Non-Market Valuation in Equilibrium

by

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Dissertation submitted in partial fulfillment of the requirements for the degree of
Doctor of Philosophy in the Department of Economics
in the Graduate School of Duke University
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Abstract

This dissertation investigates the non-market value of environmental quality in several contexts with attention paid to equilibrium effects. Chapter One contributes to the ongoing debate concerning the effect of various actions taken by the U.S. Environmental Protection Agency under CERCLA, commonly known as the Superfund Program, on housing prices. The study differs from national sample analyses and site-specific analyses by providing policy-relevant estimates of the hedonic price function in a particular region for the average site. Further, an estimate of the effect on housing prices is given for each of the major events that occur under a typical Superfund remediation. Using house and time-varying census tract fixed effects, I find a 7.3% increase in sales price for houses within 3 km of a site that moves through the complete Superfund program. The analysis gives evidence of positive price appreciation for housing markets and serves as a lower bound for measuring remediation benefits. Chapter Two proposes a new dynamic general equilibrium model of residential location choice with social spillovers and uses it to evaluate the equilibrium consequences of changes in pollution exposure. In particular, I investigate the hypothesis of "minority move-in," which postulates that disproportionate exposure to pollution results from minorities and low-income households trading off such exposure for lower housing costs. Second, I address the question of whether economic incentives caused by differences in willingness to pay across socioeconomic status can explain why polluters disproportionately locate near disadvantaged populations in order to minimize expenses from collective action bargaining over the negative externality. Simulations indicate "minority move-in" likely does account for some of the imbalance in expo-

sure to pollution across socioeconomic status. Further, general equilibrium estimates reveal that equilibrium sorting behavior widens the gap in willingness to pay for environmental quality between minority and white households, and between high and low-income households. The disparity in general equilibrium willingness to pay to avoid toxic emissions provides economic incentives for polluters to target disadvantaged populations. Chapter Three investigates how information contained in the U.S. Environmental Protection Agency's Toxic Release Inventory program affects prices in the housing market. First, I use a reduction in the reporting requirement threshold in 2001 as a quasi-experiment to determine whether prices change for existing firms who, as a result of the change, must report. Second, the existence of a reporting threshold creates a discontinuity in treatment than can be exploited. I estimate a regression discontinuity model that assumes that site unobservables are balanced in a neighborhood of the discontinuity. Using a difference-in-differences estimator for the first specification, I find that listing a site in the Toxic Release Inventory lowers prices by 3.1% within a three kilometer radius of the site, and that the effect is stronger at shorter distances. The regression discontinuity model produces qualitatively similar results that are smaller in magnitude but still significant. The results suggest that households to capitalize the information contained in the Toxic Release Inventory. However, since the treatment sites under consideration have virtually no emissions, these results do not contradict previous findings in the literature that toxic air emissions are unrelated to prices. Rather, they suggest that households might be concerned about the dangers of toxic chemicals that might result from an emergency or catastrophic accident.

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1

Introduction

Non-market valuation aims to assign value or worth to things which are not explicitly bought and sold in markets. Often, economists are interested in the marginal value of attributes or amenities of products or property which are "inseparable". Examples might include the dollar value of an additional unit of horsepower in an automobile or the willingness to pay for an extra bathroom in a single family home. Intuitively, if the econometrician can observe many different cars or different houses selling at different prices, it might be possible to estimate these marginal values based on the covariance in characteristics and transaction prices.

The three separate chapters in this dissertation are all exercises in non-market valuation. Chapters One and Three both seek to measure the marginal value of living in proximity to either hazardous waste sites or toxic air emitting facilities. These studies seek a partial equilibrium estimate of the value of distance. A partial equilibrium framework tries to determine the marginal value of a unit of some characteristic or amenity, *holding all else equal*.

In many circumstances, a partial equilibrium framework will suffice to inform the economist of the values that are under question. However, if the researcher wants to know what effect a non-marginal change in an amenity (like the addition of several toxic facilities into one neighborhood) partial equilibrium analysis may miss important feedback effects. In such cases, its not feasible that the change in question would leave *all else equal*, and any feedback effects would need to be attributed to the change in amenity.

Chapter Two proposes an equilibrium framework that allows for the estimation of general equilibrium willingness to pay for neighborhood amenities. In that study, I build a framework that allows households to care not only about the level of pollution in their neighborhood, but also the identity of their neighbors. When trying to value a non-marginal change in the number of toxic facilities in the neighborhood, attention must be paid to how the neighborhood demographics might change overtime, especially if households of different races and incomes have different preferences over pollution and housing prices.

I demonstrate that general equilibrium feedback effects can have significant impacts on willingness to pay estimates. White and minority households, as well as rich and poor households, are estimated to have different preferences over toxic air emissions. For a large counterfactual change in emissions and facilities, these differences in preferences cause the neighborhood demographics and prices to shift, which in turn implies feedback effects for household utility. I demonstrate that these effects can be on the order of 50% greater than traditional partial equilibrium estimates. The method is flexible enough to be applied to other situations and contexts.

Non-market valuation is a powerful element of the applied micro-economist's toolkit. In many situations, partial equilibrium or marginal valuation techniques are sufficient. In environmental economics, oftentimes valuations for non-marginal changes in the level of environmental quality are the object of research. The methods that follow can be used to gain insight into the valuation that society has for environmental quality.

2

Hazardous Waste Hits Hollywood: Superfund and Housing Prices in Los Angeles

2.1 Introduction

The valuation of environmental disamenities has long been of interest to economists and policy makers. First, there has been a desire to conduct proper cost-benefit analysis for environmental remediation activities. This was manifested in Executive Orders 12291 and 12866, issued by Presidents Reagan and Clinton, respectively, requiring a thorough cost-benefit analysis of regulatory actions undertaken by the federal government (Reagan (1981); Clinton (1993)). Second, accurately assigning a value to a non-marketed amenity or disamenity poses a non-trivial challenge. One such disamenity that has received much attention in the literature is the so-called "Superfund" program, enacted by Congress in 1980 via the Comprehensive Environmental Response, Compensation, and Liability Act (CERCLA). Managed by the U.S. Environmental Protection Agency (EPA), the Superfund program identifies and coordinates hazardous waste remediation and has accountable costs. However, the actual welfare benefit of action is under dispute as economists continue to debate the existence of observable benefits.

In order to place a value on non-marketed amenities, such as distance from hazardous waste sites, economists often employ hedonic pricing models of the housing market. In such models, price is regressed on a vector of attributes of each house that is sold. In this context, proximity to hazardous waste is traditionally included as a housing characteristic alongside the number of bedrooms, the number of bathrooms, square footage, and so on. Isolation of the variation in price that is directly attributable to the presence of hazardous waste sites provides an estimate of the value of these disamenities to homeowners.

While the hedonic method can be used to understand the value of actions taken in the past, it can also be useful to analyze the future impact of decisions. In order to formulate rational policies, regulators and policy makers need estimates of the impact their various potential actions are likely to have. The motivation of this paper is to demonstrate a simple, consistent hedonic analysis that can be applied virtually anywhere in the country that sufficient housing transactions data exist to provide policy-relevant estimates of the Superfund program's various effects on the housing market. Additionally, the methods demonstrated below are easily adapted to a variety of local amenity valuation applications.

Intuitively, it will be necessary to use past remediation decisions under the Superfund program to infer what impact similar, future actions will have on the housing market. Trying to use the outcomes experienced under the remediation of one particular site to infer potential outcomes under remediation of a different site is troublesome since any estimates derived from a single site are subject to the unique influences of the time and place of those remedial activities, as well as the idiosyncratic attributes, both observed and unobserved, of the site itself. Prudence would require that multiple sites be used to provide an actionable estimate that at least attempts to average out the various sources of idiosyncratic noise.

The econometric specification of this paper pivots from the standard "distance-to-site" measure to account for the intensity of the environmental disamenity to simply counting the number of sites within a certain distance of each house which, as will be demonstrated, provides for an easy inclusion of multiple sites. This accounting method implicitly assumes that all Superfund sites are homogeneous. An alternative to this method which treats sites heterogeneously and explicitly controls for distance would entail including a separate regressor accounting for the distance from each house to each relevant site.[1] This procedure can become unwieldy as the number of sites and site statuses under study grows. Furthermore, the heterogeneous impacts of sites can be obscured if sites tend to be clustered, negating the benefit of treating each site heterogeneously.

Even if consistent estimates of site-specific effects are successfully attained, they will have to be averaged in some way to derive a policy-relevant estimate to use on future sites and actions. Attempting to select a single site-specific estimate based on observables is dubious since sites can vary significantly in unobservable ways, the preferences of the homeowners in the vicinity of the site that give rise to the hedonic price schedule can vary from site to site, and the natural terrain features surrounding each site, which can amplify or mitigate potential health risks, vary. However, treating sites homogeneously from the outset permits the easy calculation of an estimate that implicitly averages out potentially confounding idiosyncrasies, without relying on questionable selection rules.

Once it has been resolved to treat sites homogeneously to derive "average-site" estimates, it is necessary to select a group of sites to operate on. It has been previously stated that the goal of this study is to derive policy-relevant estimates. Since any price effects that result from Superfund activities are likely to be local in scope, rational policy makers are likely to be interested what impact future actions at a

[1] Another alternative that is prominent in the literature is using only the distance to the nearest site. Unlike the method mentioned in the text above, this method treats sites homogeneously. As will be demonstrated in Section 5, the nearest site method provides similar results to the site-counting method. However, the site-counting method has the advantage of mitigating the omitted variables problem of multiple nearby sites when Superfund sites are geographically clustered and provides estimates of the average price effects that are easily interpretable.

given site will have on the local housing market. It is the stance of this author that the optimal set of sites to use to understand the interaction between the Superfund program and housing prices are the set of sites in the local housing market. Using a national sample of sites will bring in idiosyncrasies from Superfund sites and housing markets that are not relevant. This paper utilizes the 29 Superfund sites in the Los Angeles Combined Statistical Area (CSA) to derive estimates of implicit price effects of Superfund remediation activities on housing prices *in Los Angeles*. While the methods displayed below are applicable in any metropolitan area, the estimates I provide are not.

Before proceeding, it is vital to be clear on exactly what is meant by "remediation". The Superfund program has many different steps that stand to influence the housing market in disparate ways which are relevant to decision making. Some actions, such as the physical removal of toxic waste, directly improve the quality of the environment and reduce health risks. Other actions, like listing a site on the National Priorities List, are administrative and signal future risk reductions, which can operate on the expectations of agents in the housing market. This paper separately controls for administrative and physical actions and provides estimates of the impact on housing prices that results from each.

This study contributes to the literature in two additional ways. First, this paper adds specifically to the Superfund literature by introducing the "Construction Complete" designation as an explicit site status. Whereas most studies focus only on listing and delisting from the National Priorities List, including the date that each site was labeled as "Construction Complete" allows me to delineate completion of the physical remediation of the site from the administrative action of deleting it from the National Priorities list. Generally, many months pass between the time a site is listed as "Construction Complete" and the time the site is deleted from the National Priorities list. Accordingly, the use of deletion as a proxy for cleanup can be troublesome, since many houses sold before deletion could in fact have received the cleanup treatment. Accurately categorizing site activities according to their administrative and physical status allows me to explore the varied impacts that changes in expectations, information, and environmental quality separately have on the housing market.

Second, I demonstrate the potential for unobserved housing and neighborhood quality to be correlated with Superfund incidence. In the Los Angeles CSA, census tracts that are near Superfund sites tend to be lower-income, higher-minority neighborhoods while census tracts that are near Superfund sites that receive remediation tend to be higher-income, lower-minority neighborhoods. This raises the concern that unobserved neighborhood quality could bias the results. Similarly, houses that are near Superfund sites tend to have lower prices after demeaning by neighborhood level prices and conditional on their observables. This suggests that within a neighborhood, the houses closer to Superfund sites might be different in unobservable ways from those houses at the far end of the neighborhood. By employing a housing transactions panel dataset with time-varying neighborhood fixed effects, I can simultaneously control for both sources of endogeneity. The identification strategy requires the assumption that house level unobservables are constant over time. I provide evidence from housing transactions data that changes in house-level unobservables are unlikely to be correlated with Superfund site proximity, which indicates the failure of this assumption should not induce biased estimates. Specifically, I use a binary response model to show that Superfund site status changes do not increase the likelihood of nearby home improvements. Identification results from within-neighborhood variation in price changes.

I find that in Los Angeles, the Superfund program has had a positive impact on prices. Perhaps most interestingly, the largest price change is seen after a site is designated “Construction Complete.” On average, listing a site on the Final NPL raises prices 1.6%, designating a site listed on the Final National Priorities list as “Construction Complete” raises prices of nearby houses 2.8%, while proposing a site to the Final NPL has no significant effect on price. These results contrast with Greenstone and Gallagher (2008)[2], who found no significant effect in listing sites on the Final National Priorities list, and lead to a different conclusion than they reach about the value of the Superfund program[3]. Additionally, I find ignoring "Construction Complete" causes the estimate of the effect of deleting a site from the National Priorities List to not be statistically different than zero, which is a result consistent with the findings of Noonan et al. (2007). However, using "Construction Complete" as the indicator of "cleanup" rather than deletion from the National Priorities list provides a very different conclusion about the value of "cleanup". From the beginning of the Superfund siting process to deletion from the National Priorities List, nearby houses experience a statistically significant increase in price of 7.3%.

The remainder of this paper is divided into four sections. Section 2 provides some background on the CERCLA legislation and previous research. Section 3 describes the data used in this research. Section 4 introduces the main econometric model, Section 5 presents the results, and Section 6 concludes.

2.2 Background CERCLA and the Hedonic Pricing Model

2.2.1 CERCLA

On December 11, 1980, the United States Congress passed the Comprehensive Environmental Response, Compensation and Liability Act (CERCLA). CERCLA gave EPA, amongst other things, broad powers to respond to hazardous waste dangers and created a trust fund to pay for all of the actions undertaken - referred to as the "Superfund." Response could take the form of either a short-term, urgent cleanup to mitigate imminent human health dangers, a long-term remediation and liability search, or some combination thereof. To organize and facilitate the cleanup of the

[2] In this study, the authors do not separately estimate the effects of Final Listing on the NPL and Deletion from the NPL. Given this specification, the "Construction Complete" designation is irrelevant.

[3] The results of this paper, however, are not contradictory. It could be the case that, on average, Americans in the national housing market do not value Superfund site remediation, but the housing market participants in the Los Angeles local housing market do. This highlights the importance of regional analysis and the perils of applying national average estimates universally.

nation's most dangerous sites, EPA created the National Priorities List (NPL). The NPL represents the collection of sites that EPA deems of highest priority for cleanup.

In order to be listed on the NPL, a site must first be identified by EPA. The agency becomes aware of a site through various communication channels (state environmental agencies, public comment, etc.), and then proposes a site to be listed on the NPL in the Federal Register, if warranted. Once placed in the Federal Register, EPA accepts comments and will then place the site on the Final NPL if it meets certain criteria. These criteria include a previously administered Hazard Ranking Score (HRS) of sufficient intensity, the state environmental authority designating the site a top-priority, or whether the U.S. Public Health Service recommends removing people from the proximity of a site and EPA finds it more cost-effective to use the long-term remediation process (i.e. the Superfund program) versus its emergency cleanup powers. Once a site is on the Final NPL, a Record of Decision is issued detailing the remedy to be implemented.[4] Once the prescribed remedy has been constructed and all hazards are contained, EPA designates the site as "Construction Complete." Prior to deletion, EPA will post plans to delete the site in a local newspaper and solicit public comment. Once monitoring confirms that health hazards have been contained and deletion is deemed appropriate, EPA will enter notification of deletion in the Federal Register.

While EPA naturally has many steps in between, the major, publicized actions center around the proposal of a site to the NPL, the listing of a site on the NPL, designating the site as "Construction Complete," and the deletion of the site from the NPL, all of which entail some public announcement or entry into the Federal Register. I use these major actions as representing EPA's position to the public on the relative risk posed by each site in subsequent analysis. Section 3 below explains how I translate Superfund site status into a measure of environmental quality.

[4] For a description of the remedy selection process, see Gupta et al. (1996)

2.2.2 Literature Review

The literature on estimating the impact of the Superfund Program on housing values is vast. Kiel and Williams (2007) have an excellent survey of the results to this point. Two main approaches have been followed when trying to estimate the value or impact of local disamenities. The first approach is to take a known disamenity and try to determine how distance from the site impacts the selling price of a home. This requires individual transaction data for each house sold in a given period and generally uses hedonic pricing theory stemming from Rosen (1974) with distance from a site as a housing attribute. According to this method, if the researcher knows a sufficient amount of information about each house, he or she can control for these characteristics and isolate the effect of distance from the nearest site. However, this approach has its limits in terms of calculating marginal willingness to pay. Many papers have noted the plethora of problems with Rosen's two-step procedure of first estimating the hedonic price function and then regressing the estimated coefficient on demand characteristics (Brown and Rosen (1982); Bartik (1987a,b); Epple (1987)). Dealing with the issues raised by these authors is beyond the scope of this paper.

Michaels and Smith (1990) demonstrate the heterogeneity in willingness to pay for site removal across housing "sub-markets" by studying how distance to the closest hazardous landfill affects the price of a house, while controlling for whether or not the sale took place after site discovery, in the suburban Boston area. Kohlhase (1991) runs separate hedonic price regressions for the Houston market for three years: 1976, 1980, and 1985. These years correspond to a pre-Superfund time, a time concurrent with the creation of the passing of CERCLA, and a period after all sites in the Houston area were placed on the NPL. Her results show that distance-from-site has a positive influence on price once the sites are listed on the NPL. In their paper, Kiel and Zabel (2001) focus on the two Superfund sites in Woburn, MA. They are interested in the premium paid for distance from the nearest site but do not allow multiple sites to enter into the hedonic price function. Further, they do not employ panel data. Rather, they estimate the price function at several points in time, and interestingly, determine that price effects cease past three miles. Kiel and Williams (2007) conduct site-specific analysis on a national sample of Superfund sites to demonstrate the heterogeneity of treatment effects across the country. By demonstrating a clear heterogeneity in effects across housing markets, their results highlight the limits of using national housing market analyses for determining the value of cleaning a particular site. These studies attempt to control for neighborhood effects by assigning census tract characteristics for each house they see transact, which could be endogenous and measured with error when applied to intra-censal years.

Exceptions to the "nearest-site" approach include Hite et al. (2001); Gayer et al. (2000). In the former study, the authors use proximity to each landfill in Franklin County, Ohio as a separate regressor in the hedonic price function, finding a significant influence on prices for all landfills. The latter examines how much residents are willing to pay to avoid the risk of cancer before and after the Remedial Investigation for each NPL site in Grand Rapids, MN is released. They find that consumers's perception of cancer risks are overestimated before EPA releases a detailed estimate of the risks and therefore pay a much higher premium for houses farther from the site before the release of the report. They find that even at the inflated perceived risk levels, the upper-bound for the willingness to pay for the six sites in the area to be cleaned is about one-sixth of the remediation cost. It should be noted, however, that none of the above research utilizes fixed-effects regressions to control for all neighborhood unobservables or individual, unobserved house attributes.

Additionally, as pointed out by Farber (1998), site-distance regression models have the inherent problem of a correlation between the location of hazardous waste and economic opportunity, thus confounding the relative appeal of living near what could be an employment center. Gayer et al. (2000) also criticizes the site-distance approach as falsely assuming that remediation alleviates both the health risks and aesthetic attributes that may impact housing values. Cameron (2006) demonstrates the possibility of directional heterogeneity in the effect of distance. Her analysis explains how a given distance in a down-wind direction from an odorous Superfund site is very different than traveling that same distance in the up-wind direction. Ignoring direction can conflate the effects. Adequately controlling for neighborhood unobservables can address the concerns of the first criticism. Second, deriving an estimate for price effects throughout the life cycle of a Superfund site can capture how the market reacts to remediation. Third, directional heterogeneity could pose a problem in my setting since only proximity is employed. However, averaging over many sites should serve to mitigate the idiosyncratic effects of house-site relationships.

The second approach in the literature looks at how median housing prices vary across counties or census tracts with respect to the number or characterization of environmental disamenities contained within. This strand begins with Greenberg and Hughes (1992) in a study of the New Jersey housing market. They compare the median home values in communities with and without a Superfund site and find that rural counties with Superfund sites had lower rates of appreciation than rural control counties. This result was not repeated for urban counties. Noonan et al. (2007) study the effect that Superfund site cleanup has on block-group level prices using a national sample. They estimate a system of equations to capture price and non-price effects of an NPL site being deleted. Like this study, they use a first-differencing approach to control for time-invariant neighborhood unobservables. However, they do not control for time-variant unobservables nor can they control for within-neighborhood heterogeneity since they employ aggregated data.

Most recently, Greenstone and Gallagher (2008) examine how housing prices vary across census tracts with and without an NPL site. Recognizing the problem of correlation between unobserved census tract attributes and the presence of a NPL site, they utilize a regression discontinuity design centered around the assumption that sites with similar HRS scores are likely to be in census tracts with similar unobserved characteristics. They note the fact that when EPA began operating the Superfund program, Congress mandated that they select 400 sites to place on the initial NPL list. Using HRS scores to determine the top 400 sites, EPA placed those sites with an HRS score of 28.5 or higher on the NPL, creating a "quasi-experimental" discontinuity in treatment. As a result, they find no statistically significant evidence that census tracts on opposing sides of the discontinuity had differing housing market outcomes. However, they are unable to account for housing market outcomes at a more localized level than the census tract.

? build on the regression discontinuity design of Greenstone and Gallagher using confidential census block-level data and intra-tract price variation. Using a fixed effects regression, they find that block-level median prices respond positively to the deletion of NPL sites. Additionally, quantile regression analysis of intra-tract prices reveals that the lower quantiles of the census tract price distribution respond more aggressively to Superfund site remediation than other parts of the distribution.

Interest in life-cycle effects spans both strands of the Superfund literature. All of the studies above acknowledge the idea that any given Superfund site will have different effects on the housing market as its classification under CERCLA changes. Some studies only focus on estimating the effect of sites of a particular status: Noonan et al. (2007) (Deleted NPL sites), Greenstone and Gallagher (2008) (Final or Deleted NPL sites), Greenberg and Hughes (1992) (Final NPL sites). Other studies estimate an effect for a separate time period that corresponds to each of the target site's statuses: Kiel and Zabel (2001); Kiel and Williams (2007). However, there is little discussion about the statistical significance of the changes to the distance gradient and they do not consider "Construction Complete" as an official status[5]. This paper adds to this area of the literature by contributing these two novel features.

[5] Kiel and Williams (2007) specify a "Cleanup" time period which corresponds to the time between the commencement of clean up activities and delisting from the NPL. While this strategy is in a similar spirit to my own strategy, they treat a site as cleaned when it is removed from the NPL whereas I treat the site as cleaned when the EPA ceases remediation activities. Several months can pass between these two events.

2.2.3 Review of Hedonic Theory

As previously mentioned, the research noted above draws on hedonic pricing theory to characterize how environmental amenities are valued in the housing market. Hedonic pricing equilibrium, as explained in Rosen (1974), provides a framework for analyzing the implicit price for attributes of a differentiated product. [6] The differentiated product is housing and the attribute of interest is the environmental quality surrounding the house. The analogy is developed by assuming homeowners act like firms supplying housing to the market. Each firm has a production function that shapes inputs into a finished product: a house. Some firms are located near hazardous waste sites, which makes the cost of supplying a clean environment very high. These firms find it profitable to supply a low level of environmental quality. Conversely, some firms are located in pristine areas, giving them a low cost of production. These firms find it profitable to supply a high amount of environmental quality. Heterogeneity in firms gives rise to a continuum of offer curves, defined as the locus of prices and quantities that maximize each firm's profit.

On the consumer side of the market, individuals search for a house to maximize their utility, which is defined over wealth and a vector of housing attributes. A consumer's bid curve for a given attribute is the locus of prices and quantities that give a maximum utility. A key assumption is that the market for houses is supplied by a continuum of firms that make available a set of houses in which every possible combination of housing attributes exists. This allows consumers to select the bundle of characteristics that maximizes their utility, given the price. A hedonic equilibrium is reached by consumers and suppliers transacting in the market and the implicit price for each attribute is set where the marginal consumer's bid curve becomes tangent to the marginal seller's offer curve.

Empirically, the covariance between price and the quantity of a given attribute

[6] For a full analysis of extending hedonic pricing equilibrium to the housing market, see Palmquist (1984).

identifies the location of the intersection of bid and offer curves. Regressing price on the attributes of the house provides a linear approximation to the slope of the price gradient at the observed equilibrium. However, a common challenge is dealing with the difficulty of observing all characteristics of a given house. If the researcher can effectively control for the omitted variables that are likely to be correlated with observed attributes, estimates of the slope allow inference about the relative importance of the various attributes in setting the price. This paper demonstrates the importance of controlling for these omitted variables.

2.3 Data

The data used in this analysis comprises two parts: housing transactions data and Superfund hazardous waste site data. In the following subsections, I describe these two datasets in detail, as well as discuss potential sources of omitted variables bias.

