The Impact of Return-to-Office Mandates on Equity Analysts[*]

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Abstract

Using a difference-in-differences design, we examine how brokerage firms' return-to-office (RTO) mandates affect analysts' forecast quality. We find that RTO mandates improve forecast accuracy, particularly for younger, less experienced, and female analysts, as well as those residing in Democratic states or facing greater time pressure. The effect is stronger for stocks that are less central to analysts' careers, suggesting strategic effort allocation. RTO mandates also enhance forecast timeliness. These findings support the hypothesis that in-office work increases labor productivity. However, analyst turnover rises following RTO mandates, though this trend fades by 2022-2023 as most brokerages adopt RTO policies.

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1. Introduction

The COVID-19 pandemic catalyzed an enormous uptake in working from home (WFH) in the US and many other countries.[1] Several years after the pandemic, WFH remains a feature of workplaces. For example, Buckman et al. (2025) show that, as of January 2025, WFH accounts for about one quarter of all paid workdays among Americans aged 20-64. The debate about the cost and benefits of WFH has recently received heightened interest.[2] Existing studies have focused on employees performing routine tasks (i.e., data entry and call processing) and information technology professionals. WFH varies widely depending on the occupation and employee characteristics (Bartik et al., 2020; Dingel and Neiman, 2020; Barrero et al., 2023; Hansen et al., 2023; Burdett et al., 2024; Buckman et al., 2025) and the effects of WFH may also vary depending on these factors (Dutcher, 2012). In this paper, we study how WFH affects sell-side equity analysts.

We use brokerage houses' return-to-office (RTO) mandates to study the effects of WFH on analysts in a difference-in-differences design. In the period from 2020 to 2023, we manually identify 35 RTO mandates for which we can conduct this test. The richness of the setting allows us to compare analysts who are required to return to the office (treated) and others (control) while requiring both the treated and control to perform the same tasks: forecasting the same firms' earnings for the same fiscal year and at roughly the same time.

The analyst setting is unique in several aspects. First, analysts are high-skilled workers whose tasks are high-stakes, creative, coordination intensive, and time-consuming, providing a

[1] See Barrero et al. (2020), Bartik et al. (2020), Aksoy et al. (2022), Bick et al. (2023), and Brynjolfsson et al. (2020). See Barrero et al. (2021a, 2023) for reviews.
[2] See news reports on RTO policies announced at Amazon (Wile, 2024), Washington Post (Mullin, 2024), JPMorgan Chase (Kowitt, 2025), and the Federal government (Murphy, 2025).

rare opportunity to examine WFH on these types of occupations. Second, we can obtain many analyst characteristics, such as their age, gender, and experience, allowing us to examine the heterogeneity of the WFH effects across analysts. Third, individual analysts' performance can be objectively measured. In contrast, many of the existing studies are based on survey data. Individual analysts forecast the earnings of the firms they cover, and we can compare their forecasts with the realized earnings to measure the accuracy of their forecasts. Individual analyst forecasts are also dated, and we can measure whether their forecasts are timely. Besides these quality measures, we can also measure the quantity (i.e., the number of firms they cover and the frequency with which they update their forecasts).

Our analysis reveals that RTO mandates significantly increased analysts' forecast accuracy. The effect is sizable. Before RTOs, the median forecast error was 0.714 (i.e., 21.42 cents for a $30 stock). After the RTO mandates, relative to control analysts, the forecast error of treated analysts decreased by 0.106, which is 14.7% of the pre-RTO level. The positive RTO effect on forecast accuracy holds in the 2020-2021 period as well as in the 2022-2023 period, suggesting that the effect is not unique to the pandemic environment. In fact, the positive RTO effect holds in 26 out of the 35 RTO events. Analyst forecasts also become timelier after RTO mandates. We do not see a significant change in the number of firms they cover or their updating frequency.

Our identification strategy relies on the assumption that RTO events occur exogenously. We argue that it is plausibly so. First, most of our analysts work for a big financial firm with many other lines of operations, and the RTO mandates apply not only to equity analysts. Hence, the RTO policies are unlikely to result from poor analyst performance.[3] Second, empirically, we find that

[3] Ding and Ma (2024) and Flynn, Ghent, and Nair (2025) study the determinants of firms' RTO policies. Ding and Ma (2024) use a sample of S&P 500 firms and document that managers impose RTO policies not because they believe RTO will increase firm value but to reassert control over employees and blame them as a scapegoat for bad firm

the treated and control analysts exhibit parallel trends in the pre-period. The treatment effect starts precisely when RTO mandates are implemented. Although whether to implement RTO mandates or not may be endogenous, their exact timing is unlikely. In most cases, the RTO policies are announced several months in advance. It is difficult to argue that firm managers anticipate their analysts' performance will improve in several months and have now decided to implement an RTO mandate accordingly.

There is strong cross-sectional heterogeneity. First, forecast accuracy increases more for younger and less experienced analysts, consistent with the hypothesis that younger and less experienced analysts may miss out on valuable mentoring opportunities when working from home. Second, forecast accuracy increases more for female analysts than for male analysts. One possible reason is that female analysts may get more distraction when working from home. Third, across forecasts, RTO mandates have a stronger effect on an analyst's first forecast after earnings announcements than other forecasts. It is well-known that analysts are expected to issue new forecasts in the few days after each earnings announcement. When making these forecasts, analysts may face more time pressure. Fourth, the effects are stronger for analysts residing in Democratic states than in Republican states. We speculate that analysts living in Republican states were more likely to have voluntarily worked in the office than other analysts even before the return-to-office mandates.[4]

The evidence shows that RTO mandates increase analysts' productivity, suggesting that working from home is associated with more distraction and/or less effective collaboration. How

performance. Flynn, Ghent, and Nair (2025) use s sample of Russell 3000 firm and find that office rent, firm size and CEO gender are important determinants.

[4] We cannot observe whether an analyst is working from home nor how often s/he works from home and cannot test this speculation. However, in the general population, firms located in Democratic states/counties are more likely to allow their workers to engage in WFH (Kahn, 2023; Zarate et al., 2024).

will analysts react accordingly? We predict that, when working from home, analysts will reallocate their more limited attention to stocks that are more important to their careers. Consistent with this conjecture, we find that the RTO effects concentrate on relatively less important stocks to analysts' careers, i.e., stocks with smaller market capitalization, lower volume, or lower institutional holding.

RTO mandates may also bring in costs. One widely discussed cost of RTO mandates is that employees need to spend more commuting time and lose more personal autonomy (Aksoy et al., 2023; Barrero et al., 2021b; Barrero et al., 2025).[5] As a result, employers who require RTO may face more retention challenges (Bloom, Han, and Liang, 2024; Dijcke, Gunsilius, and Wright, 2024; Ding et al., 2024). Consistently, we find that RTO mandates increase analysts' annual turnover from 15.2% to 17.6%. However, this effect exists only among the RTOs in 2020-2021 and disappears in 2022-2023. A likely reason is that most firms have implemented RTO mandates in the later years.

Our primary contributions are to the emerging literature on how work-from-home arrangements affect employee productivity, work patterns, and well-being. We review the literature in more detail in Section 2. Within this literature, the studies most related to ours are on how WFH affects labor productivity. Existing studies have focused on jobs involving relatively simple tasks, such as call center employees (Bloom et al., 2015; Battiston, Kirchmaier, and Vidal, 2021; Emanuel and Harrington, 2024), data entry employees (Atkin, Shinde, and Schoar, 2023), employees doing simple administrative work (Choudhury, Foroughi, and Laron, 2019; Angelici and Profeta, 2023; Choudhury et al., 2024), or tasks without significant collaboration, such as chess players (Künn, Seel, and Zegners, 2022). The only exceptions are Yang et al. (2022), Gibbs,

[5] Consistent with this, evidence shows that many workers are willing to take pay cuts to work from home (Mas and Pallais, 2017; Barrero et al., 2021a; He, Neumark, and Weng, 2021; Aksoy et al., 2022; Maestas et al., 2023; Lewandowski, Lipowska, and Smoter, 2024; Cullen, Pakzad-Hurson, and Perez-Truglia, 2025).

Mengel, and Siemroth (2023), Shen (2023), Bloom, Han, and Liang (2024), and Duchin and Sosyura (2025). Yang et al. (2022), Gibbs, Mengel, and Siemroth (2023), and Shen (2023) study IT professionals. They study the abrupt WFH shift caused by COVID-19, which may limit the external validity of their findings. Bloom, Han, and Liang (2024) study engineers, marketing and finance employees in the Airfare and IT divisions of a Chinese travel agent company. Their main finding is hybrid working improves retention but has no significant impact on performance. Duchin and Sosyura (2025) document that remotely working CEOs are associated with lower firm performance and insider reviews.

We highlight two distinctions of our study. First, to the best of our knowledge, we are one of the first to study how RTO policies affect high-skilled employees in the finance industry. How WFH affects finance labor productivity is important by itself. As Barrero et al. (2025) show, WFH is most prevalent in finance across all industries. Our findings should also inform other occupations involving creative and collaboration-intensive tasks, which are understudied and most existing studies focus on abrupt WFH shift caused by COVID-19. Second, the existing studies either examine productivity using survey or based on the quantity dimension of output. In comparison, we can measure both the quantity and quality dimension of analyst productivity objectively. We find that RTOs do not decrease the quantity of analyst output but decrease the quality dimension. Analysts also shift their effort to stocks that are more important to their careers. All these findings highlight the importance of considering both dimensions.

