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The gender effects of COVID: evidence from equity analysts

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Abstract

We use COVID-19 and sell-side analysts as an experiment to study the effects of gender on labor productivity. We find that the forecast accuracy of female analysts declined more than that of male analysts, especially when schools were closed and among analysts who were more likely to have young children, were inexperienced, were busier, or lived in southern states of the US. Relative to male analysts, females also reduced their forecast timeliness and resorted to more heuristic forecasts but did not reduce coverage or updating frequency. Relative to pre pandemic, female analysts' careers were more negatively affected than male analysts'. Overall, our results show that the pandemic impacted female analysts more than males through the quality of their forecasts but not the quantity.

Keywords COVID-19 · Pandemic · Financial analysts · Gender gap · Decision heuristics

JEL Classification G14 · G20 · J4 · J16 · J24 · J32

1 Introduction

A persistent gender gap exists in business, especially at the top echelons, with women underrepresented. For example, women represented less than 10% of CEOs, CFOs, and board directors at listed firms (Wolfers 2006; Adams and Ferreira 2009; Huang and Kisgen 2013), among mutual fund managers (Atkinson et al. 2003; Niessen-Ruenzi and Ruenzi 2019), venture capital general partners (Ewens and Townsend 2020; Gompers et al. 2022), and sell-side security analysts (Kumar 2010;

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Fang and Huang 2017). Studies have also demonstrated that women face higher hurdles in being successful.[1] The financial advisor industry is more forgiving of misconduct by men than by women (Egan et al. 2022). Female analysts benefit less from connections in both job performance and others' subjective evaluations (Fang and Huang 2017) and get lower media coverage (Kumar 2010).

The spread of the COVID-19 pandemic and subsequent countermeasures such as school closures and social distancing are likely to exacerbate these gaps. School and daycare center closures increased childcare needs dramatically. Due to social distancing requirements, many parents had little choice other than to take care of their children themselves, at least at the beginning of the pandemic. Given that mothers took responsibility for a much larger share of childcare and household duties than fathers (Alon et al. 2020; Deryugina et al. 2021), we expect women to be more affected than men during the pandemic. Hence, the pandemic provides a natural experiment to study the effects of childcare and household duties on the gender gap and labor productivity.

Challenges exist in estimating the gender effect of the pandemic. In many sectors, men and women performed different tasks that were affected differently. The COVID-19 pandemic caused not only a public health crisis but also an economic one. It had heterogeneous impacts on different sectors and different types of jobs. For example, the pandemic had a larger impact on the sectors with more women employees, contributing to a larger increase in the unemployment rate of women than men (Adams-Prassl et al. 2020; Alon et al. 2020; Cajner et al. 2020; Mongey et al. 2021). Even within the same sector, men and women might have performed different tasks that were affected differently. Distinguishing the gender effect from other possible effects (such as sector or task effects) is important from a policy perspective. If sector composition is driving the widening of the gender gap, relief policy should target sectors instead of gender.

This paper uses a difference-in-differences approach to study how the pandemic affected female and male sell-side equity analysts differently. The richness of the setting allows us to compare female and male analysts while requiring them to perform the same tasks: forecasting the same firms' earnings for the same fiscal quarter. Hence, our estimate allows us to have a clean gender effect estimate that is free of confounding factors.

The analyst setting is unique in several other dimensions. First, in contrast to many other sectors, the analyst sector features superior skills of female analysts. Female analysts issue more accurate forecasts, and their revisions have a stronger market impact. Kumar (2010) attributes this pattern to gender-based self-selection. Specifically, due to the perception of discrimination in the analyst labor market, only female analysts with superior forecasting abilities enter the profession. As a result of such selection, a typical female analyst is more skilled than a male analyst. Second, the analyst job is time-consuming and requires analysts to provide timely updates

[1] For example, female fund managers receive significantly lower inflows (Atkinson et al. 2003; Niessen-Ruenzi and Ruenzi 2019). Female-led startups experience significantly more difficulty garnering interest and raising capital (Ewens and Townsend 2020; Hebert 2020). Benson et al. (2021) document that women are significantly less likely to be promoted despite receiving higher performance evaluations.

on covered firms upon receiving new information. Such a job requirement allows researchers to capture the potential impacts of an increased parenting and household burden on job performance. Third, individual analysts' performance can be objectively measured. Individual analysts forecast the earnings of the firms they cover, and we can compare their forecasts with the realized earnings to obtain a direct and objective measure of individual productivity. Individual analysts issue their forecasts relatively frequently, giving us a timely measure of their accuracy. In our empirical analysis, we focus on one-quarter-ahead earnings forecasts. Last, analysts' forecast announcements are dated, allowing us to study the dynamics of analyst forecast behaviors before and during the pandemic. This setting also allows us to measure not only the *quantity* (i.e., the number of firms covered and updating frequency) but also the *quality* (i.e., accuracy and timeliness) of analyst forecasts. In contrast, the literature on the gender effect of COVID-19 has focused on studying quantity alone.

Our analysis reveals that female analysts' forecast error significantly increased relative to that of male analysts due to the pandemic, but changes in the number of firms covered and updating frequency were not related to analyst gender. The effect is economically sizable. The relative increase in forecast error is 14.5% of the unconditional average. It is well known that forecasts made closer to earnings announcements are more accurate. The median number of days from the forecast to the earnings announcement date is 49 days. In our baseline specification, the pandemic effect is equivalent to forcing female analysts to make their forecasts 86 days, instead of 49 days, before earnings announcements.

Consistent with the conjecture that the parenting burden disproportionately fell on the shoulders of women, we find that these results are stronger when schools were closed and among analysts who were more likely to have young children.[2] The results are also stronger among analysts living in southern states, where gender role attitudes are, in general, more traditional (Rice and Coates 1995; Ke 2021), and among analysts who were less experienced and busier (i.e., covered more stocks) before the pandemic. All this is consistent with the view that the pandemic was a time allocation shock that affected female analysts more strongly.

Consistent with the Hirshleifer et al. (2019) finding that judgments and decisions made under greater pressure, distraction, or fatigue (i.e., a decline in decision quality after an extensive session of decision-making) tend to be made more heuristically, we find that, as a result of the pandemic, relative to male analysts, female analysts herded more closely with the consensus forecast, had a higher likelihood of reissuing their previous outstanding forecast, and had a higher likelihood of issuing a rounded forecast. Female analysts were also less likely to provide timely forecasts right after firms' earnings announcements, relative to male analysts.

Since the pandemic reduced the informativeness of female analysts' forecasts, we expect the stock market to react accordingly. Consistent with Kumar (2010), we find that the forecast revisions made by female analysts had a stronger market response

[2] We proxy the likelihood of having young children using analyst age. Ideally, we would like to have information about analysts' family structure, such as whether the analyst was married and how many children he/she needed to take care of. Unfortunately, such information is difficult to access.

before the pandemic. This pattern reversed during the pandemic, although the during-pandemic difference was not statistically significant.

This widening gender gap decayed quickly and disappeared by May and June of 2020. In March 2020, at the outset of the pandemic, the relative increase in female analysts' forecast error was more than 20% of the unconditional average forecast error. In April and May, the relative increase in female analysts' forecast error shrank to around 10% of the unconditional average. From June to August, it shrank further, to less than 5% of the unconditional average, and became statistically insignificant. Our finding that the gender gap widened most at the pandemic outbreak and started to shrink around May and June is consistent with Alon et al. (2022), who study the gender gap in unemployment. However, Alon et al. (2022) report that, in the general population, the widening gender gap persisted much longer. The difference suggests that the analysts were affected less severely relative to the general population.

In the last part of the paper, we examine whether COVID had any long-term impact on analysts' careers. Our analysis suggests that before the pandemic, the odds of becoming an all-star analyst and working for a top brokerage firm were 53.8% and 191% higher for female analysts compared to male analysts, respectively. In 2020 and 2021, the pattern reversed. The odds ratio implies that female analysts are 27.7% less likely to become an all-star analyst and 36.7% less likely to work for a top brokerage firm. The changes are both economically and statistically significant.

Overall, the evidence is consistent with the parenting and household burden falling disproportionately on the shoulders of women. One alternative explanation is that women become more pessimistic during an economic downturn, and female analysts' forecasts became more pessimistic and less accurate.[3] Examining a direct measure of forecast optimism, we find no evidence that forecasts made by female analysts became more pessimistic relative to forecasts by male analysts during the pandemic. Besides, we use the 2007–2009 global financial crisis as a placebo and do not find any evidence of a widening gender gap during that economic downturn. Another possibility is that the pandemic increased the competitiveness of the analyst job with a rising unemployment rate and the challenging job market. Studies show that men have a stronger preference for, and a better ability to respond to, increased competitiveness (Gneezy et al. 2003; Niederle and Vesterlund 2007; Reuben et al. 2015). However, this alternative story is inconsistent with the placebo results from the global financial crisis, which hit the financial industry more severely than the COVID-19 pandemic.

Our finding is consistent with several recent studies that document a widening gender gap during the pandemic. Cajner et al. (2020) document that employment declines caused by the pandemic were about four percentage points greater for women than for men. Coibion et al. (2020) document that the pandemic caused more women than men to quit the labor force. Alon et al. (2020) and Alon et al. (2022) argue that the pandemic had a larger impact on sectors with high female employment shares, which would explain part of the unequal employment declines.

[3] One possible reason is that women had more woman friends who were more negatively affected by the pandemic. Like the experience effect (Malmendier and Nagel 2011; D'Acunto et al. 2021), this might have led women to have more pessimistic expectations about the pandemic than men.

