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Contamination and Incomplete Information::

Bounding Implicit Prices using High-Profile Leaks

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Abstract

Incomplete information can lead households to underprice environmental disamenities in the housing market. To bound true implicit prices, researchers sometimes turn to high-profile cases involving significant media and community attention. However, prior research also finds that high-profile cases can lead to “stigma” effects that may confound interpretation of implicit prices. This study compares these opposing effects at the highest profile underground storage tank releases across the United States over the last thirty years. We utilize covariate matching and estimate difference-in-differences hedonic regressions at each site, and then conduct a cross-site meta-analysis to estimate the average treatment effects. We find an average housing price depreciation of 2% to 6% upon discovery of a release, which is an upper bound on the implicit price of contamination at more typical sites. Following cleanup, we find a housing price appreciation of a similar magnitude, suggesting that even in high-profile cases, surrounding neighborhoods do not experience persistent stigma.

Keywords

contaminated site; groundwater; hedonic; meta-analysis; property value; underground storage tank; UST

JEL Classification

Q24; Q51; Q53

1. INTRODUCTION

To inform policy, economists often turn to property values to reveal homeowners’ preferences regarding exposures to air, soil, and water contamination. However, incomplete information can lead households to underprice these disamenities in the housing market. Recent studies suggest that housing prices are affected by improved information and disclosure requirements (Pope, 2008a;2008b). At the same time, widely-available

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information can lead to an opposite stigma effect, manifested in persistent negative price impacts even after cleanup. Evidence for stigma effects is strongest around high-profile contaminated sites, and weaker at less-publicized sites (Kiel & Williams, 2007; McCluskey & Rausser, 2003; Messer et al., 2006; Taylor et al., 2016). Stigma can confound any property price appreciation resulting from cleanup, and might suggest that households overreact to “too much” information, or that shifts in demographics lead to a broader decline in neighborhood quality (McCluskey & Rausser, 2003).

To address the challenge of incomplete information, this study estimates the implicit price of contamination at high-profile sites, where information levels are relatively high and market participants are better informed. We estimate the implicit price of contamination at 16 of the highest-profile underground storage tank (UST) releases that have occurred in the United States during the last thirty years. Although previous studies have estimated hedonic models using data for select high-profile release sites in particular counties (Zabel & Guignet, 2012), our paper is unique in covering a broad set of high-profile UST releases across the U.S. These cases have garnered significant press coverage and public concern, and help assure that we are studying sites where ample information was readily available. As a result, homebuyers are more likely to be informed about the presence of contamination.

At the same time, at least some of these well-known cases span decades between the discovery of a release and final cleanup, similar to the conditions at the Superfund sites studied by Messer et al. (2006). If there is any post-cleanup stigma capitalized in housing values around UST releases, then we would expect to find it among the high-profile sites analyzed in this study.

There are over half a million active USTs in the United States, and almost two million that are no longer in use (US Environmental Protection Agency, 2012). USTs are often located at gas stations and industrial facilities. More than half a million of these active and inactive USTs have leaked chemicals into the environment (US Environmental Protection Agency, 2014). While leaking USTs can pose risks to human health and the environment (Jenkins et al., 2014), most releases are detected early and are not widely publicized. In these cases, information is sparse, especially among homebuyers.

Studies of other environmental disamenities suggest that sellers are better informed than buyers, as information regarding local disamenities is more readily available to those living nearby (Pope, 2008a;2008b). Prior hedonic case studies suggest that surrounding residential property values are not adversely impacted by a typical UST release, but do find evidence of significant declines in the value of homes near a subset of releases that received attention from the surrounding community (and in some cases the media) (Zabel & Guignet, 2012), and in cases where households were explicitly informed of the disamenity (Guignet, 2013).

Based on our unique dataset of high-profile incidents, our analysis uses a two-step methodology to minimize the influence of confounding price effects. Site-specific difference-in-differences (DID) hedonic regressions are first estimated, followed by an internal meta-analysis of the resulting estimates. In the first step, we use hedonic regressions to estimate how house prices change following two types of milestone events– the initial

discovery of the release and completion of cleanup. We control for pre-existing site-specific housing market trends by employing a DID framework that compares the price changes in neighborhoods near the release site against the price changes in neighborhoods further away. Prior to estimating the hedonic regressions, we further ensure the comparability of our control and treated groups by matching house transactions around each site using exact covariate and coarsened exact matching procedures (Blackwell et al., 2009; Iacus et al., 2012). We find considerable heterogeneity in how property values respond to both the discovery of a release and cleanup, sometimes appearing to cause very large property value changes, sometimes yielding negligible changes, and others times exhibiting counterintuitive effects.

The DID model used to estimate site-specific treatment effects relies on the assumption that in the absence of an UST-related event, the outcome of interest (in our case house prices) would have followed similar trends in both the control and treated groups (Gamper-Rabindran & Timmins, 2013), conditional on all observed characteristics, including the presence of uncontaminated gas stations. This assumption could be violated if unobserved influences on house prices are correlated with proximity to the site *and* the timing of events. Our diagnostic analysis of pre-event price trends suggests that for two sites in our sample, the estimated treatment effects could potentially be confounded by local trends in the housing market.

To address this concern, we synthesize the estimated property value impacts across the 16 sites using an internal meta-analysis. The meta-analysis combines the coefficients and statistical uncertainty from each hedonic regression to estimate an aggregate distribution of the percent changes in house prices. Analyzing multiple sites in a meta-analytic framework allows us to generate robust estimates of the average property value impacts (among high profile sites) and reduce the influence of local unobserved trends at any one site. Our 16 high-profile releases occurred in dispersed housing markets over a 30-year time period. This spatial and temporal variation in data for the release discovery and cleanup events allows us to use meta-analysis to reduce idiosyncratic biases associated with any individual site, lending greater confidence to a causal interpretation of the estimated average price effects.

Overall the results suggest that households' revealed values towards environmental risks appear rational in this information-rich setting. During the five-years following the discovery of a release, on average, the value of houses as far as 2 or 3 km from the site decrease by 2% to 6%. This represents an upper bound on the implicit price of contamination from a more typical UST release and levels of contamination. During the five-years following cleanup, property prices rebound by an average of 4% to 9%, suggesting that there is no lingering stigma after the threat is eliminated, even in the information-rich setting provided by the high-profile cases.

2. EMPIRICAL STRATEGY

To identify candidate high-profile release sites we consulted EPA's Office of Underground Storage Tanks (OUST), all ten EPA Regional Offices, state and local environmental agencies, and the Association of State and Territorial Solid Waste Management Officials

(ASTSWMO). We supplemented these efforts with internet searches and by reviewing ASTSWMO (2012) and relevant academic literature.¹ We define “high-profile” releases as those that received significant attention from the media and/or the surrounding community, and used a consistent set of guidelines for identifying such releases (see Appendix A). Public attention in many of these cases originated in part from overt activities to address contaminated drinking water, including connecting households to new water sources, distributing bottled water, or installing filtration systems. In some of these cases health advisories were issued. In others, attention stemmed from accumulations of combustible vapors. Many of the high-profile cases involved human health risks and/or large and severe contamination plumes.

Our objective was to obtain comprehensive spatial coverage across the contiguous United States. Figure 1 maps the 40 high-profile sites we identified in 23 different states.² Ideally, all 40 sites would have been included in this study, but for a variety of reasons 24 were excluded. Some sites in industrial and rural areas clearly had too few residential transactions for statistical analysis. Release events in three cases were too recent or distant in time to align with available property transaction data. Finally, in 13 cases, a variety of miscellaneous factors constrained the availability of required data; for example, state disclosure laws sometimes prohibited the release of property transaction data.

We located sufficient property transaction data to examine 16 high-profile UST release sites located in 10 states (see Figure 1). A comparison of socio-economic characteristics of the communities surrounding the 16 included sites versus the 24 that were excluded from the study suggests some differences. For example, based on data from the 2000 US Census, we find that, on average, communities around included sites tend to have a higher median annual income (\$57,160 versus \$34,761) and population density (1,316 people per square mile versus just 332 people), and a lower percentage of blue collar workers (19% versus 28%).³ Given our preference for areas with a sufficient number of residential housing transactions for statistical analysis, these differences are not surprising.

Table I explains the nature of contamination at all 16 included sites, some of which had multiple concerns. At nine sites, gasoline contaminated the groundwater in private drinking water wells, affecting dozens to hundreds of nearby homes. Three sites involved contamination in public water wells, affecting entire towns. Contaminated water poses health risks from ingestion, skin contact, and inhalation of fumes, all of which can cause neurological damage, blood disorders, cancer, and other adverse health outcomes (Jenkins et al., 2014). At six of the sites, combustible vapors migrated to occupied structures, posing acute risks of fire or explosion. At one site, contamination impacted surface water. Methyl tertiary butyl ether (MTBE) (a potentially harmful gasoline additive) was a concern at nine of the 16 sites.⁴ Almost all the releases were at retail gas stations, except one at an industrial facility.

¹See Guignet et al. (2016) for details.

²See Appendix A, Table A. 2 for a list of all 40 high-profile release sites identified, and some descriptive statistics of the surrounding community.

³Two-sample t-tests suggest that these differences are statistically significant, with p-values < 0.05.

