THE EFFECTS OF PROPRIETARY INFORMATION ON CORPORATE DISCLOSURE AND TRANSPRENCY: EVIDENCE FROM TRADE SECRETS

Stephen A. Glaeser

A DISSERTATION

in

Accounting

For the Graduate Group in Managerial Science and Applied Economics

Presented to the Faculties of the University of Pennsylvania

in

Partial Fulfillment of the Requirements for the
Degree of Doctor of Philosophy

2018

Supervisor of Dissertation

Robert Holthausen
Professor of Accounting

Co-Supervisor of Dissertation

Christopher Armstrong
Associate Professor of Accounting

Graduate Group Chairperson

Catherine Schrand, Celia Z. Moh Professor, Professor of Accounting

Dissertation Committee:

Robert Holthausen, Professor of Accounting
Christopher Armstrong, Associate Professor of Accounting
Wayne Guay, Professor of Accounting

This dissertation is dedicated to my wife, Chloe, who has been, and continues to be, a constant source of support and advice. This dissertation would not have been possible without her sacrifices and help.

ACKNOWLEDGMENT

I am grateful to the members of my dissertation committee, Chris Armstrong (co-chair), Wayne Guay, and Bob Holthausen (co-chair), for their guidance and feedback. I thank Mary Barton, Mary Billings, Matt Bloomfield, Brian Bushee, Matt Cedergren, Chloe Glaeser, Mirko Heinle, S.P. Kothari, Rick Lambert, Gidget Murray, Tom Lys, Cathy Schrand, Dan Taylor, Jake Thomas, Florin Vasvari, Robert Verrecchia, and workshop participants at the Wharton School, the Carnegie Mellon Accounting Mini Conference Emerging Scholars Session, the 2016 Deloitte/AAA Doctoral Consortium, the University of Illinois at Chicago, the University of Southern California, the University of Michigan, Duke University, the University of North Carolina, the University of Chicago, Stanford University, Yale University, INSEAD, the Kellogg School of Management, and Indiana University for helpful comments and suggestions. I thank the authors for providing data from the following papers: Searle (2010); Dyreng, Lindsey, and Thornock (2013); Heider and Ljungqvist (2015); and Kogan, Papanikolaou, Seru, and Stoffman (2016). All errors are my own. I also thank Anne Beatty, Brian Mittendorf, and Darren Roulstone for encouraging me to pursue a PhD and providing guidance during my time at Ohio State.

I gratefully acknowledge the financial support of the Wharton School of the University of Pennsylvania, the Wharton Risk Management Center's Russell Ackoff Doctoral Student Fellowship, and the Mack Institute for Innovation Management Research Fellowship.

ABSTRACT

THE EFFECTS OF PROPRIETARY INFORMATION ON CORPORATE DISCLOSURE AND TRANSPARENCY: EVIDENCE FROM TRADE SECRETS

Stephen Glaeser

Robert Holthausen

Christopher Armstrong

I examine the effects of proprietary information on corporate transparency and voluntary disclosure. To do so, I develop and validate two measures of firms' reliance on trade secrecy: one based on 10-K disclosures and one based on subsequent litigation outcomes. I complement these measures by using the staggered passage of the Uniform Trade Secrets Act as a shock to trade secrecy. I find that firms that begin to rely more heavily on trade secrecy substitute increased voluntary disclosure of *nonproprietary* information for decreased disclosure of *proprietary* information. The total effect of trade secrecy is a decrease in corporate transparency.

TABLE OF CONTENTS

LIST OF TABLES

LIST OF ILLUSTRATIONS

The Effects of Proprietary Information on Corporate Disclosure and Transparency: Evidence from Trade Secrets

Introduction

I examine the effects of proprietary information on corporate disclosure and transparency. Prior work on the effects of proprietary information largely focuses on the effects of product market competition, and finds conflicting evidence that product market competition both encourages and discourages voluntary disclosure.[1] In this paper, I document the effects of a different type of proprietary information – trade secrets. Trade secrets are information that derive future economic value from not being appropriable by competitors (e.g., unpatented innovations). Because accounting reports largely focus on historical activity, they likely have limited ability to communicate the value of trade secrets (Dechow, Ge, and Schrand, 2010).

Prior work suggests that managers respond to decreases in the ability of accounting reports to communicate future economic value by increasing their voluntary disclosure.[2] However, managers cannot publicly disclose trade secret information to investors without also revealing that information to competitors and other third parties. Therefore, I predict

[1] E.g., empirically, Bamber and Cheon, 1998; Harris, 1998; Botosan and Harris, 2000; Botosan and Stanford, 2005; Rogers and Stocken, 2005; Verrecchia and Weber, 2006; Berger and Hann, 2007; Dedman and Lennox, 2009; Li, 2010; Bens, Berger, and Monahan, 2011; Ellis, Fee, and Thomas, 2012; Ali, Klasa, and Yeung, 2014; Huang, Jennings, and Yu, 2016; and Tian and Yu, 2017. E.g., analytically, Verrecchia, 1983; Dye, 1985; Dye, 1986; Darrough and Stoughton, 1990; Wagenhofer, 1990; Feltham, Gigler, and Hughes, 1992; Feltham and Xie, 1992; Darrough, 1993; and Gigler, 1994.

[2] See, e.g., Guay, Samuels, and Taylor, 2016. Coller and Yohn, 1997; Balakrishnan, Billings, Kelly, and Ljungqvist, 2014; Billings, Jennings, and Lev, 2015; and Barth, Landsman, and Taylor, 2017 find that managers respond to decreases in corporate transparency by increasing their voluntary disclosure.

that trade secrecy discourages the voluntary disclosure of information that is proprietary with respect to the trade secret (Verrecchia, 1983). Because of this nondisclosure, investors may be unable to ascertain the value of a firm's trade secret, increasing information asymmetry between investors and managers.

Nevertheless, managers may be able to ameliorate the information asymmetry around trade secrecy by increasing their disclosure of information that is nonproprietary with respect to the trade secret. For example, managers can release information about future earnings without revealing specifics about their trade secrets. Therefore, I predict managers will substitute increased voluntary disclosure of *nonproprietary* information for decreased disclosure of *proprietary* information when protecting trade secrets. However, managers' increased disclosure of nonproprietary information is unlikely to address fully the increased information asymmetry around trade secrets because the source of the asymmetry is the undisclosed proprietary information. Therefore, I predict that the net effect of trade secrecy is increased information asymmetry between managers and investors, and among investors (Kim and Verrecchia, 1997).[3]

Beyond the role of trade secrecy in informing us about the effects of proprietary information on corporate disclosure and transparency, examining trade secrets is also important because of their economic significance. The U.S. Chamber of Commerce estimates that publicly traded U.S. companies own $5 trillion in trade secrets, equivalent

---

[3] Kim and Verrecchia (1997) show that disclosure of managers' private information can theoretically increase or decrease information asymmetry among investors, depending on whether managers' private information would complement or substitute for investors' private information. I assume trade secrecy increases the information advantage of informed investors because managers are typically also investors and because some investors may be better equipped to discern the unknown value of trade secrets.

to approximately 20% of total market capitalization (Chamber of Commerce, 2016). Prior survey evidence suggests that firms view trade secrecy as more important than patents for protecting the value of innovations and choose not to patent the majority of their innovations.[4] Yet, despite their economic importance, our understanding of the effects and determinants of trade secrecy is limited (e.g., Cohen, 2010). The most likely reason for this gap in our understanding is that identifying the presence of trade secrets is challenging. In this paper, I use three separate, but complementary, empirical approaches to overcome this challenge.

First, I use the staggered passage of the Uniform Trade Secrets Act (UTSA) by different states at different times in a differences-in-differences research design (Png, 2017). The UTSA increased the obtainable remedies for misappropriation of trade secrets, extended many statutes of limitations on trade secret litigation, and reduced uncertainty about the legal protections afforded to trade secrets (Samuels and Johnson, 1990). Consequently, firms protected by the UTSA are more likely to pursue trade secrecy. A benefit of the UTSA tests is that they allow me to draw causal inferences about the effects of trade secrecy if the differences-in-differences assumptions are satisfied. However, compliance with the UTSA was imperfect because firms affected by the UTSA did not have to pursue trade secrecy, and unaffected firms were not prevented from pursuing trade secrecy. Therefore, a potential limitation of the UTSA tests is that they estimate a treatment effect for marginal compliers only. If the effects of trade secrecy are heterogeneous this

[4] See, e.g., Arundel and Kabla, 1998; Cohen, Nelson, and Walsh, 2000; Arundel, 2001; and Jankowski, 2012.

marginal treatment effect may not generalize to the average effect of trade secrecy (e.g., Angrist, Imbens, and Rubin, 1996; Blundell and Dias, 2009; Glaeser and Guay, 2017).

To address the potentially limited generalizability of the UTSA tests, I develop a disclosure-based measure to identify firms protecting trade secrets. Regulation S-K requires firms with valuable trade secrets to discuss the risk of misappropriation in the 10-K. Therefore, I identify the presence of a trade secret using 10-K discussions of trade secrecy. A strength of this measure is that it is broadly available: 41% of sample 10-Ks include a discussion of trade secrecy. However, a potential limitation of this measure is that it reflects the presence, but not the value, of a trade secret (i.e., it is imprecise).[5]

To address the potential imprecision of my disclosure-based measure, I develop a litigation-based measure of the value of individual trade secrets (Lerner, 2006; Searle, 2010). I base this measure on the court's assessment of trade secrets during trade secret misappropriation rulings and settlements. Specifically, I search all firms' 10-Ks for references to trade secret cases and collect settlement and ruling data for these cases from Lexis Nexis and Public Access to Electronic Records (PACER). I augment this search with all trade secret misappropriation cases tried criminally under the Economic Espionage Act (EEA). I use these cases to construct a measure of the value and development date of subsequently misappropriated trade secrets. A benefit of this measure is that it uses

[5] Bhattacharya and Ritter (1983) highlight how imprecision is an inherent and unavoidable aspect of equilibria characterized by information asymmetry. Their arguments applied to my setting suggests that if I could perfectly identify the value distribution of trade secrets across firms, then investors should be able to as well. If investors can identify the value of trade secrets, there should be no pooling of different firm types and no information asymmetry. Therefore, imprecision in the measurement of trade secret values is necessary for trade secrecy to result in information asymmetry (Berger, 2011 makes similar arguments regarding competition, agency costs, and disclosure). Researchers can overcome this limitation by examining information that was unobserved by investors, or only revealed *ex post* (e.g., Bhattacharya and Ritter, 1983; Bens et al., 2011).

information revealed *ex post* to measure information that was *ex ante* confidential. A potential limitation of this measure is that it is subject to selection biases because the revealed trade secret must be misappropriated and a subsequent ruling or non-confidential settlement agreement must reveal its value and development date.

I validate my litigation-based and disclosure-based measures as proxies for firms with trade secrets in a number of ways. I find that both measures are related to determinants of trade secrecy drawn from theory and survey responses, including firm size, research and development expenditures, and the legal protections afforded trade secrets. I also find that the proxies are negatively related to future patent filings and patent citations, and positively related to one another.

My empirical findings are summarized as follows. Firms that begin pursuing trade secrecy increase their propensity to redact portions of the 10-K, consistent with these firms limiting their disclosure of proprietary information. Redactions are a fitting (inverse) measure of proprietary disclosure in my setting because they must be approved by the SEC, and one of the few allowable justifications is a desire to protect proprietary information (17 CFR 200.80(b)(4)). Firms that begin pursuing trade secrecy also increase their propensity to issue earnings guidance and the total quantity of guidance they issue. To the extent that earnings information is nonproprietary with respect to trade secrets, this finding suggests that firms with trade secrets substitute nonproprietary disclosure for proprietary disclosure. Finally, firms that begin pursuing trade secrecy experience decreases in transparency, as reflected by increases in analyst absolute forecast errors, analyst disagreement, bid-ask spreads, and share illiquidity.

My findings are largely consistent across all three of my empirical approaches. Throughout my main tests, I include firm fixed effects to control for time invariant aspects of the firm, year fixed effects to control for common macroeconomic shocks, and controls for time-varying firm and state attributes. I also show that my inferences from using the UTSA as a shock to trade secrecy do not appear to be explained by pre-existing differential trends for firms affected by the UTSA. Collectively, my findings are consistent with my theoretical predictions and appear robust to alternative explanations.

I extend my main tests by examining the types of information that firms with trade secrets redact from the 10-K. I classify redacted information based on a modified version of the classification introduced by Boone, Floros, and Johnson (2016). Because this classification requires hand collection, I examine the subset of firms that adopt or cease trade secrecy and a matched sample of firms that do not. I find that firms that begin pursuing trade secrecy increase their propensity to redact information related to research, supplier, and customer agreements. These findings shed light on how firms use redactions to withhold information related to their research plans, production materials, and product information to protect their trade secrets.

I contribute to the literature that examines the effects of proprietary information on voluntary disclosure by documenting evidence that managers substitute nonproprietary disclosure for proprietary disclosure when relying on proprietary information. Prior empirical work in this area largely focuses on the effects of product market competition and finds mixed results. Several explanations have been offered for these mixed results, including the endogeneity of product market competition (Beyer, Cohen, Lys, and Walther, 2010; Berger, 2011), the difficulty of accurately measuring product market competition

(Ali et al., 2014), and the ambiguity of the relation between product market competition and the proprietary costs of different disclosure methods (Leuz, 2004; Lang and Sul, 2014). I address these potential issues by examining the effect of a plausibly exogenous shock to trade secrecy, not relying on measures of product market competition, and by examining disclosure methods that arguably have unambiguously low and high proprietary disclosure costs with respect to trade secrets.

I also contribute to the literature that explores the determinants of corporate innovation. The majority of studies in this literature use the number of patents granted to the firm, and/or the number of citations these patents receive, as a proxy for corporate innovation. However, my findings suggest that a reduction in patenting can also be explained by an increased use of trade secrecy, rather than solely by a decrease in innovation. My findings support the assertion of Horstmann, MacDonald, and Slivinski (1985, pg. 838) that, “patent counts are a flawed measure of innovative activity; firms simply do not patent all innovations.”

Finally, I build on the existing survey evidence on trade secrets by documenting large-sample evidence of the determinants and consequences of trade secrecy. Cohen (2010, pg. 192) highlights the importance of understanding trade secrecy, “Thus, the study of the use of secrecy has only begun, but is quite important, not only to help us understand the determinants of innovative activity and performance, but also for policy. Policy discussions on the strength of patents, for example, should proceed in light of firms’ other options for protecting their innovations.” The theoretical literature provides a rich set of predictions about trade secrets, many with potential policy implications. To the best of my knowledge, these predictions are untested on large samples of publicly traded firms. By developing and validating a new empirical measure of trade secrecy, my work may also help future researchers test these predictions.

I organize the remainder of the paper as follows. I provide background information on trade secrets and trade secret laws in Section 2. I describe my research design in Section 3 and discuss my sample, data sources, and variable measurement in Section 4. I present my results in Section 5 and provide concluding remarks in Section 6.

Background

2.1 Trade secrets

The UTSA defines a trade secret as information, including a formula, pattern, compilation, program, device, method, technique, or process, that:

(i) derives independent economic value, actual or potential, from not being generally known to, and not being readily ascertainable by proper means by, other persons who can obtain economic value from its disclosure or use, and

(ii) is the subject of efforts that are reasonable under the circumstances to maintain its secrecy.

Legal and economic scholars often refine this definition by adding the modifier "continuous" to economic value to distinguish trade secrets from ephemeral pieces of information, such as a secret bid or an undisclosed earnings number (Milgrim, 1967). Famous examples of trade secrets include the process for manufacturing WD-40 and General Electric's machine for manufacturing synthetic diamonds.

Logically, if trade secrecy has costs in the form of increased information asymmetry, then trade secrecy must also have benefits to explain why some firms use secrecy to protect innovations instead of relying solely on patent protections. One such benefit of trade secrecy is avoiding the disclosure costs of patenting.[6] The patent office dictates that patent applications include a sufficiently detailed disclosure of the innovation such that a person skilled in the relevant area could recreate the innovation independently of the inventor. Competitors can use the publicly disclosed patent application in conjunction with their own research efforts to surpass the patented innovation in quality, or to "invent around" the patent.[7]

Patenting is also costly because patent protections are limited by the scope of the granting government's judicial authority and by the patent term, whereas the patent disclosure is not. Accordingly, patenting will not protect the patentee from international, black market, or post-patent term competitors, but will provide these competitors with the blueprint for the innovation. Patenting is also potentially costly because patenting reveals information about costs and processes to customers and suppliers, and may expose patentees to litigation from non-practicing entities. Consistent with patenting representing a costly alternative to trade secrecy, the European Union Community Innovation Survey, the U.S. Census Bureau and National Science Foundation's Business Research and Development Innovation Survey (BRDIS), and the Carnegie Mellon Survey of Industrial

---

[6] E.g., Horstmann et al., 1985; Grossman and Helpman, 1991; Scotchmer, 1991; O'Donoghue, 1998; Anton and Yao, 2004; Saidi and Zaldokas, 2017.

[7] Harabi (1995) surveys 358 Swiss R&D experts and finds that "the ability of competitors to 'invent around' patented innovations and the perception that patent documents require the 'disclosure of too much information' are considered as the most important constraints on the effectiveness of patents."

R&D find that respondents rate secrecy as more valuable than patents for protecting innovations and chose not to patent the majority of their innovations.[8]

2.2 10-K discussions of trade secrecy

Regulation S-K requires firms with valuable trade secrets to discuss the risk of trade secret misappropriation in the 10-K. While firms have leeway in determining what warrants discussion, trade secrets are often reflected in firms' product offerings. Consequently, the existence of a trade secret is usually public information, while how the trade secret works is not (e.g., it was well known that General Electric had a trade secret because of the existence of the synthetic diamonds, what was not known was how General Electric's trade secret worked). Competitors cannot appropriate the trade secret without understanding how it works (i.e., it nature). Firms are therefore willing to acknowledge the existence of a trade secret in the 10-K, without revealing its nature.

Firms also receive legal benefits from disclosing the existence of a trade secret. Successfully litigating trade secret misappropriation requires plaintiffs to establish that the secret was "the subject of efforts that are reasonable under the circumstances to maintain its secrecy." Failure to do so will invalidate the plaintiff's case. Plaintiffs must also establish that the trade secret was sufficiently valuable such that the misappropriation resulted in economic harm. Failure to do so can result in reduced damage awards and a failure to secure an injunction. Firms can submit 10-K disclosures as evidence that the secret was sufficiently valuable to merit mention in the annual report, and that the firm was taking actions to protect its value.

[8] Respectively, Arundel and Kabla, 1998 and Arundel, 2001; Jankowski, 2012; Cohen, Nelson, and Walsh, 2000.