However, we do not view that our results apply to all other types of occupations or any other types of WFH arrangements. It is possible that hybrid work is better than full WFH and better than five days in the office. See Choudhury et al. (2024) and Bloom et al. (2024). Most RTO policies in our sample require two to three days in the office. Toward the end of our sample period,

four RTO mandates require five days in office. Among these five RTOs, we find a negative coefficient similar to what we see for other RTO policies. However, we view this result as preliminary as the sample is too small. Overall, we think there is plenty of room for researchers to study other occupations and other types of WFH arrangements.

2. Literature Review

The literature on WFH is burgoening. Broadly, there are two streams of literature: (1) documenting the characteristics of WFH and (2) the effects of WFH on labor productivity, work pattern, and well-being, as well as on firm performance and city performance.

2.1 Basic facts about WFH

Before the COVID-19 pandemic, about 5% of full workdays were supplied from home (Barrero et al., 2021a). Based on the Survey of Working Arrangements and Attitudes, the ratio increased to 60% during the pandemic outbreak. With the dissipation of the pandemic effects, the ratio quickly decreased to 25%-30% at the end of 2022 and has been stable since then (Barrero et al., 2025). See also Barrero et al. (2020), Bartik et al. (2020), Brynjolfsson et al. (2020), Criscuolo et al. (2021), Aksoy et al. (2022), Bick et al. (2023), Brynjolfsson et al. (2023), Hansen et al. (2023), Davis (2024), Makridis and Schloetzer (2024), and Buckman et al. (2025).

Besides this general pattern, the literature has documented that WFH varies widely depending on the nature of the work, including education, industry, occupation, and life-cycle career progression (Bartick et al., 2020; Barrero et al., 2023; Burdett et al., 2024; Hansen et al., 2023; Buckman et al., 2025). For example, Dingel and Neiman (2020) document that 37% of jobs in the United States can be performed entirely at home. In particular, among the Business and Financial Occupations group, the ratio is 88%.

2.2 WFH and employees

There are both benefits and costs associated with WFH. On the one hand, WFH may lead to better work-life balance as employees can manage their schedules more flexibly, accommodating personal responsibilities such as childcare or other household duties (Barrero et al., 2025); WFH can also save commuting time and expenses (Barrero, Bloom, and Davis, 2021). On the other hand, WFH may be associated with shirking or distractions at home (i.e., household duties, a lack of a dedicated workspace, or lack of self-control); in-personal communications, including collaboration and mentoring, may be more effective than virtual communications; younger and less experienced workers can also benefit more from interacting with more senior coworkers with in-personal communications and WFH may impede career development (Bloom et al., 2015; Barrero, Bloom, and Davis, 2022; Emanuel and Harrington, 2024).

Consistent with the benefits of WFH, studies document that employees value the option of WFH, and many are willing to take pay cuts to do so (Mas and Pallais, 2017; Barrero et al., 2021a; He, Neumark, and Weng, 2021; Aksoy et al., 2022; Maestas et al., 2023; Lewandowski, Lipowska, and Smoter, 2024; Cullen, Pakzad-Hurson, and Perez-Truglia, 2025), especially women, people with children, and those with longer commutes (Aksoy et al., 2022). Ding et al. (2024) and Bloom, Han, and Liang (2024) find that WFH improve employee retention.

Consistent with the costs of WFH, Frakes and Wasserman (2016) find that patent examiners are much more likely to procrastinate reviews when they start working from home. WFH increases communication costs for emergency phone operators (Battiston, Kirchmaier, and Vidal, 2021), software engineers at Microsoft (Yang et al., 2022), and IT professionals in India (Gibbs, Mengel, and Siemroth, 2023). Emanuel, Harrington, and Pallais (2023) find that feedback

exchanged between colleagues dropped after the move to remote work. Atkin, Schoar, and Shinde (2023) find, relatively office workers, a decline in learning for workers at home.

It has also been documented that workers self-sort—ambitious or skillful workers may shy away from WFH (Emanuel and Harrington, 2024); subgroups that face additional demands on their attention when at home, such as those with children or other home care responsibilities (Atkin, Shinde, and Schoar, 2023).

The most related to ours is the literature on WFH and labor productivity. Bloom et al. (2015) showed that both hours worked and productivity increased during WFH for call center employees at a Chinese travel agent firm. Emanuel and Harrington (2024) study call center employees from a Fortune 500 firm and find that WFH lowers productivity by 4%. Bloom, Han, and Liang (2024) found hybrid work reduced attrition without significantly reducing lines of code written. Choudhury, Foroughi, and Laron (2021) study examiners for the US Patent and Trademark Office and find that allowing them to work from anywhere increased their productivity by 4.4%. Künn, Seel, and Zegners (2022) find that professional chess players performed worse when competing online during COVID-19 than in-person tournaments. Angelici and Profeta (2024) found that giving workers locational and temporal flexibility one day per week reduced absences and improved self-perceived productivity and well-being. Choudhury et al. (2024) study employees performing administrative tasks in the human resources department and the microfinance department of BRAC, the world's largest non-governmental organization. They find that hybrid work is better than either fully WFH or fully in-office in terms of employee work-life balance and

job satisfaction, but no effect on productivity. In a lab experiment, Dutcher (2012) documents an adverse impact of WFH on US undergraduates doing data entry tasks.[6]

2.3 Other consequences of WFH

Other studies have examined WFH arrangements' impact on wages (Barrero et al., 2022; Liu and Su, 2023), entrepreneurial activities (Barrios, Hochberg, and Yi, 2024; Gupta, Gupta, and Simintzi, 2024; Kwan et al., 2024), female employment (Harrington and Kahn, 2023; Ho, Jalota, and Karandikar, 2024; Tito, 2024; Hsu and Tambe, 2025), disability employment (Bloom, Dahl, and Rooth, 2024), asset prices and firm performance (Bai et al., 2021; Davis, Hansen, and Seminario-Amez, 2020; Gupta et al., 2022, 2023; Pagano et al., 2023; Barry et al., 2024; Favilukis et al., 2024; Ramani, Alcedo, and Bloom, 2024; Kwan, Matthies, and Yuskavage, 2025). At the more macro level, WFH can also change the geographic distribution of economic activities (Gupta et al., 2022, 2023; Delventhal et al., 2022; Alipour et al., 2023; Brueckner, Kahn, and Lin, 2023; Delventhal and Parkhomenko, 2023; Duranton and Handbury, 2023; Bick et al., 2024; Coskun et al., 2024; Davis, Ghent, and Gregory, 2024; Akan et al., 2025; and Monte et al., 2025).

3. Data, Methodology, and Summary Statistics

3.1 Data

Our primary data source is the Institutional Brokers' Estimate System (I/B/E/S) database. We obtain stock market data (prices, trading volumes, shares outstanding, and cumulative adjustment factors for shares and prices) from the Center for Research in Security Prices (CRSP) database. Institutional ownership information is from Thomson-Reuters 13F Holdings. We

[6] There are also several early studies based on survey data (Bellmann and Hubler, 2020; Barrero, Bloom, and Davis, 2021; Burdett et al., 2024). Several studies examine smart working arrangements. Choudhury, Foroughi, and Laron (2021) compare WFH with working-from-anywhere, and Angelici and Profeta (2024) study flexible time schedules.

manually collect analyst characteristics from public sources such as LinkedIn and broker websites. The United States 2020 presidential election results are from the Federal Election Commission. This section discusses the background information pertaining to RTO mandates and the data.

3.1.1 Return-to-office mandates

We collect RTO mandate information for brokerage houses by utilizing a news search method similar to Ding and Ma (2024) and Flynn, Ghent, and Nair (2025). We identify 335 brokers appearing in the I/B/E/S database between 2020 and 2023. We discuss the details of identifying brokers in Section 3.1.2. For each broker, we conduct manual searches on Google. Specifically, for each broker firm, we search the firm name and these keywords: "return to office," "return to office mandate," "back to office," "onsite work," "hybrid work," "work from home," and "remote work." We obtain 432 news reports on RTOs and read through them individually.

We select our sample of RTO policy adjustments based on the following procedures. First, many RTO policy adjustments received multiple news coverages, and we remove duplicates. Second, many RTO policy adjustments are likely to have limited impact, such as policies that modify COVID-related protocols (e.g., changes in mask requirements and policies further targeting unvaccinated employees) and policies that track attendance or issuance reminders (e.g., tracking employee IDs, reminding employees about attendance expectations, and "letters of education"). We exclude these adjustments. Third, we exclude the RTO policies that do not target equity analysts (e.g., some RTO policies only require senior sales and trading employees to return to the office) and policies targeting overseas offices (e.g., London and Hong Kong) in predominantly US-based brokerages. Fourth, our empirical exercise requires information on the date of implementation of the RTO policy. We exclude the policies for which the implementation dates are too vague (e.g., "gradually ramping up returns" and "first part of 2021"). However, in

cases where news reports do not provide a precise implementation date but use phrases such as "earlier this month" or "this week," we consider the first day of the mentioned period as the RTO date. For references to "end of this month," we use the last day; for "mid month," we use the 15th of the month.

