Data breaches and corporate liquidity management

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Abstract

This paper investigates the effects of data breach disclosure laws and the subsequent disclosure of data breaches on the cash policies of corporations in the United States. Exploiting a series of natural experiments regarding staggered state-level data breach disclosure laws, we find that the passage of mandatory disclosure laws leads to an increase in cash holdings. Our finding suggests that mandatory data breach disclosure laws increase the risks related to data breaches. Further, we find firms that suffer data breaches adjust their financial policies by holding more cash as well as decreasing external finance and investment.

1 Introduction

Data are among a corporation's most valuable assets. Financial policies associated with data breaches are important, particularly in the wake of the increase in the frequency and severity of data breaches and the rising wave of cybersecurity regulations.[1] With billions of sensitive corporate and consumer data compromised and thousands of breaches disclosed each year, data breaches are a growing concern for corporate boards in the United States as they face mounting regulatory pressure to cope with cybersecurity risks. For example, Fuhrmans (2017) asserts, "Cyber threats have zoomed to the top of chief executives' worry lists for fear a data breach could cost them their jobs and take down their businesses." Moreover, the fallout from other data breaches, such as the attacks on Equifax, Home Depot, Yahoo, and Target, provide compelling evidence that the risks to data security matter at the highest corporate levels. Although cybersecurity regulations and data breaches have attracted huge media attention and are now key boardroom issues of concern to corporate executives, they have received little attention in the empirical finance literature.

In this paper, we contribute to the growing debate on cybersecurity risks and how firms can insulate themselves, at least partially, from the adverse effects of data breach risks. We explore several questions related to data breaches and financial outcomes and policies. For instance, do laws that mandate the disclosure of data breaches and compel corporations to account for their data insecurity affect corporate liquidity management? Do firms change their financial policies to reflect changes in risk exposure due to a successful cyberattack on them? Do firms handle the direct and indirect costs of data breaches by changing their financial policies? To address these questions, we exploit plausibly exogenous state-level variation in data breach disclosure laws and subsequently disclosed data breaches.

Before 2002, US firms were not obligated to disclose data breaches. During the period from 2002 to 2016, state-level disclosure laws mandating firms to publicly disclose data breach incidents were passed in almost all the US states. In 2002, California became the first state to pass a data breach disclosure law. Since the passage of the California law, the other 49 states passed similar laws. While the details of the disclosure laws vary across the states, their central theme is consistent: firms should publicly disclose data breaches. Since mandatory state-level

[1] For instance, the European Union's General Data Protection Regulation went into effect on May 25, 2018.

disclosure laws prevent firms from sweeping data breaches under the rug, the mandatory breach disclosure requirement transforms private information about the practices of firms into public information, causing firms to face higher risks and costs relating to publicly disclosed data breaches. Even though the likelihood of experiencing a data breach could remain unchanged, a firm must publicly disclose data breaches, which significantly increases its risks and costs. This institutional background offers a unique avenue through which we can examine the impact of mandatory data breach laws and disclosed breaches on corporate financial policies.

Exposure to and disclosure of a data security breach create direct and indirect costs. The disclosure of data breaches can potentially subject firms to regulatory investigations and fines, litigation, media scrutiny and reputation damage, customer loss, revenue decline, and increased cash flow risks and possibly threaten a firm's bottom line, as well as shareholder value, among other things. In short, mandatory data breach disclosure laws increase a firm's ex ante risk of incurring future costs related to disclosed breaches.

A corporate policy that could be particularly sensitive to the increased risks and costs of data breaches following the passage of a mandatory disclosure law is the decision to accumulate cash. Prior literature underlines the relation between risk and cash holdings. Froot and Stein (1998) model the risk management choices of firms in situations in which some risks are not perfectly hedgeable. Froot and Stein (1998) find that, when it is difficult to completely hedge a particular type of risk, firms resort to increasing their cash holdings. Similarly, if hedging is impossible or when firms face high hedging transaction costs for a particular type of risk, an alternative way to minimize risk exposure is to accumulate cash (Brav et al. (2005); Han and Qiu (2007); Riddick and Whited (2009)). Bates et al. (2009) highlight the importance of the precautionary motive in determining corporate cash holdings. Consistent with this argument, Ucar (2019) reports that firms located in areas with a more creative culture have higher levels of risk exposure and accumulate more cash, consistent with the precautionary motive. Acharya et al. (2012) examine the relationship between cash holdings and credit risk and find that riskier firms accumulate more cash. Chen et al. (2020) exploit a unique setting in China and show that firms with lower internal control quality are more likely to hold more cash. Additionally, Arena and Julio (2015) find that firms facing litigation risks significantly increase their holdings of cash. Romanosky et al. (2014) empirically investigate the impact of data breaches on litigation risk, and the outcomes of data breach litigation. They show that, between 2000 and 2011, the overall settlement rate of known federal lawsuits due to data breaches was around 50%, and defendants even settle 30% more often when plaintiffs allege financial loss from a data breach or when faced with a class action data breach suit.

We hypothesize that data breach risk exposure is a significant determinant of a firm's decision to accumulate cash. Following the mandatory data breach disclosure laws, we advance that firms can ex ante adjust cash policies to help hedge against the increasing risks and future costs related to data breaches. Thus, we expect that, following the passage of mandatory state-level data breach disclosure laws, all else being equal, a firm with higher exposure to risk from data breach disclosures will increase its cash holdings. Similarly, firms that are victims of successful data breaches increase their cash holdings in anticipation of settlement costs related to the disclosed data breach.

Firms can obtain insurance contracts to protect themselves from the direct costs of data breaches. However, in practice, cyber insurance does not cover consequential damages; hence, it is difficult (if not impossible) to completely hedge or insure the majority of the costs associated with a disclosed data breach incident (especially indirect or hard-to-quantify costs such as reputation damage, loss of value of intellectual property, or customer loss).[2] Importantly, no cyber insurance contract provides complete or full coverage prevention and/or incident response services. For instance, no cyber insurance policy provides coverage for reputational costs, which, according to the 2018 and 2017 IBM/Ponemon Institute data breach studies, constitutes the largest indirect per capita cost and the largest data breach cost category in the United States. In many cases, determining and obtaining the right policies can be challenging due to difficulties in understanding and assessing vulnerabilities to breaches. Therefore, practitioners, advance that an efficient way firms can manage the potential risks and costs of a data breach is to increase cash reserves. For instance, Deloitte's 2017 risk management, strategy, and analysis report argue that the creation of sufficient cash reserves improves a firm's ability to quickly recover and survive data breaches.

To identify the effect of data breach disclosure laws on firm cash holdings, we construct tests that exploit time-varying exogenous changes in state-level data breach disclosure laws. The

[2]Some intangible damages take months to emerge (varying across firms), which makes a standard process of evaluation difficult to create.

staggered timing of the passage of the state-level disclosure laws plausibly provides a natural experiment framework to empirically explore how these regulatory changes affect corporate cash policies. Similar to previous studies (Beck et al. (2010); Francis et al. (2014)), we use a difference-in-differences approach to examine how firms change their cash policies in response to changes in state-level data breach disclosure laws. Our baseline regression results indicate an economically important positive relation between state-level data breach disclosure laws and corporate cash holdings. First, the results are robust to the exclusion of the financial crisis period, to the exclusion of firms based in California, which account for 18% of observations in our sample, and to the re-estimation of our baseline specification in first differences.

Second, while the staggered timing of the passage of the disclosure laws provides exogenous changes to data security reporting and transparency, unobservable shocks coinciding with state-level data breach disclosure laws could be the primary cause of changes in cash holdings. If this is the case, then the changes in corporate cash holdings we attribute to state-level disclosure laws reflect mere association, rather than causality. Our strategy of using multiple shocks helps isolate the impact of disclosure laws on cash holdings from other confounding factors, thus mitigating omitted variable concerns. Additionally, the timing and passage of state-level disclosure laws are beyond the control of any individual firm; that is, firms have no influence or control over the years in which disclosure laws are passed. Even if the enactment of various state-level data breach disclosure laws is anticipated, firms could have started changing their financial policies prior to the laws' passage, which should disfavor us finding any impact of the changes in the state-level disclosure laws on our outcome of interest.

Nevertheless, we address this possibility by conducting placebo or falsification tests following an approach similar to that of Cornaggia et al. (2015). We randomly allocate the various states to the distribution of data breach disclosure law years. This preserves the correct distribution of the disclosure law years but randomly assigns the states to the disclosure law years. We expect our re-estimated results to be weaker if unobservable shocks do not exist in our testing framework; that is, falsely assumed disclosure law events should have no effect on cash holdings. Indeed, our randomized disclosure law estimation results show no effect on corporate cash holdings. The non-result in this test discounts an omitted variable bias.

After providing robust evidence of an aggregate increase in corporate cash holdings following the passage of various state-level data breach disclosure laws, we turn to the analysis of actual data breaches. We first examine the association between data breaches and certain observable firm characteristics. Our findings, suggestive and exploratory and not definitive determinants of data breaches, show that data breaches are more likely for large and old firms, financially constrained firms, and high-growth firms.