Providing evidence for subsequent trade secret litigation is not the only legal benefit of disclosing the existence of trade secrets in the 10-K. Prior to March 16, 2013, the United States was one of the few countries that determined patent rights using a first to invent legal doctrine and not a first to file legal doctrine. Under a first to invent doctrine, patent protections are invalidated if, “the invention was made in this country by another who had not abandoned, suppressed, or concealed it” (35 U.S.C. § 102(g)). Consequently, if a competitor reverse engineers a firm’s trade secret and patents it, the trade secret firm can still make use of the patented innovation (35 U.S.C. § 273(b)). However, the first party to file a patent application has the prima facie right to sole patent protections. To bypass the patent, the original inventor must engage in a legal proceeding and establish that they first conceived of the invention and took affirmative steps to make the invention publicly known. Firms can use the 10-K disclosure of a trade secret as a way to establish first conception and public knowledge.

2.3 The Uniform Trade Secrets Act (UTSA)

The Uniform Law Commission (ULC) published the UTSA in 1979, and amended it in 1985. The prefatory note to the UTSA states the original motivations behind the act:

> “A valid patent provides a legal monopoly for seventeen years in exchange for public disclosure of an invention. If, however, the courts ultimately decide that the Patent Office improperly issued a patent, an invention has been disclosed to competitors with no corresponding benefit. In view of the substantial number of patents that the courts invalidate, many businesses now elect to protect commercially valuable information by relying on the state trade secret protection law…
>
> “…Notwithstanding the commercial importance of state trade secret law to interstate business, this law has not developed satisfactorily. In the first place, its development is uneven. Although there typically are a substantial number of reported decisions in states that are commercial centers, this is not the case in less populous and more agricultural jurisdictions. Secondly, even in states in which there has been significant litigation, there is undue uncertainty concerning the parameters of trade secret protection, and the appropriate remedies for misappropriation of a trade secret.”

Prior to the UTSA, trade secret civil law was governed by unevenly developed and uncodified common law remedies across states. The ULC developed the UTSA to harmonize and codify the interstate legal treatment of trade secrets. States that pass the UTSA reduce the uncertainty of the legal protections afforded to trade secrets, increase obtainable remedies for trade secret violations, and extend the statute of limitations on trade secret litigation (e.g., Samuels and Johnson, 1990).

While the UTSA reduced the uncertainty of the legal protections afforded to trade secrets, it did not eliminate all uncertainty. The UTSA's protections only extend to appropriation via "improper means." Improper is a legal term whose exact definition likely varies on a case-by-case basis. However, the UTSA is clear that the term "improper" extends beyond illegal activities to include "otherwise lawful conduct which is improper under the circumstances; e.g., an airplane overflight used as aerial reconnaissance to determine the competitor's plant layout during construction of the plant. E. I. du Pont de Nemours & Co., Inc. v. Christopher, 431 F.2d 1012 (CA5, 1970), cert. den. 400 U.S. 1024 (1970). Because the trade secret can be destroyed through public knowledge, the unauthorized disclosure of a trade secret is also a misappropriation." The UTSA is also clear that improper means does not include reverse engineering.

Despite the name, the UTSA does not have a perfectly uniform effect across states. Some states slightly alter the language or provisions of the original UTSA and the state common law remedies that existed prior to the UTSA also differ. Consequently, the strength of the UTSA relative to the pre-existing common law differs across states. Nonetheless, I do not attempt to model the heterogeneous change in enforcement across states because doing so would require making subjective judgments about the relative importance of potentially incomparable effects (e.g., it is unclear how the effect of increasing allowable damages from double to treble in one state compares to the effect of allowing injunctive relief in another). I instead choose to minimize the number of subjective choices I make and model the effect of the UTSA's passage using a simple indicator. I summarize the passage of the UTSA by state and year in Appendix A.

Research Design

3.1 Identifying firms with trade secrets

3.1.1 Disclosure-based measure of trade secrecy

To identify firms with trade secrets, I search all 10-K filings on the SEC's EDGAR database for reference to "trade secrecy" or "trade secret."[9] I create an indicator, *Trade Secrecy*, that equals one in each firm-year that the 10-K includes a reference to either. I list several examples in Appendix B. I limit analyses that include *Trade Secrecy* to the post-1996 period, because EDGAR electronic filing was not mandatory for all firms until after May 5, 1996. I present descriptive statistics for *Trade Secrecy* in Table 1, Panel A. In 41% of sample firm-years *Trade Secrecy* equals one. The transition probabilities suggest that *Trade Secrecy* is "sticky." Firms that pursue trade secrecy one year also pursue trade secrecy the following year 83% of the time. Firms that do not pursue trade secrecy one year pursue trade secrecy the following year only 15% of the time. I tabulate the sample prevalence of *Trade Secrecy* by year in Figure 1 and *Trade Secrecy* "adoptions" and

[9] I use wildcard operators in all text searches to ensure that my searches capture modifications of the search words (e.g., trade secret, trade secrets, etc.).

cessations" by year in Figure 2. Both figures indicate that trade secrecy changes are not concentrated in any particular year.

Table 1

Panel A: Disclosure-based measure descriptive statistics

This Table presents descriptive statistics for my sample. My main sample is constructed from the intersection of CRSP and Compustat (accounting and stock price data) for the time period 1980 to 2013. Some tests require intersecting the main sample with other datasets. All tests using 10-K filing data are for the post-1996 period only, as the electronic filing of financial statements on EDGAR was not mandatory until after May 5, 1996. I exclude utilities (SIC codes 4900–4942) due to prevalence of regulation in that industry. Panel A reports descriptive statistics for my disclosure-based measure of trade secrecy, Panel B reports descriptive statistics for my litigation-based measure of trade secrecy. Panel C reports descriptive statistics for the other variables used in the study.

Trade secrets (disclosure measure)

Variable	Observations	Mean
Trade Secrecy (% of firm-years)	92,736	41%

Trade Secrecy Migration Matrix:

	Trade Secrecy = 1 (t+1)	*Trade Secrecy* = 0 (t+1)
Trade Secrecy = 1 (t=0)	83%	17%
Trade Secrecy =0 (t=0)	15%	85%

FF-48 Industries with the highest prevalence of trade secrecy:

Industry Name:	% of firm-years:
Pharmaceuticals	80%
Computers	74%
Measuring & Control Equipment	71%
Medical Equipment	71%
Business Services	66%
Electronic Equipment	64%
Recreation	53%
Electrical Equipment	52%
Machinery	48%
Rubber & Plastic Products	47%

FF-48 Industries with the lowest prevalence of trade secrecy

Industry Name:	% of firm-years:
Construction	23%
Beer and Liquor	23%
Trading	21%
Coal	20%
Transportation	17%

Petroleum & Natural Gas	15%
Real Estate	14%
Banking	11%
Metallic & Industrial Metal Mining	10%
Precious Metals	3%

Figure 1

This figure presents the percentage of sample firms that pursue trade secrecy each year. The percentage of firms for which *Trade Secrecy* = 1 (year) appears on the *y*-axis (*x*-axis).

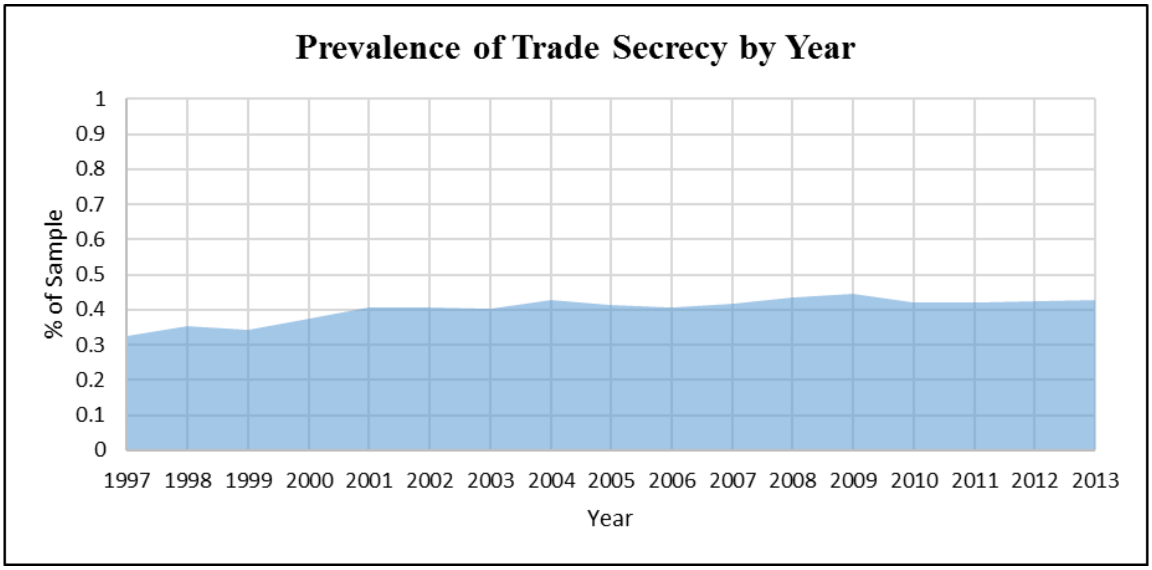
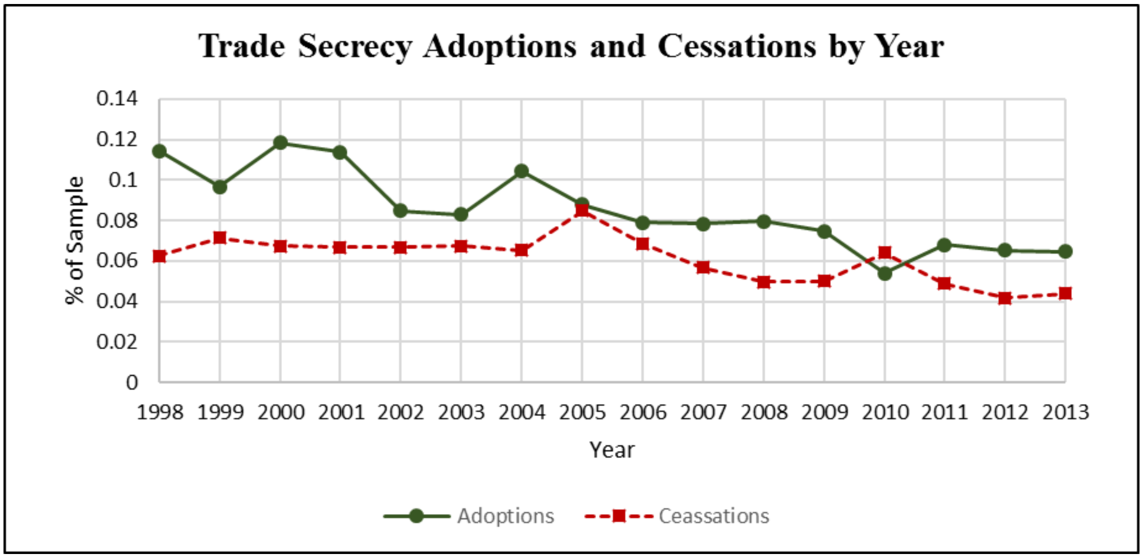


Figure 2

This figure presents the percentage of sample firms that adopt or cease trade secrecy each year. The percentage of firms for which *Trade Secrecy* changes (year) appears on the *y*-axis (*x*-axis). The dashed red line represents cessations and the solid green line represents adoptions.



I compare my measure to the 2011 BRDIS conducted by the U.S. Census Bureau to assess the potential degree of measurement error in the measure. Title 13 of the U.S. Code requires firms to respond truthfully to the BRDIS and guarantees the confidentiality of their responses. Because the survey is confidential, I cannot directly compare my measure to individual responses. However, because the BRDIS is confidential and truthful reporting is legally required, the survey results should be an unbiased and accurate reflection of the prevalence of trade secrecy.

The BRDIS is sent to a representative sample of U.S. firms, regardless of their R&D spending, size, industry, or public nature. Among surveyed firms with 1,000-4,999 employees, 28.9% rated trade secrecy as a "very important" form of intellectual property protection and 10.3% as a "somewhat important" form of intellectual property protection (the remaining 60.8% rated trade secrecy as "not important").[10] These frequencies are similar to the 42% frequency of trade secrecy among my sample firms in 2011 (the same year as the survey).

I also list the ten Fama-French 48 industries in which firms are most likely to pursue trade secrecy, and the ten Fama-French 48 industries in which they are least likely to do so, in Table 1, Panel A.[11] Firms in seemingly innovative industries are the most likely to discuss trade secrecy in the 10-K, while firms in seemingly non-innovative industries are the least likely. The industry prevalence of my measure is also similar to the results of the 2011 BRDIS: surveyed firms in the Computers and Electronic Parts; Petroleum and Coal Products; and Chemicals Industries (including drugs) were the most likely to report that trade secrets were important, while firms in the Real Estate and Rental Planning; Mining, Extraction, and Support; and Finance and Insurance industries were the least likely to respond that trade secrets were important.[12]

3.1.2 Litigation-based measure of trade secrecy

A potential concern with *Trade Secrecy* is that it is imprecise because it measures the presence of trade secrets, but not their value. To address the imprecision of *Trade Secrecy*, I use litigation outcomes to identify information about trade secret values that was *ex ante* unobserved by investors, but that was revealed *ex post*.

[10] I compare my sample to surveyed firms with 1,000 or more employees, to ensure rough equivalence in size. Similar results hold for firms with 5,000-9,999 employees (29.1% as very important and 9.5% as somewhat important), 10,000-24,999 employees (32.6% and 9.8%), and 25,000+ employees (55.1% and 15.2%).

[11] I exclude utilities from the sample due to the prevalence of regulation in that industry, which may impede or alter the nature of innovation.

[12] Note that the BRDIS uses different industry classifications.

Specifically, I search all 10-K filings on the SEC's EDGAR database for lawsuit keywords (e.g., "lawsuit," "plaintiff," "civil suit," etc.) within 200 words of the phrases "trade secret" or "trade secrecy," but not within 200 words of the phrase "risk factors." This results in over 3,000 potential trade secret lawsuits. I then read the associated 10-K disclosures to identify trade secret lawsuits, and augment these lawsuits with the 95 trade secret cases tried criminally under the Economic Espionage Act (EEA) assembled by Searle (2010).[13] I search for these court cases on Lexis Nexis and PACER to identify 1) the value of the trade secret, 2) the valuation method, 3) the development date of the trade secret, 4) the lawsuit dates, and 5) the nature of the trade secret.

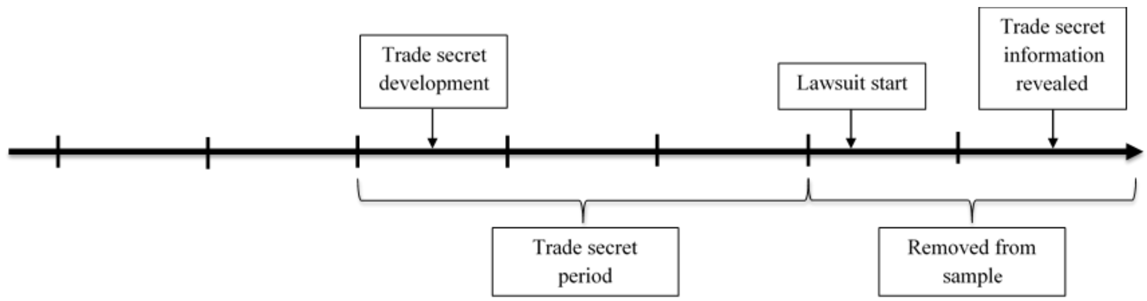
From this search, I identify 134 trade secret misappropriation cases with all necessary information available (751 firm-years). The average firm-year value of these trade secrets is $84.15 million, equivalent to 32% of the firm's market value of equity. Importantly, this is the court's (or settlement) assessment of the value of the trade secret, not the total damages or settlement amount. Figure 3 describes the timing of variable measurement for my litigation-based measure, *Trade Secret $*. I use the consumer price index to inflation adjust trade secret values from the revelation date to the trade secret period (e.g., a trade secret revealed in 2008 but developed in 2001 is inflation adjusted from 2008 dollars to 2001 dollars, 2002 dollars, etc.). I remove firm-years after the firm is involved in trade secret litigation in all analyses to minimize the risk that my results reflect

[13] Available here: https://research-repository.st-andrews.ac.uk/handle/10023/1632. I thank Nicola Searle for generously making the data publicly available. The EEA, enacted October 11, 1996, made the theft or misappropriation of a trade secret a federal crime. However, the EEA did not supplant state common law for civil actions. As such, most industrial trade secret cases are tried under state laws.

indirect effects of trade secrecy via litigation. I take the natural logarithm of one plus *Trade Secret $* because I expect the effects of trade secrecy to be proportional.

Figure 3

This figure presents the timeline of variable measurement for my litigation-based trade secrecy measure. Each dash represents the beginning or end of a firm-year.



I present descriptive statistics for *ln*(*Trade Secret $)* in Table 1, Panel B. The *ln*(*Trade Secret $)* sample begins in 1997 because it is constructed using information gathered from EDGAR. There are 751 firm-years in which the firm has a trade secret that is subsequently revealed via misappropriation (i.e., where *ln*(*Trade Secret $)* > 0). In 78% of these firm-years, *Trade Secrecy* = 1.[14] I also present descriptive statistics on the valuation method and the trade secret type in Table 1, Panel B. Due to the small sample of subsequently revealed trade secrets I do not examine the industry prevalence of *ln*(*Trade Secret $)*.

Table 1, continued

Panel B: Litigation-based measure descriptive statistics

This Table presents descriptive statistics for my sample. My main sample is constructed from the intersection of CRSP and Compustat (accounting and stock price data) for the time period 1980 to 2013.

[14] The 22% of firm-years in which *Trade Secrecy* = 0 largely occur early in the life of the trade secret before the trade secret is commercialized and has yet to affect performance. In unreported analyses, I find that changes in performance and *Trade Secrecy* occur simultaneously, changes in R&D spending first precede changes in *Trade Secrecy* by 3-4 years, and changes in redactions occur simultaneously with changes in *Trade Secrecy*. These results suggest that firms discuss trade secrets in the 10-K when the trade secret is revealed via changes in performance or in their product offerings (i.e., when the trade secret is commercialized).

Some tests require intersecting the main sample with other datasets. All tests using 10-K filing data are for the post-1996 period only, as the electronic filing of financial statements on EDGAR was not mandatory until after May 5, 1996. I exclude utilities (SIC codes 4900–4942) due to prevalence of regulation in that industry. Panel A reports descriptive statistics for my disclosure-based measure of trade secrecy, Panel B reports descriptive statistics for my litigation-based measure of trade secrecy. Panel C reports descriptive statistics for the other variables used in the study.

