DOES CLEANUP OF HAZARDOUS WASTE SITES RAISE HOUSING VALUES?

EVIDENCE OF SPATIALLY LOCALIZED BENEFITS

by

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Abstract (230 words)

Previous tract-level analysis of hazardous waste remediation under the Superfund program has failed to detect evidence of economic benefits being capitalized into nearby median housing values (Greenstone and Gallagher, 2008). Our study exploits high-resolution restricted-access census block data to provide direct evidence that these benefits are, in fact, *large* but *highly localized* in space. Comparing blocks located near National Priority List (NPL) sites that received cleanup treatment with those near similar NPL sites that that were not cleaned, we find that median housing values appreciate by 19.0% for blocks lying < 1km and by 5.8% for blocks lying < 3km from those sites. Recognizing that most analysts only have access to publicly available tract-level data, we also show that evidence of these localized effects can be found by examining the entire within-tract housing value distribution, rather than simply focusing on the median. Our tract level analysis detects larger appreciation at the 10th percentile of the housing values (20.9%) than at the median (16.3%), and the effect of cleanup becomes insignificant beyond the 60th percentile. Finally, our block and tract level results are confirmed by additional evidence from geocoded proprietary housing transactions data describing four large housing markets. These data show explicitly that it is the cheaper houses within a tract that are more likely to be exposed to waste sites within one kilometer, explaining their greater appreciation from site cleanup.

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1. Measuring Localized Benefits with Tract-Level Housing Data

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. Under that program, the Environmental Protection Agency (EPA) has placed some of the most seriously contaminated sites on its National Priorities List (NPL) (Sigman, 2008; Sigman and Stafford, 2011). As of January 2010, EPA had classified 1,627 sites as NPL sites.[1]

The economic benefits from the Superfund program have been questioned. Most recently, Greenstone and Gallagher's (2008) influential study compared sites with similar risk scores (i.e., those which were listed on the NPL and those that narrowly missed listing) and found that Superfund cleanup had little or no effect on tract *median* housing values within three miles (≈5km). In contrast, our analysis uses three levels of housing data (i.e., measured at the house, block and tract levels) and re-examines Greenstone and Gallagher's (2008) empirical specification. In contrast to their results, we find strong evidence of benefits from Superfund cleanup. Specifically, our analysis of high geographical resolution data (i.e., restricted-access block data) provides direct evidence that these benefits are, in fact, *large* but *highly localized* in space. Comparing blocks located near similar National Priority List (NPL) sites that received the cleanup with those near NPL sites that that were not cleaned, we find that housing values appreciate by 19.0% for blocks < 1km and by 5.8% for blocks lying < 3km from remediated sites.

[1] http://www.epa.gov/superfund/sites/npl/

Recognizing that most analysts only have access to publicly available tract-level data, we propose a refinement to the hedonic method: specifically, we detect these localized effects by examining the *entire within-tract housing value distribution*, rather than simply focusing on the tract median. In support of this effort, we construct the housing value distribution for each tract in our sample using tract-level counts of houses at various price intervals. Our tract-level analysis detects larger appreciation at the 10^{th} percentile of the housing values (20.9%) than at the median (16.3%), and the effect of cleanup becomes statistically insignificant beyond the 60^{th} percentile. We find that examining the full housing value distribution can have important policy implications. In our example of the effect of Superfund cleanup, a focus on the median would have understated the larger effects at the lower tails of the housing values. One can imagine other situations in which the distribution of benefits is such that a focus on the mean or median could lead to a failure to detect any treatment impacts, should those impacts exist only in the tails of the distribution of housing values.

Finally, our block and tract-level results are confirmed by additional evidence from geocoded proprietary housing transactions data describing four large housing markets. These data show explicitly that it is the cheaper houses within each tract are more likely to be exposed to waste sites within one kilometer, explaining their greater appreciation from site cleanup.

Our method makes it practical to apply well-established hedonic techniques to value public goods with highly localized effects using unrestricted tract-level data. For at least two reasons, this approach has the potential for wide application. First, a growing number of studies document that amenities or disamenities are highly localized (e.g., at the sub-tract level) with effects that decline rapidly with distance. Davis (forthcoming) detects strong adverse effects of power plants on prices of houses that are located within two miles, weakening effects between two to five miles, and no effect beyond five miles. Campbell, Giglio and Pathak (2009) find that each nearby foreclosure lowers the price of a house by about 2% if it takes place at zero distance and 1% if it takes place at a distance of 0.05 miles. Rossi-Hansberg, Sarte and Owens (2010) find that subsidies for home improvements raise neighborhood housing prices, but that the effects halve every 1,000 feet and extend to only 2,500 - 3,500 feet. Linden and Rockoff (2008) report that, when a sex offender moves into a neighborhood, this reduces prices of houses within $1/10^{th}$ of a mile by 4%, but there are no effects beyond that distance. The results of these studies suggest that the effects of policy could be substantial (particularly in densely populated areas) but are likely to be missed by analyses focused on median values derived from coarse geographic definitions.

In practice, many hedonic studies are forced to rely on tract-level data because of its nationwide coverage and public availability (Hanna, 2007; Greenstone and Gallagher, 2008; Grainger, 2010). Finer resolution data alternatives are often inaccessible – house transaction data are proprietary and expensive, or limited in coverage, while block-level data have restricted access.[2] For these reasons, we expect the methodology described below to be particularly useful.

2 Potential Benefits from Superfund Cleanup

There are three major milestones in the NPL process – proposal, listing, and deletion – at which the EPA publicizes information about the site by entering information into the Federal Register and soliciting public comment. These milestones, by providing information to the housing market, have the potential to influence housing values. The NPL process begins with a

[2] Block-level Decennial Census data are restricted to users at the Census Data Centers (Davis, forthcoming) and (ii) individual house transactions data are proprietary and typically expensive (Oberholzer-Gee and Mitsunari, 2006) or limited in their coverage to specific counties (Davis, 2004; Linden and Rockoff, 2008; Pope 2008; Zabel and Guignet 2010).

preliminary assessment and site inspection; based on that assessment, the EPA may propose a site to the NPL in the Federal Register. Information collected during the preliminary assessment and site inspection is used to calculate a Hazard Ranking System (HRS) score.[3] The EPA then lists the site on the final NPL if it meets at least one of three criteria – (i) the HRS 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. Finally, 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.

There are two channels – one direct and one indirect – through which deletion from the NPL can increase housing values. First, cleanup reduces health risks and other disamenities associated with a site. Second, cleanup may prompt further development in the area surrounding a site, including the potential for re-zoning from a lower-value commercial use to higher-value residential (even luxury) development. As long as this sort of development occurs *conditional* on cleanup being undertaken, our study correctly considers the benefits from it to be part of the benefits from Superfund cleanup. For example, the Empire Canyon Daly West Mine Superfund site in Utah underwent extensive remediation under the Superfund program. After remediation, the landowners leased that site for the development of a luxury resort, including a hotel, spa and condominium project (EPA, 2008). This outcome represents one mechanism through which remediation can be translated into higher housing prices. Our analysis would be biased if causality went in the opposite direction – e.g., if landowners decided to build a luxury resort

[3] 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.

