Firm-Developed Apps and

Analysts' Use of Traditional Information Sources

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Abstract: Analysts view fewer EDGAR filings, participate in fewer conference calls, ask shorter questions, and arrange fewer meetings with executives after firms launch mobile apps. Analysts' forecast errors are higher following app launches, especially among analysts who rely less on traditional sources and who mention app-related information during conference calls. Investors react less strongly to forecasts of analysts who mention app-related information. Our findings suggest that, for analysts in our sample, app-related information crowds out traditional information sources.

JEL Classification: M40, M41, G10, G14, G17, G24, G40, G41, L81, O33

1. Introduction

Firms develop mobile apps to improve interactions with customers and facilitate sales (Keyes 2018; Narang and Shankar 2019). Data contained in mobile apps and measures of app traffic are used by market participants, such as sell-side equity analysts, to predict a firm's performance. For example, J.P. Morgan uses the mobile app intelligence platform Apptopia to track mobile app downloads, revenue, and advertising data in the Apple and Google Play app stores. UBS uses wait times in the Shanghai Disney App as a performance metric for the park. Many analysts cut their forecasts for Netflix in early February of 2022, citing weak app downloads despite net subscriber growth (Gallagher 2022).[1] It is increasingly common for analysts to inquire about firm-developed apps during conference calls and incorporate this "alternative data" into their research (Chi, Hwang, and Zheng 2024).[2] In this paper, we ask how the existence of mobile apps alters how analysts acquire information from traditional sources such as company filings, earnings conference calls, and meetings with company executives.

Information about firm-developed mobile apps is easily accessible to analysts. Yet it is unclear how the availability of mobile app information affects analysts' reliance on traditional information sources. On the one hand, analysts may use traditional sources more intensely, because information generated from mobile apps may complement traditional sources. That is, analysts can more easily "piece together the mosaic of information" with the availability of mobile app data (Hutton, Lee, and Shu 2012). On the other hand, mobile app information may crowd out traditional sources. First, mobile apps provide real-time information which enables analysts to promptly

[1] See Appendix B for more examples.

[2] Alternative data refers to value-relevant data outside the traditional sources such as earnings announcements, corporate filings, and analyst reports. Examples of alternative data include mobile app usage data, satellite imagery, point-of-sale data, social media, Google searches, and firms' online media activity (Denev and Amen 2020). Across different types of alternative data provided by external vendors to analysts, mobile app usage data comes the second most frequently used after point-of-sale data (Chi et al. 2024).

revise their forecasts before related information appears in traditional sources. Second, data such as app downloads and traffic are provided by third-party app stores, making them free of management bias. Third, app downloads and traffic, specifically, are usually reported at a granular product or geographical level and provide richer information than traditional sources (Chi et al. 2024). Lastly, analysts may emphasize alternative data because these types of data are in vogue (Alnabulsi 2021). Therefore, the impact of app-related data on analysts' use of traditional sources remains an open question.

We collect the release dates of firm-developed mobile apps from Google Play and Apple App Stores[3] and document the following changes in analyst activity subsequent to the app launches. First, analysts view firms' EDGAR filings less frequently. Second, fewer analysts participate in earnings conference calls. Third, analysts ask shorter questions during earnings conference calls. Fourth, analysts make fewer in-person visits to companies, which are measured by taxi rides (Kirk and Piao 2022; Choy and Hope 2023). Overall, analysts appear to less intensively use traditional information sources, consistent with alternative data crowding-out traditional information sources.

We next explore the mechanisms underlying the observed crowding-out effect. First, we document that earnings are, on average, more predictable after an app launch. We observe lesser analyst reliance on traditional information sources (i.e. conference call participation, question length, and in-person visits) for firms where there is greater improvement in earnings predictability after the app launch. Second, we document lesser analyst reliance on traditional information sources for app-adopting firms when at least one analyst has mentioned apps during conference calls. Overall, analysts appear to reply less on traditional sources when mobile app data improves earnings predictability and when analysts pay attention to app data.

[3] We also report several alternative app-related measures in Appendix C.

We next explore potential changes in analyst forecast accuracy. Analysts may issue more accurate forecasts by taking advantage of the granular, timely, and verifiable nature of mobile app information and the expected improvements in earnings predictability. In contrast, analysts may issue less accurate forecasts if they forego value-relevant information obtainable from traditional sources. Our empirical results support the latter prediction. Following an app launch, analyst earnings forecasts and sales forecasts become marginally less accurate, and earnings forecasts become marginally more dispersed, consistent with analysts' over-relying on mobile app information, under-relying on traditional information sources, or both. We also document that the observed lower accuracy and higher dispersion are exacerbated for firms where analysts' reliance on traditional sources decreases the most.

Our baseline analysis of analyst performance is conducted at the firm level, so we cannot distinguish between the performance of analysts who rely on mobile app data and those who do not. To establish a more direct link between analysts who rely on mobile app data and forecast accuracy, we analyze earnings call transcripts and identify analysts who mention mobile apps during the call as those who likely use app-related information (hereafter, affected analysts). We document that forecast accuracy is incrementally lower for affected analysts.

Finally, we examine if investors understand the diminished ability of affected analysts (i.e. analysts who mention apps during conference calls) following app launches. Using a three-day cumulative abnormal return (CAR) around forecast dates, we show that the market reaction to earnings and sales forecast revisions are weaker for affected analysts. These findings suggest that investors perceive analysts who mention mobile apps to be less informative.

Our study contributes to the literature in several ways. First, it adds to the literature on analysts' information acquisition (Abarbanell 1991; Trueman 1994; Epstein and Palepu 1999;

Fischer and Stocken 2010; Gibbons, Iliev, and Kalodimos 2021) and forecasting performance (Truman 1994; Mikhail, Walther, and Willis 1997; Clement 1999; Easterwood and Nutt 1999; Hong and Kubik 2003). In contrast to the literature documenting benefits of more information, we document that mobile app launches lead analysts to use traditional information sources less and result in worse forecasting performance even when mobile apps improve earnings predictability on average.

Our study also contributes to the growing literature on analysts' use of alternative data such as satellite images, credit card transactions, and social media (Kang, Stice-Lawrence, and Wong 2021; Katona, Painter, Patatoukas, and Zeng 2022). Using satellite image data, Gerken and Painter (2023) document that analysts rely more on local signals when there is less firm-wide information, and that geographic concentration of analysts increases forecast errors. Dessaint, Foucault, and Fresard (2024) find that the introduction of social media platform improves informativeness of short-term forecasts. Fang, Huang, Roychowdhury, and Sletten (2023) show that enhanced access of mobile internet technology and rollout of productivity apps improve analysts' forecast accuracy and timeliness. Closer to our work, Chi et al. (2024) examine analysts' references to alternative data in written reports. Chi et al. (2024) find that analysts who cite mobile app downloads issue more accurate forecasts, and these forecasts bring stronger market reactions. In contrast, we find that forecast quality deteriorates following firms' mobile app launches.[4]

[4] The contrasting findings can be explained in several ways. First, while Chi et al. (2024) use 30 large public firms making up the Dow Jones Industrial index, we use a more representative sample of firms that belong to industries, in which at least 10% of firms have adopted mobile apps. Chi et al. document that the mean and median market capitalization of sample firms are in the top percentile of market capitalization of firms in CRSP/Compustat universe. Our sample's mean and median market capitalization are between the 50th and 75th percentile in the Compustat universe. Second, while Chi et al.'s inference can result from the endogenous choice of analysts to refer to alternative data, our inference is less exposed to this issue as we employ the mobile app releases as informational shocks to analysts. Third, while Chi et al. show that a poor information environment can explain the adoption of alternative data, we focus on implications for information acquisition, unconditional on the firm's information environment.

More broadly, our paper relates to research regarding the role of alternative data in capital markets (Blankespoor, Miller, and White 2014; Chi and Shanthikumar 2017; Lerman 2020; Kang et al. 2021; Dichev and Qian 2022). Several studies have shown that alternative data improves market efficiency (e.g., Katona et al. 2022; Dessaint et al. 2024). Stice (2023) shows that low-quality information crowds out high-quality information in Google search results. In addition, Twitter, the business press, and the internet shape price formation and information transfer (Drake, Roulstone, and Thornock 2012; Blankespoor et al. 2014; Drake, Quinn, and Thornock 2017; Blankespoor, deHaan, and Marinovic 2020). Our study adds to this discussion by examining the effect of a new source of information – firm mobile apps – on capital markets.

Furthermore, our study can help various stakeholders understand the potential benefits and costs associated with mobile apps. First, our study is of interest to regulators because of the documented trade-off between mobile app information and mandatory disclosures (see Stice 1991 and Guest 2021). We show that mobile app information can displace public disclosures and established private sources of information in the decision-making process of analysts. To the extent that this new source of information substitutes public disclosures, our results contribute to the recent debate on the usefulness of public disclosures. Second, our study is of interest to analysts because it provides insight into the usefulness of firm mobile apps for forecasts. As mobile apps may deteriorate forecasting performance, analysts need to exercise caution in how they allocate their efforts between alternative data versus traditional sources of information. Finally, our study is of interest to investors because it sheds light on the externalities associated with the introduction of a public information source – mobile apps.

The paper proceeds as follows. Section 2 develops hypotheses. Section 3 describes the sample and variable measurement used in the analyses. Section 4 presents the research design and empirical results. Section 5 provides additional analyses. Section 6 concludes.

2. Hypothesis Development

2.1 Analysts' Use of Traditional Information Sources

EDGAR filings, earnings conference calls, and private meetings with the management are critical inputs for analysts. A survey performed by Brown, Call, Clement, and Sharp (2015) shows that analysts rank private communication with management, earnings conference calls, and recent 10-K or 10-Q filings as the second, third, and seventh most useful information source, respectively. Moreover, these sources are both public (EDGAR filings) and private (earnings conference calls and private meetings with management).[5]

Mobile app information can either complement or crowd out traditional information sources. On the one hand, analysts can use EDGAR filings in tandem with app information to comprehend the often-noisy signals that follow the release of one or more firm apps. Furthermore, mobile app information can serve as an impetus to communicate more frequently with management, either in-person or through conference calls.[6] Additionally, analysts may utilize app data alongside traditional sources, if they are aware of the growing market-wide concern regarding the over-reliance on alternative data and the noise it generates (Hope 2016).

