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Valuing the Benefits of Superfund Site Remediation: Three Approaches to Measuring Localized Externalities
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ABSTRACT

We apply three complementary approaches designed to identify the localized effects of Superfund site remediation under the CERCLA, examining data at the level of (i) the census tract (paying attention to within tract heterogeneity), (ii) the census block, and (iii) individual house transaction. Our analysis of the within-tract housing value distribution detects statistically and economically significant appreciation in the lower tails resulting from hazardous waste cleanup; deletion of a site raises tract-level housing values by 18.2% at the 10th percentile, 15.4% at the median, and 11.4% at the 60th percentile. These tract results are confirmed by (i) house transaction data that show cheaper houses within each tract are more likely to be exposed to waste sites within one kilometer, explaining their greater appreciation from site cleanup, and (ii) high-resolution census block data that show greater appreciation among blocks lying closer to the cleaned sites. House-level repeat-sales data confirm results from our national level census analysis by showing that deletion raises housing values relative to proposal in specific markets, such as northern New Jersey, but they also uncover a great heterogeneity in the effects of remediation across markets, with no statistical effects from deletion relative to proposal detected in Los Angeles metro, southwestern Connecticut or Boston metro.

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1 Introduction

In the late 1970's, events at Love Canal and the Valley of Drums raised public concern over the health and environmental risks associated with contaminated waste sites. In response to these and other similar incidents, the US Congress established the Superfund program under the Comprehensive Environmental Response, Compensation and Liability Act (CERCLA) of 1980. The Environmental Protection Agency (EPA) has since identified more than 47,000 hazardous waste sites potentially requiring cleanup actions and has placed some of the most seriously contaminated sites on its National Priorities List (NPL). By the end of fiscal year 2007, EPA had classified 1,569 sites as NPL sites (GAO, 2008).

While the costs of Superfund remediation can, in principle, be easily measured, the benefits of these activities are much more difficult to quantify. In particular, there is no explicit market for the clean-up of hazardous waste. Economists have therefore turned to a market where proximity to hazardous waste is an attribute of a marketed commodity – i.e., housing.[1] Controlling for housing and neighborhood attributes, covariation of market price (or owner's stated value) and exposure to Superfund sites at various stages of remediation can be used to measure the change in housing values associated with these cleanup activities. In theory, Superfund cleanup can cause appreciation in house prices by reducing public perception of health or environmental hazards (Hamilton and Viscusi, 1999) and by spurring additional development projects. However, a recent influential study that examines changes in census tract median housing values in response to listing on *or* deletion from the NPL concludes that Superfund cleanup (which costs an average of $40 million per site) fails to significantly raise nearby housing values (Greenstone and Gallagher, 2008). Recognizing that resources are scarce and that government action in one area (e.g., Superfund) might preclude cleanup activities in another (e.g., water or air), an author of that study recommends a policy shift towards "reliance on less ambitious clean-ups like the erection of fences, posting of warning signs around the sites, and simple containment of toxics."[2]

Our study separately re-evaluates the benefits from three different phases of the Superfund remediation process – proposal, listing, and deletion. Importantly, it is the deletion stage that signals the end of the EPA's cleanup process. We use three different approaches that exploit variation in data at the level of (i) the census tract (paying particular attention to within-tract heterogeneity), (ii) the census block, and (iii) the individual housing transaction. These approaches are designed to identify *localized* effects of Superfund cleanup that may not be detectable using census tract median housing values. Our *census tract* approach, using public data, is able to discern separate effects at the lower percentiles of the housing value distribution within each tract. This is important, because census tracts can be large, and Superfund sites are

[1] Our study is narrowly limited to benefits discernible from the hedonic property value models and is, hence, conservative in its conclusions. Many benefits are outside the scope of our study (e.g., certain types of ecosystem benefits). These benefits are discussed in a 2006 review by the Scientific Advisory Body (2006).
[2] Hirsch (2008) downloaded from http://web.mit.edu/newsoffice/2008/superfund-0805.html

not likely to be randomly distributed within them. If sites tend to be situated in low-value neighborhoods, cleanup activities may only result in appreciation for cheaper homes. The impacts of cleanup may, therefore, be imperceptible in the tract's median value, and hard to discern even at the mean. Our *census block* analysis, using restricted-access data, directly addresses the problem of localized externalities by going inside the census tract and examining median housing values at a very fine level of geographic precision.[3] Our *repeat-sales* analysis, using proprietary data from Dataquick Information Services, measures the effects of Superfund cleanup activities at an even finer level of geographic and temporal precision, and makes use of actual transaction prices instead of owners' stated values.

To identify the effect of site status on housing values in the tract and block analyses, we compare changes in values between 1990 and 2000 to changes in exposure to (i) sites that are deleted from the NPL, (ii) sites that were listed but not deleted, and (iii) sites that were proposed but not listed. Our identification strategy relies both on our model and our sample restrictions. First, our panel analysis, which is based on the standard hedonic model, helps identify the effect of site status on housing values. By taking a first-difference between 2000 and 1990 data, we control for time invariant unobservables that are correlated with site status. Second, we restrict our analysis to neighborhoods that (i) lie within 3km buffers of NPL sites that received a hazardous ranking score (HRS) in 1982 within a narrow interval,[4] and (ii) do not lie near other post-1982 NPL sites. The first sample restriction, proposed in Greenstone and Gallagher (2008), can isolate the effect of site status because treatment (i.e., listing on the NPL) is discrete for sites that exceed an exogenous HRS cutoff, while unobservables are likely to be continuous across that regulatory threshold in that sample. The second sample restriction strengthens the first so as to avoid the confounding effects from cleanup activities at other nearby NPL sites. The empirical strategy for our third approach uses house-level repeat-sales data to control for both time-invariant unobservable heterogeneity at the level of the house and time-varying unobservable heterogeneity at the level of the census tract.

Our results at these three levels of analysis reveal that deletion, which signals the end of cleanup activities, significantly raises the value of nearby owner-occupied houses on average at the national-level, but that there is considerable heterogeneity in this effect across metro-specific housing markets. Our tract analysis finds that deletion of a site raises housing values significantly at the lower deciles of the within-tract housing value distribution – by 18.2% at the 10th percentile, 15.4% at the median, and 11.4% at the 60th percentile. Greater appreciation of housing values at the lower percentiles is compatible with NPL sites being in close proximity to lower value houses within a tract. We confirm this spatial pattern in three of four housing markets with transactions data describing individual houses. Results at the block level are

[3] Census tracts are often significantly larger than census blocks. There are as many as 350 census blocks in some census tracts.
[4] For the sake of comparison with previous work that has used larger distances, we also check the sensitivity of our results to 5km buffers.

compatible with the tract level, showing that the deletion of sites raises nearby housing prices by 19.0% and 5.8% for blocks lying < 1km and < 3km from the site.

Our analysis of repeat-sales data uncovers evidence of significant heterogeneity in the effects of Superfund site remediation across metro areas. We find that deletion (measured relative to proposal) causes a sizable appreciation in housing values in northern New Jersey (11.3%), but we find no statistically significant effect of deletion (measured relative to proposal) for LA metro, southwestern Connecticut or metro Boston. While the appreciation in New Jersey indicates that some neighborhoods do recover post-cleanup, the lower prices in Boston at deletion relative to pre-discovery (-6.1%) suggest, conversely, that stigma against contaminated sites and neighborhoods can also persist despite cleanup.

What can be learned from this study?

Typically, researchers are only able to access publicly available census tract data. Recognizing this fact, we demonstrate that our method, which accounts for within tract heterogeneity, can detect benefits from cleanup that may be understated or missed by studies that focus on median tract-level housing values. We then use block data to directly verify that our tract results are not mere artifacts of our method. Indeed, our estimates at the tract and block-levels are similar – the 18.2% appreciation at the 10^{th} percentile of the within tract housing values is comparable to the 19.0% for blocks lying within 1km. Moreover, we use house-level transaction data to verify the story underlying our quantile approach in three out of four markets – i.e., that it is the cheaper houses within each tract which are more likely to be exposed to NPL sites. These results suggest that our quantile approach provides a practical method for estimating the impact of localized externalities, even without access to proprietary or restricted access data.

Nevertheless, our proposed method has its limits. Not all localized benefits detectable with finer resolution block- and house-level data may be found by an analysis of the tract-level housing price distribution. However, given the reality that many hedonic studies are forced rely on tract-level data (Hanna, 2007; Greenstone and Gallagher, 2008; Grainger, 2010), the advantages of which include accessibility and nationwide coverage, our extension avoids an important source of bias that results from a narrow focus on the mean or median tract-level housing values.

We draw two conclusions from our results that are directly relevant for evaluating the Superfund remediation program. First, the average effect of Superfund site remediation on housing values across all markets is positive and significant. Moreover, we note that benefits measured by appreciation in housing values provide only part of the overall benefits from Superfund cleanup. Importantly, these results speak to the aggregate benefit of the Superfund program, which could be compared with program costs in an efficiency analysis. It also provides the strongest available information with which to perform a cost-benefit analysis of a particular candidate site, in the absence of estimates from specific-metro areas or site-specific information. Second, while Superfund remediation raises housing values on average at the national-level, there is considerable variation in that effect across housing markets. This heterogeneity suggests that to perform a cost-benefit analysis of a particular candidate site, metro-specific estimates, which assess the remediation of multiple sites in the relevant regional housing market, would be appropriate.

Outline

The remainder of this study proceeds as follows. Section 2 discusses the main milestones of the Superfund remediation process and provides a brief review of the literature. Section 3 describes our three alternative strategies for identifying the impacts of site remediation based on tract-level, block-level, and house-level data. Section 4 describes the empirical specifications used in each of these approaches. Section 5 discusses our data sets and Section 6 reports the results of our analyses. Section 7 concludes and discusses some possible extensions to our work. Appendices A1 and A2 provide details on the construction of variables. Appendix A3 provides a comparison of our study with Greenstone and Gallagher (2008).

2 Benefits from Superfund cleanup

2.1 Superfund Milestones

Our analyses focus on the three major milestones in the National Priorities List (NPL) process, in which the EPA publicizes information about the site to members of the public by entering information into the Federal Register and soliciting public comment. These three stages are (i) proposal to, (ii) listing on, and (iii) deletion from the NPL. To measure the benefits from Superfund site cleanup, we focus on deletion from the NPL, which signals that cleanup is completed and no further response is required to protect human health or the environment. While "construction complete" (a designation given to sites prior to deletion) has been often used by EPA since 1990 as an indicator of successful remediation, we chose instead to use deletion for this purpose.[5] There are three reasons for this choice – (i) the construction complete designation may be applied before final cleanup levels have been achieved, (ii) the deletion designation is accompanied by a public announcement in the Federal Register and a major newspaper, and (iii) relying on deletion instead of construction complete is a conservative

[5] See Probst and Sherman (2004) for a discussion of the construction complete designation. The use of construction complete instead of deletion acknowledges an important reality of many Superfund sites – namely, that sites may not actually be fully cleaned-up for decades following the completion of construction work associated with remediation activities. This may be particularly true, for example, when groundwater has been contaminated. In these cases, construction complete is a very important milestone from a policy-making perspective.

strategy for valuing the benefits of cleanup.[6] In our census analyses, we recover values for each of these three stages of the remediation process, but do not include "discovery" as it is not identified under most of the sample cuts we use with census data. In our house-level analyses, however, we are able to include the discovery designation as we do not need to rely on these sample cuts for identification. Below, we describe these (and other) major milestones in detail.

Discovery, Preliminary Assessment, and Site Investigation

A site is discovered when the EPA becomes aware of it from the general public or from local and state environmental agencies. The EPA then conducts a preliminary assessment, which is designed to distinguish, based on limited data, between sites that pose little or no threat to human health and the environment and sites that may pose a threat and require further investigation. If the preliminary assessment results in a recommendation for further investigation, a site inspection is performed. Investigators collect environmental and waste samples to determine what hazardous substances are present at a site, whether these substances are being released into the environment, and if so, whether they have reached nearby targets. Information collected during the preliminary assessment and site inspection is used to calculate a Hazard Ranking System (HRS) score.[7]

Proposal and Listing on the NPL

Based on the preliminary assessment and site inspection, the EPA proposes a site to the NPL in the Federal Register. The EPA then accepts public comments on the sites for 60 days and responds to public comments. The EPA will list the site on the NPL if it meets at least one of three criteria – (i) the HRS score is of sufficient magnitude, (ii) the state environmental authority designates the site to be a top priority, or (iii) the US Public Health Service recommends removing all people in close proximity to the site.

Cleanup Process

After the site achieves listing status on the NPL, the cleanup process commences in a series of steps. The remedial investigation collects data to characterize site conditions, assess risks to human health and the environment, and evaluate the potential performance and cost of

[6] In particular, if some of the benefits of cleanup are actually captured by construction complete but we lump that designation in with listing, it will make it more difficult to find evidence of benefits associated with moving a site from listing to deletion.
[7] The HRS score serves as a numerically based screening device that uses information from initial, limited investigations. Sites with an HRS score of 28.5 or greater are eligible for listing on the NPL and require the preparation of an HRS scoring package. The story behind the 28.5 cutoff is described in detail below.

alternative treatment technologies. The feasibility screening process develops and evaluates alternative remedial actions. The EPA publishes the Record of Decision (ROD), a public document that explains cleanup alternatives, and makes this document available to the public through the EPA online ROD system.[8] The technical specifications for cleanup remedies and technologies are designed in the remedial design phase. The actual cleanup operations are implemented in the remedial action phase.

Deletion from the NPL

Deletion of a site from the NPL requires that the necessary actions for remediation have been completed and the site no longer poses a threat to human health. Prior to deletion, the EPA posts plans to delete the site in a local newspaper and solicits public comment. Once deletion is deemed appropriate, the EPA will enter notification of deletion in the Federal Register.

2.2 Superfund Cleanup and Housing Market Effects

At least three Superfund milestones – proposal, listing, and deletion – can influence neighborhood housing prices. Proposal of a site to the NPL may reduce neighborhood housing prices when this action provides new information to the housing market that contamination is severe enough to warrant the potential listing of that site on the NPL (although, if the housing market expects that proposal signals that the site is likely to be remediated, this countervailing factor will dampen the extent of that depreciation). Housing prices have been found to decline due to perceived health risks from a nearby site (Hamilton and Viscusi, 1999)[9] and from the stigma associated with that contaminated site.[10] Listing of a site on the NPL is associated with two countervailing forces; (i) it may reduce housing prices by confirming the severe nature contamination of that site, but (ii) it may also increase housing prices by signaling that the site will be remediated.

[8] A ROD contains site history, site description, site characteristics, community participation, enforcement activities, past and present activities, contaminated media, the contaminants present, scope and role of response action and the remedy selected for cleanup.
[9] Davis (2004) finds that information on health risks are capitalized into housing values. In particular, the emergence of a cancer cluster of pediatric leukemia resulted in the depreciation in housing values in a Nevada county relative to a nearby county.
[10] Stigma (Fischoff, 2001) can indirectly drive down housing prices even if well-informed buyers, who do not object in principle to buying the houses in question, are willing to pay less because they expect future stigmatization to reduce future demand for these houses. Messer et al (2006) describe "when residents or potential buyers are extraordinarily fearful of a site, they may respond by shunning the site.… If risks are perceived as being excessive, people replace calculations of risk versus benefit with a simple heuristic of shunning, the avoidance of the stigmatized object."

Deletion from the NPL, which is the focus of our study, can increase housing prices through two channels. First, cleanup directly reduces the health risks and disamenities from a site. Second, cleanup may prompt further development in the area surrounding the site, including the potential for re-zoning from a lower-value commercial use to higher-value luxury development. Because cleanup does increase the likelihood that these development projects are undertaken, we attribute housing price appreciation via this indirect channel to the cleanup operations.

As long as the development occurs *conditional* on cleanup being undertaken, our study correctly attributes the benefits from that development as a component of the benefit from Superfund cleanup. For example, the Empire Canyon Daly West Mine Superfund site in Utah underwent extensive remediation under the Superfund program. After the remediation, the landowners leased that site for the development of a luxury resort, including a hotel, spa and condominium project (EPA, 2008). Such an outcome does not present a problem for our analysis, but rather represents the sort of mechanism through which remediation is translated into higher housing prices. An estimation problem would only arise if the causality went in the other direction – e.g., landowners decide to build a luxury resort (which was going to raise nearby housing prices regardless of EPA actions), and the EPA responds by moving the site through the process to deletion. Our literature review has not uncovered evidence of such actions by the EPA.

2.3 Benefits from Superfund Cleanup

The appreciation of housing values is one dimension of the potential benefits from Superfund cleanup. In particular, the appreciation of housing values will reflect the perceived reduction in risk among homebuyers.[11] Other potential benefits may not be accounted for by the local homebuyers, such as downstream ecological benefits. To the extent that buyers do not fully incorporate the reduction in risks in their valuation, the measured appreciation is a lower-bound estimate.[12] Other potential benefits include reduced health risks that are typically measured by the value of a statistical life.

[11] Gayer et al (2000) find that residents around seven NPL and non-NPL sites in Grand Rapids Michigan update their perception of risks when the EPA released their assessment of the sites. They find that consumers' perceptions of cancer risks are overestimated before the EPA releases a detailed estimate of the risks; those consumers therefore pay a much higher premium for houses further from the site before the release of the report. They find that even at the inflated perceived risk levels, the upper-bound for the willingness to pay for the six sites in the area to be cleaned is about one-sixth of the remediation cost. However, one criticism of this study is its focus on one health outcome alone (i.e., cancer understates the overall health risks).

[12] The appreciation of housing values will reflect the perceived reduction in risk and stigma among homebuyers. Therefore, any reduction in risks that are not factored into the perceived risks, due to incomplete information or individuals' erroneous assessment of risks, will not be capitalized in housing values.

Our tract and block analyses estimate the *average* appreciation in housing prices across sites, though the rate of appreciation is likely to differ across sites. Variation in appreciation in housing values may arise from the final use of the sites, the neighborhood attributes, and the extent of cleanup (see Section 6.2, "*Estimation Issue #2: Variation in extent of remediation*" for details). Cleanup in areas with high demand for housing or potential for development may lead to greater appreciation in housing values than cleanup in sites that are surrounded by other disamenities such as high crime rates and brownfields.[13]

2.4 Previous Studies

The large literature that seeks to measure the value of Superfund site remediation has been exhaustively reviewed in Schultze et al (1995), Kiel and Williams (2007), Sigman (2008) and EPA (2009).[14] First, we briefly describe the hedonic approach that examines median housing values in locations that vary in the number or status of sites contained within. We then provide a short summary of papers that use a second approach that considers a single site and determines how proximity to it impacts the selling price of nearby homes. That effect is measured with a distance gradient that typically varies with site status.

