Pipeline Incidents and Property Values: A Nationwide Hedonic Analysis

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Abstract: Economic impacts of energy infrastructure incidents have been a focus of attention, especially since the rapid pipeline expansion due to the domestic shale oil and gas boom. While previous studies focus on individual pipeline incidents, we provide the first nationwide assessment of pipeline incidents' impacts on housing prices using data from 864 gas distribution pipeline incidents and 17 million property transactions from 2010 to 2020. A difference-in-differences analysis finds that a pipeline incident decreases housing prices by 4%–6% on average. We explore the heterogeneous impacts of incidents with different characteristics. These heterogeneous impacts can potentially explain conflicting results from previous studies.

Key words: Pipeline, Hazardous Materials, Housing Values, Hedonics, Difference-in-differences

JEL Codes: Q51; Q53; Q35

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1 Introduction

Pipelines are critical infrastructure for transporting various types of hazardous materials (HAZMAT), including petrochemicals such as natural gas, crude oil, and other petroleum products as well as carbon dioxide (Masters, 2022). Since 2004, the unprecedented development of shale oil and shale gas in the United States has created enormous demand for oil and gas transportation from the U.S. Heartland to the coastal regions. An estimated three million miles of pipeline deliver trillions of cubic feet of natural gas and hundreds of billions of tons of liquid petroleum products every year (Fan, 2016). This surge in pipeline transportation heightens concerns about pipeline safety and the socioeconomic costs associated with pipeline incidents. In the past 20 years, the United States has witnessed 12,308 pipeline incidents, with an average of 650 incidents per year, resulting in 306 casualties and more than $6 billion in property damage (PHMSA, 2021b). Recent high-profile pipeline explosions remind us again that pipeline incidents cost lives and create grave environmental damage (Ly, 2019; Weikel, 2011; Vigdor and Delkic, 2021). These concerns motivate the comparison of pipelines with other modes of energy transportation, such as rail (Covert and Kellogg, 2017; Tang et al., 2020).

Researchers have long been interested in using property-based hedonic analysis to estimate the indirect costs associated with pipeline transportation of HAZMAT. However, most existing literature on pipeline incidents is limited to the impacts of individual incidents (Wilde, Loos, and Williamson, 2012; Herrnstadt and Sweeney, 2017; McElveen, Brown, and Gibbons, 2017) or pipeline construction announcements (Hilterbrand Jr, 2019; Boslett and Hill, 2019; Fruits, 2008) on nearby residential property values. The spatiotemporal coverage of existing studies is restricted, leading to mixed and inconclusive results. To better inform a rigorous cost-benefit analysis of HAZMAT transportation via pipelines, studies with broader coverage are needed to increase external validity.

In this article, we provide the first nationwide assessment of pipeline incidents' impacts on housing prices based on 864 gas distribution pipeline incidents from 2010 to 2020.[1] We focus on the incidents of gas distribution pipelines because a large portion of significant pipeline incidents occurred on gas distribution lines. Also, most gas distribution lines stretch through densely populated residential areas, making them more likely to impact housing prices. The purpose of this study is two-fold. First, we estimate the average effects of 864 incidents on housing prices. Second, we explore the heterogeneity in the impacts of gas pipeline incidents, which helps explain why previous single-incident studies produce inconclusive results.

We collect data on the 864 gas distribution pipeline incidents in urban areas from PHMSA and

[1] Our dataset contains incidents associated with four types of pipelines: hazardous pipelines; gas-gathering pipelines; gas transmission lines; and, gas distribution lines. Included incidents are associated with either federally regulated or state-regulated natural gas distribution pipelines.

merge it with the Zillow Transaction and Assessment (ZTRAX) Dataset.[2] The resulting dataset covers the 46 contiguous U.S. states (the data excludes Maine and Vermont) between 2010 and 2020. More than one-third of included incidents involve fatalities, injury, explosion, evacuation, or more than $50,000 in direct damage (in 1984 dollars). In addition, the incident dataset contains a rich set of incident characteristics including the type of HAZMAT, the degree of damage, the number of residents evacuated, and others, allowing us to estimate the heterogeneous effects of pipeline incidents.

We adopt a difference-in-differences (DID) framework to formally estimate the impacts of pipeline incidents on housing prices using different subsets of the incidents while controlling for a rich set of housing features and fixed effects. To construct the treatment and control groups, we apply a data-driven approach, binscatter regressions, to identify the spatial and temporal extents of the impacts of a pipeline incident. Results of binscatter regressions suggest significant post-incident price divergence between houses within 1,000 meters and houses located further away. We define houses within 1,000 meters of incident sites consist of the treatment group while houses between 1,000 meters and 2,000 meters whose values are not directly affected by incidents comprise the control group. In terms of the temporal extent of incident effect, the price divergence persists about 500 days after the incident.

Our main results suggest that pipeline incidents on average dampen housing prices within 1,000 meters by 6%. The back-of-the-envelope calculation suggests an equivalent of $14 billion dollars loss (in 2014 dollar) over a 10-year period. In addition, we report a series of important findings on the heterogeneity in pipeline incident effects. Specifically, we find an incident that raises public awareness due to evacuation, explosion, or ignition poses a more severe impact on housing prices. Incidents that occur on private land or incidents occur on above-ground pipeline lead to a higher impact on housing prices compared to those on an underground pipeline. In general, the more salient an incident is, the more likely that nearby housing prices response to the impact. Since we adopt a quasi-experimental framework, the hedonic estimation identifies the movement along the ex post price function and serves as the lower bound of general equilibrium welfare (Banzhaf, 2021). Our main findings pass multiple robustness checks and falsification tests.

This study directly relates and contributes to the literature on the impact analysis of environmental hazards from energy infrastructure incidents. Previous studies in this area examine the impacts of underground storage facilities (Guignet et al., 2018), shale gas production (Muehlenbachs, Spiller, and Timmins, 2015), rail transportation of HAZMAT (Tang et al., 2020), and nuclear power

[2]Data provided by Zillow through Zillow Transaction and Assessment Dataset (ZTRAX). Interested readers can find more information on accessing the data at http://www.zillow.com/ztrax. The results and opinions are those of the author(s) and do not reflect the position of Zillow Group.

plants (Isakson and Ecker, 2018). On the topic of pipeline incident impacts, however, studies based on individual incidents produce mixed findings (Fruits, 2008; Wilde, Loos, and Williamson, 2012; Herrnstadt and Sweeney, 2017; McElveen, Brown, and Gibbons, 2017; Hilterbrand Jr, 2019; Boslett and Hill, 2019). We contribute to the literature by providing a nationwide assessment for pipeline incidents with more external validity and by reconciling conflicting results from previous studies as heterogeneous incident impacts. More broadly speaking, we can consider pipeline incidents as a form of information disclosure since many residents either do not know of the existence of pipelines, or do not fully appreciate the potential risks. Results from this study also contribute to the theoretical and empirical literature on how information disclosure affects housing prices as discussed by Pope (2008a). The following section provides a more detailed literature review.

This article proceeds as follows: Sections 2 and 3 introduce the background of gas pipeline incidents and related literature. Section 4 discusses the data. Section 5 includes formal evaluation of the impacts of incidents on nearby housing prices. Section 6 shows the estimation results and robustness checks. Section 7 concludes.

2 Background

Policy changes and technological innovation have contributed to the boom of domestic shale gas development since the early 2000s (Wang and Krupnick, 2013). Since then, the total domestic natural gas consumption has grown 12.5% (about 27.5 trillion cubic feet), compared with consumption in 2000 (Vetter et al., 2019). These factors triggered a surge in the construction of pipelines across the United States for distributing newly discovered natural gas from the U.S. Heartland to the coastal regions. According to PHMSA, the newly constructed natural gas distribution pipeline ranges from 24,000 to 50,000 miles during the past 15 years.[3] Among all types of natural gas pipelines, gas distribution pipeline accounts for more than 80% of the total mileage of gas pipelines in residential areas (Pless, 2011). In 2018, roughly three million miles of natural gas pipelines transported almost 28 trillion cubic feet of gas nationwide, which is roughly 13 times the volume of Mount Everest (Kelso, 2020). The natural gas distribution industry has grown to $169 billion in 2021 and it has supported 4.1 million U.S. jobs in 2020. (IBIS World, 2021; The Empowerment Alliance, 2020).

Even though some claim that pipelines are the safest transportation approach for crude oil and natural gas (Kenneth and Taylor, 2015), aging pipelines have become a potential threat to safety 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 and overseeing nationwide pipeline transportation of

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

HAZMAT. Once an incident occurs, the pipeline operator is expected to seek help from emergency correspondents at the local, state, or federal levels[4] and submit an incident report within 30 days (PHMSA, 2021a). Due to these mandatory reporting rules, PHMSA has integrated comprehensive pipeline incident data going back to the 1970s, and precise longitude and latitude information are available starting in 2010. In this study, we are able to geocode and match incidents with nearby properties after 2010.