Most other studies on the gender effects of the pandemic focus on academics from various fields.[4] These studies measure academic productivity either by survey or by counting the number of working papers or journal submissions.

We highlight two distinctions of our study. First, the existing studies either examine the employment rate or the *quantity* of academic research output. In comparison, our findings emphasize the *quality* dimension of productivity. Interestingly, we find no gender difference in terms of the quantity of analysts' research output. That finding suggests the importance of considering both quantity and quality in measuring productivity. Second, another concern with the above studies is that the pandemic might have heterogeneous impacts along other dimensions that correlate with gender, confounding the estimation of the gender effect. For example, men and women might have different subfield expertise. The pandemic created new research opportunities, and these new opportunities benefited female and male academics unequally.[5]

In sociology and other non-finance fields, there is a well-established strand of literature on the motherhood penalty: having children hurts women in terms of pay, perceived competence, and benefits (Budig and England 2001; Anderson et al. 2002; Correll et al. 2007). Although the role of gender in finance has received extensive attention (Barber and Odean 2001; Goldsmith-Pinkham and Shue 2023; and studies cited at the beginning of this paper), the motherhood penalty is relatively under-exploited. Our findings provide indirect evidence of a motherhood penalty among finance professionals. We await future research that will conduct more direct tests and comprehensive investigations into this area.

Our study is also related to the burgeoning accounting and labor literature. One strand of this literature examines the role of accounting information in labor markets. Researchers have examined how employees react to earnings announcements (Choi et al. 2023a, b; deHaan et al. 2023) and the revelation of financial misconduct (Carnes et al. 2023; Toeh et al. 2023). Choi et al. (2023a, b) find that employees of firms with lower financial reporting quality have higher wages. The second strand of the literature examines the causes and consequences of the accounting labor markets. Accounting labor supply is affected by occupational licensing (Cascino et al. 2021; Barrios 2022) and local financial fraud (Choi et al. 2023a, b). Audit personnel salaries are positively correlated with audit quality (Hoopes et al. 2018), and tax planning knowledge diffuses via the labor market of tax department employees. We contribute to this literature by providing evidence of how the pandemic affected the gender gap and labor productivity of equity analysts.

[4] See for example, Amano-Patino et al. (2020), Andersen et al. (2020), Barber et al. (2021), Cui et al. (2022), Deryugina et al. (2021), King and Frederickson (2020), Kruger et al. (2023), Myers et al. (2020), and Vincent-Lamarre et al. (2020).

[5] King and Frederickson (2020) and Cui et al. (2022) document a surge in the number of preprints newly uploaded to several preprint depositories such as bioRxiv (a preprint server mainly for biological science), arXiv (a preprint server mainly for physics, math, computer science, and statistics), and the Social Science Research Network (SSRN), consistent with the pandemic creating new research opportunities. Evidence shows that female researchers are underrepresented in the new and flourishing area of COVID-19 research for many fields (Vincent-Lamarre et al. 2020), including economics (Amano-Patino et al. 2020) and medical research (Andersen et al. 2020). Chari and Goldsmith-Pinkham (2017) report a large dispersion of the fraction of female authors across NBER Summer Institute programs.

Our study is one of the first to study how the pandemic affected analysts. Equity analysts are important information intermediaries, and their proper functioning is critical to the functioning of capital markets. Landier and Thesmar (2020) and Hong et al. (2021) use analyst forecasts to study the market's earnings expectations during the pandemic. Dechow et al. (2021) study the relationship between implied equity duration and analyst forecast revisions in response to the pandemic. However, they do not study the gender effect. Du (2023), a concurrent paper, also finds that during the initial phases of the pandemic, female analysts were more adversely affected than male analysts. While both papers address a similar fundamental question, there are important differences in the scope and granularity of the analyses. Du (2023) focuses more on the role of motherhood; our study offers more comprehensive analyses of analyst forecasting behavior. The two papers also report different results on forecast accuracy, which we discuss in more detail in the results section.

Finally, we contribute to the literature studying how pressure, distraction, and fatigue affect decision-making by providing plausibly causal evidence that distraction hurts decision quality (e.g., Hirshleifer et al. 2009, 2019; Driskill et al. 2020).

2 Data

Data on analysts' earnings per share (EPS) forecasts are collected from the Institutional Brokers' Estimate System (I/B/E/S) Detail History file covering the period from January 2019 to August 2020. We focus on one-quarter-ahead EPS forecasts, as we want to study analysts' timely forecast activities. We use the unadjusted file to mitigate the rounding problem in I/B/E/S and adjust for stock splits so that the forecasts and EPS are comparable. CRSP had not updated the daily stock return data to 2020 when we started work on this project. We obtain daily stock prices from Compustat North America and follow Bessembinder et al. (2023) to compute daily returns for individual stocks.

Our sample starts with the 2,351 analysts who provided earnings forecasts in 2019. First, we identify an analyst's last name, first initial, and brokerage affiliation using the I/B/E/S Detail Recommendation file. The majority of analysts participated in firms' earnings conference calls. From the earnings conference call transcripts provided by FACTSET Events & Transcripts, we obtain participants' full names and affiliations and match them with the I/B/E/S analysts.[6] An analyst's full name is identified if the last name, first initial, and brokerage affiliation match the equivalent information from the conference call transcripts.[7] Through this procedure,

[6] An earnings conference call typically has two parts: a management presentation and a Q&A between analysts and firm managers. We use text parsing tools to go through each transcript and extract the full names and affiliations of all conference call participants. Due to career concerns, sell-side analysts have a strong incentive to participate in earnings conference calls hosted by their covered firms, as information conveyed during such calls provides important inputs to their forecasts and recommendations (Mayew et al. 2013; Jung et al. 2015).

[7] 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.

we identify 2,097 analysts' full names. Second, we hand collect analysts' gender, location, and college graduation year from LinkedIn. If an analyst does not have a LinkedIn profile, we conduct a Google search. In a small number of cases, we can locate these analysts from other professional web pages. If LinkedIn or other web searches do not return sufficient information, we infer analysts' gender based on their first names. This step results in 1,968 analysts with gender information. Analysts' age is generally not directly available, and we calculate it by assuming that analysts graduated from college at age 22.[8]

Our primary dependent variable of interest is analyst forecast accuracy, inversely proxied by analysts' percentage of absolute forecast error (*Forecast Error*). *Forecast Error* for analyst i on stock j's EPS of quarter q issued at time t is equal to the absolute value of actual company EPS minus the EPS forecast of analyst i for firm j at time t, divided by the stock's price 12 months prior to the quarterly earnings announcement date and multiplied by 100.

$$Forecast\ Error_{i,j,q,t} = 100 * \frac{\left| Actual\ EPS_{j,q} - Forecasted\ EPS_{i,j,q,t} \right|}{Price_{j,q-4}}$$

Given time constraints, analysts may reduce the quantity of their forecasts to maintain their forecast quality (i.e., accuracy). We use two measures to capture the quantity dimension of analyst forecasts. The first measure is *Firms Covered*, which we define as the number of unique firms an analyst covers. The second measure is *Updating Frequency*, which we define as the number of forecasts an analyst issues for every firm they cover at the monthly level.

We calculate several other variables to capture analysts' forecast behavior. Following Clement and Tse (2005), we define $Herding_{i,j,q,t}$ as a dummy variable that takes the value of one if analysts i's forecast of company j's EPS of quarter q is between the consensus forecast at time t and the analyst's previous forecast, zero otherwise. Following Dechow and You (2012), we define $Rounding_{i,j,q,t}$ as a dummy variable that takes the value of one if a forecast ends with zero or five in the penny digit, zero otherwise. Following Hirshleifer et al. (2019), we define $Reissue_{i,j,q,t}$ as a dummy variable that takes the value of one if a forecast is reissued, zero otherwise. Hirshleifer et al. (2019) argue that analysts tend to resort to more heuristic decisions under greater pressure, distraction, or fatigue by herding more closely with the consensus forecast, reissuing their previous outstanding forecasts, and issuing a rounded forecast. Following deHaan et al. (2017), we define $Timeliness_{i,j,q,t}$ as a dummy variable that takes the value of one if analyst i issues an EPS forecast within days $[0, +2]$ of firm j's earnings announcement at quarter q, zero otherwise.

Table 1 reports the summary statistics of the main variables in our analysis for female and male analysts separately. Panel A is based on the entire sample. In Panel B,

[8] Many analysts work in teams (Fang and Hope 2021). For forecasts issued by teams, the analysts identified in the I/B/E/S data set are typically the lead analysts. Fang and Hope (2021) provide evidence that team membership can improve forecast accuracy. Given that some teams have both female and male analysts, we expect that focusing on the gender of the lead analysts leads to an underestimate of the gender effects.

