JOURNAL OF FINANCIAL AND QUANTITATIVE ANALYSIS Vol. 58, No. 2, Mar. 2023, pp. 615–646

doi:10.1017/S0022109022000205

Trust and Debt Contracting: Evidence From the Backdating Scandal

Veljko Fotak
University at Buffalo School of Management and Università Bocconi Sovereign Investment Lab
veljkofo@buffalo.edu

Feng (Jack) Jiang
University at Buffalo School of Management
fjiang6@buffalo.edu (corresponding author)

Haekwon Lee
University of Sydney Business School
haekwon.lee@sydney.edu.au

Erik Lie
University of Iowa Tippie College of Business
erik@lie@uiowa.edu

Abstract

We study the effect of trust on debt contracting. We find that, after the revelation of option backdating, borrowers that likely backdated their previous option grants pay higher interest rates on loans. This adverse effect is mitigated by CEO replacements. Results are similar for public debt, but only if a third party identified the backdaters. After the backdating revelation, firms that engaged in backdating increase their reliance on public debt, and those without access to the public debt market experience capital constraints.

> What investors want to give their money when the integrity of that management team is in question? Yet executives who are found to have … backdated their options will find their integrity challenged.
>
> Former SEC Chief Accountant Lynn Turner, Sept. 2006.

I. Introduction

Gambetta (1988) defines trust as "the expectation that another person will perform actions that are beneficial, or at least not detrimental, to us regardless of our capacity to monitor those actions." Furthermore, Arrow (1972) observes that "virtually every commercial transaction has within itself an element of trust,

We thank Gennaro Bernile, Michael Dambra, Xian Gu, Sahn-Wook Huh, Bill Kross, Bill Megginson, Yihui Pan, Kelly Shue, Inho Suk, Xinyuan Tao, Cristian Tiu, Alexander Wagner, Brian Wolfe, and Fei Xie, conference participants at the Financial Management Association (FMA) Annual Meeting, the Australasian Finance and Banking Conference, the Midwest Finance Association (MFA) Annual Meeting, the European Meeting of the FMA, and the UIBE International Conference, and seminar participants at Florida State University, the University of Missouri, Binghamton University (SUNY), and the University at Buffalo for their valuable feedback and comments.

certainly any transaction conducted over a period of time." It follows that lending is especially sensitive to trust.[1]

Lenders offer funding for projects with uncertain payoffs to borrowers whose commitment to repay their debts cannot be guaranteed ex ante. In an incomplete contracting setting where lenders cannot verify or contract on a borrowing firm's activities, trust mitigates adverse selection and moral-hazard problems, which, in turn, affect debt contracting. First, trust alleviates lenders' concerns about the information risk that borrowers present. Consistently, a stream of studies focusing on information transparency establish that unethical behavior affects the credibility of corporate disclosure (Karpoff, Lee, and Martin (2008), Chakravarthy, deHaan, and Rajgopal (2014), and Amel-Zadeh and Zhang (2015)). Second, trust reduces lenders' concern with expropriation risk via asset substitution.

We use the public revelation of executive stock option backdating in 2006 as a negative shock to trustworthiness. Backdating of stock options is the practice of retroactively choosing a grant date with a particularly low stock price and is generally considered to be a manifestation of lax ethical norms in the firm (e.g., Armstrong and Larcker (2009)). Early studies document unusual patterns in stock prices surrounding option grant dates (Yermack (1997), Aboody and Kasznik (2000), and Chauvin and Shenoy (2001)). Lie (2005) postulates that backdating explains the V-shaped price pattern around grant dates. This led to substantial media attention, including a lengthy article in *The Wall Street Journal* (WSJ) on Mar. 18, 2006 (Forelle and Bandler (2006)) that formed the basis for a Pulitzer Prize in public service and educated the public about the pervasive practice (Bernile and Jarrell (2009)).[2] We hypothesize that, in the midst of this debacle, bank lenders' superior ability to gather and process information allowed them to identify specific client firms likely to have backdated option grants.

An underlying assumption of our study is that backdating is suggestive of more widespread unethical behavior, some of which adversely affects lenders. In support of this assumption, Grullon, Kanatas, and Weston (2010) find that firms headquartered in highly religious countries are less likely to backdate options, pay excessive compensation, and manage earnings; Biggerstaff, Cicero, and Puckett (2015) report that firms with CEOs who benefit from option backdating are more likely to engage in earnings management and other corporate misdeeds; and Liu (2016) finds that firms with high corruption culture are associated with more option backdating, earnings management, and insider trading. Furthermore, the "upper echelon theory" suggests that unethical behavior by leaders corrupts overall corporate culture and behavior.[3]

[1]The role of trust in lending has long been recognized by practitioners, who often speak of the five Cs of credit (Character, Capital, Capacity, Collateral, and Conditions), with character proxying for trustworthiness (Duarte, Siegel, and Young (2012)).

[2]Extant research shows that backdating was widespread around the turn of the century, with as many as 30% of public firms engaging in the practice (Heron and Lie (2007), (2009), Bizjak, Lemmon, and Whitby (2009), Collins, Gong, and Li (2009), and Bebchuk, Grinstein, and Peyer (2010)).

[3]Numerous studies find support for the "upper echelons theory" of corporate behavior (Hambrick and Mason (1984), Hambrick (2007)), which posits that unethical corporate culture originates from the actions and attitudes of those at the top level of corporate leadership. For example, Bamber, Jiang, and Wang (2010) find that manager's personal disclosure style affects corporate disclosure.

Our bank loan sample includes 7,530 loans to U.S. firms between 2000 and 2012, of which 34% are extended to firms identified as likely backdaters. To gauge the effect of the backdating revelation on loan spreads, we use a difference-in-differences (DID) estimator in a regression framework, which effectively controls for time trends and firm characteristics. The results from the DID estimation suggest that the backdating revelation elevated the average loan spreads for backdaters (relative to nonbackdaters) by 18 basis points (bps). The effect holds up to a battery of robustness and placebo tests. This suggests that a negative shock in trustworthiness triggers a higher cost of bank loans.

We next examine whether a change in CEO reduces the adverse effects of this shock to trustworthiness. That is, we hypothesize that the presence of a CEO under whose watch unethical acts were committed lowers trust in the firm – conversely, trust should be (partially or entirely) restored if the unethical CEO is no longer present, regardless of the reason for this change. Consistent with this conjecture, our empirical analysis reveals that a change in CEO mitigates the adverse effect of backdating on loan costs.

After studying the responses from bank lenders, we investigate whether public debt markets (i.e., bond investors) react differently to the revelation of backdating. Extant literature documents that lenders across these two markets differ in their ability to gather and process information, incentive to monitor borrowers, and flexibility in renegotiating contracts (Diamond (1984), (1991), Fama (1985), Berlin and Loeys (1988), Houston and James (1996), James and Smith (2000), Bharath, Sunder, and Sunder (2008), and Hasan, Hoi, Wu, and Zhang (2017)). If bond investors, like bank lenders, become aware of specific firms likely to have backdated option grants, we would expect a stronger reaction from bond investors, because the misconduct comes as a greater surprise to them and they are less able to tackle challenging borrowers. On the other side, it is unlikely that all bond investors have the expertise to analyze the option granting process in search of misdeeds, and with their dispersed holdings, bond investors lack incentives to perform proper and independent due diligence (Boot, Milbourn, and Schmeits (2006), Bolton, Freixas, and Shapiro (2012)). On this basis, we do not expect the bond market to punish likely backdaters.

Our DID tests reveal no change in the cost of public debt following the revelation of backdating. While this is consistent with our conjecture that bondholders are unlikely to identify backdaters, it is also consistent with the revelation of backdating not being relevant to the bond market. To explore this further, we examine a subsample of firms that were identified as backdaters in public reports by either WSJ or Glass-Lewis & Co. (Glass-Lewis) – for brevity, we refer to those firms as "identified" backdaters. For this smaller sample, we observe that the cost of public debt increases and the prices of outstanding bonds decrease. Combined, the results suggest that the backdating revelation is relevant to the bond market, but that bond investors are unlikely to gather and process relevant data to identify the culprits.

Given the limited reaction from bond investors following the revelation of option backdating, we expect that backdaters increase their reliance on public over private debt. Our findings support this conjecture; we document that, post-revelation, backdaters are more likely to seek public (rather than private) debt financing and issue a greater share of public debt.

Finally, Stiglitz and Weiss (1981) suggest that lenders respond to higher information risk and agency costs of debt not only by charging higher interest rates and employing nonprice risk-mitigating loan terms, but also by rationing capital. Indeed, we find that the backdating revelation leads to greater financial constraints. In particular, we document that backdaters' investments and cash holdings become more sensitive to cash flows. Moreover, in a subsample analysis, we find that the increased financial constraints are limited to firms without access to public debt markets (i.e., firms without credit ratings), consistent with firms with access borrowing more from public debt markets to dodge financial constraints.

Our research contributes to a budding stream of finance literature that focuses on the role of trust in lending markets. For instance, Moro and Fink (2013) find that small- and medium-size enterprises that enjoy high levels of trust are less constrained in their borrowing and Lewicki, McCallister, and Bies (1998) find that trust reduces monitoring costs. But these studies, as well as the broader literature on trust in financial markets, mostly focus on country-level measures of trust.[4] The role of firm trustworthiness (and how it affects access to, modality of, and cost of debt financing) is less understood.

We further contribute to the growing literature on the effect of corporate culture on corporate behavior and performance (e.g., Benmelech and Frydman (2015), Biggerstaff et al. (2015), Guiso, Sapienza, and Zingales (2015), Liu (2016), Pan, Siegel, and Wang (2016), and Cline, Walking, and Yore (2018)). Our results show that the revelation of malfeasance leads to both a higher cost of debt and increased financial constraints on firms. Furthermore, our results not only echo the evidence in Graham, Li, and Qiu (2008) and Chen (2016) that corporate misreporting led to higher cost of bank loans, but also highlight heterogeneous reactions from the lenders, which, in turn, affect the choice of public versus private debt financing.

II. Data

A. Backdating of Executive Option Grants

For our empirical analysis, we identify firms that are likely to have backdated stock options, consistent with prior studies (e.g., Bizjak et al. (2009), Collins et al. (2009), and Bebchuk et al. (2010)). We do not rely on a list of actual backdaters for

[4]Past studies have shown that a higher level of trust increases economic growth (Knack and Keefer (1997), La Porta, Lopez-de-Silanes, Shleifer, and Vishny (1997), and Zak and Knack (2001)) and financial development (Guiso, Sapienza, and Zingales (2004), (2008)) and lead to more cross-border trade and investment (Guiso, Sapienza, and Zingales (2009)). Gurun, Stoffman, and Yonker (2018) show that trust plays a critical role in financial intermediation by exploiting the geographic dispersion of the victims of the Madoff Ponzi scheme. From a corporate perspective, Pevzner, Xie, and Xin (2015) investigate country-level trust and how it affects investors' reaction to firms' financial disclosures, while Duarte et al. (2012) and Bottazzi, Da Rin, and Hellman (2016) find that trust facilitates access to capital in the context of mergers and acquisition transactions. Giannetti and Wang (2016) study corporate financial misconduct and show that federal securities enforcement actions lead to reduced stock market participation of households in the fraudulent firm's state.

three reasons. First, there simply does not exist a complete list of actual backdaters (much less a list of backdated grants), but only incomplete lists of firms that have been investigated for, accused of, or admitted to backdating. Second, investigations and enforcement actions by the Securities and Exchange Commission (SEC), the Internal Revenue Service, and the Department of Justice are biased toward cases for which there is solid evidence of illegal behavior. Small firms, less egregious cases of backdating, and backdating that violated fewer, if any, securities regulations, tax regulations, and criminal laws have generally gone under the radar. Third, any list of enforcement actions comes with a significant time lag, sometimes years, after the initiation of the investigations. In contrast, our identification methodology uses publicly available and relatively timely data disclosed by the firms and could have been implemented by market participants like banks after the backdating revelation to identify the culprits.

We follow the methodology by Lie (2005) and Bizjak et al. (2009) to identify firms that are likely to have backdated executive option grants. We first obtain the sample of stock option grants to CEOs from the Thomson Financial Insider Filing database. This database captures insider transactions reported on SEC forms 3, 4, 5, and 144. We restrict the sample to transactions that occurred between Jan. 1996 and Dec. 2002.[5] We further require that stock returns be available from 20 trading days before to 20 trading days after the grant date. Finally, following Heron and Lie (2009), we only include grants to the CEO, President, or Chairman of the Board.[6] We eliminate duplicate grants that occur on a given grant date so that we have only one grant "event" for a given date for each firm. Like other studies, we focus on unscheduled awards, because these grants are much more likely to be manipulated (Heron and Lie (2007), (2009)). A grant is identified as "scheduled" if it is issued on the same date, ±1 day, as the preceding year; otherwise, it is classified as unscheduled. Our final CEO option grants sample consists of 29,421 grants across 4,326 companies over the period 1996–2002.