2.3.1 Housing Data

The housing transactions data for this analysis comes from Dataquick Information Systems, a real estate information aggregator. The data provides a record of each single family housing transaction, attached and detached, that took place between 1988 and 2008 for Los Angeles, Ventura, Orange, San Bernardino, and Riverside counties[7]. The dataset contains many observable characteristics for each house (e.g. number of bedrooms, square footage, etc.) as well as the transaction price, loan amount, transaction date, latitude and longitude coordinates and the year 2000 census tract. Each property is uniquely identified in the data which allows the creation of a panel data set. In an effort to remove outliers, houses observed in the top and bottom 1% of the price and square footage distribution are dropped, as well as the top 1% of the number of bedrooms and the number of bathrooms distribution. Houses with missing attribute or location data are also dropped from the dataset.

[7] These five counties make up the Los Angeles-Long Beach-Riverside Combined Statistical Area, more commonly referred to as "Greater Los Angeles"

Table 2.1: Single Sale Houses vs. Panel Houses

Category	One Sale	Multi-Sale
Mean Sq. Footage	1,737.42	1,603.25
Mean # of Rooms	5.02	4.66
Mean # of Bathrooms	2.20	2.17
Mean # of Bedrooms	3.14	2.97
Mean Year Built	1969.9	1970.97
Mean Price	$232,408	$227,612
Number of Houses by repeat sales:		
2 Sales		459,967
3 Sales		169,806
4 Sales		48,298
5 Sales		10,191
6 Sales		1,796
7 Sales		220
8 Sales		19
9 Sales		4
Total Houses	974,562	690,301
Total Observations in Final Sample	974,562	1,664,863

Counties Covered: Los Angeles, Riverside, Ventura, Orange, San Bernardino. Dollars reported in year 2000 dollars. Prices used were de-trended using a monthly Case-Shiller Los Angeles Housing Price Index for comparibility.

The main analysis of this paper makes use of a panel dataset and house level fixed effects, whereas certain alternative specifications relax the panel requirement. Sample selection could be an issue if the set of houses that only sell once are substantially different in unobservable ways. Table 2.1 provides the summary statistics for the two sets of houses. Houses that have only sold once tend to be slightly bigger, have more rooms, and sell for approximately $4,000 more.

An unfortunate feature of the transactions data is that Dataquick will overwrite the characteristics recorded for a given property in previous transactions if a newer transaction is recorded with different and presumably updated information[8]. How-

[8] Conducting the analysis with only the last observed transaction for each house, which has accurate structural information for the transaction date, does not change the results in meaningful ways. Using only one transaction per house precludes the use of house-level fixed effects, but comparing the results that utilize only the last sale to the alternate specifications that do not use house fixed

ever, in certain circumstances, if the renovation is on the scale of a large addition or major construction, the transaction will be flagged as having such an improvement. The implication for panel analysis is not being able to reliably observe changes to properties, since any moderate change made to the property is retroactively applied to all records in the data. As a result, all observable characteristics will drop out of any repeat sales analysis. To combat the presence of homes that likely have changed in substantial ways, homes that are observed to appreciate (depreciate) more than 50% on an annualized basis, have the major construction data flag, transact with a loan amount greater than the transaction price by $5,000, or are observed to transact twice or more in any 12 month span are dropped from the sample.

2.3.2 Superfund Site Data

The U.S. Environmental Protection Agency makes available via its website a comprehensive data set detailing the names and locations of all hazardous waste sites reported to EPA and the actions taken at these sites.[9] Most important to this research, EPA details the date that the site was discovered, the date any site was promoted to the Proposed National Priorities list, the date any site moved from the Proposed NPL to the Final NPL, and the date any site was deleted from the Final NPL. While not confidential, the dates that sites were listed as "Construction Complete" and the verified location coordinates are not available in this database and were provided directly by EPA. For this study, attention is restricted to the set of hazardous waste sites that were proposed to be listed on the Final NPL (NPL sites) by January 1, 2008. The principle reason for this restriction is that EPA has not verified the longitude and latitude coordinates for non-NPL sites. As a result, I have a complete record of action and location for 29 NPL sites within the five counties in

effects reveal no significant differences.

[9] The CERCLIS database can be downloaded in ASCII text format at http://www.epa.gov/superfund/ sites/phonefax/products.htm

Table 2.2: Superfund Sites in Greater Los Angeles Area

Site Status	(1) 1990	(2) 2000	(3) 2007
DISCOVERED	7	2	1
PROPOSED	0	1	0
FINAL	20	22	21
COMPLETE	0	3	5
DELETED	0	0	2
Total	27	28	29

the Los Angeles area; all of which were at least proposed to the Final NPL in the timeframe in question.[10]

Table 2.2 provides a summary of how the Superfund site profile for the Greater Los Angeles area has evolved over the sample period. At the end of 1990, there were no sites that were being proposed to the Final NPL, listed as "Construction Complete", or Deleted from the NPL. By the end of 2000, there were still no Deleted NPL sites and only one site proposed to the NPL. As would be expected, the majority of the remediation activities took place later in the sample period. Census data would be unable to provide estimates of the price effect of deleting a site from the NPL since there would be no variation in the data.

2.3.3 Neighborhood Unobservables

The estimation methods of this paper utilize neighborhood fixed effects to control for potentially correlated unobserved neighborhood quality. One of the more common definitions of neighborhood found in the literature is the census tract. Since the Dataquick data provide the census tract in which each property resides, I employ this definition of neighborhood as well.

[10] One Superfund site was removed from the proposed NPL list and never listed. This site was dropped from the analysis since it is likely very different from the other sites that were all eventually listed.

Previous studies that utilize housing transactions data generally use observable characteristics of the tract to control for heterogeneity in neighborhoods. However, there has been little attention paid to the influence that unobservable neighborhood attributes can have on Superfund price effects. If the incidence of Superfund sites is correlated with the presence of unfavorable unobserved neighborhood quality then there could be a downward bias on the estimate of Superfund exposure. Conversely, having a correlation between cleanup and favorable unobserved quality could inflate the impact of remediation on home prices.

Table 2.3 provides evidence that in the Greater Los Angeles area, both types of bias could be present. Columns (1) and (2) compare the means of year 2000 census tract observables for tracts that never had Final NPL sites within 3 km of the tract borders (non-NPL tracts) to those that did (NPL tracts). Column (3) provides the p-value for a two-sided means-equivalence test. Clearly, tracts that have NPL sites in them or nearby have significantly lower incomes, less college educated adults, and higher fractions of minority residents. If there is correlation between observed and unobserved quality, these observable differences would suggest that tracts near Superfund sites might have lower unobservable quality than tracts that aren't near Superfund sites, leading to a negative bias on estimates in the absence of adequate controls.

Columns (4) - (6) compare observables of NPL tracts that were declared "Construction Complete" against those NPL tracts that were not. The tracts that are near Final NPL sites that eventually become cleaned have significantly less college educated adults and minorities. There are no significant differences in income, poverty rates, vacant housing units and proportions of owner occupied housing. This suggests that the EPA selection mechanism for site remediation is uncorrelated with tract level characteristics, both observed and unobserved. The same cannot be said, however, about the decision to delete sites from the National Priorities List. Columns (7) - (9) show that NPL tracts near sites that are eventually deleted from the NPL have

Table 2.3: Census Tract Characteristics in Los Angeles CSA

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Final NPL			Cons. Complete			Deleted NPL		
	No	Yes	p-value	No	Yes	p-value	No	Yes	p-value
Median Income	50,237	45,743	**.000**	45,089	47,508	.177	44,355	61,534	**.000**
% under Poverty Line	15.71	15.37	.571	15.30	15.55	.790	15.87	9.699	**.000**
Sq Km.	28.95	9.353	.354	11.42	3.788	.338	9.924	2.859	.586
% College Grad	0.155	0.130	**.000**	0.135	0.116	**.031**	0.125	0.189	**.000**
Population Dens	3,829	3,644	.306	3,484	4,073	**.020**	3,719	2,791	**.025**
Housing Dens	1,308	1,114	**.003**	1,110	1,124	.850	1,131	924.2	.090
% Minority	42.60	50.70	**.000**	52.39	46.11	**.000**	51.74	38.89	**.000**
% Vacant	5.480	3.867	**.000**	4.111	3.210	.099	4.029	2.036	**.025**
% Owner Occupied	55.64	54.28	.284	54.56	53.51	.661	53.40	64.25	**.005**
No. of Tracts	2,878	495		361	134		455	40	

Notes: Columns (1), and (2) display mean characteristics from the 2000 U.S. Census for tracts in the sample by Final NPL exposure. Columns (4) and (5) display mean characteristics of tracts by "Construction Complete" treatment. Columns (7) and (8) show mean characteristics of tracts by Deletion from NPL treatment. Columns (3), (6), and (9) show the p-values for mean-equivalence tests. Boldface numbers indicate rejection of the null of equivalence at the 5% level.

significantly higher incomes, lower poverty rates, more college educated adults, less minorities, less vacant housing, higher proportions of owner occupied housing and are less densely populated. If observable quality covaries positively with unobservable quality, the sites that are deleted from the NPL might be in tracts with very positive unobservables and naïve estimates of the price effect of deletion could be positively biased.

2.3.4 House Level Unobservables

Controlling for unobserved heterogeneity at the neighborhood level may not be sufficient to remove all omitted variable bias. When using tract-level fixed effects, identification comes from within-tract variation. Given that the average size for tracts near NPL sites is approximately 9.4 square kilometers, the econometrician might be concerned that houses in the part of a tract near a Superfund site could look very different in unobserved ways than houses in another part of the tract. If houses near Superfund sites tend to be less well maintained and updated than their counterparts within the tract, estimates without house-level controls for endogeneity could be seriously biased.

Table 2.4 provides a comparison of means of household observables for houses near NPL sites. To be able to make comparisons across time and space, I subtract the average value for houses that sold in the same tract and year for each observable. Columns (1) and (2) compare houses that are within 3 kilometers of a Final NPL site to those that are not within 3 kilometers of a Final NPL site, Columns (4) and (5) compare houses within 3 kilometers of "Construction Complete" sites to houses that are not, and Columns (7) and (8) compare houses within 3 kilometers of Deleted NPL sites to those that are not. Columns (3), (6), and (9) provide the p-values for two-sided mean equivalence t-tests. A comparison of the p-values for the observable characteristics show that by treatment type, the groups of treated and un-treated houses look very similar to the econometrician. However, with the exception of the "Construction Complete" category, the respective groups have very different prices; the houses near the sites have lower prices. While it may not be surprising that the only difference between houses that are near sites and those that aren't is that nearby houses have lower prices, the data presented so far cannot distinguish between the case where lower prices are caused by Superfund site proximity and the case where lower prices are caused by lower unobserved housing quality near the sites. When the information contained in Table 2.4 is compared to the final results that control for unobserved heterogeneity, the argument for the presence of lower unobserved quality becomes very strong.

2.4 Empirical Model

2.4.1 Hazardous Waste Measures

This paper departs from the prevalent measure for hazardous waste exposure, distance to the nearest site, by counting the number of sites around each house. This method requires selecting some maximum distance between a house and a site beyond which

Table 2.4: House Level Average Deviations from Tract-Level Means

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Final NPL			Cons Complete			Deleted NPL		
	Yes	No	p-value	Yes	No	p	Yes	No	p
Price	-880.5	53.51	**.000**	-514.5	2.334	.555	-4,589	4.720	**.012**
Bedrooms	-0.001	6.41e-05	.561	-0.006	2.89e-05	.338	-0.029	2.95e-05	**.040**
Bathrooms	0.002	-0.000	.200	-0.003	1.18e-05	.633	-0.019	1.93e-05	.101
Sq. Footage	0.202	-0.012	.870	-3.237	0.015	.472	-16.94	0.017	.073
Year Built	0.046	-0.003	.130	0.187	-0.001	.093	-0.202	0.000	.389
Obs.	152,435	2,508,130		12,016	2,648,549		2734	2,657,831	

Notes: Columns (1), and (2) display the average demeaned characteristics for houses in the sample by Final NPL treatment. Columns (4) and (5) display the average demeaned characteristics for houses by "Construction Complete" treatment. Columns (7) and (8) display the average demeaned characteristics for houses by Deletion from NPL treatment. Treatment for a house is defined as a house being within 3 kilometers of any site of the given type. Each variable for each house is demeaned by the mean value observed for houses that sold in the same tract and year. Columns (3), (6), and (9) show the p-values for mean-equivalence tests. Boldface numbers indicate rejection of the null of equivalence at the 5% level.

there is assumed to be no price effect. As noted previously, some researchers have tried to estimate the distance at which hazardous waste sites cease to affect home values with varying results. For this study's main specification, I count the number of sites that are within a 3 kilometer radius of each house to measure the amount of hazardous waste exposure. However, the results are robust to other specifications of distance.

Furthermore, to gain insight into how the life-cycle events of the Superfund program separately influence the housing market, I include a measure of each of these steps. As previously mentioned, in the context of my model, each hazardous waste site can be in one of five stages at any given time: the $DISCOVERED$ period is the time after EPA is aware of a potential hazard but before being proposed to the NPL; $PROPOSAL$ is the time after a site is proposed to the NPL but before a final listing on the NPL; $FINAL$ is the time after a site is listed on the Final NPL but before the site is designated "Construction Complete"; $COMPLETE$ is the time between designation as "Construction Complete" and deletion from the NPL; $DELETED$ is the time after EPA removes the site from the NPL. These categories measure the major actions of the Superfund program that housing market participants are likely to be interested in: the discovery of a hazardous waste site, the potential for future cleanup, the promise of future cleanup, the completion of cleanup and the verification of safety. Looking at the variation of housing prices as the density of each type of Superfund site in close proximity varies can explain how each step is separately valued in the market.

Since I know the location of each house and the location of each site, I can calculate the distance between each pair. Furthermore, for each site, I know the timeframe for each of the five periods above. Using transaction dates from the housing data, I can create a snapshot of the hazardous waste profile around every house, specific to the day it was sold. The result is a vector, for each house transaction, of counts of each of the five "types" of Superfund sites nearby on the transaction date.

2.4.2 Main Specification

The general specification of this paper, given in Equation (2.1), assumes the hedonic price function is linear in attributes. The price of any house i, sold at time t, in census tract j, is a function of a vector of observable attributes of that house and a constant X_{it}, the vector of hazardous waste site type counts in various stages, Z_{it}, a fixed effect for which census tract - year the house was sold in, δ_{jt}, an unobserved house attribute, γ_i, and an i.i.d. mean zero disturbance term, ε_{it}. Data restrictions preclude me from observing changes in observable characteristics X_{it}, therefore the subscript t is dropped.

$$\ln Price_{it} = \alpha X_i + \beta Z_{it} + \delta_{jt} + \gamma_i + \varepsilon_{it} \tag{2.1}$$

Since X_i is constant over time, the coefficient α is unidentified. β is the coefficient of interest and measures the effect that the density of hazardous waste sites in various stages in the Superfund program have on the selling price of a home.

This specification explicitly accounts for both sources of bias discussed in Section 3. The neighborhood level unobservable, δ_{jt} captures all determinants of price common to houses that transact in a given tract and year. It's worth noting that allowing the tract effect to vary by year is a relaxation of the stronger assumption of time invariant neighborhood fixed effects. Additionally, using observable characteristics from census years for data that is intra-censal implicitly assumes that the tract is constant over time. This specification requires no such assumption. House-level unobserved quality is controlled for by γ_i. Time-invariant unobservables specific to each house that could bias estimates, such as aesthetics, hardwood floors, gardens, abundant sunlight, historical significance, etc., will all be contained in this term.

2.4.3 Flexible Distance Specification

The main specification assumes that the effect on price is constant inside a certain radius. This restriction can be relaxed by splitting the radius of impact into expanding concentric circles. For example, the main specification can be modified to count the number of sites of each type separately that are present 0 - 1 km, 1 - 2 km, and 2 - 3 km from each house. Under this alternate specification, Z_{it} from Equation (2.1) expands in dimension to have three sets of counts for each site status. Unobserved heterogeneity is controlled for in an identical manner, and the model allows for distance from the site to have heterogenous impact.

2.4.4 Identification

Identification in the model described in Equation (2.1) requires successfully removing both δ_{jt} and γ_i, the time-varying neighborhood fixed effect and the time-invariant house level fixed effect, respectively. Careful attention must be paid to the method employed to remove these unobservables. On the surface, Equation (2.1) is nothing more than a panel regression model with a neighborhood fixed effect that changes overtime. However, since the data are an unbalanced panel, using pre-packaged fixed effects routines or least-squares dummy variable methods will be inconsistent.

To see why this is the case, consider for simplicity that each house sells only twice.

First differencing Equation (2.1) yields:

$$\ln Price_{it} - \ln Price_{it-1} = \beta(Z_{it} - Z_{it-1}) + (\delta_{jt} - \delta_{jt-1}) + (\gamma_i - \gamma_i) + (\varepsilon_{it} - \varepsilon_{it-1}) \tag{2.2}$$

$$\ln \bar{Price}_{it} = \beta \bar{Z}_{it} + (\delta_{jt} - \delta_{jt-1}) + \bar{\varepsilon_{it}} \tag{2.3}$$

After first differencing, Equation (2.3) still has two unobserved terms: The neighborhood effect in time t and the neighborhood effect in time $t-1$. To mean difference away δ_{jt}, I need to take the average of all first-differenced variables over the set of observations that had a sale in neighborhood j and in time t. The problem arises from the fact that each observation in this set has a second neighborhood unobservable that is not necessarily the same time as δ_{jt-1}. Mean differencing removes δ_{jt} from Equation (2.3), but still leaves δ_{jt-1} and the average second neighborhood effect for all houses that sold in t. Using dummy variables for the neighborhood-year fixed effects in Equation (2.3) and running OLS will leave this unobserved "remainder" term and provide inconsistent estimates. To successfully remove both δ_{jt} and δ_{jt-1} from Equation (2.3), I must mean-difference by taking the average over the houses in neighborhood j that sold in both years t and $t-1$. This removes all unobserved terms and provides a consistent estimate of β. Identification is driven by within-tract variation Superfund site exposure. Since the hazardous waste sites are always in existence and the distance between sites and houses aren't changing, identification comes from site status changes.

The identification strategy outlined above is built on several assumptions. First, β is not indexed by either time or space, which implies that preferences over Superfund site exposure remain constant throughout the entire sample period and the entire Los Angeles area. Second, the house-level effect γ_i is assumed to be constant over time and data limitations force the assumption of fixed housing observables. Third, counting the sites in a circle around each house assumes the effect is constant within the radius of the circle.

Spatial variance in preferences is documented in the literature by site-specific studies. The variance in estimates across the country and even within cities demonstrates both a heterogeneity in Superfund sites and in preferences in local housing markets. While there is no doubt a spatial distribution of preferences in Los Angeles[11], this issue is intentionally abstracted from in order to obtain an average estimate for the region. The robustness of the assumption of time-invariant preferences is examined in the next section by allowing β to vary by time periods. Additionally, I will examine the assumption of constant house level observables and unobservables. The evidence from the data suggests that improvements in housing quality are not positively correlated with site status changes. Lastly, the robustness of the assumption of constant impact within a 3 kilometer radius is examined by use of the flexible distance specification and by examining results under various sizes of radii.

Finally, the research design raises concerns of correlated error terms. First, to control for correlation in the error terms for each property i, standard errors are clustered on properties. Second, there is concern that error terms in Equation (2.1) can be spatially autocorrelated. To test for spatial autocorrelation, I group houses by their census tract and calculate Moran's I (MI) test statistic developed in Moran (1950) under various weighting schemes.[12] For all choices of weights, MI returned values very close to zero, indicating that the residuals are not substantially spatially autocorrelated.

2.5 Results

2.5.1 Main Results

Site Count Specification

The results for the estimation of Equation (2.1) can be found in Column (1), Panel A of Table 2.5. Estimates of the various elements of β are listed with standard er-

[11] See Redfearn (2009) for a discussion of spatial variation of preferences in hedonic studies and an application to light rail access in the Los Angeles area.

[12] See Case (1991) for a discussion of Moran's I and testing for spatial autocorrelation in spatial demand models.

Table 2.5: Empirical Results - Main Specification

	(1)	(2)	(3)	(4)	(5)
	A. Main Estimation Results				
DISCOVERED	0.039**	-0.003	0.028**	-0.022**	0.042**
	(0.013)	(0.003)	(0.005)	(0.002)	(0.013)
PROPOSED	0.046**	-0.030**	0.010	0.012*	0.064**
	(0.016)	(0.006)	(0.007)	(0.005)	(0.016)
FINAL	0.062**	-0.019**	0.017**	0.069**	0.071**
	(0.015)	(0.002)	(0.006)	(0.001)	(0.015)
COMPLETE	0.091**	-0.012*	0.049**	0.167**	
	(0.016)	(0.005)	(0.006)	(0.003)	
DELETED	0.073**	-0.017*	0.014	0.392**	0.069**
	(0.018)	(0.008)	(0.008)	(0.005)	(0.019)
Square Footage		0.037**		0.047**	
		(0.000)		(0.000)	
No. of Bedrooms		0.051**		-0.020**	
		(0.000)		(0.001)	
No. of Bathrooms		-0.008**		0.021**	
		(0.000)		(0.001)	
	B. Site Status Change Estimates				
Δ*PROPOSED*	0.007	-0.027**	-0.018**	0.035**	0.022**
	[0.445]	[0.000]	[0.000]	[0.000]	[0.008]
Δ*FINAL*	0.016*	0.011	0.007	0.057**	0.007
	[0.042]	[0.077]	[0.123]	[0.000]	[0.357]
Δ*COMPLETE*	0.028**	0.007	0.032**	0.098**	
	[0.000]	[0.200]	[0.000]	[0.000]	
Δ*DELETED*	-0.018	-0.005	-0.036**	0.224**	-0.002
	[0.103]	[0.593]	[0.000]	[0.000]	[0.842]
Tract-Year Fixed Effects	Yes	Yes	No	No	Yes
House Fixed Effects	Yes	No	Yes	No	Yes
Obs.	1,664,863	2,660,565	1,664,863	2,660,565	1,664,863

*Notes:*The standard error is below each coefficient in parenthesis and clustered on houses.* , and ** denote significance at the 5% and 1% levels, respectively. Column (1) is the main specification, Column (2) uses neighborhood fixed effects only, Column (3) uses house fixed effects only, Column (4) is OLS on all observations with no fixed effects. The regressions in Columns (3) and (4) have yearly dummies to control for the trend in the housing market. Column (5) uses both fixed effects but ignores "Construction Complete".

rors in parenthesis underneath. EPA discovering an eventual NPL site is associated with home values rising by nearly 4%. Then, as the site is proposed, listed on the Final NPL and designated "Construction Complete", prices continue to rise. Panel B provides analysis of these step-by-step differences. Values in this panel represent the difference in the price effect that result from moving from one site status to the next. Below each value in brackets is the p-value from a Wald equivalence test. These tests reveal that proposing a site to the NPL and deleting a site from the NPL do not have a statistically significant effect on prices, whereas listing a previously proposed site on the Final NPL has a significantly positive effect of 1.6% and designating a Final NPL site as "Construction Complete" has a significantly positive effect of 2.8%.

Columns (2) - (4) present estimates using different combinations of fixed effects: Column (2) presents results without house level fixed effects and all single sale houses are included, Column (3) presents results without tract-year fixed effects but with year dummies to control for temporal market effects, and Column (4) presents the results with no fixed effects at all. The estimates of the life-cycle changes in site status are all insignificant when not controlling for fixed effects except for $PROPOSED$, which is negative. Ignoring the negative impact that unobserved housing quality has on prices obscures the positive effects of the Superfund program. Column (3) reveals that the absence of house-level fixed effects induces more bias than the absence of tract-level fixed effects as the estimates are somewhat similar to the main specification. However, Column (4) demonstrates the general influence of unobserved heterogeneity. Without controlling for the unobserved quality of houses and neighborhoods in close proximity of Superfund sites, the estimated benefit of designating a Final NPL site as "Construction Complete" is 9.8% and the benefit of taking a "Construction Complete" site off of the NPL is an additional 22.4%. Finally, Column (5) provides results from a model that uses the same fixed effects as Column (1) but ignores "Construction Complete" as a site status. Under this specification, all price improvements are realized at site discovery and proposal to the NPL. This finding is consistent with previous studies that ignored "Construction Complete" and found no effect for final listing on the NPL and deletion from the NPL.

One counterintuitive result is the large, positive coefficient on $DISCOVERED$. It might be expected that discovering a nearby site is posing health risks would negatively impact prices. One possibility is that rising housing prices cause discovery. If prices are rising exogenously by an influx of new buyers, then hazardous waste spills have a higher probability of being discovered. This can lead to a situation where sites are being discovered only after additional residents enter the area and drive up prices.

It is unlikely, however, that the same mechanism would make "Construction Complete" an endogenous milestone. Site discovery is more or less instantaneous; a phone call made by a developer to the EPA could constitute discovery. However, a site attaining the "Construction Complete" designation must first have completed the entire remediation plan set forth in the Record of Decision, which is a function of the chemicals released and geographic features surrounding the site. Since the remediation plan usually takes many months or years to complete, it is unlikely that the EPA could "fast-track" a remediation completion in response to rapid development or price appreciation around a site, making the "Construction Complete" designation determined by positive price improvements.