2.1. Hedonic Property Value Model

In the first stage of our analysis we estimate separate hedonic regressions for each high-profile site, denoted by $s = 1, \dots, 16$. The objective is to estimate the short-run capitalization effects associated with the discovery of a release and completion of cleanup. Based on several considerations, we only include transactions up to five years before or after these events (or less if transaction data are not available or another milestone takes place sooner). This period is long enough to (1) capture the housing market's response to contamination and cleanup events, (2) observe a sufficient number of house transactions for statistical analysis, and (3) allow for a comparable number of post-event years at most sites (see Figure 2). At the same time, this period is short enough that it reduces the potential for unobserved temporally correlated confounding factors.⁵

We restrict the residential transaction data around each site to ensure comparability across the site-level datasets, and to provide a fairly homogenous set of house transactions and facilitate a cleaner quasi-experiment. We focus on transactions of single-family homes and townhomes within 10 km of each site. Additionally, attention is restricted to similar homes using a combination of exact covariate matching and coarsened exact matching (CEM) techniques (Blackwell et al., 2009; Iacus et al., 2012). CEM involves first dividing the relevant continuous attribute space into discrete bins, and then matching homes that fall within the same set of discrete attribute bins (i.e., have the same value for the “coarsened” attributes). Our matching procedure matches homes in the treated group (i.e., homes within some distance δ of a high-profile site) to the control group (i.e., homes located δ to 10 kilometers from that same high-profile site). The distance δ is determined based on the data, as described in section 4.1. The matches are based on exact covariate values in terms of the transaction year and number of gas stations within 500 meters, and the coarsened values corresponding to the number of bathrooms and parcel acreage.⁶ Both treated and control transactions are dropped from the estimating sample if they are not matched to a home simultaneously based on all exact or coarsened matching covariates. A weight of one is then given to all treated homes that could be matched to at least one control home. A treated home can potentially be matched to more than one control home, and one control home can potentially be matched to more than one treated home, and so all maintained control homes are given a positive weight that may be less than or greater than one. The hedonic regressions discussed next are then estimated using this matched sample weighting scheme.

We believe such matching methods are particularly advantageous in the hedonic property value context, where there are numerous important attributes defining a housing bundle, and thus where it is often impractical to match on all key attributes. At the same time, alternative

⁴MTBE is a contaminant and suspected carcinogen that was previously used as a gasoline oxygenate to reduce air emissions. Some states have set limits for MTBE in drinking water and EPA issued a drinking water advisory for it in 1997. MTBE has generally not been used as a gasoline additive since the early 2000s (Jenkins et al., 2014), and so it is often associated with older, sometimes previously unknown, releases.

⁵One could estimate the capitalization effects simply using all the data, no matter how many years of post-event transaction data are available, but comparing capitalization effects estimated from time periods of significantly different lengths (sometimes as much as 10 or 20+ years, compared to just 2 or 3 years) would be inappropriate.

⁶The coarsened attribute values or bins are 0 baths, 1 bath, 2 baths, 3-5 baths, and more than 6 baths, and acreage is binned using the default statistical-based binning algorithm following Sturge's rule (Blackwell et al., 2009). Exact matches for missing dummies for acres and baths are also included (when applicable).

matching algorithms can be used to reduce the dimensionality to a single measure (e.g., Mahalanbois metric, or propensity score), but may overly-generalize key details. Due to its advantageous, CEM has been recently utilized in hedonic property value studies (Groves & Rogers, 2011; Qiu et al., 2017).

Previous literature indicates that the impacts of contamination vary with distance from the site. We sort houses into discrete one-kilometer distance bins, using functional forms similar to past hedonic studies (Gamper-Rabindran & Timmins, 2013; Zabel & Guignet, 2012). We allow potential price impacts to extend out to 5 kilometers (km), and thus define the treated group as houses within $\delta = 5$ km of the site.⁷ Houses outside of 5 km from the high-profile site entail the “control” group, and help identify broader housing market trends. The functional form of the hedonic models appears in equation (1).

$$\ln p_{ijts} = x_{ijt}\beta + \sum_{d=1}^5 \left(\psi_s^d D_{is}^d + \theta_{se}^d \{event_{tse} \times D_{is}^d\} \right) + \mathbf{M}_t\alpha + \nu_j + \varepsilon_{ijts} \quad (1)$$

The dependent variable $\ln p_{ijts}$ is the natural logarithm of the price of house i in neighborhood j at time t near high-profile site s .

The control vector x_{ijt} contains house structure, parcel, neighborhood, and other control variables, including the number of gas stations within 200 and 500 meters of the house. Controlling for nearby (uncontaminated) gas stations is particularly relevant because it helps us isolate land use effects that would otherwise bias the price impacts of environmental contamination and cleanup (Taylor et al., 2016). The vector $\mathbf{M}_t$ contains dummy variables denoting year and quarter fixed effects, ν_j is a neighborhood fixed effect (at the census tract level), and ε_{ijts} is a normally distributed zero-mean error term.

The distance bins are represented by the dummy variables D_{is}^d , for $d = 1, \dots, 5$ km.

Coefficients ψ_s^d account for differences in housing bundles at the various distances from the high-profile site. The distance bin dummies are also interacted with $event_{tse}$ – a dummy variable equal to 1 for transactions that occur after milestone event e (i.e., release discovery or cleanup completion) at UST site s , and 0 otherwise.

Due to concerns that the hedonic equilibrium might shift over longer time periods (Kuminoff et al., 2010), in one case we split the transaction data into separate datasets and estimate individual hedonic regressions for each event. However, in two cases a site experienced two high-profile events close together in time, and in such cases the regressions include a separate set of variables denoting each event, and both events are included in the same regression. This was done to maintain a sufficient number of transactions to identify the numerous other parameters not of primary interest, while at the same time controlling for

⁷See section 4.1 for details.

earlier release events that would otherwise confound the estimated treatment effects corresponding to the subsequent cleanup events.

Houses within $\delta = 5$ km from the site that are sold before high-profile event e are considered the “treated group before the treatment”, and so ψ_s^d captures these baseline effects (which are allowed to vary across the one-kilometer bins, $d = 1, \dots, 5$ km). Houses within 5 km that are sold after event e are the “treated” groups, with θ_{se}^d capturing the post-event price differential. All else equal, θ_{se}^d is the average treatment effect on the treated, which again is allowed to vary with distance from the site.

Since the dependent variable is in logs and the event-distance interaction ($event_{tse} \times D_{ts}^d$) variables of interest are binary, we calculate the percent change in price as:

$$\% \Delta p_{se}^d = (\exp(\theta_{se}^d) - 1) \times 100 \quad (2)$$

2.2. Internal Meta-Analysis

We use meta-analysis to synthesize and compare the estimated price impacts from equation (2) for the 16 different sites. First, we calculate the average impacts for release discovery and cleanup across the different sites. Second, we use meta-regressions to investigate the determinants of price impacts across sites. Importantly, this allows us to study the effects of discovery and cleanup, and how those effects vary with site-specific characteristics, such as contamination type and exposure pathway.

Meta-analysis has become an important tool for synthesizing work from multiple studies and models. Notable meta-analyses in environmental economics include applications in air quality (Smith & Huang, 1995), water quality (Johnston et al., 2005; Van Houtven et al., 2007), and land contamination at Superfund sites (Kiel & Williams, 2007; Messer et al., 2006).

A common concern with meta-analyses that aggregate estimates across different studies is whether the effects under examination are comparable measures and estimated in a consistent fashion (Nelson & Kennedy, 2009). Here we estimate the primary hedonic regressions directly, ensuring the inputs to our meta-analysis are calculated using the same methodology and functional form, and controlling for the same housing and neighborhood attributes.

Our first meta-analytic step is to estimate average price effects across sites. Average impacts are estimated for each one-kilometer distance bin, after the release discovery and cleanup completion events. We focus on these two milestones because of their importance and commonality across many sites. Following best practices (Nelson & Kennedy, 2009), we calculate a weighted average of the post-event percent changes in price ($\% \Delta p$) from equation (2), where the weight given to each observation is based on the inverse variance of the primary estimate from the hedonic regressions; thereby giving greater weight to more

precise estimates. The chosen weighting scheme follows the random effect-size (RES) model (Nelson, 2015), which presumes that each estimate of % p is a random draw from a *different* underlying distribution of the true price effect at each site. Although we are allowing the true price effects to differ across sites, in calculating the RES mean of % p we are averaging these different price impacts.⁸

The second step in our meta-analysis is to examine price effect heterogeneity across different sites, distances, and both milestone events using meta-regressions. The meta-regression model appears in equation (3).

$$\% \Delta \hat{p}_{se}^d = \gamma_1 EVENT_{se} + \gamma_2 (EVENT_{se} \times DIST_d) + u_{se}^d \quad (3)$$

The dependent variable observations are the percent changes in price estimated from equation (2).⁹ The vector $EVENT_{se}$ includes two dummy variables denoting the two different milestone events – the discovery of a release and completion of cleanup. The variable $DIST_d$ ranges from 1 to 5, and denotes distance buffer d corresponding to the estimate from the hedonic equation (1). Equation (3) is estimated using a meta-regression RES model (Borenstein et al., 2010; Nelson & Kennedy, 2009).

3. DATA

Our property data include house-level transactions for each of the 16 locations, usually the county where the high-profile release occurred.¹⁰ These data were obtained from states, counties, and municipalities, and include detailed information on house and parcel attributes such as age, number of bathrooms, interior square feet, and lot acreage. The transaction data begin as early as 1980 (for three Florida counties), and end as late as 2014 (for Chittenden, Vermont). We use Geographic Information Systems (GIS) to calculate the distance from each house to nearby gas stations, urban centers, major roads, and most importantly, the corresponding high-profile UST site. To learn more about each release, including dates of milestone events, we reviewed press articles and relevant association, EPA, and state environmental department reports.

Figure 2 illustrates the years covered by the property transaction data and the dates of the milestone events. For example, we have transaction data spanning 2005-2013 for Los Angeles County, CA, where UST contamination affected nearby public water wells, causing widespread impacts. There was a single milestone within the available transaction data timeframe – cleanup completion in February of 2011.

⁸In contrast, the fixed effect-size (FES) model assumes that there is a common, shared mean (or effect-size) among all sites (Nelson, 2015; Borenstein et al., 2010). Under the FES model, each estimate of % p from the different sites would be viewed as a random draw from the *same* underlying distribution of a common true price effect. We adopt the RES model here. Due to differences in housing markets, populations, leak origin and extent, publicity, and many other factors, there is no reason to suspect that the underlying true price effects at each high-profile site are the same.

⁹There are 19 milestone events across the 16 sites. Multiplying the 19 milestones by the five distance bins yields a total of 95 meta-observations.

¹⁰One county, Suffolk County, NY, was the location of two high profile releases (Northville Industries and Smithtown Exxon-Mobil).