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[5] While analysts can leverage their private knowledge and gain further information complementarities by asking questions, earnings conference calls are public in that investors can easily have access to conference call dialogs post-Regulation-FD era (Mayew, Sharp, and Venkatachalam 2013). However, we classify earnings conference call as a private information source because other market participants (e.g., retail investors) cannot interactively participate.

[6] See Appendix B for examples of how analysts use mobile app downloads in earnings conference calls or analyst reports.

On the other hand, the perceived benefits of mobile app data may undermine traditional information sources, leading to a shift in market participants' attention away from previously used information sources for the following reasons (Chi et al. 2024). First, instead of waiting for quarterly or annual public filings or conference calls, analysts can resort to instant app information as a faster and more convenient source of data. By doing so, analysts can issue more timely and accurate forecasts (Shroff, Venkataraman and Xin 2012). Second, mobile app information is obtained by third-party app stores, making it free from management bias. This makes app store figures more trustworthy than the ones provided by management, and analysts may have less incentive to seek or verify information from EDGAR filings or through interactions with the management. Third, apps offer a detailed breakdown of downloads by app store, region, brand, and product, enabling analysts to issue more precise predictions. For example, analyzing the download growth of each brand can better predict sales growth. As a result, the granularity of mobile app information can reduce analysts' reliance on executive figures reported in aggregate numbers.[7] Finally, compared to other forms of alternative data such as satellite images and credit card transactions, information on mobile app downloads is highly accessible, making it easier for analysts to incorporate it into their forecasts.

Moreover, analysts may ride the current wave of alternative data and pay more attention to mobile app information solely because it is considered part of this wave (Ravenpack 2016; Chi et al. 2024). Since a firm's app launch can either positively or negatively affect the way analysts use traditional information sources, we state the following hypothesis in null:

[7] The SEC requires firms to report the operating segments at a disaggregate level. However, firms only need to disclose them if the combined revenue of the segment exceeds the 75% of consolidated revenue, which analysts may find is relatively less informative.

Hypothesis 1: *Firm-developed mobile apps are not associated with analysts' use of traditional information sources.*

2.2 Analysts' Forecasting Performance

Given a potential shift in analysts' use of traditional sources, how might this ultimately affect their forecasting performance? To assess this, we examine the implications for earnings and revenue forecasts, as there is likely a correlation between app downloads and these performance metrics. On the one hand, the availability of real-time data permits analysts to issue more accurate and timely forecasts. Furthermore, the verifiability of app information enables analysts to make more objective judgments about a firm's future performance. Finally, the granularity of app information empowers analysts to issue more precise forecasts.

On the other hand, app downloads can often be a noisy measure of future performance, potentially failing to improve analysts' forecasting accuracy. For example, analysts may encounter difficulties in distinguishing between relevant and irrelevant apps due to the abundance of free apps that are not necessarily tied to a firm's bottom line.[8] Moreover, as firms release more apps, confusion and complexity may rise, reducing the accuracy of analysts' performance predictions. Finally, even if app-related information proves to be useful, analysts may lose vital information from traditional sources, worsening forecasting performance. Given these possible outcomes, we state the following hypothesis in null:

Hypothesis 2: *Firm-developed mobile apps are not associated with analysts' forecasting performance.*

[8] According to Statista (2022), as of July 2022, 97 (95) percent of apps are free in the Google Play and Apple App Stores.

3. Sample

We obtain analyst forecast data from I/B/E/S, financial data from Compustat, and stock market data from CRSP. We collect information on the release dates of mobile apps on Google Play and Apple App Stores since 2008 through a website called Appfigures. We limit our sample to English-based apps that originate in the United States. Additionally, we only include firms with assets greater than $1 million.

To examine the effect of mobile app launches on analysts' use of filings, we collect EDGAR filing views of analysts from the SEC website. The EDGAR search traffic database discloses information about which filings are accessed on which date and time by individual IPs. However, due to privacy issues, only the first three sections of IPs are disclosed, and the last section is encrypted. To derive a broker-level traffic measure, we follow a multi-step process. First, we use the cipher mapping table of Chen, Cohen, Gurun, Lou, and Malloy (2020) to map the last encrypted IP section to actual numbers. Next, we collect the IP address of each broker associated with the analyst (Gibbons et al. 2021) from the American Registry for Internet Numbers (ARIN) website, considering all the available IPs for each broker. Finally, we match the identified broker-level IP address with the IP of the server log file provided by the SEC.

To investigate the impact of mobile app launches on analysts' use of earnings conference calls, we first download the earnings conference call transcripts from the S&P Capital IQ. This dataset provides the narratives spoken by each speaker. We can distinguish between narratives spoken by executives and analysts, and whether they are questions or answers. For our study, we focus on the questions posed by analysts during conference calls.

Additionally, we use taxi ride patterns in New York City to proxy for private communication between analysts and managers (Kirk and Piao 2022; Choy and Hope 2023). We first collect trip-level NYC data from the Taxi and Limousine Commission (TLC) website. This data shows the pick-up and drop-off date and time, as well as locations in latitude and longitude. Second, we collect the location of brokers and firms in NYC, in latitude and longitude using Google Maps. If there are multiple offices for a brokerage firm, we consider them all. We match the pick-up and drop-off zones with the closest firm or brokerage offices and ensure that the distance between each (pickup or dropoff) zone and (firm or brokerage) office is within 0.05 miles following Kirk and Piao (2022). To ensure that analysts and management meet during normal business hours, we exclude the trips from 8 P.M. to 7 A.M. and during weekends.

3.1 Measuring App Launch

To measure app launch indicator, we rank apps by their popularity within each firm and consider the three most used apps. This step is necessary because firms typically release multiple apps, and some apps are rarely used. Next, we code the earliest release date of the three apps as the day when a firm launches an app. *App Exists* is coded as one if firm's reporting date is after this release date, and zero otherwise. Also, not all industries release apps. For example, firms in the trucking industry rarely launch mobile apps for customers. Therefore, to create a reasonable comparison group, we limit the sample to GIC two-digit industries, where at least 10% of firms have launched apps on any date during our sample period.

3.2 Descriptive Statistics

Table 1 presents the descriptive statistics for the variables used in the analyses. On average, firms are covered by thirteen analysts (=*exp*(2.531)). 20% of firm-year observations have mobile apps. Brokerage firms view EDGAR filings approximately 28 times a year (=*exp*(3.337)). During earnings conference calls, around four analysts asked questions (=$exp(1.466)$). Additionally, there were about 9,977 taxi trips (=$exp(9.208)$) during business hours between firm headquarters and brokerages.[9] The mean values of $Earnings\ Forecast\ Error_{ijt}$ and $Sales\ Forecast\ Error_{ijt}$ indicate that actual earnings miss analysts' earnings forecasts by 1.5% of the stock price, while actual sales miss analysts' sales forecast by 4.6% of the stock price as of the beginning of the year.

[Insert Table 1 here]

4. Research Design and Findings

4.1 Hypothesis 1: The Effect on Analysts' Use of Traditional Information Sources

Following Gibbons et al. (2021), we estimate the following OLS model at the firm-year level:

$$\begin{aligned} Log(EDGAR\ Views_{it}) = {} & \beta_0 + \beta_1 App\ Exists_{it} + \beta_2 Log(Mgt.\ Forecasts_{it}) \\ & + \beta_3 Size_{it} + \beta_4 Sales\ Growth_{it} + \beta_5 Leverage_{it} + \beta_6 Capex_{it} \\ & + \beta_7 \#Segments_{it} + \beta_8 ROA_{it} + \beta_9 BHAR_{it} \\ & + \beta_9 Log(Analyst\ Following_{it}) + \beta_{10} Log(Other\ EDGAR\ Views_{it}) \\ & + Firm\ fixed\ effects\ + Year\ fixed\ effects + \varepsilon_{it}, \end{aligned} \tag{1a}$$

where subscripts i and t index firm and fiscal year, respectively. The dependent variable, $Log(EDGAR\ Views_{it})$ is the natural logarithm of one plus the total views of firm i's filings viewed by brokerage firms in year t. This measure captures the intensity of usage of public information by analysts. $App\ Exists_{it}$ is an indicator variable equal to one if a firm has released their mobile apps before or during the year t, and zero otherwise. Under H1, our coefficient of interest is β_1. A positive (negative) coefficient would suggest that the use of mobile app information and traditional data sources are complementary (substitutive).

[9] We cannot rule out the possibility of capturing trips between other businesses in the same locations as firm headquarters and brokerages. However, this measurement error will bias against detecting the effect of firms' app releases on analysts' private information acquisition.

In addition, we include the following control variables. *Log(Mgt. Forecasts$_{it}$)* is the natural logarithm of one plus the number of management forecasts for year *t*. *Size$_{it}$* is the natural logarithm of total assets plus one. *Sales Growth$_{it}$* is the percentage change in sales revenue from year *t*-1 to *t*. *Leverage$_{it}$* is total debt, scaled by total assets. *Capex$_{it}$* is capital expenditures, scaled by total assets. *#Segments$_{it}$* is the number of business segments identified from Compustat Segment file. *ROA$_{it}$* is income before extraordinary items, scaled by total assets as of the beginning year. *BHAR$_{it}$* is the 12-month value-weighted-index-adjusted abnormal return between the last and current earnings announcement date. *Log(Analyst Following$_{it}$)* is the natural logarithm of one plus the number of unique analysts following the firm during a year. Finally, we further control for the number of other EDGAR filing views excluding the brokerage views (*Log(Other EDGAR Views$_{it}$)*). Across all model specifications, we include firm fixed effects to control for time-invariant firm characteristics and year fixed effects to control for time-varying trends. Standard errors are clustered at the firm level to allow for arbitrary correlation of errors within each firm.

Column (1) of Table 2 presents the result of estimating equation (1a). The coefficient for the variable of interest, *App Exists$_{it}$*, is negative and statistically significant at the 1% level (Coeff. = -0.197; t-stat = -4.60), indicating that brokerage firms view fewer EDGAR filings after the firm releases its mobile app. This suggests that, all else being equal, the launch of a mobile app is associated with a decrease in analysts' acquisition of public information sources. In economic terms, a shift from non-release to release status leads to a 4% decrease (=0.197/5.231) in the interquartile range of broker-level filing views.