Flexible Distance Specification

Table 2.6 provides estimates of the flexible distance estimator. In Panel A, Columns (1) - (3) list the coefficients of the regression corresponding to the main specification where the counts of each site type are broken out by whether they fall within 0 - 1 kilometers, 1 - 2 kilometers, or 2 - 3 kilometers. They are presented horizontally for ease of comparison. Moving down each column documents how, for a given distance band, the estimated coefficient changes as site status changes. Panel B provides these differences by distance bands along with p-values from Wald tests of equivalence. Moving across any row in Panel A allows comparison of the effect a site of that type has on prices as distance increases. Panel C lists the difference in coefficients with p-values from Wald tests of equivalence underneath in brackets. Finally, Panel D reports the difference in site status change effects and tests for equivalence between distance bands.

Several important facts emerge from Table 2.6 Panel B. First, the effect of deleting a site has a negative and significant effect at 0 - 1 km and 1 - 2 km, whereas the main specification reports no significant effect for deletion. Second, the effect of

Table 2.6: Flexible Distance Specification

	(1) 0-1 km	(2) 1 -2 km	(3) 2 - 3 km	(4) (2) - (1)	(5) (3) - (2)	(6) (3) - (1)
	A. Distance Band Estimates			C. Distance Coefficient Differences		
DISCOVERED:	0.031	0.023	0.045**	-0.008	0.022	0.014
	(0.02)	(0.016)	(0.013)	[0.637]	[0.094]	[0.449]
PROPOSED:	0.032	0.067**	0.042**	0.035	-0.025	0.010
	(0.033)	(0.022)	(0.016)	[0.275]	[0.192]	[0.774]
FINAL:	0.069**	0.081**	0.061**	0.012	-0.020	-0.008
	(0.026)	(0.019)	(0.015)	[0.581]	[0.212]	[0.743]
COMPLETE:	0.110**	0.128**	0.085**	0.018	-0.043*	-0.025
	(0.029)	(0.021)	(0.016)	[0.480]	[0.014]	[0.350]
DELETED:	0.066*	0.074**	0.078**	0.008	0.004	0.012
	(0.032)	(0.025)	(0.019)	[0.765]	[0.851]	[0.684]
	B. Site Status Change Estimates			D. Site Status Change Distance Coefficient Differences		
$\Delta PROPOSED$	0.001	0.044**	-0.003	0.043	-0.047**	
	[0.967]	[0.003]	[0.710]	[0.116]	[0.001]	
$\Delta FINAL$	0.037	0.014	0.019**	-0.023	0.005	
	[0.140]	[0.331]	[0.017]	[0.372]	[0.673]	
$\Delta COMPLETE$	0.041**	0.047**	0.024**	0.006	-0.023**	
	[0.004]	[0.000]	[0.000]	[.652]	[0.002]	
$\Delta DELETED$	-0.044*	-0.054**	-0.007	-0.010	0.061**	
	[0.045]	[0.001]	[0.563]	[0.645]	[0.004]	
Obs.			1,664,863			

Notes:The standard error is below each coefficient in parenthesis and clustered on houses.* and ** denote significance at the 5% and 1% levels, respectively. Columns (1)-(3) are coefficients from the same regression, arranged horizontally for comparability. Panel A provides the estimates from using site counts for each type in each distance band. Panel B provides the estimate of the various site status changes by distance band with p-values in brackets underneath. Panel C tests the equality of the coefficients across distance bands with p-values in brackets underneath. Panel D tests the equality of of site status change estimates as distance increases.

designating a site "Construction Complete" has positive, significant effects on price that are stronger at closer distances. Panel D reveals that this effect is significantly lower at the farthest distance band. Third, while Panel C indicates that the coefficient estimates for each site type don't vary by distance band, testing the equivalence of the effects of status change across distances reveal that the effect of proposing a site to the NPL, designating a site "Construction Complete", and deleting a site from the NPL are significantly different. The site status changes are not statistically different for the 0 - 1 km distance band and the 1 - 2 distance band, but moving to 2 - 3 km shows a marked change. Moreover, the estimate of the price effects of site status changes tend *towards zero* for the outer distance band. This result accords with intuition that the effects of Superfund sites should diminish as distance increases.

2.5.2 Robustness

In this subsection I provide results demonstrating the robustness of the results presented so far. First, I determine whether houses near Superfund sites are more likely to receive improvements relative to other houses. Second, I examine the assumption of a time-constant hedonic price surface and find that the preferences that give rise to the implicit prices appear to be relatively constant over time. Next, I vary the size of the exposure circles used in the main specification and show similar conclusions can be drawn about the Superfund program's effects. Finally, I compare my results that employ the site-counting method to more traditional nearest-site methods.

Unobserved Housing Quality Improvements

Concern remains that properties with unobserved changes are resident in the dataset. If changes in properties are not correlated in any way with Superfund site exposure, then unobserved property improvements should not bias any results. However, in a repeat-sales model, the price effects of proximity to Superfund sites are identified by changes in site status. If, for example, there is a correlation between home improvement and Superfund site remediation, the estimated price effects of hazardous waste cleanup will be biased upwards as it will be impossible to distinguish between those paying for improved environmental quality and those paying for improved housing.[13]

As a first-order test, it is possible to check whether the properties that have been dropped from the sample as properties with suspected major improvements are more likely to be in close proximity to Superfund sites than those that aren't likely to

[13] From a policy perspective, this might be irrelevant. If homeowners find it profitable to improve their houses when the environmental quality increases around their home, then unobserved quality and environmental quality are complements. Price improvements due to unobserved housing quality improvements that are a direct result of Superfund remediation should still be attributed to the Superfund program. However, for this paper, I will attempt to distinguish between the avenues that any price appreciation may take.

have received a major improvement. Recall that properties suspected of unobserved improvements and those marked has having major improvements are removed from the main dataset. To conduct the test, for each property I note which transaction(s) cause it to be removed from the main sample. Any property that is improved is then treated as improved for any subsequent transactions I observe. Since a repeat-sales model will be positively biased if homes are receiving unobserved improvements as Superfund sites are being remediated, I want to test the relationship between changes in housing quality and changes in Superfund site status.

Consider the following binary response model:

$$Pr(Y_{it} = 1 | Z = Z_{it}) = Z'_{it}\beta \tag{2.4}$$

where $Y_{it} = 1$ if house i is renovated by time t and $Y_{it} = 0$ otherwise, Z_{it} is a $(K \times 1)$ vector of hazardous waste measures for house i. In this linear probability model, if $\beta_k > 0$, then proximity to a site of status $k \in \{DISCOVERED, PROPOSED, FINAL, COMPLETE, DELETED\}$ increases the probability that a house would receive an unobserved improvement. However, the threat to identification of positive price effects attributable to Superfund site remediation comes from changes in site status being correlated with changes in unobserved quality. Accordingly, finding $\beta_{k+1} - \beta_k < 0$ provides some evidence that unobserved changes in housing quality are not correlated with advancing a Superfund site from site status k to $k+1$.

The identification strategy of the main specification uses within-tract variation to estimate price effects of Superfund remediation. Accordingly, to test this identification strategy, I want to estimate the binary response model in Equation (2.4) controlling for the same tract level unobservables as in the main specification to ensure that there is no within-tract correlation between remediation activities and unobserved improvements[14]. This concern motivates the use of a linear probability model (LPM), where fixed effects are more easily handled than in probit and logit

[14] I thank the anonymous referee who highlighted the distinction between testing the identification assumptions "within-tract" versus "between-tract".

Table 2.7: "Improved" Properties Probit Results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Dependent Variable = $I[IMPROVED = 1]$						Δ
$DISCOVERED$	0.067**					-0.002	
	(0.003)					(0.005)	
$PROPOSED$		0.016**				-0.045**	-0.042**
		(0.006)				(0.007)	[0.00]
$FINAL$			-0.037**			-0.109**	-0.065**
			(0.003)			(0.006)	[0.00]
$COMPLETE$				-0.053**		-0.162**	-0.053**
				(0.004)		(0.007)	[0.00]
$DELETED$					-0.043**	-0.186**	-0.024**
					(0.007)	(0.009)	[0.00]
$PRICE$	-0.000**	-0.000**	-0.000**	-0.000**	-0.000**	-0.000**	
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	

*Notes:*The robust standard error is below each coefficient in parenthesis. Column (7) provides p-values for the Wald test of equality between that row's coefficient and the previous one in brackets. * , and ** denote significance at the 5% and 1% levels, respectively. All specifications difference away time-varying neighborhood fixed effects and household level fixed effects.

estimators. The well-known drawback of the LPM is the potential for predicted probabilities to fall outside of the range $[0, 1]$. However, since the sign of the coefficient is main empirical question of this exercise, these concerns do not preclude its utilization.

Table 2.7 contains the results of the binary response model under various specifications for Z_{it}. Columns (1) - (5) use a count of the number of sites of the given type within 3 kilometers and prices at time t as regressors. Of the five site types, only $DISCOVERED$ and $PROPOSED$ sites within 3 kilometers of a house have a significantly positive impact on the probability of major improvement. $PROPOSED$, $COMPLETE$ and $DELETED$ all have significantly negative. Column (6) indicates including all sites types in the same regression provides stronger results. Here, proximity to all site types decreases the probability of seeing an improvement, with all but the coefficient for $DISCOVERED$ site counts being significant at the 1% level. Column (7) provides the difference in coefficients $\beta_{k+1} - \beta_k$ for moving a site from site status k to $k+1$. The corresponding p-values for the Wald test testing the equality of the coefficients are provided in brackets below each coefficient difference. For each transition in site status, the change in probability that a house receives an improvement is negative. The implication of these results is that designating a site "Construction Complete" or having a site deleted from the NPL nearby does not increase the probability that the house is significantly improved. However, this test only considers major and significant improvements since it is not possible to discern minor improvements in the data. On balance, Table 2.7 supports the assumption that major home improvements are not positively correlated with Superfund site remediation. If minor home improvements are correlated positively with major improvements, then these results might also suggest that bias resulting from minor improvements is slight.

Time Varying Preferences

The identification strategy for the main specification assumes that β is the same in all periods. This can be a troubling assumption given the twenty year sample period. Its very plausible that the hedonic price surface shifts throughout the time period. To check the sensitivity of the results to this assumption, I estimate a similar model to the main specification that allows preferences to change over time. I break the sample period into four 5-year periods and estimate separate coefficients for each.

Consider rewriting Equation (2.1) the following way:

$$\begin{aligned}\ln Price_{it} = \alpha X_i + \beta_1 Z_{it} * \mathbf{I}\{t \in E_1\} + \beta_2 Z_{it} * \mathbf{I}\{t \in E_2\} + \\ \beta_3 Z_{it} * \mathbf{I}\{t \in E_3\} + \beta_4 Z_{it} * \mathbf{I}\{t \in E_4\} + \delta_{jt} + \gamma_i + \varepsilon_{it} \quad (2.5)\end{aligned}$$

where $\mathbf{I}\{\cdot\}$ is the indicator function, $\{E_1, E_2, E_3, E_4\}$ correspond to the first, second, third and fourth 5-year era in the dataset, $\{\beta_1, \beta_2, \beta_3, \beta_4\}$ are the time varying parameters, and X_i, α, and Z_{it} have the same interpretation as in the main specification. If these parameters are not statistically different from each other, then identification concerns of the main specification should be mitigated. Note that this example differences away the unobserved error terms the in the same way as the main specification.

Table 2.8 provides the coefficient estimates for Equation (2.5). Columns (5) - (7) provide the p-values for the corresponding Wald equivalence tests, where p-values that

Table 2.8: Estimates of Time Varying Parameters

Years	(1) 1988 - 1992 $\beta_{1,k}$	(2) 1993 - 1997 $\beta_{2,k}$	(3) 1998 - 2002 $\beta_{3,k}$	(4) 2003 - 2008 $\beta_{4,k}$	(5) $\beta_2 - \beta_1$	(6) *p*-values $\beta_3 - \beta_2$	(7) $\beta_4 - \beta_3$
DISCOVERED	0.044**	0.052**	0.056**	0.029	.501	.800	.174
	(0.016)	(0.019)	(0.016)	(0.018)			
PROPOSED	0.066**	-	0.037*	0.132**	-	**.046†**	**.020**
	(0.019)		(0.018)	(0.040)			
FINAL	0.081**	0.072**	0.071**	0.066**	.146	.957	.214
	(0.017)	(0.017)	(0.016)	(0.016)			
COMPLETE	-	0.034	0.083**	0.128**	-	.433	**.000**
		(0.065)	(0.017)	(0.018)			
DELETED	-	-	-	0.090**	-	-	-
				(0.020)			
Obs.				1,664,863			

*Notes:*The standard error is below each coefficient in parenthesis and clustered on houses.* and ** denote significance at the 5% and 1% levels, respectively. Coefficients omitted for collinearity are marked with a "-". Boldfaced *p*-values indicate significance at the 5% level.
†This *p*-value corresponds to the Wald test $\beta_3 - \beta_1 = 0$ since *PROPOSED* is omitted in Column (2).

are significant at the 5% level are in boldface. The coefficients on *DISCOVERED* and *FINAL* appear stable over time. Preferences seem to shift from the third era to the fourth era for *COMPLETE*. However, four of the seven sites that were eventually designated "Construction Complete" were assigned that status between 2003 - 2008 and one "Construction Complete" site was deleted in that same span. From 1993-1997, just one site became "Construction Complete" and two sites attained this status from 1998 - 2002. Since most of the "Construction Complete" activity occurred in the last era, its not surprising that the estimates in that timeframe are much more precise than other timeframes, leading to a statistically significant change.

The variance in the estimates for *PROPOSED* is likely a function of the small window during which a site is proposed to the Final NPL. For the sites in the Los Angeles CSA, the median number of days a site stays proposed to the Final NPL is 248 days. Accordingly, the number of houses observed to transact near a site categorized as *PROPOSED* is much smaller than the other site statuses, leading to less precisely estimated preferences. Both sites that were deleted from the Final NPL were deleted in the final era which prevents the estimation of preferences in other time periods.

Table 2.9: Main Specification Under Various Radii

	(1) 1 km	(2) 3 km	(3) 5 km
DISCOVERED	0.004	0.039**	0.039**
	(0.016)	(0.013)	(0.009)
PROPOSED	-0.027	0.046**	0.051**
	(0.030)	(0.016)	(0.010)
FINAL	-0.006	0.062**	0.050**
	(0.021)	(0.015)	(0.010)
COMPLETE	-0.004	0.091**	0.081**
	(0.024)	(0.016)	(0.010)
DELETED	-0.009	0.073**	0.070**
	(0.027)	(0.018)	(0.013)
Tract-Year Fixed Effects	Yes	Yes	Yes
House Fixed Effects	Yes	Yes	Yes
Obs.			1,664,863

Notes:The standard error is below each coefficient in parenthesis and clustered on houses.* and ** denote significance at the 5% and 1% levels, respectively.

However, for the more evenly distributed site statuses $DISCOVERED$ and $FINAL$, preferences cannot be distinguished significantly intertemporally which supports the assumption made in the main specification of time-invariant preferences.

Additionally, it should be noted that in this specification, β is varying not only over time, but also over Superfund sites.[15] Since different sites are changing their status in each era, the coefficient in each period is a function of a unique set of sites. This fact may actually enhance the robustness of this test. The coefficients that change over time might actually not reflect changing underlying preferences, but rather reflect preferences for a different set of sites. However, coefficients that tend to be stable over time do so in spite of the fact that the set of sites is changing. While I cannot distinguish between the two causes for coefficients that do change, I can conclude that the other coefficients appear stable over time regardless of the changing sets of Superfund sites.

Varying Exposure Radii

Since the main results of this paper use the number of sites in a 3 kilometer radius around each house as the measure of Superfund exposure, the choice of 3 kilometers versus other distances needs to be examined. While the flexible distance specification examines heterogeneous effects within the 3 kilometer radius, it is unclear how sensitive the results are to the choice of 3 kilometers. In this subsection, I run the main specification of counting the number of sites of each type in a radius around each house at 2 different radii: 1 kilometer and 5 kilometers. Kiel and Zabel (2001) estimate the maximum distance for price effects to be three miles, or 4.828 kilometers, so I use 5 kilometers as a comparison on the larger side. Conversely, it is interesting to see how the estimates change when restricting the exposure window to only 1 kilometer.

Table 4.5 contains the results of the regression defined in Equation (2.1) under 1, 3, and 5 km radii, respectively. A comparison of Columns (2) and (3) reveals that changing the radius from 3 km to 5 km has little impact on the sign, significance and magnitude of the coefficients. However, when restricting the radius to 1 km the estimates all become insignificant. While intuition would suggest that the impact of Superfund sites on the housing market should be larger closer to the sites, these results suggest that homeowners that sort themselves in close proximity to Superfund sites might have a lower preference for avoiding hazardous waste exposure. Individuals who are responsive to the risks of living near Superfund sites live farther away, so as sites are remediated, homeowners that are sensitive to environmental quality capitalize the improvement at the farther distances.

Nearest-Site Results

The prevailing method for capturing exposure to Superfund sites is to use the distance to the nearest site as the measure of disamenity. If Superfund sites are clustered

[15] Thanks to the anonymous referee who elucidated this point.

and many houses are in close proximity to multiple sites, nearest-site methods can suffer from an omitted variables problem that the site-counting method will not. Conversely, if clustering of Superfund sites is not prevalent, then the site-counting method is similar to using distance to the nearest site. In the limit, if no house has more than one house nearby, then the site-count method reduces to the nearest-site method with the exposure measure becoming a simple function of distance equal to 1 if a site is within a certain radius, and 0 otherwise.

To compare the results using the site-count method to the traditional distance to nearest site method, I employ a proximity function similar to that found in Kiel and Zabel (2001):

$$PROX_i = \max(0, D_0 - D_i) \tag{2.6}$$

where D_i is the distance from house i to the nearest site and D_0 represents the maximum distance of Superfund site influence, which is set to 5 km.[16] To make a fair comparison, I maintain the use of both sets of fixed effects. Consider the following regression equation:

$$\ln Price_{it} = \alpha X_i + \beta' Z_{it} * PROX_i + \delta_{jt} + \gamma_i + \varepsilon_{it} \tag{2.7}$$

where β is a (5×1) vector of parameters, Z_{it} is a (5×1) vector of dummy variables indicating the status of the nearest site to house i at the time of sale, $PROX_i$ is the proximity function in Equation (2.6), and δ_{jt} and γ_i are the tract-year and house-level fixed effects, respectively.

Column (1) of Table 2.10 provides the results of estimating Equation (2.7) while Column (2) provides the differences in coefficients and the associated p-value from the Wald test of equivalence. The coefficients are most easily interpreted as the marginal value for a house with $PROX_i = 1$, or a house that is 4 km away from a Superfund site. These estimates are somewhat similar to those found in the main

[16] As mentioned in the previous section, Kiel and Zabel (2001) find $D_0 = 3$ miles, which is roughly 5 km.

results. Specifically, the largest and most significant improvement in price, 2.3%, comes when a site is designated “Construction Complete”. These results show an improvement in price when a site is proposed to the Final NPL whereas the previous results measured the improvement at final listing on the NPL. Both sets of results indicate no significant change in price when a site is deleted from the NPL.

Column (3) provides estimation results when additional regressors measuring the proximity to the second nearest site are included. The goal of this regression is to capture the extent to which multiple sites can contribute to an omitted variables problem. In the Los Angeles CSA, 21% of the Superfund sites are within 3 miles of another Superfund site. This could cause *a priori* concern that estimates of the value of distance from a Superfund site might be attenuated if as distance from one site increases, distance to a second omitted site decreases. A comparison of the results in Columns (1) and (3) appear to allay those concerns. Adding the additional controls for the second site has virtually no effect on the coefficients or in the magnitude and significance of the differences in coefficients, reported in Column (4). This implies that clustering of sites doesn’t appear to pose a significant problem in the Los Angeles area. When clustering isn’t a problem, the estimates from a “nearest-site” regression should be similar to the results under the main specification.

2.5.3 Discussion

One of the main goals of this study is to separately value the life-cycle effects of Superfund sites in a region. The positive impact on prices of designating a site as “Construction Complete” is the most robust finding. The effects are seen at small and large radii, are positive over time, and are invariant to the use of site-counts or distance to the nearest site. This result comes in contrast to those studies in the literature that focus solely on deletion from the NPL as the indicator of site remediation. My results suggest information about the remediation of Superfund sites is communicated well before deletion from the NPL takes place, causing the effect of

Table 2.10: Distance to Nearest Site Estimates

	(1) β_k	(2) Δ	(3) β_k	(4) Δ
$PROX_1$*$\mathbf{I}\{DISCOVERED\}$	0.019**		0.019**	
	(0.005)		(0.005)	
$PROX_1$*$\mathbf{I}\{PROPOSED\}$	0.029**	0.010**	0.030**	0.011**
	(0.006)	[.001]	(0.006)	[.000]
$PROX_1$*$\mathbf{I}\{FINAL\}$	0.031**	0.002	0.034**	0.004
	(0.006)	[.468]	(0.006)	[.236]
$PROX_1$*$\mathbf{I}\{COMPLETE\}$	0.054**	0.023**	0.057**	0.023**
	(0.006)	[.000]	(0.006)	[.000]
$PROX_1$*$\mathbf{I}\{DELETED\}$	0.046**	-0.008	0.049**	-0.008
	(0.007)	[.069]	(0.007)	[.061]
$PROX_2$*$\mathbf{I}\{DISCOVERED\}$			0.017	
			(0.014)	
$PROX_2$*$\mathbf{I}\{PROPOSED\}$			-0.002	
			(0.017)	
$PROX_2$*$\mathbf{I}\{FINAL\}$			0.036*	
			(0.015)	
$PROX_2$*$\mathbf{I}\{COMPLETE\}$			0.125	
			(0.098)	
$PROX_2$*$\mathbf{I}\{DELETED\}$			-0.022	
			(0.052)	
Tract-Year Fixed Effects	Yes		Yes	
House Fixed Effects	Yes		Yes	

Notes:The standard error is below each coefficient in parenthesis and clustered on houses.* and ** denote significance at the 5% and 1% levels, respectively. Column (1) provides the results from Equation (7) controlling for both sets of fixed effects. Column (2) provides statistical significance tests for site status changes. Column(3) adds controls for the second nearest site. Column (4) provides statistical significance tests for the specification in Column (3).

deletion to be insignificant. Additionally, my main results show that there is a significant increase in prices once a site is actually listed on the Final NPL, suggesting that the promise of future remediation registers positively on prices.

Second, the importance of controlling for unobserved heterogeneity cannot be overstated. While the importance of house-level fixed effects appear to be relatively greater, not controlling for both house-level and neighborhood-level unobserved quality will lead to biased estimates. Many of the existing site-specific studies ignore unobserved housing quality and control for neighborhood quality by using the most recent census data, which might not be constant throughout the sample period. Using tract-year fixed effects eliminates this problem, while effectively controlling for these unobserved elements.

Third, while the results of this study are only applicable to the Greater Los Angeles area, they are generalizable to any site in the region and the methods are generalizable to any metropolitan area where housing transactions data are available. A balance is struck between national studies, which impose homogeneity on Superfund sites and preferences, and site-specific studies, which allow for heterogeneous sites effects but are hard to apply to other settings. The result is a method that measures the idiosyncratic preferences of a region while averaging over a small sample of sites, allowing estimates to be applied to new sites outside of the current study.[17]

Finally, the importance of omitted variable bias in studies that utilize distance to the nearest Superfund site as the measure of exposure is examined and found to be minimal in the Los Angeles area. The results show that adding the distance to the second nearest site has little impact on the estimates for proximity. In this case, the results mimic those of the site-count method introduced in this paper. However, the mitigation of the omitted variable bias is likely do more to the use of house-level fixed effects than to the general robustness of nearest-site methods. In a fixed-effects model, distance to the second site is differenced away, unless the site changes status between sales. For any given supply of houses in close proximity to multiple sites, the subset of observations where the "second site" changes status simultaneously with the "first site" will be smaller, reducing the impact that the "second site" has on repeat sales estimates. Since the site-count method doesn't ignore the presence of multiple sites and provides similar results to existing methods in the absence of omitted variables bias, it is demonstrated to be a valuable addition to the literature on valuing environmental disamenities.

[17] Applying the estimates to sites within the study is also a benefit of this study. For instance, policymakers have an estimate of the value of taking a particular site that is currently on the Final NPL to "Construction Complete" status.

2.6 Conclusion

This paper argues policy relevant estimates of the price effects in the housing market of Superfund site remediation requires a regional or city level analysis. If policymakers require an estimate of the value of remediating a particular site, national sample estimates derived from the average preferences of U.S. homeowners, not of the local homeowners who will be affected by action, may be insufficient. Conversely, estimates derived from site-specific analyses might not be externally applicable if the analysis was in a different region or a different site, since estimates will be a function of a particular site's idiosyncrasies. Averaging over multiple sites and measuring the preferences of a particular region provides estimates that are applicable to other sites in the region of interest. This paper demonstrates a method for obtaining consistent estimates of the various life-cycle events of Superfund site remediation that is generalizable to any area where housing transactions data are available.

The results suggest that different actions undertaken by EPA communicate different things to the housing market. Listing a site on the Final NPL is shown to increase nearby housing prices by 1.6%. This suggests that at the time of listing, the externality of hazardous waste proximity was already capitalized and the promise of remediation is enough to move prices upward. Once EPA designates a site as "Construction Complete", local housing prices gain an additional 2.8%. Deletion of an NPL site seems to have no significant impact on prices. While the absence of an effect for deleting a site from the NPL is in line with estimates from previous research, the results for the impact of listing a site on the Final NPL and designating a site as "Construction Complete" demonstrate a break from the literature that attempts to estimate average effects across multiple sites.