Next, we test the impact of exposure to and the disclosure of data breaches on changes in corporate cash holdings. We find that firms increase their cash holdings in response to a data breach incident. To test that the results are not driven by differences in characteristics between breached and non-breached firms, we follow Hainmueller and Xu (2013) and use the entropy balancing approach, and we still find a positive relation between data breaches and corporate cash holdings. Although we find a positive relation between data breaches and cash holdings, one could argue that the increase in cash balances following disclosure laws should be used up by breached firms in meeting the cash outlays associated with the breach incident. However, while firms build up their cash reserves after the enactment of the laws, it is possible breached firms build up cash because of an unusually bad breach incident. Thus, a firm's cash needs are plausibly unanticipated when an unusually bad breach occurs, and they react by increasing their cash holdings. We examine this possibility by running a test that relates the ex post cash build up to the severity of data breaches. As a proxy for the expected costs of unusually bad breach incidents, we use the stock market reaction to the breach announcement. Specifically, we use an event study methodology to estimate the cumulative abnormal returns (CARs) around the data breach announcement. We then rank the CARs into terciles and estimate three levels of data breach severity. The results show that firms with severe breach incidents accumulate more cash, suggesting that severely breached firms are those that face the need to build up more cash.

We examine the possible underlying mechanisms through which firms build up cash after a data breach incident. Breached firms can potentially build up their cash balances by increasing their use of external debt and equity financing. However, given that breached firms are probably viewed as a higher credit risk by financial markets, we expect that they would find it difficult to raise external financing, and, therefore, external financing activities should decline after the disclosure of a data breach incident. Indeed, we find that both external equity and debt financing activities decline after a data breach incident. Given the lack of increase in external financing after a data breach incident, a test of investment as another possible mechanism naturally follows; that is, we test whether breached firms forgo investment in order to build up cash after a data breach incident. We find an economically important reduction in investment in both capital expenditure and acquisitions for firms exposed to data breaches, suggesting that firms reduce their investments to accumulate cash after a data breach incident.

Our paper makes several key contributions to the corporate finance literature. It is closely related to a vast literature that studies corporate cash holdings (see important contributions from Almeida et al. (2004); Arena and Julio (2015); Bates et al. (2009); Froot and Stein (1998); Han and Qiu (2007)). Our finding of data breach disclosure laws and actual data breaches affecting cash holdings is consistent with the precautionary cash holdings literature. However, the key contribution of our paper is the identification afforded by the staggered state-level laws and how firms can insulate themselves from the adverse effects of data breaches through liquidity management. Equally important, our paper provides evidence indicating that mandating the disclosure of data breaches amplifies the adverse consequences associated with future data breaches, with important implications for corporate liquidity policies. This result is consistent with that of Chen et al. (2020), who investigate how the quality of internal controls shapes the cash holding policies of Chinese firms.

The paper also contributes to the growing literature on data breach disclosure laws and actual data breaches. It complements the stream of research that examines the effects of data breach disclosure laws (Ashraf and Sunder (2018); Romanosky et al. (2011)). It also contributes to the stream of studies that examine the negative consequences of data breaches (Garg (2019); Gatzlaff and McCullough (2010); Kamiya et al. (2020); Lin et al. (2019); Mikhed and Vogan (2018); Rosati et al. (2017);).

This paper also adds new evidence to the law and finance literature. Specifically, our paper complements previous studies (e.g., Falato and Sim (2014); He (2018); Klasa et al. (2018); Qiu and Wang (2018)) that examine the impact of regulatory changes on corporate financial policies. While most of the explanations for corporate cash holding policies focus on firm-specific factors, recent studies show that firms change their financial policies in response to regulatory changes. For instance, He (2018) exploits reforms of state covenants-not-to-compete laws to capture exogenous changes in barriers for talent competition and finds that firms increase cash balances when the competition for talent intensifies. Falato and Sim (2014) exploit the staggered state-level changes in research and development (R&D) tax credits to examine the impact of innovation on corporate cash holdings. They find that firms increase (decrease) cash balances in response to increase (cuts) in R&D tax credits. We advance that data breaches represent a key operational risk that financial managers need to hedge against.

The remainder of the paper is structured as follows. Section 2 provides background information on the state-level disclosure laws. Section 3 describes the data and methodology. Sections 4 and 5 discuss the empirical results. Section 6 concludes the paper.

2 Background

2.1 State-level data breach disclosure laws in the United States

In the United States, the requirement to disclose data breaches is mainly governed by state laws. Prior to 2002, US firms were not obligated to disclose data breaches. Motivated by the data breach incident at the Stephen P. Teale Data Center, which affected the personally identifiable information of all 265,000 state employees, in 2002 California became the first state in the United States to enact a data breach disclosure law. The California data breach notification law was the first of its kind and has since served as the model for many other state-level disclosure laws.

As of March 29, 2018, all 50 US states, the District of Columbia, and the US territories of the Virgin Islands, Puerto Rico, and Guam had enacted mandatory disclosure laws requiring business and non-business entities to disclose and notify affected parties of data security breaches.[3] Although details vary across states, the primary objective of all state-level data security breach disclosure laws is to obligate firms to publicly disclose data breaches involving non-public personally identifiable information, such as Social Security numbers (Romanosky et al. (2011)). Since the reasons for the changes in the state-level data breach disclosure laws were unrelated to the financing policies of firms, employing these legal reforms as exogenous shocks helps mitigate potential endogeneity concerns.

[3]See the various state disclosure laws from the National Conference of State Legislatures, at http://www.ncsl.org/research/telecommunications-and-information-technology/security-breach-notification-laws.aspx.

2.2 Related literature

Our work contributes to the growing literature on data breach disclosure laws and actual data breaches. A number of papers study the effects of mandatory data breach disclosure laws. For instance, Ashraf and Sunder (2018) examine the net effect of data breach disclosure laws on shareholder risk. They focus on explaining changes in the cost of equity following the passage of data breach laws and find that the cost of equity is significantly impacted by data breach laws, consistent with the idea that the increase in risk exposure prompts firms to react proactively to reduce exposure to data breach risks. Our paper complements this study by examining the link between mandatory data breach disclosure laws, data breaches, and corporate cash holdings.

Another stream of data breach research examines the negative consequences of data breaches. For instance, Lending et al. (2018) find that data breach firms have one-year buy-and-hold abnormal returns of -3.5%. Additionally, banks with breaches experience a significant decline in deposits, and non-banks experience significant declines in sales in the long run. Romanosky et al. (2014) empirically investigate the impact of data breaches on litigation risk and the outcomes of data breach litigation. The authors show that federal lawsuits due to data breaches result in a high settlement rate of about 50%. They further show that the number of records compromised is positively related to the probability of being sued, and victims that claim financial harm from the data breach are more likely to sue the firm. Kamiya et al. (2020) report that successful data breaches are associated with significant declines in shareholder value, a deterioration in credit ratings, increased cash flow volatility, adversely affect sales growth, and decreased investments in the short run and result in an increase in the risk assessment of target firms. Gatzlaff and McCullough (2010) find that data breach announcements have a significant negative effect on shareholder wealth. Rosati et al. (2017) find that data breach announcements have a significant positive effect on the bid–ask spread and trading volumes. Lin et al. (2019) find statistically significant evidence of opportunistic insider trading in the months prior to data breach announcements. Garg (2019) finds that the effects of a data breach are not isolated to the breached firm, but spill over to peer firms. Mikhed and Vogan (2018) examine the interaction of consumers with the credit market and their use of credit following data breaches. The authors find that the victims of data breaches increase the acquisition of fraud protection services, are more likely to freeze their credit files, are likely to opt out of credit offers, but do not significantly switch lenders.

3 Data and Methodology

3.1 Data Collection

To examine the effects of the staggered state-level data breach disclosure laws on cash holdings, we collect initial firm-level data from the merged Center for Research in Security Prices (CRSP)/Compustat database for the period 1997–2015. This period covers the majority of the years in which the states passed data breach disclosure laws. Our sample period begins five years before California passed the first state-level data breach disclosure law, in 2002, and ends five years after Mississippi passed a similar law, in 2010. Following prior literature (Bates et al. (2009); Opler et al. (1999)), we exclude all financial firms—that is, those with Standard Industrial Classification (SIC) codes 6000–6999— because their cash holdings include inventories of marketable securities and they are also required to meet statutory capital requirements. We exclude utility companies (SIC codes 4900–4999) because their cash holdings are possibly subject to regulatory supervision in some states. We further drop observations with negative or missing total book assets. This yields a final sample of 56,646 firm–year observations.

