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Trade Secrets Law and Corporate Disclosure: Causal Evidence on the Proprietary Cost Hypothesis

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ABSTRACT

This study exploits the staggered adoption of the inevitable disclosure doctrine (IDD) by U.S. state courts as an exogenous shock that generates variations in the proprietary costs of disclosure. We find that firms respond to IDD adoption by reducing the level of disclosure regarding their customers' identities, supporting the proprietary cost hypothesis. Our results are stronger for firms in industries with a higher degree of entry threats, for firms in more volatile industries, and for firms with a lower degree of external financing dependence. Overall, this study represents one of the first efforts in identifying the causal effect of proprietary costs of disclosure on the supply of disclosure.

JEL codes: D82; D83; K11; L1; M41

Keywords: proprietary costs; corporate disclosure; customer identity; inevitable disclosure doctrine; trade secrets law

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1. Introduction

Corporate disclosure reduces information asymmetry between managers and shareholders, which may increase a firm's stock liquidity and decrease its cost of capital. Thus, absent any costs, value-maximizing managers have incentives to fully disclose their private information. Full disclosure, however, is rarely observed in the capital market. One potential constraint to full disclosure is that some disclosures may damage a firm's competitive position in its product market. Verrecchia [1983] shows that, in the presence of proprietary costs, partial disclosure can be optimal, with the level of disclosure decreasing in the proprietary costs of disclosure.

In this study, we seek to provide causal evidence on the proprietary cost hypothesis. Although the impact of proprietary costs of disclosure on the decision to disclose proprietary information is theoretically unambiguous, the empirical literature to date fails to provide conclusive evidence on the proprietary cost hypothesis, possibly because of the following challenges (Berger [2011]).[1] First, the proprietary costs of disclosure are generally unobservable to researchers and thus researchers often have to use industry structure to approximate variations in proprietary costs. The relation between industry structure and proprietary costs, however, is theoretically ambiguous. For example, product market concentration can be either positively or negatively correlated with proprietary costs of disclosure, depending on whether firms face existing competitors or the threat of entry, and on whether firms compete primarily on the basis of price or long-run capacity decisions (e.g., Darrough and Stoughton [1990], Verrecchia [1990a, b], Wagenhofer [1990], Darrough [1993], Gigler, Hughes, and Rayburn [1994], Clinch and Verrecchia [1997], Penno [1997], Dye [1998]). This theoretical issue is further complicated by measurement errors in industry structures (e.g., Ali, Klasa, and Yeung [2009], Berger and Hann [2007], Dedman and Lennox [2009]). For example, Ali, Klasa, and Yeung [2009] demonstrate that using industry concentration measures from the Census (including both private and public firms) overturns many results of previous literature using Compustat concentration measures.

Second, measures of proprietary costs of disclosure (such as industry structure) may actually capture omitted capital market benefits and agency costs of disclosure. As one example, industry structure measures are often influenced by the number of firms in an industry. The greater the number of firms, the lower are firms' capital market benefits associated with their own disclosure because capital market participants learn from information spillovers provided by related firms.[2] Thus, it is unclear whether

[1] See subsection 2.3 for a detailed review of the empirical literature on proprietary costs of disclosure.

[2] See, for example, Admati and Pfleiderer [2000], Baginski and Hinson [2016], Shroff, Verdi, and Yost [2016], Breuer, Hombach, and Müller [2016], and Berger, Minnis, and Sutherland [2017].

the relation between high competition (measured by low concentration) and low disclosure is due to high proprietary costs or low capital market benefits. Segment disclosure is another context in which proprietary costs, capital market benefits, and agency costs are complicatedly entwined. It is often argued that managers do not disclose segment information to hide highly profitable segments for proprietary cost motives (Berger and Hann [2007]).[3] However, given that the average profitability is publically available in the income statement, managers must also hide some unprofitable segments, or otherwise the reported average profitability will reveal the truth (Leuz [2004]). Thus, it is possible that managers are in fact hiding unprofitable segments through nondisclosure to avoid shareholder scrutiny and enjoy a quiet life or to maintain the level of stock prices.[4] From these examples, we can see that it is difficult to disentangle the effects of proprietary costs, agency costs, and capital market benefits on disclosure choices. A related challenge plaguing this literature is that the information disclosed by firms is often not merely or not even majorly relevant for competitors. For instance, while management earnings forecasts are informative to investors, it is unclear what specific competitive advantage a firm sacrifices in disclosing earnings forecasts shortly before the actual earnings announcements (Lang and Sul [2014]).

We strive to address these challenges using a research design with two key features. First, we exploit a trade secrets law setting that provides plausibly exogenous shifts in the proprietary costs of disclosure, holding other determinants of disclosure approximately fixed. Unlike most studies in the prior literature, this setting does not rely on the theory and measurement of industry structure. Second, we investigate a disclosure item that is unambiguously known to the firm and is primarily more relevant to competitors than to other stakeholders. This feature further helps mitigate concerns about correlated omitted variables related to agency costs or capital market benefits of disclosure.[5]

Specifically, our empirical strategy is based on the staggered adoption of the inevitable disclosure doctrine (IDD) by U.S. state courts since the 1970s (Klasa et al. [2017]). The IDD is a theory in trade secrets law that grants the employer (i.e., the plaintiff) an injunction to prevent a current or former employee (i.e., the defendant) from working for another company, if the

[3] Managers also claim that they "do not want to explicitly reveal sensitive proprietary information 'on a platter' to competitors, even if such information could be partially inferred by competitors from other sources, such as trade journals or trade conferences" (Graham, Harvey, and Rajgopal [2005, p. 62]).

[4] Cho [2015] finds that more transparent segment disclosure is associated with less severe agency problems and higher investment efficiency.

[5] This exercise is in the spirit of Leuz [2004], which distinguishes between disclosures that are more likely to be proprietary and those that are more relevant to capital markets. The author looks at cash flow statements, which are likely not proprietary but relevant to capital markets.

employer demonstrates that the employee will *inevitably* disclose the employer's trade secrets in the performance of the employee's new position. According to the Restatement (Third) of Unfair Competition $39 (1995), a trade secret is "any information that can be used in the operation of a business or other enterprise and that is sufficiently valuable and secret to afford an actual or potential economic advantage over others." Examples of trade secrets include customer lists, a method of production, or a secret formula for a soft drink.[6] The IDD applies to every employee and every business secret of a firm and it does not require evidence of actual or even threatened misappropriation. Thus, adoption of the IDD essentially blocks one of the most important channels through which a firm's rivals can obtain its confidential information.

We argue that adoption of the IDD helps generate exogenous variations in a firm's proprietary costs of disclosure. First, the IDD increases a firm's proprietary costs of disclosure by increasing the marginal value of disclosure to its rivals through a substitution channel. That is, with less access to trade secrets via employees' job switching, rival firms would rely more heavily on a firm's public disclosures in discovering its proprietary information. Second, for information to be protectable under trade secrets laws (including the IDD), it must not be readily ascertainable by proper means by persons who could obtain economic value from its use. For example, once a firm discloses a piece of information, such as customer lists, in its financial reports, the information loses its trade secret status.[7] Thus, to take advantage of the protection offered by the IDD, firms must not disclose their proprietary information to the capital markets because such disclosure essentially makes the information less protectable by the IDD in the product market.[8] Put another way, adoption of the IDD increases the marginal benefits of nondisclosure. Based on these arguments, we predict that firms disclose less in response to adoption of the IDD.

An important consideration in testing the proprietary cost hypothesis is "to identify disclosure choices that impose significant costs on the firm in terms of its competitive environment" (Lang and Sul [2014, p. 272]). In this study, we focus on a firm's disclosure choices about its customers (Ellis, Fee, and Thomas [2012]). Specifically, we examine how adoption of the IDD impacts a firm's decision to disclose the identity of its important customers. Information about customers can help investors to assess the level and volatility of a firm's current and future revenues (e.g., Fee, Hadlock, and Thomas [2006], Dhaliwal et al. [2016]). The same information, however, is also of obvious value to the firm's current and potential

[6] See, for example, *ConFold Pac. Inc. v. Polaris Indus.*, 433 F.3d 952,959 (7th Cir. 2006).

[7] Trade secrets are different from patents. In the case of patents, the firm needs to disclose the confidential information in exchange for patent protection for a finite period of time.

[8] As suggested by Quinto and Singer [2014], an important defense for a trade secret misappropriation is to provide evidence that the plaintiff's trade secret could be obtained from public sources without significant costs.

product market rivals in competing with the firm for market share. Moreover, a 2002 survey conducted by ASIS International (thereafter, the ASIS survey) shows that the most frequent type of proprietary information loss incident involves customer-related information, including customer identity, preferences, and pricing.[9] Thus, we argue that disclosure choice regarding customer identity is an ideal proxy for proprietary information disclosure for our research question. As discussed by Ellis, Fee, and Thomas [2012], an important feature of this disclosure proxy is that we can ascertain that the information in question (i.e., the identity of customers) exists, whereas in other contexts, such as in the case of management forecasts, it is often unclear whether nondisclosure reflects managerial choices or the fact that the firm has no information to disclose. This feature helps mitigate the concerns that nondisclosure is driven by agency costs because it is difficult for managers to hide the information for agency cost reasons when investors can ascertain managerial endowment of such information (Dye [1985], Jung and Kwon [1988]).

Using a panel of 28,547 U.S. firms over the 1994–2010 period and a difference-in-differences (DiD) method, we find that firms headquartered in states that adopt the IDD significantly reduce the level of disclosure regarding their customers' identities, by approximately 7–9% after the IDD is adopted, relative to firms headquartered in states that do not adopt the IDD. In the regression model, we control for standard time-varying firm-, industry-, and state-level characteristics that potentially influence the level of a firm's disclosure. We also include state, industry, and year indicators to control for time-invariant, unobservable state and industry characteristics, as well as economy-wide shocks. The results are also robust to controlling for time-invariant, unobservable firm characteristics through inclusion of firm fixed effects. In dynamic analyses, we find that the decreases in disclosure mainly occur in one or two years after the adoption of the IDD, but not before.

In cross-sectional analyses, we find that the effect of the IDD on the concealment of customer identities is statistically and economically more pronounced for firms in industries with a higher degree of entry threats and industries with more volatile sales, suggesting that it is more costly to lose trade secrets for these firms. On the other hand, the effect of the IDD on the concealment of customer information is significantly weaker when the firm exhibits a higher level of external financing dependence, suggesting that capital market benefits of disclosure mitigate the effect of the IDD on nondisclosure. Overall, the cross-sectional results provide further support for the causal effect of the IDD on disclosure of customer identities.

The IDD of a firm's home state can be applicable even if an employee is moving to a rival located in another state (Klasa et al. [2017]). However,

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[9] The report is available at https://foundation.asisonline.org/FoundationResearch/Publications/Documents/trendsinproprietaryinformationloss.pdf.

using a firm's headquarters-state's IDD adoption to measure the firm's overall exposure to the IDD can still be problematic for multistate firms. In robustness tests, we address this issue in two ways. First, we restrict our sample to firms with a majority of their employees working in the headquarters states. Second, we estimate a weighted-measure of IDD exposure that takes into account the number of employees in each state in which the firm has subsidiaries or branches. Our results are robust to both methods.

To remove the effects of unobserved variation in local conditions, we exploit the geographic richness of our data to sharpen our identification. Assuming that the developments of the economic conditions are similar in neighboring states, we can difference away unobservable changes in local conditions by comparing firms in each adopting state with their peers in neighboring, nonadopting states. Using control firms from immediate neighbors and shared-border fixed effects, we find that the effect of the IDD on disclosure continues to be significant, implying that unobserved local confounding effects are unlikely to be the main drivers of our results. Further, we conduct a placebo test in which we randomly assign the adoption (rejection) date to each IDD adopting (rejecting) state and reestimate the effect of these pseudo-IDD adoptions (rejections) on firms' disclosure of customer identities. We repeat this exercise 1,000 times and plot the probability density function of the placebo coefficients. The results reveal that such random assignments are associated with an insignificant effect on customer information disclosure. These analyses suggest that our main finding is unlikely to be driven by unmodeled factors. Finally, to maximize state-level variations, we collapse firm-year observations into state-year observations. The results continue to hold in state-year level regressions.

Our study contributes to the financial reporting and disclosure literature by identifying the causal effect of proprietary costs on firms' voluntary disclosure decisions. The effect has been elusive because of the difficulties in separating agency and proprietary cost reasons of disclosure. Our study exploits IDD adoptions that generate exogenous shifts in the proprietary costs of disclosure. Moreover, we examine a disclosure item that is unambiguously known to the firm and is more useful to competitors than to other stakeholders. The information under our consideration, customer identity, is clearly proprietary in nature, and its revelation can significantly erode a firm's competitive advantages, compared to disclosure measures used in prior research. This study also relates to the broad literature on how law affects corporate policies, particularly financial reporting and disclosure policies. Prior literature generally focuses on laws that provide protection for investors or creditors (e.g., La Porta et al. [1998], Leuz, Nanda, and Wysocki [2003], Cohen, Dey, and Lys [2008]). Our research advances this literature by examining how intellectual property and labor laws affect corporate disclosure. Finally, our study is related to the study of Klasa et al. [2017], which examines the effect of the IDD on financial leverage. We note, however, that our inferences are unaltered even after we control for the leverage effect of the IDD.[10]

The remainder of this paper proceeds as follows. Section 2 discusses the legal background of the IDD, reviews related literature on the proprietary cost hypothesis, and presents our empirical predictions. Section 3 describes the data and variable measurements. Section 4 presents the main empirical results and cross-sectional analyses. Section 5 discusses additional tests and robustness checks. Finally, section 6 sets forth our conclusions.

