Essays in Residential Choice and Non-Market Valuation

by

Tatyana Kuzmenko

Department of Economics
Duke University

Date: ______________________
Approved:

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Christopher D. Timmins, Supervisor

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Frank A. Sloan

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Gale A. Boyd

____
Charles M. Becker

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
2012

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Abstract

This dissertation focuses on non-market valuation of environmental goods, using housing values and residential location decisions to infer the value of local environmental quality. It makes two contributions to the literature:

First, it explores the role of expected longevity in environmental risks valuation in a framework where individuals face multiple mortality and morbidity risks. The presence of background mortality risk from the causes unrelated to pollution reduces the willingness to pay for environmental risks reduction, because the competing risk lowers the chance that the consumer will be alive to benefit from environmental quality improvements. Using data from the Health and Retirement Study, we find that individuals with shorter expected lifespan are more likely to reside near toxins-emitting facilities. The effect of the longevity expectations is significant as compared to other determinants of environmental risk exposure identified in the previous literature, such as level of education, race, and wealth. These findings imply that the expected longevity can be an important source of heterogeneity in environmental risks valuation.

Second, we find evidence that owner-assessed home values "lag behind" market prices, and that this lag has the potential to substantially alter values ascribed to local amenities using property-value hedonic techniques. We hypothesize that long-standing homeowners lack an incentive to gather recent information on housing markets, and that their ignorance is reflected in the bias in their self-reported housing values. We first demonstrate that bias in self-reported home values is significantly correlated with tenure. We then examine how tenure length affects homeowners' valuation of changes in a particular local amenity – exposure to sites on the EPA's National Priorities List (i.e., the "Superfund" program). We find that recent movers report a value of a site's deletion from that list (signaling the end of the Superfund remediation process) that is 30-50% greater than that expressed by long-standing owners. This difference is significant and can have important implications for the results of cost-benefit analysis.

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1

Introduction

Economists have long been interested in examining the determinants and consequences of individuals' location decisions. As suggested by Tiebout (1956), people sort themselves across space according to their preferences for local amenities that differ across neighborhoods. This simple insight plays a major role in public and environmental policy evaluation. Environmental initiatives often become a subject of policy debate, because convincing estimates of their costs and benefits are not readily available. As explicit markets for environmental goods do not exist, economists commonly rely on observation of place of residence decisions to infer the economic value of these non-market goods to individuals. The idea that people select their residence location based partly on their preferences for local public goods underlies hedonic property models that are widely used to measure the values of non-market goods.

This dissertation contributes to the vast literature that uses housing values and residential location decisions to infer the value of local environmental quality. It is organized as following:

Chapter 2 focuses on residential sorting with respect to environmental disamenities. In particular, it identifies a source of heterogeneity in preferences for environmental quality that has been overlooked in previous studies - expected longevity. The presence of background mortality risk from the causes unrelated to pollution reduces the willingness to pay for environmental risks reduction, because the competing risk lowers the chance that the consumer will be alive to benefit from environmental quality improve- ments. Using data from the Health and Retirement Study, I find that individuals with shorter expected lifespan are more likely to reside near toxins-emitting facilities. In order to control for the presence of measurement error and endogeneity of longevity expectations, I instrument for subjective life expectancy with parental longevity which proxies for genetic or hereditary components of the person's health. The effect of the longevity expectations is significant as compared to other determinants of environmental risk exposure identified in the previous literature, such as level of education, race, and wealth. These findings imply that the expected longevity can be an important source of heterogeneity in environmental risks valuation.

Chapter 3 turns attention to the measurement of the value of local environmental goods using property value information. The property value hedonic framework holds that the value of a house is determined by its component characteristics, including neighborhood amenities and disamenities (see Palmquist (2005) and Taylor (2003) for a summary of the theory behind hedonic valuation and a thorough discussion of the hedonic literature). In empirical applications, researchers often use datasets such as decennial Census or American Housing Survey, where home values are assessed by their owners. The standard (implicit) assumption is that homeowners can accurately asses the value of their home and take into account any changes in home and neighborhood characteristics. This chapter challenges this assumption and demonstrates that changes in amenities are unlikely to be instantly reflected in house values as homeowners need time to acquire and process new information. In particular, we find evidence that owner-assessed home values ”lag behind” market prices, suggesting that homeowners rely on outdated information. This has important implications for the use of homeowners’ stated housing values to estimate the value of local public goods and amenities - recent movers are likely to be more informed about current state of neighborhood amenities and home prices. Long-standing owners, on the other hand, do not have many incentives to exert effort to learn about the neighborhood changes; they are subsequently more likely to rely on outdated information. With this as a backdrop, we examine the differential impact of Superfund remediation actions on owner-assessed housing values. We find that recent movers report a value of a site’s deletion from that list (signaling the end of the Superfund remediation process) that is 30-50% greater than that expressed by long-standing owners. This difference is significant and can have important implications for the results of cost-benefit analysis.

This dissertation contains collaborative work. In particular, Chapter 3 is a revised version of a working paper produced jointly with my adviser Christopher D. Timmins. Dr. Timmins and I are regarded as equally contributing authors of this paper.

2

Expected Longevity and Demand for Clean Environment

This paper explores the role of expected longevity in determining the demand for clean environment in a framework where individuals face multiple mortality and morbidity risks. The presence of background mortality risk from the causes unrelated to pollution reduces the willingness to pay for environmental risks reduction, because the competing risk lowers the chance that the consumer will be alive to benefit from environmental quality improvements. Using data from the Health and Retirement Study, I find that individuals with shorter expected lifespan are more likely to reside near toxins-emitting facilities. In order to control for the presence of measurement error and endogeneity of longevity expectations, I instrument for subjective life expectancy with parental longevity which proxies for genetic or hereditary components of the person's health. The effect of the longevity expectations is significant as compared to other determinants of environmental risk exposure identified in the previous literature, such as level of education, race, and wealth. These findings imply that the expected longevity can be an important source of heterogeneity in environmental risks valuation.

2.1 Introduction

Without convincing estimates of their benefits and costs, environmental initiatives often become a subject of policy debate. In the absence of explicit markets for environmental goods, economists typically infer the value of these non-market goods from the various decisions individuals make in the labor and housing markets. While the individuals might differ in their willingness to pay (WTP) for health and mortality risks reduction, it is standard practice to focus on the estimates that reflect preferences of an average consumer. Some environmental programs disproportionately affect individuals who differ significantly from the average. For example, most of the benefits of air quality regulations come from mortality and morbidity risk reductions among people who are elderly and have pre-existing impairments. If these individuals have preferences for spending on risk reductions that differ from those of the population average, an analysis based on the average wealth-risk trade-offs will not reflect their preferences. In the recent past, it has been suggested that since older people have fewer years of life left, they may have lower WTP for mortality and morbidity risk reduction. This proposition has become a subject of a strident and continuous debate in the literature that only intensified when the US Environmental Protection Agency's attempt to use age adjustments in benefit values generated a public outcry (Evans and Smith 2008 provide an overview of the media reaction). Numerous studies have contributed to this debate (Aldy and Viscusi 2007 and Krupnick 2007 review the revealed and stated preference evidence, respectively); however, the vast majority of them have focused on the heterogeneity in the risk-wealth trade-offs with respect to age rather than the quantity of life left. It is still not clear whether differences in life expectancy play a significant role as a source of heterogeneity in the WTP for risk reduction.

This paper examines the relationship between life expectancy and the demand for clean environment. The theoretical framework used in this paper follows the literature that examines health investments and wealth-risk trade-off when there are multiple causes of death (Dow et al., 1999; Eeckhoudt and Hammitt, 2001). The presence of background mortality risk from the causes unrelated to pollution reduces the WTP for environmental risks reduction, because the competing risk lowers the chance that the consumer will be alive to benefit from environmental quality improvements. I extend the previous theoretical analysis by demonstrating that background mortality risk reduces the WTP for non-deadly health risks as well.

I test the model implications in the context of environmental risks valuation. My empirical analysis relies on Health and Retirement Study (HRS). Using the restricted-access Geographic Information HRS supplement that provides information on the survey participants' residence location at the census tract level, I combine the HRS individual data with information on polluting facilities obtained from the EPA's Toxic Release Inventory (TRI) database to examine the relationship between the individual expected longevity and proximity to toxins-emitting facilities. In order to control for the presence of measurement error and endogeneity of longevity expectations, I instrument for subjective life expectancy with parental longevity which proxies for genetic or hereditary components of the person's health, so it is not correlated with the direct effect of pollution on health. This instrumental strategy follows prior studies that employ the HRS data to examine the effect of longevity expectations on health investments (Fang et al. 2007) or retirement and wealth accumulation (Bloom et al. 2007)[1].

I find that the perceived mortality risks influence the residential choice in ways that are consistent with the model predictions: the individuals who expect to live for a shorter period of time are indeed more likely to live near TRI facilities, ceteris

[1] Similar to Fang et al. (2007) and Bloom et al. (2007), I find that the OLS estimates are much smaller in magnitude than the IV estimates. This comparison suggests significant attenuation bias in the OLS results due to a measurement error in the expected longevity variable.

paribus. The results are robust across a variety of alternative specifications. Most importantly, I find that results are not particularly sensitive to changes in specifications involving the alternative wealth measures; thus, the relationship between longevity expectations and pollution exposure is unlikely to be driven by unobserved differences in income and wealth.

Moreover, the effect of longevity expectations on these decisions is profound as compared to other determinants of environmental risk exposure identified by the previous literature, such as level of education, race, and wealth. For example, the effect of 40 to 50-percentage point decrease in that probability is comparable to the effect of being black. These findings imply that expected longevity can be an important source of heterogeneity in environmental risks valuation.

This paper makes several contributions to the literature:

First, it identifies a particular source of heterogeneity for non-market valuations - the source that has been overlooked in previous studies. The findings of this paper are consistent with the view that the willingness to pay for risk reduction might be lower among the older individuals because of the differences in life expectancy. However, the vast majority of the previous studies focused on the effects of age rather than longevity. By using elicited longevity expectations, this study provides direct evidence supporting the idea that preferences for environmental risks reduction can vary with expected lifespan.

Second, this study provides an empirical test of the theoretical analysis of the effects of background risks on the willingness to pay WTP for risk reduction (the related literature is reviewed in the next section); moreover, this is the first paper to do that in the environmental context.

Finally, my findings have important implications for the literature that measures the effects of pollution on human health (e.g., see Brunekreef and Holgate (2002) for an overview of studies on the health effects of air pollution). While researchers often point out that pollution exposure might be endogenously determined (e.g. Neidell 2004), there has been little empirical evidence to support this claim. This paper fills in this gap by documenting the effect of genetic health endowment on the level of pollution exposure.

The remainder of the paper is organized as follows. Section 2 reviews the additional related literature. Section 3 presents a simple conceptual framework to illustrate the link between expected longevity and the demand for risk reduction. Section 4 describes the data sets used in the analysis, Section 5 presents the findings. Finally, Section 6 concludes.