As discussed in Section I, we presume that investors became widely aware of the practice of option backdating from media articles. Given the strong media coverage starting in the spring of 2006, we identify 2006 as a "watershed" period and label the pre-2006 period (ending on Dec. 31, 2005) as "pre-revelation" and the post-2006 period (starting Jan. 1, 2007) as the "post-revelation" period.[7]

B. Syndicated Loan Sample

We obtain syndicated loan data from the Thomson Reuters Loan Pricing Corporation Deal Scan database (DealScan). DealScan includes loans, high-yield

[5]Similar to Bizjak et al. (2009), we begin with 1996 because it is the first year Thomson began collecting data on option grants and we end our sample period in 2002 because Heron and Lie (2009) report that the incidence of backdating drops dramatically after the implementation of new insider reporting guidelines associated with the passage of the Sarbanes–Oxley Act in Aug. 2002.

[6]We include all three categories because, in many instances, CEOs identify themselves by an alternate title (such as "President") in their SEC filings (Heron and Lie (2009)).

[7]There is a substantial gap between the 1996–2002 period used to measure backdating and the backdating revelation in 2006. To the extent that the lack of trust is revealed in some other form in the interim, our analysis is biased toward not finding an incremental effect upon the backdating revelation in 2006.

https://doi.org/10.1017/S0022109022000205 Published online by Cambridge University Press

bonds, and private placement transactions from around the world. It also includes data on loan pricing, contract details, terms and conditions, and information on loan participants (borrower and lender identities and sparse accounting data). The loans are organized by "package" and "facility." Each package represents a loosely defined "deal" and might contain one or multiple facilities (on average, there are 1.5 loans in each package). All loans within the same package share the same borrower, but the identity of the lender or the composition of the lending syndicate, type of loan, loan initiation date, and other contract characteristics can vary between loans from the same package.

For each loan, we estimate the cost to the borrower as the all-in-drawn spread, defined as the total annual cost, including a set of fees and fixed spread, paid over LIBOR for each dollar used under the loan commitment. Henceforth, we refer to the all-in-drawn spread as "spread." We further record the loan maturity (at initiation, in months), the facility amount (in USD), the number of lenders, lenders' headquarters address, indicator variables identifying collateralized loans and senior loans, and information on the number of financial and general covenants. We also create indicator variables based on the database fields identifying "loan type" and "loan purpose."

We limit our sample to loans identified as "364-Day Facility," "Bridge Loan," "Term Loan" of all types, "Revolver Line" of all maturities, and "Other Loan." We further exclude i) loans whose status is "Canceled" or "Rumor," ii) loans with missing or conflicting syndicate information, and iii) loans to financial institutions and utilities. Finally, we only include loans to U.S. firms that issue options to executives as part of their compensation packages, to ensure that we use comparable benchmark firms. The sample period covers 6 years prior to the revelation of backdating (i.e., loans initiated between Jan. 1, 2000 and Dec. 31, 2005) and 6 years following the revelation (i.e., loans initiated between Jan. 1, 2007 and Dec. 31, 2012). Our final loan sample includes 7,530 loans to 1,847 firms.

C. Corporate Bond Samples

The main bond sample comprises newly issued bonds by U.S. companies as reported by the Thomson Reuters SDC Global New Issues database. For each bond issue, SDC provides detailed information, including the interest rate (spread), the issue date, yield-to-maturity (YTM), maturity, proceeds, and rating. We record the BOND_SPREAD as the difference between the YTM of the corporate bond of interest and that of a U.S. Treasury bond with the closest maturity, measured in basis points. We exclude convertible bonds, bonds without information on spread, maturity, and necessary financial statement information for the issuer, and bonds issued by financial institutions and utilities. Similar to our loan sample, we only include bonds by firms issuing options to CEOs as part of their compensation package. Our final sample includes 3,130 bonds issued by 645 firms from 2000 through 2012.

In addition, we collect secondary-market data for use in a "bond event study" analysis, as described in Section IV.A. The raw data we use originated from the Trade Reporting and Compliance Engine (TRACE) database.

https://doi.org/10.1017/S0022109022000205 Published online by Cambridge University Press

D. Additional Data

We obtain borrower-level accounting information from the Compustat database. Firm size (log of total assets), coverage (coverage ratio), leverage (debt-to-asset ratio), profitability (ROA), valuation (Tobin's Q), firm age, cash volatility, and bankruptcy risk (Altman *Z*-score) are used as firm-level control variables. We use Professor Michael Roberts' DealScan-Compustat link file to merge the syndicated loan data and the option grant data.[8] The number of analysts covering the firm is measured by the number of analysts issuing earnings forecasts for the relevant firm during the previous year based on data from the Thomson Reuters IBES database. The Appendix includes a full list of variables with definitions and sources.

III. The Effect on Bank Loans

A. Descriptive Statistics

Table 1 presents summary statistics for price and nonprice loan terms and borrower characteristics. Of 7,530 loans with complete data, 34% involve borrowers that we identify as "backdaters." On average, there are 0.46 backdated grants per borrower in our sample and 1.5 backdated grants per backdater. The average loan is for $379 million, has a maturity of 47 months and an all-in-drawn spread equal to 229 bps over LIBOR, and includes 8.67 syndicate members and 6.28 covenants. The borrowers in our sample are fairly large and mature, with average assets of $3.9 billion and an average age of 24 years.

TABLE 1

Descriptive Statistics

Table 1 presents summary statistics of observations for the variables listed in the Appendix for the sample of 7,530 loans from 2000 to 2012. All loans are to U.S. borrowers that issue option grants to executives.

	N	Mean	25th %	Median	75th %	Std. Dev.
Loan Characteristics						
SPREAD	7,530	229.43	125.00	200.00	300.00	155.30
MATURITY (months)	7,530	47.40	36.00	54.00	60.00	21.46
LOAN_SIZE (in $ millions)	7,530	378.56	50.00	150.00	400.00	917.31
NUMBER_OF_LENDERS	7,530	8.67	3.00	6.00	12.00	8.72
COLLATERAL	7,530	0.72	0.00	1.00	1.00	0.45
NUMBER_OF_COVENANTS	7,530	6.28	4.00	5.00	9.00	3.46
Borrower Characteristics						
BACKDATER	7,530	0.34	0.00	0.00	1.00	0.47
NUMBER_OF_BACKDATED_GRANTS	7,530	0.46	0.00	0.00	1.00	0.76
TOTAL ASSETS (in $ millions)	7,530	3,912.22	335.75	986.49	2,859.40	11,571.50
COVERAGE	7,530	35.05	2.96	6.16	14.36	311.27
LEVERAGE	7,530	0.32	0.16	0.28	0.43	0.24
PROFITABILITY	7,530	0.14	0.07	0.13	0.20	0.92
TOBIN'S Q	7,530	1.67	1.10	1.38	1.86	1.19
FIRM_AGE (in years)	7,530	23.54	10.00	17.00	36.00	16.23
ALTMAN_Z	7,530	1.57	0.84	1.64	2.41	1.81
NUMBER_OF_ANALYSTS	7,530	7.96	0.00	5.00	12.00	9.03
CASH_FLOW_VOLATILITY	7,530	0.05	0.03	0.05	0.06	0.04

[8]Michael Roberts extends the link data used in Chava and Roberts (2008) that includes DealScan and COMPUSTAT links for the period between 1983 and Aug. 2012. The file is available at http://finance.wharton.upenn.edu/~mrrobert.

https://doi.org/10.1017/S0022109022000205 Published online by Cambridge University Press

Before we implement the DID analysis, we check whether backdaters and nonbackdaters are similar in important dimensions before the event. Similarity in observable firm characteristics mitigates the concern that the estimated effect is an artifact of systematic differences in treatment and control firms. Table A of the Supplementary Material shows the comparison of key firm characteristics between backdaters and nonbackdaters before the event. The evidence suggests that there is no statistically significant difference in TOTAL_ASSETS, COVERAGE, LEVERAGE, PROFITABILITY, TOBIN'S_Q, and ALTMAN_Z between backdaters and nonbackdaters.

B. Loan Spreads: Univariate Evidence

We first test whether the revelation of option backdating leads to an increase in the cost of bank loans of firms. To separate temporal trends from the effect of the event of interest, we use a DID methodology and compare differences in loan terms to backdaters to those to nonbackdaters before and after the revelation. We match each loan to a backdater to a loan to a nonbackdater from the same industry, sharing the same loan purpose with the closest initiation date, maturity, and firm size. We do so both for pre-revelation and post-revelation loans, and then compute DID estimators.

Before presenting formal results, we plot the mean spreads on loans to backdaters and nonbackdaters (from the matched sample), by year, in Figure 1. The evidence from the pre-revelation period reveals no violation of the "parallel trends" assumption, with the difference in spreads between loans to backdaters and nonbackdaters hovering around zero. After the revelation, the difference in spreads is

FIGURE 1

The Average All-in-Drawn Spread (Backdaters vs. Nonbackdaters)

Figure 1 presents the average all-in-drawn spread (SPREAD) on loans to backdaters and nonbackdaters and the difference in average all-in-drawn spread (SPREAD) between loans to backdaters and nonbackdaters. The sample includes loans to U.S. borrowers issuing option grants to executives spanning the years 2000–2012. Loans to backdaters and nonbackdaters are matched by loan purpose and industry; from this set, the loan with the closest initiation date, maturity, and borrower firm size is selected. Statistical significance for the difference in spreads is tested using a two-sample t-test. Results are deemed "significant" for p-values below 10%.

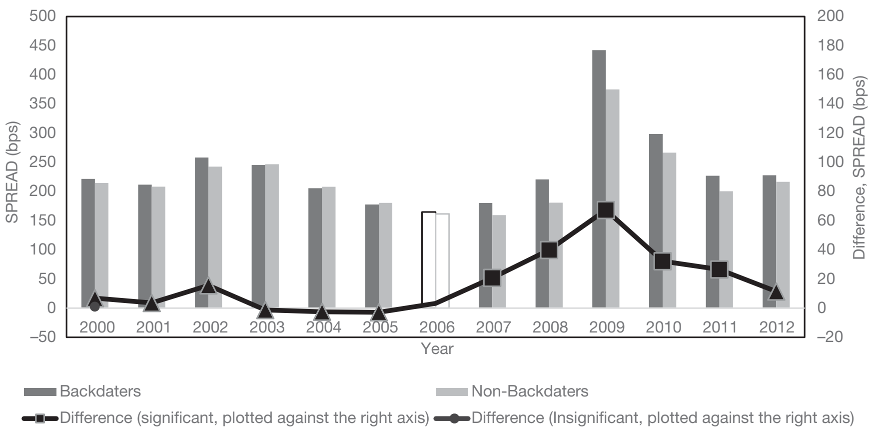

TABLE 2

Univariate Difference-in-Differences Analysis

Table 2 compares means of variables related to price and nonprice terms of matched loans. All variables are defined in the Appendix. The sample includes loans to U.S. borrowers issuing option grants to executives from 2000 to 2012. Loans to backdaters are compared to loans to nonbackdaters. Loans are matched by loan purpose and industry; from this set, the loan with the closest initiation date, maturity, and borrower firm size is selected. The difference-in-differences (DID) between pre-revelation and post-revelation is reported in the last column. Tests for significance of mean differences are implemented as paired *t*-tests with standard errors clustered at the borrower level; tests of significance are two-sided. *, **, and *** denote statistical significance at the 0.10, 0.05, and 0.01 levels, respectively.

	Pre-Revelation		Difference	Post-Revelation		Difference	DID
	Backdater	Nonbackdater		Backdater	Nonbackdater		
	1	2	2	3	4	3 – 4	(3 – 4) – (1 – 2)
SPREAD	206.16	206.4	−0.24	235.92	211.5	24.42***	24.66***
			−0.04			−2.64	−3.04
MATURITY (months)	45.3	45.32	−0.02	52.47	52.48	−0.01	0.01
			−0.16			−0.06	−0.04
LOAN_SIZE (in $ millions)	308.06	300.31	7.75	600.54	523.51	77.03	69.28
			−0.25			−1.26	−1.3
NUMBER_OF_LENDERS	9.26	8.61	0.65	9.33	9.04	0.29	−0.36
			−1.57			−0.57	−0.68
COLLATERAL	70.76	70.08	0.68	64.71	62.16	2.54	1.87
			−0.32			−0.78	−0.66
NUMBER_OF_COVENANTS	6.64	6.37	0.26*	5.35	5.08	0.27	0.01
			−1.69			−1.25	−0.04
No. of Obs.	1,327	1,327	1,327	629	629	629	

positive and statistically significant in all years except in 2012, and the magnitude appears economically significant.[9]

Table 2 presents the means of variables related to price and nonprice loan terms by subperiods (pre- and post-revelation) and *t*-tests for differences in means. During the pre-revelation period, we find no statistically significant difference in spreads between loans to backdaters and nonbackdaters. During the post-revelation period, however, we find that the spreads on loans to backdaters are higher than the spreads on loans to nonbackdaters by 24 bps (236 bps for backdaters vs. 212 bps for nonbackdaters), which is both statistically and economically significant. The DID analysis indicates that the difference in spreads between loans to backdaters and nonbackdaters increases by about 25 bps after the backdating revelation, and the increase is statistically significant at the 1% level.