2. *Trade Secrets Law, the IDD, and Proprietary Costs of Disclosure*

2.1 THE IDD

According to the ASIS survey, U.S. companies likely experience proprietary information and intellectual property losses of more than $50 billion each year. The survey also reports that the most frequent types of incidents are losses of trade secrets related to a firm's customers, strategy plans, and financial data. In addition, the survey reveals that deliberate actions of current and former employees are a primary threat to proprietary information. In the civil arena, protection of trade secrets in the United States is provided for almost exclusively under state law. Historically, trade secrets law has evolved as common law, which is the accumulation of precedents set by decisions of courts in previous cases. In 1979, the National Conference of Commissioners on Uniform State Laws approved the Uniform Trade Secrets Act (UTSA) and recommended it for enactment in all states. By 2014, 48 states, Washington, DC, and the U.S. Virgin Islands had adopted either the 1979 version of the UTSA or the 1985 amended version of the UTSA. The exceptions are Massachusetts, North Carolina, New York, and Texas. Under Section 2 of the UTSA, the owner of a trade secret can obtain injunctive relief to prevent actual or threatened misappropriation. Misappropriation means disclosure or use of a trade secret by the defendant to the plaintiff's detriment.

The IDD is a theory of common law that substantially enhances protection of trade secrets. Trade secrets misappropriation is seen as inevitable if an employee with knowledge of a firm's trade secrets accepts an equivalent position with a competitor of the firm. In such a situation, the employee who wants to do good work will inevitably rely on all information or knowledge she has, including her former employer's trade secrets. State courts that recognize the IDD can grant temporary injunctive relief (in rare cases, a permanent injunction) to the plaintiff firm to prevent the employee from working for the new employer. Thus, the IDD reduces incidents of actual or threatened disclosure or uses of trade secrets by departing employees and significantly enlarges protection of confidential information.

[10] Our results are also robust to controlling for other potential confounds, such as investment and financing activities.

Note that the IDD is a common law theory independent from the statutory law of UTSA in the sense that recognition of the IDD by state courts does not require adoption of the UTSA in the state and that adoption of the UTSA does not imply recognition of the IDD. To obtain relief under Section 2 of the UTSA, the employer needs to prove that the former employee has actually misappropriated the trade secret (e.g., the former employee has used the trade secret at the disadvantage of the plaintiff) or is threatening to misappropriate it (e.g., the former employee has bad faith intention to disclose the trade secret to the competing firm), whereas under the IDD, evidence for actual or threatened misappropriation is not required for obtaining injunctive relief (Quinto and Singer [2014], Png and Samila [2013]). The UTSA requires the firm to establish actual wrongdoing by the employee, such as disclosure or misappropriation of trade secrets or bad faith. Under the IDD, the plaintiff only needs to show that the employee would be employed in such a capacity that she would "inevitably" disclose the trade secrets. As a result, a substantial advantage of the IDD is that firms can take actions to prevent the harm before the harm is done, whereas under the UTSA the firm normally can only act after the harm has already been done.

Despite its long existence, the IDD appears to be more seriously recognized and practiced only after the prominent case of *PepsiCo Inc. v. Redmond* (the PepsiCo case). In 1994, there was fierce competition between PepsiCo Inc. and Quaker Oats Company in the "sports drinks" and "new age drinks" markets. Both companies saw 1995 as an important year for their products. William Redmond worked for PepsiCo from 1984 to 1994 and was promoted to general manager of the California business unit in 1994. The position gave Redmond access to PepsiCo's strategic and annual operating plans, which included information concerning pricing, customers, and new delivery systems. On November 8, 1994, Redmond accepted a position with Quaker as its Vice President—Field Operations for its Gatorade division. On November 16, 1994, PepsiCo filed a suit in District Court in the State of Illinois, seeking a temporary restraining order to enjoin Redmond from assuming his duties at Quaker and to prevent him from disclosing trade secrets or confidential information to his new employer.

The Court of Appeals affirmed the District Court's order enjoining Redmond from assuming his responsibilities at Quaker for six months, and permanently barred him from disclosing PepsiCo's trade secrets and confidential information. As summarized by the Court of Appeals:

> Having shown Redmond's intimate knowledge of [PepsiCo's] plans for 1995, PepsiCo argued that Redmond would inevitably disclose that information to Quaker in his new position, at which he would have substantial input as to Gatorade and Snapple pricing, costs, margins, distribution systems, products, packaging and marketing, and could give Quaker an unfair advantage in its upcoming skirmishes with PepsiCo.[11]

[11] *PepsiCo Inc. v. Redmond*, 54 F.3rd 1262 (7th Cir, 1995).

Since 1995, the PepsiCo case has been widely followed by other state courts.[12] According to data assembled by Klasa et al. [2017], 10 of 21 states whose courts adopted the IDD ruled on the doctrine in the years after 1995. As in Klasa et al. [2017], we argue that adoption of the IDD is exogenous to the economic conditions of firms and their headquarters states because of the common law nature of the doctrine. Recognition of the IDD prompted by the PepsiCo case is even more likely to be exogenous to the adopting states' business and economic activities given that the precedent set by the PepsiCo case only applies in Illinois.

2.2 IMPACT OF THE IDD ON DISCLOSURE

The equilibrium level of disclosure for a firm results from its manager's tradeoff between the benefits and costs of disclosure. In this paper, we argue that adoption of the IDD represents an exogenous shock that increases the marginal costs of proprietary information disclosure and the marginal benefits of *nondisclosure*. As discussed in subsection 2.1, an important channel for a firm's competitor to learn about the firm's proprietary information is to hire the firm's departing employees who have access to such information. Adoption of the IDD reduces the risk that a firm loses its confidential information to its rivals through employee movements. Two related effects follow. First, with less access to trade secrets via employees' job switching, rival firms will rely more heavily on a firm's public disclosures in discovering its proprietary information. This "substitution" effect increases the marginal cost of disclosure for the firm because the disclosure is now more valuable *to its rivals*. Second, the stronger legal protection of trade secrets essentially increases the economic value of proprietary information by increasing the "property rights" of a firm over its proprietary information. This effect also increases the proprietary costs of disclosure because the disclosed information is of higher economic value *to the disclosing firm* under the protection of the IDD.

Virtually any type of information is potentially protectable under the IDD. However, for a piece of information to be protectable, the holder of the information should try to keep it secret such that the only way the secret can be unmasked is via unlawful activities. As explained by the Iowa Supreme Court: "There is virtually no category of information that cannot, as long as the information is *protected from disclosure to the public*, constitute a trade secret."[13] Thus, adoption of the IDD increases the opportunity cost of public disclosure or the marginal benefit of nondisclosure because public disclosure of proprietary information makes the information nonprotectable under the IDD. Based on the above arguments, we predict that adoption of the IDD decreases the level of proprietary information disclosure because it increases the marginal cost (marginal benefit) of disclosure (nondisclosure).

[12] Note, however, that the PepsiCo case is not the precedent-setting case for Illinois.

[13] *Economy Roofing & Insulating Co. v. Zumaris*, 538 N.W.2d 641, 647 (Iowa 1995).

2.3 RELATED LITERATURE

Our study is related to the literature on testing the proprietary cost hypothesis of disclosure. To approximate variation in proprietary costs, prior literature has mainly resorted to variation in industry structure. Such variation has been used to explain voluntary disclosures of information that is useful for multiple stakeholders. For example, Harris [1998] argues that firms disclose less information about their operations in less competitive industries to protect the abnormal profits of these operations. Consistent with this argument, she finds that operations with slower rates of abnormal profit adjustment and higher market share concentration are less likely to be reported as industry segments. Consistent with Harris [1998], Bens, Berger, and Monahan [2011] find that the industry speed of abnormal profit adjustment is negatively related to the probability of aggregation in segment disclosures for firms with value-enhancing diversification. Using a German setting, Leuz [2004] finds that firms are more likely to voluntarily provide segment data when entry barriers are relatively high and when firm profitability is low. In addition, firms having more heterogeneous segment profits in their mandatory reports (implying that the reported average profitability in the income statement is less informative) are less likely to provide segment data when reporting is still voluntary. Botosan and Stanford [2005] find that firms use the latitude afforded by Statement of Financial Accounting Standards No. 14 to hide profitable segments operating in less competitive industries. In contrast, Berger and Hann [2007] do not find evidence that proprietary costs affect disclosure of profitable segments. Using a survey-based measure of competition, Dedman and Lennox [2009] show that managers are more likely to withhold information about sales and costs of sales if they perceive that current or potential competition is strong.

Verrecchia and Weber [2006] find that firms in more competitive industries are more likely to redact information in their financial reports. Similarly, Li [2010] shows that firms in more competitive industries are less likely to provide management forecasts. Boone, Floros, and Johnson [2016] show that initial public offering firms with higher research and development (R&D) expenditures and in more competitive product markets are more likely to redact information from their Securities and Exchange Commission registration filings. On the other hand, Bamber and Cheon [1998] find that firms in more concentrated industries issue less precise earnings forecasts. Ali, Klasa, and Yeung [2014] attribute the mixed findings of the prior research to errors in measuring industry concentration using Compustat data. Using a refined measure of industry concentration that takes into account private firms, these authors show that manufacturing firms in more concentrated industries disclose less due to proprietary costs of disclosure.

Our study differs from prior research in that we do not rely on theory and empirical measures of industry structure in testing the proprietary cost hypothesis. Instead, we exploit events that generate plausibly exogenous variations in the marginal proprietary costs of disclosure through a substitution channel (i.e., a rival's choice of obtaining confidential information through the firm's public disclosure versus through hiring the firm's employees). Therefore, our study can provide an assessment on the magnitude of the elasticity of proprietary information supply with respect to proprietary costs of disclosure.

Our identification is further strengthened by investigating a disclosure item, namely, customer identity, which is unambiguously known to the firm and is relatively more useful to competitors than to other stakeholders. Our focus on disclosure of customer identity is closely related to the study of Ellis, Fee, and Thomas [2012], who first examine comprehensively the determinants of customer information disclosure. These authors show that firms with more R&D and advertising expenses tend to disclose less information about their customers, consistent with the proprietary cost hypothesis. Again, our contribution over Ellis, Fee, and Thomas [2012] lies in the identification of the causal effect of proprietary costs on voluntary disclosure. Lack of identification generally leads to alternative interpretations, and thus documenting causal effects and the associated elasticities are pivotal for empirically testing theories (Leuz and Wysocki [2016]).

Our study is also closely related to concurrent research by Aobdia [2017]. Similar to our study, Aobdia [2017] argues that noncompete agreements increase proprietary costs of disclosure because these agreements reduce information leakage through employee movements. Using a time-invariant enforcement index constructed by Garmaise [2011], Aobdia [2017] finds a negative association between a state's propensity to enforce noncompete agreements and disclosure activities (e.g., management forecasts) of firms headquartered in this state. As discussed earlier, the advantage of our research lies in the research design: the use of a plausibly exogenous shock to proprietary costs of disclosure and a disclosure item that better captures proprietary information.[14] Moreover, the IDD arguably provides much stronger and far-reaching protection of trade secrets than noncompete agreements. As noted by Garmaise [2011], noncompete agreements are mostly effective when workers seek to switch jobs within a state.

Finally, our study is related to the emerging literature on the effect of the IDD on corporate policies. Klasa et al. [2017] argue that the risk of losing intellectual property to rivals is an important competitive threat and the adoption of the IDD in a firm's home state reduces such a risk. They find that firms rebalance the capital structure and increase financial leverage after the IDD adoption, consistent with the idea that decreased competitive threat induces firms to choose less conservative capital structures. Our

[14] In practice, effective enforcement of noncompete agreements requires the firm to have employment contracts with such agreements. This can introduce an additional selection problem in the research design. In contrast, the impact of the IDD does not require the existence of noncompete agreements.

study extends this literature by examining the effect of the IDD adoption on corporate disclosure policies.

3. Data

3.1 SAMPLE

Our sample begins with all firm-year observations in the 10-K Headers Database constructed by Bill McDonald.[15] The database provides information about the historical locations of each firm's headquarters during the period 1994–2010. The Compustat database, by contrast, provides information only on each firm's most recent state of headquarters. Because our key identification strategy requires the accurate determination of firms' historical locations, we focus on the 1994–2010 sample in our main tests. In fact, Klasa et al. [2017] find that about 9.3% of Compustat firms relocated from one state to another, which in our view, could introduce substantial measurement errors in the IDD variable. In addition, the adoption and status of the IDD in each state are likely to be better known after the prominent PepsiCo case. Nonetheless, we subsequently conduct a robust check by starting the sample period from 1977 following Klasa et al. [2017] in section 5.