[Insert Table 2 here]

Viewing EDGAR filings requires a low-level effort. To acquire more information, analysts may exert more effort by participating in earnings calls and asking informative questions. To gain insights into changes in such analysts' activities after a firm's app launch, we estimate the following OLS regression:

$$
\begin{aligned}
&Log(Participating\ Analysts_{it})\ or\ Log(Question\ Length_{it}) = \beta_0 + \beta_1 App\ Exists_{it} \\
&\quad + \beta_2 Log(Mgt.\ Forecasts_{it}) + \beta_3 Size_{it} + \beta_4 Sales\ Growth_{it} \\
&\quad + \beta_5 Leverage_{it} + \beta_6 Capex_{it} + \beta_7 \#Segments_{it} + \beta_8 ROA_{it} \\
&\quad + \beta_9 BHAR_{it} + \beta_9 Log(Analyst\ Following_{it}) \\
&\quad + Firm\ fixed\ effects + Year\ fixed\ effects + \varepsilon_{it},
\end{aligned}
\tag{1b}
$$

where the dependent variable is *Log(Participating Analysts$_{it}$)* or *Log(Question Length$_{it}$)*. *Log(Participating Analysts$_{it}$)* is the natural logarithm of one plus the average number of analysts asking questions to firm *i* during conference calls in year *t*. *Log(Question Length$_{it}$)* is the natural logarithm of one plus the average length (number of words) of questions raised by analysts during conference calls in year *t*. Our coefficient of interest continues to be β_1, which captures the impact of app launch on analysts' use of private information sources. We use the same set of controls as in equation (1a) except for *Log(Other EDGAR Views$_{it}$)*.

Columns 2 and 3 of Table 2 report the result of estimating equation (1b). The coefficient on *App Exists$_{it}$* is negative and significant in both columns (Col (1): Coeff. = -0.057, t-stat = -3.08; Col (2): Coeff. = -0.027, t-stat = -2.73), indicating that analysts participate less and ask shorter questions during conference calls after a firm's mobile app launches. In economic terms, this represents 9% decline (=-0.057/0.604) in the interquartile range of the number of participating analysts and 9% decline (=-0.027/0.306) in the interquartile range of the length of questions asked during conference calls. This suggests that analysts gather fewer cues from managers after the information about app downloads becomes available.

Analysts can infer private signals from managements' answers to their questions, but conference calls are not a private source for analysts, because conference calls are immediately shared with investors in accordance with Regulation FD. Therefore, we explore analysts' effort in information acquisition by examining the taxi-ride pattern between brokerages and firm headquarters following the firm mobile app launches.[10] To this end, we estimate the following OLS regression:

$$Log(Taxi\ Rides_{it}) = \beta_0 + \beta_1 App\ Exists_{it} + \beta_2 Log(Mgt.\ Forecasts_{it}) + \beta_3 Size_{it} \\ + \beta_4 Sales\ Growth_{it} + \beta_5 Leverage_{it} + \beta_6 Capex_{it} + \beta_7 \#Segments_{it} \\ + \beta_8 ROA_{it} + \beta_9 BHAR_{it} + \beta_9 Log(Analyst\ Following_{it}) \\ + Firm\ fixed\ effects + Year\ fixed\ effects + \varepsilon_{it}, \quad (1c)$$

where the dependent variable, $Log(Taxi\ Rides_{it})$, is the natural logarithm of one plus the number of taxi rides between the firm *i* and analysts made during the year *t*. We are also interested in the coefficient β_1 and we use the same set of controls as in equation (1b).

Column (4) of Table 2 presents the results of estimating equation (1c). The coefficient for the variable of interest, $App\ Exists_{it}$, is negative and statistically significant at the 5% level (Coeff. = -0.114; t-stat = -2.13), suggesting a decline in the number of taxi rides following a mobile app launch. This pattern implies a decrease in private communication between firms and analysts after the firms launch mobile apps. In economic terms, a shift from non-app-release to app-release status leads to a 7% decrease (=-0.114/1.597) in the interquartile range of number of taxi rides in the following year, indicating a meaningful impact of app launch on private communication. In summary, our evidence suggests that analysts make less efforts to collect information from traditional public and private sources following firm mobile app launches.

Our results are robust to alternative measures of mobile app launch. First, we use daily app downloads to estimate the popularity of a mobile app. Second, we use the cumulative number of apps released by a firm during the sample period. Lastly, instead of using the earliest launch date of the three most used apps, we use the earliest launch date of all apps within a firm to define *App Exists*. We find consistent results, in Appendix C, across all measures of information acquisition.[11]

[10] All firms and brokerage firms included in this analysis have headquarters in New York City.
[11] App download data are available from 2017, while the data used to calculate *Log(EDGAR Views)* are available until 2016 and the data used to calculate *Log(#Taxi Rides)* are available until 2015. Therefore, the dependent variables

We next search for conditions under which analysts change their use of traditional sources following mobile app launches. If analysts can better predict earnings using app information, they may acquire less information from traditional sources. To empirically test this prediction, we examine whether earnings predictability improves after app releases and whether such improvement reduces reliance on traditional sources. We adopt methodology from Das, Levine, and Sivaramakrishnan (1998) and employ an autoregressive model using all available years of data for a firm:

$$EPS_{it} = \beta_0 + \beta_1 EPS_{it-1} + \varepsilon_{it} \quad (2a)$$

$$EPS_{it} = \beta_0 + \beta_1 EPS_{it-1} + \beta_2 App\ Exists_{it-1} + \beta_3 EPS_{it-1} \times App\ Exists_{it-1} + \varepsilon_{it} \quad (2b)$$

We calculate *Standardized Prediction Error*$_{it}$ for each model, where *Standardized Prediction Error*$_{it}$ is the absolute difference between actual EPS and predicted EPS from each model, scaled by share price. We measure *Prediction Improvement*$_{it}$ as *Standardized Prediction Error*$_{it}$ of equation (2a) minus that of equation (2b). Panel A of Table 3 shows the standard predicted error and prediction improvement. We find that the mean of *Prediction Improvement*$_{it}$ is 0.146, with t-statistic equals to 2.06. This implies that app launch can significantly improve earnings predictability.

Next, we estimate the OLS regression using equations (1a) to (1c) by adding *Prediction Improvement*$_{it}$ as well as its interaction with *App Exists*$_{it}$. Table 3, Panel B presents the estimation. We find that *App Exists*$_{it}$ remains negative and significant across columns. Further, the interaction term *App Exists*$_{it}$×*Prediction Improvement*$_{it}$ is insignificant when dependent variable is the number of EDGAR filing views, suggesting that app information reduces public information acquisition regardless of earnings predictability of app information. In the last three columns, we find that the

Log(EDGAR Views) and *Log(#Taxi Rides)* are dropped when the number of downloads is used as an alternative measure of mobile app launch.

interaction term is negative and significant when dependent variable is the number of participating analysts, the length of questions raised during conference calls and the number of taxi rides, suggesting that analysts reduce usage of information sources as earnings predictability of app information increases.

To further boost confidence that analysts are using app data, we identify analysts who mention apps at least once during the prior three years for any of the firms they follow and treat these analysts as being more likely to use app data. Next, we create a firm-level *App Mention* indicator which equals one if the firm is followed by at least one app-referencing analyst, and zero otherwise. We interact this indicator with *App Exists* and examine if information acquisition reduction is more prominent among firms that both have apps and app-mentioning analysts. Panel C of Table 3 presents the estimation results. Except for the taxi-ride analysis, we find a pattern consistent with our prediction, providing more direct evidence on analysts' app usage and information acquisition.

[Insert Table 3 here]

4.2 Hypothesis 2: The Effect on Analysts' Forecasting Performance

In this section, we examine the effect of firms' mobile app launches on analysts' forecasting accuracy by estimating the following OLS specification at the firm-year level:

$$\begin{aligned} \textit{Forecasting Performance}_{it} = {} & \beta_0 + \beta_1 \textit{App Exists}_{it} + \beta_2 \textit{Log(Mgt. Forecasts}_{it}\textit{)} \\ & + \beta_3 \textit{Size}_{it} + \beta_4 \textit{Sales Growth}_{it} + \beta_5 \textit{Leverage}_{it} + \beta_6 \textit{Capex}_{it} \\ & + \beta_7 \textit{\#Segments}_{it} + \beta_8 \textit{ROA}_{it} + \beta_9 \textit{BHAR}_{it} \\ & + \beta_9 \textit{Log(Analyst Following}_{it}) + \textit{Firm fixed effects} \\ & + \textit{Year fixed effects} + \varepsilon_{it}, \end{aligned} \tag{3}$$

where *Forecasting Performance*$_{it}$ is a placeholder for three variables: *Earnings Forecast Error*$_{it}$, *Sales Forecast Error*$_{it}$, and *Forecast Dispersion*$_{it}$. *Earnings Forecast Error*$_{it}$ is the firm-year average of analyst EPS forecast errors, where analyst EPS forecast error is computed as one hundred times the absolute value of the difference between one-year ahead actual EPS and EPS forecast, scaled by the stock price as of the beginning year t. *Sales Forecast Error*$_{it}$ is the firm-year average of analyst sales forecast errors, where analyst sales forecast error is computed as one hundred times the absolute value of the difference between one-year ahead actual revenue and revenue forecast, scaled by the market value of equity as of the beginning year t. If the app launch improves (deteriorates) analysts' forecasting performance, we expect β_1 to be negative (positive), using any of these two variables. *Forecast Dispersion*$_{it}$ is one hundred times the standard deviation of EPS forecasts, scaled by the stock price as of the beginning year t. If app launch leads to more (fewer) disagreements among analysts, β_1 would be positive (negative). We use the same set of controls as in equations (1b)-(1c).

Table 4 presents the results of estimating equation (3). In Column (1), we document that earnings forecast errors increase following the launch of mobile apps (Coeff. = 0.161; t-stat = 1.79). In economic terms, a shift from non-release to release status results in a 16-basis point increase in earnings forecast errors, which corresponds to of 10% of the interquartile range (=0.161/1.557).