Most gas distribution pipelines go through areas with dense populations and some connect to residential properties directly, which galvanizes concerns for human health and safety. However, researchers have not adequately quantified the economic impacts of pipeline incidents on the residential community and the environment. According to the statistics from PHMSA, pipeline incidents account for about \$500 million direct cost each year (PHMSA, 2021b). In this study, the estimated indirect cost of pipeline incidents due to property value loss is about three times higher than the direct cost gauged by PHMSA. Another missing part in PHMSA's calculation is gas leackage. Between 2000 and 2011, U.S. consumers paid more than \$20 billion for natural gas that escaped into the air due to pipeline failure (Vetter et al., 2019). A study in progress reports that natural gas leakage in residential areas in Massachusetts led to a \$12,000 loss in housing prices on average (Shen et al., 2021). In addition, pipeline failure may generate other unexpected negative externalities. For example, Xu and Xu (2020) study the effects of pipeline hazards on credit risk. They find that pipeline-present areas have a lower origination rate compared with pipeline-free areas and pipeline incidents further magnify by 1.8%.

3 Literature Review

This study relates to two strands of literature. First, we provide new empirical evidence on the impacts of energy infrastructure incidents. 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-pay for houses within 0.25 miles of multiple leaking underground storage tank sites, compared with those located further away. Muehlenbachs, Spiller, and Timmins (2015) find large negative impacts of shale gas leaks on nearby groundwater-dependent homes. Tang et al. (2020) explore the impact of derailments on nearby housing values and find significantly negative, but short-term impacts within one mile of derailment sites. Boes, Nüesch, and Wüthrich (2015) identify the negative impact of the 2011 Fukushima nuclear power incident on rents near nuclear power plants in Switzerland.

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

Tanaka and Zabel (2018) and Zhu et al. (2016) obtain similar results by using U.S. housing market data and Chinese land markets near nuclear power plants, respectively. Moreover, Nolte et al. (2021) 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. Our research provides additional evidence on the impacts of energy infrastructure incidents on nearby housing values using nation-wide data. Different from incidents related to other energy infrastructures, nearby residents are often unaware of the existence of pipelines and incidents.

Previous studies are inconclusive about how pipeline incidents impact nearby housing value. By exploring a single pipeline incident, Herrnstadt and Sweeney (2017) and McElveen, Brown, and Gibbons (2017) find little evidence that nearby housing prices capitalized the incident. Wilde, Williamson, and Loos (2014) and Fruits (2008) find that neither announcement nor construction of pipeline has significant impact on nearby residential housing prices. Wilde, Loos, and Williamson (2012) summarize previous results and conclude that, based on sales data, there is no evidence that proximity to pipelines reduces property values. Hansen, Benson, and Hagen (2006) find that owners sell houses closer to a pipeline at a discount after an explosion, but not before. However, there is no formal test of the difference between these coefficients. Lee et al. (2021) study a underground pipeline explosion in Taiwan and find negative effects on housing prices that may persist over longer periods. Hilterbrand Jr (2019) uses an experiment to find that disclosure of pipeline location affects buyers' offers for a nearby property. The heterogeneity of different pipeline incidents may be the cause of the above inconclusive results. For instance, an incident associated with underground pipeline may be ignored and has little effect on property value. Our study contributes to the literature by providing nationally representative estimates using pipeline incidents over the past decade as well as evidence for heterogeneity in different pipeline incidents.

Second, we showcase the value of information disclosure under the hedonic framework via salient signals like pipeline incidents. Researchers widely employ hedonic models in the empirical literature (Sieg et al., 2002; Banzhaf, 2020; Bishop et al., 2020; Bishop and Timmins, 2018; Bishop, 2008; Ma, 2019; Bishop and Murphy, 2011), which increasingly discusses the role of information disclosure and where home buyers' preferences captured by the hedonic model depend on the amount of information available to them. Pope (2008a) is among the first to consider how asymmetric information between buyers and sellers can affect the hedonic price gradient and show that airport noise disclosure reduces the value of houses. Furthermore, Pope (2008b) studies seller disclosure 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. In most cases, home buyers do not know the exact location of a pipeline until salient signals, such as a pipeline incident, emerge. Pipeline incidents compel home buyers to re-assess their bidding for a property. Our paper reconciles previous inconclusive results and shows that the significant and large impacts of incidents on housing prices often closely relate to the extent to which an incident is salient and visible.

4 Data

4.1 Pipeline Incident Data

PHMSA defines an event as a pipeline incident if it meets at least one of the following conditions: (a) a death or personal injury requiring hospitalization; (b) estimated property damage of \$50,000 or more; or, (c) unintentional estimated gas loss of three million cubic feet or more. Between 1986 and 2020, PHMSA recorded 8,263 pipeline incidents associated with four types of gas pipelines, including distribution lines, gathering lines, transmission lines, and liquid natural gas (LNG) lines. Among those gas pipelines, gas distribution lines account for the majority (55%) of incidents. Compared with other types of pipelines, gas distribution lines mostly go through or directly connect to populated regions, which makes it possible to analyze the impacts of pipeline incidents on residential property values. Figure A1 in the Appendix shows the distribution of the gas-distribution-pipeline incidents nationwide between 2010 and 2020. Most gas distribution pipeline incidents (darker areas) are clustered in metropolitan areas. In this study, we analyze the 864 incidents associated with gas distribution lines in urban places between 2010 and 2020.[5] We focus on incidents in the past decade for two reasons. First, data before 2010 have imprecise coordinate information, which may lead to serious measurement error if included. Second, PHMSA changed the incident reporting requirements after 2010 when they began requiring additional information regarding an incident, such as the locality and the amenity of incident sites. We can therefore leverage that information to perform comprehensive analyses on the heterogeneous effects. Furthermore, we narrow our focus to the gas distribution pipeline incidents in urban areas where most housing transaction data are available.[6]

In addition to basic information on the location, time, and cause of each incident, our dataset provides additional details on each incident, such as the number of fatalities and injuries, the estimated volume of gas unintentionally released, the estimated direct damage cost, the number of

[5] In total, the PHMSA pipeline incident dataset includes information on 1,222 incidents between 2000 and 2020, among which 976 occurred in urban areas. We remove 112 incidents with few house transactions within about 13 kilometers (eight miles) of incidents and focus on the other 864 incidents in our analyses.

[6] We define urban areas both as urban areas and urban clusters according to 2010 census data.

public evacuees, whether an explosion or ignition occurred, and whether the incident occurred above ground or underground. Among all incidents included in this study, 32% are due to unintentional excavation, 32% are due to damage by outside forces, and 7% are the result of natural force damage. Furthermore, about 10% of incidents include fatalities, while more than 50% involve injuries or explosions, respectively. More than half of the incidents led to public evacuation or ignition. Around 40% occurred in a private house or above ground, respectively (See Table B1 in the Appendix). Based on PHMSA's criterion for pipeline incidents, we identify 239 incidents that involved death or personal injury requiring hospitalization, 482 incidents that caused property damage of $50,000 or more,[7] and 159 incidents that generated a gas loss of a million cubic feet or more.[8]

4.2 Housing Transaction Data

We compile housing transaction data from the ZTRAX database. The ZTRAX database contains about 491 million unique transactions associated with 161 million unique owner-occupied houses across all 50 U.S. states, Washington D.C., and Puerto Rico. The housing transaction data include detailed information on the sales amount, the date of transaction, and house structural characteristics. Housing structural characteristics include the year of build, the number of bathrooms, the number of bedrooms, the square footage, and indicators for air conditioners and a fireplace.

In the data cleaning process, we remove transactions with sale prices lower than $1,000, as well as those with missing primary structural characteristics, such as the number of bathrooms or bedrooms. For simplicity, we only use SFH transactions that are arms-length transactions. We deflate all sale prices to 2000 dollars using the Federal Housing Finance Agency's (FHFA) state-quarter house price index.

4.3 Linking Housing Transactions to Pipeline Incidents

We link the housing transactions to pipeline incidents based on their coordinates. Specifically, we treat each included incident site as the center and generate a total of 864 incident buffer zones with a radius of 13 kilometers (eight miles).[9] We select housing transactions that occurred within the 864 buffer zones for further analyses.[10] We match each transaction with incidents based on their timings of occurrence. We assign each house to either pre- or post-incident groups based on the

[7] PHMSA deflates all costs to 1984 dollars.

[8] PHMSA also defines a pipeline incident as "serious" if fire is not the cause, but causes fatality or injury. If an incident is either "serious" or brings about property damage of $50,000 or more as well, PHMSA refers to the incident as "significant." We identify 223 "serious" incidents and 571 "significant" incidents in the incident database.

[9] The eight-mile radius is a generous estimate of the potential impact range of a pipeline incident. The associated buffer zone covers sufficient transactions to measure the extent of incident impacts on housing transactions. See Section 5 for more details.

[10] In the data matching process, we exclude transacted houses less than five meters from the incident site as such transacted houses may receive large direct damage.

time difference between the transaction date and incident date. A complication arises when more than two incident buffers overlap. Transacted houses that fall within the overlapping areas may link to more than one incident. For simplicity, we link properties transacted after multiple incidents only to the post-incident group of the last incident.[11]

In summary, we obtain about 17 million unique property transactions associated with the 864 incidents. An incident, on average, matches with 106,036 transactions. The total number of transactions before and after an incident are balanced. We leverage the 17 million transactions to identify the spatiotemporal extents of incident impacts using the binscatter regressions and thus construct the treatment and control groups for DID analyses. In the DID analysis, the sample size shrinks to 19,435 transactions and 219 associated incidents. The substantially lower number of observations is due to three reasons: first, the defined treatment and control groups cover circle areas with a 2-kilometer, instead of a 13-kilometer (8-miles), radius from incident sites; second, the temporal extent of pre- and post-incident groups decreases from 1000 days to 500 days before and after incidents; third, we remove transactions with incomplete information on housing characteristics.