There were sufficient transaction data to examine 19 milestones across the 16 high-profile sites. As illustrated in Figure 2, most sites have only one event occurring within the available transaction data timeframe, but at three sites both release and cleanup are observed. Table II shows the observed number of release discovery and cleanup milestones, and offers some examples of events we classified as such.

A full description of the timeline of activities at each of the 16 high-profile sites is provided by Guignet et al. (2016). To give a better sense of the data, we describe a single high-profile release site in detail here. While no individual high-profile site is completely representative, we selected the Green Valley Citgo case in Monrovia (Frederick County), Maryland because it provides an example of the most common milestone event (the discovery of a release) and most common exposure pathway (contamination of private drinking water wells).

In 2004, a gasoline release was discovered by the Maryland Department of Environment (MDE) at the Green Valley Citgo gas station. MTBE was detected in the drinking water supply well that serviced a nearby shopping center. This initial detection was identified in our data as the “Release Discovered” event. Nearby residents relied on private potable wells, so their drinking water supply was also threatened. Groundwater monitoring wells were drilled, and over the next few years MTBE and benzene were detected at concentrations above the state action levels. In response, the water supply well was closed and filtration systems were installed at other nearby groundwater wells.

In 2007, MDE directed the County Health Department to mail letters to all residents located within a half mile of the gas station, notifying them of the contamination. In 2008, the UST system and contaminated soil were removed. New UST and treatment systems were installed. Our data for this county only includes transactions through 2009. Although we are unable to examine subsequent events, it is worth noting that by April 2010 over 200 private drinking water wells had been sampled, of which six were contaminated with MTBE levels above the state action level. The six residences received filtration systems and subsequent monitoring. As of 2015, the filtration systems remained in effect at these wells, and the case remained open, with natural attenuation and ongoing groundwater monitoring in place.

Table III includes descriptive statistics of all sales used in the hedonic regression for the Green Valley Citgo site. The statistics only reflect data used to estimate the regression; specifically, of our matched sample of transactions, consisting of single-family and town houses within 10 km of the release site and within the five-year period prior to or following the discovery of the release. The presented descriptive statistics are also weighted according to the matching algorithm. We have listed all variables that were used in the regressions for any of the 16 high-profile sites, even though some of these variables were not available for every location.

4. RESULTS

4.1. Determining the Spatial Extent

We establish the appropriate spatial extent of the price effects following an approach similar to Linden and Rockoff (2008), and later adapted by Muehlenbachs et al. (2015). We use

local polynomial regressions to non-parametrically examine the housing price gradients around each high-profile site before and after its most prominent observed milestone. As an example, consider again the Green Valley Citgo case in Frederick County, MD, where we observe the discovery of a release in 2004.

To minimize potentially confounding price effects over time, for this preliminary exercise we focus on sales that occurred only two years before or after the event. We maintain the spatial limit of 10 km. To further control for broader market trends over time we “de-trend” the prices around each site.¹¹ The data are then separated into two groups – sales before versus after the milestone – and plotted against distance to the site. Two curves are fitted using local polynomial regressions to depict the pre- and post-event price gradients. As shown in Figure 3 for the Green Valley Citgo example, at greater distances the price gradients are relatively similar, suggesting that the milestone event had little effect. However, at least in this example, we see that the prices of houses closest to the site were noticeably lower after the event. This price differential disappears at around two kilometers.

We applied this same exercise separately to each of the 16 high-profile sites, and found considerable variation in what the appropriate spatial extent of any price effect might be.¹² In all cases, however, we did not find evidence that the price impacts extend beyond 5 km and therefore chose that as the appropriate distance for the subsequent hedonic analyses. To ensure a representative meta-dataset that is comparable across sites, in our primary analysis we estimate the price effects out to the same five-kilometer distance for all sites, even if these price effects sometimes turn out to be zero. Doing so avoids any self-selection bias in the subsequent meta-analysis because we include all estimates for the farther distance bins, not just those where a significant effect was found.

4.2. Difference-in-Differences Diagnostics

In an ideal DID quasi-experiment, a comparison of the price time trends across the treated and control groups should reveal two things. First, often referred to as the “common trends” assumption (Angrist & Jorn-Steffen, 2009), we should see that prices of the treated and control group homes follow relatively similar (or parallel) trends prior to the treatment (e.g., the discovery of a release), and then diverge afterwards, if there is in fact a noticeable treatment effect. Second, the price trends of the control group should be relatively similar both before and after the treatment event, all else constant.

For each high-profile site and study period, we estimate a hedonic regression that includes the same set of covariates described in equation (1), but excludes the interaction terms with respect to the treatment events (release discovery and cleanup). Following the semi-parametric procedure outlined by Haninger et al. (2017), we take the estimated residuals and use local polynomial regressions to visually depict how price trends over time differ across

¹¹The de-trended prices are calculated by first estimating a linear regression of price on a series of year dummy variables (with the first year omitted), and then adding the residuals back to the constant term.

¹²Estimating the price gradients around just a single site and event in this fashion makes the curves potentially more susceptible to confounding price factors that may be associated with a particular locale and time period (compared to situations that allow for an examination of multiple sites within a single housing market, as done by Linden and Rockoff (2008) and Muehlenbachs et al. (2015)). Nonetheless, this exercise is still informative, and we argue that such noise is reduced by our subsequent hedonic regression analyses, and further minimized in the internal meta-analysis, where we do then analyze across multiple sites.

the treated and control groups, without explicitly controlling for the timing of the treatment events. For purposes of this diagnostic exercise, treated houses are defined as those within 0-2 km from the high-profile site, and control houses are those in the 5-10 km buffer. The 0 to 2 km radius is chosen based on previous findings in the literature (Zabel & Guignet, 2012) and the diagnostic exercise described in section 4.1. If a measurable price impact exists, it is in this 0-2 km bin where we would expect to observe it. This assumption is later relaxed in our primary hedonic regression analysis and meta-analysis, where the price effects are allowed to vary for each one-kilometer bin.

First consider the 12 high-profile sites where just release discovery is observed within a study period. As displayed in Figure 4, among 10 of the 12 cases we see that the pre-discovery price trends are fairly similar, often with the 95% confidence intervals largely overlapping for most (if not all) of the pre-discovery period. In some cases (e.g., the Bithlo and DeLeon Springs sites in Florida), we do see a discrete drop in treated group home prices after the discovery of a release, but this is not the case among many of the sites, suggesting that if there is a price decrease post-discovery then it may be relatively small. In contrast, we see that the control group price trends are often fairly stable across the pre- and post-discovery threshold, at least relative to the treated group. At two sites, however, the control group prices also appear to shift post-treatment – the LaSalle site in Colorado and the Palm City Cluster in Florida. These same two sites exhibit evidence that may lead one to question the “common trends” assumption prior to treatment, especially at the Palm City Cluster in Florida. Figure 6 includes two additional sites where release discovery and cleanup were observed within the same study period. Focusing on release discovery, we again see that the “common trends” assumption is likely reasonable for these two sites.

Next consider the five sites where cleanup completion is observed, as shown in figures 5 and 6. When focusing on cleanup completion, there is no reason to suspect that the “common trends” assumption prior to cleanup holds. By definition, the control and treated group prices may already be different if there was a noticeable impact from the preceding release. Nonetheless, these graphs are insightful as they demonstrate that the second condition for a valid quasi-experiment does hold. In all five cases, the control group price trends seem relatively constant across the cleanup event. Although we do not always see a discontinuous jump among the treated group prices upon completion of cleanup, we do often see some divergence, often with the treated group prices starting to diverge and increase prior to completion. This is not surprising because other cleanup activities may be observed leading up to the final cleanup completion milestone.

In summary, our semi-parametric diagnostic examination of the price trends suggests that when considering the 16 high-profile release sites (and the corresponding 14 release discovery events and 5 cleanup completion events), in only two cases did the data provide evidence that local factors around that site may confound the comparison. In such cases, causal inferences may not be valid, at least not when considering either site in isolation.

In general, we caution against drawing firm conclusions from the results for any single site, particularly because at some sites there are a small number of identifying transactions within some of the one-kilometer distance bins. Nonetheless, we argue that our meta-analysis as a

whole provides a valid quasi-experimental setting that yields estimates of the average treatment effects on the treated that can reasonably be interpreted as causal.

Visual inspection of the semi-parametric price trends depicted in figures 4, 5, and 6 suggest no systematic pre-treatment differences in the treatment and control groups that would invalidate the DID assumptions across all sites and treatment events. Many hedonic property value studies have employed a similar DID approach where the treatment group consists of houses within a certain proximity of a disamenity, and the control group consists of more distant houses. For example, Linden and Rockoff (2008) examine how neighboring house values respond to a registered sex offender moving into the neighborhood. Haninger et al. (2017) examine how cleanup of a brownfield impacts surrounding house values. Both studies estimate hedonic regressions that include over 150 “sites” (i.e., residence of a sex-offender, or a brownfield) where the respective treatments took place, allowing them to exploit both spatial and temporal variation in the “treatments”. Both studies provide similar diagnostic evidence that a DID interpretation of their results is appropriate.

Part of the reason these studies are so compelling is because they had the advantage of examining multiple sites within a single hedonic regression. We speculate that if one examined the pre-treatment price trends at homes around the location of a single registered sex-offender or brownfield, then surely at some individual locations the “common trends” assumption would not hold, and thus a DID interpretation would not be appropriate for that individual site if examined in isolation. However, when considering numerous sites at once, their analyses as a whole provide credible DID settings.

In that same vein, we argue that by examining multiple sites, our subsequent internal meta-analysis provides a reasonable quasi-experimental setting. We have no a priori expectation that any biases across sites for an estimated treatment effect should fall in one direction or another. An expectation that is confirmed by the above semi-parametric diagnostic exercise. The 16 high profile sites all exist in different property markets and the milestone events are spaced widely over time. Therefore, any idiosyncratic biases are reduced when estimating the average treatment effects in our meta-analysis (Nelson & Kennedy, 2009). In addition, pooling sites in the meta-analysis improves statistical efficiency of our estimates by averaging across primary estimates, some of which may be imprecisely estimated due to few identifying transactions in the initial hedonic regressions.

