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# Individual Lawyers, the SEC Revolving Door, and Comment Letters<sup>☆</sup>

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## Abstract

Government officials, advocacy groups, and the business press have raised concerns that former SEC employees may continue to influence the SEC after leaving the agency. Using a hand-collected database of individual lawyers that represent firms in responding to SEC comment letters, we examine the impact of individual lawyers, and lawyers formerly employed by the SEC, on the comment letter process. We document significant differences between lawyers and law firms in their clients' resistance to SEC comment letters, and find that firms that retain former SEC employees are larger, more profitable, and more likely to have received a comment letter raising accounting issues. After matching on lawyer, comment letter, and firm characteristics, we find evidence consistent with former SEC employees increasing resistance in the comment letter process: conversations involving former SEC employees involve more negotiation, and result in fewer financial statement amendments.

*Keywords:* SEC comment letters, external counsel, revolving door, regulatory capture  
*JEL:* G18, K22, M41, M48

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<sup>☆</sup>As this is a preliminary draft, please do not cite or circulate without permission. Comments are most welcome.

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## 1. Introduction

*“More than half the high-ranking SEC officials who left the agency since January 2016 landed at law and financial firms [...] Firms tout the expertise their recently acquired colleagues gained at the agency. Government watchdogs, however, cite concerns about the possible influence these individuals have over their former co-workers”.* (Ramonas, 2017, Bloomberg Law)

*“The revolving door between the government and the private sector has long been presumed to lead to the capture of regulators by industry interests.”* (Zheng, 2015)

Between 2001 and 2012, 455 former Securities and Exchange Commission (hereafter SEC) employees, over 10 percent of the number of employees at the SEC in the fiscal year 2012, disclosed in post-employment statements that they intended to represent an external party before the SEC. Of the 2,118 post-employment statements filed during the period, about two-thirds were filed by individuals formerly with the Division of Enforcement or the Division of Corporation Finance, which play critical roles in regulating corporate disclosure and enforcing securities law. Furthermore, these numbers represent only a low estimate of the impact of the revolving door between the SEC and the private sector because post-employment statements are required only within two years of leaving the SEC.<sup>1</sup>

The flow of personnel and expertise from the SEC to the private sector has been the subject of scrutiny and debate by government officials, advocacy groups, the business press, and academics in recent years. A 2013 report by the Project On Government Oversight (POGO) expressed the concern that “a rapidly spinning revolving door can weaken the agency’s protection of investors, enable regulated entities to exert undue influence, demoralize other government employees, and damage the public’s trust” (Smallberg, 2013, p. 37), and much of the recent media coverage on the issue has skewed negatively.<sup>2</sup>

Critics of the revolving door cite the risk of *regulatory capture*—the risk that regulators “created to act in the public interest [...] end up acting directly or indirectly in the interests of those they regulate.” (Brown, 2016, p. 1). One mechanism for regulatory capture is that

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<sup>1</sup>The number of SEC employees (4,388) is the number of full-time equivalents reported in its 2012 budget justification to Congress (see SEC, n.d., p. 15). Post-employment statement information is from the Project On Government Oversight’s SEC Revolving Door Database (see Project On Government Oversight, n.d.).

<sup>2</sup>For example, see “SEC Lawyer One Day, Opponent the Next” (McGinty, 2010a, *The Wall Street Journal*); “SEC ‘Revolving Door’ Under Review” (McGinty, 2010b, *The Wall Street Journal*); “SEC staff’s ‘revolving door’ prompts concerns about agency’s independence” (Hilzenrath, 2011, *The Washington Post*); and “S.E.C.’s Revolving Door Hurts Its Effectiveness, Report Says” (Protess & Craig, 2013, *The New York Times*).

the revolving door incentivizes individuals at regulatory agencies to act in the interest of prospective employers while they are at the agencies.<sup>3</sup> A second mechanism that is closely related to and may exacerbate the first is that individuals formerly at regulatory agencies may continue to influence former colleagues at the agency; this mechanism is a primary concern of POGO’s 2013 report.<sup>4</sup>

On the other hand, an alternative argument is that the revolving door improves regulatory compliance, and that ethics rules should safeguard against conflicts of interest that may arise. In a 2005 speech, then-SEC Commissioner Paul Atkins said that the revolving door improves compliance in the private sector because “people with regulatory backgrounds become good proselytes for lawful behavior” (Atkins, 2005); and in 2007 he said that

“people who leave government to return to industry can help to instill a proper sense of the importance of complying with legal obligations [...] strong ethics rules guard against potential conflicts of interest when people leave the government for the private sector.” (Atkins, 2007)

Former employees of regulatory agencies may also improve regulatory compliance because of the experience and expertise gained at the agencies. Law firms, for example, frequently mention in their lawyers’ online biographies if the lawyers were formerly employed by the SEC, suggesting that this experience is valuable to their clients. This argument is also related to the human capital hypothesis, an alternative story to regulatory capture: an agency official may “have an incentive to invest in and/or demonstrate his human capital skills while at the regulatory agency to increase his future prospects in industry” (deHaan et al., 2015, p. 66, see also Zheng, 2015).

The legal and political science literatures have discussed the determinants and consequences of regulatory capture and the revolving door between the public and private sectors since the 1970s.<sup>5</sup> However, empirical evidence on the impact of individual lawyers and the revolving door on financial reporting remains sparse despite the role played by the SEC in

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<sup>3</sup>See deHaan et al. (2015) for recent empirical evidence for and Zheng (2015) for a detailed discussion of this mechanism, and of the alternative human capital hypothesis.

<sup>4</sup>For example, “the mere fact that so many waiver requests involve former officials could influence the way people at the agency think about regulatory relief” (Smallberg, 2013, p. 9); see also Ramonas (2017, *Bloomberg Law*): “Government watchdogs, however, cite concerns about the possible influence these individuals have over their former co-workers”.

<sup>5</sup>Legal studies on the revolving door include Lacovara (1978), Morgan (1980), Mundheim (1980), Johnson (1983), Roberts, Jr. (1992), Parker (1998), McGuire (2000), and Zaring (2013). Analytical studies in political science on regulatory capture include Stigler (1971), Peltzman (1976), Bernheim & Whinston (1986), Tirole (1986), Laffont & Tirole (1991), and Che (1995); see also Dal Bó (2006) for a review of the analytical and empirical literature on regulatory capture.

regulating corporate disclosure. [deHaan et al. \(2015\)](#) find that SEC employees who leave the SEC to join private law firms are associated with more aggressive enforcement while at the SEC, consistent with the human capital hypothesis. Both [deHaan et al. \(2015, see Table 6\)](#) and [Shive & Forster \(2017, see Table IX\)](#) examine the impact of former regulatory agency employees, and both find only weak evidence that they influence enforcement outcomes after leaving the agencies.<sup>6</sup>

In our study, we examine whether the involvement of external counsel who were formerly with the SEC (“SEC-affiliated lawyers”) is associated with resistance in the SEC comment letter process. This setting allows us to directly examine the impact of the SEC revolving door on financial reporting, because the comment letter process is a critical element of the financial reporting system in the United States. The Sarbanes-Oxley Act requires the SEC to review each company’s filings at least every three years, and the SEC issues comments when its “staff identifies instances where it believes a company can improve its disclosure or enhance its compliance” ([SEC, 2017](#)).<sup>7</sup> Furthermore, firms often involve external counsel in responding to comment letters: external counsel were involved in over a third of our initial sample of comment letter conversations (see [Table 1](#); see also [Bozanic et al., 2016](#)).

In order to identify the involvement of SEC-affiliated lawyers in the SEC comment letter process, we retrieve the names of the individual lawyers and law firms involved in each comment letter conversation from Audit Analytics, and hand-collect the characteristics of the individual lawyers from public sources, restricting the hand-collection to lawyer-law firms with at least three conversations in our initial sample. Our hand-collected dataset includes the former SEC employment status, educational background, and other background characteristics of 593 unique lawyers. After attrition our final sample comprises 2,288 comment letter conversations initiated between 2005 and 2012 that involve external counsel, and about 7.6 percent of the sample involve lawyers who were formerly employed by the SEC.

We first examine the extent to which the variation between individual lawyers explains the variation in three sets of outcomes after a firm receives an SEC comment: (1) the

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<sup>6</sup>Specifically, both [deHaan et al. \(2015\)](#) and [Shive & Forster \(2017\)](#) find evidence in their respective settings that former regulatory agency employees reduce damages or fines in regulated firms; however they find weak or no evidence when examining other proxies of enforcement outcomes. A number of empirical studies have also examined the revolving door in other contexts: [Lucca et al. \(2014\)](#) examine the revolving door between bank regulators and the private sector, and [Luechinger & Moser \(2014\)](#) examine stock market reactions to U.S. Department of Defense appointments. See also [Dal Bó \(2006, p. 216–217\)](#) for a review of studies on the impact of regulatory capture on outcomes in the utilities industries and in trade regulation.

<sup>7</sup>Consistent with the critical role of the SEC comment letter process in the financial reporting system, the literature has found that comment letters improve disclosures ([Bozanic et al., 2017](#)), have spillover effects on other firms’ disclosures ([Brown et al., 2017](#)), and are associated with higher insider trading than normal before public disclosure of the comment letters ([Dechow et al., 2016](#)).

amount of negotiation, proxied by the timeliness of comment letter resolution; (2) 10-K or 10-Q amendment filings; and (3) future comment letter conversations, restatements, and litigation. We carry out two sets of tests; in one we require three observations per lawyer and law firms, and in the other we require ten observations. Across specifications, we find that individual lawyer fixed effects and individual law firm fixed effects are both statistically significant in explaining variation in negotiation and amendments, relative to control variable. Furthermore, lawyer fixed effects have significantly greater explanatory power than law firm fixed effects. We find mixed results across specifications for future conversations, restatements, and litigation.

Next, we examine the involvement of SEC-affiliated lawyers in the comment letter process. We find significant differences between firms that involve SEC-affiliated lawyers, and firms that only involve non-affiliated lawyers. In particular, firms that retain former SEC employees are significantly more likely to be responding to accounting-related comment letters from the SEC: 85 percent of comment letter conversations involving SEC-affiliated lawyers were initiated with a comment letter mentioning accounting issues, compared to 78 percent for conversations involving non-affiliated lawyers. Firms that retain SEC-affiliated lawyers are also larger, older, more profitable, and more likely to have been sued recently.

Finally, we examine the impact of SEC-affiliated lawyers on resistance in the comment letter process after matching conversations that do and do not involve SEC-affiliated lawyers. After matching on all controls—lawyer and law firm characteristics, initial comment letter characteristics, and firm controls—we find that involving an SEC-affiliated lawyer is associated with significantly more negotiation, and a significantly lower likelihood and number of amendments. We interpret our findings as consistent with SEC-affiliated lawyers successfully exerting their influence to reduce compliance with SEC comments. We find no evidence for an association between SEC affiliation and future conversations, restatements, and litigation over the two years following the initial comment letter, suggesting that the influence of SEC-affiliated lawyers is persistent over time.

Our study aims to contribute to two overlapping streams of research: the literature on the impact of external counsel on financial reporting, and the literature on SEC comment letters. The literature on external counsel is sparse likely due to data availability limitations, but recent studies suggest that lawyers and law firms can affect the financial reporting system in specific settings.<sup>8</sup> In particular, our study is related methodologically to [Bozanic et al. \(2016\)](#), who find that external counsel involvement increases resistance to the disclosure of

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<sup>8</sup>See, for example, [deHaan et al. \(2015\)](#), [Bozanic et al. \(2016\)](#), [Dechow & Tan \(2017\)](#).

new information following the receipt of a comment letter, and also affects other financial reporting outcomes. However, our research question is most closely related to [Heese et al. \(2017\)](#), who also examine regulatory capture in the SEC comment letter process, but from a different perspective: they find that firms are *more* likely to receive a comment letter if they are politically connected, based on Political Action Committee contributions or lobbying expenditure, contrary to the regulatory capture hypothesis. Our study, in contrast, examines outcomes after receiving a comment letter, and we examine regulatory capture in the SEC comment letter process via lawyers who were formerly employed by the SEC.

## 2. Hypothesis development

### 2.1. Individual lawyers

Recent studies have examined the role of corporate general counsel in firms' disclosure policies, financial reporting quality, and risk management. For example, [Kwak et al. \(2012\)](#) document that firms with general counsel in their top management teams are more likely to issue less optimistic and more accurate management forecasts, especially bad news forecasts. However, [Hopkins et al. \(2014\)](#) show that firms with highly compensated general counsel are associated with worse financial reporting quality and adopt more aggressive accounting policies. [Ham & Koharki \(2016\)](#) find evidence that the appointment of corporate general counsel to senior management is associated with increases in credit risk.

In our study, in contrast, we examine the impact of firms' *external* counsel. Our study is therefore related to [Bozanic et al. \(2016\)](#), who find that involvement of external counsel in the SEC comment letter process is associated with greater resistance to disclosing new information. Our study extends this line of research by examining the impact of individual lawyers' characteristics on the comment letter process. Individual lawyers could have a significant impact on the comment letter process. For example, from Audit Analytics' comment letters database, the lawyer involved in the most conversations has represented at least 30 companies in responding to 59 comment letters since 2005. Anecdotal evidence also suggests that client firms may select specific lawyers based on their individual expertise: law firms regularly include biographies of their lawyers on their websites, with detailed information including educational background and employment history.

The prior literature has also shown that variation between individuals play important roles in related contexts, including audit quality and credit ratings. For example, [Gul et al. \(2013\)](#) document that variation between individual auditors affects audit quality, and that this variation can partially be explained by individual characteristics such as education and Big N audit firm experience. [Guan et al. \(2016\)](#) find that school ties between audit partners

and executives of their clients impair audit quality: for example, auditors with school ties to their clients are more likely to issue favorable opinions, and their clients have higher discretionary accruals and are more likely to restate earnings. [Fracassi et al. \(2016\)](#) find that variation between individual credit analysts is associated with variation in firms' long-term credit ratings, and affects the cost of debt.