5 Quantifying the Impacts of Pipeline Incidents on Housing Values

Investigating the changes in housing values before and after a pipeline incident is a revealed preference approach to measure the implicit costs of the incident. We first leverage binscatter regressions to measure the spatiotemporal extents of incidents' impacts. Based on the results in the first step, we then estimate the incident impacts using a DID framework. We examine the estimation results by incorporating several robustness checks and falsification tests.

5.1 Measuring the Spatial and Temporal Extents of Incident Impacts

In order to carry out the DID analysis, we first need to define the spatial coverage of the treatment and control groups as well as the duration of the pre- and post-periods. To maximize the sample comparability, houses in the control groups should be as close to the incident locations as those in the treatment groups as possible, but are not affected by the incidents. Similarly, the post-period should be as long as the impact persists. To achieve these goals, we need to determine the spatial and temporal extents of incidents impacts.

We adopt a data-driven approach, namely binscatter regression, to uncover the relationship

[11] As to be discussed in the next section, to better serve our identification strategy, we limit houses linked to each incident to those that located within 2 kilometers away from an incident and transacted 500 days before and after the incident.

between housing values and distance between incident sites and houses. Binscatter regression has gained popularity because it provides a flexible yet parsimonious way to visualize and summarize large dataset in a regression setting (Cattaneo et al., 2021). Compared to applications of polynomial regression and canonical binscatter, binscatter regression provides optimal bin structure and robust confidence band to facilitate statistical inference (Cattaneo et al., 2021). In addition, binscatter regression offers a semi-linear approximation on the relationship between housing values and the distance to incident sites. In this study, we expect the impacts of incidents on housing values to gradually diminish with the distance from incident sites. Figure 1 depicts the logarithmic form of housing values against the distance from incident sites based on the 17 million unique property transactions within 13 kilometers (eight miles) of 864 pipeline incidents. The blue circle and red square represent transaction prices before and after incidents, respectively, and the colored bands are the 90% confidence intervals. The graphical result suggests that the average housing values within 1,000 meters of incident sites plummet after incidents occur, but the two housing value bands converge beyond the 1,000-meter range. Therefore, we determine that the spatial extent of pipeline incidents' impacts on housing values is approximately 1,000 meters. We assign houses within 1,000 and 2,000 meters of an incident to the control group.[12] The summary statistics indicate that houses in the control are similar to those in the treatment groups.

Similarly, we employ the binscatter regression to identify the temporal extent of the incident impact. Figure 2 plots the logarithmic form of housing values against time (days) before and after the incidents. The blue circles represent the trend of housing values of properties within 1,000 meters of incident sites while the red squares denote that of properties between 1,000 and 2,000 meters away. Figure 2 indicates that the average housing values within 1,000 meters drop after incidents while, in comparison, the average housing prices in the control group manifest a stable trend over time. The price dive is noticeable and significant during the first 500 days after the incident and seems to persist at least 1,000 days. We choose 500 days as the duration of the pre- and post-periods, separated by the date of the incident.

5.2 Difference-in-Differences: Estimation of the Average Impact of Pipeline Incidents on Housing Values

We now formally estimate the size of incident impacts using a DID framework. Based on the results of binscatter analyses, we construct four groups for the DID analysis: a pre-incident control group; a post-incident control group; a pre-incident treatment group; and a post-incident treatment group. Specifically, we group properties located within 1,000 meters of incident sites as treatment groups,

[12] See Section 5.3 for an analysis on the covariates balance between those two groups of houses.

while we categorize those between 1,000 and 2,000 meters away as control groups. We define the pre- and post-period by using a time window of 500 days before and after the incident occurrence date. We specify the DID model as:

$$\begin{aligned} ln(Price)_{it} = &\alpha_0 + \alpha_1 Treat_i + \alpha_2 Post_t + \gamma Treat_i \times Post_t \\ &+ \theta_y + \eta_j + \tau_p + \omega_z \times t + \beta_1 X_i + \beta_2 X_i \times t + \epsilon_{ijpt} \end{aligned} \tag{1}$$

where $ln(Price)_{it}$ is the logarithmic form of the transaction price for the house i associated with a pipeline incident that occurs at time t; and, $Treat_i$ is a dummy variable, which equals 1 if house i is within the 1,000-meter buffer of an incident, 0 otherwise. In our setting, a house with treatment status $Treat_i$ equal to 1 belongs to the treatment group, while those with $Treat_i$ equal to 0 belong to the control group. $Post_t$ is a dummy equal to 1 if a house within 500 days after the occurrence of an incident, and is equal to 0 if the transaction happened within 500 days before the incident. When a transaction falls into two or more overlapped incident buffer zones, for simplicity, we link the specific transaction to only one of these incidents—the transaction serves in either the post-incident groups of an incident or in the pre-incident groups of another incident, but not both.

We also add flexible fixed effects to control time-invariant or location-invariant unobservables. τ_p is the incident fixed effect that removes unobservables specific to the time and location of each incident. The location fixed effects, η_j, controls for time-invariant unobservables at the census tract level. θ_y is year-quarter fixed effects, which controls for macroeconomic shocks influencing all housing transactions each year. Due to the vast spatiotemporal extents of transaction data, we also add the state-specific trend, $\omega_z \times t$, to allow differential trends in the housing market of each state.[13] X_i represents the house characteristics as shown in Table 1. We also include the interactions between house characteristics and yearly trend $X_i \times t$ in some model specifications to allow consumers' preferences to evolve over time (Kuminoff and Pope, 2013, 2014). We cluster standard errors at the incident level.

In our DID framework, the parameter of interest is γ, which is identified by the difference in the average housing values of treatment groups before and after an incident compared with property values in the control groups associated with that incident. Equation (2) spells out the estimated

[13]We avoid adding census-tract-by-year or county-by-year fixed effects in the model because the incidents included in the DID analysis are sparsely located nationwide such that few incidents happened in the same census tract or county. Incorporating census-tract-by-year or county-by-year controls may absorb all the variations needed for model identification.

treatment effect:

$$
\begin{aligned}
\gamma = & (E[ln(Price)^1_{i1}|Treat_i = 1] - E[ln(Price)^1_{i0}|Treat_i = 1]) \\
& - (E[ln(Price)^0_{i1}|Treat_i = 0] - E[ln(Price)^0_{i0}|Treat_i = 0])
\end{aligned}
\tag{2}
$$

where the superscripts on $ln(Price)$ denote the counterfactual treatment status (1 if the transacted house is within 1,000 meters of an incident, 0 otherwise) regardless of the incident occurrence status. The main identifying strategy relies on the parallel-trends assumption where

$$
\begin{aligned}
& E[ln(Price)^0_{i1}|Treat_i = 1] - E[ln(Price)^0_{i0}|Treat_i = 1] \\
& = E[ln(Price)^0_{i1}|Treat_i = 0] - E[ln(Price)^0_{i0}|Treat_i = 0]
\end{aligned}
\tag{3}
$$

This key assumption requires that, in the absence of treatment status, the potential log of the house transaction price in the treated group follows the same trend as that in the control group.

Empirically, we can test the plausibility of the parallel trend assumption by event analyses. Specifically, we interact the treatment dummy variables with year dummy variables as follows:

$$
\begin{aligned}
ln(Price)_{it} = & c_0 + \sum_{\tau=-6, \tau \neq -1}^{6} \alpha_\tau [1(t \in [\tau \times 90, (\tau+1) \times 90]) \times Treat_i] + \\
& + \theta_y + \eta_j + \tau_p + \omega_z \times t + \beta_1 X_i + \beta_2 X_i \times t + \epsilon_{ijpt}
\end{aligned}
\tag{4}
$$

where we define fixed effects, state-year trends, house characteristics, and their year trends similar to equation (1). The parameter of interest is α_τ. τ is the index for time groups. Using 540 days before and after an incident, we cluster 90 days as one group such that there are both six pre- and post-incident groups. τ ranges from -6 to 6. Group 0 includes transactions within 90 days after an incident. We set group -1 as the reference group. Each α indicates the difference between the control and treatment groups in each group after holding the controls. The parallel trends assumption is not contradicted if α_{-6} to α_{-2}, which represent days -540 to -180, are statistically insignificant.

5.3 Summary Statistics and Covariates Balance Check

In total, we identify 219 out of 864 pipeline incidents where there are, on average, 88 housing transactions nearby[14]. Figure A4 in the Appendix presents the spatial distribution of the 219 incidents compared to the full 864 incidents. The majority of the included 219 incidents are clustered in the Midwest (e.g., IA, IN, OH, KY) and the East Coast (e.g., NY, PA, MA, NJ); however, regions

[14]Pipeline incidents that occurred in remote places and non-urban regions and therefore involve a small number of houses are removed.

with dense populations on the West Coast and the South also witnessed multiple incidents across the years (See Figure A5 in the Appendix). The occurrences of the 219 incidents distribute evenly across the entire study time frame (see Figures A2 and A3 in the Appendix). We compare the characteristics of the selected 219 incidents with that of the full 864 incidents and calculate the summary statistics in Table B1 in the Appendix. In general, the relative share of different types of incidents in the DID sample resembles those in the full 864 incidents, with the exception that the 219 incidents include slightly higher proportions of explosion or ignition.