Table 1 Summary statistics

Panel A: Analyst brokerage and age distribution					
	# of analysts	# of brokerage	Age in 2019		
			Mean	p10	p90
Male	1,744	177	37	22	52
Female	224	74	34	22	48
Full sample	1,968	184	37	22	52

Panel B: Forecast activities				
	Female Analysts			
Variables	Obs	Mean	Stdev	Median
Forecast Error	41,409	1.118	3.209	0.258
Firms Covered	218	12.197	8.275	11.000
Updating Frequency	36,827	1.124	0.353	1.000
Herding	10,537	0.361	0.480	0.000
Reissue	41,409	0.470	0.499	0.000
Rounding	41,409	0.172	0.377	0.000
Timeliness	41,409	0.491	0.500	0.000
Forecast Age	41,409	4.248	0.878	3.908
	Male Analysts			
Variables	Obs	Mean	Stdev	Median
Forecast Error	343,428	1.145	3.383	0.254
Firms Covered	1,716	12.871	8.419	13.000
Updating Frequency	307,659	1.116	0.340	1.000
Herding	84,675	0.340	0.474	0.000
Reissue	343,428	0.511	0.500	1.000
Rounding	343,428	0.189	0.391	0.000
Timeliness	343,428	0.502	0.500	1.000
Forecast Age	343,428	4.331	0.870	3.940

This table reports the summary statistics of the analysts by gender. Panel A reports the number of analysts, the number of brokerage firms affiliated with these analysts, and the analysts' age distribution. Panel B reports the summary statistics on analysts' forecast activities. We calculate the statistics in Panel B using the pre-pandemic data from January 2019 to February 2020. *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 quarterly earnings announcement date. *Firms Covered* is the number of unique firms for which the analyst issued at least one forecast over the sample period. *Updating Frequency* is the number of forecasts an analyst issues for every firm they cover at the monthly level. *Herding* is a dummy variable that takes the value of one for forecasts that are between the analyst's prior forecast and the consensus forecast, zero otherwise. *Reissue* is a dummy variable that takes the value of one if a forecast is reissued, zero otherwise. *Rounding* is a dummy variable that takes the value of one if a forecast ends with zero or five in the penny digit, zero otherwise. *Timeliness* is a dummy variable that takes the value of one if an analyst issues an EPS forecast within days $[0,+2]$ of a firm's quarterly earnings announcement, zero otherwise. *Forecast Age* is the natural logarithm of the number of days from the forecast to the earnings announcement date

we calculate analyst forecast characteristics using the pre-pandemic data. We winsorize the continuous variables at the 1% and the 99% levels to mitigate the impact of outliers. There were 1,968 unique analysts in our sample, 224 of whom were female. The mean age of male and female analysts was 37 and 34, respectively. In total, female analysts made 41,409 forecasts, and male analysts made 343,428. The fraction of female analysts in our sample is similar to that reported by Fang and Huang (2017). These analyses were affiliated with 184 unique brokerage firms.

Female and male analysts showed remarkably similar forecast characteristics before the pandemic outbreak. Female analysts had a slightly lower *Forecast Error* than male analysts. The mean *Forecast Error* was 1.118 (e.g., 33.5 cents for a \$30 stock) and 1.145 (e.g., 34.4 cents for a \$30 stock) for female and male analysts, respectively. This finding is consistent with Kumar (2010), who finds that female analysts issue more accurate forecasts. Both female and male analysts covered a similar number of firms (12.197 for females and 12.871 for males). Every month, female analysts issued 1.124 forecasts for every firm they covered, and male analysts issued 1.116 forecasts. Female analysts issued a higher fraction of herded forecasts (36.1% for females vs. 34.0% for males). Female analysts issued a lower fraction of rounded forecasts (17.2% for females and vs. 18.9% for males) and had a lower likelihood of reissuing a previous forecast (47.0% for females vs. 51.1% for males). Male analysts made more timely forecasts (49.1% for females vs. 50.2% for males). However, all the differences are economically small. *Forecast Age* is the natural logarithm of the number of calendar days from the forecast to the earnings announcement date (Clement 1999). Female and male analysts showed some difference in *Forecast Age*. On average, forecasts issued by the female and male analysts were announced about 70 and 76 days before the earnings announcements, respectively.

3 Empirical results

3.1 Forecast accuracy

3.1.1 Baseline results

Our main prediction, based on the existing literature and the assumption that mothers increased their childcare time more than fathers did, is that COVID-19 affected female analysts more than male analysts, and female analysts issued less accurate EPS forecasts after the pandemic outbreak than male analysts. To conduct the test, we estimate the following regression model:

$$Forecast\ Error_{i,j,q,t} = \beta_1 Female_i * Post_t + \beta_2 X_{i,j,q,t} + \gamma_{j,q} + \delta_{i,j} + \theta_t + \varepsilon_{i,j,q,t}$$

where i indicates analysts, j indicates firms, q indicates the fiscal quarter to which the analyst's forecast applies, and t indicates the day when the analyst issues the forecast. $Female_i$ is a dummy variable that takes the value of one if analyst i is female, zero otherwise. $Post_t$ is a dummy variable that takes the value of one if a forecast is issued after the COVID-19 outbreak, zero otherwise. Specifically, we define the post-period to be from March 1, 2020, onward. We choose March 1, 2020, because the surge of diagnosed COVID-19 cases started in early March 2020, and a majority of the states issued mandatory school closing orders in March 2020. Our results are similar if we use March 15, 2020, as the cutoff. $X_{i,j,q,t}$ is a set of control variables. The primary variable of interest is $Female_i * Post_t$. If female analysts were affected more by COVID-19, we expect $\beta_1 > 0$.

In all of the specifications, we include the *Firm*×*Fiscal Quarter* fixed effects ($\gamma_{j,q}$). With the *Firm*×*Fiscal Quarter* 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 quarter.[9] Of our sample firms, 88.4% are covered by both female and male analysts, allowing us to estimate the gender effects with these fixed effects. Depending on the specification, besides the *Firm*×*Fiscal Quarter* fixed effects, we include several other groups of fixed effects. In the most stringent specification, we have *Analyst* × *Firm* fixed effects ($\delta_{i,j}$) and year-month fixed effects (θ_t). Note that $Female_i$ is absorbed by the *Analyst* × *Firm* fixed effects, and $Post_t$ is absorbed by the year-month fixed effects. The *Analyst* × *Firm* fixed effects also absorb the *Analyst* fixed effects.

Given the granularity of our panel data, we can estimate all of these high-dimensional fixed effects simultaneously. With all these fixed effects included, our estimated effect comes from comparing the change in forecast accuracy by female analysts from pre- to during-COVID-19 periods, relative to the change in forecast accuracy of male analysts covering the same stock over the same period. 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*. Our results hold with the standard set of controls (see Table A1 in the Internet Appendix). As expected, with our stringent fixed effects, there is little remaining variation of these control variables. Hence, we do not include them in our analysis. We cluster our standard errors by analyst. Our results are similar if we double-cluster standard errors by analyst and forecast month.[10]

Table 2 reports the regression results. In column (1), we add the *Firm* × *Fiscal Quarter* and *Analyst* fixed effects. In column (2), we add the *Firm* × *Fiscal Quarter*, *Analyst* × *Firm*, and year-month fixed effects. In column (3), we further add *Forecast Age*. The coefficient of *Forecast Age* is strongly positive, consistent with the finding in prior literature that forecasts issued closer to earnings announcements are more accurate (Clement 1999). The coefficient of *Female* ∗ *Post* is around 0.16 in all three specifications, suggesting that our results are not sensitive to changes in empirical specifications. The coefficient is statistically significant at the 1% level in

[9] There is another widely used method to control for the firm- or time-specific factors that affect forecast accuracy (Jacob et al. 1999; Clement 1999; Hong et al. 2000; Cowen et al. 2006). With this method, researchers adjust the accuracy of an analyst's EPS forecasts for a particular firm at a given time by subtracting the mean level of accuracy for all analysts who make forecasts for the same firm and time period within a comparable forecast horizon. We prefer the fixed effects method advocated by Gormley and Matsa (2014), who show that the method of demeaning the dependent variable with respect to the group can produce inconsistent estimates and distort inference.

[10] Researchers often include only the last forecast of each analyst-firm-quarter. We include all the forecasts. We prefer to include all the forecasts, as each forecast contains additional information. Given the suddenness of the pandemic, our doing so allows us to better pin down the dynamic effect. As shown in Fig. 1, the effect of the pandemic changed significantly at the monthly frequency. Nevertheless, in Table A1 of the Internet Appendix, we show that our results are similar if we include only the last forecast of each quarter and apply other widely used filters.

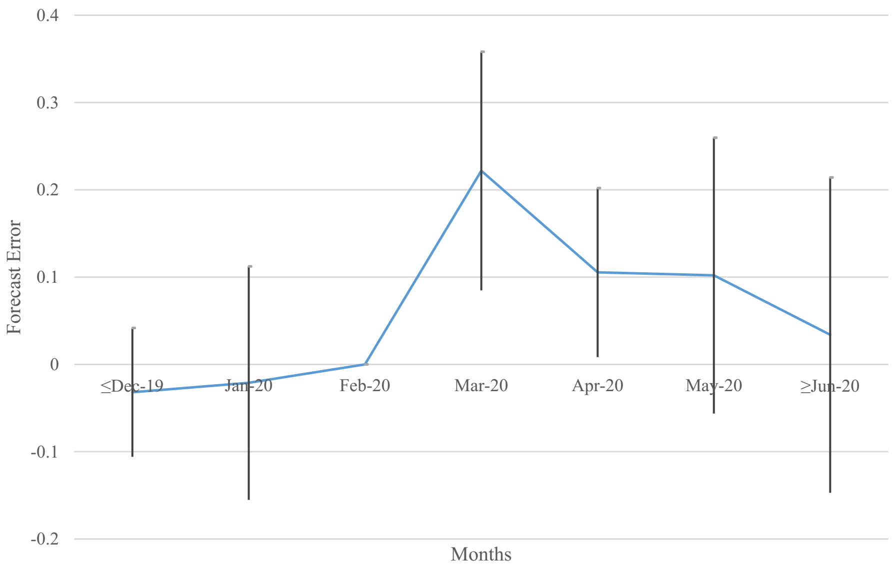

Fig. 1 Dynamic effects of forecast accuracy. This figure plots the gender difference (the point estimates and their 95% confidence intervals) in forecast error by seven subperiods using the model in column 2 of Table 3. The seven subperiods are each of the five months around March 2020; December 2019 or before; and June 2020 or after. *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 quarterly earnings announcement date. We estimate the gender differences relative to that of February 2020 in a full model with firm*fiscal quarter, year-month, and analyst*firm fixed effects and *Forecast Age* control. The confidence intervals are based on standard errors clustered at the analyst level

all three specifications. These results indicate that, relative to male analysts, female analysts' earnings forecasts became less accurate after the pandemic outbreak. This finding is consistent with our prediction.