2.2 Related Literature

My framework builds upon Eeckhoudt and Hammitt (2001) and Liu and Rettenmaier (2007) in their theoretical analysis of background mortality risk effects on the WTP for mortality risk reduction and risk-taking behavior. The former study shows that an increase in background mortality risk reduces the marginal willingness to pay for risk reduction, and the latter finds that individuals will respond to a new mortality risk by increasing their exposure to financial and other mortality risks. Dow et al. (1999) examine complementarities between health investments in the prevention of cause-specific mortality risks. Their analysis yields testable predictions that are in line with the findings by Eeckhoudt and Hammitt (2001) and Liu and Rettenmaier (2007).

Interestingly enough, the literature suggests that the effect of health status on the willingness to pay for risk reduction or health investments might differ from the one found for mortality risks. Bleichrodt et al. (2003) show that the willingness to pay for health improvements *increases* with the severity and probability of occurrence of comorbidities. Strand (2006) finds that the effect of health status on the valuation of environmental improvements is ambiguous; Hammitt (2002) suggests that the same holds in the context of VSL. Some empirical studies report that WTP for mortality risk reduction is higher for people with select serious health conditions, while other find that WTP does not vary appreciably with health status (Alberini et al. 2004, Alberini et al. 2006, Krupnick et al. 2002, Andersson 2007).

There are a few studies that examine the effect of life expectancy or health endowment on the WTP for life protection in a dynamic framework. Hammitt (2002) suggests that this effect is ambiguous, because individuals face a trade-off between spending to increase their likelihood of survival and conserving wealth for consumption in future periods. Ehrlich (2000) analyzes the individual's demand for life protection and longevity in a life-cycle context and finds that the effect of health endowment on life protection is the result of several opposing effects. Ehrlich and Yin (2005) perform numerical simulations using a calibrated version of the same model and find that better health endowment increases the value of life saving.

There is limited empirical research testing those theoretical findings. Fang et al. (2007) is the most closely related to my paper. They test whether greater life expectancy increases the investment in health using the data from the Health and Retirement Study. As life expectancy may be endogenous in the decision rule for health investment, it is instrumented by parental longevity. They find clear evidence for this effect with respect to smoking, but mixed evidence with respect to heavy drinking and obesity.

Dow et al. (1999) provide a theoretical and empirical analysis of the incentives introduced by disease-specific mortality interventions. Their theoretical analysis implies that an exogenous reduction in one of the mortality risks creates health investment incentives that reduce mortality from other causes. They find strong empirical evidence in support of their theory: the availability of vaccines during pregnancy significantly increases birthweight - the effect that cannot be attributed to medicine. Ganz (2000) relies on the framework used by Dow et al. (1999) to analyze how a specific health behavior, smoking, is related to measures of external threat. He finds that exposure to lifetime trauma and living in an unsafe neighborhood is correlated with smoking status, but not with smoking intensity, concluding that the support for the framework was mixed. The author also notes that this relationship may be not causal.

Finally, Evans and Smith (2008) examine the relationship between risk-wage trade-off, health status, and complementarity between consumption and labor supply. They find that those in excellent health require significantly greater compensation to accept risk increases compared to those who are not; at the same time, people facing future longevity threats are willing to pay more for risk reduction.

2.3 Model

Basic Framework and Assumptions

Consider an individual who derives utility from wealth (w) and from the state of the world in which this wealth is consumed. Let $L(w)$ denote the utility if alive, $D(w)$ - utility if dead (this can be interpreted as the consumer preferences regarding bequests as well as consumption during the part of the period she is still alive).

It is assumed that:

- $D' \geqslant 0$, $L' > 0$, $D'' \leqslant 0$, $L'' \leqslant 0$: the individual is non-satiated and risk-averse with respect wealth.

- $D(w) < L(w)$: the individual always prefers life to death for all relevant values of w

- $D'(w) < L'(w)$: the marginal dollar generates more satisfaction if alive. This is a common assumption in the literature (see e.g. Eeckhoudt and Hammitt 2001, Liu and Rettenmaier 2007, Bleichrodt et al. 2003). Viscusi and Evans (1990) and Sloan et al. (1998) provide empirical evidence supporting this assumption.

Reference case - no competing risks

If the probability of death is p, the individual's expected utility is:

$$EU(p) = pD(w) + (1-p)L(w). \tag{2.1}$$

The individual may be willing to give up some of his wealth in order to reduce the risk of death. The expression for the marginal willingness to pay (MWTP) for a reduction in p is obtained by differentiating the expected utility with respect to w and p while keeping EU constant:

$$MWTP = \frac{dw}{dp} = \frac{L(w) - D(w)}{pL'(w) + (1-p)D'(w)}. \tag{2.2}$$

The MWTP for risk reduction is equal to the utility difference between survival and death divided by the expected marginal utility of wealth. The population mean MWTP for a reduction in fatality risk is denoted by the value of a statistical life (VSL), since it is equivalent to the monetary value of a risk reduction that would save one statistical life (Jones-Lee 1974, Eeckhoudt and Hammitt 2001). Since $D'(w) < L'(w)$, the MWTP for risk reduction increases with baseline risk.

Effect of a competing mortality risk on the MWTP

Now let the risk p be a "specific mortality risk" (such a workplace accident), and suppose that the individual also faces an independent mortality risk π. How would the WTP for reduction in p depend on the competing risk π?

The individual's expected utility now is:

$$EU(p,\pi) = (1 - (1-p)(1-\pi))D(w) + (1-p)(1-\pi)L(w). \tag{2.3}$$

The WTP for reduction of the risk p is given by:

$$WTP(\pi) = \frac{dw}{dp} = \frac{(1-\pi)(L(w) - D(w))}{(1-(1-p)(1-\pi))D'(w) + (1-p)(1-\pi)L'(w)} \tag{2.4}$$

$WTP(\pi)$ represents marginal willingness to pay per unit reduction of the specific risk p, not the total mortality risk. It depends on the level of the background risk π: a reduction in the specific risk by Δp reduces total mortality by $(1-\pi)\Delta p$.

Simple algebra shows that:

$$\frac{\partial WTP(\pi)}{\partial \pi} = \frac{D'(L-D)}{((1-(1-p)(1-\pi))D' + (1-p)(1-\pi)L')^2} \leqslant 0 \tag{2.5}$$

So an increase in the background risk π cannot increase WTP for reduction of the specific mortality risk p, since the competing risk reduces the chance that the consumer will benefit from a reduction in the specific risk. Eeckhoudt and Hammitt (2001) call it *"why bother?"* effect.

Eeckhoudt and Hammitt (2001) note that in usual VSL applications where risks are small, the effect of background mortality risk is likely negligible. However, they point out that this effect might be important, e.g. in air quality applications, where risks are believed to be concentrated among people who are elderly and have pre-existing impairments (so they face more sizable background mortality risks).

Effect of a competing mortality risk on the optimal risk level

Next I show that, given an equilibrium wealth-risk trade-off $m(p)$, an individual who faces higher background risk level π will choose to take on more of the specific risk p.

A consumer's problem is given by

$$\max_p \; (1-(1-p)(1-\pi))D(w_0 + m(p)) + (1-p)(1-\pi)L(w_0 + m(p)), \tag{2.6}$$

where w_0 is the initial wealth, $m(p)$ is the "market price of risk", $m'(p) \geqslant 0$. The first-condition of this optimization problem is

$$\begin{aligned} F = &[(1-(1-p)(1-\pi))D'(w_0+m(p)) + (1-p)(1-\pi)L'(w_0+m(p))]m'(p) \\ &+ (1-\pi)(D(w_0+m(p)) - L(w_0+m(p)) = 0 \end{aligned} \tag{2.7}$$

Solving this equation yields the optimal risk level $p = p(\pi, w_0)$. Next, differentiating the FOC with respect to π while taking into account that p is a function of π, we obtain:

$$\frac{\partial F}{\partial \pi} + \frac{\partial F}{\partial p}\frac{dp}{d\pi} = 0, \quad or \quad \frac{dp}{d\pi} = -\left(\frac{\partial F}{\partial p}\right)^{-1}\frac{\partial F}{\partial \pi}. \tag{2.8}$$

$\frac{\partial F}{\partial p} \leqslant 0$ is the second-order optimality condition for the consumer problem (6). Thus the sign of $\frac{dp}{d\pi}$ is the same as the sign of

$$\frac{\partial F}{\partial \pi} = m'(1-p)(D'-L') - (D-L) = \frac{m'D'}{1-\pi} \geqslant 0, \tag{2.9}$$

where the second equality is obtained using the first-order optimality condition (7). Therefore, the optimal risk level $p(\pi)$ does not decrease in background mortality risk π.

This result is similar to the one reported by Liu and Rettenmaier (2007) who provide a theoretical analysis of exogenous mortality risk effects on risk-taking behavior and find that people who face higher mortality risk are more likely to engage in risky activities.

While this model implies that background mortality risk decreases the WTP for mortality risk reduction, the same results do not necessarily hold for non-deadly health risks. If both p and π are morbidity risks, the effect of comorbidity on the

WTP for risk reduction depends on the assumptions about the effect of two health conditions on the individual health level. In fact, Bleichrodt et al. (2003) show that the willingness to pay for health improvements *increases* with the severity and probability of occurrence of comorbidities in case where the two diseases are additive in their effects or reinforce each other. Intuitively, a person in ill health values her remaining health more (diminishing marginal utility), so they are willing to pay more for health improvements. However, it is straightforward to show that if background risk π is a mortality risk, then the MWTP for non-deadly health risk reduction still decreases in π (details can be found in the Appendix).

2.4 Data and Empirical Specification

2.4.1 Data

To investigate the effect of expected longevity on residential choice and environmental risks exposure, I combine data drawn from the Health and Retirement Study (HRS) with information obtained from the Toxic Release Inventory (TRI) database, constructing a set of variables characterizing the level of toxic chemical releases and other waste management activities near each respondent's home. Information about the residence location for HRS respondents was obtained from the restricted HRS Cross-Wave Geographic Information dataset. This section provides a basic description of these data.

The University of Michigan Health and Retirement Study (HRS)[2] is a longitudinal panel study that surveys a representative sample of more than 26,000 Americans over the age of 50 every two years. This survey collects information about income, work, assets, pension plans, health insurance, disability, physical health and functioning. Most importantly for this study, it reports the respondents' subjective longevity

[2] The HRS is sponsored by the National Institute on Aging (grant number NIA U01AG009740) and is conducted by the University of Michigan.

expectations.

Since publicly available HRS datasets provide only aggregated information on some of the variables such as geographic location, I use the restricted HRS Cross-Wave Geographic Information dataset[3] to obtain more detailed information. In this data set, geographic identifiers reflect residence of respondents at the census tract level. Census tracts are small, relatively permanent statistical subdivisions of a county. Census tracts have 4,000 residents on average, and are designed to be homogeneous with respect to population characteristics, economic status, and living conditions. For one of the robustness checks, I also use the infomation about the respondents' place of birth (reported at the state level).