Trade secrets (litigation measure)

Variable	Observations	Mean	Std	25th	Median	75th
Trade Secret $ (in 1,000,000's)	58,305	0.37	9.22	0.00	0.00	0.00
ln(Trade Secret $)	58,305	0.08	1.19	0.00	0.00	0.00
Trade Secret $ (in 1,000,000's), positive subsample	751	84.12	147.63	5.51	31.35	88.10
ln(Trade Secret $), positive subsample	751	16.92	1.88	15.52	17.26	18.29
Trade Secrecy, positive subsample	751	78%	.	.	.	.

Trade secret litigation data

Trade secret type:	% of positive firm-years
Software	36%
Strategic/Customer	23%
Method	18%
Machine	13%
Formula	11%

Valuation method:	% of positive firm-years
Settlement	29%
Award	28%
Damages	22%
Reasonable Royalty	14%
Cost	7%

3.2 The determinants of trade secrecy

I begin my empirical analysis by documenting the determinants of my measures of trade secrecy. I do so to validate the construct validity of the measures, and because no prior non-survey work documents the determinants of trade secrecy. To accomplish these goals, I estimate two regressions of the following basic form:

$$Trade\ Secret\ Measure = \beta_0 + \beta_1 UTSA_{i,t} + \beta_2 Inevitable\ Disclosure\ Doctrine_{i,t} + \beta_3 Noncompete\ Enforcement\ Index_{i,t} + \beta_4 ln(Size)_{i,t} + \beta_5 Leverage_{i,t} + \beta_6 Return\ on$$

$$Assets_{i,t} + \beta_7 Market\ to\ Book_{i,t} + \beta_8 Return_{i,t} + \beta_9 \sigma Returns_{i,t} + \beta_{10} Loss\ Indicator_{i,t} + \beta_{11} Special\ Items_{i,t} + \beta_{12} Blockholders_{i,t} + \beta_{13} R\&D\ Expenditures_{i,t} + \beta_{14} Missing\ R\&D\ Indicator_{i,t} + IndustryFE + YearFE + \varepsilon_{i,t+1} \quad (1)$$

where *i* and *t* index firms and time, respectively. In the first specification, I use $Pr[Trade\ Secrecy_{i,t+1} = 1]$ as the dependent variables in a Probit regression. In the second specification, I use $ln(Trade\ Secret\ \$)_{i,t+1}$ as the dependent variable in an OLS regression. In all analyses using *ln(Trade Secret $)* as an independent or dependent variable, I remove all firm-years for which *Trade Secrecy = 1* to minimize the risk that the control group includes firms with trade secrets.[15]

I include three variables to capture regulatory shocks that should affect firms' incentives to use trade secrecy. The first is an indicator for whether the firm's headquarters state has passed the UTSA. I predict that firms protected by the UTSA are more likely to pursue trade secrecy because the UTSA increased the protections afforded trade secrets. The second is an indicator for whether the judiciary of the firm's headquarters state has applied the inevitable disclosure doctrine. The inevitable disclosure doctrine allows courts to find that a former employee will inevitably reveal any proprietary information they learned at their previous place of employment. The third is the noncompete enforceability index, which measures the likelihood the state judiciary will enforce a noncompete agreement (Garmaise, 2009; Aobdia, 2018).

[15] Alternatively, I could include *Trade Secrecy* as an additional control. However, *Trade Secrecy* and *ln(Trade Secret $)* are both measures of trade secrecy. Consequently, including one as a control for the other introduces a bad control problem. I do not exclude firm-years where Trade S*ecret Value* > 0 from regressions with *Trade Secrecy* as the dependent variable because in almost every firm-year in which this is true *Trade Secrecy* = 1.

I predict that firms protected by the inevitable disclosure doctrine or headquartered in states characterized by higher values of the noncompete enforcement index are more likely to pursue trade secrecy because these regulations reduce the risk that former employees will subsequently reveal trade secrets to competitors.[16] However, I do not focus on these regulations in my differences-in-differences tests because they affect the pursuit of trade secrecy through the specific channel of limits on employee mobility. Limits on employee mobility may affect firm outcomes through mechanisms other than trade secrecy. Consequently, it is not clear that any effect of these shocks is due solely to their effect on trade secrecy.

I include two separate measures of firms' concerns about priced adverse selection in external capital markets. The first measure is the firm's size as measured by the natural logarithm of its market value of equity. Larger firms typically have a lower cost of capital, are less affected by declines in the quality of their information environment, and may also have more internal resources (e.g., Archer and Faerber, 1966; Maksimovic and Phillips, 2013). The results of the 2011 BRDIS also suggest that larger firms, as measured by number of employees, are more likely to use trade secrecy. The second measure is the firm's leverage, as measured by long-term debt plus short-term debt scaled by total assets. Highly levered firms are more likely to be capital constrained and more concerned about the adverse selection costs of trade secrecy as a result (e.g., Myers and Majluf, 1984). I predict that firms concerned about priced adverse selection in external capital markets will

[16] Consistent with employee mobility being costly for firms with trade secrets, Erkens (2011) finds that R&D intensive firms in industries that rely on trade secrecy are more likely to use time-vested stock-based pay.

be less likely to use trade secrecy because of the information asymmetry effects of doing so (Glaeser, Michels, and Verrecchia, 2017).

I include the firm's annual R&D expenditures, scaled by total assets, to capture observable investments in innovation.[17] Following prior work, I replace missing values of R&D with zeroes (see Koh and Reeb, 2015 for a review of how prior work handles missing values of R&D). I predict that trade secrecy will be positively related to R&D expenditures, as trade secrecy is used to protect innovations. The results of the 2011 BRDIS also suggest that firms with R&D spending are more likely to rely on trade secrecy. Following Koh and Reeb (2015), I include an indicator for whether data on a firm's R&D expenditures is missing. I predict that firms with trade secrets will report their R&D expenditures, because firms with trade secrets must credibly communicate to investors that they have invested in innovation. I also include the firm's market to book ratio and predict that it will be positively related to trade secrecy, as trade secrets are intangible assets.

I include a variety of measures of firm performance (e.g., stock returns, return on assets, and an indicator for when the firm's net income is negative). I predict that historical firm performance will be negatively related to pursuing trade secrecy because poorly performing firms may innovate to improve their competitive position and choose to protect any resulting innovations with trade secrecy. I also include the number of blockholders, measured as the number of shareholders listed on Thomson Reuters who hold 5% or more of the firm's equity. I predict that the presence of blockholders will be positively related to pursuing trade secrecy, for two reasons. First, managers may be able to reduce the

[17] Hall and Ziedonis (2001) show that contemporaneous annual R&D expenditures parsimoniously measure the effect of historical R&D spending (i.e., the effect of R&D stocks).

information asymmetry costs of pursuing trade secrecy by credibly informing blockholders privately about the nature of trade secrets. Second, blockholders are sophisticated investors who can better discern the value of a trade secret, potentially reducing the expected information asymmetry costs of pursuing trade secrecy.

Finally, I follow prior literature on corporate transparency and disclosure and include special items scaled by total assets and the standard deviation of monthly returns as additional controls (e.g., Guay et al., 2016). I include year fixed effects to control for common macroeconomic shocks and time trends. I do not make predictions about the sign of the coefficients on these variables. In my cross-sectional determinants regression I include industry indicators to control for the differing industry prevalence of trade secrecy. In subsequent specifications, I replace the industry indicators with firm indicators to isolate the effects of trade secrecy to within-firm variation. I base my inferences on standard errors clustered by year and headquarters state.

3.3 The effects of trade secrecy

In this section, I describe how I examine the effects of trade secrecy. Throughout my analyses, I draw inferences from several separate, but complementary, empirical specifications. The first examines changes in outcomes that occur after a firm's reliance on trade secrecy changes:

$$\textit{Dependent Variable}_{i,t+1} = \beta_0 + \beta_1 \textit{Trade Secrecy Measure}_{i,t} + \gamma' X_{i,t} + \textit{FirmFE} + \textit{YearFE} + \varepsilon_{i,t} \quad (2)$$

I estimate Eq. (2) separately for both measures of trade secrecy (i.e., *Trade Secrecy* and *ln(Trade Secret $)*). The firm fixed effects in Eq. (2) control for the effect of any time-invariant aspects of the firm. To control for time-varying observable aspects of the firm, I include the vector $X_{i,t}$, which includes the determinants from Eq. (1). Consequently, I control for a great many alternative explanations for a relation between trade secrecy and firm outcomes. Nonetheless, the validity of my inferences may be threatened if, for example, firms are more likely to pursue trade secrecy when they anticipate changes in the dependent variable (i.e., selection).

To reduce concerns about selection, I also examine changes in outcomes after the passage of the UTSA by the firm's headquarters state. Specifically, I estimate the following generalized differences-in-differences specification:

$$\textit{Dependent Variable}_{i,t+1} = \beta_0 + \beta_1 \textit{UTSA}_{i,t} + \gamma' X_{i,t} + \textit{FirmFE} + \textit{YearFE} + \varepsilon_{i,t} \quad (3)$$

The coefficient estimate on *UTSA* captures the causal effect of trade secrecy on affected firms as long as the differences-in-differences assumptions are satisfied (Blundell and Dias, 2009).

The first differences-in-differences assumption is the parallel trends assumption (i.e., the assumption that the outcome for firms affected by the passage of the UTSA would have been the same as the outcome for unaffected firms, had they not been affected by the UTSA). The parallel trends assumption is satisfied as long as the passage of the UTSA was otherwise exogenous with respect to *changes* in the outcomes that I examine. Because outcomes in the absence of treatment are unobservable, the parallel trends assumption is inherently untestable. However, Ribstein and Kobayashi (1996) present evidence that the adoption of uniform laws, including the UTSA, is not driven by lobbying interests, but instead by the efforts of the ULC. Their finding suggests that the adoption of the UTSA was largely exogenous with respect to the outcomes of firms headquartered in adopting states.[18]

The second differences-in-differences assumption is the stable unit treatment value assumption ("SUTVA"). The SUTVA requires that the treatment status of one firm does not affect the potential outcomes of other firms. The SUTVA may be violated in my patenting tests because trade secrecy causes less disclosure of innovative activity via the patenting process. This reduced disclosure could lead to a reduction in the patenting rates of firms otherwise unaffected by the UTSA if patent disclosures cause innovative spillovers (e.g., Bloom, Schankerman, and Van Reenan, 2013). Consequently, such a violation of SUTVA would work against documenting a negative effect of trade secrecy on patenting activity.

The final differences-in-differences assumption is the perfect compliance assumption. Perfect compliance requires that no firms received the treatment in the pre-treatment period and that all firms in the treatment group—and only those firms—received the treatment in the post-treatment period. The perfect compliance assumption is violated in the UTSA setting because some firms in the control group adopt trade secrecy, some firms in the treatment group adopt trade secrecy prior to the passage of the UTSA, and some firms in the treatment group do not adopt trade secrecy.[19] However, a violation of the perfect compliance assumption does not prevent causal inference (e.g., Angrist et al., 1996; Blundell and Dias, 2009).

---

[18] I also find no evidence that firms relocate their headquarters to states that have passed the UTSA (e.g., the correlation between changes in headquarters states and changes in UTSA protection is -0.0072).

[19] Few quasi-natural experiments feature perfect compliance. For example, individuals may flee the country to avoid a random military draft or willingly enlist prior to a random draft (Angrist et al., 1996).

When compliance is imperfect, the monotonicity assumption replaces the perfect compliance assumption (e.g., Angrist et al., 1996; Heckman, Urzua, and Vytlacil, 2006). The monotonicity assumption is the assumption that treatment has a monotone effect on the behavior of affected firms (i.e., that there are no “defiers”). The monotonicity assumption would be violated in the UTSA setting if some affected firms cease trade secrecy, but would not have ceased trade secrecy in the absence of the UTSA. It is unlikely the monotonicity assumption is violated because the UTSA increased the protections afforded trade secrecy, and it is unlikely that increased protections would *cause* a firm to cease trade secrecy.[20]

Because compliance with the UTSA was imperfect, Eq. (4) identifies the LATE of “marginal adopters.” Marginal adopters are the firms that would not have pursued trade secrecy absent the additional protection of the UTSA. The outcomes of firms that pursue trade secrecy regardless of whether they are protected by the UTSA (i.e., “always adopters”) and those that never pursue trade secrecy (i.e., “never adopters”) are differenced out by the firm and year fixed effects. Therefore, the coefficient estimate on *UTSA* in Eq. (4) is a weighted average of zero effect for never adopters and always adopters, and the causal effect on marginal adopters (Blundell and Dias, 2009; Armstrong, Glaeser, and Huang, 2017).

I examine the effect of trade secrecy on patenting activity, proprietary disclosure, nonproprietary disclosure, and corporate transparency. I measure patenting activity using

[20] Although it is unlikely that the monotonicity assumption is violated in my setting, any violation of the monotonicity assumption will attenuate estimates so long as the effects of adopting and ceasing trade secrecy are symmetric (e.g., Heckman, et al., 2006).

the number of patents filed by the firm and the number of subsequent citations the firm receives on patents filed. I take the natural logarithm of one plus the number of patents filed in a year (*Patents Filed*), and of the citations received on those patents (*Patent Citations*) because I follow prior literature and assume that common shocks to patenting effect patenting rates proportionally.

To measure proprietary disclosure, I search the firm's 10-K filing for redaction keywords (e.g., "confidential treatment," "redacted," "CT order," etc.) Redactions are an attractive measure of (inverse) proprietary disclosure in my setting because it is likely that managers use redactions to protect information that could reveal a trade secret. For example, one of the few legal justifications the SEC will accept for redacting portions of a required filing is a desire to protect trade secrets (17 CFR 200.80(b)(4)). Following Verrecchia and Weber (2006), I use an indicator for whether the firm redacts portions of its 10-K as an inverse measure proprietary disclosure.

I measure nonproprietary disclosure using the number of management earnings forecasts issued during the twelve months after the filing of the 10-K. Manager forecasts are an attractive measure of nonproprietary disclosure in my setting because it is unlikely that forecasts can reveal proprietary information about a trade secret, and because forecasts are one of the most significant disclosure choices a manager can make (see Hirst, Koonce, and Venkataraman, 2008 for a review).[21] Following prior work, I use the natural logarithm of one plus the number of forecasts (*Forecast Frequency*) and an indicator that takes the

[21] Of course, manager forecasts can be proprietary with respect to other types of information (e.g., Huang et al., 2016).

value one if the manager releases at least one forecast in the subsequent year (*Guider*) (e.g., Guay et al, 2016).

Finally, I measure corporate transparency using information asymmetry between investors and managers, and among investors. To measure information asymmetry between investors and managers I use the absolute value of analyst forecast errors and analyst forecast dispersion. Following prior work, I take the natural logarithm of one plus both variables. When using analyst dispersion and analyst error as dependent variables, I follow prior work and include the natural logarithm of one plus the number of analysts following the firm as an additional control. To measure information asymmetry among investors, I use both the average bid-ask spread and the average of the Amihud (2002) measure of illiquidity.

3.4 The parallel trends assumption

To ensure that my differences-in-differences results are unexplained by pre-existing differential trends, I estimate the following differences-in-differences regression:

$$\textit{Dependent Variable}_{i,t+1} = \beta_0 + \beta_1 \textit{UTSA}_{i,t=-1,-2,-3} + \beta_2 \textit{UTSA}_{i,t=0} + \beta_3 \textit{UTSA}_{i,t=1,2,3} + \beta_4 \textit{UTSA}_{i,t>3} + \gamma' X_{i,t} + \textit{FirmFE} + \textit{YearFE} + \varepsilon_{i,t} \quad (4)$$

Eq. (4) is identical to Eq. (3), except that I include the variable UTSAi,t=-1,-2,-3 and separate UTSA into three variables based on the years since the UTSA's passage. A significant coefficient estimate on UTSAi,t=-1,-2,-3 suggests that firms affected by the UTSA were trending differently prior to the law's passage. I separate UTSA into three variables based on the time since the law's passage to document whether the effect of the UTSA is gradual or immediate.

Data and Sample Descriptives

I examine three regulations that affect firms based on their headquarters state: the passage of the UTSA, the application of the inevitable disclosure doctrine, and the components of the noncompete enforcement index (Garmaise, 2009).[22] To correct Compustat's headquarters data for errors, I use the Exhibit-21 headquarters data first described in Dyreng, Lindsey, and Thornock (2013) and the hand collected headquarters data first described in Heider and Ljungqvist (2015).[23]

I obtain data on patents and patent citations from Kogan, Papanikolaou, Seru, and Stoffman (2016). The authors download the entire history of U.S. patent documents from the Google Patents database and match the patent assignee to CRSP. Following prior work, I use the patent's file date instead of its grant date, as there is typically at least a year's lag between file date and grant date. I address the truncation bias in patent data by including fixed effects in all tests and only including observations that occur prior to the last three years of the patent database (Hall, Jaffe, Trajtenberg, 2001).[24] Consequently, my tests on patenting activity are limited to the 1980-2007 period, because the patent database ends in 2010.

---

[22] Most trade secret cases are tried based on the law of the plaintiff's "principle place of business," which is usually interpreted as the firms' headquarters (e.g., Almeling, Snyder, Sapoznikow, and McCollum, 2010).

[23] The SEC EDGAR header data is available on Scott Dyreng's website: https://sites.google.com/site/scottdyreng/Home/data-and-code/EX21-Dataset. The Heider and Ljungqvist (2015) data was provided by request. In cases where the two disagree, I use the SEC EDGAR header data. I thank the authors of both studies for making their data publicly available.

[24] Patent data involves truncation bias because citations are a forward-looking measure, and because patents do not appear in the database until they are granted (e.g., a patent granted in the last year of the database will have received very few citations and some filed patents that are in process in the year the database ends will not yet appear because they have yet to be granted).

I require non-missing data for control variables from the Compustat and CRSP databases in all tests. I obtain data on analyst following and analyst forecasts properties from I/B/E/S. Data on manager earnings guidance comes from the I/B/E/S guidance database, which begins in 1995. My text search of 10-Ks on EDGAR identifies 17% of 10-K filings as redacted, nearly matching the 16% rate of 10-K redaction documented by Verrecchia and Weber (2006) in their hand collected sample. My tests on the propensity to redact the 10-K begin in 1997 because I limit them to the period after the electronic filing of 10-Ks became mandatory.

