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"Essays on Financial Analysts, Auditors, and Industry Peers"

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CARNEGIE MELLON UNIVERSITY

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Abstract

This dissertation investigates how external factors shape a firm's information environment. A transparent firm information environment is important to the efficiency of capital markets. To make informed decisions, economic agents such as investors, creditors, and suppliers rely not only on the information provided by firms, but also on other sources outside the firms' control. The dissertation aims to expand our understanding of how external factors—namely, financial analysts' earnings forecasts, the quality of auditors' assurance services, and peer firms' influence—contribute to a firm's information environment.

In the first essay, I find evidence of bounded rationality in financial analysts and how it affects the information that analysts produce. I argue that analysts face a limited attention constraint. Consequently, they must strategically choose which information to pay attention to and which information to ignore when making earnings forecasts. This prevents them from fully utilizing the available information to produce the most accurate forecasts. I rely on rational inattention theory (Sims 2003) to develop and formalize the relationships among the factors that determine analyst attention and how analyst attention affects forecast accuracy. To map my theoretical predictions to the data, I construct a novel measure of attention that varies across stocks followed by the same analyst during the same fiscal period. I find that analysts tend to pay

more attention to firms with volatile earnings and firms in their industry specialization while paying less attention to those that are new to their portfolio. Importantly, I find that attentive analysts are more accurate, and the effect of attention is larger for inexperienced analysts and stocks with highly volatile earnings. My findings imply that it is not just analyst coverage that matters; it is attentive coverage that reduces the information asymmetry between firms and investors.

In the second essay, I study whether and to what extent the job satisfaction of employees affects the quality of their auditing services. To do so, I utilize a novel large-scale data set on job satisfaction from Glassdoor.com and an identification approach based on local precipitation to pin down the direction of causality and to show that employee job satisfaction has a significant positive effect on audit quality. Further, the evidence suggests that satisfied employees are better at detecting significant accounting irregularities but remain the same as other employees at detecting minor accounting errors. Among job satisfaction indicators, these effects are driven by management quality and career opportunities. Overall, these findings demonstrate the importance of individual audit employees to the audit process and have practical implications for audit firms' treatment of employees and audited firms' information environments.

In the third essay, I investigate the strategic interactions among industry peers with respect to financial reporting behaviors. Exploiting quasi-exogenous variation in the timing of peers' earnings announcements based on the SEC's threshold-based reporting deadline rules, I present evidence that peers have a disciplining effect on firm disclosure timing decisions. I find that firms respond to early peer announcements by announcing their own earnings early. Furthermore, I show evidence suggesting that peer effects operate under two mechanisms: disciplining and information transfer. Discipline occurs because investors may infer bad news from delay relative to reporting peers. Information transfer occurs because peer reports contain information that is directly value relevant for the focal firm. Importantly, peer effects impose a significant spillover cost in the form of increased audit fees on firms facing peer pressure to report early. My findings highlight a novel externality of financial reporting regulation with benefits and costs that should be considered by policymakers concerned with the timing of earnings information releases.

Contents

Chapter 1

Rational Inattention and Analyst Forecast Accuracy

1.1 Introduction

Equity analysts are expected to gather relevant information to produce accurate earnings forecasts for all the firms they cover (Healy and Palepu 2001). However, like any other decision makers, analysts may be subject to a limited attention constraint. Because analysts typically follow many firms at the same time (Clement 1999; Jacob, Lys, and Neale 1999), the more firms they follow, the less attention they can devote to each firm. Moreover, when exposed to a vast amount of information coming from many sources, analysts must strategically decide which information to process more carefully, and which information to process less carefully or to ignore. These decisions in turn directly affect their ability to produce reliable earnings forecasts. In this paper, I investigate what determines an analyst's attention, and how her attention level subsequently affects the accuracy of her earnings forecasts.

Although little is known about the role of rational inattention in analyst forecasts, ample evidence in economics and finance suggests that investors behave differently under a limited attention constraint. For example, investors tend to focus on high attention-grabbing stocks heavily covered in the media, respond weakly to earnings announcements made on Fridays when inattention is more likely, and underreact when many news events are occurring on the same date (DellaVigna and Pollet 2009; Barber and Odean 2008; Hirshleifer, Lim, and Teoh 2009). Van Nieuwerburgh and Veldkamp (2009) provide an analytical model showing that under a limited attention capacity, investors prefer to specialize in a subset of assets rather than diversify across all assets. My paper represents the first attempt to examine the impact of attention in the analyst setting, establishing attention as an important factor that influences analyst forecasting behavior. My study also represents a departure from the existing literature on analyst characteristics that affect forecast accuracy, because I investigate a factor that dynamically changes over time and within the analyst's firm portfolio instead of the ones that are either constant from year to year or affect forecasts for all firms followed by the analyst in the same way (e.g., all-star status, brokerage resources, years of experience).

I formally develop and test four main hypotheses. Leveraging the theory of rational inattention (Sims 1998, 2003), I consider a stylized analytical model of the analyst forecasting earnings of one firm under the limited attention constraint, to guide my empirical analysis. The economic trade off she faces is between the ability to produce more accurate forecasts and her attention cost. The model yields intuitive predictions that can be mapped into the data. First, analyst attention is an increasing function of earnings volatility. Second, analyst attention is a decreasing function of the marginal cost of paying attention. Third, forecast accuracy decreases in the firm's earnings volatility. Finally, forecast accuracy increases in the analyst's attention level.

To test these predictions, I introduce the following measure of attention: the *abnormal number of revisions* after controlling for forecast age, firm-year fixed effects, and analyst-year fixed effects. This measure is intended to capture the amount of discretionary attention an analyst allocates to a firm on top of what is expected, because there may be alternative reasons that an analyst would issue a revised forecast (e.g., the firm might have issued a new disclosure). The rationale is as follows.

First, I argue that the number of revisions is a revealed preference measure of attention. Intuitively, issuing a revised forecast is a costly activity to analysts, both in terms of effort and reputation. It is unlikely that an analyst would pay little to no attention before revising her forecast. Therefore, a revision is an outcome of attention, and a higher number of revisions implies a higher level of attention. Next, to eliminate alternative explanations as to why an analyst may revise more or less frequently, I augment the baseline measure with three other ingredients.

The first ingredient is forecast age—the timing of the most recent forecast the analyst issues for the fiscal year. This controls for cases in which an analyst pays close attention but issues only a few (but perhaps more accurate) revisions during the year. In this case, I would expect attentive analysts to spend more time conducting research and issue their last revision closer to the end of the fiscal year. In other words, it is unlikely that an analyst who revises three times but whose most recent forecast is in March pays more attention than an analyst whose most recent and only forecast is in November, assuming that the fiscal year end is December 31. To put it differently, for analysts forecasting the same firm and whose most recent forecasts are on the same day, the one revising more during the year pays more attention than the one who revises less.

The second ingredient is analyst-year fixed effects. This controls for time-varying conditions that affect analysts' abilities, workloads, and the average characteristics of the firms in their portfolios. For example, talented analysts may process information more efficiently and therefore issue fewer revisions during the year. On the other hand, analysts who follow many firms in a given fiscal year may be physically constrained from paying close attention to each individual firm they follow. By controlling for analyst-year fixed effects, I effectively exploit within analyst-year variation in which all forecasts are issued by the same analyst, thereby mitigating concerns about coverage decisions and confounding effects from unobservable analyst ability, workload, and any other time-varying analyst characteristics.

The third ingredient is firm-year fixed effects. This forces the identifying variation to come from analysts with different levels of attention who follow the same stock in the same year. In particular, it could be the case that the number of forecast revisions positively depends on the number of disclosures and management guidance reports the firms release during the year. Thus, not controlling for this aspect would overestimate not only the measure of attention but also its estimated effect on forecast accuracy. In particular, the firm-year fixed effects allow me to exploit within firm-year variation, absorbing the effect of any firm-year level characteristics and any event happening to the firm in a given year.

My analysis yields results that are consistent with my theoretical predictions. First, I find evidence suggesting that analyst attention level increases in firm earnings volatility, measured by the ROA volatility of the past twenty quarters. Second, using firm-specific experience and industry specialization as inverse proxies for the marginal cost of attention, I find that there is a positive relationship between both proxies and attention level, indicating that the lower the marginal cost of attention, the higher the attention level.

Third, I find that forecast accuracy is negatively related to earnings volatility on average. This is consistent with my prediction that accuracy would be lower for firms that are more difficult to forecast. Fourth, attention has a first-order effect on an analyst's forecast accuracy. Specifically, I find that a one standard deviation increase in attention improves forecast accuracy by an average of 4.6%. This is an economically meaningful impact, considering that one standard deviation in other known factors in the literature such as firm-specific experience or industry specialization improve accuracy by roughly 0.5%.

Next, I show that there are heterogeneous effects of attention on forecast accuracy depending on attention determinants. By interacting my attention measure with earnings volatility and proxies for the marginal cost of attention (firm-specific experience or specialization), I find that the effect of attention on accuracy is stronger for highly volatile firms, and weaker when analysts have firm-specific experience or industry specialization. The former result is sensible because one would expect that the marginal benefit of paying one extra unit of attention to forecasting a volatile firm would be higher than that of a stable firm. Likewise, the latter result is also intuitive, in that when an analyst has some degree of familiarity with the firm, we would expect the marginal benefit of paying attention to be lower.

Prior literature also suggests that analysts may sometimes experience pressure from the brokerage firms to bias their forecasts optimistically to cater to management (Lin and MacNichols 1998; Michaely and Womack 1999; Dechow, Hutton, and Sloan 2000; Lin, McNichols, and O'Brien 2005). If analysts had this additional forecasting objective, then we would expect that they would choose an optimal upward bias, given the attention constraint. In other words, we would observe a higher level of optimism for attentive analysts on average. However, I find that attentive analysts are less optimistic in their forecasts compared to consensus and that they tend to revise downwards more often. This suggests that though analysts sometimes face pressure to bias their forecasts upwards, they, on average, allocate attention so as to maximize forecast accuracy.

An alternative explanation to my findings could come from costly information acquisition (Verrecchia 1982). The theory of rational inattention and costly information acquisition are similar, in that an agent incurs some cost to have more precise information signals. Although, there is a subtle difference—in costly information acquisition models, agents can acquire information and do not pay attention to it; my empirical analysis cannot completely rule out this alternative explanation, because a forecast revision is a product of both acquiring and paying attention to (i.e., using) the new information.

This paper draws on the theoretical literature on rational inattention (e.g., Sims 1998, 2003; Van Nieuwerburgh and Veldkamp 2009) and the literature on the behavioral effects of inattention (e.g., Hirshleifer and Teoh 2003; Hirshleifer et al. 2009; Dellavigna and Pollet 2009; Barber and Odean 2008), and analysts' characteristics influencing their forecast accuracy (e.g., Mikhail, Walther, and Willis 1997; Clement 1999; Jacob et al. 1999). I extend these literatures by examining how analysts allocate attention and whether attention affects forecast accuracy. My study contributes to the above three strands of literature. First, I add to the inattention literature in finance and accounting by being the first to show evidence that analysts suffer from a limited attention constraint. Second, I contribute to the analyst literature by providing a measure of analyst attention, studying its determinants, and offering rigorous empirical findings showing that attention, a dynamically changing factor, is an economically important factor improving forecast accuracy. Additionally, I show that attentive analysts are less optimistic in their forecasts. Third, I complement the theory of rational inattention literature by lending empirical support for the use of rational inattention theory in accounting and finance research, and I hope to widen the avenue for future studies in this topic, both analytically and empirically.

Finally, it is worth noting the distinction between my study and that of Jacob et al. (1999). In their paper, they find that forecast accuracy is positively associated with analyst quarterly forecast frequency, which they interpret as the analyst's effort. I extend and complement their findings by (i) creating a four-component measure of attention that helps mitigate various alternative explanations for the positive association observed between forecast frequency and accuracy, (ii) investigating the determinants of attention, and (iii) explicitly linking heterogeneity in the forecast accuracy effects to these attention determinants.

1.2 Related Literature

1.2.1 Prior Literature on Factors Affecting Forecast Accuracy

My paper is part of the literature on financial analysts that studies the factors affecting their forecast accuracy. Maines, McDaniel, and Harris (1997) find that the level of accuracy increases with analyst experience. Additionally, Mikhail et al. (1997) and Jacob et al. (1999), respectively, show that firm-specific experience and industry specialization have a positive effect on forecast accuracy. On the other hand, Clement (1999) demonstrates that job complexity, proxied by the number of firms and industries followed, negatively affects analyst forecast accuracy. There are also other characteristics not at the analyst level that influence forecast accuracy. For example, Chopra (1998) finds that the accuracy of analyst forecasts increases if the economy is growing fast. However, Hope and Kang (2005) show that macroeconomic uncertainties are negatively associated with analyst forecast accuracy. Changes in accounting standards have been shown to have an effect on analyst forecast accuracy by a number of studies (Daske, Hail, Leuz, and Verdi 2008; Ashbaugh and Pincus 2001; Horton, G. Serafeim, and I. Serafeim 2013; Byard, Li, and Yu 2011; Cotter, Tarca, and Wee 2012). My paper contributes to this literature by offering a measure to capture analyst attention and showing that attention is another analyst characteristic that positively affects forecast accuracy.

It is worth noting that my paper does not investigate the level of attention a firm gets from the financial market, but rather the level of attention each analyst pays to the forecast earnings of one firm in her portfolio. For example, Bhushan (1989) examines the relationship between firm characteristics and analyst following. He finds that higher return variability results in a higher number of analysts following. My study is different, in that it focuses on the analyst's optimization problem, in which she decides how much attention to devote to forecasting earnings of a firm, whereas Bhushan's (1989) findings speak to the brokerage firms' problem, in which they decide how many analysts should be assigned to follow each firm. My finding extends Bhushan's result by showing that conditional on the number of analysts following a firm, each analyst would pay a different level of attention to that firm depending on the relative earnings volatility of the firms in each analyst's portfolio.

1.2.2 Prior Analytical Literature on Rational Inattention Theory

My paper also relates to the emerging theoretical literature on the rational inattention of economic agents, pioneered by Sims (1998, 2003). This theory models attention as information flow and limited attention as a bound on the information flow (See Appendix B for more details). Attention is formally treated as a costly, scarce resource to be optimally allocated. A few studies in finance and economics have studied the effect of rational inattention on investors' behaviors. For example, van Nieuweburgh and Veldkamp (2009) show that under the limited attention constraint, investors prefer specializing in only the group of assets they are familiar with, rather than diversifying across all assets. In a separate study, Mondria (2010) finds that attention allocation can impact portfolio choice and asset price comovement. Huang and Liu (2007) provide an analytical model demonstrating that rational inattention to important news may make investors over- or underinvest. Inattention has also been modeled in reduced forms in the literature. For example, Hirshleifer and Teoh (2003) study how firms change their information disclosure policies based on an exogenously given fraction of inattentive investors, who are assumed to have different information structures on the firm fundamentals.

I add to this literature by being the first to apply rational inattention theory to the analyst setting and formally develop my testable hypotheses about which factors determine attention level as well as the relationship between attention and forecast accuracy. My empirical analysis also contributes to this literature by lending empirical support to the use of rational inattention theory in accounting and finance research.

1.2.3 Prior Empirical Literature on the Effects of Inattention

Although there is scant empirical evidence of analyst inattention, there is ample evidence on the behavioral effects and consequences of inattention in investor behaviors. For example, DellaVigna and Pollet (2009) find evidence suggesting that investor response to earnings announcements on Fridays, when inattention is more likely, is lower than that on other weekdays. DeHaan, Shevlin, and Thornock (2015) use various proxies for investor and market attention, such as the number of EDGAR 8-K downloads, abnormal Google search volume, and the number of earnings-related news articles, to show that investors are less attentive on Fridays. Hirshleifer et al. (2009) show evidence of market underreaction to an earnings announcement if it occurs on a date on which many other firms also announce earnings. Barber and Odean (2008) show that investors are net buyers of attention-grabbing stocks such as those heavily covered in the media or those that have experienced a high level of abnormal trading volumes, or with extreme one-day return.

My study adds to the literature by investigating the role of rational inattention in the financial analyst setting and by providing empirical evidence showing that analysts suffer from a limited attention constraint and that attentive analysts provide more accurate forecasts and are less optimistic compared to their peers.

1.3 Hypothesis Development

1.3.1 Research Setting

The hypotheses of this paper are formally developed based on rational inattention theory by Sims (1998, 2003). The theory posits that attention is a scarce and costly resource to be allocated. Everyone's attention span has limits. Therefore, when facing information from various sources, each person has to decide which information to pay attention to carefully and which information to ignore. Economic agents cannot utilize any piece of information given to them to the optimal extent unless they pay a sufficient amount of attention. Departing from conventional economic research, the theory says that an economic agent can get more out of an information signal by paying more attention. For example, if two people with comparable information processing abilities are given one hour to read the same edition of *The Wall Street Journal*, the information retained by each person ex post may be different depending on how much attention they devote to reading the newspaper.

Applying this concept to the financial analyst setting, I argue that analysts, too, have a limited attention constraint. The economic trade-off they face is between the cost of paying attention and the benefit of producing more accurate earnings forecasts. This setting allows me to study which factors influence an analyst's attention allocation across the firms she follows and how her forecast accuracy is subsequently affected by the attention levels she chooses.

I operationalize the above economic story by examining a stylized analytical model. Appendix B contains a formal treatment of the model and its predictions. I consider an analyst whose job is to forecast the earnings of a firm. She can observe the firm's earnings volatility and its expected value based on past earnings (i.e., the distribution of earnings is known). During the fiscal year, she receives additional information that helps her make her forecasts.[1] This information signal tells her more about what the true value of the earnings may be at the end of the fiscal year. Conditional on the signal, the analyst makes her forecasts.

However, she needs to pay attention, and there is a marginal cost to paying each unit of attention. This cost can be interpreted as the opportunity cost of devoting one unit of attention to forecasting earnings (as opposed to doing something else, such as forecasting the earnings of other firms, making recommendations, or leisure). This marginal cost in reality is reflected in the analyst's own characteristics. For example, industry specialization and following a firm for a long time could help an analyst process information faster or communicate with management more efficiently. Therefore, firm-specific experience and industry specialization reduce the marginal cost of paying attention.

The analyst's objective is to minimize her forecast error as well as her marginal attention cost. The assumption that analysts' objective is to minimize forecast error is supported by prior analytical research (e.g., Beyer 2008). Empirical evidence also supports the assertion that analysts who issue more precise forecasts enjoy higher compensation and a better reputation (Stickel 1992;

[1] For example, the analyst could use the firm's disclosures, management guidance reports, peers' forecasts, information from conference calls or press releases, and so on.

Keane and Runkle 1998; Mikhail, Walther, and Willis 1999; Jackson 2005; Fang and Yasuda 2008).[2]

1.3.2 Predictions and Hypothesis Development

Under the above setting, the model predicts that the analyst will pay more attention to the firms whose earnings are more volatile and pay less attention otherwise. Intuitively, it does not require a lot of attention to forecast the earnings of a stable firm, because the current year's earnings will likely be similar to those from previous years. On the other hand, the analyst may want to pay more attention when studying a difficult-to-forecast firm (i.e., a highly volatile firm). This leads to my first testable hypothesis:

H1a: Analyst attention level increases with the firm's earnings volatility.

Next, the model predicts that the higher the marginal cost of paying attention, the lower the analyst attention level. Intuitively, if it is too costly for the analyst to pay attention to a firm, she will opt to pay less attention to forecasting the earnings of that firm. In contrast, if it is less costly to pay attention, then she will pay more attention, to increase the chance of accurately forecasting the firm's earnings. As discussed above, the (inverse) proxies for the marginal cost of attention are the analyst's firm experience or the industry specialization. The second hypothesis is:

H1b: Analyst attention level decreases with the marginal cost of paying attention. Specifically, attention level increases with firm-specific experience or industry specialization.

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[2] I abstract away from other incentives that an analyst might face. For example, Hayes (1998) and Jackson (2005) suggest that analyst incentives could be based on stock recommendations or trading commissions. Other incentives are based on selection bias (McNichols and O'Brien 1997), access to management (Lim 2001), career concerns (Hong and Kubik 2003), and underwriting business (Dugar and Nathan 1995; Lin and McNichols 1998; Michaely and Womack 1999). Because the focus of my study is the role of attention on forecast accuracy, by limiting the objective function to minimizing forecast error (and attention cost), I can have the simplest model that generates predictions about the relationships between the variables of interest.

I next extend the argument to examine how forecast accuracy is affected by earnings volatility and the analyst's chosen level of attention. The third prediction from the model is that analyst forecast accuracy is lower for volatile firms. The intuition is straightforward, in that it is more difficult to accurately forecast the earnings of a highly volatile firm, whereas it is simpler to be more accurate when forecasting the earnings of a stable firm. This leads to my third hypothesis:

H2a: Forecast accuracy decreases with the firm's earnings volatility.

Finally, the model predicts that analysts who pay more attention produce more accurate forecasts on average. Intuitively, given the same information report (e.g., a firm's disclosure report), the analysts who pay more attention can extract more information out of the report than the ones who pay less attention. Thus, the former analysts are expected to produce more accurate forecasts. The final hypothesis is stated as follows:

H2b: Forecast accuracy increases in the analyst's attention level.

Various empirical challenges can test these hypotheses. First, I need a measure of analyst attention. Second, endogeneity concerns coming from selection and omitted variables prevent me from obtaining unbiased estimates of the effect of attention on forecast accuracy. The next section discusses my attention measure as well as the endogeneity concerns.

1.4 Attention Measure

The attention measure is defined as the *abnormal number of revisions* after controlling for forecast age, firm-year fixed effects, and analyst-year fixed effects. The idea is to capture the discretionary portion of an analyst's attention beyond what is expected. The following four subsections discuss each component one by one.

1.4.1 Baseline Measure – Number of Forecast Revisions

The first and main component of my analyst attention measure is the number of forecast revisions of the analyst forecasting Earnings Per Share (EPS) of a firm in a fiscal year, denoted by $Attention_{ijt}$, where i is the analyst, j is the firm, and t is the fiscal year. Issuing revised forecasts is a costly activity because it not only requires the analyst to do her due diligence to decide what the new EPS estimate should be (effort cost), but it also entails a reputational cost that she has to take into account if the revised forecast turns out to be inaccurate. In other words, it is unlikely that an analyst would randomly issue a forecast revision without having carefully thought about it. Thus, I argue that the number of forecast revisions is a revealed preference measure of attention, in the sense that a revision must be an outcome of attention and a higher number of revisions implies a higher level of attention.

Nevertheless, there could be a number of alternative scenarios in which the number of revisions alone cannot satisfactorily capture the attention level of an analyst. For example, an analyst could pay close attention to a firm and issue only a few high-quality forecasts. Similarly, less talented analysts may make mistakes and have to subsequently issue many revisions. The information environment of the firm also influences how many revisions an analyst may need to issue. Therefore, the baseline measure of attention is augmented by three other ingredients, each of which is discussed below.

1.4.2 Controlling for the Timing of the Most Recent Forecast

The timing of the most recent forecast of the fiscal year tells us more about how much attention an analyst pays when forecasting a firm. If the analyst in fact pays a lot of attention and issues only a few forecasts, I argue that her last forecast should be issued closer to the end of the fiscal year. Intuitively, it is unlikely that an analyst whose last forecast is issued in March pays a lot of attention to forecasting for a firm whose fiscal year ends on December 31. On the other hand, if her last forecast is issued in November, it is reasonable to believe that she may have paid attention despite not issuing many forecasts before that. Thus, by controlling for $ForecastAge$, the number of days between the earnings announcement date and the most recent forecast date, I can take into account the timing aspect of attention. Specifically, among analysts whose most recent forecast is on the same day, the ones who issue more revisions pay more attention. Figure 1.1a graphically shows this intuition.

1.4.3 Controlling for Analyst-Year Fixed Effects

Although the concepts of attention and information processing ability are different from one another, they both have an effect on an analyst's number of forecast revisions. The existence of the latter could, therefore, potentially confound my measure of attention. The information processing ability of an analyst depends on various factors such as her talent, ability, experience, specialization, workload, work complexity, resources, the average characteristics of the firms in their portfolios, and so on. Each of these factors can influence both her capacity to revise her forecasts and her forecast accuracy. Analysts can also be selected into forecasting certain firms because of these factors. Therefore, a naïve comparison among different analysts with different abilities and workload would not bring us the unbiased effect of attention on forecast accuracy.

For example, talented analysts may issue fewer forecasts but more accurate forecasts because they can process information at a faster and more effective rate. Similarly, analysts who follow a large number of firms may not have time to revise frequently and make, consequently, less accurate forecasts. Therefore, to control for both observable and unobservable analyst-time-varying characteristics affecting the number of revisions (including but not limited to the ones mentioned above), I include analyst-year fixed effects as the third component of my attention measure.

Figure 1.1b shows graphically the intuition about why these fixed effects could help highlight the attention component of the number of revisions in the presence of analyst's talent, work complexity, or resources from the brokerage firm. The fixed effects allow me to look at one analyst in one year at a time and compare the numbers of revisions she issues across the firms in her portfolio in that year. Thus, the same analyst in the same year pays more attention to the forecasting earnings of the firm for which she issues more revisions.

1.4.4 Controlling for Firm-Year Fixed Effects

The amount of attention may depend on the information environment of the firm the analyst is trying to forecast the earnings of. For instance, it may be easier to pay attention and issue revised forecasts for companies who release many management guidance reports or hold many press conferences during the year. Furthermore, the effect of attention on forecast accuracy may also be confounded by these factors simply because the analyst has more and perhaps better information to work with. To take into account the mechanical relationship between the number of forecast revisions and the firm's information environment, I control for firm-year fixed effects, which is the fourth and final ingredient of my attention measure.

Figure 1.1c shows graphically the intuition about why these fixed effects could help highlight the attention component of the number of revisions in the presence of differential firm information environments. The fixed effects allow me to compare among analysts who follow the same firm in the same year. In this context, all analysts are subject to the exact same firm's information environment; therefore, the analysts who revise more pay more attention.

Another advantage of using firm-year fixed effects is that they can control for any potential omitted variables, observable or unobservable, at the firm-year level that might bias the estimated effects of attention on forecast accuracy. These firm-year variables include, but are not limited to, the firm's fundamentals, the quantity of disclosure and management guidance reports from the firm in a given year, how spread out the reports are, the aggregate financial reporting quality or the disclosure quality of the firm in a given year, CEO change, how often the consensus forecast changes, and the number of analysts following the firm. The last two variables are factors outside of the firm's control, and they reflect the competition among analysts. The more analysts following a firm, the more likely that each analyst revises more frequently, due to the pressure from competition and the availability of peers' forecasts.

1.4.5 Attention Measure – Abnormal Number of Revisions

Taking together all four components, the attention measure is effectively the *abnormal number of revisions* after controlling for forecast age, firm-year fixed effects, and analyst-year fixed effects. This measure captures the amount of discretionary attention an analyst allocates to a firm on top of what is expected of her, given the nature of her portfolio and the information environments of the firms she follows. Figures 1.2a and 1.2b give visual representations of the number of revisions and the residual attention measure. Figure 1.2a shows the attention allocation across the firms in a particular analyst's portfolio in the year of 2010. This analyst follows 10 firms in 2010. The blue bars represent the "raw" numbers of forecast revisions, whereas the green bars depict the residual attention measure. There are significant differences between these two values. For example, for firm #1, the number of revisions is 2, while the attention measure is only about 0.13. This implies that even though this analyst revises twice, her true attention level is not very different from those of other analysts who forecast the exact same firm with comparable ability and firm portfolio, as well as with a similar timing of the most recent forecast. On the other hand, the number of revisions for firm #2 is 0, but the attention measure is approximately -2.1. This implies that not revising means a significantly lower attention level when comparing among analysts who all follow firm #2 with similar ability and firm portfolio and timing of the most recent forecast.

Figure 1.2b shows how an analyst's attention to a particular firm changes over time. The blue bars show the "raw" revision numbers, and the green bars show the residual attention. Similar to Figure 1.2a, Figure 1.2b shows significant differences between the pairs. For example, whereas the number of revisions is 2 in both 2010 and 2014, the attention measure is 1.2 in 2010 and -0.5 in

2014. This implies that in 2010, revising twice is actually more than the expected amount of revisions when comparing across similar analysts who forecast this same firm, whereas in 2014, revising twice is less than the expected amount of attention. One explanation could be that compared to 2010, this firm released many more guidance reports in 2014 but this analyst did not pay enough attention to revise accordingly.

1.5 Data and Sample Selection

I use the I/B/E/S Detail History with Actuals tape for individual analyst EPS forecasts. Panel A of Table 1.1 reports the sample selection process. The initial I/B/E/S dataset contains over 4.3 million analyst forecasts from 1981 to 2015. Analyst codes are used to identify analysts. These codes remain with the analyst even if she switches brokerage firms. However, analyst codes sometimes do not distinguish between individuals and teams of analysts. I use the I/B/E/S broker translation file to eliminate teams of analysts from the sample.

Because the I/B/E/S dataset is left censored, it is not possible to tell how much experience some analysts have prior to the first year of available data. Following prior literature (Clement 1999), analysts who appear in the dataset in the initial two years (1981 and 1982) are excluded from the sample to mitigate this problem. The dataset is then merged with Compustat for firm fundamentals data. Next, the most recent forecasts of each analyst-firm-year tuple are retained to calculate analyst forecast accuracy. Finally, I merge the dataset with I/B/E/S guidance for management guidance data and ExecuComp for CEO change data. Panel B of Table 1.1 shows the characteristics of the final sample. There are 9,748 analysts, 3,150 firms, and 769 brokerage firms in the sample. The total number of analyst-firm-year observations is 337,624, spanning the period from 1992 to 2015.

The main variables of interest are defined as follows. First, $Attention_{ijt}$ is the number of EPS forecast revisions analyst i made for firm j in fiscal year t. Earnings volatility, $Volatility_{jt}$, is the ROA volatility of firm j as of year t. It is calculated as the standard deviation of ROA in the past 20 quarters, with the condition that there are at least 8 non-missing observations. For forecast accuracy, I consider two variables, named $Accuracy_{ijt}$ and $Accuracy\_S_{ijt}$. They are defined as

$$Accuracy\_{}_{ijt} = -\log(|Forecast_{ijt} - Actual_{jt}|/|Actual_{jt}|)$$

$$Accuracy\_S_{ijt} = -\log(|Forecast_{ijt} - Actual_{jt}|/|StockPrice_{jt}|)$$

where $Forecast_{ijt}$ refers to the most recent EPS forecast analyst i makes for firm j in fiscal year t, $Actual_{jt}$ is the actual EPS reported by firm j for fiscal year t, and $StockPrice_{jt}$ is the closing price of firm j for fiscal year t. Two measures are similar, except that the latter uses stock price as a deflator, which is more often used in the analyst literature (Butler and Lang 1991; Dhaliwal, Radhakrishnan, Tsang, and Yang 2012). I apply log transformations to both measures to have a more direct map between the closed form solution of the analytical model and the relationship of the variables in the data (See Appendix B). Using the log also has another advantage: easy interpretation of the results. It is also worth noting that because firm-year fixed effects are included in the regressions, the accuracy measures can be viewed as relative accuracy among analysts forecasting the same firm j in year t. As noted in the literature, using relative measures is important, as they take into account any firm- and time- specific factors affecting forecast accuracy (Jacob et al. 1999; Clement 1999; Hong et al. 2000; Cowen et al. 2006).

Table 1.2 reports the summary statistics of the final sample (See Appendix A for variable descriptions). The mean of *Accuracy* and *Accuracy_S* is 3.12 and 6.30, respectively. The mean and median of *Attention* are 2.89 and 3, respectively. This suggests that a typical analyst revises about three times during a fiscal year for each firm she follows. The average years of firm-specific experience is 3.38, while the median is 2. The mean and median numbers of firms an analyst follows are about 16.58 and 16, while the average number of industries an analyst would follow in a given year is 3.54 and the median is 3, suggesting that a typical analyst follows firms in more than two industries. This, together with the fact that the average number of firms an analyst follows is 17, implies that the analysts in the sample are likely to have a specialization in at least one of the industries. In fact, the mean of *Specialization* is 60%, while the median is 1. Finally, Table 1.3 presents the Pearson correlations between the variables.

1.6 Empirical Design and Results

1.6.1 Effect of Volatility on Analyst Attention

To test the first hypothesis (*H1a*), I use the following specification:

$$Attention_{ijt} = \beta_0 + \beta_1 Log(Volatility_{it}) + \beta_2 ForecastAge_{ijt} + \delta' Controls_{jt} + \theta_{it} + \lambda_{dt} + \varepsilon_{ijt},$$

where $Attention_{ijt}$ is the number of revisions analyst i issues when forecasting earnings of firm j during fiscal year t, $Volatility_{it}$ is the volatility of returns on assets of firm j calculated using data from the previous 20 quarters ending in the fourth quarter of fiscal year t, $ForecastAge_{ijt}$ is the number of days between the earnings announcement date and the date on which the most recent forecast is issued, θ_{it} are the analyst-year fixed effects, and λ_{dt} are the industry-year fixed effects. I perform a log transformation on $Volatility_{it}$ in the regression to have a direct mapping from the closed form relationship between $Volatility_{it}$ and $Attention_{ijt}$ in the analytical model to the data.

Firm-year fixed effects are not used because $Volatility_{it}$ is a firm-year level variable; hence, β_1 cannot be estimated if they are included in the regression. Instead, industry-year fixed effects are used to control for any industry trends and characteristics that may be correlated with both earnings volatility and analyst attention. However, without firm-year fixed effects, β_1 may still be subject to omitted variable bias. For example, large firms may attract more attention, but their earnings are likely to be less volatile. Young firms may attract less attention and may have less volatile earnings. If these factors are not controlled for, β_1 will either underestimate or overestimate the true effect of earnings volatility on attention. Therefore, I include in $Controls_{jt}$ variables capturing various firms' financial characteristics, the number of analysts following, the number of management guidance reports, and an indicator for CEO change.

Table 1.4 presents the results. All three specifications include *ForecastAge*, analyst-year fixed effects, and industry-year fixed effects. Column (1) shows the result when no additional controls are included. To account for the potential factors not captured by the fixed effects, columns (2) and (3) control for *NumAnalyst, Lev, Size, Loss, ROA, BM, Growth, OCF, FirmAge, CEOSwitch, NumGuid_Ann, and NumGuid_Qtr* (See Appendix A for variable description). Standard errors are either clustered at the analyst level or double-clustered at the analyst level and firm level to allow for possible correlation among forecasts made by the same analyst and forecasts made for the same firm.

