effects			
Firm-Analyst	Yes	Yes	Yes
Month-Year	Yes	Yes	Yes
Fit statistics			
Observations	2,931,311	2,931,311	2,931,311
R^2	0.93625	0.93625	0.93625

Clustered (Firm-Analyst) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

estimates are picking up some time variation in the strength of analyst forecasts. Secondly, we test whether the treatment effect varies with the expected outcome of the game by drawing on betting odds prior to the start of the game. Subsequently, we perform a 'leave-one-out' procedure wherein we systematically remove each winning school from our regression. It is our aim to show that no individual school is driving the headline result. In the fourth, we remove US-based analysts working outside of New York. Doing so allows us to eliminate

Figure 4: Brokerage Effects

This figure presents coefficient estimates of the event study regression outlined in equation (2), with the treatment variables defined as $\widehat{W}_{i,y}$ for 'winners' of the NCAA Championship game, and $\widehat{L}_{i,y}$ for 'losers.' These treatment variables measure the proportion of analysts working in the same brokerage as analyst i who were winners and losers respectively. The coefficient should be interpreted as the impact on an analyst's forecast if 100% of their colleagues were 'winners.' We cluster standard errors at the firm-analyst level.

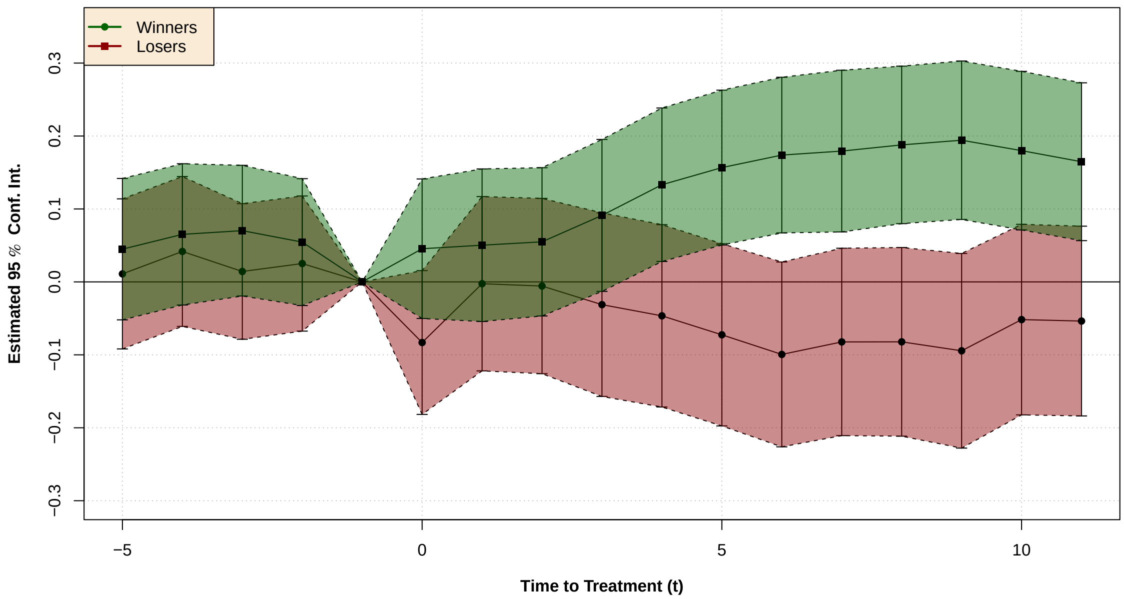


the possibility that analysts who work in close proximity to colleges that win the NCAA championship are influenced by their local surroundings rather than their peers.[15] Finally, we conduct a standard placebo test.

[15] Whilst 93.6% of analysts work in New York, no New York based college football team has ever appeared in the NCAA Championship Final game. See Figure 1 and Table 5 for more details.

Figure 5: Robustness - Firm-Analyst-Month Fixed Effect

This figure presents coefficient estimates of the event study regression outlined in equation (2), controlling for a firm-analyst-month fixed effect.



4.3.1 Firm-Analyst-Month Fixed Effect

It is possible that analysts post more or less optimistic forecasts in certain months of the year. To ensure that such time variation is not driving our results, we control for a 'firm-analyst-month' fixed effect. We are thus comparing an analyst's forecasts to the forecasts they posted for the same firm in the same calendar month (e.g. September), rather than across the entire year. Our specification remains otherwise unchanged. The results can be found in Figure 5; controlling for this time variation does not significantly affect our point estimates, nor the statistical significance of our results.

4.3.2 Unexpected Game Outcomes

One would expect that the size of the treatment effect varies depending on whether the game outcome was 'expected' or not. To establish a notion of 'close' games, we draw on odds from betting markets prior to the NCAA Championship game.[16] Odds are generally reported relative to a fair $100 bet. If betting on Alabama winning over LSU currently has odds $-\$X$, a bet of $\$100 + X$ will result in a profit of $100 in case of payout. Negative odds can therefore be interpreted as the corresponding team being expected to win. In our sample, the odds for the winning team range from -10 (Alabama in 2013) to 11 (Ohio State in 2003), with a mean of -0.17. Naturally, the odds for the losing team are derived by multiplying those of the winner by negative one. Our findings are documented in Table 9.

Our results suggests that while winners are seemingly unaffected by the odds of their victory, losers react more strongly if their team was expected to win (i.e. the odds for a bet on their team were negative). Whilst in our static estimation, detailed in Table 7, we fail to find a statistically significant impact for losers, we now find a highly statistically significant and negative effect on subsequent forecasts, with more unlikely losses (i.e. negative loser odds) deepening the negative effect, and vice versa.

4.3.3 Leave-One-Out Estimation

One concern with our identification strategy is the relatively limited number of different schools that made it to the NCAA finals in our sample period (see Table 5). While the concern that individual schools are driving the results does not invalidate our approach as such, we strive to demonstrate that the treatment effect is not due to a single school. To alleviate such concerns, we return to our dynamic regressions specification in (2), dropping individual

[16] We obtain the odds on the day of the game from *The Lines*.

Table 9: Robustness - Unexpected Wins/Losses

This table reports coefficient estimates from the following regression, $\mathbb{E}_{i,j,t}[EPS_{j,y}] = \phi_{i,j} + \tau_t + \alpha_w w_{i,y} + \alpha_w^o w_{i,y} \times odds_c^w + \alpha_l l_{i,y} + \alpha_l^o l_{i,y} \times odds_c^l + \beta_w \widehat{W}_{i,y} + \beta_l \widehat{L}_{i,y} + \Gamma X_{j,y} + u_{i,j,t}$. $w_{i,y}$ ($l_{i,y}$) is a dummy that takes on a value of one for an analyst that is a winner (loser) of the college championship game. $odds_c^x$ is the odds of college c attended by analyst i either winning ($x = w$) or losing ($x = l$) the NCAA final. Standard errors are clustered at the firm-analyst-month-level.

