Environmental Hazards and Mortgage Credit Risk: Evidence from Texas Pipeline Incidents[*]

Minhong Xu[†] University of Illinois at Urbana-Champaign
404 Mumford Hall,
1301 W. Gregory Drive,
Urbana, IL 61801, USA
mxu23@illinois.edu

Yilan Xu University of Illinois at Urbana-Champaign
309 Mumford Hall,
1301 W. Gregory Drive,
Urbana, IL 61801, USA
yilanxu@illinois.edu

[*] The authors thank Daniel McMillen, Erica Myers, Kathy Baylis, Madhu Khanna, Mark Borgschulte, Peter Christensen, and Sandy Dall'Erba for helpful comments. The authors benefit from the discussions with participants in the 63th Annual North American Meetings of the Regional Science Association International and the pERE Seminar of Department of Agricultural and Consumer Economics in the University of Illinois at Urbana-Champaign. The research project was funded by the National Institute of Food and Agriculture (NIFA) at the United States Department of Agriculture (#ILLU-470-367).

[†] Corresponding author.

Environmental Hazards and Mortgage Credit Risk: Evidence from Texas Pipeline Incidents

Abstract

This paper examines the effects of pipeline infrastructure and the associated incidents on mortgage lenders' credit decisions using the evidence from the 2005-2011 Home Mortgage Disclosure Act (HMDA) loans in Texas. Empirical results of a difference-in-difference approach show a permanently lower origination rate by 1.6% in the pipeline-present areas compared to the pipeline-free areas, which was further enlarged by 1.8% whenever new incidents happened. The permanent difference in credit access reflects lenders' concerns about the collateral value and borrowers' repayment ability. The elevated risk perceptions after the incidents indicate lenders' aversion to the uncertainties about the direct environmental liabilities. Moreover, lenders were more likely to deny low- to middle-income borrowers. Lenders' risk management strategies also evolved with the ease of securitization in the secondary market.

Keywords: bank lending; securitization; pipeline hazards; risk assessment

JEL Classification: G11; G21; Q5

Introduction

The rapidly rising number of pipeline incidents in recent years has called public attention to pipeline hazards. In the past decade, there have been 6,313 pipeline leaks, spills, and other incidents, leading to 145 fatalities, 614 injuries and over 4,398 million dollars in property damage.[1] Although most pipelines are constructed in rural areas, a growing population is exposed to the hazards due to urban sprawl over the years. In addition to the immediate harm to life and property, pipeline incidents generate a negative and persistent externality on housing prices (Simons, 1999; Simons *et al*., 2001; Hansen *et al*., 2006), and often result in environmental contamination in the neighborhoods involved (Islam *et al*., 2016; Matheny, 2016; Medina, 2016). Both housing prices drop and the potential environmental hazards pose a direct threat to the value of properties nearby, which increases the risks of mortgage lending to these neighborhoods. The purpose of this study is to examine how the existing pipeline infrastructure and the associated incidents impact mortgage lenders' perceptions of environmental risks and thus affect their lending and securitization decisions.

Mortgage lenders view environmental hazards as top concerns among a myriad of external risk factors when evaluating potential lending profitability. Their reservations first stem from the potential impairment of the collateral, which may occur right after contaminants are discovered and persist in the long run due to environmental stigma damages (McCluskey and Rausser, 2001 and 2003). Moreover, borrowers' repayment ability could also be weakened by the costs associated with required investigation and remediation (Davis and Levy, 2012). The presence of

[1] Data Source: Authors' calculation using the data from the United States Department of Transportation Pipeline and Hazardous Materials Safety Administration. The original data are available at https://www.phmsa.dot.gov/pipeline/library/data-stats

environmental hazards thus increases the credit risk of the borrowers with limited financial viability. More importantly, under the current environmental laws, lenders themselves may be held liable for the entire cleanup of the contaminated sites in many circumstances. For example, the federal Superfund statute protects a foreclosing lender from the direct environmental liabilities if it makes commercially reasonable efforts to sell the property at the earliest practicable time. Despite the broad statutory protection, bright-line rules such as a fixed period of time for subsequent sale are absent for this safe harbor, which creates uncertainties for lenders seeking both the exemption protection and the optimal time to sell the foreclosed property in a challenged market (Ahrens and Langer, 2008; Gracer and Leas, 2008). Moreover, as petroleum-related contamination is not covered by the Superfund exemption protection, it poses a particular concern to lenders dealing with oil pipeline hazards, in which case lenders could be subject to the liability imposed by other state and federal laws.

Perceiving the potential environmental risks and uncertainties, lenders may avoid or at least limit the number of loans involving contaminated properties (Davis and Levy, 2012). However, when the expansion of mortgage securitization enabled lenders to transfer the credit risk easily to investors in the secondary market, lenders may choose to originate the risky loans and package them for securitization (Jimenez and Saurina, 2006; Dell'Ariccia *et al*., 2012; Keys *et al*., 2012; Simkovic, 2013). Such originate-to-distribute model prevailed before the Great Recession when the secondary market was full of private financial institutions (Berndt and Gupta, 2009; Purnanandam, 2011). Compared with private securitizers, the Government-Sponsored Enterprises (GSEs) have always been maintaining prudent guidelines towards the hazards from oil and gas storage and pipeline transportation that are directly related to the properties securing the loans.[2] As private mortgage securitization virtually disappeared after the Great Recession, the secondary market was dominated by the GSEs, which was accompanied with a sharp tightening of underwriting standards during the same time (Simkovic, 2013). Considering the marketability of the loans after the crisis, lenders could adjust their risk management strategies accordingly in response to the evolution of securitizers' guidelines.

In this study, we empirically investigate mortgage lenders' risk perceptions of pipelines using the evidence from the 2005-2011 Home Mortgage Disclosure Act (HMDA) loans in Texas. Texas has the largest pipeline infrastructure in the United States, with more than 439,771 miles of pipelines representing roughly one-sixth of the total pipeline mileage of the entire country (Railroad Commission of Texas, 2017). We focus on the nonmetropolitan ("nonmetro") Census tracts of Texas where pipeline infrastructure is more concentrated and pipeline failures occur more frequently compared to the metropolitan ("metro") areas. A considerable number of these loans are from lenders that do not have any physical branches in the nonmetro counties where the collateral securing the loan is located, because lenders with branches only in the nonmetro areas are exempt from the HMDA data reporting requirements (Igan *et al*., 2012) Focusing on these loans enables us to observe lenders' risk-taking in a context of severe information asymmetry between lenders and borrowers (Xu and Zhang, 2012).

We employ a difference-in-difference (DID) method to estimate the effects of pipeline infrastructure and the related incidents on mortgage lending. We define all the Census tracts where pipelines exist as the treatment group ("pipeline-present areas") and the Census tracts free

---

[2] See more at https://www.fanniemae.com/content/guide/servicing/d1/1/01.html

of any pipeline infrastructure as the control group (“pipeline-free areas”). We exploit the exogenous variations in the timing, location, and the associated property damage to estimate the treatment effects of pipeline incidents. The DID model measures the effects of pipeline infrastructure by the permanent difference in lenders’ credit decisions between the treatment and the control groups. It further captures the treatment effects of pipeline incidents by the relative change in the credit outcomes between the two groups across the years with versus without any incidents.

We find that an average mortgage loan in the pipeline-present areas was 1.6% less likely to be originated compared to that in the pipeline-free areas after controlling for borrowers’ creditworthiness and demographic characteristics. We interpret the permanent group difference in credit availability from two aspects. First, we find that the denied loans in the treatment group were 3% more likely to be rejected due to insufficient collateral, which suggests that lenders perceived the properties in the pipeline-present areas as having lower collateral value. Second, we identify that the permanent difference in origination rates emerged only among the low- to middle- income borrowers but not among the upper-income borrowers. The results indicate that the differential lending behaviors also resulted from lenders’ concerns about borrowers’ ability to repay a loan in case of remediation costs required for environmental contamination.

We further find that the difference in origination rates between the pipeline-present and the pipeline-free areas enlarged by 1.8% whenever an incident happened. We find no evidence showing that the severity of property damage of the incidents affected lenders’ credit decisions, which implies that it could be the information shock rather than the actual damages of the incidents that increased lenders' risk perceptions. Another possibility could be that lenders were not informed of the real losses promptly because the investigation and public disclosure usually takes time. Meanwhile, we find that the average loan amount in the pipeline-present areas did not change after the incidents, which suggests that immediate property depreciation in the affected neighborhoods could be minimum. We interpret the decreased origination rates as lenders' response to the uncertain future costs of bearing the direct environmental liabilities. We find that such temporary change in lending behaviors disappeared one year after, which is consistent with the findings that lenders' uncertainties would diminish as cleanup was completed and regulatory compliance was achieved (Jackson, 2001). Again, the treatment effects of incidents emerged only among the low- to middle-income borrowers. We run a series of robustness checks to rule out the possibility that borrowers of certain characteristics sorted to the pipeline-present areas after the incidents happened, as we find no simultaneous changes in borrowers' creditworthiness, demographic composition, or any confounding risk factors observed by lenders in the incidents-affected areas. Nor do we find any evidence of sorting based on the density of pipeline infrastructure or the frequency of pipeline incidents.

To investigate how the ease of securitization has affected lenders' risk management strategies for different segments of the population, we split the baseline sample into subsamples by the years before, during, and after the late 2000s subprime mortgage crisis as well as by the income level of borrowers. We find that before the Great Recession lenders managed pipeline hazards in a passive way by taking actions only after observing real pipeline incidents. Specifically, they denied the low- to middle-income borrowers and sold the loans from the upper-income borrowers to the GSEs when the incidents happened. Our findings suggest that the GSEs were able to screen the loans by the observable signals such as borrowers' income when they cannot discern the localized environmental risks. We also find that lenders started to lower the origination rates in the pipeline-present areas especially among the lower-income borrowers after the crisis. The findings suggest that the tightened securitization market during this period forced lenders to manage pipeline risks more aggressively by avoiding the properties exposed to pipelines in a systematic way.

This study complements and extends the studies on how natural or man-made disasters have affected people's risk perceptions of environmental hazards (Nelson, 1981; Gamble and Downing, 1982; Michaels and Smith, 1990; Kolhase, 1991; Kiel, 1995; Carroll *et al*., 1996; Pope, 2008; Naoi *et al*., 2009; McCoy and Walsh, 2014). Existing studies focus on the information asymmetry in environmental risks between sellers and buyers in the housing market, covering a broad range of topics such as earthquake, toxic waste sites, nuclear power plants, chemical plants, incinerators, landfills, flood zones, and wildfire hazards. Highly publicized incidents or public information disclosure, serving as exogenous information shock, could significantly increase buyers' marginal willingness-to-pay for the distance from the hazardous sites. Our study adds a novel perspective of the mortgage market in which lenders are more sensitive to information and own richer resources to evaluate environmental conditions and manage the potential risks. The empirical evidence obtained in the study confirms the expected concerns of lenders about pipeline risks, which expands the scarce literature concerning mortgage lenders' risk perceptions related to environmental contamination (Healy and Healy, 1992; Wilson and Alarcon, 1997; Worzala and Kinnard, 1997; Bond *et al*., 1998; Jackson, 2001).