Consistent with my prediction, the coefficient of *Log(Volatility)* is positive and statistically significant. This indicates that conditional on the analyst's own ability and her firm portfolio (analyst-year fixed effects), firm characteristics, and the timing of the last forecast, the analyst on average allocates more attention to the firms with a higher level of earnings volatility. The economic magnitude is also not trivial, as a 100% increase in volatility (e.g., from the median of volatility to the 75th percentile) is associated with an increase of 5.2 in the number of forecast revisions on average.

1.6.2 Effect of Marginal Attention Cost on Analyst Attention

To test the second hypothesis (*H1b*), I use the following specification:

$$Attention_{ijt} = \beta_0 + \beta_1 FirmExp_{ijt} + \beta_2 Specialization_{ijt} + \beta_3 ForecastAge_{ijt} + \theta_{it} + \gamma_{jt} + \varepsilon_{ijt},$$

where $FirmExp_{ijt}$ is the firm-specification experience—the number of years analyst i has followed firm j up to year t, $Specialization_{ijt}$ is an indicator variable if analyst i has an industry specialization forecasting firm j, θ_{it} are the analyst-year fixed effects, and γ_{jt} are the firm-year fixed effects.

Here, β_1 and β_2 are expected to be positive to indicate that analysts pay more attention when the marginal cost of attention is lower. As discussed in 4.1, the inclusion of analyst-year and firm-year fixed effects not only complements the attention measure, but also eliminates alternative explanations pertaining to trends and characteristics at the analyst and firm level.

Table 1.5 presents the results of the regressions. All specifications include *ForecastAge*, analyst-year fixed effects, and industry-year fixed effects. Columns (1) and (2) test the effect of *FirmExp* on *Attention*, while columns (3) and (4) test the effect of *Specialization* on *Attention.* The last two columns (5) and (6) include both marginal attention cost proxies. Standard errors are clustered at the analyst level in the first column of each pair, and doubled clustered at the analyst level and firm level in the second column.

Consistent with the prediction, the coefficients on *FirmExp* and *Specialization* are positive and statistically significant, indicating that analysts pay more attention when their marginal attention cost is low. The coefficients for both are smaller when both are included in the regression, because analysts who cover a firm for many years are likely to have a specialization in that firm's industry. Thus, ordinary least squares would overestimate the effect of each variable when the other is not included. Holding everything else constant, one additional year of following the firm increases attention by 0.1, while having an industry specialization increases attention by 0.153 units.

1.6.3 Effect of Volatility on Forecast Accuracy

To test the third hypothesis (*H2a*), I use the following specification:

$$Accuracy_{ijt} = \beta_0 + \beta_1 Log(Volatility_{it}) + \beta_2 ForecastAge_{ijt} + \delta' Controls_{jt} + \theta_{it} + \lambda_{dt} + \varepsilon_{ijt},$$

where $Accuracy_{ijt}$ is the accuracy of the most recent forecast analyst i issues for firm j in year t. Similar to 6.1, industry-year fixed effects are used instead of firm-year fixed effects so that β_1 can be estimated. Consequently, $Controls_{jt}$ is included to control for potential omitted variable bias coming from firm characteristics similar to those used in section 1.6.1.

Table 1.6 summarizes the relationship between volatility and analyst forecast accuracy. All specifications include *ForecastAge*, analyst-year fixed effects, industry-year fixed effects, and additional firm-level controls (See Appendix A for the list of firm-level control variables). Columns (2) and (4) also control for *FirmExp* and *Specialization*. Standard errors are double clustered at both the analyst level and firm level in all regressions.

The coefficient on *Log(Volatility)* is negative and statistically significant at a 1% confidence level, consistent with my prediction. The coefficient is also economically significant because a 1% increase in volatility is associated with roughly an 18.4% decrease in forecast accuracy on average.

1.6.4 Effect of Attention on Forecast Accuracy

To test the fourth hypothesis (*H2b*), I use the following specification:

$$Accuracy_{ijt} = \beta_0 + \beta_1 Attention_{ijt} + \beta_2 FirmExp_{ijt} + \beta_3 Specialization_{ijt}$$
$$+ \beta_4 ForecastAge_{ijt} + \theta_{it} + \gamma_{jt} + \varepsilon_{ijt}.$$

β_1 is the coefficient of interest and is expected to be positive. Again, the inclusion of analyst-year θ_{it} and firm-year γ_{jt} fixed effects not only complements the attention measure, but also eliminates alternative explanations pertaining to year-varying characteristics (trends) at the analyst and firm level.

Table 1.7 shows the results from estimating the above equation: the effect of attention on forecast accuracy. The dependent variables are *Accuracy* and *Accuracy_S*. The independent variable of interest is *Attention*. All specifications control for *ForecastAge*, analyst-year fixed effects, and firm-year fixed effects. Columns (2) and (4) include FirmExp and Specialization as additional controls. Standard errors are double-clustered at the analyst and the firm level.

The coefficients of *Attention* across all specifications are positive and statistically significant. This is consistent with my prediction that the more attention an analyst pays, the more accurate her forecast is on average. The coefficient decreases slightly after controlling for *FirmExp* and *Specialization* (columns (2) and (4)). This is expected, as these factors positively correlate with both attention and forecast accuracy; hence, omitting them would inflate the effect of attention.

To allow for comparison among the effects, all right-hand side variables in Table 1.7 are deflated by their corresponding standard deviation. In this context, one standard deviation increase in *Attention* increases forecast accuracy by approximately 4.6%. This is significantly larger than the effect of FirmExp and Specialization, where one standard deviation increase in each variable respectively increases forecast accuracy by roughly only 0.5%. The results suggest that attention is an important factor affecting analyst forecast accuracy.

1.7 Additional Analyses

1.7.1 Heterogeneous Effects of Attention on Forecast Accuracy

Having established the impact of attention on accuracy, I extend my analysis to investigate whether the positive effect of attention on forecast accuracy is either enhanced or attenuated with the three attention determinants. To address this question, I estimate the following the following regression equation:

$$\begin{aligned} Accuracy_{ijt} = {} & \beta_0 + \beta_1 Attention_{ijt} + \beta_1 Attention_{ijt} \times X_{ijt} + \beta_3 FirmExp_{ijt} \\ & + \beta_4 Specialization_{ijt} + \beta_5 ForecastAge_{ijt} + \theta_{it} + \gamma_{jt} + \varepsilon_{ijt,} \end{aligned}$$

where X_{ijt} is either $Log(Volatility_{it})$, $FirmExp_{ijt}$, or $Specialization_{ijt}$.

First, by interacting *Log(Volatility)* with Attention, I examine if the marginal effect of attention on accuracy is larger for highly volatile firms. Though this effect is not explicitly investigated in the stylized analytical model, I hypothesize that the marginal benefit of paying attention would be smaller when earnings are stable, because it is easier to accurately predict the current year's earnings based on past earnings. On the other hand, the marginal benefit of paying attention would be higher when earnings are more volatile. Consistent with this prediction, columns (1) and (4) of Table 1.8 show that the coefficients of the interaction term are positive and statistically significant. This suggests that attention matters more when volatility is high.

However, I expect that the attention effect would be attenuated with firm-specific experience and specialization. The intuition is that having followed a firm in the past or having an industry specialization likely allows the analyst to read and process the information of a particular firm faster and more efficiently. Thus, the marginal benefit of paying more attention in such cases would be smaller.

Table 1.8 shows that, consistent with my prediction, the coefficients of the two interaction terms are both negative and statistically significant. This suggests that having expertise in forecasting a specific firm or an industry specialization attenuates the positive effect of attention on forecast accuracy, even when controlling for analysts' ability via analyst-year fixed effects. However, the attenuation effects are rather small in magnitude: a 0.05% decrease in accuracy per one additional year of firm specific experience and a 0.06% decrease when the analyst has an industry specialization. This suggests that attention is an important factor affecting forecast accuracy.

1.7.2 Analyst Attention and Revising Behaviors

Prior research has found that analysts are sometimes strategically optimistic in their forecasts. For example, this arises when analysts face pressure from brokerage firms to bias their forecasts optimistically to maintain relationships with management because that allows these financial institutions to provide lucrative underwriting or public offering services (Lin and MacNichols 1998; Michaely and Womack 1999; Dechow et al. 2000; Lin et al. 2005). If the objective function of the analyst is to issue optimistic forecasts (as opposed to minimizing forecast error, as my analytical model assumes), then the analyst will pay more attention to choose an optimal amount of positive bias in the forecasts.

To show that this is not the case, I investigate whether attentive analysts are less optimistic. I consider three different measures to capture an analyst's optimism: *NetUpwardRev* (the number of upward revisions minus the number of downward revisions), *LastFirstDiff* (the signed difference between the value of the analyst's last forecast and her first forecast), *Bias* (the difference between the analyst's last forecast and consensus forecast scaled by the standard deviation of all forecasts made for the same firm). The first measure reflects the analyst's tendency to revise upward or downward on average. The second measure shows how conservative the analyst is against her initial forecast, and the third measure shows how optimistic or pessimistic she is compared to her peers. The regression model is

$$Y_{ijt} = \beta_0 + \beta_1 Attention_{ijt} + \beta_2 FirmExp_{ijt} + \beta_3 Specialization_{ijt}$$
$$+ \beta_4 ForecastAge_{ijt} + \theta_{it} + \gamma_{jt} + \varepsilon_{ijt,}$$

where Y_{ijt} is one of the three measures above.

Table 1.9 shows the results of these tests. The coefficients of *Attention* are negative and statistically significant at a 1% confidence level across all specifications. This implies that attentive analysts, on average, are less optimistic in their forecasts than their peers.

8 Conclusion

I investigate how analysts allocate attention level across the firms they follow and whether attention level affects their forecasting behavior, namely earnings forecast accuracy and analyst optimism. Consistent with the prediction from rational inattention theory, when an agent possesses a limited attention span and it is costly to pay attention, my results suggest that analysts suffer from an attention constraint that in turn influences their forecasting ability.

Specifically, using the number of forecast revisions as the baseline measure of analyst attention level augmented by the timing of the most recent forecast, firm-year and analyst-year fixed effects, I find that analysts tend to pay more attention to highly volatile firms and firms they have more experience or specialization following. Furthermore, attention has a positive impact on forecast accuracy, and this effect is stronger when analysts have less experience or specialization following a firm. I also find that although earnings volatility is negatively related to forecast accuracy, the marginal benefit of paying attention is higher when analysts are forecasting the earnings of volatile firms. Finally, I show that attentive analysts are more likely to issue downward revisions and are less optimistic compared to their peers.

Exploiting variation within firm-year and analyst-year allows me to construct a measure of attention that could rule out several alternative explanations. First, analysts may simply revise more when firms release many voluntary disclosures; if such disclosures are of high quality, analysts' revised forecasts would be more accurate by default, irrespective of the attention they pay. Firm-year fixed effects would take into account this situation by controlling for any changes in each firm's information environment that are observable to all analysts. Second, analyst-year fixed effects control for analyst time-varying analyst characteristics pertaining to each analyst's workload and ability, both of which could drive the positive relationship between attention and accuracy.

This paper has important implications for investors, firms, and brokerages. First, my results suggest that investors should pay more attention to attentive analysts if they want to access the most accurate forecast before making investment decisions. Second, my findings imply that it is not just analyst coverage that matters; it is attentive coverage that reduces the information asymmetry between firms and investors. Finally, brokerage firms may allocate attentive analysts to follow difficult to forecast firms when accuracy is a priority.

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Appendix 1A	
Variable Definitions	
Main Variables	
$Attention_{ijt}$	The number of EPS forecast revisions analyst i made for firm j in fiscal year t.
$Volatility_{jt}$	ROA volatility of firm j as of year t. It is calculated as the volatility of ROA in past 20 quarters with the condition that there are at least 8 non-missing observations.
$Accuracy_{ijt}$	The log accuracy of analyst i's last forecast for firm j in year t. It is calculated as $-\log(\lvert Forecast_{ijt} - Actual_{jt}\rvert / \lvert Actual_{jt}\rvert)$.
$Accuracy\_S_{ijt}$	Alternative measure of forecast accuracy commonly used in the analyst literature. It is calculated as $-\log(\lvert Forecast_{ijt} - Actual_{jt}\rvert / \lvert StockPrice_{jt}\rvert)$, where $StockPrice_{jt}$ is the stock price of firm j at the end of the fiscal year t.
$FirmExp_{ijt}$	The first proxy for marginal attention cost—firm specific experience, measured as the number of years analyst i has been following firm j up to year t.
$Specialization_{ijt}$	The second proxy for marginal attention cost—indicator equals 1 if analyst i has an industry specialization in forecasting firm j in year t, and 0 otherwise. An analyst is specialized in industry d during year t if she follows at least 5 firms in industry d.
$ForecastAge_{ijt}$	The number of days between when analyst i's most recent forecast for firm j in year t was issued and the earnings announcement date.
Control Variables (*$Controls_{jt}$*)	
$NumAnalyst_{jt}$	The number of analysts following firm j during year t.
Lev_{jt}	Firm j's leverage in year t (Total liability/Total assets).
$Size_{jt}$	Firm j's size in year t (Log(Total assets)).
$Loss_{jt}$	Indicator variable equals 1 if firm j incurs a net loss in year t, and 0 otherwise.
ROA_{jt}	Firm j's return on assets in year t (Net income/Total assets).
BM_{jt}	Firm j's book to market ratio in year t (seq/(prcc_f × csho)).
$Growth_{jt}$	Firm j's growth rate in year t ((sale – lagsale)/lagsale).
OCF_{jt}	Firm j's operating cash flow in year t scaled by total assets.
$FirmAge_{jt}$	Firm j's age as of year t.

$CEOSwitch_{jt}$	Indicator variable equals 1 if firm j has a CEO switch during year t.
$NumGuid\_Ann_{jt}$	Number of annual guidance reports from firm j in year t.
$NumGuid\_Qtr_{jt}$	Number of quarterly guidance reports from firm j in year t.
Other Dependent Variables	
$NetUpwardRev_{ijt}$	Number of upward revisions minus number of downward revisions from analyst i forecasting firm j in year t.
$LastFirstDiff_{ijt}$	The value of analyst i's last forecast minus the value of her first forecast for firm j in year t.
$Bias_{ijt}$	Analyst i's bias against consensus forecast for firm j in year t. It is calculated as $(Forecast_{ijt} - Consensus_{jt})/Dispersion_{jt}$ where $Consensus_{jt}$ is the most recent consensus forecast and $Dispersion_{jt}$ is the standard deviation of all individual most recent forecasts made for firm j in year t.
Analyst-Year Variables for Data Description Purposes	
$NumFirm_{it}$	The number of firms analyst i follows during year t.
$NumInd_{it}$	The number of industries analyst i follows during year t.

Appendix 1B – Analytical Model

In this appendix, I outline a stylized analytical model of an analyst forecasting earnings of a firm and incorporate the theory of rational inattention to derive my predictions. Subsection 1.B.1 provides a brief overview of limited attention modeling. Subsection 1.B.2 outlines the main model and subsection 1.B.3 discusses the theoretical predictions.

1.B.1 Modeling Limited Attention

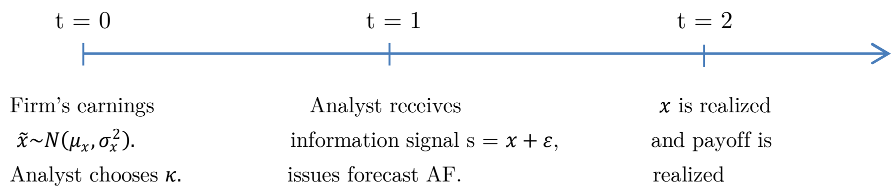
Sims (1998, 2003) proposes modeling attention as information flow and limited attention as a bound on the information flow. He suggests that, based on a large literature on information theory, information is quantified as how much uncertainty it can resolve, and the uncertainty of a random variable is its entropy. Suppose $x \sim \mathrm{N}(\mu_x, \sigma_x^2)$ is a variable the agent is interested in, and she receives an information signal $s = x + \varepsilon$, where $\varepsilon \sim \mathrm{N}(0, \sigma_\varepsilon^2)$ is assumed to be independent of x. The entropy of x is $\mathrm{H}(x) = \frac{1}{2}\log_2(2\pi e\sigma_x^2)$, and the entropy of $x|s$ is $\mathrm{H}(x|s) = \frac{1}{2}\log_2(2\pi e\sigma_{x|s}^2)$. The limited attention constraint says that the reduction in uncertainty about x after receiving signal s is bounded by the amount of attention κ paid. Formally, the constraint is expressed as follows $\mathrm{H}(x) - \mathrm{H}(x|s) \leq \kappa$, or equivalently:

$$\frac{\sigma_x^2}{\sigma_\varepsilon^2} \leq 2^{2\kappa} - 1. \qquad (*)$$

The noise in the signal is interpreted as arising from the agent's own nervous system. If the agent receives signal s and pays κ amount of attention, she in fact creates a bound for the noise variance.

1.B.2 Model

To generate the predictions, I consider a stylized model that can provide key insights into the relationship between attention, a firm's characteristics, and forecast accuracy. I examine a single-period model of an analyst forecasting a firm's reported earnings. The following timeline shows the sequence of events.



The firm's technology generates stochastic unmanaged earnings $x \sim N(\mu_x, \sigma_x^2)$. The realized value of the unmanaged earnings will be reported truthfully by the manager at the end of the period. The analyst is endowed with κ_{max} amount of attention. Then, she receives an information signal about the firm's earnings, $s = x + \varepsilon$ where $\varepsilon \sim N(0, \sigma_\varepsilon^2)$. The noise ε is assumed to be independent of x. The analyst is subject to a limited attention constraint. As shown in section 1.B.1, if she pays κ amount of attention where $0 \leq \kappa \leq \kappa_{max}$, then the attention constraint is $(*)$. The intuition of $(*)$ is that the amount of uncertainty regarding earnings reduces after the analyst receives the signal, but this reduction depends on the analyst's level of attention. Mathematically, the reduction in uncertainty cannot exceed κ.

The cost of paying attention is $\mu\kappa$, where $\mu > 0$ is the marginal of attention. This cost can be interpreted as the opportunity cost of paying one unit of attention on forecasting x. Because I abstract away from the multiple firm setting, the exogenously given μ also encompasses the benefit and the cost of paying attention to other activities, such as forecasting other firms and leisure.

The remaining amount of attention $\kappa_{max} - \kappa$ can be interpreted as the analyst's attention devoted to other activities not explicitly investigated in the model.

The analyst's objective is to minimize her forecast error as well as her attention cost. This assumption is supported by prior research on an analyst's objective function (e.g., Beyer 2008). Moreover, empirical evidence supports the assertion that analysts who issue more precise forecasts enjoy higher compensation and better reputations (Stickel 1992, 1998; Mikhail et al. 1999; Jackson 2005; Fang and Yasuda 2008).[3] Under this setup, I can define the equilibrium.

Definition 1 An equilibrium consists of the analyst's forecasting rule, *AF(.)*, and attention $\kappa(.)$ such that:

(i) At t = 1, given attention level κ, the forecasting rule $AF(\kappa, s)$ minimizes

$$\min_{\mathrm{AF}} \mathrm{E}\left[(x - AF)^2 + \mu\kappa \mid s\right]$$

$$s.t. \quad \frac{\sigma_x^2}{\sigma_\varepsilon^2} \leq 2^{2\kappa} - 1 \text{ and } s = x + \varepsilon.$$

(ii) At t = 0, given forecasting rule $AF(\kappa, s)$, the optimal attention level $\kappa(\sigma_x^2, \mu)$ minimizes

$$\min_{0 \leq \kappa \leq \kappa_{max}} \mathrm{E}\left[(x - AF)^2 + \mu\kappa\right]$$

$$s.t. \quad \frac{\sigma_x^2}{\sigma_\varepsilon^2} \leq 2^{2\kappa} - 1 \text{ and } s = x + \varepsilon.$$

[3] I abstract away from other incentives that an analyst might face. For example, Hayes (1998) and Jackson (2005) suggest that analyst incentive could be based on stock recommendations or trading commissions. Other incentives are ones based on selection bias (McNichols and O'Brien 1997), access to management (Lim 2001), career concerns (Hong and Kubik 2003), and underwriting business (Dugar and Nathan 1995; Lin and McNichols 1998; Michaely and Womack 1999). Because the focus of my study is the role of attention on forecast accuracy, by limiting the objective function to minimizing forecast error (and attention cost), I can have the simplest model that generates predictions about the relationships between the variables of interest.

A straightforward application of the first order conditions and properties of normal distribution gives a closed form solution of the unique equilibrium of the model, stated in Proposition 1.

Proposition 1 There exists an equilibrium defined as follows:

(i) The analyst's optimal forecasting rule given signal s and attention κ is

$$AF(\kappa,\ s) = \mathrm{E}[x|s] = \ 2^{-2\kappa}\mu_x + (1-2^{-2\kappa})s.$$

(ii) The analyst's optimal level of attention in $[0,\ \kappa_{\max}]$ is

$$\kappa(\sigma_x^2,\ \mu) = -\frac{1}{2}\log_2(\mu) + \frac{1}{2}\log_2(2\sigma_x^2 \ln 2)\ .$$

Proposition 2 Suppose κ is exogenously given, with the firm's earnings volatility σ_x^2l; the expected analyst forecast accuracy is

$$\text{Accuracy} = -\,\mathrm{E}[|AF(s,\ \kappa) - x|] = -\sqrt{\frac{2^{1-2\kappa}\sigma_x^2}{\pi}}.$$

1.B.3 Theoretical Predictions

Performing comparative statics on the optimal level of attention and expected forecast accuracy, I derive the following theoretical predictions:

Corollary 1

(i) From Proposition 1, the optimal level of attention κ^* is decreasing in the marginal cost of attention μ and increasing in the firm's earnings volatility σ_x^2.

(ii) From Proposition 2, the expected analyst forecast accuracy is decreasing in the firm's earnings volatility σ_x^2 and increasing in the level of attention κ.

1.B.4 Proofs

1.B.4.1 *Proof of Proposition 1*

(i) In equilibrium, the attention constraint binds: $\frac{\sigma_x^2}{\sigma_\varepsilon^2} = 2^{2\kappa} - 1$. The Lagrange function is

$$L = E[(x - AF)^2 + \mu\kappa \ |s] + \lambda\left(2^{2\kappa} - 1 - \frac{\sigma_x^2}{\sigma_\varepsilon^2}\right)$$

The first order condition is

$$0 = \frac{dL}{dAF} = \frac{d}{dAF}\int_{-\infty}^{\infty}\left((x - AF)^2 + \mu\kappa\right) f_{x|s}(x)\, dx = \int_{-\infty}^{\infty} -2(x - AF)\, f_{x|s}(x)\, dx$$

Thus, $\displaystyle\int_{-\infty}^{\infty} AF f_{x|s}(x)\, dx = \int_{-\infty}^{\infty} x\, f_{x|s}(x)\, dx \Leftrightarrow AF \int_{-\infty}^{\infty} f_{x|s}(x)\, dx = E[x|s] \Leftrightarrow AF = E[x|s]$.

Because $x \sim N(\mu_x, \sigma_x^2)$, $s \sim N(\mu_x, \sigma_\varepsilon^2)$, and $\frac{\sigma_x^2}{\sigma_\varepsilon^2} = 2^{2\kappa} - 1$, $AF = E[x|s] = 2^{-2\kappa}\mu_x + (1 - 2^{-2\kappa})s$.

(ii) Let $\alpha = 2^{-2\kappa}$, a strictly decreasing function in κ. The optimization problem for the optimal κ becomes

$$\min_{\alpha} \ E\left[(x - AF(\alpha, s))^2 - \frac{\mu \log_2 \alpha}{2}\right]$$

$$\text{s.t.} \quad \frac{\sigma_x^2}{\sigma_\varepsilon^2} \le \frac{1}{\alpha} - 1$$

In equilibrium, the constraint binds; thus, $\sigma_\varepsilon^2 = \frac{\alpha\sigma_x^2}{1-\alpha}$. On the other hand,

$$E[(x - AF(\alpha, s))^2 - \frac{\mu \log_2 \alpha}{2}] = E(x - AF(\alpha, s))^2 - \frac{\mu \log_2 \alpha}{2}$$

$$= E\left[\left(x - \alpha\mu_x - (1-\alpha)s\right)^2\right] - \frac{\mu \log_2 \alpha}{2}$$

$$= E[x^2 + \alpha^2\mu_x^2 + (1-\alpha)^2 s^2 - 2x\alpha\mu_x - 2x(1-\alpha)s + 2\alpha\mu_x(1-\alpha)s] - \frac{\mu \log_2 \alpha}{2}$$

$$= E[x^2] + \alpha^2\mu_x^2 + (1-\alpha)^2 E[s^2] - 2E[x]\alpha\mu_x - 2(1-\alpha)E[xs] + 2\alpha\mu_x(1-\alpha)E[s] - \frac{\mu \log_2 \alpha}{2}$$

$$= \alpha\sigma_x^2 - \frac{\mu \log_2 \alpha}{2}.$$

Where the last equality comes from the fact that E$[x^2]$, E$[x]$, E$[s]$, E$[s^2]$, E$[xs]$, E$[\varepsilon]$, and E$[\varepsilon^2]$ are known because $x \sim N(\mu_x, \sigma_x^2)$, $\varepsilon \sim N(0, \sigma_\varepsilon^2)$, $s = x + \varepsilon$, and x and ε are independent.

A straightforward calculation from the FOC gives the equilibrium level of attention:

$$\alpha^* = 2^{-2\kappa^*} = \frac{\mu}{2\ \sigma_x^2 ln2}, \text{ or equivalently, } \kappa^* = -\frac{1}{2} log_2\left(\frac{\mu}{2\ \sigma_x^2 ln2}\right).$$

1.B.4.2 *Proof of Proposition 2*

Under the assumption that κ is exogenously given, then part (i) of Proposition 1 gives us

$$AF = 2^{-2\kappa}\mu_x + (1 - 2^{-2\kappa})s \sim N(\mu_x, (1 - 2^{1-2\kappa})\sigma_x^2)$$

Thus, using properties of variance and covariance, we have

$$x - AF \sim N(0, 2^{-2\kappa}\sigma_x^2)$$

Hence, $|x - AF|$ follows a half normal distribution where E$[|x - AF|] = \sqrt{\frac{2^{1-2\kappa}\sigma_x^2}{\pi}}$. Thus, accuracy is

$$Accuracy = -\text{E}[|\text{x} - \text{AF}|] = -\sqrt{\frac{2^{1-2\kappa}\sigma_x^2}{\pi}}$$

1.B.4.3 *Proof of Corollary 1*

(i) Given the optimal level of attention $\kappa,$ we have

$$\frac{\partial\kappa}{\partial\mu} = -\frac{1}{2\mu ln2} < 0 \text{ and } \frac{\partial\kappa}{\partial\sigma_x^2} = \frac{1}{2\sigma_x^2 ln2} > 0\ .$$

(ii) Given the formula for Accuracy, we have

$$\frac{\partial Accuracy}{\partial\sigma_x^2} = -\frac{1}{2\sqrt{\pi}}\frac{2^{2-2\kappa}(ln2)\sigma_x^2}{\sqrt{2^{1-2\kappa}\sigma_x^2}} < 0 \text{ and } \frac{\partial Accuracy}{\partial\kappa} = \frac{1}{2\sqrt{\pi}}\frac{2^{1-2\kappa}}{\sqrt{2^{1-2\kappa}\sigma_x^2}} > 0.$$

Figure 1.1a. Attention and Forecast Age

This figure shows how controlling for the second ingredient, Forecast Age, which is the number of days between the earnings announcement date and the date on which the last forecast is issued, can further highlight the attention aspect of the baseline measure—the number of forecast revisions. It allows me to take into account cases where an analyst may revise less often but pay a lot of attention. If that is the case, the timing of her most recent forecast would be near the end of the fiscal year. Controlling for this timing aspect allows us to compare analysts whose most recent forecasts are on the same date. If analyst 1 revises more, then she pays more attention to forecasting firm i.

Figure 1.1b. Attention and Analyst-Year Fixed Effects

This figure shows how controlling for the third ingredient, Analyst-Year fixed effects, can further highlight the attention aspect of the baseline measure—the number of forecast revisions. The fixed effects allow us to look at forecasts made by one analyst during a year at a time. Thus, the ability and the overall job complexity of the analyst during a year are the same for all forecasts. If analyst j revises more for firm 1, then she pays more attention to forecasting firm 1.

Figure 1.1c. Attention and Firm-Year Fixed Effects

This figure shows how controlling for the fourth ingredient, Firm-Year fixed effects, can further highlight the attention aspect of the baseline measure—the number of forecast revisions. The fixed effects allow me to compare forecasts made by different analysts for the same firm in the same year. Thus, all of these analysts are subject to the same information environment of the firm. If analyst 1 revises more for firm i, then she pays more attention to forecasting firm i.

Figure 1.2a. Attention Allocation Across Firms

This figure shows one instance of attention allocation of a particular analyst in the fiscal year 2010. The x-axis refers to the firm IDs. This analyst follows 10 firms in 2010. The blue bars show the "raw" number of forecast revisions issued for each firm in the analyst's portfolio in 2010. The green bars show the corresponding (implicit) attention levels, which are the residuals of the regression of the number of revisions on forecast age, analyst-year fixed effects, and firm-year fixed effects.

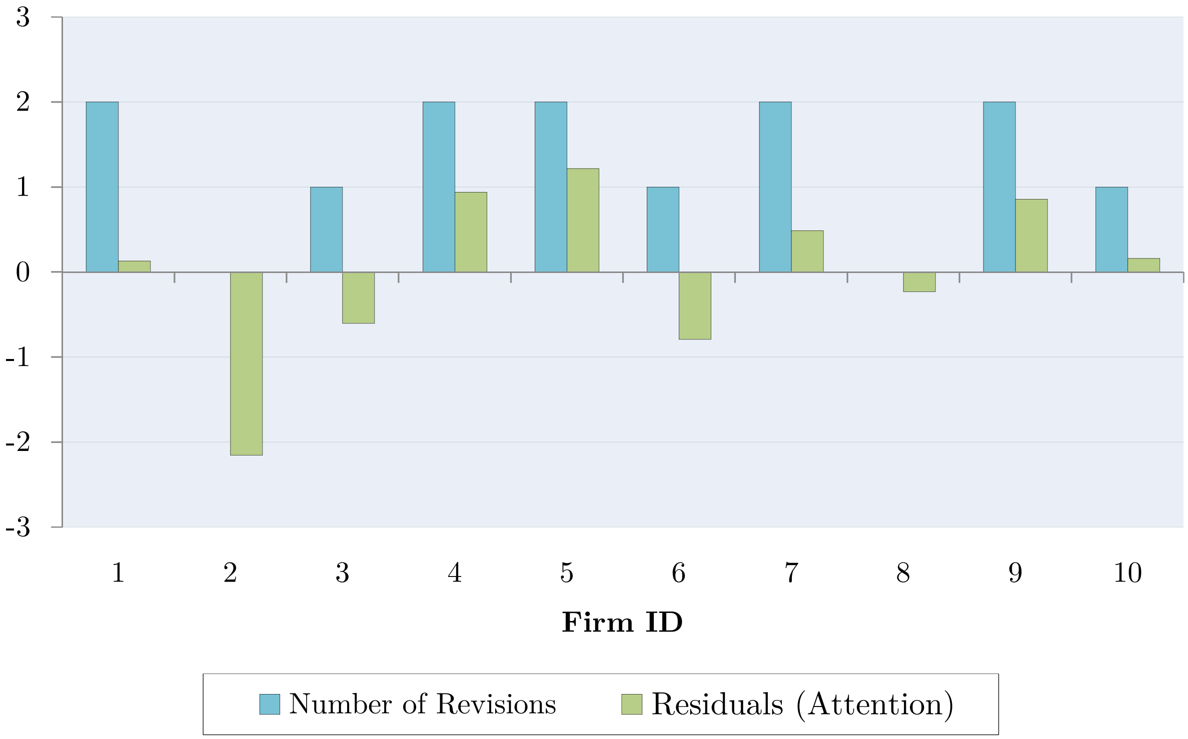


Figure 1.2b. Attention Level Over Time

This figure shows the attention levels that a particular analyst allocates to a particular firm over time. The x-axis refers to time measured in years. This analyst follows this firm continuously from 2009 to 2014. The blue bars show the "raw" numbers of forecast revisions issued for this firm in different years. The green bars show the corresponding (implicit) attention levels, which are the residuals of the regression of the number of revisions on forecast age, analyst-year fixed effects, and firm-year fixed effects.

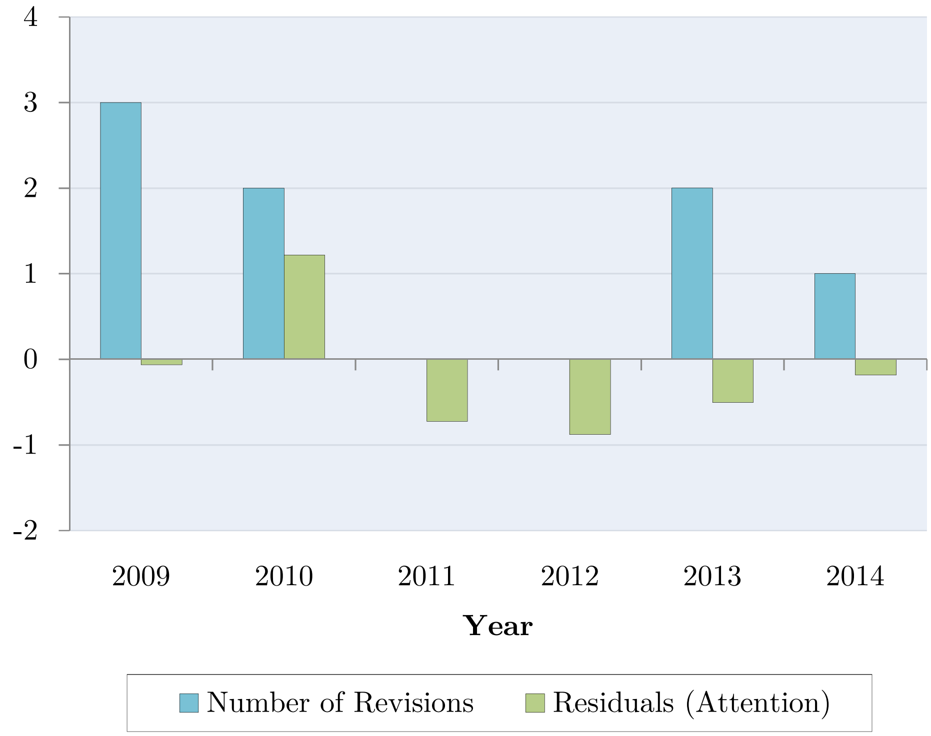


Table 1.1. Sample Selection

Panel A. Initial Sample and Selection Process

	No. of observations
Initial I/B/E/S Dataset *(1981 – 2015)*	4,301,738
- Drop groups of analysts.	(706,233)
- Drop analysts who first appear in 1981 and 1982 to mitigate left-censoring issue with I/B/E/S data.	(84,591)
- Merge with Compustat.	(958,906)
- Drop missing data.	(225,361)
- Keep only most recent forecasts.	(1,729,033)
- Merge with I/B/E/S Guidance.	(90,818)
- Merge with ExecuComp.	(169,172)
Final Sample:	337,624

Panel B. Final Sample's Characteristics (1992 - 2015)

	No. of analysts	No. of firms	No. of brokers	No. forecasts
Final Sample	9,748	3,150	769	337,624

Table 1.2. Summary Statistics

This table presents summary statistics of the variables used in the paper. The summary statistics are computed using the Final Sample (see Table 1.1). Variable definitions appear in Appendix 1A.

