Pipeline Incidents and Property Values[†]

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Abstract: The rapid expansion of pipelines during the U.S. shale oil and gas boom drew attention to the economic consequences of pipeline incidents. This study investigates the impacts of 426 gas distribution pipeline incidents on property values in the United States between 2010 and 2020. We find that only incidents that are both severe and above ground (which we define as high-profile incidents) have adverse effects on nearby property values, while other incidents have no measurable impact. A difference-in-differences analysis finds that high-profile incidents significantly decrease property values within 1,000 meters by about 8.0%, and the negative impact can persist for eight years on average. Furthermore, we find a drop in transaction volume that lasts a short period after the incidents, suggesting an initial demand-side response. In contrast to the strong effects of pipeline incidents, we did not find statistically significant price effects from pipeline installation. We also demonstrate that there is substantial heterogeneity by the type of incident and that results based on individual incidents should be generalized with caution.

Keywords: Pipelines, Hazardous Materials, Housing Prices, Property Value Hedonics, Difference-in-Differences

JEL Codes: Q51; Q53; Q35

1 Introduction

Pipelines are critical infrastructure for transporting various types of hazardous materials (HAZMAT), including natural gas, crude oil, and other petroleum products (Masters, 2022). Since 2004, the development of shale oil and shale gas has boosted the demand for oil and gas transportation from the U.S. heartland to the coastal regions. The surge in pipeline transportation heightened concerns about pipeline safety and the socioeconomic costs associated with pipeline incidents. Over the past 20 years, the United States has suffered 306 casualties and more than \$6 billion in property damage resulting from pipeline incidents (PHMSA, 2021*b*). Nonetheless, pipeline safety records have not improved—the incident rate has stayed about the same for many years (Kelso, 2021), and serious incidents continued to happen (Ly, 2019; Weikel, 2011; Vigdor and Delkic, 2021). It is of great policy relevance to better understand the economic cost of pipeline incidents for comparison with other types of energy transportation, such as rail (Covert and Kellogg, 2017; Tang et al., 2020).

Existing hedonic studies on pipelines and property values are mostly on the effects of proximity to pipelines (McElveen, Brown and Gibbons, 2017; Hilterbrand Jr, 2019; Boslett and Hill, 2019; Herrnstadt and Sweeney, 2022) and the construction (including announcements) of pipelines (Wilde, Loos and Williamson, 2012; Hilterbrand Jr, 2019; Boslett and Hill, 2019) on nearby property values. Studies on pipeline incidents are scarce and limited to single incidents. Two existing studies in the United States include Hansen, Benson and Hagen (2006) for an incident in Bellingham, WA, in 1999 and Herrnstadt and Sweeney (2022) for an incident in San Bruno, California, in 2010. Both studies find modest adverse effects within limited areas around the incidents. It is likely that other incidents have different impacts based on the severity of the incident and the characteristics of the pipeline. Research with broader coverage is needed to support the design of nationwide policies regarding the construction and management of pipelines.

There are three types of pipelines transporting natural gas, including gas-gathering lines, gas transmission lines, and gas distribution lines. This study leverages Pipeline and Hazardous Materials Safety Administration (PHMSA) data and investigates the property value impacts of 426 incidents on gas distribution pipelines in urban areas between 2010 and 2020. We focus on urban gas distribution pipelines since they often go through densely populated areas, and incidents on those pipelines are more likely to cause significant impacts on property values. Using the rich set of incident characteristics in PHMSA data, we categorize incidents based on severity and pipeline characteristics: "high-profile", which are both severe (involving explosion, ignition, or fatalities) and above ground; "only severe or above-ground"[1]; and, "neither severe nor above-ground" incidents. We analyze these groups separately and show that high-profile incidents have distinct impacts compared to the other groups.

We link the PHMSA incident data to the Zillow Transaction and Assessment (ZTRAX) Dataset[2]to quantify pipeline incident impacts on property values. We first assess the spatiotemporal scales of incident impacts. Two alternative methods, linear polynomial regressions (LPR) (Linden and Rockoff, 2008; Muehlenbachs, Spiller and Timmins, 2015; Haninger, Ma and Timmins, 2017) and regressions with fine spatial or temporal bins (Guignet et al., 2023*b*; Lu et al., 2023), both show that the impacts of high-profile incidents reach between 1,000–1,500 meters and last approximately 3,000 days. We do not observe significant price effects in the other incident groups.

We employ a difference-in-differences (DID) framework to quantify the property value impacts of different incident types. Based on the scale analyses, we designate transactions within 1,000 meters of an incident site as the treatment group and transactions between 1,500 and 3,000 meters as the control group. We exclude properties in the middle regions of the treatment and control groups to prevent potential treatment spillover effects. Since our preferred econometric specification includes incident-by-year fixed effects, the key assumption for our DID design is that, in the absence of the incident, transactions in treatment and control groups of each incident follow a parallel trend that can be different from other incidents. As the parallel trend assumption is more likely to hold in shorter periods, our main analysis uses pre- and post-incident periods of 2,000 days, and robustness checks are conducted with shorter time windows. After refining our treatment and control groups and pre-/post-periods based on scale analyses, we retain 310 incidents associated with 369,926 nearby housing transactions for the main DID analysis.

DID estimates indicate that high-profile incidents significantly decrease nearby property values by 8.2%, whereas other types of incidents do not exert noticeable impacts on property values. Our DID estimation based on a quasi-experimental framework can be interpreted as the capitalization effect for pipeline incidents (Kuminoff and Pope, 2014). Our main results survive numerous robustness checks and cannot be produced by placebo incidents with mismatched locations and dates. A policy-relevant question is how the ob-

[1]Our analysis shows that "severe only" and "above-ground only" incidents have similar results. Therefore, to save space and increase the statistical power of regression analyses, we present results with these two groups combined.

[2]Data provided by the Zillow Group (http://www.zillow.com/ztrax). The results and opinions are those of the authors and do not reflect the position of Zillow Group.

served impacts on property value attenuate over time. We leverage transaction observations within 4,000 days before and after incidents to explore the long-term effects of high-profile incidents. Results indicate that the impacts of high-profile incidents are persistent, with an average negative impact of 8.0% within 3,000 days.

Besides the price effects of pipeline incidents, we conduct two additional analyses to explore the mechanism of pipeline impacts. First, a high-profile incident may affect transaction volumes—the direction of the effect depends on whether sellers or buyers have a stronger reaction to pipeline incidents. We conduct a DID analysis on transaction volumes before and after high-profile incidents. We find evidence for negative effects on transaction volume that last for a short period after incidents, suggesting a strong initial reaction from the demand side. Second, we focus on a subsample of high-profile incidents with data on installation dates and examine the potential effects of pipeline installation on property values. The DID analysis does not find statistically significant impacts of pipeline installation on property values.

This study contributes to the literature on hedonic studies of energy infrastructures. Using 426 incidents in 24 states that happened over the course of 11 years, this paper improves upon previous single-incident studies by providing more general results. We also provide more detailed information with enhanced external validity for potential cost-benefit analysis, facilitating the comparison between pipelines and other modes of energy transportation. We find that while most incidents (i.e., non-high-profile incidents) have no statistically detectable impact, more noticeable incidents (i.e., high-profile incidents) have stronger impacts, affect larger areas, and persist longer than what previous studies suggest (Hansen, Benson and Hagen, 2006; Herrnstadt and Sweeney, 2022). In addition, this study advances our understanding of the mechanisms through which negative environmental events impact housing values. The fact that housing prices only respond to high-profile incidents is consistent with the interpretation of pipeline incidents as information shocks (Hansen, Benson and Hagen, 2006). Since local residents were likely aware of above-ground pipelines before the incidents, the effective information seems to be related to the risk, not the existence, of the pipelines. This is supported by the null results based on pipeline installation found in our study and others (e.g., Wilde, Loos and Williamson (2012)). The drop in transaction volume soon after the incident suggests a strong initial reaction on the demand side.

This article proceeds as follows. Section 2 introduces the background of gas pipeline incidents and related literature. Section 3 describes the data and the data-processing procedure. Section 4 discusses scale analyses using both semi-parametric and parametric approaches. Section 5 presents the formal DID analyses on the price impacts in both short-term and long-term. Section 6 shows the results of robustness checks and placebo tests. Section 7 provides mechanism analyses. Finally, Section 8 concludes.

2 Background and Literature Review

This section introduces the institutional background, including the trends, information, and costs, of incidents occurring on gas pipelines in the United States. Additionally, it provides an overview of existing literature related to this study.

2.1 Background

Policy changes and technological innovations have contributed to the boom of domestic shale gas development since the early 2000s (Wang and Krupnick, 2013). From 2000 to 2010, total domestic natural gas consumption grew 12.5% (about 27.5 trillion cubic feet) (Vetter et al., 2019). The shale gas expansion triggered a surge in the construction of pipelines to transport newly discovered natural gas from the U.S. heartland to the coastal regions. According to PHMSA, over the last 15 years, annual natural gas distribution pipeline construction ranged from 24,000 to 50,000 miles.[3] Among all types of natural gas pipelines, gas distribution pipelines account for more than 80% of the total mileage of gas pipelines in residential areas (Pless, 2011).

Even though pipeline transportation is considered one of the safest approaches to transporting crude oil and natural gas (Kenneth and Taylor, 2015), aging pipelines are a potential safety threat as more than half of the pipelines were installed 40 years ago (Sider and Friedman, 2016). In the United States, PHMSA is responsible for regulating the pipeline transportation of HAZMAT. When an incident occurs, the pipeline operator is expected to seek help from emergency correspondents at the local, state, or federal level[4] and submit an incident report within 30 days (PHMSA, 2021*a*). Due to these mandatory reporting rules, PHMSA has compiled comprehensive pipeline incident data since the 1970s. Precise longitude and latitude information are available for the year 2010 and after, which allows us to match incidents with nearby properties for hedonic analyses.

PHMSA defines an event as a pipeline incident if it causes death or personal injury requiring hospitalization, property damage of $50,000 or more, or more than three million

[3] Detailed information can be found at PHMSA's website: https://www.phmsa.dot.gov/.

[4] In our dataset, the time difference between incident occurrence and operator arriving on site is very small, implying a nearly immediate response to pipeline incidents.

cubic feet of gas loss. When a pipeline incident occurs, residents can receive information about the incident in several ways. The first is through media and news reports. Based on an internet search of 73 selected incidents with an explosion, we find that at least two different newspapers reported on 35 of the events, and national news covered 34[5]. The second is a formal notice from the governments or responsible companies. One example is that pipeline companies send letters to nearby households notifying them of their proximity to pipelines, as discussed in Herrnstadt and Sweeney (2022). Third, residents can learn about pipeline incidents through direct observation and word-of-mouth.[6] For example, Messer et al. (2006) find that delayed clean-up of Super-Fund sites attracts attention from the public and creates stigma among surrounding communities, causing potential homeowners to shun properties in the affected regions. Pipeline incidents may serve as informational cues that influence potential homebuyers' assessment of risks living next to pipelines, thus affecting their willingness to pay for a house nearby, similar to the discussion in Guignet et al. (2023*a*).

Despite ongoing research, economists have not yet adequately quantified the impacts of pipeline incidents on residential communities and the environment. According to PHMSA statistics, a high-profile incident on a gas distribution line accounts for direct costs (such as private property damage and repairs) of about \$4 million on average (PHMSA, 2021*b*). However, studies on the indirect impact of pipeline incidents on housing values are scarce (Hansen, Benson and Hagen, 2006; Herrnstadt and Sweeney, 2022). Shen et al. (2021) quantify the direct impacts of residential gas leaks. In contrast to the incidents on distributional pipelines, which are public hazards, gas leaks mostly only affect one property. They find that properties within 20 meters of a gas leak (presumed to be the property in which the leak occurred) experience an average property value loss of \$12,000 (2019 value). Our results complement Shen et al. (2021) by quantifying the external impacts of pipeline incidents. Beyond property values, pipeline failure may generate unexpected negative externality in other areas. For example, Xu and Xu (2020) study the effects of pipeline hazards on credit risk and find that pipeline-present areas have a lower loan origination rate compared with pipeline-free areas, and pipeline incidents further magnify this effect by 1.8%.

[5] Web-search results are available from the authors upon requests.

[6] In our dataset, the general public reports 8% of pipeline incidents.

2.2 Literature Review

This study relates to two strands of property value hedonics literature. First, we provide empirical evidence on the impact of energy infrastructure incidents on housing value. Economists have shown increased interest in the implicit costs, such as property value loss, of incidents involving HAZMAT underground storage tanks, nuclear power plants, shale gas facilities, railroad, and industrial facilities. For example, Zabel and Guignet (2012) and Guignet et al. (2018) find a negative impact of underground storage tank leaks on housing values and home sales when owners reveal the leakage information. Isakson and Ecker (2018) find lower willingness-to-pays for houses within 0.25 miles of multiple leaking underground storage tank sites compared with those further away. Muehlenbachs, Spiller and Timmins (2015) report that shale gas leaks have large negative impacts on nearby groundwater-dependent homes. Boes, Nüesch and Wüthrich (2015) find that the 2011 Fukushima nuclear power incident poses negative impacts on rents near nuclear power plants in Switzerland. Furthermore, Tanaka and Zabel (2018) and Zhu et al. (2016) observe similar price effects in regions near nuclear power plants in the United States and China, respectively.

Recently, Tang et al. (2020) explore the impact of derailments on nearby housing values and find significantly negative but temporary impacts within one mile of derailment sites. Guignet and Nolte (2024) find that the discovery of contamination and subsequent investigation of treatment, storage, and disposal facilities depreciate the property values within 750 meters of a facility. Similarly, Guignet et al. (2023*b*) demonstrate home values decline by 5%–8% from industrial chemical accidents that impact nearby communities. As for pipeline incidents, Herrnstadt and Sweeney (2022) study the 2010 San Bruno natural gas pipeline incident in California and find that housing prices within 500 feet of a pipeline decrease by approximately 2% when the issue receives public attention. Hansen, Benson and Hagen (2006) document that property values within 50 feet of a pipeline explosion in Bellingham, Washington, decreased by 4.6% after the incident, with the negative impacts reducing to 0.2% at a distance of 1,000 feet. Lee et al. (2021) study an underground pipeline explosion in Taiwan and find that a pipeline incident decreases the average housing prices in the associated city by 2.9%. Existing studies only focus on individual pipeline incidents. Our results demonstrate different types of pipeline incidents have highly heterogeneous impacts. Compared to individual pipeline incident studies, our results may have more external validity. Our study contributes to the literature by systematically analyzing hundreds of pipeline incidents across the United States, thus providing generalizable insights into which types of pipeline incidents are more likely to affect housing prices.