We further show the balance of covariate distributions in the treatment and control groups. Table 1 presents descriptive statistics of housing prices and characteristics of our data sample. Columns (1) and (2) report the mean and associated standard error (in parentheses) of covariates for the pre-incident control and treatment groups while columns (3) and (4) denote the same information of post-incident control and treatment groups. We employ the Standardized Difference to assess the balance in covariates between the pre-incident groups and post-incident groups, respectively. An absolute standardized difference below 0.10 indicates covariate balance between groups (Austin, 2009). Results in columns (5) and (6) show that almost all housing covariates are balanced in the pre-incident groups and post-incident groups.

6 Results

6.1 DID Results

Table 2 reports the DID estimations of local impacts of the gas distribution pipeline incidents. From columns (1) to (4), we incrementally add fixed effects and year trend controls. Column (1) adds year-quarter, block group fixed effect, and incident fixed effect. Starting from column (2), we change the level of spatial fixed effects from block group to census tract, and further include the state-specific trend in column (3). Lastly, the specification in column (4) considers the change of home buyers' preferences by incorporating the interaction between house characteristics and year dummies. The number of observations changes by specification due to missing block group or census tract information for some houses. All specifications have house and neighbor characteristics included in Table 1.

All DID models consistently show a negative local impact of pipeline incidents on housing prices. The coefficient of $Treat_i \times Post_t$ ranges from about 4% to 6%. The effect estimation in column (4) suggests that pipeline incidents dampen the average house price within a 1,000-meter radius of incidents by 6.1%, which is statistically significant at the 5% level. The most flexible model in column (4) is our preferred specification.

We estimate equation (4) to test the plausibility of the parallel trends assumption. The event study analysis adopts the control and treatment group definition and includes house characteristics. We cluster all transactions in 90 days and generate seven temporal groups in the pre-incident period and 6 groups in the post-incident period. Figure 3 shows how the price difference between treatment and control groups evolves before and after the occurrence of pipeline incidents. In the pre-incident period (i.e., negative group numbers), the price difference between the treatment and control groups is not statistically different from zero. After the incidents, the price difference between treatment and control groups is negative and significant, especially in the initial 90 days post-incident. After 270 days, property prices in the treatment group partially recover but do not fully return to the pre-incident level, implying a possible long-term impact of the incidents on nearby housing prices.

6.2 Heterogeneous Effects

We leverage different subsets of incidents to evaluate the heterogeneous effects of various types of incidents on nearby housing prices. Specifically, we examine impacts of incidents with different severity, location, and positions (above ground or underground). We also explore the accumulated effects of multiple incidents on housing values.

Table 3 shows estimations of the impacts of incidents that involve fatality or injuries. The local impact of incidents with fatality or injury is about 2.3%, which is not statistically significant. However, incidents without fatality or injury have a larger statistically significant impact (about 5.7%) on nearby housing prices. This result is counter intuitive and difficult to explain.

Table 3 also shows that incidents causing public evacuations, explosion, or ignition led to statistically significant and negative impacts on properties within 1 kilometer compared with houses 1–2 kilometers away. However, the incidents not causing public evacuations, explosion, or ignition did not see statistically significant or large impacts on nearby property. On average, pipeline incidents with public evacuations lowered nearby housing prices by 7.6%. Moreover, explosion or ignition would dampen the nearby housing prices by 14.5% and 8.3%, respectively.

Next, we examine the potentially differential impacts due to whether an incident occurred on privately owned land or public land. As shown in Table 3, only incidents that occur on private land yield a negative and statistically significant estimate; however, incidents that happened on public land did not induce a large or statistically significant impact. Incidents that occurred on private land likely took place in population-dense areas, and were thereby more salient to the residents. However, most public lands are separated from residential regions, making incidents on public lands less observable to private home owners. In addition, the results in table 3 show that incidents that occur above ground bring housing prices down by about 12.5%, which is a much larger effect than those that occurred underground.

Lastly, we examine the cumulative impacts of multiple incidents on housing values. Although our DID framework allows a transaction to match with only one incident regardless of the temporal difference between the transaction date and incident date, we denote the number of historical incidents associated with each property that falls within those incidents' buffer zones before its transaction. For instance, we may link a transacted property with an incident that occurred on June 1, 2016, in the DID regression though we have tagged it as having experienced five historical incidents beforehand. Based on results in Table 3, if a house experiences less than five incidents before the transaction, the impact is around 6% with marginal statistical significance. In addition, ceteris paribus, the value of a house that has experienced more than five incidents declines by 12.1% on average. Our results indicate that an incident occurring in areas with a historically high density of pipeline incidents would drive more housing value loss.

6.3 Robustness Checks

We conduct a series of robustness tests, including: (a) implementing a triple-difference analysis by incorporating extra transactions farther away from each incident site; (b) adopting alternative treatment and control groups to re-run the DID model; (c) performing a falsification test by artificially adjusting the occurrence date of incidents 500 days and 1,000 days earlier; (d) incorporating a matching and regression method to further examine the original estimations; and, (e) adopting different control groups by extending the distance bandwidths of control groups for the DID model (i.e., 1,000–2,500 meter radius, 1,000–3,000 meter radius, 1,000–3,500 meter radius, and 1,000–4,000 meter radius.).

To construct the placebo treatment group, we first draw a 2,000-meter-wide band that is 3,000 meters away from each incident site. Transacted properties that reside within the created donut-like band (i.e., between 3,000 and 5,000 meters away from each incident site) make up the placebo treatment group. Correspondingly, we define transacted properties between 2,000 and 3,000 meters and between 5,000 and 6,000 meters away from incident site as the placebo control group. Figure 4 gives an example of the placebo treatment and control groups. The black dot denotes an incident. The red band (1,000 meters wide) represents the treatment group and the green region (1,000 meters wide) is the control group. The yellow band (2,000 meters wide) is the placebo treatment group. Lastly, the two blue donut-like areas serve as the placebo control groups of the incident. We specify the triple-difference model as:

$$
\begin{aligned}
ln(Price)_{ipt} =& \alpha_0 + \alpha_1 Treat_i + \alpha_2 Post_t + \alpha_3 Incident_{ipt} + \gamma_1 Treat_i \times Post_t + \\
& \gamma_2 Treat_i \times Incident_{ipt} + \gamma_3 Post_t \times Incident_{ipt} + \gamma Treat_i \times Post_t \times Incident_{ipt} \\
& + \theta_y + \eta_j + \tau_p + \omega_z \times t + \beta_1 X_i + \beta_2 X_i \times t + \epsilon_{ipt}
\end{aligned} \tag{5}
$$

where $Incident_{ipt}$ is a dummy variable that equals 1 if house i transacted at time t is located in the 2,000-meter radius of an actual pipeline incident p and 0 if located in the 2,000-meter radius of the associated placebo pipeline incident. From equation (5), γ is the parameter of interest. The placebo treatment and control groups contribute to a third degree of difference that helps further dampen any regional shocks on housing markets and improve the estimation accuracy. We thus expect to see a similar estimation on the incident impact using the triple-difference model. In Table 4 column (1), $Post \times Treat \times Incidents$ shows a statistically significant estimation of -0.058 at the 5% level. The estimation result is similar to that from the DID model.

The second robustness check is to apply alternative treatment and control groups for our DID model. Presumably, alternative treatment and control groups may generate a null effect of incident on housing prices. To this end, we utilize transacted properties in the placebo treatment and control groups from the triple-difference design (i.e., the yellow and blue regions in Figure 4) and re-run the DID model. As expected, results in Table 4 column (2) provide evidence for the null effect of incidents on housing prices using the placebo treatment and control groups.

As a falsification test, we artificially move the occurrence date of incidents 500 days or 1,000 days earlier. A plausible causal relationship between pipeline incidents and housing values implies that any incident with a manipulated occurrence date would generate null impacts on housing prices. As expected, Table 5 columns (1)–(4) indicate insignificant and near-zero estimation of incidents with manipulated occurrence date.

We further incorporate a matching and regression method to improve balance by adjusting for pre-incident observable differences between these two groups. We incorporate the propensity score matching method to improve the covariate balance between the treatment and control groups by adjusting for pre-incident observable differences. Specifically, we match each transacted house in the treatment group with one house in the control group based on their housing characteristics, followed by running the DID model on the matched samples. We obtain 9,827 matched observations in total. The regression results in Table 6 are quite similar to our main results.

Lastly, we check if the estimates are sensitive to the choice of control groups. By adjusting the bandwidths of control groups from a 1,000–2,000 meter radius to 1,000–2500 meters, 1,000–3,000 meters, 1,000–3,500 meters, or 1,000–4,000 meters and following model specification (4) in Table 2, Table 7 shows that the coefficients range from -0.04 to -0.052 with statistical significance at least at the 10% level. Including extra transactions seems to dilute the estimations, but our main conclusions on incident impacts on housing values remain.

6.4 Back-of-the-envelope Calculation

To further quantify the economic cost of pipeline incidents, we perform back-of-the-envelope calculations on the economic costs in terms of housing value reduction. We calculate that the average price of houses within a 1,000-meter radius of pipeline incidents before the incident is $175,728. Moreover, there are on average 953 houses within a 1,000-meter radius around each of the 1,222 incidents. Incorporating the information above and the DID estimates, we find that the pipeline incidents caused around $10–$14 billion (U.S. dollars) in loss over a 10-year period due to nearby house devaluation, which we can treat as the lower bound of the loss from house devaluation because we only use transacted house information instead of housing stock. This is equivalent to around 1.4% of total U.S. house transaction values in 2014. (Statista, 2020)

7 Discussion and Conclusion

Pipeline safety and the economic impacts of pipeline incidents are of great concern to the public. Based on a nationwide gas distribution pipeline incident dataset combined with a comprehensive house transaction dataset, we provide the first nationwide assessment on the impact of 864 gas distribution pipeline incidents on nearby housing values. The DID analyses on a subset of these incidents show that housing values decline by 4%–6% on average when pipeline incidents occur nearby.

