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Essays on Disclosure Regulations

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In the series of papers, I attempt to investigate the economic consequences of disclosure regulations and I am interested in exploring the novel context of risk disclosure beyond traditional financial reporting, such as cybersecurity, flood risk, consumer protection, and global adoption of integrated reports and environmental, social and governance (ESG) executive compensation metrics. My first paper focuses on positive spillover effects of disclosure policy in the U.S. flood risk disclosure setting, my second paper focuses on negative unintended consequences of disclosure policy in the U.S. data breach disclosure setting, and my third paper investigates the interplay between external disclosure policy and internal incentives in the international integrated reporting setting.

Aggregate Spillover Effects of Mandatory Transparency: Evidence from Flood Risk Disclosure in the U.S.

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An online appendix is available at the end of the manuscript.

Abstract

This paper provides novel evidence that mandatory transparency has aggregate spillover effects on the local economy. Using the staggered adoption of state level flood risk disclosure laws in the U.S., I empirically examine whether mandatory flood risk disclosure affects aggregate home mortgage lending. I find that aggregate census tract level mortgage loan value, loan value growth rates and loan-to-value ratio are significantly greater after states require sellers to disclose flood risk information in real estate transactions. These positive spillover effects only show up for properties with low flood risk, and are significantly greater relative to those with high flood risk *ex ante*. The spillover effects are more concentrated in states with more stringent legislation. Moreover, such legislation leads to an increase in collateral values linked to mortgage loans. A contiguous border-county approach confirms my results. Collectively, my findings suggest that mandatory transparency has welfare effects on the local economy on aggregate through a reduction in credit risk. Policymakers should consider the spillover from regulated to unregulated industries when drafting regulations.

Keywords: Disclosure, Regulation, Policy Spillover, Credit Market

JEL Classification Codes: G21, K25, M41, M48, Q54, Q58, R31

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1. Introduction

Does mandatory transparency policy have aggregate spillover effects on the local economy? Mandatory disclosure serves as a public policy instrument to encourage or discourage certain behaviours and many accounting studies document the direct effects[1] of corporate disclosure regulations on firm level outcomes (Christensen, Floyd, Liu, & Maffett, 2017; Leuz & Wysocki, 2016; Rauter, 2017). However, little is known about *aggregate* spillover effects of reporting regulations, especially real resource allocation across different industries (Leuz & Wysocki, 2016). Spillover in this study is defined as information that spreads from a regulated industry (where new disclosure is mandatory) to an unregulated industry.

The lack of prior research on aggregate spillover effects is particularly noteworthy because pure redistribution effects among agents within a regulated industry should not be a concern for policymakers (Hirshleifer, 1971) and information spillover from a regulated to an unregulated industry constitutes a prime justification for regulation (Admati & Pfleiderer, 1988; Breuer, 2017; Breuer, Leuz, & Vanhaverbeke, 2019; Dye, 1990). Second, determining the magnitude of aggregate effects is a first step towards welfare analysis for disclosure and reporting regulation (Leuz & Wysocki, 2016).

To test my research question, staggered mandatory flood risk disclosure across different states in the U.S. provides a relevant setting and I empirically examine whether mandatory transparency of flood risk has aggregate spillover effects on the mortgage industry. First, the value of complete and credible information is particularly relevant in the flood risk setting. Prior studies document that the market is inefficient in fully pricing information about flood risk on aggregate due to a combination of a lack of information and misinformation (Hino & Burke,

[1] I define direct effects in this study as the influence of mandatory laws on the disclosing firms' behaviors or those of their peer firms within the regulated industry (i.e., the industry where the new disclosure is mandated).

2020; Rachel Cleetus, 2019). Similarly, Bakkensen's (2018) analytical model highlights how the absence of accurate flood risk information can threaten the efficiency of coastal housing markets and can create additional welfare costs through mortgage processes. Second, mandated flood information is particularly comprehensible to consumers in the flood risk setting. The key to designing an effective mandatory transparency system is to ensure that information is easy to understand and embedded into consumers' and disclosers' decision-making (Fung et al., 2007). The rating systems on a standard disclosure form satisfy this requirement. Third, flood risk is considered an increasingly important type of climate risk by the scientific community and is becoming a first-order concern for millions of Americans. In terms of economic impact, a 1.8 meters sea level rise would inundate areas currently home to six million Americans (Hauer, Evans, & Mishra, 2016) and nearly one trillion dollars of coastal residential real estate is at risk. Fourth, the enforcement system in the flood risk disclosure setting assures a governance mechanism for better transparency to protect consumers. Failure to comply makes sellers liable to consumers for up to three times the cost of actual damages and a court may impose consumers' costs and legal fees on sellers.

I develop hypotheses for both the average and heterogeneous effects of flood risk disclosure. First, in terms of average effects, I hypothesize that flood risk disclosure affects mortgage lending by reducing uncertainty of the value of housing collateral. For lenders, uncertainty about collateral value is a primary source of credit risk in mortgage lending (Avery et al., 1996; Harrison & Seiler, 2015; Jokivuolle & Peura, 2003; Sinha, 2019). Flood risk significantly affects the value of house collaterals (Bernstein et al., 2019). However, due to the unawareness of the public (NRDC, 2019), high acquisition costs[2] (Kusisto 2019), low quality[3]

[2] The data set is dauntingly large and does not allow users to search by address and it is time-consuming to submit a written request to FEMA, gather data and then analyze risk information (Kusisto 2019).
[3] The maps are backwards-looking and often out of date, with 1 in 6 maps being over 20 years old (Bakkensen, 2018) and they do not take into account future sea-level rises risk (Kusisto, 2019).

(Bakkensen, 2018; Kusisto 2019) and legal constraints[4] (The Post and Courier, 2018), the flood risk information is incomplete and incredible under a voluntary disclosure regime. Mandatory disclosure of flood risk on a standard disclosure form for consumers in home sales leaves sellers no choice about whether and how to disclose. Failure to provide correct information, or supplying misinformation, leads to significant monetary penalties. Therefore, a form that provides readily available, high quality information reduces the information acquisition costs of accurately understanding the flood risk of traded properties and consequently mitigates uncertainty over the collateral value of such properties. This reduction in uncertainty helps mortgage lenders make better lending decisions. Regardless of the content of other disclosure, flood risk disclosure generally results in an increase in mortgage lending.

However, there are several reasons to believe that whether and how mortgage lenders respond to the mandates of flood risk disclosures is *ex ante* unclear. Firstly, selection behaviors and substitution effects of disclosure regulation might create net social losses (Dranove et al., 2003 and Christensen et al., 2020). Similar to the healthcare industry, the real estate market has differentiated pricing structures and complex multiple participants (sellers, buyers, appraisers, agents, lenders, insurance companies and other intermediaries), ex-ante disclosure in the industry may not be directly relevant. It is possible that the mandates of the regulation incentivise lenders opportunistically to select and redistribute resources to "lemons" (high flood risk) or just insure the risk through insurance companies. Given the costs of reporting the information, on net, this leads to the same or even higher levels of resources used and the same or even lower social welfares.

Secondly, the real estate market distinguishes from the theoretical market in which asset prices reflect all relevant information. Real estate transaction costs are high and assets are rarely

[4] Under the Privacy Act of 1974, FEMA cannot reveal a property's flood insurance claim history to anyone other than the homeowner, and the homeowner has to request it in writing (The Post and Courier, 2018).

perfect substitutes for one another. The impact of the disclosure regulation significantly depends on investors' (buyers') sophistication and costs of acquiring them. Lenders with greater resources will be better able to gather the information even before the mandates and thus benefit much less from the regulation than lenders with smaller resources. The net benefits for the society is *ex ante* unclear.

Thirdly, the arguments above assume that all lenders use mandatory disclosure forms and share homogeneous rational expectations. Because of bounded rationality, even in the presence of relevant information, individuals are prone to a host of cognitive distortions that may lead them to make decisions far different from those predicted in a world of perfect rationality (Fung et al., 2007). Similarly, Bakkensen et al.'s (2017) analytical model shows that agents hold heterogeneous beliefs about flood risk information and hence the housing market implications may be starkly different. If lenders do not use the standard forms or share heterogeneous beliefs and responses to flood risk information, the flood risk disclosure may not have any effect on mortgage lenders' decision-making processes.

Fourthly, the aforementioned tests do not distinguish types of properties within a state in the sense that all properties are assumed to be identically affected in terms of mortgage loans following mandates of flood risk disclosure regulation. Although in a poorer information environment, creditors can probably distinguish which borrowers have lower credit risk properties, and it is possible that mandatory disclosure regulation enables them to better identify which borrowers are more deserving. I hypothesize that average effects are likely to be conditional on properties' flood risk exposures (Kothari, Li, & Short, 2009). Creditors will probably respond to regulation by increasing lending to properties with low flood risk, since lenders' assessments of property values increase as credit risk decreases. This can occur either by injecting more lending into the industry on aggregate or by redistributing resource allocation from "lemons" (high flood risk) to "peaches" (low flood risk). Hence, I predict that following

mandatory flow risk disclosure regulation, mortgage lending will increase for properties whose geographic locations have low flood risk, and will either decrease or remain constant for those identified as having high flood risk.

I obtain mortgage loan information data through the Home Mortgage Disclosure Act (HMDA) data set from 1990 to 2013, which comprises around 500 million observations. Census tract level residential real estate prices come from the Office of Federal Housing Enterprise Oversight. I collect census tract level demographic attributes from the U.S. Census Bureau. I use the National Oceanic and Atmospheric Administration's (NOAA's) sea level rise calculator to identify each mortgage property's exposure to flood risk (Bernstein et al., 2019). I focus on how mandatory flood risk disclosure affects three measures of mortgage lending activity: loan value, loan value growth rate and loan-to-value ratio.

First, importantly for my empirical analysis, the timing of the adoption of the disclosure laws was approximately randomly assigned to states and thus additional state-level factors do not affect the decisions to regulation. I find that state-level economic, political and fiscal factors do not predict the implementation of flood risk disclosure legislation, which suggests that the timing of a legislation appears to be unrelated to relevant local economic conditions. To alleviate the concerns that the timing of mandates correlates also with states' flood risk ex ante, I firstly include state-level flood risk as an additional predictor in the predictive regression analyses. The untabulated results show that the states' flood risk does not predict the timing of mandates neither. Secondly, I use contiguous border-county analyses, which include only counties that located with different border-county pair, presumably share similar characteristics, the results still hold.

Second, consistent with my prediction, I find that the mandates of flood risk disclosure law lead to significant increases in mortgage loan value, loan value growth and loan-to-value

ratio at the aggregate census tract level. In terms of economic magnitude, my difference-in-difference (DID) estimators suggest that mandatory flood risk disclosure, on average, increases loan value by 51 thousands of dollars and 12 percentage points, loan value growth rate by 0.13 and loan to value ratio by 0.15. My results are robust to controlling for census tract and year fixed effects in all specifications. I verify that the parallel trend assumption holds in my DID design. Specifically, I find that there are no significant differences in mortgage loan value, loan value growth or loan-to-value ratio for my sample in the pre-treatment period. The effects of states' flood risk disclosure laws occur after those laws come into force but not before. Accordingly, the evidence is consistent with my hypothesis that disclosure leads to a decrease in uncertainty over collateral value in the mortgage industry, thus influencing mortgage lending activity.

To validate my results, I explore cross-sectional variations based on a property's flood risk ex ante and legal regime intensity. I segregate coastal properties into those with high sea level rise risk and those with low risk. My findings indicate that the impact of flood risk disclosure laws on home mortgage loans is only concentrated on property census tracts with low sea level rise risk, but not those with high sea level rise risk. The effects are significantly larger in low risk census tracts relative to high risk census tracts. Second, 29 states have mandated flood risk disclosure laws but with some key differences. In particular, state laws may vary in the items required for disclosure. I create indexes that rank these laws based on their stringency, and find that the documented positive spillover effects of home mortgage lending occur more for states that enacted laws with a relatively strong design.

Next, I explore one of the potential channels through which the disclosure laws affect creditors' lending decisions: collateral value. First, I assess the impact of flood risk disclosure laws on house prices. My results indicate that the mandates of flood risk disclosure regulation result in a shift in the level of sale prices where increases are significantly greater in low risk

census tracts, relative to high risk census tracts. Second, I find that increases in mortgage loans only arise for those properties experiencing high increases in collateral value. Third, removing observations experiencing high increases in house collateral values in the distribution results in the disappearance of loan effects.

To confirm the information channel, it would be expected that the impact of the disclosure regulation significantly depends on investors' (buyers') sophistication and costs of acquiring them. In other words, the lenders with higher expertise and more local knowledge would benefit less from the mandates of the disclosure regulation. Therefore, we would expect the effects would be weaker for lenders with larger size. Indeed, untabulated results confirm the predictions.

Finally, to alleviate the concern that my results are driven by unobservable local factors happening in the same state-year as my treatment, I conduct a further analysis in which I construct a sample of contiguous counties located on different sides of a state-pair border (Dube et al., 2010). My DID estimators based on the tracts-level mortgage within pairs of two adjacent counties on opposite sides of the state borders confirm my main findings.

The study has both academic and policy implications. First, I document *aggregate* spillover effects of mandatory transparency policy. The literature on mandatory disclosure[5] suggests that both financial reporting regulations and non-financial disclosure regulations affect the behaviours of disclosing firms by lowering their adverse selection and moral hazard cost.[6] Recent lines in the literature examine the influence of disclosure of one firm on their peers within the same industry and industrial spillovers at the transaction level (Aobdia & Cheng, 2018; Breuer, 2017; Breuer et al., 2019; Sinha, 2018). However, prior studies have scarce

[5] See Roychowdhury, Shroff, & Verdi (2019) and Leuz & Wysocki (2016).
[6] See Cho (2015); Graham et al. (2011); Granja (2018); Kraft et al. (2018); Leuz et al. (2008); Shroff (2017); Christensen et al. (2017); Rauter (2017).

evidence on the *aggregate* spillovers of disclosure regulation from regulated to unregulated industries, a key justification for policy implementations. My study speaks to the last stream of disclosure literature by examining how mandatory transparency policy generates aggregate spillover effects on the local economy (i.e., the mortgage industry). As such, this paper responds to the call for research by Leuz & Wysocki (2016) and Roychowdhury et al. (2019) on the market-wide or aggregate effects of disclosure regulations and provides initial evidence of a welfare analysis of mandatory transparency policy.

Second, from a policymaker's point of view, it is important to highlight that mandatory transparency can motivate market participants to introduce real resources to enlarge the economy. A perfectly functioning housing market needs complete information on external effects such as industrial hazards (Sinha, 2018) and flood risk. The created incentives should not simply motivate a redistribution from "lemons" to "peaches" but work toward introducing new resources to enlarge the economy as a whole. However, the exposure of flood risk in real estate is not self-evident, and consequently, proper market transparency requires correcting this information gap. Flood risk disclosure laws constitute a policy instrument aimed at filling information gaps. While Bakkensen (2018) analytically quantifies the value of high-quality flood risk information and the welfare costs of mispricing in mortgage origination, empirical evidence on the aggregate effects is limited. My study exploits cross-sectional variations in flood risk from different regions to quantify the value of accurate flood risk information.

Third, I explore the novel context of risk disclosure outside the traditional setting where corporations report to financial markets. Mandatory corporate financial reporting is only one component of the larger targeted transparency policy family, and mandatory disclosure existed in the U.S. long before mandatory corporate reporting (e.g., the 1906 Pure Food and Drug Act). Economic papers focus on the impact of mandatory disclosures on product quality and consumer choices (Jin & Leslie, 2003; Mathios, 2000). I explore the novel transparency policy

on consumer protection. The findings in the flood risk disclosure setting that go beyond corporate reporting also have potential implications for the corporate sector. For example, the positive spillovers of transparency between sellers and consumers may also apply to mergers and acquisitions (M&A) transactions between target companies and buyer companies. Any disclosures within an M&A transaction could possibly serve as an alternative information channel for multiple participants from multiple industries.

The rest of the study is organized as follows. Section 2 introduces the institutional background and Section 3 includes my empirical predictions. Section 4 discusses the data and descriptive statistics. Section 5 presents the research design and the main empirical results. Section 6 presents some additional empirical analysis. Section 7 concludes the study.

2. State Flood Risk Disclosure Legislations

2.1. Background on U.S. flood risk disclosure legislations

Rising sea levels have increased the risk of coastal flooding over the last few decades and become the most economically damaging impact of climate change for many coastal locations (Buchanan et al., 2017). Bernstein et al. (2019) suggest that the durability of real estate investments, combined with the fact that real estate is by far the largest asset for most U.S. households, should make the predicted effects of sea level rise a first-order concern for millions of Americans. However, many homebuyers are completely unaware of whether their intended home has flooded before (NRDC, 2019). The National Flood Insurance Program, or NFIP, managed by the Federal Emergency Management Agency (FEMA) provides low-cost insurance to people whose homes are susceptible to flooding, helping homeowners to quickly rebuild after a flood. Over the last two decades, hurricanes Katina (2005), Sandy (2012) and Harvey (2017) have caused the NFIP to become burdened with enormous amounts of debt. One central criticism is the lack of a public, open-data system to ensure that all homebuyers have easier

access to flood hazard information. As a condition of NFIP participation, states adopt comprehensive flood-risk disclosure requirements in a standard form for real estate transactions that provide homebuyers with the right to know about a property's flood risk.

From 1992 to 2017, 29 states introduced laws requiring disclosure of flood risk in real estate transactions (see Table 1 panel A). Most regulated states are in coastal areas with significant flood hazard potential. The first state to adopt a flood risk disclosure law was Wisconsin in 1992, and the last three adoption states were Delaware, Mississippi and Nebraska in 2017. Although the required content of disclosure varies significantly across different states, the basic information required is similar. It typically requires sellers to complete a standard form, named Real Property Seller Disclosure, which includes descriptive questions about various factors, such as frequency of flood, flood insurance coverage, flood damage, flood history and flood zone classification. This report must be given to all prospective buyers prior to a buyer making an offer to purchase. Beyond these general requirements, states may also require more detailed disclosure. For instance, California requires sellers to disclose information about a property's dam inundation and has an additional form called Natural Hazard Disclosure, besides Real Property Seller Disclosure. Some states require the disclosure of measures and steps to remedy flood problems (i.e. Mississippi and Louisiana), while other states require disclosing information about drainage (e.g. District of Columbia, Illinois, Indiana and Michigan).

Overall, the intent of disclosure requirement is to make flood risk information readily available to buyers, to make them better equipped to identify and avoid purchasing a flood-prone home or take measures to reduce the risk of damage. This information benefits not only homebuyers, but also has positive spillover effects for another sector of the economy: the mortgage lending industry.

2.2. Predictors of U.S. flood risk disclosure legislations

States may introduce flood risk disclosure legislations in times of local economic stagnation, which could pose a threat to my identification strategy. I assess whether economic, political and fiscal factors explain the introduction of flood risk disclosure legislations using a predictive regression. The outcome, *Flood risk disclosure*, is an indicator variable equalling one if a state introduces a flood risk disclosure legislation in a given year. Appendix defines the state-level variables included in each specification.

Table 1 panel B presents the results. In both column 1 and column 2 (state fixed effects are included or not), I find that lagged state economic, political, and fiscal measures do not significantly predict the introduction of a flood risk disclosure legislation. I find that these variables do not have significant predictive power. Overall, state economic, political and fiscal conditions do not seem to drive the passage of flood risk disclosure legislations. The lack of predictability is consistent with the presence of considerable frictions in the passage and implementation of these legislations.

< INSERT TABLE 1>

3. Empirical Predictions

In debt contracting, lenders usually have an information disadvantage when undertaking downside risk. Therefore, assessing credit risk becomes particularly important and lenders consider information across a wide range of factors, including the nature and value of the property serving as loan collateral. Flood risk significantly affects the value of properties (Bernstein et al., 2019). However, many people are unaware of the true risks of flooding to their properties because of a combination of a lack of information and misinformation[7] (Rachel

[7] Available at: https://riskcenter.wharton.upenn.edu/digital-dialogues/improving-flood-risk-disclosure/

Cleetus, 2019). There are reasons to believe that states' flood-risk disclosure regulation makes more complete and credible information available to lenders in the mortgage industry.

Information Completeness. The degree of unravelling information depends on whether information is costly to acquire (Jovanovic, 1982; Mathios, 2000). First, many people are completely unaware of whether their target properties have flooded before (NRDC, 2019). While the Federal Emergency Management Agency (FEMA) publishes official flood maps, these data are still of little practical use to ordinary people, since the data set is dauntingly large and does not allow users to search by address (Kusisto 2019).

Second, the opportunity costs of voluntary disclosure are high, as it is time-consuming to submit a written request to FEMA, gather data and then analyse risk information (Kusisto 2019). Third, under the Privacy Act of 1974, FEMA cannot reveal a property's flood insurance claim history to anyone other than the homeowner, and the homeowner has to request it in writing (The Post and Courier, 2018). The Privacy Act prevents local government from sharing addresses with the public. So while government knows where the riskiest properties are, the average person has no way of knowing, even if they are about to buy a property.

Last, property agents or brokers are unlikely to provide such information because of their commission fees being closely associated with a property's value and their fiduciary responsibility to the seller, not the buyer (Garmaise & Moskowitz, 2004). To be more specific, brokers are usually compensated by a commission that is a percentage of the sale price, which incentivised them to encourage buyers to pay more for a property and in many jurisdictions the buyer's broker is legally a subordinate of the seller's broker and thus has a fiduciary responsibility to the seller, not the buyer (Garmaise & Moskowitz, 2004). The arguments suggest that whether brokers will provide unbiased information depends significantly on the relative benefits of high reputation and costs of commissions loss and litigation risk. Property

appraisals usually do the work fairly quickly and typically don't actively search and obtain extra information and they are not a source of valuable information. Also, the marginal benefits of voluntary disclosure are endogenous and depend upon buyer expectations (Lewis, 2011). Bakkensen (2018) proposes a heterogeneous rational belief model and suggests that whether buyers value flood risk information depends on the distribution of beliefs and other market characteristics. Their survey also highlights that 40% of flood zone respondents say they are "not at all" worried about flooding over the next ten years. Thus the likelihood of sellers' ability to charge a premium by signalling low risk though voluntary disclosure is limited.

Information Credibility. Revealing information may not occur if users have difficulty in credibly conveying it (Mathios, 2000; Shavell, 1994). FEMA maps have a number of drawbacks as a credible source of information for users. First, FEMA maps are backwards-looking and often out of date, with 1 in 6 maps being over 20 years old (Bakkensen, 2018). Second, they do not take into account future sea-level rises or increased risk of more intense hurricanes (Kusisto, 2019). Third, their maps are based on agreements between local communities and the federal government. If a community does not participate in the programme, no one can find information about a property on a map. Voluntary participants are not able to provide credible market-wide information about each property's flood risk in a region.

As Akerlof (1970) shows, under a voluntary disclosure regime, information asymmetry caused by low information completeness and credibility may prevent properties with low flood risk being traded in the market. Consequently, the inability of lenders and buyers to identify low flood risk properties also inhibits any incentives for sellers to take measures to reduce the risk of damage (Jin & Leslie, 2003).

In contrast, the mandatory disclosure of flood risk leaves sellers no choice over whether to display information about every traded property. Furthermore, sellers have no choice about

the method of disclosure, since a standard format is stipulated. More importantly, failure to comply makes sellers liable to buyers for up to three times the actual damage and a court may impose a buyer's costs and legal fees on the seller. These requirements increase the quality of product information available to lenders. On the other hand, the mandatory flood risk disclosure form must be given to all prospective buyers prior to a buyer making an offer to purchase. This form with high quality information readily available reduces the information acquisition cost of accurately understanding the flood risk of traded properties. This is consistent with Gao, Jiang, & Zhang's (2019) notion that the adoption of common accounting standards generates both a "precision effect", i.e. completeness and credibility, and a "network effect", i.e. comparability. The mitigation of information asymmetry reduces lenders' uncertainty about credit risk and enables them to estimate the value of housing collateral. Regardless of disclosure content, on average, flood risk disclosure will help lenders make better lending decisions.

It is probably the case that not all properties within a state will be equally affected by mandatory flood risk disclosure. Kothari, Li, & Short (2009) highlight not only a unidirectional link between capital market effects and the quality of disclosure, but also the influence of the content of disclosure. Therefore, the direction of the effects of flood risk disclosure depends on both average (quality) and heterogeneous (contents) effects (Sinha, 2019). Before the introduction of regulation, lenders used available information to determine which properties were likely to have a lower flood risk than others. To the extent that the information environment before regulation was incomplete and inaccessible to permit lenders to distinguish reliably between properties with high and low flood risk, a partial pooling of equilibrium was the result. If regulation increases completeness and/or credibility (or both), then creditors are likely to respond by increasing lending to properties with low flood risk, since lenders' assessments of property values increase and credit risk decreases. This can occur by either introducing more lending into the industry and directing it towards good properties (low flood

risk) or reducing the amount of lending supplied to bad properties (high flood risk). Thus, I predict that following mandates of flood risk disclosure regulation, mortgage lending increases for properties' whose geographic locations have a low flood risk, and mortgage lending either decreases or is unchanged for those identified as having a high flood risk.

However, there are several reasons to believe that whether and how mortgage lenders respond to the mandates of flood risk disclosures is *ex ante* unclear. Firstly, as Dranove et al. (2003) and Christensen et al. (2020) suggest, greater transparency may lead to unintended consequences in terms of selection behaviours and accordingly substitution effects and the net benefits on price efficiency significantly depends on the price structure of a specific industry. Similar to the healthcare industry (Dranove et al., 2003 and Christensen et al., 2020), the real estate market has differentiated pricing structures and complex multiple participants (sellers, buyers, appraisers, agents, lenders, insurance companies and other intermediaries), ex-ante disclosure in the industry may not be directly relevant. It is possible that the mandates of the regulation incentivise lenders opportunistically to select and redistribute resources to "lemons" (high flood risk) or just insure the risk through insurance companies. Given the costs of reporting the information, on net, this leads to the same or even higher levels of resources used and the same or even lower social welfares.

Secondly, the real estate market distinguishes from the theoretical market in which asset prices reflect all relevant information. Real estate transaction costs are high, many of the investors are amateurs (particularly for residential property), and assets are rarely perfect substitutes for one another. Even a small fraction of uninformed or optimistic buyers can lead to inflated property valuation. The impact of the disclosure regulation significantly depends on investors' (buyers') sophistication and costs of acquiring them. Lenders with greater resources will be better able to gather the information even before the mandates and thus benefit much

less from the regulation than lenders with smaller resources. The net benefits for the society is *ex ante* unclear.

Thirdly, the arguments above assume that all lenders use mandatory disclosure forms and share homogeneous rational expectations. Because of bounded rationality, even in the presence of relevant information, individuals are prone to a host of cognitive distortions that may lead them to make decisions far different from those predicted in a world of perfect rationality (Fung et al., 2007). Similarly, Bakkensen et al.'s (2017) analytical model shows that agents hold heterogeneous beliefs about flood risk information and hence the housing market implications may be starkly different. If lenders do not use the standard forms or share heterogeneous beliefs and responses to flood risk information, the flood risk disclosure may not have any effect on mortgage lenders' decision-making processes. Therefore, the spillover effects on the mortgage lending industry are ultimately an empirical question.

4. Data and Descriptive Statistics

4.1. Data and sample selection

Home Mortgage Lending. My mortgage data come from a comprehensive sample of mortgage applications and originations that have been collected by the Federal Financial Institutions Examinations Council under the provision of the Home Mortgage Disclosure Act (HMDA). Under this provision, the vast majority of mortgage lenders are required to report (Dagher & Kazimov, 2015). The HMDA data is available at the loan application level. It records information on the year of application (the data are available on an annual frequency), the amount of loan, the lender's decision, characteristics of the applicant (income, race, gender), and the location of the property down to the census tract level. HMDA data became available in the early 1990s, and my sample spans 1990 to 2013 period. It starts three years before Rhode Island mandates the flood risk disclosure law in 1993 and ends three years before Pennsylvania

mandates the flood risk disclosure law in 2010.[8] The original sample I start with includes 500 million observations. After restricting the sample to institutions which report to either Office of the Comptroller of the Currency (OCC), Federal Reserve System (FRS), or Federal Deposit Insurance Corporation (FDIC) the number of observations is reduced to 211 million. I restrict to mortgage applications made for owner-occupied home purchases of conventional single family properties (Dagher & Kazimov, 2015). To minimize noise I follow Dagher & Kazimov (2015) and Dell'Ariccia, Igan, & Laeven (2012), and drop applications below 25 thousand dollars and above a million dollars as well as applications with missing income and census-tract. I aggregate HMDA data up to the census tract level and obtain census tract level demographic attributes such as population, race, gender, house units from the U.S. Census Bureau (Atif & Amir, 2009). I end up with 319,758 census tract-year observations.