To include an RTO policy adjustment event in our difference-in-differences test, we require the broker not to have a second RTO event in the six-month period around the RTO implementation. For eight RTO events, the brokers did not issue any earnings forecasts on US firms. These brokers are either small or foreign. Our final sample consists of 35 RTO events involving 23 unique brokers. Table 1 lists these events. All these events involve brokerage houses strengthening their RTO requirements. From 2000 to 2023, the number of RTO events each year was 2, 12, 16, and 5, respectively. The relatively small number of RTO events in 2023 is consistent with the overall pattern in WFH. Based on surveys (e.g., the Survey of Working Arrangements and Attitudes and the Census Household Pulse Survey), in the general population, the percentage of paid full days worked from home has been stable since the end of 2022 (Barrero et al., 2025).

To get a sense of what was discussed in these news reports, we sent the original 432 RTO news reports to ChatGPT and asked it to extract five key phrases from each report.[7] We then combine them. Figure 1 displays the frequency of these phrases. The news reports discuss the positive sides of RTOs, such as organizational culture and in-person collaboration. They also discuss the negative sides of RTOs, such as work-life balance, employee autonomy, and employee retention. Another frequent topic is employee safety. This is primarily due to RTOs in earlier years

[7] Internet Appendix Section A presents the prompt we provided to ChatGPT.

when COVID was still a significant concern. Overall, these topics are similar to the ones we discussed in the Introduction and the Literature Review.

3.1.2 Analyst forecast data

Data on analysts' earnings per share (EPS) forecasts are collected from the I/B/E/S Detail History file covering the period from 2020 to 2023. We focus on one-year-ahead EPS forecasts following the literature.[8] We focus on the US firms; the reporting currency is the US dollar, and the firm is covered by CRSP. We use the unadjusted file to mitigate the rounding problem in I/B/E/S and adjust for stock splits (using the adjustment factor from CRSP) so that the forecasts and the actual EPS are comparable. We restrict the sample to forecasts made at least two days before the earnings announcements.[9]

Although the I/B/E/S data set provides a broker identifier, it does not give the broker names. It does not provide analysts' full names but just the last names and first initials. We identify analysts' full names and the names of their brokerages, relying on the fact that most analysts participate in firms' earnings conference calls (Mayew, Sharp, and Venkatachalam, 2013; Jung, Wang, and Zhang, 2015; Li and Wang, 2024; Ben-Rephal et al., 2025). From the earnings conference call transcripts provided by Seeking Alpha, we use text parsing tools to go through each transcript, extract the full names and affiliations of all conference call participants, and match them with I/B/E/S.[10] If we cannot find a match from Seeking Alpha, we conduct a Google search based on an analyst's last name and first initial. Through this procedure, we obtain full names and

[8] See Clement and Tse (2005), Malloy (2005), Hong and Kacperczyk (2010), Kumar (2010), Bradley, Gokkaya, and Liu (2017), and Hirshleifer et al. (2019) for examples.
[9] If we restrict the sample to forecasts with a minimum forecast horizon of 30 days, our sample will reduce by approximately 2%. In this test, the coefficient on *Treat*Post* is -0.110 (t = -3.26), very similar to our baseline.
[10] Very often, brokerage names are spelled differently. We conduct manual matching of brokerage names for analysts whose last names and first initials match across I/B/E/S and the earnings conference call transcript data.

affiliations for almost all analysts whose last name and first initial are available in I/B/E/S.[11] In the sample period, we identify 335 brokers, which we use for the RTO news searches, as discussed in Section 3.1.1.

We manually collect analysts' gender, location, and college graduation year from LinkedIn. We conduct Google searches if an analyst does not have a LinkedIn profile or the LinkedIn profile information is insufficient. In a small number of cases, we can locate these analysts from other professional web pages. If the gender information is still missing, we infer analysts' gender based on their first names. Analysts' age is generally not directly available. We calculate it by assuming that analysts graduated from college at age 22.

3.2 Methodology

We adopt a stacked difference-in-differences (DID) design. We implement a stacked DID design instead of a staggered DID to mitigate the problem of negative weights (De Chaisemartin and D'Haultfoeuille, 2020; Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021; Sun and Abraham, 2021). In the test, we use the 35 RTO events (see Section 3.1) as the treatment group. We use the three months before an event as the pre-period and the three months after an event as the post-period. In our sample, some brokers adjusted their RTO policies multiple times. Choosing too long a pre-period will lead it to overlap with other RTO events. Section 4.1.4 examines a longer post-period and finds a similar result.

The unit of analysis is at the forecast level. For each RTO event, we identify the firms for which the treated broker issued at least one forecast in the event period. The treatment group

[11] Our sample has about 3,700 analysts. For about 280 analysts, information on analysts' last names and first initials is not available in I/B/E/S. This is either because I/B/E/S reports a team (instead of an individual) or because an analyst is covered in the EPS file but not in the recommendation file (where the information on analysts' last names and first initials is from). Among the remaining analysts, we can get their full names and affiliations for virtually all of them.

consists of forecasts issued by analysts from the treated broker on these firms. The control group also consists of forecasts on these firms but is issued by other brokers. Among the control sample, we exclude forecasts issued by brokers that also had RTO policy adjustments in the event period and those from brokers with uncertain RTO timing that potentially overlaps with the event window.[12]

3.3 Summary statistics

In Table 2, we report the summary statistics of the main variables in our analysis. We calculate analyst forecast characteristics using the pre-period data. We winsorize the continuous variables at the 1% and the 99% levels to mitigate the effect of extreme values.

Our primary dependent variable of interest is analyst forecast accuracy, inversely proxied by analysts' percentage absolute forecast error (*Forecast Error*). Forecast error is probably the most widely studied analyst output in the finance and accounting literature. *Forecast Error* for analyst *i* on stock *j*'s EPS of fiscal year *y* issued at time *t* is equal to the absolute value of actual company EPS minus the EPS forecast, divided by the stock's price one year before the annual earnings announcement date and multiplied by 100.

$$Forecast\ Error_{i,j,y,t} = 100 * \frac{\left|Actual\ EPS_{j,y} - Forecasted\ EPS_{i,j,y,t}\right|}{Price_{j,y-1}}$$

[12] For example, on December 21, 2021, Research Capital announced that "With the resurgence of Covid-19 cases and the Omicron variant, we have made the decision to defer our January 4 target date for our staff return to our offices until a future date. The safety of our associates and clients is, and will always be, a top priority" (https://researchcapital.com/covid-19). We cannot obtain any additional RTO policy news on Research Capital. As a result, we do not know whether and if so when Research Capital implemented its RTO policy in the period from January 4, 2022 to the end of our sample. If an RTO event's six-month event window overlaps (partially or fully) with this ambiguous period, we exclude Research Capital from its control group.

Given time constraints, analysts may issue their forecasts with a delay or reduce the quantity of their forecasts to maintain their forecast quality. We define $Timeliness_{i,j,y,t}$ as a dummy variable that takes the value of one if the time gap between a forecast and the firm's most recent earnings announcement is no more than 13 trading hours and zero otherwise. For the analysis of *Timeliness*, we include only the first forecast issued after the most recent earnings announcement. We use two measures to capture the quantity dimension of analyst forecasts. The first measure is *Firms Covered*, defined as the number of unique firms an analyst covers. The second measure is *Updating Frequency*, which we define as the average number of forecasts an analyst issues for a firm they cover. We measure analyst experience by the number of years an analyst has appeared in the I/B/E/S database.

Table 2 shows the summary statistics. We calculate all the statistics using the pre-period data. Across all the events, we have 1,603 treated analysts with about 25,000 forecasts in the pre-period. The control group is bigger, with about 23,500 and about 194,000 forecasts. Relative to the control group, on average, the treated analysts are 3.9% more likely to be female, 1.9 years younger, and 1.8 years less experienced. Treated analysts cover 2.3 fewer firms and have 0.16 higher updating frequency. At the forecast level, treated analysts have a higher forecast error than control analysts. There is no significant difference in forecast age or forecast timeliness.

Overall, the treatment and control groups differ significantly in several characteristics. Although this does not necessarily violate the identification assumption (parallel trends), we conduct a propensity-score-matched analysis in Section 4.1.4. In Internet Appendix Section B, we report the summary statistics after removing the brokers without RTO news from our sample. In this sample, the treatment-control differences become significantly smaller for all the characteristics, and the differences in gender, updating frequency, and forecast error become

insignificant. In Section 4.1.4, we also report a robustness test after removing brokers without RTO news from our sample.