Next, to examine the effect of actual data breaches on corporate cash holdings, we obtain data on data breaches from a chronological listing of disclosed data breaches available from the Privacy Rights Clearinghouse (PRC) for the period 2005–2018.[4] PRC is a consumer advocacy not-for-profit organization that has been publishing all disclosed data breaches in the United States since 2005. The PRC data provides relevant disclosed data breach information. We collect information about the name of the breached firm, the breach disclosure date, a description of the breach incident, and, if available, the total number of records breached. We identify 329 nonfinancial business firms as having disclosed a data breach over the 2005–2018 sample period. We then manually merge the disclosed breaches with the CRSP/Compustat data.

[4]See https://www.privacyrights.org/.) PRC is the most cited source of information relating to data breaches (e.g., Kamiya et al. (2020); Romanosky et al. (2014)).

3.2 Estimation Technique

The staggered timing of the passage of the state-level disclosure laws enables us to use states that had not yet passed disclosure laws at a given time (including states that eventually enacted disclosure laws, as well as states that never enacted them within the period of study), to control for potential confounding effects. Therefore, we use a difference-in-differences approach to empirically explore the effects of the disclosure laws on cash holdings. Our model setup mirrors that of Francis et al. (2014). We estimate

$$Cash_{i,s,t} = \alpha + \beta \mathit{Disclosure\ Law(0/1)}_{s,t} + \gamma X_{i,s,t} + \theta_s + \delta_t + \rho_j + \nu_i + \varepsilon_{i,s,t}, \quad (1)$$

where i, s, and t index firm, state, and time, respectively. The dependent variable, *Cash*, is cash and marketable securities scaled by total book assets; $\mathit{Disclosure\ Law(0/1)}_{s,t}$ is a dummy variable that switches to one the year after the focal state passed the disclosure law; $X_{i,s,t}$ is a vector of controls; θ_s represents a set of state dummies that account for state-level unobservable factors that could be correlated with the data breach disclosure laws, and thus bias our estimates; δ_t represents year dummies to control for secular shocks in cash holdings coinciding with the passage of the disclosure laws; and ρ_j and ν_i capture industry and firm fixed effects, respectively.[5] The term $\varepsilon_{i,s,t}$ is a random error term. We cluster standard errors by state, because the treatment is defined at the state level. Alternatively, we follow Falato and Sim (2014) and re-estimate the baseline difference-in-differences specification using first differences. As explained earlier, state-level disclosure laws charge firms operating within the state (with data breach laws) with the responsibility of disclosing data breaches. Firms can operate in additional states besides their headquarters state and can thus be partly exposed to a data breach disclosure law prior to their home state passing a similar law. However, focusing on the states in which firms are headquartered is a conservative approach, since it essentially downward biases β in Eq.(1), which should result in an underestimation of our treatment effect. In other words, firms that are partly pre-exposed to a data breach disclosure law will have a weaker reaction when a similar law is passed in their home state.

[5]The industry dummies are constructed based on the 49-industry classification of Fama and French (1997).

In addition, to examine the impact of data breaches on cash holdings, we follow Garg (2019) and estimate

$$Cash_{i,t} = \alpha + \beta Breach(0/1)_t + \gamma X_{i,t} + \rho_j + \delta_t + \varepsilon_{i,t}, \tag{2}$$

where *Breach(0/1)* is a dummy variable set to one if a firm i discloses a data breach in time t; and zero otherwise. All other variables maintain their previous definitions, and robust standard errors are estimated by clustering at the firm level.

3.3 Variables

We use the most traditional measure of cash in the literature (Bates et al. (2009); Opler et al. (1999)) as our dependent variable. We measure *Cash* as cash and marketable securities scaled by total book assets.

As explained earlier, in addition to the dummies capturing disclosure laws, we construct variables to capture the impact of actual data breaches on cash holdings. The variable *Breach(0/1)* is a dummy that equals one for breached firms in the year of the data breach; and zero otherwise. We treat multiple breaches in a particular year as a single breach. The breaches are relatively evenly distributed over the sample period, reducing concerns that the data breaches are clustered in time.

We follow the literature (Bates et al. (2009); Opler et al. (1999)) in our empirical testing and control for several variables that affect firm cash policy. Specifically, we control for *Firm Size*, *Firm Age*, *Book Leverage*, *Market-to-book*, *Cash Flow*, *Capital Expenditure*, *Acquisition Expenditure*, *Dividend Paying Firms (0/1)*, *R&D Expenditure*, *Net Working Capital*, and *Industry Cash Flow Volatility*. The definitions of all the variables are detailed in the Appendix. We winsorize all variables at the 1st and 99th percentiles to minimize the influence of outliers. Table 1 provides summary statistics of the key variables used in this study. We report the mean, standard deviation, 25th percentile, median, and 75th percentile.

4 State-Level Disclosure Laws and Corporate Cash holdings

In this section, we test how firms' cash holdings change with the passage of state-level data breach disclosure laws.

4.1 Baseline Regression Results

Table 2 presents the baseline estimation results. We include state, year, industry, and firm fixed effects in various specifications. Column (1) includes controls for year, industry, and state fixed effects. In Column (2), we include year and firm fixed effects. California is one of the largest and important state economies in the United States, where some of the world's largest corporations (e.g., Apple, Google, and Facebook) are headquartered.[6] Therefore, in Column (3), we exclude California and re-estimate Eq.(1). Our aim is to isolate the effects of state-level data breach disclosure laws; therefore, in Column (4), we purposely exclude the financial crisis period (2007–2009) from the sample period. In Column (5), the standard errors are two-way clustered by state and year. Finally, in Column (6), we follow Falato and Sim (2014) and re-estimate our baseline regression using first differences. We thus control for firm fixed effects, as well as industry and state fixed effects.

Across all these specifications, the coefficient associated with *Disclosure Law(0/1)* is positive and statistically significant, which shows that, following the passage of state-level data breach disclosure laws, firms increased their cash holdings. We use the results in Column (1) of Table 2 to gauge the economic importance of state-level disclosure laws on corporate cash holdings. In Column (1), the coefficient associated with *Disclosure Law(0/1)* is 0.0076. All else being equal, an increase in cash holdings by 0.0076 corresponds to a 3.8% increase from mean cash holdings (0.2008) and 7.3% of median cash holdings (0.1044) for our sample firms.

A concern with the difference-in-differences design is that a change in firm cash holdings can precede a change in disclosure laws. We test for pre-existing trends by introducing a series of timing dummies in our baseline specification. The result from the timing effects estimation, which is reported in the Appendix for brevity, indicates that the baseline results are not driven by pre-existing trends. This finding implies that treated firms increase their cash holdings only after, and not prior to, changes in state-level data breach disclosure laws.

4.2 Falsification Test

Omitted variables coinciding with the timing of the passage of the state-level disclosure laws could drive the baseline results. If this is the case, then the changes in corporate cash holdings

[6] Firms headquartered in California account for 18% of the observations in our sample.

we attribute to state-level disclosure laws reflect mere association rather than causality. However, the disclosure laws were enacted at different times, providing multiple exogenous shocks, affecting the various states at different times. This enables us to overcome an obvious challenge facing single- shock studies, where the coincidence of a potential omitted variable and the single shock could affect the economic outcomes of interest. While the staggered nature of the enactment of the state-level disclosure laws makes it unlikely for an omitted variable to coincide with the disclosure laws, as explained earlier, we still address this concern by conducting a falsification test in a manner similar to that of Cornaggia et al. (2015).

We follow a two- step process. First, for each year, we randomly assign firms to the various states. Next, we randomly assign the states into the distribution of years when the various disclosure laws were passed. This helps maintain the actual distribution of years in which the various states passed the disclosure laws; however, it disrupts the correct assignment of the states to the years in which the laws were passed. Therefore, an unobservable shock that occurs at approximately the same time as the passage of the state-level data breach disclosure laws would still reside within the baseline testing framework and, hence, drive the cash results. In contrast, if no such unobservable shocks exist, then we expect that the incorrect assignments of the firms and states to the disclosure law years should weaken our results for the re-estimated baseline specification. Thus, the falsely assumed disclosure law events should have no effect on cash holdings. Indeed, we do not find statistically significant effects of the state-level disclosure laws on corporate cash holdings following the random assignment. As reported in Table 3, the coefficients on the *Disclosure Law(0/1)* variable in Columns (1) to (4) are all statistically nonsignificant and, hence, not different from zero. The non-results in this test discount an omitted variable bias.

4.3 Role of Financial Constraints

So far, our findings support the hypothesis that firms increase their cash holdings in response to the passage of data breach disclosure laws. We now examine the role of financial constraints in the effect of disclosure laws on corporate cash holdings. Specifically, following To et al. (2018) and Francis et al. (2014), we sort firms into financially constrained and unconstrained groups based on firm size, firm age, and dividend payout ratio.[7] For each year, we rank the firms over the sample period and categorize firms in the bottom terciles of the size, age, and dividend payout distributions as financially constrained. Note that the rankings are performed on an annual basis and this approach allows firms to migrate between groups in different years. We then create dummy variables for the three financial constraint measures. We create the dummy variables *Small Firms(0/1)*, *Young Firms(0/1)*, and *Non-dividend Payer(0/1)*, which we set to one for firms in the bottom of the size, age, and dividend payout distributions, respectively, and zero otherwise. We interact the various financial constraint measures with the *Disclosure Law(0/1)* dummy to examine the role of financial constraints in the effects of disclosure laws on the cash policy of firms. In Table 4, we include year and firm fixed effects in all specifications. Table 4 presents the findings for the role of financial constraints. In Columns (1) to (3), the variable of interest is the interaction of the financial constraint measure with the disclosure law dummy. The results in Columns (1) to (3) indicate that the positive effect of disclosure laws on corporate cash holdings is driven by financially constrained firms.