Flood Risk Disclosure Laws. I obtain disclosure regulation implementation years and contents of regulation from states' website and Natural Resources Defense Council. Table 1 panel A provides disclosure timeline for individual states. Figure 1 displays the number of financial institutions disclosing flood risk in their annual report by each calendar year. The trend indicates that flood risk becomes an increasingly important type of risk for creditors over time.

< INSERT TABLE 1>

< INSERT FIGURE 1>

Real Estate Prices. Residential real estate prices come from the Office of Federal Housing Enterprise Oversight (Chaney et al., 2012; Favara & Imbs, 2015). The OFHEO provides a Home Price Index (HPI), which is a broad measure of the movement of single-family

[8]I focus on coastal states which mandate flood risk disclosure laws. I begin in 1990 because the first flood risk disclosure law was mandated in 1993. And I end in 2013 because the latest flood risk disclosure law with available data was mandated in 2010. HMDA data are available until 2017. Two states that mandate a flood risk disclosure law in 2017 are removed to assure all census tract have available data three years before and three years after the law became effective.

home prices in the United States. Because of the breadth of the sample, it provides more information than is available in other house price indices (Chaney et al., 2012). In particular, the HPI is available at the census tract level since 1975. I use census tract level real estate prices as a proxy for the value of traded properties. Following Chaney et al. (2012) and Favara & Imbs (2015), I instrument local real estate prices using the interaction of long-term interest rates and local housing supply elasticity to control for the potential endogeneity of local real estate prices. Local housing supply elasticities are provide by Saiz (2010) and are available for 95 metropolitan statistical areas (MSAs). As a measure of long-term interest rates, I use the "contract rate on 30-year, fixed rate conventional home mortgage commitments" from the Federal Reserve website, between 1990 and 2013 (Chaney et al., 2012).

Flood Risk Identification. To differentiate cross-sectional flood risks across census tract where properties locate, I determine the census tract level exposure to flood risk for all properties within my sample utilizing the National Oceanic and Atmospheric Administration's (NOAA's) sea level rise (SLR) calculator (Bernstein et al., 2019; Marcy et al., 2011). The NOAA provides detailed SLR shapefiles that describe the latitude and longitudes that will be inundated following a 1-6 foot increase in average global ocean level. I utilize geographic mapping software ArcGIS pro to assess the exposure level of each census tract within a coastal county which mandates the flood risk disclosure regulation. Figure 2 provides a census tract map of the transactions that involve exposed properties. The exposed census tracts concentrate in the gulf region, Washington State, and along with the eastern seaboard. Figure 3 provides a clearer representation of sea level rise exposure in California and Louisiana respectively.

< INSERT FIGURE 2>

< INSERT FIGURE 3>

4.2. Descriptive statistics

I define different variables in Appendix. In particular, *Loan* is the average amount of mortgage loan originated (in thousands of dollars) in census tract i's year t (Favara & Imbs, 2015). *Ln(loan)* is the log amount of average mortgage loans originated (in thousands of dollars) in census tract i's year t. *Loan growth* is the percentage change of mortgage loans originated in census tract i from year t to year t-1. *Application* is the total number of mortgage loan applications in census tract i's year t. *Ln(application)* is the log transformation of *Application*. I control for census tract level attributes, including gender (*Male*), race (*White*), population (*Population*) and house units (*House units*), and also applicants' income growth.

Table 2, Panel A presents the descriptive statistics for the main variables for my sample. 46% (54%) of census tract-year observations in my sample are after (before) the mandates of a flood risk disclosure law. The mean *Loan* is 231 thousand dollars and average loan amount growth rate is 17%. The average number of mortgage applications within a census tract is 26. The percentages of male and white population within a census tract are balanced.

In Panel B of table 2, I present Pearson correlations. The correlations between my dependent variables (*Loan, Ln(loan) and Loan growth*) and variable of interest (*Post*, an indicator variable that takes the value of 1 after the mandates of flood risk disclosure laws) are positive and significant (p-value<0.01). *Application* and *ln(application)* are negatively correlated with *Post* at 0.05 significance level[9]. The majority of control variables are significantly correlated with mortgage loan value variables.

< INSERT TABLE 2>

5. Research Design and Results Discussion

5.1. Impact of flood risk disclosure laws on mortgage loan value

[9] The total number of applications is closely related to the time trend. After controlling for time trend, *Application* and *ln(application)* are positively and significantly correlated with *Post* (p-value<0.01).

5.1.1 Baseline analyses

I use a difference-in-difference approach to examine how the mandates of the flood risk disclosure laws affect home mortgage loan value at the aggregate census tract level.[10] My primary analyses follow the prior literature (e.g., Armstrong, Balakrishnan, & Cohen, 2012; Bertrand & Mullainathan, 2003) and utilize the following specification:

$$Loan_{i,t} = \alpha + \beta_1 Post_{i,t} + \Sigma\beta_2 Controls_{i,t} + \Sigma\beta_3 Census\ Tract\ Fixed\ Effects_{i,t} + \Sigma\beta_4 Year\ Fixed\ Effects_{i,t} + \varepsilon_{i,t}$$

My variable of interest is *Post*. I include census tract fixed effects and year fixed effects. β_1 essentially captures a difference-in-difference estimator where the control group is census tracts in states that have not yet mandated a flood risk disclosure law as of year t or mandate a flood risk disclosure law effectively prior to year t (Bertrand and Mullainathan 2003; Armstrong, Balakrishnan, and Cohen 2012). I cluster standard errors by state of census tracts because *Post* is a state-level variable (Klasa et al., 2018). Census tract level demographic attributes, such as gender, race, population and house units, are added to control for census tract level heterogeneity.

Table 3 reports my baseline results where columns (1), (3), (5) are without controls and columns (2), (4), (6) are with controls. The coefficients for *Post* are positive and significant in all six specifications, indicating that the mandates of flood risk disclosure laws have a positive and statistically significant impact on mortgage loan values. Specifically, the estimated coefficients in *Post* imply that after the mandates of flood risk disclosure laws, on average, affected census tracts' average mortgage loan value increase by 51 thousands of dollars, loan

[10] I choose census tract level because properties' flood risk is defined at their census tract geographic location level, which is a more accurate identification strategy. I obtain similar results on mortgage loan value at the aggregate county level (Results are tabulated in the Online Appendix).

value increase by 12% and loan value growth rate increase by 0.13. Overall, the results from Table 3 suggest that flood risk disclosure laws increase the value of home mortgage loans.

< INSERT TABLE 3>

5.1.2 Dynamic analyses

Next, I examine the validity of the parallel trend assumption embedded in my DID design. Essentially, I investigate when the changes in mortgage loan values occurred relative to the mandates of the flood risk disclosure laws. To this end, I create a series of indicator variables *Effective* indexed t+i from t-3 to t+3 with t=0 being the year of mandates of a flood risk disclosure law in the state where the properties are located. The variables take the value of one if a law is mandated within t-i. I report the results in Table 4. In all specifications, I find that the coefficients on $Effective^{-3}$, $Effective^{-2}$ and $Effective^{-1}$ are statistically insignificant, have inconsistent signs and have a small magnitude, while the coefficients on $Effective^{0}$, $Effective^{+1}$, $Effective^{+2}$ and $Effective^{+3}$ are all consistently positive and statistically significant. Overall, these results support the validity of the parallel trend assumption in my setting.

< INSERT TABLE 4>

There may be a concern that a specific state and its idiosyncrasies generate my results. For example, California is the most populous U.S. state and home to many financial institutions. Results tabulated in the Online Appendix indicate that my conclusions hold if I exclude this state and in fact hold if I exclude any given state from my tests.

5.1.3 Cross-sectional analyses

Flood risk ex ante. So far, I examine the average effects of flood risk disclosure law on mortgage loan value regardless of the content of disclosures. The disclosure could either be good or bad depending upon the risk of flood inundation ex ante. From a collateral value

perspective, high environmental risk is bad news for lenders, whereas low environmental risk is good news for lenders. To test the differential effects of good versus bad news disclosures, I partition the sample based on property census tracts' exposure of flood risk ex ante. In the baseline analyses, I define any census tract that would be inundated at highest high tide with a 6 foot global average sea level rise to be high risk (Bernstein et al., 2019). Consistent with my hypothesis, I expect mortgage lending to increase for properties' geographic locations with low flood risk, and mortgage lending either decreases or is unchanged for those identified as with high flood risk.

Table 5 reports results of this analysis, where *SLR risk* equals to one if six feet or less of SLR would put the property underwater (NOAA SLR viewer; Marcy et al., 2011; Bernstein et al., 2019). Panel A is without controls and panel B is with all the controls. Consistently in both panel A and panel B, I find the impact of flood risk disclosure laws on home mortgage loan value only presents in census tract with low sea level rise risk, but not in census tract with high sea level rise risk. The effects are significantly larger in low risk census tract, relatively to high risk census tract (p-value<0.01). In particular, the estimated coefficients of *Post* imply that after the mandates of flood risk disclosure laws, on average, affected low risk census tracts' average mortgage loan value increase by 54 thousands of dollars, loan value increase by 13% and loan value growth rate increase by 0.14. The results indicate due to an increase in information completeness and credibility (or both), creditors respond by increasing lending to properties with low flood risk since their assessment of property value increase and credit risk decrease. The resources come from introducing more lending to the industry as a whole rather than from reducing the amount supplied to others. This finding suggests that although there is an overall increase in mortgage lending following mandates of flood risk disclosure regulation, the increase is attributable to properties with low flood risk *ex ante*.

< INSERT TABLE 5>

5.2. Impact of flood risk disclosure laws on mortgage loan-to-value ratio

It is well known that house prices display considerable geographic heterogeneity in the United States (Saiz, 2010; Favara & Imbs, 2015). Such heterogeneity can arise from differences in housing supply elasticities, for instance because of local costs, land use regulation, or geographic restrictions (Saiz, 2010; Gyourko & Saiz, 2006). It can also come from the demand side of the market, such as income, demographic factors and amenities (Favara & Imbs, 2015). Thus when estimating the mortgage loan to value ratio at the local level, it is very important to address this source of endogeneity. Following Atif & Amir (2009), Chaney et al. (2012) and Favara & Imbs (2015), I instrument census tract level real estate prices by interacting local housing elasticities with aggregate shifts in the interest rate. Local housing supply elasticities are provided by Saiz (2010) and are only available for 95 metropolitan statistical areas (MSAs). So for this section of analyses, I end up with 111, 233 census tract-year observations from period of 1990 to 2013. As a measure of long-term interest rates, I use the "contract rate on 30-year, fixed rate conventional home mortgage commitments" from the Federal Reserve website, between 1990 and 2013. I then use the instrumented real estate prices to calculate the census tract-level loan to house value ratio.

Table 6 reports the baseline result by replacing the dependent variable with *Loan-to-value*, measured as the average census tract loan value change divided by instrumented house price index (Saiz, 2010). The coefficient is positive and significant both with and without controls, suggesting that borrowers are able to finance a greater percentage of property value after flood risk disclosure regulation. This results confirm the notion that mortgage lenders use improved and easier available flood risk disclosure information to analyze credit risk associated with the collateral, thereby impacting the mortgage lending activity.

< INSERT TABLE 6>

Table 7 reports the parallel path assumption, consistent with the findings in section 5.1.2 in which the effects of states' flood risk disclosure laws occur after those laws come into force but not before. Table 8 display the cross-sectional results conditional on the properties' flood risk exposures. The results are consistent with my main findings in section 5.1.3 where the increase in *Loan-to-value* are significantly greater in low risk census tract, relatively to high risk census tract.

< INSERT TABLE 7>

< INSERT TABLE 8>

5.3. Channel:

5.3.1 Housing collaterals

Though section 5.2 indicates the positive spillover effects of mandatory flood risk disclosure to consumers on home mortgage loan value ratio, the potential channels through which laws affect creditors' lending decisions remained uninvestigated. In this section, I try to explore one of the potential channels, collateral value, and I assess the impact of flood risk disclosure laws on house values and its association with the mortgage loans.

Table 9 presents the baseline result by replacing the dependent variable with *Home price index*, from Office of Federal Housing Enterprise Oversight. The coefficient is positive and significant both with and without controls, suggesting that the mandates of flood risk disclosure regulation result in a shift in the level of sale prices. Table 10 display the cross-sectional results conditional on properties' flood risk exposures. The results are consistent with my main findings where the increase in house prices are significantly greater in low risk census tract, relatively to high risk census tract.

Next, I investigate if house collateral values drive the increase in mortgage loans after the mandates of flood risk disclosure. I conduct two additional analyses. First, I rank observations on the basis of changes in the average house values in years after the mandates of a disclosure law versus years before. Then I form two new variables *Highpost* and *Lowpost* based on the change in house values and the influence of the disclosure mandates. Specifically, *Highpost* is an indicator variable equal to one if a census tract in a state mandates flood risk disclosure law and experiences high increase in house collateral value, and zero otherwise. *Lowpost* equals one if a census tract in a state mandates flood risk disclosure law and experiences low increase in house collateral value, and zero otherwise. Results in table 11 show that increase in mortgage loans only shows up for those properties experiencing high increase in collateral values. In the second test, I reproduce my baseline results (similar to ones reported in table 3) but remove observations experiencing high increase in house collateral values in the distribution, my baseline results become statistically insignificant and magnitude decrease significantly. Results are tabulated in the Online Appendix.

Overall, the results in this section suggest that changes in the level of housing collateral value could be one of the potential mechanism through which flood risk disclosure laws affect the credit market.

< INSERT TABLE 9>

< INSERT TABLE 10>

< INSERT TABLE 11>

5.3.2 **Disclosure regulation intensity**.

As discussed in section II, flood risk disclosure laws generally require lenders to complete a standard form named Real Property Seller Disclosure. However, their requirements

vary across states. I consider the strictness of the law. The strictness of disclosure laws can have a differential impact on the quality of information and in turn affect the degree of reduced uncertainty and risk in estimating collateral value. Extant research suggests that regulation design can significantly affect its effectiveness of the law and amount of spillovers it creates (e.g., Fung, Graham, & Weil, 2007; Meyer & Rowan, 1977). I surmise that states where the law is stricter prompt a more significant response from properties' locations that are covered. I consider the following key dimensions of flood risk disclosure law in my *Lawindex* [11] (NRDC, 2019): whether a law requires disclosure of (a) flood damage; (b) flood nature and frequency, (c) flood insurance; (d) flood zone classification; (e) flood damage to the property; (f) federal disaster aid coverage; (g) an additional form; (h) drainage and (i) measures to remedy the problem. I first create an indicator for each one of the above dimensions and assign a value of 1 to the indicators when the answer to each of the dimensions is yes. I then construct an index (*Lawindex*) by summing up the indicators. I then sort the index into two groups with high *Lawindex* and low *Lawindex*. Lastly, I create an indicator variable *Strictpost (Weakpost)* to capture those scenarios where the law is likely more (less) stringent. *Strictpost* is an indicator variable equal to one if a census tract in a state mandates flood risk disclosure law with high *Lawindex,* and zero otherwise. *Weakpost* is an indicator variable equal to one if a census tract in a state mandates flood risk disclosure law with low *Lawindex*, and zero otherwise.

Table 12 and table 13 present the results. Consistent with the notion that the strictness of the law plays a facilitator role in improving information quality and reducing the uncertainty of estimating collateral value, the coefficients for *Strictpost* are all positive and significant across the columns. On the other hand, the coefficients for *Weakpost* are all with much smaller magnitudes and lower statistical significance. Taking together, these findings suggest that the

[11] Available at https://www.nrdc.org/flood-disclosure-map.

flood risk disclosure laws have significantly smaller positive spillover effects on home mortgage loan value when the regulation is weaker. The positive spillovers show up mainly when the regulation is likely more stringent and are significantly greater relative to states with less stringent regulations. These findings have important policy implications as the federal regulators design a national level flood risk disclosure law. Mandatory transparency policy must be carefully designed to achieve their objectives.

< INSERT TABLE 12>

< INSERT TABLE 13>

6. Additional Investigations

6.1. Contiguous border-county analysis

To alleviate the concern that my results are driven by unobservable local factors happening in the same tract-year as my treatment, I conduct a further analysis in which I construct a sample of contiguous counties located on different sides of a state-pair border (Dube et al., 2010). What this approach does is to compare the outcomes of adjacent counties separated by state borders, where the two states are differently affected by the regulation change. The underlying assumption is that the counties are immediately adjacent to each other and thus they are very similar in observable aspects. More importantly, they are very similar in unobservable aspects that are very difficult to control for. Prior research (Dube et al., 2010; Huang, 2008) shows that contiguous border counties provide significantly better control groups than randomly selected counties or counties chosen using a propensity score matching approach.

I use the contiguous border-county approach to identify the effects of state-level flood risk disclosure regulation on census-tract level mortgage lending within pairs of two adjacent counties on opposite sides of the state borders. I identify adjacent county-pair along the state borders using the US Census Bureau County Adjacent file. I then merge this dataset with my

main sample and end up with 98,002 tract-year observations for loan value sample, and 22,748 for loan to value ratio sample.

Table 14 presents my DID estimator based on the tracts-level mortgage within pairs of two adjacent counties on opposite sides of the state borders. I continue to find statistically significant effects of flood risk disclosure regulation on mortgage lending in most specifications.

Though not perfectly, the evidence from my border-county analysis mitigates concerns that results of my tract-level analysis are driven by unobservable local macroeconomic conditions happening the same time as my treatment.

< INSERT TABLE 14>

6.2. Impact of flood risk disclosure laws on mortgage loan applications

In the next set of my analyses, I investigate whether flood risk disclosure regulation affects the number of mortgage loan application. Results tabulated in the Online Appendix show, on average, the flood risk disclosure regulations do not significantly increase the number of applications. However, conditional on the regulation intensity, the census tracts under stricter law (defined the same as in section 5) increase the number of applications by 6 and 23%, respectively. Conditional on regulation intensity, the increase in the number of mortgage loan applications are significantly greater in low risk census tract, relatively to high risk census tract.

6.3. Impact of flood risk disclosure laws on mortgage loan-to-income ratio

In section 5.2., the calculation of loan-to-value ratios requires data on local housing supply elasticities provided by Saiz (2010) and only available for 95 metropolitan statistical areas (MSAs). Hence the sample size is reduced. As a robustness check, following Favara & Imbs (2015), I investigate whether flood risk disclosure regulation affects applicants' loan to income ratio. LTI is measured as average loan value divided by average applicants' income.

Results tabulated in the Online Appendix are consistent with the findings in previous analysis, that is, conditional on the regulation intensity, the census tracts under stricter law (defined the same as in section 5) increase loan to income ratio by 25%. Conditional on regulation intensity, the increase in loan to income ratio is significantly greater in low risk census tract, relatively to high risk census tract.

6.4. Impact of flood risk disclosure laws on new house constructions

So far, my analysis focus on the demand side effects, and in this section, I investigate the supply side aggregate effects. Specifically, I examine whether the quantity of new house constructions changes after the mandates of flood risk disclosure laws. Housing permits serve as my measure of new quantity (Glaeser et al., 2008). Permits data are available at the county level from the U.S. Bureau of the Census (Glaeser et al., 2008). Results tabulated in the Online Appendix show that quantity of new house constructions increase by around 17% after the disclosure mandates. The results indicate that the disclosure mandates have aggregate spillover effects also on the housing supply.

6.5. Impact of flood risk disclosure laws on loan loss provisions

In my last part of analysis, I investigate whether home mortgage lenders that are depository institutions indeed change their behaviors after the mandates of flood risk disclosure laws. Using the geographic locations of banks' branches as an alternative identification strategy, I test the impact on banks' loan loss provisions. Results tabulated in the Online Appendix show that banks decrease the amount of loan loss provisions after the disclosure mandates. The results are consistent with my main findings since the increase in information completeness and credibility due to disclosure mandates reduces banks' credit risk, making them adjust downwards their loan loss provisions.

7. Conclusion

Mandatory corporate disclosure is used by policymakers to decrease the information asymmetry between firms and their stakeholders, and to improve the efficiency of capital markets. Many empirical studies in the accounting literature have documented the *direct* effects of corporate disclosure on *firm level* outcomes.[12] However, the lack of evidence on aggregate spillover effects prevents policymakers from offering full justification for regulations. Moreover, most empirical studies focus on corporate settings mandated by the SEC or other security market regulators. Product disclosure regulations for consumers by non-SEC regulators can also have material implications for capital markets and the local economy. Corporate disclosure is a small part of a broad family of targeted transparency policies, with the aim of stipulating or prohibiting certain behaviours (Fung et al., 2007a; Leuz & Wysocki, 2016). It is important to study disclosure and transparency as a research topic that goes beyond corporate reporting (Leuz & Wysocki, 2016).

In this paper, I study the aggregate spillover effects of mandatory disclosure by exploring the novel setting of state mandated transparency for consumer protection, which require sellers to disclose a property's flood risk in real estate transactions. I exploit plausibly exogenous variation in the adoption of flood risk disclosure across different states in the U.S.

Using mortgage lending and sea level rise risk data, I find that aggregate census tract level mortgage loan value, loan value growth rates and loan-to-value ratio are significantly greater after states require sellers to disclose flood risk information in real estate transactions. These positive spillover effects on mortgage loans only show up for property census tract locations with low flood risk, and are significantly greater relatively to those with high flood risk *ex ante*. The effects are more concentrated in those states with more stringent legal requirements. Moreover, such law leads to increases in collateral value linked to mortgage loans.

[12] I define direct real effects in this study as the influence on disclosing firms' behaviours or those of peer firms within the regulated market (where new disclosure is mandated).

These findings highlight that mandatory transparency in consumer protection does indeed have material spillover effects on the local economy, generating aggregate welfare effects on it.

Collectively, my findings suggest that mandatory disclosure potentially creates aggregate welfare effects on the local economy. Policymakers should not only consider the direct influence on disclosing firms or their peers within this regulated industry, but also take into account the spillover from regulated to unregulated industry when designing regulations. Specifically, first, to make mandatory disclosure policy effective, it is not enough for a disclosure system to provide "just facts". Rather, the policy needs to aggregate, simplify and benchmark the facts so that the resulting decisions fit the objectives that motivated disclosure in the first place (Fung et al., 2007). Second, the incentives created should not simply motivate a redistribution from "lemons" to "peaches", but rather introducing new resources to enlarge the economy as a whole. Third, the findings in this setting that go beyond corporate reporting also have potential implications for the corporate sector. For example, the positive spillover of transparency between sellers and buyers may also apply to M&A transactions between target and buyer companies. And disclosure within an M&A transaction may serve as an alternative information channel for multiple participants from a range of industries.

My paper's findings have several caveats. First, my paper cannot directly address national-level benefits of disclosure regulation. My identification strategy relies on state level heterogeneity in mandate timing and census tract level flood-risk exposure, strengthening my identification but preventing me from quantifying country-level effects. Second, my paper is not able to rule out other possible mechanisms besides the collateral channel, e.g. the insurance channel from the secondary mortgage industry (Sinha, 2019). Thirdly, the flood risk regulation could have large impact on new neighborhood compared with old one because real estate prices in old neighborhood with a longer history and experience are more likely to include flood risk *ex ante*. Unfortunately, I am not able to differentiate between old and new neighborhoods at the

local scale since I could not observe the property transactional level characteristics. More granular data on the neighborhood characteristics will help me confirm the disclosure regulation channel and explore more variations at the local neighborhood level. Moreover, my paper does not emphasize the optimal scope of disclosure regulation and, in particular, it does not suggest that more disclosure regulation is “always” desirable.

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Figure 1

Number of Flood Risk Disclosures over the Calendar Year

The following figure plots the number of flood risk disclosures over calendar years from the year 1993 to 2013. The Y variable is the total number of financial institutions disclosing flood risk in their annual report. I base the fitted line on a linear regression. Flood risk disclosures are identified using key word search in annual reports.

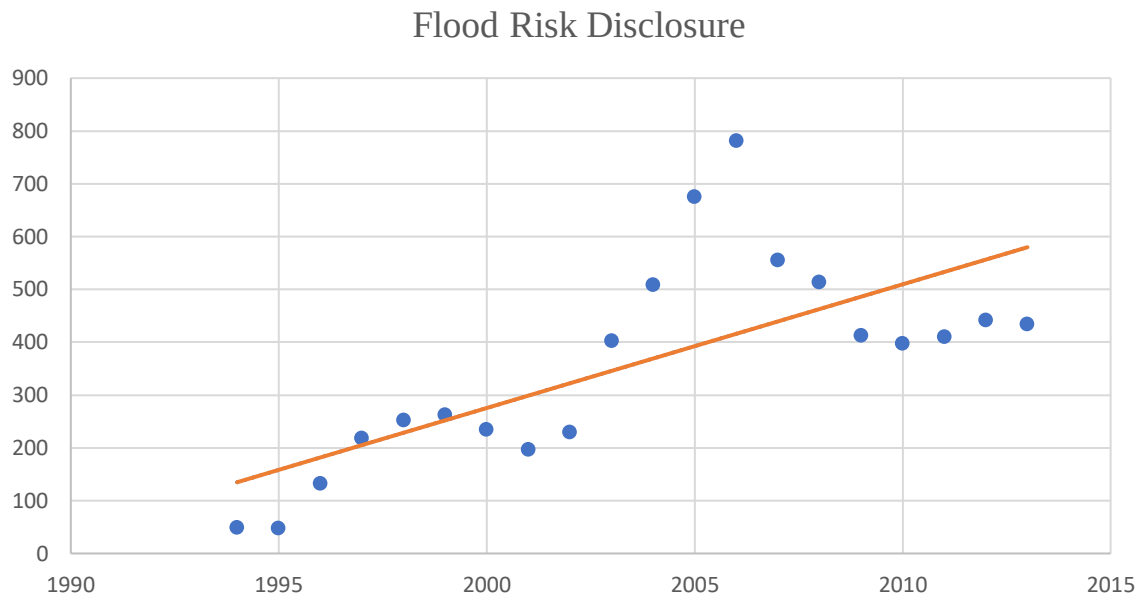

Figure 2

Sea Level Exposures by Census Tract

The following figure displays the sea level exposure of properties in coastal census tracts within the continental United States with mandatory flood risk disclosure law. Exposure is measured as an indicator variable that takes a value of one if a property's census tract will be affected by 0 to 6 feet of sea level rise. I utilize the NOAA's SLR calculator to define the SLR exposure of each property's census tract.

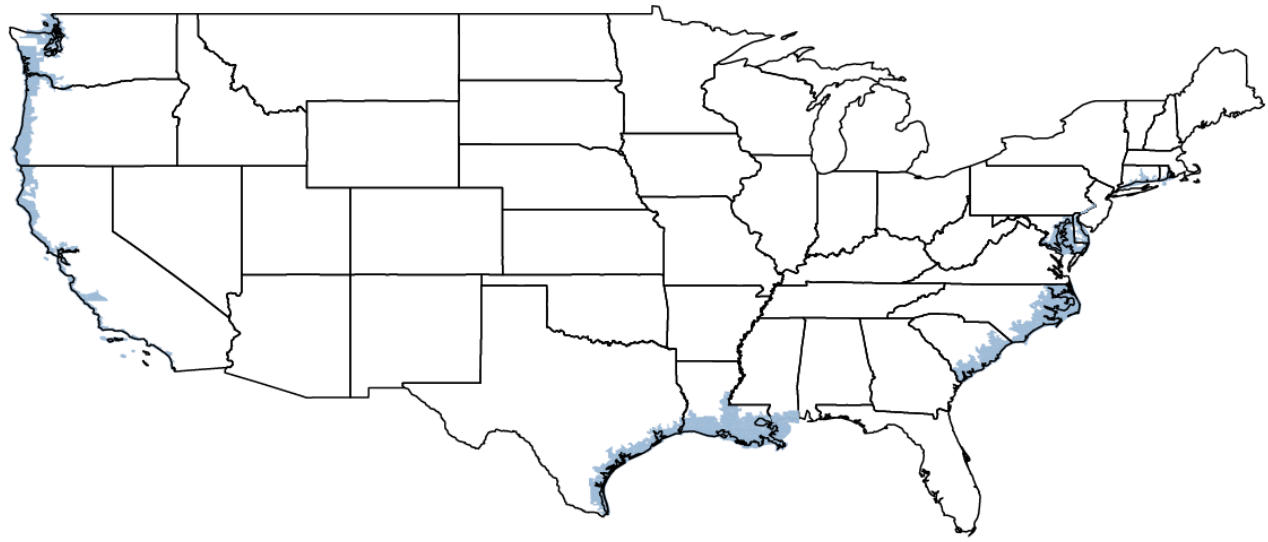

Figure 3

Sea Level Exposures in California and Louisiana

The following figure displays the sea level exposure of properties in coastal census tracts within California and Louisiana separately. Exposure is measured as an indicator variable that takes a value of one if a property will be affected by 0 to 6 feet of sea level rise. I utilize the NOAA's SLR calculator to define each property's SLR exposure.

