Economic Determinants and Consequences of Corporate Disclosures

by

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Professor Panos N. Patatoukas, Chair

Professor Omri Even-Tov, Co-Chair

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Professor Steven Davidoff Solomon

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Abstract

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Corporate disclosures are important means for managers to communicate firm performance and are essential information sources for investors. This dissertation examines corporate disclosures from two distinct angles: economic determinants and consequences of corporate disclosures. I investigate these two topics separately in Chapters 1 and 2.

In Chapter 1, The Jobs Act Did Not Raise IPO Underpricing, my co-authors and I examine how the amount of pre-IPO disclosures affects IPO pricing. While the intended goal of the 2012 JOBS Act was to ease access to capital for Emerging Growth Companies (EGCs), prior studies, notably Barth et al. (2017), find evidence of an increase in IPO underpricing and a higher cost of equity capital for EGC issuers. Using a difference-in-differences design, we find that changes in overall IPO market conditions explain the seeming increase in IPO underpricing. In fact, EGC issuers that take advantage of the accounting disclosure relief afforded by the Act raise capital at higher pre-IPO multiples. These reduced-accounting disclosure EGCs have more speculative valuation profiles and lower institutional ownership and are more likely to destroy long-term shareholder value in the IPO aftermarket. Overall, this chapter offers an alternative perspective on the effect of the JOBS Act on IPO pricing.

In Chapter 2, I examine whether and how the recognition of operating leases on balance sheets influences managerial leasing decisions. In 2019 the FASB implemented ASC 842, which requires companies to capitalize operating leases. Given prior operating lease disclosure in footnotes, it was unclear if capitalization would affect financial statement users or managers. I find that firms' use of operating leases decreases upon the new standard's adoption and that this decline is driven by lease-intensive and less-levered firms. I also document a systematic substitution of operating leases with short-term and variable leases, both of which can still be left off balance sheet. The latter finding suggests that the FASB has not entirely succeeded in preventing firms from opportunistically omitting liabilities from balance sheets. My study responds directly to the FASB's call for research that examines the new standard's unintended consequences on managerial leasing decisions. Overall, I provide evidence that disclosure location (footnote vs. balance sheet) exerts a real effect on managerial behavior.

To my family

Contents

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Chapter 1

The Jobs Act Did Not Raise IPO Underpricing

Co-Authored with Omri Even-Tov[1] and Panos N. Patatoukas[2]

1.1. Introduction

What is the effect of the Jumpstart Our Business Startups (JOBS) Act on IPO pricing? The JOBS Act was signed into law on April 5, 2012, with the objective to improve access to the public capital market for growth companies and catalyze U.S. job creation and economic growth. Title I of the JOBS Act amended the Securities Act and the Exchange Act and has been widely recognized as the most significant relaxation of securities regulation in decades.

Title I of the JOBS Act allows Emerging Growth Companies (EGCs)— issuers with pre-IPO revenues of less than $1 billion (BN)—a set of provisions designed to "de-risk" and "de-burden" their IPO process. The de-risking provisions allow EGCs the choice to confidentially file a draft registration statement and to test the waters by engaging in private communications with certain institutional investors prior to the public disclosure of the registration statement. The de-risking provisions are intended to enhance the ability to conduct a successful registered offering and to facilitate capital formation at a lower cost. The de-burdening provisions allow EGCs to scale back financial accounting and executive compensation disclosures in their IPO filings, delay auditor attestation on internal controls pursuant to Section 404(b) of the Sarbanes-Oxley (SOX) Act, and to adopt new or revised GAAP standards using private company effective dates.

Whereas the intended goal of the JOBS Act was to ease access to capital for growth companies, prior research finds evidence of higher IPO underpricing for EGC issuers. To illustrate, Barth et al. (2017) compare post-JOBS Act EGC issuers to pre-JOBS Act issuers below the $1BN revenue cutoff that would have qualified for EGC status had the Act been in effect at the time of their IPO. Their pre-post JOBS Act comparison shows a larger jump of the aftermarket price relative to the offer price, which they interpret as evidence of an increase in IPO underpricing and a higher cost of equity capital for EGC issuers. Chaplinsky et al. (2017) separate Smaller Reporting Companies (SRCs), which they define as issuers with a public float below $75MN, from the general population of EGCs and find consistent evidence of a larger IPO jump for EGC issuers post-JOBS Act. Other related studies find similar evidence (e.g., Gupta and Israelsen 2016; Agarwal et al. 2017).

Prior research interprets evidence of an increase in the IPO underpricing of EGC issuers as an outcome that is attributable to the JOBS Act rather than contemporaneous changes in market conditions. With respect to the effect of the JOBS Act on IPO underpricing, the running hypothesis is that valuation uncertainty is more pronounced for EGC issuers post-JOBS Act, especially for

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those who adopt more of the Title I disclosure relief provisions. Greater valuation uncertainty leads to heavier pre-market discounting by underwriters and translates into higher cost of equity for EGC issuers. Within this context, the price jump in the immediate aftermarket relative to the offer price multiplied by the number of shares offered in the IPO is risk compensation accruing to the IPO capital providers and, at the same time, money left on the table for issuers. This interpretation presumes that the immediate aftermarket price is an unbiased estimate of fundamental value.

The prior evidence of an increase in the cost of equity capital for EGC issuers poses a major conundrum because it implies that the 2012 JOBS Act has not achieved its intended goal of easing access to capital for growth companies. Adding to the conundrum, the evidence appears to be at odds with key empirical facts. First, relative to the depressed level of IPO activity pre-JOBS Act, there was a marked increase in IPO issuance activity post-JOBS Act especially among EGCs (e.g., Dambra et al. 2015). Second, most eligible issuers voluntarily chose to adopt the EGC status. Indeed, we find only a few instances where issuers were eligible for EGC status but did not choose to adopt this status. Third, EGCs have elected to avail themselves of the disclosure reliefs afforded by Title I of the Act at an increasing rate. The evidence is especially perplexing when considering the cost savings from reduced disclosures and the deferral of the SOX internal control audit requirement. Chaplinsky et al. (2017) highlight that the potential cost savings are not significant enough to offset the additional cost associated with higher IPO underpricing, which they estimate at $21MN worth of money left on the table for the average issuer.

So, what could explain the conundrum? We observe that the post-JOBS Act period overlaps with the longest-ever bull market in U.S. history. This observation is relevant for two reasons. First, there is long-standing evidence that companies either choose to delay their IPOs until a bull market or choose to go public in response to favorable market conditions (e.g., Ritter and Welch 2002). Second, it is known that IPO returns are cyclical and display peaks and troughs that are highly correlated with IPO volume and prevailing market conditions (e.g., Ritter 1991; Baker and Wurgler 2006; Yung et al. 2008). Third, there is evidence dating back to Ritter (1991) that positive first-day returns tend to be followed by negative long-run returns for new issuers, which is consistent with overpricing in the immediate IPO aftermarket. Ritter (1991) points out that "*…Firms choose to go public when investors are willing to pay high multiples…reflecting optimistic assessments of the net present value of growth opportunities. The negative aftermarket performance…is due to disappointing realizations of the subsequent net cash flows*".

In this chapter, we hypothesize that contemporaneous changes in overall IPO market conditions contribute to the seeming increase in the IPO underpricing of EGC issuers post-JOBS Act. To separate the effect of the JOBS Act from contemporaneous changes in overall IPO market conditions, we implement a difference-in-differences (DID) research design. The DID zeroes in on the differential pre-post JOBS Act change between the treatment group of EGC issuers and a control group of unaffected issuers. As the control group, we use large issuers with pre-IPO revenues over $1BN. This research design controls for intertemporal changes in overall IPO market conditions that are common across the treatment group of EGC issuers and the control group of large issuers. The idea behind using large issuers as the control group is simple. Large issuers were not eligible for EGC status and were not affected by any of the Title I provisions afforded to EGCs under the JOBS Act until the second half of 2017, when the confidential filing was extended to all issuers (SEC Announcement, June 29, 2017). Therefore, the pre-post JOBS Act comparison of large issuers offers a "placebo" test of the effect of the JOBS Act on IPO pricing. Even though the JOBS Act had no bearing on large issuers above the $1BN revenue cutoff, both

EGC and large issuers were affected by contemporaneous changes in overall IPO market conditions.

The treatment group consists of 202 issuers that went public from the beginning of 2009 to April 4, 2012, that would have qualified for EGC status had the Act been in effect at the time of their IPO and 380 EGC issuers post-JOBS Act that went public between April 5, 2012, and the end of 2015. To identify a consistent treatment group, we exclude smaller reporting companies (SRCs) from the general population of EGC issuers. This is because effective February 4, 2008, more than four years before the JOBS Act was signed into law, SRC issuers already qualified for several de-burdening provisions. These provisions were similar to those afforded by Title I of the JOBS Act, including the reduced accounting and executive compensation disclosure provisions (SEC Release, No. 33-8876). Over our sample period, SRCs were also exempt from providing an auditor attestation on the effectiveness of internal controls pursuant to Section 404(b) of the SOX Act as non-accelerated filers.

The control group of large issuers consists of 39 large issuers that went public from the beginning of 2009 to April 4, 2012, and 56 large issuers post-JOBS Act that went public by the end of 2015. While large issuers account for only 14% of the IPO volume between 2009 and 2015, they account for as much as 48% of the aggregate IPO proceeds and 46% of the aggregate IPO value. Large issuers also account for as much as 85% of aggregate revenues and 79% of aggregate employment across all IPOs between 2009 and 2015. These statistics highlight the economic importance of the control group of large issuers in the U.S. IPO market.

The DID estimator captures the difference of the pre-versus-post trends in the treatment group of issuers below the $1BN revenue cutoff relative to the control group of large issuers above the $1BN revenue cutoff. We acknowledge that a natural control group of perfectly comparable but unaffected issuers does not exist, which makes it impossible to design the perfect DID. This shortcoming, however, does not invalidate the choice of large issuers as a control group that was unaffected by the Title I provisions afforded by the JOBS Act. The DID estimator differences away permanent differences in outcomes between the treatment group of EGC issuers and the control group of large issuers as well as any common trend affecting both issuer groups. In addition, our regression model specifications include a wide array of issuer characteristics and fixed effects to control for known cross-sectional determinants of IPO pricing.

In the first set of results, we examine the effect of the JOBS Act on IPO aftermarket returns. Focusing on the treatment group of EGCs, the pre-post JOBS Act comparison shows that first-day returns increased by 6.7 percentage points from 13.5% in the pre-JOBS Act period to 20.2% in the post-JOBS Act period. Our pre-post JOBS Act comparison of EGCs hews closely to prior studies. Turning to the control group of large issuers, we find a placebo effect of similar magnitude. The first-day returns of large issuers increased by 6.6 percentage points from 6.4% in the pre-JOBS Act period to 13.0% in the post-JOBS Act period. This placebo effect cannot be attributed to the JOBS Act because large issuers are not eligible for EGC status and were not affected by any of the Title I provisions afforded to EGC issuers. The DID regression results show that the differential pre-post change between the treatment group of EGCs vis-à-vis the control group of large issuers is indistinguishable from zero. Put differently, while we observe an increase in IPO returns for both the treatment and control groups in the post-JOBS act period, the differences between the two groups are the same pre-versus-post JOBS Act. This parallel trend is what one would expect in the absence of a treatment effect due to the passage of the JOBS Act. Put differently, the seeming increase in the IPO underpricing of EGCs is attributable to contemporaneous changes in overall IPO market conditions rather than the passage of the JOBS Act.

In the second set of results, we examine the effect of the JOBS Act on pre-IPO valuation multiples. Valuation multiples are widely used in practice when pricing IPOs (e.g., Kim and Ritter 1999; Guo et al. 2005). This analysis offers a direct test of changes in pre-market discounting by underwriters. The idea is simple. Focusing on EGC issuers after the passage of the JOBS Act, an increase in IPO underpricing would imply heavier pre-IPO discounting and, therefore, a decrease in pre-IPO valuation multiples relative to the pre-JOBS Act period. Our results do not support this notion. In fact, the pre-post comparison shows an expansion rather than a contraction of the pre-IPO revenue multiples for the treatment group of EGC issuers. While the pre-post JOBS Act comparison for the control group of large issuers also shows an expansion of their multiples, the DID regression results show an expansion in the multiples of EGC issuers. Together, the evidence shows that EGC issuers raise capital at higher pre-IPO valuation multiples, which challenges the idea that EGCs leave more money on the table due to higher IPO underpricing.

To shed light on the origins of this pre-IPO valuation premium, we exploit heterogeneity in the use of Title I provisions. We find that EGCs have increasingly taken an à la carte approach to adopting most provisions. By the end of 2015, 96% of EGCs used the testing-the-waters provision and 95% of EGCs chose to file their draft registration statements confidentially. Virtually all EGCs that took advantage of the de-risking provisions also availed themselves of the reduced compensation disclosure provision and delayed auditor attestation on internal controls. Furthermore, between 2012 and 2015, most EGCs opted out of the provision to adopt new or revised accounting standards using private company effective dates (delay GAAP provision).[3] In our sample, we find that the main source of heterogeneity in the use of Title I provisions is the choice to present only two years of audited financial statements and two years of selected financial data in the IPO filing, rather than the previously required three years of audited financial statements and five years of selected financial data. By the end of 2015, we observe that 48% of EGCs had an operating history of more than two years and elected to take advantage of the reduced-accounting disclosure provision. Given the limited variation in the use of other Title I provisions, we zero in on variation in the use of the reduced-accounting disclosure provision.

Our cross-sectional tests provide evidence that the pre-IPO valuation premium is concentrated in reduced-accounting EGC issuers. What could explain the pre-IPO valuation premium of reduced-accounting EGC issuers? If the pre-IPO valuation premium reflects overpricing, we should observe that reduced-accounting EGCs are associated with a higher probability of long-term underperformance. Our evidence supports this prediction. Building on Bessembinder (2018), we construct an indicator of long-term value destruction for IPOs that underperform the stock market index in the three years after going public. Our evidence shows that reduced-accounting EGCs are nearly 1.4 times more likely to destroy long-term shareholder value relative to non-reduced accounting EGC issuers.

Our reconstruction of the typical issuer profile shows that reduced-accounting EGCs are smaller, more R&D-intensive, and are significantly more likely to have a history of losses relative to non-reduced accounting EGC issuers. They also have lower institutional ownership, which

[3] The average number of JOBS Act provisions used by EGC issuers has increased from 4.1 in 2012 to 5.3 in 2015, with as much as 97% of the pooled sample of EGC issuers using at least four provisions. We provide a detailed discussion of the use of Title I provisions in Section 1.3.3.

indicates higher individual investor ownership. These characteristics are associated with more speculative valuation profiles and a higher tendency for overpricing (e.g., Purnanandam and Swaminathan 2004; Field and Lowry 2009; Aboody et al. 2018; Patatoukas et al. 2021). The cross-sectional differences across reduced-accounting and non-reduced accounting EGCs imply that it is not the reduced-accounting provision choice *per se* leading to more prevalent overpricing but rather the fundamental characteristics of EGCs issuers that choose to scale back their financial accounting disclosures.

This chapter contributes to research on the effect of the JOBS Act on IPO pricing. Relative to prior work, we provide evidence that changes in overall IPO market conditions coincident with the passage of the JOBS Act explain the seeming increase in the IPO underpricing of EGC issuers. In contrast to prior evidence that EGCs leave more money on the table due to higher pre-market discounting by underwriters, we find that EGCs raise capital at significantly higher pre-IPO valuation multiples even though they have more speculative valuation profiles and are more likely to destroy long-term shareholder value for IPO aftermarket investors. A relevant implication is that inferences regarding the effect of the JOBS Act are confounded by contemporaneous changes in market conditions. In this sense, this chapter relates to Leuz and Wysocki's (2016) assessment that regulatory studies are often confounded by "*overall time trends that are concurrent with the regulatory change*".

With respect to prior studies on the effect of the JOBS Act on IPO pricing, we conclude that caution is warranted when interpreting evidence as a causal outcome of the regulatory change. Different from Barth's et al. (2017) focus on the pre-versus-post comparison of issuers below the $1BN revenue cutoff, our DID research design controls for intertemporal changes in overall IPO market conditions that are common across the treatment group of EGC issuers and the control group of large issuers. And different from Chaplinsky's et al. (2017) focus on the pre-post comparison of SRC and non-SRC EGCs, we identify large issuers as a control group that are not eligible for EGC status and were not affected by any of the Title I provisions afforded by the JOBS Act. Indeed, even though SRCs already qualified for several de-burdening provisions before the passage of the JOBS Act, they were afforded Title I de-risking provisions of testing the waters and confidential filing as well as the delay GAAP de-burdening provision only after the passage of the JOBS Act.

The JOBS Act was an ambitious piece of legislation and more research is warranted on its effects. This chapter offers an alternative perspective on the effect of the JOBS Act on IPO pricing and investor protection in the IPO aftermarket. Our evidence of confounding time trends in the IPO market echoes long-standing evidence on the cyclical nature of IPO pricing and the tendency of growth IPO stocks to become overpriced in the immediate aftermarket, especially those going public during hot IPO markets (e.g., Ritter 1991). Our evidence that individual investors may have been disproportionately exposed to shareholder value destruction post-JOBS Act could inform the SEC's efforts to facilitate capital formation while protecting the interests of Main Street investors. This study is particularly timely in light of the SEC's rule allowing all new issuers, including EGC and non-EGC issuers, to test the waters with certain institutional investors prior to filing a registration statement (SEC Release, No. 33-10699 effective December 3, 2019).[4]

[4] We provide a more detailed discussion of the SEC's new rule in Section 1.2.2.

1.2. Background

1.2.1. The JOBS Act

At the U.S. Treasury Department's Access to Capital Conference on March 22, 2011, Treasury Secretary Tim Geithner postulated that "*The financial crisis caused a great deal of damage to the capacity of innovators to access the capital markets*". Following the conference, an IPO Task Force was formed to study the relationship between IPO volume and job growth. Composed of a group of venture capitalists, entrepreneurs, investors, investment bankers, academics, and former government officials, the IPO Task Force concluded that the decline in the number of IPOs in recent years had resulted in considerable job loss and damage to the U.S. economy (e.g., Latham & Watkins 2013). To help spur U.S. job creation and economic growth the Jumpstart Our Business Startups (JOBS) Act was signed into law on April 5, 2012. The JOBS Act was intended to revitalize the U.S. economy by making it easier for growth companies to raise capital. Title I of the JOBS Act used the IPO Task Force's report to guide the implementation of an IPO "on-ramp" to smooth the transition from private to public corporate status.

Title I of the JOBS Act created a new category of issuers called Emerging Growth Companies (EGCs). These companies are eligible for a reduction in various regulatory, disclosure, and compliance requirements if their annual revenues are less than $1BN in the most recent complete fiscal year and if, as of December 8, 2011, they have not sold common equity under a registration statement. The revenue cutoff is amended every five years to account for inflation. As of April 12, 2017, the $1BN revenue cutoff was raised to $1.07BN.

The EGC status is temporary and expires five years after the IPO date or when any of the following three scenarios occur: (a) annual revenues exceed the $1BN cutoff, (b) the company has more than $1BN in non-convertible debt issuances within the past three years, or (c) the company becomes a large-accelerated filer, defined as a company with an aggregate market value of common equity held by its non-affiliates of $700MN or more. The EGC status cannot be regained once it has been lost. Certain regulatory requirements, such as obtaining auditor attestation on internal controls, are phased in during the five-year IPO on-ramp period, unless the company loses its status earlier by exceeding the EGC thresholds.

1.2.2. Title I provisions

Title I of the JOBS Act allows EGC issuers a set of provisions designed to de-risk and to de-burden the IPO process. Appendix 1 summarizes the de-risking and the de-burdening provisions. EGC issuers can choose to use all, some, or none of the Title I provisions during their IPO on-ramp period.

The de-risking provisions include confidential filing of the IPO draft registration statement and testing-the-waters communications. The confidential filing provision allows issuers to submit a draft of their IPO registration statement to the SEC for confidential review as long as the initial confidential submission and all amendments are publicly filed with the SEC no later than 15 days before the start of the issuer's IPO roadshow. The testing-the-waters provision allows issuers to assess investor interest in a proposed offering either before or after filing a registration statement. Under this provision, issuers can communicate directly with potential investors that are qualified institutional buyers or institutions that are accredited investors prior to the registration statement's public disclosure.

The de-burdening provisions include a reduced financial statement disclosure provision, which allows companies to report in their IPO registration statement only two years of audited financial statements and two years of selected financial data, rather than the previously required three years of audited financial statements and five years of selected financial data; a reduced executive compensation disclosure provision, which exempts companies from providing a compensation, discussion, and analysis section in their IPO registration statement and reduces compensation disclosure to only three named executive officers, including the CEO and the two other highest-paid executives, instead of five named executive officers; an exemption from auditor attestation on internal controls under Section 404(b) of the SOX Act; and an option to follow private company effective dates for new or revised GAAP standards.

1.2.3. EGC issuers vs. SRC issuers

While the JOBS Act introduced EGCs as a new category of issuers, under the Smaller Reporting Company Regulatory Relief and Simplification Rule, effective February 4, 2008, SRCs already qualified for several de-burdening provisions. These provisions were similar to those afforded by Title I of the JOBS Act, including the reduced accounting and executive compensation disclosure provisions (SEC Release, No. 33-8876). Furthermore, SRCs were also exempt from providing an auditor attestation on the effectiveness of internal controls pursuant to Section 404(b) of the SOX Act because qualifying as a smaller reporting company automatically made a registrant a non-accelerated filer.[5]

Pursuant to the passage of the JOBS Act, all Title I provisions apply to both EGC and SRC issuers alike. Appendix 1 illustrates that while the de-risking provisions were new to both EGCs and SRCs, only the delay GAAP de-burdening provision was new to SRC issuers. Because SRCs already qualified for several de-burdening provisions *before* the JOBS Act was signed into law, we exclude SRCs from the general population of EGC issuers to identify a consistent treatment group.[6]

1.2.4. EGC issuers vs. large issuers

Large issuers with pre-IPO revenues over $1BN are not eligible for EGC status and were not affected by any of the Title I provisions afforded to EGC issuers under the JOBS Act. Since then, some provisions, such as confidential filing and testing the waters, have been extended to all issuers. Therefore, the pre-post JOBS Act comparison for the group of large issuers offers a placebo test of the effect of the JOBS Act on IPO pricing. Importantly, even though the JOBS Act had no bearing on issuers above the $1BN revenue cutoff, both EGC and large issuers were affected by contemporaneous changes in overall IPO market conditions. Different from prior

[5] We note that effective September 10, 2018; that is, after the end of our sample period, the SEC changed its SRC definition to expand the number of registrants that qualify for reduced disclosures (SEC Release, No. 33-10513). The new thresholds for a registrant to qualify as an SRC is an estimated public float of less than $250MN or annual revenues of less than $100MN and an estimated public float of less than $700MN. Under the new rule, qualifying as an SRC will no longer automatically make a registrant a non-accelerated filer.

[6] Prior studies on the effect of the JOBS Act treat SRC issuers as both treatment and control firms. As we also explain in Section 1.4.1.2, Barth et al. (2017) combine SRCs with the general population of EGCs and focus on the pre-post JOBS Act comparison of new issuers below the $1BN revenue cutoff. Chaplinsky et al. (2017) separate SRCs from the general population of EGCs and focus on the differential pre-post JOBS Act change between non-SRC EGCs and SRC issuers. The Supplementary Appendix confirms that our inferences are not sensitive when we include SRCs as part of either the control group or the treatment group.

research, our DID research design controls for intertemporal changes in overall IPO market conditions that are common across the treatment group of EGC issuers and the control group of large issuers.

1.2.5. Prior research on the effect of the JOBS Act on the IPO market

With respect to the effect on IPO activity, Dambra et al. (2015) use a DID research design to control for contemporaneous changes in IPO market conditions across developed economies and argue that the JOBS Act had a positive effect on IPO volume in the U.S. market. Dambra et al. (2015) conclude that the JOBS Act has helped re-energize the U.S. IPO market by de-risking the IPO process and reducing the probability of a withdrawn IPO. Consistent with Dambra et al. (2015), Cheng (2015) provides evidence that the de-burdening provisions had little effect on the composition of IPO firms. More recently, Dathan and Xiong (2021) argue that the testing-the-waters provision of the JOBS Act is associated with a decrease in the number of firms going public.

With respect to the effect on IPO underpricing, Barth et al. (2017) compare post-JOBS Act EGC issuers to pre-JOBS Act issuers below the $1BN revenue cutoff that would have qualified for EGC status had the Act been in effect at the time of their IPO. Their pre-post JOBS Act comparison shows a larger jump of the aftermarket price relative to the offer price, which they interpret as evidence of an increase in IPO underpricing and higher cost of equity capital for EGC issuers. While Barth et al. (2017) focus on the simple pre-versus-post comparison of issuers below the $1BN revenue cutoff, our DID research design controls for intertemporal changes in overall IPO market conditions that are common across the treatment group of EGC issuers and the control group of large issuers. Effectively, our DID research design zeroes in on the differential pre-post JOBS Act change between the treatment group of EGC issuers and the control group of large issuers.

Chaplinsky et al. (2017) separate SRC issuers, which they define as issuers with a public float below $75MN, from the general population of EGC issuers and find consistent evidence of a larger IPO price jump for non-SRC EGCs post-JOBS Act. Other related studies find similar evidence (e.g., Gupta and Israelsen 2016; Agarwal et al. 2017). Chaplinsky et al. (2017) also explore the effect of the JOBS Act on the direct issuance costs for EGCs, including accounting, legal, and underwriting fees, and do not find evidence that potential cost savings offset the indirect cost associated with higher IPO underpricing.

Prior studies on the effect of the JOBS Act have treated SRC issuers in inconsistent ways. While Barth et al. (2017) combine SRCs with the general population of EGCs and focus on the pre-post JOBS Act comparison of new issuers below the $1BN revenue cutoff, Chaplinsky et al. (2017) separate SRCs from the general population of EGCs and focus on the differential pre-post JOBS Act change between non-SRC EGCs and SRC issuers. Relative to prior research, we separate SRCs from the general population of EGCs to identify a consistent treatment group of affected issuers. This is because SRCs already qualified for several de-burdening provisions before the passage of the JOBS Act. We then zero in on the differential pre-post JOBS Act change between the treatment group of non-SRC EGCs and the control group of large issuers. In Section 1.4.1.2, we confirm that the inferences regarding the effect of the JOBS Act on IPO pricing are not sensitive when we include SRCs as part of either the control group or the treatment group.

With respect to the effect on the IPO information environment, Dambra et al. (2018) implement a DID research design to identify the effect of IPO analyst participation as allowed by the JOBS Act on EGC-affiliated analysts (i.e., analysts employed by members of the EGC issuer's IPO underwriting syndicate). They find that EGC-affiliated analysts become more optimistic relative to non-affiliated analysts after the JOBS Act and conclude that greater analyst participation in the IPO process results in less accurate analyst research. Focusing on IPO aftermarket trading, Honigsberg et al. (2015) find that immediately following the IPO individual investors are less likely to trade in the stocks of EGCs that provide less disclosure, but this effect reverses during the two weeks of trading after the offering. In a recent study, Esmer et al. (2020) provide evidence that the confidential filing provision of the JOBS Act affects litigation risk during the pre-IPO period by making the IPO process less salient.

