NBER WORKING PAPER SERIES

WHO BENEFITS FROM HAZARDOUS WASTE CLEANUPS? EVIDENCE FROM THE HOUSING MARKET

Alecia W. Cassidy
Elaine L. Hill
Lala Ma

Working Paper 30661
http://www.nber.org/papers/w30661

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
November 2022, Revised March 2024

The authors would like to thank Nicolai Kuminoff, Amanda Ross, and Dennis Guignet, as well as seminar participants at the University of Alabama, University of Albany, ASSA Annual meeting, AERE Summer Conference, APPAM Fall Conference, and SEA Annual Conference for valuable feedback. The authors gratefully acknowledge the EPA Office of Land and Emergency Management (OLEM) for providing program expertise and access to the data. We especially thank David Hockey, Rachel Horton, and Norman Birchfield at the OLEM for their partnership. This work was conducted while Lala Ma was appointed to an Oak Ridge Institute for Science and Education (ORISE) Research Participation Program at the EPA OLEM. All errors are our own. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research or of the EPA.

Who Benefits from Hazardous Waste Cleanups? Evidence from the Housing Market
Alecia W. Cassidy, Elaine L. Hill, and Lala Ma
NBER Working Paper No. 30661
November 2022, Revised March 2024
JEL No. Q51,Q52,Q53,Q58

ABSTRACT

The Resource Conservation and Recovery Act (RCRA) manages cleanup of hazardous waste releases at over 3,500 sites across the US, which covers approximately 17.5% of all developed land in the country. This paper evaluates the national and distributional housing market impacts of cleanups performed under RCRA and estimates the program's impacts on neighborhood change. We find that cleanups near residential properties yield significant, yet localized, increases in home prices, and that impacts are concentrated in the lower deciles of the price distribution. Importantly, we find no evidence of sorting along socio-demographic dimensions in response to cleanup. Our findings suggest that cleanup benefits accrue to the residents who are the original “hosts” of pollution and could correct pre-existing disparities in exposure to land-based contamination.

Alecia W. Cassidy
Department of Economics, Finance
and Legal Studies
University of Alabama
200 Alston Hall, Box 870224, 361 Stadium
Tuscaloosa, AL 35487
awcassidy1@cba.ua.edu

Elaine L. Hill
Department of Public Health Sciences
Department of Economics
University of Rochester
265 Crittenden Boulevard
Rochester, NY 14642
and NBER
elaine_hill@urmc.rochester.edu

Lala Ma
University of Kentucky
Gatton College of Business & Economics
Department of Economics
lala.ma@uky.edu

1 Introduction

The Resource Conservation and Recovery Act (RCRA) aims to "protect human health and the environment from the potential hazards of waste disposal" (U.S. EPA, 2014). The Corrective Action Program, established under RCRA, investigates and cleans releases of hazardous waste at RCRA facilities, which are in operation, unlike Superfund sites. The impacts of this particular program are potentially widespread: As of fiscal year 2011, the RCRA Corrective Action Program tracked 3,747 sites,[1] which spanned 17,946,593 acres (U.S. EPA, 2011).[2] This program alone covers approximately 17.5% of all developed land in the US.[3] Beginning in 1999, the program began prioritizing facilities across the nation for cleanup, with the goal to control human exposure and contain migration of contaminated groundwater.

This paper evaluates the benefits of cleanups performed under the RCRA Corrective Action Program by estimating the impacts of cleanup on national housing prices and neighborhood change. We define exposure to RCRA cleanups based on residential proximity. We quantify the program's housing market impacts using all cleanups conducted under the program across the continental US and data from the 1990, 2000, and 2010 Decennial Censuses and the 2006–2010 American Community Survey.[4] We use spatial variation in the distance between facilities and Census tract boundaries and variation in the timing of cleanup to identify housing market impacts. We are most concerned with the distributional impacts of cleanup, and thus estimate impacts at each decile of the price distribution.

A concentration of RCRA sites in disadvantaged neighborhoods means that cleanup efforts could reduce inequitable pollution gaps documented in environmental justice

[1] 3,779 sites were tracked by 2019.

[2] For comparison, allocation of sites and acres across the EPA's 4 out of 5 major programs are 3,781,758 acres from 1,718 Superfund sites, 494,997 underground storage tanks (covering 494,997 acres), and 69,646 acres from 8,000 brownfields.

[3] Calculation is based on a U.S. EPA (2008) estimate of 102.5 million acres of developed land in the US.

[4] Our use of Decennial Census data allows us to investigate whether our hypotheses hold on a nationally representative sample of houses. An alternative approach would be to use transaction data from county offices, but that data is not nationally representative for two reasons. First, only a select sample of houses are sold in every year, and more frequently transacted houses are vastly over-represented. Second, transaction data from county deed offices such as those in Zillow Ztrax and Corelogic is particularly unreliable in states like Texas, where it does not represent sales, but rather an opaque combination of mortgage values and whatever the buyer and seller want to portray the price as. There are twelve states in the United States where buyers and sellers do not need to record sale prices. For more details, see the discussion on p. 11.

studies (Banzhaf, Ma and Timmins, 2019). The positive distributional effects may not, however, materialize if cleanups trigger re-sorting in response to price changes, altering the composition of those exposed to cleaned sites. We follow our hedonic analysis with an investigation of the extent to which RCRA cleanups altered neighborhood composition to evaluate to whom cleanup benefits accrue. We first estimate reduced-form regressions of cleanup on 17 different socioedemographic and housing-related outcomes from the Census. We then apply a structural sorting model to test whether willingness to pay (WTP) for RCRA cleanup systematically differs between white, Black, and Hispanic households to evaluate the scope for heterogeneous preferences to drive environmental gentrification. The additional structure of the empirical sorting model alleviates an identification challenge with using aggregate population changes (i.e., without information on residential origin and destination) to infer preference to avoid pollution (Depro, Timmins and O'Neil, 2015). Moreover, this model allows moving to be costly, an important source of bias in hedonic pricing analyses (Bayer, Keohane and Timmins, 2009).

We find that cleanup increases home prices of properties in the same tract as the facility, but does not have a price impact beyond the immediate tract. The housing impacts are higher in percentage terms for properties in the lower deciles of the price distribution, with an 11% increase in price for the 1^{st} decile, and no evidence of increase for the 9^{th} decile.[5] This suggests that cleanups raised housing prices for the least advantaged residents living on tracts near facilities. Localized impacts also suggests a limited scope for endogenous neighborhood changes, which we test for using both reduced-form and structural approaches.[6]

Our findings are robust to two important alternative approaches. First, our results are robust to allowing for heterogeneous impacts by cleanup timing (Gardner, 2022;

[5]One important caveat to interpreting our results in terms of willingness-to-pay is that it is unclear how quantile capitalization effects relate to capitalization effects estimated using individual house prices. The median houses in different periods could have very different characteristics. The potential for within-tract sorting further muddies the interpretation. Therefore, we cannot necessarily assume results from Rosen (1974) and Banzhaf (2020) will hold. Furthermore, Banzhaf and Farooque (2013) show that median housing prices are only weakly correlated with variables of interest, including prices from individual transactions, ozone, and income (with correlations of 0.543. -0.425, and 0.284, respectively) using cross-sectional data. If results generalize to our panel setting, low correlations between median housing values and variables of interest would imply downward attenuation of our estimates compared to the truth.

[6]The finding of extremely localized housing impacts alleviates the concern that demand for RCRA cleanup appears in the wage gradient: with housing price impacts limited to within a tract, an individual may choose to alter their exposure to RCRA sites without switching jobs.

Callaway and Sant'Anna, 2021). Second, our substantive conclusions are corroborated when we estimate quantile treatment effects using a supplemental dataset containing individual housing transaction data from Ohio and Pennsylvania.

We investigate sorting along socio-demographic dimensions using both a reduced-form and a structural approach. Using reduced-form regressions, we find no statistically significant impacts of cleanup on any of the 17 socio-economic and housing-related indicators from the Census. Our structural sorting model also suggests that, for a majority of states, WTP to avoid cleanup is not significantly different between different racial groups. If anything, our evidence suggests that Black residents have higher WTP in some states, suggesting that cleanup would be unlikely to trigger displacement of low socioeconomic status groups that is consistent with environmental gentrification. This implies that the benefits of cleanup accrued to those living closest to the facilities, who tended to be more disadvantaged compared to those living farther from the facilities. This is particularly important given recent advances in the literature. Hausman and Stolper (2020) show that, in a framework where people undervalue a clean environment and have partial information, residential sorting on willingness-to-pay leads to an equilibrium where deadweight loss due to pollution is higher for economically disadvantaged segments of the population.[7] Furthermore, Bakkensen and Ma (2020) demonstrate that well-intentioned policies in the housing market can have significant distributive effects, leading the least well-off residents to take on even more exposure to an environmental bad. If RCRA cleanups do not induce sorting along socio-demographic dimensions, they could be corrective of the type of pre-existing disparities that Hausman and Stolper (2020) call attention to, and they are unlikely to exacerbate existing inequities as Bakkensen and Ma (2020) find.

Our contribution is to provide *nationally representative* evidence of the *distributional* impacts of RCRA cleanups. Cleanup of hazardous waste releases under RCRA affects a non-trivial share of developed lands in the US. However, there has been little work to assess the impact of the environmental benefits of RCRA on housing markets. While some studies have examined the housing price impacts of wastes managed under RCRA for specific areas within the country (Smith and Desvousges, 1986; Kinnaman, 2009), these studies cannot speak to whether the cleanup impacts

[7]Partial information could broadly include factors like inattention to the nuisance or misperception of risks (Bakkensen and Barrage, 2021).

hold more generally. In contrast, our scope is national.

Others have estimated the housing market impacts of polluting facilities nationwide, but focus on other types of nuisances. Examples include river pollution by wastewater treatment plants (Keiser and Shapiro, 2018), Toxic Release Inventories (Currie, Davis, Greenstone and Walker, 2015; Mastromonaco, 2015), brownfield sites (Linn, 2013; Haninger, Ma and Timmins, 2017; Ma, 2019), Superfund sites (Currie, Greenstone and Moretti, 2011; Greenstone and Gallagher, 2008; Gamper-Rabindran, Mastromonaco and Timmins, 2011; Kohlhase, 1991; Gayer, Hamilton and Viscusi, 2000), and Areas of Concern in the Great Lakes (Cassidy, Meeks and Moore, 2023).[8] Importantly, few studies have tested for sorting in response to these types of remediation activities, which could alter the individuals who ultimately experience the benefits of environmental improvements.[9] Since RCRA facilities are widespread in the US, the scope for cleanup activities to trigger endogenous neighborhood change, or "environmental gentrification" (Banzhaf and McCormick, 2007), is a real concern given its potential to affect both the overall and distribution of cleanup benefits.[10]

Our paper complements work by Guignet and Nolte (Forthcoming), who also study the RCRA program with a national scope. Our study differs from theirs in three important ways. First, our research questions are different: their study focuses on average welfare impacts of the cleanups, whereas we characterize effects across the price distribution and thoroughly investigate post-cleanup neighborhood change and sorting. Second, they focus only on housing price responses to cleanup of the most hazardous sites (a subset of the sites we study), whereas we analyze the entire cleanup program. Third, Guignet and Nolte (Forthcoming) employ Zillow ZTrax data on individual transactions collected from county assessors, whereas we use publicly available Census data to take advantage of abundant demographic information that is more

[8] As Banzhaf (2021) illustrates, the capitalization effects found in these papers can be interpreted as a lower bound on welfare effects under assumptions such as a time-invariant hedonic gradient. More work is needed to discern whether the formal results by Banzhaf (2021) characterizing assumptions under which quasi-experiments can reveal a lower bound on welfare extend to the case of quantiles derived from aggregate data.

[9] One example of a study that undertakes a reduced-form investigation of sorting is Greenstone and Gallagher (2008), who find no effect on price and also no sorting. In contrast, we find that there is an effect on price but no sorting. The latter means that cleanup has benefits specifically to those living closest to the sites, whereas the former is a finding of no benefits.

[10] Furthermore, measurement of cleanup benefits from housing price gradients will omit associated compensating wage differentials and understate the value of cleanup (Roback, 1982). Gentrification poses a host of threats to identification, including invalidating the interpretation of hedonic gradients as representative of welfare (Banzhaf, 2021) and willingness-to-pay (Kuminoff and Pope, 2014).

suitable to our research questions. Our effect sizes on housing prices are comparable, which is not surprising given evidence from the literature that estimates produced using decile-level census data can detect similar effects to individual-transaction data (Gamper-Rabindran and Timmins, 2013).[11] With Zillow Ztrax no longer available to researchers, it is important to show that results like those of Guignet and Nolte (Forthcoming) replicate using publicly-available data.

The paper is organized as follows. Section 2 provides a background on the Resource Conservation and Recovery Act and its cleanup program. Section 3 describes our data sources and the data construction process. We describe our empirical hedonic and sorting models in section 4 and present the corresponding results in section 5. Section 6 discusses the implications of the results, and Section 7 concludes.

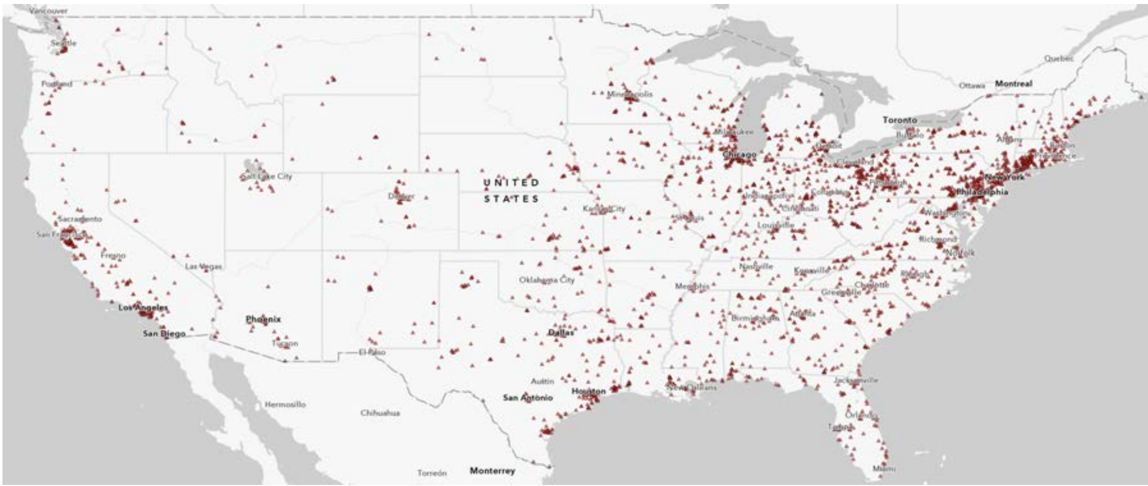
2 Background

The Resource Conservation and Recovery Act (RCRA) was enacted in 1976 by Congress. The Act consists of ten subtitles, where the two major programs under RCRA are subtitles C and D, which respectively regulate hazardous waste and non-hazardous solid waste. Subtitle C, under which cleanups of hazardous waste are conducted, sets regulations for the handling (i.e., creation, management, and disposal) of hazardous waste and is codified in Title 40 of the Code of Federal Regulations (CFR). There are three main types of RCRA hazardous waste handlers: (1) generators, (2) transporters, and (3) facilities that treat, store, or dispose of waste. Generators are then subdivided into three groups based on the amount and type of hazardous waste that is generated – Very Small Quantity Generators (VSQG), Small Quantity Generators (SQG), and Large quantity generators (LQG).[12] As of 2009, there were 460 Treatment, Storage, and Disposal Facilities (TSDF's), 18,000 transporters, and 14,700 large quantity generators. Subtitle C regulations, importantly, grant the EPA the authority to require cleanup for any release of hazardous waste to

[11] We provide a more detailed discussion of possible biases in our price regressions due to our use of tract-level data and compare our identification strategy with that of Guignet and Nolte (Forthcoming) on p. 24. We also show robustness to the use of individual transaction data in Appendix Section B.

[12] VSQG's generate 100 kilograms or less per month of hazardous waste or one kilogram or less per month of acutely hazardous waste; SQG's generate more than 100 kilograms, but less than 1,000 kilograms of hazardous waste per month; LQG's generate 1,000 kilograms per month or more of hazardous waste or more than one kilogram per month of acutely hazardous waste.

Figure 1: National Map of RCRA sites in Corrective Action Program



Notes Location of sites in the RCRAinfo Corrective Action Program cleanups as of 2019.

all environmental media at both RCRA-permitted and non-permitted facilities. The cleanup program, known as the RCRA Corrective Action Hazardous Waste Cleanup Program, is the focus of this paper.

The Corrective Action (CA) Program, established under the Hazardous and Solid Waste Amendments to RCRA in 1984, investigates and cleans releases of hazardous waste at RCRA facilities (Figure 1). Unlike the Superfund program, sites managed under this cleanup program are in operation. There are three types of corrective actions, which represent how facilities are brought into the program: (1) Permitted Corrective Actions - cleanup actions incorporated through permitting requirements for sites that already have (or are seeking) a permit, (2) Corrective Action Orders - enforcement orders if a release is identified, and (3) Voluntary Corrective Action - a voluntary agreement between a facility and the administering authority. The first two types make up the predominant share of corrective actions.

Beginning in 1999, efforts took place to reform the cleanup process and remove bureaucratic hurdles to accelerate the pace of cleanups. The EPA identified RCRA facilities with the potential for unacceptable exposure to pollutants and/or for ground water contamination. Facilities were chosen based on the National Corrective Action Prioritization System (NCAPS), which categorizes facilities as High, Medium, or Low priority.[13] The ranking is predominantly based on waste type, waste volume, release

[13] This is somewhat similar to the Hazard Ranking System (HRS) used by Superfund, except requires less detailed input.

pathways (ground water, surface water, air, and soil), and the potential for human and ecosystem exposure. In some cases, the ranking can also depend on compliance history or special conditions (e.g. regional initiatives). Most RCRA facilities were ranked by 1993.

The program set cleanup (or risk reduction) targets based on two environmental indicators (EI) established by the Government Performance Results Act of 1993 (GPRA): (1) Current Human Exposures Under Controls (or the Human Exposure EI), and (2) Migration of Contaminated Groundwater Under Control (or the Groundwater EI). A positive Human Exposure EI determination indicates that there are no "unacceptable" human exposures to contamination that can be reasonably expected under current land- and groundwater-use conditions.[14] A positive Groundwater EI determination indicates that the migration of contaminated groundwater has stabilized, and that monitoring will be conducted to confirm that contaminated groundwater remains within the original area of contamination.[15,16] With two cleanup targets established, the EPA set goals to control of human exposure and migration of contaminated groundwater. The cleanup process is carried out in five (general) steps: (1) an initial site assessment is conducted to gather information on a site's conditions, releases, and exposure pathways, (2) the nature and extent of the contamination is characterized at the site, (3) interim actions are performed to control any ongoing risks to human health and the environment, (4) remedial alternatives are evaluated, and (5) the selected remedy is implemented.

To the extent that housing market participants are aware of these facilities and the corrective actions that have taken place, a portion of the cleanup benefits should be capitalized into housing prices. If, however, households are unaware, then the

[14]From the form used to report the completion of the corrective action, found at https://www3.epa.gov/region1/cleanup/rcra/CA725.pdf: "...'Current Human Exposures Under Control' ... indicates that there are no 'unacceptable' human exposures to 'contamination' (i.e., contaminants in concentrations in excess of appropriate risk-based levels) that can be reasonably expected under current land- and groundwater-use conditions (for all 'contamination' subject to RCRA corrective action at or from the identified facility (i.e., site-wide))."

[15]"Unacceptable" contamination levels refer to contaminant concentrations in excess of appropriate risk-based levels.

[16]From the form used to report the completion of the corrective action, found at https://www3.epa.gov/region1/cleanup/rcra/CA750.pdf: "...'Migration of Contaminated Groundwater Under Control' ... indicates that migration of 'contaminated' groundwater has stabilized, and that monitoring will be conducted to confirm that contaminated groundwater remains within the original 'area of contaminated groundwater' (for all groundwater 'contamination' subject to RCRA corrective action at or from the identified facility (i.e., site-wide))"

benefits of corrective actions might not be reflected in the housing market; this does not mean that cleanups have no value, since the public may still value these cleanups *had it known* about them (Cassidy, 2023; Gayer et al., 2000; Ma, 2019). We next test for housing impacts with data.

3 Data

3.1 Data Sources

Data come from the following sources: (1) RCRAinfo Corrective Action Program cleanups, and (2) US Census Bureau Decennial Censuses in 1990, 2000, and 2010.

RCRA Corrective Action Program Cleanups Data on Corrective Actions (CA) cleanups come from the RCRAinfo database, which is publicly available from the EPA. We begin with all sites listed in the 2005, 2008, and 2020 CA baselines as of September 2019 (Figure 1). Each facility is identified by a unique waste handler identifier. Several attributes of the handler are available, including the location of each facility, the primary industry to which it belongs (3-digit NAICS code), whether the facility is a waste generator, transporter, or treatment, storage, or disposal facility (commonly referred to as a TSDF), and the NCAPS ranking.[17]

We focus on the two Environmental Indicators (EIs) to define our cleanup event.[18] The data entry system for the EIs worked as follows: the government official would review data associated with the site, and enter the corrective action into the system with a status code of "NO" or "IN" if the objectives had not yet been achieved.[19] The government official would enter the status code of "YE" if the objectives had been achieved. This means that for some facilities, there are multiple dates associated with the corrective action. We take the date of cleanup to be the date on which the last entry was made for either of the two EIs, whichever comes later. We take the date at which cleanup began to be the date of the first entry associated with either of the two EIs, whichever comes earlier. Our post-cleanup indicator variable is missing whenever the year of observation overlaps with the time that cleanup is in-progress

[17] As the NCAPS ranking can change over time, we use the earliest NCAPS ranking associated with a facility.

[18] See Section 2 for details on EIs.