Table 1, continued

Panel C: Other descriptive statistics

This Table presents descriptive statistics for my sample. My main sample is constructed from the intersection of CRSP and Compustat (accounting and stock price data) for the time period 1980 to 2013. Some tests require intersecting the main sample with other datasets. All tests using 10-K filing data are for the post-1996 period only, as the electronic filing of financial statements on EDGAR was not mandatory until after May 5, 1996. I exclude utilities (SIC codes 4900–4942) due to prevalence of regulation in that industry. Panel A reports descriptive statistics for my disclosure-based measure of trade secrecy, Panel B reports descriptive statistics for my litigation-based measure of trade secrecy. Panel C reports descriptive statistics for the other variables used in the study.

Other firm characteristics

Variable	Observations	Mean	Std	25th	Median	75th
Regulatory variables:						
Inevitable Disclosure Doctrine (% of firm-years)	176,343	42%	.	.	.	.
Noncompete Enforcement Index	176,343	3.47	2.38	1.00	4.00	5.00
UTSA (% of firm-years)	176,343	50%	.	.	.	.
Compustat & CRSP variables:						
Amihud Illiquidity	107,746	1.56	5.33	0.01	0.08	0.63
Bid-Ask Spread	107,746	2.73	3.53	0.30	1.47	3.63
Leverage	176,343	0.23	0.22	0.04	0.18	0.36
ln(Size)	176,343	4.92	2.25	3.29	4.79	6.45
Loss (% of firm-years)	176,343	34%	.	.	.	.
Market to Book	176,343	1.90	1.74	1.01	1.30	2.02
Missing R&D (% of firm-years)	176,343	49%	.	.	.	.
R&D	176,343	0.04	0.09	0.00	0.00	0.03
Return on Assets	176,343	-0.04	0.25	-0.03	0.02	0.07
Returns	176,343	0.13	0.64	-0.26	0.03	0.35

Special Items	176,343	-0.01	0.06	0.00	0.00	0.00
σReturns	176,343	0.15	0.09	0.08	0.12	0.18
Thomson-Reuters variables:						
Blockholders	176,343	0.99	1.42	0.00	0.00	2.00
Google Patents variables:						
ln(Patent Citations)	141,571	0.44	0.99	0.00	0.00	0.00
ln(Patents Filed)	141,571	0.82	1.77	0.00	0.00	0.00
I/B/E/S variables:						
Guider (% of firm-years)	99,139	33%	.	.	.	.
ln(Analyst Dispersion)	53,084	0.47	0.58	0.11	0.25	0.58
ln(Analyst Error)	64,127	0.76	0.86	0.15	0.43	1.06
ln(Analysts)	64,127	1.46	0.95	0.69	1.39	2.20
ln(Forecast Frequency)	99,139	0.62	0.98	0.00	0.00	1.10
EDGAR variables:						
Redacted 10-K (% of firm-years)	92,736	17%	.	.	.	.

Results

5.1 Determinants of trade secrecy

I present the results of estimating Eq. (1), which models my measures of trade secrecy as a function of determinants, in Table 2. I present the results when using *Trade Secrecy* as the dependent variable in a Probit regression in column (1), and when using *ln(Trade Secret $)* as the dependent variable in an OLS regression in column (2). I list the sign of my predictions for each variable in this table because I make a large number of predictions. I first interpret the predicted probabilities from the Probit model with all variables evaluated at their sample means.

Table 2

Determinants of trade secrecy

Column (1) of this table presents a Probit regression of my disclosure-based measure of trade secrecy, *Trade Secrecy*, as a function of cross-sectional determinants. Column (2) presents an OLS regression of my litigation-based measure of trade secret values, *ln(Trade Secret $)*, as a function of cross-sectional determinants. All variables are as defined in Appendix C. *t*–statistics appear in parentheses and are based on standard errors clustered by headquarters state and year. ***, **, and * denote statistical significance at the

0.01, 0.05, and 0.10 levels (two–tail), respectively. Sample descriptive characteristics are found in Table 1. I list my predictions for the sign of coefficients, where applicable.

Variable:	Prediction:	*Trade Secrecy* (1)	*ln(Trade Secret $)* (2)
UTSA	+	0.569***	0.366***
		(7.93)	(4.35)
Inevitable Disclosure Doctrine	+	0.358***	0.252***
		(8.77)	(4.04)
Noncompete Enforcement Index	+	0.004	-0.027
		(0.21)	(-1.47)
ln(Size)	+	0.041***	0.078***
		(6.93)	(3.12)
Leverage	-	-0.275***	-0.135**
		(-4.73)	(-2.29)
Return on Assets	-	0.035	0.376
		(0.38)	(1.18)
Market to Book	+	0.045***	-0.002
		(4.47)	(-0.13)
Returns	-	-0.070***	-0.039**
		(-8.59)	(-1.99)
σReturns	?	1.941***	0.597***
		(14.35)	(3.04)
Loss	+	0.187***	0.052
		(5.84)	(0.81)
Special Items	?	-0.493***	-0.748
		(-3.53)	(-1.40)
Blockholders	+	0.054***	0.018
		(11.04)	(1.53)
R&D	+	2.769***	4.521*
		(9.35)	(1.70)
Missing R&D	-	-0.278***	-0.226**
		(-6.02)	(-2.30)
Industry Fixed Effects		Yes	Yes
Year Fixed Effects		Yes	Yes
Observations		92,736	50,831
Pseudo R^2		0.268	.
Adjusted R^2		.	0.056

The results suggest that firms concerned about priced adverse selection in external capital markets avoid trade secrecy. In particular, the model predicts that a one standard deviation increase in firm leverage results in a 2.3 percentage point decrease in the probability that the firm pursues trade secrecy (*z*-statistic of -4.73). Similarly, the model predicts that a one standard deviation increase in firm size results in a 3.6 percentage point increase in the probability that the firm pursues trade secrecy (*z*-statistic of 6.93).

I find that two of the three regulatory shocks I examine are arguably the largest determinants of pursuing trade secrecy. The model predicts that firms protected by the inevitable disclosure doctrine are 16.4 percentage points more likely to pursue trade secrecy than unprotected firms (*z*-statistic of 8.77). The effect of the UTSA is even larger – the model predicts that firms protected by the UTSA are 21.8 percentage points more likely to pursue trade secrecy than unprotected firms (*z*-statistic of 7.93). The third regulatory shock I examine, the noncompete enforcement index, is also positively related to the pursuit of trade secrecy, although the relation is far from statistically significant (*z*-statistic of only 0.21).

Firms with more intangible assets, as measured by R&D expenditures and market to book ratios, appear more likely to pursue trade secrecy. In particular, the results suggest that a one standard deviation increase in R&D expenditures (market to book ratios) is associated with a 9.9 (3.1) percentage point increase in the probability that the firm pursues trade secrecy (*z*-statistics of 9.35 and 4.47, respectively). The model predicts that firms that do not report their R&D expenditures are 10.8 percentage points less likely to pursue trade secrecy, consistent with firms with trade secrets having to credibly communicate their investments in innovation, t (*z*-statistic of -6.02).

The presence of blockholders is also statistically significantly related to the pursuit of trade secrecy. The model predicts that a one standard deviation increase in the number of blockholders results in a 2.9 percentage point increase in the probability that the firm pursues trade secrecy (*z*-statistic of 11.26). This result is consistent with concentrated ownership facilitating trade secrecy, potentially because managers can privately communicate the nature of trade secrets to select shareholders, or because blockholders are sophisticated investors who can better discern the value of a trade secret.

I also find that poorly performing firms are more likely to pursue trade secrecy, possibly for manager agency reasons or because they must innovate to improve their competitive position. Specifically, firms with a prior accounting loss are 13.4 percentage points more likely to pursue trade secrecy (*z*-statistics 5.84). Similarly, a one standard deviation increase in returns is associated with a 1.7 percentage point decrease in the probability the firm pursues trade secrecy (*z*-statistic of -8.59). My third measure of firm performance, return on assets, is not statistically significantly related to the pursuit of trade secrecy (*z*-statistic of 0.38).

The results in column (2) when using *ln(Trade Secret $)* as the dependent variable are similar to the results in column (1) when using *Trade Secrecy*, although less statistically significant (possibly because of the smaller sample size). Specifically, the results in column (2) suggest that firms protected by the UTSA have trade secrets worth 44.2% more, and firms protected by the inevitable disclosure doctrine have trade secrets worth 28.7% more (*t*-statistics of 4.35 and 4.04, respectively).[25] The results in column (2) also suggest that the elasticity of trade secret value to firm size is 0.08%, and that a one standard deviation increase in firm leverage is associated with trade secrets worth 9.9% less (*t*-statistics of

---

[25] The coefficient estimates of 0.366 and 0.252 refer to the natural logarithm of trade secret values, so the laws are associated with a exp(0.366) -1 = 44.2% and exp(0.252)-1 = 28.7% greater value of trade secrets.

3.12 and -2.29, respectively). The results also suggest that a one standard deviation increase in R&D spending is associated with trade secrets worth 50.2% more, and that firms that do not report their R&D expenditures have trade secrets worth 20.2% less (*t*-statistics of 1.70 and -2.30, respectively). Finally, the results suggest that a one standard deviation increase in firm returns is associated with trade secrets worth 2.5% less (*t*-statistics of -1.99).

Collectively, I find evidence in Table 2 that my measures of trade secrecy are positively related to observable investments in innovation and the legal predictions afforded trade secrets, suggesting that the measures identify firms with trade secrets. I also find that the measures are negatively related to size and leverage, consistent with my prediction that smaller firms and more levered firms will avoid trade secrecy because of greater concerns about the cost of information asymmetry. My results that the prevalence of trade secrecy is increasing in firms' R&D expenditures and size are also consistent with the results of the BRDIS.

5.2 Trade secrecy and patenting

I present the results of estimating Eqs. (2) and (3) with my measures of patenting activity as dependent variables in Table 3. The results in column (1) suggest that the passage of the UTSA caused a 1.3% decline in relative average patent filings by affected firms (*t*-statistic of -1.94). The results in column (2) suggest that firms that adopt trade secrecy, as measured by 10-K discussions, also experience a 2.1% decline in relative average patent filings (*t*-statistic of -2.40). In contrast, the results in column (3) suggest there is no statistically significant relation between changes in patent filings and changes in trade secret values, as measured by *ln(Trade Secret $)*. However, the results in column (6) suggest that the elasticity of patent citations with respect to *ln(Trade Secret $)* is -0.04%

(*t*-statistic of -2.94). Similarly, the results in column (5) suggest that firms that adopt trade secrecy, as measured by 10-K discussions, experience a 6.5% decline in relative patent citations (*t*-statistic of -2.68). Finally, there is no evidence of a statistically significant relation between the passage of the UTSA and changes in patent citations in column (4). In total, the results in Table 3 suggest that trade secrecy causes a decline in patenting activity and suggests the two methods of protecting innovations are substitutes.

Table 3

Trade secrecy and patenting

This Table presents results from estimating OLS regressions of patenting activity as a function of the UTSA and my measures of trade secrecy. Firm and year fixed effects are included in all columns. All variables are as defined in Appendix C. *t*–statistics appear in parentheses and are based on standard errors clustered by headquarters state and year. ***, **, and * denote statistical significance at the 0.01, 0.05, and 0.10 levels (two–tail), respectively. Sample descriptive characteristics are found in Table 1.

Variable:	*ln(Patents Filed)*			*ln(Patent Citations)*		
	(1)	(2)	(3)	(4)	(5)	(6)
UTSA	-0.013*	.	.	-0.004	.	.
	(-1.94)	.	.	(-0.15)	.	.
Trade Secrecy	.	-0.021**	.	.	-0.067***	.
	.	(-2.40)	.	.	(-2.68)	.
ln(Trade Secret $)	.	.	-0.006	.	.	-0.037***
	.	.	(-0.74)	.	.	(-2.94)
Inevitable Disclosure Doctrine	-0.031	-0.031**	-0.018***	-0.061	-0.066	-0.048
	(-1.60)	(-2.36)	(-2.87)	(-1.60)	(-0.91)	(-0.82)
Noncompete Enforcement Index	-0.000	0.002	0.005*	-0.007	0.001	0.012
	(-0.05)	(0.42)	(1.84)	(-0.75)	(0.12)	(1.53)
ln(Size)	0.097***	0.077***	0.041***	0.151***	0.111***	0.090***
	(5.99)	(5.28)	(7.04)	(9.05)	(4.22)	(6.89)
Leverage	-0.007	-0.007	0.016	-0.061*	-0.050	0.124*
	(-0.49)	(-0.27)	(0.55)	(-1.70)	(-0.74)	(1.74)
Return on Assets	-0.001	0.014	-0.007	0.068*	0.118*	-0.031
	(-0.06)	(0.66)	(-0.31)	(1.89)	(1.77)	(-0.47)
Market to Book	-0.015***	0.000	-0.005	-0.019**	0.032***	0.006
	(-3.61)	(0.11)	(-0.93)	(-2.22)	(3.62)	(0.60)
Returns	-0.026***	-0.033***	-0.014***	-0.033**	-0.061**	-0.035***
	(-3.65)	(-3.12)	(-2.87)	(-2.45)	(-2.15)	(-3.74)
σReturns	0.121**	0.160	0.034	0.182	0.463*	0.163***
	(2.37)	(1.63)	(0.86)	(1.62)	(1.67)	(3.42)
Loss	0.018***	0.017***	0.003	0.045***	0.047***	0.011
	(3.23)	(4.04)	(0.43)	(4.07)	(3.77)	(1.00)
Special Items	0.005	0.056*	0.010	0.001	-0.105	0.012

	(0.21)	(1.83)	(0.12)	(0.01)	(-1.21)	(0.09)
Blockholders	-0.001	-0.003	0.000	-0.004	-0.006	0.003
	(-0.57)	(-0.79)	(0.12)	(-0.84)	(-1.07)	(0.98)
R&D	0.361***	0.202**	0.179	0.921***	0.642**	0.403
	(4.69)	(2.27)	(0.76)	(4.79)	(2.42)	(0.43)
Missing R&D	-0.038***	-0.037	-0.008	-0.078***	-0.064	-0.007
	(-2.99)	(-1.47)	(-0.22)	(-3.24)	(-1.39)	(-0.11)
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	141,571	60,292	43,510	141,571	60,292	43,510
Adjusted R^2	0.848	0.888	0.917	0.769	0.795	0.855

5.3 Trade secrecy and proprietary disclosure

I present the results of estimating Eqs. (2) and (3) with my measure of proprietary disclosure as dependent variables in Table 4. The results in column (1) suggest that the passage of the UTSA caused a 2.7 percentage point relative increase in the probability that the managers of affected firms redact portions of the 10-K (*t*-statistic of 2.29). The results in column (2) suggest that firms that adopt trade secrecy, as measured by 10-K discussions, also experience a 2.1 percentage point relative increase in the probability that the manager redacts portions of the 10-K (*t*-statistic of 5.03). Finally, the results in column (3) suggest that a doubling of trade secret values results in a 0.4 percentage point increase in the probability that the manager redacts portions of the 10-K (*t*-statistic of 1.86). These effects represent 15.9%, 12.9%, and 2.6% of the sample average, respectively, suggesting they are economically significant.[26] I conclude that trade secrecy causes a decline in proprietary disclosure, consistent with the arguments of Verrecchia (1983).

---

[26] A potential concern with these results is that I define *Redacted 10-K* using a text search. An alternative is to use confidential treatment order forms (CTOFs) from the EDGAR database to measure redactions. While this method is likely more accurate than a text search, the resulting tests are less powerful because CTOFs are not available on EDGAR until 2009. Nonetheless, I examine the robustness of my inferences to using the count of CTOFs filed by the firm in a given year as the dependent variable in Eqs. (2) and (3). I find a

Table 4

Trade secrecy and proprietary disclosure

This Table presents results from estimating OLS regressions of 10-K redaction as a function of the UTSA and my measures of trade secrecy. Firm and year fixed effects are included in all columns. All variables are as defined in Appendix C. *t*–statistics appear in parentheses and are based on standard errors clustered by headquarters state and year. ***, **, and * denote statistical significance at the 0.01, 0.05, and 0.10 levels (two–tail), respectively. Sample descriptive characteristics are found in Table 1.

Variable:	*Redacted 10-K*		
	(1)	(2)	(3)
UTSA	0.027**	.	.
	(2.29)	.	.
Trade Secrecy	.	0.022***	.
	.	(5.03)	.
ln(Trade Secret $)	.	.	0.004*
	.	.	(1.86)
Inevitable Disclosure Doctrine	0.006	0.001	0.002
	(0.71)	(0.09)	(0.47)
Noncompete Enforcement Index	-0.004*	-0.003	0.001
	(-1.71)	(-1.12)	(0.21)
ln(Size)	0.003	0.003	-0.000
	(0.92)	(0.89)	(-0.01)
Leverage	0.009	0.007	0.011
	(0.63)	(0.48)	(0.58)
Return on Assets	-0.002	-0.001	-0.011
	(-0.15)	(-0.08)	(-0.72)
Market to Book	-0.002	-0.002	-0.004**
	(-1.57)	(-1.26)	(-1.97)
Returns	0.003	0.002	0.004**
	(1.64)	(1.40)	(1.98)
σReturns	0.038***	0.037***	0.033
	(3.38)	(3.32)	(1.62)
Loss	0.002	0.002	0.006**
	(0.77)	(0.83)	(2.25)
Special Items	-0.036	-0.036	0.045
	(-0.70)	(-0.71)	(1.04)
Blockholders	0.003*	0.002	0.002
	(1.74)	(1.61)	(1.24)
R&D	0.053	0.049	0.104

positive relation between changes in this alternative measure of redactions and changes in *Trade Secrecy*, although this relation is statistically insignificant at conventional levels (coefficient estimate of 0.01, *t*-statistic of 1.02). However, I find a positive and statistically significant relation between the passage of the UTSA and changes in this alternative measure of redactions (coefficient estimate of 0.02, *t*-statistic of 2.13). The relation between changes in *ln(Trade Secret $)* and changes in this alternative measure of redactions is very low power (coefficient estimate <0.00, *t*-statistic of -0.14, with large standard errors). Using CTOFs to measure the "true" rate of redactions suggests *Redacted 10-K* has a type I error rate of 15% and a type II error rate of 23%.

	(1.18)	(1.10)	(1.08)
Missing R&D	-0.012	-0.012	-0.007
	(-1.05)	(-1.05)	(-0.57)
Firm Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Observations	92,736	92,736	58,305
Adjusted R^2	0.669	0.669	0.657

5.4 Trade secrecy and nonproprietary disclosure

I present the results of estimating Eqs. (2) and (3) with my measures of nonproprietary disclosure as dependent variables in Table 5. The results in column (1) suggest that the passage of the UTSA caused a 2.4 percentage point relative increase in the probability that the managers of affected firms initiate guidance (*t*-statistic of 3.47). The results in column (2) suggest that firms that adopt trade secrecy, as measured by 10-K discussions, experience a 1.3 percentage point relative increase in the probability that the manager initiates guidance (*t*-statistic of 2.99). However, there is no evidence in column (3) of a statistically significant relation between changes in trade secret values, as measured by *ex post* litigation outcomes, and changes in the probability that the manager initiates guidance.