Alternatively, we could have estimated a single hedonic regression that pools the data across all study periods and locations, similar to the nationwide hedonic analysis by Haninger et al. (2017), but in our context it would be inappropriate to make the necessary assumption that these 16 spatially and temporally disjointed markets constitute a single hedonic market. These 16 sites are in 15 different counties scattered across the US, and the study periods cover various intervals of time over more than three decades. For these reasons, we estimate separate hedonic regressions for each site and then synthesize the results using meta-analytic techniques.

Analogous to the aforementioned site-specific exercise, Figure 7 displays the meta-analytic counterpart to what is often referred to as an “event analysis” (Hanna & Olivia, 2010). We

exploit the panel structure of the metadata, taking advantage of the fact that the treatment (the discovery of a release in this case) occurs at different locations and points in time. Considering all 14 release discovery events, Figure 7 displays the synthesized price trend differential between “treated” houses (again assumed to be within 0 to 2 km from a site for purposes of this diagnostic exercise) and “control” houses (5 to 10 km from a site).¹³ The confidence intervals are fairly wide, mainly due to the small sample sizes when dividing transactions into one year increments in the underlying regressions that generate this exploratory graph.

Nonetheless, the near zero and statistically insignificant price differences in the left portion of the graph suggest that, on average, there is no significant differences in price between the treated and control groups prior to a release being discovered. Zero falls well within the 95% confidence interval and half of the point estimates land just slightly below the x-axis. This similarity in the pre-treatment price trends is at least consistent with the idea that the meta-analysis as a whole provides a valid quasi-experimental setting, even when such an interpretation may not be appropriate among one or two of the individual study locations in the primary hedonic analyses. We emphasize that the purpose of this exercise is to produce a visual test of the appropriateness of the necessary DID assumption that the control group reflects a reasonable counterfactual.

The point estimates in Figure 7 for the years following release discovery remain below the x-axis for all the post-discovery years, but these differences are not statistically indistinguishable from zero. This suggests that there may *not* be a measurable average treatment effect. That said, these suggestive results could also be an artifact of the data limitations when trying to perform a less restrictive diagnostic exercise such as this. The hedonic regression analyses discussed next allow us to impose additional structure, and thus more thoroughly investigate any post-treatment price effects.

4.3. Hedonic Regression Results

Following equation (1), we estimate separate hedonic property value regressions for each study area using the CEM matched sample and weights. As explained in Section 2.1, the estimated price changes can be interpreted as relatively short-term effects since we only include transactions up to five years after an event (or less if transaction data are not available or another milestone takes place sooner). The hedonic regressions include census tract fixed effects, annual and quarterly time dummies, and an extensive suite of attributes describing the housing structure, parcel, and location. The control variables include a fairly consistent set of attributes across the different sites (see Table III).

Following equation (2), Table IV shows the estimated percent changes in price after each event ($\% \Delta p$), for each high profile site and one-kilometer treatment buffer. There is noticeable heterogeneity among the 95 estimates of $\% \Delta p$, in terms of sign, magnitude, and statistical significance. This is not surprising, given the differences in distance and milestone events. Note that a repeated model number in the first column of Table IV indicates instances where multiple events are included in the same hedonic regression.

¹³See Appendix B for details.

Considering our example Green Valley Citgo site, Table IV shows that the discovery of the release led to small, but statistically significant, declines of 1.9% and 1.5% at houses in the 0 to 1 km and 1 to 2 km bins, respectively, and even smaller and insignificant estimates for the further three bins.

Several other sites yield intuitive results, with initial property price declines upon the discovery of a release (e.g., the LaSalle and Jacksonville Exxon sites, and the Henderson Corner Pantry), and an increase in prices after cleanup (e.g., Charnock Well Fields, LaSalle, and Northville Industries). At other sites there are no significant effects on property values (e.g., Bithlo site), or mixed and even counterintuitive results (e.g., Montvale and Tuckahoe sites).¹⁴

To summarize and better understand the results, first consider the discovery of a release and the estimated % p estimates displayed in Figure 8, which corresponds to houses in the 0 to 1 km bin for each of the 14 sites where we observed release discovery. The majority of the point estimates are negative or close to zero, but there is a wide range, from a 34.5% decline to a 6.6% (marginally significant) increase. The meta-data reveal significant heterogeneity in the price impacts of release discovery across the different sites ($\chi^2(13) = 250.59$, $p = 0.000$). We estimate the corresponding RES mean across the 14 included sites. As shown at the bottom of Figure 8 the overall RES mean is a significant 5.0% depreciation ($p = 0.008$).

Table V shows the RES mean effects of release discovery for each one-kilometer bin. Beyond one kilometer the discovery of a high-profile UST release (on average) has no significant effect on house values. We emphasize that this is an average effect, and within each of the one-kilometer bins the meta-data suggest statistically significant heterogeneity across the high-profile sites ($p = 0.01$). The unweighted means for which each estimate is given equal weight are also shown for comparison, and suggest significant negative impacts extending out to 3 km. However, we prefer the RES mean estimates for the reasons discussed in section 2.2.

At five sites the data allowed us to examine the price impacts after cleanup completion. Considering price impacts in the 0 to 1 km buffer, for example, we found that house prices appreciated at all but one of the sites after cleanup. This appreciation, however, was only significant at three of the five sites. Nonetheless, the overall RES mean suggests a 5.3% increase after cleanup). The unweighted mean suggests a similar average appreciation post-cleanup in the 0 to 1 km bin.

The RES mean results in Table V show that the average increase in prices post-cleanup extends to houses in the 1 to 2 km buffer, with a 7.5% appreciation. There is weak evidence of an average 4% appreciation extending out to the 2 to 3 km and 3 to 4 km buffers ($p = 0.144$ and $p = 0.011$, respectively). As was the case for release discovery, for each one-kilometer bin we find statistically significant heterogeneity in the impact of cleanup completion across the high-profile sites ($p = 0.05$).

¹⁴For some of these sites, such as the Bithlo site, additional caution is warranted in interpreting the results due to a low number of identifying observations within each of the 1 km bins (see Guignet et al. (2016)).

Finally, we examine the sensitivity of the RES mean estimates of release discovery and cleanup completion to the exclusion of any given site. This is done to confirm whether the results are sensitive to the inclusion of a potential outlier site, and in particular to the inclusion of either of the two sites where pre-discovery price trends suggest that the “common trends” assumption may not necessarily hold. The results are presented in Appendix C. Briefly, the estimated RES mean impacts of release discovery are remarkably robust, this includes when estimates from the LaSalle site in CO or the Palm City Cluster in FL are excluded. Overall, in the 0 to 1 km bin, the estimates for discovery of a release range from a 3.4% to 5.8% decrease in price, and are statistically significant across all 14 iterations. The estimated price declines in the 1 to 2 km buffer range from 0.6% to 2.1%, and as before are generally insignificant (or marginally significant, at best). The results corresponding to cleanup appreciation are also fairly robust in terms of sign and magnitude, but do move around a bit more compared to release discovery. This is not surprising since there are only five sites where cleanup completion was observed to begin with.

In the next section we use meta-regression techniques to more thoroughly examine these price effects and synthesize the results.

4.4. Meta-Regression Results

We pool all 95 estimates of % p from the primary hedonic regressions, estimate meta-regressions, and present the main results in Table VI. The first meta-regression model (Column 1) is a RES model following equation (3), where the right-hand side variables include a separate intercept for release discovery and cleanup completion. We also include a continuous variable denoting each one-kilometer bin from 1 to 5. This is interacted with both the release discovery and cleanup completion dummy variables to allow the price impact gradients to differ across the two milestones.

Following the discovery of a release, the results suggest a marginally significant 5.9% average price decrease to houses immediately adjacent to a high-profile site (i.e., $distance=0$). The magnitude of this decline diminishes with distance from the site, as suggested by the positive (albeit insignificant) coefficient on the interaction term $distance \times discovered$. We also see a positive price impact from the completion of cleanup, as suggested by the marginally significant 8.9% appreciation corresponding to the dummy variable *cleaned*. From the negative (but again insignificant) coefficient on $distance \times cleaned$, it seems that this post-cleanup appreciation also diminishes as distance from the site increases. It is difficult to interpret the magnitude and relation of these coefficients by themselves, and so figures 9 and 10 depict the price impact gradients based on these results. On average the discovery of a high-profile release and its subsequent cleanup impact property values in an intuitive fashion—prices decrease upon the discovery of a release, but then later increase after cleanup efforts are complete. A series of Wald tests suggest that the average post-cleanup appreciation is of an equal magnitude as the initial decrease, suggesting that property values, on average, fully rebound after cleanup. We note, however, that the analysis is limited to property value impacts within a relatively short five-year period, and does not represent any longer-term price effects that may be present. Furthermore, at only three of the

high-profile sites did we observe both the discovery of a release *and* the completion of cleanup.

Figures 9 and 10 also illustrate that the price effects diminish farther from the site, becoming statistically insignificant at a distance of about three kilometers. Although we impose a linear trend in the meta-regression specification, this is somewhat consistent with the weighted averages in Table V, where no functional form for the distance gradient was imposed. Model 2 in Table VI is a Random Effects (RE) Panel regression model, which is a recommended alternative when more than one estimate is taken from a primary study (Nelson and Kennedy, 2009), or in our case from a high-profile release site. The RE Panel model includes a random intercept for each high-profile site and allows the error terms to be correlated at the site-level. The estimates corresponding to the discovery of a release and cleanup are similar to the corresponding RES model (Model 1). Table VII shows the estimated percent changes in price based on the meta-regression coefficients. Comparing the price changes for each distance bin, we see models 1 and 2 yield very similar results, particularly within the closest distance bins. .