Hedonic Approach #1: Comparison of housing values across neighborhoods

Greenberg and Hughes (1992) study seventy-seven communities in New Jersey and find that sale prices of houses in Superfund communities appreciate by less than those in non-Superfund communities. Noonan et al (2007) study the effect of Superfund remediation activities on housing values measured at the block-group level using a national sample. Like our analysis, they use fixed effects to control for time-invariant neighborhood unobservables. Unlike our analysis, they compare those block groups that are close to waste sites with other block groups across contiguous US.

We build most directly upon Greenstone and Gallagher (2008), who examine how tract *median* housing prices vary depending upon whether they contain a site that has been listed on the NPL or one that has been proposed but not listed. Their study makes two important methodological contributions. First, they argue that studies examining the impact of Superfund cleanup should restrict their comparisons to only those neighborhoods that host NPL sites – in other words, the approach taken in Greenberg and Hughes (1992) and Noonan et al (2007) may

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[13] The Gowanus Canal in Brooklyn, which was listed on the NPL in 2010, is an example of an area of high residential demand and development potential. In February 2009, the city of New York granted a rezoning request for residential housing along the waterway (Navarro, 2010). Conversely, high crime and brownfields may limit the housing price appreciation from cleanup at some sites. Nevertheless, equity considerations may still play a part in the decision to support their cleanup.

[14] In particular, see Table 4 in Schultze et al (1995) and Tables 1 and 2 in EPA (2009).

lead to biased estimates. In particular, Greenstone and Gallagher argue that the comparison of neighborhoods that host NPL sites (in the proposed or deleted phase) with those that do not will yield biased estimates because unobservables will differ systematically across these two types of neighborhoods.[15]

Second, to control for unobservables that may be correlated with listing, Greenstone and Gallagher apply a regression discontinuity (RD) design that draws on the early institutional history of the NPL. In the first year of Superfund legislation, the EPA's assessment process identified 687 of the most dangerous sites. Budget constraints forced the EPA to choose only 400 sites to list on the NPL, and the EPA employed the HRS ranking to choose those sites that posed the greatest risks. In the HRS ranking of these sites, it turned out that an HRS score of 28.5 served as the cutoff between the 400th treated and 401st untreated site. Greenstone and Gallagher's RD analysis examines 227 sites with HRS scores that were 12 points above or below the 28.5 regulatory cutoff, thus exploiting the dichotomous treatment at the regulatory cutoff, while assuming that the unobservables were continuous across that cutoff.[16]

There are four aspects of Greenstone and Gallagher's analysis that suggest a downward bias in their estimates of the benefits from Superfund cleanup (Smith, 2006). First, to measure the effect of Superfund cleanup on housing values, Greenstone and Gallagher examine the effect of a variable that conflates two distinct milestones in the Superfund process – listing and deletion.[17] Conflation of these treatments is required in order for them to be able to implement their instrumental variable strategy. Specifically, Greenstone and Gallagher use the 1982 HRS score to instrument for the variable indicating that a site has been listed on (or deleted from) the NPL by 2000; that one variable cannot separately instrument for the two milestones of listing and deletion. However, one might worry that conflation of listing and deletion will bias downward the estimated benefits from cleanup. In particular, listing has ambiguous overall effects on housing prices, while deletion is likely to raise housing prices. In contrast, our study measures the value of cleanup by examining the effect of deletion from the NPL on housing

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[15] Table II in Greenstone and Gallagher shows that these two sets of tracts differ significantly in their observables and by extension, are likely to differ in their unobservables.

[16] Greenstone and Gallagher (2008) note that the use of the regulatory cutoff at HRS equal to 28.5 is a good empirical strategy for at least three reasons. First, the HRS scores assigned to the 1982 sample were established before the 28.5 threshold was set. It is unlikely that scores were manipulated to obtain Superfund treatment, particularly in the narrow range just above or below the cutoff. Second, the HRS score is a noisy measure of risk, and thus, the true risk is likely to be similar around the 28.5 cutoff. Third, there was no evidence that the sites below 28.5 were "safe."

[17] While their primary analysis deals with the values placed on owned housing, Greenstone and Gallagher also examine the sensitivity of *rental* rates to Superfund site remediation. When looking at rental rates, they do separate the effects of different Superfund milestones – (1) on the NPL but a ROD has not been issued, (2) a ROD and/or cleanup has been initiated but not completed, and (3) construction complete or deleted from the NPL. They report that the separate milestones do not influence rental rates. Our study focuses on owned housing and three Superfund milestones - proposal, listing, and deletion, because EPA publication in the Federal Register and solicitation of public comment at these milestones can provide information to the housing market resulting in the capitalization of that information into housing values.

prices; this effect is measured separately from that of NPL listing.[18] As we separately measure listing and deletion, the 1982 HRS score instrumental variable (IV) strategy cannot be used in our study.

Second, to measure the effect of cleanup, we would ideally compare tracts exposed to sites that are cleaned up to the counterfactual that those sites were not cleaned up. To serve as the counterfactual (i.e., the "non-treatment" baseline), Greenstone and Gallagher use tracts exposed to both sites proposed to NPL (but not listed) *and* sites that were never proposed to the NPL (i.e., non-NPL sites). However, tracts that host non-NPL sites may have systematically different unobserved attributes compared with tracts that host NPL sites. If non-NPL sites are associated with "better" unobservables than sites proposed to (but not listed on) the NPL, this would create an elevated baseline of housing values against which the benefit of treatment is measured (thus biasing downward the estimated benefit from treatment). In contrast, our study uses only tracts exposed to NPL sites in their pre-proposed stage as the "non-treatment" baseline; these sites are more likely to be similar to the NPL sites that have progressed to deletion. Appendix A3 describes in detail the differences in the construction of our data set and Greenstone and Gallagher's data set.

Third, in defining the neighborhood over which the cleanup exerts a localized effect, Greenstone and Gallagher consider attributes of tracts falling within buffers of three and five miles around sites.[19] However, panel data studies of the association between hazardous waste sites and housing prices have detected effects at a maximum distance of 2 to 2.5 miles (≈ 3.2 to 4km) with a mean estimated price effect of 7.4% (reviewed in Jenkins et al, 2006). Greenstone and Gallagher's larger neighborhood definition may therefore encompass both houses that are affected and unaffected by the effect of cleanup, which will dilute the estimated benefits of that treatment. In contrast, we define the affected neighborhood using smaller buffers of 3 km (≈2 miles) around the sites and test the sensitivity of our results to larger buffers of 5km (≈3 miles).

Fourth, Greenstone and Gallagher's focus on the median housing value at the census tract level may miss or understate the benefits from Superfund cleanup if such benefits are *localized*. Given the fairly large size of many census tracts, we suspect that there may be significant within-tract heterogeneity in housing values. In particular, we demonstrate that, within a tract, cheaper houses are more likely to be located in closer proximity to sites, and that the cleanup of sites will have a greater impact on the values of these subsets of houses. Therefore, to capture the impact on these subsets of houses within the tract, we use two approaches. First, we examine the *deciles* of the tract-level housing value distribution, not simply the median. Second, we examine housing values at the level of the census *block*, which is a higher-resolution geographical unit than the census tract, and therefore less prone to problem of within-unit heterogeneity.

[18] Other studies have measured the distinct effects of these various milestones (Kiel and Zabel, 2001; Cameron and McConnaha, 2006; Kiel and Williams, 2007) and treated these milestones as distinct (Sigman, 2001).
[19] Greenstone and Gallagher's choice of three miles is based on the EPA's use of that distance in their HRS calculations, as in most cases, contaminants can migrant to at least this distance (Greenstone and Gallagher, 2008).

Appendix A3 provides the most direct comparison possible of our methodological approach and that used by Greenstone and Gallagher (2008). First, we construct an "intersection sample" (i.e., the intersection of our two data sets). Using that sample we then recover results similar to Greenstone and Gallagher using their methodology. Finally, we show how those results change (in particular, how significant effects of NPL deletion are realized) when we (i) separately control for the listing and deletion site statuses, (ii) exploit panel variation to control for time-invariant unobservable tract attributes, and (iii) look at points on the within-tract housing value distribution besides the median.

Hedonic Approach #2: Distance from the disamenity

The second approach is to take a known site, determine how distance from it impacts the selling price of nearby homes, and determine how that distance gradient varies with site status. This approach requires individual transaction data for each house sold in a given period and uses hedonic pricing theory stemming from Rosen (1974), with distance from a site serving as the main housing characteristic of interest. Papers employing the single-site "distance approach" primarily rely on cross-sectional variation in house prices.[20] Kiel and Zabel (2001) focus on the two Superfund sites in Woburn, MA. They are interested in the premium paid for distance to the nearest site but do not allow multiple sites to enter into the hedonic price function. While they do not employ panel data, they do estimate the price function at several points in time. Kiel and Williams (2007) similarly estimate separate hedonic price functions based on distance to the closest site, repeating the process for each of 74 NPL sites spread across 13 US counties. They find significant evidence of heterogeneity in the effect of Superfund remediation activities.

The primary strength of the distance-based studies is that they allow for a great deal of heterogeneity in calculating the benefits associated with site remediation. In particular, these single-site analyses can recover a different set of estimates pertaining to the homes surrounding each site. This flexibility, however, comes at a cost. First, sites do not exist in isolation – a significant fraction of NPL sites are located in close proximity to other NPL sites, creating the potential for important omitted variable bias.[21] There may be other location-specific unobservables (besides NPL sites) that will also bias the estimated impact of the NPL site in question. For example, there is the potential for correlation between the location of hazardous waste and economic opportunity to confound the disamenities of living near a site with the relative appeal of living near what could be an employment center (Farber, 1998). Similarly, if there is correlation between remediation decisions and unobserved neighborhood quality,

[20] A second subset of papers employing the "distance approach" use information in repeat sales of individual houses to control non-parametrically for time invariant unobserved heterogeneity at the house level (Miller, 1982; Case et al., 2006).
[21] 37.7% of NPL sites in our data have another site within 4.59 km, which is the mean distance to a site in Kiel and Williams (2007) plus two standard deviations.

estimates of the effect of hazardous waste cleanup could be upwardly biased. To the extent that these unobservables do not vary over time, panel data distance analyses will be able to control for them.

A second potential problem arises in terms of how one should use these heterogeneous marginal effects generated from a single-site distance analysis for policy. In particular, given a newly proposed candidate site, it is not clear which of the many estimated heterogeneous marginal effects one should apply to it. When single-site distance analyses are performed separately on each of a number of sites (as in Kiel and Williams (2007)), the average effect that one recovers is based on the characteristics of the sites that happened to be chosen, which may not be representative of all sites. There is no reason to expect that this average would apply to all sites in the program in general. With access to detailed site attribute data, one could match a new candidate site to a previously valued site based on these attributes. However, with that sort of data, we could allow for explicitly heterogeneous marginal effects in our multi-site analysis as well.

Do the methods described in the previous sub-section solve these problems? Not necessarily. In particular, estimates from national analyses may not be accurate measures of the hedonic price schedule in a particular region, since there is no reason to expect preferences for environmental quality to be constant across the nation. There is, however, a trade-off – site-specific analyses uncover idiosyncratic price gradients for specific sites in specific places, but these estimates cannot be generalized to different sites in different locations. A more valuable resource would be a set of estimates that are generalizable across many sites in a particular region – i.e., a regional or city-level analysis that accounts for the idiosyncratic preferences of the area while averaging over all hazardous waste sites the region. Imposing homogeneity on hazardous waste sites in a region is a more palatable assumption than imposing homogeneity over sites in a country, and the resultant estimates should be more applicable within the region than estimates from the national sample.

These concerns motivate our use of housing transactions data later in the paper. Specifically, to uncover the regional implicit price for environmental quality as it relates to Superfund sites, it is necessary to use the regional housing market, not simply housing transactions in a radius around a site. However, a natural consequence of incorporating all sites in a region is the potential for houses to be located near multiple sites. This problem is exacerbated in regions where Superfund sites are clustered spatially. To address this problem, we adopt an approach that incorporates all sites in a particular region into the analysis and treats them homogeneously; the results should be interpreted as price effects from the "average site" in the metropolitan area or region. That effect will differ, however, across regions.

We would suggest that there is value in both multi-site, cross-neighborhood analyses and single-site, distance-based analyses. Single-site analyses are particularly useful for recovering a distribution of heterogeneous marginal effects, but may be prone to bias associated with unobserved neighborhood attributes. A national model (like the one we estimate here) sacrifices heterogeneity but does control for omitted variables better in deriving an unbiased estimate of average program effects.

3 Estimation Method

3.1 Geographical Resolution: Tracts, Blocks and Houses

We conduct our study at three different levels of geographic resolution – census tracts, census blocks, and individual houses. A summary of the strengths and weaknesses of each of these three approaches, along with the ways in which they complement one another, is presented in Table 1.

In our census tract analysis, we look for evidence that Superfund remediation, which we expect to have highly localized effects, has greater impact at the lower percentiles of within-tract house value distributions. The Decennial Census provides counts of houses with owners' stated values in various intervals, allowing us to calculate the discrete distribution of house values within each tract. These intervals are described in Appendix Table A1. We use straight lines to connect the midpoints of these intervals portrayed in a cumulative distribution function histogram; we then read the cumulative distribution function of house values in each census tract from those lines. Percentiles read off of these distribution functions are then used as dependent variables in our empirical analysis.

We expect Superfund remediation to have greater influence on house values in the lower percentiles of the within-tract house value distribution if Superfund sites lie closer to the lower value houses within each tract; we document, using proprietary housing transactions data, that this is indeed the within-tract spatial distribution for three out of four housing markets. The tract analysis takes snapshots of the NPL status in 1990 and 2000, and relates changes in percentiles of the housing value distribution to changes in the status of the nearby NPL sites between 1990 and 2000.

We also use restricted-access census block data to measure appreciation in house values when these effects are highly localized. The block data directly improve upon the tract analysis, as the coarse geographical resolution that accompanies census tracts may obscure potential benefits from site cleanup.[22] Like the tract analyses, the block analysis takes snapshots of the NPL status in 1990 and 2000, and relates changes in housing values to changes in the status of the nearby NPL sites between 1990 and 2000.

[22] The main advantage of the tract analysis is that it can be conducted with publicly available data. Use of the block data requires special sworn status and project approval from the Census Bureau.

Finally, repeat-sales data from Dataquick Information Services provide detailed information for every housing transaction (i.e., sales prices, date of sale, housing characteristics, and geographic coordinates) in a variety of metropolitan statistical areas (MSA's). Pairing this with information about every Superfund site in each of these MSA's (and its status at each point in time) allows for valuation of each stage of the remediation process. There are many advantages of this approach over the tract and block analyses described above. (i) These data provide even finer spatial and temporal resolution than do the restricted-access decennial census block data. (ii) By using actual transaction prices, we overcome the problems of misreported or biased self-reported stated values in tract and block census data.[23] (iii) Working with one MSA at a time, we are able to detect heterogeneity in the welfare effects of remediation across cities. (iv) Considering one MSA at a time also allows us to use a more realistic definition of the housing market confronting potential home buyers (i.e., a single MSA as opposed to the entire county). (v) By tracking the same house across multiple transactions, we are able to account for house fixed effects in a way that is not possible using repeated cross-sectional census data. The main disadvantages encountered with repeat-sales data is that the sample of houses actually taken to market more than once in the sample period may not be a random sample of the underlying population of all houses, and that the sample of sites considered may not be nationally representative. However, as seen in Table 5, the set of houses in our data that sell multiple times are similar in terms of prices to houses that sell only once. It is also possible that the house fixed effect assumption (i.e., that unobserved house attributes do not change over time) may be violated, although we make sample cuts (described below) in an effort to avoid this problem.

3.2 Identification Strategy: Tract and Block Analysis

In our baseline specification, we examine tracts that lie in 3km buffers surrounding NPL sites. Taking a conservative approach, a tract is included in a buffer if any part of it is found using GIS software to intersect with the 3km buffer.[24] The tract and block analyses take snapshots of the NPL status of each tract in 1990 and 2000 and relate changes in percentiles of the housing value distribution to changes in those site statuses. We compare changes in owner-occupied housing values between 1990 and 2000 to changes in exposure to (i) sites that are

[23] Kiel and Zabel (1999) compare owner assessments and actual sale prices for three metropolitan areas over eleven years. They find that, although owners generally overestimate their values by 5%, the differences in these two values are not related to housing or owner characteristics other than tenure in the house. DiPasquale and Somerville (1995) find that price series based on transaction prices and owner-reported values have very similar time-series patterns, although they can differ near market turning points. They also report that owner estimates of value are consistently higher than reported transaction prices.

[24] The disadvantage of this approach is that an entire tract can be quite large. An alternative approach would be to examine only sub-segments of tracts that fall within the 3km buffer. However, doing so would necessitate an assumption that each tract is internally spatially homogeneous, which we show to be untrue.

proposed for the NPL but not listed, (ii) sites that are listed on the NPL but not deleted, and (iii) sites that are deleted from the NPL. Our tract and block analyses rely on two complementary identification strategies: sample restrictions and modeling strategy.

Sample restrictions

We delineate three samples of neighborhoods for comparison; the third is our strictest and most preferred. First, in our "all-NPL" sample, we examine tracts that contain land inside the buffers surrounding 1,722 sites that have been proposed to the NPL (before Jan 1, 2010); many of these sites subsequently went on to further stages of remediation. Our assumption is that the set of tracts in buffers surrounding sites are likely to be more similar to one another than the set of all tracts in the country. The main difference among the tracts in our all-NPL sample is with respect to their receipt of treatment – i.e., some of the sites in these neighborhoods were listed on the NPL, and some of those sites were subsequently deleted from the NPL.

In our second sample, we build upon the Greenstone and Gallagher regression discontinuity (RD) research design. In our RD sample, we examine neighborhoods that host a subset of the ever-proposed NPL sites (i.e., those that were scored in 1982 and whose scores lie between 16.5 and 40.5). Our RD sample comprises 1,454 tracts surrounding 212 sites. While the RD design aims to address the unobservables correlated with listing, we expect that the balancing of unobservables across both sides of the treatment discontinuity may help control for unobservables that are correlated with deletion as well.

Our third sample corrects for one additional confounder – tracts in the RD sample may lie in close proximity to other sites that can be quite heterogeneous. In particular, tracts in our RD sample may lie near (i) sites with HRS82 < 16.5 and HRS82 > 40.5 and (ii) sites proposed to the NPL in later years (which were not scored in the 1982 sample). We therefore assemble a *strict* RD sample using only those tracts from the RD sample that are not themselves within 3km of another site that was unscored in 1982 or whose 1982 HRS score lies outside the [16.5, 40.5] interval. Deleting tracts that violate this rule leaves our *strict* RD sample with 818 tracts surrounding 187 sites. Tables 3(a) and 3(b) summarize the differences between the RD and strict RD samples.