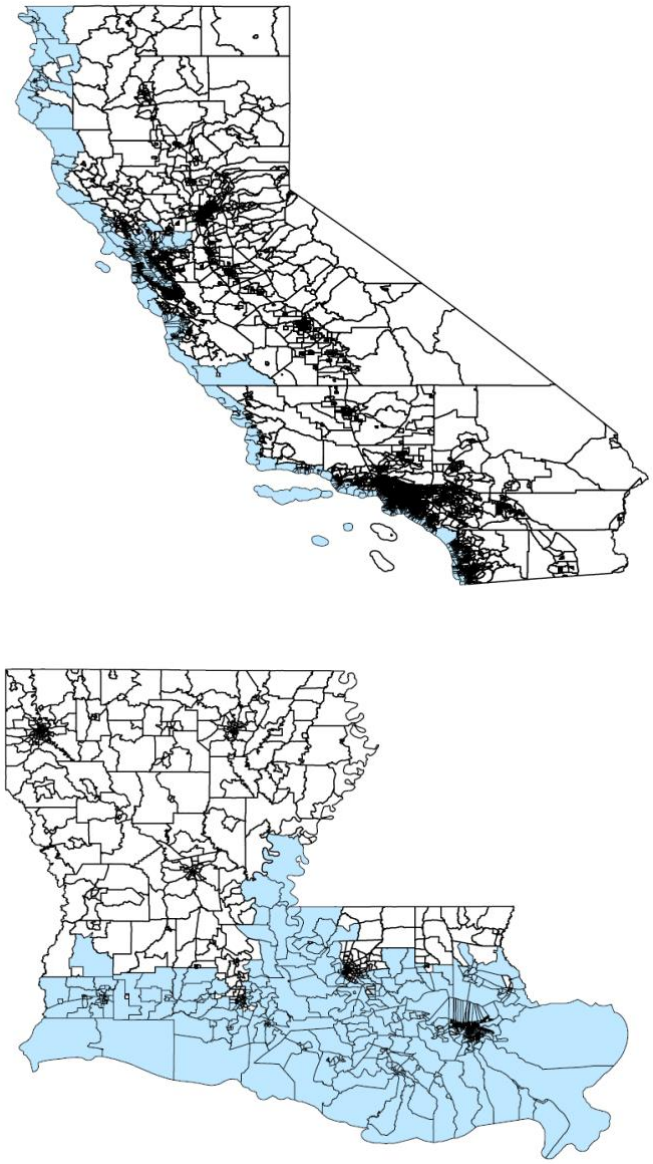

Table 1

Panel A: Time Distribution of When Coastal States Mandates a Flood Risk Disclosure Law

The following table displays the year in which each coastal state originally mandates a flood risk disclosure law. The data are coded from each state's website and verified based on information from Natural Resources Defense Council.

Year of Mandates	States
1993	Rhode Island
1996	Hawaii, Washington
1998	North Carolina
1999	District of Columbia
2000	California
2002	South Carolina
2003	Oregon
2004	Connecticut, Louisiana
2005	Maryland
2008	Texas
2010	Pennsylvania

Panel B: Predictive Regressions

This table examines whether a state's economic, political, fiscal, or entrepreneurial conditions predict the adoption of flood risk disclosure legislation for the sample period 1990 to 2013. The dependent variable is an indicator equal to one (*Flood risk disclosure*) if a state has adopted a flood risk disclosure legislation in that year and zero otherwise. All independent variables are lagged by one year relative to the dependent variable. Each column includes year fixed effects, while the even-numbered columns also include state fixed effects. Standard errors are reported in parentheses and clustered by state. ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

	Flood risk disclosure	
VARIABLES	(1)	(2)
GDP growth	-0.001	-0.001
	(0.001)	(0.001)
Property tax	0.134	0.471
	(0.092)	(0.915)
Income tax	-0.013	0.070
	(0.019)	(0.180)
Ln(Population)	0.005	0.088
	(0.006)	(0.104)
Employment rate	-0.075	-0.308
	(0.093)	(0.338)
Revenue/GDP	-0.466	-0.566
	(0.372)	(0.521)
Expenditure/GDP	0.387	0.395
	(0.537)	(0.828)
Debt/GDP	0.205	0.773*
	(0.187)	(0.437)
Establishment entry rate	0.004	0.004
	(0.003)	(0.005)
Net job creation rate	-0.004	-0.005
	(0.005)	(0.005)
Establishment exit rate	-0.002	-0.005
	(0.004)	(0.005)
Democratic control	-0.047	-0.031
	(0.055)	(0.058)
Republican control	-0.055	-0.060
	(0.057)	(0.063)
Observations	680	680
R-squared	0.043	0.057
State FE	NO	YES
Year FE	YES	YES
Cluster at State	YES	YES

Table 2

Table 2 Panel A reports summary statistics of key variables for the full sample from 1990 to 2013. Panel B presents Pearson correlations, with the correlation coefficients with a significance level of 0.05 or better in bold. All continuous variables are winsorized to the 1st and 9th percentiles of their distributions.

Panel A: Descriptive Statistics

VARIABLES	N	Mean	Std. Dev.	Q1	Median	Q3
Testing Variables						
Loan	319,758	230.638	170.011	115.828	175.273	286.667
Ln(loan)	319,758	5.218	0.656	4.752	5.166	5.658
Loan growth	319,758	0.172	0.713	-0.305	-0.002	0.434
Application	319,758	25.901	24.022	9.000	19.000	35.000
Ln(application)	319,758	2.897	0.946	2.303	2.996	3.584
Post	319,758	0.463	0.499	0.000	0.000	1.000
Control Variables						
Male	319,758	48.818	2.378	47.500	48.800	50.000
White	319,758	68.744	25.360	52.900	76.100	89.500
Income growth	319,758	1.379	4.449	-0.542	-0.003	1.177
House unit	319,758	7.420	0.401	7.156	7.451	7.716
Population	319,758	8.314	0.415	8.051	8.357	8.622

Panel B: Pearson Correlation Coefficients (n=319,758)

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
(1) Post	1.000										
(2) Loan	**0.447**	1.000									
(3) Ln(loan)	**0.497**	**0.926**	1.000								
(4) Loan growth	**0.299**	**0.409**	**0.421**	1.000							
(5) Application	**-0.002**	**0.056**	**0.114**	**-0.017**	1.000						
(6) Ln(application)	**-0.066**	**0.048**	**0.111**	**-0.014**	**0.871**	1.000					
(7) Population	**0.105**	**0.069**	**0.120**	**-0.016**	**0.403**	**0.384**	1.000				
(8) Male	**0.080**	**0.115**	**0.143**	**0.032**	**-0.008**	**-0.030**	**0.052**	1.000			
(9) White	**-0.113**	**-0.051**	**-0.038**	**-0.063**	**0.161**	**0.216**	**-0.028**	**0.115**	1.000		
(10) House unit	**0.086**	**0.011**	**0.055**	**-0.010**	**0.411**	**0.399**	**0.851**	**-0.034**	**0.131**	1.000	
(11) Income growth	**0.091**	**0.138**	**0.148**	**0.238**	**0.245**	**0.257**	**0.035**	**0.034**	**-0.063**	**0.018**	1.000

Table 3

Effects of Flood Risk Disclosure Laws on Home Mortgage Loan Value

The table reports results from OLS regressions of home mortgage loan value on the indicator for the mandates of the flood risk disclosure law. The sample spans the 1990-2013 period and include 319,758 census tract-year observations. The dependent variables are *Loan* (average amount of mortgage loan originated (thousands of dollars) in census tract i's year t), *Ln(loan)* (log amount of average mortgage loans originated (thousands of dollars) in census tract i's year t) and *Loan growth* (the percentage change of mortgage loans originated in census tract i from year t to year t-1). *Post* is an indicator variable equal to one if a census tract in a state mandates flood risk disclosure law, and zero otherwise. Control variables are defined in the Appendix. Census tract-fixed effects and year-fixed effects are included. All continuous variables are winsorized to the 1st and 99th percentiles. Standard errors are corrected for heteroskedasticity and clustering at state level (robust standards errors are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

VARIABLES	*Loan*	*Loan*	*Ln(loan)*	*Ln(loan)*	*Loan growth*	*Loan growth*
Post	52.821**	50.673**	0.125**	0.119**	0.161**	0.129**
	(23.236)	(22.264)	(0.051)	(0.049)	(0.070)	(0.058)
Population		49.932*		-0.007		-0.038
		(27.952)		(0.103)		(0.134)
Male		-5.981**		-0.002		-0.004
		(2.064)		(0.002)		(0.002)
White		0.372		0.001		0.000
		(0.763)		(0.002)		(0.002)
House unit		-42.468		0.077		-0.050
		(31.652)		(0.089)		(0.102)
Income growth		1.370***		0.006***		0.034***
		(0.123)		(0.001)		(0.004)
Observations	319,758	319,758	319,758	319,758	319,758	319,758
Adjusted R-squared	0.743	0.744	0.803	0.805	0.192	0.231
Tract FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
Cluster at State	YES	YES	YES	YES	YES	YES

Table 4

Dynamic Effects of Home Mortgage Loan Value Around the Mandates of the Flood Risk Disclosure Law

The table reports results from OLS regressions of home mortgage loan value on the indicator for the mandates of the flood risk disclosure law. The sample spans the 1990-2013 period and include 319,758 census tract-year observations. *Effective*$^{-3}$, *Effective*$^{-2}$, *Effective*$^{-1}$, *Effective*^{0}, *Effective*$^{+1}$, *Effective*$^{+2}$, *Effective*$^{+3}$, which are equal to one if the census tract is in a state that will mandate the flood risk disclosure law in two years, will mandate the flood risk disclosure law in two years, will mandate the law in one year, mandates the law, mandated the law in year ago, mandated the law two years ago, mandated the law three or more years ago, respectively, and zero otherwise. The dependent variables are *Loan* (average amount of mortgage loan originated (thousands of dollars) in census tract i's year t), *Ln(loan)* (log amount of average mortgage loans originated (thousands of dollars) in census tract i's year t) and *Loan growth* (the percentage change of mortgage loans originated in census tract i from year t to year t-1). Control variables are defined in the Appendix. Census tract-fixed effects and year-fixed effects are included. All continuous variables are winsorized to the 1st and 99th percentiles. Standard errors are corrected for heteroskedasticity and clustering at state level (robust standards errors are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

VARIABLES	*Loan*	*Loan*	*Ln(loan)*	*Ln(loan)*	*Loan growth*	*Loan growth*
Effective $^{-3}$	-5.070	-5.488	-0.006	-0.007	-0.008	-0.020
	(8.955)	(8.753)	(0.029)	(0.028)	(0.038)	(0.034)
Effective $^{-2}$	2.943	2.086	0.014	0.011	0.006	-0.017
	(15.771)	(15.475)	(0.037)	(0.036)	(0.058)	(0.056)
Effective $^{-1}$	14.196	12.992	0.044	0.039	0.043	0.012
	(14.877)	(14.465)	(0.031)	(0.030)	(0.048)	(0.044)
Effective ^{0}	28.970*	27.085*	0.081**	0.075**	0.093*	0.057
	(14.807)	(14.019)	(0.032)	(0.031)	(0.044)	(0.036)
Effective $^{+1}$	37.676*	35.515*	0.114**	0.107**	0.139**	0.098**
	(18.062)	(17.161)	(0.041)	(0.040)	(0.054)	(0.043)
Effective $^{+2}$	46.947*	44.346*	0.136**	0.127**	0.166**	0.114**
	(21.559)	(20.501)	(0.049)	(0.048)	(0.064)	(0.050)
Effective $^{+3}$	104.418**	101.097**	0.213**	0.203**	0.276**	0.218**
	(44.955)	(43.589)	(0.077)	(0.075)	(0.112)	(0.091)

Controls	NO	YES	NO	YES	NO	YES
Observations	319,758	319,758	319,758	319,758	319,758	319,758
Adjusted R-squared	0.753	0.755	0.805	0.806	0.195	0.234
Tract FE	YES	YES	YES	YES	YES	YES
Yes FE	YES	YES	YES	YES	YES	YES
Cluster at State	YES	YES	YES	YES	YES	YES

Table 5

Effects of Flood Risk Disclosure Laws on Home Mortgage Loan Value in Census Tract with Greater Ex Ante Flood Risk

The table reports results from OLS regressions of home mortgage loan value on the indicator for the mandates of the flood risk disclosure law conditional on census tracts' ex ante flood risk. The sample spans the 1990-2013 period and include 40,707 census tract-year observations for high flood risk census tract and 279,051 census tract-year observations for low flood risk census tract. The dependent variables are Loan (average amount of mortgage loan originated (thousands of dollars) in census tract i's year t), Ln(loan) (log amount of average mortgage loans originated (thousands of dollars) in census tract i's year t) and Loan growth (The percentage change of mortgage loans originated in census tract i from year t to year t-1). Post is an indicator variable equal to one if a census tract in a state mandates flood risk disclosure law, and zero otherwise. SLR risk is an indicator variable equal to one if six feet or less of SLR would put the property underwater (NOAA SLR viewer; Marcy et al., 2011; Bernstein et al., 2019). Panel A is without controls and panel B is with controls. Control variables are defined in the Appendix. Census tract-fixed effects and year-fixed effects are included. All continuous variables are winsorized to the 1st and 99th percentiles. Standard errors are corrected for heteroskedasticity and clustering at state level (robust standards errors are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Panel A	*SLR risk=1*	*SLR risk=0*	*SLR risk=1*	*SLR risk=0*	*SLR risk=1*	*SLR risk=0*
VARIABLES	*Loan*	*Loan*	*Ln(loan)*	*Ln(loan)*	*Loan growth*	*Loan growth*
Post	21.623	56.819**	0.037	0.136**	0.067	0.173**
	(21.862)	(23.382)	(0.048)	(0.052)	(0.065)	(0.072)
	(1)	(2)	(3)	(4)	(5)	(6)
Test of differences [p-values]	(2)-(1): 0.004		(4)-(3): 0.002		(6)-(5): 0.002	
Controls	NO	NO	NO	NO	NO	NO
Observations	40,707	279,051	0.801	0.803	40,707	279,051
Adjusted R-squared	0.750	0.741	0.812	0.814	0.220	0.188
Tract FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
Cluster at State	YES	YES	YES	YES	YES	YES

Panel B	*SLR risk=1*	*SLR risk=0*	*SLR risk=1*	*SLR risk=0*	*SLR risk=1*	*SLR risk=0*
VARIABLES	*Loan*	*Loan*	*Ln(loan)*	*Ln(loan)*	*Loan growth*	*Loan growth*
Post	20.474	54.481**	0.035	0.130**	0.055	0.138**
	(21.121)	(22.372)	(0.047)	(0.051)	(0.056)	(0.060)
	(1)	(2)	(3)	(4)	(5)	(6)
Test of differences [p-values]	(2)-(1): 0.004		(4)-(3): 0.002		(6)-(5): 0.004	
Controls	YES	YES	YES	YES	YES	YES
Observations	40,707	279,051	40,707	279,051	40,707	279,051
Adjusted R-squared	0.752	0.743	0.802	0.804	0.254	0.228
Tract FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
Cluster at State	YES	YES	YES	YES	YES	YES

Table 6

Effects of Flood Risk Disclosure Laws on Home Mortgage Loan Value Ratio (Instrumented)

The table reports results from OLS regressions of home mortgage loan value on the indicator for the mandates of the flood risk disclosure law. The sample spans the 1990-2013 period and include 111,233 census tract-year observations. The dependent variable is *Loan-to-value* (Loan value divided by instrumented house price index (Saiz 2010)). *Post* is an indicator variable equal to one if a census tract in a state mandates flood risk disclosure law, and zero otherwise. Control variables are defined in the Appendix. Census tract-fixed effects and year-fixed effects are included. All continuous variables are winsorized to the 1st and 99th percentiles. Standard errors are corrected for heteroskedasticity and clustering at state level (robust standards errors are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

VARIABLES	*Loan-to-value*	*Loan-to-value*
Post	0.196**	0.151**
	(0.061)	(0.050)
Controls	NO	YES
Observations	111,233	111,233
Adjusted R-squared	0.218	0.250
Tract FE	YES	YES
Year FE	YES	YES
Cluster at State	YES	YES

Table 7

Dynamic Effects of Home Mortgage Loan Value Ratio (Instrumented) Around the Mandates of the Flood Risk Disclosure Law

The table reports results from OLS regressions of home mortgage loan value on the indicator for the mandates of the flood risk disclosure law. The sample spans the 1990-2013 period and include 111,233 census tract-year observations. *Effective*$^{-3}$, *Effective*$^{-2}$, *Effective*$^{-1}$, *Effective*^{0}, *Effective*$^{+1}$, *Effective*$^{+2}$, *Effective*$^{+3}$, which are equal to one if the census tract is in a state that will mandate the flood risk disclosure law in two years, will mandate the flood risk disclosure law in two years, will mandate the law in one year, mandates the law, mandated the law in year ago, mandated the law two years ago, mandated the law three or more years ago, respectively, and zero otherwise. The dependent variable is *Loan-to-value* (Loan value divided by instrumented house price index (Saiz 2010)). Control variables are defined in the Appendix. Census tract-fixed effects and year-fixed effects are included. All continuous variables are winsorized to the 1st and 99th percentiles. Standard errors are corrected for heteroskedasticity and clustering at state level (robust standards errors are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

VARIABLES	*Loan-to-value*	*Loan-to-value*
Effective^{-3}	-0.015	-0.033
	(0.027)	(0.030)
Effective^{-2}	0.046	0.017
	(0.053)	(0.044)
Effective^{-1}	0.090	0.054
	(0.049)	(0.042)
Effective^{0}	0.133***	0.081**
	(0.041)	(0.028)
Effective^{+1}	0.158***	0.102**
	(0.045)	(0.036)
Effective^{+2}	0.201***	0.134***
	(0.052)	(0.038)
Effective^{+3}	0.353***	0.281***
	(0.099)	(0.079)
Controls	NO	YES
Observations	111,233	111,233
Adjusted R-squared	0.222	0.253
Tract FE	YES	YES
Year FE	YES	YES
Cluster at State	YES	YES

Table 8

Effects of Flood Risk Disclosure Laws on Home Mortgage Loan Value Ratio in Census Tract with Greater Ex Ante Flood Risk

The table reports results from OLS regressions of home mortgage loan value on the indicator for the mandates of the flood risk disclosure law conditional on census tracts' ex ante flood risk. The sample spans the 1990-2013 period and include 10,066 census tract-year observations for high flood risk census tract and 101,167 census tract-year observations for low flood risk census tract. The dependent variable is *Loan-to-value* (Loan value divided by instrumented house price index (Saiz 2010)). *Post* is an indicator variable equal to one if a census tract in a state mandates flood risk disclosure law, and zero otherwise. SLR is an indicator variable equal to one if six feet or less of SLR would put the property underwater (NOAA SLR viewer; Marcy et al., 2011; Bernstein et al., 2019). Panel A is without controls and panel B is with controls. Control variables are defined in the Appendix. Census tract-fixed effects and year-fixed effects are included. All continuous variables are winsorized to the 1st and 99th percentiles. Standard errors are corrected for heteroskedasticity and clustering at state level (robust standards errors are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	SLR risk=1	*SLR risk=0*	*SLR risk=1*	*SLR risk=0*
VARIABLES	*Loan-to-value*	*Loan-to-value*	*Loan-to-value*	*Loan-to-value*
Post	0.042	0.209***	0.017	0.162***
	(0.056)	(0.057)	(0.053)	(0.045)
	(1)	(2)	(3)	(4)
Test of differences [p-values]	(2)-(1): 0.000		(4)-(3): 0.000	
Controls	NO	NO	YES	YES
Observations	10,066	101,167	10,066	101,167
Adjusted R-squared	0.250	0.215	0.281	0.247
Tract FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Cluster at State	YES	YES	YES	YES

Table 9

Channel: Effects of Flood Risk Disclosure Laws on House Price

The table reports results from OLS regressions of house price index on the indicator for the mandates of the flood risk disclosure law. The sample spans the 1990-2013 period and include 274,885 census tract-year observations. The dependent variable is *Home price index* (Home Price Index from Office of Federal Housing Enterprise Oversight). *Post* is an indicator variable equal to one if a census tract in a state mandates flood risk disclosure law, and zero otherwise. Control variables are defined in the Appendix. Census tract-fixed effects and year-fixed effects are included. All continuous variables are winsorized to the 1st and 99th percentiles. Standard errors are corrected for heteroskedasticity and clustering at state level (robust standards errors are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

VARIABLES	*Home price index*	*Home price index*
Post	52.623**	51.273**
	(17.224)	(17.135)
Controls	NO	YES
Observations	274,885	274,885
Adjusted R-squared	0.863	0.866
Tract FE	YES	YES
Year FE	YES	YES
Cluster at State	YES	YES

Table 10

Channel: Effects of Flood Risk Disclosure Laws on House Price in Census Tract with Greater Ex Ante Flood Risk

The table reports results from OLS regressions of House Price on the indicator for the mandates of the flood risk disclosure law conditional on census tracts' ex ante flood risk. The sample spans the 1990-2013 period and include 30,779 census tract-year observations for high flood risk census tract and 244,106 census tract-year observations for low flood risk census tract. The dependent variable is *Home price index* (Home Price Index from Office of Federal Housing Enterprise Oversight). *Post* is an indicator variable equal to one if a census tract in a state mandates flood risk disclosure law, and zero otherwise. *SLR risk* is an indicator variable equal to one if six feet or less of SLR would put the property underwater (NOAA SLR viewer; Marcy et al., 2011; Bernstein et al., 2019). Control variables are defined in the Appendix. Census tract-fixed effects and year-fixed effects are included. All continuous variables are winsorized to the 1st and 99th percentiles. Standard errors are corrected for heteroskedasticity and clustering at state level (robust standards errors are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	SLR risk=1	*SLR risk=0*	*SLR risk=1*	*SLR risk=0*
VARIABLES	*Home price index*	*Home price index*	*Home price index*	*Home price index*
Post	28.519	56.110***	26.491	54.832***
	(18.510)	(16.938)	(16.642)	(17.004)
	(1)	(2)	(3)	(4)
Test of differences [p-values]	(2)-(1): 0.005		(4)-(3): 0.003	
Controls	NO	NO	YES	YES
Observations	30,779	244,106	30,779	244,106
Adjusted R-squared	0.873	0.862	0.878	0.866
Tract FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Cluster at State	YES	YES	YES	YES

Table 11

Channel: Link House Collateral Value with Mortgage Loan

The table reports results from OLS regressions of home mortgage loan on the indicator for the mandates of the flood risk disclosure law conditional on the change in house collateral value. The sample spans the 1990-2013 period. *Highpost* is an indicator variable equal to one if a census tract in a state mandates flood risk disclosure law and experiences high increase in house collateral value, and zero otherwise. *Lowpost* is an indicator variable equal to one if a census tract in a state mandates flood risk disclosure law and experiences low increase in house collateral value, and zero otherwise. Control variables are defined in the Appendix. Census tract-fixed effects and year-fixed effects are included. All continuous variables are winsorized to the 1st and 99th percentiles. Standard errors are corrected for heteroskedasticity and clustering at state level (robust standards errors are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

VARIABLES	*Loan*	*Ln(loan)*	*Loan growth*	*Loan-to-value*
Highpost	103.942***	0.175***	0.202***	0.133*
	(17.751)	(0.042)	(0.053)	(0.068)
Lowpost	6.383	0.076*	0.071	0.044
	(14.843)	(0.042)	(0.049)	(0.069)
F-test for differences [p-values]	0.000	0.005	0.004	0.028
Controls	YES	YES	YES	YES
Observations	319,758	319,758	319,758	111,233
Adjusted R-squared	0.757	0.806	0.233	0.249
Tract FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Cluster at State	YES	YES	YES	YES

Table 12

Disciplinary Effects of Stricter Flood Risk Disclosure Law on Home Mortgage Loan Value

The table reports results from OLS regressions of home mortgage loan value on the indicator for the mandates of the flood risk disclosure law conditional on the law intensity. The sample spans the 1990-2013 period and include 319,758 census tract-year observations. *Strictpost* is an indicator variable equal to one if a census tract in a state mandates flood risk disclosure law with high *Lawindex*, and zero otherwise. *Weakpost* is an indicator variable equal to one if a census tract in a state mandates flood risk disclosure law with low *Lawindex*, and zero otherwise. *Lawindex* is constructed based on dimensions of flood risk disclosure law intensity: whether a law requires disclosure of (a) flood damage; (b) flood nature and frequency, (c) flood insurance; (d) flood zone classification; (e) flood damage to the property; (f) federal disaster aid coverage; (g) an additional form; (h) drainage and (i) measures to remedy the problem. I assign each one of the above dimensions as value of 1 and calculate the total strictness index by summing them together. I then sort the index into two groups with high *Lawindex* and low *Lawindex*. Control variables are defined in the Appendix. Census tract-fixed effects and year-fixed effects are included. All continuous variables are winsorized to the 1st and 99th percentiles. Standard errors are corrected for heteroskedasticity and clustering at state level (robust standards errors are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

VARIABLES	*Loan*	*Loan*	*Ln(loan)*	*Ln(loan)*	*Loan growth*	*Loan growth*
Strictpost	103.277***	100.661***	0.172***	0.165***	0.230***	0.180**
	(32.814)	(32.046)	(0.052)	(0.051)	(0.072)	(0.063)
Weakpost	24.147*	22.716*	0.098*	0.093*	0.121*	0.101*
	(12.314)	(12.240)	(0.046)	(0.045)	(0.062)	(0.053)
F-test for differences [p-values]	0.016	0.014	0.045	0.051	0.030	0.067
Controls	NO	YES	NO	YES	NO	YES
Observations	319,758	319,758	319,758	319,758	319,758	319,758
Adjusted R-squared	0.752	0.753	0.804	0.805	0.193	0.232
Tract FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
Cluster at State	YES	YES	YES	YES	YES	YES

Table 13

Disciplinary Effects of Stricter Flood Risk Disclosure Law on Home Mortgage Loan Value Ratio

The table reports results from OLS regressions of home mortgage loan value on the indicator for the mandates of the flood risk disclosure law conditional on the law intensity. The sample spans the 1990-2013 period and include 111,233 census tract-year observations. *Strictpost* is an indicator variable equal to one if a census tract in a state mandates flood risk disclosure law with high *Lawindex*, and zero otherwise. *Weakpost* is an indicator variable equal to one if a census tract in a state mandates flood risk disclosure law with low *Lawindex*, and zero otherwise. *Lawindex* is constructed based on dimensions of flood risk disclosure law intensity: whether a law requires disclosure of (a) flood damage; (b) flood nature and frequency, (c) flood insurance; (d) flood zone classification; (e) flood damage to the property; (f) federal disaster aid coverage; (g) an additional form; (h) drainage and (i) measures to remedy the problem. I assign each one of the above dimensions as value of 1 and calculate the total strictness index by summing them together. I then sort the index into two groups with high *Lawindex* and low *Lawindex*. Control variables are defined in the Appendix. Census tract-fixed effects and year-fixed effects are included. All continuous variables are winsorized to the 1st and 99th percentiles. Standard errors are corrected for heteroskedasticity and clustering at state level (robust standards errors are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

VARIABLES	*Loan-to-value*	*Loan-to-value*
Strictpost	0.250***	0.187***
	(0.058)	(0.053)
Weakpost	0.148**	0.120**
	(0.055)	(0.044)
F-test for differences [p-values]	0.021	0.100
Controls	NO	YES
Observations	111,233	111,233
Adjusted R-squared	0.219	0.250
Tract FE	YES	YES
Year FE	YES	YES
Cluster at State	YES	YES

Table 14

Contiguous Border-county Analysis

The table reports results from OLS regressions of home mortgage loan on the indicator for the mandates of the flood risk disclosure law in adjacent counties. County pair-fixed effects and year-fixed effects are included. All continuous variables are winsorized to the 1st and 99th percentiles. Standard errors are corrected for heteroskedasticity and clustering at state-border level and individual state level (robust standards errors are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

VARIABLES	*Loan*	*Ln(loan)*	*Loan growth*	*Loan-to-value*
Post	28.479**	0.070*	0.061	0.119**
	(11.901)	(0.036)	(0.034)	(0.052)
Controls	YES	YES	YES	YES
Observations	98,002	98,002	98,002	22,748
Adjusted R-squared	0.509	0.603	0.265	0.268
County-pair FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Cluster at State	YES	YES	YES	YES
Cluster at State Border	YES	YES	YES	YES

Appendix:

Variable Names	Definitions	**Source**
Variables for main analyses		
Loan	The average amount of mortgage loan originated (thousands of dollars) in census tract i's year t.	*Home Mortgage Disclosure Act*
Ln(loan)	The log amount of average mortgage loans originated (thousands of dollars) in census tract i's year t.	*Home Mortgage Disclosure Act*
Loan growth	The percentage change of mortgage loans originated in census tract i from year t to year t-1.	*Home Mortgage Disclosure Act*
Loan-to-value	The average loan value change originated in census tract i's year t divided by instrumented price index (Saiz 2010).	*Home Mortgage Disclosure Act;* *Office of Federal Housing Enterprise Oversight*
Application	The total number of mortgage loan applications in census tract i's year t.	*Home Mortgage Disclosure Act*
Ln(application)	The log total number of mortgage loan applications in census tract i's year t.	*Home Mortgage Disclosure Act*
Male	The percentage of male in total population in census tract i's year t.	*U.S. Census Bureau*
White	The percentage of white in total population in census tract i's year t.	*U.S. Census Bureau*
Income growth	The percentage change of applicants' annual income in census tract i from year t to year t-1.	*Home Mortgage Disclosure Act;* *U.S. Census Bureau*
House unit	The log of total number of housing units in in census tract i's year t.	*U.S. Census Bureau*
Population	The log of total number of populations in in census tract i's year t.	*U.S. Census Bureau*
Post	An indicator equals one if census tract i's year t is after state j has effectuated a flood risk disclosure law, and zero otherwise.	*Natural Resources Defense Council*
SLR risk	An indicator equals to one if six feet or less of SLR would put the property underwater (NOAA SLR viewer; Marcy et al., 2011; Bernstein et al., 2019).	*National Oceanic and Atmospheric Administration sea level rise shapefile*

Variables for predictive regressions

GDP growth	Gross Domestic Product (GDP) at the state-year level.	*U.S. Bureau of Economic Analysis*
Property tax	Ratio of property taxes to total taxes at the state-year level	*Annual Survey of State and Local Government Finances.*
Income tax	Ratio of individual income taxes to total taxes at the state-year level	*Annual Survey of State and Local Government Finances.*
Population	Population at the state-year level.	*U.S. Bureau of Economic Analysis*
Employment rate	State employment rate in a given year in percentage points.	*U.S. Bureau of Economic Analysis*
Revenue/GDP	Ratio of revenue to Gross Domestic Product at the state-year level	*Annual Survey of State and Local Government Finances.*
Expenditure/GDP	Ratio of expenditure to Gross Domestic Product at the state-year level	*Annual Survey of State and Local Government Finances.*
Debt/GDP	Ratio of debt to Gross Domestic Product at the state-year level.	*Annual Survey of State and Local Government Finances.*
Establishment entry rate	State-year establishment entry rate in percentage points	*Census' Business Dynamics Statistics*
Net job creation rate	state-year job creation rate in percentage points	*Census' Business Dynamics Statistics*
Establishment exit rate	State-year establishment exit rate in percentage points	*Census' Business Dynamics Statistics*
Democratic control	Indicator variable for whether a state is controlled by Democrats.	*National Conference of State Legislatures*
Republican control	Indicator variable for whether a state is controlled by Republicans	*National Conference of State Legislatures*

Online Appendix

Aggregate Spillover Effects of Mandatory Transparency:

Evidence from Flood Risk Disclosure in the U.S.

