# How Do Accounting Practices Spread?
# An Examination of Law Firm Networks and Stock Option Backdating*

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**Abstract**

We hypothesize that one way that accounting practices spread is through law firm connections. We investigate this prediction by examining companies that avoided reporting compensation expense by engaging in stock option backdating. We hypothesize that executives engaged in backdating because they were desensitized to its inappropriateness when they learned through their legal counsel that other companies were engaging in this practice. We identify backdating companies through backdating-related restatements of earnings. Using network analysis, we document that backdating companies are more highly connected with other backdating companies via shared law firms. Logistic regressions indicate that the odds of a company backdating are 53 to 88 percent higher when its law firm has another client that backdates. We find that sharing a law firm is incremental to the impact of board interlocks and geographic location for explaining backdating. Finally, we document that law firms that have more clients that restate earnings due to backdating also have more other clients that are "lucky" (grant options at low prices). This is consistent with the practice of backdating spreading to other client companies but only some companies being forced to restate. Our evidence is consistent with law firms acting as "system supporters" in enabling executives to engage in backdating.

**Keywords**: accounting practices, stock options, backdating, law firms, directors, geographic location, network analysis

**JEL Classifications**: J33, K22, K42, L14, M41, M43, M45

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## Abstract

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# 1. INTRODUCTION

Accounting practices evolve through time as the nature of business transactions change. Much of the details of accounting practices are not written down in formal rules by rule-making bodies such as the FASB. Instead, companies make detailed choices for themselves. Research evidence suggests that when individuals make decisions they use their own judgment but this judgment is influenced by the opinion of experts and the consensus opinion of peers.[1] Within the institutional framework of accounting this suggests that when corporate executives make accounting choices they are influenced by their own judgment, the opinion of experts such as their auditor and legal counsel, and the choices made by peer companies.

The objective of our paper is to provide insight into how accounting practices spread. There are many examples of what appear to be "bad" accounting practices growing in popularity until regulators put an end to the practice. These include the structuring of M&A deals to meet "pooling of interest" requirements; the structuring of deals to write-off in-process R&D; the structuring of securitizations to keep special purpose entities off the books; and the structuring of leases to meet operating lease requirements. How exactly do these practices gain momentum and acceptability and is there a way that such acceptability could be reversed without regulation? Better understanding the answer to these questions could help auditors and rule-making bodies reverse bad trends in accounting choices before the necessity of rule changes, regulation, or punishment.

[1] Shiller (2015, Chapter 10) discusses how several early experiments are consistent with herding behavior. For example, Milgram (1974) had subjects administer electric shocks to a confederate who acted in pain. The subjects continued to give shocks when the experimenters (who they believed were experts) told them no permanent tissue damage would occur. This was interpreted as the power of authority. An information-based interpretation of this finding is that the subjects assumed, that the experts knew what they were doing. Milgram found weaker results when subjects thought the experimenters were not experts. Asch (1952) did experiments where the subject had to guess the length of line segments and a group of confederates unanimously gave the wrong answer. He found that a third of the time the subjects gave the wrong answer, and suggested this was due to social pressure. However, Deutsch and Gerard (1955) suggest that another interpretation is that the subjects were reacting to the knowledge that a large group of people had reached a judgment different from theirs and, based on prior experience, assumed that the group was almost certainly right.

The spread of any particular accounting practice is likely to be contextual and in this paper, we focus on the role of law firms in the stock option backdating scandal. We believe this setting has several advantages for conducting our research. First, stock option backdating occurred for several years without public knowledge. This suggests that the practice spread through inter-company networks, and so increases the reliability of the results we find when analyzing the relation between law firm networks and backdating. Second, backdating companies were required to restate their earnings in years when the backdating occurred. This allows us to both identify companies that have backdated and identify the years in which the backdating occurred. It also allows us to determine the timing of the backdating relative to other companies in the law firm network. Finally, we are able to identify the law firm that provided legal counsel to the company through Form S-8 filings. Form S-8 registers securities that can be used in employee-based compensation plans, and includes an opinion of counsel on the legality on the securities issued (see SEC 2015, p. 9). Therefore, we can identify the law firm that counseled the company on its stock compensation plans and the years in which it did so.

We contend that there was ambiguity in the accounting rules (APB 25) surrounding the exact meaning of the term "measurement date" for stock option grants. This ambiguity allowed flexibility in the interpretation of the grant date and likely led to compensation committees or executives relying on the advice of experts (auditors, attorneys, or compensation consultants) or peers (other directors or executives) in determining the grant date.[2] Our study focusses on the role of the external counsel and we hypothesize that because the external counsel plays an active role in setting up employee stock plans, they could also have played a role in spreading the practice of backdating through informal discussion with executives and directors concerning the measurement date choices made by other clients. As a consequence,

[2] Milliron and Weil (2017) provide a discussion of the ambiguity of the accounting rules relating to stock options. The ambiguity is also discussed by the *The Wall Street Journal* (see Ng 2005) on reinterpretation of guidance in FAS 123 that suggests that the grant date is not necessarily when the compensation committee approves the awards but when the employee is informed of the awards. Perlis and Johnson (2007, p. 9) also suggest that experts such as auditors, compensation consultants and the legal counsel were likely to have been consulted by boards about the grant date. More recently, Mohliver (2018) argues that backdating could have been viewed as liminal (legally questionable but not clearly illegal) until after control agents noticed and scrutinized the practice.

executives could have engaged in backdating because they were desensitized to its inappropriateness after learning that other companies were engaging in this practice. In other words, the executive's judgment was affected by the fact that, on average, experts tend to be correct, and when a large group of people come to a judgment it is likely to be correct. Of course, self-interest plays a role, but self-interest is not the only influential factor in decision making.

We identify a company as a backdating company if it records a backdating-related restatement. We examine whether backdating companies are unusually highly clustered to one another via law firm links (to avoid confusion we use the word "firm" in reference to a law firm and "company" for a business entity). To do so, we construct networks each year where nodes represent companies and a link from one company to another represents a shared law firm over which backdating could have propagated by the end of the year. We examine the extent to which companies are connected to one another via law firm links, for backdating companies compared to the same number of randomly-chosen companies, based on 10,000 simulations per year. We find that in almost every combination of year, clustering measure, and matching methodology tested, backdating companies are more highly clustered to one another than are 90 to 99 percent of randomly-chosen companies.

Next, we provide regression analyses to determine whether a company is more likely to backdate options in a given year when it is represented by a law firm that had represented a backdating company. Using a sample of 13,912 company-years between 1997 and 2006 in which stock options were granted, we define a *LawFirmLink* variable that reflects whether a company is represented by a law firm that currently or previously represented another client company in a year that it backdated options. We use several variations of the *LawFirmLink* variable based on the period the linked company's backdating occurred, and several specifications with and without matching. As a starting point, the unconditional probability of a backdating restatement is quite low (about 3.4 percent of company-years). However, the probability that a company backdated when it is linked to another backdating company via shared law firms is 6.07 percent, more than three times more likely than a company that is not linked to a backdater (1.93 percent). Further, our logistic regressions, which control for many potentially-confounding factors, reveal that the odds of a company backdating are 53 to 88 percent higher when the company is linked to another backdating company via shared law firms than when it is not, *ceteris paribus*.

We also examine the distribution of backdating companies across law firms, and show that they are concentrated in certain law firms. Among the 23 larger law firms (those with over 40 option-granting clients) the proportion of clients that backdate range from zero to about fourteen percent and the difference in proportions is significant ($p < 5\%$). In contrast, for the eight larger audit firms (with over 40 option-granting clients), the difference is not significant ($p = 36.3\%$), and the proportions range from 1.4 to 3.7 percent. This evidence suggests that the practice of backdating was clustered among clients of certain law firms and that certain law firms could have been aware of and potentially spread the practice. In contrast, it appears that none of the larger audit firms were heavily involved, since we find no statistical evidence of clustering of backdating clients in specific audit firms.

We provide two additional tests that provide further insight into the role of law firms in backdating. Our first test examines the direction of causality. We predict that companies learn about backdating from their law firms, but an alternative explanation is that companies that wish to engage in backdating switch to law firms that have backdating clients. We analyze companies that change law firms, but find no support for the "switching" story. Our second test examines whether law firms with backdating clients are more likely to have *other* client companies that could have backdated. Prior literature argues that "lucky" grant dates are likely to be correlated with backdating (Lie 2005). We define a "lucky" CEO grant date as one that occurs on a day with the lowest or second lowest closing price during a 21 day window surrounding the grant date (ten days before and ten days after the grant date). We document that a company that did not record a backdating-related restatement is more likely to have a "lucky" grant when their law firm has other clients that backdated. For example, among large law firms, a non-restating company has a 19.9

percent chance of being "lucky" when their law firm has a high proportion of other clients that backdated versus 15.3 percent otherwise ($\chi^2$ test p-value < 0.01). This suggests that law firms played a role in spreading the practice of backdating, but that not all of their backdating clients restated earnings. Overall our results suggest that lawyers at certain law firms could have been aware of the backdating practice but took no action to stop its spread, or could have inadvertently played a role in spreading the practice through informal discussion with clients about the option granting practices of other client firms.[3]

Our results contribute to the literature in three ways. First, our paper contributes to the growing literature that suggests that lawyers influence financial reporting quality and other corporate practices. For example, Kwak et al. (2012) suggest that earnings guidance is different when the top management team includes a lawyer. They find that these companies provide more bad news forecasts, have less optimistic guidance, and have larger stock market reactions to their forecasts. Hopkins, Maydew, and Venkatachalam (2014) suggest that earnings quality is different for companies with a highly compensated in-house legal counsel. They find that these companies are able to engage in more aggressive accounting that remains in compliance with GAAP. Bozanic, Choudhary, and Merkley (2018) examine whether companies hiring an external law firm have different outcomes from SEC comment letter inquiries. They find that when lawyers are involved, the outcomes are more likely to include confidential treatment requests and less likely to result in amendments to periodic filings. Our results suggest that external law firms could play influential roles in corporate governance practices (i.e., equity-based incentives) that have consequences for the quality of earnings (i.e., restatements due to backdating).

Second, our paper builds on research that examines whether stock option backdating spread via social networks. Armstrong and Larcker (2009) and Bizjak, Lemmon, and Whitby (2009) identify companies with "lucky" grant dates and assume that these companies engaged in backdating. The analysis in both

[3] Westaby (2012, p. 5 and p. 33) in his discussion of network theory, describes *observers* as playing a peripheral role - they are entities that observe (or are aware of) the people involved in goal pursuit; *system supporters* are entities that support others in goal pursuit and improve the likelihood of goal success.

papers suggest that board members spread the practice of backdating. Bizjak et al. (2009) and Sivadasan (2010) further suggest that sharing geographic locations increases the likelihood of backdating. We build on these papers by suggesting that external law firms are professional experts that were directly involved in structuring employee compensation plans. Thus law firms were in a unique and influential position for spreading the backdating practice. Our evidence suggests that law firm connections are incremental to director links and geographic location in explaining stock option backdating.

Finally, our paper builds on research that examines whether accounting quality is influenced by social networks. Chiu, Teoh, and Tian (2013) provide evidence that a company is more likely to restate earnings when it shares a director with another company that restates earnings. Their results suggest that board of directors' networks influence accounting quality. Our paper focuses on stock option backdating and suggests that in the case of backdating related restatements, law firm connections are incremental to director networks in influencing accounting quality.

## 2. PRIOR LITERATURE ON STOCK OPTION BACKDATING

The literature on the practice of stock option backdating began with a line of research that uncovered evidence that stock option awards were timed favorably, that is they tended to occur on days with low stock prices relative to the days before or after the grant date. Yermack (1997) finds that companies have cumulative abnormal returns of over two percent over the 50 trading days following CEO stock option grants. He finds weaker evidence of negative cumulative abnormal returns in the 20 days before the award date. Aboody and Kasznik (2000) find evidence that companies with fixed stock options award schedules "time" the option award date by delaying good news and rushing forward bad news. Lie (2005) finds that the effect is greater for unscheduled awards, and proposed that "at least some of the awards are timed retroactively" (p. 802). Heron and Lie (2007) and Narayanan and Seyhun (2008) provide further evidence suggesting that backdating contributed to favorable option grant timing.

Following Lie (2005), an investigation by the *Wall Street Journal* that broke in March 2006 brought the practice of option backdating to the attention of the public, and 24 class action lawsuits relating to options backdating were filed in 2006 alone (Cornerstone Research 2008).[4] By 2007 the SEC was investigating "well over 100 backdating cases" (SEC 2007) and the *Wall Street Journal*'s Options Scorecard had over 140 companies listed as having "come under scrutiny for past stock-option grants and practices" (WSJ 2007).

The companies accused of stock option backdating faced significant negative consequences. The first announcement of a backdating allegation is associated with negative abnormal returns of approximately seven percent over the trading days leading up to and around the announcement (Bernile and Jarrell 2009; see also Carow, Heron, Lie, and Neal 2009). In addition, CEOs and CFOs are more than three times more likely to face forced dismissals in backdating companies than matching control companies (Efendi, Files, Ouyang, and Swanson 2013, p. 86).[5] Similarly, Ertimur, Ferri, and Maber (2012) find that significant reputation penalties, proxied by votes withheld and turnover, accrue to directors of backdating companies after the announcement of a backdating investigation. Although, they find no evidence that the damaged reputational capital follows the directors to directorships at other non-backdating companies.

There is a clear alternative accounting practice to backdating, which is simply to recognize an expense for the difference between the current market price and the exercise price. Why did so many companies choose to select the suboptimal accounting choice of backdating? Researchers have suggested that tax-related incentives can increase the likelihood of backdating (Dhaliwal, Erickson, and Heitzman 2009); as does poor corporate governance (e.g., Collins, Gong, and Li 2009 and Bebchuk, Grinstein, and Peyer 2010) and the potential trade-off between cash compensation and options (Veld and Wu 2009).

[4] See the *Wall Street Journal*, "The Perfect Payday" (Forelle and Bandler 2006) and "How the Journal Analyzed Stock-Option Grants" (Forelle 2006). The *Wall Street Journal* was awarded the 2007 Pulitzer Prize in Public Service for its investigation into stock option backdating.

[5] In addition, Edelson and Whisenant (2009) develop a measure of undisclosed backdaters and suggest that these companies also suffered negative consequences. See also Maremont 2009 and the *New York Times*, "Behind the Fade-Out of Options Backdating Cases" (Henning 2010) and "End of the Options Backdating Era" (Henning 2013).

Furthermore, Ertirmur et al. (2012) argue that backdating is driven more by compensation-related motives than financial reporting motives because the reputational penalties are more severe for compensation committee members than audit committee members. The financial reporting consequences of backdating could therefore be a side-effect of executives trying to increase their compensation without revealing this to their boards. Ertirmur et al.'s (2012) finding that the directors did not suffer reputational penalties at other non-backdating companies suggest that the directors may have been unaware of the practice, and were not expected to have spread the practice to other companies.

Although tax-related, compensation-related, and financial reporting-related motives are likely to play a role in the decision to backdate, they do not explain how backdating spread. Our hypothesis is that backdating executives were desensitized to their poor judgment when their legal counsel did not articulate the importance of defining the grant date when setting up the stock compensation plans and more generally may have informally conveyed practices adopted by other client companies. In other words, executives "herded" to backdating because they relied too heavily on the opinion of experts and the knowledge that peer companies were engaging in the practice.

Consultations with the external legal counsel is not the only way that backdating is likely to have spread. Prior research suggests that backdating could also have spread through shared directorships or through communication between executives in the same geographic location (e.g., Bizjak, Lemmon, and Witby 2009 and Sivadasan 2010). Bizjak, Lemmon, and Witby (2009) argue that director links between companies allow the *knowledge* of the practice of backdating to spread between companies. They classify an option grant as backdated if the difference between post-grant and pre-grant stock returns exceeds a cutoff level based on a random sample of trading days. They find using logistic regressions that the odds of starting to backdate option grants is significantly positively associated with having a board member who is on the board of a backdating company. Similarly, Armstrong and Larcker (2009) argue that "backdating may be the result of social influence" (p. 51) in the sense that backdating by a linked company helps to legitimize the practice in the focal company. Using a sample of 140 companies identified as backdating by the *Wall Street Journal*, Armstrong and Larcker (2009) find that backdating companies are more connected to one another via board interlocks compared to a simulated distribution of the degree of connectedness in randomly-drawn samples of non-backdating companies. They suggest that "boards of directors may be an important part of the social mechanism related to the diffusion and justification of backdating behavior" (p. 54).[6] We view this line of research as complementary to ours, and investigate whether law firm networks are incrementally informative in explaining backdating over board interlocks and geographic location for our sample of company-years.

## 3. HYPOTHESIS DEVELOPMENT

When a company registers securities to be issued under employee benefit plans, Regulation S-K requires an "opinion of counsel as to the legality of the securities being registered" for original issuance securities (SEC 2015, p. 9). The opinion is typically included in the registration as Exhibit 5 or 5.1, and states that the securities will be validly issued, fully paid, and non-assessable.[7] The close involvement of legal counsel in stock option plans raises the question: could law firms have played a role in the practice of stock option backdating? For example, in a recent trial of a CEO convicted for his role in option backdating, the CEO's attorney alleged that the external counsel "signed off on the company's backdating of stock options", although the judge "expressed no interest in passing off blame to [the company's] outside counsel" (Koppel 2010).