Modeling strategy

Our panel strategy controls for time-invariant unobservables that cause neighborhoods to have an above or below average distribution of housing values both before and after their receipt of treatment. We begin with the standard hedonic specification that relates tract or block housing values with contemporaneous tract or block attributes. We then use panel data techniques, differencing across the 2000 and 1990 specifications in order to control for time invariant unobservables at the tract or block level.

Summarizing our identification strategy, we rely on (i) the RD or strict RD samples to ensure that sites which are listed, and those that are not yet in listed, are similar in their unobservables, and (ii) panel methods to further control for time-varying unobservables. To identify the effect of deletion, we rely primarily on the panel methods to control for time-invariant unobservables.

4 Regression Models

4.1 Census Tracts – Main Specification

We begin with a basic hedonic regression model relating owner-occupied housing prices to the characteristics of the house and the neighborhood.

$$(1) \quad lnH_{k,t}^{\theta} = \beta_{1,t}^{\theta} P_{k,t} + \beta_{2,t}^{\theta} L_{k,t} + \beta_{3,t}^{\theta} D_{k,t} + \beta_{4,t}^{\theta} X_{k,t} + \nu_k^{\theta} + \varepsilon_{k,t}^{\theta}$$

The subscript k indexes tracts that lie within a 3km buffer of an ever-proposed NPL site. A tract is included as long as any part of it falls within the 3km buffer. $lnH_{k,t}^{\theta}$ is the natural log of the θ^{th} percentile of owner-occupied housing values in tract k in year t ($t = 1990, 2000$). X is a vector containing characteristics of the housing stock along with the socioeconomic and demographic attributes of the tract. These variables and the housing value distributions are summarized in Tables 3 (a) and (b) for our RD and strict RD samples. ν_k^{θ} are time-invariant tract-level unobservables specific to houses in the θ^{th} percentile, and $\varepsilon_{k,t}^{\theta}$ is a tract-percentile-year unobservable.

Our main variable of interest is the exposure of the tract in 1990 or 2000 to sites that are deleted by that time period. Other variables of interest are exposure of the tract to sites that are proposed or listed. Exposure is defined as the share of the land area in a tract that falls into 3km buffers surrounding NPL sites.[25] Specifically, we first use GIS to draw 3km buffers around each NPL site. A tract's exposure to NPL sites at each stage of remediation is then defined as the ratio of its area of overlap with the 3km buffers drawn around sites at that stage to its total area. Further detail on the calculation of tract exposures, including illustrative maps, is included in Appendix A2. We also describe in more detail below how we handle situations in which a tract is simultaneously exposed to multiple sites at the same stage of the remediation process.

[25] Note that EPA defines site location by the geocoordinates of the site's centroid. Sites may vary greatly in size, however, and we would expect the geographic "reach" of larger sites to be greater. Without specific GIS information describing the boundaries of all sites, our best option is to use centroid geocoordinates to indicate location.

We use $D_{k,1990}$ to represent exposure of tract k to sites that are deleted by 1 Jan 1990, and $D_{k,2000}$ to represent the corresponding measure for 1 Jan 2000. $P_{k,1990}$ and $P_{k,2000}$ similarly represent exposure of the tract to sites that are proposed by 1 Jan 1990 and 1 Jan 2000, respectively. $L_{k,1990}$ and $L_{k,2000}$ correspond to listed status.

Next, we take the difference between the 1990 and 2000 regression models (restricting parameters to be constant over time), thereby removing the effect of time-invariant tract-percentile unobservables.[26]

$$(2) \qquad lnH_{k,2000}^{\theta} - lnH_{k,1990}^{\theta} = \beta_1^{\theta}\left(P_{k,2000} - P_{k,1990}\right) + \beta_2^{\theta}\left(L_{k,2000} - L_{k,1990}\right) + \beta_3^{\theta}\left(D_{k,2000} - D_{k,1990}\right) + \beta_4^{\theta}\left(X_{k,2000} - X_{k,1990}\right) + \left(\varepsilon_{k,2000}^{\theta} - \varepsilon_{k,1990}^{\theta}\right)$$

The coefficient β_3^{θ} provides a measure of the appreciation of housing values at the θ^{th} percentile resulting from deletion of the site relative to the pre-proposal baseline. Consider tracts whose land area falls within the 3km buffer and suppose that the site status changes from listed in 1990 to deleted in 2000. The variable measuring the share of the tracts' exposure to deletion goes from 0 to 1 for tracts that are fully within the 3km buffer, and from 0 to a positive value (bounded at one) for tracts that are partially within the 3km buffer. Accounting for the natural log in the dependent variable, a one-unit increase in exposure to deleted sites would imply that house values at the θ^{th} percentile appreciate by $100\left[exp\left(\beta_3^{\theta} - \frac{1}{2}V\left(\beta_3^{\theta}\right)\right) - 1\right]$. (Kennedy, 1981) Because this transformation does not significantly change any of our results, we ignore it and simply discuss parameter estimates in the text of this report in order to streamline the exposition. Table 2 summarizes the interpretation of all the coefficients.

The change in exposure to proposed sites and the change in exposure to sites in the final listing stage similarly capture the change in house values associated with these steps in the remediation process. When a site moves from being not proposed in 1990 to the proposed category in 2000, the variable marking the change in exposure to proposed sites for a tract lying fully inside the 3km buffer takes the value 1. A negative β_1^{θ} indicates that the θ^{th} percentile of the housing value distribution drops in response to increased exposure to proposed sites. When the site progresses from proposed status in 1990 to listed status in 2000, the change in exposure

[26] Our conservative interpretation of the coefficients in the panel analysis is that they measure the capitalization into the housing values resulting from the cleanup (Kuminoff and Pope, 2010). Capitalization into housing values is in itself valuable information for policymakers in judging the benefits from Superfund cleanup and affects the local economy including the property tax base. If the coefficients are in fact stable over time, the estimates can be further interpreted as measures of willingness-to-pay. To our knowledge, with two years of data, it is not possible to test this assumption of stability of coefficients.

Looking across deciles, we assume only that the tract-level unobservable affecting the θ^{th} percentile house in 1990 has to be the same tract-level unobservable affecting the θ^{th} percentile house (whatever house that may be) in 2000. We do not take the restrictive interpretation that the θ^{th} percentile house in 1990 has to be the same θ^{th} percentile house in 2000.

to proposed sites for a tract lying fully inside the buffer takes the value -1, offsetting the original change when the site was originally proposed.

4.3 Census Blocks

The cross-section and panel regression models for census blocks are defined analogously to equations (1) and (2), except that (i) block median values replace within-tract percentiles of the house value distribution, and (ii) exposure is defined by a count of Superfund sites at each stage of remediation lying within a certain distance of the centroid of each block. The baseline sample is comprised of blocks contained in all census tracts that have some overlap with the 3km buffer surrounding an NPL site. Table 4 summarizes the block data for the all-NPL sample. Summary statistics for the RD sub-samples could not be released because of census clearance rules.

We run two separate regression specifications at the block level, each with a unique set of variables of interest. In the first block-level regression model, we include the counts of NPL sites located less than d km from the centroid of census block k at time t that are proposed ($P_{k,t}^{d}$), listed ($L_{k,t}^{d}$), and deleted ($D_{k,t}^{d}$). We use four distances (i.e., < 0.25, < 0.5, < 1, and < 3km) – one appears in each of four separate specifications. In the second block-level regression model, we include the counts of NPL sites located within various distance *bands* from the centroid of the census block k at time t that are proposed ($P_{k,t}^{\underline{d},\overline{d}}$), listed ($L_{k,t}^{\underline{d},\overline{d}}$), and deleted ($D_{k,t}^{\underline{d},\overline{d}}$). We simultaneously use four distance bands (i.e, 0-1, 1-2, 2-3, and 3-5km) in a single specification.

The interpretation of the coefficients on the count variables is as follows. Consider a case in which a site is within 0.25km of the centroid of block k. The site is listed on the NPL in 1990, but is deleted before 2000. The variable measuring the change in the number of sites that are deleted from the NPL lying within 0.25km from the centroid of the block takes the value 1, while the variable indicating the change in the number of listed sites takes the value -1. The net effect on house values in this block from the site being deleted relative to when the site was in the pre-proposal stage is β_3.

4.4 House Analysis

In this part of the analysis, we turn our attention to repeat-sales household level data from Dataquick Information Systems. In particular, we use information on how Superfund remediation activity at nearby sites affects actual housing transaction prices. We begin our discussion of the house-level model by considering the following hedonic price function:

$$lnP_{i,j,t} = Z_{i,j,t}^{'}\beta + \xi_{j,t} + \mu_i + \varepsilon_{i,j,t} \qquad (3)$$

i indexes individual houses, j indexes census tracts, and t indexes times at which house i is sold. $lnP_{i,j,t}$ measures the natural log of the transaction price, $Z_{i,j,t}$ represents a vector of Superfund site exposure variables (in particular, the number of Superfund sites at various stages of remediation – discovered, proposed, listed, and deleted – within 3km of house i), $\xi_{j,t}$ is a scalar neighborhood unobservable that is allowed to vary over time,[27] and μ_i represents unobserved time-invariant attributes of house i. Similar to the census tract/block analysis, each coefficient in this specification measures the effect of exposure to a site of a particular status relative to the pre-discovery stage.

Consider a house i that sells at two points in time, A and B (in this analysis, we restrict our attention to houses that sell more than once).[28] Differencing the hedonic price function across repeat sales:

$$(4) \qquad \underbrace{lnP_{i,j,B} - lnP_{i,j,A}}_{ln\tilde{P}_{i,j,BA}} = \underbrace{(Z_{i,j,B} - Z_{i,j,A})}_{\tilde{Z}_{i,j,BA}}{}'\beta + \underbrace{(\xi_{j,B} - \xi_{j,A})}_{\tilde{\xi}_{j,BA}} + \underbrace{(\mu_i - \mu_i)}_{0} + \underbrace{(\varepsilon_{i,j,B} - \varepsilon_{i,j,A})}_{\tilde{\varepsilon}_{i,j,BA}}$$

While this procedure eliminates μ_i, we are still left with $\tilde{\xi}_{j,BA}$ (i.e., the change in neighborhood unobservable between periods A and B). Therefore, we proceed by taking the average of each "~" variable across all houses in j that sold in years A and B (denoted by the set Ω_j^{BA}):

$$(5) \quad \overline{lnP}_{j,BA} = \frac{1}{N_j^{BA}} \sum_{i \in \Omega_j^{BA}} ln\tilde{P}_{i,j,BA} \qquad \bar{Z}_{j,BA} = \frac{1}{N_j^{BA}} \sum_{i \in \Omega_j^{BA}} \tilde{Z}_{i,j,BA} \qquad \bar{\varepsilon}_{j,BA} = \frac{1}{N_j^{BA}} \sum_{i \in \Omega_j^{BA}} \tilde{\varepsilon}_{i,j,BA}$$

Differencing-out these average values:

$$(6) \qquad \underbrace{ln\tilde{P}_{i,j,BA} - \overline{lnP}_{j,BA}}_{ln\hat{P}_{i,j,BA}} = \underbrace{(\tilde{Z}_{i,j,BA} - \bar{Z}_{j,BA})}_{\hat{Z}_{i,j,BA}}{}'\beta + \underbrace{(\tilde{\xi}_{j,BA} - \tilde{\xi}_{j,BA})}_{0} + \underbrace{(\tilde{\varepsilon}_{i,j,BA} - \bar{\varepsilon}_{j,BA})}_{\hat{\varepsilon}_{i,j,BA}}$$

$$(7) \qquad ln\hat{P}_{i,j,BA} = \hat{Z}_{i,j,BA}{}'\beta + \hat{\varepsilon}_{i,j,BA}$$

OLS estimation of equation (7) controls for time-invariant unobserved heterogeneity at the house level, and time-varying unobserved heterogeneity at the tract level. In other specifications

[27] In our analysis, we treat census tracts as neighborhoods, fitting with the definition of a tract used by the Census Bureau.

[28] In our analysis, we pool multiple price differences for houses that sell more than twice in the same regression and cluster standard errors at the level of the house. Table 5 reports summary statistics for the set of houses that sell once along with the set of houses that sell multiple times. In general, the set of repeat-sales houses look quite similar in most dimensions, although multi-sale houses do tend to be smaller and less expensive. See Gatzlaff and Haurin (1997) for a discussion of sample selection bias in repeat-sales models.

designed to demonstrate the importance of these controls, we estimate the model using (i) only house fixed effects, (ii) only tract-by-year fixed effects, or (iii) no fixed effects at all. In (i) and (iii) we do include a set of year dummies to control for market-wide appreciation.

5 Data

5.1 Data Sources

Census tract data are from Geolytics Neighborhood Change Database, which has reapportioned census data from 1980, 1990 and 2000 into census tract boundaries that are fixed in 2000. Restricted-access census block data for 1990 and 2000 are from the US Census Bureau and were analyzed at the Triangle Census Research Data Center. Housing transactions data are from Dataquick Information Systems, used under a licensing agreement with Duke University Department of Economics. The EPA provided detailed data on all sites ever proposed to the National Priority List. The HRS 1982 scores that we use to construct our RD and strict RD sample of sites (described in section 5.4) are from the dataset compiled and published by Greenstone and Gallagher (2008). We also use their dataset to construct the "intersection sample" which we use to provide a comparison of our study with theirs (see Appendix A3). The Consumer Price Index used to deflate housing prices is compiled by the Bureau of Labor Statistics and is based upon a 1982 Base of 100.[29]

5.2 Construction of Compatible Tract and Block Samples

To construct our sample of tracts, we use shapefiles from Geolytics to identify all tracts that fall within 3km buffers of NPL sites. To identify the block sample, we begin with the Geolytics tract identifiers from the tract sample. First, using these Geolytic tract identifiers, we find the corresponding tracts in the restricted access data. The restricted data uses ESRI shapefiles to create the crosswalks between the 1990 and 2000 data. The Geolytics and ESRI shapefiles, while similar, are not identical. In particular, several tract identifiers exist in the Geolytics tract data but not in the ESRI-based restricted access data. Second, after identifying the relevant tracts in the restricted access data, we identify the corresponding blocks within those tracts. Third, we calculate the distance between the centroids of these blocks and the NPL sites. We then restrict our block sample to include only those blocks whose centroids fall within 5km of the nearest NPL site. Doing so provides us with a natural treatment group (i.e., those blocks falling within a 3km buffer around each site) and a corresponding set of control blocks (i.e., those blocks lying 3-5km from each site). This restriction leads to several blocks being dropped

[29] http://inflationdata.com/inflation/Consumer_Price_Index/HistoricalCPI.aspx?rsCPI_currentPage=1

due to their large sizes. Third, we compare our tracts and block samples, and find substantial, though not complete overlap between the two. To summarize, the first difference stems from the differences in the Geolytics and ESRI shapefiles. The second difference stems from dropping blocks whose centroids lay > 5km from the nearest site.

We end up with two tract samples – (i) the initial sample, which includes all tracts that fall within 3km buffers around the NPL sites; and (ii) the final sample, which drops tracts that are not represented by any corresponding blocks in the restricted access block sample.[30] Our report, which aims to compare tract and block results, provides regression results from this second tract sample. Regression results based on the first tract sample, available from the authors, are similar to those using the second tract sample.

We repeat our analysis using a 5km sample. To construct that sample of tracts, we identify all tracts that fall within 5km buffers of NPL sites. The corresponding construction of the block sample drops blocks whose centroids lie more than 7km from the nearest NPL sites.

5.3 Construction of the House Sample

The housing transactions dataset from Dataquick contains many observable characteristics for each house (e.g. number of bedrooms, square footage, etc.) as well as the transaction price, loan amount, transaction date, latitude and longitude coordinates and the year 2000 census tract identifier. Each property is uniquely identified in the data, which allows the creation of a panel data set at the house-level. In an effort to remove outliers, houses observed in the top and bottom 1% of the price and square footage distribution are dropped, as well as the top 1% of the distributions of the number of bedrooms and the number of bathrooms. Houses with missing attribute or location data are also dropped from the dataset.

An unfortunate feature of the transactions data is that Dataquick overwrites the characteristics recorded for a given property in previous transactions if a newer transaction is recorded with different and presumably updated information. However, in certain circumstances, if the renovation is on the scale of a large addition or major construction, the transaction will be flagged as having such an improvement. The implication for panel analysis is an inability to observe some changes to properties, since any moderate change made to the property is retroactively applied to all records in the data. As a result, all observable characteristics will drop out of any repeat sales analysis. To combat the presence of homes that likely have changed in substantial ways, homes that are observed to appreciate (depreciate) more than 50% on an annualized basis, have the major construction data flag, transact with a loan

[30] To be specific, only few tracts are dropped. The strict RD sample contains 830 tracts that represented in both the block and final tract samples, after dropping 19 tracts that were not represented in the block sample. The corresponding figure for the RD sample is 1,482 tracts, after dropping 19 tracts that were not represented in the block sample. The corresponding figure for the All-NPL sample is 10,062 tracts, after dropping 133 tracts.

amount greater than the transaction price by $5,000, or are observed to transact twice or more in any 12 month span are dropped from the sample. This procedure, of course, runs the risk of dropping any houses that appreciated precisely because of improvements made in response to Superfund site remediation. This is, therefore, another way in which our valuation exercise is conservative.

Even with house fixed effects, concern still remains that properties with unobserved changes are resident in the dataset. If changes in properties are not correlated in any way with Superfund site exposure, then unobserved property improvements should not bias any results. However, in a repeat-sales model, the price effects of proximity to Superfund sites are identified by changes in site status. If, for example, there is a correlation between home improvement and Superfund site remediation, the estimated price effects of hazardous waste cleanup will be biased upwards as it will be impossible to distinguish between those paying for improved environmental quality and those paying for improved housing. Mastromonaco (2010) checks for this possibility, but finds no evidence that changes in Superfund site status (proposed, listed, deleted) affects the likelihood of an improvement.

We use data from four housing markets – northern New Jersey, the Los Angeles Metropolitan Area, the Boston Metropolitan Area, and southwestern Connecticut. They were chosen because Dataquick began collecting information in these housing markets prior to 1990. Because much of the activity associated with the Superfund program occurred during the 1990's, having coverage back to this point is important for a successful analysis.

5.4 Construction of the Sample of Sites: All-NPL, RD and Strict RD

We begin with all sites that have been ever proposed to the NPL (before Jan 1, 2010) in the contiguous US. Of these, we excluded 10 sites that had been proposed to the NPL but whose date of proposal was not recorded.[31] Our all-NPL sample contains tracts surrounding 1,722 ever-proposed sites. Our RD sample contains tracts surrounding 221 sites scored in 1982 whose HRS scores are between 16.5 and 40.5. Our strict RD sample contains tracts surrounding 187 sites from the RD sample that are not proximate to any other sites not contained in that sample. The progress of these sites through the Superfund milestones is shown in the flowcharts in Figures 1-3.[32]

[31] The only information we have on these 10 sites are they were eventually recorded as non-NPL sites.
[32] All but two sites had EPA verified geo-coordinates; for those two sites (NJN000206276, NYN000206282), the EPA provided two sets of coordinates for each site were identical.