4. Empirical Results

4.1 Forecast accuracy

4.1.1 Baseline results

To test the impact of RTO policies on analysts, we estimate the following regression model:

$$Forecast\ Error_{i(e),j,y,t} = \beta_1 Treat_{i(e)} * Post_{t(e)} + \beta_2 X_{i(e),j,y,t} + \gamma_{j,y} + \delta_i + \theta_e + \varepsilon_{i(e),j,y,t}$$

where $i(e)$ indicates analyst i in event e, j indicates firms, y indicates the fiscal year, and t indicates the year-month when an analyst issues the forecast. $Treat_{i(e)}$ is a dummy variable that takes the value of one if analyst i works for the RTO broker in event e and zero otherwise. $Post_{t(e)}$ is a dummy variable that takes the value of one if a forecast is issued after the RTO implementation and zero otherwise. $X_{i(e),j,y,t}$ is a set of control variables. $\gamma_{j,y}$, δ_i, and θ_e are sets of fixed effects. The primary variable of interest is *Treat*Post*. If RTO mandates decrease analyst forecast errors, we expect $\beta_1 < 0$.

Table 2 reports the regression results of the baseline difference-in-differences test. In all specifications, we include the firm*fiscal year, calendar year-month, and event fixed effects. With the firm*fiscal year and calendar year-month fixed effects, we essentially compare different analysts' forecast accuracy by requiring them to perform the same tasks – forecasting the same firms' earnings of the same fiscal year – in the same calendar month.[13] Depending on the

[13] Researchers often adjust an analyst's forecast for a firm-year by subtracting the mean accuracy of all analysts' forecasts for the same firm-year issued within a comparable time window (Clement, 1999; Jacob et al., 1999; Hong et al., 2000; Cowen et al., 2006; Bradley, Gokkaya, and Liu, 2017; Harford et al., 2019). We prefer the fixed effects

specification, besides these fixed effects, we include several other fixed effects. Given the granularity of our panel data, we can estimate all of these high dimensional fixed effects simultaneously. Given this stringent empirical specification, we need to control only factors that vary at the analyst-firm-time level. We, therefore, only include *Forecast Age* as a control. We cluster our standard errors by event. Our results are similar if we cluster at event*broker level or double-cluster by event and broker.

In column (1) of Table 2, we add the firm*fiscal year, calendar year-month, and event fixed effects. The coefficient of *Treat*Post* is -0.096 (t = -2.65). In column (2), we further add analyst fixed effects. The coefficient of *Treat*Post* becomes -0.106 (t = -3.23). In column (3), we add *Forecast Age,* and the coefficient of *Treat*Post* and its statistical significance are almost unchanged relative to those in column (2).[14] The coefficient of *Treat*Post* is around -0.10 in all three specifications, suggesting that our results are not sensitive to changes in empirical specifications. These results show that relative to control analysts, analysts who are required to return to office became more accurate in forecasting earnings.

The economic magnitude of the relative increase in forecast accuracy is sizable. The median value of *Forecast Error* for the treated group is 0.714 (see Table 1). Hence, the relative increase in forecast accuracy is 14.7% of the unconditional median, which is an economically meaningful effect (e.g., 18.27 cents rather than 21.42 cents on a $30 stock).

method advocated by Gormley and Matsa (2014), who show that the method of demaning the dependent variable with respect to a group can produce inconsistent estimates.

[14] The coefficient of *Forecast Age* is counterintuitive. Prior literature finds that forecasts issued closer to earnings announcements are more accurate, hence we should expect a positive coefficient on *Forecast Age* (Clement, 1999). The negative coefficient is because of our high dimensional fixed effects, which have largely control for the effect of forecast age. In unreported results, if we remove firm-fiscal year fixed effects, the positive coefficient of *Forecast Age* will be resumed.

4.1.2 Dynamic effects

Barrero et al. (2023) and Etheridge et al. (2023) stress the importance of adaptation over time when assessing the productivity effects of WFH. To test the dynamic treatment effect, we separate the event period into six event months and interact each event month dummy with the treatment dummy. Table 3 reports the results. We use the month right before the RTO policy (i.e., month *t*-1) as the base case and evaluate the dynamic of the RTO effect for each of the other months relative to month *t*-1. The two columns differ in whether we added *Forecast Age* as a control, and the two columns show very similar results.

The treatment effects are negative in the post-period for all three event months. Based on column (2), the coefficient of *Treat*Month t*+1 is -0.052 (t = -1.70), which is statistically significant at the 10% level. In month *t*+2, the coefficient becomes -0.107 (t = -4.05); in month 3, it is -0.115 (t = -2.92). Both are highly statistically significant, and the magnitudes increase relative to month *t*+1. The relative increase in the RTO effect from month *t*+1 to the next two months indicates that there might have been some transition costs from working from home to returning to the office. The coefficients of *Treat*Month t*-3 and *Treat*Month t*-2 are small and statistically non-distinguishable from zero, suggesting a parallel pre-trend between the treatment and control. The parallel pre-trend supports the validity of the identification assumption of the difference-in-differences approach.

We also note that the RTO effect starts to appear precisely right after its implementation, providing further credibility for the identification strategy. While it may be impossible to rule out the endogeneity of RTO events completely, it is difficult to argue that the exact timing of these policies is endogenous. In most cases, RTO policies are announced several months in advance. It

is unlikely that brokerage houses could foresee a specific month in which their analysts would become more accurate and strategically schedule their RTO policies accordingly.

4.1.3 By event

Figure 2 displays the estimate of the difference-in-differences effect (i.e., the coefficient on *Treat*Post*) by event. Specifically, we rerun the difference-in-differences regression event by event and omit the event fixed effects. It is evident that the difference-in-differences coefficients are negative for most events (26 out of 35). Most extreme coefficients show up for RTOs involving relatively small brokers where the sample is relatively small. The equal-weighted mean of the event-level coefficients is -0.149 (t = -2.84). If we weigh each event by its number of observations (i.e., downweight the events with a smaller sample), the mean becomes -0.156 (t = -5.76).[15] These results show that the RTO effect is consistent across many events and is not driven by a small number of events.

4.1.4 Robustness

We report several robustness tests in Table 4. While the pre-period trends appear parallel, Table 1 demonstrates differences in observable characteristics between the treated and control analysts. To help reduce concerns that these differences may confound our results, we perform a matching exercise where we match analysts on these characteristics. Our matching exercise begins with a logit regression at the event-analyst level. We regress a binary variable indicating whether an analyst is treated in an event on a host of analyst characteristics, including analyst gender, firms covered, updating frequency, and forecast accuracy.[16] All matching variables are measured using

[15] This method can be considered Fama and MacBeth (1973) regressions. In the first case, we weigh each event equally. In the second case, we weigh each event by its importance (i.e., the number of observations).
[16] We omit analyst age because age is missing for a significant fraction of the sample.

data from the pre-period. For each treated analyst, we identify up to ten non-treated analysts from the same event closest to the treated in terms of propensity score with a standard tolerance of a 0.005 caliper. The match is implemented with replacement. In column (1) of Table 4, we estimate the DID regression based on this matched sample. We add the *Pair*Post* fixed effects to ensure that we focus on the difference-in-differences effects within pairs. The results show that the coefficient on *Treat*Post* continues to be significantly negative. Its magnitude is also similar to that in the baseline regressions.

For some brokers, we could not find any RTO news. It is possible that they did not implement any RTO policies. It is also possible that these brokers were small, and their RTO policies did not get media coverage. In column (2), we report the results of removing these brokers from our control. The coefficient of *Treat*Post* decreases slightly from -0.105 to -0.093 and continues to be statistically significant. As reported in Internet Appendix Section B, removing brokers without RTO news from the control renders the treatment and control analysts more comparable. This finding further mitigates the concerns that the treatment-control differences in characteristics may confound our results. Given the similar magnitude of the *Treat*Post* coefficients between the baseline and column (2), we prefer the baseline specification as it has more power.

In columns (3) and (4), we report the results by subperiods based on the RTO implementation year. The health concern was still heightened in the early years and became significantly mitigated in later years. The results show that the RTO effect is very similar between the 2020-2021 period and the 2022-2023 period. The similarity in results between the two subperiods suggests that the finding is not due to the unique environment caused by the pandemic.

Such a finding is consistent with Li and Wang (2024), who document that the pandemic's impact on analysts largely dissipated by the middle of 2020.

In column (5), we extend the post period to nine months. The coefficient of *Treat*Post* and its statistical significance are very similar to those in the baseline. In column (6), we report the results with a more stringent set of fixed effects. Specifically, we add calendar year-month, analyst*event, firm*fiscal year*event, and event month*event fixed effects. This more stringent set of fixed effects can further ensure we compare the treated and control analysts within a given event. The coefficient on *Treat*Post* becomes -0.149 (t = - 4.85). Both its magnitude and the statistical significance increase relative to the baseline specifications in Table 2.

In column (7), we report the results of clustering the standard errors by event*broker. The t value is -2.81. In untabulated results, if we cluster by event and broker (two-dimensional clustering), the t value is -2.95. To avoid illiquid stocks, we drop a firm-fiscal year if the stock price one year before the annual earnings announcement date (i.e., the denominator used in calculating forecast error) is below $1 and report this result in column (8). The results continue to be similar to the baseline findings.