The Toxics Release Inventory (TRI) is a database containing information on toxic chemical releases and other waste management activities in the United States. The database is publicly available from the Environmental Protection Agency (EPA) and contains information reported annually. Each year since 1987, companies that produce or handle more than a specified amount of a listed toxic chemical must report it to the TRI. Over the years, there have been multiple changes in this database, including changes in reporting thresholds, in the list of chemicals reported etc. In order to control for these changes, all regression specifications include year fixed effects.

To construct the measures of TRI exposure, for each census tract centroid, I count the number of TRI facilities within 1 mi and 3 mi. This approach is preferred to counting the number of facilities that fall within each tract, since tract areas and shapes vary greatly depending on the population density and its spatial distribution.

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[3] HRS Cross-Wave Geographic Information dataset is one of the restricted datasets that are available to researchers from the HRS under specific contractual conditions. The details can be found on the HRS website (http://hrsonline.isr.umich.edu/index.php?p=reslis).

I am grateful to Frank A. Sloan for providing invaluable support and guidance during the process of obtaining the permission to use the restricted Cross-Wave Geographic Information dataset for this project.

A circle with 1 mile radius is larger in area than 54% of all tracts, and circle with 3 mile radius is larger in area than 79% of all tracts (tract areas were population-weighted). In order to take into account the heterogeneity among TRI facilities, I also calculate toxicity-weighted amounts of all emissions in the TRI facilities located within 1 mi and 3 mi from each tract centroid, based on weights developed by the EPA and available in its Risk Screen Environmental Indicators model (RSEI). In the main specification, I use a dummy variable indicating the presence of TRI facilities within 1 mile from the tract centroid.

2.4.2 Empirical specification

The main estimation sample consists of all HRS respondents for whom the residence location is known, and all the variables included in the analysis are non-missing. I exclude the respondents who live in a nursing home at the time of the interview, since their residence choice might be constrained.

The dependent variable in the baseline specification is a dummy equal to one if there are any TRI facilities within 1 mile from the centroid of the tract the respondent resides. As a robustness check, I include specifications where the dependent variable measures the number of TRI facilities or the amount of toxicity weighted chemical releases within 1 mile from the tract centroid. Since the last two variables are highly skewed, I use their logarithms[4].

To measure the expected longevity, I use the respondent's assessment of the probability that he/she will live to be 75 or more[5]. There is sizable literature that evaluates the the subjective probabilities of survival in the HRS, documenting both the

[4] The number of TRI facilities and the amount of toxic releases are equal to zero for locations that do not have any TRI facilities nearby, so I add 1 to their values before taking the logarithms.

[5] Other expected longevity questions in the HRS elicited the respondent's subjective probability of surviving to age 80/85/90/95/100. The set of longevity expectation questions varies across survey waves and depends on the respondent's age. I use some of these additional longevity expectations variables in one of the robustness checks and find that the results remain qualitatively unchanged

features of these variables that make them useful in the economic analysis, and their drawbacks. It was found that the subjective survival probabilities measure mortality risk fairly reasonably: they aggregate well to averages that are close to their life table counterparts, covary plausible with the determinants of actual mortality outcomes, and predict mortality reasonably well (Hurd and McGarry 1995, Hurd and McGarry 2002, Smith et al. 2001). At the same time, the subjective survival probabilities seem to suffer from the presence of measurement error and heaping of responses(Gan et al. 2005): many respondents systematically provided focal-point answers (0, 0.5 or 1), presumably due to uncertainty about their chances of survival or cognition problems. To correct for measurement error, I use the estimation strategy proposed by Bloom et al. (2007) and Fang et al. (2007) who instrument subjective survival probabilities using information on current age, or age at death, of the respondent's parents. Bloom et al. (2007) demonstrate that the instrumented survival probability performs substantially better than the reported survival probability as a predictor of long-term mortality.

In addition to measurement error, the IV estimator also addresses the potential endogeneity of subjective longevity expectations, a problem that would arise if these expectations were affected by individual's residence location decisions or correlated with unobservable characteristics. There are several reasons why the expected longevity might be endogenous in the equation reflecting the relationship between expected longevity and proximity to TRI facilities. First, there is reversed causality: individuals who live near toxins-emitting facilities may report lower longevity expectations, because they realize that exposure to pollution had affected their health adversely. Second, a decision to avoid pollution might be correlated with other investments in individual's health: since the same set of characteristics determines both the individual's exposure to pollution and the level of other health investments, it follows that people who choose to live near pollution sources might be more likely to care less about their health (as an indication of this, they are likely to consume less preventive care, more likely to smoke or have unhealthy diets); as a result, their expected lifespan is shorter. Using parental longevity as an instrument for expected longevity addresses the endogeneity problem that arises due to the reasons discussed above, because parental longevity proxies for genetic or hereditary components of the person's health, which is not correlated with the direct effect of pollution and unhealthy behaviors on health. At the same time, poor health endowment may affect residential choice through other channels, in particular, poor health may lead to lower earnings and wealth, which, in turn, may affect the willingness to pay for clean environment. However, it can be argued that this problem is resolved by including detailed controls for health, income, and wealth in the regression specification. Moreover, I find that the results are not particularly sensitive to changes in specifications involving these controls. The robustness of the estimates to health and additional wealth measures suggest that the correlation between longevity expectations and pollution exposure is unlikely to be driven by unobserved differences in income and wealth.

Control variables include the respondents' age, race, ethnicity, gender, marital status, education, parents' education, house value, and household income and net wealth (the last three variables are in logarithms, since they are skewed), and health indicators. The set of health indicators include: self-assessed health (rated from 1 for excellent health to 5 for poor), the number of limitations in activities in daily living (ADLs)[6], and binary indicators for whether the respondents was ever diagnosed with hypertension, diabetes, cancer, lung disease, heart problems, or mental health problems[7]. The preferred specification include county fixed effects that control for

[6] The ADLs include walking across a room, dressing, bathing, eating, getting in and out of bed, and using the toilet.

[7] More specifically, the respondents were asked to indicate whether or not a doctor has ever told the respondent he/she had the following conditions: 1) high blood pressure or hypertension; 2) diabetes

unobserved differences between locations such as access to labor markets; however, the estimated coefficients on longevity expectations are similar without them.

Summary statistics for the main estimation sample are reported in Table 2.1. The sample includes 50,136 observations, 10.3% of the sample are black and 60.9% are female. The mean age of the respondents in the sample is 57.1 years, and the mean subjective probability of living to age 75 is equal to 66.9%. About 24% of the individuals in the sample reside in tracts located within 1 mile from a TRI facility, and about 65% - in tracts within 3 miles.

2.5 Results

2.5.1 Main Findings

Table 3.6 reports the main IV results using parents' age and vital status as an instrument for life expectancy. I find strong evidence that those who report lower subjective probability of survival are more likely to live near TRI facilities. The results imply that, ceteris paribus, a 10-percentage point[8] increase in subjective probability of living to age 75 reduces the probability of residing within 1 mile from a TRI facility by 2.5 to 2.9 percentage points. As 23.6% of the respondents in the sample reside within 1 mile from a TRI facility, this change corresponds to about 10 to 12% decrease. The effect of the longevity expectations is fairly profound as compared to other determinants of the TRI exposure: a 1-percentage point increase in subjective probability of living to age 75 reduces the probability of residing within 1 mile from a TRI facility by about 0.25 percentage points - almost the same as the

or high blood sugar; 3) cancer or a malignant tumor of any kind except skin cancer; 4) chronic lung disease except asthma such as chronic bronchitis or emphysema; 5) heart attack, coronary heart disease, angina, congestive heart failure, or other heart problems; 6) stroke or transient ischemic attack (TIA); 7) emotional, nervous, or psychiatric problems

[8] As can be seen from the table of summary statistics (Table 2.1), the mean subjective probability of living to age 75 is equal to .669 with standard deviation equal to .273, so 10-percentage point change corresponds to less than 1/2 of one standard deviation.

effect of one additional year of education (which is equal to 0.3 percentage points); and the effect of 40 to 50-percentage point decrease in that probability is comparable to the effect of being black (about 11.2 to 14.5 percentage points).

Table 2.3 reports the first stage results from the two stage least squares. The dependent variable is expected probability of living to age 75 (scaled from 1 to 100 in this table only). Parents' ages (current or at death) and their vital status have large and significant effects on the longevity expectations. For example, respondents whose father (mother) is alive report the probability of living to age 75 about 10 (12) percentage points higher than those whose parents are deceased, ceteris paribus. The estimated coefficients on most control variables have the expected sign: respondents who are wealthier, more educated, or female, report higher subjective expected longevity, and respondents with any health problems expect to live less.

In the OLS results (reported in Table 2.4) the coefficients on longevity expectations have the same sign but are much smaller in magnitude than in the IV results (Fang et al. (2007) report similar relationship between their IV and OLS estimates for smoking when using the same instrumental variables). This comparison suggests significant attenuation bias in the OLS results due to a measurement error in the expected longevity variable.

2.5.2 Robustness Checks and Discussion

This subsection shows that the main results about the effect of longevity expectations on the TRI exposure are robust to a number of alternative data-coding choices and specification changes. It also considers a few issues of potential concern regarding the interpretation of the findings.

More wealth measures. One possible concern is that the observed correlation between longevity expectations and pollution exposure might be driven by unobserved differences in income and wealth: since people with poor health endowment (proxied by parental longevity) might earn lower wages, work less due to health limitations, and accumulate more wealth (Smith 1999), they might be unable to afford clean environment. However, I find that the longevity expectations affect the residential choice even controlling for income and wealth in the main specification. In order to control for additional dimensions of wealth, I include several wealth variables that are not closely correlated, such as housing wealth, financial non-housing wealth, value of vehicles, amount of non-housing debt. The results are reported in Table 2.6. Comparing column 1 (no wealth controls) and column 2 (main specification), I find that the estimated effect of the longevity expectations on the TRI exposure decreases in magnitude but still remains significant once wealth is included in the regression. However, once total wealth is controlled for, adding more wealth measures (column 3) has little effect on the estimated coefficients. The robustness of the estimates to wealth measures suggest that the correlation between longevity expectations and pollution exposure is unlikely to be driven by unobserved differences in income and wealth.

Results by wave. Another reason for concern is changing reporting requirements in the TRI data. In order to make sure that these changes do not drive the results, I repeat the analysis for each wave of data separately and find that the coefficient on longevity expectations has the same sign as in the pooled data regressions, thought the coefficients vary across waves (Table 2.7).

Selected Subsamples. Yet another possible concern about these results is that parental longevity might not be a valid instrument for the longevity expectations of those survey participants who reside in the same area they grew up: in this case, both participant's and parental longevity might be affected by TRI emissions. Thus, I repeat the analysis for a subsample of the respondents who no longer live in the state they were born. The results are reported in Table 2.10, column (1). For this subsample, the results are qualitatively similar to the main ones: a 10-percentage point increase in subjective probability of living to age 75 reduces the probability of residing within 1 mile from a TRI facility by 4.3 percentage points vs. 2.6 percentage points for the entire sample.