1.3. Research design and data

1.3.1. Model specification

Our first research objective is to separate the effect of the JOBS Act on the IPO pricing of EGC issuers from contemporaneous changes in overall IPO market conditions. Our DID zeroes in on the pre-post JOBS Act comparison of the treatment group of (non-SRC) issuers below the \$1BN revenue cutoff vis-à-vis the control group of large issuers. We acknowledge that a natural control group of perfectly comparable but unaffected issuers does not exist, which makes it impossible to design the perfect DID. This shortcoming, however, does not invalidate the choice of large issuers as a control group of unaffected issuers in our setting. The DID estimator differences away permanent differences in outcomes between the treatment group of EGC issuers and the control group of large issuers as well as any common trend affecting both issuer groups. To control for the effect of cross-sectional differences in IPO pricing determinants, the regression model specifications include a wide array of issuer characteristics and fixed effects as right-hand-side variables. We implement the DID research design using the following regression model

$$Y_i = \alpha + \beta_1 EGC_i \times POST_i + \beta_2 EGC_i + \beta_3 POST_i + \sum_k \gamma_k \times C_{k,i} + \sum_j \delta_j \times \theta_{j,i} + \varepsilon_i. \quad (1)$$

We estimate the model in equation (1) using pooled cross-sectional OLS regression. For the left-hand-side variables (Y_i), we consider IPO aftermarket returns and pre-IPO valuation multiples. For each issuer, we measure IPO returns as buy-and-hold market-adjusted returns from the IPO offering price to the closing price at the end of the first day, first week, and first month of trading. We use the CRSP value-weighted index as the market index. We measure the pre-IPO valuation multiple as the ratio of IPO value divided by pre-IPO revenues. Turning to the right-hand-side variables, EGC_i is an indicator variable for issuers that went public before the passage of the JOBS Act that would have qualified for EGC status had the Act been in effect at the time of their IPO and EGC issuers post-JOBS Act that went public by the end of 2015, $POST_i$ is an indicator variable for issuers that went public after the JOBS Act's passage on April 5, 2012, $C_{k,i}$ is a vector of issuer characteristics, and $\theta_{j,i}$ is a vector of sector fixed effects. The coefficient on EGC_i captures the difference between the treatment and control groups prior to the passage of the JOBS Act. The coefficient on $POST_i$ captures the pre-versus-post JOBS Act trend in the control group. The coefficient on the interaction $EGC_i \times POST_i$ is the DID estimator and captures the difference of the pre-versus-post trends in the treatment group relative to the control group.

Following prior research on IPO pricing determinants, the vector $C_{k,i}$ includes firm age, pre-IPO assets and revenues, IPO proceeds, the fraction of shares retained by pre-IPO shareholders, the offer price revision, the number of days in registration, return on assets (ROA), R&D intensity, CAPEX intensity, along with indicator variables for negative earnings, negative book value of equity, positive R&D, VC backing, software technology companies, biotech companies, the listing stock exchange, reputable underwriters, and Big-4 auditors, as well as the aggregate number of IPOs in registration and the average return of NASDAQ stocks measured during the 90 days leading to the IPO (e.g., Lowry and Schwert 2002, 2004; Loughran and Ritter 2004; Lowry and Murphy 2007; Lowry et al. 2010; Liu and Ritter 2010, 2011; Chaplinsky et al. 2017). The vector of sector fixed effects $\theta_{j,i}$ is based on the two-digit Global Industry Classification Standard (GICS) taxonomy.

Throughout the chapter, we use two-tailed tests when testing for statistical significance. Bertrand et al. (2004) explore the issue of serial correlation in outcome variables in the context of DID estimators and show that OLS standard errors understate the standard deviation of the estimated treatment effects. The issue of serial correlation is especially relevant in the JOBS Act setting due to low-frequency changes in IPO returns (e.g., Loughran and Ritter 2004). To address time-series and cross-sectional residual dependence, we base statistical inferences on standard errors clustered by two-digit GICS code and IPO month. Our inferences are not sensitive when we use one-way clustered standard errors either by industry or IPO month.

1.3.2. Sample construction and descriptive statistics

Our initial sample begins with 974 U.S. issuers that filed their registration statements on Form S-1 and went public between January 1, 2009, and December 31, 2015. We obtain offering data from SDC, stock market data from CRSP, and accounting data from Compustat. We obtain underwriter rank and founding year data from Jay Ritter's website. We restrict the list to offerings of common shares with an offer price above $1, non-missing first-day closing price, and pre-IPO total assets above $1MN. The restricted sample of 801 IPOs excludes unit offerings, rights offerings, ADRs, limited partnership interests, closed-end funds, and REITs. Following prior studies, we exclude issuers below the $1BN revenue cutoff that filed their first registration statement before the JOBS Act and went public after the Act was signed into law (40 cases). This sample filter ensures that all eligible issuers for EGC status could benefit from the Title I provisions. For consistency, we exclude issuers above the $1BN revenue cutoff that filed their first registration statement before the Act and went public after the Act (11 cases). Following prior studies, we exclude post-JOBS Act issuers that were eligible for EGC status but did not adopt this status (9 cases).[7] Given that SRCs already qualified for several de-burdening provisions prior to the passage of the JOBS Act, we exclude 64 issuers that identify as SRCs in their IPO registration statements.[8]

The final sample includes 677 U.S. IPOs from January 1, 2009, to December 31, 2015. Appendix 2 summarizes the sample construction. The sample period balances the pre- and post-JOBS window centered on the passage of the JOBS Act. Given that we investigate shareholder

[7] We observe that three out of these nine cases were trading over-the-counter prior to their S-1 filing. With respect to the remaining six cases, we find that five of them would have lost the EGC status in the year after IPO by exceeding the EGC thresholds. The remaining one case is an older company that emerged from bankruptcy.
[8] We observe that 61 out of the 64 SRCs in our data had gross proceeds below $75MN. On closer inspection, we find that the remaining three cases had projected public float in the immediate IPO aftermarket below $75MN.

value destruction three years after going public, our dataset effectively covers the ten-year period from January 1, 2009, to December 31, 2018.[9]

Figure 1 presents the timeline of the DID research design. The treatment group includes 202 issuers below the $1BN revenue cutoff that went public before the JOBS Act that would have qualified for EGC status had the Act been in effect at the time of their IPO and 380 EGC issuers that adopted the EGC status post-JOBS Act and went public by the end of 2015.[10] The control group includes 39 large issuers above the $1BN revenue cutoff that went public between the beginning of 2009 and before the JOBS Act took effect and 56 such large issuers post-JOBS Act that went public by the end of 2015.

Panel A of Table 1 reports pooled aggregate statistics between 2009 and 2015 highlighting the economic significance of large issuers in the U.S. IPO market relative to EGC issuers. While large issuers account for 14% of the IPO volume (95 deals), they account for 48% of aggregate IPO proceeds and 46% of aggregate IPO value.[11] Moreover, large issuers account for as much as 85% of aggregate revenues and 79% of aggregate employment. Panel B of Table 1 reports the distribution of new issuers across GICS sectors. We observe that the most represented sector among EGC issuers is Healthcare (37%), followed by Information Technology (25%). In comparison, the most represented sector among large issuers is Consumer Discretionary (23%), followed by Industrials (15%), and Healthcare (14%).

Panel C of Table 1 reports the pooled empirical distributions of key variables. Appendix 3 provides detailed variable definitions. We observe that the closing price in the first month of trading is almost 21% higher than the offer price for the average issuer in our sample. Several issuers have a history of operating losses, with 56% of our sample reporting negative book value of equity in the pre-IPO year. Furthermore, we observe that 55% of issuers in our sample destroy shareholder value for IPO aftermarket investors because they underperform the cumulative performance of the stock market index in the three years after going public.

Panel D of Table 1 compares the affected group of EGC issuers and the unaffected group of large issuers. The comparison shows that EGCs are significantly younger, smaller, and less profitable; they also invest significantly more in R&D and CAPEX per dollar of revenues, they are more likely to have VC funding, and they are less likely to engage with high quality underwriters and Big-4 auditors. In addition, EGCs have significantly more positive IPO aftermarket returns and tend to be valued at higher pre-IPO multiples. Relative to large issuers, EGCs have significantly lower levels of institutional ownership and a higher frequency of long-term shareholder value destruction. Despite these differences, both issuer groups are affected by contemporaneous changes in overall IPO market conditions. The comparison of means shows that the two groups are indistinguishable from one another in terms of the number of IPOs in

[9] In additional analysis, we find consistent results when we expand our sample period forward to end in December 2019. Our expanded coverage over the four years between January 1, 2016, and December 31, 2019, increases our sample of EGC issuers by 308 deals and our sample of large issuers by 41 deals.

[10] We note that there are 35 non-SRC issuers with pre-IPO revenues of less than $1 billion from December 8, 2011, to April 5, 2012; that is, the period during which the JOBS Act was applied retroactively. Our inferences are unchanged when we exclude these cases from our sample.

[11] We measure IPO value as the product of the offer price times the total number of shares outstanding (including all share classes) in the company after the IPO (see Appendix 3 for variable definitions).

registration in the 90 days prior to the IPO and the average buy-and-hold return of all NASDAQ-traded stocks during the 90 days prior to the IPO.

1.3.3. Title I provisions

We obtain data on the use of Title I provisions among EGC issuers from Ernst & Young. We expand Ernst & Young's database by manually collecting information on the use of de-risking and de-burdening provisions from the offering documents of EGC issuers. Panels A and B of Table 2 report the frequency of Title I provisions adopted by EGCs over time and across sectors, respectively. The evidence highlights that EGC issuers have elected to avail themselves of the Title I provisions at an increasing rate. The average number of JOBS Act provisions used by EGC issuers has increased from 4.1 in 2012 to 5.3 in 2015, with 97% of the pooled sample of EGC issuers using at least four provisions. By the end of 2015, Table 2 shows that 96% of EGCs used the testing-the-waters provision and that 95% of EGCs chose to file their draft registration statements confidentially. Virtually all EGCs that took advantage of the de-risking provisions also availed themselves of the reduced compensation disclosure provisions, and delayed auditor attestation on internal controls.[12]

In the three years after the passage of the JOBS Act, the main source of heterogeneity in the use of Title I provisions across EGCs is the reduced-accounting disclosure provision. The reduced-accounting disclosure provision allows companies to present only two years of audited financial statements and two years of selected financial data in their IPO filing, rather than the previously required three years of audited financial statements and five years of selected financial data, respectively. By the end of 2015, we find that 48% of EGCs had an operating history of more than two years and elected to take advantage of the reduced-accounting disclosure provision afforded by the JOBS Act. Looking across sectors, the Healthcare sector stands out for two reasons. First, the 171 Healthcare EGCs account for as much as 45% of our sample of EGC issuers post-JOBS Act. Second, we observe that as many as 74% of Healthcare EGCs elected to take advantage of the reduced-accounting disclosure provision, which is the highest rate of adoption of this provision across sectors.[13]

Overall, the substantial overlap in the use of Title I provisions implies that the choice to de-risk and de-burden may be inseparable from the choice of eligible issuers to adopt the EGC status to begin with. Consistent with recent IPO market overviews produced by major accounting firms (e.g., PwC 2018; Ernst & Young 2019), our evidence is consistent with an à la carte approach to adopting most provisions afforded by the JOBS Act.

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[12] We also point out that 92% of EGCs opted out of the provision to adopt new or revised accounting standards using private company dates. The choice to opt out from the delay GAAP provision is generally preferred by investors and analysts as it makes the financial statements of EGCs more comparable to those of other public companies (e.g., PwC 2018). More recently, however, new EGC registrants are increasingly using the delay GAAP provision because doing so gives them more time to adopt major new standards on revenue recognition, leases, and credit losses (e.g., Ernst & Young 2019).

[13] We note that the frequency of reduced-accounting EGCs has further increased in more recent years and this upward trend is mostly explained by the popularity of this provision among Healthcare EGC issuers. Between 2016 and 2019, we find that 65% of EGCs with an operating history of more than two years chose to present only two years of audited financial statements and two years of selected financial data in their IPO filing. Despite the upward trend, the reduced-accounting provision remains the main source of heterogeneity in the use of Title I provisions across EGC issuers.

1.4. Empirical results

1.4.1. IPO aftermarket returns

In our first set of results, we zero in on the IPO aftermarket returns for the treatment group of EGC issuers vis-a-vis the control group of large issuers. We measure IPO returns relative to the offer price at daily, weekly, and monthly horizons after the offer date.

1.4.1.1. Portfolio and regression results

Panels A and B of Table 3 report the portfolio mean values of market-adjusted buy-and-hold stock returns cumulated from the IPO offer price to the closing price at the end of the first day $ARET[D]$, first week $ARET[W]$, and first month of trading $ARET[M]$ for EGC and large issuers, respectively.[14] Focusing on the treatment group of issuers below the $1BN revenue cutoff, the pre-post JOBS Act comparison in Panel A of Table 3 shows that the first-day return increased by 6.7 percentage points from 13.5% in the pre-JOBS Act period to 20.2% in the post-JOBS Act period. The pre-post return spread is 8.4 percentage points at the end of the first week of trading and 12.6 percentage points at the end of the first month of trading. We note that our pre-post JOBS Act comparison of IPO returns for the treatment group of EGC issuers hews closely to prior work. Different from our study, however, prior research interprets the larger IPO jump as de facto evidence of an increase in IPO underpricing and more money left on the table for EGCs after the passage of the JOBS Act.

Turning to the control group of large issuers, we find an effect of similar magnitude in the immediate IPO aftermarket. The pre-post JOBS Act comparison in Panel B of Table 3 reveals that the first-day returns of large issuers increased by 6.6 percentage points from 6.4% in the pre-JOBS Act period to 13.0% in the post-JOBS Act period. The pre-post return spread is 8.2 percentage points at the end of the first week of trading and 11.4 percentage points at the end of the first month of trading. This placebo effect cannot be attributed to the JOBS Act because large issuers are not eligible for EGC status and were not affected by any of the Title I provisions afforded to EGC issuers. The pre-post JOBS Act comparison across issuer groups implies that the differences between the treatment group of EGC issuers below the $1BN revenue cutoff and the control group of large issuers above the $1BN revenue cutoff are similar over time. This parallel trend is what one would expect in the absence of a treatment effect due to the passage of the JOBS Act.

Table 4 reports the DID regression results. The odd (even) columns report regression results before (after) controlling for issuer characteristics and sector fixed effects.[15] The slope coefficient on the interaction $EGC \times POST$ captures the pre-versus-post JOBS Act difference in the average returns for the treatment group of non-SRC EGC issuers below the $1BN revenue cutoff minus the pre-versus-post difference in the average returns for the control group of large issuers above the $1BN revenue cutoff. Across model specifications, the estimated difference-in-differences are indistinguishable from zero. This finding holds for different return windows, ranging from the first day to the first month after the IPO. This finding also holds after the inclusion of issuer characteristics and sector fixed effects as right-hand-side variables. Focusing on first-day

[14] In untabulated analyses, we find consistent results measuring excess IPO returns relative to a portfolio of seasoned companies matched based on industry, size, and book-to-market.

[15] For brevity, we suppress the output. Table A1 in the Supplementary Appendix reports the coefficient estimates on the issuer characteristics and shows that they are generally consistent with prior research on the determinants of IPO aftermarket returns.

returns, we observe that the slope coefficient on the EGC indicator is significantly positive before the inclusion of issuer characteristics and sector fixed effects as right-hand-side variables. However, it becomes indistinguishable from zero after the inclusion of these variables. This finding implies that the pre-JOBS Act difference in the first-day returns of EGC issuers relative to large issuers is captured by cross-sectional differences in fundamental characteristics.

We report additional robustness tests in Table A2 of the Supplementary Appendix. First, Columns 1 and 2 confirm that our inferences are unchanged using propensity score matched DID regression. Second, an alternative regression discontinuity design would zero in on issuers within a tight bandwidth just above and just below the \$1BN revenue cutoff. However, the density of observations around the \$1BN revenue cutoff is very low, which makes it impossible to implement a regression discontinuity design. To illustrate, a $+/-100\text{MN}$ bandwidth around the \$1BN revenue cutoff captures only 1.7% of EGC issuers (10 cases) and 3.2% of large issuers (3 cases). As we cannot zero in on issuers near the cutoff, we investigate whether our results are driven by "mega" issuers that are further away from the cutoff. Columns 3 and 4 report consistent results after dropping issuers with pre-IPO revenues in excess of \$10BN (11 cases). Third, Alhusaini et al. (2020) provide evidence that an issuer categorization as a "Unicorn" increases investor demand for its shares and leads to more positive first-day returns. One question might be whether our results are sensitive to the inclusion of Unicorn IPOs. Columns 5 and 6 report the DID regression results for IPO returns after excluding Unicorn IPOs and confirm that the coefficient on the interaction $EGC \times POST$ remains indistinguishable from zero.[16]

1.4.1.2. Relation to prior research on the effect of the JOBS Act on IPO pricing

As we explain in Section 1.2.3, prior studies on the effect of the JOBS Act have handled SRCs as part of both the treatment and control group. Barth et al. (2017) combine SRCs with the general population of EGCs and focus on the pre-post JOBS Act comparison of IPOs below the \$1BN revenue cutoff. Whereas Barth et al. (2017) handle SRCs as part of the treatment group, we separate SRCs from the general population of EGCs to identify a consistent treatment group of affected issuers. This is because SRCs already qualified for several de-burdening provisions before the JOBS Act was signed into law. We then zero in on the differential pre-post change between the treatment group of non-SRC EGCs and the control group of large issuers. Nevertheless, Columns 1 and 2 of Table A3 in the Supplementary Appendix confirm that the estimated coefficient on $EGC \times POST$ remains indistinguishable from zero when we include SRC issuers (64 cases) as part of the treatment group.

With respect to Barth et al. (2017), we note that their sample covers the pre-JOBS Act period between July 1, 2009, and April 4, 2012, and the post-JOBS Act period between April 5, 2012, and December 31, 2013. Additionally, Barth et al. (2017) explain that in untabulated analyses, they do not find a significant change in the IPO aftermarket returns of large issuers post-JOBS Act, which is different from our evidence of a significant placebo effect on the pricing of

[16] Alhusaini et al. (2020) identify November 2, 2013, as the time of the introduction of the Unicorn category with the publication of a TechCrunch article that coined this term for the first time. Using data from CB Insights' tracker of billion-dollar VC-backed exits, we identify 18 Unicorn IPOs between November 2, 2013, and December 31, 2015, which is close to the annual Unicorn IPO activity detailed in Figure 1 of their paper. From these 18 Unicorn IPOs, we identify 4 foreign issuers that do not enter our analysis because our sample focuses on issuers who filed their registration statements on Form S-1. Focusing on the remaining 14 Unicorn IPOs, we identify 12 non-SRC EGC issuers, 1 SRC EGC issuer, and 1 large issuer.

large issuers. They attribute the lack of significance to low power. To ensure that our results are not due to sample period differences, Columns 3 and 4 of Table A3 report the DID regression results for their restricted pre-post JOBS Act period. The restricted sample includes 194 (127) non-SRC EGC issuers in the pre (post) period and 38 (28) large issuers in the pre (post) period. Our evidence confirms that the estimated coefficient on $EGC \times POST$ is indistinguishable from zero in this restricted pre-post JOBS Act period. Moreover, to mitigate the impact of influential observations, we further report robust regression results based on Yohai's (1987) MM-estimator. Columns 5 and 6 in Table A3 confirm that the estimated coefficient on $EGC \times POST$ is indistinguishable from zero in the robust regression.

With respect to Chaplinsky et al. (2017), we point out that they use SRCs as the control group, which is opposite to Barth's et al. (2017) inclusion of SRCs in the treatment group.[17] As we also explain in Section 1.2.3, the group of SRC issuers does not offer a control group of entirely unaffected issuers. Indeed, even though SRCs already qualified for several de-burdening provisions pre-JOBS Act, it is only after the passage of the JOBS Act that SRC issuers were afforded Title I de-risking provisions and the delay GAAP de-burdening provision. In addition, while we identify SRCs using hand-collected information directly from the IPO registration statements, Chaplinsky et al. (2017) broadly define SRCs as all issuers with less than $75MN in gross proceeds. This broad definition overclassifies issuers as SRCs. To illustrate, our sample covers as many as 186 cases of non-SRC EGC issuers with less than $75MN in gross proceeds. These 186 non-SRC EGC issuers would have been misclassified as SRCs based on Chaplinsky's et al. (2017) definition.

Table A4 in the Supplementary Appendix provides evidence that the misclassification of non-SRC EGC issuers as SRCs leads to spurious evidence of an increase in first-day returns. First, we report DID regression results using SRCs rather than large issuers as the control group where we classify SRCs following Chaplinsky's et al. (2017) broad definition as issuers with IPO proceeds below $75MN. Consistent with their evidence of an increase in first-day returns for EGCs versus SRCs, Column 1 shows that the coefficient on $EGC \times POST$ is significantly positive. However, after the inclusion of issuer characteristics and sector fixed effects in Column 2, the coefficient on the interaction term, while remains relatively intact in terms of magnitude, becomes statistically insignificant. Second, we repeat this analysis but identify SRCs by hand-collecting information directly from the IPO registration statements. The DID regression results in Columns 3 and 4 show that the interaction term is indistinguishable from zero both before and after the inclusion of our control variables.

Viewed as a whole, these results highlight that the misclassification of non-SRC EGC issuers as SRCs can lead to spurious evidence of a differential post- effect on the first-day returns of EGC issuers relative to SRC issuers. Nevertheless, we reiterate that the group of SRCs does not offer a control group of entirely unaffected issuers to begin with.

[17] Different from Barth et al. (2017), Chaplinsky et al. (2017) use a pre-JOBS Act window that stretches as far back as January 1, 2003. Bertrand et al. (2004), however, show that the use of a long time series is problematic for DID estimators when there is serial correlation in the dependent variable. The use of a long time series is especially problematic for research on the effect of the JOBS Act due to serial correlation in IPO market returns.

1.4.2. Pre-IPO valuation multiples

The portfolio and regression results provide evidence that changes in overall IPO market conditions coincident with the passage of the JOBS Act explain the seeming increase in the IPO underpricing of EGC issuers. While prior studies point to the pre-versus-post JOBS Act increase of first-day returns as conclusive evidence of an increase in the cost of equity capital for EGC issuers, we provide evidence that EGCs raise capital at significantly higher pre-IPO valuation multiples post-JOBS Act.

Multiples are widely used in practice when valuing IPOs (e.g., Kim and Ritter 1999; Guo et al. 2005). Our analysis of pre-IPO valuation multiples offers a direct test of pre-market discounting by underwriters. Higher IPO underpricing would imply heavier pre-IPO discounting and, therefore, lower pre-IPO valuation multiples for EGC issuers going public post-JOBS Act. We measure pre-IPO valuation multiples based on the ratio of IPO value divided by pre-IPO fundamentals.[18] Because pre-IPO earnings and book value multiples are negative and, therefore, not meaningful for most new issuers in our sample, we use pre-IPO revenues. [19] To mitigate small and zero denominator problems, we require pre-IPO revenues of at least $10MN. In untabulated results, we find similar results using alternative minimum revenue cutoffs, including $1MN, $5MN, or $20MN.

Starting with the portfolio results, the pre-post JOBS Act comparisons in Table 5 reveal an *expansion*, rather than a contraction, of pre-IPO valuation multiples for the treatment group of EGC issuers. Panel A of Table 5 shows that the average pre-IPO revenue multiple of EGC issuers increased from $6.3\times$ in the pre-JOBS Act period to $10.5\times$ in the post-JOBS Act period. Consistent with a change in overall IPO market conditions, the pre-post JOBS Act comparison in Column 2 also shows a modest expansion in the pre-IPO revenue multiples for the control group of large issuers. The average multiple of large issuers increased from approximately $0.9\times$ in the pre-JOBS Act period to $1.1\times$ post-JOBS Act period. Focusing on median values, Panel B of Table 5 provides consistent evidence of expansion in the pre-IPO revenue multiples for the treatment group of EGC issuers. Turning to the DID regression results, Column 1 of Table 6 shows that the coefficient on the interaction $EGC \times POST$ is significantly positive, which is consistent with a differential effect on EGC issuers' pre-IPO multiples relative to large issuers. Column 2 of Table 6, however, shows that the coefficient on the interaction becomes indistinguishable from zero after controlling for issuer characteristics and sector fixed effects. The implication here is that after we account for cross-sectional differences in characteristics, EGCs are indistinguishable from large issuers in terms of their pre-post JOBS Act change in pre-IPO valuation multiples.

Overall, our evidence departs from prior research concluding that EGCs leave more money on the table due to higher IPO underpricing. If it were truly the case that the JOBS Act resulted in higher pre-market discounting by underwriters, we would have detected a contraction rather than

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[18] With respect to the use of forward multiples, we note that sell-side analysts' coverage typically begins only after the IPO at the end of a quiet period of 25 days following the offering. Therefore, we cannot calculate pre-IPO valuation multiples using analysts' projections of future value drivers.

[19] Revenue multiples are popular in practice because unlike earnings and book value multiples, which are negative for many young and growth companies, revenue multiples can be computed more broadly. Indeed, the majority of IPOs in our sample have a history of losses with 58% of them reporting negative net income and 56% reporting negative equity prior to their IPO. The frequency of new issuers reporting negative book value decreases from 56% pre-IPO to 48% in the first quarter post-IPO due to a variety of factors, such as the conversion of preferred stock to common stock (e.g., Dudley and James 2018).

an expansion of pre-IPO valuation multiples among EGCs after the passage of the JOBS Act. To shed light on this effect, we next exploit heterogeneity in the use of Title I provisions across EGC issuers.

1.4.3. Reduced-accounting versus non-reduced accounting EGC issuers

Our earlier evidence on the use of Title I provisions shows that the main source of heterogeneity across EGCs is the choice to present only two years of audited financial statements and two years of selected financial data, rather than the previously required three- and five-years' worth of data, respectively. Therefore, our cross-sectional tests zero in on heterogeneity in the use of the reduced-accounting disclosure provision.

Given that the reduced-accounting disclosure choice is voluntary, EGCs that are expected to derive the greatest benefits are the most likely to take advantage of the provision. Consistent with this broad idea, we find that reduced-accounting EGCs raise capital at significantly higher pre-IPO valuation multiples. Specifically, Panel A of Table 7 reports mean values across three groups: (a) the pre-JOBS Act group of issuers below the \$1BN revenue cutoff, (b) the post-JOBS Act group of EGCs that did not adopt the reduced-accounting disclosure provision (non-reduced-accounting EGCs), and (c) the post-JOBS Act group of EGCs that adopted the reduced-accounting disclosure provision (reduced-accounting EGCs). The first set of portfolio results shows that while the $(b) - (a)$ spread between non-reduced-accounting EGCs and pre-JOBS Act issuers below the \$1BN revenue cutoff is indistinguishable from zero, the $(c) - (b)$ spread between reduced-accounting EGCs and non-reduced accounting EGCs is significantly positive. It follows that the pre-IPO valuation premium is concentrated in EGC issuers that take advantage of the reduced-accounting disclosure provision.

If the pre-IPO valuation premium reflects overpricing, we should observe that reduced-accounting EGCs are associated with a higher probability of long-term underperformance. To test this prediction, we build on Bessembinder's (2018) measurement of shareholder wealth creation and create an indicator of long-term value destruction for IPOs that subsequently underperform the stock market index $I(Value\ Destruction)$.[20] We focus on the three years after going public because prior studies on the long-term performance of IPOs typically focus on this window (e.g., Ritter 1991; Loughran and Ritter 1995; Carter et al. 1998). The value destruction indicator is equal to one if the buy-and-hold return from the IPO offer price to the closing price at either the end of the third year of trading or the delisting date is below the buy-and-hold return of the CRSP value weighted market index over the same period. The second set of portfolio results in Panel A of Table 7 reports the frequency of value-destructive IPOs across EGC groups.

The evidence shows that nearly two-thirds or 65.7% of reduced-accounting EGCs underperformed the market portfolio in the three years after going public. In comparison, the frequency of value-destructive deals is 48.6% for non-reduced-accounting EGC issuers. Together, the evidence suggests that reduced-accounting EGCs are significantly more prone to overpricing in the immediate IPO aftermarket. To shed light on the implications for individual investors, we explore variation in institutional ownership (IO) across EGC groups. We measure IO as the % of shares held by institutions that report their quarterly holdings in SEC Form 13F and N-30Ds. In

[20] In untabulated analyses, we find consistent results using an indicator for IPOs that underperform relative to a portfolio of seasoned companies matched based on industry, size, and book-to-market. We also find consistent results using an indicator for IPOs that underperform relative to 10-year Treasury bonds.

general, lower IO indicates higher individual investor ownership and lower investor-base sophistication (e.g., Nagel 2005). We use the average level of IO over the three-year period starting from the IPO date. We find similar results using the first available value of IO after the IPO date.