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Table A1

Channel: Exclude High Increase in Collateral Values

This table reports results from OLS regression of mortgage loan on the indicator for the mandates of the flood risk disclosure law after removing observations with high increase in housing collateral values. Control variables are defined in the Appendix. Census tract-fixed effects and year-fixed effects are included. Standard errors are corrected for heteroskedasticity and clustering at state level (robust standards errors are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

VARIABLES	*Loan*	*Ln(loan)*	*Loan growth*	*Loan-to-value*
Post	22.459	0.073	0.079	0.039
	(14.282)	(0.044)	(0.054)	(0.073)
Controls	YES	YES	YES	YES
Observations	206,137	206,137	206,137	59,191
Adjusted R-squared	0.678	0.728	0.208	0.231
Tract FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Cluster at State	YES	YES	YES	YES

Table A2

Effect of Flood Risk Disclosure Law on Home Mortgage Loan Applications

The table reports results from OLS regressions of home mortgage loan quantity on the indicator for the mandates of the flood risk disclosure law conditional on the law intensity. The sample spans the 1990-2013 period and include 319,758 census tract-year observations. *Strictpost* is an indicator variable equal to one if a census tract in a state mandates flood risk disclosure law with high *Lawindex*, and zero otherwise. *Weakpost* is an indicator variable equal to one if a census tract in a state mandates flood risk disclosure law with low *Lawindex*, and zero otherwise. *Lawindex* is constructed based on dimensions of flood risk disclosure law intensity: whether a law requires disclosure of (a) flood damage; (b) flood nature and frequency, (c) flood insurance; (d) flood zone classification; (e) flood damage to the property; (f) federal disaster aid coverage; (g) an additional form; (h) drainage and (i) measures to remedy the problem. I assign each one of the above dimensions as value of 1 and calculate the total strictness index by summing them together. I then sort the index into two groups with high *Lawindex* and low *Lawindex*. Control variables are defined in the Appendix. Census tract-fixed effects and year-fixed effects are included. All continuous variables are winsorized to the 1st and 99th percentiles. Standard errors are corrected for heteroskedasticity and clustering at state level (robust standards errors are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

VARIABLES	*Application*	*Application*	*Ln(application)*	*Ln(application)*
Strictpost	6.726***	6.089***	0.263***	0.232***
	(1.557)	(1.358)	(0.074)	(0.062)
Weakpost	1.973	1.273	0.122	0.094
	(1.971)	(1.859)	(0.104)	(0.095)
F-test for differences [p-values]	0.024	0.015	0.086	0.080
Controls	NO	YES	NO	YES
Observations	319,758	319,758	319,758	319,758
Adjusted R-squared	0.682	0.701	0.749	0.769
Tract FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Cluster at State	YES	YES	YES	YES

Table A3

Effect of Flood Risk Disclosure Laws on Home Mortgage Loan Applications in Census Tract with Greater Ex Ante Flood Risk

The table reports results from OLS regressions of home mortgage loan quantity on the indicator for the mandates of the flood risk disclosure law conditional on law intensity and census tracts' ex ante flood risk. The sample spans the 1990-2013 period and include 40,707 census tract-year observations for high flood risk census tract and 279,051 census tract-year observations for low flood risk census tract. *Strictpost* is an indicator variable equal to one if a census tract in a state mandates flood risk disclosure law with high *Lawindex*, and zero otherwise. *Weakpost* is an indicator variable equal to one if a census tract in a state mandates flood risk disclosure law with low *Lawindex*, and zero otherwise. *Lawindex* is constructed based on dimensions of flood risk disclosure law intensity: whether a law requires disclosure of (a) flood damage; (b) flood nature and frequency, (c) flood insurance; (d) flood zone classification; (e) flood damage to the property; (f) federal disaster aid coverage; (g) an additional form; (h) drainage and (i) measures to remedy the problem. I assign each one of the above dimensions as value of 1 and calculate the total strictness index by summing them together. I then sort the index into two groups with high *Lawindex* and low *Lawindex*. *SLR risk* is an indicator variable equal to one if six feet or less of SLR would put the property underwater (NOAA SLR viewer; Marcy et al., 2011; Bernstein et al., 2019). Panel A is without controls and panel B is with controls. Control variables are defined in the Appendix. Census tract-fixed effects and year-fixed effects are included. All continuous variables are winsorized to the 1st and 99th percentiles. Standard errors are corrected for heteroskedasticity and clustering at state level (robust standards errors are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Panel A	*SLR risk=1*	*SLR risk=0*	*SLR risk=1*	*SLR risk=0*
VARIABLES	*Application*	*Application*	*Ln(application)*	*Ln(application)*
Strictpost	3.866*	7.283***	0.116*	0.289***
	(2.001)	(1.445)	(0.062)	(0.075)
Weakpost	-0.493	2.379	-0.042	0.150
	(2.739)	(1.901)	(0.068)	(0.112)
	(1)	(2)	(3)	(4)
F-test for differences [p-values]	(2)-(1): 0.064		(4)-(3): 0.032	
Controls	NO	NO	NO	NO
Observations	40,707	279,051	40,707	279,051
Adjusted R-squared	0.673	0.683	0.742	0.751
Tract FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Cluster at State	YES	YES	YES	YES

Panel B	*SLR risk=1*	*SLR risk=0*	*SLR risk=1*	*SLR risk=0*
VARIABLES	*Application*	*Application*	*Ln(application)*	*Ln(application)*
Strictpost	3.649*	6.605***	0.106*	0.256***
	(2.005)	(1.232)	(0.057)	(0.063)
Weakpost	-0.855	1.630	-0.057	0.121
	(2.722)	(1.763)	(0.065)	(0.102)
	(1)	(2)	(3)	(4)
F-test for differences [p-values]	(2)-(1): 0.077		(4)-(3): 0.038	
Controls	YES	YES	YES	YES
Observations	40,707	279,051	40,707	279,051
Adjusted R-squared	0.695	0.702	0.766	0.770
Tract FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Cluster at State	YES	YES	YES	YES

Table A4

Effect of Flood Risk Disclosure Law on Home Mortgage Loan Income Ratio

The table reports results from OLS regressions of home mortgage loan value on the indicator for the mandates of the flood risk disclosure law conditional on the law intensity. The sample spans the 1990-2013 period and include 319,758 census tract-year observations. The dependent variable is *Loan-to-income* (loan value divided by applicants' income). *Strictpost* is an indicator variable equal to one if a census tract in a state mandates flood risk disclosure law with high *Lawindex*, and zero otherwise. *Weakpost* is an indicator variable equal to one if a census tract in a state mandates flood risk disclosure law with low *Lawindex*, and zero otherwise. *Lawindex* is constructed based on dimensions of flood risk disclosure law intensity: whether a law requires disclosure of (a) flood damage; (b) flood nature and frequency, (c) flood insurance; (d) flood zone classification; (e) flood damage to the property; (f) federal disaster aid coverage; (g) an additional form; (h) drainage and (i) measures to remedy the problem. I assign each one of the above dimensions as value of 1 and calculate the total strictness index by summing them together. I then sort the index into two groups with high *Lawindex* and low *Lawindex*. Control variables are defined in the Appendix. Census tract-fixed effects and year-fixed effects are included. All continuous variables are winsorized to the 1st and 99th percentiles. Standard errors are corrected for heteroskedasticity and clustering at state level (robust standards errors are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

VARIABLES	*Loan-to-income*	*Loan-to-income*
Strictpost	0.250*	0.247*
	(0.135)	(0.128)
Weakpost	-0.087	-0.084
	(0.100)	(0.099)
F-test for differences [p-values]	0.054	0.053
Controls	NO	YES
Observations	319,758	319,758
Adjusted R-squared	0.313	0.314
Tract FE	YES	YES
Year FE	YES	YES
Cluster at State	YES	YES

Table A5

Effect of Flood Risk Disclosure Laws on Home Mortgage Loan Income Ratio in Census Tract with Greater Ex Ante Flood Risk

The table reports results from OLS regressions of home mortgage loan value on the indicator for the mandates of the flood risk disclosure law conditional on census tracts' ex ante flood risk. The sample spans the 1990-2013 period and include 40,707 census tract-year observations for high flood risk census tract and 279,051 census tract-year observations for low flood risk census tract. The dependent variable is *Loan-to-income* (loan value divided by applicants' income). *Strictpost* is an indicator variable equal to one if a census tract in a state mandates flood risk disclosure law with high *Lawindex,* and zero otherwise. *Weakpost* is an indicator variable equal to one if a census tract in a state mandates flood risk disclosure law with low *Lawindex,* and zero otherwise. *SLR risk* is an indicator variable equal to one if six feet or less of SLR would put the property underwater (NOAA SLR viewer; Marcy et al., 2011; Bernstein et al., 2019). Control variables are defined in the Appendix. Census tract-fixed effects and year-fixed effects are included. All continuous variables are winsorized to the 1st and 99th percentiles. Standard errors are corrected for heteroskedasticity and clustering at state level (robust standards errors are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	SLR risk=1	*SLR risk=0*	*SLR risk=1*	*SLR risk=0*
VARIABLES	*Loan-to-income*	*Loan-to-income*	*Loan-to-income*	*Loan-to-income*
Strictpost	-0.037	0.296**	-0.039	0.292**
	(0.132)	(0.133)	(0.131)	(0.126)
Weakpost	-0.146	-0.082	-0.139	-0.080
	(0.122)	(0.102)	(0.119)	(0.101)
	(1)	(2)	(3)	(4)
Test of differences [p-values]	(2)-(1): 0.014		(4)-(3): 0.012	
Controls	NO	NO	YES	YES
Observations	40,707	279,051	40,707	279,051
Adjusted R-squared	0.261	0.321	0.262	0.322
Tract FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Cluster at State	YES	YES	YES	YES

Table A6

Effect of Flood Risk Disclosure Laws on House Construction

The table reports results from OLS regressions of counties' new house constructions on the indicator for the mandates of the flood risk disclosure law. The sample spans the 1990-2013 period and include 13,569 county-year observations. *Construction unit* is natural log of the number of new conventional single family properties at county i's year t. *Construction unit* is natural log of the number of new buildings constructed at county i's year t. Control variables are defined in the Appendix. Census tract-fixed effects and year-fixed effects are included. All continuous variables are winsorized to the 1st and 99th percentiles. Standard errors are corrected for heteroskedasticity and clustering at state level (robust standards errors are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

VARIABLES	*Construction unit*	*Construction unit*
Post	0.169**	0.166**
	(0.063)	(0.062)
Controls	YES	YES
Observations	13,569	13,569
Adjusted R-squared	0.942	0.935
County FE	YES	YES
Year FE	YES	YES
Cluster at State	YES	YES

Table A7

Effect of Flood Risk Disclosure Laws on Loan Loss Provisions at Bank Branch Level

The table reports results from OLS regressions of banks' loan loss provisions on the indicator for the mandates of the flood-risk disclosure law. The sample spans the 1990-2013 period and include 158,153 bank-year-quarter observations. Column 1 include the full sample, and column 2 restricts to three years before and three years after the mandates. The dependent variable is *Loan loss provisions* is the ratio of loan loss provisions bank i's year-quarter t scaled by the total assets. *Size* is the natural log of bank i's total assets in year t. *Leverage* is the ratio of assets to book value of equity; *Roa* is the ratio of net income to total assets. *Roe* is the ratio of net income to total equity; *Londep* is the ratio of average balance of loans to average balance of deposits. *Tar* is the ratio of trading assets to total assets. Bank branch-fixed effects and year-quarter-fixed effects are included. All continuous variables are winsorized to the 1st and 99th percentiles. Standard errors are corrected for heteroskedasticity and clustering at state level (robust standards errors are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

VARIABLES	*Loan loss provisions*	*Loan loss provisions*
Post	-0.023*	-0.018**
	(0.012)	(0.008)
Size	0.046***	0.040***
	(0.011)	(0.013)
Leverage	0.003***	0.006***
	(0.001)	(0.001)
Roa	-20.849***	-14.589***
	(1.774)	(1.434)
Roe	-0.009***	-0.008**
	(0.002)	(0.003)
Loandep	0.224***	0.220***
	(0.013)	(0.034)
Tar	0.019***	0.029***
	(0.005)	(0.008)
Observations	158,153	35,601
R-squared	0.524	0.460
Bank Branch FE	YES	YES
Year*Quarter FE	YES	YES
Cluster at State	YES	YES

Table A8

County-level Analysis

The table reports results from OLS regressions of home mortgage loan on the indicator for the mandates of the flood risk disclosure law at county-year level. County fixed effects and year-fixed effects are included. All continuous variables are winsorized to the 1st and 99th percentiles. Standard errors are corrected for heteroskedasticity and clustering at state-border level and individual state level (robust standards errors are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

VARIABLES	*Loan*	*Ln(loan)*	*Loan growth*	*Loan-to-value*
Strictpost	55.772	0.211**	0.055*	0.032**
	(32.630)	(0.081)	(0.026)	(0.014)
Weakpost	14.846	0.082	0.039*	0.008
	(10.008)	(0.055)	(0.021)	(0.017)
F-test for differences [p-values]	0.165	0.013	0.520	0.093
Controls	YES	YES	YES	YES
Observations	13,834	13,834	13,834	1,730
Adjusted R-squared	0.699	0.725	0.065	0.128
County FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Cluster at State	YES	YES	YES	YES

Table A9

Effects of Flood Risk Disclosure Laws on Home Mortgage Loan Value (Excluding State of California)

This table reports results from OLS regression of mortgage loan on the indicator for the mandates of the flood risk disclosure law after removing observations which are in the state of California. Control variables are defined in the Appendix. Census tract-fixed effects and year-fixed effects are included. Standard errors are corrected for heteroskedasticity and clustering at state level (robust standards errors are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

VARIABLES	*Loan*	*Ln(loan)*	*Loan growth*	*Loan-to-value*
Post	19.757*	0.060*	0.077*	0.107*
	(9.282)	(0.030)	(0.041)	(0.057)
Controls	YES	YES	YES	YES
Observations	224,996	224,996	224,996	53,953
Adjusted R-squared	0.698	0.750	0.229	0.254
Tract FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Cluster at State	YES	YES	YES	YES

Mandatory Data Breach Disclosure and Insider Trading

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Mandatory Data Breach Disclosure and Insider Trading

Abstract

Using the staggered adoption of state-level data breach notification laws, we examine whether mandatory breach disclosure affects insider selling. Trading profits are greater after states require firms to disclose data breaches. The effect is concentrated among firms with a greater ex ante breach risk and those that do not increase investment after the passage of law. Firms that are located in states that implement stricter versions of the law and those that are exposed to a higher breach risk increase their cyber-related investment under the new legal regime. This absence of investment leads to an increase in breach risk, which is associated with more idiosyncratic crashes. These crashes and the lack of cyber-related investments are linked to the profitability of insider sales. Our study reveals some negative capital market externalities of mandatory disclosure laws designed to protect customers and citizens at large.

Keywords: Cybersecurity, Data breach, Regulation, Disclosure, Insider trading

JEL Classification Codes: G18; M41; K22; K24

1. Introduction

The 2019 Global Risks Report by the World Economic Forum ranked cyber risk as one of the top 10 risks in terms of both likelihood and impact. Cybersecurity and its related disclosure have become a significant concern for regulators. For instance, the Security Exchange Commission (SEC) formed a Cyber Unit to investigate cyber-related delinquencies in 2017 and updated its guidelines on cyber risk disclosure in 2018. Successful cyberattacks can have a material effect on target firms (e.g., Kamiya et al. 2020). Cyber risk is a multifaceted threat that can include the destruction of physical assets (e.g., Aramco), the theft of intellectual property (e.g., Nortel), or damage to electronic systems (e.g., Maersk). However, the threat of most concern to the public may be massive data losses that violate individual privacy. From 2005 to 2010, data breaches compromised an estimated 350 million records (Shaw 2010). In 2018 alone, 447 million records were compromised.[13] High-profile data leaks, such as those at Equifax or Marriott, make headlines on a regular basis, and regulators have published a slew of legal instruments to address cyber risk and related disclosure issues.[14] In its 2018 guidelines, the SEC specifically mentions that insider trading based on nonpublic information about cyber risk or cyber incidents is prohibited (SEC 2018).[15] Recent high-profile SEC investigations of data breach-related insider trading further highlight this concern. For instance, the SEC charged executives at Equifax with insider trading related to the data breach in 2017.[16]

Given the relevance of this issue, we examine the effect of breach disclosure regulation in the United States (US). There is a long-held agreement among regulators that "Sunlight is

[13] https://www.statista.com/statistics/273550/data-breaches-recorded-in-the-united-states-by-number-of-breaches-and-records-exposed/

[14] For example, the European Union recently issued the General Data Protection Regulation, a wide-ranging data protection legislation.

[15] https://www.sec.gov/rules/interp/2018/33-10459.pdf

[16] https://www.lexology.com/library/detail.aspx?g=902fabbe-fab1-4251-afb2-037f64355c02

said to be the best of disinfectants."[17] Instead of issuing regulations that require certain behaviors, regulators often believe that they can mandate the disclosure of undesired behaviors to achieve the same goals more efficiently (e.g., Fung, Graham, and Weil 2007). However, the findings of academic studies on the effectiveness of this approach are rather mixed, as regulations often induce unintended behaviors or other externalities.[18] For instance, if sellers of consumer goods are required to disclose negative information after testing the quality of their products, they may choose not to run such tests (Matthews and Postlewaite 1985; Shavell 1994). We consider potential spillovers in the financial market from the regulation of personal data by examining the staggered adoption of mandatory data breach disclosure laws (triggered by state-level actions) on insider and firm behavior.

As of 2021, there is no federal law designed specifically for data breach disclosures. However, all states and the District of Columbia implemented data breach regulations between 2003 and 2018. These laws require organizations to notify affected parties (e.g., consumers or employees) of data breaches after the firm discovers them. As the regulation of financial markets is mostly the purview of the federal government, state regulators did not focus on the potential effects of their data breach laws on SEC-regulated matters. Thus, the staggered adoption of plausibly exogenous state laws provides a quasi-experimental setting in which to test the effects of mandatory breach disclosures on insider trading behaviors.

The effect of these laws on insider trading is undetermined ex ante. On the one hand, the mandated data breach disclosure may prompt opportunistic insider trading, particularly opportunistic sales. Existing studies often link the public revelation of bad news to opportunistic insider sales ahead of negative news announcements (e.g. Ke, Huddart, and

[17] This quote is attributed to Justice Louis D. Brandeis (https://www.brandeis.edu/legacyfund/bio.html).
[18] See Benston 1973; Fung, Graham, and Weil 2007; Gao, Wu, and Zimmerman 2009; Leuz and Wysocki 2016; Berger 2011.

Petroni 2003; Dechow, Lawrence, and Ryans 2016; Ryan, Tucker, and Zhou 2016). Thus, it is plausible that mandated data breach disclosures may also increase managers' incentives to sell their shares to avoid potential future losses. Mandating breach disclosures reveals adverse events that may not have surfaced othwerwise. If a manager believes that his or her firm may suffer from a data breach in the future or has already suffered from an undisclosed breach, he or she may sell his or her shares ahead of disclosure to avoid potential losses.

On the other hand, the mandated data breach disclosure may not lead to opportunistic insider trading (nor even to a reduction in insider trading) because the new laws induce additional costs (e.g., reputational or monetary) if a breach is revealed publicly. The additional costs may prompt firms to take preemptive measures to prevent data breaches and mitigate their economic impact if they occur. Under such a scenario, firms may reduce the number of material events that offer an opportunity for insider sales. Further, the additional scrutiny brought by the new regulation may deter managers from engaging in insider trading to exploit their private information on cyber risk.

Using a generalized difference-in-difference (DID) design, we find that insider sales' profits are significantly larger after states implement data breach laws.[19] We verify that the parallel trend test shows no significant difference in selling profits between our treated and control samples in the pretreatment period. Our results hold if we focus on firm headquarters to identify whether the change is affected by a given state's breach law or if we consider the different states in which a firm has significant operations. Our results are also robust to multiple specification checks. For example, they hold if we drop any given state. These findings suggest that the mandated disclosure of data breaches may have the unintended consequence of prompting executives to sell their shares ahead of potential future occurrences, and as a result,

[19] In this context, profits are defined as losses avoided by trades.

of the potential revelation of data breaches. As such, we predict and find that our results are stronger if the ex ante risk of breaches is greater.

We consider three placebo tests to further support our results. First, data breach laws may trigger the revelation of mostly bad news but not good news. This gives insiders incentives to avoid possible future losses by selling their stocks, but it should not affect purchases. Consistent with this view, we find that data breach laws impact insider sales but not insider purchases. Second, we estimate our baseline specification for routine and nonroutine sales separately. Previous studies (e.g., Cohen, Malloy, and Pomorski 2012; Ali and Hirshleifer 2017) report that nonroutine trades capture insiders' opportunistic use of nonpublic information in their trading strategies and earn significantly higher profits for executives. Consistent with this view, the effect of the law on insider selling profits is significant only in the sample of nonroutine sales. Third, financial institutions are covered by a specific federal regulation (i.e., the Gramm–Leach–Bliley Act of 1999) and are thus typically exempt from additional state-level requirements. We expect that data breach laws should not affect executives working in the financial sector.

Finally, we investigate the mechanisms that explain our findings. Prior literature (e.g., Hilary, Segal and Zhang (2016)) find a statistically significant but economically small market reaction around the announcements of all data breaches. In contrast, Kamiya, Kang, Kim and Milidonis Stulz (2020) document an economically significant effect of hacking on firm operations. Importantly, Florackis et al. (2020) find that cyber-risk can be predicted using a textual analysis from 10K filings, suggesting that insiders and possibly savy investors have a long term vision of the risk faced by a firm. In this context, the effect of data breaches on stock prices may be smoothed over long periods of time. As a result, managers from high risk firms may either work on mitigating the underlying risk or engage in insider selling when states mandate disclosures of data breaches. Consistent with this view, we find that firms that are

located in states that implement stricter versions of the breach laws and those that are exposed to a higher breach risk increased their cyber-related investment under the new legal regime.[20] In contrast, we find the effect of disclosure laws on insider trading is concentrated among firms that do not increase investment after the passage of the laws. This absence of incremental investment is associated an increase in revealed data breaches.[21] Firms that have a greater ex ante risk of breaches or suffer from a breach ex post are more likely to also suffer from an idiosyncratic crash. These crashes and the lack of cyber-related investments are linked to the profitability of insider sales.

Our findings make several contributions. First, the SEC is concerned with insiders using nonpublic information to trade on cyber risks, as well as actual data breach incidents. To improve cyber risk disclosure, the SEC issued guidelines in 2011 and 2018. However, a number of senators and house representatives have repeatedly introduced the Data Security and Breach Notification Act as a bill.[22] Our results show that strict breach disclosure laws can have real effects on firm behavior by providing incentives to invest more in cyber security programs. Our results also demonstrate the possible effects of these laws on the behavior of informed agents. Trading on nonpublic information is one of the most significant threats to the SEC's goal of "leveling the playing field" for different investors in the capital market. Our study informs the SEC of how insider traders use cyber-related nonpublic information and how such behavior might be affected by other noncapital market disclosure regulations.

[20] As of 2018, every state has adopted data breach laws, but there are some key differences between states. In particular, the laws vary in regard to their requirements and specificity. We form indexes that rank these laws based on their stringency.

[21] Existing studies demonstrate that public disclosures of data breaches increase substantially after a state implements a data breach law (e.g., Romanosky, Telang, and Acquisti 2011; Ashraf and Sunder 2018).

[22] For instance, one was introduced in 2013 by Sen. John D. Rockefeller, and another was introduced by Rep. Marsha Blackburn in 2015.

Second, previous studies of insider trading have examined the issues: (1) the existence and determinants of insider trading (e.g., Acharya and Johnson 2010; Ke, Huddart, and Petroni 2003; Marin and Olivier 2008; Jin and Kothari 2008; Bernile, Hu, and Tang 2016; Alldredge and Cicero 2015; Gao and Huang 2016; Lin, Sapp, Ulmer, and Parsa 2020),[23] (2) the effects or consequences of insider trading (e.g., Jenter 2005; Ahern 2017; Piotroski and Roulstone 2005), and (3) the disciplinary mechanisms that can restrict insider trading (e.g., Garfinkel 1997; Bettis, Coles, and Lemmon 2000; Lenkey 2014; Jagolinzer, Larcker, and Taylor 2011). Our study is related most closely to the third stream of research. For example, prior studies suggest that opportunistic insider trading is reduced when insider trading regulations are implemented; when firms set restrictions, such as blackout windows, for insider trading; when insiders are required to disclose their trading faster than before; and when the media disseminates the disclosure (e.g., Brochet 2010; Dai, Parwada, and Zhang 2015; Jagolinzer, Larcker, and Taylor 2011). Our findings also indicate that weak legal designs may exacerbate the problems and lead to negative unintended consequences.

