Housing Prices, Labor Markets, and Corporate Decisions: Cross-market Consequences of Disclosure Regulation

Xi Chen
University of Bristol
x.chen@bristol.ac.uk

Gilles Hilary
Georgetown University
gilles.hilary@georgetown.edu

Abstract

Economic reviews of regulation typically offer partial-equilibrium analyses of costs and benefits. Using a concrete example, we show the importance of considering a more general approach. We evidence that laws designed to better disclose flood risk lead to higher housing prices that reflect this greater market transparency. However, these laws also negatively affect local firms through the labor channel. Specifically, rising house prices tighten local labor markets. Consistent with potential employees being forced to relocate to cheaper areas, we find that this effect is stronger where transparency is more relevant, renting is prevalent, inhabitants are cost-burdened, commuting is limited, and local employment is already high. In response, local firms adopt more conservative financial and investment policies, particularly when local employment markets are already tight and employers are labor-intensive. Firms experience lower profitability, discuss labor and housing issues more, employ fewer people, and relocate more often to lower housing cost areas.

JEL Classification Codes: G38; J30; K25; G32

Keywords: transparency, housing market, labor

We would like to thank Sudipto Dasgupta, Francois Derrien, David McLean, Charly Porcher, Jason Schloetzer, Xiaoli Tian, and Xiaofei Zhao, as well as the workshop participants at Georgetown University, the University of Bristol, the University of Pittsburgh, and the Annual Corporate Finance Conference at Lancaster University, for their valuable insights.

I. INTRODUCTION

Leuz and Wysocki (2016) note that regulators increasingly conduct cost-benefit analyses of new regulations but add that "Naturally, a quantitative cost-benefit analysis requires evidence on causal effects as a critical input. However, such evidence is difficult to generate, especially when it comes to long-run and *general-equilibrium* (emphasis added) effects." Better disclosure is typically seen as a way to improve economic efficiency by reducing frictions, and many benefits have been identified in the prior literature (see Hilary and McLean 2023 for a recent discussion). In contrast, research that focuses on an increase in economic frictions caused by better disclosure is more limited. A relatively rare example is Leuz et al. (2008), who show that some firms decided to delist after the enactment of the Sarbanes Oxley Act. However, the literature typically examines regulatory costs and benefits in a partial equilibrium framework. That is, researchers analyze the effect on the market that is directly affected by the regulation but not the rest of the economy. Unfortunately, this approach may yield incomplete insights. For example, Hazilla and Kopp (1990) show how focusing on private instead of social costs is misleading to ascertain the effects of regulations and conclude (p. 871) that "It is difficult to overemphasize the importance of approaching policy analysis from a general equilibrium perspective." We revisit this issue by demonstrating, through a specific example, that a partial equilibrium analysis may lead to incorrect insights in a more general framework that considers cross-market consequences. By expanding the scope of analysis to encompass multiple interconnected markets, we reveal potential effects that a narrower examination can overlook. More specifically, we examine how an exogenous increase in housing market transparency creates economic frictions in the labor and financial markets.

We focus on this setting for several reasons. First, the prior literature (e.g., Garmaise and Moskowitz 2004) stresses that information concerns are particularly important in the housing market since it is highly illiquid with idiosyncratic assets, complex pricing structures, and multiple types of market participants. Second, the housing sector is at the heart of the economy and affects multiple connected markets (e.g., labor, industrial, financial) and economic agents (e.g., households, firms, regulators). As such, the housing market offers a unique conduit to study the effects of disclosure regulation in a more general framework. Indeed, housing represents the largest expense for American households, and many are overburdened by this cost (Harvard 2018). A further increase in housing prices places many individuals in a very precarious economic position. If they are not sufficiently compensated for the increased cost of living, individuals may be forced to relocate to a lower-cost area. In turn, this makes local labor markets tighter and creates difficulties for local employers, who face increased rigidity in their labor market. We build on this reasoning and ask two questions. First, we examine the effect of an exogenous increase in housing prices (induced by a legally mandated improved disclosure) on local labor markets. Second, we examine whether and how these housing costs affect the corporate decisions of these local employers. To answer them, we exploit the quasi-natural experiment created by the staggered adoption of mandatory flood risk disclosure laws (FRDL) in the United States.

Coastal flood risk is a significant concern. For example, a rise in sea level of 1.8 meters would inundate areas that are currently home to six million Americans, which would disrupt their livelihood and damage economic activities in the process (Hauer et al. 2016). While the existence of this risk is well understood on a broad level, the precise estimation of its potential impact at the dwelling level is harder to ascertain (e.g., Kusisto 2019). To address this issue, 13 states located in coastal areas introduced laws requiring the disclosure of flood risk in real estate transactions from 1996 to 2017. These flood risk disclosure laws (FRDL) require all sellers to provide a standard form, which includes descriptive questions about various factors (such as flood frequency and insurance coverage) to all prospective buyers before the contract is signed (FEMA 2022). Overall, the disclosure requirement intends to make flood information readily available to buyers at the dwelling level. Using a reasoning commonly applied to financial assets, we expect this additional transparency to increase the average price of real assets. Consistent with this view, Sinha (2022) shows a similar effect when laws improving the disclosure of pollution risk around oil and gas facilities increase the average collateral value for properties located in nearby areas. Indeed, we confirm that flood risk disclosure laws affect housing prices in our sample. The effect is significant, unanticipated, and persistent (e.g., Keane and Neal 2023, Roth 2022, Freyaldenhoven et al. 2021). It holds controlling for market time trends. This abrupt increase in the average local house prices creates a plausibly exogenous shock to employees' costs of living and, as such, to local labor markets and employers.

We provide different additional elements to establish the validity of our instrument. First, we focus on the timing of the laws as opposed to whether a state eventually passes a law (i.e., "when," not "if"). This mitigates the risk that our instrument is correlated with an underlying (but unidentified) state feature. This timing is uncorrelated with a large number of economic, political, and social explanatory variables. We also empirically investigate the possibility that an unspecified omitted variable causes our different results (Cinelli and Hazlett 2020) and find that it is unlikely to be the case. We fail to identify a pre-trend in tests that are sufficiently powerful to detect it (Roth 2022) and the instrument passes different versions of F-tests (e.g., Freyaldenhoven et al. 2021, Young 2022) that confirm the presence of a strong post-shock effect. Lastly, we use numerous comparative statics and ancillary tests throughout the study to support the validity of our instrumental analysis. For example, we find that the effect of the laws is stronger in coastal counties.

Next, we show that local wages increase following the rise in housing prices. This result holds even if we use procedures that allow for a hypothetical exclusion violation of our instrument (Berkowitz et al. 2012, Nevo and Rosen 2012) or focus on coastal counties while controlling the contemporaneous labor market situation in non-coastal counties of the same state. The results are robust to procedures designed to address a hypothetical weakness of the instrument (Keane and Neale 2023). In essence, employees require higher compensation to satisfy their participation constraint in the labor market, which makes hiring and retaining employees more difficult for employers. The effect on local wages is stronger where counties are coastal, local pre-shock employment is higher, fewer people own their dwellings, more people are burdened by housing costs, and residency and employment are more tightly linked (i.e., less frequent commute).

Next, we show that employers are impacted by the increase in housing prices. Specifically, firms adopt more conservative capital structures and investment policies after the increase in housing prices. These effects are stronger in areas when local labor market conditions are already tight, and firms are labor-intensive and own less real estate collateral. Additionally, firms experience lower returns on assets (ROA) and are more likely to relocate to a lower housing cost area. Lastly, we present different empirical results to validate our channel that links our different results. First, prior literature suggests that firms with higher operating leverage display lower financial leverage and a lower level of investment (e.g., Mauer and Triantis 1994; Matsa 2018; Kahl et al. 2019). We find that the increase in real estate prices is associated with higher operating leverage and greater reported uncertainty for local employers. Second, we find that affected firms discuss housing and labor issues more in the Management Discussion and Analysis (MD&A). Lastly, we find that firms impacted employ fewer people.

Our first contribution is to evidence that additional disclosure can paradoxically increase the amount of friction in the economy, even if it improves efficiency in a specific market. We provide evidence on the costs and benefits of disclosure regulation that are not limited to the market that is directly affected and exemplify the need to consider potential effects in closely interconnected ones (Hazilla and Kopp 1990). More specifically, we show that mandatory disclosure in the housing market affects firms by changing the value of the human capital that they employ. These findings stand in contrast to results that typically show that an increase in disclosure leads to more efficient investment and leverage decisions. As such, they evidence the difficulty of making claims about welfare implications in disclosure studies. For example, the SEC's proposal about standardized climate risk disclosure has received a great deal of attention. Our study informs current regulatory debate by showing that even if the proposed regulation was beneficial for the specific constituents it designed for (financial markets), this additional disclosure may also give rise to unforeseen frictions beyond the costs associated with compliance.

In doing this, we extend the prior accounting literature that has linked the labor market and firm decisions. For example, Bova and Yang (2017) show how product and labor markets interact to establish equilibrium in employees' compensation decisions, whereas other studies examine the influence of labor and product market competition on firms' voluntary disclosure and audit quality (Aobdia 2018; Aobdia and Cheng 2018; Aobdia et al. 2020). We also extend the finance literature suggesting that an increase in the value of collateral assets increases the corporate leverage ratio (Cvijanović 2014) and investment level (Gan 2007; Chaney et al. 2012). In contrast, our results suggest a different and opposite effect of real estate prices on leverage and investment through a labor channel. This finding has macroeconomic implications. Models (e.g., Bernanke and Gertler 1989; Kiyotaki and Moore 1997) explain how a business downturn can damage asset values, thus reducing debt capacity and depressing investment, which exacerbates the downturn. In contrast to this pro-cyclical effect, our results open the door for an opposite counter-cyclical effect, which is associated with an increase in housing prices.

Lastly, aside from our general findings regarding the effect of disclosure outside the market that is directly regulated, our results speak to the effect of the specific shock that we consider, i.e., a series of mandated flood disclosure laws. As such, we add to the literature studying the effects of climate risk on firm decisions (e.g., Ginglinger & Moreau 2019; Dang et al. 2022). We show that climate risk affects firm activities through the loss of an intangible asset (i.e., human capital), which is an important mechanism that is distinct from the effect of the loss of tangible assets that prior studies have explored (Crouzet et al. 2022). Further, there is a large literature on the effect of disclosure on financial assets but the study of the effect on real assets is more limited. We contribute to this stream of research by extending and complementing the results in Sinha (2022).

The rest of the study is organized as follows: Section II develops our hypotheses. Section III presents our main empirical specifications. Section IV describes our sample and instrument. Section V presents the empirical results linking housing prices and local labor market conditions. Section VI relates the effect of housing prices and firm behavior, and Section VII provides a conclusion.

II. HYPOTHESIS DEVELOPMENT

Housing is an important component of employee welfare. On average, Americans spend close to 40% of their expenditures on housing. The percentage is higher for low-income families (from 41% for the lowest quintile of income to 32% for the highest quintile). By comparison, the next 30% of expenditures are approximately evenly split between food and transportation for low- and medium-income households (bottom terciles) and slightly less for the upper third of the distribution (Whitmore-Schanzenbach et al. 2016). Moreover, housing costs are excessive for many households. For example, reports (e.g., Cole and Kiersz 2017) suggest that "between renters and owners, nearly 39 million American households—33%—are paying more than they can afford for their homes." Naturally, some cross-sectional variation across areas exists. For example, data from the Joint Center for Housing Studies of Harvard University indicates that the maximum percentage of "burdened households" (defined as spending more than 30% of their income on housing) is 44.1% in "Los Angeles-Long Beach-Anaheim" in California while the minimum is 13.7% in "Ponca City" in Oklahoma (Harvard 2018). The dataset also suggests that the average rate by metropolitan area is 5% higher in coastal states than in non-coastal states. In economic terms, housing costs represent a binding constraint for many individuals.