The lack of evidence of a positive price impact of Superfund site remediation from hedonic pricing studies has called into question the return on investment for the high cost of site remediation. This paper demonstrates a clear, positive impact on prices as a result of the Superfund program, the largest of which comes directly from the act of cleaning a site, not deleting it from the NPL. While this result comes with the caveat of being applicable only to the Los Angeles CSA, the point is made that one-size-fits-all national estimates obscure the heterogeneity in regional preferences. There is no doubt that there are certain areas of the country which are likely to place low value on site remediation and some areas which will place a high value on site remediation. Any remediation decision should be made in reference to the preferences of the local populace, not the average preferences of the nation or the idiosyncratic preferences of homeowners around one particular site remediated in the past. This study establishes the influence of this distinction in approach on conclusions drawn about the benefits to the housing market of the Superfund program.

3

A Dynamic General Equilibrium Analysis of the Equity Implications and Welfare Effects of Disproportionate Exposure

3.1 Introduction

The past quarter century has seen a rise in interest from policy makers, researchers, and community stakeholders in the equity effects of environmental policy. Those concerned with the distributional inequities of pollution exposure argue that minorities and low-income households have been disproportionately burdened by environmental pollution and are unfairly targeted by the siting of environmental disamenities such has hazardous waste disposal facilities or toxic manufacturing facilities. Policy makers have been responsive to these arguments. In February, 1994, President Clinton issued Executive Order 12898, mandating that all federal agencies consider all disproportional environmental impacts that federal policies and regulations might have on disadvantaged populations.

Two landmark studies by the Government Accounting Office (1983) and the Commission on Racial Justice (1987) documented the existence of disproportionate exposure to pollution by providing evidence that hazardous waste facilities were more likely to be located in minority areas. While there were some studies that contradicted the finding that minorities were disproportionately targeted,[1] the subsequent literature produced a plurality of evidence that minorities and low income households were bearing an unequal burden.[2] This paper takes disproportionate exposure to pollution as the point of departure and examines the outstanding debate over its cause. The "minority move-in" or "market dynamics" hypothesis suggests that disproportionate exposure can be explained by minority and low-income households choosing to exchange higher exposure levels for lower housing prices. The disproportionate siting hypothesis maintains that firms and regulators target low-income and minority neighborhoods when deciding where to locate pollution generating facilities. Properly distinguishing between these alternative stories has important implications for the formation of policy to address the inequity.

To address these research questions, I examine homeowner location decisions in the San Francisco-Oakland-San Jose Metropolitan Statistical Area, also known as the "Bay Area." First, using a detailed single-family housing transactions database, I set up and estimate a dynamic housing selection model based on the estimator of Bayer et al. (2011) (henceforth BMMT.) Households receive utility each period from neighborhood amenities, are forward-looking, and face moving costs if they decide to relocate. Given the dynamic nature of the problem, it is necessary to account for these features in order to accurately describe the propensity for households to sort in response to pollution exposure. The model allows me to estimate households marginal willingness to pay to avoid toxic emissions from, and proximity to, facilities in the EPAs Toxic Release Inventory (TRI) program. These facilities manufacture, process, and/or release large quantities of toxic chemicals into the air and groundwater, and are required to report the quantity of annual releases to the EPA, pursuant to the

[1] See, for example, Anderton et al. (1994)

[2] See Been (1993); Been and Gupta (1997); Pastor et al. (2001, 2005); Morello-Frosch et al. (2001); Sadd et al. (1999)

Emergency Planning and Community Right to Know Act.

Second, using insights from the Industrial Organization literature on dynamic oligopolies, I propose a new dynamic general equilibrium sorting framework that represents an improvement on the existing general equilibrium residential sorting models found in the public and environmental economics literatures. The available general equilibrium models of housing selection are all static.[3] A static sorting model is insufficient to answer the research questions adequately for several reasons. First, I am interested in the equilibrium dynamics that result from a change in pollution in order to characterize the extent that different racial groups contribute to minority move-in. Counterfactuals in a static model generally ignore the transition dynamics. Second, static models make it difficult to incorporate moving costs and wealth accumulation, two important aspects of homeownership. Ignoring moving costs can over-predict equilibrium re-sorting, which could lead to erroneous conclusions about minority move-in and gentrification. Furthermore, changes in wealth that result from a change in the amount of surrounding pollution are an important component to welfare calculations. Ignoring the impact of wealth on households misrepresents their willingness to pay.

With few exceptions,[4] researchers have employed reduced form evidence to try to characterize minority move-in and disproportionate siting. However, an increase in an area's minority population following an exogenous increase in pollution, for example, is insufficient to identify minority move-in since this data doesn't reveal where the new residents were prior. Identification of minority move-in requires minority households to increase their exposure to pollution.[5] My model allows me to simulate household behavior to directly test the "minority move-in" hypothesis by looking for correlations between migration flows and changes in pollution exposure. With a

[3] Bayer et al. (2010); Ferreyra (2007); Sieg et al. (2004)

[4] Depro and Timmins (2009); Depro et al. (2011)

[5] See Depro et al. (2011) for a full discussion and examples of the failure of identification in reduced form analyses.

dynamic general equilibrium simulation model, it is possible to determine whether minorities follow degradations in environmental quality in exchange for lower prices and if white, high-income households pay higher prices to flee pollution.

Moreover, the disproportionate siting hypothesis would be supported if low-income and minority neighborhoods, which tend to be over exposed to pollution, have lower willingesses to pay to avoid exposure. This variation could provide economic incentives for firms to target these populations. If households have heterogeneous preferences over environmental quality and the socio-economic status of their neighbors, accurate willingness to pay estimates must account for any equilibrium sorting behavior that would result from a change in pollution. Since differences in willingness to pay are at the core of the disproportionate siting hypothesis, a general equilibrium framework is needed. I am able to directly estimate general equilibrium willingness to pay by income and race and aggregate to the neighborhood level to quantify the variation in motivation to resist the generation of externalities.

Partial equilibrium estimates from the model reveal important heterogeneity in preferences across both race and income. At the margin, I find that low-income households are willing to payup to $22 annually for a ten percent reduction in toxic emissions at the mean exposure level, while high-income households are willing to pay up to $41, which is approximately 85% higher. Additionally, whites are willing to pay up to $273 to avoid an additional TRI facility in their neighborhood, while minorities are willing to pay up to $163, which is 40% less. After calculating the general equilibrium willingness to pay for a non-marginal increase in TRI facilities and emissions, I find that white households' general equilibrium willingness to pay avoid a two-facility increase in their neighborhood is up to 57.39% higher than partial equilibrium estimates. Further, minority general equilibrium willingness to pay to avoid a much as 20.92% *lower* than the corresponding partial equilibrium estimates. Intuitively, since white and high income households are estimated to have a higher willingness to pay for emissions reductions, an exogenous increase in emissions leads to the neighborhood increasing the fraction of minorities and decreasing in mean income. As a result, minority households that remain now reside near more households of their own race, which offsets the welfare loss of increased pollution.

Simulations reveal that low-income, low-wealth households of both races have a tendency to accept higher pollution levels in exchange for lower prices. Conversely, high-income, high-wealth households have a tendency to move to more expensive neighborhoods in exchange for lower emissions. Even though this behavior isn't exclusive to minorities, it nevertheless conforms to the spirit of the "minority move-in" hypothesis. Moreover, I calculate the difference in aggregate willingness to pay between a representative white and minority neighborhood for a one unit increase in pollution and find the willingness to pay is one and a half times larger in the representative white neighborhood. Such large differences in willingness to pay could incentivize polluters to seek neighborhoods that have less motivation to resist.

The remainder of this paper proceeds as follows: Section 2 reviews the relevant recent literature, Section 3 describes the sources of data and the nature of the TRI program, Section 4 documents the dynamic housing model being employed, Section 5 outlines the four-stage estimation procedure, Section 6 provides preference estimates and marginal willingness to pay estimates, Section 7 discusses the proposed counterfactual scenarios, and Section 8 concludes and discusses extensions of this research.

3.2 Literature Review

Following the literature documenting disproportionate exposure, also known as the "environmental justice" literature, researchers began to ask whether minorities were being targeted by siting decisions or were they "moving to the nuisance," following pollution and associated lower housing costs. This question was raised by Been (1993) and later addressed by Been and Gupta (1997). Looking at the changes in demographics after siting of toxic storage and disposal facilities (TSDFs), they found no significant effect attributable to the placement of the facility. Pastor et al. (2001)

find that disproportionate siting contributes more to the unequal exposure of minorities to TSDFs in Los Angeles county than does "minority move-in". More recently, Banzhaf and Walsh (2008) provided evidence that households do migrate in response to changes in toxic emissions from facilities that participate in the U.S. Environmental Protection Agency's (EPA) Toxic Release Inventory program. Additionally, Depro and Timmins (2009) provide evidence that minority and low-income households make a trade off between lower air quality and more housing services, oftentimes deciding to take on more air pollution in exchange for a larger house.

Second, researchers began to ask how economic incentives could give rise to disproportionate siting. Hamilton (1995b) suggests three economic explanations for disproportionate siting. First, policy makers and industry decision makers gain direct utility from exposing minorities to pollution, or the "Racism" explanation. Second, polluters and communities participate in Coasian bargaining, where the firm seeks to release pollution in communities that would require the least compensation for internalizing the externality, or the "Coasian" explanation. Lastly, polluters target communities that are least likely to organize and take political and legal action to obstruct the releases of the polluter, or the "Collective Action" explanation. Finding no correlation between siting and demographic variables that would be associated with low willingness to pay for environmental quality, he concludes that the most likely explanation is Collective Action.[6]

Additionally, this paper is related to several strands of research outside of the environmental justice literature. First, as previously mentioned, there is a large literature that uses static sorting models to estimate marginal willingness to pay for local amenities. Second, several papers have postulated general equilibrium models of local jurisdictions that push forward willingness to pay estimation to include equilibrium

[6] While Hamilton (1995b) distinguishes between Coasian Bargaining and Collective Action, I will consider these the same motivations. Both explanations predict that polluters, who have the right to pollute, will choose the "path of least resistance." This means that the community with the lowest incentive to resist, due to some combination of low willingness to pay for clean environment and high negotiation costs, will be targeted by the polluter.

behavior. Third, this paper contributes to the growing analysis of the EPA's Toxic Release Inventory program.

Many authors have turned to structural sorting models to estimate the marginal willingness to pay of local amenities. Epple and Sieg (1999) postulates a vertical sorting model and uses observed differences in the distribution of income within and across neighborhoods to identify the marginal willingness to pay for public goods. Others have used horizontal sorting models to value racial characteristics of neighborhoods, (Bajari and Kahn, 2005; Bayer et al., 2007), school quality (Bayer et al. (2007)), and congestion (Bayer and Timmins, 2005, 2007).

There are also several recent examples of general equilibrium models. A few papers build off the vertical equilibrium model of Epple and Sieg (1999) to study air quality (Sieg et al. (2004)) and open space (Walsh (2007)). Bayer et al. (2010) construct an equilibrium sorting model to study racial segregation, and Timmins (2007) values the impact of climate change in Brazil. Ferreyra (2007) studies the impacts of private school vouchers and residential choices. All of the above models, however, are static in nature.

Additionally, there is a growing literature related to studying the Toxic Release Inventory (TRI) program. Greenstone (2003) uses the TRI database to conclude that there is no evidence that the Clean Air Act Amendments caused polluters to switch the media of pollution from air to ground or water. Hamilton (1995a) found that at the inception of the TRI program, publicly traded corporations experienced significant, negative returns on their stock prices as a result of the quantity of pollution they released became public. Hamilton (1999) found that after the inception of the TRI program, plants whose toxic pollution portfolio resulted in a higher number of expected cancer cases reduced their pollution emissions by a greater amount. Brooks and Sethi (1997) finds that TRI emissions exposure is higher for minorities, lower income groups, groups with lower educational attainment, and groups that vote less often. Bui and Mayer (2003) perform a hedonic analysis at the zip code level that utilizes fixed effects and find no relationship between changes in TRI emissions exposure and prices. Banzhaf and Walsh (2008) find that households migrate over time in response to changes in TRI facility emissions. Further, they find an increase in the amount of emissions in a neighborhood is associated with lower incomes. The present study seeks to contribute willingness to pay estimates to avoid residing near these facilities.

3.3 TRI Program and Housing Data

The data in this paper come from three main sources. Data on the location of and releases from the facilities in the Toxic Inventory Release program are published yearly by the U.S Environmental Protection Agency. Housing transactions data are derived from Dataquick Information Systems. Income and racial information of homeowners is matched from publicly available data published by the Federal Financial Institutions Examination Council pursuant to the Home Mortgage Disclosure Act. I will discuss each of these elements in turn.

3.3.1 Toxic Release Inventory Program

The Emergency Planning and Community Right-to-Know Act (EPCRA) was signed into law by President Reagan on October 17, 1986. EPCRA had its roots in two separate chemical spills by the Union Carbide Company. First, on December 3, 1984, forty tons of methyl isocyanate gas (MIC), a compound that is lethal to humans, was released into the air at a pesticide manufacturing plant in Bhopal, India, resulting in tens of thousands of casualties. Then, on August 11, 1985, MIC was released from a Union Caribide plant in Institute, West Virginia, although on a much smaller scale, resulting in no deaths. These events motivated the passage of EPCRA in order to prepare for an emergency chemical spill and to properly inform the public of all potential risks of nearby industrial facilities.

Section 313 of EPCRA required the EPA to create the Toxic Release Inventory (TRI) by collecting and making public information relating to the possession and release of certain toxic chemicals by facilities in certain industries. The TRI database contains the quantities, types, and release pathways of toxic chemicals released by eligible facilities, as well as the location of those facilities. While this information is self-reported to the EPA, the agency has the ability to levy civil penalties for violations of ECPRA, and can force the rectification of those violations. The TRI program, however, is simply an accounting and reporting program; there are no limits or controls on the releases of chemical compounds.[7] For each reporting year, eligible firms are required to file a "Form R," detailing their chemical use profile by July 1st of the following year.

Data under the TRI program first became available in 1987. Since its inception in 1987, there have been four major changes to the program.[8] First, in 1994, the "Phase 1" expansion added 286 chemicals to the inventory list, bringing the total to 602 chemicals. Second, "Phase 2" expanded the range of facilities to include non-manufacturing facilities within the same industry classification codes. At the time, EPA estimated that this would require an additional 6,000 new facilities to begin participation in the program. Phase 1 became effective in the 1995 reporting year data and Phase 2 became effective in the 1998 reporting data. Third, effective in 2000, the courts ruled phosphoric acid was not subject to reporting under EPCRA, and was dropped from TRI. Lastly, effective in the 2001 reporting year, the minimum threshold for lead and lead compounds usage was reduced from 10,000 pounds per year to 100 pounds per year. Each of these changes has the potential to significantly alter the set of facilities in the Bay Area that are required to report over the course

[7] However, several chemicals that firms are required to report under the TRI program are regulated under other statutes, such as the Clean Air Act Amendments (CAAA) or the Resource Conservation and Recovery Act (RCRA).

[8] In addition, there was a voluntary emissions reduction program, the "33/50" initiative, coordinated under the TRI program from 1991-1995. Gamper-Rabindran (2006) found little evidence that this program effectively reduced emissions.

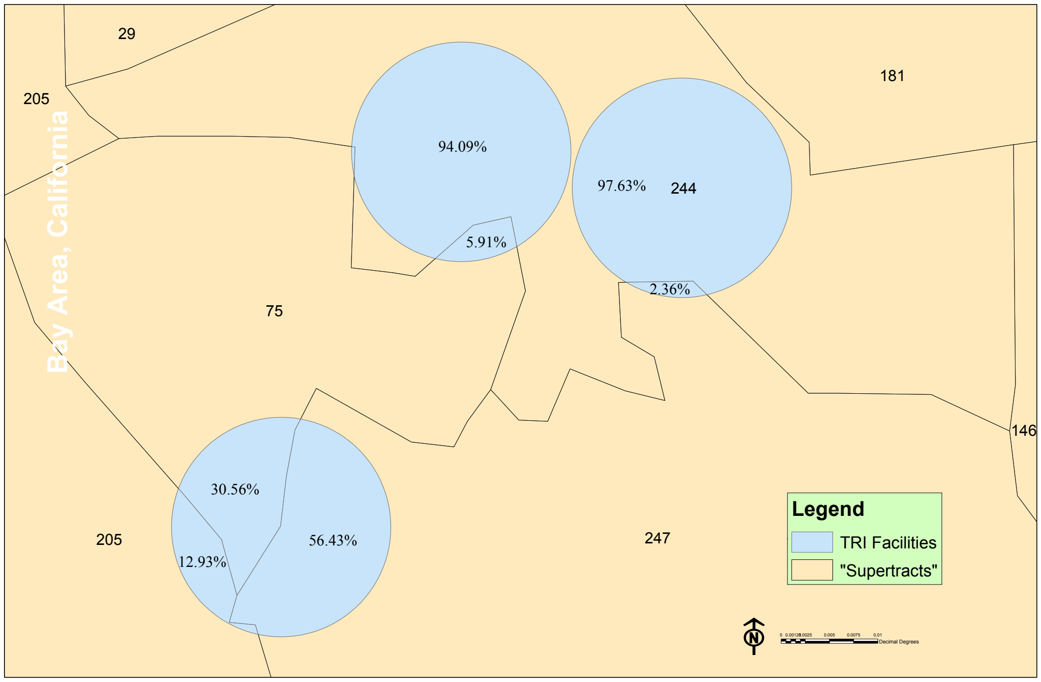

FIGURE 3.1: TRI Facility - Neighborhood Assignments

of the study period.

TRI facility release and location data is available for each year from 1987 to 2009. However, not every facility reports in every year. This could be a result of production activities ceasing, production activities being reduced such that the total quantity of toxic chemicals falls below the threshold, or a plant switching production to output that doesn't require the use of toxic chemicals. As a practical matter, I will treat each facility as existing in years between the first observed year and the last observed year. Using the longitude and latitude of each plant, I can match the facilities panel data set to the housing data set using GIS software. Further, because of the shifting reporting requirements before 2001, I will only use TRI data from 2001 and after.

TRI facilities can influence residents, and consequently the housing market, in several ways. First, the facilities generally release quantities of toxic chemicals into the local environment. Second, the facilities can be visual disamenities. I follow the lead of Banzhaf and Walsh (2008) to match TRI facilities and their emissions to neighborhoods. Using ArcGIS mapping software, I plot all of the TRI facilities on a map of the neighborhoods in the Bay Area. Then, I draw a circle with a radius of 1 kilometer around each site. The circle represents the facility's area of impact. Next, I calculate the area of the circle that overlaps with each neighborhood. The facility's emissions in that year are assigned proportionally to the adjacent neighborhoods based on the fraction of the area of the facility's radius in each neighborhood. Similarly, to control for the effects caused by sites simply existing, I distribute the "existence" of each facility according to the same area-weighted proportions.

Figure 3.1 provides an example. The circles are the 1 kilometer buffers centered around a TRI facility. The percentages represent the percent of the area of the circle that intersects with a neighborhood. Take, for example, neighborhood 75. This neighborhood has 30.56% of Site 1 and 5.91% of Site 2. In this case, I will take 30.56% of Site 1's emissions and 5.91% of Site 2 emissions and assign them to neighborhood 75. I will therefore count neighborhood 75 as having $.3056 + .0591 = .3647$ sites. Finally, I will divide by the area of neighborhood 75 to get both an emissions density and a TRI facility density. In the example above, neighborhood 247 has 0.5879 sites. However, since it is clearly much bigger than neighborhood 75, its site density will be lower.

3.3.2 Housing Data

The complete housing transactions dataset used in this paper comes from a matching of housing transactions data from the San Jose-San Francisco-Oakland Combined Statistical Area to home mortgage applicant data made available pursuant to the Home Mortgage Disclosure Act (HMDA). The housing transactions database contains information on every residential property sold from 1988 to 2008 in the six counties of Alameda, Contra Costa, Marin, San Francisco, San Mateo, and Santa Clara. A wide range of structural characteristics, such as square footage, lot size, number of bedrooms and bathrooms, geographic coordinates, census tracts, transaction price, loan amounts, mortgage lender names, and the full names of both the buyer and seller.

The features of the transactions database allow me to follow both houses and residents over time. Properties are uniquely identified in the dataset, so it is possible to know exactly when a former buyer decides to move. Additionally, the presence of buyer and seller names permits me to follow some sellers over time, provided they decide to purchase a home in the same metropolitan area. The first feature informs me whether or not a particular household decided to move or stay in their current house in every period. This information will be used in the second stage of estimation to estimate moving costs. The second feature provides information about the relative frequency at which households choose the "outside option."

The HMDA dataset is an anonymous compendium of all mortgage applications received by eligible mortgage lenders that details, amongst other things, demographics like the sex, race, and income of borrowers, as well as census tract, county, and loan amount. I am able to match individual housing transactions to the mortgage demographic information via an algorithm that looks to match loan application data to actual sales. The algorithm searches for matches based on loan amount, year, county, census tract, and lender name. Matches are ranked in quality, and I use only high quality matches.[9]

As previously mentioned, households are modeled as choosing over neighborhoods. To create neighborhoods, I utilize an algorithm that amalgamates adjacent census tracts until the combined group of tracts has a population as close to 5,000 owner occupied housing units as possible. This process ensures that observed differences in aggregate neighborhood choices aren't due to differences in housing supply or physical size. Neighborhood prices are taken to be the median observed transaction price in each year.

Additionally, since it is possible to know the race of a seller if they buy again in the dataset, a subset of transactions can be used to create an estimate of the flows of

[9] For example, a match of exact loan amount, location and lender name would be the highest match quality. Matching a loan amount that varies by $10,000 and has a 1 digit discrepancy in the census tract identifier would be a lower quality match.

different race groups into and out of each neighborhood in each period. Using decennial racial data as guideposts, I update the racial mix in each neighborhood for each period under study. The minority share in each neighborhood is taken as given from the 1990 Census. In each year and each neighborhood, the transactions for which the buyer and seller are known are used to calculate a flow of each race group. The flows are linked over time to update the stock of each race in each period. These stocks are taken as the minority shares in each neighborhood.

Minority share of a neighborhood is one of the major amenities being controlled for in this study. Since it is derived from the housing transactions data, it is necessary to understand how much measurement error it may introduce. A natural test of the accuracy of the imputed neighborhood mean income and racial percentages is to compare my predicted values for year 2000 to the actual reported Census figures for the year 2000. When comparing my data to the census data, the correlation coefficients for percentage of Hispanic residents, percentage of African American residents, and percentage of white residents are all over 0.98. Figure 1 provides a scatterplot of my predicted values against the reported census values. I've added a 45 degree trend line for reference.

3.3.3 Additional Neighborhood Amenities

To further control for the heterogeneity in neighborhoods, I add neighborhood level data on violent crime rates and ground-level ozone pollution. Violent crime data is drawn from the RAND California Community Statistics database. In the six counties in the Bay Area, violent crime rates are available for 80 separate cities. The violent crime rate is defined as the number of violent crimes per 100,000 residents. Violent crimes include rape, homicide, aggravated assault and robbery. To calculate a neighborhood level violent crime rate for each year, I first calculate the distance-weighted average crime rate for each property in the sample using longitude and latitude coor-

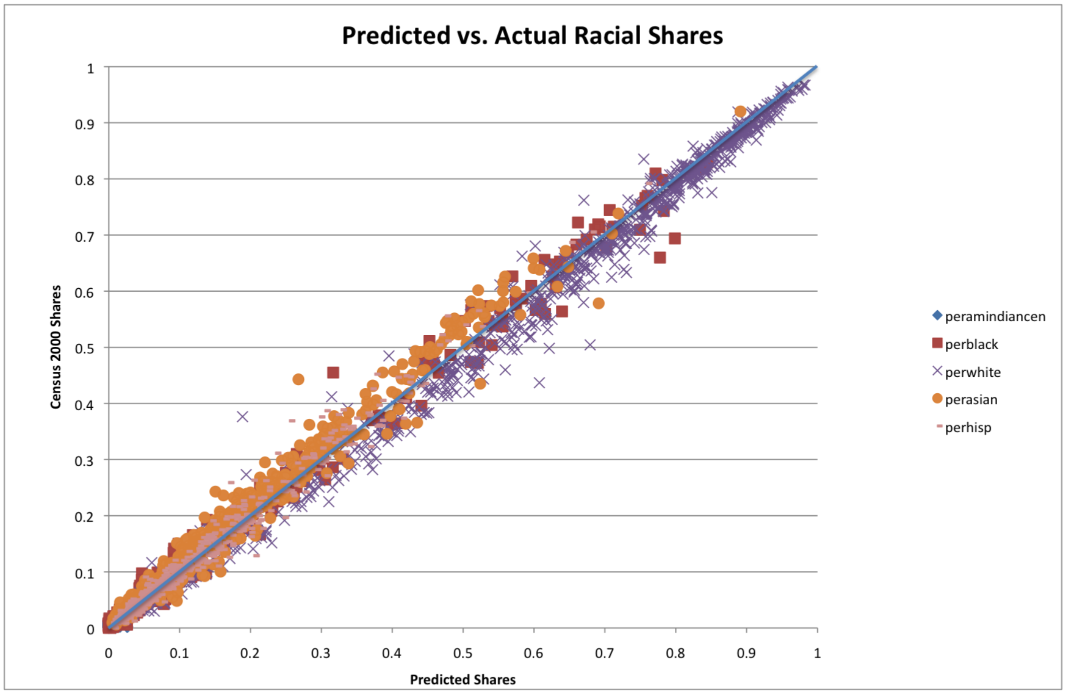

FIGURE 3.2: Neighborhood Attribute Validation

dinates. Then, I take the average violent crime rate over all houses observed to sell in that neighborhood in that year.