We also investigate whether a breach of trust causes lenders to employ nonprice mechanisms to mitigate the risk level of a lending contract. Our analysis of the nonprice loan terms includes loan maturity, syndicate size, loan size, a dummy variable identifying collateralized loans (secured vs. unsecured), and the number of covenants. We observe no statistically significant difference in loan maturity, size, number of lenders in the syndicate, and the frequency of use of collateral between loans to backdaters and nonbackdaters in either the pre- or post-revelation period. Further, the DID is statistically insignificant. Although loans to backdaters contain on average 0.26 more covenants compared to loans to nonbackdaters in the pre-revelation period (statistically significant at the 10%

[9]A concern is that the post-revelation period overlaps with the financial crisis. Our DID analysis should be immune to the concern that the financial crisis contaminates our analysis. We further show that our results are robust if we exclude loans initiated during the financial crisis.

level), we find no difference in the number of covenants between the two subsamples in the post-revelation period.

C. The Effect on Loan Spreads

In this section, we employ DID analysis to gauge the effect of the backdating revelation on the cost of bank loans. We do so by running the following loan-level OLS regression:

$$(1) \quad Y_{l,i,j,t} = \alpha_1 + \alpha_2 \text{BACKDATER}_i \times \text{POST\_REVELATION}_t + \alpha_3 \text{BACKDATER}_i + \alpha_4 X_{l,i,t} + \eta_t + \delta_j + \varepsilon_{i,t}.$$

The dependent variable $Y_{l,i,j,t}$ is the all-in-drawn spread for loan l to firm i in industry j at time t. The main variable of interest is the interaction term between a dummy variable identifying a backdating borrower and a dummy variable identifying the post-revelation period ($\text{BACKDATER}_i \times \text{POST\_REVELATION}$). $X_{l,i,t}$ is a vector of firm and loan-level controls. In particular, we control for firm characteristics that past studies find to be associated with the cost of debt in prior literature, including TOTAL_ASSETS, COVERAGE, LEVERAGE, PROFITABILITY, TOBIN'S_Q, FIRM_AGE, SPECULATIVE_RATING, ANALYSTS, CASH_VOLATILITY, INSTITUTIONAL_BLOCKHOLDER, and ALTMAN_Z, as defined in the Appendix.

As option backdating is potentially associated with weak governance (Bernile and Jarrell (2009), Bebchuk et al. (2010)), we add a binary variable, INSTITUTIONAL_BLOCKHOLDER, set equal to 1 if an institutional investor owns a share of 5 percentage points or higher of voting stakes in the firm and 0 otherwise. We use more granular proxies for governance in robustness tests presented in Section III.D.7, but those impose more stringent data requirements, which affects the usable sample size.

Following Graham et al. (2008) and Engelberg, Gao, and Parsons (2012), we also control for loan characteristics, including loan size, loan maturity, and use of collateral.[10] Lastly, we include the default spread (the monthly yield spread between BAA and AAA corporate bond indices), the term spread (the monthly yield spread between a 10-year and a 3-month Treasury bond), and fixed effects for loan initiation year (η_t), borrower industry (δ_j, 2-digit SIC codes), loan type, loan purpose, and debt seniority. The POST_REVELATION dummy is never included separately (aside from the interaction term) because it is absorbed by year fixed effects. The standard errors are adjusted for heteroscedasticity and clustered at the borrower level.

[10]We recognize that price and nonprice terms of the loans are codetermined during negotiations between members of the lending syndicate and the borrower. However, it is difficult to disentangle each component without any theory-based simultaneous model. Our previous analysis based on matched loans indicates that loans to backdaters do not differ substantially in terms of nonprice loan characteristics, which suggests that this simultaneous determination is less likely to affect our findings regarding loan spreads. We nevertheless tackle the joint determination of loan terms in robustness tests in Section III.D.9.

https://doi.org/10.1017/S0022109022000205 Published online by Cambridge University Press

A valid DID estimation requires at least two conditions. First, the backdaters and nonbackdaters should be similar in important dimensions before the event; the only difference between the two groups should be that the former previously engaged in option backdating but the latter did not. Second, there should not be a difference in the trend in cost of loan before the event (the so-called "parallel trend assumption"). The evidence we presented earlier suggests that both conditions are satisfied.

Column 1 of Table 3 reports the DID estimation results. The coefficient of the interaction (BACKDATER × POST_REVELATION) is positive and statistically significant at the 1% level, suggesting that the revelation of option backdating significantly increases the cost of bank loans for backdaters. The impact is also economically meaningful; the cost of bank loans for backdaters increases by 18 bps after the revelation period, representing an 8% increase relative to the average loan spread of 229 bps. Column 2 of Table 3 shows that the results are robust if we replace BACKDATER with BACKDATING_FREQUENCY (the natural logarithm of the number of likely backdated option grants over the period of 1996–2002).

A natural follow-up question is how long bank lenders continue to react to the perceived shock to trust. The preliminary evidence reported in Figure 1 reveals that

TABLE 3

Regressions of Loan Spreads

Table 3 reports DID estimation. The response is the all-in-drawn spread (SPREAD). All variables are defined in the Appendix. The sample includes loans to U.S. borrowers issuing option grants to executives spanning the years from 2000 to 2012. We control for nonprice terms of loans by including log(LOAN_SIZE), log(MATURITY), and COLLATERAL. Firm characteristics are controlled by including, as (unreported) control variables, log(TOTAL_ASSETS), log(COVERAGE), LEVERAGE, PROFITABILITY, TOBIN'S_Q, log(FIRM_AGE), SPECULATIVE_RATING, log(NUMBER_OF_ANALYSTS), INSTITUTIONAL_BLOCKHOLDER, CASH_FLOW_VOLATILITY, and ALTMAN_Z. CREDIT_SPREAD and TERM_SPREAD are included as (unreported) macroeconomic control variables. The model includes fixed effects for loan-initiation year, 2-digit SIC code, loan type, and loan purpose. *t*-statistics from two-sided tests of significance are reported in parentheses with standard errors clustered by firm. *, **, and *** denote statistical significance at the 0.10, 0.05, and 0.01 levels, respectively.

	SPREAD	
BACKDATER	−1.61 (0.37)	
BACKDATING_FREQUENCY		0.85 (0.17)
BACKDATER × POST_REVELATION	18.24*** (2.58)	
BACKDATING_FREQUENCY × POST_REVELATION		16.06** (1.97)
log(LOAN_SIZE)	−15.41*** (8.67)	−15.35*** (8.62)
log(MATURITY)	−23.82*** (4.83)	−23.79*** (4.83)
COLLATERAL	60.98*** (16.35)	60.87*** (16.34)
Intercept	Yes	Yes
Firm characteristics	Yes	Yes
Macroeconomic controls	Yes	Yes
Loan type, loan purpose FE	Yes	Yes
Year FE	Yes	Yes
Industry FE	Yes	Yes
No. of obs.	7,530	7,530
Adjusted R^2	0.557	0.556

https://doi.org/10.1017/S0022109022000205 Published online by Cambridge University Press

the impact declines after 2009 and is not significant after 2011, suggesting that the impact persists for up to 5 years. To test this formally, we extend our post-event period and investigate separately the impact of backdating on the spread of loans issues in the 5 years following the revelation (2007–2012) and in the subsequent 5-year period (2013–2018). The results (untabulated) indicate that loans to backdaters tend to display higher spreads (compared to loans to nonbackdaters) by about 15 bps during the period 2007–2012. In contrast, loans issued to backdaters during 2013–2018 have higher spreads, but the magnitude of the effect is smaller (8 bps) and the estimated coefficients are not statistically significant at conventional levels. This analysis provides further support that the impact of backdating persists for approximately 5 years.

D. Robustness and Placebo Tests

1. Excluding Borrowers Investigated by the SEC and Borrowers That Manipulate Earnings

Biggerstaff et al. (2015) use option backdating to identify corporate executives with "questionable ethics." They find that firms with CEOs personally benefiting from option backdating are more likely to engage in financial reporting fraud. Similarly, Liu (2016) uses cultural background information on key company insiders to create an index of a firm's general attitude toward opportunistic behavior and finds that firms with a high corruption index are more likely to engage in accounting fraud. Accordingly, we recognize that the increase in the cost of loans we observe might be due not to the revelation of unethical behavior, but to the revelation of material new information about future cash flows (e.g., liabilities arising from legal cases caused by option backdating) or past cash flows (e.g., earnings restatements). To address these concerns, we first replicate our regression analysis of loan spreads while excluding backdating firms undergoing SEC investigations, as those firms experience a higher risk of legal liabilities. We present our findings in the first column of Table 4. In additional tests, we further exclude lenders restating earnings. This reduces the sample size from 7,530 loans to 4,957 loans, but the results are similar (column 2 of Table 4). In particular, the spreads increase by about 21 bps for backdaters for the smaller sample.

2. Excluding Loans Initiated During the Financial Crisis

The post-revelation period overlaps with the financial crisis of 2007–2009. Our DID methodology should be immune to the concern that the financial crisis contaminates our analysis. Nevertheless, in column 3 of Table 4, we show that our results persist if we exclude loans initiated during the financial crisis (i.e., loans initiated from July 1, 2007, to Mar. 31, 2009; (Khandani and Lo (2011), Ben-David, Franzoni, and Moussawi (2012)).

3. Excluding Post-SOX Backdaters

The Sarbanes–Oxley Act (SOX) introduced new reporting guidelines, requiring disclosure of option grants within 2 days of the event starting in Aug. 2002. Heron and Lie (2007) find that SOX reduced most, but not all, backdating. To ensure that our results are not driven by firms that continue to backdate option

TABLE 4

Robustness Tests

Table 4 reports results from regression analyses. The response is the all-in-drawn spread (SPREAD). All variables are defined in the Appendix. The sample includes loans to U.S. borrowers issuing option grants to executives spanning the years from 2000 to 2012. We control for (unreported) firm characteristics as in Table 3. CREDIT SPREAD and TERM SPREAD are included as (unreported) macroeconomic control variables. The model includes fixed effects for loan-initiation year, 2-digit SIC code, loan type, and loan purpose. Results for subsamples are reported in columns 1–6: column 1 is based on a sample excluding firms that have been investigated by the SEC for backdating option grants, column 2 is based on a sample excluding firms that have been investigated by the SEC for backdating option grants or that have restated their earnings, column 3 is based on a sample excluding loans during the recent financial crisis – that is, loans initiated during the period from July 1, 2007 to Mar. 31, 2009, column 4 is based on a sample excluding firms that continue to backdate after the adoption of Sarbanes–Oxley Act (2002), column is based on 5 a sample excluding loans with acquisition as the primary financing purpose, and column 6 is based on a sample excluding loans granted to borrowers that are engaging in M&A during the 1 year around the loan initiation date. In column 7, we control for CEO_DELTA and CEO_Vega. In column 8, we control for FOUNDER_CEO. In columns 9 and 10, we control for governance measures: INSTITUTIONAL_OWNERSHIP (column 9) and GOVERNANCE_SCORE (column 10). In column 11, we include firm fixed effects instead of industry fixed effect. *t*-statistics from two-sided tests of significance are reported in parentheses with standard errors clustered by firm. *, **, and *** denote statistical significance at the 0.10, 0.05, and 0.01 levels, respectively.