Higher housing prices mean tighter labor markets. When housing prices increase, the purchasing power of both renters and owners is affected. The cost of using a dwelling goes up in both cases through a direct effect (rent for renters and property taxes for owners)[1] and an indirect effect, which is created by an increase in the general cost of living (e.g., the cost of schooling children or receiving healthcare is likely to increase when real estate prices are higher). For example, reports (e.g., Graff 2021) suggest that "making $300,000 in San Francisco can still mean you're living paycheck-to-paycheck." This increase in the cost of living means that it is more difficult for employees to satisfy their participation constraint to join or remain in the workforce. The newspaper article titled "High housing costs are one reason behind the 'Great Resignation'" summarizes this point.[2] Surveys indicate that "41% of employed Americans would be willing to

[1] There may also be an opportunity cost for owners if the expected returns of their real estate investment decrease after a price increase.
[2] https://www.marketwatch.com/story/high-housing-costs-are-one-reason-behind-the-great-resignation-heres-where-workers-want-to-move-11638908578

take a pay cut or accept a new job with a lower salary to move to a more affordable location,"[3] and many follow through. For example, Zillow indicates that "the average interstate mover in 2021 moved to a ZIP code where homes were about $35,800 cheaper than where they came from."[4] Anecdotal reports suggest that people have been leaving California (or are not moving in) in record numbers because of the high cost of living, high housing prices, and high property taxes.[5] It is not just the employees relocating but also firms. Relocation companies indicate that "the primary reason [for a company] to relocate, as of now, is workforce."[6] Indeed, firms across many industries from the real estate firm CoStar to Tesla have relocated due to housing costs.[7] This suggests that firms are also affected by an increase in the price of residential housing.

Ultimately, if wages do not increase sufficiently to compensate for a drop in purchasing power, some individuals quit the local workforce and relocate elsewhere. The increase in housing prices impacts local employers either way. Consistent with this view, Abowd and Vilhuber (2012) show that the 35 metropolitan statistical areas (MSAs) that experienced the most extreme real estate price bubbles in the late 2000s also experienced a precipitous drop in stable employment, which was much steeper than the drop in the overall economy. As a result, wages should increase in areas that are affected by an increase in housing prices, although not necessarily enough to maintain the size of the existing labor supply.

---

[3] https://blog.coldwellbanker.com/wp-content/uploads/2021/12/03114154/Coldwell_Banker_Fall_Sentiment_Survey_Key_Findings_II.pdf

[4] https://www.zillow.com/research/moving-to-affordability-2021-30470/

[5] https://www.movingapt.com/top-reasons-why-people-are-moving-out-of-california/

[6] https://nationalpeo.com/business-relocation-analysis-5-real-reasons-to-relocate/

[7] CoStar CEO indicated that he was "acutely aware of how difficult it is now for his employees, some of them making $45,000 or $50,000 a year, to afford to live in cities like his hometown, Washington, D.C." when he decided the location of his new business (https://www.washingtonpost.com/news/digger/wp/2016/10/27/ceo-explains-precisely-why-companies-are-moving-jobs-to-smaller-cities/). Musk indicated "It's tough for people to afford houses (in the San Francisco Bay Area) and lots of people have to come from far away" to get to work at the company's factory and offices. There are limits on how big you can scale in the Bay Area." (https://www.marketwatch.com/story/tesla-is-moving-from-california-to-austin-where-house-prices-surged-45-over-the-last-year-11633708786).

In turn, these higher labor costs affect local employers by reducing their operating flexibility. For example, higher wages mean higher break-even points, greater difficulty to optimize the labor-to-capital ratio, or greater challenges when an expansion is needed. As such, firms facing pressure from labor markets have greater difficulty sustaining sufficient profitability when sales are low in difficult years and benefit less from the good periods of the business cycle. At least as far back as Lev (1974), the literature has recognized that firms with higher operating leverage are intrinsically riskier and more volatile. Consistent with the view that labor flexibility and operating flexibility are linked, Chen et al. (2011) relate labor unions to the degree of operating flexibility and find that these less flexible firms have a higher cost of equity.

This lack of operating flexibility has consequences for corporate decisions.[8] For example, Kahl et al. (2019) show that high fixed-cost firms have lower leverage than low fixed-cost firms and that these firms behave as though they are financially constrained, even if by traditional measures they are not. Therefore, we expect that firms facing tighter labor markets due to housing conditions display more conservative financial (i.e., lower leverage) and investment (i.e., lower investment) policies than those not facing these conditions. Consistent with these predictions, Michaels et al. (2019)' model shows that wages and financial leverage are negatively related due to costly external financing. Augustin et al. (2021) show that firms suffering from price rigidities display lower financial leverage and lower profits. While an increase in the value of assets owned by the firm can make contracts with various stakeholders more complete (e.g., Chaney, Sraer, and Thesmar 2012; Cvijanovic 2014), housing price volatility (i.e., the volatility of the value of real estate assets not owned by the firm) has the opposite effect and creates frictions in corporate financial and investment policies.

[8] The literature also links the operating leverage due to labor and asset returns (e.g., Favilukis and Lin 2016, Donangelo, Gourio, Kehrig and Palacios (2019).

To summarize, we expect that an exogenous increase in disclosure should be associated with higher wages (H1) and with more conservative financing and investment policies (H2) through higher housing prices.

III. MAIN EMPIRICAL SPECIFICATIONS

To investigate these possibilities, we estimate the following panel regression models:

$$(1)\ Wages_{ct} = \alpha_c + \gamma_t + \beta_1\ \widehat{Housing\ Prices}_{ct} + \beta_2\ GDP^{c}_{ct} + \varepsilon_{ct}$$

We use a Two-Stage Least Square (2SLS) specification, where *FRDL* is the instrument. We define *FRDL* as an indicator variable that takes the value of one if the firm is located in county c, where the state has adopted a flood risk disclosure law in year t, and zero otherwise. We elaborate the instrument in Section IV. The regression model includes *GDP (*i.e., the natural log of the county-level gross domestic product in county c in year t), a set of county-fixed effects α_c, that controls for socio-economic characteristics, and year-fixed effects γ_t:

$$(2)\ Corporate\_Outcomes_{it} = \alpha_i + \gamma_{jt} + \beta_1\ \widehat{Housing\ Prices}_{ct} + \beta_3\ X^{f}_{it} + \varepsilon_{i,t}$$

where *Corporate_Outcomes* is a vector of variables associated with corporate investment and financial policy. Following Klasa et al. (2018), we define *Lev* as a continuous variable that is equal to the book value of long-term debt plus debt in current liabilities minus cash holdings divided by the market value of the assets. Conversely, we define *Invest* as a continuous variable that is equal to the ratio of investment (CAPX) divided by lagged plant, property, and equipment (PPE).

The regression model also includes a set of control variables X^{f}_{it}, firm-fixed effects α_i, and industry-year-fixed effects γ_{jt}.[9] The firm-fixed effects control for time-invariant omitted firm characteristics and ensure that estimates of β reflect the average within-firm changes in leverage over time rather than simple cross-sectional correlations. The industry year-fixed effects account

[9] We define the industry fixed effects at the two-digit SIC code level.

for both transitory nationwide factors, such as macroeconomic conditions, and industry-wide time-varying heterogeneity. The firm-level control variables $X^f{}_{it}$ include variables that are commonly found in financial policy regressions (e.g., Lemmon et al. 2008; Serfling 2016). They include size, the market-to-book ratio (a proxy for growth opportunities), ROA (a proxy for profitability), the proportion of assets that are fixed (a proxy for potential collateral), the volatility of ROA (a measure of risk), and an indicator variable for whether the firm paid a common dividend (a proxy for financial constraints). We correct the estimated standard errors for heteroscedasticity and cluster standard errors at the state-year level.[10] We provide detailed variable definitions in Appendix A. We winsorize continuous variables to the 1st and 99th percentiles.

IV. SAMPLE, DESCRIPTIVE STATISTICS, AND INSTRUMENT

Data

We focus on coastal states that passed a flood disclosure law to obtain a consistent set of meaningful shocks. The sample period is from 1995 to 2018. It starts one year before Hawaii and Washington mandated the disclosure laws in 1996 and ends one year after Delaware and Mississippi mandated the disclosure law in 2017. Appendix B provides a more precise timeline for each state.[11] Residential real estate prices come from the Office of Federal Housing Enterprise Oversight (OFHEO, 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 home prices in the U.S. This index is a weighted repeat sales index, which essentially measures the average price

[10] Clustering at the firm level marginally improves the statistical significance of our results (untabulated).
[11] By 2022, 15 states in coastal areas have passed a disclosure law. We drop two states (Alaska and Rhode Island) with very few observations (six firms are located in Alaska and thirty-two in Rhode Island) for several reasons. First, these two states adopted the law in 1993. We increase the homogeneity of our main sample by dropping these two early adopters. Second, some of our data are not readily available before 1994 (e.g., the 10K header, *Uncertainty* or *Flood Risk*). Adding counties from Alaska and Rhode Island does not affect our conclusions (untabulated). We also note that the District of Columbia is sometimes classified as a coastal state. We do not use observations from this jurisdiction in our main analysis but doing so does not change our main conclusions (untabulated).

changes in repeat sales (Bogin et al. 2019). It provides more information than is available in other house price indices because of its breadth of coverage (e.g., Chaney et al. 2012). We use county-level real estate prices as a proxy for the representative value of residential housing. Following Cascino et al. (2021), *Wages* is the natural log amount of the average annual wage in county c in year t. Our county-level sample covers approximately 600 counties in 13 states over 24 years (13,708 country-year observations). We provide descriptive statistics for this sample in Panel A of Table 1. The mean and median values of the different variables are reasonably close to each other. Untabulated results indicate that the univariate correlations between *Housing_Prices* and either *FRDL* or *Wage*s are both positive and significant at the 1% level.

Our firm-level sample consists of all U.S. firms in the merged CRSP/Compustat database (excluding utilities, finance, insurance, and real estate companies),[12] for which we can construct the variables that were used in our main capital structure and investment tests. We follow the previous literature (e.g., Gan 2006, Chaney et al. 2012, Cvijanović 2014, Mao 2021) and proxy for the firm's location using its headquarters. We identify a firm's location at the county level based on its disclosures in the header section of 10-K/Qs filed on EDGAR.[13] The final sample contains 37,468 firm-year observations.[14] We present descriptive statistics in Table 1, Panel B that are broadly consistent with those in Klasa et al. (2018).

We next present the Pearson correlations for the firm-level variables in Table 1, Panel C. The correlations between *FRDL* and our different corporate outcome variables (*Lev*, *Invest*) are significantly negatively correlated at the 1% level. The correlation between $\widehat{Housing\_Prices}$ and *Lev* is significantly negative. Although the univariate correlation between $\widehat{Housing\_Prices}$ and

[12] SIC codes between 4900 and 4999 and 6000 and 6999.
[13] We thank Bill McDonald for making the data available at https://sraf.nd.edu/data/augmented-10-x-header-data/.
[14] Variations in data requirements across tests lead to different sample sizes in some ancillary tests.

Invest is positive, they become significantly negative at the 1% level when we adjust for the cross-sectional variation through firm-fixed effects. These correlations provide initial evidence for our hypotheses and suggest that our later results are not caused by the inclusion of inappropriate controls. Further, most of the control variables are significantly correlated with the dependent variables and have the predicted signs (e.g., Klasa et al. 2018; Dang et al. 2022).

< Insert Table 1>

Instrument

We use the mandatory flood risk disclosure laws as a series of exogenous shocks to employees' cost of living. We rely on a standard economics of information mechanism to motivate why these laws should affect housing prices. On a global scale, flood risk is a material, multi-form, and evolving risk (Kruczkiewicz et al. 2022). However, while inhabitants of coastal states are aware of this risk, its consequence at the dwelling level are harder to evaluate. For example, lawyers specializing in climate and water issues note that flood risk "really sets potential home buyers to be in a bad situation where they are buying property where they are not fully informed of the risk" (see Moran, 2018). Studies (e.g., Scata 2022) confirm this intuition. As a consequence, this leads to opacity in real estate markets. To address this issue, 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 (McClurkan 2019; Kusisto 2019). Indeed, the fact that so many states (and countries outside the US) passed these laws confirms that obtaining unbiased estimates of idiosyncratic flood risk at the dwelling level is difficult (FEMA).[15]

[15] The Federal Emergency Management Agency (FEMA) states "disclosing flood risk information during real-estate is a timely and effective way to enable [homebuyers] to make better risk-informed investment decisions."