From the initial sample, we delete financial firms (SIC code from 6000 to 6999) and firms headquartered outside the United States. After further data requirements including necessary customer disclosure and financial data, we are left with a final sample of 28,547 firm-year observations over the period 1994–2010. Panel A of table 1 presents a more detailed description of our sample construction process.

3.2 THE IDD INDICATOR

We use the Klasa et al. [2017] coding of adoption of the IDD. This coding is based on a list of IDD-related cases collected from prior legal studies (Kahnke, Bundy, and Liebman [2008], Malsberger [2004], Waldref [2012], Wiesner [2012]). Klasa et al. [2017] then identify all the precedent-setting cases from the initial list using the procedure described in subsection 3.3 of their paper. Appendix B, replicated from Klasa et al. [2017], lists all the precedent-setting cases for the period 1919–2011. Our identification strategy requires the assumption that firms are aware of the precedent-setting cases of their headquarters states. We argue that this assumption is likely to be valid for the following reasons. First, the protection of trade secrets is critical for firms' competitive advantage and thus firms are expected to closely monitor the development of trade secrets laws. Second, it is quite probable that the extensive media coverage of prominent cases, such as the PepsiCo suit, significantly increases the public awareness of the IDD and prompts firms in other states to more actively track the precedent-setting

[15] The database is available at http://www3.nd.edu/˜mcdonald/10-K_Headers/10-K_Headers.html. The original data set has 170,413 firm-year observations. A total of 168,599 firm-year observations have sufficient information on business headquarters.

TABLE 1

Sample Selection and Summary Statistics

Panel A: Sample selection

McDonald's headquarters data (1994–2010)	168,599
Deleting firms without customer (company) reporting	(128,156)
Deleting financial firms	(2,065)
Deleting firms without customer representing 10% or more of their sales	(9,048)
Deleting firms located in foreign countries	(447)
Deleting firms without financial information in previous year	(336)
Observations for main regression (Max.)	28,547
Observations that are eligible for small business reporting	12,340
Observations that are not eligible for small business reporting	16,207

Panel B: Sample distribution

State	Firm-Year Observations	% IDD = 1
AR	117	0.85
AZ	352	0.00
CA	5,945	0.00
CO	915	0.00
CT	693	0.96
DC	28	0.00
DE	61	1.00
FL	1,200	0.42
GA	585	0.78
HI	46	0.00
IA	96	0.94
ID	87	0.00
IL	978	1.00
IN	291	0.94
KS	116	0.27
KY	120	0.00
LA	232	0.00
MA	1,678	0.99
MD	459	0.00
ME	38	0.00
MI	591	0.51
MN	926	1.00
MO	350	0.59
MS	62	0.00
MT	53	0.00
NC	481	1.00
NE	84	0.00
NH	121	0.00
NJ	1,520	1.00
NM	43	0.00
NV	151	0.00
NY	2,554	1.00
OH	823	0.62
OK	299	0.00
OR	364	0.00
PA	1,130	1.00
RI	93	0.00

(*Continued*)

TABLE 1—*Continued*

Panel B: Sample distribution

State	Firm-Year Observations	% IDD = 1
SC	122	0.00
SD	15	0.00
TN	275	0.00
TX	2,588	0.52
UT	325	0.85
VA	495	0.00
VT	32	0.00
WA	434	0.86
WI	365	0.00
WV	36	0.00
WY	26	0.00
Total	28,547	0.506

Panel C: Descriptive statistics

	Observations	Mean	Median	SD
Ratio 1	28,547	0.447	0.333	0.444
Ratio 2	28,547	0.465	0.351	0.462
IDD	28,547	0.506	1.000	0.500
Intangibility	28,547	0.039	0.000	0.089
Advertising	28,547	0.010	0.000	0.034
Size	28,547	4.592	4.573	2.164
Abnormal ROA	28,547	0.442	0.406	0.322
BIG N	28,547	0.723	1.000	0.447
HHI	28,547	0.255	0.196	0.185
Missing RD	28,547	0.370	0.000	0.483
RD to sales	28,547	0.236	0.012	0.835
Analyst following	28,547	4.737	2.000	7.254
IO	28,547	0.280	0.123	0.329
MA	28,547	0.179	0.000	0.384
SEO	28,547	0.056	0.000	0.229
GDP	28,547	2.909	3.100	2.759
Unemployment	28,547	5.437	5.283	1.491

cases in their own states.[16] Although most of the precedent-setting cases were not widely discussed in the popular media, we find that all of these cases were discussed in legal journals (including professional education materials) and were cited by corresponding state courts within one year of the precedent-setting dates. Moreover, it is important to note that almost all public firms have in-house legal counsels overseeing legal matters including those related to labor and employment. Further, there is substantial empirical evidence showing that General Counsels play an important advisory role in firms' financial reporting and disclosure decisions (e.g., Kwak, Ro, and

[16] Other cases in appendix B that received notable media attention are *Uncle B's Bakery v. O'Rourke*, 920 F. Supp. 1405 (N.D. Iowa 1996), *Novell Inc. v. Timpanogos Research Group Inc.*, 46 U.S.P.Q.2d 1197 (Utah D.C. 1998), and *Procter & Gamble Co. v. Stoneham*, 747 N.E.2d 268 (Ohio Ct. App. 2000).

Suk [2012], Hopkins, Maydew, and Venkatachalam [2014]). Therefore, it is reasonable to assume that firms are keenly aware of and understand the implications of the IDD precedent-setting cases.[17]

A firm headquartered in one state could be subject to common laws of multiple states. However, there are several reasons that the law of the headquarters state is the applicable law in a large number of trade secret-related disputes. First, in determining which state's law governs a trade secret-related dispute, courts often favor the state with the most significant relationship to the dispute (Ingle [2013]), which in many cases turns out to be the headquarters state (Jones [2014]). Second, the law of headquarters state is important because of the *lex loci contractus* ("law of the place where the contract is made") principle in labor and employment laws (Pollard [2014]). In addition, the firm can designate the law of the headquarters state as the applicable law using the "choice of law provision" in employment contracts (Steinmeyer and Freeman [2016]). Alternatively, the firm could resort to the 'choice of forum provision' to select the court in the state where it is headquartered to resolve any disputes (Bertram [2013], Jones [2014]).

As noted in section 1 and Klasa et al. [2017], the IDD of a firm's home state is applicable even if the employee intends to move to a rival that is located in another state. In many of the IDD precedent-setting cases listed in appendix B (e.g., *Branson Ultrasonics Corp. v. Stratman*, 921 F. Supp. 909 (D. Conn. 1996), and *Del Monte Fresh Produce Co. v. Dole Food Co. Inc.*, 148 F. Supp. 2d 1326 (S.D. Fla. 2001)), the state law of the plaintiff's headquarters was applied by the court because the location of company headquarters was "the principal place of business" and had the most significant relationship to the dispute, although the defendant was moving to a different state. In discussing the applicable laws in trade secret misappropriation cases, Crowell & Moring LLP [2011] cites a Massachusetts District Court case enforcing a Massachusetts noncompete clause against a California resident working for a California company, despite California's public policy against noncompetes and despite case law casting doubt on the existence of a trade secret exception to this policy under California law.[18] Crowell & Moring LLP [2011, p. 1] summarizes:

> The Court determined that California's interest in the case was weaker than or at best equal to Massachusetts' interest because the employment contract was negotiated by a Massachusetts company (the former employer) for an employee who worked at least in part in Massachusetts, and any harm caused by its violation would be felt in Massachusetts.

[17] To the extent that some firms may not be aware of these cases, our empirical results should be biased toward the null. In addition, Klasa et al. [2017] document significant stock market reactions to a home state court's final decision to adopt the IDD, suggesting that the capital market is aware of the IDD legal events.

[18] See *Aspect Software Inc. v. Barnett*, F. Supp.2d, No. CIV.A. 11-10754-DJC (D. Mass. May 27, 2011).

During our sample period, there are 11 cases in which state courts adopted the IDD and 3 cases in which state courts rejected the IDD after recognizing it in prior years. The IDD indicator is then constructed using the dates of precedent-setting cases. Specifically, for the 11 states whose courts adopted the IDD during our sample period, the IDD indicator is zero in all years prior to the date of the precedent-setting cases and 1 afterwards. For the seven states whose courts adopted the IDD before 1994 and never reversed it during our sample period, we set the IDD indicator equal to 1. For the three states whose courts adopted the IDD before 1994 but rejected it during our sample period, we set the IDD indicator equal to 1 before the precedent-setting case and zero afterwards.[19] For the remaining 27 states whose courts did not adopt the IDD at any time in history, we set the IDD indicator equal to zero in all years.[20]

Panel B of table 1 presents the frequency of firm-year observations in each state during our sample period, together with the mean value of the IDD indicator. In 27 states, the average value of the IDD indicator is zero due to the fact that these states did not adopt the IDD throughout our sample period. In seven states, the average value of the IDD indicator is 1 because these states adopted the IDD before our sample period and never rejected the IDD during our sample period. In 14 states, the average value of the IDD indicator is between zero and 1, suggesting that these states either adopted or rejected the IDD during our sample period. The number of firm-years located in these 14 states is 9,887, representing about 35% of the total sample size. Moreover, it appears that the number of firms spreads reasonably well across these states, although relatively more observations are from Texas (2,588). Overall, there are substantial variations in the adoption status of the IDD across states and over time and observations are not extremely concentrated in one or two states. Nevertheless, in robustness tests, we conduct state-level regressions to mitigate the concern that there are insufficient state-level variations in our firm-level regressions.

To explore potential state-level confounds, appendix C presents an analysis of whether the adoption or rejection of the IDD is correlated with any state-level political or economic factors. Appendix C shows that Democratic-leaning states are more likely to adopt and less likely to reject the IDD. The economic activity of public firms (i.e., total size and number of public firms) appears to be negatively related to the adoption of the IDD and positively related to the rejection of the IDD. State gross domestic product (GDP) growth rate has a positive effect on the likelihood of the adoption of the IDD and state unemployment rate has a positive

[19] For simplicity, we refer to the impact of changes in court opinions regarding the IDD as the impact of IDD adoption.

[20] We have confirmed these states' stances through extensive online search. Most of the 27 states did not address the IDD in history. Four of the 27 states explicitly rejected the IDD (CA in 2002, LA in 1967, MD in 2004, VA in 1999), although none of them ever adopted the IDD prior to the rejection.

effect on the likelihood of the rejection of the IDD. To the extent that these state-level factors are correlated with proprietary costs of disclosure, our empirical findings could suffer from omitted-variable biases. To address this problem, in our main tests, we control for state GDP growth rate and unemployment rate.[21] Moreover, in a robustness check, we restrict the sample of control firms to those in neighboring states to further control for unobserved variation in local conditions.

3.3 DISCLOSURE OF CUSTOMER IDENTITY

Regulation S-K requires firms to disclose sales to and the identity of any customer that accounts for more than 10% of a firm's consolidated sales revenues if the loss of the customer would have a material adverse effect on the firm's performance. In practice, however, there is great variation in the reporting of customer information. According to the Compustat database, many firms disclose the existence of major customers but do not reveal the identity of these customers. Thus, it appears that enforcement of Regulation S-K is weak in practice and that disclosure of customer information is largely voluntary. Still, many firms disclose smaller customers (generating less than 10% of a firm's total sales) with or without revealing the names of these customers.

In our study, we examine a firm's decision to conceal the identities of its reported customers. Revealing the identities of a firm's customers helps rival firms approach these customers and estimate the productive capacity of the disclosing firm (Ellis, Fee, and Thomas [2012]). In addition, customer identity information may enable competitors to infer the pricing strategy and cost structure of a firm's products. Thus, we argue that disclosure choices regarding customer identity impose significant costs on a firm in terms of its competitive positions.

We construct two simple measures of customer identity disclosure. The first measure, *Ratio 1*, is the percentage of reported customers without identity information. The second measure, *Ratio 2*, is a sales-weighted version of the first measure. Specifically, for *Ratio 2*, in calculating the percentage of unidentified customers, we assign a weight to each reported customer based on the amount of sales to this customer. In constructing these measures, we require that a firm has nonmissing customer information from Compustat Segment Files and that it has at least one customer that accounts for more than 10% of its total sales. Ellis, Fee, and Thomas [2012] find that concealment of customer information is largely a disclosure policy in that few firms conceal the names of some customers but reveal the names of other customers in a specific year. Based on this observation, instead of using ratios, we also use an indicator variable to capture the disclosure policy

[21] We note, however, that our results are robust to controlling for all the variables in the table of appendix C (see table 4 of the online appendix).

and find similar results for all tests. We use the ratios in our tabulated results because they are more precise.