Table 5

Trade secrecy and nonproprietary disclosure

This Table presents results from estimating OLS regressions of manager forecasting activity as a function of the UTSA and my measures of trade secrecy. Firm and year fixed effects are included in all columns. All variables are as defined in Appendix C. *t*–statistics appear in parentheses and are based on standard errors clustered by headquarters state and year. ***, **, and * denote statistical significance at the 0.01, 0.05, and 0.10 levels (two–tail), respectively. Sample descriptive characteristics are found in Table 1.

Variable:		*Guider*			*ln(Forecast Frequency)*	
	(1)	(2)	(3)	(4)	(5)	(6)
UTSA	0.024***	.	.	0.066***	.	.
	(3.47)	.	.	(3.15)	.	.
Trade Secrecy	.	0.013***	.	.	0.054***	.
	.	(2.99)	.	.	(3.20)	.

ln(Trade Secret $)	.	.	-0.001	.	.	0.015***
	.	.	(-0.58)	.	.	(2.63)
Inevitable Disclosure Doctrine	0.015**	0.006	0.002	0.029**	0.010	0.010
	(2.00)	(0.69)	(0.21)	(2.01)	(0.86)	(0.31)
Noncompete Enforcement Index	0.001	0.003*	0.005*	0.001	0.005**	0.009
	(0.62)	(1.81)	(1.81)	(0.20)	(2.38)	(1.47)
ln(Size)	0.078***	0.077***	0.077***	0.158***	0.159***	0.152***
	(17.32)	(13.50)	(10.88)	(17.40)	(14.81)	(12.13)
Leverage	0.095***	0.103***	0.098***	0.216***	0.231***	0.161***
	(7.30)	(7.64)	(4.57)	(6.03)	(6.09)	(2.73)
Return on Assets	0.045***	0.049***	0.035*	0.058	0.083*	0.019
	(3.13)	(3.05)	(1.77)	(1.52)	(1.87)	(0.43)
Market to Book	-0.014***	-0.014***	-0.015***	-0.041***	-0.042***	-0.041***
	(-5.22)	(-4.69)	(-3.20)	(-5.86)	(-5.35)	(-3.91)
Returns	-0.016**	-0.014*	-0.014**	-0.010	-0.006	-0.017
	(-2.34)	(-1.85)	(-2.25)	(-0.73)	(-0.43)	(-1.56)
σReturns	-0.017	-0.027	-0.008	-0.305***	-0.342***	-0.161**
	(-0.64)	(-0.90)	(-0.20)	(-3.64)	(-3.73)	(-2.05)
Loss	-0.029***	-0.032***	-0.022***	-0.082***	-0.091***	-0.058***
	(-6.17)	(-6.08)	(-4.71)	(-9.19)	(-10.37)	(-6.84)
Special Items	-0.042	-0.041	-0.080	-0.040	-0.043	-0.123
	(-1.02)	(-0.91)	(-1.38)	(-0.45)	(-0.42)	(-1.08)
Blockholders	0.008***	0.006***	0.005**	0.024***	0.020***	0.015***
	(4.25)	(2.93)	(2.56)	(5.13)	(3.72)	(2.59)
R&D	0.027	0.043	0.013	0.082	0.140	0.017
	(0.62)	(0.87)	(0.12)	(0.98)	(1.52)	(0.08)
Missing R&D	-0.004	0.008	0.005	-0.018	0.006	0.004
	(-0.29)	(0.52)	(0.24)	(-0.59)	(0.22)	(0.10)
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	99,139	85,959	58,305	99,139	85,959	58,305
Adjusted R^2	0.634	0.654	0.654	0.719	0.736	0.725

I repeat the sequence in columns (4) through (6) with the natural logarithm of one plus the number of forecasts issued by the manager as the dependent variable. The results in column (4) suggest that the passage of the UTSA caused a 6.8% increase in relative average forecasting activity by the managers of affected firms (*t*-statistic of 3.15). The results in column (5) suggest that firms that adopt trade secrecy, as measured by 10-K discussions, experience a 5.5% increase in relative average forecasting activity (*t*-statistic of 3.20). Finally, in contrast to the results in column (3), there is a statistically significant relation between changes in trade secret values, as measured by *ex post* litigation outcomes, and changes in forecasting activity. In particular, the results suggest that the elasticity of forecasting activity with respect to *ln(Trade Secret $)* is 0.02% (*t*-statistic of 2.63). In total, the results in Table 5 suggest that firms substitute nonproprietary disclosure for proprietary disclosure when relying on trade secrecy.

5.5 Trade secrecy and information asymmetry between managers and investors

I present the results of estimating Eqs. (2) and (3) with my measures of information asymmetry between investors and managers as dependent variables in Table 6. The results in column (1) suggests that the passage of the UTSA caused a 5.3% increase in the relative average of absolute analyst forecast errors for affected firms (*t*-statistic of 2.35). The results in column (2) suggest that firms that adopt trade secrecy, as measured by 10-K discussions, experience a 1.6% increase in the relative average of absolute analyst forecast errors (*t*-statistic of 2.04). However, I find no statistically significant relation between changes in trade secrecy values, as measured by ex post litigation outcomes, and changes in analyst forecast errors.[27]

[27] In unreported analyses, I find that trade secrecy has no effect, or a weakly positive effect, on earnings informativeness, suggesting the information asymmetry effects I document are not due to declines in the ability of GAAP to accurately reflect the production process. In particular, I estimate Eqs. (2) and (3) with firms' cumulative abnormal earnings announcement returns, defined as the firm's return over the day prior to the day after the annual earnings announcement minus the average return of firms in the same decile over the same period, as the dependent variable. I further modify Eqs. (2) and (3) by including firms' unexpected earnings, calculated as actual earnings per share minus the analyst consensus forecast, deflated by stock price at end of quarter (*Unexpected Earnings*). I interact firms' unexpected earnings with my trade secrecy variables. I also modify the resulting specification to include an indicator for whether unexpected earnings were negative and by interacting unexpected earnings with all of the included controls. In the majority of specifications, there is no statistically significant or economically meaningful relation between changes in trade secrecy and changes in the value relevance of unexpected earnings. The lone exceptions are when using the passage of the UTSA as a shock to trade secrecy after modifying the baseline specification (*UTSA* x *Unexpected Earnings* coefficient estimates of 0.106 and 0.118 and t-statistics of 1.94 and 2.24 when including a loss indicator and when interacting controls, respectively).

Table 6

Trade secrecy and information asymmetry between managers and investors

This Table presents results from estimating OLS regressions of information asymmetry between managers and investors as a function of the UTSA and my measures of trade secrecy. Firm and year fixed effects are included in all columns. All variables are as defined in Appendix C. *t*–statistics appear in parentheses and are based on standard errors clustered by headquarters state and year. ***, **, and * denote statistical significance at the 0.01, 0.05, and 0.10 levels (two–tail), respectively. Sample descriptive characteristics are found in Table 1.

Variable:	*ln(Analyst Error)*			*ln(Analyst Dispersion)*		
	(1)	(2)	(3)	(4)	(5)	(6)
UTSA	0.052**	.	.	0.031**	.	.
	(2.35)	.	.	(2.41)	.	.
Trade Secrecy	.	0.016**	.	.	0.000	.
	.	(2.04)	.	.	(0.06)	.
ln(Trade Secret $)	.	.	0.003	.	.	0.008**
	.	.	(0.75)	.	.	(2.13)
Inevitable Disclosure Doctrine	0.048**	0.044*	0.049**	0.012	0.000	-0.004
	(2.12)	(1.65)	(2.29)	(0.94)	(0.00)	(-0.29)
Noncompete Enforcement Index	-0.002	0.001	0.003	-0.001	-0.000	0.001
	(-0.66)	(0.31)	(0.32)	(-0.52)	(-0.04)	(0.16)
ln(Size)	-0.117***	-0.122***	-0.118***	-0.081***	-0.088***	-0.083***
	(-10.41)	(-10.79)	(-8.38)	(-14.22)	(-13.79)	(-8.15)
Leverage	0.215***	0.177***	0.231***	0.116***	0.093***	0.125**
	(7.33)	(7.68)	(3.85)	(3.44)	(2.80)	(2.28)
Return on Assets	0.019	0.036	-0.169*	-0.023	-0.011	-0.261**
	(0.38)	(0.66)	(-1.76)	(-0.49)	(-0.26)	(-2.58)
Market to Book	0.013**	0.016**	0.009	0.017***	0.019***	0.013*
	(1.96)	(2.40)	(0.69)	(3.16)	(3.28)	(1.71)
Returns	-0.073***	-0.074***	-0.097***	-0.064***	-0.060***	-0.077***
	(-6.90)	(-6.87)	(-6.01)	(-8.00)	(-7.69)	(-6.90)
σReturns	0.369***	0.337***	0.395***	0.329***	0.292***	0.394***
	(4.24)	(3.93)	(2.94)	(4.37)	(3.91)	(3.41)
Loss	0.115***	0.109***	0.105***	0.148***	0.136***	0.134***
	(6.63)	(5.88)	(3.52)	(10.75)	(10.25)	(5.82)
Special Items	0.047	-0.004	0.261	0.204*	0.182	0.463**
	(0.48)	(-0.04)	(1.61)	(1.71)	(1.52)	(2.07)
Blockholders	0.006**	0.006**	0.014**	0.002	0.001	0.006
	(2.40)	(2.15)	(2.37)	(0.76)	(0.30)	(1.08)
R&D	-0.047	-0.098	-0.119	-0.134	-0.126	0.037
	(-0.37)	(-1.04)	(-0.34)	(-1.07)	(-1.03)	(0.09)
Missing R&D	0.013	0.012	-0.038	0.008	0.008	0.006
	(0.59)	(0.50)	(-0.86)	(0.34)	(0.34)	(0.21)
ln(Analysts)	-0.043***	-0.041***	-0.039**	0.025***	0.023***	0.019
	(-5.52)	(-4.98)	(-2.21)	(3.32)	(3.30)	(1.54)
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Observations	64,127	56,895	33,388	53,084	47,235	27,552
Adjusted R^2	0.438	0.439	0.508	0.454	0.455	0.544

In contrast, I do find a statistically significant relation between changes in trade secrecy values and analyst dispersion in column (6). The results suggest that a doubling of trade secret values is associated with a 0.8% increase in analyst dispersion (*t*-statistic of 2.13). I also find evidence that the passage of the UTSA caused a 3.1% increase in analyst dispersion (*t*-statistic of 2.41), although I find no evidence of a relation between changes in *Trade Secrecy* and changes in analyst dispersion. In total, the results suggest that relying on trade secrecy results in information asymmetry between managers and investors.[28]

5.6 Trade secrecy and information asymmetry among investors

I present the results of estimating Eqs. (2) and (3) with my measures of information asymmetry among investors as dependent variables in Table 7. The results in column (1) suggests that the passage of the UTSA caused a relative average increase in the bid-ask spread of affected firms of 0.177, equivalent to 6.5% of the sample average (*t*-statistic of 3.13). Similarly, the results in column (2) suggest that firms that adopt trade secrecy, as measured by 10-K discussions, experience a relative average increase in bid-ask spreads of 0.059, equivalent to 2.2% of the sample average (*t*-statistic of 2.56). The results in

[28] In unreported analyses, I find no evidence of a relation between changes in trade secrecy and changes in manager forecast precision, relative bias, or accuracy. In particular, I find a coefficient estimate (*t*-statistic) of 0.003(0.52), 0.004(0.66), and 0.002(1.04) on *UTSA*, *Trade Secrecy*, and *ln(Trade Secret $)*, respectively, when using manager forecast precision (defined as -1 * the forecast range) as the dependent variable in Eqs. (2) and (3). I find a coefficient estimate (*t*-statistic) of 0.001(0.12), -0.015(-0.73), and 0.001(0.19) when using manager relative bias (defined as the difference between the manager's forecast and the analyst consensus forecast) as the dependent variable and of 0.018(0.84), 0.033(0.95), and -0.01(-1.01) when using manager forecast error (defined in the same way as analyst forecast error) as the dependent variable. This suggests that the results in Table 6 reflect increases in information asymmetry, and not general increases in uncertainty.

column (3) suggest that a doubling of the value of the firm's trade secrets is associated with a 3% relative average increase in bid-ask spreads (t-statistic of 3.11).[29]

Table 7

Trade secrecy and information asymmetry among investors

This Table presents results from estimating OLS regressions of information asymmetry among investors as a function of the UTSA and my measure of trade secrecy. Firm and year fixed effects are included in all columns. All variables are as defined in Appendix C. *t*–statistics appear in parentheses and are based on standard errors clustered by headquarters state and year. ***, **, and * denote statistical significance at the 0.01, 0.05, and 0.10 levels (two–tail), respectively. Sample descriptive characteristics are found in Table 1.

Variable:	*Bid-Ask Spread*			*Amihud Illiquidity*		
	(1)	(2)	(3)	(4)	(5)	(6)
UTSA	0.177***	.	.	0.226***	.	.
	(3.13)	.	.	(3.28)	.	.
Trade Secrecy	.	0.059**	.	.	0.035	.
	.	(2.56)	.	.	(0.85)	.
ln(Trade Secret $)	.	.	0.030***	.	.	0.006
	.	.	(3.11)	.	.	(0.26)
Inevitable Disclosure Doctrine	0.092*	0.123*	0.156*	0.087	-0.019	0.044
	(1.87)	(1.86)	(1.78)	(1.08)	(-0.25)	(0.29)
Noncompete Enforcement Index	-0.043***	-0.019	-0.047***	-0.056*	-0.034*	-0.074**
	(-3.86)	(-1.41)	(-3.70)	(-1.81)	(-1.69)	(-2.13)
ln(Size)	-0.797***	-0.571***	-0.657***	-1.204***	-1.021***	-1.178***
	(-11.61)	(-12.65)	(-12.11)	(-9.14)	(-7.66)	(-7.68)
Leverage	0.810***	0.456***	0.597***	0.842***	0.256	0.153
	(7.21)	(2.94)	(3.52)	(3.58)	(1.16)	(0.42)
Return on Assets	-0.957***	-0.688***	-0.680**	-1.554***	-0.997***	-1.795***
	(-7.48)	(-4.91)	(-2.17)	(-4.11)	(-3.10)	(-2.66)
Market to Book	0.017	-0.004	0.027	0.160***	0.164***	0.198***
	(1.04)	(-0.27)	(1.02)	(4.49)	(5.02)	(4.27)
Returns	-0.344***	-0.222***	-0.288***	-0.281***	-0.174***	-0.231***
	(-6.77)	(-4.43)	(-4.79)	(-4.13)	(-2.66)	(-2.74)
σReturns	0.144	-0.515	-0.342	-0.694	-1.332**	-0.876
	(0.36)	(-1.38)	(-0.63)	(-1.16)	(-2.00)	(-0.96)
Loss	0.341***	0.188***	0.248***	0.571***	0.334***	0.399***
	(7.06)	(4.55)	(4.64)	(5.77)	(4.66)	(4.16)
Special Items	0.906***	0.567**	0.715	1.743***	0.776	1.627
	(3.85)	(1.99)	(1.53)	(3.36)	(1.26)	(1.43)
Blockholders	-0.024*	-0.012	-0.007	-0.047**	-0.044**	-0.051**
	(-1.91)	(-1.03)	(-0.41)	(-2.24)	(-2.11)	(-2.04)
R&D	-2.036***	-1.472***	-0.761	-3.967***	-2.879***	-0.678
	(-5.04)	(-4.02)	(-0.70)	(-5.85)	(-3.83)	(-0.39)

[29] Including fiscal year-end closing price as a control does not significantly affect these results; coefficient estimate (*t*-statistic) of 0.0.174(3.10), 0.059(2.55), and 0.030(3.14) on *UTSA*, *Trade Secrecy*, and *ln(Trade Secret $)*, respectively (Cheong and Thomas, 2017).

Missing R&D	0.071	0.031	0.022	0.029	0.110	0.173
	(0.90)	(0.62)	(0.31)	(0.29)	(1.17)	(1.22)
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	107,746	72,645	49,153	107,746	72,645	49,153
Adjusted R^2	0.798	0.801	0.815	0.651	0.642	0.666

In contrast, I find no evidence in columns (5) and (6) of a relation between changes in my trade secrecy measures and changes in the Amihud (2002) illiquidity measure. However, I document a positive and statistically significant relation between the passage of the UTSA and changes in the illiquidity of affected firms' stock. Specifically, the results in column (4) suggest that the law's passage caused a relative increase in average illiquidity of 0.226, equivalent to 14.5% of the sample average (*t*-statistic of 3.28). In total, the results suggest that relying on trade secrecy causes information asymmetry among investors, in addition to between managers and investors.[30]

5.7 Additional state controls

Arguably, the most important assumption of my differences-in-differences tests is that the passage of the UTSA was exogenous with respect to changes in firm outcomes, conditional on the model's controls (i.e., the parallel trends assumption). A potential concern is that states that passed the UTSA were dissimilar to states that did not, and that these differences affected firm outcomes. Ribstein and Kobayashi (1996) find that this was

[30] In unreported analyses, I find that the relation between *Trade Secrecy* and information asymmetry declines the longer the firm pursues trade secrecy, consistent with subsequent performance revealing the value of the trade secret. In contrast, the relation between *Trade Secrecy* and redactions, forecasts, and patenting activity remains unchanged, or even grows, the longer the firm pursues trade secrecy. These results are consistent with firms continuing to protect the nature of the trade secret, forecast activity today representing a commitment to subsequent forecasting activity, and firms changing how they protect innovations when relying on trade secrecy, respectively.

not the case, and that states pass uniform laws mainly because of the efforts of the ULC.[31] I also find no observable systematic differences between states that passed the UTSA and states that did not. Nonetheless, in this section, I modify Eq. (3) by including the additional controls for state characteristics described in Table 8, Panel A.

Table 8

Panel A: Additional state controls

This Table presents definitions and descriptive statistics for additional state controls.

Additional state controls

Implied Contract Doctrine	An indicator equal to one if the headquarters state judiciary has applied the implied contract doctrine (Autor, Donohue, and Schwab, 2006).
R&D Tax Credit	Headquarters state statutory rate at which firms can claim a R&D tax credit.
Republican Governor	An indicator equal to one if the headquarters state's governor identifies as Republican.
Republican Legislature	An indicator equal to one if all houses of the headquarters state's legislature have a majority of members who identify as Republican and zero otherwise.
State Economic Growth	The change in gross state product, scaled by the beginning gross state product.
Manager Tax Rate	Highest combined federal and headquarters state income tax rate, assuming the individual is in top brackets at both the federal and state levels, married filing jointly with $150,000 in deductible property taxes, and allowing for deductibility of state income taxes in states where applicable (Armstrong, Glaeser, Huang, and Taylor, 2017).
State Corporate Tax Rate	Highest headquarters state corporate tax rate (Ljungqvist, Zhang, and Zuo, 2017).
Investment Tax Credit	Headquarters state statutory rate at which firms may claim an investment tax credit.