Dependent Variable:		$\mathbb{E}_{i,j,t}[EPS_{j,y}]$	
Model:	(1)	(2)	(3)
Variables			
Win	0.1154***		0.1035***
	(0.0124)		(0.0132)
Win × Winner Odds	-0.0018		0.0002
	(0.0020)		(0.0020)
Lose		-0.0645***	-0.0447***
		(0.0150)	(0.0158)
Lose × Loser Odds		0.0157***	0.0138***
		(0.0023)	(0.0024)
Controls	Yes	Yes	Yes
Fixed-effects			
Firm-Analyst-Month	Yes	Yes	Yes
Month-Year	Yes	Yes	Yes
Controls			
	Yes	Yes	Yes
Fit statistics			
Observations	3,072,433	3,072,433	3,072,433
R^2	0.98542	0.98542	0.98542

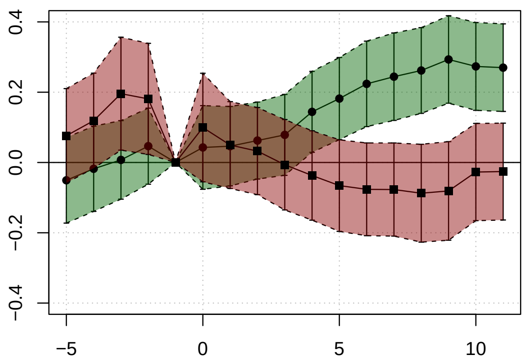
Clustered (Firm-Analyst-Month) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

universities from the sample. Figure 6 presents the dynamic difference-in-differences plots for the ten samples when removing one of the finalists from Table 5.

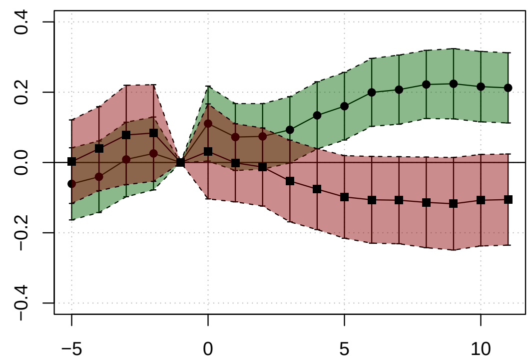
As can be seen from Figure 6, the dynamic effect does not change substantially, in size and significance, when removing any of the focal schools. We therefore conclude that the

Figure 6: Robustness - Leave-One-Out

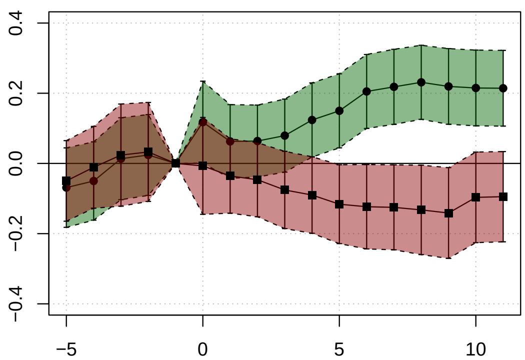
We assess the robustness of our results by re-estimating the dynamic effects when leaving out all analysts associated with one of the schools that won/lost the NCAA final. The panels in this figure correspond to the analogues of Figure 3 but with all observations related to the focal college removed from the sample.



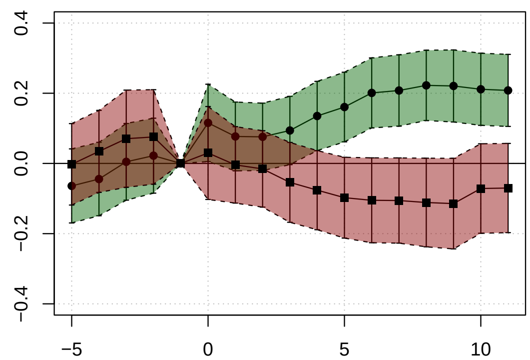
(a) Alabama



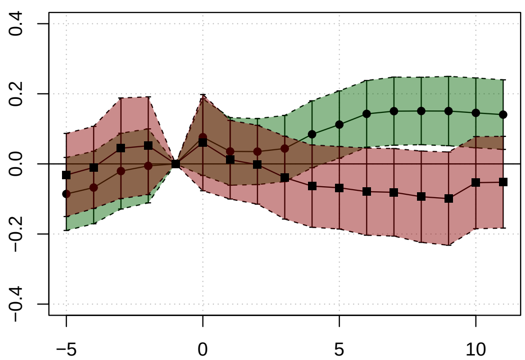
(b) Auburn



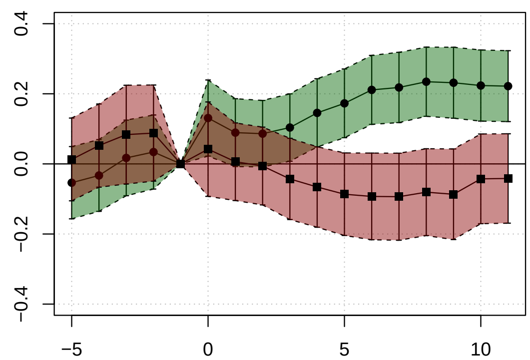
(c) Clemson



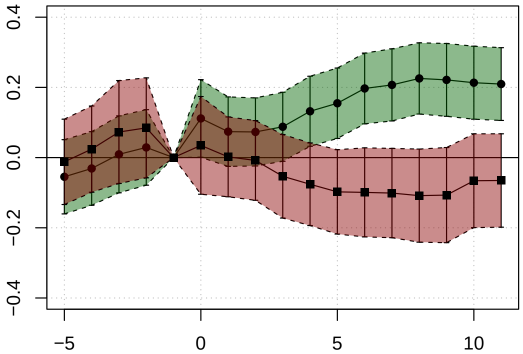
(d) Florida State



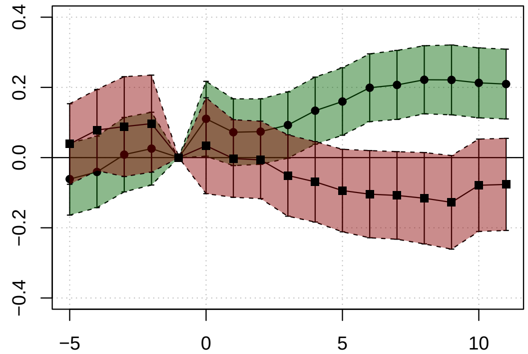
(e) LSU



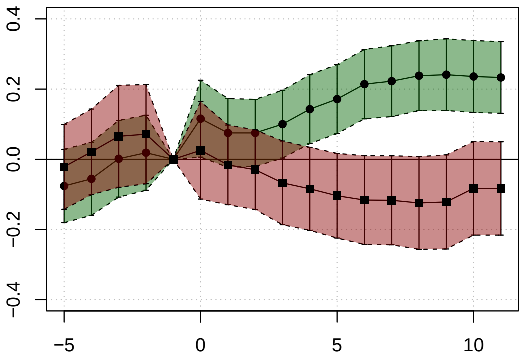
(f) Miami



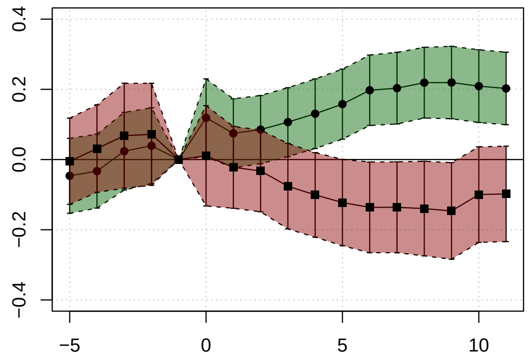
(g) Ohio State



(h) Oklahoma



(i) USC



(j) Texas

results aren't driven by any single college.

