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Essays in Environmental Economics

by

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Committee in charge:

Professor Enrico Moretti, Chair
Professor David Card
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Abstract

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The first chapter of the dissertation examines the learning process that economic agents use to update their expectation of an uncertain and infrequently observed event. The standard Bayesian updating model is restrictive in that it reflects the strong neo-classical assumption that economic agents efficiently incorporate new information with all available information when updating beliefs. I consider the case of flooding and estimate the effect of first-hand experience on flood insurance take-up. I compile a new nation-wide panel dataset of large regional floods and flood insurance policies in the US. First, I show that flood insurance take-up in flooded communities increases by 9% after a flood and then steadily declines, fully dissipating after 9 years. Floods do not affect take-up in geographically neighboring non-flooded communities unless these communities are in the same media market. The take-up rate in non-flooded communities that share a media market with a flooded community is one-third as large as in flooded communities. I interpret this evidence using the standard Beta-Bernoulli Bayesian learning model and a Beta-Bernoulli model that includes a forgetting/first-hand experience parameter. I find that the standard Bayesian model can not explain both the spike in insurance in the year of a flood *and* the decay rate of this effect on insurance take-up in the years after the flood. I conclude that the evidence is most consistent with a Bayesian model augmented with a forgetting/first-hand experience parameter.

The second chapter of my dissertation examines the causal link between localized exposure to hazardous waste pollutants from motor vehicle exhaust and adverse human health outcomes for newborns. I explore whether an exogenous event–the 1994 Northridge Earthquake–can be used as a quasi-experiment to test how birth outcomes change from a sudden and unexpected increase in pollution. The Northridge Earthquake closed down portions of four busy highways in Los Angeles, CA for periods of 1-6 months. The highway traffic was diverted onto secondary roads that previous to the earthquake had a much lower traffic volume. The paper focuses on two health outcomes for newborns: birth weight and gestation period. Infants born preterm or with low birth weight are less likely to survive infancy, more likely

to suffer from childhood illness, and have lower future earnings. Overall the results of this study are inconclusive due to the relatively small number of new births included in the sample design. However, the results do suggest that a mother's race, age, and level of education are more important than proximity to a highway. Being a minority race, a teenage mother, or not having any college education are correlated with lower birth weight. The size of these correlations are approximately an order of magnitude larger than the point estimates for the effect of living in close proximity to a road with heavy traffic.

The third chapter of the dissertation uses the housing market to develop estimates of the local welfare impacts of Superfund sponsored clean-ups of hazardous waste sites. We show that if consumers value the clean-ups, then the hedonic model predicts that they will lead to increases in local housing prices and new home construction, as well as the migration of individuals that place a high value on environmental quality to the areas near the improved sites. We compare housing market outcomes in the areas surrounding the first 400 hazardous waste sites chosen for Superfund clean-ups to the areas surrounding the 290 sites that narrowly missed qualifying for these clean-ups. We find that Superfund clean-ups are associated with economically small and statistically indistinguishable from zero local changes in residential property values, property rental rates, housing supply, total population, and the types of individuals living near the sites. These findings are robust to a series of specification checks, including the application of a regression discontinuity design based on knowledge of the selection rule. Overall, the preferred estimates suggest that the local benefits of Superfund clean-ups are small and appear to be substantially lower than the $43 million mean cost of Superfund clean-ups.

To Mariana.

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Chapter 1

Learning about an Infrequent Event: Evidence from Flood Insurance Take-up in the US

1.1 Introduction

This paper examines the learning process that economic agents use to update their expectation of an uncertain and infrequently observed event. A common model of individual learning is Bayesian updating. The standard Bayesian updating model is restrictive in that it reflects the strong neo-classical assumption that economic agents efficiently incorporate new information with all available information when updating beliefs. In this paper I test how well the standard Beta-Bernoulli Bayesian model performs using a new panel dataset on flooding and the purchase of flood insurance.

Flooding in the US is economically significant. Flood damages averaged $6 billion per year from 1955-1999 ([94]). Homeowner insurance policies explicitly exempt coverage for damage due to flooding. Homeowners in the US must decide each year whether to purchase a separate flood insurance policy. I test homeowner learning of the probability of flooding in their community using the timing of flood insurance purchase.

The combination of readily available statistical information, but infrequent personal experience, makes flooding a good context in which to study learning. In the US, historical flooding information and detailed engineering flood maps are accessible to all citizens. Given the large amount of available flooding information, one might find it surprising if the incidence of a new flood changes existing beliefs. The contribution of this paper is to test how well the standard Bayesian learning model fits observational data for an uncertain and infrequently observed event.

The first goal of this paper is to document whether homeowners update their beliefs over the likelihood of future floods after observing a large regional flood.[1] In particular, I am interested in estimating how beliefs change both in the year of a flood and in the years immediately after a flood. To answer this question I construct a new and unique nationwide community-level panel dataset on flood insurance policies and the timing of large regional floods. The dataset includes information on all flood insurance policies in the US for each calender year and whether a community is hit by a Presidential Disaster Declaration (PDD) flood that year.

I use the change in the number insurance policies at the community level as a measure of changing homeowner beliefs. The logic of looking at flood insurance policies is that the decision to purchase flood insurance reveals changing beliefs over the expectation of future floods. The federal government sets the rates for flood insurance and insurance is available for purchase by homeowners before and after each flood at nearly identical rates. A simple homeowner flood insurance model implies that the demand for flood insurance increases as the expected probability of a future flood increases.

I use a flexible event study framework that nonparametrically estimates the causal effect of large regional floods on the local take-up of flood insurance. I find strong evidence of an immediate rise in the fraction of people covered by flood insurance in a flooded community in the year of a flood. The effect peaks after one year at 9% (perhaps reflecting delayed adjustment)–then begins to steadily decline, fully dissipating after 9 years. The event study covers an 18 year panel and includes community and state by year fixed effects.[2] The identifying assumption is that, conditional on a community's geography and calender time trends, whether or not the community is flooded in a particular year is random.

The increase in the take-up of flood insurance is strong evidence that homeowners update their belief of future floods when their community is flooded. I also examine whether homeowners who live in communities that are close to a flood, but not directly affected, "learn" about risks from the experience of their neighbors. I consider two different measures for proximity to a flood: geographic distance and media exposure.

First, I consider homeowners in communities within flooded counties that are not hit by the flood.[3] Flood insurance take-up in these communities is approximately one-third as large as take-up in flooded communities in the same county. Next, I estimate the effect on take-up in communities near to, but outside, flooded counties. The effect of a nearby flood on insurance take-up for these communities is economically small and only marginally significant. Finally, I run the event study analysis using the media definition of indirect exposure to a flood. I identify communities in non-flooded counties that share a media market with flooded communities. Insurance take-up in the year of a flood in these non-flooded communities is economically and statistically significant. The coefficient estimate

[1] All property owners (e.g. business owners) can purchase insurance, but for the ease of exposition in this paper I refer to flood insurance policy holders as homeowners.

[2] A community is defined as a local political entity (e.g. village, town, city).

[3] The data for large regional floods used in the paper are Presidential Disaster Declaration floods. These floods are declared at the US county level.

is about one-third as large as in flooded counties and persists for 6 years. Controlling for the geographic distance from a flooded county does not change the statistical or economic significance of sharing the same media market.

The second goal of the paper is to compare three different models of homeowner learning and provide evidence as to which model best explains the observed homeowner flood insurance purchasing behavior. I first consider the (standard) Beta-Bernoulli Bayesian learning model. Homeowners use information on yearly floods to update their expectation of a future flood in their community. Current and past yearly flood information is weighted equally when updating beliefs over floods. This model implies that as the stock of information increases, the effect of a new observation becomes smaller (and eventually zero). In most communities in the US there are many decades of detailed historical flood records. Thus, in the context of this model, it is surprising that we observe an economically significant increase in the purchase of flood insurance after a flood.

The second learning model I consider is a modified Beta-Bernoulli Bayesian model that includes a forgetting/first-hand experience parameter. This model is motivated by the empirical finding that there is both a spike in the number of flood insurance policies in the year of a flood, and a relatively fast decay of insurance take-up in the years after a flood. Taken together, this behavior suggests that homeowners may not be considering all available past flood information. One way to model this possibility is with a parameter that discounts past information ([29]; [79]).

The third learning model incorporates the other main empirical finding of the event study. Homeowners in *non-flooded* communities take up insurance after a flood, provided they live in the same media market as a community that is flooded. One explanation is that homeowners update their belief of a flood in their community based on the flood risk information content of a nearby flood. The media happens to be how homeowners learn about floods that don't directly hit their community. The third learning model includes a parameter that allows nearby floods to influence the expectation of a future flood.

Next, I test the standard Beta-Bernoulli Bayesian and discounted Beta-Bernoulli learning models. I take advantage of the fact that the standard Beta-Bernoulli learning model is a special case of the discounted learning model when the discount parameter equals one. I use 50 years of observed floods to generate a time series of flood beliefs under the assumption that each model represents the true homeowner learning process. I then select the model that generates the flood beliefs which minimize the mean square error of a function that assumes a log-log relationship between insurance take-up and the conditional expectation (belief) of a future flood.[4] A learning model with a discount parameter value of .95 minimizes the mean square error using non-linear least squares, and provides an acceptable fit to the data using a Chi Square test. Moreover, I can reject the standard Beta-Bernoulli model at the 1% significance level using a Chi Square test. I conclude that a homeowner learning model

[4] I have not yet generated probabilities using the 3rd learning model, but plan to incorporate these results into a later draft of the paper.

that allows past information to be discounted is most consistent with observed homeowner insurance take-up.

Overall, this paper provides evidence that the standard Bayesian model may not be a good model of learning when considering uncertain, but infrequently observed events. The findings of this paper also suggest two specific policy implications. First, collectively homeowners have a short memory and don't appear to equally weigh all past flood information. If a policy goal is for homeowners to self-insure against floods, then offering multi-year insurance contracts or opt out contracts could help prevent homeowners from dropping their insurance after years when there is no flood ([68]). Second, there is clear evidence that the news media is a channel through which homeowners "learn" about their flood risk even when they are not directly impacted by a flood. This suggests that an information campaign could be successful in increasing aggregate flood insurance penetration in regions (markets) where there is currently a low level of take-up. However, the information campaign would need to consistently emphasize flood risks since the lasting effects of the "learning" for homeowners who don't experience floods is short.

Other studies have used survey evidence to show that beliefs about the likelihood of a future natural disaster increase immediately following personal experience with a disaster (e.g. [74]; [85])[5] To my knowledge, this is the first paper to use panel data from an economically important setting to test how well the standard Bayesian updating model explains learning about an uncertain and infrequently observed event. The key to testing the model is using the complete insurance take-up "impulse response function" following a new flood.

There are a number of related literatures. Bayesian updating models have been used in labor economics, for example, to model employer learning of employee productivity (e.g. [13]; [49]; [15]; [64]). Bayesian updating models have also been applied to study learning about the natural environment (e.g. [69]; [41]). More flexible models of learning that relax some of the restrictions of the standard Bayesian models include: [29] and [63].[6] Two recent papers examine the effect of first-hand experience on the interpretation of available information using panel data sets ([60]; [79].[7] Finally, [45] and [83] provide detailed descriptions of flood insurance in the US including some household level characteristics of policy holders.

The remainder of the paper is structured as follows. Section 1.2 provides institutional details on flood insurance and flooding in the US, and outlines the data used for analysis. Section 1.3 presents the event study insurance take-up estimation results. Section 1.4 interprets the insurance take-up results using a homeowner flood insurance purchasing model. Section 4 also outlines three homeowner learning models. Section 1.5 tests the learning models and section 1.6 concludes.

[5] [74] coined the term *natural disaster syndrome* to describe this response. An environmental engineer in California involved with flood plain management refers to this as the *hydro-illogical cycle*.

[6] [29] also provide a succinct overview of 'choice reinforcement' learning models.

[7] A number of other studies examine the effect of first-hand experience in a laboratory setting (e.g. [99]; [19]; [61]).

1.2 Flooding and Flood Insurance in the US

The first objective of this paper is to document whether being hit by a large regional flood leads homeowners to reevaluate their belief about the likelihood of future floods. Homeowner flood beliefs are unobservable. This paper uses the timing of the purchase of flood insurance as evidence of changing homeowner beliefs over future floods. The goals of this section are to summarize the relevant institutional details regarding the purchase of flood insurance, and to introduce and describe the flooding and flood insurance data used in the paper.

1.2.1 The National Flood Insurance Program

Flood insurance was not available to home or business owners in the US for most of the 20th Century.[8] The federal government created the National Flood Insurance Program (NFIP) in 1968. The NFIP sets flood insurance premiums at "actuarial" rates based on historical flood data and detailed community flood maps created by the Army Corps of Engineers. Engineering data and historical observations are used to determine expected damage. The expected damage based rates are then increased by $30-40\%$ to cover the expenses of running the program.[9]

To simplify the rate setting process the NFIP specifies a limited number of nationally designated flood zones. The Corps of Engineers flood maps divide each part of each community as falling into one of approximately 10 flood zones. The zones with the highest flood risk correspond to the 100 year flood plain. Different premium base rates are offered for each zone and adjusted within each zone according to a number of factors.[10]

Homeowners decide whether to purchase flood insurance each calender year.[11] Flood insurance polices are sold by private insurance companies at the rates specified by the NFIP. Flood insurance and risk information is transmitted to home and business owners in a number of ways. First, private insurance companies market flood insurance to homeowners. The companies are compensated by the NFIP for each flood insurance policy transaction. Second, each community offering NFIP insurance posts detailed publicly accessible copies of the

[8]The reasons stated for no private flood insurance market include: the lack of accurate flood risk information that could prevent averse selection and repeated losses on the same policy-holders, and the view that many homeowners are unwilling to pay actuarially fair prices ([1]; [16]).

[9]The exception to this rate setting process are structures built before 1975 (or the introduction of NFIP in each community). The rates for these structures are lower and approximately equal to expected flood damage ([10])

[10]See [11] for more details regarding the rate setting process.

[11]Flood Insurance can only be purchased in those communities that officially participate in the NFIP. Community participation in the NFIP is not mandatory and requires that a community commit to following certain flood plain management principals (e.g. building materials and structural designs). However if a community does not participate in the NFIP then residents of the community are not able to avail themselves of some other federal programs (e.g. Department of Veteran Affairs loan guarantees, and grants to rebuild after a Presidential Disaster Declaration).

Corps of Engineers flood maps. These maps allow each homeowner to precisely identify the location of his home and its corresponding flood zone. Third, flood zone documents are required at the time of purchase or construction of a new home or business if the home or business is within the 100 year flood plain.[12]

One important implication of the NFIP rate setting process is that premium rates are unaffected by whether your home is flooded. The base premium rates (and adjustments) for the 10 nationally designated flood zones are set for the entire country. The NFIP expects that some communities will be flooded each year. For the years included in the panel analysis (1980-2007), the base flood rates for the various zones remain virtually unchanged in real dollars. This aspect of the year to year rate setting process for flood insurance is markedly different from many other insurance markets. For example, most car insurance companies will substantially raise premium rates for a driver the year after an accident.

1.2.2 Flood Insurance Data

All flood insurance policies in the US are sold through the National Flood Insurance Program (NFIP). Through a Freedom of Information Act Request, I received NFIP data on all flood insurance policies from 1980-2007.[13] Figure 1.1 shows that the number of flood insurance policies has increased steadily from about 1.5 million in 1978 to 5.5 million in 2007. This paper focuses on the decision to purchase flood insurance after a large regional flood. The paper does not attempt to explain the overall trend in flood insurance take-up. The event study estimation of in section 1.3 flexibly controls for the aggregate time trend of figure 1.1, so as to estimate the causal impact of regional floods on insurance take-up.

The NFIP data are aggregated at the community level for each calender year. There are several limitations of using the aggregated flood insurance policy count data. I am not able to distinguish between new and continuing flood policies. If the total number of flood insurance policies increases in a community then it is clear that this must include some new policies, but the exact composition of new and continuing policies is unknown. A second limitation, is that the NFIP does not currently track which policies are for properties located in the 100 year flood plain.[14]

I supplement the NFIP insurance data with information I generate directly from each

[12] There are often building restrictions on new structures within the 100 year flood plain. In addition, all new structures that have a bank loan underwritten by the federal government are ostensibly required by law to have current flood insurance for the duration of the loan. However, this law does not appear to be widely enforced ([45]; [9]).

[13] I would like to thank Tim Scoville, NFIP Systems Development Manager, and Andy Neal, NFIP Actuary, for their assistance in providing and interpreting the data.

[14] Surprisingly, the NFIP is also unable to distinguish between new and continuing policies or determine which policies are for structures in the 100 year flood plain. The reason for this is that all of the policy transactions occur by private insurance companies. Until recently, the NFIP has not acquired and retained these data from the private insurers. The NFIP is currently revamping its data storage system to keep track of this information in the future.

community's Corps of Engineers flood zone map. In 2003 the NFIP began a process to digitize each community's flood map. I use GIS software to generate three descriptive variables for each community with a digital flood map: the percent of the community in the (100 year) flood plain, the percent of the community in the 100-500 year flood plain, and the percent of the community outside both of these designations.[15]

Table 1.1 displays summary information for the subset of communities in my primary sample with non-missing digital flood maps. Panel A lists the percent of a community that falls within each of the three flood map designations. The mean (median) percent of a community's land area that falls with the flood plain is 14 (8) percent. The vast majority of each community is within the Corps of Engineers estimated 500 year flood plain. The median amount of each community falling outside the 500 year flood plain is just 4%. Panel B divides flood insurance take-up in 1980, 1990, 2007 by whether the community contains more than or less than the median amount of the community land within the (100 year) flood plain. Not surprisingly, the number of flood insurance policies per person is higher in those communities with more land in the flood plain. For example, in 2007 the mean number of policies in communities with more than the median amount (8%) of land zoned in the flood plain is 35, while those communities with less than the median have a mean of 8 policies.

1.2.3 Presidential Disaster Declaration Floods

One challenge in answering the primary research questions of this study is to find nationally representative flood information to link to the community level flood insurance panel data. Presidential Disaster Declaration floods provide this opportunity. In the next two subsections, I describe the Presidential Disaster Declaration process and the flood data used in this paper.

The Disaster Relief Act of 1950 established the Presidential Disaster Declaration (PDD) system. The legislation formalized a process through which state governments can request federal assistance in responding to natural disasters that occur in their state. The rationale is for the federal government to provide assistance when natural disasters are of a scale that local and state governments are unable to effectively manage the disaster on their own. The first Presidential Disaster Declaration occurred in Georgia in 1953 in response to tornados. Since 1953, natural disasters that have led to Presidential Disaster Declarations include: droughts, earthquakes, fires, floods, hurricanes, and severe storms.

The declaration process has several steps. The governor of a state must write an official letter to the President requesting that a Presidential Disaster Declaration be declared for specific counties in the state. The formal request for a Presidential Disaster Declaration is sent after local and state officials have had time to assess the damage. In the letter the governor outlines the scope of the disaster including weather and damage information

[15] Through a Freedom of Information Act Request I received copies of all digitized community flood maps. As of May 2009, there were digital maps available for approximately one quarter of the communities.

collected by local agencies. The letter must specify the list of counties in the state that would be part of a Presidential Disaster Declaration. Historically three-quarters of flooding Presidential Disaster Declaration requests have been granted.[16]

A Presidential Disaster Declaration opens the door to two major types of disaster assistance. The largest component of disaster assistance in Public Assistance. Public Assistance is available to local and state governments, as well as, non-profit organizations located in a PDD county. These groups can access grant money to remove debris, repair infrastructure, and to aid in reconstruction of public buildings. The damage must have been caused by the natural disaster.[17]

The second type of disaster assistance is Individual Assistance. Individual Assistance is available to homeowners and residents in Disaster Declaration counties. Home and Business owners can access low interest disaster loans to rebuild. Direct cash assistance is also available for temporary and emergency expenses such as covering the cost of interim housing.[18]

1.2.4 Flood Data

This paper uses Presidential Disaster Declaration events as a data source of large regional floods. I downloaded information on all Presidential Disaster Declarations involving flooding from the Public Risk Institute (PERI) website.[19] The data collected include the date of the Presidential Disaster Declaration, the type of disaster, location information (state and county), and an estimate of disaster cost. I only consider Disaster Declarations that list coastal storms, severe storms, hurricane, or floods as the primary type of disaster.

Figure 1.2 displays the number of flooding Presidential Disaster Declarations by county from 1990-2007. Figure 1.2 is created using the same dataset used to run the event study analysis in Section 1.3. All communities participating in the National Flood Insurance Program that have non-missing population data for the 1990-2007 panel are included in the event study analysis. There are 2704 such counties (or county equivalents). This includes approximately 90% of all US counties. The vertical axis of figure 1.2 measures the percent of counties with each number of Presidential Disaster Declarations. Nearly every county in the sample, 92%, is hit by at least one Presidential Disaster Declaration flood during the 18 years from 1990-2007. The median number of PDD floods for a county is three. There are twelve counties with ten or more PDD floods. Eleven of these twelve counties are located in North Dakota near to the Red River.

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[16] In 1986 the Federal Emergency Management Agency established a set of criteria to use when evaluating whether to grant a declaration request. These criteria included estimated damage costs. Nevertheless, there is institutional discretion when deciding whether to grant requests ([46]; [98]).

[17] The Stafford Act of 1988 specifies that the federal government will cover at least 75% of the replacement value of infrastructure or building repairs. States are required to pay the remaining 25% as a condition of receiving the federal Public Assistance money.

[18] In 2007 the threshold for housing assistance was capped at $28,200.

[19] I would like to thank Richard Sylves for helpful conversations about these data.

Figure 1.3 shows a county delineated map of the continental US. The map is color coded based on the number of Presidential Disaster Declarations from 1990-2007. The darker the shade of red the fewer the number of floods. Bright red corresponds to counties with 9 or more PDD floods, while black counties have zero floods. The white counties are those excluded from the analysis.

One assumption of this paper is that community-level flood probabilities are constant from 1958-2007. Overall this is consistent with the view of the NFIP and the CORPS of Engineers. The flood designations in only about 1% of the community flood maps have been modified since the maps were first created in the 1960's and 1970's. Figure 1.4 provides support for this assumption using county-level cost data.[20] Figure 1.4 plots the mean per capita county flood cost (2008 $) from 1969-2004. The data are National Weather Service flood cost information collected by the National Climatic Data Center[21] There is a great deal of year to year variation in mean county flood costs, but overall, there is no national trend over this period.

Presidential Disaster Declaration floods are determined at the county level. However, not all communities within a county may be effected by the flood. I construct a variable to identify which communities in PDD counties are "hit" by each Presidential Disaster Declaration using information on claims via the Public Assistance program. As described above, state and local governments–as well as non-profits–are entitled to grant money to repair infrastructure and rebuild structures damaged by flooding in counties included in a Presidential Disaster Declaration. Through a Freedom of Information Act Request, I received a datafile that lists the location of <u>every</u> Public Assistance damage claim paid out from 1990-2007.[22] There are more than 800,000 unique observations. All observations are linked to the Presidential Disaster Declaration under which it was filed. From these data I create an indicator variable for whether a community within a PDD county is hit by a particular flood. I consider a community to be hit if there is at least one Public Assistance claim with a damage location within the community.

I am able to match between 90-95% of the Public Assistance claims to a NFIP community.[23] I almost certainly fail to code some communities as being hit by a Presidential Disaster flood due to the non-matched claims data. The effect on the event study regression estimates will be to bias insurance take-up coefficient estimates after a hit towards zero.[24]

[20]Ideally we would want to observe community level flood costs, but these are not available for this time period.

[21]The Hazards and Vulnerability Research Institute at The University of South Carolina compiled and cleaned the National Climatic Data Center data and maintain the data as part of the "Spatial Hazard Events and Losses Database for the United States" (SHELDUS). I would like to thank Chris Emrich for assistance in interpreting the SHELDUS cost data. The SHELDUS data are in levels. I adjusted the data using US Census county population estimates. See data appendix for more details.

[22]I would like to thank Deni Taveras and Paul Weschler for preparing the data and shepherding the data request through the FOIA process.

[23]See Data Appendix for details on matching.

[24]In the event study regressions I identify the effect on insurance take-up of being hit off of those communi-

Panel C of Table 1.2 provides summary statistics for the percent of communities in PDD counties hit by Presidential Disaster Declarations from 1990-2007. Overall, 32% of communities in counties with a Presidential Disaster Declaration are hit by a PDD county level flood in the year of a flood. The percent of communities hit by a PDD flood is similar for those communities with less than the median amount of community land mapped in the flood plain, as it is for communities with more than the median amount of land considered within the flood plain. 29% of less than median communities are hit by a PDD flood whereas 35% of more than median communities are hit.

1.3 Econometric Model and Estimation Results

The first goal of this paper is to document whether home and business owners update their beliefs over future flooding after exposure to a large flood. I use changes in the number of homeowners with flood insurance policies as a measure of changing beliefs. The economic model underlying the relationship between floods, flood beliefs, and flood insurance will be discussed in detail in the next section. The key prediction of the model is that if homeowner beliefs over future floods increase then more homeowners will purchase flood insurance. This paper uses the timing of large regional flood events as exogenous events that potentially lead homeowners to revise upwards their beliefs of future floods. This section discusses the statistical model and the main estimation results.

1.3.1 Event Study Empirical Specification

I use a flexible event study framework that nonparametrically estimates the causal effect that large regional floods have on the take-up of flood insurance. Equation (1.1) shows the main estimating equation.

$$ln(takeup_{ct}) = \sum_{\tau=-T}^{T} \beta_\tau W_{c\tau} + \alpha_c + \gamma_t + \epsilon_{ct} \tag{1.1}$$

The unit of observation is a community calender year. The dependent variable in equation (1.1), $ln(takeup_{ct})$, is Log Flood Policies Per Person for community c in year t. The independent variables of interest are the event time indicator variables, $W_{c\tau}$. These variables track the year of a Presidential Disaster Declaration hit and the years immediately preceding and following a hit. The indicator variable W_{c0} equals 1 if community c is hit by a flood in that calender year. The indicator variable $W_{c\tau}$ equal 1 if a community is hit by a Disaster

ties that are not hit by the flood. Accidently assigning hit communities to the not hit group will bias upwards the insurance take-up of the non-hit group (assuming that there is a positive correlation between being hit by a flood and take-up), and bias downwards the coefficient estimate of the take-up of hit communities relative to the non-hit group.

Declaration in $-\tau$ years. Many communities are hit by more than one PDD flood during the event study. For these communities each flood is coded with its own set of indicator variables.[25]

In most of the specifications of equation (1.1) I bin the $W_{c\tau}$ by creating a single indicator variable for the end periods. The bin indicator variables serve a practical purpose. I am most interested in the years shortly before and after a flood. The event time indicator variables, $W_{c\tau}$, near the tails of the event study are identified off of many fewer observations and therefore have large standard errors. Binned indicator variables pool the effect on take-up over multiple event years to increase statistical power.[26]

Equation (1.1) also includes community fixed effects, α_c, and calendar year fixed effects γ_t. These fixed effects control for unobserved (and unchanging) community characteristics and yearly factors. Community geography is important in predicting the likelihood of a flood. The underlying community geography includes surface characteristics, such as the percent of a community located in the flood plain, and location specific factors such as average rainfall. Year fixed effects account for year to year changes in NFIP institutional factors and other yearly trends that may effect take-up.

The preferred specification of equation (1.1) replaces the year fixed effects, γ_t, with a full set of state by year fixed effects. The state by year fixed effects nonparametrically control for state specific time trends. ϵ_{ct} is a stochastic error term. Standard errors from the estimation of equation (1.1) are clustered at the state level. Finally, the causal interpretation of equation (1.1) comes from the assumption that whether a community is hit by a flood in a particular year is random conditional on community and year (or state by year) fixed effects.

The event time indicator variable W_{c-1} is normalized to zero when I estimate equation (1.1). In practice this is done by excluding W_{c-1} from the regression. Normalizing W_{c-1} to zero provides for a useful interpretation of the remaining event time indicators in equation (1.1). The estimated coefficients for all other event time variables are interpreted as the percent change in the take-up of flood insurance in community c <u>relative</u> to the year before a flood. In other words, the event study answers the question: "How much greater is the take-up of flood insurance in each year after a flood compared to the year before a flood?"