While the main sample excludes nursing home residence, it still might include people who live with their children or other persons who take care of the respondent. For certain elderly respondents who cannot live independently, the residence location might be determined by their caregiver. In order to address this concern, I repeat the analysis for a subsample of households that consist only of the respondents and their spouses and include no children or other household members, and find that the results are qualitatively similar to the main ones (Table 2.10, column 2).

Longevity expectations at older age. The main results are based on a sample of respondents that were asked about their subjective probability to live to age 75. Singificant share of the HRS respondents were not asked about living to age 75, since they were close to or past that age at the time of the interview. Instead, older HRS respondents were asked about their subjective probability to live to age 80, 85, 90, 95, or 100, depending on their current age. I find that the results are qualitatively unchanged if I repeat the analysis for that older HRS subsample using subjective probability of living to age 80/85/90/95/100 divided by the life table probability of survival to that age (Table 2.9).

Other measures of TRI exposure. In the baseline specification (Table 3.6), the dependent variable is a dummy indicating the presence of any TRI facilities within 1 mile from the tract centroid. While this specification allows for easy interpretation, it can mask important differences among exposed locations, as the number of TRI facilities near any given location and the amount of toxic releases vary significantly. As can be seen from Panel A of Table 2.8, the results are robust to the choice of TRI exposure measures: ceteris paribus, a 10-percentage point increase in subjective probability of living to age 75 reduces the count of TRI facilities within 1 mile from the respondent's tract of residence by 2.8 percentage points, and the amount of the toxicity-weighted releases by 46.7 percentage points. This change corresponds to a decrease approximately equal to 12 to 14% of the mean value of the respective variable.

Residential Choice vs. Environmental Justice. A natural question is whether the association between expected longevity and exposure to environmental risks documented is driven by individual residential choices or is instead induced by siting decisions of polluting facilities. In particular, the observed correlation can be interpreted as an evidence of discrimination or other injustice (just as greater exposure of minority communities to environmental hazards is often attributed to discrimination in the environmental justice literature[9]).

Two observations cast some doubt on the importance of discriminatory siting practices in explaining the correlation in the presence of TRI facilities and populations with lower expected longevity. First, there is evidence that discrimination against certain groups and communities is less common that it may appear from the early environmental justice documents, such as Bullard (1990). The literature offers and finds empirical support for several alternative explanations for why exposure to environmental risks may vary, including differences in willingness to pay for environmental amenities and variations in the propensity of communities to engage in collective action (Hamilton 1995, Noonan 2008, Banzhaf and Walsh (2008)). Second, longevity expectations are unobservable, and it is much harder to discriminate based on unobservable characteristics.

Wind direction. Since a significant share of TRI releases are airborne, wind patterns could cause significant heterogeneity among TRI proximate communities,

[9] There is large literature that examines the association between local environment quality and socioeconomic and demographic characteristics of the residents. The environmental justice movement asserts that minority and poor communities face greater exposure to pollution than white households. Bullard (1990), UCC (1987) document the beginnings of the environmental justice movement, Bowen (2002) and Brulle and Pellow (2006) provide reviews of the recent developments.

with downwind locations having worse air quality than upwind locations within the same distance from a polluting facility. Thus, pollution exposure variables based on proximity to TRI facilities would measure the actual environmental risk with error. It is unlikely, however, that lack of information on wind direction could significantly bias the results. The exposure variables are used as dependent variables in my analysis, and measurement error in dependent variable results in less precise estimates, not bias. Also, while economists rarely take wind patterns into account, there is some evidence that they are unlikely to change the results: e.g. Davis (2011) uses wind direction information for one of the validity tests when he studies the effect of power plants on local housing values and rents. He finds "no evidence of a disproportionate impact on homes downwind of plants".

2.6 Conclusion

This study has used data on residence location of HRS respondents to examine the effect of expected longevity on environmental risks valuation. Using the parental longevity as an instrument for individual longevity expectations, I find that individuals with shorter expected lifespan are more likely to reside near toxins-emitting facilities. Moreover, the effect of the longevity expectations is significant as compared to other determinants of environmental risk exposure identified in the literature, such as level of education, race, and wealth. The results are robust across a variety of alternative specifications.

The findings of this paper are consistent with the view that the willingness to pay (WTP) for risk reduction might be lower among the older individuals because of the differences in life expectancy. By using elicited longevity expectations, this study provides direct evidence supporting this idea, while the previous literature focused on the effect of age rather than the remaining quantity of life.

The next step in this research project is to analyze the link between expected longevity and the WTP for environmental risk reduction in the property hedonics framework, examining the capitalization of local environmental quality indicators into housing prices. This extension will allow me to obtain monetary estimates of the WTP and quantify the effect of the differences in expected longevity on the value of environmental amenities. In particular, I can test whether shorter life expectancy decreases the WTP for clean environment.

2.7 Appendix: WTP for morbidity risks

This section shows that if the background risk π is a mortality risk, then the MWTP for non-deadly health risk reduction still decreases in π.

Suppose an individual faces a mortality risk with probability π and an independent non-deadly health risk with probability p, so that the possible states of the world include death, survival in bad health, and survival in good health. The individual's utility is denoted by $D(w)$, $B(w)$, and $G(w)$ if he dies, survives in bad health, and survives in good health, respectively. It is assumed that $D(w) < B(w) < G(w)$ (utility increases in health, and survival is still preferred to death), and that $D'(w) < B'(w) < G'(w)$.

The expected utility is

$$EU(p, \pi) = \pi D(w) + (1-\pi)pB(w) + (1-\pi)(1-p)G(w). \tag{2.10}$$

The MWTP for reduction in health risk p is

$$WTP(\pi) = \frac{dw}{dp} = \frac{(1-\pi)(G-B)}{\pi D' + (1-\pi)pB' + (1-\pi)(1-p)G'} = \frac{(1-\pi)(G-B)}{EU'(w)} \tag{2.11}$$

Differentiating this expression with respect to π yields

$$\frac{\partial WTP(\pi)}{\partial \pi} = -\frac{D'(G-B)}{(\pi D' + (1-\pi)pB' + (1-\pi)(1-p)G')^2} \leqslant 0 \tag{2.12}$$

Given the initial wealth w_0, the consumer's problem is to maximize the expected utility (10) s.t. $w = w_0 + m(p)$

The first-order optimality condition is

$$F = m'(p)[\pi D'(w) + (1-\pi)pB'(w) + (1-\pi)(1-p)G'(w)] + (1-\pi)(B(w) - G(w)) = 0. \tag{2.13}$$

The solution to this equation yields the optimal risk level $p = p(\pi, w_0)$.

Differentiating the FOC with respect to π , we obtain

$$\frac{dp}{d\pi} = -\left(\frac{\partial F}{\partial p}\right)^{-1} \frac{\partial F}{\partial \pi}. \tag{2.14}$$

$$\begin{aligned}\frac{\partial F}{\partial p} &= 2(1-\pi)(B' - G')m' + (m')^2[\pi D'' + (1-\pi)pB'' + (1-\pi)(1-p)G''] \\ &+ m''[\pi D' + (1-\pi)pB' + (1-\pi)(1-p)G']\end{aligned} \tag{2.15}$$

$\frac{\partial F}{\partial p} \leqslant 0$ is in fact the second-order optimality condition for the consumer problem. Under the assumptions made, this condition holds if the market wealth-risk trade-off is not too convex (that is, if $m''(p) < 0$, or $m''(p)$ is positive but small).

$$\frac{\partial F}{\partial \pi} = G - B - m'[-D' + pB' + (1-p)G'] = \frac{m'D'}{1-\pi} \geqslant 0, \tag{2.16}$$

where the second equality is obtained using the first-order optimality condition.

As the background mortality risk π increases, the individual will take more of the non-fatal health risk p.

Table 2.1: Summary statistics.

Variable	Mean	Std. Dev.
Black	0.103	0.304
Asian	0.036	0.186
Hispanic	0.067	0.249
Female	0.609	0.488
Married	0.807	0.394
Education years	13.133	2.755
Mother's education, years	10.111	3.457
Father's education, years	9.773	3.865
Age	57.087	5.314
Self-assessed health	2.462	1.074
ADLs count	0.124	0.525
Subjective probability to live 75+	0.669	0.273
High blood pressure	0.366	0.482
Diabetis	0.106	0.307
Cancer	0.069	0.254
Lung desease	0.053	0.225
Heart problems	0.116	0.320
Stroke	0.026	0.158
Mental health problems	0.105	0.307
Smoker	0.187	0.390
Net household wealth (ln)	11.928	2.031
Total household income (ln)	10.761	1.292
Respondent works	0.663	0.473
House value (ln)	11.648	0.921
Dad's age (current or at death)	71.596	13.261
Mom's age (current or at death)	75.162	12.150
Father alive	0.188	0.391
Mother alive	0.414	0.493
TRI facilities within 1mi	0.236	0.424
TRI facilities within 3mi	0.628	0.483
Facilities count within 1mi (ln)	0.235	0.467
Facilities count within 3mi (ln)	1.019	1.001
Tox-weighted releases within 1mi (ln)	3.378	6.998
Tox-weighted releases within 3mi (ln)	11.018	9.960
Number of observations	50,136	

Table 2.2: Longevity expectations and TRI exposure, IV results.

	Dep var: dummy for TRI facilities within 1mi			
	1	2	3	4
Subj. prob. to live 75+	-0.269***	-0.291***	-0.249***	-0.261***
	(0.104)	(0.108)	(0.088)	(0.092)
Asian	0.017	0.017	0.013	0.013
	(0.02)	(0.02)	(0.019)	(0.019)
Black	0.144***	0.145***	0.112***	0.112***
	(0.016)	(0.016)	(0.015)	(0.016)
Hispanic	0.052***	0.048***	0.086***	0.084***
	(0.017	(0.017)	(0.019)	(0.019)
Married	-0.015	-0.015	-0.001	-0.001
	(0.009	(0.009)	(0.008)	(0.009)
Education years	-0.006***	-0.005***	-0.003**	-0.003**
	(0.002)	(0.002)	(0.002)	(0.002)
Mother's education	-0.001	-0.001	0.000	0.000
	(0.002)	(0.002)	(0.001)	(0.001)
Father's education	0.000	0.001	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Income	0.008***	0.008***	0.004**	0.004**
	(0.002)	(0.002)	(0.002)	(0.002)
Net hh wealth (ln)	0.001	-0.001	0.000	0.000
	(0.002)	(0.002)	(0.001)	(0.001)
House value	-0.014***	-0.014***	-0.039***	-0.039***
	(0.005)	(0.005)	(0.004)	(0.005)
Age	0.002***	0.003***	0.002***	0.002***
	(0.001)	(0.001)	(0.001)	(0.001)
Female	0.018**	0.018**	0.017**	0.017**
	(0.009)	(0.009)	(0.007)	(0.008)
County FE	No	No	Yes	Yes
Health conditions	No	Yes	No	Yes
N	50,136	50,014	50,136	50,014
J-stat	7.41	7.54	4.38	3.84
J-test p-value	0.1919	0.1835	0.4965	0.5724

* significant at 10%; ** significant at 5%; ***significant at 1%.
Standard errors are reported in parentheses. Wave dummies and a constant are included in all specifications. Standard errors are clustered at the respondent level.