Overall, the results in Table 4 show that our baseline results are robust to various specifications. In most specifications, the coefficients on *Treat*Post* are also close to that in the baseline. In some cases, the statistical significance is slightly lower, likely due to smaller samples in these tests. When we add more stringent fixed effects (column (6)), both the coefficient on *Treat*Post* and its statistical significance increase.

4.2 Potential economic mechanisms and supporting evidence

In this subsection, we conduct empirical tests to examine the underlying economic mechanisms of the above-documented RTO effect. Motivated by the discussion from the literature and the RTO news reports (Figure 1), RTOs might have increased analysts' productivity through two non-mutually exclusive channels. First, when working from home, analysts may face more distractions. Second, analyst collaboration may become less effective when working from home, especially for younger and less experienced analysts.[17]

In subsection 3.2.1, we examine the timeliness and the quantity aspect of forecasts to have a complete understanding of analyst productivity. In subsection 3.2.2, we examine whether the RTO effect depends on time pressure. In subsection 3.2.3, we examine cross-analyst heterogeneity. In subsection 3.2.3, we examine whether analysts strategically shift their efforts.

4.2.1 Timeliness and quantity

Forecast accuracy is just one dimension of analyst productivity, albeit it may be the most crucial dimension. In this subsection, we investigate RTOs' effects on several other dimensions of analyst productivity: forecast timeliness and forecast quantity (i.e., firms covered and updating frequency). The analysis of timeliness is at the forecast level, but we only include the first forecast of an analyst after a firm's earnings announcement. Timeliness is a dummy variable that takes the value of one if the time gap between the quarterly earnings announcement and a forecast is no

[17] There are two additional interesting hypotheses. First, another way to test the collaboration channel is to compare analysts who work in a team and analysts who work sololy. To the best of our knowledge, in the literature, the only way to measure this is to count the number of authors on a research report (Fang and Hope, 2021). However, we find that in most brokers, the majority of reports are either all authored by multiple authors or all authored by one author. We speculate that such a pattern is because of different "cultures" in signing these reports instead of reflecting whether a report is authored by one analyst or by a team. Second, long commuting analysts may be affected differentially than short commuting analysts. Unfortunately, such a test requires information on analyst home addresses, which is difficult to obtain.

more than 13 trading hours and zero otherwise. We also define an alternative continuous measure of timeliness. Specifically, we count the number of trading hours since the latest earnings announcement. The results in Panel A of Table 5 show that after RTOs, analyst forecasts become more timely. After RTOs, the probability of issuing timely reports increases by 1.4%-1.7% (depending on the specification) for the treated analysts relative to the control analysts. On average, after RTOs, the time gap between earnings announcements and analyst forecasts reduces by 5.7-7.8 log percentage points.

We measure *Firms Covered* as the number of unique firms on which an analyst issued at least one forecast. We measure *Updating Frequency* as the average number of forecasts per firm issued by an analyst. The analysis of these two variables is at the analyst-period level. For each analyst, we have two observations: one for the pre-period and one for the post-period. In rare cases, an analyst does not issue any forecasts in a period; we set the value of both measures as zero. In the regressions, we either use *Firms Covered* and *Updating Frequency* or take the natural logarithm. Given that the analysis is at the analyst-period level, we add analyst and event fixed effects. The results in Panel B of Table 5 indicate that the coefficient on *Treat*Post* is positive for all the specifications but is statistically significant only for Log(*Firms Covered*).

The results show that RTOs increase analyst forecast timeliness. There is also some weak evidence that RTOs increase the number of firms an analyst covers. Overall, like the results on forecast accuracy, these results also suggest that analysts become more productive after RTOs.

4.2.2 The first forecasts

Many analysts issue updates more than once between two consecutive earnings announcements. Typically, analysts face time pressure for the first forecast after an earnings

announcement. As we show in Table 2, more than half of analysts issue their first forecasts within 13 trading hours after earnings announcements.

Table 6 compares the RTO effects on the first forecasts and on other forecasts. If working from home is associated with more distraction and/or less effective collaboration, we expect the RTO effect to be stronger when analysts face time pressure. Table 6 confirms this prediction. The two subsample differences are large and statistically significant, with a *p*-value of 0.018.

4.2.3 Cross-analyst heterogeneity

We examine the cross-analyst heterogeneity to shed more light on the underlying mechanism of the RTO effects. We examine these characteristics: analyst experience, age, gender, and the political environment of their residing states.

One argument against WFH is that WFH impedes communication. Younger and less experienced employees can miss out on valuable mentoring opportunities and chances to better acquaint themselves with corporate culture. In Panel A of Table 7, we split analysts into various groups based on their experience and estimate the RTO effect for each subsample. The coefficients on *Treat*Post* are monotonic with respect to experience: less experienced analysts benefit more from RTO mandates. In the first two columns of Panel B, we examine the age effect and find that RTO mandates increase younger (<=35 years old) analysts' forecast accuracy but not for the older ones (>35 years old).

It is well documented that childcare and other household duties disproportionately fall on the shoulders of women (Du, 2023; Li and Wang, 2024). We expect that when female analysts work from home, they may get more distractions than male analysts. Hence, female analysts' forecast accuracy may increase more than males' due to RTO mandates. The results in columns

(3) and (4) are consistent with this prediction, although the subsample difference is not statistically significant.

In the last three columns of Panel B, we examine the effect of political leaning. Whether a state is Democratic or Republican is defined based on the 2020 presidential election using data from the Federal Election Commission. For the Democratic state analysis, we also report a version after excluding New York. Kahn (2023) documents that, in the general population, firms located in Democratic states are more likely to engage in WFH. Hence, we speculate that analysts living in Republican states were more likely to have voluntarily worked in the office than other analysts even before the return-to-office mandates. As a result, RTO mandates are more significant shocks to analysts residing in Democratic states than those in Republican states. The results are consistent with this conjecture.

Taken together, the cross-analyst heterogeneity tests are consistent with our conjecture that working from home is associated with more distractions and/or less efficient collaborations.

4.2.4 Effort allocation

If working from home is associated with more distractions and/or less efficient collaborations, we expect analysts will reallocate their attention to stocks that are more important to their careers. We follow Harford et al. (2019) to measure the relative importance of a firm in an analyst's portfolio, using three characteristics: market capitalization, dollar institutional ownership, and dollar trading volume. Market capitalization is measured at the end of the month before RTO, dollar institutional ownership is measured at the most recent quarter-end before RTO (we set missing institutional ownership as zero), and dollar trading volume is calculated over the 12 months preceding RTO. For each of these three characteristics, we rank all the firms that an analyst

covers (defined as firms the analysts issued at least one forecast during the 12 months preceding RTO) into terciles. The terciles of firms with the highest market capitalization (or volume, or institutional ownership) are considered stocks important to an analyst. We also construct an importance index. Specifically, we determine a stock's percentiles for each of the three characteristics and use the average of these percentiles as the importance index.

Table 8 reports the *Treat*Post* coefficients for each subsample defined based on a measure of importance. The results are consistent across different measures of importance. The RTO effects are negative but mostly insignificant for the stocks that are most important to analysts (highest terciles). The RTO effects are more prominent for the least important stocks (lowest terciles). We also report the *Treat*Post* coefficients after combing the middle and lowest terciles. The differences between the highest tercile and the middle+lowest group are statistically significant at the 5% level for all the four importance measures except for volume.

Overall, these results show that when working from home, analysts strategically shift their attention to the firms that are more important to their careers.

4.3 Analyst turnover

Finally, we examine whether RTO mandates affect analyst turnover. We include all the treated and control analysts and track whether they leave their current broker in the 12 months following an RTO implementation. *Turnover* is a dummy variable that takes the value of one if an analyst leaves his/her current broker in the 12 months following an RTO and zero otherwise. Table 9 reports the results using a linear probability model (Panel A) and a Cox hazard model (Panel B). For the linear probability model, we exclude the RTOs in 2023 as we need 12 months of data to

measure *Turnover*. We include the 2023 data for the hazard as the hazard model allows data censorship.

In column (1) of Panel A, the coefficient on *Treat* is 0.024 (t = 2.20), suggesting that treated analysts have a turnover rate 2.4 percentage points higher than the control analysts. The average turnover rate in our sample is 15.2 percentage points. A 2.4 percentage points change is a 15.8% increase, which is economically sizable. The hazard regression indicates an 18.6% (i.e., exp(0.171)-1) increase in turnover after RTO, similar to the linear probability estimate.[18] Also, interestingly, the effect exists only in the 2020-2021 period. The effect disappears in later years, likely because most brokers have implemented RTO mandates, and analysts who value WFH may be unable to find better alternatives.