Previous studies (at least as far back as Botosan 1997) have shown that reducing the estimation risk by improving transparency increases the average value of financial assets; we apply the same reasoning to real assets. Providing dwelling-specific information increases or decreases asset values (depending on the new information) if market participants had a good estimation of the average risk in a local market but an incomplete understanding of the cross-sectional variations within a market before the enactment of the laws. However, removing the idiosyncratic uncertainty from the estimation process should lead to higher average prices, even if the average risk was well calibrated before the law. Essentially, there is a pooling equilibrium between high- and low-risk dwellings in local housing markets before the enactment of these laws. By providing more precise information to prospective buyers, the laws separate them within markets based on their idiosyncratic flood risk. This decrease in parameter uncertainty and information asymmetry reduces both the average premium for uncertainty-averse buyers and the adverse selection in the housing markets.[16] Further, this initial shock to the willingness to pay induced by better disclosure also affects the collateral value of housing, which facilitates mortgage origination and creates a positive feedback loop that further supports price increases. Our reasoning is broadly consistent with the motivation expressed in Sinha (2022) who shows that increasing the disclosure about pollution around oil and gas sites reduces uncertainty about local pollution risk and leads to a subsequent increase in collateral value and mortgage availability. Chen (2021) also shows that FRDL increases the likelihood of obtaining a mortgage. We also note that out setting is somewhat analogous to catastrophe ("cat") bonds.[17] Although we are not aware of research on the effect of disclosure on cat bonds, prior studies have shown that their pricing is particularly affected by

[16] There is uncertainty if the flood risk cannot be precisely characterized by anyone and information asymmetry if sellers know more about the effect on the dwelling than buyers. Both cases strike us as plausible.
[17] A cat bond is an asset that pays a coupon with a default clause linked to a natural disaster. A house pays a rent (or saves the cost of a rent) unless it is damaged by a disaster.

parameter uncertainty (Hofer et al. 2021) as well as ambiguity aversion and loss aversion (Bantwal and Kunreuther 2000). Once the housing market has been shocked through an information channel, labor markets are affected through a neo-classical mechanism (i.e., higher prices lead to a lower supply of labor through relocation).

We purposely focus on the timing of the laws (as opposed to the passage of the laws) in our main tests (i.e., "when," not "if").[18] We believe that this timing is plausibly exogenous. These laws were passed with natural disaster considerations in mind (Moran 2018; McClurkan 2019; Kusisto 2019) and should not be affected by the socio-economic situation of the states. To support this empirically, we estimate the following panel regression model:

$$(3)\ FRD_{s,t} = \alpha + \gamma_t + \beta_1\ Disasters_{st} + \beta_2\ Controls^{s}_{st} + \varepsilon_{s,t}$$

FRD equals one if a state has adopted a flood risk disclosure legislation in that year and zero otherwise. *Disasters* is the average number of natural disasters that may cause flooding (such as coastal storms, floods, hurricanes, or typhoons) and have affected a given state in the past three years. *Controls* is a vector of 18 state-level socio-economic variables. Table 1, Panel D, shows that *Disasters* is significantly positive (and remains so if we drop the other 18 controls). This suggests that states are more likely to pass FRDL when disasters are more salient but the effects we consider occur several years after the original disaster. Thus, these original calamities are unlikely to affect our conclusions (controlling for *Disasters* in our subsequent main market- and firm-level regressions does not alter our conclusions and *Disasters* is insignificant in all these regressions). None of the state-level economic, political, and fiscal factors predicts the timing of FRDL adoption.

We also note that our setting is further immune from a potential endogenous relation, as we focus on labor markets (controlling for different socio-economic variables), not on housing

[18] However, using non-adopting coastal states in stacked regression tests does not affect our conclusions (untabulated).

markets. In our context, a reverse causality would mean that when state legislatures expect an increase in wages (or an increase in housing prices caused by increased wages) and, at the same time, a drop in both corporate leverage and investment relative to the national average (several years in the future and independently of the current socio-economic situation), they systematically respond by passing laws that mandate housing flood risk disclosures. This strikes us as implausible, particularly in terms of public policy response.

We next test whether local housing prices increase following the disclosure regulation by estimating Equation (4) (this is essentially the first stage of our main specifications):

$$(4)\ Housing\_Prices_{c,t} = \alpha_c + \gamma_t + \beta_1\ FRDL_{ct} + \beta_2\ GDP^{c}_{ct} + \beta_3\ Disasters_{st} + \varepsilon_{c,t}$$

where α_c and γ_t are county-fixed effects and year-fixed effects, respectively. We find in Table 1, Panel E, that flood risk disclosure laws generate a positive shock to housing prices, which increases the costs of living for employees in these counties. Our conclusions are not affected if we drop one state at a time (e.g., California) or use stacked regressions (Cengiz et al., 2019, Baker *et al.* 2022).[19] We find that local GDP conditions and past disasters also affect housing prices.[20] These results hold controlling for year and county fixed effects that control for generic secular trends in the market. In contrast, we find there is no significant spillover to neighboring states when focal states mandate the law (untabulated). Our results also hold if we drop the neighboring counties located at the state-borders.

In terms of economic magnitude, the coefficient measuring the effect on housing prices is approximately 8%. This is consistent with the 9.4% increase in average property prices identified

[19] This approach necessitates that we use all coastal states, irrespective of whether they implement a disclosure law. This may introduce undesirable heterogeneity in the sample between adopters and non-adopters. However, our results hold when we focus exclusively adopters in our main tests.
[20] Whether past realized disasters affect housing prices is an empirical question as what matters for the price is the future risk. Changes in expectations may or may not be correlated with past realizations.

by Sinha (2022).[21,22] We note that while risk is valued by using the expected value of the cost adjusted for a coefficient of risk aversion, (parameter) uncertainty (Knight 1921, Ellsberg 1961) can be handled using the worst-case (or "maxmin") approach (Gilboa and Schmeidler 1989) and may lead to a drastic effect on prices (Epstein and Wang 1995, Williams 2015). In this case, the relevant statistic is the range upper bound, not the expected value. Di Maggio et al. (2017) find that a one standard deviation reduction in their measure of local economic uncertainty leads to a 10% increase in local real estate transactions. To put things in perspective, suppose that a house can suffer from a flood that destroys half of its value only once over 30 years and that the upper bound of the yearly probability of flood decreases by 0.5% (holding the expected value of the flood likelihood and expected loss constant). This represents about 7% of the initial house value.[23]

Our objective is not to fully characterize housing prices but to identify an exogenous source of variations in these prices. As such, variables that affect housing prices (e.g., the cost of concrete) but are not simultaneously strongly correlated with FRDL are not a concern. As noted above, the timing of FRDL is uncorrelated with a large vector of potential correlates. Nevertheless, to characterize the likelihood that a correlated omitted variable explains the relationship between housing prices and FRDL, we consider the procedure described in Cinelli and Hazlett (2020). We find that the effect of this hypothetical variable needs to be implausibly large to bring down the estimated effect of *FRDL* to zero.[24] This result and the other procedures we implement suggest

[21] Sinha (2022) finds that the average property price rises from $181,680 pre-legislation to $198,780 post-legislation.
[22] Calibrating the effect with respect to prior findings on the magnitude of the flood price discount is challenging because estimates of this discount vary greatly from one study to another. For example, Hino and Burke (2021, page 1) note that "while the majority of studies suggest a price penalty for being in the floodplain, point estimates range from a −75.5% penalty to a 61.0% bonus". Given this very large range of estimates, it is very difficult to know what the real magnitude is, which supports our focus on parameter uncertainty.
[23] Allowing for either multiple floods, an impact on the estimated loss percentage or a utility function with a coefficient of uncertainty aversion leads to a larger effect. The 7% effect is calculated as 0.5 * (1-0.995^30).
[24] For example, *FRDL* remains significant if we allow for the presence of an unspecified correlated omitted variable that is three times as strong as GDP.

that the likelihood of an unspecified correlated omitted variable explaining our results is low. For example, comparative statics indicate that a hypothetical omitted variable would have to be both unconditionally and conditionally correlated with *FRDL* to affect our results. For instance, if states passed laws potentially increasing housing pricing at the same time as FRDL, they would need to disproportionally affect counties with high flood risk to explain our results. To this point, we construct *Coastline* that is equal to one if a county is a coastline one and zero otherwise. The results (untabulated) indicate that the interaction between *Coastline* and *FRDL* is positively significant at the 1% level. This and the fact our results hold controlling for state-year fixed effects show that our findings are not simply caused by state time trends. Instead, this result is consistent with the notion that the effect of separating the pooling equilibrium is greater when the risk is more important (e.g., coastal counties). However, we note that if real estate prices increase in an area for exogenous reasons, the prices in the surrounding areas are also likely to be affected by a shift in demand. As such, we do not expect the effect of the laws to be exclusively limited to areas that are directly affected by flood risk and consider all counties in the states that are covered by the disclosure laws. However, as noted above, there is no discernable spill-over to neighboring states, the level of which our treatment is defined at. These results alleviate the concerns about a potential violation of the Stable Unit Treatment Value Assumption (SUTVA).

Next, to better understand the dynamic of the relation between housing prices and FRDL, we create a series of event year indicator variables *FRDL* indexed from t-5 to t+4, with t=0 being the year a flood risk disclosure law is in effect (Panel A of Figure 1). The variables take the value of one if a law is mandated within t-i, and zero otherwise and is normalized at t-1. We find that the coefficients from $FRDL^{-5}$ *to* $FRDL^{-2}$ are statistically insignificant, both individually and jointly (the p-value of an F-test of joint significance is 0.68), have a small magnitude, and do not exhibit a clear trend. In contrast, the coefficients from $FRDL^{+1}$ to $FRDL^{4+}$ are all consistently positive, and statistically significant, both individually and jointly (the p-value of the F-test is 0.00).

< Insert Figure 1>

Roth (2022) notes that F-tests can be underpowered to detect pre-trends but offers a statistical approach to investigate this issue. Since there is little theory or institutional guidance about what the hypothetical pre-trend may be, we consider the empirics. The (insignificant) average point estimate from t-5 to t-2 is -0.02 while the average of the post-FRDL period is 0.08. In light of these estimates, a reasonable possibility may be +0.01 or +0.02 per period. The power of the test is 0.79 (this value is essentially the 80% benchmark often used as an acceptable degree of power, Cohen 1988) when we consider a trend of 0.01. The power increases to 0.97 if we consider a trend of 0.02. In other words, our tests seem powerful enough to detect reasonable forms of pre-trends. Further, the likelihood ratios are low (0.15 in the first case and 0 in the second), suggesting that our results are more consistent with a parallel trend than with the hypothesized alternatives. Panel B of Figure 1 graphically represents these patterns for a hypothesized trend of +0.01 per year. Lastly, we use the method described by Freyaldenhoven et al. (2021). Inconsistent with the presence of a pre-trend, we fail to reject the hypothesis that the pre-shock coefficients are jointly different from zero (p-value is 0.33). In contrast, we reject the hypothesis that the coefficients are jointly equal to zero post-shock (p-value is 0.00). These different findings further support the appropriateness of our instrument.