Table 2.3: First stage: Individual's subjective longevity expectations.

	Dep var: Subj. prob. to live 75+			
	1		2	
	Coeff	St.Error	Coeff	St.Error
Mom's age (current or at death)	0.151***	0.017	0.148***	0.017
Dad's age (current or at death)	0.144***	0.015	0.139***	0.015
Mother alive	11.709***	3.663	12.447***	3.659
Father alive	10.005**	4.48	10.474**	4.474
Mother's age if alive	-0.111**	0.044	-0.121***	0.044
Father's age if alive	-0.097*	0.054	-0.104*	0.054
Asian	-0.297	1.041	-0.439	1.049
Black	7.283***	0.651	6.989***	0.656
Hispanic	-2.095**	0.859	-2.774***	0.87
Married	.101	0.449	0.121	0.447
Education years	0.446***	0.081	0.453***	0.081
Mother's education	0.278***	0.072	0.283***	0.072
Father's education	0.046	0.062	0.040	0.061
Income	0.215*	0.119	0.216*	0.12
Net hh wealth (ln)	0.01	0.088	-0.019	0.088
House value	1.905***	0.228	1.848***	0.227
Age	0.438***	0.038	0.486***	0.039
Female	4.594***	0.377	4.484***	0.38
Self-assessed health	-7.442***	0.178	-6.740***	0.185
ADLs count	-2.399***	0.355	-2.132***	0.359
High blood pressure			-0.261	0.368
Diabetis			-1.756***	0.633
Cancer			-2.782***	0.689
Lung desease			-4.400***	0.865
Heart problems			-3.263***	0.59
Stroke			-2.386	1.147
Mental health problems			-2.386**	0.612
Const	-2.485	3.912	-1.276	
N	50,136		50,014	
R^2	0.17		0.174	
F-stat for excluded instruments	61.16		56.68	

* significant at 10%; ** significant at 5%; ***significant at 1%.
Wave dummies and a constant are included in all specifications. Standard errors are clustered at the respondent level.

Table 2.4: OLS vs IV.

Dep var: dummy for TRI facilities within 1mi		
	OLS	IV
Subj. prob. to live 75+	-0.01	-0.261***
	0.010	0.092
Asian	0.018	0.013
	0.019	0.019
Black	0.096***	0.112***
	0.014	0.016
Hispanic	0.095***	0.084***
	0.018	0.019
Married	-0.003	-0.001
	0.008	0.009
Education years	-0.005***	-0.003**
	0.001	0.002
Mother's education	-0.001	0.000
	0.001	0.001
Father's education	-0.001	-0.001
	0.001	0.001
Income	0.004*	0.004**
	0.002	0.002
Net hh wealth (ln)	0.000	0.000
	0.001	0.001
House value	-0.042***	-0.039***
	0.004	0.005
Age	0.001**	0.002***
	0.001	0.001
Female	0.006	0.017**
	0.007	0.008
Self-assessed health	0.003	-0.014**
	0.003	0.007
ADLs count	-0.009*	-0.014***
	0.005	0.005
County FE	Yes	Yes
Health conditions	Yes	Yes
N	50,014	

* significant at 10%; ** significant at 5%; ***significant at 1%.
Standard errors are reported in parentheses. Wave and county dummies and a constant are included in all specifications. Standard errors are clustered at the respondent level.

Table 2.5: Comparison with smoking.

	Smoking	TRI exposure
Subj. prob. to live 75+	-0.376***	-0.261***
Black	0.001	0.013
Asian	-0.002	0.112***
Hispanic	-0.125***	0.084***
Married	-0.077***	-0.001
Education years	-0.012***	-0.003**
Mother's education	0.002	0.000
Father's education	0.000	-0.001
Income	-0.004*	0.004**
Net hh wealth (ln)	-0.006***	0.000
House value (ln)	-0.018***	-0.039***
Age	-0.004***	0.002***
Female	-0.028***	0.017**
Self-assessed health	0.005	-0.014**
ADLs count	-0.031***	-0.014***
High blood pressure	-0.054***	-0.006
Diabetis	-0.058***	0.019*
Cancer	-0.030**	-0.019
Lung desease	0.111***	0.002
Heart problems	-0.039***	-0.007
Stroke	0.002	0.015
Mental health problems	0.041***	-0.004
Mean of the dependent var	0.187	0.236

* significant at 10%; ** significant at 5%; ***significant at 1%.
Wave and county dummies and a constant are included in all specifications. Standard errors are clustered at the respondent level.

Table 2.6: Robustness Check: More wealth measures.

	Dep var: dummy for TRI within 1mi		
	1	2	3
Subj. prob. to live 75+	-0.278***	-0.261***	-0.258***
	(0.091)	(0.091)	(0.091)
Income	-0.000	0.004**	0.005**
	(0.002)	(0.002)	(0.002)
Net hh wealth (ln)		0.000	0.001
		(0.001)	(0.001)
House value		-0.039***	-0.038***
		(0.004)	(0.005)
Vehicles value			-0.002*
			(0.001)
Non-housing debt			0.001*
			(0.001)
Housing wealth			0.001
			(0.001)
Financial (non-housing) wealth			0.000
			(0.001)
Capital income			-0.001*
			(0.001)
Respondent works			0.011*
			(0.006)
N	50014	50014	50008

* significant at 10%; ** significant at 5%; ***significant at 1%.
Standard errors are reported in parentheses. All specification include county dummies, year dummies, individual health conditions, and demographic characteristics. Standard errors are clustered at the respondent level.

Table 2.7: Robustness Check: Results by wave.

	Dep var: dummy for TRI within 1mi		
Year	Coeff	St. Error	N of obs
1992	-0.122	(0.105)	7,943
1994	-0.239*	(0.145)	6,607
1996	-0.305**	(0.154)	5,203
1998	-0.411***	(0.128)	6,407
2000	-0.419**	(0.163)	5,637
2002	-0.062	(0.156)	4,717
2004	-0.094	(0.144)	5,752
2006	-0.282*	(0.166)	4,267
2008	-0.252	(0.182)	3,481

* significant at 10%; ** significant at 5%; ***significant at 1%.
Standard errors are reported in parentheses and are clustered at the respondent level.

Table 2.8: Robustness Check: Alternative Exposure Measures.

	Dep var:		
Subj. prob to live 75+	TRI facilities dummy	Facilities count	Tox-weighted releases
	Panel A: Within 1mi		
Coeff.	-0.261***	-0.280***	-4.670***
St. Error	(0.092)	(0.104)	(1.530)
	Panel B: Within 3mi		
Coeff.	-0.152	-0.336*	-4.026**
St. Error	(0.095)	(0.191)	(1.928)

* significant at 10%; ** significant at 5%; ***significant at 1%.
Standard errors are reported in parentheses and are clustered at the respondent level.

Table 2.9: Robustness Check: Alternative Expected Longevity Variable.

	Dep var (within 1mi):		
Relative subj.prob of survival	TRI facilities dummy	Facilities count	Tox-weighted releases
Coeff.	-0.144*	-0.154*	-3.045**
St. Error	(0.077)	(0.080)	(1.223)
N	56,327		

* significant at 10%; ** significant at 5%; ***significant at 1%.
Standard errors are reported in parentheses and are clustered at the respondent level.

Table 2.10: Robustness Checks: Selected Subsamples.

	Dep var: dummy for TRI within 1mi	
	(1) Moved since childhood	(2) No children in the hh
Subj. prob. to live 75+	-0.433***	-0.179*
St. Error	(0.122)	(0.101)
N	25,963	30,491

* significant at 10%; ** significant at 5%; ***significant at 1%.
Standard errors are reported in parentheses and are clustered at the respondent level.

3

Persistence in Housing Wealth Perceptions: Evidence from the Census Data

Using of a unique combination of restricted-access decennial Census data and a proprietary data set containing the details of housing transactions from several U.S. metropolitan areas, we find evidence that owner-assessed home values "lag behind" market prices, and that this lag has the potential to substantially alter values ascribed to local amenities using property-value hedonic techniques. We hypothesize that long-standing homeowners lack an incentive to gather recent information on housing markets, and that their ignorance is reflected in the bias in their self-reported housing values. We first demonstrate that bias in self-reported home values is significantly correlated with tenure. We then examine how tenure length affects homeowners' valuation of changes in a particular local amenity – exposure to sites on the EPA's National Priorities List (i.e., the "Superfund" program). We find that recent movers report a value of a site's deletion from that list (signaling the end of the Superfund remediation process) that is 30-50% greater than that expressed by long-standing owners. This difference is significant and can have important implications for the results of cost-benefit analysis.

3.1 Introduction

A large literature in both economics and psychology has documented the tendency of people to adhere to prior beliefs and choices, either because learning and processing new information takes time, or out of a reluctance to accept new information that contradicts previously held beliefs (Lord et al., 1979; Barberis and Thaler, 2003). In macroeconomic models, learning (or the lack thereof) can represent an important source of persistence in the economy (Milani 2007). Behavioral finance studies have also found that investors adhere to prior beliefs in spite of new information (Barberis et al. 1998). Similarly, uncertainty or an unwillingness to consider new information can cause persistence in housing wealth perceptions. However, most empirical housing market models abstract from uncertainty (e.g. Glaeser and Gyourko (2006) present a dynamic, rational expectations model of house price formation). Only a few recent studies demonstrate that home sellers rely on prior information when they determine their list prices, and that this behavior can explain some of the puzzling features of the housing market, such as the persistence of sales price appreciation rates in the short-run and positive price-volume correlation (Genesove and Mayer, 2001; Anenberg, 2010). However, both of these studies draw their conclusions from the behavior of *home sellers* who can differ from *home owners*. In particular, sellers can be more knowledgeable about the housing market, as they may have undertaken substantial research of housing market conditions before listing their homes. Also, most home sellers use the services of realtors, whose job is to provide their clients with information about the current state of the housing market[1].

In this paper, we test whether homeowners - particularly long-standing home-

[1] Several studies suggest that real estate agents might have an incentive to convince their clients to to sell their houses too cheaply and too quickly. Levitt and Syverson (2008) and Rutherford et al. (2005) find that agents list and sell their own homes for a higher price than their clients' properties. On the other hand, Hendel et al. (2009) find that using realtor services delivers no price premium to sellers compared to For Sale By Owner platform.

owners - systematically rely on outdated information when they assess their home values. In contrast to previous studies, we examine housing wealth perceptions of all homeowners, not only those currently exposed to housing market. It is precisely those homeowners not exposed to the housing market for whom we expect the failure to include recent information to be the most severe.