With respect to changes in ownership structure, Barth et al. (2017) find an overall increasing trend in IO for EGCs post-JOBS Act. Consistent with this trend, we find that a 7.2 percentage point increase in IO across EGC issuers. Separating EGCs based on whether they availed themselves of the reduced accounting provision uncovers distinct dynamics across issuer groups. The third set of portfolio results in Panel A of Table 7 reveals that the increasing trend in institutional ownership is primarily due to the group of non-reduced-accounting EGC issuers. While the pre-JOBS Act level of IO for issuers below the \$1BN revenue cutoff is 51.3%, it increases by 11 percentage points to 62.3% for non-reduced accounting EGCs, and only by 2.6 percentage points to 53.9% for reduced accounting EGC issuers. The $(c)-(b)$ spread is negative indicating that the post-JOBS Act level of IO is significantly lower for reduced-accounting EGCs relative to non-reduced accounting EGC issuers. One relevant implication is that individual investors may have been disproportionately exposed to shareholder value destruction in the IPO aftermarket post-JOBS Act. This evidence is consistent with Field and Lowry's (2009) conclusion that while institutional investors have the ability to use publicly available information to avoid the worst-performing IPO stocks, individual investors tend to ignore firm fundamentals when investing in IPO stocks.

To complement the portfolio analysis, Panel B of Table 7, reports regression results from the following model

$$Y_i = \alpha + \beta_1 EGC_i \times POST_i \times RA_i + \beta_2 EGC_i \times POST_i + \beta_3 EGC_i + \beta_4 POST_i + \sum_k \gamma_k \times C_{k,i} + \sum_j \delta_j \times \theta_{j,i} + \varepsilon_i \tag{2}$$

The right-hand-side variable RA_i is an indicator variable for the post-JOBS Act group of reduced-accounting EGC issuers. With respect to the interpretation of the regression estimates in equation (2), the coefficient on EGC captures the pre-JOBS Act difference between large issuers above the \$1BN revenue cutoff and issuers below the \$1BN revenue cutoff. The coefficient on $POST$ captures the pre-versus-post JOBS Act trend in large issuers. The coefficient on the interaction $EGC \times POST$ captures the pre-versus-post trend in the group of non-reduced-accounting EGCs post JOBS Act and issuers below the \$1BN revenue cutoff pre-JOBS Act relative to the pre-versus-post trend in the group of large issuers. The coefficient on the triple interaction $EGC \times POST \times RA$ captures the post-JOBS Act difference between reduced-accounting EGCs and non-reduced-accounting EGC issuers.

The regression results are consistent with the portfolio analysis. When the outcome variable is the pre-IPO valuation multiple, the coefficient estimate on $EGC \times POST \times RA$ is significantly positive and the coefficient estimate on $EGC \times POST$ is indistinguishable from zero. In combination, these coefficient estimates confirm that the pre-IPO valuation premium is concentrated in reduced-accounting EGC issuers. The evidence also confirms that the group of reduced-accounting EGCs is associated with higher likelihood of shareholder value destruction and lower level of IO relative to non-reduced accounting EGC issuers, respectively. Importantly, the coefficient estimates on the triple interaction $EGC \times POST \times RA$ become indistinguishable from zero after controlling for issuer characteristics and sector fixed effects. This result points to fundamental differences across EGC groups.

Following this lead, Table 8 compares issuer characteristics across reduced- and non-reduced accounting EGC issuers. While our objective is not to build an exhaustive selection model, the comparison reveals key differences across EGC groups. Reduced-accounting EGCs tend to smaller, more R&D intensive, and less profitable, as indicated by the lower return on assets and the higher frequency of negative earnings and book value of equity, relative to non-reduced accounting EGC issuers. These characteristics are associated with more speculative valuation profiles and a higher tendency for overpricing (e.g., Purnanandam and Swaminathan 2004; Field and Lowry 2009; Aboody et al. 2018; Patatoukas et al. 2021). Furthermore, reduced-accounting EGCs are also more likely to be biotech issuers with VC funding and are less likely to engage reputable IPO underwriters.

The cross-sectional differences across EGC groups imply that it is not the reduced-accounting provision choice *per se* leading to more prevalent overpricing but rather the fundamental characteristics of EGCs that choose to avail themselves of this provision. We further note that evidence that reduced-accounting EGCs have more speculative valuation profiles and are more prone to long-term shareholder value destruction is consistent with the view that individual investors are attracted to lottery-type stocks (e.g., Barberis and Huang 2008; Han and Kumar 2013; Kumar 2009).[21]

1.4.4. Predicting reduced-accounting EGCs

In this section, we examine whether we can predict EGCs' decision regarding the election of the reduced-accounting disclosure provision. If fundamental characteristics drive the decision, we should be able to predict the decision using those characteristics.

Using a set of issuer characteristics described in Section 1.3.1, we classify whether an EGC elects the reduced-accounting provision. The outcome variable is an indicator variable that takes the value of 1 for EGCs electing the provision and 0 for other EGCs. To make sure that predictability is not sensitive to the choice of machine learning algorithms, we model using six different classifiers. Specifically, we use Naive Bayes, AdaBoost, Random Forest, Logistic Regression, Support Vector Machine (SVM), and K-Nearest Neighbor algorithms. The sample includes 380 EGCs from April 5, 2012, to December 31, 2015. From this sample, we randomly choose 80% of EGCs (304 issuers) to train our models and use the rest (76 issuers) to evaluate the prediction performances.

Table 9 presents the results of reduced-accounting EGC prediction. Across different classifiers (rows) and evaluation metrics (columns), we consistently find that our machine learning models perform well in predicting the reduced-accounting provision election. For example, we find that the accuracy of our models ranges from 82% to 83%. The evidence in this table further supports the idea that it is not the reduced-accounting provision choice *per se* leading to more

[21]Lottery-type stocks have positively skewed returns and earn negative average excess returns. In untabulated analyses, we find that reduced-accounting EGCs with below-median institutional ownership have underperformed the stock market index by -38.6% in the three years post-IPO. This observation highlights the risks to Main Street investors from actively targeting EGC issuers in the IPO aftermarket.

prevalent overpricing but rather the fundamental characteristics of EGCs drive the provision election decision.

1.5. Conclusion

Using a DID design, we provide evidence that changes in overall IPO market conditions coincident with the JOBS Act explain the seeming increase in the IPO underpricing of EGC issuers. In contrast to prior evidence that EGCs leave more money on the table due to higher pre-market discounting by underwriters, we find that EGCs raise capital at significantly higher pre-IPO valuation multiples. This pre-IPO valuation premium is concentrated in EGCs that take advantage of the reduced-accounting disclosure provision of the JOBS Act. Reconstructing the typical issuer profile, we document that reduced-accounting EGCs have more speculative valuation profiles, lower institutional ownership, and are more likely to destroy long-term shareholder value. A relevant implication is that inferences regarding the effect of the JOBS Act on IPO pricing are confounded by overall time trends that are concurrent with the passage of the Act.

Overall, our study offers an alternative perspective on the effect of the JOBS Act on IPO pricing. Different from Barth's et al. (2017) focus on the pre-versus-post comparison of issuers below the $1BN revenue cutoff, our DID research design controls for intertemporal changes in overall IPO market conditions that are common across the treatment group of EGC issuers and the control group of large issuers. And different from Chaplinsky's et al. (2017) focus on the pre-post comparison of SRC and non-SRC EGCs, we identify large issuers as a control group that, unlike SRCs, are not eligible for EGC status and were not affected by any of the Title I provisions afforded by the JOBS Act.

With respect to policy making, our evidence that individual investors may have been disproportionately exposed to shareholder value destruction post-JOBS Act could inform the SEC's efforts to facilitate capital formation while protecting the interests of Main Street investors. The evidence is especially timely considering the SEC's rule extending EGC accommodations to non-EGC issuers (SEC Release, No. 33-10699). Under this rule, effective December 3, 2019, all initial registrants can test the waters with certain institutional investors prior to filing a registration statement. This rule found support as one that will result in additional offerings and more investment opportunities without raising significant investor protection concerns (SEC Public Statement, 9/26/2019).

We do not dispute that enhancing the ability to conduct a successful registered offering would ultimately provide more opportunities to invest in public companies. Yet, our evidence highlights that regulators should balance the benefits of increasing the number of IPO registrants against the costs of enabling speculative issuers to go public with reduced financial disclosures. With respect to investor protection in the IPO aftermarket, the quality of IPOs is as important, if not more so, than the quantity of IPOs. On the part of Main Street investors, our evidence calls attention to the risks of actively targeting IPO stocks with speculative valuation profiles.

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Appendices, figures, and tables

Appendix 1
Overview of Title I provisions

Provision Type	Provisions	EGC issuers	Large issuers	SRC Issuers
De-risking	Testing the Waters	New	Not applicable	New
De-risking	Confidential Filing	New	Not applicable	New
De-burdening	Reduced Accounting	New	Not applicable	Old
De-burdening	Reduced Compensation	New	Not applicable	Old
De-burdening	Omit CDA	New	Not applicable	Old
De-burdening	Delay SOX	New	Not applicable	Old
De-burdening	Delay GAAP	New	Not applicable	New

Testing the Waters: The testing-the-waters provision allows issuers to engage in oral or written communications with potential investors that are qualified institutional buyers or institutions that are accredited investors prior to filing a registration statement.

Confidential Filing: The confidential filing provision allows issuers to submit a draft of their IPO registration statement to the SEC for confidential review as long as the initial confidential submission and all amendments are publicly filed with the SEC not later than 15 days before the start of the issuer's IPO roadshow.

Reduced Accounting: The reduced-accounting provision allows companies to present only two years of audited financial statements and two years of selected financial data in their IPO filing, rather than the previously required three years of audited financial statements and five years of selected financial data.

Reduced Compensation: EGC issuers may provide compensation disclosure for three named executive officers instead of five.

Omit CDA: The omit CDA provision allows EGC issuers to omit a Compensation Discussion and Analysis (CDA) section.

Delay SOX: EGC issuers may choose to delay having internal control over financial reporting audited by independent registered public accounting firm under Section 404(b) of the Sarbanes-Oxley Act.

Delay GAAP: The delay GAAP provision allows EGC issuers to delay adopting new or revised accounting standards until those standards apply to private companies.

Appendix 2
Sample construction steps

	Dif.	Obs.
All issuers who filed their registration statements on Form S-1 and went public between January 1, 2009, and December 31, 2015.		974
Restrict sample to offerings of common/ordinary (Class A and Class B) shares that are not unit offerings, rights offerings, ADRs, limited partnership interests, closed-end funds, and REITs.	–146	828
Exclude issuers with offer price below $1, missing first-day closing price, and pre-IPO total assets below $1MN.	–27	801
Exclude issuers that filed their first registration statement before the JOBS Act and went public after the Act.	–51	750
Exclude issuers post-JOBS Act that were eligible for EGC status but did not adopt the status.	–9	741
Exclude issuers that identified as SRCs in their IPO registration statements.	–64	677

Appendix 3
Key variable definitions

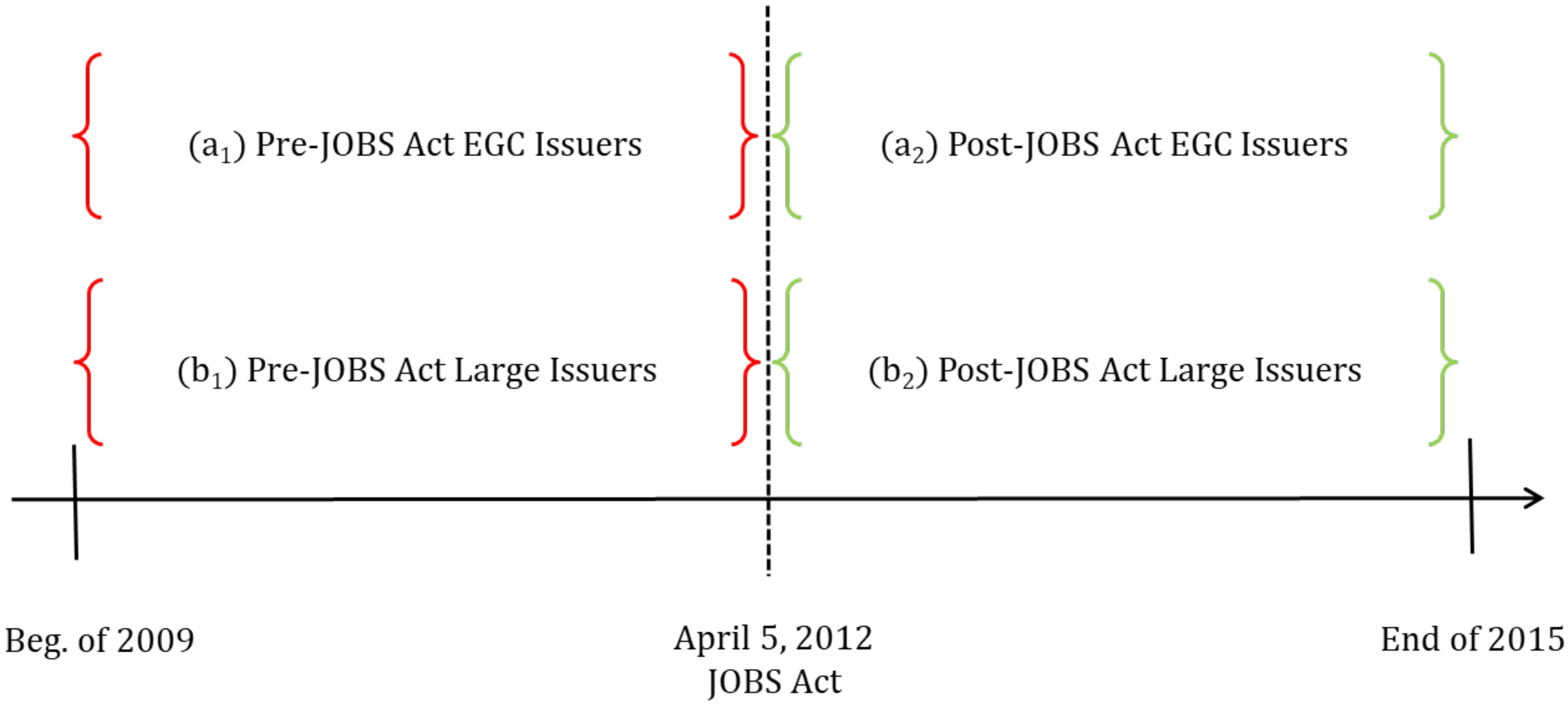
Outcome Variables	
$ARET[D, W, M]$	Buy-and-hold market-adjusted return from the offer price to the closing price at the end of the first day (D), first week (W), and first month (M) of trading. We use the CRSP value-weighted index including distributions for the market index. Specifically, we calculate the return for firm i in year t as follows: $$\prod_{1}^{T}(1 + r_{it}) - \prod_{1}^{T}(1 + r_{mt}).$$
$IPO\ Value/Revenues$	The ratio of IPO value divided by pre-IPO revenues. IPO value measured as the product of the offer price times the total number of shares outstanding (including all share classes) in the company after the IPO ($IPO\ Value$) We require pre-IPO revenues to be at least $10MN.
$I(Value\ Destruction)$	An indicator variable $= 1$ if the buy-and-hold return from the IPO offer price to the closing price at either the end of the third year of trading or the delisting date is below the buy-and-hold return of the CRSP value weighted stock market index including distributions over the same period.
$Instit.Owner.$	Fraction of shares outstanding held by institutions that report their quarterly holdings in SEC Form 13F and N-30Ds. We use the average level of institutional ownership over the three-year period starting from the IPO date.

Issuer Characteristics	
$log(Age)$	Natural logarithm of firm age measured as one plus the difference in years between the IPO date and the firm's founding or incorporation date. We obtain founding year data from Jay Ritter's website.
$log(AT)$	Natural logarithm of the dollar amount of total assets ($MN) in the most recent complete fiscal year prior to the IPO.
$log(Revenues)$	Natural logarithm of one plus revenues ($MN) in the most recent complete fiscal year prior to the IPO.
$log(Proceeds)$	Natural logarithm of total dollar gross proceeds ($MN) excluding the overallotment option.
$\%Retained$	Fraction of shares outstanding in the company that is retained by pre-IPO shareholders.
$\%\ \Delta\ Offer\ Price$	Percentage change in offer price from the midpoint of the preliminary offer price range.

$log(Days\ to\ IPO)$	Natural logarithm of the number of days between the S-1 filing date and the IPO date.
$Return\ on\ Assets$	Net income divided by total assets both measured in the most recent complete fiscal year prior to the IPO. To mitigate the effect of influential observations, we winsorize absolute values of ROA that are greater than 100 percent.
$R\&D\ Intensity$	R&D expense divided by total assets both measured in the most recent complete fiscal year prior to the IPO. To mitigate the effect of influential observations, we winsorize absolute values of R&D intensity that are greater than 100 percent.
$CAPEX\ Intensity$	Capital expenditure divided by total assets both measured in the most recent complete fiscal year prior to the IPO. To mitigate the effect of influential observations, we winsorize absolute values of CAPEX intensity that are greater than 100 percent.
$I(NI < 0)$	An indicator variable that $= 1$ if the company reports negative net income in the most recent complete fiscal year prior to the IPO; $= 0$ otherwise.
$I(BVE < 0)$	An indicator variable that $= 1$ if the company reports negative book value of equity in the most recent complete fiscal year prior to the IPO; $= 0$ otherwise.
$I(R\&D > 0)$	An indicator variable that $= 1$ if the company reports positive R&D expense in the most recent complete fiscal year prior to the IPO; $= 0$ otherwise.
$I(VC)$	An indicator variable $= 1$ if the issuer has venture-capital backing; $= 0$ otherwise.
$I(Soft\ Tech)$	An indicator variable $= 1$ if the issuer is in the Internet Software & Services industry (GICS Code 451010) or the Software industry (GICS Code 451030); $= 0$ otherwise.
$I(Bio\ Tech)$	An indicator variable $= 1$ if the issuer is in the Biotechnology industry (GICS Code 352010) or the Pharmaceutical industry (GICS Code 352020); $= 0$ otherwise.
$I(NASDAQ)$	An indicator variable $= 1$ if the issuer is listed on NASDAQ; $= 0$ otherwise.
$I(NYSE)$	An indicator variable $= 1$ if the issuer is listed on NYSE; $= 0$ otherwise.
$I(HQU)$	An indicator variable that $= 1$ if Loughran and Ritter's (2004) IPO underwriter rank score is $= 9$; $= 0$ otherwise. We obtain underwriter rank data from Jay Ritter's website.

$I(BIG4)$	An indicator variable = 1 if the issuer is audited by Deloitte, Ernest & Young, KPMG, or PwC; = 0 otherwise.
$\# \ IPO_{-90}$	Number of IPOs in registration in the 90 days prior to the IPO.
$NASDAQ_{-90}$	Average buy-and-hold return of all NASDAQ-traded stocks during the 90 days prior to the IPO.

Figure 1
Illustration of research design



This figure illustrates our DID research design. The DID estimates the differential effect of the JOBS Act on the treatment group of EGCs; that is, $(a_2) - (a_1)$, relative to the control group of large issuers; that is, $(b_2) - (b_1)$. The JOBS Act was signed into law on April 5, 2012. The pre-JOBS Act period begins on January 1, 2009. The post-JOBS Act period ends on December 31, 2015. Throughout the chapter, we refer to issuers below the $1BN revenue cutoff as EGC issuers even though the term “EGC” was introduced only after the passage of the JOBS Act.

Table 1
Empirical distributions

Panel A: Aggregate statistics for U.S. operating company IPOs, Jan. 2009-Dec. 2015.

	(a) EGC Issuers	(b) Large Issuers	(b)/(a + b)
Number of Issuers	582	95	14%
Proceeds ($BN)	89.2	82.2	48%
IPO Value ($BN)	441.1	375.4	46%
Revenues ($BN)	89.6	511.1	85%
Employees (000s)	556.5	2,140.8	79%

Panel B: Sample distribution across sectors.

	All Issuers	EGC Issuers	%	Large Issuers	%
Healthcare	228	215	36.9%	13	13.7%
Information Technology	157	146	25.1%	11	11.6%
Consumer Discretionary	86	64	11.0%	22	23.2%
Financials	82	71	12.2%	11	11.6%
Industrials	48	34	5.8%	14	14.7%
Energy	35	28	4.8%	7	7.4%
Consumer Staples	20	11	1.9%	9	9.5%
Materials	15	7	1.2%	8	8.4%
Utilities	6	6	1.0%	0	0.0%
Total	677	582	100.0%	95	100.0%

Panel C: Empirical distributions of key variables.

	Mean	Std. Dev.	p25	p50	p75
$ARET[D]$	0.168	0.280	–0.001	0.091	0.252
$ARET[W]$	0.175	0.282	–0.006	0.103	0.280
$ARET[M]$	0.205	0.324	–0.001	0.135	0.327
$IPO\ Value/Revenues$	7.500	18.738	1.343	3.333	7.335
$I(Value\ Destruction)$	55%	50%	0%	100%	100%
$Instit.Owner.$	0.586	0.278	0.401	0.595	0.800
Age	21.9	26.3	8.0	12.0	21.0
AT ($\$MN$)	1,932.7	11,507.9	43.8	123.6	861.8
$Revenues$ ($\$MN$)	887.3	4,715.6	22.2	91.1	342.8
$Proceeds$ ($\$MN$)	253.1	706.6	70.4	107.8	222.2
$\%Retained$	73%	16%	69%	76%	82%
$\%\Delta(Offer\ Price)$	–3%	15%	–13%	0%	7%
$Days\ to\ IPO$	112	130	37	76	117
$Return\ on\ Assets$	–0.19	0.39	–0.38	–0.03	0.03
$R\&D\ Intensity$	0.21	0.30	0.00	0.05	0.32
$CAPEX\ Intensity$	0.06	0.09	0.01	0.03	0.06
$I(NI < 0)$	58%	49%	0%	100%	100%
$I(BV < 0)$	56%	50%	0%	100%	100%
$I(R\&D > 0)$	60%	49%	0%	100%	100%
$I(VC)$	50%	50%	0%	100%	100%
$I(Soft\ Tech)$	16%	37%	0%	0%	0%
$I(Bio\ Tech)$	24%	43%	0%	0%	0%
$I(NASDAQ)$	58%	49%	0%	100%	100%
$I(NYSE)$	41%	49%	0%	0%	100%
$I(HQU)$	50%	50%	0%	0%	100%
$I(BIG4)$	83%	37%	100%	100%	100%
$\#\ IPO_{-90}$	57.8	16.9	48.0	59.0	72.0
$NASDAQ_{-90}$	6.4%	8.8%	–0.1%	6.5%	11.9%

Panel D: Mean value comparison of EGC and large issuers.

	EGC Issuers	Large Issuers	Difference	T-stat
$ARET[D]$	0.179	0.103	0.076***	(2.65)
$ARET[W]$	0.182	0.132	0.050*	(1.73)
$ARET[M]$	0.214	0.147	0.067*	(1.82)
$IPO\ Value/Revenues$	8.876	1.023	7.853***	(4.36)
$I(Value\ Destruction)$	0.572	0.389	0.183***	(4.82)
$Instit.Owner.$	0.561	0.756	–0.195***	(–4.89)
Age	17.1	51.3	–34.2***	(–5.86)
$AT\ (\$MN)$	407.6	11,276.0	–10,868.4**	(–2.48)
$Revenues\ (\$MN)$	153.9	5,380.2	–5,226.3***	(–5.17)
$Proceeds\ (\$MN)$	153.2	864.9	–711.7***	(–6.36)
$\%Retained$	0.723	0.749	–0.026	(–0.68)
$\%\Delta(Offer\ Price)$	–0.025	–0.040	0.014	(0.67)
$Days\ to\ IPO$	103.8	165.2	–61.4***	(–6.15)
$Return\ on\ Assets$	–0.229	0.016	–0.246*	(–1.87)
$R\&D\ Intensity$	0.245	0.009	0.235**	(2.15)
$CAPEX\ Intensity$	0.062	0.032	0.031**	(2.34)
$I(NI < 0)$	0.617	0.347	0.269**	(2.01)
$I(BV < 0)$	0.608	0.263	0.345***	(4.17)
$I(R\&D > 0)$	0.651	0.274	0.378***	(2.71)
$I(VC)$	0.581	0.011	0.570***	(4.33)
$I(Soft\ Tech)$	0.182	0.011	0.172	(1.11)
$I(Bio\ Tech)$	0.271	0.032	0.240	(1.40)
$I(HQU)$	0.469	0.674	–0.205***	(–3.06)
$I(BIG4)$	0.811	0.979	–0.168***	(–3.40)
$\#\ IPO_{-90}$	58.0	56.6	1.4	(0.95)
$NASDAQ_{-90}$	0.062	0.073	–0.011***	(–2.76)

This table provides descriptive statistics for our sample of 677 U.S. IPOs from January 1, 2009, to December 31, 2015. Panel A reports aggregate statistics separately for the treatment group of EGC issuers and the control group of large issuers. Panel B reports the sample distribution across two-digit GICS sectors. Panel C reports the pooled empirical distributions of key variables. Panel D compares the mean values of outcome variables and issuer characteristics for EGC and large issuers. We report T-statistics in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively, using two-tailed tests. Standard errors are clustered by two-digit GICS code and IPO month. Appendix 2 provides detailed variable definitions.

Table 2
Title I provision frequencies

Panel A: Title I provision frequencies across years.

		De-Risking Provisions		De-Burdening Provisions					Use of Provisions	
IPO Year	Obs.	TTW	Confid.	Red. Acc.	Red. Comp.	Omit CDA	Delay SOX	Delay GAAP	Sum of Provisions	% Of ≥4 Provisions
2012	15	73%	67%	13%	80%	73%	100%	7%	4.1	67%
2013	112	87%	92%	35%	91%	97%	100%	15%	5.1	96%
2014	153	92%	96%	54%	97%	98%	100%	9%	5.4	98%
2015	100	96%	95%	48%	96%	99%	100%	8%	5.3	99%
Pooled	380	91%	93%	45%	94%	97%	100%	11%	5.2	97%

Panel B: Title I provision frequencies across sectors.

		De-Risking Provisions		De-Burdening Provisions					Use of Provisions	
GICS Sector	Obs.	TTW	Confid.	Red. Acc.	Red. Comp.	Omit CDA	Delay SOX	Delay GAAP	# Of Provisions	% Of ≥4 provisions
Healthcare	171	99%	97%	74%	96%	99%	100%	9%	5.7	99%
Information Tech.	84	88%	94%	10%	90%	96%	100%	6%	4.8	95%
Financials	44	73%	84%	27%	89%	91%	100%	11%	4.7	89%
Consumer Discret.	42	83%	93%	24%	93%	95%	100%	5%	4.9	95%
Energy	13	77%	85%	46%	100%	100%	100%	15%	5.2	100%
Industrials	11	82%	91%	36%	100%	100%	100%	36%	5.5	100%
Consumer Staples	6	100%	100%	33%	100%	83%	100%	17%	5.2	100%
Utilities	6	100%	83%	50%	100%	100%	100%	50%	5.8	100%
Materials	3	67%	67%	0%	100%	100%	100%	67%	5.0	100%
Pooled	380	91%	93%	45%	94%	97%	100%	11%	5.2	97%

This table reports the frequency distribution of EGC issuers electing each provision afforded by Title I of the JOBS Act over time (Panel A) and across two-digit GICS sectors (Panel B). The post-JOBS Act sample includes 380 EGC issuers from April 5, 2012, to December 31, 2015, Appendix 1 provides a detailed description of the Title I provisions.

Table 3
IPO aftermarket returns: Portfolio analysis

Panel A: EGC issuers.

	Obs.	IPO returns		
		$ARET[D]$	$ARET[W]$	$ARET[M]$
(a) EGC Pre	202	0.135***	0.128***	0.132***
		(3.29)	(2.99)	(2.80)
(b) EGC Post	380	0.202***	0.211***	0.258***
		(7.70)	(8.00)	(7.28)
(b) − (a)		**0.067****	**0.084****	**0.126****
		(2.18)	(2.22)	(2.06)

Panel B: Large issuers.