Prior research has not directly examined the impact of law firms on the propagation of the practice of backdating. Bizjak et al. (2009, 4843) discuss the possibility that law firms or compensation consultants could be another way that backdating spread but argue:

[6] Two studies have examined the role of board interlocks in the spread of aggressive tax reporting. Brown (2011) finds that the adoption of the corporate-owned life insurance shelter spreads via board interlocks, and Brown and Drake (2014) find that companies with board interlocks with companies with relatively low effective tax rates have lower effective tax rates themselves.

[7] See American Bar Association (2004) for a summary of the legal opinions in SEC filings.

"The fact that no outside counsels or compensation consultants have been targeted for action suggests that they might not have played a prominent role in the spread of this practice across companies."

However, the SEC focuses enforcement actions against officers of the company since these are the individuals responsible for the reporting.[8] In addition, a lack of litigation does not necessarily imply that law firms were not involved. For example, there may be hurdles to litigation against external counsel. Perlis and Johnson (2007, p. 9) point out that relationships with third parties such as lawyers and auditors may be very valuable to companies, and that director and officer insurance policies often entitle the insurers (rather than the company itself) to pursue claims against the third parties.

If the practice of backdating spread via law firms, then a company would be more likely to have backdated when it shares a law firm with another company that had backdated. It would also have left observable evidence in the structure of the shared law firms between companies. Specifically, backdating companies will have an unusually large amount of shared law firms. We refer to shared law firms between companies as law firm links. We provide two predictions:

***P1***: *Backdating companies are unusually highly clustered to one another via law firm links, relative to randomly-selected companies.*

***P2***: *A company is more likely to backdate stock options if it is represented by a law firm that currently or previously represented another company in a year during which it backdated options.*

We test *P1* using network analysis and *P2* using logistic regressions.

## 4. SAMPLE AND DATA

We identify backdating companies using AuditAnalytics' Non-Reliance Restatements database. We restrict the data to restatements involving option backdating (category 48 in AuditAnalytics), and define a company's backdating period as the period for which the company is restating (*res_begin_date* to

[8] We identified 27 firms that faced SEC Enforcement actions for backdating stock options in the Accounting and Auditing Enforcement Releases database (see Dechow, Ge, Larson, and Sloan 2011). We found that the majority of cases involved the CFO or CEO. Non-executive independent directors were sued in only three of the firms.

*res_end_date*). A company-year is defined as backdating if it overlaps with the company's backdating period. Figure 1 provides the distribution of the start and end years of the backdating periods for the 123 backdating companies in our sample. The median (average) company backdated for six (seven) years (untabulated). The frequency of backdating increased gradually from the mid-1990s, but most firms had stopped by 2006, around the time *Wall Street Journal* brought the practice to the attention of the public (see Forelle and Bandler 2006 and Forelle 2006).

We next construct a sample of company-years with at-the-money CEO grants. Table 1 Panel A describes our sample construction. Because EDGAR filing only became mandatory for all US public firms in 1996, we construct links between companies and law firms based on data beginning in 1996, and begin our sample in 1997 to allow for at least one prior year of data. Our sample period ends in 2006, around the time stock option backdating was brought to the attention of the public and the practice became less frequent. Our initial sample therefore comprises Compustat company-years between 1997 and 2006. We require availability of PERMNO and CIK, resulting in a sample of 71,117 company-years.

We next require the company to have issued stock options during the year. We identify option grants using the Thomson Reuters Insider Filings database.[9] We require the availability of either CUSIP or ticker to facilitate merging with other datasets. Following prior research (e.g. Heron and Lie, 2007; Bernile and Jarrell, 2009; and Bebchuk et al., 2010) we omit option grants that are not at-the-money. Most stock options are granted at the money, and the practice of backdating adjusts the timing of the grant date retroactively so that the options are at the money (Heron and Lie, 2007). Similar to Bebchuk et al. (2010), we require each grant's exercise price to be within one percent of the closing price of the grant date or the

[9] The Compustat variable *optgr* also captures stock option grants, but is only available from 2001. The Insider Filings database is based on SEC Forms 3, 4, 5, and 144. We use Table 2, which includes data on option grants and exercises. Consistent with prior research (e.g. Narayanan and Seyhun, 2008), we restrict the data to observations with cleanse indicators other than "A" or "S", which indicate the lowest-accuracy records. We require the derivatives to be coded as a type of stock option (derivative type DIREO, DIRO, EMPO, ISO, NONQ, CALL, or OPTNS), and for the transaction to be an acquisition rather than a disposition. We also remove records labeled as amendments, which "could be used to try to cover up for backdating after an investigation is started" (Edelson and Whisenant, 2009, p. 5).

trading day before the grant date.[10] This results in a sample of 34,715 company-years during which stock options were issued at-the-money. We further restrict the data to stock option grants to CEOs and remove scheduled grants under the assumption that backdating can only occur when the grant date is unscheduled.[11] This reduces the sample to 18,591 company-years. Finally, we require closing stock prices to be available for the grant date and at least ten days on either side of the grant date in order to construct luck-based measures of options backdating. This leaves us with 18,505 company-year observations for our 1997 to 2006 sample period.

We next link each company to the law firm that had represented it in its Form S-8 filings using Lexis Securities Mosaic's Law Firm Relationships database. Lexis obtains its data from SEC filings, which all US public companies were required to file electronically beginning in 1996. The database provides information on the filing date of each Form S-8 along with the name of the law firm associated with the filing, and company identifiers.[12] We first check the first date and last date a given law firm is associated with a given company. The first date is the first S-8 filing recorded in the database and the last date is the most recent Form S-8 filing. If a company's *last* Form S-8 is before the end of our sample period (e.g., 2004), then we assume that its relationship with the law firm named in the filing extends to the end of our sample period (i.e., 2006).[13] We are able to obtain law firm links for 13,721 company-years directly from Lexis. The remaining company-year observations were either not included in Lexis at all, or were cases where the company's first Form S-8 was filed after the relevant company-year. For these remaining company-years, we collect the law firm name by hand based on the most recent Form S-8 filing from

[10] Furthermore, if the exercise price is within one percent of the closing price on the eve of the grant date and not the grant date itself, we use the former as the grant date.

[11] As in prior studies (e.g. Aboody and Kasznik 2000; Lie 2005; Heron and Lie 2007; Heron and Lie 2009; Bebchuk et al. 2010), we use anniversary grants as proxies for unscheduled grants. Specifically, we define a scheduled CEO grant as a CEO grant within one day of the anniversary of a CEO grant in the previous year.

[12] In the case of Form S-8 filings, the law firm recorded by Lexis are usually the ones that provide the Exhibit 5 or 5.1 opinions. We restrict the data to form types coded by Lexis Securities Mosaic as S-8, S-8 POS or S-8/A, and we standardize the law firm names, for example by removing punctuation and suffixes such as "PC" and "LLP". We use the EDGAR index files to obtain the companies' CIK numbers because the data identifies filings by accession number.

[13] We rerun our results after omitting observations for which the time between S-8 filing and the year-end is greater than one year and three years respectively, and find that our inferences are unchanged.

EDGAR. We are able to hand-collect law firms directly from the SEC EDGAR database for 1,515 additional company-year observations. This results in us obtaining law firm links for 15,236 company years.[14]

For each company-year, we calculate the time elapsed since the most recent Form S-8 filing in our dataset. The results (untabulated) indicate that the average (median) elapsed time is 583 days (386 days) and the 10th and 90th percentiles are 78 days and 1,368 days respectively. In other words, for half of the observations, the time that elapsed between the S-8 filing and the company's fiscal year-end was about a year. At the company level, the average elapsed time between the last year the company appears in the sample and the company's last S-8 filing is 675 days (median = 455 days). We require availability of variables used in the regressions, which reduces the sample to 13,912 company-years. We run the regressions using three different specifications: without matching, and using two different propensity matching methods. Our primary inferences are unchanged across all three specifications. For brevity, we only report results based on matching the characteristics of backdating and non-backdating companies (Section 6.2 provides more details on our matching methodology). After applying the matching procedure, the sample is reduced to 10,312 company-years.

Panel B of Table 1 reconciles the initial 171 unique companies that restated earnings due to backdating as reported on AuditAnalytics, to the 141 unique backdating companies we identify that have at-the-money option grants. Panel C of Table 1 provides the sample examining the impact of board interlocks. We obtain board member data from the Institutional Shareholder Services (formerly RiskMetrics) database on WRDS. We use the Directors Legacy file that covers the period 1996 to 2006. The data comprises board membership information for the calendar years in which companies' annual meetings

[14] After applying this procedure. the majority (95.9%) of the 15,236 company-years are linked to only one law firm during the year. The remaining 4.1% of company-years may have multiple law firms per year either because they filed separate Forms S-8 with different law firms, or because of the small number of S-8 filings (3.8%) coded by Lexis Securities Mosaic as having multiple law firms. Our inferences are unchanged when we omit company-years linked to more than one law firm.

occurred.[15] We merge the data with our sample of company-years, assigning board members to company-year observations by CUSIP, and assuming that a director is on a company's board throughout the fiscal year during which the annual meeting occurred. Because the database covers directors in the S&P 1500, our sample size is reduced by about 66 percent for the board interlocks tests. A similar decline in sample size is also noted by Chiu, Teoh, and Tian (2013) who use the same data source. After constructing the director links and merging the data with the sample, we obtain a sample of 4,671 company-years for the board interlocks tests. The sample size is further reduced to 3,776 company-years after matching.

Panel D of Table 1 provides our sample for tests examining geographic links. We restrict the sample to companies headquartered (Compustat: *loc*) in the United States and require data on the companies' city and state (Compustat: *city* and *state*). This results in a sample of 13,707 company-years comprising 4,671 unique companies. After matching, the sample size is further reduced to 10,186 company-years. Figure 2 provides the proportions of the 4,671 companies (in light blue) and the 119 that backdated (in red) in the 15 states with the largest proportions of companies in the sample. California has the largest proportion of sample companies (20.8%), followed by Texas (8.9%). Approximately 49.6% of backdating companies are headquartered in California. We include a dummy variable, $California_{it}$ in our regressions to control for this clustering.

## 5. NETWORK ANALYSIS

### 5.1 Descriptive Evidence on Law Firms' Clients

Table 2 Panel A provides data on the relationship between backdating and law firm size, based on the sample of 5,159 companies with law firm data available. Here we define law firm size as the number of sample companies a law firm represented over our sample period. The panel indicates that there are a large

[15] We remove observations where the year is different from the year of the annual meeting (*meetingdate*), and observations where *legacy_director_id-cusip-meetingdate* is duplicated. We use the legacy director ID (*legacy_director_id*) as the director identifier instead of the current director ID (*director_detail_id*), in accordance with the WRDS KnowledgeBase's recommendation to use the most populated director ID for the sample period.

number of small law firms that represented five or fewer companies (812 of 1,080, or 75.2% of unique law firms) and a small number of large law firms that represented more than 40 companies (23 of 1,080, or 2.1% of unique law firms). Columns 3 and 4 provide the number and percentage of law firms that have backdating clients in each group. Of the 812 smallest law firms, 21 or 2.6% had clients that backdated. As the size of a law firm increases, the probability that it had a backdating client increases. Thus, of the 23 largest law firms, 20 or 87.0% had backdating clients. Columns 5 to 7 concern the probability that a given client backdated during the sample period. About 2.4% of unique companies backdated, and the proportion is highest (4.3%) for the clients of the largest law firms.[16]

For comparison, we provide information on the auditors of companies in our sample and the proportion of each audit firm's clients that backdated. Table 2 Panel B shows the number and proportion of unique clients that backdated during the sample period while represented by the eight audit firms with more than 40 clients in the sample and by other audit firms, respectively. We drop observations without auditor data from Compustat or that were unaudited. Among the Big 5 audit firms, the proportion of clients that backdated range from 1.4% (Arthur Andersen) to 3.3% (Deloitte & Touche).

If the practice of backdating spread via law firms, we would expect backdating companies to be more concentrated in certain law firms than others. Figure 3 Panel A shows the proportion of each large law firm's clients that backdated during the sample period. We compare this against large audit firms in Panel B. The proportion varies widely within large law firms, from 0% to 13.8%, and the difference in proportions is significant, with a p-value of 2.0%. On the other hand, the proportion is not significantly different between large audit firms at conventional significance levels (p-value = 36.3%). In Panels C and D, we restrict the analyses to companies in California, the state with the most backdaters in our sample. We find large variation in the proportion of law firms' clients that backdated (p-value = 10.5%), while the

[16] In Columns 5 and 6 of Panel A and Columns 3 and 4 of Panel B, the number of clients per bin do not sum to the total number of clients because a company may be linked to multiple law firms or multiple audit firms.

proportion is similar across audit firms (p-value = 96.4%). These results suggest that certain large law firms were more involved with the practice of backdating than others. In contrast, backdating clients appear randomly distributed among large audit firms.

### 5.2 Law Firm Network Results

Network analysis has been used in epidemiological studies to examine the role of social connections. Christakis and Fowler (2007), for example, find that obesity spreads via friendship and family links. Rosenquist, Murabito, Fowler, and Christakis (2010) find that alcohol consumption by friends and relatives is associated with a person's alcohol consumption. In this paper, instead of people, we focus on companies, and instead of the spread of disease or alcohol, we focus on the spread of option backdating via law firms.

We use two network algorithms to visually display the evidence: the Kamada and Kawai (1989) and Fruchterman and Reingold (1991) algorithms. Both algorithms position nodes based on their connections with other nodes: the former is based on the shortest paths between nodes, while the latter is based on modeling attractive forces between connected nodes and repulsive forces between unconnected nodes. The size of each node is based on company size, defined as the natural logarithm of beginning market value.

Figure 4 depicts the law firm links between companies that backdated at *any time* in our sample period. In Panel A, the network is drawn based on the Kamada and Kawai (1989) algorithm, and in Panel B, it is drawn based on the Fruchterman and Reingold (1991) algorithm. Each node represents one of the 123 unique backdating companies, and a link from node $i$ to node $j$ indicates that a law firm link from $i$ to $j$ existed at some point during the sample period. In other words, during one of $j$'s fiscal years $t$, it had at least one law firm that represented $i$ at $t$ or earlier. Figure 4 indicates that the network is highly clustered, with a large subcomponent comprising companies that are connected to one another, and several small subcomponents and unconnected nodes.[17] There are 69 companies (56.1%) in the largest subcomponent, and 6 (4.9%) in the second-largest subcomponent.

Figure 5 examines the propagation of backdating over time. We focus on the largest component of the network drawn using the Kamada-Kawai layout in Panel A of Figure 4, and report the networks every second year starting in 1997. For each network diagram, we restrict the nodes to companies that had entered the sample by the corresponding year, and color nodes red if the company had backdated by then and light blue otherwise. The law firm links between companies are constructed based on data up to each year, in contrast with Figure 4, which depicts links at any time in the sample period. This allows us to document the development of the network over time. We observe that from 2003, the only companies that have not backdated are outside the highly connected central cluster.

In the remainder of this section we provide a more formal examination of whether backdating companies are unusually clustered via the law firm links. Table 3A provides descriptive statistics on the networks each year between 1997 and 2001 and Table 3B between 2002 and 2006. Panel A of the tables indicates that there are substantially more links than companies, and that the number of companies and links peak in 2001. The large number of links relative to companies is due to law firms having multiple clients and companies using the services of more than one law firm. The mean number of law firms per link is close to one, suggesting that if one company is linked to another it is generally via only one law firm.

Panel B of Tables 3A and 3B present characteristics of subsets of the network each year. The initial increase in the relative size of the largest component of the network is likely because the law firm links are constructed based on data beginning in 1996. In later years, a company's law firm would be more

[17] A component of a network comprises a set of nodes that are connected to each other via one or more links. This includes nodes linked via other nodes: if node $i$ is linked to $j$ and $j$ is linked to $k$, nodes $i$ and $k$ are in the same component. Here and elsewhere in the paper, we ignore the direction of the links when deciding whether two nodes are in the same component.

likely to have represented another company in the sample at some point since 1996. Beginning in 2001, the largest components of the networks each year comprise more than 60% of the companies that year.

Panels C of Tables 3A and 3B present descriptive statistics on the in-degree distribution of backdating and non-backdating companies respectively, each year. The in-degree of a given node is the number of other nodes that link to the given node. For example, the mean in-degree for non-backdating companies of 14.1 in 2000 indicates that, on average, a non-backdating company's law firm has had 14.1 other clients in the data. The smaller median in-degree every year is consistent with there being several law firms with large client bases. The mean in-degree of backdating companies is significantly greater than that of non-backdating companies in all years, except the first. For example, in 2000 the mean in-degrees for non-backdating and backdating companies were 14.1 and 33.4 respectively. This suggests that backdating companies tend to be represented by larger law firms that have had more clients that issue options.

While Tables 3A and 3B provide evidence of differences in the in-degrees of backdating and non-backdating companies within the full network, Table 4 provides evidence of abnormal clustering between backdating companies. Beginning with the sample of 15,236 company-years with law firm data, each year we compute measures of clustering between backdating companies, and compare those measures to the simulated distribution of clustering between the same number of randomly-selected companies. We use two measures of the extent of clustering: the clustering coefficient, and the shortest path between nodes, as defined below.