5 Effects of Actual Data Breaches

We now turn to the analysis of actual data beaches. We begin by first examining the association between data breaches and certain observable firm characteristics.

5.1 Association between Data Breaches and Observable Firm Characteristics

In this section, we examine the determinants of data breaches. Since past research provides no theoretical guidance regarding the determinants of data breaches, we consider the findings of this paper to be suggestive and exploratory, and not definitive determinants of data breaches. We report the results of the penalized maximum likelihood logistic regression in Table 5. The dependent variable, *Breach(0/1)*, is a dummy variable that equals one for breached firms in the year of the data breach, and zero otherwise. The observable firm characteristics include *Firm*

[7]Farre-Mensa and Ljungqvist (2016) argue that known measures of financial constraints do not actually measure financial constraints, since constrained firms have no trouble raising debt. However, their definition of constraint has been criticized as being too lenient , since, according to their definition, every time a firm raises debt or equity, we could say they are unconstrained, even though these firms could be passing up many very promising projects.

Size, *Firm Age*, *Market-to-book*, *Book Leverage*, *Capital Expenditure*, *Acquisition Expenditure*, *Dividend Paying Firms (0/1)*, *R&D Expenditure*, *Return on Assets*, and *Cash*.

The results show that the coefficients of *Firm Size* and *Firm Age* are positive and statistically significant at less than the 1% level, suggesting that data breaches are more likely in large and visible firms. This finding reflects the view that larger and older firms could possess greater amounts of relevant records, data, or proprietary information that could be attractive to data intruders. Additionally, the results show that data breaches are more likely at financially constrained firms (captured by firm dividend- paying status), profitable firms (measured by their return on assets), R&D- intensive firms, and firms with high growth potential. As Doyle et al. (2007) suggest, financially constrained firms might not have adequate resources (either money or time) to invest in effective data security systems, since that might not be their priority. In addition, the positive association between data breaches and growth opportunities suggests that growth can stress the effectiveness of a firm's internal control system; that is, a rapidly growing firm could outstrip its internal data security system and require more time, personnel, and processes to keep its internal data security systems up-to-date (Doyle et al. (2007)).

5.2 Data Breaches and Cash Holdings

In this section, we examine the impact of actual data breaches on corporate cash holdings. Table 6 presents the baseline estimation results. In Columns (1) and (2), we estimate Eq. (2) using contemporaneous and lagged breaches, respectively. Table 6 shows that the coefficient associated with $\textit{Breach(0/1)}_t$ in Column (1) is positive and statistically significant at less than the 1% level, revealing a positive relationship between data breaches and corporate cash holdings. This result is economically important; given the $\textit{Breach(0/1)}_t$ coefficient of 0.03, all else being equal, an increase in cash holdings of 0.03 corresponds to a 13.7% increase from mean cash holdings (0.2185). In Column (2), the coefficient on $\textit{Breach(0/1)}_{t-1}$ is positive and statistically significant at less than the 1% level. This result suggests breached firms continue to hold more cash in the year following the data breach.

While we find a positive relation between data breaches and cash holdings, one could still argue that the extra cash balance that was built up following the enactment of the disclosure laws should be used up by breached firms to meet the cash outlays associated with the breach incident. However, while firms build up their cash reserves after the enactment of the laws, it is possible that breached firms build up cash because of an unusually bad breach incident. Thus, the cash needs are plausibly unanticipated in the case of an unusually bad breach, and firms that are subject to such a breach increase their cash holdings.

We proxy for the severity of data breaches using the stock market's reaction to a breach announcement. We use an event study methodology to estimate the CARs around the data breach announcement. We calculate the CARs during the five-day event window around the disclosure of data breaches. We then sort the CARs into terciles and estimate three levels of data breach severity. The variable *Severe Breach(0/1)* proxies for the expected costs of an unusually bad breach, and it is a dummy set to one for a breach with CARs in the lowest tercile (highest mean negative returns), *Moderate Breach(0/1)* is set to one for a breach with CARs in the middle tercile, and *Low Breach(0/1)* is set to one for a breach with CARs in the highest tercile (least mean negative returns).

Columns (3) and (4) of Table 6 show the results of a test that relates the ex post cash buildup to the severity of the breach incident. The breach severity levels are contemporaneous in Column (3) but lagged in Column (4). The results in Column (3) show that the coefficient associated with *Severe Breach(0/1)*$_t$ is positive and statistically significant at less than the 5% level, revealing that firms with severe breach incidents accumulate more cash than firms with moderate- and low-severity breach incidents in the breach year. This result is economically important; given a coefficient of 0.0348 for *Severe Breach(0/1)*$_t$, all else being equal, an increase in cash holdings by 0.0348 corresponds to a 15.9% increase from mean cash holdings (0.2185). In Column (4), one year after the breach, the results again show that firms with severe breaches accumulate more cash.

Finally, to address the concern that breached firms are inherently different from non-breached firms, we implement the entropy balancing method of Hainmueller and Xu (2013). By using this matching procedure, we ensure that the treated firms (breached firms) are equivalent to the control firms (non-breached firms), which alleviates any concerns that differences in firm characteristics are influencing our results. The results for the entropy-balanced sample, available in the Appendix, confirm a positive relation between data breaches and corporate cash holdings.

5.3 Mechanisms

Our evidence so far suggests that firms increase their cash balances following exposure to a data breach incident. In this section, we explore possible underlying mechanisms through which firms build up cash. Specifically, we examine whether external financing and investment activities are possible underlying mechanisms through which breached firms accumulate cash.

5.3.1 Data Breaches and External Financing

Firms that experience data breaches could build up their cash balances by increasing their use of external debt and equity financing. However, given that breached firms are probably viewed as a higher credit risk by the financial markets and therefore face higher costs of capital, we expect that they would find it difficult to raise external financing, and, therefore, external financing activities should decline after the disclosure of a breach incident. To test this, we specify the following model:

$$\textit{External Finance}_{i,t} = \alpha + \beta \textit{Breach(0/1)}_t + \gamma X_{i,t} + \rho_j + \delta_t + \varepsilon_{i,t}, \quad (3)$$

Following Dierker et al. (2019), we use two measures of external financing: (1) *External Equity Financing*, estimated as the ratio of the difference between the sale of common and preferred stocks (SSTK) and the purchase of common and preferred stocks (PRSTKC) to total assets at the beginning of the year, and (2) *External Debt Financing*, estimated as the ratio of long-term debt issuance (DLTIS) minus long-term debt reduction (DLTR) to total assets at the beginning of the year. The term $X_{i,t}$ is a vector of controls. All other variables maintain their previous definitions, and robust standard errors are estimated by clustering at the firm level.

Table 7 reports the estimation results for the effects of data breaches on external financing activities. In Columns (1) and (3), we examine the effect of data breaches on external financing. The coefficients of *Breach(0/1)* for *External Equity Financing* and *External Debt Financing* are -0.014 and -0.0171, respectively, both statistically significant at the 1% level, as well as economically important. Given *Breach(0/1)* coefficients of -0.014 and -0.0171, respectively, for *External Equity Financing* and *External Debt Financing*, all else being equal, a decrease in *External Equity Financing* by 0.014 corresponds to a 32% decrease from mean *External Equity Financing* (0.0436), and a decrease in *External Debt Financing* by 0.0171 corresponds to a 62%

decrease from mean *External Debt Financing* (0.0275). In Columns (2) and (4), we examine the impact of data breach severity on the external financing activities of firms. Overall, the results show that external financing declines following a data breach incident, and firms are even less likely to raise external finance when they experience an unusually severe breach incident.

5.3.2 Data Breaches and Corporate Investment

Given the lack of increase in external financing after a data breach incident, a natural follow-up mechanism that requires testing is whether breached firms forgo investment to build up cash after a data breach incident. That is, do firms forgo investment to accumulate cash after a data breach incident? To examine this possibility, we follow Arena and Julio (2015) and estimate the investment regression

$$\mathit{Investment}_{i,t} = \alpha + \beta \mathit{Breach(0/1)}_t + \gamma X_{i,t} + \rho_j + \delta_t + \varepsilon_{i,t}, \tag{4}$$

where $\mathit{Investment}_{i,t}$ is investment in either *Capital Expenditure* or *Acquisition Expenditure*, with *Capital Expenditure* measured as the ratio of capital expenditure to total book assets at the beginning of the year and *Acquisition Expenditure* measured as the ratio of acquisitions to total book assets at the beginning of the year. The term $X_{i,t}$ is a vector of controls. All the other variables maintain their previous definitions, and robust standard errors are estimated by clustering at the firm level.