Given the high correlation between app downloads and a firm's revenue, we further examine how app launch events influence analysts' ability to predict future sales. Interestingly, in Column (2) of Table 4, we consistently find a positive and statistically significant association between app launches and sales forecast errors (Coeff. = 0.386; t-stat = 1.66), suggesting a decline in sales forecasting accuracy. In economic terms, a shift from non-release to release status leads to a 39-basis point increase in sales forecast errors, which corresponds to 9% of the interquartile range (=0.386/4.434).[12]

[12] This result may potentially be explained by the noisy signals of the app downloads. However, untabulated result indicates that the app downloads and contemporaneous sales growth are highly correlated, suggesting that the app downloads provide a reasonably clear signal of firm performance.

Finally, we examine the relationship between mobile app launches and forecast dispersion among analysts. In Column (3), using forecast dispersion as a dependent variable, we find that earnings forecast consensus decreases (dispersion increases) after the launch of a mobile app (Coeff. = 0.090; t-stat = 1.88). In economic terms, a transition from non-release to release status leads to a 9-basis point increase in dispersion, which corresponds to 10% of its interquartile range (=0.090/0.872).[13]

Collectively, these results suggest that there is a deterioration in the forecasting performance among intermediaries following firms' mobile app launches.

[Insert Table 4 here]

4.2.1 Analysts' Lesser Use of Traditional Sources

A mechanism through which firms' app launches could increase forecast errors is analysts' lesser reliance on traditional sources in favor of mobile app information.[14] To test this mechanism, we examine whether the adverse effect of app launches on forecasting quality is exacerbated by lower contemporaneous use of traditional information sources. We augment equation (3) by including this proxy for lower information acquisition and its interaction with our app launch indicator. The variable *Lesser Use of Traditional Sources*$_{it}$ is negative one times the principal component of *Log(EDGAR Views*$_{it}$*)*, *Log(Participating Analysts*$_{it}$*)*, and *Log(Question Length*$_{it}$*)*. Higher values indicate a lower tendency to acquire data from these three traditional sources. We do not include *Log(Taxi Rides*$_{it}$*)* because of the limited number of observations. The coefficient of interest is the interaction term between this new measure and app launch indicator.

[13] In untabulated analyses, we also find that poor forecasting performance lingers up to two to three years ahead for any performance measures, suggesting that deterioration is not a short-term effect.
[14] While over-reliance on mobile app information could also potentially drive a deterioration in forecasting performance, we lack ways to empirically test this conjecture.

Table 5 presents the results. The main effect of *App Exists$_{it}$* remains positive and significant for earnings forecast error and dispersion. More importantly, we find the interaction term is positive and significant across all our forecast measures. This pattern suggests that lower reliance on EDGAR and conference calls following the launch of mobile apps results in worse forecasting by analysts. This result supports the notion that under-reliance on traditional information sources can lead to a loss of relevant information, ultimately resulting in a decline in forecasting quality even when alternative data becomes available.

[Insert Table 5 here]

4.2.2 Analysts' Reference to Mobile Apps

We distinguish between analysts who are likely to use app information and those who are less likely to use it, and estimate the following OLS specification at the *forecast-firm-year* level:

$$\begin{aligned} \textit{Forecasting Performance}_{ijt} = {} & \beta_0 + \beta_1 \textit{App Exists}_{it} + \beta_2 \textit{App Mention}_{ijt} \\ & + \beta_3 \textit{App Exists}_{it} \times \textit{App Mention}_{ijt} \\ & + \beta_4 \textit{Log(Mgt. Forecasts}_{it}) + \beta_5 \textit{Size}_{it} + \beta_6 \textit{Sales Growth}_{it} \\ & + \beta_7 \textit{Leverage}_{it} + \beta_8 \textit{Capex}_{it} + \beta_9 \textit{\#Segments}_{it} + \beta_{10} \textit{ROA}_{it} \\ & + \beta_{11} \textit{BHAR}_{it} + \beta_{12} \textit{Log(Analyst Following}_{it}) \\ & + \beta_{13} \textit{General Experience}_{ijt} + \beta_{14} \textit{Firm Experience}_{ijt} \\ & + \beta_{15} \textit{Industry Experience}_{ijt} + \beta_{16} \textit{\#Firms Covered}_{ijt} \\ & + \beta_{17} \textit{\#Industries Covered}_{ijt} + \beta_{18} \textit{Top 10 Broker}_{ijt} \\ & + \beta_{19} \textit{Allstar}_{ijt} + \textit{Firm fixed effects} + \textit{Year fixed effects} + \varepsilon_{it}, \end{aligned} \tag{4}$$

where subscript *j* indexes analyst. *Forecasting Performance$_{ijt}$* is the placeholder for earnings and sales forecast errors of individual analysts. The variable *App Mention$_{ijt}$* is an indicator variable equal to one if the analyst mentions mobile apps during earnings conference calls in the past three years, and zero otherwise. We assume that these analysts are more likely to use mobile app information and would be more affected by app launches. If the forecasting quality of affected analysts deteriorates more than less-affected analysts, we would find β_3 to be positive.

We include the same set of controls used in equation (3) and additionally include analyst-level controls such as the number of years the analyst has issued forecasts (*General Experience$_{ijt}$*), the number of years the analyst has been following the firm (*Firm Experience*$_{ijt}$), the number of years the analyst has been following the SIC two-digit industry of the firm (*Industry Experience*$_{ijt}$), the number of unique firms that the analyst covers during the year (*#Firms Covered*$_{ijt}$), the number of unique SIC two-digit industries the analyst covers during the year (*#Industries Covered*$_{ijt}$), top brokerage firm in terms of employment level (*Top 10 Broker*$_{ijt}$), and all-star analyst status (*Allstar*$_{ijt}$).

Table 6 presents the result of estimating equation (4). Consistent with our prediction, we find that the coefficient on *App Exists*$_{it}$×*App Mention*$_{ijt}$ is positive and significant when the dependent variable is either *Earnings Forecast Error*$_{ijt}$ (Coeff. = 0.013; t-stat = 1.81) or *Sales Forecast Error*$_{ijt}$ (Coeff. = 0.003; t-stat = 2.35). These results suggest that analysts that are more likely to use app downloads are more likely to exhibit lower forecasting quality following the mobile app launches.

[Insert Table 6 here]

5. Additional Analyses

5.1 Stock Market Reaction Around Analyst Forecasts Following App Launch

To investigate the effect of mobile app launches on market participants, we examine if the market responds differently to forecasts issued by affected and non-affected analysts.[15] Investors may overreact to forecasts issued by affected analysts because investors overweight alternative data. Conversely, investors may underreact to forecasts from these analysts for a few reasons. First, investors can easily obtain signals about future firm performance through app downloads, which are publicly available. As a result, given their limited attention and resources, investors may rely

[15] In Appendix D, we present the results that the percentage change in mobile app downloads from day *t*-2 to *t*-1 explains stock return in day *t*. This suggests that market participants may perceive mobile apps, specifically the number of downloads for each mobile app, as a source of information predicting a firm's future prospects.

less on analysts' forecasts. Second, app information may preempt some signals provided by analysts. Finally, investors may be aware that the forecasting quality of analysts using app information worsens after app launch. To test the conflicting predictions, we estimate the following OLS regression model at the forecast-firm-year level:

$$
\begin{aligned}
CAR_{ijt} = {} & \beta_0 + \beta_1 App\ Exists_{it} + \beta_2 App\ Mention_{ijt} + \beta_3 App\ Exists_{it} \times App\ Mention_{ijt} \\
& + \beta_4 \Delta Forecast_{ijt} + \beta_5 App\ Exists_{it} \times \Delta Forecast_{ijt} + \beta_6 App\ Mention_{ijt} \times \Delta Forecast_{ijt} \\
& + \beta_7 App\ Exists_{it} \times App\ Mention_{ijt} \times \Delta Forecast_{ijt} + \beta_8 Log(Mgt.\ Forecasts_{it}) \\
& + \beta_9 Size_{it} + \beta_{10} Sales\ Growth_{it} + \beta_{11} Leverage_{it} + \beta_{12} Capex_{it} + \beta_{13} \#Segments_{it} + \beta_{14} ROA_{it} \\
& + \beta_{15} BHAR_{it} + \beta_{16} Log(Analyst\ Following_{it}) + \beta_{17} General\ Experience_{ijt} \\
& + \beta_{18} Firm\ Experience_{ijt} + \beta_{19} Industry\ Experience_{ijt} + \beta_{20} \#Firms\ Covered_{ijt} \\
& + \beta_{21} \#Industries\ Covered_{ijt} + \beta_{22} Top\ 10\ Broker_{ijt} + \beta_{19} Allstar_{ijt} \\
& + Firm\ fixed\ effects + Year\ fixed\ effects + \varepsilon_{it},
\end{aligned}
\tag{5}
$$

where CAR_{ijt} is three-day market-adjusted cumulative abnormal return around earnings or sales forecast date. $\Delta Forecast_{ijt}$ is the placeholder for $\Delta EPS\ Forecast_{ijt}$ and $\Delta Sales\ Forecast_{ijt}$. The variable $\Delta EPS\ Forecast_{ijt}$ is the difference between current EPS estimate and previous EPS estimate for the same forecasting period, scaled by stock price. The variable $\Delta Sales\ Forecast_{ijt}$ is the difference between current sales estimate and previous sales estimate for the same forecasting period, scaled by market value of equity. The coefficient of interest is β_7.

Table 7 presents the results of estimating equations (5). As predicted, we find that the market reacts positively to the directional change in EPS and sales forecast. More importantly, we find that the coefficients on $App\ Exists_{it} \times App\ Mention_{ijt} \times \Delta EPS\ Forecast_{ijt}$ (Coeff. = -0.683; t-stat = -3.94) and $App\ Exists_{it} \times App\ Mention_{ijt} \times \Delta Sales\ Forecast_{ijt}$ (Coeff. = -0.180; t-stat = -4.46) are both negative and significant. These findings suggest that the market reaction to forecast revisions is muted following the launch of mobile apps and when the forecasting analyst explicitly mentions apps in conference calls. Collectively, our findings suggest that the informativeness of affected analysts' forecasts decreases following app launch.