**Mean clustering coefficient.** For a given node $i$, the clustering coefficient measures the extent to which the nodes that are connected to $i$ are connected to each other (Watts and Strogatz, 1998; Barrat et al., 2004).[18] For example, if all companies shared the same law firm at $t$, they would each have a clustering coefficient of 1. For this measure we ignore the directions of the links. Isolated nodes and nodes that are linked with only one other node are assigned clustering coefficients of

[18] More precisely, the clustering coefficient for a given node is defined as follows. (Watts and Strogatz 1998, p. 441) If a given node $i$ is linked from or to $k$ other nodes, the maximum number of undirected links that could exist between the $k$ nodes is $\frac{k(k-1)}{2}$. The clustering coefficient for node $i$ is then the proportion of the $\frac{k(k-1)}{2}$ links that exist.

zero. Following Watts and Strogatz (1998), we use the average clustering coefficient over all nodes in a network.

**Mean shortest path.** Analogous to Armstrong and Larcker (2009), we measure the minimum number of law firm links needed to reach one company from another. We consider only directed paths between nodes, and if two companies are not linked at all, the shortest path between them is defined as the longest possible path through the network plus one.[19] We use the average shortest path over all possible directed paths between unique companies in the network.[20]

We illustrate the computation of the mean clustering coefficient and mean shortest path using a simple network in Exhibit 1.

To test prediction *P1*, each year we compare the mean clustering coefficient and the mean shortest path of the network of backdating companies against that of 10,000 draws of same-sized networks of randomly selected companies. For a given year, each simulated network is constructed by drawing the same number of random companies as backdating companies, and the mean clustering coefficient and shortest path are computed for the network.

We illustrate the comparison between the network of backdating companies and *one* random network for several years in our sample period in Figure 6. Figure 6 presents diagrams of the networks for the three years with the largest numbers of backdating companies, alongside same-sized networks of companies chosen randomly from the sample in the same year. Nodes shaded in dark red correspond to companies that backdated and nodes in light blue with darker outlines indicate companies that did not backdate that year. We observe qualitatively from the layout of the nodes that backdating companies are highly clustered to one another via law firms, relative to randomly-selected non-backdating companies.

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[19] A directed path is a set of links between nodes that are in the same direction; for example, $i \rightarrow j \rightarrow k$ is a directed path from *i* to *k* but $i \rightarrow j \leftarrow k$ is not. We note that the links may not be transitive (i.e. $i \rightarrow j \rightarrow k$ does not necessarily imply $i \rightarrow k$) because law firm representation may change over time for a given company.

[20] Unlike Armstrong and Larcker (2009), we do not use the median shortest path because a large proportion of the pairs of companies are not linked at all, particularly in the randomly-selected samples: in our networks the median distance between two companies would often be infinite. This difference between our networks and the networks reported in Armstrong and Larcker (2009) suggest that companies are less clustered to one another via shared law firms than shared directors.

In addition, the networks of backdating companies have higher mean clustering coefficients (CC) and lower mean shortest paths (SP) in each of the years. The mean shortest paths of the networks of randomly-selected companies are close to the number of companies in the networks, consistent with a very low degree of connectivity between the companies.

Each year we draw 10,000 random networks to construct the empirical distributions of the mean clustering coefficient and shortest path. To mitigate the impact of differences between backdating and non-backdating companies, before carrying out the simulations we omit industries with little or no representation in the backdating sample, defined as two-digit SIC codes that comprise less than one percent of backdating observations. This precaution ensures that there is overlap between the industries of the backdating and the non-backdating samples. In additional tests, we find similar results when we omit this restriction, and when we instead control for differences in size and geography.[21]

Table 4 presents the results of the simulation. The shaded columns on the left present the mean clustering coefficient and shortest path for the networks of backdating companies each year, and the unshaded columns in the center present the distributions of both measures from 10,000 simulations of same-sized networks comprising randomly-selected companies each year. The shaded columns on the right document the empirical p-values each year. This is the percentage of simulated networks that are more clustered than the network of backdating companies. The results suggest that backdating companies are significantly more highly clustered to one another via law firm links relative to randomly-selected companies. Panel A indicates that across years, the mean clustering coefficient among backdating

[21] The mean clustering coefficient and shortest paths are computed in R using the `transitivity` and `mean_distance` functions from the igraph package (Csardi and Nepusz, 2006). For both measures, we assign each link the same weight. In additional tests untabulated for brevity, we run simulations with size and state matching respectively instead of industry matching. We match by size by omitting non-backdating companies with market capitalization beyond the first and third quartile of the market capitalization of backdating companies at the start of each year. We match by state by omitting company-years in states that have low representation in the backdating sample, defined as less than one percent of backdating observations. We also run the simulations without either restriction. Across every combination of year, matching method, and clustering measure, a total of 80 unique combinations, we find that the backdating network is more clustered than 90 percent to 99 percent of random networks, with only one exception (the clustering coefficient under state matching in 1997, the year with the fewest backdating observations in the sample).

companies is greater than that of 90 percent to 99 percent of the networks constructed from randomly-selected companies. Panel B indicates that every year the mean shortest path among backdating companies is less than that of at least 95 percent of the networks constructed from randomly-selected companies. This evidence is consistent with backdating companies being highly connected to each other.

Overall the results presented from our law firm network analysis are consistent with *P1* and suggest that companies that backdate appear to be unusually clustered to one another.

## 6. REGRESSION ANALYSIS

Our next set of tests use regression analysis to examine whether using a law firm that has a backdating client increases the probability that a company will backdate. We examine *P2* by estimating the following logistic model:

$$\text{logit}(Backdating_{it}) = \alpha + \beta\ LawFirmLink_{it} + \gamma\ Controls_{it} + \epsilon_{it} \qquad (1)$$

where $Backdating_{it}$ is a dummy variable equal to one if company *i* backdated in fiscal year *t* and zero otherwise; $LawFirmLink_{it}$ is a dummy variable equal to one if company *i* is represented by a law firm during *t* that had represented another company *j* during the period when company *j* was backdating and zero otherwise, and $Controls_{it}$ is a vector of control variables. We estimate the logistic model for company-years in which options were granted and data is available (see Table 1), and use several variations of $LawFirmLink_{it}$ as follows:

$LawFirmLink_{it}$: one if company *i* is represented by a law firm at *t* that also represented another company *j* during *t* or earlier in a year that it backdated, and zero otherwise;

$LawFirmLink_{it}^{t}$: one if company *i* is represented by a law firm at *t* that also represented another company *j* during t in a year that it backdated, and zero otherwise; and

$LawFirmLink_{it}^{t-1}$: one if company $i$ is represented by a law firm at $t$ that also represented another company $j$ <u>during *t-1*</u> in a year that it backdated, and zero otherwise.[22]

Exhibit 2 provides example timelines that illustrate the differences. Each panel depicts three fiscal years for companies $i$ and $j$. Company $j$ is a backdating company and company $i$ is linked to company $j$ via law firm Abc LLP in various fiscal years. In Panel A, company $i$'s law firm (Abc LLP), represented company $j$ throughout all three years. Here company $j$ only backdated in fiscal year *t-2,* so we have: $LawFirmLink = 1$, $LawFirmLink^t = 0$, and $LawFirmLink^{t-1} = 0$. In Panel B, company $i$'s law firm (Abc LLP), represented company $j$ only in its most recent fiscal year $t$, during which company $j$ also backdated. Because the link period overlaps with $t$ but not $t-1$, $LawFirmLink = 1$, $LawFirmLink^t = 1$, and $LawFirmLink^{t-1} = 0$. In Panel C, company $i$'s law firm (Abc LLP), represented company $j$ only in fiscal year *t-1*, during which company $j$ also backdated. Due to the firms having different fiscal year ends, the link period overlaps with both $t$ and $t-1$, so $LawFirmLink = LawFirmLink^t = LawFirmLink^{t-1} = 1$.

We include controls in the regression model based on prior literature on the determinants of options backdating. Collins et al. (2009) include controls for company size, high-technology companies, and auditor type, and Veld and Wu (2014) include a variable for dispensable cash, defined as cash minus interest expenses scaled by total assets, because an "alternative for option backdating is to pay cash while leaving the existing options intact" (p. 1051). Both Collins et al. (2009) and Veld and Wu (2014) included controls for stock volatility, since the potential gain from stock option backdating increases with the

[22] We note that the definition of a law firm link in the regression analyses differs slightly from the definition used in the network analyses. The regression analyses use comprehensive samples of backdaters and non-backdaters to examine the relationship between backdating and being linked to a backdater, so whether *LawFirmLink* takes the value of 0 or 1 depends on whether linked companies were backdaters. In contrast, our network analyses examine the degree of clustering within specific subsamples (e.g. within backdating companies or within randomly-chosen companies), so links are based on shared law firms with any other company within the subsamples. When constructing the *LawFirmLink* variables, we assume that a law firm represents *j* (the "other" company) every year between the first and last year the law firm was associated with *j* while it was a backdater. This is an important precaution that reduces errors due to sample attrition and data incompleteness. For example, if *j* backdated and was represented by a law firm between 2002 and 2004, but 2003 data is missing due to sample attrition or law firm data unavailability, other companies that had the same law firm as *j* at 2003 may have the variable $LawFirmLink_{it}^{t}$ erroneously coded as zero.

variation in stock prices.[23] We construct the control variables as follows: $Size_{i,t-1}$ is the natural logarithm of beginning market value (Compustat: *csho* × *prcc_f*); $HighTech_{it}$ equals 1 if the company's SIC code is between 7370 and 7379 inclusive; $Auditor_{it}$ equals 1 if the company's auditor is one of the Big 5 audit companies (Compustat: *au* between 1 and 8); $DispCash_{i,t-1}$ is cash and cash equivalents less interest expenses scaled by total assets (Compustat: (*che* − *xint*) / *at*), at the beginning of the year; and $PriceVol_{it}$ is the standard deviation of daily stock price during the fiscal year. We include a dummy variable $California_{it}$, equal to 1 if the company's state (Compustat: *state*) is California, due to the large proportion of backdating companies located in California. We also include year fixed effects to take into account changes in the frequency of backdating across the sample period.

The network analysis in Tables 3A and 3B indicate that backdating companies have more incoming law firm connections, suggesting that they have larger law firms. In addition, Panel A of Table 2 suggests that larger law firms have a greater proportion of backdating clients. This suggests that *LawFirmLink* and law firm size are correlated. We define the size of a company's law firm each year ($LawFirmSize_t$) as the natural logarithm of the number of unique companies the law firm represented between 1996 and the end of each year.[24] We replicate our estimation of Equation 1 using law firm size as the independent variable, and also estimate Equation 1 within subsamples partitioned by law firm size.

## 6.1 Descriptive Statistics

Table 5 presents summary statistics for key variables used in our analyses. The *Backdating* variable has a mean of 0.034, indicating that backdating occurs in 3.4% of company-years. The *LawFirmLink* variables indicate that between 28.5% and 35.2% of all observations are linked to a backdating company

[23] Collins et al. (2009) use the standard deviation of returns over 60 months while Veld and Wu (2014), whose level of analysis is the individual option grant, use the standard deviation of daily stock prices in the month of the option grant. We use daily returns over the entire fiscal year because our level of analysis is the company-year.
[24] If a company has more than one law firm during *t*, we sum of the numbers of unique clients across law firms.

via a law firm depending on the definition of the link.[25] The median log law firm size is 2.77, corresponding to the median company-year being associated with a law firm that has 16.0 clients. Approximately 89.2% of observations are audited by one of the Big 5 audit companies, 13.6% of observations are high-technology companies and 22.2% are headquartered in California. The median company size is 5.787, corresponding to a market value of about $327 million, and on average, companies have dispensable cash of 22.8% of total assets. The average price volatility over the fiscal year is 3.592, and the lower median of 2.269 suggests that the price volatility is positively skewed.

Panel B of Table 5 provides Pearson and Spearman correlations. $Backdating_{it}$ is significantly positively correlated with the *LawFirmLink* variables as expected, with Pearson correlations between 11 percent and 12 percent (t-statistics between 12.98 and 14.73), depending on the link definition. $Backdating_{it}$ is also significantly positively correlated with law firm size, company size and whether the company is headquartered in California, with respective Pearson correlations of 11 percent (t = 13.20), 11 percent (t = 13.20) and 13 percent (t = 15.73). It is also weakly positively correlated with the company having a Big 5 auditor, with a Pearson correlation of 4 percent (t = 4.64). As expected, the three *LawFirmLink* variables are highly correlated with each other, with correlations of at least 85 percent. In addition, the *LawFirmLink* variables are highly correlated with law firm size, with Pearson correlations between 61 percent and 64 percent.

Panel A of Table 6 provides a comparison of backdating companies (*Backdating* = 1) to non-backdating companies (*Backdating* = 0). Consistent with the correlations, the tests of difference in means indicate that backdating companies are significantly larger, more likely to be in high-technology industries, more likely to have a Big N auditor, have more disposable cash, have greater stock price volatility, and are more likely to be located in California. All differences are significant at the one percent

[25] Note that the *LawFirmLink* variables reflect the proportion of sample companies that are linked directly to a backdating company at a given time. In contrast, in our network analyses the size of the largest connected component (e.g. 63.9% in 2002 in Table 3B) refers to the largest proportion of sample companies that are linked to each other either directly or indirectly via other companies.

level. In columns 6 to 9 we propensity match the samples each year based on the control variables. We use a full matching procedure (Rosenbaum, 1991) that assigns weights to observations, and we drop observations that are outside the support of the propensity score.[26] After propensity score matching the differences for all control variables become statistically insignificant.

Panel A of Table 6 also reports the three variations of *LawFirmLink* before and after matching on the control variables (we do not match on *LawFirmLink*). The average backdating company has a 63 percent chance of being linked to another backdating company (*LawFirmLink* = 0.63). In contrast, around 34 percent of non-backdating firms are linked to a backdating company via their law firms (*LawFirmLink* = 0.34). This suggests that a company is almost twice as likely to be linked via a law firm to another backdating company if it is a backdater. Columns 6 to 9 provides comparisons after matching on the control variables. The differences in *LawFirmLink* remain statistically significant, consistent with law firm links being important for explaining backdating after controlling for other company characteristics. We explore this in more detail in Table 7.

Panel B of Table 6 takes a different perspective. Here we compare companies that are linked to a backdating company via their law firm (*LawFirmLink* = 1) to those that are not linked via a law firm (*LawFirmLink* = 0). Note that approximately 63 percent of backdaters and 34 percent of non-backdaters have *LawFirmLink* = 1. Thus, in this comparison, these companies are pooled together and we analyze the characteristics of both backdating and non-backdating companies that are linked to a backdaters via their law firms. The differences in means for the control variables are all highly significant and in the same direction as in Columns 2 to 5 of Panel A, suggesting that companies that share a law firm with a backdater are different from companies that do not, but that they have similar characteristics to backdaters. For companies that share a law firm with a backdating company, the probability of being a backdater is

[26] Following our matching procedure for the network simulations (Table 4), when matching backdating and non-backdating observations, we do not drop backdating observations. Nevertheless, when we drop both backdating and non-backdating observations that are outside the support of the propensity score, our sample remains almost identical (decreasing by only three company-years), and our inferences are unchanged.

6.07 percent. This probability is over three times that of the probability of being a backdater for companies that do not share a law firm with a backdating company (1.93 percent). After propensity score matching on control variables, the difference in likelihood of being a backdating company (i.e., *Backdating* =1) remains significant.

### 6.2 Regression Analysis of Law Firm Links

Table 7 presents the results from estimating Equation 1 for the three variations of the *LawFirmLink* variable. Regressions (1) to (4) present the results without matching, and regressions (5) to (7) present the results after matching backdating and non-backdating observations each year as described in Panel A of Table 6. In all regressions, the *LawFirmLink* variables are significantly positively related to the odds of backdating. At the bottom of Table 7 we compute the economic significance of the result in terms of both odds ratios and probabilities.

We compute the odds ratios taking the exponentials of the corresponding coefficients. For regression (1) with no control variables, the odds ratio is 3.287, suggesting that with no controls, the odds of backdating is about three times as high when a company is linked to another backdating company via its law firm, than when a company is not.[27] For the regressions (2) to (4) with control variables included, the odds ratios range between 1.568 to 1.88, and after matching in regressions (5) to (7) the odds ratios range from 1.531 to 1.876. This suggests that after controlling for firm characteristics, the odds of backdating are between 53.1 percent and 88.1 percent higher if a company is linked to a backdating company via a

[27] The odds ratio from regression (1) can also be directly compared to the proportions reported in Panel B of Table 6. The probability of being a backdating company given that the company is linked by its law firm to another backdating company is 6.072 percent (Table 6 Panel B). This means that the probability of a linked company *not* backdating is (1 - 0.06072) or 93.928% and so the odds of backdating when *LawFirmLink* equals one is equal to 0.06072 / 0.93928 = 0.06465. In contrast, the odds of backdating when *LawFirmLink* equals zero is 0.01929 / (1 - 0.01929) = 0.01967. The odds ratio reported in Table 7 is calculated as 0.06465 / 0.01967 = 3.287 (i.e., the odds of backdating is about three times as high when a company is linked to another backdating company via its law firm than when a company is not linked to a backdating company). To put this odds ratio in context, note that the unconditional probability of being a backdating company in the population is small – just 3.4% but being linked via a law firm to a backdating company almost doubles this probability to 6.07%.

law firm than if it is not, *ceteris paribus*.[28] Next, we compute the probability of backdating conditional on whether or not a company is linked to a backdating company via a law firm, for the median company. Specifically, we substitute the estimated intercept and coefficients at Table 7, and the median values for each of the control variables, into the regression models, and set *LawFirmLink* to be either zero or one. This allows us to compute the estimated probability of backdating when *LawFirmLink* is either zero or one, for a company with the median values of the control variables. In regression (6), for example, the median company has a 3.75 percent probability of backdating if it is not linked, but a 6.50 percent probability of backdating if it is linked, a 73.15 percent increase in the probability of backdating. Across the regressions with matching (columns 5 to 7), the probability that the median company backdates is 50.1 to 81.8 percent higher if it is linked to a backdating company via a law firm. In remaining tables we provide results using propensity score matching based on backdating company characteristics.