5. Conclusions

In this paper, we study the effect of return-to-office mandates of brokerage houses on their analysts. Our difference-in-differences approach compares treated analysts and control analysts performing the same tasks: forecasting the same firms' earnings of the same fiscal year and at roughly the same time. We find that, relative to controls, treated analysts' earnings forecast accuracy increases after the RTO mandates. This effect is stronger for younger, less experienced female analysts and when analysts face more time pressure. Evidence also suggests that when working from home, analysts strategically shift their limited attention to stocks that are more important to their careers. These findings are consistent with the hypothesis that working from home is associated with more distraction and/or less effective collaboration. We also find that RTO

[18] In the linear probability model, we control for event and broker fixed effects. In the hazard model, we include the event fixed effects but not the broker fixed effects. Instead of adding the broker fixed effects, we stratify the data by broker. In hazard specifications, stratification is a more flexible control of time-invariant effects than fixed effects. See An et al. (2024) for another application of the hazard model in finance.

mandates increase analyst turnover. However, this finding has disappeared in recent years, perhaps because most brokers have implemented RTO mandates, and it is difficult for analysts to find suitable alternatives.

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Figure 1. Topics covered by RTO news reports

This figure displays the key topics covered by RTO news reports. We use ChatGPT to extract these topics. Please see Section 3.1.1 for details.



Figure 2. Estimates by event

This figure illustrates the estimated difference-in-differences effect, represented by the coefficient on *Treat*Post*, for each event. The events are ordered based on their respective RTO implementation dates.

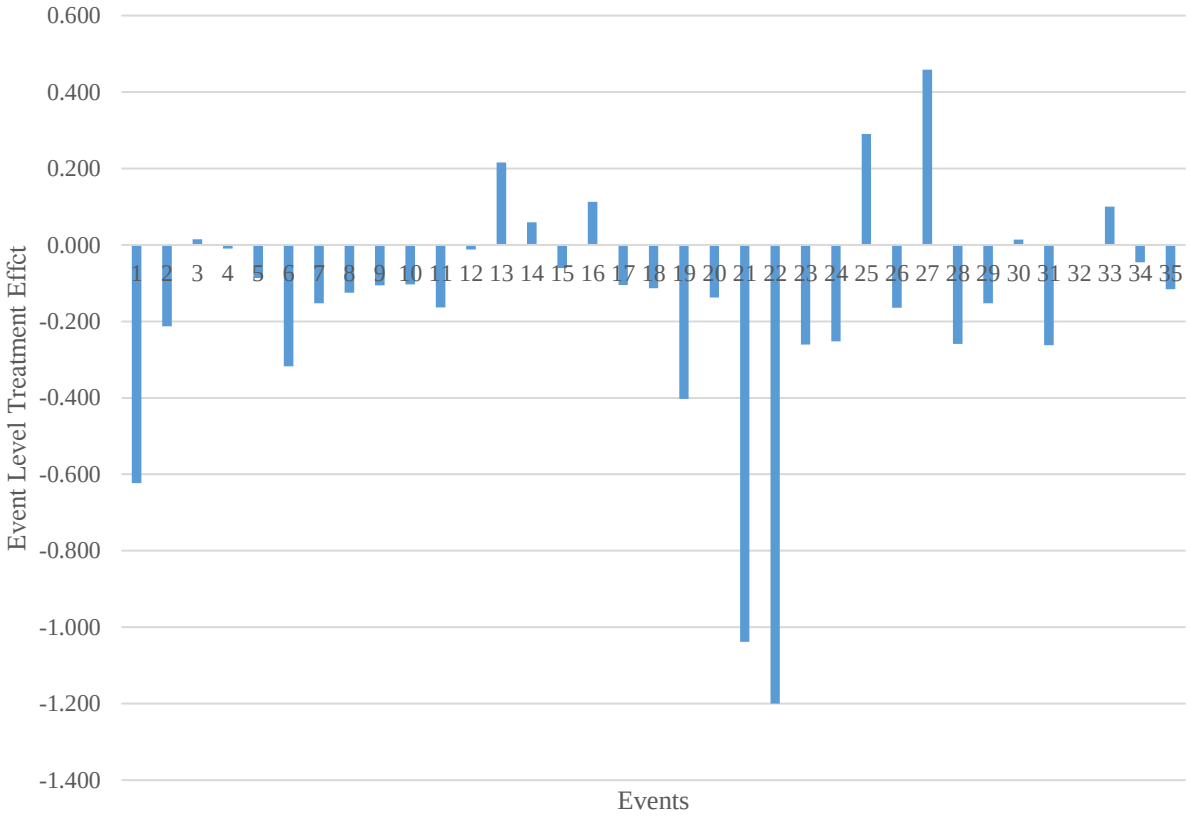


Table 1. The list of RTO events

This table presents the RTO events included in our difference-in-differences analysis. For further details, refer to Section 3.1.

Number	Broker	RTO date	Number	Broker	RTO date
1	Stifel	5/18/2020	19	CIBC	3/21/2022
2	KBW	5/18/2020	20	BNP Paribas	4/6/2022
3	Goldman Sachs	6/14/2021	21	Scotiabank	4/11/2022
4	Barclays	6/15/2021	22	TD Securities	6/1/2022
5	JPMorgan	7/1/2021	23	Credit Suisse	7/1/2022
6	Mizuho	9/1/2021	24	Jefferies	9/1/2022
7	BNP Paribas	9/1/2021	25	Scotiabank	9/1/2022
8	Morgan Stanley	9/7/2021	26	Goldman Sachs	9/6/2022
9	Bank of America	9/7/2021	27	Capital One	9/6/2022
10	Deutsche Bank	9/7/2021	28	RBC	9/30/2022
11	Stifel	9/28/2021	29	Stifel	10/4/2022
12	KBW	9/28/2021	30	KBW	10/4/2022
13	Bradesco	9/30/2021	31	RBC	5/1/2023
14	Societe Generale	10/1/2021	32	Capital One	5/2/2023
15	Mizuho	2/1/2022	33	Truist	6/30/2023
16	National Bank Financial	2/28/2022	34	Goldman Sachs	8/22/2023
17	Bank of America	3/1/2022	35	HSBC	10/1/2023
18	Wells Fargo	3/14/2022			

Table 2. Summary statistics

This table reports the summary statistics of the analysts by the treatment and control. Panel A reports the number of analysts, gender, age, and experience. Panel B reports the summary statistics on analysts' forecast activities. We calculate the statistics using the pre-RTO data. *Forecast Error* is 100 times the absolute difference between the forecasted EPS and the realized EPS, divided by the stock's price 12 months prior to the earnings announcement date. *Timeliness* is a dummy variable that takes the value of one if the time gap between a forecast of a firm and the firm's most recent earnings announcement is no more than 13 trading hours and zero otherwise. *Firms Covered* is the number of unique firms for which the analyst issued at least one forecast in the pre-period. *Updating Frequency* is the average number of forecasts an analyst issues for a firm they cover. *Forecast Age* is the natural logarithm of the number of days from the forecast to the earnings announcement date. We calculate the *t*-values for the treatment-control differences by clustering at the event level.

	Treatment				Control				Difference	
	N	Mean	Median	STD	N	Mean	Median	STD	Mean	t
Panel A. Analyst level										
Male	1,575	0.858	1	0.349	21,919	0.898	1	0.303	-0.039***	(-3.48)
Age	1,110	41.343	41	8.280	14,918	43.227	43	9.123	-1.884***	(-5.34)
Experience	1,603	11.619	8.548	10.465	23,459	13.398	10.335	11.576	-1.779***	(-4.76)
Firms covered	1,603	10.419	7	9.515	23,459	12.710	12	10.444	-2.291***	(-3.72)
Updating frequency	1,603	1.620	1.462	0.678	23,459	1.459	1.278	0.560	0.161***	(5.00)
Panel B. Forecast activities										
Forecast error	24,989	2.433	0.713	5.119	193,920	1.989	0.548	4.489	0.444***	(5.28)
Forecast age	24,989	5.316	5.375	0.620	193,920	5.330	5.366	0.571	-0.014	(-0.62)
Timeliness	21,705	0.569	1	0.495	166,869	0.550	1	0.497	0.019	(1.59)

Table 3. Baseline regressions on forecast error

This table reports the results on how RTO mandates affected analyst forecast errors. The dependent variable is *Forecast Error*, defined as 100 times the absolute difference between the forecasted EPS and the realized EPS, divided by the stock's price 12 months prior to the earnings announcement date. *Treat* is a dummy variable that takes the value for analysts working for a broker that implemented RTO mandates and zero otherwise. *Post* is a dummy variable that takes the value of one for forecasts issued after an RTO implementation and zero otherwise. *Forecast Age* is the natural logarithm of the number of days from the forecast to the earnings announcement date. Standard errors are clustered at the RTO event level, and *t*-statistics are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	Dep.Var = *Forecast Error*		
	(1)	(2)	(3)
*Treat*Post*	-0.096**	-0.106***	-0.105***
	(-2.65)	(-3.23)	(-3.21)
Treat	0.000	0.038	0.038
	(0.01)	(1.20)	(1.18)
Post	0.005	0.004	0.000
	(0.22)	(0.20)	(0.00)
Forecast Age			-0.101**
			(-2.18)
Observations	435,218	435,202	435,105
R-squared	0.215	0.535	0.735
Firm-Fiscal year FE	Y	Y	Y
Calendar year-month FE	Y	Y	Y
Event FE	Y	Y	Y
Analyst FE	N	Y	Y
Cluster	event	event	event