4.3.4 Excluding US-based Analysts Outside of New York

To alleviate concerns that our results are driven by brokerages located in close proximity to colleges that win/lose the NCAA final, we exclude all US-based analysts outside of New York. In this respect, we limit the possibility that the local atmosphere is driving the forecast results.

Table 10 presents our findings from a simple difference-in-differences estimation where we once again find a positive and statistically significant brokerage effect. We also report

Table 10: Robustness - NY and International Analysts

This table reports coefficient estimates from the following regression, $\mathbb{E}_{i,j,t}[EPS_{j,y}] = \phi_{i,j} + \tau_t + \alpha_w w_{i,y} + \alpha_l l_{i,y} + \beta_w \widehat{W}_{i,y} + \beta_l \widehat{L}_{i,y} + \Gamma X_{j,y} + u_{i,j,t}$, excluding from the sample all US-based analysts operating outside of New York. $w_{i,y}$ ($l_{i,y}$) is a dummy that takes on a value of one for an analyst that is a winner (loser) of the college championship game. $\widehat{W}_{i,y}$ ($\widehat{L}_{i,y}$) represents the proportion of winners (losers) that analyst i works alongside in year y. Standard errors are clustered at the firm-analyst-level.

Dependent Variable:		$\mathbb{E}_{i,j,y}[EPS_{j,y}]$	
Model:	(1)	(2)	(3)
Variables			
$w_{i,y}$	0.0504***		0.0486***
	(0.0122)		(0.0128)
$\hat{W}_{i,y}$	0.3587***		0.3640***
	(0.0525)		(0.0529)
$l_{i,y}$		-0.0245	-0.0132
		(0.0169)	(0.0174)
$\hat{L}_{i,y}$		0.0514	0.0690
		(0.0643)	(0.0648)
Controls	Yes	Yes	Yes
Fixed-effects			
Ticker-Analyst-Month	Yes	Yes	Yes
Month-Year	Yes	Yes	Yes
Fit statistics			
Observations	2,509,621	2,509,621	2,509,621
R^2	0.98811	0.98811	0.98811

Clustered (Ticker-Analyst-Month) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

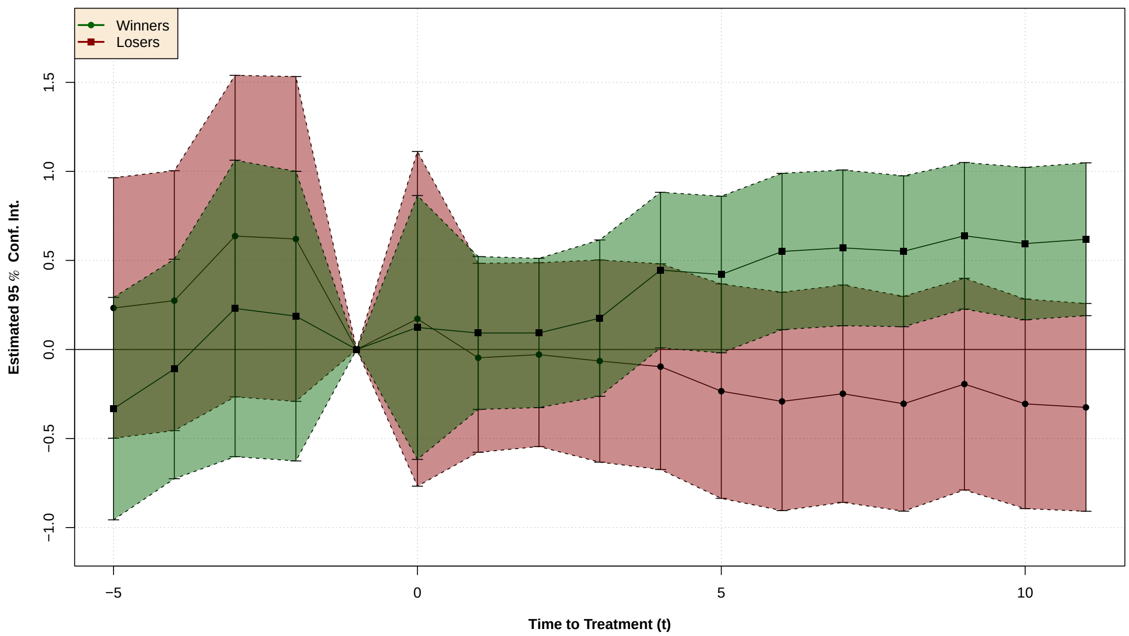
the event study plot in Figure 7, where the same basic pattern emerges.

4.3.5 Placebo Test

We run two placebo tests designed to shed light on the plausibility of our two results, i.e. that 'winners' react to the NCAA Championship game, and that this effect spreads to their

Figure 7: Placebo Test - NY and International Analysts

This figure presents coefficient estimates from the following regression $\mathbb{E}_{i,j,t}[EPS_{j,y}] = \phi_{i,j} + \tau_t + \alpha_w w_{i,y} + \alpha_l l_{i,y} + \beta_w \widehat{W}_{i,y} + \beta_l \widehat{L}_{i,y} + \Gamma X_{j,y} + u_{i,j,t}$, excluding from the sample all US-based analysts operating outside of New York. $w_{i,y}$ ($l_{i,y}$) is a dummy that takes on a value of one for an analyst that is a winner (loser) of the college championship game. $\widehat{W}_{i,y}$ ($\widehat{L}_{i,y}$) represents the proportion of winners (losers) that analyst i works alongside in year y. Standard errors are clustered at the firm-analyst-level.