6 Results

6.1 Summary Statistics

Tables 3 (a) and (b) summarize the tract data. In particular, they report means and standard deviations of tract attributes for the set of tracts surrounding the sites that we use in our RD and strict RD samples. Table 4 reports summary statistics for the 323,682 census blocks that correspond to the all-NPL sample. Table 5 reports sample sizes and average prices for the full sample of housing transactions from four separate housing markets – the Boston and Los Angeles metropolitan areas, northern New Jersey, and southwestern Connecticut.

6.2 Tract Analysis

Deletion of sites from the NPL – Main Results

Our preferred estimates are from the panel analysis of tracts that are in the strict RD sample. Recall that this sample and specification are designed to deal with several potential confounders. The panel specification controls for time-invariant unobservables that are correlated with sites' statuses. The RD sample controls for unobservables (time-invariant or time-varying) that are correlated with sites' listing; the strict RD sample additionally controls for heterogeneity associated with exposure to sites other than the 1982 sites that have narrow HRS scores. Observations are weighted by tract counts of owner-occupied housing units, and robust standard errors account for heterosekedacity.

Table 6 shows results from the panel specification for the strict RD sample. The results indicate that the deletion of a site from the NPL raises nearby housing values, but the appreciation, in percentage terms, is more prominent at the lower deciles of the within-tract housing value distribution. As seen in panel A, carrying a site through the remediation process to deletion raises house values by between 17.4% and 20.0% between the 10^{th} and 40^{th} percentiles, and by 15.4% at the median and 11.4% at the 60th percentile. All of these effects are statistically and economically significant. The effects of deletion are not statistically significant above the 60th percentile of the housing value distribution. The higher percentage appreciation among the lower deciles of the housing value distribution is partly due to the lower absolute value of these houses. Panel B presents the results using housing value levels as the dependent variable. The appreciation amounts to $5,980 at the 10th percentile, $9,530 at the 30th percentile, and $8,796 at the median. Deletion does not lead to a statistically significant appreciation in housing values at the upper deciles of the within-tract distribution, though the point estimate at the 70^{th} percentile is significant at the 10% level.

Table 7 shows results from the panel specification for the RD sample. The results mirror those from the strict RD sample except the magnitudes of deletion effects are even greater.

Carrying a site through the remediation process to deletion from the NPL raises nearby housing values, and the appreciation, in percentage terms, is more prominent at the lower deciles of the within-tract housing value distribution. The appreciation amounts to 30.0% at the 10th percentile, 21.1% at the median, and 15.1% at the 90th percentile. Similar to the strict RD sample, appreciation measured in levels in the RD sample is also concentrated at the lower percentiles. The appreciation amounts to $9,924 at the 10th percentile and $8,622 at the 80th percentile.

Deletion of sites from the NPL – Other Specifications

We examine a variety of alternative specifications for the strict RD sample and RD sample. First, we estimate an unweighted specification for the strict RD sample and the RD sample. Results from those regressions, presented in Table 8(a) columns 1-3 and 4-6 are comparable to those from the weighted regressions. For the strict RD sample, we continue to find that carrying a site through the remediation process to deletion raises housing values at the lower deciles. In particular, the unweighted regressions indicate that house values appreciate by 20.8% at the 20th percentile and 16.8% at the median. For the RD sample, we find that deletion raises housing values by 33.1% at the 20th percentile, 23.9% at the median, and 24.8% at the 80th percentile.

Second, to address the possibility that the errors are spatially correlated, we calculate clustered standard errors. Ideally, we should cluster the standard errors over groups of tracts that lie in close proximity to one another, as these errors are likely to be spatially correlated. However, because the next available level of geographical identifier is the county, we take the practical step of estimating standard errors clustered at the county level. The drawback of this approach is that clustering on too aggregate a geographical region will lead to overly large standard error estimates. Table 8(a) presents standard errors clustered at the county level, estimated for the strict RD sample (columns 7-9) and the RD sample (columns 10-12). The estimates for the RD sample are statistically significant, but the estimates for the smaller strict RD sample are no longer statistically significant with clustering.

Third, to explore the spatial extent of the effects of deletion, we repeat our analysis using 5km buffers when creating the strict RD and RD samples. Table 8(b) columns 1-3 reveals that with the larger definition of neighborhoods near NPL sites, deletion no longer yields statistically significant estimates of appreciation in housing values in the strict RD sample. Comparison of these results with our earlier results from Table 6, where neighborhoods are defined narrowly using 3km buffers, suggests that the larger neighborhood definition lumps nearby affected houses with unaffected houses, thereby diluting the effects of deletion. For the RD sample, these larger definitions of neighborhoods near NPL sites still yield results indicating that deletion leads to statistically significant appreciation in housing values (Table 8(b), columns 4-6), but these point estimates are less than half the magnitude of those measured with the narrower 3km definition of neighborhoods in Table 7.

Finally, for comparison, we show results from the all-NPL sample in Table 8(c) (with and without weights). In contrast to the RD and strict RD samples, the all-NPL sample does not control for unobserved heterogeneity between treated and untreated tracts. We find estimates that are much smaller in magnitude in the all-NPL sample (i.e., 3.1% at the median and 3.4% at the 80th percentile in the weighted regression and 3.1% at the 80th percentile in the unweighted regression).

Listing of a site to the NPL

Considering the estimates recovered from the tract strict RD sample in Table 6, we find that listing does not have a statistically significant positive effect on housing prices. Using the RD sample in Table 7, we find statistically significant effects in the lower deciles. There, when a tract goes from zero to full exposure, housing values appreciate by 15.1% at the 10th percentile, 10.4% at the 30th percentile, and 7.5% at the median. The block analysis of the strict RD sample finds smaller magnitudes of appreciation for listing than deletion, particularly within smaller distances from the sites. As described in Table 10, reaching final listing status results in statistically significant appreciation, ranging from 13.3% to 5.2% in blocks lying < 1km and < 3km from the site. In the block analysis using the RD sample, described in Table 11, the effect is approximately 4% in blocks lying < 1km and < 3km from the site.

The smaller magnitude of appreciation from listing compared to deletion can be explained by the countervailing pressures on housing values that arise when a site is listed – final listing reduces housing values by confirming the severe nature contamination of that site, but it also increases housing values by signaling that the site will be remediated.[33]

Proposal of a site to the NPL

Taken as a whole, our results indicate that proposal to the NPL results in the depreciation of nearby housing values. The effect of proposal measured using the all-NPL sample ranges from -7.2% to -8.9%, as seen in Table 8(c). The RD tract analysis, seen in Table 7, indicates that proposal results in depreciation in housing values ranging from -\$5,355 at the median to -\$11,648 at the 90th percentile. Estimates in percentage terms are not statistically significant.

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[33] Note that, according to how we define Superfund remediation milestones, sites having achieved the "construction complete" designation (without having been deleted from the NPL) will be coded as being in the listing stage. The positive effects of listing that we find in the RD sample could be indicative of value that the housing market places on the construction complete designation.

Although the results in the strict RD samples suggest a much larger depreciation in response to proposal on the NPL, we treat these results with caution. Only four sites (affecting 19 tracts and blocks therein), whose proposal status changed during the 1990's, drive these results; therefore, the results may not extend to other sites and tracts. The depreciation in the strict RD tract sample ranges from -39.6% to -44.7% (see Table 6). Note that this problem of a few sites driving the results does not affect the estimates pertaining to deletion and listing in the strict RD sample because there exists fairly sizable variation in the exposure to listed and deleted sites between 1990 and 2000 (as seen in Figure 1). Similarly, this issue does not arise in RD sample (for proposal, listing or deletion).

Depreciation in nearby housing values in response to the proposal of a site to the NPL can be explained by two channels. First, the proposal of the site may have provided new information to the market about the presence or severity of a harmful site. Second, even if the market is already aware of the site and the extent of contamination, the proposal of site to the NPL may further decrease housing values by stigmatizing the neighborhood (Messer et al, 2006). Nevertheless, the proposal-induced depreciation is more than offset at the lower end of the within-tract housing value distribution by the time the sites are deleted. As described earlier, the estimated coefficients on deletion, which measure the effect of deletion on housing values relative to values at the pre-proposal stage, indicate that the Superfund remediation process, taken in its entirety, lead to an overall appreciation in housing values.

The revelation of new information to the market by the proposal of the site to the NPL and the subsequent depreciation in housing values is not a cost that is appropriately attributed to the Superfund program. In contrast, depreciation resulting from stigmatization of the neighborhood following the proposal of the site to the NPL can be viewed as a cost of the program. Messer et al (2006) suggest ways in which the Superfund program can reduce the stigmatization of neighborhoods, such as by expediting the remediation process.

Estimation Issue #1: Time varying observables and deletion status

The estimation issue in identifying the effect of deletion is that the ever-proposed sites that are eventually deleted in our study period may differ systematically from ever-proposed sites that do not reach this milestone in our study period. While correlated unobservables are certainly a legitimate concern, we find evidence in the literature that suggests they may not be a serious problem in the current context. Sigman's (2001) study of the pace of progress at Superfund sites suggests that the impact of correlated unobservables on our estimate of deletion is likely to be limited. In particular, our panel approach addresses variables (observed or unobserved) that are time invariant. The following variables are modeled as time-invariant in Sigman (2001) – socioeconomic characteristics, voter turnout, the technical complexity of the cleanup, and the presence of potentially liable parties (PRPs) responsible for the cleanup. Our panel approach controls for time-varying observables (listed in Table 3(b)), but any time-varying unobservables could potentially create a problem for our estimation strategy. Factors modeled as time-varying in Sigman's study are, however, found to have little influence on sites' progress. Public funding does not influence the progress from listing to the Record of Decision (ROD), and legislative influence does not affect the sites' progress from listing through ROD to construction complete. While public funding does influence the pace of progress from listing to ROD, Sigman (2001) notes that most funding for cleanup at this stage comes from PRPs under their agreement with the EPA.

Estimation Issue # 2: Variation in extent of remediation

A related issue is whether sites that receive the cleanup treatment are likely to have received systematically more intensive cleanup than comparison sites that have yet to receive cleanup. If this concern is valid, then our estimates are larger on average than those that would be realized by the cleanup of sites in general.

Our estimation strategy addresses this issue by using fixed effects to control for time-invariant unobservables, and by comparing sites that are as similar in terms of their HRS scores (which reduces the possibility for variation in the extent of cleanup to arise from time-varying unobservables). Moreover, previous studies suggest that the extent of cleanup does not vary systematically based on neighborhood characteristics. The EPA did not choose less permanent cleanup options for sites with lower median household income or with greater shares of non-white residents at the zipcode level (Gupta et al., 1996). The expenditure to avert an average cancer case in NPL sites was not influenced by mean income or minority population within a 1-mile ring of NPL sites (Hamilton and Viscusi, 1999). Among the less hazardous sites, however, variation can arise from constituents' political activity (Hamilton and Viscusi, 1999).

Hamilton and Viscusi (1999) note that although EPA's directive set a baseline of cleanup standards, cleanup is set at more stringent levels in states with stricter standards.[34] However, the state-level source of variation in the extent of cleanup would only bias our study if we were to systematically compare cleanup in sites located in more stringent states with sites yet to be cleaned that are located in less stringent states.

[34] Viscusi and Hamilton (1999) provide details on this point. The 1991 EPA directive set a baseline of cleanup standards – the cumulative carcinogenic site risk to an individual based on reasonable exposure for both current and future land use is less than 10^{-4} and the non-carcinogenic hazard quotient is less than one – but in practice the cleanup goal is more stringent. In turn, variation in environmental cleanup targets can arise from state-level variation in environmental standards. The 1986 Congress directed that remedial actions must meet federal standards that are "applicable, relevant or appropriate" requirements (ARARs), and these actions must generally meet state ARARs, if stricter than federal ones.

Estimation Issue #3: Defining exposure to overlapping sites

Consider a tract that overlaps with the 3km buffers surrounding two NPL sites with the same remediation status at a given point in time. Suppose that the tract contains three distinct areas of overlap: (a) overlap with the buffer of the first NPL site, (b) overlap with the buffer of the second NPL site, and (c) overlap with the area of intersection between the buffers of the first and second NPL sites. We calculate the tract's exposure as the sum of areas (a), (b) and (c). In other words, we do not double-count the area of intersecting overlap (c). This approach limits the maximum exposure for a tract to sites at any particular stage of remediation to be 1. The drawback of this approach is that it does not account for the possibility that simultaneous exposure to two deleted sites may have different implications for housing value appreciation compared with exposure to one deleted site – it simply controls for share of a tract's area that is exposed to *any* deleted site. The alternative of simply adding together areas of intersection between buffers of multiple NPL sites (so that the exposure variable could exceed 1) is, however, equally arbitrary. One might suspect that the marginal impact of exposure to additional sites lies somewhere between these two extremes, declining with additional sites.

This potential problem turns out to be not much of a problem at all in our preferred sample (i.e., strict RD). In particular, we examine the extent to which tracts in the strict RD sample contain areas of overlap with intersecting buffers of two or more sites at the same stage of remediation at the same point in time. As reported in Table A2.1, which can be found in Appendix A2, this happens in very few tracts. Specifically, in only 5 instances out of 1,666 tract x year combinations (i.e., 833 tracts observed in 2 years) do we find overlap with intersecting buffers of deleted sites, and in only 12 instances do we find overlap with intersecting buffers of listed sites.

Supplementary Analysis: Spatial distribution of Superfund sites in relation to cheaper houses within the tract

We find that the appreciation of housing values in response to site listing and deletion is concentrated in the lower deciles of the within-tract distribution of housing values (particularly when we consider our RD and strict RD samples). These results make sense if NPL sites are located closer to the lower-value houses within each tract. We directly examine whether this is the case using transactions data from the four housing markets described in Table 5. We first make housing transactions comparable over time by differencing from each the mean price amongst all houses that transacted in the particular housing market in the same year. Within each tract, we then calculate the percentiles of the distribution of demeaned prices (pooled over time). We next determine the number of Superfund sites (across all stages of remediation) within 1km of each house. Binning houses into within-tract deciles, we report the average exposure of houses in each decile to sites at that distance. To facilitate the comparison across different markets (which have different total numbers of sites), we normalize each exposure variable by the average exposure to sites within that market (i.e., the line describing exposure in each market is centered around one).

The results are summarized in Figure 4. In three out of the four markets, there is stark evidence that houses in the lowest deciles of the within-tract price distribution are more likely to be exposed to NPL sites than are houses in the higher deciles. For metro LA, houses in the first decile are 9.8% more likely than the average house in that market to be exposed to a site, while houses in the second decile are 5.7% more likely. Houses in the ninth decile are 2.1% less likely than the average to be exposed at this distance. For Boston and southwestern Connecticut the pattern is even more severe. Houses in the first decile in Boston are 29.1% more likely to be exposed to an NPL site, while the comparable number for southwestern Connecticut is 37.6%. At the second decile, the numbers are 21.6% and 27.7%, respectively. Houses in Boston in the ninth decile are 25.8% less likely than the average to be exposed at this distance, while the comparable number for southwestern Connecticut is 20%. Of the four markets we consider, the pattern is weakest in northern New Jersey. There, the highest likelihood of exposure comes at the 2nd decile (5.5% more likely) while the lowest comes at the 6th, 8th, and 9th deciles, which are all 4.7% less likely than the average to be exposed. These results provide strong confirmation that the patterns we recover in the tract-level analysis are being driven by the heterogeneous exposure across the within-tract distribution that we postulate.

6.3 Block Analysis

Deletion of a site from the NPL

We directly investigate localized externalities from Superfund cleanup by examining median housing values at a very fine level of geographic precision – the census block. Analogous to our tract analyses, the panel estimates from the strict RD sample are our preferred estimates, since that sample and specification address several potential confounders. Results from the block analysis confirm our earlier results from the tract analysis. As seen in Table 10, carrying a site through the remediation process to deletion from the NPL results in statistically significant appreciation of median house values and the appreciation is larger in blocks located closer to the site. Median housing values appreciate on average by 19.9%, 19.0% and 5.8% in blocks lying < 0.5km, < 1km, and < 3km from the site. Median housing values in blocks lying < 0.25 km do not show statistically significant appreciation, most likely because few houses are located within these short distances of Superfund sites, leading to imprecise estimates. Considering the distance bands (all of which are included in each regression specification), we find a similar pattern of greater appreciation in blocks closer to the site. Carrying a site through the remediation process to deletion raises median house values by 19.4% in blocks lying 0-1 km from the site, and by only 8.4% in blocks lying 2-3km from the site. The results for the strict

RD, which is likely to contain only a few houses within each block, unsurprisingly, are heavily influenced by outliers. For example, the point estimate for the 1-2km distance band is negative though statistically insignificant. Deletion raises median house values by 20.7% in blocks lying 3-5km from the site, possibly suggesting preferential sorting whereby those who choose to live some distance from the site (3-5km) have greater willingness-to-pay for remediation. Noteworthy, the magnitude of the appreciation for the median house value at blocks lying < 1km away from the site (19.0%) is comparable with that for the lower deciles of the house value distribution at the tract-level (17.4%-20.0% at the 10^{th} to the 40^{th} percentile). In addition, the cross-sectional estimates from the strict RD sample (not shown) indicate that reaching deletion causes depreciation of house values in blocks lying near the site; this highlights the importance of differencing between 2000 and 1990 in order to remove correlated time invariant unobservables.

Next we compare our preferred estimates from the strict RD sample with those from the RD and all-NPL samples. While the strict RD sample has the advantage of narrowing the unobservables, the larger RD and all-NPL samples have the advantage of larger sample sizes and thus their results are less prone to outliers. Results from the RD sample, presented in Table 11, indicate a smaller magnitude of appreciation than those from the strict RD sample. In particular, carrying a site through the remediation process to deletion from the NPL is associated with only 7.1% and 5.8% appreciation in median house values in blocks lying < 1km and < 3km from the site. Similarly, reaching deletion is associated with appreciation of only 7.8%, 11.2%, 2.9% and 2.9% in blocks lying 0-1km, 1-2km, 2-3km and 3-5km, respectively.[35] Results from the RD sample indicate that the magnitude of house value appreciation from reaching deletion declines as one moves further from the site. Results from the all-NPL sample in Table 12 are similar in magnitude to those from the RD sample, ranging from 8.1% to 6.5% for blocks that lie < 0.5km to < 3km from the site.

As was the case in the tract-level analysis, results using block-level data and standard errors clustered at the tract-level are comparable to those described in Tables 10-12. The comparison of the 3km and 5km block results are similar to our earlier comparison of the 3km and 5km tract results. Block results using the 5km strict RD sample are not statistically significant, while estimates from the 5km RD and All-NPL samples are about half the magnitude of the corresponding estimates from the 3km samples. These results underscore the *localized* nature of the benefits from Superfund cleanup. These results are available on request from the authors.