The economic magnitude of the relative decrease in forecast accuracy for female analysts is sizable. As shown in Table 1, the mean of *Forecast Error* in our sample before the pandemic is about 1.1. Hence, the relative decrease in forecast accuracy for female analysts, as estimated in Table 2, is 14.5% of the unconditional mean, an economically meaningful effect (e.g., 38.4 cents rather than 33.5 cents on a $30 stock). In column (3), the coefficient of *Female*Post* is 55.5% (i.e., 0.1591/0.2865) of the coefficient of *Forecast Age*. *Forecast Age* is a well-known factor affecting forecast accuracy. The pandemic's differential impact on male and female analysts is equivalent to having female analysts forecast significantly earlier than male analysts. The median *Forecast Age* is 3.9, implying 49 days (exp(3.9)). Evaluating at the median, the pandemic's impact is equivalent to having male analysts forecast 49 days before earnings announcements and having female analysts forecast 86 days before earnings announcements.

We report several robustness tests in Table A1 of the Internet Appendix. In column (1), we report the results of double-clustering the standard errors by analyst and forecast month. In column (2), we winsorize *Forecast Error* at the 2% and 98% levels. In column (3), we focus on the period from September 2019 to August 2020.

Table 2 Baseline regressions on forecast error

	Dep.Var = *Forecast Error*		
	(1)	(2)	(3)
*Female*Post*	0.1579***	0.1559***	0.1591***
	(3.17)	(2.93)	(3.00)
Post	-0.3961***		
	(-8.00)		
Forecast Age			0.2865***
			(14.06)
Adj R^2	0.723	0.721	0.724
Obs	448,978	448,034	448,034
Firm*Fiscal quarter FE	Yes	Yes	Yes
Year-month FE	No	Yes	Yes
Analyst*Firm FE	No	Yes	Yes
Analyst FE	Yes	No	No

This table reports the results on how COVID-19 affected the female and male analysts differently. 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 quarterly earnings announcement date. *Female* is a dummy variable that takes the value of one for female analysts, zero for male analysts. *Post* is a dummy variable that takes the value of one for forecasts issued from March 2020 to August 2020. *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 analyst level, and *t*-statistics are reported in the parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively

Under this choice, the pre-period and the post-period have the same length. In column (4), we use March 2019 to August 2019 as the pre-period to control for the possible seasonality effect in analyst forecasts. In column (5), we control for a group of analyst characteristics. Most of these analyst characteristics are slow-moving. Thus, it is not surprising that most variables are statistically insignificant, as we have already included the *Analyst* × *Firm* fixed effects. In column (6), we keep only the last forecast for each analyst-firm-quarter. In column (7), we apply the data filters following Hirshleifer et al. (2009).[11] In column (8), we take the natural log of *Forecast Error* as the dependent variable. Overall, our results are similar across these different specifications.

Du (2023) reports that the pandemic had no asymmetric impact on female and male analysts in forecast accuracy.[12] In Section B of the Internet Appendix, we find that the difference is mainly caused by the difference in sample selection. Du's

[11] Specifically, in addition to keeping only the last forecast for each analyst-firm-quarter, we require that the forecasts are issued or reviewed in the last 60 calendar days before earnings announcements. We also delete observations when earnings or forecasts are greater than the stock price or when the stock price is less than one dollar before split adjustment.

[12] Du (2023) reports some weak evidence that female analysts' forecast accuracy decreased relative to male analysts' among low institutional ownership firms.

(2023) pre-period sample over-weights forecasts that are made late in an earnings cycle, leading to sample imbalance between the pre-period and post-period. Such imbalance is a result of the combination of two factors: she only includes the first forecast for each analyst-firm-quarter, and her pre-period is short and does not cover a full earnings cycle. Although we are unaware of any other studies that only include the first forecast of each analyst-firm-quarter when studying forecast accuracy, our results are nevertheless robust to such a choice once the unbalanced sample problem is mitigated by extending the pre-period.

3.1.2 Dynamic effects

To test the dynamic treatment effect, we separate the whole sample period into seven subperiods: each of the five months around March 2020; December 2019 or before; and June 2020 or after. Then, we interact these subperiod dummy variables with the *Female* dummy and run similar panel regressions to those in Table 2 while including the year-month fixed effects.

Table 3 reports the results. In column (1), we include the *Firm* × *Fiscal Quarter* and the year-month fixed effects. In this specification, we can estimate the gender difference for each subperiod. The results show that female analysts' forecast error was smaller in the pre-period than male analysts'. The statistical significance of the estimation of the pre-period gender difference is weak, perhaps because our sample is much smaller than that of Kumar (2010). In the first three months after the pandemic outbreak, female analysts' forecast error became significantly bigger than male analysts'. By June 2020, the difference was still positive but became insignificant.[13]

In column (2), we further include the *Analyst* × *Firm* fixed effects. In this specification, we can no longer estimate the gender difference for each subperiod. We use the month right before the pandemic outbreak (i.e., February 2020) as the base case and evaluate the gender differences for each of the other subperiods relative to that of February 2020. We find similar results – that female analysts' forecast error relative to male analysts' increased the most in March 2020 and gradually shrank in subsequent months, and that by May 2020 the difference had become insignificant.

Figure 1 displays the results graphically. We plot the estimated coefficients (and 95% confidence intervals) of the interaction between subperiod dummy variables with the Female dummy and control for *Firm* × *Fiscal Quarter*, *Analyst* × *Firm*, and year-month fixed effects and *Forecast Age*. The figure shows that female analysts' forecast errors (relative to male analysts') increased the most in March and April 2020. The effect in May 2020 is similar to that of April 2020 but becomes insignificant. The difference becomes much smaller and continues to be insignificant afterward.

We speculate that two factors might have contributed to the dissipation of the effect by June 2020. First, society started to recover from its initial panic. For example, workers got used to working from home and having online meetings. Working

[13] We group June–August 2020 into one group. The results are qualitatively similar if we conduct the analysis month by month.

Table 3 Dynamic effects of forecast accuracy

	Dep.Var = *Forecast Error*	
	(1)	(2)
*Female**(*December 2019 or before*)	-0.0155	-0.0319
	(-1.31)	(-0.85)
*Female**(*January 2020*)	-0.0320	-0.0211
	(-0.51)	(-0.31)
*Female**(*February 2020*)	-0.0816*	
	(-1.79)	
*Female**(*March 2020*)	0.2309***	0.2218***
	(3.37)	(3.19)
*Female**(*April 2020*)	0.1271***	0.1054**
	(3.22)	(2.14)
*Female**(*May 2020*)	0.1251*	0.1021
	(1.84)	(1.27)
*Female**(*June 2020 or after*)	0.0467	0.0340
	(0.62)	(0.37)
Forecast Age	0.2809***	0.2864***
	(13.90)	(14.06)
Adj R^2	0.726	0.724
Obs	448,990	448,034
Firm*Fiscal quarter FE	Yes	Yes
Year-month FE	Yes	Yes
Analyst*Firm FE	No	Yes

This table presents the dynamic effects on how COVID-19 affected the female and male analysts differently. 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 quarterly earnings announcement date. *Female* is a dummy variable that takes the value of one for female analysts and zero for male analysts. (December 2019 or before), (January 2020), …, and (June 2020 or after) are seven subperiod dummy variables. *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 analyst level, and *t*-statistics are reported in the parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively

mothers might also have asked relatives to help with childcare and household work. Second, June 2020 coincided with the opening of many summer camps. And Parolin and Lee (2021) report that in-person visits to schools rebounded in June 2020. It is worth noting that the pattern we observe from analysts is similar to that of the gender gap in unemployment in the general population – a quick sharp widening of the gender gap at the pandemic outbreak and a significant reversal starting in May 2020 (Alon et al. 2022). By July 2020, about half of the initial widening had been closed. However, unlike in equity analysts, in the general population the widening gender gap persisted much longer. The difference suggests that analysts were affected less severely than the general population.

Together, Table 3 and Fig. 1 report two important findings. First, the pandemic affected female analysts more than male analysts. The effect started right after the pandemic outbreak and became weaker afterward. Second, there were no pre-event trends in gender difference. The latter suggests that the parallel trends assumption underlying our difference-in-differences estimation is likely valid.

3.2 Potential economic mechanisms and supporting evidence

In this subsection, we conduct empirical tests to examine the underlying economic mechanisms of the above-documented widening of the analyst gender gap. Our main conjecture is that the parenting burden disproportionately fell on the shoulders of women. Such an asymmetric increase in parenting burden caused a more significant time allocation shock to female analysts than to male analysts.