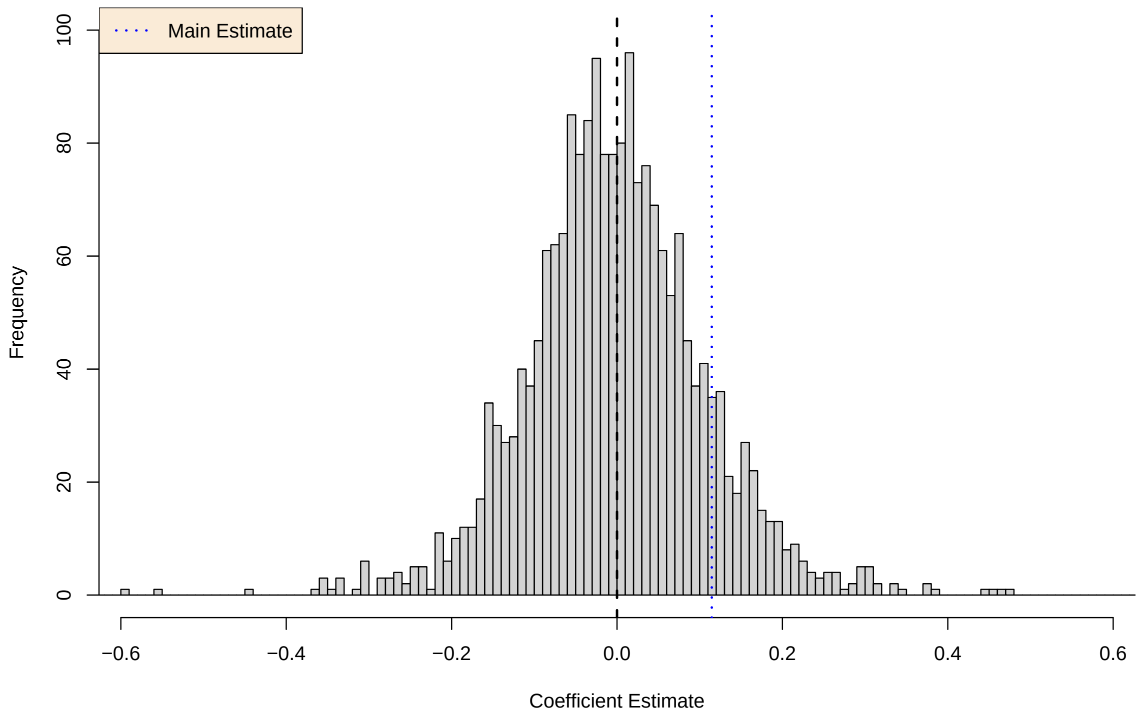


colleagues. In the first, we randomly assign the indicator $w_{i,y}$ to 'non-winners,' and set $w_{i,y}$ equal to zero for known 'winners.' We maintain the same proportion of 'winners' to 'non-winners' in our placebo test. In the second, we randomly assign a proportion of 'winners' in a given brokerage-year, $\widehat{W}_{i,y}$, to brokerages that we know had no 'winners' in that year, and set $\widehat{W}_{i,y}$ equal to zero for brokerages with known non-zero proportions. When randomly assigning proportions of 'winners,' we resample $\widehat{W}_{i,y}$ from the empirical distribution. In both cases, we perform these tests 2,000 times and report the distribution of coefficients.

Figure 8 presents the results of our first exercise. The distribution of estimates is centered

Figure 8: Placebo Test - Effect on 'Winners'

This figure presents coefficient estimates of our first placebo test. In that exercise, we randomly assign a winner indicator, $w_{i,y}$, to known non-winners, whilst setting the same indicator to zero for all known winners. The specification is then the same as in our main analysis: $\mathbb{E}_{i,j,t}[EPS_{j,y}] = \phi_{i,j} + \tau_t + \alpha_w w_{i,y} + \alpha_l l_{i,y} + \Gamma X_{j,y} + u_{i,j,t}$. We repeat this 2,000 times and report the distribution of our coefficient of interest, α_w, in the figure. We find a distribution of coefficients centered around zero, with a mean of -0.0005, and a median value of -0.0044. We include our coefficient estimate from our main analysis (0.1212) as the dotted line to the right of 0. Roughly 87.5% of our placebo test coefficient estimates are lower than our main estimate.



around zero, with 87.5% of the estimated coefficients lying to the left of our main analysis estimate. It is worth noting that, across our entire sample, we have very few 'winners' relative to the size of the control group (see Table 6). This may explain why the distribution of placebo estimates is fairly broad, and why there are not an insignificant number of estimates that exceed our results in the non-placebo case.

Figure 9: Placebo Test - Brokerage Spillover

This figure presents coefficient estimates of our second placebo test. In this exercise, we randomly assign a proportion of winners, $\widehat{W}_{i,y}$, to brokerage-years that we know to have no winners, whilst setting the same proportion to zero for all brokerage-years with known winners. The specification is then the same as in our main analysis: $\mathbb{E}_{i,j,t}[EPS_{j,y}] = \phi_{i,j} + \tau_t + \alpha_w w_{i,y} + \alpha_l l_{i,y} + \beta_w \widehat{W}_{i,y} + \beta_l \widehat{L}_{i,y} + \Gamma X_{j,y} + u_{i,j,t}$. We repeat this 2,000 times and report the distribution of our coefficient of interest, β_w, in the figure. We find a distribution of coefficients centered around zero, with a mean of 0.00005, and a median value of 0.01159. We include our coefficient estimate from our main analysis (0.4115) as the dotted line to the right of 0. Roughly 99.65% of our placebo test coefficient estimates are lower than our main estimate.

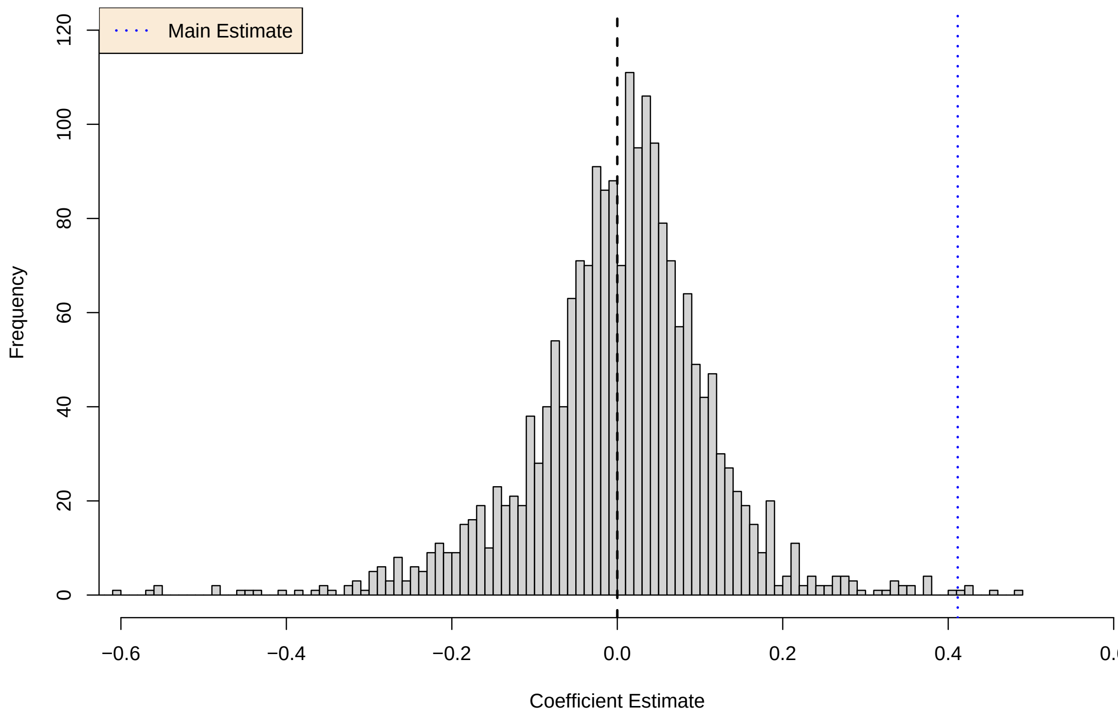


Figure 9 shows the results of our second exercise. The distribution of estimates is centered around zero, with 99.65% of the estimated coefficients lying to the left of our main analysis estimate. In comparison to our first case, we have many more brokerage years affected by the variable $\widehat{W}_{i,y}$, compared to $w_{i,y}$. This feature of our second exercise offers one explanation for why the distribution over placebo coefficients is significantly tighter around zero, as compared to the first.