(which was going to raise nearby housing prices regardless of EPA actions), and the EPA responded by moving the site through the remediation process more rapidly. Our review of the literature does not suggest that this is the case. Moreover, our analysis employs (i) sample restrictions to ensure that we are making comparisons among tracts that are similar to one another aside from their receipt of cleanup, and (ii) panel methods to control for time-invariant unobservable differences in tracts.

Any hedonic estimates of the benefits of Superfund cleanup come with three caveats. First, benefits are understated if homeowners ignore downstream benefits. Second, the appreciation of housing values reflects homebuyers' perceptions of risk reductions, and these perceptions, though influenced by the information that EPA provides,[4] may not fully reflect true reductions in risks. Third, we cannot account for changes in housing values that result from the cleanup of nearby sites undertaken outside the Superfund program because data describing these sites are unavailable.[5] Fourth, like previous studies, we treat Superfund cleanup as a marginal change to the overall housing market.[6] With the assumption that the hedonic schedule does not shift, we can interpret our capitalization results in a marginal willingness-to-pay framework (Kuminoff and Pope, 2010). Finally, we follow the majority of the hedonics literature and simply analyse the value of marginal changes along the hedonic price function. We do not attempt to identify the marginal willingness to pay function, given the difficulties inherent in

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[4] For example, Gayer, Hamilton and Viscusi (2000) find that residents around seven NPL and non-NPL sites in Grand Rapids Michigan updated their perceptions of risks when the EPA released information about those sites from remedial investigations.

[5] The EPA does not maintain a list of verified coordinates of non-NPL sites. This data limitation has constrained other studies (Kiel and Williams, 2007; Noonan, Krupka and Baden, 2007; Greenstone and Gallagher, 2008).

[6] We assume that the cleanup of a single superfund site may not significantly alter the hedonic price function, unlike a policy that, for example, affects the quality of a high percentage of the schools in a district..

such a task; see Kuminoff, Smith and Timmins (2010) for a discussion. Bishop and Timmins (2011) describes these difficulties in more detail and suggest empirical approaches to deal with them.

While our study focuses on the effects of deletion from the NPL, we note that at least two other Superfund milestones can also influence nearby housing values. 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 increases in health risks (Hamilton and Viscusi, 1999)[7] and stigma (Fischoff, 2001, Messer et al., 2006) associated with a contaminated site.[8] Unlike proposal and deletion, listing of an NPL site 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 a site has been placed on the path towards remediation.

2.1 Previous Studies on Valuing Superfund Benefits

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),

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[7] Davis (2004) finds that information on health risks are capitalized into housing values – i.e. the emergence of a cancer cluster resulted in the depreciation in housing values in a Nevada county relative to those in a nearby county.

[8] Messer et al. (2006) note that "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."

EPA (2009) and Sigman and Stafford (2011). We briefly describe the hedonic approach that examines median housing values in locations that vary in the number or characterization of sites contained within.[9] 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, Krupka and Baden (2007) study the effect of Superfund remediation activities on housing values measured at the block-group level using a national sample, and employ an instrumental variables approach to separate direct and indirect effects of cleanup. 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 the contiguous US.

We build most directly upon Greenstone and Gallagher (2008) (hereafter GG), 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 particular, GG 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.[10]

Second, to control for unobservables that may be correlated with listing, GG apply a regression discontinuity (RD) design that draws on the early institutional history of the NPL. In

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[9] A second approach takes a particular site and determines how distance from it impacts the selling price of nearby homes. That effect is measured with a distance gradient that typically varies with site status (Kiel and Zabel, 2001; Kiel and Williams, 2007).

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

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 sites. GG'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.[11] We denote these scores completed in 1982 as the 1982 HRS scores.

2.2 Our Empirical Specification

Our estimation strategy compares blocks (or tracts) located within 3km of NPL sites that received the cleanup treatment with those within 3km of NPL sites that that were not cleaned. Using GG's insights on the 1982 HRS scores, we restrict our comparison to ever-proposed NPL sites whose 1982 HRS scores lie within a narrow interval. Beyond that, our estimation strategy differs in a number of important ways from that of GG. We contend that these differences avoid three potential sources of downward bias in GG (Smith, 2006).

First, our study measures the cleanup "treatment" by examining the effect of deletion from NPL, the Superfund milestone that signals the end of cleanup. This effect is measured

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[11] GG 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.

separately from that of NPL listing.[12] In contrast, GG examine the effect of "listing" – a variable that conflates two distinct milestones in the Superfund remediation process – listing and deletion. This conflation, which allows their instrumental variable to cleanly identify the effect of listing, comes at the cost of biasing downward their estimate of cleanup; listing has ambiguous overall effects on housing prices, while deletion is likely to raise housing prices.[13] Our approach, which measures deletion separately from listing, relies on panel methods to control for time-invariant unobservables.[14] We argue that our approach of measuring cleanup using the correct Superfund milestone of deletion (but without the GG IV strategy) will incur less bias than GG's IV approach of measuring cleanup by conflating the correct deletion milestone and the incorrect listing milestone.

Second, our study measures the "non-treatment" baseline using blocks (or tracts) near NPL sites that are not cleaned. The comparison between blocks lying near listed sites that are cleaned ("treatment" sites) and those lying near sites are listed but not yet cleaned ("non-treatment" sites) narrows the range of unobservables, thus yielding more accurate estimates of the benefits from cleanup. In contrast, as the "non-treatment" baseline, GG 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). That approach can lead to a download bias in the estimates of benefits. The non-NPL sites are likely associated with "better" unobservables than sites proposed to (but not

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[12] 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).

[13] GG 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.

[14] We describe evidence from previous studies as an indirect strategy to address concerns from time-varying unobservables.

listed on) the NPL. Thus, these non-NPL sites may elevate the baseline of housing values against which the benefit of treatment is measured.