Meta-regression models 3 and 4 are the same as the preceding RES and RE Panel models, but are now estimated using a sub-sample that excludes meta-observations corresponding to the LaSalle and Palm City Cluster sites, both of which exhibited price trends that, when considered in isolation, suggest the “common trends” assumption may be inappropriate. As shown in columns (3) and (4) of Table VI, the exclusion of these sites yields milestone intercept estimates that are smaller in magnitude, but of a similar sign and, in some cases, statistical significance. The *discovered* $\times$ *distance* interaction terms are also similar, but the corresponding *cleaned* $\times$ *distance* interactions are smaller, suggesting a flatter price gradient with respect to cleanup. Comparing the % *p* estimates in Table VII, the price gradient with respect to release discovery is always the same sign, but about half of that suggested by Models 1 and 2, and are generally statistically insignificant (with the exception of homes immediately adjacent to a site in model 3). The price impact gradient with respect to cleanup is robust, but is again roughly half the size of the effects suggested by models 1 and 2, at least among the closest distance bins. A final set of models are estimated to examine price effect heterogeneity across the 16 high-profile sites based on the exposure pathways of concern, socio-demographic characteristics of the surrounding city or town, and the presence of MTBE contamination. MTBE is often associated with historical UST releases, and is a challenge to clean up.

Separate variants of the RES meta-regression in column 1 of Table VI are re-estimated to include interaction terms between the intercepts for release discovery and cleanup completion and dummy variables denoting that (1) private potable well water contamination, (2) vapor intrusion, or (3) MTBE contamination were primary concerns, as well as continuous variables denoting (4) median household income, (5) population density, and (6) the percent of workers that are blue collar in the surrounding community.¹⁵ The respective results are presented in columns 1 through 6 of Table VIII.

¹⁵Socio-demographic variables were obtained from the US Census Bureau's Decennial Census and American Community Survey. Each site was matched to the Census designated “place” (e.g., city or town or unincorporated area), and the socio-demographic values

The results are only suggestive, but do provide some evidence of potential price impact heterogeneity in terms of vapor intrusion being a concern and population density. Interestingly, any potential heterogeneity seems to be in terms of the release discovery. The evidence suggests no observed price impact heterogeneity in response to cleanup events. Interpretation of the magnitude of these heterogeneous impacts directly from the estimated coefficients in Table VIII is difficult. To demonstrate these effects, we consider a few illustrative examples based on houses within 0 to 1 kilometer of the site. Based on column (2), the discovery of a high-profile release not involving vapor intrusion corresponds to an insignificant 2.4% decrease in price, but all else constant, a release discovery involving vapor intrusion corresponds to a significant 9.6% decrease ($p\text{-value} \leq 0.01$). Considering the median population density of 10,313 people per square mile, the results from column (5) suggest that release discovery corresponds to a 4.7% ($p \leq 0.05$) decrease in price, an effect that seems to be slightly larger when evaluated at the more rural 25th percentile of 2,917 people per square mile, suggesting a decrease of 6.6% ($p \leq 0.01$).¹⁶

We find no evidence of statistically significant heterogeneity in the impacts of a release or cleanup across different sites based on MTBE contamination, median household income, or the percent of blue color workers living in the surrounding neighborhood. The latter is in contract to the findings of Kiel and Williams (2007), who in their meta-analysis of housing price impacts around Superfund sites found that the listing of a site on the National Priorities List (NPL) was less likely to have the expected negative price effect in areas with a higher percent of blue collar workers.

Given the small subset of 16 high-profile release sites examined in this study, our analysis of price impact heterogeneity should be considered exploratory and the policy implications tentative. The lack of any robust evidence of price impact heterogeneity with respect to whether private well contamination or MTBE were of concern, suggests that transfer of our results to other *high-profile* UST releases may be appropriate. This preliminary finding is potentially useful for policy-makers, particularly given the legacy nature, and sometimes heightened attention, given to MTBE contamination in groundwater.¹⁷

The preliminary evidence that prices may decline more so in cases where release discovery involves the more pressing need to address exposures from vapor intrusion and in more rural towns with a lower population density deserves further attention, but could suggest the need to account for such heterogeneity when evaluating policies.

corresponding to the closest year when data were available, relative to the year of each milestone event. Median household income is measured in (\$ 1,000s USD), population density is measured as 1,000 people per square mile, and percent blue collar workers is based on the number of workers in “production, transportation, and material moving occupations” or “construction, extraction, and maintenance occupations”, and rescaled to range from 0 to 100.

¹⁶The estimated price impacts with respect to vapor intrusion are statistically different ($\chi^2_{(1)} = 5.10$, $p=0.0239$), as are the heterogeneous price impacts corresponding to population density ($\chi^2_{(1)} = 4.00$, $p=0.0454$).

¹⁷For example, EPA held a Blue Ribbon Panel on the topic (<http://archive.epa.gov/mtbe/web/html/action.html>), and the American Cancer Society has directed considerable resources to the study of MTBE (<http://www.cancer.org/cancer/cancercauses/othercarcinogens/pollution/mtbe>).

5. CONCLUSION

When households are well-informed, analysis of revealed preferences will unveil more accurate public values of environmental disamenities. Contaminated sites where media and public attention are high (and where risks may be as well) can provide upper bound estimates of implicit prices. However, past research focused on high-profile contaminated sites finds evidence of stigma during and after cleanup (McCluskey & Rausser, 2003; Messer et al., 2006), which would confound estimates of the true implicit price of remediating contamination. Other research not limited to high-profile sites finds no clear evidence of stigma (Kiel & Williams, 2007; Taylor et al., 2016).

This study examines 16 high-profile UST releases across the United States that were widely publicized, involved significant community concern, and in most cases were severe. A two-step methodology is employed to estimate how housing values respond to releases of contamination and the subsequent completion of cleanup. Site specific hedonic regressions are estimated for each of the 16 study areas by combining exact covariate matching and coarsened exact matching techniques, with a quasi-experimental difference-in-differences (DID) framework. We then conduct an internal meta-analysis of the resulting hedonic estimates.

Compared to highly refined quasi-experimental property value studies (Haninger et al., 2014; Haninger et al., 2017; Linden & Rockoff, 2008; Muehlenbachs et al., 2015), this study was constrained by having only one (or in one case, two) high-profile UST sites in each study area. Although great steps were taken to minimize potential omitted variable biases, in two cases there are concerns that our site-specific estimates are susceptible to unobserved local and temporally varying influences. However, as the aforementioned studies minimized omitted variable bias by looking at many sites within a single housing market, our internal meta-analysis looks at many sites across the United States, with the release discovery and cleanup completion treatment events occurring at different points between 1985 and 2013. To the extent that any correlated time-variant effects are idiosyncratic across sites, this spatial and temporal variation improves our identification of the average treatment effects, and lends greater confidence to a causal interpretation. Diagnostic examination of semi-parametrically estimated price trends suggest that there are no unobserved confounding trends in the data that are systematically present across the 16 sites considered. Of course, we cannot fully rule out the possibility that local unobserved factors may bias our results, but we believe we provide credible evidence that a causal interpretation is reasonable.

The results suggest significant heterogeneity in the price effects across sites, but on average bound the implicit price of UST contamination at a 2% to 6% depreciation upon the discovery of a release; an effect that extends to houses up to 2 or 3 km from the site. The results confirm the importance of information, though contamination severity may also have contributed. We find a price rebound of a similar magnitude and slightly farther spatial extent once cleanup is complete – a 4% to 9% appreciation. These findings suggest that there is no residual stigma after cleanup, even among these high-profile UST releases that involved significant media and community attention, and in some cases occurred over prolonged periods. This is in contrast to findings that high-profile events can cause stigma

(Dale et al., 1999; Messer et al., 2006), but is in agreement with Taylor et al. (2016), who found little evidence of stigma after controlling for general land use externalities (as we do here by accounting for uncontaminated gas stations in the primary hedonic regressions).

Overall, households' revealed values for risk appear rational in the high-information setting provided by these high-profile releases. Our findings suggest that high profile cases are useful for bounding the true implicit prices of contamination and the subsequent cleanup. The results are largely in line with recent nationwide hedonic property value studies of superfund sites (Gamper-Rabindran & Timmins, 2013) and brownfields (Haninger et al., 2017). We find significant price impacts, for similar spatial extents, though of a slightly smaller magnitude. This suggests that high-profile UST releases and cleanups may be perceived in a similar fashion as these other types of contaminated sites.

Since our analysis focuses on the most high-profile UST releases, we emphasize that the results should *not* be extrapolated to the broader set of more typical leaking UST sites. Nonetheless, our findings provide useful insights for assessing policies that prevent and clean up contaminated sites. The results demonstrate the upper reaches of cleanup benefits to nearby residents. Further, to the extent that policies help prevent high-profile situations, either by preventing releases in the first place or detecting them early to minimize damages, our results may be closer to an average of the avoided property value losses. Given the high number and broad distribution of USTs across the country, the latter benefit may be quite substantial.

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APPENDIX

Appendix A. High Profile UST Releases: Guidelines for Identification, and Socioeconomic Characteristics

Table A. I.

Guidelines and Criteria Used for Identifying High-Profile Release Cases.

What we are looking for?	
•	We are targeting " <i>high-profile</i> " releases from underground storage tanks that meet the following qualifying criteria.
	i. A site that has received media attention or a significant amount of concern from the surrounding community.
	ii. Some major events such as leak discovery, cleanup, closure of investigation, publicity, and so on, have occurred roughly within the last 10 or 15 years. However, we will consider cases in

What we are looking for?

which the discovery might have occurred long ago, but the cleanup or other events are more recent or on-going. In other words, these dates should be *rough* guidelines.

iii. Release site is located within or near a residential neighborhood (ideally suburban or urban, but could be rural).

iv. Please note:

- While these cases may involve human health risks and/or large and severe contamination plumes, this does not necessarily have to be the case.

In summary, we are looking for the worst cases that have come up. These are the ones that most individuals in your office have heard of, and where the surrounding community is aware of the release and/or associated activities.

Table A. 2.

Socioeconomic Information for Communities Surrounding High-Profile UST Release Sites: Sites Excluded and Included from the Empirical Analysis.