[19] Data on the EIs was entered by EPA and EPA's state, tribal and local government partners.

for a given facility; this way, we are not capturing what happens during cleanup.

The main advantage of using the EIs to define the cleanup event is that most RCRA facilities have achieved at least one of the EIs.[20] The majority of facilities have at least one of the two EI's.[21] This contrasts with other corrective action milestones we could have chosen to define cleanup. The most obvious alternative definition of cleanup would be to use the two final remedy indicators, "Performance Standards Attained" and "Corrective Action Process Terminated," as these were the ultimate goal of the program. But, many facilities did not have an entry for either of the two final remedies.[22] Furthermore, the majority of facilities have not yet achieved the final remedy indicators– many of the dates that do exist in the database for the two final remedies are future dates at which the facility plans to meet the final remedy milestone, some occurring as late as 2050.[23]

Decennial Census Data From the decennial Censuses, we collect census tract-level statistics on the value of owner-occupied housing (reported in various price bins), counts of houses sold in each price bin, and neighborhood demographic characteristics related to, e.g., race, income, and education. Using the counts of the number of houses in each price bin, we construct deciles of the price distribution for each census tract following Gamper-Rabindran et al. (2011).[24] Tracts may expand or condense over time; this necessitates a method to compare tract-level information over time. We do this with the Longitudinal Tract Data Base (LTDB) (Logan, Xu and Stults, 2014),

[20] Not every facility is subject to both EIs. For example, if groundwater contamination is not a concern, then the Groundwater EI will not be entered.

[21] Out of the 1,514 RCRA facilities in our dataset, only 44 facilities did not have an entry for either of the two EIs. See the subsection on Data Construction (subsection 3.2) for details on the set of RCRA facilities in the analysis.

[22] Specifically, 800 of the 1,514 RCRA facilities in our dataset did not have a final remedy entry. See the subsection on Data Construction (subsection 3.2) for additional details.

[23] Nevertheless, we show an event study graph in Figure A.10 in the appendix where we define the beginning of cleanup as the date of the first entry of both EIs and both final remedies, and the cleanup date as the date of the last entry of both EIs and both final remedies. See Section 5.2 for a discussion of this figure, which is difficult to interpret.

[24] Gamper-Rabindran et al. (2011) show their decile approach can detect similar magnitudes of benefits from cleanup as approaches using repeat-sales data. An alternative source of census data, which would give higher frequency housing price data, would be the American Community Survey (ACS) (e.g., the ACS 1-year estimates). However, There is a tradeoff between higher temporal frequency and geographic coverage: the ACS 1-year estimates only sample areas with populations of 65,000 or more; while the ACS 5-year estimates sample all areas, statistics are an average over a five year period. Given that RCRA presence and cleanup could be relatively quick and endogenous to community size, we choose to use the Decennial censuses.

which interpolates census summary statistics from different decennial censuses into estimates based on 2000 or 2010 tract boundaries. Using the LTDB, we construct a panel data set of year 2000 census tracts over the three decennial census years: 1990, 2000, and 2010.

It is worth understanding how housing price data collected from Decennial Censuses might compare with individual transaction data. The first consideration here is the use of survey data in this context. In collecting Decennial Census data, homeowners are asked, "About how much do you think this house and lot, apartment, or mobile home (and lot, if owned) would sell for if it were for sale?" The disadvantage of using survey data is that homeowners might have an inaccurate view of how much their home is worth. The advantage of using survey data over individual transaction data is that not every house is sold every year, and so frequently transacted houses tend to be over-represented in transaction data. Those also tend to be more likely to be "flipped."[25]

The second consideration is geographic data coverage- twelve states in the United States are non-disclosure states, where transaction data does not represent sale prices. Instead, the recorded transaction values are an opaque combination of mortgage values and whatever the buyer and seller want to portray the price as. For example, Cassidy (2023) notes, "Looking through county records, one can see such sale prices as '\$10,' 'Love and affection,' and 'My boat,' calling into question the data quality of data sources based only off of county records in the Texan context." These states are: Alaska, Idaho, Kansas, Missouri (some counties), Mississippi, Louisiana, Wyoming, Utah, Texas, North Dakota, New Mexico, and Montana. These non-disclosure states account for 18% of observations in our sample, and 14.2% of sites in the cleanup program.

It is also worth discussing our use of deciles of the housing price distribution. These are the most appropriate dependent variable because our research question is about distributional impacts. An additional advantage of using percentiles (or any moment of the distribution) of the housing price instead of housing price data itself is that percentiles have a less skewed distribution. Researchers often use the logarithm of price as the dependent variable in regressions with transaction data to alleviate concerns with outliers posing a threat to small-sample inference when the distribution

[25]Despite these drawbacks, we do present robustness checks using individual transaction-level data in Appendix Section B.

of the dependent variable is extremely skewed. We show histograms of these price percentiles in Figures A.1 and A.2 in the appendix.[26]

3.2 Data Construction & Summary Statistics

We construct a national dataset linking our outcomes of interest (housing and sociodemographic variables) to RCRA cleanups. For the housing data, we first identify all census tracts for which any portion of the tract's boundary is within a 10-kilometer (km) buffer of a RCRA Corrective Action baseline facility. Using this spatial relationship, we then create a census tract-by-year level data set that describes whether the nearest RCRA site has been cleaned by January 1 of that year, the deciles of the census tract's housing price distribution, and sociodemographic characteristics of the tract in that particular year.

We limit the sample to tracts within 10 km (minimum distance) of RCRA sites in order to avoid comparing neighborhoods that are very different. For example, Table 1 provides summary statistics of census tract characteristics by whether a tract is within 10 km of any RCRA facility.[27] Areas with facilities have lower housing prices in the higher deciles of the price distribution, have lower income, and are more diverse. They are slightly less likely to be on public assistance, or below the poverty line. That these observable characteristics are correlated with RCRA site location suggests that other correlated, unobserved factors may exist. Our initial sample limitation thus removes some of these potential unobserved confounders, assuming that the composition of tracts is relatively constant around 10 km away from the RCRA sites.[28]

We further limit the sample to areas within 10km of a single RCRA facility so that cleanup timing is well-defined. A concern is that this sample restriction reduces

[26] To get a sense of the how the skewness compares between these percentile variables and raw transaction price data, one can compare these histograms to appendix Figure B.2, which presents the distribution of raw transaction prices from Ohio and Pennsylvania for the treatment and comparison bins. Also note that the quantile treatment effects from our robustness check using raw transaction data are invariant to monotonic transformations, including the logarithmic transformation, as discussed on p. 75.

[27] The initial dataset of RCRA facilities contains 3,779 sites, but 156 sites did not have any data within 10 km.

[28] Note that our identification strategy does not rely on the characteristics being similar, but rather that tracts different distances away from the RCRA site (but all within 10 km of an RCRA site) would have parallel counterfactual trends in our outcome variables. To probe this assumption, we provide diagnostic event-study figures for all price deciles and all neighborhood characteristics in the appendix.

the external validity of the results, since doing so removes more than 50 percent of the tracts.[29] Table 2 examines whether these areas are different than the rest of the census tracts. Generally, tracts near a single facility seem to be more well-off than those near multiple RCRA facilities.[30]

4 Empirical Models

4.1 Housing Price Impacts

We begin with the following difference-in-differences (DID) strategy to estimate the impact of RCRA cleanups on housing prices:

$$Y_{it}^{k} = \beta_1 Post_{it} + \beta_2 Near_i^d + \beta_3 Near_i^d \times Post_{it} + \gamma_{sy} + \gamma_{by} + \gamma_i + \epsilon_{it} \qquad (1)$$

In (1), Y_{it}^{k} is the k^{th} decile of the house price in tract i in Census year t.[31] $Post_{it}$ is an indicator variable that takes value 1 if the site nearest to the tract has been cleaned up by time t and 0 otherwise.[32] $Near_i^d$ is an indicator variable that takes value 1 if tract i is within distance $d < D$ km of the RCRA site, where D is the cutoff distance from sites beyond which we do not use observations.[33] Our main results use $D = 10$, and we only use sites within 10 km of at most one RCRA site. γ_{sy} is a set of state-by-year fixed effects. γ_{by} is a set of distance bin-by-year fixed effects,[34] γ_i is

[29] Based on these limitations, we are left with a final sample of Census Tracts near 1,514 RCRA sites, out of a total of 3,623 sites that had Census data within 10 km.

[30] Specifically, tracts near a single facility have higher housing values at higher deciles of the price distribution, higher average household income, lower unemployment rates, are less likely to be college-educated, and have lower shares of Hispanic and Black population than those excluded from our sample.

[31] $t = 1990, 2000$, or 2010

[32] It is missing, and thus the observation is not included in the regression, if the census year coincides with the time between the first EI entry and the last EI entry recorded for the nearest RCRA facility; see Section 3 for more details. The goal here is to exclude situations where we know that the census year falls in an interim period during which the achievement of the EI is in-progress. Had we defined $Post_{it}$ as simply an indicator for the census year being after the last recorded EI entry, we would expect to find a pre-trend in housing prices because the cleanup actually began before our $Post_{it}$ indicator switched on.

[33] In equation (1), we include the variable $Near_i^d$ by itself for purposes of exposition. However, the corresponding parameter β_2 cannot be separately identified with the inclusion of tract fixed effects γ_i.

[34] In this specification, there are two distance bins- Near and Far. The number of distance bins is expanded in equation (2).

a set of tract fixed effects, and ϵ_{it} is a (hopefully idiosyncratic) error term.

The coefficient on the interaction between the two indicators, β_3, estimates the change in price Y_{it}^k after RCRA cleanup for units near the site relative to the same change for those far from the site. This parameter represents the causal impact of cleanup on price under the assumption that the changes in prices of tracts far from (but still within a vicinity of D km of) a RCRA site represent a valid counterfactual for what would have happened to tracts near the site if the nearby RCRA site was not cleaned.

The baseline regressions include tract, bin by year, and state by year fixed effects. The tract fixed effects account for idiosyncratic time-invariant features of the tract and net out unobservables that might be correlated with being near a RCRA site.[35] The staggered treatment timing in our context allows us to use bin-by-year fixed effects to net out time-varying unobservables affecting all homes in each distance bin. We use data from homes in each bin near to facilities that were not cleaned up in a given year, and homes near facilities that were not cleaned up by the end of our sample; this data can be used as a counterfactual for bin-specific price dynamics. Bin-by-year fixed effects address the concern that homes closer to and far away from RCRA sites are not on parallel trajectories over time; for example, awareness of the harms associated with living near a RCRA site could grow over time nation-wide and could mean that price growth in the near bin lags price growth in the far bin. Lastly, state-by-year fixed effects allow for time-varying trends at the state-level that coincide with cleanup and affect housing price.[36]

The DD model above *a priori* assumes an exposure distance ending at d km. A method researchers often use to determine the exposure distance is to flexibly fit a curve between pre- and post- event price data and distance, and use where the curves cross to determine exposure. The method was popularized by Linden and Rockoff (2008), and is employed in Figure A.9 in the appendix. In the figure, the 95% confidence intervals for the two fitted curves overlap for the entire range of

[35] Tract-level fixed effects render time-invariant tract-level controls unnecessary. We worry that time-variant controls might be endogenous to housing prices.

[36] Our results are generally robust to exclusion of state by year fixed effects, but not to the exclusion of bin by year fixed effects. This indicates that perhaps the homes closer to and far away from RCRA sites are not on parallel trajectories over *time*; this is not an issue if we are willing to assume that price growth for the homes farther away from RCRA sites serves as a valid counterfactual for those in the near bin over *event time*. We provide detailed event study graphs as reassuring suggestive evidence in 5.2.

distance we study, leading to no conclusive exposure distance cutoff.

Instead, we will estimate cleanup impacts at 1 km distance bins to empirically determine the point at which exposure to RCRA sites no longer matter. Because our measure of distance from a tract is the minimum distance from the nearest facility to the *boundary* of the tract, we break up the $[0, 1)$ km bin into a 0 km bin (for tracts on which the facility resides) and a bin that contains tracts whose boundary is $\in (0, 1]$ km. We specify seven distance bins, indexed by $d = 0, ..., 6$, the last of which captures distances from 5-10 km. From (1), we substitute $Near_i^d$ with a 0 km bin (indicating the facility is on the tract) and 1 km distance bin indicators up to 5 km ($Dist_i^{(d-1,d]}$ for $d = 1, \ldots, 5$):

$$Y_{it}^k = \alpha_1 Post_{it} + \alpha_2^0 Dist_i^0 \cdot Post_{it} + \sum_{d=1}^{5} \alpha_2^d \left(Dist_i^{(d-1,d]} \cdot Post_{it} \right) + \delta_{st} + \delta_{bt} + \delta_i + \epsilon_{it} \tag{2}$$

Since we exclude the $d = 6$ distance bin indicator from the summation in (2), all effects α_2^d are relative to this distance bin, and α_1 can be seen to capture effects for this bin. In other words, the coefficients α_2^d for $d = 0, \ldots, 5$, would return the impact of cleaning up a RCRA site located in bin d relative to the impact of an additional cleanup between 5 and 10 kilometers away.

Our definition of distance to site is based on the distance from tract to site since our unit of analysis is a tract. Tracts can be greater than 1 square km in area, so our 0 km bin will naturally contain houses close but not next to the site, and analogously, each of our tract-based distance bins will be nominally smaller than the actual distance from house to site. In standalone appendix section B, we provide summary statistics describing how tract-based distance bins translate to actual distance to site for transaction data from Ohio and Pennsylvania, as well as a thorough discussion of how distances compare. The average actual distance is approximately 2.5 km for our 0 km bin, and increases by about four times in the comparison bin (5-10 km). Distance from tract to site serves as a good proxy for distance from house to site, at least relatively speaking.

In what follows, we additionally present robustness checks and alternative price specifications, including an event study specification, a series of estimators and corresponding event studies which are robust to heterogeneity over time and cleanup timing, and specifications designed to capture potential heterogeneity by facility characteristics and reliance on public water. The methodology used for each is described alongside corresponding results in Section 5. As an additional robustness check, standalone appendix section B uses individual transaction data from Ohio and Pennsylvania to estimate quantile treatment effects using a recentered influence function regression.

4.2 Neighborhood Composition and Sorting

The preceding property value hedonic model investigates how RCRA cleanups have impacted housing prices at different points in the price distribution. If the price effects vary across the distribution and since pollution is often located in the less desirable neighborhoods within a locality, then remediation has the potential to reverse exposure to such nuisances and decrease gaps in pollution exposure based on socioeconomic status. Of course, the positive distributional impacts may be completely undone by re-sorting in response to cleanup. This would be made more likely if cleanup effects are large enough to trigger endogenous neighborhood change, further altering the composition of a neighborhood.

We will undertake two tests for neighborhood change- a reduced-form approach described in Section 4.2.1, and a structural approach described in Section 4.2.2.

4.2.1 Reduced-Form Investigation of Neighborhood Change

We assess the potential for re-sorting using the Decennial Census. We first check whether cleanups yielded changes in the composition of residents by changing the dependent variable in our main specification (equation 1) to be one of the following 17 outcomes from the Census:

- Income/Education: average household income, percent below poverty, percent college educated, percent on public assistance, and percent of the population that is unemployed

- Demographic: percent of the population that is Black, percent of homes with a female head of household, percent of the population that is Hispanic, population density, percent of the population that is white, and percent of the population that is under 18 years old

· Housing: percent of homes with four or more bedrooms, percent of homes built in the last 5 years, percent that are mobile homes, percent of households that moved in the last five years, percent of homes that are owner occupied, and percent of homes that are vacant

An advantage of checking for changes in neighborhood composition in this manner is that we can estimate cleanup's impacts at the exact same geographic scale and with the same power as our hedonic analysis, making the tests comparable.[37] Any significant changes in neighborhood composition would suggest that differential sorting in response to cleanups took place, potentially un-doing any positive distributional effects of cleanup.

4.2.2 Structural Sorting Model

A limitation of our reduced form checks on demographic changes, however, is that without knowing the characteristics of the origin and destination of a mover, it is difficult to determine whether a person is actually moving away from or towards pollution. This identification concern was raised by Depro et al. (2015) on using aggregate data to test for sorting behavior.[38] Thus, sorting behavior may be present even without aggregate changes in neighborhood composition. We apply a structural sorting model to census data, as proposed in Depro et al. (2015), to overcome this identification problem and test for sorting behavior. We modify their approach to accommodate our strategy to control for unobserved heterogeneity correlated with pollution.

We build a simple model of how people sort into neighborhoods. Suppose that an individual, at time period t, observes the characteristics of all locations in that

[37] Note that studies using disaggregated price data typically only have access to data on socio-economics and other housing-related outcomes at a more disaggregated level. This often means the setup of the hypotheses they test are not comparable. This could result in rejection of the null hypothesis of no effect on housing prices but failure to reject the null hypothesis of no effect on socio-demographic outcomes, simply because more disaggregated housing data often results in increased precision of estimates when studying localized treatment effects.

[38] Sorting behavior is characterized by the tendency to stay or move between locations, i.e. "transition probabilities". For example, if there are 2 locations, then there are 4 values that characterize movement (including the decision to stay in a particular location). Aggregate data by location, however, only reveal how each location's population changed. The identification issue boils down to trying to identify more variables (i.e., the 4 transition probabilities governing movement) with insufficient information (i.e., overall population changes at 2 locations). See Depro et al. (2015) for specific examples of the identification problem.

period, and decides whether to move to a different location by time period $t+1$. Specifically, she chooses whether to live in one of J neighborhoods within a county (characterized by census tracts), to move out of the county $(J+1)$, or to stay in her current location. Individual i's preference for tract j follows:

$$U^i_{j,t} = \delta_{j,t} + \epsilon^i_{j,t} \tag{3}$$

where $\delta_{j,t}$ represents the average utility that all residents receive from living in tract j at time t; $\epsilon^i_{j,t}$ is the idiosyncratic utility that the individual receives from locating in j, which is assumed to be distributed Type I Extreme Value. The mean utility, which captures the attractiveness of location j at time t, can be thought of as a quality of life index (e.g., Blomquist, Berger and Hoehn (1988), Kahn (1995), Albouy (2016)) that is determined by the location's attributes:

$$\delta_{j,t} = X_{j,t}\beta + \xi_{j,t} \tag{4}$$

These characteristics include ones that are observed $(X_{j,t})$, such as proximity to RCRA sites, and those that are unobserved $(\xi_{j,t})$. The coefficient β on a particular X, e.g., RCRA cleanup, represents the preference for cleanup, where $\beta > 0$ $(\beta < 0)$ means that the individual derives positive (negative) utility from cleanup and would sort towards (away from) cleaned locations. Moreover, differences in β by socioeconomic status would reveal differential sorting behavior.

If the individual chooses to move to tract j during a period t (from, e.g., tract k), then she incurs a financial moving cost $MC_{j,k,t}$, characterized as 3 percent of the average housing value of the origin location (tract k) plus 3 percent of the average house value of the destination location (tract j) during time t. Thus, the utility that she receives from moving from k to j is:

$$\Delta U^i_{j,k,t} - \mu MC_{j,k,t} = (\delta_{j,t} - \delta_{k,t}) + \left(\epsilon^i_{j,t} - \epsilon^i_{k,t}\right) - \mu MC_{j,k,t} \tag{5}$$

The individual will choose the location that maximizes her utility. Given the distribution of the idiosyncratic error term ϵ, the share of people that moves from k to j in the population during time t is characterized by the following logit probability:

$$s_{j,k,t} = \frac{e^{\delta_{j,t} - \delta_{k,t} - \mu MC_{j,k,t}}}{\sum_\ell e^{\delta_{\ell,t} - \delta_{k,t} - \mu MC_{\ell,k,t}}} \tag{6}$$

Similarly, the share of people staying in tract k in time t is given by:

$$s_{k,k,t} = \frac{1}{\sum_{\ell} e^{\delta_{\ell,t} - \delta_{k,t} - \mu MC_{\ell,k,t}}} \tag{7}$$

By definition, the population in tract j in $t+1$ is the sum of all people who move to j from each of the $J+1$ neighborhoods during time t. We can therefore use the above shares to relate population counts across time periods t and $t+1$ in the following manner:

$$pop_j^{t+1} = \sum_{k=1}^{J+1} s_{j,k,t} pop_k^t \tag{8}$$

To estimate the preference parameters governing moving decisions, we then use equations 6 through 8 to predict two quantitative measures that are available in the Census data: (1) the total and share of the population in each tract, and (2) the share of the population that stayed in the current residence. For consistency, we use the decennial census years $(t, t+1) = (1990, 2000)$ and $(t, t+1) = (2000, 2010)$, similar to our hedonic model. The following describes the prediction of these shares:

1. We obtain the total and share of a particular group R, e.g., non-Hispanic Black, for each tract in different census years. We denote the population of group R in year t for tract j as $pop_j^{R,t}$. Dividing both sides of equation 8 by the total population in the region and using group-specific movement shares (i.e., $s_{j,k,t}^R$), we can predict the share of group R living in tract j at time $t+1$ using the time t population shares:

$$\sigma_{R,j}^{t+1} = \sum_{k=1}^{J+1} s_{j,k,t}^R \sigma_{R,k}^t \tag{9}$$

   Here, $\sigma_{R,k}^t$ and $\sigma_{R,j}^{t+1}$ are the share of group R, respectively, living in tract k at time t and tract j at $t+1$.