Third, our study defines the neighborhood narrowly in space in order to capture the localized benefits of cleanup. Our tract analysis uses smaller 3km (≈2 miles) buffers around sites and tests the sensitivity of our results to larger 5km (≈3 miles) buffers. Our choice is based on panel data studies on the association between hazardous waste sites and housing prices that 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). In contrast, GG consider attributes of tracts falling within buffers of 3 and 5 miles around sites; this larger neighborhood definition may therefore encompass both houses that are affected and unaffected by cleanup, and thus dilute the estimated benefits of that treatment.[15]

3 Estimation Method

Our approach to measure *localized* benefits is two-fold. First, we measure these benefits directly using high geographic resolution data measured at the census *block level.* The block analysis reveals that benefits from cleanup are sizable but highly localized. Second, we demonstrate our proposed refinement of the hedonic method, aimed at providing more accurate estimates of localized benefits, when analysts are restricted to using publicly available tract-level data. We repeat our analysis at the tract-level, examining numerous points along the within-tract distribution of housing values (including, but not limited to, the median) in order to measure the heterogeneous appreciation of housing values in response to cleanup. Finally, we provide

[15] GG'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).

supplementary analysis using geo-coded house-level data to document the spatial pattern of housing values within tracts and their proximity to Superfund sites. Our tract-level finding that cleanup causes greater appreciation at the lower percentiles of within-tract house value distribution is consistent with our finding that Superfund sites lie in closer proximity to the lower-value houses within each tract.

3.1 Identification Strategy: Tract and Block Analyses

The tract and block analyses take snapshots of the NPL status of each site in 1990 and 2000.[16] We compare changes in owner-occupied housing values in census units lying in 3km buffers surrounding sites between 1990 and 2000 to changes in exposure to (i) sites that are 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

[16] Ideally, we would examine changes over a long enough time period to detect changes in housing prices, but over a short enough time period so that parameters of the hedonic price function are stable. Like other decennial census based studies, we are constrained by the decadal frequency in data collection. More frequently collected census data, such as the American Community Survey, is not collected at a sufficient level of density for our analysis.

with respect to their receipt of treatment – i.e., while some of the sites in these neighborhoods remained proposed or listed on the NPL, other sites were subsequently deleted from the NPL.

In our second sample, we build upon the GG 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.

Our third sample corrects for one additional confounder – tracts in the RD sample may lie in close proximity to other sites. 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 was outside the [16.5, 40.5] interval. Deleting tracts that violate this rule leaves our *strict* RD sample with 818 tracts surrounding 187 sites.

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. 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-invariant unobservables. As in GG, we rely on the RD approach primarily to identify the effect of listing. To identify the effect of deletion, we rely on the panel methods to further control for time-invariant unobservables.

4 Regression Models

4.1 Census Blocks - Specification

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

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

The baseline sample is comprised of blocks contained in all census tracts that have some overlap with the 5km buffer surrounding each NPL site.[17] The subscript k indexes these blocks. As our analysis measures counts of sites located within 3km from the centroid of these blocks, we label this the "3km" block sample. The dependent variable $lnH_{k,t}$ is the natural log of the median owner-occupied housing values in block k in year t $(t = 1990, 2000)$. The vector $X_{k,t}$ contains characteristics of the housing stock along with the socioeconomic and demographic attributes of the block. ν_k are time-invariant block-level unobservables, and $\varepsilon_{k,t}$ is an unobservable that varies with both year and block.

Our main variable of interest (i.e., exposure to Superfund sites that are deleted from the NPL) is defined by a count of deleted sites lying inside a radius of specified distance of the centroid of each block. 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 ($P^d_{k,t}$), listed ($L^d_{k,t}$), and deleted ($D^d_{k,t}$). We use four distances (i.e., < 0.25, < 0.5, <

[17] 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.

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 ($P_{k,t}^{d,\overline{d}}$), listed ($L_{k,t}^{d,\overline{d}}$), and deleted ($D_{k,t}^{d,\overline{d}}$). We simultaneously use four distance bands (i.e, 0-1, 1-2, 2-3, and 3-5km) in a single specification. The control variables in the block specifications include the first twenty variables listed in Table I.

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 block unobservables.[18]

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

Consider the specification with the counts of sites that lie within 0.25km from the block centroid. The coefficient β_3 measures the effect from the deletion of one additional site located within 0.25km of the block centroid, on median block housing values measured relative to the pre-proposal stage. The coefficients for other specifications are defined analogously. Recognizing the log dependent variable, a positive β_3 indicates that house values appreciate by

[18] The 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 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 measuring willingness-to-pay. Mastromonaco (2010), using high frequency transactions price data from Los Angeles, finds that stability of the hedonic price function is a reasonable assumption. With just two years of census data, we are not able to similarly test this assumption in our application.

$100\left[exp\left(\beta_3 - \frac{1}{2}V(\beta_3)\right) - 1\right]$ percent as a result of a one unit (i.e., 0 to 1) increase in the count of deleted sites. In practice, this transformation has little impact on our block and tract results, so we ignore it in order to simplify the discussion of our estimates.

4.2 Census Tracts - Specification

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.[19] The cross-section and panel regression models for census tracts are then defined analogously to equations (1) and (2), with the following modifications. First, within-tract percentiles of the house value distribution replace block median values.[20] Second, exposure is defined as the share of the land area in a tract that falls into 3km buffers surrounding NPL sites. 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.[21] The coefficient β_3^θ measures the appreciation of house values at the θ^{th} percentile as a result of a one unit (i.e., 0 to 1) increase in exposure of the tract to a deleted site. Table II

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[19] 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.

[20] Looking across deciles, we assume that the tract-level unobservable affecting the θ^{th} percentile house in 1990 is 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.

[21] Gamper-Rabindran, Mastromonaco and Timmins (2011) provides details on the calculation of tract exposures, including illustrative maps. In section 6.2 below, we describe how we handle situations in which a tract is simultaneously exposed to multiple sites at the same stage of remediation.

summarizes the interpretation of the coefficients. Table IIIB lists the control variables (X) in Equation (2).

5 Data

Data Sources

Restricted-access census block data for 1990 and 2000 are from the US Census Bureau. Proprietary housing transactions data are from Dataquick Information Systems. Census tract data come from the Geolytics Neighborhood Change Database, which has reapportioned census data from 1980, 1990 and 2000 into census tract boundaries that are fixed in 2000.

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.[22,23] 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.

Detailed data on all sites ever proposed to the National Priority List were provided by the EPA. 1982 HRS scores come from the dataset compiled and published by GG. We also use their dataset to construct an "intersection sample", which we use to carry out a direct comparison of our study with theirs (see section 6.4). 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.

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[22] For details on the intervals, see Gamper-Rabindran, Mastromonaco and Timmins (2011).
[23] We only consider the effect of NPL site remediation on residential property values. See Ihlanfeldt and Taylor (2004) for an analysis of the effects on commercial real estate values.

Summary Statistics

Table I reports summary statistics for census blocks that correspond to the all-NPL sample.[24] Tables IIIA and IIIB summarize the tract data in the RD and Strict RD samples.

Sites: All-NPL, RD and Strict RD

We begin with all sites that have ever been proposed to the NPL (before Jan 1, 2010) in the contiguous US. Of these, we exclude 10 sites that had been proposed to the NPL but whose date of proposal was not recorded.[25] Our final all-NPL sample contains 1,722 ever-proposed sites.[26] Our RD sample contains 221 sites scored in 1982 whose HRS scores are between 16.5 and 40.5. Our strict RD sample contains 187 sites. The flowcharts in Figures I-II show the progress of the strict RD and RD sites through the Superfund milestones.