Lastly, there might be a concern with the "exclusion restriction" associated with our instrument, that is with the possibility that FRDL have an effect on labor markets and corporate decisions through a channel other than housing prices. This possibility is not directly testable in its most general form but this does not strike us as likely in our setting. Theoretically, we do not see a mechanism that would generate this result. For example, while improving the transparency in the residential housing markets should affect housing prices directly, it is not obvious how this should have a direct effect on wages. Similarly, we do not see a mechanism that affects corporate decisions outside real estate prices. Further, the unspecified mechanism would have to simultaneously yield an increase in wages and a decrease in corporate investment without going through the housing channel. Empirically, we show below that this unspecified effect would have to be highly correlated with housing prices. For example, we apply the procedure described in Cinelli and Hazlett (2020) to all our specifications to detect unspecified omitted variables and always find that the magnitude would have to be implausibly large to bring down the estimated effects to zero. Further, in the cross-section, this alternative channel to housing prices would have to trigger a disproportionate effect on units that are more sensitive to housing prices. In the time series, it would need to show a lag in its effect consistent with the effect of FRDL on housing prices. Finally, as discussed below, our results in the next section hold if we use the estimation procedures outlined by either Nevo and Rosen (2012) or Berkowitz, Caner, and Fang (2012) which allow for "imperfect" instruments that violate the exclusion restriction to some degree.

IV. HOUSING PRICES AND LABOR MARKET CONDITIONS

We next consider the effect of a housing price increase on labor markets. We expect employees to relocate to a cheaper environment if they are not fully compensated for the increase in housing cost, and consequently, we expect the local labor market conditions to become tighter for employers as the supply of labor drops. Additionally, we expect local wages to increase if employees require higher compensation to participate in the local workforce. We present the 2SLS results from Equation (1) in Table 2. Consistent with our expectations (H1), we find in column 1 that wages increase as housing prices increase. The economic effect is such that an increase by one standard deviation of housing prices stimulated by the disclosure regulation shock leads to an increase in the wage level by 12% of its standard deviation.[25] The t-statistic associated with *FRDL* in the first stage is equal to 5.41. Inconsistent with the presence of a weak instrument, different versions of F-tests for excluded instruments are all highly significant, with a p-value of 0.000. For example, the value of the first-stage F-test is above 22 (the Stock-Yogo critical value is 16.38), and the statistic of the Cragg-Donald Wald test for weak instruments is 1,051. Following Young (2022), we implement the Montiel-Olea and Pflueger (2013)'s test and find the effective F-statistic to be 29.27 (the critical value is 23.11). Keane and Neale (2023) suggest that robust tests like the Anderson-Rubin one should be used in lieu of the t-test, even with strong instruments. We find that the Anderson-Rubin Wald test and Stock-Wright S statistics for weak-instrument-robust inferences are 7.82 and 7.03, respectively, and are both significant at the 5% level. Using stacked regressions does not affect our conclusions (but since the literature on stacked regression in the context of 2SLS is not well-developed, we focus on the traditional approach). We also implement two procedures to address a potential exclusion violation of our instrument. First, we use the fractionally resampled Anderson–Rubin (FAR) discussed in Berkowitz, Caner, and Fang (2012) to handle hypothetical exclusion violations of the instrument. Both the full sample and the FAR p-values for the Anderson-Rubin test are below the 0.05 cut-off. Second, we implement the approach outlined in Nevo and Rosen (2012). We obtained lower and upper bounds of the confidence interval of 0.36 and 0.98, respectively, indicating that hypothetically violating the assumption of strict exclusion restriction would not invalidate our conclusions. Our results are also robust to using a reduced form specification (i.e., we replace $\widehat{Housing\ Prices}$ with *FRDL*). The presence

[25] = (0.629*0.168)/0.859

of an unspecified correlated omitted variable needs to be three times as strong as *GDP* to bring down the estimated effect of $\widehat{Housing\ Prices}$ to zero (Cinelli and Hazlett 2020).

< Insert Table 2>

We then consider different comparative statics to increase the validity of our results. First, we examine if the effect is stronger in markets where the flood risk is more important. To investigate this idea, we follow Wooldridge (2010) and interact *Coastline* both with housing prices and with our instrument of *FRDL* in the first stage to create extra instruments for the interaction term. We use a similar approach throughout the analysis when we consider the interactions between the instrument and a partitioning variable. We report the results in column 2. Consistent with our expectations, the baseline results are stronger in coastal counties (at the 1% level). Among other things, this result shows that our findings are not simply a general reflection of state labor trends. To further explore this idea, we estimate our baseline regression (Equation 1) controlling for the regional unemployment rate (i.e., the average unemployment for each coast-year; we cannot meaningfully control for the national level unemployment as we already include year fixed-effects). Alternatively, we estimate Equation 1 using only coastal counties and control for the average unemployment rate in non-coastal counties in the same state-year. Untabulated results show that our conclusions are unaffected in either specification. These results confirm that we are not simply picking national or regional trends in the labor markets or state-level trends unrelated to the laws we consider.

Second, we consider the structure of the housing market. The shock to housing prices may affect renters and owners differently. While both renters and owners are impacted by the income effect (i.e., rents, property taxes, and general cost of leaving increase for both), owners also benefit from an increase in wealth. To investigate this idea, we create *Low_Own*, which is an indicator variable that takes the value of one if the percentage of house ownership in the county is below the sample median, and zero otherwise.[26] We then instrument the interaction between *Housing Prices* with *Low_Own* and report the results in column 3. Consistent with our expectations, the baseline results are stronger in counties with a higher percentage of rentals (at the 1% level).

Third, we expect that the effect of housing prices on wages should be stronger when many inhabitants are already cost-burdened before the increase in housing prices. If this is the case, households should be in a precarious position to absorb any additional housing costs. To verify the prediction, we instrument the interaction between *Housing Prices* and *High_Burden* and re-estimate Equation (3). *High_Burden* is an indicator variable that is equal to one if the ratio of the median home price for existing home sales to the median household income is greater than the sample median before the disclosure law's enactment, and zero otherwise. The data for the burden is available for only MSA areas which leads to a large attrition. The results in column 4 indicate that the interaction is significant at the 1% level.

Next, we consider the effect of commuting. We expect the effect of housing prices on the local labor market to be stronger when there is a greater connection between housing and labor markets. We create *High_Commute*, which is an indicator variable that takes the value of one if the sum of the number of resident workers who commute either from or to another county divided by the number of resident workers is above the sample median, and zero otherwise. We then instrument the interaction between *Housing Prices* with *High_Commute*.[27] We report the results

[26] We obtained the data on ownership from the Census Bureau. However, we were unable to obtain it for the years before 2010. Consequently, we use the 2010 county-level value for this test.
[27] We obtained the data on commuting from the Bureau of Transportation Statistics for the year 2019 but were unable to obtain it for earlier years.

in column 5 and find that the baseline results are weaker (at the 1% level) in counties with a higher percentage of resident workers who work in another county.[28]

Lastly, we examine whether the effect of an increase in housing prices is greater in areas where the labor market was already tight before the shock to housing prices, as slack in the supply of labor may allow the market to proceed without much disruption. To do this, we create *High_Labor_Constr* (an indicator variable that takes the value of one if the mean employment rate values before the mandates are above the sample median, zero otherwise). We then instrument the interaction between *Housing Prices* with *High_Labor_Constr*. The results in column 5 indicate that the interaction is significant at the 1% level.[29] Overall, these different results are consistent with our first hypothesis (H1), which relates higher housing prices with higher wages.

VI. HOUSING PRICES AND CORPORATE OUTCOMES

Housing Prices, Leverage, and Investment

We consider the effect of $\widehat{Housing\,Prices}$ on corporate policies. We expect $\widehat{Housing\,Prices}$ to be negatively correlated with leverage and investment. We present the results for leverage in Table 3. Consistent with our expectations, column 1 shows that $\widehat{Housing\,Prices}$ is negatively associated with *Lev* at the 1% level. Inconsistent with a weak instrument, the F-statistic that is associated with *FRDL* in the first stage is equal to 55.21, the value of the Cragg-Donald Wald F-test for a weak instrument is 3,031 and the Montiel-Pflueger effective F-statistic is 99.62. The statistic for the Anderson-Rubin Wald test is 9.43 (significant at the 1% level) while the FAR p-value is below the 0.05 cut-off (Berkowitz et al. 2012).[30] We obtain similar results

[28] We reach the same conclusion if we consider *High_Commute_From* (based on the number of workers who commute from another county to the county under consideration divided by the number of workers in the county) and *High_Commute_Away* (based on the number of resident workers who commute to another county divided by the number of resident workers in the county).
[29] Since we use a constant for the partitioning variable, our indicator variables absorb it.
[30] To handle dimensionality, we only include industry-year fixed-effects in Montiel-Pflueger and FAR tests.

using stacked regressions. Moreover, the effect of an unspecified correlated omitted variable needs to be three times as strong as *Size* to bring down the estimated effect of $\widehat{Housing\ Prices}$ to zero in this specification (Cinelli and Hazlett 2020). To address potential non-linearities, we define Lev_q as the rank-based variable representing the quartile of Lev for the observation. Column 2 shows that $\widehat{Housing\ Prices}$ is negatively associated with Lev_q at the 5% level. The control variables have signs that are consistent with other studies (Klasa et al. 2018; Dang et al. 2022). For example, larger firms with greater fixed assets have higher financial leverage.

We then focus on corporate investment in Table 4 and consider both *Invest* and *Invest_q* (the analog of Lev_q). $\widehat{Housing\ Prices}$ is negatively associated with investment in both columns 1 and 2 but it is only significant with *Invest_q* at the 5% level, which suggests the presence of non-linearities in our sample. The statistics of the Cragg-Donald Wald, Montiel-Pflueger, and Anderson-Rubin Wald tests are 3,031, 86.67, and 4.46, respectively, in the latter case. We re-estimate Model (2) with *FRDL* forwarded two ($FRDL^{2+}$) or three periods ($FRDL^{3+}$) in columns 3 and 4. The results indicate that $\widehat{Housing\ Prices}^{2+}$ and $\widehat{Housing\ Prices}^{3+}$ are negatively and statistically significantly associated with *Invest* at the 10% and 5% level, respectively. In other words, while some firms are particularly sensitive to the effect of an increase in housing prices on investment and react immediately, it takes more time for the rest of the distribution to be affected.

< Insert Tables 3 and 4>

We also note that we use the firm's headquarters to determine its location to be consistent with the previous literature on real estate and collateral value. However, this measures the effect of the housing shock on the workforce with noise, as the workforce may not be exclusively located near the headquarters (the same is true for previous studies that focused on the collateral value). To ensure that we are not drawing undue conclusions, we create a "restricted" sample of firms that are “local” (below the mean geographic dispersion data reported by Garcia and Norli (2012))[31] and industrial (sic codes between 2000 and 3999). Although this segment is materially smaller (9,476), we still find lower leverage and investment levels after an increase in housing prices. In contrast, we do not observe the same results when we focus on dispersed firms or service firms (for which the workforce is likely to be more dispersed).

The Labor Channel

We then empirically investigate the channel that links our results in Tables 2 and 3 by considering different comparative statics that increase the validity of our results. First, we examine if the effect of the housing shock on corporate decisions is stronger for firms that are more labor-intensive before the enactment of *FRDL*. We instrument the interaction between housing prices and pre-shock labor intensity. In Panel A of Table 5, the coefficients of the instrumented interactions are negatively significant as expected. Second, we examine if the effect of the housing shock on corporate decisions is stronger for firms with higher labor market constraints. We expect this to be the case in areas with tight labor markets before the regulation. We instrument the interaction between housing prices and pre-shock employment (a measure of labor constraint). The instrumented interactions in Panel B have the predicted signs and are statistically significant.