One potential concern about the practical validity of our disclosure measures is that the concealment of a customer's name may not be helpful if the firm has ever disclosed the name of the customer in the past. However, since the customer-supplier relationship does not last forever, it is difficult for outsiders to know whether the nondisclosure is due to the loss of the particular customer or the firm's choice not to reveal the name of the customer.[22] In other words, the trade secrets in question are the identities of the firm's *current* customers. Moreover, firms develop new customers from time to time (if not every year) and it may be more important to conceal the new customers' identities.[23] Finally, even if we assume that competitors can have knowledge about the firm's customer list from other sources, managers may still have incentives to conceal the names of these customers in their *current* financial reports if the IDD is adopted. The nondisclosure of the customer information in public reports increases the chances that the firm obtains an injunction to prevent key employees (who personally maintain the relationships with the customer) from joining competing firms, which in turn, make it more difficult for competitors to capitalize on the information they know.

3.4 CONTROL VARIABLES AND DESCRIPTIVE STATISTICS

The main control variables are taken from Ellis, Fee, and Thomas [2012]. We include R&D expenditures to sales, advertisement expenditure to sales, and intangible assets scaled by total assets to control for a firm's proprietary costs of disclosure. We argue that these measures, however, may also capture other economic conditions, such as a firm's growth potential and risk profile. We further include an indicator variable that indicates whether the value of R&D is missing from Compustat. We also include four-digit Standard Industrial Classification (SIC) industry concentration ratio to control for the effect of competition on disclosure. To capture capital market benefits of disclosure, we include two indicator variables to indicate whether the firm announces an seasoned equity offering (SEO) or mergers and acquisitions (M&A) in the year following the disclosure year. We include firm size and auditor size to capture the tendency for transparency and compliance. Larger firms and firms with Big N auditors are likely to be more transparent and more likely to comply with disclosure rules. Finally, we include state GDP growth rate and state unemployment rate to capture the economic conditions of the firm's headquarters state. Appendix A presents definitions of all variables and panel C of table 1 reports descriptive statistics for the main regression variables.

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[22] According to the Compustat Segment database, about one-third of the disclosed relationship lasts for one year. For the remaining two-thirds, the median (mean) length of relationship is around three (five) years (e.g., Cen, Dasgupta, and Sen [2016]).

[23] Using a random sample of 100 firms (1,012 firm-years), we manually check the mix of significant customers. On average, the percentage of new customers as a fraction of all customers (including both identified and unidentified customers) is 24%, and the percentage of new customers as a fraction of total identified customers is 34%.

4. *Empirical Results*

4.1 IMPACT OF IDD ON DISCLOSURE: EVENT-TIME PLOTS

Figure 1 depicts the impact of IDD on customer information disclosure, based on the fitted lines of three-order polynomial regressions. We include high-order polynomials to allow for the possibility of nonlinearity around the cut off time.[24] The figure illustrates visually the impact of IDD adoption on disclosure of customer identity. Panel A of figure 1 shows the results using *Ratio 1* as the *Y*-axis variable (percentage of unidentified customers) and panel B of figure 1 shows the results of using *Ratio 2* as the *Y*-axis variable (sales-weighted percentage of unidentified customers). In each panel, the *X*-axis represents the distance in months to the date of IDD adoption, with negative values indicating preadoption months and positive values indicating postadoption months. The figures in both panels show a clear discontinuity at the date of IDD adoption.

Figure 1 shows that the average percentage of unidentified customers increases after adoption of the IDD. Numerically, the average percentage of unidentified customers, measured by *Ratio 1* (*Ratio 2*) is 0.429 (0.447) before IDD adoption. After adoption of the IDD, this figure changes to 0.464 (0.482), representing an 8.16% (7.83%) increase. The increase in the percentage of unidentified customers is more pronounced immediately after adoption of the IDD, with the effect of the IDD becoming more moderate with the passage of time. Overall, the visual results are consistent with our prediction that adoption of the IDD decreases the level of disclosure of proprietary information.

4.2 IMPACT OF IDD ON DISCLOSURE: DID REGRESSIONS

We next use a DiD regression to examine how adoption of the IDD by state courts impacts the level of disclosure regarding customer identity. Following Bertrand and Mullainathan [2003], we estimate the following model:

$$\ln(1 + NonDisclosure)_{i,s,t} = \beta IDD_{s,t} + \gamma X_{i,s,t} + \omega_i + \theta_s + \tau_t + \varepsilon_{i,s,t}, \quad (1)$$

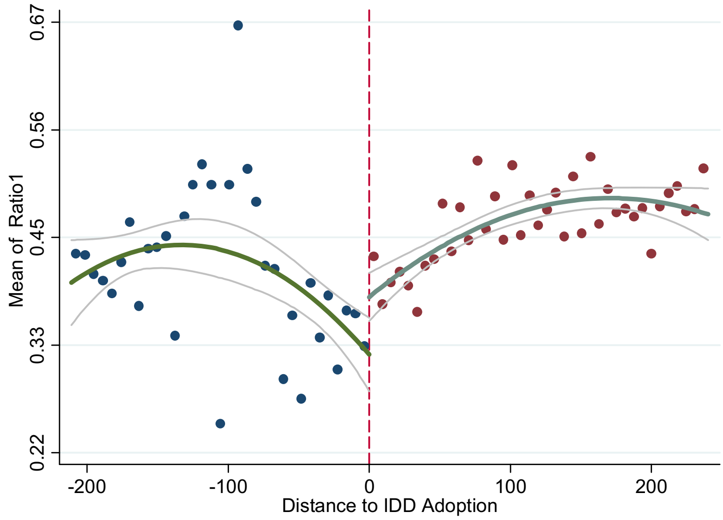
where:

i = firm i,
s = the state of the firm's headquarters,
t = the year.

The variable *NonDisclosure* is one of the two customer information disclosure measures: *Ratio 1* or *Ratio 2*. Note that higher values of *Ratio 1* or *Ratio 2* indicate lower levels of disclosure of customer information. *IDD* is a dichotomous variable indicating whether the state court adopted the IDD

[24] The results are qualitatively the same if we use linear regressions without higher order polynomials.

Panel A: Percentage of unidentified customers (*Ratio 1*)



Panel B: Sales-weighted percentage of unidentified customers (*Ratio 2*)

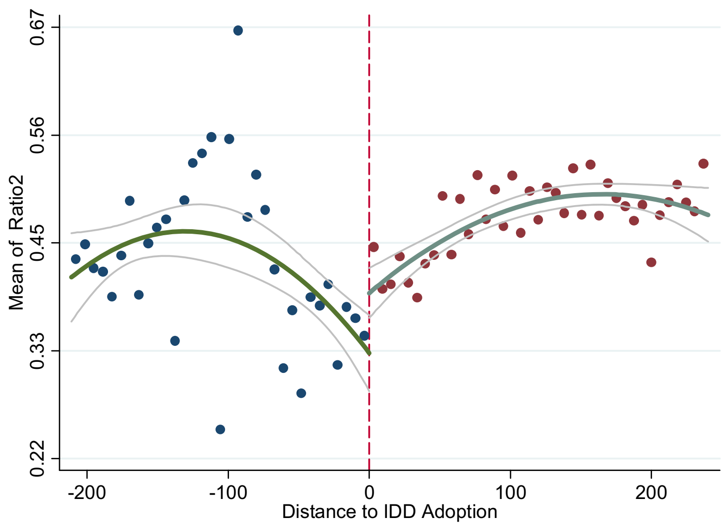


FIG. 1.——The X variable is the time distance (months) to IDD adoption date. In panel A, the Y variable is the percentage of unidentified customers in the segment reporting. In panel B, the Y variable is the sales-weighted percentage of unidentified customers in the segment reporting. This measure captures the level of disclosure on both the customer name and related sales. We divide each side of the distance to IDD adoption into an optimal number of bins (i.e., min{sqrt(N), 10 × ln(N)/ln(10)}, where N is the number of observations), and each dot represents the mean value of the Y variable within a bin. The fitted line is based on a quadratic regression.

in the firm's state of headquarters by year t, X is the vector of control variables defined in section 3, ω_i is the firm/industry fixed effect, θ_s is the state fixed effect, and τ_t is the year fixed effect. We include state fixed effects because of the fact that some firms change their state of headquarters location during the sample period. The set of fixed effects allows us to control for time-invariant differences in disclosure across states and omitted firm/industry characteristics, as well as intertemporal economy-wide shocks to disclosure levels, such as various disclosure regulations. To address concerns about autocorrelation, we cluster standard errors at the state level given that the key independent variable of interest is at the state level (Imbens and Wooldridge [2009]). The coefficient β captures the average changes in disclosure levels of firms headquartered in the IDD-adopting states (i.e., the treatment states) relative to the contemporaneous changes in disclosure levels of firms headquartered in unaffected states (i.e., the control states). An important advantage of the test is that different states adopted the IDD at different times, which allows a given adopting state to be both a treatment and a control state. Moreover, the specification is not affected by the fact that some states did not adopt the IDD during our sample period and some states adopted the IDD before the start of our sample period (Klasa et al. [2017]).

Table 2 presents the DiD regression results. Columns 1 and 3 of table 2 report the results of using *Ratio 1* as the dependent variable and columns 2 and 4 report the results based on *Ratio 2*. In columns 1 and 2, the coefficient on *IDD*, β, is positive and significant at the 1% level. The magnitudes of the coefficient (0.022 and 0.024, respectively, in columns 1 and 2) suggest that firms increase the likelihood of concealing customer identity by 7.12% (7.56% for *Ratio 2*) following adoption of the IDD, which is economically meaningful.[25] To put these figures in perspective, the economic impact of R&D and advertising expenditures (a change from its 10th–90th percentile) documented by Ellis, Fee, and Thomas [2012] is 3.25% and 2.91%, respectively.

In columns 3 and 4, we replace industry fixed effects with firm fixed effects. We retain state fixed effects because of the fact that some firms change their state of headquarters location during the sample period. The impact of the IDD on nondisclosure of customer identity continues to be significant. The magnitudes of the coefficient (0.028 and 0.027, respectively, in columns 3 and 4) suggest that firms increase the likelihood of concealing customer identity by 9.06% (8.74% for *Ratio 2*) following adoption of the IDD, which is economically significant. Overall, the results provide strong support for an economically meaningful effect of the proprietary costs of

[25] Since the dependent variable is in the form of $\ln(1 + y)$, its first derivative should be $\Delta y/(1 + y)$, and the economic magnitude is estimated by $\beta \times (1 + y)/y$. Thus, the economic magnitudes of the coefficients are 7.12% $(0.022 \times (1 + 0.447)/0.447)$ and 7.56% $(0.024 \times (1 + 0.465)/0.465)$, respectively.

TABLE 2

IDD and Nondisclosure of Customer Information

	1 *Ratio 1*	2 *Ratio 2*	3 *Ratio 1*	4 *Ratio 2*
IDD	0.022***	0.024***	0.028**	0.027**
	(0.005)	(0.006)	(0.011)	(0.012)
Missing RD	0.024***	0.031***	0.012	0.007
	(0.008)	(0.009)	(0.013)	(0.013)
RD to Sales	−0.010***	−0.009***	−0.003	0.002
	(0.003)	(0.003)	(0.005)	(0.006)
Intangibility	0.029	0.033	0.038	0.018
	(0.029)	(0.029)	(0.032)	(0.036)
Advertising	0.145	0.130	0.098	0.021
	(0.092)	(0.096)	(0.067)	(0.074)
Size	−0.018***	−0.015***	−0.010**	−0.009**
	(0.002)	(0.002)	(0.004)	(0.003)
HHI	0.041	0.050*	−0.023	−0.025
	(0.029)	(0.027)	(0.041)	(0.042)
BIG N	−0.019**	−0.021**	−0.010	−0.006
	(0.008)	(0.009)	(0.009)	(0.009)
MA	0.019***	0.018***	0.012***	0.010***
	(0.007)	(0.006)	(0.004)	(0.003)
SEO	−0.019**	−0.016**	0.004	0.006
	(0.009)	(0.008)	(0.007)	(0.007)
GDP	−0.001	−0.000	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Unemployment	0.002	0.005	0.000	0.002
	(0.003)	(0.003)	(0.004)	(0.004)
State fixed	Y	Y	Y	Y
Two-digit industry fixed	Y	Y	N	N
Year fixed	Y	Y	Y	Y
Firm fixed	N	N	Y	Y
Cluster at state	Y	Y	Y	Y
N	28,547	28,547	28,547	28,547
Adjusted-R^2	0.070	0.064	0.621	0.611

This table reports results on the main effect of IDD on customer identity disclosure. The dependent variable is natural log of (1+ *Ratio 1* or *Ratio 2*), where *Ratio 1* and *Ratio 2* are the percentage and sales-weighted percentage of unidentified customers in the segment reporting, respectively. *Missing RD* is an indicator variable that takes the value of 1 if the amount of R&D expenditures is missing, and zero otherwise. *RD to Sales* is defined as total R&D divided by total sales. *Intangibility* is defined as intangible assets divided by total sales. *Advertising* is defined as total advertising expenditure divided by total sales. *Size* is defined as natural log of total assets. *BIG N* takes the value of 1 for a Big N auditor, and zero otherwise. *HHI* is the Herfindahl-Hirschman index based on total sales. *MA* takes the value of zero if there is an M&A in the following year, and zero otherwise. *SEO* takes the value of 1 if there is a seasoned equity offering in the following year, and zero otherwise. *GDP* is defined as GDP growth of the state. *Unemployment* is the unemployment rate of the state. The sample consists of 28,547 firm-years in 1994–2010. We report in parentheses standard errors that are clustered within state, and are robust to heteroskedasticity.