We also showcase the importance of information disclosure in estimating the impact of pipeline incidents on property value. The amount of information available about a property and its surrounding environment shapes homebuyers' bidding prices for a property. Pope (2008*a*) is among the first to consider how asymmetric information between buyers and sellers can affect the hedonic price gradient and shows that airport noise disclosure reduces the value of houses. Furthermore, Pope (2008*b*) studies seller disclosures for flood zones and finds a significant decline in housing prices in flood zones after disclosures. Walsh and Mui (2017) use a disclosure law to explore the impact of information on a hedonic analysis. Guignet et al. (2018) argue that incomplete information can lead housing markets to underprice disamenities. Hilterbrand Jr (2019), through a survey, finds that disclosure of pipeline location affects homebuyers' offers for a nearby property. However, it is inconclusive whether the existence of pipelines affects property values nearby. Wilde, Loos and Williamson (2012) summarize results in previous decades and conclude that there is no evidence that proximity to pipelines reduces property values. Similar to our findings, Wilde, Williamson and Loos (2014) find that neither the announcement nor the construction of a pipeline has a significant impact on nearby residential housing prices. McElveen, Brown and Gibbons (2017) find an insignificant impact of pipelines using spatial models. In the United States, the Pipeline Public Awareness Program makes information about pipelines and the approximate position of pipelines available to homebuyers, yet many homebuyers do not know the exact location of a pipeline until salient signals, such as a pipeline incident, occur. (Hansen, Benson and Hagen, 2006; Herrnstadt and Sweeney, 2022)

3 Data

This section discusses the data sources of pipeline incidents and housing transactions. Additionally, it outlines our data-processing procedure, detailing the preparation of different sample sets for different analyses step by step.

3.1 Pipeline Incident Data

The PHMSA pipeline incident database includes all 1,222 incidents on gas distribution pipelines between 2010 and 2020, 976 of which occurred in urban areas. We study urban pipeline incidents in the past decade since, unlike earlier data, pipeline incident data after 2010 have precise coordinates and more detailed information about incident characteristics.

Given the varying data requirements and criteria for different parts of our analyses, we use different subsets of incidents as the analysis progresses (see Section 3.3 for more details).

In addition to basic information on the location, time, and cause of each incident, our dataset provides additional details, such as the number of fatalities, the estimated volume of gas unintentionally released, the estimated direct damage cost, the occurrence of explosion or ignition, and whether the incident occurred above ground or underground. In our regression analysis, we categorize the incidents into three groups (*a*) high-profile (both severe and above-ground); (*b*) only severe or above-ground; and (*c*) neither severe nor above-ground.

3.2 Housing Transaction Data

We compile housing transaction data from the ZTRAX database. The ZTRAX database contains about 491 million transactions associated with 161 million owner-occupied houses across all 50 states, Washington D.C., and Puerto Rico (Nolte et al., 2024) . The final dataset for our hedonic analysis includes real property transactions 4,000 days before and after each incident occurring between 2010 and 2020 from 24 states. The housing transaction data includes detailed information on sales price, the date of the transaction, and house structural characteristics, including the year built, number of bathrooms, number of bedrooms, square footage, and indicators for air conditioners and fireplaces.

In the data cleaning process, we remove transactions with sale prices lower than $1,500, as well as those with missing primary structural characteristics, such as the number of bathrooms or bedrooms. We only include arms-length transactions of single-family homes in the regression analysis[7]. We deflate all sale prices to 2020 dollars using the Federal Housing Finance Agency's state-quarter house price index.

3.3 Merging Property Transactions and Pipeline Incidents

We identify properties at risk of being affected by pipeline incidents based on their proximity to the incident sites. Among the 976 urban incidents between 2010 and 2020, we focus on 426 urban incidents that we can link to residential property transaction observations containing complete housing attributes in a five-kilometer radius 4,000 days before and after incidents[8]. To ensure the external validity of our findings, we assess the representativeness of the 426 incidents included in the study by comparing them with the 976 urban incidents.

[7] Appendix C provides more details about our approach to processing Zillow housing data, taking into account the guidelines of Nolte et al. (2024), although we do not strictly adhere to them.

[8] Complete housing attributes include the year built, number of stories, bathrooms and bedrooms, lot size, and indicators for air conditioners and fireplaces. The dropped incidents are mostly those in non-disclosure states where transaction data is sparse.

Table B1 in the appendix shows a balanced severity and location distribution between our sample and the complete pool of urban incidents.

At the start of the analysis, we match each transaction to multiple incidents, thereby generating more than five million observations of transactions in total, with four million unique transactions. We then proceed with the analysis in two steps. First, we conduct scale analyses based on the 426 included incidents to determine the spatial and temporal range of property value impacts from different types of pipeline incidents. Among all incidents retained for scale analyses, 126 are high-profile, 145 are only severe or above-ground, and 155 are neither severe nor above-ground.

Second, we design the treatment and control areas for our DID analysis based on the results of scale analyses. Therefore, we keep transactions within the treatment/control areas and before/after periods of at least one incident. To ensure clean identification, we apply the following rules to match each transaction to one unique incident: (*a*) we remove transactions assigned to the treated-after groups of multiple incidents because they confound the impacts of more than one incident; (*b*) we retain transactions linked to the treated-after group of only one incident, and we do not use them in any other incidents, which prevents the use of already-treated observations in the control group; and, (*c*) we randomly assign a transaction to an incident when the above two steps still allow for one transaction to belong to the before period or control group for more than one incident.

After applying those data cleaning steps, we retain an incident if it has transactions in both control and treatment areas. These procedures leave us with 310 incidents linked with 369,926 unique transactions for the DID analysis on short-term impacts. On average, an incident is matched with 1,193 unique transactions. As shown in Table B2 in the appendix, a linked house is 48 years old and has a lot size of 9,890 square feet, 1.4 stories, 2.8 bedrooms, 0.9 air conditioners, 0.35 fireplaces, and 1.65 full baths, on average. The average distance to the hospital is 3.86 km, while the distances to the nearest school and university are 0.79 km and 3.79 km, respectively. Among the 310 incidents, 95 are high-profile, 110 are only severe or above-ground, and 105 are neither severe nor above-ground. For the long-term analysis, we follow the same procedure but extend the temporal range of impacts, retaining 125 high-profile incidents.

In addition to price impacts, we investigate the impacts of incidents on the volume of property transactions in Section 7.1. We summarize the number of transactions within a 3,000-meter radius and 1,000 days before and after 95 high-profile incidents[9]. Table 2

[9] In this step, we tally all transactions occurring before and after incidents. We relax the data matching requirements used in the DID analysis for price effects, and allow transactions to be linked to multiple incidents.

presents the descriptive statistics for transaction volumes pre- and post-incidents across six time windows (i.e. 30, 40, 50, 60, 70, and 80 days). Within the 70 days before and after incidents, the maximum number of transactions during the pre-incident period is generally higher than that in the post-incident period, on average.

4 Spatial and Temporal Scale Analyses of Incident Impacts

We conduct scale analyses based on LPR to gauge the spatiotemporal extent of incident impacts. We carry out the analysis in two steps. First, we estimate a price function with controls for observable housing attributes, location attributes, and spatial-temporal fixed effects:

$$ln(Price_{ipt}) = \alpha_0 + \theta_{pt} + \omega_{mt} + \eta_j + \beta X_{it} + \phi L_i + \varepsilon_{ipt} \tag{1}$$

where $ln(Price_{ipt})$ represents the natural log of the transaction price of house i associated with incident p at time t. Since each house i belongs to block group j and county m, we suppress j and m when i appears in the subscript. A vector of housing attributes variables observed at the time of the transaction, X_{it}, includes the age of the house, lot size, number of stories, number of bedrooms, number of full baths, and the presence of central air conditioning and fireplaces, while L_i is a vector of location characteristics, including distance to the nearest hospital, distance to the nearest school, and distance to the nearest university. To control for heterogeneous price trends at different incident locations, we include an incident-by-year fixed effect, θ_{pt}. We include county-quarter fixed effects, ω_{mt}, to control for seasonal shocks in different housing markets. We also include location-fixed effects at the block-group level, η_j, to account for all time-invariant unobserved factors associated with a block group. The disturbance term, ε_{ipt}, is clustered at the incident level. We derive the adjusted housing price based on the predicted residual from the above model.

In the second step, we conduct a bivariate LPR[10] with the adjusted housing price as the dependent variable to recover the price gradient by distance or time. Specifically, we apply the analysis on all available transactions within five kilometers of and 4,000 days before and after 426 included incidents. Since incidents with different characteristics may have distinct spatial and temporal scales, we assess the three types of incidents separately.

The left panel in Figure 2 shows the price gradient by distance for the three types of

[10] The bandwidth for the distance gradient is about 300 meters, while the bandwidth is about 120 days for the time gradient. Results using other bandwidths are available from authors upon request.

incidents. The blue solid line indicates the dynamics in adjusted transaction price before incidents, while the orange solid line shows the adjusted housing price after incidents. The dashed lines are their 95% confidence intervals. For high-profile incidents, adjusted transaction prices within about 1,000 - 1,500 meters of the incident sites diverge significantly before and after incidents. Nonetheless, we do not observe notable disparity in adjusted transaction prices before and after incidents for the two other types of incidents, implying null impacts of those incidents on nearby property values.

Leveraging results on price gradient by distance, we further assess the price dynamics of houses within 1,500 meters of incident sites and those between 1,500 and 3,000 meters. The right panel in Figure 2 exhibits the price gradient before and after the three types of incidents. The blue solid line represents the adjusted sale prices of the 1,500-to-3,000 meters group 4,000 days before and after an incident, while the orange solid line indicates the adjusted housing prices of the within 1,500 meters group. The dashed lines are 95% confidence intervals. For high-profile incidents, pre-incident trends in transaction prices of the two groups resemble each other; however, post-incident, the adjusted transaction price of the within 1,500 meters group decreases significantly while that of the control group remains relatively stable. The divergence in adjusted transaction prices of the two groups persists until about 3,000 days post-incident. For the other two types of incidents, transaction prices between the two groups do not differ significantly.

In sum, the scale analysis indicates that the spatial extent of property value impacts from high-profile incidents covers at least 1,500 meters, while the temporal duration extends to about 3,000 days. In addition, we do not find evidence for property value impacts from the two other types of incidents based on the results of the above scale analysis.

To further confirm the spatial extent of price impact, we conduct a regression with fine spatial bins with treatment groups being eight distance bands reaching 2,000 meters from the incident.[11]. Figure A1a shows the estimations with 95% confidence intervals for all distance bins of treatment groups. Property value impacts within 1,000 meters of high-profile incidents are negative and statistically significant. Impacts beyond 1,500 meters become economically small and statistically insignificant. Impacts between 1,000 meters and 1,500 meters are ambiguous since the estimates are not statistically significant but still have substantial magnitude.

Similarly, we further conduct a regression with fine temporal bins to provide formal

[11] The fine spatial bins regression regresses log transaction price on the indicators for eight distance bins for houses within 5-250, 250-500, . . . , and 1,750-2,000 meters from incidents, respectively. The control group is a distance bin between 2,000 and 2,500 meters. Similar to a DID framework, we include interaction terms between the distance bin indicators and the post-incident indicator to measure the impacts of incidents. Other control variables and fixed effects are similarly specified as in equation 1

statistical evidence for the temporal extent of property value impacts from high-profile incidents. Building on the results from Figure A1a, transactions within 1,500 meters of an incident form the treatment group, while those within 1,500 and 3,000 meters comprise the control group. The model mirrors the fine spatial bin model but replaces distance bins with a series of 365-day time dummies ranging from 11 years before to 10 years after the incident. Figure A1b reports the estimations on all time dummies. Regression results suggest a negative and persistent property value impact for at least eight years post-incident.

The scale of impacts estimated from LPR and regressions with fine spatial/temporal bins should be interpreted as the impact range that can be detected using the pooled incident sample within each incident type. It is possible that some incidents' impacts reach further, but they are diluted by more localized incidents and cannot be detected in the pooled analysis. Therefore, the scale of impacts found here should not be interpreted as a universal range or maximum range of price impacts for all pipeline incidents.

5 Quantifying the Property Value Impacts of Pipeline Incidents

This section initially delves into our identification strategy, as derived from the scale analyses, and assesses the covariates balance in the sample for the DID analysis. Additionally, it discusses the quantification of the short-term and long-term price impacts, as well as an event-study analysis.

5.1 Identification Strategy and Covariates Balance

We adopt a DID framework to formally estimate the average treatment effect of pipeline incidents on property values. Guided by the scale analyses in the previous section, we define the treatment areas as circles centered on the incident sites with a radius of 1,000 meters. We define the control areas as rings, also centered on incident sites, ranging from 1,500 to 3,000 meters from the site. We exclude transactions between 1,000 and 1,500 meters since we cannot conclusively assign them to either group, and this exclusion prevents potential bias due to treatment spillovers. We employ transactions within a 2,000-day or 4,000-day window for short-term and long-term DID analyses, respectively. Since our scale analyses do not provide any evidence for the property value impacts from non-high-profile incidents, we apply the same quasi-experimental design for DID analysis on the two types of incidents. Our identification strategy thus relies on the price differences between treatment and control groups before and after the incidents. We relax the key parallel assumption of the canonical DID framework by allowing the price trends between treatment and control groups to vary across different incidents as long as these trends remain parallel within each incident. Figure 1 illustrates the quasi-experimental design for the DID framework.

Based on the quasi-experimental design, we retain 310 incidents for the DID analysis on short-term impacts. Among these incidents, 95 are high-profile, 110 are only severe or above-ground, and 105 are neither severe nor above-ground. Figure 1 presents the spatial distribution of the 310 different incidents included for short-run DID analysis. The majority of the included incidents are clustered in the Midwest (e.g., Iowa, Indiana, Ohio, and Kentucky) and the East Coast (e.g., New York, Massachusetts, and New Jersey). Regions with dense populations on the West Coast (e.g., San Francisco Bay Area) and the South (e.g., Houston) also witnessed multiple incidents over the years.

We further examine the balance of covariate distributions in the treatment and control groups. Panel A of Table 1 presents descriptive statistics of housing prices and characteristics of transactions associated with 95 high-profile incidents for our short-term DID analysis. Columns (1) and (2) report the mean and standard deviation (in parentheses) of covariates of the control and treatment groups before incidents, while Columns (3) and (4) present the same information for control and treatment groups after incidents. We employ the standardized difference to assess the balance in covariates between the pre-incident groups and post-incident groups, respectively.(Imbens and Rubin, 2015) We follow Austin (2009) and consider 0.1 (in absolute value) as indicative of balance. Results in Columns (5) and (6) suggest generally balanced covariates between control and treatment groups both before and after incidents, with standardized differences of a few housing attributes slightly higher than 0.1. In Section 6.4, we adopt a propensity score matching and regression approach to examine the robustness of the DID results using a subset of transactions with balanced covariates. Additionally, Table B4 in the Appendix shows that covariates are mostly balanced for the two other types of incidents.