Third, we consider several additional variables that conceptually link our results in Tables 2 and 3. First, we consider the effect of the shock on operating leverage. We expect this leverage to increase as employers become more constrained by labor markets. We measure operating leverage as the sensitivity of the innovations in the growth rate of operating costs to the innovations in the growth rate of sales and define *OL* as one minus the coefficient on the innovations in the

[31] We thank Diego Garcia for making the data available at https://leeds-faculty.colorado.edu/garcia/page3.html

growth rate of sales (Lev 1974).[32] We define $High\_OL$ as an indicator variable that is equal to one if OL is in the top quartile of the sample distribution for operating leverage and zero otherwise and re-estimate Equation (2) using this new dependent variable. We find in column 1 of Panel C of Table 6 that $\widehat{Housing\ Prices}$ is positively correlated with a high $High\_OL$ at the 5% level. Relatedly, we consider the effect of the housing shocks on the uncertainty faced by employers. We follow Loughran and McDonald (2011) and define $Uncertain$ as the natural log of one plus the number of words that are connoting uncertainty based on Loughran and McDonald (2011)'s dictionary in the 10-K in a given firm-year. We expect that firms that face a greater degree of operating leverage will also face more uncertainty. We re-estimate Equation (2) using this new dependent variable (controlling for the length of the 10-K report). Results in column 2 show that the perceived uncertainty increased because of the increase in housing prices. Next, we reinforce the link between our empirical findings in Table 3 with the frictions identified in Table 2. If our results were driven by an unspecified correlated omitted variable or if a reverse causality leads to an increase in housing prices because of the action of local employers, we would not expect these employers to be concerned with labor and housing cost. We create two variables that measure the number of words related to labor (*Labor_MDA*) and housing (*Housing_MDA*).[33] Columns 3 and 4 show that the effect of the instrumented housing price is significantly positive. These results suggest that the effect of the shock is "material" in a financial reporting sense. We also examine the effect of the increase in housing prices on employment at the firm level. To this end, we define *Nbr(Emp)* as the log of the number of employees that are working for firm i in year t. We find that

[32] Following Kahl et al. (2019), we compute operating using data from the past seven years. We also discard the period t to t+7 from the test window to ensure that the post-law calculation is not contaminated by the data in the pre-treatment period.
[33] We use 'appoint', 'employ', 'hire', 'hiring', 'job', 'labor', 'position', 'staff', 'worker', 'workforce', 'recruit', 'salar', 'talent', 'wage' for *Labor_MDA* and 'home', 'house', 'housing' for *Housing_MDA*.

$\widehat{Housing\ Prices}$ is negatively and statistically significantly associated with *Nbr(Emp)* at the 1% level. [34]

< Insert Tables 5 and 6>

The Collateral Channel

Although our focus is on the labor channel, we relate our main results to the traditional collateral channel in an untabulated analysis. We estimate Model (2a) with either *Lev* or *Invest* as the dependent variable and instrument the interaction between *Housing Prices* and *Pre Collaterals* (defined as the mean value of the sum of the buildings, land, and improvement and construction in progress before the disclosure law's enactment). The instrumented interaction is significantly positive at the 1% level in both cases. Consistent with Cvijanović (2014), firms that own a large amount of real estate can use these assets as collateral to borrow more when their value increases. Consistent with Gan (2007), they can also invest more. These results show real estate prices affect tangible investment through the following two channels: the traditional collateral channel (positively) and the new labor channel (negatively). The net effect between the two is generally undetermined, unless we impose more structure, for example through comparative statics. In our sample of high housing cost areas and periods, the labor channel dominates the collateral channel.

Additional Consequences

We conclude this section by exploring some of the consequences of our main findings. First, given that employers affected by the disclosure laws exhibit more constrained and less efficient corporate policies, we expect them to become less profitable than those that do not face the same pressure. To investigate this possibility, we estimate Equation (2) but use *ROA* as the

[34] We do not form predictions regarding the wage bill. In the short run, the wage per employee should increase, while the number of employees decreases. The net effect is uncertain. In the long run, firms should substitute capital for labor; however, the rate of substitution is unclear and probably heterogeneous. Furthermore, the wage bill is not systematically available in COMPUSTAT, which makes a further empirical analysis challenging.

dependent variable. Consistent with our expectations, the results in Column 1 of Table 7 show that ROA declines when housing prices increase. The effect is statistically significant at the 1% level. Lastly, we examine whether firms relocate more frequently once housing prices have increased because of the disclosure laws. To this end, we define *Relocate* as an indicator variable that is equal to one if the firm relocates to another county between year t and t+1, and zero otherwise. We estimate Equation (2) using this new dependent variable. We report the results in column 2. The coefficient that is associated with $\widehat{Housing\ Prices}$ is significantly positive at the 1% level in column 2. We then define *ΔHousing Prices* as the change in the log of housing prices in the county where the firm is located between t and t+1. We find that the mean of *ΔHousing Prices* is negative and significantly different from zero (t-statistic = -2.24) for firms that are covered by a disclosure law after removing the time trend. We estimate Equation (2a) using this new dependent variable and restrict the estimation to firms that relocate during this period. The results in column 3 show that the coefficient associated with $\widehat{Housing\ Prices}$ is significantly negative at the 1% level. In other words, firms are more likely to relocate after experiencing an exogenous increase in housing prices, and conditional on relocating, they move to an area with lower housing costs.[35] These different findings further support our interpretation of the main results.

< Insert Table 7>

VII. CONCLUSION

Prior studies have shown that increasing disclosure limits frictions in the economy. For example, the literature has shown that better risk disclosure increases the value of houses (Sinha 2022). Another stream (e.g., Chaney et al. 2012) has shown that an increase in real assets can benefit firms and households by increasing collateral value. In contrast, we show that an increase

[35] We acknowledge that this last result could also be explained by mean reversion.

in the housing market transparency caused by a better risk disclosure can also have a detrimental effect through a labor channel. We use the timing of mandatory flooding disclosure laws that abruptly increase average local house prices as an instrument. The effect on housing prices is significant, unanticipated, and persistent. The instrument is neither weak nor confounded by market trends. We then show that local labor markets become tighter because of this increase in housing prices. The effect on local wages is stronger when counties are situated on the coastline, fewer inhabitants own their dwellings, more households are burdened by housing costs, fewer people commute, and local pre-shock employment is high. In essence, employees require higher compensation to satisfy their participation constraint in the labor market, which makes hiring and retaining employees a costlier proposition for employers. In turn, tighter labor markets decrease operating flexibility and increase uncertainty for local employers. Affected employers discuss housing and labor issues more, employ fewer people and adopt more conservative capital structures and investment policies. The effects are stronger in areas where labor markets are tight before the shock, and for firms that are more labor-intensive and own less real estate collateral. Affected employers display a lower ROA and are prompted to relocate to a low housing cost area.

References

Abowd, J.M., Vilhuber, L., 2012. Did the housing price bubble clobber local labor market job and worker flows when it burst? *American Economic Review* 102, 589-93

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

Aobdia, D. and Cheng, L., 2018. Unionization, product market competition, and strategic disclosure. *Journal of Accounting and Economics*, 65(2-3), pp.331-357.

Aobdia, D., Li, Q., Na, K., Wu, H., 2020. The Bright Side of Labor Market Power: Evidence from the Audit Industry. *Working paper.*

Augustin, P., Cong, L.F., Corhay, A., Weber, M., 2021. Price Rigidities and Credit Risk. *Chicago Booth Research Paper*

Baker, A.C., Larcker, D.F., Wang, C.C., 2022. How much should we trust staggered difference-in-differences estimates? *Journal of Financial Economics* 144, 370-395

Bantwal, V.J. and Kunreuther, H.C., 2000. A cat bond premium puzzle? *The Journal of Psychology and Financial Markets* 1(1), 76-91.

Berkowitz, D., M. Caner, and Y. Fang. 2012. The validity of instruments revisited. *Journal of Econometrics* 166, 255–266.

Bernanke, B., Gertler, M., 1989. Agency costs, net worth, and business fluctuations. *The American Economic Review* 79, 14-31

Bogin, A., Doerner, W., Larson, W., 2019. Local house price dynamics: New indices and stylized facts. *Real Estate Economics* 47, 365-398

Botosan, C.A., 1997. Disclosure level and the cost of equity capital. *Accounting Review*, 323-349

Bova, F., Yang, L., 2017. Employee bargaining power, inter-firm competition, and equity-based compensation. *Journal of Financial Economics* 126, 342-363

Cascino, S., Tamayo, A., Vetter, F., 2021. Labor market effects of spatial licensing requirements: Evidence from CPA mobility. *Journal of Accounting Research* 59, 111-161

Cengiz, D., Dube, A., Lindner, A., Zipperer, B., 2019. The effect of minimum wages on low-wage jobs. *The Quarterly Journal of Economics* 134, 1405-1454

Chaney, T., Sraer, D., Thesmar, D., 2012. The collateral channel: How real estate shocks affect corporate investment. *American Economic Review* 102, 2381-2409

Chen, H.J., Kacperczyk, M., Ortiz-Molina, H., 2011. Labor unions, operating flexibility, and the cost of equity. *Journal of Financial and Quantitative Analysis* 46, 25-58

Chen, X., 2021. Real effects of flood risk disclosure on home mortgage lending. Working Paper

Cinelli, C., Hazlett, C., 2020. Making sense of sensitivity: Extending omitted variable bias. *Journal of the Royal Statistical Society:* Series B (Statistical Methodology) 82, 39-67

Cohen, J. 1988. Statistical Power Analysis for the Behavioral Sciences. New York: Routledge.

Cole, L.L., Kiersz, A., 2017. Harvard researchers say one-third of Americans overpay for housing — and renters have it the worst. *Business Insider*

Crouzet, N., Eberly, J., Eisfeldt, A., Papanikolaou, D., 2022. Intangible Capital, Non-Rivalry, and Growth. Working Paper

Cvijanović, D., 2014. Real estate prices and firm capital structure. *The Review of Financial Studies* 27, 2690-2735

Dang, V. A., Gao, N., and Yu, T. (Accepted/In press). Climate Policy Risk and Corporate Financial Decisions: Evidence from the NOx Budget Trading Program. *Management Science*

Di Maggio, M, Kermani, A., Ramcharan, R. and Yu E. G., 2017, Household Credit and Local Economic Uncertainty, FRB of Philadelphia Working Paper No. 17-21.

Ellsberg, D., 1961. Risk, Ambiguity, and the Savage Axioms. *Quarterly Journal of Economics* 75 (4), 643-669.

Epstein, L.G. and Wang, T., 2004. Intertemporal asset pricing under Knightian uncertainty. In Uncertainty in Economic Theory (pp. 445-487).

Favara, G. and Imbs, J., 2015. Credit supply and the price of housing. *American Economic Review*, 105(3), pp.958-992.

Favilukis, J., Lin, X., 2016. Does wage rigidity make firms riskier? Evidence from long-horizon return predictability. *Journal of Monetary Economics* 78, 80-95

Freyaldenhoven, S., C. Hansen, J. Pérez Pérez, J. M. Shapiro, 2021, Visualization, identification, and estimation in the linear panel event-study design. NBER Working Paper 29170.

Gan, J., 2007. Collateral, debt capacity, and corporate investment: Evidence from a natural experiment. *Journal of Financial Economics* 85, 709-734

Garcia, D., Norli, Ø., 2012. Geographic dispersion and stock returns. *Journal of Financial Economics* 106, 547-565

Garmaise, M.J. and Moskowitz, T.J., 2004. Confronting information asymmetries: Evidence from real estate markets. *The Review of Financial Studies*, 17(2), pp.405-437.

Gilboa, I. and Schmeidler, D., 1989. Maxmin expected utility with non-unique prior. *Journal of Mathematical Economics*, 18(2), pp.141-153.

Ginglinger, E., Moreau, Q., 2019. Climate risk and capital structure. Université Paris-Dauphine Research Paper

Graff, A., 2021. How making $300,000 in San Francisco can still mean you're living paycheck-to-paycheck. *SFGATE*

Harvard, 2018. Many renters burdened by housing costs in 2018. *Harvard Joint Center for Housing Studies*

Hauer, M.E., Evans, J.M. and Mishra, D.R., 2016. Millions projected to be at risk from sea-level rise in the continental United States. *Nature Climate Change*, 6(7), pp.691-695.

Hazilla, M., Kopp, R.J., 1990. Social cost of environmental quality regulations: A general equilibrium analysis. *Journal of Political Economy* 98, 853-873.

Hilary, G. and McLean, R.D., 2023. Financial Decision Making: An Overview. The Handbook of Financial Decision Making.

Hino, M. and Burke, M., 2021. The effect of information about climate risk on property values. *Proceedings of the National Academy of Sciences*, 118(17), p.e2003374118.