Motivated by the influence of individual lawyers in the comment letter process, and the importance of individual lawyers' characteristics to clients of law firms, we first establish whether variation in measures of resistance to SEC comment letters can be explained by variation between individual lawyers. Our first hypotheses, in null form, is therefore as follows:

**Hypothesis 1.** *There is no difference between individual lawyers in their clients' resistance to SEC comment letters.*

## *2.2. Impact of SEC affiliation*

Our next goal is to examine whether a specific characteristic of individual lawyers—former employment with the SEC—affects resistance to SEC comment letters. The flow of personnel from the SEC to the private sector has been the subject of debate and scrutiny by a wide range of stakeholders, with critics concerned about the risk of regulatory capture (see [Smallberg 2013](#), for example). One mechanism for regulatory capture of the SEC is that the revolving door between the SEC and the private sector may incentivize individuals at the SEC to act in the interest of prospective employers while they are at the agency. For example, [deHaan et al. \(2015\)](#) find that SEC employees who leave the SEC to join private law firms are associated with *more* aggressive enforcement while they are at the SEC, inconsistent with regulatory capture.

A second mechanism is that individuals formerly at the SEC may influence SEC staff in favor of their clients after they join the private sector. This mechanism is a primary concern of POGO's 2013 report (see [Smallberg, 2013](#)), and has received negative attention from the business press in recent years. For example, [McGinty \(2010a, \*The Wall Street Journal\*\)](#) highlighted how a former enforcement lawyer at the SEC represented a client in responding to an SEC request for information just eleven days after leaving the SEC. The article also cites a former SEC lawyer's comment that the "training and expertise gained at the SEC is put to use for the benefit of those working against the interests of investors".

On the other hand, former SEC employees may in fact improve regulatory compliance by their clients. Then-SEC Commissioner Paul Atkins said that former employees of regulatory



agencies encourage compliance in the private sector, and that ethics rules safeguard against conflicts of interest (Atkins, 2005, 2007). Furthermore, the evidence in deHaan et al. (2015) suggests that SEC employees believe that aggressive enforcement is valuable to the private sector. We therefore examine two competing hypotheses:

## **Hypothesis 2.**

*2a: Involvement of an SEC-affiliated lawyer is associated with greater resistance to SEC comment letters.*

*2b: Involvement of an SEC-affiliated lawyer is associated with lower resistance to SEC comment letters.*

### *2.3. Measures of resistance*

In this study, we examine three sets of outcomes after a firm receives a comment letter from the SEC: (1) the extent of negotiation; (2) 10-K or 10-Q amendment filings; and (3) future comment letter conversations, restatements, and litigation.<sup>9</sup>

If an external counsel uses his or her influence in the comment letter process for the benefit of a client, we would expect to observe greater negotiation with the SEC, and fewer amendment filings as a result of the SEC’s comments. We proxy for the extent of negotiation by respondents using the timeliness of resolution, similar to Bozanic et al. (2017). Specifically, we use three proxies for negotiation: the time in days between the first and last letters in the conversation, the number of comment letters in the conversation, and a dummy if the conversation takes at least two rounds of exchanges between the firm and the SEC to resolve.<sup>10</sup> Next, similar to Bozanic et al. (2016), we measure amendment filings using 10-K or 10-Q amendments filed between the first comment letter in the conversation and 90 days after the last comment letter. We use two proxies: the number of amendments filed, and a dummy for whether an amendment was filed.

Greater resistance to SEC comments could lead to an increased likelihood of future comment letter conversations, restatements, and even litigation, if the firm is ultimately forced to correct its disclosures. However, we caution that this prediction would not hold if, for example, the influence of external counsel on the SEC is persistent over the long run,

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<sup>9</sup>Please see [Appendix A](#) for formal variable definitions.

<sup>10</sup>That is, the conversation comprises at least five letters. We note that the timeliness of resolution has also been used as a proxy for the cost of remediation (Cassell et al., 2013), which could be correlated to whether a firm retains an SEC-affiliated lawyer. We mitigate this issue by matching along characteristics of the initial comment letter in one of the specifications of our tests of Hypothesis 2.

so that a firm is able to avoid corrections over multiple years. Following [Bozanic et al. \(2016\)](#), we examine future comment letter conversations and restatements during the two years following the start of the focal conversation. Using a two-year window for future conversations captures “off-cycle” comment letters because the SEC reviews company filings at least once every three years ([Bozanic et al., 2016](#)). Similarly, we use securities lawsuit filings during the two years following the start of the conversation.

### 3. Research design

#### 3.1. Individual lawyers

We examine whether variation between individual lawyers explains outcomes after receiving a comment letter by testing the joint significance of individual lawyer fixed effects. In our main tests, we compare models with lawyer fixed effects, initial comment letter controls, and firm controls against base models with only initial comment letter controls and firm controls.<sup>11</sup> A statistically significant improvement in model fit suggests that variation between individual lawyers is incremental to the control variables in explaining variation in comment letter outcomes. In addition, we compare the joint significance of individual lawyer fixed effects with that of law firm fixed effects.

We therefore estimate the following regression models for each of the response variables:

$$\text{Comment Letter Outcome} = \alpha + \gamma \text{ Controls} \tag{1}$$

$$\text{Comment Letter Outcome} = \alpha + \beta \text{ Law Firm Fixed Effects} + \gamma \text{ Controls} \tag{2}$$

$$\text{Comment Letter Outcome} = \alpha + \beta \text{ Lawyer Fixed Effects} + \gamma \text{ Controls} \tag{3}$$

For each response variable, we use likelihood ratio tests to compare the goodness of fit of Models 1 and 3, and Vuong tests to compare Models 2 and 3, because the latter models are non-nested. We begin our analyses using law firms involved in at least three conversations. We also replicate the tests using law firms involved in at least ten conversations, which mitigates overfitting but reduces the sample to conversations involving more experienced law firms.

#### 3.2. Impact of SEC affiliation

We examine the impact of SEC affiliation on comment letter outcomes by regressing each outcome variable against a dummy variable (*SEC*) that takes the value of one if the firm

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<sup>11</sup>Please see Section 3.3 for details on our control variables, and Appendix A for variable definitions.

referenced an SEC-affiliated lawyer in a comment letter, and zero otherwise:

$$\text{Negotiation} = \alpha + \beta \text{ SEC} + \gamma \text{ Controls} \quad (4)$$

$$\text{Amendments} = \alpha + \beta_1 \text{ SEC} + \beta_2 \text{ NumLetters} + \gamma \text{ Controls} \quad (5)$$

$$\text{Future Outcomes} = \alpha + \beta_1 \text{ SEC} + \beta_2 \text{ NumLetters} + \beta_3 \text{ Amendment} + \gamma \text{ Controls} \quad (6)$$

We include lawyer, law firm, comment letter, and firm controls as detailed in Section 3.3. We include a proxy for the extent of negotiation, *NumLetters*, in Equations 5 and 6, so that the regressions estimate the effect of involving SEC-affiliated lawyers directly, controlling for any indirect effect via the extent of negotiation. Likewise, we include *Amendment*, a dummy variable indicating whether a firm filed an amendment, in Equation 6.

To further mitigate bias due to confounding, we propensity-match treatment ( $SEC = 1$ ) and control ( $SEC = 0$ ) conversations. When estimating Equations 4, 5, and 6, we match treatment and control conversations using the following logistic models respectively:

$$\text{SEC} = \alpha + \gamma \text{ Covariates} \quad (7)$$

$$\text{SEC} = \alpha + \beta \text{ NumLetters} + \gamma \text{ Covariates} \quad (8)$$

$$\text{SEC} = \alpha + \beta_1 \text{ NumLetters} + \beta_2 \text{ Amendment} + \gamma \text{ Covariates} \quad (9)$$

We propensity-match treatment and control conversations using two methods with different sets of covariates:

- In our **first matching method**, we include lawyer and law firm characteristics as covariates, because they are important sources of difference between treatment and control observations (see Table 7).
- In our **second matching method**, we use all the control variables: lawyer and law firm characteristics, initial comment letter controls, and firm controls.

For both methods, we include industry fixed effects as covariates to take into account potential differences between departments within the SEC (see Baugh et al. 2017, Appendix A). We use full matching (Rosenbaum, 1991), which carries out one-to-many matching of treatment ( $SEC = 1$ ) and control ( $SEC = 0$ ) observations to minimize the differences in their propensity scores within subgroups. Observations not in the support of the propensity score are omitted before matching, and the matching procedure constructs a set of weights

that we apply to each observation remaining in the sample.<sup>12</sup>

In additional analyses, we control for variation between *law firms* by replacing the law firm expertise control variable (*LawFirmExp*) with law firm fixed effects. For these tests, we restrict the data to conversations represented by law firms involved in at least 10 conversations in order to mitigate the problem of perfect separation. For example, for our amendments tests, before imposing this restriction about 38 percent of the law firms in the sample are not involved in comment letters conversations that lead to amendments. However, a caveat of this restriction is that it reduces the sample to conversations involving more experienced law firms.<sup>13</sup>

### 3.3. Control variables

We use three sets of control variables in this study: lawyer and law firm characteristics, characteristics of the initial comment letter in the conversation, and firm controls. Please see [Appendix A](#) for detailed definitions of the variables used in our study, including the control variables.

Our lawyer and law firm controls are constructed from our hand-collected database described in more detail in Section 4. In order to control for the impact of securities law expertise (e.g. [Bozanic et al., 2016](#)), we include proxies for lawyers’ and law firms’ experience in the comment letter process—the number of comment letter conversations involving the lawyer or law firm over the previous year (*LawyerExp* and *LawFirmExp* respectively). We further control for other characteristics that may be correlated with expertise, including the lawyers’ education (*TopSchool* and *NumDegrees*) and age (*LawyerAge*). We include a control for the lawyer’s gender (*Female*), and a control for the number of lawyers referenced during the conversation (*NumLawyers*) because this would be mechanically correlated with the probability that a given lawyer was formerly employed by the SEC.

Next, we control for a set of characteristics of the initial comment letter in the con-

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<sup>12</sup>When matching along lawyer and law firm characteristics, we carry out the matching procedure within years. In other words, a treatment conversation can only be matched to control conversations that begin in the same calendar year. When matching along all controls, we relax this requirement and match within two-year windows, due to perfect separation within certain years; that is, in certain years, specific combinations of covariates perfectly predict the treatment.

<sup>13</sup>When estimating the model with law firm fixed effects, we do not use the above propensity-matching methods because the procedures involve further attrition. This additional attrition would then generate more law firms involved in less than 10 conversations that would have to be omitted from the sample, which would then necessitate repeating the matching procedure, and so on iteratively. We avoid this problem by starting with the sample after matching on all controls (the second matching method), and running the regressions without weighting. This allows us to maintain the restriction of at least 10 conversations per lawyer, while omitting extreme conversations that are outside the support of the propensity score.

versation, and a set of firm characteristics, similar to [Bozanic et al. \(2016\)](#). We include *NumFilings*, the number of SEC filings referenced by the initial comment letter, and *Acc*, *RegSK*, *RegSX*, *Risk*, and *Regis*, dummy variables equal to one if the initial comment letter referenced accounting rule, Regulation S-X, Regulation S-K, risk factor, or registration issues. We include a dummy variable, *Other*, equal to one if the comment letter references issues that do not fit other categories, as coded by Audit Analytics. We control for a large set of firm characteristics, including size, age, profitability, R&D, returns, and returns volatility, and past restatements and litigation (see [Appendix A](#)). Finally, we include industry fixed effects based on one-digit SIC codes to control for possible variation between offices at the SEC. Review of filings at the SEC’s Division of Corporation Finance is organized by office, with a given office covering a broad industry group; see, for example, [Baugh et al. \(2017, Appendix A\)](#).

## 4. Sample and descriptives

### 4.1. Sample selection and data sources

We retrieve comment letter conversations from Audit Analytics’ Comment Letter Conversations database, beginning with 70,940 completed comment letter conversations initiated between 2005 and 2012.<sup>14</sup> Our sample period and initial sample selection procedure (see Panel A of [Table 1](#)) follows [Bozanic et al. \(2016\)](#) closely. We require the firms in the conversations to be covered by Compustat as of the most recent fiscal year, and by CRSP as of the starting date of the conversation. This reduces the sample to 24,449 conversations involving 6,920 unique firms. Next, we require conversations to reference a 10-K filing, further reducing the sample to 12,252 conversations.<sup>15</sup> Similar to [Bozanic et al. \(2016\)](#), we omit conversations with only one letter and conversations below the bottom percentile of the time to resolution.<sup>16</sup> These steps result in an initial sample of 11,974 comment letter conversations involving 4,932 unique firms.

Next, we retrieve and require legal advisor data (see Panel B of [Table 1](#)). We retrieve data on law firm personnel referenced in firms’ replies to SEC comment letters (i.e., CORRESP files) using the People Search tool on [auditanalytics.com](#), restricting the search to comment

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<sup>14</sup>In other words, we require the first comment letter date (*first\_letter\_date*) to be between 2005 and 2012 inclusive.

<sup>15</sup>Comment letter conversations are unlikely to reference a 10-Q filing without referencing a 10-K filing: the sample size at this step (12,252 conversations) is only slightly smaller than the sample size had we included all periodic filings (12,462 conversations).

<sup>16</sup>We define the time to resolution as the number of days between the first and last comment letter in the conversation. The bottom percentile before this attrition step is seven days.