	SPREAD										
	No Investigated Firms	No Restatement and Investigated Firms	No Loans During Financial Crisis	Excluding Post-SOX Backdaters	Drop Acquisition Related Loan Deals	Drop Borrowers Engaging in M&A Deals (−1 to +1 Years)	Including CEO Delta and Vega	Including Founder CEO	Institutional Ownership	Governance Score	Firm FE
	1	2	3	4	5	6	7	8	9	10	11
BACKDATER	−1.86	−4.24	−1.35	−2.34	−1.7	−3.14	−1.87	−5.24	0.43	−2.99	
	−0.41	−0.76	−0.31	−0.53	−0.37	−0.69	−0.39	−1.08	−0.09	−0.61	
BACKDATER × POST_REVELATION	18.91***	20.71**	17.52**	18.47**	14.44**	15.50**	19.12**	21.43***	15.86**	20.04**	21.73***
	−2.69	−2.34	−2.3	−2.57	−2.01	−2.03	−2.46	−2.8	−2.03	−2.27	−2.71
log(LOAN_SIZE)	−15.60***	−12.96***	−16.13***	−15.52***	−14.90***	−14.42***	−12.53***	−10.35***	−16.54***	−15.64***	−15.60***
	−8.63	−6.17	−8.85	−8.57	−7.71	−7.42	−5.77	−4.92	−9.14	−7.14	−7.35
log(MATURITY)	−24.40***	−26.52***	−27.46***	−23.30***	−36.24***	−26.08***	−18.02***	−25.98***	−19.32***	−20.46***	−8.75*
	−4.89	−4.34	−5.33	−4.81	−7	−4.96	−2.73	−3.9	−3.8	−3.42	−1.76
COLLATERAL	61.81***	61.01***	60.46***	61.38***	53.84***	61.34***	61.62***	60.99***	61.48***	61.26***	43.08***
	−16.3	−13.55	−15.79	−16.01	−14.13	−14.86	−15.37	−15.8	−15.15	−13.78	−7.14
INSTITUTIONAL_BLOCKHOLDER	−5.5	−3.8	−4.53	−6.96*	−3.64	−5.59	−10.71**	−6.75			−0.79
	−1.37	−0.79	−1.12	−1.74	−0.83	−1.32	−2.45	−1.54			−0.15
							−6.46***				
							−3.82				
							−1.26				
							−0.93				
								17.11*			
								−1.66			
									−33.68***		
									−3.73		
										−0.35	
										−0.59	
Intercept	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm, macroeconomic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Loan type and purpose, year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No
Firm FE	–	–	–	–	–	–	No	No	No	No	Yes
Observations	7,408	4,957	6,731	7,288	6,097	6,213	4,684	4,435	6,197	5,361	7,530
Adjusted R^2	0.558	0.568	0.563	0.557	0.588	0.552	0.627	0.64	0.561	0.555	0.517

grants post-SOX, we exclude from our sample 242 loans that were granted to firms that continue to backdate after the adoption of SOX. Column 4 of Table 4 reports that, once more, backdaters pay a higher cost of loan post-revelation, by about 18.5 bps.

4. Excluding Loans Associated With Acquisitions and Borrowers Involved in M&As

Biggerstaff et al. (2015) find that backdating firms are more likely to engage in (unprofitable) acquisitions. Loan pricing can be heavily affected by corporate events such as mergers and acquisitions. To ensure robustness, we exclude from our sample either i) loans whose primary purpose is recorded in Dealscan as an "Acquisition" or ii) loans to borrowers that are involved in a merger or acquisition within one calendar year of the loan initiation. Columns 5 and 6, respectively, of Table 4 show that our main findings are robust to these exclusions.

5. Controlling for CEO Delta and Vega

All firms in our sample offer performance-based compensation to their managers. But the extent of performance-based pay, both in absolute terms and as a proportion of total compensation, varies across firms. Carver, Cline, and Hoag (2013) document that, at the firm level, backdating is itself associated with the proportion of performance pay. To ensure that our results are not biased by omitted variables relating to the extent of performance pay, we include CEO_DELTA, measuring the sensitivity of CEO wealth to a 1% change in the firm's stock price, and CEO_VEGA, measuring the sensitivity of CEO wealth to a 1% change in the standard deviation of the firms' stock price (following Guay (1999), Core and Guay (2002), and Coles, Daniel, and Naveen (2006)), as additional control variables. Column 7 of Table 4 shows that, once more, backdaters pay a higher cost of loan post-revelation, by about 18.7 bps.

6. Controlling for "Founder Effects"

The presence of a founder CEO is linked to a higher likelihood of backdating (Carver et al. (2013)). Because the presence of a founder CEO might also affect debt contracting, we include a control variable identifying founder CEOs. Following the methodology in Jenter and Lewellen (2015), we set FOUNDER_CEO equal to 1 if the borrower's CEO was appointed at least 1 year before the firm's first inclusion in the Compustat database and 0 otherwise. Our findings, presented in column 8 of Table 4, indicate that the results are robust to the inclusion of this additional control variable.

7. Alternative Controls for Borrowers' Internal Governance

Management's involvement in option backdating is suggestive of a governance breakdown (Bernile and Jarrell (2009), Bebchuk et al. (2010)). To rule out the governance channel as the source of our results, all the models discussed so far have included a binary variable, INSTITUTIONAL_BLOCKHOLDER, set equal to 1 if the firm has an institutional shareholder owning more than 5% of voting shares and 0 otherwise, as extant literature finds that the presence of institutional blockholder is associated with better monitoring.

For robustness, the model presented in column 9 of Table 4 controls for the extent of institutional ownership (as extant literature finds that higher institutional ownership is associated with better monitoring), while the model in column 10 controls for an index of the quality of corporate governance. In both cases, we exclude the INSTITUTIONAL_BLOCKHOLDER variable. In constructing our governance index, we use data from the Institutional Shareholder Services (ISS) database and employ the 36 governance standards (mirroring Chung and Zhang (2011)).[11] Our sample size drops due to data-availability issues, but the main results prove robust to the use of these alternative governance metrics.

8. Robustness to Firm Fixed Effects

To ensure that the coefficient of BACKDATER × POST_REVELATION is estimated as the difference between the pre- and post-revelation cost of debt for the same borrower, the model in column 11 of Table 4 includes borrower fixed effects. The magnitude of the estimated impact of the disclosure of unethical behavior on loan spreads is similar (i.e., the average spread increases by 22 bps for backdaters after the revelation of option backdating).

9. Codetermination of Price and Nonprice Loan Terms

Price and nonprice loan terms might be codetermined, in that they are the result of a complex negotiation between borrowers and loan arrangers. Like Bharath, Dahiya, Saunders, and Srinivasan (2011), we employ a two-stage least squares instrumental variable framework to address this concern. In untabulated tests, we find that our results are robust to the use of this alternative methodology.

10. Identification of Backdaters

To further establish robustness, we employ an alternative method to identify backdaters. In particular, we use the list of option grants that are likely to have been backdated from Bebchuk et al. (2010) to construct a dummy variable (LUCKY_CEO) that equals 1 if the firm is likely to have backdated option grants to CEOs and 0 otherwise.[12] In untabulated analysis, we find that the coefficient of the interaction variable LUCKY_CEO × POST_REVELATION is positive and statistically significant.

11. Placebo Tests

As placebo tests, we estimate our main regressions using either 2003 or 2004 as the treatment year. Unless the practice of backdating leaks materially to banks before 2006 (of which we have no evidence), we expect the coefficients for the backdating interaction terms to be insignificant in these placebo tests. Table 5 shows that the coefficients for the backdating interaction term are statistically indistinguishable from zero in our placebo tests. In sum, the results suggest that the increase in spread for backdaters occurs in 2006, and not in 2003 or 2004.

---

[11] We use the 36 governance standards index by Chung and Zhang (2011) because the related data are available for a large portion of our sample, unlike other metrics employed in extant literature. For robustness, we replicate our analysis also by using the G-index as described by Gompers, Ishii, and Metrick (2003). Data availability constrains our sample to 3,441 observations, but results are largely consistent, finding an increase in spreads of about 15 bps post-revelation for backdaters.

[12] Bebchuk et al. (2010) identify a grant as "lucky" if it was given at the lowest stock price of the month. The data are available at http://www.law.harvard.edu/faculty/bebchuk/data.shtml.

TABLE 5

Placebo Tests

Table 5 reports results from regression analyses. The response is the all-in-drawn spread (SPREAD). All variables are defined in the Appendix. The sample includes loans to U.S. borrowers issuing option grants to executives spanning the years from 2000 to 2012. In columns 1 and 2 (columns 3 and 4), we identify 2003 (2004) as the revelation period. We control for (unreported) firm characteristics as in Table 3. CREDIT_SPREAD and TERM_SPREAD are included as (unreported) macroeconomic control variables. *t*-statistics from two-sided tests of significance are reported in parentheses with standard errors clustered by firm. *, **, and *** denote statistical significance at the 0.10, 0.05, and 0.01 levels, respectively.

	Post-Revelation (2003)		Post-Revelation (2004)	
	SPREAD		SPREAD	
BACKDATER	5.92 (1.15)		3.81 (0.80)	
BACKDATING_FREQUENCY		7.84 (1.31)		6.22 (1.10)
BACKDATER × POST_REVELATION	−2.28 (0.37)		0.94 (0.15)	
BACKDATING_FREQUENCY × POST_REVELATION		−2.50 (0.35)		−0.14 (0.02)
log(LOAN_SIZE)	−15.20*** (8.50)	−15.20*** (8.51)	−15.23*** (8.52)	−15.21*** (8.52)
log(MATURITY)	−23.68*** (4.80)	−23.65*** (4.79)	−23.70*** (4.80)	−23.67*** (4.80)
COLLATERAL	60.78*** (16.32)	60.73*** (16.34)	60.81*** (16.32)	60.75*** (16.33)
Intercept	Yes	Yes	Yes	Yes
Firm characteristics	Yes	Yes	Yes	Yes
Macroeconomic controls	Yes	Yes	Yes	Yes
Loan type, loan purpose FE	Yes	Yes	Yes	Yes
Year, Industry FE	Yes	Yes	Yes	Yes
No. of obs.	7,530	7,530	7,530	7,530
Adjusted R^2	0.556	0.556	0.556	0.556

E. The Effect of Management Changes

Replacing fraudulent CEOs is likely to be interpreted as an attempt by the borrowers to rebuild trust. Thus, we conjecture that a change in CEO who personally benefited from backdating alleviates the negative effect of the revelation of backdating on loan costs.

To test our conjecture, we construct a dummy variable that captures whether borrowers changed their CEO after 2002, which is the last year used to identify backdating. For this purpose, we exclude CEO retirements and deaths, following Janney and Gove (2019). [13] We interact this variable with BACKDATER and POST_REVELATION. Column 1 of Table 6 reports the results. While backdaters experience an increase in the cost of debt of 38 bps, this effect is nearly negated for firms with a new CEO. The three-way interaction between BACKDATER, POST_REVELATION, and NEW_CEO is associated with a negative coefficient of 32 bps, indicating that, for backdaters with a new CEOs (i.e., firms that experienced backdating under a prior CEO), the increase in the cost of debt is only 1 bp. In the second column of Table 6, we conduct a similar analysis by substituting the dummy variable identifying backdaters with BACKDATING_FREQUENCY. The coefficient of the three-way interaction between BACKDATING_FREQUENCY,

[13] We rely on the ExecuComp database identify CEO turnover events, which causes our sample size to shrink for this analysis.

TABLE 6
New CEOs

Table 6 reports results from regression analyses. The response is the all-in-drawn spread (SPREAD). All variables are defined in the Appendix. The sample includes loans to U.S. borrowers issuing option grants to executives spanning the years 2000–2012. We control for (unreported) firm characteristics as in Table 3. CREDIT_SPREAD and TERM_SPREAD are included as (unreported) macroeconomic control variables. The models include fixed effects for loan initiation year, 2-digit SIC code, loan type, and loan purpose. The models also include NEW_CEO. *t*-statistics from two-sided tests of significance are reported in parentheses with standard errors clustered by firm. *, **, and *** denote statistical significance at the 0.10, 0.05, and 0.01 levels, respectively.

	SPREAD	
BACKDATER × POST_REVELATION	38.00***	
	(3.63)	
BACKDATING_FREQUENCY × POST_REVELATION		33.62***
		(2.87)
BACKDATER × POST_REVELATION × NEW_CEO	−32.07**	
	(2.10)	
BACKDATING_FREQUENCY × POST_REVELATION × NEW_CEO		−27.92*
		(1.65)
Intercept	Yes	Yes
Firm characteristics, macroeconomic controls	Yes	Yes
Loan type, loan purpose, year, industry FE	Yes	Yes
No. of obs.	4,324	4,324
Adjusted R^2	0.659	0.658

POST_REVELATION, and NEW_CEO is negative and statistically different from zero at the 10% level.[14]

In the test described above, we do not consider whether the CEO left the firm for reasons specifically related to backdating. As an additional test, we identify CEO changes that occurred in the years after the backdating revelation in 2006, which are more likely triggered by backdating. In particular, we split the dummy variable for CEO changes into two dummy variables depending on whether the CEO change occurred between Mar. 2006 and July 2010 or some other time after 2002.[15] In untabulated results, we find that the coefficients for both dummy variables (when interacted with BACKDATING and POST_REVELATION) are negative, but it is only statistically significant at the 0.10 level for the dummy variable indicating a CEO change during the years after the backdating revelation.[16]

IV. The Effect on Bonds and the Choice Between Private and Public Debt

A. The Effect on Bond Spreads

Prior literature documents that bank lenders and bond investors differ in many important ways. First, banks have superior access to information (Fama (1985),

[14]Replacing CEOs could weaken executive power and reinforce corporate governance, or it could increase management risk. Consistent with the latter, Pan, Wang, and Weisbach (2018) report that the loan spread increases following executive turnover. In our sample of nonbackdaters, we find no change in loan spread associated with management changes.