Hofer, L., Gardoni, P. and Zanini, M.A., 2021. Risk-based CAT bond pricing considering parameter uncertainties. *Sustainable and Resilient Infrastructure*, 6(5), pp.315-329.

Kahl, M., Lunn, J., Nilsson, M., 2019. Operating leverage and corporate financial policies. In: AFA 2012 Chicago Meetings Paper

Keane, M. and Neal, T., 2023. Instrument strength in IV estimation and inference: A guide to theory and practice. *Journal of Econometrics*.

Kiyotaki, N., Moore, J., 1997. Credit cycles. *Journal of Political Economy* 105, 211-248

Klasa, S., Ortiz-Molina, H., Serfling, M., Srinivasan, S., 2018. Protection of trade secrets and capital structure decisions. *Journal of Financial Economics* 128, 266-286

Knight, F.H., 1921. Risk, Uncertainty and Profit. Houghton Mifflin Company, Boston, 682-690.

Kruczkiewicz, A, Cian F., Monasterolo I., Di Baldassarre G., Caldas A., Royz M., Glasscoe M., Ranger N., van Aalst M., 2022, Multiform flood risk in a rapidly changing world: what we do not do, what we should and why it matters, *Environmental Research Letters* 17 (8).

Kusisto L., 2019. Is Your Home at Risk of Flooding? The Data Is Hard to Find. *Wall Street Journal*.

Lemmon, M.L., Roberts, M.R., Zender, J.F., 2008. Back to the beginning: persistence and the cross-section of corporate capital structure. *The Journal of Finance* 63, 1575-1608

Leuz, C., Triantis, A. and Wang, T.Y., 2008. Why do firms go dark? Causes and economic consequences of voluntary SEC deregistrations. *Journal of Accounting and Economics*, 45(2-3), pp.181-208.

Leuz, C., Wysocki, P.D., 2016. The economics of disclosure and financial reporting regulation: Evidence and suggestions for future research. Journal of Accounting Research 54, 525-622

Lev, B., 1974. On the association between operating leverage and risk. *Journal of Financial and Quantitative Analysis* 9, 627-641

Loughran, T., McDonald, B., 2011. When is a liability not a liability? Textual analysis, dictionaries, and 10-Ks. *The Journal of Finance* 66, 35-65

Mao, Y., 2021. Managing innovation: The role of collateral. *Journal of Accounting and Economics* 72, 101419

Matsa, D.A., 2018. Capital structure and a firm's workforce. *Annual Review of Financial Economics* 10, 387-412

Mauer, D.C., Triantis, A.J., 1994. Interactions of corporate financing and investment decisions: A dynamic framework. *The Journal of Finance* 49, 1253-1277

McClurkan, G.., 2019. Commentary: As Flood Threat Grows, Florida Must Mandate Flood Risk Disclosure. *Insurance Journal.*

Michaels, R., Beau Page, T., Whited, T.M., 2019. Labor and capital dynamics under financing frictions. *Review of Finance* 23, 279-323

Olea, J.L.M. and Pflueger, C., 2013. A robust test for weak instruments. *Journal of Business & Economic Statistics*, 31(3), pp.358-369.

Moran Greta., 2018. Your future home could be in a flood zone — and no one's required to tell you. *Grist Magazine*.

Nevo A., Rosen A. 2012. Identification with imperfect instruments, *The Review of Economics and Statistics* 94 (3). 659-671.

NRDC., 2019. How States Stack up on Flood Disclosure: *Natural Resources Defense Council*

Roth, J, 2022. Pretest with Caution: Event-Study Estimates after Testing for Parallel Trends, *AER: Insights* 4(3), 305–322.

Scata Joel., 2022. Undisclosed Flood Damage Financially Soaks Home Buyer: *Natural Resources Defense Council*.

Serfling, M., 2016. Firing costs and capital structure decisions. *The Journal of Finance* 71, 2239-2286

Sinha, Kirti, 2022, Fracking Disclosure, Collateral Value, and the Mortgage Market, *The Accounting Review* 97 (5), 427-454.

Whitmore Schanzenbach, D., R. Nunn, L. Bauer and M, Mumfor, 2016. Where Does All the Money Go: Shifts in Household Spending Over the Past 30 Years. The Hamilton Project. https://www.brookings.edu/wp-content/uploads/2016/08/where_does_all_the_money_go.pdf

Williams, Christopher D., 2015, Asymmetric Responses to Earnings News: A Case for Ambiguity, *The Accounting Review* 90 (2), 785-817.

Wooldridge, J.M., 2010. Econometric analysis of cross section and panel data. MIT Press.

Young, A., 2022. Consistency without inference: Instrumental variables in practical application. *European Economic Review*, 147, p.104112.

Appendix A: Variable definitions

Variable Names	Definitions	Source
FRDL	An indicator variable equals one if county c's year t is after state j has enacted a flood risk disclosure law, and zero otherwise.	Natural Resources Defense Council and state laws
Housing Prices	The natural log of the residential real estate Home Price Index in county c in year t.	Office of Federal Housing Enterprise Oversight
$\widehat{Housing\ Prices}$	The predicted value of *Housing Prices* based on the presence of a disclosure law affecting the county where the firm i is headquartered in year t.	
Wages	The natural log amount of county-level annual mean wages in county c in year t.	QCEW variable name "avg_annual_pay."
Coastline	An indicator variable that is equal to one if a county is a coastline one, and zero otherwise.	U.S. Census Bureau
Low_Own	An indicator variable that takes the value of one if the percentage of house ownership in county c is below the sample median, and zero otherwise.	U.S. Census Bureau
High_Burden	An indicator variable that takes the value of one if the ratio of the median home price for existing home sales to the median household income in county c is greater than the sample median before the disclosure law's enactment, and zero otherwise.	Harvard Joint Center for Housing Studies
High_Commute	An indicator variable that takes the value of one if the sum of the number of resident workers who commute to another county divided by the number of resident workers is above the sample median, and zero otherwise.	U.S. Bureau of Transportation Statistics
High_Labor_Constr	An indicator variable that takes the value of one if county c's mean employment rate before the disclosure law's enactment is above the sample median, and zero otherwise.	U.S. Census Bureau
GDP	The natural log of the county-level gross domestic product in county c in year t.	U.S. Census Bureau
Lev	The book value of long-term debt (dltt) plus debt in current liabilities (dlc) minus cash holdings (che) divided by the market value of assets (prcc_f∗csho+at-ceq) for firm i in year t.	COMPUSTAT
Lev_q	A rank-based variable representing the quartile of *Lev* for the observation	COMPUSTAT
Invest	The ratio of investment (capx) divided by lagged plant, property, and equipment (lagppent) for firm i in year t.	COMPUSTAT
Invest_q	A rank-based variable representing the quartile of *Invest* for the observation	COMPUSTAT
High_OL	The operating leverage is the sensitivity of the innovations in the growth rate of operating costs (xoprq) to the innovations in the growth rate of sales (saleq). *OL* is one minus the coefficient on the innovations in the growth rate of sales. *High_OL* is an indicator variable that takes on the value of one if the firm-year value of *OL* is in the top quartile of the sample distribution, and zero otherwise.	COMPUSTAT
Uncertain	The natural log of one plus the number of words that connotates uncertainty (based on Loughran and McDonald's dictionary [2011]) in the 10-K for firm i in year t.	Loughran and McDonald (2011)
Length	The natural log of one plus the total number of words in the 10-K for firm i in year t.	Loughran and McDonald (2011)
Labor_MDA	The natural log of one plus the number of words that connotate labor in the Management Discussion and Analysis (MD&A) for firm i in year t.	SEC Edgar

Housing_MDA	The natural log of one plus the number of words that connotate housing in the MD&A for firm i in year t.	SEC Edgar
Fixed Assets	The book value of property, plant, and equipment (ppent) divided by the book value of assets (at) for firm i in year t.	COMPUSTAT
ROA Volatility	The standard deviation of a firm's return on assets over the previous five years (firms are required to have at least three years of data during the prior five years to enter the sample).	COMPUSTAT
Dividend	An indicator variable that is equal to one if a firm pays common dividends (dvc) for firm i in year t, and zero otherwise.	COMPUSTAT
Size	The natural log of total assets (at) for firm i in year t.	COMPUSTAT
Market-to-Book	The market value of assets (prcc_f∗csho+at - ceq) divided by the book value of assets (at) for firm i in year t.	COMPUSTAT
Pre_Labor_Constr	The natural log of county c's mean employment rate before the disclosure law's enactment.	U.S. Census Bureau
Pre_Labor_Intens	The natural log of firm i's mean ratio of employees to Plant, Property and Equipment (PPE) before the disclosure law's enactment.	COMPUSTAT
ROA	The return on assets measured as operating income before depreciation (oibdp) divided by the book value of assets (at) for firm i in year t.	COMPUSTAT
Nbr(Emp)	The natural log of the number of employees working for firm i in year t.	COMPUSTAT
Relocate	An indicator variable that is equal to one if the firm relocates to another county between year t and t+1, and zero otherwise.	COMPUSTAT
Δ(Housing Prices)	The change in housing prices in the county where the firm is located between t and t+1 for those that relocated between t and t+1.	Office of Federal Housing Enterprise Oversight
FRD	An indicator equal to one if a state has adopted a flood risk disclosure legislation in that year and zero otherwise	Natural Resources Defense Council and state laws
Disasters	The average number of natural disasters that may cause flooding (such as coastal storms, floods, hurricanes, or typhoons) and have affected a given state in the past three years.	Federal Emergency Management Agency

Appendix B: Time Distribution of Disclosure Law Adoption

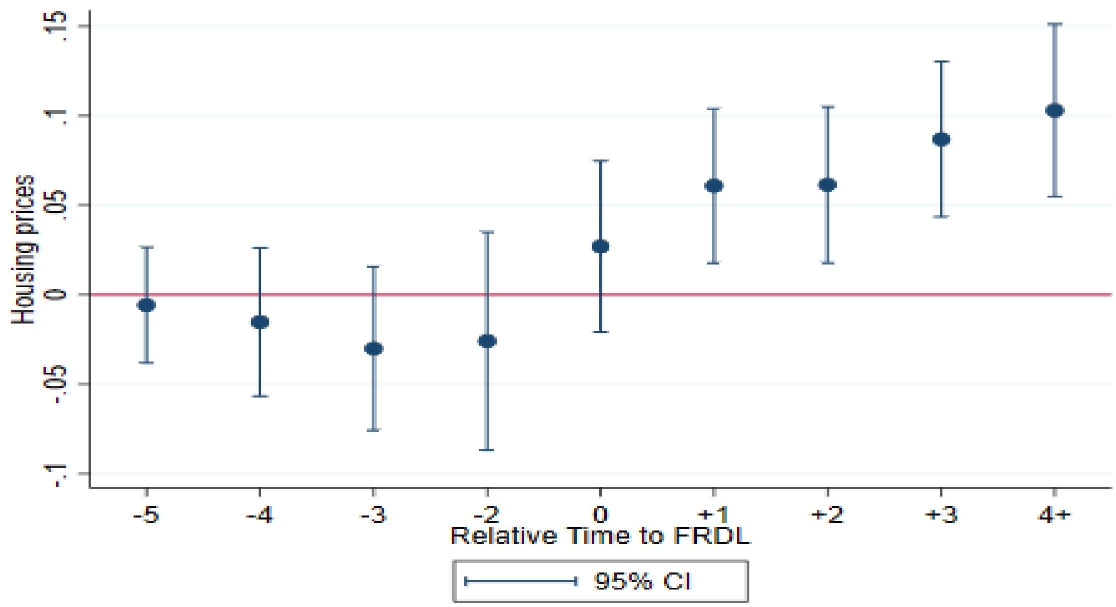
The following table displays the year in which each coastal state originally mandates a flood risk disclosure law. We code the data using each state's website and verify the information using data from the Natural Resources Defense Council.