In subSect. 3.2.1, we examine the quantity of forecasts to have a complete understanding of analyst productivity. In subSect. 3.2.2, we examine the effect of school closures. In subSect. 3.2.3, we examine cross-analyst heterogeneity to shed more direct light on the conjectured mechanism. In subSect. 3.2.4, we investigate several other measures of analyst forecast behaviors. In subSect. 3.2.5, we examine one alternative mechanism. In the last subsection, we conduct two placebo tests by randomly assigning gender to analysts in our sample and examining the 2007–2009 global financial crisis.

3.2.1 Quantity: firms covered and updating frequency

If analysts' time became more constrained during the pandemic, analysts might face a tradeoff between forecast quality (i.e., accuracy) and forecast quantity (i.e., firms covered and updating frequency). Table 4 examines whether the pandemic affected female and male analysts differently regarding updating frequency (Panel A) and firms covered (Panel B).

We measure *Updating Frequency* at the analyst-firm-month level. Specifically, we define updating frequency as the number of forecasts issued by analyst i in month t for firm j. To estimate the effect of the pandemic, we use the same model as in Table 2 but replace the dependent variable with updating frequency. In this test, we do not include *Forecast Age*, as this variable is not well defined for *Updating Frequency*. In both specifications, the coefficient of $Female * Post$ is insignificant. The magnitude is also tiny. The coefficient is between 0.0028 and 0.0035. The average updating frequency is about 1.12 for both female and male analysts. Therefore, 0.0028 or 0.0035 is negligible.

We measure *Firms Covered* as the number of unique firms for which an analyst issued at least one forecast. The analysis 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 the regressions, we either use *Firms Covered* or the natural logarithm of *Firms*

Table 4 Updating frequency and firms covered

Panel A. Updating frequency

	Dep. Var = log(*Updating Frequency*)		Dep. Var = *Updating Frequency*	
	(1)	(2)	(3)	(4)
*Female*Post*	0.0013	0.0019	0.0028	0.0035
	(0.25)	(0.35)	(0.33)	(0.39)
Post	0.0408***		0.0660***	
	(16.25)		(16.08)	
Adj R^2	0.118	0.160	0.119	0.160
Obs	398787	397801	398787	397801
Firm*Fiscal quarter FE	Yes	Yes	Yes	Yes
Year-month FE	No	Yes	No	Yes
Analyst*Firm FE	No	Yes	No	Yes
Analyst FE	Yes	No	Yes	No

Panel B. Firms covered

	Dep.Var = log(*Firms Covered*)		Dep.Var = *Firms Covered*	
	(1)	(2)	(3)	(4)
*Female*Post*	0.0878	0.0115	0.4517	-0.0033
	(0.85)	(0.42)	(0.54)	(-0.01)
Post	-0.0665**	-0.1478***	-1.0500***	-1.7160***
	(-1.97)	(-13.80)	(-3.70)	(-18.68)
Female	-0.0936		-0.6740	
	(-1.28)		(-1.13)	
Adj R^2	0.001	0.905	0.003	0.907
Obs	3,537	3,138	3,537	3,138
Analyst FE	No	Yes	No	Yes

This table reports how COVID-19 affected analysts' updating frequency (Panel A) and the number of firms covered (Panel B). In Panel A, the dependent variable is *Updating Frequency* or the natural logarithm of *Updating Frequency*. We measure updating frequency at the analyst-firm-month level. Specifically, updating frequency is defined as the number of forecasts issued by analyst *i* in month *t* for firm *j*. In Panel B, the dependent variable is either *Firms Covered* or the natural logarithm of *Firms Covered*. The analysis is at the analyst-period level. *Firms Covered* is the number of unique firms for which an analyst issued at least one forecast over the pre- or post-period. *Female* is a dummy variable that takes the value of one for female analysts and zero for male analysts. *Post* is a dummy variable that takes the value of one for forecasts issued from March 2020 to August 2020. Standard errors are clustered at the analyst level, and *t*-statistics are reported in the parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively

Covered as our dependent variable. Our results indicate that, in all the specifications, the coefficient of $Female * Post$ is never significant, suggesting that the pandemic did not have an asymmetric impact on female and male analysts in terms of the number of firms followed.[14]

[14] *Updating Frequency* and *Firms Covered* do not seem to have extreme values. Nevertheless, in Table A2, we find similar results if we winsorize them at the 1% and 99% levels.

3.2.2 The school closure effect

We measure the school closure effect in two ways. In the first analysis, we define school closure based on the official policy. In the second analysis, we define school closure based on in-person school visits.

A. Official school closure policy

We obtain our school closure and reopening dates at the state level from Ballotpedia, which tracks state-level orders related to school openings and closures.[15] Most states leave reopening decisions to local health officials, schools, school boards, and districts. We define the reopening date as when the schools in a state were officially allowed to reopen to in-person instruction as long as the school district met certain health-related criteria. As an example, we define August 17, 2020, as the reopening date for Arizona, which, according to Ballotpedia, was the date when schools in Arizona were officially allowed to reopen to in-person instruction if they met metrics the state Department of Health released in the week of August 3. By the end of August 2020, 18 states had allowed school reopening. The remaining states reopened schools after August 2020.

We create three dummy variables: *BeforeClosure*, *DuringClosure*, and *AfterClosure*. *BeforeClosure* (*DuringClosure*) is a dummy that equals one if a forecast was issued before (when) schools were closed in the state where the analyst lived, zero otherwise. Recall that we define the post-period to be from March 1, 2020, onward. Most states ordered schools to close in mid-March. *BeforeClosure* includes the days in early March. *AfterClosure* is a dummy that equals one if a forecast was issued when schools were reopened, zero otherwise.

Panel A of Table 5 reports the results. We replace the *Female*Post* variable in the baseline model with three variables: *Female*Post*BeforeClosure*, *Female*Post*DuringClosure*, and *Female*Post*AfterClosure*. The results show that the gender effect is strongest and most significant when schools are closed and becomes insignificant when schools are allowed to open. The significant coefficient of *Female*Post*BeforeClosure* suggests that after the pandemic outbreak in March but before the official school closure, female analysts might have already been spending more time taking care of their families.

B. In-person visits based on mobile phone data

The official school closure periods may not measure actual school closures. School reopening does not mean that school activities were back to the pre-pandemic normal. With the health concerns, many parents chose not to send their children back to school. Many schools decided not to open even after they were allowed to. Many schools did not resume their after-school programs. Schools still needed

[15] See https://ballotpedia.org/School_responses_to_the_coronavirus_(COVID-19)_pandemic_during_the_2020-2021_academic_year.

Table 5 The school closure effect

Panel A: Official school closure policy			
	Dep.Var = *Forecast Error*		
	(1)	(2)	(3)
*Female*Post*BeforeClosure*	0.1670**	0.1173**	0.1405**
	(2.13)	(2.25)	(2.33)
*Female*Post*DuringClosure*	0.2173***	0.2738***	0.2465***
	(3.35)	(2.58)	(2.72)
*Female*Post*AfterClosure*	0.0714	0.0842	0.0926
	(0.98)	(1.11)	(1.22)
*Post*BeforeClosure*	-0.3791***		
	(-6.99)		
*Post*DuringClosure*	-0.2325***		
	(-4.89)		
*Post*AfterClosure*	-0.4068***		
	(-6.08)		
Forecast Age			0.2778***
			(12.94)
Adj R^2	0.726	0.723	0.726
Obs	353,493	352,840	352,840
Firm*Fiscal quarter FE	Yes	Yes	Yes
Year-month FE	No	Yes	Yes
Analyst*Firm FE	No	Yes	Yes
Analyst FE	Yes	No	No
P values	0.2099	0.1115	0.1930

Panel B: In-person visits based on foot-traffic data			
	Dep.Var = *Forecast Error*		
	(1)	(2)	(3)
*Female*Post*	0.0887	0.0822	0.0980
	(1.56)	(1.36)	(1.62)
*Female*Post*%SchoolClosed*	0.1188*	0.1219*	0.1029
	(1.74)	(1.74)	(1.48)
Post	2.2378**		
	(2.37)		
Post%SchoolClosed*	-2.5940***		
	(-2.70)		
%SchoolClosed	-2.6358***	0.2069	0.2124
	(-2.77)	(0.64)	(0.66)
Forecast Age			0.2775***
			(13.03)
Adj R^2	0.727	0.724	0.727
Obs	357,123	356,468	356,468
Firm*Fiscal quarter FE	Yes	Yes	Yes
Year-month FE	No	Yes	Yes

Table 5 (continued)

Analyst*Firm FE	No	Yes	Yes
Analyst FE	Yes	No	No

This table reports the results of the school closure effect. In Panel A, we use the official school closure and reopening dates at the state level from Ballotpedia to capture the school closure effect. In Panel B, we use the in-person visits based on foot-traffic data constructed by Parolin and Lee (2021) to capture the school closure effect. 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 quarterly earnings announcement date. *Female* is a dummy variable that takes the value of one for female analysts and zero for male analysts. *Post* is a dummy variable that takes the value of one for forecasts issued from March 2020 to August 2020. *BeforeClosure* (*DuringClosure*) is a dummy that equals one if a forecast was issued before (when) schools were closed in the state where the analyst resided, zero otherwise. *AfterClosure* is a dummy that equals one if a forecast was issued when schools were reopened, zero otherwise. *%SchoolClosed* is defined as the fraction of schools closed in a state in a given month. *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 analyst level, and *t*-statistics are reported in the parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively

to follow social distancing. In many cases, the maximum permitted enrollment was lower than the pre-pandemic level. As a result, many students could not return to school even if their parents wanted them to.