Table 1: Sample selection

	Unique conversations	Unique firm-years	Unique firms
<i>Panel A: Initial sample selection</i>			
Conversations initiated between 2005 and 2012	70,940	-	-
Require Compustat coverage	35,845	25,409	9,674
Require CRSP coverage	24,449	18,661	6,920
Restrict to conversations that reference a 10-K filing	12,252	12,072	4,965
Omit conversations with only one letter	12,054	11,968	4,944
Omit conversations in the bottom 1% of time to resolution	11,974	11,900	4,932
<i>Panel B: Requiring legal advisor data and variables for analysis</i>			
Restrict to conversations referencing legal counsel <sup>†</sup>	4,033	4,016	2,317
Require lawyer-law firm to have at least three conversations <sup>‡</sup>	2,481	2,468	1,289
Require the availability of hand-collected lawyer characteristics	2,455	2,442	1,275
Require the availability of variables used in the analyses	2,288	2,277	1,201

<sup>†</sup> We also omit 61 conversations in which the lawyer was not named. The 4,033 conversations at this step involve 2,485 unique lawyers and 491 unique law firms.

<sup>‡</sup> After requiring lawyer-law firms with at least three conversations, there are 603 unique lawyers and 199 unique law firms associated with the conversations in the sample.

letters referencing periodic filings. Of the 11,974 conversations in our initial sample, 4,033 conversations have replies by firms in which lawyers were referenced.<sup>17</sup> In other words, firms involved lawyers in about 34% of conversations, close to the proportion documented by [Bozanic et al. \(2016\)](#). In these 4,033 conversations, firms referenced a total of 2,485 unique lawyers and 491 unique law firms.

We next require the availability of hand-collected data on the background characteristics of lawyers referenced in firms' replies. For each unique lawyer-law firm combination, the characteristics we collect include the lawyer's educational history, gender, and SEC affiliation before joining the law firm, if any. We collect the data from public sources, primarily LinkedIn profiles and biographies on law firm websites. We restrict our hand-collection to the lawyer-law firms with at least three conversations in our initial sample, because our tests of lawyer and law firm fixed effects require multiple observations per lawyer or law firm.<sup>18</sup> This restricts the sample to 2,481 conversations involving 603 unique lawyers and 199 unique law firms.

<sup>17</sup>We omitted 61 conversations in which the lawyers were not named.

<sup>18</sup>Our cutoff of three conversations also reflects a trade-off between spending substantially more time to collect a more comprehensive sample, and focusing on the lawyers and law firms that have greater representation in the sample.

We were able to collect data for 593 unique lawyers from 194 unique law firms. After restricting our sample to conversations with hand-collected lawyer-law firm data, we have 2,455 unique conversations involving 1,275 unique firms. In other words, unavailability of public sources for our hand-collection resulted in about a 1.0% decline in our sample size, and about a 1.7% decline in unique lawyers.<sup>19</sup> Finally, we require the availability of variables used in our analyses, resulting in a final sample of 2,288 conversations involving 1,201 unique firms. The number of unique firm-years in the sample, 2,277, is very close to the sample size, indicating that the conversations in our sample map almost uniquely to firm-years.

#### 4.2. Descriptive statistics: lawyer characteristics

Our hand-collected dataset comprises 596 unique combinations of lawyers and law firms. We collect the data at the lawyer-law firm level to take moves between law firms into account. For example, to collect the *SEC* variable, we examine whether the lawyer was employed by the SEC before joining the focal law firm. Nevertheless, the lawyers in our sample very rarely switch law firms: there are 593 unique lawyers in our data, meaning that at most only three lawyers switched law firms. In addition, there are 194 unique law firms in our sample, indicating that each law firm has three lawyers representing firms in our sample on average.

Table 2 and Figure 1 show descriptive statistics for our dataset.<sup>20</sup> Panel A of Table 2 documents the characteristics of the 593 unique lawyers in our dataset. On average, a lawyer is referenced in about 4.8 comment letter conversations, and are almost always associated with only one unique law firm throughout the sample. About 5.6% of lawyers had been employed with the SEC, about 59.7% attended a university with a top-14 law school, and about 14.8% of the lawyers are female. Most lawyers have two degrees—generally a bachelor’s degree and a Juris Doctor degree.<sup>21</sup> Panel B of Table 2 documents the proportion of lawyers that attended each university with a top-14 law school; of these universities, Harvard is the most represented, with 13.0% of lawyers, while the University of Chicago is the least represented (1.7%).<sup>22</sup>

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<sup>19</sup>To the extent that missing lawyer characteristics are due to lawyer retirements, our sample may be biased very slightly towards comment letter conversations involving younger lawyers.

<sup>20</sup>Unless otherwise stated, the descriptives in this subsection are based on raw values, without winsorization or other outlier removal method.

<sup>21</sup>We were unable to collect educational information for a small proportion of the lawyer-law firm observations: 1.5% have no information on educational institutions, and 7.0% have no information on degree dates. To avoid further sample size reduction, we assume that lawyers have a minimum of two degrees, and that lawyers for whom educational institutions could not be retrieved did not attend universities with top-14 law schools. However, cases with missing degree dates (needed to construct *LawyerAge*) are dropped from final sample.

<sup>22</sup>The proportions do not sum to the total proportion of lawyers attending top universities because some lawyers may attend top universities twice, for example at the undergraduate and graduate level respectively.

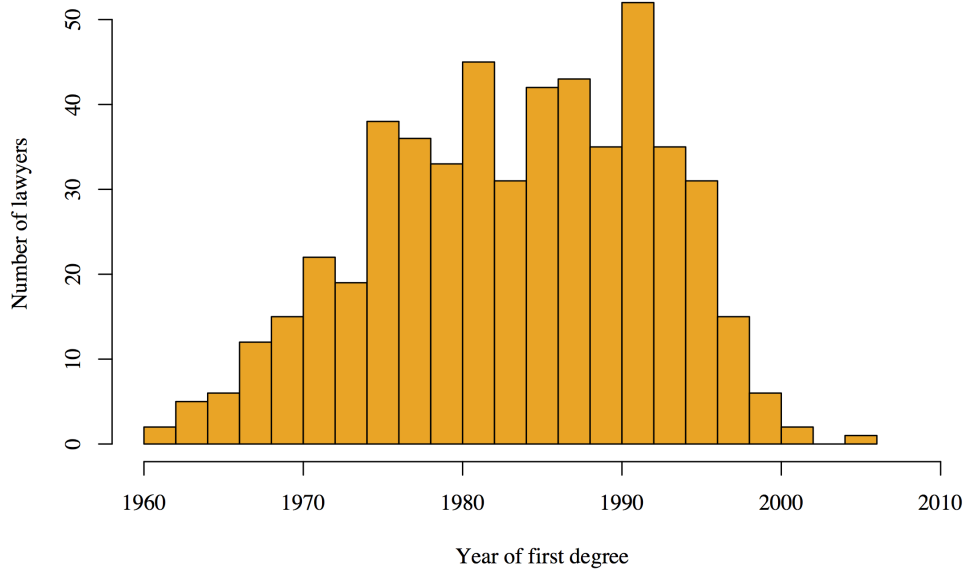
Table 2: Descriptives of the lawyer characteristics database

<i>Panel A: Characteristics per unique lawyer</i>								
	N	Mean	SD	Q1	Q25	Q50	Q75	Q99
# Conversations	593	4.782	3.003	3	3	4	5	18
# Law firms	593	1.005	0.071	1	1	1	1	1
<i>SEC</i>	593	0.056	0.229	0	0	0	0	1
<i>TopSchool</i>	593	0.597	0.491	0	0	1	1	1
<i>NumDegrees</i>	593	2.155	0.385	2	2	2	2	3
<i>Female</i>	593	0.148	0.356	0	0	0	0	1
<i>Panel B: University representation</i>								
University	# of Lawyers		% of Lawyers					
Harvard University	77		13.0					
University of Virginia	39		6.6					
University of Pennsylvania	37		6.2					
Stanford University	35		5.9					
Georgetown University	32		5.4					
New York University	32		5.4					
Yale University	32		5.4					
Cornell University	31		5.2					
University of California, Berkeley	28		4.7					
University of Michigan	28		4.7					
Columbia University	25		4.2					
Duke University	19		3.2					
Northwestern University	14		2.4					
University of Chicago	10		1.7					
Universities with top-14 law schools	354		59.7					
Without top-14 law schools	239		40.3					
Total	593		100					

This table shows descriptive statistics of our hand-collected database of lawyer characteristics, which comprises 596 unique combinations of lawyers and law firms. In Panel A, # Conversations is the number of comment letter conversations referencing the lawyer, before requiring the availability of other variables, and # Law firms is the number unique law firms the lawyer is associated with in our sample; the other variables in Panel A are as defined in [Appendix A](#). Panel B documents the proportion of unique lawyers attending each university with a top-14 law school. The total proportions may not sum to the proportion of lawyers attending any university with a top-14 law school because lawyers may attend more than one top university.



Figure 1: Year the lawyers earned their first degrees



This figure shows the histogram of the earliest years each lawyer in our sample earned a degree. Our sample includes 593 unique lawyers, 551 (92.9%) of whom have degree year data.

Figure 1 shows a histogram of the first year each lawyer in our sample earned a degree, for the 551 lawyers for whom degree year data is available. The median lawyer in our sample earned his or her first degree in 1985, about 23 years before the middle of our sample period. About 50% of the lawyers earned their first degree between 1977 and 1991 inclusive.

#### 4.3. Descriptive statistics: comment letter conversations

Table 3 documents descriptive statistics for our sample at the conversation level, which is the unit of observation in our study. There are 2,288 conversations in our final sample. At the conversation level, we reduce the impact of outliers by winsorizing annually count variables such as *NumAmend* and *NumFilings* at the top percentile, and other non-discrete variables such as *ConvTime* and *ROA* at the top and bottom percentiles.<sup>23</sup> Please see Appendix A for variable definitions.

Panel A of Table 3 shows descriptive statistics for the outcome variables. The median log-transformed conversation time is 4.159, corresponding to 63 days. A conversation comprises five letters at the median, and 55.2% of conversations have at least five letters. There is high variation in conversation length: at the first quartile a conversation lasts 35 days

<sup>23</sup>We do not winsorize variables based on returns, specifically *BHR* and *StdRet*.

and comprises three letters, while at the third quartile a conversation lasts 108 days and comprises six letters. About 19.3% of conversations result in a 10-K or 10-Q amendment, and the mean number of amendments is higher, at 0.263, because some conversations result in more than one amendment. Respectively, 59.5%, 13.2%, and 5.2% of conversations are followed by a new comment letter conversation, an adverse restatement, and a class action lawsuit over the two years following the start of the conversation.

Panel B of Table 3 shows descriptive statistics for the lawyer and law firm characteristics.<sup>24</sup> About 7.6% of conversations involve a lawyer who previously worked at the SEC. This is higher than the 5.6% at the lawyer level (see Panel A of Table 2), suggesting that on average, a lawyer who was previously employed by the SEC is involved in more conversations in the sample than a lawyer who was not. On average, the most experienced lawyer in the conversation was involved in close to one other conversation in the year before the focal conversation, the most experienced law firm in the conversation was involved in about nine other conversations, and a conversation involves about 1.15 lawyers. About 65.3% of conversations involve a lawyer who attended a university with a top-14 law school, and most lawyers have at most two degrees. The median log-transformed age of the oldest lawyer in the conversation is 3.296, corresponding to 26 years between the start of the conversation and the year of the lawyer’s first degree, and 15.2% of conversations involve female lawyers.

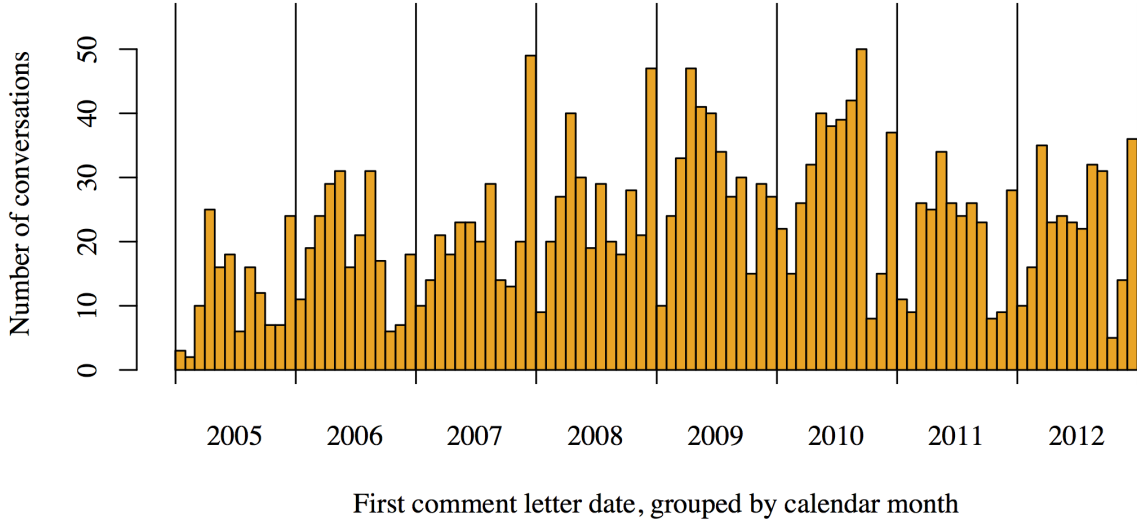
Panel C of Table 3 shows descriptive statistics for the comment letter controls. These variables are based on the initial comment letter in a conversation. The initial comment letters reference about two filings on average, and most (79.0%) reference accounting rule issues as coded by Audit Analytics. In addition, Regulation S-X, Regulation S-K, risk factor, and registration issues are referenced 17.1%, 59.1%, 12.5%, and 17.0% of the time respectively. Many issues, however, do not fit Audit Analytics’ categories: 82.7% had issues not specifically classified by Audit Analytics.