Year of Mandates	States
1996	Hawaii, Washington
1998	North Carolina
2000	California
2002	South Carolina
2003	Oregon
2004	Connecticut, Louisiana
2005	Maryland
2008	Texas
2010	Pennsylvania
2017	Delaware, Mississippi

Figure 1. Effect of Disclosure Laws on Housing Prices

The following figures plot coefficient results from regressions of housing prices on the indicator for timing of the mandates of the flood risk disclosure law. Panel A plots our estimated coefficients and Panel B used the method proposed by Roth (2022). The sample spans the 1995-2018 period. County fixed effects and year fixed effects are included. All continuous variables are winsorized to the 1st and 99th percentiles. The 95% confidence interval is shaded around the coefficients.

Panel A.



Panel B.

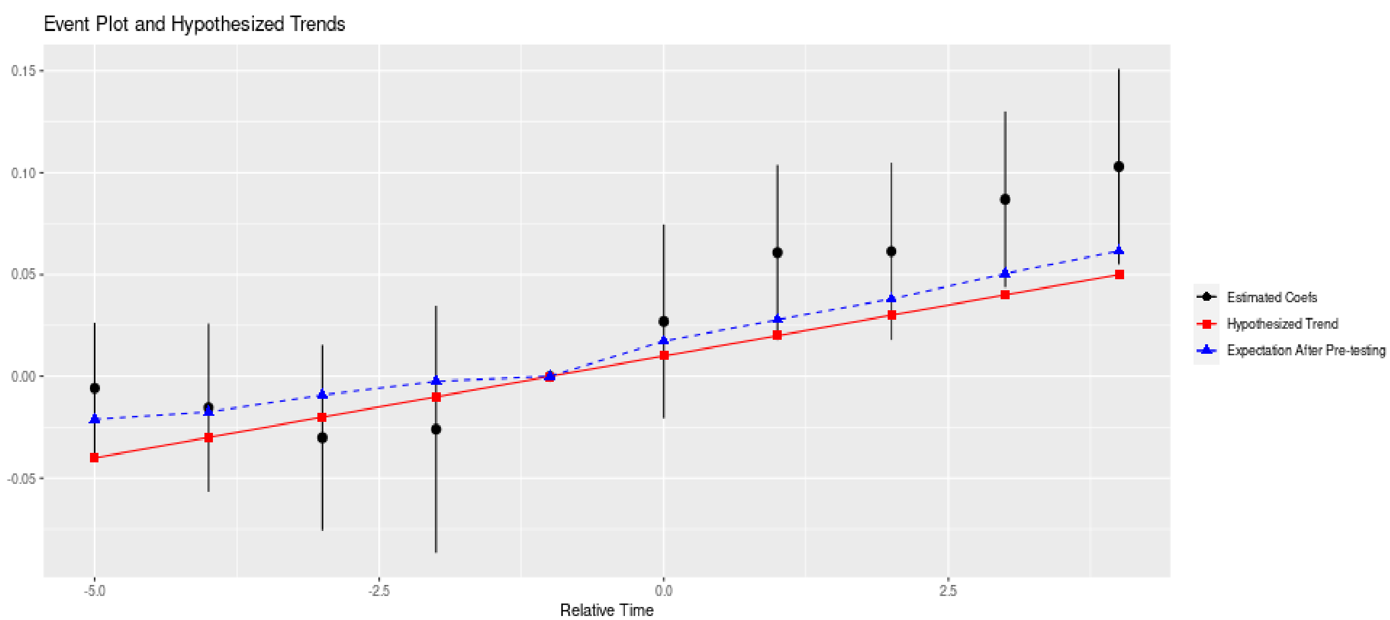


Table 1. Descriptive Statistics

Table 1, Panels A and B report a summary of the statistics of key variables at the county- and firm-levels, respectively. Panel C reports the univariate correlations at the firm level, with the correlation coefficients that have a significance level of 0.05 or better in bold. Panel D reports the relations between disclosure laws and potentially correlated variables. Panel E reports the effects of a mandated flood risk disclosure on county-level residential real estate prices. Variable definitions are in Appendix A. We winsorize all continuous variables at the first and 99th percentiles of their distributions.

Panel A: County Year Sample Descriptive Statistics

	N	Mean	Std. Dev.	Q1	Median	Q3
FRDL	13,708	0.614	0.487	0.000	1.000	1.000
Housing Prices	13,708	5.467	0.557	5.044	5.360	5.832
Wages	13,708	9.014	0.859	8.362	8.997	9.797
GDP	13,708	14.639	1.460	13.594	14.409	15.510

Panel B: Firm Year Sample Descriptive Statistics

	N	Mean	Std. Dev.	Q1	Median	Q3
$\widehat{Housing\ Prices}$	37,468	6.315	0.364	6.010	6.412	6.679
Lev	37,468	0.024	0.269	-0.119	0.005	0.179
Invest	37,468	0.472	0.801	0.119	0.244	0.495
High_OL	21,110	0.250	0.433	0.000	0.000	0.000
Uncertain	27,704	1.267	0.343	1.001	1.273	1.524
Size	37,468	5.075	2.418	3.524	5.123	6.724
Market-to-Book	37,468	3.759	11.907	1.170	1.685	2.836
Fixed_Asset	37,468	0.245	0.243	0.061	0.151	0.360
ROA_Volatility	37,468	0.365	1.639	0.030	0.065	0.160
Dividend	37,468	0.209	0.406	0.000	0.000	0.000

Panel C: Pearson Correlation Coefficients

Panel C presents the Pearson correlations, with the correlation coefficients that have a significance level of 0.05 or better in bold.

Variables	1	2.	3.	4.	5.	6.	7.	8.
1. FRDL	1.00							
2. $\widehat{Housing\ Prices}$	**0.91**	1.00						
3. Lev	**-0.14**	**-0.29**	1.00					
4. Invest	**-0.03**	**0.04**	**-0.14**	1.00				
5. Size	**0.10**	-0.00	**0.16**	**-0.11**	1.00			
6. Market-to-Book	**0.05**	**0.087**	**0.02**	**0.05**	**-0.34**	1.00		
7. Fixed_Asset	**-0.14**	**-0.509**	**0.43**	**-0.14**	**0.18**	**-0.04**	1.00	
8. ROA_Volatility	**0.06**	**0.077**	0.01	**0.11**	**-0.35**	**0.56**	**-0.04**	1.00
9. Dividend	-0.00	**-0.208**	**0.13**	**-0.15**	**0.43**	**-0.08**	**0.20**	**-0.10**

Panel D: Disclosure Laws and Potential Correlated Variables

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 1995 to 2018. The dependent variable is an indicator equal to one (*FRD*) if a state has adopted a flood risk disclosure legislation in that year and zero otherwise. *Disasters* is the yearly average number of natural disasters that may cause flooding (such as coastal storms, floods, hurricanes, or typhoons) and have affected a given state in the past three years (we multiply the coefficient by 100 for readability). *Housing prices* is the natural logarithm of the state-year housing price index. *GDP growth* is the natural logarithm of gross Domestic Product (GDP) at the state-year level. *Income tax* is the ratio of individual income taxes to total taxes at the state-year level. *Population* is the natural logarithm of the population at the state-year level. *Employment rate* is the state employment rate in a given year in percentage points. *Revenue/GDP* is the ratio of revenue to Gross Domestic Product at the state-year level. *Debt/GDP* is the ratio of debt to Gross Domestic Product at the state-year level. *Establishment entry rate* is the state-year establishment entry rate in percentage points. *Net job creation rate* is the state-year job creation rate in percentage points. *Establishment exit rate* is the state-year establishment exit rate in percentage points. *Democratic* is the natural logarithm of the number of Democrats in the Senate. *Republican* is the natural logarithm of the number of Republicans in the Senate. *Education* is the state-year percentage of adults with less than a high school diploma. *Ethnicity* is the state-year percentage of the white population divided by total population. *Gender* is the state-year percentage of males in the total population. *Construction* is the natural logarithm of the state-year total housing construction expenditures. *Property sale* is the natural logarithm of the sum of property sales. *Reallocation rate* is the state-year reallocation rate in percentage points. All independent variables are lagged by one year relative to the dependent variable. It includes state-fixed effects and year-fixed effects. Standard errors are corrected for heteroskedasticity and clustering at the state level (robust t statistics are in parentheses). ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively.

	FRD
Disasters	0.142***
	(5.27)
Housing prices	0.256
	(0.98)
GDP growth	-0.354
	(-1.07)
Income tax	0.107
	(0.19)
Population	-0.146
	(-0.30)
Employment rate	-0.270
	(-0.15)
Revenue/GDP	-1.501
	(-1.07)
Debt/GDP	-0.651
	(-0.50)
Establishment entry rate	-0.028
	(-0.56)
Net job creation rate	-0.003
	(-0.17)
Establishment exit rate	-0.041
	(-0.89)
Democratic	0.039
	(0.30)
Republican	-0.008
	(-0.11)
Education	-1.572

	(-0.98)
Ethnicity	0.074
	(0.10)
Gender	-1.986
	(-0.98)
Construction	0.073
	(1.49)
Property sale	0.007
	(0.96)
Reallocation rate	0.005
	(0.42)
Constant	6.106
	(1.01)
Observations	312
R-squared	0.1312
State FE	Yes
Year FE	Yes

Panel E. Disclosure Laws and Housing Prices

The table reports results from regressions of housing prices on the indicator for the mandates of the flood risk disclosure law (FRDL). The sample spans the 1995-2018 period. The dependent variable is *Housing Prices*, a natural log of the home price index. *FRDL* is an indicator variable equal to one if the county c's year t is after state j has effectuated a flood risk disclosure law, and zero otherwise. *Disasters* is the yearly average number of natural disasters that may cause flooding (such as coastal storms, floods, hurricanes, or typhoons) and have affected a given state in the past three years (we multiply the coefficient by 100 for readability). County fixed effects and year fixed effects are included. All continuous variables are winsorized to the 1st and 99th percentiles.

	Housing Prices
FRDL	0.079***
	(4.72)
GDP	0.110***
	(8.96)
Disasters	-0.047***
	(-4.62)
Intercept	3.830***
	(21.47)
Observations	13,708
R-squared	0.9750
County FE	Yes
Year FE	Yes

Table 2: Housing Prices and Wages

Table 2 reports the effects of instrumented housing prices on wages. Variable definitions are presented in Appendix A.

	Wages	*Wages*	*Wages*	*Wages*	*Wages*	*Wages*
$\widehat{Housing\ Prices}$	0.629***	0.563***	0.358*	0.450	0.797***	0.411*
	(3.45)	(3.04)	(1.80)	(1.59)	(4.83)	(1.97)
$\widehat{Housing\ Prices \times Coastline}$		0.405***				
		(6.14)				
$\widehat{Housing\ Prices \times Low\_Own}$			0.411***			
			(6.07)			
$\widehat{Housing\ Prices \times High\_Burden}$				0.233***		
				(3.85)		
$\widehat{Housing\ Prices \times High\_Commute}$					-0.319***	
					(-4.58)	
$\widehat{Housing\ Prices \times High\_Labor\_Constr}$						0.485***
						(7.79)
GDP	0.142***	0.161***	0.151***	0.072	0.143***	0.122***
	(4.38)	(5.37)	(4.83)	(1.30)	(4.59)	(3.58)
Observations	13,708	13,708	13,708	4,602	13,708	13,708
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Cragg-Donald Wald F statistic	1,051	558.7	525.8	127.2	544.8	523.7
Anderson-Rubin Wald test F statistic	7.82	18.74	25.08	12.46	29.45	33.28

Table 3. Housing Prices and Leverage

The table reports results from two-stage least square (2SLS) regressions of financial leverage on the instrumented county-level house prices by disclosure laws. The sample spans the 1995–2018 period. *Lev* is the book value of long-term debt plus debt in current liabilities minus cash holdings divided by the market value of assets for firm i in year t. *Lev_q* is a rank-based variable representing the quartile of *Lev* for the observation. We include firm-fixed effects and industry-year-fixed effects. Variable definitions are in Appendix A. We winsorize all continuous variables to the first and 99th percentiles.