6 Results

6.1 Block Results: Direct Evidence of Localized Effects

The analysis of median housing values at the block level provides direct evidence for the localized benefits from Superfund cleanup. Results using our strict RD sample are described in Table IV. Carrying a site through the remediation process to deletion from the NPL results in statistically significant appreciation of median house values, and appreciation is larger in blocks located nearer to the site. Median housing values appreciate by 19.9%, 19.0%, and 5.8% in

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[24] Summary statistics for the RD and strict RD block samples have not been released by the Census Bureau due to confidentiality reasons.
[25] The only information we have on these 10 sites is that they were eventually recorded as non-NPL sites.
[26] 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.

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 only few houses are located within that short distance to the Superfund sites (leading to imprecise estimates).

Considering the distance bands, 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.[27] Noteworthy, the magnitude of the appreciation for the median house value at blocks lying < 1km away from the site (19.0%) is comparable to that for the lower deciles of the house value distribution at the tract-level in Table VII (i.e., 18.1% to 20.9% between the 10th and 40th percentiles).

Given the smaller size of the strict RD sample, results may be sensitive to 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, suggesting preference-based sorting whereby those who choose to live some distance from the site (3-5km) have greater willingness-to-pay for remediation. Noteworthy, 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 1990 and 2000 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 range of unobservables, the larger RD and all-NPL samples have the advantage of larger sample sizes; results may be biased but less prone to the effects of outliers. Results from the RD sample,

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[27] Note that all four distance bands are included simultaneously in the same regression specification.

presented in Table V, indicate a smaller magnitude of appreciation than those from the strict RD sample. In particular, carrying a site through the remediation process to deletion 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 rates of only 7.8%, 11.2%, 2.9% and 2.9% in blocks lying 0-1km, 1-2km, 2-3km and 3-5km, respectively.[28] 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 VI 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.

Sensitivity analysis

We conduct two sets of sensitivity analyses. First, to address the possibility that errors are spatially correlated, we estimate standard errors clustered at the tract level. These results are compatible to those described in Tables IV-VI. Second, to investigate the spatial extent of the benefits, we repeat the block analysis with 5km buffers. 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. Results for these sensitivity analyses are available on request from the authors.

[28] Deletion causes smaller appreciation in median house in blocks lying closest to the site (0-1km) than in blocks lying slightly further away (1-2km). These results suggest preference-based sorting whereby those who choose to live closest to the site exhibit a lower willingness-to-pay for remediation.

Potential estimation issues

We discuss two potential estimation issues, which on examination, are not likely to negate our inference. First, identifying the effect of deletion will be made more difficult if ever-proposed sites that are eventually deleted differ systematically from ever-proposed sites that do not reach this milestone in our study period. However, Sigman's (2001) study of the pace of progress at Superfund sites, suggests that the extent of bias on our estimate of deletion is likely to be limited. In particular, our panel approach deals with time-invariant unobservable variables. 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 is not able to control for time-varying correlated unobservables, but factors modeled as time-varying in Sigman's study are found to have limited or no influence on sites' progress. Public funding, for example, 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 ROD to listing, Sigman (2001) notes that most funding for cleanup at this stage comes from PRPs under their agreement with the EPA.

A second 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 other sites in general. Our estimation strategy addresses this issue using fixed effects to control for time-invariant unobservables, and by comparing sites that are as similar in terms of their 1982-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 with observed neighborhood characteristics. For instance, 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 zip-code level (Gupta et al., 1996). Similarly, 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; 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.[29] However, the state-level source of variation in the extent of cleanup does not bias our study because we do not systematically compare cleanup in sites located more stringent states relative to sites yet to be cleaned located less stringent states.

6.2 Tract Results: Indirect Evidence of Localized Benefits

Our block results establish that benefits from Superfund cleanup are large but localized. Repeating our analysis at the tract level, we find that the localized effects, detected at the block level, are manifest at the tract level. In particular, we find the appreciation of housing values vary within the tract, with greater percentage appreciation in the lower tail of the housing price distribution.

[29] 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. In practice, the cleanup goal is more stringent. 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 and states' standards if stricter.

Our preferred estimates are from the panel analysis of tracts that are in the strict RD sample. Observations are weighted by tract counts of owner-occupied housing units, and robust standard errors account for heterosekedacity. These results are presented in Table VII. 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 18.1% to 20.9% between the 10th and 40th percentiles, and by 16.3% at the median and 12.2% at the 60th percentile. 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 $6,605 at the 10th percentile, $8,923 at the 30th percentile, and $8,244 at the median. Deletion does not lead to a statistically significant appreciation in housing values at the 60th and higher deciles of the within-tract distribution.

Table VIII shows results from the panel specification for the RD sample. Similar to the results from strict RD sample, we find that appreciation (measured in percentages) in the RD sample is concentrated in the lower percentiles. Specifically, we find evidence of 30.8% appreciation at the 10th percentile, 22.5% at the median, and 16.9% at the 90th percentile. While the coefficients in the strict RD are statistically significant up to the 60th percentile only, the RD estimates are statistically significant even for the upper percentiles.

Deletion of sites from the NPL – Sensitivity Analysis

We examine a variety of alternative specifications for the strict RD and RD samples. First, we estimate an unweighted specification. Results from those regressions, presented in Table IX 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 errors are spatially correlated, we estimate clustered standard errors. Ideally, we should cluster the standard errors over groups of tracts that lie in close proximity, 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 IX columns (7)-(9) and (10)-(12) present standard errors clustered at the county level, estimated for the strict RD sample and the RD sample, respectively. 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 to create the strict RD and RD samples. Table X columns (1)-(3) reveal that using 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 VII, where neighborhoods are defined narrowly using 3km buffers, suggests that the larger neighborhood definition lumps nearby affected houses with more distant unaffected houses, thereby diluting the effects of deletion. For the RD sample, these larger definitions of neighborhoods near NPL sites still yield statistically significant appreciation effects of deletion (Table X, columns (4)-(6)), but these point estimates are about half the magnitude of those measured with the narrower 3km definition of neighborhoods (seen in Table VIII).

Finally, for comparison, we show results from the all-NPL sample in Table XI (with and without weights). The all-NPL sample does not control for unobserved heterogeneity between treated and untreated tracts as do the RD and strict RD sample. We find estimates that are much smaller in magnitude in the all-NPL sample (i.e., about 6.0% at the median and 6.1% at the 80th percentiles in the weighted regression and about 3.2% at the 80th percentile in the unweighted regression).