Ground-level ozone data is available from the California Air Resources Board (CARB). There are several different measures of ozone concentrations provided. I chose to use the same measure used by the EPA to set the National Ambient Air Quality Standards (NAAQS) pursuant to the Clean Air Act. For a county to attain the standard for ground level ozone, the three year average of the fourth-highest daily maximum 8-hour average ozone concentration at each monitor must not exceed 0.075 parts per million (ppm).[10] To be clear, to calculate this measure, the designated state-level authority takes ozone readings every hour. For each day in a calendar year, they calculate the highest average concentration over an 8 hour period. Within each year, these readings are ranked and the fourth highest reading within a year is averaged with the corresponding reading from the previous two years. To calculate

[10] This is the standard as of May 27, 2008. The previous standard was 0.08 ppm.

Table 3.1: Housing Transactions Summary Statistics

	Mean	Standard Dev.	Min	Max
Year Built	1965	22.37	1838	2008
Square Footage	1,553	646.6	400	20,000
No. of Bathrooms	1.981	0.727	0.500	10
No. of Bedrooms	3.004	0.950	1	10
Price (000's)	512.2	264.3	82.22	2,321
Wealth/Down Payment (000's)	133.4	133.7	-1,429	2,168
Income (000's)	130.1	113.7	0	9,007
Loan Amount (000's)	378.8	178.1	5.069	2,630
Applicant Asian	0.285	0.452	0	1
Applicant Afr. American	0.035	0.184	0	1
Applicant Hispanic	0.187	0.390	0	1
Applicant White	0.493	0.500	0	1
N				307,473

Notes: Prices , wealth, and incomes are reported in thousands of 2000 dollars. Properties with over 20,000 sq. ft., ten bathrooms, or ten bedrooms are dropped from the sample.

neighborhood level ozone concentrations, I follow a procedure analogous to that which was used for the crime data. A distance-weighted average of readings is calculated for each house using the distance between a given house and each of CARB's pollution monitors. For each year and neighborhood, the neighborhood ozone level is the average house-level concentration level taken over houses that have sold in that year.

3.3.4 Summary Statistics

Table 3.1 provides the summary statistics for the housing transactions data covering years 2001 to 2008. Prices and incomes have been indexed by inflation to year 2000 dollars for comparability over time. In this version of the paper, I am dividing race into two categories, whites and minorities. This provides a 50/50 split in the data in terms of race. However, it is clear that the minority group comprises three separate groups, Asians, Hispanics, and African-Americans, in disproportionate amounts. Future research will include breaking out minority into its constituent parts. Later results on the preferences of minorities should be viewed with this definition of mi-

Table 3.2: Neighborhood Attribute Summary Statistics

	2001				2008			
	Mean	Std. Dev	Min	Max	Mean	Std. Dev	Min	Max
Panel A. All Neighborhoods								
Price	419.4	167.1	161.1	1,280	479.1	235.5	125.2	1,403
Mean Income	123.8	39.29	60.89	300.1	137.5	44.24	70.96	332.7
% White	0.659	0.201	0.0599	0.973	0.608	0.209	0.048	0.967
TRI Plant Density	0.0425	0.102	0	0.785	0.0295	0.0682	0	0.4420
Weighted Emissions	400.3	2,147	0	26,668	260.0	1,642	0	19,228
Violent Crime	382.8	180.0	118.7	1,249	348.2	171.8	98.32	1,035
Ozone Concentration	0.0570	0.00851	0.0408	0.0788	0.0589	0.00709	0.0409	0.0795
No. of Obs				270				270
Panel B. Non - TRI Neighborhoods								
Price	464.6	168.6	191.8	1,280	508.1	233.4	144.6	1,403
Mean Income	134.1	40.43	77.07	300.1	144.4	46.61	83.35	332.7
% White	0.718	0.188	0.249	0.973	0.636	0.215	0.195	0.967
TRI Plant Density	0	0	0	0	0	0	0	0
Weighted Emissions	0	0	0	0	0	0	0	0
Violent Crime	354.6	163.6	118.7	1,121	333.7	155.6	98.32	998.4
Ozone Concentration	0.0574	0.00851	0.0424	0.0788	0.0584	0.00698	0.0426	0.0795
No. of Obs				139				153
Panel C. TRI Neighborhoods								
Price	371.5	152.0	161.1	1,223	441.1	233.9	125.2	1,328
Mean Income	112.8	34.98	60.89	290.1	128.5	39.33	70.96	312.9
% White	0.597	0.195	0.0599	0.922	0.572	0.195	0.0479	0.903
TRI Plant Density	0.0875	0.132	8.25e-06	0.785	0.0681	0.0902	9.65e-05	0.442
Weighted Emissions	825.0	3,031	0	26,668	599.9	2,459	0	19,228
Violent Crime	412.8	192.0	159.4	1,249	367.2	189.9	120.0	1,035
Ozone Concentration	0.0565	0.00851	0.0408	0.0787	0.0594	0.00721	0.0409	0.0787
No. of Obs				131				117

Notes: Prices and incomes are reported in thousands of year 2000 dollars.

nority in mind.

Table 3.2 provides the summary statistics for neighborhood attributes. The table has three panels: Panel A is the full sample of neighborhoods, Panel B is the subset of neighborhoods that have no TRI facilities, and Panel C is the subset of neighborhoods with TRI facilities. Additionally, statistics are provided for 2001 and 2008, the first and last year of the data, respectively, and the change between 2001 and 2008. Panel A reveals some important trends occurring in the Bay Area over this period. First, there is price appreciation in housing values. Second, the average income of residents is 11.1% higher in 2008 than in 2001. Third, the average percentage of whites in a neighborhood drops five percentage points. Fourth, the number of TRI facilities that are active are decreasing. Finally, violent crime rates are falling while ground level ozone concentrations are rising slightly. The fact that during the sample period the Bay Area is becoming more expensive, richer, less white and less polluted underscores the importance of utilizing a dynamic model since households' expectations over how these variables evolve over time weigh heavily on their decision making.

Comparisons between Panel B and Panel C reveal that the set of neighborhoods with TRI facilities appear to be quite different from the set with no TRI facilities. In 2001, neighborhoods with a zero TRI facility density have approximately 25% higher housing prices, 12.1% less minorities, 18.9% higher average incomes and 14.1% lower violent crime rates compared to their counterparts. In 2008, non-TRI neighborhoods have 15% higher prices, 6.4% less minorities, 12.4% higher average incomes and 9.1% lower violent crime rates. Moreover, the two sets of neighborhoods are changing in different ways. The neighborhoods with no TRI facilities in 2001 saw their prices rise 15.67% over the period, compared to a 12.29% increase for the other neighborhoods. Additionally, minority shares in non-TRI neighborhoods increased at a slower rate. The percentage of white residents in non-TRI neighborhoods dropped by 4.67%, compared to a 5.62% drop in TRI neighborhoods. Finally, crime rates dropped faster in neighborhoods without any TRI facilities in 2001.

3.4 Dynamic Model

The household decision model builds on recent work of BMMT. In this setting, infinitely lived households $i \in \{1, \ldots, N\}$ must decide each period t upon a neighborhood $j \in \{0, 1, \ldots, J\}$ in which to reside. Households are differentiated by their wealth, income, and race. Wealth is determined by the equity a household has in its home. As housing prices fluctuate, or households decide to move and pay moving costs, their wealth is affected. Accordingly, their expectations must account for how prices will contribute to their future utility as it relates to wealth accumulation and the incidence of moving costs. Moreover, since moving is costly, households must forecast how their flow utility is likely to change in future periods since they may be, in effect, "locked in."

Since agents are infinitely lived and their decisions depend on the state of the world today and expectations over future states, the problem can be formulated as a standard dynamic programming problem. There are both observed and unobserved state variables. Observed state variables include neighborhood observables, X_{jt}, individual characteristics, Z_{it}, and a decision variable, $d_{i,t-1}$. X_{jt} includes attributes of the neighborhood such as the average price of housing, the percent of residents who are white, and environmental quality. Individual characteristics, Z_{it}, include a household's wealth, income, and race. The decision variable, $d_{i,t-1}$ records the neighborhood that the household chose in the last period.

The unobserved state variables comprise an unobserved neighborhood attribute, ξ_{ijt}, and an unobserved idiosyncratic stochastic term, ϵ_{ijt}. The unobserved neighborhood attribute is allowed to vary across individuals, which is a generalization of the way that unobserved heterogeneity typically enters in the I/O literature.[11] ϵ_{ijt} is a random utility shock that affects the utility that household i derives from neighborhood j in period t. For simplicity, let $\Upsilon_t = \{X_{jt}, \xi_{ijt}\}_{j=1}^{J}$ be the information set available to household i at time t. Then, the complete state vector includes the information set, individual characteristics, and last period's decision, and will be denoted as $s_{it} = \{\Upsilon_{it}, Z_{it}, d_{i,t-1}\}$.

The full flow utility in this model encompasses the utility that a household derives from the neighborhood attributes of their residence and any moving costs that must be paid in that period. Letting $u_{ijt} = u(X_{jt}, Z_{it}, \xi_{ijt}, \epsilon_{ijt})$ and moving costs be

[11] A whole class of models originated by Berry (1994) and Berry et al. (1995) treat the unobserved attribute as vertically differentiating characteristic, agreed upon in value by all buyers. In practice, ξ varies by individual types.

a function of the states, $MC_{it} = MC(Z_{it}, X_{d_{i,t-1},t})$, the full flow utility for household i in period t is given by $u_{ijt}^{MC} = u_{ijt} - MC_{it}I[j \neq d_{i,t-1}]$. It is important to note that moving costs are only a function of the household's attributes and the attributes of the neighborhood that is being left. Since selling a house typically involves paying a real estate broker 6% of the selling price, it is natural to think of moving costs being determined by the conditions of the place a household would be leaving. States transition according to a Markov probability process $q = q(s_{it+1}, \epsilon_{it+1}|s_{it}, \epsilon_{it})$ and β is the discount factor.

Agents maximize their lifetime expected utility function, choosing a sequence of locations $\{d_{it}\}$ to maximize:

$$E\left[\sum_{s=t}^{\infty} \beta^{s-t} \left(u(X_{st}, Z_{st}, \xi_{ijs}, \epsilon_{ijs}) - MC_{is}I[j \neq d_{i,s-1}]\right) |s_{it}, \epsilon_{it}, d_{it}\right] \tag{3.1}$$

Given the Markov transition probabilities, the sequence of decisions can be represented as an optimal decision rule, which is a function of the state variables: $d^* = d(s_{it}, \epsilon_{it})$. With agents following the optimal decision rule, the problem can be represented with a Bellman's Equation:

$$V(s_{it}, \epsilon_{it}) = \max_j \left\{u_{ijt}^{MC} + \beta E\left[V(s_{it+1}, \epsilon_{it+1})|s_{it}, \epsilon_{it}, d_{i,t-1} = j\right]\right\} \tag{3.2}$$

I follow Rust (1987) and make two additional assumptions to simplify the problem:

Assumption 1 (Additive Separability)**.** *The idiosyncratic error term, ϵ, is separable from the period utility function, u.*

Accordingly, the full period utility function, u_{ijt}^{MC} can be expressed as:

$$u_{ijt}^{MC} = u(X_{jt}, Z_{it}, \xi_{ijt}) - MC_{it}I[j \neq d_{i,t-1}] + \epsilon_{ijt}. \tag{3.3}$$

Assumption 2 (Conditional Independence)**.** *The idiosyncratic stochastic utility term, ϵ, is i.i.d. Type I Extreme Value with density q_ϵ. Additionally, conditional on s_t and d_t, ϵ_{t+1} is independent of s_{t+1}.*

Assumption 2 allows the Markov transition probabilities to be factored according to:

$$q(s_{t+1}, \epsilon_{t+1}|s_t, \epsilon_t, d_t) = q(s_{t+1}|s_t, d_t)q_\epsilon(\epsilon_{t+1}) \tag{3.4}$$

With Assumptions (1) and (2), the value function can be represented by a set of choice specific value functions, $v_j^{MC}(s_{it})$, each of which provides the current and expected utility for a specific choice:

$$v_j^{MC}(s_{it}) = u(X_{jt}, Z_{it}, \xi_{ijt}) - MC_{it}I[j \neq d_{i,t-1}] + \beta E\left[V(s_{it+1}, \epsilon_{it+1}|s_{it}, d_{it} = j)\right] \tag{3.5}$$

Since moving costs are the same for all choices conditional on moving, it will be convenient for estimation to separate the choice specific value functions into choice specific parts and moving costs:

$$v_j^{MC}(s_{it}) = v_j(s_{it}) - MC_{it}I[j \neq d_{i,t-1}] \tag{3.6}$$

$$v_j(s_{it}) = u(X_{jt}, Z_{it}, \xi_{ijt}) + \beta E\left[V(s_{it+1}, \epsilon_{it+1}|s_{it}, d_{it} = j)\right] \tag{3.7}$$

3.5 Estimation

The estimation procedure advances in four separate stages. First, normalized choice specific value functions are estimated from the individual transactions data. Second, observed household decisions to move or stay in the current residence every period are used to identify moving costs and the normalizing constant on utility. Third, the choice specific value functions and moving costs are used to back out the value of flow utility. Finally, flow utility is regressed on neighborhood attributes and fixed effects to determine the marginal utility of the observables.

3.5.1 Stage 1: Recovery of Normalized Choice-Specific Value Functions

Each observed housing transaction in the data represents a choice of a neighborhood conditional on moving. As mentioned at the end of the previous section, the decision-relevant value conditional on moving is provided by Equation (3.7). As a result, conditional on deciding to move, the household must maximize its utility by solving

$\max_j[v_j(s_{it}) + \epsilon_{ijt}]$ in each period. Since I've assumed in Assumption 2 that the idiosyncratic error terms are distributed Type I Extreme Value, the CCP estimation techniques developed by Hotz and Miller (1993) are available to estimate the choice specific value functions.

To aid in estimation, households will be divided into types, τ, based on their observable characteristics, wealth, income, and race. In practice, I divide wealth into twenty-five \$10,000 bins, from \$0 to \$240,000 and above. The initial value of wealth is specified as the difference between the purchase price and the loan amount. Income is reported directly and is divided into three quantiles.[12] Race is divided into whites and minorities. The result provides for $25 \times 3 \times 2 = 150$ possible values of τ. I write $v_{jt}^{\tau} = v_j(s_{it})$ whenever the observables of household i dictate that the household's type is τ in period t. Using the τ superscripts, Equation (3.7) can be rewritten as:

$$v_{jt}^{\tau} = u_{jt}^{\tau} + \beta E\left[\log\left(\sum_{k=0}^{J} \exp\left(v_{kt+1}^{\tau_{t+1}} - MC_{t+1}^{\tau_{t+1}} I[k \neq j]\right)\right) | s_{it}, d_{it} = j\right] \tag{3.8}$$

where

$$\log\left(\sum_{k=0}^{J} \exp\left(v_{kt+1}^{\tau_{t+1}} - MC_{it+1} I[k \neq j]\right)\right) = E_{\epsilon}\left[\max_k v_{kt+1} + \epsilon_{ikt+1}\right]$$

It is important to note that a household's wealth can change in one of two ways. First, the value of the household's residence can vary period to period, increasing or decreasing the amount of wealth the household has. Second, if a household decides to move in the current period, their wealth will be reduced by the amount of their realtor fees.[13] As a result, the expectations in Equation (3.8) also include the household's expectations about their type in the following period.

For the observed households, conditional on moving, optimal decision making

[12] Incomes reported over \$1 million are capped at \$1 million.

[13] The only measure of wealth available in the housing transactions data is home equity. It's not possible to control for other measures of wealth and saving that might be changing as well.

requires that they select a neighborhood j, such that $v^{\tau}_{jt} + \epsilon_{ijt} > \max_{k \neq j} v^{\tau}_{kt} + \epsilon_{ikt}$. Since ϵ is i.i.d. TIEV, the probability that a type τ household selects neighborhood j in period t is given by:

$$P^{\tau}_{jt} = \frac{e^{v^{\tau}_{jt}}}{\sum_k e^{v^{\tau}_{kt}}} \tag{3.9}$$

A feature of the logit probabilistic choice model is that utility is defined up to an additive constant. When attempting to recover the choice specific value functions, it is only possible to recover a normalized value $\widetilde{v}^{\tau}_{jt} = v^{\tau}_{jt} - m^{\tau}_{t}$. The normalizing constants m^{τ}_{t} will be estimated in the second stage. Using a variant of the conditional choice probability inversion of Hotz and Miller (1993) inspired by Berry (1994), I can calculate the normalized choice specific value functions with consistent estimates of $\widehat{P}^{\tau}_{jt}$:

$$\widetilde{v}^{\tau}_{jt} = \log(\widehat{P}^{\tau}_{jt}) - \frac{1}{J}\sum_{k} \log(\widehat{P}^{\tau}_{kt}) \tag{3.10}$$

The most straightforward way to obtain an estimate of $\widehat{P}^{\tau}_{jt}$ would be to place each observation into a bin based on the time period and the households type, and calculate the share that each choice gets amongst members of that bin. However, there are two problems with this approach. First, the preponderance of types and having access to only a finite amount of data makes this option infeasible since there would be some bins that have no observations, which would imply that those choices provide utility of negative infinity. Second, observations on the selection of the outside option are of a significantly different nature and lower frequency.

To solve these issues, I proceed in two steps. First, the inside shares are estimated separately from the outside shares. I use a flexible random utility specification that includes polynomials and interactions in all observable neighborhood and individual characteristics, in addition to a unobserved, time variant neighborhood fixed effect.

$$U_{i,j,t} = F(X_{jt}, Z_{it}, \delta_{jt}; \alpha) + \varepsilon_{ijt} \tag{3.11}$$

The errors are assumed to be TIEV, and the results of this flexible logit are then used to calculate the predicted probability that each type makes each choice in each period.

$$\widehat{\sigma}(X_{jt}, Z_\tau; \hat{\alpha}, \hat{\delta_{jt}}) = \frac{e^{F(X_{jt}, Z_\tau, \widehat{\delta_{jt}}; \widehat{\alpha})}}{\sum_k e^{F(X_{kt}, Z_\tau, \widehat{\delta_{kt}}; \widehat{\alpha})}} \tag{3.12}$$

Second, data limitations require a more parameterized approach to estimating the outside shares. Recall that a choice of the outside option is observed as a household selling their house and not choosing to purchase another home in the time period covered by the dataset. I estimate the probability that someone chooses the outside option as a simple binary choice logit, where the decision variable is equal to 1 if the household takes the outside option, and zero otherwise. I allow the probability to vary by time and observable characteristics of the households. The likelihood that a household chooses the outside option is given by:

$$\pi_{it} = \frac{\exp\{\lambda_0 + \lambda_1' Z_{it} + \lambda_2 t\}}{1 + \exp\{\lambda_0 + \lambda_1' Z_{it} + \lambda_2 t\}} \tag{3.13}$$

A likelihood function is formed based on Equation (3.13) and maximized. The maximum likelihood estimate $\hat{\lambda}$ is then used to calculate the predicted share of the outside option for each type, $\hat{P}_{0t}^{\tau}$. Since the inside shares are the market shares of each choice conditional on selecting an inside option, the unconditional probabilities of choosing option j are given by:

$$\widehat{P}_{jt}^{\tau} = (1 - \widehat{P}_{0t}^{\tau}) * \widehat{\sigma}(X_{jt}, Z_\tau; \hat{\alpha}, \hat{\delta_{jt}}) \tag{3.14}$$

The estimates $\{\widehat{P}_{jt}^{\tau}\}_{j=0}^{J}$ are fed through Equation (3.10) to calculate the values of all choice specific value functions, $\{\widetilde{v}_{jt}^{\tau}\}_{j=0}^{J}$.

3.5.2 Stage 2: Moving Costs

The goal of the second stage of estimation is to estimate moving costs faced by households and to uncover the normalizing constants to obtain value functions that are comparable across types. The latter is necessary to evaluate the expectations over future changes in type. The former will be accomplished by utilizing the information contained in the observed frequency of moves to infer what level moving costs must be. I will begin by discussing the former.

Moving for homeowners is costly. There are psychological costs, like disrupting neighborhood relationships, changing school districts, and general stress. Additionally, there is a financial cost to selling a home, namely, realtor fees. To estimate moving costs, I will assume that MC_{it} comprises psychological moving costs, $PMC(Z_{it})$, and the financial cost of paying 6% of the selling price of ones home to a real estate agent, $FMC(Z_{it}, X_{d_{i,t-1}t})$, both of which are functions of the neighborhood being left. Since I can observe how many periods a given household decides to stay, and in which period they move, it is possible to use the structure already imposed on the problem to identify these costs.

Consider the problem facing a household that is deciding in period t whether or not to stay in their home, which is in neighborhood j. They will remain in their neighborhood if their neighborhood provides greater utility than every other neighborhood, minus moving costs:

$$v_{jt}^{\tau} + \epsilon_{ijt} > \max_{k \neq j} \left[v_{kt}^{\tau'} + \epsilon_{ikt} \right] - PMC(Z_{it}) \tag{3.15}$$

The value of the other neighborhoods is evaluated at the wealth level that results from paying the financial moving costs. For example, if a household with wealth of \$100,000 sells their house worth \$200,000, their financial moving costs are \$12,000, which makes their wealth now \$88,000.

Equation (3.15) is insufficient for estimation, since I do not know the value of the unnormalized choice specific value functions. Substituting $\widetilde{v}_{jt}^{\tau} = v_{jt}^{\tau} - m_t^{\tau}$ into Equation (3.15) results in:

$$\widetilde{v}_{jt}^{\tau} + \epsilon_{ijt} > \max_{k \neq j} \left[\widetilde{v}_{kt}^{\tau'} + \epsilon_{ikt} \right] - PMC(Z_{it}) - \left(m_t^{\tau} - m_t^{\tau'} \right) \tag{3.16}$$

The difference $m_t^\tau - m_t^{\tau'}$ has an intuitive interpretation. The difference in the normalizing constants provides the difference in utility between the wealth levels that define τ and τ'. Accordingly, it is possible to parameterize this utility difference as a function of the amount of wealth that would cause such a drop in utility. For estimation, I make the following parameterizations:

$$\begin{aligned} m_t^\tau - m_t^{\tau'} &= FMC_{it}'\gamma_i^f \\ FMC_{it} &= 6\% * Price_{i,d_{i,t-1}} \\ \gamma_i^f &= Z_{it}'\gamma_1^f + \gamma_0^f \\ PMC_{it} &= Z_{it}'\gamma_1^p + \gamma_0^p \end{aligned}$$

Using Equation (3.16) and these parameterizations, it's possible to write the likelihood of a given household choosing to stay in period t:

$$P(d_{i,t} = j | d_{i,t-1} = j) = \frac{e^{\widetilde{v}_{jt}^\tau}}{e^{\widetilde{v}_{jt}^\tau} + \sum_{k \neq j} e^{\widetilde{v}_{kt}^\tau - FMC_{it}\gamma_i^f - PMC_{it}}} \tag{3.17}$$

Psychological moving costs are identified by the overall frequency of "stays" vs. "moves." The marginal value of wealth, γ^f, is identified off the fact that individuals with more expensive homes, who pay higher fees, are less likely to move if wealth is valuable. Once γ^f is estimated, the differences $m_t^\tau - m_t^{\tau'}$ can be calculated, and the unnormalized choice specific value functions, v_{jt}^τ, determined. This is accomplished by setting the mean unnormalized choice specific value function of the lowest wealth type to zero, and using the fact that $v_{jt}^\tau - v_{jt}^{\tau'} = (\widetilde{v}_{jt}^\tau + m_t^\tau) - (\widetilde{v}_{jt}^{\tau'} + m_t^{\tau'})$.

3.5.3 Stage 3: Recoving Expectations and Flow Utility

The third stage of estimation deals with solving for household's expectations. Recall Equation (3.8):

$$v_{jt}^\tau = u_{jt}^\tau + \beta E \left[\log \left(\sum_{k=0}^{J} \exp \left(v_{kt+1}^{\tau_{t+1}} - MC_{t+1}^{\tau_{t+1}} I[k \neq j] \right) \right) | s_{it}, d_{it} = j \right]$$

Stages 1 and 2 have yielded estimates of the left-hand side of this equation. In order to uncover flow utility, and ultimately marginal willingness to pay, the expectation in this equation must be evaluated. Instead of estimating the transition probabilities of each of the state variables (one of which is unobserved) and solving a costly fixed-point problem to determine the value function, I will estimate the transition probabilities of the choice specific value functions,

$$v_{jt}^{\tau} = \eta_0^{\tau} + \sum_{l=1}^{L} v_{j,t-l}^{\tau}\eta_{1,l}^{\tau} + \sum_{l=1}^{L} X_{j,t-l}\eta_{2,l}^{\tau} + \eta_3^{\tau} t + \varepsilon_{jt}^{\tau}, \tag{3.18}$$

where L is the number of time-lagged variables being used and ε is a standard normal error draw.[14] While estimating the transition probabilities of the value functions precludes the need to estimate the transition probabilities of all the states, it is still necessary to estimate the transition probabilities of price, since future prices determine future wealth levels. Price is modeled similarly as

$$Price_{jt} = \nu_0 + \sum_{l=1}^{L} X_{j,t-l}\nu_{1,l} + \nu_2 t + \omega_{jt}, \tag{3.19}$$

where ω_{jt} is an i.i.d. standard normal error draw.