[15]We use July 2010 as cutoff because it is the last date for SEC charges related to stock option backdating (https://www.sec.gov/spotlight/optionsbackdating.htm).

[16]In another untabulated test, we added a CEO change dummy for retirements and deaths (which we excluded in our definition of CEO changes above). The coefficient for this dummy (when interacted with backdating and post-revelation) is close to zero.

James and Smith (2000), and Bharath et al. (2008)). Thus, relative to bank lenders, bond investors are more sensitive to changes in information (Leland and Pyle (1977), Diamond (1984), Boyd and Prescott (1986), Houston and James (1996), and Denis and Mihov (2003)). Second, banks have stronger incentives to engage in costly monitoring (Berlin and Loeys (1988), Diamond (1991), and Houston and James (1996)). Third, banks are more effective in disciplining firms and they have more flexibility in renegotiating contracts than public debt holders (Gertner and Scharfstein (1991), Park (2000), and Denis and Mihov (2003)). Consequently, if bond investors were aware of the identity of specific firms likely to have backdated option grants, we expect bond investors to be more susceptible to the potential information risk (due to their disadvantage in access to information) and asset substitution (due to their inefficient and costly monitoring and reduced flexibility in contract renegotiating), and, in turn, react more forcibly to the revelation of backdating, compared to bank lenders.

However, the identification of backdating requires access to relevant data, a detailed understanding of the option granting process, and subsequent statistical analysis of compensation data. We have several reasons to believe that, relative to banks, bond investors are at a disadvantage in this process. First, bond investors have access to less data and other information that facilitates the identification of backdating. Second, (institutional) bond investors typically manage larger and less concentrated portfolios (due to a higher number of borrowers) than loan officers, which leads to a limited attention problem (Hirshleifer and Teoh (2003), Cornaggia, Hund, and Nguyen (2018), and Li (2018)). Third, (institutional) bond investors (e.g., pension fund managers) have less incentives to perform costly due diligence (Bolton et al. (2012)).[17] If bond investors fail to identify likely backdaters, we would expect to observe no reaction.

To examine the effect on the cost of bond, we replicate the earlier loan cost analysis using a sample of public debt issues. We include controls for borrower characteristics as in Table 3. In addition, we control for bond characteristics, including BOND_SIZE, BOND_MATURITY, CALLABLE_BOND, PUTTABLE_BOND, SUBORDINATED_BOND, and BOND_RATING. We include PRIOR_BOND_ISSUANCE, a dummy variable identifying firms that have issued bonds in the past.

Table 7 reports the results. We find no evidence of a statistically significant increase in the cost of public debt for backdaters after the revelation of executive option backdating. In the base specification, we find that the coefficient estimate for the interaction BACKDATER $\times$ POST_REVELATION is negative and small (-0.90 bps), and not statistically different from zero. We similarly find no economically or statistically significant results when replacing the binary variable identifying backdaters with BACKDATING_FREQUENCY. The results are robust to the inclusion of fixed effects and the exclusion of 1,005 bonds in our sample that are callable or puttable.

Forelle and Bandler (2006) and Glass-Lewis identified publicly a list of possible backdaters (Carow, Heron, Lie, and Neal (2009)), which obviated the need

[17]Pension funds are key investors in corporate bond market, and their managers' compensation only marginally depends on the ex post return of the assets they manage (Bolton et al. (2012)).

https://doi.org/10.1017/S0022109022000205 Published online by Cambridge University Press

TABLE 7

Regressions of Bond Spreads

Table 7 reports results from regression analyses. The response is the BOND_SPREAD, computed as the difference between the yield to maturity of a bond and the yield to maturity of a risk-free bond measured in basis points. All variables are defined in the Appendix. The sample includes bonds issued by U.S. firms issuing option grants to executives from 2000 to 2012. We control for (unreported) firm characteristics as in Table 3 and for the following bond characteristics: log(BOND_SIZE), log(BOND_MATURITY), CALLABLE_BOND, PUTTABLE_BOND, SUBORDINATED_BOND, BOND_RATING, and PRIOR_BOND_ISSUANCE. CREDIT_SPREAD and TERM_SPREAD are included as (unreported) macroeconomic control variables. The model includes fixed effects for bond-issuance year and the 2-digit SIC code. In columns 3 and 4, we include firm fixed effects instead of industry fixed effects. *t*-statistics from two-sided tests of significance are reported in parentheses with standard errors clustered by firm. *, **, and *** denote statistical significance at the 0.10, 0.05, and 0.01 levels, respectively.

	BOND_SPREAD			
BACKDATER	9.34			
	(1.12)			
BACKDATING_FREQUENCY		11.99		
		(1.32)		
BACKDATER × POST_REVELATION	−0.90		0.09	
	(0.08)		(0.01)	
BACKDATING_FREQUENCY × POST_REVELATION		−4.14		−0.35
		(0.31)		(0.04)
log(BOND_SIZE)	5.15	5.23*	−1.42	−1.42
	(1.64)	(1.67)	(0.64)	(0.65)
log(BOND_MATURITY)	12.87***	13.02***	17.00***	17.00***
	(3.72)	(3.77)	(5.76)	(5.76)
CALLABLE_BOND	−33.11***	−33.05***	1.33	1.34
	(4.79)	(4.79)	(0.26)	(0.26)
PUTTABLE_BOND	−5.58	−5.59	1.44	1.44
	(0.85)	(0.85)	(0.26)	(0.26)
SUBORDINATED_BOND	−31.77**	−31.34**	−6.78	−6.77
	(2.47)	(2.44)	(0.59)	(0.59)
BOND_RATING	−17.87***	−17.83***	−14.66***	−14.66***
	(6.24)	(6.23)	(12.26)	(12.26)
PRIOR_BOND_ISSUANCE	−13.44	−13.11	−9.58	−9.59
	(1.46)	(1.43)	(1.12)	(1.12)
Intercept	Yes	Yes	Yes	Yes
Firm characteristics	Yes	Yes	Yes	Yes
Macroeconomic controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	No	No
Firm FE	No	No	Yes	Yes
No. of obs.	3,130	3,130	3,130	3,130
Adjusted R^2	0.738	0.738	0.812	0.812

for bond investors to identify backdaters on their own.[18] Thus, to the extent that bond investors struggled to identify backdaters, we conjecture that they respond more strongly to this subset of firms. To test our conjecture, we partition our sample of likely backdaters into those that either Forelle and Bandler (2006) or Glass-Lewis identified ("identified" backdaters) versus others ("nonidentified"), and then examine the bond spread effect for each sample. For comparison, we also examine the bank loan spread effect for each sample.

Table 8 reports the results. Among identified backdaters, the bond spread increases 95 basis points from the pre-revelation period to the post-revelation period, which is statistically significant at the 0.10 level and greater than the analogous increase of 70 basis points for the bank loan spread. For nonidentified

[18]Glass-Lewis & Co. published a list identifying 257 firms that had announced internal reviews, Securities and Exchange Commission inquiries, or Justice Department subpoenas related to their past stock-option grants as of Mar. 20, 2007. For more information, see http://www.glasslewis.com/.

TABLE 8

Univariate Analysis: Investigated Backdaters Versus Noninvestigated Backdaters

Table 8 presents the mean loan spread (SPREAD) and bond spread (BOND_SPREAD). All variables are defined in Appendix. The sample includes loans or bonds to U.S. borrowers issuing option grants to executives from 2000 to 2012. In the first row, the loans to the investigated backdaters (those listed in Forelle and Bandler (2006) ("WSJ") or the Glass-Lewis report) are compared with the matched loans to noninvestigated backdaters. Loans are matched by loan purpose and industry; from this set, the loan with the closest initiation date, maturity, and borrower firm size is selected. In the second row, the bonds to the investigated backdaters (listed in WSJ or the Glass-Lewis report) are compared with the bonds to noninvestigated backdaters. Bonds are matched by type of call features and industry; from this set, the bond with the closest initiation date, maturity, and borrower firm size is selected. The difference-in-differences (DID) between pre-revelation and post-revelation is reported in the last column. Tests for significance of mean differences between groups are implemented as two-sided *t*-tests. *, **, and *** denote statistical significance at the 0.10, 0.05, and 0.01 levels, respectively.

		Pre-Revelation		Difference		Post-Revelation		Difference	DID
		Backdaters (in WSJ or Glass-Lewis)	Nonbackdaters			Backdaters (in WSJ or Glass-Lewis)	Nonbackdaters		
	Obs.	1	2	1 – 2	Obs.	3	4	3 – 4	(3 – 4) – (1 – 2)
SPREAD	78	181.96	188.43	–6.47 (0.36)	63	311.63	241.52	70.11* (1.84)	76.58*** (2.69)
BOND_SPREAD	31	306.00	313.10	–7.10 (0.46)	48	364.56	270.08	94.48* (1.93)	101.60* (1.87)
		Backdaters (Neither in WSJ Nor Glass-Lewis)	Nonbackdaters			Backdaters (Neither in WSJ Nor Glass-Lewis)	Nonbackdaters		
	Obs.	1	2	1 – 2	Obs.	3	4	3 – 4	(3 – 4) – (1 – 2)
SPREAD	1,021	211.71	210.68	1.03 (0.15)	424	252.79	229.10	23.69** (2.38)	22.65** (2.40)
BOND_SPREAD	387	236.81	238.83	–2.02 (0.10)	467	254.85	265.85	–11.00 (0.72)	–8.98 (0.61)

https://doi.org/10.1017/S0022109022000205 Published online by Cambridge University Press

backdaters, the bond spread decreases an insignificant 11 basis points from pre-revelation period to the post-revelation period, compared to an increase of 24 basis points for the bank loan spread. The results suggest that backdating is relevant for both bank loan and bond spreads, but bond investors seem to lack the ability or incentives to identify backdating on their own.

Lastly, we conduct a bond event study to investigate the impact of the backdating revelation on bond prices in the secondary market. We measure daily abnormal bond returns by subtracting matched portfolio returns based on bond ratings and maturity, mirroring the methodology used in Bodnaruk and Rossi (2016). To calculate portfolio returns, we use volume-weighted portfolios as suggested by Bessembinder, Kahle, Maxwell, and Xu (2008). The returns are estimated for the period between Mar. 6, 2006 (10 trading days before WSJ published the "Perfect Payday" article) and June 30, 2006.

In untabulated analysis, we compare the cumulative abnormal bond returns for nonbackdaters, nonidentified backdaters, and identified backdaters. The mean returns for the former two groups are modestly negative and statistically indistinguishable from zero (respectively, −0.50 percentage points for nonbackdaters and −0.81 percentage points for nonidentified backdaters). However, the mean return for the set of identified backdaters is an astounding −7.53 percentage points and statistically different from zero at the 0.01 level. These results corroborate the notion that the bond market responds negatively to the revelation of backdating, but only if the identity of backdaters is revealed in public reports.

B. The Effect on the Choice of Private Versus Public Debt

The choice of public versus private debt financing depends on lender monitoring, ease of renegotiation in case of distress, future access to capital, and concerns about protecting proprietary information (e.g., Leland and Pyle (1977), Campbell and Kracaw (1980), Diamond (1984), (1991), Fama (1985), Berlin and Loeys (1988), Rajan (1992), and Park (2000)). Because our results thus far suggest that the backdating revelation increases the spread on bank loans relative to that on bonds, we conjecture that, holding the above constant, the backdating revelation tilts backdaters' preference toward bonds as a source of debt.

To test our conjecture, we follow Bharath et al. (2008) and model the choice of debt issue (public vs. private) in a linear probability framework. The response variable is a binary variable equal to 1 if the debt issue is public (i.e., a bond) and 0 if the debt issue is private (i.e., a bank loan). We control for firm characteristics, including TOTAL_ASSETS, COVERAGE, LEVERAGE, PROFITABILITY, TOBIN'S_Q, FIRM_AGE, INSTITUTIONAL_BLOCKHOLDER, and ALTMAN_Z, as defined in the Appendix. We also control for PRIOR_BOND_ISSUANCE using a dummy variable that equals 1 if the firm has issued bonds at any point in time prior to the debt issuance in question. Our main variable of interest is, as before, the interaction between BACKDATER and POST_REVELATION. The sample includes both private and public debt issues.