Columns (1) and (2) of Table 8 report the results for investment in *Capital Expenditure*, and Columns (3) and (4) present the results for investment in *Acquisition Expenditure*. In Columns (1) and (3), we examine, respectively, the effect of data breaches on capital expenditure and acquisition expenditure. The *Breach(0/1)* coefficients for *Capital Expenditure* and *Acquisition Expenditure* are -0.0072 and -0.0209, respectively. All else being equal, a decrease in *Capital Expenditure* by 0.0072 corresponds to an 11.8% decrease from mean *Capital Expenditure* (0.061), and a decrease in *Acquisition Expenditure* by 0.0209 corresponds to a 52% decrease from mean *Acquisition Expenditure* (0.0399). In Columns 2 and 4, we examine the impact of data breach severity on capital expenditure and acquisition expenditure, respectively. Overall, the results in Table 8 show that investment in both capital and acquisition expenditures decline following a data breach incident, especially when the breach is severe. This result is consistent with the view that firms forgo investment in capital expenditure and acquisitions to accumulate cash.

6 Conclusion

Prior to 2002, US firms were not obligated to disclose data breaches. From 2002 to 2016, state-level disclosure laws mandating firms to disclose data breaches were passed in almost all the US states. These disclosure laws impose "disclosure costs" and inadvertently compel firms to internalize the costs (including negative externalities, such as identity theft) of their data insecurity. We argue that, holding constant the underlying likelihood of experiencing a data breach, mandatory data breach disclosure laws increase the cash flow risk associated with future data breaches, and firms build up balance sheet liquidity as a precautionary measure. We provide such evidence by studying the effects of data breach disclosure laws and actual data breaches on the cash holding policies of US public firms.

Exploiting a series of natural experiments regarding staggered state-level data breach disclosure laws, we show that the passage of mandatory data breach disclosure laws leads to an increase in corporate cash holdings, particularly among financially constrained firms. Although we cannot entirely rule out the possibility that an omitted variable affected by data breach disclosure laws is driving firm cash holdings, the staggered nature of the state-level laws and the evidence from a falsification test make such a possibility unlikely. The finding is also robust to a dynamic effect estimation that addresses the parallel trends assumption. Additionally, we probe the impact of actual data breaches on corporate cash holdings and find that firms increase their cash balances following a data breach, as well as decrease their external financing and investment activities.

A Appendix

Table A.0.1: Variable definitions

This table provides the definitions of the key variables. The accounting data are from Compustat and the breach data are from the Privacy Rights Clearinghouse website (https://www.privacyrights.org).

Variable	Definition
Cash	Cash and marketable securities scaled by total book assets at the beginning of the year
External Debt Financing	Ratio of long-term debt issuance minus long-term debt reduction to total assets at the beginning of the year
External Equity Financing	Ratio of the difference between the sale of common and preferred stocks and the purchase of common and preferred stocks to total assets at the beginning of the year
Breach(0/1)	1 for breached firms in the year of the data breach, and 0 otherwise
Severe Breach(0/1)	1 for a breach with CARs in the lowest tercile (most negative returns)
Moderate Breach(0/1)	1 for a breach with CARs in the middle tercile
Low Breach(0/1)	1 for a breach with CARs in the highest tercile (least negative returns)
Disclosure Law(0/1)	1 for periods after the enactment of the state-level data breach notification laws, and 0 otherwise
Firm Age	Natural logarithm of the number of years a firm has been listed in the merged CRSP/Compustat database
Market-to-book	Ratio of total book assets less the book value of common equity plus the total market value of equity, all divided by total book assets
Firm Size	Natural logarithm of total book assets
Book Leverage	Ratio of total book debt (short-term debt plus long-term debt) to total book assets
Cash Flow	Ratio of earnings after interest, dividends, and taxes but before depreciation to book assets
Capital Expenditure	Ratio of capital expenditure to total book assets at the beginning of the year
Acquisition Expenditure	Ratio of acquisitions to total book assets at the beginning of the year
Dividend Paying Firms (0/1)	1 in the year a firm pays dividends, and 0 otherwise; set to zero if missing
R&D Expenditure	Ratio of R&D expenses to total book assets at the beginning of the year
Net Working Capital	Ratio of net working capital to net assets
Industry Cash Flow Volatility	Standard deviation of industry average cash flows for the previous 10 years; at least 3 years of observations required

Table A.0.2: Timing of data breach notification laws

This table presents the timing of the notification laws, by state, between 2002 and 2010. During this period, 46 states passed data breach notification laws. The distribution of years is by the year a state first enacted the disclosure law. Disclosure laws enactment years are available at https://www.perkinscoie.com/en/news-insights/security-breach-notification-chart.html.

2002	2005	2006	2007	2008	2009	2010
California	Arkansas	Arizona	Maryland	Alaska	Missouri	Mississippi
	Connecticut	Colorado	Massachusetts	Iowa		
	Delaware	Hawaii	Oregon	Oklahoma		
	Florida	Idaho	Texas	South Carolina		
	Georgia	Kansas	Wyoming	Virginia		
	Illinois	Michigan		West Virginia		
	Indiana	Montana				
	Louisiana	Nebraska				
	Maine	New Hampshire				
	Minnesota	Pennsylvania				
	Nevada	Utah				
	New Jersey	Vermont				
	New York	Wisconsin				
	North Carolina					
	North Dakota					
	Ohio					
	Rhode Island					
	Tennessee					
	Washington					

A.1 Bootstrap placebo estimation

While the non-results of the placebo test discount an omitted variable bias, it does not rule out the possibility of the spurious significance of the baseline results. To address this concern, we perform a random bootstrap placebo regression (by replicating the random placebo distribution 100 times) to obtain a random bootstrap placebo distribution of betas. We are thus able to obtain a distribution of placebo coefficients associated with *Disclosure Law(0/1)* from which we can assess whether *Disclosure Law(0/1)* in our baseline specification is spuriously significant. We expect to find a significantly positive coefficient for the bootstrap placebo distribution if temporal trends are driving the baseline estimation results. The bootstrap estimation results in Table A.1.1 rule out the possibility that the beta of interest in the baseline estimation is spuriously significant.

Table A.1.1: Bootstrap placebo estimation

This table reports the estimation results of the bootstrap -placebo distribution. To perform the random bootstrap placebo estimation, we replicate the random placebo distribution 100 times to obtain a distribution of placebo coefficients associated with *Disclosure Law(0/1)*. Again, the dependent variable in all the columns is *Cash*, measured as cash and marketable securities scaled by total book assets at the beginning of the year. Bootstrap standard errors are reported in parentheses, with 1%, 5%, and 10% levels of statistical significance denoted by ***, **, and *, respectively.

	Dependent Variable: *Cash*			
Variables	(1)	(2)	(3)	(4)
Disclosure Law(0/1)	0.0012	0.0008	0.0007	0.0007
	(0.0029)	(0.0029)	(0.0027)	(0.0018)
Firm Size	-0.0114***	-0.0109***	-0.0110***	-0.0105***
	(0.0007)	(0.0007)	(0.0007)	(0.0018)
Market-to-book	0.0057***	0.0094***	0.0079***	0.0021***
	(0.0015)	(0.0020)	(0.0017)	(0.0008)
Firm Age	-0.0232***	-0.0241***	-0.0212***	-0.0301***
	(0.0010)	(0.0012)	(0.0010)	(0.0014)
Book Leverage	-0.1467***	-0.1531***	-0.1399***	-0.0872***
	(0.0161)	(0.0164)	(0.0153)	(0.0103)
Cash Flow	-0.0052***	-0.0052**	-0.0049**	-0.0005
	(0.0018)	(0.0025)	(0.0023)	(0.0006)
Capital Expenditure	-0.0144	-0.1026***	-0.0706***	-0.0053
	(0.0356)	(0.0223)	(0.0194)	(0.0128)
Acquisition Expenditure	-0.0063	-0.0054	-0.0052	-0.0027
	(0.0389)	(0.0399)	(0.0388)	(0.0232)
Dividend Paying Firms (0/1)	-0.0152***	-0.0185***	-0.0073***	0.0093***
	(0.0022)	(0.0020)	(0.0020)	(0.0019)
R&D Expenditure	0.2145***	0.2838***	0.1909***	0.0192**
	(0.0229)	(0.0283)	(0.0213)	(0.0078)
Net Working Capital	-0.0002	-0.0002	-0.0001	-0.0175***
	(0.0112)	(0.0149)	(0.0134)	(0.0030)
Industry Cash Flow Volatility	0.0218***	0.0908***	0.0272***	-0.0023
	(0.0042)	(0.0033)	(0.0040)	(0.0039)
Year Fixed Effects	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	No	Yes	No
State Fixed Effects	No	Yes	Yes	No
Firm Fixed Effects	No	No	No	Yes
Observations	56,272	56,272	56,272	56,272
Adj. R^2	0.4619	0.4453	0.4948	0.0692