Empirical Method

We investigate mortgage lenders' potential reactions in both lending and securitization activities to measure their perceived risks of pipeline hazards. First, we expect lenders to decrease the probability of originating the loans exposed to pipeline risks. Instead, they are more likely to deny the loans or conditionally approve the loans. In the latter case, the conditionally approved loans are subject to final verification such as initial inspection, property appraisal, and any other stipulations before they move on to final approval.[3] Moreover, lenders may transfer the credit risk in their portfolio to the secondary market by securitizing the mortgages subject to pipeline hazards. They could sell the originated loans to either the GSEs or private institutions that purchase and package loans for sale to investors (Simkovic, 2013). In summary, we investigate four potential outcomes for each loan, including the likelihoods of being originated and denied, the probability of being sold to any purchasers in the secondary market, and the particular prospect of being sold to the GSEs. In the last two circumstances, we focus on the loans that have already been originated by lenders.

We treat the existence of pipeline infrastructure as a potential hazard and thus define all the Census tracts with the presence of pipeline infrastructure as the treatment group. Then we define the remaining Census tracts where no pipelines have been built as the control group. We start the analysis from estimating the following linear probability model:

$$y_{ijt} = \beta_0 + \beta_1 pipe_j + \beta_2 pipe_j * incid_{jt} + \gamma \mathbf{x}_{ijt} + \varphi \mathbf{z}_{jt} + L_i + C_i + Y_t + \varepsilon_{ijt} \tag{1}$$

[3] See more at https://www.ffiec.gov/hmda/faqreg.htm#action

where i indexes individual loan, j indexes Census tract, and t indexes year. The dependent variable y_{ijt} is the discrete credit outcome for loan i, the treatment indicator $pipe_j$ denotes whether any pipeline goes through Census tract j, $incid_{jt}$ indicates whether any pipeline incident happens in Census tract j of year t, and ε_{ijt} is the individual-specific error term. This model controls for a vector of individual-specific covariates, $\mathbf{x}_{ijt}$, a vector of Census tract-level characteristics, $\mathbf{z}_{jt}$, lender fixed effects, L_i, county fixed effects, C_i, and year fixed effects, Y_t. Thus, our findings are not driven by different business models of lenders, any time-invariant county-level characteristics, or any common economic shocks to the whole market in a given year. The coefficient β_1 captures the average difference in the credit outcomes of individual loans between the treatment and the control groups, which is referred to as the permanent effects. The coefficient β_2 measures any additional difference across the two groups whenever pipeline incidents occur, which is referred to as the incident effects.

Data

Home Mortgage Data

We get access to the information regarding home mortgage lending activities from the Home Mortgage Disclosure Act (HMDA) data, which are maintained by the Federal Financial Institutions Examination Council (FFIEC). The HMDA data cover both depository institutions (banks, savings associations, and credit unions) and non-depository institutions (for-profit mortgage lending institutions), as long as they meet the reporting criteria, such as whether the total assets of the institution exceeded the coverage threshold and whether the institution had a branch or office in an MSA on the preceding December 31.[4] Berkovec and Zorn (1996) estimate that the lenders covered by the HMDA constitute roughly 80% of the total U.S. mortgage originations. These covered lenders are required to report their credit decisions on every mortgage loan they receive, the loan amount, the location of the property tied to the loan up to the Census tract level, the borrowers' characteristics, and the type of purchasers if the loan is sold in the same calendar year.

In this study, we focus on all the depository institutions covered by the HMDA, which differ systematically from the non-depository institutions in that the former obtain funds mainly through accepting deposits from the public and are usually subject to more rigorous regulatory scrutiny. We limit the sample to the conventional, one to four-family, owner-occupied, and first-lien home purchase loans. With these restrictions, all the loans in our sample fall below the Federal Housing Finance Agency's (FHFA) conforming loan limits for Texas of each year. We also exclude the loans from the applicants who are not natural persons, such as a business and a corporation or partnership, which can be identified by the absence of the applicants' demographic characteristics. Further, we drop the loan applications withdrawn by the applicants or closed due to incompleteness, and the loans whose preapproval request were denied by the financial institutions or approved but not accepted by the applicants. Finally, we exclude the loans purchased by an institution to avoid double-counting, since these loans were reported by both the originating institution and the purchasing institution (Dell'Ariccia *et al*., 2012).

[4] For more information see https://www.ffiec.gov/hmda/reporter.htm

Each loan in our final sample falls into one of the following three statuses: (1) originated, (2) denied by the financial institutions in the current calendar year, (3) approved but not accepted. The HMDA codes a loan as status (2) if the borrower has supplied all the necessary information but fails lenders' creditworthiness conditions. Instead, if the borrower has met the creditworthiness conditions but fails any other requirements, such as clear-title requirements and acceptable property survey, which leads the loan not to be consummated, then the loan is coded as status (3).[5] We examine the first two statuses to identify the change in the likelihoods of the loans of being originated and denied. Thus, the difference between the treatment effects on the two outcome variables reflects the change in the likelihood of the loans of being approved by lenders but not accepted by borrowers.

The HMDA requires lenders to document up to three reasons for each denied loan voluntarily. These reasons could be borrowers' ineligibility in debt-to-income ratio, employment history or credit history, insufficient collateral or cash, unverifiable information, incomplete credit application, and denied mortgage insurance. In particular, debt-to-income ratio would be indicated as a denial reason if the applicant had insufficient income for the amount of credit requested, or had excessive obligations in relation to income. Employment history would be indicated if the applicant had temporary or irregular employment. Insufficient collateral value would be indicated if the applicant did not have sufficient type or value of collateral determined by the appraisal of a qualified expert.[6] In our sample, lenders reported at least one denial reason for 79% of the denied loans. We define a set of denial reason dummies for each specific reason according to whether the reason was reported among one of the three denial reasons provided by

[5] See more at https://www.ffiec.gov/hmda/faqreg.htm#action
[6] See more explanations at https://www.ffiec.gov/hmda/pdf/2013guide.pdf

the lender. Thus, we can compare the reasons for the denied loans from the pipeline-present areas with those from the pipeline-free areas with versus without the occurrence of real incidents.

The HMDA further indicates whether a loan is sold in the same calendar year if the loan has been originated. Although the precise identity of the purchasing institution is not provided, the HMDA data report whether the purchaser is private or government-owned or sponsored. Therefore, we consider an originated loan to be sold to the secondary market if a purchaser type can be identified in the HMDA data. Moreover, according to whether the purchaser is Fannie Mae (FNMA), Ginnie Mae (GNMA), Freddie Mac (FHLMC) or Farmer Mac (FAMC), we can identify whether the loan is sold to the GSEs.

For control variables, we collect additional individual-level and Census-tract level information from the HMDA data. We control for the loan amount in dollars, the applicant's annual gross income in dollar amount, gender, race, ethnicity, and the presence of co-applicants. Also, we take into account of Census tract-level time-variant characteristics based on the borrowers' geography, including population density and median family income, as well as time-invariant characteristics including minority population percentage, the number of owner-occupied units, and the number of 1- to 4-family units.

Pipeline Infrastructure and Pipeline Incidents

Our measure of pipeline infrastructure is based on the shapefiles of pipelines provided by the U.S. Energy Information Administration. Figure 1 illustrates the geographic locations of major pipelines that transport crude oil, hydrocarbon gas liquids (HGL), natural gas, and petroleum products in Texas. In the figure, we use the 2003 Rural-Urban Continuum Code to distinguish the nonmetro areas from the metro areas. By comparison, 86% of the nonmetro Census tracts are covered by pipelines, while this percentage in the metro areas is only 52%. Moreover, the average length of pipelines in the nonmetro Census tracts is more than eight times as long as that in the metro Census tracts. We also identify the geographic locations of the petroleum refineries in Texas using the shapefiles provided by the U.S. Energy Information Administration.[7] We calculate the distance from the centroid of each Census tract to the nearest operable petroleum refineries in Texas to control for the impact of refinery facilities, which would otherwise confound the permanent effects of pipeline infrastructure.

Our measure of pipeline incidents is obtained from the Pipeline and Hazardous Materials Safety Administration (PHMSA) of the U.S. Department of Transportation. The data contain records for the full universe of incidents reported by the operators of federally-regulated and state-regulated natural gas and hazardous liquid pipelines. For each reported incident, the PHMSA records the date, location, causes, and consequences regarding fatality, injury, and total property damage. These incidents take the form of leak, rupture, spill, ignition, or explosion and could be caused by multiple reasons. Among all the pipeline incidents the occurred in Texas from 2005 to 2011, 26% were caused by corrosion failure, 29% by equipment failure, 12% by excavation damage, 7% by incorrect operation, 10% by material or weld failure, 3% by natural force damage, 5% by other outside force damage, and the remaining 7% were caused by other unrecorded reasons.

[7] The data are vailable at https://www.eia.gov/maps/layer_info-m.php

Since the HMDA data use Census tract IDs from the Census 2000 series for the data from 2005 to 2011, we project the locations of all the incidents with nonmissing geographic coordinates over this period onto the 2000 Texas Census tract map. Then, we aggregate the number and the associated property damage of the incidents to the Census tract level, which is the lowest geographic level that can be identified in the HMDA data. Figure 2 illustrates each Census tract's total property damage over the seven years. The average financial loss due to pipeline incidents in the nonmetro Texas is nearly eleven times as much as that in the metro areas, which is consistent with the distribution pattern of the pipeline infrastructure across the two regions.

Sample

In this study, we limit the sample to the nonmetro Texas to obtain more precise estimates of the effects of pipeline hazards. We construct a pooled cross-sectional dataset for the loans in 746 Census tracts of 174 Texas nonmetro counties from 2005 to 2011. Each observation represents an individual loan application in one year, with information on the credit decisions of the lender, the existence of pipeline infrastructure, the history of pipeline incidents in the Census tract of the mortgaged properties, and other individual- and Census tract-level characteristics.

In total, we identify 315 pipeline incidents taking place in 160 nonmetro Census tracts from 2005 to 2011 in our sample. Figure 3 plots the annual counts of the incidents and the inflation-adjusted total property damage for each year over the period. The annual frequency of pipeline incidents ranges from the minimum of 33 in 2008 to the peak of 58 in 2009. Meanwhile, the total financial loss varies from 3.25 million dollars in 2006 to 116.5 million dollars in 2005, which is highly correlated with the severity of the incidents in each year.

Table 1 reports the summary statistics by the treatment and control groups. On average, the treatment group had a lower loan origination rate by 1.3 percentage points, a higher loan denial rate by 1.8 percentage points, a lower share of loans sold to the secondary market by 6.1 percentage points, and a lower share of loans sold to the GSEs by 4.4 percentage points. By comparison, while 5.9% of the mortgaged properties in the treatment group were affected by pipeline incidents, the percentage in the control group was only 0.1%. Meanwhile, compared to the average property damage of $101,036 in the treatment group, the financial loss in the control group was almost trivial. The extremely rare incidence and the minor property damage reflect the spillover effects from the adjacent Census tracts, which could happen when oil or gas spills or other contaminants migrate from the pipeline-present neighborhoods to these pipeline-free areas.[8] In our model, we ignore the spillover effects to the control group so that the estimates should be interpreted as a lower bound of the true treatment effects.

Next, comparing the creditworthiness and the demographic characteristics of the borrowers across the two groups, we find that the loan-to-income ratio in the treatment group (1.75) was slightly lower than that in the control group (1.82). Also, 49% of the borrowers in the treatment group had co-applicants, which was higher than the percentage in the control group (46.1%). Further, the treatment group had a lower share of Latinos by 8.2 percentage points and whites by

[8] We identify one Census tract in Stephens County that does not have any pipeline infrastructure but once experienced oil spill due to tank overfill in 2010.