The expectations are evaluated by simulation in the following way. For each (j, t, τ), take a draw of $(\varepsilon^r, \omega^r)$. Use (ω^r, j, t) to calculate the value of $Price_{j,t+1}^r$. Using the fact that $Wealth_t = Price_t - loan_t$, holding the loan amount fixed provides the new wealth level, and consequently, the new value of τ_{t+1}^r. Next, use $(\varepsilon^r, j, t, \tau_{t+1}^r)$ to calculate the choice specific value function for every j in the next period. Then set

$$u_{jt}^{\tau,r} = v_{jt}^{\tau} - \beta \log\left(\sum_{k=0}^{J} \exp\left(v_{kt+1}^{\tau_{t+1},r} - MC_{t+1}^{\tau_{t+1},r} I[k \neq j]\right)\right).$$

[14] In practice, $L = 2$. However, only one lag is used for TRI density and emissions density since several neighborhoods have zero TRI facilities year-after-year. Including two lags of these variables induces a multicollinearity problem.

Let R be the number of simulated draws of $(\varepsilon^r, \omega^r)$ that are taken. Then, the estimated value of u^τ_{jt} equals $\frac{1}{R}\sum_r u^{\tau,r}_{jt}$. Repeating this process for all types, neighborhoods, and time periods provides estimates of every flow utility.

3.5.4 Stage 4: Decomposing Flow Utility

In the last stage, I wish to determine how the type specific flow utilities vary with observable characteristics of both the neighborhoods and individuals. In this stage, I can treat the neighborhood unobservable characteristic, ξ^τ_{jt}, as an error term in a regression:

$$u^\tau_{jt} = \alpha_\$ r W_\tau + \alpha^{\tilde{\tau}}_X X_{jt} + \alpha^{\tilde{\tau}}_T + \alpha^{\tilde{\tau}}_C + \xi^{\tilde{\tau}}_{jt} \tag{3.20}$$

where $\tilde{\tau}$ represents the type of household when considering only income and race. Accordingly, Equation (3.20) assumes that preferences vary by income and race, and wealth enters the flow utility function as an annuity payment at rate r. X_{jt} are all neighborhood observables: price, percentage of whites, TRI density, emissions density, ozone concentration, and violent crime rates. $\alpha^{\tilde{\tau}}_T$ is a set of time dummies and $\alpha^{\tilde{\tau}}_C$ is a set of county fixed effects.

Since the full sales price of a home likely doesn't enter flow utility in each period, for this regression I create a rental price of housing using 5% of neighborhood price level. In effect, this captures the costs of property taxes, insurance, and upkeep that are likely to vary with price. Much like the second stage in models that follow Berry et al. (1995), there is an endogeneity between rents and the unobserved neighborhood attribute. However, assuming that the marginal value of a dollar of wealth is equal to the marginal flow utility of a dollar of income, I already have an estimate of the coefficient on rent and the wealth annuity payment, γ^f.[15] As such, I can move rent and wealth to the left-hand side of the equation, and estimate the following regression,

$$u^\tau_{jt} + \hat{\gamma}_f(p_{jt} - rW_\tau) = \alpha^{\tilde{\tau}}_X X'_{jt} + \alpha^{\tilde{\tau}}_T + \alpha^{\tilde{\tau}}_C + \xi^{\tilde{\tau}}_{jt}, \tag{3.21}$$

[15] Assuming that the MVW = MUW forces the rate of return on an annuity payment to exactly equal (1-β) Explain this here or in appendix?

Table 3.3: Stage 2 Moving Cost Estimates

	Coefficient	Standard Error
γ_0^p	9.131**	(.036)
γ_1^p	-0.009	(.021)
t	-0.073**	(.004)
γ_0^f	4.166**	(.121)
γ_1^f	-0.268**	(.064)

*indicates significance at the 5% level. **indicates significance at the 1% level. The units for the above coefficients are as follows: γ_0^p is in units of utility, γ_1^p is in utils per hundred thousand dollars in income, γ_0^f is utils per hundred thousand dollars, and γ_1^f is utils per hundred thousand dollars per hundred thousand dollars in income.

where X'_{jt} is the vector of observable attributes with price removed.

3.6 Empirical Results

In this section, I provide and discuss the estimation results from two stages: Stage 2 and Stage 4. Stage 2 provides estimates of moving costs, while Stage 4 provides the flow utility estimates. These are used, in turn, to calculate marginal willingness to pay.

3.6.1 Moving Costs

The estimates from the second stage likelihood maximization are provided in Table 3.3. Psychological moving costs are allowed to vary by a household's income and tenure. Income is measured in hundred thousands of dollars. The bottom panel reports the marginal value of wealth estimates, which are derived from financial moving costs, FMC. Again, financial moving costs and income are in hundreds of thousands of dollars. The MVW is positive and significant, and is decreasing income. These estimates are used subsequently to estimate Stage 4. For context, these estimates imply that a household that earns $100,000 annually places a lifetime value on psychological

Table 3.4: Stage 4 Marginal Willingness to Pay Estimates (Annual $)

Income	% White	Mean Income	TRI Density	Emissions Density	Ozone Cons.	Violent Crime
			White Households			
Low	1,070.15**	310.65**	-9.43	-22.11**	979.54**	338.33**
	(41.77)	(11.21)	(10.04)	(2.42)	(118.98)	(22.85)
Mid	1,350.75**	396.05**	-39.45**	-24.86**	754.23**	321.79**
	(47.8)	(8.35)	(11.12)	(2.83)	(95.13)	(17.77)
High	2,435.61**	629.49**	-273.35**	-41.02**	183.59	316.78**
	(103.58)	(9.79)	(22.4)	(5.08)	(151.09)	(39.94)
			Minority Households			
Low	-1,321.37**	77.89**	-155.23**	-19.87**	1,120.83**	-134.63**
	(36.39)	(15.12)	(8.00)	(1.97)	(94.55)	(23.71)
Mid	-1,106.11**	194.46**	-129.83**	-21.07**	945.91**	-191.90**
	(27.75)	(11.13)	(8.16)	(2.16)	(82.34)	(19.06)
High	-580.03**	468.18**	-163.35**	-27.81**	446.09**	-332.16**
	(30.37)	(11.01)	(11.6)	(3.69)	(169.61)	(41.54)

Figures are calculated in year 2000 dollars. Bootstrapped standard errors are provided in parenthesis below each estimate. All estimates are significant at the 5% level.

moving costs of $234,062.

3.6.2 Marginal Willingness to Pay Estimates

Estimates of annual marginal willingness to pay are reported in Table 3.4. The first column presents the marginal willingness to pay for a ten percentage point increase in the fraction of white neighbors. The estimates vary from $1,070.15 to $2,435.61 for whites, and -$1,321.37 to -$580.03 for minorities. These estimates compare favorably to other recent estimates for racial preferences in the literature. For example, the estimates of Bayer et al. (2007) imply that African-Americans are willing to pay $1,054 for a ten percent increase in the fraction of African-Americans in their neighborhood. BMMT find that whites are willing to pay $585.35 to $3,932.49, depending on their income, for a ten percent increase in the fraction of neighborhood whites.

The third and fourth columns present the marginal willingness to pay estimates for the TRI variables. First, I provide an estimate of the willingness to pay for a one facility increase in the number of TRI facilities. Since the measure is a density, I present the estimate for the median-sized neighborhood, 15.68 square kilometers, roughly half the size of the campus of Stanford University. The estimates range from -\$9.43 to -\$273.35 for whites and -\$129.83 to -\$163.35 for minorities. The third row lists the willingness to pay for a ten percent increase in emissions density, calculated at the mean observed neighborhood emission density level, conditional on having a TRI facility.[16]

The observed variation in marginal willingness to pay for amenities projects that general equilibrium re-sorting will play a strong role in counterfactual scenarios. Any adjustments to the number of TRI facilities or emissions being released will be valued differently according to socio-economic status. In turn, as the demographics of each neighborhood adjust, feedback effects will impact the willingness to pay. Accordingly, partial equilibrium marginal willingness to pay estimates are likely insufficient for welfare analysis.

3.7 Counterfactual Simulations

3.7.1 Key Assumption for General Equilibrium WTP Estimates

The results in the previous section demonstrate the heterogeneity in willingness to pay for neighborhood amenities and neighborhood minority shares exhibited by households. It was suggested previously that any policy that targeted, for example, toxic emissions from TRI facilities would cause different households to react differently. The partial equilibrium results suggest that a significant policy intervention that reduces the amount of emissions in a particular neighborhood could cause whites to find the neighborhood relatively more attractive, which could lead to a change in the percentage of whites living in that neighborhood. Since minorities prefer living amongst other minorities, this second order effect could be welfare reducing. Conversely, one might expect prices to rise, given both race types place a positive value on the absence of emissions. Utility gleaned from wealth accumulation due to price

[16] The mean emissions density in the sample is 767.97 pounds per square kilometer. In the median neighborhood, this corresponds to 12,041.77 pounds of toxic releases in one year. The estimates can be thought of a increase of 1,204.177 pounds in the typical neighborhood.

appreciation may offset any welfare losses due to demographic shifts. While the intuition behind the nature and direction separate general equilibrium considerations might have on willingness to pay is clear, when applied simultaneously, it is unclear *a priori* whether general equilibrium willingness to pay estimates will be higher or lower than partial equilibrium estimates. Additionally, its possible that the distribution of willingness to pay would vary across neighborhoods according to the initial minority share in each neighborhood. For example, are whites more willing to pay for a reduction of pollution in a neighborhood that is predominately white than a predominately minority neighborhood?

To answer these questions, I calculate the general equilibrium willingness to pay for total reduction in neighborhood toxic emissions for several neighborhoods of varying initial demographic in the sample. The validity of these estimates will rest on the assumption that the counterfactual experiment being examined wouldn't change the equilibrium behavior of all households in the area. In effect, this assumption requires households' preferences and expectations about the evolution of the state variables to be the same under the counterfactual as they are under baseline conditions.

If the assumption holds, I can utilize the estimated equilibrium strategy functions, $\widehat{\sigma}(X_{jt}, Z_{\tau})$, to map state variables into actions.[17] This becomes a valuable tool that can be used to calculate the value function for households under baseline and counterfactual states. Bajari et al. (2007) develop a forward simulation procedure that they use to quickly calculate value functions in their estimation routine. The logic behind this procedure is simple. The value function is simply a discounted sum of flow utility. With estimates of how agents will act conditional on states, an initial state, and a known flow utility function, the value function can be calculated simulating paths of play through time. Once a sufficient number of periods has been reached

[17] Under this framework, it is not possible to examine renters by assuming they are homeowners without moving costs and without wealth accumulation. The equilibrium mapping from states to actions implicitly captures the behavior of homeowners. To simulate the behavior of renters, a full solution for expectations is required.

to make future periods discounted to insignificant amounts, the flows of utility are discounted back to the present, and this sum is an approximation of the value function. To achieve a proper approximation, a large number of simulated paths must be calculated and the resultant value function approximations averaged together. I provide a detailed explanation of the mechanics of the simulation procedure in the appendix.

The forward simulation procedure has many advantages. First, the estimation routine has already produced the required strategy functions and flow utility functions for each type of household. Second, simulating paths of play admits crucial features of the decision model, including moving costs, endogenously determined neighborhood attributes, and wealth accumulation. At each period, households face moving costs when deciding where to locate, and if they choose to relocate, experience the loss of utility. Also, each household's residential decision affects the minority share in each neighborhood, which endogenously updates throughout time for each neighborhood. Further, as prices adjust to any policy experiment, households' wealth can either increase or decrease, affecting their utility and decision making in future periods.

Perhaps most importantly, Hicksian willingness to pay is straightforward to calculate once value functions are known. In this context, Hicksian willingness to pay, WTP_{GE}, can be calculated as

$$V(s_t^B, W_i, \tilde{Z}_i) = V(s_t^{CF}, W_i - WTP_i^{GE}, \tilde{Z}_i) \tag{3.22}$$

where s_t^B is the baseline state, s_t^{CF} is the counterfactual state, W_i is household i's wealth level, and $\tilde{Z}_i$ is household i's income and race. WTP_i^{GE} is interpreted as the amount of wealth household i would have to surrender under the counterfactual state to attain the same utility available under the baseline state.

3.7.2 General Equilibrium Estimates

Table 3.5 reports the Hicksian willingness to pay for an addition of two TRI facilities, each with the average amount of emissions, to two representative neighborhoods: a

Table 3.5: GE Willingness to Pay Estimates (Annual $)

Income	Race	PE WTP (1)	GE w/o Move (2)	GE WTP (3)	Δ (4)	% Difference (5)
Neighborhood M: % White = 25%, Income $83K						
Low	White	-85.46	-145.04	-134.5	-49.04	-57.39 %
Mid	White	-156.38	-228.41	-183.7	-27.32	-17.47 %
High	White	-693.47	-797.23	-434.98	258.49	37.27 %
Low	Minority	-383.63	-303.60	-303.37	80.25	20.92 %
Mid	Minority	-334.1	-270.45	-269.59	64.52	19.31 %
High	Minority	-424.2	-389.24	-359.47	64.74	15.26 %
Neighborhood W: % White = 75%, Income = $158K						
Low	White	-85.46	-112.95	-112.94	-27.48	-32.16 %
Mid	White	-156.38	-188.69	-187.8	-31.41	-20.09 %
High	White	-693.47	-729.70	-724.21	-30.75	-4.43 %
Low	Minority	-383.63	-327.78	-304.71	78.91	20.57 %
Mid	Minority	-334.10	-287.87	-271.68	62.42	18.68 %
High	Minority	-424.20	-390.32	-368.26	55.95	13.19 %

Notes: Figures are reported in year 2000 dollars. Willingness to pay estimates are averaged over wealth groups and reported for each intersection of income level and race. All estimates are calculated for a household starting in the given neighborhood in the first period.

minority neighborhood, M and a white neighborhood, W. The corresponding partial equilibrium estimates for an equivalent reduction in emissions are provided for comparison. The second column provides the general equilibrium estimates for a household starting in the target neighborhood that is not given the option to move. Comparing Columns (1) and (2) shows the effect of changing neighborhood demographics on welfare. Column (3) provides willingness to pay estimates where the household is allowed to re-optimize. Column (4) calculates the difference between Columns (3) and (1), and Column (5) represents the difference as a percentage.

The same patterns are seen in each neighborhood. To understand the effect of changing demographics on household welfare, it is instructive to compare Columns (1) and (2). In both neighborhoods, the increase in pollution causes the fraction of minority residents to increase. This results in white households having relatively worse welfare outcomes compared to partial equilibrium and minorities having better welfare outcomes relative to partial equilibrium.

Column (3) reveals that the option to move to a new optimal neighborhood benefits all groups. Interestingly, high-income white households that begin in the representative minority neighborhood have the most to gain from the option to leave. Intuitively, these households might wish to leave the neighborhood *a priori*, but high moving costs make staying the optimal choice. The additional pollution then lowers the utility provided by the current neighborhood even further, making the available gain in utility from changing neighborhoods greater than moving costs. Thus, in a dynamic setting, the option value for leaving a neighborhood over time impacts household welfare. Welfare losses for high-income whites are over 37% lower because of the option value.

3.7.3 Minority Move-In

The counterfactual simulator requires utilizing estimated household decision functions and transition probabilities to simulate the path of all the neighborhoods for both baseline and counterfactual initial states. As a result, it's possible to trace out the change in the fraction of minorities in each neighborhood over time. With this information, I can examine the complementary hypotheses of "minority move-in" and "white flight."

"Minority move-in" (MMI hereafter) holds that observed disproportionate exposure results from minorities seeking out neighborhoods with higher pollution because housing prices are lower. Conversely, "white flight" attributes the observed exposure patterns to an initial allocation of pollution that is equitably distributed but market dynamics have white households outbidding minority households for areas with lower pollution levels.

Depro et al. (2011) (DTO hereafter) provide several examples of how traditional reduced form analyses are not capable of identifying MMI using observed changes in population. MMI implies that minorities and low-income households are seeking higher levels of pollution to obtain other amenities, for example, lower housing prices. The intuition for their result follows from the fact that it is not possible to know whether minorities or low-income households are increasing their exposure to pollution by observing aggregate changes in subpopulations because the observer doesn't know where the residents in the target neighborhood came from.

DTO illustrate the non-identification of MMI mathematically by a simple three neighborhood city, with neighborhoods A, B, C and two periods. Let X_k^t be the population in neighborhood $k \in \{A, B, C\}$ in period $t \in \{1, 2\}$. The system can be represented as a simple linear system of equations

$$\begin{pmatrix} s_{A,A} & s_{A,B} & s_{A,C} \\ s_{B,A} & s_{B,B} & s_{B,C} \\ s_{C,A} & s_{C,B} & s_{C,C} \end{pmatrix} \begin{pmatrix} X_A^1 \\ X_B^1 \\ X_C^1 \end{pmatrix} = \begin{pmatrix} X_A^2 \\ X_B^2 \\ X_C^2 \end{pmatrix},$$

where $s_{i,j}$ is the share of residents that move from neighborhood j to neighborhood i. Let $Z_{i,j}$ be the difference in pollution exposure between neighborhood j in period 1 and neighborhood i in period 2. An analyst testing for MMI would use $corr(s_{i,j}, Z_{i,j}) > 0$ as evidence that the subpopulation under study is moving to the nuisance. However, with data only on X, the pairwise neighborhood flows s are unidentified since the system of equations has nine unknowns and 6 data points. DTO provide several numerical examples where using the correlation between ΔX_k and $Z_{k,k}$ to test for MMI leads to both false positives and false negatives.

The assumptions of the general equilibrium framework presented in this section provide identification of MMI. To test for MMI, I first explicitly calculate all pairwise neighborhood flows $s_{i,k}$ by simulating household behavior for T periods. The benefit of the general equilibrium framework is that these neighborhood flows represent household migration to all amenities, including percent white, which is a function of equilibrium sorting. Second, I simply calculate the change in amenities, $Z_{i,j}$, for all pairs of neighborhoods. Finally, for each household type, I regress neighborhood flows, $s_{i,j}$, on changes in amenities, $Z_{i,t}$, for all $j \neq i$. Finding a positive, significant coefficient on change in emissions, or $\frac{\partial s}{\partial emit}$, indicates that type of household "comes to the nuisance."

In Figure 3.3, the top graph plots the coefficients for the change in ln($emissions$) on the y-axis using $T = 20$.[18] The x-axis plots wealth levels and each line represents an income-race household type. The overall pattern is clear: for all types, low wealth households tend to follow increases in pollution, whereas high wealth households do the opposite. Furthermore, the degree of "coming to the nuisance" has an inverse relationship with income, with the lowest income households having higher population flows. Lastly, within each income group, minority households have a higher tendency to follow emissions than whites. The second graph plots the coefficients for the change in TRI site density. The gradient with respect to wealth and income is largely the same, however, there is a clear separation in the coefficients for whites and minorities.

The classic explanation of MMI has minorities seeking lower prices and accepting higher emissions. The last graph of Figure 3.3 plots the coefficients for the change in price. The results support the canonical description of MMI. For each type, lower wealth households are moving to areas with lower prices and higher emissions, whereas high wealth households seek lower emissions and higher prices. The three plots provide evidence in support of the MMI hypothesis. Over time, low wealth, and in some cases minorities, are taking on higher levels of pollution in exchange for lower prices.

3.7.4 Disproportionate Siting

The previous section provided evidence that whites and high income households will outbid minorities and low income households for cleaner environments. There also is evidence to support the claim that minorities "come to the nuisance." Next, I want to examine the claim that polluters disproportionately site facilities and release pollution in low-income and minority areas. However, without access to detailed firm level panel-data on pollution activities and locational decisions, it is difficult to test this hypothesis directly. Fortunately, Coase (1960) provided the foundation for a

[18] The coefficients for all types with one exception are statistically significant at the 5% level. The coefficient for high-income minorities with wealth level 3 is not statistically significant

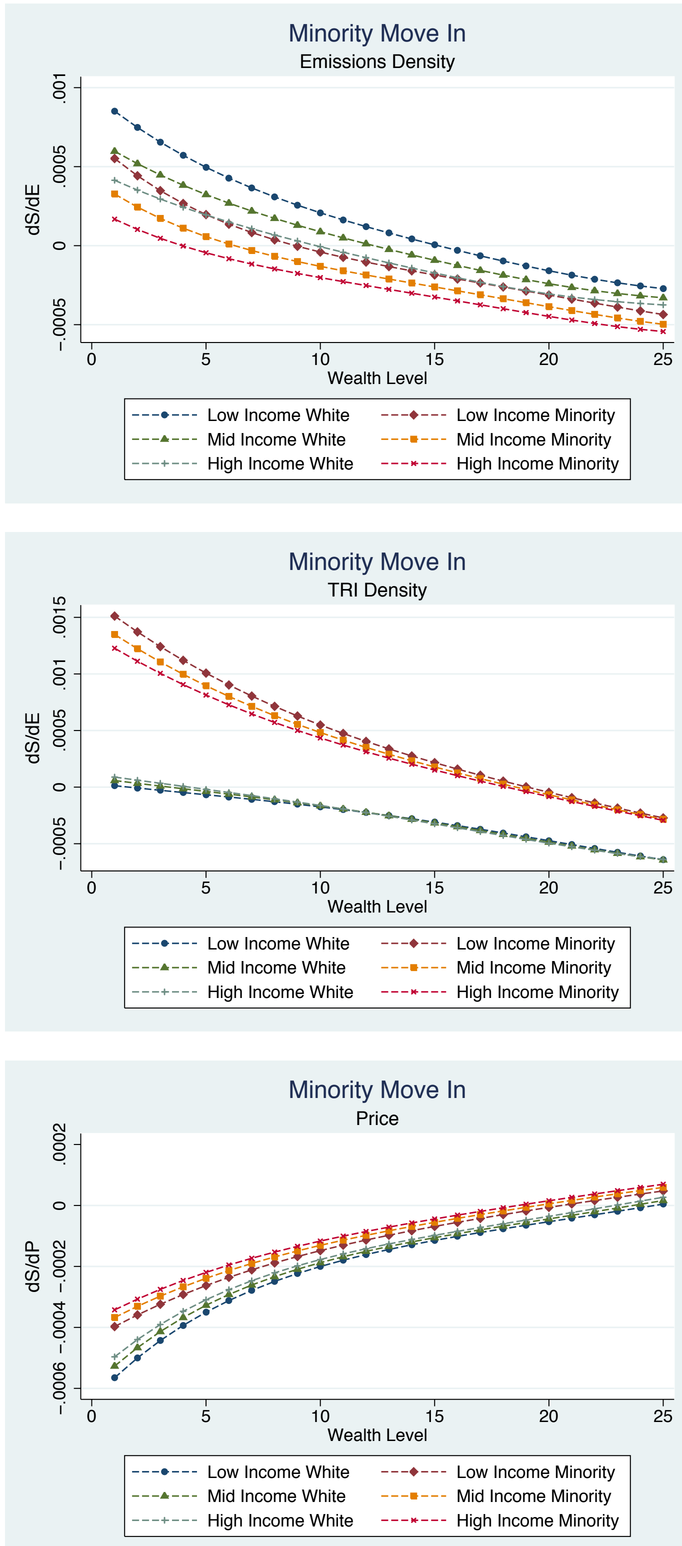

FIGURE 3.3: Minority Move-In

theoretical argument that suggests polluters will target neighborhoods that have the lowest willingness to pay to avoid pollution. In this section, I investigate if substantial differences in willingness to pay could lead firms to target disadvantaged populations, resulting in "disproportionate siting" of emissions.

The Coase Theorem takes many different forms in the literature. In this setting, TRI facilities have the rights to generate pollution and households receive the externality of that pollution. With property rights with the polluters, under Coasian bargaining, households will have the incentive to negotiate a payment with the polluter to reduce the externality. Households will negotiate to the point where the payment and transaction cost of bargaining equals their willingness to pay to avoid the pollution. In reality, this mechanism is more likely to take the form of collective political action to prevent a facility from being sited nearby, or a public relations campaign to induce the polluter to reduce emissions. However, the "strength" of the action taken by local residents is going to be a function of the willingness to pay they have to avoid the externality generated by the polluter. Knowing they will face obstruction, profit maximizing firms will seek to locate in neighborhoods where this willingness to pay is the lowest. Minority and low-income neighborhoods displaying a significantly lower willingness to pay to avoid TRI emissions suggests that the disproportionate exposure observed in the data could be the result of a complex Coasian bargaining game between residents and industry.