	Mean	Median	P25	P75	SD
Accuracy	3.124	3.248	2.218	4.201	1.525
Accuracy_S	6.302	6.362	5.274	7.412	1.618
Log(Volatility)	-4.481	-4.451	-5.120	-3.757	1.130
Attention	2.894	3.000	1.000	4.000	2.229
ForecastAge	139.866	105.000	93.000	170.000	76.904
FirmExp	3.382	2.000	1.000	5.000	3.457
NumFirm	16.575	16.000	11.000	21.000	7.755
NumInd	3.544	3.000	2.000	5.000	2.358
Specialization	0.599	1.000	0.000	1.000	0.490
NumAnalyst	17.331	16.000	10.000	23.000	9.624
Lev	0.562	0.559	0.402	0.712	0.237
Size	8.258	8.221	7.053	9.502	1.618
Loss	0.135	0.000	0.000	0.000	0.342
ROA	0.045	0.049	0.015	0.089	0.127
BM	-0.781	0.413	0.254	0.633	171.873
Growth	0.121	0.078	0.000	0.183	0.555
OCF	0.105	0.101	0.056	0.151	0.087
FirmAge	25.124	22.000	13.000	37.000	13.683
CEOSwitch	0.107	0.000	0.000	0.000	0.309
NumGuid_Ann	1.904	0.000	0.000	4.000	3.106
NumGuid_Qtr	1.459	0.000	0.000	2.000	2.680
NetUpwardRev	-0.980	-1.000	-2.000	0.000	2.159
LastFirstDiff	-0.061	0.000	-0.120	0.080	0.475
Bias	0.052	0.000	-0.520	0.523	1.659

Table 1.3. Correlation Table

This table presents the Pearson correlation matrix of the main variables used in the paper. For brevity, *NetUpwardRev*, *LastFirstDiff*, and *Bias* are not included in this table. Variable definitions appear in Appendix 1A.

		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
(1)	*Accuracy*	1.000									
(2)	*Accuracy_S*	0.840	1.000								
(3)	*Log(Volatility)*	-0.222	-0.167	1.000							
(4)	*Attention*	0.049	0.021	0.050	1.000						
(5)	*ForecastAge*	-0.221	-0.201	0.007	-0.500	1.000					
(6)	*FirmExp*	0.054	0.013	-0.058	0.164	0.000	1.000				
(7)	*NumFirm*	0.041	0.020	-0.100	0.082	-0.072	0.169	1.000			
(8)	*NumInd*	0.038	0.038	0.060	-0.040	0.006	0.053	0.370	1.000		
(9)	*Specialization*	0.008	-0.016	-0.057	0.128	-0.082	0.101	0.167	-0.364	1.000	
(10)	*NumAnalyst*	0.066	0.091	0.030	0.192	-0.087	0.073	-0.023	-0.141	0.110	1.000
(11)	*Lev*	-0.003	-0.127	-0.335	0.010	-0.020	0.081	0.087	-0.079	0.082	-0.038
(12)	*Size*	0.190	0.079	-0.401	0.127	-0.086	0.167	0.067	-0.158	0.116	0.517
(13)	*Loss*	-0.371	-0.323	0.223	0.044	0.027	-0.022	-0.033	-0.030	0.015	-0.033
(14)	*ROA*	0.249	0.268	-0.052	-0.009	-0.027	0.020	0.000	0.056	-0.028	0.045
(15)	*BM*	0.003	0.004	-0.004	0.001	-0.003	-0.002	0.000	-0.002	0.002	0.004
(16)	*Growth*	0.017	0.059	0.046	-0.013	-0.004	-0.047	-0.007	-0.024	0.011	0.010
(17)	*OCF*	0.215	0.260	0.113	0.043	-0.022	0.000	-0.032	0.022	-0.011	0.182
(18)	*FirmAge*	0.122	0.019	-0.176	0.038	-0.035	0.195	0.060	0.054	0.000	0.067
(19)	*CEOSwitch*	-0.047	-0.063	0.040	0.012	0.008	0.015	-0.010	-0.001	-0.002	0.033
(20)	*NumGuid_Ann*	0.208	0.121	-0.060	-0.045	0.004	0.048	-0.041	0.049	-0.026	0.009
(21)	*NumGuid_Qtr*	0.073	0.061	0.112	0.044	-0.007	-0.014	-0.081	0.023	-0.024	0.082

		(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)
(1)	*Accuracy*										
(2)	*Accuracy_S*										
(3)	*Log(Volatility)*										
(4)	*Attention*										
(5)	*ForecastAge*										
(6)	*FirmExp*										
(7)	*NumFirm*										
(8)	*NumInd*										
(9)	*Specialization*										
(10)	*NumAnalyst*										
(11)	*Lev*										
(12)	*Size*	1.000									
(13)	*Loss*	-0.132	1.000								
(14)	*ROA*	0.016	-0.508	1.000							
(15)	*BM*	0.000	-0.018	0.008	1.000						
(16)	*Growth*	-0.059	-0.038	0.040	0.000	1.000					
(17)	*OCF*	-0.093	-0.311	0.500	0.009	-0.002	1.000				
(18)	*FirmAge*	0.440	-0.091	0.046	0.001	-0.096	-0.056	1.000			
(19)	*CEOSwitch*	0.025	0.077	-0.061	-0.021	-0.027	-0.012	0.036	1.000		
(20)	*NumGuid_Ann*	0.124	-0.110	0.070	0.004	-0.023	0.046	0.129	0.003	1.000	
(21)	*NumGuid_Qtr*	-0.072	-0.008	0.011	0.004	-0.012	0.107	-0.063	0.016	0.328	1.000

Table 1.4. Effect of Earnings Volatility on Analyst Attention

This table presents the fixed effects OLS estimates of the effects of earnings volatility on analyst attention level. The independent variable is *Log*($Volatility_{jt}$), the log of the ROA volatility of firm *j* up to the end of year *t*. The dependent variable, $Attention_{ijt}$, is the number of forecast revisions analyst *i* makes for firm *j* during fiscal year *t*. All specifications control for *ForecastAge*, industry-year fixed effects and analyst-year fixed effects. Industry-year fixed effects are used instead of firm-year fixed effects because *Volatility* is a firm-year level variable. Because firm-year fixed effects are not used, additional firm-level controls are included: *NumAnalyst, Lev, Size, Loss, ROA, BM, Growth, OCF, FirmAge, CEOSwitch, NumGuid_Ann, and NumGuid_Qtr* (See Appendix A for variable description). Column (1) presents estimates when no controls are included. Columns (2) and (3) present estimates when controls are included. Standard errors are reported in parentheses and clustered at the analyst level in columns (1) and (2), double-clustered at the analyst and at the firm levels in column (3). *, **, *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	Dependent Variable: *Attention*		
	(1)	(2)	(3)
Log(Volatility)	0.0313***	0.0521***	0.0521***
	(0.0052)	(0.0052)	(0.010)
ForecastAge	-0.0109***	-0.0108***	-0.0108***
	(0.0001)	(0.0001)	(0.0001)
$Controls_{jt}$	No	Yes	Yes
Industry-Year FE	Yes	Yes	Yes
Analyst-Year FE	Yes	Yes	Yes
Clusters	Analyst	Analyst	Analyst, Firm
N	337,624	337,624	337,624
adj. R-sq	0.576	0.584	0.584

Table 1.5. Effect of Marginal attention cost on Analyst Attention

This table presents the fixed effects OLS estimates of the effects of marginal attention cost on analyst attention level. The independent variables of interest are $FirmExp_{ijt}$, firm-specific experience, and Specialization, the indicator for industry specialization. They are inversed proxies for marginal attention cost. The dependent variable, $Attention_{ijt}$, is the number of forecast revisions analyst i makes for firm j during fiscal year t. $Attention_{ijt}$ is augmented by controlling for *ForecastAge*, firm-year fixed effects, and analyst-year fixed effects. Columns (1) and (2) present estimates when only *FirmExp* is used. Columns (3) and (4) present estimates when only *Specialization* is used. Columns (5) and (6) present estimates when both marginal attention cost proxies are used. Standard errors are reported in parentheses and clustered at the analyst level in columns (1), (3), and (5), and double-clustered at the analyst and at the firm levels in columns (2), (4), and (6). *, **, *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	Dependent Variable: *Attention*					
	(1)	(2)	(3)	(4)	(5)	(6)
FirmExp	0.1010***	0.1010***			0.0997***	0.0997***
	(0.0023)	(0.0025)			(0.0023)	(0.0023)
Specialization			0.2105***	0.2105***	0.1531***	0.1531***
			(0.0149)	(0.0149)	(0.0145)	(0.0145)
ForecastAge	-0.0101***	-0.0101***	-0.0100***	-0.0100***	-0.0101***	-0.0101***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Firm-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Analyst-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Clusters	Analyst	Analyst, Firm	Analyst	Analyst, Firm	Analyst	Analyst, Firm
N	337,624	337,624	337,624	337,624	337,624	337,624
adj. R-sq	0.661	0.661	0.653	0.653	0.662	0.662

Table 1.6. Effect of Earnings Volatility on Forecast Accuracy

This table presents the fixed effects OLS estimates of the effects of earnings volatility on analyst forecast accuracy. The independent variable is $Log(Volatility_{jt})$, the log of the ROA volatility of firm *j* up to the end of year *t*. The dependent variables are *Accuracy* and *Accuracy_S*, both of which are measures of analyst forecast accuracy. All specifications control for *ForecastAge*, industry-year fixed effects and analyst-year fixed effects. Industry-year fixed effects are used instead of firm-year fixed effects because *Volatility* is a firm-year level variable. Because firm-year fixed effects are not used, additional firm-level controls are included: *NumAnalyst, Lev, Size, Loss, ROA, BM, Growth, OCF, FirmAge, CEOSwitch, NumGuid_Ann, and NumGuid_Qtr* (See Appendix A for variable description). Columns (1) and (3) present estimates when proxies for marginal attention cost are not included. Columns (2) and (4) present estimates when they are included. Standard errors are reported in parentheses and double-clustered at the analyst and at the firm levels in all specifications. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	DV: *Accuracy*		DV: *Accuracy_S*	
	(1)	(2)	(3)	(4)
Log(Volatility)	-0.184***	-0.184***	-0.250***	-0.250***
	(0.0125)	(0.0125)	(0.0153)	(0.0153)
ForecastAge	-0.0029***	-0.0029***	-0.0033***	-0.0033***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
FirmExp		0.0002		-0.0045**
		(0.0017)		(0.0020)
Specialization		-0.0263*		-0.0514**
		(0.0145)		(0.0231)
$Controls_{jt}$	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes
Analyst-Year FE	Yes	Yes	Yes	Yes
N	337,624	337,624	337,624	337,624
adj. R-sq	0.375	0.375	0.394	0.394

Table 1.7. Effect of Attention on Forecast Accuracy

This table presents the fixed effects OLS estimates of the effects of attention level on analyst forecast accuracy. The dependent variables of interest are *Accuracy* and *Accuracy_S*, both of which are measures of analyst forecast accuracy. The independent variable, $Attention_{ijt}$, is the number of forecast revisions analyst i makes for firm j during fiscal year t. $Attention_{ijt}$ is augmented by controlling for *ForecastAge*, firm-year fixed effects, and analyst-year fixed effects. Marginal attention cost proxies *FirmExp* and *Specialization* are included in Columns (2) and (4) as additional controls. All right-hand-side variables are scaled by their respective standard deviation to facilitate comparison among the magnitudes of each marginal effect. Standard errors are reported in parentheses and double-clustered at the analyst and at the firm levels in all specifications. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	DV: *Accuracy*		DV: *Accuracy_S*	
	(1)	(2)	(3)	(4)
Attention	0.0464***	0.0454***	0.0476***	0.0466***
	(0.0029)	(0.0029)	(0.0029)	(0.0029)
ForecastAge	-0.261***	-0.262***	-0.264***	-0.264***
	(0.0046)	(0.0046)	(0.0047)	(0.0047)
FirmExp		0.0056**		0.0055**
		(0.0024)		(0.0024)
Specialization		0.0049*		0.0050*
		(0.00265)		(0.00267)
Firm-Year FE	Yes	Yes	Yes	Yes
Analyst-Year FE	Yes	Yes	Yes	Yes
N	337,624	337,624	337,624	337,624
adj. R-sq	0.770	0.770	0.794	0.794

Table 1.8. Heterogeneous Effects of Attention on Forecast Accuracy

This table presents fixed effects OLS estimates of the heterogeneous effects of attention on forecast accuracy interacting with attention determinants: earnings volatility and marginal attention cost. Columns (1) through (3) use *Accuracy* as the dependent variable while columns (4) through (6) use *Accuracy_S* as the dependent variable, both of which are measures of analyst forecast accuracy. The independent variable, $Attention_{ijt}$, is the number of forecast revisions analyst i makes for firm j during fiscal year t. $Attention_{ijt}$ is augmented by controlling for *ForecastAge*, firm-year fixed effects, and analyst-year fixed effects. Columns (1) and (4) examine whether attention has a varying effect on forecast accuracy depending on the level of earnings volatility. Columns (2) and (5) examine whether attention has a varying effect on forecast accuracy depending on how long the analyst has been following a firm. Columns (3) and (6) examine whether attention has different effects on forecast accuracy depending on whether the analyst has an industry specialization. In all specifications, the control variables are *FirmExp* and *Specialization*. Standard errors are reported in parentheses and double-clustered at the analyst and at the firm levels in all specifications. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	DV: *Accuracy*			DV: *Accuracy_S*		
	(1)	(2)	(3)	(4)	(5)	(6)
Attention × *Log(Volatility)*	0.0059***			0.0061***		
	(0.0011)			(0.0011)		
Attention × *FirmExp*		-0.0005**			-0.0005**	
		(0.0003)			(0.0003)	
Attention × *Specialization*			-0.0064***			-0.0063***
			(0.0020)			(0.0020)
Attention	0.0464***	0.0222***	0.0245***	0.0482***	0.0227***	0.0250***
	(0.0049)	(0.0015)	(0.0019)	(0.0049)	(0.0015)	(0.0019)
ForecastAge	-0.0034***	-0.0034***	-0.0034***	-0.0034***	-0.0034***	-0.0034***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Analyst-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
N	337,624	337,624	337,624	337,624	337,624	337,624
adj. R-sq	0.77	0.77	0.77	0.794	0.794	0.794

Table 1.9. Attention and Revising Behaviors

This table presents the fixed effects OLS estimates of the effect of attention on analyst revising behaviors. The independent variable, $Attention_{ijt}$, is the number of forecast revisions analyst i makes for firm j during fiscal year t. $Attention_{ijt}$ is augmented by controlling for *ForecastAge*, firm-year fixed effects, and analyst-year fixed effects. Columns (1) and (2) examine whether attention level affects the tendency to issue upward revisions. The dependent variable is *NetUpwardRev,* which is the difference between the number of upward revisions and the number of downward revisions. Columns (3) and (4) examine whether attention level affects an analyst's optimism. The dependent variable is *LastFirstDiff*, which is the difference between the value of the last forecast and the first forecast. Columns (5) and (6) examine whether attention level affects an analyst's bias against consensus, a measure of optimism. The dependent variable is *Bias*, which is the difference between the analyst's forecast and the consensus forecast, deflated by the standard deviation of all forecasts for the same firm-year. Standard errors are reported in parentheses and double-clustered at the analyst and at the firm levels in all specifications. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	NetUpwardRev		*LastFirstDiff*		*Bias*	
	(1)	(2)	(3)	(4)	(5)	(6)
Attention	-0.0328***	-0.0331***	-0.0165***	-0.0162***	-0.0150***	-0.0153***
	(0.0056)	(0.0056)	(0.0009)	(0.0009)	(0.0014)	(0.0014)
ForecastAge	0.0004***	0.0004***	0.0001***	0.0001***	0.0005***	0.0005***
	(0.0001)	(0.0001)	(0.00001)	(0.00001)	(0.0001)	(0.0001)
FirmExp		0.0015		-0.0011***		0.0011
		(0.0015)		(0.0003)		(0.0010)
Specialization		-0.0063		0.0011		0.0046
		(0.0126)		(0.0022)		(0.0078)
Firm-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Analyst-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
N	337,624	337,624	337,624	337,624	337,624	337,624
adj. R-sq	0.584	0.584	0.709	0.709	0.573	0.573

Chapter 2

The Impact of Audit Employee Job Satisfaction on Audit Quality

2.1 Introduction

Auditors provide independent assurances about the credibility of accounting information by expressing an opinion on whether a company's financial statements are presented fairly and free of material misstatements. To improve resource allocation and contracting efficiency, financial markets rely on these auditors to detect abnormalities in financial reports, which range from accounting errors and GAAP violations to management fraud (Blackwell, Noland, and Winters 1998; Minnis 2011). Therefore, variation in auditor work performance is a key input to financial markets. However, little is known about how auditor work performance is affected by employee characteristics, despite the literature advocating for audit research at the employee level (DeFond and Francis 2005; Church, Davis, and McCraken 2008; Gul, Wu, and Yang 2013; DeFond and Zhang 2014; Li, Qi, Tian, and Zhang 2017). In this paper, I investigate the effect of audit employee job satisfaction on audit quality.

In the audit profession, it is unclear ex ante whether job satisfaction positively or negatively impacts audit quality. Satisfied auditors may perform better, but they may be optimistic and fail to exercise enough professional skepticism to detect accounting manipulation. Since employee job satisfaction is a factor that management can influence, it is beneficial to see whether the net effect is, in fact, positive. In spite of the potential economic and practical importance of this effect, it is challenging to study the impact of job satisfaction on audit quality, due to the lack of available data. Without employee level data, such effects may be masked by audit firm-level characteristics.

To overcome this data limitation, I utilize a novel dataset from Glassdoor.com, an online rating website in which former and current employees write reviews about their employers. The novelty of the dataset is threefold. First, it provides access to employee job satisfaction via company ratings included in the reviews written by users of the site. Second, if a company has multiple offices, Glassdoor separates user reviews according to office locations. This feature allows me to exploit variation within each audit firm at a given point in time. Finally, ratings vary over time as reviews are collected. Thus, Glassdoor data provides a unique setting to investigate the effect of job satisfaction on the work performance of audit employees. From this data, I construct my measure of job satisfaction as the average employee rating at the office conducting the audit during the auditing period.

Following the literature (DeFond and Zhang 2014; Ashbaugh, LaFond, and Mayhew 2003; Myers, L. A. Myers, and Omer 2003; Menon and Williams 2004; Gul, Fung, and Jaggi 2009; Krishnan, Wen, and Zhao 2011; Lennox and Li 2012), I use various proxies for audit quality: performance-matched absolute discretionary accruals and signed discretionary accruals (Kothari, Leone, and Wasley 2005) to capture within GAAP manipulation, restatements, and Accounting and Auditing Enforcement Releases (AAERs) to capture egregious accounting irregularities. I find a statistically and economically significant positive relationship between audit employee job satisfaction and audit quality. A one unit increase in job satisfaction (on a scale from 1 to 5) reduces absolute abnormal accruals by 0.7 standard deviations. This implies that satisfied audit employees, on average, produce significantly higher-quality audits than unsatisfied audit employees. I also find a negative relationship between employee satisfaction and signed discretionary accruals. This indicates that higher levels of job satisfaction not only reduce the amount of upward opportunistic earnings management, but also reflect the fact that audit employees are responding to the demand for accounting conservatism in practice. I similarly find that satisfied audit employees decrease the client firm's likelihood of receiving a "Big R" restatement[4] and AAER. However, I find a statistically insignificant effect of job satisfaction on "Little r" restatement. These findings suggest that satisfied employees are more likely to detect and catch egregious accounting irregularities as opposed to minor accounting errors.

Finally, I offer suggestive evidence on which aspects of employee job satisfaction audit firms may influence to improve audit quality. Using the other ratings provided by Glassdoor, I show that the satisfaction coming from employees' perceptions of management quality and career opportunities has a statistically significant effect on audit quality, whereas their perception of

[4] "Big R" restatements refer to misstatements that require the firm to file an Item 4.02 Statement of Non-Reliance in an 8-K filing and reissue the financial statements in question. "Little r" restatements, on the other hand, refer to misstatements that only require the firm to adjust to the prior period information in the current year instead of reissuing the financial statements (Choudhary, Merkley, and Schipper 2017).

other aspects, such as work-life balance and salary and benefits, does not. These results suggest that management quality and career opportunities are two channels through which job satisfaction positively relates to audit quality, and has corresponding prescriptive implications for audit firms interested in maximizing employee productivity.

To promote a causal interpretation of my estimates, I take several steps to design empirical tests that address endogeneity concerns. My preferred specifications include a restrictive set of fixed effects. I include city-year fixed effects to capture both time-invariant city characteristics and city-wide trends, including changes in business patterns or local financial reporting and fraud patterns that may drive both job satisfaction and audit quality. I also include industry-year fixed effects to account for differences in the industry characteristics and trends that could influence the nature of the audit process and the satisfaction of employees working on these audits. For example, firms in industries with high levels of intangible assets may be more difficult and less satisfying to audit than those in industries with a high fraction of tangible assets (DeFond and Zhang 2014). I include auditor-year fixed effects to compare audits conducted by the same audit firm in the same year, alleviating selection concerns relating to client-auditor matching. These fixed effects also control for any auditor-year variables that may govern the working environment for all offices of the same audit firm, including audit firm-level policies that affect both employee satisfaction and audit quality (e.g., extended work day). Finally, audit-office fixed effects capture time-invariant office characteristics, such as office culture, fixed expectation for performance, or procedural policies about how each audit should be conducted.

Although these fixed effects focus identifying variation on time series variation in job satisfaction at the audit office level and eliminate several identifying challenges, a few issues remain. First, review data are self-reported. This poses a self-selection concern and prevents the generalizability of my results. Second, reverse causality could be an issue, as employees can be more satisfied with their jobs following good audits and less satisfied following bad ones. Third, there could also be correlated omitted variables. For instance, though the fixed effects control for most unobservable factors that may prevent identification, it is not possible to identify and measure the ability of the employees performing each audit. To address these endogeneity concerns, I use an instrumental variable approach to pin down the magnitude and direction of causality.

An ideal instrument for job satisfaction would match the frequency and geographic location of the audit engagement. It must also be uncorrelated with audit quality through any other channel other than job satisfaction. To this end, I exploit the *abnormal* rainfall fluctuation in 39 major U.S. cities *during* the auditing period as an instrument. The relevance of this instrument is grounded in prior evidence linking weather to satisfaction from psychology, physiology, and behavioral economics.[5] The first stage regression estimates establish the economic and statistical relevance of weather on job satisfaction. The exclusion criterion requires that weather affects audit quality only through audit employees. Because I focus on the weather *after* the fiscal year end of the client, the plausibly exogenous instrument should not affect the firm performance or any variables pertaining to the firm for that fiscal year, including the manager's decision to manage earnings. My second stage estimates provide statistically and economically significant evidence that links employee satisfaction to audit quality.

Finally, I conduct two additional tests to address potential alternative explanations. The first alternative explanation comes from the unobservable satisfaction of the accounting employees at the client firms. Even though I include the client firm's financial reporting quality proxies in the regression, these proxies may be imperfect. To demonstrate that my findings are driven by audit firm employee satisfaction and not client firm employee satisfaction, I limit the sample to observations in which the audit office's location and the client firm's location are different. This forces the instrument, which is precipitation at the audit office, to affect audit employee job satisfaction only. In fact, the identifying variation in job satisfaction used in the second stage is unrelated to the client firm's employee satisfaction. The second alternative explanation arises from the potential violation of the instrument's exclusion restriction. One could argue that precipitation

[5] For instance, Keller, Fredrickson, Ybarra, Côté, Johnson, Mikels, Conway, and Wager (2005), Schwarz and Clore (1983), and Dennisen (2008) provide evidence showing that people are generally happier on sunny days and sadder on rainy or overcast days. Isen and Patrick (1983) and Bassi, Colacito, and Fulghieri (2013) demonstrate that risk-taking behavior is affected by sentiment.

affects audit quality by affecting or delaying audit employees' work schedules. To address this concern, I control for the number of days it takes to complete each audit. This ensures that the instrumented variation in job satisfaction is unrelated to changes in audit time. In both tests, I find quantitatively similar results to the baseline regression.

It is worth noting that I do not claim to study the effect of audit teams on audit quality. This would be beyond the scope of Glassdoor data. Because there is significant variation of workload, hours worked, team members' characteristics and experience, the number of team members, pressure of superiors, etc., it would be interesting and beneficial to the audit literature if future research can overcome this data limitation. This paper, in contrast, offers insight into how overall employee satisfaction in an audit office at a given time impacts audit quality, using a plausibly valid instrument that is unrelated to the above characteristics of each audit engagement but is related to employee job satisfaction.

This paper makes several contributions to the literature. First, it contributes to the audit quality literature by introducing a novel office-level dataset and using it to demonstrate that employee satisfaction is an economically significant determinant of audit quality. I show that this employee characteristic is important in helping audit firms detect egregious accounting irregularities rather than minor accounting errors. In this sense, this paper responds to the call from researchers and policymakers to investigate the role of auditors on audit quality at the employee level instead of the audit firm or audit office level (DeFond and Francis 2005; Church et al. 2008). To the best of my knowledge, this paper is the first to offer empirical evidence on how lower level audit employees in the United States affect audit quality. Second, I show that these effects are driven mainly by management quality and career opportunities, rather than compensation and work-life balance. This distinction is important, as it highlights how the job satisfaction of audit employees is different from that of other employees. My results provide prescriptive advice for audit firm management concerning the link between employee satisfaction and audit quality. Third, my study also complements the literature on job satisfaction and work performance by offering large-scale archival evidence on the positive causal effect of satisfaction on audit employee performance (Judge, Thoresen, Bono, and Patton 2001). Finally, this paper contributes to the literature on the impact of weather on human behaviors in accounting and finance by documenting the effect of precipitation on audit employee job satisfaction (Kamstra, Kramer, and Levi 2003, DeHaan et al. 2017). Thus, my results provide supporting evidence for the external validity of the experimental and survey-based literature on weather, job satisfaction, and productivity.

My paper is also part of an emerging literature using Glassdoor data in accounting research. For example, Ji, Rozenbaum, and Welch (2017) use Glassdoor ratings to study how corporate culture affects financial reporting quality. My study is different from theirs, in that I focus on the employees at audit firms, whereas they focus on the employees at the client firms. A concurrent paper by Khavis and Krishnan (2018) uses Glassdoor ratings to study the determinants of audit employee job satisfaction, focusing on the association between work-life balance and audit quality. In contrast, my paper studies how the overall job satisfaction of audit employees impacts audit quality using weather-induced variation in job satisfaction to investigate the magnitude of the effects.

My findings have important implications for the auditing practice. First, they shed light on the practical importance of audit firms' treatment of their employees. If audit firms want to achieve better employee performance, then keeping their employees satisfied may be a relatively inexpensive way to achieve such a goal. Second, my results suggest that because there are significant differences between employee satisfaction across offices, audit firms could adopt practices and cultural values that facilitate employee satisfaction in some offices and adapt them to the other offices to improve audit quality. Finally, my findings imply that, when choosing an auditor, client firms may consider the work environment of audit employees, because it can affect the credibility and quality of their financial statements.

2.2 Related Literature & Hypothesis Development

2.2.1 Prior Literature on Audit Quality

Identifying ways to improve audit quality is one of the main goals of audit research. Prior literature has found a number of factors affecting audit quality from both the supply and demand side of the audit industry. For example, Meyers et al. (2003) find that longer auditor tenure is associated with higher-quality audits. Gul et al. (2009) extend this finding by showing a weaker association between shorter auditor tenure and lower earnings quality for firms audited by industry specialists compared to non-specialists. However, longer audit partner tenure is found to impair audit quality (Carey and Simnett 2006). On the client side, Menon and Williams (2004) find that firms employing former partners as officers or directors tend to have lower-quality audits. These findings imply that although long-tenured ties between accounting firms and their clients may improve audit quality through efficiency and trust, these relationships could decrease audit quality because of the lack of auditor independence.

Using Big 6 indicators as a proxy of the perceived reputation of CPA firms, Kim, Chung, and Firth (2003) show that Big 6 auditors are more effective than non-Big 6 auditors in deterring opportunistic income increasing earnings. Similarly, Francis, Reichelt, and Wang (2005) offer evidence showing that the perceived auditor reputation for industry expertise at the national and local level is highly valued by clients. Lennox and Li (2012) find no statistical correlation between the auditor's exposure to litigation risk and audit quality, and find that clients do not necessarily prefer being audited by unlimited liability partnership auditors. Michas (2011), using data primarily from the World Bank's Reports on Standards and Codes, shows that Big N audit quality is higher in countries with a more developed audit profession.

The literature also examines the effect of various audit-office characteristics on audit quality. Garven and Taylor (2015) and Francis and Yu (2009) provide a positive link between office size and audit quality. Li (2009) investigates the question of whether auditor independence at the office level suffers when clients are large and economically significant firms. Office size has also been found to be a determinant of auditor reputation (Notbohm 2010). However, direct evidence on how audit employees affect audit quality is limited. These studies typically use survey data or focus on high-level employees such as partners. For example, Prawitt, Smith, and Wood (2009) find that internal audit quality is negatively related to earnings management, by using a composite score based on survey responses from chief audit executives. Using data from China, Gul et al.

(2013) and Li et al. (2017) show that audit quality is affected by characteristics of partners or senior managers in the audit firms who sign the audit reports.

Instead of investigating the impact of office-level and industry factors on audit quality, my paper contributes to the literature by providing findings that tie external audit quality with the performance of audit employees working at each audit office, making use of a large-scale archival data set.

2.2.2 Prior Literature on Job Satisfaction and Productivity

This paper also speaks to the literature on the relationship between job satisfaction and productivity. This topic has been investigated largely in the psychology and behavioral economics literature, in various settings and professions. Hoppock (1935) defines job satisfaction as "any combination of psychological, physiological and environmental circumstances that cause a person truthfully to say I am satisfied with my job." Prior work in these fields generally documents a positive correlation between satisfaction and performance; however, they tend to disagree about the magnitude and causal direction of such a correlation. Conducting analyses on small sample-sized survey data from various professions, Bateman and Organ (1983), Petty, Mcgee, and Vender (1984), Judge et al. (2001), and Mamiseishvili and Rosser (2011) find that job satisfaction positively correlates with employee productivity.

At the company level, Edmans (2011) shows how employee satisfaction impacts firm value. He analyzes the relationship between employee satisfaction and long-run stock returns in a sample of companies listed in the "100 Best Companies to Work for in America." He finds that these companies earned an annual four-factor alpha of 3.5% from 1984 to 2009, and 2.1% above industry benchmarks, compared to those who are not on that list. This study, however, does not speak directly to the productivity of employees as a result of job satisfaction and is also difficult to generalize to companies not on the list.

2.2.3 Prior Literature on The Effect of Weather on Human Behaviors

My paper also adds to the extant literature on the relationship between weather, sentiment, and productivity from psychology, behavioral economics, and finance. As mentioned before, there is a rich literature on the effect of weather on human emotion and cognitive ability. Wright and Bower (1992) show that bad weather can reduce an individual's cognitive ability and concentration level and may even facilitate pessimism. Howarth and Hoffman (1984) provide evidence demonstrating that unpleasant weather can induce sadness, depression, or anxiety. In the corporate world setting, Saunders (1993) shows that the NYSE rises more on sunny days, while Kamstra et al. (2003) provide evidence linking investors' risk attitude with seasonal affective disorder. Cortes et al. (2016) find that happy credit officers are more likely to give credit approvals. Graham, Harvey, and Puri (2015) show that managers use their personal instincts when making corporate decisions. DeHaan et al. (2017) find that financial analysts' responses to earnings announcements are more muted with pessimism.

2.2.4 Hypothesis Development

The studies above suggest that it is important to understand how an employee characteristic such as job satisfaction affects audit quality. However, it is unclear ex-ante whether the effect is positive or negative. On the one hand, employees are more likely to be satisfied if they are either overpaid or underworked, both of which negatively affect audit firms. Traditional theory on cost efficiency (e.g., Taylor 1911) and principal-agent theory suggest that audit firms may, by saving costs and paying employees at their reservation wages, keep employees from being overly satisfied in order to prevent them from slacking off. Some empirical studies offer evidence in line with this view. Cortes et al. (2016) find that happier loan officers are more likely to give credit approvals, and the borrowers of these loans are more likely to default in the future. DeHaan et al. (2017) show that pessimism induces slower or no response to an earnings announcement from financial analysts. Thus, audit firms may not want high levels of employee job satisfaction if they believe that optimistic audit employees may fail to exercise enough professional skepticism to detect potential financial misstatements.