[Insert Table 7 here]

6. Conclusion

Firm-developed mobile apps have enabled firms to interact with and learn about their customers. Data regarding mobile apps also allow market participants to better understand the firms and better project firms' sales and profits. As this alternative data becomes available to sell-side analysts, the data may change the way analysts acquire information by allocating more or less weights to traditional sources of information. In this study, we shed light on how mobile apps affect information acquisition and processing by analysts.

Using a unique dataset of firm-year level mobile app launches between 2008 and 2020, we document that analysts view fewer EDGAR filings, participate less and ask shorter questions in conference calls, and take fewer taxi trips to firm headquarters following the app launches. Analysts use traditional information sources even less when the app-related information improves forecasting performance and when analysts mention mobile apps in conference calls. Yet we find higher forecast errors after app launches, especially among analysts who use the traditional sources less and among those who refer to mobile apps in their reports. Finally, we document a muted market response to forecasts issued by analysts who refer to mobile apps, suggesting that the market perceives these forecasts to be less informative.

Our results suggest that analysts place more weight on alternative information sources, specifically mobile app information, and less weight on traditional sources, in part because they view alternative information as more value relevant. We also find that analysts' forecasting quality deteriorates following app launches, possibly due to stale information revealed by app-related information. Furthermore, we find that the negative effect of app launches on forecasting quality is exacerbated by lower concurrent use of traditional information sources.

Our evidence has implications for researchers and regulators. First, we contribute to research on information acquisition. Our results suggest that additional information sources (in this case, mobile app downloads) can decrease predictive outcomes by crowding out traditional sources of information (Stice 2023). Second, we contribute to the literature on the use of alternative data. While the extant literature has examined the value and consequences of alternative data, this literature has been silent on the trade-offs between information sources. Our study highlights the role played by mobile apps in affecting the use of information sources by market participants. Finally, our results should be of interest to various stakeholders, including regulators, analysts, and investors.

Appendix A
Variable Definition

Variable	Definition
Log(EDGAR Views$_{it}$)	The natural logarithm of one plus the total number of Broker EDGAR filing views during year *t*.
Log(Participating Analysts$_{it}$)	The natural logarithm of one plus the average number of analysts participating in a conference call in year *t*.
Log(Question Length$_{it}$)	The natural logarithm of one plus the average length of questions (number of words) per question asked during the conference call in year *t*.
Log(Taxi Rides$_{it}$)	The natural logarithm of one plus the number of yellow and green taxi rides between the brokerage firm and the firm in New York City in year *t*.
App Exists$_{it}$	Indicator variable equal to one if a firm has released an app on or before year *t*, and zero otherwise. Top 3 used apps are considered when setting the earliest date.
App Mention$_{it}$	Equals one if the firm is followed by at least one analyst that mentions mobile apps during conference calls in the past 3 years, and zero otherwise.
Log(#Mgt. Forecasts$_{it}$)	The natural logarithm of one plus the number of management forecasts for year *t*.
Size$_{it}$	Natural logarithm of one plus total assets.
Sales Growth$_{it}$	Sales growth from year *t*-1 to *t*.
Leverage$_{it}$	Total debt, scale by total asset.
Capex$_{it}$	Capital expenditures, scaled by total assets.
#Segments$_{it}$	The number of business segments.
ROA$_{it}$	Income before extraordinary items, scaled by total asset in the beginning year.
BHAR$_{it}$	12-month value-weighted index-adjusted abnormal return between the last and current earnings announcement date.
Log(#Analysts Following$_{it}$)	The natural logarithm of one plus the number of unique analysts following the firm during the year.
Log(Other EDGAR Views$_{it}$)	The natural logarithm of one plus the total number of EDGAR filing views other than brokerage views during year *t*.
Prediction Improvement$_{it}$	The *Standardized prediction error$_{it}$* of equation (2b) minus *Standardized prediction error$_{it}$* of equation (2a).
Standardized Prediction Error$_{it}$	One hundred times the difference between actual EPS and predicted EPS, scaled by the share price in year *t*. Predicted EPS is estimated from equation (2a) and equation (2b).
Earnings Forecast Error$_{it}$	100 multiplied by firm-year average of analysts' EPS forecast errors. Analysts' EPS forecast error is computed as the absolute value of the difference between one-year ahead actual EPS and EPS forecast, scaled by stock price as of beginning year.
Sales Forecast Error$_{it}$	100 multiplied by firm-year average of analysts' sales forecast errors. Analysts' sales forecast error is computed as the absolute value of the difference between one-year ahead actual revenue (in dollar amount) and revenue forecast scaled by the market value of equity as of the beginning year.
Forecast Dispersion$_{it}$	100 multiplied by standard deviation of EPS forecasts, scaled by stock price as of beginning year.
Sales Growth$_{it}$	The percentage growth rate in sales revenue from year *t*-1 to *t*.
Lesser Use of Traditional Sources$_{it}$	The product of negative one and the principal component of *Log(EDGAR Views$_{it}$)*, *Log(Participating Analysts)*, and *Log(#Question Length$_{it}$)*. Due to limited sample, we do not include *Log(#Taxi Rides$_{it}$)* to compute the principal component.

App Mention$_{ijt}$	Equals one if the analyst *j* mentions mobile apps during conference calls in the past 3 years for any following firms, and zero otherwise.
General Experience$_{ijt}$	The total number of years the analysts have issued forecasts.
Firm Experience$_{ijt}$	The total number of years the analysts have been following the firm.
Industry Experience$_{ijt}$	The total number of years the analysts have been following the same SIC two-digit industry of the firm.
#Firms Covered$_{ijt}$	The number of firms the analyst covers during the year.
#Industries Covered$_{ijt}$	The number of unique SIC two-digit industries the analyst covers during the year.
Top 10 Broker$_{ijt}$	Equals one if the analyst is working for a brokerage firm, which is in a top decile in terms of the number of analysts working for the firm, and zero otherwise.
Allstar$_{ijt}$	Equals one if the analyst is all-star analyst in year *t*, and zero otherwise.
CAR$_{ijt}$	Cumulative abnormal return between the day before and after the analyst forecast.
ΔEPS Forecast$_{ijt}$	The difference between current EPS estimate and previous EPS estimate (for the same forecasting period), scaled by stock price.
ΔSales Forecast$_{ijt}$	The difference between current sales estimate and previous sales estimate (for the same forecasting period), scaled by market value of equity.
#App Downloads$_{it}$	The natural logarithm of one plus the number of app downloads during year *t*, and zero for firms without App Exists (restricted to fiscal year period after 2017).
Cumulative #App Exists$_{it}$	The logarithm of one plus the cumulative number of apps released in year *t*.
App Exists_Alt$_{it}$	Indicator variable equal to one if a firm *i* has release an app on or before year *t*, and zero otherwise. If a firm launches multiple apps, we consider the earliest launch date. All apps from both the firm and its subsidiaries are considered.
Daily Return$_{it}$	The raw return of stock *i* on day *t*.
Download Growth$_{it}$	The percentage change in the number of downloads from day *t*-1 to day *t*.

Appendix B
Examples of Analysts' Usage of Mobile App Information

(1) Texas Roadhouse, Inc., Q1 2021 Earnings Call, Apr 29, 2021
Jared Garber (Analyst): (…) We've seen the app downloads really accelerate along with those updates that you made kind of at the back half or the back part of last year and into this year. I wanted to get a sense of how you're using maybe the data through that app and what you're learning about the customer and who that customer is it's using the app and whether they may be different from the customer that you traditionally see in a Texas Roadhouse? Or if it's current users adopting that technology?

(2) Jubilant FoodWorks Limited, Q2 2021 Earnings Call, Nov 12, 2020
Aditya Soman (Analyst): (…) where you've got sort of your total app downloads has gone up to, say, 44 million from 25 million. But we are not seeing the same level of increase in total orders, and we've also seen a price increase. So would that mean that the number of orders per consumer who has downloaded the app gone down significantly?

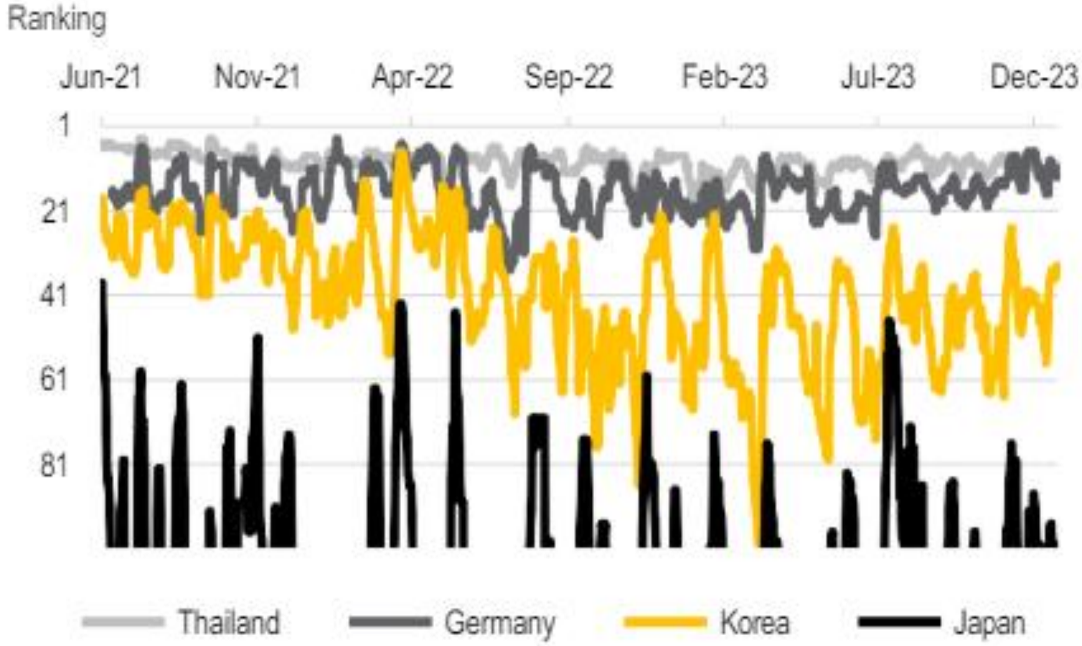
(3) Expedia Group, Inc., Q4 2019 Earnings Call, Feb 13, 2020
Christopher Kuntarich (Analyst): How are you guys thinking about driving app downloads from here? It seems like you guys have gotten some good download growth. But should we be thinking about a rebuild of the app? Or spending to drive app downloads? Or expanding a loyalty program? Yes, just any color you can share beyond what you guys have shared so far.