	Obs.	IPO returns		
		$ARET[D]$	$ARET[W]$	$ARET[M]$
(a) Large Pre	39	0.064***	0.084***	0.080***
		(3.48)	(7.49)	(5.73)
(b) Large Post	56	0.130***	0.166***	0.194***
		(4.87)	(6.49)	(5.97)
(b) − (a)		**0.066*****	**0.082*****	**0.114*****
		(3.24)	(3.03)	(3.20)

This table explores variation in the IPO aftermarket returns for our sample of EGC and large issuers pre- and post-JOBS Act. The pre-period is from the beginning of 2009 to April 4, 2012, and the post-period is from April 5, 2012, to December 31, 2015. We measure buy-and-hold market-adjusted returns from the IPO offer price to the closing price at the end of the first day (D), first week (W), and first month of trading (M). We report T-statistics in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively, using two-tailed tests. Standard errors are clustered by two-digit GICS code and IPO month. The sample includes 677 U.S. IPOs from January 1, 2009, to December 31, 2015.

Table 4
IPO aftermarket returns: DID regression analysis

	$Dependent\ Variable =$					
	$ARET[D]$		$ARET[W]$		$ARET[M]$	
	(1)	(2)	(3)	(4)	(5)	(6)
$\boldsymbol{EGC \times POST}$	**0.001**	**-0.027**	**0.002**	**–0.014**	**0.012**	**–0.035**
	(0.02)	(–0.58)	(0.05)	(–0.26)	(0.21)	(–0.52)
EGC	0.072*	–0.011	0.044	–0.036	0.052	–0.047
	(1.88)	(–0.25)	(1.13)	(–0.96)	(1.15)	(–0.78)
$POST$	0.066***	0.050	0.082***	0.066**	0.114***	0.119***
	(3.28)	(1.57)	(3.06)	(1.97)	(3.23)	(2.64)
Issuer Characteristics	No	Yes	No	Yes	No	Yes
Sector Fixed Effects	No	Yes	No	Yes	No	Yes
Adj. R^2	2%	27%	2%	23%	3%	18%
Obs.	677	677	677	677	677	677

This table reports DID regression results zeroing on the differential pre-post JOBS Act change between the treatment group of EGC issuers (582 cases) and the control group of large issuers (95 cases). The set of left-hand-side variables includes the buy-and-hold market-adjusted returns from the IPO offer price to the closing price at the end of the first day (D), first week (W), and first month of trading (M). The set of right-hand-side variables includes the indicator for EGC issuers (EGC), the indicator for the post-JOBS Act period ($POST$), the interaction $EGC \times POST$, a vector of issuer characteristics described in Appendix 2 (also itemized in Section 1.3.1), and sector fixed effects based on two-digit GICS codes. We report T-statistics in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively, using two-tailed tests. Standard errors are clustered by two-digit GICS code and IPO month. The sample includes 677 U.S. IPOs from January 1, 2009, to December 31, 2015.

Table 5
Pre-IPO valuation multiples: Portfolio analysis

Panel A: Mean values.

	EGC Issuers		Large Issuers	
	Obs.	$IPO\ Value/\ Revenues$	Obs.	$IPO\ Value/\ Revenues$
(a) Pre	174	6.261*** (7.70)	39	0.910*** (7.96)
(b) Post	273	10.543*** (4.37)	56	1.102*** (11.45)
(b) − (a)		**4.282**** (2.28)		**0.192*** (1.81)

Panel B: Median values.

	EGC Issuers		Large Issuers	
	Obs.	$IPO\ Value/\ Revenues$	Obs.	$IPO\ Value/\ Revenues$
(a) Pre	174	3.645*** (0.00)	39	0.725*** (0.00)
(b) Post	273	4.946*** (0.00)	56	0.901*** (0.00)
(b) − (a)		**1.301***** (0.01)		**0.177** (0.32)

This table explores variation in the pre-IPO revenue multiples of EGC and large issuers pre- and post-JOBS Act. The pre-period is from the beginning of 2009 to April 4, 2012, and the post-period is from April 5, 2012, to December 31, 2015. We measure the pre-IPO revenue multiple as the ratio of IPO value divided by pre-IPO revenues. We measure IPO value as the product of the offer price times the number of shares outstanding in the company across all shares after the IPO. To mitigate the effect of influential observations, we require pre-IPO revenues to be at least $10MN. Panel A (Panel B) presents mean (median) values and report two-tailed t-values (p-values) in parentheses obtained from t-tests (Wilcoxon signed-rank tests). ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively. Standard errors are clustered by two-digit GICS code and IPO month. The sample includes 542 IPOs from January 1, 2009, to December 31, 2015.

Table 6
Pre-IPO valuation multiples: DID regression analysis

	$Dependent\ Variable = IPO\ Value/\ Revenues$	
	(1)	(2)
$\boldsymbol{EGC \times POST}$	**4.090****	**3.802**
	(2.16)	(1.33)
EGC	5.351***	–10.146*
	(6.61)	(–1.67)
$POST$	0.192*	–4.819
	(1.83)	(–1.59)
Issuer Characteristics	No	Yes
Sector Fixed Effects	No	Yes
Adj. R^2	3%	35%
Obs.	542	542

This table reports DID regression results zeroing on the differential pre-post JOBS Act change in pre-IPO revenue multiples between the treatment group of EGC issuers with pre-IPO revenues of at least $10MN (447 cases) and the control group of large issuers (95 cases). The set of left-hand-side variable is pre-IPO revenue multiples. The set of right-hand-side variables includes the indicator for EGC issuers (EGC), the indicator for the post-JOBS Act period ($POST$), the interaction $EGC \times POST$, a vector of issuer characteristics described in Appendix 2, and sector fixed effects based on two-digit GICS codes. We report T-statistics in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively, using two-tailed tests. Standard errors are clustered by two-digit GICS code and IPO month. The sample period is from January 1, 2009, to December 31, 2015.

Table 7
Reduced-accounting EGC issuers: Variation in pre-IPO valuation multiples, shareholder value destruction, and institutional ownership

Panel A: Portfolio analysis.

	$IPO\ Value\ /\ Revenues$		$I(Value\ Destruction)$		$Instit.Owner.$	
	Obs.	Mean Values	Obs.	Mean Values	Obs.	Mean Values
(a) EGC Pre	174	6.261*** (7.70)	202	0.589*** (40.94)	187	0.513*** (17.21)
(b) EGC Post| RA = 0	188	6.996*** (5.50)	208	0.486*** (13.39)	203	0.623*** (19.25)
(c) EGC Post| RA = 1	85	18.389*** (3.82)	172	0.657*** (94.31)	168	0.539*** (32.07)
(b) − (a)		**0.735** (0.99)		**–0.104***** (–6.55)		**0.110*** (1.93)
(c) − (b)		**11.394**** (2.47)		**0.171***** (3.46)		**–0.083***** (–3.49)

Panel B. DID regression analysis.

	$Dependent\ Variable =$					
	$IPO\ Value/\ Revenues$		$I(Value\ Destruction)$		$Instit.Owner.$	
	(1)	(2)	(3)	(4)	(5)	(6)
$\boldsymbol{EGC \times POST \times RA}$	**11.394****	**4.373**	**0.171*****	**0.099**	**–0.083*****	**–0.024**
	(2.48)	(1.33)	(3.48)	(1.32)	(–3.50)	(–1.25)
$\boldsymbol{EGC \times POST}$	**0.542**	**2.701**	**–0.025**	**0.034**	**0.041**	**–0.042**
	(0.71)	(1.20)	(–0.32)	(0.37)	(0.46)	(–0.50)
EGC	5.351***	–9.200*	0.153***	0.040	–0.201***	0.039
	(6.60)	(–1.68)	(3.01)	(0.34)	(–4.21)	(0.65)
$POST$	0.192*	–4.669	–0.079	–0.127	0.068	0.084
	(1.83)	(–1.60)	(–0.86)	(–1.18)	(0.97)	(1.16)
Issuer Characteristics	No	Yes	No	Yes	No	Yes
Sector Fixed Effects	No	Yes	No	Yes	No	Yes
Adj. R^2	7%	35%	3%	6%	8%	23%
Obs.	542	542	677	677	643	643

This table provides evidence of variation in pre-IPO revenue multiples, variation in the frequency of shareholder value destruction, and institutional ownership. We measure the pre-IPO revenue multiple as the ratio of IPO value divided by pre-IPO revenues. To mitigate the effect of influential observations, we require revenues to be at least $10MN. We create an indicator variable of long-term value destruction for IPOs that underperform the stock market index in the three years after going public measured from the offering price $I(Value\ Destruction)$. We measure institutional ownership as the fraction of shares outstanding held by institutions that report their quarterly holdings in SEC Form 13F and N-30Ds. We use the average level of institutional ownership over the three-year period starting from the IPO date. Panel A of Table 7 reports average values across three groups of issuers: (a) the pre-JOBS Act group of issuers below the $1BN revenue cutoff that would have qualified for EGC status had the Act been in effect at the time of their IPO ($EGC\ Pre$), (b) the post-JOBS Act group of EGC issuers that did not adopt the reduced-accounting disclosure provision ($EGC\ Post|\ RA = 0$), and (c) the post-JOBS Act group of EGC issuers that adopted the reduced-accounting disclosure provision ($EGC\ Post|\ RA = 1$). Panel B of Table 7 reports DID regression results. The slope coefficient on the triple interaction $EGC \times POST \times RA$ captures the post-JOBS Act difference between the group of reduced-accounting EGC issuers and the group of non-reduced-accounting EGC issuers. Appendix 2 provides detailed variable definitions. We report T-statistics in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively, using two-tailed tests. Standard errors are clustered by two-digit GICS code and IPO month. The sample includes 677 U.S. IPOs from 2009 to 2015.

Table 8
Reduced-accounting vs. non-reduced-accounting EGCs

	$RA = 0$	$RA = 1$	Difference	t-stat
Age	17.120	13.140	–3.981	(–1.29)
AT (\$MN)	521.275	240.447	–280.828*	(–1.72)
$Revenues$ (\$MN)	225.487	49.060	–176.427***	(–3.80)
$Proceeds$ (\$MN)	183.400	111.822	–71.578***	(–3.63)
$\%Retained$	0.763	0.706	–0.057***	(–2.79)
$\%\Delta(Offer\ Price)$	–0.010	–0.044	–0.034***	(–3.39)
$Days\ to\ IPO$	65.683	52.105	–13.578**	(–2.01)
$Return\ on\ Assets$	–0.135	–0.467	–0.331***	(–4.11)
$R\&D\ Intensity$	0.171	0.424	0.252***	(3.66)
$CAPEX\ Intensity$	0.066	0.050	–0.016*	(–1.79)
$I(NI < 0)$	0.567	0.826	0.258***	(3.03)
$I(BV < 0)$	0.558	0.779	0.221*	(1.89)
$I(R\&D > 0)$	0.582	0.791	0.209	(1.28)
$I(VC)$	0.500	0.721	0.221	(1.60)
$I(Soft\ Tech)$	0.284	0.029	–0.255	(–1.44)
$I(Bio\ Tech)$	0.120	0.628	0.508***	(5.84)
$I(HQU)$	0.548	0.285	–0.263***	(–3.21)
$I(BIG4)$	0.822	0.808	–0.014	(–0.35)
$\#\ IPO_{-90}$	60.125	62.407	2.282***	(3.55)
$NASDAQ_{-90}$	0.049	0.044	–0.005	(–0.92)

This table compares the mean values of issuer characteristics for EGCs that did not adopt the reduced-accounting disclosure provision afforded by the JOBS Act ($RA = 0$) and EGCs that took advantage of the reduced-accounting provision ($RA = 1$). The reduced-accounting provision allows companies to present only two years of audited financial statements and two years of selected financial data, rather than the previously required three years of audited financial statements and five years of selected financial data. Appendix 2 describes in detail all issuer characteristics. We report T-statistics in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively, using two-tailed tests. Standard errors are clustered by two-digit GICS code and IPO month. The sample includes 208 non-reduced-accounting EGCs and 172 reduced-accounting EGCs from April 5, 2012, to December 31, 2015.

Table 9
Predicting reduced-accounting EGCs

	AUC	Accuracy	Precision	Recall
Naive Bayes	0.91	0.83	0.86	0.74
AdaBoost	0.90	0.83	0.84	0.76
Random Forest	0.89	0.82	0.86	0.71
Logistic Regression	0.88	0.82	0.88	0.68
SVM	0.84	0.83	0.84	0.76
K-Nearest Neighbor	0.82	0.83	0.86	0.74

This table reports reduced-accounting EGC prediction results. We present the area under the ROC curve (AUC), accuracy, precision, and recall scores for various machine learning classifiers. Specifically, we use Naive Bayes, AdaBoost, Random Forest, Logistic Regression, Support Vector Machine (SVM), and K-Nearest Neighbor algorithms. The set of features includes a vector of issuer characteristics described in Appendix 2 (also itemized in Section 1.3.1). The sample includes 208 non-reduced-accounting EGCs and 172 reduced-accounting EGCs from April 5, 2012, to December 31, 2015. From this sample, we randomly choose 80% of EGCs (304 issuers) to train our models and use the rest (76 issuers) to evaluate them.

Table A1
IPO aftermarket returns: DID regression analysis complete output

	$Dependent\ Variable =$					
	$ARET[D]$		$ARET[W]$		$ARET[M]$	
$\boldsymbol{EGC \times POST}$	**–0.027**	**(–0.58)**	**–0.014**	**(–0.26)**	**–0.035**	**(–0.52)**
EGC	–0.011	(–0.25)	–0.036	(–0.96)	–0.047	(–0.78)
$POST$	0.050	(1.57)	0.066^{**}	(1.97)	0.119^{***}	(2.64)
log (Age)	–0.003	(–0.20)	0.000	(–0.02)	0.001	(0.06)
log (AT)	–0.020	(–1.54)	–0.014	(–1.20)	–0.008	(–0.43)
log ($Revenues$)	–0.006	(–0.31)	–0.008	(–0.50)	–0.020	(–1.03)
Log(*Proceeds*)	0.027	(0.67)	0.021	(0.61)	0.015	(0.46)
$\%Retained$	0.106	(0.78)	0.091	(0.73)	0.065	(0.49)
$\%\Delta(Offer\ Price)$	0.680^{***}	(9.60)	0.643^{***}	(9.13)	0.539^{***}	(6.12)
$log(Days\ to\ IPO)$	$–0.044^{**}$	(–2.27)	–0.037	(–1.37)	$–0.045^{*}$	(–1.67)
$Return\ on\ Assets$	0.109^{**}	(2.14)	0.179^{***}	(4.11)	0.136^{***}	(3.69)
$R\&D\ Intensity$	$–0.087^{***}$	(–3.01)	–0.029	(–0.70)	$–0.086^{*}$	(–1.95)
$CAPEX\ Intensity$	0.050	(0.22)	–0.026	(–0.18)	0.050	(0.44)
$I(NI < 0)$	0.009	(0.50)	0.005	(0.20)	$–0.034^{**}$	(–2.06)
$I(BVE < 0)$	0.013	(0.41)	0.013	(0.42)	0.027	(0.84)
$I(R\&D > 0)$	–0.051	(–1.58)	–0.038	(–0.97)	–0.029	(–0.67)
$I(VC)$	0.081^{***}	(4.05)	0.088^{**}	(2.58)	0.096	(1.51)
$I(Soft\ Tech)$	0.077^{***}	(3.82)	0.021	(1.10)	–0.017	(–0.67)
$I(Bio\ Tech)$	0.058	(1.28)	0.048	(1.06)	0.108^{**}	(2.14)
$I(NASDAQ)$	0.108^{***}	(2.63)	0.122^{**}	(2.20)	0.171^{***}	(2.74)
$I(NYSE)$	0.066	(1.48)	0.072^{*}	(1.72)	0.125^{**}	(2.56)
$I(HQU)$	0.036	(1.63)	0.030	(1.06)	0.047	(1.52)
$I(BIG4)$	0.012	(0.65)	–0.001	(–0.03)	0.020	(0.94)
$\#\ IPO_{-90}$	0.000	(0.33)	0.000	(0.32)	0.000	(–0.56)
$NASDAQ_{-90}$	0.258^{***}	(3.53)	0.160^{**}	(2.04)	0.209^{*}	(1.74)
Sector Fixed Effects	Yes		Yes		Yes	
Adj. R^2	27%		23%		18%	
Obs.	677		677		677	

This table reports DID regression results for our sample of EGC and large issuers pre- and post-JOBS Act. The set of left-hand-side variables includes the buy-and-hold market-adjusted returns from the IPO offer price to the closing price at the end of the first day (D), first week (W), and first month of trading (M). The set of right-hand-side variables includes the indicator for EGC issuers (EGC), the indicator for the post-JOBS Act period ($POST$), the interaction $EGC \times POST$, a vector of issuer characteristics described in Appendix 2, and sector fixed effects based on two-digit GICS codes. We report T-statistics in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively, using two-tailed tests. Standard errors are clustered by two-digit GICS code and IPO month. The sample includes 677 U.S. IPOs from January 1, 2009, to December 31, 2015.

Table A2
First-day returns: Additional robustness tests

	Dependent Variable $= ARET[D]$					
	P-Score matched		Exclude mega issuers		Exclude unicorn IPOs	
	(1)	(2)	(3)	(4)	(5)	(6)
$\boldsymbol{EGC \times POST}$	**0.004**	**0.008**	**–0.007**	**–0.018**	**–0.007**	**–0.024**
	(0.21)	(0.21)	(–0.19)	(–0.36)	(–0.21)	(–0.54)
EGC	0.070^{*}	0.022	0.068^{*}	–0.019	0.072^{*}	–0.012
	(1.67)	(0.51)	(1.72)	(–0.37)	(1.88)	(–0.27)
$POST$	0.034^{***}	0.032	0.074^{***}	0.038	0.065^{***}	0.050
	(2.62)	(1.51)	(3.69)	(1.06)	(3.18)	(1.44)
Issuer Characteristics	No	Yes	No	Yes	No	Yes
Sector Fixed Effects	No	Yes	No	Yes	No	Yes
Adj. R^2	1%	29%	2%	27%	1%	27%
Obs.	452	452	666	666	664	664

This table reports DID regression results zeroing on the differential pre-post JOBS Act change in first-day returns. The sample in columns (1) and (2) includes propensity-score matched EGC and large issuers. We match pre- with post-JOBS Act issuers separately in the treatment and control groups using nearest-neighbor propensity-score matching (without replacement) by sector. We estimate the propensity scores using the entire vector $C_{k,i}$ of issuer characteristics. The sample in columns (3) and (4) excludes large issuers with pre-IPO revenues in excess of \$10BN. The sample in columns (5) and (6) excludes unicorn IPOs. Using data from CB Insights' tracker of billion-dollar VC-backed exits, we identify 12 Unicorn non-SRC EGC issuers and 1 Unicorn large issuer between November 2, 2013, and December 31, 2015. The sample period is from January 1, 2009, to December 31, 2015. The set of right-hand-side variables includes the indicator for EGC issuers (EGC), the indicator for the post-JOBS Act period ($POST$), the interaction $EGC \times POST$, a vector of issuer characteristics described in Appendix 2, and sector fixed effects based on two-digit GICS codes. We report T-statistics in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively, using two-tailed tests. Standard errors are clustered by two-digit GICS code and IPO month.

Table A3
First-day returns: Relation to Barth et al. (2017)

	Dependent Variable $= ARET[D]$					
	Include SRCs in our treatment group		Restrict pre-post period		Robust regression	
	(1)	(2)	(3)	(4)	(5)	(6)
$\boldsymbol{EGC \times POST}$	**0.005**	**–0.005**	**–0.019**	**–0.061**	**–0.027**	**–0.001**
	(0.18)	(–0.12)	(–0.44)	(–1.05)	(–1.09)	(–0.04)
EGC	0.066*	–0.036	0.070*	–0.007	0.052***	0.022
	(1.93)	(–0.82)	(1.89)	(–0.17)	(2.93)	(0.86)
$POST$	0.066***	0.057*	0.121***	0.081**	0.040**	0.038**
	(3.28)	(1.69)	(3.89)	(2.18)	(2.13)	(1.99)
Issuer Characteristics	No	Yes	No	Yes	No	Yes
Sector Fixed Effects	No	Yes	No	Yes	No	Yes
Adj. R^2	2%	24%	4%	32%	0%	31%
Obs.	741	741	387	387	677	677

This table reports DID regression results zeroing on the differential pre-post JOBS Act change in first-day returns. In columns (1) and (2), we include SRCs in the treatment group of EGC issuers. In columns (3) and (4), we restrict our baseline sample within the period between July 1, 2009, and December 31, 2013. In columns (5) and (6), we report robust regression results based on Yohai's (1987) MM-estimator for our baseline sample. Appendix 2 provides the variable definitions. We report T-statistics in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively, using two-tailed tests. Standard errors are clustered by two-digit GICS code and IPO month.

Table A4
First-day returns: Relation to Chaplinsky et al. (2017)

	$Dependent\ Variable\ = ARET[D]$			
	Use below \$75MN proceed issuers as only control group		Use SRC issuers as only control group	
	(1)	(2)	(3)	(4)
$\boldsymbol{EGC \times POST}$	**0.131****	**0.100**	**–0.038**	**–0.055**
	(2.29)	(1.54)	(–0.44)	(–0.60)
EGC	0.072**	0.038	0.054	–0.050
	(2.08)	(1.14)	(0.93)	(–0.88)
$POST$	–0.003	0.003	0.105	0.096
	(–0.13)	(0.07)	(1.50)	(0.99)
Issuer Characteristics	No	Yes	No	Yes
Sector Fixed Effects	No	Yes	No	Yes
Adj. R^2	9%	25%	1%	24%
Obs.	646	646	646	646

This table reports DID regression results zeroing on the differential pre-post JOBS Act change in first-day returns. The sample in columns (1) and (2) consists of SRC EGCs, defined as IPO issuers with gross proceeds below \$75MN as the control group and non-SRC EGC issuers as the treatment group. The sample in columns (3) and (4) consists of SRC EGCs, identified using hand-collected information directly from the IPO registration statement, as the control group and non-SRC EGC issuers as the treatment group. The sample period is from January 1, 2009, to December 31, 2015. The set of right-hand-side variables includes the indicator for EGC issuers (EGC), the indicator for the post-JOBS Act period ($POST$), the interaction $EGC \times POST$, a vector of issuer characteristics described in Appendix 2, and sector fixed effects based on two-digit GICS codes. We report T-statistics in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively, using two-tailed tests. Standard errors are clustered by two-digit GICS code and IPO month.

Chapter 2

Operating Lease Capitalization and Managerial Leasing Decisions

2.1. Introduction

Operating leases are a popular source of corporate financing. In 2016, the International Financial Reporting Standard (IFRS) Foundation estimated that US public firms held approximately $1 trillion in off-balance-sheet operating lease liabilities (IFRS 2016). Under Statement of Financial Accounting Standard No. 13, Accounting for Leases (hereafter "the legacy standard"), operating leases were disclosed as future payments in footnotes, but they were not capitalized on balance sheets. Some financial statement users asserted that the discrete reporting benefits of off-balance-sheet operating leases, such as the appearance of lower debt-to-equity ratios, materially incentivized managers to use them. Their argument prompted claims that the transparency of underlying economic activities was compromised (SEC 2005; FASB 2016). To address these concerns, the Financial Accounting Standards Board (FASB) issued a new lease standard that mandates operating lease capitalization: Accounting Standards Update 2016-02, Leases (Topic 842), codified as ASC 842 (hereafter "ASC 842" or "the new standard").

In this study, I examine the effect of ASC 842's adoption on firms' leasing decisions to discern whether the transition from footnote disclosure to balance sheet recognition has real effects on managerial behavior. Specifically, I study whether firms' operating lease use decreases and whether firms substitute operating leases with other financing sources upon ASC 842's implementation. Investigating these questions bears policy implications, as the FASB is currently conducting a post-implementation review of the new standard (FASB 2020). To aid the evaluation process, the FASB issued a call for accounting scholars to examine whether firms' operating lease use changed upon the standard's adoption and whether operating leases were replaced by asset purchases or short-term and variable leases (Botosan 2021 pp. 33 & 36). My study responds directly to the call by examining these questions.

The mechanism by which ASC 842 affects leasing decisions is simple. Under the legacy standard, managers may have believed that financial statement users do not properly account for operating lease footnote disclosures due to disclosure processing costs (Blankespoor et al. 2020). In this case, managers may have utilized operating leases for reporting benefits (e.g., the presentation of lower debt-to-equity ratios). However, the capitalization of operating leases eliminates managers' perceived reporting benefits. As a result, managers may reduce their use of operating leases and substitute them with other financing sources.

In contrast, managers may not have believed that using off-balance-sheet operating leases offers meaningful reporting benefits. The vast majority of prior studies on operating leases document that financial statement users incorporate the information in operating lease footnote disclosures into their decision-making process (Wilkins and Zimmer 1983; Ely 1995; Dhaliwal et al. 2011; Altamuro et al. 2014; Kraft 2015). Consistent with this evidence, ASC 842's opponents contended that the capitalized amounts would not furnish new information to financial statement users because footnote disclosures adequately represent a firm's leasing activity.[22] Considering these arguments, managers may not have used operating leases for reporting benefits, in which case the capitalization mandate may subsequently have no effect on leasing decisions. Therefore, the impact of capitalization on managerial leasing decisions is an open empirical question.

To study the effect of ASC 842's implementation on operating lease use, I exploit the staggered nature of the new standard's adoption and apply a difference-in-differences (DID) research design. ASC 842 applies to fiscal years beginning after December 15, 2018. The first firms to adopt are calendar year (CY) firms whose fiscal year runs from January 2019–December 2019. I follow prior studies on the adoption of other standards (Ferri et al. 2018; Gipper 2021) and consider these firms as treatment firms. All other firms—non-calendar year (Non-CY) firms—are used as control firms; these firms adopt the new standard in fiscal years ending at some point in 2020. I define year based on the fiscal year-end date and end the sample in 2019. Therefore, the only firm-year observations in my main sample that are affected by ASC 842 are 2019 CY firms. To account for systematic differences between CY and non-CY firms, I follow prior studies and use entropy-balanced and propensity score-matched (PSM) samples that have similar firm characteristics between the two groups (e.g., Ferri et al. 2018; Dambra et al. 2020).

To study the real effects of disclosure vs. recognition, I examine the effects of adoption rather than those catalyzed by ASC 842's announcement in 2016. Since managers can anticipate adoption, they may modify leasing decisions before the adoption year. A failure to account for these preliminary reactions would bias statistical tests *against* observing a reduction in operating lease use upon adoption. Nonetheless, given the lack of material incentive to forego reporting benefits early, I expect that managers will likely begin to change reporting-related leasing decisions in the adoption year.[23] This supposition is reinforced by studies that find managers often make myopic investment decisions based on reporting incentives (e.g., Kraft et al. 2018).

The main sample consists of 6,279 firm-year observations from 2016 to 2019 for 1,640 publicly traded non-financial US firms that are not a penny stock and that have enough operating leases to merit reporting on financial statements. The primary measure of operating leases is *OperLease*, which is defined as one-year-ahead operating lease payments multiplied by 100 and scaled by beginning total assets.

The main DID analysis shows that upon ASC 842's adoption, there is a marked decline in CY firms' use of operating leases relative to that of non-CY firms. This reduction suggests that disclosure location (footnote vs. balance sheet) has real effects on leasing decisions. I also examine whether other financing sources replace operating leases. I find no evidence of changes in the use of comprehensive financing sources—which consist of operating leases, asset purchases, finance

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22 Echoing these sentiments, Disney contended that "investor needs are being sufficiently met with the existing lease accounting model (i.e., disclosure of committed operating lease payments)." Many other companies, such as Bank of America, GAP Inc., and Hilton, agreed with Disney's assertion.