On the other hand, modern theory on human capital (McGregor 1960; Zingales 2000) argues that firms view employees as important and non-expendable organizational assets who can generate value for companies via their creativity, judgement, and decision-making, as well as their ability to build relationships with clients, all of which are features of the audit profession. Therefore, these theories argue that it is in the shareholders' interest to keep the employees satisfied, as this strategy would increase retention, incentives, and long-term benefits to the firms. Consistent with this view, Edmans (2011) finds a positive correlation between employee satisfaction and shareholder returns, and job satisfaction needs not induce slack. Because employee job satisfaction is a factor that management can influence, it is beneficial to see whether the net effect of employee job satisfaction on audit quality is, in fact, positive. Thus, the main testable hypothesis of this paper is as follows:

H_0: Audit employee job satisfaction positively affects audit quality.

2.3 Data

I combine four datasets for this paper: audit firm ratings from Glassdoor.com, audit data from Audit Analytics, firm fundamentals from Compustat, and weather data from the National Oceanic and Atmospheric Administration database.

2.3.1 Glassdoor.com

Glassdoor.com is a website where current or past employees can anonymously write reviews of the companies for which they are currently working or have worked in the past. Based in Mill Valley, California, Glassdoor was founded in 2007. As shown in Panel A of Table 2.1, the company has been collecting data since 2008. As of 2015, it contains over 2.5 million reviews for nearly all companies (roughly 230,000) in the United States.[6] The site has gained popularity, as it is also a place for users to access salary data and job interview questions. It receives approximately 32.2 million visitors per month.[7] Figure 2.1 shows trends in search volumes for Glassdoor, demonstrating the increasing popularity of the site.

For the purpose of this study, I limit my attention to company ratings, which are in the review section of the website. Each review typically consists of three components: (1) a whole star (from 1 to 5 stars) rating indicating the overall job satisfaction level, (2) employment status (past or

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[6] The site also tracks data for many foreign companies, though I limit my analysis to U.S. firms only.

[7] Statistics are from quantcast.com

current job), (3) text review detailing the pros and cons of working for a company. Once a review is written, anyone can freely access it. Users will look at these reviews to get a glimpse of what it is like to work for a specific company based on the experience of past and current employees. Figure 2.2 provides a snapshot of the Deloitte page on Glassdoor. There is an aggregate rating for the company (calculated using a proprietary algorithm that utilizes all past individual reviews with an emphasis on more recent ones). Figure 2.3 shows a snapshot of two individual reviews left by two employees of Deloitte, in which visitors can see the individual whole star ratings as well as the complementary text reviews.

The novelty of the dataset comes from the fact that it tracks job satisfaction not only at the employee level, but also at the office level. For example, Deloitte has offices in nearly all major cities in the United States. Glassdoor, by asking its users to indicate which office location of the company they work for, allows access to data at each of the Deloitte offices. Figure 2.4 shows the time-series variation in job satisfaction at each of the Big 4 auditors. Figure 2.5 presents the cross-sectional variation in job satisfaction in various offices of Deloitte (larger bubbles reflect higher ratings). Figure 2.6 shows the job satisfaction trend of Deloitte employees at several major offices. Figures 2.4 through 2.6 demonstrate significant variation in job satisfaction across offices and over time, suggesting that it is crucial to capture employee satisfaction at the time and location of the audit. This provides the basis for my empirical analysis because, in practice, each firm is audited by one office of an audit firm (or at least, there is a lead office for every audit),[8] and each firm is audited at different points in time, depending on the client firm's fiscal year end.

Glassdoor achieves a representative sample of users for its employer reviews using their "give to get" model.[9] This policy requires users to submit a contribution, typically in the form of a review, in order to receive full access to all available information on Glassdoor, including information about salaries and interview questions. As a result, the majority of users who leave

[8] In fact, the name of whichever audit office performing the audit process is indicated in the auditor's comment section in any 10-K fillings.

[9] Detailed information on the "Give to get" policy can be found at: https://help.glassdoor.com/article/Give-to-get-policy/en_US

reviews do not necessarily have a strong disapproval or approval of their respective employers. They simply provide their share of information in exchange for whatever they find valuable on the website. Therefore, the policy mitigates polarization bias in employer reviews. In particular, it reduces the tendency for only polarized 1-star and 5-star reviews, as is common in Yelp and Amazon product reviews data. Figures 2.7a and 2.7b, respectively, show the distributions of Glassdoor ratings for all companies and only auditors. The distributions in both cases look bell-shaped and left-skewed with a peak at four, providing evidence that reviews do not tend to be either extremely favorable or extremely unfavorable with little middle ground.

Additionally, Glassdoor has taken a number of measures to ensure data quality. Data quality can be separated into two main categories: the authenticity of the person writing the reviews and the integrity of the actual content of the reviews. For the former, user accounts are closely monitored to prevent instances in which one person can create multiple accounts and rate multiple companies. Specifically, their fraud detection program detects when multiple accounts are email verified from the same IP address. Glassdoor also employs several machine learning and fraud detection models running constantly to eliminate fraudulent and inappropriate content. These models scan for words, keep track of IP addresses, and reject any review violating Glassdoor's community guidelines.[10]

Like the U.S. Bureau of Labor Statistics, Glassdoor relies on the self-reported employment status of contributors. Nonetheless, there is no strategic incentive for Glassdoor users to systematically misstate their employment status, because their platform neither rewards nor punishes certain employment statuses.

To cope with incentivized reviews in which employers offer their employees perks in exchange for favorable ratings, Glassdoor allows its community to flag inappropriate reviews that users suggest are incentivized. Glassdoor removes such content if it can conclude that its community

[10] Information regarding Glassdoor's community guidelines can be found at: https://help.glassdoor.com/article/Give-to-get-policy/en_US

guidelines were violated. Although no measures can completely ensure perfect data quality, these policies provide greater assurance about the integrity of Glassdoor data.

2.3.2 Audit Analytics and Compustat

I use the Audit Analytics Audit Opinion file. Panel B of Table 2.1 reports the descriptive statistics of the Audit Analytics dataset. It covers the period from 2008 to 2015 and contains approximately 130,000 auditor-client-year observations of approximately 1,000 unique audit firms and 32,000 companies. The important feature of this dataset is the fact that it tells us which auditor office performs which audit. Furthermore, it contains the date on which the audit stops (the sign date as indicated in 10-K filings) and the fiscal year of the corresponding audit report. This allows me to identify the time window to calculate the job satisfaction level *during* the audit process instead of using a running rating average over all reviews up to the time of interest or the yearly average of ratings, as those would reflect less accurately how audit employees feel during the time they perform their work. Isolating this time period allows for a better analysis and inferences about the impact of employee job satisfaction level on audit quality.

Compustat North American Annual Fundamentals tape provides data on firms' fundamentals, many of which are needed for the calculation of discretionary accruals as well as the regression analysis, which will be described in detail in the next section. The dataset also covers a period from 2007 to 2015. Panel C of Table 2.1 reports the descriptive statistics of the Compustat dataset. There are approximately 17,000 unique firms in the sample out of roughly 120,000 firm-year observations.

2.3.3 Weather Data – Precipitation

I exploit variation in local precipitation as an exogenous variation to employee job satisfaction, which serves as a basis for my IV identification strategy. I obtain data on local weather from the Integrated Surface Database of the National Oceanic and Atmospheric Administration (NOAA ISD-Lite), a government entity that monitors various weather indicators from the oceans and the atmosphere. The database contains hourly weather observations from numerous weather stations in the United States. Liquid precipitation is measured and reported over a six-hour accumulation period (in millimeters) in which higher numbers indicate more precipitation. All weather data between 0AM to 23PM (local time) of the same day are retained to accurately capture the weather characteristics at each weather station .

2.3.4 Aggregating Data

The last panel of Table 2.1 shows the descriptive characteristics of the final sample used for the baseline regression analysis. This sample is obtained by merging Glassdoor, Audit Analytics, and Compustat. There are 5,681 auditor-office-firm-year observations where Big 4 Audit Firms account for roughly 95% of the observations. There are 39 cities across the United States in the final sample. The number of observations decreases significantly after the merge, for a few reasons. First, job satisfaction level is calculated using a small window during which the audit process happens; thus, it requires that Glassdoor have reviews posted during those specific months. In reality, reviews for each audit office of each audit firm may not be available every single month, therefore limiting the ability to obtain job satisfaction data for some audits. For example, KPMG Boston in 2011 can conduct several audits throughout the year for various companies, but if Glassdoor has reviews for KPMG Boston only from January, February, April, and May, job satisfaction level near the end of the year cannot be calculated. Consequently, observations that have firms being audited at the end of the year by KPMG Boston will be dropped. Second, cases in which audit mergers happen but are not well differentiated in Glassdoor are also excluded from the final sample. Finally, missing data from Compustat when computing discretionary accruals also causes the reduction in the number of observations.

2.4 Measurements

2.4.1 Measurement of Audit Quality

In order to evaluate the effect of job satisfaction on audit quality, I employ various proxies as suggested in the literature: the absolute value of discretionary accruals, signed discretionary accruals, restatements, and AAERs (e.g., DeFond and Zhang 2014; Becker, DeFond, Jiambalvo, and Subramanyam 1998; Ashbaugh et al. 2003; Myers et al. 2003; Butler et al. 2004; Menon and

Williams 2004; Gul et al. 2009; Prawitt et al. 2009; Krishnan et al. 2011; Michas 2011; Lennox and Li 2012).

The intuition for the first two measures is that the magnitude of earnings management should be low if audit quality is high. As continuous measures, they are intended to capture within GAAP earnings manipulation. Discretionary accruals are performance-matched abnormal accruals calculated based on the modified Jones Model (Jones 1991; Dechow, Sloan, and Sweeney 1995; Kothari et al. 2005). In particular, I follow the version of Kothari et al. (2005) and calculate discretionary accruals to be the residuals of the following cross-sectional regression for each year:

$$TA_{it} = \beta_0 + \beta_1\left(\frac{1}{Asset_{it-1}}\right) + \beta_2(\Delta Rev_{it} - \Delta Rec_{it}) + \beta_3 PPE_{it} + \beta_4 ROA_{it} + \varepsilon_{it},$$

where total accruals (TA_{it}) is calculated as the change in non-cash current assets minus the change in current liabilities excluding the current portion of long-term debt, minus depreciation and amortization, scaled by lagged total assets. $Asset_{it}$ is the total assets of firm i in year t. ΔRev_{it}, ΔRec_{it}, PPE_{it}, and ROA_{it} represent, respectively, the change in revenue, change in receivables, property, plant and equipment, and return on assets, all of which are scaled by lagged total assets $Asset_{it-1}$. The use of lagged total assets as a deflating factor helps mitigate the concern over the heteroskedasticity of the residuals. Taking the absolute value of the residuals, I obtain the performance matched absolute value of discretionary accruals ($AbsDA$). The residual itself is the signed discretionary accruals (DA).

Restatements and AAERs, on the other hand, are more direct measures of audit quality. According to DeFond and Zhang (2014), restatements and AAERs are direct measures of audit quality, as they indicate that the auditor erroneously issued an unqualified opinion on materially misstated financial statements. The advantage of using these measures is that they are strong evidence of poor audit quality because they imply that the auditor failed to detect accounting misconduct. The disadvantage, however, is that not having a restatement or an AAER is not an indication of good audit quality. For AAERs, I define the variable AAER as an indicator variable equal to 1 if there is an AAER issued subsequent to year t for an accounting irregularity in year t, and 0 otherwise.

For restatements, I separate them into two categories: "Big R" and "Little r" (Choudhary et al. 2017). "Big R" restatements refer to misstatements that require the firm to file an Item 4.02 Statement of Non-Reliance in an 8-K filing and reissue the financial statements in question. "Little r" restatements, on the other hand, refer to misstatements that only require the firm to adjust the prior period information in the current year instead of reissuing the financial statements. This distinction allows me to investigate whether satisfied employees are more likely to detect more serious misstatements or minor accounting mistakes. I define *BigR and LittleR* as indicators equal to 1 if a Big R restatement and a Little r restatement, respectively, were associated with the financial statements in question, and 0 otherwise.

2.4.2 Measurement of Job Satisfaction

Job satisfaction at each audit office is defined as the average of the individual Glassdoor ratings posted for each office during a given time window. Due to the limitation of the data, it is not possible to identify exactly the individuals in the audit office who performed each audit, or the senior individuals who perform the reviewing process to calculate their average job satisfaction. A good proxy of their collective satisfaction at any given time period is the average job satisfaction of all available ratings at that office during that period.

Although using an office-wide satisfaction level may induce measurement error, measurement error is unlikely to correlate with weather or audit quality. Furthermore, measurement error in this particular setting does not pose a significant threat to identification, as it gives rise to a specific type of bias called attenuation bias (See Appendix 2B). In other words, measurement error biases the coefficient estimate towards zero but does not change its sign.

Using data from audit analytics, I am able to identify the date on which the audit process ends. In almost all cases, an audit process takes place over the course of 2 to 4 months. For example, in Yahoo Inc.'s 10-K for the fiscal year ending December 31, 2014, the San Jose office of PwC indicates that the end date of the audit process was February 26, 2015. This indicates that the audit process should have happened over the course of January and February and possibly December as preparation for the audit.

As indicated in section 2.3.2, job satisfaction level is calculated using a small window during which the audit process happens; thus, it requires that Glassdoor have reviews posted during those specific months. In reality, reviews for each audit office of each audit firm may not be available every single month, therefore limiting my ability to obtain job satisfaction data for some audits if I were to use the reviews only during the two-month audit periods. This in turn reduces the statistical power of the tests. Due to this data limitation, I use a baseline measure of job satisfaction level as the average ratings over the last two months of the auditing period, plus one month before and one month after (four in total). In the example above, the employee satisfaction level of the PwC office in San Jose when conducting the audit for Yahoo Inc. would be the average rating in December 2014, January 2015, February 2015, and March 2015.

Furthermore, as an attempt to capture bias-free employee satisfaction, I retain only the Glassdoor ratings in which users indicated that they were current employees at the time of leaving the review.[11] This eliminates observations in which a former employee who quit in 2010 decided to leave a review in 2014. All the above considered, I measure satisfaction as follows:

$$Satisfaction_{ijkt} = Job\ satisfaction\ level\ of\ employees\ at\ office\ j\ of\ auditor\ i,$$
$$auditing\ firm\ k\ during\ fiscal\ year\ t.$$

2.4.3 Control Variables

As argued by Defond and Zhang (2014), audit quality is closely tied to financial reporting quality because auditors can have good quality audits if the client financial reporting quality is high. Therefore, following the literature on factors affecting audit quality, I use the following set of controls, which capture the financial reporting quality of the audited firms, along with various sets of fixed effects. The first control is *LogMC,* which is the log of the company's market value of equity based on the closing price of its shares at the end of the fiscal year. This variable of the firm controls for the variation in company size. *BM* is the company's book to market ratio of equity and reflects growth opportunities of the companies. The next three variables control for the company's financial condition. *Lev, ROA, C-ratio*, and *OCF* represent the firm's debt to assets

[11] Please refer to section 2.3.1 for a discussion about potential concerns over Glassdoor data on employment status.

ratio, return on assets,[12] current ratio (current debt/current asset), and operating cash flow scaled by total assets, respectively.

2.5 Results

2.5.1 Summary Statistics

Table 2.2 presents the summary statistics of the final sample. I obtain the mean, median, standard deviation, maximum, and minimum, as well as important percentiles of the variables used in the analysis. The mean of the absolute discretionary accruals is 0.263, while the median is 0.078. The median in this case represents more of an accurate depiction of the magnitude we would expect for a firm's absolute discretionary accruals. The mean rating is approximately 3.68, while the median rating is 3.69. For AAER, the mean is 0.0039 and the median is 0. This is not surprising and is consistent with prior literature, because AAERs are relatively rare events (DeFond and Zhang 2014). Similarly, the mean of *BigR* is 0.044, while the mean of *LittleR* is 0.137, indicating that restatements related to minor accounting errors are more common.

Table 2.3 presents the correlation table. It shows that job satisfaction is negatively correlated with *absDA*, *DA*, *BigR,* and *AAER.* This offers initial evidence supporting the prediction that employees with higher levels of job satisfaction produce higher-quality audits.

2.5.2 Baseline Regression Results

To test the relationship between job satisfaction and audit quality, I use the following specification:

$$AuditQuality = \beta_0 + \beta_1 Satisfaction + \beta_2 LogMC + \beta_3 BM + \beta_4 Lev + \beta_5 ROA$$
$$+\beta_6 C\_ratio + \beta_7 OCF + Fixed\ Effects + \varepsilon \qquad (1)$$

where *AuditQuality* is either *absDA*, *DA*, *BigR*, *LittleR*, or *AAER*, the subscript is *ijkt* (which is omitted for ease of reading). As before, *i* refers to the audit firm (e.g., Deloitte); *j* refers to the office location (e.g., New York City); *k* refers to the client firm (e.g., Google Inc.) and *t* refers to the fiscal year end. Notice that each auditor office can audit several clients around the same time. Thus, these observations would have the same value of *Satisfaction.* To allow for possible serial

[12] Note, this variable is not scaled by lagged total asset as it was for the calculation of discretionary accruals.

correlation among audits conducted by the same office, I clustered the standard errors at the auditor office level.

To mitigate potential bias from omitted variables, I progressively include several sets of fixed effects. First, I add industry-year fixed effects to control for heterogeneity across industry and industry trends. These fixed effects mitigate the concern that both employee satisfaction and audit quality would be lower when auditing firms in industries that are more difficult to audit. Second, I include city-year fixed effects to control for geographical characteristics that may affect both individual job satisfaction and audit quality. These fixed effects also capture city-wide trends that operate at a yearly frequency, such as changes in business patterns or local financial reporting fraud patterns.

Third, to account for the baseline differences across audit offices, I control for audit-office fixed effects. For example, due to differences in working culture and preferences for hiring certain types of employees, the Deloitte office in New York may be different from the Deloitte office in Houston. Two offices may have different fixed guidelines and expectations about how all audits should be conducted. Furthermore, difference offices may have different reviewing processes by senior management to make sure that procedures and judgments align with documented results. Hence, the inclusion of audit-office fixed effects helps alleviate these concerns, because they absorb all time-invariant characteristics unique to each office. Finally, I control for auditor-year fixed effects. This allows me to compare audits conducted by the same audit firm in the same year, which helps to alleviate selection concerns related to client-auditor matching. They also control for any auditor-year variables that may govern the working environment for all offices of the same audit firm in a year (e.g., audit firm's CEO influence).

The regression results for discretionary accruals are presented in Table 2.4. The dependent variables are either *absDA* or *DA*. In each case, the columns differ in the set of fixed effects used to control for unobservable factors affecting both audit quality and satisfaction. Consistent with my prediction, the coefficients of job satisfaction across all specifications are negative and statistically significant.[13] Based on my preferred specification (columns (4) and (8)), an additional unit increase in job satisfaction level (or 1 average star increase) is associated with an approximately 0.025 decrease in the magnitude of discretionary accruals (signed and absolute). This is an economically meaningful impact, considering that the 25$^{\text{th}}$ percentile of *absDA* is 0.029 and the median is 0.078, while the 25$^{\text{th}}$ percentile of *DA* is -0.059 and the median is 0.011.

Table 2.5 reports the logistic regression estimates of the job satisfaction effect on restatements. The dependent variables are either *BigR* or *LittleR*. The coefficient of *Satisfaction* is negative in both cases and across all specifications. The effect, however, is statistically significant only for Big R restatements, but not Little r restatements. The results suggest that job satisfaction helps prevent serious misstatements that would require the filing of an Item 4.02 Statement of Non-Reliance, but it does not have an effect on detecting minor accounting errors.

Table 2.6 shows the results for *AAER*s. The number of observations dropped as more fixed effects are added happens because many fixed effect indicators predict failure perfectly (i.e., AAER is 0 for all observations under the same fixed effect). The logistics coefficient estimates are also negative and statistically significant. This suggests that higher audit employee satisfaction is associated with a lower likelihood of receiving an AAER.

In summary, the results in Tables 2.4, 2.5, and 2.6 suggest that job satisfaction is positively associated with audit quality and that satisfied audit employees are more likely to detect and prevent egregious financial irregularities as opposed to minor accounting errors.

2.6 Instrumental Variable Approach

2.6.1 Instrument Validity

The baseline specification is subject to a few endogeneity problems. First, selection bias comes from two sources. The first comes from the fact that Glassdoor ratings are self-reported. Therefore, we may not have a random sample of Glassdoor users, which may skew the distribution of ratings.

[13] The coefficient in column (5) is not statistically significant, but that in column (6) is. This suggests that geographical characteristics help isolate the right variation in job satisfaction for the empirical test.

Even though the rating distribution shown in figure 2.7b alleviates this concern, unobservable factors could still be influencing the decision to leave a review on the website.

The second source of selection bias comes from the fact that firm and auditor matching is strategic. Bird, Ho, Li, and Ruchti (2016) show that firms whose accounting departments consist of alumni from a specific audit firm, say Deloitte, tend to hire new talent from Deloitte in the future. This effect could be attributed to an efficiency reason: If Deloitte is the auditor of the firm, then it will be easier to perform the audit, as Deloitte employees are familiar with how Deloitte typically conducts its audits. However, this also creates concern over auditor independence. In a follow-up paper, Bird et al. (2017) present evidence of a negative relationship between the proportion of alumni among firms' accounting employees and the number of financial misstatements. Consequently, audit quality may be high due to the alumni effect as opposed to my variable of interest—job satisfaction. Therefore, selection bias coming from strategic matching could bias the estimates and prevent causal inference.

Second, there could also be reverse causality: audit quality influences the level of employee satisfaction. Consequently, the estimates in the baseline regression will not reflect the causal impact of job satisfaction on audit quality.

The third endogeneity comes from measurement errors of Glassdoor ratings and could therefore render coefficient estimates biased. However, as discussed in detail in Appendix 2B, this source of measurement errors in this case should not bias the direction of the causal effect, as it is known to give rise to attenuation bias, which would simply bias the estimated effect of job satisfaction toward zero, relative to the true effect.

Finally, there could be correlated omitted variables (e.g., ability and experience of the employees, workload, and nature of the audit work for each particular client firm). These omitted variables could bias the estimated effect of job satisfaction on audit quality.

To address the above endogeneity concerns and to pin down the direction of causality, I use variation in local precipitation as an exogenous shock to employee job satisfaction. In my setting, a valid instrument must be relevant to the job satisfaction level, but only affect audit quality through the job satisfaction channel. With city-year fixed effects, the instrument can also be interpreted as the abnormal amount of precipitation at each location in given year. Thus, the fact that cities such as Seattle are rainier than others is taken into account.

The instrument should satisfy the relevance condition, as it has been widely documented in the psychology and experimental economics that weather, and rain specifically, negatively affects agents' mood and, thus, satisfaction. In their literature review published in the *Journal of Applied Psychology*, Judge et al. (2017) state that there are two key drivers of within-person variability in job satisfaction: the flow of moods within an individual and the events she experiences on a day-to-day basis. Because there is substantial variability in both weather and moods over time, individual satisfaction also varies significantly over time. Thus, I expect a negative relationship between precipitation and employee job satisfaction.

2.6.2 Measure of Precipitation

To capture the precipitation level at each audit office, I identify the NOAA weather station closest to each city in the final sample. Using the longitude-latitude coordinates of the weather station and the coordinates of the zip code within the audit office's city that has the highest population, I calculate the distances between these zip codes and the weather stations and choose the station with the shortest distance. However, it is worth noting the trade-off between the shortest distance and availability of data. There are cases in which the closest station does not track precipitation or have too many missing observations. In those cases, the second closest (or the third closest) station is used instead. However, the distance from the city's most populated zip code to the chosen weather station is never more than 10 miles.

After identifying the weather station corresponding to each audit office, the weather variable *Precipitation* is calculated as the average value of all observations over the audit period. Table 2.2 shows the summary statistics of *Precipitation*. The average (median) *Precipitation* is 32.70 (32.05) millimeters. The data reveal large time-series variation, as the standard deviation of *Precipitation* is 11.84, a magnitude roughly comparable to one third of the mean. All the above considered, I measure precipitation as follows:

$$Precipitation_{ijkt} = The\ average\ of\ liquid\ precipitation\ over\ the\ audit\ period$$

$$at\ the\ location\ of\ office\ j\ of\ auditor\ i\ \ auditing\ firm\ k$$
$$in\ fiscal\ year\ t.$$

2.6.3 IV Regression Results

Table 2.7 reports the IV regression results, in which the first column refers to the first stage regression and the last two columns refer to the second stage regression. The regression model for the first stage regression is

$$Satisfaction = \beta_0 + \beta_1 Precipitation + \beta_2 LogMC + \beta_3 BM + \beta_4 Lev + \beta_5 ROA$$
$$+\beta_6 C\_ratio + \beta_7 OCF + Fixed\ Effects + u. \qquad (2)$$

As shown in column (1), the coefficient of *Precipitation* is negative and statistically significant, suggesting that rainy weather has a negative impact on job satisfaction. Specifically, a one millimeter increase in liquid precipitation reduces job satisfaction level by 0.004. From a standard deviation perspective, a one standard deviation increase in precipitation reduces employee satisfaction by 0.047. This reduction is not economically trivial, as the scale for *Satisfaction* is from 1 to 5, with the majority of observations ranging from 3.0 to 4.0. Moreover, the instrument is statistically relevant to job satisfaction level because the 1st stage F-statistics are greater than 10, the weak instruments threshold. This implies that the instrument is unlikely to be related to employee satisfaction by chance.

The second stage regression is performed using the following model:

$$AuditQuality = \beta_0 + \beta_1 \widehat{Satisfaction} + \beta_2 LogMC + \beta_3 BM + \beta_4 Lev + \beta_5 ROA$$
$$+\beta_6 C_{ratio} + \beta_7 OCF + Fixed\ Effects + v \qquad (3),$$

where $\widehat{Satisfaction}$ is the predicted value coming from the first stage regression equation (2), and *AuditQuality* is either *absDA* or *DA*. As shown in columns (2) and (3) of Table 2.7, the coefficient of *Satisfaction* is negative and statistically significant in both cases. This again provides additional support for my prediction that employees with high job satisfaction level perform better. In terms of the economic magnitude, a one unit increase in job satisfaction level induced by the exogenous weather instrument decreases the absolute discretionary accruals by approximately 0.36 (or 0.7 standard deviation of *absDA*) and signed discretionary accruals by 0.58 (or approximately one standard deviation of *DA*), which is much larger than that of the OLS estimates. This implies that the endogeneity problems (selection, attenuation from measurement errors, reverse causality, and omitted variables) in the OLS specification significantly bias the estimates downwards. For example, a strict management team at the office level may positively affect audit quality at the expense of employee satisfaction. Similarly, possible collusion between the client firm and the audit team may reduce audit quality while increasing audit employee satisfaction. These alternative stories generate biases in the opposite direction and reduce the magnitude of the effect in the OLS estimates. Taken together, the results from Table 2.7 suggest that the job satisfaction of auditors' employees is a significant determinant of audit quality and that there is a positive causal link from job satisfaction to audit quality.

2.7 Which Aspects of Job Satisfaction Matter?

After establishing that the job satisfaction effect exists and is positive, I investigate potential ways that management could change employee satisfaction levels to positively impact firms, given that manipulating weather to achieve this goal is infeasible. When leaving a review on Glassdoor, users are not only asked to provide an overall rating for the company, but also asked to rate the company in four different metrics. Those are: Career Opportunity ratings, Compensation and Benefits ratings, Senior Management ratings, and Work Life Balance ratings. A rating on Culture and Value was introduced in mid-2012; thus, I do not use it in the following test, as it reduces the number of observations and consequently the statistical power of the tests. Similar to the overall rating, these whole stars ratings reflect how the employees perceive these aspects while working at the audit firms. It is also worth noting that the overall rating, though related to these sub-ratings, is not computed from them. Therefore, to obtain insights into which aspects of employee job satisfaction matter the most to audit quality, I re-run the baseline analysis, replacing *Satisfaction* with each of these ratings. The results are reported in table 2.8. Specifications (1) to (4) show the OLS regression estimates when *absDA* is the dependent variable. Specifications (5) to (8) show the logistics regression estimates when *BigR* is the dependent variable. The last four specifications present the logistics regression estimates when *AAER* is the dependent variable.

Because each of these ratings speaks to the overall job satisfaction of employees, I expect that they have a positive relationship with audit quality. Consistent with my prediction, table 2.9 shows that the coefficients of all ratings are negative. However, only the coefficients of *Career Opportunity* and *Upper Management* are statistically significant and robust across different proxies of audit quality. The results in the table suggest that employee perception of career opportunity and management quality are the two most important aspects of job satisfaction affecting audit quality. These findings offer prescriptive pointers for management at audit firms on how to improve audit quality via employee job satisfaction and highlight how the job satisfaction of audit employees is different from that of other employees.[14]

2.8 Robustness – Alternative Explanations

2.8.1 Results Controlling for Auditing Time

The exclusion restriction requires that the instrument may not affect audit quality through channels other than job satisfaction. A potential issue arises if abnormal precipitation systematically affects an audit employee's work schedule, which in turn impairs audit quality. However, if rain causes employees to be late for work, they still need to finish all the work necessary to complete the audit. Thus, the impaired audit quality is likely due to changes in job satisfaction due to precipitation rather than the inability to complete the required work. However, to address this alternative explanation, I re-run the regressions with *Days* as an additional control, which proxies for the number of days it takes to complete the audit (Auditor Signed Date – Fiscal Year End Date).

The results are shown in Table 2.9. The coefficient of *Satisfaction* is still negative and statistically significant across all specifications with all four proxies of audit quality. The

[14] Note that the sub-ratings are perceptions of employees about the job characteristics in question and not the characteristics themselves. According to the psychology literature (e.g., Judge, Weiss, Kammeyer-Mueller, and Hulin 2017), these ratings are a product of both the fluctuation in mood of the employees and the events related to the job characteristics in question that the employees experience. To the extent that the employees are unable to perfectly differentiate the two factors, manipulation of either one would lead to a change in their perceptions of the job characteristics in question, and consequently, a change in audit quality.

magnitude is also very similar to that of Table 2.4 and Table 2.5. This finding suggests that it is unlikely that the instrumental variable estimates are driven by changes in auditing time.

2.8.2 Do Client Firms' Employees Explain the Results?

A potential confounding factor in the baseline analysis is client firms' accounting and internal audit employees. Although I include controls for the financial reporting quality of the client firms, the satisfaction level of their accounting employees may still be a correlated omitted variable. This is because satisfied employees at a client firm may produce higher-quality reports and may also increase the audit firm's employee job satisfaction due to interaction. To attenuate this problem and isolate the audit firm's employees from the client firm's employees, I perform the analysis with a subsample containing only observations in which the location of the audit office and the location of the audited firm are different. This creates a setting in which the instrument, which is precipitation at the audit office, can shock the job satisfaction of employees at the audit office, but not at the client's location.

The results are presented in Table 2.10. The number of observations drops to 3,554, due to the different location restriction. Column (1) presents the first stage regression. The effect of precipitation on job satisfaction is still negative and statistically significant in this subsample. Columns (2) and (3) show the second stage regression estimates. The coefficient of *Satisfaction* is still negative and statistically significant. Furthermore, the economic magnitude is similar to that in the full sample. Thus, the results in Table 2.10 suggest that the effects are unlikely driven solely by client firm employees.

2.9 Conclusion

Using a novel dataset, I am able to capture the job satisfaction levels of the employees working at various audit firm offices across the United States. Using absolute discretionary accruals, signed accruals, Big R restatements, Little r restatements, and AAERs as proxies of audit quality, I find that there is a positive association between employee job satisfaction and audit quality, and that satisfied employees are more likely to detect and prevent egregious accounting irregularities as opposed to minor accounting errors. To facilitate a causal interpretation of the results, I exploit variation in local rainfall during the auditing period as a plausibly exogenous shock to the job satisfaction of audit employees. With this natural experiment, I find that employee satisfaction has an economically significant positive impact on audit quality. These findings are robust to alternative explanations pertaining to auditing time and employee job satisfaction at the client firms.

Further, I show that career opportunities and management quality are the two most important aspects of employee job satisfaction that affects audit quality. These findings have prescriptive implications for audit firms seeking to maximize employee performance and clients seeking to maximize financial statement credibility.