(4) Mobile TeleSystems Public Joint Stock Company, Q2 2021 Earnings Call, Aug 19, 2021
Alexander Vengranovich (Analyst): (…) I think, in the first quarter, the number of the MyMTS app monthly active users was roughly around 24.6 million customers, if I'm correct. And in this quarter, it was roughly around 24.5 million. So I'm just wondering whether it has any ground behind it? So that was -- if I'm correct.

(5) Walt Disney Co, UBS analyst report, April 6, 2017 (as cited in Chi et al. 2024)
The UBS Evidence Lab analyzed App data that provides wait times for the 24 Shanghai Disneyland attractions that have wait times associated with them. Our analysis covers the thirteen-week period from November 6, 2016, through January 29, 2017.

(6) Americas Beer, J.P. Morgan analyst report, July 11, 2023
We relaunch our use of the Apptopia platform, tracking downloads and usage of mobile phone apps globally (launched originally in Oct 2020: Beer? There's an app for that) – most notably examining efforts by Anheuser-Busch InBev (ABI; OW) in Lati Amerca and its Brazilian-listed subsidiary AmBev (OW, covered by Lucas Ferreira) in several markets, as well as for Heineken (N) in Mexico and Brazil.

(7) PUBG Mobile's Google Play Rev ranking trend graph, J.P. Morgan analyst report, January 4, 2024



Source: Apptopia.

Appendix C
Alternative Measure of App Exists

This table provides results of regressing the number of a brokers' EDGAR filings, the average number of analysts participating in a conference call, the average length of analysts' questions during a conference call, and the taxi rides between analysts and firms in New York City during year *t* on *App Exists* as well as covariates. In Panel A, the dependent variables *Log(EDGAR Views)* and *Log(#Taxi Rides)* are dropped due to the sample limit. Standard errors are clustered by firm and t-statistics are presented in parentheses. ***, **, and * denote significance levels at the 1%, 5%, and 10% levels, respectively. Appendix A presents variable definitions.

Panel A: Number of App Downloads as an Alternative Measure

	(1)	**(2)**
Dependent Variable =	***Log(Participating Analysts$_{it}$)***	***Log(Question Length$_{it}$)***
#App Downloads$_{it}$	**-0.005***	**-0.003***
	(-1.82)	**(-1.64)**
Log(Mgt. Forecasts$_{it}$)	0.006	0.009
	(0.51)	(0.95)
Size$_{it}$	-0.038***	-0.015***
	(-5.04)	(-2.76)
Sales Growth$_{it}$	0.028	0.035*
	(1.09)	(1.87)
Leverage$_{it}$	-0.002	0.055**
	(-0.08)	(2.39)
Capex$_{it}$	0.181	0.149
	(0.71)	(0.80)
#Segments$_{it}$	-0.005	-0.001
	(-1.32)	(-0.45)
ROA$_{it}$	-0.029	0.063
	(-0.53)	(1.56)
BHAR$_{it}$	-0.012	-0.014
	(-0.79)	(-1.25)
Log(Analyst Following$_{it}$)	0.177***	0.137***
	(12.78)	(13.48)
Industry fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
No. Observations	1,307	1,307
Adjusted R^2 (%)	23.1	22.4

Panel B: Cumulative Number of App Exists as an Alternative Measure

	(1)	(2)	(3)	(4)
Dependent Variable =	***Log(Taxi Rides$_{it}$)***	***Log(EDGAR Views$_{it}$)***	***Log(Participating Analysts$_{it}$)***	***Log(Question Length$_{it}$)***
Cum. #App Exists$_{it}$	**-0.282*****	**-0.082*****	**-0.031****	**-0.119***
	(-5.78)	**(-3.96)**	**(-2.40)**	**(-1.79)**
Log(Mgt. Forecasts$_{it}$)	-0.002	-0.020**	-0.007	-0.004
	(-0.09)	(-2.20)	(-1.01)	(-0.18)
Size$_{it}$	0.201***	-0.019	0.014	-0.012
	(6.42)	(-1.28)	(1.11)	(-0.27)
Sales Growth$_{it}$	0.142***	-0.007	-0.001	-0.004
	(3.96)	(-0.42)	(-0.09)	(-0.48)
Leverage$_{it}$	0.007	-0.039	-0.023	0.094
	(0.09)	(-1.20)	(-1.03)	(1.29)
Capex$_{it}$	0.996***	0.213	0.219**	0.004
	(2.57)	(1.32)	(1.97)	(0.01)
#Segments$_{it}$	0.009	0.002	-0.002	0.007
	(0.89)	(0.53)	(-0.80)	(0.54)
ROA$_{it}$	-0.046	0.036	0.016	0.027
	(-0.37)	(0.96)	(0.60)	(0.72)
BHAR$_{it}$	0.008	0.002	-0.007	-0.010
	(0.48)	(0.24)	(-1.27)	(-0.36)
Log(Analyst Following$_{it}$)	-0.004	0.206***	0.067***	0.040*
	(-0.12)	(15.56)	(7.85)	(1.73)
Log(Other EDGAR Views$_{it}$)	0.509***			
	(28.78)			
Firm fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
No. Observations	10,247	10,247	10,247	520
Adjusted R^2 (%)	92.2	62.4	53.9	0.973

Panel C: App Exists Including all Firms and Subsidiaries

	(1)	(2)	(3)	(4)
Dependent Variable =	***Log(EDGAR Views$_{it}$)***	***Log(Participating Analysts$_{it}$)***	***Log(Question Length$_{it}$)***	***Log(Taxi Rides$_{it}$)***
App Exists_Alt$_{it}$	**-0.183*****	**-0.057*****	**-0.030*****	**-0.102****
	(-4.11)	**(-2.99)**	**(-2.84)**	**(-2.02)**
Log(Mgt. Forecasts$_{it}$)	0.000	-0.019**	-0.007	0.001
	(0.01)	(-2.12)	(-1.00)	(0.03)
Size$_{it}$	0.195***	-0.020	0.014	-0.013
	(6.16)	(-1.35)	(1.07)	(-0.30)
Sales Growth$_{it}$	0.141***	-0.007	-0.001	-0.005
	(3.93)	(-0.43)	(-0.09)	(-0.55)
Leverage$_{it}$	-0.000	-0.041	-0.023	0.090
	(-0.00)	(-1.26)	(-1.04)	(1.25)
Capex$_{it}$	1.007***	0.218	0.219**	0.003
	(2.59)	(1.35)	(1.96)	(0.01)
#Segments$_{it}$	0.012	0.003	-0.002	0.006
	(1.10)	(0.69)	(-0.75)	(0.43)
ROA$_{it}$	-0.037	0.037	0.017	0.032
	(-0.30)	(1.01)	(0.63)	(0.86)
BHAR$_{it}$	0.007	0.001	-0.007	-0.010
	(0.42)	(0.19)	(-1.30)	(-0.35)
Log(Analyst Following$_{it}$)	-0.006	0.205***	0.067***	0.035
	(-0.18)	(15.45)	(7.83)	(1.50)
Log(Other EDGAR Views$_{it}$)	0.516***			
	(28.54)			
Firm fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
No. Observations	10,247	10,247	10,247	520
Adjusted R^2 (%)	92.1	62.3	53.9	97.3

Appendix D
Daily Returns and One-day Lagged Download Growth

This table provides the regression results of estimating the percentage change in app downloads in day *t-1* as a function of daily return in *t*. Standard errors are clustered by firm and t-statistics are presented in parentheses. ***, **, and * denote significance levels at the 1%, 5%, and 10% levels, respectively. Appendix A presents variable definitions.

	(1)
Dependent Variable =	***Daily Return$_t$***
Download Growth$_{t-1}$	**0.0003****
	(2.18)
Daily Return$_{t-1}$	-0.021***
	(-8.37)
Firm fixed effects	Yes
Year fixed effects	Yes
Month fixed effects	Yes
Weekday fixed effects	Yes
No. Observations	567,961
Adjusted R^2 (%)	1.1%

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Table 1
Descriptive Statistics

This table provides descriptive statistics for the variables used in our analyses. All continuous variables are winsorized at 1st and 99th percentiles.

	N	Mean	Std. Dev.	p25	p50	p75
$Log(EDGAR\ Views_{it})$	10,247	3.337	2.354	0.000	4.127	5.231
$Log(Participating\ Analysts_{it})$	10,247	1.466	0.444	1.145	1.417	1.749
$Log(Question\ Length_{it})$	10,247	2.912	0.259	2.773	2.933	3.079
$Log(Taxi\ Rides_{it})$	520	9.208	1.081	8.445	9.399	10.042
$App\ Exists_{it}$	10,247	0.200	0.400	0.000	0.000	0.000
$Log(Mgt.\ Forecasts_{it})$	10,247	0.771	0.836	0.000	0.000	1.609
$Size_{it}$	10,247	7.646	1.693	6.404	7.524	8.793
$Sales\ Growth_{it}$	10,247	0.090	0.250	-0.013	0.059	0.157
$Leverage_{it}$	10,247	0.261	0.242	0.045	0.235	0.390
$Capex_{it}$	10,247	0.045	0.041	0.017	0.033	0.061
$\#Segments_{it}$	10,247	4.896	3.035	3.000	5.000	7.000
ROA_{it}	10,247	0.033	0.120	0.008	0.042	0.086
$BHAR_{it}$	10,247	0.040	0.448	-0.220	-0.011	0.209
$Log(Analyst\ Following_{it})$	10,247	2.531	0.606	2.079	2.565	2.996
$Log(Other\ EDGAR\ Views_{it})$	10,247	6.475	4.155	0.000	8.610	9.359
$Earnings\ Forecast\ Error_{it}$	10,247	1.540	2.114	0.244	0.677	1.801
$Sales\ Forecast\ Error_{it}$	10,247	4.598	6.168	0.864	2.108	5.298
$Forecast\ Dispersion_{it}$	10,247	0.925	1.166	0.223	0.491	1.095
$Lesser\ Use\ of\ Traditional\ Sources_{it}$	10,247	-0.301	1.041	-1.131	-0.446	0.576
$App\ Mention_{ijt}$	229,131	0.173	0.378	0.000	0.000	0.000
$General\ Experience_{ijt}$	229,131	11.178	6.851	5.000	10.000	17.000
$Firm\ Experience_{ijt}$	229,131	5.382	4.533	2.000	4.000	7.000
$Industry\ Experience_{ijt}$	229,131	8.837	6.431	3.000	7.000	13.000
$\#Firms\ Covered_{ijt}$	229,131	5.115	2.647	3.000	5.000	7.000
$\#Industries\ Covered_{ijt}$	229,131	17.561	7.822	13.000	17.000	22.000
$Top\ 10\ Broker_{ijt}$	229,131	0.096	0.294	0.000	0.000	0.000
$Allstar_{ijt}$	229,131	0.096	0.294	0.000	0.000	0.000
CAR_{ijt}	158,659	-0.001	0.076	-0.032	0.000	0.033
$\Delta EPS\ Forecast_{ijt}$	158,659	-0.001	0.010	-0.002	0.000	0.002
$\Delta Sales\ Forecast_{ijt}$	158,659	-0.002	0.042	-0.007	0.000	0.006