2. We also obtain the share of the population that stayed in the current residence from the Census.[39] Our model predicts the share of people who chose to stay in their time t locations at time $t+1$ using aggregate population counts in each

[39]The survey asks in what year the householder (for 1980 on) moved into the dwelling unit (apartment, house, or mobile home). IPUMS then recodes the responses as the number of years ago that the householder moved into the unit. Given our model, people who move within a tract will be not experience a change in mean utility, but will still incur a moving cost.

location:

$$\%Stay_R^{t+1} = \frac{\sum_{k=1}^{J+1} s_{k,k,t} pop_{R,k}^t}{totpop_t} \tag{10}$$

With the moving share predictions (equation 9), the stay share predictions (equation 10), and the corresponding estimates from the Census, we estimate our parameters of interest using the following two-step procedure:

Step 1 We first solve for the moving cost parameter μ and the vector of mean utilities $\delta_{j,t}$ using a bisection method that nests a Berry (1994) contraction mapping. Specifically, given a guess of μ, we use equation 9 and a guess of the mean utilities at time t, $\delta_{j,t}^{(old)}$, to predict the population shares at $t+1$. We update the vector of mean utilities to be $\delta_{j,t}^{(new)}$ according to the following rule until the vector of mean utilities has converged:

$$\delta_{j,t}^{(new)} = \delta_{j,t}^{(old)} + \log \sigma_j^{t+1} - \log \tilde{\sigma}_j^{t+1} \tag{11}$$

Recall that σ_j is our prediction of population shares (based on a guess of the parameters); we add a "$\tilde{}$" to indicate the corresponding shares from the data. We next combine the converged vector $\delta_{j,t}$ and the initial guess of the moving cost parameter μ to predict the share of stayers using equation 10. We then update our guess of μ using a bisection method, solving for the vector of mean utilities ($\delta_{j,t}$) at each guess of μ. We repeat this process separately for each group R and for each time period, $(t, t+1) = (1990, 2000)$ and $(t, t+1) = (2000, 2010)$, resulting in group- and time-specific μ and $\delta_{j,t}$ estimates. Lastly, since we limit focus on within-county relocation decisions (but allow individuals to leave the county as a catchall decision), we estimate the model separately for each county.

Step 2 We next stack the mean utility estimates for each group R, in each county and each time period (2000 and 2010), and decompose the mean utility with respect to tract characteristics (interacted with race indicators) to recover preferences. Before doing so, we make the mean utilities comparable across groups, time, and location by dividing the mean utility estimates for a particular group/time/location by the corresponding moving parameter estimate, $\widehat{\delta}_{j,t}^R = \delta_{j,t}^R \,/\, \mu_t^R$. In the final step, we estimate the following specification:

$$\widehat{\delta}_{j,t}^R = \beta_0 + \beta_1 Dist_j^0 + \beta_2 Dist_j^0 \times 1[R = B] + \beta_3 Dist_j^0 \times 1[R = H] + \gamma_j + \xi_{j,t} \tag{12}$$

where $1[R = B, H]$ is a group indicator and $Dist_j^0$ are as previously defined, and γ_j is a tract fixed effect. The groups we examine are non-Hispanic white ($R = W$), non-Hispanic Black ($R = B$), and Hispanic ($R = H$). The coefficients β_2 and β_3 are, respectively, the Black-white gap and Hispanic-white gap in willingness to pay to live in location j, before and after RCRA cleanup. In this stacked regression, each observation is a particular race in a particular tract, in either 2000 or 2010.

5 Results

We organize our results as follows. In Section 5.1, we discuss the results of our main specification. In Section 5.2, we present event studies to check for pre-trends driving our main results. In Section 5.3, we use estimators robust to heterogeneity in both time and treatment timing. In Section 5.4, we probe for heterogeneous impacts by facility characteristics and reliance on public water. In Section 5.5, we present and discuss results from our reduced-form and structural investigations of sorting.

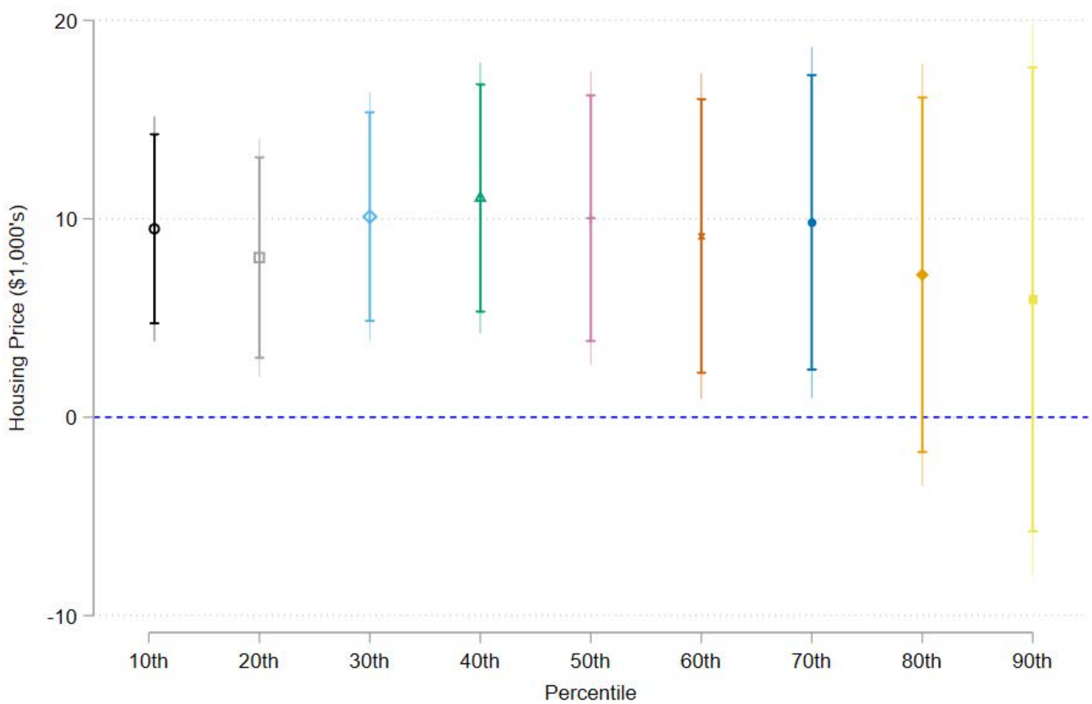
5.1 Impacts on Housing Prices

We test the specifications proposed in equations (1) and (2) on all nine deciles of the price distribution and from within 10 km away from an RCRA facility. To start with, we employ a flexible exposure buffer specification (2) to see how far the treatment effects might extend. Table 3 presents the initial set of regressions for each decile of the price distribution. We are interested in the coefficients on the interaction effects between distance bins and the post-cleanup indicator. We excluded the 5–10 km bin, so all of our estimates can be interpreted as the differential effect of cleanup on homes in a particular distance bin and homes in the 5–10 km bin. Except for the 0 km distance bin, the coefficients on the interaction terms are mostly insignificant. The 0 km bin stands out as having large and significant impacts for most price deciles.

Summary statistics by whether the tract was in the 0 km bin can be found in Table A.1 in the appendix. Housing prices and income tend to be higher in the 0 km bin, but those living in the 0 km bin are more likely to be Black, less likely to be college graduates, and are more likely to be below the poverty line or on public assistance.

Figure 2 plots the coefficients for the change from before to after cleanup in the

Figure 2: 0 km bin coefficients by decile from Table 3.



0 km distance bin (α_2^0) across the deciles of the price distribution. The effects are largest in percentage terms for the lowest percentiles of the price distribution. For the tenth percentile price distribution, the effect is \$9,491, on a base average price of \$84,237– a change of 11.2%. While effects are diminishing in percentage terms, they stay roughly the same in levels until about the 80th percentile. The effects become less statistically significant after the 60^{th} percentile of the price distribution, and are not statistically distinguishable from 0 for the 80^{th} or 90^{th} percentiles.

Results from a more parsimonious specification, following equation (1), are presented in Table 4, where we have grouped all homes 0–10 km away from the facility into one group so that we have just two distance bins. The impacts on the price percentiles range from \$7,828 to \$10,881 depending on the percentile, and are statistically significant at the 10% level, except for the impact on the 90^{th} percentile of the price distribution. Although the magnitudes appear to be relatively similar in levels, in percentage terms, the impacts are stronger for the lower percentiles of the price distribution. For example, the relative impact on the 10^{th} percentile price for the 0 km bin versus the 0–10 km bin is around 10.1%, but for the 90^{th} percentile price specification, the percentage difference is only approximately 3.0%.

In Table 5, we show the coefficients on our 0 km $\times$ Post dummies from our parsimonious specification for each price decile, varying the fixed effects and level of clustering for standard errors. Each cell is a single coefficient from a separate regression. In column 1, we test robustness to the exclusion of state-year fixed effects. We find that when we only use bin by year fixed effects and do not use state-year fixed effects as in our main regressions (as in Table 4), the estimates are slightly less precise but similar in magnitude. In the second column, we reproduce our main results from Table 4 for comparison. In the third through fifth columns, we show that the results are less precise when clustering on county, site-by-bin, and site, but the overall pattern of significance holds up (results are significant for the 10^{th}-70^{th} percentiles, and insignificant for the 80^{th} and 90^{th} percentiles).[40]

[40] It is ideal to cluster at the level of treatment (Bertrand, Duflo and Mullainathan, 2004; Abadie, Athey, Imbens and Wooldridge, 2022), but the level of treatment is ambiguous in our context–census tracts are varying distances away from RCRA sites, and thus the impacts from before to after could theoretically differ by these distances. Because we pool the impacts over bins, perhaps bin by site is the level of treatment from the standpoint of our estimation, even though the underlying level of treatment will vary by site. Because RCRA sites are cleaned up at different times, there is also an argument to be made for clustering at the site level to allow for correlation between bins around a given site. Therefore, we show all of these clustering levels.

It is worth addressing how these results compare to those in Guignet and Nolte (Forthcoming), and explaining how our different data sources and identification strategies might affect precision of our estimates. Guignet and Nolte (Forthcoming) state that a main advantage of their paper over ours is the use of individual transaction data. However, their sample size drops from 28,312 identifying transactions to just 829 in their house fixed effects sample, indicating that variation in their study is not coming from repeated sales of individual houses, but rather from sales of *different* houses in the same tract in different years. So, even if their lefthand-side variable differs at the individual sale level, the identifying variation still is within tract over time- identical to ours.

Moreover, there are often two concerns with aggregated data: aggregation bias and loss of precision. Aggregation bias would attenuate our estimates towards zero, and we would expect large confidence intervals due to loss of precision. However, we still detect economically and statistically significant effects comparable to those of Guignet and Nolte (Forthcoming). While the comparison is hard to interpret since we study deciles of the price distribution and Guignet and Nolte (Forthcoming) study average effects, we are doubtful that aggregation bias is a major problem in our context.

Considering that (a) their primary source of variation is cross-sectional, and (b) we find economically and statistically significant impacts similar to theirs, it does not appear that the use of aggregated data is a major drawback of our study compared with theirs.

Furthermore, an even broader point applies- with Zillow no longer available to researchers, it is useful to show that results like those of Guignet and Nolte (Forthcoming) replicate using publicly-available data. That our study finds similar effects to Guignet and Nolte (Forthcoming) is further evidence that using Census tract data can detect similar effects as individual-level transaction data, a finding documented in Gamper-Rabindran and Timmins (2013).

We additionally include a standalone supplementary appendix (Section B) which shows robustness of our conclusions to using individual-level transaction data from Ohio and Pennsylvania. Section B.1 puts our effects into context by describing how Census tract-based treatment translates to distance from a given house to a RCRA site. Section B.2.1 describes our quantile treatment effects model and assumptions. Section B.2.2 shows quantile treatment effect results. The broad conclusion is that our main results are robust for the lowest deciles of the price distribution, but not the higher deciles. We find the strongest evidence of a positive cleanup effect for the first decile, both in economic and statistical terms.

Overall, we document robust evidence of capitalization of RCRA cleanups into housing prices, especially at the lower deciles of the price distribution. This could indicate either that citizens are aware of and directly value the cleanups, or that they value the redevelopments and other aspects of area revitalization that are sometimes bundled with the cleanups. No matter which is the case, the impacts we document here are noteworthy given the vast scope and expense of the RCRA cleanup program– the program has provided \$97.3 million in federal grant funding to state governments.[41]

5.2 Event Study of Impacts on Housing Prices

One potential threat to identification is differential pre-trends between the houses closest to the RCRA sites and those further away. As suggestive evidence that differential pre-trends do not drive our results, we produce an event-study graph that depicts treatment effects over time. That is, we graph the coefficients for the 10^{th} percentile price from the following regression, for the 0 km bin, treating the $5-10$ km distance bin as a control group:

$$Y_{it}^k = \sum_{\tau} \beta_{1\tau} \mathbb{1}\left\{t \in [\tau, \tau+2)\right\} + \sum_{\tau \neq -2} \beta_{2\tau} Near_i^0 \times \mathbb{1}\left\{t \in [\tau, \tau+2)\right\} + \gamma_{sy} + \gamma_{by} + \gamma_i + \epsilon_{it} \tag{13}$$

In the above, Y_{it}^k is the house price (kth percentile), $\mathbb{1}\left\{t \in [\tau, \tau+2)\right\}$ is a dummy variable that takes value 1 if the Census year t is between τ and $\tau+2$ years relative to the cleanup period of the nearest site and 0 otherwise, and $Near_i^0$ is a dummy variable that takes value 1 if the facility is on tract i. Excluding one event time in the second summation scales the treatment effect in the two years just prior to cleanup to 0 for ease of interpretation.

The interpretations of parameters in our event study differs slightly from the standard event study because the far bin is able to serve as a control group for the

[41]This statistic is current as of September 2021; see:
https://www.epa.gov/rcra/resource-conservation-and-recovery-act-rcra-overview.

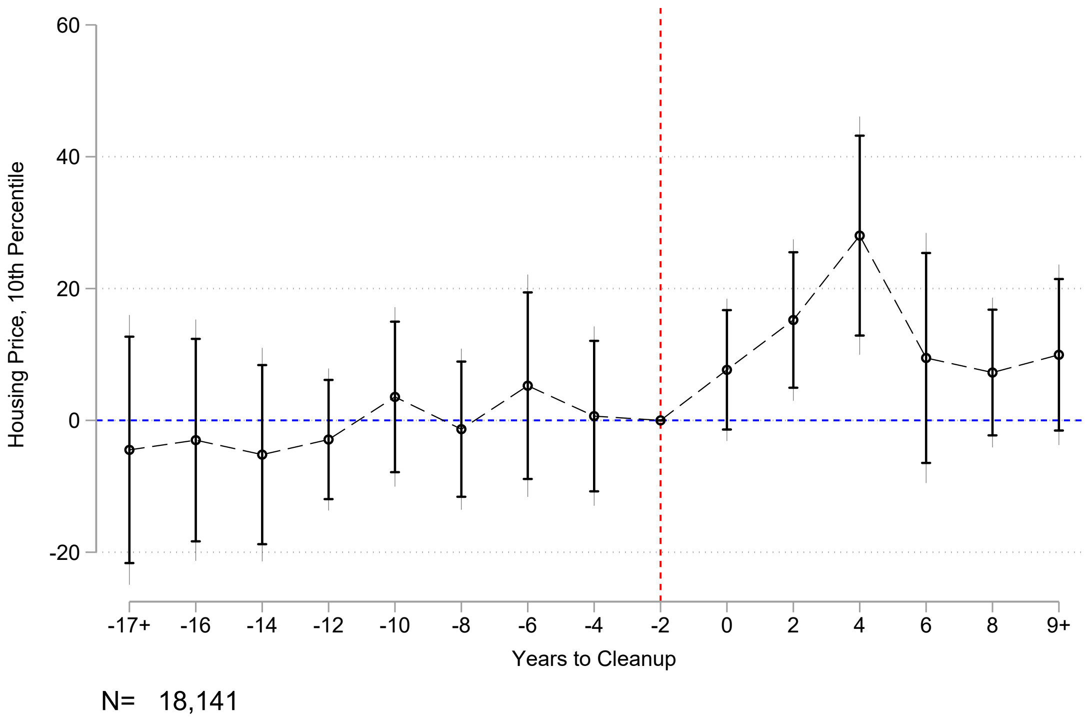
near bin in every event time due to the inclusion of a full set of event time indicators $\mathbb{1}\{t \in [\tau, \tau+2)\}$. As such, $\beta_{1\tau}$ is interpreted as the housing price for homes in the far bin during event time $[\tau, \tau+2)$, net of bin-by-census year, state-by-census year, and tract-level averages. $\beta_{2\tau}$ is interpreted as the difference in the housing price for homes in the near and far bins during event time $[\tau, \tau+2)$, net of bin-by-census year, state-by-census year, and tract-level averages.

It is worth emphasizing that the setup of our event study takes advantage of variation in when sites were cleaned up relative to census years. This is similar to the setup in Asker, Brunner and Ross (2022). We could alternatively measure event time in decades, but that approach fails to leverage the rich data we have on cleanup timing. Our setup can be seen as an unbalanced panel in event time– we only have 3 observations for each site. In Figure A.11 of the appendix, we plot the number of sites represented in each event time in our regressions. While event time -17 has the most sites represented (nearly double the sites represented in other event years), the number of sites is more uniform for other event times.

We focus mainly on the 10th percentile since that was the decile for which we found the strongest effect, in percentage terms. We plot $\beta_{2\tau}$ over time in Figure 3. The figure shows that home prices for the first decile of the distribution increase immediately following cleanup, peaking about 4 years after cleanup, and subsequently decrease but do not reach their pre-cleanup levels within 8 years. We also plot $\beta_{2\tau}$ over time for other percentiles of the housing price distribution in Figures A.4 and A.5 in the Appendix and observe a similar pattern through the 50th percentile.

As an additional robustness check, in Figure A.10 in the appendix, we show the event study using an alternative definition of cleanup that uses information on the date of final remedy events when those are non-missing. In particular, we define the beginning of cleanup as the date of the first entry of both EIs and both final remedies, and the cleanup date as the date of the last entry of both EIs and both final remedies (and discard observations where the Census year falls in between these two dates). The challenge here is that very few of these final remedy events occur in the early years of the program, and so the coefficients on the latter years in event time are based on very few final remedy events. The sample size is also cut significantly due to the fact that we leave out situations where the Census year is between the start and end of cleanup. The event study graph appears to oddly dip after 6 years post-cleanup. However, the interpretation of the dip is unclear because the graph

Figure 3: Event Study for the 10^{th} Percentile of the Housing Price Distribution



Notes This figure shows the coefficient representing the difference in the near and far bins over time. We use the same fixed effects as in the main regression. The coefficient for the two years just prior to the cleanup (at position -2) is normalized to 0 by excluding the dummy on Near× Event time=-2 from the regression. Data from the during-cleanup phase is dropped from the analysis, so time 0 represents the two-year immediately following cleanup completion. Whiskers marked with horizontal lines and vertical protruding segments indicate 95% and 99% CIs respectively, clustering at the tract level. Prices are denominated in thousands of dollars.

masks heterogeneity in what cleanup is. Because the final remedies occurred later in our panel and most facilities have not yet achieved one, the composition of which milestone the event time is based on (whether it is a final remedy or an EI) is changing with event time. In particular, higher event times are less likely to correspond to final remedy events. We might expect that the effect of a final remedy on property values is larger than the effect of an EI because the criteria to achieve a final remedy is more stringent. If final remedies produce larger effects and are underrepresented in later event times, this could produce the odd dip in the graph. Still, this alternative definition suggests housing impacts peak in year 4 and possibly persist 9+ years after.

5.3 Robustness to Heterogeneity Over Time and Cleanup Timing

It is not necessarily the case that our estimates are subject to the criticisms from the staggered adoption differences in differences literature (Gardner, 2022; de Chaisemartin and D'Haultfœuille, 2022; Wooldridge, 2021; Callaway and Sant'Anna, 2021). Indeed, cleanups take the form of a staggered roll-out that constitutes part of our identifying variation. However, we have a control group (far) for the treated (near) in every period, which is not the setup inherent to the staggered DD literature. Further, we have shown that the results are robust to controlling for the treatment timing, by controlling for the post-period in every regression, and by using event time dummies in event studies. This means that the coefficients in our event study plots can be interpreted as differential impacts at a given event time between tracts near and far from cleanup sites.

On the other hand, one might still worry about the fact that, within the near group, the already-treated units might serve as a control group for the treated units, a source of potential bias which is not necessarily solved by the presence of the far bin. To alleviate this concern, we undertake a series of robustness checks following Callaway and Sant'Anna (2021). We focus on the 10th decile of the housing price to parallel our emphasis in other sections of this paper.

The robustness checks are shown in Table 6. For comparison purposes, the first column reproduces our two-way fixed effects estimate with the same controls as our event study model in (13)- that is, it contains bin by year, state by year, tract, and event time fixed effects. The estimated treatment effect on 0 km $\times$ Post can be interpreted as the difference between the near and far bin, from before to after cleanup.

While the first column is an ad-hoc way to address potential heterogeneity over cohorts and time, the second through fifth columns are fully robust to treatment effect heterogeneity along these two potentially important dimensions.

In the second column, we present the two-stage difference-in-differences estimator due to Gardner (2022). The method parallels the design in other parts of our paper more closely than other heterogeneity-robust estimators, and is also most similar in spirit to the TWFE estimates with event time effects in column 1. Estimation proceeds as follows. In the first stage, we regress the price on all the fixed effects (bin by year, state by year, tract, and event time). We construct the residuals from that regression by subtracting estimated fixed effects off of the outcome variable. In the second stage, we regress those residuals on our independent variable of interest, 0 km $\times$ Post. The standard errors are then bootstrapped, clustering at the tract level.

Column 3 presents a simple regression-adjustment adaptation of the estimator in Callaway and Sant'Anna (2021). The design adjusts for all of the fixed effects in our event study model, allowing the dependence on each of these fixed effects to vary by treated and control units. The Callaway and Sant'Anna (2021) method separately estimates an average treatment effect on the treated (ATET) for each survey year and cohort. One requirement is that the survey years and cohorts are measured in the same time units. So, in this sub-section of the paper, we will think of cleanup cohorts, and by extension event time, as denominated in survey years, rather than in time between the survey year and cleanup period.[42] The technique also requires us to exclude sites that were cleaned up before the 1990 Census, since those are already treated at the beginning of the panel. Therefore, the cohorts are defined such that $c \in \{2000, 2010\}$ for observations that are treated during our panel, and the three time periods are $y \in \{1990, 2000, 2010\}$. The method employs a separate regression to estimate what the ATET would be for each cohort $c \in \{2000, 2010\}$ and year $y \in \{2000, 2010\}$, with observations not cleaned up by 2010 serving as control units. Observations in the far bin serve as an additional control group, with the effect for the far bin netted out by the event time fixed effects as in column 1, to fully capture the timing of cleanup and ensure that the estimated coefficient captures a differential effect between the near and far census tracts, as is the case throughout our paper.