I estimate equation (1.1) on a panel of communities over two different time periods: (i) 1980-2007, (ii) 1990-2007. These time periods are selected based on data availability. Community-level flood insurance policy data are available beginning in 1978, but the community-level population data is not as available until 1980. Thus, the 28 year period from 1980-2007 is the longest panel for which I can estimate flood insurance take-up for a

[25] For example, Hazlehurst, GA is hit by a Presidential Disaster Declaration in 1991 and 2004. Thus for Hazlehurst, GA in Year 2000, $W_{c9} == 1$ since it has been 9 years since the 1991 PDD and $W_{c-4} == 1$ since it is 4 years before the 2004 PDD.

[26] For example, in the 1990-2007 panel event study $W_{c,17} = 1$ only if there is a Presidential Disaster Declaration in 1990. In the 1990-2007 panel event study I create $W_{c,early} = 1$ if $\tau \in [-17, -11]$ and $W_{c,late} = 1$ if $\tau \in [17, 11]$. Equation (1.1) is then estimated with these 2 bin indicator variables rather than including the individual variables $W_{c,11}, ..., W_{c,17}$ and $W_{c,-11}, ..., W_{c,-17}$.

large sample of communities. In all of these regressions the definition of a flood is whether a homeowner resides in a community that is in a Presidential Disaster Declaration county. For the period 1990-2007, I can use a more detailed definition of a flood hit. Beginning in 1990 I confirm whether a PDD flood declared at the county-level damaged infrastructure or public buildings in each community in the county. I estimate equation (1.1) over this period using the community-level definition of a flood.

I am also interested in estimating the take-up of flood insurance for communities not directly hit by a flood.

$$ln(takeup_{ct}) = \sum_{\tau=-T}^{T} \beta_\tau W_{c\tau} + \sum_{\tau=-T}^{T} \lambda_\tau N_{c\tau} + \alpha_c + \gamma_t + \epsilon_{ct} \tag{1.2}$$

I estimate equation (1.2) when I consider "neighboring" communities that were not directly hit by a flood. Equation (1.2) is identical to equation (1.1), except that it also includes event time indicator variables for neighboring communities, $N_{c\tau}$.

Finally, two flood data coding decisions deserve comment. First, occasionally a community is hit by more than one PDD flood in the same calender year.[27] I don't distinguish between communities hit by one or more than one PDD flood in a particular year when estimating equation (1.1). The reason for this is that the flood insurance policy count data are aggregated by year. I am concerned with whether a community is hit by *any* flood in a calender year. Second, for the 1990-2007 panel I only consider leads and lags for a Presidential Disaster Declaration if the PDD occurred within the time frame of the event study. Therefore the $W_{c\tau}$ indicator variables all equal 0 for a community with respect to any event that occurs outside the event study window. I run a number of robustness checks to test the sensitivity of this coding decision. For the 1980-2007 panel I can control for the timing of Presidential Disaster Declarations before 1980.

1.3.2 Estimation Results for Communities Hit by a Flood

Figure 1.5 plots the event time indicator coefficients, β_τ, from the estimation of equation (1.1) with state by year fixed effects on the 1990-2007 panel. Event time is plotted on the x-axis. Year zero corresponds to a year a community is hit by a PDD flood, while years $-1, ..., -10$ and $1, ..., 10$ are the years before and after a flood respectively. I bin the tail ends of the event study, so the leftmost (rightmost) point on the graph is a pooled coefficient for the years -11 to -17 (11 to 17). The results are normalized to the year before a flood hit. The plotted event time coefficients can be interpreted as the percent change in the take-up of flood insurance policies in the community relative to the year before a flood. The bands

[27] Conditional on a community being in a county with a Presidential Disaster Declaration in a particular year, 11% of the time there are more than one PDD's in the same year (for communities in the 1990-2007 panel).

around each coefficient represent the 95% confidence interval and show whether the point estimate is statistically different from zero. Standard errors are clustered at the state level.

There is no event year time trend in the years before a flood. The effect of a *future* flood is not statistically different from zero for all time periods before the flood. The point estimates for the pre-flood event years range from -1.4% to 1.5%. In the year of a flood there is an 7% increase in the take-up of flood insurance relative to the year before a flood. Take-up peaks at 9% the year after a flood. Flood insurance take-up after the flood remains positive and statistically significant for 10 years. After 10 years, flood insurance take-up is not statistically different relative to the year before a flood.

Figure 1.6 plots the event time indicator variables from a specification of equation (1.2) that includes separate indicator variables for communities hit by a PDD flood, and for neighboring communities in PDD counties that are not hit. When compared to figure 1.5, the coefficient point estimates for communities hit by a flood are approximately a percentage point lower, but the overall interpretation remains the same. Take-up in communities in PDD counties not hit by a flood is 2-3% for the first 6 years after a flood (years 2 and 3 are significant at 10% level). The take-up response is about one-third as large as that for communities directly hit by the flood.

Table 1.2 shows the point estimates and standard errors for figure 1.3 (column 2) and figure 1.4 (column 4), as well as, estimates from specifications with year fixed effects. Overall, the point estimates and standard errors for specifications of equation (1.1) with year fixed effects are larger than those with state specific time trends. Comparing column (2) to column (1) we see that take-up in the year of a flood is two percentage points lower, while the effect of a flood persists for one additional year. A similar pattern holds when looking at estimates of equation (1.2) that specifically control for the impulse response function of non-hit communities in PDD counties. The point estimates of the non-hit communities are shifted down by 1-2 percentage points, but remain statistically significant for the first 5 years after a flood.

Figure 1.7 estimates equation (1.1) on the 1990-2007 panel, where each post flood event time variable is interacted with an indicator variable for whether or not the PDD flood is above or below the median flood cost. I use the PERI Presidential Disaster Declaration flood cost variable to distinguish between large and small floods.[28] Not surprisingly, flood insurance take-up in communities hit by above median cost PDD floods is greater than in those communities hit by below median cost floods. The slope of the take-up impulse response function is similar following the two types of floods. The entire above median cost flood insurance take-up impulse response function is shifted upwards relative to that for below median cost floods. An F-test which tests the null hypothesis of no difference between the post flood event time coefficients can be rejected at the 5% level for the first three flood years and the 8th year after a flood.

[28]The PERI cost variable is not inclusive of all costs, but is a consistent measure of flood costs from 1990-2007.

Estimation of equation (1.1) on the 1980-2007 panel has the advantage of a longer panel with more PDD floods. I am also able to specifically control for PDD floods that occurred before 1980.[29] The geographic definition of a flood for the event study regressions using the 1980-2007 panel is whether a homeowner lives in a community that is part of a county included in a Presidential Disaster Declaration.[30] Using the county as the geographic designation of a flood averages the effect of a flood on take-up over those communities that were hit by a flood and those not hit by a flood.

Figures 1.8 plots the event time coefficients from the estimation of equation (1.1) on the 1980-2007 panel. All of the event time coefficient estimates before the year of a PDD flood are not statistically different from zero and economically small. The point estimates range from -0.5% to 1.0%. In the year of a flood there is 5.7% increase in the take-up of flood insurance relative to the year before a flood. Flood insurance take-up peaks the year after a flood at 7.4%. The effect of a flood on the take-up of insurance persists for 6 years.

Table 1.3 includes event time coefficient estimates of equation (1.2). The estimates in column (3) are from a specification that controls for all PDD floods from the 1970s. The point estimates are shifted up 1-2 percentage points relative to the panel that doesn't control for floods before the sample. The standard errors are smaller and the effect on take-up is significant (at the 5% level) for the first 14 years after a flood.

1.3.3 Estimation Results for Neighboring Communities

This subsection returns to the question of whether homeowners update expected flood beliefs if they are not directly hit by a flood. The last subsection shows that homeowners in Presidential Disaster Declaration counties who live in communities not directly hit by the flood respond to a nearby flood by purchasing flood insurance. The take-up of flood insurance is about one-third as large in the non-hit communities relative to the hit communities.

Next, I use the 1980-2007 panel to estimate the effect on homeowner take-up in communities in counties <u>not</u> included in the Presidential Disaster Declaration, but "near" to a PDD county. Figures 1.9, 1.10, and 1.11 plot event time coefficients for communities in PDD counties and neighboring counties from 3 separate regressions of equation (1.2) using the 1980-2007 panel that controls for PDD floods from the 1970s. Figure **??** considers a geographic definition of a neighbor. I define a geographically neighboring community as a community in one of the 5 closest counties as measured by distance between county centroids.[31] Figure **??** considers a community to be a neighbor if it belongs to the same media market. Nielson Media Research classifies each US county as belonging to a primary radio

[29]Controlling for floods that occurred before 1980 guards against the possibility that the post-flood impulse response for floods in the years just before the start of the panel (i.e. the 1970s) are confounding the interpretation of the coefficient estimates.

[30]I am not able to determine whether a community is "hit" by a PDD flood before 1990.

[31]I would like to thank Juan Carlos Suarez Serrato for creating and sharing the datafile that lists all US counties and the 10 closest counties as measured by Euclidean distance between county centroids.

and television media market. There are 210 unique designated media markets (DMAs).[32] [33] Flood insurance take-up in a neighboring community, which is not part of the Presidential Disaster Declaration, is similar under both definitions. Take-up in the year of a nearby PDD flood is about one-quarter of that in communities that are in the flooded county, and persists for 9 years. Figure 1.11 estimates equation (1.2) with event time indicators for each type of neighbor. Interestingly, the effect on take-up of being a geographic neighbor is no longer statistically significant from zero. The statistical significance of being within the same media market remains significant for the first 5 years after a flood.[34] The effect on communities not in a PDD county, but in the same media market is unchanged if I estimate a specification of equation (1.2) that includes geographic and media neighbor event time indicators, as well as, a complete set of event time indicators for the interaction.[35]

Table 1.4 shows event time coefficient estimates from a specification of equation (1.2) that includes state by year fixed effects, but pools the pre-flood event time period to improve statistical power. Column (1) estimates homeowner take-up for communities in counties that are geographic neighbors to a PDD county, defined as the 5 closest counties, but not included in the Presidential Disaster Declaration.[36] The coefficients from the geographic neighbor peaks at 1.8% and are significant at the 5% only in the 2nd year after a flood. In contrast, flood insurance take-up is 2.3% higher in media neighbor communities (column 2) in the year of nearby PDD flood. The media neighbor event time coefficient estimates remain between 2.5% and 3.5% and are significant at the 1% level for the first 6 years.

Columns (3) and (4) of Table 1.4 consider both geographic and media neighbors. There is no effect on take-up of being a geographic neighbor after controlling for whether a community is in the same media market. The point estimate for insurance take-up for communities in

[32] I would like to thank James Snyder for sharing the DMA data. Synder and Stromberg (2010) use these data to estimate how press covered effects citizen knowledge, politicians' actions, and policy. The data were first collected and used by [18] and [17].

[33] The primary media market can change over time for a county. Nielson Media Research released new county DMA classifications in 1980, 1990, and 2000. For those counties that change media markets over time, I assume that a county is in a media market until the year the new DMA data are released.

[34] The first year after a flood is significant at the 10% level. The effect of being in the same media market remains statistically significant when I consider the closest, 3 closest, and 10 closest centroid counties.

[35] The effect on media neighbor communities is unchanged if I estimate a specification of equation (1.2) that includes geographic and media neighbor event time indicators, as well as, a complete set of event time indicators for the interaction. Identification of the indirect (neighbor) effect of a large regional flood on take-up in figures **??**, **??**, and **??** uses cross state variation. An example from 1992, detailed in the data appendix, shows how cross-state variation assists in the identification of the geographic and media neighbor flood insurance take-up.

[36] I also run specifications that consider the geographic neighbor to be all adjacent counties. The point estimates for the adjacent county specification are similar, but have less statistical power. The adjacent county file, *Contiguous County File, 1991*, was created by The Inter-University Consortium for Political and Social Research (www.icpsr.umich.edu). The *Contiguous County File, 1991* includes counties that share a boarder, are connected by a major road, or are connected due to "significant economic ties". I only consider those counties that share a boarder.

the same media market remains virtually unchanged. Table 1.4 taken together with the estimates from columns (2) and (4) of Table 1.2 imply that homeowners update their beliefs over future flooding if they live in a community hit by a flood, or if they are in the same media market as a community hit by a flood.

1.4 Economic Framework: Insurance Model and Learning Models

In this section I present a simple flood insurance model and three alternative homeowner learning models. The goals are twofold. First, provide an economic framework to interpret the empirical results from the last section. Second, outline three theories of learning and belief formation, motivated by the empirical results of section 1.3, that have been used in the broader learning literature. Section 1.5 presents evidence as to which theory of learning is most consistent with observed flood insurance take-up.

1.4.1 Insurance Model

Each year homeowners purchase the level of flood insurance that maximizes their expected utility given their belief about the probability of a flood.

$$max_{q_{ict}} E_t[u(q_{ict}, w_i, l_i, r_i, p_{ict})] = p_{ict} * u(w_i - l_i - r_i q_{ict} + q_{ict}) + (1 - p_{ict}) * u(w_i - r_i q_{ict}) \quad (1.3)$$

q_{ict} is the level of flood insurance selected by homeowner i in community c in year t. There are four parameters. The parameter of interest is p_{ict}, the homeowner belief of the yearly flood probability in time t. w_i is homeowner wealth and l_i is the amount of flood damage conditional on being hit by a flood. $r_i \in (0, 1)$ is the dollar rate per $1 of flood insurance. Each homeowner chooses the level of insurance, q^*_{ict}, that maximizes expected utility at the end of the calender year after observing whether there is a flood and updating beliefs p_{ict}.

$$f(q_{ict}, w_i, l_i, r_i, p_{ict}) \equiv p_{ict}(1-r_i)(w_i - l_i - r_i q_{ict} + q_{ict}) * u' - (1-p_{ict}) r_i * u'(w_i - r_i q_{ict}) = 0 \quad (1.4)$$

Equation (1.4) defines $f()$ as an implicit function equal to the first order condition for the homeowner flood insurance problem. q^*_{ict} solves the implicit function. w_i, l_i, r_i are all constant parameters. The insurance rate is set by the federal government and to a close approximation is fixed in real dollars. An assumption of this paper is that homeowner beliefs over flood damages are fixed. Homeowner wealth, in contrast to the assumption of this paper, is certain to vary over time. In particular, in a year of a flood, those homeowners without flood insurance are likely to have a negative shock to their wealth. Provided flood insurance is a normal good, then the demand for insurance would decrease and bias me towards not observing an effect.

If a homeowner's belief over future flooding increases, then the utility maximizing level of flood insurance will increase. The comparative static, $\frac{\partial q^*_{ict}}{\partial p_{ict}} > 0$, by the implicit function theorem, provided $u' > 0$ and $u'' < 0$.[37] Figure 1.12 plots homeowner insurance demand as a function of beliefs. q^*_{ict} is plotted on the vertical axis with a horizontal line at $q^*_{ict} = 0$. $p_{ict} \in [0, 1]$ is plotted along the horizontal line. $\bar{p_{ic}}$ is the cutoff value of p_{ict} such that $q^*_{ict} = 0$. If the belief over future flooding in year t is greater than $\bar{p_{ic}}$, then homeowner i living in community c will purchase flood insurance for that calender year. $\bar{p_{ic}}$ varies by homeowner depending on the parameters w_i, l_i, r_i, and each homeowner's level of risk aversion.

I observe flood insurance count data aggregated at the community level. Figure 1.13 shows the relationship between the number of community level flood insurance policies and beliefs over future floods. On the vertical axis is the number of flood insurance policies in the community: $Q_{ct} = \sum_{i=1}^{I} \mathbf{1}(q^*_{ict} > 0) = \sum_{i=1}^{I} \mathbf{1}(p_{ict} > \bar{p_{ic}})$. The horizontal line again plots flood probabilities. Similar to figure 1.12, each homeowner's $q^*_{ict}(p_{ict})$ can be plotted in figure 1.13. I have plotted this function for a sample community of three homeowners.

For ease of exposition, let's assume that all homeowners in the community are impacted by a flood in the same way and use the same learning process when adjusting beliefs over future floods. If this were the case, then $p_{ict} = p_{ct}$ so that each year, everyone in the community shares the same flood belief. The dashed vertical line in figure 1.13 represents a hypothetical (universally shared) flood belief for each homeowner in the community. p_{ct} is greater than the flood insurance cutoff point for homeowners 1 and 2, but not for homeowner 3. Homeowners 1 and 2 will purchase flood insurance. It is important to emphasize that although each homeowner's belief of a flood is the same, that the demand function for flood insurance varies for each homeowner.

Figure 1.13 helps to clarify two points. First, I assume a continuous range of homeowner insurance cut-off points ($\bar{p_{ic}}$) in each community. In other words, for a change in p_{ct}, there will be a marginal homeowner just willing to purchase (if $dp_{ict} > 0$) or fail to renew an insurance policy (if $dp_{ict} < 0$). Second, although other researchers have noted an increase in the average level of community wide insurance coverage among policy holders after a flood, this doesn't necessarily follow from the assumptions of this paper ([83]). There are two effects of an increase in community flood beliefs (a shift of the dotted line to the right): (i) existing policy holders will purchase more insurance, and (ii) new "marginal" homeowners will decide to purchase insurance. The average level of flood insurance in a community (conditional on having insurance) depends on the composition of these two effects.[38]

[37] By the Implicit Function Theorem (IFT) we can write $\frac{\partial q^*_{ict}}{\partial p_{ict}} = -\frac{\partial f/\partial p_{ict}}{\partial f/\partial q^*_{ict}}$, where f is equation (1.4). Note that to apply the IFT two conditions on f must hold. First, equation (1.4) must be continuously differentiable at (q^*_{ict}, p_{ict}), given the values of the fixed parameters w_i, l_i, r. Second, $\partial f/\partial q^*_{ict} \neq 0$ at (q^*_{ict}, p_{ict}). I assume that these two conditions hold.

[38] The interpretation of the community aggregated insurance policy count data is similar if we relax the strict assumption that all homeowners in the same community perceive each flood the same when updating beliefs. We could adjust figure 1.13 so that there is a dashed vertical line specific to each homeowner.

1.4.2 Homeowner Learning Models

One of the conclusions from the event studies of Section 1.3 is that homeowners react to a new flood by purchasing flood insurance. I model the observed take-up in flood insurance as the utility maximizing decision from an annual homeowner insurance purchasing problem. The underlying assumption is that homeowners use the information implicit in a new flood event to update their expectation over the probability of a future flood hit. In other words, the changing homeowner beliefs towards future floods is driving the dynamics of insurance take-up after a flood.

Floods potentially provide new information for homeowners about their underlying flood risk. Standard (neo-classical) economic models assume fully rational economic agents. In the context of flooding, this implies that homeowners would use the Beta-Bernoulli Bayesian learning model to synthesize existing information and update beliefs.[39] In this model, large yearly regional floods, y_t, are distributed Bernoulli where the probability of a flood in a given year for community c is: $P(y_t = 1) = p$. Each community's yearly flood draw is assumed to be independently drawn from a stationary flood distribution with parameter p. The probability of a flood in a given year, p, is assumed to be distributed $Beta(\alpha, \beta)$[40]. The first two moments of $p \sim Beta(\alpha, \beta)$ are $E[p] = \frac{\alpha}{\alpha+\beta}$ and $Var[p] = \alpha\beta(\alpha+\beta)^2)(1+\alpha+\beta)$.

I assume that homeowners observe whether there is a flood in a given year and update their expectation of a future flood. The conditional mean and variance are:

$$E[p|S_t, t] = \frac{S_t + \alpha}{t + \alpha + \beta} \tag{1.5}$$

$$Var[p|S_t, t] = \frac{(S_t + \alpha)(t - S_t + \beta)}{(t + \alpha + \beta)^2(1 + \alpha + \beta + t)} \tag{1.6}$$

t is the number of yearly observations (time periods) $S_t = \sum_{s=1}^{t} y_s$ is the number of observed floods. α and β are fixed parameters from the Beta distribution. The parameters α and β determine the initial belief over flooding. Homeowners use the conditional flood expectation equation to update this belief each year.

The event study results from section 1.3 suggest that two features not captured by the classical Beta-Bernoulli Bayesian model may be important in modeling the underlying homeowner flood belief learning process. The first feature is the spike in flood insurance policies in hit communities in the year of a flood, combined with a post-flood impulse response function where the effect of the flood is statistically zero after approximately 10 years. The classical Beta-Bernoulli model implies that as the stock of information increases, then the effect of a new observation will become small (and eventually zero). In most communities in the US there are many decades of detailed historical flood records. The large spike in flood insur-

[39] The discussion of the Beta-Bernoulli statistical model closely follows [31].

[40] The Beta distribution is the conjugate prior for the Bernoulli distribution ([42]) and used in most Bernoulli Bayesian models for convenience.

ance coupled with the relatively fast decay of this effect suggest that homeowners may not be considering all of the past flood information. One way to model this possibility is with a parameter that discounts past information ([29]; [79]).

The second learning model I consider is a Discounted Beta-Bernoulli Bayesian model. The Discounted Beta-Bernoulli model introduces one additional parameter, δ, into the conditional expectation updating equation of the Beta-Bernoulli Bayesian model. This model has the appealing feature of reducing to the Beta-Bernoulli Bayesian model when $\delta = 1$. The conditional mean updating equation under the Discounted Beta-Bernoulli model is given by equation 1.7.

$$E[p|S_t^{'}, t^{'}] = \frac{S_t^{'} + \alpha}{t^{'} + \alpha + \beta} \tag{1.7}$$

$t^{'} = \sum_{s=1}^{t} \delta^{t-s}$ is the number of yearly observation "equivalents". $S_t^{'} = \sum_{s=1}^{t} y_s \delta^{t-s}$ are weighted flood observations. $\delta \in [0, 1]$ is a weighting parameter.

The data I observe and the event study estimation results in section 1.3 are aggregated at the community level. The conditional flood expectation equations 1.5 and 1.7 both model individual homeowner learning of the probability of future floods. If all of the homeowners use equation 1.7, then we can interpret δ as a measure of "forgetting" in the community. All homeowners discount past flood information, so δ in the community level equation is the average amount of "forgetting". On the other hand, if some homeowners update according to equation 1.7 and other homeowners update using equation 1.5, then when we aggregate to the community level, δ becomes a weighting parameter between individuals using the two different updating equations ("forgetful" homeowners and fully rational homeowners). Following the logic of the reinforcement learning literature, those homeowners who don't have first hand experience with floods will discount past flood information ([60]; [61]; [63]).[41] Those homeowners with first-hand experience do not discount the past flood information (i.e. $\delta = 1$).

The second feature of flood insurance take-up is that homeowners react to a flood by purchasing flood insurance if they are in the same media market. Take-up of insurance is approximately one-third as large in communities not directly hit, but in the same media market as a flooded community. Overall, the hit and non-hit media market communities appear to have similar take-up impulse response function decay rates after a flood. The geographic distance from the flood doesn't appear to matter in the decision to purchase insurance if homeowners are not hit by the flood.

Equation 1.8 is a learning model that incorporates the two main features from the event study results of section 1.3.

$$E[p|S_t^{'}, N_t^{'}, t^{'}] = \frac{S_t^{'} + \sigma N_t^{'} + \alpha}{t^{'} + \alpha + \beta} \tag{1.8}$$

[41] Survey evidence on the importance of first hand experience in forming beliefs over the likelihood of a natural disaster include, for floods: [75] and [74], and on earthquakes: [85].

First, just like the Discounted Beta-Bernoulli model, equation (1.8) includes a discount parameter δ to account for the possibility of "forgetting". There are also 2 new terms: $\sigma \in [0,1]$ and $N_t^{'} = \sum_{s=1}^{t} n_s \delta^{t-s}$, where n_s is an indicator variable equal to 1 if a neighboring community is hit by a flood and your community is not hit by the flood. Together σ and $N_t^{'}$ capture the possibility that homeowners may update their beliefs over a future flood when there is a large regional flood that doesn't hit their community.

There are two ways to view the event study result for non-flooded communities and interpret this model. First, homeowners in non-flooded communities learn about nearby flood through the media and update their beliefs over future flooding regardless of their underlying flood risk. Second, and perhaps more realistically, homeowners in non-flooded communities update flood beliefs using the information content of neighboring floods. A nearby flood may contain relevant information on the likelihood that a geographically similar community will be flooded in the future. It follows from this interpretation, that the media is the channel through which most homeowners not directly hit by the flood learn of a geographically neighboring flood. Nevertheless, it is the underlying information content of the flood that leads homeowners in non-flooded communities to take-up flood insurance.[42]

1.5 Comparing the Learning Models

This section uses the learning models and the complete history of Presidential Disaster Declaration floods to generate a time series of flood probabilities for each community. I then compare the simulated homeowner beliefs over future flooding under each learning model with the observed take-up of flood insurance.

I use the learning equations 1.5 and 1.7 to generate county-level homeowner flood beliefs using the complete 50 year time series of Presidential Disaster Declaration floods.[43] To determine the starting values for the county-level α and β parameters I make several assumptions. I assume that the realized Presidential Disaster Declarations over the 50 year period from 1958-2007 approximates the true national distribution of large county-level floods. The representative homeowner in 1958 knows the national county flood probability distribution, but doesn't know where his county is located in this distribution. Therefore, in 1958 the representative homeowner assumes that he is in the mean county from the national county flood distribution.[44]

[42] I plan to test this interpretation comparing the media effect by different distance intervals from the flood. This assumes that overall, communities closer to the flood are more similar to flooded communities than communities farther away. I also plan to use the flood maps to match on community characteristics to see if non-flooded communities in the same media market respond differently depending on how similar their underlying geographic characteristics are to the flooded communities.

[43] Note that I have not yet simulated the probabilities for the 3rd model (equation 1.8).

[44] [41] uses similar assumptions to determine homeowner initial beliefs over the probability of being diagnosed with cancer.

Under the above assumptions, I derive the starting values by matching the first two moments of the empirical county flood probability distribution of the 50 year Presidential Disaster Declaration history to the first two moments of the Beta Distribution. This gives two equations and two unknowns (the parameters α and β). Matching the first two moments: $\alpha = 2.87$ and $\beta = 21.87$. I use the same sample of US counties in matching these moments (N=2704) as are included in the baseline 1980-2007 event study regressions.[45] Figure 1.14 shows the empirical distribution of yearly county-level PDD flood probabilities from 1958-2007.

To generate a county level time series of yearly flood probabilities using the Discounted Beta-Bernoulli homeowner learning model (equation 1.7) I must also specify a value for δ. I use a two step process to determine the best fitting δ. First, I use equation (1.6) to generate 26 separate flood probability time series for each county under the initial starting values $\alpha = 2.87$ and $\beta = 21.87$, PDD flood data from 1958-2007, and $\delta = 0.80, 0.81, \ldots, 1.05$. Second, I select the time series of flood probabilities, $p(\delta)_{ct}$, that minimizes the mean square error of equation 1.9.[46]

$$ln(takeup_{ct}) = \alpha + \beta_t lnp(\delta)_{ct} + \alpha_c + \gamma_t + \epsilon_{ct} \tag{1.9}$$

Equation 1.9 is the same as event study estimating equation, except that here I replace the event time dummy variables with log flood probability.[47] The independent variable of interest is the Discounted Beta-Bernoulli flood probability, $p(\delta)_{ct}$. The flood probabilities are specific to a community, but vary only at the county level. A $\delta = .95$ best fits equation (1.9) using the 1990-2007 panel of communities. This is true regardless of whether equation (1.9) is specified with year or state by year fixed effects. I focus on the 1990-2007 event study panel since this allows for 32 years of "burn in" time before the first flood in the panel. The longer the history of flooding information used to generate flood beliefs, the less relevant are the values of the initial parameters in determining updated flood beliefs.[48]

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[45] The empirical moments are the same if I use the slightly larger number of counties included in the 1990-2007 panel.

[46] This two step process is equivalent to a single estimation procedure using non-linear least squares where I minimize over both β_t and δ simultaneously, except that I only consider 26 values for δ in the range $\delta = 0.80, 0.86, \ldots, 1.05$. I do not estimate δ to the 3rd decimal place.