To mitigate these issues, we take advantage of an alternative measure of school closure constructed by Parolin and Lee (2021). Using foot-traffic data from SafeGraph, Parolin and Lee (2021) define a school as ''closed'' or ''mostly closed'' if it experiences a year-over-year decline in in-person visits of at least 50% for a specific month.[16] The foot-traffic data provide a more direct and perhaps more accurate measure of school closure. We create an interaction variable between *Female*Post* and *%SchoolClosed*, which is defined as the fraction of schools closed in a state in a given month. Based on this continuous measure of school closure, Panel B of Table 5 shows that the coefficient of *Female*Post*%SchoolClosed* is positive and marginally statistically significant in two of the three specifications. The results are consistent with our conjecture that the relative decrease in female analysts' performance is more pronounced when a majority of the schools in their area are closed.

Overall, the results on school closures, although statistically weak, are consistent with our conjecture that the parenting burden disproportionately fell on the shoulders of women, and that this asymmetric change drove the relative decrease in female analysts' forecast accuracy.

3.2.3 Cross-analyst heterogeneity

To provide direct evidence of the parenting burden explanation, ideally we would like to have information about analysts' family structure, such as whether the analyst was married and how many children he or she needed to take care of. Such

[16] SafeGraph collects anonymized GPS data from users' mobile phone apps (i.e., weather or mapping apps, etc.) for more than 6 million points of interest (POIs).

information is difficult to access. The results based on school closure are consistent with the parenting burden interpretation. In this subsection, we further substantiate this interpretation by exploring the cross-sectional heterogeneity in analyst characteristics. Specifically, we evaluate analyst age, firms covered, and experience. We also examine whether an analyst lived in a southern state, where gender role attitudes are, in general, more traditional (Rice and Coates 1995; Ke 2021). We use the Census Bureau's designation to define southern states.

If parenting burden was the main reason for the widening gender gap, we would expect that the gender gap increased most among the analysts who were most likely to have young children. We would also expect that the pandemic increased the gender gap more for busier analysts and relatively inexperienced analysts because their time was more likely to be constrained. Similarly, we would expect the pandemic to have increased the gender gap more for analysts living in southern states.

Panel A of Table 6 reports the results on age. We split all the analysts into four groups based on their age: less than 30, between 30 and 40, between 40 and 50, and older than 50. We then run the same panel regressions as in our baseline regressions (Table 2) on each subsample. Consistent with our conjecture, the coefficient of *Female* $*$ *Post* is largest and most significant when analysts' age is between 30 and 40. The *F*-test indicates that the differences in the pandemic effect between the 30–40 group and the other three groups are statistically significant ($p < 0.05$). Relative to the analysts between 30 and 40 years old, the younger analysts were less likely to have children, and the senior analysts were more likely to have older or grown-up children (and less demanding childcare duty).

In Panel B, we split our sample into two groups, based on the number of firms covered, analyst total experience, and analyst location. *Firms Covered* is defined as the number of firms covered by the analyst in a year. *Total Experience* is defined as the number of years since the analyst issued the first forecast for any firm. We calculate both *Firms Covered* and *Total Experience* using data before the pandemic. We expect that analysts who needed to cover a larger number of firms were busier. As the pandemic serves as a time allocation shock, we expect the gender gap to become wider among busier analysts. We also expect the gender gap to become wider among inexperienced analysts and analysts living in southern states.

The results in Panel B are consistent with these conjectures. The coefficient of *Female* $*$ *Post* is larger and more significant for analysts with a larger number of firms to follow, for inexperienced analysts, and for analysts living in southern states. The difference in the coefficient of *Female* $*$ *Post* is statistically significant at the 1% level for *Firms Covered* but insignificant for the other two subsample analyses, although the differences are always economically sizable.

Taken together, the cross-analyst heterogeneity tests and the school closure results are consistent with our conjecture that the parenting burden disproportionately fell on the shoulders of women and caused the widening gender gap in forecast quality.

Table 6 Cross-analyst heterogeneity

Panel A: Subsample test based on analyst age

	Dep.Var = *Forecast Error*			
	Age < = 30	30 < Age < = 40	40 < Age < = 50	Age > 50
*Female*Post*	0.1438	0.2812***	0.1383*	0.1476
	(0.51)	(4.60)	(1.74)	(1.21)
Forecast Age	0.3390***	0.2741***	0.2787***	0.3159***
	(5.58)	(14.13)	(12.12)	(12.09)
Adj R^2	0.718	0.745	0.718	0.698
Obs	17,472	145,396	171,336	108,788
Firm*Fiscal quarter FE	Yes	Yes	Yes	Yes
Year-month FE	Yes	Yes	Yes	Yes
Analyst*Firm FE	Yes	Yes	Yes	Yes
F-test (p-value)		0.013		

Panel B: Subsample test based on firms covered, total experience, and location of analysts

	Dep. Var = *Forecast Error*					
	Firms covered		**Total experience**		**Location**	
	< = median	> median	< = median	> median	Southern States	Other States
*Female*Post*	0.0593	0.3189***	0.2162**	0.0977*	0.3228**	0.1298**
	(0.82)	(4.32)	(2.53)	(1.72)	(2.47)	(2.14)
Forecast Age	0.2657***	0.3091***	0.2938***	0.2806***	0.3757***	0.2719***
	(11.82)	(14.16)	(12.85)	(13.58)	(7.19)	(15.90)
Adj R^2	0.740	0.707	0.712	0.737	0.722	0.722
Obs	210,175	233,166	218,713	224,917	66,424	381,246
Firm*Fiscal quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-month FE	Yes	Yes	Yes	Yes	Yes	Yes
Analyst*Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
F-test (p-value)	0.009		0.224		0.221	

This table reports cross-analyst heterogeneity tests on forecast accuracy. In Panel A, we split our sample into four groups based on analysts' ages: less than 30, between 30 and 40, between 40 and 50, and older than 50. In panel B, we split our sample into three groups based on the number of firms covered by an analyst, analyst total experience, and analyst location. *Firms Covered* is the number of firms covered by the analyst in a year. *Total Experience* is the number of years since the analyst issued the first forecast for any firm. 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 quarterly earnings announcement date. *Female* is a dummy variable that takes the value of one for female analysts and zero for male analysts. *Post* is a dummy variable that takes the value of one for forecasts issued from March 2020 to August 2020. *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 analyst level, and *t*-statistics are reported in the parentheses. The last rows report the *p*-values of the *F*-test for whether the difference in the coefficient of *Female*Post* is statistically significant. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively

3.2.4 Forecast heuristics and timeliness

We now examine whether female analysts who were likely overburdened by childcare and other household duties resorted more to heuristics when making forecasts and issued less timely forecasts. Following Hirshleifer et al. (2019), we consider three measures of decision heuristics. We expect that, relative to male analysts, female analysts were more likely to issue a herding forecast, more likely to reissue their previous forecast, and more likely to issue a rounded forecast during the pandemic than before the pandemic.

To provide further evidence of female analysts being overburdened during the pandemic, we can look at situations in which the job is more demanding. One such situation is earnings announcements, as analysts are expected to update their forecast within a short window following the announcement (as can be seen from the high unconditional probability of updating immediately after earnings announcements in Table 1). We expect that relative to male analysts, female analysts were less likely to update their forecasts immediately after earnings announcements during the pandemic than before the pandemic.

We use a similar difference-in-differences specification as our baseline regression to analyze *Herding*, *Reissue*, *Rounding*, and *Timeliness*. All four of these dependent variables are dummy variables. Hence, we use a linear probability model to incorporate our fixed effects to avoid the incidental parameters problem of nonlinear models such as logic and probit (Neyman and Scott 1948; Lancaster 2000).

Table 7 reports all the results. For *Herding*, *Reissue*, and *Rounding*, we run three specifications, as in our baseline results. For all three variables, the coefficients of $Female * Post$ are not sensitive to the model specifications. Hence, we focus on the most stringent specification with all the fixed effects and the control of *Forecast Age*. The coefficients of $Female * Post$ are positive in Panels A-C. These results show that relative to male analysts, female analysts were more likely to issue a herding forecast, reissue their previous forecast, and issue a rounded forecast during the pandemic than before the pandemic. For *Timeliness*, we do not control for *Forecast Age* because it mechanically correlates with *Timeliness*. Female analysts' forecast timeliness exhibited a relative decrease, as indicated by a negative coefficient of $Female * Post$ in Panel D.

The economic magnitude of these coefficients is non-trivial. The results indicate that relative to male analysts, during the pandemic, female analysts' likelihood of issuing a herding forecast increases by 2.68 percentage points, their likelihood of reissuing their previous forecast increases by 4.01 percentage points, their likelihood of issuing a rounded forecast increases by 1.30 percentage points, and their likelihood of issuing a timely earnings forecast decreases by 3.03 percentage points. These changes represent 7.44% (2.68%/36.0%), 8.53% (4.01%/47.0%), 7.56% (1.30%/17.2%), and 6.17% (3.03%/49.1%) relative to the unconditional mean values of female analysts before the pandemic, respectively.