To illustrate the point, consider a simplified setting where all facilities are owned by the same firm. Further, assume that the cost to the firm of adding a new facility in a given neighborhood is the same across neighborhoods. Since I know the distribution of demographics of households in each neighborhood, I can calculate a population-weighted willingness to pay for the increase in facilities and emissions in each neighborhood.[19] Under these assumptions, neighborhoods with the highest will-

[19] Since each neighborhood was constructed to contain roughly 5,000 households, I calculate the average willingness to pay based on the distribution of types in the neighborhood and then multiply by 5,000.

Table 3.6: Aggregate Willingness to Pay for Two Facility Increase

	PE	GE	(GE - PE)
Neighborhood M	-1,527,059	-1,315,481	211,578
Neighborhood W	-1,900,420	-1,944,979	-44,559
Difference	373,361	629,498	256,137
% Difference	24.4%	47.9%	

ingness to pay for reductions in emissions are the most likely to "win" the implicit bargaining game for those reductions.

Table 3.6, provides the partial and general equilibrium willingnesses to pay for a two facility increase in TRI sites, weighted by the distribution of types and aggregated over all households for Neighborhoods W and M. The table reveals that by using simple partial equilibrium estimates, the non-marginal change in pollution would cause 24.4% more disutility in the white neighborhood than in the minority neighborhood. This difference in aggregate willingness to pay can be viewed as the relative likelihood of collective action between the two neighborhoods.

Performing the same exercise with general equilibrium estimates reveals a much larger disparity between the two neighborhoods. When comparing the aggregate general equilibrium willingness to pay to avoid the change in pollution between neighborhoods, the difference is 47.9%, approximately twice as much. The intuition behind this result is clear. An increase in pollution has the effect of increasing the amount of minorities in a neighborhood, which in turn makes white households worse off and minorities better off. As a result, to garner the same dollar amount of "resistance" in both neighborhoods, the firm would have to increase pollution by at least one and a half units in the minority neighborhood for every one unit of pollution in the predominately white neighborhood.

The above exercise demonstrates that aggregate willingness to pay for emissions reductions can vary substantially across neighborhoods, depending on neighborhood socio-economic demographics. While firms have the right to release emissions into the environment, they can face resistance to these releases from local residents. Rent seeking firms might find it profitable to maintain lower emissions in areas where residents have higher willingnesses to pay for emissions reductions. In areas where willingness to pay is lower, the costs of reducing emissions might outweigh obstruction by local residents since the intensity of their resistance is tied to willingness to pay. As a result, the simple model of bargaining and collective action predicts higher emissions levels in areas with higher minorities.

3.8 Conclusions

This paper examines the veracity of both the "minority move-in" and disproportionate siting explanations for the unequal distribution of pollution exposure documented in the environmental justice literature. I develop a dynamic, general equilibrium framework that enables me to simulate household behavior to directly test whether or not disadvantaged populations "come to the nuisance." Additionally, the model permits the calculation of general equilibrium willingness to pay for changes in neighborhood amenities that account for welfare changes associated with equilibrium resorting and wealth accumulation for homeowners. These estimates permit an analysis of the variation in neighborhood willingness to pay to avoid pollution, and whether this variation could provide economic incentive for polluters to target disadvantaged populations.

The large state space makes the inclusion of dynamics into estimation problematic and complicates solving for general equilibrium outcomes. However, with a few simplifying assumptions, I am able to model households as forward looking decision makers that can accumulate wealth and face moving costs. These features of the model are crucial to the validity of the results in several ways. First, "minority move-in" is inherently a dynamic problem. Ignoring forward looking behavior and moving costs would likely over-predict the amount of re-sorting that takes place in response to changes in pollution exposure. Second, when dealing with homeowners who have preferences over the identity of their neighbors, accurate welfare analysis requires accounting for changes in welfare associated with wealth accumulation and changes neighborhood amenities that result from equilibrium behavior. My model allows me to predict household behavior and calculate general equilibrium willingness to pay that accounts for both wealth accumulation and re-sorting.

Equilibrium simulations show that for both races, poor households have a significant tendency to trade off higher pollution exposure for lower prices. The behavior conforms to the essence of the "minority move-in" hypothesis. Furthermore, I find that wealthy households behave in the opposite manner, as they have a tendency to reduce their pollution by paying higher housing prices. These results support the idea that disproportionate exposure to pollution is a function of market dynamics. Households will "vote with their feet" in response to pollution levels, with those households with the lowest wealth moving into areas with higher pollution levels.

Equilibrium welfare calculations also support the case that disproportionate exposure is a result of polluters responding to economic incentives. The willingness to pay to avoid a one-unit increase in pollution is one and a half times higher in a sample white neighborhood than in a sample minority neighborhood. This disparity in willingness to pay could reflect a similar disparity in the strength of the collective action across neighborhoods. Firms would find it profitable to locate externality generating operations in neighborhoods that are less likely to try and block or reduce toxic emissions. I find evidence that the neighborhoods where residents have lower willingness to pay tend to be minority and low income neighborhoods. Firms have economic incentives to target disadvantaged populations, which can explain, at least in part, the patterns of disproportionate exposure. The conclusion is that both market dynamics and disproportionate siting can contribute to the pattern of exposure seen in the literature.

4

What Did You Know, and When Did You Know It? Capitalization of Information in the Toxic Release Inventory

4.1 Introduction

Economists often use air quality, broadly defined, as a measure of environmental quality that households consider when making residential choices. While some general information about air quality is easily ascertained (e.g. seeing smog or smelling malodorous chemicals), detailed information about the quality and toxicity of local air requires some analysis of complicated governmental reports and datasets. This raises questions about exactly what information households use when choosing a neighborhood to live in or a house to purchase. If the researcher specifies a measure of environmental quality that is beyond the sophistication of ordinary consumers, concern grows that correlated unobservable attributes might be driving results.

The content of households' information sets is not just a concern of economists. Policy makers designed the Toxic Release Inventory (TRI) partly based on the idea that if the EPA provides detailed, facility-level information on toxic emissions to the public that firms will be incentivized to reduce the amount of pollution they produce.

TRI was authorized by the Emergency Planning and Community Right to Know Act (EPCRA) in 1986, which passed Congress on the heels of the Union Carbide disaster in Bhopal, India. The legislation sought to provide transparency about the types and quantities of hazardous chemicals to the communities that were most likely to be impacted by their release. The goal of pollution reduction might be hard to obtain if the TRI program isn't providing the correct information to the public.

Since the inception of the TRI, toxic emissions have fallen in the United States. For example, from 1989 to 1999, emissions in the U.S. have fallen 40%[1]. The EPA reports that disposal and releases of covered chemicals has fallen approximately 30% from 2001 to 2010 (U.S. Environmental Protection Agency (2010)). However, evidence that the public internalizes information on toxic emissions, for example in the housing and stock markets, is mixed.[2] Accordingly, it's difficult to claim that emissions are falling as a result of public pressure if its unclear that households and investors act on emissions data.

In light of the mixed evidence on household reaction to emissions data, this paper seeks to understand if households react to information content in the TRI not directly related to emissions. Part of the original intent of EPCRA was to inform households about the presence of toxic chemicals in their communities and to prepare them for an emergency release of hazardous materials. Site reporting requirements to the TRI are based upon onsite quantities of covered chemicals. Having a nearby site listed in the TRI informs households not only of the quantity and types of emissions, but the threat of a potential emergency spill of chemicals. If households are more sensitive to living near TRI facilities for fear of catastrophe rather than toxic air emissions, evaluating the impact of the TRI program solely on emissions data might be insufficient. Furthermore, if there is significant stigma or public pressure associated with meeting

[1] See Bui and Mayer (2003) for discussion.

[2] See Hamilton (1995a); Khanna et al. (1998); Bui (2005); Bui and Mayer (2003); Banzhaf and Walsh (2008)

the reporting thresholds for the TRI, the reduction in emissions seen in the data may be a result of firms' incentive to drop below the reporting requirements.

Generally speaking, the existing literature on TRI emissions valuation can suffer from two empirical problems. First, basic cross-sectional hedonic analysis that measure the implicit price for emissions or TRI site proximity could be subject to omitted variables bias if unobserved housing or neighborhood quality is correlated with TRI site incidence. Second, panel data models that try to difference away this unobserved heterogeneity might suffer from a lack of meaningful variation in the data. Cross-sectional variation in emissions might be meaningful to households whereas year-to-year changes in emissions might not be detectable or important to residents. Moreover, entry and exit of firms in TRI data might only represent firms rising above and falling below reporting thresholds, while site existence and all visual disamenities associated with the site remain constant. These issues might produce insignificant estimates, leading to conclusions that housing markets do not capitalize information in the TRI.

To help understand the effect of information in the TRI on households, I use a discontinuous change in the reporting thresholds for one of the most toxic chemicals, lead, to design two separate quasi-experimental empirical models. In 2001, the EPA lowered the reporting requirement for manufacturing or possessing lead from 10,000 pounds to 100 pounds. As a result, firms that were using more than 100 pounds but less than 10,000 pounds were no longer exempt from TRI reporting. First, I use a difference-in-differences (DID) estimator to exploit the regulatory change to test for whether the new information provided in the TRI affect the appreciation rate of houses in proximity to lead-using sites that previously didn't report to the TRI. Second, I apply a regression discontinuity (RD) model that selects facilities that are just above and just below the 10,000 pound threshold prior to 2001 to isolate the price impact of TRI status. The key assumption for the RD model is that sites around the threshold have similar unobservable attributes and that selecting houses around the discontinuity is an effective control for correlated unobservables.

The difference between the change in housing prices around a site that was always reporting to the TRI and the change in housing prices around a site that was in existence but not reporting until the regulatory change can identify the capitalization of TRI information. This is the intuition behind the DID estimator. Since the sites are in existence before having to report to the TRI program, all visual attributes of the site can be assumed to be constant before and after the dissemination of TRI information. In this setup, the control group is the set of houses in proximity to sites TRI sites not affected by the regulatory change. Subtracting the change in prices seen in the control group from the change in prices of the treatment group (i.e., houses in proximity to lead-using sites that report for the first time in 2001) differences away both site specific heterogeneity and price trends affecting facilities that handle and release toxic chemicals. The resulting difference can be interpreted as the effect that TRI site listing has on housing prices.

The RD design estimates the impact of TRI site status on prices in the cross section. Prior to 2001, there were sites using lead in large quantities that were not required to report to the TRI. Simple OLS estimates that use a dummy variable equal to one if a plant was required to report to the TRI could be biased since the sites that did report were necessarily larger consumers of lead and therefore might look unobservably worse than non-reporting TRI sites. The RD design attempts to balance these unobservables by restricting the sample to only those sites immediately above and below the 10,000 lbs reporting threshold. Intuitively, these sites will be using similar amounts of lead, but those that happen to be above the exogenous threshold get the TRI "treatment".

The TRI database is provided by EPA and is supplemented by data on point-source emissions from the California Air Resources Board (CARB). This important supplemental database allows me to identify facilities that were in existence and did not have to report to the TRI program. Identification requires knowledge of whether facilities that appear for the first time in the TRI data after the regulatory change were in fact already in existence. The TRI data reports whether or not a facility possesses lead and I match these facilities to the CARB data to determine the earliest reported date of existence. This provides a set of "treated" sites for the quasi-experimental approach.

Using rich micro-data on individual housing transactions in Santa Clara County, California, I find that the provision of new information in the form of TRI reporting does have a significant and negative impact on housing prices. By employing the DID estimator, I find that the regulatory change of lowering the reporting threshold for lead lowered prices of houses within 3 kilometers of a site forced to report under the new standard by 3.1%. Furthermore, I find that the effect on prices is stronger for houses in closer proximity. The effect increases in magnitude to -3.8% for houses within 2 kilometers of a site, and it increases in magnitude further to -4.7% for houses within 1 kilometer of a site. The RD estimates are similar qualitatively, but smaller in magnitude. Significant effects of TRI site status are not found at a radius of 3 kilometers, but are estimated to be -1.3% at 2 kilometers, and -2.5% at 1 kilometer.

These results suggest that information about toxic chemicals is capitalized into housing prices. Interestingly, since most of the "treated" sites have very little to no emissions, the results concur with a recent study by Bui and Mayer (2003) that finds changes in housing prices are not related to changes in TRI emissions. However, my results imply a different conclusion about the relationship between the TRI program and the housing market. While emissions in the TRI do not appear to be related to housing prices, information in the TRI about the chemicals being housed in large quantities on-site does have an impact on housing prices. As a result, large declines in the amount of emissions nationwide reported in the TRI could be a result of firms striving to reduce their levels of toxic chemicals in order to satisfy safety concerns of local residents. Toxic air emissions fall as a consequence of the quantities of on-site chemicals are reduced.

The paper proceeds as follows. Section 2 provides more detail on the background of the TRI program. Section 3 discusses the relevant literature, Section 4 introduces the empirical model in detail, Section 5 details the datasets used in this paper, Section 6 provides the estimation results, and finally, Section 7 concludes.

4.2 Toxic Release Inventory Background

Section 313 of the Emergency Planning and Community Right-to-know Act required the EPA to create the Toxic Release Inventory (TRI) by collecting and making public information relating to the possession and release of certain toxic chemicals by facilities in certain industries. The legislation was passed on the heels of the Union Carbide disaster in Bhopal, India, where a leak of methyl isocyanate gas was responsible for the deaths of thousands of people. The TRI serves the dual purpose of informing local emergency planning officials of the specific potential risks at covered facilities as well as informing the public at large, who were given the "right-to-know" by EPCRA. The TRI program, however, is simply an accounting and reporting program; there are no limits or controls on the releases of chemical compounds.[3] For each reporting year, eligible firms are required to file a "Form R," detailing their chemical use profile by July 1st of the following year. The information is generally released to the public in the following December. The TRI was seen as a market-based regulation as opposed to a command-and-control regulation. Instead of government regulators dictating the types and quantities of chemicals that could be released, the goal was to reduce emissions by shedding light on the activities of firms, who would in turn react to public pressure by reducing emissions.

Data under the TRI program first became available in 1987. Since its inception in 1987, there have been four major changes to the program.[4] First, in 1994, the

[3] However, several chemicals that firms are required to report under the TRI program are regulated under other statutes, such as the Clean Air Act Amendments (CAAA) or the Resource Conservation and Recovery Act (RCRA).

[4] In addition, there was a voluntary emissions reduction program, the "33/50" initiative, coordinated under the TRI program from 1991-1995. Gamper-Rabindran (2006) found little evidence that

“Phase 1” expansion added 286 chemicals to the inventory list, bringing the total to 602 chemicals. Second, “Phase 2” expanded the range of facilities to include non-manufacturing facilities within the same industry classification codes. At the time, EPA estimated that this would require an additional 6,000 new facilities to begin participation in the program. Phase 1 became effective in the 1995 reporting year data and Phase 2 became effective in the 1998 reporting data. Third, effective in 2000, the courts ruled phosphoric acid was not subject to reporting under EPCRA, and was dropped from TRI. Lastly, effective in the 2001 reporting year, the minimum threshold for lead and lead compounds usage was reduced from 10,000 pounds per year to 100 pounds per year. As previously mentioned, this last regulatory change is the focus of this empirical analysis.

4.3 Literature Review

Several recent papers have turned their attention to the Toxic Release Inventory. A group of papers have focused on how stock market returns and firms were affected by information on releases. Hamilton (1995a) finds that stock market returns on the day TRI data were initially released were negatively correlated with emissions quantity. Khanna et al. (1998) finds that negative stock market returns were correlated with decreases in reported emissions over time but an increase in off-site waste disposal. Bui (2005) discusses a technical issue that could affect the robustness of the findings of the previous studies while finding no significant relationship between stock market returns and emissions for petroleum firms.

Other authors have looked at the relationship between TRI emissions and the housing market. Perhaps most importantly, Bui and Mayer (2003) study the relationship between changes in emissions and changes in housing prices in Massachusetts. Using a first-differenced hedonic approach, they find no significant statistical relation-

this program effectively reduced emissions.

ship between changes in emissions and prices from 1987 to 1992. Their results call into question the notion that public pressure has led to the observed decline in reported TRI emissions since the program's inception.

Brooks and Sethi (1997) examine the characteristics of communities that have higher exposure to TRI emissions. Their results indicate that communities with higher levels of emissions tend to have higher proportions of black residents, less active voters, more renters, and lower educated households. These findings suggest that neighborhoods hosting TRI facilities may be different from other neighborhoods in unobservable ways. Banzhaf and Walsh (2008) examine the "Tiebout Hypothesis" which states that households will "vote with their feet" and move the location that provides the best combination of prices and public goods. Looking at the changes in TRI emissions and changes in population between 1990 and 2000, they find robust evidence that population flows are positively correlated with reductions in pollution. In the context of their model, if households place a negative value on toxic air emissions, they will migrate to areas that have lower amounts of toxic air. Their results suggest that TRI emissions do enter household decision making.

This paper is also related more broadly to the literature on hedonic analysis of environmental quality. Several papers have used proximity to a potential disamenity as the measure of environmental quality, for example, when looking at hazardous waste sites. Kiel and Zabel (2001) and Kiel and Williams (2007) use distance to the nearest Superfund site as a neighborhood amenity and find in many circumstances proximity to sites is inversely correlated with prices. Gamper-Rabindran et al. (2011) and Mastromonaco (2011) employ various fixed-effects specifications and find that housing prices in proximity to hazardous Superfund sites respond positively to site remediations.

Lastly, quasi-experimental designs are increasingly being used in the valuation of environmental quality. Hallstrom and Smith (2005) employ a difference-in-differences approach to examine whether the housing market responds to catastrophic events such as hurricanes. Looking in Lee County, Florida, they find that prices in special flood zones dropped as a result of Hurricane Andrew, suggesting that the hurricane updated households' expectations about the magnitude of damage a major storm can bring. Greenstone and Gallagher (2008) uses a regression discontinuity design based on the Hazardous Ranking Score assigned to Superfund sites at the inception of the program. The statute required EPA to create an arbitrary cut-off point in the continuous hazard score for treatment, which in turn created a set of sites that were very similar but had variation in treatment status. Using the RD design to control for unobservables, they find that listing sites on the National Priorities List had no significant impact on prices.

4.4 Hedonic Pricing Model

The empirical model stems from the hedonic price theory of Rosen (1974). Under hedonic price theory, the price of a product, or in this case a house, is a function of the attributes of the product in the information set of market participants, both observable and unobservable. With many observations of houses with sufficient variation in attributes and transaction prices, it is possible to estimate a marginal price for an additional unit of each attribute.

The empirical question in this paper is whether or not the emissions and toxic chemical data provided in the TRI program enters the information set of consumers. I use the hedonic price model to test for a change in the implicit price for proximity to sites forced to report to the TRI program as a result of the exogenous rule change. The existence and visual impact of the site is within the information set of buyers and sellers before and after the regulatory shift. Once the rule changed, requiring several existing plants to report to the TRI program for the first time introduces new information about the emissions of those firms to buyers and sellers. If this information was already known, or if the information was not important to households, the implicit price for proximity to such a site shouldn't be significantly different after the regulation changes. However, since the visual disamenity and knowledge of the existence of the site doesn't change, finding a significant change in the implicit price for proximity to these sites indicates a change in the information set and a capitalization of the emissions information provided by the TRI program.

4.4.1 Difference in Differences Model

I proceed by specifying the price of house h in time t as:

$$\ln P_{ht} = \theta X_{ht} + \alpha L_{ht} + \beta Z_{ht} + \gamma L_{ht} Z_{ht} + \eta_h + \varepsilon_{ht} \tag{4.1}$$

In Equation (4.1), X_{ht} are the observable attributes of house h, which can include proximity to TRI sites not affected by the lead threshold. L_{ht} is an indicator variable that equals 1 if house h at time t is in proximity to a lead TRI site reporting to the TRI for the first time in 2001, and zero otherwise. Z_{ht} is an indicator variable that equals one if house h sold after the year 2001 TRI data was released. TRI lead sites that are required to report to the TRI program at the initial standard might be different in nature than sites that didn't initially have to report. This specification allows the effect that each type has on the housing market to differ. η_h is a house-specific unobservable attribute that is potentially correlated with TRI proximity. To control for the correlated unobservable attribute, Equation (4.1) can be differenced with a second observation of house h. η_h can be removed by differencing the equation for period t with the equation for period $s < t$. The result of differencing provides:

$$\begin{aligned} \ln P_{ht} - \ln P_{hs} = \theta(X_{ht} - X_{hs}) + \alpha(L_{ht} - L_{hs}) + \beta(Z_{ht} - Z_{hs}) \\ + \gamma(Z_{ht} L_{ht} - Z_{hs} L_{hs}) + (\varepsilon_{ht} - \varepsilon_{hs}) \end{aligned} \tag{4.2}$$

In this equation, γ is the effect of the regulatory change on the marginal price for proximity to the affected TRI lead sites.

When TRI proximity status L_{ht} changes over time due to entry and exit, the term

$(Z_{ht}L_{ht} - Z_{hs}L_{hs})$ does not reduce to a dummy variable. For example, consider two houses, A and B, each with one transaction in period t and one transaction in period s, with $s < t$. For both houses, the two transactions bridge the regulatory change, which implies $Z_{At} = Z_{Bt} = 1$ and $Z_{As} = Z_{Bs} = 0$. However, A is always in proximity to a facility, whereas B is only proximate to a facility for the second transaction because of entry. This implies $L_{At} = L_{As} = 1$ and $L_{Bt} = 1, L_{Bs} = 0$. These will enter the above equation in the same way:

$$(Z_{At}L_{At} - Z_{As}L_{As}) = 1 \times 1 - 0 \times 1 = 1$$

$$(Z_{Bt}L_{Bt} - Z_{Bs}L_{Bs}) = 1 \times 1 - 0 \times 0 = 1$$

The implication of this scenario is that the empirical model cannot distinguish between a house that experiences only a change in information via the regulatory change and a house that experiences a change in proximity status that is concurrent with the regulatory change. This is a conflation of two situations which we might expect to have very different impacts on price appreciation.

The model can be estimated by restricting the sample to houses that didn't experience a change in site status between sales, or $h' \in \{h : L_{ht} = L_{hs}\}$. Under this restriction, Equation (4.2) becomes

$$\ln P_{ht} - \ln P_{hs} = \theta(X_{ht} - X_{hs}) + \beta(Z_{ht} - Z_{hs}) + \gamma(Z_{ht} - Z_{hs})L_h + (\varepsilon_{ht} - \varepsilon_{hs}) \quad (4.3)$$

The term $(Z_{ht} - Z_{hs})$ can be represented by a dummy variable since $s < t$ and the regime change is irreversible.

4.4.2 Regression Discontinuity Model

The discontinuity in who must report to the TRI program prior to 2001 can be exploited in estimation. Consider the following hedonic price equation defining the price of a house:

$$\ln P_{ht} = \beta TL_{ht} + \theta X_{ht} + \eta_h + \varepsilon_{ht} \quad (4.4)$$

where TL_{ht} are lead TRI sites that reported prior to 2001, X_{ht} are structural characteristics of the house and an indicator for non-lead TRI site proximity, η_h is a house level unobservable, and t takes place before the lead requirement changed. In the sample of houses that are in proximity to at least one lead using site (reporting or not), β is the implicit price of the nearby site being listed on the TRI.

Sites that are reporting to the TRI prior to the lead threshold necessarily have a higher annual usage of lead and might be unobservably worse to the housing market. Accordingly, households in proximity to these facilities might have worse unobservable characteristics than the houses that are near lead site below the threshold. As a result, straightforward estimation of Equation 4.4 may lead to biased estimates since TL_{ht} may be correlated with lower unobserved quality.

If unobservables are correlated with the amount of lead being used by a facility, sites just above and just below the threshold should have similar unobservables. However, those sites just above the cutoff receive the treatment of listing in the TRI whereas sites just below do not. The regression discontinuity design estimates Equation 4.4 on the set of houses only near reporting and non-reporting lead sites whose lead usage is just above and just below the threshold. Ideally, unobservables for these sites are balanced above and below the threshold and an unbiased estimate of β is obtained.

4.5 Data

The data used in this paper come from several sources. First, housing transactions data are taken from Dataquick Information Systems. Second, TRI data is taken directly from EPA. Lastly, facility location and existence data is supplemented by data from the California Air Resource Board, pursuant to the Air Toxics "Hot Spots" Emissions Inventory program. In this section, I describe each of these datasets in turn.

4.5.1 TRI Data

The TRI database contains the quantities, types, and release pathways of toxic chemicals released by eligible facilities, as well as the location of those facilities. While this information is self-reported to the EPA, the agency has the ability to levy civil penalties for violations of ECPRA, and can force the rectification of those violations. TRI facility release and location data is available for each year from 1987 to 2009. However, not every facility reports in every year. This could be a result of production activities ceasing, production activities being reduced such that the total quantity of toxic chemicals falls below the threshold, or a plant switching production to output that doesn't require the use of toxic chemicals. As a practical matter, I will treat each facility as existing in years between the first observed year and the last observed year. Using the longitude and latitude of each plant, I can match the facilities panel data set to the housing data set using the longitude and latitude coordinates.