[47] This assumes a Log-Log relationship between flood insurance take-up and the belief of a future flood. I also consider other specifications of this equation that include $lnp(\delta)^2_{ct}$ and $lnp(\delta)^3_{ct}$. The F Statistic for the estimation of equation 1.9 is consistently larger without the squared and cubed terms. The t Statistic on the coefficient for the squared and cubed terms is not statistically significant under most δ specifications.

[48] I plan to test the sensitivity of the starting values by using other starting value assumptions including: (i) Matching the moments of regional distributions (rather than the national distribution), and (ii) use each county's 50-year empirical mean as the first moment. Approach (i) assumes that homeowners know the county flood probability distribution for their region (e.g. Southeast US), but not where in this regional distribution their county is located. Approach (ii) assumes that homeowners know the "true" county flood probability in 1957 as approximated by the 1958-2007 empirical mean ($p_{i1957} = \frac{\alpha}{\alpha+\beta}$). Changing the numerical values of α and β, while keeping p_{i1957} fixed is analogous to changing the degree of certainty that homeowners have

I compare the homeowner learning models by observing how the simulated Beta-Bernoulli and Discounted Beta-Bernoulli (δ = .95) evolve after a PDD flood. I observe the event study time path of simulated probabilities under each model by estimating equation (1.1) using log simulated probabilities as the dependent variable. Figure 1.12 graphs the Beta-Bernoulli (δ=1) simulated probability event time coefficients. On the same graph I include the coefficient estimates for the specification of equation (1.1) with take-up as the dependent variable (i.e. figure 1.5). There is a 6.2% change in the classical Beta-Bernoulli probability in the year a community is hit by a PDD flood. Ten years after a flood, there is still a statistically significant 3.2% increase in the belief of a future flood, relative to the year before a PDD flood hit. The change in flood beliefs is 2.4% and statistically significant for the pooled coefficient for 11-17 years after a flood.

Figure 1.16 plots the event time coefficients from estimation of equation (1.1) with the Discounted Beta-Bernoulli (δ=.95) probability as the dependent variable. Again, figure 1.5 is plotted on the same graph for comparison. There is a 9.6% jump in the Discounted Beta-Bernoulli probability in the year of a PDD flood hit. Ten years after a flood the flood belief point estimate is 2.4% and statistically significant. The point estimate for the pooled 11-17 event year coefficient is 1.0% and not statistically significant.

Figures 1.15 and 1.16 suggest that homeowner learning model that allows for "forgetting"/ "first-hand experience" better fits the observed take-up in flood insurance. The flood insurance model implies that the demand for flood insurance should be positive if $dp_{ict} > 0$. The simulated probabilities from the classical Beta-Bernoulli (δ=1) learning model are positive for the entire event study, while flood insurance take-up is statistically zero after 10 years. However, the Discounted Beta-Bernoulli (δ=.95) probabilities are zero by the end of the event study.

A Beta-Bernoulli learning model with δ=.95 fits the observed take-up of flood insurance better than a model with δ=1. A Chi Square test rejects the (classical) Beta-Bernoulli learning model at the 1% significance level and fails to reject the Discounted Learning Model (δ=.95). I consider each hypothesized learning model to be the true model and the simulated probabilities from each model as data.

I test the fit of the insurance take-up coefficients with the simulated probabilities from the Discounted Beta-Bernoulli learning model under each value of $\delta \in [.80, .81, ..., 1.05]$ using the 1990-2007 panel. I consider the fit for event time years 1 to 11.[49] The χ^2 test statistic is: $\sum_{\tau=1}^{11} \frac{(\hat{q}_\tau - p_\tau)^2}{\hat{\sigma}_\tau^2}$, where $\hat{q}_\tau$ is the coefficient estimate for take-up for each year after a flood, p_τ is the simulated event time probability (given δ), and $\hat{\sigma}_\tau^2$ is the estimated variance for each

over their initial beliefs. I plan to generate updated flood beliefs using several pairs of values of α and β to represent different levels of homeowner certainty.

[49]Event time year 11 is the binned event time indicator for years 11-17. I don't include the year of a flood (τ= 0) in the χ^2 test results presented in the paper. The reason is that the estimated take-up coefficient in the year of the flood is likely to be biased downwards due to a mechanical delay in insurance take-up after a flood. The rejection results of the χ^2 test don't change if I include the coefficient from the year of a flood.

take-up coefficient.[50] The test statistic is (asymptotically) distributed χ^2 with 8 degrees of freedom.[51]

Figure 1.17 plots the χ^2 test statistic for the Beta-Bernoulli Discounted learning model under each value of δ. A line is drawn through the points to form a U-shaped curve. The minimum point on curve is when δ=.95. Two horizontal lines are drawn on the graph. The lower (upper) line is the critical value of the χ^2 statistic for rejection of the model at the 10% (1%) significance level. I can reject at the 1% level that the learning model with $\delta = 1$ fits the observed flood insurance take-up. I fail to reject models with δ between .91 and .98.

1.6 Conclusion

In this paper, I examine the learning process that economic agents use to update their expectation of an uncertain and infrequently observed event. In doing so, I compile a new and unique nationwide community-level panel dataset on large regional floods and the purchase of flood insurance policies. The logic of looking at flood insurance policies is that the decision to purchase flood insurance reveals changing beliefs over the expectation of future floods.

Flooding in the US is economically significant. Flood damages averaged $6 billion per year from 1955-1999 ([94]). Homeowner insurance policies explicitly exempt coverage for damage due to flooding. Homeowners in the US must decide each year whether to purchase a separate flood insurance policy. The combination of readily available statistical information, but infrequent personal experience, makes flooding a good context in which to study learning. The contribution of the paper is to test how well the standard Bayesian learning model fits observational data for an uncertain and infrequently observed event.

I estimate the *causal* effect that flooding has on the take-up of flood insurance using an event study framework that controls for the underlying propensity for a community to flood, and aggregate trends in flood insurance purchase. I find that there is an immediate rise in the fraction of people covered by flood insurance in a flooded community in the year of a flood. The effect peaks after one year at 9% (perhaps reflecting delayed adjustment)–then begins to steadily decline, fully dissipating after 9 years.

There is also strong evidence that homeowners in non-flooded communities react to a nearby flood by purchasing insurance. Insurance take-up in non-flooded communities is one-third as large as in flooded communities provided the non-flooded community is in the same media market as a flooded community. There is no effect on take-up in geographically close communities not in the same media market.

I interpret these findings using three different models of homeowner learning. The first learning model I consider is a Beta-Bernoulli Bayesian model. The Beta-Bernoulli Bayesian

[50] The Chi Square Test Statistic assumes that the covariance between moments of the post-flood impulse response function are zero.

[51] There are 11 moments used in estimation and 3 parameters, α, β, δ in equation (9), giving 8 degrees of freedom.

model implies that as the stock of information increases, the effect of a new flood becomes small (and eventually zero). The second learning model, a modified Beta-Bernoulli model, accounts for the possibility that homeowners discount past information when updating beliefs. The third learning model is motivated by the event study results that show that homeowners update their belief of a future flood using information about floods in other communities that are flooded.

I test the first two learning models by simulating changes in homeowner beliefs using the flood data over 50 years. A learning model with a discount parameter equal to .95 best fits take-up as a function of the simulated probabilities via non-linear least squares. Using a Chi Square test, I can reject that the (standard) Beta-Bernoulli Bayesian model fits observed take-up at the 1% significance level. I conclude that a homeowner learning model that allows past information to be discounted is most consistent with observed homeowner insurance take-up.

The findings of this paper suggest two specific policy implications. First, collectively homeowners have a short memory and don't appear to equally weigh all past flood information. If a policy goal is for homeowners to self-insure against floods, then offering multi-year insurance contracts or opt out contracts could help prevent homeowners from dropping their insurance after years when there is no flood ([68]). Second, there is clear evidence that the news media is a channel through which homeowners "learn" about their flood risk even when they are not directly impacted by a flood. This suggests that an information campaign could be successful in increasing aggregate flood insurance penetration rates. However, the information campaign would need to consistently emphasize flood risks since the lasting effects of the "learning" for homeowners who don't experience floods is short.

There are at least two immediate extensions to this paper. First, do homeowners forget at different rates based on personal experience? The flood insurance policy data I use to test the learning model are aggregated at the community-level. I can't distinguish between different policy holders in the community. The next step is to test whether take-up after a flood is different in communities with different migration rates. This will give an indication as to whether take-up varies by the proportion of residents who have personal experience with a previous flood. The hypothesis is that homeowners with first-hand experience are *less* likely to forget. The implication of less forgetting is a lower initial take-up response in the year of a flood and a slower decay of this response in the years following the flood. Ideally, I would like to access household level data on flood policy holders. I am in conversation with the National Flood Insurance Program officials to be able to access a recent sub-sample of policy data that would enable me to distinguish between new and continuing insurance policies and the length of residence.

The second extension is to further explore the empirical finding that homeowners in *non-flooded* communities also take-up insurance after a flood, provided they live in the same media market as a community that is flooded. I plan to use community level map characteristics, historical weather data, and the correlation in past floods to examine whether homeowners in non-flooded communities respond differently to a nearby flood based on how similar the flooded communities are to their community.

Table 1.1: Community Flood Insurance Statistics and Flood Map Characteristics

Year	**1980**	**1990**	**2007**
Panel A: Community Policies Per 1,000 Persons			
Communities < Median 100 Year Flood Plain	7 (1)	4 (1)	8 (2)
Communities > Median 100 Year Flood Plain	21 (2)	22 (2)	35 (4)
Flood Map Designation	**100 Year**	**100-500 Year**	**Outside Flood Plain**
Panel B: % Communities by Flood Designation			
Percent of Community	14 (8)	87 (77)	4 (0)
Sample of Communities	**All**	**< Median 100 Yr**	**> Median 100 Yr**
Panel C: % Communities "Hit" by a PDD			
Percent "Hit" (receiving Public Assistance)	32	29	35

Figure 1.1: Increasing Trend Line for Flood Insurance in US

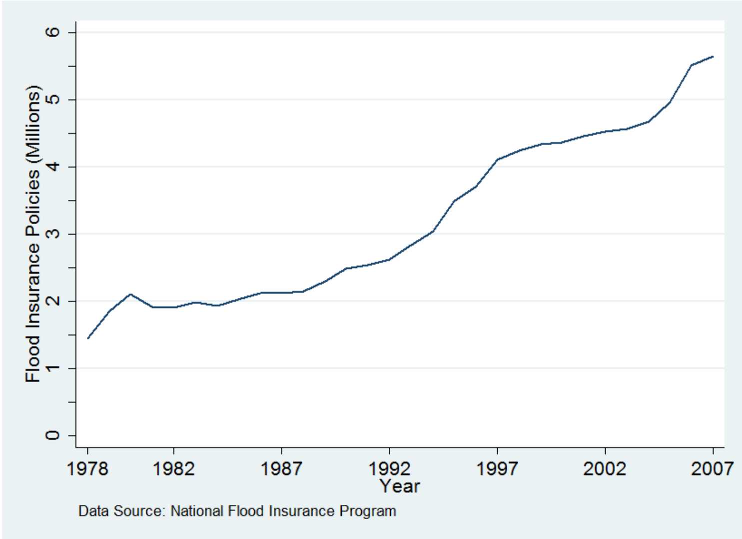


Table 1.2: Event Time Estimation for Panel 1990-2007, Hit and Not Hit Communities

	(1)	(2)	(3)	(4)
Panel A: Community in a Presidential Disaster Declaration County and Hit by the Flood				
Year of Flood	0.1038 (0.0164)***	0.0680 (0.0162)***	0.0791 (0.0103)***	0.0683 (0.0090)***
1 Year after Flood	0.1080 (0.0178)***	0.0769 (0.0125)***	0.0914 (0.0082)***	0.0748 (0.0078)***
2 Years after Flood	0.1235 (0.0192)***	0.0866 (0.0135)***	0.0837 (0.0101)***	0.0699 (0.0080)***
3 Years after Flood	0.1006 (0.0206)***	0.0746 (0.0150)***	0.0705 (0.0113)***	0.0565 (0.0102)***
4 Years after Flood	0.0931 (0.0144)***	0.0576 (0.0147)***	0.0753 (0.0117)***	0.0559 (0.0133)***
5 Years after Flood	0.0714 (0.0135)***	0.0382 (0.0158)**	0.0707 (0.0121)***	0.0505 (0.0137)***
6 Years after Flood	0.0573 (0.0148)***	0.0298 (0.0149)**	0.0625 (0.0123)***	0.0500 (0.0122)***
7 Years after Flood	0.0566 (0.0149)***	0.0371 (0.0166)**	0.0568 (0.0130)***	0.0497 (0.0121)***
8 Years after Flood	0.0561 (0.0170)***	0.0304 (0.0163)*	0.0594 (0.0153)***	0.0488 (0.0135)***
9 Years after Flood	0.0143 (0.0158)	-0.0069 (0.0174)	0.0321 (0.0160)**	0.0224 (0.0146)
10 Years after Flood	-0.0088 (0.0177)	-0.0239 (0.0178)	0.0227 (0.0163)	0.0106 (0.0165)
11-17 Yrs after Flood	-0.0126 (0.0220)	-0.0139 (0.0227)	0.0132 (0.0202)	0.0236 (0.0189)
Panel B: Communities in a Presidential Disaster Declaration County and Not Hit by the Flood				
Year of Flood		0.0399 (0.0148)***		0.0179 (0.0091)**
1 Year after Flood		0.0328 (0.0163)**		0.0267 (0.0106)**
2 Years after Flood		0.0400 (0.0172)**		0.0220 (0.0114)*
3 Years after Flood		0.0273 (0.0177)		0.0223 (0.0119)*
4 Years after Flood		0.0407 (0.0186)**		0.0321 (0.0138)**
5 Years after Flood		0.0383 (0.0187)**		0.0338 (0.0146)**
6 Years after Flood		0.0326 (0.0161)**		0.0212 (0.0149)
7 Years after Flood		0.0237 (0.0137)*		0.0121 (0.0148)
8 Years after Flood		0.0323 (0.0133)**		0.0199 (0.0146)
9 Years after Flood		0.0311 (0.0124)**		0.0202 (0.0154)
10 Years after Flood		0.0249 (0.0120)**		0.0249 (0.0148)*
11-17 Yrs after Flood		0.0237 (0.0137)*		0.0116 (0.0108)
Pre-Flood Indicators	X	X	X	X
Community FE	X	X	X	X
Calender Year FE	X	X		
State by Year FE			X	X
Observations	191,970	191,970	191,970	191,970
Communities	10,665	10,665	10,665	10,665
R-Squared	.1737	.1774	.2180	.2191

Note that I first demean all of the data using a community fixed effect transformation. Each column contains coefficients from 4 separate regressions of equation (1') on the community demeaned data. Standard errors are corrected for the reduced number of degrees of freedom. Columns (2) and (4) include yearly event time indicator variables for communities in PDD counties not hit by the flood. The pre-flood indicator variables for hit and non-hit communities are included in the estimating equation, but excluded from the table for space considerations. None of the pre-flood coefficients are significant at the 5% level. Standard errors clustered at state level. Significance level: *** 1%, ** 5%, * 10%

Table 1.3: Event Time Estimation for Panel 1980-2007, Communities in Flooded Counties

	(1)	(2)	(3)
Panel A: Years Before a Community is Located in a Presidential Disaster Declaration Flooded County			
2 Years before Flood	0.0113 (0.0217)	0.0009 (0.0162)	0.0146 (0.0149)
3 Years before Flood	0.0248 (0.0213)	0.0060 (0.0167)	0.0185 (0.0151)
4 Years before Flood	0.0291 (0.0212)	0.0100 (0.0190)	0.0226 (0.0174)
5 Years before Flood	0.0162 (0.0209)	0.0066 (0.0197)	0.0191 (0.0183)
6 Years before Flood	0.0125 (0.0193)	0.0084 (0.0185)	0.0204 (0.0167)
7 Years before Flood	0.0061 (0.0167)	0.0092 (0.0184)	0.0212 (0.0163)
8 Years before Flood	0.0130 (0.0176)	0.0090 (0.0174)	0.0212 (0.0150)
9 Years before Flood	0.0020 (0.0145)	-0.0009 (0.0155)	0.0102 (0.0129)
10 Years before Flood	-0.0101 (0.0137)	-0.0054 (0.0158)	0.0057 (0.0132)
11 Years before Flood	-0.0015 (0.0140)	-0.0031 (0.0161)	0.0084 (0.0136)
12 Years before Flood	0.0036 (0.0142)	0.0035 (0.0161)	0.0147 (0.0137)
13 Years before Flood	0.0055 (0.0133)	0.0058 (0.0143)	0.0172 (0.0133)
14 Years before Flood	0.0082 (0.0183)	0.0050 (0.0160)	0.0160 (0.0151)
15 Years before Flood	0.0040 (0.0116)	0.0150 (0.0119)	0.0253 (0.0110)**
16-27 Yrs before Flood	-0.0110 (0.0309)	0.0258 (0.0216)	0.0366 (0.0221)
Panel B: Years After a Community is Located in a Presidential Disaster Declaration Flooded County			
Year of Flood	0.0903 (0.0260)***	0.0570 (0.0165)***	0.0711 (0.0158)***
1 Year after Flood	0.0921 (0.0258)***	0.0744 (0.0173)***	0.0871 (0.0170)***
2 Years after Flood	0.1032 (0.0287)***	0.0702 (0.0188)***	0.0828 (0.0185)***
3 Years after Flood	0.0838 (0.0274)***	0.0593 (0.0181)***	0.0767 (0.0174)***
4 Years after Flood	0.0832 (0.0255)***	0.0594 (0.0187)***	0.0765 (0.0177)***
5 Years after Flood	0.0729 (0.0264)**	0.0577 (0.0205)***	0.0797 (0.0194)***
6 Years after Flood	0.0617 (0.0265)**	0.0444 (0.0215)**	0.0656 (0.0192)***
7 Years after Flood	0.0498 (0.0247)**	0.0342 (0.0205)*	0.0582 (0.0173)***
8 Years after Flood	0.0537 (0.0221)**	0.0334 (0.0172)**	0.0548 (0.0153)***
9 Years after Flood	0.0398 (0.0209)*	0.0269 (0.0199)	0.0478 (0.0165)***
10 Years after Flood	0.0245 (0.0180)	0.0269 (0.0186)	0.0462 (0.0161)***
11 Years after Flood	0.0267 (0.0183)	0.0189 (0.0160)	0.0374 (0.0150)**
12 Years after Flood	0.0088 (0.0183)	0.0032 (0.0175)	0.0287 (0.0144)*
13 Years after Flood	-0.0041 (0.0195)	0.0003 (0.0179)	0.0283 (0.0140)**
14 Years after Flood	0.0182 (0.0219)	0.0085 (0.0183)	0.0322 (0.0151)**
15 Years after Flood	0.0112 (0.0160)	-0.0059 (0.0184)	0.0242 (0.0141)*
16-27 Yrs after Flood	-0.0390 (0.0367)	-0.0194 (0.0288)	0.0049 (0.0165)
Controls 1970 Floods			X
Community FE	X	X	X
Calender Year FE	X	X	
State by Year FE			X
Observations	265,412	265,412	265,412
Communities	9,479	9,479	9,479
R-Squared	.1503	.2038	.2052

Table 1.4: Event Time Estimation, Panel 1980-2007, Communities in Neighboring Counties

	(1)	(2)	(3)	(4)
Panel A: Take-up for Communities in Presidential Disaster Counties				
Year of Flood	0.0680 (0.0146)***	0.0806 (0.0151)***	0.0822 (0.0156)***	0.0822 (0.0155)***
Panel B: Take-up in Communities Neighboring Presidential Disaster Counties				
Geographic Neighbor				
Year of Flood	0.0128 (0.0075)*		0.0016 (0.0077)	-0.0065 (0.0135)
1 Year after Flood	0.0147 (0.0077)*		0.0018 (0.0080)	0.0030 (0.0103)
2 Years after Flood	0.0177 (0.0075)**		0.0063 (0.0074)	0.0133 (0.0095)
3 Years after Flood	0.0147 (0.0085)*		0.0016 (0.0080)	0.0188 (0.0110)*
4 Years after Flood	0.0077 (0.0095)		-0.0051 (0.0093)	0.0102 (0.0127)
5 Years after Flood	0.0069 (0.0087)		-0.0060 (0.0088)	-0.0037 (0.0110)
6 Years after Flood	0.0016 (0.0075)		-0.0055 (0.0079)	-0.0072 (0.0095)
7 Years after Flood	0.0013 (0.0092)		-0.0036 (0.0091)	-0.0027 (0.0121)
8 Years after Flood	0.0029 (0.0096)		-0.0033 (0.0106)	0.0028 (0.0130)
9 Years after Flood	0.0025 (0.0093)		-0.0042 (0.0106)*	0.0026 (0.0122)
10 Years after Flood	-0.0028 (0.0103)		-0.0064 (0.0124)	0.0013 (0.0123)
Media Neighbor				
Year of Flood		0.0294 (0.0064)***	0.0294 (0.0061)***	0.0278 (0.0064)***
1 Year after Flood		0.0325 (0.0052)***	0.0324 (0.0051)***	0.0326 (0.0049)***
2 Years after Flood		0.0311 (0.0060)***	0.0290 (0.0056)***	0.0304 (0.0056)***
3 Years after Flood		0.0333 (0.0075)***	0.0327 (0.0070)***	0.0358 (0.0070)***
4 Years after Flood		0.0278 (0.0082)***	0.0296 (0.0078)***	0.0323 (0.0080)***
5 Years after Flood		0.0263 (0.0093)***	0.0284 (0.0098)***	0.0290 (0.0101)***
6 Years after Flood		0.0126 (0.0078)	0.0146 (0.0086)*	0.0145 (0.0087)*
7 Years after Flood		0.0066 (0.0085)	0.0081 (0.0087)	0.0083 (0.0089)
8 Years after Flood		0.0091 (0.0102)	0.0104 (0.0112)	0.0116 (0.0116)
9 Years after Flood		0.0113 (0.0091)	0.0127 (0.0104)	0.0137 (0.0107)
10 Years after Flood		0.0049 (0.0097)	0.0074 (0.0114)	0.0084 (0.0117)
Geographic and Media Neighbor				
Year of Flood				0.0099 (0.0113)
1 Year after Flood				-0.0019 (0.0097)
2 Years after Flood				-0.0095 (0.0085)
3 Years after Flood				-0.0238 (0.0105)**
4 Years after Flood				-0.0222 (0.0108)**
5 Years after Flood				-0.0040 (0.0103)
6 Years after Flood				0.0021 (0.0073)
7 Years after Flood				-0.0022 (0.0113)
8 Years after Flood				-0.0120 (0.0122)
9 Years after Flood				-0.0130 (0.0095)
10 Years after Flood				-0.0153 (0.0091)
Controls 1970 Floods	X	X	X	X
Community FE	X	X	X	X
State by Year FE	X	X	X	X
Observations	265,412	265,412	265,412	265,412
Communities	9,479	9,479	9,479	9,479
R-Squared	.2060	.2061	.2070	0.2071

Figure 1.2: Histogram of Presidential Disaster Declarations by County 1990-2007

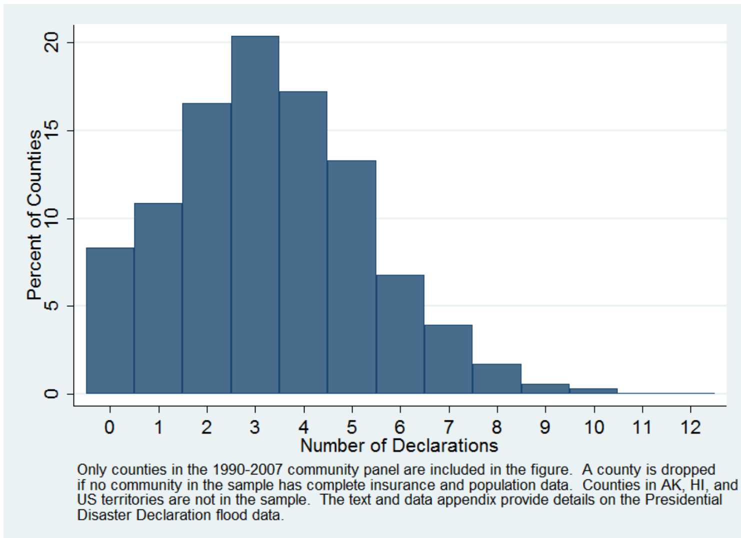


Figure 1.3: US Map with shades of red for county PDD Intensity

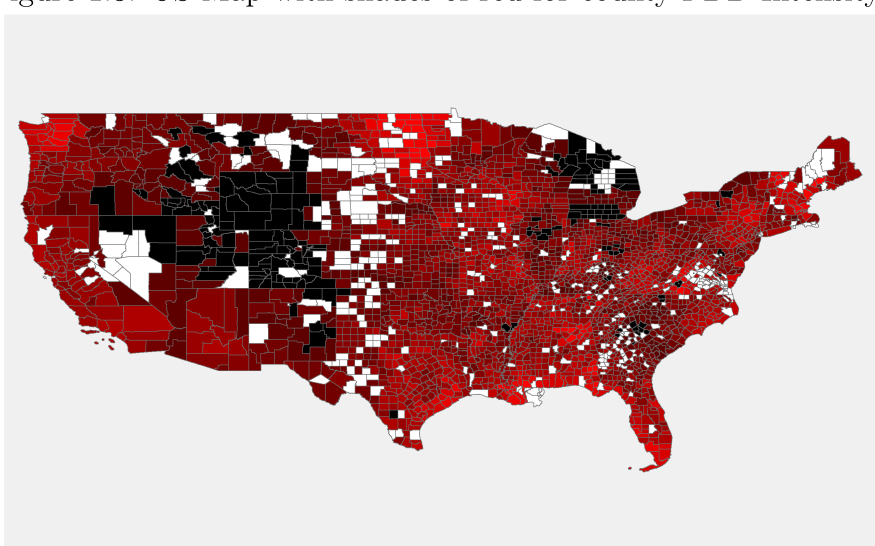


Figure 1.4: Mean Per Capita County Flood Cost 1969-2004 (All US)

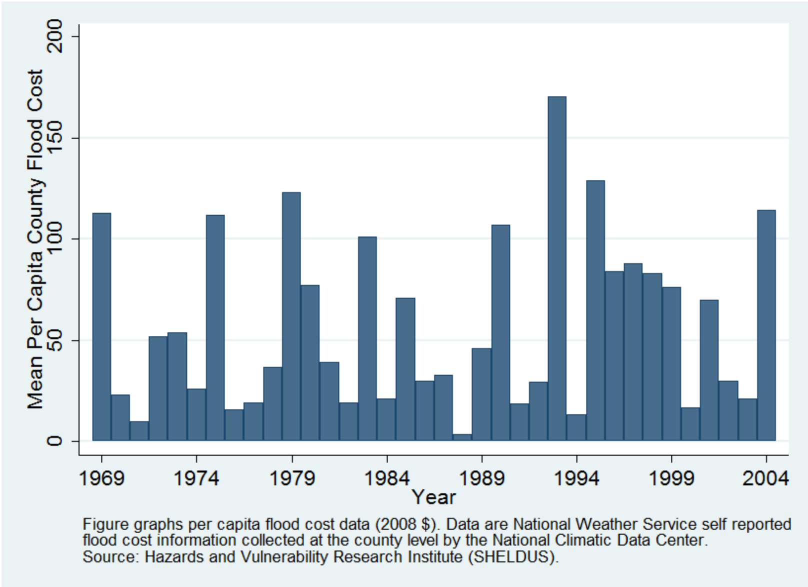


Figure 1.5: Community Flood Insurance Take-up After Hit Disaster Declaration Flood 1990-2007