Census Designated Place (City/Town)	County	State	Median Household Income	Population per Square Mile	% Workers Blue Collar
COMMUNITIES WITH AN EXCLUDED SITE					
Falkville	Morgan County	AL	34,107	191	51%
Glennville	Kern County	CA	31,310	81	22%
South Tahoe	El Dorado County	CA	34,624	91	16%
Climbing Hill CDP ²	Woodbury County	IA	38,509	119	43%
Lockridge	Jefferson County	IA	29,861	37	54%
Roselawn	Newton County	IN	48,577	36	45%
Lawrence	Douglas County	KS	34,734	219	15%
Chillum	Prince George's County	MD	41,277	1,651	20%
Miles City	Custer County	MT	29,940	3	18%
Ronan	Lake County	MT	21,698	18	30%
Boone	Watauga County	NC	20,523	137	7%
Rougemont Unincorporated Area ²	Durham County	NC	43,337	769	13%
Bancroft	Cuming County	NE	28,571	18	23%
Lincoln	Lancaster County	NE	40,629	298	21%
Gallup	McKinley County	NM	35,005	14	18%
Santa Fe	Santa Fe County	NM	40,184	68	14%
Bellmore	Nassau County	NY	75,665	4,655	14%
Hardesty	Texas County	OK	28,000	10	40%
Hopkins	Richland County	SC	17,386	424	39%
Timber Lake	Dewey County	SD	26,250	3	18%
Greenfield	Weakley County	TN	26,889	60	45%
Ferron City	Emery County	UT	37,708	2	38%
Gunnison	Sanpete County	UT	33,793	14	26%
Winneconne	Winnebago County	WI	42,194	357	33%
COMMUNITIES WITH AN INCLUDED SITE					
Santa Monica	Los Angeles County	CA	50,468	2,344	7%

Census Designated Place (City/Town)	County	State	Median Household Income	Population per Square Mile	% Workers Blue Collar
Lasalle	Weld County	CO	43,750	45	36%
Bithlo	Orange County	FL	33,937	988	43%
DeLeon Springs	Volusia County	FL	31,813	402	35%
Palm City	Martin County	FL	62,109	228	11%
Fallston	Harford County	MD	83,626	496	10%
Jacksonville	Baltimore County	MD	43,079	1,260	11%
Monrovia	Frederick County	MD	65,974	295	21%
Hendersonville	Henderson County	NC	31,440	238	31%
Montvale	Bergen County	NJ	92,089	3,776	8%
East Setauket	Suffolk County	NY	85,351	1,556	10%
Smithtown	Suffolk County	NY	79,694	1,556	14%
West Hempstead	Nassau County	NY	71,231	4,655	15%
Pascoag	Providence County	RI	47,972	1,504	27%
Tuckahoe	Henrico County	VA	54,967	1,102	9%
Essex Junction	Chittenden County	VT	52,951	272	15%

¹Population density corresponds to the surrounding county.

²Data for these sites were unavailable for 2000 and/or at the Census Designated Place (CDP) level, and so county level employment data for 2010 were used instead.

Appendix B. Meta-data Quasi-Experimental Diagnostics

Based on the site specific pre-treatment price trends, a quasi-experimental interpretation may not necessarily be valid for the primary hedonic regressions at two of the high-profile UST sites, when considered alone. However, perhaps such an interpretation is reasonable when considering all sites as a whole. We posit that potentially confounding factors and the resulting idiosyncratic biases are reduced by examining multiple sites in our internal meta-analysis. In a valid quasi-experiment, diagnostic comparisons of the treated and control groups should suggest similar trends in the outcome of interest prior to the treatment event, and then, if there is a measurable treatment effect, demonstrate a divergence in these trends after treatment. Analogous to what is often referred to as an “event analysis” (Hanna & Olivia, 2010), a price differential graph of the corresponding meta-data (i.e., % p estimates from the primary hedonic regressions) is developed and semi-parametrically estimated.

For each of the 14 high-profile sites where we observed the discovery of a release, we estimate the hedonic regression in equation A.1. Similar to the primary hedonic analyses discussed in section 2.1, the corresponding samples in this diagnostic exercise are constrained to only sales within 5 years before or after the discovery of the release, and transactions are weighted according to the exact covariate and coarsened exact matching procedure discussed. A key difference, however, is that here the regression for each of the sites is estimated using only houses within 0-2 km (the treated group) or within 5-10 km (the control group) of the high-profile site.

The treatment event (release discovery in this case) occurred at a different date for each site, ranging from the Deleon Springs release in June, 1985 in Florida, to the Pearl Street Gulf release in May, 2011 in Vermont. Treatment assignment is fairly well dispersed over space and time, and so even if the primary hedonic estimates from one specific location suffered from an omitted variable bias, we posit that the probability of the same confounding influences systematically existing at all treatment locations and times is relatively low.

For each high-profile site s , time was normalized with respect to the release date, and transactions were grouped into one year bins relative to the date of the release. Following this normalization, $T_{\tau s}$ is a vector of dummy variables denoting that the transaction took place in year τ relative to the date the release was discovered at site s . An interaction term between the time dummy variables and a dummy variable equal to one for homes within 0-2 km of the high-profile site (D_{is}^{0-2km}) is also included. The below hedonic regression is estimated separately for each of the 14 high-profile sites:

$$\ln p_{ij\tau s} = x_{ij\tau} \beta + T_{\tau s} \gamma_{\tau s} + (T_{\tau s} \times D_{is}^{0-2km}) \alpha_{\tau s} + \nu_j + \varepsilon_{ij\tau s} \quad (\text{A.1})$$

The vector $x_{ij\tau}$ contains all structural, parcel, neighborhood, and other control variables that are not of direct interest, ν_j is a neighborhood fixed effect (at the census tract level), and $\varepsilon_{ij\tau s}$ is a normally distributed and assumed mean zero error term. The coefficients to be estimated are β , $\gamma_{\tau s}$, $\alpha_{\tau s}$, and ν_j . The coefficient of interest is $\alpha_{\tau s}$ reflects the price differential between the time trends for the treated and control groups. Taking the estimates of $\alpha_{\tau s}$ for each of the high-profile sites s , we estimated the percent difference in prices between the treated and control groups in year τ as:

$$\% \Delta \hat{p}_{\tau s} = (e^{\hat{\alpha}_{\tau s}} - 1) \times 100 \quad (\text{A.2})$$

These estimates compose the meta-data used to semi-parametrically graph the price trend differential at the meta-analytic level. Notice that the release discovery or “treatment” date is not specified in equation (A.1). These estimates reflect the general trends over time without imposing any structure with respect to when the treatment occurs.

Next the meta-data ($\% \Delta \hat{p}_{\tau s}$) are used to estimate the following random effect size (RES) meta-regression:

$$\% \Delta \hat{p}_{\tau s} = \lambda_0 + \lambda_1 T_{\tau s} + e_{\tau s} \quad (\text{A.3})$$

where λ_0 is a constant term to be estimated, and λ_1 is a coefficient vector that reflects the average price differential in each year τ between the treated and control groups. Note that five years before the release is discovered ($\tau = -5$) is the omitted category. The estimated price trend differential is depicted in Figure 7 in the main text. Again, we emphasize that no structure was imposed relative to the release discovery date. This trend merely reflects the

difference between how the treated group prices changed over time relative to the control group.

The confidence intervals in Figure 7 are fairly wide given the small number of transactions in the treated group when divided into annual bins, but the point estimates suggest an interesting story. Most importantly, the near zero and statistically insignificant price differences in the left portion of the graph suggest that, on average, there is no significant differences in price between the treated and control groups prior to a release being discovered. This similarity in the pre-treatment price trends suggests that the meta-analysis, as a whole, provides a reasonable quasi-experimental setting, lending some support to our primary hedonic and meta-analyses in sections 4.3 and 4.4.

The point estimates in the right portion of the graph are also very close to zero, and although the point estimates are slightly more negative for all years after the discovery of a release, these differences are not statistically indistinguishable from zero. This suggests that there may not be a measurable average treatment effect from the discovery of a release. That said, these suggestive results could also be an artifact of the data limitations when trying to perform a less restrictive diagnostic exercise such as this. The confidence intervals are fairly wide in this scoping exercise because it involves chopping the data into relatively fine one-year bins in order to semi-parametrically allow prices to vary over time and across the control and treated groups. The primary transaction data are fairly scant when divided into such short time-intervals. This in turn, translates to wide confidence intervals when estimating and graphing the average differential trends over time. We examine these post-treatment price effects more formally in the main analysis (sections 4.3 and 4.4).

As a further sensitivity analysis, we redefine the treatment group to include all houses within the 0 to 5 km buffer. Figure B.1 shows that there is still no evidence of a pre-treatment price differential at the treated homes, even with the slightly tighter confidence intervals due to the larger number of transactions in the primary hedonic regressions when defining the treatment group extent out to the full five kilometers. The figure also suggests no evidence of a decrease in property values following a nearby release, but this is to be expected since the treatment group now includes many more houses that are relatively distant from the UST site. Although we do not necessarily expect that the treatment group extends out to 5 km at most sites, we do want to allow for that possibility. Doing so allows the meta-analysis to inform us how far the price impacts extend on average.

Appendix C. Leave-one-out Analysis of Random Effect Size Means.

Table C 1.

Sensitivity of RES Release Discovery Mean Percent Changes in Price to Iteratively Excluding One Site.

Distance Bin (km)	All Sites	Site Excluded:										
		2	3	4	5	7	8	9	10	11	13	14
0 to 1	-5.04 ***	-3.39 *	-5.33 ***	-3.77 **	-4.70 **	-5.08 **	-4.36 **	-5.32 **	-5.16 **	-5.79 ***	-5.54 ***	-5.40 **

Distance Bin (km)	All Sites	Site Excluded:										
		2	3	4	5	7	8	9	10	11	13	14
1 to 2	-1.65	-1.50	-1.62	-1.92	-1.46	-2.10	-1.60	-1.76	-1.16	-1.86 [*]	-1.96 [*]	-2.15 [*]
2 to 3	-2.06	1.15	-2.51	-3.59 [*]	-0.64	-2.49	-2.25	-2.45	-1.65	-2.09	-2.65	-3.03
3 to 4	-1.39	1.51	-1.58	-2.87	-1.48	-1.74	-1.50	-1.37	-1.53	-1.66	-1.81	-1.70
4 to 5	0.04	0.23	-0.02	-0.14	0.09	0.08	-0.19	0.08	0.22	-0.01	-0.06	-0.06

Note:

p<0.01,

**
p<0.05,

*
p<0.1. Averages calculated using the 'metan' command in Stata 14. RES weights are based on the inverse variance of the primary study estimates (see section 2.2).