5 Within-Brokerage Spillovers and 'Bro-ness'

Having established that the college football shock influences winning/losing analysts, and that the shock of winning appears to spread to peers who work alongside winners, we now turn to investigating whether specific brokerages react more or less to the shock of having a colleague 'winner.'

We are interested in the brokerage characteristics that are associated with an environment in which spurious shocks to individual analyst spill over to their coworkers. To do this, we first need to quantify the degree to which the shocks discussed in Section 3 propagate through the network of fellow analysts. We do so by estimating the following regression specification, which includes a brokerage-specific coefficient β_b:

$$\mathbb{E}_{i,j,t}[EPS_{j,y}] = \phi_{i,j} + \tau_t + \beta_0 \widehat{W}_{i,y} + \sum_{b \in B} \beta_b \widehat{W}_{i,y} \times \mathbb{1}\{i, t \in b\} + \Gamma X_{j,y} + u_{i,j,t} \tag{4}$$

In the specification above, i indexes analysts, j indexes firms that are covered by analysts, and b indexes brokerages.

Most importantly for us, $\mathbb{1}\{i, t \in b\}$ is a dummy variable which takes on a value of one if analyst i is working at brokerage b in year t. We restrict our attention to brokerages that have at least one non-zero observation for $\widehat{W}_{i,y}$.

Since analyst-brokerage relationships are sticky and there are a limited number of brokerages, we construct our measure (the 'Bro Score') as a time-invariant variable.[17] Furthermore, since $\widehat{\beta}_b$ is a noisy measure, we rank the coefficients and derive a relative ranking of brokerages, a measure we term 'Bro Rank.' Table 11 reports this ranking, the estimated 'Bro Score,' as well as the number of analysts employed at these brokerages and the unique firms covered by them.

Armed with our measures, we are interested in brokerage characteristics that are associated with particularly low/high levels of 'Bro-ness.' As discussed in the introduction, males are disproportionately more likely to describe themselves as 'avid' fans of college sports than females.[18] With this in mind, we use the gender of the analysts we were able to identify to construct the share of female analysts at the brokerages. We then test whether firms with higher 'Bro Ranks' have more or less female representation than those with lower values of the measure.

We move on to using data from MSCI/KLD to construct ESG (Environment, Social, and Governance) scores of the brokerages we have 'Bro Scores' for. ESG scores track the number of strengths and concerns that a given business has across six dimensions of ESG concerns: (1) environmental, (2) human rights, (3) diversity, (4) governance, (5) employment relations, and (6) community engagement. The score for each dimension is simply the sum of strengths within that dimension, minus the sum of concerns. Strengths and concerns are binary, taking only a value of one or zero. Overall ESG scores are constructed by simply summing the scores of all six dimensions. Using this data, we test whether brokerages with higher 'bro scores' are more or less 'responsible' in their business practices.

[17] The average number of brokerages an analyst works at in our sample is 1.18, the corresponding figure for IBES is slightly greater at 1.49, partly due to brokerage mergers, which are handled differently therein.

[18] 37% of men describe themselves as 'avid' fans, vs. 14% of women. These figures were reported in a recent (January 2023) poll of 2,201 American adults, published by Morning Consult, a global decision intelligence company.

Table 11: Bro Score & Bro Rank

This table reports the ranking of brokerages in our sample based on a measure, which we call the 'Bro Score.' This index captures the ease with which spurious shocks to one analyst at the brokerage spill over to their colleagues. It corresponds to the estimate $\widehat{\beta}_b$, which we obtain from the following regression $\mathbb{E}_{i,j,t}[EPS_{j,y}] = \phi_{i,j} + \tau_t + \beta_0 \widehat{W}_{i,y} + \sum_{b \in B} \beta_b \widehat{W}_{i,y} \times \mathbb{1}\{i \in b\} + \Gamma X_{j,y} + u_{i,j,t}$. We further report the number of analysts working at these firms and the number of unique firms covered at any point over the 2000-2022 sample period.

Rank	Name	# Firms Covered	# Analysts	Bro Score
1	Prudential Equity Group	156	15	19.84
2	Sidoti & Co	1162	179	18.53
3	Piper	54	6	16.75
4	Stifel	1314	125	16.24
5	RBC	1484	198	15.94
6	BMO	229	25	15.64
7	Oppenheimer	1307	124	15.16
8	Goldman Sachs	1792	318	14.15
9	Deutsche Bank	7	4	14.15
10	Raymond James	826	69	14.09
11	Canaccord	29	4	14.06
12	Wells Fargo	1354	158	14.03
13	AG Edwards	7	4	13.78
14	HSBC	115	55	12.56
15	Merrill Lynch	1983	437	12.52
16	Macquarie	3	2	12.52
17	Jefferies	1791	234	12.48
18	B Riley Securities	695	61	11.22
19	Credit Suisse	614	65	10.90
20	William Blair & Co	762	73	10.55
21	Morningstar Investment	820	97	9.78
22	Banc of America	839	87	8.11
23	Barclays	1274	147	2.25
24	Lehman Brothers	41	7	2.25
25	Morgan Stanley	6	2	1.86
26	Suntrust Robinson	289	18	0.79

In a similar vein, we also use data from (Li et al., 2021) on corporate culture measures to test whether our 'Bro Scores' are associated with any of the five measures documented in that paper. Those dimensions are: (1) Integrity, (2) Teamwork, (3) Innovation, (4) Respect, and (5) Quality. These scores are increasing in salience, i.e. a high score for teamwork indicates the positive presence of that quality.

To assess the relationship between these brokerage-level variables and our index, we regress the measures of interest on the index as well as the log of the index.

$$measure_{b,y} = \tau_t + bro\_index_b + \Gamma X_{b,y} + \varepsilon_{b,y} \tag{5}$$

We are regressing the measure on the index, rather than vice versa, to exploit variation in the former over time. We control for a year fixed effect, τ_y, and include a number of brokerage-level controls: specifically, the number of analysts at the brokerage, the number of firms the brokerage covers, the number of forecasts that brokerage reports, and the average size (in total assets), book-to-market, earnings, cash holdings, and book-value-per-share of the firms that the brokerage covers.

5.1 Female Representation

We first look at female representation; the coefficient estimates are reported in Table 12. We find that the larger the 'Bro Score,' and the higher the 'Bro Rank,' the lower the proportion of female analysts working at the brokerage. The size of the coefficient is economically significant: moving from the brokerage with the lowest rank on the 'Bro Score' to the brokerage with the highest is associated with a fall in the overall proportion of female analysts of 6.24%. Given that the average proportion of female analysts within brokerages across our sample period is only 13.7%, this is a sizeable gap across the index.