Indeed, we find evidence that owner-assessed home values "lag behind" market prices, suggesting that homeowners rely on outdated information. Specifically, the difference between market prices and the owner-reported values are larger if market conditions are changing quickly (i.e., prices are growing or falling rapidly). This has important implications for the use of homeowners' stated housing values to estimate the value of local public goods and amenities - recent movers are likely to be more informed about current state of neighborhood amenities and home prices. Long-standing owners, on the other hand, do not have many incentives to exert effort to learn about the neighborhood changes; they are subsequently more likely to rely on outdated information. With this as a backdrop, we examine the differential impact of Superfund remediation actions on owner-assessed housing values and find that the effect of site proposal and deletion on home values reported by long-standing owners have the same sign and smaller magnitude than on home values reported by recent movers.

It is worth noting that we use the restricted-access decennial Census data as our main data source. By doing so, our paper is tied to a vast empirical literature that exploits home values from the Census to uncover people's preferences for a wide range of spatially delineated non-market goods, such as school quality, crime, and clean environment[2]. Our results suggest that there is a delay in capitalization

[2] Examples of applied research that use Census house values include measuring the value of environmental amenities (Greenstone and Gallagher 2008, Gamper-Rabindran et al. 2011, Bayer et al. 2009), explaining urban dynamics (Glaeser and Gyourko 2005), testing the Tiebout model predictions (Epple and Sieg 1999, Banzhaf and Walsh 2008), measuring quality of life (Blomquist

of changes in local amenities. In the hedonic literature seeking to estimate the willingness to pay for spatially delineated non-market goods such as school quality or a clean environment, price evolution is often ignored due to data limitations. However, several recent studies document delays in the capitalization of amenities into house prices: Cellini et al. (2010) for school bond passage, Case et al. (2006) for the revelation of groundwater contamination. Lang (2010) examines the dynamics of air quality capitalization in house values in the American Housing Survey (AHS) data and finds that marginal willingness to pay for reduction in particulate matter quadruples from two to ten years.

This paper also contributes to the literature on the accuracy of owner-reported home values. A number of studies have examined the accuracy of self-reported housing wealth by comparing those values with sales prices using data from the American Housing Survey (e.g. Kiel and Zabel 1999 and references therein) or the Health and Retirement Survey (Benítez-Silva et al. 2008). Agarwal (2007) use a proprietary dataset on HELOCs issued to owner-occupants to compare owner-assessed house values with lender-appraised home values[3]. While the above studies provide important information about the accuracy of the owner-assessed house values, characterize the distribution of the bias and document its correlation (or lack of thereof) with various household and neighborhood characteristics, very few of them provide a rationale for differences between transaction prices and homeowners' valuations. The authors of several studies hypothesize that the accuracy of homeowners' estimates can be related to the housing and business cycles, but the empirical evidence is mostly mixed or not statistically significant (DiPasquale and Somerville 1995, Kiel

et al. 1988), explaining racial segregation (Bajari and Kahn 2005), examining the determinants of homeownership (Painter 2000), and measuring preferences for schools and neighborhoods (Bayer et al. 2007).

[3] It might be debatable whether the appraised home values reflect market home values accurately, as home appraisals can sometimes be manipulated by mortgage industry players; see, e.g. Ashcraft and Schuermann (2008).

and Zabel 1999).

The rest of the paper is organized as follows: Section 2 presents the data. Section 3 reports the results of our analysis of the determinants of homeowner bias, and Section 4 describes how these biases affect the results of hedonic exercise to value the effects of proposal to, listing on, or deletion from the National Priorities List under the EPA's Superfund program. Section 5 concludes.

3.2 Data

Our analysis relies on two large data sets: the restricted use Decennial Census and information about real estate transactions collected by DataQuick Inc.

3.2.1 Restricted-Access Census data

The primary dataset used in this analysis is drawn from the restricted-access version of the 1990 and 2000 Decennial Censuses. These data provide rich information on the demographic and socioeconomic characteristics of all households who filled out the long-form questionnaire (approximately 15 percent of the entire U.S. population). For each household member, the Census reports age, race, education, employment status, and income. In addition, the Census publishes basic housing unit characteristics, such as the age of the building, number of rooms and bedrooms. House values are self-reported.

Housing locations are specified at the level of the census block. Census blocks are the smallest geographic area for which the Bureau of the Census collects and tabulates decennial census data. They are typically formed by streets, roads, streams, and other geographic features. In cities, a census block often corresponds to a city block.

3.2.2 DataQuick Real Estate Transactions Data

Purchased from a real estate data aggregator, DataQuick Inc., these data provide actual transaction prices and include detailed information about housing characteristics. Transaction variables include a unique property identifier, sales price, transaction date, and location information (census tract and block group, latitude, longitude). Geographically, our final sample includes housing units located in select metro areas from seven states[4] for which we have transactions data recorded back into year 1990. Using the ESRI ArcGIS software, we assigned census block identifiers to all observations in the sample based on latitude and longitude. We dropped properties for which latitude and longitude were missing or miscoded (i.e., block group information obtained based on the house latitude and longitude differed from the block group reported by DataQuick).

DataQuick also provides detailed housing characteristics: number of bedrooms, number of rooms, year when the house was built, number of baths, lot size, square footage, and more. Unfortunately, these characteristics are only recorded at the time of the most recent recorded transaction. To overcome this data limitation, we dropped housing units that have likely undergone major improvements. To this end, we performed several cuts to the sample. First, we drop all housing units that sold more than four times during the period from 1988 to 2001. These houses are less likely to be representative of typical residential housing transactions as they may be bought for investment purposes or "flipped". For similar reasons, we drop properties that sold multiple times in the same year. Next, we screen properties for land-only sales or rebuilds and drop all transactions for which the year built is missing or the transaction date is prior to the year built, and properties with loan amounts greater than transaction amounts. Finally, we drop all properties that appreciated by more

[4] We use data from 27 counties located in the following states: AZ, MA, NC, NV, OR, RI, WA

than 50% or depreciated by more than by 50% on an annualized basis.

DataQuick housing attributes were re-coded to match the format of the corresponding variables in the census data: (i) the year when house was built was re-coded in 9 categories, (ii) the lot size was re-coded in 3 categories, (iii) the number of rooms was top-coded at 9, and (iv) the number of bedrooms was top-coded at 5. We only use transactions that occurred close to the Census date (1989-1990 and 1999-2000, respectively) to estimate the model.

3.3 Homeowners' Bias and Its Determinants

In this section we examine the difference between homeowner-assessed values and market values at the housing unit level. In particular, we test whether homeowner-reported house values "lag behind" market prices. If homeowners do not fully incorporate recent housing market movements into their response, we will observe that they understate their home values when prices are rising, and overstate them when prices are falling; thus, we expect the difference between homeowner-assessed values and market values to be negatively correlated with the appreciation rate. In addition, we seek to identify the neighborhood characteristics that are correlated with the homeowner's bias.

3.3.1 Empirical Strategy

The ideal data set for this analysis would contain both owner-reported home values and corresponding home market values as assessed at the time of the survey. Census data contains the former, but not the latter. Our empirical strategy overcomes this problem by augmenting the Census data with market-based home values constructed using the DataQuick transactions dataset.

The empirical strategy is simple and consists of two steps. In the first step, we use transactions data to estimate a prediction equation for home prices (details follow below). This equation uses only covariates that are also available in the Census data, so we are then able to use our coefficient estimates to impute the current market price for each housing unit in the Census sample.

In the second step, we calculate the homeowner's bias as the difference between her stated home value and corresponding market price prediction. We then examine the determinants of that bias by regressing it on select household and neighborhood characteristics, as well as local housing market indicators (in particular, housing appreciation rates). We show that - consistent with theoretical predictions - after controlling for house and neighborhood characteristics, homeowners' biases are negatively correlated with home appreciation rates. As we will see below, this sort of systematic bias in stated housing values has the potential to significantly bias property value hedonic estimates[5].

An important question is whether transaction price is indeed the most relevant measure of home value. One can argue that transaction prices are affected by the housing cycle: during a housing "bubble", home sales prices might be higher than "true" home values determined by fundamentals. Conversely, during a down market, foreclosed properties may be sold below their true market price. However, the wording of the Census question suggests that homeowners are expected to report the amount of money for which the house could be sold were it for sale. Thus, we examine the accuracy of the owner-reported house values as compared to the best available estimate of the house market value.

[5] There are a number of alternative potential imputation strategies that were considered but ultimately proved impractical. The most obvious candidate is to match census observations with transactions data based on the observable characteristics and year of move-in information. However, in most cases the set of the observed characteristics (census block, rooms, bedrooms, 9 categories of year built, 3 categories of lot size, and 6 categories of move-in date) does not allow for unique merge. Moreover, the procedure would only provide information on how much did a particular house cost at the time of purchase, not market values that always correspond to the time of the census. Finally, since we have transaction information going back only as far as 1988, use of this strategy would eliminate more than 90% of the houses in the 1990 census sample.

Market Price Prediction

Given the complexity of housing markets, it is clear that the limited set of observable housing characteristics available in the census might be insufficient to explain the variation of home prices across locations; therefore, the model explaining market prices also draws upon detailed location information. We employ a standard hedonic framework: the logarithm of the market price P is a function of a set of unit and neighborhood characteristics:

$$P_{itb} = X'_{it}\beta_1 + N'_{bt}\beta_2 + \alpha_b + \sum D_s\mu_s + \epsilon_i, \tag{3.1}$$

P_{itb} – (log) price,

X_{it} – house attributes,

N_{tb} – block characteristics (time-varying),

α_b – block fixed effect,

D_s – month of sale dummies

House attributes include variables common to both datasets – number of rooms, number of bedrooms, year that the home was built (tabulated in 9 categories), and lot size (tabulated in 3 categories).

Block characteristics include block socio-demographics (share Black, share Asian, share Hispanic, median household income, share of residents who are high school drop-outs, unemployment rate, and share of residents older than 65 or younger than 18), block housing characteristics (housing structure type – attached, detached, multifamily, number of rooms, bedrooms, and age for a median home; share of renters, share of recent movers, and share of vacant housing units), measures of local business activity (earnings), proximity to hazardous waste sites, and time-varying dummies for MSA or school district.

Explaining Homeowner Bias

In the second step, we examine the relationship between homeowners' biases and housing market conditions. More precisely, we calculate the difference between (log) owner-assessed home values and (log) market price estimates for those homes and regress it on county-level home appreciation rates within one year before the census date, and a set of controls including block demographic and socioeconomic characteristics and length of tenure:

$$lnV_{it} - \widehat{lnP_{it}} = \text{Appr}_{ct}\theta_1 + N'_{bt}\theta_2 + \text{Ten}_{it} + u_{it} \tag{3.2}$$

The choice of control variables is (at least partially) driven by the fact that we have to use predicted market price $\widehat{P}$ as a measure of home market value P. There are two main implications of observing home market values with an error, so the regression specification aims to address these issues.