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Appendix 2A

Variable Definitions	
Independent Variable	
Satisfaction$_{ijkt}$	Job satisfaction level of employees at office j of auditor i auditing firm k during fiscal year t. It is calculated as the average Glassdoor ratings over the 4 months period surrounding the last two months of the audit process.
Career Opportunity	Calculated same as above except that ratings for Career Opportunity were used instead of the overall ratings.
Salary and Benefits	Calculated same as above except that ratings for Salary and Benefits were used instead of the overall ratings.
Upper Management	Calculated same as above except that ratings for Upper Management were used instead of the overall ratings.
Work Life Balance	Calculated same as above except that ratings for Work Life Balance were used instead of the overall ratings.
Precipitation$_{ijkt}$	The average of liquid precipitation over the last 2-month audit period at the location of office j of auditor i auditing firm k in fiscal year t.
Dependent Variable	
DA	Signed discretionary accruals under Kothari et al.'s (2005) performance matched accruals model.
AbsDA	Absolute value of signed discretionary accruals.
BigR	Indicator variable equals to 1 if there is a Big R restatement associated with the financial statements in question, and 0 otherwise.
LittleR	Indicator variable equals to 1 if there is a Littler r restatement associated with the financial statements in question, and 0 otherwise.
AAER	Indicator variable equals to 1 if the client firm receives an AAER for the fiscal year of consideration, and 0 otherwise.
Control Variables	
LogMC	Log of market capitalization
BM	Book to market value of equity
ROA	Returns on assets

Lev	Debt to assets ratio
C_ratio	Current ratio
OCF	Cash flow from operating activities scaled by total assets
Days	The number of days between fiscal year end and auditor sign date

Appendix 2B – Measurement Error

This appendix shows why measurement error gives rise to a specific type of bias known as attenuation bias. Suppose the true satisfaction level of audit employees is X^*, but we can only proxy for it using the office-level measure of satisfaction $X = X^* + \varepsilon_2$, where ε_2 can be a combination of random noise and satisfaction level of other non-audit employees in the same office. The true regression analysis of interest is $Y = \beta X^* + \varepsilon_1$, where Y is audit quality, $\text{cov}(X^*, \varepsilon_1) = 0$ (i.e., measurement error is the endogeneity concern), and $\text{cov}(\varepsilon_1, \varepsilon_2) = 0$ (i.e., two sources of noise are unrelated to each other). The true regression equation can be rewritten as $Y = \beta X^* + \varepsilon_1 = \beta X + (\varepsilon_1 - \beta\varepsilon_2)$. It can be shown that

$$\hat{\beta} = \frac{\text{cov}(X, Y)}{\text{var}(X)} = \frac{\text{cov}\left(X^* + \varepsilon_2, \beta X^* + \varepsilon_1\right)}{\text{var}\left(X^* + \varepsilon_2\right)}$$

Thus, $$\text{plim}\ \hat{\beta} = \left[\frac{\text{Var}\left(X^*\right) + \text{Cov}(X^*, \varepsilon_2)}{\text{Var}\left(X^*\right) + \text{Var}(\varepsilon_2) + 2\text{Cov}(X^*, \varepsilon_2)}\right]\beta$$

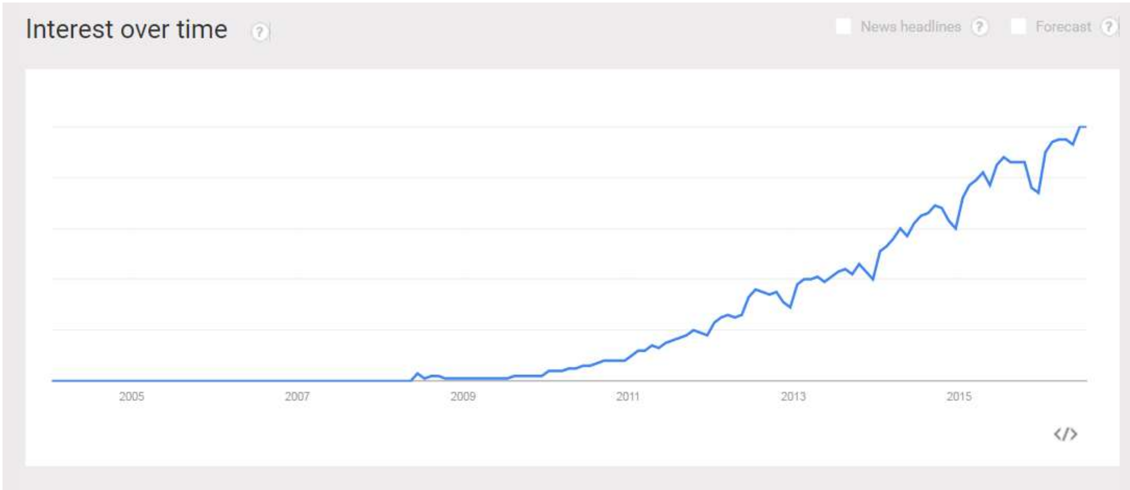
Though employees in other lines of work might have different levels of satisfaction compared to audit employees in the same office, there is no reason to believe that there is a systematically negative relationship between the two. That is, $\text{Cov}(X^*, \varepsilon_2)$ cannot be less than 0.

If the satisfaction level of non-audit employees in the office is uncorrelated or positively correlated to that of audit employees, then $\text{Cov}(X^*, \varepsilon_2) \geq 0$. Consequently, the quantity in front of β ought to satisfy the following condition:

$$0 < \frac{\text{Var}\left(\text{X}^*\right) + \text{Cov}(\text{X}^*, \varepsilon_2)}{\text{Var}\left(\text{X}^*\right) + \text{Var}(\varepsilon_2) + 2\text{Cov}(\text{X}^*, \varepsilon_2)} < 1$$

This causes attenuation bias because the estimated effect $\hat{\beta}$ will always be smaller than the true effect β. However, because their signs are the same, qualitative inferences on the effect of X^* on Y is unaffected.

Figure 2.1. Glassdoor Search Volume (obtained from Google Trend)



Note: Numbers represent search interest relative to the highest point on the chart for the given region and time. A value of 100 is the peak popularity for the term. A value of 50 means that the term is half as popular. Likewise, a score of 0 means the term was less than 1% as popular as the peak.

Figure 2.2. Snapshot of Deloitte page on Glassdoor

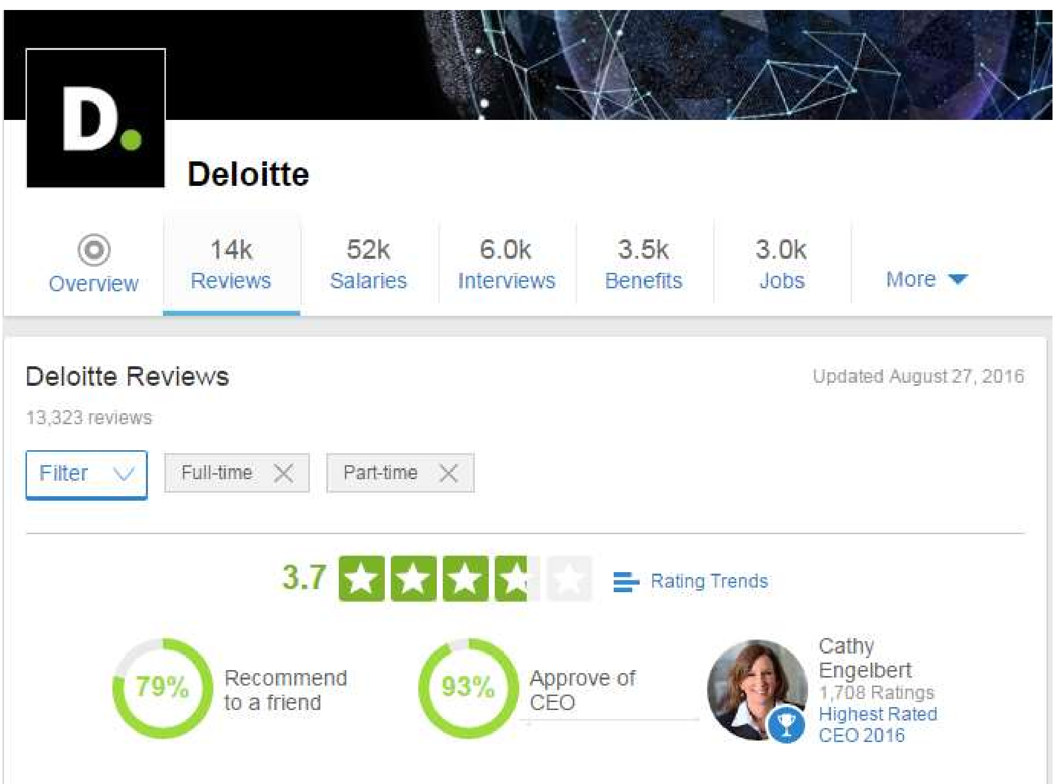
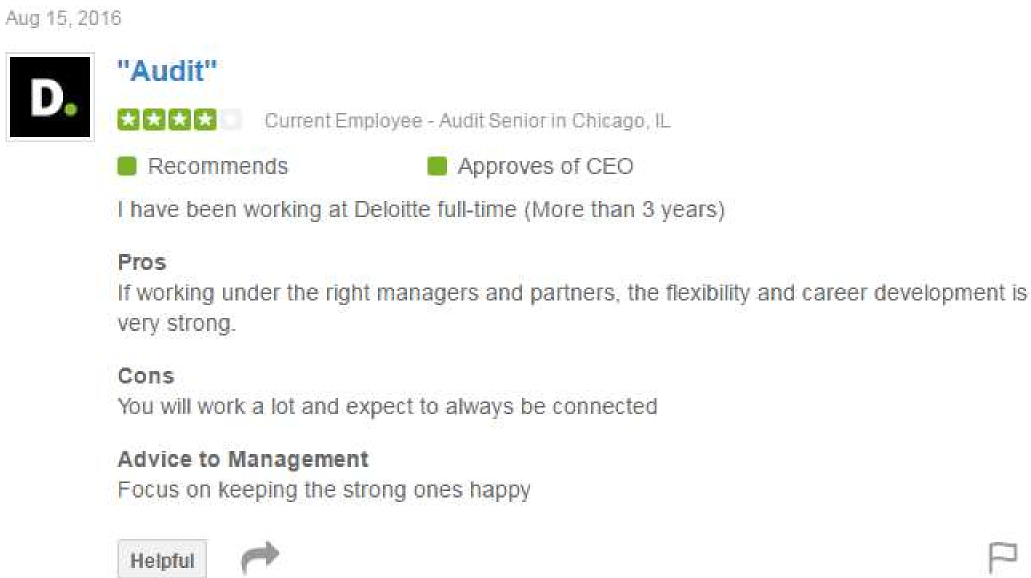


Figure 2.3. Snapshots of individual reviews left by users on Deloitte page



Aug 15, 2016

D.

"Audit"

Current Employee - Audit Senior in Chicago, IL

Recommends

Approves of CEO

I have been working at Deloitte full-time (More than 3 years)

Pros

If working under the right managers and partners, the flexibility and career development is very strong.

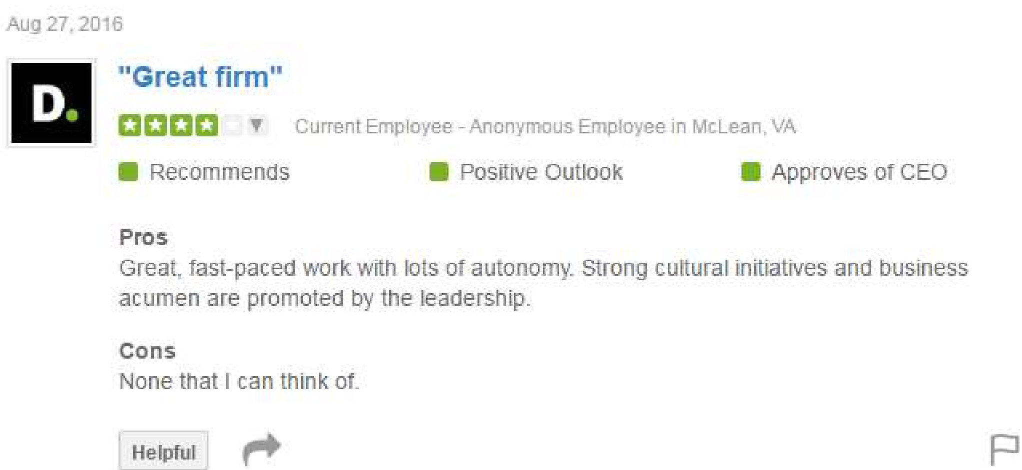
Cons

You will work a lot and expect to always be connected

Advice to Management

Focus on keeping the strong ones happy

Helpful



Aug 27, 2016

D.

"Great firm"

Current Employee - Anonymous Employee in McLean, VA

Recommends

Positive Outlook

Approves of CEO

Pros

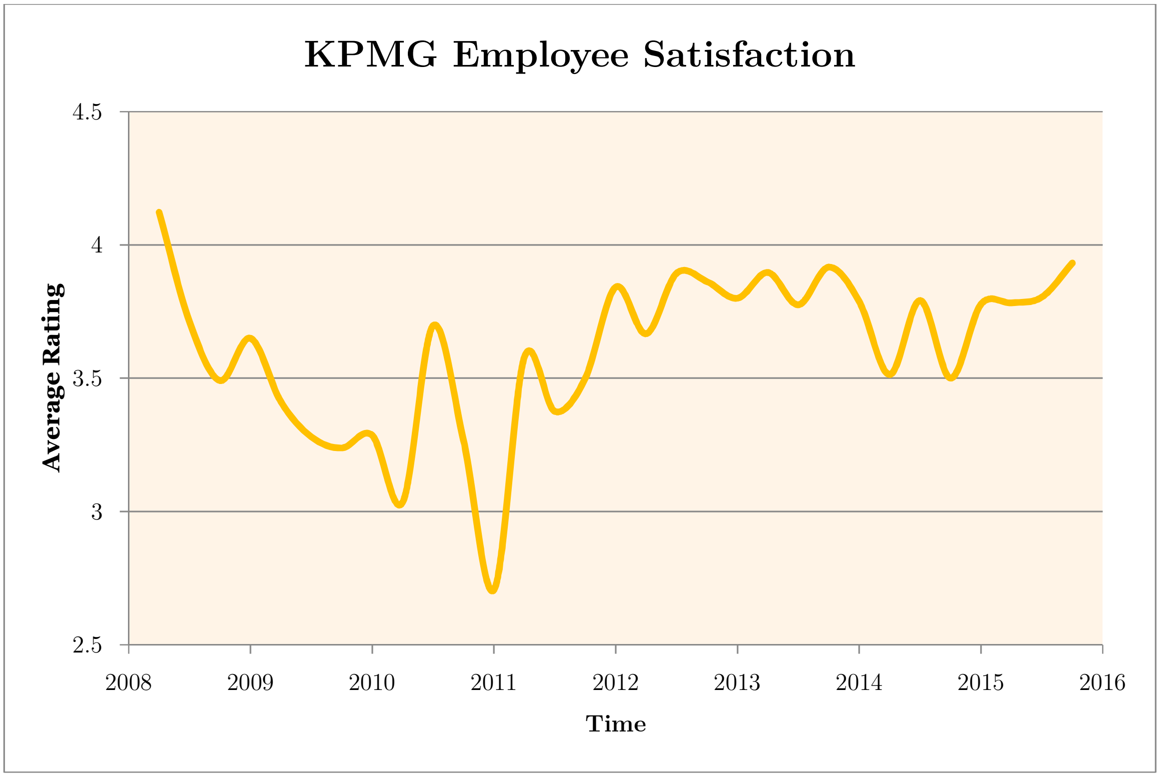
Great, fast-paced work with lots of autonomy. Strong cultural initiatives and business acumen are promoted by the leadership.

Cons

None that I can think of.

Helpful

Figure 2.4. Monthly average employee satisfaction of each Big 4 firm


KPMG Employee Satisfaction
4.5
4
3.5
3
2.5
Average Rating
2008
2009
2010
2011
2012
2013
2014
2015
2016
Time

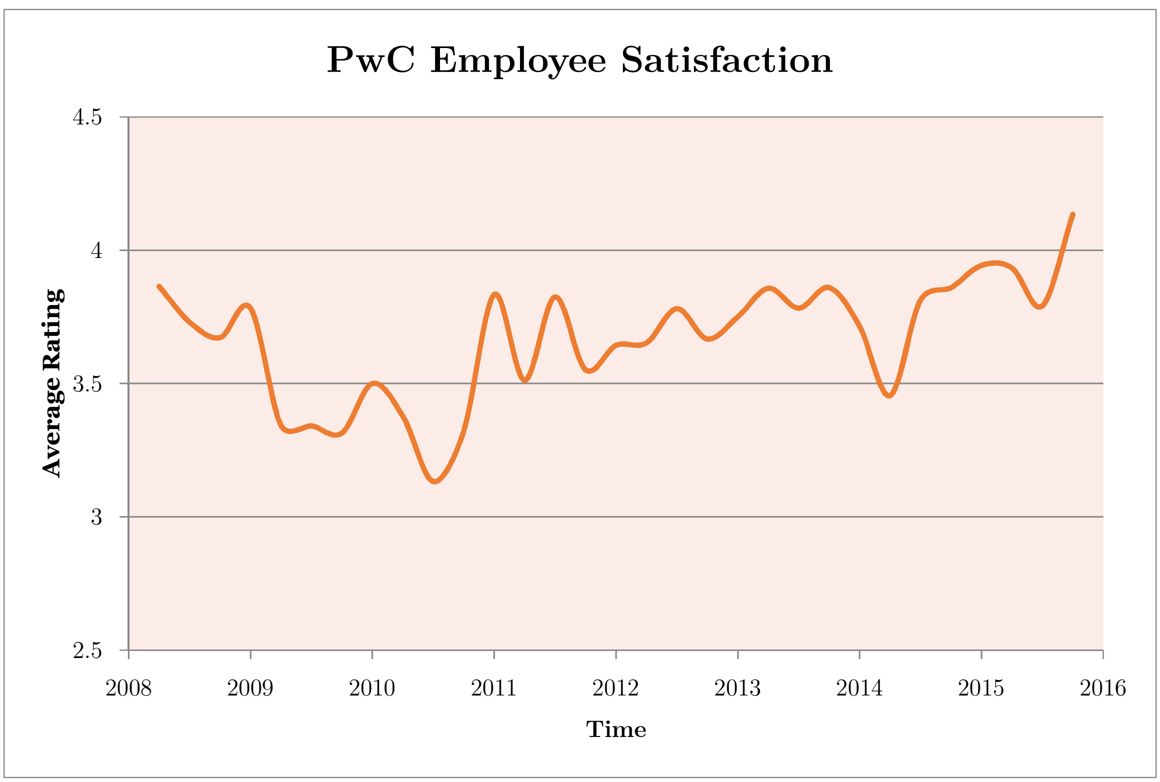

PwC Employee Satisfaction
4.5
4
3.5
3
2.5
Average Rating
2008
2009
2010
2011
2012
2013
2014
2015
2016
Time

Figure 2.5. Cross-sectional variation in employee satisfaction at various Deloitte offices in 2011

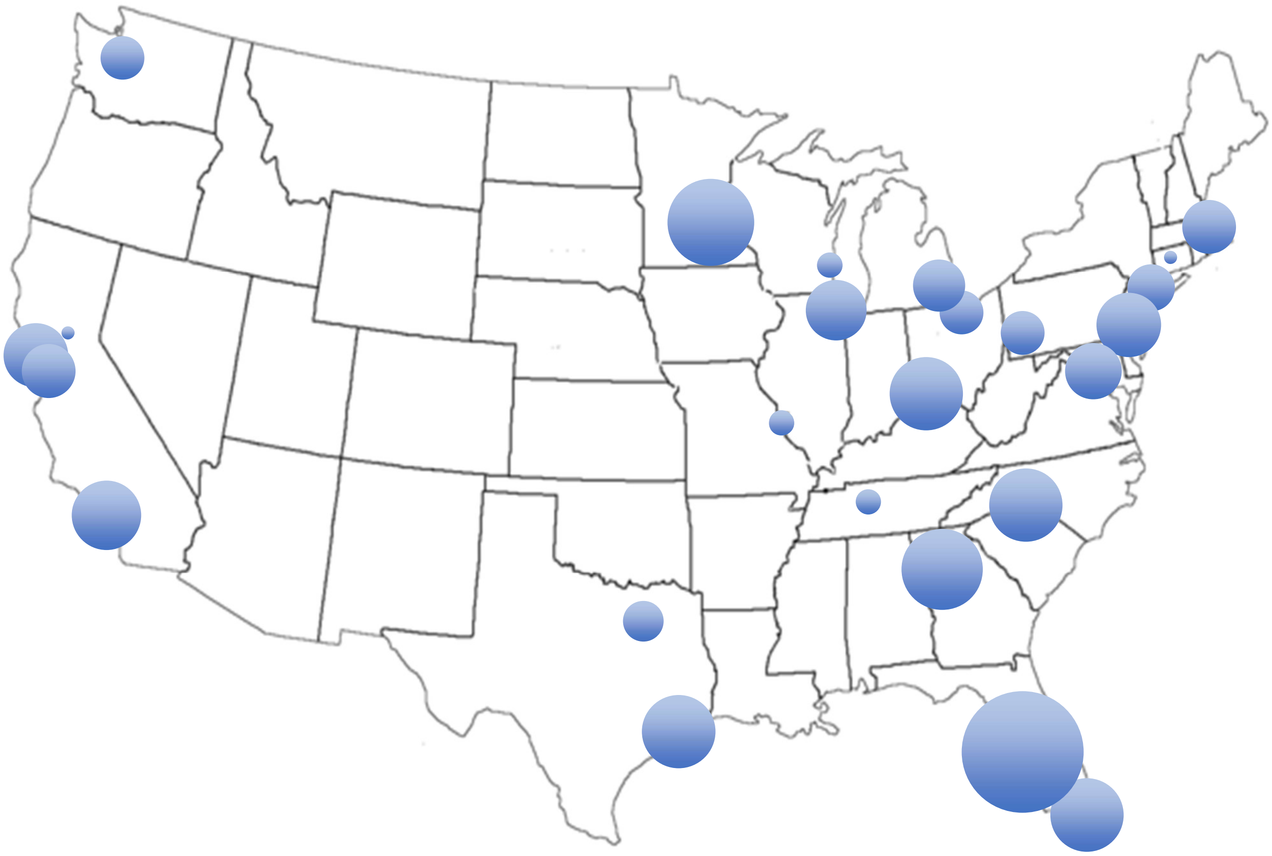
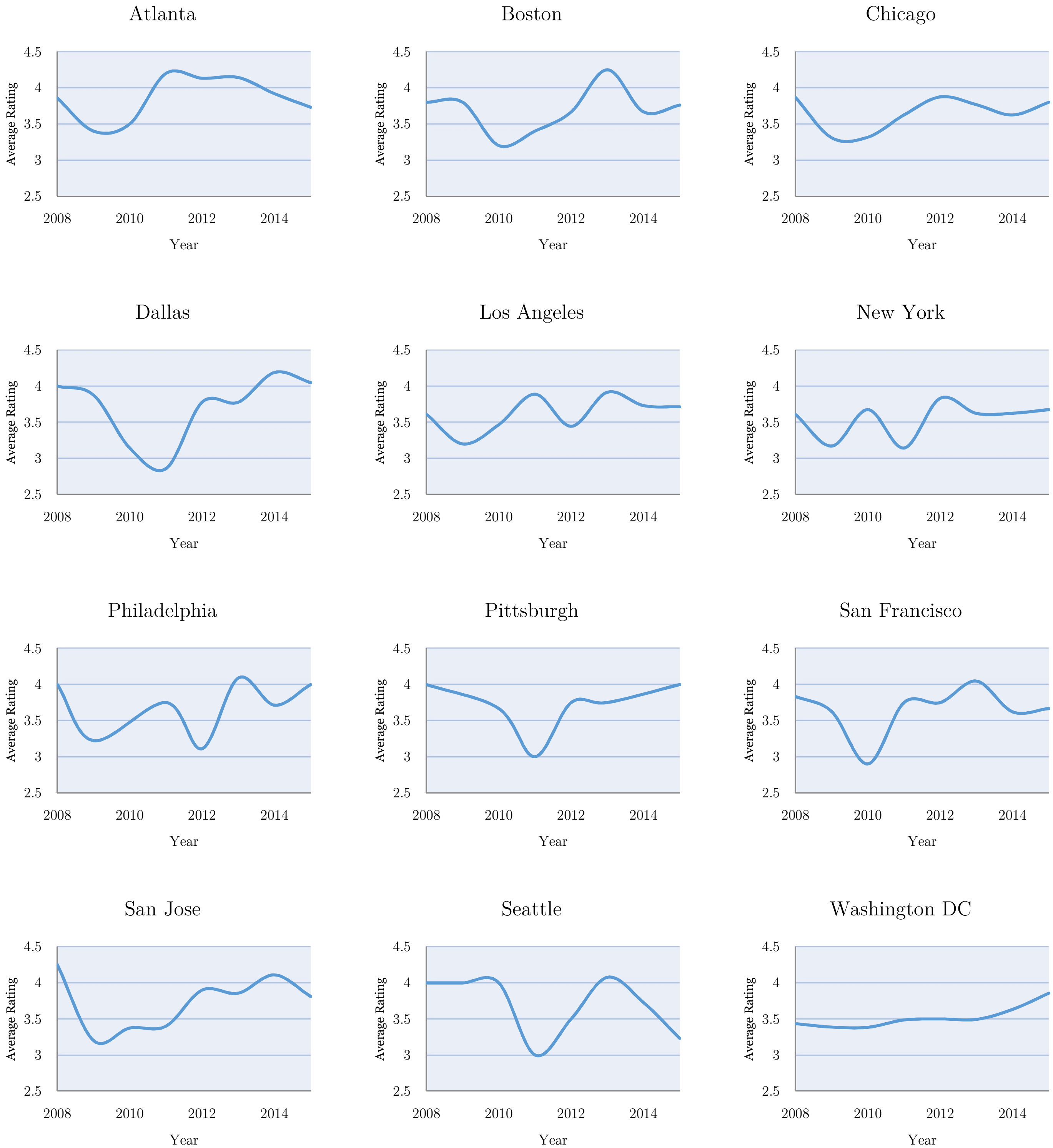


Figure 2.6. Employee satisfaction level over time at various Deloitte offices[15]



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[15] Job satisfaction level in this figure is calculated as the yearly average of individual ratings.

Figure 2.7a. Glassdoor individual rating distribution for all companies from 2008 to 2015

Figure 2.7b. Glassdoor individual rating distribution for audit firms from 2008 to 2015

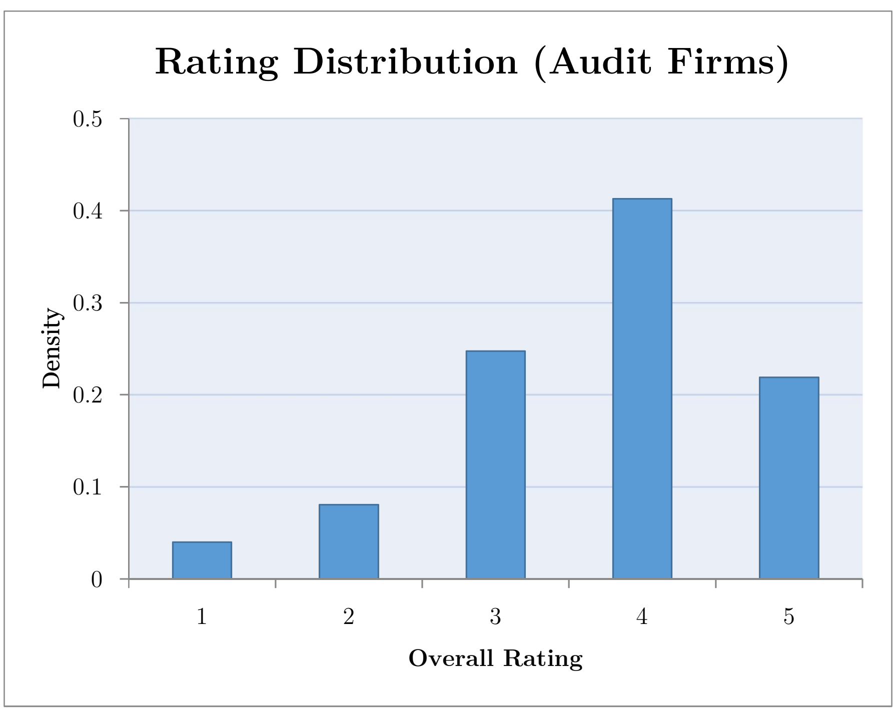

Table 2.1. Descriptive Statistics

Panel A. Glassdoor Dataset (2008 – 2015)

Dataset	No. firms	No. observations
Initial	223,489	2,434,177

Panel B. Audit Analytics Dataset (2008 – 2015)

Dataset	No. firms	No. auditors	No. observations
Initial	32,228	994	129,025

Panel C. Compustat Dataset (2008 – 2015)

Dataset	No. firms	No. observations
Initial	17,049	112,412

Panel D. Final sample (1983 -2015)

Dataset	No. auditors	No. firms	No. offices	No. Observations
Final Sample	33	1,932	149	5,681

Observations by year[16]	No. Observations
2008	875
2009	765
2010	520
2011	491
2012	792
2013	1,023
2014	985
2015	230
Total	5,681

[16] The number of observations in 2015 is significantly smaller because Glassdoor data at the time this paper was written only covered up to the month of October in 2015. Thus, all firms with fiscal year ends falling in the second half of the year are dropped, as there are no ratings to compute satisfaction levels.

Table 2.2. Summary Statistics

This table presents summary statistics about the main variables used in the paper. The summary statistics are computed using the Final Sample (see Table 2.1). Variable definitions appear in Appendix 2A.

	Mean	SD	25^{th}	Median	75^{th}
Satisfaction	3.6830	0.5670	3.3333	3.6923	4.0000
absDA	0.2628	0.5084	0.0287	0.0778	0.2460
DA	0.0760	0.5673	-0.0585	0.0114	0.1020
BigR	0.0435	0.2039	0.0000	0.0000	0.0000
LittleR	0.1366	0.3435	0.0000	0.0000	0.0000
AAER	0.0039	0.0621	0.0000	0.0000	0.0000
LogMC	6.9188	1.8607	5.6994	6.9485	8.2227
BM	0.5133	0.6860	0.2337	0.4304	0.7163
ROA	-0.0513	0.4143	-0.0355	0.0318	0.0718
Lev	0.5375	0.3851	0.3293	0.5079	0.6789
C_ratio	2.9076	3.0215	1.3315	2.0308	3.2834
OCF	0.0381	0.2612	0.0357	0.0822	0.1298
Career Opportunity	3.9554	0.5491	3.6000	4.0000	4.3333
Salary & Benefits	3.4297	0.5181	3.1429	3.4444	3.7500
Upper Management	3.5277	0.6046	3.1600	3.5000	4.0000
Work Life Balance	3.1212	0.7114	2.6250	3.0833	3.6000
Precipitation	32.7038	11.8424	24.0000	32.0536	40.0244

Table 2.3. Correlation Table

This table presents the correlation matrix of the main variables used in the paper. The correlation coefficients are computed using the Final Sample (see Table 2.1). Appendix 2A contains the descriptions of all variables.

		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)
(1)	*Satisfaction*	1.0000																
(2)	*absDA*	-0.0010	1.0000															
(3)	*DA*	-0.0101	0.6020	1.0000														
(4)	*BigR*	-0.0113	0.0107	0.0191	1.0000													
(5)	*LittleR*	0.0308	-0.0419	-0.0184	0.0456	1.0000												
(6)	*AAER*	-0.0151	-0.0183	-0.0047	0.1600	0.0340	1.0000											
(7)	*LogMC*	0.0706	-0.0766	-0.0421	-0.0912	0.0375	-0.0062	1.0000										
(8)	*BM*	0.0332	-0.1150	-0.0272	0.0298	0.0015	0.0058	-0.1890	1.0000									
(9)	*ROA*	0.0230	-0.1570	-0.0921	-0.0457	0.0438	-0.0080	0.3570	0.1130	1.0000								
(10)	*Lev*	0.0041	0.0282	0.0237	0.0266	0.0237	-0.0173	-0.0251	-0.3620	-0.4380	1.0000							
(11)	*C_ratio*	-0.0287	0.1950	0.1160	-0.0227	-0.0565	-0.0068	-0.1680	0.0160	-0.0409	-0.3550	1.0000						
(12)	*OCF*	0.0266	-0.1950	-0.1280	-0.0351	0.0520	-0.0094	0.3790	0.0832	0.8340	-0.2760	-0.1100	1.0000					
(13)	*Career Opportunity*	0.6780	0.0200	-0.0023	-0.0218	0.0226	-0.0211	0.0944	0.0200	0.0241	-0.0131	0.0111	0.0201	1.0000				
(14)	*Salary & Benefits*	0.5440	0.0076	0.0200	-0.0236	0.0051	0.0092	0.0057	0.0376	-0.0005	0.0112	-0.0062	0.0032	0.4630	1.0000			
(15)	*Upper Management*	0.7300	-0.0218	0.0058	-0.0031	0.0050	-0.0056	0.0234	0.0546	0.0101	-0.0047	-0.0099	0.0121	0.6200	0.4940	1.0000		
(16)	*Work Life Balance*	0.4890	-0.0598	0.0014	-0.0068	-0.0287	0.0046	-0.0625	0.1150	0.0151	0.0130	-0.0403	0.0221	0.2260	0.2880	0.5120	1.0000	
(17)	*Precipitation*	-0.1180	0.0675	0.0458	-0.0012	0.0036	-0.0090	-0.0546	0.0030	0.0135	-0.0175	0.0269	0.0087	-0.1200	-0.0988	-0.1170	0.0128	1.0000

Table 2.4. Job Satisfaction and Discretionary Accruals

This table presents OLS estimates to analyze the effects of job satisfaction on discretionary accruals. The dependent variables are *absDA* and *DA*, the absolute value of performance-matched discretionary accruals and signed discretionary accruals, respectively (Kothari et al. 2005). The independent variable of interest is *Satisfaction*, which is calculated as the average of all Glassdoor ratings over the audit period at the auditor office conducting the audit in consideration. The control variables are *LogMC* (Log of market capitalization), *BM* (Book to market ratio), *ROA* (returns on assets), *Lev* (Debt to assets ratio), *C_ratio* (current ratio), and *OCF* (operating cash flow scaled by total assets). Various sets of fixed effects are used to control for unobservable characteristics and/or trends at the city, industry, audit firm, and audit office level. Standard errors are clustered at the auditor office level, and reported in parentheses. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

	OLS Regressions							
	Dependent Variable: *absDA*				Dependent Variable: *DA*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Satisfaction	-0.0147*	-0.0174**	-0.0206**	-0.0255***	-0.0140	-0.0197*	-0.0200*	-0.0251**
	(0.00826)	(0.00859)	(0.00869)	(0.00927)	(0.0110)	(0.0103)	(0.0115)	(0.0116)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects:								
Industry-Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City-Year	No	Yes	Yes	Yes	No	Yes	Yes	Yes
AuditOffice	No	No	Yes	Yes	No	No	Yes	Yes
Auditor-Year	No	No	No	Yes	No	No	No	Yes
N	5,681	5,681	5,681	5,681	5,681	5,681	5,681	5,681
adj. R-sq	0.576	0.576	0.573	0.571	0.325	0.329	0.324	0.319

Table 2.5. Job Satisfaction and Restatements

This table presents logistic regression estimates to analyze the effects of job satisfaction on restatements. The dependent variables are *BigR* and *LittleR*, an indicator of a Big R restatement and an indicator of little r restatement, respectively. The independent variable of interest is *Satisfaction*, which is calculated as the average of all Glassdoor ratings over the audit period at the auditor office conducting the audit in consideration. The control variables are *LogMC* (Log of market capitalization), *BM* (Book to market ratio), *ROA* (returns on assets), *Lev* (Debt to assets ratio), *C_ratio* (current ratio) and *OCF* (operating cash flow scaled by total assets). Various sets of fixed effects are used to control for unobservable characteristics and/or trends at the city, industry, audit firm and audit office level. Standard errors are clustered at the auditor office level, and reported in parentheses. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

	Logistic Regressions							
	Dependent Variable: BigR				*Dependent Variable: LittleR*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Satisfaction	-0.286**	-0.399***	-0.420***	-0.470**	0.0607	-0.0737	-0.0430	-0.0306
	(0.132)	(0.134)	(0.163)	(0.188)	(0.0748)	(0.101)	(0.105)	(0.109)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects:								
AuditOffice	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City-Year	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Industry-Year	No	No	Yes	Yes	No	No	Yes	Yes
Auditor-Year	No	No	No	Yes	No	No	No	Yes
N	4,745	4,305	3,061	2,981	5,376	5,182	4,542	4,511
adj. R-sq	0.048	0.070	0.188	0.195	0.065	0.094	0.152	0.159

Table 2.6. Job Satisfaction and AAERs

The following table shows the logistic regression estimates of the effect of audit employee job satisfaction on the odds of receiving an AAER. The dependent variable is *AAER*, an indicator for receiving an AAER associated with the audit in question. The independent variable of interest is *Satisfaction*, which is calculated as the average of all Glassdoor ratings over the audit period at the auditor office conducting the audit in consideration. The control variables are *LogMC* (Log of market capitalization), *BM* (Book to market ratio), *ROA* (returns on assets), *Lev* (Debt to assets ratio), *C_ratio* (current ratio), and *OCF* (operating cash flow scaled by total assets). Various sets of fixed effects are used to control for unobservable characteristics and/or trends at the city, industry, audit firm, and audit office level. Standard errors are clustered at the auditor office level, and reported in parentheses. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

	Logistic Regressions			
	Dependent Variable: *AAER*			
	(1)	(3)	(4)	(5)
Satisfaction	-0.477*	-0.622**	-1.223**	-1.320**
	(0.263)	(0.303)	(0.499)	(0.578)
Controls	Yes	Yes	Yes	Yes
Fixed Effects:				
Industry-Year	No	Yes	Yes	Yes
City-Year	No	Yes	Yes	Yes
AuditOffice	No	No	Yes	Yes
Auditor-Year	No	No	No	Yes
N	5,681	479	313	299
Pseudo R-sq	0.039	0.189	0.239	0.231

Table 2.7. Job Satisfaction Effect – Instrument Variable Regressions

This table presents IV regression estimates of the job satisfaction effect on audit quality. Column (1) presents the first stage regression in which the dependent variable is *Satisfaction* and the instrument is *Precipitation*, the average precipitation at the audit office during the audit period. Columns (2) and (3) present the second stage regression in which the dependent variables are *absDA* and *DA*, the absolute value of performance-matched discretionary accruals and signed discretionary accruals, respectively (Kothari et al. 2005), and the independent variable of interest is the instrumented *Satisfaction*. The control variables are *LogMC* (Log of market capitalization), *BM* (Book to market ratio), *ROA* (returns on assets), *Lev* (Debt to assets ratio), *C_ratio* (current ratio), and *OCF* (operating cash flow scaled by total assets). Various fixed effects are used to control for unobservable characteristics and/or trends at the city, industry, audit firm, and audit office level. Standard errors are clustered at the auditor office level, and reported in parentheses. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

	First Stage	Second Stage	
Dependent Variable:	*Satisfaction*	*absDA*	*DA*
	(1)	(2)	(3)
Precipitation	-0.00417***		
	(0.00083)		
$\widehat{Satisfaction}$		-0.362*	-0.576*
		(0.212)	(0.333)
Controls	Yes	Yes	Yes
Fixed Effects:	Yes	Yes	Yes
Industry-Year	Yes	Yes	Yes
City-Year	Yes	Yes	Yes
AuditOffice	Yes	Yes	Yes
Auditor-Year	Yes	Yes	Yes
N	4,956	4,956	4,956
adj. R-sq	0.456	0.502	0.169
1st Stage F-stat		25.54	25.54

Table 2.8. Which Aspects of Job Satisfaction Matter for Audit Quality?