We illustrate our findings further in Figure 10, were we plot the 'Bro Rank' and the percentage of female analysts of the brokerage along with a line of best fit. A clear upward

Table 12: Female Representation and 'Bro Score'

This table presents coefficient estimates from the regression of female representation as a percentage at a given brokerage on our 'Bro Rank' and 'Bro Score.' These measures capture the degree to which our college football shock diffuses between analysts working at the same brokerage. We control for the average book-to-market, size, earnings, cash and short-term asset holdings, and dividends-per-share of the firms the brokerage covers, and the number of firms, analysts, and forecasts that the brokerages make. Standard errors are clustered at the year-level.

Dependent Variable:	Proportion of Females	
Model:	(1)	(2)
Variables		
'Bro Rank'	0.0024***	
	(0.0004)	
log('Bro Score')		-0.0287***
		(0.0045)
Controls	Yes	Yes
Fixed-effects		
Year	Yes	Yes
Fit statistics		
Observations	415	352
R^2	0.28471	0.31468

Clustered (Year) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

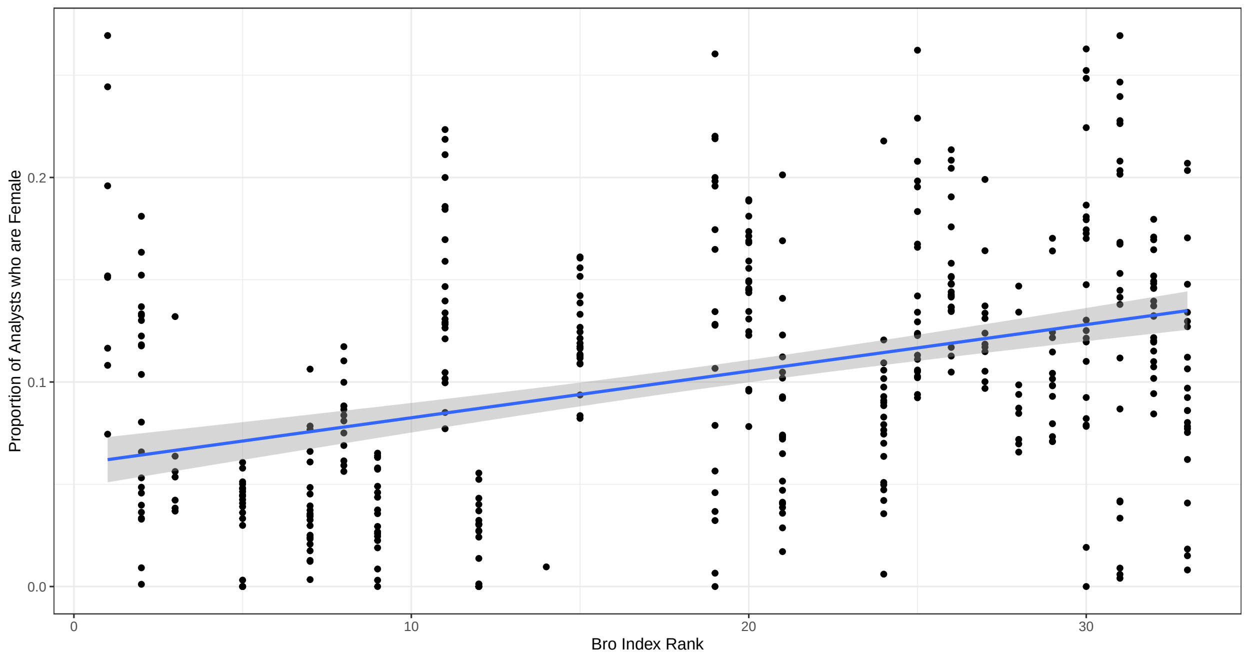
slope is apparent, again consistent with brokerages with a higher 'Bro Rank' having fewer female analysts working at that brokerage.

5.2 ESG Scores

We now turn to the ESG profiles of the brokerages, investigating whether our 'Bro Score' correlates with any one of the six dimensions mentioned above, as well as with the aggregated ESG score. Our results for ESG can be found in Table 13. We find that brokerages

Figure 10: Female Representation and 'Bro Rank'

This figure plots brokerage 'Bro Rank' alongside the proportion of female analysts who work at the brokerage. We describe the construction of our 'Bro Rank' measure in Section 5. This measure is designed to capture the strength of the diffusion of the shock of winning the NCAA College Football National championship game to colleagues of analysts who were 'winners;' i.e. attended the winning college.



with higher 'Bro Scores' have lower MSCI/KLD scores in the environmental, employment relations, and community engagement dimensions, higher MSCI/KLD scores in the human rights dimension, and lower ESG scores overall.

5.3 Measures of Corporate Culture from Li et al. (2021)

Finally we consider the corporate culture scores constructed in Li et al. (2021). We take the log of these scores so that our coefficients have a natural interpretation. Our results can be found in Table 14. Consistent with the idea of sentiment diffusion occuring amongst

Table 13: ESG Scores and 'Bro Score'

This table presents coefficient estimates from the regression of ESG Scores as collected by MSCI/KLD on our 'Bro Score' variable. This 'Bro Score' measure captures the degree to which our college football shock diffuses between analysts working at the same brokerage. The MSCI 'ESG' scores measure the number of strengths across various ESG dimensions net of the number of concerns in those dimensions. These ESG scores are constructed according to the six dimensions outlined by MSCI/KLD: (1) environmental, (2) human rights, (3) diversity, (4) governance, (5) employment relations, and (6) community engagement. Standard errors are clustered at the year-level. We use the same controls as in Table 12.

Dependent Variables:	ENV	HUM	DIV	GOV	EMP	COM	ESG
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Variables							
log('Bro Score')	-0.3068***	0.2254***	-0.1035	-0.0293	-0.2884***	-0.4083***	-1.136***
	(0.1064)	(0.0727)	(0.1484)	(0.0842)	(0.0851)	(0.0557)	(0.2854)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed-effects							
Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics							
Observations	188	158	188	188	188	188	188
R^2	0.37386	0.33001	0.43420	0.55275	0.55842	0.42385	0.40704

Clustered (Year) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

peers who interact day-to-day, brokerages with higher 'Bro Scores' have significantly higher teamwork scores. Perhaps surprisingly, given stereotypes surrounding sports culture, we also find that higher 'Bro Scores' correlate with higher 'Respect' scores. However, it is worth noting that this result is broadly consistent with our finding on the 'Human' component of ESG scores, as documented in Section 5.2.

Taken collectively, these results suggest that observable features of a workplace social network help to explain the diffusion of sentiment shocks within that network.

Table 14: Corporate Culture Scores from (Li et al., 2021)

This table presents coefficient estimates from the regression of Corporate Culture Scores, taken from Li et al. (2021), on our 'Bro Score' variable. This 'Bro Score' measure captures the degree to which our college football shock diffuses between analysts working at the same brokerage. The Corporate Culture scores pertain to five key aspects of culture: (1) Integrity, (2) Teamwork, (3) Innovation, (4) Respect, and (5) Quality. Standard errors are clustered at the year-level. We use the same controls as in Table 12.