23 To illustrate this scenario, suppose that prior to the new standard, a firm was leasing primarily for reporting benefits. Although assets and liabilities will eventually increase post-ASC 842, the firm can still benefit from reporting lower assets and liabilities before its adoption. Thus, the firm has no reason to act before the adoption year to reduce operating leases and replace them with debt-financed assets, since doing so would mean that assets and liabilities would increase prematurely. Similarly, the firm is unlikely to substitute operating leases with short-term or variable leases before the adoption year, because the latter types offer the same reporting benefits as operating leases but are often more costly.

leases, short-term leases (leases with an initial term of 12 months or less), and variable leases (leases with payments that depend on an economic outcome, such as sales).[24] This result suggests that the reduction in operating leases is offset by an increase in alternate financing sources. Indeed, I document that the non-operating lease portion of the comprehensive financing sources increases significantly upon ASC 842's implementation.

In the next set of results, I examine whether the elimination of perceived off-balance-sheet reporting benefits is the channel through which ASC 842 affects managerial leasing decisions. To do so, I explore heterogeneity in ASC 842's impact on leasing decisions. I document that the decline in operating lease use and the substitution effects are driven by lease-intensive firms, i.e., those that likely benefited most from off-balance-sheet treatment of operating leases. I find that the economic magnitude of operating lease reduction amongst these firms is significant enough to materially affect their debt-to-equity, and thus appropriate for the reporting channel explanation.

I also explore the cross-sectional variation in ASC 842's impact across debt-to-equity partitions. I illustrate that a firm's debt-to-equity ratio—the ratio that is presumably most relevant for managers who utilize operating leases for reporting benefits—is more sensitive to operating lease changes for less-levered firms. This pattern suggests that less-levered (more-levered) firms are more (less) incentivized to alter leasing decisions for reporting reasons. Further, more-levered firms are more likely to use operating leases to expand debt capacity—one of the most important non-reporting reasons for leasing (Eisfeldt and Rampini 2009). Considering Caskey and Ozel's (2019) finding that non-reporting incentives play a primary role in leasing decisions, reporting incentives are only a second-order consideration. For these reasons, I expect attenuated ASC 842 effects for more-levered firms and more significant effects for less-levered firms.

In contrast, to the extent that banks do not consider operating lease disclosures when drafting debt covenants, it is possible that managers reduce operating leases to avoid violating covenants (Cornaggia et al. 2015). If this alternative debt contracting channel motivates the changes in leasing decisions that I report, I should observe a greater reduction in operating leases for more-levered firms since they are more likely to verge on covenant thresholds.

Consistent with the reporting channel argument, I document that changes in managerial leasing decisions are driven by less-levered firms. This finding is consistent with prior literature that provides evidence that banks utilize operating lease footnote disclosures (e.g., Wilkins and Zimmer 1983; Altamuro et al. 2014). Therefore, debt contracting is unlikely a viable explanation for the changes in leasing decisions.

In the last set of analyses, I determine which sources replace operating leases. I find no evidence that managers shift toward asset purchases or finance leases. Given this lack of evidence, the last possible sources of substitution among comprehensive financing sources are short-term and variable leases. Importantly, these leases are permitted to be left off balance sheet under both the legacy and the new standards. I find that, upon ASC 842's adoption, CY firms' use of short-term and variable lease expenses increases significantly. Overall, these results indicate that managers transition from operating leases (because they no longer offer reporting benefits) to

[24] ASC 842 changes the designation of capital leases to finance leases. For consistency, I use the term finance lease to refer to all non-operating capitalized leases. In addition, short-term leases and variable leases are both considered operating leases under ASC 842. However, to simplify the discussion, I refer to operating leases as leases that are neither short-term, variable, or finance.

short-term and variable leases (because they do) and thus provide further support for the reporting channel. In summary, this study's findings suggest that the recognition of operating leases on balance sheets has a significant impact on managerial leasing decisions.

2.2. Existing research, background, and hypothesis

2.2.1. This study's contribution to existing research

The FASB's call for research regarding ASC 842's real effects highlights the importance and relevance of my research question. In this section, I further substantiate its significance by detailing my contribution to related streams of literature.

One of the studies most closely related to mine is Christensen et al. (2021), who also examine the real effects of ASC 842. They show that firms that utilize operating leases invest more efficiently in the year preceding the standard's adoption. My study is distinct in two ways: (i) I focus on managerial decisions following the standard's adoption and (ii) examine firms' use of operating leases for reporting reasons.

More broadly, I contribute to the literature on recognition vs. disclosure. Most prior studies concentrate on whether and when the market treats disclosed values differently than recognized values (e.g., Aboody 1996; Davis-Friday et al. 1999; Ahmed et al. 2006; Bratten et al. 2013; Michels 2017). While some studies also consider the real effects of recognition vs. disclosure, they focus on *income statement* recognition, e.g., Skantz's (2012) investigation of the impact of income statement recognition of stock options on CEO compensation. As distinct from these studies, I contribute to the literature by examining the real effects of *balance sheet* recognition.[25] As I highlight below, balance sheet vs. income statement recognition may have different implications. Thus, it is essential to study them separately.

One stream of literature on the real effects of recognition vs. disclosure concentrates on the consequences of lease capitalization. Imhoff and Thomas (1988) explore the effects of finance lease capitalization under the legacy standard's adoption and find that firms replace finance leases with operating leases. Unlike ASC 842, which only alters balance sheet figures (balance sheet effects), finance lease capitalization rules affect both balance sheet and income statement figures (balance sheet *and* income statement effects).[26] Furthermore, since finance leases front-load expenses, finance lease capitalization (capitalizing operating leases as finance leases) likely results in lower income for many companies.[27]

[25] Because the incorporation of disclosed information vs. recognized information during the decision-making process varies by user group (Imhoff et al. 1993), it is crucial to investigate managers' behavior alongside market reactions.
[26] As I discuss in Section 2.2.2, ASC 842 does not affect income and cash flow statements because accounting for operating leases in those statements is largely unchanged from the legacy standard (ASC 842-20-25-6).
[27] Operating lease costs are allocated on a straight-line basis and expensed in the income statement as a single lease cost. In contrast, finance leases are expensed as two components: interest and depreciation. Compared to operating leases, finance leases incur higher expenses in the earlier years of the lease term and lower expenses in the later years, as the interest component of the expense declines over time (El-Gazzar et al. 1986; Lipe 2001). El-Gazzar et al. (1986) explain that as long as leasing activities increase on a nominal basis, finance leases possess income disadvantages over operating leases because newer leases on an aggregate basis will be greater than older leases. They further explain that virtually all firms experienced nominal growth in leasing activity during the inflationary period around the mandatory capitalization of finance leases under the legacy standard in 1978.

It is vital to study balance sheet effects separately from income statement effects because they may have differing consequences. For example, Chen et al. (2019) also study finance lease capitalization rules and document their negative impact on investments. Whereas they examine lease capitalization's dual balance sheet and income statement effects, I isolate balance sheet effects and find no evidence that they alone result in lower investments. Thus, it can be inferred that while income statement effects lower investments, balance sheet effects do not.[28] Overall, I contribute to the literature on lease capitalization by isolating balance sheet effects and by examining how those effects distinctly drive managerial decisions.[29]

Similar to Imhoff and Thomas (1988), Caskey and Ozel (2019) examine lease-related reporting incentives. They find that non-reporting incentives (e.g., using operating leases to expand debt capacity) are the primary drivers of leasing decisions and that reporting incentives mostly play a secondary role. These findings make intuitive sense and highlight the importance of examining reporting incentives for a subset of firms that are less likely to use operating leases for non-reporting reasons. Indeed, I only document ASC 842's adoption effects for this subset.

In addition, I add to the nascent literature on ASC 842. Chatterjee (2020) and Ma and Thomas (2021) both find that operating lease use decreases during the transition period (2016–2018) relative to the years before the new standard's announcement in 2016 (i.e., they examine the effects of ASC 842's announcement). Chatterjee theorizes that managers may acquire new information about their own leases that motivates them to alter leasing plans throughout the transition period. This reasoning is consistent with that sustained in the literature on lease capitalization (Chen et al. 2019; Christensen et al. 2021). For my study, it is crucial to control the managerial learning channel as well as other factors that affect leasing decisions during the transition period. I do so by using CY and non-CY firms as treatment and control firms, respectively. Because managerial learning and other factors are common to both CY and non-CY firms during the transition period, the DID research design likely differences away the effects that Chatterjee (2020) and Ma and Thomas (2021) document (Roberts and Whited 2013).

In other contemporaneous studies, Milian and Lee (2020) find negative stock returns around the first quarter of operating lease capitalization. Hill et al. (2021) document a large increase in leverage upon ASC 842's implementation but find that the market reaction to this new information is marginal. Lastly, Binfare et al. (2020) discover that some firms manipulate discount rates when valuing the capitalized amount of operating leases. As distinct from these papers, I focus on the real effects of ASC 842's adoption.

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[28] In an untabulated analysis, I confirm that income statement effects are the driver of lower investments and reconcile my results with Chen et al. (2019). To perform this analysis, I exploit the main difference between the International Accounting Standard Board's new lease standard (IFRS 16) and ASC 842. Unlike ASC 842, IFRS 16 changes lease accounting in both balance sheet and income statements. I find that IFRS 16-adopting firms (firms that experience both balance sheet and income statement effects) show lower investment upon implementation of the new lease rules relative to ASC 842-adopting firms (firms that experience only balance sheet effects). Because the common component of balance sheet effects is likely nullified by the DID research design, the reduction in IFRS 16-adopting firms' investment can be attributed to income statement effects.

[29] Prior research on lease determinants constructively capitalize operating leases and study how as-if capitalized amounts correlate with various determinants (see Lipe (2001) and Spencer and Webb (2015) for reviews). Unlike their examination of off-balance-sheet operating leases, I assess the consequences of the shift from a footnote disclosure regime to a recognition regime.

2.2.2. Institutional background on ASC 842

The legacy standard mandated that companies capitalize finance leases and leave operating leases off balance sheet. Critics maintained that this provision allowed managers to employ operating leases opportunistically in order to avoid capitalizing liabilities (e.g., SEC 2005). In response to these claims, in 2006, the FASB and the International Accounting Standards Board (IASB) began updating the existing lease standard (FASB 2009). In February 2016, the FASB released Accounting Standards Update 2016-02, codified as ASC 842, which overhauled the legacy standard. ASC 842 became effective for public companies in fiscal years beginning after December 15, 2018, but early adoption was permitted.

The new standard's main innovation is the recognition of substantially all operating lease liabilities along with corresponding right-of-use operating lease assets. Specifically, companies are required to capitalize the present value of future operating lease payments. Although operating lease recognition bears a significant impact on balance sheets, it does not affect income statements since companies continue to expense leases in the same fashion as mandated by the legacy standard (ASC 842-20-25-6). They must also disclose information regarding leases (e.g., remaining lease terms and discount rates).[30]

To lower firms' transition costs to comply with ASC 842, the FASB introduced a set of practical expedients that, in effect, allow companies to "continue to account for leases that commence before the effective date in accordance with previous GAAP unless the lease is modified, except that companies are required to recognize a right-of-use asset and a lease liability for all operating leases" (FASB 2016).[31]

When the FASB released ASC 842, IASB also announced IFRS 16, mandating operating lease capitalization. Unlike ASC 842's retention of expensing operating leases as a single lease expense (ASC 842-20-25-6), IFRS 16 changes how operating leases are expensed. Specifically, operating leases are expensed as two line items: interest and depreciation. As discussed in Section 2.2.1, this change likely results in lower income for many IFRS 16-adopting firms. To focus solely on lease capitalization's balance sheet effects, I study the sample of US public firms.

2.2.3. Hypothesis development

Under the legacy lease standard, financial statement users may or may not have been able to adequately estimate operating leasing activity via footnote disclosures. They might have found it straightforward to estimate the as-if capitalized amount of operating leases because this calculation only requires a simple present value technique (Bratten et al. 2013). In contrast, information processing costs may have deterred financial statement users from monitoring, acquiring, and analyzing footnote disclosures (Blankespoor et al. 2020).

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[30] To the extent that this information is costly to disclose, the reduction in operating lease use that I document could be driven by that consideration. However, for this alternative channel to make sense, companies would have to reduce the level of operating leases to below their materiality threshold. Otherwise, they would still have to report the same degree of footnote disclosure. I find that only two firms have non-zero operating leases in 2018 and zero leases in 2019. Therefore, this cost is not a viable driver of the decrease in operating lease use following ASC 842's adoption.

[31] These practical expedients allow researchers to study the consequences of capitalization while controlling for most of the other minor differences between the new and the legacy standard. In an untabulated analysis, I find that 95 out of 100 randomly selected firms elect practical expedients.

If managers presumed that financial statement users do not properly account for operating lease footnote disclosures, they may have used operating leases for reporting benefits. For example, by recognizing a lower amount of assets and liabilities, managers are able to present more favorable financial ratios, e.g., debt-to-equity (Imhoff et al. 1991). These benefits may have varied implications: lower cost of debt (e.g., Lim et al. 2017) and higher executive compensation (e.g., Imhoff et al. 1993).[32] But once ASC 842 is implemented, these perceived benefits are eliminated. As a result, managers may reduce their use of operating leases.

On the other hand, if managers believed that operating lease footnote disclosure provided adequate representation of leasing activity, then the capitalization of operating leases should not alter managerial behavior. Consistent with this argument, ASC 842's opponents conjectured that lease capitalization would not furnish new information to financial statement users. This argument is credible considering extant evidence that equity and debt market participants incorporate disclosed lease information into their decision-making processes (e.g., Wilkins and Zimmer 1983; Ely 1995; Altamuro et al. 2014; Kraft 2015).

In light of these conflicting arguments, it is ex-ante unclear whether ASC 842 will lead to a reduction in operating lease use. I state the first hypothesis in the alternative form.

Hypothesis 1: Firms' use of operating leases decreases after ASC 842's adoption.

Several factors influence operating lease usage (Lipe 2001; Spencer and Webb 2015). Perhaps the most important reason for leasing is the bankruptcy treatment of true leases, which can be proxied by operating leases. Compared to a lender with a secured asset, lessors are better positioned to repossess the underlying asset in the event of bankruptcy and thus can implicitly extend more credit. Consistent with this explanation, prior research documents that financially-constrained firms utilize operating leases to expand debt capacity (Sharpe and Nguyen 1995; Eisfeldt and Rampini 2009). Tax incentives also play an important role (Myers et al. 1976; Graham et al. 1998), as do financial conditions, firm growths, and flexible capacity demands (Beatty et al. 2010; Caskey and Ozel 2019).

If managers perceived off-balance-sheet reporting benefits, those benefits would have been among the many considerations managers weighed when making leasing decisions prior to ASC 842. After cumulatively assessing the benefits and costs of various factors, they would choose to lease if the net benefits of doing so exceed other financing sources—namely, short-term and variable leases, finance leases, and asset purchases. However, crucially, ASC 842 eliminates off-balance-sheet reporting benefits.[33] Therefore, if the advantages of operating leases decrease to the point that they are no longer more net beneficial than other financing sources, then managers may

[32] Off-balance-sheet treatment of lease liabilities can directly affect enterprise value. However, its impact is likely minimal, as equity market values may account for operating leases, and operating leases only compose a fraction of a company's value. In addition, utilizing operating leases instead of capitalized assets lowers cash flow expectations and EBITDA. However, these effects are not germane to ASC 842 because accounting for operating leases in income statements and cash flow statements is largely unchanged from the legacy standard.

[33] While ASC 842 removed off-balance-sheet reporting benefits, it is unlikely that it bore a significant impact on the other relative advantages and disadvantages of operating leases vis-a-vis debt-financed assets and short-term and variable leases. For example, even after the new standard, companies can still enjoy tax benefits through debt financed asset purchases. Borrowing rates for debt financing and implicit borrowing rates for operating, short-term, and variable leases are likely affected by a similar magnitude, if affected at all.

substitute operating leases with other financing sources.[34] As long as a material amount of potential operating leases is rendered less beneficial, substitution, on average, should occur. On the contrary, if managers did not use operating leases for reporting benefits, then off-balance-sheet treatment would not factor into their cost-benefit analysis during either the pre- or post-ASC 842 periods. Therefore, ASC 842 should not affect the use of other financing sources.

Because it is ex-ante unclear whether managers were using operating leases for reporting benefits, the substitution of operating leases with other financing sources is an open empirical question. I state the next hypothesis in the alternative form.

Hypothesis 2: Short-term and variable leases, finance leases, and asset purchases replace operating leases upon ASC 842's adoption.

2.3. Research design and sample

2.3.1. Measure of operating leases and comprehensive financing sources

It would be ideal to observe changes in leasing decisions by investigating whether managers modify lease terms or cancel plans to initiate new leases. However, because these actions are not observable, I estimate operating lease activity using future operating lease payments.

Firms are required to report operating lease payments due in each of the next five years and the sum of all payments due thereafter. Nonetheless, following Sharpe and Nguyen (1995) and Beatty et al. (2010), I measure operating lease use based only on one-year-ahead payments.[35] Specifically, I define operating lease use (*OperLease*) as one-year-ahead operating lease payments multiplied by 100 and scaled by beginning total assets.[36] Because companies make periodic payments, the changes in one-year-ahead payments sufficiently capture changes in all operating leases. In contrast, because other future payments do not account for lease renewals (Lim et al. 2003; Eisfeldt and Rampini 2009), they are known to underestimate future operating lease activity.[37] Appendix 1 reports variable definitions.

In the second part of the chapter, I examine whether firms substitute operating leases with other financing sources upon the new standard's adoption. As shown in Appendix 2, I consider operating leases, short-term leases, variable leases, finance leases, and asset purchases as potential

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[34] Consider the following example: A firm needs laptop computers. To determine whether to lease or to buy them, the firm considers reporting benefits and tax deductions. For simplicity, assume all other factors are the same or irrelevant. Operating leases have reporting benefits, and asset purchases have tax benefits. If the firm values the former over the latter (i.e., leasing is more net beneficial), it will likely lease laptop computers before ASC 842. However, once ASC 842 eliminates reporting benefits, leasing becomes less net beneficial than purchasing. Consequently, the firm will likely turn to asset purchases as soon as possible to derive the tax benefits of doing so. A similar example can be constructed comparing operating leases vs. short-term and variable leases.

[35] I do not attempt to constructively capitalize operating leases (i.e., compute the as-if capitalized amount of operating leases) because I am only interested in the changes in operating lease use. Examining changes in one-year-ahead payments accomplishes this goal without introducing estimation errors in the constructive capitalization process.

[36] I utilize beginning assets as the scaler variable to avoid the mechanical expansion of CY firms' ending assets in 2019. The numerator and the denominator have only a one-year gap, as the numerator is measured in the current year.

[37] For example, consider a truck lease with a remaining lease term of two years. While one- and two-year-ahead operating lease payments include payments for that specific lease, other future operating lease payments (e.g., three-year-ahead lease payments) do not. However, once the truck lease ends in two years, the lessee will likely lease another vehicle unless it decides to reduce operations. Because this potential lease is not reflected in the lease payments, three-year-ahead lease payments (and those further ahead) underestimate future operating lease use.

financing sources. The second row in Appendix 2 lists how I measure these sources. I capture operating, short-term, and variable leases using rental expense (*RentExp*), which is defined as rental expense multiplied by 100 scaled by beginning total assets. The individual components of *RentExp* are difficult to measure since companies rarely report all individual expense amounts under the legacy standard. However, I overcome this issue by using one-year-ahead operating lease payments (*OperLease*) measured in the current year to proxy for current-year operating lease expense. Because companies make periodic payments, one-year-ahead payments should serve as a reasonable proxy.[38] Accordingly, I define short-term and variable leases (*STVL*) as *RentExp* minus *OperLease*.[39] I note that short-term lease payments (payments for leases with an *initial* term of 12 months or less) are distinct from one-year-ahead operating lease payments (payments for leases with a *remaining* lease term of 12 months or less).[40]

I measure finance leases and asset purchases using *DepExp* and *IntExp*, which are defined as depreciation and interest expenses, respectively, both multiplied by 100 and scaled by beginning total assets.[41] These expense line items should capture any changes in finance leases and asset purchases, since they are used to expense finance leases and asset purchases. I define all financing sources using income statement figures because it is crucial to compare the dollar amount of reduction in operating leases with the dollar amount of increase in other financing sources. To test for the findings' robustness, I also use alternative measures of asset purchases and finance leases, namely, capital expenditures (*Capex*) and capitalized finance lease obligations (*FinanceLeases*).

As shown in the last row of Appendix 2, I define comprehensive financing sources (*CompFin*) as the sum of all expenses mentioned above. Using this measure, I examine whether the reduction in the operating lease component of *CompFin* leads to the overall reduction in *CompFin* or is offset by the non-operating lease portion of *CompFin* (*NonLease*/*CompFin*), which is defined as one minus the ratio of *OperLease* to *CompFin*.

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[38] I expect a strong positive correlation between one-year-ahead operating lease payments and operating lease expense. Although this is not testable, I infer it from the strong positive correlation between one-year-ahead and two-year-ahead operating lease payments (0.99 and 0.98 for Pearson and Spearman correlations, respectively). I acknowledge that one-year-ahead operating lease payments measure operating lease expense with errors. Namely, (i) any changes in operating lease use during the year will be overestimated, and (ii) leases that expire during the following year will be underestimated. I highlight that the *OperLease* analysis in Table 3 and the *CompFin* analysis in Table 4 are free of such errors and sufficiently show a systematic substitution of operating leases with other financing sources. Similarly, the findings in Table 3 combined with the *RentExp* analysis in Table 8 substantively document the replacement of operating leases with short-term and variable leases.

[39] Because I cannot further decompose *STVL*, I study short-term and variable leases together. Also, I winsorize *STVL* at zero because negative values are not economically meaningful. Nonetheless, allowing for negative values does not change any inferences.

[40] Under the legacy standard (ASC 840-20-50-2), short-term lease payments are not included in one-year-ahead operating lease payments. Under ASC 842, companies may choose to combine short-term and one-year-ahead operating lease payments and capitalize them together (ASC 842-20-25-2), but such a merger is highly unlikely since there is no material incentive to capitalize short-term leases. That said, a failure to account for the possibility that companies may combine short-term and operating leases under the new standard biases statistical tests *against* documenting a systematic substitution of operating leases with short-term leases. This is because I underestimate CY firms' short-term leases only in 2019. I note that the inferences remain the same using two-year-operating lease payments, which never include short-term lease payments. These results are available upon request.

[41] As shown in Appendix 2 by the “ETC” box, depreciation, amortization, and interest expenses may change for reasons other than modifications in asset purchases and finance leases. Although this “ETC” portion introduces bias to my estimates, I expect this bias to be minimal because there is no material reason for the “ETC” portion to differentially change for CY firms relative to non-CY firms upon ASC 842's adoption.

2.3.2. Research design

Based on ASC 842's staggered adoption, I employ a difference-in-differences (DID) research design to study the new standard's effect on managerial leasing decisions. Since ASC 842 is effective for fiscal years that begin after December 15, 2018, the first firms to adopt are the calendar year (CY) firms whose fiscal year runs from January–December 2019. All other firms—non-calendar year (non-CY) firms—adopt the new standard during fiscal years that begin in 2019 but end in 2020. Throughout the study, I define year based on fiscal year-end dates. Therefore, the only firms that adopt ASC 842 in 2019 are CY firms. I employ CY firms as the treatment group following prior studies (Ferri et al. 2018; Gipper 2021). All non-CY firms are defined as control firms. As discussed in Section 2.7.2, the inferences remain the same using a subset of non-CY firms. Appendix 3 explains treatment and control group assignment.

To study the effect of ASC 842's adoption on operating lease use, I compare CY treatment firms' post-period operating lease use to that of the pre-period relative to the pre-post difference for non-CY control firms. Specifically, I estimate the following ordinary least square regression:

$$Y_{i,t} = \beta_0 + \beta_1 2019 \times CY_{i,t} + \sum \gamma Controls_{i,t} + \varepsilon_{i,t}, \qquad (1)$$

where i and t index firms and years, respectively. For the left-hand-side variable ($Y_{i,t}$), I use operating lease use (*OperLease*), as defined above. Turning to the right-hand-side variables, *2019* is an indicator variable for year 2019 based on fiscal year-end date. *CY* is an indicator variable for CY firms. *Controls* is a vector of operating lease determinants (Imhoff and Thomas 1988; Eisfeldt and Rampini 2009; Beatty et al. 2010). Specifically, I include lagged one-year-ahead operating lease payments (*lag_OperLease*) to control for mean-reversion; the natural log of equity market value (*Size*), dividend scaled by total assets (*Dividend*), an indicator variable for loss (*Loss*), and cash scaled by total assets (*Cash*) to control for financing constraints; marginal tax rate (*MTR*) to control for tax incentives; and an indicator variable for a Big 4 auditor (*Big4*) to control for the level of monitoring on opportunistic use of operating leases. In an additional analysis discussed in Section 2.7.2, I add volatility of sales growth (*Volatility*) to control for demands of flexible capacity; cash flows (*CFO*) to additionally control for financing constraints; imputed borrowing cost (*BorrowingCost*) and debt-to-equity (*Debt/Equity*) to control for financing conditions; profit margin (*PM*) to additionally control for tax conditions; and asset growth (*AssetGrowth*) to control for firms' growth (Sharpe and Nguyen 1995; Eisfeldt and Rampini 2009; Caskey and Ozel 2019). Since these control variables are restrictive, I do not include them in the main analyses. All control variables are measured using the most recent value available at the beginning of the year. Appendix 1 provides variable definitions. Because I focus on studying changes in firms' leasing behavior, I also include firm fixed effects and examine within-firm variation. Lastly, I add year fixed effects to account for aggregate economic shocks. Standard errors are clustered by firm to account for errors that correlate over time.

In this chapter, I winsorize all continuous variables at 1% and 99% by year. Nonetheless, it is possible that outliers might become evident only after I examine them at a multi-dimensional level. To address this possibility, I conduct additional analyses to identify outliers at a multi-dimensional level using the Isolation Forest algorithm. This algorithm is an anomaly detection algorithm that uses a tree ensemble method to detect outliers (Liu et al. 2008). Specifically, it randomly splits the feature space until a point is isolated. Since anomaly points are distant from other points, the algorithm can isolate them with relatively few splits. On the other hand, normal points are located close to other normal points in the feature space and thus require a significantly greater number of random splits to isolate them. Using this logic, Isolation Forest identifies points that are easily isolated as anomaly points. In untabulated analyses, I find that all of the inferences in this chapter remain the same using the Isolation Forest algorithm to detect outliers.

As I discuss in Section 2.3.4 below, CY and non-CY firms differ in some respects. To enhance the comparability of the two groups, I follow Ferri et al. (2018) and conduct entropy balancing analyses in addition to unweighted baseline analyses throughout the study. The entropy-balanced sample is obtained by reweighting non-CY firm observations based on the average firm values of the covariates discussed above (Hainmueller 2012). I also conduct propensity score matching (PSM) analysis for all of the main analyses. Using nearest neighbor matching without replacement, I match CY firms to non-CY firms on firm average values of the covariates discussed above within the same Fama French 12 industries.

2.3.3. Sample construction

The initial sample begins with all public US firm-years in Compustat from 2016 to 2019. The main sample starts in 2016 in order to observe pre-adoption trends and ends in 2019 when CY firms adopt the standard. I remove firms that change their fiscal year-ends because that decision might correlate with the adoption of the new standard. I also remove observations that do not offer sufficient data to measure operating lease use and all independent variables.[42] I require comprehensive financing sources (*CompFin*) to be greater than zero.[43] In addition, I exclude financial firms (SIC 6000-6999) and penny stocks (stock price below $5), since their financing decisions are based on unique factors (e.g., Sengupta 1998). I omit firms with missing values in 2018 or 2019 (i.e., require at least one observation before and after the adoption) to ensure that singleton observations are excluded from the sample (Correia 2015). Lastly, I exclude 11 firms that adopt ASC 842 either early or late.[44] The final sample contains 1,640 unique firms and 6,279 firm-year observations. Appendix 4 presents detailed sample construction steps.

2.3.4. Descriptive statistics

I present the sample compositions in Table 1. Panel A presents the number of observations across years. There are 4,427 CY firm observations and 1,852 non-CY firm observations. Most firms appear in the sample all four years. Panel B reports the sample compositions across 12 Fama French industries. Consistent with findings in prior research (e.g., Smith and Pourciau 1988), the composition of CY and non-CY firms varies across industries. For example, all utility firms and most oil and gas firms are CY firms. On the other hand, firms in the consumer nondurables industry are balanced among the CY and non-CY groups. I conduct year-by-year analyses in Figures 1 and 3 and robustness tests in Table 9 to alleviate concerns that industry composition drives my findings.