A.2 Timing effects

In Table A.2.1, we examine the timing effects of the data breach disclosure laws on corporate cash holdings. A concern with the difference-in-differences design is that a change in firm cash holdings could precede a change in disclosure laws. To address this concern, we follow He (2018) and introduce a series of timing dummies in our baseline specification. The variables *Disclosure Law(0/1)*$^{-2}$, *Disclosure Law(0/1)*$^{-1}$, *Disclosure Law(0/1)*^{0}, *Disclosure Law(0/1)*$^{+1}$, and *Disclosure Law(0/1)*$^{2+}$ take the value of one two years and one year prior to, the year of disclosure law, and one year and two or more years after the disclosure law's passage in the state of the firm's headquarters, respectively, and zero otherwise. Note that the timing dummy *Disclosure Law(0/1)*^{0} is measured differently from *Disclosure Law(0/1)*, as specified in Eq. (1). Here, the timing dummy *Disclosure Law(0/1)*^{0} is set to one in the year of the disclosure law's passage, whereas, in Eq. (1), *Disclosure Law(0/1)* is set to one the year after disclosure law is passed. If changes in cash holdings preceded the changes in disclosure law, then we expect the timing dummies prior to the disclosure law to be positive and statistically significant. In Table A.2.1, we include year, industry, state, and firm fixed effects in various specifications. The coefficients associated with *Disclosure Law(0/1)*$^{-2}$, *Disclosure Law(0/1)*$^{-1}$, and *Disclosure Law(0/1)*^{0} are all nonsignificant, indicating that treated firms increased cash holdings only after, and not prior to, changes in the state-level data breach disclosure laws.

Table A.2.1: Timing of changes in corporate cash holdings around disclosure laws

This table presents estimation results of the timing of the effect of changes in data breach disclosure laws on cash holdings. The dependent variable is *Cash* measured as cash and marketable securities scaled by beginning total book assets. *Disclosure Law(0/1)*$^{-2}$, *Disclosure Law(0/1)*$^{-1}$, *Disclosure Law(0/1)*^{0}, *Disclosure Law(0/1)*$^{+1}$ and *Disclosure Law(0/1)*$^{2+}$, takes the value of one two years prior to disclosure law, one year prior to disclosure law, year of the disclosure law, one year post disclosure law, and two years or more post disclosure law, respectively, and zero otherwise. Standard errors clustered by state are reported in parentheses with less than 1%, 5%, and 10% levels of statistical significance denoted by ***, **, and *, respectively.

	Dependent Variable: *Cash*		
Variables	(1)	(2)	(3)
Disclosure Law(0/1)$^{-2}$	0.0014	0.0015	0.0056
	(0.0028)	(0.0026)	(0.0031)
Disclosure Law(0/1)$^{-1}$	0.0060	0.0051	0.0062
	(0.0037)	(0.0034)	(0.0044)
Disclosure Law(0/1)^{0}	0.0082	0.0073	0.0069
	(0.0051)	(0.0047)	(0.0051)
Disclosure Law(0/1)$^{+1}$	0.0091*	0.0078*	0.0102*
	(0.0047)	(0.0046)	(0.0060)
Disclosure Law(0/1)$^{2+}$	0.0184***	0.0169***	0.0146**
	(0.0062)	(0.0057)	(0.0070)
Firm Size	-0.0109***	-0.0110***	-0.0095***
	(0.0027)	(0.0022)	(0.0019)
Market-to-book	0.0095***	0.0080***	0.0024***
	(0.0018)	(0.0015)	(0.0008)
Firm Age	-0.0242***	-0.0213***	-0.0319***
	(0.0038)	(0.0044)	(0.0050)
Book Leverage	-0.1534***	-0.1400***	-0.0850***
	(0.0220)	(0.0206)	(0.0116)
Cash Flow	-0.0053**	-0.0049***	-0.0005
	(0.0020)	(0.0018)	(0.0006)
Capital Expenditure	-0.1028***	-0.0708**	-0.0222
	(0.0366)	(0.0280)	(0.0135)
Acquisition Expenditure	-0.0054	-0.0052	-0.0023
	(0.0049)	(0.0048)	(0.0023)
Dividend Paying Firms (0/1)	-0.0182***	-0.0072**	0.0094***
	(0.0048)	(0.0028)	(0.0033)
R&D Expenditure	0.2838***	0.1908***	0.0203***
	(0.0377)	(0.0281)	(0.0072)
Net Working Capital	-0.0002	-0.0001	-0.0162***
	(0.0005)	(0.0004)	(0.0022)
Industry Cash Flow Volatility	0.0907***	0.0273***	-0.0031
	(0.0071)	(0.0068)	(0.0064)
Year Fixed Effects	Yes	Yes	Yes
Industry Fixed Effects	No	Yes	No
State Fixed Effects	Yes	Yes	No
Firm Fixed Effects	No	No	Yes
Observations	56,646	56,646	56,646
Adj. R^2	0.4456	0.4951	0.0692

A.3 Entropy balancing estimation

To address concerns that breached firms are inherently different from non-breached firms, we implement the entropy balancing of Hainmueller and Xu (2013). An advantage of this matching approach is that it endogenously determines a weighting among the covariates of the control and treatment groups, thereby avoiding the need to manually search for balance between the two groups, which is mostly an iterative process subject to tradeoffs that are arbitrarily decided upon in other matching methods, such as the propensity score matching. We match firms on three moments (i.e., mean, variance, and skewness) of all the control variables used in the baseline regression. By using this matching procedure, we ensure that the treated firms (breached firms) are equivalent to the control firms (non-breached firms), which alleviates concerns that differences in firm characteristics are influencing our results. The results for the entropy-balanced sample are presented in Table A.3.1. Across Columns (1) to (3), the results are statistically significant and positive coefficient estimates, revealing a positive relation between data breaches and corporate cash holdings.

Table A.3.1: Data breaches and cash holdings: Entropy-balanced sample

This table examines the effect of data breaches on cash holdings from the entropy-balanced sample. We match firms on three moments (i.e., mean, variance, and skewness) of all the control variables used in the baseline regression. The dependent variable in all columns is *Cash*, which is measured as cash and marketable securities scaled by total book assets at the beginning of the year. Linearized standard errors are shown in parentheses, with 1%, 5%, and 10% levels of statistical significance denoted by ***, **, and *, respectively.

	Dependent Variable: *Cash*		
Variables	(1)	(2)	(3)
Breach(0/1)	0.0209**	0.0184**	0.0145*
	(0.0082)	(0.0084)	(0.0087)
Firm Size	-0.0170***	-0.0167***	-0.0176***
	(0.0048)	(0.0047)	(0.0050)
Firm Age	-0.0116*	-0.0122**	-0.0059
	(0.0064)	(0.0062)	(0.0061)
Book Leverage	-0.0668**	-0.0626**	-0.0290
	(0.0277)	(0.0286)	(0.0231)
Market-to-book	0.0003***	0.0003***	0.0003***
	(0.0001)	(0.0001)	(0.0001)
Cash Flow	0.0262**	0.0254**	0.0102
	(0.0125)	(0.0122)	(0.0093)
Capital Expenditure	-0.5378***	-0.5137***	-0.2031*
	(0.1140)	(0.1129)	(0.1170)
Acquisition Expenditure	-0.3826***	-0.3896***	-0.4125***
	(0.0543)	(0.0532)	(0.0595)
Dividend Paying Firms (0/1)	-0.0399***	-0.0395***	-0.0234**
	(0.0115)	(0.0113)	(0.0110)
R&D Expenditure	0.2414**	0.2374**	0.1348*
	(0.1169)	(0.1161)	(0.0726)
Net Working Capital	-0.0040**	-0.0039**	-0.0019
	(0.0016)	(0.0016)	(0.0012)
Industry Cash Flow Volatility	0.0951***	0.1001***	0.0342
	(0.0195)	(0.0185)	(0.0355)
Year Fixed Effects	No	Yes	Yes
Industry Fixed Effects	No	No	Yes
Observations	41,177	41,177	41,177
Adj. R^2	0.2508	0.2631	0.3902

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Table 1: Summary statistics

This table reports the summary statistics of the key variables. Panel A reports the summary statistics for the state-level disclosure law sample over the period 1997 to 2015. Panel B covers the data breach subsample, where the sample period is from 2011 to 2016. See Table A.0.1 for the variable definitions.