Finally, Panel D of Table 3 shows descriptive statistics for the firm controls. Unless otherwise stated, the firm controls are based on the most recent fiscal year ended before the start of a conversation. The median log-transformed firm size and age are 6.607 and 2.608 respectively, corresponding to a market capitalization of \$740 million, and about 13 years since the firm’s first appearance in CRSP. On average, the firms’ book-to-market ratio is 0.575, return on assets is about  $-1\%$ , and R&D intensity is about 6.2%. 63.9% of firms were

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<sup>24</sup>These variables have been aggregated to the conversation level in the case of conversations involving more than one lawyer or law firm. Specifically, we use the maximum values of *LawyerExp*, *LawFirmExp*, *NumDegrees*, and *LawyerAge* for such conversations. About 14.2% of conversations involve more than one lawyer.

Figure 2: Distribution of conversation start dates over time



This figure shows the distribution of the first comment letter dates of the 2,288 conversations in our final sample, over the sample period.

incorporated in Delaware, 94.9% of firms are domestic, and 79.7% are audited by a Big Four firm. The average raw buy-and-hold return over the 12 months prior to the conversation is 8.1%, and the standard deviation of returns over the 12 months is 0.139. The percentage of firms with adverse restatements and with class action litigation filings over the three years before the conversation is 19.0% and 7.0% respectively.

Figure 2 plots a histogram of the start dates of the conversations in our final sample, grouped by month. There are relatively more observations in the middle years of our sample period: slightly less than half (47.2%) of the conversations in our sample began between 2008 and 2010 inclusive. There is also some within-year pattern to the initial comment letter dates: conversations are generally less likely to be initiated in January and February, and in certain years there are large increases in initiations in December.

## 5. Individual lawyers

### 5.1. At least three clients

To test Hypothesis 1, we examine whether lawyer fixed effects have incremental explanatory power over the control variables and over law firm fixed effects, for each of the outcome variables. In other words, for each outcome variable we compare the goodness of fit of Equations 1 and 3, and Equations 2 and 3 respectively. In this subsection, we require lawyers and

Table 3: Descriptives of the comment letter conversations sample

	N	Mean	SD	Q1	Q25	Q50	Q75	Q99
<i>Panel A: Outcome variables</i>								
<i>ConvTime</i>	2,288	4.149	0.797	2.485	3.584	4.159	4.691	5.923
<i>NumLetters</i>	2,288	5.280	2.567	2	3	5	6	14
<i>MultiRound</i>	2,288	0.552	0.497	0	0	1	1	1
<i>NumAmend</i>	2,288	0.263	0.598	0	0	0	0	2
<i>Amendment</i>	2,288	0.193	0.395	0	0	0	0	1
<i>FutureConv</i>	2,288	0.595	0.491	0	0	1	1	1
<i>FutureRes</i>	2,288	0.132	0.338	0	0	0	0	1
<i>FutureLit</i>	2,288	0.052	0.223	0	0	0	0	1
<i>Panel B: Lawyer and law firm characteristics</i>								
<i>SEC</i>	2,288	0.076	0.266	0	0	0	0	1
<i>LawyerExp</i>	2,288	0.930	1.651	0	0	0	1	6
<i>LawFirmExp</i>	2,288	9.212	9.402	0	2	6	12	39
<i>NumLawyers</i>	2,288	1.147	0.369	1	1	1	1	2
<i>TopSchool</i>	2,288	0.653	0.476	0	0	1	1	1
<i>NumDegrees</i>	2,288	2.175	0.386	2	2	2	2	3
<i>LawyerAge</i>	2,288	3.254	0.316	2.485	3.045	3.296	3.497	3.829
<i>Female</i>	2,288	0.152	0.359	0	0	0	0	1
<i>Panel C: Initial comment letter controls</i>								
<i>NumFilings</i>	2,288	1.942	1.007	1	1	2	2	5
<i>Acc</i>	2,288	0.790	0.408	0	1	1	1	1
<i>RegSX</i>	2,288	0.171	0.376	0	0	0	0	1
<i>RegSK</i>	2,288	0.591	0.492	0	0	1	1	1
<i>Risk</i>	2,288	0.125	0.330	0	0	0	0	1
<i>Regis</i>	2,288	0.170	0.376	0	0	0	0	1
<i>Other</i>	2,288	0.827	0.378	0	1	1	1	1
<i>Panel D: Firm controls</i>								
<i>Size</i>	2,288	6.612	1.828	2.624	5.316	6.607	7.897	11.244
<i>FirmAge</i>	2,288	2.528	0.804	0.792	1.997	2.608	3.032	4.370
<i>BTM</i>	2,288	0.575	0.688	-0.508	0.250	0.457	0.757	3.007
<i>ROA</i>	2,288	-0.010	0.206	-0.839	-0.022	0.026	0.074	0.412
<i>RD</i>	2,288	0.062	0.114	0	0	0	0.082	0.547
<i>Delaware</i>	2,288	0.639	0.481	0	0	1	1	1
<i>Domestic</i>	2,288	0.949	0.220	0	1	1	1	1
<i>BigFour</i>	2,288	0.797	0.402	0	1	1	1	1
<i>BHR</i>	2,288	0.081	0.610	-0.848	-0.281	-0.00000	0.299	2.390
<i>StdRet</i>	2,288	0.139	0.094	0.031	0.080	0.117	0.171	0.525
<i>PastRes</i>	2,288	0.190	0.392	0	0	0	0	1
<i>PastLit</i>	2,288	0.070	0.255	0	0	0	0	1

This table shows descriptive statistics for the variables used in the study, for the 2,288 comment letter conversations in our final sample. Please see [Appendix A](#) for variable definitions. Each year, we winsorize count variables such as *NumAmend* and *NumFilings* at the top percentile, and other non-discrete variables such as *ConvTime* and *ROA* at the top and bottom percentiles. We do not winsorize variables based on returns, specifically *BHR* and *StdRet*.

law firms to have at least three clients in the sample. This requirement reduces the sample to 2,279 conversations involving 179 unique law firms and 556 unique lawyers. Table 4 documents our results; please see Appendix A for formal variable definitions. In Section 5.2, we replicate the analyses after requiring lawyers and law firms to have a minimum of ten clients, which reduces the sample size dramatically but mitigates overfitting.<sup>25</sup>

Panel A of Table 4 reports model fits using only comment letter and firm controls, including year and industry fixed effects (Equation 1). The first five columns suggest that the comment letter and firm controls explain between 7.43 and 10.46 percent of the variation in negotiation and amendments. However, there is more variation in the extent to which the controls explain outcomes over the two years following the start of the comment letter conversations: they explain about 8.69 percent of the variation in future comment letters, but only two to four percent of the variation in future litigation and restatements. Panel B of Table 4 suggests that adding law firm fixed effects increases the adjusted R-squared across all models by between 5.73 and 88.11 percent despite the penalty from adding more variables. The results of the likelihood ratio tests show that the law firm fixed effects are jointly significant in explaining all of the outcome variables except *FutureConv*, relative to the controls.

Finally, we examine lawyer fixed effects in Panel C of Table 4. Across all outcome variables, variation between individual lawyers is jointly significant in explaining the variation in outcomes relative to the control variables, with the adjusted R-squared increasing substantially in all outcomes despite the penalty to adding additional explanatory variables. In addition, the results of the Vuong tests indicate that models with controls and lawyer fixed effects fit significantly better than models with controls and law firm fixed effects, again with the adjusted R-squared increasing in all outcomes.

## 5.2. At least ten clients

We next replicate the analyses in Section 5.1, except that we require lawyers and law firms to have at least ten observations in the sample. The results are documented at Table 5. This reduces the sample size to only 415 conversations involving 18 unique law firms and 28 unique lawyers. Requiring a minimum of ten observations mitigates overfitting, but an important caveat is that this sample may not be representative of our full sample; in particular, it would be biased towards clients of the largest law firms and most experienced lawyers.

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<sup>25</sup>In addition, we use linear models across all outcomes so that measures of model fit (particularly the adjusted R-squared) is comparable across models, and to avoid the problem of complete separation in binary models.

Table 4: Impact of lawyer fixed effects, requiring three observations per lawyer and law firm (N = 2,279)

<i>Panel A: Models with comment letter and firm controls only</i>								
	<i>ConvTime</i>	<i>NumLetters</i>	<i>MultiRound</i>	<i>NumAmend</i>	<i>Amendment</i>	<i>FutureConv</i>	<i>FutureRes</i>	<i>FutureLit</i>
$\bar{R}^2$	8.96%	8.29%	7.43%	10.46%	8.80%	8.69%	3.85%	2.28%
$\chi^2_{\emptyset}$	249.27	232.44	211.26	287.20	245.21	242.44	124.86	87.96
(p-value)	(< 1%)	(< 1%)	(< 1%)	(< 1%)	(< 1%)	(< 1%)	(< 1%)	(< 1%)
<i>Panel B: Models with controls and law firm fixed effects</i>								
	<i>ConvTime</i>	<i>NumLetters</i>	<i>MultiRound</i>	<i>NumAmend</i>	<i>Amendment</i>	<i>FutureConv</i>	<i>FutureRes</i>	<i>FutureLit</i>
$\bar{R}^2$	10.09%	11.02%	7.86%	12.25%	10.97%	8.77%	5.41%	4.30%
$\chi^2_{\emptyset}$	466.03	489.87	410.21	521.60	488.49	432.85	350.42	323.84
(p-value)	(< 1%)	(< 1%)	(< 1%)	(< 1%)	(< 1%)	(< 1%)	(< 1%)	(< 1%)
$\% \Delta \bar{R}^2_C$	12.55%	33.00%	5.73%	17.08%	24.64%	0.91%	40.31%	88.11%
$\chi^2_C$	216.76	257.43	198.94	234.40	243.28	190.41	225.56	235.88
(p-value)	(2.52%)	(< 1%)	(13.48%)	(< 1%)	(< 1%)	(24.88%)	(< 1%)	(< 1%)
<i>Panel C: Models with controls and lawyer fixed effects</i>								
	<i>ConvTime</i>	<i>NumLetters</i>	<i>MultiRound</i>	<i>NumAmend</i>	<i>Amendment</i>	<i>FutureConv</i>	<i>FutureRes</i>	<i>FutureLit</i>
$\bar{R}^2$	13.22%	15.10%	8.93%	13.25%	12.32%	9.90%	13.10%	9.53%
$\chi^2_{\emptyset}$	1,001.02	1,050.84	890.89	1,001.69	977.49	915.34	997.67	906.12
(p-value)	(< 1%)	(< 1%)	(< 1%)	(< 1%)	(< 1%)	(< 1%)	(< 1%)	(< 1%)
$\% \Delta \bar{R}^2_C$	47.56%	82.23%	20.15%	26.62%	40.05%	13.94%	239.82%	317.34%
$\chi^2_C$	751.74	818.41	679.63	714.49	732.28	672.90	872.81	818.16
(p-value)	(< 1%)	(< 1%)	(< 1%)	(< 1%)	(< 1%)	(< 1%)	(< 1%)	(< 1%)
$\% \Delta \bar{R}^2_{LF}$	31.11%	37.01%	13.64%	8.15%	12.36%	12.92%	142.20%	121.86%
$Z^2_{LF}$	11.59	8.82	12.13	8.61	10.90	12.39	10.41	7.24
(p-value)	(< 1%)	(< 1%)	(< 1%)	(< 1%)	(< 1%)	(< 1%)	(< 1%)	(< 1%)

This table documents the model fit when regressing each of the conversation outcome variables against respective sets of controls and fixed effects using least squares, and examines the joint significance of lawyer fixed effects, comparing it with the joint significance of law firm fixed effects. We restrict the data to lawyers and law firms with at least three observations in the final sample, reducing the sample to 2,279 conversations involving 179 unique law firms and 556 unique lawyers. Panel A documents the fit of models with only comment letter and firm controls. Panel B documents the fit of models with controls and law firm fixed effects, and examines the joint significance of law firm fixed effects. Panel C documents the fit of models with controls and lawyer fixed effects, examines the joint significance of lawyer fixed effects, and compares the joint significant of lawyer and law firm fixed effects. Definitions:

$\bar{R}^2$ : Adjusted R-squared of the model.

$\chi^2_{\emptyset}$ : Likelihood ratio test statistic, comparing against the intercept model.

$\% \Delta \bar{R}^2_C$ : % increase in adjusted R-squared, relative to the model with controls only.

$\chi^2_C$ : Likelihood ratio test statistic, comparing against the model with controls only.

$\% \Delta \bar{R}^2_{LF}$ : % increase in adjusted R-squared, relative to the model with controls and law firm fixed effects.

$Z^2_{LF}$ : Vuong test statistic, comparing against the model with controls and law firm fixed effects.