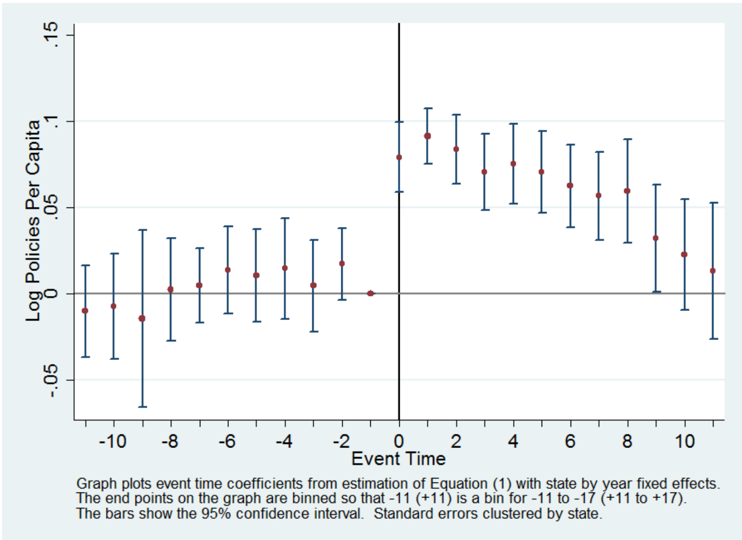


Figure 1.6: Insurance Take-up after Floods 1990-2007, Hit and Non-Hit PDD Communities

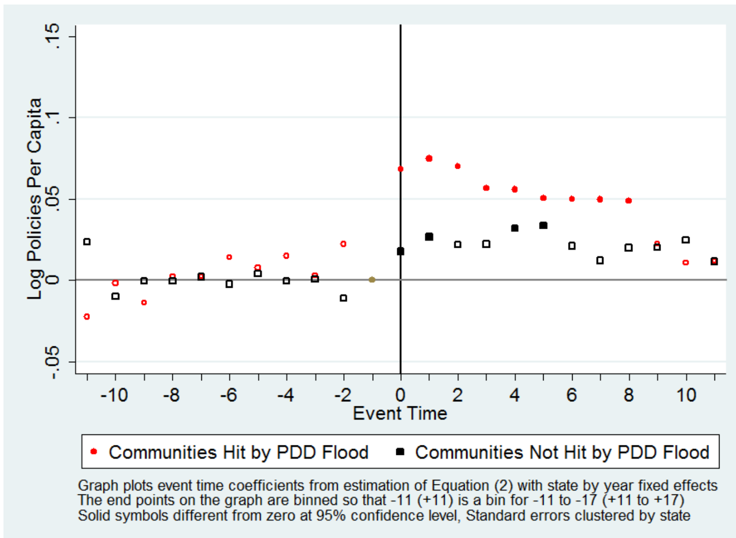


Figure 1.7: Take-up after Floods 1990-2007 by Above and Below Median Cost

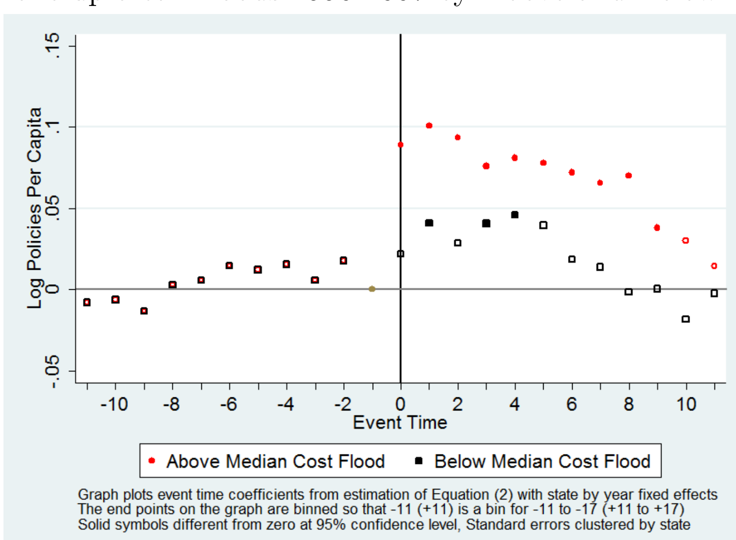


Figure 1.8: Community Flood Insurance Take-up after Hit by PDD Floods 1980-2007

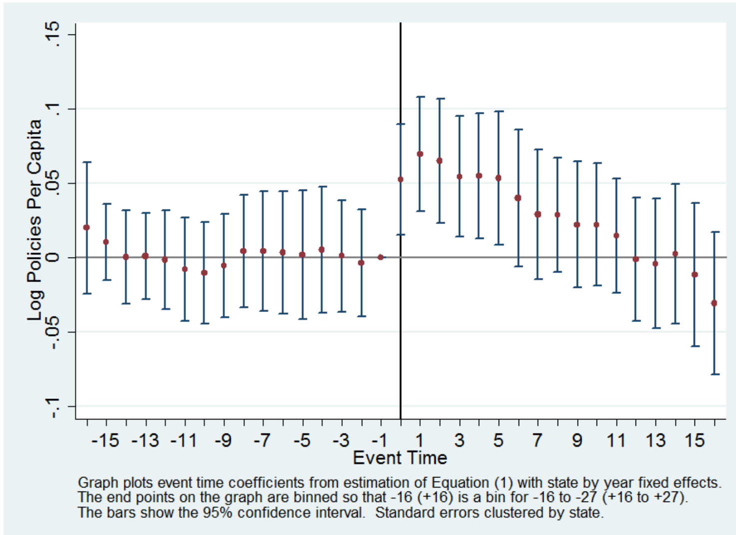


Figure 1.9: Take-up after Floods 1980-2007, PDD Communities and Geographic Neighbors

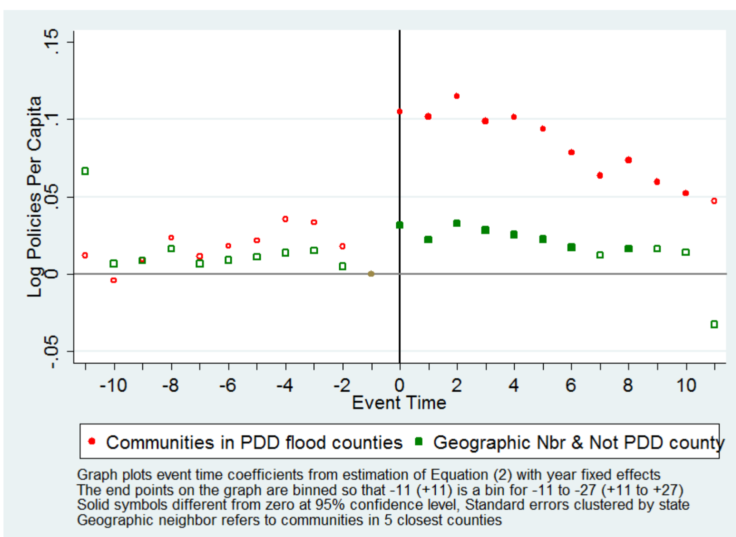


Figure 1.10: Take-up after Floods 1980-2007, PDD Communities and Media Neighbors

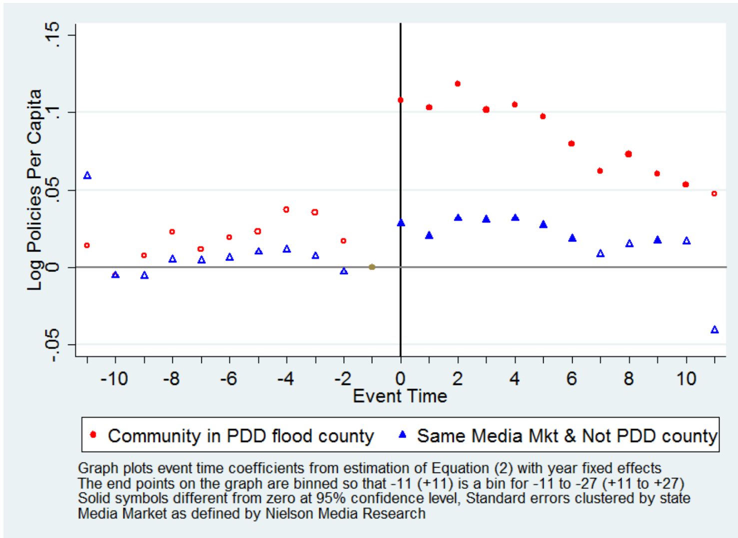


Figure 1.11: Take-up after Floods 1980-2007, PDD Communities, Distance and Media Neighbors

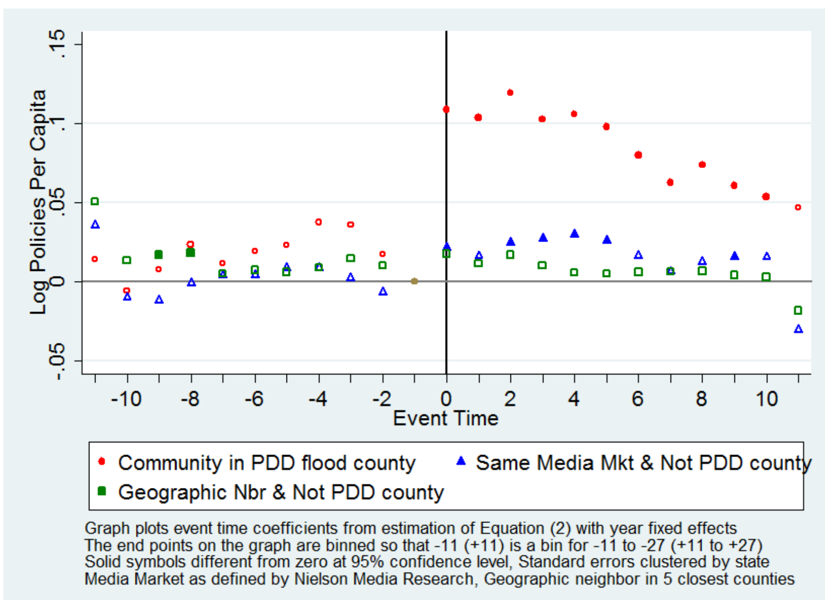


Figure 1.12: Homeowner Flood Insurance Demanded as a Function of Flood Beliefs

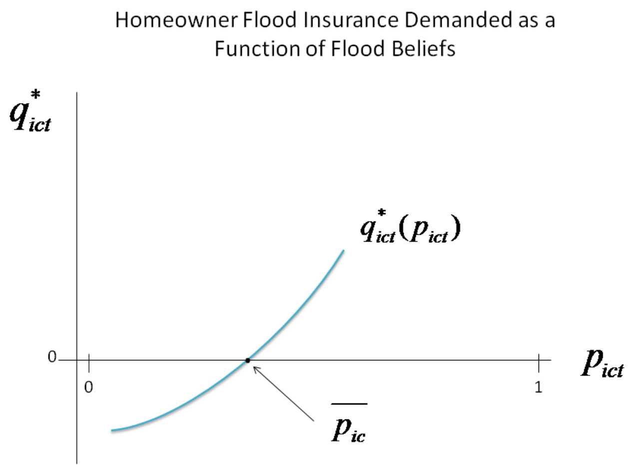


Figure 1.13: Number of Community Flood Insurance Policies Demanded as a Function of Flood Beliefs

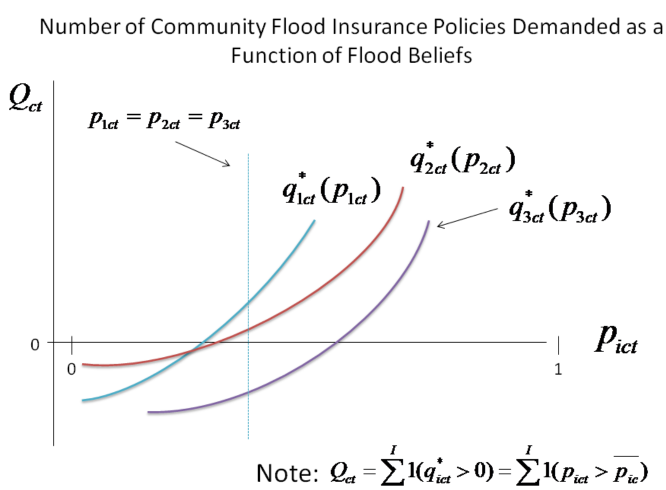


Figure 1.14: Distribution of US Counties by Likelihood of Flooding Disaster Declaration from 1958-2007

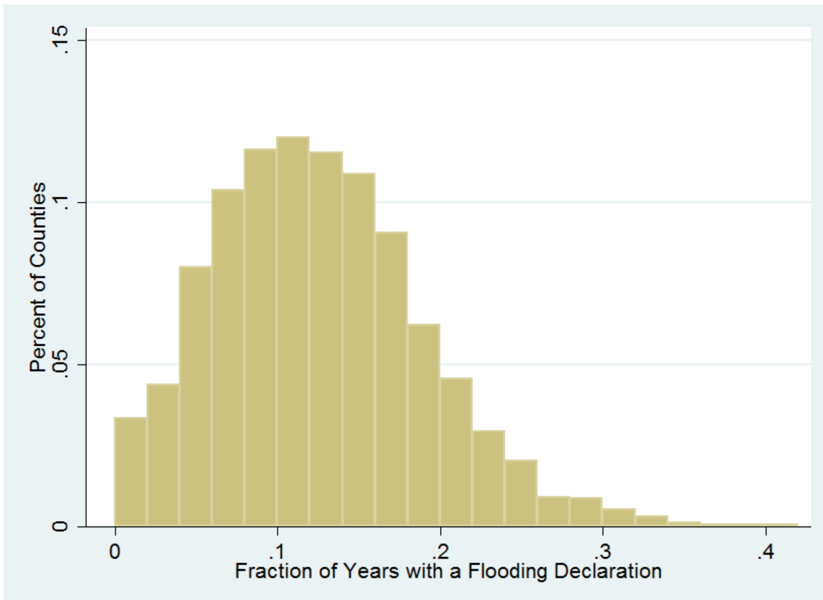


Figure 1.15: Flood Probabilities and Insurance Take-up after Disaster Declaration Floods 1990-2007

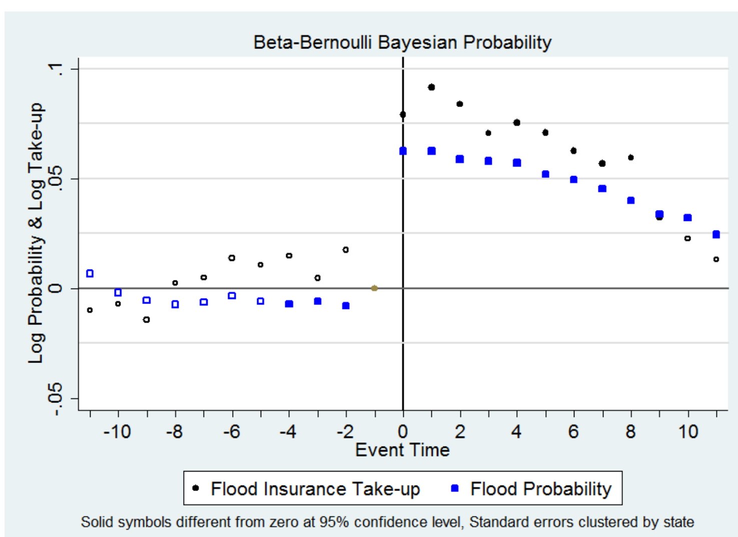


Figure 1.16: Flood Probabilities and Insurance Take-up after Disaster Declaration Floods 1990-2007

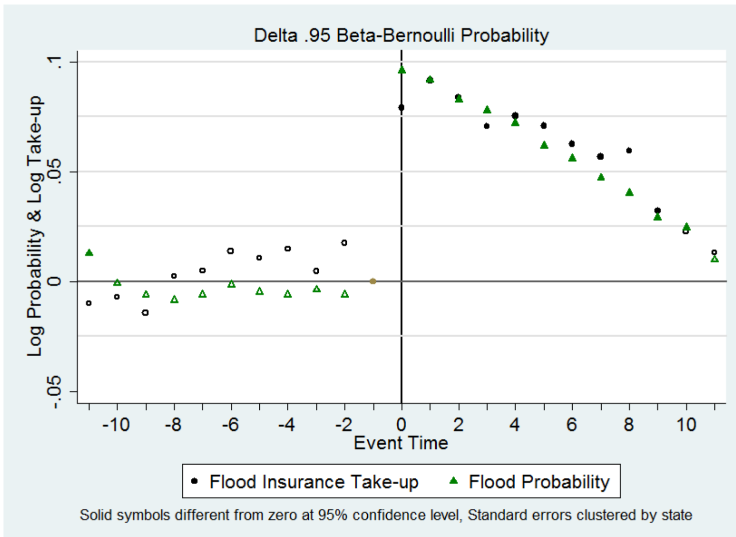
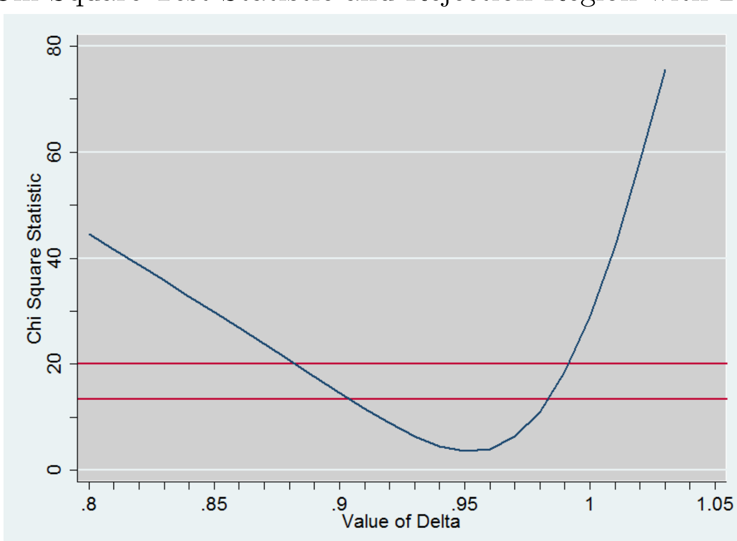


Figure 1.17: Chi Square Test Statistic and Rejection Region with Delta 0.80-1.05



Chapter 2

Motor Vehicle Air Pollution and Infant Health

2.1 Introduction

The goal of this paper is to examine the causal link between localized exposure to hazardous pollutants from motor vehicle exhaust and adverse health outcomes. Motor vehicles release Carbon Monoxide (CO) and fine ($< 2.5\mu m$) and ultrafine ($< 0.1\mu m$) particulate matter which tend not to mix with other compounds. These pollutants are thought to have a very localized pollution distribution around the emission source.[1]

Air pollution is a public good. The costs of polluting are often not considered when consumers are making driving decisions or when firms are making production decisions. The likely result is too much air pollution. The US Federal Government has restricted air pollution through a number of measures including technology mandates (e.g. catalytic converter), fuel restrictions (e.g. unleaded gasoline), and pollution emission standards (e.g. Federal Tier 1 and Tier 2 legislation).

Historically, concern over vehicle pollution has focused on its contribution to the total level of emissions and the effect on ambient air quality. Motor vehicle exhaust contributed an estimated 44% of the total CO emissions in the US in the year 2000 ([6]). More recently there has been increased concern over the contribution of vehicle pollution to global climate change.[2] Scientific studies have estimated that a significant portion of CO and particulate pollution from motor vehicles remains within a short distance of the roadway ([62]). Thus

[1] For example, the Environmental Protection Agency models vehicle pollution from roadways using the Hybrid Roadway Model (HYROAD). This model uses an impact distance of 500 meters. For documentation: $http://www.epa.gov/scram001/dispersion\_alt.htm$

[2] In 2002 the California State Legislature passed AB 1493 "Pavley Global Warming Bill" requiring future reductions in greenhouse gas emissions from new motor vehicles ([4]). In 2007, the US Supreme Court ruled that the US Environmental Protection Agency could regulate green house gas emissions from motor vehicles ([8])

there is the potential for differential exposure to substantial levels of air pollution, even among residents living in adjacent neighborhoods.

I use an exogenous natural event-the 1994 Northridge Earthquake in Los Angeles County, CA-to test whether the reassignment of motor vehicle pollution after households have made their location decision has any impact on infant health outcomes. The Northridge Earthquake damaged portions of several major highways and altered vehicle traffic in Los Angeles. One of the highways damaged, Interstate 10, was the most trafficked roadway in the world at the time of the earthquake with an estimated 261,000 vehicles a day. Residents living along the alternative driving routes were exposed to elevated levels of vehicle air pollution during the 3 months it took to repair Interstate 10. Residents living near to the closed portion of Interstate 10 were exposed to less vehicle air pollution.

Overall, the results of the study are inconclusive. The empirical estimates of the effect of changing traffic volumes on infant birth health are imprecise. However, it does appear that any localized effect of vehicle pollution on infant health in Los Angeles during the early 1990's is of much less importance than measured correlations between infant health and a mother's race, education, or age.

2.2 Brief Review of the Literature

The field of epidemiology has long been interested in the association between air pollution and human health. For example, [87] finds that hospital emissions for repertory illnesses increase dramatically when a local steel mill is open and operating. [78] find that residents living near major point source emissions sources are more than 100% more likely to have an asthmatic episode.

A more recent sub-literature has focused on the association between ambient air pollution and infant health (e.g. [103]; [77]; [102]). These studies have linked ambient air pollution to low birth weight (LBW), preterm birth, and perinatal mortality. Most of these studies fail to consider the endogenous location decision of households and how this could bias the health results.

One of the most prominent study examining the link between localized motor vehicle pollution and human health is [102]. Wilhelm and Ritz create a distance-weighted measure of exposure to air pollutants due to the proximity between each residence and all major roadways. The study examines the complete universe of low birth weight (LBW) and preterm births within Los Angeles County, California from 1994-1996. Wilhelm and Ritz use their distance-weighted measure of exposure to compare those mothers with LBW or preterm births to a randomly selected control group from the same zip codes. The primary finding of the study is that there is an 8% increase in the risk of having a preterm birth if the mother lived in housing within the highest quintile of vehicle emissions exposure (relative to the lowest quintile) at the time of giving birth.

However, [102] has several significant shortcomings. First, economic theory predicts that individuals will select where to live by which housing location best matches their preferences over housing attributes and neighborhood amenities. In particular, standard economic models suggest that individuals living near heavily trafficked roads may be very different than individuals who live in the same neighborhoods (i.e. zip codes), but much farther away from these roads (e.g. [93]; [66]). Further, if a study doesn't control for all individual, housing, and neighborhood characteristics that affect housing decisions then conventional estimation techniques that rely on controlling for covariates are likely to give biased estimates. A large economic literature demonstrates that biased estimation is an important practical consideration in housing location studies (e.g. [26]; [54]).

A growing literature of influential papers in economics has examined the causal link between air pollution and infant mortality (e.g. [32]; [33]; [39]; [40]). These papers allow for individuals to select their housing based on the existing level of air pollution. Exogenous variation in the level of ambient air pollution is then used to identify changes in health outcomes. These studies all find a causal relationship between ambient air pollution and human health. This paper follows in the spirit of these economic studies, while focusing on the local differential impact of motor vehicle emissions on human health.

2.3 Northridge Earthquake as a Quasi-Experimental Research Design

On January 17, 1994 the Northridge Earthquake measuring 6.8 on the Richter scale hit the city of Los Angeles. The earthquake injured over 9,000 people and cost an estimated $44 billion in economic damage ([86]). The earthquake also shut down portions of four major highways that traverse Los Angeles, thereby disrupting traffic flows and shifting hundreds of thousands of vehicles to alternate driving routes. Seven miles of Interstate 10 (I-10) was shut down for approximately 3 months. Additionally, portions of I-5, SR-118, and SR-14 were closed for periods ranging from one to six months.

The Northridge Earthquake unexpectedly altered the exposure of a subpopulation of Los Angeles County residents to much different levels of vehicle air pollution. Figure 2.1 shows Interstate 10 (I-10) and the post-quake detours ([84]). Prior to the earthquake this section of I-10 was the most trafficked roadway in the world with an estimated 261,000 vehicles a day. I-10 was reopened 85 days after the earthquake. During this time, there was an official eastbound detour on Jefferson Boulevard and a westbound detour on Venice Boulevard. Both detours used 3 miles of city streets. The Department of Transportation determined that 42% of the pre-quake traffic on I-10 used the primary detours.

Figure 2.1 also shows the Westbound SR-118 Detours ([84]). Prior to the earthquake, there were approximately 121,000 vehicles per day on SR-118 (measured just west of the SR-118 and I-405 interchange). SR-118 was closed for 1-month. During this time approximately 50% of the traffic used one of the three detours. There were three westbound detours.

Nordhoff St. was used for traffic from the south. Devonshire St. was used by traffic traveling west on SR-118. Rinaldi St. was used for traffic from the north. All three detours rerouted traffic back to SR-118 between Tampa Ave and Reseda Blvd.

Figure 2.2 shows the I-5 and SR-14 detours ([84]). This portion of I-5 was closed for 4 months. Pre-earthquake traffic on I-5 north of SR-14 was 133,000 vehicles per day. During the 2 weeks immediately following the earthquake all of the traffic was diverted onto Lyons Ave and San Fernando Road. After the initial 2 weeks a detour was established on The Old Road. This detour ran parallel to I-5 and carried between 88,000 and 97,000 vehicles per day. After The Old Road detour opened, the traffic flow on San Fernando Road stabilized at 22,000 vehicles a day (up from 3,900 vehicles a day before the quake). The SR-14 detour included northbound traffic on Foothill Boulevard.

Ideally there would be pollution monitoring data that could verify that the reallocation of traffic led to changes in the pollution levels. Unfortunately, no monitoring stations were located along any of the roadways that received an increase or decrease in traffic flow after the earthquake.[3] A significant weakness of this research design is not having actual air pollution measurements .[4]

Another potential concern with this research design is that the earthquake led to substantial housing relocation immediately after the earthquake. This could be the case if homes were destroyed or if the earthquake changed the demand for particular housing locations. Surprisingly, relatively few homes were destroyed by the earthquake. Additionally, there are a large number of fault lines in LA County. Thus, both forced relocation and immediate demand changes such as relocation away from fault lines appear not to be a significant issue for this study.

The statistical power to detect birth weight and gestation period changes is the largest concern. This will become more clear when discussing the estimation results. The low statistical power comes from the relatively low number of births by mothers in very close proximity to the effected portion of the highways. This is despite the fact that the most trafficked highway in the world had traffic diverted through the second largest city in the US. The speed with which the highways were repaired further reduced the number of births potentially effected by changes in vehicle pollution exposure.

[3]There is one monitoring station (Newhall) that is ideally situated, but it didn't begin collecting data until 1999.

[4]I plan to contact the California Department of Transportation in the hope that there may have been temporary monitoring of vehicle emissions during the period of traffic redirection. An alternative possibility is to use another event that shutdown traffic on a major highway near to a permanent monitoring station. I could use this event as a benchmark for how changes in traffic volume would be expected to effect local (measurable) pollutant levels in Los Angeles.

2.4 Data

The infant health outcomes of interest are low birth weight (LBW) and preterm birth. These health outcomes are selected for a number of reasons. First, infants are one of the most vulnerable segments of the population (World Health Organization 2006). Second, infants born preterm or with LBW are less likely to survive infancy, more likely to suffer from childhood illness, and have lower future earnings ([73]; [27]). Third, it is potentially easier to establish a causal link between adverse health outcomes and pollution for infants because of the much shorter history of pollution exposure. Finally, these outcomes are recorded on birth certificates and available for a large segment of the population.

The data used in this paper come from two sources. Most birth certificate information is from the 1993 and 1994 California Birth Cohort Files.[5] These files include birth records for all births in California. I requested permission to access birth records from zip codes in Los Angeles County that are in the vicinity of the highways damaged from the Northridge Earthquake.[6] Information available in the Birth Cohort Files and used as data for this project include: birth date, birth weight, gestation period, mother's education, mother's race, and mother's age.

The second data source are birth record files from LA County. These records are separate from the California Birth Cohort Files, and unlike the state files, include the mother's address at the time she gave birth. I use unique birth record numbers included in both the state and county files to link the mother's address at the time of birth with the more detailed birth information from the Cohort Files.

GIS software is used to determine the distance from each mother's home address to each of the major highways damaged by the Northridge Earthquake. I also calculate the distance from each address to the detour routes established after the earthquake.[7] There are 25,897 births in the selected Los Angeles zip codes in 1993. Of these, 24,408 (94.3%) have address information for the mother. I am able to geocode 21,424 addresses (82.7% of total births). For 1994, there are 24,108 births. Address information is non-missing for 22,650 (94.0%) of the births. I am able to geocode 19,904 addresses (82.3%).