We find significant negative impact of pipeline incidents on nearby housing values, which contradicts some previous studies such as McElveen, Brown, and Gibbons (2017), Herrnstadt and Sweeney (2017), and Wilde, Loos, and Williamson (2012), while confirming studies like Lee et al. (2021) and Hilterbrand Jr (2019). Existing studies often focus on a single pipeline incident. As we show in this paper, pipeline incidents have heterogeneous effects and one incident might not be representative of others. Based on nation-wide 864 historical incidents and transaction data, our estimates provide a more representative response of housing values due to pipeline incidents. Insignificant impacts from pipeline incidents in previous literature may underestimate the implicit costs of pipeline incidents, thus preventing policymakers from performing a comprehensive and accurate cost-benefit analysis on newly pipeline constructions. Future research could examine whether there is a heterogeneous response among different population characteristics such as race, income level, etc. Moreover, it is interesting to incorporate pipeline construction announcement information and investigate if residents react to pipeline announcements due to potential environmental hazard risks.

Acknowledgements

The authors gratefully thank Dermot Hayes, Desire Kedagni, John Winters and Emily Berg for insightful suggestions on an early version of this paper. We also wish to thank George Gardner, Dennis Guignet and Amit Batabyal, as well as the seminar participants at the 2021 AERE meetings, the 2021 AAEA meetings, the 2021 NAREA meetings, and the 2022 WRSA meetings for helpful comments and discussions. We would like to thank the Pipeline and Hazardous Materials Safety Association for sharing helpful information and answering questions. We thank Nathan Cook for providing excellent language help and editing assistance. Tang thanks Huazhong Agricultural University Scientific & Technological Self-innovation Foundation (Program No. 2662021JGQD002) for partial support of his research. Any errors are our own.

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Main Figures

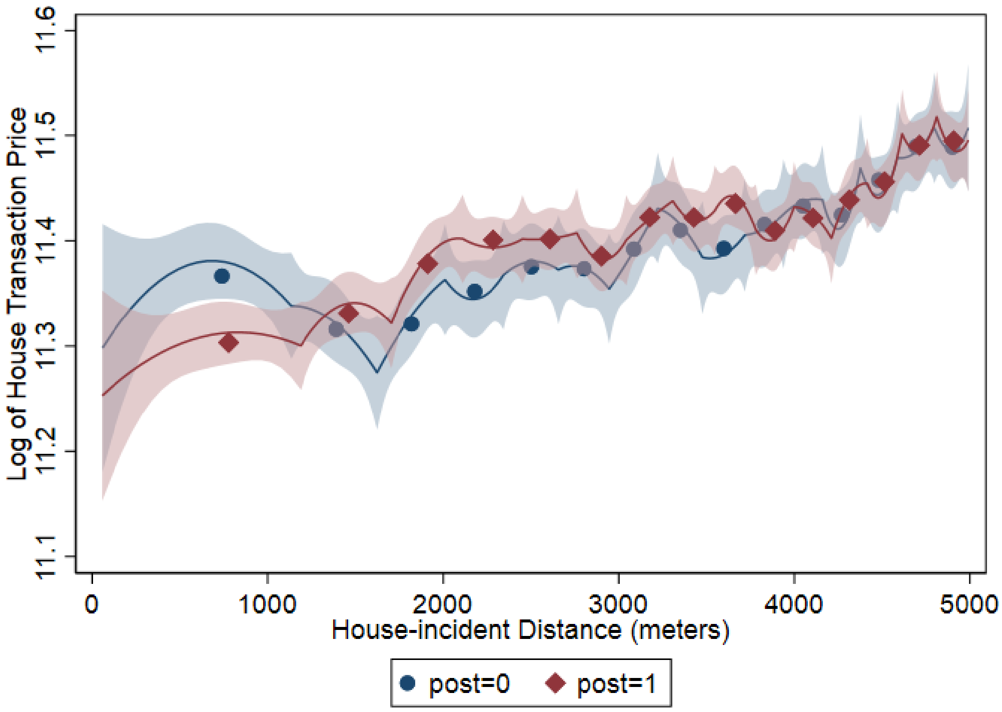

Figure 1: Price function estimates by distance (meters) before and after pipeline incidents.

Note: This graph shows the binscatter regression results of price gradient relative to distance. The blue and red bands indicate the 90% confidence interval of price gradient before (post=0) and after (post=1) pipeline incidents, respectively.

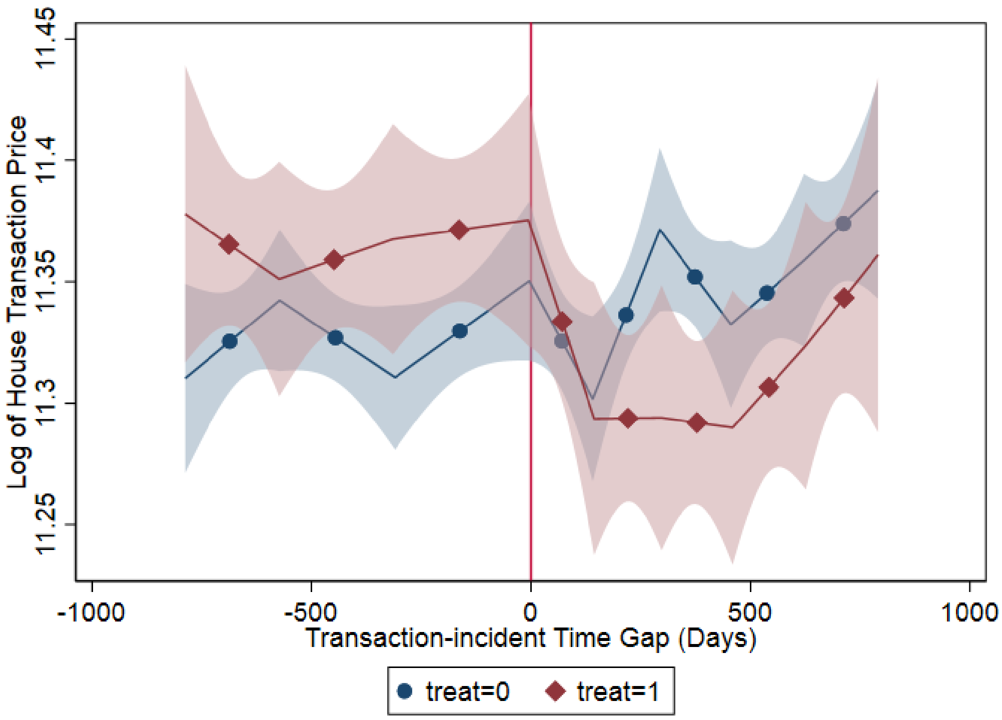

Figure 2: Price function estimates by time (days) before and after pipeline incidents.

Note: This graph shows the binscatter regression results of price gradient relative to the occurrence date of incidents. The red and blue bands indicate the 90% confidence interval of price gradient of treatment (treat = 1) and control (treat = 0) groups, respectively.

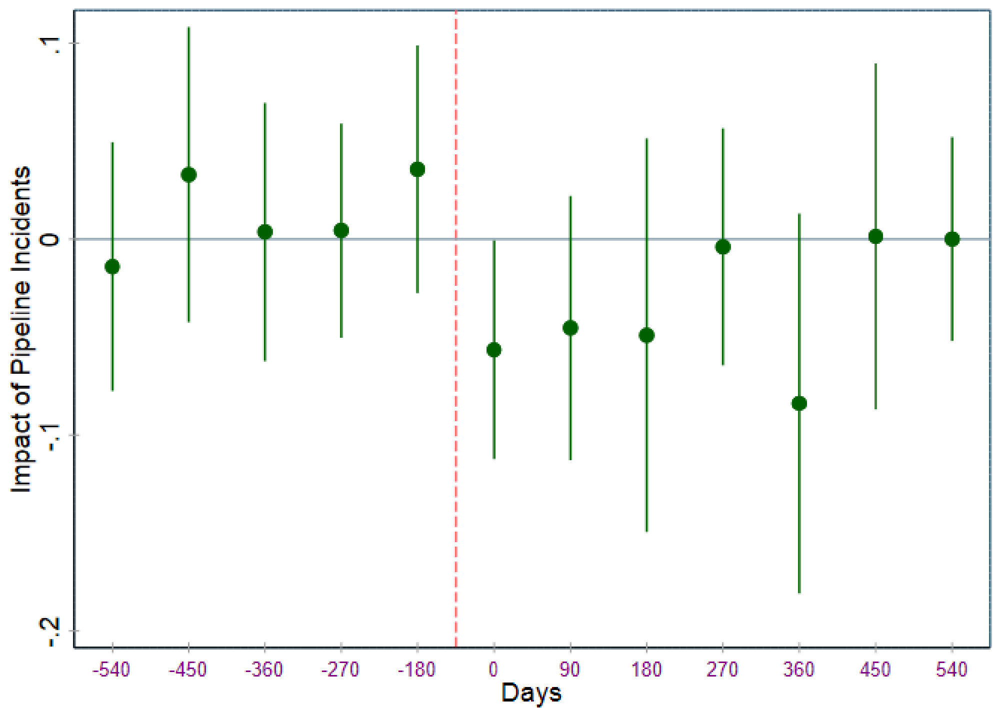

Figure 3: Price difference between treatment and control group before and after pipeline incidents.