Potential Estimation Issue: Defining exposure to overlapping sites

We consider one potential estimation issue in the tract analysis arising from our definition of 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: (i) overlap with the buffer of the first NPL site, (ii) overlap with the buffer of the second NPL site, and (iii) overlap with the area of intersection between the buffers of the first and second NPL sites. We calculate the tract's exposure to sites at this particular level of remediation as the sum of areas (i), (ii) and (iii). In other words, we do not double-count the area of intersecting overlap (iii). 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.

This potential problem turns out not to be an important practical issue in our preferred (i.e., strict RD) sample. In particular, we consider 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. This happens in very few tracts. Specifically, in only 5 instances out of 1,636 tract x year combinations (i.e., 818 tracts observed in two different 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.

6.3 Supporting Evidence for Tract Analysis From House-Level Data

Our tract results are consistent with the observation that NPL sites are located closer to the lower-value houses within each tract. We provide evidence for this spatial distribution using geo-coded transactions data between from four housing markets – metro Los Angeles (for transactions between 1988 and 2008); metro Boston, southwestern Connecticut, and northern New Jersey (for transactions between 1988 and mid-2009).[30] We first make housing transactions

[30] These four cities were chosen because data were available for an extended period that includes a great deal of Superfund activity. Data for each of the housing markets includes the following counties: northern NJ (Sussex, Passaic, Bergen, Warren, Morris, Essex, Hudson, Hunterdon, Somerset, Union, Middlesex, Mercer, and Monmouth), Boston (Essex, Middlesex, Norfolk,

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 (at any stage of remediation) within one kilometer 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).

As seen in Figure III, in three out of the four markets, there is stark evidence that houses in the lowest deciles of the within-tract house value distribution are more likely to be exposed at close distances 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 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 second decile (5.5% more likely) while the lowest comes at the sixth, eighth, and ninth,

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Plymouth, and Suffolk), southwestern CT (Fairfield, New Haven), and Los Angeles (Ventura, San Bernadino, Orange, Los Angeles, and Riverside). See Gamper-Rabindran, Mastromonaco and Timmins (2011) for details.

which are all 4.7% less likely than the average to be exposed. Together, these results confirm that the patterns we recover in the tract-level analysis are being driven by the heterogeneous exposure across the within-tract distribution.

6.4 Comparison with Previous Research

We provide a comparison of our method with that of Greenstone and Gallagher (GG) to the extent possible given the limitations posed by our different samples and methods. These differences are listed in Table XII.[31] We can replicate most closely GG's RD specification, which relates sites' statuses to tract attributes on which these sites are located. Details on the construction of an "intersection" sample are provided in Table XIII. This exercise provides support for our method: to identify the effects of cleanup, we find that it is necessary to examine the effect of deletion at the lower deciles of the housing value distribution and to apply panel methods that control for time-invariant unobservables. When we apply GG's IV approach to the intersection sample, we find, as they do, that "listing" does not raise median housing values. However, when we apply our method to the intersection sample, we find (as we do in our main analysis) that deletion raises the lower deciles of the within tract housing values.

Here, we estimate regression models that are comparable to GG's model, which relates 2000 tract housing values to 1980 tract characteristics including the 1980 mean housing values. As an aside, we choose not to apply this specification in our main analysis out of concern over its inherent bias.[32] We begin by applying the GG method to the "intersection" sample – i.e., using

[31] For details, see Gamper-Rabindran, Mastromonaco and Timmins (2011).
[32] GG argue that the 1980 tract attributes are correlated with the 2000 attributes, but are pre-determined with respect to Superfund site status. However, the GG specification may lead to biased parameter estimates, as described in Appendix A3 of Gamper-Rabindran, Mastromonaco and Timmins (2011). There, we derive the GG specification from underlying 1990 and 2000

[HRS82>28.5] to define an instrument for listing on the NPL by 2000 ([NPL_{2000}=1]).[33] However, the instrument has two drawbacks in the intersection sample. First, as seen in Table XIV, the instrument exhibits a t-statistic of 2.38 in the first stage relationship, indicating a weak-instruments problem (Staiger and Stock, 1997). Second, the instrument cannot separately measure the effects of listing and deletion.

We next apply our method to the intersection sample in three progressive steps. (i) We introduce the panel approach in Table XV row (3), 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 in row (4) 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 ($\Delta L_{2000\text{-}1990}$) and changes in exposure to deletion ($\Delta D_{2000\text{-}1990}$). (iii) We examine not only the median, but also the upper and lower deciles 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.

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hedonic price functions. We find that the resulting regression error will likely be correlated with the key variables appearing on the right-hand-side of the regression. In particular, the resulting regression error will contain the unobserved determinants of the 1980 median house value, so that the 1980 median housing value will naturally be correlated with it. Because year 2000 covariates are relegated to the regression error, 1980 covariates will be correlated with the error term as well if they are correlated with the 2000 covariates, which we might expect to be the case. The main variable of interest (year 2000 NPL status) will also be correlated with the regression error term if it is correlated with year 2000 covariates.

[33] Our 1980 covariates, listed in Table XIV correspond to those used in GG with one exception – we were unable to include the percentage of households with air conditioning in our list.

Table XV rows (1) and (2) indicate that when we apply the IV approach to the intersection sample, we obtain results comparable to those in GG.[34] The listing on the NPL in 2000, instrumented by the 1982 HRS score, does not lead to statistically significant appreciation in housing values. In contrast, Table XV row (4) indicates that when we apply all three features of our method to the intersection sample (i.e., the panel strategy, separate estimation of listing and deletion, and examination of housing values at the lower quantiles), we find results that are comparable (albeit at smaller magnitudes) to those obtained in our main analysis of our RD and strict RD tract samples. 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 (keep in mind the relatively small size of the intersection sample). There is, however, no statistically significant effect at or above the 30^{th} percentile. We conclude that results in the "intersection" sample are similar to those derived from our main sample (i.e., when we apply our method to our strict RD and RD samples). Both analyses find that deletion leads to greater appreciation at lower deciles but detect no such evidence at the upper deciles. Nevertheless, the magnitudes of appreciation in the lower deciles are smaller in our "intersection" sample than in our main strict RD and RD samples.

[34] Our coefficient in Table XV Row 1 Column 3 is comparable to that in GG's Table IV Panel A Column 7. Both coefficients are positive but insignificant. Our point estimate is slightly larger than that estimated by GG. 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.

6.5 Other Superfund Milestones

Listing of a site on the NPL

Listing on the NPL leads, in general, to smaller appreciation in housing values than does deletion. The block-level 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 IV, 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 V, the effect is approximately 4% in blocks lying < 1km and < 3km from the site. The tract analysis finds no statistically significant effect for listing in the strict RD sample (Table VII), unlike our earlier results for deletion. The tract analysis does, however, find statistically significant effects of listing in the RD sample, but the magnitude of appreciation is smaller than for deletion. In particular, the RD tract results in Table VIII show that listing leads to an appreciation of 16.5% at the 10th percentile, 9.9% at the median, and 8.2% at the 80th percentile.