In the fourth and fifth columns, we add propensity score weighting. The resulting estimator is called augmented inverse propensity weighting and is doubly robust- that is, it are robust to mis-specification of either one of the propensity or outcome models. In column 4, we use site characteristics (whether the site is a TSDF and the site's NCAPS status) to predict the propensity of 0 km $\times$ Post. In the fifth column, we additionally add tract characteristics to the treatment propensity model-total housing units, population density, percent Black, and percent Hispanic.

We find that the treatment effect estimate is similar across all five columns, with

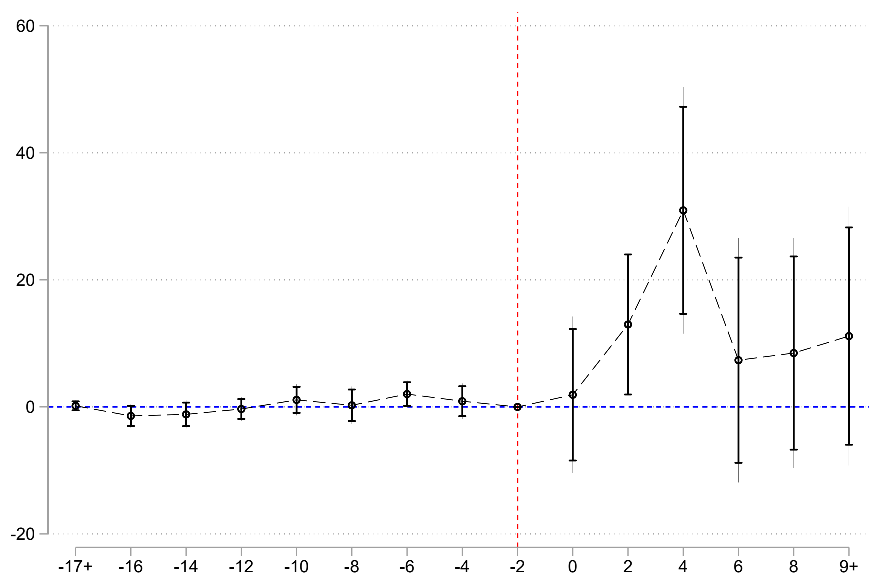
[42] For example, if a site was cleaned up in 2006-2007, the observation of that site in the 1990 Census will be event time -2, the observation of that site in the 2000 Census will be event time -1, and the observation of that site in the 2010 Census will be event time 1.

effect sizes varying between a housing price increase of \$10,517 and \$13,229 due to cleanup. The main effects are fully robust to all five alternative specifications, demonstrating that heterogeneous effects by cleanup timing and year are unlikely to bias our main estimates.

Table 6 also provides p-values that correspond to the hypothesis test where the null hypothesis is that pre-period coefficients are all equal to 0. We cannot reject the null hypothesis of no pre-trend, across any of the five columns.

This finding complements a visual examination of the event studies associated with the four robustness checks (Figures 4 and 5). Figure 4 shows the event study version of the 2-stage difference-in-differences procedure (Column 1 of Table 6). The simple two-stage least squares procedure does not impose that time and event time are in the same units, so the setup allows us to keep the richer definition of event time, making the visual comparison more similar to the event studies in other sections of the paper. We do not find evidence that the estimates are driven by pre-trends.

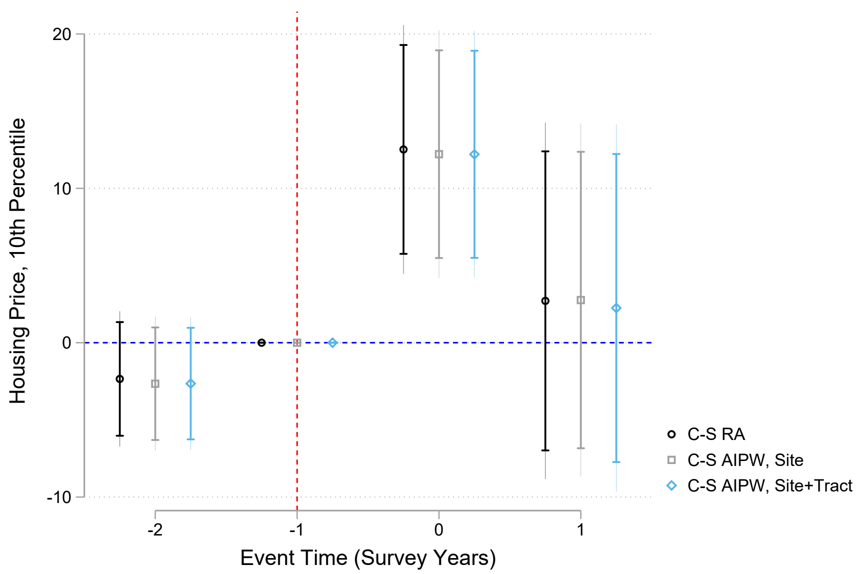
Figure 4: 2-stage Difference-in-differences Event Study (Gardner, 2022)



Notes This figure shows the coefficient representing the difference in the near and far bins over event time for the 10th percentile of price, estimated using 2-stage difference-in-differences. The coefficient for the two years just prior to the cleanup (at position -2) is normalized to 0 by excluding the dummy on Near× Event time=-2 from the regression. Data from the during-cleanup phase is dropped from the analysis, so time 0 represents the two-year period immediately following cleanup completion.Standard errors are bootstrapped, clustering at the tract level. Whiskers marked with horizontal lines and vertical protruding segments indicate 95% and 99% CIs respectively.

Figure 5 shows the event study version of columns 3-5 of Table 6. These event studies have the property that all effects are measured relative to the survey year just before treatment, following the recent recommendations in Roth (2024).[43] The event study coefficients are similar across the three models. Survey event time 0 indicates a clear and large effect, but the effects become noisy in period 1. The large standard errors in period 1 are likely due to the fact that the majority of cleanups in our dataset happen between 2000 and 2010, so we do not see many Census tracts at survey event time 1. In conclusion, we find no evidence that pre-trends drive the results of these robustness checks.

Figure 5: Event Studies for Callaway and Sant'Anna (2021) Estimates



Notes This figure presents the event study corresponding to each of the three robustness checks appearing in columns 3-5 of Table 6. Whiskers marked with horizontal lines and vertical protruding segments indicate 95% and 99% CIs respectively, clustering at the tract level. Prices are denominated in thousands of dollars.

5.4 Heterogeneous Impacts by Facility Characteristics and Reliance on Public Water

Next, we examine whether there are heterogeneous effects on price by NCAPS status by dividing our regression into samples consisting of tracts near high, medium, and low NCAPS status facilities. Overall, we find the strongest effects for the tracts near medium NCAPS status facilities, though the results are often less significant than in our main regressions, perhaps because of the smaller sample sizes. We caution

[43]The transformations for the coefficients comes from Koren (2024).

against over-interpretation of these results, because the timing of cleanups depended on NCAPS status of the facility. As depicted in Figure A.3 in the Appendix, high priority facilities were more likely to be cleaned up earlier in our panel. Therefore, we are not able to distinguish differential treatment impacts by NCAPS status from treatment effects that vary over time.[44]

An important aside is worth noting here: at first glance, it might appear that the NCAPS statuses differing over time point to a potential composition problem with detection of our main estimates. However, it is worthwhile to note that this concern is mitigated by the fact that, in our event studies, we control for both calendar year and *event time*, and do not find a pre-trend. We are able to do this because the far bin can serve as a counterfactual for the near bin in every time period. The finding of no pre-trend indicates that, at the very least, differential treatment timing across sites is not driving our effects.

In the Appendix, we also show results when we limit the sample to only Treatment, Storage and Disposal Facilities (TSDF's) and only Large Quantity Generator (LQG) facilities, respectively (Table A.2). The results are stronger in magnitude and statistical significance for both of these subsamples. This is as expected — TSDF facilities were subject to more stringent cleanup requirements under the RCRA,[45] and LQGs generate more waste than other categories of RCRA facilities.

In the third through sixth column of Table A.2, we divide our sample into three groups based on the fraction of homes using public water sources on the tract, and run our baseline regressions for each subsample, to test for differential price impacts found in other contexts (e.g. Muehlenbachs, Spiller and Timmins, 2016). Private water sources are more likely to be located on or near a residence, whereas public water sources are likely to be much farther away (Hill and Ma, 2019). Private water systems also have different (and generally less stringent) regulatory requirements under the Safe Drinking Water Act (SDWA) than public water systems.[46] These differences suggest that responses to any change in environmental threats at a property

[44]In contrast with the setting of Chay and Greenstone (2005), we cannot leverage discontinuities in an underlying continous variable that determines NCAPS scores. NCAPS is a computer-based system that simply categorizes sites into one of several statuses, but is designed to use and require less site data than would be needed to rank sites based on the Hazardous Ranking Score (HRS). See, e.g., US Environmental Protection Agency (1993).

[45]See https://www.epa.gov/sites/production/files/2015-07/documents/tsdf05.pdf

[46]See information on SDWA compliance at https://www.epa.gov/compliance/safe-drinking-water-act-sdwa-compliance-monitoring.

may be different if water quality is of concern to households.[47] We mostly find no significant effects for these subsamples of tracts. It is unclear what effects should be expected. We might expect that there is a mitigated effect from being on a tract that is mostly served by public water, seeing as the distance in our housing analysis is measured from the tract boundary to the nearest facility, rather than from the public water source that serves the house to the nearest facility. However, if there is a strong correlation between RCRA sites and public water system sources, we might not be able to separately identify effects by high and low proportions of tracts served by public water. Furthermore, we would only expect that effects would differ by private vs public water if people are aware of exposures in their public water and aware of source locations for the public water system, which is a strong assumption. No differential effects by water source might indicate that housing values change because the cleanups are visible to those living on nearby properties and thus salient, irrespective of the water source.

5.5 Impacts on Neighborhood Composition and Sorting

We first test whether RCRA cleanups impacted various socio-economic and housing-related indicators from the Census in a reduced-form framework. Specifically, we examine 17 other outcomes, and find no statistically significant impacts on any of them.[48] In Table 8, we explore impacts on five income and education-related outcomes: average household income, percent below poverty, percent college educated, percent on public assistance, and percent of the population that is unemployed. The impacts are neither statistically nor economically significant.[49] In Table 9, we explore impacts on six demographic outcomes: percent of the population that is Black, percent of homes with a female head of household, percent of the population that is Hispanic, population density, percent of the population that is White, and percent of the population that is under 18 years old. No impacts are statistically significant, with percent Hispanic experiencing the largest change in percentage terms, at about

[47]For example, Muehlenbachs et al. (2016) do not find any housing price impacts of fracking near the house for individuals on public water, but find large negative effects for those relying on private water systems.

[48]This is particularly surprising given that we would expect to find statistically significant effects at the 10 percent level for one out of every 10 outcomes studied, even if no true impacts existed.

[49]The strongest impact in percentage terms is a 0.35 (percentage points) increase in unemployment on a base of 4.824 percent, the sign of which is counter-intuitive.

5% of baseline. But, the sign of the coefficient contradicts a gentrification story, especially given the environmental justice literature finding that Hispanics tend to "come toward the nuisance," or at least do not flee it Depro et al. (2015). In Table 10, we explore impacts on six housing-related outcomes: percent of homes with four or more bedrooms, percent of homes built in the last 5 years, percent that are mobile homes, percent of households that moved in the last five years, percent of homes that are owner occupied, and percent of homes that are vacant. The impacts are not statistically or economically significant.[50]

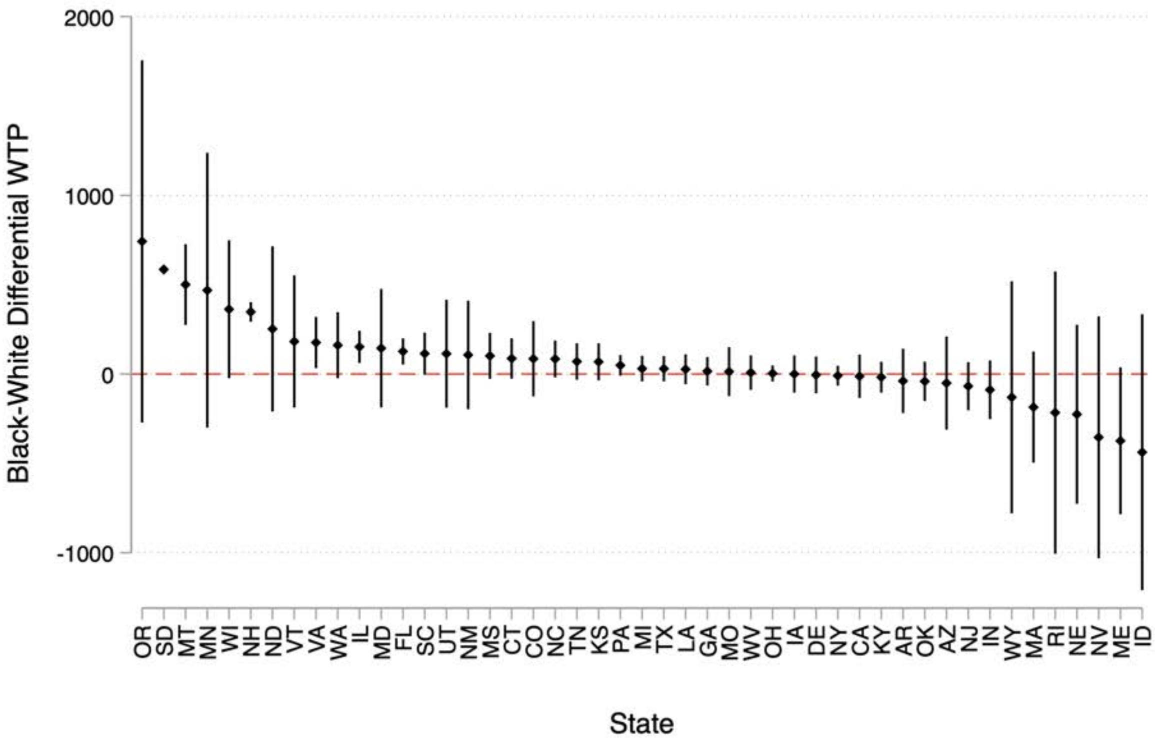
One might be concerned that socio-economic and housing-related indicators would not respond immediately to RCRA cleanups. Households might sort on distance to RCRA facilities with a lag because of moving frictions, even if prices adjust immediately, which would bias us towards finding no sorting even when sorting was indeed happening. To visualize the timing of potential impacts, we also produce event-study graphs similar to those we make for housing impacts in Figures A.6 through A.8. We see no clear evidence of lagged effects.

One trend of note is the upward trend in Average HH Income, which at first glance might seem to point to a gentrification story. On this point, we first note that in column 1 of Table 7, that we cannot reject the null hypothesis of no effect- the confidence interval is five times larger than the coefficient, so we do not find any evidence of gentrification. Even more importantly, the magnitude of the point estimate is only $178, when the average income is $44,209.50. This is an economically insignificant magnitude, so we do not find evidence that gentrification is driving our results.

Despite the reduced form exploration of neighborhood change showing no evidence of gentrification or sorting, we are sympathetic to the view that a reduced form approach might not uncover these effects. Thus, in light of the identification concerns raised in Depro et al. (2015), we also test for differential sorting by race using a structural sorting model. Table 11 presents our differential willingness to pay (WTP) estimates from the mean utility decomposition. We estimate the decomposition using mean utility estimates for the year 2000, 2010, or pooling the estimates from the two decennial years. We limit to tracts containing a RCRA site that has been cleaned by

[50]The strongest impact in percentage terms is an increase of 0.39 (percentage points) in the percent of people living in mobile homes on a base of 7.138 percent, the sign of which is counter-intuitive.

Figure 6: Black-White Differential in WTP



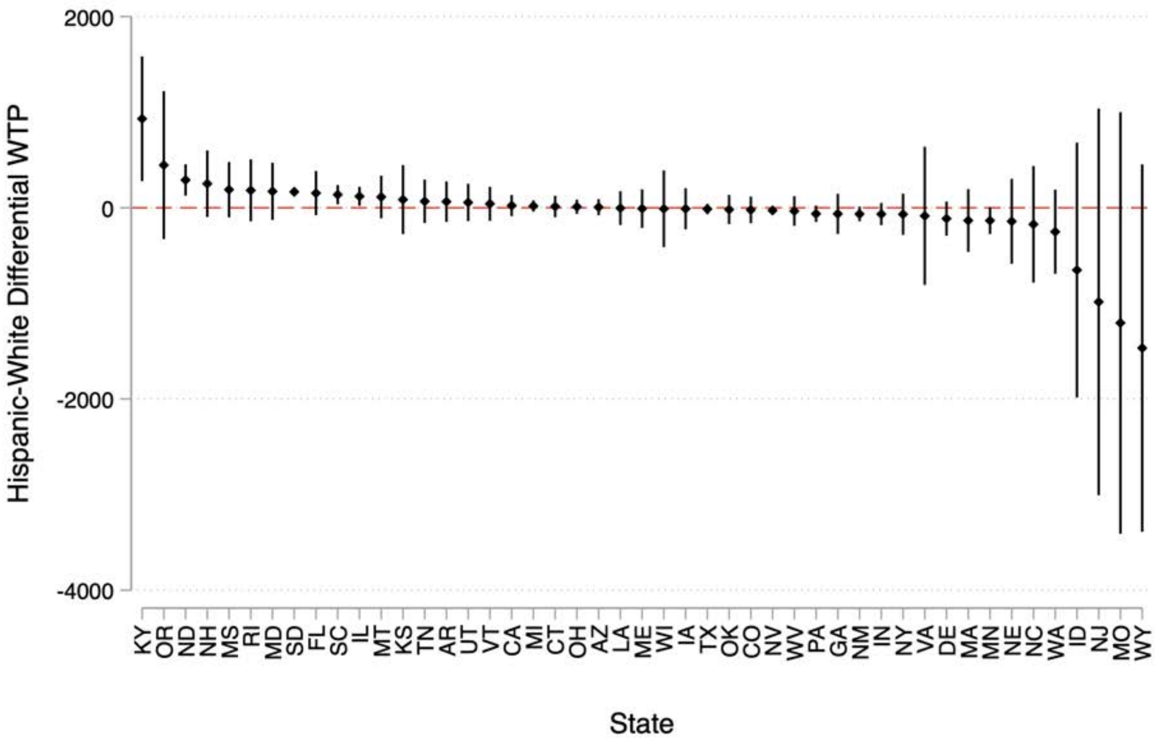
Notes This figure presents state-by-state estimates of the Black-White Differential in WTP from our structural sorting model.

the corresponding decennial year for this analysis, so the estimates corresponding to the year 2000 use changes between 1990 and 2000, and the estimates corresponding to the year 2010 use changes between 2000 and 2010. All specifications include tract fixed effects (and year fixed effects for the pooled model).

Note that a finding of differential willingness-to-pay would indicate a differential *tendency to move*. The estimates should not be thought of as an *actual* willingness to pay from a welfare standpoint, because they do not account for the fact that racial groups might be differentially informed.

Using 2000 as the post-period (so that estimates are based off of changes between 1990 and 2000), we find some weak evidence that Black residents have lower WTP relative to white residents. However, estimates using 2010 as the post-period and the pooled sample with both years suggest that WTP for Black residents are actually higher than that for white residents. In the pooled sample, Black residents are, on

Figure 7: Hispanic-White Differential in WTP



Notes This figure presents state-by-state estimates of the Hispanic-White Differential in WTP from our structural sorting model.

average, willing to pay \$74 more for cleanup than white residents. The WTP for cleanup is not statistically different between Hispanic and white households. The average WTP may be underscored by significant heterogeneity across geography. We investigate the WTP differentials by state and plot the estimates in Figures 6 and 7 for Black and Hispanic groups, respectively. While in some states, the Black group has a significantly higher willingness to pay than their white counterparts, most of the estimated WTP differential are small in magnitude and statistically insignificant. These findings are consistent with our previous tests that find no changes in neighborhood composition in response to RCRA cleanups.

6 Discussion

Our empirical findings yield two broad conclusions. First, we found housing price impacts that were limited to the tracts nearest RCRA sites and strongest in relative terms for the lower deciles of the price distribution. This finding that home prices increased after cleanup is most prominent and robust for the 10th percentile. It is not driven by a pre-trend, and it is nearly unchanged when we implement heterogeneity-robust difference in difference estimators. It is also robust to using transaction-level data to estimate quantile treatment effects.

Several mechanisms could underlie these price impacts, such as stigma (Messer, Schulze, Hackett, Cameron and McClelland, 2006) and endogenous changes in non-targeted amenities (Banzhaf and Walsh, 2013). One important caveat is in order. Unfortunately, the interpretation of price impacts only applies to homeowners. Welfare effects could be negative for poor and minority renters if rents follow home prices and renters are relatively immobile.

Second, we found that RCRA cleanups were unlikely to cause residents to re-sort. Two approaches were used- we found no evidence of neighborhood change using a reduced-form model, and we found no evidence of a differential tendency to sort by race using a structural sorting model.

Taken together, our findings imply that the benefits of cleanup accrued to residents who already lived near the sites, and helped the poorer segment of the housing market more than the richer segment. Our findings are inconsistent with a gentrification story in which more well-off citizens move closer to the sites after cleanup, changing the composition of the population of the tracts on which the sites are located. One possible explanation for the finding of no sorting along these socio-demographic dimensions is that the RCRA cleanups were not a large enough shock, relative to moving costs, to induce increased moving (Palmquist, 1992a). This is corroborated by the finding of no effect on the percent of households who moved in the last 5 years in our reduced-form investigation of neighborhood change.