Additional state controls descriptive statistics

Variable	Observations	Mean	Std	25th	Median	75th

[31] In unreported analyses, I use the passage of the Uniform Transfers to Minors Act (UTMA) as an instrument for the passage of the UTSA. The UTMA was a non-commercial law published by the ULC in 1983, and subsequently adopted by 49 states (http://www.uniformlaws.org/Act.aspx?title=Transfers%20to%20Minors%20Act). The UTMA is likely a viable instrument because the passage of the UTMA is related to the passage of the UTSA for non-commercial reasons (e.g., lobbying by the ULC), but is not related to business lobbying or commercial activity. I find that the UTMA is a relevant instrument in the disclosure samples (first stage *t*-statistics > 6.00 and coefficient estimates >.60). It is less relevant in the other samples (*t*-statistics of 2.01 to 3.14, coefficient estimates of .26 to .40). In the second stage, the coefficients are very similar to the coefficients in my main UTSA tests, but are only statistically significant at conventional levels when using redactions and forecasting activity as dependent variables (coefficient estimates of 0.057, 0.058, 0.097 and *t*-statistics of 2.11, 2.13, and 1.74 when using *Redacted 10-K*, *Guider*, and *ln(Forecast Frequency)* as the dependent variable, respectively).

Additional state controls :						
Implied Contract Doctrine	176,343	0.70	0.46	0.00	1.00	1.00
R&D Tax Credit	176,343	0.03	0.05	0.00	0.00	0.06
Republican Governor	176,343	0.50	0.50	0.00	0.00	1.00
Republican Legislature	176,343	0.37	0.48	0.00	0.00	1.00
State Economic Growth	176,343	-0.04	0.09	-0.07	-0.05	-0.03
Manager Tax Rate	176,343	0.39	0.14	0.37	0.42	0.46
State Corporate Tax Rate	176,343	0.07	0.03	0.05	0.07	0.09
Investment Tax Credit	176,343	0.01	0.02	0.00	0.00	0.02

I present the results of estimating the modified Eq. (3) with those dependent variables that were statistically significantly related to the passage of UTSA in prior tests as dependent variables in Table 8, Panel B. I do not report coefficient estimates or test statistics for prior control variables in the interest of parsimony. The results in Table 8, Panel B suggest that the inclusion of these additional controls has no significant effect on the coefficient estimates for *UTSA*.[32,33] That these additional controls do not alter the coefficient estimate on *UTSA* suggests that they were not systematically related to the passage of the UTSA, supporting the parallel trends assumption.

Table 8

Panel B: Additional state controls analysis

This Table presents results from estimating OLS differences-in-differences regressions of prior dependent variables as a function of the UTSA. Controls included in prior analyses are included in all columns, but coefficients and test statistics are not reported. Firm and year fixed effects are included in all columns. Controls for the number of analysts following the firm are only included in columns (5) and (6). All non-additional state control variables are as defined in Appendix C. Additional state controls are as defined in Table 8, Panel A. *t*–statistics appear in parentheses and are based on standard errors clustered by headquarters state and year. ***, **, and * denote statistical significance at the 0.01, 0.05, and 0.10 levels (two–tail), respectively. Sample descriptive characteristics are found in Table 1. Additional state control descriptive characteristics are found in Table 8, Panel A.

Variable:	*ln(Patents Filed)*	*Redacted 10-K*	*Guider*	*ln(Forecast Frequency)*

[32] None of the eight *F*-statistics comparing the equality of the UTSA coefficients between Table 8, Panel B and prior tests is statistically significant at conventional levels.

[33] Similarly, I do not find that these additional controls affect prior results on the effects of trade secrecy when using *Trade Secrecy* or *ln(Trade Secret $)* to measure firms' reliance on trade secrecy.

	(1)	(2)	(3)	(4)
UTSA	-0.018**	0.028**	0.025***	0.089***
	(-2.07)	(2.24)	(3.40)	(4.43)
Implied Contract Doctrine	0.005	0.010	-0.008	0.029
	(0.36)	(0.64)	(-0.59)	(0.98)
R&D Tax Credit	0.074	0.021	0.094	0.083
	(0.36)	(0.25)	(0.71)	(0.32)
Republican Governor	-0.015*	0.003	-0.005	-0.024
	(-1.79)	(0.90)	(-0.87)	(-1.41)
Republican Legislature	0.010	-0.005	0.003	0.017
	(0.80)	(-1.01)	(0.57)	(1.02)
State Economic Growth	-0.032	0.017	0.037	0.204
	(-0.28)	(0.40)	(0.54)	(1.26)
Manager Tax Rate	-0.124	-0.023	0.155	-0.241
	(-0.88)	(-0.19)	(1.25)	(-1.01)
State Corporate Tax Rate	-0.354	0.102	-0.359	-0.285
	(-0.94)	(0.65)	(-1.25)	(-0.69)
Investment Tax Credit	1.111**	-0.184	-0.589***	-2.322***
	(2.17)	(-1.61)	(-2.64)	(-4.28)
Prior Controls	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	141,571	92,736	99,139	99,139
Adjusted R^2	0.848	0.669	0.634	0.719

Table 8

Panel B continued: Additional state controls analysis

Variable:	*ln(Analyst Error)* (5)	*ln(Analyst Dispersion)* (6)	*Bid-Ask Spread* (7)	*Amihud Illiquidity* (8)
UTSA	0.048**	0.035***	0.198***	0.217***
	(2.01)	(2.58)	(3.79)	(3.56)
Implied Contract Doctrine	0.013	0.029	-0.035	-0.015
	(0.85)	(1.15)	(-0.53)	(-0.14)
R&D Tax Credit	-0.127	-0.125	-0.583	1.144**
	(-0.74)	(-0.69)	(-1.27)	(2.03)
Republican Governor	-0.012	-0.004	-0.029	-0.057
	(-1.03)	(-0.53)	(-0.96)	(-1.64)
Republican Legislature	0.006	-0.005	0.074	0.083*
	(0.44)	(-0.49)	(1.59)	(1.74)
State Economic Growth	0.002	0.026	0.302	0.355
	(0.03)	(0.35)	(1.14)	(0.79)
Manager Tax Rate	0.004	-0.170	-0.132	-0.136
	(0.03)	(-1.13)	(-0.23)	(-0.18)
State Corporate Tax Rate	0.586**	0.408	0.524	1.634**

	(2.00)	(1.07)	(0.57)	(2.15)
Investment Tax Credit	-0.060	-0.170	-2.481**	-1.950**
	(-0.45)	(-0.68)	(-2.21)	(-2.49)
Prior Controls	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	64,127	53,084	107,746	107,746
Adjusted R^2	0.438	0.454	0.798	0.651

5.8 The parallel trends assumption

I present the results of estimating Eq. (4) with those dependent variables that were statistically significantly related to the passage of UTSA in prior tests as dependent variables in Table 9. I include the additional controls described in Section 5.7. I do not report coefficient estimates or test statistics for prior control variables in the interest of parsimony. None of the eight dependent variables I examine are statistically significantly related to the UTSA prior to the law's passage. However, the coefficient estimate on UTSA, $_{t=-1,-2,-3}$ when using *ln(Patents Filed)* as the dependent variable is economically large and nearly statistically significant (coefficient estimate of -0.023, *t*-statistic of -1.63). One potential explanation is that firms began relying on trade secrecy prior to the UTSA's passage. However, the other results do not support this explanation.[34]

Table 9
The parallel trends assumption

This Table presents results from estimating OLS differences-in-differences regressions of prior dependent variables as a function of UTSA t=-1,-2,-3; UTSA t=0; UTSA t=1,2,3; UTSA t>3. Prior controls and additional state controls are included in all columns, but coefficients and test statistics are not reported. Firm and year fixed effects are included in all columns. Controls for the number of analysts following the firm are only included in columns (5) and (6). All variables are as defined in Appendix C. t–statistics appear in parentheses and are based on standard errors clustered by headquarters state and year. ***, **, and * denote statistical significance at the 0.01, 0.05, and 0.10 levels (two–tail), respectively. Sample descriptive characteristics are found in Table 1.

[34] In particular, if I use my measures of trade secrecy as the dependent variable in Eq. (4) there is essentially no effect in the pre-period (coefficient estimates of 0.004(-0.049) and *t*-statistics of 0.42(-1.01) on *UTSA, t=-1,-2,-3* when using *Trade Secrecy*(*ln(Trade Secret $)*) as the dependent variable in unreported analyses.

Variable:	*ln(Patents Filed)*	*Redacted 10-K*	*Guider*	*ln(Forecast Frequency)*
	(1)	(2)	(3)	(4)
UTSA, t=-1,-2,-3	-0.023	-0.002	-0.004	-0.013
	(-1.63)	(-0.26)	(-0.77)	(-1.33)
UTSA, t=0	-0.024	0.000	-0.005	0.010
	(-1.31)	(0.02)	(-0.65)	(0.52)
UTSA, t=1,2,3	-0.023	0.017*	0.006	0.008
	(-1.42)	(1.71)	(1.54)	(0.61)
UTSA, t > 3	-0.021**	0.028**	0.024**	0.090***
	(-2.03)	(2.25)	(2.56)	(3.94)
Additional Controls	Yes	Yes	Yes	Yes
Prior Controls	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	141,571	92,736	99,139	99,139
Adjusted R^2	0.848	0.669	0.634	0.720

I conclude that in total my results are not explained by pre-existing differential trends, although some caution should be used when interpreting the effect of the UTSA on patenting activity. I also conclude that the effects of the UTSA occur relatively gradually after the passage of the law, as evidence by the small, statistically insignificant coefficient estimates on $UTSA_{i,t=0}$.

5.9 Extension: The nature of withheld information

In this section, I extend my main analyses by examining the type of information firms withhold to protect their trade secrets. To do so, I collect information on redacted material contracts and classify them based on a modified version of the classification introduced by Boone et al. (2016). Because this analysis requires extensive hand collection, I limit the analysis to the sample of firms that adopt or cease trade secrecy and a matched sample of control firms that did not.

5.9.1 Matching procedure

I use propensity score matching to match firms that change their reliance on trade secrecy to samples of control firms that did not. Specifically, I estimate the propensity score for the adoption of *Trade Secrecy* (defined as *Trade Secrecy* changing from 0 to 1 between two years) and the cessation of *Trade Secrecy* (defined as *Trade Secrecy* changing from 1 to 0 between two years) as a function of the change in my 13 control variables. I then separately match trade secret adopters and trade secrecy ceasers to firms that did not adopt or cease trade secrecy. I one-to-one nearest neighbor match without replacement. I assess the quality of the resulting match by examining covariate balance between treatment firms and control firms in Table 10, Panel A. None of the differences in means between the treatment sample and the control sample are statistically significant, suggesting the match is high quality.

Table 10

Panel A: Matched sample covariate balance

This Table presents the difference in means for a sample of firms that adopt or cease trade secrecy, and a sample of firms that do not. The samples are one-to-one matched without replacement based on nearest neighbor propensity scores calculated using the change in control variables. All variables are as defined in Appendix C. *t*–statistics appear in parentheses and are based on standard errors clustered by headquarters state and year. ***, **, and * denote statistical significance at the 0.01, 0.05, and 0.10 levels (two–tail), respectively.

	Trade Secret Adopters	Matched Sample	Difference in Means	*t*-statistic of the Difference
Change Inevitable Disclosure Doctrine	0.006	0.005	0.001	(0.04)
Change Noncompete Enforcement Index	0.012	0.013	-0.001	(-0.20)
Change ln(Size)	-0.057	-0.055	-0.002	(-0.45)
Change Leverage	0.015	0.016	-0.001	(-0.66)
Change Return on Assets	-0.030	-0.032	0.002	(0.65)
Change Market to Book	-0.269	-0.258	-0.011	(-0.45)
Change Returns	-0.063	-0.070	0.007	(0.48)

Change σReturns	0.006	0.007	-0.001	(-0.88)
Change Loss	0.016	0.021	-0.005	(-0.67)
Change Special Items	-0.006	-0.005	-0.001	(-0.56)
Change Blockholders	0.126	0.120	0.006	(0.36)
Change R&D	0.006	0.006	0.000	(0.25)
Change Missing R&D	0.000	0.000	0.000	(0.11)
Number of Observations	9,762	9,762	.	.

	Trade Secret Ceasers	Matched Sample	Difference in Means	*t*-statistic of the Difference
Change Inevitable Disclosure Doctrine	0.004	0.006	-0.002	(-0.68)
Change Noncompete Enforcement Index	0.000	0.007	-0.007	(-0.69)
Change ln(Size)	-0.002	0.002	-0.004	(-0.32)
Change Leverage	0.007	0.006	0.001	(0.24)
Change Return on Assets	-0.007	-0.007	0.000	(0.00)
Change Market to Book	-0.044	-0.029	-0.015	(-0.69)
Change Returns	-0.040	-0.032	-0.008	(-0.47)
Change σReturns	0.000	-0.001	0.001	(0.62)
Change Loss	0.021	0.027	-0.006	(-0.77)
Change Special Items	-0.001	-0.002	0.001	(0.23)
Change Blockholders	0.100	0.079	0.021	(1.01)
Change R&D	0.000	0.000	0.000	(0.32)
Change Missing R&D	-0.002	0.002	-0.004	(-1.25)
Number of Observations	5,859	5,859	.	.

5.9.2 Redacted material contract types

I focus this analysis on the redaction of material contracts, consistent with prior work on redactions (e.g., Verrecchia and Weber, 2006; Boone et al., 2016). Item 601 of regulation S-K requires firms to file any material contracts as exhibits and include an exhibit table with the 10-K. The exhibit table provides a centralized repository of the number of material contracts, the number of redacted contracts, and the nature of the contracts. To further limit the extent of hand collection, I focus on firm-years where *Redacted 10-K* changed.[35]

I classify material contracts using the procedure outlined in Boone et al. (2016). I modify their classification for my setting by considering customer and suppler contracts separately; combining credit, lease, and stockholder contracts into a single category; and separating research and consulting contracts, including the latter with peer contracts. These changes result in the seven contract variables describes in Table 10, Panel B. I report descriptive statistics for these new variables in Table 10, Panel B. I provide abridged examples of an exhibit table and each redacted contract type in Appendix D.

Table 10

Panel B: Redacted material contracts

This Table presents definitions and descriptive statistics for redacted material contract types. The material contract type classification is based on a modified version of the classification introduced by Boone et al. (2016).

Redacted Material Contract Types

License or Royalty	The change in the number of redacted material contracts related to license or royalty agreements, divided by the total number of contracts in the year that *Redacted 10-K* changed.
Peer or Consulting	The change in the number of redacted material contracts related to non-research joint ventures, strategic alliances, partnerships, co-branding, advertising, or consulting agreements, divided by the total number of contracts in the year that *Redacted 10-K* changed.
Research	The change in the number of redacted material contracts related to research and development agreements, alliances, or partnerships, divided by the total number of contracts in the year that *Redacted 10-K* changed.
Capital or Acquisition	The change in the number of redacted material contracts related to credit, leasing, or shareholder agreements or acquisition activity, divided by the total number of contracts in the year that *Redacted 10-K* changed.
Employment	The change in the number of redacted material contracts related to employment arrangements, divided by the total number of contracts in the year that *Redacted 10-K* changed.

[35] Consequently, I do not capture the effect of small changes in redactions (e.g., moving from six redacted contracts to five). I focus on major changes in redactions because I expect this to be a powerful setting that does not require hand collection across all 31,242 firm-years. Ignoring the information in small redaction changes should work against finding statistically significant results.

Supplier or Purchase	The change in the number of redacted material contracts related to manufacturing, inventory, distribution, vendor, production, or purchase from/by other parties, divided by the total number of contracts in the year that *Redacted 10-K* changed.
Customer or Sale	The change in the number of redacted material contracts related to manufacturing, inventory, distribution, vendor, production, or sale to/for other parties, divided by the total number of contracts in the year that *Redacted 10-K* changed.

Redacted material contract descriptive statistics

Variable	Observations	Mean	Std	25th	Median	75th
Redacted contract types:						
License or Royalty	31,242	0.00	0.14	0.00	0.00	0.00
Peer or Consulting	31,242	0.00	0.08	0.00	0.00	0.00
Research	31,242	0.04	0.48	0.00	0.00	0.00
Capital	31,242	0.00	0.15	0.00	0.00	0.00
Employment	31,242	0.01	0.29	0.00	0.00	0.00
Supplier or Purchase	31,242	0.03	0.32	0.00	0.00	0.00
Customer or Sale	31,242	0.03	0.32	0.00	0.00	0.00

I then use my redacted contract type variables as dependent variables in the following changes regression:

$$\Delta Contract\ Type_{i,t} = \beta_0 + \beta_1 \Delta Trade\ Secrecy_{i,t} + \gamma' \Delta X_{i,t} + YearFE + \varepsilon_{i,t} \qquad (5)$$

I predict that the redaction of license and royalty contracts will be positively related to changes in trade secrecy, as these contracts often include specification and feature information that may help competitors misappropriate a trade secret. For this same reason, I predict that the redaction of customer or sales contracts will be positively related to changes in trade secrecy. I also predict that the redaction of research contracts will be positively related to changes in trade secrecy, as research contracts include information about innovative projects that may be protected by trade secrecy. Finally, I predict that the redaction of supplier or purchase contracts will be positively related to changes in trade secrecy, as these contracts often include key information about the materials used in the production of trade secrets.

In contrast, I predict that the redaction of peer or consulting contracts will be unrelated to changes in trade secrecy, as it is unlikely that *non-research* joint venture, advertising, or consulting agreements include proprietary information about a trade secret. For this same reason, I predict that the redaction of capital or acquisition contracts will be unrelated to changes in trade secrecy. Finally, I predict that the redaction of employment contracts will be unrelated to changes in trade secrecy because employment contracts rarely include proprietary information about trade secrets.

5.9.3 Changes in trade secrecy and changes in redacted contract types

I present the results of estimating Eq. (5) with each of my seven measures of redacted material contract types as the dependent variable in Table 11. I list the sign of my predictions for each contract type because I make a large number of predictions. I do not report coefficient estimates or test statistics for control variables in the interest of parsimony.