First, the observed set of housing units in the transaction dataset is not a random draw from the entire stock of housing. It has been documented by housing market researchers (e.g. DiPasquale and Somerville (1995)) that recently purchased homes differ from the rest of the housing stock in many attributes, such as number of rooms, bedrooms, and proximity to the central business district. Also, house renovations are often completed just before a house is listed for sale or shortly after the sale takes place. Failure to take sample selection into account can cause the predicted home values to be biased (e.g., if homes that sell have more upgrades, then predicted home values will be biased upward). In order to control for unobserved differences between homes that have been purchased recently (and are similar in their characteristics to homes in the transaction data set) versus those that were purchased a long time ago, we include length of tenure in the regression specification.

Second, it is not straightforward to determine whether homeowners' misestimation is related to household characteristics, because correlation between prediction error and household characteristics can distort that inference [6] We overcome this difficulty by including census block demographic and socioeconomic characteristics in the regression specification. On the one hand, census block characteristics are quite informative about a block's residential demographics; on the other hand, block characteristics are not correlated with the house value prediction error, since those characteristics were included in the market price regression specification (i.e., OLS prediction error is not correlated with regressors).

3.3.2 Findings

This subsection reports our main results regarding the relationship of homeowners' bias with home appreciation rates at the time of the survey, as well as with the various location characteristics. The results are reported in Table 3.1

The main objects of interest are the estimates of the coefficient on home appreciation, which are reported in Rows 1-3. The second and fourth columns report estimates from a model that allows this coefficient to differ across census years (1990 and 2000), while the first and third columns report results from a specification that pools both 1990 and 2000 census data together. In all specifications, we find that the estimated coefficient on home appreciation is negative, which implies that homeowners understate their home values when homes appreciate, and vice versa. Using the results from the preferred specification presented in the final column, we find that a 1% increase in yearly home appreciation rate is associated with 0.79% increase in the difference between owner-reported home values and estimated market values in

[6] To see why using the predicted market value $\hat{P}$ instead of P can cause problems, consider the following example. Consider two observationally equivalent homes (that is, they are located on the same block, have the same number of rooms and bedrooms, built in the same period and have similar lot sizes). Suppose one of them is actually better in some unobserved dimension (e.g., has larger square footage). If the owners report their home values accurately, the stated value for the first home will be higher. But since our predicted market value estimates will be the same for both homes, we would conclude that the first owner is overstating her home's value, and vice versa. Given that home quality is likely correlated with income, we would then erroneously conclude then that high-income homeowners tend to overstate their home values.

2000, and with only 0.23% increase in 1990.

To put these findings into context, consider the following example: according to the Federal Housing Finance Agency (FHFA) data, in year 2000, the nationwide FHFA house price index increased by 6.93%, which correspond to average homeowner overestimating their home value by 5.48% (relative to the situation with zero home appreciation rates). However, some markets experienced much higher growth in housing prices during the same period: for example, in San Francisco-San Mateo-Redwood City MSAD, FHFA house price index increased 21.85% in 2000, which corresponds to increase in 17.26% in homeowner's bias.

Further, the existing literature provides some evidence suggesting that homeowner's uncertainty and biased beliefs may have significant welfare implications. Agarwal (2007) finds that homeowners who overestimate their home values by more than 10%, have a 14% higher risk of defaulting on their home equity loans. Anenberg (2010) finds that uncertainty on the housing market causes home sellers to make suboptimal listing decisions (in particular, sellers who put their homes on the market after a period where prices declined tend to overstate the value of their home) and estimates that seller would be willing to pay up to 7.33% of the home price for having full information about the distribution of buyer valuations for his home.

Table 3.2 reports the coefficients on census block demographic and socioeconomic characteristics. We find that homeowners who live in predominantly Asian or Hispanic neighborhoods, and neighborhoods with a high percentage of elderly, are more likely to understate their home values. Conversely, residents of blocks with many unoccupied housing units (offered for sale, intended for seasonal use, or vacant for other reasons) tend to overstate their home values. The magnitude of the observed associations is far from trivial: for example, a 1% increase in the share of Asians among block residents is associated with a 0.24% decline in the homeowners' bias; a 1% increase in share of homes that are vacant for other reasons (i.e., not for sale,

Table 3.1: Results: Bias and Appreciation Rate

	Appreciation Rate Effect			
Pooled 1990 and 2000	-0.313†		-0.306†	
1990 Census data		-0.195*		-0.231*
2000 Census data		-0.954***		-0.792***
Tenure Length Included	No	No	Yes	Yes

† significant at 10%; * significant at 5%; *** significant at 0.1%.
Block characteristics included but coefficients not shown. All estimates are based on a sample of $N = 503,667$ housing units.

rent, or intended for seasonal use) is associated with 0.34% overestimation of home values.

3.4 Stated Values and Amenities Valuation

In this section, we examine the process of home value capitalization associated with changes in localized environmental amenities. Importantly, homeowners who base their assessed values on lagged information are likely to ignore information in recent changes; as such, their assessed values may fail to fully reflect changes to local amenities. Given the results of the previous section, we examine how the effect of such changes varies with tenure length in particular.

The property value hedonic framework holds that the value of a house is determined by its component characteristics, including neighborhood amenities and disamenities.[7] However, *changes* in amenities are unlikely to be instantly reflected in house values as homeowners need time to acquire and process new information. Recent movers are likely to be more informed about the current state of neighborhood amenities and home prices, as potential home buyers usually research the neighborhood before making a big purchase. Long-standing owners, on the other hand, do not have an incentive to exert effort to learn about the changes in neighborhood attributes; they are, therefore, more likely to rely on outdated information.

It follows that the effect of changes in amenities on the stated home values re-

[7] See Palmquist (2005) and Taylor (2003) for a summary of the theory behind hedonic valuation and a thorough discussion of the hedonic literature.

Table 3.2: Results: Bias and Block Characteristics

Share of homes/households:	Coeff.	St.err.
Vacant, for rent	-0.124	(0.072)
Vacant, for sale	0.167*	(0.071)
Vacant, seasonal use	0.282*	(0.108)
Vacant, other reason	0.336***	(0.067)
Homes recently sold	-0.043*	(0.017)
Owner occupied	0.078*	(0.032)
New homes	0.003	(0.018)
With 2nd mortgage	-0.015	(0.011)
No mortgage	0.038	(0.026)
Single mom households	0.043	(0.041)
Mobile homes	0.022	(0.030)
Attached	-0.030	(0.022)
Black	0.018	(0.072)
Asian	-0.241***	(0.037)
Hispanic	-0.106*	(0.046)
18 and younger	-0.033	(0.066)
65 and older	-0.117**	(0.041)
With college degree	-0.027**	(0.010)
School dropouts	0.026	(0.018)
Unemployed	0.058**	(0.017)
On public assistance	-0.048	(0.033)

Robust standard errors (clustered at the county level) in parentheses.
* significant at 5%; ** significant at 1%; *** significant at 0.1%.
Tenure length dummies and appreciation rate included but not shown.

ported by long-standing owners should be smaller in magnitude than on those reported by recent movers. To test this supposition, we examine the differential impact of Superfund remediation actions on the owner-assessed housing values as reported by new vs. longstanding owners.

Superfund Program Background

The Superfund program was established by US Congress under the Comprehensive Environmental Response, Compensation and Liability Act (CERCLA) of 1980. The Act gave the EPA the right to identify, evaluate, and conduct remedial clean-up of the dangerous hazardous waste sites included in National Priorities List (NPL). Since the passage of the Superfund legislation, more than 1,500 sites located in almost every state have been placed on the National Priorities List (NPL). There is a large literature that seeks to measure the value of hazardous waste site remediation conducted through the Superfund program; for recent reviews, see Greenstone and Gallagher (2008) or Gamper-Rabindran et al. (2011) and references therein.

We examine changes in house values in response to three major stages in the Superfund remediation process – proposal to, final listing on, and deletion from the NPL.

3.4.1 Stated Values and Amenities Valuation: Specification

We estimate the following panel regression specification relating owner-occupied home values to home and block characteristics:

$$\begin{aligned} lnV_{itb} &= X'_{itb}\delta + \alpha_b + P_{bt}\beta_P + L_{bt}\beta_L + D_{bt}\beta_D + \\ &+ M_{it}\big(\gamma \ + P_{bt}\lambda_P + F_{bt}\lambda_F \ + D_{bt}\lambda_D\big) + \epsilon \end{aligned}$$

The subscript b indexes blocks and i indexes individual owner-occupied housing units. lnV_{itb} is the natural log of owner's stated value of housing unit i located in block b in year t (t=1990, 2000). X_{itb} is a vector of house and block characteristics, α_b represents unobserved time-invariant block characteristics, and we include a vector of time-varying census division dummy variables in order to control for regional variation of housing value changes.

The main variables of interest measure exposure of each block to Superfund sites. We include the counts of NPL sites located within specified distances from the centroid of census block b at time t that are proposed (P_{bt}), listed (L_{bt}), and deleted (D_{bt}). We use four distances: < 1, < 2, < 3, and < 5km. In an alternative specification, we include the counts of NPL sites located within various distance *bands* (0-1, 1-2, 2-3, and 3-5km) from the centroid of census block b at time t that are proposed, listed, and deleted.

In order to disentangle the effects of remediation actions on new and long-standing owners' valuations, we include the interaction of the counts of NPL sites with the recent-mover indicator, M_{it}. That dummy variable is defined to be equal to 1 for housing units where the householder moved in within the last ten years, and zero otherwise (results are similar if recent movers are defined as those moved in within the last 5 years). We also include the recent mover indicator to control for unobserved differences between homes with different length of tenure.

The sample is comprised of all owner-occupied housing units contained in all census tracts that have some overlap with the 3km buffer surrounding an NPL site.

3.4.2 Stated Values and Amenities Valuation: Results

Results are reported in Table 3.3 and Table 3.4. For each Superfund remediation action (proposal, listing, and deletion), we report estimates for baseline change in home values associated with that action, and estimated coefficients on the *New Owner* – action interaction. The latter reflects the difference between recent movers' and long-standing owners' valuation of the changes.

As can be seen from the first row of coefficients in Table 3.3, proposal to NPL is associated with house value depreciation. This effect is consistent with previous studies (Gayer et al. 2000, Gamper-Rabindran et al. 2011) – home values fall when EPA evaluates the site and confirms that it poses a threat to human health; this process usually receives a lot of media coverage. Importantly, this effect is greater for new owners. Home values reported by long-standing homeowners fall by 1.9% to 3.3%, depending on the distance from the site. Home values reported by recent movers fall by 0.7% to 0.9% more than home values reported by long-standing owners.