Table 8 Panel A examines the sensitivity of our findings to law firm size. Regression (1) shows that companies with larger law firms are significantly more likely to backdate, but regression (2) shows that the impact of law firm size is subsumed by $LawFirmLink_t$. In regression (3), we omit company-years that were linked to the law firm with the greatest number of backdating clients. We provide this regression to ensure that the results are generalizable and not due to one law firm. The coefficient on $LawFirmLink_t$ is 0.296 is statistically significant and of a similar magnitude to 0.349 reported for the full model in regression (2). Regressions (4) and (5) partition the sample by median $LawFirmSize_t$ each year. The coefficients on *LawFirmLink* are statistically significant in both regressions, suggesting that being linked to a backdating company via a law firm connection, whether the law firm is small or large, increases the probability of backdating. In Panel B of Table 8, we replicate our main regressions using variations of our *LawFirmLink* variables. *LFLinkDiffInd* is defined in the same way as *LawFirmLink*, except that they take the value of one only if the focal company is linked via a law firm to a backdating company with a different

[28] The pseudo R-squares are lower in the regressions with matching because after matching backdating and non-backdating companies on the control variables, the control variables no longer contribute explanatory power to the model.

two-digit SIC code. All variations of *LFLinkDiffInd* are positively related to backdating, suggesting that our findings are not driven by spreading of backdating along shared industries.

### 6.3 The Influence of Peers: Director Links and Geographical Links

We next investigate two possible ways that a company could learn about peer backdating activities beyond learning it from their legal counsel. The first is via a director who sits on a backdating company's board and the second is by communicating with other executives that live in the same city. To examine the effect of board interlocks we augment Equation 1 as follows:

$$\text{logit}(Backdating_{it}) = \alpha + \beta_1 LawFirmFLink_{it} + \beta_2 DirLink_{it} + \gamma\ Controls_{it} + \epsilon_{it} \tag{2}$$

Where $DirLink_{it}$ is equal to one if at least one of company *i*'s directors at *t* was on the board of company *j* during <u>*t* or earlier</u> in a year that it backdated, and zero otherwise.

Table 9 Panel A provides descriptive statistics for the sample with director information before and after matching on backdating company characteristics. Panel A indicates that 20 percent of backdating companies are linked to another backdating company via a director. In contrast, 12 percent of non-backdating companies are linked to backdating companies via board interlocks. This difference is statistically significant (p = 0.04%). However, after matching on control variables, *DirLink* is no longer significant (p = 66.7%). Panel B of Table 9 provides our logistic regressions with propensity score matching between backdating and non-backdating companies. Regression (1) suggests that director links are not associated with backdating after matching. Regressions (2) to (4) show that law firm links remain statistically and economically significant in explaining backdating when director links are included in the models. In regression (2), for example, $LawFirmLink_t$ is associated with an odds ratio of 1.365 while $DirLink_t$ is associated with an odds ratio of 1.110, and only $LawFirmLink_t$ is statistically significant at conventional significance levels.

The results in Table 9 contrast with Bizjak et al. (2009) who find that board interlocks are significant even when they include various controls. Bizjak et al. develop an *ex ante* measure of the likelihood of backdating based on the grant dates occurring when the stock price is at a local low. In contrast, we have an *ex post* measure of backdating based on restatements. The advantage of their approach is a larger sample size. The disadvantage is the possibility that some of the companies did not backdate but were either lucky or timed news releases. In contrast, our sample has a high degree of certainty that backdating occurred, but we sacrifice power. That is, some of the control firms could have had directors who spread the backdating but the firm did not restate earnings and so we did not identify these firms.

Another way that a company could learn about backdating is from interactions with other executives at peer companies that backdate. We do not have a direct measure of executive interaction but we expect that executives located in the same city are more likely to interact with each other than executives located further apart. We estimate the following regression:

$$\text{logit}(Backdating_{it}) = \alpha + \beta_1 LawFirmLink_{it} + \beta_2 GeoLink_{it} + \gamma\ Controls_{it} + \epsilon_{it} \quad (3)$$

where $GeoLink_{it}$ is equal to one if the city in which company *i* is headquartered at *t* is the headquarters of company *j* during <u>*t* or earlier</u> in a year that it backdated, and zero otherwise.

We report descriptive statistics before and after matching and the regression results in Table 10. Panel A shows that before matching, backdating companies are about twice as likely as non-backdating companies to be linked to a backdating company via geographic links. After matching, the *GeoLink* and *LawFirmLink* variables remains significant. Panel B provides the results from estimating Equation 3. Regression (1) indicates that a company has about 1.5 times the odds of backdating when it is in a city that headquartered another backdating company. Regressions (2) to (4) show that when both law firm links and geographic links are included in the models, both are statistically significant in explaining backdating.[29] Regression (5) adds director links to regression (4), but as with our findings in Table 9, director links are not significant, whereas the *GeoLink* and *LawFirmLink* variables remain significant.

### 6.4 Do Companies Switch to Law Firms that Allow Backdating?

It is possible that rather than law firms influencing companies, the direction of causality runs in the opposite direction. For example, an executive could learn about backdating from another executive at a backdating company and then hire that company's law firm to advise on the practice. If companies hire law firms to facilitate the practice of backdating, this would give rise to two testable predictions. First, backdating would be positively associated with switching law firms. Second, the association between backdating and switching law firms would be greater for companies with tainted law firms (because switching would have little or no effect on backdating if it resulted in a company not having tainted law firms).

We define a variable $LFChange_{it}$ as equal to one if at least one of company *i*'s law firms at *t* did not represent *i* in a prior year, and we restrict the sample to company-years where data on the company's law firms in a prior year is available. In other words, $LFChange_{it}$ indicates the presence of a new law firm at year *t*. About 13.4% of company-years in this sample had $LFChange_{it}$ equal to one. Positive associations between $Backdating_{it}$ and $LFChange_{it}$, and between $Backdating_{it}$ and *LawFirmLink* × *LFChange*, would be consistent with companies switching to tainted law firms to facilitate the practice of backdating. We provide the regression results at Table 11. When both *LFChange* and *LawFirmLink* × *LFChange* are included in Equation 1, we find that *LFChange* and *LawFirmLink* × *LFChange* are statistically

[29] We perform several untabulated robustness checks. First, we add a new dummy variable for companies in Massachusetts in addition to the *California* dummy, because Massachusetts also has a disproportionate share of backdating companies. Second, we use state fixed effects instead of the *California* dummy after restricting the data to states with backdating observations. Third, we use industry (two-digit SIC) fixed effects instead of the *HighTech* dummy after restricting the data to industries with backdating observations. Fourth, we examine the impact of alternative specifications on our regression analyses by replicating our main tests (Table 7) using linear probability models instead of logistic models, and using two alternative matching methods, nearest neighbor matching and subclassification matching. In all checks, our inferences are unchanged.

insignificant and that *LawFirmLink* remains significant. These results suggest that our findings are not explained by companies selecting new law firms.

### 6.5 Do Law Firms with Backdating Clients have Other "Lucky" Grant Clients?

We contend that executives were willing to engage in backdating because they learned of the practice from their law firm. Our empirical analysis identifies backdating companies via restatements. However, it is possible that *other* companies engaged in backdating but did not restate their earnings. Prior literature suggests that grant date "luck" is likely to be correlated with backdating (e.g., Bebchuk et al. 2010). Our next test examines whether law firms with more backdating clients were also more likely to have other clients that had "lucky" grants. In other words, if a law firm spread the practice of backdating among its clients, but only some of its clients restated, then we should observe that *other clients* were more likely to have "lucky" grants.

We define a company-year as lucky when a CEO grant date during the year had one of the two lowest closing prices during the period beginning (ending) ten trading days before (after) the grant date. Therefore, a single CEO grant has a 2/21 or 9.5 percent probability of being lucky by random chance. We find that companies grant options on average 1.27 times per year. A company-year in our sample has about a 12.1 percent probability of being lucky by random chance, assuming grant dates are random and independent of each other.[30]

Panel A of Table 12 provides a contingency table that displays whether a company-year is restated for backdating and whether the grant date is "lucky." If "luck" perfectly captured backdating restatements, then we would expect 100% of backdating company-years to be "lucky." The results indicate that 34 percent of backdating company-years are lucky versus 17.1 percent for other company-years. Thus, a

[30] Note that 12.1% is an approximation because luck may not be independent: if a grant is backdated in a given company-year, then other grants for that company-year are more likely to be backdated. In addition, the proportion of "lucky" grants could be higher than 12.1% even in the absence of backdating when companies time news releases (e.g., Aboody and Kasznick 2000).

backdating restatement year is twice as likely to have a "lucky" grant than a regular company-year observation but not all backdating restatements are due to grants being made on the lowest two days of the month.

We next examine the role of law firms. Panel B focuses on companies <u>that did not</u> have backdating restatements. We then analyze the probability of a "lucky" grant in law firms that had backdating clients compared to law firms that did not have backdating clients. The results indicate that the probability that a company is "lucky" is about 9.2 percent higher when its external counsel has another client that backdated (17.8 percent versus 16.3 percent). Note that this test excludes company-years that are identified as backdating via restatements and focus only on non-backdating restatement companies.

Panel C performs the same analysis as Panel B, but we divide the law firms by whether more than four percent of their clients backdated. This four percent threshold is based on the overall proportion of backdating clients identified in large law firms (those with more than 40 client companies) in Panel A of Figure 3. This test is more powerful since it mitigates the effect of backdating companies that are associated with a law firm by chance. This test shows that the probability of being "lucky" is 16.8 percent greater when a company's external counsel has a larger proportion of backdating clients (18.8 percent versus 16.1 percent). Panel D focuses on larger law firms with more than 40 unique clients during the sample period. Within this subsample, the probability of having a "lucky" grant is about 30.1 percent greater for companies with external counsel with larger proportions of backdating clients (19.9 percent versus 15.3 percent).

In summary, Table 12 indicates that law firms with more backdating clients appear to have *other* client companies that are more likely to be "lucky." If a "lucky" grant reflects a backdated grant, then this is consistent with even greater clustering of backdating among clients of certain law firms than reflected in our preceding tests. The results in Table 12 therefore provide additional support for our predictions.

## 7. CONCLUSION

In this paper we ask the question: How do accounting practices spread and in particular how do suboptimal accounting practices spread? Better understanding the answer to this question is important because poor accounting practices can distort economic reality as reflected in the financial statements. This, in turn, reduces the usefulness and reliability of accounting information for monitoring management and the efficient allocation of resources in capital markets.

For a suboptimal accounting rule to spread there must be a demand by executives for the distortion because they see a benefit either to themselves or for their company. However, even though self-interest is important for explaining decision making, other factors also influence judgment. We suggest that a suboptimal accounting practice is more likely to spread when executives learn about the practice from an "expert" who can both explain the practice and confirm that the practice has worked for other companies. The combination of both an "expert" and "the power of the crowd" endorsing the practice can desensitize the executive to the inappropriateness of the practice and sway them to the self-interested suboptimal choice.

We use the stock option backdating scandal to investigate this question. We hypothesize that law firms spread the practice by alerting their clients to this choice and informing their clients that other companies had engaged in the practice. We provide evidence consistent with this explanation. We show that backdating companies are highly connected to each other through the law firms that they used. Our regression analysis indicates that the odds of backdating are between 1.53 and 1.88 times as high when a company's law firm represents or represented another client in a year that it backdated.

Our research raises several questions for future research. Can we learn more about the spread of other suboptimal corporate practices through the use of network analysis? For example, Brown (2011) suggests that the spread of aggressive tax shelters was influenced by board of director networks. Did these directors also recommend the use of certain law firms in creating these shelters and hence do we see law firm links influencing tax decisions? In addition, could the company's general counsel also play a role in the accounting choices made by companies? More generally, is there a way to stop poor accounting and tax practices spreading before regulators such as the FASB, SEC and IRS need to intervene either to change the rules or prosecute parties involved? The spread of opportunistic corporate practices offers profit opportunities to lawyers, investment bankers, and consultants and can benefit individual executives and companies, but they can have important social consequences when these practices are determined to be illegal. Tax and accounting scandals reduce the credibility and reputation of capital markets and impose large emotional, reputational and financial costs on the identified perpetrators.

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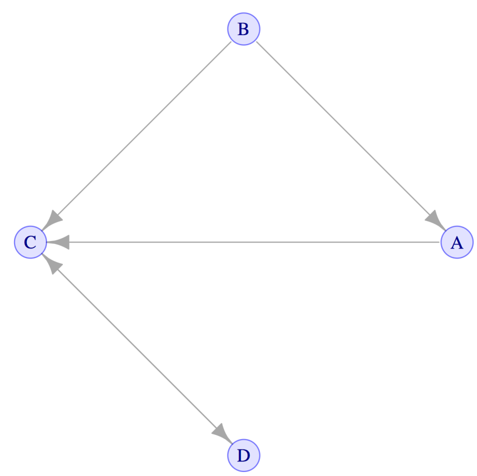
**Exhibit 1**
**Calculation of clustering coefficients and shortest path in a network**

| Node | Linked nodes | Possible links between nodes in any direction | Link Exists | Clustering Coefficient |
|---|---|---|---|---|
| A | B, C | B ↔ C | Yes | 1 |
| B | A, C | A ↔ C | Yes | 1 |
| C | A, B, D | A ↔ B | Yes | |
| | | A ↔ D | No | 0.333 |
| | | B ↔ D | No | |
| D | C | None | N/A | 0 |
| | | Sum of clustering coefficients (4 nodes) | | 2.333 |
| | | **Mean clustering coefficient (2.333 / 4)** | | **0.583** |

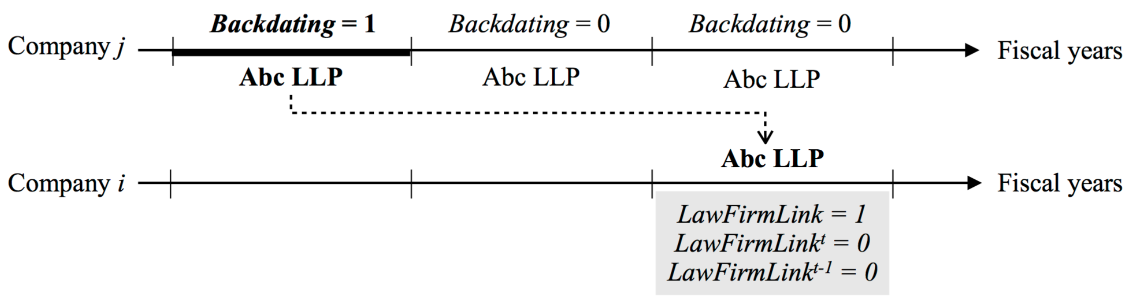
| Node | Shortest path | Length of shortest path |
|---|---|---|
| A | *to B*: (none) | 4 |
| | *to C*: A → C | 1 |
| | *to D*: A → C → D | 2 |
| B | *to A*: B → A | 1 |
| | *to C*: B → C | 1 |
| | *to D*: B → C → D | 2 |
| C | *to A*: (none) | 4 |
| | *to B*: (none) | 4 |
| | *to D*: C → D | 1 |
| D | *to A*: (none) | 4 |
| | *to B*: (none) | 4 |
| | *to C*: D → C | 1 |
| Sum of shortest paths (12 paths) | | 29 |
| **Mean shortest path (29 / 12)** | | **2.417** |



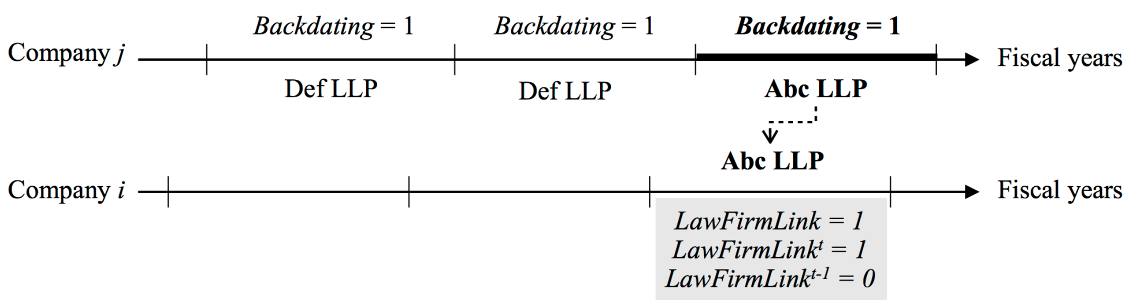

The clustering coefficient of a given node is the proportion of the potential undirected links between the nodes it is linked from or to that exist. Isolated nodes and nodes that are linked from or to only one node are assigned clustering coefficients of zero. The mean clustering coefficient is the mean clustering coefficient over all nodes. The shortest path between two nodes is the minimum number of links needed to reach one node from another, taking the direction of the links into account. The shortest path between nodes that are not linked by any directed path is defined as the maximum possible path length in the network plus one. The mean shortest path of the network is the sum of all shortest paths divided by the total possible number of directed paths.