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[42] This step eliminates around 50% of firms. In its 2016 report, the IFRS Foundation also notes that only 62% of North American firms disclose operating leases (IFRS 2016).

[43] As long as a firm has non-zero one-year-ahead operating lease payments, I replace missing values of depreciation, amortization, interest, and rental expenses with zeros because not all firms have these expenses. However, removing observations with missing values does not change any of the inferences. These results are available upon request.

[44] Similar to other standards, the number of firms that adopt ASC 842 early is extremely low because there is no material incentive to do so. For example, Lee and Lee (2020) find that less than 1% of the firms adopt the new revenue standard (ASC 606) early. Similarly, Gipper (2021) identifies only 14 firms that adopt SEC's CD&A mandate early.

Table 2, Panel A reports descriptive statistics separately for CY and non-CY firms. The average value of CY (non-CY) firms' operating lease use, *OperLease*, is 1.59% (2.66%) of beginning total assets. The average comprehensive financing sources (*CompFin*) equals 7.81% and 8.17% of beginning total assets for CY and non-CY firms, respectively. The average *Size* is 7.64 ($2.1 billion) and 7.58 ($2.0 billion) for CY and non-CY firms, respectively. Other firm characteristics reveal some differences. For example, while 27% of CY firm observations report a loss, only 15% of non-CY firm observations do.

I conduct additional tests using entropy-balanced and PSM samples. Table 2, Panel B reports means and standard deviations for the entropy-balanced sample and confirms that means and standard deviations are effectively the same between CY firms and entropy-balanced non-CY firms. Similarly, in Table 2, Panel C, the PSM samples show more similar statistics between the two groups relative to the two groups in the main sample.

In Table 2, Panel D, I compare means and standard deviations between the pre-ASC 842 (2016-2018) and post-ASC 842 (2019) periods. To capture 2019 values in the post-period, I use the lead values of the control variables, which are originally measured at the beginning of each year. Focusing on the control variables, I find that *Size* $_{t+1}$, *Loss* $_{t+1}$, and *MTR* $_{t+1}$ are higher and *Cash* $_{t+1}$ is lower in 2019. Nonetheless, I find in an untabulated analysis that the pre-post differences are similar between CY and non-CY firms, indicating that it is unlikely that the aforementioned differences drive this study's results. Furthermore, as I discuss in Section 2.7.2, I find that the inferences remain unchanged when I interact controls with the indicator variable for 2019 and allow the controls to have a different impact in that year.

Table 2, Panel E presents pairwise correlation coefficients. *OperLease* shows a strong positive correlation with *RentExp* as the *OperLease* portion dominates *RentExp*. In addition, this panel reveals the persistence of leasing decisions, as evidenced by a strong positive correlation between *OperLease* and *lag_OperLease.* Focusing on the Spearman correlations, I document that *Size*, *Dividend*, profitability, and *MTR* are negatively correlated with operating leases, consistent with prior research (e.g., Beatty et al. 2010).

2.4. ASC 842 and managerial leasing decisions

2.4.1. Operating lease use

Table 3 reports the main regression results estimating changes in operating lease use (*OperLease*) upon the adoption of the new standard. Columns (1) and (2) present univariate analyses without any controls and fixed effects for CY and non-CY firms, respectively. The negative coefficient of -0.142 on *2019* in column (1) suggests a statistically significant reduction in operating lease use for CY firms upon the new standard's adoption. I defer the discussion of economic magnitude to Section 2.5.2. As expected, in column (2), the coefficient on *2019* is statistically indistinguishable from zero because non-CY firms do not adopt the new standard during the main sample period (2016–2019). Combining columns (1) and (2), column (3) presents the DID analysis. The coefficient on *2019×CY* of -0.134 is negative and statistically significant, suggesting that CY firms' reduction in operating lease use is incremental to non-CY firms' changes.

Table 3, column (4) presents the K-Means Clustering fixed effects results.[45] As discussed in Section 2.3.4, CY and non-CY firms are different in many dimensions. Since these observable differences or any other unobservable differences may drive the results in this study, I control for their effects by including K-Means Clustering fixed effects and firm characteristics as the set of controls. I find that the coefficient on *2019*×CY remains negative and significant.

Table 3, column (5) reports the main specification results, estimating equation (1).[46] This specification includes firm characteristics that are known to correlate with leasing decisions as well as firm and year fixed effects. Column (6) presents the entropy balancing analysis, which uses CY firms and entropy-balanced non-CY firms with similar observable characteristics. Lastly, column (7) presents PSM analysis, which employs propensity score-matched CY and non-CY samples. In alignment with Hypothesis 1, I consistently document a reduction in operating lease use for CY firms, as evidenced by the negative and statistically significant coefficients on *2019*×*CY*.[47] In Section 2.7, I conduct a battery of robustness tests and confirm that the inference of a reduction in operating lease use remains the same for alternative specifications, when employing different measures of operating leases, and in various samples.

2.4.2. Parallel trends, reversal of diverging trends, and placebo analyses

To test for parallel trends, I examine year-by-year DID in operating lease use (e.g., Baik et al. 2021). I also test for the reversal of the diverging trends in operating lease use by extending the sample to 2020 and adding *CY* interacted with an indicator variable for 2020.[48] Specifically, I estimate the following ordinary least square regression:

$$\begin{aligned} OperLease_{i,t} = \; & \beta_0 + \beta_1 CY_{i,t} + \beta_2 2017 \times CY_{i,t} + \beta_3 2018 \times CY_{i,t} + \beta_4 2019 \times CY_{i,t} \\ & + \beta_5 2020 \times CY_{i,t} + \sum \gamma Controls_{i,t} + \varepsilon_{i,t}. \end{aligned} \tag{2}$$

Figure 1 plots the coefficients obtained from estimating equation (2). This model uses the difference in 2016 operating lease use between CY and non-CY firms as the base difference (β_1). The coefficients on the interaction terms— β_2, β_3, β_4 and β_5—report the differences in 2017, 2018, 2019, and 2020, respectively, incremental to the difference in 2016. The solid lines represent the two-tailed 90% confidence interval around each point estimate. The figure shows that the differences in 2017 and 2018 are statistically indistinguishable from the base difference,

[45] K-Means Clustering is an unsupervised machine learning algorithm that forms K groups based on Euclidean distances in a data space formed by a set of features (Hartigan and Wong 1979). Specifically, the algorithm finds groups so that the sum of squared distance between each data point in a cluster and the centroid of the cluster is minimized. In this study, I use a set of firm characteristics described in Section 2.3.4 as the features. If the relationships between firm characteristics and leasing decisions are perfectly linear, then adding firm characteristics as control variables in regression models should be sufficient. However, since that is unlikely the case, adding fixed effects of groups generated using firm features likely provides incremental benefits.

[46] I suppress the coefficients on the *intercept*, *2019*, and *CY* because I include firm and year fixed effects.

[47] These findings are statistically significant, as evidenced by the t-statistics. I do not compare the average firm-specific reduction in operating lease use (i.e., DID coefficient) to the standard deviation calculated using the pooled panel data mainly because almost all of the variation in operating lease use originates across-firm rather than within-firm. For example, the standard deviation of CY firms' operating lease use is 2.11 (shown in Table 2) and that using the 2018 CY firm sample (i.e., across-firm variation) is 2.09.

[48] For CY firms in 2020, the scaler variable—lagged total assets—includes operating lease right-of-use (ROU) assets because these firms start capitalizing them in 2019. In order to consistently define the scaler variable, I obtain CY firms' 2019 ROU assets from FactSet and subtract them from total assets. I note that all other firm-years do not include ROU assets in total assets.

confirming the parallel trends in the pre-ASC 842 period. I document a statistically significant difference in *OperLease* from the base difference only in 2019 when CY firms adopt ASC 842. Additionally, the figure illustrates that when non-CY firms also adopt the new standard in 2020, the difference in *OperLease* converges to the base difference, as the two groups no longer account for operating leases dissimilarly.

Figure 1 also serves as a placebo test to alleviate concern that other regulatory changes that took effect around ASC 842's implementation drive the reduction in operating lease use. For example, the bonus depreciation provision contained in the Tax Cuts and Jobs Act (TCJA), signed into law in December 2017, may have rendered operating leases less attractive for CY firms than for non-CY firms. If this were the case, I should observe CY firms' differential reduction in 2018 and not in 2019. Furthermore, as I discuss in Section 2.6.1, I find no evidence of CY firms' differential changes in depreciation expense. Thus, the TCJA does not seem to be a viable explanation for operating lease reduction in 2019. In addition, many firms in a single industry have the same fiscal year-ends, and thus a confounding factor may affect certain industries that consist mainly of CY firms. This concern can be alleviated by the evidenced reversal of the operating lease use difference in 2020, which demonstrates that when non-CY firms adopt the standard in 2020, they experience a similar effect as that of CY firms. Unless a confounding factor only affects CY firms (or their industries) starting in 2019 and non-CY firms (or their industries) in 2020, the evidence in Figure 1 suggests a causal link between ASC 842 and a reduction in operating leases.

2.4.3. Substitution effects

In Table 4, I test Hypothesis 2 and examine whether CY firms' reduction in operating lease use is offset by an increase in other financing sources. If such substitution occurs, I should not find a significant reduction in comprehensive financing sources (*CompFin*). In column (1), I conduct a DID analysis without any controls or fixed effects. Column (2) adds control variables and fixed effects and estimates equation (1). In columns (3) and (4), I conduct entropy balancing and PSM analyses, respectively. Throughout the columns, the coefficients on *2019×CY* remain statistically indistinguishable from zero, showing that CY firms' comprehensive financing sources do not decrease in 2019 relative to non-CY firms. This finding, in conjunction with the reduction in operating lease use I discuss in Section 2.4.1, strongly suggests a systematic substitution of operating leases with other financing sources upon ASC 842's implementation.

To directly investigate the substitution effects, I test for the changes in the non-operating lease portion of comprehensive financing sources (*NonLease/CompFin*). Column (5) presents the baseline results, excluding control variables and fixed effects. Column (6) adds control variables and fixed effects to column (5). In columns (7) and (8), I conduct entropy balancing and PSM analyses, respectively. The coefficients on the interaction term are positive and statistically significant throughout the columns, indicating that the non-operating lease portion of comprehensive financing sources increases upon the standard's adoption. I interpret the economic magnitudes in Section 2.5.2. Overall, Table 4's evidence points to a systematic substitution of operating leases with other financing sources.

2.5. Channel tests

2.5.1. Lease-intensity partitions

In this section, I test whether the elimination of reporting benefits is the channel through which ASC 842 affects managerial leasing decisions. If managers linked operating leases to reporting advantages during the pre-ASC 842 period, then lease-intensive firms likely benefited most from leaving operating leases off balance sheet. In this case, lease-intensive firms should experience a greater reduction in reporting benefits once operating leases are capitalized. Thus, I expect ASC 842's impact on leasing decisions to be stronger for lease-intensive firms.

Table 5 reports the lease intensity partition analysis. I partition firms into quartile groups from low (Q1) to high (Q4) based on average operating lease use during the pre-adoption period. Thus, each quartile contains CY and non-CY firms with similar pre-adoption levels of lease intensity. Then I estimate equation (1) separately for the samples in each quartile to examine whether ASC 842 affects all four groups (i.e., without cross-sectional variation) or only some of them (i.e., with cross-sectional variation).[49] I conduct this channel test in order to assess the standard's impact on both operating lease use (*OperLease*) and substitution effects (*NonLease*/*CompFin*) and present the results in columns (1)-(4) and (5)-(8), respectively.

Table 5, Panel A presents the unweighted baseline regression results. For the firms with low operating lease intensity shown in columns (1) and (2), the coefficients on *2019×CY* are statistically indistinguishable from zero. In columns (3) and (4), the coefficients are negative and statistically significant, suggesting that ASC 842 exerts a pronounced negative impact on operating lease use for firms with a high pre-adoption level of lease intensity. Substitution effects are also driven by lease-intensive firms. Among the quartile partitions shown in columns (5)-(8), the coefficients on the interaction term for only the top two quartiles (Q3 and Q4) show increases in *NonLease*/*CompFin*. Table 5, Panel B reports entropy balancing analysis results. All inferences remain the same as those reported in Panel A. Overall, the evidence in Table 5 indicates that the firms that experience a greater reduction in perceived reporting benefits are the driver of the main findings, confirming the reporting channel.

2.5.2. Economic magnitude of ASC 842's impact on managerial leasing decisions

Caskey and Ozel (2019) show that a significant amount of operating leases needs to be altered to materially affect a firm's financial ratios. Thus, in my study, it is crucial to examine whether the reduction in operating leases I document demonstrates sufficient economic magnitude to materially affect ratios.

I focus on debt-to-equity because of its acute sensitivity to operating lease changes. Other financial ratios will be affected to a lesser degree. For example, return on assets will be minimally impacted because operating leases are much smaller relative to total assets than they are to total debt. Other leverage ratios such as liabilities-to-assets will also be affected significantly less because (i) liabilities and assets are bigger than debt, and (ii) operating lease changes affect both liabilities and assets in the same direction. For example, Caskey and Ozel (2019) explain that in order to reduce the liabilities-to-assets ratio by 10% for a company with a liabilities-to-assets ratio of 0.8, the company would need to reduce operating leases by 29% of its assets. In contrast, as I

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[49] As an alternative model specification, I replace *2019×CY* with triple interactions of *2019*, *CY*, and indicator variables of four quartile groups and estimate one pooled regression. I find that all inferences remain the same.

illustrate below, companies can reduce their debt-to-equity ratios by 10% with a significantly smaller reduction in operating leases. In general, for a company that wishes to decrease its debt-to-equity ratio by x percent, the changes in operating leases satisfy:

$$\frac{Debt - \Delta Leases}{Equity} = \frac{Debt}{Equity} \times (1 - x) \;\Rightarrow\; \Delta Leases = x \times \frac{Debt}{Assets} \times Assets. \qquad (3)$$

For example, a company that has a debt-to-assets ratio of 0.2 (the median debt-to-assets for the Q4 sample discussed in Section 2.5.1) only needs to decrease operating leases by 2% of its assets to reduce its debt-to-equity by 10% (x = 0.1). Figure 2, Panel A plots the amount of reduction in operating leases as a percentage of total assets needed to achieve a given debt-to-equity improvement for various debt-to-asset assumptions.

Next, I turn to the interpretation of coefficients in Table 5, Panel A. I focus on the Q3 and Q4 samples because those are the drivers of my findings. The coefficient on the interaction term for the Q4 (Q3) sample in the *OperLease* analysis is -0.422 (-0.121), indicating that one-year-ahead operating lease *payments* decrease by 0.422% (0.121%) of beginning total assets. This magnitude translates to a decrease in operating leases by 4.2% (1.2%) of beginning total assets.[50] According to Panel A of Figure 2 (both Q3 and Q4 samples have the median debt-to-assets of 0.2), the economic magnitudes of 4.2% and 1.2% are significant enough to lower debt-to-equity ratios by a meaningful amount—roughly 6-21 percent. Clearly, the economic magnitude of the reduction in operating lease use is appropriate for the reporting channel.

For the *NonLease/CompFin* analysis in Table 5, Panel A, the coefficient on *2019×CY* for the Q4 (Q3) sample is 0.061 (0.044), indicating that the non-lease portion of *CompFin* increases by 6.1 (4.4) percentage points. Relative to CY firms' operating lease portion of *CompFin* in the pre-ASC period—44.8% (25.5%)—the coefficient of 0.061 (0.044) represents a 13.6% (17.3%) increase, which indicates that a significant portion of operating leases is replaced by other financing sources.

2.5.3. Debt-to-equity partitions

Figure 2, Panel A reveals a dynamic relationship between leverage and reporting benefits. Specifically, it shows that the effects of operating lease reduction on debt-to-equity decreases with leverage. In Panel B of Figure 2, I repeat the analysis across a more popular measure of leverage—debt-to-equity. This panel consistently shows that more-levered firms' debt-to-equity ratios are not as sensitive to operating lease changes, suggesting that these firms will be less incentivized to change operating leases for reporting reasons. Thus, if the reporting channel drives managerial leasing decisions, I expect that ASC 842 will have attenuated effects for more-levered firms and more significant effects for less-levered firms.

Given Caskey and Ozel's (2019) assertion that firms' leasing decisions are driven more by non-reporting reasons than reporting incentives, I expect attenuated effects for more-levered firms.

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[50] Following Beatty et al. (2010), I compute as-if capitalized amounts of operating leases by assuming that one-year-ahead payments continue in perpetuity and that cost of capital is 10% (i.e., I divide the coefficient by 10%). I note that this is a rough estimate. Although I follow Beatty et al. and use the 10% discount rate, this computation will likely underestimate actual operating lease changes because many firms will have a discount rate lower than 10% in 2019. On the other hand, the assumption that one-year-ahead payments continue in perpetuity likely results in overestimation, since other future operating lease payments do not include potential lease renewals, as discussed in Section 2.3.1.

These firms are more likely to utilize operating leases to expand debt capacity—one of the major non-reporting reasons for leasing (Sharpe and Nguyen 1995; Eisfeldt and Rampini 2009). Thus, it is unlikely that reporting reasons are a determining factor in more-levered firms' leasing decisions.

In contrast, if firms reduce operating leases to avoid violating debt covenants once operating leases are capitalized (debt-contracting channel), ASC 842's effects should be more pronounced for more-levered firms, since these firms are more likely to verge on covenant violation thresholds. In this section, I test whether the reporting channel or the debt-contracting channel explains the effects of ASC 842.

Table 6 reports the debt-to-equity partition analysis. I estimate equation (1) separately for the quartile portfolio of firms partitioned by the average debt-to-equity ratio in the pre-adoption period. Firms in the highest (lowest) quartile are firms that are relatively more (less) levered. For this analysis, I only include firms with at least one year of positive equity before ASC 842.

Table 6, Panel A presents the unweighted baseline results. As with the previous table, I report *OperLease* results in columns (1)-(4) and *NonLease*/*CompFin* results in columns (5)-(8). Columns (1), (2), (5), and (6) report the DID coefficients for lower debt-to-equity quartile firms (Q1 and Q2). Starting with the *OperLease* analyses, the coefficients on *2019×CY* in columns (1) and (2) are both negative and statistically significant, suggesting that these less-levered firms reduce operating lease use. For the *NonLease/CompFin* analyses, the coefficients in columns (5) and (6) are positive and statistically significant, indicating that operating leases are substituted with other financing sources. Columns (3), (4), (7), and (8) present the results for higher debt-to-equity quartile firms (Q3 and Q4 samples). In all of these columns, the coefficients on the interaction term are statistically indistinguishable from zero, suggesting that ASC 842 does not bear much impact on more-levered firms. Panel B of Table 6 reports the entropy balancing analysis results and confirms the cross-sectional variation across debt-to-equity partitions. Overall, the findings in Table 6 provide evidence that ASC 842's impact on managerial leasing decisions is driven by less-levered firms that were (i) more sensitive to operating lease changes, (ii) less likely to use operating leases to expand their debt capacity, and (iii) less likely to violate covenants. These findings are consistent with the reporting channel and inconsistent with the debt-contracting channel.

2.6. Asset purchases, finance leases, and short-term and variable leases

2.6.1. Asset purchases and finance leases

In this section, I investigate whether asset purchases or finance leases replace operating leases upon ASC 842's adoption. As discussed in Section 2.3.1, I capture changes in these financing sources by using depreciation expense (*DepExp*) and interest expense (*IntExp*). In Table 7, I employ the same structure as Table 4 and present the baseline results in the first two columns with and without control variables and fixed effects, followed by entropy balancing and PSM analysis results in the next two columns. In Panel A of Table 7, *DepExp* results are shown in columns (1)-(4), and *IntExp* results are reported in columns (5) and (8). Throughout the columns, the coefficients on *2019×CY* are statistically indistinguishable from zero, with the exception of column (7). While the substitution of operating leases with asset purchases or finance leases would result in a positive coefficient on *2019×CY*, none of the coefficients are positive.

In Table 7, Panel B, I repeat the analysis with alternative measures of asset purchases (*Capex*) and finance leases (*FinanceLeases*). I follow corporate finance literature (e.g., Kaplan and

Zingales 1997) and define *Capex* as capital expenditures multiplied by 100 and scaled by beginning capital.[51] Similarly, I define *FinanceLeases* as capitalized finance lease obligations multiplied by 100 and scaled by beginning capital. Once again, the coefficients on *2019×CY* are statistically indistinguishable from zero. Overall, the evidence consistently suggests that neither asset purchases nor finance leases replace operating leases.

2.6.2. Short-term and variable leases

The findings thus far have ruled out asset purchases and finance leases as the possible sources that serve to offset operating lease reduction. Now I turn to the final potential source: short-term and variable leases. Unlike operating leases, both short-term and variable leases can be left off balance sheet under the new standard.[52] However, these leases are often more costly.[53] Thus, managers may consider the tradeoff between off-balance-sheet reporting benefits and higher costs when they determine whether to replace operating leases with short-term or variable leases.

Table 8 presents the DID regression results that examine the substitution of operating leases with short-term and variable leases. I start by studying the changes in CY firms' rental expense (*RentExp*), which consists of operating, short-term, and variable leases, as shown in Appendix 2. Column (1) reports the baseline DID analysis without controls and fixed effects. The coefficient on *2019×CY* of 0.159 is positive and statistically significant. This finding is robust to including controls and fixed effects in column (2) and to entropy balancing and PSM analyses in columns (3) and (4), respectively. Importantly, the reduction in *OperLease* I document in the first part of the chapter strongly suggests that CY firms' operating lease expense component of *RentExp* is lower in 2019. Therefore, the evidence of an increase in *RentExp* is likely driven by short-term and variable leases, indicating a systematic substitution of operating leases with short-term and variable leases. I note that the positive coefficients on *2019×CY* do not necessarily mean that firms lease more short-term and variable leases to offset the reduction in operating leases. They may simply reflect the higher cost of short-term and variable leases as compared with operating leases.

Although the *RentExp* analysis in columns (1)-(4) sufficiently shows ASC 842's substitution effects, I directly test for the increase in the use of short-term and variable leases by studying changes in *STVL*, which is defined as *RentExp* minus *OperLease*, as discussed in Section 2.3.1. As before, columns (5) and (6) present the baseline results with and without controls and fixed effects, respectively. Entropy balancing and PSM results are shown in columns (7) and (8). As expected, the coefficients on the interaction term are positive and significant in all columns, confirming that the increase in *RentExp* is driven by short-term and variable leases (*STVL)*.

Figure 3 depicts the substitution of operating leases with short-term and variable leases for CY firms in Panel A and non-CY firms in Panel B. For each year, I report operating lease expense and short-term and variable lease expenses as a percentage of *RentExp*. I proxy operating lease expense with one-year-ahead operating lease payments. Short-term and variable leases are defined as rental expense minus operating lease expense. Panel A shows that the percentages are

[51] The inferences are unchanged when I use beginning total assets as the scaler variable.
[52] Short-term leases can be left off balance sheet by electing practical expedients. I find that 95 out of a randomly selected 100 firms elect practical expedients. Variable leases are not capitalized unless the payments depend on “an existing index or rate such as the Consumer Price Index or the prime interest rate” (FASB 2016).
[53] Short-term leases are often more costly for many reasons, one of which is faster depreciation of the underlying asset's value earlier in its lifespan. Variable leases are costly because the lessee assumes the risk of changes in the economic outcome tied to the lease payments.

relatively flat during the preparation period (2016–2018). In contrast, starting in 2019, when CY firms adopt the new standard, the operating lease percentages decrease from 91% in the prior year to 76 percent. Accordingly, the short-term and variable lease portion increases from 9% in 2018 to 24% in 2019, confirming the substitution effects. Non-CY firms' percentages are presented in Panel B. During the main sample period of 2016–2019, non-CY firms do not evidence changes in the composition of *RentExp*, since they have not yet adopted the standard. It is only when I extend the sample to 2020, the first year that non-CY firms also adopt ASC 842, that I document the replacement of operating leases with short-term and variable leases. Without acknowledging the effect of ASC 842, it is challenging to explain the existence of such significant substitution effects only for CY firms in 2019 and non-CY firms in 2020.

2.7. Robustness tests

2.7.1. Alternative ways to address the differences between CY and non-CY firms

Throughout this study, I conduct entropy balancing analyses to alleviate a potential concern that the systematic difference between CY and non-CY firms drives my results. In the following analyses, I explore alternative ways to address that concern.

Table 9 reports operating lease use analyses.[54] I present the DID coefficients obtained by estimating modifications of equation (1) or by using different samples. In columns (1)-(3), I add additional interaction variables to equation (1). In column (1), I include interaction variables of controls and an indicator variable for calendar year firms (*CY*), allowing the control variables to have a different impact on CY and non-CY firms. In column (2), I add interaction variables of controls and *2019* to allow for the differential impact of controls in 2019. Lastly, I interact controls with both *CY* and *2019* and report the results in column (3). The *2019*×*CY* coefficients in all three columns remain negative and statistically significant, confirming the robustness of the findings.

By design, CY and non-CY firms have different fiscal periods. For example, firms with a January year-end and December year-end in 2019 are both considered a firm-year in 2019, but they have 11 non-overlapping months. In this extreme example, any economic shock in 2019 will likely affect December year-end firms more than January year-end firms. To confirm that these non-overlapping months are not the driver of my findings, I sequentially drop non-CY firms using June and September as cut-off points and present the results in Table 9, columns (4) and (5), respectively. As evidenced by the coefficients on the interaction term, CY firms still show a marked decrease in operating lease use relative to non-CY firms that operate within similar time frames (e.g., non-overlapping months $\leq$ 3 for the September cutoff sample). Furthermore, the research design used in column (5) is consistent with the selection of control firms by Ferri et al. (2018) and Gipper (2021). While they use September, October, and November year-end firms as the control group, I include all non-CY firms in the main sample to increase the sample size.

Lastly, CY and non-CY firms have differences in industry composition. As shown in Table 1, some industries consist primarily of CY firms. In Table 9, column (6), I exclude the top four industries with the highest CY firm frequency and show that the inference does not change.[55]

[54] For brevity, I only present the regression results using *OperLease* as the dependent variable. I note that the regression analyses using *NonLease/CompFin* or *STVL* as the dependent variable are also not sensitive to the specifications and samples used in Table 9. These results are available upon request.

[55] I exclude the following four industries: oil and gas; utilities; healthcare, medical equip., and drugs; and other.

2.7.2. Different measures, samples, and controls

As discussed in Section 2.3.1, I employ only one-year-ahead operating lease payments to measure operating lease use because other future operating lease payments underestimate subsequent operating lease use. Furthermore, since I am solely interested in estimating changes in operating lease use, I do not attempt to constructively capitalize operating leases. Nonetheless, I test for the robustness of my findings by adopting alternative measures of operating lease use.

In Table 10, column (1), I estimate equation (1) using *OL_PMT_All*, a measure that is calculated based on the first five years of future operating lease payments. Specifically, this measure is defined as as-if capitalized operating leases (the present value of operating lease payments in the next five years) multiplied by 100 and scaled by beginning total assets. Following Beatty et al. (2010) and Graham et al. (1998), I use 10% as the discount rate.[56] I use this static discount rate rather than one that varies year-to-year in order to hold constant the effects of discount rate changes. In column (2), I construct Graham et al.'s (1998) measure of operating leases (*OL_GLS*). This measure is defined as as-if capitalized operating leases (defined above) multiplied by 100 and scaled by the lagged market value of the firm, where the market value of the firm is defined as the sum of as-if capitalized operating leases plus the market value of equity plus total assets minus the book value of equity. I note that both *OL_PMT_All* and *OL_GLS* likely underestimate the changes in operating lease use because they include other future operating lease payments, which are known to underestimate future operating lease use (as discussed in Section 2.3.1). Nevertheless, both measures show a statistically significant reduction in operating lease use.