[35] Deletion causes smaller appreciation in median house values in blocks lying closer to the site (0-1km) than in blocks lying slightly further away (1-2km). These results suggest preferential sorting whereby those who choose to live closest to the site have a lower willingness-to-pay for remediation. These persons may choose to locate close to the site because they work on the sites.

Proposal to and listing on the NPL

For the strict RD sample, proposal to the NPL again results in large and statistically significant depreciation of median house values ranging from -24.7% to -37.3% in blocks lying <1km and < 3km from the site. As was the case in the tract analysis, this result is driven by blocks in 19 tracts (described in Table 9) that were exposed to four sites in the strict RD sample that changed proposal status during the 1990's. As was the case in the tract analysis, this issue does not affect any of the other strict RD block-level results.

The estimates from the RD and all-NPL samples yield mixed results with respect to site proposal. Results based on the RD sample suggest that proposal has small positive effects on housing values, while the all-NPL sample yields small negative effects except at the farthest distance.

In contrast to our earlier strict RD tract results (which found no effect), reaching listing status results in statistically significant appreciation using the panel specification and block-level data. This effect ranges from 13.3% to 5.2% in blocks lying < 1km and < 3km from the site. In this case, either (i) the promise of cleanup associated with final listing appears to outweigh the effect of confirming a site's contamination level, or (ii) the inclusion of construction complete in with the listing status leads to appreciation.

6.4 House Analysis

Using repeat-sales transactions data from Dataquick Information Services, we are able to model the effect of exposure to NPL sites at various stages of the remediation process at a very fine level of geographic and temporal resolution. In particular, seeing individual houses transact on multiple occasions allows us to control for all time-invariant unobservable house attributes with house fixed effects. Seeing houses at a fine level of geographic precision (i.e., latitude and longitude coordinates) allows us to also control for time-varying neighborhood unobservables using census tract fixed effects. Controlling for both of these forms of heterogeneity turns out to be important for our results.

We report results for each of the four housing markets discussed in Table 5 – the Los Angeles and Boston metropolitan areas, northern New Jersey, and southwestern Connecticut. In particular, Tables 13 (a) and (b) report results for each market using site counts within a 3km radius of each house. For each market, columns (1) - (4) report results from different specifications that are designed to illustrate the importance of each fixed effect control. Our preferred estimates are presented in column (1) which includes both house fixed effects and tract-by-year fixed effects, as specified in equations (7) - (11). Column (2) drops the house fixed effects, but maintains the tract-by-year fixed effects. This specification will control for neighborhood unobservables that may co-vary with Superfund exposure but will not control for within-tract house-level unobservable heterogeneity. The opposite is true of the specification in column (3), which controls for house fixed effects but omits tract-by-year fixed effects. The specification in column (4) does not include either set of fixed effects. Note that, in specifications (3) and (4), we do include a set of year fixed effects to control for market-level price movements; in specifications (1) and (2), the tract-by-year fixed effects serve this role.

Focusing first on the results in Table 13(a), we find evidence of benefits from Superfund cleanup in both northern New Jersey and Los Angeles metro. In describing the benefits from Superfund remediation in our house-level analysis, we focus on the changes in housing values at deletion relative to the proposal stage. We do not focus on the value of deletion relative to pre-discovery in order to avoid incorporating housing value appreciation at the discovery stage that may be caused by other factors. Specifically, in New Jersey and Los Angeles, moving from pre-discovery to the discovery stage, houses within 3km of a site sell for 15.8% and 3.2% more, respectively, after that site is discovered. We do not interpret this appreciation in housing values as a causal effect of the discovery process. Instead, we suspect that this pattern could arise when sites are discovered because an area is experiencing new construction. That new construction would cause both the appreciation and contribute at the same time to discovery of the contaminated sites.

Overall, deletion relative to the proposal stage raises housing values in northern New Jersey and metro Los Angeles, by 11.3% and 1% respectively. Breaking these values into their component parts, we see that in northern New Jersey, listing (measured relative to proposal) causes a large increase – i.e., 12.8% in housing values. Deletion (measured relative to listing) results in an additional 1% increase in house prices, suggesting that most of the benefits of cleanup have already been capitalized at the listing stage, reflecting either market expectations about cleanup or the role of the construction complete designation. In the Los Angeles Metropolitan Area, we find smaller magnitudes of appreciation; listing relative to proposal raising housing values by 0.7% and deletion relative to listing raises housing values by only 0.3%.

We note that, housing values after deletion are higher than those at the pre-discovery stage (29% in New Jersey and 5.1% in Los Angeles), but these entire values should not be interpreted as the benefits from the Superfund program. As described, these values incorporate some housing value appreciation that is due to confounding events, such as re-development, at the discovery stage.

Next we consider results in Table 13(b) for Boston and Southwestern Connecticut which exhibit a different pattern of housing prices in response to Superfund milestones described earlier for northern New Jersey and Los Angeles. In Boston, prices drop by 7.6% with discovery, suggesting the discovery stage provides negative information to the housing market about the presence of contamination. Proposal and listing have very little effect on housing prices, but moving from listing to deletion does lead to a 1.6% improvement in housing values (although that effect is not statistically significant). In contrast to New Jersey and Los Angeles, housing values in Boston after deletion are lower than those at the pre-discovery stage, suggesting stigma against contaminated sites and neighborhoods can persist despite cleanup. Finally, we turn to results from southwestern Connecticut where we fail to find evidence of significant effects at any stage of the cleanup process.

In order to demonstrate the role played by house and tract-by-year fixed effects in controlling for unobserved heterogeneity, consider the results in columns (2) - (4) of each table. We find that, without house fixed effects, the overall benefit of deletion decreases in Boston, Los Angeles, and New Jersey. This suggests that, in these markets, it is the lower priced houses within each census tract that receive Superfund treatment. This is consistent with the results of the tract analysis, where it was the houses in the lower tail of the within-tract value distribution that were disproportionately exposed to Superfund sites (and the remediation of those sites). Without house fixed effects, the unobservables that make those houses "low-value" get confounded with the remediation activity, biasing downward the value ascribed to it.

Conversely, taking out tract-by-year fixed effects, our estimates will be biased by the effect of changing unobserved neighborhood attributes. When the overall benefit of deletion increases as a result of taking out these fixed effects (e.g., as in Boston), it suggests that otherwise improving neighborhoods were the ones that received treatment. Indeed, failing to account for correlation with changing neighborhood attributes would lead one to grossly overstate the benefits of site deletion in Boston. The opposite is suggested, in terms of how treatment was allocated, where the overall effect of remediation instead gets worse when tract-by-year fixed effects are removed (e.g., as in New Jersey, Los Angeles, and Connecticut).

Without any fixed effect controls (neither house nor tract-by-year), results are highly variable but generally exhibit much less evidence of benefits from Superfund remediation. This simply confirms that, on average, Superfund sites are located nearer to lower-value homes (confirming one of the main conclusions of our census analysis) and cleanup activities tend to take place in otherwise declining neighborhoods.

In summary, while our national-level census results imply significant benefits from site remediation when considering the Superfund program as a whole, our house-level results reveal that these benefits vary considerably across housing markets. We find that deletion (measured relative to proposal) causes a sizable appreciation in housing values in northern New Jersey (11.3%), but we find no statistically significant effect of deletion (measured relative to proposal) for LA metro, southwestern Connecticut or metro Boston. While the appreciation seen in New Jersey indicates that some neighborhoods do recover post-cleanup, the lower prices in Boston at deletion relative to pre-discovery (-6.1%) suggest, conversely, that stigma against contaminated sites and neighborhoods can also persist despite cleanup.

7 Conclusions

Benefits from Superfund cleanup

Using three different but complementary approaches, our study finds that deletion of sites from the NPL (which signals the completion of the Superfund cleanup process) raises owner-occupied housing prices on average, but that there is considerable heterogeneity across specific housing markets. Our national-level tract analysis finds that deletion of a site, measured relative to the pre-proposal stage, significantly raises housing values – by 18.2% at the 10th percentile, 15.4% at the median, and 11.4% at the 60th percentile. This appreciation is concentrated at the lower deciles; no statistically significant appreciation is detected above the 60th percentile. Results at the block level indicate that the deletion of a site raises housing prices by 19.0% and 5.8% for blocks lying within 1km and 3km, respectively. Our tract and block results are remarkably similar – the 17.4% to 20.0% appreciation at the 10^{th} to the 40^{th} percentile of the within tract housing values is comparable to the 19.0% for blocks lying within 1km.

Our repeat sales analysis reveals considerable heterogeneity across metropolitan areas. We find that deletion (measured relative to proposal) causes a sizable appreciation in housing values in northern New Jersey (11.3%), but we find no statistically significant effect of deletion (measured relative to proposal) for Los Angeles, southwestern Connecticut or Boston. While the appreciation in New Jersey indicates that some neighborhoods do recover post-cleanup, the lower prices in Boston at deletion relative to pre-discovery (-6.1%) suggest, conversely, that stigma against contaminated sites and neighborhoods can also persist despite cleanup.

Our study and GG's study reinforce each other in one important way – taken together, they rule out the case that benefits from a cleanup, measured as capitalization into housing values, extend across a large area. Our finding that benefits are highly localized within the tract – relative to the case if benefits were diffused over a larger area – will make it more difficult for aggregate benefits to exceed the costs of cleanup. Whether these benefits exceed cleanup costs is, however, an empirical question to be addressed should good estimates of costs become available (GAO, 2010).

While our study focuses on the impact of deletion, we note that proposal to the NPL results in moderate depreciation in housing values in both the tract and block analyses when using the RD or all-NPL samples. Nevertheless, the depreciation associated with proposal is more than offset at the bottom of the within-tract housing value distribution by the time the sites are deleted. With regards to listing, the tract analysis finds no detectable effects in the strict RD sample, while the block analysis finds that housing prices appreciate by 13.3% and 5.2% in blocks lying < 1km and < 3km from the site. At the block level, the positive effect of final listing in signaling that the site will be remediated appears to trump the negative effect of confirming the severity of the contamination of that site.

How can the estimates from our study inform the evaluation of the Superfund program?

Our two estimates – (i) national-level estimates from the tract and block analyses and (ii) metropolitan area specific estimates from the house transaction analyses – provide complementary information for the evaluation of the Superfund program. The national-level estimate is useful for a benefit-cost analysis of the entire Superfund program. In particular, these estimates indicate that the gross benefit of the Superfund program is positive; this gross benefit could be compared with program costs in an efficiency analysis. The national-level estimate also provides the best available information with which to perform a cost-benefit analysis of a particular candidate site when estimates are unavailable for the specific housing market containing that site. Conversely, estimates that assess the remediation of multiple sites in a particular housing market are useful when performing a cost-benefit analysis of a specific site within that market. These estimates capture features specific to that particular housing market, while avoiding biases associated with estimates based on a single site. Estimates based on a particular housing market, of course, would not be applicable to sites outside that market.

Methodological issues in estimating Superfund cleanup with census tract data

Our analysis highlights a methodological issue that arises in assessing the impact of Superfund site remediation on housing prices using the Decennial Census data, given the highly localized nature of the externality in question. Most importantly, when using publicly available census data, the analyst must consider within-tract heterogeneity, paying particular attention to the lower deciles of the housing value distribution. Deletion from the NPL causes greater appreciation of housing values at the lower deciles (in percentage terms) in part because NPL sites tend to be situated near low-value houses within tracts. We show this explicitly for three out of four markets using geo-coded housing transaction data. Directly addressing the question of within-tract heterogeneity by using high-resolution census block data yields similar results.

Nevertheless, our proposed method has its limits. Not all localized benefits detectable with finer resolution block- and house-level data may be found by an analysis of the tract-level housing price distribution. However, given the reality that many hedonic studies are forced rely on tract-level data (Hanna, 2007; Greenstone and Gallagher, 2008; Grainger, 2010), the advantages of which include accessibility and nationwide coverage, our extension avoids an important source of bias that results from a narrow focus on the mean or median tract-level housing values.

Caveat: The role of non-NPL sites

We raise one important caveat with respect to our results. We have assumed that only the status of NPL sites influences house prices, and we have ignored changes in the status of other nearby hazardous waste sites that were not placed on the NPL. Proximate non-NPL sites may (or may not) have been cleaned-up by state or other federal agencies outside the Superfund program. At present, the EPA does not maintain a list of verified coordinates of non-NPL sites to use in creating controls for the confounding impact of these sites. While the EPA does catalogue information on sites that had been discovered but never proposed for listing on the NPL, the geo-coordinates for those sites have not been verified as reliable (in many cases, numerous different geo-coordinates are recorded for a single site), and there is no information about remedial actions that may have been taken by other state or federal agencies. Indeed, this data limitation has constrained other studies (Greenstone and Gallagher, 2008; Kiel and Williams, 2007; Noonan et al. 2007). It is difficult to sign the direction in which the presence of non-NPL sites might bias conclusions about NPL sites. We highlight this as an important area for future data collection efforts.

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Table 1 – Summary of the Tract, Block and House Analyses

	Tract	Block	House
Level	National level	National level	MSA-level
Goal	Estimate national level average benefits.	Estimate national level average benefits.	Estimate metro-specific average benefits from multiple sites.
Accessibility	*Publicly available*	Restricted access to Census Research Data Centers	Proprietary and expensive
Spatial Resolution	Census tract	Census block *Finer geographical resolution than tracts.*	Individual houses *Finer geographical resolution than tracts and blocks.*
Temporal Resolution	Decade (1990 & 2000)	Decade (1990 & 2000)	Annual *Finer temporal resolution.*
Prices	Stated values which are prone to misreporting	Stated values which are prone to misreporting	Sales value *Overcomes the misreporting of stated values.*
Housing market	Assumes national market	Assumes national market	MSA-specific markets
	National-level analysis are less prone to local (time-varying) unobservables.	*National-level analysis are less prone to local (time-varying) unobservables.*	MSA-level analysis are more prone to local (time-varying) unobservables.
	Less realistic definition of housing markets that buyers face.	Less realistic definition of housing markets that buyers face.	*More realistic definition of housing of housing markets that buyers face.*
			Detects the heterogeneity across markets.

Notes: Italics indicates strengths of the research design.

Table 2 – Interpretation of Coefficients

Change in treatment	Estimated effect
Not Proposed to Proposed	β_1
Not Proposed to Listed	β_2
Not Proposed to Deleted	β_3
Proposed to Listed	β_2-β_1
Proposed to Deleted	β_3-β_1
Listed to Deleted	β_3-β_2
No change in Status	Omitted case

Table 3(a): Summary Statistics, Census Tract Analysis RD and Strict RD Samples

Sample	Strict RD (n=881)				RD (n=1,454)			
Year	1990	1990	2000	2000	1990	1990	2000	2000
Variables	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Prices of owner-occupied housing at various percentiles								
10th	$38,785	$27,747	$41,105	$24,449	$42,814	$31,497	$44,529	$27,999
20th	$46,998	$32,631	$48,959	$28,141	$51,582	$36,826	$52,877	$33,058
30th	$53,404	$36,540	$55,357	$32,008	$58,324	$41,027	$59,447	$37,352
40th	$59,486	$40,688	$61,447	$35,772	$64,565	$44,960	$65,589	$41,124
50th	$65,579	$44,759	$67,754	$41,856	$70,772	$48,787	$72,087	$46,254
60th	$72,513	$50,210	$74,849	$46,628	$77,860	$53,919	$79,102	$50,947
70th	$80,846	$56,785	$83,339	$52,647	$85,959	$59,291	$87,588	$57,341
80th	$91,830	$64,733	$95,564	$62,736	$96,770	$66,070	$99,556	$67,070
90th	$110,989	$77,747	$117,900	$82,793	$115,183	$77,336	$120,877	$84,360
Exposure to sites on the National Priority List								
Proposal	0.01	0.08	0.00	0.02	0.05	0.19	0.01	0.09
Listed	0.35	0.37	0.24	0.33	0.45	0.39	0.39	0.40
Deletion	0.02	0.10	0.14	0.30	0.02	0.12	0.13	0.28

Table 3(b): Summary Statistics, Census Tract Analysis RD and Strict RD Samples

Sample	Strict RD (n=881)				RD (n=1,454)			
Year	1990	1990	2000	2000	1990	1990	2000	2000
Variables	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Covariates								
% unit occupied	91.4	7.2	91.8	6.8	92.2	6.7	92.6	6.6
% owner occupied	67.5	20.3	68.0	21.2	66.1	21.4	66.6	22.2
Housing unit density	0.0006	0.0009	0.0006	0.0009	0.0006	0.0010	0.0006	0.0010
Population density	0.001	0.002	0.001	0.002	0.001	0.002	0.002	0.002
% Black	11.4	22.6	13.3	23.7	11.8	22.6	13.8	23.3
% Hispanic	3.8	8.0	5.9	10.7	5.4	10.5	8.1	13.5
% under 18 years old	25.2	5.7	25.1	5.8	25.1	5.8	25.1	5.8
% high school dropout	25.2	12.4	19.5	11.4	25.3	12.8	19.8	11.8
% college educated	18.6	13.4	22.2	15.5	18.5	13.1	22.5	15.5
% below poverty	13.0	11.6	12.8	10.7	12.7	11.7	12.3	10.5
% public assistance	8.0	7.3	8.6	7.3	7.8	7.2	8.3	6.9
% female headed HH	23.9	16.2	25.8	15.9	23.7	16.0	25.5	15.5
Mean HH income ($1,000s)	$37	$16	$53	$23	$38	$16	$55	$23
% attached homes	7.2	17.3	7.7	17.2	7.3	16.6	8.0	16.9
% mobile homes	6.7	10.8	6.5	11.0	6.4	11.4	6.1	11.2
% 0-2 bedrooms	28.5	16.1	28.1	15.9	28.4	16.1	28.1	15.8
% 3-4 bedrooms	67.2	15.2	67.4	15.2	67.0	15.5	67.2	15.4
% units built within 5 years	8.6	10.8	7.5	9.7	7.9	10.4	7.1	9.2
% units built within 10 years	15.2	17.0	13.7	14.9	13.8	16.0	12.7	14.3
% same house in the last 5 years	56.0	12.9	56.7	12.6	56.0	13.2	56.4	12.7

Notes: Housing unit density and population density are in counts per m^2

Table 4 – Summary Statistics, Census Block Analysis (N=323,682)