The following table shows the regression results of the effects of various aspects of job satisfaction on audit quality, namely *Career Opportunity*, *Salary and Benefits*, *Upper Management*, and *Work Life Balance*. Each independent variable of interest is calculated as the average of the corresponding Glassdoor ratings over the audit period at the auditor office conducting the audit in consideration. The dependent variable, audit quality, is measured either as *absDA*, the absolute value of performance-matched discretionary accruals (Kothari et al. 2005), *BigR*, an indicator equal to 1 if there is a Big R restatement associated with the audit, or AAER, an indicator equal to 1 if there is an *AAER* associated with the audit. The control variables are *LogMC* (Log of market capitalization), *BM* (Book to market ratio), *ROA* (returns on assets), Lev (Debt to assets ratio), *C_ratio* (current ratio), and *OCF* (operating cash flow scaled by total assets). All specifications include City-Year, Industry-Year, Auditor-Year, and Auditor Office fixed effects. Standard errors are clustered at auditor office level, and reported in parentheses. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

	OLS Regressions				Logistic Regressions				Logistic Regressions			
	Dependent Variable: *absDA*				Dependent Variable: *BigR*				Dependent Variable: *AAER*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Career Opportunity	-0.0187**				-0.316*				-2.250*			
	(0.00844)				(0.174)				(1.397)			
Salary and Benefits		-0.0169				-0.437**				0.174		
		(0.0108)				(0.184)				(0.552)		
Upper Management			-0.0151*				-0.391**				-2.931**	
			(0.00860)				(0.166)				(1.223)	
Work Life Balance				-0.0107				-0.592***				-0.831
				(0.00773)				(0.138)				(0.960)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects:												
Industry-Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City-Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
AuditOffice	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Auditor-Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	5,681	5,681	5,681	5,681	2,981	2,981	2,981	2,981	299	299	299	299
adj./pseudo R-sq	0.571	0.571	0.571	0.571	0.193	0.195	0.195	0.199	0.258	0.213	0.285	0.224

Table 2.9. Robustness - Results Controlling for Auditing Time in Days

This table presents IV and logistics regression estimates controlling for the number of days it takes to complete each audit. The dependent variable is either *absDA*, *DA*, *BigR,* or *AAER*. The independent variable of interest is *Satisfaction*, which is calculated as the average of all Glassdoor ratings during the audit period at the auditor office conducting the audit in consideration. Days is a variable capturing the number of days between the fiscal year end and the auditor sign date. The control variables are *LogMC* (Log of market capitalization), *BM* (Book to market ratio), *ROA* (returns on assets), *Lev* (Debt to assets ratio), *C_ratio* (current ratio), and *OCF* (operating cash flow scaled by total assets). Various fixed effects are used to control for unobservable characteristics and/or trends at the city, industry, audit firm, and audit office level. Standard errors are clustered at the auditor office level, and reported in parentheses. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

	IV	IV	Logit	Logit
Dependent Variable	*absDA*	*DA*	*BigR*	*AAER*
	(1)	(2)	(3)	(4)
Satisfaction	-0.364*	-0.573*	-0.440**	-1.390**
	(0.216)	(0.335)	(0.188)	(0.721)
Days	-0.0001	0.0001	0.0042	0.0254**
	(0.0002)	(0.0003)	(0.0029)	(0.0101)
Controls	Yes	Yes	Yes	Yes
Fixed Effects:				
Industry-Year	Yes	Yes	Yes	Yes
City-Year	Yes	Yes	Yes	Yes
AuditOffice	Yes	Yes	Yes	Yes
Auditor-Year	Yes	Yes	Yes	Yes
N	4,956	4,956	2,981	299
adj./pseudo R-sq	0.501	0.170	0.199	0.269
1st Stage F-stat	25.03	25.03		

Table 2.10. Robustness - Separating Job Satisfaction of Audit Employees from Client Firm Employees

This table presents IV and logistics regression estimates limited to the subsample containing observations in which the audit office and the client firm are not in the same location. Column (1) presents the first stage regression in which the dependent variable is *Satisfaction* and the instrument is *Precipitation*, the average precipitation at the audit office during the audit period. Columns (2) and (3) present the second stage regression in which the dependent variables are *absDA* and *DA*, the absolute value of performance-matched discretionary accruals and signed discretionary accruals, respectively (Kothari et al. 2005), and the independent variable of interest is the instrumented *Satisfaction*. The control variables are *LogMC* (Log of market capitalization), *BM* (Book to market ratio), *ROA* (returns on assets), *Lev* (Debt to assets ratio), *C_ratio* (current ratio), and *OCF* (operating cash flow scaled by total assets). Various fixed effects are used to control for unobservable characteristics and/or trends at the city, industry, audit firm, and audit office level. Standard errors are clustered at the auditor office level, and reported in parentheses. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

	First Stage	Second Stage	
Dependent Variable:	*Satisfaction*	*absDA*	*DA*
	(1)	(2)	(3)
Precipitation	-0.00545***		
	(0.00908)		
$\widehat{Satisfaction}$		-0.342*	-0.500*
		(0.197)	(0.270)
Controls	Yes	Yes	Yes
Fixed Effects:	Yes	Yes	Yes
Industry-Year	Yes	Yes	Yes
City-Year	Yes	Yes	Yes
AuditOffice	Yes	Yes	Yes
Auditor-Year	Yes	Yes	Yes
N	3,554	3,554	3,554
adj. R-sq	0.467	0.535	0.261
1st Stage F-stat		36.76	36.76

Chapter 3

The Disciplining Effect of Peers: Evidence from the Timing of Earnings Announcements

3.1 Introduction

In the first quarter of 2012, *Forbes* noted in an article that Wells Fargo would move up its earnings announcement date to coincide with that of competitor JPMorgan Chase, which appears to be “yet another way the two banking giants are competing for attention.”[17] Leading up to 2012, JPMorgan Chase accelerated its earnings announcements from 16 to 13 days after the end of a fiscal quarter, while Wells Fargo also accelerated from 22 to 13 days. Since then, the two banks have almost always announced earnings on the same date (See figure 3.1). Whether this correlation in timing choices is a product of common shocks or a strategic reporting game in which peer influence plays a central role remains unclear.

The extant literature finds that firms engage in strategic timing of earnings announcements to influence investor perceptions of their earnings news but offers limited insights into whether these timing decisions are independent of their peers’ decisions. For example, good news is typically reported early to maximize positive market reactions, whereas bad news is often reported late, delayed until Fridays, or announced after trading hours to minimize negative reactions (Kross 1981; Givoly and Palmon 1982; Dellavigna and Pollet 2009; deHaan, Shevlin, and Thornock 2015; Livnat and Zhang 2015; Johnson and So 2017). However, because a firm’s earnings announcements can influence the market perception of another firm (Firth 1976; Foster 1981; Dye and Sridhar 1995), firms may have an incentive to consider peer earnings announcement timing when making their own timing decisions.

In this paper, I investigate the existence and magnitude of peer effects in earnings announcement timing, as well as the mechanisms through which these effects operate. Further, I characterize the benefits and costs associated with peer effects that are relevant to policymakers concerned with the timeliness of earnings releases.

[17] “Wells Fargo Will Announce Earnings Early To Share Spotlight With JPMorgan,” *Forbes* 2012.

To guide my empirical analysis, I rely on classic disclosure theory (Dye 1985; Jung and Kwon 1988) to provide a rationale and a mechanism for peer effects. According to this theory, uncertainty about managers' information endowments makes investors unable to distinguish between managers who have not disclosed because they have bad news and those who have yet to receive earnings information. Thus, the former can hide behind the latter and enjoy a higher market valuation rather than disclosing the bad news and seeing a sharp drop in price. The disclosure strategy is threshold-based, in that only news better than or equal to a threshold value is disclosed. In a dynamic multifirm setting (Dye and Sridhar 1995), investors can update their beliefs concerning the probability that a firm has finished working on its earnings report (i.e., been endowed with information) conditional on how many peer firms have already been able to do so. As the information endowment uncertainty decreases in the number of firms that have announced earnings, the disclosure threshold will decrease accordingly. This relationship leads to my main theoretical prediction: firms will announce earnings early if peer firms announce earnings early.

Despite being intuitive, estimating peer effects in this context is empirically challenging. First, there is Manski's reflection problem: firm A affects firm B directly and also indirectly through B's effect on A (Manski 1993). This simultaneity makes it challenging to pin down whose changes are driving the other's and the magnitude of the effect. Second, there could be unobservable reasons for firms in the same industry to select themselves into a certain temporal order, or some unobservable common shocks affecting many firms at the same time. Therefore, ordinary least squares regressions do not produce unbiased estimates. To overcome these challenges, I rely on the SEC's regulation on periodic report deadlines as a source of plausibly exogenous variation in earnings announcement timing. Under this requirement, firms with a public float of less than $75 million have 45 days to file their quarterly reports, whereas firms with floats above this cutoff have 40 days instead. This distinction allows for a fuzzy regression discontinuity design in which being above the cutoff serves as an instrument for the timing of a peer firm's announcement.

To see the intuition behind the regression discontinuity, consider a focal firm A with two peer firms B and C; we are interested in how variation in the timing of firm B's and firm C's earnings announcements affects firm A's timing choice. However, changes in firm B's or C's timing of earnings announcements are also dependent on firm A's choice, because A is also their peer (Manski's reflection problem), and further, common shocks could affect all three firms' timing decisions. Suppose that firm B's public float is just above the cutoff imposed by regulation (e.g., \$76 million), whereas firm C's public float is just below the cutoff (e.g., \$74 million). The two peer firms are, thus, very similar except that firm B has a stricter reporting requirement than firm C. The resulting differences in firm B's and firm C's earnings announcement timing can be attributed to the regulation, as opposed to firm A's timing or common shocks. Hence, the variation in earnings announcement timing induced by the regulation is plausibly exogenous.

I define industry peer groups using three-digit SIC codes, considering two firms as peers if they have the same industry code and fiscal quarter end. Measuring timeliness using the number of days between a fiscal quarter end and an actual announcement date, I show evidence for statistically and economically significant peer effects in the timing of earnings announcements. In particular, my preferred peer effects estimate suggests that if eight peer firms report one day sooner, then the focal firm will report one day sooner. The result is robust when I measure timeliness as the deviation from the firm's reporting strategy in the same quarter of the previous year.

The mechanism from disclosure theory generates three additional predictions indicating that peer effects serve as a *disciplining* force deterring the strategic delay of bad earnings news. First, peer effects are stronger on firms trying to hide bad news. The intuition is that, compared to firms with bad news, firms with good news already have the incentive to announce earnings early; thus, peer pressure is not going to cause them to announce earnings significantly earlier than they already intend. Figure 3.2 provides a visual proof of this intuition. Defining bad news as missing the analyst consensus forecast, I find evidence consistent with this prediction. The estimate suggests that peer effects, on average, are 26% stronger for firms with bad news.

Second, I predict that peer effects are more pronounced in larger industries with higher numbers of firms. This is because when there is a high number of firms in an industry, the number of firms that have announced earnings by a given date is higher on average. This drives the disclosure threshold down faster and forces managers to disclose bad news sooner. Using the number of firms in an industry as a proxy for industry size, I find that peer effects, on average, are 34% more pronounced in larger industries. This is an economically significant magnitude, and it suggests that in smaller industries, firms have more leeway to strategically delay earnings news, as there are fewer companies to be compared to.

Third, I predict that peer effects are weaker when firms experience monitoring from other sources. The intuition is that both peer effects and monitoring can reduce strategic delays of earnings news, yet the upper bound for disclosure timing is firms' announcing earnings the moment this information becomes available to them. I use public float and the fraction of institutional investors as proxies for the intensity of monitoring from other sources. I find that firms that experience a higher level of monitoring are roughly 31% less affected by peer effects, consistent with the disciplining mechanism prediction.

Next, I investigate another channel through which peer effects in earnings announcement timing operate: the information transfers from peer firms' announcements (Firth 1976; Foster 1981; Han and Wild 1990; Freeman and Tse 1992; Thomas and Zhang 2008). The idea is that, for example, if a peer firm's earnings announcement contains information that negatively (positively) affects a non-announcing firm, the market will lower (raise) its valuation of the latter. Consequently, the disclosure threshold of the non-announcing firm would decrease (increase), causing it to accelerate (delay) its announcements. Using the market-adjusted cumulative abnormal returns of the non-announcing firm around the peer's announcement date as a proxy for the information transfer, I find that peer effects are stronger (weaker) when there is negative (positive) information transfer.

Finally, I investigate the potential regulatory spillover costs associated with peer effects in earnings announcement timing that could be relevant for policymakers. I hypothesize that because of peer effects firms may face an increase in accounting-related costs even if they are not directly affected by the regulation. To test this hypothesis, I use audit fees as a proxy for accounting-related costs. I find that, on average, audit fees for a focal firm increase by approximately 1% per peer firm affected by the SEC regulation. Part of the increase in audit fees is because of the pressure to catch up with peer firms that are forced by the regulation to announce earnings early. On the other hand, audit fees may also increase due to auditors becoming busier as the regulation causes more firms to announce earnings sooner (Duguay, Minnis, and Sutherland 2018; López and Peter 2011). The economic magnitude of this spillover effect is significant, considering the direct effect of the regulation on the affected firms is a 25% increase in audit fees.

My study contributes to the literature on strategic timing of earnings announcements (e.g., Kross 1981; deHaan et al. 2015) and peer effects in corporate behaviors (e.g., Beatty, Liao, and Yu 2013; Leary and Roberts 2014). First, I bridge the two strands of literature by showing evidence that firms strategically choose earnings announcement timing based on peer firms' timing decisions. Because bad news is sometimes reported early,[18] an implication of my study is that peer

[18] In my sample, 30% of earnings news reported between 10 and 15 days from a fiscal quarter end is bad news (i.e., missing analyst consensus forecasts).

effects could be an important missing factor that explains this phenomenon. That is, reporting bad news early could be due to peer firms' early reporting.

Second, the paper also sheds light on the underlying mechanisms through which peer effects operate. I show that these effects are a result of a disciplining mechanism that deters firms from strategically delaying earnings news. I also show that another mechanism underlying firms' responses to peers' timing is dependent on the effect of information transfers coming from peer earnings announcements. Third, to my knowledge, my paper is the first to use a novel setting that makes use of regulatory exogenous changes in earnings announcement timing to explicitly examine the causal effects of peer earnings announcement timing.

Finally, by utilizing the SEC regulation to reveal the benefits and the costs associated with peer effects, my paper also speaks to the externality of regulations on financial reporting that is relevant for policymakers (e.g., Duguay et al. 2018). On the one hand, my findings suggest that regulations on timing for some firms, through peer effects, can promote the timeliness of earnings information in a whole industry. This is important because accounting information must be made available in a timely manner to be relevant to investors.[19] On the other hand, such regulations can also impose spillover costs on firms that are not directly affected. These results may inform regulators should there be future changes to reporting deadlines.

The rest of this chapter proceeds as follows: Section 3.2 discusses related literature and hypothesis development. Section 3.3 describes the institutional background regarding the SEC's regulation, identification strategy, and the data. Section 3.4 presents the empirical findings. Section 3.5 discusses the policy implications, and Section 3.6 concludes.

[19] FASB's Concept Statement No. 8 and SEC Release No. 33-8089.

3.2 Related Literature and Hypothesis Development

3.2.1 Related Literature

3.2.1.1 Literature on the Timing of Earnings Announcements

A large literature on the strategic timing of earnings releases dates back to the 1980s. However, most studies in this literature assume that timing decisions are independent of the influence of peer firms. For example, early theoretical and empirical research finds that managers tend to delay the reporting of bad news and accelerate the reporting of good news (Bowen et al. 1992; Chambers and Penman 1984; Givoly and Palmon 1982; Penman 1984; Kross 1981; Kross and Schroeder 1984; Trueman 1990). Because managers are concerned about negative market reactions to bad news, they have an incentive to delay such news in hopes of influencing market perceptions. Compensation and career concerns are viewed as the main managerial motivations for this behavior. Executive compensation is directly tied to stock performance (Healy and Palepu, 2001). Moreover, managers' track records on a variety of corporate decisions form their reputation, potential capability, and suitability as a manager. Thus, they would have an incentive to take strategic actions in order to avoid accumulating negative shocks and build a good reputation (Fama 1980; Holmstrom and Ricart I Costa 1986; Holmstrom 1999; Gibbons and Murphy 1992).

Recent research examines how managers opportunistically choose the timing of earnings releases to minimize negative reaction. These studies offer a behavioral explanation for why and how managers can strategically time their earnings releases. They find that bad news tends to be released either on Fridays or outside of trading hours, when market scrutiny is believed to be less intense (Bagnoli, Clement, and Watts 2005; Dellavigna and Pollet 2009; Michaely, Rubin, and Vedrashko 2016; deHaan et al. 2015; Lyle, Rigsby, Stephan, and Yohn 2017; Gennotte and Trueman 1996). The idea is that when market attention is low, the impact of negative news may be less severe. Firms may want investors to sit on the information over the weekend before making hasty sell decisions. Even if the market can fully unravel the news, the managers still prefer a gradual decrease in price as opposed to a sharp drop, which may trigger more negative attention and induce panic selling. Using data on the rescheduling of earnings release dates, Livnat and Zhang (2015) and Johnson and So (2017) find that managers who reschedule earnings announcements to an earlier date are more likely to report good news, whereas managers who reschedule to a later date tend to report bad news. Supporting the inattention hypothesis, these papers find that investors, upon observing a rescheduled date, fail to unravel the information embedded in this signal.

In addition to the market inattention argument, the literature also points out alternative explanations for the observed pattern of delayed bad news. For example, Doyle and Magilke (2009) and Michaely, Rubin, and Vedrashko (2014) find that managers may decide to announce earnings outside of trading hours to allow investors time to absorb the information and to level the playing field amongst investors. Michaely et al. (2016) and Kolasinski and Li (2010) show that insider trading is another reason for the opportunistic timing of earnings releases. Crabtree and Kubick (2014) find that tax avoidance is associated with less timely annual earnings releases. Brown et al. (2012) argue that delays of pro forma earnings releases could occur when managers need more time to manipulate earnings.

My study is also related to that of Su (2015), a study of the effect of bank monitoring on the timing of earnings announcements. The author finds that in the presence of a bank lending relationship, firms are less likely to announce earnings later than they did in the prior year. My study extends this finding by showing that peer effects are also a disciplinary force preventing the strategic delays of earnings news and that peer effects are less impactful in the presence of other, more direct forms of monitoring.

3.2.1.2 Literature on Corporate Peer Effects

Despite the observation made by Givoly and Palman (1982) that annual earnings reporting patterns seem to be related to intra-industry patterns, scant empirical evidence documents whether these correlations are a product of a strategic game played among industry peers. Moreover, research in economics, finance, and accounting has documented in various contexts that firms tend to mimic behaviors of their peers: Albuquerque (2009) (relative performance evaluation); Beatty et al. (2013) and Chen and Ma (2017) (investment decisions); Bird et al. (2018) (corporate tax paying); Gong, Li, and Shin (2011) and Lewellen (2017) (CEO compensation); Leary and Roberts (2014) (financing decisions); Grennan (2019) (dividend payments). Valerie (2016) shows that false financial statements can distort peer firms' decisions. Therefore, because the timing of earnings releases conveys some information regarding the true fundamentals and is subjected to managerial discretion, managers may strategically make timing decisions conditional on the influence of peer firms.

My study is perhaps most closely related to that of Tse and Tucker (2010). Using duration analysis, they study the herding tendency of managers of firms in the same industry when they issue management guidance. Tse and Tucker find that companies tend to issue bad news warnings soon after their peers issue their warnings but do not exhibit the same herd behaviors for good news alerts. They argue that if managers believe the market is less likely to hold them responsible when other firms also issue bad news, they would have the incentive to cluster their guidance with their peers'. Doing so makes it appear that an exogenous factor is negatively affecting many firms in their industry, minimizing the blame for reporting bad news.

My study complements the findings in Tse and Tucker (2010) by demonstrating that peer influence is present in both voluntary and mandatory disclosures. My study is also different from theirs in the mechanism through which peer firms' behaviors induce changes in a focal firm's behavior. Whereas they find herding evidence for management earnings warnings due to strategic clustering of bad news (but not good news), I show that it is the disciplinary mechanism that causes peer effects in earnings announcement timing. That is, regardless of the nature of the news, investors will interpret disclosure delays as implying bad news. Thus, to avoid being incorrectly judged by the market because peer firms have released good news early, the non-announcing firms would have an incentive to announce earnings earlier than they would like, even if they have bad news.

3.2.2 Hypothesis Development

The hypotheses of this paper are built on classic disclosure theory pioneered by Dye (1985), Jung and Kwon (1988), and Dye and Sridhar (1995). The central argument from the theory is that managers can delay disclosure of bad news as long as uncertainty as to whether the managers actually have information remains sufficiently high. This uncertainty about managers' information endowments allows bad firms to hide behind those who simply have yet to receive information. The optimal disclosure strategy is threshold-based: withhold if the news is worse than the relevant threshold and disclose otherwise. In the earnings announcement context, the information signal is the earnings news, and the uncertainty of the information endowment is the uncertainty about when a firm finalizes its earnings report.

The optimal threshold strategy changes over time in a dynamic and multifirm setting. Investor posterior belief about the probability that a firm has its earnings report ready is updated as more and more peer firms disclose their earnings.[20] As long as this probability is increasing with the number of firms already disclosed, the disclosure threshold will decrease accordingly. The intuition is that it is more difficult for a firm to delay an earnings announcement when the market

[20] The uncertainty in a dynamic context is whether the accounting department has finalized the accounting reports and made sure that earnings numbers are ready to be announced.

believes that it already has its earnings information. Thus, in the presence of peer firms, the disclosure threshold decreases as the number of firms that have already disclosed increases. Thus, if more firms disclose their earnings information early, the thresholds of other firms decrease sooner, inducing them to disclose announce earnings sooner. This causes firms that would otherwise delay bad news to disclose their earnings information earlier. On the other hand, if more firms disclose their earnings late, then the thresholds of the non-announcing firms decrease more slowly, allowing them to keep delaying their earnings announcements.[21] This logic leads to the first hypothesis:

> ***H1****: Peer effects in the timing of earnings announcements exist: Firms announce earnings early if their peers announce earnings early.*

The reasoning above implies that peer timing of earnings announcements can be viewed as an implicit disciplining force deterring firms from strategically delaying their news. To provide additional support for this argument, I derive three additional hypotheses using the same logic from disclosure theory.

First, I predict that firms with bad earnings news suffer more from peer effects. The intuition is illustrated in figure 3.2. The key observation is that the optimal disclosure threshold decreases over time because information endowment uncertainty naturally decreases over time. However, in the presence of peer firms, the threshold will decrease at a faster rate due to investors' updating their beliefs on the basis of how many firms have already disclosed. If a signal is in the good news

[21] Some firms have a set announcing strategy, such as the first Tuesday of February. In this context, setting a late announcement strategy can signal firm types. Bad-type firms are more likely to announce earnings late. For example, they may anticipate a higher probability of having bad earnings news in the future, engage in tax avoidance (Crabtree and Kubick 2014), or manipulate earnings (Brown, Christensen, and Elliott 2012). The disclosure theory intuition for peer effects remains similar: firms choose a pre-set strategy with peer firms in mind in which the later the date, the worse the firm appears to the market. When peer firms move their earnings announcements to earlier dates, investors will revise their belief about whether the current pre-set announcement strategy of the focal firm is considered late.

region, peer effects will accelerate the reporting date by the amount represented by the blue arrow. If a signal is in the bad news region, peer effects will accelerate the reporting date by the amount represented by the red arrow. Since the threshold line given peer effects is steeper than the one with no peer effects, it must be that bad news firms experience stronger pressure from peer firms. The intuition is that because firms with good news already have an incentive to report as early as they can, adding peer effects into the equation is not going to make them report significantly earlier. Firms with bad news, in contrast, want to report late. Thus, peer effects would cause them to report much earlier than they would prefer. The second hypothesis is stated as follows:

> ***H2****: Firms with bad earnings news suffer more from peer effects when making timing decisions.*

Second, if the implicit disciplining mechanism is true, I would expect peer effects to be stronger in larger industries. Intuitively, a firm is subject to more comparisons when there are more firms in its industry. When there are more firms in an industry, number of firms that have already announced earnings by a certain day is higher on average. Consequently, the disclosure thresholds of the remaining firms will decrease faster, forcing them to announce earnings sooner. The third hypothesis is stated as follows:

> ***H3****: Peer effects are stronger in larger industries.*

Third, I argue that peer effects play a less significant role in disciplining strategic timing behaviors in the presence of other direct forms of monitoring. The intuition is simple. If monitoring is perfect, strategic delays are not possible. Consequently, we would expect firms to immediately announce their earnings as soon as they have the information. Thus, disclosure timing has a lower bound, the date of information receipt (i.e., when a firm's accounting department finalizes its earnings report). Increasing monitoring, therefore, must be of diminishing effectiveness because of this lower bound. Thus, the role of peer effects in deterring strategic timing behaviors is less pronounced when other forms of monitoring are also in place. This leads to the final hypothesis:

H4: *Peer effects are weaker when firms experience other forms of monitoring.*

3.3 Research Setting and Data

3.3.1 Identification Strategy and SEC Regulation

While intuitive, estimating peer effects empirically is challenging because the timing of earnings announcements is an endogenous decision. Companies in the same industry may strategically select into certain temporal positions because of unobservable factors. Furthermore, a firm's timing decision may be an input to another firm's timing decision, and vice versa. This is known in the peer effects literature as the reflection problem (Manski, 1993). The simultaneity underlying Manski's reflection problem makes it difficult to pin down exactly whose action is driving another's action, and by how much. Thus, I exploit the SEC's regulation on reporting deadlines to overcome these complications.

Under the regulation, all "large accelerated filers" (companies with public float[22] values greater than or equal to $700 million) must file their annual reports within 60 days of their fiscal year-ends. All "accelerated filers" (companies with public float values greater than or equal to $75 million but less than $700 million) must file their annual reports within 75 days of their fiscal

[22] "Public float" refers to the portion of firm shares owned by public investors instead of company affiliates. A shareholder is deemed a company affiliate if she or he is an officer, a director, or an owner of at least 10% of the total number of the company's shares (SEC Release No. 33-7391). The market value of public float used for filing status determination is based on the stock price of the company as of the last trading day of the previous second fiscal quarter. Filing status is updated in every fourth quarter. For example, filing status for 2010Q4, 2011Q1, 2011Q2, and 2011Q3 were determined using stock price on the last trading day of 2010Q2.

year-ends. All other filers[23] have 90 days to file their annual reports. For quarterly reports, the deadline is 40 days for both accelerated filers and large accelerated filers, whereas it is 45 days for nonaccelerated filers (see figure 3.3). This unique feature of the regulation allows use of a fuzzy regression discontinuity design to exogenously shock the earnings announcement timing of the firms subjected to stricter reporting deadlines. The design is fuzzy because there is no absolute guarantee of compliance, despite the costly market consequences a firm might face if it fails to report by its deadline (Alford, Jones, and Zmijewski 1994; Dee, Hillison, and Pacini 2010; Impink, Lubberink, van Pragg, and Veenman 2011).

I focus on the first cutoff at $75 million in this paper because both accelerated and large accelerated filers are subjected to the same deadline requirement for quarterly earnings reports. The cutoff serves as a quasi-natural experiment that separates similar firms into two groups, one just above and one just below the threshold. Firms slightly above the threshold receive the treatment (i.e., stricter deadlines), whereas firms slightly below the threshold do not. Because treatment status is determined solely by public float, it is important to nail down the natural relationship between public float and the time taken to announce earnings. Therefore, to estimate the peer effects, I employ the following two-stage instrumental fuzzy regression discontinuity design,

$$Days_{jq} = \beta_0 + \beta_1 Treat_{jq} + f(PublicFloat_{jq}) + \rho X_{iq} + \mu Y_{jq} + FixedEffects + u_{jq}, \qquad (1)$$

and

$$Days_{iq} = \beta_0 + \beta_2 \widehat{Days}_{jq} + g(PublicFloat_{jq}) + \gamma X_{iq} + \delta Y_{jq} + FixedEffects + \varepsilon_{iq}, \qquad (2)$$

[23] Firms with public float values below $75 million have various filing statuses: nonaccelerated filers, smaller reporting companies, and emerging growth companies. Henceforth, for brevity they are collectively referred to as nonaccelerated filers because they are subject to the same deadline requirements.

for all firm pairs $((i, j),\ i \neq j)$ in the same three-digit SIC code and same fiscal period end. $Days_{iq}$ is the number of days from firm i's fiscal quarter end to its earnings announcement date; $Days_{jq}$ is the number of days from firm j's fiscal quarter end to its earnings announcement date; $Treat_{jq}$ is a dummy variable equal to 1 if firm j is either a large accelerated filer or an accelerated filer at the time of fiscal quarter q, and 0 otherwise; $PublicFloat_{jq}$ is firm j's public float used to determine its filing status for fiscal quarter q; and $f(.)$ and $g(.)$ are two flexible polynomials used to accurately capture the relationship between *PublicFloat* and *Days*. The preferred specification uses cubic fits based on the Akaike information criterion. The results, however, are robust to different choices of polynomial fits. From this point forward, firm i is the focal firm, while firm j is the peer firm (in regression tables its variables have a *_p* suffix). To allow for potential correlation of the error terms of observations from the same firm and observations with the same peer firm, standard errors are two-way clustered at the focal firm and the peer firm level.

Given that the relevant cutoff for earnings announcement timing is at $75 million, $Treat_{jq}$ is equivalently defined as an indicator equal to 1 if firm j's public float value is greater than or equal to $75 million, and 0 otherwise. X_{iq} and Y_{jq} are two vectors of controls for firm i and firm j, respectively. The controls include firm fundamentals such as the log of market capitalization, book to market ratio, leverage, and returns on assets, as well as an indicator for late reporting cases. I also include year-quarter fixed effects to control for time trends and differences between the four quarters that may have an impact on reporting lags, as well as industry fixed effects to control for time-invariant heterogeneity at the industry level.

Because the regulation is intended for the actual filings of 10-Ks and 10-Qs, it is not obvious if it will affect earnings announcement timing, which typically occurs beforehand. From a theoretical standpoint, affected firms, even if they were able to comply with the stricter deadline before being affected, have to operate knowing that their margin of safety for hitting the deadline is tighter. Thus, it is riskier for them to maintain the same accounting technology, as random shocks such as natural disasters or accidents may cause them to miss the stricter deadline. This leads to an incentive to improve their accounting technology to make sure that in the worst possible scenario they would still be able to report by the tighter deadline. The result of such changes is a higher probability of completing accounting reports sooner, which leads to a higher probability of announcing earnings sooner. Figure 3.4 shows changes in announcement lags near the cutoff. There is a difference of three days between firms slightly below and slightly above the cutoff, initial evidence supporting the theoretical prediction.