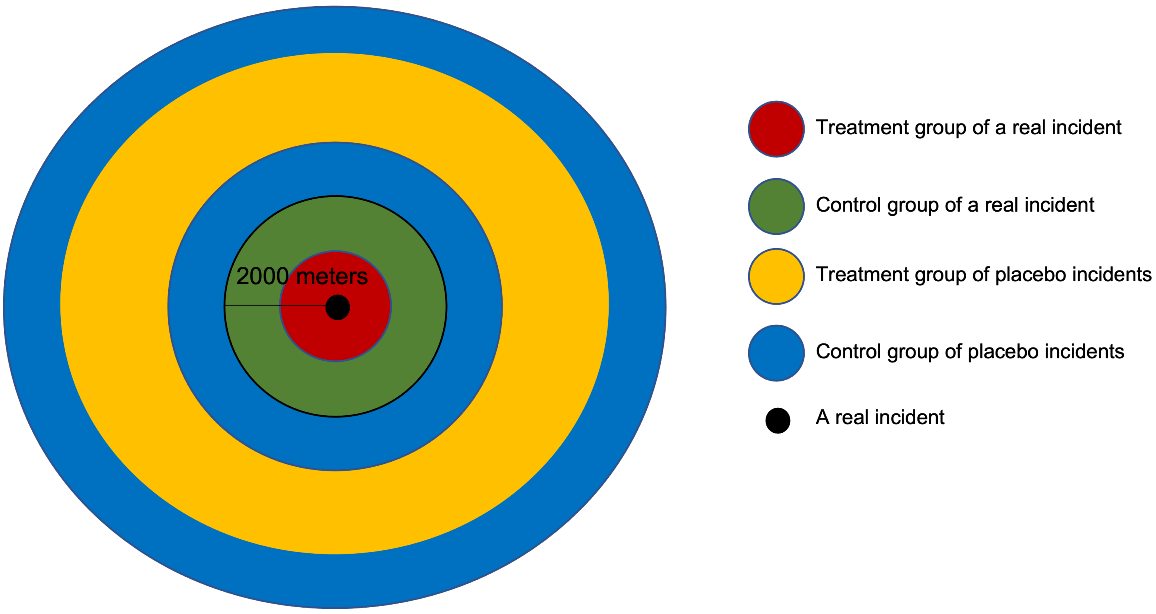

Figure 4: Illustration of triple difference construction.

Note: The graph shows the placebo treatment group in DDD analysis. The black dot denotes an incident. The red band (1,000 meters wide) represents the treatment group and the green region (1,000 meters wide) is the control group. The yellow band (2,000 meters wide) is the placebo treatment group. The two blue donut-like areas serve as the placebo control groups of the incident. Each ring is 1,000 meters wide.

Main Tables

Table 1: Descriptive Statistics for House Transaction Data in Control and Treatment Groups

	Pre-Control	Pre-Treatment	Post-Control	Post-Treatment	Pre-Diff	Post-Diff
Sales Amount ($1000)	178.4(278)	178.2(273)	202.5(318)	205.7(323)	0.00	-0.01
House Age (day)	16,956(1,003)	16,864(940)	18,043(1,113)	18,014(1,113)	0.09	0.02
Lot Size (1000 ft^2)	11.56(53)	12.25(88)	11.71(59)	10.65(50)	-0.01	0.02
No. of Stories	1.52(.49)	1.51(.48)	1.47(.50)	1.48(.50)	0.03	-0.01
Total Rooms	4.82(3.3)	5.00(3.4)	4.53(3.4)	4.75(3.3)	-0.05	-0.06
Total Bedrooms	2.70(1.4)	2.86(1.3)	2.58(1.4)	2.67(1.4)	0.11*	-0.06
Full Baths	1.49(.72)	1.49(.74)	1.52(.78)	1.49(.74)	0.01	0.04
Half Baths	0.61(.59)	0.61(.63)	0.58(.58)	0.61(.60)	0.01	0.06
Air Conditioner	.82(.38)	.79(.40)	.82(0.38)	.81(.39)	0.06	0.04
No. of Fireplaces	0.32(.47)	0.32(.47)	0.28(.45)	0.25(.44)	-0.01	0.05
# of Obvs.	4,900	1,678	9,560	3,297	-	-

Note: Standard errors in parentheses.
*: Imbalance between control and treatment group.

Table 2: Difference-in-differences Estimation Results—–Local Impacts

	(1)	(2)	(3)	(4)
Post $\times Treat$	$-.0428$	$-.0311$	$-.0458^{*}$	$-.0629^{**}$
	(.0311)	(.0312)	(.0272)	(.0276)
Post	.0014	-.0122	-.0197	.0063
	(.0294)	(.0329)	(.0282)	(.0329)
Treat	.0170	-.0097	.0140	.0284
	(.0394)	(.0344)	(.0348)	(.0342)
Covariates	Yes	Yes	Yes	Yes
Neighborhood Charact.	Yes	Yes	Yes	Yes
Year/Quarter FE	Yes	Yes	Yes	Yes
Pipeline Incidents FE	Yes	Yes	Yes	Yes
State Linear Trends	No	No	Yes	Yes
Spatial FE	Census Tract	Block Group	Census Tract	Census Tract
Covariates Specific Year Trend	No	No	No	Yes
# of Obvs.	19,435	19,123	19,435	19,435

Note: this table shows estimation results of equation 1 with different specifications. All models are based on 19,435 observations within two kilometers of 219 pipeline incidents. Column (1) includes no state linear trends and covariates specific year trend with census tract as spatial fixed effects; Columns (2) through (4) increasingly include state linear trends and covariates specific year. Column (2) also changes the spatial fixed effects to block group level. Columns (3) and (4) change back to census tract level fixed effects. Standard errors in parentheses.
* * *:statistically significant at 1% level.
**: statistically significant at 5% level.
*: statistically significant at 10% level.

Table 3: Heterogeneous Effects of Pipeline incidents on Housing Values

	$Post \times Treat$	$Post$	$Treat$	# of Obs.
Fatality=0 or Injury=0	-.0586**	.0014	.0201	18,401
	(.0290)	(.0305)	(.0377)	
Fatality>0 or Injury>0	.0234	.0125	-.0992	3,831
	(.0740)	(.0737)	(.0707)	
With Pub. Evacuees	−.0794**	.0019	.0547	11,527
	(.0351)	(.0355)	(.0517)	
Without Pub. Evacuees	-.0359	.0461	−.0167	7,895
	(.0424)	(.0597)	(.0387)	
With Explosion	−.1567***	.0839	.0741	7,710
	(.0431)	(.0768)	(.0435)	
Without Explosion	-.0257	.0082	.0115	12,584
	(.0299)	(.0347)	(.0457)	
With Ignition	−.0867**	.0273	.0426	13,563
	(.0334)	(.0356)	(.0408)	
Without Ignition	.0308	.0271	−.0692*	5,852
	(.0459)	(.0493)	(.0410)	
Private Land	−.0794**	.0205	.0594	11,514
	(.0338)	(.0393)	(.0396)	
Public Land	.0323	.0974	-.1372***	4,176
	(.0764)	(.0732)	(.0459)	
Above ground	−.1331***	.0736	.0116	7,522
	(.0396)	(.0657)	(.0293)	
Underground	-.0028	.0088	.0187	11,905
	(.0378)	(.0351)	(.0531)	
Experience 1 Inci.	-.0641*	.0384	.0169	11,432
	(.0372)	(.0384)	(.0368)	
Experience 2 to 5 Inci.	-.0684*	.0348	.0402	13,881
	(.0355)	(.0500)	(.0393)	
Experience 5+ Inci.	−.1286*	.2386	.0256	7,075
	(.0778)	(.2395)	(.0434)	
Covariates		Yes		
Neighborhood Charact.		Yes		
Year-Quarter FE		Yes		
Pipeline Incidents FE		Yes		
State Linear Trends		Yes		
Covariates Specific Year Trend		Yes		
Spatial FE		Census Tract		

Note: this table shows estimation results of equation 1 with different sub-samples. It shows: (1). estimations of the impacts of incidents that related to fatality or injuries; (2). results of incidents causing public evacuations, explosion, or ignition; (3). estimation of the potentially differential impacts due to whether an incident occurred on privately owned land or public land; (4). the cumulative impacts of multiple incidents on housing values. All the estimations are based on column (4) in table 2. Standard errors in parentheses.

∗ ∗ ∗:statistically significant at 1% level.

∗∗: statistically significant at 5% level.

∗: statistically significant at 10% level.