The lack of evidence of sorting is a hopeful one in light of recent work. Hausman and Stolper (2020) show that when there is partial information in the housing market, people undervalue a clean environment, and households sort according to their willingness to pay for a clean environment on this partial information, and deadweight loss due to pollution is higher for low-income households. If people do not sort after

RCRA cleanups, then these cleanups theoretically could mitigate any pre-existing exposure disparities between the rich and poor that stem from the channels that Hausman and Stolper (2020) pinpoint (partial information and under-valuation of a clean environment). Bakkensen and Ma (2020) present the case where well-meaning policies can cause significant sorting that exacerbates pre-existing disparities in exposure to an environmental bad between advantaged and disadvantaged groups. Our finding that there is no evidence of sorting shows that this need not always be the case.

The fact that we find no sorting also aids in interpretation of our estimates. Our findings suggest that perhaps the shock to housing prices was geographically localized enough to not substantively change the hedonic price schedule, because nearby comparable houses were completely unaffected (Palmquist, 1992b). In this special case, equilibrium prices move with marginal willingness to pay and marginal willingness to pay is reflected by capitalization effects (Kuminoff and Pope, 2014). The fact that we find little evidence of effects on the tracts in distance bins other than the 0 km bin (on which the site is located) suggests that impacts were indeed geographically localized. On the other hand, as mentioned by Kuminoff and Pope (2014), the plausibility of the assumption that the shock to the housing market did not shift or alter the hedonic gradient depends on the magnitude of the change in the distribution of the public good in general.[51]

7 Conclusion

This paper evaluates the housing market impacts of cleanups conducted under the Resource Conservation and Recovery Act (RCRA), an expansive hazardous waste cleanup program. We find that the positive environmental impacts from RCRA cleanups are reflected in the housing market. The price increases that we find are driven by cleanups concentrated among the lowest price deciles of the census tract in which the RCRA facility is located: prices increase by 11% for the 1^{st} decile of the price distribution, and we detect no evidence of a price increase for the 9^{th} decile. This indicates cleanups raise housing values of the poorest segments of the popu-

[51] Kuminoff and Pope (2014) also point out that macro boom and bust cycles could alter the hedonic gradient. We must assume that, for example, shocks to wealth do not impact implicit valuation of water quality or other environmental improvements due to RCRA cleanup.

lation, which are likely to face other disadvantageous circumstances in life and are typically more vulnerable to the deleterious effects of pollution (see, e.g. Apelberg, Buckley and White, 2005).

Furthermore, we find that the benefits of cleanups accrued to those living closest to the sites and, notably, do not find that cleanups induced re-sorting. This is consistent with the localized price impacts that we find, but somewhat surprising given how expansive RCRA cleanups were and the recent literature that has highlighted the potential for policies to worsen underlying inequities (Hausman and Stolper, 2020; Bakkensen and Ma, 2020). Our results point to a hopeful conclusion- cleanup can help those living nearest to nuisances.

A fruitful direction for future work would be to further explore the attributes of both environmental policies and housing market conditions that explain such neighborhood dynamics. Further research could also examine the relationships between policy scope, information provision, and compositional changes in housing markets to determine the distribution of benefits across various groups from different types of policies.

8 Tables

Table 1: Attributes by whether there is a RCRA site within 10 km

	$\geq$ 1 Site within 10 km		No Sites within 10 km			
Attribute	Mean	St. Dev.	Mean	St. Dev.	Δ Mean	T-Statistic
Price 10^{th} Percentile	78.68	73.09	67.57	74.38	11.11	18.75
Price 50^{th} Percentile	128.12	111.89	130.42	125.15	-2.31	-2.46
Price 90^{th} Percentile	202.05	161.99	239.28	196.71	-37.23	-27.42
Vacant	4.97	4.40	5.00	3.80	-0.03	-1.18
Owner Occupied	10.06	17.61	9.08	16.82	0.98	6.76
Mobile	14.62	24.95	8.33	16.70	6.29	36.57
Moved in Last 5 years	24.94	6.94	25.14	6.19	-0.20	-3.81
Moved in 5-10 years ago	14.13	9.52	12.87	8.50	1.26	16.82
Moved in 10+ years ago	30.76	13.22	31.59	13.20	-0.83	-9.73
Built in Last 5 years	13.50	12.78	14.25	10.41	-0.75	-8.01
Built 6-10 years ago	7.27	8.70	5.04	6.09	2.23	44.03
Built 10-20 years ago	7.89	7.14	13.38	12.21	-5.48	-60.90
Built 20-30 years ago	62.45	24.19	71.75	17.33	-9.30	-53.70
Built 30-40 years ago	4.02	9.00	11.04	15.65	-7.02	-68.42
Built 40+ years ago	32.32	15.99	29.65	14.30	2.68	27.20
0 Bedrooms	15.78	8.35	19.12	7.79	-3.34	-70.61
1 Bedroom	51.82	18.63	51.18	15.62	0.63	5.17
2 Bedrooms	9.06	12.23	9.41	11.12	-0.35	-4.47
3 Bedrooms	7.57	8.61	9.33	8.06	-1.76	-30.87
4 Bedrooms	16.93	14.16	19.16	12.11	-2.23	-26.19
5+ Bedrooms	16.02	11.99	16.38	9.88	-0.36	-5.53
Unemployment	15.35	12.50	13.70	9.17	1.66	25.22
Hispanic	35.07	28.17	32.03	22.88	3.04	14.78
Black	2.57	5.41	1.64	3.18	0.92	28.17
Under Age 18	14.10	12.53	9.87	8.65	4.24	49.11
College Graduate	28.74	12.25	29.01	11.20	-0.27	-2.81
Female Head of Household	37.84	15.38	42.01	12.37	-4.16	-36.79
Below Poverty Line	13.52	10.60	14.04	9.32	-0.51	-6.29
On Public Assistance	3.22	4.11	3.44	3.98	-0.22	-6.59
Mean Household Income ($)	37,927.98	19,621.39	39,909.34	20,173.15	-1,981.36	-12.72

Notes This table compares houses $<$ 10 km from a RCRA site (the sample restriction we use) to houses that are not within 10 km of any RCRA sites. The number of census tracts and RCRA sites included varies by census outcome because of missing outcome data.

Table 2: Attributes by 1 Site vs Multiple Sites, within 10 km

	1 RCRA Site		Multiple RCRA Sites			
Attribute	Mean	St. Dev.	Mean	St. Dev.	Δ Mean	T-Statistic
Price 10^{th} Percentile	74.82	71.53	80.56	73.77	-5.74	-7.00
Price 50^{th} Percentile	126.46	112.84	128.92	111.42	-2.47	-1.93
Price 90^{th} Percentile	209.28	167.69	198.51	159.00	10.77	5.93
Vacant	4.62	3.66	5.12	4.68	-0.50	-12.71
Owner Occupied	8.35	15.29	10.89	18.56	-2.54	-13.45
Mobile	11.88	21.56	15.94	26.32	-4.06	-14.92
Moved in Last 5 years	25.49	6.25	24.68	7.24	0.81	11.10
Moved in 5-10 years ago	14.05	9.10	14.16	9.72	-0.12	-1.11
Moved in 10+ years ago	28.84	12.60	31.68	13.40	-2.84	-20.98
Built in Last 5 years	12.40	10.80	14.03	13.60	-1.63	-12.50
Built 6-10 years ago	5.47	5.70	8.13	9.70	-2.65	-36.55
Built 10-20 years ago	9.18	8.52	7.28	6.28	1.90	21.34
Built 20-30 years ago	68.37	20.66	59.60	25.23	8.78	33.86
Built 30-40 years ago	6.49	11.65	2.83	7.09	3.66	32.39
Built 40+ years ago	33.59	15.72	31.71	16.08	1.88	11.98
0 Bedrooms	17.27	6.84	15.06	8.90	2.20	33.41
1 Bedroom	49.11	16.71	53.12	19.35	-4.01	-22.32
2 Bedrooms	10.99	12.91	8.12	11.78	2.87	23.11
3 Bedrooms	9.44	9.15	6.67	8.19	2.78	32.31
4 Bedrooms	19.48	13.70	15.70	14.21	3.78	28.63
5+ Bedrooms	16.40	11.28	15.84	12.30	0.56	5.40
Unemployment	13.44	10.55	16.27	13.23	-2.83	-27.31
Hispanic	30.24	25.70	37.40	29.00	-7.16	-23.54
Black	1.73	3.43	2.97	6.10	-1.24	-26.14
Under Age 18	11.27	10.46	15.47	13.20	-4.20	-32.27
College Graduate	28.20	11.92	29.00	12.39	-0.80	-5.96
Female Head of Household	41.05	13.99	36.30	15.77	4.75	28.65
Below Poverty Line	14.49	10.41	13.05	10.66	1.44	12.07
On Public Assistance	3.26	3.91	3.21	4.20	0.05	1.13
Mean Household Income ($)	39,969.41	20,079.73	36,945.56	19,320.46	3,023.85	13.71
TSDF Y/N	0.81	0.39	0.80	0.40	0.01	2.38
Number of waste types	60.62	120.00	70.44	132.24	-9.82	-6.32
High NCAPS score	0.33	0.47	0.32	0.47	0.01	2.13
Medium NCAPS Score	0.31	0.46	0.25	0.43	0.06	11.66
Low NCAPS Score	0.22	0.41	0.29	0.45	-0.07	-13.68

Notes This table compares houses $<$ 10 km from a single RCRA site (the sample restriction we use) to houses that are $<$ 10 km from multiple RCRA sites.

Table 3: Price Impacts of Cleanup by Decile

Dep. var: Price^{kth}	10^{th}	20^{th}	30^{th}	40^{th}	50^{th}	60^{th}	70^{th}	80^{th}	90^{th}
0 km × Post	9.4918***	8.0418***	10.1089***	11.0443***	10.0313***	9.1301**	9.8181**	7.1750	5.9377
	(2.8930)	(3.0691)	(3.1942)	(3.4820)	(3.7641)	(4.1912)	(4.5091)	(5.4313)	(7.1086)
0–1 km × Post	2.7062	3.6720	5.1836	5.5141	3.8300	5.3180	1.0503	-0.0501	-6.0578
	(3.3070)	(3.5913)	(3.9806)	(4.1184)	(4.3263)	(4.3893)	(4.8160)	(5.4001)	(6.7792)
1–2 km × Post	4.6696	6.2325*	7.0380**	7.3014*	7.4820*	5.9950	3.7900	1.3857	0.0967
	(3.1835)	(3.4341)	(3.4685)	(3.7814)	(4.2286)	(4.5916)	(5.0197)	(5.3974)	(6.5801)
2–3 km × Post	2.5046	2.8561	-0.6422	-2.2952	-0.6419	0.9686	1.0725	4.4387	5.7709
	(3.2995)	(3.4377)	(3.7641)	(3.9309)	(4.1815)	(4.6058)	(5.1192)	(5.3534)	(6.6665)
3–4 km × Post	0.4799	-0.6491	-4.4054	-4.2010	-5.6106	-8.3130	-9.6016*	-10.6847*	-12.9543*
	(3.6266)	(3.9310)	(4.5067)	(4.6346)	(4.7414)	(5.1718)	(5.6723)	(6.0561)	(7.0404)
4–5 km × Post	2.8453	3.4443	2.6748	2.1373	-0.5198	-2.1971	-6.1666	-8.7399	-7.3248
	(3.9165)	(4.1219)	(4.3421)	(4.5600)	(4.9560)	(5.3630)	(5.8743)	(6.1486)	(7.7445)
Post	-9.1096***	-8.7993***	-8.6341***	-10.0310***	-9.7195***	-10.4657***	-8.7655***	-10.1641***	-8.7011***
	(1.4556)	(1.6148)	(1.7671)	(1.8510)	(1.9446)	(2.0597)	(2.2178)	(2.4420)	(2.8436)
Avg Price	84.237	105.596	121.620	136.055	150.867	166.843	186.184	212.498	257.237
R-squared	0.861	0.886	0.894	0.901	0.905	0.906	0.906	0.906	0.897
Clusters	10,740	10,740	10,740	10,740	10,740	10,738	10,737	10,734	10,727
Observations	29,880	29,880	29,880	29,880	29,880	29,875	29,873	29,865	29,837

Notes We use all tracts within 10 km of at most one RCRA facility in this regression. All regressions include fixed effects for tract, bin by year, and state by year. The excluded category is 5–10 km. All standard errors are clustered on census tract.

Table 4: Price Impacts of Cleanup by Decile, Near-Far Comparison

Dep. var: Price^{kth}	10^{th}	20^{th}	30^{th}	40^{th}	50^{th}	60^{th}	70^{th}	80^{th}	90^{th}
0 km × Post	8.5238***	6.9287**	9.4981***	10.5700***	9.9013***	9.2569**	10.8819**	8.5958*	7.8284
	(2.7327)	(2.8803)	(2.9705)	(3.2615)	(3.5386)	(3.9733)	(4.2751)	(5.1897)	(6.8588)
Post	-8.2603***	-7.8386***	-8.1778***	-9.7091***	-9.7397***	-10.7273***	-9.9431***	-11.6997***	-10.7103***
	(1.0877)	(1.1958)	(1.3098)	(1.3757)	(1.4495)	(1.5469)	(1.6815)	(1.8344)	(2.1841)
Avg Price	84.237	105.596	121.620	136.055	150.867	166.843	186.184	212.498	257.237
R-squared	0.859	0.884	0.893	0.900	0.904	0.905	0.905	0.905	0.896
Clusters	10,740	10,740	10,740	10,740	10,740	10,738	10,737	10,734	10,727
Observations	29,880	29,880	29,880	29,880	29,880	29,875	29,873	29,865	29,837

Notes We use all tracts within 10 km of at most one RCRA facility in this regression. All regressions include fixed effects for tract, bin by year, and state by year. The excluded category is homes $\in (0, 10]$ km away from a facility. All standard errors are clustered on census tract.

Table 5: Price Impacts of Cleanup, Robustness to Exclusion of State-Year FE and Alternative Clustering Levels

Dep. var: Price^{kth}	FE Comparison:		Alternative Clustering Levels:		
Percentile:	Bin×Yr FE Only:	Both FE:	County:	Site×Bin:	Site:
10^{th}	9.1066***	8.5238***	8.5238**	8.5238**	8.5238**
	(2.8968)	(2.7327)	(3.4277)	(3.3885)	(3.6565)
20^{th}	7.6071**	6.9287**	6.9287*	6.9287*	6.9287*
	(3.2101)	(2.8803)	(3.9117)	(3.7853)	(4.0989)
30^{th}	10.0605***	9.4981***	9.4981**	9.4981**	9.4981**
	(3.4061)	(2.9705)	(4.1488)	(3.9423)	(4.2747)
40^{th}	10.9537***	10.5700***	10.5700**	10.5700**	10.5700**
	(3.9048)	(3.2615)	(4.4707)	(4.2896)	(4.6342)
50^{th}	10.5370**	9.9013***	9.9013**	9.9013**	9.9013**
	(4.3158)	(3.5386)	(4.8175)	(4.6412)	(4.8811)
60^{th}	9.8805**	9.2569**	9.2569*	9.2569*	9.2569*
	(4.8537)	(3.9733)	(5.0709)	(5.0226)	(5.1517)
70^{th}	11.4557**	10.8819**	10.8819*	10.8819**	10.8819*
	(5.1972)	(4.2751)	(5.7284)	(5.4192)	(5.7221)
80^{th}	9.1638	8.5958*	8.5958	8.5958	8.5958
	(6.3762)	(5.1897)	(6.7132)	(6.3631)	(6.5406)
90^{th}	7.0919	7.8284	7.8284	7.8284	7.8284
	(8.1296)	(6.8588)	(8.0898)	(7.7761)	(7.9513)

Notes We use all tracts within 10 km of at most one RCRA facility in these regressions. All regressions include fixed effects for tract and bin by year, and the second two columns add state by year fixed effects. The excluded category is the tracts whose boundary lies $\in (0, 10]$ km away from a facility. Each cell is the treatment effect on the 0 km bin from a separate regression using the price percentile at the left-hand column of the table. Standard errors in the first two columns are clustered on census tract, and standard errors in the third through fifth are clustered on county, site by bin, and site.

Table 6: 10th Percentile Price Impacts, Allowing for Heterogeneous Effects by Treatment Timing

Dep. var: Price^{10th}	(1) TWFE	(2) 2SDD	(3) RA	(4) AIPW	(5) AIPW
0 km × Post	10.5168***	13.2291***	11.0503***	10.7984***	10.7135***
	(3.4190)	(4.6832)	(3.9263)	(3.9043)	(3.9091)
Outcome Model:					
Event Time FE	Y	Y	Y	Y	Y
Treatment Propensity Model:					
Site Characteristics				Y	Y
Tract Characteristics					Y
p-val, Null: Pre-Period Coefs=0	0.3585	0.4384	0.2946	0.2306	0.2280
Clusters	6,500	6,350	6,838	6,838	6,838
Observations	18,141	17,782	18,446	18,446	18,446

Notes We use all tracts within 10 km of at most one RCRA facility in this regression. All regressions include fixed effects for tract, bin by year, and state by year. All standard errors are clustered on census tract. TWFE refers to the two-way fixed effects model, modified to include event time dummies. 2SDD is the Gardner (2022) two-stage difference in differences estimator. RA is the regression adjustment adaptation of the estimator in Callaway and Sant'Anna (2021). AIPW refers to the augmented inverse propensity weighting estimator presented in Callaway and Sant'Anna (2021). Prices are denominated in thousands of dollars.

Table 7: Price Impacts by NCAPS Status

Dep. var: Price^{kth}					
		NCAPS Status			
Percentile:	All	High	Medium	Low	Missing/Unranked
10^{th}	8.5238***	3.6613	7.2929	-1.8579	1.2025
	(2.7327)	(3.3009)	(4.6727)	(5.0658)	(17.4600)
20^{th}	6.9287**	3.1332	10.4181**	-9.7361*	-3.4617
	(2.8803)	(3.6960)	(5.3030)	(5.8261)	(17.1730)
30^{th}	9.4981***	3.7821	12.1062**	-7.5779	7.6752
	(2.9705)	(3.9134)	(5.6706)	(6.3950)	(16.2783)
40^{th}	10.5700***	3.2492	14.6898**	-7.1922	5.8101
	(3.2615)	(3.9526)	(6.5155)	(7.3178)	(17.9105)
50^{th}	9.9013***	-0.0282	14.9787**	-7.5432	8.5581
	(3.5386)	(3.9751)	(7.2562)	(7.8084)	(18.3511)
60^{th}	9.2569**	-1.8480	19.3074**	-12.0598	-2.0319
	(3.9733)	(4.3180)	(8.2850)	(9.2057)	(18.9720)
70^{th}	10.8819**	-3.0862	21.2054**	-13.3433	5.9095
	(4.2751)	(5.0048)	(9.1602)	(9.9158)	(19.4921)
80^{th}	8.5958*	-3.8843	17.6420*	-10.1323	-3.3198
	(5.1897)	(6.2014)	(10.1990)	(12.6544)	(22.5935)
90^{th}	7.8284	-9.1183	4.7238	8.3674	-3.1203
	(6.8588)	(9.4805)	(14.6158)	(17.0037)	(26.7092)

Notes We use all tracts within 10 km of at most one RCRA facility in this regression. All regressions include fixed effects for tract, bin by year, and state by year. The excluded category is the tracts whose boundary lies $\in (0, 10]$ km away from a facility. Each cell is the treatment effect on the 0 km bin from a separate regression using the price percentile at the left-hand column of the table. All standard errors are clustered on census tract.

Table 8: Impacts on Income and Education-related Variables, Near-Far Comparison

Dep. var:	Avg HH Income	% Below Poverty	% College Educated	% on Public Assistance	% Unemployment
0 km × Post	178.4495	-0.0731	-0.0397	-0.0865	0.3564
	(583.3852)	(0.4913)	(0.3039)	(0.3049)	(0.8439)
Post	-174.9712	-0.0753	-0.0463	0.3681***	0.3346***
	(213.0314)	(0.1233)	(0.0873)	(0.0956)	(0.1251)
Avg Outcome	44209.503	12.731	14.724	4.639	4.824
R-squared	0.915	0.884	0.915	0.734	0.695
Clusters	10,780	10,778	10,783	10,780	9,186
Observations	30,025	30,017	30,039	30,024	18,375

Notes We use all tracts within 10 km of at most one RCRA facility in this regression. All regressions include fixed effects for tract, bin by year, and state by year. The excluded category is homes $\in (0, 10]$ km away from a facility. All standard errors are clustered on census tract.

Table 9: Impacts on Demographic Variables, Near-Far Comparison

Dep. var:	% Black	% Female Head of Household	% Hispanic	Population Density	% White	% Under 18
0 km × Post	-0.1733	0.1537	0.4933	142.9196	-0.9163	-0.2204
	(0.3903)	(0.5047)	(0.4175)	(140.5545)	(0.6071)	(0.2803)
Post	-0.0375	0.1405	-0.3150**	-16.4337	0.6309***	-0.2147***
	(0.1242)	(0.1434)	(0.1227)	(36.5323)	(0.1739)	(0.0754)
Avg Outcome	11.584	32.790	9.500	4481.756	74.676	25.029
R-squared	0.965	0.898	0.952	0.791	0.959	0.870
Clusters	10,783	10,780	10,783	10,783	10,783	10,783
Observations	30,042	30,024	30,042	30,042	30,042	30,042

Notes We use all tracts within 10 km of at most one RCRA facility in this regression. All regressions include fixed effects for tract, bin by year, and state by year. The excluded category is homes $\in (0, 10]$ km away from a facility. All standard errors are clustered on census tract.