Table 2
The Effect of Firm-Developed Apps on Information Acquisition of Analysts

This table reports results of regressing the number of a brokers' EDGAR filings, average number of analysts participating in a conference call, average length of analysts' questions during a conference call, and the taxi rides between analysts and firms in New York City during year *t* on App Exists indicator as well as covariates. Standard errors are clustered by firm and t-statistics are presented in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. Appendix A presents variable definitions.

	(1)	**(2)**	**(3)**	**(4)**
Dependent Variable =	***Log(EDGAR Views$_{it}$)***	***Log(Participating Analysts$_{it}$)***	***Log(Question Length$_{it}$)***	***Log(Taxi Rides$_{it}$)***
App Exists$_{it}$	**-0.197*****	**-0.057*****	**-0.027*****	**-0.114****
	(-4.60)	**(-3.08)**	**(-2.73)**	**(-2.13)**
Log(Mgt. Forecasts$_{it}$)	-0.001	-0.020**	-0.007	-0.003
	(-0.05)	(-2.15)	(-1.02)	(-0.12)
Size$_{it}$	0.196***	-0.020	0.014	-0.009
	(6.20)	(-1.32)	(1.08)	(-0.22)
Sales Growth$_{it}$	0.141***	-0.007	-0.001	-0.005
	(3.95)	(-0.42)	(-0.08)	(-0.55)
Leverage$_{it}$	-0.003	-0.042	-0.023	0.098
	(-0.03)	(-1.29)	(-1.07)	(1.35)
Capex$_{it}$	1.000***	0.216	0.219**	-0.001
	(2.57)	(1.34)	(1.95)	(-0.00)
#Segments$_{it}$	0.011	0.002	-0.002	0.008
	(1.04)	(0.66)	(-0.76)	(0.59)
ROA$_{it}$	-0.042	0.036	0.016	0.029
	(-0.34)	(0.97)	(0.60)	(0.76)
BHAR$_{it}$	0.007	0.002	-0.007	-0.010
	(0.45)	(0.21)	(-1.28)	(-0.36)
Log(Analyst Following$_{it}$)	-0.005	0.205***	0.067***	0.037
	(-0.16)	(15.49)	(7.85)	(1.60)
Log(Other EDGAR Views$_{it}$)	0.515***			
	(28.85)			
Firm fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
No. Observations	10,247	10,247	10,247	520
Adjusted R^2 (%)	92.1	62.3	53.9	97.3

Table 3

Association between Prediction Improvement and Information Acquisition Following App Exists

This table provides the improvement in EPS prediction and the association between prediction improvement and information acquisition following App Exists. In Panel A, we employ the autoregressive model of equations (2a) and (2b) using all available years for firm. We then calculate standardized absolute prediction error for each model. *Standardized Prediction Error*$_{it}$ is one hundred times the absolute value of the difference between Actual EPS and Predicted EPS, scaled by the share price in year *t*. *Prediction Improvement*$_{it}$ is the *Standardized Prediction Error*$_{it}$ of equation (2a) minus *Standardized Prediction Error*$_{it}$ of equation (2b). In Panel B, we estimate equation (1) by augmenting the prediction improvement variable as well as its interaction with *App Exists* indicator. In Panel C, we estimate equation (1) by augmenting the *App Mention* indicator as well as its interaction with *App Exists* indicator. Standard errors are clustered by firm and t-statistics are presented in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. Appendix A presents variable definitions.

Panel A: Prediction Improvement

	Mean	t-stat.	Std. Dev.	p25	p50	p75
Standardized Prediction Error$_{it}$ in equation (2a)	**32.092**	**44.23**	155.259	2.476	5.140	16.839
Standardized Prediction Error$_{it}$ in equation (2b)	**31.946**	**46.71**	146.354	2.455	5.161	17.024
= *Prediction Improvement*$_{it}$	**0.146**	**2.06**	15.101	-0.064	0.015	0.246

Panel B: Prediction Improvement and Information Acquisition following App Exists

Dependent Variable =	(1) *Log(EDGAR Views$_{it}$)*	(2) *Log(Participating Analysts$_{it}$)*	(3) *Log(Question Length$_{it}$)*	(4) *Log(Taxi Rides$_{it}$)*
App Exists$_{it}$	-0.197***	-0.058***	0.080***	-0.273***
	(-4.58)	(-3.13)	(9.31)	(-3.59)
Prediction Improvement$_{it}$	0.000	0.001***	0.018***	0.047**
	(1.37)	(14.33)	(2.49)	(2.27)
App Exists$_{it}$×Prediction Improvement$_{it}$	**0.003**	**-0.004****	**-0.019*****	**-0.043****
	(0.82)	**(-2.36)**	**(-2.57)**	**(-2.32)**
Log(Mgt. Forecasts$_{it}$)	-0.001	-0.019**	-0.021***	-0.058
	(-0.06)	(-2.12)	(-3.29)	(-1.17)
Size$_{it}$	0.196***	-0.019	0.110***	-0.012
	(6.18)	(-1.25)	(14.81)	(-0.20)
Sales Growth$_{it}$	0.141***	-0.007	-0.046***	-0.014
	(3.94)	(-0.43)	(-3.40)	(-0.74)
Leverage$_{it}$	-0.002	-0.046	0.066***	-0.091
	(-0.02)	(-1.40)	(3.43)	(-0.74)
Capex$_{it}$	1.000***	0.217	0.041	-0.451
	(2.57)	(1.34)	(0.37)	(-0.53)
#Segments$_{it}$	0.011	0.002	0.002	-0.007
	(1.05)	(0.64)	(0.87)	(-0.36)
ROA$_{it}$	-0.047	0.044	0.054**	0.123
	(-0.37)	(1.17)	(2.06)	(1.27)
BHAR$_{it}$	0.007	0.002	0.008	0.073*
	(0.42)	(0.30)	(1.35)	(1.78)
Log(Analyst Following$_{it}$)	-0.006	0.206***	0.063***	0.055
	(-0.18)	(15.51)	(7.67)	(1.01)
Log(Other EDGAR Views$_{it}$)	0.515***			
	(28.83)			
Firm fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
No. Observations	10,247	10,247	10,247	520
Adjusted R^2 (%)	92.1	62.3	42.8	88.3

Panel C: Interaction between App Exists and App Mention

	(1)	(2)	(3)	(4)
Dependent Variable =	***Log(EDGAR Views$_{it}$)***	***Log(Participating Analysts$_{it}$)***	***Log(Question Length$_{it}$)***	***Log(#Taxi Rides$_{it}$)***
App Exists$_{it}$	-0.035	-0.008	-0.004	-0.134**
	(-0.60)	(-0.31)	(-0.29)	(-2.21)
App Mention$_{it}$	-0.060**	0.019*	0.003	-0.063
	(-2.27)	(1.83)	(0.61)	(-1.16)
App Exists$_{it}$×App Mention$_{it}$	**-0.190*****	**-0.060****	**-0.025***	**0.028**
	(-3.38)	**(-2.33)**	**(-1.72)**	**(0.56)**
Log(Mgt. Forecasts$_{it}$)	-0.001	-0.020**	-0.005	-0.005
	(-0.06)	(-2.17)	(-0.96)	(-0.21)
Size$_{it}$	0.195***	-0.019	0.011	-0.011
	(6.18)	(-1.27)	(1.26)	(-0.27)
Sales Growth$_{it}$	0.142***	-0.006	0.003	-0.004
	(3.96)	(-0.41)	(0.29)	(-0.40)
Leverage$_{it}$	0.013	-0.042	-0.022	0.091
	(0.15)	(-1.27)	(-1.24)	(1.29)
Capex$_{it}$	0.989***	0.226	0.231**	-0.007
	(2.54)	(1.40)	(2.33)	(-0.02)
#Segments$_{it}$	0.011	0.002	-0.001	0.007
	(1.07)	(0.63)	(-0.49)	(0.51)
ROA$_{it}$	-0.040	0.037	0.018	0.019
	(-0.32)	(1.01)	(0.76)	(0.54)
BHAR$_{it}$	0.007	0.001	-0.007	-0.013
	(0.39)	(0.20)	(-1.22)	(-0.44)
Log(Analyst Following$_{it}$)	0.007	0.203***	0.073***	0.045*
	(0.21)	(15.33)	(9.20)	(1.78)
Log(Other EDGAR Views$_{it}$)	0.515***			
	(28.83)			
Firm fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
No. Observations	10,247	10,247	10,247	520
Adjusted R^2 (%)	92.1	62.3	54.0	97.3

Table 4
The Effect of App Exists on Analysts' Forecasting Performance

This table reports results of regressing analysts' earnings forecast error, sales forecast error, and forecast dispersion on App Exists indicator as well as covariates. Standard errors are clustered by firm and t-statistics are presented in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. Appendix A presents variable definitions.