Table 4: Difference-in-Differences Estimation Results: Placebo Test

	DDD (1)	DID (2)
$Post \times Treat \times Incidents$	$-.0580^{**}$	
	(.0257)	
$Post$	-.0232	-.0161
	(.0182)	(.0231)
$Treat$	-.0064	-.0069
	(.0101)	(.0101)
$Post \times Treat$	.0035	.0031
	(.0102)	(.0105)
$Incidents$	$-.1010^{***}$	
	(.0271)	
$Post \times Incidents$	$.03848^{***}$	
	(.0218)	
$Treat \times Incidents$	.0243	
	(.0323)	
Covariates	Yes	Yes
Neighborhood Charact.	Yes	Yes
Year-Quarter FE	Yes	Yes
Pipeline Incidents FE	Yes	Yes
State Linear Trends	Yes	Yes
Spatial FE	Census Tract	Census Tract
Covaraites Specific Year Trend	Yes	Yes
# of Obvs.	75,850	56,331

Note: this table shows estimation results of equation 5 as well as equation 2. Column (1) provides the result of a triple-difference analysis by incorporating extra transactions farther away from each incident site. Column (2) shows estimation of applying only alternative treatment and control groups for our DID model. Model specifications are similar to column (4) in table 2. Standard errors in parentheses.

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

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

$*$: statistically significant at 10% level.

Table 5: Difference-in-Differences Estimation Results: Falsification Test

	500 days (1)	500 days (2)	1000 days (3)	1000 days (4)
$Post \times Treat$	.0326	.0344	$-.0044$	-.0026
	(.0359)	(.0298)	(.0273)	(.0232)
$Post$	.0310	.0551	.0437*	.0521**
	(.0345)	(.0305)	(.0251)	(.0253)
$Treat$	-.0067	-.0105	-.0041	$-.0119$
	(.0459)	(.0348)	(.0353)	(.0364)
Covariates	Yes	Yes	Yes	Yes
Neighborhood Charact.	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes
Pipeline Incidents FE	Yes	Yes	Yes	Yes
State Linear Trends	Yes	Yes	Yes	Yes
Spatial FE	Census Tract	Census Tract	Census Tract	Census Tract
Covaraites Specific Year Trend	No	Yes	No	Yes
# of Obvs.	15,711	15,711	20,386	20,386

Note: this table provides the result of a falsification test by artificially adjusting the occurrence date of incidents 500 days and 1,000 days earlier. The model specification of column (1) and (3) are based on column (3) in table 2 while column (2) and (4) are based on column (4) in table 2. Standard errors in parentheses.
$***$:statistically significant at 1% level.
$**$: statistically significant at 5% level.
$*$: statistically significant at 10% level.

Table 6: Robustness Checks: Propensity Score Matching - Difference-in-Differences

	(1)	(2)
$Post \times Treat$	$-.0646^{*}$	$-.0670^{*}$
	(.0365)	(.0357)
$Post$	.0262	.0451
	(.0405)	(.0456)
$Treat$	.0479	.0527
	(.0379)	(.0368)
Covariates	Yes	Yes
Neighborhood Charact.	Yes	Yes
Year-Quarter FE	Yes	Yes
Pipeline Incidents FE	Yes	Yes
State Linear Trends	Yes	Yes
Spatial FE	Census Tract	Census Tract
Covariates Specific Year Trend	No	Yes
# of Obvs.	9,827	9,827

Note: this table shows estimation results of one robustness check by incorporating a matching and regression method to improve balance between the treatment and control groups. Both estimations are based on 9,827 observations. The model specification of column (1) is the same as column (3) in table 2 while column (2) follows the column (4) in table 2. Standard errors in parentheses.
$***$:statistically significant at 1% level.
$**$: statistically significant at 5% level.
$*$: statistically significant at 10% level.

Table 7: Difference-in-Differences Estimation Results: Change Control Groups

	1000-2500 (1)	1000-3000 (2)	1000-3500 (3)	1000-4000 (4)
$Post \times Treat$	$-.0519^{**}$	$-.0394^{*}$	$-.0457^{**}$	$-.0428^{**}$
	(.0246)	(.0223)	(.0215)	(.0216)
$Post$	.0070	-.0005	.0113	.0116
	(.0210)	(.0202)	(.0174)	(.0159)
$Treat$	.0147	.0098	.0155	.0150
	(.0352)	(.0332)	(.0312)	(.0308)
Covariates	Yes	Yes	Yes	Yes
Neighborhood Charact.	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes
Pipeline Incidents FE	Yes	Yes	Yes	Yes
State Linear Trends	Yes	Yes	Yes	Yes
Spatial FE	Census Tract	Census Tract	Census Tract	Census Tract
Covaraites Specific Year Trend	Yes	Yes	Yes	Yes
# of Obvs.	30,662	44,169	59,692	77,386

Note: this table shows estimations of the robustness check that adopts different control groups in the DID model. Column (1) to (4) provides results of models with distance bandwidths from 1000–2,500 meter radius, 1,000–3,000 meter radius, 1,000–3,500 meter radius, and 1,000–4,000 meter radius, respectively. Model specifications are similar to column (4) in table 2. Standard errors in parentheses.
$***$:statistically significant at 1% level.
$**$: statistically significant at 5% level.
$*$: statistically significant at 10% level.

Appendix A: Additional Figures

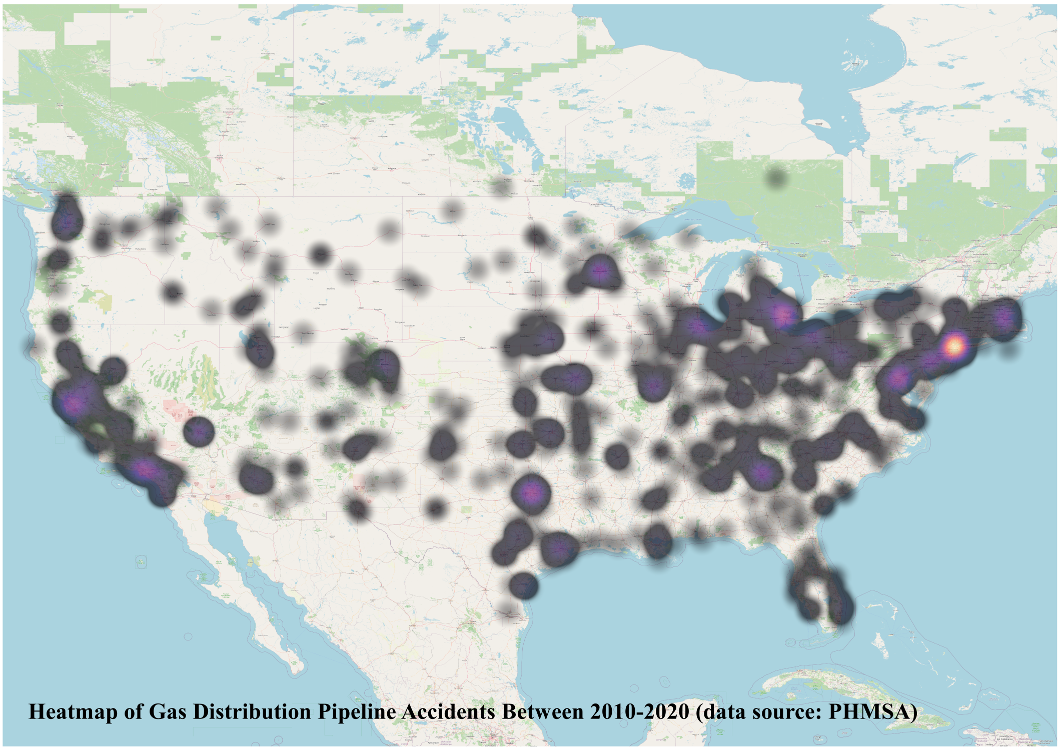

Figure A1: Heatmap of gas distribution pipeline incidents, 2010–2020.

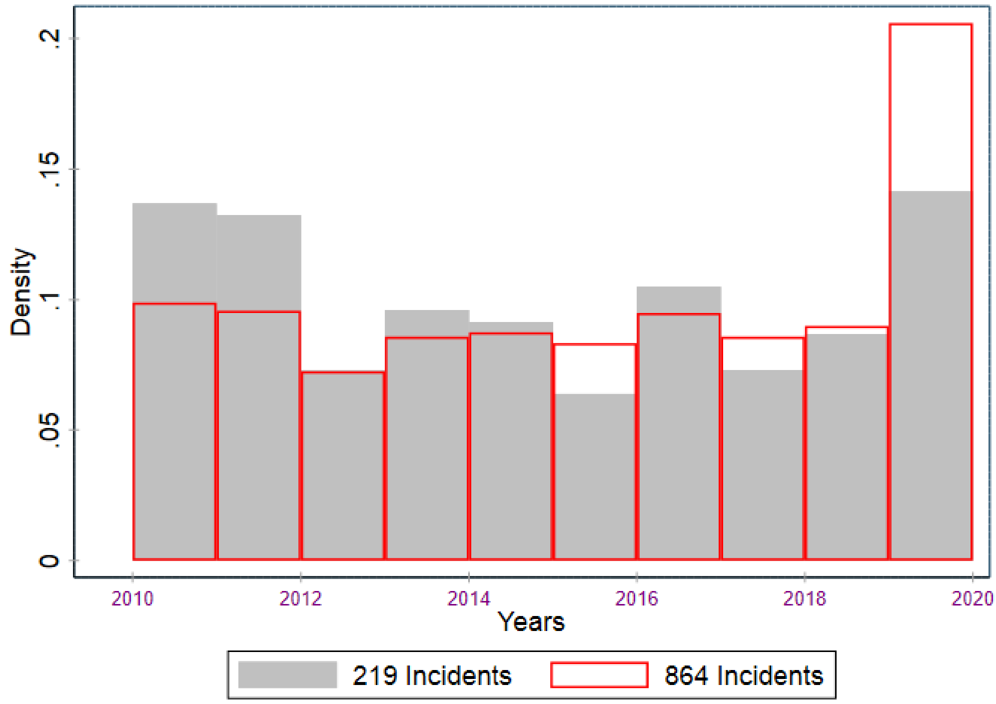

Figure A2: Distribution of gas distribution pipeline incidents, 2010–2020.