Table 10: Impacts on Housing-Related Variable, Near-Far Comparison

Dep. var:	% 4+ Bedrooms	% Built in Last 5 Years	% Mobile Home	% Moved in Last 5 Years	% Owner Occupied	% Vacant
0 km × Post	-0.1301	0.1058	0.3937	-0.8510	0.3398	-0.2343
	(0.4075)	(0.6280)	(0.5223)	(0.9967)	(0.5167)	(0.4114)
Post	-0.2555**	-0.7785***	-0.1297	-0.6267***	-0.2268	0.0861
	(0.1249)	(0.1982)	(0.2153)	(0.2258)	(0.1470)	(0.1086)
Avg Outcome	18.573	9.134	7.138	30.278	68.716	9.688
R-squared	0.912	0.682	0.790	0.741	0.951	0.873
Clusters	10,779	10,779	10,780	10,762	10,780	10,781
Observations	30,019	30,019	30,027	29,960	30,027	30,030

Notes We use all tracts within 10 km of at most one RCRA facility in this regression. All regressions include fixed effects for tract, bin by year, and state by year. The excluded category is homes $\in (0, 10]$ km away from a facility. All standard errors are clustered on census tract.

Table 11: Heterogeneous Sorting Estimates (In Dollars)

Year:	2000	2010	All
0 km × Black	-153.2*	77.62***	73.89***
	(84.72)	(26.98)	(26.61)
0 km × Hispanic	566.1	-36.22	-28.61
	(443.4)	(36.67)	(36.79)
Observations	18,684	68,279	86,963
R-squared	0.334	0.343	0.218

Notes This table presents the differences in WTP in dollars between different racial groups from a regression of stacked mean utilities estimates on distance-by-race interactions, where mean utility estimates result from estimation of the structural model of location choice separately by race. The omitted group is the mean utility for white residents. Mean utility estimates are either based on movements from 1990 to 2000 (column 1), 2000 to 2010 (column 2), or both time periods (column 3). All specifications include tract fixed effects and column 3 additionally includes a year-pair fixed effect. All standard errors are clustered on census tract.

References

Abadie, Alberto, Susan Athey, Guido Imbens, and Jeffrey Wooldridge, "When Should You Adjust Standard Errors for Clustering?," *Quarterly Journal of Economics*, 2022.

Albouy, David, "What are cities worth? Land rents, local productivity, and the total value of amenities," *Review of Economics and Statistics*, 2016, *98* (3), 477–487.

Apelberg, Benjamin J., Timothy J. Buckley, and Ronald H. White, "Socioeconomic and Racial Disparities in Cancer Risk from Air Toxics in Maryland," *Environmental Health Perspectives*, 2005, *113* (6), 693–699.

Asker, Erdal, Eric Brunner, and Stephen Ross, "The Impact of School Spending on Civic Engagement: Evidence from School Finance Reforms," Technical Report, National Bureau of Economic Research 2022.

Bakkensen, Laura A and Lint Barrage, "Going Underwater? Flood Risk Belief Heterogeneity and Coastal Home Price Dynamics," *The Review of Financial Studies*, 11 2021, *35* (8), 3666–3709.

Bakkensen, Laura and Lala Ma, "Sorting Over Flood Risk and Implications for Policy Reform," *Journal of Environmental Economics and Management*, November 2020, *104* (102362).

Banzhaf, H Spencer, "Panel Data Hedonics: Rosen'S First Stage as a "Sufficient Statistic"," *International Economic Review*, 2020, *61* (2), 973–1000.

_ , "Difference-in-Differences Hedonics," *Journal of Political Economy*, 2021, *129* (8), 2385–2414.

_ **and Omar Farooque**, "Interjurisdictional housing prices and spatial amenities" Which measures of housing prices reflect local public goods?," *Regional Science and Urban Economics*, 2013, *43* (4), 635–648.

_ **and Randall P Walsh**, "Segregation and Tiebout sorting: The link between place-based investments and neighborhood tipping," *Journal of Urban Economics*, 2013, *74*, 83–98.

_ **, Lala Ma, and Christopher Timmins**, "Environmental justice: Establishing causal relationships," *Annual Review of Resource Economics*, 2019, *11*, 377–398.

Banzhaf, Spencer and Eleanor McCormick, "Moving Beyond Cleanup: Identifying the Crucibles of Environmental Gentrification," 2007.

Bayer, Patrick, Nathaniel Keohane, and Christopher Timmins, "Migration and hedonic valuation: The case of air quality," *Journal of Environmental Economics and Management*, 2009, *58* (1), 1–14.

Berry, Steven T, "Estimating Discrete-Choice Models of Product Differentiation," *RAND Journal of Economics*, 1994, *25* (2), 242–262.

Bertrand, Marianne, Esther Duflo, and Sendhil Mullainathan, "How Much Should We Trust Differences-In-Differences Estimates?*," *The Quarterly Journal of Economics*, 02 2004, *119* (1), 249–275.

Blomquist, Glenn C, Mark C Berger, and John P Hoehn, "New Estimates of Quality of Life in Urban Areas," *American Economic Review*, 1988, pp. 89–107.

Callaway, Brantly and Pedro H.C. Sant'Anna, "Difference-in-Differences with multiple time periods," *Journal of Econometrics*, 2021, *225* (2), 200–230. Themed Issue: Treatment Effect 1.

Cassidy, Alecia, "How Does Mandatory Energy Efficiency Disclosure Affect Housing Prices?," *Journal of the Association of Environment and Resource Economists*, 2023, *10* (3).

_ , Robyn C. Meeks, and Michael R. Moore, "Cleaning up the Great Lakes: Housing market impacts of removing legacy pollutants," *Journal of Public Economics*, 2023, *226*, 104979.

Chay, K.Y. and M. Greenstone, "Does Air Quality Matter? Evidence from the Housing Market," *Journal of Political Economy*, 2005, *113* (2), 376–424.

Currie, Janet, Lucas Davis, Michael Greenstone, and Reed Walker, "Environmental Health Risks and Housing Values: Evidence from 1,600 Toxic Plant Openings and Closings," *American Economic Review*, 2015, *105* (2), 678–709.

_ , Michael Greenstone, and Enrico Moretti, "Superfund cleanups and infant health," *American Economic Review*, 2011, *101* (3), 435–441.

Depro, Brooks, Christopher Timmins, and Maggie O'Neil, "White Flight and Coming to the Nuisance: Can Residential Mobility Explain Environmental Injustice?," *Journal of the Association of Environmental and Resource Economists*, 2015, *2* (3), 439–468.

de Chaisemartin, Clément and Xavier D'Haultfœuille, "Two-way fixed effects and differences-in-differences with heterogeneous treatment effects: a survey," *The Econometrics Journal*, 06 2022, *26* (3), C1–C30.

Dube, Arindrajit, "Minimum Wages and the Distribution of Family Incomes," *American Economic Journal: Applied Economics*, October 2019, *11* (4), 268–304.

Firpo, Sergio, "Efficient Semiparametric Estimation of Quantile Treatment Effects," *Econometrica*, 2007, *75* (1), 259–276.

_ , Nicole M. Fortin, and Thomas Lemieux, "Unconditional Quantile Regressions," *Econometrica*, 2009, *77* (3), 953–973.

Gamper-Rabindran, Shanti and Christopher Timmins, "Does cleanup of hazardous waste sites raise housing values? Evidence of spatially localized benefits," *Journal of Environmental Economics and Management*, 2013, *65* (3), 345–360.

_ , Ralph Mastromonaco, and Christopher Timmins, "Valuing the Benefits of Superfund Site Remediation: Three Approaches to Measuring Localized Externalities," *National Bureau of Economic Research No. w16655*, 2011.

Gardner, John, "Two-stage differences in differences," *arXiv preprint arXiv:2207.05943*, 2022.

Gayer, T., J.T. Hamilton, and W.K. Viscusi, "Private Values of Risk Tradeoffs at Superfund Sites: Housing Market Evidence on Learning about Risk," *Review of Economics and Statistics*, 2000, *82* (3), 439–451.

Greenstone, Michael and Justin Gallagher, "Does hazardous waste matter? Evidence from the housing market and the superfund program," *Quarterly Journal of Economics*, 2008, *123* (3), 951–1003.

Guignet, Dennis B. and Christoph Nolte, "Hazardous Waste and Home Values: An Analysis of Treatment and Disposal Sites in the U.S.," *Journal of the Association of Environmental and Resource Economists*, Forthcoming, pp. 1–54.

Haninger, Kevin, Lala Ma, and Christopher Timmins, "The Value of Brownfield Remediation," *Journal of the Association of Environmental and Resource Economists*, 2017, *4* (1), 197–241.

Hausman, Catherine and Samuel Stolper, "Inequality, Information Failures, and Air Pollution," *National Bureau of Economic Research No. w26682*, 2020, pp. 1–75.

Havnes, Tarjei and Magne Mogstad, "Is universal child care leveling the playing field?," *Journal of Public Economics*, 2015, *127*, 100–114. The Nordic Model.

Hill, Elaine and Lala Ma, "Fracking, Drinking Water, and Infant Health," *Working Paper*, 2019.

Kahn, Matthew E, "A Revealed Preference Approach to Ranking City Quality of Life," *Journal of Urban Economics*, 1995, *38* (2), 221–235.

Keiser, David A and Joseph S Shapiro, "Consequences of the Clean Water Act and the demand for water quality," *The Quarterly Journal of Economics*, 2018, *134* (1), 349–396.

Kinnaman, Thomas C, "A landfill closure and housing values," *Contemporary Economic Policy*, 2009, *27* (3), 380–389.

Kohlhase, Janet E, "The Impact of Toxic Waste Sites on Housing Values," *Journal of Urban Economics*, 1991, *30* (1), 1–26.

Koren, Miklos, "EVENTBASELINE: Stata module to correct event study after xthdidregress," 2024.

Kuminoff, Nicolai V and Jaren C Pope, "Do 'Capitalization Effects' For Public Goods Reveal The Public's Willingness To Pay?," *International Economic Review*, 2014, *55* (4), 1227–1250.

Linden, Leigh and Jonah E Rockoff, "Estimates of the Impact of Crime Risk on Property Values from Megan's Laws," *American Economic Review*, 2008, *98* (3), 1103–1127.

Linn, Joshua, "The Effect of Voluntary Brownfields Programs on Nearby Property Balues: Evidence from Illinois," *Journal of Urban Economics*, 2013, *78*, 1–18.

Logan, John R, Zengwang Xu, and Brian J Stults, "Interpolating US decennial census tract data from as early as 1970 to 2010: A longitudinal tract database," *The Professional Geographer*, 2014, *66* (3), 412–420.

Ma, Lala, "Learning in a Hedonic Framework: Valuing Brownfield Remediation," *International Economic Review*, 2019, *60* (3), 1355–1387.

Mastromonaco, Ralph, "Do Environmental Right-to-Know Laws Affect Markets? Capitalization of Information in the Toxic Release Inventory," *Journal of Environmental Economics and Management*, 2015, *71*, 54–70.

Messer, Kent D, William D Schulze, Katherine F Hackett, Trudy A Cameron, and Gary H McClelland, "Can Stigma Explain Large Property Value Losses? The Psychology and Economics of Superfund," *Environmental and Resource Economics*, 2006, *33* (3), 299–324.

Muehlenbachs, Lucija, Elisheba Spiller, and Christopher Timmins, "The Housing Market Impacts of Shale Gas Development," *American Economic Review*, 2016, *106* (2), 475–475.

Palmquist, Raymond B, "A note on transaction costs, moving costs, and benefit measurement," *Journal of Urban Economics*, 1992, *32* (1), 40–44.

_ , "Valuing Localized Amenities," *Journal of Urban Economics*, 1992, *31* (1), 59–68.

Roback, Jennifer, "Wages, Rents, and the Quality of Life," *Journal of Political Economy*, 1982, *90* (6), 1257–1278.

Rosen, S., "Hedonic Prices and Implicit Markets: Product Differentiation in Pure Competition," *Journal of Political Economy*, 1974, *82* (1), 34–55.

Roth, Jonathan, "Interpreting Event-Studies from Recent Difference-in-Differences Methods," *arXiv preprint arXiv:2401.12309*, 2024.

Smith, V Kerry and William H Desvousges, "The value of avoiding a LULU: hazardous waste disposal sites," *The Review of Economics and Statistics*, 1986, pp. 293–299.

US Environmental Protection Agency, "Environmental Fact Sheet: The National Corrective Action Prioritization System," *Office of Solid Waste and Emergency Response*, January 1993, *EPA 530-F-92-027* (OS-305), 1–2.

U.S. EPA, "Report on the Environment, Chapter 4 Exhibit 4-2," 2008.

_ , "OSWER Cross-Program Revitalization Measures," 2011.

_ , "Resource Conservation and Recovery Act (RCRA) Orientation Manual," 2014. Available online at: $https://www.epa.gov/sites/production/files/2015-07/documents/rom.pdf$.

Wooldridge, Jeffrey M, "Two-way fixed effects, the two-way mundlak regression, and difference-in-differences estimators," *Available at SSRN 3906345*, 2021.

A Appendix

A.1 Appendix Tables

Table A.1: Attributes by Near vs. Far from One RCRA Site, within 10 km

	0 km		$\in (0, 10]$ km			
Attribute	Mean	St. Dev.	Mean	St. Dev.	Δ Mean	T-Statistic
Price 10^{th} Percentile	78.24	73.61	50.64	48.09	27.60	18.06
Price 50^{th} Percentile	131.54	116.58	90.48	71.96	41.07	18.17
Price 90^{th} Percentile	216.19	172.65	160.17	115.69	56.02	16.83
Vacant	4.57	3.64	4.95	3.82	-0.37	-4.05
Owner Occupied	8.48	15.32	7.37	15.00	1.11	2.32
Mobile	12.28	22.15	9.00	16.45	3.28	6.19
Moved in Last 5 years	25.45	6.33	25.76	5.71	-0.31	-1.82
Moved in 5-10 years ago	14.50	9.29	10.84	6.83	3.66	17.48
Moved in 10+ years ago	28.88	12.71	28.56	11.86	0.33	1.04
Built in Last 5 years	12.08	10.81	14.68	10.44	-2.60	-8.45
Built 6-10 years ago	5.35	5.65	6.37	5.94	-1.03	-6.98
Built 10-20 years ago	9.05	8.57	10.12	8.02	-1.07	-4.63
Built 20-30 years ago	68.65	20.74	66.40	19.92	2.26	3.72
Built 30-40 years ago	6.15	11.81	8.91	10.18	-2.76	-9.10
Built 40+ years ago	33.72	15.75	32.62	15.52	1.10	2.82
0 Bedrooms	17.33	6.87	16.86	6.61	0.47	3.14
1 Bedroom	48.92	16.93	50.44	14.98	-1.52	-3.77
2 Bedrooms	11.17	13.27	9.72	9.92	1.46	5.63
3 Bedrooms	9.53	9.36	8.86	7.55	0.67	3.53
4 Bedrooms	19.62	14.01	18.54	11.24	1.08	3.91
5+ Bedrooms	16.50	11.58	15.68	8.87	0.82	3.88
Unemployment	13.50	10.82	13.02	8.41	0.48	2.31
Hispanic	29.68	26.05	34.18	22.66	-4.50	-6.65
Black	1.76	3.52	1.54	2.67	0.22	3.08
Under Age 18	11.34	10.70	10.77	8.52	0.57	2.23
College Graduate	27.88	12.15	30.50	9.88	-2.63	-9.16
Female Head of Household	40.90	14.28	42.10	11.65	-1.20	-3.45
Below Poverty Line	14.80	10.75	12.32	7.21	2.48	11.22
On Public Assistance	3.33	4.02	2.77	3.05	0.55	6.10
Mean Household Income ($)	40,957.37	20,591.30	32,994.46	14,172.11	7,962.91	19.24
TSDF Y/N	0.81	0.39	0.78	0.42	0.03	2.55
Number of waste types	60.63	118.27	60.53	131.16	0.10	0.02
High NCAPS score	0.33	0.47	0.36	0.48	-0.03	-2.06
Medium NCAPS Score	0.31	0.46	0.33	0.47	-0.02	-1.63
Low NCAPS Score	0.22	0.41	0.19	0.39	0.03	2.32

Notes This table compares houses 0 km from a single RCRA site to houses that are $\in (0, 10]$ km from a single RCRA site.

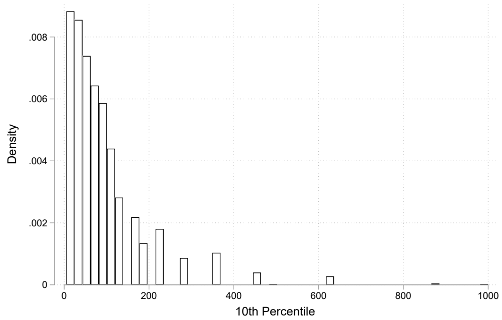
Table A.2: Price Impacts by Facility Type and Fraction Public Water

Dep. var: Price^{kth}		Facility Type		% Public Water		
Percentile:	All	TSDF	LQG	$\leq 25\%$	$\in (25, 75]\%$	$> 75\%$
10^{th}	8.5238***	12.3207***	9.8336***	7.5638	0.8563	7.6721*
	(2.7327)	(3.1076)	(2.9931)	(6.7903)	(3.3526)	(4.1351)
20^{th}	6.9287**	11.7884***	8.8640***	10.0520	-1.8943	5.0638
	(2.8803)	(3.2258)	(3.1425)	(8.9578)	(3.6178)	(4.2597)
30^{th}	9.4981***	13.6336***	11.0404***	13.3258	-0.2832	7.2794*
	(2.9705)	(3.3232)	(3.2455)	(10.6356)	(3.8708)	(4.2699)
40^{th}	10.5700***	15.0819***	12.7801***	16.8465	0.2889	7.9905*
	(3.2615)	(3.6442)	(3.5362)	(11.5052)	(4.3687)	(4.7200)
50^{th}	9.9013***	13.4716***	11.2307***	7.2309	-2.7493	8.3676*
	(3.5386)	(3.8765)	(3.8569)	(11.3457)	(5.5776)	(4.9168)
60^{th}	9.2569**	13.0722***	10.6358**	6.4052	-4.7048	7.8372
	(3.9733)	(4.3767)	(4.4251)	(13.0150)	(5.5110)	(5.6461)
70^{th}	10.8819**	15.1481***	11.8838**	2.2439	-5.2422	9.9295*
	(4.2751)	(4.6631)	(4.7561)	(15.8652)	(6.2181)	(5.8650)
80^{th}	8.5958*	11.7011**	9.5374	-3.8169	-7.0856	6.3709
	(5.1897)	(5.7679)	(5.7991)	(16.8982)	(7.8940)	(6.9885)
90^{th}	7.8284	14.4111**	8.0151	2.7766	-12.6007	6.3587
	(6.8588)	(7.0173)	(7.5623)	(23.2883)	(13.6068)	(7.8739)

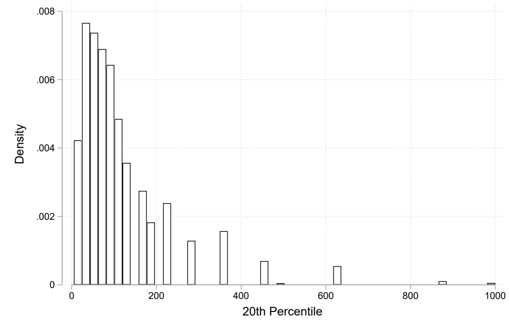
Notes We use all tracts within 10 km of at most one RCRA facility in this regression. All regressions include fixed effects for tract, bin by year, and state by year. The excluded category is the tracts whose boundary lies $\in (0, 10]$ km away from a facility. Each cell is the treatment effect on the 0 km bin from a separate regression using the price percentile at the left-hand column of the table. All standard errors are clustered on census tract.

A.2 Appendix Figures

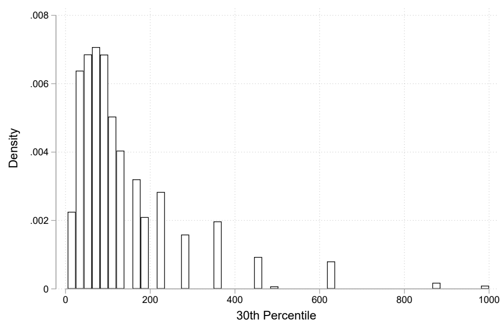
Figure A.1: Histograms: 10^{th}–50^{th} percentiles of housing price



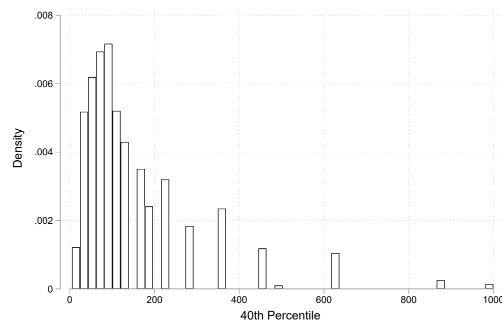
(a) 10^{th} Percentile



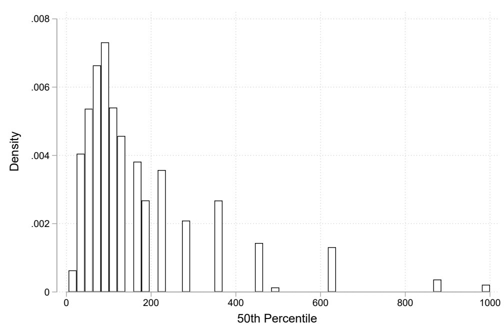
(b) 20^{th} Percentile



(c) 30^{th} Percentile



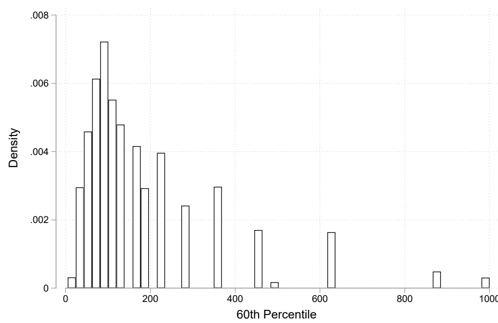
(d) 40^{th} Percentile



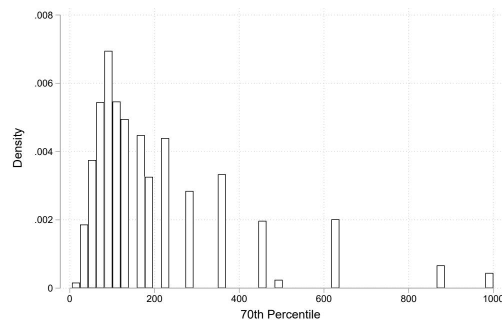
(e) 50^{th} Percentile

Notes Each graph above depicts a histogram for the corresponding percentile of prices.