	1990		2000	
	Mean	Std. Dev.	Mean	Std. Dev
% Unit Occupied	0.945	0.082	0.948	0.082
% Owner occupied	0.719	0.243	0.732	0.244
Housing Unit Density	1.182	2.150	1.298	3.181
Population Density	2.911	4.181	3.193	6.493
% Black	0.102	0.245	0.122	0.253
% Hispanic	0.039	0.112	0.114	0.212
% under 18 years old	0.245	0.099	0.246	0.102
% Female Head of HH	0.262	0.153	0.285	0.160
% living in the same house in the last 5 years	0.401	0.340	0.605	0.333
% 25 Years & High School Dropout	0.176	0.218	0.140	0.200
% 25 Years & BA Degree	0.137	0.191	0.168	0.222
% Below Poverty Line	0.100	0.209	0.099	0.200
% Public Assistance	0.030	0.087	0.013	0.050
Mean Household Income	55212	45361	60443	52018
% Attached Homes	0.058	0.164	0.061	0.191
% Mobile Homes	0.042	0.141	0.034	0.157
% 0-2 Bedrooms	0.356	0.356	0.344	0.358
% 3-4 Bedrooms	0.608	0.357	0.617	0.361
% Units Built within 5 years	0.063	0.180	0.040	0.152
% Units Built within 10 years	0.116	0.249	0.076	0.210
Median Value of Housing Prices	140531	115778	141817	129107
Counts of Proposed Sites within 2-5km	0.0003	0.018	0.0001	0.010
Counts of Proposed Sites within 5km	0.002	0.044	0.001	0.025
Counts of Proposed Sites within 1km	0.010	0.100	0.003	0.056
Counts of Proposed Sites within 3 km	0.082	0.307	0.028	0.165
Counts of Final Listing Sites within 2-5km	0.001	0.039	0.002	0.045
Counts of Final Listing Sites within 5km	0.008	0.090	0.011	0.104
Counts of Final Listing Sites within 1km	0.041	0.205	0.053	0.234
Counts of Final Listing within 3 km	0.423	0.662	0.513	0.728
Counts of Deleted Sites within 2-5km	0.00005	0.007	0.0004	0.019
Counts of Deleted Sites within 5km	0.0002	0.015	0.002	0.044
Counts of Deleted Sites within 1km	0.001	0.035	0.010	0.099
Counts of Deleted Sites within 3 km	0.011	0.106	0.096	0.311

Table 5 – Summary Statistics, House Level Analysis

	Northern New Jersey		Los Angeles Metro		Boston Metro		Southwestern Conn	
	One Sale	Multi Sale	One Sale	Multi Sale	One Sale	Multi Sale	One Sale	Multi Sale
Average Price	$277,405	$252,952	$272,855	$252,836	$255,598	$245,787	$258,274	$260,317
Number of houses by repeat sales								
2 sales		101,751		613,180		163,295		83,053
3 sales		21,169		274,527		60,407		30,134
4 sales		3,619		100.904		19,001		8,661
5 sales		504		30,737		4,788		1,831
Total Houses	471,267	127,043	1,211,770	1,019,348	410,865	247,491	188,436	123,679
Total observations in final sample	471,267	284,005	1,211,770	2,607,242	410,865	607,755	188,436	300,307

Table 6 – Panel Analysis (1990-2000) of the strict RD sample, i.e., all tracts within 3km buffer of the 221 NPL sites with 1982 HRS score in [16.5, 40.5] and not within 3km of any other ever proposed NPL sites (weighted by the number of owner-occupied housing units).

Panel A: Dependent variable Δ Log Price of owner-occupied housing units

Percentile	10	20	30	40	50	60	70	80	90
Δ Proposal	-0.432***	-0.396***	-0.412***	-0.405***	-0.420***	-0.447***	-0.441***	-0.403***	-0.417***
	(0.148)	(0.115)	(0.111)	(0.112)	(0.112)	(0.115)	(0.116)	(0.120)	(0.132)
Δ Listing	0.030	0.054	0.051	0.046	0.030	0.001	-0.006	-0.018	-0.065
	(0.078)	(0.052)	(0.051)	(0.052)	(0.050)	(0.052)	(0.052)	(0.054)	(0.068)
Δ Deletion	0.182*	0.200***	0.185***	0.174***	0.154**	0.114*	0.098	0.086	0.034
	(0.094)	(0.069)	(0.067)	(0.066)	(0.065)	(0.067)	(0.067)	(0.068)	(0.080)
R-sqr	0.307	0.360	0.347	0.366	0.365	0.364	0.369	0.375	0.332

Panel B: Dependent variable Δ Price Level of owner-occupied housing units

Percentile	10	20	30	40	50	60	70	80	90
Δ Proposal	-14,278**	-15,325***	-17,460***	-18,661***	-23,678***	-24,837***	-22,512**	-20,736*	-26,366*
	(5,712)	(5,778)	(6,152)	(6,785)	(8,184)	(9,001)	(10,270)	(11,350)	(13,669)
Δ Listing	2,034	3,546	3,595	3,478	2,270	939	1,374	27	-6,982
	(2,556)	(2,551)	(2,681)	(2,888)	(2,987)	(3,438)	(3,865)	(4,319)	(6,364)
Δ Deletion	5,980*	8,758**	9,530**	9,650**	8,796**	7,724	10,511*	10,605	3,456
	(3,408)	(3,510)	(3,762)	(4,097)	(4,371)	(4,919)	(6,169)	(6,821)	(8,662)
R-sqr	0.269	0.294	0.292	0.291	0.275	0.259	0.320	0.304	0.226

Notes: Control variables are listed in Table 3(b). 818 observations. Robust standard errors in parentheses.
*** significant at the 1% level, ** 5% level and * 10% level.

Table 7 – Panel Analysis (1990-2000) of the RD sample, i.e., all tracts within 3km buffer of the 221 NPL sites with 1982 HRS score in [16.5, 40.5] (weighted by the number of owner-occupied housing units).

Panel A: Dependent variable Δ Log Price of owner-occupied housing units									
Percentile	10	20	30	40	50	60	70	80	90
Δ Proposal	0.037	0.004	0.011	-0.002	-0.014	-0.021	-0.034	-0.036	-0.033
	(0.041)	(0.041)	(0.036)	(0.035)	(0.035)	(0.035)	(0.036)	(0.034)	(0.034)
Δ Listing	0.151***	0.112***	0.104**	0.088**	0.075*	0.069	0.067	0.058	0.026
	(0.046)	(0.043)	(0.044)	(0.044)	(0.044)	(0.044)	(0.044)	(0.042)	(0.041)
Δ Deletion	0.300***	0.257***	0.248***	0.228***	0.211***	0.200***	0.194***	0.185***	0.151***
	(0.051)	(0.047)	(0.047)	(0.047)	(0.047)	(0.047)	(0.046)	(0.044)	(0.044)
R-sqr	0.252	0.291	0.284	0.294	0.293	0.290	0.293	0.301	0.277
Panel B: Dependent variable Δ Price of owner-occupied housing units									
Percentile	10	20	30	40	50	60	70	80	90
Δ Proposal	1,057	-2,300	-1,685	-3,190	-5,355*	-6,867**	-11,181**	-12,247**	-11,648**
	(3,177)	(3,661)	(2,450)	(2,695)	(2,951)	(3,339)	(4,824)	(5,198)	(5,705)
Δ Listing	5,066***	3,233	3,149	2,187	1,331	256	-1,425	-2,524	-7,987*
	(1,853)	(2,102)	(2,210)	(2,414)	(2,598)	(2,826)	(3,314)	(3,484)	(4,334)
Δ Deletion	9,924***	9,359***	10,373***	9,822***	9,381***	8,769***	8,866**	8,622**	3,494
	(2,207)	(2,487)	(2,492)	(2,708)	(2,934)	(3,168)	(3,715)	(3,963)	(4,902)
R-sqr	0.213	0.237	0.247	0.249	0.240	0.231	0.251	0.244	0.204

Notes: Control variables are listed in Table 3(b). 1454 observations. Robust standard errors in parentheses.
*** significant at the 1% level, ** 5% level and * 10% level.

Table 8(a) – Panel Analysis (1990-2000) of the Strict RD and RD Tract Samples (Alternative Specifications)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Dependent variable for all columns:	Δ Log Price of owner-occupied housing units											
Sample		Strict RD			RD			Strict RD			RD	
Weights		No			No			Yes			Yes	
Std Error		Robust			Robust		Robust & Clustered on County			Robust & Clustered on County		
Percentile	20	50	80	20	50	80	20	50	80	20	50	80
Δ Proposal	-0.479***	-0.473***	-0.452***	0.020	-0.053	-0.058*	-0.396**	-0.420**	-0.403**	0.004	-0.014	-0.036
	(0.139)	(0.118)	(0.125)	(0.040)	(0.038)	(0.034)	(0.165)	(0.165)	(0.170)	(0.077)	(0.073)	(0.069)
Δ Listing	0.103	0.061	0.021	0.191***	0.102**	0.120***	0.054	0.030	-0.018	0.112	0.075	0.058
	(0.065)	(0.056)	(0.059)	(0.048)	(0.044)	(0.042)	(0.097)	(0.094)	(0.100)	(0.091)	(0.092)	(0.085)
Δ Deletion	0.208***	0.168**	0.109	0.331***	0.239***	0.248***	0.200	0.154	0.086	0.257**	0.211**	0.185*
	(0.079)	(0.070)	(0.074)	(0.054)	(0.047)	(0.046)	(0.123)	(0.121)	(0.123)	(0.106)	(0.105)	(0.095)
R-sqr	0.256	0.310	0.322	0.206	0.245	0.258	0.360	0.365	0.375	0.291	0.293	0.301
Obs.	818	818	818	1454	1454	1454	818	818	818	1454	1454	1454

Notes. Control variables are listed in Table 3(b). *** significant at the 1% level, ** 5% level and * 10% level.

Table 8(b) – Panel Analysis (1990-2000) for Tracts Within 5km Buffer

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable: Δ log price of owner-occupied housing units						
Sample		Strict RD			RD	
Percentile	20	50	80	20	50	80
Δ Proposal	-0.525***	-0.516***	-0.493***	-0.037*	-0.042**	-0.050***
	(0.117)	(0.112)	(0.115)	(0.021)	(0.019)	(0.019)
Δ Listing	-0.032	-0.078**	-0.119***	0.065**	0.052**	0.041*
	(0.037)	(0.037)	(0.036)	(0.026)	(0.024)	(0.024)
Δ Deletion	0.033	-0.020	-0.068	0.129***	0.117***	0.102***
	(0.046)	(0.045)	(0.044)	(0.025)	(0.024)	(0.024)
Obs.	1131	1131	1131	2674	2674	2674
R-sqr	0.371	0.359	0.357	0.271	0.253	0.286

Notes: Control variables are listed in Table 3(b). Robust standard errors in parenthesis.

*** significant at the 1% level, ** 5% level and * 10% level.

Table 8(c) – Panel Analysis (1990-2000) of the All-NPL Tract Sample

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable: Δ Log price of owner-occupied housing units						
Weights		Yes			No	
Percentile	20	50	80	20	50	80
Δ Proposal	-0.072***	-0.081***	-0.075***	-0.084***	-0.089***	-0.076***
	(0.013)	(0.012)	(0.012)	(0.013)	(0.013)	(0.012)
Δ Listing	-0.002	-0.002	-0.004	-0.016	-0.015	-0.020*
	(0.011)	(0.011)	(0.011)	(0.012)	(0.011)	(0.011)
Δ Deletion	0.014	0.031*	0.034*	-0.013	0.021	0.031*
	(0.019)	(0.019)	(0.018)	(0.020)	(0.018)	(0.018)
Obs.	9674	9674	9674	9674	9674	9674
R-sqr	0.228	0.243	0.245	0.134	0.175	0.179

Notes: Control variables are listed in Table 3(b). Robust standard errors are in parentheses.
*** significant at the 1% level, ** 5% level and * 10% level.

Table 9 – Tracts in the strict RD sample that experienced non-zero change in exposure to proposed sites.

NPL Site	Site Status	Date	Tract	Δ Share of Tract Exposed to Proposed Sites
COD007063530	Discovery	3/1/1978	08031000202	0.47
	Proposal to NPL	5/10/1993	08031001101	0.30
	Cost Recovery	6/27/2006		
	Bankruptcy Settlement	3/13/2009		
MED980504393	Discovery	12/1/1979	23031005100	-0.37
	Proposal to NPL	6/24/1988	23031005200	-0.09
	Final listing on NPL	2/21/1990	23031005300	-0.30
			23031025100	-0.01
ORD009051442	Discovery	7/1/1979	41051003100	-0.01
	Proposal to NPL	1/22/1987	41051003200	-0.16
	Final listing on NPL	2/21/1990	41051003301	-0.60
	Deletion	11/14/1994	41051003401	-0.51
			41051003501	-0.14
			41051003601	-1.00
			41051003602	-0.94
			41051003603	-0.18
			41051003702	-1.00
			41051003802	-1.00
			41051003803	-0.84
PAD980692487	Discovery	6/1/1980	42039110600	-0.12
	Proposal to NPL	6/24/1988	42039110700	-0.18
	Final listing on NPL	2/21/1990		

Table 10: Block-Level Panel Analysis (1990-2000): Summary of Marginal Effects of Remedial Actions, Strict RD Sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Counts by	Distance below a cutoff				Distance Bands			
EPA action	0.25km	0.5km	1km	3km	0-1km	1-2km	2-3km	3-5km
Proposal	0.143	-0.305	-0.247**	-0.373***	-0.279***	-0.465***	-0.398***	-0.175***
	(-0.203)	(0.187)	(0.096)	(0.030)	(0.095)	(0.050)	(0.039)	(0.042)
Listing	0.188	0.078	0.133***	0.052***	0.129***	-0.019	0.086***	0.209***
	(0.139)	(0.085)	(0.047)	(0.018)	(0.047)	(0.030)	(0.024)	(0.029)
Deletion	0.121	0.199**	0.190***	0.058***	0.194***	-0.001	0.084***	0.207***
	(0.248)	(0.100)	(0.054)	(0.020)	(0.054)	(0.033)	(0.027)	(0.031)

Notes: Obs=26,035. Robust standard errors in parenthesis. *** significant at the 1% level, ** 5% level and * 10% level.

Table 11: Block-Level Panel Analysis (1990-2000): Summary of Marginal Effects of Remedial Actions, RD Sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Counts by		Distance below a cutoff					Distance Bands	
EPA action	0.25km	0.5km	1km	3km	0-1km	1-2km	2-3km	3-5km
Proposal	0.211	-0.033	0.029	0.052***	0.026	0.105***	0.029**	0.034***
	(0.254)	(0.072)	(0.032)	(0.010)	(0.032)	(0.018)	(0.012)	(0.013)
Listing	0.027	0.033	0.039*	0.038***	0.041**	0.077***	0.018*	0.033***
	(0.069)	(0.040)	(0.021)	(0.008)	(0.021)	(0.013)	(0.010)	(0.010)
Deletion	-0.051	0.069	0.071***	0.058***	0.078***	0.112***	0.029**	0.029**
	(0.116)	(0.050)	(0.027)	(0.010)	(0.027)	(0.016)	(0.012)	(0.013)

Notes: Obs=49,044. Robust standard errors in parenthesis. *** significant at the 1% level, ** 5% level and * 10% level.

Table 12: Block-Level Panel Analysis (1990-2000): Summary of Marginal Effects of Remedial Actions, All-NPL Sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Counts by		Distance below a cutoff					Distance Bands	
EPA action	0.25km	0.5km	1km	3km	0-1km	1-2km	2-3km	3-5km
Proposal	0.011	-0.010	-0.020**	-0.006**	-0.013	-0.005	-0.002	0.014***
	(0.043)	(0.020)	(0.009)	(0.003)	(0.009)	(0.005)	(0.004)	(0.004)
Listing	0.061*	0.052***	0.037***	0.036***	0.043***	0.043***	0.033***	0.035***
	(0.033)	(0.016)	(0.007)	(0.002)	(0.007)	(0.004)	(0.003)	(0.003)
Deletion	0.049	0.081***	0.061***	0.065***	0.072***	0.083***	0.059***	0.066***
	(0.066)	(0.025)	(0.011)	(0.003)	(0.011)	(0.006)	(0.005)	(0.005)

Notes: Obs=323,682. Robust standard errors in parenthesis. *** significant at the 1% level, ** 5% level and * 10% level.

Table 13(a) – House-Level Analysis
Marginal Effect of Sites Within 3km Buffer

	Northern New Jersey				Los Angeles Metropolitan Area			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Discovery	0.158***	0.076***	0.008	-0.144***	0.032***	-0.028***	0.010**	-0.128***
	(0.032)	(0.015)	(0.014)	(0.009)	(0.012)	(0.005)	(0.004)	(0.003)
Proposal	0.142***	0.198***	0.064***	-0.043***	0.040***	-0.015**	0.024***	-0.103***
	(0.031)	(0.023)	(0.015)	(0.013)	(0.014)	(0.007)	(0.006)	(0.004)
Listing	0.247***	0.015***	0.156***	-0.032***	0.047***	-0.007**	0.000	-0.004**
	(0.027)	(0.004)	(0.011)	(0.002)	(0.013)	(0.003)	(0.005)	(0.002)
Deletion	0.255***	0.108***	0.222***	0.093***	0.050***	-0.050***	-0.015**	0.283***
	(0.030)	(0.010)	(0.013)	(0.006)	(0.016)	(0.011)	(0.007)	(0.007)
Tract-by-Year Fixed Effects	Yes	Yes	No	No	Yes	Yes	No	No
House Fixed Effects	Yes	No	Yes	No	Yes	No	Yes	No

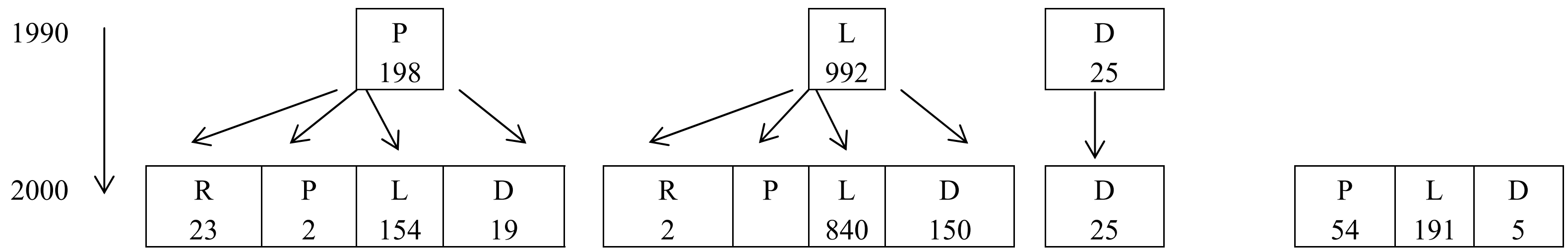
Robust standard errors in parentheses. *** significant at the 1% level, ** 5% level and * 10% level. Note that columns (3) and (4) do contain year fixed effects to flexibly control for trends in housing prices at the market level.