## Exhibit 2
## Examples depicting the construction of the law firm link variables

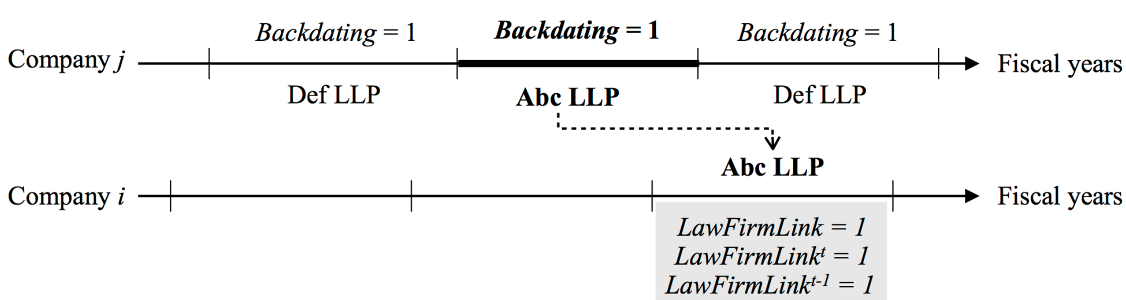
**Panel A**: Company *i* is linked via a law firm to company *j*, which backdated before *t-1*




**Panel B**: Company *j* backdated during *t-2* to *t*, but only had the same law firm during *t*




**Panel C**: Company *j* backdated during *t-2* to *t*, but only had the same law firm during *t-1* and *t*




Panels A, B, and C present example timelines explaining the construction of the *LawFirmLink* variables. Each panel depicts three fiscal years for companies *i* and *j*. In company *i*'s third fiscal year, it was represented by the law firm Abc LLP, which also represented company *j*. Assuming that the law firms did not represent any other backdating company, the value of the *LawFirmLink* variables for company *i* at its third fiscal year depends on when the law firms represented company *j* and when the latter backdated.

**Table 1**
**Sample selection for company-level tests**

| Sample selection | Full sample | | Backdating | |
|---|---|---|---|---|
| | Company-years | Unique companies | Company-years | Unique companies |
| *Panel A: Main analyses* | | | | |
| Compustat company-years, 1997 to 2006 | 116,643 | 19,450 | | |
| Require availability of PERMNO and CIK | 71,117 | 12,397 | | |
| At-the-money option grants during the year | 34,715 | 7,717 | 824 | 141 |
| Restricted to unscheduled CEO grants | 18,591 | 6,142 | 504 | 128 |
| Require daily stock prices around the grant date | 18,505 | 6,105 | 504 | 128 |
| Observations with law firm data available | 15,236 | 5,159 | 475 | 123 |
| Availability of variables for the regressions | 13,912 | 4,762 | 471 | 123 |
| After applying propensity score matching | **10,312** | 3,712 | 471 | 123 |
| *Panel B: Reconciliation to Audit Analytics* | | | | |
| Companies with Backdating code (48) | | | | 171 |
| Companies with Compustat coverage | | | | 158 |
| Companies with CRSP coverage | | | | 152 |
| Companies with Thomson Reuters coverage | | | | 146 |
| Companies with at-the-money option grants | | | | 141 |
| *Panel C: Director links* | | | | |
| Availability of variables for the regressions | 13,912 | 4,762 | 471 | 123 |
| Availability of director data from ISS | 4,671 | 1,520 | 215 | 65 |
| After applying propensity score matching | **3,776** | 1,381 | 215 | 65 |
| *Panel D: Geographic links* | | | | |
| Availability of variables for the regressions | 13,912 | 4,762 | 471 | 123 |
| Availability of city and state data | 13,707 | 4,671 | 462 | 119 |
| After applying propensity score matching | **10,186** | 3,654 | 462 | 119 |

**Table 2**
**The distribution of client companies involved in backdating for law and audit firms**

**Panel A: Law firms (LF) and client backdating**

| (1) LF Size Bins based on number of clients | (2) Number of Law Firms | (3) No. of LF with clients that backdated | (4) % of LF with clients that backdated (3 / 2) | (5) No. of client companies in each LF size bin | (6) No. of companies that backdated | (7) % of companies that backdated (6 / 5) for each LF bin |
|---|---|---|---|---|---|---|
| (0, 5] | 812 | 21 | 2.6% | 1,355 | 21 | 1.5% |
| (5, 10] | 113 | 17 | 15.0% | 797 | 19 | 2.4% |
| (10, 20] | 85 | 16 | 18.8% | 1,161 | 21 | 1.8% |
| (20, 40] | 47 | 22 | 46.8% | 1,292 | 33 | 2.6% |
| (40, Max] | 23 | 20 | 87.0% | 1,617 | 70 | 4.3% |
| Any | 1080 | 96 | 8.9% | 5,159 | 123 | 2.4% |

**Panel B: Audit firms and client backdating**

| | Audit Firm | No. of clients | No. of clients that backdated | % of clients that backdated |
|---|---|---|---|---|
| 1 | Ernst & Young | 1,314 | 37 | 2.8% |
| 2 | PwC | 1,198 | 36 | 3.0% |
| 3 | KPMG | 905 | 23 | 2.5% |
| 4 | Deloitte & Touche | 820 | 27 | 3.3% |
| 5 | Arthur Andersen | 722 | 10 | 1.4% |
| 6 | Grant Thornton | 209 | 4 | 1.9% |
| 7 | BDO Seidman | 164 | 6 | 3.7% |
| 8 | McGladrey and Pullen | 41 | 1 | 2.4% |
| 9 | Other | 512 | 4 | 0.8% |
| | Any | 4,848 | 123 | 2.5% |

Panels A and B show descriptive statistics of companies by the type of law firm or audit firm. In Panel A we partition law firms by the number of unique companies each law firm represented over our sample period. In Panel B we group audit firms by the eight audit firms with more than 40 clients in the sample, and other audit firms. We use the sample of 15,236 company-years with law firm data available, and further require auditor data from Compustat (Compustat: *au*) for Panel B. In the rightmost three columns of both panels we show the number of clients within the law firms or audit firms bin, and the number and proportion of those clients that backdated during the sample period. The numbers in each bin may not sum to the total numbers of clients because a company may be linked to multiple law and audit firms, for example in the case of companies that change law or audit firm during the sample period. The number of individual audit firms in the "Other" category is not available because not all auditors are coded individually on Compustat.

**Table 3A**
**Characteristics of the law firm networks each year, 1997 to 2001**

| Fiscal year | 1997 | 1998 | 1999 | 2000 | 2001 |
|---|---|---|---|---|---|
| *Panel A: Basic descriptives* | | | | | |
| No. of companies | 1,202 | 1,467 | 1,526 | 1,587 | 1,706 |
| No. of links | 10,607 | 17,323 | 19,320 | 22,926 | 33,108 |
| No. of unique law firms | 451 | 483 | 494 | 522 | 502 |
| Mean law firms per link | 1.000 | 1.004 | 1.002 | 1.003 | 1.002 |
| *Panel B: Subsets of the network* | | | | | |
| Size of largest component (#) | 218 | 507 | 714 | 914 | 1,130 |
| Size of largest component (%) | 18.1% | 34.6% | 46.8% | 57.6% | 66.2% |
| Size of next-largest component (#) | 27 | 48 | 24 | 14 | 22 |
| Size of next-largest component (%) | 2.2% | 3.3% | 1.6% | 0.9% | 1.3% |
| Unconnected companies (#) | 218 | 200 | 193 | 204 | 187 |
| Unconnected companies (%) | 18.1% | 13.6% | 12.6% | 12.9% | 11.0% |
| *Panel C: Distribution of in-degree* | | | | | |
| *Non-backdating companies* | | | | | |
| No. of companies | 1,188 | 1,447 | 1,493 | 1,556 | 1,653 |
| Mean in-degree | 8.7 | 11.6 | 12.4 | 14.1 | 18.8 |
| Median in-degree | 3 | 5 | 6 | 6 | 7 |
| Std. dev. of in-degree | 14.8 | 18.9 | 17.8 | 22.3 | 29.7 |
| *Backdating companies* | | | | | |
| No. of companies | 14 | 20 | 33 | 31 | 53 |
| Mean in-degree | 17.9 | 27.3 | 25.8 | 33.4 | 37.6 |
| Median in-degree | 9 | 11 | 13 | 19 | 17 |
| Std. dev. of in-degree | 21.5 | 30.2 | 27.4 | 35.6 | 43.7 |
| *Backdating companies - non-backdating companies* | | | | | |
| Difference in mean in-degree | 9.2 | 15.7 | 13.5 | 19.3 | 18.8 |
| t-statistic | 1.600 | 2.321 | 2.812 | 3.008 | 3.109 |

Table 3A (Table 3B) provides descriptive statistics of the law firm networks each year from 1997 to 2001 (2002 to 2006) inclusive. Each year $t$ we construct networks in which nodes represent companies in the sample at $t$, and a link from company $j$ to company $i$ exists at $t$ if at least one of $i$'s law firms at $t$ also represented $j$ between 1996 and the end of $t$. A node $i$ is labeled as backdating at $t$ if $Backdating_{it}$ = 1. A component of a network comprises a set of nodes that are connected to each other via one or more links, ignoring the direction of the links. The in-degree of a given node is the number of other nodes that link to the node.

**Table 3B**
**Characteristics of the law firm networks each year, 2002 to 2006**

| Fiscal year | 2002 | 2003 | 2004 | 2005 | 2006 |
|---|---|---|---|---|---|
| *Panel A: Basic descriptives* | | | | | |
| No. of companies | 1,603 | 1,629 | 1,660 | 1,570 | 1,286 |
| No. of links | 29,051 | 30,576 | 31,655 | 28,545 | 19,497 |
| No. of unique law firms | 473 | 490 | 486 | 459 | 418 |
| Mean law firms per link | 1.002 | 1.001 | 1.002 | 1.004 | 1.005 |
| *Panel B: Subsets of the network* | | | | | |
| Size of largest component (#) | 1,025 | 1,169 | 1,264 | 1,246 | 976 |
| Size of largest component (%) | 63.9% | 71.8% | 76.1% | 79.4% | 75.9% |
| Size of next-largest component (#) | 44 | 14 | 35 | 8 | 10 |
| Size of next-largest component (%) | 2.7% | 0.9% | 2.1% | 0.5% | 0.8% |
| Unconnected companies (#) | 161 | 177 | 161 | 137 | 135 |
| Unconnected companies (%) | 10.0% | 10.9% | 9.7% | 8.7% | 10.5% |
| *Panel C: Distribution of in-degree* | | | | | |
| *Non-backdating companies* | | | | | |
| No. of companies | 1,540 | 1,551 | 1,585 | 1,500 | 1,248 |
| Mean in-degree | 17.6 | 17.9 | 18.1 | 17.4 | 14.6 |
| Median in-degree | 7 | 7 | 8 | 7 | 6 |
| Std. dev. of in-degree | 26.7 | 28.4 | 28.4 | 26.8 | 19.6 |
| *Backdating companies* | | | | | |
| No. of companies | 63 | 78 | 75 | 70 | 38 |
| Mean in-degree | 31.9 | 35.7 | 38.8 | 33.9 | 32.8 |
| Median in-degree | 18 | 17 | 20 | 20 | 19.5 |
| Std. dev. of in-degree | 36.4 | 42.5 | 47.0 | 40.1 | 29.8 |
| *Backdating companies - non-backdating companies* | | | | | |
| Difference in mean in-degree | 14.4 | 17.7 | 20.7 | 16.5 | 18.1 |
| t-statistic | 3.099 | 3.644 | 3.780 | 3.394 | 3.727 |

This table continues Table 3A for years 2002 to 2006 inclusive.

**Table 4**
**Clustering in networks of backdating and randomly-selected companies from the same industries**

*Panel A: Clustering coefficient (CC)*

| | Backdating | | Distribution of mean CC from 10,000 simulations each year | | | | | | | | Empirical p-values |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Year | No. | **Mean CC** | Mean | StdD | P50 | P75 | P90 | P95 | P99 | Max | |
| 1997 | 13 | **0.179** | 0.024 | 0.074 | 0.000 | 0.000 | **0.000** | 0.231 | 0.308 | 0.538 | 9.58% |
| 1998 | 20 | **0.350** | 0.051 | 0.084 | 0.000 | 0.150 | 0.150 | 0.200 | **0.300** | 0.475 | 0.11% |
| 1999 | 33 | **0.247** | 0.097 | 0.083 | 0.091 | 0.152 | 0.212 | **0.242** | 0.303 | 0.455 | 4.29% |
| 2000 | 29 | **0.345** | 0.092 | 0.087 | 0.103 | 0.138 | 0.207 | 0.241 | **0.310** | 0.497 | 0.38% |
| 2001 | 52 | **0.411** | 0.207 | 0.077 | 0.205 | 0.258 | 0.308 | 0.337 | **0.391** | 0.520 | 0.51% |
| 2002 | 59 | **0.474** | 0.229 | 0.074 | 0.228 | 0.278 | 0.325 | 0.353 | **0.407** | 0.573 | 0.09% |
| 2003 | 75 | **0.486** | 0.265 | 0.065 | 0.263 | 0.308 | 0.348 | 0.373 | **0.424** | 0.504 | 0.03% |
| 2004 | 72 | **0.449** | 0.255 | 0.065 | 0.255 | 0.300 | 0.339 | 0.363 | **0.409** | 0.534 | 0.17% |
| 2005 | 69 | **0.498** | 0.242 | 0.067 | 0.242 | 0.286 | 0.330 | 0.355 | 0.400 | **0.496** | 0.00% |
| 2006 | 38 | **0.344** | 0.167 | 0.089 | 0.160 | 0.228 | 0.282 | **0.320** | 0.386 | 0.526 | 2.67% |

*Panel B: Shortest Path (SP)*

| | Backdating | | Distribution of mean SP from 10,000 simulations each year | | | | | | | | Empirical p-values |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Year | No. | **Mean SP** | Mean | StdD | Min | P1 | P5 | P10 | P25 | P50 | |
| 1997 | 13 | **12.167** | 12.854 | 0.197 | 11.077 | 12.077 | **12.538** | 12.538 | 12.846 | 12.846 | 1.50% |
| 1998 | 20 | **19.103** | 19.748 | 0.245 | 17.558 | 18.900 | **19.300** | 19.411 | 19.700 | 19.800 | 2.78% |
| 1999 | 33 | **31.554** | 32.546 | 0.308 | 28.263 | 31.445 | **31.970** | 32.157 | 32.424 | 32.636 | 1.34% |
| 2000 | 29 | **27.414** | 28.575 | 0.322 | 25.506 | **27.431** | 27.966 | 28.177 | 28.448 | 28.655 | 0.91% |
| 2001 | 52 | **47.125** | 50.820 | 0.689 | 43.729 | **48.230** | 49.537 | 49.995 | 50.561 | 50.983 | 0.22% |
| 2002 | 59 | **55.524** | 57.630 | 0.704 | 50.905 | 55.161 | **56.314** | 56.746 | 57.311 | 57.786 | 1.68% |
| 2003 | 75 | **70.414** | 72.881 | 1.065 | 60.036 | 69.063 | **70.814** | 71.576 | 72.431 | 73.115 | 3.45% |
| 2004 | 72 | **62.890** | 70.001 | 1.013 | 61.946 | **66.543** | 68.058 | 68.684 | 69.544 | 70.239 | 0.03% |
| 2005 | 69 | **63.051** | 67.227 | 0.859 | 61.124 | **64.320** | 65.601 | 66.130 | 66.837 | 67.411 | 0.26% |
| 2006 | 38 | **35.353** | 37.177 | 0.465 | 33.513 | **35.584** | 36.270 | 36.600 | 36.980 | 37.270 | 0.59% |

This table shows the mean clustering coefficient and mean shortest path for the law firm networks of backdating companies each year (shaded columns on the left), and the distributions of the mean clustering coefficient and mean shortest path from 10,000 simulations of same-sized networks of companies selected randomly each year (unshaded columns in the middle). The empirical p-value each year (shaded column on the right) is the proportion of the 10,000 simulated networks that are more clustered than the network of backdating companies. Before the random selection, we omit industries with little or no representation in the backdating sample (two-digit SIC codes that comprise less than one percent of backdating observations). Each year $t$, a law firm network is defined as follows: companies in the network comprise companies in the sample at $t$, and a law firm link from company $j$ to company $i$ exists at $t$ if at least one of $i$'s law firms at $t$ also represented $j$ between 1996 and the end of $t$.