[5] In order to access these files I had to first receive permission from the California Committee for the Protection of Human Subjects (CPHS).

[6] Birth records were requested from the following zip codes: 90005, 90006, 90006, 90007, 90008, 90011, 90014, 90015, 90016, 90018, 90019, 90021, 90025, 90034, 90035, 90037, 90062, 90064, 90066, 90089, 90230, 90232, 90401, 90403, 90404, 91040, 91202, 91206, 91207, 91208, 91303, 91304, 91306, 91307, 91311, 91321, 91324, 91325, 91326, 91330, 91331, 91335, 91340, 91342, 91343, 91344, 91345, 91350, 91351, 91352, 91354, 91355, 91356, 91367, 91381, 91401, 91402, 91403, 91405, 91406, 91411, 91423, 91436, 91501, 91502, 91504, 91505, 91506, 91523, 91601, 91602, 91605, 91606, 91607.

[7] I would like to thank UC Berkeley's Geospatial Information Facility (GIF), and in particular, Kevin Koy and Jeremy Freund for their assistance in calculating these distances.

2.5 Empirical Specifications

I first discuss the preferred econometric approach to estimating the relationship between infant birth outcomes and exposure to vehicle pollution. This approach is laid out by the following equation:

$$weight_{ijd} = 1(quake_{ijd})\delta + X^{'}\beta + \gamma_i + \rho_j + \epsilon_{ijd} \tag{2.1}$$

The dependent variable of interest in equation (2.1) is infant birth weight in grams born to mother i, in birth order j, where the mother's home address at the time of birth is within interval d from the highway. I also consider gestation period (in days) and the probability of having a birth of dangerously low weight (LBW) as other outcome variables. I follow previous literature and define LBW as a birth weight of less than 2,500 grams. I estimate the version of equation (2.1) with LBW as the dependent variable using a logit specification.

The independent variable of interest is whether the period of pregnancy coincided with the Northridge Earthquake. The Northridge Earthquake temporarily shifted vehicle traffic on several Los Angeles highways. The assumption is that the earthquake also effected the level of vehicle air pollution. I estimate separately the above equation for mothers who lived within distance interval d of a road that had <u>less</u> traffic and within distance interval d that had <u>more</u> traffic. For example, I estimate the equation for mothers living within .25 miles of the portion of Interstate 10 that was closed down, and for mothers living within .25 miles of the Interstate 10 detour routes.

The identifying assumption is that the Northridge Earthquake is an exogenous event that randomly caused damage and closed down portions of highways in Los Angeles. It is possible that demand for housing is partially determined by knowledge of the earthquake fault lines in Los Angeles. The preferred econometric specification considers the same mothers living in the same homes in the year before the earthquake and in the year of the earthquake. Thus, even if mothers select where to live based on the fault lines, the estimation results only consider mothers who gave birth during the time period when traffic flow was impacted by the earthquake, as well as, in the year before the earthquake.

γ_i controls for mother fixed effects and ρ_j controls for birth order effects. $X^{'}$ is a vector of controls that include: an intercept, a mother's education, age, and race, as well as whether the mother smoked during pregnancy or received prenatal care.

I estimate equation 2.1 for different distance intervals. The hypothesis is that the effect of increased (decreased) traffic should differentially effect birth outcomes depending on the distance from the highway. Once distance from the highway reaches some maximum distance, $\bar{d}$, there will no longer be an effect on birth outcomes of the increased (decreased) traffic. I statistically test for the difference between the coefficient estimates for δ for different distance intervals. In particular, I test between estimates for very close distance intervals and intervals that would not plausibly be impacted by the increase in traffic. In this way I can control for other factors that effect birth outcomes that don't depend on the distance from the highway.

The equation I actually estimate in this paper differs from the preferred specification in several significant ways. I do not include mother fixed effects or birth order fixed effects. Nor do I include controls for whether a mother smoked during pregnancy or received prenatal care. I exclude these variables to increase the statistical power of the estimation. There are too few births during the period of highway closure to obtain precise point estimates for the coefficient of interest using the preferred specification. Importantly, even thought the actual estimating equation doesn't include mother fixed effects, the coefficient estimates are still conditioned on distance intervals from the highway. Although, I am not comparing the same mothers, I do compare mothers who chose to live the same distance from the same highways.

Equations (2.2) and (2.3) provide the empirical specifications estimated in the paper.

$$\begin{aligned} weight_i = \; & 1(noquake_i)\theta_0 + 1(quake_i)\theta_1 + 1(noquake_i)1(dist < .25)\theta_2 \\ & +1(quake_i)1(dist < .25)\theta_3 + X^{'}\beta + \epsilon_i \end{aligned} \tag{2.2}$$

$$\begin{aligned} weight_i = \; & 1(noquake_i)\delta_0 + 1(quake_i)\delta_1 + 1(noquake_i)1(dist < .25)\delta_2 \\ & +1(noquake_i)1(dist.25 - .5)\delta_3 + 1(noquake_i)1(dist.5 - .75)\delta_4 \\ & +1(quake_i)1(dist < .25)\delta_5 + 1(quake_i)1(dist < .25 - .5)\delta_6 \\ & +1(quake_i)1(dist < .5 - .75)\delta_7 + X^{'}\beta + \epsilon_i \end{aligned} \tag{2.3}$$

In addition to the differences from equation (2.1) discussed in the previous paragraph, equations (2.2) and (2.3) interact indicator variables for whether a mother is exposed to earthquake induced traffic changes with the distance from a mother's home to the highway. The same control variables are included in $X^{'}$ as in equation (2.1) except that I no longer include an intercept. I only consider mothers living less than half a mile from the highway when estimating equation (2.2), and mothers less than one mile from the highway when estimating equation (2.3). The interpretation of θ_3 (θ_2) in equation (2.2) is the difference in birth weight between a mother exposed (not exposed) to earthquake induced traffic while living less than one quarter mile from the highway relative to a mother living between one quarter and one half mile from the highway.[8]

2.6 Estimation Results

Table 2.1 provides preliminary evidence on the importance of proximity to a highway for infant birth outcomes. Panel A of Table 2.1 presents coefficient results from an OLS regression of birth weight on distance from the highway and a set of indicator control variables.

[8] The rationale for including indicator variables for pregnancies during the post-earthquake period and pregnancies not during the post-earthquake period is that it facilitates tests between the exposure periods, while controlling for distance from the highway.

Distance is measured as a continuous variable in miles ranging from 0 to the distance threshold ($\bar{d}$) specified in each column heading. Columns (1) and (2) consider a threshold of one half mile, while columns (3) and (4) set the threshold at 1 mile. A birth is included if the distance from the mother's residence to at least one of the four major highways impacted by earthquake is less than the distance threshold.[9] Columns (1)-(3) use births from 1993 only, while column (4) pools births from 1993 and 1994. The advantage of pooling the births is that there is a larger sample size. However, there is the concern that the distance variable for births from 1994 is confounded by traffic volume changes due to the Northridge Earthquake.

The coefficient point estimate for distance to a highway is remarkably consistent across the four specifications in Panel A. The point estimate is negative and imprecisely measured in all specifications. Distance from a highway has very little explanatory power. In column (1), the R-squared for the fit of the specification is zero to 4 decimal places.

The covariates included in columns (2)-(4) are indicator variables for: whether the mother is African American, Asian, or of another racial identity, whether the mother is a teenager, and whether the mother has any college education. The reference category is a white mother with at least one year of college education who was not a teenager at the time she gave birth. The point estimates are negative and significant at the 1% level for all three race indicator variables. For example, the point estimate for African American in column (2) suggests that a child born to an African American mother weighs 189 grams less than a child born to a white mother. Overall, the point estimates for the race indicator variables are an order of magnitude larger than the point estimates for distance to a highway. The point estimates for being a teenage mother and for mothers without any college education are also negative and significant at least the 5% level.

In panel B I estimate a model where I transform the weight and distance variables by taking the natural log. The estimation results reinforce the interpretation of panel A. (Log) distance from a highway has very little explanatory power in explaining (log) birth weight. The point estimates for the covariates are again negative and an order of magnitude larger. In column (2) the estimation results suggest that infants born to African American mothers weigh 7% less than infants born to white mothers.

In panel C I estimate a logit model where the dependent variable is whether an infant is born weighing less than 2,500 grams.[10] The rationale for this model is that pollution may increase the likelihood of dangerously low weight births, while at the same time not having a large impact on mean birth weight. There is no evidence that distance to a highway is correlated with births weighing less than 2,500 grams. Similar to Panels A and B, an indicator variable for African American mothers has the largest statistical effect. In column (2), the estimate suggests that African American mothers are 9% more likely to have a child with low birth weight.

[9] Note that only births from the specified zip codes are considered (please refer to the Data Section). In principle it is possible for the mother's residence to be less than the threshold distance to more than one of the four highways. However, there are very few births where this is the case.

[10] Note that panel C displays average partial effects calculated from the odds ratio logit estimation results.

Table 2.2 tests the unconditional means for the three birth outcomes: low birth weight, weight, and gestation period. The goal is to examine whether there is evidence for a vehicle pollution infant health damage gradient based on distance from one of the 4 highways in Los Angeles. Each panel in table 2.2 compares the means for births that occur within a quarter of a mile to those between a quarter of a mile and half a mile, and for births within half a mile to those between one and one and a half miles. Panel A pools births within a specified distance from Interstate 5, Interstate 10, Interstate 405, and Highway SR-118. Panels B, C, and D consider births in close proximity to Interstate 10, Interstate 5, and Highway SR-118 respectively.

Overall there is little evidence in support of an infant health damage gradient. Infant birth outcomes don't appear to be worse for those infants born to mothers living close to the highways. Only 5 of the 24 mean birth outcome comparisons in table 2.2 are statistically significant. Three of the statistically significant comparisons are for gestation period. All three statistically significant gestation period results suggest that mothers exposed to greater traffic have longer pregnancies (i.e. more healthy). However, the difference in length for these estimates is less than one day. The strongest results in favor of a hypothesis that proximity to busy highways is harmful for infant birth health are in Panel A. Both birth weight variables are significantly different when comparing births to mothers living less than half a mile to a highway with births to mothers living between a mile and a mile and a half from a highway. However, this specification should be interpreted with caution. Mothers living over a mile away from a highway are likely to be different than mothers living in close proximity. Table 2.1 shows that observable covariates such as mother's race and education are importantly correlated with birth outcomes. These variables are not controlled for in any of the mean comparisons in Table 2.2.

Panels A and B of Table 2.3 present estimation results from equations (2.2) and (2.3) on a the sample of births in close proximity to the Interstate 10 detour routes. The sample includes those mothers who were exposed to the potential pollution from a shift in traffic for the entire time period of Interstate 10's closure, or who were pregnant during the same calender days in the previous year.[11] Table 2.3 explores whether exposing mothers to <u>more</u> traffic leads to worse infant health outcomes at birth. In principle I could also examine whether there are improved health outcomes for mothers living in close proximity to the portion of Interstate 10 that had an unexpected reduction in traffic due to road closure. Unfortunately, geocoding of the mother home addresses for the sample of zip codes with birth information from LA County birth records revealed that there were surprisingly few births along the section of Interstate 10 that was closed after the Northridge Earthquake. This is true for both 1993 and 1994. I do not provide estimation results of equations (2.2) and (2.3) for the other LA County highways impacted by the earthquake. The other highways were either closed for too short of a time period, did not displace enough traffic, or had

[11]Births that occurred between April 11, 1993 and October 8, 1993 or April 11, 1994 and October 8, 1994 are included in the sample.

detour routes too close to the initial highway.

Columns (2) and (3) of Panel A provide estimation results of equation (2.2). Again, like in Table 2.2, there is little evidence of a infant health damage gradient with respect to distance from the highway. The *earthquake* coefficient provides the mean weight in grams (column 2) or mean gestation period (column 3) for an infant born to a mother living between one quarter and one half miles from the detour roadways during the period while traffic was rerouted. None of the four $< .25mile$ coefficients are significantly different from zero, implying that there is no difference in birth outcomes for mothers living closer or farther away from the vehicle traffic. We may not expect $< .25miles$ & *noearthquake* to be different from zero since before the earthquake the detour route was not a major traffic artery. We would expect $< .25miles$ & *earthquake* to be negative and significantly different from zero. We may not expect $< .25miles$ & *noearthquake* to be different from zero since before the earthquake the detour route was not a major traffic artery. Column (1) provides estimates for a logit estimation of equation (2.2) with an indicator for low birth weight as the dependent variable. Neither of the coefficient estimates for the $< .25mile$ variables are significantly different from zero. For each of the three dependent variables (columns) I test the hypothesis that the two $< .25mile$ coefficients are equivalent. The p-values for this test are displayed in the last row of panel A. I fail to reject the hypothesis for each specification.

Panel B of table 2.3 provide estimation results of equation (2.3). The main difference from the top panel, is that unlike Panel A, panel B considers four distance intervals up to one mile from the Interstate 10 detour route. Nevertheless, the conclusions are the same as from Panel A. There is no evidence that birth health outcomes are worse for births to mothers closer to the detour route. This is true both before and after the earthquake. I am unable to reject the null hypothesis of equality between the two $< .25mile$ coefficients in each equation.[12]

2.7 Conclusion

The goal of this paper is to examine the causal link between localized exposure to hazardous pollutants from motor vehicle exhaust and adverse health outcomes. One of the primary challenges in estimating the relationship between vehicle pollution and health is that individuals can usually select the level of pollution to which they are exposed. The concern is that unless we control for all characteristics that effect the selection of pollution then empirical estimates will likely be biased due to omitted variables and accurate comparisons can not be made.

This paper focuses on two health outcomes for newborns: birth weight and gestation period. I explore whether an exogenous event–the 1994 Northridge Earthquake–can be used

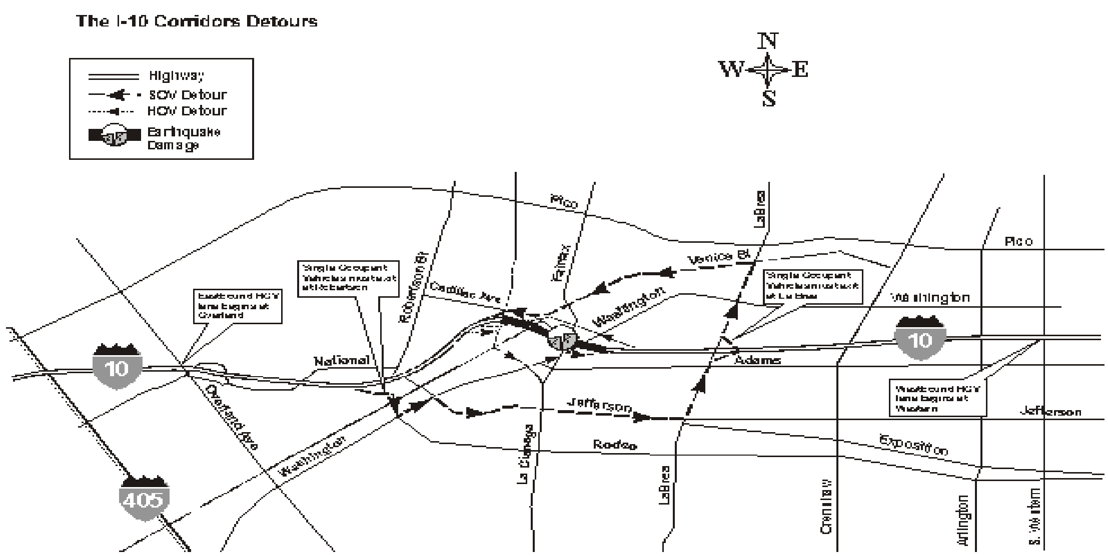
[12]The R-squared statistic for the fit of the regression model for columns (2) and (3) is very large. This is due largely to the fact that I don't include an intercept in these models. The R-squared from similar models with an intercept is between 0.0000 and 0.0300 (and similar to that for the regressions in Table 2.1)

as a quasi-experiment to test how birth outcomes change from a sudden and unexpected increase in pollution. The assumption is that mothers do not move homes after the earthquake to avoid increases in pollution due to rerouted traffic.

Overall the results of this study are inconclusive due to the relatively small number of new births included in the sample design. However, the results do suggest that a mother's race, age, and level of education are more important than proximity to a highway. Being a minority race (e.g. African American), a teenage mother, or not having any college education are correlated with lower birth weight. The size of these correlations are approximately an order of magnitude larger than the point estimates for the effect of living in close proximity to a road with heavy traffic.

Figure 2.1: Interstate 10 and SR118 Highway Closures and Traffic Detours

Interstate 10 Road Closure and Detour



Westbound SR-118 Detours

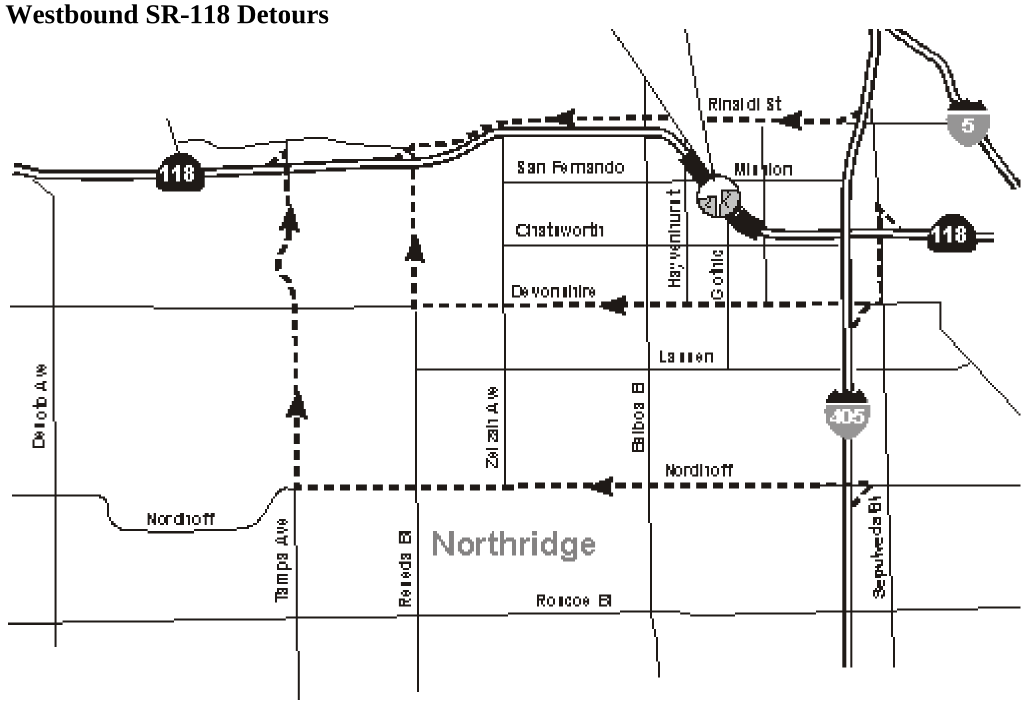


Figure 2.2: Interstate 5 Highway Closure and Traffic Detours

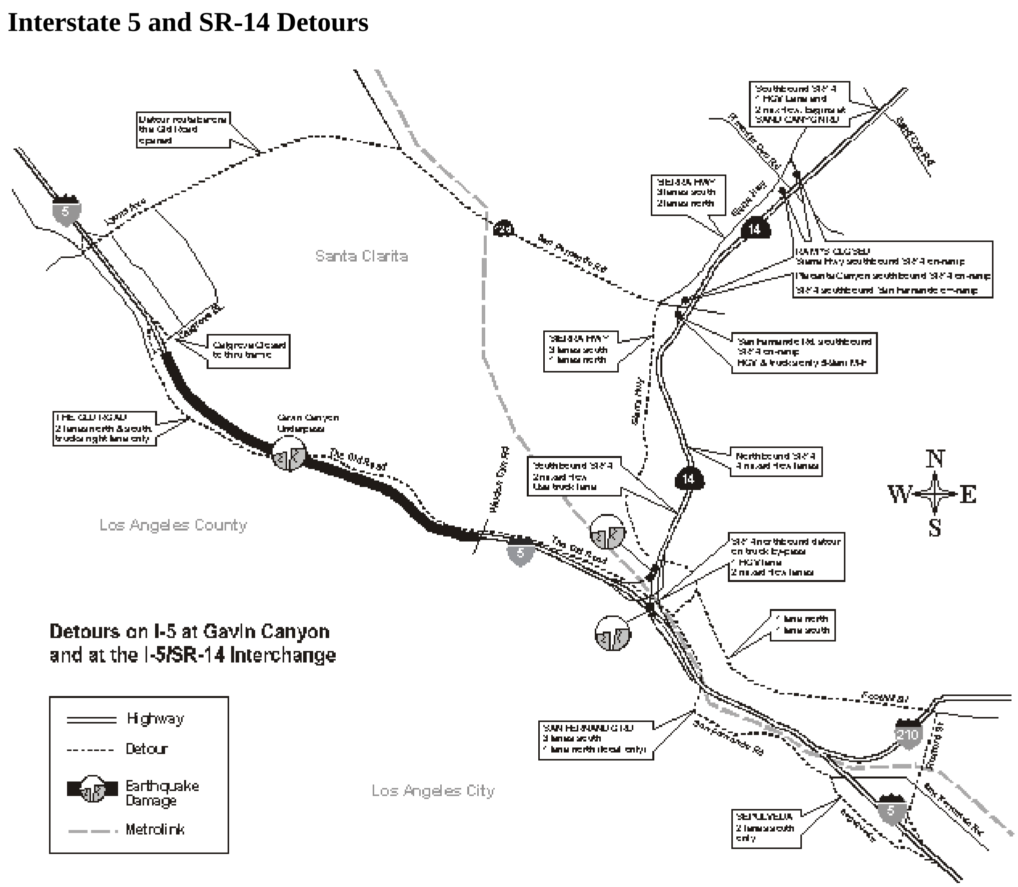


Table 2.1: Correlation between the Distance from LA Highways and Infant Birth Outcomes

	(1)	(2)	(3)	(4)
	< .5 miles & 1993	< .5 miles & 1993	< 1 mile & 1993	< 1 mile & 1993-94
A. Dependent Variable: Birth Weight (grams)				
distance	-23 (57)	-22 (56)	-22 (21)	-23 (15)
african american		-189 (23)***	-177 (18)***	-188 (13)***
asian		-85 (37)**	-100 (29)***	-103 (20)***
other/unknown race		-185 (40)***	-158 (31)***	-168 (22)***
teen mother		-82 (25)***	-86 (19)***	-90 (13)***
no college		-44 (17)**	-35 (13)**	-22 (9)**
constant	3,355 (16)***	3,428 (21)***	3,419 (15)***	3,405 (11)***
R-squared	0.0000	0.0158	0.0143	0.0167
observations	5,736	5,736	9,816	18,840
B. Dependent Variable: Log Birth Weight (grams)				
log distance	-0.003 (0.004)	-0.003 (0.004)	-0.003 (0.003)	-0.004 (0.002)**
african american		-0.070 (0.008)***	-0.065 (0.007)***	-0.069 (0.005)***
asian		-0.020 (0.014)	-0.026 (0.010)**	-0.030 (0.007)***
other/unknown race		-0.057 (0.015)***	-0.047 (0.011)***	-0.055 (0.008)***
teen mother		-0.018 (0.009)**	-0.022 (0.007)***	-0.026 (0.005)***
no college		-0.017 (0.006)***	-0.012 (0.005)**	-0.008 (0.003)**
constant	8.093 (0.007)***	8.118 (0.008)***	8.114 (0.005)***	8.110 (0.004)***
R-squared	0.0001	0.0150	0.0128	0.0156
observations	5,736	5,736	9,816	18,840
C. Dependent Variable: Indicator Low Birth Weight (<2,500 grams)				
distance	0.016 (0.038)	0.008 (0.029)	-0.001 (0.008)	-0.001 (0.006)
african american		0.093 (0.041)**	0.070 (0.013)***	0.069 (0.010)***
asian		-0.029 (0.020)	-0.008 (0.012)	0.005 (0.009)
other/unknown race		0.040 (0.030)	0.039 (0.017)**	0.043 (0.013)***
teen mother		-0.010 (0.011)	0.004 (0.007)	0.010 (0.006)*
no college		0.014 (0.011)	0.010 (0.006)*	0.004 (0.004)
R-squared	0.0001	0.0244	0.0180	0.0162
observations	5736	5,736	9,816	18,840

Notes: Panels A. and B. display the estimated coefficients and standard errors from 4 separate regressions. Panel C. displays the marginal effects for each coefficient from 4 separate logit estimations. A constant is included in each logit estimation (but not in the table). The sample for columns (1) and (2) are those births in 1993 within.5 miles of interstate 5, interstate 10, interstate 405, or highway SR118 and also in a zip code within 405, or highway SR118 and also in a zip code within "close" proximity to the portions of these roads that were closed in 1994. Columns (3) and (4) restrict the sample to births within 1 mile. Column (4) includes births from both 1993 and 1994. See the data appendix for more details regarding the selection of LA county zip codes. Statistical significance: *** for 1% level, ** for 5% level, * for 10% level.

Table 2.2: Mean Birth Weight and Gestation Period by Distance from Highway during 1993

	(1)	(2)	(3)	(4)	(5)	(6)
	< .25 Miles	≥ .25 & < .5	t-test (1) v. (2)	< .5 Miles	≥ 1 & < 1.5	t-test (4) v. (5)
Panel A: All Highways						
Low Birth Weight	0.063 (0.003)	0.062 (0.005)	0.868	0.065 (0.002)	0.056 (0.004)	**0.046****
Weight	3342 (8)	3349 (11)	0.608	3314 (8)	3334 (18)	0.417
Gestation	276 (0.24)	275 (0.34)	**0.042****	276 (0.17)	276 (0.30)	**0.061***
Sample Size	5520	2834	8354	10938	3529	14467
Panel B: Interstate 10						
Low Birth Weight	0.066 (0.005)	0.072 (0.007)	0.456	0.068 (0.008)	0.069 (0.004)	0.893
Weight	3314 (12)	3314 (17)	0.973	3314 (8)	3334 (18)	0.293
Gestation	275 (0.39)	274 (0.54)	**0.026****	275 (0.27)	276 (0.57)	0.298
Sample Size	2383	1242	3625	4732	1071	5803
Panel C: Interstate 5						
Low Birth Weight	0.059 (0.008)	0.051 (0.010)	0.538	0.059 (0.006)	0.046 (0.006)	0.140
Weight	3361 (19)	3377 (26)	0.610	3373 (13)	3375 (16)	0.931
Gestation	276 (0.56)	277 (0.79)	0.294	276 (0.392)	276 (0.457	0.802
Sample Size	832	473	1305	1760	1154	2914
Panel D: Highway SR118						
Low Birth Weight	0.072 (0.011)	0.052 (0.012)	0.260	0.070 (0.007)	0.048 (0.007)	**0.048****
Weight	3368 (28)	3354 (33)	0.741	3344 (18)	3361 (18)	0.511
Gestation	275 (0.841)	274 (0.991)	0.527	275 (0.57)	276 (0.56)	0.114
Sample Size	516	326	842	1160	828	1988

Notes: Birth outcomes are for infants born in close proximity to Interstates 5, 10 and 405, and Highway SR118 in 1993 near to the sections of the highways damaged and closed down by the Northridge Earthquake in 1994. Columns (1), (2), (4), and (5) restrict the sample to include births where a mother's residence at the time of birth is a specific distance from the highway. Columns (3) and (6) provide p-values for a test of the statistical difference between the means. Weight is in grams. Low Birth Weight is defined as less than 2,500 grams. Gestation is measured in days. The results for the same comparisons for 1994 give very similar results.