5.2 Short-term Property Value Impacts

To provide evidence for the plausible causal impacts of incidents on property values, we first utilize transactions occurring 2,000 days before and after incidents to quantify short-term property value impacts. Although the results of our scale analyses indicate the property value impact lasts longer, a narrower time period for the DID analysis may enable cleaner identification, as the parallel trend assumption is more likely to hold in a shorter period. Moreover, property price trends within 2,000 days post-incident do not exhibit a rebound, which motivates the bunching of the first 2,000 days.[12]. We specify our DID model as:

$$ln(Price_{ipt}) = \alpha_0 + \alpha_1 Treat_{ip} + \alpha_2 Post_{ipt} + \gamma Treat_{ip} \times Post_{ipt} \qquad (2)$$
$$+ \rho X_{it} \times year_t + \phi L_i + \theta_{pt} + \omega_{mt} + \eta_j + \varepsilon_{ipt}$$

where $Treat_{ip}$ denotes the treatment group designation for house i, which equals 1 if the house is within a 1,000-meter radius of incident p and 0 if the house is between 1,500 meters and 3,000 meters from the incident; and, $Post_{ipt}$ is a dummy variable that equals 1 if house i is sold within 2,000 days post-incident p, and 0 if the transaction occurs within 2,000 days pre-incident. Additionally, in some specifications, we further include the interactions between housing attributes and yearly trend $X_{it} \times year_t$ to allow the hedonic function to evolve over time (Kuminoff and Pope, 2013). In one alternative specification, we further interact the housing attributes with incident fixed effects to account for heterogeneous trends in different housing markets where different incidents occur. To formally estimate the decay of the treatment effect as distance increases, one alternative specification replaces $Treat_{ip}$ with indicators for three equal distance bins, B^d_{ip}, for $d \in \{1, 2, 3\}$, which equals 1 if a house is within 5–500, 500–1,000, or 1,000–1,500 meters from incident p, respectively. We define all other notations similarly as in equation 1. Standard errors are clustered at the incident level. A potential concern with our identification strategy is that the varied timing of incident occurrences resembles a staggered DID design, which leads to negative weights bias when averaging housing-price treatment effects from many sub-experiments (i.e., 2x2 DID framework for an individual incident) (Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021). Our study design circumvents this problem by matching the treatment group of each incident with a specific control group. In addition, in data processing, we ensure that we do not include already-treated houses in the control group.

We perform the short-term DID analysis on the three types of incidents separately. Table 3 reports the results of our DID analysis for short-run impacts using a series of model specifications. As for high-profile incidents, the estimation result of Model (1) suggests that, *ceteris paribus*, the occurrence of a high-profile incident significantly decreases average property values within 2,000 days and 1,000 meters of the incident site by 10.5%[13], controlling for housing attributes and location characteristics, as well as block-group fixed effects. Models (2) and (3) incrementally add county-quarter fixed effects and incident-by-

[12] Table 7 in Section 6 provides robustness checks with various shorter time windows

[13] We use $(exp(\hat{\gamma}) - 1) * 100$ to calculate the percentage change.

year fixed effects, respectively. Estimation of the impacts of high-profile incidents decreases slightly to 8.4% when we control for the incident-specific temporal trends. Another concern with our quasi-experimental design is that the parallel trend assumption might be valid only for properties within a local area, not those farther away. The inclusion of incident-by-year fixed effects relaxes the common trend assumption and allows for differential trends for each incident as long as price trends in the control and treatment groups for the same incident follow a parallel trend.

Within a single market, the coefficient estimates on housing attributes represent the gradient of the hedonic price function for those attributes. In our setting, the 95 high-profile incidents cluster around various cities/regions indicating different housing markets; thus, we can interpret the coefficients of the control variables as the weighted average of the hedonic implicit prices across different markets. To better control for housing attributes in the multi-market setting, we interact all control variables with incident fixed effects in Model (4). Estimations on the incident impacts remain consistently at the level of 8.0%. Furthermore, Model (5) allows the hedonic price function to evolve over time. Estimations on the incident impacts remain consistent at around 8.2%. Lastly, Model (6) estimates the property value impacts of high-profile incidents based on three distance bins. The property value impacts exhibit a decreasing trend as the distance between the property and incident sites increases. Beyond 1,000 meters, the impacts become statistically insignificant and undetectable. The decline in price impacts along distance could be attributed to the decay of information (less buyers and sellers farther away from incident sites know about the event) and the decay in perceived risk. Table B5 presents the coefficients of all the independent variables based on Model (3), which finds significantly increased housing prices along with a larger lot size, more stories, bedrooms, full baths, and the presence of central air conditioning and fireplace.

The results of Columns (7) and (8) report the estimation results on only severe or above-ground incidents and neither severe nor above-ground incidents, respectively, using the same specifications in Model (5). As expected, we detect no significant property value impacts for these two types of incidents. A smaller treatment group (i.e., 5 - 750m) is also examined for the non-high-profile incidents as presented in Table B6 in the Appendix, confirming null price effects. Taken together, the results of the DID analysis indicate that the impacts of pipeline incidents are likely to be heterogeneous based on incident characteristics and the specific conditions of each property market. Finally, results in Column (9) suggest no significant price effects based on the 310 heterogeneous incidents. When we analyze a group of pipeline incidents together, the estimated impact is the average across incidents with more weight assigned to incidents with more available transactions. We can interpret the estimate in Column (9) as null price impacts on a statistically representative property associated with pipeline incidents.

5.3 Examining the Parallel Trend Assumption

To test the parallel trend assumption, we adopt a variant of the DID model as shown below:

$$ln(Price_{ipt}) = \alpha_0 + \alpha_1 Treat_{ip} + \sum_{\tau=-15,\tau\neq 0}^{16} \beta_\tau B_\tau + \sum_{\tau=-15,\tau\neq 0}^{16} \gamma_\tau B_\tau \times Treat_{ip} \quad (3)$$
$$+ \rho X_{it} \times year_t + \phi L_i + \theta_{pt} + \omega_{mt} + \eta_j + \varepsilon_{ipt}$$

where B_τ is an indicator that equals 1 if the transaction occurred during the time bin τ. Each time bin covers 125 days, from 2,000–1,875 days before ($\tau = -15$) to 1,875–2,000 days after ($\tau = 16$) the incident. The event study includes all transactions occurring 2,000 days before and after an incident, resulting in 16 lead bins prior to incidents and 16 lag bins after incidents (τ ranges from -15 to 16), respectively. We set transactions within 125 days before an incident as the reference group ($\tau = 0$). We define all other fixed effects and control variables similarly as in equation 1. Standard errors are clustered at the incident level.

Figure 3 shows the point estimations (dark points) and associated 95% confidence intervals (red I-segment) for all time bins 2,000 days (about 5.5 years) before and after incidents. The dashed line indicates the 125 days before incidents, serving as the base group for comparison. Overall, all coefficients circle around the zero horizontal line, indicating that estimations on bins prior to incidents are statistically insignificant. A Wald test suggests that the differences in all point estimations for pre-incident bins are not significant ($p = .33$), providing evidence for the validity of the parallel trend assumption in our setting.

Additionally, Figure 3 shows that all estimations on lag bins after incidents position lower than the zero line, indicating that the average property values within 1,000 meters of incidents decline compared to properties within 1,500–3,000 meters. The general trend in the coefficients of all lag bins after incidents also suggests persistent property value impacts in the long term.

5.4 Long-term Property Value Impacts

The above results indicate that a high-profile incident will decrease nearby property value for at least 2,000 days. A policy-relevant question is whether the observed property value impacts become a persistent shock on the local housing market. To explore the property value impacts in the long term, we include transactions occurring 4,000 days before and after incidents to conduct a DID analysis based on two variants of equation 2. As we extend the time windows of transactions, we eventually obtain 125 high-profile incidents associated with 191,251 transactions for analysis.

Table 4 reports the results of three DID models. Model (1) uses equation 2 based on transactions 4,000 days before and after 125 high-profile incidents. Compared to properties within 1,500–3,000 meters of incidents, the average property values within 1,000 meters of incidents are lower by 7.5% in the 4,000 days after incidents. Furthermore, we replace the $Post_{ipt}$ with four time dummies (i.e., *Period: 0–1,000 days,..., or 3,000–4,000 days*), indicating each 1,000-day time window after incidents in equation 2. Results in Model (2) suggest that the decay of pipeline impact only happens after 2,000 days. The price impacts decrease from approximately 8.2% during the first 1,000 days to 6.3% between 2,000 and 3,000 days and become statistically undetectable thereafter. We specify Model (3) similarly to Model (2) but use only two time dummies to indicate the initial period of 3,000 days and the period thereafter. Results of Model (3) confirm a long-lasting but non-permanent property value impact of 8.0% within eight years (3,000 days) after high-profile incidents, on average.

6 Robustness Checks

In this section, we carry out a series of robustness checks to address potential concerns in terms of treatment estimates, missing values, covariates balance, control group selection, and alternative explanations of regression results.

6.1 Placebo Incidents

If the observed price impact is indeed caused by the incidents, then the effect should not exist if the same study design is applied to locations without an incident. To examine the robustness of our main findings, we implement our preferred DID model on placebo incidents that are located next to the real ones. To this end, we draw two placebo incidents, positioning 6,000 meters to the west and east of a high-profile incident but occurring at the same time. The placebo incidents have the same control and treatment area design as the actual incident. The design of placebo tests is illustrated in Figure A5 in the Appendix. We apply our preferred DID model (i.e., Model (5) in Table 3) separately on the west-side and the east-side placebo incidents. Columns (1) and (2) in Table 5 report the results. As expected, no significant price effect is observed for either east-side or west-side placebo incidents.

A potential threat to our identification strategy is the violation of the parallel trend assumption in price trends between control and treatment areas of an incident. For instance, high-profile incidents may occur more frequently in low-income areas with inadequate maintenance, and these areas may be experiencing declining price trends relative to surrounding areas. To address this concern, we test for pre-trends in the event analysis in Section 5.3. Here, we provide an additional test by artificially shifting the occurrence date of incidents 500 or 1,000 days earlier to check for spurious impacts (Haninger, Ma and Timmins, 2017; Tang et al., 2020). A causal relationship between pipeline incidents and housing values implies that an incident with a false occurrence date would generate null impacts on housing prices. As expected, Columns (3) and (4) of Table 5 show insignificant and near-zero estimations of incidents with manipulated occurrence dates, which further suggests that residents are unlikely to engage in any anticipatory behaviors that may affect housing prices.

6.2 Missing Values

Given the relatively high ratio of missing observations caused by incomplete housing attributes, we check the robustness of our results to missing values in Table 6.[14] First, we follow Zhang, Phaneuf and Schaeffer (2022) and run our preferred DID model without any controls for housing attributes (Column (1)). In this specification, the variation of housing attributes not absorbed by the fixed effects will remain in the error term, potentially inflating standard errors for coefficient estimates. Second, we code missing values as zero and then include missing value indicators for each housing attribute in the regression (Guignet et al., 2023*a*,*b*). This specification assigns a fixed effect for observations missing certain housing attributes while retaining the variation of that housing attribute for non-missing observations. Third, we impute missing values using the group mean substitution method (Sim et al., 2015). Specifically, we replace missing characteristics using the mean values

[14]These missing methods are applied to all transactions associated with 95 high-profile incidents.

for price quantiles within each block group or census tract (Column (3))[15]. Lastly, as an alternative specification (Column (4)), we further include missing indicators in case the imputed values have systematic bias.[16] Results from Table 6 show that the estimations for price impacts of high-profile incidents maintain around 8%–9% across all model specifications with $p < .05$.

6.3 Alternative Control Group Areas and Time Windows

Next, we check if the estimates are sensitive to the choice of control groups. We derive three alternative control groups, with ranges of 1,000–2,500 meters, 2,000–3,500 meters, and 2,500–4,000 meters. We run our preferred DID model (Column (5) in Table 3) using these alternative controls. Columns (1) to (3) of Table 7 show that the estimations for price impacts range from -0.083 to -0.087 with statistical significance at the 5% level. Our findings remain robust.

Similarly, we further examine if our estimations are robust to various shorter time windows. Presumably, utilizing transactions within a time window shorter than 2,000 days may potentially yield similar or even larger impacts compared to our findings in Section 5.2. We apply the preferred DID model to three subsamples based on time windows of 500 days, 1,000 days, or 1,500 days. Results in Columns (4) to (6) of Table 7 indicate significant and negative price impacts of incidents, estimated at approximately 9.0%.

6.4 Propensity Score Matching and Regression

The next robustness check incorporates a matching and regression method to improve the covariates balance between transactions in the treatment and control groups. We deploy the propensity score matching technique to adjust for differences in pre-incident observables. Specifically, we match each transacted house in the treatment group with one house in the control group based on all housing attributes, yielding 29,452 matched observations in total. Figures A3 and A4 present the distributions of each housing attribute in the treatment (solid line) and control (dashed line) groups before (left panel) and after (right panel) propensity score matching, respectively. The balanced distributions indicate a successful adjustment, providing suggestive evidence for the common support assumption (Imbens,

[15] If all transactions in the block group have missing observations, we use the within-quantile average for each census tract. We do not use higher geographical levels for imputation as it would introduce larger measurement errors.

[16] Table B3 indicates that the expectations of all housing attributes do not differ significantly before and after the implementation of group mean substitution.

2015). Regression results based on the matched sample in Table B7 confirm the robustness of our main results.

6.5 Physical Damages and Lock-in Periods

Lastly, we address two additional concerns regarding physical damages and lock-in periods. First, the price effects may capture price changes stemming from physical damage to properties. We attempt to address this concern by conducting DID models that exclude houses positioned very close to incident sites (i.e., a 10, 40, 70, and 100-meter radius from incidents). If our results are mostly driven by direct damage to nearby houses, the estimated impact should sharply decline once houses at the centers of incidents are excluded. Results in Table B8 indicate that, as the circle of exclusion expands, the estimated impact only experiences a modest decline. While we cannot completely rule out the possibility of price effects due to physical damage to the houses, this exercise seems to suggest that such direct damage is not driving our results.

In many cases, parties involved in a transaction may negotiate in advance to create a lock-in period when the selling price and mortgage rate remain fixed before the transaction can be settled and documented. If an incident occurs during the lock-in period of a transaction, the transaction price would fail to capture the impacts of the pipeline incident and thus bias our estimation. To alleviate the potential bias due to the lock-in period issue, we exclude all transactions within the initial one to six months of the post-incident period and re-run our preferred DID model. Results in Table B9 show that our main findings remain robust.

7 Mechanism Analyses

This section discusses two analyses regarding transaction volume and pipeline installations, respectively, providing evidence for channels through which pipeline incidents affect the local housing market.

7.1 Transaction Volume

In addition to the price impacts, high-profile incidents may affect the local housing market through other channels, including limiting the liquidity of properties (Depro and Palmquist, 2012; Guignet and Martinez-Cruz, 2018; Irwin and Wolf, 2022). While a direct analysis of property liquidity is hindered by a lack of house listing data, we are able to analyze the changes in transaction volume before and after incidents, providing insights on reactions from the supply and/or demand side. On the one hand, assuming pipeline incidents impact transaction volume, incidents may stimulate intentions among some homeowners to sell and relocate, resulting in increased supply and, consequently, lower prices and increased quantity. On the other hand, potential homebuyers may be wary of buying houses near incident sites, leading to a reduction in both price and quantity due to weakened demand.