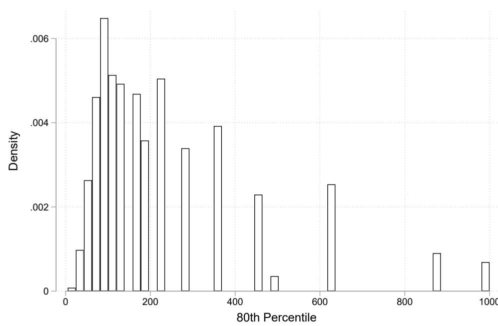
Figure A.2: Histograms: 60^{th}–90^{th} percentiles of housing price over time



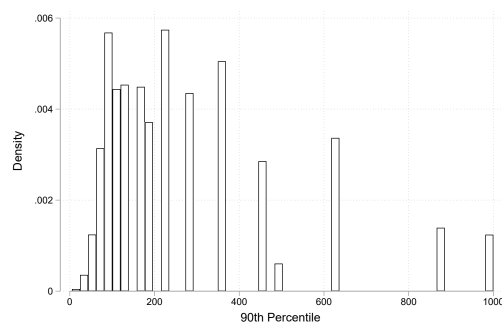
(a) 60^{th} Percentile



(b) 70^{th} Percentile



(c) 80^{th} Percentile



(d) 90^{th} Percentile

Notes Each graph above depicts a histogram for the corresponding percentile of prices.

Figure A.3: Cleanup Year by NCAPS status

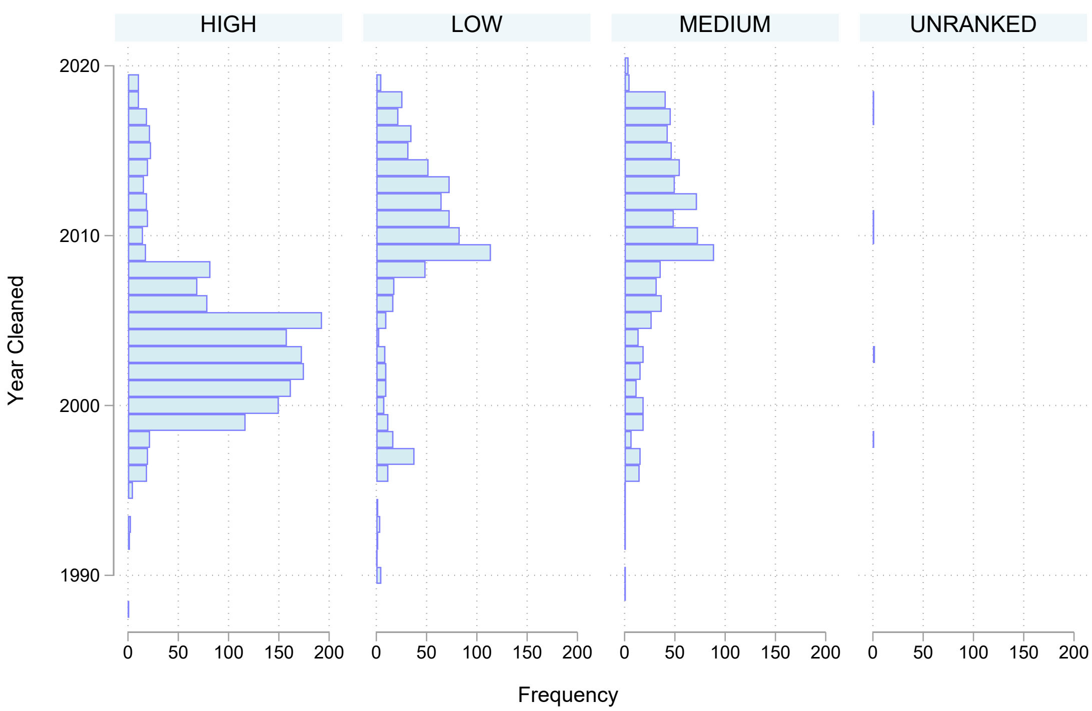
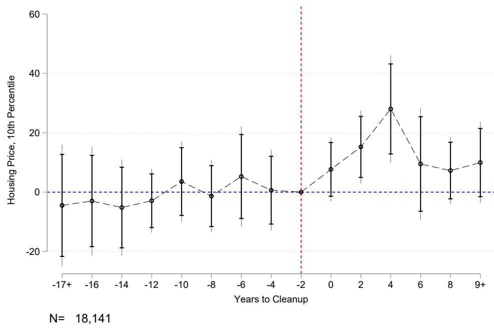
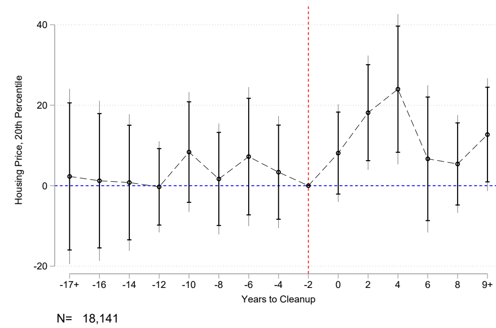


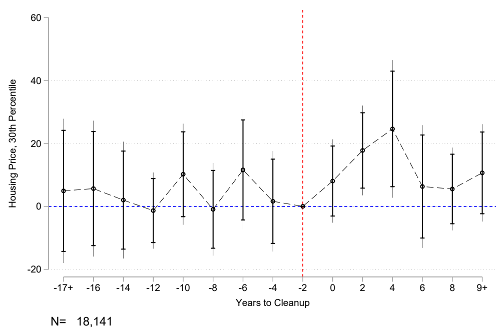
Figure A.4: 10^{th}–50^{th} percentiles of housing price over time



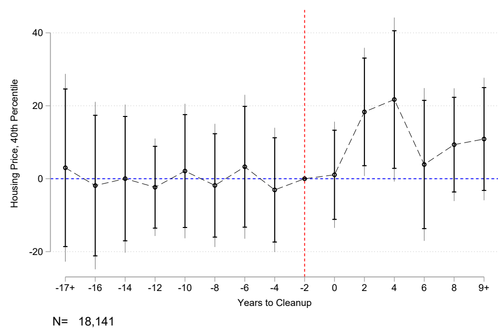
(a) 10^{th} Percentile



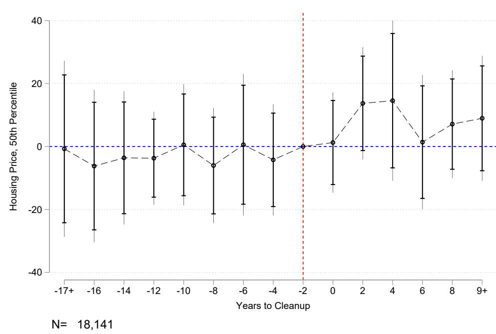
(b) 20^{th} Percentile



(c) 30^{th} Percentile



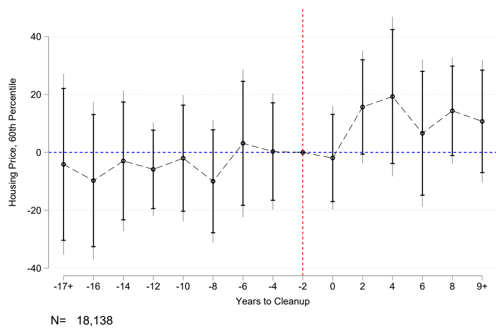
(d) 40^{th} Percentile



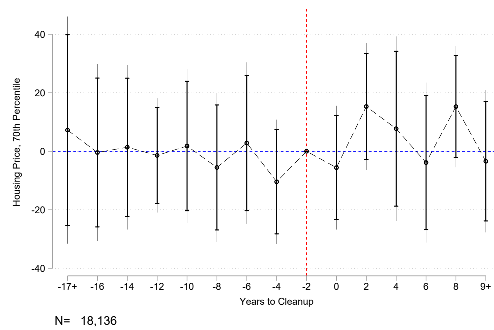
(e) 50^{th} Percentile

Notes This figure shows the coefficient representing the difference in the near and far bins over event time for each outcome indicated in the sub-caption. We use the same fixed effects as in the main regression. The coefficient for the two years just prior to the cleanup (at position -2) is normalized to 0 by excluding the dummy on Near$\times$ Event time=-2 from the regression. Data from the during-cleanup phase is dropped from the analysis, so time 0 represents the two-year period immediately following cleanup completion.

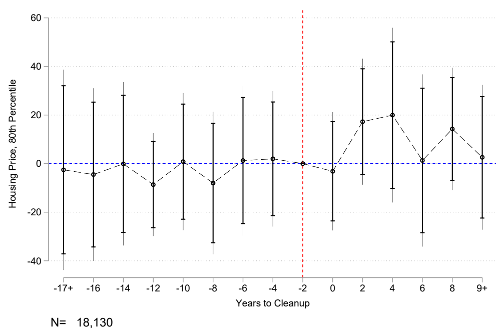
Figure A.5: 60^{th}–90^{th} percentiles of housing price over time



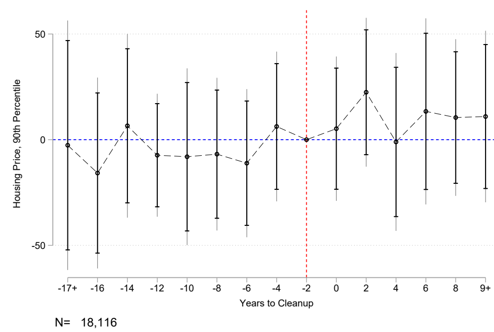
(a) 60^{th} Percentile



(b) 70^{th} Percentile



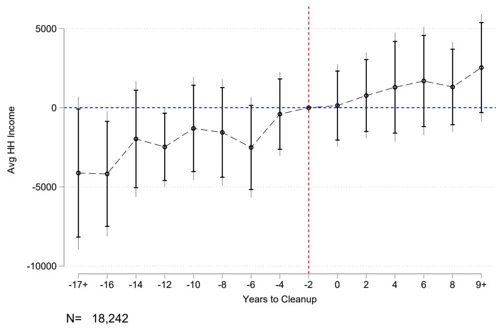
(c) 80^{th} Percentile



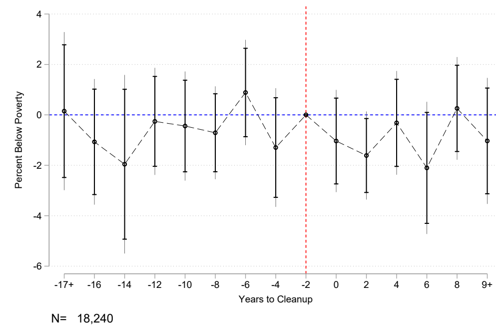
(d) 90^{th} Percentile

Notes This figure shows the coefficient representing the difference in the near and far bins over event time for each outcome indicated in the sub-caption. We use the same fixed effects as in the main regression. The coefficient for the two years just prior to the cleanup (at position -2) is normalized to 0 by excluding the dummy on Near× Event time=-2 from the regression. Data from the during-cleanup phase is dropped from the analysis, so time 0 represents the two-year period immediately following cleanup completion.

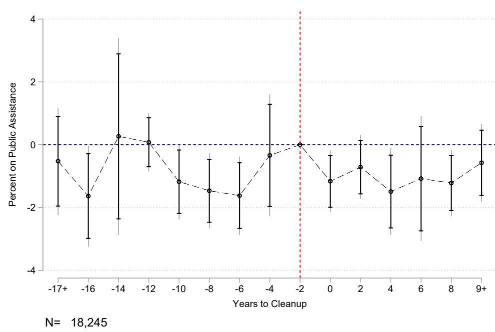
Figure A.6: Income and Education-related variables over time



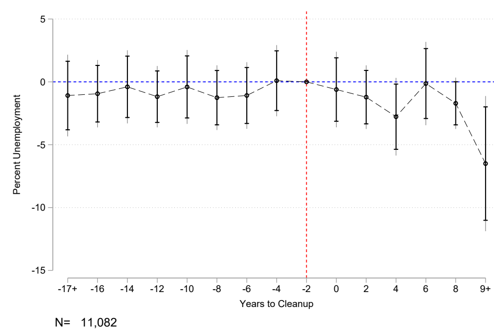
(a) Avg HH Income



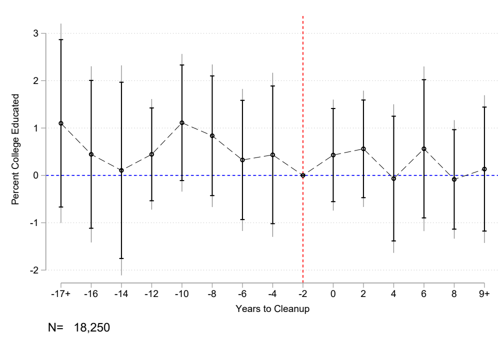
(b) % Below Poverty Line



(c) % Public Assistance



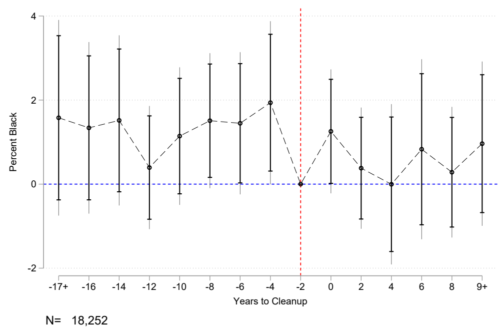
(d) % Unemployment



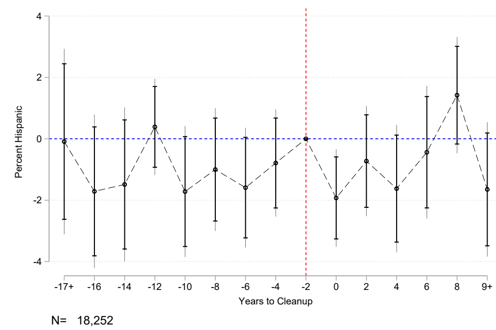
(e) % College Educated

Notes This figure shows the coefficient representing the difference in the near and far bins over event time for each outcome indicated in the sub-caption. We use the same fixed effects as in the main regression. The coefficient for the two years just prior to the cleanup (at position -2) is normalized to 0 by excluding the dummy on Near× Event time=-2 from the regression. Data from the during-cleanup phase is dropped from the analysis, so time 0 represents the two-year period immediately following cleanup completion.

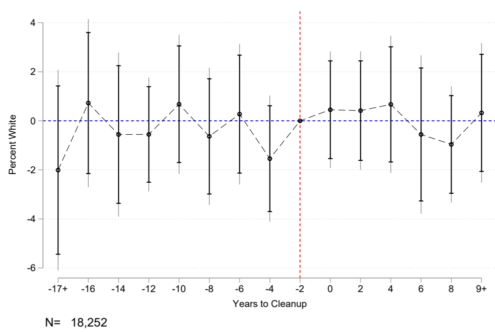
Figure A.7: Demographic variables over time



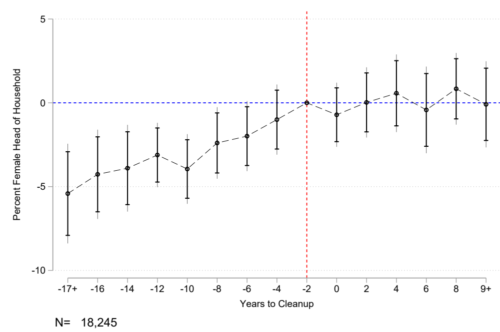
(a) % Black



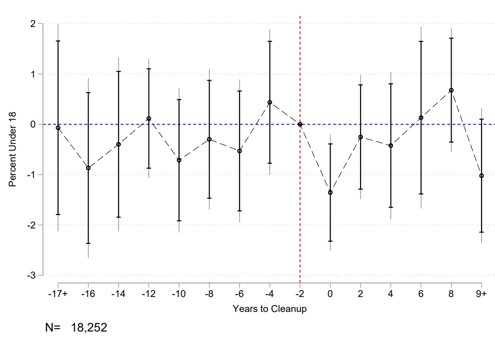
(b) % Hispanic



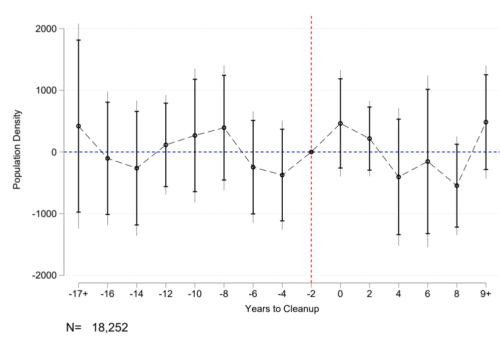
(c) % White



(d) % Female Head of Household



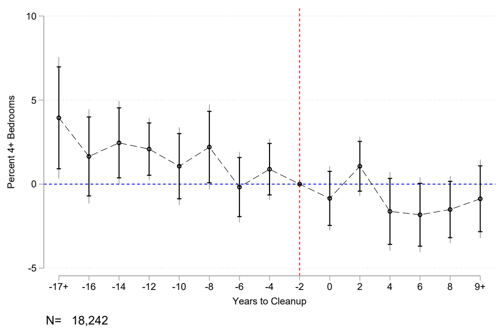
(e) % Under 18



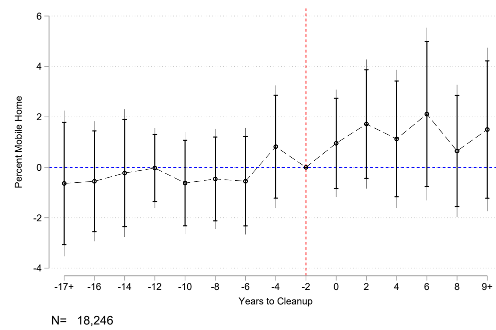
(f) Population Density

Notes This figure shows the coefficient representing the difference in the near and far bins over event time for each outcome indicated in the sub-caption. We use the same fixed effects as in the main regression. The coefficient for the two years just prior to the cleanup (at position -2) is normalized to 0 by excluding the dummy on Near× Event time=-2 from the regression. Data from the during-cleanup phase is dropped from the analysis, so time 0 represents the two-year period immediately following cleanup completion.