0.2 percentage points. It also had a higher proportion of male applicants by 2.6 percentage points and blacks by 0.4 percentage points.

We further show that the two groups differ from each other in Census tract-level characteristics. By comparison, the pipeline-present areas featured both a lower population density and a lower percentage of minority population. Besides, the treatment group had higher Census tract median family income, more owner-occupied units, and more 1- to 4- family units, which were higher than the statistic of the control group by 7.3%, 28%, and 25%, respectively. Finally, on average the Census tracts in the treatment group are located closer to the petroleum refineries (110 km) compared to those in the control group (127 km).

Given the different observable characteristics between the treatment and the control groups, we use a difference-in-difference model to take into account the permanent difference in the credit availability across the two groups. We also control for a rich available set of observable covariates to address the concerns about the omitted variable bias. Moreover, we demonstrate the common trends in the observable characteristics across the two groups over the years with and without pipeline incidents to show that the estimated treatment effects are not confounded by any changes in borrowers' creditworthiness or demographic composition.

Baseline Results

We begin by estimating the effects of pipeline infrastructure and pipeline incidents on lenders' credit decisions using equation (1). Results are presented in Table 2. The dependent variables are whether a loan is originated (Column 1), whether it is denied (Column 2), whether a loan is sold to the secondary market if it has been originated (Column 3), and whether it is sold to the GSEs (Column 4). In the last two cases, we restrict the sample to all the originated loans. All standard errors are clustered at the Census tract level.

The main coefficients of interest in Table 2 include the effects of pipeline infrastructure, i.e., the coefficient of the treatment dummy $pipe_j$, and the treatment effects of new incidents, i.e., the coefficient of the interaction term $pipe_j * incid_{jt}$. The coefficient of the treatment dummy in Column 1 indicates that a permanent difference existed in the origination rates between the treatment and the control groups, all else being equal. On average, an average loan in the pipeline-present areas was significantly less likely to be originated by 1.6%, and the estimate is significant at the 5% level. In Column 2, the likelihood of the loan being denied in the treatment group was higher by 1.1%, but the evidence is statistically insignificant. Since the loans that failed to be originated could be either denied or approved but not accepted, the difference in the absolute value of the estimates of Column 1 and Column 2 indicates the effects on the probability of the loan being conditionally approved. Next, the coefficients of the interaction terms in the first two columns show that lenders further decreased the likelihood of originating the loans in the affected Census tracts by 1.8% and increased the likelihood of denying the loans by 1.7% whenever new incidents happened.

In Column 3 and Column 4, we restrict the sample to all the originated loans to examine the sales of the loans to the secondary market in a current year. The coefficient of the treatment dummy in

Column 3 indicates that the loans originated in the pipeline-present areas were 1.5% less likely to be sold in the securitization market, but the evidence is only statistically significant at the 10% level. In Column 4, although the probability of being sold to the GSEs was slightly lower by 0.3% for the loans in the treatment group, the difference is statistically insignificant. Next, the positive signs of the interaction terms in the two columns suggest that when new incidents happened lenders tended to sell more originated loans in the affected areas for securitization, but the evidence is statistically insignificant.

The coefficients of the control variables in Table 2 are consistent with our expectation. For example, the loans from those who were male and white and those who had a higher annual gross income, a lower loan-to-income ratio, and a co-applicant were more likely to be originated, while the loans from the Latino and black applicants were less likely to be originated. These findings are statistically significant at the 1% level. Meanwhile, the likelihood of a loan being originated is also positively correlated with some Census-tract level characteristics such as population density and the median family income at the significance level of 1%. The estimated coefficients of the control variables in Column 2 show a consistent pattern, which take the opposite signs compared to those in Column 1.

We also estimate the marginal effects of the monetary loss in pipeline incidents by replacing the incident dummy, $incidjt$, with the total property damage for each Census tract in equation (1). The estimates of the permanent group difference in the four credit outcomes are consistent with the results in Table 2.[9] However, we do not identify strong evidence showing significant marginal effects of the property damage on individual loans' credit outcomes, which indicates that lenders did not base their credit decisions on the severity of the incidents. Compared with the findings in Table 2, our results suggest that it could be the information shock itself rather than the salience of the incidents that affected lenders' risk perceptions. However, as it usually takes weeks or even months to investigate an incident and disclose the results to the public, lenders' weak response to the real loss of the incidents could also result from the delay in the information disclosure.

Timing of the Incidents

Before and After an Incident

We expect that the current and the lagged status of pipeline incidents could have effects on lenders' credit decisions while the future incidents should not. If the leads of the treatment were significantly different from zero, then the common trends assumption would be violated. It would also suggest that there could be unobservable factors correlated with the occurrence of pipeline incidents that explained lenders' credit decisions. We add one lead and one lag of the interaction term to equation (1) and estimate the model using the data from 2006 to 2010.

In Table 3, the lead terms are statistically insignificant across the four columns, which indicate that within one year before the incidents there was no additional significant difference in the mortgage credit availability between the treatment and control groups. Hence, lenders' changing

---

[9] We do not report the estimation results due do space limit. The table is available upon request.

credit decisions in the years when pipeline incidents occurred were less likely to be driven by factors other than the exogenous shock of the incidents. Moreover, although the occurrence of pipeline incidents significantly affected the likelihoods of the loans being originated and denied in the treatment group, we do not identify any significant persistent effects on lenders' credit decisions. The lag terms of the incidents are statistically insignificant across all the columns, which means that the incidents shock quickly died out one year after. The short-lived incidents effects are likely to result from lenders' uncertainties about the post-incidents remediation responsibility and costs, which would diminish as long as the cleanup liability is assigned. Jackson (2001) has similar findings that remediation status has a statistically significant effect on lenders' risk perceptions for a contaminated property. In his study, the percentage of lenders that would not provide a mortgage loan due to excessive environmental risks decreases from 93.2% before the cleanup to 4.2% by the time when the remediation has been completed.

Early versus Late Incidents

In the baseline analysis, we aggregate the loan applications and the pipeline incidents to the annual observations at the Census tract level, since the loan's decision date is not available in the public HMDA data. However, as pipeline incidents could happen across all the months of a year, those occurred later of the year would only affect a portion of the loans issued that year. Thus, the analysis on an annual basis would result in underestimates of the true effects of pipeline incidents. In this section, we demonstrate the potential bias by comparing the effects of the incidents that happened at the beginning of each year with the baseline treatment effects.

Specifically, we only include the Census tracts that experienced the first incident as early as in the first quarter of the year while excluding the Census tracts that had their first incident later than March. We expect these early incidents to affect most of the loans issued during the year and result in the estimated treat effects closer to the true effects. Since the PHMSA data only report the date for the incidents occurred before 2010, we run the robustness check using the sampling period from 2005 to 2009. In Table 4, we report the treatment effects of both the incidents that happened in the first quarter of the year (Panel A) and those that happened at any time of the year (Panel B). We find that the former generates a treatment effect with a larger magnitude on the likelihood of a loan being originated (-3.3%) compared to the latter (-1.9%). Consistently, the treatment effect from the early incidents on the loans' denial rate (3.0%) is also larger in magnitude than the effect estimated using the whole set of the incidents (2.3%). Meanwhile, the early incidents were also more influential on the likelihood of selling an originated loan to the GSEs compared to the average effect of all the incidents (4.0% versus 1.5%).

No Evidence of Sorting

The baseline results suggest that lenders further adjusted their lending decisions whenever new incidents happened on top of the permanent difference in the credit availability between the pipeline-present and the pipeline-free areas. Our preferred explanation is that lenders denied the risky loans due to the elevated risk perceptions following the incidents. In this subsection, we rule out the possibility that the pool of potential borrowers in the treatment group could have changed after the incidents when less creditworthy borrowers sorted to the pipeline-present neighborhoods. Due to the limitation of the HMDA data (Munnel *et al.*, 1996), we cannot observe all the factors contributing to a borrower' creditworthiness. However, we can make use of the available characteristics of the borrowers and the loans in the dataset to test all the potential consequences of sorting that could be observed by us. Our robustness checks also help to exclude the possibility of sorting caused by the borrowers' unobservable features that are correlated with the observable measures of their creditworthiness.

Borrowers' Characteristics

We first test the relative changes in the creditworthiness and the demographic characteristics of the borrowers in the treatment group when the incidents happened compared to the trend in the control group. Specifically, we replace the dependent variables of equation (1) with borrowers' individual-level characteristics, including the loan amount, annual gross income, loan-to-income ratio, the presence of co-applicants, gender, ethnicity, and race, taking into account the same set of Census tract-level characteristics, fixed lender effects, fixed county effects, and fixed year effects. While we allow the existence of a permanent difference between the treatment and the control groups, we expect that new incidents did not lead to further changes in these characteristics. In Table 5, we do not find any significant permanent group difference in either the creditworthiness or the demographic features of the borrowers except that the loan amount and the annual gross income of borrowers in the treatment group was significantly higher than that in the control group. Moreover, when new incidents happened, there was no further change in the average quality of borrowers in the areas affected by the incidents. Our findings indicate that the pool of borrowers in the years when pipeline incidents happened was not significantly different from that in the incidents-free years, which provides evidence for the common trends of the covariates. The results support the view that the treatment effects were unlikely to be caused by the changes in the creditworthiness and the demographic composition of borrowers after the incidents.

Loans' Denial Reasons

Another concern is that the treatment effects could also be confounded by factors that are unobservable to researchers but correlate with lenders' credit decision. For example, lenders could deny the loans from those risk-loving borrowers who are willing to bear the environmental hazards in the neighborhoods affected by pipeline incidents. In this subsection, we make use of the denial reasons provided by the HMDA data to cast light on the changes in the unobservable characteristics of borrowers in the incidents-affected areas. We restrict the sample to all the denied loans to which lenders provided at least one denial reason. Then, we re-estimate equation (1) by replacing the dependent variable with a dummy indicating each denial reason to examine the change in the likelihood of the loans being rejected for each particular reason when pipeline incidents happened.

In Table 6, we only find weak evidence in Column 2 showing that lenders were more likely to reject the loans for the reason of employment history by 2% when pipeline incidents occurred. The estimate is significant only at the 10% level. This evidence indicates that the occurrence of pipeline incidents led lenders to be more cautious about the borrowers who had temporary and irregular employment. Although some of these borrowers might show decent current income, lenders concerned more about the uncertainties embedded in the unstable employment that could affect their ability to make regular payments over the whole term of a loan. Compared to the insignificant change in the likelihood of denying a loan for the reason of debt-to-income ratio (Column 1), our findings suggest lenders' worries in the borrowers' repayment ability in the long run rather than the short run when environmental hazards are identified.

Across all the other columns, we do not find any significant change in the likelihood of denying the loans for any other reason in the incidents-affected areas. For instance, Column 1 shows that there was no significant change in the likelihood of the loans being rejected due to unqualified debt-to-income ratio, which suggests that the risk preference of borrowers in the treatment group did not change disproportionally when pipeline incidents occurred compared to that of the control group. In Column 4, we find that the denied loans in the treatment group had 3% higher likelihood of being rejected due to insufficient collateral value at the significance level of 5%. The results indicate that systematic difference in housing values could exist between the treatment and the control groups, part of which could be attributed to the easement and restrictions on land titles and the inconvenience brought about by the utility services from the pipeline operators. However, when incidents occurred, we find no further change in the probability of the loans being rejected due to insufficient collateral value, which suggests that collateral depreciation did not happen right after the incidents. The robustness check helps rule out the simultaneous changes in the unobservable factors such as borrowers' risk preference and immediate housing devaluation that could affect lenders' credit decisions when pipeline incidents happened.