The smaller magnitude of the appreciation from listing compared to deletion can be explained by the countervailing pressures on housing values that arise when a site is listed – i.e., listing reduces housing values by confirming the severe nature of site contamination, but it also increases housing values by signaling that the site will be remediated.[35] Nevertheless, the

[35] 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 final listing stage. The positive effects of final listing that we find in the RD sample could be indicative of value that the housing market places on construction complete.

promise of cleanup associated with final listing appears to outweigh the effect of confirming a site's contamination level.

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 all-NPL block and tract samples provide consistent evidence of depreciation resulting from the proposal of a site. Reaching proposal status, seen in Table VI, results in a depreciation of -2.0% in blocks lying < 1km and -0.6% in blocks < 3km from the site. Depreciation measured using the tract sample ranges from -6.8% to -8.9%, as seen in Table XI. The RD tract analysis, seen in Table VIII, indicates that proposal results in depreciation in housing values ranging from -$1,264 at the 40 percentile to -$7,395 at the 90th percentile, but these point estimates are not statistically significant. However, the results from the RD block sample (Table V) provide a counterintuitive finding that proposal leads to appreciation in housing values of 5.2% in blocks lying < 3km from the site.

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. These results are, therefore, specific to these sites and may not extend to other sites and tracts. The depreciation in the strict RD block sample ranges from -24.7% to -37.3% in blocks lying < 1km and < 3km from the site (see Table IV) and the depreciation in the strict RD tract sample ranges from -40.7% to -46.0% (see Table VII). 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 samples because of the fairly sizable variation in the exposure to listed and deleted sites over the

1990s time period (as seen in Figure I). Nor does this issue arise in RD samples (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 provided new information to the market about the presence of a harmful site, or about its severity. 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).[36] Nevertheless, the proposal-induced depreciation is more than offset at the lower tails of the within tract housing values by the time the sites are deleted. As described in section 6.1, our estimated coefficients on deletion, which measures 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, leads to an overall appreciation in housing values.

7 Conclusion

Our study shows that benefits from remediating environmental disamenities, exemplified by Superfund remediation, can be highly localized. Restricted access block data at fine level of geographical resolution provides direct evidence on this point. We find that owner-occupied housing values appreciate by 19.0% for blocks lying < 1km from remediated sites but by only 5.8% for blocks lying < 3km from those sites. The localized nature of these benefits (e.g., at the sub-tract level) have important methodological implications for analysts who are forced to rely to publicly available data that are typically published at coarse geographical resolution (e.g. at the

[36] The revelation of new information to the market and the subsequent depreciation in housing values is not a cost that is appropriately attributed to the Superfund program. 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.

tract-level). In particular, the analyst must consider heterogeneity within those units, paying particular attention to the tails of the housing value distribution. Otherwise, the standard hedonic approach of focusing on *median* housing values may understate or fail to detect these benefits.

Our proposed refinement to the hedonic method – the consideration of the entire distribution of the housing values within the tract – enables the possible detection of localized benefits at the sub-tract level using tract-level data. When we apply this refined method to the evaluation of the benefits from Superfund remediation, along with several changes to address other estimation issues, we find results that stand in contrast to Greenstone and Gallagher's (2008) tract-level study. While they conclude that cleanup of Superfund sites yields little or no benefits to nearby *median* property values, we detect significant within-tract heterogeneity. Cleanup causes greater appreciation (in percentage terms) of housing prices at the lower deciles – i.e., by 20.9% at the 10th percentile, 16.3% at the median, and 12.2% at the 60th percentile of the within tract distribution and no statistically significant effect beyond the 60th percentile. One can imagine other situations in which the distribution of benefits is such that a focus on the mean or median could lead to a failure to detect any treatment impacts, should those impacts exist only in the tails of the distribution of housing values.

Our further investigation reveals that within-tract heterogeneity is partly explained by the spatial distribution of Superfund sites. Geo-coded house transaction data for three out of four metro areas indicate that the closer proximity of these sites to cheaper houses within the tracts can explain the larger (percentage) appreciation of housing values at the lower deciles. Noteworthy, the 18.1% to 20.9% appreciation detected at the 10th to 40th percentiles of housing values at the tract-level correspond closely to the 19.0% appreciation for blocks lying < 1km from remediated sites.

Our study and GG's study reinforce each other in one important way – taken together, they rule out the case that the benefits from a cleanup, measured as capitalization into housing values, are manifest across a large area. Our finding that benefits are highly localized within the tract – relative to the case if benefits were to appear over a larger area – may make it more difficult for the aggregate benefits of a cleanup to exceed the costs. Whether they do or not depend largely on nearby population density and is an empirical question to be addressed should good estimates of costs become available (GAO, 2010).

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. Nor can analyses like that conducted here detect heterogeneity in the valuation of cleanup across housing markets that may be evident in house-level data (Gamper-Rabindran, Mastromonaco and Timmins, 2011; Kiel and Williams, 2007).[37] 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.

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[37] Consistent with results in our present study that deletion raises housing values, on average, across the national sample of sites, our house-level analysis in Gamper-Rabindran, Mastromonaco and Timmins (2011) finds that deletion (measured relative to proposal) causes a sizable appreciation in housing values in northern New Jersey (11.3%). Further, our house-level analysis detects considerable heterogeneity in the effects of cleanup across housing markets, with sizable appreciation in New Jersey and no statistically significant effects in 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 associated with contaminated sites and neighborhoods can also persist despite cleanup.

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TABLE I
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 II

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 IIIA

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 IIIB

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.
Co-variates								
% 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 IV

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 V

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 VI

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 VII

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	-0.422***	-0.407***	-0.425***	-0.415***	-0.429***	-0.458***	-0.460***	-0.418***	-0.424***
	(0.147)	(0.119)	(0.115)	(0.115)	(0.115)	(0.118)	(0.120)	(0.125)	(0.136)
Δ Listing	0.050	0.069	0.065	0.061	0.046	0.017	0.006	-0.004	-0.044
	(0.079)	(0.055)	(0.054)	(0.054)	(0.051)	(0.053)	(0.055)	(0.057)	(0.071)
Δ Deletion	0.209**	0.208***	0.191***	0.181***	0.163**	0.122*	0.094	0.085	0.050
	(0.095)	(0.072)	(0.070)	(0.069)	(0.068)	(0.069)	(0.070)	(0.071)	(0.084)
R-sqr	0.324	0.357	0.353	0.368	0.367	0.364	0.357	0.358	0.328
Panel B: Dependent variable Δ Price level of owner-occupied housing units									
Percentile	10	20	30	40	50	60	70	80	90
Δ Proposal	-14,230**	-16,451***	-19,043***	-20,254***	-25,740***	-27,631***	-27,722***	-26,163**	-31,229**
	(5,780)	(6,009)	(6,481)	(7,125)	(8,721)	(9,539)	(10,070)	(11,312)	(14,615)
Δ Listing	2,665	4,031	4,111	4,142	2,988	1,646	1,159	93	-6,042
	(2,617)	(2,633)	(2,857)	(3,082)	(3,235)	(3,707)	(4,061)	(4,599)	(7,055)
Δ Deletion	6,605*	8,358**	8,923**	9,083**	8,244*	7,080	6,845	6,917	2,341
	(3,505)	(3,614)	(3,946)	(4,305)	(4,696)	(5,239)	(5,602)	(6,335)	(9,167)
R-sqr	0.278	0.298	0.301	0.295	0.275	0.264	0.252	0.250	0.227

Notes: Control variables are listed in Table II. No obs.=818. Robust standard errors in parentheses. Statistically significant at * 10%; ** 5%; *** 1%.