Consistent with expectations, deletion of a nearby site from the NPL, signaling the end of the cleanup process, raises home values. Home values reported by long-standing owners increase by 7.1% within 1 km from the cleaned-up Superfund site, and by 0.8% to 1.6% at greater distances. Values of homes owned by recent movers indicate a larger increase at distances larger than 1 km – by 1.4% to 1.8% larger than values of homes owned by long-standing owners. At distances less than 1 km, home values reported by recent movers appreciate less than those reported by long-standing owners, but the difference is not statistically significant.

Table 3.4 reports results from the regression specification with distance bands

Table 3.3: Marginal Effects of Remedial Actions, by distance below a cutoff

	Distance			
Action	1 km	2 km	3 km	5 km
Proposal	-.0281	-.0333***	-.0205***	-.0188***
New Owner × Proposal	-.0093	-.0065	-.0077*	-.0072*
Final Listing	.0302**	-.0008	.0044	.0047
New Owner × Final	-.0055	-.0082***	-.0041**	-.0037**
Deletion	.0711***	.0161	.0083	.0126*
New Owner × Deletion	-.0207	.0137*	.0193***	.0183***

* significant at 5%; ** significant at 1%; *** significant at 0.1%.
All estimates are based on a sample of 2,366,972 housing units that are in the neighborhood of 1,722 Superfund sites

Table 3.4: Marginal Effects of Remedial Actions, by distance band

	Distance Bands			
Action:	0–1 km	1–2 km	2–3 km	3–5 km
Proposal	-.0281	-.0362***	-.0123	-.0188*
New Owner × Proposal	-.0093	-.0065	-.0105*	-.0075
Final Listing	.0302**	-.0099	.0077	.0027
New Owner × Final	-.0055	-.0091***	-.0016	-.0029
Deletion	.0711***	-.0010	.0002	.0161
New Owner × Deletion	-.0207	.0235***	.0245***	.0183**

* significant at 5%; ** significant at 1%; *** significant at 0.1%.
All estimates are based on a sample of 2,366,972 housing units that are in the neighborhood of 1,722 Superfund sites.

(0-1, 1-2, 2-3, and 3-5km) rather than distances (< 1, < 2, < 3, and < 5km). The results follow the same pattern: the effect of site proposal and deletion on home values reported by long-standing owners have the same sign and smaller magnitude than on home values reported by recent movers. The difference is statistically significant at the 5% level in most cases.

With these estimates in hand, we can now ask the practical question of how much bigger would the aggregate value of the clean-up be if everyone incorporated the most recent information into their stated housing value – i.e., if each respondent were a recent mover? To answer this question, we get the estimates of the clean-up valuation

Table 3.5: Effect on home values for new owners only

	Distance Bands			
	0-1 km	1-2 km	2-3km	3-5km
Proposal	-0.0374	-0.0427	-0.0228	-0.0263
Final Listing	0.0247	-0.0190	0.0061	-0.0002
Deletion	0.0504	0.0225	0.0247	0.0344

for recent movers and compare them with the baseline estimates (same specification, but no new/old owner distinction). Results for four distance bands are reported in Table 3.5 (estimates for new owners only) and Table 3.6 (baseline estimates); results for distance circles are similar. As expected, the "baseline" estimated effects of Superfund actions (i.e. from the model with no *New Owner* – action interaction) lie in between the respective estimates for old and new owners. For homes located more than 1 km away from a deleted Superfund sites, the value of clean-up based on new owners' valuations is 30 to 50% bigger that the one based on the responses of all owners. At 0-1 km distance the coefficient on the interaction term (*New Owner* × *Deletion*) is negative (though not statistically significant), so the baseline estimate is bigger than the one based on recent movers.

These findings suggest that estimates of the housing market capitalization effect of Superfund site deletion can be understated by 30-50% relative to a situation in which all census respondents incorporated the information held by recent movers. This difference, however, can be mitigated or exacerbated by the changes in mobility associated with changes in amenities: as Depro and Palmquist (2012) suggest, homeowners are more likely too move when local environmental quality is substantially different (better or worse) from the levels at the time of purchase.

3.5 Conclusion

The results of hedonic valuation exercises like that conducted here are the basis for much of the cost-benefit analysis done to analyze major regulations in the US. An

Table 3.6: Baseline model (average effect)

	Distance Bands			
	0-1 km	1-2 km	2-3km	3-5km
Proposal	-0.032	-.0383***	-.0165*	-.0217**
Final Listing	.0284*	-0.0134	0.0074	0.0018
Deletion	.06226***	0.0104	0.0119	.0250**

* significant at 5%; ** significant at 1%; *** significant at 0.1%.

analysis of the housing market impacts of the Superfund site remediation process indicates that estimates of the costs associated with site proposal and listing and the benefits associated with site deletion, may all be biased towards zero because census respondents who have not recently purchased their house do not fully factor new information into the housing value that they report. In particular, by combining census stated housing value data with information on housing transactions prices from DataQuick Inc., we demonstrate that long-standing homeowners tend to under-report housing values when those values are appreciating, suggesting that they do not fully incorporate recent housing market developments into their response. This is understandable given that such information can only be collected at a cost; recent buyers, however, have an incentive to incur that cost as part of the house-buying process. They are therefore more likely to incorporate improvements or deterioration in local amenities (such as proximity to a newly proposed, listed, or deleted Superfund site) into their census response. Accounting for this discrepancy in information, we find that estimates of the housing market capitalization effect of Superfund site deletion are understated by 30-50% relative to a world in which all census respondents incorporated the information held by recent movers. This difference is is significant, and could have important implications for cost-benefit analysis.

FIGURE 3.1: Fragment of the 2000 Census questionnaire containing the question about the home value

What is the value of this property; that is, how much do you think this house and lot, apartment, or mobile home and lot would sell for if it were for sale?

- ☐ Less than $10,000
- ☐ $10,000 to $14,999
- ☐ $15,000 to $19,999
- ☐ $20,000 to $24,999
- ☐ $25,000 to $29,999
- ☐ $30,000 to $34,999
- ☐ $35,000 to $39,999
- ☐ $40,000 to $49,999
- ☐ $50,000 to $59,999
- ☐ $60,000 to $69,999
- ☐ $70,000 to $79,999
- ☐ $80,000 to $89,999
- ☐ $90,000 to $99,999
- ☐ $100,000 to $124,999
- ☐ $125,000 to $149,999
- ☐ $150,000 to $174,999
- ☐ $175,000 to $199,999
- ☐ $200,000 to $249,999
- ☐ $250,000 to $299,999
- ☐ $300,000 to $399,999
- ☐ $400,000 to $499,999
- ☐ $500,000 to $749,999
- ☐ $750,000 to $999,999
- ☐ $1,000,000 or more

4

Conclusions

This dissertation contributes to the literature that uses housing values and residential location decisions to infer the value of environmental quality. In particular, it focuses on the role of preferences heterogeneity and persistence in the valuation of localized environmental goods.

The heterogeneity of preferences for environmental goods is important for policy evaluation for several reasons. First, if people differ in their willingness to pay for local amenities, or they respond to the changes differently, then the effects and welfare implications of a new policy will depend on the characteristics of the target population. Further, due to residential sorting, hedonic estimates of the might depend on the sample characteristics: people who choose higher level of risk exposure, are willing to pay less for risk reduction. In addition, the distributional consequences often play an important role in environmental policy evaluation.

This study identifies expected longevity as a source of heterogeneity for non-market valuations - the source that has been overlooked in previous studies. I find the evidence that individuals with shorter expected lifespan are more likely to reside near toxins-emitting facilities. Moreover, the effect of the longevity expectations is significant as compared to other determinants of environmental risk exposure identified in the literature, such as level of education, race, and wealth.

These findings are consistent with the idea that the willingness to pay (WTP) for mortality and morbidity risk reduction might be lower among the older individuals because of the differences in life expectancy. By using elicited longevity expectations, this study provides direct evidence supporting this idea, while the previous literature used age as a proxy for the remaining quantity of life. Controlling for various sources of heterogeneity might help to reconcile the estimates based on differing samples.

This dissertation also examines the role of information and housing wealth persistence in the valuation of local environmental goods. Since homeowners need time to acquire and process new information, changes in amenities are unlikely to be instantly reflected in house values. The empirical analysis provides evidence that owner-assessed home values "lag behind" market prices, suggesting that homeowners rely on outdated information. Specifically, the difference between market prices and the owner-reported values are larger if market conditions are changing quickly (i.e., prices are growing or falling rapidly). This has important implications for the use of homeowners' stated housing values to estimate the value of local public goods and amenities - recent movers are likely to be more informed about current state of neighborhood amenities and home prices. Long-standing owners, on the other hand, do not have many incentives to exert effort to learn about neighborhood changes; they are subsequently more likely to rely on outdated information. We examine the differential impact of Superfund remediation actions on owner-assessed housing values and find that the effect of site proposal and deletion on home values reported by long-standing owners have the same sign and smaller magnitude than on home values reported by recent movers.

Further research might consider extending the ideas explored in this dissertation in the context of environmental justice or environmental health literature.

For instance, racial disparities in expected longevity might help to explain the observed correlation between race and environmental quality documented in the literature. The environmental justice movement asserts that minority and poor communities face greater exposure to pollution than white households (Bullard (1990), UCC (1987) document the beginnings of the environmental justice movement, Bowen (2002) and Brulle and Pellow (2006) provide reviews of the recent developments). However, for environmental policy design, it is essential to understand the forces that give rise to the existing equilibrium (Noonan 2008). Future research might explore the effect of racial differences in longevity on residential choice, thus adding to the literature that distinguish between different explanations for why exposure to environmental risks may vary across communities.

In addition, the interpretation of the residential location choice as one of the health investment decisions has important implications for measuring the effects of pollution on human health. Since the same set of characteristics determines both the individual's exposure to pollution and the level of other health investments, it follows that people who choose to live near pollution sources are likely to invest less in their health (as an indication of this, they are likely to consume less preventive care, more likely to smoke or have unhealthy diets). The correlation between pollution exposure levels and health investment decisions can bias the estimates of the impact of pollution on health. While researchers often point out that pollution exposure might be endogenously determined (e.g. Neidell 2004), this problem is usually addressed by controlling for confounding factors with individual or location fixed effects and exploiting variation of pollution levels over time (this strategy allows measuring only short-term effects of pollution; in addition, fixed effects estimates are more prone to suffer from the attenuation bias if exposure to pollution is measured with error). However, there is little empirical evidence on the extent of this endogeneity. It is possible to contribute to the literature by documenting the correlation between pollution exposure and health behaviors and by examining some of the previously ignored sources of the heterogeneity in the willingness to pay for environmental quality, such as longevity expectations.

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Biography

Tatyana V. Kuzmenko was born on March 15, 1975 in Bilibino, Russia. She has earned her BS and MS in Economics at Novosibirsk State University (Russia), MA in Economics at New Economic School (Russia), and PhD in Economics at Duke University.