Cumulative Sorting Effects

As the occurrence of pipeline incidents usually signals the insecurity of the pipeline infrastructure in a neighborhood, the history of pipeline incidents could gradually shape the potential homebuyers' risk perceptions towards the neighborhood in the long run. If the lower origination rates of the loans in the pipeline-present areas were caused by the selection of risky households into these communities, then the selection problem would be more severe in the Census tracts of higher intensity of pipelines, since historically pipeline incidents could happen more frequently in these areas. Likewise, the selection issue would also be more noticeable in the Census tracts with higher frequencies of pipeline incidents over a given period, since more regular and intense incidents could send stronger signals to the potential homebuyers seeking less attractive neighborhoods.

In this subsection, we test whether it is the cumulative sorting effects that explain our baseline results. First, we split the pipeline-present areas by the length of the pipeline infrastructure in each Census tract into four quartile groups. In Table 7, we examine the observable characteristics of both borrowers and neighborhoods across the pipeline-free Census tracts and the first and the fourth quartile groups for all the loan applications from 2005 to 2011. We report the mean characteristics for each group from Column 1 to Column 3 with the standard deviation in parenthesis. We also report the difference with the p-value in parenthesis for an unpaired two-sample mean-comparison t-test in Column 4 and Column 5 respectively to compare the characteristics between the pipeline-free areas and the first quartile group and those between the first and the fourth quartile groups. If sorting existed, we would identify riskier borrowers in the pipeline-present areas and especially in the higher quartile group where pipeline infrastructure was more intense. However, we do not find any evidence of sorting during this period. For example, the t-test results in Column 4 show that the loans in pipeline-free areas had a higher loan-to-income ratio indicating higher credit risk. The applicants in the first quartile group were more likely to be black and less likely to be white, but they were more likely to live in the neighborhoods that feature a lower minority percentage, a higher median household income level, more owner-occupied units, and more 1- to 4- family units. Moreover, the t-test results in Column 5 show that the borrowers in the fourth quartile group had a significantly higher loan amount and income level but had a loan-to-income ratio statistically comparable to that in the first quartile group. They also had a higher likelihood of having co-applicants and living in better neighborhoods. All the available measures of borrowers' creditworthiness indicate that the borrowers in the upper quartile group turned out to be less risky compared to those in the group with a lower intensity of pipelines.

We further categorize all the Census tracts by the total number of pipeline incidents that they encountered from 2005 to 2011 into four groups, in which the Census tracts did not experience any incident or experienced one incident, two incidents, and three or more incidents, respectively.[10] If homebuyers did select themselves to less attractive neighborhoods in response to the signals sent by the incidents over the seven years, then by the end of the period riskier homebuyers would have gradually sorted to the communities where incidents occurred more frequently. In Table 8, we examine the same set of features across the four groups for all the loan applications in 2011, which is the last year of our sample. We report the mean characteristics for each group from Column 1 to Column 4 with the standard deviation in parenthesis. We also

[10] We also split the Census tracts by the quartile of their total property loss from pipeline incidents between 2005 and 2011. We obtain similar conclusions as we get from Table 8. The results are available upon request.

report the difference of the characteristics between the incidents-free Census tracts and the areas with one incident as well as the difference between the Census tracts with one incident and those with three or more incidents in Column 5 and Column 6 respectively with the p-value for the t-test in parenthesis. Again, we do not find strong evidence of sorting. For example, the t-test results in Column 5 indicate that the average loan amount and the borrowers' annual gross income in the Census tracts with one incident were significantly higher than those in the incidents-free areas. However, the loan-to-income ratios were statistically indistinguishable across the two groups. Moreover, the borrowers in the one-incident group were more likely to be male, white, and had a co-applicant. They also had a higher chance of living in the neighborhoods with a lower population density and minority percentage, a higher median household income level, and more owner-occupied units and 1- to 4- family units. Next, the t-test results in Column 6 indicate that the borrowers in the one-incident group and the three- or more-incidents group were statistically similar in terms of the loan amount, the annual gross income, the loan-to-income ratio, and the presence of co-applicants. While the neighborhoods in the Census tracts with three or more incidents had a higher minority percentage, we do not find strong statistical evidence showing that the two groups had a significantly different median family income level. Above all, the evidence indicates that the loans in the areas experiencing more frequent incidents were not riskier than those with less or none incidents at the end of the period of 2005-2011.

Heterogeneous Treatment Effects

Different Income Groups

In this subsection, we examine whether pipeline hazards had the same effects on the credit access of different income groups. All things being equal, the required costs of environmental investigation and remediation for a contaminated property could impose a greater financial burden on the lower-income borrowers in the incidents-affected areas. For this reason, we expect lenders to have particular concerns about the repayment capability of lower-income borrowers who have weaker financial viability. Thus, we follow the rule provided by the Federal Financial Institutions Examination Council to categorize the baseline sample into the low- to middle-income group and the upper-income group according to whether the borrower's annual gross income is below or above 120% of the Texas nonmetropolitan median family income.

We estimate the heterogeneous treatment effects using the two subsamples respectively and report the results in Table 9. In Column 1, we find that the low- to middle-income borrowers in the pipeline-present areas had a lower origination rate by 2.5% compared to their counterparts in the pipeline-free areas at the 5% level. Whenever new incidents occurred, the difference in the origination rates further enlarged by 3.5% at the 1% level. The corresponding pattern appears in the denial rates. By contrast, we do not find statistically significant or economically significant difference in the origination rates and denial rates among the upper-income borrowers as a result of the presence of pipeline infrastructure or pipeline incidence. The findings are consistent with our hypothesis that one of the lenders' concerns about pipeline hazards come from borrowers' inability to repay the loan especially when cleanup liability is required. On the other hand, the results indicate that lenders tended to exclude the lower-income borrowers instead of the higher-income borrowers from their portfolio to manage the potential credit risk.

In Column 3, we also find that the loans from the low- to middle-income borrowers were 2% less likely to be sold to securitizers due to pipeline hazards at the 10% level. Meanwhile, we do not find any significant difference in Column 7 when comparing the sales status of the loans from the upper-income borrowers between the pipeline-present areas and the pipeline-free areas. In the securitization market, although lenders may sometimes hold the risky loans in their portfolio out of the reputational considerations (Agarwal *et al*., 2012), more often they adversely select the risky loans for securitization to transfer the credit risk (Jimenez and Saurina, 2006; Dell'Ariccia *et al*., 2012; Keys *et al*., 2012; Simkovic, 2013). On the other hand, while the secondary market investors are usually unaware of the localized environmental risks, they can make purchase decisions following standards such as the "obligation ratios" that relate the applicant's housing expense and total debt burden to total income (Munnel *et al.*, 1996). In our study, we do not have enough information to distinguish the actions chosen by lenders. Neither can we identify which party plays a dominant role since securitization involves the actions of both sellers and buyers. Nevertheless, our findings suggest lower marketability of the loans related to pipeline hazards from the low- to middle-income borrowers who are usually deemed as having higher default risk in case of environmental hazards. We infer that even though purchasers in the secondary market cannot recognize the environmental risks hidden behind they can always screen the loans relying on the observable characteristics of borrowers.

Before, During, and After the Subprime Mortgage Crisis

In this subsection, we examine how lenders' risk-taking evolved with the stringency of securitizers' guidelines. Specifically, we follow the business cycle reference dates provided by the National Bureau of Economic Research (NBER) to split the baseline sample by the years before (2005 - 2007), during (2008-2009), and after (2010-2011) the late 2000s subprime mortgage crisis.[11] We further divide each period's subsample by the borrowers' income level to identify the target population for which lenders' risk management strategy was designed in each period. We estimate equation (1) using each specified subsample and report the results in Table 10.

Before the financial crisis (Panel A), the origination rate in the treatment group was insignificantly different from that in the control group among both the low- to middle-income borrowers and the upper-income borrowers. Whenever pipeline incidents happened, lenders reduced the origination rate in the affected areas by 3.4% for the low- to middle-income borrowers at the 5% level. By contrast, lenders did not adjust the loan origination rate for the upper-income counterparts in response to the incidents. In the secondary market, however, lenders managed to sell the risky loans from the upper-income borrowers to the GSEs, as is shown that in the incidents-affected areas the originated loans of this group were 2.9% more likely to be sold to the GSEs at the 5% level. The different risk management strategies towards the two income groups further indicate lenders' concerns about the default risk of the lower-income borrowers. Moreover, our findings demonstrate lenders' ability to package and sell the incidents-affected loans successfully even to the GSEs that are supposed to be most cautious about environmental hazards. Meanwhile, as lenders were only able to sell the risky loans from the upper-income borrowers, the findings suggest that the purchasers in the secondary market relied on the observable characteristics to screen the loans for sale when they only had limited information about the localized risks.

[11] According to NBER, the most recent financial crisis lasted from December 2007 to June 2009. See more at http://www.nber.org/cycles.html

During the subprime mortgage crisis (Panel B), we do not find statistically significant evidence showing lenders' differential credit decisions towards pipeline hazards among the low- to middle-income borrowers regardless of whether real incidents happened, though all the coefficients show the expected signs and the magnitudes of the estimated effects are even greater compared to those in the previous period. However, since this period lenders have begun to manage the pipeline risks associated with the loans from the upper-income borrowers by decreasing the origination rate in the pipeline-present areas by 3.0% at the 10% level.

After the crisis (Panel C), lenders' credit decisions started to be significantly different across the treatment and the control groups especially in the low- to middle-income borrowers. As is shown in Column 1 and Column 2, the origination rate in the treatment group was significantly lower by 5.9% compared to that in the control group at the 10% level. Correspondingly, the denial rate was significantly higher by 6.4% at the 5% level. The magnitudes of both estimates are more than three times bigger than those for the full sample. By comparison, there was no statistically significant permanent difference in the origination rates among the upper-income borrowers, which reconfirms lenders' concerns about the lower-income borrowers' repayment ability. We do not identify strong evidence showing lenders' response to the occurrence of new incidents after the crisis. Neither do we find significantly more sales of risky loans in the securitization market during this period.

In sum, our results indicate that before the Great Recession lenders did not take systematically different actions towards the loans subject to potential pipeline risks. Instead, lenders exploited the originate-to-distribute model to manage pipeline hazards only when real incidents happened. It was only after the crisis that lenders started to distinguish the origination rate in the pipeline-present areas from that in the pipeline-free areas in a systematic way. The cyclicality of lenders' underwriting standards reflects the stringency of mortgage securitizers' guidelines across different periods. In particular, lenders' reliance on securitization as a risk management strategy coincided with the period of the mid-2000s, during which the deteriorating lending standards in the secondary market significantly reduced lenders' incentives to carefully screen and monitor borrowers' creditworthiness (Jimenez and Saurina, 2006; Rajan *et al*., 2011; Dell'Ariccia *et al*., 2012; Keys *et al*., 2012; Simkovic, 2013). The tightened securitization after the Great Recession forced lenders to manage the pipeline-related credit risk by discriminating the loans in the pipeline-present areas aggressively, which led to a particular credit crunch among the low- to middle-income borrowers during the period.