TABLE VIII

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: ΔLog price of owner-occupied housing units									
	10	20	30	40	50	60	70	80	90
Δ Proposal	0.054	0.028	0.014	0.007	-0.002	-0.007	-0.013	-0.015	-0.021
	(0.040)	(0.038)	(0.038)	(0.038)	(0.038)	(0.037)	(0.037)	(0.035)	(0.035)
Δ Listing	0.165***	0.132***	0.121***	0.109**	0.099**	0.092**	0.092**	0.082*	0.053
	(0.046)	(0.044)	(0.045)	(0.046)	(0.046)	(0.046)	(0.046)	(0.044)	(0.043)
Δ Deletion	0.308***	0.264***	0.251***	0.237***	0.225***	0.214***	0.204***	0.195***	0.169***
	(0.051)	(0.048)	(0.049)	(0.049)	(0.049)	(0.049)	(0.049)	(0.046)	(0.046)
R-sqr	0.259	0.285	0.283	0.291	0.292	0.289	0.285	0.292	0.272
Panel B: Dependent variable: ΔPrice of owner-occupied housing units									
	10	20	30	40	50	60	70	80	90
Δ Proposal	3,328	1,075	-242	-1,264	-2,877	-3,840	-5,556	-6,348	-7,395
	(2,177)	(2,253)	(2,443)	(2,656)	(2,894)	(3,186)	(3,744)	(4,135)	(5,099)
Δ Listing	6,160***	4,876**	4,583**	3,963	3,527	2,860	2,236	1,310	-2,928
	(1,802)	(2,062)	(2,304)	(2,516)	(2,713)	(2,929)	(3,152)	(3,348)	(4,169)
Δ Deletion	10,867***	10,371***	10,786***	10,621***	10,690***	10,468***	10,129***	9,898***	6,830
	(2,100)	(2,351)	(2,608)	(2,828)	(3,068)	(3,298)	(3,541)	(3,801)	(4,778)
R-sqr	0.217	0.239	0.246	0.245	0.235	0.228	0.216	0.215	0.201

Notes: Control variables are listed in Table II . No obs.=1454. Robust standard errors in parentheses. Statistically significant at * 10%; ** 5%; *** 1%.

TABLE IX

PANEL ANALYSIS (1990-2000) OF THE TRACT STRICT RD AND RD 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*	-0.407**	-0.429**	-0.418**	0.028	-0.002	-0.015
	(0.139)	(0.118)	(0.125)	(0.040)	(0.038)	(0.034)	(0.167)	(0.165)	(0.174)	(0.077)	(0.077)	(0.069)
Δ Listing	0.103	0.061	0.021	0.191***	0.102**	0.120***	0.069	0.046	-0.004	0.132	0.099	0.082
	(0.065)	(0.056)	(0.059)	(0.048)	(0.044)	(0.042)	(0.100)	(0.093)	(0.104)	(0.094)	(0.096)	(0.089)
Δ Deletion	0.208***	0.168**	0.109	0.331***	0.239***	0.248***	0.208	0.163	0.085	0.264**	0.225**	0.195*
	(0.079)	(0.070)	(0.074)	(0.054)	(0.047)	(0.046)	(0.129)	(0.125)	(0.129)	(0.108)	(0.108)	(0.099)
R-sqr	0.256	0.310	0.322	0.206	0.245	0.258	0.357	0.367	0.358	0.285	0.292	0.292
Obs.	818	818	818	1454	1454	1454	818	818	818	1454	1454	1454

Notes: Control variables are listed in Table II. Statistically significant at * 10%; ** 5%; *** 1%.

TABLE X

PANEL ANALYSIS (1990-2000) FOR TRACTS WITHIN 5KM BUFFERS OF NPL SITES

	(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	-0.572***	-0.559***	-0.544***		-0.033	-0.038*	-0.049**
	(0.111)	(0.107)	(0.110)		(0.020)	(0.020)	(0.019)
Δ Listing	-0.028	-0.073*	-0.114***		0.063**	0.056**	0.040*
	(0.038)	(0.038)	(0.038)		(0.025)	(0.024)	(0.024)
Δ Deletion	0.044	-0.014	-0.061		0.131***	0.120***	0.101***
	(0.047)	(0.046)	(0.046)		(0.025)	(0.025)	(0.024)
Obs.	1131	1131	1131		2673	2673	2673
R-sqr	0.377	0.367	0.356		0.288	0.294	0.293

Notes: Control variables are listed in Table II. Robust standard errors in parentheses.
Statistically significant at * 10%; ** 5%; *** 1%.

TABLE XI

PANEL ANALYSIS (1990-2000) OF THE TRACT ALL-NPL 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	-0.068***	-0.079***	-0.075***	-0.084***	-0.089***	-0.076***
	(0.012)	(0.012)	(0.012)	(0.013)	(0.013)	(0.012)
Δ Listing	0.012	0.013	0.009	-0.016	-0.016	-0.020*
	(0.011)	(0.010)	(0.010)	(0.012)	(0.011)	(0.011)
Δ Deletion	0.043**	0.060***	0.061***	-0.010	0.023	0.032*
	(0.018)	(0.017)	(0.016)	(0.020)	(0.018)	(0.018)
R-sqr	0.236	0.248	0.248	0.136	0.176	0.180

Notes: Control variables are listed in Table II. No. obs=9672. Robust standard errors are in parenthesis. Statistically significant at * 10%; ** 5 %; *** 1%.