Two important features of the TRI data need to be highlighted. First, the cycle of data collection and data release by the EPA results in TRI data being made public in December of the following year. Reports are due to the EPA by July of the reporting year, and after processing the data, EPA releases the data in December. As a result, I treat December 1, 2002 as the day that information about the emissions and chemical usage of treatment sites became available to the market. Second, usage amounts, which form the basis of reporting requirements, are reported as ranges, not as a continuous amount. The RD sample is formed by including sites that in 2001 report the maximum amount of lead onsite during the year as either 1,000 to 9,999 pounds (usage code "3") or 10,000 to 99,999 pounds (usage code "4"). For sites that don't report until after the threshold change, I assume their first reported maximum lead amount in 2001 was true in previous years.

The California Air Resource Board (CARB) maintains a similar database of facilities and point source emissions. This database was created as part of the Air Toxics

Table 4.1: TRI Sites: Average Releases (lbs)

	N	Mean	Std Dev.	Min	Max
Non-lead TRI Sites	275	12,547	35,802	0	460,668
Lead TRI Sites	82	6,033	20,748	0	164,251
Treatment Sites	12	0.553	1.323	0	4.444
Full Sample	369	10,692	32,562	0	460,668

*Notes:*Based on facilities reporting to the TRI in years 1996 - 2009.

"Hot Spots" Information and Assessment Act. This legislation has many of the same stated goals as the TRI program, namely to identify toxic chemicals in communities and to inform the public. Generally speaking, any facility that manufactures, uses or releases toxic chemicals and releases more than 10 tons of criteria air pollutants must file with the state level inventory program[5]. The reporting requirements for this program are lower than for TRI, but the amount of data available isn't as broad. However, the dataset allows me to identify facilities that were in existence prior to their first reporting date in the TRI. This is crucial for identifying the set of sites who were in existence prior to the lead threshold that reported for the first time in 2001.

Merging the CARB data with the TRI data based on facility name and address, I was able to match 218 of the 369 TRI sites in my sample. Of those 218 sites, 12 sites existed in the CARB data prior to 2001 and reported for the first time in 2001 in the TRI as a lead using site. These 12 sites will be referred to as the "treated sample" throughout the remainder of this paper. Table 4.1 provides an enumeration of the different site types and the average facility-level toxic releases of sites in each category. 275 sites are non-lead TRI sites, 82 sites use lead and either reported before or after the threshold change in 2001. From the table its clear that lead-using TRI sites release approximately half as many pounds of emissions as do non-lead using

[5] Criteria air pollutants are those regulated under the U.S. Clean Air Act. They include ozone, particulate matter, carbon monoxide, sulfur dioxide, nitrogen oxides, and lead.

Table 4.2: Single Sale Houses vs. Panel Houses

Category	One Sale	Multi-Sale
Mean Sq. Footage	1,716	1,559
Mean # of Rooms	7.01	6.59
Mean # of Bathrooms	2.19	2.08
Mean # of Bedrooms	3.31	3.10
Mean Year Built	1971.2	1971.58
Mean Price	$465,175	$416,693
Number of Houses by repeat sales:		
2 Sales		50,855
3 Sales		18,992
4 Sales		5,470
5 Sales		1,271
Total Houses	106,614	76,588
Total Observations in Final Sample	106,614	186,921

Counties Covered: Santa Clara. Dollars reported in year 2000 dollars. Prices used were de-trended using a monthly Case-Shiller Los Angeles Housing Price Index for comparability.

TRI sites. Furthermore, the sites that were reported to the TRI for the first time in 2001 as a result of the threshold reduction have virtually no emissions. This has important implications for the interpretation of any estimated treatment effects since households would be reacting to even though emissions are reported to be close to zero.

4.5.2 Housing Data

The housing transactions data for this analysis comes from Dataquick Information Systems, a real estate information aggregator. The data provides a record of each single family housing transaction, attached and detached, that took place between 1988 and 2008 for Santa Clara County, California. The dataset contains many observable characteristics for each house (e.g. number of bedrooms, square footage, etc.) as well as the transaction price, loan amount, transaction date, latitude and longitude coordinates and the year 2000 census tract. Each property is uniquely identified in the data which allows the creation of a panel data set. In an effort to remove outliers, houses observed in the top and bottom 1% of the price and square footage distribution are dropped, as well as the top 1% of the number of bedrooms and the number of bathrooms distribution. Houses with missing attribute or location data are also dropped from the dataset.

The main analysis of this paper makes use of a panel dataset and house level fixed effects. Sample selection could be an issue if the set of houses that only sell once are substantially different in unobservable ways. Table 4.2 provides the summary statistics for the two sets of houses. Houses that have only sold once tend to be slightly bigger, have more rooms, and sell for approximately 12% more.

An unfortunate feature of the transactions data is that Dataquick will overwrite the characteristics recorded for a given property in previous transactions if a newer transaction is recorded with different and presumably updated information[6]. However, in certain circumstances, if the renovation is on the scale of a large addition or major construction, the transaction will be flagged as having such an improvement. The implication for panel analysis is not being able to reliably observe changes to properties, since any moderate change made to the property is retroactively applied to all records in the data. As a result, all observable characteristics will drop out of any repeat sales analysis. To combat the presence of homes that likely have changed in substantial ways, homes that are observed to appreciate (depreciate) more than 50% on an annualized basis, have the major construction data flag, transact with a loan amount greater than the transaction price by $5,000, or are observed to transact twice or more in any 12 month span are dropped from the sample.

Table 4.3 provides the summary statistics for the various housing samples used in estimation. Column (1) describes the full sample of houses. Column (2) details the base sample which is the set of all houses with at least 1 site of any type less than

[6] Conducting the analysis with only the last observed transaction for each house, which has accurate structural information for the transaction date, does not change the results in meaningful ways. Using only one transaction per house precludes the use of house-level fixed effects, but comparing the results that utilize only the last sale to the alternate specifications that do not use house fixed effects reveal no significant differences.

Table 4.3: Housing Samples

Means	(1) Full Sample	(2) Base Sample	(3) DID Sample	(4) RD Control	(5) RD Sample
TRI Non-lead	0.536	0.868	0.761	0.854	0.981
Treatment Site	0.102	0.166	0.122	0.166	0.184
TRI Lead	0.409	0.662	0.625	0.978	0.999
Year Built	1,971	1,971	1,971	1,969	1,969
Lotsize	5,901	4,932	4,649	4,668	4,361
No. Sq Feet	1,616	1,518	1,468	1,481	1,444
No. Bathrooms	2.125	2.069	2.028	2.019	1.988
No. Bedrooms	3.179	3.071	2.991	3.014	2.926
No. Rooms	6.744	6.488	6.345	6.428	6.293
Price ($)	434,302	401,608	433,347	336,710	330,429
Observations	293,535	181,288	66,193	73,463	44,361

Notes: Prices in year 2000 dollars.

three kilometers away. Column (3) summarizes the sample used in the difference-in-differences estimator. As mentioned previously, these are the set of houses from the base sample that had no change in the number of treatment sites between sales. Column (4) describes the set of houses that sold prior to the announcement of the year 2001 TRI data that were within three kilometers of a site that used lead. By comparison, the RD sample detailed in Column (5) is the set of houses that are in proximity to a site whose lead usage was just above or just below the initial cut off of 10,000 lbs.

Houses in the base sample, which are in proximity to TRI facilities, tend to be on smaller lots, have less square footage, less bathrooms, less bedrooms, less rooms, and lower prices. This is consistent with TRI facilities being located in less desirable neighborhoods. The DID sample appears to have higher prices than the base sample, but this is likely due to the fact that houses that transacted early in the dataset (before the treatment sites were first observed) are dropped from this sample. The RD control sample reveals that the set of houses that surround at least one facility

Table 4.4: Main Results

	(1) OLS	(2) DID - Indicators	(3) DID - Site Counts
γ	-0.047***	-0.031***	-0.018***
	(0.006)	(0.006)	(0.004)
Treatment Site	0.018***	-	-
	(0.005)		
TRI Non-Lead	-0.172***	-0.008*	0.002**
	(0.003)	(0.004)	(0.001)
TRI Lead	-0.078***	0.003	0.006**
	(0.002)	(0.004)	(0.003)
β	-0.010	-0.039**	-0.039*
	(0.021)	(0.018)	(0.021)
Observations	181,288	66,193	56,289
R-squared	0.999	0.456	0.451

Notes: *** Denotes significance at the 1% level, ** denotes significance at the 5% level, * denotes significance at the 10% level. Standard errors are in parentheses below each estimate.

using lead looks similar to the base sample of houses that can be in proximity to any type of TRI site. The RD sample indicates that the RD design doesn't appear to be selecting a different type of house.

4.6 Results

4.6.1 Difference in Differences

Table 4.4 presents the results of the difference in differences analysis. All specifications include an indicator for proximity to non-lead TRI sites and lead TRI sites not affected by the threshold reduction. Column (1) presents the coefficients from the OLS regression, Equation 4.1. Here, the point estimate for the effect of the threshold change on housing prices is -4.7%. The implicit price for proximity to TRI sites is negative and significant, but the coefficient for proximity to treatment sites is positive. The non-treatment sites, which were reporting to the TRI under the older, higher standard, likely have worse impacts on the housing market. Within this set of sites, the treatment sites would have a relatively positive effect on prices. Prices drop by 17.2% for houses within 3 kilometers of a non-lead TRI site and 7.8% for lead TRI sites. Without any controls for endogeneity, it is possible that these estimates are biased. If TRI sites are located in areas with low neighborhood unobservables that cause prices to appreciate at a lower rate, these coefficients would be biased downwards.

Column (2) provides the estimates from the difference in differences regression from Equation 4.3. After controlling for unobservables, the estimates for proximity to TRI sites are much closer to zero and insignificant. This suggests that the correlated unobservables were driving the negative estimates in the OLS regression. However, the coefficient of interest, the treatment effect of the regulatory change, is still negative and highly significant. The listing of existing sites in the TRI as a result of the lead threshold reduction lowers nearby housing prices by 3.1%. This suggests that the release of new information about emissions and/or quantities of onsite toxic chemicals is acted upon by housing market participants and capitalized into prices. Moreover, since the set of treatment sites have virtually no emissions, its likely that this treatment effect is attributable to household concerns about the risks of living in proximity to toxic chemicals, not the effects of toxic releases.

In the first two specifications, TRI sites were only allowed to affect houses via a binary proximity variable indicating that the house was near at least one site. Given the quantity of facilities in Santa Clara County, houses that are in close proximity to several sites are not uncommon. Column (3) provides the results of Equation (4.3) where TRI indicator variables are replaced by an integer count of the number of sites of each type that are within 3 kilometers of the property. The specification assumes that the logarithm of price is linear number of sites. Interestingly, these results indicate that the average home value dropped 1.8% for *each* TRI site within proximity. In the data there are 2,116 homes that were within 3 kilometers of two TRI sites

Table 4.5: Varying Radii Results

	(1) 1 km	(2) 2 km	(3) 3 km
γ	-0.047***	-0.038***	-0.031***
	(0.011)	(0.007)	(0.006)
TRI Non-Lead	0.002	-0.001	-0.008*
	(0.006)	(0.004)	(0.004)
TRI Lead	-0.006	0.010*	0.003
	(0.009)	(0.005)	(0.004)
β	-0.043**	-0.041**	-0.039**
	(0.018)	(0.018)	(0.018)
Observations	66,193	66,193	66,193
R-squared	0.456	0.456	0.456

Notes: *** Denotes significance at the 1% level, ** denotes significance at the 5% level, * denotes significance at the 10% level. Standard errors are in parentheses below each estimate.

forced to report in 2001 as a result of the threshold reduction. These properties, on average, experienced an 3.6% drop in value.

4.6.2 Varying Proximity

Table 4.4 presents results under the assumption that the "radius of impact" that a TRI site can have on nearby houses is three kilometers. Table 4.5 examines this assumption and relationship between distance and price effects. If proximity to TRI site matters, one might expect to see stronger price effects closer to the site that diminish as distance increases. In this table, Equation (4.3) is estimated for various radii of impact. Column (1) starts with a 1 kilometer radius, Column (2) uses 2 kilometers, and Column (3) reproduces the results from Table 4.4 that uses 3 kilometers.

Comparing the results across columns conforms with intuition. Within 1 kilometer, prices drop on average by 4.7%. After expanding to 2 kilometers, the price effects are smaller in magnitude, with prices dropping on average at 3.8%. These figures compare with a price decline of 3.1% at 3 kilometers. The impact of the

Table 4.6: RD Results

	(1)	(2)	(3)	(4)	(5)	(6)
Radius	1 km		2 km		3 km	
Specification	OLS	RD	OLS	RD	OLS	RD
TRI Lead (treatment)	-0.033***	-0.025***	-0.013***	-0.013**	-0.058***	-0.069
	(0.006)	(0.007)	(0.004)	(0.006)	(0.016)	(0.057)
TRI non-lead	-0.003	0.003	0.002	0.003	0.002	0.007
	(0.004)	(0.005)	(0.005)	(0.006)	(0.007)	(0.014)
No. Bedrooms	-0.008***	-0.005	-0.007**	-0.004	-0.007***	-0.004
	(0.003)	(0.004)	(0.003)	(0.004)	(0.003)	(0.004)
No. Sq Feet	0.560***	0.574***	0.563***	0.574***	0.563***	0.576***
	(0.010)	(0.013)	(0.010)	(0.013)	(0.010)	(0.013)
No. Bathrooms	0.011***	0.019***	0.011***	0.018***	0.010***	0.018***
	(0.004)	(0.005)	(0.004)	(0.005)	(0.004)	(0.005)
No. Rooms	0.007***	0.004*	0.007***	0.004*	0.007***	0.004*
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Year Built	0.003***	0.003***	0.003***	0.003***	0.003***	0.003***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Lotsize	0.113***	0.118***	0.114***	0.119***	0.114***	0.119***
	(0.002)	(0.003)	(0.002)	(0.003)	(0.002)	(0.003)
Observations	73,342	44,267	73,342	44,267	73,342	44,267
R-squared	0.999	0.999	0.999	0.999	0.999	0.999

Notes: *** Denotes significance at the 1% level, ** denotes significance at the 5% level, * denotes significance at the 10% level. Standard errors are in parentheses below each estimate.

regime change is felt more for houses that are closer to the site than ones that are farther away. This is consistent with the fact that the risk households face from a catastrophic accident decreases with distance.

4.6.3 Regression Discontinuity Results

Table 4.6 provides the estimates for the regression discontinuity analysis from Section 4.4.2. The sample of houses for this analysis is all houses that sold before the threshold change that were in proximity to a lead site, either TRI or pre-TRI. Since each house is near at least one facility, the coefficient of interest is the coefficient on the indicator for TRI lead sites. This provides the marginal value of having a nearby facility listed in the TRI. In addition to this indicator variable, each specification contains controls for non-lead sites, structural characteristics of each house, and a census-tract level neighborhood fixed effect. Columns (1) and (2) display the estimates from Equation (4.4) on both the pre-2002 sample and the "RD Sample" which controls for unobservables using a radius of impact of 1 kilometer. Under the pre-2002 sample, the OLS estimate of the effect on prices of having a TRI site listed is -3.3%. When moving to the "RD Sample", the estimate of the price effect becomes less negative, but is precisely estimated to be -2.5%.

Columns (3) and (4) repeat this analysis for an expanded impact radius of 2 kilometers. Without using the "RD Sample", the TRI listing price effect is estimated to be -1.3%. Adding the RD controls has little effect on the point estimate. The last columns repeat the analysis for a radius of 3 kilometers. While the OLS specification shows a precisely estimated negative estimate (-5.8%), this effect disappears when the "RD Sample" is employed. Like Table 4.5, Table 4.6 suggests that the effects are stronger closer to the facility and diminish as distance increases.

Taken on whole, the price effects of TRI information are less negative under the RD specification than under the DID specification. Each method appears to control for unobserved heterogeneity and both methodologies indicate that the TRI program provides information to the housing market. Moreover, this information negatively impacts houses near listed facilities, and the effect decreases with distance.

4.7 Conclusion

This paper examines the role that information about the risks posed by facilities reporting to the TRI program has on the housing market. The results indicate that households negatively value living in close proximity to facilities that are required to report to the TRI. Interestingly, the analysis concurs with previous research that suggests that emissions from TRI plants do not affect housing prices. This suggests that the TRI program communicates to the housing market information about the risks of potential catastrophic releases of toxic chemicals.

First, I use a change in the lead reporting threshold requirements of the TRI program in 2001 to estimate the price effects of listing existing facilities in the TRI. Employing a difference-in-differences estimator, I find that drops in local housing prices around these facilities are estimated to be 3.1%. This result is robust to varying the radius of impact from 1 to 3 kilometers, as well as accounting for the presence of multiple sites. Second, I use the discontinuity created by the original 10,000 lbs reporting threshold for lead to estimate the effect of TRI designation. The regression discontinuity design assumes that in the neighborhood of the reporting threshold, facilities have similar unobservables. With this specification, estimates of the price effects of TRI listing are smaller, but still significant at tighter proximity measures.

The findings of this paper suggest that the Toxic Release Inventory program is communicating information to local residents as designed. However, the evidence doesn't support that household react to the detailed emissions reports, but rather to the knowledge that there are toxic chemicals in large quantities present in their vicinity. The results also suggest that there may be an alternative mechanism driving the observed reductions in emissions over the lifespan of the TRI program. If local residents are more sensitive to large quantities of onsite chemicals than to emissions, firms may have an incentive to avoid scrutiny by operating under TRI reporting thresholds. Observed emissions reductions may be in fact driven by public pressure, but only a byproduct of firms seeking to avoid detection. Under this scenario, emissions may not actually be falling but becoming under reported; an outcome that was surely not the intent of policy makers.

Appendix A

Equilibrium Simulation Procedure

A.1 Counterfactual Simulation Procedure

I follow the lead of Benkard et al. (2010) to simulate outcomes under counterfactual states. In their study of airline mergers, they assume that counterfactual industry structures do not alter the strategies played by airlines. This allows estimated conditional choice probabilities, which are mappings from states to actions, to predict the actions taken by players in a game for any initial state. In my application, from the first stage estimation procedure, I have estimates of a flexible function that maps states to choice specific value functions. For any state, I can calculate the value of all choice specific value functions for each type of household, which provides their optimal choice. With this information, I can simulate all households' choices and "roll forward" the universe of neighborhoods to update the observable variables in each neighborhood for each period.

The intuition behind the "forward simulation" procedure in Bajari et al. (2007) is that a good approximation of the value function for any given state is the discounted sum of flow utility from that point into the indefinite future. In my setting, I employ a two step procedure to approximate the value function in this way. The first step assumes a continuum of households to simulate the states (exogenous and endogenous) in subsequent periods. The second step takes a representative household and simulates paths of play through the simulated state space, summing flow utility along each path. Discounting these flows provides the approximated value function.

A.2 State Space Forward Simulation

Since the "universe" in this model is all the neighborhoods in the Bay Area, it is necessary to simulate the set of observable and unobservable states for each neighborhood. First, I start all simulations using observed year 2008 data as the initial state, s_0. Moreover, I use the empirical distribution of household characteristics to calculate type-specific "within" shares for each neighborhood, $\{\{\phi_j^\tau\}_{\tau=1}^T\}_{j=1}^J$, and for the metropolitan area overall $\{S_\tau\}_{\tau=1}^T$. Second, I estimate the following equation to get the transition probabilities for violent crime, ozone concentration and the type-specific neighborhood unobservable:[1]

$$X_{jt} = \rho_0 + \rho_1 X_{jt-1} + \delta_j + \eta_{jt} \tag{A.1}$$

Third, I assume that market clearing can be accomplished by an equilibrium price function. I estimate a flexible hedonic neighborhood-level price function:

$$P_{j,t} = \alpha_0 + g(X_{j,t};\alpha_x) + \theta_j^1 + \theta_t^2 + \varepsilon_{j,t} \tag{A.2}$$

where $g(\cdot)$ contains a second order polynomial and all interactions in observable characteristics, θ^1 and θ^2 are neighborhood and year fixed effects, respectively. The result is a function, $P(X,\hat{\alpha})$, that predicts equilibrium prices based on observables.

With knowledge of the distribution of household types, estimated state transition probabilities, and an equilibrium price function, it's possible to simulate the evolu-

[1] The type specific neighborhood unobservable is taken from the estimation procedure as the residual from the Stage 4 regression.

tion of the metropolitan area for T periods. I utilize the following algorithm to take $s_t = \{X_{jt}, P_{jt}\}_{j=1}^{J}$, the state at time t to s_{t+1}:

- Step 1: For each type τ, calculate the choice probabilities $\widehat{\sigma}(X_{jt}, Z_\tau; \hat{\alpha})$
- Step 2: Use Equation (3.10) to calculate the choice specific value functions, $\tilde{v}_{jt}^{\tau}$
- Step 3: Use Stage 2 estimates to set normalizations to get v_{jt}^{τ}
- Step 4: Take draws of the idiosyncratic error, $\epsilon_{j'}$, for each type τ and neighborhood j and solve optimization problem: $\max_{j'} v_{j't}^{\tau} - MC(X_{jt}, P_{jt})I_{j \neq j'} + \epsilon_{j'}$
- Step 5: Use optimal decisions to update within neighborhood type specific shares, $\{\{\phi_j^\tau\}_{\tau=1}^{T}\}_{j=1}^{J}$
- Step 6: Calculate % white in each neighborhood using $\{\{\phi_j^\tau\}_{\tau=1}^{T}\}_{j=1}^{J}$, take draws from η to update $(\bar{X}_{jt+1}, \xi_{jt+1})$, and calculate prices in t+1 using $P(X_{t+1}, \hat{\alpha})$

To obtain the simulated T-period state space, I simulate the evolution of the state space 1,000 times. Each simulation represents a path of play initiated by the initial state, s_0. The final value for each X_{jt} is taken to be the average value obtained in the 1,000 simulations.

A.3 Value Function Approximation

The forward simulation of T periods of the state space utilizes all aspects of the decision model, including moving costs, wealth accumulation, and forward looking behavior. However, since households were treated as a continuum, calculating the welfare of a specific household isn't possible. The goal of the second step of the simulation procedure is to calculate the value function, $V(s_0, Z_\tau)$, for each τ. For simplicity, since the counterfactual policy is the remediation of one neighborhood, I only calculate the welfare implications for residents of the target neighborhood.

To approximate the value function for a type τ household in the target neighborhood facing s_0, I need to simulate a large number of sequences of flow utility, allowing for wealth accumulation, moving costs, and forward looking decision making. Starting at $t = 1$, the recursive procedure to calculate welfare is the following:

- Step 1: Calculate the choice probabilities $\widehat{\sigma}(X_{jt}, Z_\tau; \hat{\alpha})$
- Step 2: Use Equation (3.10) to calculate the choice specific value functions, $\tilde{v}_{jt}^{\tau}$
- Step 3: Use Stage 2 estimates to set normalizations to get v_{jt}^{τ}
- Step 4: Take draws of the idiosyncratic error, $\epsilon_{j'}$, for each neighborhood j and solve optimization problem: $\max_{j'} v_{j't}^{\tau} - MC(X_{jt}, P_{jt})I_{j\neq j'} + \epsilon_{j'}$
- Step 5: Calculate discounted flow utility for period t, $\beta^{(t-1)}(U(X_{j't}, P_{j't}, \xi_{j't}, Z_{\tau_t}) - MC(X_{jt}, P_{jt})I_{j\neq j'} + \epsilon_{j'})$
- Step 6: Update wealth level and type: $W_{t+1} = W_t + (P_{j',t+1} - P_{j,t})$; $Z_{\tau_t} \Rightarrow Z_{\tau_t+1}$
- Step 7: Update new "home" neighborhood $j = j'$
- Step 8: Move to Step 1 if $t < T$

Once this algorithm terminates, I calculate one simulated value function by summing the discounted flow utilities from Step 5. Averaging over 1,000 simulations provides the approximated welfare under state s_0. This procedure allows households to re-sort to other neighborhoods and pay moving costs. Additionally, wealth is updated every period, which has two effects on welfare. First, since wealth enters the utility function, positive wealth accumulation can directly increase utility. Second, wealth changes impact a household's type, τ, which determines how choice specific value functions are calculated each period. For example, a household that starts out with low wealth and accumulates wealth over time will begin to "act wealthy."

A.4 Summary

The two step procedure presented above relies on the assumption that the initial state vector, s_0, is not sufficiently different as to change the expectations and equilibrium behavior of households. This assumption implies that the estimated conditional choice probabilities can be used to predict optimal decisions. If this assumption doesn't hold, households will make suboptimal decisions, obscuring true welfare effects. However, the presence of substantial moving costs will make not moving the optimal decision. If this is constant over a wide range of equilibrium behavior profiles, the failure of the key assumption is likely to be washed out by moving costs, increasing the confidence in the simulated welfare effects.

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Biography

Ralph Anthony Mastromonaco was born June 29, 1983 in Monroeville, Pennsylvania. He has earned a B.S.B.A, majoring in Finance, and a B.A., majoring in Economics, in 2005 from the University of Pittsburgh in Pittsburgh, Pennsylvania, where he graduated Summa Cum Laude. He was also inducted as a member of Phi Beta Kappa and Beta Gamma Sigma honor societies. In 2008, he earned an M.A. in Economics from Duke University. In 2012, he completed his Doctor of Philosophy in Economics at Duke University. At Duke University, he was awarded a Graduate Fellowship award for the 2007 - 2008 academic year, and the Graduate School Summer Research Fellowship in 2009 and 2010. Beginning in the fall of 2012, he will be an Assistant Professor in Economics at the University of Oregon in Eugene, Oregon.