	Lev	*Lev_q*
$\widehat{Housing\ Prices}$	-0.099***	-0.300**
	(-2.99)	(-2.40)
Size	0.013***	0.041***
	(4.12)	(3.60)
Market-to-Book	0.000**	0.000
	(2.47)	(0.13)
ROA	-0.010***	-0.064***
	(-3.85)	(-7.42)
Fixed_Assets	0.366***	1.475***
	(25.08)	(24.83)
ROA_Volatility	0.001	-0.003
	(0.91)	(-0.42)
Dividend	-0.027***	-0.123***
	(-6.87)	(-6.69)
Observations	37,468	37,468
Firm FE	Yes	Yes
Industry-year FE	Yes	Yes
Cragg-Donald Wald F statistic	3,031	3,031
Anderson-Rubin Wald test F statistic	9.43	5.37

Table 4. Housing Prices and Investment

The table reports results from two-stage least square (2SLS) regressions of financial leverage and investment on the instrumented county-level house prices by disclosure laws. The sample spans the 1995–2018 period. Column one and two is instrumented using *FRDL*, column three is instrumented using $FRDL^{2+}$ and column four is instrumented using $FRDL^{3+}$. *Invest* is the ratio of investment (capx) divided by lagged plant, property, and equipment (lagppent) for firm i in year t. *Invest_q* is a rank-based variable representing the quartile of Invest for the observation. We include firm-fixed effects and industry-year-fixed effects. Variable definitions are in Appendix A. We winsorize all continuous variables to the first and 99th percentiles.

	Invest	*Invest_q*	*Invest*	*Invest*
$\widehat{Housing\ Prices}$	-0.184	-0.369**		
	(-1.02)	(-2.03)		
$\widehat{Housing\ Prices}^{2+}$			-0.362*	
			(-1.93)	
$\widehat{Housing\ Prices}^{3+}$				-0.476**
				(-2.36)
Size	0.080***	0.185***	0.080***	0.080***
	(4.89)	(15.08)	(4.92)	(4.94)
Market-to-Book	0.004***	0.009***	0.004***	0.004***
	(3.41)	(8.76)	(3.40)	(3.39)
ROA	0.078***	0.078***	0.078***	0.078***
	(5.14)	(5.74)	(5.13)	(5.12)
Fixed_Assets	0.058	-0.350***	0.061	0.064
	(0.86)	(-5.60)	(0.90)	(0.93)
ROA_Volatility	0.069***	0.045***	0.069***	0.069***
	(4.95)	(5.14)	(4.98)	(5.00)
Dividend	-0.035***	-0.025	-0.033***	-0.032**
	(-2.77)	(-1.04)	(-2.66)	(-2.59)
Observations	37,468	37,468	37,468	37,468
Firm FE	Yes	Yes	Yes	Yes
Industry-year FE	Yes	Yes	Yes	Yes
Cragg-Donald Wald F	3,031	3,031	2,348	1,711
Anderson-Rubin Wald test F	1.11	4.46	4.69	8.06

Table 5. Housing Prices, Labor, and Firm Behavior

The table reports the results from the 2SLS regressions of the relations among housing prices, labor, and firm behaviors. Column one and two is instrumented using *FRDL*, column three is instrumented using FRD^{2+} and column four is instrumented using FRD^{3+}. *Pre_Labor_Intens* is the natural log of firm i's mean ratio of employees to Plant, Property and Equipment (PPE) before the disclosure law's enactment. *Pre_Labor_Constr* is the natural log of county c's mean employment rate before the disclosure law's enactment. The sample spans the 1995–2018 period. We include firm-fixed effects and industry-year-fixed effects. We winsorize all continuous variables to the 1st and 99th percentiles.

Panel A. Firms' Labor Intensity

	Lev	*Invest*	*Invest*	*Invest*
$\widehat{Housing\ Prices \times Pre\_Labor\_Intens}$	-0.018**	-0.178**		
	(-2.49)	(-2.01)		
$\widehat{Housing\ Prices^{2+} \times Pre\_Labor\_Intens}$			-0.176**	
			(-2.31)	
$\widehat{Housing\ Prices^{3+} \times Pre\_Labor\_Intens}$				-0.163**
				(-2.30)
$\widehat{Housing\ Prices}$	-0.100***	-0.974**		
	(-3.05)	(-2.35)		
$\widehat{Housing\ Prices}^{2+}$			-1.093***	
			(-2.96)	
$\widehat{Housing\ Prices}^{3+}$				-1.056***
				(-3.06)
Size	0.017***	0.095***	0.095***	0.095***
	(5.31)	(4.90)	(4.98)	(5.00)
Market-to-Book	0.001***	0.006***	0.006***	0.006***
	(4.31)	(3.96)	(3.96)	(3.96)
ROA	-0.009***	0.076***	0.075***	0.075***
	(-3.16)	(4.04)	(4.02)	(4.03)
Fixed_Assets	0.398***	0.052	0.054	0.051
	(25.84)	(0.70)	(0.73)	(0.68)
ROA_Volatility	-0.000	0.075***	0.075***	0.075***
	(-0.14)	(3.96)	(3.99)	(3.99)
Dividend	-0.034***	-0.046***	-0.045***	-0.045***
	(-8.51)	(-3.45)	(-3.36)	(-3.38)
Observations	29,102	29,102	29,102	29,102
Firm FE	Yes	Yes	Yes	Yes
Industry-year FE	Yes	Yes	Yes	Yes
Cragg-Donald Wald F	1,372	1,372	1,087	805
Anderson-Rubin Wald test F	5.88	3.87	7.26	6.75

Panel B. Labor market constraint

	Lev	*Invest*	*Invest*	*Invest*
$\widehat{Housing\ Prices \times Pre\_Labor\_Constr}$	-1.155**	-9.003**		
	(-2.00)	(-2.18)		
$\widehat{Housing\ Prices^{2+} \times Pre\_Labor\_Constr}$			-11.425***	
			(-2.69)	
$\widehat{Housing\ Prices^{3+} \times Pre\_Labor\_Constr}$				-10.024**
				(-2.31)
$\widehat{Housing\ Prices}$	-0.156***	-0.621*		
	(-3.43)	(-1.69)		
$\widehat{Housing\ Prices}^{2+}$			-0.949**	
			(-2.43)	
$\widehat{Housing\ Prices}^{3+}$				-1.017**
				(-2.46)
Size	0.013***	0.081***	0.081***	0.081***
	(4.17)	(4.88)	(4.92)	(4.93)
Market-to-Book	0.000**	0.004***	0.004***	0.004***
	(2.48)	(3.42)	(3.41)	(3.40)
ROA	-0.010***	0.078***	0.078***	0.077***
	(-3.84)	(5.12)	(5.10)	(5.09)
Fixed_Assets	0.366***	0.056	0.059	0.062
	(25.15)	(0.81)	(0.86)	(0.90)
ROA_Volatility	0.001	0.068***	0.068***	0.068***
	(0.87)	(4.97)	(5.00)	(5.03)
Dividend	-0.027***	-0.034***	-0.032**	-0.031**
	(-6.79)	(-2.65)	(-2.50)	(-2.43)
Pre_Labor_Constr	6.842*	57.856**	73.336***	64.465**
	(1.82)	(2.19)	(2.70)	(2.32)
Observations	37,468	37,468	37,468	37,468
Firm FE	Yes	Yes	Yes	Yes
Industry-year FE	Yes	Yes	Yes	Yes
Cragg-Donald Wald F	1,528	1,528	1,105	747
Anderson-Rubin Wald test F	6.27	4.91	5.98	4.22

Table 6. Housing Prices, Market Frictions, and Firm Behavior

The table reports the results from the 2SLS regressions of the relations among housing prices, labor, and firm behaviors. *High_OL* is an indicator variable that takes on the value of one if the firm-year value of *OL* is in the top quartile of the sample distribution, and zero otherwise. *Uncertain* is the natural log of one plus the number of words that connotates uncertainty (based on Loughran and McDonald's dictionary [2011]) in the 10-K for firm i in year t. *Length* is the natural log of one plus the total number of words in the 10-K for firm i in year t. *Labor_MDA* is the natural log of one plus the number of words that connotates labor in the Management Discussion and Analysis (MD&A) for firm i in year t. *Housing_MDA* is the natural log of one plus the number of words that connotates housing in the MD&A for firm i in year t. *Nbr(Emp)* is the natural log of the number of employees. Variable definitions are shown in Appendix A. The sample spans the 1995–2018 period. We include firm-fixed effects and industry-year-fixed effects. We winsorize all continuous variables to the 1st and 99th percentiles.

	High_OL	*Uncertain*	*Labor _MDA*	*Housing MDA*	*Nbr(Emp)*
$\widehat{Housing\ Prices}$	0.291**	0.162***	1.097***	0.479***	-0.278***
	(2.26)	(2.96)	(4.47)	(3.01)	(-4.43)
Size	-0.028***	0.051***	0.080***	0.029***	0.670***
	(-4.21)	(18.62)	(8.41)	(4.39)	(93.42)
Market-to-Book	0.000	0.000	-0.001	-0.001	0.005***
	(0.03)	(0.24)	(-0.57)	(-1.18)	(7.17)
ROA	0.011	-0.012***	-0.026**	-0.012**	-0.087***
	(1.37)	(-4.44)	(-2.40)	(-1.98)	(-13.24)
Fixed_Assets	0.038	-0.031**	0.072	-0.023	0.843***
	(0.98)	(-2.12)	(1.06)	(-0.42)	(11.71)
ROA_Volatility	0.028***	-0.004*	-0.001	-0.003	-0.018***
	(3.36)	(-1.86)	(-0.10)	(-1.00)	(-3.83)
Dividend	0.006	-0.013**	0.011	-0.009	0.015
	(0.62)	(-2.40)	(0.42)	(-0.47)	(1.53)
Length		-0.322***	0.083***	0.041***	
		(-24.59)	(5.43)	(4.29)	
Observations	15,701	27,704	22,317	22,317	36,370
Firm FE	Yes	Yes	Yes	Yes	Yes
Industry-year FE	Yes	Yes	Yes	Yes	Yes
Cragg-Donald Wald F	1,453	1,959	599.6	1,960	2,841
Anderson-Rubin Wald F	13.37	10.48	7.15	14.89	18.74

Table 7. Additional Consequences

The table reports the results from the 2SLS regressions of relocation on the instrumented county-level house prices by disclosure laws. The sample spans the 1995–2018 period. Columns 1–2 use the full sample, while column 3 focuses on firms that relocate at t+1. *ROA* is the operating income before depreciation divided by the book value of assets. *Relocate* is an indicator variable that is equal to one if the firm relocates to another county between year t and t+1, and zero otherwise. *Δ(Housing Prices)* is the change in housing prices in the county where the firm is located between t and t+1. Variable definitions are shown in Appendix A. We include industry-year fixed effects in columns 1-2 and year fixed effects in column 3.

	ROA	*Relocate*	*Δ(House Prices)*
$\widehat{Housing\ Prices}$	-0.326***	0.052**	-1.152***
	(-3.56)	(2.08)	(-2.72)
Size	0.270***	-0.005***	0.041
	(15.33)	(-2.67)	(1.08)
Market-to-Book	-0.048***	-0.000	0.000
	(-15.48)	(-0.58)	(0.13)
ROA		-0.002	-0.002
		(-0.57)	(-0.06)
Fixed_Assets	-0.450***	-0.021	-0.859***
	(-4.32)	(-1.45)	(-3.15)
ROA_Volatility	-0.171***	-0.002	0.052
	(-7.81)	(-0.86)	(0.76)
Dividend	-0.033***	-0.001	-0.199
	(-3.70)	(-0.27)	(-1.19)
Observations	37,468	32,452	653
Firm FE	Yes	Yes	Yes
Industry-year/Year FE	Yes	Yes	Yes
Cragg-Donald Wald F	3,034	2,670	6.415
Anderson-Rubin Wald F	12.22	4.21	3.28