We investigate the change in the total number of transactions before and after incidents. Unlike the price analysis, we retain the one-to-many transaction-incident links to avoid losing transactions due to data processing. Considering the possible heterogeneous spatial and temporal ranges compared to the price market, we again explore the scales of the impacts on transaction volume. Figure A2 depicts the evolving impact of incidents on the transaction volumes across three 250-meter-wide distance bins, ranging from 30 to 80 days before and after incidents. Table B10 in the appendix includes regression results based on different treatment groups (e.g., 600m, 700m, 800m, 900m, or 500-1,000m) and a 60-day time window. Based on those results, we detect a short-lived and localized negative volume impact in the 500-meter radius of incidents within 60 days. We further apply our DID analysis by assigning houses within 500 meters as the treatment group and those between 500 and 1,000 meters as the control group. Table 8 presents a significant volume reduction of about 0.5 transactions in the initial two months post-incidents, accounting for nearly a 24% decline within 30 days and a smaller 10% drop within 60 days. Although we are unable to rule out supply-side responses, the reduction in transaction volume indicates a substantial demand-side reaction due to salient information shock.

7.2 Impacts of Pipeline Installations

A potential mechanism for incidents to affect housing values is by revealing the existence of nearby pipelines. If true, then pipeline installation should cause a similar effect. To test this argument, we investigate if pipeline installation suppresses housing values. Using information on the installation year of pipelines associated with 15 incidents, we construct the corresponding pre- and post-installation periods. Moreover, considering the one- to two-year pipeline permitting period, we conduct DID analyses around the event times that are one and two years before the completion of installation to test for anticipation effects. As Table 9 shows, there is no obvious anticipation effect before installation and also no statistically significant effects after installation. These findings are consistent with existing studies that find null effects for pipeline installation (Wilde, Williamson and Loos, 2014;

McElveen, Brown and Gibbons, 2017). However, we should interpret the null result on the installation effect with caution. First, the confidence intervals for the installation effect are fairly large; thus, we cannot conclusively rule out the installation effect. Second, if the installation effect is zero, our result is not enough to evaluate whether the lack of response is rational or not. To answer that question requires the quantification of objective risk, which is beyond the scope of this study.

8 Discussion and Conclusions

Pipeline safety and the economic impacts of pipeline incidents are of great concern to the public and policymakers. Based on 426 gas distribution pipeline incidents in urban areas across 24 states between 2010 and 2020, we find that high-profile pipeline incidents that are both severe and above-ground have, on average, a -8.2% property value impact within 2,000 days. The negative impact remains around -6.3% between 2,000 and 3,000 days and disappears after that. These findings are robust to a series of checks and cannot be reproduced with placebo incident dates and locations. However, we do not find statistically significant impacts from non-high-profile incidents. The price impacts from high-profile incidents substantially surpass the two individual incidents studied by Hansen, Benson and Hagen (2006) and Herrnstadt and Sweeney (2022), respectively, in terms of intensity, spatial extent, and duration. For example, the incident in Hansen, Benson and Hagen (2006) decreases property value by 4.6%, and the effect disappears within 333 meters (1,000 feet) and one year, while the incident in Herrnstadt and Sweeney (2022) decreases property value by 2% within 167 meters (500 feet) and one year. It is possible that the previous low estimates are due to the type of incident being studied: both of these studies consider severe and underground incidents for which we find no statistically significant effect. Using broader scale analysis, we contribute to the literature on the impacts of pipeline incidents, and other localized environmental incidents in general, by showing that there can be substantial heterogeneity by incident types, and that results from case studies should be generalized with caution.

To put the magnitude of the price effects into perspective, we perform a back-of-the-envelope calculation of housing value loss. The pre-incident average price of a house within a 1,000-meter radius of a high-profile pipeline incident is $204,247. A treatment effect of approximately -8.0% on the treatment group results in an average value loss of $16,340 (2020 dollar), compared to a loss of $6,000 — $10,000 (2020 dollar) for a typical house affected by derailments involving HAZMAT (Tang et al., 2020). More importantly, the effects from high-profile pipeline incidents persist for 3,000 days, compared to about 480 days for rail incidents. Therefore, high-profile pipeline incidents seem to have more intense and persistent effects on surrounding properties than derailments involving HAZMAT. A complete comparison of the two transportation modes requires calculating marginal costs, and the results from this paper provide useful information to back that calculation.

The loss in property values also surpasses the direct costs of the incidents. In our DID sample, there are 95 high-profile incidents with an average direct cost of about $4 million per incident. For each incident, there is an average of 360 transactions over 3,000 days (i.e., the temporal extent of incidents) suffering an average of 8.0% price decline. Therefore, the overall indirect property value loss from these transacted houses is about $6 million. Note, however, that this calculation underestimates the indirect loss because it only includes transacted single-family houses, and the incidents likely also impact other types of properties as well as residents in properties not transacted. The high indirect costs relative to direct costs suggest that there may be underinvestment in pipeline safety if policymakers only consider direct costs. Also, relative to other types of incidents, the distinct effects of high-profile incidents mean that resources should be shifted toward the prevention of severe and above-ground incidents.

In terms of mechanism, the result that only high-profile incidents have an impact aligns with findings by existing studies that only the most significant environmental incidents affect home values (Zabel and Guignet, 2012; Guignet et al., 2023*b*,*a*). The most likely explanation is that only salient events produce enough information shocks to prompt action. From the incidents, the residents could learn about the existence of nearby pipelines for the first time. The lack of installation effect seems to suggest that this kind of information alone is not a major driver of the loss in property value. The fact that some incidents on above-ground pipelines, which are observable all the time, can affect property values also suggests that other factors are at play. Therefore, the effective new information that residents receive is more likely about the additional risk of pipelines, not the mere existence of pipelines. The observed initial drop in transaction volume suggests that this information has reached the buyers.

Despite our robustness checks that exclude properties very close to the incident sides, we cannot rule out direct impacts. First, imprecise geolocation can cause some damaged houses to be placed away from the incident sites. Second, a severe incident can lower property values by decreasing amenities. The amenity impact channel may coexist and complement the information channel: the sight of burned houses is a constant reminder about the potential risks associated with pipelines. Delayed clean-up and reclamation may create stigma that deter potential homebuyers, further impeding the recovery of local housing market (Messer et al., 2006).

A prominent feature of the pipeline impact identified in this paper is the very long span (i.e., 3,000 days) of the impact, which is an important driver of the elevated indirect cost of these incidents. The solution to prolonged incident impact depends on the mechanism: if the persistence is caused by slow cleanup and rebuilding, then the process should be expedited to limit damage to property values; if incidents cause concerns for future risks, then efforts should be made to reinforce safety measures in the affected area and assuage concerns. Additional research is needed to ascertain why pipeline impacts are so persistent and what policies can help the price recovery.

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Main Figures

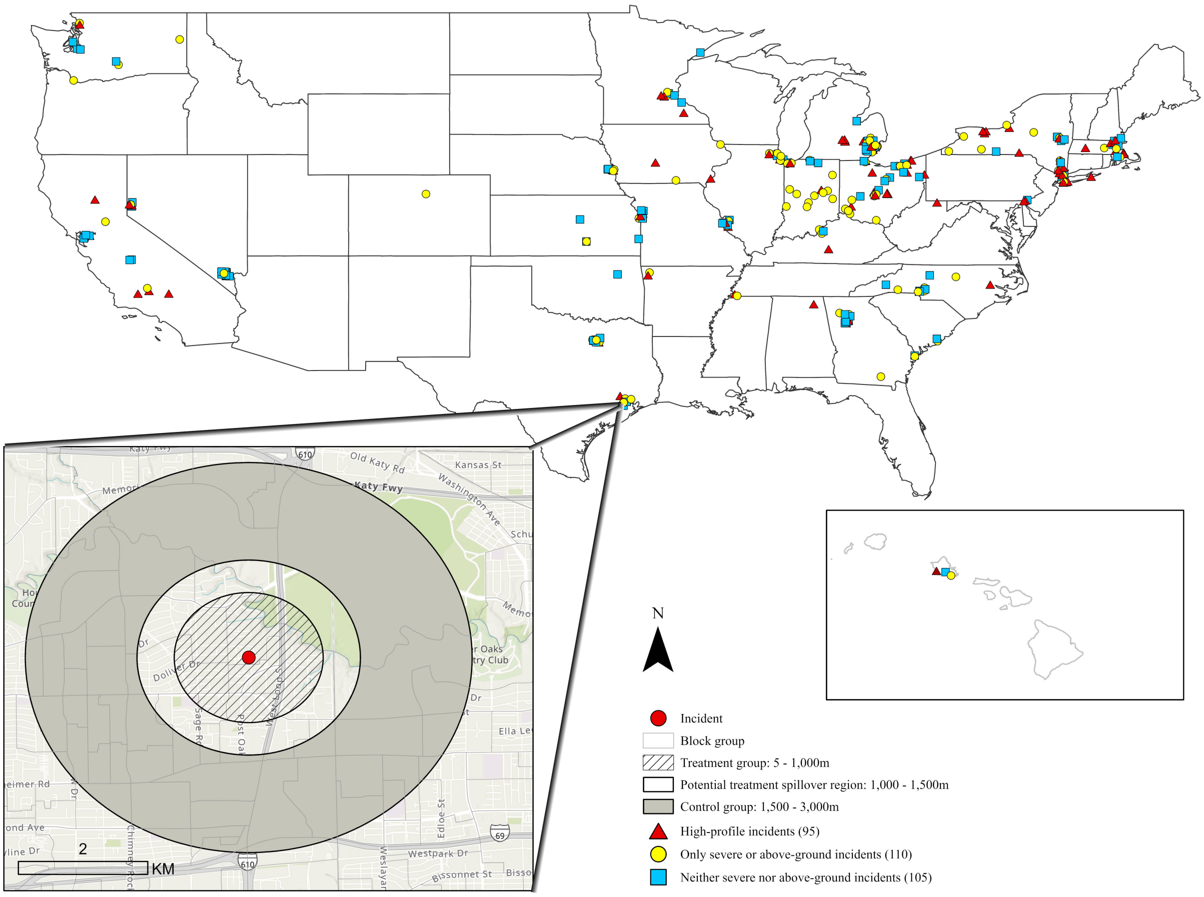


Figure 1: Gas distribution pipeline incident distribution for the 310 incidents in our DID analysis and the quasi-experimental design.

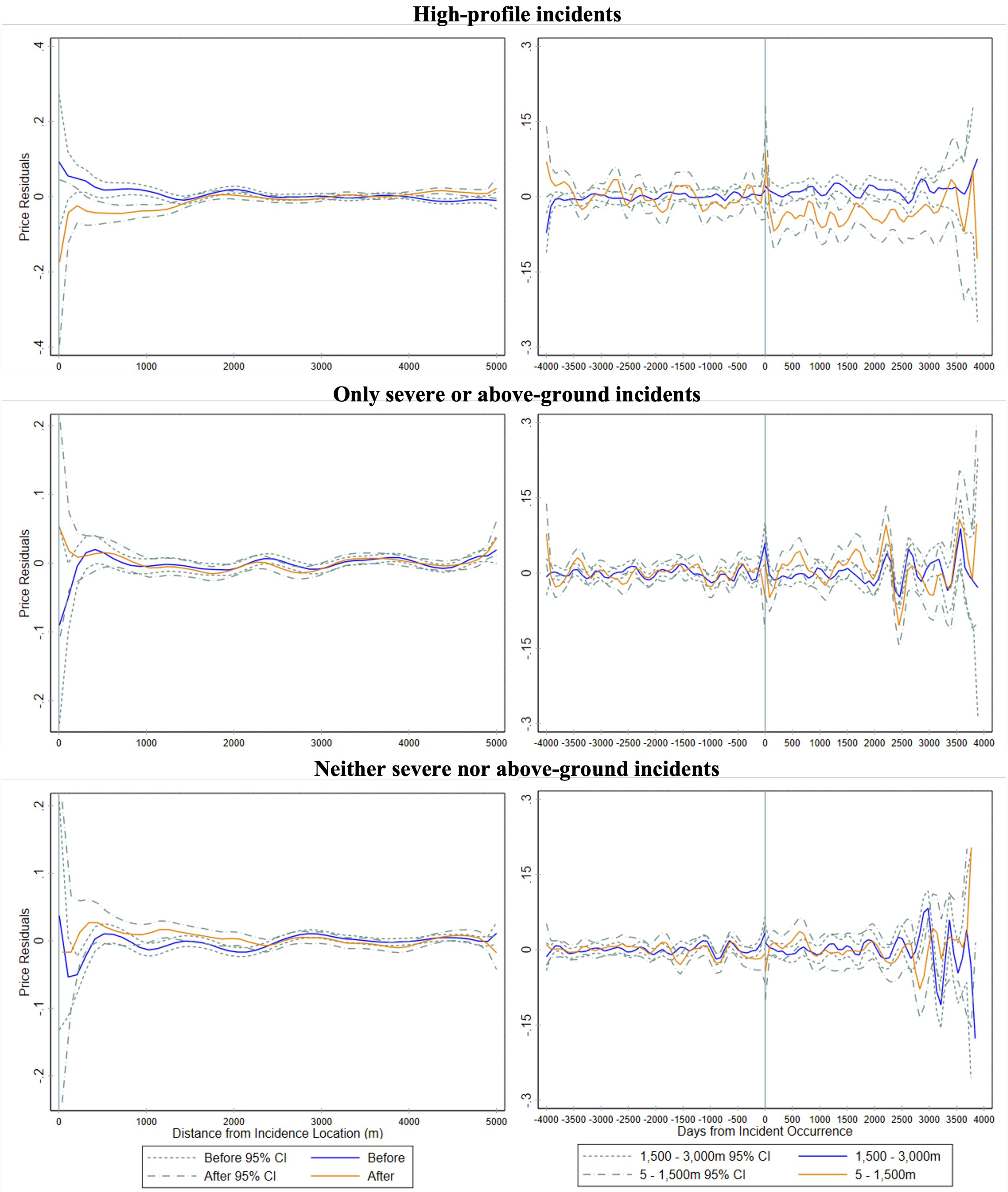


Figure 2: Local linear polynomial regressions for high-profile, only severe or above-ground, and neither severe nor above-ground incidents.

Note: the left-hand side panels present the price residual function by distance (m). The right-hand side panels show the price residual function by time (days). The upper, middle, and bottom subfigures are high-profile, only severe or above-ground, and neither severe nor above-ground incidents, respectively.