Dependent Variables: Model:	log(Integrity) (1)	log(Teamwork) (2)	log(Innovation) (3)	log(Respect) (4)	log(Quality) (5)
Variables					
log('Bro Score')	0.1823	0.3977***	0.2326***	1.304***	-0.1706
	(0.1296)	(0.1335)	(0.0787)	(0.1075)	(0.1059)
Controls	Yes	Yes	Yes	Yes	Yes
Fixed-effects					
Year	Yes	Yes	Yes	Yes	Yes
Fit statistics					
Observations	375	375	375	375	374
R^2	0.18676	0.17147	0.35269	0.32905	0.13029

Clustered (Year) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

6 Firm and Market Responses

In this section, we assess whether forecast changes induced by the college football shock lead to firm or stock market level responses. We begin by investigating firm-level responses. Several papers have documented that managers engage in earnings management to meet, or attempt to meet, analyst forecasts (Almeida et al., 2016; Bhojraj et al., 2009; Shore, 2023). Using our novel source of plausibly exogenous variation in analyst forecasts, we run an instrumental variable regression to see if earnings respond in a fashion consistent with previous work. We confirm that this is the case: earnings respond close to one-to-one to forecast shocks. Subsequently, we assess how the stock market reacts to the forecast shock. Previous findings are inconclusive on the degree to which the stock market parses out information from analyst forecasts (Gleason and Lee, 2003; Shore, 2023; So, 2013). Consistent with Shore (2023), we find that the stock market does not react to the forecast shock. We interpret this evidence as supporting the claim that investors are able to parse out variation in forecasts driven by sentiments rather than fundamentals.

6.1 Firm Level Response

To identify a firm-level reaction to forecast shocks, we implement an instrumental variable design. Our identifying assumption is that the number of analysts that 'won' the NCAA National Championship game that cover a firm in a given year is orthogonal to the business conditions of that firm. As such, the instrument that we use is precisely that number, which we label $W_{j,y}$.[19] Let $A_{j,y}$ be the set of analysts that cover firm j in year y. Then $W_{j,y}$ is defined in the following way:

$$W_{j,y} = \sum_{i \in A_{i,j}} w_{i,y} \tag{6}$$

To test for the relevance of our instrument, we begin by conducting a first stage regression of our instrument on the consensus earnings forecast for a given firm-year, controlling for a firm and a year fixed effect, as well as several firm-year level covariates. Here we make use

[19]We choose the sum of winners rather than the proportion in this case, as our matched dataset of analysts to college attendance is only a subsample of the universe of analysts. Given that we are interested in looking at reactions to changes in the consensus forecast, which naturally includes the universe of non-stale analyst forecasts, we view the sum of 'winners' covering a firm to be a more appropriate measure of firm exposure, as it does not rely as explicitly on the size of the matched sample. In a robustness check, we run the same exercise using the proportion of 'winners,' and find similar point estimates, albeit with a weaker first stage, and hence limited statistical significance. This is not all together surprising, given the aforementioned concerns surrounding our use of a subset of forecast data.

of the IBES Summary dataset to collect the IBES consensus forecasts; this is the consensus forecast that is typically used for market tests (Bartov et al., 2002; Brown, 2001; Lim, 2001). We use the most recent consensus forecast prior to the forecast period end-date as the measure that earnings performance are compared against; again, this is precisely because market tests are typically performed relative to this measure. We use the mean forecast (IBES Summary item 'MEANEST') in our main analysis, although the results are near identical if we instead use the median forecast (IBES Summary item 'MEDEST'). As in the exercises conducted above, we standardize the consensus earnings forecast, and the earnings variable itself, this time at the firm level, to avoid problems associated with scale. We then run a standard instrumental variable regression.

Our findings are presented in Table 15. We report results for both unadjusted values of the consensus forecast and firm-level earnings-per-share, as well as for the standardized variables. In both cases, we find an F-statistic that is above the Stock and Yogo (2002) cutoff of 15, meeting the requirement of strong instruments. We find that for a one standard deviation increase in the consensus forecast, earnings increase by 0.8019 standard deviations, and we cannot rule out a one-to-one relationship.

6.2 Asset Market Response

While there is ample evidence that the information contained in analyst forecasts can move markets (Gleason and Lee, 2003; So, 2013), in a fully-rational benchmark without informational frictions, investors would be able to filter out any noise in these forecasts and form beliefs based exclusively on the relevant new information. If this was the case, any spurious increases in forecasts should leave asset prices unaffected.

To address the question of how changes in forecasts that are unrelated to company funda-

Table 15: IV Estimation - Firm Response

This table presents our instrumental variable regression of firm-level earnings, $EPS_{j,y}$, on the consensus analyst earnings forecast, $\mathbb{E}_{j,y-1}[EPS_{j,y}]$, where the consensus forecast is instrumented using the number of analysts who cover firm j in year y who attended the college that won the NCAA National Championship Football game in the year y, $W_{j,y}$. We control for a firm and a year fixed effect, market value, book-to-market, lagged assets, stock price, and dividends per-share, and we cluster standard errors at the firm level. The consensus forecast is taken from the IBES Summary dataset.

Dependent Variables:	$\mathbb{E}_{j,y-1}[EPS_{j,y}]$	$EPS_{j,y}$	Standardized $\mathbb{E}_{j,y-1}[EPS_{j,y}]$	Standardized $EPS_{j,y}$
IV stages	First	Second	First	Second
Model:	(1)	(2)	(3)	(4)
Variables				
$W_{j,y}$	0.0648***		0.0235***	
	(0.0124)		(0.0060)	
$\mathbb{E}_{j,y-1}[EPS_{j,y}]$		0.9467***		
		(0.2513)		
Standardized $\mathbb{E}_{j,y-1}[EPS_{j,y}]$				0.8019***
				(0.2422)
Controls	Yes	Yes	Yes	Yes
Fixed-effects				
Firm	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
Fit statistics				
Observations	52,053	52,053	51,710	51,710
R^2	0.70372	0.54320	0.98575	0.97609
F-test (1st stage)	57.443	57.443	17.216	17.216
Wu-Hausman, p-value		0.55886		0.38183

Clustered (Ticker) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

mentals affect markets, we conduct another instrumental variable exercise. Our approach is broadly in line with the procedure detailed in Section 6.1, though we substitute firm returns for firm earnings as our dependent variable. Again, our identifying assumption is that the number of analysts that won the NCAA National Championship game that cover a firm in a given year is orthogonal to the business conditions of that firm.

Following Carhart (1997) and Fama and French (2015) we compute the exposure of all stocks to the four most common risk factors. Using a rolling monthly estimation procedure, we estimate factor loadings by regressing the excess return on stock j at time t on the following factors:

$$r_{j,t} - r_{rf,t} = \alpha_j + \beta_{j,m}(r_{m,t} - r_{rf,t}) + \beta_{j,smb} SMB_t + \beta_{j,hml} HML_t + \beta_{j,mom} MOM_t \tag{7}$$

The rolling window that we select is the 60 months prior to the month of the return. Using the coefficient estimates $\left(\widehat{\beta}_m, \widehat{\beta}_{smb}, \widehat{\beta}_{hml}, \widehat{\beta}_{mom}\right)^\top$ we compute the abnormal return of stock j by subtracting the expected from the realized return.

$$AR_{j,t} = r_{j,t} - \mathbb{E}[r_{j,t}] \tag{8}$$

We then run our instrumental variable regression using both raw returns, as well as our constructed abnormal returns, as the dependent variable. We control for a firm fixed effect, an industry-month-year fixed effect, and a host of firm-year level covariates. See Table 16 for the coefficient estimates.