Table 3. Dynamic effects of forecast accuracy

This table presents the dynamic effects of RTOs on analyst forecast accuracy. The dependent variable is *Forecast Error*, defined as 100 times the absolute difference between the forecasted EPS and the realized EPS, divided by the stock's price 12 months prior to the earnings announcement date. *Treat* is a dummy variable that takes the value for analysts working for a broker that implemented RTO mandates and zero otherwise. *Month t+k* is a dummy variable that takes the value of one for forecasts issued in month *t+k* relative to an RTO implementation and zero otherwise, where *k* ranges from -3 to 3. *Forecast Age* is the natural logarithm of the number of days from the forecast to the earnings announcement date. Standard errors are clustered at the RTO event level, and *t*-statistics are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	Dep.Var = *Forecast Error*	
	(1)	(2)
*Treat*Month t-3*	0.048	0.050
	(0.90)	(0.93)
*Treat*Month t-2*	-0.005	-0.004
	(-0.13)	(-0.10)
*Treat*Month t+1*	-0.054*	-0.052*
	(-1.75)	(-1.70)
*Treat*Month t+2*	-0.109***	-0.107***
	(-4.08)	(-4.05)
*Treat*Month t+3*	-0.116***	-0.115***
	(-2.94)	(-2.92)
Treat	0.027	0.025
	(0.74)	(0.69)
Forecast Age		-0.144**
		(-2.66)
Observations	435,105	435,105
R-squared	0.735	0.735
Firm-Fiscal year FE	Y	Y
Calendar year-month FE	Y	Y
Event FE	Y	Y
Analyst FE	Y	Y
Cluster	event	event

Table 4. Robustness tests

This table presents various robustness tests. Column (1) reports regression results using a propensity-score matched sample. We also add the Pair*Post fixed effects. Column (2) excludes brokers without any RTO-related news from the control. Columns (3) and (4) provide subperiod analyses. Column (5) extends the post-period to nine months. Column (6) introduces more stringent fixed effects, including firm*fiscal year*event, event month*event, and analys*tevent fixed effects. Column (7) clusters standard errors at the event*broker level. Column (8) excludes penny stocks from the analysis.

	Dep.Var = *Forecast Error*							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	PSM	Remove no news controls	2020-2021	2022-2023	(-3, 9)	Stringent FEs	Different cluster	Remove penny stocks
*Treat*Post*	-0.111**	-0.093**	-0.116***	-0.117**	-0.098***	-0.149***	-0.105***	-0.105***
	(-2.69)	(-2.24)	(-3.47)	(-2.22)	(-3.40)	(-4.85)	(-2.81)	(-3.19)
Treat	-0.028	0.035	0.066**	0.054	0.054	0.512	0.038	0.036
	(-0.78)	(0.89)	(2.13)	(1.42)	(1.64)	(1.08)	(1.27)	(1.15)
Post		-0.002	0.003	0.009	-0.029		0.000	-0.000
		(-0.08)	(0.11)	(0.31)	(-1.42)		(0.00)	(-0.01)
Forecast Age	-0.005	-0.196***	-0.142	-0.199***	-0.145***	-0.216**	-0.101***	-0.098**
	(-0.11)	(-3.33)	(-1.23)	(-4.54)	(-4.66)	(-2.57)	(-4.92)	(-2.12)
Observations	264,584	226,473	204,846	229,911	644,099	431,713	435,105	434,877
R-squared	0.779	0.758	0.758	0.730	0.691	0.794	0.735	0.731
Firm*fiscal year FE	Y	Y	Y	Y	Y	N	Y	Y
Calendar year-month FE	Y	Y	Y	Y	Y	Y	Y	Y
Event FE	Y	Y	Y	Y	Y	N	Y	Y
Analyst FE	Y	Y	Y	Y	Y	N	Y	Y
Pair*Post FE	Y	N	N	N	N	N	N	N
Firm*fiscal year*event FE	N	N	N	N	N	Y	N	N
Event month*event FE	N	N	N	N	N	Y	N	N
Analyst*event FE	N	N	N	N	N	Y	N	N
Cluster	event	event	event	event	event	event	event*broker	event

Table 5. Forecast timeliness and forecast quantity

This table reports the results of how RTO mandates affect forecast timeliness, the number of firms covered, and updating frequency. *Trading hour gap* is the number of trading hours between a forecast and the most recent earnings announcement. *Timeliness* is a dummy variable that takes the value of one if *Trading hour gap* is no more than 13 trading hours and zero otherwise. *Firms Covered* is the number of unique firms that an analyst issued at least one forecast in the pre-period or in the post-period. *Updating Frequency* is the average number of forecasts an analyst issues for a firm they cover. Standard errors are clustered at the analyst level, and *t*-statistics are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A. Timeliness

Dep.Var =	*Timeliness*		*Log*(1+*Trading hour gap*)	
	(1)	(2)	(3)	(4)
*Treat*Post*	0.017**	0.014**	-0.078**	-0.057*
	(2.23)	(2.04)	(-2.25)	(-1.93)
Treat	-0.014***	-0.008*	0.061***	0.036*
	(-3.40)	(-1.86)	(2.84)	(1.70)
Post	-0.065***	-0.070***	0.304***	0.313***
	(-4.11)	(-3.02)	(4.29)	(3.55)
Observations	335,964	335,836	335,964	335,836
R-squared	0.382	0.442	0.405	0.514
Firm-Fiscal year FE	Y	Y	Y	Y
Calendar year-month FE	Y	Y	Y	Y
Event FE	Y	Y	Y	Y
Analyst FE	N	Y	Y	Y
Cluster	event	event	event	event

Panel B. Firms covered and updating frequency

Dep. Var =	*Firms Covered*	*log*(1+*Firm Covered*)	*Updating Frequency*	*log*(1+*Updating Frequency*)
	(1)	(2)	(3)	(4)
*Treat*Post*	0.117	0.033**	0.039	0.018
	(1.52)	(2.29)	(1.21)	(1.34)
Treat	-0.499***	-0.071***	-0.034	-0.017**
	(-3.86)	(-5.28)	(-1.67)	(-2.17)
Post	0.124	-0.004	-0.019	-0.007
	(0.82)	(-0.25)	(-0.50)	(-0.45)
Observations	55,448	55,448	55,448	55,448
R-squared	0.854	0.783	0.548	0.461
Analyst FE	Y	Y	Y	Y
Event FE	Y	Y	Y	Y
Cluster	event	event	event	event

Table 6. The first forecasts

This table reports how RTO mandates affected analyst forecast errors for two subsamples. Column (1) reports the result of the subsample of the first forecasts issued after the most recent earnings announcement. Column (2) reports the results of other forecasts. The dependent variable is *Forecast Error*, defined as 100 times the absolute difference between the forecasted EPS and the realized EPS, divided by the stock's price 12 months prior to the earnings announcement date. *Treat* is a dummy variable that takes the value for analysts working for a broker that implemented RTO mandates and zero otherwise. *Post* is a dummy variable that takes the value of one for forecasts issued after an RTO implementation and zero otherwise. *Forecast Age* is the natural logarithm of the number of days from the forecast to the earnings announcement date. Standard errors are clustered at the RTO event level, and *t*-statistics are reported in parentheses. The last row reports the *p*-values of the *F*-test for whether the between-subsample difference in the coefficients on *Treat*Post* is statistically significant. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	Dep.Var = *Forecast Error*	
	(1)	(2)
	First forecasts	Other forecasts
*Treat*Post*	-0.160***	0.100
	(-5.12)	(0.92)
Treat	0.052*	-0.087
	(1.77)	(-0.91)
Post	0.036	-0.082
	(1.06)	(-1.39)
Forecast Age	-0.057	-0.212***
	(-1.12)	(-3.76)
Observations	336,917	94,824
R-squared	0.735	0.775
Firm-Fiscal year FE	Y	Y
Calendar year-month FE	Y	Y
Event FE	Y	Y
Analyst FE	N	Y
Cluster	event	event
F-test (*p*-value)	0.018	

Table 7. Cross-analyst heterogeneity

This table reports the cross-analyst heterogeneity tests on forecast accuracy. In Panel A, we split our sample based on analyst's experience. In panel B, we split our sample by gender, age, or political leaning of the state an analyst resides in. See Table 3 for variable definitions. Standard errors are clustered at the analyst level, and *t*-statistics are reported in parentheses. The last row reports the *p*-values of the *F*-test that tests whether the between-subsample difference in the coefficient of *Treat*Post* is statistically significant. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Experience