Beatty et al. (2010) assume that one-year-ahead payments continue in perpetuity and thus calculate as-if capitalized operating leases as one-year-ahead payments divided by 10 percent. In Table 10, column (3), I find that the inference remains the same when using Beatty et al.'s measure (*OL_BLW*), which is defined as as-if capitalized operating leases multiplied by 100 and scaled by the sum of lagged as-if capitalized operating leases and beginning PP&E. In column (4), the inference again remains the same after changing the scaler variable in *OperLease* to lagged sales (*OL/Sales*). For the dependent variables in columns (3) and (4), I require the scaler variables to be strictly positive.

When constructing the main sample, I exclude financial firms and penny stocks because their financing decisions are different than those of other firms. Nonetheless, my results are robust to including financial firms in the sample, as shown in column (5) of Table 10. Furthermore, the inferences remain the same after replacing the penny stock requirement with a minimum asset requirement of $20 million, as shown in column (6).[57] For the last robustness test shown in column (7), I add six other control variables that are discussed in Section 2.3.2. As these variables are more restrictive, I conduct this robustness test on a smaller sample (4,344 observations). Once again, the inferences remain the same.[58]

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[56] This finding is robust to using 3%, 5%, or 7% discount rates.

[57] This finding is robust to removing the minimum asset requirement (adding all penny stocks) or replacing the minimum cutoff with $5 million, $10 million, or $100 million. These results are available upon request.

[58] The inferences for the *NonLease/CompFin* and *STVL* analyses (i.e., employing *NonLease/CompFin* and *STVL* as the dependent variables) remain the same when I use the expanded samples employed in columns (5) and (6) and when I add the additional control variables shown in column (7). These results are available upon request.

2.8. Lease disclosure

In addition to lease capitalization, ASC 842 requires firms to disclose information about their leases. To understand what kind of information is disclosed, I examine the top 100 firms in terms of market capitalization and use various natural language processing techniques to extract keywords from lease disclosures. Specifically, I use Term Frequency-Inverse Document Frequency (TF-IDF), Key Bidirectional Encoder Representations from Transformers (KeyBERT), and Yet Another Keyword Extractor (YAKE).

TF-IDF captures the frequency of terms in documents after penalizing terms that appear across many documents. For example, the article 'the' will appear many times in lease disclosures but since all disclosures will contain this article, it will be penalized. KeyBERT leverages BERT embeddings to extract keywords that are most similar to the document itself. YAKE computes scores for each term based on factors such as frequency, case, and position.

In Table 11, I present the top 20 keywords extracted using the aforementioned 3 methods. A few observations are worth noting. First, companies disclose operating lease information more often than finance lease information. This makes economic sense as for many companies, operating leases are more common than finance leases. Second, companies discuss various components that go into computing total lease amounts. Specifically, they disclose discount rates, future lease payments, and weighted average remaining lease terms. Third, they also discuss operating lease cost, which is how companies expense leases. Lastly, retail and facility are important keywords. We can infer that retail or facility space leases are important for many companies and that companies discuss them in detail.

2.9. Conclusion

In this chapter, I investigate the effects of balance sheet recognition vs. disclosure on managerial behavior by studying the impact of ASC 842's capitalization requirement on leasing decisions. I show that the standard's adopters significantly reduce operating lease use and substitute operating leases with short-term and variable leases. And I provide empirical support that the elimination of off-balance-sheet reporting benefits is the channel through which ASC 842 affects managerial leasing decisions.

My study is directly in line with the FASB's interest in assessing the new standard's impact on managerial leasing behaviors. While the Board implemented ASC 842 to prevent firms from opportunistically leaving liabilities off balance sheet, my findings suggest that they have not entirely succeeded in this goal. Furthermore, the Board is also interested in whether ASC 842 increased the transparency of underlying leasing transactions. Because I focus solely on ASC 842's effect on firms, I do not directly test whether the FASB also succeeds in amplifying transparency for financial statement users. This question merits further consideration and exploration in future studies.

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Appendices, figures, and tables

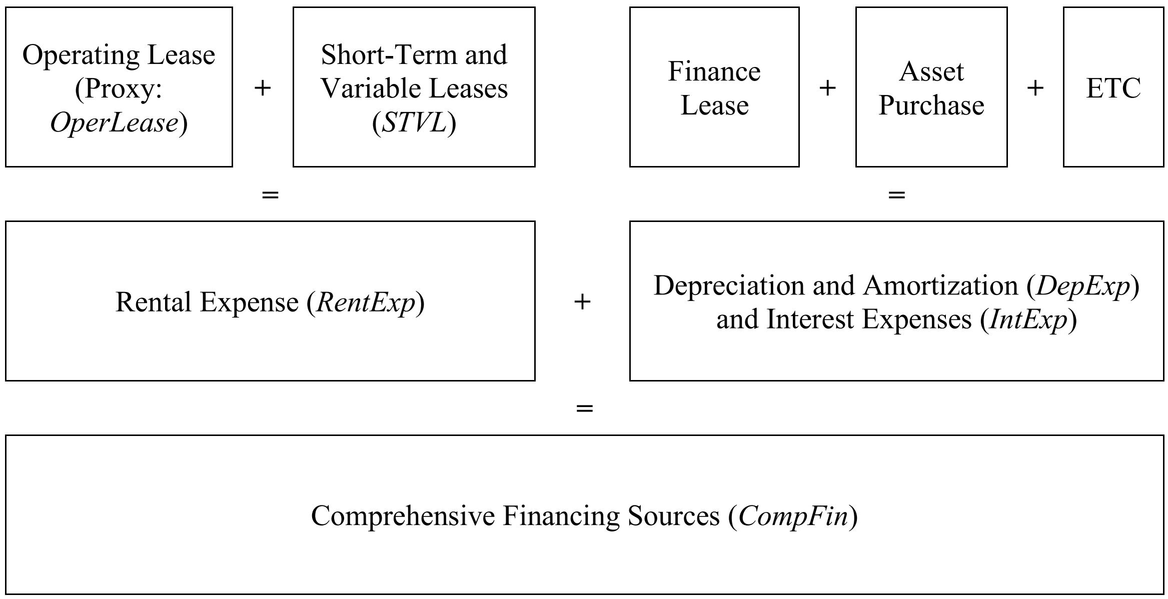
Appendix 1
Variable definitions

Variable	Definition
Dependent Variables:	
OperLease	Operating lease use measured as one-year-ahead operating lease payments (Compustat item #96) multiplied by 100 and scaled by beginning total assets (Compustat item #6)
OL_PMT_All	Operating lease use measured as the as-if capitalized amount of operating leases (the present value of operating lease payments due in the next five years using a 10% discount rate; Compustat items #96 and #164-167) multiplied by 100 and scaled by beginning total assets (Compustat item #6)
OL_GLS	Graham et al.'s (1998) measure of operating lease use defined as the as-if capitalized amount of operating leases (the present value of operating lease payments due in the next five years using a 10% discount rate; Compustat items #96 and #164-167) multiplied by 100 and scaled by the lagged market value of the firm, where the market value of the firm is measured as the as-if capitalized amount of operating leases plus total assets (Compustat item #6) minus book value of equity (Compustat item #60) plus the product of the fiscal-year-end stock price (Compustat item #24) times shares outstanding (Compustat item #25)
OL_BLW	Beatty et al.'s (2010) measure of operating lease use defined as the as-if capitalized amount of operating leases (one-year-ahead operating lease payments divided by 10%; Compustat item #96) multiplied by 100 and scaled by the sum of the lagged as-if capitalized amount of operating leases and beginning PP&E (Compustat item #8)
OL/Sales	Operating lease use measured as one-year-ahead operating lease payments (Compustat item #96) multiplied by 100 and scaled by lagged sales (Compustat item #12)
DepExp	Depreciation and amortization expense (Compustat item #14) multiplied by 100 and scaled by beginning total assets (Compustat item #6)
IntExp	Interest and related expenses (Compustat item #15) multiplied by 100 and scaled by beginning total assets (Compustat item #6)
Capex	Capital expenditures (Compustat item #128) multiplied by 100 and scaled by beginning PP&E (Compustat item #8)
FinanceLeases	Capitalized finance lease obligations (Compustat item #84) multiplied by 100 and scaled by beginning PP&E (Compustat item #8)
RentExp	Rental expense (Compustat item #47) multiplied by 100 and scaled by beginning total assets (Compustat item #6)

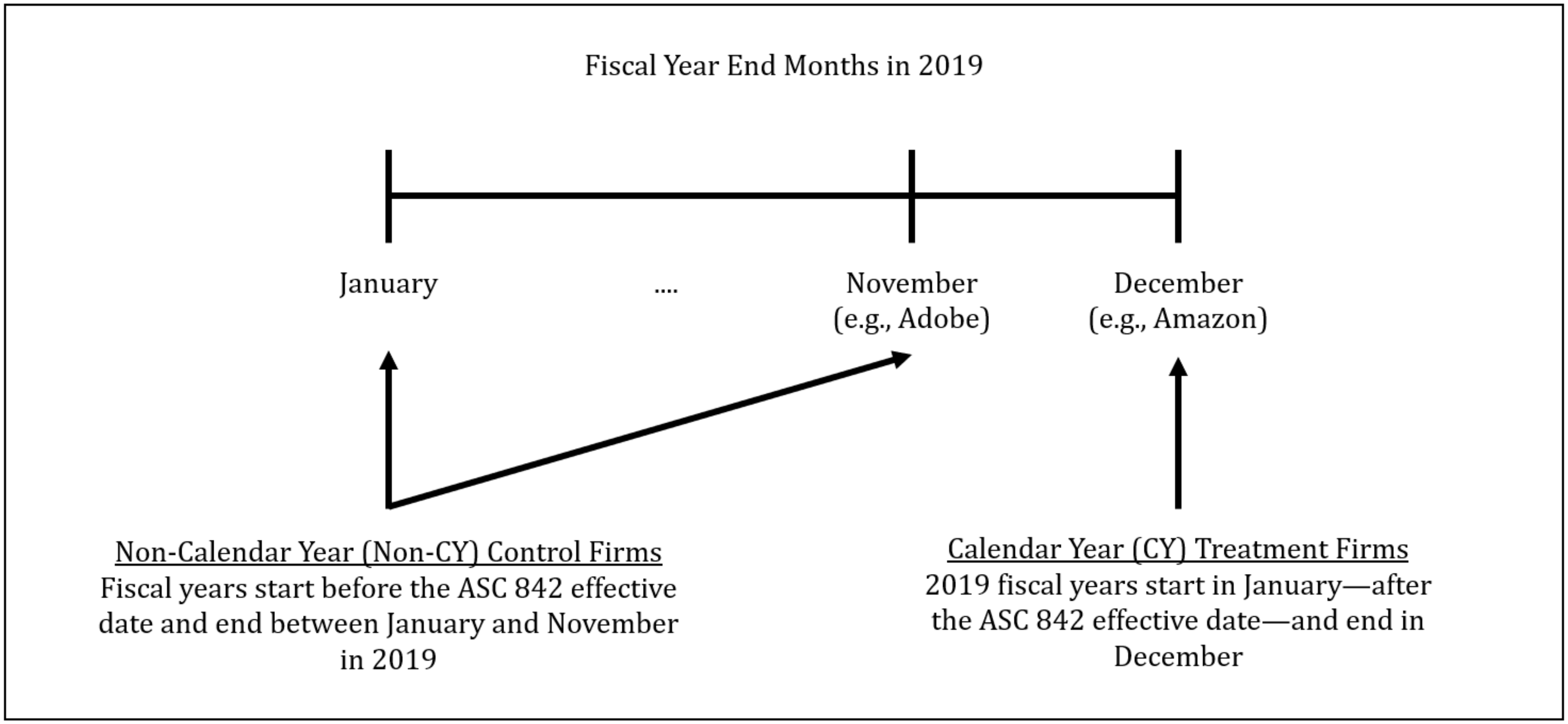
STVL	Short-term and variable leases defined as the non-operating lease component of rental expense multiplied by 100 and scaled by beginning total assets (i.e., *RentExp – OperLease*); the variable is winsorized at zero
CompFin	Comprehensive financing sources defined as the sum of rental, depreciation, amortization, and interest expenses multiplied by 100 and scaled by beginning total assets (i.e., *RentExp* + *DepExp* + *IntExp*)
NonLease/CompFin	Non-operating lease portion of comprehensive financing sources (i.e., 1- *OperLease* / *CompFin*)
Independent Variables:	
2019	An indicator variable that equals one if the fiscal year ends in 2019, and zero otherwise
CY	An indicator variable that equals one if the firm ends the fiscal year in December, and zero otherwise
Control Variables:	
lag_OperLease	Lagged operating lease use measured as lagged one-year-ahead operating lease payments (Compustat item #96) multiplied by 100 and scaled by beginning total assets (Compustat item #6)
Size	The natural log of the equity market value from the prior year, where the equity market value is defined as the fiscal-year-end stock price (Compustat item #24) times shares outstanding (Compustat item #25)
Dividend	Dividends (Compustat item #21) for the prior year scaled by beginning total assets (Compustat item #6)
Loss	An indicator variable that equals one if income before extraordinary items (Compustat item #18) from the prior year is negative, and zero otherwise
Cash	Beginning cash holdings (Compustat item #1) scaled by beginning total assets (Compustat item #6)
MTR	The firm's marginal tax rate in the prior year. I obtain the marginal tax rate data from John Graham's website and correct missing values following Graham and Mills (2008)
Big4	An indicator variable that equals one if the firm employed one of the Big 4 auditors (Deloitte, EY, KPMG, or PwC) in the prior year, and zero otherwise
Additional Controls:	
Volatility	The volatility of sales growth measured as the variance of annual change in ln(sales) over the past five years (Compustat item #12); the sales variable is winsorized at 0.01 to minimize selective elimination of small firms (Caskey and Ozel 2019)
CFO	The sum of lagged net income (Compustat item #172) and lagged depreciation and amortization (Compustat item #14) scaled by beginning total assets (Compustat item #12)

BorrowingCost	Lagged interest expense (Compustat item #15) scaled by the sum of beginning current and non-current debts (Compustat items #34 and #9)
Debt/Equity	The sum of beginning current and non-current debts (Compustat items #34 and #9) scaled by beginning book value of equity (Compustat item #60)
PM	Lagged net income (Compustat item #172) scaled by lagged sales (Compustat item #12)
AssetGrowth	The growth rate in beginning total assets from the prior year (Compustat item #12)

Appendix 2
Composition of comprehensive financing sources



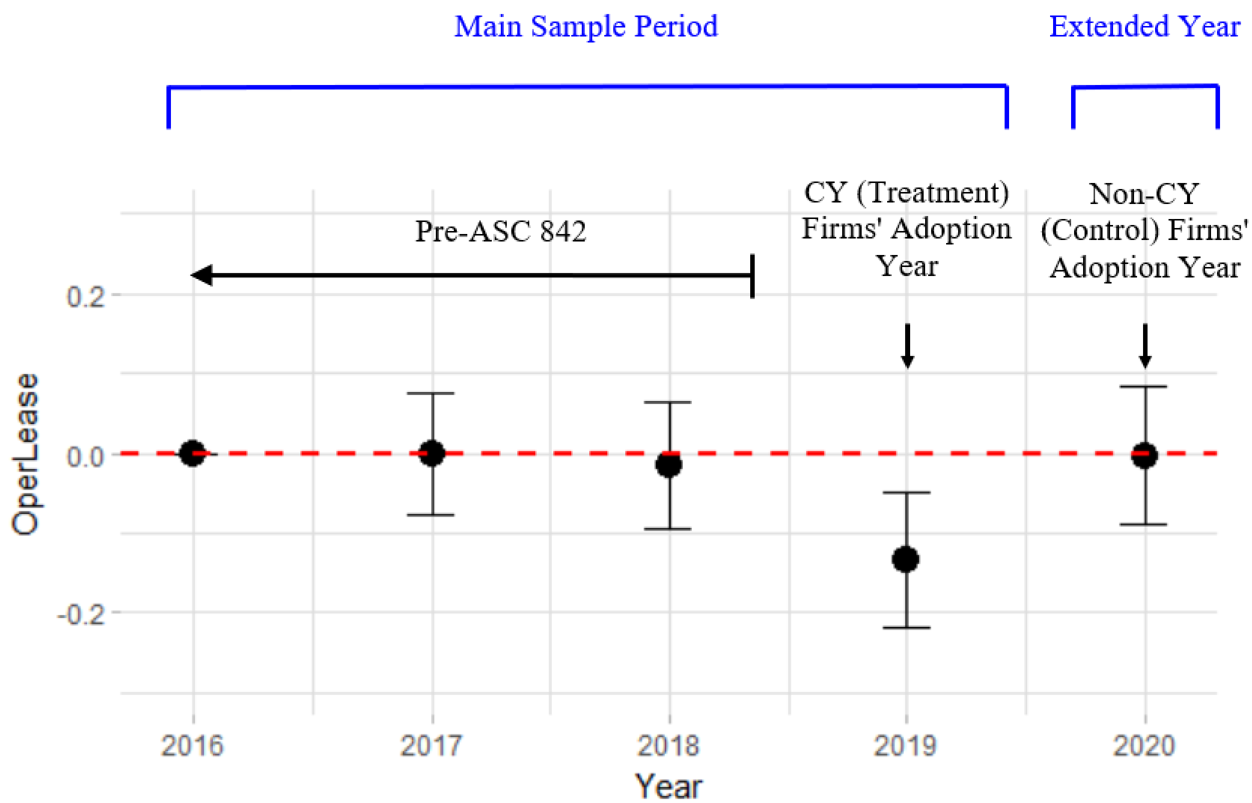
Appendix 3
Treatment and control group assignment



Appendix 4
Sample construction

	# of Obs.		# of Unique Firms		
	Obs.	Diff.	All	CY	Non-CY
US public firms between 2016 and 2019	24,221		6,430	5,201	1,229
Exclude firms that change their fiscal year-ends	24,162	(59)	6,418	5,190	1,228
Require one-year-ahead operating lease payment data	11,732	(12,430)	3,216	2,508	708
Require non-missing variables to calculate all independent variables	11,359	(373)	3,197	2,502	695
Require comprehensive financing sources (*CompFin*) to be greater than 0	11,335	(24)	3,188	2,494	694
Exclude financial firms (SIC 6000-6999)	9,712	(1,623)	2,739	2,073	666
Exclude penny stocks (stock price below $5)	7,369	(2,343)	2,267	1,696	571
Require firms to have observations in both 2018 and 2019	6,314	(1,055)	1,651	1,173	478
Exclude firms that adopt ASC 842 either early or late	6,279	(35)	1,640	1,162	478

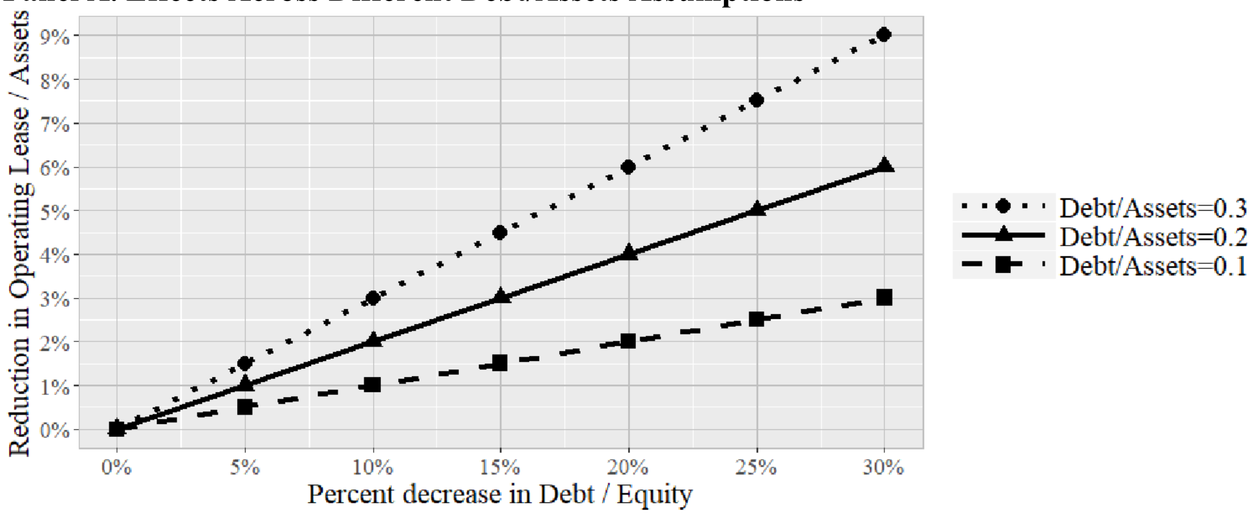
Figure 1
Parallel trends and reversal of operating lease use difference



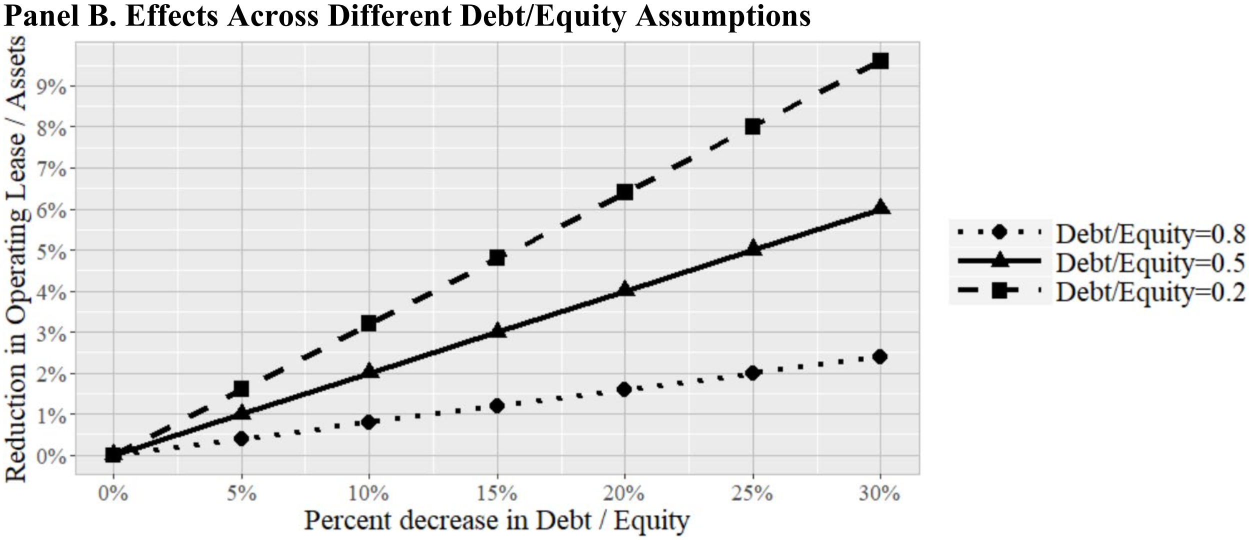
This figure portrays the year-by-year difference-in-differences in operating lease use between calendar year (CY) and non-calendar year (non-CY) firms using the 2016 difference as the base difference. I measure operating lease use (OperLease) as the operating lease payments due in the following year multiplied by 100 and scaled by beginning total assets. CY firms are those that end the fiscal year in December, and non-CY firms are all other firms. The solid lines represent the two-tailed 90% confidence interval around each point estimate. The coefficients and confidence intervals are obtained by estimating equation (2). The y-axis represents operating lease use (OperLease), and the x-axis represents calendar years in which the fiscal year ends. CY (non-CY) firms adopt ASC 842 in 2019 (2020). The sample is an extended sample of 7,788 firm-year observations.

Figure 2
Effects of the reduction in operating leases on Debt/Equity

Panel A. Effects Across Different Debt/Assets Assumptions



Panel B. Effects Across Different Debt/Equity Assumptions



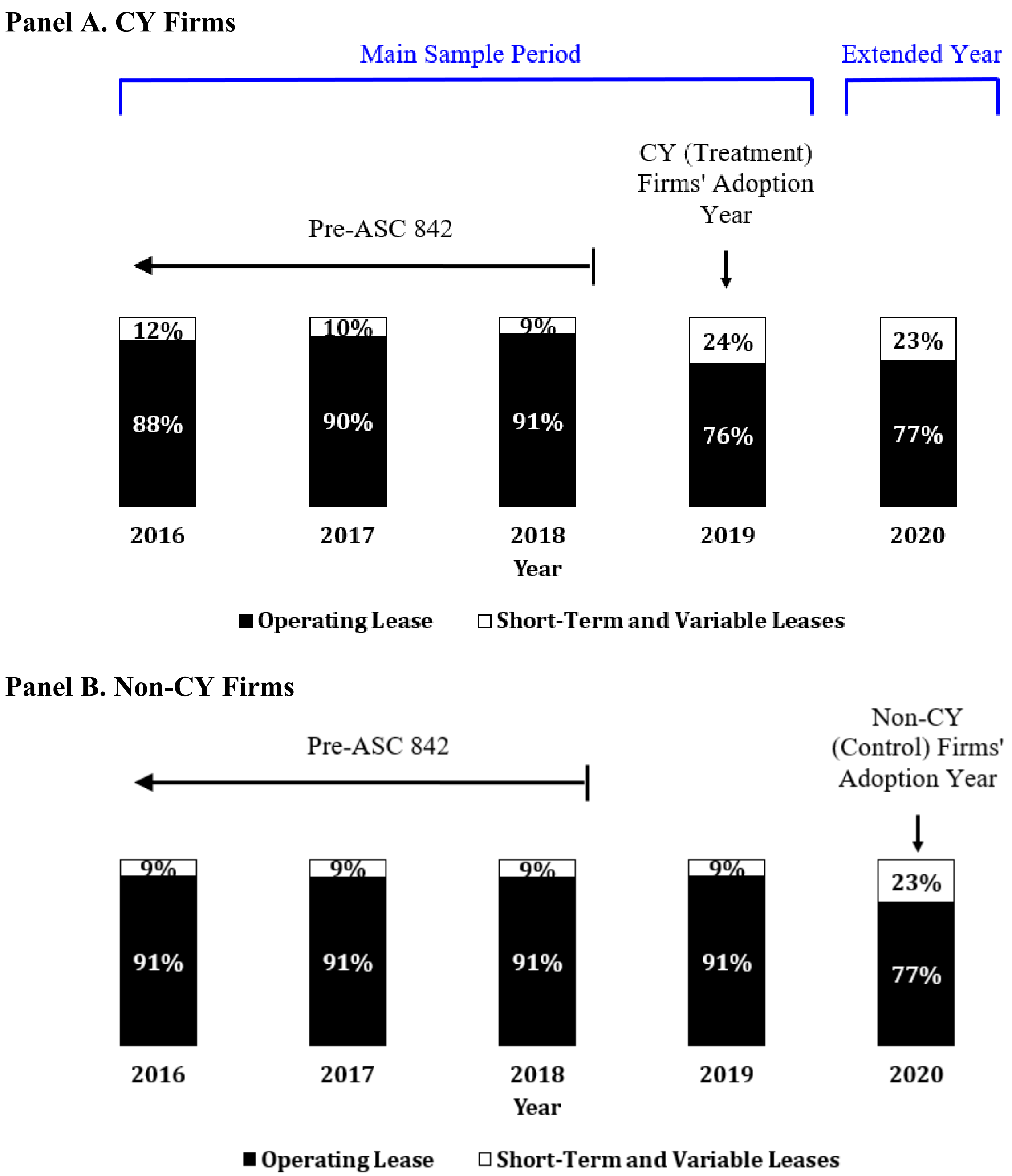
This figure illustrates the amount of operating lease reduction as a percentage of total assets needed to achieve a given debt-to-equity improvement for various Debt/Assets and Debt/Equity assumptions in Panels A and B, respectively. For example, a firm with a debt-to-assets ratio of 0.2 in Panel A can decrease operating leases by 2% of total assets in order to decrease its debt-to-equity ratio by 10 percent. Panel A plots the following equation discussed in Section 2.5.2.

$$\Delta Leases = x \times \frac{Debt}{Assets} \times Assets$$

Panel B assumes an Equity/Assets ratio of 0.4 (the median Equity/Assets ratio for the Q4 sample discussed in Section 2.5.1) and plots the following equation:

$$\Delta Leases = x \times \frac{Debt}{Equity} \times \frac{Equity}{Assets} \times Assets$$

Figure 3
Annual composition of rent expense—Operating leases vs. short-term and variable leases



This figure portrays annual operating lease and short-term and variable lease expenses as a percentage of rental expense separately for calendar year (CY) firms in Panel A and non-calendar year (Non-CY) firms in Panel B. CY firms are the ones that end the fiscal year in December, and non-CY firms are all other firms. I use one-year-ahead operating lease payments as the proxy for operating lease expense. Short-term and variable lease expenses are measured as rental expense minus operating lease expense. CY (non-CY) firms adopt ASC 842 in 2019 (2020). The sample is an extended sample of 7,788 firm-year observations.