Table 5: Impact of lawyer fixed effects, requiring ten observations per lawyer and law firm (N = 415)

<i>Panel A: Models with comment letter and firm controls only</i>								
	<i>ConvTime</i>	<i>NumLetters</i>	<i>MultiRound</i>	<i>NumAmend</i>	<i>Amendment</i>	<i>FutureConv</i>	<i>FutureRes</i>	<i>FutureLit</i>
$\bar{R}^2$	14.77%	9.74%	7.96%	9.63%	5.78%	7.22%	5.04%	1.73%
$\chi^2_{\emptyset}$	101.87	78.10	69.98	77.60	60.27	66.65	57.04	42.81
(p-value)	(< 1%)	(< 1%)	(< 1%)	(< 1%)	(< 1%)	(< 1%)	(< 1%)	(14.29%)
<i>Panel B: Models with controls and law firm fixed effects</i>								
	<i>ConvTime</i>	<i>NumLetters</i>	<i>MultiRound</i>	<i>NumAmend</i>	<i>Amendment</i>	<i>FutureConv</i>	<i>FutureRes</i>	<i>FutureLit</i>
$\bar{R}^2$	18.00%	11.43%	10.60%	10.80%	7.14%	6.94%	6.34%	1.01%
$\chi^2_{\emptyset}$	138.08	106.09	102.22	103.12	86.45	85.55	82.88	59.90
(p-value)	(< 1%)	(< 1%)	(< 1%)	(< 1%)	(< 1%)	(< 1%)	(< 1%)	(21.10%)
$\% \Delta \bar{R}^2_C$	21.93%	17.35%	33.25%	12.09%	23.56%	-3.86%	25.71%	-41.91%
$\chi^2_C$	36.22	27.99	32.24	25.52	26.18	18.90	25.85	17.09
(p-value)	(< 1%)	(6.23%)	(2.06%)	(11.12%)	(9.57%)	(39.83%)	(10.34%)	(51.72%)
<i>Panel C: Models with controls and lawyer fixed effects</i>								
	<i>ConvTime</i>	<i>NumLetters</i>	<i>MultiRound</i>	<i>NumAmend</i>	<i>Amendment</i>	<i>FutureConv</i>	<i>FutureRes</i>	<i>FutureLit</i>
$\bar{R}^2$	21.80%	15.46%	12.52%	12.95%	10.71%	9.16%	3.92%	6.21%
$\chi^2_{\emptyset}$	169.39	137.03	122.83	124.88	114.33	107.20	83.94	93.94
(p-value)	(< 1%)	(< 1%)	(< 1%)	(< 1%)	(< 1%)	(< 1%)	(3.33%)	(< 1%)
$\% \Delta \bar{R}^2_C$	47.66%	58.69%	57.33%	34.44%	85.27%	26.92%	-22.18%	258.78%
$\chi^2_C$	67.52	58.92	52.86	47.29	54.06	40.55	26.90	51.13
(p-value)	(< 1%)	(< 1%)	(< 1%)	(1.28%)	(< 1%)	(5.90%)	(52.35%)	(< 1%)
$\% \Delta \bar{R}^2_{LF}$	21.10%	35.22%	18.07%	19.94%	49.94%	32.02%	-38.10%	517.63%
$Z^2_{LF}$	2.74	2.63	2.12	2.10	2.38	2.42	0.14	2.63
(p-value)	(< 1%)	(< 1%)	(1.68%)	(1.77%)	(< 1%)	(< 1%)	(44.44%)	(< 1%)

This table documents the model fit when regressing each of the conversation outcome variables against respective sets of controls and fixed effects using least squares, and examines the joint significance of lawyer fixed effects, comparing it with the joint significance of law firm fixed effects. We restrict the data to lawyers and law firms with at least ten observations in the final sample, reducing the sample to 415 conversations involving 18 unique law firms and 28 unique lawyers. Panel A documents the fit of models with only comment letter and firm controls. Panel B documents the fit of models with controls and law firm fixed effects, and examines the joint significance of law firm fixed effects. Panel C documents the fit of models with controls and lawyer fixed effects, examines the joint significance of lawyer fixed effects, and compares the joint significant of lawyer and law firm fixed effects.

Definitions:

$\bar{R}^2$ : Adjusted R-squared of the model.

$\chi^2_{\emptyset}$ : Likelihood ratio test statistic, comparing against the intercept model.

$\% \Delta \bar{R}^2_C$ : % increase in adjusted R-squared, relative to the model with controls only.

$\chi^2_C$ : Likelihood ratio test statistic, comparing against the model with controls only.

$\% \Delta \bar{R}^2_{LF}$ : % increase in adjusted R-squared, relative to the model with controls and law firm fixed effects.

$Z^2_{LF}$ : Vuong test statistic, comparing against the model with controls and law firm fixed effects.

The results documented in Panel A of Table 5 suggest that at conventional significance levels, the controls are significant in explaining all the comment letter outcomes variables with the exception of *FutureLit*. The results in Panel B suggest that variation between law firms is significant or weakly significant in explaining the variation in conversation length and amendments relative to the control variables; however the impact of law firms is mixed in explaining future comment letters, restatements, and litigation.

Panel C of Table 5 documents the joint significance of lawyer fixed effects relative to the firm and comment letter controls, and compares the fit of models with lawyer fixed effects with models with law firm fixed effects. We find that across all comment letter outcomes other than *FutureRes*, the variation between individual lawyers is significant in explaining comment letter outcomes relative to comment letter and firm controls, with an increase in adjusted R-squared between 26.92 and 258.78 percent. Similarly, across all outcomes other than *FutureRes*, models with controls and lawyer fixed effects fit significantly better than models with law firm fixed effects.

## 6. SEC affiliation

### 6.1. Univariate tests

To examine Hypothesis 2, we first examine univariate differences in outcomes after the receipt of an SEC comment letter, between firms do and do not involve SEC-affiliated lawyers ( $SEC = 1$  or  $0$ ). We report the univariate differences at Table 6.

Panel A of Table 6 documents the results based on the full sample. The univariate results generally suggest that SEC affiliation is associated with significantly more negotiation. The difference in log-transformed time to resolution (*ConvTime*) is 0.16, indicating that conversations take about 17 percent longer ( $e^{0.16} = 1.17$ ) to resolve when an SEC-affiliated lawyer is involved. In addition, the involvement of an SEC-affiliated layer is associated with about half an additional comment letter on average, and a nine percentage point higher probability of that the conversation takes at least two exchanges of letters to resolve. Involving an SEC-affiliated lawyer is also associated with slightly fewer amendments and a slightly lower likelihood of an amendment, but only the former is significant at conventional significance levels. There is no evidence at conventional significance levels that SEC-affiliation is associated with future conversations, restatements, or litigation.

Panels B and C of Table 6 examine the univariate differences within sample partitions. In Panel B, the columns on the left are based on conversations where lawyer expertise is at or below the median each year and the columns on the right are based on conversations where



Table 6: Impact of representation by an SEC-affiliated lawyer, univariate tests

<i>Panel A: Overall sample (N = 2,288)</i>								
Means / Prop.	<i>SEC</i> = 0		<i>SEC</i> = 1		Diff.	(p-value)		
<i>ConvTime</i>	4.14		4.30		0.16**	(1.19%)		
<i>NumLetters</i>	5.24		5.74		0.50**	(3.52%)		
<i>MultiRound</i>	0.55		0.63		0.09**	(2.46%)		
<i>NumAmend</i>	0.27		0.18		-0.09**	(1.45%)		
<i>Amendment</i>	0.20		0.17		-0.03	(37.61%)		
<i>FutureConv</i>	0.59		0.63		0.04	(38.40%)		
<i>FutureRes</i>	0.13		0.13		0.00	(100%)		
<i>FutureLit</i>	0.05		0.06		0.01	(59.62%)		

<i>Panel B: Partitioning by lawyer expertise</i>								
Means / Prop.	<i>LawyerExp</i> ≤ annual median				<i>LawyerExp</i> > annual median			
	<i>SEC</i> = 0	<i>SEC</i> = 1	Diff.	(p-value)	<i>SEC</i> = 0	<i>SEC</i> = 1	Diff.	(p-value)
<i>ConvTime</i>	4.13	4.30	0.16*	(8.70%)	4.14	4.30	0.16*	(7.17%)
<i>NumLetters</i>	5.22	5.85	0.63*	(5.40%)	5.29	5.67	0.38	(25.71%)
<i>MultiRound</i>	0.55	0.68	0.13**	(3.80%)	0.55	0.61	0.06	(28.76%)
<i>NumAmend</i>	0.27	0.18	-0.09*	(9.32%)	0.26	0.18	-0.08	(10.16%)
<i>Amendment</i>	0.19	0.17	-0.02	(64.76%)	0.20	0.16	-0.03	(42.97%)
<i>FutureConv</i>	0.58	0.48	-0.10	(11.39%)	0.62	0.73	0.11**	(3.77%)
<i>FutureRes</i>	0.14	0.14	0.003	(100%)	0.12	0.12	0.01	(100%)
<i>FutureLit</i>	0.05	0.06	0.01	(78.17%)	0.06	0.07	0.01	(82.47%)

<i>Panel C: Partitioning by lawyer education</i>								
Means / Prop.	<i>TopSchool</i> = 0				<i>TopSchool</i> = 1			
	<i>SEC</i> = 0	<i>SEC</i> = 1	Diff.	(p-value)	<i>SEC</i> = 0	<i>SEC</i> = 1	Diff.	(p-value)
<i>ConvTime</i>	4.13	4.24	0.11	(20.73%)	4.14	4.39	0.25**	(1.21%)
<i>NumLetters</i>	5.19	5.57	0.39	(15.11%)	5.27	6.01	0.75	(10.14%)
<i>MultiRound</i>	0.55	0.64	0.09*	(9.58%)	0.54	0.63	0.08	(21.07%)
<i>NumAmend</i>	0.30	0.20	-0.10*	(6.00%)	0.25	0.15	-0.11**	(2.68%)
<i>Amendment</i>	0.22	0.18	-0.05	(26.51%)	0.18	0.15	-0.03	(52.14%)
<i>FutureConv</i>	0.56	0.63	0.07	(21.21%)	0.61	0.63	0.02	(79.86%)
<i>FutureRes</i>	0.13	0.14	0.01	(87.35%)	0.13	0.12	-0.01	(85.18%)
<i>FutureLit</i>	0.05	0.05	0.00	(100%)	0.05	0.09	0.04	(27.09%)

This table documents univariate differences in comment letter outcomes when an SEC-affiliated lawyer is involved (*SEC* = 1) and otherwise (*SEC* = 0). Please see [Appendix A](#) for variable definitions. For the binary outcome variables (*MultiRound*, *Amendment*, *FutureConv*, *FutureRes*, and *FutureLit*) we examine the difference in proportions using chi-squared tests with p-values computed by Monte Carlo stimulation with 10,000 replicates. For the other outcome variables we examine the difference in means using t-tests.

lawyer expertise is above the median each year. The results suggest that much, but not all, of the impact of SEC affiliation is driven by lawyers with lower expertise: the differences for *NumLetters* and *MultiRound* are statistically significant among low-expertise lawyers and clearly insignificant at conventional significance levels for high-expertise lawyers. However, the impact of expertise is less clear for other variables. In Panel C, the sample is partitioned on whether the conversation involved a lawyer who attended a university with a top-14 law school. The results here are mixed; for example, the difference in *ConvTime* is driven more by lawyers from top schools, while the difference in *MultiRound* is driven more by lawyers not from top schools.

## 6.2. Potential confounders

Other covariates, however, could confound the relationship between representation by an SEC-affiliated lawyer and our outcome variables. For example, if SEC-affiliated lawyers are more experienced on average, they may be more effective at helping companies deal with SEC comments in a timely fashion due simply to their experience, which would work in the opposite direction to our hypothesis. Alternatively, more experienced counsel may exercise more care in dealing with SEC comments in order to protect their reputations, reducing the timeliness of resolution.

Table 7 documents the difference between the treatment ( $SEC = 1$ ) and control ( $SEC = 0$ ) subsamples in each of the control variables. Panel A documents that the subsamples are significantly different in all but one of the lawyer and law firm characteristics in our study. Conversations involving SEC-affiliated lawyers involve slightly more lawyers on average, and lawyers who are more experienced, less likely to be from top schools, have more degrees, are older, and are more likely to be female. In particular, the most experienced lawyer for a treatment conversation was involved in 2.21 conversations in the previous year on average, more than double the average number for control conversations (0.82). Panel B documents that treatment and control SEC conversations have statistically indistinguishable initial comment letter characteristics, with two exceptions. Firms that retain former SEC employees are about seven percentage points more likely to have received a comment letter concerning accounting issues, and about five percentage points more likely to have received a comment letter issues related to Regulation S-X.

Panel C of Table 7 documents that the treatment and control subsamples have significantly different firm characteristics. Firms that involve SEC-affiliated lawyers are significantly larger, older, and more profitable, suggesting that hiring former SEC lawyers is more costly on average, or that more reputable firms place greater value on the expertise or influence of former SEC employees. Treatment firms are also significantly less R&D-intensive

Table 7: Relationship between control variables and SEC affiliation

Means / Prop.	$SEC = 0$	$SEC = 1$	Diff.	(p-value)
<i>Panel A: Lawyer and law firm characteristics</i>				
<i>LawyerExp</i>	0.82	2.21	1.39***	(0.00%)
<i>LawFirmExp</i>	9.19	9.52	0.33	(51.52%)
<i>NumLawyers</i>	1.14	1.23	0.09**	(1.70%)
<i>TopSchool</i>	0.67	0.38	-0.29***	(0.01%)
<i>NumDegrees</i>	2.17	2.23	0.06*	(8.09%)
<i>LawyerAge</i>	3.24	3.39	0.15***	(0.00%)
<i>Female</i>	0.14	0.24	0.10***	(0.12%)
<i>Panel B: Initial comment letter controls</i>				
<i>NumFilings</i>	1.93	2.05	0.11	(17.71%)
<i>Acc</i>	0.78	0.85	0.07**	(4.31%)
<i>RegSX</i>	0.17	0.22	0.05*	(9.46%)
<i>RegSK</i>	0.59	0.57	-0.02	(63.81%)
<i>Risk</i>	0.13	0.10	-0.02	(40.56%)
<i>Regis</i>	0.17	0.19	0.02	(53.25%)
<i>Other</i>	0.83	0.83	0.002	(100%)
<i>Panel C: Firm controls</i>				
<i>Size</i>	6.56	7.30	0.74***	(0.00%)
<i>FirmAge</i>	2.50	2.81	0.30***	(0.00%)
<i>BTM</i>	0.57	0.59	0.02	(77.33%)
<i>ROA</i>	-0.01	0.02	0.03***	(0.62%)
<i>RD</i>	0.06	0.04	-0.02***	(0.02%)
<i>Delaware</i>	0.64	0.61	-0.04	(36.39%)
<i>Domestic</i>	0.95	0.95	-0.00	(100%)
<i>BigFour</i>	0.79	0.83	0.04	(20.57%)
<i>BHR</i>	0.08	0.07	-0.01	(85.47%)
<i>StdRet</i>	0.14	0.13	-0.01	(21.85%)
<i>PastRes</i>	0.19	0.22	0.04	(26.93%)
<i>PastLit</i>	0.07	0.12	0.05***	(0.92%)

This table documents univariate differences in the control variables when an SEC-affiliated lawyer is involved ( $SEC = 1$ ) and otherwise ( $SEC = 0$ ). Please see [Appendix A](#) for variable definitions. For binary control variables, we examine the difference in proportions using chi-squared tests with p-values computed by Monte Carlo stimulation with 10,000 replicates. For the other control variables we examine the difference in means using t-tests.

than control firms. Finally, firms that involve SEC-affiliated lawyers have a five percentage point greater probability of having faced a class action lawsuit in recent years, suggesting that firms may view representation by a former SEC lawyer as a way to protect against further litigation.