The remainder of this study is organized as follows: Section 2 introduces the institutional background, Section 3 discusses the data and descriptive statistics, Section 4 presents the research design and the main empirical results, Section 5 presents additional empirical analysis, and Section 6 concludes the study.

2. Institutional Background

As of 2020, there is no comprehensive US federal law governing the disclosure of data breaches.[24] However, in 2002, California was the first state to adopt a data breach law, which

[23] For example, Lin et al. (2020) finds some insider trading activities 55 to 72 days before the breach announcements. Our study does not focus on disclosed breach events in the post period. Rather, we investigate the impact of the mandatory disclosure regulations itself.

[24] However, firms under the jurisdiction of the SEC or those falling under specific statutes (e.g., Health Insurance Portability and Accountability Act of 1996) may have additional specific requirements.

became effective in 2003. Between 2002 and 2018, all states adopted such laws, which are broadly consistent in their approaches, but vary regarding the establishment of specific provisions. Many of these laws contain provisions pertaining to the definition and coverage of breaches, required notification details, notification timelines, penalties, and enforcement. Aside from some relatively minor variations, the first dimension is largely similar across states. A data breach is generally defined as a situation in which an unauthorized person or entity obtains sensitive information. The breached entity is liable to notify the affected parties of the incident and, in some cases, third parties, such as credit agencies or the Attorney General must also be notified.

Although the content of the notification varies significantly across states, the basic requirement is similar. It typically includes a general description of the incident, such as the date of the breach and the information that was leaked. However, some states may require more detailed disclosures. For instance, California requires that any delay in disclosure caused by a law enforcement request be disclosed, and Florida requires the disclosure of firm policies regarding breaches and their remedies. States can require breached entities to either disclose the incident as soon as possible (e.g., District of Columbia) or to do so before a specific deadline (e.g., no later than 45 days in Ohio). While some states do not specify penalties for violating the law (e.g., Georgia), others do (e.g., Alaska). These penalties vary significantly across states. The last major dimension—enforcement—also varies greatly across states. At one end of the spectrum, some states disallow private rights of action (e.g., Florida), while at the other extreme, some states (e.g., Iowa) specifically require entities to disclose breaches to the Attorney General and allow this office to bring lawsuits against entities that violate the law.

From the regulators' perspective, the *intent of breach disclosure* requirement is to make the incidents publicly known to induce firms to invest more in improving their data protection (e.g., through better cybersecurity) and to allow victims to remedy their situations more swiftly.

The effect on financial markets is not a primary concern, although these laws can have material effects on the integrity of financial markets.

3. Data and Descriptive Statistics

Table 1 summarizes our sample selection procedure. Our initial sample includes the insider transactions of firms listed on the NYSE, AMEX, or NASDAQ that are covered in the Thomson Reuters Insider Filings (Form 4) for the 2000 to 2017 period.[25] The sample begins three years before California implemented its data breach law in 2003 and ends three years after Florida and Kentucky implemented their data breach laws in 2014. Our implementation timeline by state is consistent with Kamiya et al. (2020) and is presented in Appendix A.

The insider transaction data contains insider trading information from directors, officers, and beneficial owners with holdings greater than 10% of a firm's stock. Until August 2002, all of these insider transactions were subject to disclosure requirements, as defined in Section 16 of the Exchange Act of 1934, and subsequently to the requirements of Section 403 of the Sarbanes–Oxley (SOX) Act.[26] Our analyses focus on insiders' open market sales; hence, we exclude option exercises, private transactions, and open-market purchases from our main tests (e.g., Cohen, Malloy, and Pomorski 2012; Dai et al. 2016).

We further limit the sample by requiring that share codes in the CRSP database be 10 or 11, and we exclude the following transactions from the sample: (1) transactions with fewer than 100 shares or those with trading prices less than $2, (2) transactions with traded prices outside the range between the daily low and high prices reported in CRSP, (3) transactions in which the number of shares exceeds the total number outstanding in CRSP, (4) transactions in which the number of shares traded exceeds the total daily trading volume in CRSP, and (5)

[25] Three states (New Mexico, Alabama, and South Dakota) implemented the data breach law in 2017 and 2018.
[26] In a robustness check, we exclude the observations before 2002 and obtain qualitatively similar results.

those involving regulated firms in the financial or utility industries (firms with SIC codes between 6000 and 6999 or between 4900 and 4999; Dai et al. 2016).[27]

These restrictions result in a sample of 31,249 firm-year observations. We combine the initial sample with COMPUSTAT/CRSP data. After merging and deleting observations with missing data, we obtain a final sample of 28,039 firm-year observations.[28]

< INSERT TABLE 1 >

We define our variables in Appendix B. *Sell Profits* is the profitability of insider sales defined as the losses avoided by selling shares. If insiders' trades reflect information already impounded in stock prices, average insider trading profitability should be zero. In contrast, insider trading profitability will be greater than zero when managers trade on their private information. Similar to Skaife, Veenman, and Wangerin (2013), we measure insider trading profits as the one-year buy-and-hold abnormal return on the stock multiplied by the value of the trade (in millions of dollars) multiplied by minus one. This approach allows us to incorporate the effect of trade materiality, whereas focusing on trading intensity alone (i.e., returns) would ignore the predictive ability of this materiality with respect to future stock price performance. We aggregate individual transactions at the firm-year level (to account for the fact that breaches are firm-level incidents).

The descriptive statistics for our main variables are presented in Panel A of Table 2. As shown in the table, 67% (33%) of the firm-year observations in our sample are after (before) the implementation of a data breach law. The mean of *Sell Profits* is around 200,000 dollars. The average *Size* of firms in our sample is 6.7 in market capitalization (which translates to 5,695 million in market capitalization). The mean of stock return volatility is 0.03, and the

[27] Adding utilities back to the sample does not affect our conclusions.
[28] Variations in data requirements across tests lead to different sample sizes in some ancillary tests.

mean *Book-to-Market Ratio* is 0.5. We find that 30% of the observations are from firms reporting a loss (*Loss*=1), and 56% are from firms reporting non-zero research and development (R&D) expenditures (*R&D Dummy*). In general, the magnitude of our variables is consistent with the literature (e.g., Skaife, Veenman, and Wangerin 2013; Chi, Pincus, and Teoh 2014).

The Pearson correlations are presented in Panel B of Table 2. The correlation between our dependent variable (*Sell Profits*) and our variable of interest (*Post*, an indicator variable that takes the value of 1 after the implementation of data breach laws, and 0 otherwise) is negative. At first glance, this suggests that states' breach laws reduce insiders' opportunistic selling behavior. However, previous studies (e.g., Brochet 2010) show that the enforcement of general anti-insider trading provisions and the speed of Form 4 disclosure have increased over time. After controlling for this trend, the univariate correlation between *Post* and *Sell Profits* becomes significantly positive (at the 10% level). The majority of the control variables are significantly correlated with insider selling profits and have the predicted signs.

< INSERT TABLE 2>

4. Research Design and Main Empirical Results

4.1 Impact of data breach laws on insider trading

We use a DID approach to examine how the implementation of data breach laws affects the insider sale behaviors of executives working for firms headquartered in the affected states. Following previous studies (e.g., Bertrand and Mullainathan 2003), we use the following specification:

$$Sell\ Profits_{j,t} = \alpha + \beta_1 Post_{j,t} + \Sigma\beta_2 Controls_{j,t} + \Sigma\beta_3 Firms\ Fixed\ Effects_{j,t} + \Sigma\beta_4 Year\ Fixed\ Effects_{j,t} + \varepsilon_{j,t}.$$

Our variable of interest is *Post.* We include firm- and year-fixed effects. β_1 essentially captures a DID estimator in which the control group is as follows: firms in states that have not yet implemented a data breach law as of year t nor implemented a data breach law effectively prior to year t (e.g., Bertrand and Mullainathan 2003; Klasa et al. 2018). We cluster standard errors by the state in which the headquarters are located because *Post* is a state-level variable (Klasa et al. 2018). Clustering by firm or industry does not affect our results (untabulated).[29]

We control for firm size (*Size*) because Seyhun (1986) finds that insiders buy more in smaller firms and sell more in larger firms, and Lakonishok and Lee (2001) find that insiders trade more profitably in smaller firms. We also control for the book-to-market ratio (*Book-to-Market Ratio*) because prior research shows that insiders trade more actively in low book-to-market firms (e.g., Rozeff and Zaman 1998). Following Brochet (2010), we include a *Loss* indicator variable to control for a firm's financial performance. Following Aboody and Lev (2000), we control for an R&D indicator variable (*R&D Dummy*) because higher R&D intensity may be associated with greater information asymmetry. We also include *Return Volatility*, the standard deviation of daily stock returns over the fiscal year (e.g., Ravina and Sapienza 2010), and *Dividend*, defined as cash dividend scaled by shareholder equity, to control for growth opportunities (e.g., Chi, Pincus, and Teoh 2014). Following Kallunki et al. (2018), we use firm-fixed effects to control for the effects of possible omitted time-invariant, firm-specific factors that affect insider trading, and we include yearly indicator variables to control for time-specific effects.

The results of our main regression are reported in Panel A of Table 3. The coefficient for *Post* is positive and significant (p-value<0.01), indicating that the implementation of the data breach laws has an impact on the sales behaviors of insiders. The results are robust to

[29] Our untabulated results can be found in our Internet Appendix at https://docdro.id/MqVrGvL.

controlling for industry*year-fixed effects. The control variables generally have the predicted signs.

Next, we investigate the parallel trend assumption embedded in our DID design. Although the assumption is theoretically untestable, Roberts and Whited (2013) suggested a parallel trend test which can provide supporting evidence for the assumption. Essentially, we investigate when the changes in insider selling behavior occur relative to the implementation of the data breach laws. To this end, we create a series of indicator variables, *Effective* indexed t+i from t-2 to t+2, with t=0 being the year of implementation of a data breach law in the state in which the firm is headquartered. The variables take the value of 1 if a law is passed within t-i. We report the results in Panel B of Table 3. We find that the coefficients on $Effective^{-2}$ and $Effective^{-1}$ are statistically insignificant, whereas the coefficients on $Effective^{0}$, $Effective^{+1}$, and $Effective^{+2}$ are all consistently positive and statistically significant. Overall, these results support the validity of the parallel trend assumption in our setting.

< INSERT TABLE 3>

Next, we investigate whether firms that are expected (ex ante) to be more affected by the law are indeed more affected ex post. To do this, we create two new variables—*Relevance* and *BreachRisk* —that measure the firm's exposure to technology risk. We define *Relevance* as an indicator variable that takes the value of 1 if the Information Technology Officer (i.e., Chief Technology Officer, Chief Information Officer, or Chief Security Officer) is among the top management team tracked by ExecuComp, and 0 otherwise. This is the case for approximately 27% of our sample (4,613 out of 16,918 based on available data from ExecuComp).[30] We define *Breach Risk*, a binary variable, in the following way. We first identify a list of data breach disclosures in year t from the Private Rights Clearinghouse

[30] We remove firms that are not covered by Execucomp for the tests that involve *Relevance* .

database.[31] We identify peer firms based on Hoberg and Phillips (2016)'s products similarity measure (calibrated at the three-digit SIC code level). For a given year, we classify firms that have been breached in a window starting 3 years before and ending three years after as well as their peer firms as high risk firms (*Breach Risk* = 1). We classify the remaining firms as low risk firms (*Breach Risk* = 0). We report the results of this analysis in Table 4. Consistent with our expectations, the results indicate that the effect of the law on insider trading is concentrated among firms for which technology is an important issue and those that suffer from a greater ex ante risk of being breached.

< INSERT TABLE 4>

4.2. Placebo tests

We conduct different placebo tests to support our main findings. First, we consider routine and nonroutine trades separately. We expect the effect of the laws to be concentrated among nonroutine trades, as these are more likely to capture information-based opportunistic transactions. To test this conjecture, we follow previous studies (e.g., Cohen, Malloy, and Pomorski 2012). We categorize an insider as a routine trader if he or she has been trading in the same month for at least the past three consecutive years. We categorize the other insiders who sell in the period under consideration as opportunistic (i.e., nonroutine) traders. We then aggregate insider trading profits at the firm-year level. The results reported in column 1 of Panel A of Table 5 show that, as expected, the laws do not affect routine transactions (the untabulated results show that *Post* continues to be significant in the sample of opportunistic trades).

[31] See http://www.privacyrights.org/data-breach

Second, both significant data breach risk and actual data breaches are bad news for firms. If insiders possess related nonpublic information and trade based on this, it should prompt insiders to sell their shares to avoid loss. The same rationale does not apply to insider purchases, as it is unlikely that the laws will generate unexpected good news for firms. Thus, we expect that state breach laws should not affect insider purchases. The results reported in column 2 of Panel A of Table 5 confirm this intuition, showing that breach laws have no significant impact on insider purchases.

Finally, the Gramm–Leach–Bliley Act of 1999 created a federal regulation that addresses data protection requirements for financial institutions. As this federal law predated the state breach laws, financial institutions are typically exempted from state law coverage. Thus, we expect that these laws should not affect insider trading behaviors among financial institutions. The results reported in column 3 of Panel A of Table 5 confirm this prediction.

4.3 Robustness tests

We also conduct a series of robustness checks to validate our main findings. First, we restrict our definition of insiders to officers and directors, excluding large shareholders. Arguably, officers and directors might have firsthand information about their firm. Thus, they may better assess the risk of future breaches, the occurrence of extant breaches, and their possible consequences. This may not be the case for large shareholders (i.e., other "external" corporate "insiders," as defined by Thomson Reuters). Our untabulated results indicate that our conclusions hold if we limit our observations to officers and directors.

Second, there may be a concern that a specific state and its idiosyncrasies generate our results. For example, California is the first state to adopt breach laws and is home to many data-driven firms. One concern could be that our results are driven by these firms. Our untabulated

results indicate that our conclusions hold if we exclude California, and, indeed, any given state, from our tests.

Third, the SOX Act, enacted by Congress in 2002, substantially modified the corporate environment and disclosure requirements in regard to insider trading (our main sample starts in 2000). In particular, Section 403 requires insiders to report their trades to the SEC on Form 4 within two business days, which significantly increases the timeliness of insider trading reports. Until August 2002, the requirement was to file the form within 10 days of the close of the calendar month in which the transaction had occurred. Our untabulated results indicate that our conclusions hold if we restrict our sample to the post-SOX era.

Fourth, we use firm year–level data to indicate that breaches are occurring at that level. Consequently, we use the effective year rather than the effective date of state laws in our main specification because most controls and cross-sectional partitioning variables are at the firm-year level. To investigate whether this finer partition affects our results, we also estimate our baseline model at the trade level, thus allowing us to use the effective date of the law to measure the treatment period. Our untabulated results indicate that our conclusions hold.

Finally, our baseline model uses the location of the headquarters (identified from the firm's 10K reports) to determine whether a firm has been affected by a data breach law.[32] We adopted this design choice for two reasons. First, courts often favor the law of the state in which the headquarters are located. Second, most state laws require disclosures only when the number of affected records exceeds certain thresholds in the state (e.g., Jones 2014; Steinmeyer and Freeman 2016). It is more probable for these thresholds to be exceeded in the states in which the data are likely to be stored. To mitigate the potential effect of this assumption, we use

[32] Our main results hold if we use the location from Compustat instead of the one reported in the 10K reports.

subsidiary data from Exhibit 21 to identify firms' business locations.[33] These data capture the states and countries in which a firm discloses material subsidiaries, as required by the SEC.[34] In this alternative specification, we use the first affected state, instead of the headquarter state, as our identification strategy.

To validate our approach, we identify a list of data breaches using the Private Rights Clearinghouse's Chronology of Data Breaches (e.g., "Privacy Database," hereafter, Romanosky, Hoffman, and Acquisti 2014).[35] We merge this list with our sample of firms for which we have an Exhibit 21 and insider trading data. In support of our approach, close to two-thirds of the breaches occurred in the state in which the firms are headquartered, and the vast majority (80%) of the breaches happened in a state referenced in an Exhibit 21. Further, the average number of leaked records is 10 times larger if the breach occurs in a state that is flagged in Exhibit 21 compared with breaches in unflagged states.

We then replicate our baseline specification using an alternative definition of *Post*. We define *Post 21* as an indicator variable equal to 1 if the firm's headquarters or a material subsidiary are located in a state that has implemented the data breach law, and 0 otherwise. The results are reported in the first column of Panel B of Table 5

Consistent with the idea that the location of the headquarters plays a disproportionate role, our baseline finding continues to hold but is weaker in this alternative approach. Next, we create a variable that better captures the importance of the laws for a firm. To this end, we define a continuous variable—*Post 21 Weight*—as the proportion of affected states' population

[33] The SEC requires firms to disclose the name and jurisdiction of incorporation for all significant subsidiaries locations in Exhibit 21 of the Form 10-K, providing the most granular publicly available disclosure of the company's operations.
[34] Item 601 of SEC Regulation S-K (§229.601), cited as 17 CFR 229.601(b) (21), requires that registrants list all of their significant subsidiaries in Exhibit 21 of their 10K filling. The data are available until 2014.
[35] The data can be found at http://www.privacyrights.org/data-breach. Sources for the database include the Open Security Foundation listserve, Databreaches.net, Personal Health Information Privacy, National Association for Information Destruction, and the California Attorney General.

over all operating states' population. The denominator is the sum of the population of all states in which a firm reports material subsidiaries. The numerator is the population in states that have already implemented the laws and in which the firm reports material subsidiaries. The results reported in column two indicate that our baseline finding continues to hold.

< INSERT TABLE 5>

5. Channels

5.1 Cyber-investment.

One intended objective of data breach notification laws is to encourage firms to improve their cybersecurity posture and thus to reduce cyber-related risks. We expect this to be true when firms have greater incentives to correct the underlying risk. We next investigate whether firms increase cyber investment once a data breach law has been passed. We use SEC filings and press releases to capture firms' investments in cybersecurity. Appendix C lists the specific keywords we use.[36]

To measure the legal incentives to engage in this investment, we measure the strictness of law. As discussed in Section II, data breach laws vary in their stringency and specificity. The strictness of breach laws can have a differential impact on insiders' incentives or opportunities to avoid losses stemming from the disclosure of data breaches (e.g., Meyer and Rowan 1977; Fung, Graham, and Weil 2007). Building on past research (e.g., Joerling 2010; Peters 2014; Romanosky, Teland, and Acquisti 2011; Shaw 2010), we consider four key dimensions: (a) whether a law explicitly allows enforcement by the state Attorney General, (b) whether a law imposes an explicit deadline by which firms must disclose a data breach after it has been discovered, (c) whether a law specifies explicit penalties for violating it, and (d) whether a law

[36] We assume that a firm continues to engage in a material cyber-investment in the three years following its announcement.

specifies the disclosure items in details.[37] We first create an indicator for the different categories and assign a value of 1 to each indicator when the relevant condition is met (0 otherwise). We then construct an index (*Law Index*) by totaling the four indicators. We then create two indicator variables, *Strict Post* (equals 1 if *Post* equals 1 and *Law Index* is equal to or greater than the median, and 0 otherwise) and *Weak Post* (equals 1 if *Post* equals 1 and *Law Index* is lower than the median, and 0 otherwise).

Column 1 of Panel A of Table 6 shows that data breach laws increase cyber investments, but the effect is significant only for firms that are located in states with more stringent versions. We find no similar effect for those located in states with less stringent laws. This result suggests that firms covered by strong laws internalize the cost of privacy breaches and respond by improving their security posture. This was the intended objective of these laws.

Furthermore, we investigate whether firms that have greater exposure to breach risk are more likely to increase their cyber investments under the new regime. We expect that these firms are more sensitive to the potential revelation of future breaches. As a result, they should have higher incentives to improve their cyber security hoping to reduce future breach incidents and related news disclosures. As expected, results presented in columns (2) and (3) show that firms with greater breach risk (captured by *Post Breach Risk* and *Post Relevance*) have more cyber investment after the implementation of the breach laws. Finally, the last column shows that that trading profits are higher for firms that do not increase their cyber-investment after the passage of the laws.

< INSERT TABLE 6>

[37] Some of the laws have been amended since their original implementation. To avoid adding noise and confounding factors, we focus on data breach laws at their first effective dates and in the forms in which they are first implemented, rather than their amended editions.

5.2. Channels

We then connect our different findings by investigating the associations between cyber investment, (ex ante) breach risk, (ex post) breaches, price skewness, and insider trading profits.

We report the results of this analysis in Panel B of Table 6. In columns 1 and 2, we use *Breach* as defined above (i.e., those identified in the Private Rights Clearinghouse database). For these tests, we restrict the sample to the post-2005 period, as the data on breaches are not available prior to that year. Breaches measure the manifestations of realized risk that impact prices, but we note that changes in risk perception—for example, if peer firms are breached—can have a similar effect. We continue to use *Relevance* and *Breach Risk* to measure the ex ante risk. We investigate the effect of (ex post) breaches and of the (ex ante) risk on prices by considering the skewness of the distribution.[38] To this end, we define *Ncskew* as the third moment of firm-specific weekly returns for each sample year (multiplied by minus one) and divided by the standard deviation of firm-specific weekly returns raised to the third power (e.g., Chen, Hong, and Stein 2001). Finally, we connect sudden drops in prices (measured by the distribution's skewness) with insider trading profit.

Our results in Column 1 indicate that the likelihood of a breach occurring is higher when firms do not announce an increase in their cyber investment. Columns 2, 3, and 4 indicate that breaches and breach risk affect returns and lead to idiosyncratic crashes (this result is consistent with Florackis et al. (2020) who find that 10K discussions of breach risk is associated with a higher likelihood of future idiosyncratic crashes), which in turn translate into trading opportunities (column 5). Untabulated results indicate that *Relevance* is significantly

[38] We do not include firm-fixed effects when *Relevance* and *Breach Risk* are the dependent variables as these variables are slow moving. We do include them when we consider *Breach*.

associated with the presence of breaches (*Breach Risk* is mechanically correlated with *Breach*). As discussed above, Table 4 shows that trading profitability is higher when the firm-level breach risk is higher in the cross-section while Florackis et al. (2020) show that breach risk affects prices in the time series. Taken together, these findings suggest that state-level breach regulations influence managers' trading behavior through their effect on firms' cyber investments and return distributions.

6. Conclusions

The growing frequency and size of data breaches in recent years have increased regulators' concerns about information security and disclosure. Recent cases of data breaches followed by insider trading on related nonpublic information have also raised concerns that insiders might trade on their private information about cyber risks and data breaches (e.g., Equifax). Using the staggered adoption of data breach laws across different states, we test whether the mandated disclosure of bad news affects insiders' selling behavior. Our findings indicate that mandated data breach disclosures have prompted insiders to sell their shares to avoid future losses, likely due to the fear that breach disclosures will put downward pressure on stock prices. Firms that are located in states in which the laws are relatively stricter have experienced an increase in cyber security investment. In essence, these different results suggest that strong laws incentivize firms to take corrective actions to minimize the risk of data leakages. However, these disclosure laws were not designed to enhance the functioning of financial markets but, rather, to help consumers and citizens to better handle the perils of data breaches. Interestingly, they had some negative consequences on the integrity of financial markets.

The paper's findings have several caveats. Firstly, the alternative explanation could be that an increase in trade secret protection in the U.S. may also will reduce firms' proprietary costs of information leakage and thus incentivizes firms to integrate, distribute, and share

information internally (Andreicovici, Bormann and Hombach, 2021). Firms with higher internal information transparency and investment in integrated entreprise systems (ES) might lead to better chance to exploit their information advantage, thus more opportunities of insider selling behaviors. The adoption of trade secret law in the U.S. concentrate on the period from 1964 through 2006, and the adoption of data breach law spans from 2003 through 2018. The mandates of the latter might be correlated with the former. Secondly, we cannot rule out the possibility that data breach may affect peer firms with the same industry or the industry as a whole. This could result in spillover effects of disclosures in the industries across states, influencing the Stable Unit Treatment Value Assumption (SUTVA) (Armstrong and Kepler, 2018). We did a robustness check (untabulated results) by including industry level (peer firms) insider selling profits as an additional control variable to capture time-varying industry level insiders' sale behaviors. Including it does not affect our main findings. Thirdly, it is plausible that states in which more technology firms are located are more likely to mandate the data breach notification laws. To alleviate the concern that adoption of laws might be endogenously driven by one specific state, we drop one state at a time (especially the state of California). We observe the results still hold, though we acknowledge that the evidence is limited.

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Appendix A: Time Distribution of the Implementation of Data Breach Notification Laws by State

Effective Year	*States*
2003	CA
2004	
2005	WA, AR, DE, GA, NY, NC, ND, TN
2006	WI, MN, MT, PA, PR, RI, OH, CO, CT, AZ, ID, IL, IN, NE, NV, NJ, LA, ME
2007	WY, DC, MA, MI, NH, HI, OR, UT, KS
2008	IA, OK, MD, WV,VA
2009	AK, MO, TX, SC
2010	
2011	MS
2012	VT
2013	
2014	FL, KY
2015	
2016	
2017	NM
2018	AL, SD

This table displays the year in which each state originally implemented a data breach law.

Appendix B: Variable Definitions

Variables	Descriptions
Sell Profits	Market-adjusted (CRSP value-weighted index as market portfolio) abnormal return over 12 months following the trade, multiplied by the value of trade (in millions of dollars). We multiply this value by -1.
Post	Equals 1 if firm i's year t is after firm i's home state j has implemented a data breach law, and 0 otherwise.
Post 21	Equals 1 if the firm's headquarters or material subsidiaries are located in a state that mandates the disclosure of data breaches, and 0 otherwise.
Post 21 Weight	A continuous variable that uses the proportion of affected states' population over all operating states' population to measure the coverage of disclosure laws.
Size	Natural log of firm i's market value of equity in the year over which trading is measured.
Book-to-Market Ratio	The book value of equity divided by market capitalization.
Loss	Equals 1 if a firm reports negative net income in year t, and 0 otherwise.
R&D Dummy	Equals 1 if a firm has positive R&D expenses, and 0 otherwise.
Dividend	Cash dividends scaled by shareholders' equity (SEQ).
Return Volatility	The standard deviation of daily stock returns during the fiscal year.
Relevance	Equals to 1 if firm has a chief technology officer, chief information officer, chief security officer, chief information security officer, in the top management team, and 0 otherwise.
Breach Risk	Equals to 1 if the firm is facing a high ex ante risk of databreach, and 0 otherwise. We first identify a list of data breach disclosures in year t from http://www.privacyrights.org/data-breach. We then find each data breach firm's peer firm in data breach disclosure year window [-3, 3] based on Hoberg and Phillips (2016)'s products similarity measure (calibrated to be as granular as three-digit SIC codes). We classify data breach firms and their peer firms as high data breach risk firms in year t, and rest firms as low risk firms.
Law Index	Constructed based on four dimensions of data breach disclosure law intensity: (a) whether a law requires the firm to notify the Attorney General and allows the Attorney General to bring law suits, (b) whether a law imposes an explicit deadline by which firms must disclose a data breach after it has been discovered, (c) whether a law specifies explicit penalties for violating it, and (d) whether a law specifies the details of the disclosure items. We assign each of the above

	dimensions a value of 1 or 0 and total the four indicator variables.
Strict Post	Equals 1 if *Post* equals 1 and *Law Index* is equal to or greater than the median value, and 0 otherwise.
Weak Post	Equals 1 if *Post* equals 1 and *Law Index* is smaller than the median value, and 0 otherwise.
Post Breach Risk	Equals to 1 if *Post* equals 1 and *Breach Risk* equals to 1, and 0 otherwise.
Post Relevance	Equals to 1 if *Post* equals 1 and *Relevance* equals to 1, and 0 otherwise.
Post no Breach Risk	Equals to 1 if *Post* equals 1 and *Breach Risk* equals to 0, and 0 otherwise.
Post no Relevance	Equals to 1 if *Post* equals 1 and *Relevance* equals to 0, and 0 otherwise.
Ncskew	The negative of the third moment of firm-specific weekly returns for each sample year divided by the standard deviation of firm-specific weekly returns raised to the third power (Chen, Hong, and Stein 2001; Kim, Li, and Zhang 2011).
Cyberinvest	Equals 1 for year t to t+2 if a firm announces that it invests in a cyber security-related program in year t, and 0 otherwise.
No Cyberinvest	Equals 1 for year t to t+2 if a firm *does not* announce that it invests in a cyber security-related program in year t, and 0 otherwise.
Post No Invest	Equals to 1 if a firm is headquartered in a state that effectuates the data breach disclosure law but does not increase to invest in cyber security-related issues after the mandates, and 0 otherwise.
Post Invest	Equals to 1 if a firm is headquartered in a state which effectuates the data breach disclosure law and invest in cyber security-related issues after the mandates, and 0 otherwise.
Breach	Equals 1 if a firm experienced a data breach incident in year t+1, and 0 otherwise
Post High Ncskew	Equals 1 if a firm is headquartered in a state that effectuates the data breach disclosure law and experiences a large increase in negative stock price skewness, and 0 otherwise.
Post Low Ncskew	Equals 1 if a firm is headquartered in a state that effectuates the data breach disclosure law and experiences a small increase in negative stock price skewness, and 0 otherwise.