***, **, and * indicate significance at the 0.01, 0.05, and 0.10 levels, respectively, using two-tailed tests.

disclosure. That is, adoption of the IDD exogenously increases the proprietary costs of disclosure and firms respond to this shock by significantly reducing disclosure levels of proprietary information.

Regarding the control variables, we find that the coefficient on the indicator variable, *Missing RD*, is positive and significant. It appears that firms without R&D expenditures are more likely to conceal the identity of customers, contrary to the proprietary cost hypothesis. However, firms with missing R&D from Compustat may be those firms with high proprietary costs that conceal information regarding their R&D expenditures (Koh and Reeb [2015]). The coefficient on R&D expenditure is negative and significant, which is inconsistent with the findings in Ellis, Fee, and Thomas [2012]. However, the coefficient becomes positive if we do not include the missing R&D indicator variable. The coefficients of intangible assets and advertising expenditures are insignificant. The coefficient on industry concentration is negative and significant. Given the complex nature of the theory and measurement of industry concentration, we draw no inference from this result.[26] Consistent with Ellis, Fee, and Thomas [2012], firms disclose more before SEO announcements. However, the coefficient on M&A announcement is positive and somewhat unexpected.[27] In our regression, we include the control variables to account for the potential omitted variables problem. However, we caution against drawing inferences from the coefficients of these control variables because they may be endogenously chosen and thus suffer from alternative explanations.

4.3 DYNAMICS

A causal interpretation of the effect of the IDD adoption on customer identity disclosure in our DiD regressions requires that the treatment and the control firms follow parallel trends absent of changes in the status of the IDD adoption. To test the validity of our empirical strategy, we next introduce lead-lag terms in our DiD regression. In table 3, *IDD Adoption*$^{-2}$, *IDD Adoption*$^{-1}$, *IDD Adoption*^{0}, *IDD Adoption*$^{+1}$, and *IDD Adoption*$^{2+}$ are equal to 1 if the firm is headquartered in a state that will adopt the IDD in two years, will adopt the IDD in one year, adopted the IDD in the current year, adopted the IDD one year ago, and adopted the IDD two or more years ago, respectively, and zero otherwise.[28] We use firm-fixed-effect specifications to make sure that we are comparing within-firm trends in the disclosure of customer identity between the treatment and the control firms and that firm fixed effects hold the sample composition constant (Bertrand and Mullainathan [2003]). For both *Ratio 1* (column 1) and *Ratio 2* (column 2), the coefficients on *IDD Adoption*$^{-2}$, *IDD Adoption*$^{-1}$, and *IDD Adoption*^{0} are insignificant, indicating no evidence of the existence of a pre-IDD trend in the disclosure of customer identity between the treatment and the control firms. In column 1, the coefficient on *IDD Adoption*$^{2+}$ is positive and

[26] Table 1 of the online appendix shows that our results are robust to controlling for product market concentration estimated based on U.S. Census data (Ali, Klasa, and Yeung [2014]). In our main tests, we use the traditional concentration measure because measures based on U.S. Census data reduce the sample size substantially.

[27] Our inferences are not affected if we further control for concurrent SEO or M&A announcements.

[28] For the full sample, it is difficult to include both adoption and rejection events. Following Klasa et al. [2017], in an alternative specification, we control for *IDD Rejection*, which equals one if the state in which the firm is headquartered has rejected the previously adopted IDD by year *t*, and zero otherwise. The results are robust.

TABLE 3

Dynamics of IDD Adoption Effect on Nondisclosure of Customer Information

	Ratio 1	*Ratio 2*
IDD Adoption^{-2}	−0.020	−0.018
	(0.019)	(0.020)
IDD Adoption^{-1}	−0.011	−0.025
	(0.026)	(0.026)
IDD Adoption^{0}	−0.022	−0.013
	(0.020)	(0.021)
IDD Adoption^{+1}	0.020	0.025*
	(0.016)	(0.014)
IDD Adoption^{2+}	0.022**	0.022*
	(0.010)	(0.011)
Other Controls	Y	Y
State fixed	Y	Y
Firm fixed	Y	Y
Year fixed	Y	Y
F: $IDD^{-2} + IDD^{-1} + IDD^{0} = 0$	p-value = 0.35	p-value = 0.35
F: $IDD^{+1} + IDD^{2+} = 0$	p-value = 0.08	p-value = 0.03
N	28,547	28,547
Adjusted-R^2	0.621	0.611

This table reports results on the dynamic effect of IDD on customer identity disclosure. The dependent variable is natural log of (1+ *Ratio 1* or *Ratio 2*), where *Ratio 1* and *Ratio 2* are the percentage and sales-weighted percentage of unidentified customers in the segment reporting, respectively. *IDD Adoption*$^{-2}$, *IDD Adoption*$^{-1}$, *IDD Adoption*^{0}, *IDD Adoption*$^{+1}$, and *IDD Adoption*$^{2+}$ are equal to 1 if the firm is headquartered in a state that will adopt the IDD in two years, will adopt the IDD in one year, adopted the IDD in the current year, adopted the IDD one year ago, and adopted the IDD two or more years ago, respectively, and zero otherwise. The same set of control variables as in table 2 are included but not reported for conciseness. The sample consists of 28,547 firm-years in 1994–2010. We report in parentheses standard errors that are clustered within state, and are robust to heteroskedasticity.

***, **, and * indicate significance at the 0.01, 0.05, and 0.10 levels, respectively, using two-tailed tests. See appendix A for all variable definitions.

significant, and in column 2, the coefficients on both *IDD Adoption*$^{+1}$ and *IDD Adoption*$^{2+}$ are positive and significant, suggesting that firms begin to reduce the disclosure of customer identity in one or two years after adoption of the IDD. Figure 1 of the online appendix presents a graphical illustration of the IDD adoption effect in event time.

In the dynamic analysis of table 3, the benchmark period includes all years prior to year –2. Next, we conduct an alternative dynamic analysis restricting the sample to two years before and two years after the IDD adoption or rejection event. Table 2 of the online appendix reports the results. In table 2 of the online appendix, the base year is the IDD adoption or rejection year. Panel A of table 2 in the online appendix presents the dynamic results for IDD adoptions. The coefficients on *IDD Adoption*$^{-2}$ and *IDD Adoption*$^{-1}$ are insignificant, suggesting no evidence of reverse causality or the violation of the parallel trends assumption. The coefficient on *IDD Adoption*$^{+1}$ is positive but insignificant while the coefficient on *IDD Adoption*$^{+2}$ is positive and significant, suggesting that the effect of the IDD on the nondisclosure of customer information starts to manifest in the second year after the adoption. Panel B of table 2 in the online appendix

TABLE 4

IDD and Nondisclosure of Customer Information: Cross-Sectional Analysis

	1 *Ratio 1*	2 *Ratio 2*	3 *Ratio 1*	4 *Ratio 2*	5 *Ratio 1*	6 *Ratio 2*
IDD × *Entry Rate*	0.007***	0.008***				
	(0.001)	(0.001)				
IDD × *Stable Industry*			−0.019*	−0.030***		
			(0.011)	(0.011)		
IDD × *External Financing Dependence*					−0.006***	−0.007***
					(0.001)	(0.002)
Other control	Y	Y	Y	Y	Y	Y
State fixed × year fixed	Y	Y	Y	Y	Y	Y
Two-digit industry fixed × year fixed	Y	Y	Y	Y	Y	Y
Firm fixed	Y	Y	Y	Y	Y	Y
Cluster at state	Y	Y	Y	Y	Y	Y
N	27,378	27,378	28,547	28,547	28,429	28,429
Adjusted-R^2	0.040	0.036	0.039	0.033	0.039	0.034

This table reports results on the effect of IDD on customer identity disclosure conditional on the industry-level entry rate and external financing dependence of a firm's primary industry. The dependent variable is natural log of (1+ *Ratio 1* or *Ratio 2*), where *Ratio 1* and *Ratio 2* are the percentage and sales-weighted percentage of unidentified customers in the segment reporting, respectively. *Entry Rate* is the industry-level entry rate as constructed as in Klapper, Laeven, and Rajan [2006]. *External Financing Dependence* of the industry is constructed as in Rajan and Zingales [1998]. *Stable Industry* is an indicator that takes the value of 1 if the standard deviation of firm sales revenues within the industry during the past 10 years is below the median, and zero otherwise. The same set of control variables as in table 2 are included but not reported for conciseness. Since we control for state × year fixed effects, the coefficients on IDD are absorbed. Similarly, industry × year fixed effects absorb the coefficients on the industry-level variables. Because of computational problems of including multiple high-dimensional fixed effects, we operationalize fixed effects through within-group transformations. This approach yields consistent estimators but leads to lower R^2 (Greene [2003]). The sample consists of 28,547 firm-years in 1994–2010. We report in parentheses standard errors that are clustered within state, and are robust to heteroskedasticity.

***, **, and * indicate significance at the 0.01, 0.05, and 0.10 levels, respectively, using two-tailed tests. See appendix A for all variable definitions.

reports the dynamic analysis for IDD rejections. In all specifications, the coefficients on all dynamic variables are insignificant. Given the small number of IDD rejection events, we conjecture that the results are likely due to limited power of the dynamic test.

4.4 CROSS-SECTIONAL VARIATION IN THE IMPACT OF IDD ON DISCLOSURE

We next explore potential cross-sectional variations in the impact of the IDD on disclosure. It is arguably more important for a firm to protect its customer identity information when the firm faces a higher degree of entry threats. Thus, we expect that the effect of the IDD adoption on customer information nondisclosure is more pronounced for firms in industries with a higher degree of entry threats. To measure the degree of entry threats, we use the industry entry rates constructed by Klapper, Laeven, and Rajan [2006]. Columns 1 and 2 of table 4 report the results of interacting the degree of entry threats with the IDD indicator. Besides the control variables in equation (1) and firm fixed effects, we further include the interaction of state and year fixed effects and the interaction of industry and year fixed effects, which effectively remove time-varying state and industry effects.[29] Consistent with our prediction, results in columns 1 and 2 of table 4 show that adoption of the IDD has a stronger impact on the nondisclosure of customer information for firms in industries with higher entry threats. Specifically, the coefficients suggest that a one standard deviation increase in the industry entry rate would increase the impact of the IDD on nondisclosure by approximately 60–70%. The results are both statistically and economically significant.

For an industry that is old and stable, it is likely that the key players in this industry already have a lot of knowledge about the identities of their competitors' customers. As such, the proprietary costs of disclosing customer identity information are lower for firms in more stable industries. Consistent with this argument, columns 3 and 4 of table 4 show that the adoption of the IDD has a weaker impact on the nondisclosure of customer information for firms in industries with less volatile sales.

Managers can reduce their firms' cost of capital by reducing information asymmetry through increased levels of disclosure. Given that managers tend to trade off the benefits of disclosure in the capital market with proprietary costs of disclosure in the product market, we expect that the impact of the IDD on nondisclosure of customer information should be less pronounced if firms exhibit a greater reliance on external financing. Following Rajan and Zingales [1998], we estimate external financing dependence as the industry median ratio of the capital expenditures minus operational cash flow divided by capital expenditures (RZ index). Columns 5 and 6 of table 4 report the results of interacting external financing dependence and the IDD indicator. Consistent with our prediction, the impact of the IDD on customer identity nondisclosure is less pronounced for firms exhibiting a higher degree of external financing dependence. Specifically, a one standard deviation increase in the RZ index would lead to about 50–60% decreases in the effect of the IDD on nondisclosure. The results are again statistically and economically significant.[30]

Overall, the effect of the IDD adoption on nondisclosure of customer identity is more pronounced for firms facing higher threats of losing trade secrets and the effect is less pronounced for firms with a higher dependence on external financing. These cross-sectional results provide further support for a causal interpretation of our main results because it is less

[29] The IDD main effect and year effect are subsumed by state-year and industry-year fixed effects. For brevity, we only present the coefficients of the variables of interest, the interactions between IDD and cross-sectional variables, for all cross-sectional variations analyses.

[30] In additional tests, we also interact the IDD indicator with other proxies of potential capital market benefits, such as the likelihood of equity and debt issuance, institutional ownership, and analyst following. However, coefficients on most of the interaction terms are statistically insignificant, with the exception of debt issuance likelihood, for which we find that the impact of the IDD on disclosure is less pronounced for firms with a higher likelihood of issuing debt in the future (see table 3 of the online appendix).

likely that unobserved omitted variables can explain both the main results and the cross-sectional results.