Table C 2.

Sensitivity of RES Cleanup Completion Mean Percent Changes in Price to Iteratively Excluding One Site.

Distance Bin (km)	All Sites	Site Excluded:				
		1	2	12	13	16
0 to 1	5.28 ^{**}	4.19	3.69 [*]	6.03	7.04 ^{***}	5.64 ^{**}
1 to 2	7.53 ^{**}	7.20 [*]	5.00	11.26 ^{**}	5.74	10.90 ^{***}
2 to 3	3.60	1.87	3.63	5.16 ^{**}	2.64	4.61
3 to 4	4.36 ^{**}	3.12 ^{**}	4.75 ^{**}	4.86 ^{**}	3.73 [*]	5.48 ^{***}
4 to 5	-0.27	-1.20	0.74	-0.03	-0.22	-0.67

Note:

p<0.01,

**
p<0.05,

*
p<0.1. Averages calculated using the 'metan' command in Stata 14. RES weights are based on the inverse variance of the primary study estimates (see section 2.2).

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Figure 2.

Years of Transaction Data Available and Milestone Events by Study Area.

Note: Case names are followed by study area (usually county) names. Red vertical lines denote the date of a release discovery event, and green vertical lines denote the date of cleanup completion.

Figure 3.
Local Polynomial Regression Price Gradients: Before and After Release Discovered
Milestone, Green Valley Citgo, Maryland.

Bithlo Site, FL

DeLeon Springs Site, FL

Green Valley Citgo Site, MD

Hendersonville Site, NC

Jacksonville Exxon, MD

LaSalle Site, CO

Figure 4.

Local Polynomial Regressions of Control and Treated Group Price Trends relative to Release Discovery.

Note: Vertical solid black line denotes the date of release discovery. Treated group of house transactions are denoted by the solid blue line. Control homes are denoted by the long-dash redline. The light grey short-dash lines denote the 95% confidence intervals. For purposes of this diagnostic exercise, treated houses are defined as those within 0-2 km from a high-profile site, and control houses are those in the 5-10 km buffer. A gaussian kernel density function and 365-day bandwidth are used. Residuals calculated from hedonic regressions of the natural log of the transaction price, using the matched sample and weights. Control

variables include attributes of the house structure, location, tract level fixed effects, annual and seasonal fixed effects, and the vector of one-kilometer bins denoting proximity to the high-profile site.

Figure 5.

Local Polynomial Regressions of Control and Treated Group Price Trends relative to Cleanup Completion.

Note: Vertical dashed black line denotes the date of cleanup completion. Treated group of house transactions are denoted by the solid blue line. Control homes are denoted by the long-dash red line. The light grey short-dash lines denote the 95% confidence intervals. For purposes of this diagnostic exercise, treated houses are defined as those within 0-2 km from a high-profile site, and control houses are those in the 5-10 km buffer. A gaussian kernel density function and 365-day bandwidth are used. Residuals calculated from hedonic regressions of the natural log of the transaction price, using the matched sample and weights. Control variables include attributes of the house structure, location, tract level fixed effects, annual and seasonal fixed effects, and the vector of one-kilometer bins denoting proximity to the high-profile site.

Figure 6.

Local Polynomial Regressions of Control and Treated Group Price Trends relative to Release Discovery and Cleanup when both occur within Study Period.

Note: Vertical solid black line denotes the date of release discovery and the vertical dashed black line denotes the date of cleanup completion. Treated group of house transactions are denoted by the solid blue line. Control homes are denoted by the long-dash redline. The light grey short-dash lines denote the 95% confidence intervals. For purposes of this diagnostic exercise, treated houses are defined as those within 0-2 km from a high-profile site, and control houses are those in the 5-10 km buffer. A gaussian kernel density function and 365-day bandwidth are used. Residuals calculated from hedonic regressions of the natural log of the transaction price, using the matched sample and weights. Control variables include attributes of the house structure, location, tract level fixed effects, annual and seasonal fixed effects, and the vector of one-kilometer bins denoting proximity to the high-profile site.

Figure 7.

Meta-analytic Price Trend Differential between Control and Treated Groups.

Note: For purposes of this diagnostic figure, treated houses are defined as those within 0-2 km from a high-profile site, and control houses are those in the 5-10 km buffer. See Appendix B for details.

Figure 8.

Release Discovered: Percent changes in Price for 0 to 1 km Distance Bin and Random Effect Size Mean.

Note: The x-axis is the percent change in price, and the estimated percent changes in price and 95% confidence intervals are shown for each corresponding high-profile release site.

The size of the grey boxes depicts the relative weights given to each meta-observation when calculating the Random Effect Size (RES) mean. The weights are the inverse variances of the estimates (see section 2.2). The diamond at the bottom depicts the RES mean, and the width of the diamond demonstrates the 95% confidence interval.

Figure 9.

Price Impact Gradient of Release Discovery: Random Effect Size Meta-Regression.

Note: Dashed lines depict 95% confidence interval; ** $p < 0.05$, * $p < 0.1$. Based on coefficient estimates from Model 1 in Table VI.

Figure 10.

Price Impact Gradient of Cleanup: Random Effect Size Meta-Regression.

Note: Dashed lines depict 95% confidence interval; ** $p < 0.05$, * $p < 0.1$. Based on coefficient estimates from Model 1 in Table VI.

Figure B.1.

Meta-analytic Price Trend Differential Between Control and Treated Groups.

Note: In contrast to Figure 7, here the treated group houses are defined as those within 0-5 km from a high-profile site, rather than within 0-2 km.

Table I.

Nature of Contamination at 16 Included High-Profile Release Sites.

Contamination Issue	Number of Sites
Private residential groundwater wells contaminated	9
Combustible vapors	6
Public well fields affected	3
Surface water contamination	1
Groundwater contamination with residents not using well water	2

Note: Categories are not mutually exclusive. Some sites had multiple types of contamination issues.

Table II.

Types of Milestone Events at High-Profile UST Release Sites.

Event Type	# of Events	Examples
Release Discovered	14	Release occurred, Previously unknown release found, Previously resolved investigation re-opened
Cleanup Complete	5	Cleanup completed, Investigation closed and deemed safe by regulators
Total	19	

Table III.

High-Profile UST Site Example: Transaction Dataset Summary Statistics, Green Valley Citgo (Frederick County, MD).

Variable	Obs	Mean	Std. Dev.	Min	Max
Sale price (2013\$)	4,286	417,600	138,465	104,657	809,476
Age of house (years)	4,279	24.62	18.88	0	176
Age missing (dummy)	4,286	0.00	0.05	0	1
Age ²	4,279	962.38	2,225.66	0	30,976
Townhome (dummy)	4,286	0	0	0	0
Total # of bathrooms	4,286	2.29	0.63	1	5.5
Baths missing (dummy)	NA	NA	NA	NA	NA
Interior square footage	4,284	1,922.62	719.11	672	5675
Interior sqft. missing (dummy)	4,286	0.00	0.02	0	1
Parcel acreage	4,286	1.11	0.97	0.09	16
Acres missing (dummy)	NA	NA	NA	NA	NA
Air conditioning (dummy)	4,286	0.80	0.40	0	1
Air conditioning missing (dummy)	NA	NA	NA	NA	NA
Basement (dummy)	4,286	0.84	0.36	0	1
Basement missing (dummy)	NA	NA	NA	NA	NA
Porch (dummy)	3,944	0.95	0.23	0	1
Porch missing (dummy)	4,286	0.06	0.23	0	1
Pool (dummy)	4,286	0.05	0.21	0	1
Pool missing (dummy)	NA	NA	NA	NA	NA
Distance to nearest urban cluster (km)	4,286	15.13	3.83	5.59	20.11
Located in public water service area (dummy)	4,286	0.22	0.41	0	1
Distance to nearest major road (km)	4,286	2.42	1.67	0.03	6.90
Located in 100-year flood zone (dummy)	4,286	0.00	0.06	0	1
Located on waterfront (dummy)	NA	NA	NA	NA	NA
# of gas stations within 200 meters	4,286	0.00	0.02	0	1
# of gas stations within 200-500 meters	4,286	0.01	0.11	0	1
Distance to High-Profile Site (meters)	4,286	5,927	2,462	206	9,996
0 to 1 km of High-Profile Site (dummy)	4,286	0.031	0.172	0	1
1 to 2 km of High-Profile Site (dummy)	4,286	0.041	0.198	0	1
2 to 3 km of High-Profile Site (dummy)	4,286	0.078	0.269	0	1
3 to 4 km of High-Profile Site (dummy)	4,286	0.117	0.322	0	1
4 to 5 km of High-Profile Site (dummy)	4,286	0.064	0.244	0	1

Note: "NA" denotes variables that are not available for Frederick County. Descriptive statistics weighted based on sample weights from exact and coarsened exact matching (CEM) of select covariates (see section 2.1).

Table IV.

Primary Hedonic Results: Percent Change in Price after Milestone Events.