Conclusion

This study provides empirical evidence on mortgage lenders' perceptions of environmental risks using data of the mortgage loans and pipeline incidents in Texas. We find that permanent difference in lenders' origination rates and denial rates existed between the pipeline-present areas and the pipeline-free areas. We interpret the difference in terms of lenders' perceived risks of pipelines, which is shown to come from their concerns about both the collateral value and borrowers' repayment ability. We also find that lenders further lowered the credit availability to the pipeline-present areas whenever new incidents happened, which reflects their higher risk perceptions in response to the incidents. We interpret lenders' changing risk perceptions as evidence of their aversion to the uncertainties about the potential environmental liabilities, which would diminish soon after the cleanup responsibilities are assigned after the incidents. Finally, we show that lenders' risk-taking reflected the stringency of securitizers' guidelines across different periods over the financial crisis. Before the crisis, the originate-to-distribute business model helped lenders to manage pipeline hazards passively, while securitizers in the secondary market screened the loans by the observable characteristics when they lack sufficient information about the localized risks. After the tightening of securitization post the crisis, lenders managed pipeline hazards more aggressively by avoiding the lower-income borrowers in the pipeline-present areas due to the concerns about the default risk. Although our analysis is based on the characteristics of the borrowers and the loans available in the HMDA data, we run a series of robustness checks to test the potential consequences of sorting due to both the observable factors and the unobservable factors that are correlated with the observable measures of borrowers' creditworthiness. Our robustness checks rule out the possibility of simultaneous or cumulative sorting of risky borrowers into the neighborhoods where pipelines were constructed densely and where pipeline incidents occurred frequently.

This study contributes to the policy debate on mortgage lenders' liability dilemma under the current environmental laws. While the federal Superfund and a series of state analogs have been designed to create a safe harbor from cleanup liabilities for lenders, a qualified exemption is still subject to when and how lenders interact with the contaminated properties as well as judicial interpretation on a case-by-case basis. Moreover, as the state statutes may differ from the federal scheme, lenders also have to be cautious about the state-specific qualifications for the liability exemption to apply within the state's jurisdiction (Ahrens and Langer, 2008; Sigel and Bandza, 2014). As we show in this study, a significant number of lenders are still reluctant to make loans to the neighborhoods subject to the potential environmental risks. From a policy perspective, lenders' fear of environmental liabilities could lead to a credit crunch to otherwise creditworthy borrowers. More importantly, it could impede the real estate investment and slow down the redevelopment of the historically contaminated sites. This study calls for policy makers' involvement in seeking ways to reduce the deterrent effects of environmental contamination and the associated cleanup liabilities.

This study also sheds new light on mortgage lenders' risk management strategies towards environmental hazards. The empirical evidence suggests that lenders once managed the pipeline-related credit risk by denying the risky loans from the low- to middle-income borrowers and relying on the securitization market to sell the risky loans from the upper-income borrowers. Although we only examine the discrete lending decisions, lenders could always capitalize pipeline hazards into mortgage prices by requiring borrowers to pay the risk premium as a condition of closing the loan. Moreover, lenders may consider using tailored environmental insurance products to insure against the unknown environmental hazards (Bressler, 2002; Davis and Levy, 2012). Lenders may require borrowers to purchase the environmental insurance policies that mainly cover owners for their cleanup expenses above a certain amount for the unforeseen environmental conditions. Alternatively, lenders may use the secured creditor environmental insurance policies, which could pay off the outstanding balance in the event of a default or cover the cleanup costs after foreclosing on the loan.

Finally, our study informs the public discourse about pipeline hazards and identifies the population at risk. We show that pipeline hazards have resulted in a direct loss of mortgage credit access to the low- to middle-income borrowers. Hence, environmental externalities of pipelines could become a potential barrier for those credit constrained families to sustain homeownership. Therefore, enhancing pipeline safety has significant policy implications for lower-income borrowers to improve their mortgage credit availability as well as homeownership opportunity, which is a primary vehicle for these households to accumulate wealth and economic opportunity.

References

Agarwal, S., Y. Chang and A. Yavas. 2012. Adverse Selection in Mortgage Securitization. *Journal of Financial Economics*. 105(3): 640-660.

Ahrens, M.H. and D.S. Langer. 2008. Environmental Risks for Lenders Under Superfund: A Refresher for the Economic Downturn. *Bloomberg Corporate Law Journal*. 3: 482-493.

Berkovec, J. and P. Zorn. 1996. How Complete is HMDA? HMDA Coverage of Freddie Mac Purchases. *Journal of Real Estate Research*. 11(1): 39-56.

Berndt, A. and A. Gupta. 2009. Moral Hazard and Adverse Selection in the Originate-to-Distribute Model of Bank Credit. *Journal of Monetary Economics*. 56(5): 725-743.

Bond, S.B., W.N. Kinnard Jr., E.M. Worzala and S.D. Kapplin. 1998. Market Participants' Reactions Toward Contaminated Property in New Zealand and the USA. *Journal of Property Valuation and Investment*. 16(3): 251-272.

Bressler, A. 2002. Environmental Insurance Changes the Game for Commercial Lenders. International Risk Management Institute. https://www.irmi.com/articles/expert-commentary/environmental-insurance-changes-the-game-for-commercial-lenders

Carroll, T.M., T.M. Clauretie, J. Jensen and M. Waddoups. 1996. The Economic Impact of A Transient Hazard on Property Values: The 1988 Pepcon Explosion in Henderson, Nevada. *Journal of Real Estate Finance and Economics*. 13(2): 143-167.

Davis, A.N. and A.D. Levy. 2012. Lending on Contaminated Properties: Using Environmental Insurance to Manage Environmental Risk and Get the Deal Closed. Corporate LiveWire. October 18, 2012. http://www.shipmangoodwin.com/files/17329_CorporateLiveWire.pdf

Dell'Ariccia, G., D. Igan and L. Laeven. 2012. Credit Booms and Lending Standards: Evidence from the Subprime Mortgage Market. *Journal of Money, Credit and Banking*. 44(2-3): 367-384.

Gamble, H.B. and R.H. Downing. 1982. Effects of Nuclear Power Plants on Residential Property Values. *Journal of Regional Science*. 22(4): 457-478.

Gracer, J. and C. Leas. 2008. Lender Beware: Navigating the Superfund Safe Harbor During Workouts and Foreclosures. *New York Law Journal*. July 14, 2008. http://www.newyorklawjournal.com/id=1202422925354/Lender-Beware

Hansen, J.L., E.D. Benson and D.A. Hagen. 2006. Environmental Hazards and Residential Property Values: Evidence from a Major Pipeline Event. *Land Economics*. 82(4): 529-541.

Healy, P.R. and J.J. Healy Jr. 1992. Lenders' Perspectives on Environmental Issues. *The Appraisal Journal*. 60(3): 394-398.

Igan, D., P. Mishra and T. Tressel. 2012. A Fistful of Dollars: Lobbying and the Financial Crisis. *NBER Macroeconomics Annual*. 26(1): 195-230.

Islam, A., A. Ahmed, M. Hur, K. Thorn and S. Kim. 2016. Molecular-Level Evidence Provided by Ultrahigh Resolution Mass Spectrometry for Oil-Derived DOC in Groundwater at Bemidji, Minnesota. *Journal of Hazardous Materials*. 320: 123-132.

Jackson, T.O. 2001. Environmental Risk Perceptions of Commercial and Industrial Real Estate Lenders. *Journal of Real Estate Research*. 22(3): 271-288.

Jimenez, G. and J. Saurina. 2006. Credit Cycles, Credit Risk, and Prudential Regulation. *International Journal of Central Banking*. 2(2): 65-98.

Keys, B.J., A. Seru and V. Vig. 2012. Lender Screening and the Role of Securitization: Evidence from Prime and Subprime Mortgage Markets. *The Review of Financial Studies.* 25(7): 2071-2108.

Kiel, K.A. 1995. Measuring the Impact of the Discovery and Cleaning of Identified Hazardous Waste Sites on House Values. *Land Economics*. 71(4): 428-435.

Kolhase, J.E. 1991. The Impact of Toxic Waste Sites on Housing Values. *Journal of Urban Economics*. 30(1): 1-26.

Matheny, K. 2016. 30 Years Later, Contamination Remained at Site of Pipeline Spill. Detroit Free Press. May 10, 2016. http://www.freep.com/story/news/local/michigan/2016/05/07/enbridge-line5-oil-spill-hiawatha-national-forest/83507228/

McCluskey, J.J. and G.C. Rausser. 2001. Estimation of Perceived Risk and Its Effect on Property Values. *Land Economics*. 77(1): 42-55.

McCluskey, J.J. and G.C. Rausser. 2003. Hazardous Waste Sites and Housing Appreciation Rates. *Journal of Environmental Economics and Management*. 45(2): 166-176.

McCoy, S.J. and R.P. Walsh. 2014. W.U.I. on Fire: Risk, Salience and Housing Demand. National Bureau of Economic Research Working Paper Series. http://www.nber.org/papers/w20644

Medina, D.A. 2016. Sioux' Concerns Over Pipeline Impact On Water Supply 'Unfounded,' Company Says. NBC News. September 13, 2016. http://www.nbcnews.com/news/us-news/sioux-s-concerns-over-pipeline-impact-water-supply-unfounded-company-n647576

Michaels, R.G. and V.K. Smith. 1990. Market Segmentation and Valuing Amenities with Hedonic Models: The Case of Hazardous Waste Sites. *Journal of Urban Economics*. 28(2): 223-341.

Munnel, A., G.M.B. Tootell, L.E. Browne and J. McEneaney. 1996. Mortgage Lending in Boston: Interpreting HMDA Data. *The American Economic Review*. 86 (1): 25-53.

Naoi, M., M. Seko and K. Sumita. 2009. Earthquake Risk and Housing Prices in Japan: Evidence Before and After Massive Earthquakes. *Regional Science and Urban Economics*. 39(6): 658-669.

Nelson, J.P. 1981. Three Miles Island and Residential Property Values: Empirical Analysis and Policy Implications. *Land Economics*. 57(3): 363-372.

Pope, J.C. 2008. Do Seller Disclosures Affect Property Values? Buyer Information and the Hedonic Model. *Land Economics*. 84(4): 551-572.

Purnanandam, A. 2011. Originate-to-Distribute Model and the Subprime Mortgage Crisis. *The Review of Financial Studies.* 24(6): 1881-1915.

Railroad Commission of Texas. 2017. Retrieved from http://www.rrc.state.tx.us/pipeline-safety/ on March 15, 2017

Rajan, U., A. Seru and V. Vig. 2011. The Failures of Models that Predict Failure: Distance, Incentives and Defaults. *Journal of Financial Economics*. 115(2): 237-260.

Sigel, G. and A.J. Bandza. 2014. Lender Liability Under Environmental Laws for Real Estate and Corporate. In *Environmental Law in Illinois Corporate and Real Estate Transactions*. W.J. Anaya, ed. Chicago, IL: Arnstein and Lehr LLP. Chapter 4.

Simkovic, M. 2013. Competition and Crisis in Mortgage Securitization. *Indiana Law Journal*. 88(1): 1-60.

Simons, R.A. 1999. The Effect of Pipeline Ruptures on Noncontaminated Residential Easement-Holding Property in Fairfax County. *The Appraisal Journal*. 67(3): 255-263.

Simons, R.A., K. Winson-Geideman and B.A. Mikelbank. 2001. The Effects of An Oil Pipeline Rupture on Single-Family House Prices. *The Appraisal Journal*. 69(4): 410-418.