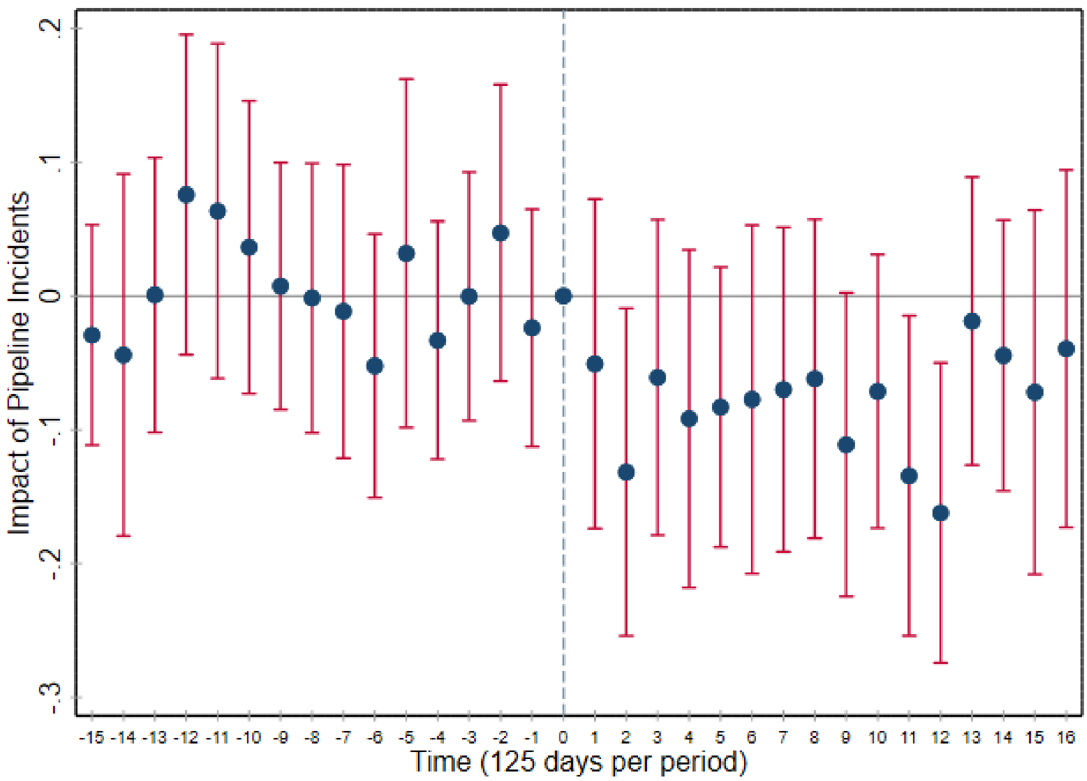


Figure 3: Event study: Price difference and 95% confidence intervals between treatment and control group before and after high-profile incidents in the short-term.

Note: Each point represents 125 days. Point 0 is the reference group, including 125 days before the incidents. This figure covers 2,000 days before and after high-profile incidents.

Main Tables

Table 1: Covariates Balance Check - High-profile Incidents

Panel A: high-profile incident samples	Pre-Control	Pre-Treatment	Post-Control	Post-Treatment	Pre-Diff	Post-Diff
	(1)	(2)	(3)	(4)	(5)	(6)
Sales Price ($1000)	203.99(207.51)	172.66(199.76)	203.69(207.16)	166.82(205.77)	0.15*	0.17*
House Age (year)	46.31(3.72)	46.25(3.88)	50.03(2.44)	50.02(2.45)	0.01	0.01
Lot Size (1,000 ft^2)	11.10(12.18)	9.85(9.95)	11.08(11.76)	10.68(12.03)	0.11*	0.03
No. of Stories	1.44(0.47)	1.46(0.49)	1.40(0.46)	1.42(0.46)	-0.04	-0.03
Total Bedrooms	2.63(1.34)	2.66(1.29)	2.66(1.29)	2.56(1.28)	-0.02	0.07
Full Baths	1.60(0.76)	1.55(0.72)	1.60(0.76)	1.50(0.73)	0.06	0.13*
Air Conditioner	0.92(0.26)	0.86(0.34)	0.92(0.25)	0.87(0.33)	0.2*	0.18*
Fireplaces	0.34(0.47)	0.28(0.44)	0.32(0.46)	0.23(0.42)	0.13*	0.20*
Dist. to hospital (km)	4.35(3.76)	4.78(5.55)	4.18(3.63)	4.35(4.75)	-0.09	-0.03
Dist. to School (km)	0.79(1.08)	0.79(2.22)	0.80(1.57)	0.83(2.51)	-0.01	-0.01
Dist. to University (km)	3.71(3.38)	3.98(5.00)	3.75(3.39)	4.11(4.53)	-0.06	-0.08
N	51,568	8,439	41,919	6,467		
Panel B: PSM matched samples						
Sales Price ($1000)	194.57(210.46)	172.66(199.76)	188.66(199.87)	166.82(205.77)	0.1*	0.1*
House Age (year)	46.06(3.81)	46.25(3.88)	50.07(2.48)	50.02(2.45)	-0.05	0.02
Lot Size (1,000 ft^2)	10.56(11.74)	9.85(9.95)	10.37(11.03)	10.68(12.03)	0.06	-0.02
No. of Stories	1.46(0.47)	1.46(0.49)	1.40(0.46)	1.42(0.46)	-0.01	-0.02
Total Bedrooms	2.61(1.34)	2.66(1.29)	2.60(1.31)	2.56(1.28)	-0.03	0.03
Full Baths	1.54(0.75)	1.55(0.72)	1.53(0.72)	1.50(0.73)	-0.01	0.03
Air Conditioner	0.85(0.35)	0.86(0.34)	0.87(0.33)	0.87(0.33)	-0.02	-0.01
Fireplaces	0.26(0.44)	0.28(0.44)	0.24(0.43)	0.23(0.42)	-0.03	0.03
Dist. to hospital (km)	4.65(4.66)	4.78(5.55)	4.41(4.39)	4.35(4.75)	-0.02	0.01
Dist. to School (km)	0.80(1.48)	0.79(2.22)	0.82(1.92)	0.83(2.51)	0.01	-0.01
Dist. to University (km)	4.00(4.36)	3.98(5.00)	3.91(3.74)	4.11(4.53)	0.01	-0.04
N	8,082	8,422	6,496	6,452		

Note: This table presents the descriptive statistics for both overall samples and PSM samples for the high-profile incidents. Columns (1) to (4) report the mean and standard deviation (in parentheses) for pre-control, pre-treatment, post-control, and post-treatment groups, respectively. Columns (5) and (6) show the standardized difference between treatment and control groups in pre- and post-incident periods.

$*$: The standardized difference between control and treatment groups is greater than .1

Table 2: Summary Statistics - Transaction Volumes by Treatment and Control Groups

Days from occurrence date	Pre-treatment period					Post-treatment period				
	N	Mean	S.D.	Min.	Max.	N	Mean	S.D.	Min.	Max.
30 days	304	1.67	2.07	0	18	304	1.77	2.29	0	16
40 days	324	2.12	2.63	0	23	324	2.22	2.91	0	21
50 days	328	2.65	3.18	0	27	328	2.71	3.37	0	24
60 days	340	3.74	4.94	0	47	340	3.64	4.55	0	33
70 days	346	4.29	5.55	0	47	346	4.08	5.06	0	38
80 days	356	4.65	5.96	0	49	356	4.47	5.67	0	42

Note: This table presents the descriptive statistics for housing transaction volumes both before and after high-profile incidents across six different time windows (i.e. 30, 40, 50, 60, 70, and 80 days before and after high-profile incidents.)

Table 3: Difference-in-Differences Estimation Results

	Log of Transaction Price								
	High-profile						Only severe or above-ground	Neither severe nor above-ground	All
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Post × Treat	-0.1110***	-0.1105***	-0.0875***	-0.0821***	-0.0854***		0.0275	0.0256	-0.0037
	(0.0395)	(0.0393)	(0.0306)	(0.0306)	(0.0294)		(0.0182)	(0.0196)	(0.0138)
Post	0.0322	0.0330	0.0137	0.0126	0.0328	0.0338*	-0.0113	0.0101	0.0098
	(0.0261)	(0.0261)	(0.0207)	(0.0200)	(0.0202)	(0.0202)	(0.0170)	(0.0172)	(0.0104)
Treat	0.0127	0.0118	-0.0008	0.0027	0.0012		-0.0375	-0.0106	-0.0162
	(0.0395)	(0.0391)	(0.0359)	(0.0339)	(0.0354)		(0.0343)	(0.0281)	(0.0172)
Post × bin 1 (5-500m)						-0.0920**			
						(0.0359)			
Post × bin 2 (500-1,000m)						-0.0871***			
						(0.0281)			
Post × bin 3 (1,000-1,500m)						-0.0298			
						(0.0201)			
bin 1 (5-500m)						-0.0262			
						(0.0374)			
bin 2 (500-1,000m)						-0.0391			
						(0.0306)			
bin 3 (1,000-1,500m)						-0.0497**			
						(0.0209)			
House attributes	Yes	Yes	Yes						
Location attributes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Block-group FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County-quarter FE		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Incident-year FE			Yes	Yes	Yes	Yes	Yes	Yes	Yes
House attributes × year					Yes	Yes	Yes	Yes	Yes
House attributes × incident				Yes					
N	108,420	108,415	108,393	108,392	108,393	126,594	141,331	120,202	369,926
Adj. R^2	0.669	0.670	0.687	0.693	0.688	0.693	0.714	0.718	0.709

Note: This table shows the short-term impacts of incidents on housing prices with different specifications and incident types. Column (1) only includes housing attributes, neighborhood attributes, and block-group fixed effect; Columns (2), (3), and (5) incrementally include county-quarter fixed effect, incident-year fixed effect, and covariates that interact with the year fixed effect. Column (4) replaces the covariates that interact with the year fixed effect with those that interact with incidents. Column (6) classifies the treatment group into 5–500 meter, 500–1,000 meter, and 1,000–1,500 meter bins, and reports bin-level estimates. Columns (7)–(9) present results for only severe or above-ground, neither severe nor above-ground incidents, and all incidents, respectively. Errors are clustered at the incident level for all regressions. Covariates include all the characteristics in Table B3 except Sales Amount. Standard errors are in parentheses.
$***$: statistically significant at 1% level. $**$: statistically significant at 5% level. $*$: statistically significant at 10% level.

Table 4: Difference-in-differences Estimation Results: Long-term Effects

	Log of Transaction Price		
	(1)	(2)	(3)
Post × Treat	-0.0784***		
	(0.0287)		
Post	0.0323*		
	(0.0184)		
Treat	-0.0129	-0.0125	-0.0126
	(0.0302)	(0.0303)	(0.0302)
Treat × Period: 0 - 1,000 days		-0.0853***	
		(0.0297)	
Treat × Period: 1,000 - 2,000 days		-0.0919***	
		(0.0342)	
Treat × Period: 2,000 - 3,000 days		-0.0647**	
		(0.0316)	
Treat × Period: 0 - 3,000 days			-0.0830***
			(0.0295)
Treat × Period: 3,000 - 4,000 days		0.0164	0.0139
		(0.0328)	(0.0324)
Period: 0 - 1,000 days		0.0335*	
		(0.0182)	
Period: 1,000 - 2,000 days		0.0438	
		(0.0355)	
Period: 2,000 - 3,000 days		0.0272	
		(0.0337)	
Period: 0 - 3,000 days			0.0356*
			(0.0181)
Period: 3,000 - 4,000 days		0.0496	0.0636*
		(0.0479)	(0.0360)
Location attributes	Yes	Yes	Yes
Block-group FE	Yes	Yes	Yes
County-quarter FE	Yes	Yes	Yes
Incident-year FE	Yes	Yes	Yes
House attributes × year	Yes	Yes	Yes
N	191,251	191,251	191,251
Adj. R^2	0.674	0.674	0.674

Note: This table follows the model specification in Column (5) of Table 3 and shows the long-term impacts of incidents on housing prices, spanning 4,000 days before and after the incidents. Column (1) shows the overall impact on housing prices over the 4,000-day post-incident duration. Column (2) classifies the post-incident period into four intervals: 0–1,000 days, 1,000–2,000 days, 2,000–3,000 days, and 3,000–4,000 days, respectively, and provides estimates for each period. Column (3) groups the first three intervals (0–3,000 days) to examine the overall effect. Errors are clustered at the incident level for both regressions. Covariates include all the characteristics in Table B3 except Sales Amount. Standard errors are in parentheses.
$*\,*\,*$: statistically significant at 1% level.
$**$: statistically significant at 5% level.
$*$: statistically significant at 10% level.

Table 5: Robustness Check: Placebo Tests

	Log of Transaction Price			
Placebo tests	West placebo incidents	East placebo incidents	500 days before incidents	1000 days before incidents
	(1)	(2)	(3)	(4)
Post × Treat	-0.0048	-0.0031	-0.0285	0.0313
	(0.0250)	(0.0227)	(0.0297)	(0.0268)
Post	0.0745	0.0626**	0.0345*	0.0034
	(0.0495)	(0.0255)	(0.0192)	(0.0226)
Treat	0.0188	-0.0040	0.0311	0.0072
	(0.0162)	(0.0179)	(0.0458)	(0.0491)
Location attributes	Yes	Yes	Yes	Yes
Block-group FE	Yes	Yes	Yes	Yes
County-quarter FE	Yes	Yes	Yes	Yes
Incident-year FE	Yes	Yes	Yes	Yes
House attributes × year	Yes	Yes	Yes	Yes
N	73,729	58,934	29,501	28,611
Adj. R^2	0.705	0.691	0.686	0.693

Note: This table presents two primary placebo tests by adopting placebo incidents and replacing temporal ranges. Columns (1) and (2) report the estimated impacts from placebo incidents to the west and east of real incidents, respectively. Columns (3) and (4) display the results obtained by artificially shifting the incident occurrence date 500 days and 1,000 days ahead. Covariates include all the characteristics in Table B3 except Sales Amount. Standard errors are in parentheses.
$***$: statistically significant at 1% level.
$**$: statistically significant at 5% level.
$*$: statistically significant at 10% level.

Table 6: Robustness Check: Missing Samples

	Log of Transaction Price			
	No Controls	Imputing Zero	Group Mean Substitution	
	(1)	(2)	(3)	(4)
Post × Treat	-0.0902**	-0.0861**	-0.0805**	-0.0817**
	(0.0346)	(0.0354)	(0.0319)	(0.0318)
Post	0.0180	0.0150	0.0540**	0.0536**
	(0.0272)	(0.0271)	(0.0240)	(0.0241)
Treat	0.0128	0.0203	0.0187	0.0223
	(0.0414)	(0.0376)	(0.0362)	(0.0356)
House attributes		Yes		
Location attributes			Yes	Yes
Block-group FE	Yes	Yes	Yes	Yes
County-quarter FE	Yes	Yes	Yes	Yes
Incident-year FE	Yes	Yes	Yes	Yes
House attributes × year			Yes	Yes
Missing indicators		Yes		Yes
N	222,138	222,138	222,005	222,005
Adj. R^2	0.720	0.730	0.733	0.734

Note: This table addresses the issue of missing samples resulting from incomplete housing attributes. We implement several robustness checks to assess the sensitivity of missing data. Column (1) removes housing attributes from the regression. Column (2) reports results by coding missing house attributes as zero and further adding missing indicators for each housing attribute. In Column (3), we impute missing values using the average price within price quantiles in each block group or census tract, while Column (4) further incorporates missing indicators. Errors are clustered at the incident level for all regressions. Covariates include all the characteristics in Table B3 except Sales Amount. Standard errors are in parentheses.