Table 7 Forecast heuristics and timeliness

Panel A. Herding			
Dep.Var = *Herding*			
	(1)	(2)	(3)
*Female*Post*	0.0201**	0.0268**	0.0268**
	(1.98)	(2.35)	(2.35)
Post	-0.1455***		
	(-25.50)		
Forecast Age			0.0001
			(0.03)
Adj R^2	0.064	0.066	0.066
Obs	132,531	130,468	130,468
Firm*Fiscal quarter FE	Yes	Yes	Yes
Year-month FE	No	Yes	Yes
Analyst*Firm FE	No	Yes	Yes
Analyst FE	Yes	No	No
Panel B. Rounding			
Dep.Var = *Rounding*			
	(1)	(2)	(3)
*Female*Post*	0.0106**	0.0130**	0.0130**
	(1.98)	(2.36)	(2.37)
Post	-0.0013		
	(-0.53)		
Forecast Age			-0.0044***
			(-4.36)
Adj R^2	0.076	0.071	0.071
Obs	448,978	448,034	448,034
Firm*Fiscal quarter FE	Yes	Yes	Yes
Year-month FE	No	Yes	Yes
Analyst*Firm FE	No	Yes	Yes
Analyst FE	Yes	No	No
Panel C. Reissuance			
Dep.Var = *Reissue*			
	(1)	(2)	(3)
*Female*Post*	0.0397***	0.0408***	0.0401***
	(4.82)	(4.82)	(3.24)
Post	0.0964***		
	(24.38)		
Forecast Age			-0.0682***
			(-8.98)
Adj R^2	0.128	0.189	0.196
Obs	448,978	448,034	448,034
Firm*Fiscal quarter FE	Yes	Yes	Yes

Table 7 (continued)

Year-month FE	No	Yes	Yes
Analyst*Firm FE	No	Yes	Yes
Analyst FE	Yes	No	No

Panel D. Timeliness		
Dep.Var = *Timeliness*		
	(1)	(2)
*Female*Post*	-0.0297***	-0.0303***
	(-4.10)	(-3.99)
Post	-0.1992***	
	(-46.73)	
Adj R^2	0.239	0.375
Obs	448,978	448,034
Firm*Fiscal quarter FE	Yes	Yes
Year-month FE	No	Yes
Analyst*Firm FE	No	Yes
Analyst FE	Yes	No

This table reports the results on analyst forecast heuristics and timeliness. We use three measures of forecast heuristics. The dependent variables are *Herding* in Panel A, *Reissue* in Panel B, *Rounding* in Panel C, and *Timeliness* in Panel D. *Herding* is a dummy variable that takes the value of one for forecasts that are between the analyst's own prior forecast and the consensus forecast, zero otherwise. *Reissue* is a dummy variable that takes the value of one if a forecast is reissued, zero otherwise. *Rounding* is a dummy variable that takes the value of one if a forecast ends with zero or five in the penny digit, zero otherwise. *Timeliness* is a dummy variable that takes the value of one if an analyst issues an EPS forecast within days $[0, +2]$ of a firm's quarterly earnings announcement, zero otherwise. *Female* is a dummy variable that takes the value of one for female analysts and zero for male analysts. *Post* is a dummy variable that takes the value of one for forecasts issued from March 2020 to August 2020. *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 analyst level, and *t*-statistics are reported in the parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively

3.2.5 Forecast optimism

One alternative explanation for the forecast accuracy result is that female analysts became more pessimistic when facing a large adverse shock. One possible reason for this is that women had more woman friends, who were more negatively affected by the pandemic. Due to the experience effect (Malmendier and Nagel 2011; D'Acunto et al. 2021), such asymmetric exposure might lead women to have more pessimistic expectations about future economic prospects than men. If female analysts were overly pessimistic, such a gender difference in pessimism might explain why female analysts' forecasts became less accurate during the pandemic.

Our results rule out this alternative explanation. We measure analysts' forecast optimism directly. Specifically, we construct a measure of *Forecast Optimism*, defined as forecasted EPS minus actual EPS scaled by the 12-month lagged stock price and multiplied by 100. A lower value of *Forecast Optimism* indicates more pessimistic earnings forecasts. This alternative explanation predicts that female

Table 8 Forecast optimism

	Dep.Var = *Forecast Optimism*		
	(1)	(2)	(3)
*Female*Post*	0.0810	0.0817	0.0835
	(1.32)	(1.25)	(1.27)
Post	-0.9307***		
	(-18.59)		
Forecast Age			0.1646***
			(9.69)
Adj R^2	0.469	0.474	0.476
Obs	448,978	448,034	448,034
Firm*Fiscal quarter FE	Yes	Yes	Yes
Year-month FE	No	Yes	Yes
Analyst*Firm FE	No	Yes	Yes
Analyst FE	Yes	No	No

This table reports the results on how COVID-19 affected analysts' forecast optimism. The dependent variable is *Forecast Optimism*, defined as 100 times the difference between the forecasted EPS and the realized EPS, divided by the stock's price 12 months prior to the quarterly earnings announcement date. *Female* is a dummy variable that takes the value of one for female analysts and zero for male analysts. *Post* is a dummy variable that takes the value of one for forecasts issued from March 2020 to August 2020. *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 analyst level, and *t*-statistics are reported in the parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively

analysts will make more pessimistic forecasts than male analysts. We replace *Forecast Error* with *Forecast Optimism* in a regression model that is otherwise identical to our baseline analysis and report the results in Table 8. The results show that the coefficient of $Female * Post$ is insignificantly different from zero, suggesting that female analysts are not more pessimistic than male analysts during the pandemic. We also find a positive and significant coefficient on *Forecast Age*, consistent with analysts' tendency to "walk down" their estimates to a level that firms can beat at the official earnings announcement (Richardson et al. 2004).

3.2.6 Two placebo tests – assigning gender randomly and examining the 2007–2009 global financial crisis

We conduct the first placebo test by randomly assigning gender to analysts in our sample and keeping the gender ratio the same as in the actual data. We rerun the baseline regression (column (3) of Table 2) and save the coefficient of the placebo *Female*Post*. We repeat this procedure 1,000 times and plot the distribution of the estimated placebo coefficients of *Female*Post* in Fig. 2. The dashed line represents the actual coefficient of *Female*Post* from column (3) of Table 2. The figure shows clearly that the actual coefficient of *Female*Post* falls in the extreme right tail of the

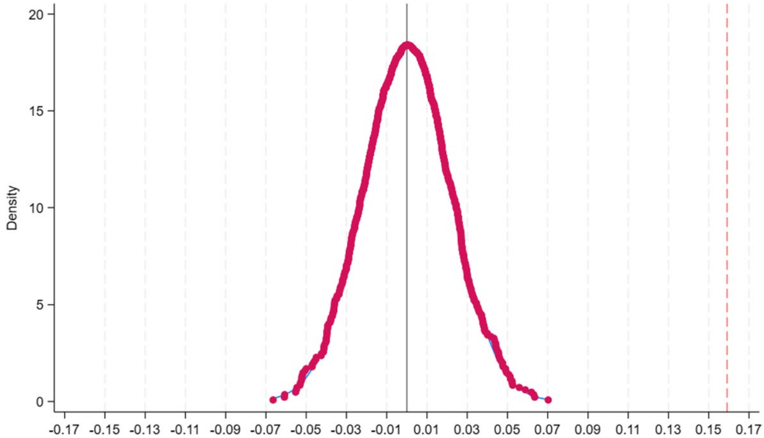

Fig. 2 Placebo test – assigning gender randomly. This figure plots the distribution of the estimated coefficients on the placebo *Female*Post* from 1,000 bootstrap simulations of the specification in column (3) of Table 2. Specifically, we randomly assign female gender to male analysts in our sample, using the same proportions as actual female analysts. We rerun the baseline regression (column 3 of Table 2) and save the coefficient of the placebo *Female*Post*. We repeat this procedure 1,000 times. The dashed line represents the actual coefficient of *Female*Post* from column (3) of Table 2

distribution of the placebo estimates, suggesting that the significant female effect documented in our main analysis is unlikely to be spurious.

Female analysts' forecast accuracy may decrease more than male analysts' for other reasons. For example, studies show that men have a stronger preference for and a better ability to respond to increased competitiveness (Gneezy et al. 2003; Niederle and Vesterlund 2007; Reuben et al. 2015). If this applies to analysts, we should find a similar widening analyst gender gap during other economic crises. To examine this possibility, we use the 2007–2009 global financial crisis to conduct another placebo test. If female analysts tend to do more poorly than male analysts during economic downturns, we expect female analysts' forecast errors to increase more than male analysts' during the 2007–2009 crisis. If the increasing household responsibility drove the reduced forecast accuracy of female analysts in 2020, we would not expect female and male analysts to be affected asymmetrically in the 2007–2009 crisis, because it was not a shock to household responsibility. Table A4 shows that the 2007–2009 global financial crisis did not increase female analysts' forecast error more than male analysts', suggesting that the reduced forecast accuracy of female analysts is unlikely to be driven by alternative explanations.

3.3 The market reaction

In this subsection, we examine whether investors were aware of the less accurate forecasts issued by female analysts during the pandemic. We estimate the following regressions:

$$CAR_{i,j,q,t} = \beta_1 + \beta_2 Female_i * Post_t + \beta_3 Frev_{i,j,q,t} + \beta_4 Frev_{i,j,q,t} * Female_i * Post_t + \beta_5 Frev_{i,j,q,t} * Female_i + \beta_6 Frev_{i,j,q,t} * Post_t + \gamma_{j,q} + \delta_{i,j} + \theta_t + \epsilon_{i,j,q,t}$$

The dependent variable $CAR_{i,j,q,t}$ is the three-day cumulative abnormal return for firm j centered on the forecast revision of quarter q's EPS issued by analyst i at time t. Abnormal return is defined as raw return minus the return of the value-weighted CRSP market index. The variable $Frev_{i,j,q,t}$ is forecast revision, defined as the difference between the current quarterly earnings forecast of analyst i for firm j at time t and the earnings forecast for the same firm-quarter issued immediately before the current forecast, scaled by the 12-month lagged stock price. To calculate forecast revision, we require an analyst to have issued both a current and prior earnings forecast for the same firm-quarter. We calculate forecast revision relative to an analyst's previous forecast instead of relative to the market consensus because changes relative to one's previous forecast are more informative (Stickel 1991; Gleason and Lee 2003). We also add *Forecast Age* and its interaction with forecast revision, *Frev*Forecast Age*, as additional controls in column (3). The coefficient of *Frev*Forecast Age* is positive, suggesting that forecasts issued earlier in a quarter likely convey more novel information to investors despite being less accurate on average.