### 6.3. Descriptives before and after matching

Due to the differences between treatment and control observations summarized in Section 6.2, our analyses control for potential confounding. We include the control variables in our regressions to mitigate linear confounding, and also implement the two propensity matching methods described in our research design (see Section 3.2).

Table 8 shows the impact of matching treatment ( $SEC = 1$ ) and control ( $SEC = 0$ ) observations on the control variables using both matching methods.<sup>26</sup> The two matching procedures reduce the sample size from 2,288 to 1,529 and 1,635 respectively as observations outside the support of the propensity score are omitted. Table 8 reports the means and differences in means after these observations are omitted. Columns 2 to 5 are based on the means before applying the matching weights, and columns 6 to 9 are based on the means after applying the matching weights. Panel A of Table 8 is based on the first matching method, in which we match along the lawyer and law firm characteristics. The differences between treatment and control firms in most of the lawyer and law firm characteristics are significant before weighting, but all the differences become statistically insignificant at conventional significance levels after weighting. Panel B of Table 8 is based on the second matching method, in which we match along all controls. After weighting, the differences are insignificant for all controls.

### 6.4. Extent of negotiation

We first examine the impact of representation by SEC-affiliated lawyers in comment letter conversations on the extent of negotiation following receipt of a comment letter. Similar to Bozanic et al. (2017), we proxy for the extent of negotiation using the timeliness of resolution. Our variables, *ConvTime*, *NumLetters*, and *MultiRound*, are based either on the length of the conversation in days, or on the number of comment letters in the conversation.

Figures 3 and 4 plot the distributions of the length of the conversations in days, scaled logarithmically (*ConvTime*), and the raw number of letters in the conversation (*NumLetters*), respectively. Figure 3 shows density plots, where the area of the plot within a given range give the probability that the variable falls within the range. Figure 4 shows histograms

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<sup>26</sup>For brevity we only include descriptives for propensity matching based on Model 7.

Table 8: Impact of our matching procedures on the control variables

<i>Panel A: Matching on lawyer and law firm characteristics</i>								
	Means before weighting (N = 1,529)				Means after weighting (N = 1,529)			
	<i>SEC</i> = 0	<i>SEC</i> = 1	Diff.	(p-value)	<i>SEC</i> = 0	<i>SEC</i> = 1	Diff.	(p-value)
<i>Lawyer and law firm characteristics</i>								
<i>LawyerExp</i>	0.98	1.64	0.66***	(0.00%)	1.44	1.64	0.20	(48.24%)
<i>LawFirmExp</i>	8.65	8.54	-0.11	(71.55%)	7.96	8.54	0.58	(68.95%)
<i>NumLawyers</i>	1.16	1.24	0.08**	(3.47%)	1.20	1.24	0.04	(64.76%)
<i>TopSchool</i>	0.59	0.41	-0.18***	(0.01%)	0.38	0.41	0.03	(50.76%)
<i>NumDegrees</i>	2.19	2.23	0.03	(30.45%)	2.21	2.23	0.01	(78.63%)
<i>LawyerAge</i>	3.31	3.37	0.07**	(1.39%)	3.35	3.37	0.02	(60.63%)
<i>Female</i>	0.18	0.25	0.07*	(4.70%)	0.27	0.25	-0.02	(59.69%)
<i>Initial comment letter controls</i>								
<i>NumFilings</i>	1.93	2.13	0.21**	(3.89%)	1.88	2.13	0.25***	(0.47%)
<i>Acc</i>	0.78	0.83	0.05	(12.63%)	0.74	0.83	0.09***	(0.80%)
<i>RegSX</i>	0.17	0.23	0.06*	(7.77%)	0.14	0.23	0.09***	(0.31%)
<i>RegSK</i>	0.61	0.58	-0.03	(41.40%)	0.65	0.58	-0.07*	(8.92%)
<i>Risk</i>	0.14	0.10	-0.04	(25.69%)	0.13	0.10	-0.03	(38.78%)
<i>Regis</i>	0.17	0.20	0.04	(29.41%)	0.17	0.20	0.03	(29.52%)
<i>Other</i>	0.83	0.84	0.01	(86.96%)	0.82	0.84	0.02	(61.60%)
<i>(Firm controls omitted for brevity)</i>								
<i>Panel B: Matching on all controls</i>								
	Means before weighting (N = 1,635)				Means after weighting (N = 1,635)			
	<i>SEC</i> = 0	<i>SEC</i> = 1	Diff.	(p-value)	<i>SEC</i> = 0	<i>SEC</i> = 1	Diff.	(p-value)
<i>Lawyer and law firm characteristics</i>								
<i>LawyerExp</i>	0.94	1.82	0.88***	(0.00%)	1.92	1.82	-0.10	(37.42%)
<i>LawFirmExp</i>	9.06	8.90	-0.16	(72.79%)	7.69	8.90	1.21	(18.36%)
<i>NumLawyers</i>	1.17	1.24	0.07*	(7.27%)	1.23	1.24	0.01	(73.83%)
<i>TopSchool</i>	0.60	0.39	-0.21***	(0.00%)	0.39	0.39	0.01	(94.89%)
<i>NumDegrees</i>	2.20	2.23	0.03	(36.74%)	2.22	2.23	0.01	(63.76%)
<i>LawyerAge</i>	3.30	3.37	0.07***	(0.24%)	3.39	3.37	-0.01	(61.47%)
<i>Female</i>	0.17	0.24	0.07**	(3.53%)	0.22	0.24	0.02	(34.83%)
<i>Initial comment letter controls</i>								
<i>NumFilings</i>	1.94	2.05	0.11	(49.25%)	2.06	2.05	0.00	(79.25%)
<i>Acc</i>	0.80	0.85	0.05	(10.46%)	0.86	0.85	-0.01	(95.76%)
<i>RegSX</i>	0.18	0.21	0.03	(40.75%)	0.22	0.21	-0.01	(86.11%)
<i>RegSK</i>	0.58	0.58	0.00	(69.84%)	0.56	0.58	0.02	(51.09%)
<i>Risk</i>	0.11	0.10	-0.01	(69.83%)	0.10	0.10	0.00	(95.66%)
<i>Regis</i>	0.17	0.20	0.03	(42.41%)	0.19	0.20	0.00	(73.69%)
<i>Other</i>	0.84	0.82	-0.01	(46.97%)	0.81	0.82	0.01	(70.36%)
<i>(Firm controls omitted for brevity)</i>								

This table documents the impact of our matching procedure on the control variables. We report the treatment and control means and the difference in means after omitting observations not in the support of the propensity score. Columns 2 to 5 are based on the means before applying the weights constructed by the procedures; columns 6 to 9 are based on the weighted means. In Panel A, we match only on lawyer and law firm characteristics, and in Panel B, we match on all the control variables. The matching is carried out within groups of years as described in Section 6.3, and the p-values are based on t-statistics adjusted for year group fixed effects.

scaled to proportions, so a bar shows the probability of the variable taking a given value. The figures in red are based on conversations with SEC-affiliated lawyers ( $SEC = 1$ ), and those plotted in blue are based on conversations without SEC-affiliated lawyers. In each figure, Panel A is based on all conversations in the sample, and Panel B is based on all conversations remaining after matching on all controls, without applying the weights.<sup>27</sup>

Figure 3 shows that the density plots for conversation length are shifted to the right for conversations with SEC-affiliated lawyers, relative to conversations without SEC-affiliated lawyers. In other words, the probability that a conversation takes a longer time to resolve is greater for conversations with SEC-affiliated lawyers. For example, Panel B suggests that when *ConvTime* is between about three and four, corresponding to conversations lasting between 19 and 54 days, the probability density is greater for conversations without SEC-affiliated lawyers. In contrast, when the length of the conversations is greater than this range, the probability density is almost always greater for conversations with SEC-affiliated lawyers. The confidence intervals, plotted as dashed lines, show that the average *ConvTime* is significantly different between the subsamples at the 10 percent level.

Likewise, Figure 4 shows that the distribution of the number of comment letters in the conversation are shifted to the right for conversations with SEC-affiliated lawyers, relative to conversations without SEC-affiliated lawyers. For both the full and matched samples, the probability that a conversation comprises four or less comment letters is greater for conversations without SEC-affiliated lawyers. In particular, about 30 percent of conversations without SEC-affiliated lawyers comprise three comment letters (generally corresponding to conversations with one round of letters exchanged), while the proportion is respectively 22 and 23 percent for conversations with SEC-affiliated lawyers, in each of the samples.

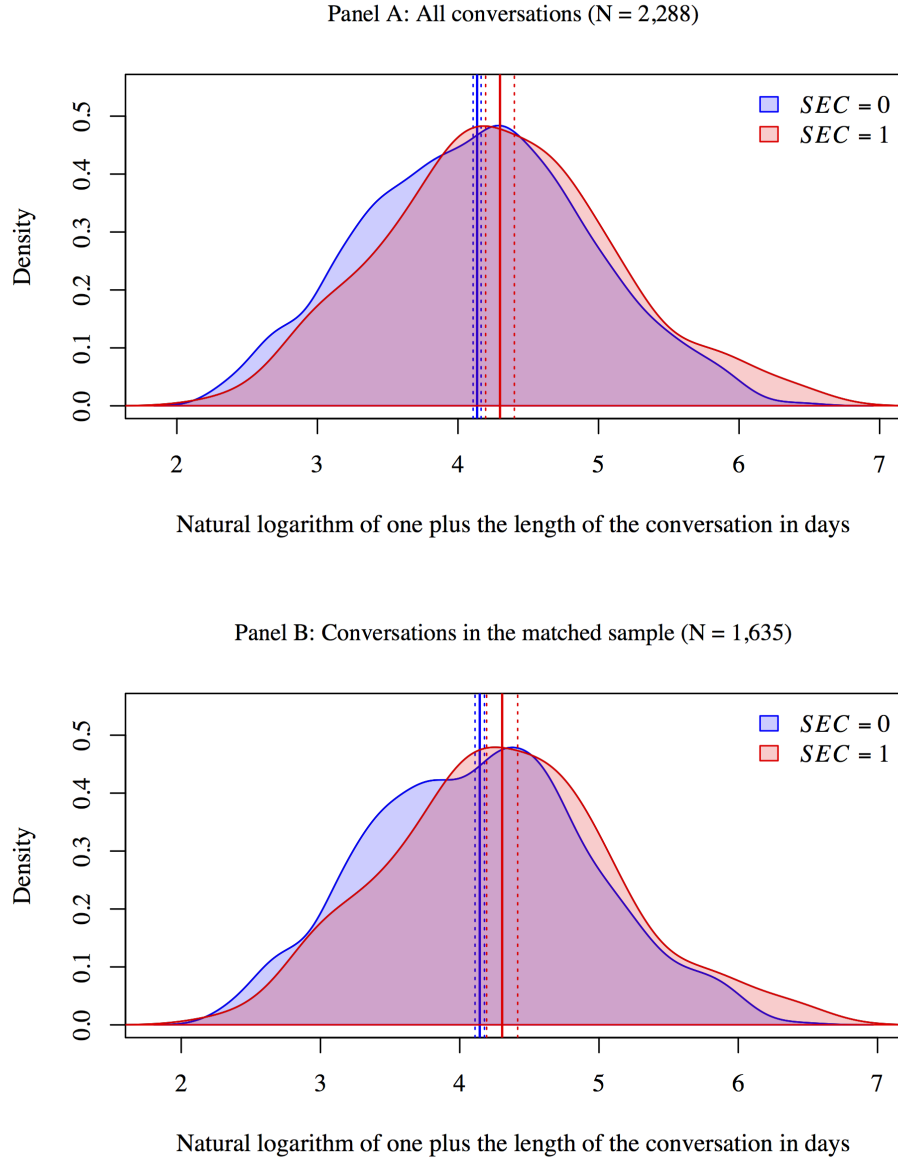
Table 9 presents the results from estimating Equation 4 under several different specifications. In Panels A and B we respectively apply the two matching methods described in Section 3.2. After matching, the sample sizes are 1,529 and 1,635 respectively, which are lower than the full sample of 2,288 conversations because as part of the matching procedure we omit observations outside the support of the propensity score.