4.5 CROSS-SECTIONAL ANALYSIS: OTHER MEASURES OF PRODUCT MARKET COMPETITION

In this section, we further explore the potential moderating effects of several alternative measures of production market competition (e.g., Harris [1998], Botosan and Stanford [2005], Berger and Hann [2007], Bens, Berger, and Monahan [2011], Ali, Klasa, and Yeung [2014]). As discussed earlier, prior research has used these measures to capture the proprietary costs of disclosure and produced mixed evidence. Thus, ex ante, we do not have clear predictions regarding the interacting effects between the IDD indicator and these competition measures. Instead, our motivation here is to examine whether and how these industry competition variables can sort firms on the extent of their proprietary costs of disclosure in our context.

The first industry competition variable we examine is the abnormal profit persistence measure constructed by Harris [1998], which is used in subsequent studies of Botosan and Stanford [2005] and Berger and Hann [2007], among others. Harris [1998] argues that industries with higher abnormal profit persistence should have lower competition. Columns 1 and 2 of table 5 report the results of interacting the IDD indicator with the level of abnormal profit persistence. We find that the effect of the IDD on nondisclosure of customer identity is marginally less pronounced for firms in industries with high abnormal profit persistence (low competition).

The next variable we examine is Bens, Berger, and Monahan's [2011] combined measure of competition capturing both competition from potential new entrants (proxied by the level of capital expenditures) and current rivals (proxied by industry price-to-cost margin). Columns 3 and 4 report the results. The effect of the IDD on disclosure is less pronounced for firms in industries with lower competition (industries with a higher level of capital expenditures and higher price-to-cost margins). In fact, the effect of the IDD on nondisclosure is insignificant for firms in industries with low competition as measured by Bens, Berger, and Monahan's [2011] method.

Finally, we examine the effect of the concentration ratio of industry market shares. Theoretically, firms should face a higher risk of losing their customers to competitors if there is intense competition in their industry. On the other hand, concentration of market shares can be driven partially by trade secrets, and firms with more concentrated product markets can have more valuable trade secrets. Columns 5 and 6 report the results of interacting the IDD indictor with a Compustat-based measure of industry concentration. The effect of the IDD on nondisclosure of customer identity is marginally more pronounced for firms in more concentrated industries. In columns 7 and 8, we interact the IDD indicator with a U.S. Census–based measure of concentration that includes both private and public firms (e.g., Ali, Klasa, and Yeung [2014]). In contrast to the results in columns 5 and 6, the effect of the IDD on nondisclosure is significantly less pronounced for

TABLE 5

Cross-Sectional Analysis: Other Measures of Product Market Competition

	1 *Ratio 1*	2 *Ratio 2*	3 *Ratio 1*	4 *Ratio 2*	5 *Ratio 1*	6 *Ratio 2*	7 *Ratio 1*	8 *Ratio 2*
IDD × *Abnormal ROA*	−0.019	−0.019*						
	(0.011)	(0.010)						
IDD × *Less Compete*			−0.031**	−0.032**				
			(0.009)	(0.009)				
IDD × *HHI*					0.058*	0.049		
					(0.034)	(0.035)		
IDD × *HHI Census*							−0.233***	−0.232**
							(0.077)	(0.090)
Other control	Y	Y	Y	Y	Y	Y	Y	Y
State fixed × year fixed	Y	Y	Y	Y	Y	Y	Y	Y
Two-digit industry fixed × year fixed	Y	Y	Y	Y	Y	Y	Y	Y
Firm fixed	Y	Y	Y	Y	Y	Y	Y	Y
Cluster at state								
N	28,547	28,547	28,547	28,547	28,547	28,547	17,371	17,371
Adjusted-R^2	0.039	0.033	0.040	0.034	0.039	0.033	0.021	0.019

This table reports results on the effect of IDD on customer identity disclosure conditional on the different measures of product market competition. The dependent variable is natural log of (1+ *Ratio 1* or *Ratio 2*), where *Ratio 1* and *Ratio 2* are the percentage and sales-weighted percentage of unidentified customers in the segment reporting, respectively. *Abnormal ROA* is defined as industry abnormal profit persistence (Harris [1998]). The definition of *Less Compete* follows Bens, Berger, and Monahan [2011]. *HHI Census* is the U.S. Census industry concentration index. The same set of control variables as in table 2 are included but not reported for conciseness. Since we control for state × year fixed effects, the coefficients on IDD are absorbed. Similarly, industry × year fixed effects absorb the coefficients on the industry-level variables. Because of computational problems of including multiple high-dimensional fixed effects, we operationalize fixed effects through within-group transformations. This approach yields consistent estimators but leads to lower R^2 (Greene [2003]). The sample consists of a maximum 28,547 firm-years in 1994–2010. We report in parentheses standard errors that are clustered within state, and are robust to heteroskedasticity.

***, **, and * indicate significance at the 0.01, 0.05, and 0.10 levels, respectively, using two-tailed tests. See appendix A for all variable definitions.

firms in more concentrated industries. A one standard deviation increase in the Census-based concentration ratio would lead to 44–46% decreases in the effect of the IDD on nondisclosure.

Overall, the effect of the IDD on nondisclosure of customer identity appears to be more pronounced for firms in more competitive industries, when competition is measured using Harris's [1998] abnormal profit persistence proxy, Bens, Berger, and Monahan's [2011] combined measure of competition, and the U.S. Census–based industry concentration ratio. To the extent that these measures capture proprietary costs of disclosure without substantial errors, the cross-sectional results suggest that ex ante proprietary costs of disclosure enhance the impact of the IDD on the concealment of customer identity.

5. *Additional Tests and Robustness Checks*

In this section, we conduct several additional tests and robustness checks to further buttress the causal effects of the IDD adoption on the disclosure of customer identities.

5.1 GEOGRAPHICAL DISPERSION OF EMPLOYEES

Our main tests rely on the adoption of the IDD in a firm's state of headquarters, where arguably most of the firm's employees with access to trade secrets are employed (Klasa et al. [2017]). However, using a firm's headquarters-state's IDD adoption to measure the firm's overall exposure to the IDD can involve substantial measurement errors for firms that have a geographically dispersed workforce. To capture the overall exposure to the IDD protection more accurately, we estimate a weighted-measure of IDD exposure that takes into account the number of employees for multistate firms in each state that the firm has subsidiaries, branches, or plants. Specifically, for each firm-year observation, we obtain the number of employees working in each state and in the year from the National Establishment Time Series (NETS) database. The database provides a comprehensive record of all business establishments in the United States since 1989.[31] We then calculate a firm's weighted exposure to the IDD in a year as follows:

$$\textit{Weighted IDD} = \sum_{s=1}^{s} \frac{\textit{employee}_{i,s,t} \times \textit{IDD}_{s,t}}{\sum_{s=1}^{s} \textit{employee}_{i,s,t}}, \tag{2}$$

where:
$\textit{employees}_{i,s,t}$ = firm i's number of employees in state s in year t,$\textit{IDD}_{s,t}$ = the IDD adoption indicator for state s in year t.

Table 6 reports the results of estimating equation (1) after replacing *IDD* with *Weighted IDD*. The coefficients on the weighted version of IDD adoption are 0.030 and 0.031, respectively, for regressions with *Ratio 1* and *Ratio 2* as the dependent variables. The coefficients are economically larger than those reported in table 2, suggesting that ignoring the geographic dispersion of workforce likely understates firms' response to the IDD.

As an additional way to address the measurement error problem, we also reestimate equation (1) using a reduced sample of firms with a majority of their employees working in the headquarters states. Table 7 reports the results. The coefficients on the *IDD Adoption* indicator are 0.041 and 0.042, respectively, for regressions with *Ratio 1* and *Ratio 2* as the dependent variables. The effects are again larger than those reported in table 2, confirming our observation that the estimates in our main test are conservative due to geographic dispersion of the workforce.[32]

[31] We merge our main sample with NETS by company names. Approximately half of our sample firms are covered by NETS. We assume that firms that do not have plant-level data in NETS do not have operations in multiple states. In robustness tests, we reestimate the weighted IDD effects using only observations covered by NETS and the results continue to hold.

[32] Note, however, that the IDD coefficients in tables 6 and 7 are not statistically larger than those in table 2.

TABLE 6
IDD and Nondisclosure of Customer Information: Weighted Treatment Effect

	1 *Ratio 1*	2 *Ratio 2*
Weighted IDD	0.030***	0.031***
	(0.008)	(0.010)
Missing RD	0.011	0.006
	(0.013)	(0.013)
RD to Sales	−0.003	0.002
	(0.005)	(0.005)
Intangibility	0.037	0.018
	(0.032)	(0.036)
Advertising	0.099	0.022
	(0.066)	(0.073)
Size	−0.010***	−0.010***
	(0.004)	(0.003)
HHI	−0.023	−0.026
	(0.041)	(0.042)
BIG N	−0.010	−0.006
	(0.009)	(0.009)
MA	0.012***	0.010***
	(0.004)	(0.003)
SEO	0.004	0.006
	(0.007)	(0.007)
GDP	0.001	0.001
	(0.001)	(0.001)
Unemployment	0.001	0.003
	(0.004)	(0.004)
State fixed	Y	Y
Two-digit industry fixed	N	N
Year fixed	Y	Y
Firm fixed	Y	Y
Cluster at state	Y	Y
N	28,547	28,547
Adjusted-R^2	0.621	0.611

This table reports results on the effect of weighted average IDD exposure on customer identity disclosure. For each state in which the firm has establishments, we use the ratio of the number of employees in that state to total number of employees as the weight to estimate the *Weighted IDD*. For firms without detailed information on the fraction of nonheadquarters employees, we assume all employees are working in the headquarters. The dependent variable is natural log of (1 + *Ratio 1* or *Ratio 2*), where *Ratio 1* and *Ratio 2* are the percentage and sales-weighted percentage of unidentified customers in the segment reporting, respectively. The sample period is 1994–2010. The same set of control variables as in table 2 are included but not reported for conciseness. The sample consists of a maximum 28,547 firm-years in 1994–2010. We report in parentheses standard errors that are clustered within state, and are robust to heteroskedasticity.

***, **, and * indicate significance at the 0.01, 0.05, and 0.10 levels, respectively, using two-tailed tests. See appendix A for all variable definitions.

5.2 STATE-LEVEL CONFOUNDS

As discussed in section 3, adoption of the IDD is identified based on judicial decisions, which are more likely to be driven by the merits of precedent-setting cases than state economic conditions. Nevertheless, to address the potential omitted variables problem, we include in our regression models observed state-level economic conditions, in the form of state growth and unemployment rates. In table 4 of the online appendix, we include more

TABLE 7

IDD and Nondisclosure of Customer Information: Firms with Majority of Employees at Headquarters

	1	2
	Ratio 1	*Ratio 2*
IDD	0.041***	0.042***
	(0.013)	(0.016)
Missing RD	0.013	0.004
	(0.016)	(0.017)
RD to Sales	−0.003	0.002
	(0.006)	(0.006)
Intangibility	0.030	0.016
	(0.044)	(0.050)
Advertising	0.114	0.071
	(0.091)	(0.109)
Size	−0.011***	−0.012***
	(0.004)	(0.004)
HHI	−0.037	−0.033
	(0.056)	(0.059)
BIG N	−0.022*	−0.021*
	(0.012)	(0.013)
MA	0.017***	0.014**
	(0.006)	(0.006)
SEO	0.011	0.013*
	(0.008)	(0.008)
GDP	0.002	0.002
	(0.001)	(0.001)
Unemployment	0.000	0.002
	(0.004)	(0.005)
State fixed	Y	Y
Two-digit industry fixed	N	N
Year fixed	Y	Y
Firm fixed	Y	Y
Cluster at state	Y	Y
N	19,717	19,717
Adjusted-R^2	0.625	0.616

This table reports results on the effect of IDD on customer identity disclosure based on a subsample of firms with a majority of their employees working in the headquarters (i.e., the *fraction of number of non-headquarters employees* is less than 50%). For firms without detailed information on the fraction of nonheadquarters employees, we assume all employees are working in the headquarters. The dependent variable is natural log of (1 + *Ratio 1* or *Ratio 2*), where *Ratio 1* and *Ratio 2* are the percentage and sales-weighted percentage of unidentified customers in the segment reporting, respectively. The sample period is 1994–2010. The same set of control variables as in table 2 are included but not reported for conciseness. The sample consists of maximum 19,717 firm-years in 1994–2010. We report in parentheses standard errors that are clustered within state, and are robust to heteroskedasticity.