Table 2.3: Quasi-Experimental Regressions

	(1)	(2)	(3)
Dependent Variable:	Indicator LBW	Weight (grams)	Gestation (days)
A. < .5 Miles from Interstate 10 Detour			
no earthquake	-- --	3409 (46)***	275.3 (1.5)***
< .25 miles & no earthquake	0.006 (0.016)	-5 (45)	0.8 (1.5)
earthquake	-- --	3459 (46)***	278.4 (1.5)***
< .25 miles & earthquake	0.024 (0.022)	-66 (46)	-0.2 (1.5)
black	0.081 (0.022)***	-168 (23)***	-4.9 (1.2)***
asian	0.022 (0.053)	-69 (108)	0.2 (3.5)
other minority	0.047 (0.057)	-221 (105)**	-4.0 (3.4)
teen mother	0.008 (0.019)	-114 (53)**	-1.2 (1.7)
no college	0.012 (0.013)	-65 (38)*	0.0 (1.2)
R-squared	--	0.9703	0.9953
observations	1,308	1,308	1,308
p-value for test of equality for <.25 mile coefficients	0.4226	0.3501	0.8553
B. < 1 Mile from Interstate 10 Detour			
no earthquake	-- --	3374 (41)***	275.8 (1.4)***
< .25 miles & no earthquake	0.093 (0.251)	9 (40)	0.1 (1.3)
≥ .25 miles & < .5 miles & no earthquake	-0.129 (0.254)	32 (39)	-0.2 (1.3)
≥ .5 miles & < .75 miles & no earthquake	0.150 (0.240)	-20 (38)	0.4 (1.3)
earthquake	-- --	3333 (41)***	276.5 (1.4)***
< .25 miles & earthquake	-0.019 (0.276)	3 (42)	1.1 (1.4)
≥ .25 miles & < .5 miles & earthquake	-0.552 (0.301)*	65 (41)	1.5 (1.4)
≥ .5 miles & < .75 miles & earthquake	-0.707 (0.287)**	102 (39)***	1.0 (1.3)
black	1.092 (0.177)***	-152 (29)***	-4.7 (1.0)***
asian	0.262 (0.543)	-38 (74)	0.5 (2.5)
other minority	0.295 (0.541)	-168 (74)**	-1.7 (2.5)
teen mother	0.326 (0.247)	-80 (43)*	0.4 (1.4)
no college	0.346 (0.193)*	-54 (29)*	-0.3 (1.0)
R-squared	--	0.9706	0.9952
observations	2,040	2,040	2,040
p-value for test of equality for <.25 mile coefficients	0.9312	0.9721	0.7189

Notes: The sample for Panel A contains births to mothers living within .5 miles of one of the Interstate 10 detours established after the Northridge Earthquake between between April 11, 1993 and October 8, 1993 or April 11, 1994 and October 8, 1994. Panel A estimates versions of equation (2), while panel B estimates versions of equation (3) (see text for details).

Chapter 3

Does Hazardous Waste Matter? Evidence from the Housing Market and the Superfund Program

[Coauthored with Michael Greenstone]

3.1 Introduction

The estimation of individuals' valuations of environmental amenities with revealed preference methods has been an active area of research for more than three decades. There are now theoretical models outlining revealed preference methods to recover economically well defined measures of willingness in a variety of settings, including housing markets, recreational choices, health outcomes, and the consumption of goods designed to protect individuals against adverse environmentally-induced outcomes ([67]; [5] contain reviews). The application of these approaches, however, is often accompanied by seemingly valid concerns about misspecification that undermine the credibility of any findings. Consequently, many are skeptical that markets can be used to determine individuals' valuations of environmental amenities.[1]

[1]Further, the increasing reliance on stated preference techniques to value environmental amenities is surely related to dissatisfaction with the performance of revealed preference techniques. See [58] and [44] for discussions of stated preference techniques

Hazardous waste sites are an example of an environmental disamenity that provokes great public concern. The 1980 Comprehensive Environmental Response, Compensation, and Liability Act, which became known as Superfund, gave the EPA the right to place sites that pose an imminent and substantial danger to public welfare and the environment on the National Priorities List (NPL) and to initiate remedial clean-ups at those sites. Through 2005, approximately $35 billion (2005$) in federal monies and an unknown amount of private funding has been spent on Superfund clean-ups, and yet remediations are incomplete at roughly half of the nearly 1,600 sites.[2] The combination of these high costs and the absence of convincing evidence of its benefits makes Superfund a controversial program ([7]).

This paper uses the housing market to estimate the welfare consequences of Superfund sponsored clean-ups of hazardous waste sites. The empirical challenge is that the evolution of housing market outcomes (e.g., prices) proximate to the Superfund sites in the absence of the clean-ups is unknown. The development of a valid counterfactual is likely to be especially challenging, because the sites assigned to the NPL are the most polluted ones in the US. For example, what would have happened to housing prices in Love Canal, NY, in the absence of the famous Superfund clean-up there?

As a solution, we implement a quasi-experiment based on knowledge of the selection rule that the EPA used to develop the first NPL in 1983. The EPA was only allocated enough money to conduct 400 clean-ups. After cutting the list of candidate sites from 15,000 to 690, the EPA invented and implemented the Hazardous Ranking System (HRS) that assigned each site a score from 0 to 100 based on the risk it posed, with 100 being the most dangerous. The 400 sites with the highest HRS scores (i.e., exceeding 28.5) were placed on the initial NPL in 1983, making them eligible for Superfund remedial clean-ups. We compare the evolution of housing market outcomes between 1980 and 2000 in areas near sites that had initial HRS scores above and below the 28.5 threshold. We also implement a regression discontinuity design ([36]) to focus the comparisons among sites with scores near the threshold.

To structure the analysis, we model the consequences of a quasi-experiment that leads to an exogenous change in a local amenity in the context of the hedonic method ([66]; [93]). We show that if consumers value the clean-ups, then there are two empirical predictions. First, the improvement at the site should lead to increases in the demand and supply of local housing and, in turn, increases in the prices and quantities of houses. Second, the improvement should lead to sorting such that the share of the population living near the improved sites that places a high value on environmental quality increases. The implication is that an exclusive focus on housing prices as in previous quasi-experimental hedonic studies ([34]; [76]) may obscure part of the welfare gain.

The results suggest that individuals place a small value on a hazardous waste site's inclusion on the NPL and subsequent clean-up. Specifically, we find that a site's placement on the NPL is associated with economically small and statistically indistinguishable from zero local changes in residential property values, property rental rates, housing supply, total

[2]Throughout the paper, monetary figures are reported in 2000 $'s, unless otherwise noted

population, and the types of individuals living near the site. These findings are robust to a wide variety of specification checks, and they hold whether they are measured 7 (in 1990) or 17 (in 2000) years after placement on the NPL. Overall, these findings suggest that the mean local benefits of a Superfund clean-up as measured through the housing market are substantially lower than our estimated average cost of $43 million per Superfund clean-up.

The conventional hedonic approach compares areas surrounding NPL sites with the remainder of the US. In contrast to the HRS research design, the conventional approach produces estimates that suggest that gains in property values exceed the mean costs of clean-up. However, these regressions also produce a number of puzzling results that undermine confidence in the approach's validity. Further, there is evidence that the conventional approach is likely to confound the effect of the presence of a NPL site with other determinants of housing market outcomes. Notably, the HRS research design appears to greatly reduce the confounding.

The study is conducted with the most comprehensive data file ever compiled by the EPA or other researchers on the Superfund program and its effects. The resulting database has information on all 1,400 Superfund hazardous waste sites as of 2000, the sites that narrowly missed placement on the initial NPL, and census-tract level housing market outcomes for 1980 (before the release of the first NPL), 1990, and 2000. Consequently, this study is a substantial departure from the previous Superfund/hazardous waste site hedonic literature, which is entirely comprised of examinations of one or a handful of sites and collectively covers just 30 different sites ([95]; [82]; [72]; [70]; [51]; [52]; [71]; [80]; [65]; [81]; [50]).[3]

The paper proceeds as follows. Section I provides background on the Superfund program and how the HRS research design may allow for credible estimation of the effects of Superfund clean-ups on housing market outcomes. Section II discusses how to use hedonic theory to provide an economic interpretation for the results from the HRS research design. Section III details the data sources and provides some summary statistics. Sections IV and V report on the econometric methods and empirical findings, respectively. Section VI interprets the results, while VII concludes.

[3]Using EPA estimates of the probability of cancer cases and the costs of Superfund clean-ups, [100] find that at the median site expenditure the average cost per cancer case averted by the clean-up exceeds $6 billion. This health effects approach requires knowledge of the toxins present and the pathways they travel, the health risk associated with a toxic by pathway pair, the size of the affected population, the pathway-specific exposure, and the willingness to pay to avoid mortality/morbidity. Due to the state of scientific uncertainty associated with each step, we think this approach is unlikely to produce credible benefit estimates.

3.2 The Superfund Program and a New Research Design

3.2.1 History and Broad Program Goals

Before the regulation of the disposal of hazardous wastes by the Toxic Substances Control and Resource Conservation and Recovery Acts of 1976, industrial firms frequently disposed of wastes by burying them in the ground. Love Canal, New York offers perhaps the most infamous example of these disposal practices. Throughout the 1940s and 1950s, this area served as a landfill for industrial waste, receiving more than 21,000 tons of chemical wastes. After New York state investigators found high concentrations of dangerous chemicals in the air and soil at Love Canal, concerns about the safety of this area prompted President Carter to declare a state of emergency in 1978, an action that led to the relocation of the area's 900 residents. The Love Canal incident helped to galvanize support for addressing the legacy of industrial waste, a movement that culminated in the creation of the Superfund program in 1980.

The centerpiece of the Superfund program, and this paper's focus, is the long-run remediation of hazardous waste sites.[4] These multi-year remediation efforts aim to reduce permanently the serious, but not imminently life-threatening, dangers caused by hazardous substances. By the end of 2005, the EPA has placed 1,552 sites, thereby chosen for these long-run clean-ups. The next subsection describes the selection process, which forms the basis of our research design.

3.2.2 Site Assessment and Superfund Clean-Ups Processes

As of 1996, environmental activities, neighborhood groups, and other interested parties had referred more than 40,000 hazardous waste sites to the EPA for possible inclusion on the NPL. Since there are limited resources available for these clean-ups, the EPA follows a multi-step process to identify the most dangerous sites.

The final step of the assessment process involves the application of a Hazardous Ranking System (HRS), a rating system reserved for the most dangerous sites. The EPA developed the HRS in 1982 as a standardized approach to identify the sites that pose the greatest threat to humans and the environment. The original HRS evaluated the risk for exposure to chemical pollutants along three migration 'pathways': groundwater, surface water, and air. The major determinants of risk along each pathway for a site are the toxicity and concentration of chemicals present, the likelihood of exposure and proximity to humans, and the size of the potentially affected population. EPA officials also consider non-human

[4] The Superfund program also finds immediate removals, which are short-term responses to environmental emergencies aimed at diminishing an immediate threat. These actions are not intended to remediate the underlying environmental problem and are not exclusive to hazardous waste sites on the NPL.

impacts, but they play a relatively minor role in determining the HRS score.

The HRS produces a score that ranges from 0 to 100, with 100 being the highest level of risk. From 1982-1995, the EPA assigned all hazardous waste sites with a HRS score of 28.5 or greater to the NPL. These sites are the only ones that are eligible for Superfund remedial clean-up. The Data Appendix provides further details on the determination of HRS test scores and their role in assignment to the NPL.

Once a site moves onto on the NPL, it generally takes many years until the clean-up is complete. The first step is a further study of the extent of the environmental problem and how best to remedy it, an assessment that is summarized in the Record of Decision (ROD), which also outlines recommended clean-up actions for the site. After workers finish physical construction of all clean-up remedies, removing immediate threats to health, and putting long-run threats "under control," the EPA gives a site a "construction complete" designation. The final step is the agency's deletion of the site from the NPL.

3.2.3 1982 HRS Scores as the Basis of a New Research Design

This paper's goal is to obtain reliable estimates of the effect of Superfund sponsored clean-ups of hazardous waste sites on housing market outcomes in areas surrounding the sites. The empirical challenge is that NPL sites are the most polluted in the US, so it is likely that there are unobserved factors that covary with both proximity to hazardous waste sites and housing prices. Although this possibility cannot be tested directly, it is notable that proximity to a hazardous waste site is associated with lower population densities, lower household incomes, higher percentages of high school dropouts, and a higher fraction of mobile homes among the housing stock.

Consequently, cross-sectional estimates of the association between housing prices and proximity to a hazardous waste site may be severely biased due to omitted variables.[5] In fact, the possibility of confounding due to unobserved variables has been recognized as a threat to the use of the hedonic method to develop reliable estimates of individuals' willingness to pay for environmental amenities since its invention ([97]). This paper's challenge is to develop a valid counterfactual for the housing market outcomes near Superfund sites in the absence of their placement on the NPL and clean-up.

A feature of the initial NPL assignment process that has not been noted previously by researchers may provide a credible solution to the likely omitted variables problem. In the first year after the legislation's passage, groups and individuals referred 14,697 sites to the EPA, which then investigated them as potential candidates for remedial action. Through an initial assessment process, the EPA winnowed this list to the 690 most dangerous sites.

[5] Cross-sectional models for housing prices have exhibited signs of misspecification in a number of other settings, including the relationships between land prices and school quality, air pollution, and climate variables ([26]; [34]; [43]). Incorrect choice of functional form is an alternative source of misspecification ([57]; [37]). Other potential sources of biases of published hedonic estimates include measurement error and publication bias ([25]; [20])

Although the Superfund legislation directed the EPA to develop a NPL of "at least" 400 sites (Section 105(8)(B) of CERCLA), budgetary considerations caused the EPA to set a goal of placing exactly 400 sites on the NPL.

The EPA developed the HRS to provide a scientific basis for determining the 400 out of the 690 sites that posed the greatest risk. Pressured to initiate the clean-ups quickly, the EPA developed the HRS in about a year, applied the test to the 690 worst sites, and ranked their scores from highest to lowest. A score of 28.5 divided numbers 400 and 401, so the initial NPL published in September 1983 was limited to sites with HRS scores exceeding 28.5. See the Data Appendix for further details.

The central role of the HRS score provides a compelling basis for a research design that compares housing market outcomes near sites with initial scores above and below the 28.5 cut-off for at least three reasons. First, it is unlikely that sites' HRS scores were manipulated to affect their placement on the NPL, because the 28.5 threshold was established after the testing of the 690 sites was completed. The HRS scores therefore reflected the EPA's assessment of the risks posed by each site, rather than the expected costs or benefits of clean-up.

Second, the HRS scores are noisy measures of risk, so it is possible that true risks are similar above and below the threshold. This noisiness results from the scientific uncertainty about the health consequences of exposure to the tens of thousands of chemicals present at these sites.[6] Further, there was no evidence that sites with HRS scores below 28.5 posed little risk to health. The Federal Register specifically reported that the "EPA has not made a determination that sites scoring less than 28.50 do not present a significant risk to human health, welfare, or the environment" and that a more informative test would require "greater time and funds" (Federal Register, September 21, 1984).[7]

Third, the selection rule that determined placement on the NPL is a highly nonlinear function of the HRS score. This allows for a quasi-experimental regression discontinuity design that compares outcomes at sites "near" the 28.5 cut-off. If the unobservables are similar or change smoothly around the regulatory threshold, then the regression discontinuity approach will produce causal estimates of the impact of Superfund clean-ups on housing market outcomes.[8]

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[6] A recent history of Superfund's makes this point. "At the inception of EPA's Superfund program, there was much to be learned about industrial wastes and their potential for causing public health problems. Before this problem could be addressed on the program level, the types of wastes most often found at sites needed to be determined, and their health effects studied. Identifying and quantifying risks to health and the environment for the extremely broad range of conditions, chemicals, and threats at uncontrolled hazardous wastes sites posed formidable problems. Many of these problems stemmed from the lack of information concerning the toxicities of the over 65,000 different industrial chemicals listed as having been in commercial production since 1945" ([3], p. 3-2).

[7] One way to measure the crude nature of the initial HRS test is by the detail of the guidelines used for determining the HRS score. The guidelines used to develop the initial HRS sites were collected in a 30 page manual. Today, the analogous manual is more than 500 pages.

[8] The research design of comparing sites with HRS scores "near" the 28.5 is unlikely to be valid for sites

An additional feature of the analysis is that an initial score above 28.5 is highly correlated with eventual NPL status but is not a perfect predictor of it. This is because some sites were rescored, with the later scores determining whether they ended up on the NPL.[9] The subsequent analysis uses an indicator variable for whether a site's initial (i.e., 1982) HRS score was above 28.5 as an instrumental variable for whether a site was on the NPL in order to purge the potentially endogenous variation in NPL status.

3.3 Using Hedonics to Value Changes in Local Environmental Quality Due to Superfund Clean-ups

An explicit market for a clean local environment does not exist. The hedonic price method is commonly used to infer the economic value of non-market amenities like environmental quality to individuals. To date, its empirical implementation has generally been in cross-sectional settings where it is reasonable to assume that consumers and producers have already made their optimizing decisions. This section briefly reviews the cross-sectional equilibrium. It then discusses how an improvement in local environmental quality due to a Superfund clean-up leads agents to alter their utility and profit-maximizing decisions and the resulting new equilibrium. The purpose of this discussion is to devise an empirical strategy to infer the welfare consequences of Superfund clean-ups using decennial Census data.

3.3.1 A Brief Review of Equilibrium in the Hedonic Model

Economists have estimated the association between housing prices and environmental amenities at least since [89] and [90]. However, [93] and [66] were the first to give this correlation an economic interpretation. In the Rosen formulation, a differentiated good is described by a vector of its characteristics, $\mathbf{C} = (c_1, c_2, ..., c_n)$. In the case of a house, these characteristics may include structural attributes (e.g., number of bedrooms), neighborhood public services (e.g., local school quality), and local environmental amenities (e.g., distance from a hazardous waste site). Thus, the market price of the i^{th} house can be written as:

$$P_i = P(c_{i1}, c_{i2}, ..., c_{in}) \tag{3.1}$$

that received an initial HRS score after 1982. This is because once the 28.5 cut-off was set, the HRS testers were encouraged to minimize testing costs and simply determine whether a site exceeded the threshold. Consequently, testers generally stop scoring pathways once enough pathways are scored to produce a score above the threshold.

[9] As an example, 144 sites with initial scores above 28.5 were rescored and this led to 7 sites receiving revised scores below the cut-off. Further, complaints by citizens and others led to rescoring at a number of sites below the cut-off. Although there has been substantial research on the question of which sites on the NPL are cleaned-up first (see, e.g., [96]), we are unaware of any research on the determinants of a site being rescored.

The partial derivative of P() with respect to the j^{th} characteristic, $\partial P/\partial c_i$, is referred to as the marginal implicit price. It is the marginal price of the j^{th} characteristic implicit in the overall price of the house, holding constant all other characteristics.

In the hedonic model, the locus between housing prices and a characteristic, or the hedonic price schedule (HPS), is generated by the equilibrium interactions of consumers and producers. It is assumed that markets are competitive, all consumers rent one house at the market price, and utility depends on consumption of the numeraire, X (with price equal to 1), and the vector of house characteristics:

$$u = u(X, \mathbf{C}) \tag{3.2}$$

The budget constraint is expressed as $I - P - X = 0$, where I is income.

Maximization of (2.2) with respect to the budget constraint reveals that individuals choose levels of each of the characteristics to satisfy $\frac{\partial U/\partial c_j}{\partial U/\partial x} = \partial P/\partial c_j$. Thus, the marginal willingness to pay for c_j (e.g., local environmental quality) must equal the marginal cost of an extra unit of c_j in the market.

It is convenient to substitute the budget constraint into (2.2), which gives $u = u(I - P, c_1, c_2, ..., c_n)$. By inverting this equation and holding all characteristics of the house but j constant, an expression for willingness to pay for c_j is obtained:

$$B_j = B_j(I - P, c_j, C^*_{-j}, u^*) \tag{3.3}$$

Here, u^* is the highest level of utility attainable given the budget constraint and C^*_{-j} is the optimal quantities of other characteristics. This is referred to as a bid (or indifference) curve, because it reveals the maximum amount that an individual would pay for different values of c_j, holding utility constant.

Heterogeneity in individuals' bid functions due to differences in preferences and/or incomes leads to differences in the chosen quantities of a characteristic. This is depicted in Figure 1a, which plots the HPS and bid curves for c_j of three consumer types. The consumers are denoted as types #1, #2, and #3, and potentially there are an unlimited number of each type. Each bid function reveals the standard declining marginal rate of substitution between c_j and X (because $X = I - P$). The three types choose houses in locations where their marginal willingness to pay for c_j is equal to the market determined marginal implicit price, which occur at c_{j1}, c_{j2}, and c_{j3}, respectively. Given market prices, these consumers' utilities would be lower at sites with higher or lower levels of local environmental quality.

The other side of the market is comprised of suppliers of housing services. We assume that suppliers are heterogeneous due to differences in their cost functions. This heterogeneity may result from differences in the land they own. For example, it may be very expensive to provide a high level of local environmental quality on a plot of land located near a steel factory. By inverting a supplier's profit function, we can derive its offer curve for the characteristic c_j:

$$O_j = O_j(c_j, \mathbf{C}^*_{-j}, \Pi^*) \tag{3.4}$$

where Π^* is the maximum available profit given its cost function and the HPS. Figure 1a depicts offer curves for three types of suppliers. With this set-up, individuals that live in a house that they own would be both consumers and suppliers and their supplier self would rent to their consumer self.

The HPS is formed by tangencies between consumers' bid and suppliers' offer functions. At each point on the HPS, the marginal price of a housing characteristic is equal to an individual's marginal willingness to pay for that characteristic and an individual supplier's marginal cost of producing it. From the consumer's perspective, the gradient of the HPS with respect to local environmental quality gives the equilibrium differential that compensates consumers for accepting the increased health risk and aesthetic disamenities associated with lower local environmental quality. Put another way, areas with poor environmental quality must have lower housing prices to attract potential homeowners, and the HPS reveals the price that allocates consumers across locations. Thus, the HPS can be used to infer the welfare effects of a marginal change in a characteristic. From the suppliers' perspective, the gradient of the HPS reveals the costs of supplying a cleaner local environment.

3.3.2 What are the Consequences of a Large Change in Environmental Quality in the Hedonic Model?

This study assesses the impacts of Superfund remediations of hazardous waste sites, which intend to cause non-marginal improvements in environmental quality near the site. This subsection extends and fleshes out the hedonic model to describe the theoretical impacts of these clean-ups on consumers, suppliers, and social welfare. Any impacts on the labor market are ignored, because wage changes don't affect welfare since any gains (losses) for workers are offset by losses (gains) for firms ([91]).

We focus on the case where the overall HPS does not shift in response to the increased supply of "clean" sites so there are not changes in relative prices.[10] The assumption of a constant HPS may be valid because to date only 670 Superfund sites have been completely remediated. They are located in just 624 of the 65,443 US census tracts, which constitute a small part of the US housing market.

Now, consider the clean-up of a hazardous waste site that increases local environmental quality in the neighborhood surrounding the site from c_{j1} to c_{j3} as in Figure 1a. It is evident from the HPS that the rental price of housing near the improved site will rise to p_3. For type #1 consumers, the increase in the rental rate exceeds their willingness to pay for the clean-up. Consequently, their neighborhood has become too expensive, given their preferences and income, and the clean-up reduces their utility.

[10]See [23] and [67] for more general discussions of the welfare impacts of non-marginal amenity improvements (including price changes). [54] also present a brief discussion of these issues.

The result is that consumers will migrate between communities to restore the equilibrium. The type #1 consumers that had chosen the improved site based on its previous rental price and environmental quality will move to a house with their originally chosen and optimal values of p and c_j (i.e., p_1 and c_{j1}). Additionally, some type #3 consumers will move near the newly cleaned-up site, where they will consume c_{j3} at a price of p_3. So assuming zero moving costs, the key result is that some consumers will change locations, but their utility is unchanged because they choose locations with their original c_j and p.[11]

One consequence of this taste-based sorting is that the residents of the improved neighborhood will have greater unobserved taste for environmental quality and/or higher incomes.[12] Thus, the marginal resident will be less tolerant of exposure to hazardous waste. We test for this taste-based sorting below.

In this set-up, land owners near the site are the only agents whose welfare is affected by the clean-up. If residential and commercial land markets are perfectly integrated, then the higher rental rates are a pure benefit for all landowners because the change in environmental quality is costless for them. In this case, the supply of residential land is effectively fixed so all adjustments occur through prices.

It is possible that the residential and non-residential land markets are not perfectly integrated, perhaps due to zoning laws, which are costly to change ([53]). In this case, the increase in rental prices is still a pure benefit for owners of residential land near the site. The higher rents for residential land will cause some owners of non-residential land to find it profitable to convert their land to residential usage. Presumably, the pre-clean-up rental rate of the converted land had been higher when in the non-residential sector and/or there may be costs associated with conversion (e.g., legal fees associated with rezoning), so the benefits for owners of converted land are smaller than for owners of land that was already used for residential housing. Ultimately, the benefits of conversion determine the shape of the supply curve of residential land near the site and the welfare gain for these land owners. The empirical analysis tests for supply responses.

To summarize, there are four predicted impacts of an amenity improvement. First, the price of land (and housing) near the improved site will increase (except in the unlikely case where the supply of residential land is perfectly elastic). Second, consumers will respond with taste-based sorting. Third, the supply of residential land (and housing) near the site is likely to increase. Fourth, the entire welfare gain accrues to land owners. We next discuss how to test these predictions with decennial Census data.

[11] For simplicity, we assume zero moving costs although this surely isn't correct. In the presence of moving costs, renters are made worse off by the amount of the moving costs. See [24] on the impacts of moving costs on the valuation of air pollution.

[12] See [21] and [30] for evidence of migration induced by environmental changes. In principle, the new residents' incomes could have a direct effect on individuals' valuations of living in the community. We ignore this possibility here because this will not create any social benefits as long as the benefits from living near high income individuals are sufficiently linear.

3.3.3 Can We Learn about the Welfare Effects of Superfund Clean-ups from Decennial Census Data?

Three decades after the publication of the original Rosen article, the hedonic approach to estimating the value of non-marginal amenity changes has not met with great empirical success for at least three reasons. First, the consistent estimation of the HPS, which is the foundation of all welfare calculations, has proven to be extremely challenging due to omitted variables ([34]; [43]). Second, the estimation of even a single individual's/taste type's bid function is also made quite difficult, because it is impossible to observe the same individual facing two sets of prices in a cross-section.[13] The difficulty of this task was underscored by [48] and [22] who showed that taste-based sorting undermines efforts to infer consumers' bid functions from the HPS.[14] Third, the implementation of the full blown approach requires estimates of bid functions for <u>all</u> consumers and cost functions for <u>all</u> suppliers in the economy. This is a tremendous amount of information, and there is a consensus that existing data sources are not up to the task.

In light of these challenges to implementing the hedonic approach, this subsection considers how decennial census data on housing and demographic variables can be used to learn about the welfare effects of Superfund clean-ups. There are at least two features of these data that merit noting because they affect the form and interpretation of the subsequent empirical analysis.

The first feature is that census tracts are the smallest unit of observation that can be matched across the 1980, 1990, and 2000 censuses. This means that it is infeasible to observe individuals over time and therefore to obtain estimates of their bid and cost functions. Consequently, we consider the impacts of a clean-up in the context of census tract-level demand and supply functions for residential land, which are determined by the bid and cost functions of local consumers and suppliers.

We begin with the case where the supply curve for residential land near a hazardous waste site is perfectly inelastic, which is likely to be the case in the short-run, and demand is downward sloping. This is depicted in Figure 1b with S1 and D1 and equilibrium outcome (P1, Q1). Now, consider an exogenous increase in environmental quality due to a clean-up. The improvement raises current residents' valuation of living near the formerly dirty site and, as sketched out in the previous subsection, with free migration individuals with even higher valuations of environmental quality will move in. The net result is that the demand curve for residential housing near the improved site shifts out. This is depicted as D2 and causes prices to increase to P2 but leaves quantities unchanged.

With a parallel shift in the demand curve and no change in the HPS, the welfare gain is the sum of the shaded areas A1 and A2 in Figure 1b. This equals the mean change in price

[13] [93] proposed a 2-step approach for estimating bid functions (and offer curves). He later wrote, "It is clear that nothing can be learned about the structure of preferences in a single cross-section" ([92], p. 658).

[14] In a recent paper, [47] outline the assumptions necessary to identify the demand (and supply) functions in an additive version of the hedonic model with data from a single market.

times the number of residential plots of land and entirely accrues to suppliers or landowners. From a practical perspective, the challenge is to accurately measure the change in house or residential land prices near the improved site.