Panel A: Disclosure law sample (1997-2015)						
Variables	Obs	Mean	SD	25th	Median	75th
Cash	56,646	0.2008	0.2231	0.0285	0.1044	0.2913
Firm Size	56,646	5.6136	2.0593	4.0819	5.5717	7.0455
Firm Age	56,646	7.3003	0.8889	2.0179	5.0435	11.0918
Market-to-book	56,646	2.1592	2.9919	1.1104	1.5113	2.3119
Cash Flow	56,646	-0.3758	4.8386	-0.0150	0.0684	0.1210
Book Leverage	56,646	0.2362	0.3177	0.0229	0.1863	0.3528
Capital Expenditure	56,646	0.0657	0.1248	0.0174	0.0364	0.0729
Acquisition Expenditure	56,646	0.0451	0.1672	0	0	0.0144
R&D Expenditure	56,646	0.0709	0.1895	0	0.0021	0.0738
Industry Cash Flow Volatility	56,646	0.3440	0.5270	0.0825	0.1403	0.2499
Net Working Capital	56,646	-0.0584	1.0372	-0.0589	0.0636	0.2157
Dividend Paying Firms (0/1)	56,646	0.2767	0.4473	0	0	1
Panel B: Data breach subsample (2005-2018)						
Breach(0/1)	42,893	0.0067	0.0776	0	0	0
Cash	42,893	0.2185	0.2398	0.0426	0.1251	0.3062
Firm Size	42,893	6.5181	2.1207	4.7692	6.3149	7.8124
Firm Age	42,893	7.3113	0.7520	1.8000	5.6335	10.1718
Market-to-book	42,893	2.0807	3.2412	0.5860	1.0410	1.9100
Cash Flow	42,893	-0.5429	5.9620	-0.0121	0.0716	0.1194
Book Leverage	42,893	0.2356	0.3177	0.0180	0.1895	0.3468
Capital Expenditure	42,893	0.0610	0.3175	0.0145	0.0322	0.0658
Acquisition Expenditure	42,893	0.0399	0.7749	0	0	0.0127
External Equity Financing	42,893	0.0436	0.2193	-0.0093	0	0.0077
External Debt Financing	42,893	0.0275	0.1297	-0.0129	0	0.0226
R&D Expenditure	42,893	0.0725	0.2369	0	0.0036	0.0674
Industry Cash Flow Volatility	42,893	0.4858	0.8114	0.0818	0.1507	0.2696
Net Working Capital	42,893	-0.1650	9.7913	-0.0793	0.0224	0.1637
Dividend Paying Firms (0/1)	42,893	0.3472	0.4761	0	0	1

Table 2: State-level disclosure laws and cash holdings

This table reports the estimation results of Eq. (1), which tests whether firms change their cash holdings in response to changes in state-level data breach disclosure laws. The dependent variable is *Cash* and the functional form is linear. The variable *Disclosure Law(0/1)* is a dummy that switches to one the year after the focal state passes the disclosure law. The definitions of the control variables are presented in Table A.0.1. In Columns (1) to (4), standard errors are clustered by state, but, in Column (5), the standard errors are two-way clustered by state and year. The specification in Column (6) is estimated using ordinary least squares in first differences to remove firm fixed effects in the levels equation and to control for year, industry, and state fixed effects. Heteroskedasticity-consistent standard errors are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	Dependent Variable: *Cash*					
	Baseline	Baseline w/firm FE	Baseline exc. California	Baseline exc. Fin. Crisis	State & Year clustered SE	First-difference Estimation
	(1)	(2)	(3)	(4)	(5)	(6)
Disclosure Law(0/1)	0.0076**	0.0056**	0.0032**	0.0078**	0.0076***	0.0026**
	(0.0031)	(0.0027)	(0.0042)	(0.0038)	(0.0028)	(0.0015)
Firm Size	-0.0110***	-0.0094***	-0.0132***	-0.0100***	-0.0110***	0.0179***
	(0.0022)	(0.0019)	(0.0014)	(0.0021)	(0.0009)	(0.0046)
Market-to-book	0.0080***	0.0023***	0.0079***	0.0071***	0.0080***	0.0005
	(0.0015)	(0.0008)	(0.0019)	(0.0014)	(0.0018)	(0.0004)
Firm Age	-0.0213***	-0.0320***	-0.0171***	-0.0217***	-0.0213***	-0.0031
	(0.0044)	(0.0051)	(0.0028)	(0.0045)	(0.0014)	(0.0027)
Book Leverage	-0.1400***	-0.0851***	-0.1283***	-0.1711***	-0.1400***	-0.0484***
	(0.0206)	(0.0116)	(0.0185)	(0.0210)	(0.0150)	(0.0098)
Cash Flow	-0.0049***	-0.0005	-0.0044**	-0.0004	-0.0049***	-0.0003
	(0.0018)	(0.0006)	(0.0018)	(0.0016)	(0.0012)	(0.0003)
Capital Expenditure	-0.0709**	-0.0221	-0.0537***	-0.0666**	-0.0709***	-0.0191***
	(0.0280)	(0.0135)	(0.0181)	(0.0285)	(0.0206)	(0.0066)
Acquisition Expenditure	-0.0052	-0.0023	-0.0041	-0.0045	-0.0052	-0.0038
	(0.0048)	(0.0023)	(0.0037)	(0.0041)	(0.0050)	(0.0038)
Dividend Paying Firms (0/1)	-0.0072**	0.0094***	-0.0066**	-0.0087***	-0.0072***	0.0027
	(0.0028)	(0.0033)	(0.0031)	(0.0028)	(0.0021)	(0.0017)
R&D Expenditure	0.1909***	0.0204***	0.1726***	0.1790***	0.1909***	0.0194***
	(0.0281)	(0.0072)	(0.0262)	(0.0288)	(0.0220)	(0.0042)
Net Working Capital	-0.0001	-0.0162***	-0.0001	-0.0344***	-0.0001	-0.0116***
	(0.0004)	(0.0022)	(0.0004)	(0.0039)	(0.0005)	(0.0014)
Industry Cash Flow Volatility	0.0273***	-0.0031	0.0207***	0.0238***	0.0273***	0.0005
	(0.0068)	(0.0064)	(0.0056)	(0.0067)	(0.0056)	(0.0046)
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	No	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	No	Yes	Yes	Yes	Yes
Observations	56,646	56,646	47,526	48,551	56,646	47,117
Adj. R^2	0.4939	0.0691	0.4658	0.5083	0.4287	0.1291

Table 3: Randomization of state-level data breach disclosure laws

This table reports the estimation results of Eq. (1) with randomized disclosure laws. We follow a two-step process in the randomization test. First, for each year, we randomly assign firms to the various states. Next, we randomly assign the states within the distribution of years in which the various disclosure laws were passed. We thus maintain the actual distribution of years in which the various states passed the disclosure laws; however, this approach disrupts the correct assignment of the states to the years in which the laws were passed. The dependent variable in all the columns is *Cash*, measured as cash and marketable securities scaled by total book assets at the beginning of the year. Standard errors clustered by state are reported in parentheses, with 1%, 5%, and 10% levels of statistical significance denoted by ***, **, and *, respectively.

	Dependent Variable: *Cash*			
Variables	(1)	(2)	(3)	(4)
Disclosure Law(0/1)	0.0008	0.0008	0.0007	0.0008
	(0.0024)	(0.0023)	(0.0023)	(0.0022)
Firm Size	-0.0104***	-0.0109***	-0.0110***	-0.0095***
	(0.0022)	(0.0027)	(0.0022)	(0.0020)
Market-to-book	0.0083***	0.0094***	0.0079***	0.0023***
	(0.0016)	(0.0018)	(0.0015)	(0.0008)
Firm Age	-0.0232***	-0.0241***	-0.0212***	-0.0319***
	(0.0050)	(0.0039)	(0.0044)	(0.0051)
Book Leverage	-0.1464***	-0.1531***	-0.1399***	-0.0849***
	(0.0220)	(0.0221)	(0.0207)	(0.0116)
Cash Flow	-0.0048**	-0.0052**	-0.0049***	-0.0005
	(0.0019)	(0.0020)	(0.0018)	(0.0006)
Capital Expenditure	-0.0747**	-0.1026***	-0.0706**	-0.0218
	(0.0293)	(0.0366)	(0.0280)	(0.0134)
Acquisition Expenditure	-0.0055	-0.0054	-0.0052	-0.0023
	(0.0050)	(0.0049)	(0.0048)	(0.0023)
Dividend Paying Firms (0/1)	-0.0139***	-0.0185***	-0.0073**	0.0092***
	(0.0035)	(0.0048)	(0.0028)	(0.0033)
R&D Expenditure to assets	0.2076***	0.2838***	0.1909***	0.0202***
	(0.0307)	(0.0377)	(0.0281)	(0.0072)
Net Working Capital	-0.0002	-0.0002	-0.0001	-0.0162***
	(0.0004)	(0.0005)	(0.0004)	(0.0022)
Industry Cash Flow Volatility	0.0273***	0.0908***	0.0272***	-0.0031
	(0.0068)	(0.0071)	(0.0068)	(0.0064)
Year Fixed Effects	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	No	Yes	No
State Fixed Effects	No	Yes	Yes	No
Firm Fixed Effects	No	No	No	Yes
Observations	56,272	56,272	56,272	56,272
Adj. R^2	0.4613	0.4446	0.4938	0.0688

Table 4: Disclosure laws and corporate cash holdings: Role of financial constraints

For each year, we rank the firms over the sample period and categorize those in the bottom terciles of the size, age, and dividend payout distributions as financially constrained. We then create dummy variables for the three financial constraint measures. We create the dummy variables *Small Firms(0/1)*, *Young Firms(0/1)*, and *Non-dividend Payer(0/1)* and set them to equal one for firms in the bottom of the size, age, and dividend payout distributions, and zero otherwise. Standard errors clustered by state are reported in parentheses, with 1%, 5%, and 10% levels of statistical significance denoted by ***, **, and *, respectively.