***, **, and * indicate significance at the 0.01, 0.05, and 0.10 levels, respectively, using two-tailed tests. See appendix A for all variable definitions.

state-level variables and the results continue to be robust. To further isolate potential confounding factors, we next restrict the control states of each adopting state to its neighbors (e.g., Ljungqvist, Zhang, and Zuo [2017]). Suppose the adoption of the IDD is driven by unobserved changes in local conditions and firms respond to these changes rather than to adoption of the IDD. In such a case, both firms in the adopting states and in the neighboring, nonadopting states would spuriously appear to react to adoption of the IDD because unobserved changes in economic conditions are likely to spill across state borders. To remove these potential confounding effects, we match each adopting state with its neighboring states and include only the neighboring states in the control sample. If state neighbors are exposed to roughly the same local economic conditions, we can then difference away the unobserved confounds using a shared border fixed effect. Essentially, we are comparing firms in each adopting state with their peers in neighboring, nonadopting states. Table 8 reports the results of estimating the effect of IDD from variations within shared-border states. We find that the effect of the IDD on nondisclosure of customer information continues to be significant.

5.3 PLACEBO TEST

To further enhance the credibility of our results, we next conduct a placebo test. Toward this end, for each state that adopted (rejected) the IDD, we randomly select a pseudo-IDD adoption (rejection) year and construct an indicator variable, *Placebo IDD*, using the same procedure as our main tests. We then reestimate equation (1) by replacing *IDD* with *Placebo IDD*. We repeat this exercise for 1,000 times and plot the discretized probability density of the placebo coefficients in figure 2 of the online appendix. For comparison, we also include a line for the IDD coefficient based on actual IDD events. Figure 2 of the online appendix shows that the placebo coefficient largely follows a normal distribution centered at zero (mean = 0.0008, standard error = 0.0042 when *Ratio 1* is the dependent variable; mean = 0.0007, standard error = 0.004 when *Ratio 2* is the dependent variable). The actual IDD coefficient is more than four standard deviations larger than the mean of placebo coefficients.

5.4 REMOVING NON-PRECEDENT-SETTING STATES

The IDD was developed and applied as common law on a state-to-state basis (e.g., Kahnke, Bundy, and Liebman [2008], Wiesner [2012]). An IDD precedent-setting case becomes case law of the state and the courts in that state will subsequently follow its ruling on the applicability of the IDD. A court, before ruling on a new IDD legal case, will survey the previous state decisions addressing the IDD. If the courts in a state have not considered the IDD, it remains unclear whether the IDD is viable.[33] Therefore, *relative* to these states with unclear stance, the states that have explicitly recognized the IDD should provide a stronger protection of trade secrets. In our main research design, we set the IDD indicator to zero for every year for states that never explicitly considered the IDD. To test the robustness of our results to this design choice, we next reexamine the effect of the IDD by excluding firms in these states with unsettled legal stance on the IDD.

[33] For example, the interpretation of the IDD in California had attracted years of speculation before the state court decision to reject the IDD in *Schlage Lock Company v. Whyte* (2002) 101 Cal. App. 4th 1443.

TABLE 8
IDD and Nondisclosure of Customer Information: Adjacent State Analysis

	1 *Ratio 1*	2 *Ratio 2*
IDD	0.020**	0.020**
	(0.008)	(0.008)
Missing RD	0.017*	0.019*
	(0.010)	(0.010)
RD to Sales	−0.010*	−0.008
	(0.006)	(0.006)
Intangibility	−0.004	−0.003
	(0.039)	(0.043)
Advertising	0.235*	0.207
	(0.116)	(0.130)
Size	−0.018***	−0.014***
	(0.002)	(0.002)
HHI	0.016	0.029
	(0.033)	(0.032)
BIG N	−0.014	−0.020
	(0.012)	(0.014)
MA	0.016	0.017*
	(0.010)	(0.009)
SEO	−0.005	−0.005
	(0.010)	(0.010)
GDP	−0.002	−0.002
	(0.002)	(0.001)
Unemployment	−0.001	0.003
	(0.006)	(0.006)
State fixed	Y	Y
Two-digit industry fixed	Y	Y
Matched-group fixed	Y	Y
Year fixed	Y	Y
Cluster at state	Y	Y
N	16,846	16,846
Adjusted-R^2	0.077	0.071

This table reports results on the effect of IDD on customer identity disclosure based on neighboring states matched samples. The dependent variable is natural log of (1 + *Ratio 1* or *Ratio 2*), where *Ratio 1* and *Ratio 2* are the percentage and sales-weighted percentage of unidentified customers in the segment reporting, respectively. Following the time sequence of the adoption date, for each IDD-adopting state, we match it with neighboring nonadopting states *with replacement*. This procedure yields 13 matched groups. Each group consists of one *IDD*-adopting state and multiple neighboring nonadopting states. The 13 groups are as follows: (NY: VT), (FL: AL), (DE: DC, MD), (NC: VA, SC, TN), (MN: ND, SD, WI), (IL: KY), (TX: NM, OK, LA), (MA: RI, NH), (IA: NE), (AR: MS), (WA: OR, ID), (UT: NV, CO, WY, AZ), (OH: WV). We control for the matched group fixed effects to examine the difference between treatment sample and control sample within each matched group. The sample period is 1994–2010. The same set of control variables as in table 2 are included but not reported for conciseness. We report in parentheses standard errors that are clustered within state, and are robust to heteroskedasticity.

***, **, and * indicate significance at the 0.01, 0.05, and 0.10 levels, respectively, using two-tailed tests. See appendix A for all variable definitions.

That is, we restrict our sample to those firms in the 21 states in which we can clearly identify adoption or rejection of the IDD. Columns 3 and 4 of table 9 present the results using this reduced sample. The effect of the IDD on nondisclosure of customer information is robust to this alternative research design.

TABLE 9

Removing Non-Precedent-Setting States

	1 *Ratio 1*	2 *Ratio 2*
IDD	0.029**	0.030**
	(0.011)	(0.011)
Missing RD	−0.005	−0.005
	(0.015)	(0.014)
RD to Sales	−0.008	−0.004
	(0.007)	(0.007)
Intangibility	0.065*	0.038
	(0.036)	(0.033)
Advertising	0.086	0.024
	(0.094)	(0.099)
Size	−0.012***	−0.009*
	(0.004)	(0.004)
HHI	−0.029	−0.026
	(0.056)	(0.049)
BIG N	−0.015	−0.012
	(0.011)	(0.012)
MA	0.013**	0.013**
	(0.006)	(0.005)
SEO	0.003	0.006
	(0.010)	(0.010)
GDP	0.001	0.001
	(0.001)	(0.001)
Unemployment	−0.008**	−0.005
	(0.004)	(0.004)
State fixed	Y	Y
Firm fixed	Y	Y
Year fixed	Y	Y
Cluster at state	Y	Y
N	17,537	17,537
Adjusted-R^2	0.643	0.638

This table reports results on the effect of IDD on customer identity disclosure based on a subsample of firms that are headquartered in 21 states whose IDD status can be clearly identified. The dependent variable is the natural log of (1 + *Ratio 1* or *Ratio 2*), where *Ratio 1* and *Ratio 2* are the percentage and sales-weighted percentage of unidentified customers in the segment reporting, respectively. The sample period is 1994–2010. The same set of control variables as in table 2 are included but not reported for conciseness. We report in parentheses standard errors that are clustered within state, and are robust to heteroskedasticity.

***, **, and * indicate significance at the 0.01, 0.05, and 0.10 levels, respectively, using two-tailed tests. See appendix A for all variable definitions.

5.5 ALTERNATIVE SAMPLE PERIOD AND THE EFFECT OF UTSA

To measure the variable *IDD Adoption* more accurately, we use observations during the period of 1994–2010, where we can precisely identify firms' historical headquarters locations. In this section, we reestimate equation (1) using an expanded sample starting from 1977. For firm-year observations for which we cannot identify historical locations, we use the most recently available locations. Because many states adopted the UTSA before 1994, we include an additional control variable, *UTSA*, defined similarly to *IDD*.[34] Specifically, for states that adopted the UTSA during the sample period, the UTSA indicator is zero in all years prior to the adoption year and 1 afterwards. For states that adopted the UTSA before 1977, we set the UTSA indicator to 1, and for states that never adopted UTSA, we set the indicator to zero.

Table 5 of the online appendix presents the results. The effect of the IDD adoption on disclosures of customer identities continues to be statistically and economically significant in this alternative sample. In this study, we use the 1994–2010 sample in our baseline regressions because testing power (or Type II errors) appears to be less of a concern than the measurement error problem, given that we already have a large number of observations and that we find significant results even with the shorter sample period. Interestingly, the effect of the adoption of the UTSA on the concealment of customer identity is also positive, but the impact is much smaller than that of the IDD and is statistically insignificant. The results are consistent with our earlier argument that the IDD provides much more powerful protection for trade secrets than the UTSA.

5.6 SMALL BUSINESS REPORTING SAMPLE

Although the SEC appears to require firms to disclose the identity of their major customers, many firms fail to do so. Thus, we argue that disclosure of customer identity is, to some extent, voluntary. However, interpreting our measures as representing purely voluntary disclosures is still problematic (Beyer et al. [2010]). Thus, one caveat of our research is the mixed nature of our disclosure measures. We argue, however, that the mixed nature of disclosure measures is not a substantial concern in testing the proprietary cost hypothesis, since this hypothesis should lead to the same predictions regarding the impact of the proprietary costs of disclosures on the levels of mandatory and voluntary disclosures. Nevertheless, we next partially address this concern by restricting the sample to small business firms under Regulation S-B. Regulation S-B requires firms to disclose the existence of important customers but not the names of these customers. Thus, disclosure of customer identity for this sample of firms is purely voluntary. Table 6 of the online appendix reports the results. The main results continue to hold for this small business firm sample.

5.7 IDD AND FINANCIAL LEVERAGE

Klasa et al. [2017] find that the IDD adoptions decrease competitive threats, which, in turn, increase financial leverage. Greater reliance on debt financing may reduce the capital market benefits of public disclosure because firms can communicate with banks using private channels (e.g., Dang

[34] Note that it is not necessary to control for *UTSA* in our main test because few states adopted UTSA during the period 1994–2010 and thus the effect of UTSA is largely absorbed by state fixed effects. In a robustness check, we show that the effect of the IDD on disclosure is identical after controlling for UTSA for our main test (i.e., table 2).

et al. [2014]). Thus, one potential concern is that the reduction in disclosure we observe may be driven by the shift in capital structure. In table 7 of the online appendix, we show that our results are not affected if we include financial leverage as an additional control variable, suggesting that the leverage effect is unlikely to be one of the mechanisms through which IDD adoption affects disclosure.

A related issue is that the reduction in proprietary disclosures we document may constitute an informational channel that drives the leverage results in Klasa et al. [2017]. That is, adoption of the IDD increases the proprietary costs of public disclosure and firms shift to debt financing that relies less on public disclosure. To investigate this possibility, we first replicate Klasa et al.'s results and then we control for our disclosure measures. Table 8 of the online appendix shows that the effect of the IDD on financial leverage is only mildly reduced after controlling for *Ratio 1* or *Ratio 2*, although the effect of (non)disclosure on financial leverage is significant. Overall, we conclude that the effects documented in our paper and those in Klasa et al. [2017] are largely independent.

5.8 STATE-LEVEL REGRESSIONS

In our firm-level regressions, there is a substantial variation in the number of observations across different states. If the great majority of firms were concentrated in a few states, the effective power of state-level variations in the IDD adoption would be reduced. The descriptive statistics reported in table 1, panel B, suggest that this is unlikely to be the case. Nevertheless, to increase the effective power of the tests, we conduct a state-level regression as follows. First, we estimate a firm-level regression of *Ratio 1* (*Ratio 2*) on a set of control variables, including *Missing RD, RD to Sale, Intangibility, Adverting, Size, HHI, Big N, SEO, MA, Unemployment,* and *GDP*. Second, we obtain the residuals from the first step and define state-year-level disclosure ratios (*Average Residuals Ratio 1* and *Average Residuals Ratio 2*) as the average residuals for each state-year. Third, we regress the state-year-level disclosure ratios on the *IDD* indicator, state fixed effects, and year fixed effects. Table 10 reports the state-level regression results. To mitigate the concerns about individual state-outliers, we estimate the standard errors using a jackknife approach. The coefficients on *IDD* are positive and statistically and economically significant in both regressions when *Average Residuals Ratio 1* and *Average Residuals Ratio 2* are the dependent variables, respectively, consistent with our firm-level regression results.

5.9 SEGMENT DISCLOSURES

In this section, we examine whether our main results are generalizable to segment disclosure, given that many of the prior studies on the proprietary cost hypothesis look at segment disclosure. Specifically, we investigate the effect of the IDD on segment reporting aggregation (e.g.,

TABLE 10
State-Level Regression

	1 *Average Residuals Ratio 1*	2 *Average Residuals Ratio 2*
IDD	0.113**	0.164***
	(0.057)	(0.062)
State fixed	Y	Y
Year fixed	Y	Y
N	846	846
Adjusted-R^2	0.260	0.278

This table reports results on the effect of IDD on customer identity disclosure from state-level regressions. The dependent variable is the average residual of *Ratio 1* or *Ratio 2* computed from the following steps. First, we regress the natural log of (1 + *Ratio 1* or *Ratio 2*) on a set of variables including *Missing RD*, *RD to sale*, *Intangibility*, *Adverting*, *Size*, *HHI*, *Big N*, *SEO*, *MA*, *GDP*, and *Unemployment*. Second, we estimate the residuals of the regression. Third, we calculate the mean value of residuals by each state-year, and define it as the dependent variable in the state-level regressions. The sample period is 1994–2010. We report in parentheses standard errors that are clustered within state, and are robust to heteroskedasticity.