The estimation of β_1 is a direct test for this relevance requirement of the instrument: the regulatory treatment must have a statistically significant effect on the explanatory variable of interest, $Days_{jq}$. The fitted value of $Days_{jq}$ obtained from running the first-stage regression should reflect only the variation in $Days_{jq}$ caused by the regulatory treatment of firm j's own public float. Consequently, using $\widehat{Days}_{jq}$ for the peer effects regression mitigates the endogeneity concern and Manski's reflection problem. The coefficient β_2 shows the existence of peer effects because it reflects how much sooner (later) firm i would announce its earnings in response to a 1-day acceleration (delay) of one of its peers' earnings announcements.

The regression discontinuity design breaks down if the firms are able to manipulate their public float values to get into the group with favorable treatment. This would cause selection bias because the firms just below and just above the threshold are no longer quasi-similar. McCrary (2007) offers a test to check for potential manipulation of a running variable. The intuition of the test is that if there is indeed manipulation of public float, a density plot of public float would show a clear discontinuity around the threshold, with a surprisingly high number of firms in the group with the favorable treatment and a surprisingly low number of firms in the group with the unfavorable treatment. Figure 3.5 shows the result of McCrary's density test. Since the density function seems smooth near the threshold, the figure suggests that public float manipulation is insignificant in the sample period.[24]

The SEC, in an attempt to prevent manipulation, imposes an additional rule that accelerated filers will not lose their status immediately if their public float falls below $75 million dollars. To revert to nonaccelerated status, the firm's public float must fall below $50 million. This requirement offers greater confidence for my identification strategy using the regulation as a shock. However, because the number of such instances is very low (roughly 6% of the total sample), I exclude all such observations from the sample to ease generating clean regression discontinuity graphs. The results are quantitatively similar when these observations are included.

3.3.2 Data and Sample Construction

I obtain relevant accounting data, including the dates of earnings announcements, from Compustat. Market data are obtained from the Center for Research in Security Prices (CRSP). Data on companies' public floats are taken from S&P Capital IQ. Supplementary sources of data include audit fees and filer status from Audit Analytics and analyst forecasts from the Institutional Brokers' Estimate System (I/B/E/S). The sample starts for observations with fiscal years ending

[24] It is difficult for companies to manipulate public float, due to the definition of the variable and its dependence on stock price on a specific date. There has been some evidence of manipulation in the earlier days of the focal regulation, from 2002 to 2006, when the SEC continuously amended deadline requirements (Illiev 2010; Gao 2016). Firms had little time to react to the announced regulations and were uncertain about future changes. During this period, accounting firms and law firms were also forced to change their operation and schedules because of the regulation (Lambert, Jones, Brazel, and Schott Showalter 2017). Therefore, there were more incentives to manipulate public float then. The sample for this study covers 2007 to 2016, a period in which the regulation deadline had been solidified and remained the same. This offers an explanation as to why firms have less incentive to manipulate public float in this period, which is reflected in the density plot.

in 2007 to 2016. This definition ensures that the SEC's regulation on reporting deadlines remains unchanged during the sample period.

To construct the sample, I first divide the firms into different groups. Two firms are in the same group if they have same industry definition and the same fiscal quarter end date. Following prior literature on corporate peer effects (e.g., Beatty et al. 2013, Leary and Roberts 2014, Grennan 2019), I define peer groups on the basis of three-digit SIC codes.[25] I then pair each firm *i* in an industry-quarter group with a firm *j, a* different firm in the same industry-quarter group, to create the peer sample, the main sample used in my analysis. (See figure 3.6 for a graphical illustration of the data.) This method differs from that in most peer effects studies, which use peer averages instead. However, because the regulatory treatment happens at the individual firm level, I opt to use the former approach. Thus, the results are interpreted as showing how firm *i*'s behavior changes if one of its peers reports one day sooner, instead of how firm *i*'s behavior would change if all of its peers reported one day sooner.

Although the "stacking" process yields no identical rows of data, it may bias the estimation of the regulatory effect in the first stage regression. In the example of figure 3.6, firm c appears three times on three different rows (a-c, b-c, and d-c). The first stage regression, however, will take into consideration the timing of firm c on the treatment status of firm c three times. Consequently, observations are inadvertently given more weight depending on the number of firms in each industry group. To account for this, all regressions estimated with the first stage regressions are weighted using the inverse number of firms in each industry group.

3.3.3 Descriptive Statistics

[25] Another advantage of using SIC classification is that prior research finds evidence of information transfers of earnings announcements at this level (Freeman and Tse 1992; Foster 1981; Thomas and Zhang 2008; Yip and Young 2012).

Table 3.1 shows the summary statistics of the variables used in the sample. The table demonstrates that the average number of days it takes for a firm to announce earnings is approximately 34, and the median is 32. The standard deviation is approximately 13, indicating that some firms are able to announce earnings as soon as 21 days after the quarter end and others report after the deadline. The *Treat* variable has an average of 0.91, indicating that roughly 91% of firms in the sample have public floats of no less than $75 million. The reason for this is that the sample is merged with analyst data to obtain analyst consensus forecasts, and not all firms are followed by analysts, especially smaller firms.[26]

The variable *PublicFloat* is the difference between the log of public float and log(75), defined thus to allow for different slope estimations on either side of the threshold in the regression discontinuity design. The mean and median of *PublicFloat* are roughly 2.3, which corresponds to $750 million. Approximately 2.3% of announcements are late (i.e., after the deadlines), and about 39% constitute bad earnings news (i.e., below analyst consensus forecast). Institutional investors on average own 61% of the total number of shares. A firm has a median number of 36 peer firms in the industry.

Figure 3.7 shows the distribution of quarterly earnings announcement lag in some industries. The figure suggests that there are significant differences in reporting lag not only across industries but also within industry. The figure offers suggestive evidence of peer effects consistent with the theoretical argument, most notably in the histograms of department stores and motion picture theaters: The more firms announcing earnings early there are, the sooner the remaining firms will announce earnings. Figure 3.8 shows the fraction of reporting lag variation due to within-industry variation over time. In other words, it reflects how much variation in reporting cannot be

[26] The fraction of firms below $75 million prior to my merging data with I/B/E/S is approximately 40%.

explained by industry-level characteristics. Since the fraction fluctuates around 73%, the figure indicates that observed patterns in reporting lags are due mainly to within-industry variation, suggesting that within-industry composition and reporting strategies are important determinants of the timing of earnings announcements. Thus, both figures point to the potential role of peer effects in this setting.

3.4 Peer Effects on the Timing of Earnings Announcements

3.4.1 Main Result – The Existence of Peer Effects

To test the first hypothesis, on the existence of peer effects, I run the two-stage regression discontinuity design described in Section 3. The first-stage regression corresponds to equation (1), which tests the relevance requirement of the instrument. That is, the regulation on reporting deadlines must have a statistically significant effect on the timing of earnings announcements. The results are presented in Table 3.2. The coefficient of *Treat_p* is negative and statistically significant in all specifications. The magnitude of the coefficient in the most restrictive specification is roughly -2.5. This indicates that, on average, firms receiving the regulatory treatment announce their earnings two or three days sooner than those that do not. More importantly, the instrument is statistically relevant to the timing of earnings announcements, because the F-statistics for weak instruments are above 10, implying that weak instrument concerns are unlikely.

Table 3.3 shows the main results on the existence of peer effects. Consistent with my prediction, these coefficients are also positive and statistically significant. In the last specification, the coefficient of $\widehat{Days\_p}$ is 0.13. This magnitude is not economically trivial: if eight peer firms announce earnings one day sooner, the results suggest that a focal firm would announce earnings one day sooner. Yet, because each treated firm is likely to report three days sooner, the result implies that it takes approximately three peer firms affected by the regulation to influence the focal firm to change its reporting strategy.

To ensure that the coefficient estimates are not due to the econometric choices used in the regression model, I show that the results are robust when using different polynomial fits, assigning no weights, and standard error clusters. Table 3.4 presents the results. Specifications (1) and (2) use quadratic and quartic fits, respectively. Specification (3) shows the regression results with no reversing weights. Because of how the data set is stacked, not assigning reversing weights is implicitly similar to assigning higher weights to observations in industry groups with more firms. Finally, specification (4) clusters standard errors at the industry level. Across all specifications, the peer effects estimates are positive and statistically significant. The magnitude of the effects is also similar to that in the preferred specification, around 0.13.

3.4.2 Peer Effects as a Disciplinary Mechanism

3.4.2.1 Peer Effects – Bad News vs. Good News

To test hypothesis H2 and investigate whether firms with bad earnings news are affected more by peer firms' behavior, I run the following regression:

$$
\begin{aligned}
Days_{iq} = \beta_0 + \beta_1 \widehat{Days}_{jq} + \beta_2 BadNews_{iq} + \beta_3 \widehat{Days}_{jq} \times BadNews_{iq} \ + g(PublicFloat_{jq}) \\
+ \gamma X_{iq} + \delta Y_{jq} + FixedEffects + \varepsilon_{iq}, \qquad (3)
\end{aligned}
$$

where $BadNews_{iq}$ is either an indicator equal to 1 if a firm's reported earnings per share (EPS) is below the most recent median analyst EPS forecast before an earnings announcement, or a continuous variable capturing scaled earnings surprises UE_{iq} (Wang 2014). The controls and fixed effects are identical to those in the main regression presented in Table 3.3. The coefficient of interest is the interaction term. I expect β_3 to be positive for firms with bad earnings news.

Table 3.5 summarizes the results. The first specification uses the indicator variable, whereas the second specification uses scaled earnings surprises. The coefficient of the interaction term in the first specification is positive and statistically significant. This is consistent with my prediction that bad news firms suffer more from peer effects. Similarly, the coefficient of the interaction term in the second specification is negative and statistically significant, indicating that the worse the earnings news, the stronger the peer effects. The difference in peer effects is economically meaningful. The first specification suggests that the difference is 0.0311, which is a roughly 26% increase. The results from this table offer supportive evidence that peer effects are a disciplinary force preventing managers of firms with bad earnings news from strategically delaying their earnings announcements.

3.4.2.2 Peer Effects – Small Industries vs. Large Industries

To test H3 and investigate whether firms in larger industries are affected more by peer firms' behavior, I run the following regression:

$$\begin{aligned} Days_{iq} = \beta_0 + \beta_1 \widehat{Days}_{jq} + \beta_2 Z_{iq} + \beta_3 \widehat{Days}_{jq} \times Z_{iq} \ + g(PublicFloat_{jq}) \\ + \gamma X_{iq} + \delta Y_{jq} + FixedEffects + \varepsilon_{iq}, \end{aligned} \tag{4}$$

where Z_{iq} is either the number of firms in the same industry (*NumFirm*), the lagged number of firms in the same industry (*LagNumFirm*), an indicator that the number of firms in the industry is greater than the median (*LargeIndustry*), or the fraction of the number of firms that have already disclosed earnings in the industry by the time firm i announces its earnings (*FracNumFirmAD*). The last variable allows for a direct test of the theoretical argument from disclosure theory. The controls and fixed effects are identical to those in the main regression in Table 3.3. The coefficient of interest is the interaction term. I expect β_3 to be positive for firms in larger industries.

Table 3.6 presents the results of these tests. The coefficients of the interaction terms in the first three columns are positive and statistically significant. This is consistent with my prediction on the disciplinary effect of industry peers' earnings announcements: firms in larger industries face greater pressure from peer firms. Using the indicator *LargeIndustry* in column (3) helps explain the economic magnitude of the differential peer effects. The estimate suggests that the peer effects in large industries are approximately 34% larger than those in smaller industries. Finally, the coefficient of the interaction term in column (4) is also positive and statistically significant. This is consistent with the theoretical argument from disclosure theory, in which peer effects are stronger when the fraction of the number of firms that have already announced earnings is higher.

3.4.2.3 Peer Effects – Firm Visibility

To test hypothesis H4 and investigate whether more visible firms (which are subject to other forms of monitoring) are less affected by peer firms' behavior, I run the following regression:

$$\begin{aligned} Days_{iq} = \beta_0 + \beta_1 \widehat{Days}_{jq} + \beta_2 Visibility_{iq} + \beta_3 \widehat{Days}_{jq} \times Visibility_{iq} \ + g(PublicFloat_{jq}) \\ + \gamma X_{iq} + \delta Y_{jq} + FixedEffects + \varepsilon_{iq}, \qquad (5) \end{aligned}$$

where $Visibility_{iq}$ is either firm size (*PublicFloat*), the decile of *PublicFloat* (*PLDecile*), the fraction of shares owned by institutions (*IO*), or an indicator that the fraction of institutional ownership is higher than the median (*High_IO*). The above are proxies for the higher likelihood that a firm is subject to other monitoring forces that can also deter strategic delaying of earnings announcements. The intuition is that the more visible a firm is, the more likely that many players in the financial markets have it under scrutiny, leaving the firm less opportunity to behave strategically. Controls and fixed effects are again identical to those in the main regression in Table 3.3. The coefficient of interest is the interaction term. I expect β_3 to be negative for firms with higher visibility.

Table 3.7 shows the regression results of equation (5). The coefficients of the interaction terms in all four specifications are negative and statistically significant. This is consistent with the prediction from hypothesis 2c that peer effects are relatively less important in the presence of other monitoring mechanisms. Results in columns (2) and (4) help with the interpretation of the economic magnitude. The coefficient of the interaction term in column (2) is -0.0062, indicating a 4% decrease in peer effects as a firm moves from one decile to another. The coefficient of the interaction term in column (4) is -0.0444. This suggests the peer effects for firms with more monitoring are roughly 31% less important than those for firms with less monitoring.

3.4.3 Peer Effects and Deviations from Expected Dates

An alternative way to capture the timeliness of earnings announcements is to investigate how changes in peer firms' earnings announcement timing affect a focal firm's timing relative to its expected date. Following the literature on timing of earnings announcements, I defined $Days\_Dev_{iq}$ as the difference between the number of days it takes to announce earnings and the number of days it takes to report earnings in the corresponding quarter from the previous year.

Defining timeliness in this fashion also speaks to the phenomenon in which some firms, particularly large ones, inform investors their earnings announcement dates in advance (Livnat and Zhang 2015; Johnson and So 2017; Barth, Clinch, and Ma 2018). This action of a firm offers investors an expected date for its earnings announcement. However, this does not prevent the strategic timing of earnings announcements, because firms likely take into consideration peer firms' decisions when pre-announcing their dates (e.g., how many peers have announced earnings, how many peers have pre-announced their dates, and what those dates are). The reporting game then can be viewed through the lens of choosing the dates to pre-announce, and the original intuition regarding peer effect existence still applies. Because data on the dates of preannouncements is unobservable, the next best proxy for what would be an expected date is the date from a previous year's corresponding quarter. This is precisely the definition of $Days\_Dev_{iq}$. Similar to my original prediction, I hypothesize that a firm will report sooner (later) relative to its expected date if peer firms are reporting sooner (later).

I define *Switch* as a categorical variable with the value 1 if a firm switches from a lower filing status to a higher filing status, 0 if the firm's filing status remains the same, and -1 if the firm switches from a higher filing status to a lower filling status. To test the above prediction, I run the following two-stage instrumental variable regression[27]:

$$Days\_Dev_{jq} = \beta_0 + \beta_1 Switch_{jq} + \rho X_{iq} + \mu Y_{jq} + FixedEffects + u_{iq}, \qquad (6)$$

and

$$Days\_Dev_{iq} = \beta_0 + \beta_2 \widehat{Days\_Dev}_{jq} + \gamma X_{iq} + \delta Y_{jq} + FixedEffects + \varepsilon_{iq}, \qquad (7)$$

The controls are similar to those in the previous regression models, and public float values from both firms *i* and *j* are also included. The coefficient of interest, β_2,is expected to be positive.

Table 3.8 presents the results of the above regressions. Column (1) shows the result of the first-stage regression. Consistent with the prediction, the coefficient of *Switch_p* is negative and statistically significant. The magnitude is roughly -1.5, indicating that firms moving from a lower to a higher filing status tend to report one or two days sooner than the expected date. Column (2) shows the result of the second-stage regression. The coefficient of interest is positive and statistically significant. This is consistent with the prediction that if peer firms are shocked into reporting sooner than they did the previous year, the firm will also deviate from its previous year's reporting strategy in the same direction.

[27] This is no longer a traditional regression discontinuity design, as *Switch* is not a treatment variable with a fixed threshold. The baseline approach using *Treat* as the exogenous shock no longer applies, because a firm should deviate from a previous year's reporting date only following the first time its peer receives the regulatory treatment but not in the subsequent period, if the peer's filing status remains the same.

3.4.4 Information Content of Peers' Announcements and Peer Effects in the Timing of Earnings Announcements

The literature on information transfers shows that a firm's earnings announcements may contain information on other firms in the same industry that is useful to investors.[28] This can happen in various forms. For example, a firm's earnings announcement may contain information on the overall trend of its industry, and the stock prices of other firms may increase or decrease as a result. However, for direct competitors, a firm's disclosure may contain information that negatively affects the stock price of another firm.[29] In such situations, the non-announcing firms may change their reporting strategy accordingly.

If a peer firm discloses information that negatively affects a focal firm, the market will lower its belief about the expected value of the non-announcing firm. This lowers the disclosure threshold of the non-announcing firm and thus incentivizes it to announce earnings sooner. For example, on Jan 26, 2016, KMBC 9 News, an ABC-affiliated television station, reported that "Sprint CEO Marcelo Claure moved [earnings announcement] up a week to blunt some negative market speculation occurring on Wall Street last week—speculation that drove Sprint's stock price down." In contrast, if there is a positive information transfer, the disclosure threshold will increase as the market believes the expected value of the non-announcing firm to be higher. This slows down the decreasing nature of the threshold function, allowing the firm to continue delaying its earnings announcement.

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[28] In general, research suggests that early earnings announcements from the early announcing firms in an industry provide information not only about the announcers but also about their industry peers who have not announced earnings (Firth 1976; Foster 1981; Ramnath 2002; Han and Wild 1990; Freeman and Tse 1992; Thomas and Zhang 2008; Badertscher, Shroff, and White 2013).

[29] For instance, proprietary information disclosed by one firm may change the cost of disclosure for another firm. The manager of one firm could be concerned that investors may incorrectly learn about her performance from the peer firm's announcement.

To test this prediction, I measure information transfers using the variable $CAR(i, t_j)_q$, which is the 3-day market-adjusted cumulative abnormal returns of firm i around t_j, the date firm j announces earnings for quarter q. This variable captures how the stock price of firm i changes due to the information from peer j's earnings announcement. If it is negative (positive), it implies that firm j's announcement contains information that negatively (positively) affects i. Next, I examine whether information transfer plays a role in the earnings announcement timing strategy, by running the following regressions:

$$\begin{aligned} Days_{iq} = \beta_0 + \beta_1 \widehat{Days}_{jq} + \beta_2 NegativelyAffected_{ijq} + \beta_3 \widehat{Days}_{jq} \times NegativelyAffected_{ijq} \\ + g(PublicFloat_{jq}) + \ \gamma X_{iq} + \delta Y_{jq} + FixedEffects + \varepsilon_{iq}, \quad (8) \end{aligned}$$

and

$$\begin{aligned} Days_{Dev_{iq}} = \beta_0 + \beta_1 \widehat{Days_{Dev}}_{jq} + \beta_2 NegativelyAffected_{ijq} + \beta_3 \widehat{Days_{Dev}}_{jq} \times NegativelyAffected_{ijq} \\ + \gamma X_{iq} + \delta Y_{jq} + FixedEffects + \varepsilon_{iq}, \quad (9) \end{aligned}$$

where $NegativelyAffected_{ijq}$ is an indicator variable equal to 1 if $CAR(i, t_j)_q$ is less than or equal to the 25th percentile. This definition ensures that the information transfer is negative and strong enough to warrant a reaction in a short time window. Equations (8) and (9) represent two different ways to measure timeliness. Like the previous tests, the instrument used in (8) is $Treat_{jq}$, while the instrument used in (9) is $Switch_{jq}$. The controls and fixed effects are similar to those in the previous regression models. The coefficient of interest is the interaction term and is expected to be negative.

Table 3.9 presents the regression results. Consistent with my prediction, the coefficient of the interaction term in both specifications is negative and statistically significant. It implies that peer effects are stronger when firm i is negatively affected by the information content of firm j's earnings announcement than they are when firm i is positively affected. The coefficient of $\widehat{Days\_p}$ (or $\widehat{Days\_Dev\_p}$) is also positive and statistically significant. This coefficient can be interpreted as the peer effects present when there is no information transfer. Thus, the results suggest that information transfer complements but does not completely explain peer effects on the timing of earnings announcements.

3.5 Policy Implications

The results from Table 3.7 imply that the smaller firms are, the more they will suffer from peer effects. This is a negative externality of the SEC regulation on reporting deadlines. According to the comments and letters the SEC received when it first expressed the intention to update reporting deadline requirements, one of the most frequent objections was that companies slightly above the $75 million cutoff, which were considered small at the time, would face an increased compliance burden in the form of accounting related costs and audit fees (SEC Release No. S7-08-02).

The findings of this study suggest that the regulation not only affects small firms slightly above the $75 million cutoff, but also indirectly affects the smaller firms below the cutoff. The latter firms are not subject to the stricter deadline requirement yet experience a regulatory externality due to the peer effects in earnings announcement timing. To investigate the extent to which the regulation imposes spillover costs as a result of peer effects, I run the following regression to capture one aspect of a potential increase in costs:

$$LogFees_{it} = \beta_0 + \beta_1 Treat_{jt} + f(PublicFloat_{jt}) + \rho X_{it} + \mu Y_{jt} + FixedEffects + u_{it}, \qquad (10)$$

where $LogFees_{it}$ is the natural log of the audit fees in dollar amounts firm i incurs during fiscal year t. The regression is conducted at the yearly level because quarterly information on audit fees is not available. Year-quarter fixed effects are replaced with year fixed effects. The coefficient of interest is β_1, expected to be positive, indicating that when the regulation shocks peer firms, a focal firm's audit fees also increase.

Panel A of table 3.10 reports the estimation results of equation (10). The coefficient of *Treat_p* is positive and statistically significant in all specifications. The effect is approximately 0.01, suggesting a near 1% increase in a firm's audit fees is associated with each peer firm above the cutoff. To better understand the economic significance of this spillover cost, I run a regular regression discontinuity design to investigate the direct effect of regulatory treatment on the affected firms. The results, reported in Panel B of Table 3.10, suggest that the direct effect is an approximately 25% increase in audit fees. Thus, the spillover effect at 1% per treated peer could be cumulatively nontrivial, depending on the number of firms affected by the stricter regulation.

The analysis above indicates that, through peer effects, firms suffer negative spillovers with real costs that may have not been intended. The increase in audit fees happens because firms feel pressure to catch up with their peers who are forced to announce earnings early. This evidence also complements the busy auditor argument during earnings seasons (Duguay et al. 2018; López and Peter, 2011). In other words, part of the increase in audit fees could happen because auditors, having a higher number of clients reporting early, become busier during earnings seasons, leading them to charge higher fees for all clients on average. Furthermore, audit fees are only one among other unobservable costs that a firm may also see increased (e.g., costs to hire more accountants, costs to improve accounting technology within the firm, etc.). The total spillover cost may not be trivial.

In short, the existence of peer effects in this context is of importance to policymakers because it implies that a regulation aiming to accelerate earnings announcement timing for one group of firms may also affect another. Although this externality can be beneficial to investors as it promotes the timeliness of accounting information in an entire industry, such a spillover effect can be costly to firms. Thus, the findings imply that it is important to determine the right cutoff level of public float so that the regulation can induce firms below the cutoff to announce earnings slightly sooner while keeping the spillover costs to these firms sufficiently low. Peer effects are also important when deciding the intensity of the regulation (e.g., how many days shorter), because they imply that regulation need not be very strict to achieve the intended outcomes.

3.6 Conclusion

In this paper, I investigate whether peer effects exist in earnings announcement timing. To help me draw causal inferences with greater confidence, I exploit an SEC regulation on reporting deadlines to generate quasi-exogenous variation in the timing of earnings announcements of peer firms. I show that firms announce earnings sooner if their peers also announce their earnings sooner. Firms with bad earnings news and firms in larger industries are affected more by the timing of peers' earnings announcements, whereas more visible firms that are subjected to other sources of monitoring are less affected. These results offer evidence supporting the disciplining role of peer effects in deterring strategic delays of earnings news. My results are also relevant to policymakers should there be future changes in reporting deadlines, because they highlight a spillover effect from the regulation.

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APPENDIX 3A – VARIABLE DEFINITIONS

This appendix shows the descriptions of the variables used in the paper. The variables corresponding to the peer firms have identical descriptions to those of the focal firm, but their abbreviations end with the suffix *_p*.

Main Variables:

Abbreviation	**Description**
Treat	An indicator equal to 1 if a firm's public float is equal to or greater than $75 million, and 0 otherwise.
Days	The number of days from fiscal quarter end to earnings announcement date.
PublicFloat	The difference between the log of the market value of the portion of shares of a company that are held by public investors as opposed to company affiliates, the and log (75).
BadNews	An indicator variable equal to 1 if a firm's reported earnings per share (EPS) is below the most recent median analyst EPS forecast before the earnings announcement, and 0 otherwise.
UE	The difference between the firm's reported earnings per share (EPS) and the most recent median analyst EPS forecast before the earnings announcement, scaled by the absolute value of the most recent median EPS forecast.
NumFirm	The number of firms in an industry in a fiscal quarter. Industries are defined using 3-digit SIC codes.
LargeIndustry	An indicator equal to 1 if the number of firms in an industry as of a current fiscal quarter is greater than or equal to the median number of firms, and 0 otherwise.
LagNumFirm	The number of firms in an industry in the corresponding previous fiscal quarter.
FracNumFirmAD	The fraction of the number of firms that have already announced earnings in the industry group by the time the focal firm announces its earnings.
IO	The fraction of shares owned by institutional investors as of the quarter.
High_IO	An indicator equal to 1 if the fraction of institutional ownership of a firm is greater than or equal to the median value, and 0 otherwise.
Days_Dev	The difference between the number of days it takes to report earnings from a fiscal quarter end and the number of days it takes to report earnings from the last year's corresponding quarter.

Switch	A categorical variable equal to 1 if a firm switches from a lower filing status to a higher filing status, 0 if the firm's filing status remains the same, and -1 if the firm switches from a higher filing status to a lower filling status.
LogFees	The natural log of a firm's audit fees in dollar amounts.
CAR	The 3-day market-adjusted cumulative abnormal return of a focal firm around the earnings announcement date of a peer firm.
NegativelyAffected	An indicator variable equal to 1 if the 3-day, market-adjusted, cumulative abnormal returns around the earnings announcement date of the peer firm (*CAR*) are less than or equal to the 25th percentile, and 0 otherwise

Controls:

Abbreviation	**Description**
Late	An indicator variable equal to 1 if the earnings announcement is made after the required deadline, and 0 otherwise.
LogMC	The natural log of market capitalization.
BM	Book to market ratio.
Lev	Leverage, or the ratio between total liabilities and total assets.
ROA	Returns on assets.

Figure 3.1. Reporting Lags of Wells Fargo and JPMorgan Chase

This graph shows the earnings announcement lags of Wells Fargo and JPMorgan Chase over time. The x-axis represents time, in fiscal quarters. The y-axis represents the number of days it takes to announce earnings relative to the fiscal quarter ends. The graph shows that when JPMorgan Chase gradually accelerates its earnings announcement timing, Wells Fargo does likewise, until both have similar reporting strategy.

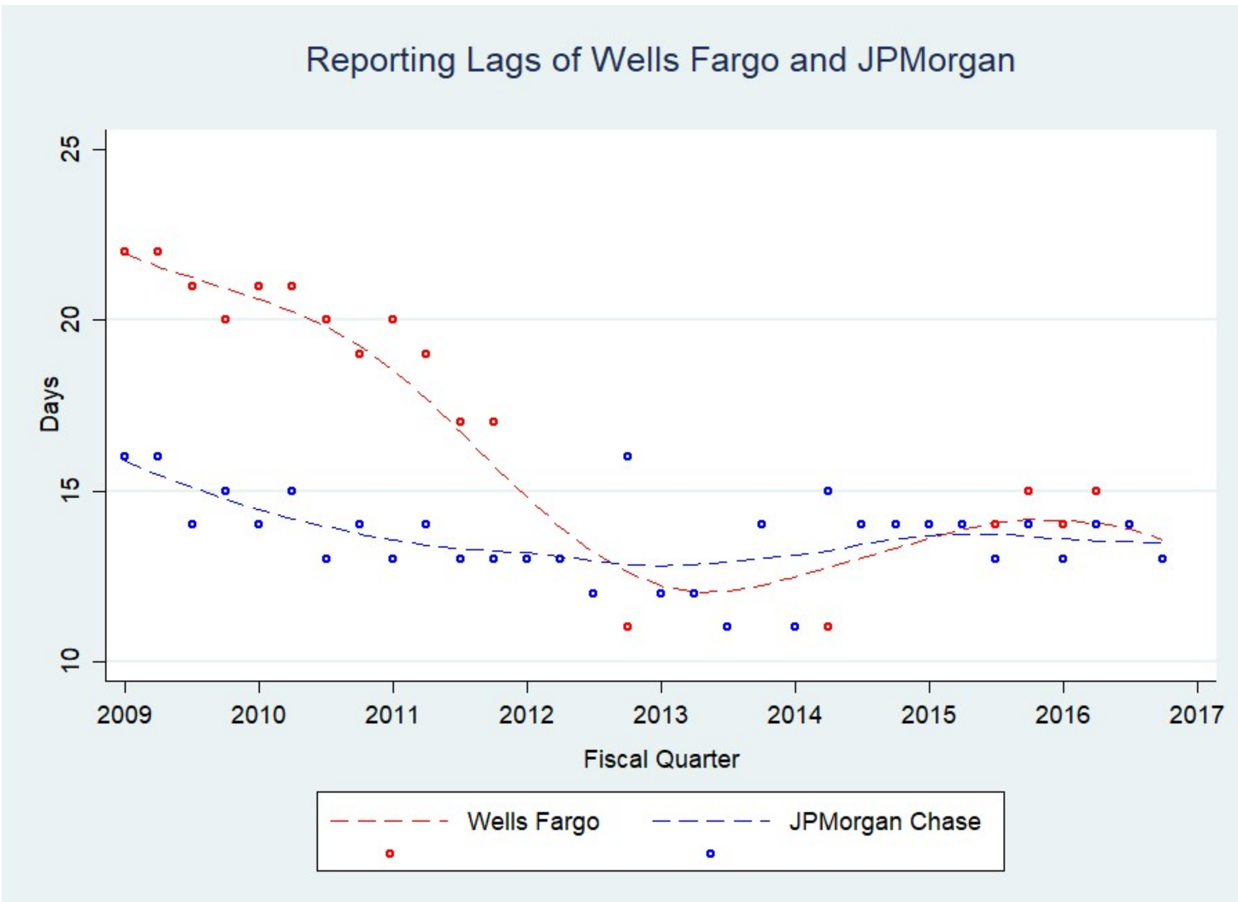

Figure 3.2. Intuition for Hypothesis 2

This graph shows the intuition behind H2: Firms with bad earnings news suffer more from peer effects. The x-axis represents time, where the origin t = 0 can be interpreted as the end of a fiscal quarter. The y-axis is the value of the earnings signal. The region shaded in blue represents good earnings signals. The region shaded in red represents bad earnings signals. The first line on the left of the graph shows the disclosure thresholds of a firm subject to peer effects over time. The second line on the right of the graph shows the disclosure thresholds of the same firm over time when peer effects do not exist. The slopes of both lines are negative because the uncertainty about a manager's information endowment naturally decreases over time, causing the disclosure thresholds to decrease over time as a result. Moreover, because peer effects cause the disclosure thresholds to decrease faster than they would in the absence of peer effects, the first line is steeper than the second. Therefore, the peer effects on a firm with good news (as seen from the blue arrow) are smaller than those on a firm with bad news (as seen from the red arrow).

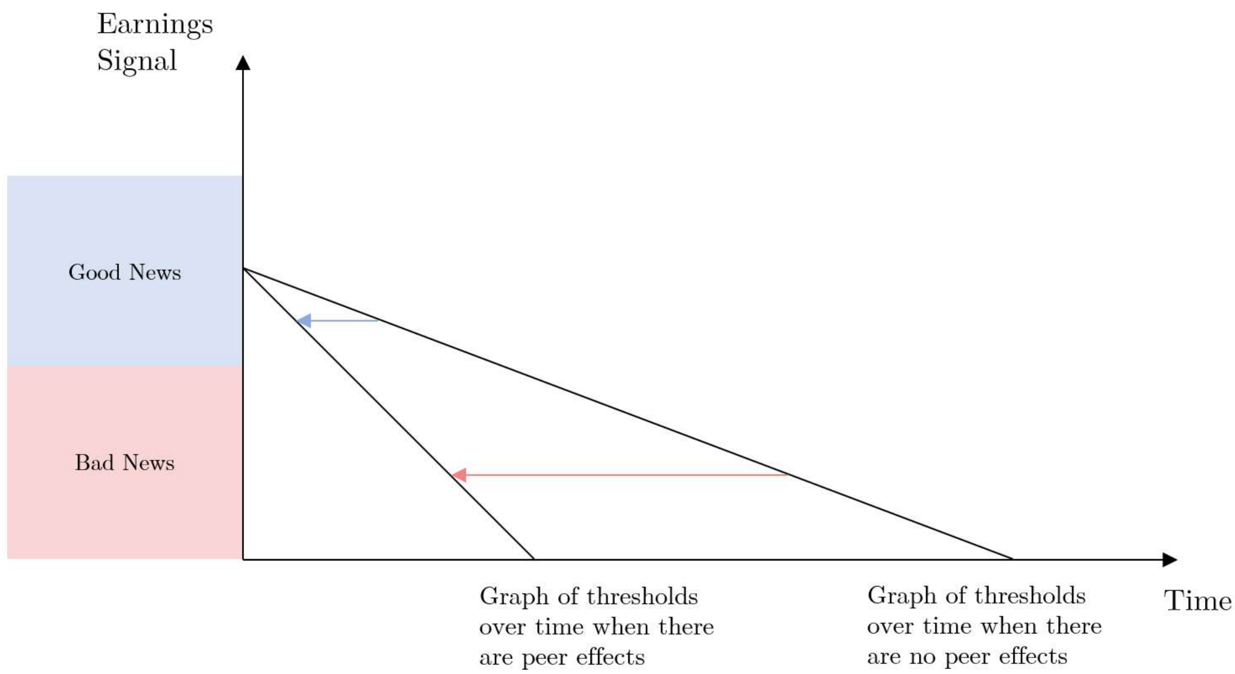

Figure 3.3. SEC's Regulation of Periodic Reporting Deadlines

This figure summarizes the SEC's regulation on the periodic reporting deadlines of publicly traded companies. Filer statuses are determined by a firm's public float, which is the market value of the portion of its shares held by public investors, as opposed to company affiliates, as of the last date of the second fiscal quarter in the previous fiscal year. Firms with public floats greater than or equal to $700 million are classified as "Large Accelerated Filers" and have 40 days to file 10-Q forms and 60 days to file 10-K forms. Firms with public floats greater than or equal to $75 million but less than $700 million are classified as "Accelerated Filers," and have 40 days to file 10-Q firms and 75 days to file 10-K forms. All remaining firms with public float values of less than $75 million have 45 days to file 10-Q forms and 90 days to file 10-K forms. (See Section 3.3.1 for additional details on the regulation.)