Wilson, A.R. and A.R. Alarcon. 1997. Lender Attitudes Toward Source and Nonsource Impaired Property Mortgages. *The Appraisal Journal*. 65(4): 396-400.

Worzala, E.M. and W.N. Kinnard Jr. 1997. Investor and Lender Reactions to Alternative Sources of Contamination. *Real Estate Issues*. 60(3): 42-47.

Xu, Y. and J. Zhang. 2012. Nonlocal Mortgage Lending and the Secondary Market Involvement. *Journal of Real Estate Literature*. 20(2): 307-322.

Table 1. Summary Statistics by the Treatment and the Control Groups, 2005-2011

	Pipeline-present Census tracts		Pipeline-free Census tracts	
	Mean	Standard deviation	Mean	Standard deviation
Dependent Variables				
Originated	0.723	0.448	0.736	0.441
Denied	0.204	0.403	0.186	0.389
Sold	0.434	0.496	0.495	0.500
Sold to the GSEs	0.223	0.417	0.267	0.442
Pipeline Incidents				
Number of pipeline incidents	0.059	0.235	0.001*	0.022
Property damage	101,036	2,685,651.50	0.119*	5.302
Borrowers' Characteristics				
Loan amount (million$)	0.117	0.101	0.105	0.089
Annual gross income (million$)	0.080	0.109	0.070	0.074
Loan-to-income raio	1.746	1.830	1.818	3.691
Co-applicants	0.490	0.500	0.461	0.499
Latino	0.172	0.377	0.254	0.435
Male	0.770	0.421	0.744	0.436
Asian	0.008	0.092	0.010	0.098
Black	0.029	0.168	0.025	0.156
White	0.948	0.221	0.950	0.217
Census Tracts' Characteristics				
Population density (thousand per km^2)	1.429	2.691	4.527	4.078
Minority population%	0.294	0.213	0.385	0.253
Median family income (million$)	4.771	1.020	4.448	1.188
Number of owner-occupied units (in 1000)	1.465	0.671	1.144	0.416
Number of 1- to 4- family units (in 1000)	2.189	0.925	1.754	0.575
Distance to the nearest refineries (1000 km)	0.110	0.061	0.127	0.068
Number of Observations		55,344		7,988

Note: The treatment group includes all the Census tracts in Texas where pipelines exist, and the control group includes the remaining Census tracts free of any pipeline infrastructure. The sample includes the HMDA loan applications for the conventional, 1- to 4-family, owner-occupied, and first-lien home purchase loans in Texas from 2005-2011. *One Census tract in Stephens County does not have any pipeline infrastructure but experienced an oil spill due to tank overfill in 2010.

Table 2. The Effects of Pipeline Incidents

	(1) Originated	(2) Denied	(3) Sold	(4) Sold to the GSEs
$pipe_j$	-0.016**	0.011	-0.015*	-0.003
	(0.008)	(0.007)	(0.008)	(0.005)
$pipe_j * incid_{jt}$	-0.018**	0.017**	0.009	0.012
	(0.009)	(0.007)	(0.008)	(0.008)
Loan amount	0.146***	-0.195***	-0.085**	-0.234***
	(0.033)	(0.037)	(0.036)	(0.028)
Annual gross income	0.064**	-0.077***	-0.048**	0.017
	(0.025)	(0.028)	(0.019)	(0.014)
Loan-to-income ratio	-0.009***	0.011***	0.029***	0.014***
	(0.003)	(0.004)	(0.003)	(0.002)
Co-applicants	0.019***	-0.014***	0.012***	0.014***
	(0.004)	(0.003)	(0.004)	(0.003)
Latino	-0.101***	0.099***	-0.079***	-0.038***
	(0.006)	(0.005)	(0.008)	(0.005)
Male	0.030***	-0.029***	0.008*	0.006
	(0.005)	(0.004)	(0.004)	(0.004)
Asian	0.004	-0.009	0.017	0.031
	(0.025)	(0.023)	(0.027)	(0.024)
Black	-0.154***	0.155***	-0.018	-0.061***
	(0.021)	(0.020)	(0.022)	(0.019)
White	0.052***	-0.056***	0.009	-0.006
	(0.016)	(0.015)	(0.019)	(0.015)
Population density	0.005***	-0.005***	0.002**	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Minority population %	0.002	-0.001	-0.019	0.016
	(0.022)	(0.020)	(0.023)	(0.018)
Median family income	0.026***	-0.025***	0.011***	0.011***
	(0.003)	(0.003)	(0.003)	(0.003)
Number of owner-occupied units	0.017	-0.012	0.033**	0.005
	(0.015)	(0.016)	(0.013)	(0.013)
Number of 1- to 4- family units	-0.007	0.005	-0.015*	-0.002
	(0.010)	(0.011)	(0.009)	(0.009)
Distance to refineries	0.036	0.135	-0.183	-0.254
	(0.198)	(0.178)	(0.186)	(0.167)
Constant	0.522***	0.308***	0.708***	0.734***
	(0.040)	(0.036)	(0.042)	(0.038)
Observations	62,244	62,244	45,104	45,104
R-squared	0.158	0.177	0.530	0.584

Notes: Columns 1 and 2 use the 2005-2011 full sample of loan applications, whereas Columns 3 and 4 use the 2005-2011 sample of the originated loans. The dependent variables are listed as the column titles. The variable $pipe_j$ is coded as 1 if pipelines are present in Census tract j, and 0 otherwise. The estimates of $pipe_j$ measure the permanent effects of pipeline infrastructure. The variable $incid_{jt}$ is coded as 1 if a pipeline incident occurred to Census tract j in year t, and 0 otherwise. The estimates of $pipe_j * incid_{jt}$ measure the effects of pipeline incidents. We also control for the year fixed effects, the county fixed effects, and the lender fixed effects. Standard errors clustered by Census tract are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 3. The Effects Before and After a Pipeline Incident

	(1) Originated	(2) Denied	(3) Sold	(4) Sold to the GSEs
$pipe_j$	-0.019**	0.012	-0.017*	-0.008
	(0.009)	(0.008)	(0.009)	(0.006)
$pipe_j$*$incid_{j,t+1}$	-0.007	0.007	0.001	-0.004
	(0.010)	(0.008)	(0.011)	(0.009)
$pipe_j * incid_{j,t}$	-0.019*	0.019**	0.002	0.002
	(0.010)	(0.009)	(0.009)	(0.010)
$pipe_j$*$incid_{j,t-1}$	-0.008	0.007	-0.001	0.001
	(0.011)	(0.010)	(0.011)	(0.010)
Observations	46,817	46,817	33,604	33,604
R-squared	0.172	0.193	0.539	0.614

Notes: Columns 1 and 2 use the 2006-2010 full sample of loan applications, whereas Columns 3 and 4 use the 2006-2010 sample of the originated loans. The dependent variables are listed as the column titles. The estimates of $pipe_j$ measure the permanent effects of pipeline infrastructure. The estimates of $pipe_j * incid_{jt}$ measure the contemporaneous effects of pipeline incidents, the estimates of $pipe_j * incid_{j,t+1}$ measure the counterfactual effects before pipeline incidents, and the estimates of $pipe_j * incid_{j,t-1}$ measure the lagged effects of pipeline incidents. We use the same control variables and fixed effects as in the baseline case (Table 2). Standard errors clustered by Census tract are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 4. Early versus Late Incidents

	(1) Originated	(2) Denied	(3) Sold	(4) Sold to the GSEs
	Panel A: Pipeline Incidents in the First Quarter of the Year			
$pipe_i$	-0.013	0.005	-0.010	0.001
	(0.008)	(0.007)	(0.009)	(0.006)
$pipe_i * incid_{it}$	-0.033**	0.030**	0.015	0.040**
	(0.016)	(0.014)	(0.013)	(0.016)
Observations	50,979	50,979	37,173	37,173
R-squared	0.163	0.185	0.523	0.574
	Panel B: Pipeline Incidents in the Whole Year			
$pipe_i$	-0.011	0.004	-0.010	-0.000
	(0.008)	(0.007)	(0.009)	(0.006)
$pipe_i * incid_{it}$	-0.019**	0.023***	0.008	0.015*
	(0.010)	(0.008)	(0.009)	(0.009)
Observations	52,791	52,791	38,467	38,467
R-squared	0.163	0.185	0.522	0.573

Notes: The analysis in this table pertains to the 2005-2009 loan applications (Columns 1 and 2) and the sample of the originated loans over this period (Columns 3 and 4). Panel A includes a sample of the Census tracts that experienced incidents as early as in the first quarter of the year. Panel B includes a sample that had incidents that occurred anytime of the year. The dependent variables are listed as the column titles. The estimates of $pipe_j$ measure the permanent effects of pipeline infrastructure. The estimates of $pipe_j * incid_{jt}$ measure the effects of pipeline incidents. We use the same control variables and fixed effects as in the baseline case (Table 2). Standard errors clustered by Census tract are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 5. Borrowers' Characteristics

	(1) Loan Amount	(2) Annual Gross Income	(3) Loan-to-income Ratio	(4) Co-applicant	(5) Latino	(6) Male	(7) White
$pipe_j$	0.010***	0.009***	-0.042	0.014	0.007	0.005	-0.003
	(0.003)	(0.003)	(0.040)	(0.010)	(0.012)	(0.007)	(0.006)
$pipe_j * incid_{jt}$	0.002	0.003	-0.021	0.012	-0.012	0.011	0.007
	(0.003)	(0.002)	(0.028)	(0.011)	(0.009)	(0.008)	(0.005)
Observations	63,332	62,244	62,244	63,332	63,332	63,332	63,332
R-squared	0.148	0.032	0.020	0.068	0.256	0.012	0.035

Notes: The table reports the estimation results using the 2005-2011 full sample of loan applications. The dependent variables are listed as the column titles. The estimates of $pipe_j$ measure the permanent effects of pipeline infrastructure. The estimates of $pipe_j * incid_{jt}$ measure the effects of pipeline incidents. We use the same control variables and fixed effects as in the baseline case (Table 2). Standard errors clustered by Census tract are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 6. Lenders' Denial Reasons

	(1) Debt-to-income Ratio	(2) Employment History	(3) Credit Hisotry	(4) Insufficient Collateral	(5) Insufficient Cash	(6) Unverifiable information	(7) Credit Application Incomplete	(8) Mortgage Insurance Denied
$pipe_j$	-0.008	-0.010	-0.016	0.030**	-0.003	0.007	0.010	0.004
	(0.018)	(0.009)	(0.019)	(0.015)	(0.011)	(0.010)	(0.011)	(0.003)
$pipe_j * incid_{jt}$	-0.015	0.020*	-0.000	0.009	-0.005	-0.018	0.007	-0.002
	(0.017)	(0.011)	(0.017)	(0.021)	(0.014)	(0.011)	(0.012)	(0.004)
Observations	9,894	9,894	9,894	9,894	9,894	9,894	9,894	9,894
R-squared	0.152	0.082	0.331	0.164	0.116	0.174	0.311	0.141

Notes: The table reports the estimation results using the 2005-2011 sample of the denied loans with denial reasons available. The dependent variables are listed as the column titles. The estimates of $pipe_j$ measure the permanent effects of pipeline infrastructure. The estimates of $pipe_j * incid_{jt}$ measure the effects of pipeline incidents. We use the same control variables and fixed effects as in the baseline case (Table 2). Standard errors clustered by Census tract are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 7. Cumulative Sorting Effects I: Census Tracts of Different Intensity of Pipelines, 2005-2011