$***$: statistically significant at 1% level.

$**$: statistically significant at 5% level.

$*$: statistically significant at 10% level.

Table 7: Robustness Check: Alternative Control Groups and Time Windows

	Log of Transaction Price					
	Alternative control group			Alternative time window		
	1000-2500m	2000-3500m	2500-4000m	+/-500days	+/-1000days	+/-1500days
	(1)	(2)	(3)	(4)	(5)	(6)
Post × Treat	-0.0825***	-0.0835**	-0.0872**	-0.1014***	-0.0717***	-0.0984***
	(0.0240)	(0.0333)	(0.0354)	(0.0318)	(0.0211)	(0.0240)
Post	0.0206	0.0585***	0.0399	0.0360	0.0275	0.0420**
	(0.0266)	(0.0215)	(0.0247)	(0.0249)	(0.0210)	(0.0206)
Treat	0.0052	0.0462	0.0789	0.0110	-0.0003	0.0089
	(0.0219)	(0.0463)	(0.0519)	(0.0632)	(0.0436)	(0.0364)
Location attributes	Yes	Yes	Yes	Yes	Yes	Yes
Block-group FE	Yes	Yes	Yes	Yes	Yes	Yes
County-quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Incident-year FE	Yes	Yes	Yes	Yes	Yes	Yes
House attributes × year	Yes	Yes	Yes	Yes	Yes	Yes
N	88,511	105,963	98,758	27,750	55,014	81,815
Adj. R^2	0.698	0.691	0.699	0.690	0.694	0.691

Note: This table presents estimations of robustness checks that incorporate different control groups and alternative time windows in our DID model. Columns (1)–(3) present the results of models with distance bandwidths ranging from 1,000-–2,500 meters, 2,000-–3,500 meters, and 2,500-–4,000 meters radii, respectively. Columns (4)–(6) narrow the time windows to 500 days, 1,000 days, and 1,500 days before and after incidents. Errors are clustered at the incident level for all regressions. Covariates include all the characteristics in Table B3 except Sales Amount. Standard errors are in parentheses.

$***$: statistically significant at 1% level.

$**$: statistically significant at 5% level.

$*$: statistically significant at 10% level.

Table 8: Mechanism - Transaction Volume

	No. of Transaction Volume					
	30 days	40 days	50 days	60 days	70 days	80 days
	(1)	(2)	(3)	(4)	(5)	(6)
Post × Treat	-0.4013*	-0.0500**	-0.4390*	-0.5119**	-0.4294	-0.2543
	(0.2042)	(0.2274)	(0.2385)	(0.2572)	(0.3284)	(0.3640)
Post	0.2960	0.3518*	0.2804	0.2738	0.1176	-0.0809
	(0.1811)	(0.2105)	(0.2401)	(0.2741)	(0.3685)	(0.3972)
Treat	-1.3026***	-1.6543***	-2.1158***	-2.5714***	-3.1411***	-3.7109***
	(0.1866)	(0.2267)	(0.2612)	(0.2864)	(0.3733)	(0.4038)
N	608	648	656	672	680	692
Adj. R^2	0.1171	0.1205	0.1286	0.1380	0.1260	0.1315

Note: This table depicts the impacts of incidents on transaction volumes across different time windows. Columns (1)–(6) outline the various durations of time windows ranging from 30 to 80 days before and after incidents. We employ the same treatment and control groups across all models. Errors are clustered at the incident level for all regressions. Standard errors are in parentheses.

$***$: statistically significant at 1% level.

$**$: statistically significant at 5% level.

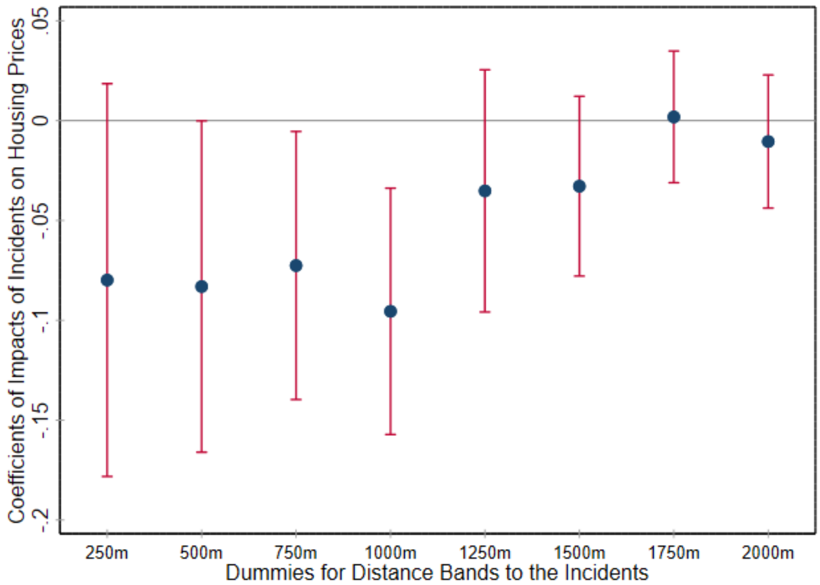
$*$: statistically significant at 10% level.

Table 9: Mechanism: Pipeline Installation

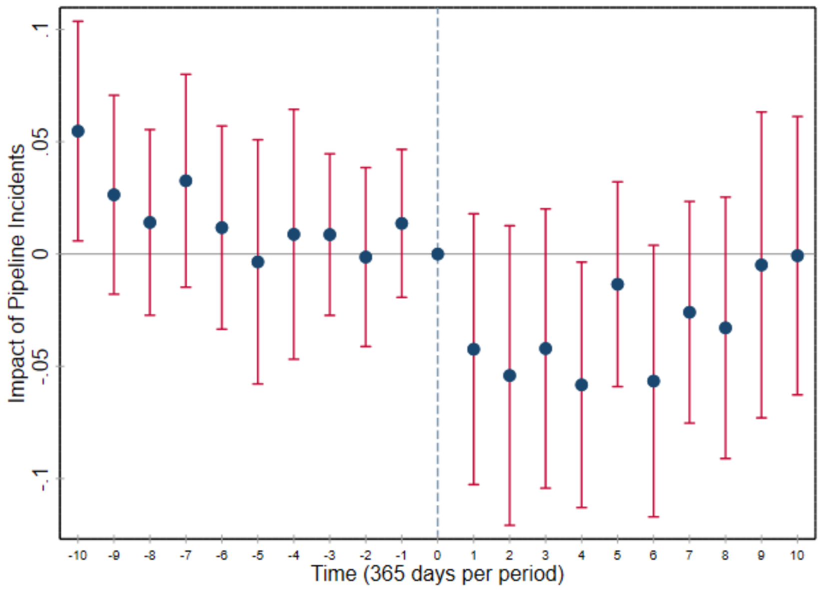
	Log of Transaction Price		
	Installation Year	One year before Installation	Two years before Installation
	(1)	(2)	(3)
Post × Treat	-0.0774	-0.0742	-0.0145
	(0.1177)	(0.1009)	(0.0763)
Location attributes	Yes	Yes	Yes
Block-group FE	Yes	Yes	Yes
County-quarter FE	Yes	Yes	Yes
Incident-year FE	Yes	Yes	Yes
House attributes × year	Yes	Yes	Yes
N	9,373	9,373	9,373
Adj. R^2	0.729	0.729	0.729

Note: This table presents the impacts of the potential pipeline construction announcement and pipeline installation on housing prices. Column (1) reports the results of post-installation effects on housing prices, while Columns (2) and (3) show the estimation of anticipated impacts one and two years before pipeline installation on housing prices, respectively. Errors are clustered at the incident level for all regressions. Covariates include all the characteristics in Table B3 except Sales Amount. Standard errors are in parentheses.
$***$: statistically significant at 1% level.
$**$: statistically significant at 5% level.
$*$: statistically significant at 10% level.

Appendix A: Additional Figures



(a) Response to high-profile incidents through 250-meter-wide spatial bins.



(b) Fine temporal bins for the long-term (one year per period).

Figure A1: Regressions with fine spatial/temporal bins for high-profile incidents in the long term.

Note: Subfigure (a) employs eight 250-meter-wide distance bins to examine the impact of high-profile incidents on housing prices. The reference group is the distance bin covering 2,000–2,500 meters. Subfigure (b) shows the estimations for fine temporal bins 4,000 days before and after the incident. Each point represents 365 days (1 year). Point 0 is the reference group, including one year before the incidents. This figure covers 11 years before and 10 years after the incidents. Confidence intervals are calculated at a 95% level.

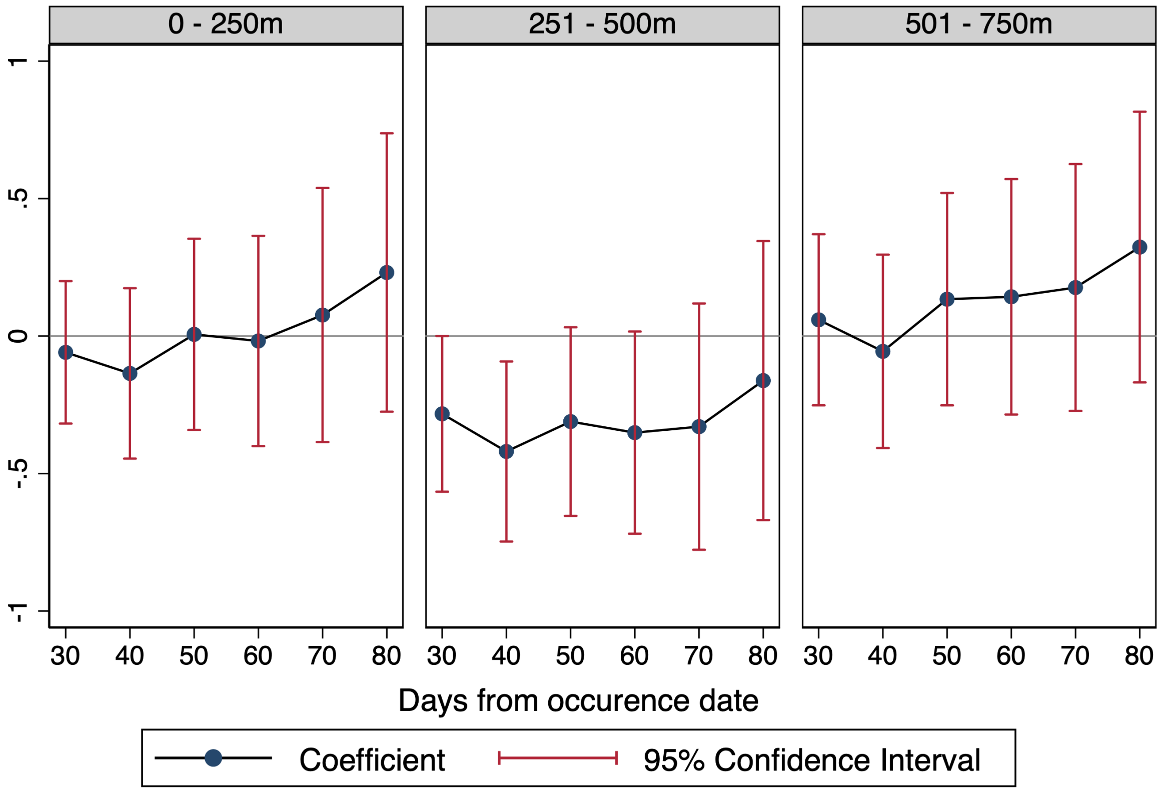
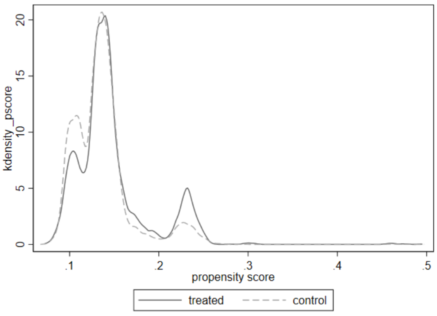
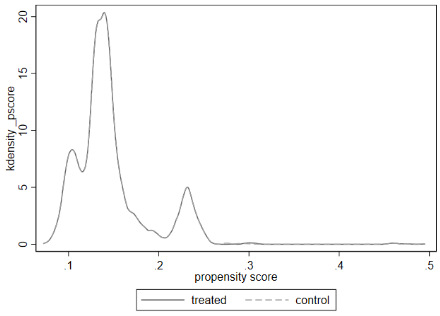


Figure A2: Difference-in-differences regression: Temporal dynamics in the volume impacts of high-profile incidents by different distance bins.

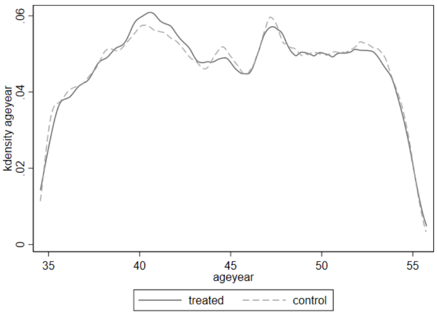
Note: This figure illustrates the evolving impact of incidents on the number of transaction volumes across three 250-meter-wide distance bins, spanning various time durations. The reference group is the distance bin covering 750–1,000 meters. Confidence intervals are calculated at a 95% level.