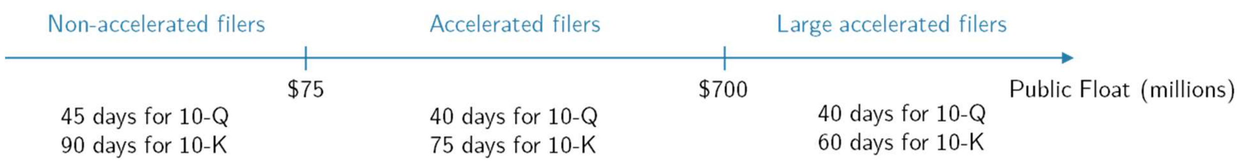

Figure 3.4. Instrument Relevance

This figure shows the effect of the regulation of interest on quarterly earnings announcement reporting lags. The x-axis represents public float in millions of dollars. The y-axis represents the number of days before a firm reports earnings, starting from its fiscal quarter end date. Each dot corresponds to a $2 million bin. A quadratic polynomial fit with different slopes on each side of the cutoff is used. The colored curves are the fitted values. The dash lines above and below each curve represent the confidence intervals. The figure suggests a difference of roughly 3 days for firms slightly below and slightly above the threshold, indicating that the regulation has an effect on firms' earning announcement lags.

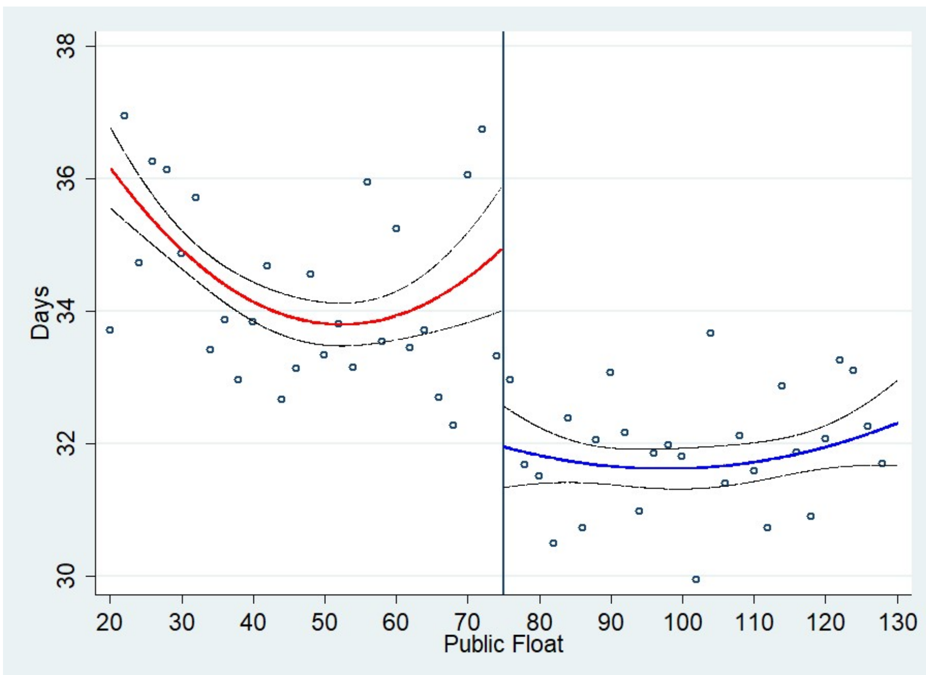

Figure 3.5. Public Float Manipulation

This figure shows the McCrary density test for manipulation of the running variable. The x-axis indicates the level of public float (in millions of dollars). The y-axis is the frequency. Each dot corresponds to a $2 million bin. Because the density function seems smooth near the threshold, the figure suggests that public float manipulation is insignificant.

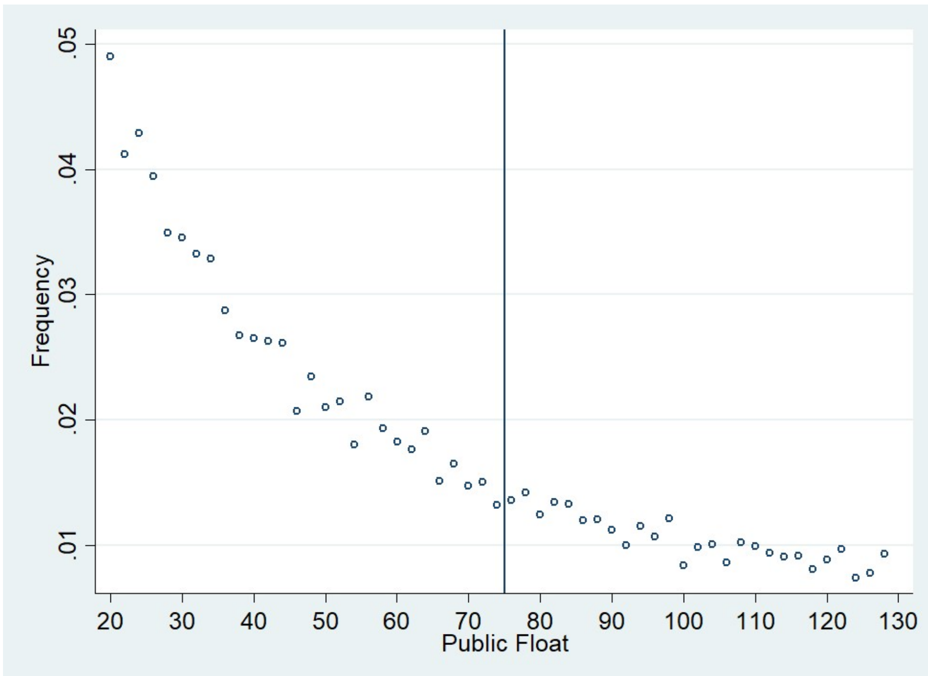

Figure 3.6. Variation Used to Estimate Peer Effects: An Example

This figure shows the variation used in the estimation of peer effects and offers a graphical illustration of the data structure. Suppose firms a, b, c, and d are in the same industry group. For each firm, there are three rows of data, corresponding to the remaining three peer firms. Each peer firm's *Days* variable, the number of days it takes to announce earnings starting from the fiscal quarter end, is instrumented by the regulatory cutoff. Thus, to estimate peer effects, only variation in reporting lags induced by regulatory treatment is used, mitigating Manski's reflection problem.

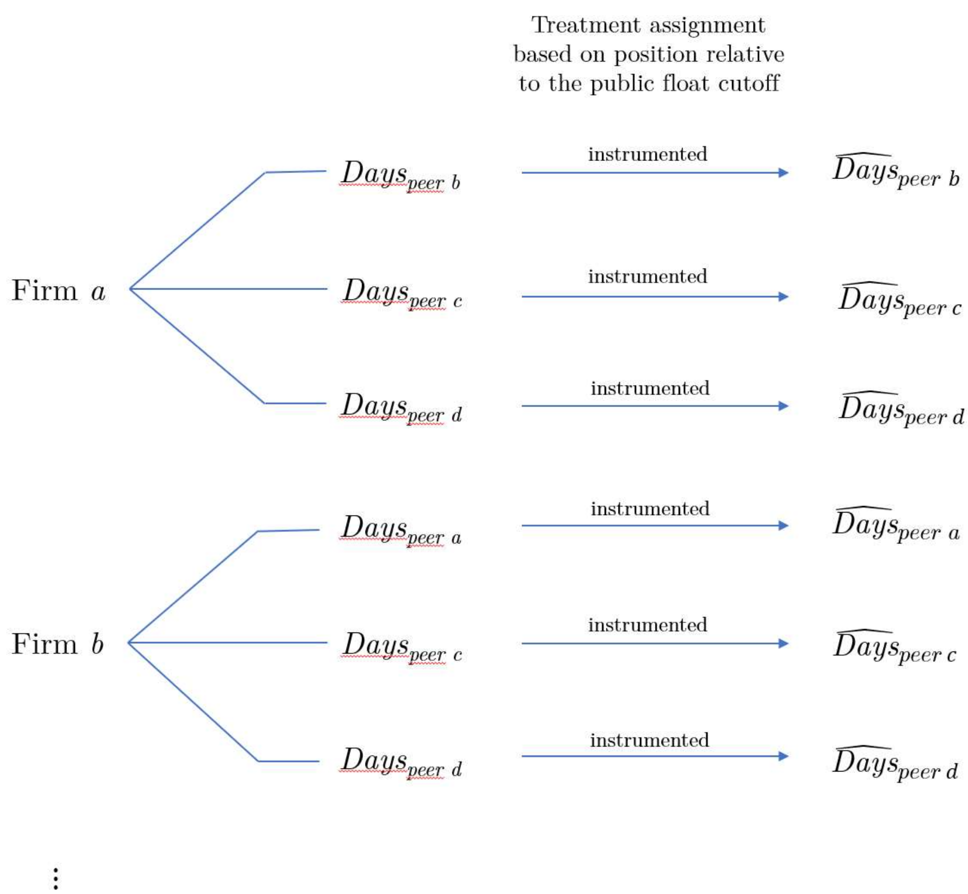

Figure 3.7. Reporting Lag Distributions in Some Industries

This figure shows the distribution of quarterly earnings announcement lags in some industries. In each histogram, the x-axis represents the number of days it takes to announce earnings. The y-axis shows the density values for the histogram. The figure suggests that there are significant differences in reporting lags, not only across industries, but also within industries. The figure offers suggestive evidence of peer effects consistent with the theoretical argument, most notably in the histograms of department stores and motion picture theaters: The more firms announcing earnings early, the sooner the remaining firms will announce earnings.

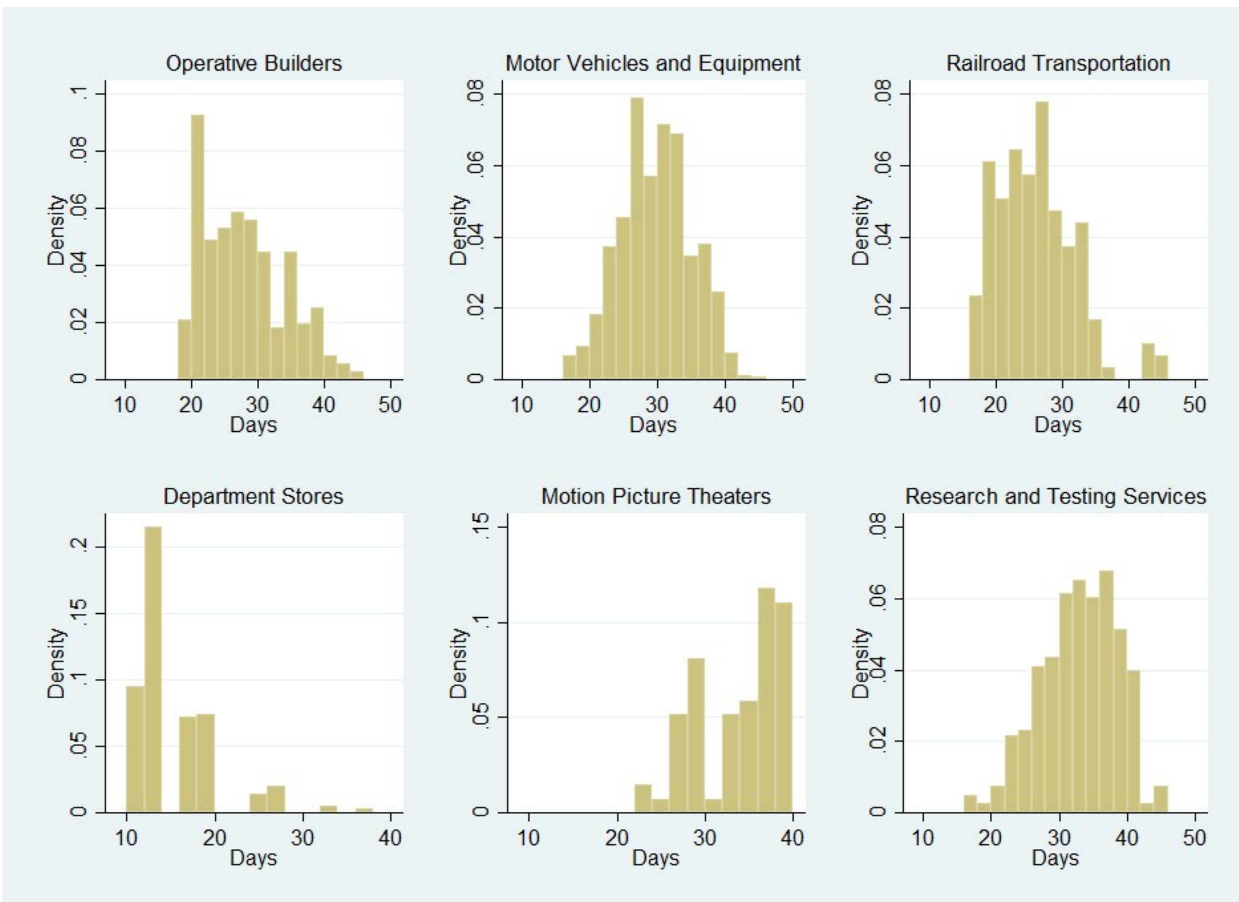

Figure 3.8. Fraction of Reporting Lag Variation Due to Within-Industry Variation

This figure shows the fraction of reporting lag variation due to within-industry variation over time. In other words, it reflects how much variation in reporting cannot be explained by industry-level characteristics. Because the fraction fluctuates around 73%, the figure suggests that observed patterns in reporting lags are due mainly to within-industry variation.

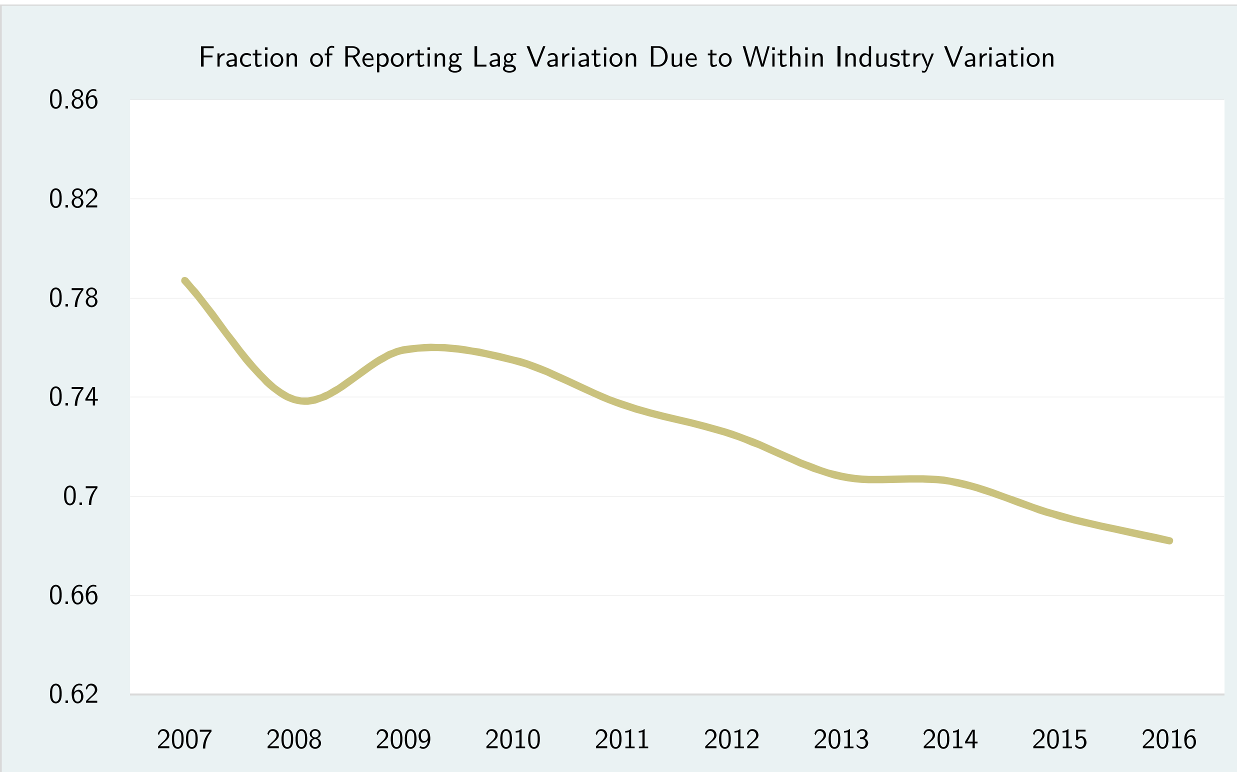

Table 3.1. Summary Statistics

This table presents the summary statistics for the regression variables of interest. Variable definitions are available in Appendix 3A.

	Mean	SD	P25	P50	P75
Days	34.4325	12.8442	26.0000	32.0000	38.0000
Treat	0.9160	0.2774	1.0000	1.0000	1.0000
PublicFloat	2.3857	1.8671	1.0939	2.2847	3.6254
Late	0.0228	0.1492	0.0000	0.0000	0.0000
LogMC	6.9410	1.8420	5.6833	6.8719	8.1170
BM	0.6154	1.8276	0.2704	0.5072	0.8272
ROA	-0.0103	0.6532	-0.0030	0.0045	0.0155
Lev	0.5909	0.3556	0.3811	0.5829	0.7950
BadNews	0.3855	0.4867	0.0000	0.0000	1.0000
UE	0.5995	4.4676	-0.6889	0.5213	2.0381
Days_Deviation	-0.1805	5.2838	-2.0000	-1.0000	1.0000
Switch	0.0574	0.3380	0.0000	0.0000	0.0000
IO	0.6104	0.2515	0.4343	0.6377	0.8069
LogFees	14.0366	1.1131	13.3047	13.9623	14.6997
NumFirm	97.3063	112.0848	12.0000	36.0000	174.0000
CAR	0.0001	0.0503	-0.0215	-0.0004	0.0207

Table 3.2. First-Stage Regression

The following table shows results of the first-stage regression of the regression discontinuity design to test the instrument relevance requirement. The dependent variable is *Days_p*, the number of days a peer firm takes starting from its fiscal quarter end to announce its earnings. The independent variable of interest is *Treat_p*, an indicator equal to 1 if the peer firm receives the treatment (i.e., is subject to a stricter deadline requirement). The running variable used for the regression discontinuity design is *PublicFloat_p*. The relevant threshold to determine treatment status is at $75 million public float. All specifications include the focal firm's characteristics and the peer firm's characteristics as controls, namely *PublicFloat*, *PublicFloat_p*, *Late*, *Late_p*, *LogMC*, *LogMC_p*, *BM*, *BM_p*, *Lev*, *Lev_p*, *ROA,* and *ROA_p* (See Appendix 3A for variable descriptions). Industry and year-quarter fixed effects are progressively included to account for time-invariant heterogeneity among industries and time trends, respectively. Standard errors are two-way clustered at the firm and the peer firm level and reported in parentheses. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

	Dependent Variable: *Days_p*		
	(1)	(2)	(3)
Treat_p	-1.7268*	-2.3041***	-2.4511***
	(0.9716)	(0.7661)	(0.7027)
Polynomial Degree	Cubic	Cubic	Cubic
Controls	Yes	Yes	Yes
Fixed Effects:			
Industry	No	Yes	Yes
Year x Quarter	No	No	Yes
Obs.	4,029,923	4,029,923	4,029,923
R^2	0.15	0.30	0.60

Table 3.3. Existence of Peer Effects

The following table shows the existence of peer effects in the timing of earnings announcements. Specifications (1), (2), and (3) show the second-stage instrumental variable estimates of the peer effects. The dependent variable is *Days*, the number of days a focal firm takes starting from its fiscal quarter end to announce its earnings. The independent variable of interest is $\widehat{Days\_p}$, the instrumented version of *Days_p*. The running variable used for the regression discontinuity design is *PublicFloat_p*. All specifications include the focal firm's characteristics and the peer firm's characteristics as controls, namely *PublicFloat*, *PublicFloat_p*, *Late*, *Late_p*, *LogMC*, *LogMC_p*, *BM*, *BM_p*, *Lev*, *Lev_p*, *ROA,* and *ROA_p* (see Appendix 3A for variable descriptions). Industry and year-quarter fixed effects are progressively included to account for time-invariant heterogeneity among industries and time trends, respectively. Standard errors are two-way clustered at the firm and the peer firm level and reported in parentheses. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

	Dependent Variable: *Days*		
	IV	IV	IV
	(1)	(2)	(3)
$\widehat{Days\_p}$	0.2469***	0.2435***	0.1301***
	(0.0922)	(0.0415)	(0.0323)
Polynomial Degree	Cubic	Cubic	Cubic
Controls	Yes	Yes	Yes
Fixed Effects:			
Industry	No	Yes	Yes
Year x Quarter	No	No	Yes
1^{st} Stage F-stat	10.91	17.53	15.43
Obs.	4,029,923	4,029,923	4,029,923
R^2	0.34	0.42	0.6

Table 3.4. Existence of Peer Effects—Robustness

The following table shows the robustness results of the existence of peer effects in the timing of earnings announcements. All specifications show the second-stage instrumental variable estimates of the peer effects. The dependent variable is *Days*, the number of days a focal firm takes starting from the fiscal quarter end to announce its earnings. The independent variable of interest is $\widehat{Days\_p}$, the instrumented version of *Days_p*. The running variable used for the regression discontinuity design is *PublicFloat_p*. All specifications include the focal firm's characteristics and the peer firm's characteristics as controls, namely *PublicFloat*, *PublicFloat_p*, *Late*, *Late_p*, *LogMC*, *LogMC_p*, *BM*, *BM_p*, *Lev*, *Lev_p*, *ROA,* and *ROA_p* (see Appendix 3A for variable descriptions). Industry and year-quarter fixed effects are included to account for time-invariant heterogeneity among industries and time trends, respectively. The specifications have different estimation choices: polynomial fits and weights. Standard errors are either two-way clustered at the firm and the peer firm level, or at the industry level, and reported in parentheses. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

	Dependent Variable: *Days*			
	IV	IV	IV	IV
	(1)	(2)	(3)	(4)
$\widehat{Days\_p}$	0.1211***	0.0592**	0.1288***	0.1301**
	(0.0330)	(0.0305)	(0.0355)	(0.0502)
Controls	Yes	Yes	Yes	Yes
Fixed Effects:				
Industry	Yes	Yes	Yes	Yes
Year x Quarter	Yes	Yes	Yes	Yes
Polynomial Degree	Quadratic	Quartic	Cubic	Cubic
Weighted Regression	Yes	Yes	No	Yes
Cluster SE	Firm, Peer	Firm, Peer	Firm, Peer	Industry
1st Stage F-stat	14.8917	10.8825	9.0107	14.1891
Obs.	4,029,923	4,029,923	4,029,923	4,029,923
R^2	0.60	0.60	0.63	0.60

Table 3.5. Peer Effects – Good News vs. Bad News

The following table shows differential peer effects on firms with bad earnings news versus firms with good earnings news, highlighting the disciplining mechanism of peer effects. The dependent variable is *Days*, the number of days the focal firm takes to announce earnings. In specification (1), the coefficient of interest is the interaction term with *BadNews*, an indicator equal to 1 if a firm has bad earnings news (i.e., a missed consensus forecast). In specification (2), the coefficient of interest is the interaction term with *UE*, scaled earnings surprise. The running variable used for the regression discontinuity design is *PublicFloat_p*. All specifications include the focal firm's and peer firm's characteristics as controls, similar to those in Table 3. Industry and year-quarter fixed effects are included to account for time-invariant heterogeneity among industries and time-trends respectively. All specifications use a cubic polynomial for the running variable. Standard errors are two-way clustered at the firm and the peer firm level and reported in parentheses. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

	Dependent Variable: *Days*	
	(1)	(2)
$\widehat{Days\_p}$	0.1203***	0.1116***
	(0.0329)	(0.0281)
$\widehat{Days\_p} \times BadNews$	0.0311**	
	(0.0140)	
$\widehat{Days\_p} \times UE$		-0.0025**
		(0.0013)
Polynomial Degree	Cubic	Cubic
Controls	Yes	Yes
Fixed Effects:		
Industry	Yes	Yes
Year x Quarter	Yes	Yes
1^{st} Stage F-stat	12.34	11.79
Obs.	4,029,923	3,388,742
R^2	0.60	0.60

Table 3.6. Peer Effects – Small vs. Large Industries

The following table shows differential peer effects on firms in small versus large industries, highlighting the disciplining mechanism of peer effects. The dependent variable is *Days*, the number of days a focal firm takes to announce earnings. The coefficients of interest are the interaction terms with *NumFirm*, *LagNumFirm*, and *LargeIndustry*. The running variable used for the regression discontinuity design is *PublicFloat_p*. All specifications include the focal firm's and peer firm's characteristics as controls, similar to those in Table 3. Industry and year-quarter fixed effects are included to account for time-invariant heterogeneity among industries and time trends, respectively. All specifications use a cubic polynomial for the running variable. Standard errors are two-way clustered at the firm and the peer firm level and reported in parentheses. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

	Dependent Variable: *Days*			
	(1)	(2)	(3)	(4)
$\widehat{Days\_p}$	0.0785***	0.0581**	0.1036***	0.0838*
	(0.0279)	(0.0254)	(0.0339)	(0.0462)
$\widehat{Days\_p}$ x *NumFirms*	0.0003***			
	(0.0001)			
$\widehat{Days\_p}$ x *LagNumFirms*		0.0003***		
		(0.0001)		
$\widehat{Days\_p}$ x *LargeIndustry*			0.0356**	
			(0.0154)	
$\widehat{Days\_p}$ x *FracNumFirmAD*				0.2692***
				(0.0397)
Polynomial Degree	Cubic	Cubic	Cubic	Cubic
Controls	Yes	Yes	Yes	Yes
Fixed Effects:				
Industry	Yes	Yes	Yes	Yes
Year x Quarter	Yes	Yes	Yes	Yes
1^{st} Stage F-stat	14.12	13.81	12.68	12.24
Obs.	4,029,923	3,798,451	4,029,923	4,029,923
R^2	0.60	0.60	0.60	0.80

Table 3.7. Peer Effects – Firm Visibility

The following table shows differential peer effects on firms in small versus large industries, highlighting the disciplinary mechanism of peer effects. The dependent variable is *Days*, the number of days a focal firm takes to announce earnings. The coefficients of interest are the interaction terms with *PublicFloat*, *PLDecile*, *IO*, and *High_IO*. The running variable used for the regression discontinuity design is *PublicFloat_p*. All specifications include the focal firm's and peer firm's characteristics as controls, similar to those in Table 3. Industry and year-quarter fixed effects are included to account for time-invariant heterogeneity among industries and time trends, respectively. All specifications use a cubic polynomial for the running variable. Standard errors are two-way clustered at the firm and the peer firm level and reported in parentheses. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

	Dependent Variable: *Days*			
	(1)	(2)	(3)	(4)
$\widehat{Days\_p}$	0.1557***	0.1621***	0.1851***	0.1450***
	(0.0362)	(0.0405)	(0.0396)	(0.0336)
$\widehat{Days\_p} \times PublicFloat$	-0.0119**			
	(0.0056)			
$\widehat{Days\_p} \times PLDecile$		-0.0062*		
		(0.0037)		
$\widehat{Days\_p} \times IO$			-0.1064***	
			(0.0374)	
$\widehat{Days\_p} \times High\_IO$				-0.0444**
				(0.0185)
Polynomial Degree	Cubic	Cubic	Cubic	Cubic
Controls	Yes	Yes	Yes	Yes
Fixed Effects:				
Industry	Yes	Yes	Yes	Yes
Year x Quarter	Yes	Yes	Yes	Yes
1st Stage F-stat	12.42	12.41	12.56	12.54
Obs.	4,029,923	4,029,923	3,847,310	3,847,310
R^2	0.60	0.60	0.60	0.60

Table 3.8. Alternative Specification: Deviation from Previous Year

The following table shows the existence of peer effects in the timing of earnings announcement using an alternative empirical model. The dependent variable is *Days_Dev*, the difference between the number of days a focal firm takes in a given fiscal quarter to announce earnings and the number of days it took in the corresponding previous fiscal quarter to announce earnings. The instrument is *Switch_p*, which is equal to -1 if a peer firm switches from a higher status to a lower status, relative to its previous year's status, 0 if the peer firm maintains the same status, and 1 if the peer firm switches from a lower status to a higher status. The explanatory variable is *Days_Dev_p*. Column (1) shows the results of the first-stage regression. Column (2) shows the results of the second-stage regression. All specifications include the focal firm's characteristics and the peer firm's characteristics as controls, namely *PublicFloat*, *PublicFloat_p*, *Late*, *Late_p*, *LogMC*, *LogMC_p*, *BM*, *BM_p*, *Lev*, *Lev_p*, *ROA,* and *ROA_p* (see Appendix 3A for variable descriptions). Industry and year-quarter fixed effects are included to account for time-invariant heterogeneity among industries and time trends, respectively. The specifications differ in the degree of polynomial function of *PublicFloat_p*. Standard errors are two-way clustered at the firm and the peer firm level and reported in parentheses. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

	First Stage	**Second Stage**
Dependent Variable:	*Days_Dev_p*	*Days_Dev*
	(1)	(2)
Switch_p	-1.5066***	
	(0.0946)	
$\widehat{Days\_Dev\_p}$		0.0567***
		(0.0186)
Controls	Yes	Yes
Fixed Effects:		
Industry	Yes	Yes
Year x Quarter	Yes	Yes
1^{st} Stage F-stat		253.1757
Obs.	3,901,558	3,901,558
R^2	0.05	0.04

Table 3.9. Peer Effects and Information Transfers

The following table shows the differential peer effects in the presence of information transfers. The dependent variable of interest is *Days*. *Days_p* and *Days_Dev_p* are measures of a peer firm's timeliness of earnings announcements. *NegativelyAffected* is an indicator variable equal to 1 if the 3-day, market-adjusted, cumulative abnormal returns around the earnings announcement date of the peer firm are less than or equal to the 25th percentile, and 0 otherwise. Both specifications (1) and (2) are second-stage regressions, with *Treat_p* and *Switch_p* being the respective instruments. All specifications include the focal firm's characteristics and the peer firm's characteristics as controls, namely *PublicFloat*, *PublicFloat_p*, *Late*, *Late_p*, *LogMC*, *LogMC_p*, *BM*, *BM_p*, *Lev*, *Lev_p*, *ROA,* and *ROA_p* (see Appendix 3A for variable descriptions). Industry and year fixed effects are included to account for time-invariant heterogeneity among industries and time trends, respectively. Standard errors are two-way clustered at the firm and the peer firm level and reported in parentheses. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

	Dependent Variable:	
	Days	*Days_Dev*
	(1)	(2)
$\widehat{Days\_p}$	0.0862***	
	(0.0321)	
$\widehat{Days\_p} \times$ *NegativelyAffected*	0.0383***	
	(0.0128)	
$\widehat{Days\_Dev\_p}$		0.0189
		(0.0200)
$\widehat{Days\_Dev\_p} \times$ *NegativelyAffected*		0.0752**
		(0.0384)
Polynomial Degree	Cubic	N/A
Controls	Yes	Yes
Fixed Effects:		
Industry	Yes	Yes
Year x Quarter	Yes	Yes
1st Stage F-stat	11.31	103.23
Obs.	3,688,355	3,466,886
R^2	0.59	0.04

Table 3.10. Evidence on Audit Fees

The following table shows evidence of changes in audit fees for a firm subjected to stricter deadlines and for a firm whose peers are subjected to stricter deadlines. Panel A shows the spillover effects on the peer firms. Panel A shows the direct effect for the treated firms. The independent variable of interest is *Treat_p* in Panel A, and *Treat* in Panel B. The dependent variable of interest is *LogFees*, the log of the dollar amount in audit fees the focal firm pays its auditor for the year. The control variables in Panel A are *PublicFloat*, *PublicFloat_p*, *LogMC*, *LogMC_p*, *BM*, *BM_p*, *Lev*, *Lev_p*, *ROA,* and *ROA_p*. The control variables in Panel B are *LogMC*, *BM*, *Lev*, and *ROA* (see Appendix 3A for variable descriptions). Industry and year fixed effects are included to account for time-invariant heterogeneity among industries and time trends, respectively. The specifications differ in the polynomial fits used for the running variable *PublicFloat_p*. Standard errors are two-way clustered at the firm and the peer firm level in Panel A and clustered at the firm level in Panel B, and reported in parentheses. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A. Spillover Effect of Regulation on Audit Fees

	Dependent Variable: *LogFees*		
	(1)	(2)	(3)
Treat_p	0.0751**	0.0487***	0.0102*
	(0.0300)	(0.0104)	(0.0062)
Polynomial Degree	Cubic	Cubic	Cubic
Controls	Yes	Yes	Yes
Fixed Effects:			
Industry	No	Yes	Yes
Year	No	No	Yes
Obs.	1,117,554	1,117,554	1,117,554
R^2	0.64	0.70	0.70

Panel B. Direct Effect of Regulation on Audit Fees

	Dependent Variable: *LogFees*		
	(1)	(2)	(3)
Treat	0.3035***	0.2782***	0.2503***
	(0.0775)	(0.0860)	(0.0852)
Polynomial Degree	Cubic	Cubic	Cubic
Controls	Yes	Yes	Yes
Fixed Effects:			
Industry	No	Yes	Yes
Year	No	No	Yes
Obs.	27,038	27,038	27,038
R^2	0.61	0.71	0.71