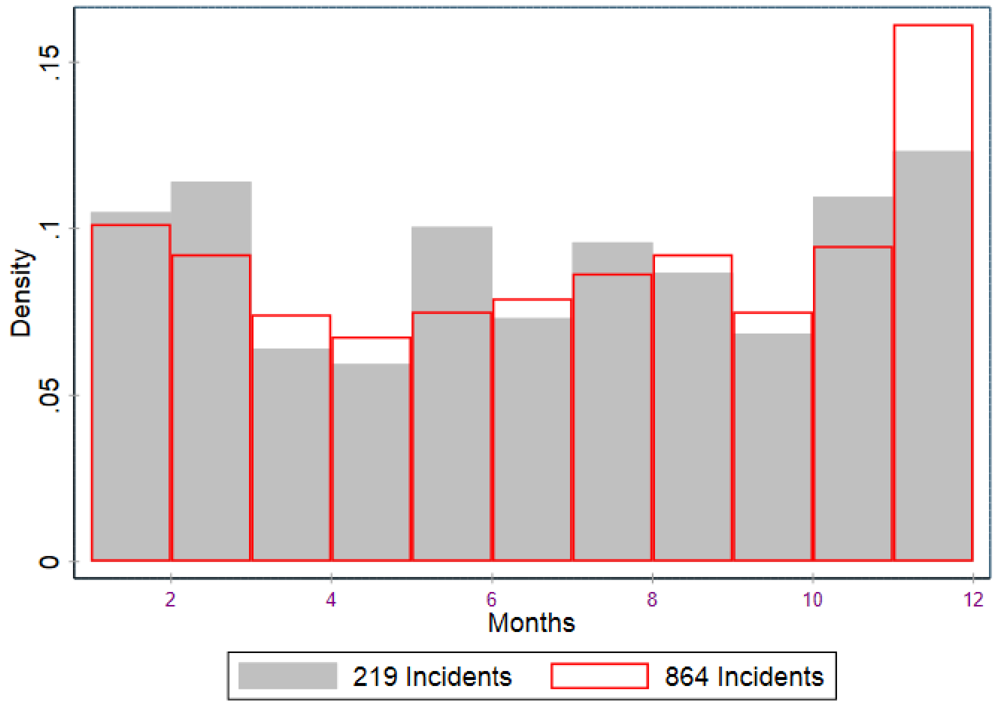

Figure A3: Distribution of gas distribution pipeline incidents by month.

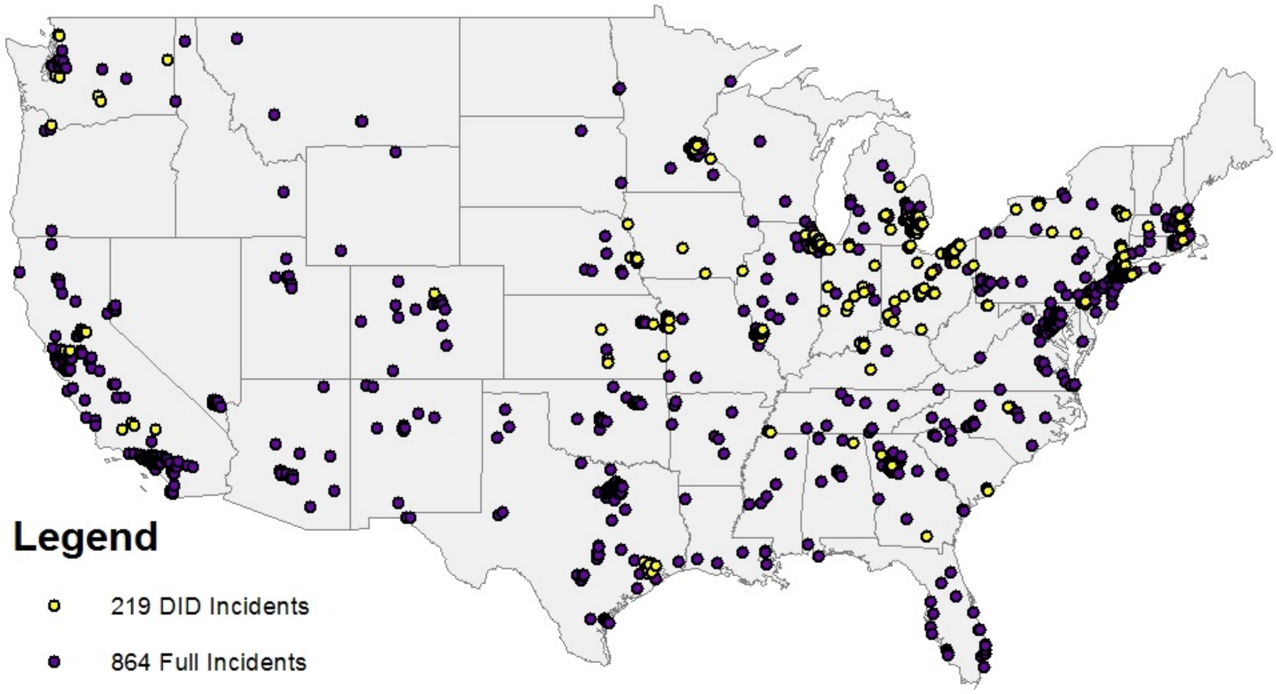

Figure A4: Gas distribution pipeline incident distribution comparison between 864 full and 219 DID incidents.

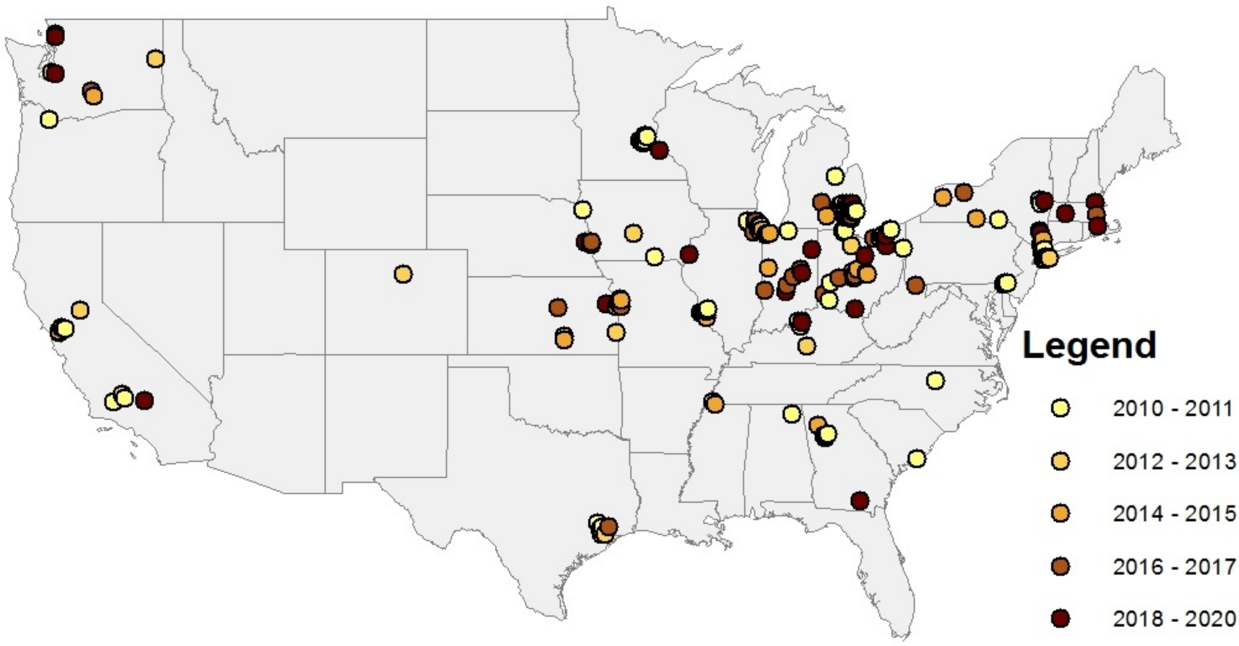

Figure A5: Gas distribution pipeline incident distribution for 219 DID incidents, 2010–2020.

Appendix B: Additional Tables

Table B1: Incident Attribute Comparison between 864 Full Incidents and 219 DID Samples

	864 Incidents			219 Incidents			Diff.
	Mean	SD	of Obvs.	Mean	SD	of Obvs.	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Fatality	.064	.244	864	.059	.255	219	.484
Injury	.220	.414	864	.224	.417	219	-.111
Public Evacuees	.520	.499	806	.575	.495	203	-.055
Explosion	.241	.428	851	.296	.457	219	-.055*
Ignition	.608	.488	864	.653	.477	219	-.044
Private Ownership	.440	.496	864	.447	.498	219	-.007
Aboveground	.377	.485	864	.388	.488	219	-.011

Note: The 864 incidents represent all gas distribution pipeline incidents in urban places we use in our binscatter regression, and the 219 incidents indicate the incidents we use in our DID regressions.

∗ ∗ ∗: statistically significant at 1% level.

∗∗: statistically significant at 5% level.

∗: statistically significant at 10% level.