Panel A of Table 9 presents the results. The coefficient of BACKDATER is statistically insignificant, suggesting that backdaters and nonbackdaters are equally likely to choose public debt over private debt prior to the backdating revelation.

https://doi.org/10.1017/S0022109022000205 Published online by Cambridge University Press

The coefficient of BACKDATER × POST_REVELATION is 0.04 and statistically significant at the 0.05 level. Thus, consistent with our conjecture, the likelihood of issuing public debt increases by 4 percentage points for backdaters after the backdating revelation. The results are similar if we use BACKDATING_FREQUENCY in place of the BACKDATER dummy. The results are also similar if we exclude firms that WSJ or Glass Lewis identify as backdaters (not tabulated).

As an additional test of backdaters' increased reliance on public debt markets, we create a metric for the proportion of new public debt as a fraction of total new debt issues. Accordingly, for each firm year, we compute the ratio between the dollar amount of new public debt issues over the sum of new public and private debt issues. Then we regress this metric on the same set of explanatory variables used in the previous test for debt choice. Panel B of Table 9 reports the results. We find that,

TABLE 9

Regressions of the Choice Between Public and Private Debt

Table 9 reports results from regression analyses. All variables are defined in Appendix. The sample includes both private debt issues (syndicated loans) and public debt (bonds) to U.S. firms issuing option grants to executives spanning the years 2000–2012. In Panel A, the response variable is equal to 1 if the debt issue is public and 0 otherwise. We control for firm characteristics including log(TOTAL_ASSETS), TOBIN'S_Q, PROFITABILITY, log(COVERAGE), LEVERAGE, ALTMAN_Z, log(FIRM_AGE), SPECULATIVE_RATING, and INSTITUTIONAL_BLOCKHOLDER. In Panel B, the response variable is the ratio of the annual dollar amount of newly issued bonds to the total annual dollar amount of newly issued total debt (both public and private). The models include fixed effects for year and the 2-digit SIC code. *t*-statistics from two-sided tests of significance are reported in parentheses with standard errors clustered by firm. *, **, and *** denote statistical significance at the 0.10, 0.05, and 0.01 levels, respectively.

Panel A. Probability of Issuing Public Debt (Linear Probability Model)

	PUBLIC_DEBT	
BACKDATER	0.004 (0.33)	
BACKDATING_FREQUENCY		0.004 (0.32)
BACKDATER × POST_REVELATION	0.04** (2.03)	
BACKDATING_FREQUENCY × POST_REVELATION		0.05** (2.06)
log(TOTAL_ASSETS)	0.05*** (12.12)	0.05*** (12.12)
TOBIN'S_Q	0.01* (1.91)	0.01* (1.92)
PROFITABILITY	0.10 (1.41)	0.10 (1.42)
log(COVERAGE)	−0.01 (1.63)	−0.01* (1.66)
LEVERAGE	0.10*** (3.68)	0.10*** (3.68)
ALTMAN_Z	0.01 (1.52)	0.01 (1.56)
log(FIRM_AGE)	0.02** (2.01)	0.02** (2.02)
SPECULATIVE_RATING	−0.23*** (11.15)	−0.23*** (11.13)
INSTITUTIONAL_BLOCKHOLDER	0.004 (0.39)	0.004 (0.38)
Intercept	Yes	Yes
Year, industry FE	Yes	Yes
No. of obs.	7,333	7,333
Adjusted R^2	0.336	0.336

(continued on next page)

TABLE 9 (continued)

Regressions of the Choice Between Public and Private Debt

Panel B. Public Debt Issues Over Total Debt Issues

	PUBLIC_DEBT/TOTAL_DEBT	
BACKDATER	−0.02 (1.05)	
BACKDATING_FREQUENCY		−0.02 (0.87)
BACKDATER × POST_REVELATION	0.06** (1.99)	
BACKDATING_FREQUENCY × POST_REVELATION		0.06** (1.98)
log(TOTAL_ASSETS)	0.08*** (12.44)	0.07*** (12.36)
TOBIN'S_Q	0.004 (0.55)	0.004 (0.54)
PROFITABILITY	0.01** (2.55)	0.01** (2.53)
log(COVERAGE)	−0.03*** (2.90)	−0.03*** (2.90)
LEVERAGE	0.17*** (4.09)	0.17*** (4.09)
ALTMAN_Z	0.03*** (4.40)	0.03*** (4.40)
log(FIRM_AGE)	0.03** (2.12)	0.03** (2.09)
SPECULATIVE	−0.19*** (7.57)	−0.19*** (7.58)
INSTITUTIONAL_BLOCKHOLDER	0.08*** (3.87)	0.08*** (3.88)
Intercept	Yes	Yes
Year, industry FE	Yes	Yes
No. of obs.	3,541	3,541
Adjusted R^2	0.379	0.379

relative to other firms, backdaters' ratio of public to total debt increases by 6% (p-value < 0.05) following the backdating revelation. And, again, the results are similar if we use the number of backdated option grants in place of the backdater dummy.

The tests presented in Table 8 reveal that bond markets do react to a subset of backdaters – those publicly identified. Accordingly, we hypothesize that publicly identified backdaters would be less likely to migrate from private to public debt markets following the revelation. To test this hypothesis, we replicate the analysis presented in Table 9 using this smaller subset of "identified" backdaters. In untabulated tests, we find that the estimated coefficients associated with the BACKDATER × POST_REVELATION and BACKDATING_FREQUENCY × POST_REVELATION interactions are positive, as in the overall sample, but not statistically significant. Hence, we are able to reject the null hypothesis (that there is no migration to public debt markets after the revelation of backdating) for the overall sample, but not for the subsample.

V. The Effect on Financial Constraints

Stiglitz and Weiss (1981) argue that lenders respond to higher risk by charging higher interest rates, employing nonprice risk-mitigating loan terms, and rationing capital. Thus, we conjecture that backdaters suffer from greater capital constraints after the backdating revelation. To test this, we employ two proxies for financial constraints to ensure robustness. The first proxy is the sensitivity of investments to cash flows (Fazzari, Hubbard, and Petersen (1988)). Fazzari et al. (1988) argue that financially constraint firms have a greater investment to cash flow sensitivity than financially unconstraint firms. In our setting, post backdating revelation, because backdating firms would face a higher cost of external financing, they would rely more on internal cash flows to fund their investments, this would predict a greater investment to cash flow sensitivity for backdating firms post backdating revelation. Alternatively, backdating firms, post backdating revelation, are expected to save more cash out of cash flows due to a higher cost of external financing. Hence, we use the sensitivity of the change in cash holdings to cash flows as the second proxy of financial constrains (Almeida, Campello, and Weisbach (2004), Erel, Jang, and Weisbach (2015)). If our conjecture is correct, we should observe that the backdating revelation inflates backdaters' sensitivity of investments and change in cash holdings to cash flows.

We define capital expenditures scaled by lagged capital as the dependent variable as in Guner, Malmendier, and Tate (2008).[19] The main explanatory variables include CASH_FLOW, BACKDATER, POST_REVELATION, and their two- and three-way interactions. We further include other firm-level characteristics (TOBIN'S_Q, log of TOTAL_ASSETS and their interactions with CASH_FLOW), and fixed effects for years, ratings, and industry. The main variable of interest is the three-way interaction BACKDATER × CASH_FLOW × POST_REVELATION.

Table 10 presents the coefficient estimates. Consistent with prior literature, we find a positive relation between investments and cash flows. Most importantly, the coefficient of the interaction term (BACKDATER × CASH_FLOW × POST_REVELATION) is positive and statistically significant, indicating that the backdating revelation increases the sensitivity of investments to cash flow for backdaters. In column 2, we find that the sensitivity of investments to cash flows is also related to the number of backdating grant contracts.

In columns 3 and 4, we test whether the sensitivity of the change in cash holdings (scaled by total assets) to cash flows increases following the backdating revelation for backdaters. The dependent variable is ΔCASH, the change in cash holdings (scaled by total assets). We find that the sensitivity of the change in cash holdings to cash flows increases significantly for backdaters following the backdating revelation. Further, we find that the magnitude of the increase is related to the number of backdated option grants.

Our earlier tests indicate that, unlike banks, public debt market responses to the backdating revelation are not discernible in the overall sample. Thus, backdaters with access to the bond market might be able to dodge the increase in capital constraints. To test this proposition, we bifurcate our sample into borrowers with credit ratings (approximately one-third of our sample) and borrowers without credit ratings, as the existence of a credit rating proxies for access to public debt markets. Then we run the regressions of investments and changes in cash holdings separately

[19]We exclude firms with negative cash flows following Allayannis and Mozumdar (2004).

https://doi.org/10.1017/S0022109022000205 Published online by Cambridge University Press

TABLE 10

Analysis of Financial Constraints

Table 10 reports results from regression analyses. The response variable in columns 1 and 2 is INVESTMENT, measured as capital expenditures scaled by total asset. The response variable in columns 3 and 4 is ΔCASH, measured as the difference between cash holdings (scaled by total asset) and lagged cash holdings (scaled by total assets). All variables are defined in Appendix. The sample includes U.S. firms with option grants for executives spanning the years from 2000 to 2012. Firms with negative cash flows are excluded. We control for firm characteristics by including log(TOTAL ASSETS), SALES_GROWTH, LEVERAGE, PROFITABILITY, TOBIN'S_Q, log(FIRM_AGE), ALTMAN_Z, DIVIDEND, and INSTITUTIONAL_BLOCKHOLDER. The model includes fixed effects for year, rating, and the 2-digit SIC code. *t*-statistics from two-sided tests of significance are reported in parentheses with standard errors clustered by firm. *, **, and *** denote statistical significance at the 0.10, 0.05, and 0.01 levels, respectively.

	INVESTMENT		ΔCASH	
BACKDATER × CASH_FLOW × POST_REVELATION	0.10*** (3.74)		0.10** (2.44)	
BACKDATING_FREQUENCY × CASH_FLOW × POST_REVELATION		0.08*** (3.17)		0.09** (2.05)
BACKDATER	0.01*** (3.87)		0.01** (2.28)	
BACKDATING_FREQUENCY		0.01*** (3.39)		0.004* (1.74)
CASH_FLOW	0.10*** (5.34)	0.07*** (4.31)	0.06*** (3.15)	0.04** (2.41)
BACKDATER × POST_REVELATION	−0.01*** (3.59)		−0.01* (1.89)	
BACKDATING FREQUENCY × POST_REVELATION		−0.01*** (3.11)		−0.01 (1.35)
CASH FLOW × POST_REVELATION	−0.07*** (3.28)	−0.04** (2.24)	−0.04* (1.88)	−0.02 (1.06)
BACKDATER × CASH_FLOW	−0.08*** (4.48)		−0.06*** (3.19)	
BACKDATING_FREQUENCY × CASH_FLOW		−0.07*** (4.65)		−0.05*** (2.65)
Intercept	Yes	Yes	Yes	Yes
Firm characteristics	Yes	Yes	Yes	Yes
Year, rating, industry FE	Yes	Yes	Yes	Yes
No. of obs.	19,287	19,287	19,287	19,287
Adjusted R^2	0.415	0.414	0.028	0.028

for these subsamples. In untabulated analysis, we find that the coefficients of all three-way interaction variables are positive. But for both regressions of investment and changes in cash holdings, the coefficient magnitudes are greater for the sample without credit ratings. Moreover, while all four coefficients of interest differ statistically from zero (at the 0.1 level or lower) for the sample without credit ratings, none of the corresponding coefficients for the sample with credit ratings differ statistically from zero.

In sum, our results indicate that the revelation of backdating raises financial constraints for backdaters. However, this only holds for backdaters without credit ratings; backdaters with credit rating appear not to be affected, presumably because they can issue bonds at the same cost as before the revelation.

VI. Validating the "Trust Channel"

In this section, we perform additional tests to validate the relation between backdating and trust. First, we investigate the impact of option backdating on trust in individual corporations. As a proxy for trust, we construct a firm-level trust index based on the raw data provided by Kinder, Lydenberg, Domini, & Co. (KLD).[20] Mirroring extant literature, we construct a trust index that combines the scores for the categories including "community," "diversity," "employee relations," "environment," "human rights," and "corporate governance." The scores for individual categories are obtained by adding "strengths" and subtracting "concerns;" the strengths and concerns for each category are scaled by the maximum strengths and concerns for each year. Additionally, we construct an alternative trust measure that specifically focuses on the trustworthiness of the firm's disclosures by combining the scores for "reporting quality" and "accounting concerns." In Table B1 of the Supplementary Material, we report that, compared to nonbackdaters, identified backdaters experienced a significant decline in corporate trust measures, while there is no discernible deterioration in trust measures for nonidentified backdaters. This suggests that corporate trust measures may not be able to fully capture all information relevant to trustworthiness, especially when the identity of backdating firms is not revealed to the public.