Table 13(b) – House-Level Analysis
Marginal Effect of Sites Within 3km Buffer

	Boston Metropolitan Area				Southwestern Connecticut			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Discovery	-0.079***	-0.056***	0.051***	0.065***	0.014	0.011	-0.274***	-0.327***
	(0.030)	(0.008)	(0.017)	(0.006)	(0.036)	(0.016)	(0.013)	(0.009)
Proposal	-0.077**	-0.095***	0.034*	-0.003	0.037	-0.049**	-0.275***	-0.397***
	(0.031)	(0.014)	(0.018)	(0.013)	(0.037)	(0.019)	(0.018)	(0.014)
Listing	-0.077***	-0.060***	0.043***	-0.088***	0.018	0.001	-0.350***	-0.217***
	(0.030)	(0.004)	(0.017)	(0.003)	(0.032)	(0.007)	(0.012)	(0.005)
Deletion	-0.061**	-0.083***	0.037**	-0.123***	-0.002	0.070***	-0.395***	-0.529***
	(0.031)	(0.011)	(0.017)	(0.006)	(0.036)	(0.025)	(0.016)	(0.022)
Tract-by-Year Fixed Effects	Yes	Yes	No	No	Yes	Yes	No	No
House Fixed Effects	Yes	No	Yes	No	Yes	No	Yes	No

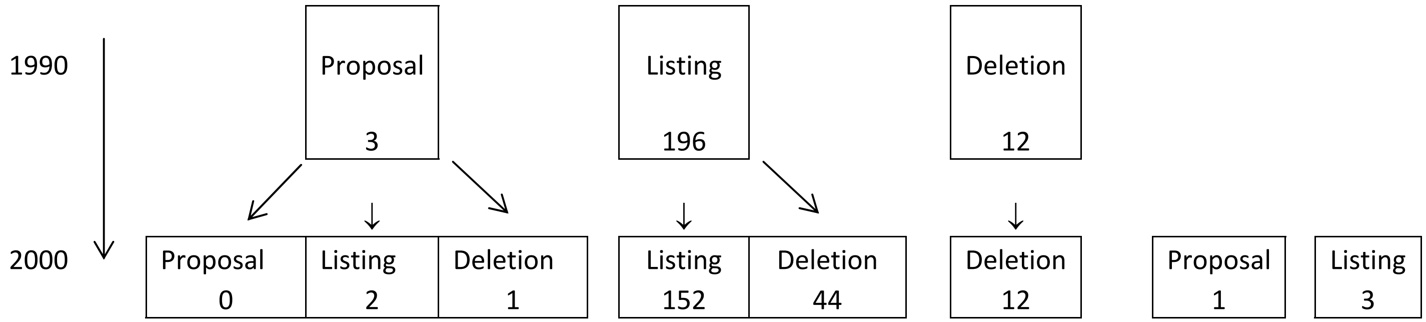
Robust standard errors in parentheses. *** significant at the 1% level, ** 5% level and * 10% level. Note that columns (3) and (4) do contain year fixed effects to flexibly control for trends in housing prices at the market level.

Figure 1 – All-NPL Sites Progress Through Superfund Milestones[36]



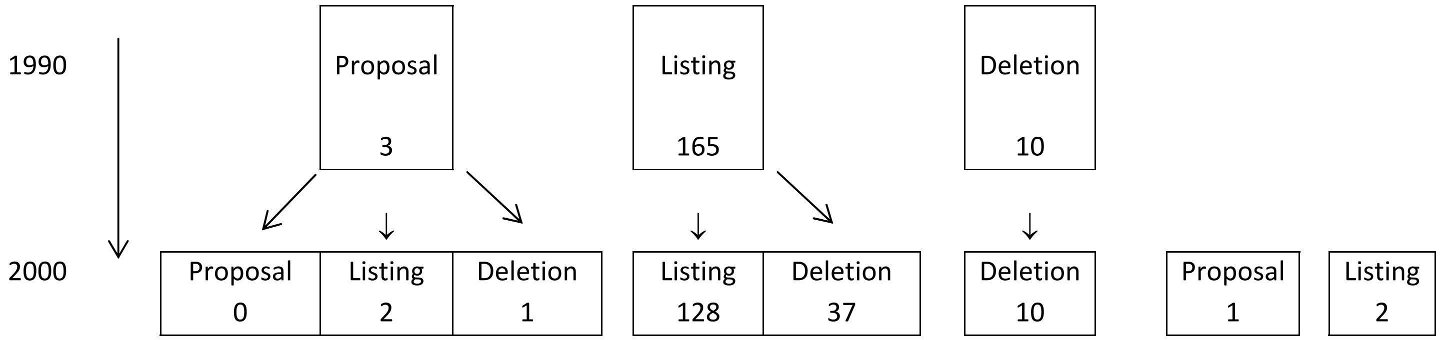
[36] R=Removed from the NPL; P=Proposed; L=Listing; D=Deletion. The All-NPL samples also include 257 other sites were either (i) proposed to the NPL after 2000 and before 2010, or (ii) proposed to the NPL before 1990 and removed from the NPL before 1990.

Figure 2 – RD Sites Progress Through Superfund Milestones[37]



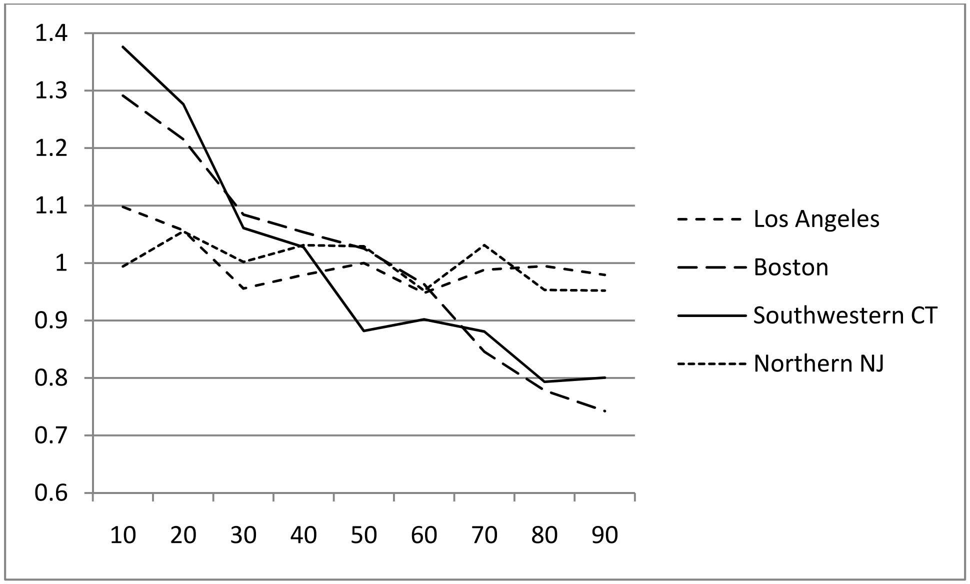
[37] Note that 6 sites were proposed to the NPL and removed from the NPL before 1990. R=Removed from the NPL; P=Proposed; L=Listing; D=Deletion.

Figure 3 – Strict RD Sites Progress Through Superfund Milestones[38]



[38] Note that 6 sites were proposed to the NPL and removed from the NPL before 1990. R=Removed from the NPL; P=Proposed; L=Listing; D=Deletion.

Figure 4 – Exposure to NPL Sites by Within-Tract Price Decile in Four Housing Markets (0 - 1km Exposure)



Appendix A1: Price Intervals for Owner-Occupied Housing in the Decennial Census

Table A1
Price Intervals for Counts of Owner-Occupied Housing in

	Year	
# Categories	1990	2000
1	<$15	<10
2	$15-20	$10-15
3	$20-25	$15-20
4	$25-30	$20-25
5	$30-35	$25-30
6	$35-40	$30-35
7	$40-45	$35-40
8	$45-50	$40-50
9	$50-60	$50-60
10	$60-75	$60-70
11	$75-100	$70-80
12	$100-125	$80-90
13	$125-150	$90-100
14	$150-175	$100-125
15	$175-200	$125-150
16	$200-250	$150-175
17	$250-300	$175-200
18	$300-400	$200-250
19	$400-500	$250-300
20	>$500	$300-400
21		$400-500
22		$500-750
23		$750-1mil
24		>$1mil

Notes: Values are in thousands of dollars

Appendix A2: Construction of variables: Exposure to Deletion, Listing and Proposal

In this appendix, we describe the construction of the tract-level variable measuring the exposure of tracts to NPL sites at the three milestones of proposal, listing, and deletion. The following two maps show the NPL sites in the Memphis Metropolitan Statistical Area (MSA) in 1990 and 2000, respectively.

Deletion

Consider tract A which is completely enclosed within the 3km buffer of North Hollywood Dump. North Hollywood Dump was in the listing stage in 1990 but reached the deleted stage by 2000. The exposure of tract A to deleted sites in 1990 is zero and the exposure of tract A to deleted sites in 2000 is the share of that tract which overlaps with the 3km buffer surrounding North Hollywood Dump. Because tract A is completely enclosed in the 3km buffer of North Hollywood Dump, this exposure amounts to 1. A comparison of the two maps indicates that the change in exposure of tract A to deleted sites between 1990 and 2000 is also 1.

Consider tract B which overlaps with a large segment of the 3km buffer surrounding Gallaway Pits. Gallaway Pits was in the listing stage in 1990 and in the deleted stage in 2000. The exposure of tract B to deleted sites is zero in 1990, but in 2000 it is the share of that tract which overlaps with the 3km buffer surrounding Gallaway Pits.

Finally, consider tract C which overlaps with the 3km buffer surrounding Chemet (a site that appears in 2000). The exposure of tract C to deleted sites is zero in 1990, but in 2000 it is the share of that tract which overlaps with the 3km buffer surrounding Chemet.

Listing

Consider the exposure of tract A to listed sites in 1990. The exposure of tract A to listed sites in 1990 is the share of tract A that overlaps with North Hollywood Dump (i.e., 1), and its exposure to listed sites in 2000 is zero. Therefore, the change in exposure of tract A between 1990 and 2000 to listed sites is -1.

Consider tract D which overlaps with Gurley Pit. Gurley Pit is in the listing stage in both 1990 and 2000. The exposure of tract D to listed sites in 1990 is the share of the tract that overlaps with Gurley Pit. That exposure in 2000 is the same. Therefore, the change in exposure of tract D to listed sites between 1990 and 2000 is zero.

Proposal

Consider tract E which overlaps with Carrier Air Conditioning Company. The Carrier site is in the proposal stage in 1990 and in the listing stage in 2000. The exposure of tract E to proposed sites in 1990 is therefore the share of the tract that overlaps with Carrier, and exposure to proposed sites in 2000 is zero. The change in exposure of tract E to proposed sites between 1990 and 2000 is then the negative of the share of tract land exposed in 1990.

Overlap

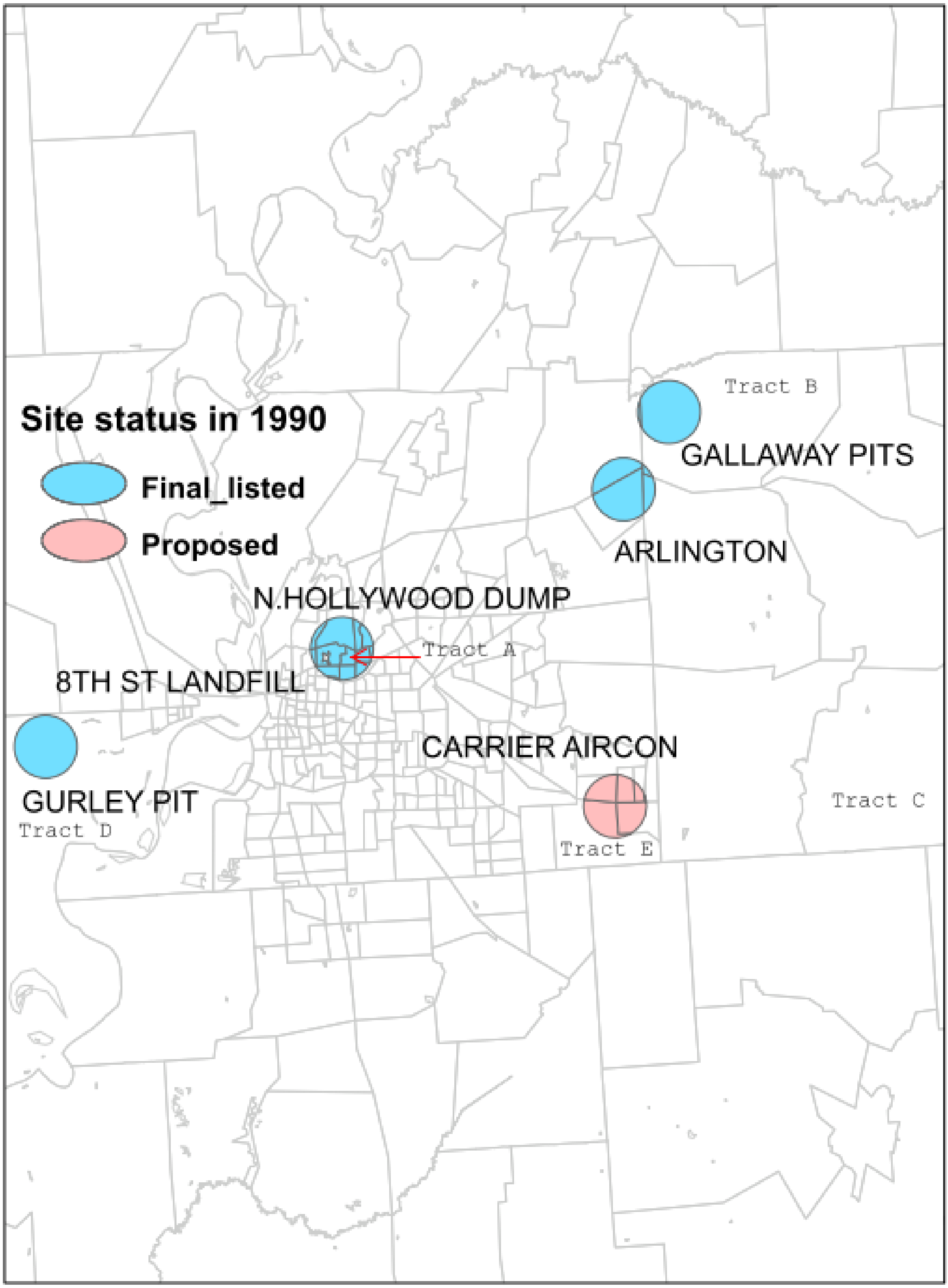
Consider the case where the 3km buffer of two NPL sites with the same status at time *t* overlap, and this overlapping area is enclosed in a tract. In calculating the exposure of the tract to those two NPL sites, we include the overlap area only once in our calculation of exposure. This is one extreme option in defining exposure, which maintains the exposure of the tract is always at or below one. The drawback of this approach is that we do not account for the possibility that exposure to two deleted sites may have stronger effects on the appreciation of housing values than the exposure to one deleted site. Nevertheless, as seen in Table A2.1, in our (preferred) strict RD sample of tracts, very few tracts intersect with areas of overlap between two or more NPL sites. Specifically, only 5 and 12 tracts intersect with areas of overlap of sites that are deleted or listed, respectively.

At the other extreme of defining exposure, we could include the overlap area twice in our calculation, resulting in some cases of exposure of the tract exceeding one. It is not clear, however, that the marginal effect of multiple exposures to sites at the same level of remediation should not be decreasing. In any case, the assumption we make about multiple exposures should have little practical consequence for our strict RD results, given the numbers reported in Table A2.1.

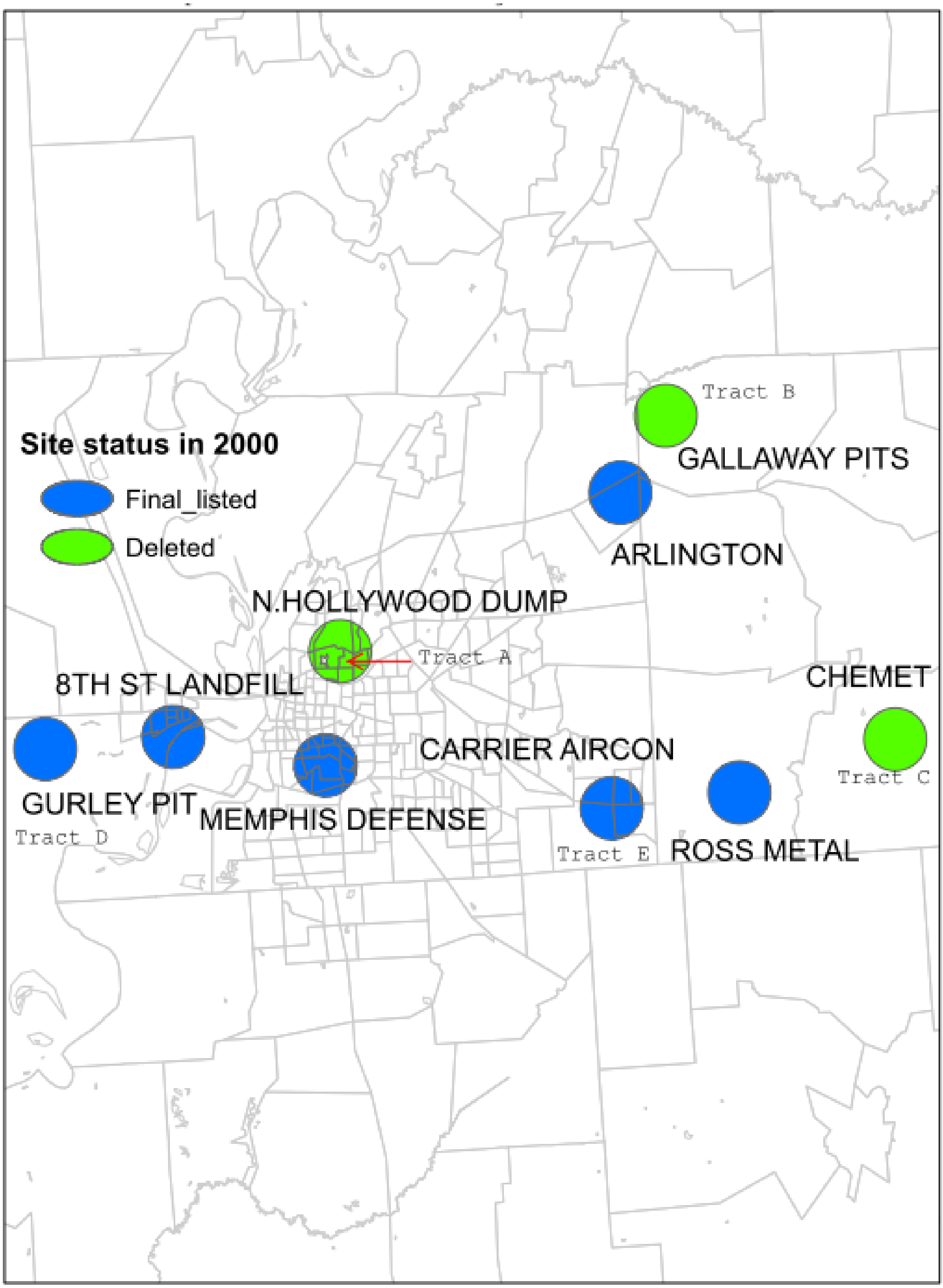
Table A2.1: Tracts in the Strict RD where buffers of two NPL sites with the same status overlap

Tract-ID	Share of tract exposed to the overlap
Panel A: Deleted sites (5 out of 1666 tracts x years)	
10003016000	39.8
42069112500	14.2
42069112600	88.5
42069112700	99.3
42069112800	8.1
Panel B: Listed sites (12 out of 1666 tracts x years)	
18061060600	0.5
21029021200	0.3
21093000100	33.5
21093000200	1.1
21111012104	10.0
21139040200	3.3
21157950100	15.7
34021004203	1.8
36063022900	4.9
42101032200	47.4
42069112500	10.6
42069112600	5.2

Memphis MSA 1990 NPL Sites



Memphis MSA 2000 NPL Sites



Appendix A3: Comparison with Greenstone and Gallagher (2008)

The differences between our study and Greenstone and Gallagher (2008) are summarized in Table A3.1. These differences include: (i) our focus on deletion as the milestone used to measure of benefits from cleanup, (ii) the construction of our data (i.e., tracts as our observation units, our sample of ever proposed NPL sites, our definition of exposure to NPL sites, and our use of smaller buffers to define neighborhoods affected by NPL sites); and (iii) our method (i.e., measuring the effects of deletion separately from listing, applying a panel method to the RD sample and examining the lower quantiles of tract-level housing values).