**Table 5**
**Summary statistics of key variables used in regression analysis**

*Panel A: Descriptive Statistics (N = 13,912)*

| | Mean | StdD | P1 | P25 | Median | P75 | P99 |
|---|---|---|---|---|---|---|---|
| $Backdating_t$ | 0.034 | 0.181 | 0 | 0 | 0 | 0 | 1 |
| $LawFirmLink_t$ | 0.352 | 0.477 | 0 | 0 | 0 | 1 | 1 |
| $LawFirmLink_t^t$ | 0.310 | 0.462 | 0 | 0 | 0 | 1 | 1 |
| $LawFirmLink_t^{t-1}$ | 0.285 | 0.451 | 0 | 0 | 0 | 1 | 1 |
| $LawFirmSize_t$ | 2.732 | 1.407 | 0.000 | 1.792 | 2.773 | 3.689 | 5.638 |
| $Size_{t-1}$ | 5.803 | 1.818 | 1.939 | 4.506 | 5.787 | 6.988 | 10.224 |
| $HighTech_t$ | 0.136 | 0.343 | 0 | 0 | 0 | 0 | 1 |
| $Auditor_t$ | 0.892 | 0.310 | 0 | 1 | 1 | 1 | 1 |
| $DispCash_{t-1}$ | 0.228 | 0.266 | -0.053 | 0.012 | 0.122 | 0.392 | 0.916 |
| $PriceVol_t$ | 3.592 | 4.331 | 0.206 | 1.182 | 2.269 | 4.347 | 21.336 |
| $California_t$ | 0.222 | 0.416 | 0 | 0 | 0 | 0 | 1 |

*Panel B: Pearson correlations above and Spearman correlations below the diagonal*

| | | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 | 11 |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 1 | $Backdating_t$ | 1 | 0.11 | 0.12 | 0.12 | 0.11 | 0.11 | 0.07 | 0.04 | 0.07 | 0.08 | 0.13 |
| 2 | $LawFirmLink_t$ | 0.11 | 1 | 0.91 | 0.86 | 0.64 | 0.11 | 0.13 | 0.08 | 0.26 | 0.06 | 0.32 |
| 3 | $LawFirmLink_t^t$ | 0.12 | 0.91 | 1 | 0.85 | 0.62 | 0.08 | 0.14 | 0.09 | 0.27 | 0.07 | 0.33 |
| 4 | $LawFirmLink_t^{t-1}$ | 0.12 | 0.86 | 0.85 | 1 | 0.61 | 0.09 | 0.13 | 0.07 | 0.26 | 0.06 | 0.31 |
| 5 | $LawFirmSize_t$ | 0.11 | 0.66 | 0.63 | 0.62 | 1 | 0.19 | 0.13 | 0.15 | 0.28 | 0.09 | 0.35 |
| 6 | $Size_{t-1}$ | 0.12 | 0.10 | 0.08 | 0.08 | 0.18 | 1 | -0.05 | 0.31 | -0.10 | 0.41 | 0.02 |
| 7 | $HighTech_t$ | 0.07 | 0.13 | 0.14 | 0.13 | 0.14 | -0.05 | 1 | 0.00 | 0.25 | 0.06 | 0.10 |
| 8 | $Auditor_t$ | 0.04 | 0.08 | 0.09 | 0.07 | 0.15 | 0.31 | 0.00 | 1 | 0.03 | 0.15 | 0.03 |
| 9 | $DispCash_{t-1}$ | 0.09 | 0.27 | 0.28 | 0.27 | 0.29 | -0.08 | 0.28 | 0.02 | 1 | 0.08 | 0.31 |
| 10 | $PriceVol_t$ | 0.09 | 0.04 | 0.05 | 0.03 | 0.08 | 0.58 | 0.01 | 0.25 | 0.03 | 1 | 0.06 |
| 11 | $California_t$ | 0.13 | 0.32 | 0.33 | 0.31 | 0.35 | 0.01 | 0.10 | 0.03 | 0.31 | 0.02 | 1 |

Panel A shows the mean, standard deviation, and selected quantiles for key variables used in the main regression analyses. The variables are defined as follows: $Backdating_{it}$ equals 1 if the company-year $\{i,t\}$ overlaps with $i$'s backdating period if it backdated, and 0 otherwise; $LawFirmLink_{it}$, $LawFirmLink_{it}^t$, and $LawFirmLink_{it}^{t-1}$ respectively equal 1 if company $i$ is linked via a law firm at $t$ to another company's backdating during $t$ or earlier, during $t$, or during $t$-$1$, and 0 otherwise; $LawFirmSize_{it}$ is the natural logarithm of the number of unique companies the law firm represented between 1996 and $t$, and if the company had more than one law firm in $t$, the sum of their numbers of unique companies is used; $Size_{i,t-1}$ is the natural logarithm of beginning market value (Compustat: *csho* × *prcc_f*); $HighTech_{it}$ equals 1 if the company's SIC code is between 7370 and 7379 inclusive; $Auditor_{it}$ equals 1 if the company's auditor is one of the Big 5 audit firms (Compustat: *au* between 1 and 8); $DispCash_{i,t-1}$ is cash and cash equivalents less interest expenses scaled by total assets (Compustat: (*che* − *xint*) / *at*), at the beginning of the year; $PriceVol_{it}$ is the standard deviation of daily stock price during the fiscal year, and $California_{it}$ equals 1 if the company was headquartered in California at $t$. All non-dummy variables are winsorized at the top and bottom percentiles each year.

**Table 6**
**Descriptive statistics before and after matching on backdating firm characteristics or law firm link characteristics**

*Panel A: Comparison of backdating to non-backdating companies*

| | Difference in Means (N = 13,912) | | | | After Propensity Score Matching on *Backdating*$_t$ (N = 10,312) | | | |
|---|---|---|---|---|---|---|---|---|
| $Backdating_t$ | 0 | 1 | Diff. | t-stat. | 0 | 1 | Diff. | t-stat. |
| *Matched Control Variables* | | | | | | | | |
| $Size_{t-1}$ | 5.77 | 6.88 | 1.12 | **12.63** | 6.95 | 6.88 | -0.07 | -1.19 |
| $HighTech_t$ | 0.13 | 0.26 | 0.12 | **7.75** | 0.22 | 0.26 | 0.04 | 1.53 |
| $Auditor_t$ | 0.89 | 0.96 | 0.07 | **5.47** | 0.96 | 0.96 | -0.01 | -1.29 |
| $DispCash_{t-1}$ | 0.22 | 0.33 | 0.11 | **8.35** | 0.31 | 0.33 | 0.02 | 0.84 |
| $PriceVol_t$ | 3.53 | 5.40 | 1.87 | **10.98** | 5.93 | 5.40 | -0.53 | -0.06 |
| $California_t$ | 0.21 | 0.52 | 0.30 | **15.59** | 0.49 | 0.52 | 0.03 | 0.37 |
| *Predicted Determinants of Backdating* | | | | | | | | |
| $LawFirmLink_t$ | 0.34 | 0.63 | 0.29 | **11.56** | 0.50 | 0.63 | 0.13 | **3.81** |
| $LawFirmLink_t^t$ | 0.30 | 0.61 | 0.31 | **13.11** | 0.45 | 0.61 | 0.15 | **5.18** |
| $LawFirmLink_t^{t-1}$ | 0.27 | 0.58 | 0.31 | **13.43** | 0.41 | 0.58 | 0.17 | **5.86** |

*Panel B: Comparison of companies linked to a backdating company (LawFirmLink =1) to companies that are not linked to a backdating company (LawFirmLink=0)*

| | Difference in Means (N = 13,912) | | | | After Propensity Score Matching on *LawFirmLink*$_t$ (N = 13,774) | | | |
|---|---|---|---|---|---|---|---|---|
| $LawFirmLink_t$ | 0 | 1 | Diff. | t-stat. | 0 | 1 | Diff. | t-stat. |
| *Matched Control Variables* | | | | | | | | |
| $Size_{t-1}$ | 5.66 | 6.07 | 0.41 | **9.63** | 5.90 | 6.05 | 0.15 | 0.57 |
| $HighTech_t$ | 0.10 | 0.20 | 0.09 | **15.74** | 0.19 | 0.19 | -0.01 | -0.17 |
| $Auditor_t$ | 0.87 | 0.93 | 0.05 | **13.39** | 0.94 | 0.93 | -0.01 | 0.34 |
| $DispCash_{t-1}$ | 0.18 | 0.32 | 0.15 | **31.77** | 0.33 | 0.32 | -0.01 | 0.08 |
| $PriceVol_t$ | 3.38 | 3.97 | 0.59 | **11.70** | 4.20 | 3.92 | -0.28 | 0.70 |
| $California_t$ | 0.13 | 0.40 | 0.27 | **39.64** | 0.41 | 0.39 | -0.02 | -0.12 |
| *Dependent Variable and Other LawFirmLink Measures* | | | | | | | | |
| $Backdating_t$ | 0.0193 | 0.0607 | 0.04 | **11.56** | 0.0325 | 0.0593 | 0.03 | **5.62** |
| $LawFirmLink_t^t$ | 0.00 | 0.88 | 0.88 | **259.01** | 0.00 | 0.88 | 0.88 | **257.66** |
| $LawFirmLink_t^{t-1}$ | 0.00 | 0.81 | 0.81 | **192.23** | 0.00 | 0.81 | 0.81 | **191.01** |

This table provides descriptive statistics for key variables in our regressions before and after matching. Panel A is based on matching backdating and non-backdating (*Backdating*$_t$ = 1 or 0) company-years on the control variables, and Panel B is based on matching linked and non-linked (*LawFirmLink*$_t$ = 1 or 0) company-years on the control variables. We carry out the matching within each year in the sample (1997 to 2006) and report t-statistics adjusted for year fixed effects. For the matched samples we report means and t-statistics weighted according to the output of the matching procedure.

## Table 7
## Logistic Regressions of the relation between backdating and law firm links

| | Logistic regressions (dependent variable: $Backdating_{it}$) | | | | | | |
|---|---|---|---|---|---|---|---|
| | | | | | Propensity Matching on Backdating Characteristics (Table 6 Panel A) | | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| $LawFirmLink_t$ (+) | 1.190*** | 0.450*** | | | 0.426*** | | |
| | (0.097) | (0.110) | | | (0.110) | | |
| $LawFirmLink_t^t$ (+) | | | 0.589*** | | | 0.578*** | |
| | | | (0.109) | | | (0.109) | |
| $LawFirmLink_t^{t-1}$ (+) | | | | 0.632*** | | | 0.629*** |
| | | | | (0.108) | | | (0.107) |
| $Size_{t-1}$ | | 0.292*** | 0.291*** | 0.291*** | -0.026 | -0.029 | -0.028 |
| | | (0.032) | (0.032) | (0.032) | (0.034) | (0.034) | (0.034) |
| $HighTech_t$ | | 0.562*** | 0.545*** | 0.541*** | 0.096 | 0.073 | 0.069 |
| | | (0.118) | (0.118) | (0.118) | (0.114) | (0.114) | (0.114) |
| $Auditor_t$ | | 0.337 | 0.324 | 0.322 | -0.300 | -0.310 | -0.294 |
| | | (0.245) | (0.245) | (0.245) | (0.247) | (0.247) | (0.247) |
| $DispCash_{t-1}$ | | 0.620*** | 0.562*** | 0.555*** | -0.07 | -0.131 | -0.152 |
| | | (0.200) | (0.201) | (0.201) | (0.193) | (0.195) | (0.195) |
| $PriceVol_t$ | | 0.030*** | 0.030*** | 0.031*** | -0.0002 | -0.00003 | 0.0002 |
| | | (0.010) | (0.010) | (0.010) | (0.009) | (0.009) | (0.009) |
| $California_t$ | | 0.995*** | 0.950*** | 0.947*** | -0.118 | -0.167 | -0.162 |
| | | (0.109) | (0.109) | (0.108) | (0.106) | (0.107) | (0.106) |
| (Intercept) | -3.929*** | -7.206*** | -7.187*** | -7.137*** | -3.423*** | -3.393*** | -3.379*** |
| | (0.077) | (0.385) | (0.385) | (0.385) | (0.399) | (0.399) | (0.399) |
| Year fixed effects | N | Y | Y | Y | Y | Y | Y |
| Observations | 13,912 | 13,912 | 13,912 | 13,912 | 10,321 | 10,321 | 10,321 |
| McFadden $R^2$ | 0.038 | 0.127 | 0.130 | 0.132 | 0.021 | 0.025 | 0.026 |
| *Odds ratios* | | | | | | | |
| *LawFirmLink* | 3.287 | 1.568 | 1.802 | 1.881 | 1.531 | 1.782 | 1.876 |
| *Probability-based effect sizes based on median values* | | | | | | | |
| *P(Backdating=1 \| LawFirmLink=0)* | 1.93% | 1.68% | 1.64% | 1.61% | 3.82% | 3.75% | 3.65% |
| *P(Backdating=1 \| LawFirmLink=1)* | 6.07% | 2.60% | 2.91% | 2.99% | 5.73% | 6.50% | 6.63% |
| %Δ in probability | 214.82% | 55.37% | 77.81% | 85.53% | 50.14% | 73.15% | 81.78% |

This table shows the results from estimating Equation 1 with and without propensity score matching and for three variations of the *LawFirmLink* variable. Estimated coefficients are presented with standard errors in parentheses. The sample comprises Compustat company-years between 1997 and 2006 inclusive during which unscheduled stock options were granted to CEOs, for which law firms could be identified, and for which data for estimating the regression model is available. A summary of our sample selection is at Table 1, and variable definitions are provided at Table 5. For columns 5 to 7, propensity score matching between backdating and non-backdating company-years is carried out for each model. The regressions are weighted using the weights from the matching procedure, and are estimated using quasibinomial link functions. Non-backdating observations outside the support of the propensity score are dropped. The p-values are labeled as follows: * if p < 0.1, * if p < 0.05, and *** if p < 0.01.

**Table 8**
**Analysis of whether the likelihood of backdating varies with law firm size (Panel A) and whether the backdating company is from a different industry (Panel B)**

*Panel A: Backdating and law firm size*

| | Logistic regressions (dependent variable: $Backdating_{it}$) | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| | Full | Full | Exclude LF Outlier | Small LF | Large LF |
| $LawFirmLink_t$ (+) | | 0.349*** | 0.296** | 0.975*** | 0.436*** |
| | | (0.133) | (0.118) | (0.199) | (0.158) |
| $LawFirmSize_t$ | 0.127*** | 0.052 | | | |
| | (0.042) | (0.051) | | | |
| (Intercept) | -3.552*** | -3.462*** | -3.592*** | -1.693* | -3.195*** |
| | (0.399) | (0.401) | (0.426) | (0.891) | (0.495) |
| Control variables | Y | Y | Y | Y | Y |
| Year fixed effects | Y | Y | Y | Y | Y |
| Observations | 10,321 | 10,321 | 9,197 | 3,674 | 5,371 |
| McFadden $R^2$ | 0.019 | 0.021 | 0.019 | 0.032 | 0.018 |
| *Odds ratios* | | | | | |
| *$LawFirmLink_t$* | | 1.418 | 1.344 | 2.651 | 1.547 |
| *$LawFirmSize_t$* | 1.135 | 1.053 | | | |

*Panel B: Law firm links conditional on industries being different*

| | Logistic regressions (dependent variable: $Backdating_{it}$) | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| $LFLinkDiffInd_t$ (+) | 0.408*** | | |
| | (0.109) | | |
| $LFLinkDiffInd_t^t$ (+) | | 0.550*** | |
| | | (0.107) | |
| $LFLinkDiffInd_t^{t-1}$ (+) | | | 0.603*** |
| | | | (0.106) |
| (Intercept) | -3.433*** | -3.406*** | -3.387*** |
| | (0.399) | (0.399) | (0.398) |
| Control variables | Y | Y | Y |
| Year fixed effects | Y | Y | Y |
| Observations | 10,321 | 10,321 | 10,321 |
| McFadden $R^2$ | 0.021 | 0.024 | 0.025 |
| *Odds ratios: LFLinkDiffInd* | 1.504 | 1.733 | 1.828 |

Panel A examines the impact of law firm size on our findings. In column 3, company-years represented by the law firm with the highest number of backdating clients are omitted. In columns 4 and 5, the samples are restricted to company-years with above and below the median *LawFirmSize* each year. In Panel B, we replace *LawFirmLink* with *LFLinkDiffInd*, which are defined in the same way as *LawFirmLink* but take the value of one only if the focal company is linked via a law firm to a backdater with a different two-digit SIC. Propensity score matching between backdating and non-backdating company-years is carried out for each model (see Table 6 Panel A). The regressions are weighted using the weights from the matching procedure, and are estimated using quasibinomial link functions. Estimated coefficients are presented with standard errors in parentheses. The p-values are labeled as follows: * if $p < 0.1$, ** if $p < 0.05$, and *** if $p < 0.01$.