	Dep. Var = *Forecast Error*						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	<=0.5 yr	<=1 yr	<=2 yr	<=3 yr	<=4 yr	<=5 yr	>5 yr
*Treat*Post*	-0.661**	-0.378**	-0.326***	-0.291***	-0.205***	-0.155***	-0.071
	(-2.16)	(-2.67)	(-3.20)	(-3.66)	(-2.80)	(-3.70)	(-1.61)
Treat	0.613*	0.067	0.161	0.122	0.075	0.066*	0.028
	(1.98)	(0.47)	(1.62)	(1.55)	(1.40)	(1.96)	(0.65)
Post	-0.041	0.041	-0.007	0.014	-0.028	-0.002	-0.022
	(-1.02)	(0.93)	(-0.21)	(0.52)	(-0.91)	(-0.06)	(-0.66)
Forecast Age	0.006	0.051	-0.054	-0.089**	-0.139***	-0.088*	-0.117**
	(0.06)	(0.78)	(-0.85)	(-2.31)	(-2.77)	(-1.78)	(-2.55)
Observations	11,295	23,454	43,351	62,661	98,825	175,441	258,757
R-squared	0.890	0.853	0.823	0.808	0.779	0.750	0.744
Firm-Fiscal year FE	Y	Y	Y	Y	Y	Y	Y
Calendar year-month FE	Y	Y	Y	Y	Y	Y	Y
Event FE	Y	Y	Y	Y	Y	Y	Y
Analyst FE	Y	Y	Y	Y	Y	Y	Y
Cluster	Event	Event	Event	Event	Event	Event	event
F-test (*p*-value)	(<=3 yr) vs. (>3y): *p* = 0.024						

Panel B: Age and gender

	Dep. Var = *Forecast Error*						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Age		Gender		State		
	<=35	Male	Male	>35	Republican	Democratic	Democratic; ex NY
*Treat*Post*	-0.323***	-0.082*	-0.082*	-0.003	0.148	-0.131***	-0.141***
	(-3.95)	(-1.90)	(-1.90)	(-0.09)	(1.02)	(-3.54)	(-3.07)
Treat	0.264***	0.017	0.017	-0.036	0.137	-0.005	0.018
	(3.26)	(0.41)	(0.41)	(-1.13)	(1.03)	(-0.11)	(0.57)
Post	-0.033	-0.002	-0.002	-0.008	0.006	0.015	0.007
	(-0.46)	(-0.10)	(-0.10)	(-0.25)	(0.09)	(0.61)	(0.17)
Forecast Age	-0.125**	-0.119**	-0.119**	-0.104***	-0.132	-0.100**	-0.120*
	(-2.63)	(-2.54)	(-2.54)	(-4.52)	(-1.55)	(-2.14)	(-1.89)
Observations	51,074	376,501	376,501	238,145	63,369	297,729	113,020
R-squared	0.805	0.737	0.737	0.755	0.754	0.755	0.783
Firm-Fiscal year FE	Y	Y	Y	Y	Y	Y	Y
Calendar year-month FE	Y	Y	Y	Y	Y	Y	Y
Event FE	Y	Y	Y	Y	Y	Y	Y
Analyst FE	Y	Y	Y	Y	Y	Y	Y
Cluster	event	event	event	event	event	event	Event
F-test (*p*-value)	0.0006		0.363			0.363	

Table 8. Effort reallocation

This table reports the RTO effects (i.e., the coefficients on *Treat*Post* in our baseline model) for various subsamples based on the relative importance of a stock in an analyst's portfolio. Within each analyst portfolio, we sort all stocks into terciles and the ones with the highest market capitalization (or volume, or institutional ownership) are considered the most important stocks. All three characteristics are measured using the most recent data before RTO. In addition, we determine a stock's percentiles for each of the three characteristics and use the average of these percentiles as the importance index. The last column reports the *p*-values of the *F*-test that tests whether the difference in the coefficient of *Treat*Post* is statistically significant between the Highest tercile and the Middle+Lowest group. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	Highest tercile	Middle tercile	Lowest tercile	Middle + Lowest	*F*-test (*p*-value)
Market capitalization	-0.034	-0.074*	-0.161**	-0.116***	
	(-1.09)	(-1.75)	(-2.56)	(-3.09)	0.023
Volume	-0.070*	-0.030	-0.198***	-0.121***	
	(-2.02)	(-0.81)	(-2.97)	(-2.82)	0.329
Institutional Ownership	-0.028	-0.079*	-0.141**	-0.125***	
	(-0.83)	(-1.95)	(-2.05)	(-3.26)	0.017
Importance index	-0.020	-0.096**	-0.150**	-0.131***	
	(-0.63)	(-2.42)	(-2.24)	(-3.33)	0.010

Table 9. Analyst turnover

This table reports the regression results of analyst turnover. The dependent variable is *Turnover*, which is a dummy variable that takes the value of one if an analyst leaves his/her current broker in the 12 months following an RTO and zero otherwise. *Treat* is a dummy variable that takes the value for analysts working for a broker that implemented RTO mandates and zero otherwise. Panel A is based on a linear probability model, and Panel B is based on a Cox hazard model. In the linear probability model, we omit the 2023 data. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel. Linear probability model

	Dep. Var = *Turnover*		
	(1)	(2)	(3)
	All	2020-2021	2022
Treat	0.025**	0.025**	-0.014
	(2.20)	(2.05)	(-0.58)
Observations	22,041	10,199	11,821
R-squared	0.154	0.188	0.200
Event FE	Y	Y	Y
Brokerage FE	Y	Y	Y
Cluster	event	event	event

Panel B. Hazard model

	Dep. Var = *Turnover*		
	(1)	(2)	(3)
	All	2020-2021	2022-2023
Treat	0.171**	0.162*	-0.127
	(2.28)	(1.70)	(-0.69)
Observations	23,967	10,221	13,746
Pseudo R-squared	0.003	0.002	0.003
Event FE	Y	Y	Y
Brokerage stratification	Y	Y	Y
Cluster	event	event	event

Internet Appendix

Section A. The prompt used to extract topics from RTO news reports

You are an expert at analyzing articles about return to office (RTO) policies. First, read and understand the article content about return to office.

IMPORTANT: When identifying keywords or phrases, DO NOT include obvious terms like "return to office", "RTO", "work from home", "remote work", or similarly obvious concepts that merely restate the general topic.

Instead, focus on extracting specific aspects, strategies, challenges, or unique perspectives discussed within the return-to-office framework. Identify deeper concepts like organizational approaches, workforce impacts, management strategies, employee experience factors, or specific implementation methods mentioned in the article. For example, instead of "hybrid work model" (which is too obvious), you might extract phrases like "departmental autonomy", "flexible scheduling", "permanent transformation", or "talent retention strategy" that capture the specific angles and nuances discussed in the article.

Your keywords should help someone understand what SPECIFIC ASPECTS of return-to-office are being discussed, assuming they already know the article is about return-to-office generally.

IMPORTANT: When analyzing each article, consider ONLY the content of the current article. Completely disregard any information, themes, or patterns from previously analyzed articles. Each analysis must be independent and based solely on the specific content provided in the current document. This ensures unbiased keyword extraction that truly reflects what is uniquely discussed in each individual article.

You MUST format your response as a JSON object with exactly this structure:

```
{
  "Key word 1": "first key phrase",
  "Key word 2": "second key phrase",
  "Key word 3": "third key phrase",
  "Key word 4": "fourth key phrase",
  "Key word 5": "fifth key phrase"
}
```

The key phrases should be short (1-3 words) but descriptive terms that capture the core concepts discussed in the article.

Section B. Summary statistics after removing no-RTO news brokers

Table IA1 reports the summary statistics after removing the brokers without RTO news.

Table IA1. Summary statistics after removing no-news brokers

This table reports the summary statistics of the analysts by the treatment and control. Compared with the summary statistics reported in the paper (Table 2), we remove brokers without RTO news. Panel A reports the number of analysts, gender, age, and experience. Panel B reports the summary statistics on analysts' forecast activities. We calculate the statistics using the pre-RTO data. *Forecast Error* is 100 times the absolute difference between the forecasted EPS and the realized EPS, divided by the stock's price 12 months prior to the earnings announcement date. *Timeliness* is a dummy variable that takes the value of one if the time gap between a forecast of a firm and the firm's most recent earnings announcement is no more than 13 trading hours and zero otherwise. *Firms Covered* is the number of unique firms for which the analyst issued at least one forecast in the pre-period. *Updating Frequency* is the average number of forecasts an analyst issues for a firm they cover. *Forecast Age* is the natural logarithm of the number of days from the forecast to the earnings announcement date. We calculate the *t*-values for the treatment-control differences by clustering at the event level.

	Treatment				Control				Difference	
	N	Mean	Median	STD	N	Mean	Median	STD	Mean	t
Panel A. Analyst level										
Male	1,575	0.858	1	0.349	9,033	0.878	1	0.328	-0.019	(-1.59)
Age	1,110	41.343	41	8.280	6,148	42.385	42	8.379	-1.042***	(-2.72)
Experience	1,603	11.619	8.548	10.465	9,142	13.267	10.383	11.168	-1.649***	(-3.87)
Firms covered	1,603	10.419	7	9.515	9,142	13.360	13	8.902	-2.942***	(-4.44)
Updating frequency	1,603	1.620	1.462	0.678	9,142	1.583	1.417	0.626	0.037	(0.75)
Panel B. Forecast activities										
Forecast error	24,989	2.433	0.713	5.119	89,881	2.180	0.599	4.835	0.253	(1.55)
Forecast age	24,989	5.316	5.375	0.620	89,881	5.350	5.375	0.534	-0.035	(-1.08)
Timeliness	21,705	0.569	1	0.495	75,332	0.575	1	0.494	-0.006	(-0.44)