Appendix C

To identify material investment programs in cybersecurity, we use the following search terms: invest in data protection, investment in data protection, investing in data protection, data protection investment, invest in data security, investment in data security, investing in data security, data security investment, invest in cybersecurity, investment in cybersecurity, investing in cybersecurity, cybersecurity investment, invest in cyber, investment in cyber, investing in cyber, cyber investment, invest in hack, investment in hack, investing in hack, hack investment, invest in data breach, investment in data breach, investing in data breach, data breach investment, invest in protecting data, investment in protecting data, investing in protecting data, protecting data investment, invest in protecting information, investment in protecting information, investing in protecting information, protecting information investment, invest in internet security, investment in internet security, investing in internet security, and protecting internet security.

Table 1

Insider Trading Sample Selection Process

Description	**Obs.**
Thomson Reuters Insider Trading Database Form 4 open market sales and aggregates at firm-year level (2000–2017)	31,249
Less: Merge with COMPUSTAT	(1,425)
Less: Observations in "NM," "AL" and "SD" states and missing headquarters from 10-K reports	(757)
Less: Missing control variables	(1,028)
Total firm-year observations	28,039
Total number of unique firms	4,892

Table 2
Descriptive Statistics and Correlations

Panel A of Table 2 A reports the summary statistics for the key variables for the full sample for the 2000 to 2017 period. Panel B presents the Pearson correlations, with the correlation coefficients with a significance level of 0.05 or better in bold. All the continuous variables are winsorized to the 1st and 99th percentiles of their distributions. *Sell Profits* is the market-adjusted (CRSP value-weighted index as market portfolio) abnormal return over the 12 months following the trade, multiplied by the value of the trade (in millions of dollars). This value is multiplied by -1 so that the losses avoided on sales have a positive sign. *Post* is an indicator variable equal to 1 if the firm is headquartered in a state that implements the data breach law, and 0 otherwise. *Loss* is an indicator variable that is equal to 1 if a firm reports a negative net income in year t. *R&D Dummy* is an indicator variable that is equal to 1 if a firm has positive R&D expenses. *Book-to-Market Ratio* is the book value of equity divided by market pitalizations. *Size* is the natural log of firm i's market value of equity in the year over which trading is measured. *Dividend* is cash dividend scaled by SEQ. *Return Volatility* is the standard deviation of daily stock returns over the fiscal year.

Panel A: Descriptive Statistics for Insider Trading Sample

Variables	N	Mean	Std. Dev.	Q1	Median	Q3
Test Variable						
Post	28,039	0.675	0.468	0.000	1.000	1.000
Dependent Variables						
Sell Profits	28,039	0.205	5.039	-0.102	0.023	0.408
Control Variables						
Loss	28,039	0.292	0.455	0.000	0.000	1.000
R&D Dummy	28,039	0.565	0.496	0.000	1.000	1.000
Book-to-Market Ratio	28,039	0.476	0.413	0.222	0.394	0.641
Size	28,039	6.672	1.851	5.429	6.603	7.835
Dividend	28,039	0.025	0.066	0.000	0.000	0.023
Return Volatility	28,039	0.032	0.017	0.020	0.027	0.039

Panel B: Pearson Correlation Coefficients (n = 28,039)

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(1) Sell Profits	1.000							
(2) Post	**-0.044**	1.000						
(3) Loss	**0.034**	**-0.038**	1.000					
(4) R&D Dummy	**0.015**	**0.074**	**0.172**	1.000				
(5) Book-to-Market Ratio	**0.014**	**-0.092**	**0.064**	**-0.158**	1.000			
(6) Size	-0.006	**0.201**	**-0.308**	0.009	**-0.339**	1.000		
(7) Dividend	**-0.014**	**0.068**	**-0.142**	**-0.043**	**-0.153**	**0.220**	1.000	
(8) Return Volatility	**0.085**	**-0.269**	**0.435**	**0.112**	**0.129**	**-0.500**	**-0.188**	1.000

Table 3
Effect of Data Breach Laws on Insiders' Selling Profits

This table reports the results of the ordinary least square (OLS) regressions of insiders' trading behaviors on the indicator for the implementation of the data breach law. Panel A reports the main effect, Panel B reports the results of the parallel trend analysis. *Sell Profits* is the market-adjusted (CRSP value-weighted index as market portfolio) abnormal return over the 12 months following the trade, multiplied by the value of the trade (in millions of dollars). This value is multiplied by -1 so that the loss avoided on sales has a positive sign. *Post* is an indicator variable that is equal to 1 if the firm is headquartered in a state that has implemented the data breach law, and 0 otherwise. $Effective^{-2}$, $Effective^{-1}$, $Effective^{0}$, $Effective^{+1}$, and $Effective^{+2}$ are equal to 1 if the firm is headquartered in a state that will implement the data breach law in two years, will implement the law in one year, is implementing the law, implemented the law one year ago, implemented the law two or more years ago, respectively, and 0 otherwise. *Loss, R&D Dummy, Book-to-Market Ratio, Size, Dividend, and Return Volatility* are defined in Appendix B. Firm-fixed and year-fixed effects are included. All the continuous variables are winsorized to the 1st and 99th percentiles. Standard errors are corrected for heteroskedasticity and clustering at the state level (robust standard errors are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Main Effect of the Breach Laws

Variables	*Sell Profits*
Post	0.282***
	(0.104)
Loss	0.202***
	(0.069)
R&D Dummy	-0.057
	(0.186)
Book-to-Market Ratio	0.735***
	(0.116)
Size	0.869***
	(0.198)
Dividend	-0.014
	(0.695)
Return Volatility	33.198***
	(8.058)
Observations	28,039
R-squared	0.202
Firm FE	YES
Year FE	YES
Cluster at State	YES

Panel B: Parallel Trend Analysis

Variables	*Sell profits*
Effective^{-2}	0.248
	(0.184)
Effective^{-1}	0.095
	(0.248)
Effective^{0}	0.373**
	(0.175)
Effective^{+1}	0.426**
	(0.166)
Effective^{+2}	0.499**
	(0.211)
Loss	0.203***
	(0.070)
R&D Dummy	-0.060
	(0.184)
Book-to-Market Ratio	0.733***
	(0.116)
Size	0.868***
	(0.199)
Dividend	-0.017
	(0.695)
Return Volatility	33.054***
	(8.175)
Observations	28,039
R-squared	0.202
Firm FE	YES
Year FE	YES
Cluster at State	YES

Table 4
Ex Ante Data Breach Risk

This table reports the results of the ordinary least square (OLS) regressions of insiders' trading behaviors on the indicator for the implementation of the data breach law, conditional on firms' ex ante data breach risk. *Relevance* is equal to 1 if firm has a chief technology officer, chief information officer, chief security officer, chief information security officer, in the top management team, 0 otherwise. *Breach Risk* is an indicator variable equal to 1 if the firm is facing a high ex ante risk of data breach, 0 otherwise. A firm is classified as such if it has been breached in years in a window encompassing the prior three years and the subsequent three years or one of its peers has been breached. *Loss, R&D Dummy, Book-to-Market Ratio, Size, Dividend, and Return Volatility* are defined in Appendix B. Firm-fixed and year-fixed effects are included. All the continuous variables are winsorized to the 1st and 99th percentiles. Standard errors are corrected for heteroskedasticity and clustering at the state level (robust standard errors are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Relevance =1	*Relevance =0*	*Breach Risk=1*	*Breach Risk=0*
	Sell Profits	*Sell Profits*	*Sell Profits*	*Sell Profits*
Post	0.928***	0.099	0.576***	0.145
	(0.286)	(0.143)	(0.197)	(0.138)
Loss	0.131	0.261**	-0.018	0.330**
	(0.139)	(0.109)	(0.122)	(0.138)
R&D Dummy	-0.249	0.136	-0.148	-0.083
	(0.595)	(0.225)	(0.504)	(0.197)
Book-to-market Ratio	1.077**	1.063***	0.623***	0.844***
	(0.438)	(0.247)	(0.162)	(0.227)
Size	1.364***	0.993***	0.600***	1.050***
	(0.302)	(0.256)	(0.115)	(0.375)
Dividend	-0.018	0.654	0.506	-0.204
	(1.173)	(1.145)	(0.878)	(0.826)
Return Volatility	82.405***	38.647***	15.802***	32.862***
	(27.469)	(8.703)	(5.313)	(10.622)
	(1)	(2)	(1)	(2)
P-value: (1)-(2)	0.018		0.024	
Observations	4,613	12,305	13,522	14,517
R-squared	0.277	0.151	0.217	0.334
Firm FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Cluster at State	YES	YES	YES	YES

Table 5

Placebo and Robustness Tests

Panel A of Table 5 reports the results of the OLS regressions of our baseline model for routine sales, insider purchases, and financial institutions. Routine insiders are those who have traded in the same month for at least the past three consecutive years, and the remainder are nonroutine insiders. The second column reports the results of the OLS regression of *Buy Profits* on indicators for the mandates of states' data breach laws. *Buy Profits* is the market-adjusted (CRSP value-weighted index as market portfolio) abnormal return over 12 months following the trade multiplied by the value of trade (in millions of dollars). In Panel B of Table 4, we use Exhibit 21 to identify firms' material subsidiaries and the data available until 2014. *Post 21* is an indicator variable that is equal to 1 if the firm's headquarters or material subsidiaries are located in a state that mandates the disclosure of data breaches, and 0 otherwise. *Post 21 Weight* is a continuous variable that uses the proportion of affected states' population over all operating states' population to measure the coverage of disclosure laws. All the continuous variables are winsorized at the 1st and 99th percentiles. Standard errors are corrected for heteroskedasticity and clustering at the state level (robust standard errors are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Routine Sales, Insider Purchase, and Financial Institutions

Variables	*Routine Sell Profits*	*Purchase Buy Profits*	*Financial Institutions' Sell Profits*
Post	0.129	-0.007	-0.076
	(0.167)	(0.007)	(0.283)
Loss	0.056	-0.022**	0.034
	(0.206)	(0.009)	(0.348)
R&D Dummy	-0.319	-0.024	-0.654***
	(0.309)	(0.020)	(0.082)
Book-to-Market Ratio	0.785***	0.000**	
	(0.215)	(0.000)	
Size	0.524***	-0.006	0.010
	(0.119)	(0.006)	(0.115)
Dividend	0.136	-0.008	0.941
	(1.282)	(0.006)	(1.057)
Return Volatility	16.320	0.133	2.336
	(10.918)	(0.214)	(2.926)
Observations	5,348	17,629	1,861
R-squared	0.260	0.250	0.274
Firm FE	YES	YES	YES
Year FE	YES	YES	YES
Cluster at State	YES	YES	YES

Panel B: Identification Using Subsidiary Data from Exhibit 21

Variables	*Sell Profits*	*Sell Profits*
Post 21	0.103**	
	(0.043)	
Post 21 Weight		0.540**
		(0.205)
Loss	0.172**	0.160**
	(0.070)	(0.072)
R&D Dummy	0.123	0.130
	(0.285)	(0.289)
Book-to-Market Ratio	0.632***	0.629***
	(0.108)	(0.115)
Size	0.889***	0.885***
	(0.190)	(0.189)
Dividend	0.794	0.767
	(0.576)	(0.592)
Return Volatility	31.215***	31.059***
	(7.768)	(7.767)
Observations	19,845	19,845
R-squared	0.253	0.254
Firm FE	YES	YES
Year FE	YES	YES
Cluster at State	YES	YES

Table 6

Channels

Panel A: Effects of Data Breach Laws and Breach Risk on Cyber Investment

This table reports the results of the OLS regressions of *Cyberinvest* on the indicators of the timing of the state's implementations of the data breach law. *Law Index* is constructed based on the following four dimensions of data breach disclosure law intensity: (a) whether a law requires the firm to notify the Attorney General and allows the Attorney General to bring law suits, (b) whether a law imposes an explicit deadline by which firms must disclose a data breach after it has been discovered, (c) whether a law specifies explicit penalties for violating it, and (d) whether a law specifies the details of the disclosure items. We assign the value of 1 or 0 to each of these dimensions and total these four indicator variables. *Strict Post* equals 1 if *Post* equals 1 and *Law Index* is equal to or greater than their medians, and 0 otherwise. Conversely, *Weak Post* equals 1 if *Post* equals 1 and *Law Index* is smaller than the median value, and 0 otherwise. *Cyberinvest* Equals 1 for year t to t+2 if a firm announces that it invests in a cyber security-related program in year t, and 0 otherwise. *Post Breach Risk* is is an indicator variable equal to one if *Post* equals 1 and *Breach Risk* equals to 1, 0 otherwise. *Post no Breach Risk* is is an indicator variable equal to 1 if *Post* equals 1 and *Breach Risk* equals to 0, and 0 otherwise. *Post Relevance* is is an indicator variable equal to one if *Post* equals 1 and *Relevance* equals to one, zero otherwise. *Post no Relevance* is is an indicator variable equal to 1 if *Post* equals 1 and *Relevance* equals to 0, and 0 otherwise. *Post No Invest* is an indicator variable equal to one if *Post* equals 1 but does not increase to invest in cyber security-related issues after the mandates, and 0 otherwise. *Post Invest* is an indicator variable equal to one if *Post* equals 1 and invest in cyber security-related issues after the mandates, and 0 otherwise. All the continuous variables are winsorized at the 1st and 99th percentiles. Standard errors are corrected for heteroskedasticity and clustering at the state level (robust standard errors are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Variables	*Cyberinvest*	*Cyberinvest*	*Cyberinvest*	*Sell Profits*
Strict Post	0.010*			
	(0.006)			
Weak Post	0.002			
	(0.006)			
Post Breach Risk		0.018***		
		(0.005)		
Post Relevance			0.022*	
			(0.012)	
Post no Breach Risk		-0.002		
		(0.006)		
Post no Relevance			0.004	
			(0.006)	
Post No Invest				0.355***
				(0.116)

Post Invest				-0.239
				(0.329)
Loss	0.001	0.001	-0.001	0.200***
	(0.003)	(0.003)	(0.004)	(0.069)
R&D Dummy	-0.020**	-0.020**	-0.032**	-0.065
	(0.008)	(0.008)	(0.012)	(0.186)
Book-to-Market Ratio	0.000	-0.000	0.002	0.735***
	(0.006)	(0.006)	(0.010)	(0.116)
Size	-0.004	-0.004	-0.007	0.869***
	(0.003)	(0.003)	(0.006)	(0.198)
Dividend	0.088**	0.088**	0.123*	0.049
	(0.043)	(0.043)	(0.062)	(0.711)
Return Volatility	-0.084	-0.060	-0.165	33.016***
	(0.130)	(0.126)	(0.215)	(8.042)
Observations	28,039	28,039	16,918	28,039
R-squared	0.533	0.533	0.483	0.202
Firm FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Cluster at State	YES	YES	YES	YES

Panel B: Cyber Investment, Data Breach, Crash Risk, and Insider Selling Profits

This table reports results from OLS regressions. We examine the association between cyber-related investments, data breach, price skewness, and insider selling profits under states. The sample period for columns 1 and 2 is from 2005 to 2017 because breach incidents data are available from 2005 only. We define *Ncskew* as the third moment of firm-specific weekly returns for each sample year (multiplied by minus 1) divided by the standard deviation of firm-specific weekly returns raised to the third power. *No Cyberinvest* equals 1 for year t to t+2 if a firm does not announce that it invests in a cyber security-related program in year t, and 0 otherwise. *Breach* equals 1 if a firm experienced a data breach incident in year t+1, and 0 otherwise. *Relevance* is equal to 1 if firm has a chief technology officer, chief information officer, chief security officer, chief information security officer, in the top management team, 0 otherwise. *Breach Risk* is an indicator variable equal to 1 if the firm is facing a high ex ante risk of databreach, 0 otherwise. *Post High Ncskew* equals 1 if a firm is headquartered in a state that effectuates the data breach disclosure law and experiences a significant increase in negative stock price skewness, and 0 otherwise. *Post Low Ncskew* equals 1 if a firm is headquartered in a state that effectuates the data breach disclosure law and experiences a small increase in negative stock price skewness, and 0 otherwise. We correct standard errors for heteroskedasticity and clustering at the year and firm levels from column 1 to column 4, and state level for column 5 (robust standards errors are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Variables	*Breach*	*Ncskew*	*Ncskew*	*Ncskew*	*Sell Profits*
No Cyberinvest	0.018**				
	(0.009)				
Breach		0.191***			
		(0.067)			
Relevance			0.033**		
			(0.017)		
Breach Risk				0.049***	
				(0.013)	
Post High Ncskew					0.542***
					(0.168)
Post Low Ncskew					-0.002
					(0.112)
Loss	0.000	0.025	0.005	0.076***	0.200***
	(0.002)	(0.025)	(0.021)	(0.015)	(0.068)
R&D Dummy	-0.002	-0.036	-0.021	-0.002	-0.079
	(0.001)	(0.064)	(0.015)	(0.012)	(0.185)
Book-to-Market Ratio	0.001	0.242***	0.122***	0.109***	0.725***
	(0.004)	(0.037)	(0.024)	(0.016)	(0.114)
Size	-0.001	-0.128***	0.006	0.023***	0.859***
	(0.002)	(0.020)	(0.005)	(0.004)	(0.195)
Dividend	-0.016	0.190	0.307***	0.042	-0.012
	(0.023)	(0.144)	(0.103)	(0.081)	(0.707)

Return Volatility	-0.087	-3.631***	4.183***	0.085	32.987***
	(0.083)	(1.399)	(0.854)	(0.619)	(7.878)
Observations	20,752	20,752	16,918	28,039	28,039
R-squared	0.241	0.197	0.013	0.012	0.202
Firm FE	YES	YES	NO	NO	YES
Year FE	YES	YES	YES	YES	YES
Cluster Firm/State	YES	YES	YES	YES	YES

When one just is not enough: The joint effect of ESG disclosure and ESG incentives on firm value

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When one just is not enough:

The joint effect of ESG disclosure and ESG incentives on firm value

Abstract

We study the economic benefits, in terms of firm value, deriving from combining corporate Environmental, Social and Governance (ESG) disclosure with ESG incentives in the context of Integrated Reporting (IR). The existing literature points at the risk of a de-coupling between 'substance' and 'symbol': on a stand-alone basis, ESG disclosure may be pure rhetoric. The same is also true for ESG incentives that *per se* may simply represent a greenwashing tactic. Based on these premises and drawing on complementarity theory, we contend that the combined adoption of both ESG disclosure and ESG incentives helps firms avoid their merely symbolic use and leads to performance benefits in terms of enhanced value creation. By means of a manual content analysis, we collect data on ESG incentives, as exhibited in CEO compensation packages, for both IR adopters and a matched sample of non-adopters from 2013 to 2018. Indeed, our results show that the combination of an IR with ESG incentives improves firms' value measured as *ex-post* realized operating cash flows in the short (one-year ahead), medium (two-year ahead) and long (three-year ahead) run. We also suggest that the specific characteristics of ESG incentives (time-orientation, completeness and specificity) matter. Therefore, not only the presence of such incentives, but also how firms design them is relevant in order to achieve the value creation benefits stemming from coupling ESG incentives with ESG disclosure.

Keywords: ESG disclosure; Integrated Reporting; ESG Incentives; complementarity; value creation.

1. Introduction

ESG (Environmental, Social and Governance) reporting seeks to promote behavioral change by requiring firms to disclose sustainable activities and how they influence the future returns of the company. Ideally, this would make managers accountable for longer-term value creation instead of myopically prioritizing short-term financial targets. However, ESG disclosure is the locus of companies' greenwashing and opportunistic behaviors as highlighted by prior research (e.g., Cho et al., 2010), corporate scandals (cf. Volkswagen Diesel Gate 2015) and, more recently, in a speech by Hans Hoogervorst, the Chair of the International Accounting Standards Body (IASB) who warned that "we should not have exaggerated expectations about sustainability reporting as an agent for change as greenwashing is rampant".[40] The scandals across the globe force regulators to pay closer attention to ownership and shareholder rights. The European Council adopted Shareholder Rights Directive (SRD II) in June 2017 and implemented in national legislations from 2018 through 2020. The key promotion of the directives is to improve both the transparency of shareholder engagement, including proxy voting, and support to adopt more ESG targets in executives' compensation plans (PWC, 2019). Understanding the value implication of those ESG practices help generate practical implications for regulators.

Starting from this debate, in this paper, we study the effects of combining corporate ESG disclosure with CEO ESG incentives. Indeed, complementarity theory states that organizational design choices can be interdependent and they can be complements or substitutes (Grabner & Moers, 2013). This implies that the effectiveness of one practice (i.e., ESG disclosure) depends on the use of another practice. Grabner, Renders, and Yang (2018)

[40] Speech, IASB Chair "What sustainability reporting can and cannot achieve", https://www.ifrs.org/news-and-events/2019/04/speech-iasb-chair-on-sustainability-reporting/, 2 April 2019.

examine the complementarity between Corporate Social Responsibility (CSR)[41] disclosure and CSR contracting, arguing that these are two complementary strategies employed by companies to demonstrate their CSR orientation to their stakeholders. Empirically, focusing on 2013, the authors find that CSR disclosure and CSR contracting are complementary and their combined use is driven by credibility concerns of CSR-related activities. We take a step forward and argue that the joint adoption of both ESG disclosure and ESG incentives may lead to performance effects and, in particular, generate positive future cash flows, thus contributing to firms' value creation. The presence of both ESG reporting and incentives may strengthen managers' long term orientation and accountability as well as decrease the relative acquisition costs of ESG information and improve the process of identifying value adding ESG initiatives.

In arguing this, we combine two streams of literature focusing on disclosure and on incentives. Existing contributions point at the risk of a potential de-coupling between substance and symbol with reference to both disclosure practices and executive compensation, especially with regard to ESG, nonfinancial issues. On a stand-alone basis, ESG disclosure may represent pure rhetoric which is just aimed at managing public impressions (e.g. Arena, Bozzolan, & Michelon, 2015; Cho, Roberts, & Patten, 2010; Melloni, Caglio, & Perego, 2017; Michelon, Pilonato, & Ricceri, 2015; Plumlee, Brown, Hayes, & Marshall, 2015). Likewise, the inclusion of ESG objectives together with financial ones in managerial incentives *per se* may be driven by institutional pressures for legitimacy (Westphal and Graebner, 2010; Westphal and Zajac, 1994; 1998; Zajac and Westphal, 1995) thus being merely symbolic (Berrone & Gomez-Mejia, 2009) and even deleterious for firm value creation (e.g., Bebchuk et al., 2002). In this paper, we contend that the joint adoption of ESG incentives and ESG disclosure helps firms avoid their merely symbolic use: for ESG disclosed metrics to become effective, they need to be

[41] In line with other contributions, in this paper, we use the term CSR and ESG interchangeably.

integrated in the spans of accountability of top managers through the use of proper incentive schemes. At the same time, ESG objectives become substantive if firms communicate the engagement of managers to stakeholders and explain through ESG disclosure how changes in ESG performance relate to changes in a firm's overall performance and to the sustainability of value creation processes over time.

For the purposes of our analysis, Integrated Reporting (IR) offers a proper institutional setting. The idea underlying our study is well-suited to the claims of the IR framework (2013) endorsed by the International Integrated Reporting Council (IIRC) and currently adopted by more than two thousand global firms. In brief, IR is an innovative form of corporate reporting that combines financial and ESG disclosure in one single document (i.e., it is an integrated financial and ESG form of disclosure). Maintaining that the purpose of IR is to explain to providers of financial capital how an organization creates value over time by means of a broad range of capitals (financial, human, intellectual, social, manufactured, natural), the IIRC expects managers' decision making processes to improve as a result of the adoption of an IR by a firm (IIRC, 2013). An IR can be leveraged upon as a performance measurement tool to monitor and understand all the multifaceted dimensions related to sustainable initiatives, including their link with financial outcomes (Busco et al., 2013). For this reason, the IIRC claims that an IR is beneficial not only in terms of improved transparency for external users but also in terms of enhanced internal decision-making and initiatives that sustain value creation over time (IIRC, 2013). Such claims are also supported by previous studies on IR suggesting that whereas traditional financial reports serve mainly an 'information function', IR may serve a 'transformation function' (Eccles and Serafeim, 2015). An IR can affect internal decision making due to its ability to represent the interconnections between financial and nonfinancial resources and performances, between different components of a business model and external factors, as well as between a firm's interrelated financial and nonfinancial capitals

on which, ultimately, the performance of the firm itself depends. The IR setting is particularly suitable for our study also because the IR Framework (IIRC, 2013) explicitly posits a link between corporate disclosure and firm internal management practices, including incentives (IIRC, 2013; Barth et al., 2017). Firms can use IR and ESG incentives in combination to escape executive short termism and managerial myopia and to focus their efforts on sustainable value creation.

Starting from these premises, our research analyzes the value creation effects stemming from the combination of Integrated Reporting and ESG incentives as exhibited in CEO compensation packages. By means of a manual content analysis of the IRs recognized by the IIRC, we collect data from 2013 to 2018 on CEO incentives as detailed in compensation package. In particular, we analyze the type of targets that they should reach (financial vs. ESG) and their time frame (short-term vs. medium/long-term). Furthermore, we collect information on ESG target completeness (the specific capitals that they are associated with) and the specificity of non-numeric targets (whether a target is identified in specific terms, e.g., Scope 2 emissions, or not, e.g., environmental performance). We do so for both IR adopters and a matched sample of non-adopters.

Our results show that the joint adoption of an IR and the inclusion of ESG incentives in CEO compensation packages improves firm value measured as *ex post* realized operating cash flows in the short (one-year ahead), medium (two-year ahead) and long (three-year ahead) run. Interestingly, we show that IR in isolation is associated with lower *ex post* realized operating cash flows, thus confirming the importance of combining ESG disclosure with complementary managerial practices reinforcing such a choice. Overall, our evidence suggests that only IR adopters also employing ESG incentives for their CEOs are able to improve their internal decision making processes and their firm value creation. We also find that the time orientation of ESG targets matters, as only the combination of IR with short-term ESG

incentives is positively associated with firm value suggesting that, when it comes to nonfinancial matters (that are more uncertain in nature) managers are conceivably more engaged by more immediate rewards. Moreover, in the additional analysis, we also illustrate that additional characteristics of ESG incentives (i.e., completeness and specificity) matter. Therefore, it is not only the presence of ESG incentives, but also how firms design them that is relevant in order to achieve the value creation benefits of coupling them with ESG disclosure. Our results are robust to alternative measures of firm value (i.e., Return on Assets and Tobin's Q). Overall, we show that the value creation implication of IR disclosure is not obvious and is determined by dominant managerial incentives in the firm's specific conditions (Gao & Sidhu, 2018; Wen, 2013).

We contribute to the existing literature in the following ways. First, adding to Grabner et al. (2018), we empirically test the value creation implications over a more extended time frame of the joint use of ESG practices over time (short and long term). We therefore lend support to Grabner & Moers (2013) pointing at the importance of studying the performance effects of a combination of management practices. Thirdly, we add more broadly to the extensive literature on agency and contracting pointing to the fact that when the preferences of shareholders and/or other stakeholders are *a priori* misaligned with the incentives of corporate managers (Jensen & Meckling, 1976), there is a need for both compensation contracts and accounting disclosure to alleviate problems of moral hazard and adverse selection (Kanodia & Sapra, 2016) to better exploit investment opportunities (Jayaraman & Wu, 2019) and more generally to obtain economic benefits. Our contribution also adds to the compensation literature on the link between sustainability incentives and firm-level financial outcomes and to the more general discussion on the role and use of nonfinancial targets in CEO compensation packages (e.g., O'Connell & O'Sullivan, 2014). Fourthly, we expand the extant accounting literature on the benefits of disclosure and on IR specifically, shedding light on the importance of coherently

combining disclosure and managerial incentives (i.e., ESG target) to foster value creation (Barth et al., 2017). As far as we know, there is also a lack of research on the implementation of both IR and ESG incentives, despite the calls for contributions in this area by both academics and practitioners (e.g., Busco et al. 2013; Serafeim 2015; Forbes 2015; Accounting for sustainability, 2014). Finally, we contribute to the debate raised by previous studies highlighting that disclosure can be detrimental in economic terms (Gao & Sidhu, 2018; Wen, 2013) if used symbolically in implementing impression management strategies (e.g. Melloni et al., 2017).