## Table 9
## The relation between backdating and law firm links and director links

*Panel A: Comparison of backdating and non-backdating companies for sample with director link*

| | Means before matching (N = 4,671) | | | | Weighted means after matching (N = 3,776) | | | |
|---|---|---|---|---|---|---|---|---|
| $Backdating_t$ | 0 | 1 | Diff. | t-stat. | 0 | 1 | Diff. | t-stat. |
| $DirLink_t$ | 0.12 | 0.20 | 0.09 | **3.53** | 0.18 | 0.20 | 0.03 | 0.44 |
| $LawFirmLink_t$ | 0.36 | 0.64 | 0.28 | **7.67** | 0.56 | 0.64 | 0.08 | 1.42 |
| $LawFirmLink_t^t$ | 0.31 | 0.61 | 0.30 | **8.53** | 0.52 | 0.61 | 0.10 | **2.09** |
| $LawFirmLink_t^{t-1}$ | 0.29 | 0.60 | 0.31 | **9.12** | 0.47 | 0.60 | 0.13 | **2.86** |
| $Size_{t-1}$ | 7.29 | 7.50 | 0.21 | **1.69** | 7.53 | 7.50 | -0.04 | -0.21 |
| $HighTech_t$ | 0.09 | 0.27 | 0.18 | **8.19** | 0.21 | 0.27 | 0.06 | **1.76** |
| $Auditor_t$ | 0.98 | 0.98 | 0.00 | -0.11 | 0.98 | 0.98 | 0.00 | 0.64 |
| $DispCash_{t-1}$ | 0.15 | 0.31 | 0.17 | **11.71** | 0.32 | 0.31 | -0.01 | -0.62 |
| $PriceVol_t$ | 4.91 | 6.51 | 1.60 | **5.34** | 7.20 | 6.51 | -0.69 | 0.11 |
| $California_t$ | 0.19 | 0.54 | 0.35 | **12.33** | 0.54 | 0.54 | 0.00 | -0.55 |

*Panel B: Impact of director links on backdating*

| | Logistic regressions (dependent variable: $Backdating_{it}$) | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| $DirLink_t$ | 0.110 | 0.104 | 0.085 | 0.095 |
| | (0.182) | (0.181) | (0.181) | (0.181) |
| $LawFirmLink_t$ (+) | | 0.311* | | |
| | | (0.167) | | |
| $LawFirmLink_t^t$ (+) | | | 0.424*** | |
| | | | (0.163) | |
| $LawFirmLink_t^{t-1}$ (+) | | | | 0.551*** |
| | | | | (0.162) |
| (Intercept) | -3.684*** | -3.695*** | -3.668*** | -3.676*** |
| | (0.752) | (0.745) | (0.743) | (0.741) |
| Control variables | Y | Y | Y | Y |
| Year fixed effects | Y | Y | Y | Y |
| Observations | 3,776 | 3,776 | 3,776 | 3,776 |
| McFadden $R^2$ | 0.021 | 0.023 | 0.025 | 0.028 |
| *Odds ratios* | | | | |
| *DirLink* | 1.116 | 1.110 | 1.089 | 1.100 |
| *LawFirmLink* | | 1.365 | 1.528 | 1.735 |

This table examines the impact of director links on our findings. $DirLink_{it}$ is a dummy variable that takes the value of one if at least one of company *i*'s directors at *t* was on the board of another company during *t* or earlier in a year that it backdated, and zero otherwise, and is constructed based on data beginning in 1996. Panel A reports the results of propensity score matching between backdating and non-backdating company-years each year. We carry out the matching within each year in the sample (1997 to 2006) and report t-statistics adjusted for year fixed effects. For the matched sample we report means and t-statistics weighted according to the output of the matching procedure. Panel B reports the results from estimating Equation 2, with propensity score matching. The data is restricted to observations with board member data available. The p-values are labeled as follows: * if p < 0.1, ** if p < 0.05, and *** if p < 0.01.

**Table 10**
**The relation between backdating and law firm links with inclusion of geographic links and director links**

*Panel A: Matching backdating and non-backdating companies on the control variables*

| | Means before matching (N = 13,707) | | | | Weighted means after matching (N = 10,186) | | | |
|---|---|---|---|---|---|---|---|---|
| $Backdating_t$ | 0 | 1 | Diff. | t-stat. | 0 | 1 | Diff. | t-stat. |
| $GeoLink_t$ | 0.28 | 0.54 | 0.25 | **11.03** | 0.42 | 0.54 | 0.12 | **4.00** |
| $LawFirmLink_t$ | 0.34 | 0.64 | 0.30 | **11.74** | 0.50 | 0.64 | 0.13 | **3.87** |
| $LawFirmLink_t^t$ | 0.30 | 0.61 | 0.31 | **13.24** | 0.47 | 0.61 | 0.15 | **4.87** |
| $LawFirmLink_t^{t-1}$ | 0.27 | 0.59 | 0.31 | **13.53** | 0.42 | 0.59 | 0.17 | **5.83** |
| $Size_{t-1}$ | 5.76 | 6.87 | 1.11 | **12.37** | 6.91 | 6.87 | -0.04 | -0.79 |
| $HighTech_t$ | 0.13 | 0.26 | 0.13 | **7.98** | 0.23 | 0.26 | 0.03 | 1.15 |
| $Auditor_t$ | 0.89 | 0.96 | 0.07 | **5.38** | 0.97 | 0.96 | -0.01 | **-1.87** |
| $DispCash_{t-1}$ | 0.22 | 0.34 | 0.11 | **8.42** | 0.31 | 0.34 | 0.03 | 0.75 |
| $PriceVol_t$ | 3.53 | 5.33 | 1.80 | **10.57** | 5.80 | 5.33 | -0.48 | 0.34 |
| $California_t$ | 0.22 | 0.53 | 0.31 | **15.70** | 0.50 | 0.53 | 0.02 | 0.23 |

*Panel B: Impact of geographic and director links on backdating*

| | Logistic regressions (dependent variable: $Backdating_{it}$) | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| $GeoLink_t$ | 0.420*** | 0.369*** | 0.367*** | 0.362*** | 0.360** |
| | (0.104) | (0.104) | (0.104) | (0.104) | (0.154) |
| $LawFirmLink_t$ (+) | | 0.400*** | | | |
| | | (0.113) | | | |
| $LawFirmLink_t^t$ (+) | | | 0.522*** | | |
| | | | (0.111) | | |
| $LawFirmLink_t^{t-1}$ (+) | | | | 0.612*** | 0.547*** |
| | | | | (0.110) | (0.161) |
| $DirLink_t$ | | | | | 0.074 |
| | | | | | (0.182) |
| (Intercept) | -3.378*** | -3.298*** | -3.269*** | -3.253*** | -3.648*** |
| | (0.402) | (0.402) | (0.402) | (0.402) | (0.742) |
| Control variables | Y | Y | Y | Y | Y |
| Year fixed effects | Y | Y | Y | Y | Y |
| Observations | 10,186 | 10,186 | 10,186 | 10,186 | 3,727 |
| McFadden $R^2$ | 0.022 | 0.026 | 0.028 | 0.031 | 0.033 |
| *Odds ratios* | | | | | |
| *GeoLink* | 1.522 | 1.446 | 1.443 | 1.436 | 1.433 |
| *LawFirmLink* | | 1.492 | 1.685 | 1.844 | 1.728 |
| *DirLink* | | | | | 1.077 |

Notes: . $GeoLink_t$ is a dummy variable equal to one if the city in which company $i$ is located at $t$ is also the location of company $j$ during $t$ or earlier in a year that $j$ backdated, and zero otherwise. $GeoLink_t$ is constructed based on data beginning in 1996. Panel A reports the results of propensity score matching between backdating and non-backdating company-years each year. We carry out the matching within each year in the sample (1997 to 2006) and report t-statistics adjusted for year fixed effects. For the matched sample we report means and t-statistics weighted according to the output of the matching procedure. The p-values are labeled as follows: * if $p < 0.1$, ** if $p < 0.05$, and *** if $p < 0.01$.

**Table 11**
**The relation between backdating and law firm links: the impact of selection**

| | Logistic regressions (dependent variable: $Backdating_{it}$) | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| $LawFirmLink_t$ (+) | 0.467*** | 0.492*** | | | | |
| | (0.116) | (0.129) | | | | |
| $LawFirmLink_t^t$ (+) | | | 0.560*** | 0.634*** | | |
| | | | (0.115) | (0.127) | | |
| $LawFirmLink_t^{t-1}$ (+) | | | | | 0.609*** | 0.654*** |
| | | | | | (0.113) | (0.125) |
| $LFChange_t$ | 0.070 | 0.036 | 0.136 | 0.117 | 0.050 | 0.015 |
| | (0.233) | (0.235) | (0.224) | (0.226) | (0.219) | (0.220) |
| $LawFirmLink_t \times LFChange_t$ | -0.146 | -0.151 | | | | |
| | (0.298) | (0.300) | | | | |
| $LawFirmLink_t^t \times LFChange_t$ | | | -0.276 | -0.312 | | |
| | | | (0.294) | (0.296) | | |
| $LawFirmLink_t^{t-1} \times LFChange_t$ | | | | | -0.108 | -0.099 |
| | | | | | (0.293) | (0.295) |
| (Intercept) | -3.090*** | -2.410*** | -3.126*** | -2.403*** | -3.135*** | -2.409*** |
| | (0.094) | (0.477) | (0.091) | (0.478) | (0.088) | (0.477) |
| Control variables | N | Y | N | Y | N | Y |
| Year fixed effects | N | Y | N | Y | N | Y |
| Observations | 7,167 | 7,167 | 7,167 | 7,167 | 7,167 | 7,167 |
| McFadden $R^2$ | 0.006 | 0.015 | 0.008 | 0.018 | 0.011 | 0.020 |
| *Odds ratios* | | | | | | |
| LawFirmLink | 1.595 | 1.636 | 1.751 | 1.885 | 1.839 | 1.923 |

This table shows the results from estimating Equation 1 for three variations of the *LawFirmLink* variable, with the addition of $LFChange_t$ and the interactions between $LFChange_t$ and *LawFirmLink*. $LFChange_t$ equals one if at least one of company *i*'s law firms at *t* did not represent *i* in a prior year; 13.4% of company-years in this sample had $LFChange_t$ equal to one. The sample is the same as in the main regression analyses, except that for each company-year we require the availability of law firm data in a prior year. As before, propensity score matching is carried out each year to match backdating and non-backdating observations. Estimated coefficients are presented with standard errors in parentheses. Other variable definitions are at Table 5. The p-values are labeled as follows: * if p < 0.1, * if p < 0.05, and *** if p < 0.01.

**Table 12**
**The likelihood of a "lucky grant" in companies that did not have a backdating restatement but shared a law firms (LF) with a backdating company**

*Panel A: All company-years (N = 15,236)*

| | Backdating-related restatement years | Other years with option grants |
|---|---|---|
| Lucky | 162 | 2,525 |
| Not lucky | 313 | 12,236 |
| % lucky | **34.1%** | **17.1%** |
| $\chi^2$ test p-value | <1% | |

*Panel B: Clean Companies: No backdating restatements (N = 14,628)*

| | LF had backdating clients | LF had no backdating clients |
|---|---|---|
| Lucky | 1,107 | 1,368 |
| Not lucky | 5,110 | 7,043 |
| % lucky | **17.8%** | **16.3%** |
| $\chi^2$ test p-value | 1.48% | |

*Panel C: Clean Companies: No backdating restatements (N = 14,628)*

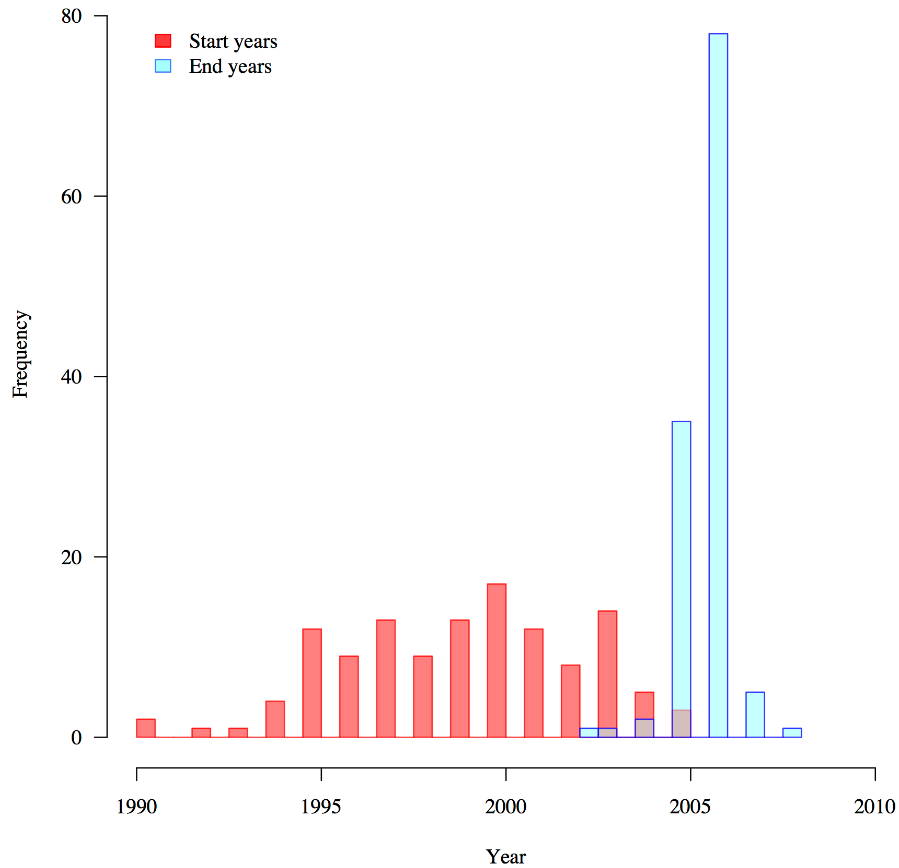
| | More than 4% of LF's clients backdated | Less than 4% of LF's clients backdated |
|---|---|---|
| Lucky | 791 | 1,684 |
| Not lucky | 3,406 | 8,747 |
| % lucky | **18.8%** | **16.1%** |
| $\chi^2$ test p-value | <1% | |

*Panel D: Clean Companies that use large law firms: No backdating restatements (N = 4,426)*

| | More than 4% of LF's clients backdated | Less than 4% of LF's clients backdated |
|---|---|---|
| Lucky | 528 | 272 |
| Not lucky | 2,126 | 1,500 |
| % lucky | **19.9%** | **15.3%** |
| $\chi^2$ test p-value | <1% | |

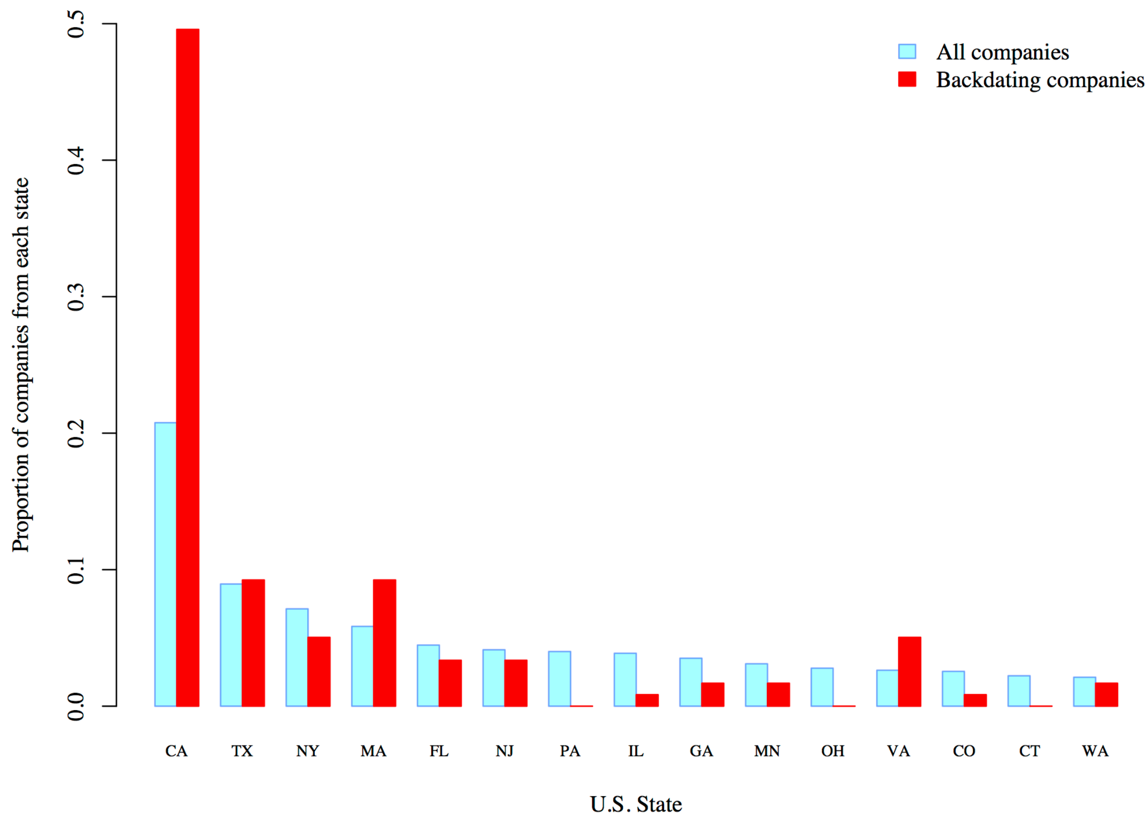
This table is based on the 15,236 company-years between 1997 and 2006 during which CEO grants occurred, and for which law firm data is available. A company-year is lucky if a CEO grant date had one of the two lowest closing prices for the period beginning (ending) ten trading days before (after) the grant date. If a company-year has only one grant date, the probability that it is lucky by chance alone is then 2 / 21 = 9.5%. Because some company-years have more than one grant date, the average number of grants per year in our sample is 1.27. If grant dates are randomly and independently assigned, the probability that a company-year is lucky by chance alone is 1.27 × 9.5% = 12.1%. Panels A to D show contingency tables that give the number of company-years that are lucky or not lucky (rows) for specific subsamples. In Panel A, the columns are based on whether a company-year had a backdating-related restatement; in Panel B, they are based on whether the company-year had a law firm that represented a backdating client; and in Panels C and D they are based on whether the company-year had a law firm for which over 4% of clients had a backdating-related restatement. Panel A, Panels B and C, and Panel D are respectively based on the full sample, the sample of companies that did not restate at any year of the sample period, and non-restating companies that use one of the 23 large law firms (with more than 40 unique clients during the sample period – see Figure 3).