		Distance from High-Profile Site (kilometers)					
Model		0 to 1	1 to 2	2 to 3	3 to 4	4 to 5	Obs.
Charnock Well Fields, CA							
1	Cleanup Complete	8.76 ^{***} (2.26)	9.30 ^{***} (2.10)	9.05 ^{***} (1.67)	9.98 ^{***} (2.46)	5.80 ^{**} (2.82)	26,552
LaSalle, CO							
2	Leak Discovered	-34.54 ^{***} (5.05)	-42.44 ^{**} (19.40)	-50.32 ^{***} (6.62)	-46.69 ^{***} (4.02)	-23.92 ^{***} (6.91)	28,227
3	Cleanup Complete	15.65 ^{***} (4.68)	58.19 ^{***} (14.61)	3.34 (6.20)	1.99 (3.46)	-6.05 ^{***} (2.06)	14,677
Bithlo, FL							
4	Leak Discovered	5.11 (9.54)	-16.01 (25.33)	21.18 (17.09)	6.61 (13.53)	24.08 (15.56)	4,969
DeLeon Springs, FL							
5	Leak Discovered	-16.12 ^{***} (1.09)	7.24 (5.92)	25.39 ^{***} (2.26)	17.25 ^{***} (4.02)	16.33 ^{**} (6.66)	5,503
Palm City Cluster, FL							
6	Leak Discovered	-11.13 ^{**} (5.09)	-43.92 ^{**} (17.32)	-55.47 ^{***} (14.28)	11.79 (27.31)	-3.85 (7.16)	18,476
Upper Crossroads, MD							
7	Leak Discovered	-5.18 ^{***} (0.92)	1.47 ^{**} (0.75)	-1.08 (1.23)	3.30 (4.62)	-0.22 (1.40)	2,696
Jacksonville Exxon, MD							
8	Leak Discovered	-12.74 ^{***} (1.79)	-2.42 (2.99)	-2.17 (1.61)	-0.03 (3.12)	3.65 (2.71)	3,736
Green Valley Citgo, MD							
9	Leak Discovered	-1.90 ^{***} (0.57)	-1.48 ^{**} (0.66)	0.42 (2.01)	-1.13 (1.05)	-0.65 (3.13)	4,286
Hendersonville Corner Pantry, NC							
10	Leak Discovered	-3.72 (2.69)	-5.43 ^{***} (1.42)	-6.80 ^{***} (2.60)	0.52 (4.75)	-1.38 (1.53)	9,074
Montvale, NJ							
11	Leak Discovered	3.23 ^{**} (1.41)	0.21 (1.91)	-3.14 [*] (1.88)	1.69 (2.81)	0.76 (2.48)	12,846
Northville Industries, NY							
12	Cleanup Complete	3.27 ^{***} (0.79)	2.78 ^{***} (1.04)	-1.50 (1.45)	2.23 (2.30)	-0.74 (1.11)	32,710
Smithtown Exxon-Mobil, NY							
12	Leak Discovered	1.38 (3.13)	3.61 (3.85)	3.44 (2.64)	3.44 (3.12)	1.07 (1.84)	
12	Cleanup Complete	-3.90 (4.23)	12.25 ^{***} (2.17)	7.11 ^{***} (2.05)	6.39 ^{***} (1.59)	-0.32 (1.98)	
West Hempstead, NY							

		Distance from High-Profile Site (kilometers)					Obs.
Model		0 to 1	1 to 2	2 to 3	3 to 4	4 to 5	
13	Leak Discovered	-1.38 (1.37)	3.96 (2.53)	5.00 *** (1.22)	2.30 *** (0.84)	0.73 (0.88)	46,572
Pascoag, RI							
14	Leak Discovered	-2.82 (3.70)	-13.06 *** (2.50)	0.57 (8.74)	-1.86 (3.57)	-1.87 (1.72)	1,472
Tuckahoe, VA							
15	Leak Discovered	2.07 (1.42)	-0.40 (2.21)	1.24 (1.81)	0.21 (1.82)	0.16 (1.53)	17,565
15	Cleanup Complete	2.96 (5.51)	-3.54 *** (1.35)	0.14 (1.66)	0.84 (1.70)	1.29 (1.48)	
Pearl Street Gulf, VT							
16	Leak Discovered	6.58 * (3.83)	-1.18 (1.15)	-0.99 (4.16)	-1.18 (2.41)	0.07 (1.86)	2,415

Note: Standard errors in parentheses.

p<0.01,

**
p<0.05,

*
p<0.1. Alternating color shades denote estimates from different hedonic regressions, as also indicated by the model number in the first column. Estimates are calculated following equation (2), using the hedonic coefficients based on regressions (equation 1) of datasets of the matched sample, and limited to transactions within 10 km of a high-profile release site, and that sold within five years before or after a release discovery or cleanup completion event.

Table V.

Meta-Analysis Unweighted and Random Effect Size (RES) Means.

	Release Discovered		Cleanup Complete	
	Unweighted	RES	Unweighted	RES
0 to 1 km	-5.15 ***	-5.04 ***	5.34 ***	5.28 **
1 to 2 km	-7.85 ***	-1.65	15.80 ***	7.53 **
2 to 3 km	-4.48 **	-2.06	2.56 **	3.60
3 to 4 km	-0.27	-1.39	4.01 ***	4.36 **
4 to 5 km	1.07	0.04	-0.01	-0.27

Note:

p<0.01,**
p<0.05,*
p<0.1. Averages calculated using the 'metan' command in Stata 14. RES weights are based on the inverse variance of the primary study estimates (see section 2.2). Unweighted means obtained using same command but equal weight given to each primary estimate.

Table VI.

Meta-Regression Results: Random Effect Size (RES) and Random Effects (RE) Panel Models.

VARIABLES	All 16 High-Profile UST Release Sites		Only Subset of Sites Consistent with Common Trends Assumption	
	(1) RES Model	(2) RE Panel Model	(3) RES Model	(4) RE Panel Model
Discovered	-5.85 [*] (3.14)	-5.86 ^{**} (2.89)	-3.26 [*] (1.76)	-2.99 (1.91)
Cleaned	8.94 [*] (5.19)	9.04 [*] (5.48)	5.04 [*] (3.03)	4.25 ^{**} (2.10)
Distance				
× Discovered	1.20 (0.95)	1.16 [*] (0.62)	1.15 ^{**} (0.53)	1.19 [*] (0.68)
× Cleaned	-1.53 (1.54)	-1.15 (0.78)	-0.47 (0.90)	-0.44 [*] (0.24)
Observations	95	95	80	80
# of Sites	16	16	14	14

Note: Dependent variable observations are the percent changes in price following each milestone event, in each one-kilometer bin, as displayed in Table IV.. Standard errors in parentheses.

^{**}
p<0.05,

^{*}
p<0.1; clustered at the high-profile site level in the Random Effects Panel Model (Models 2 and 4).

Table VII.

Estimated Percent Price Changes Based on Mega-regressions in Table VI.

VARIABLES	All 16 High-Profile UST Release Sites		Only Subset of Sites Consistent with Common Trends Assumption	
	(1)	(2)	(3)	(4)
	RES Model	RE Panel Model	RES Model	RE Panel Model
Discovered				
0 km	-5.85 [*] (3.14)	-5.86 ^{**} (2.89)	-3.26 [*] (1.76)	-2.99 (1.91)
0 to 1 km	-4.65 ^{**} (2.32)	-4.71 [*] (2.68)	-2.11 (1.30)	-1.80 (1.35)
1 to 2 km	-3.46 ^{**} (1.65)	-3.55 (2.61)	-0.96 (0.92)	-0.61 (0.98)
2 to 3 km	-2.26 [*] (1.37)	-2.40 (2.68)	0.19 (0.77)	0.58 (1.00)
3 to 4 km	-1.06 (1.67)	-1.24 (2.90)	1.34 (0.94)	1.77 (1.40)
4 to 5 km	0.14 (2.35)	-0.09 (3.22)	2.49 [*] (1.33)	2.96 (1.96)
Cleaned				
0 km	8.94 [*] (5.19)	9.04 [*] (5.48)	5.04 [*] (3.03)	4.25 ^{**} (2.10)
0 to 1 km	7.41 [*] (3.85)	7.89 [*] (4.74)	4.58 ^{**} (2.24)	3.82 ^{**} (1.92)
1 to 2 km	5.88 ^{**} (2.73)	6.75 [*] (4.00)	4.11 ^{***} (1.57)	3.38 [*] (1.76)
2 to 3 km	4.36 ^{**} (2.18)	5.60 [*] (3.29)	3.65 ^{***} (1.24)	2.94 [*] (1.62)
3 to 4 km	2.83 (2.61)	4.45 [*] (2.61)	3.18 ^{**} (1.50)	2.50 [*] (1.51)
4 to 5 km	1.30 (3.68)	3.30 [*] (2.00)	2.72 (2.14)	2.07 (1.43)

Note: Estimated percent changes in price by one-kilometer bin, calculated from the meta-regression coefficients presented in models 1 through 4 in table VI. Standard errors in parentheses.

p<0.01,

**
p<0.05,

*
p<0.1.

Table VIII.

Random Effect Size (RES) Meta-Regression Results: Price Impact Heterogeneity.

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Discovered	-8.36 ** (3.488)	-3.62 (3.195)	-6.28 * (3.588)	-11.73 ** (5.176)	-8.34 ** (3.342)	-2.03 (4.168)
Cleaned	10.12 * (5.536)	11.04 * (5.568)	7.22 (5.595)	15.35 * (9.080)	8.69 (6.958)	9.82 (6.109)
Distance						
× Discovered	1.24 (0.944)	1.24 (0.920)	1.20 (0.955)	1.18 (0.942)	1.18 (0.935)	1.19 (0.945)
× Cleaned	-1.55 (1.530)	-1.47 (1.491)	-1.49 (1.549)	-1.58 (1.528)	-1.52 (1.519)	-1.49 (1.534)
Private						
× Discovered	4.41 (2.726)					
× Cleaned	-2.60 (4.382)					
Vapor						
× Discovered		-6.19 ** (2.739)				
× Cleaned		-3.95 (4.281)				
MTBE						
× Discovered			0.71 (2.809)			
× Cleaned			3.86 (4.448)			
Median Household Income (1,000k USD\$)						
× Discovered				0.09 (0.065)		
× Cleaned				-0.07 (0.086)		
Population Density						
× Discovered					1.90 ** (0.951)	
× Cleaned					0.14 (2.978)	
% Blue Collar Workers						
× Discovered						-0.19 (0.137)
× Cleaned						-0.06 (0.227)
Observations	95	95	95	95	95	95
# of Sites	16	16	16	16	16	16

Note: Dependent variable observations are the percent changes in price following each milestone event, in each one-kilometer bin, as displayed in Table IV.. Standard errors in parentheses.

**
p<0.05,

*
p<0.1.