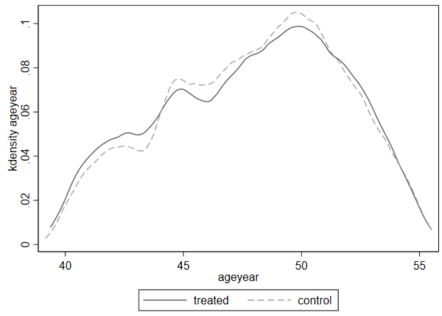
(a) Propensity score before matching



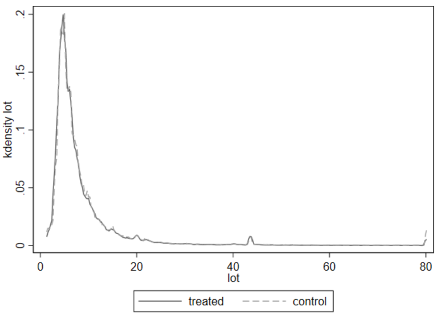
(b) Propensity score after matching



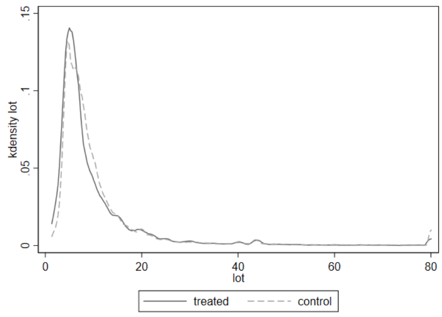
(c) Age distribution before matching



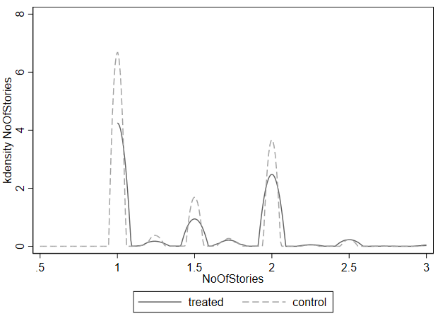
(d) Age distribution after matching



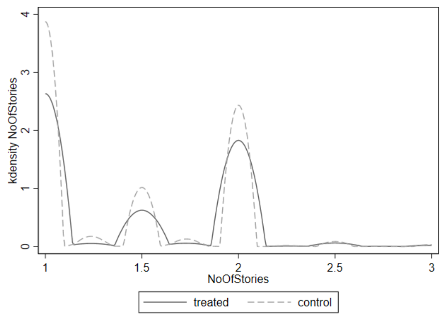
(e) Lot size distribution before matching



(f) Lot size distribution after matching



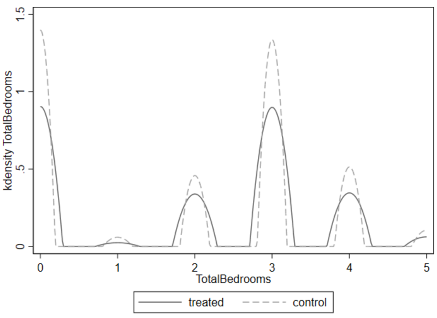
(g) No. of stories distribution before matching



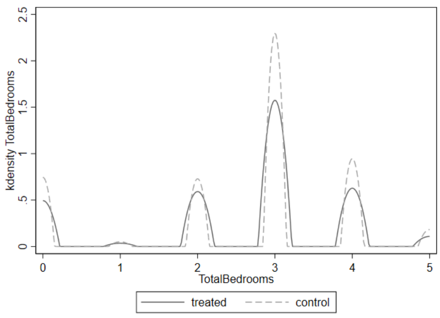
(h) No. of stories distribution after matching

Figure A3: Comparisons of house attributes before and after propensity score matching (part 1)

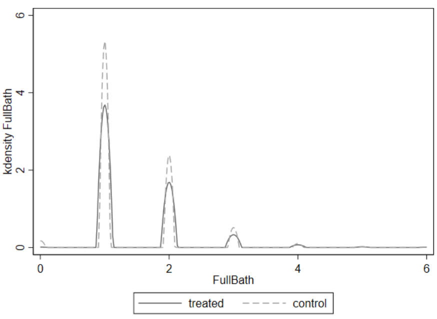
Note: This figure presents comparisons of covariate distributions before and after propensity score matching. The four left-hand side subfigures illustrate the distribution of propensity scores, age, lot size, and the number of stories between treatment and control groups before matching. The four right-hand side subfigures depict the distributions of the same covariates after matching.



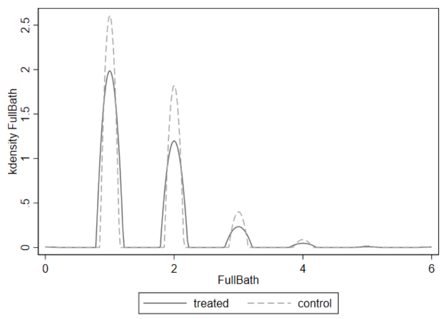
(a) No. of bedrooms distribution before matching



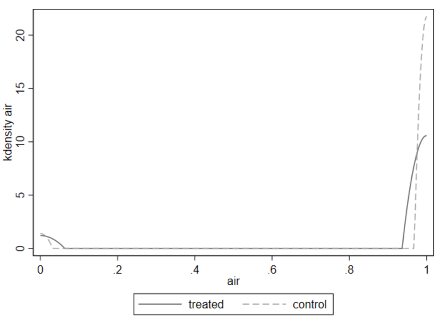
(b) No. of bedrooms distribution after matching



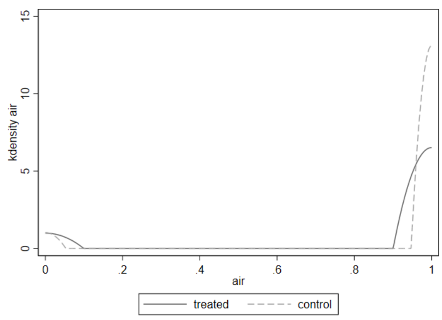
(c) No. of full baths distribution before matching



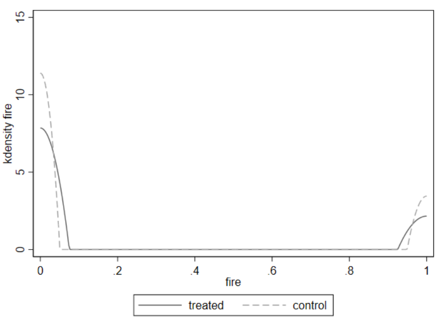
(d) No. of full baths distribution after matching



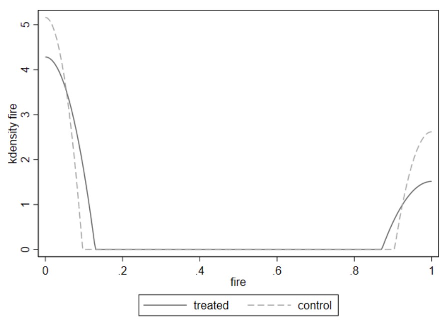
(e) Air conditioner distribution before matching



(f) Air conditioner distribution after matching



(g) Fireplace distribution before matching



(h) Fireplace distribution after matching

Figure A4: Comparisons of house attributes before and after propensity score matching (part 2)

Note: This figure presents comparisons of covariate distributions before and after propensity score matching. The four left-hand side subfigures illustrate the distribution of the number of bedrooms, the number of full baths, the presence of air conditioners and fireplaces between treatment and control groups before matching. The four right-hand side subfigures depict the distributions of the same covariates after matching.

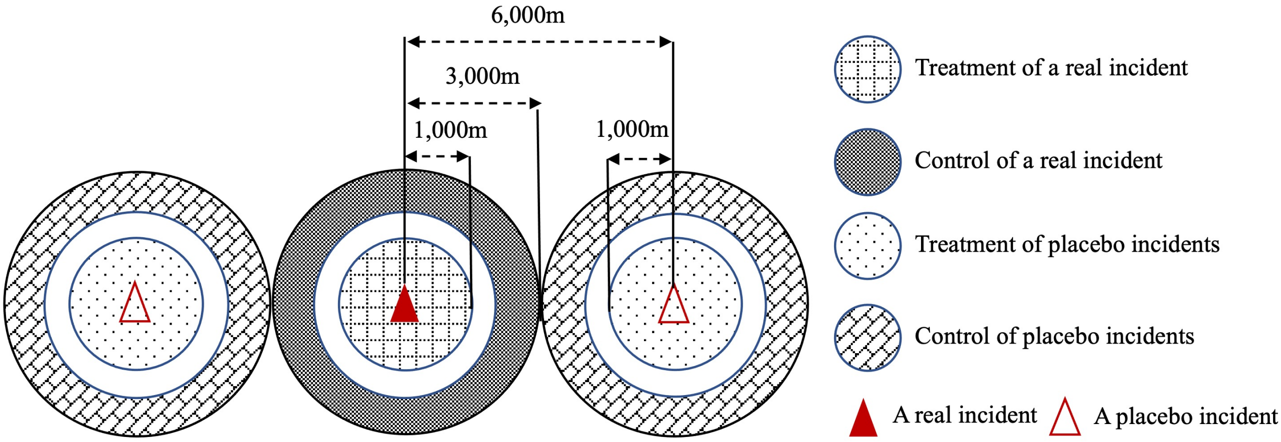


Figure A5: Illustration of construction about placebo treatment and control groups.

Note: The graph shows the construction of placebo incidents in our DID analysis. The red solid triangle denotes a real incident and the hollow triangles to the west and east are the corresponding placebo incidents. The dotted grid represents the treatment group of a real incident, and the gray one is the associated control group. We construct two placebo incidents west and east of a real incident. The loosely dotted and brick-filled bands are the treatment and control groups for a placebo incident, respectively. We remove the 500-meter-wide region with potential treatment spillover for both real and placebo incidents. The treated band spans 1,000 meters wide and the control band is 1,500 meters wide.

Appendix B: Additional Tables

Table B1: Incident Attribute Comparison between 976 Full Incidents and 426 Incident Samples

	976 incidents		426 incidents		Standardized Diff.
	Mean	S.D.	Mean	S.D.	
	(1)	(2)	(3)	(4)	(5)
Fatality	0.06	0.24	0.06	0.24	0.01
Explosion	0.24	0.43	0.26	0.44	-0.05
Ignition	0.61	0.49	0.58	0.49	0.07
Severe	0.63	0.48	0.60	0.49	0.06
Above-ground	0.38	0.48	0.33	0.47	0.10*

Note: This table presents the comparisons of key incident attributes of 976 gas distribution pipeline incidents in urban places to the 426 incidents we use in our DID regressions.
*: Incident attributes imbalance between 976 and 426 incidents.

Table B2: Summary Statistics - House Attributes for 310-Incident DID Sample

Variable	N	Mean	S.D.	Min.	Max.
Sales Price ($1000)	369,926	201.80	211.23	1.50	1262.78
House Age (year)	369,926	48.10	3.76	39.13	55.64
Lot Size ($1,000ft^2$)	369,926	9.89	9.89	1.31	80.15
No. of Stories	369,926	1.41	0.49	1	3
Total Bedrooms	369,926	2.84	1.15	0	5
Full Baths	369,926	1.65	0.77	0	6
Air Conditioner	369,926	0.89	0.32	0	1
Fireplaces	369,926	0.35	0.48	0	1
Dist. to hospital (km)	369,926	3.86	3.17	0.07	64.17
Dist. to School (km)	369,926	0.79	0.93	0.01	53.23
Dist. to University (km)	369,926	3.79	3.46	0.03	68.85

Note: This table presents the descriptive statistics of housing samples linked to 310 incidents in the DID analysis.

Table B3: Summary Statistics - House Attributes for 95 High-profile Incidents

Variable	N	Mean	S.D.	Min.	Max.
Panel A: w/o Imputation					
Sales Price ($1000)	108,393	199.22	207.01	1.50	1,262.78
House Age (year)	108,393	47.96	3.72	39.16	55.51
Lot Size (1,000 ft^2)	108,393	10.97	11.86	1.31	80.15
No. of Stories	108,393	1.43	0.47	1	3
Total Bedrooms	108,393	2.64	1.32	0	5
Full Baths	108,393	1.59	0.76	0	6
Air Conditioner	108,393	0.92	0.27	0	1
Fireplaces	108,393	0.33	0.47	0	1
Dist. to hospital (km)	108,393	4.32	3.96	0.07	64.17
Dist. to School (km)	108,393	0.80	1.51	0.03	53.23
Dist. to University (km)	108,393	3.78	3.62	0.03	68.85
Panel B: w/ Imputation					
Lot Size (1,000 ft^2)	222,005	9.40	10.41	1.31	80.15
Missing Indicator: Lot Size	222,005	0.01	0.10	0	1
No. of Stories	222,005	1.41	0.47	1	3
Missing Indicator: No. of Stories	222,005	0.03	0.17	0	1
Total Bedrooms	222,005	2.08	1.56	0	5
Missing Indicator: Total Bedrooms	222,005	0.03	0.18	0	1
Full Baths	222,005	1.45	0.70	0	6
Missing Indicator: Full Baths	222,005	0.00	0.04	0	1
Air Conditioner	222,005	0.96	0.21	0	1
Missing Indicator: Air Conditioner	222,005	0.46	0.50	0	1

Note: Panels A and B present the descriptive statistics of housing samples linked to 95 high-profile incidents in price analysis before and after group mean substitution imputation, respectively.

Table B4: Covariates Balance Check - Only Severe or Above-ground, and Neither Severe nor Above-ground Incidents

Panel A: only severe or above-ground samples	Pre-Control	Pre-Treatment	Post-Control	Post-Treatment	Pre-Diff	Post-Diff
	(1)	(2)	(3)	(4)	(5)	(6)
Sales Price ($1000)	193.46(218.21)	191.94(228.16)	192.69(220.07)	192.90(227.73)	0.01	-0.01
House Age (year)	46.12(3.81)	46.20(3.79)	50.15(2.44)	50.27(2.44)	-0.02	-0.05
Lot Size $(1,000ft^2)$	9.39(8.95)	8.83(7.90)	9.56(9.06)	8.94(8.50)	0.06	0.07
No. of Stories	1.40(0.48)	1.36(0.46)	1.39(0.47)	1.40(0.49)	0.07	-0.01
Total Bedrooms	2.79(1.12)	2.80(1.08)	2.73(1.21)	2.80(1.10)	-0.01	-0.05
Full Baths	1.54(0.73)	1.56(0.72)	1.53(0.74)	1.55(0.71)	-0.02	-0.02
Air Conditioner	0.83(0.36)	0.87(0.33)	0.81(0.38)	0.83(0.37)	-0.10*	-0.02
Fireplaces	0.33(0.47)	0.37(0.48)	0.33(0.47)	0.36(0.48)	-0.08	-0.05
Dist. to hospital (km)	3.73(2.78)	3.90(3.15)	3.82(2.96)	4.04(3.43)	-0.05	-0.06
Dist. to School (km)	0.76(0.46)	0.76(0.66)	0.75(0.48)	0.74(0.58)	-0.01	0.010*
Dist. to University (km)	3.44(3.18)	3.93(5.01)	3.70(3.61)	4.19(5.55)	-0.11*	-0.10*
N	70,150	13,285	48,669	9,227		
Panel B: neither severe nor above-ground samples						
Sales Price ($1000)	210.01(188.82)	229.46(194.13)	217.00(222.68)	218.99(228.55)	-0.1*	-0.008
House Age (year)	47.35(3.74)	47.77(3.25)	50.47(2.45)	51.02(2.20)	-0.12*	-0.23*
Lot Size $(1,000ft^2)$	9.59(9.10)	8.62(8.01)	9.69(9.03)	9.18(8.31)	0.11*	0.05
No. of Stories	1.40(0.50)	1.48(0.51)	1.36(0.50)	1.39(0.50)	-0.16*	0.06
Total Bedrooms	3.07(0.94)	3.11(0.91)	3.14(0.87)	3.13(0.91)	-0.04	0.01
Full Baths	1.83(0.79)	1.91(0.75)	1.80(0.81)	1.79(0.81)	-0.09	0.01
Air Conditioner	0.93(0.25)	0.92(0.26)	0.92(0.27)	0.86(0.33)	0.02	0.16*
Fireplaces	0.38(0.48)	0.31(0.46)	0.40(0.49)	0.35(0.47)	0.16*	0.09
Dist. to hospital (km)	3.64(2.60)	3.98(2.87)	3.18(2.20)	3.55(2.54)	-0.12*	-0.15*
Dist. to School (km)	0.83(0.59)	0.87(0.62)	0.73(0.47)	0.76(0.58)	-0.06	-0.04
Dist. to University (km)	4.04(2.77)	4.29(3.14)	3.82(3.06)	3.76(3.34)	-0.08	0.02
N	64,148	10,438	39,753	5,863		

Note: This table presents the descriptive statistics of housing samples linked to 110 only severe or above-ground, and 105 neither severe nor above-ground incidents in price analysis, respectively. Columns (1) to (4) report the mean and standard deviation (in parentheses) for pre-control, pre-treatment, post-control, and post-treatment groups, respectively. Columns (5) and (6) show the standardized difference between treatment and control groups in pre- and post-incident periods.
*: The standardized difference between control and treatment groups is greater than .1.