The results from Panels A and B suggest that across both matching procedures and across all four proxies for negotiation, involving an SEC-affiliated lawyer increases negotiation. From Panel B, which reports the results after matching on all controls, when firms involve a lawyer formerly with the SEC, the length of comment letter conversations in days is about 17 percent higher, the number of letters in the conversation is about 6.8 percent higher, and

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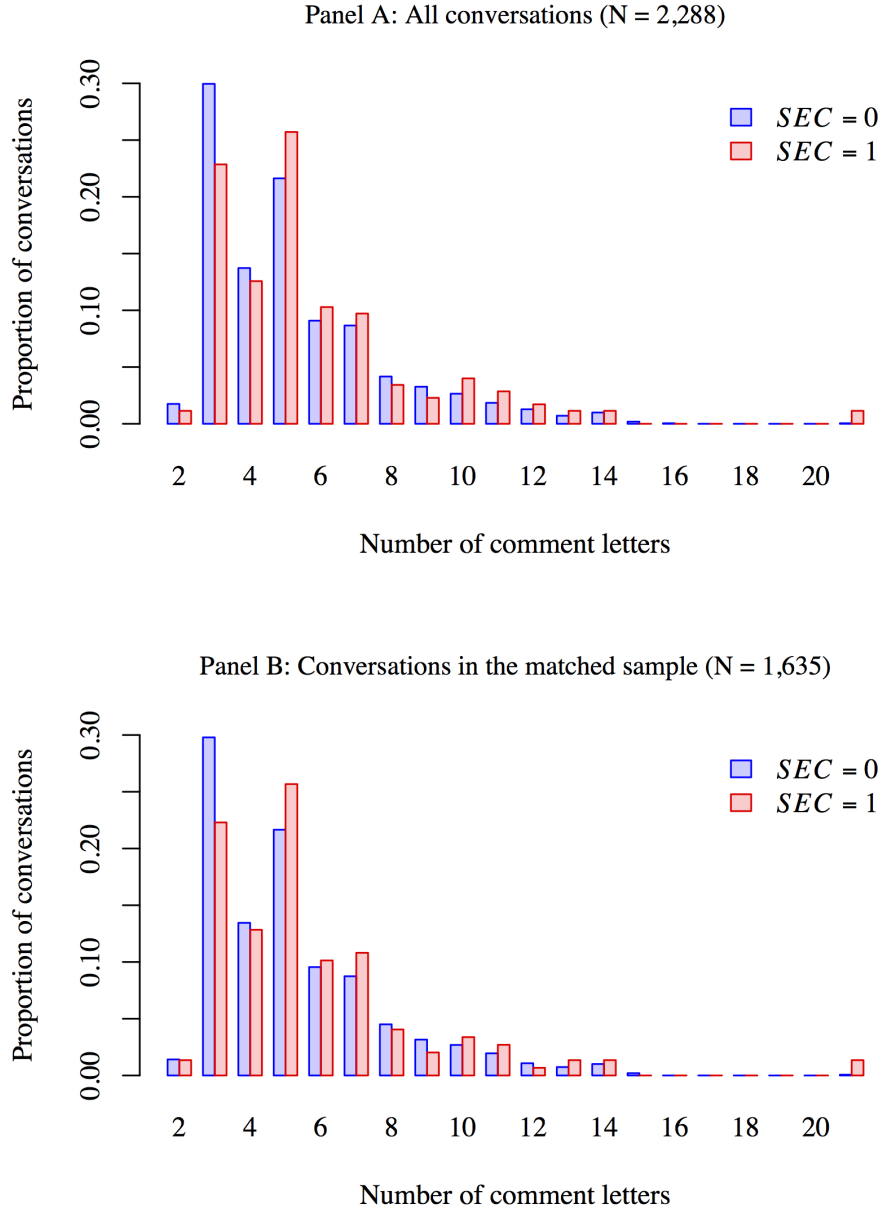
<sup>27</sup>See Section 3.2 for details on our matching procedure

Figure 3: Distribution of  $ConvTime$  by SEC affiliation



This figure shows the distribution of  $ConvTime$ , the natural logarithm of one plus the length of the conversation in days, for conversations with SEC-affiliated lawyers ( $SEC = 1$ , in red) and conversations without ( $SEC = 0$ , in blue). In Panel A the distributions are based on the full sample of conversations (N = 2,288), and in Panel B they are based on the conversations remaining in the sample after matching on all controls and omitting observations outside the support of the propensity score (N = 1,635). The densities are plotted using a Gaussian kernel. We indicate the means and 10 percent confidence bounds of each distribution with solid and dotted lines respectively.

Figure 4: Distribution of *NumLetters* by SEC affiliation



This figure shows the distribution of *NumLetters*, the number of comment letters in each conversation, for conversations with SEC-affiliated lawyers ( $SEC = 1$ , in red) and conversations without ( $SEC = 0$ , in blue). In Panel A the distributions are based on the full sample of conversations ( $N = 2,288$ ), and in Panel B they are based on the conversations remaining in the sample after matching on all controls and omitting observations outside the support of the propensity score ( $N = 1,635$ ).



the odds that the conversation has more than one round is about 63 percent higher.<sup>28</sup>

Panel C reports the regressions with law firm fixed effects included as controls. The impact of SEC affiliation remains statistically significant with the inclusion of law firm fixed effects. The coefficients reported in Panel C are larger economically than that in Panels A and B. This may be due to differences in the sample and specification; for example, the tests presented in Panel C are restricted to comment letter conversations involving larger law firms with at least ten conversations in the sample (see Section 3.2 for more information on our research design).<sup>29</sup> Panel C suggests that after controlling for law firm fixed effects, the length of comment letter conversations in days is about 34 percent higher, the number of letters in the conversation is about 17 percent higher, and the odds that the conversation has more than one round of letters is about 66 percent higher, for firms that retain former SEC employees, relative to firms that retain external counsel who were not employed by the SEC.

### 6.5. Amendments

We next examine the impact of representation by SEC-affiliated lawyers on the number and incidence of amendments filed by the firm following the commencement of the conversations (*NumAmend* and *Amend*).

Figure 5 plots the proportion of comment letter conversations that result in specific numbers of amendments (i.e. the distribution of *NumAmend*). The histograms in red are based on the treatment sample of conversations involving SEC-affiliated lawyers ( $SEC = 1$ ), while the histograms in blue are based on the treatment sample of conversations involving only lawyers who are not SEC-affiliated ( $SEC = 0$ ). Panel A is based on the full sample before matching, and Panel B is based on the sample after matching on all controls but without weighting.<sup>30</sup> The figures show that the distribution of the number of amendments is shifted to the left for treatment observations relative to control observations. In particular, in Panel B, 18.7 percent of conversations without SEC-affiliated lawyers result in amendments, while only 13.5 percent of conversations with SEC-affiliated lawyers result in amendments.

Table 10 reports the results from estimating Equation 5 under several different specifications. In Panels A and B we respectively apply the two matching methods described in Section 3.2. After matching, the sample sizes are 1,513 and 1,546 respectively, which are

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<sup>28</sup>These are computed by taking the exponentials of the coefficients reported in Columns (1), (3), and (4) in Panel B. For example, from Column 1;  $e^{0.159} = 1.17$ .

<sup>29</sup>Other than sample and specification differences, it could also be due to variation between law firms having an opposite effect to variation within law firms.

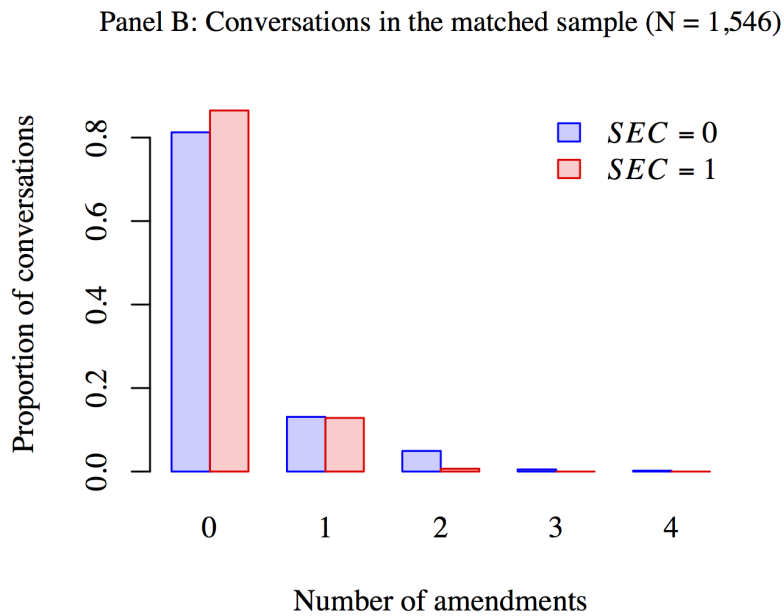
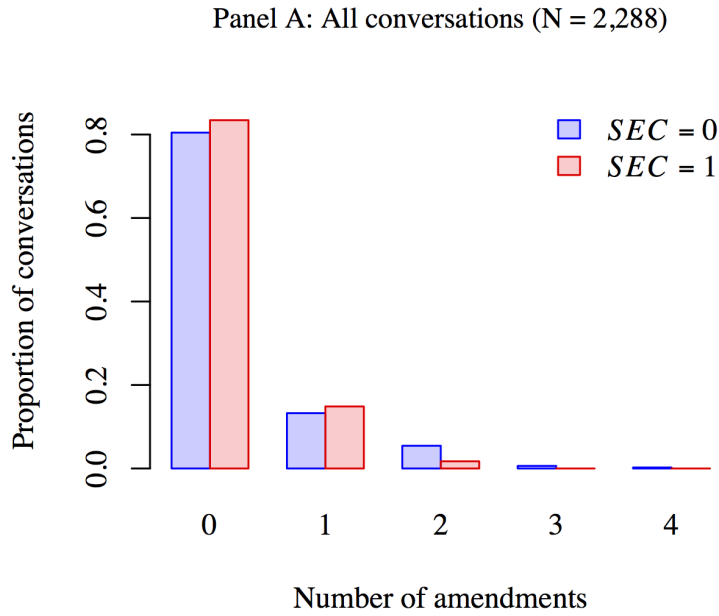
<sup>30</sup>See Section 3.2 for details on our matching procedure

Table 9: Representation by an SEC-affiliated lawyer and the extent of negotiation

<i>Panel A: Matching by lawyer and law firm characteristics</i>				
Dep. Var.	<i>ConvTime</i>	<i>NumLetters</i>	<i>NumLetters</i>	<i>MultiRound</i>
Regression	OLS	OLS	Poisson	Logit
	(1)	(2)	(3)	(4)
<i>SEC</i>	0.178** (0.070)	0.520** (0.207)	0.093** (0.038)	0.500** (0.209)
Controls	Yes	Yes	Yes	Yes
Year & Ind. FEs	Yes	Yes	Yes	Yes
Law firm FEs	No	No	No	No
Observations	1,529	1,529	1,529	1,529
Adj. / McF. R <sup>2</sup>	0.147	0.149	0.194	0.126
<i>Panel B: Matching by all controls</i>				
Dep. Var.	<i>ConvTime</i>	<i>NumLetters</i>	<i>NumLetters</i>	<i>MultiRound</i>
Regression	OLS	OLS	Poisson	Logit
	(1)	(2)	(3)	(4)
<i>SEC</i>	0.159** (0.066)	0.334* (0.200)	0.066* (0.037)	0.489** (0.205)
Controls	Yes	Yes	Yes	Yes
Year & Ind. FEs	Yes	Yes	Yes	Yes
Law firm FEs	No	No	No	No
Observations	1,635	1,635	1,635	1,635
Adj. / McF. R <sup>2</sup>	0.248	0.285	0.327	0.190
<i>Panel C: Including law firm fixed effects</i>				
Dep. Var.	<i>ConvTime</i>	<i>NumLetters</i>	<i>NumLetters</i>	<i>MultiRound</i>
Regression	OLS	OLS	Poisson	Logit
	(1)	(2)	(3)	(4)
<i>SEC</i>	0.295*** (0.096)	0.832*** (0.311)	0.153*** (0.054)	0.504* (0.281)
Controls	Yes	Yes	Yes	Yes
Year & Ind. FEs	Yes	Yes	Yes	Yes
Law firm FEs	Yes	Yes	Yes	Yes
Observations	1,167	1,167	1,167	1,167
Adj. / McF. R <sup>2</sup>	0.099	0.094	0.188	0.113

This table documents the results from estimating Equation 4 under several different specifications. In Panels A and B we apply two different matching methods, and in Panel C we require law firms to be involved in at least ten conversations, and replace *LawFirmExp* with law firm fixed effects. Please see Section 3 for details on our research design and matching, and Appendix A for variable definitions. The control variables comprise the lawyer and law firm characteristics, initial comment letter controls, and firm controls. Year fixed effects are based on the calendar year in which the conversation began, and industry fixed effects are based on one-digit SIC codes. We report adjusted R<sup>2</sup>s for the OLS models, and McFadden R<sup>2</sup>s for the poisson and logit models.

Figure 5: Distribution of  $NumAmend$  by SEC affiliation



This figure shows the distribution of  $NumAmend$ , the number of 10-K or 10-Q amendments filed between the start of each comment letter conversation and 90 days after the end of the conversation, for conversations with SEC-affiliated lawyers ( $SEC = 1$ , in red) and conversations without ( $SEC = 0$ , in blue). In Panel A the distributions are based on the full sample of conversations ( $N = 2,288$ ), and in Panel B they are based on the conversations remaining in the sample after matching on all controls and  $NumLetters$ , and omitting observations outside the support of the propensity score ( $N = 1,546$ ).

lower than the full sample of 2,288 conversations because as part of the matching procedure we omit observations outside the support of the propensity score.

The results reported in Panels A and B of Table 10 suggest that the number and occurrence of amendments resulting from comment letters are lower when a lawyer involved in the conversation was formerly employed by the SEC. For example, based on Panel B, which reports results after matching on all controls, comment letter conversations involving SEC-affiliated lawyers result in about 52 percent fewer amendments relative to conversations involving only non-SEC-affiliated lawyers, and the odds of filing an amendment are about 52 percent lower.

As in Table 9, the coefficient estimates are larger in magnitude after controlling for law firm fixed effects (Panel C), which could be due either to variation between law firms having the opposite effect to variation within law firms, or differences in the sample and specification. The results reported in Panel C suggest that after controlling for law firm fixed effects, comment letter conversations involving SEC-affiliated lawyers results in about 62 percent fewer amendments, and about a 61 percent decline in the odds of an amendment, relative to only involving non-SEC-affiliated lawyers.

### 6.6. *Future outcomes*

Finally, we examine the impact of representation by SEC-affiliated lawyers on future outcomes, specifically new comment letter conversations, adverse restatements, and litigation within the two years following the start of the focal conversation (*FutureConv*, *FutureRes*, and *FutureLit* respectively).