	Dependent Variable: *Cash*		
Variables	(1)	(2)	(3)
Disclosure Law(0/1)	-0.0219	-0.0052	0.0276***
	(0.0162)	(0.0099)	(0.0089)
Small Firms(0/1)×Disclosure Law(0/1)	0.0344**		
	(0.0153)		
Young Firms(0/1)×Disclosure Law(0/1)		0.0216*	
		(0.0128)	
Non-dividend Payer(0/1)×Disclosure Law(0/1)			0.0369***
			(0.0085)
Firm Size	0.0901***	0.0872***	0.0871***
	(0.0170)	(0.0159)	(0.0159)
Market-to-book	-0.0018	-0.0018	-0.0018
	(0.0040)	(0.0040)	(0.0040)
Firm Age	-0.0444***	-0.0440***	-0.0371**
	(0.0156)	(0.0147)	(0.0152)
Book Leverage	-0.1634***	-0.1644***	-0.1644***
	(0.0305)	(0.0305)	(0.0307)
Cash Flow	0.0022	0.0022	0.0022
	(0.0021)	(0.0021)	(0.0021)
Capital Expenditure	1.2590*	1.2606*	1.2594*
	(0.6937)	(0.6937)	(0.6937)
Acquisition Expenditure	-0.0613**	-0.0601**	-0.0597**
	(0.0299)	(0.0298)	(0.0297)
Dividend Paying Firms (0/1)	-0.0159**	-0.0166**	-0.0326***
	(0.0073)	(0.0071)	(0.0076)
R&D Expenditure	1.1079***	1.1076***	1.1076***
	(0.2035)	(0.2034)	(0.2033)
Net Working Capital	-0.0403*	-0.0403*	-0.0403*
	(0.0203)	(0.0203)	(0.0203)
Industry Cash Flow Volatility	-0.0003	0.0035	0.0067
	(0.0227)	(0.0235)	(0.0228)
Year Fixed Effects	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes
Observations	56,645	56,645	56,645
Adj. R^2	0.1528	0.1526	0.1528

Table 5: Association between data breaches and observable firm characteristics

This table presents the results for the association between data breaches and firm characteristics, using a penalized maximum likelihood logistic regression. The dependent variable is *Breach(0/1)*. Column (1) is estimated with no industry fixed effects, but Column (2) includes industry fixed effects. All the variables are defined in Table A.0.1. The sample period is from 2005 through 2018. The 1%, 5%, 10% levels of statistical significance are denoted by ***, **, and *, respectively.

	Dependent Variable: *Breach(0/1)*	
Variables	(1)	(2)
Firm Size	0.6779***	0.7089***
	(0.0442)	(0.0471)
Firm Age	0.3667***	0.4313***
	(0.0821)	(0.0858)
Market-to-book	0.0031***	0.0033***
	(0.0006)	(0.0006)
Book Leverage	0.5755***	0.5942***
	(0.0978)	(0.1736)
Capital Expenditure	0.0884***	0.1023***
	(0.0275)	(0.0276)
Acquisition Expenditure	0.03188***	0.0319***
	(0.0104)	(0.0109)
Dividend Paying Firms (0/1)	-0.3534***	-0.4123**
	(0.1418)	(0.2041)
R&D Expenditure	0.2686***	0.3431***
	(0.0722)	(0.0705)
Return on Assets	0.6619***	0.7081***
	(0.1380)	(0.1604)
Cash	1.1802	1.1845
	(0.4031)	(0.4891)
Observations	42,893	42,893

Table 6: Data breaches and cash holdings

This table reports the estimation results for the impact of data breaches on cash holdings. In Columns (1) and (2), we estimate Eq.(2), the effects of data breaches on cash holdings. In Columns (3) and (4), we estimate the effects on cash holdings of data breach severity. The variable *Breach(0/1)* is set to one for breached firms in the year of the data breach, and zero otherwise; *Severe Breach(0/1)* proxies for the expected costs of an unusually severe breach and is a dummy set to one for a breach with CARs in the lowest tercile (most negative returns); *Moderate Breach(0/1)* is set to one for a breach with CARs in the middle tercile; and *Low Breach(0/1)* is set to one for a breach with CARs in the highest tercile (least negative returns). Standard errors clustered by firm are shown in parentheses, with 1%, 5%, and 10% levels of statistical significance denoted by ***, **, and *, respectively.

	Dependent Variable: *Cash*			
Variables	(1)	(2)	(3)	(4)
Breach(0/1)$_t$	0.0299***			
	(0.0101)			
Breach(0/1)$_{t-1}$		0.0282***		
		(0.0104)		
Severe Breach(0/1)$_t$			0.0348**	
			(0.0151)	
Moderate Breach(0/1)$_t$			0.0285*	
			(0.0163)	
Low Breach(0/1)$_t$			0.0224	
			(0.0170)	
Severe Breach(0/1)$_{t-1}$				0.0379**
				(0.0187)
Moderate Breach(0/1)$_{t-1}$				0.0230
				(0.0165)
Low Breach(0/1)$_{t-1}$				0.0198
				(0.0151)
Controls	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes	Yes
Observations	42,893	42,893	42,878	42,866
Adj. R^2	0.4981	0.4981	0.4982	0.4982

Table 7: Data breaches and external financing

This table reports the estimation results for data breaches and external financing. The dependent variable in Columns (1) and (2) is external equity financing, which is estimated as the difference between the sale of common and preferred stocks and the purchase of common and preferred stocks, scaled by total assets at the beginning of the year. The dependent variable in Columns (3) and (4) is external debt financing, which is measured as the ratio of long-term debt issuance minus long-term debt reduction to total assets at the beginning of the year. The variable *Breach(0/1)* is set to one for breached firms in the year of the data breach, and zero otherwise; *Severe Breach(0/1)* proxies for the expected costs of an unusually bad breach and is a dummy set to one for a breach with CARs in the lowest tercile (most negative returns); *Moderate Breach(0/1)* is set to one for a breach with CARs in the middle tercile; and *Low Breach(0/1)* is set to one for a breach with CARs in the highest tercile (least negative returns). Standard errors clustered by firm are shown in parentheses, with 1%, 5%, and 10% levels of statistical significance denoted by ***, **, and *, respectively.

	Dependent Variable: *External financing*			
	External Equity Financing		External Debt Financing	
Variables	(1)	(2)	(3)	(4)
Breach(0/1)	-0.0140***		-0.0171***	
	(0.0051)		(0.0048)	
Severe Breach(0/1)		-0.0228***		-0.0232***
		(0.0082)		(0.0076)
Moderate Breach(0/1)		-0.0132		-0.0179**
		(0.0081)		(0.0068)
Low Breach(0/1)		-0.0056		-0.0142*
		(0.0077)		(0.0098)
Controls	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes	Yes
Observations	42,893	42,893	42,893	42,893
Adj. R^2	0.1270	0.3083	0.2550	0.2330

Table 8: Data breaches and corporate investment

This table reports the estimation results for the impact of data breaches on corporate investment. In Columns (1) and (2), the dependent variable is *Capital Expenditure*, the ratio of capital expenditure to total book assets at the beginning of the year. In Columns (3) and (4), the dependent variable is *Acquisition Expenditure*, defined as the ratio of acquisitions to total book assets at the beginning of the year. The variable *Breach(0/1)* is set to one for breached firms in the year of the data breach, and zero otherwise; *Severe Breach(0/1)* proxies for the expected costs of an unusually bad breach and is a dummy set to one for a breach with CARs in the lowest tercile (most negative returns); *Moderate Breach(0/1)* is set to one for a breach with CARs in the middle tercile; and *Low Breach(0/1)* is set to one for a breach with CARs in the highest tercile (least negative returns). Standard errors clustered by firm are shown in parentheses, with 1%, 5%, and 10% levels of statistical significance denoted by ***, **, and *, respectively.

	Capital Expenditure		Acquisitions	
Variables	(1)	(2)	(3)	(4)
Breach(0/1)	-0.0072**		-0.0209***	
	(0.0031)		(0.0040)	
Severe Breach(0/1)		-0.0092**		-0.0219***
		(0.0043)		(0.0056)
Moderate Breach(0/1)		-0.0074*		-0.0211***
		(0.0044)		(0.0062)
Low Breach(0/1)		-0.0068		-0.0136*
		(0.0053)		(0.0082)
Controls	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes	Yes
Observations	42,893	42,893	42,893	42,893
Adj. R^2	0.0237	0.0237	0.0221	0.0221