Table 9 reports the regression results. As expected, the coefficient of $Frev_{i,j,q,t}$ is positive and highly significant, indicating that analyst forecast revisions contain information and the market reacts to them. The coefficient of $Frev_{i,j,q,t} * Female_i$ is significantly positive, suggesting that, before the pandemic, female analysts' forecast revisions had more impact than male analysts' forecast revisions, consistent with Kumar (2010).

More importantly, we find that the coefficient on $Frev_{i,j,q,t} * Female_i * Post_t$ is negative and statistically significant. The magnitude of the coefficient is similar in all the specifications. The negative coefficient of $Frev_{i,j,q,t} * Female_i * Post_t$ more than fully offsets the positive coefficient of $Frev_{i,j,q,t} * Female_i$. For example, in column (3), the coefficient of $Frev_{i,j,q,t} * Female_i * Post_t$ is -0.992 (t=-3.26), and the coefficient of $Frev_{i,j,q,t} * Female_i$ is 0.699 (t=3.90). A Wald test of the null – that the sum of the two coefficients equals zero – yields a p-value of 0.23. These results show that, during the pandemic, the market reacted less strongly to female analysts' forecast revisions than to male analysts' forecast revisions, reversing the pre-pandemic pattern (although the difference is statistically insignificant).

Overall, the results show that the market was aware of the asymmetric impact of the pandemic on female and male analysts and down-weighted the forecasts issued by female analysts.

3.4 The impact on analysts' career outcomes

Did the pandemic disproportionately affect female analysts' career outcomes relative to male analysts'? On the one hand, the pandemic had a sizable impact on female analysts' performance. On the other hand, the impact was temporary. We examine this question in this section. Following the literature (Bradley et al.

Table 9 Stock market reaction to analyst forecast revisions

	Dep.Var = *CAR*(-1, + 1)		
	(1)	(2)	(3)
*Female*Post*	0.0017	0.0025	0.0025
	(0.75)	(1.02)	(1.02)
Frev	0.7629***	0.8029***	0.2319
	(6.04)	(6.46)	(0.74)
*Frev*Female*Post*	-0.9186***	-0.9959***	-0.9923***
	(-3.01)	(-3.37)	(-3.26)
*Frev*Female*	0.6667***	0.7170***	0.6990***
	(4.89)	(4.00)	(3.90)
*Frev*Post*	-0.6109***	-0.6537***	-0.6089***
	(-3.67)	(-4.08)	(-4.00)
Post	0.0030		
	(0.67)		
Forecast Age			0.0003
			(0.32)
*Frev*Forecast Age*			0.1383*
			(1.76)
Adj R^2	0.101	0.072	0.072
Obs	104,425	103,481	103,481
Firm*Fiscal quarter FE	Yes	Yes	Yes
Year-month FE	No	Yes	Yes
Analyst*Firm FE	No	Yes	Yes
Analyst FE	Yes	No	No

This table reports the results on the stock market reaction to analyst forecast revisions. The dependent variable is *CAR*(-1, + 1), which is the three-day cumulative abnormal return for firm *j* centered on the forecast revision of quarter *q*'s EPS issued by analyst *i* at time *t*, where the abnormal return is defined as raw stock return minus the return of the value-weighted CRSP market index. *Frev* is forecast revision, defined as the difference between the current quarterly earnings forecast for analyst *i* following firm *j* at time *t* and the earnings forecast for the same firm-quarter issued immediately before the current forecast, scaled by the 12-month lagged stock prices. *Female* is a dummy variable that takes the value of one for female analysts and zero for male analysts. *Post* is a dummy variable that takes the value of one for forecasts issued from March 2020 to August 2020. *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 analyst level, and *t*-statistics are reported in the parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively

2017), we measure analyst career outcomes using all-star status and whether the analyst works for a top brokerage firm.

Table 10 presents logistic regression results for the effect of COVID-19 on female analysts' career outcomes relative to male analysts'. The data is at the analyst-year level with the sample period from 2019 to 2021. In column (1), the dependent variable, *All Star*, is a dummy variable that equals one if the analyst is listed as an all-star

Table 10 The impact on analysts' career outcomes

	(1)	(2)
	All Star	*Top Broker*
Female	0.4307	1.0686**
	(1.05)	(2.17)
*Female*Covid*	-0.7553*	-1.5266***
	(-1.84)	(-3.25)
lag (All Star)	5.0990***	
	(18.06)	
lag (Top Broker)		10.1752***
		(10.94)
Firms Covered	0.5027***	-0.0895
	(3.75)	(-0.42)
Age	-0.0279**	-0.0621*
	(-2.51)	(-1.91)
Pseudo R^2	0.5721	0.9376
Obs	3323	3324
Year fixed effects	Yes	Yes

This table presents logistic regression results for the effect of COVID-19 on female analysts' career outcomes relative to male analysts'. The data is at the analyst-year level, with the sample period from 2019 to 2021. In column (1), the dependent variable is a dummy, *All Star*, that is equal to one if the analyst is listed as an all-star analyst in the current year's October issue of *Institutional Investor* magazine. In column (2), the dependent variable is a dummy variable indicating whether the analyst works for a top brokerage firm in a year. *Female* is a dummy variable that takes the value of one for female analysts and zero for male analysts. *Covid* is a dummy variable that takes the value of one for the years 2020 and 2021. We measure brokerage firm size based on the number of analysts working for the brokerage firm in a year and define the 10 largest brokerage firms as the top brokers. *Age* is the analyst's age. *Firms Covered* is the number of stocks followed by the analyst in a year. Year fixed effects are included. Standard errors are clustered at the brokerage level, and *t*-statistics are reported in the parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively

analyst in the current year's October issue of *Institutional Investor* magazine.[17] In column (2), the dependent variable is a dummy indicating whether the analyst works for a top brokerage firm in a year. We measure brokerage firm size as the number of analysts working for the brokerage firm in a given year and define the 10 largest brokerage firms as the top brokers. The variable of interest is the interaction between the *Female* dummy and the *Covid* dummy, which takes the value of one for the years 2020 and 2021. Our control variables include the lagged measure of analyst career outcomes, analyst age (*Age*), and the number of stocks covered by the analyst in a year *(Firms Covered*). We include year fixed effects and cluster the standard errors at the brokerage level.

The results in Table 10 show that, before the pandemic, female analysts had a higher chance of gaining all-star status or working for a top brokerage, as indicated

[17] We thank An-Ping Lin for sharing the all-star analyst status data with us.

by the positive coefficient of *Female*. This is potentially consistent with the finding that female analysts had superior forecasting abilities before the pandemic. However, during the pandemic, the patterns reversed – the sum of the coefficient of *Female* and the coefficient of *Female*Covid* becomes negative. The coefficients on *Female*Covid* capture the effect of COVID. The coefficients on *Female*Covid* are negative and significant for both measures of analyst career outcomes. The economic magnitude implies that, before the pandemic, the odds of becoming an all-star analyst and working for a top brokerage firm are 53.8% and 191% higher for female analysts than for male analysts, respectively. During the pandemic, the odds ratio implies that female analysts are 27.7% less likely to become an all-star analyst and 36.7% less likely to work for a top brokerage firm. The changes are both economically and statistically significant. The evidence suggests that female analysts experience more unfavorable career outcomes than male analysts. This is potentially driven by the adverse impact of the pandemic on female analyst performance. One caveat is that since we do not have direct evidence linking the differential career outcomes to the pandemic-induced performance difference, we cannot completely rule out the possibility that the differential career outcomes are caused by other factors.

4 Conclusions

In this paper, we study the effects of the COVID-19 pandemic on female and male security analysts. Our difference-in-differences approach compares female and male analysts performing the same tasks: forecasting the same firms' earnings for the same fiscal quarter. We find that, relative to male analysts, female analysts' earnings forecast accuracy fell more during the early stage of the pandemic. We conjecture that this was driven by a relative increase in childcare and other household duties for women relative to men. We find that the effect was stronger when schools were closed and among analysts who were more likely to have young children, were busier, were less experienced, and lived in southern states. Relative to male analysts during the pandemic, female analysts herded more closely with the consensus forecast, had a higher likelihood of reissuing their previous forecast, and had a higher likelihood of issuing a rounded forecast. Compared to male analysts, female analysts were also less likely to issue timely forecasts immediately following earnings announcements. This widening gender gap, however, decayed quickly and became statistically insignificant by May and June of 2020. We also document that, relative to the pre-pandemic period, female analysts experienced more unfavorable career outcomes than male analysts in 2020–2021.

The effects of COVID-19 that we document have implications beyond financial analysts and the specific setting of the pandemic. Although different sectors have different production functions, labor time is almost always one of the most important inputs. Our findings suggest that the increased childcare and household duties disproportionately fell on the shoulders of women, even among financial professionals and in a sector where females are known to have superior skills. Our findings echo policy responses that account for the disparate effects of a common adverse shock (Oleschuk 2020; Barber et al. 2021).

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