As in Tables 9 and 10, Table 11 reports the results from estimating Equation 6 under several different specifications. In Panels A and B we respectively apply the two matching methods described in Section 3.2. After matching, the sample sizes are 1,434 and 1,544 respectively, which are lower than the full sample of 2,288 conversations because as part of the matching procedure we omit observations outside the support of the propensity score. In Panel C, we include law firm fixed effects as before.

Across all model specifications—two matching methods, and with law firm fixed effects—we find no statistically significant evidence that SEC affiliation is associated with new comment letter conversations, adverse restatements, or class action litigation in the two years following the start of the focal conversation.

Table 10: Representation by an SEC-affiliated lawyer and amendments

<i>Panel A: Matching by lawyer and law firm characteristics</i>			
Dep. Var.	<i>NumAmend</i>	<i>NumAmend</i>	<i>Amendment</i>
Regression	OLS	Poisson	Logit
	(1)	(2)	(3)
<i>SEC</i>	-0.104** (0.048)	-0.579** (0.231)	-0.446 (0.304)
Controls	Yes	Yes	Yes
Year & Ind. FEs	Yes	Yes	Yes
Law firm FEs	No	No	No
Observations	1,513	1,513	1,513
Adj. / McF. R <sup>2</sup>	0.190	0.266	0.175
<i>Panel B: Matching by all controls</i>			
Dep. Var.	<i>NumAmend</i>	<i>NumAmend</i>	<i>Amendment</i>
Regression	OLS	Poisson	Logit
	(1)	(2)	(3)
<i>SEC</i>	-0.134*** (0.041)	-0.729*** (0.233)	-0.725** (0.320)
Controls	Yes	Yes	Yes
Year & Ind. FEs	Yes	Yes	Yes
Law firm FEs	No	No	No
Observations	1,546	1,546	1,546
Adj. / McF. R <sup>2</sup>	0.363	0.405	0.331
<i>Panel C: Controlling for law firm fixed effects</i>			
Dep. Var.	<i>NumAmend</i>	<i>NumAmend</i>	<i>Amendment</i>
Regression	OLS	Poisson	Logit
	(1)	(2)	(3)
<i>SEC</i>	-0.169** (0.069)	-0.968*** (0.345)	-0.941** (0.453)
Controls	Yes	Yes	Yes
Year & Ind. FEs	Yes	Yes	Yes
Law firm FEs	Yes	Yes	Yes
Observations	1,079	1,079	1,079
Adj. / McF. R <sup>2</sup>	0.158	0.323	0.230

This table documents the results from estimating Equation 5 under several different specifications. In Panels A and B we apply two different matching methods, and in Panel C we require law firms to be involved in at least ten conversations, and replace *LawFirmExp* with law firm fixed effects. Please see Section 3 for details on our research design and matching, and Appendix A for variable definitions. The control variables comprise the lawyer and law firm characteristics, initial comment letter controls, firm controls, and the number of comment letters in the conversation (*NumLetters*). Year fixed effects are based on the calendar year in which the conversation began, and industry fixed effects are based on one-digit SIC codes. We report adjusted R<sup>2</sup>s for the OLS models, and McFadden R<sup>2</sup>s for the poisson and logit models.

Table 11: Representation by an SEC-affiliated lawyer and future outcomes

<i>Panel A: Matching by lawyer and law firm characteristics</i>			
Dep. Var.	<i>FutureConv</i>	<i>FutureRes</i>	<i>FutureLit</i>
Regression	Logit	Logit	Logit
	(1)	(2)	(3)
<i>SEC</i>	-0.278 (0.218)	0.098 (0.435)	0.310 (0.583)
Controls	Yes	Yes	Yes
Year & Ind. FEs	Yes	Yes	Yes
Law firm FEs	No	No	No
Observations	1,434	1,434	1,434
McFadden R <sup>2</sup>	0.181	0.228	0.381
<i>Panel B: Matching by all controls</i>			
Dep. Var.	<i>FutureConv</i>	<i>FutureRes</i>	<i>FutureLit</i>
Regression	Logit	Logit	Logit
	(1)	(2)	(3)
<i>SEC</i>	-0.093 (0.227)	-0.159 (0.394)	0.673 (0.564)
Controls	Yes	Yes	Yes
Year & Ind. FEs	Yes	Yes	Yes
Law firm FEs	No	No	No
Observations	1,544	1,544	1,544
McFadden R <sup>2</sup>	0.226	0.281	0.426
<i>Panel C: Controlling for law firm fixed effects</i>			
Dep. Var.	<i>FutureConv</i>	<i>FutureRes</i>	<i>FutureLit</i>
Regression	Logit	Logit	Logit
	(1)	(2)	(3)
<i>SEC</i>	-0.133 (0.303)	-0.235 (0.455)	0.778 (0.650)
Controls	Yes	Yes	Yes
Year & Ind. FEs	Yes	Yes	Yes
Law firm FEs	Yes	Yes	Yes
Observations	1,069	1,069	1,069
McFadden R <sup>2</sup>	0.149	0.218	0.294

This table documents the results from estimating Equation 6 under several different specifications. In Panels A and B we apply two different matching methods, and in Panel C we require law firms to be involved in at least ten conversations, and replace *LawFirmExp* with law firm fixed effects. Please see Section 3 for details on our research design and matching, and Appendix A for variable definitions. The control variables comprise the lawyer and law firm characteristics, initial comment letter controls, firm controls, the number of comment letters in the conversation (*NumLetters*), and a dummy variable equal to one if there was an amendment (*Amendment*). Year fixed effects are based on the calendar year in which the conversation began, and industry fixed effects are based on one-digit SIC codes. We report adjusted R<sup>2</sup>s for the OLS models, and McFadden R<sup>2</sup>s for the poisson and logit models.

## 7. Conclusions

The flow of personnel and expertise from regulatory agencies to the private sector has been the subject of debate and scrutiny by a wide range of stakeholders from advocacy groups to academics. In particular, the revolving door spins rapidly at the SEC: between 2001 and 2012, 455 former SEC employees disclosed that they intended to represent an external party before the SEC, and in our sample of comment letter conversations in which external counsel were involved, 7.6 percent involved external counsel formerly employed by the SEC.<sup>31</sup> At the heart of the concern about the revolving door is the possibility of regulatory capture—the risk that regulators act “in the interests of those they regulate” (Brown, 2016, p. 1).

Regulatory capture and the revolving door have been studied by the legal and political science literatures since the 1970s. However, recent empirical evidence on the impact of the revolving door on financial reporting is sparse, and has focused on its impact on enforcement outcomes (deHaan et al. 2015; see also Shive & Forster 2017). Likewise, the literature on the impact of external counsel on financial reporting is sparse likely due to data availability limitations, but recent studies suggest that external counsel may have an important impact on the financial reporting system in specific settings (e.g. deHaan et al. 2015, Bozanic et al. 2016, and Dechow & Tan 2017).

Our study aims to contribute to these two growing streams of research by examining regulatory capture in the SEC comment letter process via lawyers who were formerly employed by the SEC. We identify the individual lawyers and law firms involved in SEC comment letter conversations using Audit Analytics, and then construct a hand-collected dataset of the background characteristics of the individual lawyers, including whether they were formerly employed by the SEC (“SEC-affiliated lawyers”). We examine three sets of outcomes following the receipt of a comment letter: the extent of negotiation, 10-K and 10-Q amendments, and future comment letter conversations, restatements, and litigation within the following two years.

In our first set of tests, we examine whether variation between individual lawyers is statistically significant in explaining variation in outcomes after a firm receives a comment letter from the SEC. Across specifications, we find that variation between individual lawyers is statistically incremental to control variables and to law firm fixed effects in explaining the extent of negotiation and amendment filings, and that results are mixed for future outcomes. In our second set of tests, we examine whether comment letter outcomes vary with

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<sup>31</sup>Former SEC employees’ post-employment disclosure information is obtained from POGO’s SEC Revolving Door database (see [Project On Government Oversight](#), n.d.).

whether an SEC-affiliated lawyer was involved, after matching on other control variables. We find that involvement of an SEC-affiliated lawyer is associated with more negotiation and fewer amendments, consistent with SEC-affiliated lawyers exerting their influence to reduce compliance with SEC comments. We find no statistical evidence that retaining an SEC-affiliated lawyer is associated with new comment letters conversations, restatements, or litigation in the two-year window following the start of the focal conversation, which could indicate that the influence of SEC-affiliated lawyers is persistent enough that the increased resistance is not corrected within two years.



## Appendix A. Variable definitions

Table [Appendix A.1](#) lists definitions for the comment letter outcome variables and the variables related to lawyer and law firm characteristics. Because our unit of analysis is the conversation, in the case of conversations in which the firm referenced more than one lawyer or law firm, the lawyer and law firm characteristics are aggregated as explained in the table.

Table Appendix A.1: Outcome variables and lawyer characteristics

Variable	Definitions
<i>Outcome variables</i>	
<i>ConvTime</i>	Natural logarithm of one plus the number of days between the first and last comment letters in a conversation.
<i>NumLetters</i>	Number of comment letters in a conversation.
<i>MultiRound</i>	One if $NumLetters \geq 5$ and zero otherwise.
<i>NumAmend</i>	Number of 10-K or 10-Q amendments filed between the date of the first comment letter in a conversations and 90 days after the last comment letter.
<i>Amendment</i>	One if $NumAmend > 0$ and zero otherwise.
<i>FutureConv</i>	One if a new comment letter conversation was initiated within the two years after the start of the focal conversation, and zero otherwise.
<i>FutureRes</i>	One if an adverse restatement was filed within the two years after the start of the focal conversation, and zero otherwise.
<i>FutureLit</i>	One if a securities class action lawsuit was filed within the two years after the start of the focal conversation, and zero otherwise.
<i>Lawyer and law firm characteristics</i>	
<i>SEC</i>	One if the firm referenced a lawyer formerly employed by the SEC in a comment letter in the conversation.
<i>LawyerExp</i>	Number of conversations in which the lawyer was involved over the year prior to the start of the focal conversation. <sup>†</sup>
<i>LawFirmExp</i>	Number of conversations in which the law firm was involved over the year prior to the start of the focal conversation. <sup>†</sup>
<i>NumLawyers</i>	Number of lawyers referenced during a conversation.
<i>TopSchool</i>	One if the firm referenced a lawyer who attended a university with a top-14 law school.
<i>NumDegrees</i>	The number of degrees earned by the lawyer, truncated on the left at two. <sup>†</sup>
<i>LawyerAge</i>	Natural logarithm of one plus the number of years between the start of a conversation and the year of the lawyer’s first degree. <sup>†</sup>
<i>Female</i>	One if the firm referenced a female lawyer in a comment letter.

This table lists the definitions for our outcome variables and variables related to lawyer or law firm characteristics. See Table [Appendix A.2](#) for definitions of our comment letter and firm controls.

<sup>†</sup> We use the maximum values of *LawyerExp*, *LawFirmExp*, *NumDegrees*, and *LawyerAge* for conversations in which the firm referenced more than one lawyer or law firm.

Table [Appendix A.2](#) lists the definitions for comment letter and firm controls. The comment letter controls are based on the first comment letter in a conversation, and unless otherwise stated, the firm controls are based on the most recent fiscal year before the first comment letter in a conversation.

Table Appendix A.2: Comment letter and firm control variables

Variable	Definitions
<i>Initial comment letter controls</i>	
<i>NumFilings</i>	The number of filings referenced in the initial comment letter.
<i>Acc</i>	One if the comment letter mentions accounting rule issues, and zero otherwise.
<i>RegSX</i>	One if the comment letter mentions Regulation S-X issues, and zero otherwise.
<i>RegSK</i>	One if the comment letter mentions Regulation S-K issues, and zero otherwise.
<i>Risk</i>	One if the comment letter mentions risk factor issues, and zero otherwise.
<i>Regis</i>	One if the comment letter mentions registration issues, and zero otherwise.
<i>Other</i>	One if the comment letter mentions issues coded by Audit Analytics as not fitting other categories, and zero otherwise.
Year FEs	Based on the calendar year in which a conversation began.
<i>Firm controls</i>	
<i>Size</i>	Natural logarithm of the firm’s market capitalization (Compustat: $prcc\_f \times csho$ ).
<i>FirmAge</i>	Natural logarithm of one plus the public age of the firm, defined as the number of days between the first comment letter in a conversation and the firm’s first appearance on CRSP, divided by the average number of days in a year.
<i>BTM</i>	Common equity ( <i>ceq</i> ) divided by the market value of equity ( $prcc\_f \times csho$ ).
<i>ROA</i>	Income before extraordinary items ( <i>ib</i> ) scaled by average assets (average <i>at</i> ).
<i>RD</i>	R&D expense ( <i>rd</i> ) scaled by average assets (average <i>at</i> ).
<i>Delaware</i>	One if the firm is incorporated in Delaware ( <i>incorp</i> = “DE”), and zero otherwise.
<i>Domestic</i>	One if the firm is domestic ( <i>loc</i> = “USA”), and zero otherwise.
<i>BigFour</i>	One if the firm is audited by a Big 4 accounting firm ( <i>au</i> is non-missing and strictly less than 9) and zero otherwise.
<i>BHR</i>	The firm’s raw buy-and-hold return over the 12 months before the first comment letter in a conversation.
<i>StdRet</i>	The standard deviation of the firm’s raw monthly return over the 12 months before the first comment letter in a conversation.
<i>PastRes</i>	One if the firm recorded an adverse restatement within the three years up to the start of a conversation.
<i>PastLit</i>	One if a securities class action lawsuit was filed within the three years up to the start of a conversation.
Industry FEs	Based on one-digit SIC codes.

This table lists the definitions for our comment letter and firm control variables. See Table [Appendix A.1](#) for definitions of comment letter outcome variables and variables related to lawyer or law firm characteristics.

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