Table 11

Trade secrecy and changes in redacted material contracts

This Table presents results from estimating OLS regressions of changes in material contract types on changes in *Trade Secrecy* and control variables. Year fixed effects are included in all columns. Controls are included in all columns, but coefficients and test statistics are not reported. All non-material contract variables are as defined in Appendix C. Material contract variables are as defined in Table 9, Panel B. *t*–statistics appear in parentheses and are based on standard errors clustered by headquarters state and year. ***, **, and * denote statistical significance at the 0.01, 0.05, and 0.10 levels (two–tail), respectively. All non-material contract sample descriptive characteristics are found in Table 1. Material contract descriptive characteristics are found in Table 10, Panel B.

Variable:	*Change in License or Royalty* (1)	*Change in Peer or Consulting* (2)	*Change in Research* (3)	*Change in Capital or Acquisition* (4)
Change in Trade	0.005	-0.000	0.291***	-0.005

Secrecy	(1.48)	(-0.01)	(4.48)	(-1.23)
Prediction:	+	0	+	0
Change in Controls	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	31,242	31,242	31,242	31,242
Adjusted R^2	0.003	0.003	0.048	0.002

Table 11, continued

Trade secrecy and changes in redacted material contracts

Variable:	*Change in Employment* (5)	*Change in Supplier or Purchase* (6)	*Change in Customer or Sale* (7)
Change in Trade Secrecy	0.017	0.192^{***}	0.149^{***}
	(0.98)	(3.60)	(3.34)
Prediction:	0	+	+
Change in Controls	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Observations	31,242	31,242	31,242
Adjusted R^2	0.002	0.025	0.021

Inconsistent with my prediction, I do not find evidence of a statistically significant relation between changes in the use of trade secrecy and changes in the redaction of license or royalty contracts in column (1). However, I find a statistically significant relation between changes in trade secrecy and changes in the redaction of research, supplier or purchase, and customer or sale contracts, consistent with my predictions. In particular, the results suggest firms that begin relying on trade secrecy, as measured by my disclosure measure, increase their redaction of research, supplier or purchase, and customer or sale agreements by 60.6%, 60%, and 46.6% of their sample standard deviation, respectively (t-statistics of 4.48, 3.60, and 3.34, respectively). As predicted, I also find no evidence of a statistically significant relation between changes in the use of trade secrecy and changes in the redaction of peer or consulting, capital or acquisition, and employment contracts. These results shed light on how firms use redactions to protect proprietary information about their trade secrets.

Conclusion

I study the determinants and consequences of trade secrecy using three complementary empirical approaches. The inferences I draw from all three approaches are largely the same: managers relying on trade secrecy reduce their voluntary disclosure of *proprietary* information and increase their voluntary disclosure of *nonproprietary* information. The total effect of trade secrecy is a decline in corporate transparency. These findings speak to the literature on proprietary costs by demonstrating that proprietary information can both increase and decrease disclosure depending on the proprietary content of the disclosure. Accordingly, my findings suggest subsequent research should distinguish between disclosures that can reveal proprietary information and those that cannot when testing theories about the effects of proprietary costs on disclosure.

I also contribute to the literature on firm innovation by showing that trade secrecy causes a decline in patenting activity. This finding suggests that the large literature that infers a decline in innovation from a decline in patenting activity is incomplete, as a substitution towards trade secrecy can also cause a decline in patenting activity. My work represents one of the few empirical studies of trade secrecy. I study a large number of determinants and consequences, but by no means an exhaustive set. However, I also develop and validate an empirical measure of firms that rely on trade secrecy, which may be of use in future research on trade secrecy.

APPENDIX

Appendix A. The UTSA by state and year

I list the year in which the UTSA was first effective for each state and the District of Columbia. Massachusetts and New York have not enacted a version of the law.

State	Year	State	Year
Alabama	1987	Montana	1985
Alaska	1988	Nebraska	1988
Arizona	1990	Nevada	1987
Arkansas	1981	New Hampshire	1990
California	1985	New Jersey	2012
Colorado	1986	New Mexico	1989
Connecticut	1983	New York	N/A
Delaware	1982	North Carolina	1981[36]
Florida	1988	North Dakota	1983
Georgia	1990	Ohio	1994
Hawaii	1989	Oklahoma	1986
Idaho	1981	Oregon	1988
Illinois	1988	Pennsylvania	2004
Indiana	1982	Rhode Island	1986
Iowa	1990	South Carolina	1992[37]
Kansas	1981	South Dakota	1988
Kentucky	1990	Tennessee	2000
Louisiana	1981	Texas	2013
Maine	1987	Utah	1989
Maryland	1989	Vermont	1996
Massachusetts	N/A	Virginia	1986
Michigan	1998	Washington	1982
Minnesota	1980	Washington D.C.	1989
Mississippi	1990	West Virginia	1986
Missouri	1995	Wisconsin	1986[38]
		Wyoming	2006

[36] The Patent, Trademark, and Copyright Committee of the North Carolina Bar Association initially sought to adopt the entirety of the UTSA, but decided that the definition sections were overly lengthy and complicated. See, e.g., Root and Blynn (1982) who draw on letters, drafts, and other papers, as well as telephone interviews in 1981 with the sponsor of the bill, Hon. Marvin D. Musslewhite, Jr. As such, I code North Carolina as having passed the UTSA from 1981 onwards.

[37] South Carolina repealed the UTSA in 1997 and replaced it with code law that has been viewed as providing, "even greater protection for trade secrets" – see Petitioner's Final Brief, Laffitte, 381 SC 460, 674 SE2d 154. As such, I code South Carolina as having passed the UTSA from 1992 onwards.

[38] Wisconsin adopted most of the UTSA's provisions but continues to use the Restatement of Torts' definition of "trade secrets." I code Wisconsin as having passed the UTSA from 1986 onwards.

Appendix B. Examples of 10-K discussions of trade secrecy

I search all 10-K filings on the SEC's EDGAR database for references to "trade secrecy" or "trade secrets." I present representative examples of such references below.

> *In order to protect its trade secrets and un-patented proprietary information arising from its development activities, AbTech Industries requires its employees, consultants and contractors to enter into agreements providing for confidentiality, non-disclosure and Company ownership of any trade secret or other un-patented proprietary information developed by employees, consultants or contractors during their employment or engagement by AbTech Industries.*
> -Abtech Holdings 10-K for the year ended December 31, 2011

> *En Pointe relies primarily on trade secrets, proprietary knowledge and confidentiality agreements to establish and protect its rights in its proprietary technologies, and to maintain its competitive position.*
> -En Pointe 10-K for the year ended September 30, 1998

> *The Company relies on trade secret protection for its proprietary deinking technology which is not covered by patent.*
> -Fort Howard Corporation 10-K for the year ended December 31, 1996

> *None of JSLT's products are covered by patents, but are produced under conditions of trade secrecy.*
> -Jet Set Life 10-K for the year ended June 30, 1999

> *We have the exclusive rights to 30 flavor concentrates developed with our current flavor concentrate suppliers, which we protect as trade secrets. We will continue to take appropriate measures, such as entering into confidentiality agreements with our contract packers and exclusivity agreements with our flavor houses, to maintain the secrecy and proprietary nature of our flavor concentrates.*
> -Jones Soda Company 10-K for the year ended December 31, 2003

> *Competitors also may obtain patents that the Company would need to license or design around. These factors also tend to limit the value of the Company's existing patents. Consequently, in certain instances, the Company may consider trade secret protection to be a more effective method of maintaining its proprietary positions.*
> -Minntech Corporation 10-K for the year ended March 31, 1996

> *The Company does not apply for patents on its speech recognition techniques that it maintains as trade secrets because of the disclosure requirements in doing so.*
> -Voice Control Systems, Inc. 10-K for the year ended December 31, 1998

Appendix C. Variable Definitions

Firm Characteristics

Amihud Illiquidity	The average daily value of the Amihud (2002) measure of illiquidity: $$Amihud\ Illiquidity = \frac{1}{Days_{i,t}}\sum_{d=1}^{Days_{i,t}} \frac{\lvert Return_{i,t,d}\rvert}{Volume_{i,t,d}} \times 10^7$$ where $Return_{i,t,d}$ is the daily return and $Volume_{i,t,d}$ is the daily dollar volume (in millions).
Bid-Ask Spread	The average daily value of the bid-ask spread, scaled by price: $$Bid - Ask\ Spread = \frac{1}{Days_{i,t}}\sum_{d=1}^{Days_{i,t}} \frac{Ask_{i,t,d} - Bid_{i,t,d}}{Price_{i,t,d}} \times 100$$ where $Ask_{i,t,d}$ ($Bid_{i,t,d}$) is the quoted closing ask(bid) and $Price_{i,t,d}$ is the closing price on day d.
Blockholders	The number of shareholders listed on Thomson Reuters with 5% or more ownership of the firm.
Guider	An indicator equal to one if the manager releases at least one earnings forecast.
Inevitable Disclosure Doctrine	An indicator equal to one if the headquarters state judiciary applies the inevitable disclosure doctrine.
Leverage	Book value of total debt to the book value of total assets.
ln(Analyst Dispersion)	The natural logarithm of one plus the standard deviation of analyst forecasts, scaled by the absolute value of actual earnings: $$ln(Analyst\ Dispersion) = ln\left(1 + \frac{\sigma AnalystForecasts_{i,t}}{\lvert Median\ Forecasted\ Earnings_{i,t}\rvert} \times 10\right)$$
ln(Analyst Error)	The natural logarithm of one plus the absolute value of the median consensus forecast, scaled by the absolute value of actual earnings: $$ln(Analyst\ Error) = ln\left(1 + \frac{\lvert Actual\ Earnings_{i,t} - Median\ Forecasted\ Earnings_{i,t}\rvert}{\lvert Actual\ Earnings_{i,t}\rvert} \times 10\right)$$
ln(Analysts)	The natural logarithm of the number of analysts with one-year ahead earnings forecasts.
ln(Forecast Frequency)	The natural logarithm of one plus the number of management earnings forecasts.
ln(Patent Citations)	The natural logarithm of one plus the number of future citations received on patents filed in the year.
ln(Patents Filed)	The natural logarithm of one plus the number of patents filed in the year.
ln(Size)	The natural logarithm of the market value of the firm's equity.
Loss	An indicator equal to one if net income is negative.
Market to Book	Market value of assets to book value of assets.

Missing R&D	An indicator equal to one if data on R&D expenditures is missing.
Noncompete Enforcement Index	The noncompete enforcement index developed by Garmaise (2009).
R&D	R&D expenditures scaled by total assets. Missing values of R&D are replaced by zeroes.
Redacted 10-K	An indicator equal to one if the 10-K filing includes mention of "confidential information" "confidential treatment" "redacted" "CT order" "FOIA" "rule 406" or "rule 24b-2."
Return on Assets	Income before extraordinary items scaled by assets.
Returns	Buy and hold return over the fiscal year.
Special Items	Special items scaled by total assets.
Trade Secrecy	An indicator equal to one if the firm's 10-K filing mentions "trade secret" or "trade secrecy."
ln(Trade Secret $)	The natural logarithm of one plus the dollar value of trade secrets revealed in trade secret legal settlements or judgements. I identify trade secret cases using all cases tried criminally under the Economic Espionage Act and text searches of 10-K filings. Data on the trade secret value and development date must be available. Dollar values are inflation adjusted from the settlement or judgement year to the years the trade secret was in use using the consumer price index.
UTSA	An indicator equal to one if the firm's headquarters state has enacted the UTSA.
UTSAi,t=-1,-2,-3	An indicator equal to one in each of the three years prior to the passage of the UTSA.
UTSAi,t=0	An indicator equal to one in the year the UTSA was first passed.
UTSAi,t=1,2,3	An indicator equal to one in each of the three years after the UTSA was first passed.
UTSAi,t>3	An indicator equal to one four years after the UTSA was first passed, and thereafter.
σReturns	The standard deviation of monthly returns.

Appendix D. Examples of redacted contract types

I hand collect the list of material contracts from the 10-K and classify redacted contracts based on a modified version of the classification introduced by Boone et al. (2016). I present an example of a 10-K table of material contracts along with representative examples of each type of redacted material contract below.

<u>Example of 10-K table of material contracts:</u>

```
(a) (3) Exhibits.

   The following Exhibits are incorporated herein by reference or are
filed
with this report as indicated below.

<TABLE>
<CAPTION>
 Number
Description
 ------                                                           -----
------
<S>         <C>
3.1*        Amended and Restated Certificate of Incorporation.
3.2**       Amended and Restated By-laws.
4.1***      Specimen common stock certificate.
4.2         See Exhibits 3.1 and 3.2 for provisions of the Amended and
Restated Certificate of Incorporation and Amended and
            Restated By-laws of the Registrant defining the rights of
holders of Common Stock of the Registrant.
10.1***     1999 Stock Incentive Plan.

…
10.24++     Software Development and Service Agreement, effective
January 15,
            2001, by and between the Registrant and BellSouth
             Telecommunications, Inc.
21.1        List of Subsidiaries.
23.1        Consent of Arthur Andersen LLP.
- ---------------
*    Incorporated by reference to Exhibit 3.2 of Predictive's
Registration
     Statement on Form S-1, No. 333-84045 ("Registration Statement No.
333-
     84045").
**   Incorporated by reference to Exhibit 3.4 of Registration Statement
No.
     333-84045.
***  Incorporated by reference to the identically numbered exhibit of
     Registration Statement No. 333-84045.
+    Non-confidential portions of this Exhibit were filed as the
identically
     numbered exhibit of Registration Statement No. 333-84045, which
non-
```

confidential portions are incorporated herein by reference. Confidential
treatment was granted for certain portions of this Exhibit pursuant to
Rule 406 promulgated under the Securities Act. Confidential portions of
this Exhibit have been filed separately with the Securities and Exchange
Commission.

++ Confidential treatment to be requested for certain portions of this
Exhibit pursuant to Rule 406 promulgated under the Securities Act.
Confidential portions of this Exhibit have been filed separately with
the Securities and Exchange Commission.

-Predictive Systems, Inc. 10-K for the year ended December 31, 2000

License or royalty:

RESTATED DRAM LICENSE AND COOPERATION AGREEMENT

This Restated DRAM License and Cooperation Agreement ("Agreement") is entered into as of February 28, 1996, by and between Alliance Semiconductor Corporation, a Delaware corporation with its principal offices at 3099 North First Street, San Jose, California, tel. (408) 383-4900; fax (408) 383-4990 (collectively, Alliance Semiconductor Corporation and its Taiwan subsidiar(ies) will be referred to as "Alliance")…

… 2.3 Notwithstanding anything to the contrary, [*] and Alliance: (i) Alliance will [*], (ii) UMC will [*], (iii) all such [*] under this Agreement, and (v) the [*]. At the request of Alliance, UMC will [*] Specifically, but without limitation, upon request by Alliance, UMC [*] If, for any reason, such [*], then UMC will [*]. To this end, UMC shall [*]. UMC hereby [*]

-Alliance Semiconductor Corporation 10-K for the year ended March 31, 1997

Peer or consulting:

ADVERTISING SALES AGENCY AGREEMENT

DATED MARCH 14, 2001

Confidential Treatment has been requested with respect to certain information contained in this Exhibit…

…6.1. The parties shall agree the Sales Budget for a particular calendar year on or before 30 November of the preceding year. If such agreement is not reached, the Sales Budget shall be the actual level of sales in US Dollars in the previous year plus a [*] increase. The Sales

Budget shall not include any amounts for political or religious Advertising revenues.

6.2. The CPP and the Rate Card shall be agreed between the parties before reaching agreement on the Sales Budget. The Agent may not propose a discount greater than [*] from the Rate Card price without the prior written consent of Studio 1+1 Group.

- Central European Media Enterprises Ltd 10-K for the year ended December 31, 2000

Research:

THIS COLLABORATION AGREEMENT (the "Agreement") is dated as of May 22, 2001 (the "Effective Date") by and between EXELIXIS, INC., a Delaware corporation having its principal place of business at 170 Harbor Way, P.O. Box 511, South San Francisco, California 94083-0511 ("EXEL"), and PROTEIN DESIGN LABS, INC., a Delaware corporation having its principal place of business at 34801 Campus Drive, Fremont, California 94555-3606 ("PDL"). EXEL and PDL are sometimes referred to herein individually as a "Party" and collectively as the "Parties."

RECITALS

A. PDL has expertise and capability in developing antibodies, in particular humanized antibodies, as pharmaceuticals.

B. EXEL has expertise and proprietary technology relating to drug discovery focused particularly on genetic model systems, genomics and computational biology and is applying such technology to discover and validate targets and products for drug discovery in a variety of disease areas.

C. PDL and EXEL desire to establish a collaboration to utilize the technology and expertise of PDL and EXEL to identify and characterize targets for the treatment of cancer and precancerous conditions, controlling cell growth, apoptosis, and proliferation, to generate antibodies directed against
such targets, and to develop and commercialize novel antibody products for diagnostic, prophylactic and therapeutic uses. …

… 1.4 "ANTIBODY TARGET" means [*]

1.5 "ANTIBODY TARGET CANDIDATE" means [*]

…

EXHIBIT D-1
THIRD PARTY TECHNOLOGY
[*]

- Exelixis 10-Q for the period ended June 30, 2001

Capital or acquisition:

SHAREHOLDERS AGREEMENT

THIS SHAREHOLDERS AGREEMENT (the "Agreement") is made and dated as of this 1st day of February 2005 by and among the individuals and entities listed as Existing Shareholders of the Company on Schedule A hereto (each an "Existing Shareholder" and, collectively, the "Existing Shareholders")…

…1.2 "Call Fair Market Value" per Ordinary Share shall be equal to the amount determined by [*].

-Monster Worldwide 8-K dated January 30, 2005

Employment:

EMPLOYMENT AGREEMENT

This Employment Agreement between CHRISTOPHER WILSON ("Executive") and RENTRAK CORPORATION, an Oregon corporation ("Corporation"), is entered into effective as of February 9, 2011 (this "Agreement"). …

… 2.2.1 Annual Bonus. Executive will be eligible to receive a cash bonus for services during each fiscal year during the Term beginning with fiscal 2012 and payable, to the extent earned, no later than June 30 of the following fiscal year. In addition, for the period from the Start Date through March 31, 2012, Executive will be eligible to earn a bonus ("Revenue Bonus") based on the net revenues for the Corporation's national linear TV network products and advertising agency and advertiser products ("Revenues") as described below ("Revenue Bonus Targets"). If the amount of Revenues is $* million, a bonus of $100,000 will be earned ("50% Revenue Bonus"). If the amount of Revenues is $* million, a bonus of $150,000 will be earned ("75% Revenue Bonus"). If the amount of Revenues is $* million or more, a bonus of $200,000 will be earned ("100% Revenue Bonus"). If the amount of Revenues is less than $* million, no bonus will be earned.