TABLE XII

COMPARISON OF OUR STUDY WITH GREENSTONE AND GALLAGHER (2008)

1. Research	Measure the effect of deletion	Measure the effect of listing
Question	relative to the pre-proposal stage.	versus non-Listing on the NPL.
2. Treatment	Deletion from the NPL (signalling the	Listing on the NPL (an intermediate
examined	end of cleanup).	Superfund milestone).
3. Non-treatment	NPL sites at the pre-proposal	NPL sites that are unlisted
baseline	stages.	and non-NPL sites.
4. Outcome	Distribution of housing values	Median tract-level housing values.
variable	within the tract.	
5. Unit of	Tracts	Sites.
observation	i.e. whose centroids fall within the	Housing and other attributes from
	buffer around the sites.	(i) tracts on which sites are located, or
		(ii) average attribute of tracts that
		overlap with the buffer around the site.
6. Sites in the	221 sites.	227 sites.
Regression	Begin with 1722 sites that are ever	Begin with 676 sites that were scored
Discontinuity	proposed to the NPL by 1/1/2010.	in 1982 (HRS-82).
(RD) sample	Then restrict to 221 sites that received	Then restrict to 332 sites that received
	16.5 ≤ HRS-82 ≤ 40.5.	16.5 ≤ HRS-82 ≤ 40.5.
		[95 out of 332 sites dropped due to
		missing 1980 variables.]
7. Affected	Buffer of 3km (≈2 miles)	Buffer of 3 miles
neighborhoods	and 5 km (≈3 miles).	and 5 miles.
8. Measure of	Share of tract that overlap with	Binary indicator :
exposure to	buffers surrounding NPL sites	1= site is listed by 2000 and
treatment	i.e. whether proposed, listed, or	0 otherwise.
	deleted.	
9. Estimation	Panel analysis (1990-2000).	HRS-82 serves as the instrumental
model	to measure separately the milestones	variable for Listed
	of proposal, listing and deletion.	on the NPL in 2000.

TABLE XIII
STEPS IN CREATING AN INTERSECTION SAMPLE BETWEEN OUR STUDY AND GREENSTONE AND GALLAGHER (2008)

	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.
	Then restrict to 332 sites that received	Then 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	There are 65 non-NPL sites in the sample.	There are no NPL sites in the sample.
Treatment	NPL2000=1 if the site is listed or 0 if unlisted.	NPL2000=1 for tracts exposed to listed or deleted sites
definition	Listed=Sites listed or beyond the listing milestone.	in 2000.
	Not listed=NPL sites that are pre-proposed	NPL2000=0 for tracts exposed to NPL sites proposed
	or proposed and non-NPL sites.	in 2000 or still pre-proposed in 2000.

TABLE XIV

FIRST-STAGE REGRESSION, GREENSTONE AND GALLAGHER IV PROCEDURE

DEPENDENT VARIABLE = NPL2000, N = 158, $R^2 = 0.501$

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 NPL2000= 0.981. *** significant at the 1% level, ** 5% level and * 10% level.

TABLE XV

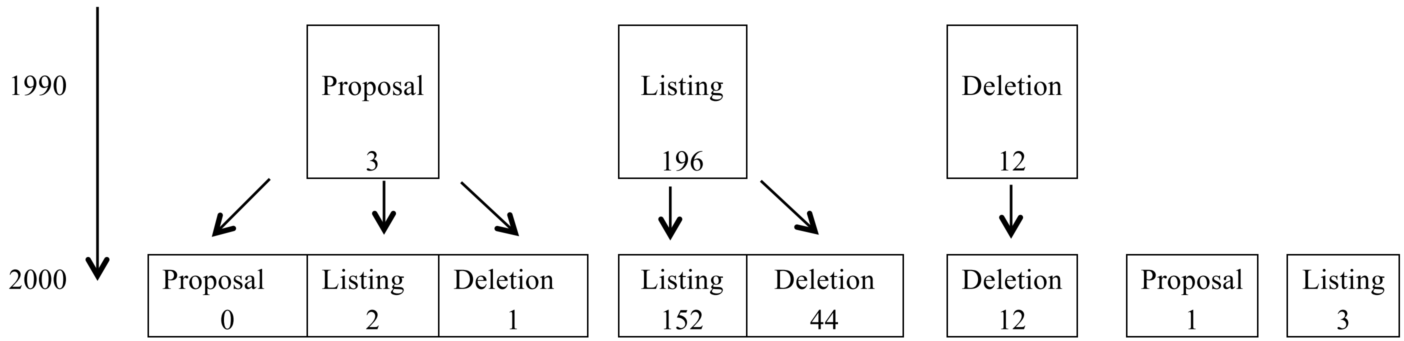
COMPARISON OF OUR METHOD WITH GREENSTONE AND GALLAGHER'S (2008)

				[1]	[2]	[3]	[4]	[5]	State
Row	Method	Dependent	Variable			Percentile			Fixed
		Variable	of Interest	10	20	50	80	90	Effects
1	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)	
2	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)	
3	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)	
4	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 listed in Table XIV. No obs=158. †p-value=0.110 and §p-value=0.114. Standard errors in parentheses.

FIGURE I

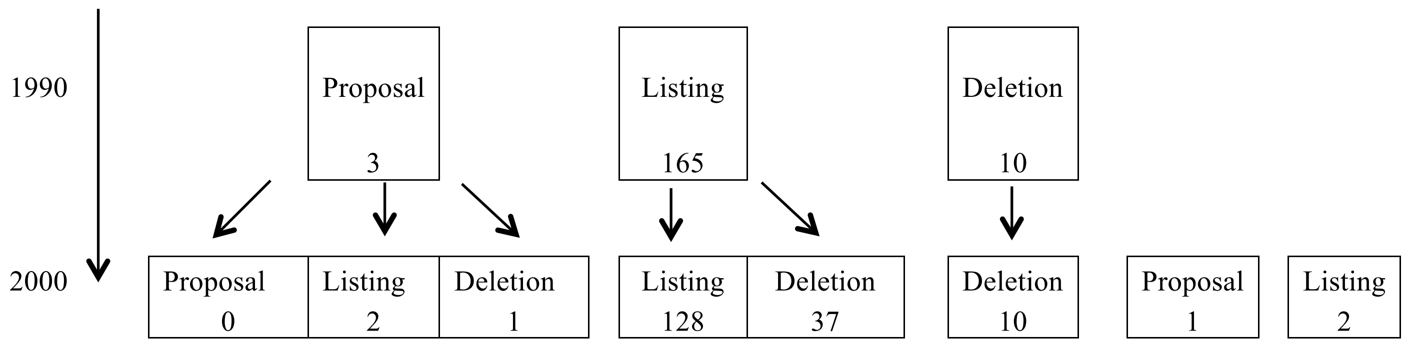
RD SITES PROGRESS THROUGH THE SUPERFUND MILESTONES



Notes: 6 sites were proposed to the NPL and removed from the NPL before 1990.

FIGURE II

STRICT RD SITES PROGRESS THROUGH THE SUPERFUND MILESTONES



Notes: 6 sites were proposed to the NPL and removed from the NPL before 1990.

FIGURE III

EXPOSURE TO NPL SITES BY WITHIN-TRACT PRICE DECILE IN FOUR HOUSING MARKETS (0 – 1 KM EXPOSURE)