Our results are consistent with the stock market correctly interpreting the forecast shock as spurious; we fail to find evidence that the shock to the forecast moves either raw or abnormal returns, despite high power in our first stage (our first stage F-statistic is 194.96). This finding is consistent with results in Shore (2023) who also shows that the stock market does not react to plausibly exogenous variation in analyst forecasts.

Table 16: IV Estimation - Asset Market Response

This table presents the results of estimating an instrumental variable regression of monthly asset returns on the standardized consensus analyst earnings forecast ($\mathbb{E}_{j,y-1}[EPS_{j,y}]$), where we standardize at the firm-level. We show results for two measures of asset returns: raw returns, $r_{j,t}$, of firm j in month-year t; and abnormal returns, $AR_{j,t}$, of firm j in month-year t, where abnormal returns are constructed based on a Carhart (1997) four factor model. We outline our procedure for constructing these abnormal returns in Section 6.2. We instrument for changes in the consensus forecast using the variable $W_{j,y}$, which measures the number of analysts that cover firm j in the year y who attended the college that won the NCAA College Football Championship Game in the year y. We control for estimated rolling betas, $\{\beta_0, \beta_{mkt}, \beta_{smb}, \beta_{hml}, \beta_{mom}\}$, book-to-market, price, assets, cash and short-term asset holdings, dividends-per-share, and past volatility.

Dependent Variables: Model:	Raw Returns (1)	Abnormal Returns (2)	Raw Returns (3)	Abnormal Returns (4)
Variables				
$\mathbb{E}_{j,y-1}[EPS_{j,y}]$	-0.0015	-0.0046		
	(0.0066)	(0.0061)		
Standardized $\mathbb{E}_{j,y-1}[EPS_{j,y}]$			-0.0027	-0.0083
			(0.0120)	(0.0112)
Controls	Yes	Yes	Yes	Yes
Fixed-effects				
Industry-Month-Year	Yes	Yes	Yes	Yes
Firm	Yes	Yes	Yes	Yes
Fit statistics				
Observations	499,768	499,768	497,898	497,898
R^2	0.44844	0.28842	0.44959	0.28827
Within R^2	0.02953	0.01314	0.02939	0.01178
F-test (1st stage)	290.37	290.37	194.96	194.96
Wu-Hausman, p-value	0.46141	0.25112	0.52634	0.28797

Clustered (Industry-Month-Year) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

7 Discussion

The relevance of social networks has received increased attention in the Economics and Finance literature recently. However, analyses have been limited to the spillover of potentially valuable and actionable information through networks, constituting a form of peer learning. To the best of our knowledge, we are the first to provide evidence of spurious information cascading through a network of professionals, which in turn act on said sentiment in a high stakes environment.

Bailey et al. (2018a,b) are among the first to provide tangible evidence on the relevance of social ties, which they measure using the social connectedness between individuals across regions derived from the Facebook social graph. Specifically, they find that house price expectations are very much driven by social interactions. These expectations then influence both the choice between renting and owning, as well as leverage when obtaining a mortgage. While it can be argued that the experiences of (geographically) distant friends are not directly applicable to the local housing market, there unarguably is portable knowledge that is applicable across different national housing markets. Their findings therefore are different from ours in two important ways. Firstly, while real estate is the most important asset in the portfolio of most households, making the purchase decision a very high stakes one, households can still be considered unsophisticated market participants potentially putting too much weight on information obtained through their social network. In contrast, the individuals in our setting are professionals and thus, arguably, less susceptible to irrelevant information. Secondly, as argued above, if there are indeed common factors across housing markets, relying on the experiences of others, even in distant locations, can improve the accuracy of expectations. In our setting, on the other hand, the shocks are orthogonal to information relevant to the decision at hand, making it all the more puzzling that individuals react to them in the first place.

Further evidence on the reaction of professionals to outside information has been provided by Kempf and Tsoutsoura (2021). In considering credit analysts, their setting is similar to ours in that the individuals under consideration have extensive experience in financial markets and have to make decisions that influence assets worth many millions to billions of dollars. However, in contrast to our sentiment shock, the authors consider the impact of partisanship on the analysts' decisions. Specifically, they compare credit rating made by analysts that are registered with either the democratic or republican party around changes in presidency. While their results are similar to ours, i.e. democrat (republican) analysts give better ratings when a democratic (republican) candidate takes office, it is hard to argue that the color of the white house is unrelated to the corporations they cover. There obviously is reason to believe that these reactions are driven by the different expectations regarding the future wellbeing of the economy. It thus cannot be concluded that any kind of reaction is spurious. Our setting is different from the aforementioned ones in that the shock we consider is orthogonal to firm fundamentals and entirely sentiment-driven in nature.

Similarly, our results are different from the recent literature on the reaction of sell-side analysts to local geographic shocks. For example, Cuculiza et al. (2021) focus on the impact of terrorist attacks on earnings forecasts, but do so at a local level; they find that analysts who are closer to a terrorist event are affected more strongly than analysts located farther away. Similarly, Kong et al. (2021) show that proximity to earthquakes lowers analysts' optimism. Whilst consistent with our findings that analyst forecasts are influenced by personal shocks that are plausibly independent of the business conditions of the firms they cover, it is difficult to see how these broad shocks could be used to identify network spillovers, precisely because the shock affects the individual as well as her network.

8 Conclusion

In this paper we provide evidence that analysts not only react to shocks that are specific to themselves, but also show that such shocks spill over to other analysts, specifically their colleagues, through their social networks. We do so using a novel, hand-collected dataset on the college attendance of 7,481 analysts over the 2000-2021 period. To the best of our knowledge, the shock we use, whether an analyst's college football team wins the NCAA finals, similarly is new to the literature.

Our findings support the claim that analyst forecasts are subject to bias and sentiment in much the same fashion as many economic decisions. This is surprising since the analysts in our sample are professionals engaged in a high stake setting, with many billions of dollars of assets being managed following their guidance. In this paper, we also show that analysts are influenced by the sentiments of agents in their social network, consistent with evidence from related work. While peers naturally react less strongly to these shocks, the effects are statistically significant and economically meaningful. Being able to trace out the flow of information between equity analysts is a major novelty in our approach.

With respect to these network effects, we find that the ease with which these spurious shocks propagate to other analysts at the same brokerage correlates with observable characteristics of the work environment. Brokerages where the degree of this diffusion is greater have lower female representation in their analyst teams, as well as lower ESG scores. Finally, we substantiate existing evidence that analyst forecasts causally influence the economic decision making of firms, while only finding limited support for a similar relationship with investors.

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