The paper is structured as follows: Section II presents the theoretical background of the study. Section III describes the sample and research design. Section IV illustrates our results and Section V provides robustness checks and additional analyses. Section VI concludes.

2. Theoretical background

2.1. Literature review

ESG disclosure

Neo-institutional theory points at the existence of "decoupling", that is, maintaining the gap between formal policies and tools that are ceremonially adopted by companies and actual organizational practices (Meyer & Rowan, 1977). There is growing interest in this concept and in symbolic management, as organizations are facing "increasing emphases on accountability and transparency" in their external environments (Bromley & Powell, 2012).

Corporate disclosure is a type of communication of information by people inside public firms towards people outside. To make substantive communication, such disclosure should include information about corporate social and environmental policies (Healy & Palepu, 2001) and intangible assets such as R&D, human capital, information technology, and brand equity, which are not considered as assets, but are highly relevant to firms (Kanodia & Sapra, 2016).

Failure to recognize such initiatives and resources could reduce the amount of information in the capital market, thus contributing to price inefficiency and poor resource allocation (Kanodia & Sapra, 2016). Nonetheless, previous contributors point out that broad, nonfinancial disclosure can be employed to "facilitate the construction of a new and different image of the company" and improve its legitimacy in the wider world (Hopwood, 2009; Melloni et al., 2017).

When it comes specifically to the reporting of ESG information, symbolic management is related to the theoretical foundations tracing back to organizational legitimation theorized by Ashforth & Gibbs (1990). Organization legitimacy is often established and maintained through the use of symbolic actions, which in turn form part of the organization's public image (Dowling & Pfeffer, 1975; Elsbach, 1994; Neu et al., 1998). The literature conceptually posits that managers do not use ESG disclosure as substantive communication to stakeholders but they rather self-servingly use it to manipulate the perceived image of their firm. Michelon et al. (2015) uncover the symbolic role of stand-alone CSR reporting, aimed at positively influencing stakeholders' perceptions, creating an appearance of concern – independently from whether it is real or not – that does not translate into actual performance. Corporate managers manipulate CSR disclosure to positively influence stakeholders' perceptions, and lead key stakeholders to mistakenly believe that the company is committed to societal expectations. Similarly, Cho et al. (2010) argue that CSR disclosure, as a means of communication, is exploited as an opportunity for greenwashing, to camouflage corporate activities, obfuscate negative performance and to protect a corporate image decoupled from reality. Managers deliberately obfuscate failures and emphasize success (symbolic enhancement) to improve their reputation or alter users' perceptions of corporate achievements to convince stakeholders to accept the management's view of society (Cho et al., 2012; Cho & Patten, 2007). Consistent with these ideas, Jones (2011) shows that companies from high environmental impact industries tend to apply a selective inclusion strategy in the graphs displayed in corporate

sustainability reports. In a similar vein, Cho et al. (2012) confirm both the presence of image enhancement and obfuscation in this type of graph. With specific reference to the IR setting, Melloni et al. (2017) find that IR early adopters manipulate disclosure through quantity and syntactical reading ease as well as thematic content and verbal tone depending on the level and type of firm performance.

Overall, the findings above provide evidence of the symbolic use of ESG disclosure, which is decoupled from the actual management practices and decision-making processes of the company. How to avoid using ESG disclosure as a merely symbolic tool and rather as a tool to redirect managerial attention towards value creation and strengthen the accountability to stakeholders is a question that still remains largely unanswered.

ESG incentives

In the compensation literature, the recent integration of sustainability criteria in the design of executive incentives has been labelled as 'CSR (corporate social responsibility) contracting' or 'pay for environmental, social and governance (ESG) performance' as opposed to the traditional 'pay for (financial) performance' (Flammer et al., 2019). Specifically, board members have started to incentivize senior executives to achieve CSR targets by tying their compensation to the firm's ESG issues, thus making them formally accountable for these outcomes. While the adoption of an incentive plan may signal to managers that the board of directors considers sustainability as an important issue, it could be merely symbolic: the degree of substance of such a plan plays an essential role in whether it really shapes managers' decision-making and ultimately affects value creation (Flammer et al., 2019). As such, the governance mechanism of incentives lacks substance and may be ineffective.

The symbolic management hypothesis could eventually contribute to explain why empirical assessments of the association between ESG incentives and firm performance

indicate ambiguous results, including positive, negative, U-shaped, or even inverse-U-shaped links (Eccles, Ioannou, & Serafeim, 2014; Margolis & Walsh, 2003). The link between ESG incentives and firm performance may be difficult to prove because the integration of nonfinancial targets in CEO compensation schemes may simply signal an appeal on the part of firms to use popular, 'modern' management practices as a heuristic for managerial effectiveness without taking into consideration their effects (Connolly et al., 1981; Staw & Epstein, 2000).

Doubts have been also raised over whether the inclusion of sustainability in CEO compensation schemes is in shareholders' best interests and can bring about economic benefits. On the contrary, this type of target might be used by powerful CEOs to 'camouflage' pay–performance sensitivity (Bebchuk et al., 2002; Bebchuk & Fried, 2003, 2004). As Staw & Epstein (2000) suggest, CEOs may be rewarded for pursuing CSR initiatives and targets regardless of their economic consequences. If managers decide to engage in CSR activities because of reputational considerations, then some of those activities may be undertaken at the expense of the shareholders (Benabou & Tirole, 2010; Moser & Martin, 2012). In this sense, sustainability may be a type of agency cost: managers receive private benefits from incorporating sustainability policies in their strategies yet with negative financial consequences for their firms (Balotti & Hanks, 1999; Brown et al., 2006). In addition, executives may 'push' to have compensation plans based on measures that cannot be thoroughly assessed and verified, such as sustainability metrics.

Finally, it should be noted that previous contributors have mainly focused on the link between sustainability incentives and firms' ESG performance while little is still known about how they might affect firm-level financial outcomes. Scholars argue that adopting ESG incentives can destroy shareholder wealth (e.g. Friedman, 1970; Galaskiewicz, 1997; Navarro, 1988; Eccles et al., 2014). In a brief form, the argument is that it pays to be sustainable and

sustainability may simply be a type of agency cost or structure cost (e.g., paying their employees living wages rather than the market wages). One notable exception is the recent paper by Flammer et al. (2019) who document that the adoption of CSR contracting leads to an increase in long-term orientation and firm value. This issue relates to the more general debate on the role and use of nonfinancial targets in the context of CEO compensation (O'Connell & O'Sullivan, 2014) and on whether non-financial metrics can be really considered by managers in their decision making processes as leading indicators of future financial performance and long-term firm value creation (e.g., Banker et al. 1996; Kaplan and Norton 1992; Levinthal 2011).

All in all, the link between ESG incentives and a firm's financial performance is a crucial and controversial question that is still partially unaddressed. Similarly, how to avoid the symbolic use of ESG incentives and contribute to firm value creation remains unclear.

2.2. Hypotheses development

To build our predictions, while we refer to the existing literature identifying a potential separation between substance and symbol in both disclosure and executive compensation practices, we get inspiration from complementarity theory. Complementarity theory states that organizational design choices can be interdependent and they can be complements or substitutes (Grabner & Moers, 2013). This implies that the value of one practice depends on the use of another practice. From an empirical point of view, there are two approaches that rely on the same type of "systems fit", either examining the performance effects of a combination of a management control practice and context, or a congruence approach, examining the association between management control practices. Grabner et al. (2018) examine the latter issue and document the existence of complementarity between CSR disclosure and CSR contracting. However, the value creation performance effects, of such complementarity have not been investigated yet, although whether and how the combined use of ESG incentives and

disclosure practices affect firm value creation is a fundamental question in terms of practical implications.

We thus take a step forward and argue that the joint use of both ESG disclosure and ESG incentives indicates a more substantive use of both practices: ESG disclosure (and related metrics) come into effect if they are integrated in the spans of accountability of managers through the use of proper incentive schemes. At the same time, ESG targets become substantive as firms communicate the engagement and accountability of managers to stakeholders and explain through proper disclosure how changes in ESG performance relate to changes in a firm's overall performance and relate to the sustainability of value creation processes over time. The combined use of ESG disclosure and incentives strengthens the ESG orientation of managers, decreases the relative acquisition costs of ESG information and improves manager success in identifying those ESG initiatives that also generate positive future cash flows, thus contributing to firm value creation. This is in line with Flammer et al. (2019) who maintain that the positive effects of CSR contracting on long-term value depend on the substance of implementation, although these authors refer to the substance degree of CSR incentives only as the extent of CSR compensation compared to total compensation. We especially focus on real effects (cash flows) aspects of firm value creation since they are less subject to manipulation than accounting numbers (Barth et al., 2017).

IR offers a proper institutional setting to study the performance effects of ESG disclosure and incentives. On the one hand, the IIRC explicitly claims that an IR is beneficial not only in terms of improved transparency for external users but also in terms of enhanced internal decision-making processes (Eccles & Serafeim, 2015). One of the potential benefits of IR touted by the IIRC is that its use can lead to breaking down functional silos, making better connections between financial and non-financial aspects of firm performance, and focusing managers' attention on long-term instead of on simply short-term strategies, leading to better

real decisions and enhanced firm value (Barth et al., 2017). According to the IIRC (2020), IR should help redirect managerial attention to integrated multiple stakeholder and capital management (i.e., integrated thinking) and improving the quality of decisions and, therefore, value creation over time. As a matter of fact, many IR adopters seek to improve value creation over the short, medium and long term tying together management practices based on a broader understanding of the resources they use and manage. Rather than narrowly focusing on financial tools, such organizations base their business decisions on interconnected information across multiple capitals, including natural, social and relationship, human, manufactured and intellectual. IR could thus reduce the costs of implementing ESG incentives and increase the success of engaging in ESG initiatives that can generate positive future cash flow. This is consistent with IR and ESG incentives to facilitate integrated thinking whereby managers recognize and are motivated to take into account interconnections between various types of resources and parts of the firm, enabling them to make better decisions and investment choices. On the other hand, to make IR's implementation substantive, it is necessary for ESG incentives to become an integral part of a firm's objective function, as reflected by CEO incentive contracts. The disclosed ESG metrics become more credible and operational when they are integrated in the spans of accountability of managers through the use of proper incentive schemes. Hence, we formulate the following hypothesis:

> *The joint use of ESG disclosure and ESG incentives as exhibited in the CEO compensation package is associated with higher firm value.*

To obtain a more nuanced picture of ESG incentives and to better understand the interplay between how ESG incentives are designed and the use of an IR we do not limit our analysis to the presence of such incentives, but we look in detail at their characteristics. In particular, we analyze the time frame (short-term vs. medium/long-term) and ESG target

completeness and specificity. Further details about such variables are provided in the next section.

3. Research design

3.1. Sample

We start with the population sample of IR adopters recognized by the IIRC[42] and we build a matched sample of IR non-adopters. Peers were identified based on three criteria: industry, size and geographical area. To capture information on CEO incentives, we collect data from 2013 to 2018 based on a manual content analysis of the corporate reports available. We collected compensation variables from four types of corporate reports — integrated reports, annual reports, governance reports or proxy statements (for US firms), and sustainability reports— downloaded from firm websites[43]. We then merged our manually collected data with data from Compustat Global, Datastream and Thomson Reuter Eikon (now called Refinitiv).

The final sample consists of 425 non-missing firm-year observations across the period 2013-2018. Table 1 summarizes our sample compositions and Panel A shows that our data are generally evenly distributed across our sample period. IR adopters come from thirteen countries with 20% of the firms from South Africa, where IR has been mandatory since 2010 for listed firms on a comply or explain basis (King III Report, 2009); the rest are voluntary IR adopters[44]. Table 1, Panel B presents the composition of the sample by industry, where the utilities and

[42] Available on http://examples.integratedreporting.org/reporters?start=A&page=1

[43] We started searching in IRs. If nothing was found in IRs then we moved to the other documents. In some cases, compensation information is scattered in multiple reports, so we combined the information. We keep track of the reports we found compensation information in.

[44] In the main analysis, we include a variable that is equal to one if IR adoption is non mandatory, zero otherwise (i.e., South African listed firms) to control for voluntary (vs. mandatory) adoption of IR. Consistent with prior studies we maintain both groups in the sample to increase the number of observations (Melloni, Caglio, & Perego, 2017). We further use this variable as an instrument to consider the endogenous choices of IR adoption and ESG incentives. In a robustness test, we also remove all the mandatory adopters and obtain similar results.

retail industries have the largest number of observations, respectively 13% and 16% of the sample.

< INSERT TABLE 1 >

3.2. Model

We argue that the joint use of IR and ESG incentives helps firms avoid symbolic communication, strengthen the ESG orientation and improve the success in identifying ESG initiatives that generate positive future cash flows, thereby ultimately contributing to firm value creation. We thus test whether the effect of one control practice (IR) on value creation increases with the use of another practice (CEO ESG incentives). In doing so, we examine the performance effects stemming from IR and CEO ESG incentives and estimate the following general fixed effects equation:

$$Operating\ Cash\ Flows_{j,t} = \alpha + \beta_1 IR_{j,t} + \beta_2 ESG\_Incentive_{j,t} + \beta_3 ESG\_Incentive\#IR_{j,t} + \Sigma\beta_n Controls_{j,t} + \Sigma\beta_i Industry_{j,t} + \Sigma\beta_m Year_{j,t} + \varepsilon_{j,t} \quad (1)$$

where $Operating\ Cash\ Flows_{j,t}$ is our proxy for value creation as it represents the ex-post realized operating cash flows (j and t denote firm and year). We employ several different proxies to capture the short-term, mid-term and long-term net cash flows from operating activities (see next section). $IR_{j,t}$ is a dummy variable denoting whether a company is an IR adopter or not. We include both industry fixed effects and year fixed effects in the model. To account for dependence across firms within the same country, we cluster standard errors at the country level.[45] As we are specifically interested in the interplay between IR and ESG incentives, our variable of interest is the coefficient of the interaction term, β_3, denoting the

[45] As we illustrate in the following, we also tried different clustering schemes as robustness checks (e.g., we cluster standard errors at firm level and industry level). We obtain qualitatively similar results (untabulated) as in our main analyses.

incremental impact of firms both adopting an IR and having ESG incentives in their CEO compensation packages. We also include a battery of controls in line with previous contributors. All variable definitions and measurements are provided in the next section and in the *Appendix*.

3.3. Variables measurement

Ex-post realized operating cash flows

We follow Barth et al. (2017) and construct different measures of the performance effects of the complementarity between IR and ESG incentives to capture firm value creation in terms of *ex-post* realized operating cash flows (*cfo*). We measure *cfo* as *ex-post* one-year ahead net cash flows from operating activities deflated by the beginning-of-period total assets. We also measure firm mid-term value creation (*cfo2, cfosum2*) and long-term value creation (*cfo3, cfosum3*). Our sample period includes the year 2013 to 2017 for mid-term value measures and 2013 to 2016 for long-term value measures. *cfo2, cfo3* is two-year ahead and three-year ahead ex post net cash flows from operating activities deflated by beginning-of-period total assets, respectively. *cfosum2* represents the sum of one-year, and two-year-ahead net cash flows from operating activities deflated by beginning-of-period total assets and *cfosum3* represents the sum of one-year, two-year and three-year-ahead net cash flows from operating activities deflated by beginning-of-period total assets. In the additional analyses, we also use return on assets (roa) and Tobin's Q as alternative measures of firm long-term value (Flammer et al., 2019).

CEO ESG incentives

We do not simply focus on the presence or absence of ESG incentives. We also capture two aspects of the design of CEO incentives, that is, the *type* of target and the *time orientation* of targets. We use different proxies to measure these aspects. In line with the IIRC framework, the type of target was classified based on the six capitals definition (IIRC Framework, 2013):

financial, manufactured, natural, human, intellectual, and social and relationship. We define an ESG target as a nonfinancial target based on one of the above capitals except for financial. In our main analysis, we capture the presence of ESG vs non-ESG targets and we use *esg_d*, which equals one if there is at least one ESG target in the CEO compensation package, and zero otherwise. Regarding the time orientation of targets, in our main analysis, we distinguish between short-term ESG targets, that is, referring to a period of up to one year, and long-term ones otherwise. *esg_st_d* [46]equals one if there is at least one short-term ESG target in the CEO compensation package, and zero otherwise, while *esg_lt_d* equals one if there is at least one long-term ESG target in the CEO compensation package, and zero otherwise. Short-term incentives are those targets defined by the company itself as short-term in compensation packages (including short-term bonus). We observe that many firms include ESG target into their CEO's short-term target, signaling the importance and relevance of ESG issues to outside investors, even for short-term retail investors.

In our additional analyses, we also measure the "completeness" of target types. Specifically, we compute the variable *complete_all_capitals* as the number of capitals that are represented in the vector of targets included in the CEO compensation package. This variable takes values from 0 to 6. *complete_nonfinancial_capitals* counts the number of capitals (excluding financial capital) that are represented in the vector of targets included in the CEO compensation package. This variable takes values from 0 to 5[47]. Non numeric target types are also classified according to their level of specificity: *esg_nonumeric_name* is equal to one if

[46] We assume the target is not there if the firms don't disclose the ESG target explicitly.

[47] The most common nonfinancial targets for CEOs are human-type targets (77%), followed by social-type targets (29%), intellectual-type targets (26%) and natural-type targets (20%). IR adopters typically link CEO compensation to human capital including employees' well-being, equity, safety, engagement, satisfaction and company's talent pool management. Social and relationship capital examples include customer relation, stakeholder relation and more generally societal objectives. Intellectual capital examples include product related aspects such as quality, innovation and productivity. With regard to natural capital, examples include resources and energy consumption, water and gas emissions.

the name of the ESG target is specified (e.g., "Scope 2 emissions"), and zero otherwise (e.g., general "environmental performance").

Control variables

Based on prior literature (Barth et al., 2017; Biddle et al., 2009; Cheng et al., 2013; Navissi et al., 2016), we include variables as controls for other factors that may be correlated with our dependent variable(s). We include an indicator variable to capture whether a firm issues a standalone CSR report in addition to its IR report, *csr_sa*. Prior literature (Cho et al., 2012; Dhaliwal et al., 2011) suggests that there might be a link between CSR disclosure and firm performance, for example, firms issuing CSR reports have a lower cost of capital. We include a control for corporate governance, *gov*, because the quality of corporate governance positively correlated with economic performance (Chen et al., 2009). We control for whether the firm is a voluntary IR adopter with the variable *non_mandatory* to control for the specific reporting regime (Melloni et al 2017). We also include an indicator variable equal to one if the firm's CEO has changed during a specific year, and zero otherwise (Karaevli, 2007). Some variables are included to control for financial performance (Barth et al., 2017): *loss* is an indicator variable equal to one if income before extraordinary items is negative, and zero otherwise; *mtb* is the market-to-book ratio of equity; *size* is the natural logarithm of market capitalization at the beginning of the year. Finally, following Biddle, Hilary, and Verdi (2009) and Barth et al. (2017), we control for cash *(cash)*, price volatility *(volatility)*, length of the operating cycle (*op_cycle*), and leverage ratios (*lev*) since these have been found previously to be related to capital investment. See Appendix for a summary of all the variables used in our analyses.

4. Results

4.1. Summary statistics and correlation analysis

Table 2 Panel A presents the descriptive statistics for our main variables. 82% of our sample firms (*esg_d* = 82%) include ESG targets when designing CEO compensation packages. 79% of our sample firms link their CEO's compensation to short-term ESG targets, whereas only 30% of our sample firms include long-term ESG targets. The mean one-year ahead *ex-post* realized operating cash flow scaled by total assets (*cfo*) is 0.10. IR adopters have significantly ($p<0.05$) higher ex-post realized operating cash flows (0.11 vs. 0.09) than non-adopters. There are a number of CEO changes in our sample period (*ceo_change* = 13%). The mean market-to-book ratio (*mtb*) is 3.02 (consistent with Barth et al., 2017; Biddle, Hilary, and Verdi, 2009).

In Table 2 Panel B, we present the Pearson correlation coefficients among our variables of interest (*ex post* operating cash flows, ESG incentives) and control variables. As expected, our ESG incentives variables are correlated among themselves and with the *iradop* in line with previous contributions.

Table 2 Panel C presents our univariate analyses which show that the percentage of IR adopters including short-term ESG incentives in their CEO compensation package is significantly ($p<0.01$) higher (0.96 vs. 0.62) than non-adopters. Also the percentage of IR adopters including long-term ESG incentives in their CEO compensation packages is significantly ($p<0.05$) higher (0.37 vs. 0.23) than non-adopters.

< INSERT TABLE 2 >

4.2. Regression results

We then examine whether the joint use of IR and ESG incentives is beneficial to firms and is thus indeed associated with short, mid-term and long-term firm value.[48] We firstly

[48] Empirically, complementarity implies a positive conditional correlation between the two practices after controlling for other variables. In this paper, we are interested in the consequences of the joint use of EGS

examine whether IR adopters including ESG targets in CEO compensation packages display better ex post realized operating cash flows (i.e., short term value). We estimate an OLS equation with different specifications of ESG incentives (i.e., *esg_d, esg_st_d, esg_lt_d*) capturing both the type of target and the time orientation of each target. In all models presented in Table 3, we include all control variables, industry fixed effects and year fixed effects, and cluster standard errors at the country level. The coefficient of the interaction term between the IR adopter dummy variable and the ESG incentives as well as ESG short-term incentives are always positive and highly significant (coefficient=0.093, 0.094 respectively, $p< 0.01$) as expected. We also analyze the variance inflation factors (VIF) to assess multicollinearity. Consistent with prior literature, the maximum VIF for the model constructed is 3.45 and is thus well below the general threshold of 10 (Bedford & Malmi, 2015).

Interestingly, neither the coefficient of the variable *iradop* nor *esg_d, esg_st_d, esg_lt_d* is significant with a positive sign. On the contrary, in our models *iradop* is negatively associated with *cfo* possibly indicating that the adoption of an IR approach is a costly investment in the short-term and that the implementation of an IR, in isolation, does not necessarily contribute to the improvement of value creation. However, in line with our expectation on complementarity, our evidence suggests that the joint use of IR adopters and ESG incentives for their CEOs is associated with benefits in terms of cash flows in the short term. These results suggest that IR adopters are able to improve their internal decision-making processes and obtain economic benefits as evidenced by an increase in future operating cash flows only when ESG disclosure is coupled with ESG incentives.

disclosure and incentives. However, to follow Masschelein and Moers (in press) we also test whether these two practices are positively correlated with each other after controlling for their determinants. We regress on IR and ESG incentives their determinants and then we calculate the conditional correlations of the residuals of the two regressions. In line with the idea that ESG disclosure and ESG incentives are used in a complementary way, we find that the regression residuals are positively correlated and conditional correlation coefficients are positive and significant (untabulated).

< INSERT TABLE 3>

Table 4 and Table 5 present the results testing whether IR adopters having ESG targets included in their CEO compensation packages have higher mid-term and long-term *ex-post* realized operating cash flows, respectively. We measure mid-term operating cash flows as two-year ahead and the sum of one-year, two-year ahead net cash flows from operating activities deflated by beginning-of-period total assets in Table 4. We measure long-term operating cash flows as three-year ahead and the sum of one-year, two-year and three-year-ahead net cash flows from operating activities deflated by beginning-of-period total assets in Table 5.

Table 4 reveals a positive and significant association between the interaction term (i.e., joint use of IR and ESG incentives) and *cfos* across all models except for that using *esg_lt_d*. Consistent with what we find using one-year ahead operating cash flows as a measure, in two out of three of our models *iradop* is negatively associated with *cfo* possibly indicating that the adoption of an IR approach is costly. At the same time, ESG targets are negatively associated (not significantly) with cash flows (*esg_d* and *esg_st_d*), possibly suggesting that the focus on ESG objectives *per se* might be detrimental to a firm's financial performance, especially if the focus is on achieving short-term ESG targets. Overall, our evidence suggests that only IR adopters also employing ESG incentives for their CEOs are able to improve their internal decision-making processes and their firm medium-term value creation as evidenced by an increase in two-year ahead operating cash flows.

Findings in Table 5 are consistent with those in Table 4, in which the positive coefficients of the interaction terms across all models indicate that IR practice combined with CEO ESG incentives improve firm long-term value creation systematically. The economic magnitudes of the effects are also larger in the mid-term and long-term compared with the short-term, indicating that CEO ESG incentives play a more important role beyond the short-

term firm value. This finding is in line with the demand-function approach, suggesting that IR effectively complements CEO ESG incentives with regard to firm net cash flows from operating activities.

< INSERT TABLE 4>

< INSERT TABLE 5>

5. Robustness checks and additional analyses

5.1. Robustness checks

Although control variables, fixed effects regressions and matched control groups help to mitigate potential omitted variables bias, they do not fully rule out the possibility that unobservable time-varying firm characteristics may drive the relationship between CEO ESG incentives and real effects. The inclusion of ESG targets in CEO compensation packages is not random and can correlate with unobservable factors that may also affect the internal decision-making outcomes. Therefore, we need an instrument for ESG incentives, that is, a variable that triggers exogenous shifts in the propensity to adopt ESG-based criteria in CEO compensation.

The specific instrument that we exploit in this paper is the mandatory requirement of adopting an IR for listed firms at the Johannesburg Stock Exchange (JSE) in South Africa starting from 2010. The issuance of the King III Report on Corporate Governance (in 2009), which mandated the IR for listed companies emphasizes the importance of considering the interests of all stakeholders (and not only of shareholders) and hence the importance of ESG accountability in CEO compensation design (Eccles, Serafeim, & Armbrester, 2012; Ioannou & Serafeim, 2012). More importantly, because the mandatory disclosure does not reflect any firm's strategic decisions, such treatment offers plausibly exogenous variation in a firm's propensity to use ESG criteria in executive compensation. The correlation table shows that the

non_mandatory variable (our instrument that is equal to one if IR adoption is non-mandatory, and zero otherwise, i.e., South African listed firms) is significantly correlated with the variables capturing ESG targets *(esg_d, esg_st_d and esg_lt_d*). Some may argue that mandatory IR disclosure could also help firms better understand their business, thus fostering improved internal decisions, which would directly affect our real effects measures. However, this improved decision making is unlikely to play a role without the channel of CEO incentives, which is also confirmed by the fact that the *non_mandatory* variable is not correlated with performance measures *(cfos)* thus confirming the soundness of our strategy. Therefore, we use a two-stage regression approach and we first regress the interaction terms (integrated reporting adopters and the *non_mandatory* variable). Our (untabulated) results indicate that *non_mandatory* is a relevant instrument (with p-value<0.01). In the second-stage regression, we then re-estimate previous models using the instrumented ESG target variables. Table 6 presents our instrumented results, which are substantially unchanged for our variables of interest.

< INSERT TABLE 6>

To alleviate the concern that our results are driven by mandatory IR adopters in South Africa and firms in the U.S., we replicate our results by removing firms in South Africa, those in the U.S. and those in South Africa and the U.S., respectively. Our untabulated analysis provides results similar to our main findings. We also repeat our analyses by using robust standard errors. Results (untabulated) are confirmed. Finally, we drop those observations where the variable *ceo_change* is equal to one. Also in this case, our findings remain unchanged (untabulated).

5.2. Additional analyses

We perform some additional analyses drawing on different proxies of the type of ESG target included in the CEO compensation package: their completeness and specificity as previously defined.

As shown in Table 7, the complementary use of an IR and of diverse ESG targets improves the ability of a company to generate operating cash flows. For IR adopters, the more diverse ESG targets are, the higher the *ex-post* realized operating cash flows. Complementarity between completeness of ESG targets (i.e., the number of different capitals included in the vector of targets for the CEO denoted as *complet_all_capitals* and *complet_nonfinancial_capitals*) and IR adopters generate incremental positive net cash flows in one-year, two-year and three-year net cash flows ahead of implementation. Consistent with our main findings, the economic benefits for IR adopters depends not only on the presence of ESG targets, but also on their completeness in representing the multifaceted capitals used by firms to generate value, especially in the long run. In addition, Table 8 shows that the increase in firm long-term value is greater for those firms whose ESG non-numeric targets are *specified* than those without a *specified* target. Therefore, the more specific the signal given to managers, the higher the economic benefits.