Table B5: Difference-in-differences: Full Results for 95 High-profile Incidents

	Log of Transaction Price	
	Coefficients	Standard errors
	(1)	(2)
Post $\times$ Treat	-0.0875***	(0.0306)
Post	0.0137	(0.0207)
Treat	-0.0008	(0.0359)
House Age (year)	-1.1589***	(0.2744)
House Age^2	0.0062***	(0.0023)
Lot Size $(1,000 ft^2)$	0.0053***	(0.0007)
No. of Stories	0.1447***	(0.0191)
Total Bedrooms	0.0510***	(0.0083)
Full Baths	0.1508***	(0.0092)
Air Conditioner	0.2965***	(0.0446)
Fireplaces	0.1179***	(0.0256)
Dist. to hospital (km)	-0.0001	(0.0180)
Dist. to School (km)	-0.0010	(0.0170)
Dist. to University (km)	0.0146	(0.0188)
House attributes	Yes	
Location attributes	Yes	
Block-group FE	Yes	
County-quarter FE	Yes	
Incident-year FE	Yes	
N	108,393	
Adj. R^2	0.687	

Note: This table presents the full results of our main DID estimates (Table 3 Column (3)) for high-profile incidents in the short term. Columns (1) and (2) show the coefficients and the standard errors accordingly. Errors are clustered at the incident level for all regressions. Covariates include all the characteristics in Table B3 except Sales Amount. Standard errors are in parentheses.
$***$: statistically significant at 1% level.
$**$: statistically significant at 5% level.
$*$: statistically significant at 10% level.

Table B6: Robustness Check: Alternative Treatment Groups for Non-high-profile Incidents

	Log of Transaction Price	
	Only severe or above-ground	Neither severe nor above-ground
	(1)	(2)
Post × Treat	0.0209	0.0107
	(0.0179)	(0.0250)
Post	-0.0127	0.0242
	(0.0163)	(0.0174)
Treat	0.0219	-0.0055
	(0.0281)	(0.0217)
Location attributes	Yes	Yes
Block-group FE	Yes	Yes
County-quarter FE	Yes	Yes
Incident-year FE	Yes	Yes
House attributes × year	Yes	Yes
N	156,491	134,571
Adj. R^2	0.715	0.720

Note: This table presents estimations of the robustness check that replaces the treatment and control groups with a 0–750 and 1,000–3,000 meters radius for non-high-profile incidents. Columns (1) and (2) provide results for only severe or above-ground, and neither severe nor above-ground incidents, respectively. Errors are clustered at the incident level for all regressions. Covariates include all the characteristics in Table B3 except Sales Amount. Standard errors are in parentheses.
$***$: statistically significant at 1% level.
$**$: statistically significant at 5% level.
$*$: statistically significant at 10% level.

Table B7: Robustness Check: Propensity Score Matching

	Log of Transaction Price	
	(1)	(2)
Post × Treat	-0.0718**	-0.0692**
	(0.0319)	(0.0306)
Post	-0.0119	0.0081
	(0.0511)	(0.0480)
Treat	-0.0564	-0.0519
	(0.0514)	(0.0505)
House attributes	Yes	Yes
Location attributes	Yes	Yes
Block-group FE	Yes	Yes
County-quarter FE	Yes	Yes
Incident-year FE	Yes	Yes
House attributes × year		Yes
N	29,437	29,437
Adj. R^2	0.700	0.701

Note: This table presents estimation results of the robustness check by incorporating the propensity score matching method to improve covariate balance between the treatment and control groups. The model specification of Column (1) is the same as Column (3) in Table 3, while Column (2) follows the specification of Column (5) in Table 3. Errors are clustered at the incident level for all regressions. Covariates include all the characteristics in Table B3 except Sales Amount. Standard errors are in parentheses.

$***$: statistically significant at 1% level.

$**$: statistically significant at 5% level.

$*$: statistically significant at 10% level.

Table B8: Robustness Check: Dropping Near-incident Samples

	Log of Transaction Price			
	Exclude first 10m	Exclude first 40m	Exclude first 70m	Exclude first 100m
	(1)	(2)	(3)	(4)
Post $\times$ Treat	-0.0850***	-0.0836***	-0.0829***	-0.0827***
	(0.0296)	(0.0297)	(0.0296)	(0.0294)
Post	0.0328	0.0331	0.0330	0.0328
	(0.0202)	(0.0202)	(0.0202)	(0.0202)
Treat	0.0012	0.0005	-0.0002	-0.0006
	(0.0354)	(0.0354)	(0.0355)	(0.0355)
Location attributes	Yes	Yes	Yes	Yes
Block-group FE	Yes	Yes	Yes	Yes
County-quarter FE	Yes	Yes	Yes	Yes
Incident-year FE	Yes	Yes	Yes	Yes
House attributes $\times$ year	Yes	Yes	Yes	Yes
N	108,388	108,343	108,264	108,175
Adj. R^2	0.688	0.688	0.688	0.688

Note: This table presents estimations of the robustness check that removes houses potentially damaged by incidents. Columns (1)–(4) provide results when we remove houses within different radii——10, 40, 70, and 100 meters away from incidents. Errors are clustered at the incident level for all regressions. Covariates include all the characteristics in Table B3 except Sales Amount. Standard errors are in parentheses.

$***$: statistically significant at 1% level.

$**$: statistically significant at 5% level.

$*$: statistically significant at 10% level.

Table B9: Robustness Check: Examining Possible Lock-in Period Impacts

	Log of Transaction Price					
	Exclude first 30days	Exclude first 60days	Exclude first 90days	Exclude first 120days	Exclude first 150days	Exclude first 180days
	(1)	(2)	(3)	(4)	(5)	(6)
Post × Treat	-0.0853***	-0.0894***	-0.0892***	-0.0885***	-0.0869***	-0.0865***
	(0.0297)	(0.0302)	(0.0304)	(0.0310)	(0.0312)	(0.0313)
Post	0.0193	0.0166	0.0090	-0.0041	-0.0038	-0.0114
	(0.0244)	(0.0319)	(0.0405)	(0.0432)	(0.0497)	(0.0592)
Treat	-0.0017	-0.0024	-0.0009	-0.0003	0.0010	0.0002
	(0.0354)	(0.0349)	(0.0346)	(0.0344)	(0.0340)	(0.0339)
Location attributes	Yes	Yes	Yes	Yes	Yes	Yes
Block-group FE	Yes	Yes	Yes	Yes	Yes	Yes
County-quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Incident-year FE	Yes	Yes	Yes	Yes	Yes	Yes
House attributes × year	Yes	Yes	Yes	Yes	Yes	Yes
N	107,556	106,622	105,753	104,894	104,042	103,206
Adj. R^2	0.688	0.687	0.687	0.687	0.687	0.688

Note: This table presents estimations of the robustness check that excludes transactions during lock-in periods. Columns (1)–(6) provide results of models where we remove transactions from the first to sixth month in the post-incident period. Errors are clustered at the incident level for all regressions. Covariates include all the characteristics in Table B3 except Sales Amount. Standard errors are in parentheses.

$***$: statistically significant at 1% level.

$**$: statistically significant at 5% level.

$*$: statistically significant at 10% level.

Table B10: Mechanism: Different Treatment Group Bandwidth

	Log of Transaction Volume				
	600m	700m	800m	900m	500-1000m
	(1)	(2)	(3)	(4)	(5)
Post × Treat	-0.3728	-0.1016	0.1468	0.1242	0.2485
	(0.4221)	(0.4151)	(0.3932)	(0.4034)	(0.3440)
Post	0.2090	0.0734	-0.0508	-0.0395	0.0112
	(0.4219)	(0.4024)	(0.3639)	(0.3420)	(0.3093)
Treat	-7.6836***	-6.2937***	-4.6779***	-2.8022***	-2.4067***
	(0.2042)	(0-6.2937)	(0.5047)	(0.4266)	(0.3521)
N	708	708	708	708	708
Adj. R^2	0.225	0.167	0.101	0.039	0.039

Note: This table presents the estimation results of how different treatment groups affect the impact of incidents on house transaction volumes. Columns (1)–(4) gradually expand the treatment group by 100 meters from the baseline group (i.e., 500 meters wide). Column (5) replaces the treatment group with the 500–1,000 meters distance bin and the control group with the 1,000–1,500 meters distance bin. The time range spans from 60 days before and after incidents for all regressions. Errors are clustered at the incident level for all regressions. Standard errors are in parentheses.
$***$: statistically significant at 1% level.
$**$: statistically significant at 5% level.
$*$: statistically significant at 10% level.

Appendix C: Data Cleaning Process

The initial data-cleaning process

To increase the research transparency, we compare our data-cleaning procedures with Nolte et al. (2024) . Specifically, Nolte et al. (2024) raise concerns and proposed solutions in five areas:

- Identifying transaction prices reflecting fair market value.
- Identifying specific types of properties (e.g., single-family homes or vacant lands).
- Linking transactions to time-variant property characteristics.
- Dealing with missing or mismeasured data for standard housing attributes.
- Geolocating transacted properties (land and buildings).

In processing the raw data from ZTRAX, we first ensure that transaction prices reflect current fair market values. We use the transaction price data (instead of assessment price data), drop transactions with prices below $1,500, transactions coded as non-arm's length, and pre-sales that happened before the building year. Nolte et al. (2024) suggest additional filters, such as 165 document types, that are challenging to implement in our context. We simplify the procedure by using winsorization at the 1^{th} and 99^{th} percentiles. The remaining non-market transactions should be biasing the estimated incident impacts toward zero since these transactions are less responsive to market forces.

To ensure a relatively clean sample, we follow the common practice in the hedonic literature only to consider single-family houses. Since we focus on urban incidents, we only keep transactions with building code RR101 for urban single-family houses. We exclude building codes (RR000 for general residences, RR102 for rural residences, and RR999 for inferred single-family houses) that do not or may not represent urban single-family houses.

As to concerns about the time-variant and missing housing attributes, we compile the most recent housing attributes information recorded before transaction. Following Nolte et al. (2024) suggestions, we conduct robustness checks based on transactions within various short time windows (see Table 7). Additionally, we employ a series of different methods to address the missing data issues in housing attributes, including a specification without any housing attributes (see Table 6), and we find that our main results remain robust.

Finally, Nolte et al. (2024) highlight geolocation inaccuracies in ZTRAX data, especially for earlier versions. We follow their advice and perform all analyses using the most recent version of ZTRAX data (downloaded in May 2021) that is available to us.

One-to-one match for price analysis

After the initial data cleaning process, the dataset comprised 244 high-profile incidents connected to 2,962,259 observations. Employing complete cases (i.e. houses with non-missing attributes) using LPR left 126 incidents with 665,688 observations. Based on the LPR results, we assign houses in a radius of 1,000 meters as the treatment group, and those between 1,500 and 3,000 meters are in the control group. We remove houses within 1,000 to 1,500 meters away from incidents for cleaner estimation. The short-term analysis limits the time window to 2,000 days before and after incidents. This shrinks the number of incidents to 111 and 117,984 samples.

To ensure clean identification, we perform a one-to-one match for each transaction, assigning it to treated-before, treated-after, control-before, or control-after groups for all potential incidents. This matching is based on the spatiotemporal range of property value impacts determined by the results the scale analyses. We carry out the following steps to correctly link transactions and incidents prior to the short-term DID regression analyses.

- Transactions assigned to the treated-after groups of multiple incidents were completely removed.
- Transactions linked to the treated-after group of only one incident were retained, while observations linked as control groups for other incidents were discarded.
- If the above two steps still allow for one transaction to be linked to the before period or control group for more than one incident, we randomly assign the transaction to one of the incidents.

The above steps result in the inclusion of 108,393 observations associated with 95 high-profile incidents in the short-term DID analysis.

We apply the same steps to other types of incidents (i.e., only severe or above-ground, and neither severe nor above-ground incidents), which leaves us 145 only severe or above-ground incidents (linked to 852,777 observations) and 155 neither severe nor above-ground incidents (associated with 774,335 observations) for scale analyses. Since these scale analyses do not discover any price effect for non-high-profile incidents, we follow the DID design of the high-profile incidents–houses in the 1,000-meter radius of are treated in the treatment group, and those located between 1,500- and 3,000-meter away are in the control group. For the short-term analysis (i.e., 2,000 days before and after incidents), we have 127 only severe or above-ground (148,320 samples) and 127 neither severe nor above-ground incidents (129,251 samples), respectively. After applying the same 3-step matching process, we have 110 only severe or above-ground incidents (141,331 observations) and 105 neither severe nor above-ground incidents (120,202 observations) for DID analysis, separately

In total, there are 310 incidents linked with 369,926 unique transactions for the DID analysis on short-term impacts. On average, an incident is matched with 1,193 unique transactions. As shown in Table B2 in the appendix, a linked house is 48 years old and has a lot size of 9,890 square feet, 1.4 stories, 2.8 bedrooms, 0.9 air conditioners, 0.35 fireplaces, and 1.65 full baths, on average. The average distance to the hospital is 3.86 km, while the distances to the nearest school and university are 0.79 km and 3.79 km, respectively. The transaction prices are deflated to 2020 dollars using the Federal Housing Finance Agency's state-quarter house price index. Table B3 Panel A reports the summary statistics for house transactions matched to the 95 high-profile incidents, which are similar to those for 310 incidents.

For the long-term analysis, we only focus on the high-profile incidents. Following the same procedure but extending the temporal range of impacts, we retain 125 high-profile incidents linked with 191,251 transactions.

Transaction volume data collection

In addition to price impacts, we also investigate the impacts of incidents on the volume of property transactions. Different from price analysis, we allow transactions to be linked to multiple incidents for the purpose of accuracy in transaction volume counting. To this end, we aggregate the number of transactions within a 3,000-meter radius and 1,000 days before and after 95 high-profile incidents. Table 2 presents the descriptive statistics for transaction volumes pre- and post-incidents across six time windows (i.e. 30, 40, 50, 60, 70, and 80 days). Within the 70 days before and after incidents, the maximum number of transactions during the pre-incident period is generally higher than that in the post-incident period, on average.