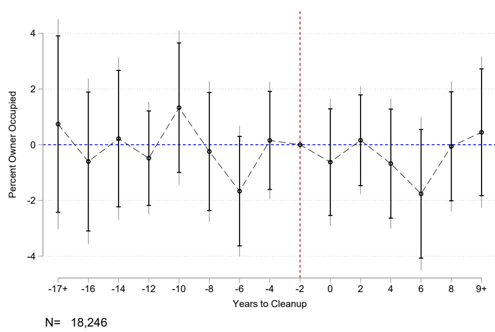
Figure A.8: Housing-related variables over time



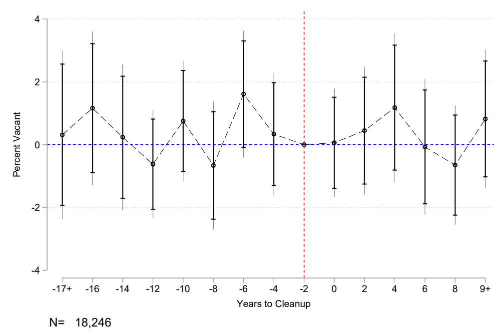
(a) % 4+ Beds



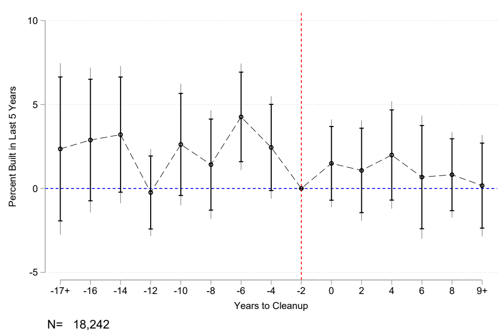
(b) % Mobile Home



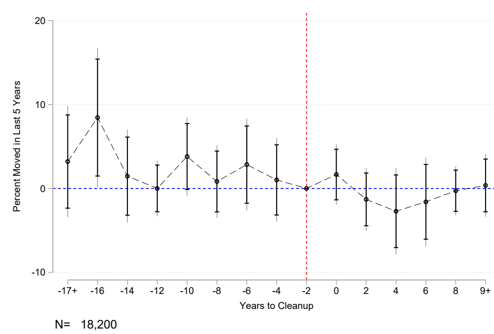
(c) % Owner Occupied



(d) % Vacant



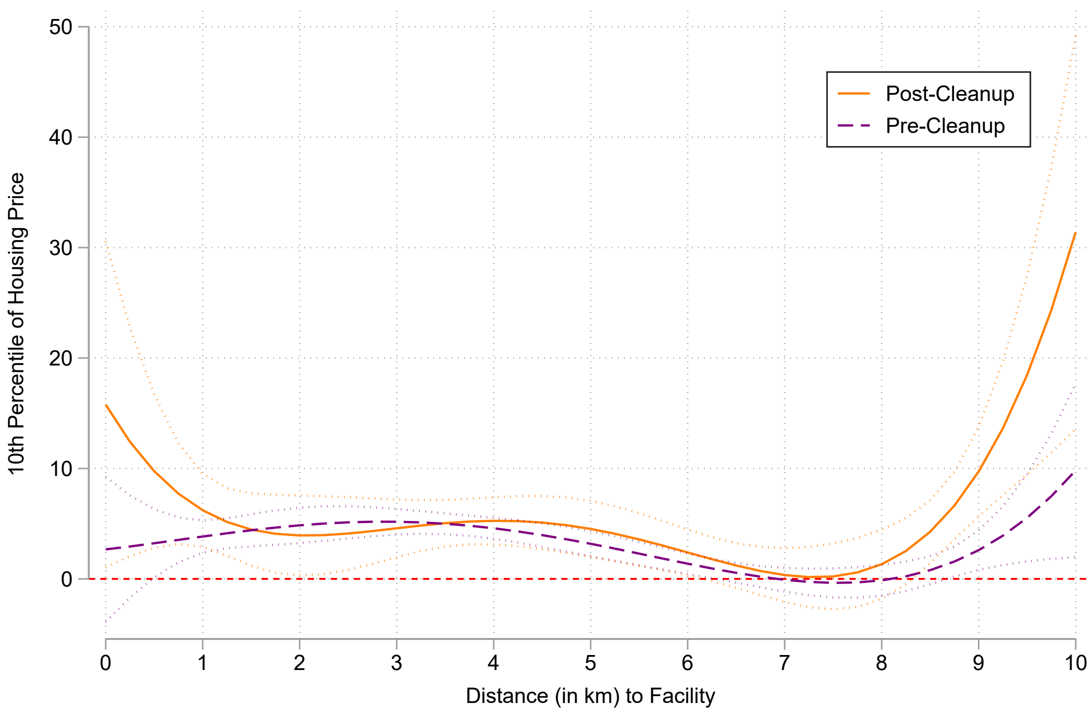
(e) % Built in Last 5 years



(f) % Moved in Last 5 Years

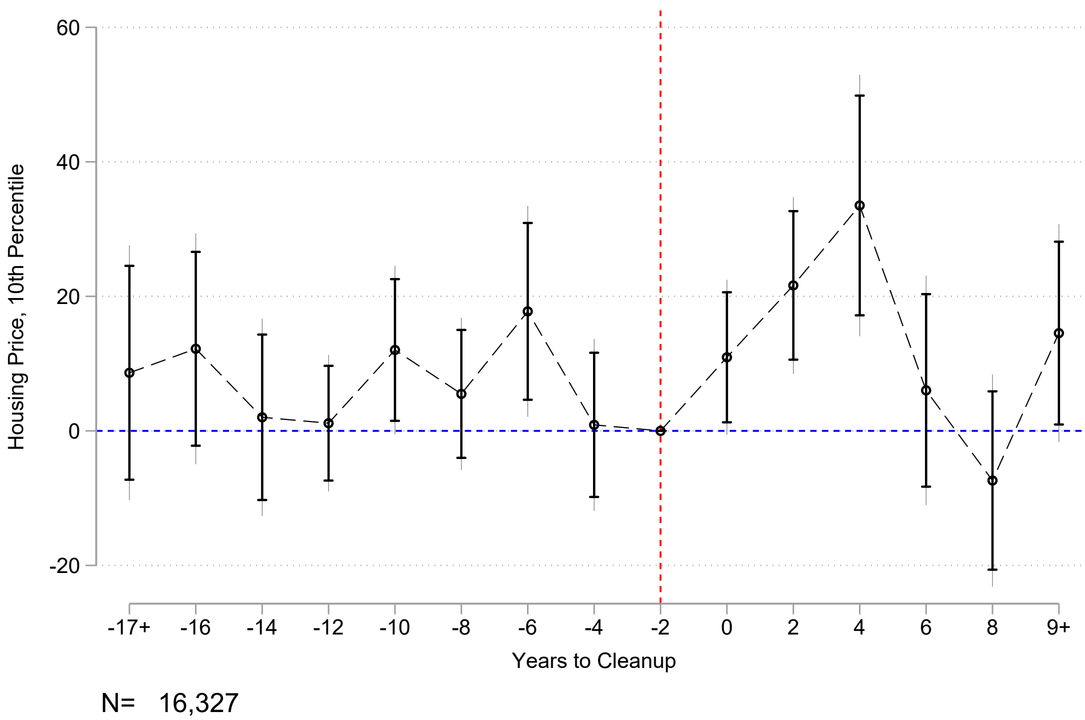
Notes This figure shows the coefficient representing the difference in the near and far bins over event time for each outcome indicated in the sub-caption. We use the same fixed effects as in the main regression. The coefficient for the two years just prior to the cleanup (at position -2) is normalized to 0 by excluding the dummy on Near× Event time=-2 from the regression. Data from the during-cleanup phase is dropped from the analysis, so time 0 represents the two-year period immediately following cleanup completion.

Figure A.9: Linden and Rockoff-style Plot



Notes This graph fits the 10th percentile of housing price to the distance to the RCRA facility, both pre- and post-cleanup. The technique was popularized by Linden and Rockoff (2008).

Figure A.10: Event Study Using Alternative Definition of Cleanup



Notes This graph depicts the event study when we define the date of cleanup as final remedy events when those are non-missing. As explained on p. 27, the composition of event types is changing over event time when we use this alternative definition of cleanup, muddling the interpretation of the graph in later years.

B Supplementary Appendix on Individual Transaction Data

We obtained Zillow Ztrax data from Ohio and Pennsylvania for comparison purposes. This data contains individual housing transactions spanning the years 1994-2019,[52] collected from publicly available sources such as county offices.

B.1 Understanding Census tract-based treatment and comparison groups

We first use the transaction data to understand our treatment effects in the context of actual distance from a house to a RCRA site. Because we employ Census tract-level data in this paper, we define distance to a RCRA site as the minimum distance from the Census tract to the site. This means that we may include in our treatment group homes relatively far from a site but still on the same tract, or we may include in our comparison group homes that are relatively close to a site. This is a form of measurement error that would cause attenuation of our estimates.

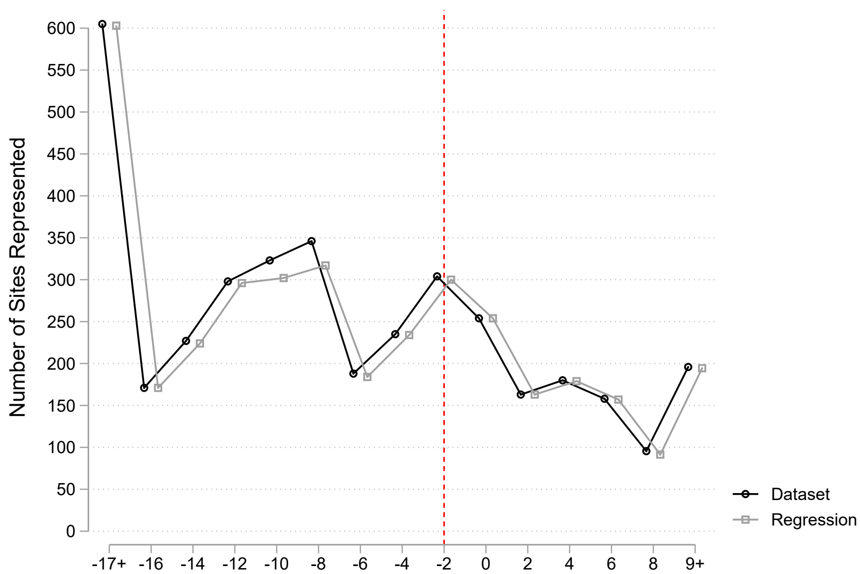
To investigate the extent of this issue, we provide information on actual distance to site in Figure B.1. In panel B.1a, we show summary statistics by bin, where the bins are defined as the minimum distance from tract to site. The actual distance is on average 4 times as large for the comparison group as for the treatment group. The median, 5th, and 95th percentiles are all increasing in census distance bin. In summary, actual distance to site is well-proxied by Census tract to site, at least relatively speaking.

Panel B.1a of the same figure shows the kernel densities of actual distance to site for both the treatment (same tract as RCRA site) and the comparison (Census tract 5-10 km away) groups. The distributions demonstrate a small amount of overlap- for the most part, houses in our census-tract based treatment group are near the sites, and those in our comparison group is far from a RCRA site.

Individual transaction data also allows us to examine the price distribution. In figure B.2, we show the kernel density plots of price for both the treatment and comparison groups, measured prior to cleanup. The distributions are similar.

[52] There are only a negligible number of observations from before 1994 (about 0.04%).

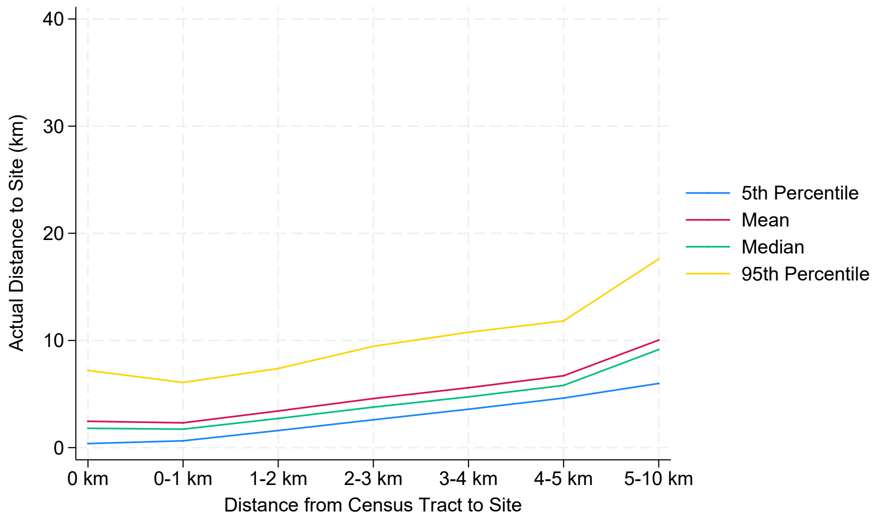
Figure A.11: Sites represented over event time



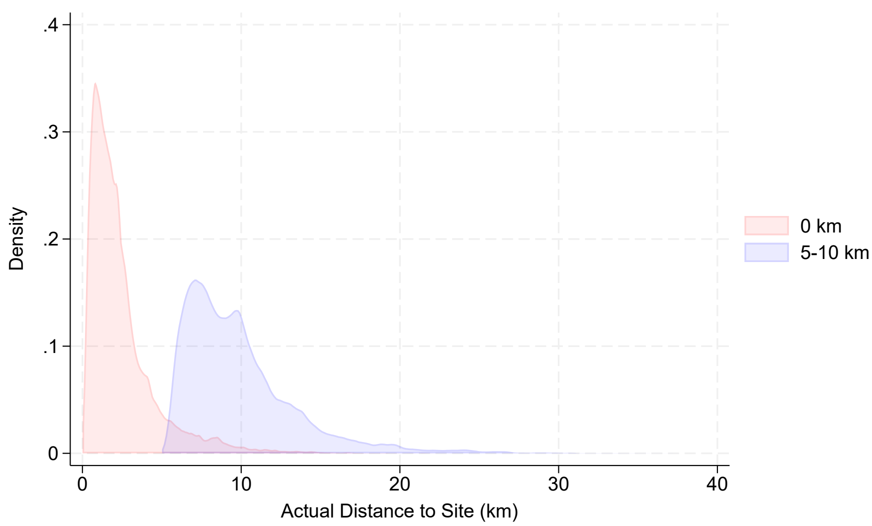
Notes This figure shows the number of RCRA sites represented in each event time, where event times are defined as the difference in years between the survey year and the cleanup period, in two-year intervals.

Figure B.1: Actual Distance to Site vs. Census Tract Distance

(a) Actual Distance: Summary Statistics by Census tract-based Distance Bin

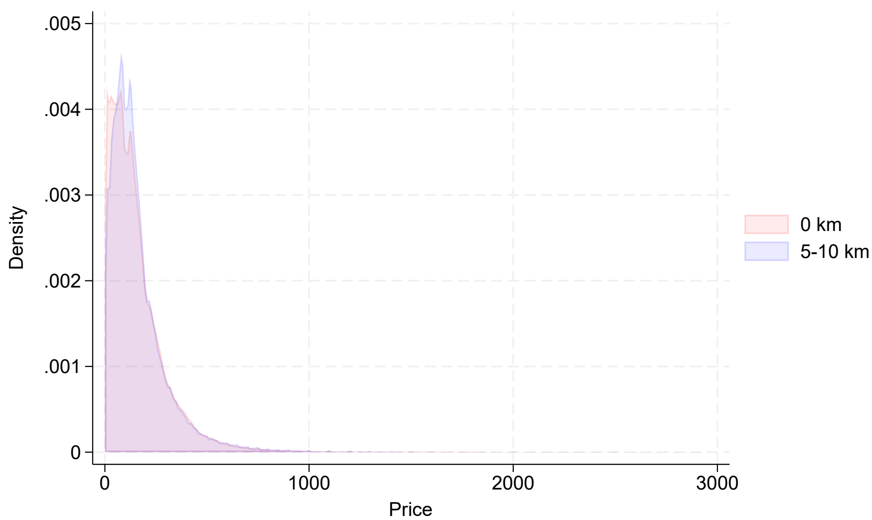


(b) Distance to Site Kernel Density, Census tract-based Treatment and Comparison Groups



Notes This figure depicts summary statistics and a kernel density of actual distances to site for houses in Ohio and Pennsylvania from the Zillow Ztrax database, for the treatment and comparison groups in our main analysis (which are defined by distance from Census tract to site).

Figure B.2: Price Kernel Density, Census tract-based Treatment and Comparison Groups



Notes This figure depicts a kernel density of transaction prices of houses in Ohio and Pennsylvania from the Zillow Ztrax database, separately for the treatment and comparison groups defined in our main analysis. Note that the figure only plots prices less than 3 million dollars (3000 in thousand dollar units) for readability. These discarded prices account for less than 1% of observations.

B.2 Quantile Treatment Effects using Individual Transaction Data

In this sub-section, we use Zillow data to show that our main results are robust to use of individual transaction-level data.

B.2.1 Quantile Treatment Effect Approach

We employ a recentered influence function approach (Firpo, 2007; Firpo, Fortin and Lemieux, 2009; Havnes and Mogstad, 2015; Dube, 2019) to estimate quantile treatment effects of cleanup on housing prices using individual housing transaction data.

The recentered influence function, or RIF, is a transformation that represents a distributional statistic.[53] For the τ^{th} quantile q_τ, the RIF is:

$$RIF(p, q_\tau) = q_\tau + \frac{\tau - \mathbb{1}\{p \leq q_\tau\})}{f(q_\tau)} \tag{B.1}$$

In the above, p is the outcome variable and $f(q_\tau)$ is the population distribution function for the outcome variable.

Estimation is a two-step process: first, obtain the RIF for each value of the outcome variable (price). The only element of the expression that varies across observations is $\mathbb{1}\{p \leq q_\tau\}$, which is estimated as a linear probability model. Second, regress the RIF against $Near_i$, $Post_t$, and $Near_i \times Post_t$:

$$RIF_i(q_\tau) = \beta_1 Near_i \times Post_t + \beta_2 Post_t + \beta_3 Near_i + \epsilon_i \tag{B.2}$$

The coefficient β_1 is interpreted as an unconditional partial effect (UPE) of cleanup on the τ^{th} quantile of the price distribution. The identifying assumption is that, in the absence of treatment, the change in shares or frequency of houses from before to after treatment around a given level of the housing price would be the same in the treatment group as in the comparison group.[54]

[53] It amounts to re-centering an influence function. In (B.1), the second term is the influence function.

[54] Following the notation in Havnes and Mogstad (2015), assume that $F_t(p)$ is the counterfactual price distribution for houses on the same tract as the site, and $G_t(p)$ is the counterfactual price distribution for houses in the comparison group.[55] We will consider two periods for simplicity, so $t = 0$ indicates pre-cleanup and $t = 1$ indicates post-cleanup. The counterfactual price distribution for post-cleanup houses is taken to be:

The estimator is invariant to monotonic transformations of the outcome variable. Therefore, it allows a common trend in the outcome variable, both in absolute and relative terms.[56] As Firpo et al. (2009) recommend, we bootstrap the standard errors (clustering at the tract level to parallel other parts of this paper).

We see this investigation as a robustness check of our main effects. Recall that our main effects are estimated at the Census tract level. To ensure that results are directly comparable, we use the exact same definition of distance here. That is, we compare houses on the same tract as a RCRA site ("0 km") to houses on a Census tract that is 5-10 km away from the nearest site. It is of course possible to use a more fine-grained definition of distance with individual transaction data, but we do not consider that to be a useful validation exercise. Figure B.1 shows that houses in Census tracts 5-10 km away are typically about four times as far from the site as those on the same tract as the site, so we are comfortable calling these two groups of houses "Near" and "Far."

B.2.2 Quantile Treatment Effect Results

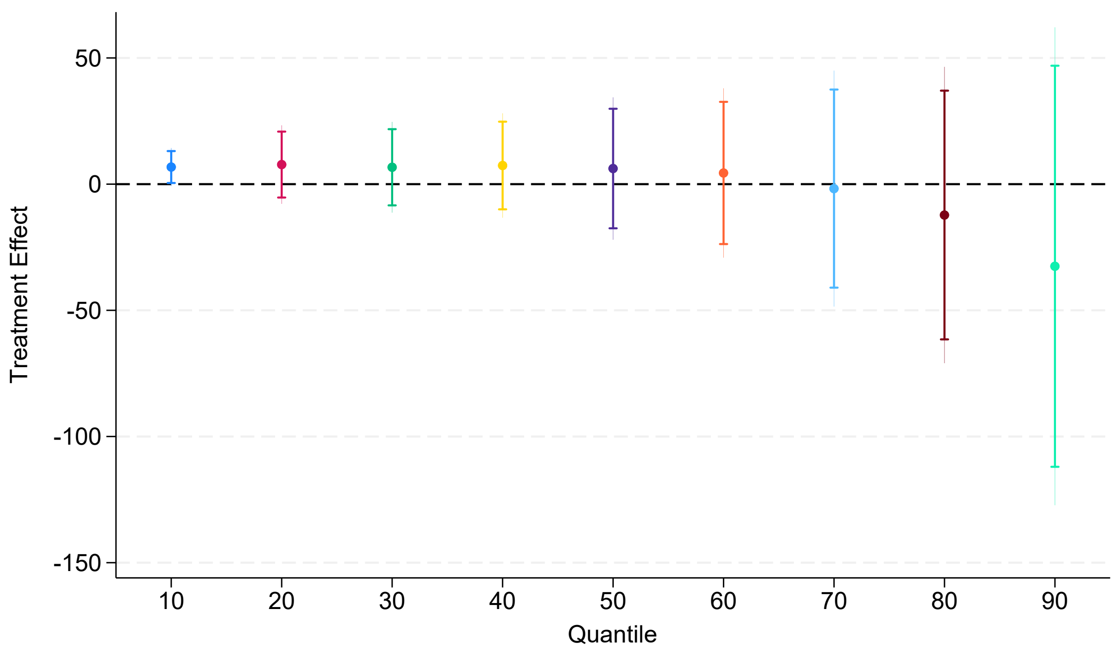
We plot the estimates of β_1 from (B.2) in Figure B.3; the full results are presented in Table B.1. The results show that only the lower quantile effects are similar to those in Figure 2. The only estimated coefficient that is statistically significant at any reasonable level of significance is the 10th percentile quantile effect, which is statistically significant at the 10% confidence level. The magnitudes of the coefficients are positive and comparable in magnitude to the main results for the lower deciles, but become negative starting at the 70th percentile of price. The substantive conclusion of our main analysis– that there is a positive cleanup effect for the lower deciles of the price distribution– is corroborated by this robustness check.

$$\mathcal{F}_1(p) = F_0(p) + G_1(p) - G_0(p) \tag{B.3}$$

So, for a given price level p, the estimated quantile treatment effect is the difference between the actual and counterfactual price distribution at that price level, net of the same difference for the comparison group.

[56]Therefore, taking a logarithmic transformation of price would not change our conclusions.

Figure B.3: Quantile Treatment Effects, 0 km vs. 5–10 km



Notes This figure depicts the estimated Quantile Treatment Effects on the OH-PA Zillow dataset. The full estimates corresponding to this figure are found in Table B.1.

Table B.1: Quantile Treatment Effects, OH-PA Transaction Data

Dep. var: $Price^{kth}$	10^{th}	20^{th}	30^{th}	40^{th}	50^{th}	60^{th}	70^{th}	80^{th}	90^{th}
0 km $\times$ Post	6.7792*	7.7515	6.6866	7.3804	6.1846	4.4280	-1.7677	-12.2430	-32.5447
	(3.8458)	(7.9435)	(9.1791)	(10.5555)	(14.3986)	(17.1275)	(23.8582)	(29.9575)	(48.3108)
0 km	-12.0161***	-16.0279**	-16.9926*	-18.9060*	-16.8883	-17.4965	-13.0027	-7.6392	3.4969
	(3.6431)	(7.6185)	(9.1187)	(10.1613)	(15.2946)	(18.8555)	(25.9299)	(35.4223)	(55.3761)
Post	-15.3814***	-17.1866***	-14.9684***	-12.6226***	-8.1662*	-4.1538	-3.5009	2.1960	-4.3552
	(2.2368)	(3.8472)	(3.9670)	(4.7311)	(4.8619)	(5.8918)	(6.4662)	(8.5378)	(14.4815)
Avg Price	28.469	52.798	76.044	98.899	124.268	150.524	182.462	232.831	326.822
Clusters	931	931	931	931	931	931	931	931	931
Observations	865,001	865,001	865,001	865,001	865,001	865,001	865,001	865,001	865,001

Notes This table presents Quantile Treatment Effect estimates using individual housing transaction data from Zillow Ztrax spanning the states of Ohio and Pennsylvania. We use transaction data from all tracts within 10 km of at most one RCRA facility in these regressions. Standard errors are bootstrapped, clustering on Census Tract to parallel other sections of this manuscript.