-Rentrak Corporation 10-Q for the period ended June 30, 2011

Supplier or purchase:

Subject: Letter Agreement No. 6-1162- PJG-064 to Purchase

Agreement No. 1663 - Pratt and Whitney Engine Model PW4074 Surge Mapping

…

STATEMENT OF WORK
- -----------------

[*CONFIDENTIAL MATERIAL OMITTED AND FILED SEPARATELY WITH THE SECURITIES AND EXCHANGE COMMISSION PURSUANT TO A REQUEST FOR CONFIDENTIAL TREATMENT]

-UAL Corporation 10-K for the year ended December 31, 1999

Customer or sale:

```
THIS SOFTWARE DEVELOPMENT AND SERVICES AGREEMENT (hereinafter
"Agreement") is made by and between BellSouth Telecommunications, Inc.,
a Georgia corporation, (hereafter "Customer") with offices located at
675 West Peachtree Street, N.E., Atlanta, Georgia 30375, and Predictive
Systems, Inc., a Delaware corporation (hereinafter "Supplier") having
an office at 2400 Century Boulevard, Atlanta, GA 30345.
…
1.6. "Deliverables" shall mean any and all system deliverables set
forth in a fully executed Order as defined in Appendix A.
…

                              Appendix A
                                 [*]
```

-Predictive Systems, Inc. 10-K for the year ended December 31, 2000

BIBLIOGRAPHY

Acharya, V. V., & Subramanian, K. V. (2009). Bankruptcy codes and innovation. Review of Financial Studies, 22 (12): 4949-4988

Agarwal, V., Vashishtha, R., & Venkatachalam, M. (2017). Mutual fund transparency and corporate myopia. Review of Financial Studies, forthcoming.

Aghion, P., Bloom, N., Blundell, R., Griffith, R., & Howitt, P. (2005). Competition and Innovation: An Inverted U Relationship, Quarterly Journal of Economics, 120 (2): 701-728

Aghion, P., Van Reenen, J., & Zingales, L. (2013). Innovation and institutional ownership. The American Economic Review, 103(1), 277-304.

Ali, A., Klasa, S., & Yeung, E. (2014). Industry concentration and corporate disclosure policy. Journal of Accounting and Economics, 58(2), 240-264.

Almeling, D. S., Snyder, D. W., Sapoznikow, M., & McCollum, W. E. (2010). Statistical analysis of trade secret litigation in state courts, A. Gonz.L.Rev., 46, 57.

Amihud, Y. (2002). Illiquidity and stock returns: Cross-section and time-series effects. Journal of Financial Markets, 5(1), 31-56.

Angrist, J. D., Imbens, G. W., & Rubin, D. B. (1996). Identification of causal effects using instrumental variables. Journal of the American Statistical Association, 91(434), 444-455.

Anton, J. J., & Yao, D. A. (2004). Little patents and big secrets: managing intellectual property. RAND Journal of Economics, 1-22.

Aobdia, D. (2018). Employee mobility, noncompete agreements, product-market competition, and company disclosure. Review of Accounting Studies, 23(1), 296-346.

Archer, S. H., & Faerber, L. G. (1966). Firm size and the cost of externally secured equity capital. The Journal of Finance, 21(1), 69-83.

Armstrong, C., Glaeser, S., & Huang, S. (2017). Corporate Hedging and the Design of Incentive-Compensation Contracts, Working paper.

Armstrong, C., Glaeser, S., Huang, S., & Taylor, D. J. (2017). The economics of managerial taxes and corporate risk taking, Working paper.

Arundel, A. (2001). The relative effectiveness of patents and secrecy for appropriation. Research Policy, 30(4), 611-624.

Arundel, A., & Kabla, I. (1998). What percentage of innovations are patented? empirical estimates for european firms. Research Policy, 27(2), 127-141.

Autor, D. H., Donohue III, J. J., & Schwab, S. J. (2006). The costs of wrongful-discharge laws. The Review of Economics and Statistics, 88(2), 211-231.

Azoulay, P., Graff Zivin, J. S., & Manso, G. (2011). Incentives and creativity: Evidence from the academic life sciences. The Rand Journal of Economics, 42(3), 527-554.

Balakrishnan, K., Billings, M. B., Kelly, B., & Ljungqvist, A. (2014). Shaping liquidity: On the causal effects of voluntary disclosure. The Journal of Finance, 69(5), 2237-2278.

Bamber, L. S., & Cheon, Y. S. (1998). Discretionary management earnings forecast disclosures: Antecedents and outcomes associated with forecast venue and forecast specificity choices. Journal of Accounting Research, 36(2), 167-190.

Barth, M. E., Landsman, W. R., & Taylor, D. J. (2017). The JOBS act and information uncertainty in IPO firms. The Accounting Review, forthcoming.

Bens, D. A., Berger, P. G., & Monahan, S. J. (2011). Discretionary disclosure in financial reporting: An examination comparing internal firm data to externally reported segment data. The Accounting Review, 86(2), 417-449.

Berger, P. G. (2011). Challenges and opportunities in disclosure research—A discussion of 'the financial reporting environment: Review of the recent literature'. Journal of Accounting and Economics, 51(1), 204-218.

Berger, P. G., & Hann, R. N. (2007). Segment profitability and the proprietary and agency costs of disclosure. The Accounting Review, 82(4), 869-906.

Bernstein, S. (2015). Does going public affect innovation? The Journal of Finance, 70(4) 1365–1403

Beyer, A., Cohen, D. A., Lys, T. Z., & Walther, B. R. (2010). The financial reporting environment: Review of the recent literature. Journal of Accounting and Economics, 50(2), 296-343.

Bhattacharya, S., & Guriev, S. (2006). Patents vs. trade secrets: Knowledge licensing and spillover. Journal of the European Economic Association, 4(6), 1112-1147.

Bhattacharya, S., & Ritter, J. R. (1983). Innovation and communication: Signalling with partial disclosure. The Review of Economic Studies, 50(2), 331-346.

Billings, M. B., Jennings, R., & Lev, B. (2015). On guidance and volatility. Journal of Accounting and Economics, 60(2), 161-180.

Bloom, N., Schankerman, M., & Van Reenen, J. (2013). Identifying technology spillovers and product market rivalry. Econometrica, 81(4), 1347-1393.

Blundell, R., & Dias, M. C. (2009). Alternative approaches to evaluation in empirical microeconomics. Journal of Human Resources, 44(3), 565-640.

Boone, A. L., Floros, I. V., & Johnson, S. A. (2016). Redacting proprietary information at the initial public offering. Journal of Financial Economics,120(1), 102-123.

Botosan, C. A., & Harris, M. S. (2000). Motivations for a change in disclosure frequency and its consequences: An examination of voluntary quarterly segment disclosures. Journal of Accounting Research, 38(2), 329-353.

Botosan, C. A., & Stanford, M. (2005). Managers' motives to withhold segment disclosures and the effect of SFAS no. 131 on analysts' information environment. The Accounting Review, 80(3), 751-772.

Bushee, B. J. (1998). The influence of institutional investors on myopic R&D investment behavior. Accounting Review, 305-333.

Cao, S., Ma, G., Tucker, J. W., & Wan, C. (2018). Technological peer pressure and product disclosure. The Accounting Review.

Chemmanur, T. J., Loutskina, E., & Tian, X. (2014). Corporate venture capital, value creation, and innovation. Review of Financial Studies, 27(8), 2434-2473

Cheong, F. S., & Thomas, J. (2017). Management of reported and forecast EPS, investor responses, and research implications. Management Science.

Cohen, W. M. (2010). Fifty years of empirical studies of innovative activity and performance. Handbook of the Economics of Innovation, 1, 129-213.

Cohen, W. M., Nelson, R. R., & Walsh, J. P. (2000). Protecting their Intellectual Assets: Appropriability Conditions and Why US Manufacturing Firms Patent (Or Not), Working paper.

Coller, M., & Yohn, T. L. (1997). Management forecasts and information asymmetry: An examination of bid-ask spreads. Journal of accounting research, 35(2), 181-191.

Cornaggia, J., Mao, Y., Tian, X., & Wolfe, B. (2015). Does banking competition affect innovation? Journal of Financial Economics, 115(1), 189-209.

Darrough, M. N. (1993). Disclosure policy and competition: Cournot vs. Bertrand. Accounting review, 534-561.

Darrough, M. N., & Stoughton, N. M. (1990). Financial disclosure policy in an entry game. Journal of Accounting and Economics, 12(1-3), 219-243.

Dass, N., Nanda, V., & Xiao, S. C. (2017). Intellectual property protection and financial markets: Patenting vs. secrecy, Working paper.

Dechow, P., Ge, W., & Schrand, C. (2010). Understanding earnings quality: A review of the proxies, their determinants and their consequences. Journal of accounting and economics, 50(2-3), 344-401.

Dedman, E., & Lennox, C. (2009). Perceived competition, profitability and the withholding of information about sales and the cost of sales. Journal of Accounting and Economics, 48(2), 210-230.

Dye, R. A. (1985). Strategic accounting choice and the effects of alternative financial reporting requirements. Journal of accounting research, 544-574.

Dye, R. A. (1986). Proprietary and nonproprietary disclosures. Journal of business, 331-366.

Dyreng, S. D., Lindsey, B. P., & Thornock, J. R. (2013). Exploring the role Delaware plays as a domestic tax haven. Journal of Financial Economics, 108(3), 751-772.

Ederer, F., & Manso, G. (2013). Is pay for performance detrimental to innovation? Management Science, 59(7), 1496-1513.

Ellis, J. A., Fee, C. E., & Thomas, S. E. (2012). Proprietary costs and the disclosure of information about customers. Journal of Accounting Research, 50(3), 685-727.

Epstein, M. A. (1998). Epstein on intellectual property. Aspen Publishers Online.

Erkens, D. H. (2011). Do Firms Use Time-Vested Stock-Based Pay to Keep Research and Development Investments Secret? Journal of Accounting Research, 49(4), 861-894.

Fang, V. W., Tian, X., & Tice, S. (2014). Does stock liquidity enhance or impede firm innovation? The Journal of Finance, 69(5), 2085-2125.

Feltham, G. A., Gigler, F. B., & Hughes, J. S. (1992). The effects of line-of-business reporting on competition in oligopoly settings. Contemporary Accounting Research, 9(1), 1-23.

Feltham, G. A., & Xie, J. Z. (1992). Voluntary financial disclosure in an entry game with continua of types. Contemporary Accounting Research, 9(1), 46-80.

Ferreira, D., Manso, G., & Silva, A. C. (2012). Incentives to innovate and the decision to go public or private. Review of Financial Studies, 27(1), 256-300.

Francis, J., & Smith, A. (1995). Agency costs and innovation some empirical evidence. Journal of Accounting and Economics, 19(2), 383-409.

Garmaise, M. J. (2009). Ties that truly bind: Noncompetition agreements, executive compensation, and firm investment. Journal of Law, Economics, and Organization.

Gigler, F. (1994). Self-enforcing voluntary disclosures. Journal of Accounting Research, 224-240.

Glaeser, S., & Guay, W. R. (2017). Identification and generalizability in accounting research: A discussion of Christensen, Floyd, Liu, and Maffett (2017). Journal of Accounting and Economics, forthcoming.

Glaeser, S. A., Michels, J., Verrecchia, R. E. (2017). Discretionary disclosure and management horizon: Evidence from patents, Working paper.

Grossman, G. M., & Helpman, E. (1991). Quality ladders in the theory of growth. The Review of Economic Studies, 58(1), 43-61.

Grossman, S. J. (1981). The informational role of warranties and private disclosure about product quality. The Journal of Law & Economics, 24(3), 461-483.

Guay, W., Samuels, D., & Taylor, D. (2016). Guiding through the fog: Financial statement complexity and voluntary disclosure. Journal of Accounting and Economics, 62(2-3), 234-269.

Hall, B., Helmers, C., Rogers, M., & Sena, V. (2014). The choice between formal and informal intellectual property: a review. Journal of Economic Literature, 52(2), 375-423.

Hall, B. H., Jaffe, A. B., & Trajtenberg, M. (2001). The NBER Patent Citation Data File: Lessons, Insights and Methodological Tools, No. w8498. National Bureau of Economic Research.

Hall, B. H., & Ziedonis, R. H. (2001). The patent paradox revisited: an empirical study of patenting in the US semiconductor industry, 1979-1995. RAND Journal of Economics, 101-128.

Harabi, N. (1995). Appropriability of technical innovations an empirical analysis. Research Policy, 24(6), 981-992.

Harris, M. S. (1998). The association between competition and managers' business segment reporting decisions. Journal of accounting research, 36(1), 111-128.

He, J. J., & Tian, X. (2013). The dark side of analyst coverage: The case of innovation. Journal of Financial Economics, 109(3), 856-878.

Heckman, J. J., Urzua, S., & Vytlacil, E. (2006). Understanding instrumental variables in models with essential heterogeneity. The Review of Economics and Statistics, 88(3), 389-432.

Heider, F., & Ljungqvist, A. (2015). As certain as debt and taxes: Estimating the tax sensitivity of leverage from state tax changes. Journal of Financial Economics, 118(3), 684-712.

Hirshleifer, D., Low, A., & Teoh, S. H. (2012). Are overconfident CEOs better innovators? The Journal of Finance, 67(4), 1457-1498.

Hirst, D. E., Koonce, L., & Venkataraman, S. (2008). Management earnings forecasts: A review and framework. Accounting Horizons, 22(3), 315-338.

Horstmann, I., MacDonald, G.M., & Slivinski, A. (1985). Patents as information transfer mechanisms: To patent or (maybe) not to patent. The Journal of Political Economy, 93(5), 837-858.

Hsu, P., Tian, X., & Xu, Y. (2014). Financial development and innovation: Cross-country evidence. Journal of Financial Economics, 112(1), 116-135.

Huang, Y., Jennings, R., & Yu, Y. (2016). Product market competition and managerial disclosure of earnings forecasts: Evidence from import tariff rate reductions. The Accounting Review.

Jankowski, J. E. (2012). Business use of intellectual property protection documented in NSF survey. NSF Info Brief, NSF, 12-307.

Kim, O., & Verrecchia, R. E. (1997). Pre-announcement and event-period private information. Journal of Accounting and Economics, 24(3), 395-419.

Kogan, L., Papanikolaou, D., Seru, A., & Stoffman, N. (2016). Technological Innovation, Resource Allocation, and Growth. Quarterly Journal of Economics, forthcoming.

Koh, P., & Reeb, D. (2015). Missing R&D. Journal of Accounting and Economics, 60(1), 73-94.

Lang, M., & Sul, E. (2014). Linking industry concentration to proprietary costs and disclosure: Challenges and opportunities. Journal of Accounting and Economics, 58(2), 265-274.

Lerner, J. (2006). Using litigation to understand trade secrets: a preliminary exploration, Working paper.

Lerner, J., Sorensen, M., & Strömberg, P. (2011). Private equity and long-run investment: The case of innovation. The Journal of Finance, 66(2), 445-477.

Leuz, C. 'Proprietary versus non-proprietary disclosures: Evidence from Germany,' in The Economics and Politics of Accounting: International Perspectives on Research Trends, Policy and Practice: Oxford University Press, 2004: 164-197.

Li, X. (2010). The impacts of product market competition on the quantity and quality of voluntary disclosures. Review of Accounting Studies, 15(3), 663-711.

Li, Y., Lin, Y., & Zhang, L. (2017). Trade Secrets Law and Corporate Disclosure: Causal Evidence on the Proprietary Cost Hypothesis. Journal of Accounting Research, forthcoming.

Ljungqvist, A., Zhang, L., & Zuo, L. (2017). Sharing Risk with the Government: How Taxes Affect Corporate Risk Taking. Journal of Accounting Research, 55(3), 669-707.

Maksimovic, V., & Phillips, G. M. (2013). Conglomerate firms, internal capital markets and the theory of the firm, Working paper.

Milgrim, R. S. (1967). Milgrim on Trade Secrets.

Myers, S. C., & Majluf, N. S. (1984). Corporate financing and investment decisions when firms have information that investors do not have. Journal of Financial Economics, 13(2), 187-221.

Nanda, R., & Rhodes-Kropf, M. (2013). Investment cycles and startup innovation. Journal of Financial Economics, 110(2), 403-418.

National Science Foundation, National Center for Science and Engineering Statistics. 2014. Business R&D and Innovation: 2011. Detailed Statistical Tables NSF 15-307. Arlington, VA. Available at http://www.nsf.gov/statistics/2015/nsf15307/.

O'Donoghue, T. (1998). A patentability requirement for sequential innovation. The Rand Journal of Economics, 654-679.

Png, I. P. (2017). Law and innovation: evidence from state trade secrets laws. Review of Economics and statistics, 99(1), 167-179.

Ribstein, L. E., & Kobayashi, B. H. (1996). An economic analysis of uniform state laws. The Journal of Legal Studies, 25(1), 131-199.

Rogers, J. L., & Stocken, P. C. (2005). Credibility of management forecasts. The Accounting Review, 80(4), 1233-1260.

Root III, J. E., & Blynn, G. M. (1982). Abandonment of Common-Law Principles: The North Carolina Trade Secrets Protection Act. Wake Forest L. Rev., 18, 823.

Saidi, F., & Zaldokas, A. (2017). How Does Firms' Innovation Disclosure Affect Their Banking Relationships? Working paper.

Samuels, L. B., & Johnson, B. K. (1990). Uniform trade secrets act: The states' response, the. Creighton L.Rev., 24, 49.

Scotchmer, S. (1991). Standing on the shoulders of giants: Cumulative research and the patent law. The Journal of Economic Perspectives, 29-41.

Searle, N. C. (2010). The economics of trade secrets: evidence from the Economic Espionage Act (Doctoral dissertation, University of St Andrews).

Seru, A. (2014). Firm boundaries matter: Evidence from conglomerates and R&D activity. Journal of Financial Economics, 111(2), 381-405.

Tian, X., & Wang, T. Y. (2014). Tolerance for failure and corporate innovation. Review of Financial Studies, 27(1), 211-255.

Tian, X. S., & Yu, M. (2017). Keeping secrets from the competitors: Examining the effects of product substitutability on disclosure redactions. Working paper.

U.S. Chamber of Commerce. (2016). The case for enhanced protection of trade secrets in the trans-pacific partnership agreement. Available at: https://www.uschamber.com/sites/default/files/legacy/international/files/Final%20TPP%20Trade%20Secrets%208_0.pdf.

Verrecchia, R. E. (1983). Discretionary disclosure. Journal of Accounting and Economics, 5, 179-194.

Verrecchia, R. E., & Weber, J. (2006). Redacted disclosure. Journal of Accounting Research, 44(4), 791-814.

Wagenhofer, A. (1990). Voluntary disclosure with a strategic opponent. Journal of Accounting and Economics, 12(4), 341-363.