In the longer run, supply is likely to be more elastic due to the conversion of non-residential land, and the remediation will lead to changes in prices and quantities. Figure 1b depicts the unrealistic polar case where supply is perfectly elastic as S2. With this supply curve, the new equilibrium combination is (P1, Q2), which reflects a substantial gain in quantities but no change in prices. The gain in welfare is entirely an increase in consumer surplus and is the sum of the shaded areas B1, B2, and A2. Previous applications of the hedonic method have generally examined prices only, so they may have understated (potentially dramatically) the welfare gain associated with amenity improvements.

It is evident that with census-tract data the development of a full welfare measure requires knowledge of the shapes of the supply and demand curves. We are unaware of a credible strategy for separately identifying supply and demand over the 10 year periods between censuses. In this situation, precise welfare calculations require ad hoc assumptions about the elasticities of supply and demand, except for the case where neither prices nor quantities change. In fact, the subsequent analysis finds small changes in prices and quantities, so our primary conclusion is that Superfund remediations did not substantially increase social welfare.

The census tract-level demographic data can also be used to test the theoretical prediction of taste-based sorting in response to remediations. An increase in the number of high income individuals or people that are likely to place a high value on environmental quality in areas near the remediated sites would provide complementary evidence that the clean-ups are valued. In contrast, a failure to find these population shifts near the sites would suggest that the clean-ups did not lead to substantial welfare gains.

The second feature of the data that merits highlighting is that they are only available in 1980, 1990, and 2000. Ideally, we would like to measure the impact of a site's placement on the NPL immediately after the announcement because all benefits are in the future and homeowners will naturally discount them by the rate of time preference. Furthermore, the clean-up itself may reduce the consumption value of living near a site in the short-run (e.g., due to increased presence of trucks).

An immediate measurement of the impact on prices would ensure that we have captured the impact of the clean-up on the value of housing services in all years. However, the first NPL was released in 1983, and housing prices cannot be observed again until 1990 or 2000. By then, some of the clean-ups will have been completed, and the time to completion for the others (relative to 1983) will have been greatly reduced. For this reason, the measurement of the impacts of the NPL designation with 1990 or 2000 Census data will overstate the properly measured benefits.

3.4 Data Sources and Summary Statistics

3.4.1 Data Sources

We constructed the most comprehensive data file ever compiled on the Superfund program. It contains detailed information on all hazardous waste sites placed on the NPL by 2000, as well as the hazardous waste sites with 1982 HRS scores below 28.5. It also includes housing price, housing characteristic, and neighborhood demographic information for areas surrounding the sites. This subsection briefly describes the data sources. The Data Appendix and [54] provide additional details.

The housing, demographic and economic data come from Geolytics's *Neighborhood Change Database*, which includes information from the 1970, 1980, 1990, and 2000 Censuses. Importantly, the 1980 data predate the publication of the first NPL in 1983. We collected the longitude and latitude for each of the hazardous waste sites and used this information to place all sites in a unique census tract.

The Geolytics data is used to form a panel of census tracts based on 2000 census tract boundaries, which are drawn so that they include approximately 4,000 people in 2000. Census tracts are the smallest geographic unit that can be matched across the 1970-2000 Censuses. The Census Bureau placed the entire country in tracts in 2000. Geolytics fit 1970, 1980, and 1990 census tract data to the year 2000 census tract boundaries to form a panel. The primary limitation of this approach is that in 1970 and 1980, the US Census Bureau only tracted areas that were considered 'urban' or belonged to a metropolitan area. The result is that the remaining areas of the country cannot be matched to a 2000 census tract, so the 1970 and 1980 values of the Census variables are missing for 2000 tracts that include these areas.

The analysis is restricted to the 48,147 out of the 65,443 2000 census tracts that have non-missing housing price data in 1980, 1990, and 2000. This sample includes 985 of the 1,398 sites listed on the NPL before January 1, 2000 and 487 of the 690 sites which were tested for inclusion on the initial NPL. The addition of the sample restriction that 1970 housing prices be nonmissing would have further reduced the sample to include just 37,519 census tracts, 708 of the NPL sites, and 353 of the 1982 HRS sites.

The subsequent analysis uses three different groupings of census tracts. The first conducts the analysis at the census tract level. The second implements an analysis among census tracts that share a border with the tracts that contain the hazardous waste sites (but excludes the tracts that contain the sites). In this case, each observation is comprised of the weighted average of all variables across these neighboring tracts, where the weights are the 1980 populations of the tracts.

The unit of observation in the third grouping is the land area within circles of varying radii that are centered at the sites. For these observations, the census variables are calculated as the weighted means across the portion of tracts that fall within the relevant circle. The weights are the fraction of each tract's land area within the relevant circle multiplied by its

1980 population.[15] In choosing the optimal radius, we attempted to balance the conflicting goals of requiring houses to be near enough to the sites so that it is plausible that residents would value a clean-up and making the area large enough so that implausibly large increases in housing prices aren't required for clean-ups to pass a cost-benefit test. In the subsequent tables, we focus on circles with radii of 2-miles and 3-miles.[16] The mean 1980 values of the housing stocks in these circles are \$311 and \$736 million and the mean (median) number of census tracts that are at least partially inside these circles are 9.9 (8) and 18.2 (12), respectively.

We also collected a number of variables about the hazardous waste sites. All HRS composite scores, as well as separate groundwater, surface water, and air pathway scores, were obtained from various issues of the Federal Register. The same source was used to determine the dates of NPL listing. The EPA provided a data file that reported the dates of the release of the ROD, initiation of clean-up, completion of remediation (i.e., construction complete), and deletion from the NPL for sites that achieved these milestones. Information on each NPL site's size in acres comes from the RODs. Finally, we collected data on the expected costs of clean-up before remediation was initiated and estimated actual costs for sites that reached the construction complete stage. Greenstone and Gallagher's (2005) Data Appendix provides more information on the costs of clean-ups (also see [88]).

3.4.2 Summary Statistics

The analysis is conducted with two samples of hazardous waste sites. The first is called the "All NPL Sample" and includes the 1,398 hazardous waste sites in the 50 US states and the District of Columbia that were placed on the NPL by January 1, 2000. The second is the "1982 HRS Sample" and is comprised of the 690 hazardous waste sites tested for inclusion on the initial NPL.

Table 1 presents summary statistics on the hazardous waste sites in these samples. The entries in column (1) are from the All NPL Sample and are limited to sites in a census tract for which there is non-missing housing price data in 1980, 1990, and 2000. After these sample restrictions, there are 985 sites, which is more than 70% of the sites placed on the NPL by 2000. Columns (2) and (3) report data from the 1982 HRS Sample. The column (2) entries are based on the 487 sites located in a census tract with complete housing price data. Column

[15] A limitation of the GIS determined circle approach is that street address level data on housing prices and the covariates is unavailable. We assign a census tract's average to the portion of the tract that falls within the circle, which is equivalent to assuming that there is no heterogeneity in housing prices or other variables within a tract.

[16] The use of a 3-mile radius is consistent with the EPA's and scientific community's positions on the distance from a Superfund site that the contaminants could be expected to impact human health. The 1982 Federal Register reports, "The three-mile radius used in the HRS is based on EPA's experience that, in most cases currently under investigation, contaminants can migrant to at least this distance. It should be noted that no commentators disagreed with the selection of three miles for technical or scientific reasons" (Federal Register July 16, 1982).

(3) reports on the remaining 189 sites located in census tracts with incomplete housing price data (generally due to missing 1980 data). 14 sites are outside of the continental United States and were dropped from the sample.

Panel A reports on the timing of the sites' placement on the NPL. Column (1) reveals that about 75% of all NPL sites received this designation in the 1980s. Together, columns (2) and (3) demonstrate that 443 of the 676 sites in the 1982 HRS Sample eventually were placed on the NPL. This number exceeds the 400 sites that Congress set as an explicit goal, because, as discussed above, some sites with initial scores below 28.5 were rescored and then received scores above the threshold qualifying them for the NPL. Panel B demonstrates that mean HRS scores are similar across the columns.

Panel C reports on the size of the hazardous waste sites measured in acres, which is available for NPL sites only. The median site size ranges between 25 and 35 acres across the samples. The means are substantially larger due to a few very large sites. The modest size of most sites suggests that any expected effects on property values are likely to be confined to relatively small geographic areas around the sites.

Panel D reveals that the clean-up process is slow. The median time until the different milestones are achieved is reported, rather than the mean, because many sites have not reached all of them yet. 198 (16) of the NPL sites in column (2) received either the construction complete or deleted designation by 2000 (1990). For this reason, we focus on changes in housing prices and quantities between 1980 and 2000. We also assess how rental rates change as sites progress through the clean-up process.

Panel E reports the expected costs of clean-up for NPL sites, and F details expected and actual costs among sites that are construction complete or deleted. The expected costs are measured before any remediation activities have begun, while actual costs are our best estimates of total remediation related expenditures assessed after the site is construction complete. We believe this is the first time these variables have been reported for the same sites. In the 1982 HRS Sample that we focus on (i.e., column (2)), the mean and median expected costs are $27.5 million and $15.0 million.

Among the construction complete sites in the 1982 HRS Sample, the mean actual costs exceed the expected costs by about 55%. We multiply the overall mean expected cost of $27.5 million by 1.55 to obtain an estimate of the mean actual costs of clean-up in the 1982 HRS Sample of $43 million. This estimate of costs understates the true costs, because it does not include the legal costs or deadweight loss associated with the collection of funds from private parties or taxes, nor does it include each site's share of the EPA's costs of administering the Superfund program. Nevertheless, it is the best available estimate and is contrasted with the estimated benefits of Superfund clean-ups in the remainder of the paper.

A comparison of columns (2) and (3) across the panels reveals that the sites with and without complete housing price data are similar on a number of dimensions. For example, the mean HRS scores conditional on scoring above and below 28.5 are remarkably similar. Further, the median size and various cost variables are comparable in the two columns. Consequently, it seems reasonable to conclude that the sites without complete housing price data are similar to the column (2) sites, suggesting the subsequent results may be externally valid to the 189 sites with missing price data.

Moreover, the sites in column (1) are similar to the sites in column (2) and (3) in size and the two cost variables. The mean HRS scores are a few points lower, but this comparison is not meaningful due to the changes in the test over time and changes in the how the scoring was conducted. Overall, the similarity of the column (1) sites with the other sites suggests that the results from the application of the HRS research design to the 1982 HRS Sample may be informative about the effects of the Superfund clean-ups of sites that were not considered for inclusion on the initial NPL.

We now graphically summarize some features of the 1982 HRS Sample. Figures 2A and 2B present the geographic distribution of the sites with 1982 HRS scores above and below 28.5, respectively. The sites in both categories are spread throughout the United States, but the below 28.5 sites are in fewer states. For example, there are not any below 28.5 sites in Minnesota, Florida, and Delaware. The unequal distributions of sites across the country pose a problem for identification in the presence of localized housing market shocks. To mitigate the influence of these shocks, we emphasize econometric models for changes in housing prices that include state fixed effects.

Figure 3 presents a histogram of the initial HRS scores where the bins are 4 HRS points wide, among the 487 sites in the 1982 HRS Sample. Notably, the EPA considered HRS scores within 4 points to be statistically indistinguishable and reflect comparable risks to human health (EPA 1991). The distribution looks approximately normal, with the modal bin covering the 36.5-40.5 range. Further, there isn't obvious bunching just above or below the threshold, which supports the scientific validity of the HRS scores and suggests that they weren't manipulated. Importantly, 227 sites have HRS scores between 16.5 and 40.5. This set is centered on the regulatory threshold of 28.5 that determines placement on the NPL and the sites constitute the regression discontinuity sample that is utilized in the subsequent analysis.

3.5 Econometric Methods

3.5.1 A Conventional Approach to Estimating the Benefits of Superfund Clean-Ups

Here, we discuss a "conventional" econometric approach to estimating the relationship between housing prices and NPL listing. This approach is laid out in the following system of equations:

$$y_{c2000} = 1(NPL_{c2000})\theta + X'_{c1980}\beta + \epsilon_{c2000}, \tag{3.5}$$

$$1(NPL_{c2000}) = X'_{c1980}\Pi + \eta_{c2000} \tag{3.6}$$

where y_c2000 is the log of the median property value in census tract c in 2000. (In practice, we examine several outcome variables, including rental rates, housing supply, and characteristics of the local population, but for clarity the remainder of this section only refer to house prices.) The indicator variable $1(NPL_{c2000})$ equals 1 only for observations from census tracts that contain (or areas near) a hazardous waste site that has been placed on the NPL by 2000. Thus, this variable takes on a value of 1 for any of the Superfund sites in column (1) of Table 1, not just those that were on the initial NPL. The vector X_{c1980} includes determinants of housing prices measured in 1980, which may also determine NPL status. ϵ_{c2000} and η_{c2000} are the unobservable components of housing prices and NPL status, respectively.

A few features of the X vector are noteworthy. First, this vector is restricted to 1980 values of the variables to avoid confounding the effect of NPL status with "post-treatment" changes in these variables that may be due to NPL status. Second, the 1980 value of the dependent variable, y_{c1980}, is included in X_{c1980} to adjust for permanent differences in housing prices across tracts and the possibility of mean reversion in housing prices. Third, to account for local housing market shocks, we emphasize results from specifications that include a full set of state fixed effects.

Fourth, in many applied hedonic papers, the vector of controls is limited to housing and neighborhood characteristics (e.g., number of bedrooms, school quality, and air quality). Mean household income and similar variables are generally excluded, because they are considered "demand shifters" and are needed to identify the bid function. This exclusion restriction is invalid if, for example, individuals treat wealthy neighbors as an amenity, which seems likely. The subsequent analysis is agnostic about which variables belong in the X vector and reports estimates that are adjusted for different combinations of the variables available in the Census data. See the Data Appendix for the full set of covariates.

The coefficient θ measures the effect of NPL status on 2000 property values, after controlling for 1980 mean property values and the other covariates. In this conventional approach, we utilize data from the entire country, so θ tests for differential housing price appreciation between census tracts with NPL sites and the rest of the country. Consistent estimation of θ requires $E[\epsilon_{c2000}\eta_{c2000}] = 0$ or that unobserved determinants of housing prices do not covary with NPL status (after adjustment for X_{c1980}). This conventional approach rests on the assumption that linear adjustment for the limited set of variables available in the Census removes all sources of confounding.

3.5.2 A Quasi-Experimental Approach based on 1982 HRS Scores

This subsection discusses the paper's quasi-experimental identification strategy that differs from the conventional one in three important aspects. First, we restrict the sample to the census tracts containing the 487 sites in the 1982 HRS Sample with complete housing price data. Thus, all observations are from tracts with sites that the EPA judged to be among the nation's most dangerous in 1982. If, for example, the $\beta's$ differ across tracts with and without hazardous waste sites or there are differential trends in housing prices in tracts with and without these sites, then this approach is more likely to produce consistent estimates.

Second, we use an instrumental variables (IV) strategy to account for the possibility of the endogenous rescoring of sites. More formally, we replace equation (6) with:

$$1(NPL_{c2000}) = X'_{c1980}\Pi + 1(HRS_{c82} > 28.5)\delta + \eta_{c2000} \tag{3.7}$$

where $1(HRS_{c82} > 28.5)$ serves as an instrumental variable. This indicator function equals 1 for census tracts with a site that has a 1982 HRS score exceeding the 28.5 threshold. We then substitute the predicted value of $1(NPL_{c2000})$ from the estimation of equation (7) in the fitting of (5) to obtain an estimate of θ_{IV}. In this IV framework, θ_{IV} is identified from the variation in NPL status that is due to a site having a 1982 HRS score exceeding 28.5.

For θ_{IV} to provide a consistent estimate of the HPS gradient, the instrumental variable must affect the probability of NPL listing without having a direct effect on housing prices. The next section will demonstrate that the first condition clearly holds. The second condition requires that the unobserved determinants of 2000 housing prices are orthogonal to the portion of the nonlinear function of the 1982 HRS score that is not explained by X_c1980. In the simplest case, the IV estimator is consistent if $E[1(HRS_{c82} > 28.5)\epsilon_{c2000}] = 0$.

The third feature of the quasi-experiment is the availability of a regression discontinuity (RD) design that is implicit in the $1(\bullet)$ function that determines NPL eligibility. The RD design can produce consistent estimates of $\theta_I V$ even if $E[1(HRS_{c82} > 28.5)\epsilon_{c2000}] \neq 0$ over the entire 1982 HRS Sample. It is important to highlight that the RD approach only provides estimates of the treatment effect at the regulatory discontinuity (i.e., $HRS = 28.5$). To extend the external validity of the RD estimates to the full 1982 HRS Sample, it is necessary to assume a homogeneous treatment effect in this sample.

The RD approach is implemented in three different ways. In the first, a quadratic in the 1982 HRS score is included in X_c1980 to partial out any correlation between residual housing prices and the indicator for a 1982 HRS score exceeding 28.5. This approach relies on the plausible assumption that residual determinants of housing price growth do not change discontinuously at the regulatory threshold. The second regression discontinuity approach involves implementing the IV estimator on the regression discontinuity sample of 227 sites with 1982 HRS scores between 16.5 and 40.5. Here, the identifying assumption is that all else is held equal in the "neighborhood" of the regulatory threshold. More formally, it is $E[1(HRS_{c82} > 28.5)\epsilon_{c2000}]|16.5 < 1982HRS < 40.5] = 0$.

Recall, the HRS score is a nonlinear function of the ground water, surface water, and air migration pathway scores. The third regression discontinuity method exploits knowledge of this function by including the individual pathway scores in the vector X_{c1980}. All three regression discontinuity approaches are demanding of the data and this is reflected in higher sampling errors.

The key feature of the quasi-experimental approach is to restrict the sample to the areas surrounding the 487 sites in the 1982 HRS Sample. Among these sites, a simple comparison of outcomes between NPL and non-NPL sites is likely to mitigate concerns about confounding associated with the conventional approach. The other two features refine the comparisons within this sample. The use of $1(HRS_{c82} > 28.5)$ as an instrumental variable for $1(NPL_{c2000})$ accounts for the possibility of the endogenous rescoring of sites. The RD design offers a potentially valid "control function" solution to any remaining concerns about confounding.

Finally, the primary focus of the housing price regressions is to conduct a cost-benefit analysis of Superfund clean-ups. Specifically, we report p-values from tests that the coefficient on the NPL indicator is large enough so that the aggregate change in housing prices exceeds the mean costs of a Superfund clean-up ($43 million in the 1982 HRS sample). This assumes that clean-up benefits are entirely reflected in local housing prices, which is equivalent to assuming that the housing supply curve is perfectly inelastic and that all benefits occur in the local housing market. Although we report whether the estimates of θ are statistically different from zero, the cost-benefit tests are more meaningful in this setting.

3.6 Empirical Results

3.6.1 Balancing of Observable Covariates

This subsection examines the comparisons that underlie the subsequent least squares and quasi-experimental estimates of the effect of NPL status on housing price growth. We begin by assessing whether NPL status and the $1(HRS_{c82} > 28.5)$ instrumental variable are orthogonal to the observable predictors of housing prices. Formal tests for the presence of omitted variables bias are of course impossible, but it seems reasonable to presume that research designs that balance the observable covariates across NPL status or $1(HRS_{c82} > 28.5)$ may suffer from smaller omitted variables bias ([14]). Further, if the observables are balanced, consistent inference does not depend on functional form assumptions on the relations between observable covariates and housing prices.

Table 2 shows the association of NPL status and $1(HRS_{c82} > 28.5)$ with potential determinants of housing price growth measured in 1980. Column (1) reports the means of the variables listed in the row headings in the 985 census tracts with NPL hazardous waste sites and complete housing price data. Column (2) displays the means in the 41,989 census tracts that neither contain a NPL site nor share a border with a tract containing one. Columns (3) and (4) report on the means in the 181 and 306 census tracts with hazardous waste sites with 1982 HRS scores below and above the 28.5 threshold, respectively. Columns (5) and (6) repeat this exercise for the 90 and 137 tracts below and above the regulatory threshold in the regression discontinuity sample. The remaining columns report p-values from tests that the means in pairs of the first six columns are equal. P-values less than 0.01 are denoted in bold.

Column (7) compares the means in columns (1) and (2) to explore the possibility of confounding in the least square approach. The entries indicate that 1980 housing prices are more than 20% lower in tracts with a NPL site. Moreover, the tracts with NPL sites have lower population densities, lower household incomes, and mobile homes account for a higher fraction of the housing stock (8.6% versus 4.7%). Overall, the hypothesis of equal means can be rejected at the 1% level for 22 of the 26 potential determinants of housing prices. Due to this confounding of NPL status, it may be reasonable to assume that least squares estimation of equation (5) will produce biased estimates of the effect of NPL status.

Columns (8) and (9) compare all tracts with hazardous wastes that have 1982 HRS scores below and above the 28.5 regulatory threshold and those in the regression discontinuity sample, respectively. It is immediately evident that by narrowing the focus to these tracts, the differences in the potential determinants of housing prices are greatly mitigated (e.g., see population density and % mobile homes). This is especially so in the regression discontinuity sample where the hypothesis of equal means cannot be rejected at the 3% level for any of the 27 variables. Notably, the differences in the means are substantially reduced for many of the variables, so the higher p-values do not simply reflect the smaller samples (and larger sampling errors).

One variable that remains a potential source of concern is 1980 housing prices in the sites' tracts and circles of 2- and 3-mile radii around the sites. The differences are greatly reduced in the 1982 HRS Sample, relative to columns (1) and (2), but they are not eliminated (although they are statistically insignificant in the circle samples). Table 4 in [54] demonstrates that the difference in prices in the sites' census tracts disappears after adjustment for 1980 housing, economic, and demographic variables. Overall, the entries suggest that the above and below 28.5 comparison, especially in the regression discontinuity sample, reduces the confounding of NPL status.

3.6.2 Conventional Estimates of the Impact of Clean-ups on Property Values with Data from the Entire US

Table 3 presents the first ever large-scale effort to test the effect of Superfund clean-ups on property value appreciation rates. Specifically, it reports the regression results from conventional approaches that involve fitting 3 least squares versions of equation (5) for 2000 housing prices on data from the entire US. The entries report the coefficient on the NPL indicator and its heteroskedastic-consistent standard error below in parentheses. The exact covariates in each specification are noted in the row headings at the bottom of the table and are described in more detail in the Data Appendix.

In Panel A, 985 observations are from census tracts that contain a hazardous waste site that had been on the NPL at any time prior to 2000. The remainder of the sample is comprised of the 41,989 observations on the tracts with complete housing price data that neither have a NPL site nor are adjacent to a tract with a NPL site. The remaining Panels use slightly different samples. In Panel B, the observations from each tract with a NPL site in the Panel A sample are replaced with the observations based on the 3-mile radius circles around the NPL sites. Panels C and D are identical to A and B, except that the set of NPL sites is restricted to those in the 1982 HRS Sample placed on the NPL by January 1, 2000; these results are a benchmark for comparison with the preferred quasi-experimental ones.

The Panel A results show that this conventional approach finds a positive association between NPL listing and housing price increases in the sites' tracts between 1980 and 2000. Specifically, the estimates indicate that housing prices grew by 4.0% to 6.7% (measured in ln points) more in tracts with a site placed on the NPL. All of these estimates would easily be judged statistically significant by conventional criteria. The column (3) estimate of 6.7% is the most reliable one, because it is adjusted for all unobserved state-level determinants of housing price growth.

Panel B explores the growth of housing prices within 3 miles of the NPL sites to summarize the total gain in housing prices. All of the estimates are statistically different from zero and imply that the placement of a site on the NPL is associated with a substantial increase in housing prices within three miles of the site. The column (3) specification indicates a precisely estimated gain in prices of 10.6%. In this sample, the 1980 aggregate value of the housing stock is $855 million and the mean cost of a clean-up is $39 million, so we test whether the change in housing prices exceeds 4.6%. The null that the clean-ups pass the cost-benefit test cannot be rejected in any of the specifications.

The own census tract results in Panel C are similar to those in A. The 3-mile radius circle results in D also indicate large increases in housing prices. The point estimates from the richer specifications are about twice as large as those in B. Further, they all indicate that Superfund passes this cost-benefit test.

It is worth emphasizing that three features of the evidence presented so far suggest that the Table 3 estimates may be unreliable. First, Table 2 demonstrated that NPL status is confounded by many variables. Second, four of the six 3-mile radius sample point estimates exceed the own census tract estimates. This seems suspicious, because it seems reasonable to expect the impact on housing prices to be greater closer to the sites, especially in light of their relatively small size (recall, the median size is less than 30 acres). Third, the point estimates from the 3-mile samples are unstable across specifications, so the exact choice of controls plays a large role in any conclusions. For example, in Panel D, the implied increase in housing prices ranges from 4.6% to 19.1%.[17]

3.6.3 Quasi-Experimental Estimates of the Impact of NPL Status on Housing Prices

We now turn to the preferred quasi-experimental approach. For the remainder of the paper, we use the 1982 HRS sites as the basis for the samples. In a few cases, which are noted, we

[17] The point estimate on the NPL indicator is especially sensitive to the choice of functional form for two controls: the number of housing units and number of owner occupied units in both Panels B and D. This likely reflects the fact that the values of these variables differ substantially between the observations on the 3-mile circles and the census tracts. It also underscores the importance of unverifiable functional form assumptions when the variables are not balanced across the areas with and without NPL sites.

focus on the subset of sites with 1982 HRS scores between 16.5 and 40.5 that form the RD sample.

Figure 4 plots the bivariate relation between the probability that a site was placed on the NPL by 2000 and its initial HRS score among the 487 sites in the 1982 HRS Sample. The plots are done separately for sites above and below the 28.5 threshold and come from the estimation of nonparametric regressions that use the [35] tricube weighting function and a bandwidth of 0.5. Thus, they represent a moving average of the probability of NPL status across 1982 HRS scores. The data points represent the mean probabilities in the same 4-unit intervals of the HRS score as in Figure 3.

The figure presents dramatic evidence that an initial HRS score above 28.5 is a strong predictor of NPL status. Virtually all sites with initial scores greater than 28.5 were placed on the NPL by 2000. The nonzero probability of NPL placement by 2000 among sites with an initial score below 28.5 is explained by rescoring. A statistical model reveals that a HRS score above 28.5 is associated with an 83% increase in the probability of placement on the NPL. In the context of the IV approach, it is evident that there is a powerful first-stage relationship.

Table 4 presents quasi-experimental estimates of the effect of NPL status on housing prices in 2000. In Panel A, the observations are from the census tracts containing the 487 hazardous waste sites in the 1982 HRS Sample. In Panel B, each observation is comprised of the average of all variables across tracts that share a border with these tracts. In Panels C and D, the sample includes the land area within circles with radii of 2 and 3 miles centered at each site's longitude and latitude. The means of the 1980 values of the total housing stock in the four samples are \$71, \$552, \$311, and \$736 million, respectively.

The column (1) specification adjusts for 1980 housing prices only and is based on the least squares fitting of equation (5). The remainder of the specifications uses the IV strategy outlined in equations (5) and (7). The controls in columns (2)-(4) are identical to the three specifications in Table 3.

The specifications in columns (5) - (7) are the three RD-style approaches that all build on the column (4) specification. In columns (5) and (6), the 1982 HRS score and its square and the individual pathway scores are added to the column (4) specification, respectively. Column (7) fits the column (4) specification on the RD sample of the 227 sites with 1982 HRS scores between 16.5 and 40.5. The sample and specification details are noted in the row headings at the bottom of the table.

The Panel A results suggest that a site's placement on the NPL has a modest impact on the growth of property values in its own census tract, relative to tracts with sites that narrowly missed placement on the NPL. The point estimates indicate an increase in prices that ranges from 0.7% to 4.7%, but they all have associated t-statistics less than two. The regression discontinuity specifications in columns (5) - (6) produce the smallest point estimates (although they are also the least precise).

Panel B presents the adjacent tract results. The point estimates from the most credible specifications in columns (4) - (7) range between -0.6% and 1.5%. Further, zero cannot be rejected at conventional levels for any of them. Thus, there is little evidence of meaningful gains in housing prices outside the site's own census tract.