Comparison of our regression model with Greenstone and Gallagher (2008)

As described in section 4.1, we begin with the standard hedonic specification that relates tract or block housing values with contemporaneous tract or block attributes, and then difference across the 2000 and 1990 specifications in order to control for time invariant unobservables at the tract or block level. In contrast, Greenstone and Gallagher's regression model relates 2000 tract median housing values to 1980 tract characteristics and 1980 tract median values; they argue that 1980 tract attributes are correlated with the 2000 attributes, but are pre-determined with respect to Superfund site status. Deriving the Greenstone and Gallagher regression specification from 1990 and 2000 hedonic price functions, however, we find that the resulting regression error will be correlated with the key variables appearing on the right-hand-side of the regression, biasing parameter estimates. To illustrate this, begin with a hedonic price function for each year:[39]

$$y_{c2000} = \theta 1(NPL_{c2000} = 1) + X'_{c2000}\beta + u_{c2000}$$

$$y_{c1980} = \theta 1(NPL_{c1980} = 1) + X'_{1980}\beta + u_{c1980}$$

Noting that $NPL_{c1980} = 0$, we can multiply the second equation by φ and then difference these two equations to obtain:

$$y_{c2000} - \varphi y_{c1980} = \theta 1(NPL_{c2000} = 1) + (X_{c2000} - \varphi X_{c1980})'\beta + (u_{c2000} - \varphi u_{c1980})$$

In order to convert this into the equation estimated by Greenstone and Gallagher, we need to (i) add φy_{c1980} to both sides of the equation, and (ii) move $X'_{c2000}\beta$ into the regression error term:

$$y_{c2000} = \theta 1(NPL_{c2000} = 1) - X'_{c1980}\psi + \varphi y_{c1980} + \underbrace{(X'_{c2000}\beta + u_{c2000} - \varphi u_{c1980})}_{\varepsilon}$$

[39] For this illustration, we use Greenstone and Gallagher's notation, where y_{c2000} refers to the median housing value in census tract c in year 2000, $1(NPL_{c2000} = 1)$ is an indicator taking the value 1 if that tract was exposed to either a listed or deleted site in that year, and X'_{c2000} is a vector of tract attributes).

where $\psi = \varphi\beta$. Note that the resulting regression error (ε) will contain the unobserved determinants of the 1980 median house value (φu_{c1980}), so that the 1980 median (or mean) housing value will naturally be correlated with it, based on the original hedonic specification. Because year 2000 covariates are relegated to the regression error, 1980 covariates (X_{c1980}) will likely be correlated with this component of the error term as well. Finally, the main variable of interest (year 2000 NPL status) will be correlated with the regression error term if it is correlated with year 2000 covariates, which we might also suspect to be the case. Greenstone and Gallagher will therefore need to rely on their regression discontinuity and IV approaches to eliminate any bias stemming from this correlation.

Direct comparison of our method with Greenstone and Gallagher (2008)

We next provide a direct comparison of our method with Greenstone and Gallagher (2008), to the extent possible given the limitations posed by our different samples and methods. We can replicate most closely their RD specification which relates sites' statuses to tract attributes on which these sites are located. We begin by constructing an "intersection" sample (i.e., the intersection of our data set and Greenstone and Gallagher's). We continue to restrict our sample to those sites that are ever proposed to the NPL. We exclude non-NPL sites because including these sites amongst the "non-treatment" observations can lead to a downward bias in the estimates of benefits from deletion. Therefore, a key difference between the samples is that Greenstone and Gallagher (2008) include 65 non-NPL sites in their analysis of 227 sites, while we exclude non-NPL sites from our "intersection" sample.

The unit of observation in our main analysis is the tract, while Greenstone and Gallagher's unit of observation is the site. To be comparable with their analysis, we therefore limit our intersection sample to include only tracts on which no more than one site is located. For their covariates, Greenstone and Gallagher use tract attributes on which these sites are located or the weighted average of values for tracts that intersect within their 3 mile buffers around the sites. In doing so, Greenstone and Gallagher (2008) assume that exposure to the sites is homogeneous across the housing distribution within the tract (i.e., the weighted average does not allow for covariates to vary systematically within tracts). Table A3.2 describes the steps in constructing our "intersection" sample and our variables.

First, we apply Greenstone and Gallagher's (2008) method – i.e., the use of [HRS82>28.5] to define an instrument for listing on the NPL by 2000 ([NPL_{2000}=1]) – to the "intersection" sample. We find two problems with this IV approach. First, the instrument is weak in the "intersection" sample; second, this single instrument cannot simultaneously address two endogenous variables (i.e., listing and deletion). Listing and deletion are distinct Superfund treatments that should be measured separately; listing has ambiguous effects on housing prices, while deletion is likely to raise housing prices. Table A3.3 shows the first-stage relationship used in the IV strategy, regressing the [NPL_{2000}=1] indicator on the instrument and the other exogenous variables. The relationship between the instrument and endogenous variable exhibits insufficient statistical significance (i.e., a t-statistic of only 2.38) to avoid the "weak instruments" problem. (Staiger and Stock, 1997). A weak instrument contributes to imprecise measurement and results that are sensitive to the specification.

We run regressions with housing prices at the *q*th decile as the dependent variable, using [HRS82>28.5] as an instrument for [NPL_{2000}=1], and employing the other 1980 control variables as in Greenstone and Gallagher.[40] Our analysis compares exposure to listing (i.e., which includes all sites that have been exposed to either listed or deleted sites as of 2000) relative to exposure to pre-proposed or proposed sites, while Greenstone and Gallagher's analysis compares the effect of listing (similarly defined) relative to the pre-proposal or proposal stages of NPL sites or non-NPL sites. The regressions shown in our Table A3.4 (first row) correspond to Greenstone and Gallagher's results described in their Table IV (Panel A, seventh column). Our coefficient at the median, shown in the third column, is similar to theirs. In particular, we find a positive (0.403) but statistically insignificant (p-value = 0.612) effect of NPL_{2000} treatment at the median. This insignificant point estimate is slightly larger than that estimated by Greenstone and Gallagher. This difference may arise from our exclusion but their inclusion of non-NPL sites as "non-treatment" sites; the latter leads to a downward bias in the estimate of benefits.

Next we examine how these results vary across the housing value distribution. In Table A3.4 (first row), reading across the columns, we see the effects of [NPL_{2000}=1] at different deciles of the housing value distribution, as estimated by regressions at the respective deciles. While none of the effects are statistically significant, we do find large, positive point estimates at the lower deciles. Unsurprisingly, given the presence of weak instruments, these results are highly sensitive to model specification. The second row shows results when state fixed effects are dropped from each specification. The wide fluctuations in point estimates associated with [NPL_{2000}=1] is indicative of a weak instruments problem.

We next apply our method to this intersection sample. Our panel approach allows us to measure separately the effects of listing and deletion, while controlling for time invariant unobservables that are correlated with both listing and deletion. Time-varying unobservables can still bias our results on listing and deletion, but the bias is likely to be limited. First, any such bias on the listing coefficient is mitigated by using the RD sample (i.e., sites that are proposed but not listed should be similar to sites that are listed). Second, Sigman (2001) suggests that time varying unobservables are likely to assert only limited bias on the deletion coefficient.[41]

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[40] Our 1980 covariates, listed in Table A3.3, correspond to those used in Greenstone and Gallagher (2008) with one exception – we were unable to include the percentage of households with air conditioning in our list.

[41] See the discussion in section 6.1.

We apply our method in three progressive steps. (i) We introduce the panel approach, but continue to measure exposure using a single treatment variable – i.e., listed on or deleted from the NPL in 2000 (NPL_{2000}) and listed on or deleted from the NPL in 1990 (NPL_{1990}). (ii) We move from the single treatment measure to two separate treatment measures – i.e., exposure of the tract to listed sites and exposure of the tract to deleted sites in each year. Thus, we examine the changes in exposure to listing ($\Delta listed_{2000\text{-}1990}$) and changes in exposure to deletion ($\Delta deletion_{2000\text{-}1990}$). (iii) We examine not only the median, but also the upper and lower quantiles of the tract-level housing value distribution. In all three steps, the 1980 covariates appear in the differenced regression specification as we allow their coefficients to vary over time.

The panel model using exposure measured with a single "listed on the NPL" variable continues to yield statistically insignificant coefficients, as seen in the third row of Table A3.4. These insignificant results are unsurprising, as this specification continues to conflate listing and deletion into a single variable. Our final specification considers the two exposure variables (listing and deletion) separately in combination with looking at the full distribution of housing values within tract. The regression yields coefficients of 0.13 and 0.126 at the 10^{th} and 20^{th} percentile respectively – these results are nearly statistically significant at conventional levels with p-values of 0.11 and 0.114, respectively (note the small size of the intersection sample that we use for this exercise; N = 158). There is, however, no statistically significant effect at or above the 30^{th} percentile. We therefore find evidence of greater appreciation from deletion at lower quantiles (albeit at smaller magnitudes), but no such evidence at the upper quantiles. These results are comparable to our main results (i.e., when we apply our method to our strict RD and RD samples). Nevertheless, the magnitudes of appreciation in the lower deciles are smaller in our "intersection" sample than in our main strict RD and RD samples.

Table A3.1: Comparison With Greenstone and Gallagher (2008); Data and Methods

	This Study	GG (2008)
Research Question	The effect of Deletion (signalling the end of cleanup) on housing values	The effect of Listing (an intermediate milestone in the Superfund process) on housing values
Data		
Sites in Regression Discontinuity (RD) sample	221 sites Begin with 1722 sites that are ever proposed to the NPL by Jan 1, 2010 (i.e. NPL sites). Restrict to 221 sites that received 16.5 ≤ HRS-82 ≤ 40.5.	227 sites Begin with 676 sites that were scored in 1982. Restrict to 332 sites that received 16.5 ≤ HRS-82 ≤ 40.5. [95 out of 332 sites dropped due to missing 1980 variables.]
Excluded sites	Excludes non-NPL sites. (Non-NPL=not proposed to NPL by 1/1/2010) Rationale for excluding non-NPL sites: Tracts hosting NPL and non-NPL sites are likely to differ systematically, violating the spirit of the RD design. The strict RD sample (187 sites) further exclude tracts exposed to sites for which (i) HRS-82 < 16.5 & HRS-82 > 40.5 or or (ii) unscored in 1982, as these sites may confound housing prices.	Includes non-NPL sites. [65 out of 227 sites are non-NPL sites]
Unit of observation	Tracts i.e. whose centroids fall within the buffer around the sites.	Sites Housing and demographic attributes from (i) tracts on which sites are located, or (ii) average attribute of tracts that overlap with the buffer around the site, thus assuming within tract homogeneity.
Affected neighborhood	Buffer of 3km (≈ 2 miles) and 5 km (≈ 3 miles). Rationale: Distance-based studies find hazardous waste sites are associated with depreciation in housing values within≈3km of the site.	Buffer of 3 miles and 5 miles.
Exposure measure	Share of tract that overlap with buffers surrounding NPL sites i.e. whether Proposed, Listed, or Deleted.	Binary indicator : 1= site is "Listed" by 2000, 0= site "Non-listed". "Listed" includes Listed, Construction Complete, Deleted; "Unlisted" includes Proposed, non-NPL.

Table A3.1 (continued): Comparison of this study and GG (2008) RD-tract-level analysis in their Data and Method		
Estimation Method		
Superfund	Measure the effects of Proposed,	Measure the effect of "Listed" vs. "Non-listed".
milestones	Listed, and Deleted relative to	"Listed"=Listed or beyond the Listed milestone.
examined	Pre-proposal as the omitted category.	"Non-listed"=Proposed or pre-proposed to NPL
	Rationale - these milestones are likely to	and non-NPL.
	have distinct effects of housing values.	
Housing values	Entire house price distribution	Median tract-level housing values.
examined	at the tract level.	
	Further analysis uses median block-level	
	housing values.	
Estimation	Sample restrictions to RD and strict RD	Sample restrictions to RD sample.
Strategy	samples.	
	Panel analysis (1990-2000).	IV for "Listed"/Non-listed" on the NPL
		using HRS-82.
	Rationale: IV using HRS-82 cannot be	IV using HRS-82 can be applied to
	applied to three variables - Proposed,	to the one variable "Listed/Non-Listed"
	Listed and Deletion.	on the NPL.

Table A3.2: Steps in creating a sample defined as closely as possible to GG's RD sample

	Greenstone and Gallagher (2008)	Our study
Sample	Start with 687 sites scored in 1982.	Start with 1722 sites ever proposed to NPL by 1/1/2010.
	Restrict to 332 sites that received	Restrict to 221 sites that received
	16.5 ≤HRS-82≤ 40.5.	16.5 ≤HRS-82≤ 40.5.
Units of	Sites.	Our study has used tracts as units of observation.
Obs.	The attributes related to the sites are from	
	tracts on which the sites are located.	
	GG is able to include 3 pair of sites In which each	For comparability with GG's sample, we restrict our sample
	pair occupies the same tract. E.g. a tract has	to 221 tracts occupied by sites. We further restrict our sample
	site A that is listed and site B that is proposed.	to 215 tracts occupied by no more than one site per tract
	The tract attributes enters as two rows of data	because can define HRS-82, used as the IV, for such
	corresponding to site A and site B, respectively.	tracts only.
Final	Only 227 sites with 1980 covariates	Only 158 tracts with 1980 covariates
sample	- 65 non-NPL sites in sample	- no NPL sites in sample
Treatment	NPL2000=1 if site Listed and 0 if site unlisted.	NPL2000=1 for tracts exposed to listed or deleted sites
definition	Listed=Listed or beyond the listing milestone.	in 2000.
	Not-Listed=NPL site that is pre-proposed or	NPL2000=0 for tracts exposed to NPL sites proposed
	or proposed and non-NPL sites.	in 2000 or still pre-proposed in 2000.

Table A3.3 – First-Stage Regression, Greenstone and Gallagher IV Procedure (State Fixed Effects Included). Dependent Variable = NPL_{2000}, N = 158, R^2 = 0.506.

1980-covariates	Mean in 1980	Coefficient		Std Error	t-stat
1[HRS-82>28.5]	0.85	0.081	**	0.034	2.38
% unit occupied	94	0.002		0.004	0.57
% owner occupied	71	0.002	*	0.001	1.66
Housing unit density	0.0002	-133.4		329.5	-0.40
Population density	0.0004	54.7		124.2	0.44
% Black	9	-0.001		0.001	-0.83
% Hispanic	3	0.005		0.004	1.14
% under 18 years old	29	0.0008		0.004	0.21
% high school dropout	36	-0.007	**	0.003	-2.64
% college educated	13	-0.003		0.003	-0.97
% below poverty line	11	-0.005		0.004	-1.45
% public assistance	8	0.006		0.004	1.37
% female headed HH	17	0.001		0.003	0.44
Mean HH income	$20,141	-1.2×10^{-5}	*	-6.2×10^{-6}	-1.97
% attached homes	3	0.001		0.002	0.57
% mobile homes	7	-0.0009		0.002	-0.48
% 0-2 bedrooms	33	-0.003		0.004	-0.78
% 3-4 bedrooms	63	-0.003		0.004	-0.88
% units built within 5 years	13	9.3×10^{-4}		0.003	0.31
% units built within 10 years	25	-0.003		0.002	-1.30
% same house in the last 5 years	57	-0.002		0.002	-1.40
Mean housing value	$60,469	1.0×10^{-6}		1.0×10^{-6}	1.03
Constant	1	1.561	**	0.598	2.61

Notes: State fixed effects are included. Mean NPL_{2000}= 0.981 * statistically significant at the 10% and ** statistically significant at 5%.

Table A3.4 – Comparison with Greenstone and Gallagher (2008); Results.

				[1]	[2]	[3]	[4]	[5]	State
Panel	Method	Dependent	Variable			Percentile			Fixed
		Variable	of Interest	10	20	50	80	90	Effects
A	2SLS	Log $Price_{2000}$	NPL 2000	1.489	0.728	0.403	0.492	0.204	Y
			mean=0.981	(1.250)	(1.002)	(0.794)	(0.895)	(0.964)	
B	2SLS	Log $Price_{2000}$	NPL 2000	0.032	-0.401	-0.310	-0.149	-0.486	N
			mean=0.981	(0.985)	(0.885)	(0.773)	(0.839)	(0.895)	
C	Panel OLS	ΔLog Price	ΔNPL	-0.108	-0.033	-0.124	-0.106	-0.169	Y
	1990-2000		mean=0.019	(0.169)	(0.153)	(0.117)	(0.109)	(0.127)	
D	Panel OLS	ΔLog Price	ΔListing	0.083	0.050	-0.044	0.030	0.043	Y
	1990-2000		mean=-0.095	(0.086)	(0.078)	(0.060)	(0.056)	(0.066)	
			ΔDeletion	0.130[†]	0.126[§]	-0.010	0.026	0.036	
			mean=0.190	(0.081)	(0.073)	(0.057)	(0.053)	(0.062)	

Notes: All specifications include 1980 covariates correspond to those listed in Table 3(b). 158 observations. [†]p-value=0.110 and [§]p-value=0.114. Standard errors in parenthesis.