< INSERT TABLE 7>

< INSERT TABLE 8>

Moreover, we employ other measures to capture firm value creation. We replace our dependent variables with *roa* and *tobinq*, and rerun Equation 3. Table 9 presents our additional results. Consistent with our main findings, the positively significant coefficients of the interaction terms indicate that the combined use of IR and ESG incentives generates positive long-term value implications. Specifically, for what concerns the effects on Tobin's q, as we find a positive relation between the joint use of IR and ESG incentives and the bid-ask spread

(untabulated), in line with the reasoning by Barth et al. (2017), we suggest that the effect on firm's value is largely attributable to real effects or improved decision making (that we proxied with the cash flow from operating activities), rather than a market effect.

< INSERT TABLE 9>

Lastly, we try to investigate the role of institutional investors on the relationship between joint adoption (IR and ESG incentives) and firm value creation. Chen, Dong and Lin (2020) argue that ESG practices can be considered as a risk management tool in institutional portfolios. Institutional investors, as more "universal onwers", are more likely to be exposed to risks from corporate operations with negative externalities (Chen, Dong and Lin, 2020). Therefore, it is in their best interest to positively influence portfolio firms' CSR commitments and minimize their overall exposure to these costs (see, e.g., Chakravarthy et al., 2014, Krüger, 2015). On the other hand, Serafeim (2015) suggests that long-term investors are more likely to buy and hold shares in companies that provide more information about their long-term prospects. Therefore, the value implications of joint use of ESG disclosures and ESG incentives significantly depend on the degree of investors' long-term horizon. Companies with higher percentage of institutional ownership are more likely to be effective in monitoring and resolving the agency conflicts. If the agency theory and conflict resolution hypothesis are true, we should expect the positive effects of joint adoption more pronounced for firms with large percentage ownership by long-term investors since dedicated, long-term investors can play an more effective monitoring role in ESG issues given the fact that short-termism is a major barrier to transition to sustainability (Serafeim, 2015).

We run cross-sectional analyses based on firms' institutional ownership. Specifically, we sort firms' percentage of institutional holdings into groups with *High Institutional Holdings* and *Low Institutional Holdings*. Table 10 present the results suggesting that the positive effects

of joint use of ESG disclosure and ESG incentives on future operating cash flows are significantly greater for firms with higher institutional holdings, especially in the mid-term and long-term. The findings are consistent with the notion that the degree of institutional holdings plays a facilitator role in external monitoring and more likely to use ESG practices to minimize the risk of negative corporate externalities.

< INSERT TABLE 10>

6. Conclusions

In this paper, we explore the value creation effects of the interplay between ESG disclosure and ESG incentives focusing on the IR context. In this respect, IR offers a proper and unique institutional setting as it is explicitly focused not only on improving transparency for external users (i.e., information function), but also on enhancing internal decision making processes (i.e., transformation function) (Barth et al., 2017; Robert G. Eccles & Serafeim, 2015).

As advocated by previous research, we offer empirical support for the economic benefits of coupling these two elements: our results consistently show that the association between IR and CEO ESG incentives improves short and long-term value creation. In this respect, we support the arguments of prior literature pointing at the risk of a potential de-coupling between substance and symbol in both executive compensation and disclosure practices, especially with regard to ESG, nonfinancial contexts. Indeed, we show that IR *per se* may be detrimental for a firm's financial performance if ESG incentives are not embedded in current business practices of IR adopters with a risk that IR will remain merely a 'symbolic practice' or 'just another report' added to the plethora of documents already published by firms (CSR reports, governance reports, etc.). At the same time, we add to the debate on the link between ESG incentives and firm financial performance showing that, to effectively support

firm value creation, managers need reporting and performance measurement tools that help them monitor and understand all the multifaceted dimensions related to sustainable initiatives, including their link with financial outcomes, and an IR may represent such a tool.

We acknowledge that our research design is subject to a number of limitations and in particular to potential endogeneity issues. Firstly, we do not find a positive significant association between the adoption of integrated reporting alone and the value creation. We interpreted it as the fact that IR is costly to adopt and company indeed implement a greenwashing strategy and symbolically commit to IR reporting without making real improvement in their business activities. Indeed we also don't find a significant improvement in ESG performance (measured as Refinitiv ESG scores). Secondly, our measure of IR adoption now is only a dummy variable. We don't have a comprehensive measure of IR adoption quality. We try to use Bloomberg ESG disclosure score as a raw proxy for the reporting quality. Including ESG disclosure controls does not affect our main findings. Thirdly, a further research direction would be to understand under what condition the joint adoption will also help the improvement of ESG performance and its interaction with firms' value creation. Additionally, future research could investigate how much incremental value that joint adoption can bring for those firms which already have high quality ESG disclosures *ex ante* versus those who does not have or have low quality ones. Lastly, we have tried to moderate endogeneity concerns with our robustness tests, nonetheless we refrain from inferring causal relationships from the evidence we present.

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Appendix 1-Variable Definitions	
Test Variables	
iradop	equal to one if it is an integrated reporting adopter (as reported in the International Integrated Reporting Council (IIRC) website), zero otherwise. Non-adopters are identified with a 1:1 matching based on size, industry and geographical area.
esg_d	equal to one if there is at least one Environmental, Social and Governance (ESG) target in the CEO compensation package, zero otherwise.
esg_st_d	equal to one if there is at least one short-term ESG target in the CEO compensation package, zero otherwise.
esg_lt_d	equal to one if there is at least one long-term ESG target in the CEO compensation package, zero otherwise.
complete_all_capitals	the number of capitals included in the compensation scheme. Values from 0 to 6
complete_nonfinancial_capitals	the number of capitals included in the compensation scheme, excluding financial capital. Values froom 0 to 5.
esg_nonumeric_d	equal to one if the name of the ESG non numeric target is specified, zero otherwise.
Dependent Variables	
cfo	one-year ahead ex post net cash flows from operating activities deflated by beginning-of-period total assets.
cfo2	two-year ahead ex post net cash flows from operating activities deflated by beginning-of-period total assets.
cfo3	three-year ahead ex post net cash flows from operating activities deflated by beginning-of-period total assets.
cfosum2	sum of one-year, and two-year-ahead net cash flows from operating activities deflated by beginning-of-period total assets
cfosum3	sum of one-year, two-year and three-year-ahead net cash flows from operating activities deflated by beginning-of-period total assets
Control Variables	
op_cycle	Natural logarithm of the sum of receivables to sales and inventory to cost of goods sold multiplied by 360.
volatility	price volatility scaled by natural logarithm of total assets.
logasset	natural logarithm of total assets.
mtb	market-to-book ratio of equity calculated as the number of common shares outstanding multiplied by end-of-year share price, divided by the book value of common shareholders' equity.
cfo_sales	ratio of cash flow from operations to sales.
slack	ratio of cash to property, plant and equipment.
cash	cash and cash equivalents scaled by total assets.
lev	leverage calculated as the ratio of total debt to the sum of total debt and the book value of common shareholders' equity.
gov	corporate governance score (Asset4 Mnemonic CGVSCORE) scaled by natural logarithm of total assets.

csr_sa	indicator variable that equals one if a firm issued a stand-alone CSR report, and zero otherwise.
loss	indicator variable that equals one if income before extraordinary items is negative, and zero otherwise.
non_mandatory	equal to one if IR adoption is non mandatory, zero otherwise (i.e., South African listed firms).
size	the natural logarithm of market capitalization at the beginning of the year.
ceo_change	indicator variables that equals one if the CEO has changed during a specific year, and zero otherwise.

TABLE 1 – Sample

Panel A: Year Composition

Fiscal Year	Firm-year	Percent
2013	68	16
2014	71	16.71
2015	69	16.24
2016	73	17.18
2017	73	17.18
2018	71	16.71
Total	425	100

Panel B: Industry Composition

Fama French 48 Industry	Firm-year	Percent
Food	16	3.76
Beer & Liquor	12	2.82
Tobacco Products	12	2.82
Healthcare	9	2.12
Medical Equipment	6	1.41
Pharmaceutical Products	12	2.82
Chemicals	20	4.71
Construction Materials	9	2.12
Construction	19	4.47
Steel Works Etc	6	1.41
Electrical Equipment	6	1.41
Precious Metals	22	5.18
Non-Metallic and Industrial Metal Mining	29	6.82
Coal	12	2.82
Petroleum and Natural Gas	21	4.94
Utilities	54	12.71
Communication	36	8.47
Personal Services	3	0.71
Business Services	19	4.47
Computers	5	1.18
Business Supplies	3	0.71
Transportation	3	0.71
Wholesale	12	2.82
Retail	70	16.47
Other	9	2.12
Total	425	100

Panel C: Country Composition		
Country	Freq.	Percent
Austria	6	1.41
Australia	6	1.41
Belgium	6	1.41
Bermuda	6	1.41
Canada	16	3.76
Switzerland	5	1.18
Denmark	12	2.82
Spain	40	9.41
Finland	6	1.41
France	6	1.41
United Kingdom	97	22.82
Ireland	6	1.41
Italy	30	7.06
Netherlands	18	4.24
Sweden	5	1.18
Singapore	6	1.41
Thailand	6	1.41
USA	59	13.88
South Africa	89	20.94
Total	425	100

Panel D: Cross Tables			
	esg_d=0	**esg_d=1**	Total
iradop=0	65 (15.29%)	145 (34.11%)	211
iradop=1	9 (2.37%)	205 (48.23%)	214
Total	74	351	425

The relative amount of observations per quadrant to total observations is in parentheses.

TABLE 2 – Descriptive Statistics

Panel A. Summary statistics						
	N	**Mean**	**Std. Dev.**	**Q1**	**Median**	**Q3**
Test Variables						
iradop	425	0.504	0.501	0.000	1.000	1.000
esg_d	425	0.826	0.380	1.000	1.000	1.000
esg_st_d	425	0.791	0.407	1.000	1.000	1.000
esg_lt_d	425	0.301	0.459	0.000	0.000	1.000
Dependent Variables						
cfo	425	0.101	0.086	0.052	0.080	0.125
Control Variables						
op_cycle	425	4.730	0.627	4.276	4.667	5.106
volatility	425	0.824	0.757	0.203	0.429	1.349
logasset	425	13.592	3.642	10.139	15.049	16.931
mtb	425	3.017	3.713	1.150	1.920	3.260
cfo_sale	425	0.175	0.149	0.062	0.139	0.256
slack	425	0.580	1.132	0.078	0.179	0.511
cash	425	0.078	0.067	0.029	0.062	0.108
lev	425	0.593	0.678	0.157	0.364	0.634
csr_sa	425	0.967	0.179	1.000	1.000	1.000
loss	425	0.122	0.328	0.000	0.000	0.000
non_mandatory	425	0.791	0.407	1.000	1.000	1.000
size	425	5.575	3.589	0.943	6.595	8.313
ceo_change	425	0.129	0.336	0.000	0.000	0.000

Panel B. Pearson correlations					
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
(1) iradop	1.000				
(2) esg_d	**0.351**	1.000			
(3) esg_st_d	**0.414**	**0.892**	1.000		
(4) esg_lt_d	**0.159**	**0.301**	**0.262**	1.000	
(5) cfo	**0.152**	-0.054	-0.044	-0.021	1.000

Panel C. Univariate analyses			
	IR adopter	non-IR adopter	Two-tailed p-value
esg_st_d	0.96	0.62	0.000
esg_lt_d	0.37	0.23	0.001

Table 2 Panel A reports summary statistics of key variables for the full sample from 2013 to 2018. All continuous variables are winsorized to the 1st and 99th percentiles of their distributions. *iradop* is an indicator equal to one if it is an integrated reporting adopter, zero otherwise. *esg_d, esg_st_d and esg_lt_d* equal one if there is at least one Environmental, Social and Governance (ESG) target in the CEO compensation package, at least one short-term ESG target, and at least one long-term ESG target, respectively, zero otherwise. *cfo* is one-year ahead ex post net cash flows from operating activities deflated by beginning-of-period total assets, respectively. Panel B presents Pearson correlations, with the correlation coefficients with a significance level of 0.05 or higher in bold. See Appendix 1 for control variable definitions. Panel C reports univariate analyses.

TABLE 3 – Integrated Reporting, ESG Targets and *Short-term* Ex-post Realized Operating Cash Flows

	Dependent Variable=cfo		
VARIABLES	*X=esg_d*	*X=esg_st_d*	*X=esg_lt_d*
Test Variables			
iradop	-0.071***	-0.070***	-0.003
	(0.019)	(0.020)	(0.008)
X	-0.025	-0.023	-0.017
	(0.029)	(0.024)	(0.031)
iradop#X	**0.093*** **	**0.094*** **	**0.043**
	(0.023)	**(0.024)**	**(0.039)**
Control Variables			
op_cycle	-0.012	-0.012	-0.010
	(0.008)	(0.009)	(0.009)
volatility	0.017**	0.019**	0.017**
	(0.008)	(0.008)	(0.008)
logasset	0.005**	0.004**	0.005**
	(0.002)	(0.002)	(0.002)
mtb	0.007***	0.007***	0.006***
	(0.002)	(0.002)	(0.002)
cfo_sale	0.150***	0.149***	0.157***
	(0.041)	(0.042)	(0.040)
slack	-0.011**	-0.011**	-0.013**
	(0.004)	(0.004)	(0.005)
cash	0.239**	0.238**	0.289**
	(0.103)	(0.102)	(0.105)
lev	-0.007	-0.006	-0.008
	(0.007)	(0.007)	(0.008)
csr_sa	-0.020	-0.021	-0.028*
	(0.015)	(0.014)	(0.015)
loss	-0.048***	-0.047***	-0.049***
	(0.008)	(0.008)	(0.008)
non_mandatory	-0.046***	-0.046***	-0.047***
	(0.015)	(0.015)	(0.016)
size	0.001	0.001	0.001
	(0.003)	(0.003)	(0.002)
ceo_change	-0.002	-0.002	-0.001
	(0.008)	(0.008)	(0.009)
Observations	425	425	425
R square	0.529	0.529	0.524
Industry FE	YES	YES	YES
Year FE	YES	YES	YES

Table 4 reports results from OLS regressions of ex post operating cash flows on the indicator for integrated reporting adopters having ESG targets in their CEO compensation package. The sample spans the 2013-2018 period and includes 425 firm-year observations. The dependent variable, *cfo* is one-year ahead ex post net cash flows from operating activities deflated by beginning-of-period total assets. *iradop* is an indicator equal to one if it is an integrated reporting adopter, zero otherwise. *esg_d*, *esg_st_d* and *esg_lt_d* equal one if there is at least one Environmental, Social and Governance (ESG) target in the CEO compensation package, at least one short-term ESG target, and at least one long-term ESG target, respectively, zero otherwise. See

the Appendix for control variable definitions. Industry-fixed effects and year-fixed effects are included. All continuous variables are winsorized to the 1st and 99th percentiles. Standard errors are corrected for heteroskedasticity and cluster at country level (robust standard errors are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

TABLE 4- Integrated Reporting, ESG Targets and *Mid-term* Ex-post Realized Operating Cash Flows

	Dependent Variable=cfo2			***Dependent Variable=cfosum2***		
VARIABLES	*X=esg_d*	*X=esg_st_d*	*X=esg_lt_d*	*X=esg_d*	*X=esg_st_d*	*X=esg_lt_d*
Test Variables						
iradop	-0.084***	-0.083**	-0.003	-0.141***	-0.140***	-0.007
	(0.028)	(0.029)	(0.010)	(0.041)	(0.042)	(0.019)
X	-0.023	-0.020	-0.014	-0.048	-0.045	-0.031
	(0.026)	(0.020)	(0.029)	(0.053)	(0.042)	(0.055)
iradop#X	**0.109*** **	**0.109*** **	**0.046**	**0.183*** **	**0.185*** **	**0.079**
	(0.032)	**(0.033)**	**(0.039)**	**(0.047)**	**(0.048)**	**(0.073)**
Controls	YES	YES	YES	YES	YES	YES
Observations	354	354	354	354	354	354
R square	0.525	0.525	0.519	0.597	0.596	0.589
Industry FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES

Table 5 reports results from OLS regressions of ex post operating cash flows on the indicator for integrated reporting adopters having ESG targets in their CEO compensation package. The sample spans the 2013-2017 period and includes 354 firm-year observations. The dependent variable, *cfo2* is two-year ahead ex post net cash flows from operating activities deflated by beginning-of-period total assets and iradop is an indicator equal to one if it is an integrated reporting adopter, zero otherwise; *cfosum2* represents the sum of one-year, and two-year-ahead net cash flows from operating activities deflated by beginning-of-period total assets. esg_d, esg_st_d and esg_lt_d equal one if there is at least one Environmental, Social and Governance (ESG) target in the CEO compensation package, at least one short-term ESG target, and at least one long-term ESG target, respectively, zero otherwise. See Appendix 1 for control variable definitions. Industry-fixed effects and year-fixed effects are included. All continuous variables are winsorized to the 1st and 99th percentiles. Standard errors are corrected for heteroskedasticity and cluster at country level (robust standard errors are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

TABLE 5 - Integrated Reporting, ESG Targets and *Long-term* Ex-post Realized Operating Cash Flows

	Dependent Variable=cfo3			***Dependent Variable=cfosum3***		
VARIABLES	*X=esg_d*	*X=esg_st_d*	*X=esg_lt_d*	*X=esg_d*	*X=esg_st_d*	*X=esg_lt_d*
Test Variables						
iradop	-0.057	-0.058	-0.001	-0.193**	-0.194**	-0.013
	(0.035)	(0.035)	(0.011)	(0.071)	(0.072)	(0.026)
X	-0.027	-0.029	-0.022	-0.074	-0.078	-0.050
	(0.027)	(0.023)	(0.026)	(0.075)	(0.061)	(0.077)
iradop#X	**0.081****	**0.084****	**0.045**	**0.254*****	**0.263*****	**0.123**
	(0.035)	**(0.035)**	**(0.034)**	**(0.078)**	**(0.077)**	**(0.101)**
Controls	YES	YES	YES	YES	YES	YES
Observations	281	281	281	281	281	281
R square	0.511	0.512	0.508	0.630	0.631	0.624
Industry FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES

Table 6 reports results from OLS regressions of ex post operating cash flows on the indicator for integrated reporting adopters having ESG targets in their CEO compensation package. The sample spans the 2013-2016 period and includes 281 firm-year observations. The dependent variable, *cfo3* is three-year ahead ex post net cash flows from operating activities deflated by beginning-of-period total assets; *cfosum3* represents the sum of one-year, two-year and three-year-ahead net cash flows from operating activities deflated by beginning-of-period total assets and iradop is an indicator equal to one if it is an integrated reporting adopter, zero otherwise. esg_d, esg_st_d and esg_lt_d equal one if there is at least one Environmental, Social and Governance (ESG) target in the CEO compensation package, at least one short-term ESG target, and at least one long-term ESG target, respectively, zero otherwise. See Appendix 1 for control variable definitions. Industry-fixed effects and year-fixed effects are included. All continuous variables are winsorized to the 1st and 99th percentiles. Standard errors are corrected for heteroskedasticity and cluster at country level (robust standards errors are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

TABLE 6 – Integrated Reporting, ESG Targets and Ex-post Realized Operating Cash Flows (Instrumental Approach)

	Dependent Variable=cfo		
VARIABLES	*X=esg_d*	*X=esg_st_d*	*X=esg_lt_d*
Test Variables			
iradop	-0.071***	-0.070***	-0.003
	(0.018)	(0.019)	(0.008)
X	-0.025	-0.023	-0.017
	(0.027)	(0.023)	(0.029)
iradop#X(instrumented)	**0.150*** **	**0.151*** **	**0.161*** **
	(0.015)	**(0.017)**	**(0.032)**
Controls	YES	YES	YES
Observations	425	425	425
R square	0.512	0.512	0.506
Industry FE	YES	YES	YES
Year FE	YES	YES	YES

Table 7 reports results from OLS regressions of ex post operating cash flows on the indicator for integrated reporting adopters having ESG targets in their CEO compensation package. The sample spans the 2013-2018 period and includes 425 firm-year observations. We use *non_mandatory* equal to one if the disclosure on ESG is voluntary, 0 otherwise, as an instrument in the first stage regression to obtain the predicted value of propensity to include ESG targets, ESG short-term or ESG long-term targets in CEO compensation package. See Appendix 1 for control variable definitions. Industry-fixed effects and year-fixed effects are included. All continuous variables are winsorized to the 1st and 99th percentiles. Standard errors are corrected for heteroskedasticity and cluster at country level (robust standard errors are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

TABLE 7 – Integrated Reporting, ESG Targets *Completeness* and Ex-post Realized Operating Cash Flows

	cfo		***cfo2***		***cfosum2***		***cfo3***		***cfosum3***	
VARIABLES	*X= complet_all_capitals*	*X= complet_nonfinancial_capitals*	*X= complet_all_capitals*	*X= complet_nonfinancial_capitals*	*X= complet_all_capitals*	*X= complet_nonfinancial_capitals*	*X= complet_all_capitals*	*X= complet_nonfinancial_capitals*	*X= complet_all_capitals*	*X= complet_nonfinancial_capitals*
Test Variables										
iradop	-0.053	-0.027	-0.061	-0.032	-0.104	-0.053	-0.052	-0.026	-0.150	-0.076
	(0.035)	(0.023)	(0.038)	(0.026)	(0.074)	(0.049)	(0.031)	(0.021)	(0.095)	(0.063)
X	-0.011*	-0.009	-0.008	-0.006	-0.020*	-0.015	-0.007	-0.004	-0.026*	-0.018
	(0.006)	(0.006)	(0.005)	(0.006)	(0.011)	(0.012)	(0.005)	(0.005)	(0.015)	(0.016)
iradop#X	**0.022***	**0.020**	**0.025***	**0.022**	**0.043**	**0.037**	**0.022***	**0.020***	**0.062***	**0.054**
	(0.010)	**(0.012)**	**(0.013)**	**(0.013)**	**(0.026)**	**(0.026)**	**(0.011)**	**(0.011)**	**(0.034)**	**(0.035)**
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	425	425	354	354	354	354	281	281	281	281
R square	0.534	0.529	0.531	0.527	0.601	0.595	0.517	0.515	0.633	0.628
Industry FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES

Table 8 reports results from OLS regressions of ex post operating cash flows on the indicator for integrated reporting adopters having ESG targets in their CEO compensation package. *complet_all_capitals* is the number of capitals in IR framework included in their CEO compensation schemes. *complete_nonfinancial_capitals* is the number of non-financial capitals in IR framework included in their CEO compensation schemes. See Appendix 1 for control variable definitions. Industry-fixed effects and year-fixed effects are included. All continuous variables are winsorized to the 1st and 99th percentiles. Standard errors are corrected for heteroskedasticity and cluster at country level (robust standard errors are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

TABLE 8 – Integrated Reporting, ESG Targets *Specificity* and Ex-post Realized Operating Cash Flows

	cfo	***cfo2***	***cfosum2***	***cfo3***	***cfosum3***
VARIABLES	*X= esg_nonumeric_d*	*X= esg_nonumeric_d*	*X= esg_nonumeric_d*	*X= esg_nonumeric_d*	*X= esg_nonumeric_d*
Test Variables					
iradop	-0.051***	-0.066**	-0.114**	-0.050	-0.179**
	(0.016)	(0.031)	(0.048)	(0.030)	(0.071)
X	-0.024	-0.020	-0.043	-0.023	-0.063
	(0.026)	(0.024)	(0.048)	(0.026)	(0.075)
iradop#X	**0.075*** **	**0.092** **	**0.159** **	**0.076** **	**0.246*** **
	(0.021)	**(0.037)**	**(0.057)**	**(0.032)**	**(0.083)**
Controls	YES	YES	YES	YES	YES
Observations	425	354	354	281	281
R square	0.527	0.522	0.595	0.510	0.629
Industry FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES

Table 9 reports results from OLS regressions of ex post operating cash flows on the indicator for integrated reporting adopters having ESG specific non numeric targets in their CEO compensation package. *esg_nonumeric_d* equals one if the name of the ESG non numeric target is specified, zero otherwise. See Appendix 1 for control variable definitions. Industry-fixed effects and year-fixed effects are included. All continuous variables are winsorized to the 1st and 99th percentiles. Standard errors are corrected for heteroskedasticity and cluster at country level (robust standard errors are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

TABLE 9 – Effect of Integrated Reporting and ESG Targets on *Other Performance*

	roa			***tobinQ***		
VARIABLES	*X=esg_d*	*X=esg_st_d*	*X=esg_lt_d*	*X=esg_d*	*X=esg_st_d*	*X=esg_lt_d*
Test Variables						
iradop	-0.027**	-0.027***	0.015	-1.181***	-1.093**	0.288*
	(0.010)	(0.009)	(0.009)	(0.403)	(0.441)	(0.154)
X	-0.016	-0.017	-0.007	-0.979***	-0.725**	-0.416
	(0.028)	(0.021)	(0.023)	(0.340)	(0.317)	(0.342)
iradop#X	**0.055*** **	**0.057*** **	**0.022**	**1.950*** **	**1.868*** **	**0.601**
	(0.019)	**(0.017)**	**(0.024)**	**(0.455)**	**(0.503)**	**(0.468)**
Controls	YES	YES	YES	YES	YES	YES
Observations	425	425	425	425	425	425
R square	0.606	0.606	0.603	0.747	0.738	0.721
Industry FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES

The table reports results from OLS regressions of firm value creation on the indicator for integrated reporting adopters having ESG targets in their CEO compensation package. The sample spans the 2013-2018 period and includes 425 firm-year observations. See Appendix 1 for control variable definitions. Industry-fixed effects and year-fixed effects are included. All continuous variables are winsorized to the 1st and 99th percentiles. Standard errors are corrected for heteroskedasticity and cluster at country level (robust standard errors are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

TABLE 10 – The Role of Institutional Investors

	cfo	***cfo2***	***cfosum2***	***cfo3***	***cfosum3***
VARIABLES	X=esg_d	X=esg_d	X=esg_d	X=esg_d	X=esg_d
	(1)	(2)	(3)	(4)	(5)
iradop	0.062	0.141**	0.181*	0.130	0.319*
	(0.051)	(0.058)	(0.091)	(0.111)	(0.162)
X	-0.031	-0.006	-0.045	0.006	-0.043
	(0.055)	(0.059)	(0.101)	(0.096)	(0.166)
iradop#X	-0.019	-0.101	-0.104	-0.088	-0.229
	(0.054)	(0.062)	(0.096)	(0.135)	(0.186)
high inst_ownership	-0.126	-0.025	0.012	-0.049	0.057
	(0.099)	(0.139)	(0.225)	(0.158)	(0.324)
iradop#high inst_ownership	-0.065**	-0.126***	-0.171***	-0.106	-0.288**
	(0.029)	(0.029)	(0.047)	(0.082)	(0.115)
X#high inst_ownership	0.025	0.007	0.035	-0.007	0.039
	(0.034)	(0.035)	(0.067)	(0.066)	(0.127)
iradop#X#high inst_ownership	**0.034**	**0.107*** **	**0.125** **	**0.089**	**0.243**
	(0.031)	**(0.031)**	**(0.053)**	**(0.100)**	**(0.140)**
Controls#high inst_ownership	YES	YES	YES	YES	YES
Observations	393	326	326	258	258
R-squared	0.620	0.700	0.754	0.678	0.796
Industry FE#high inst_ownership	YES	YES	YES	YES	YES
Year FE#high inst_ownership	YES	YES	YES	YES	YES

Table 10 reports results from OLS regressions of ex post operating cash flows on the indicator for integrated reporting adopters having ESG targets in their CEO compensation package conditional on firms' institutional ownership. The sample spans the 2013-2018 period and includes 393 (due to institutional ownership data availability) firm-year observations for column 1, 326 for column 2 and 3 and 258 for column 4 and 5. *high inst_ownership* equals to one if the institutional ownership percentage of firm i in year t is above its median values, zero otherwise. See Appendix 1 for control variable definitions. Industry-fixed effects and year-fixed effects are included. All continuous variables are winsorized to the 1st and 99th percentiles. Standard errors are corrected for heteroskedasticity and cluster at country level (robust standard errors are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.