Table 1
Sample compositions

Panel A. Number of Observations Across Years

	# of Firm-Year Observations		
	CY Firms	Non-CY Firms	Total
2016	1,017	439	1,456
2017	1,086	457	1,543
2018	1,162	478	1,640
2019	1,162	478	1,640
Total	4,427	1,852	6,279

Panel B. Number of Observations Across Industries

	# of Firm-Year Observations			
Fama French 12-Industry	CY Firms	Non-CY Firms	Total	CY/Total
1. Consumer Nondurables	207	191	398	52%
2. Consumer Durables	144	69	213	68%
3. Manufacturing	602	264	866	70%
4. Oil and Gas	258	15	273	95%
5. Chemicals and Allied Products	154	65	219	70%
6. Business Equipment	829	497	1,326	63%
7. Telephone and Television Transmission	143	35	178	80%
8. Utilities	26	-	26	100%
9. Wholesale, Retail, and Some Services	402	478	880	46%
10. Healthcare, Medical Equip., and Drugs	796	109	905	88%
12. Other	866	129	995	87%
Total	4,427	1,852	6,279	71%

This table presents the sample compositions across years separately for calendar year (CY) and non-calendar year firms (Non-CY) in Panel A and across Fama French 12 industries in Panel B. CY firms are those that end the fiscal year in December, and non-CY firms are all other firms.

Table 2
Summary statistics

Panel A: CY vs. Non-CY Firms

	CY Firms (N=4,427)		Non-CY Firms (N=1,852)		
	Mean	Std. Dev.	Mean	Std. Dev.	Mean Diff.
	(1)	(2)	(3)	(4)	(5)
OperLease	1.59	2.11	2.66	3.92	-1.07***
CompFin	7.81	4.65	8.17	5.64	-0.36**
NonLease/CompFin	0.79	0.20	0.75	0.20	0.04***
DepExp	4.30	2.84	4.05	2.25	0.26***
IntExp	1.65	1.63	1.19	1.31	0.46***
RentExp	1.85	2.42	2.92	4.20	-1.07***
STVL	0.25	0.78	0.24	0.77	0.01
lag_OperLease	1.47	1.99	2.51	3.79	-1.04***
Size	7.64	1.69	7.58	1.83	0.06
Dividend	0.02	0.03	0.02	0.03	-0.01***
Loss	0.27	0.45	0.15	0.36	0.12***
Cash	0.20	0.24	0.18	0.18	0.02***
MTR	0.25	0.12	0.28	0.10	-0.03***
Big4	0.83	0.37	0.80	0.40	0.03***

Panel B: CY Firms vs. Entropy-Balanced Non-CY Firms

	CY Firms (N=4,427)		Entropy-Balanced Non-CY Firms (N=1,852)	
	Mean	Std. Dev.	Mean	Std. Dev.
	(1)	(2)	(3)	(4)
lag_OperLease	1.47	1.99	1.47	1.99
Size	7.64	1.69	7.64	1.69
Dividend	0.02	0.03	0.02	0.03
Loss	0.27	0.45	0.27	0.45
Cash	0.20	0.24	0.20	0.24
MTR	0.25	0.12	0.25	0.12
Big4	0.83	0.37	0.83	0.37

Table 2 (*Continued*)

Panel C: PSM CY and Non-CY Firms

	PSM CY Firms (N=1,764)		PSM Non-CY Firms (N=1,780)	
	Mean	Std. Dev.	Mean	Std. Dev.
	(1)	(2)	(3)	(4)
lag_OperLease	1.78	2.21	2.58	3.85
Size	7.56	1.65	7.56	1.81
Dividend	0.02	0.03	0.02	0.03
Loss	0.18	0.39	0.15	0.35
Cash	0.17	0.18	0.19	0.18
MTR	0.27	0.11	0.28	0.10
Big4	0.80	0.40	0.79	0.41

Panel D: Pre-ASC 842 (2016-2018) and Post-ASC 842 (2019) Period Samples

	2016-2018 Sample (N=4,639)		2019 Sample (N=1,640)		Mean Diff.
	Mean	Std. Dev.	Mean	Std. Dev.	(3)-(1)
	(1)	(2)	(3)	(4)	(5)
OperLease	1.93	2.83	1.83	2.75	-0.11
CompFin	7.85	4.90	8.09	5.13	0.23
NonLease/CompFin	0.77	0.21	0.79	0.19	0.02***
DepExp	4.23	2.67	4.23	2.72	-0.01
IntExp	1.48	1.53	1.62	1.62	0.14***
RentExp	2.14	3.06	2.24	3.17	0.09
STVL	0.20	0.73	0.39	0.90	0.19***
Size $_{t+1}$	7.70	1.71	7.81	1.75	0.10**
Dividend $_{t+1}$	0.02	0.03	0.02	0.03	0.00
Loss $_{t+1}$	0.22	0.42	0.26	0.44	0.04***
Cash $_{t+1}$	0.19	0.22	0.18	0.21	-0.01*
MTR $_{t+1}$	0.25	0.11	0.28	0.08	0.03***
Big4 $_{t+1}$	0.82	0.38	0.81	0.39	-0.01

Table 2 (*Continued*)

Panel E. Correlations (Pearson Above, Spearman Below)

	(a)	(b)	(c)	(d)	(e)	(f)	(g)	(h)	(i)	(j)	(k)	(l)	(m)	(n)
(a) *OperLease*		0.71	-0.64	0.23	-0.01	0.95	0.15	0.97	-0.13	0.03	-0.04	-0.01	0.02	0.00
(b) *CompFin*	0.47		-0.13	0.75	0.41	0.75	0.32	0.69	-0.08	0.04	-0.01	-0.24	0.04	0.02
(c) *NonLease/CompFin*	-0.83	0.00		0.23	0.29	-0.55	0.08	-0.60	0.24	0.06	-0.13	-0.36	0.16	0.08
(d) *DepExp*	0.17	0.81	0.26		0.19	0.24	0.11	0.21	-0.01	0.03	0.00	-0.29	0.05	-0.02
(e) *IntExp*	-0.07	0.44	0.38	0.19		-0.01	0.01	-0.02	0.01	0.01	0.08	-0.17	-0.04	0.09
(f) *RentExp*	0.92	0.52	-0.71	0.21	-0.04		0.41	0.95	-0.12	0.04	-0.06	-0.05	0.04	0.00
(g) *STVL*	0.12	0.24	0.03	0.13	0.05	0.39		0.20	-0.01	0.07	-0.10	-0.14	0.06	0.01
(h) *lag_OperLease*	0.95	0.46	-0.78	0.16	-0.07	0.91	0.19		-0.13	0.03	-0.04	-0.03	0.03	0.00
(i) *Size*	-0.21	-0.05	0.24	0.00	0.14	-0.19	0.02	-0.21		0.17	-0.24	-0.15	0.23	0.44
(j) *Dividend*	-0.10	0.00	0.13	0.06	0.00	-0.05	0.14	-0.09	0.29		-0.24	-0.09	0.19	-0.01
(k) *Loss*	0.06	0.00	-0.09	-0.04	0.00	0.01	-0.14	0.05	-0.25	-0.35		0.39	-0.52	-0.03
(l) *Cash*	0.08	-0.27	-0.26	-0.24	-0.39	0.01	-0.18	0.04	-0.11	-0.18	0.28		-0.44	-0.05
(m) *MTR*	-0.05	0.04	0.09	0.07	0.03	-0.02	0.09	-0.03	0.20	0.26	-0.43	-0.25		0.05
(n) *Big4*	-0.04	0.02	0.07	0.00	0.17	-0.02	0.04	-0.03	0.44	0.07	-0.03	-0.06	0.07	

This table presents summary statistics. The first three panels report means and standard deviations for calendar year (CY) and non-calendar year (Non-CY) firms for the main sample (Panel A), entropy-balanced sample (Panel B), and propensity score-matched (PSM) sample (Panel C). Panel D presents means and standard deviations for the pre- and post-ASC 842 period samples. Panel E reports Pearson and Spearman correlations for the main sample. Appendix 1 provides variable definitions. All continuous variables are winsorized at 1% and 99% levels by year. The main sample includes 6,279 firm-year observations from 2016 to 2019. ***, **, and * indicate statistical significance at 1%, 5%, and 10% levels, respectively, using two-tailed tests.

Table 3
Impact of ASC 842 on operating lease use

	OperLease						
	CY Sample (1)	Non-CY Sample (2)	Main Sample (3)	Main Sample (4)	Main Sample (5)	Entropy Sample (6)	PSM Sample (7)
2019×CY			**-0.134*****	**-0.081****	**-0.142*****	**-0.157*****	**-0.105****
			(-2.89)	**(-2.20)**	**(-3.01)**	**(-3.01)**	**(-2.03)**
2019	**-0.142*****	**-0.008**	-0.008				
	(-4.82)	**(-0.24)**	(-0.24)				
CY			-1.037***	-0.001			
			(-5.42)	(-0.02)			
lag_OperLease				1.013***	0.576***	0.555***	0.628***
				(143.50)	(5.03)	(4.83)	(5.25)
Size				0.001	-0.119**	-0.093*	-0.120*
				(0.13)	(-2.28)	(-1.93)	(-1.77)
Dividend				-0.297	2.035**	1.057	2.347*
				(-0.96)	(2.04)	(1.02)	(1.88)
Loss				-0.111***	-0.083**	-0.059*	-0.019
				(-3.90)	(-2.27)	(-1.71)	(-0.40)
Cash				0.202***	0.914***	0.699**	1.220**
				(3.88)	(2.78)	(2.31)	(2.29)
MTR				-0.124	0.036	0.084	0.263
				(-1.03)	(0.28)	(0.61)	(1.44)
Big4				-0.031	0.052	0.033	0.172
				(-0.62)	(0.38)	(0.26)	(0.70)
(Intercept)	1.627***	2.664***	2.664***				
	(26.32)	(14.71)	(14.72)				
Fixed Effects	No	No	No	Cluster,Year	Firms,Year	Firm,Year	Firm,Year
Adj. R^2	0.00	0.00	0.03	0.96	0.96	0.93	0.97
Obs.	4,427	1,852	6,279	6,279	6,279	6,279	3,544

This table reports the results of regression models that estimate changes in operating lease use upon ASC 842's adoption. Operating lease use (*OperLease*) is measured as one-year-ahead operating lease payments multiplied by 100 and scaled by beginning total assets. Calendar year (CY) firms are those that end the fiscal year in December, and non-calendar year (non-CY) firms are all other firms. CY firms adopt ASC 842 in 2019. The main sample includes 6,279 firm-year observations (4,427 CY and 1,852 Non-CY) from 2016 to 2019. The entropy sample includes CY and entropy-balanced non-CY firms over the same period. The PSM sample includes propensity score-matched CY and non-CY firms. Appendix 1 provides variable definitions. Standard errors are clustered by firm. I report t-statistics in parentheses. ***, **, and * indicate statistical significance at 1%, 5%, and 10% levels, respectively, using two-tailed tests.

Table 4
Substitution of operating leases with other financing sources

	CompFin				*NonLease/CompFin*			
	Main Sample	Main Sample	Entropy Sample	PSM Sample	Main Sample	Main Sample	Entropy Sample	PSM Sample
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
2019×CY	**0.019**	**0.042**	**-0.057**	**0.054**	**0.021*****	**0.026*****	**0.025*****	**0.019*****
	(0.15)	**(0.29)**	**(-0.36)**	**(0.33)**	**(4.27)**	**(4.78)**	**(3.60)**	**(3.22)**
2019	0.221**				0.004			
	(2.17)				(1.30)			
CY	-0.365				0.037***			
	(-1.27)				(3.46)			
(Intercept)	8.109***				0.744***			
	(31.76)				(83.68)			
Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Fixed Effects	No	Firms,Year	Firm,Year	Firm,Year	No	Firms,Year	Firm,Year	Firm,Year
Adj. R^2	0.00	0.88	0.86	0.91	0.01	0.87	0.86	0.92
Obs.	6,279	6,279	6,279	3,544	6,279	6,279	6,279	3,544

This table reports the results of regression models that estimate changes in comprehensive financing sources (*CompFin*) and the non-operating lease portion of comprehensive financing sources (*NonLease/CompFin*). *CompFin* is defined as the sum of rental, depreciation, amortization, and interest expenses multiplied by 100 and scaled by beginning total assets. *NonLease/CompFin* is defined as 1 minus the ratio of *OperLease* to *CompFin*. The main sample includes 6,279 firm-year observations from 2016 to 2019. The entropy sample includes CY and entropy-balanced non-CY firms over the same period. The PSM sample includes propensity score-matched CY and non-CY firms. Controls refer to the vector of control variables discussed in Section 2.3.2. Appendix 1 provides variable definitions. Standard errors are clustered by firm. I report t-statistics in parentheses. ***, **, and * indicate statistical significance at 1%, 5%, and 10% levels, respectively, using two-tailed tests.

Table 5
Cross-sectional variation: Lease intensity partitions

Panel A: Lease Intensity Partitions—Baseline Analysis

	OperLease				*NonLease/CompFin*			
	Q1 Sample	Q2 Sample	Q3 Sample	Q4 Sample	Q1 Sample	Q2 Sample	Q3 Sample	Q4 Sample
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
2019×CY	**-0.013**	**0.002**	**-0.121***	**-0.422*****	**0.000**	**0.006**	**0.044*****	**0.061*****
	(-0.45)	**(0.05)**	**(-1.94)**	**(-2.77)**	**(0.03)**	**(0.68)**	**(3.32)**	**(4.43)**
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
FEs	F&Y	F&Y	F&Y	F&Y	F&Y	F&Y	F&Y	F&Y
Adj. R^2	0.44	0.25	0.27	0.93	0.88	0.78	0.73	0.77
Obs.	1,574	1,586	1,576	1,543	1,574	1,586	1,576	1,543

Panel B: Lease Intensity Partitions—Entropy Balancing Analysis

	OperLease				*NonLease/CompFin*			
	Q1 Sample	Q2 Sample	Q3 Sample	Q4 Sample	Q1 Sample	Q2 Sample	Q3 Sample	Q4 Sample
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
2019×CY	**-0.013**	**-0.017**	**-0.110***	**-0.529*****	**0.004**	**0.011**	**0.034****	**0.055*****
	(-0.49)	**(-0.47)**	**(-1.69)**	**(-2.60)**	**(1.02)**	**(0.94)**	**(2.55)**	**(3.13)**
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
FEs	F&Y	F&Y	F&Y	F&Y	F&Y	F&Y	F&Y	F&Y
Adj. R^2	0.59	0.30	0.35	0.89	0.91	0.82	0.74	0.75
Obs.	1,574	1,586	1,576	1,543	1,574	1,586	1,576	1,543

This table reports cross-sectional variation in ASC 842's impact on operating lease use (*OperLease*) and the non-operating lease portion of comprehensive financing sources (*NonLease/CompFin*). Firms are partitioned into quartile portfolios based on their average operating lease use before ASC 842's adoption. Panel A (Panel B) presents baseline (entropy balancing) results. Appendix 1 provides variable definitions. All specifications include firm and year fixed effects and the controls listed in Appendix 1. Standard errors are clustered by firm. I report t-statistics in parentheses. ***, **, and * indicate statistical significance at 1%, 5%, and 10% levels, respectively, using two-tailed tests.

Table 6
Cross-sectional variation: Debt-to-equity partitions

Panel A: Debt-to-Equity Partitions—Baseline Analysis

	OperLease				*NonLease/CompFin*			
	Q1 Sample	Q2 Sample	Q3 Sample	Q4 Sample	Q1 Sample	Q2 Sample	Q3 Sample	Q4 Sample
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
2019×CY	**-0.134****	**-0.229****	**-0.004**	**-0.093**	**0.058*****	**0.039*****	**0.004**	**0.012**
	(-2.02)	**(-2.04)**	**(-0.08)**	**(-0.98)**	**(4.03)**	**(3.21)**	**(0.45)**	**(1.51)**
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
FEs	F&Y	F&Y	F&Y	F&Y	F&Y	F&Y	F&Y	F&Y
Adj. R^2	0.98	0.94	0.95	0.91	0.85	0.82	0.85	0.85
Obs.	1,437	1,486	1,492	1,486	1,437	1,486	1,492	1,486

Panel B: Debt-to-Equity Partitions—Entropy Balancing Analysis

	OperLease				*NonLease/CompFin*			
	Q1 Sample	Q2 Sample	Q3 Sample	Q4 Sample	Q1 Sample	Q2 Sample	Q3 Sample	Q4 Sample
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
2019×CY	**-0.192****	**-0.269****	**-0.053**	**-0.118**	**0.059****	**0.041*****	**0.012**	**0.012**
	(-2.04)	**(-2.14)**	**(-1.01)**	**(-1.24)**	**(2.41)**	**(3.36)**	**(1.54)**	**(1.55)**
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
FEs	F&Y	F&Y	F&Y	F&Y	F&Y	F&Y	F&Y	F&Y
Adj. R^2	0.96	0.90	0.96	0.90	0.84	0.84	0.87	0.86
Obs.	1,437	1,486	1,492	1,486	1,437	1,486	1,492	1,486

This table reports cross-sectional variation in ASC 842's impact on operating lease use (*OperLease*) and the non-operating lease portion of comprehensive financing sources (*NonLease/CompFin*). Firms are partitioned into quartile portfolios based on their average levels of debt-to-equity ratios before ASC 842's adoption. Panel A (Panel B) presents baseline (entropy balancing) results. Appendix 1 provides variable definitions. All specifications include firm and year fixed effects and the controls listed in Appendix 1. I restrict the sample to firms that have at least one year of positive book value of equity before ASC 842's adoption. Standard errors are clustered by firm. I report t-statistics in parentheses. ***, **, and * indicate statistical significance at 1%, 5%, and 10% levels, respectively, using two-tailed tests.

Table 7
Substitution of operating leases with asset purchases and finance leases

Panel A: Income Statement Figures

	DepExp				*IntExp*			
	Main Sample	Main Sample	Entropy Sample	PSM Sample	Main Sample	Main Sample	Entropy Sample	PSM Sample
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
2019×CY	**-0.084**	**-0.072**	**-0.087**	**-0.059**	**-0.056**	**-0.058**	**-0.121***	**-0.015**
	(-1.18)	**(-0.90)**	**(-1.06)**	**(-0.61)**	**(-1.15)**	**(-1.03)**	**(-1.85)**	**(-0.24)**
2019	0.053				0.182***			
	(0.92)				(5.05)			
CY	0.277**				0.473***			
	(2.16)				(6.61)			
(Intercept)	4.035***				1.146***			
	(40.46)				(20.38)			
Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Fixed Effects	No	Firms,Year	Firm,Year	Firm,Year	No	Firms,Year	Firm,Year	Firm,Year
Adj. R^2	0.00	0.88	0.88	0.86	0.02	0.79	0.79	0.83
Obs.	6,279	6,279	6,279	3,544	6,279	6,279	6,279	3,544

Table 7 (Continued)

Panel B: Capital Expenditures and Capitalized Finance Lease Obligations

	Capex				*FinanceLeases*			
	Main Sample	Main Sample	Entropy Sample	PSM Sample	Main Sample	Main Sample	Entropy Sample	PSM Sample
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
2019×CY	**0.322**	**-0.036**	**0.295**	**0.870**	**-0.111**	**-0.088**	**-0.080**	**-0.446**
	(0.25)	**(-0.03)**	**(0.18)**	**(0.54)**	**(-0.37)**	**(-0.25)**	**(-0.21)**	**(-0.95)**
2019	1.137				0.277			
	(1.12)				(1.33)			
CY	2.455**				0.288			
	(2.31)				(0.76)			
(Intercept)	27.083***				2.353***			
	(32.70)				(7.83)			
Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Fixed Effects	No	Firms,Year	Firm,Year	Firm,Year	No	Firms,Year	Firm,Year	Firm,Year
Adj. R^2	0.00	0.57	0.61	0.60	0.00	0.69	0.72	0.72
Obs.	6,130	6,130	6,130	3,491	6,130	6,130	6,130	3,491

This table reports the results of regression models that estimate ASC 842's impact on asset purchases and finance leases. Panel A reports the impact on income statement figures that capture changes in asset purchases and finance leases, namely depreciation expense (*DepExp*) and interest expense (*IntExp*). Panel B reports ASC 842's effect on capital expenditures (*Capex*) and capitalized finance lease obligations (*FinanceLeases*). Appendix 1 provides variable definitions. Panel A uses the main sample of 6,279 firm-year observations from 2016 to 2019. The sample in Panel B requires beginning capital to be at least $1 million. The entropy samples include CY and entropy-balanced non-CY firms over the same period. The PSM samples include propensity score-matched CY and non-CY firms. Controls refer to the vector of control variables discussed in Section 2.3.2. Standard errors are clustered by firm. I report t-statistics in parentheses. ***, **, and * indicate statistical significance at 1%, 5%, and 10% levels, respectively, using two-tailed tests.

Table 8
Substitution of operating leases with short-term and variable leases

	RentExp				*STVL*			
	Main Sample	Main Sample	Entropy Sample	PSM Sample	Main Sample	Main Sample	Entropy Sample	PSM Sample
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
2019×CY	**0.159*****	**0.172*****	**0.152****	**0.128****	**0.183*****	**0.185*****	**0.192*****	**0.142*****
	(2.86)	**(3.10)**	**(2.45)**	**(2.01)**	**(6.18)**	**(5.39)**	**(4.71)**	**(3.48)**
2019	-0.014				0.005			
	(-0.37)				(0.25)			
CY	-1.115***				-0.047			
	(-5.42)				(-1.48)			
(Intercept)	2.929***				0.345***			
	(15.09)				(12.63)			
Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Fixed Effects	No	Firms,Year	Firm,Year	Firm,Year	No	Firms,Year	Firm,Year	Firm,Year
Adj. R^2	0.02	0.95	0.93	0.98	0.01	0.66	0.64	0.72
Obs.	6,279	6,279	6,279	3,544	6,279	6,279	6,279	3,544

This table reports the results of regression models that estimate changes in rental expense (*RentExp*) and short-term and variable lease expenses (*STVL*). *RentExp* is defined as rental expense multiplied by 100 scaled by beginning total assets. *STVL* is defined as rental expense minus one-year-ahead operating lease payments multiplied by 100 and scaled by beginning total assets. The main sample includes 6,279 firm-year observations from 2016 to 2019. The entropy sample includes CY and entropy-balanced non-CY firms over the same period. The PSM sample includes propensity score-matched CY and non-CY firms. Controls refer to the vector of control variables discussed in Section 2.3.2. Appendix 1 provides variable definitions. Standard errors are clustered by firm. I report t-statistics in parentheses. ***, **, and * indicate statistical significance at 1%, 5%, and 10% levels, respectively, using two-tailed tests.

Table 9
Alternative ways to address the systematic differences between CY and non-CY firms

	OperLease					
						Exclude
	Main	Main	Main	Fiscal year ending in		industries
	Sample	Sample	Sample	Jun. or later	Sep. or later	4,8,10&12
	(1)	(2)	(3)	(4)	(5)	(6)
2019×CY	**-0.173*****	**-0.185*****	**-0.224*****	**-0.176*****	**-0.162*****	**-0.155****
	(-3.10)	**(-3.16)**	**(-3.22)**	**(-3.40)**	**(-2.91)**	**(-2.54)**
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Controls×*CY*	Yes	No	Yes	No	No	No
Controls×*2019*	No	Yes	Yes	No	No	No
Fixed Effects	Firm, Year	Firm, Year	Firm, Year	Firm, Year	Firm, Year	Firm, Year
Adj. R^2	0.96	0.96	0.96	0.92	0.92	0.97
Obs.	6,279	6,279	6,279	5,466	4,948	4,080

This table reports the results of DID models that provide alternative ways to address the systematic differences between CY and non-CY firms. The full sample used in columns (1)-(3) contains 6,279 firm-year observations from 2016 to 2019. The subsamples used in columns (4) and (5) exclude firms that end the fiscal year before June and September, respectively. The subsample used in column (6) excludes firms in the following Fama French 12 industries: "4. Oil and Gas," "8. Utilities," "10. Healthcare, Medical Equip., and Drugs," and "12. Other." Operating lease use (*OperLease*) is measured as one-year-ahead operating lease payments multiplied by 100 and scaled by beginning total assets. Controls refer to the vector of control variables discussed in Section 2.3.2. Appendix 1 provides variable definitions. Standard errors are clustered by firm. I report t-statistics in parentheses. ***, **, and * indicate statistical significance at 1%, 5%, and 10% levels, respectively, using two-tailed tests.

Table 10
Robustness tests

	OL_PMT_All	*OL_GLS*	*OL_BLW*	*OL/Sales*	*OperLease*		*OperLease*
	Main Sample	Main Sample	Denominator > 0	Denominator > 0	Include Financials	Include Penny Stocks	Restricted Sample
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
2019×CY	**-0.290****	**-0.146***	**-3.043****	**-0.239*****	**-0.112*****	**-0.109****	**-0.129****
	(-2.01)	**(-1.69)**	**(-2.54)**	**(-3.16)**	**(-2.77)**	**(-2.39)**	**(-2.14)**
Volatility							-0.260
							(-0.93)
CFO							1.048**
							(2.02)
BorrowingCost							-0.292
							(-1.23)
Debt/Equity							-0.387**
							(-2.09)
PM							0.029
							(0.45)
AssetGrowth							-0.218***
							(-2.91)
Other Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Firm, Year	Firm, Year	Firm, Year	Firm, Year	Firm, Year	Firm, Year	Firm, Year
Adj. R2	0.96	0.95	0.79	0.92	0.96	0.95	0.95
Obs.	6,279	6,279	6,273	6,150	7,607	8,196	4,344

This table reports the robustness test results. The main sample used in columns (1) and (2) contains 6,279 firm-year observations from 2016 to 2019. For the subsamples used in columns (3) and (4), I require the denominators of the dependent variables to be strictly positive. The expanded samples in columns (5) and (6) add 1,328 financial firm-year observations and 1,917 penny stock firm-year observations, respectively, to the main sample. In column (7), I require sufficient data to calculate the additional control variables listed in the column. In columns (1)-(4), I replace *lag_OperLease*—a control variable—with the lagged dependent variable to control for mean-reverting patterns in leases. "Other Controls" refer to the vector of control variables discussed in Section 2.3.2. Appendix 1 provides variable definitions. Standard errors are clustered by firm. I report t-statistics in parentheses. ***, **, and * indicate statistical significance at 1%, 5%, and 10% levels, respectively, using two-tailed tests.

Table 11
Top 20 keywords across different keyword extraction methods

Rank	TF-IDF	KeyBERT	YAKE
1	Lease	Lease	Operating lease
2	Liability	Rental	Lease
3	Retail	Contractual	Lease liability
4	Corporate	Accrued	Lease term
5	Item	Financing	Lease payment
6	Asset	Renewal	Lease cost
7	Line	Contract	Finance lease
8	Associated	Rent	Liability operating
9	Fixed	Accounting	Lease asset
10	Payment	Amortization	Month March
11	Month	Expense	Lease expense
12	Facility	Classified	Asset lease
13	Data	Weighted average	Balance sheet
14	Arrangement	Leasing	Right-of-use asset
15	Operating	Discount	Remaining lease
16	Exceeding	Leasehold	ROU asset
17	Center	Warehouse	Lease operating
18	Multi-year	Occupancy	Variable lease
19	Component	Leaseback	Cash flow
20	ROU	Renew	Lease finance

This table reports the top 20 keywords extracted from lease disclosures using TF-IDF, KeyBERT, and YAKE. The sample includes the largest 100 public companies based on market capitalization.