**Figure 1**
**Distributions of backdating start and end years**



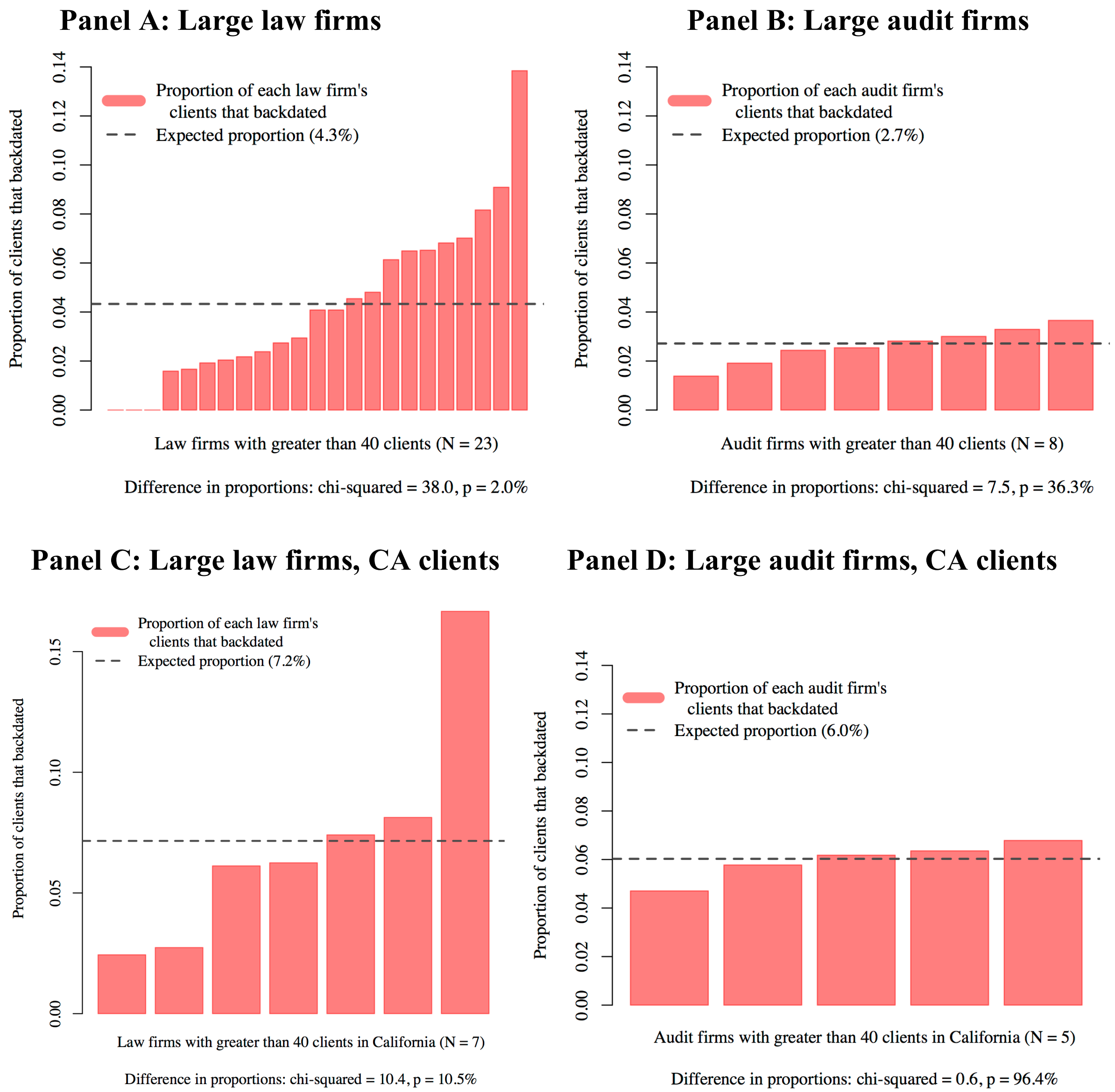

The figure shows backdating start and end years in red and light blue respectively, for the 123 backdating companies in our sample. Beginning with AuditAnalytics' Non-Reliance Restatements database, we restrict the data to restatements involving options backdating, which are coded by AuditAnalytics as category 48 restatements. For each company that filed backdating-related restatements, we define the start and end of its backdating period as the start and end dates of the period for which it is restating (*res_begin_date* and *res_end_date*). The 123 companies began backdating between January 1990 and October 2005, and ended backdating between September 2002 and March 2008.

**Figure 2**
**The proportion of the 4,671 companies and 119 backdating companies headquartered in each state**




The All companies sample of 4,671 includes the 119 backdating companies. Figure 2 is restricted to the top 15 states with option granting companies.

**Figure 3**
**Distribution of backdating clients among large law and audit firms**

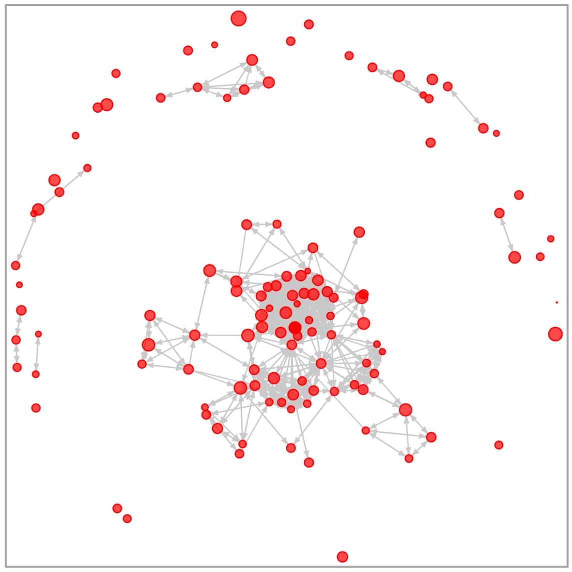



Panels A to D show the proportions of each large law or audit firm's clients that backdated during the sample period. The figures are based on the sample of company-years for which law firms could be identified (N = 15,236). In Panel B we further require availability of audit firm data from Compustat and drop firms coded as unaudited (N = 14,161), and in Panels C and D the data is further restricted to companies in California (N = 3,278 and 3,182 respectively). We compute the total number of unique clients an audit or law firm had over the sample period, and identify whether a client had backdated at a year in which it was linked to the audit or law firm. The dashed lines show the expected proportions of backdating clients per large law or audit firm, defined as the proportion of all unique clients of large law firms or large audit firms that backdated at some point during the sample period. We examine the difference in proportion of clients backdating over the large law firms and audit firms respectively using chi-squared tests. The p-value of the chi-squared tests are computed using Monte Carlo tests (Hope, 1968) with 100,000 replicates.

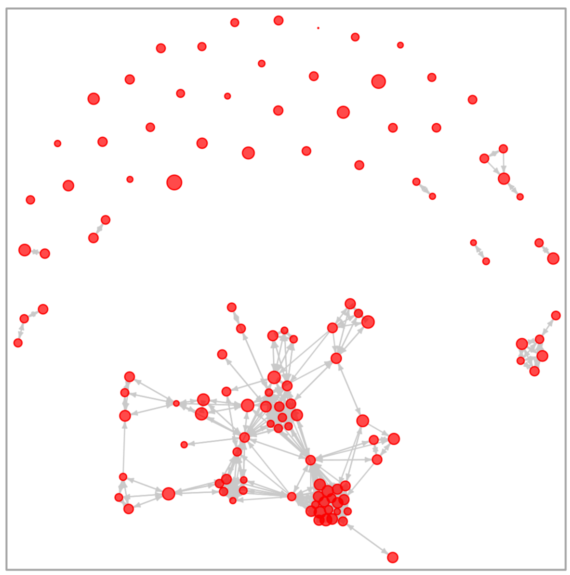
**Figure 4**

**Law firm links between the 123 backdating companies throughout the sample period**

**Panel A: Kamada-Kawai algorithm**

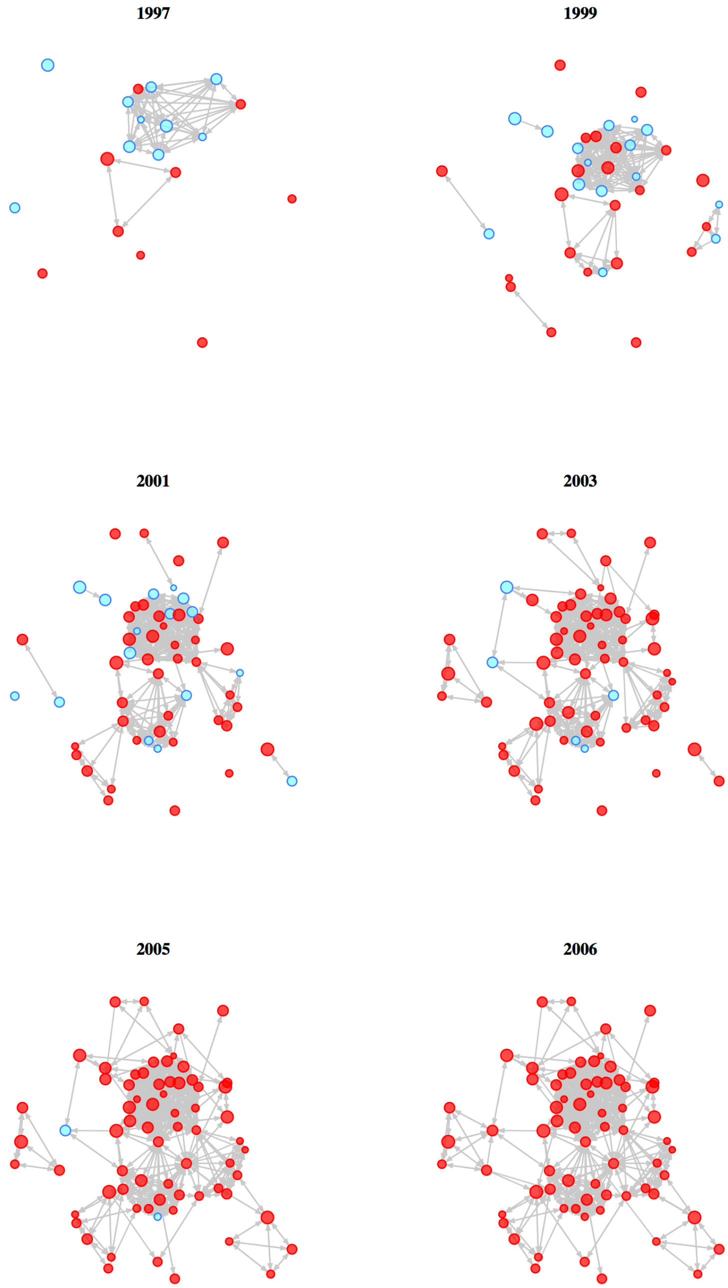


**Panel B: Fruchterman-Reingold algorithm**



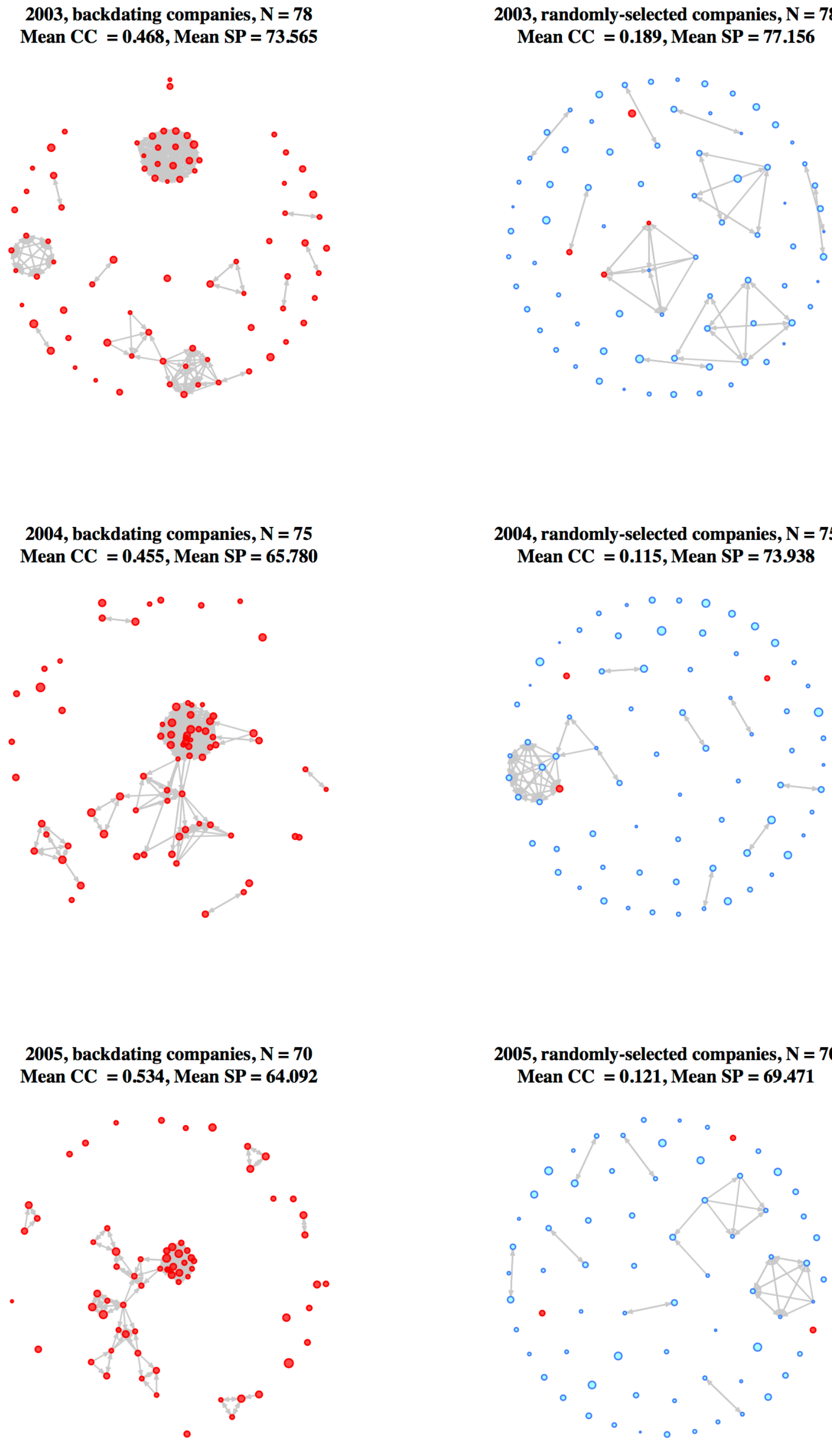
These network diagrams show the law firm links between the 123 companies that backdated at any time in the sample period. A directed link from node *i* to node *j* indicates that *j*'s law firm represented *i* in *t* or earlier. The links are constructed using data beginning in 1996, when EDGAR filing was fully phased in. The size of each node is based on the average size of the company during the sample period. The network diagram in Panel A is drawn based on the Kamada and Kawai (1989) algorithm, which positions nodes on a diagram based on the shortest paths between nodes. The network diagram in Panel B is drawn based on the Fruchterman and Reingold (1991) algorithm, which positions nodes on a diagram by modeling attractive forces between connected nodes and repulsive forces between unconnected nodes.

**Figure 5**
**Law firm links between backdating companies by year (Kamada-Kawai algorithm)**




The figures show the largest connected component of the network of backdating companies depicted in Figure 4, restricted to nodes that had entered the sample by specific years. Nodes are colored red if the company had backdated by $t$, and light blue otherwise. The links are constructed based on data up to each year $t$.

## Figure 6
## Law firm links between backdating companies and randomly-selected companies




The panels show law firm links between companies in the sample in 2003, 2004, and 2005 for backdating and randomly-selected companies. A directed link from node *i* to node *j* indicates that *j*'s law firm represented *i* in *t* or earlier. The networks are drawn using the Kamada and Kawai (1989) algorithm, the size of the nodes are based on company size, and red (light blue) nodes correspond to companies that backdated (did not backdate) that year.