	(1) Pipeline-free Census tracts	(2) The first quartile group	(3) The fourth quartile group	(4) Difference between (1) and (2) (p-value)	(5) Difference between (2) and (3) (p-value)
Loan amount	0.105	0.104	0.119	0.001	-0.015
	(0.089)	(0.096)	(0.095)	(0.360)	(0.000)
Annual gross income	0.070	0.072	0.083	-0.002	-0.012
	(0.074)	(0.081)	(0.109)	(0.075)	(0.000)
Loan-to-income ratio	1.818	1.718	1.714	0.100	0.003
	(3.691)	(1.145)	(2.912)	(0.019)	(0.908)
Co-applicants	0.461	0.451	0.515	0.011	-0.064
	(0.499)	(0.498)	(0.500)	(0.126)	(0.000)
Latino	0.254	0.260	0.162	-0.006	0.098
	(0.435)	(0.439)	(0.368)	(0.330)	(0.000)
Male	0.744	0.749	0.803	-0.004	-0.054
	(0.436)	(0.434)	(0.398)	(0.476)	(0.000)
Asian	0.010	0.013	0.005	-0.003	0.007
	(0.098)	(0.111)	(0.073)	(0.057)	(0.000)
Black	0.025	0.031	0.021	-0.006	0.011
	(0.156)	(0.174)	(0.142)	(0.006)	(0.000)
White	0.950	0.938	0.962	0.013	-0.024
	(0.217)	(0.242)	(0.192)	(0.000)	(0.000)
Population density	4.527	4.503	0.105	0.024	4.399
	(4.078)	(3.800)	(0.100)	(0.667)	(0.000)
Minority population %	0.385	0.375	0.284	0.010	0.091
	(0.253)	(0.239)	(0.199)	(0.003)	(0.000)
Median family income	4.448	4.650	4.738	-0.202	-0.088
	(1.188)	(1.237)	(0.775)	(0.000)	(0.000)

Number of owner-occupied units	1.144	1.200	1.538	-0.056	-0.338
	(0.416)	(0.438)	(0.687)	(0.000)	(0.000)
Number of 1- to 4- family units	1.754	1.872	2.261	-0.118	-0.388
	(0.575)	(0.614)	(0.909)	(0.000)	(0.000)
Distance to refineries	0.127	0.119	0.095	0.008	0.024
	(0.068)	(0.061)	(0.057)	(0.000)	(0.000)
N	7,988	13,889	13,811		

Notes:
1. The table reports, for the 2005-2011 full sample, the mean characteristics of borrowers and neighborhoods for the pipeline-free Census tracts (Column 1), the Census tracts falling into the first quartile group (Column 2), and the fourth quartile group (Column 3) of the distribution of the pipeline length in each Census tract. In parenthesis of each cell from Column 1 to Column 3 is the standard error of the characteristics of that cell.
2. The table also reports the difference of the characteristics between Column 1 and Column 2 (in Column 4) and the difference between Column 2 and Column 3 (in Column 5). In parenthesis of each cell from Column 4 and Column 5 is the p-value for an unpaired two-sample mean-comparison t-test testing the null hypothesis that the characteristics are equal across the two corresponding groups.

Table 8. Cumulative Sorting Effects II: Census Tracts of Different Frequencies of Pipeline Incidents, 2011

	(1) Incidents-free Census tracts	(2) Census tracts with one incident	(3) Census tracts with two incidents	(4) Census tracts with three or more incidents	(5) Difference between (1) and (2) (*p*-value)	(6) Difference between (2) and (4) (*p*-value)
Loan amount	0.116	0.141	0.124	0.129	-0.025	0.012
	(0.096)	(0.127)	(0.091)	(0.103)	(0.000)	(0.191)
Annual gross income	0.084	0.095	0.088	0.107	-0.012	-0.011
	(0.083)	(0.093)	(0.066)	(0.173)	(0.003)	(0.388)
Loan-to-income ratio	1.776	1.727	1.590	1.694	0.049	0.033
	(5.674)	(1.335)	(1.003)	(1.156)	(0.669)	(0.737)
Co-applicants	0.578	0.620	0.628	0.667	-0.042	-0.047
	(0.494)	(0.486)	(0.485)	(0.473)	(0.043)	(0.231)
Latino	0.197	0.207	0.177	0.219	-0.010	-0.012
	(0.398)	(0.405)	(0.383)	(0.414)	(0.563)	(0.720)
Male	0.776	0.837	0.787	0.849	-0.061	-0.012
	(0.417)	(0.370)	(0.411)	(0.359)	(0.000)	(0.689)
Asian	0.007	0.006	0.000	0.000	0.001	0.006
	(0.086)	(0.077)	(0.000)	(0.000)	(0.680)	(0.045)
Black	0.026	0.017	0.012	0.021	0.010	-0.004
	(0.161)	(0.128)	(0.110)	(0.143)	(0.085)	(0.712)
White	0.957	0.973	0.988	0.979	-0.015	-0.006
	(0.202)	(0.163)	(0.110)	(0.143)	(0.034)	(0.602)
Population density	1.793	0.567	0.092	0.150	1.226	0.416
	(2.965)	(1.376)	(0.123)	(0.197)	(0.000)	(0.000)
Minority population %	0.310	0.275	0.225	0.345	0.035	-0.069
	(0.220)	(0.193)	(0.142)	(0.166)	(0.000)	(0.000)
Median family income	5.180	5.401	5.161	5.258	-0.221	0.143
	(1.143)	(0.843)	(0.590)	(0.986)	(0.000)	(0.069)

Number of owner-occupied units	1.355	1.528	1.474	1.406	-0.173	0.122
	(0.600)	(0.646)	(0.581)	(0.756)	(0.000)	(0.042)
Number of 1- to 4- family units	2.055	2.211	2.210	2.036	-0.156	0.175
	(0.851)	(0.795)	(0.825)	(1.089)	(0.000)	(0.039)
Distance to refineries	0.114	0.099	0.095	0.098	0.015	0.001
	(0.061)	(0.058)	(0.065)	(0.044)	(0.000)	(0.816)
N	3,097	663	164	192		

Notes:

1. The table reports, for the 2011 sample, the mean characteristics of borrowers and neighborhoods for the Census tracts without experiencing any incident (Column 1), experiencing one incident (Column 2), two incidents (Column 3), and three or more incidents (Column 4) from 2005 to 2011. In parenthesis of each cell from Column 1 to Column 4 is the standard error of the characteristics of that cell.
2. The table also reports the difference of the characteristics between Column 1 and Column 2 (in Column 5) and the difference between Column 2 and Column 4 (in Column 6). In parenthesis of each cell from Column 5 and Column 6 is the p-value for an unpaired two-sample mean-comparison t-test testing the null hypothesis that the characteristics are equal across the two corresponding groups.

Table 9. Heterogeneous Treatment Effects I: Different Income Groups

	Low- to Middle-income Borrowers				Upper-income Borrowers			
	(1) Originated	(2) Denied	(3) Sold	(4) Sold to the GSEs	(5) Originated	(6) Denied	(7) Sold	(8) Sold to the GSEs
$pipe_j$	-0.025**	0.018**	-0.020*	-0.009	-0.006	0.001	-0.014	-0.001
	(0.011)	(0.009)	(0.011)	(0.008)	(0.010)	(0.009)	(0.009)	(0.008)
$pipe_j * incid_{jt}$	-0.035***	0.023**	0.002	0.007	-0.009	0.016	0.008	0.009
	(0.013)	(0.012)	(0.013)	(0.013)	(0.011)	(0.010)	(0.010)	(0.010)
Observations	26,023	26,023	17,074	17,074	36,221	36,221	28,030	28,030
R-squared	0.185	0.204	0.591	0.596	0.154	0.172	0.514	0.588

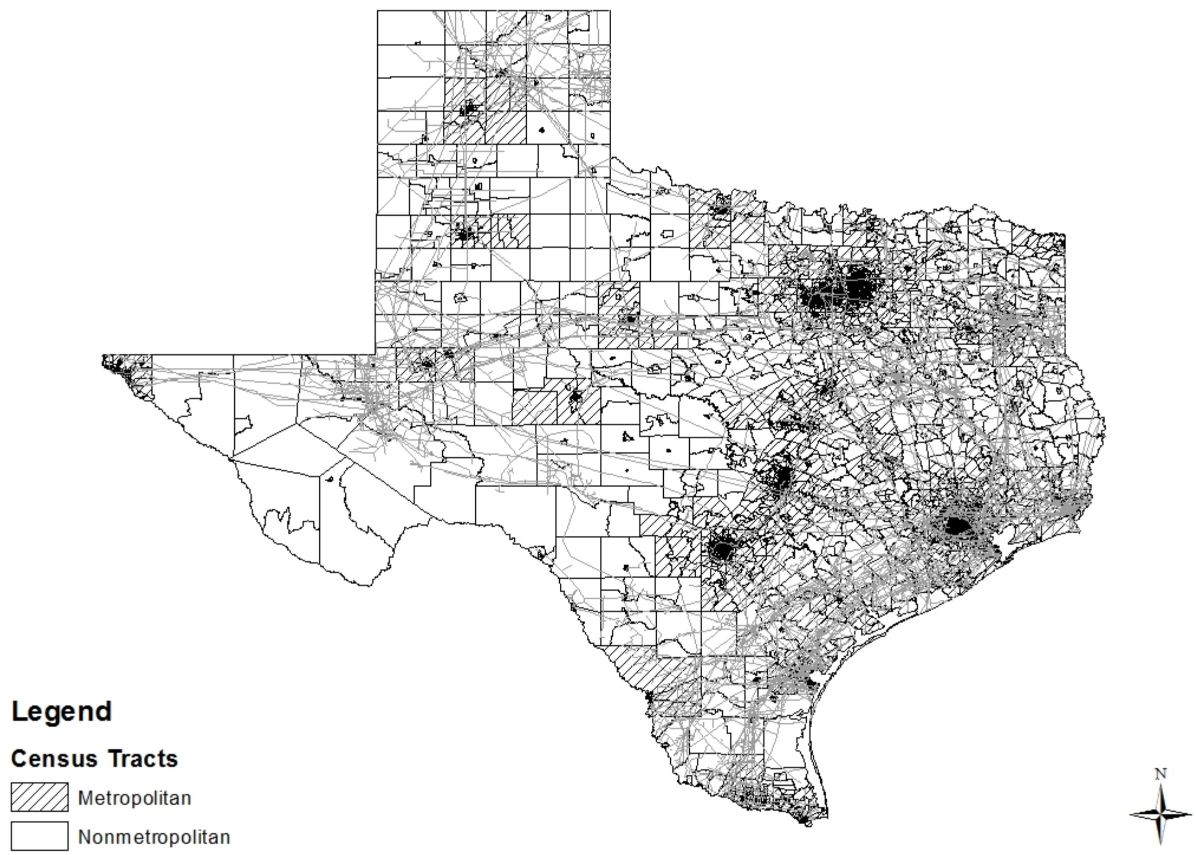
Notes: Columns 1-4 use the 2005-2011 low- to middle-income borrowers' loan applications, whereas Columns 5-8 use the 2005-2011 upper-income borrowers' loan applications. Columns 3, 4, 7, and 8 use the sample of the originated loans in the corresponding income group. The dependent variables are listed as the column titles. The estimates of $pipe_j$ measure the permanent effects of pipeline infrastructure. The estimates of $pipe_j * incid_{jt}$ measure the effects of pipeline incidents. We use the same control variables and fixed effects as in the baseline case (Table 2). Standard errors clustered by Census tract are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 10. Heterogeneous Treatment Effects II:
Before, During, and After the Subprime Mortgage Crisis by Income Group