Second, we investigate the impact of option backdating on state-wise corporate trust. As a proxy for state-level trust, we rely on the "Confidence in Institutions" surveys provided by Gallup Analytics. Mirroring the methodology used in Giannetti and Wang (2016), we analyze the relationship between the frequency of option backdating among firms headquartered in a particular state and the local level of trust in big business. Due to data availability, our sample period is 2002–2010. The results are presented in Table B2 of the Supplementary Material. The dependent variable is *Trust in Big Business*, measured as the number of survey respondents that have high confidence in the big businesses in their state divided by the total number of survey respondents. Our first variable of interest is a dummy variable that equals 1 if the number of backdaters in proportion to the total number of firms in a state is greater than the sample median and 0 otherwise. The second variable of interest is the number of backdaters in proportion to the total number of firms within the state. Consistent with our priors, backdating leads to a deteriorating level of trust in "big business" in the state in which these firms are headquartered, offering direct evidence of a link between the incidence of backdating, and trust.

Overall, our additional tests indicate that option backdating leads to deteriorating levels of trustworthiness in the involved firms, but also to lower levels of trust in big business in the affected states. Coupled with the finding that lower levels of trust are associated with higher spreads, they strongly buttress the main interpretation of our findings: that the higher cost of debt we observe is due to backdating's impact on trust.

VII. Conclusion

We investigate the effect of a shock to perceived trustworthiness on debt contracting using the revelation of the backdating scandal in 2006 as the empirical framework. We show that the backdating revelation leads to an increase in the cost

[20]This is motivated by extant literature suggesting that a firm's CSR activities are a good proxy for its social capital, and that a firm's CSR activities generate trust (e.g., Sacconi and Degli Antoni (2011), Lins, Servaes, and Tamayo (2017)).

of bank loans by 18 bps for backdaters, representing an 8% increase in spreads relative to the pre-revelation period. Furthermore, backdaters that replace their CEOs effectively curtail the increase in the cost of loans.

We also examine the effect on bond spreads. Consistent with the notion that bond markets have inferior access to relevant information and a lower incentive to screen and monitor borrowers, we find no evidence that the backdating revelation leads to an increase in bond spreads for backdaters in the overall sample. Our evidence suggests that bond investors fail to process publicly available data on option grants to gauge which borrowers have engaged in backdating. As further support, we find that the bond spread increases and bond prices decrease for a subsample of firms publicly identified as backdaters, and that backdaters shift to increase their reliance of public versus private debt following the backdating revelation.

Finally, we document that the revelation of option backdating leads to an increased sensitivity of both investments and changes in cash-holdings to cash flow among backdaters, which we interpret to mean that the backdating revelation led to greater financial constraints among the offending firms. Further bolstering this interpretation, we find that the increase in financial constraints is limited to borrowers without credit ratings. Apparently, borrowers with access to public debt markets are able to dodge the increase in financial constraints, likely because they can readily issue more bonds to fund their investment and liquidity needs.

Appendix. Variable Definitions

Terms of the Loans

MATURITY: Time to maturity (in months) at issuance. Source: Dealscan.

LOAN_SIZE: The total size of the facility committed in U.S. dollars. Source: Dealscan.

SPREAD: Amount the borrower pays in basis points over LIBOR for each dollar drawn down. Source: Dealscan.

NUMBER_OF_ LENDERS: Number of participants (including lead arranger) in the facility. Source: Dealscan.

COLLATERAL: A binary variable that equals 1 if the facility is secured and 0 otherwise. Source: Dealscan.

NUMBER_OF_COVENANTS: Number of covenants in a loan. Source: Dealscan.

RELATIONSHIP_LOAN: A dummy variable that equals 1 if the borrower received a loan arranged by the lender in the past 5 years and 0 otherwise. See Bharath et al. (2011). Source: Dealscan.

Option Backdating

BACKDATER: A dummy variable that equals 1 if the borrower is likely to have backdated option grants and 0 otherwise, following Heron and Lie (2007) and Bizjak et al. (2009). Source: Thomson Financial Insider Filing database.

BACKDATING_FREQUENCY: Natural logarithm of (1 + the number of backdated option grants). Source: Thomson Financial Insider Filing database.

https://doi.org/10.1017/S0022109022000205 Published online by Cambridge University Press

LUCKY_CEO: A dummy variable that equals 1 if the borrower has granted an option to the CEO on a day that had the lowest price of the month and 0 otherwise. Source: Bebchuk et al. (2010).

POST_REVELATION: A dummy variable that equals 1 if the loan is initiated after 2006 and 0 otherwise. Source: Dealscan.

Firm Characteristics (as of Dec. 31 of the Year Preceding Loan Initiation)

TOTAL_ASSETS: Book value of assets of the firms. Source: Compustat.

COVERAGE: Ratio of EBITDA to interest expenses from the year preceding loan initiation. Source: Compustat.

LEVERAGE: Ratio of book value of debt (total) to book value of assets from the year preceding loan initiation. Source: Compustat.

PROFITABILITY: Ratio of earnings before interest, taxes, depreciation, and amortization to sales from the year preceding loan initiation. Source: Compustat.

TOBIN'S_Q: Ratio of (book value of assets − book value of equity + market value of equity) to book value of assets from the year preceding loan initiation. Source: Compustat.

FIRM_AGE: Number of years since the first appearance in the Compustat database. Source: Compustat.

ALTMAN_Z: 1.2 (Net working capital/Total assets) + 1.4 (Retained earnings/Total assets) + 3.3 (Earnings before interest and taxes/Total Assets) + 0.6 (Market value of equity/Book value of liabilities) + 1.0 (Sales/Total assets). Source: Compustat.

CASH_FLOW_VOLATILITY: Ratio of standard deviation of quarterly net operating cash flows over 16 quarters before the loan-initiation year to book value of assets from the year preceding loan initiation. Source: Compustat.

SPECULATIVE_RATING: A dummy variable that equals 1 if the firm has speculative grade (lower than BBB−) in S&P long-term credit rating and 0 otherwise. Source: Compustat.

NOT_RATED: A dummy variable that equals 1 if the firm has an S&P long-term credit rating and 0 otherwise. Source: Compustat.

CASH_FLOW: Ratio of (income before extraordinary items + depreciation and amortization) to total asset. Source: Compustat.

INVESTMENT: Ratio of capital expenditures to total asset. Source: Compustat.

ΔCASH: Difference between cash holdings (scaled by total asset) and lagged cash holdings (scaled by total assets). Source: Compustat.

NUMBER_OF_ANALYSTS: Number of analysts following the firm during the previous year. Source: IBES.

NEW_CEO: A dummy variable that equals 1 if the CEO of the firm is different from the most recent CEO between 1996 and 2002 and 0 otherwise. If the CEO retired (replaced at the age older than 65 or identified as retired in Execucomp) or deceased (identified as dead in Execucomp), the variable equals 0. Source: Execucomp.

CEO_DELTA: Dollar change in CEO wealth associated with a 1% increase of the firm's stock price. Source: Execucomp.

CEO_VEGA: Dollar change in CEO wealth associated with a 1% increase of the standard deviation of the firm's stock price returns. Source: Execucomp.

FOUNDER_CEO: A dummy variable that equals 1 if the CEO of the firm was appointed at least 1 year before the firm's first Compustat year, following Jenter and Lewellen (2015). Source: Execucomp, Compustat.

INSTITUTIONAL_BLOCKHOLDER: A dummy variable that equals 1 if there is any institutional investor holding more than 5% of the firm's stock and 0 otherwise. Source: CDA/Spectrum.

INSTITUTIONAL_OWNERSHIP: Ratio of the number of shares held by institutional investors to the number of shares outstanding. Source: CDA/Spectrum.

GOVERNANCE_SCORE: Governance score based on 24 governance standards. See Chung, Elder, and Kim (2010). Source: Institutional Shareholder Services (ISS).

Bond Variables

BOND_MATURITY: Maturity of the bond measured in months. Source: SDC Platinum.

CALLABLE_BOND: A dummy variable that equals 1 if the bond is callable and zero otherwise. Source: SDC Platinum.

PUTTABLE_BOND: A dummy variable that equals 1 if the bond is puttable and 0 otherwise. Source: SDC Platinum.

SUBORDINATED_BOND: A dummy variable that equals 1 if the bond is subordinated bond and 0 otherwise. Source: SDC Platinum.

PRIOR_BOND_ISSUANCE: A dummy variable that equals 1 if the firm had issued a bond and 0 otherwise. Source: SDC Platinum.

BOND_RATING: The S&P bond rating at issuance. Source: SDC Platinum.

PUBLIC_DEBT/TOTAL_DEBT: Ratio of (aggregate volume of newly issued bonds in each year) to (aggregate volume of newly issued bonds in each year + aggregate volume of newly issued loans in each year): Source: SDC Platinum, Dealscan.

Choice Between Public and Private Debt

PUBLIC_DEBT: A dummy variable that equals 1 if the debt issued by the firm is public debt and 0 otherwise. Source: SDC Platinum, Dealscan.

Macroeconomic Controls

CREDIT_SPREAD: The difference between the yield of AAA corporate bonds and that of BAA corporate bonds. Source: Federal reserve board of governors.

TERM_SPREAD: The difference between the yield of 10-year Treasury bonds and that of 2-year Treasury bonds. Source: Federal reserve board of governors.

FINANCIAL_CRISIS: A dummy variable that equals 1 if the debt was issued between July 2007 and Mar. 2009 and 0 otherwise. Source: SDC Platinum, Dealscan.

Supplementary Material

Supplementary Material for this article is available at https://doi.org/10.1017/S0022109022000205.

References

Aboody, D., and R. Kasznik. "CEO Stock Option Awards and the Timing of Corporate Voluntary Disclosures." *Journal of Accounting and Economics*, 29 (2000), 73–100.

Allayannis, G., and A. Mozumdar. "The Impact of Negative Cash Flow and Influential Observations on Investment-Cash Flow Sensitivity Estimates." *Journal of Banking and Finance*, 28 (2004), 901–930.

Almeida, H.; M. Campello; and M. Weisbach. "The Cash Flow Sensitivity of Cash." *Journal of Finance*, 59 (2004), 1777–1804.

Amel-Zadeh, A., and Y. Zhang. "The Economic Consequences of Financial Restatements: Evidence from the Market for Corporate Control." *Accounting Review*, 90 (2015), 1–29.

Armstrong, C. S., and D. F. Larcker. "Discussion of 'The Impact of the Options Backdating Scandal on Shareholders' and 'Taxes and the Backdating of Stock Option Exercise Dates'." *Journal of Accounting and Economics*, 47 (2009), 50–58.

Arrow, K. J. "Gifts and Exchanges." *Philosophy and Public Affairs*, 1 (1972), 343–362.

Bamber, L. S.; X. Jiang; and I. Y. Wang. "What's My Style? The Influence of Top Managers on Voluntary Corporate Financial Disclosure." *Accounting Review*, 85 (2010), 1131–1162.

Bebchuk, L.; Y. Grinstein; and U. Peyer. "Lucky CEOs and Lucky Directors." *Journal of Finance*, 65 (2010), 2263–2401.

Ben-David, I.; F. Franzoni; and R. Moussawi. "Hedge Fund Stock Trading in the Financial Crisis of 2007–2009." *Review of Financial Studies*, 25 (2012), 1–54.

Benmelech, E., and C. Frydman. "Military CEOs." *Journal of Financial Economics*, 117 (2015), 43–59.

Berlin, M., and J. Loeys. "Bond Covenants and Delegated Monitoring." *Journal of Finance*, 43 (1988), 397–412.

Bernile, G., and G. Jarrell. "The Impact of the Options Backdating Scandal on Shareholders." *Journal of Accounting and Economics*, 47 (2009), 2–26.

Bessembinder, H.; K. M. Kahle; W. F. Maxwell; and D. Xu. "Measuring Abnormal Bond Performance." *Review of Financial Studies*, 22 (2008), 4219–4258.

Bharath, S.; S. Dahiya; A. Saunders; and A. Srinivasan. "Lending Relationships and Loan Contract Terms." *Review of Financial Studies*, 24 (2011), 1141–1203.