***, **, and * indicate significance at the 0.01, 0.05, and 0.10 levels, respectively, using two-tailed tests. See appendix A for all variable definitions.

Bens, Berger, and Monahan [2011]). Following the spirit of Bens, Berger, and Monahan [2011], we measure the discretionary aggregation of segment information by comparing reported segments in Compustat with "pseudo segments" inferred from plant-level data in NETS. The indicator variable, *Pseudo Segments*, takes the value of 1 if the firm has at least one pseudo segment that is not reported as an external segment, and zero otherwise. Table 9 of the online appendix reports the results of reestimating equation (1) by replacing the dependent variable with *Pseudo Segments*. The coefficients on *IDD Adoption* are positive and marginally significant, suggesting that firms have greater incentives to aggregate their segments in financial reports when facing higher proprietary costs of disclosure.[35]

6. *Conclusion*

This paper contributes to the financial reporting and disclosure literature by identifying and quantifying the causal effect of proprietary costs on corporate disclosure. We exploit the impact of staggered adoption of the IDD across U.S. states on firms' disclosure of their customers' identities. Adoption of the IDD exogenously increases the proprietary costs of disclosure, or equivalently, the proprietary benefits of nondisclosure, and firms respond to the shock by reducing the level of disclosure regarding their customers' identities. The results are robust to a battery of robustness checks. Using cross-sectional analyses, we find that the impact of the IDD on disclosure is driven by firms in industries with a higher degree of

[35] We caution against a strong interpretation of our results on segment disclosure. Unlike Bens, Berger, and Monahan [2011], who use confidential U.S. Census Bureau data, our NETS-based measure may involve measurement errors given that the database is publicly available. Moreover, as discussed in section 1, segment disclosure may capture agency motives.

entry threats, firms in younger and more volatile industries, and firms with a lower degree of external financing dependence.

Our paper also extends the literature on how laws impact financial reporting and disclosure. Previous research along this line has generally focused on laws pertaining to the capital markets, such as company laws and disclosure rules. In contrast, our paper studies legal protections for trade secrets, which pertains to the intellectual property and labor laws. Our evidence suggests that trade secrets laws can have potential spillover effects on capital market disclosures. Thus, to gain a more complete picture of corporate disclosure, it is important for future research to explore this type of spillover effect from noncapital market regulations.

Our study has at least two limitations. First, although we argue that adoption of the IDD is exogenous to corporate disclosure decisions, unlike in randomized experiments, we do not actually randomly assign firms into treatment and control groups (Gow, Larcker, and Reiss [2016]; Li and Zhang [2015]). As a result, there is always a possibility that some unknown changes in economic environments impact both the judicial decisions of the adopting states and the corporate disclosures of firms headquartered in these states. In addition, even if the IDD events are entirely exogenous to disclosure decisions, they provide at most indirect shocks to proprietary costs. Given the unobservable nature of proprietary costs, we cannot test whether or to what extent the IDD adoptions affect proprietary costs of disclosure. Second, our study employs a measure of disclosure that is arguably associated with significant proprietary costs, which partially addresses the concerns raised by Lang and Sul [2014]. However, by focusing on disclosures of customer information, our results may not be generalizable to other corporate disclosures, although we show that our results appear to be generalizable to segment disclosures. With these caveats in mind, we believe that our research represents a meaningful advancement in the literature on the proprietary cost hypothesis of corporate disclosure.

APPENDIX A

Variable Definitions

Variable Name	Definition
Ratio 1	Percentage of unidentified customers in the segment reporting.
Ratio 2	Sales-weighted percentage of unidentified customers in the segment reporting, measured by $\frac{\sum_k^K sales_{k,i,t} \times name\ missing\ indicator_{k,i,t}}{\sum_k^K sales_{k,i,t}}$.
Average Residuals Ratio 1 (Ratio 2)	First, we regress the natural log of (1 + Ratio 1 or Ratio 2) on a set of variables including *Missing RD*, *RD to Sale*, *Intangibility*, *Adverting*, *Size*, *HHI*, *Big N*, *SEO*, *MA*, *GDP*, and *Unemployment*. Second, we estimate the residuals of the

(*Continued*)

APPENDIX A—*Continued*

Variable Name	Definition
	regression. Third, we calculate the mean value of residuals by each state-year, and define it as the dependent variable in the state-level regressions.
IDD	Equal to 1 for the postadoption period, and zero otherwise.
IDD Adoption^{-2}	Equal to 1 if the firm is headquartered in a state that will adopt the *IDD* in two years, and zero otherwise.
IDD Adoption^{-1}	Equal to 1 if the firm is headquartered in a state that will adopt the *IDD* in one year, and zero otherwise.
IDD Adoption^{0}	Equal to 1 if the firm is headquartered in a state that adopted the *IDD* in the current year, and zero otherwise.
IDD Adoption^{+1}	Equal to 1 if the firm is headquartered in a state that adopted the *IDD* one year ago, and zero otherwise.
IDD Adoption^{2+}	Equal to 1 if the firm is headquartered in a state that adopted the *IDD* two or more years ago, and zero otherwise.
IDD Rejection	An indicator variable set to 1 beginning the year when the state of the firm's headquarters rejected the previously adopted IDD, and zero otherwise.
Weighted IDD	Employees-weighted IDD of each firm, measured by $\frac{\sum_s^s Employees_{s,i,t} \times IDD_{s,t}}{\sum_s^s Employees_{s,i,t}}$, where: $Employees_{s,i,t}$ = the total number of employees of firm i in state s in year t.
Intangibility	Intangible assets divided by total sales.
Advertising	Total advertising expenditure divided by total sales.
Size	Natural log of total assets.
Abnormal ROA	Harris's [1998] measure of industry abnormal profit persistence.
BIG N	Equal to 1 for Big N auditor, and zero otherwise.
HHI	Herfindahl-Hirschman index based on total sales.
Missing RD	Equal to 1 if R&D is missing, and zero otherwise.
RD	Total R&D divided by total sales.
External Financing Dependence	Following Rajan and Zingales [1998], we compute this as capital expenditures minus cash flow from operations divided by capital expenditures. Then, we define the industry median value as the industry level of external financing dependence.
Entry Rate	Industry level entry rate constructed as in Klapper, Laeven, and Rajan [2006].
Less Compete	Equal to 1 if the industry competition is low, and zero otherwise. We use the sales-weighted total capital expenditures for an industry to measure the barriers to entry, and the price cost margin to measure the product sustainability. Higher values of the entry barrier combined with lower values of the product substitutability ratio indicate lower levels of competition.
Stable Industry	An indicator that takes the value of 1 if the standard deviation of firm sales revenues within the industry during the past 10 years is below the median, and zero otherwise.
HHI Census	U.S. Census–based industry concentration (Ali, Klasa, and Yeung [2014]).
MA	Equal to 1 if there is an M&A in the following year, and zero otherwise.
SEO	Equal to 1 if there is a seasoned equity offering in the following year, and zero otherwise.
GDP	GDP growth of the state.
Unemployment	Unemployment rate of the state.

APPENDIX B

Precedent-Setting Legal Cases Adopting or Rejecting the Inevitable Disclosure Doctrine

State	Precedent-Setting Cases	Date	Decision
AR	*Southwestern Energy Co. v. Eickenhorst*, 955 F. Supp. 1078 (W.D. Ark. 1997)	3/18/1997	Adopt
CT	*Branson Ultrasonics Corp. v. Stratman*, 921 F. Supp. 909 (D. Conn. 1996)	2/28/1996	Adopt
DE	*E.I. duPont de Nemours & Co. v. American Potash & Chem. Corp.*, 200 A.2d 428 (Del. Ch. 1964)	5/5/1964	Adopt
FL	*Fountain v. Hudson Cush-N-Foam Corp.*, 122 So. 2d 232 (Fla. Dist. Ct. App. 1960)	7/11/1960	Adopt
FL	*Del Monte Fresh Produce Co. v. Dole Food Co. Inc.*, 148 F. Supp. 2d 1326 (S.D. Fla. 2001)	5/21/2001	Reject
GA	*Essex Group Inc. v. Southwire Co.*, 501 S.E.2d 501 (Ga. 1998)	6/29/1998	Adopt
IL	*Teradyne Inc. v. Clear Communications Corp.*, 707 F. Supp. 353 (N.D. 111. 1989)	2/9/1989	Adopt
IN	*Ackerman v. Kimball Int'l Inc.*, 652 N.E.2d 507 (Ind. 1995)	7/12/1995	Adopt
IA	*Uncle B's Bakery v. O'Rourke*, 920 F. Supp. 1405 (N.D. Iowa 1996)	4/1/1996	Adopt
KS	*Bradbury Co. v. Teissier-duCros*, 413 F. Supp. 2d 1203 (D. Kan. 2006)	2/2/2006	Adopt
MA	*Bard v. Intoccia*, 1994 U.S. Dist. LEXIS 15368 (D. Mass. 1994)	10/13/1994	Adopt
MI	*Allis-Chalmers Manuf. Co. v. Continental Aviation & Eng. Corp.*, 255 F. Supp. 645 (E.D. Mich. 1966)	2/17/1966	Adopt
MI	*CMI Int'l, Inc. v. Intermet Int'l Corp.*, 649 N.W.2d 808 (Mich. Ct. App. 2002)	4/30/2002	Reject
MN	*Surgidev Corp. v. Eye Technology Inc.*, 648 F. Supp. 661 (D. Minn. 1986)	10/10/1986	Adopt
MO	*H&R Block Eastern Tax Servs. Inc. v. Enchura*, 122 F. Supp. 2d 1067 (W.D. Mo. 2000)	11/2/2000	Adopt
NJ	*Nat'l Starch & Chem. Corp. v. Parker Chem. Corp.*, 530 A.2d 31 (N.J. Super. Ct. 1987)	4/27/1987	Adopt
NY	*Eastman Kodak Co. v. Powers Film Prod.*, 189 A.D. 556 (N.Y.A.D. 1919)	12/5/1919	Adopt
NC	*Travenol Laboratories Inc. v. Turner*, 228 S.E.2d 478 (N.C. Ct. App. 1976)	6/17/1976	Adopt
OH	*Procter & Gamble Co. v. Stoneham*, 747 N.E.2d 268 (Ohio Ct. App. 2000)	9/29/2000	Adopt
PA	*Air Products & Chemical Inc. v. Johnson*, 442 A.2d 1114 (Pa. Super. Ct. 1982)	2/19/1982	Adopt
TX	*Rugen v. Interactive Business Systems Inc.*, 864 S.W.2d 548 (Tex. App. 1993)	5/28/1993	Adopt
TX	*Cardinal Health Staffing Network Inc. v. Bowen*, 106 S.W.3d 230 (Tex. App. 2003)	4/3/2003	Reject
UT	*Novell Inc. v. Timpanogos Research Group Inc.*, 46 U.S.P.Q.2d 1197 (Utah D.C. 1998)	1/30/1998	Adopt
WA	*Solutec Corp. Inc. v. Agnew*, 88 Wash. App. 1067 (Wash. Ct. App. 1997)	12/30/1997	Adopt

This appendix is a replication of table 1 in Klasa et al. [2017].

APPENDIX C

Determinants of the IDD

	1	2
	IDD Adoption	*IDD Rejection*
Political Balance	1.869*	−1.712***
	(1.025)	(0.598)
Total Size of Listed Firms	−0.432*	−0.150
	(0.254)	(0.576)
Total Number of Listed Firms	−0.648	1.999**
	(0.946)	(0.875)
GDP	0.126**	−0.059
	(0.056)	(0.090)
Unemployment	0.279	0.847***
	(0.407)	(0.282)
State fixed	Y	N
Year fixed	Y	Y
N	238	140
Adjusted-R^2	0.352	0.494

This table reports results on the determinants of IDD adoption (rejection). The dependent variable in column 1 is an indicator variable that takes the value of 1 for a state-year that adopted IDD, and zero otherwise. The dependent variable in column 2 is an indicator variable that takes the value of 1 for a state-year that rejected IDD, and zero otherwise. *Political Imbalance* is defined as the fraction of Democratic Party members in a state's congress. *Total Size of Listed Firms* is defined as the natural log of the total assets of all listed companies headquartered in the state. *Total Number of Listed Firms* is defined as the natural log of (1+ the total number of all listed companies headquartered in the state). The sample period is 1977–2013. We report in parentheses standard errors that are clustered within state, and are robust to heteroskedasticity.

***, **, and * indicate significance at the 0.01, 0.05, and 0.10 levels, respectively, using two-tailed tests. See appendix A for all variable definitions.

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