	Low- to Middle-income Borrowers				Upper-income Borrowers			
	(1) Originated	(2) Denied	(3) Sold	(4) Sold to the GSEs	(5) Originated	(6) Denied	(7) Sold	(8) Sold to the GSEs
	Panel A: Before the Subprime Mortgage Crisis, 2005 - 2007							
$pipe_j$	-0.018	0.010	-0.019	-0.019*	0.010	-0.011	-0.008	0.004
	(0.013)	(0.010)	(0.012)	(0.011)	(0.012)	(0.011)	(0.012)	(0.010)
$pipe_j * incid_{jt}$	-0.034**	0.020	-0.002	0.006	0.003	0.018	0.018	0.029**
	(0.014)	(0.013)	(0.018)	(0.017)	(0.015)	(0.014)	(0.013)	(0.013)
Observations	16,631	16,631	11,106	11,106	22,001	22,001	16,986	16,986
R-squared	0.181	0.210	0.567	0.566	0.172	0.198	0.474	0.551
	Panel B: During the Subprime Mortgage Crisis, 2008-2009							
$pipe_j$	-0.020	0.003	-0.022	0.010	-0.030*	0.016	0.007	0.014
	(0.023)	(0.022)	(0.023)	(0.016)	(0.017)	(0.016)	(0.018)	(0.014)
$pipe_j * incid_{jt}$	-0.041	0.033	0.007	0.013	-0.026	0.019	0.011	0.023
	(0.039)	(0.035)	(0.029)	(0.025)	(0.022)	(0.020)	(0.019)	(0.015)
Observations	5,481	5,481	3,610	3,610	8,678	8,678	6,765	6,765
R-squared	0.291	0.294	0.688	0.765	0.197	0.224	0.602	0.748
	Panel C: After the Subprime Mortgage Crisis, 2010-2011							
$pipe_j$	-0.059*	0.064**	-0.031	-0.004	-0.026	0.035	-0.025	-0.011
	(0.030)	(0.028)	(0.028)	(0.021)	(0.028)	(0.023)	(0.026)	(0.017)
$pipe_j * incid_{jt}$	-0.057	0.037	0.072	0.007	-0.004	-0.001	-0.003	-0.016
	(0.037)	(0.035)	(0.044)	(0.027)	(0.027)	(0.020)	(0.023)	(0.017)
Observations	3,911	3,911	2,358	2,358	5,542	5,542	4,279	4,279
R-squared	0.278	0.284	0.646	0.745	0.216	0.220	0.594	0.699

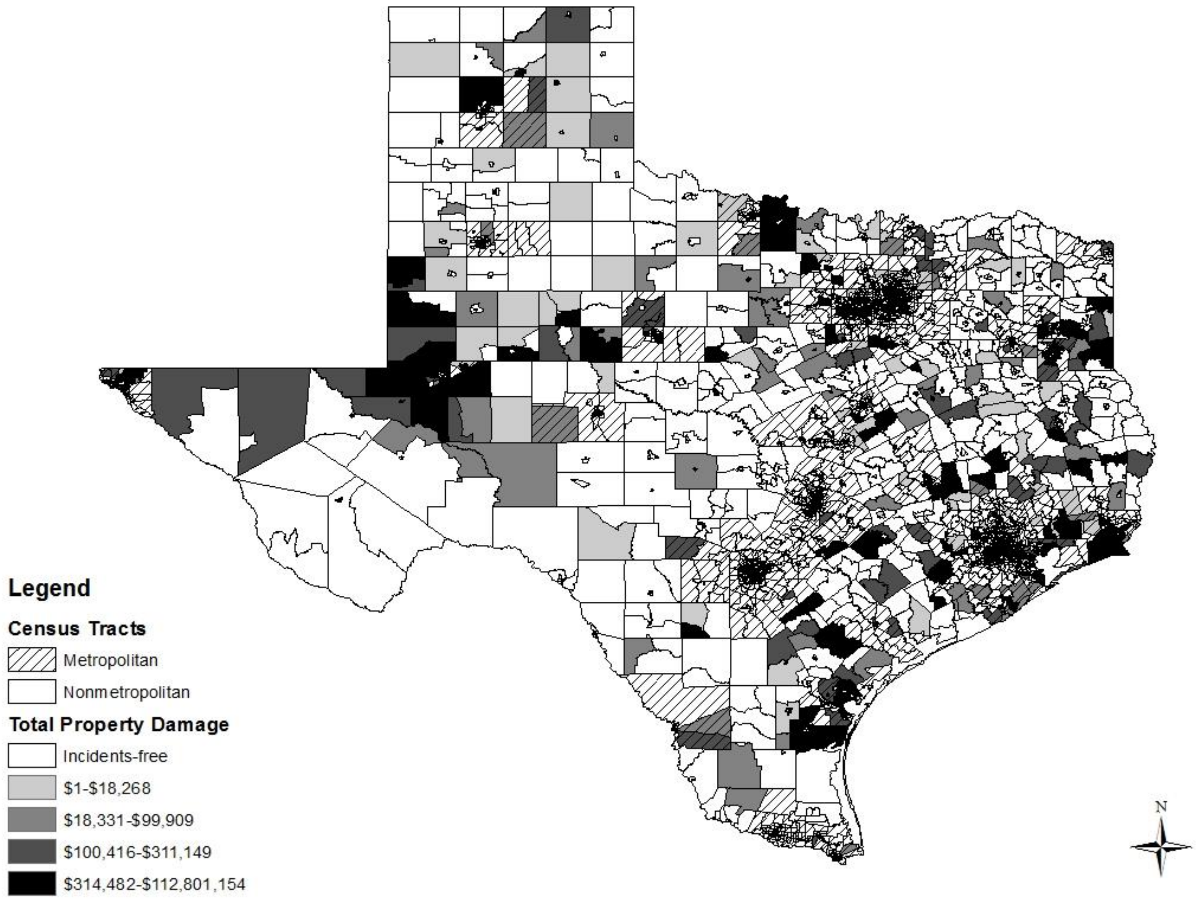
Notes: Panel A, B, and C report the estimation results using the 2005-2007, 2008-2009, and 2010-2011 sample, respectively. Columns 1-4 use the low- to middle-income borrowers' loan applications, whereas Columns 5-8 use the upper-income borrowers' loan applications. Columns 3, 4, 7, and 8 use the sample of the originated loans in the corresponding income group. The dependent variables are listed as the column titles. The estimates of $pipe_j$ measure the permanent effects of pipeline infrastructure. The estimates of $pipe_j * incid_{jt}$ measure the effects of pipeline incidents. We use the same control variables and fixed effects as in the baseline case (Table 2). Standard errors clustered by Census tract are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Figure 1. Texas Pipeline Network



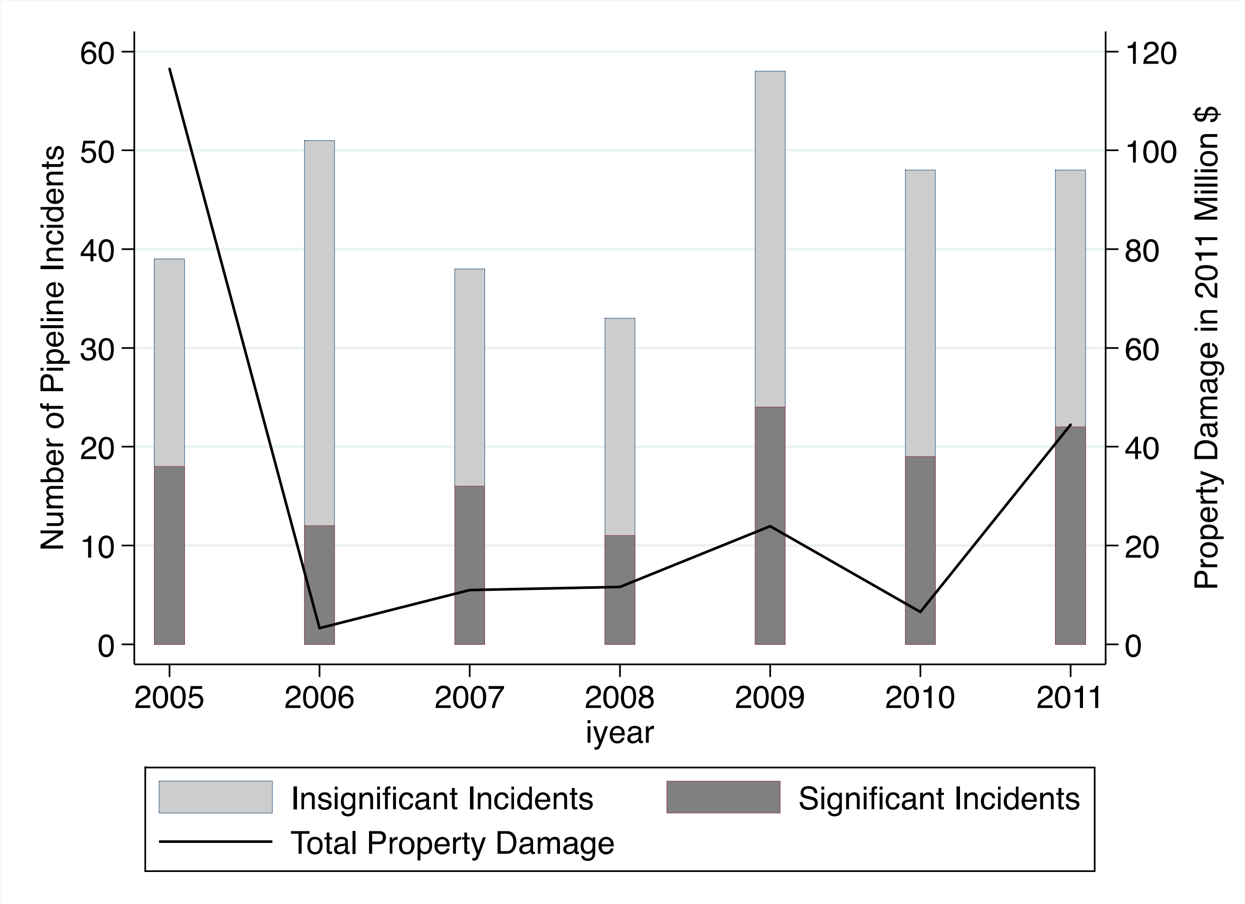
Source: Generated by the authors using data from the U.S. Energy Information Administration.

Figure 2. Quartile Distribution of Total Property Damage of Pipeline Incidents in Texas, 2005-2011



Source: Generated by the authors using data from the U.S. Department of Transportation Pipeline and Hazardous Materials Safety Administration. We convert the total property damage into 2011 U.S. dollars.

Figure 3. Pipeline Incidents in the Nonmetro Texas from 2005 to 2011



Source: Generated by the authors based on the data from the Pipeline and Hazardous Materials Safety Administration (PHMSA). According to the PHMSA, significant incidents refer to those associated with fatality, injury, or total property damage over $50,000. We convert the total property damage into 2011 U.S. dollars.