Bharath, S.; J. Sunder; and S. Sunder. "Accounting Quality and Debt Contracting." *Accounting Review*, 83 (2008), 1–28.

Biggerstaff, L.; C. D. Cicero; and A. Puckett. "Unethical Culture, Suspect CEOs and Corporate Misbehavior." *Journal of Financial Economics*, 117 (2015), 98–121.

Bizjak, J.; M. Lemmon; and R. Whitby. "Option Backdating and Board Interlocks." *Review of Financial Studies*, 22 (2009), 4821–4847.

Bodnaruk, A., and M. Rossi. "Dual Ownership, Returns, and Voting in Mergers." *Journal of Financial Economics*, 120 (2016), 58–80.

Bolton, P.; X. Freixas; and J. Shapiro. "The Credit Ratings Game." *Journal of Finance*, 67 (2012), 85–111.

Boot, A. W. A.; T. T. Milbourn; and A. Schmeits. "Credit Ratings as Coordination Mechanisms." *Review of Financial Studies*, 19 (2006), 81–118.

Bottazzi, L.; M. Da Rin; and T. Hellman. "The Importance of Trust for Investment: Evidence from Venture Capital." *Review of Financial Studies*, 29 (2016), 2283–2318.

Boyd, J. H., and E. C. Prescott. "Financial Intermediary-Coalitions." *Journal of Economic Theory*, 38 (1986), 211–232.

Campbell, T. S., and W. A. Kracaw. "Information Production, Market Signaling, and the Theory of Financial Intermediation." *Journal of Finance*, 35 (1980), 863–882.

Carow, K.; R. Heron; E. Lie; and R. Neal. "Option Grant Backdating Investigations and Capital Market Discipline." *Journal of Corporate Finance*, 15 (2009), 562–672.

Carver, B. T.; B. N. Cline; and M. L. Hoag. "Underperformance of Founder-Led Firms: An Examination of Compensation Contracting Theories during the Executive Stock Options Backdating Scandal." *Journal of Corporate Finance*, 23 (2013), 294–310.

Chakravarthy, J.; E. deHaan; and S. Rajgopal. "Reputation Repair After a Serious Restatement." *Accounting Review*, 89 (2014), 1329–1363.

Chauvin, K. W., and C. Shenoy. "Stock Price Decreases Prior to Executive Stock Option Grants." *Journal of Corporate Finance*, 7 (2001), 53–76.

Chava, S., and M. Roberts. "How Does Financing Impact Investment? The Role of Debt Covenants." *Journal of Finance*, 63 (2008), 2085–2121.

Chen, P. C. "Banks' Acquisition of Private Information about Financial Misreporting." *Accounting Review*, 91 (2016), 835–857.

Chung, K. H.; J. Elder; and J. Kim. "Corporate Governance and Liquidity." *Journal of Financial and Quantitative Analysis*, 45 (2010), 265–291.

Chung, K. H., and H. Zhang. "Corporate Governance and Institutional Ownership." *Journal of Financial and Quantitative Analysis*, 46 (2011), 247–273.

Cline, B. N.; R. A. Walking; and A. S. Yore. "The Consequences of Managerial Indiscretions: Sex, Lies, and Firm Value." *Journal of Financial Economics*, 127 (2018), 389–415.

Coles, J. L.; N. D. Daniel; and L. Naveen. "Managerial Incentives and Risk-Taking." *Journal of Financial Economics*, 79 (2006), 431–468.

Collins, D. W.; G. Gong; and H. Li. "Corporate Governance and Backdating of Executive Stock Options." *Contemporary Accounting Research*, 26 (2009), 403–445.

Core, J., and W. Guay. "Estimating the Value of Employee Stock Option Portfolios and Their Sensitivities to Price and Volatility." *Journal of Accounting Research*, 40 (2002), 613–630.

Cornaggia, K. R.; J. Hund; and G. Nguyen. "Investor Attention and Municipal Bond Returns." Working Paper, Pennsylvania State University and University of Georgia (2018).

Denis, D. J., and V. T. Mihov. "The Choice among Bank Debt, Non-bank Private Debt, and Public Debt: Evidence from New Corporate Borrowings." *Journal of Financial Economics*, 70 (2003), 3–28.

Diamond, D. W. "Financial Intermediation and Delegated Monitoring." *Review of Economic Studies*, 51 (1984), 393–414.

Diamond, D. W. "Monitoring and Reputation: The Choice between Bank Loans and Directly Placed Debt." *Journal of Political Economy*, 99 (1991), 689–721.

Duarte, J.; S. Siegel; and L. Young. "Trust and Credit: The Role of Appearance in Peer-to-Peer Lending." *Review of Financial Studies*, 25 (2012), 2455–2484.

Engelberg, J.; P. Gao; and C. A. Parsons. "Friends with Money." *Journal of Financial Economics*, 103 (2012), 169–188.

Erel, I.; Y. Jang; and M. S. Weisbach. "Do Acquisitions Relieve Target Firms' Financial Constraints?" *Journal of Finance*, 70 (2015), 289–328.

Fama, E. F. "What's Different about Banks?" *Journal of Monetary Economics*, 15 (1985), 29–39.

Fazzari, S.; G. Hubbard; and B. Petersen. "Financing Constraints and Corporate Investment." *Brookings Papers on Economic Activity*, 1 (1988), 141–206.

Forelle, C., and J. Bandler. "The Perfect Payday." *The Wall Street Journal,* available at https://www.wsj.com/articles/SB114265075068802118 (2006).

Gambetta, D. "Can We Trust Trust?" In *Trust: Making and Breaking Cooperative Relations*, D. Gambetta, ed. New York, NY: Basil Blackwell (1988), 213–237.

Gertner, R., and D. Scharfstein. "A Theory of Workouts and the Effects of Reorganization Law." *Journal of Finance*, 46 (1991), 1189–1222.

Giannetti, M., and T. Y. Wang. "Corporate Scandals and Household Stock Market Participation." *Journal of Finance*, 71 (2016), 2591–2636.

Gompers, P. A.; J. L. Ishii; and A. Metrick. "Corporate Governance and Equity Prices." *Quarterly Journal of Economics*, 118 (2003), 107–156.

Graham, J. R.; S. Li; and J. Qiu. "Corporate Misreporting and Bank Loan Contracting." *Journal of Financial Economics*, 89 (2008), 44–61.

Grullon, G.; G. Kanatas; and J. Weston. "Religion and Corporate (Mis)behavior." Working Paper, Rice University (2010).

Guay, W. "The Sensitivity of CEO Wealth to Equity Risk: An Analysis of the Magnitude and Determinants." *Journal of Financial Economics*, 53 (1999), 43–71.

Guiso, L.; P. Sapienza; and L. Zingales. "The Role of Social Capital in Financial Development." *American Economic Review*, 94 (2004), 526–556.

Guiso, L.; P. Sapienza; and L. Zingales. "Trusting the Stock Market." *Journal of Finance*, 63 (2008), 2557–2600.

Guiso, L.; P. Sapienza; and L. Zingales. "Cultural Biases in Economic Exchange." *Quarterly Journal of Economics*, 124 (2009), 1095–1131.

Guiso, L.; P. Sapienza; and L. Zingales. "The Value of Corporate Culture." *Journal of Financial Economics*, 117 (2015), 60–76.

Guner, A. B.; U. Malmendier; and G. Tate. "Financial Expertise of Directors." *Journal of Financial Economics*, 88 (2008), 323–354.

Gurun, U.; N. Stoffman; and S. E. Yonker. "Trust Busting: The Effect of Fraud on Investor Behavior." *Review of Financial Studies*, 31 (2018), 1341–1376.

Hambrick, D. "Upper Echelons Theory: An Update." *Academy of Management Review*, 32 (2007), 334–343.

Hambrick, D., and P. Mason. "Upper Echelons: The Organization as a Reflection of Its Top Managers." *Academy of Management Review*, 9 (1984), 193–206.

Hasan, I.; C. K. Hoi; Q. Wu; and H. Zhang. "Social Capital and Debt Contracting: Evidence from Bank Loans and Public Bonds." *Journal of Financial and Quantitative Analysis*, 52 (2017), 1017–1047.

Heron, R. A., and E. Lie. "Does Backdating Explain the Stock Price Pattern around Executive Stock Option Grants?" *Journal of Financial Economics*, 83 (2007), 271–295.

Heron, R. A., and E. Lie. "What Fraction of Stock Option Grants to Top Executives Have Been Backdated or Manipulated?" *Management Science*, 55 (2009), 513–525.

Hirshleifer, D., and S. H. Teoh. "Limited Attention, Information Disclosure, and Financial Reporting." *Journal of Accounting and Economics*, 36 (2003), 337–386.

Houston, J., and C. James. "Bank Information Monopolies and the Mix of Private and Public Debt Claims." *Journal of Finance*, 51 (1996), 1863–1889.

James, C., and D. C. Smith. "Are Banks Still Special? New Evidence on Their Role in the Corporate Capital-Raising Process." *Journal of Applied Corporate Finance*, 13 (2000), 52–63.

Janney, J. J., and S. Gove. "Do Executive Departures to Signal the End of a Scandal Create or Reduce Uncertainty? An Examination of Market Reaction in Stock Option Backdating Scandal Events." *Business & Society*, 58 (2019), 1209–1233.

Jenter, D., and K. Lewellen. "CEO Preferences and Acquisitions." *Journal of Finance*, 70 (2015), 2813–2852.

Karpoff, J. M.; D. S. Lee; and G. S. Martin. "The Cost to Firms of Cooking the Books." *Journal of Financial and Quantitative Analysis*, 43 (2008), 581–611.

Khandani, A. E., and A. W. Lo. "What Happened to the Quants in August 2007? Evidence from Factors and Transactions Data." *Journal of Financial Markets*, 14 (2011), 1–46.

Knack, S., and P. Keefer. "Does Social Capital have an Economic Payoff? A Cross-Country Investigation." *Quarterly Journal of Economics*, 112 (1997), 1251–1288.

La Porta, R.; F. Lopez-de-Silanes; A. Shleifer; and R. Vishny. "Trust in Large Organizations." *American Economic Review*, 87 (1997), 333–338.

Leland, H. E., and D. H. Pyle. "Informational Asymmetries, Financial Structure, and Financial Intermediation." *Journal of Finance*, 32 (1977), 371–387.

Lewicki, R. J.; D. J. McCallister; and R. J. Bies. "Trust and Distrust: New Relationships and Realities." *Academy of Management Review*, 23 (1998), 438–458.

Li, J. "Endogenous Investor Inattention and Price Underreaction to Information." Working Paper, University of Utah (2018).

Lie, E. "On the Timing of CEO Stock Option Awards." *Management Science*, 51 (2005), 802–812.

Lins, K. V.; H. Servaes; and A. Tamayo. "Social Capital, Trust, and Firm Performance: The Value of Corporate Social Responsibility during the Financial Crisis." *Journal of Finance*, 72 (2017), 1785–1824.

Liu, X. "Corruption Culture and Corporate Misconduct." *Journal of Financial Economics*, 122 (2016), 307–327.

Moro, A., and M. Fink. "Loan Managers' Trust and Credit Access for SMEs." *Journal of Banking and Finance*, 37 (2013), 927–936.

Pan, Y.; S. Siegel; and T. Y. Wang. "The Cultural Origin of Preferences: CEO Cultural Heritage and Corporate Investment." Working Paper, University of Utah, University of Washington, and University of Minnesota (2016).

Pan, Y.; T. Y. Wang; and M. S. Weisbach. "How Management Risk Affects Corporate Debt." *Review of Financial Studies*, 31 (2018), 3491–3531.

Park, C. "Monitoring and Structure of Debt Contracts." *Journal of Finance*, 5 (2000), 2157–2195.

Pevzner, M.; F. Xie; and X. Xin. "When Firms Talk, Do Investors Listen? The Role of Trust in Stock Market Reactions to Corporate Earnings Announcements." *Journal of Financial Economics*, 117 (2015), 190–223.

Rajan, R. G. "Insiders and Outsiders: The Choice Between Informed and Arm's Length Debt." *Journal of Finance*, 47 (1992), 1367–1400.

Sacconi, L., and G. D. Antoni. *Social Capital, Corporate Responsibility, Economic Behaviour and Performance*, L. Sacconi and G. D. Antoni, eds. New York, NY: Palgrave MacMillan (2011).

Stiglitz, J., and A. Weiss. "Credit Rationing in Markets with Imperfect Information." *American Economic Review*, 71 (1981), 393–410.

Yermack, D. "Good Timing: CEO Stock Option Awards and Company News Announcements." *Journal of Finance*, 52 (1997), 449–476.

Zak, P., and S. Knack. "Trust and Growth." *Economic Journal*, 111 (2001), 295–321.