	(1)	(2)	(3)
Dependent Variable =	***Earnings Forecast Error$_{it}$***	***Sales Forecast Error$_{it}$***	***Forecast Dispersion$_{it}$***
App Exists$_{it}$	**0.161***	**0.386***	**0.090***
	(1.79)	**(1.66)**	**(1.88)**
Log(Mgt. Forecasts$_{it}$)	-0.247***	-0.234*	-0.153***
	(-4.93)	(-1.83)	(-5.00)
Size$_{it}$	-0.235***	0.357**	-0.110***
	(-3.08)	(1.93)	(-2.78)
Sales Growth$_{it}$	0.038	0.326	-0.070
	(0.31)	(0.79)	(-0.90)
Leverage$_{it}$	1.227***	2.650***	0.728***
	(5.63)	(4.88)	(5.53)
Capex$_{it}$	-2.448***	-9.494***	-1.926***
	(-2.48)	(-3.90)	(-3.94)
#Segments$_{it}$	0.056***	0.074	0.022**
	(3.01)	(1.44)	(2.26)
ROA$_{it}$	-4.103***	-6.707***	-2.345***
	(-9.86)	(-9.37)	(-10.05)
BHAR$_{it}$	0.334***	1.009***	0.263***
	(5.91)	(7.13)	(7.52)
Log(Analyst Following$_{it}$)	-0.170***	-1.253***	-0.147***
	(-2.58)	(-6.88)	(-4.06)
Firm fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
No. Observations	10,247	10,247	10,247
Adjusted R^2 (%)	42.7	56.3	49.7

Table 5
The Effect of Analyst Existing Information Acquisition on the Relation between App Exists and Analysts' Forecasting Performance

This table reports results of regressing analysts' earnings forecast error, sales forecast error, and forecast dispersion on App Exists indicator, low information acquisition proxy, and their interactions, as well as covariates. *Lesser Acquisition of Traditional Information$_{it}$* is the negative value of the principal component between *Log(EDGAR Views), Log(Participating Analysts), and Log(Question Length).* Due to the sample limit, we do not include the taxi ride measures to compute the principal component. Standard errors are clustered by firm and t-statistics are presented in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. Appendix A presents variable definitions.

	(1)	**(2)**	**(3)**
Dependent Variable =	***Earnings Forecast Error$_{it}$***	***Sales Forecast Error$_{it}$***	***Forecast Dispersion$_{it}$***
App Exists$_{it}$	0.168*	0.367	0.096**
	(1.87)	(1.58)	(1.99)
Lesser Use of Traditional Sources$_{it}$	-0.023	0.205*	-0.020
	(-0.51)	(1.78)	(-0.82)
App Exists$_{it}$×Lesser Use of Traditional Sources$_{it}$	**0.106****	**0.255***	**0.072*****
	(2.17)	**(1.85)**	**(2.51)**
Log(Mgt. Forecasts$_{it}$)	-0.246***	-0.239*	-0.153***
	(-4.93)	(-1.87)	(-4.99)
Size$_{it}$	-0.238***	0.363**	-0.112***
	(-3.12)	(1.98)	(-2.83)
Sales Growth$_{it}$	0.036	0.331	-0.071
	(0.30)	(0.80)	(-0.91)
Leverage$_{it}$	1.218***	2.618***	0.723***
	(5.59)	(4.85)	(5.50)
Capex$_{it}$	-2.430***	-9.224***	-1.918***
	(-2.46)	(-3.80)	(-3.93)
#Segments$_{it}$	0.058***	0.080	0.023**
	(3.09)	(1.57)	(2.38)
ROA$_{it}$	-4.099***	-6.695***	-2.343***
	(-9.86)	(-9.35)	(-10.04)
BHAR$_{it}$	0.331***	1.004***	0.261***
	(5.88)	(7.08)	(7.48)
Log(Analyst Following$_{it}$)	-0.167**	-1.165***	-0.146***
	(-2.43)	(-6.33)	(-3.94)
Firm fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
No. Observations	10,247	10,247	10,247
Adjusted R^2 (%)	42.8	56.4	49.8

Table 6
The Effect of Analysts' Attention on Mobile Apps on the Relation between App Exists and Analysts' Forecasting Performance

This table provides the forecast-firm-year level regression results of estimating analysts' earnings forecast error and sales forecast error as a function of App Exists indicator, indicator for app mention during conference calls and their interaction, as well as covariates. *App Mention*$_{it}$ equals one if the analyst mentions mobile apps during conference calls in the past 3 years for any following firms, and zero otherwise. Standard errors are clustered by firm and t-statistics are presented in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. Appendix A presents variable definitions.

	(1)	(2)
Dependent Variable =	***Earnings Forecast Error$_{ijt}$***	***Sales Forecast Error$_{ijt}$***
App Exists$_{it}$	-0.002	-0.002*
	(-0.37)	(-1.77)
App Mention$_{ijt}$	-0.015***	-0.001
	(-3.42)	(-1.46)
App Exists$_{it}$×App Mention$_{ijt}$	**0.013***	**0.003****
	(1.81)	**(2.35)**
Log(Mgt. Forecasts$_{it}$)	0.000	-0.003***
	(0.02)	(-3.90)
Size$_{it}$	0.011**	-0.017***
	(2.26)	(-15.59)
Sales Growth$_{it}$	-0.016*	0.025***
	(-1.81)	(10.29)
Leverage$_{it}$	-0.016	0.024***
	(-1.12)	(7.87)
Capex$_{it}$	-0.135**	-0.023*
	(-1.92)	(-1.70)
#Segments$_{it}$	0.002	-0.001***
	(1.43)	(-3.70)
ROA$_{it}$	-0.025	-0.043***
	(-1.12)	(-8.16)
BHAR$_{it}$	-0.012***	0.002***
	(-3.17)	(2.55)
Log(Analyst Following$_{it}$)	0.013*	0.002
	(1.70)	(0.95)
General Experience$_{ijt}$	-0.001***	-0.000
	(-2.68)	(-1.43)
Firm Experience$_{ijt}$	-0.000	0.000
	(-0.74)	(0.61)
Industry Experience$_{ijt}$	0.000	0.000
	(0.55)	(0.84)
#Firms Covered$_{ijt}$	0.000	-0.000
	(0.34)	(-0.23)
#Industries Covered$_{ijt}$	-0.000	0.000
	(-0.53)	(1.46)
Top 10 Broker$_{ijt}$	-0.000	-0.001
	(-0.06)	(-1.14)
Allstar$_{ijt}$	-0.005	-0.002**
	(-0.97)	(-2.18)
Firm fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
No. Observations	229,131	229,131
Adjusted R^2 (%)	3.1%	34.9%

Table 7
Stock Market Reaction Around Analyst Forecasts

This table provides the forecast-firm-year level regression results of estimating analysts' earnings forecast error and sales forecast error as a function of App Exists indicator, indicator for app mention during conference calls and their interaction, as well as covariates. $App\ Mention_{it}$ equals one if the analyst mentions mobile apps during conference calls in the past 3 years for any following firms, and zero otherwise. Standard errors are clustered by firm and t-statistics are presented in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. Appendix A presents variable definitions.

	(1)	(2)
Dependent Variable =	CAR_{ijt}	CAR_{ijt}
$App\ Exists_{it}$	0.000	0.001
	(0.12)	(0.68)
$App\ Mention_{ijt}$	0.000	0.001
	(0.56)	(1.05)
$App\ Exists_{it} \times App\ Mention_{ijt}$	0.001	0.000
	(0.56)	(0.07)
$\Delta EPS\ Forecast_{ijt}$	1.876***	
	(38.90)	
$App\ Exists_{it} \times \Delta EPS\ Forecast_{ijt}$	-0.204***	
	(-2.46)	
$App\ Mention_{ijt} \times \Delta EPS\ Forecast_{ijt}$	0.336***	
	(2.81)	
$App\ Exists_{it} \times App\ Mention_{ijt} \times \Delta EPS\ Forecast_{ijt}$	**-0.683*** **	
	(-3.94)	
$\Delta Sales\ Forecast_{ijt}$		0.234***
		(26.40)
$App\ Exists_{it} \times \Delta Sales\ Forecast_{ijt}$		0.007
		(0.38)
$App\ Mention_{ijt} \times \Delta Sales\ Forecast_{ijt}$		0.117***
		(4.17)
$App\ Exists_{it} \times App\ Mention_{ijt} \times \Delta Sales\ Forecast_{ijt}$		**-0.180*** **
		(-4.46)
$Log(Mgt.\ Forecasts_{it})$	0.000	0.001
	(0.66)	(1.41)
$Size_{it}$	-0.005***	-0.004***
	(-7.51)	(-5.26)
$Sales\ Growth_{it}$	0.000	0.000
	(0.32)	(0.25)
$Leverage_{it}$	0.003	0.000
	(1.30)	(0.11)
$Capex_{it}$	-0.027**	-0.031***
	(-2.26)	(-2.61)
$\#Segments_{it}$	0.001***	0.000**
	(2.94)	(2.11)
ROA_{it}	0.007	0.040***
	(1.57)	(9.00)
$BHAR_{it}$	0.033***	0.036***
	(41.17)	(43.89)
$Log(Analyst\ Following_{it})$	-0.001	-0.002*
	(-0.97)	(-1.66)
$General\ Experience_{ijt}$	0.000	0.000
	(0.71)	(1.19)
$Firm\ Experience_{ijt}$	0.000	0.000
	(1.38)	(0.90)
$Industry\ Experience_{ijt}$	-0.000	-0.000

	(-0.40)	(-0.59)
#Firms Covered$_{ijt}$	0.000**	0.000
	(2.07)	(1.55)
#Industries Covered$_{ijt}$	-0.000	-0.000
	(-0.65)	(-0.53)
Top 10 Broker$_{ijt}$	-0.000	-0.000
	(-0.36)	(-0.56)
Allstar$_{ijt}$	-0.000	-0.000
	(-0.96)	(-0.68)
Firm fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
No. Observations	158,659	158,659
Adjusted R^2 (%)	13.2%	10.2%