Panels C and D summarize the total gain in housing prices associated with a site's placement on the NPL by using the 2- and 3-mile radius circle samples. They also report whether the clean-ups pass cost-benefit tests analogous to those in Table 3. The threshold housing price gains are 13.8% and 5.8%.

The circle sample results provide further evidence that the NPL designation has little effect on housing prices. In the columns (4) - (7) specifications, six of the eight point estimates are negative and the largest indicates an increase of just 2.3%. Further in all seven of the 2-mile specifications and the most reliable 3-mile ones, the null that the gain in housing prices exceeds the break-even threshold is rejected at conventional significance levels. These findings stand in sharp contrast to the conclusions suggested by the results from the conventional approach in Table 3.

Figure 5 provides an opportunity to better understand the source of these regression results. It plots the nonparametric regressions of 2000 residual housing prices (after adjustment for the column (4) covariates) against the 1982 HRS score in the 2-mile radius sample. The nonparametric regression is estimated separately below (dark line) and above (light line) the 28.5 threshold. It confirms that there is little association between 2000 residual housing prices and 1982 HRS scores. A comparison of the plots at the regulatory threshold is of especial interest in light of the large increase in NPL status there. It is apparent that the moving averages from the left and right are virtually equal at the threshold.

We conducted a number of other specification checks. We failed to find evidence of greater price responses in census tracts with the highest population densities, where quantity responses are more constrained. Additionally, the results are robust to several other specification checks that include using the ln of the mean (rather than the median) house price as the dependent variable, using the difference between the lns of 2000 and 1980 house prices as the dependent variable, controlling for the fraction of census tracts within the 2-mile circles with a boundary change between 1980 and 2000, testing for a price response in 1990, and adding the 1970 values of the controls (including ln 1970 housing prices) as separate covariates to adjust for pre-existing trends in the subsample where these variables are available.[18]

[18]The own census tract sample regression results for some of these specification checks are presented in [54]. That version of the paper also reports on a test of whether there was greater housing price appreciation near sites where the groundwater was heavily contaminated and residents use well water for drinking. We assumed that clean-ups would be highly valued in these areas; however this test failed to find significant evidence of differential house price appreciation in these areas. There are eleven sites in the 1982 HRS Sample where all RODs received the "no further action" classification so no remediation activities took place at them. The regression results are virtually identical to those presented in Table 4 when the observations from near these sites are dropped. Additionally, we implemented the regression discontinuity estimators without instrumenting for NPL status. This approach produced generally smaller estimated increases in house prices than those in Table 4 (in fact they are generally negative). Finally, we would have liked to test whether the effects of clean-ups differed for large sites or ones where the estimated costs of clean-up are high

These specification checks all lead to the same qualitative finding that a site's addition to the NPL has little effect on the growth of nearby housing prices nearly 20 years later. It is impossible to rule out positive impacts on prices, but the most reliable specifications fail to provide a single case where the estimated price increases exceed the costs of the clean-ups.

3.6.4 Quasi-Experimental Estimates of the Impact of Superfund Clean-Ups on Rental Rates

We now turn to using the ln median rental rates as the outcome variable. Rental units account for roughly 20% of all housing units and generally differ on observable characteristics from owner occupied homes. Part of this outcome's appeal is that rental rates are a measure of the current value of housing services, so it is possible to abstract from the problem with the housing price outcome that individuals' expectations about time until the completion of the clean-up are unknown. Further, it is possible to test whether the impact on the value of local housing services varies at different stages of the clean-ups.

Table 5 presents separate estimates of the effect of the different stages of the remediation process on the ln median rental rate from the 2-mile radius circle sample. We stack equations for 1990 and 2000 ln rental rates, so there are two observations per county. The 1980 housing characteristics variables are calculated across rental units, rather than across owner occupied units as in housing price analysis. The effects of the controls listed in the row headings are allowed to differ in 1990 and 2000.

The indicator variable for NPL status is replaced by three independent indicator variables. They are equal to 1 for sites that at the time of the observation (i.e., 1990 or 2000) were: placed on the NPL but no ROD had been issued; issued a ROD but were not completely remediated; and "construction complete" or deleted from the NPL. The instruments are the interactions of the indicator for a 1982 HRS score above 28.5 and these three independent indicators. The table reports the three point estimates and their standard errors, which allow for clustering at the site level, along with the p-value from an F-test that they are equal. The number of sites in each category and the mean HRS score are listed in brackets.

There is some evidence that higher voter turnout and per capita income are associated with the speed through which a site moves through the clean-up process and the stringency of clean-ups ([55]; [56]; [100]; [96]). For this reason, the two-stage least squares strategy is unlikely to purge these sources of endogeneity. Consequently, these three parameter estimates should be considered associational or descriptive.

There are a few important findings. First, sites in the "NPL Only" category have been on the NPL for either 7 or 17 years, but the EPA has not developed a remediation plan for them yet. The estimates from the more reliable specifications in columns (2) through (5) suggest that there is little effect on rental rates near these sites. This finding <u>contradicts</u> the "stigma" hypothesis' key prediction that a site's placement on the NPL leads to an

(so called "mega" sites) but the size and estimated cost data are only available for NPL sites.

immediate reduction in the value of housing services near the site as nearby residents revise upwards their expectation of the risk they face from the site.[19]

Second, in the more reliable specifications, 3 of the 4 estimates for the "Construction Complete or NPL Deletion" indicator are negative and zero cannot be rejected for any of them. This finding is telling, because these sites have been fully remediated and yet there is little effect on rental rates.

Third, the null that the three parameter estimates are equal cannot be rejected in any of the specifications. This finding demonstrates that the approximately zero effect on housing prices is not due to the averaging of a positive effect at fully remediated sites and a negative effect at sites where remediation is incomplete or hasn't been initiated. Overall, these results complement the housing price findings that Superfund clean-ups have small effects on the value of local housing services.

3.6.5 Quasi-Experimental Estimates of the Impact of NPL Status on Sorting

If consumers value Superfund clean-ups, then the clean-ups should cause individuals to sort such that there is an increase in the number of people who place a high value on environmental quality living near NPL sites. Table 7 tests for changes in residents' income and wealth (i.e., education) and demographic characteristic that proxy for taste for environmental quality, as well as total population. The entries report the parameter estimate and standard error on the dummy for NPL status from the same five specifications in Table 5. The sample is the 2-mile radius circles sample based on the 1982 HRS Sample sites. The means of the 1980 variable and its 2000-1980 change are in square brackets.

The estimated impacts of the NPL designation on the measures of income and wealth are inconsistent across specifications with about half positive and half negative. The null of a zero impact cannot be rejected in any of the more reliable specifications. We had hypothesized that the clean-ups would increase the demand for these areas among families with young children. However, Panel B fails to provide any meaningful evidence that the NPL designation leads to changes in the age composition of a tract's population. It is unclear how to apply the environmental justice hypothesis to a setting where environmental quality increases while prices are largely unchanged. Although the interpretation is unclear, there is some evidence that the percentage of blacks declines but none of the estimates would be judged to be statistically different from zero. Finally, the instability of the point estimates across specifications in Panel C suggests that there is little effect on total population.

Notably, this table's qualitative findings are unchanged by the inclusion of 1980 housing

[19]The stigma hypothesis is poorly defined, but one version is that a site's placement on the NPL causes nearby residents to revise their expectation of its health risk upwards permanently so that the value of nearby housing services is lower even after remediation is completed. [59] reviews the stigma literature. [80] and [81] provide empirical case study tests.

prices and housing characteristics as covariates. Overall, there is little evidence that the NPL designation is associated with changes in variables that proxy for shifts in demand for environmental quality.

3.6.6 Quasi-Experimental Estimates of the Impact of NPL Status on Housing Supply

An increase in the supply of housing units in the vicinity of a NPL site would provide evidence that Superfund clean-ups increase the value of the surrounding land. In Table 7, we test this possibility with the 2- and 3-mile radius samples, using the same five specifications from Tables 5 and 6. These results are also inconsistent across specifications. The most reasonable conclusion is that the assignment of the NPL designation has little effect on the supply of housing.

3.7 Interpretation and Policy Implications

This paper has shown that across a wide range of housing market outcomes, there is little evidence that Superfund clean-ups increase local residents' welfare substantially. In light of the significant resources devoted to these clean-ups and the claims of large health benefits, this finding is surprising. This section reviews three possible explanations.

First, the individuals that choose to live near these sites before and after the clean-ups may have a low willingness to pay to avoid exposure to hazardous waste sites. In this case, society provides these individuals a good that they don't value highly. It is possible (and perhaps likely) that there are segments of the population with a high WTP to avoid exposure to hazardous waste sites. It may even be the case that the population average WTP is substantial. However, the policy relevant parameter is the WTP of the population that lives near these sites, and this is the parameter that the paper has estimated.

Second, consumers may believe that the clean-ups do not appreciably alter the health risks of living near a Superfund site. In fact, the epidemiological literature has not found decisive evidence of substantial health benefits from the clean-ups ([101]; [38]). Consequently, consumers may believe that the reductions in risk are small and rationally place a low value on them. Of course, the discovery of large health improvements in the future could cause consumers to increase their valuations of the clean-ups and this would presumably be reflected in the housing market.[20]

[20] Another possibility is that consumers are imperfectly informed about the location of Superfund sites and their clean-ups. We think this is unlikely, because local media often devote extensive coverage to local Superfund sites and their clean-ups. Further, at least a few states (e.g., Alaska and Arizona) require home sellers to disclose whether there are hazardous waste sites in close proximity. See [41] on the capitalization of perceived health risks.

Third, the non-NPL sites may have also received complete remediations under state or local land reclamation programs. In this case, a zero result is to be expected since both NPL and non-NPL sites would have received the same treatment. We investigated this possibility by conducting an extensive search for information on remediation activities at these sites.[21]

From these investigations, we concluded that the clean-up activities were dramatically more ambitious and costly at NPL sites. For example, we were unable to find evidence of any remediation activities by 2000 at roughly 60% of the non-NPL sites. Further, among the remaining 40% of non-NPL sites where there was evidence of clean-up efforts, the average expenditure was roughly $3 million. This is about $40 million less than our estimate of the average cost of a Superfund clean-up. This difference is not surprising, because the state and local clean-ups were often limited to restricting access to the site or containing the toxics, rather than trying to achieve Superfund's goal of returning the site to its "natural state." Nevertheless, some remediation took place at these sites, so it may be appropriate to interpret the results as the impact of the additional $40 million cost of Superfund clean-ups.

In our view, the most likely explanations are that the people that choose to live near these sites don't value the clean-ups or that consumers have little reason to believe that the clean-ups substantially reduce health risks. In either case, the results mean that local residents' gain in welfare from Superfund clean-ups falls well short of the costs. Unless there are substantial benefits that are not captured in local housing markets , less ambitious clean-ups like the erection of fences, posting of warning signs around the sites, and simple containment of toxics might be a more efficient use of resources.[22]

3.8 Conclusions

This study has used the housing market to develop estimates of the local welfare impacts of Superfund sponsored clean-ups of hazardous waste sites. The basis of the analysis is a comparison of housing market outcomes in the areas surrounding the first 400 hazardous waste sites chosen for Superfund clean-ups to the areas surrounding the 290 sites that narrowly missed qualifying for these clean-ups. We find that Superfund clean-ups are associated with economically small and statistically indistinguishable from zero local changes in residential property values, property rental rates, housing supply, total population, and the types of individuals living near the sites. These findings are robust to a series of specification checks,

[21]Specifically, we filed freedom of information act requests with the EPA for information on these sites and followed any leads from these documents. We also searched the Superfund web site and the sites of state departments of environmental quality and used internet search engines. Additionally, we contacted national and regional EPA personnel and state and local environmental officials. Although we expended considerable effort in these searches, there is no centralized database about these sites so we cannot be certain that further efforts wouldn't turn up different information.

[22]It is possible that there are other benefits of these clean-ups that are not captured in the local housing market, including health and aesthetic benefits to individuals that do not live in close proximity to Superfund sites, reductions in injuries to ecological systems, and protection of ground water.

including the application of a regression discontinuity design based on knowledge of the selection rule. Overall, the preferred estimates suggest that the local benefits of Superfund clean-ups are small and appear to be substantially lower than the $43 million mean cost of Superfund clean-ups.

More broadly, this paper makes two contributions. First, it models the consequences of a quasi-experiment that improves a local amenity in the context of the hedonic model. The key theoretical findings are that if consumers value the amenity, then there will be increases in local housing prices and new home construction. Further, there will be taste-based sorting such that individuals that place a high value on the amenity will move to areas where they can consume it. Second, it contributes to a growing body of research ([26]; [34]; [43]) demonstrating that it is possible to identify research designs that mitigate the confounding that has historically undermined the credibility of conventional hedonic approaches to valuing non-market goods. Perhaps most importantly, this paper has demonstrated that the combination of quasi-experiments and hedonic theory are a powerful method to use markets to value environmental and other non-market goods.

3.9 Data Appendix

This data appendix provides information on a number of aspects of the data set that we compiled to conduct the analysis for this paper. Due to space constraints, this is an abridged version of the data appendix that is available in [54]. The longer data appendix includes details on the variables on: the size of the hazardous waste sites; whether a site has achieved the construction complete designation; the placement of sites into 2000 Census tracts; and the determination of expected and actual remediation costs.

3.9.1 Covariates in Housing Price and Rental Rate Regressions

The following are the control variables used in the housing price and rental rate regressions. They are listed by the categories indicated in the row headings at the bottom of these tables. All of the variables are measured in 1980 and are measured at the census tract level (or are the mean across sets of census tracts, for example tracts that share a border with a tract containing a hazardous waste site)[23]:

1. *1980 Ln House Price*
   ln mean value of owner occupied housing units in 1980 (note: the median is unavailable in 1980)

2. *1980 Housing Characteristics*
   total housing units (rental and owner occupied); % of total housing units (rental and

[23]In the rental regressions in Table 5, the owner occupied housing variables are replaced with renter occupied versions of the variables.

owner occupied) that are occupied; total housing units owner occupied; % of owner occupied housing units with 0, 1, 2, 3, 4, and 5 or more bedrooms; % of owner occupied housing units that are detached; % of owner occupied housing units that are attached % of owner occupied housing units that are mobile homes; % of owner occupied housing units built within last year, 2 to 5 years ago, 6 to 10 years ago, 10 to 20 years ago, 20 to 30 years ago, 30 to 40 years ago, more than 40 years ago; % of all housing units without a full kitchen; % of all housing units that have no heating or rely on a fire, stove, or portable heater; % of all housing units without air conditioning; and % of all housing units without a full bathroom.

3. *1980 Economic Conditions*
   mean household income; % of households with income below poverty line; unemployment rate; and % of households that receive some form of public assistance.

4. *1980 Demographics*
   population density; % of population Black; % of population Hispanic; % of population under age 18; % of population 65 or older; % of population foreign born; % of households headed by females; % of households residing in same house as 5 years ago; % of individuals aged 16-19 that are high school drop outs; % of population over 25 that failed to complete high school; and % of population over 25 that have a BA or better (i.e., at least 16 years of education)

3.9.2 Assignment of HRS Scores and their Role in the Determination of the NPL

The HRS test scores each pathway from 0 to 100, where higher scores indicate greater risk.[24] The pathway scores are a multiplicative function of the waste characteristics, likelihood of release, and characteristics of the potentially affected population. The logic is, for example, that if twice as many people are thought to be affected via a pathway then the pathway score should be twice as large.

The final HRS score is calculated using the following equation: $HRSScore = [(S_{gw}^2 + S_{sw}^2 + S_a^2)/3]^{1/2}$, where S_gw, S_sw, and S_a, denote the ground water migration, surface water migration, and air migration pathway scores, respectively. It is evident that the effect of an individual pathway on the total HRS score is proportional to the pathway score. (In 1990, the EPA revised the HRS test and soil became a fourth pathway.)

[24] The capping of individual pathways and of attributes within each pathway is one limiting characteristic of the test. There is a maximum value for most scores within each pathway category. Also, if the final pathway score is greater than 100 then this score is reduced to 100. The capping of individual pathways creates a loss of precision of the test since all pathway scores of 100 have the same effect on the final HRS score but may represent different magnitudes of risk. See the EPA's *Hazard Ranking System Guidance Manual* for further details on the determination of the HRS score.

HRS scores can't be interpreted as strict cardinal measures of risk. A number of EPA studies have tested how well the HRS represents the underlying risk levels based on cancer and non-cancer risks ([28]). The EPA has concluded that the late 1980s version of the HRS test is an ordinal test but sites with scores within 4 points of each pose roughly comparable risks to human health ([12]).[25]

From 1982-1995, the EPA assigned all hazardous waste sites with a HRS score of 28.5 or greater to the NPL. Additionally, the original legislation gave every state the right to place one site on the NPL without the site having to score at or above 28.5 on the HRS test. As of 2003, 38 states have used their exception. It is unknown whether these sites would have received a HRS score above 28.5. Six of these "state priority sites" were included on the original NPL released in 1983, but due to their missing HRS scores these six sites are excluded from this paper's analysis.

3.9.3 Matching of 2000 Census Tracts to 1980 and 1990 Censuses

The census tract is used as the unit of analysis, because it is the smallest aggregation of data that is available in the 1980, 1990 and 2000 US Census. As noted in the text, year 2000 census tract boundaries are fixed so that the size and location of the census tract is the same for the 1980 and 1990 census data. The fixed census tract data boundaries were provided by Geolytics, a private company. Information on how the 1980 and 1990 census tracts were adjusted to fit the 2000 census tract boundaries can be found on their website at: www.geolytics.com. Further, [54] provide some details.

3.9.4 Neighbor Samples

We use two approaches to define the set of houses outside each site's tract that may be affected by the clean-up. We refer to these sets of houses as "neighbors."

The first approach defines the neighbors as all census tracts that share a border with the tract that contains the site. GIS software was used to find each primary census tract and extract the identity of its adjacent neighbors. In the 1982 HRS sample, the maximum number of neighboring census tracts is 21 and the median is 7. The population of each adjacent census tract was used to weight the housing price, housing characteristics, and demographic variables for each tract when calculating the mean adjacent neighbor values.

The second approach defines neighbors based on circles of varying radii around the exact location of the site. GIS software is used to draw a circle around the point representing the site (generally the center of the site, but sometimes the point associated with the street address). For example in the 2 mile sample, the GIS program draws circles with radii of 2 miles around each of the sites. For a given site, data from all census tracts that fall

[25] The EPA states that the early 1980s version of the HRS test should not be viewed as a measure of "absolute risk", but that "the HRS does distinguish relative risks among sites and does identify sites that appear to present a significant risk to public health, welfare, or the environment" ([2]).

within its 2-mile radius circle (including the tract containing the site) are used to calculate the mean housing values, housing and demographic characteristics, and economic variables. To calculate these weighted means, each census tract within the circle is weighted by the product of its population and the portion of its total area that falls within the circle. For the 2 (3) mile ring the maximum number of tracts inside the ring is 80 (163), with a mean and median of 9.9 and 8 (18.2 and 12).

Finally, we were able to place 487 of the 690 sites in the 1982 HRS sample in census tracts with nonmissing house price data. We obtained the exact longitude and latitude for 483 of these sites. Thus, the circle samples have 483 observations, while the sample size for the own census tract and adjacent neighbor tract samples is 487.

Table 3.1: Summary Statistics on the Superfund Program

	All NPL Sites w/ non-Missing House Price Data (1)	1982 HRS Sites w/ non-Missing House Price Data (2)	1982 HRS Sites w/ Missing House Price Data (3)
Number of Sites	985	487	189
1982 HRS Score Above 28.5	------	306	95
A. Timing of Placement on NPL			
Total	985	332	111
# 1981-1985	406	312	97
# 1986-1989	340	14	9
# 1990-1994	166	4	3
# 1995-1999	73	2	2
B. HRS Information			
Mean Scores | HRS $\geq$ 28.5	41.89	44.47	43.23
Mean Scores | HRS < 28.5	-----	15.54	16.50
C. Size of Site (in acres)			
Number of sites with size data	920	310	97
Mean (Median)	1,187 (29)	334 (25)	10,507 (35)
Maximum	195,200	42,560	405,760
D. Stages of Clean-Up for NPL Sites			
Median Years from NPL Listing Until:			
ROD Issued	------	4.3	4.3
Clean-Up Initiated	------	5.8	6.8
Construction Complete	------	12.1	11.5
Deleted from NPL	------	12.8	12.5
1990 Status Among Sites NPL by 1990			
NPL Only	394	100	31
ROD Issued or Clean-up Initiated	335	210	68
Construction Complete or Deleted	22	16	7
2000 Status Among Sites NPL by 2000			
NPL Only	137	15	3
ROD Issued or Clean-up Initiated	370	119	33
Construction Complete or Deleted	478	198	75
E. Expected Costs of Remediation (Millions of 2000 $s)			
# Sites with Nonmissing Costs	753	293	95
Mean (Median)	$28.3 ($11.0)	$27.5 ($15.0)	$29.6 ($11.5)
95th Percentile	$89.6	$95.3	$146.0
F. Actual and Expected Costs Conditional on Construction Complete (Millions of 2000 $s)			
Sites w/ Both Costs Nonmissing	477	203	69
Mean (Median) Expected Costs	$15.5 ($7.8)	$20.6 ($9.7)	$17.3 ($7.3)
Mean (Median) Actual Costs	$21.6 ($11.6)	$32.0 ($16.2)	$23.3 ($8.9)

Table 3.2: Mean Census Tract Characteristics by Categories of the 1982 HRS Score

Table 3.2: Mean Census Tract Characteristics by Categories of the 1982 HRS Score

	NPL Site by 2000	No NPL Site by 2000	HRS < 28.5	HRS > 28.5	HRS > 16.5 & < 28.5	HRS > 28.5 & < 40.5	P-Value (1) vs. (2)	P-Value (3) vs. (4)	P-Value (5) vs. (6)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
# Census Tracts	985	41,989	181	306	90	137	-----	-----	-----
Superfund Clean-up Activities									
Ever NPL by 1990	0.7574	-----	0.1271	0.9902	0.2222	0.9854	-----	**0.000**	**0.000**
Ever NPL by 2000	1.0000	-----	0.1602	0.9902	0.2667	0.9854	-----	**0.000**	**0.000**
1980 Mean Housing Prices									
Site's Census Tract	58,045	69,904	45,027	52,137	46,135	50,648	**0.000**	**0.000**	0.084
2-Mile Radius Circle Around Site	56,020	-----	48,243	53,081	48,595	52,497	-----	0.016	0.179
3-Mile Radius Circle Around Site	56,839	-----	51,543	54,458	49,434	53,868	-----	0.257	0.126
1980 Housing Characteristics									
Total Housing Units	1,392	1,350	1,357	1,353	1,367	1,319	0.039	0.951	0.575
% Mobile Homes	0.0862	0.0473	0.0813	0.0785	0.0944	0.0787	**0.000**	0.792	0.285
% Occupied	0.9408	0.9330	0.9408	0.9411	0.9412	0.9411	**0.000**	0.940	0.989
% Owner Occupied	0.6818	0.6125	0.6792	0.6800	0.6942	0.6730	**0.000**	0.959	0.344
% 0-2 Bedrooms	0.4484	0.4722	0.4691	0.4443	0.4671	0.4496	**0.000**	0.107	0.417
% 3-4 Bedrooms	0.5245	0.5016	0.5099	0.5288	0.5089	0.5199	**0.000**	0.202	0.586
% Built Last 5 Years	0.1434	0.1543	0.1185	0.1404	0.1366	0.1397	**0.006**	0.050	0.844
% Built Last 10 Years	0.2834	0.2874	0.2370	0.2814	0.2673	0.2758	0.506	0.012	0.723
% No Air Conditioning	0.4903	0.4220	0.5058	0.4801	0.5157	0.5103	**0.000**	0.253	0.870
% Units Attached	0.0374	0.0754	0.0603	0.0307	0.0511	0.0317	**0.000**	0.040	0.297
1980 Demographics & Economic Characteristics									
Population Density	1,407	5,786	1,670	1,157	1,361	1,151	**0.000**	0.067	0.570
% Black	0.0914	0.1207	0.1126	0.0713	0.0819	0.0844	**0.000**	0.037	0.926
% Hispanic	0.0515	0.0739	0.0443	0.0424	0.0309	0.0300	**0.000**	0.841	0.928
% Under 18	0.2939	0.2780	0.2932	0.2936	0.2885	0.2934	**0.000**	0.958	0.568
% Female Head HH	0.1616	0.1934	0.1879	0.1576	0.1639	0.1664	**0.000**	0.017	0.862
% Same House 5 Yrs Ago	0.5442	0.5127	0.6025	0.5623	0.5854	0.5655	**0.000**	**0.001**	0.244
% > 25 No HS Diploma	0.3427	0.3144	0.4053	0.3429	0.3881	0.3533	**0.000**	**0.000**	0.060
% > 25 BA or Better	0.1389	0.1767	0.1003	0.1377	0.1092	0.1343	**0.000**	**0.000**	0.036
% < Poverty Line	0.1056	0.1141	0.1139	0.1005	0.1072	0.1115	**0.003**	0.109	0.716
% Public Assistance	0.0736	0.0773	0.0885	0.0745	0.0805	0.0755	0.084	0.041	0.578
Household Income	20,340	21,526	19,635	20,869	19,812	20,301	**0.000**	0.013	0.486

Table 3.3: Conventional Estimates of the Association Between NPL Status and House Prices with Data from the Entire US

	(1)	(2)	(3)
A. All NPL Sample, Own Census Tract Observation			
1(NPL Status by 2000)	0.040	0.046	0.067
	(0.012)	(0.011)	(0.009)
R-squared	0.579	0.654	0.779
B. All NPL Sample, 3-Mile Radius Circle Sample Obsevation			
1(NPL Status by 2000)	0.030	0.060	0.106
	(0.011)	(0.013)	(0.011)
Ho: > 0.046, P-Value	0.061	0.862	0.999
R-squared	0.580	0.652	0.776
C. Restrict NPL Sites to those in 1982 HRS Sample, Own Census Tract Observation			
1(NPL Status by 2000)	0.071	0.076	0.057
	(0.016)	(0.015)	(0.013)
R-squared	0.581	0.655	0.780
D. Restrict NPL Sites to those in 1982 HRS Sample, 3-Mile Radius Circle Sample Observation			
1(NPL Status by 2000)	0.046	0.143	0.191
	(0.015)	(0.021)	(0.021)
Ho: > 0.058, P-Value	0.215	0.999	0.999
R-squared	0.580	0.653	0.777
1980 Ln House Price	Yes	Yes	Yes
1980 Housing Characteristics	No	Yes	Yes
1980 Economic and Demographic Variables	No	No	Yes
State Fixed Effects	No	No	Yes

Table 3.4: Quasi-Experimental Estimates of the Effect of NPL Status on House Prices, Samples Based on the 1982 HRS Sites

					RD-Style Estimators		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. Own Census Tract							
1(NPL Status by 2000)	0.035	0.037	0.043	0.047	0.007	0.022	0.027
	(0.031)	(0.035)	(0.031)	(0.027)	(0.063)	(0.042)	(0.038)
B. Adjacent Census Tracts							
1(NPL Status by 2000)	0.071	0.066	0.012	0.015	-0.006	-0.002	0.001
	(0.031)	(0.035)	(0.029)	(0.022)	(0.056)	(0.035)	(0.035)
C. 2-Mile Radius from Hazardous Waste Sites							
1(NPL Status by 2000)	0.021	0.019	0.011	0.001	0.023	-0.018	-0.007
	(0.028)	(0.032)	(0.029)	(0.023)	(0.054)	(0.035)	(0.034)
Ho: > 0.138, P-Value	0.000	0.000	0.000	0.000	0.018	0.000	0.000
D. 3-Mile Radius from Hazardous Waste Sites							
1(NPL Status by 2000)	0.059	0.055	0.035	-0.004	-0.027	-0.024	-0.006
	(0.033)	(0.038)	(0.031)	(0.022)	(0.051)	(0.034)	(0.034)
Ho: > 0.058, P-Value	0.483	0.467	0.236	0.003	0.048	0.007	0.031
1980 Ln House Price	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Instrument for 1(NPL 2000)	No	Yes	Yes	Yes	Yes	Yes	Yes
1980 Housing Char’s	No	No	Yes	Yes	Yes	Yes	Yes
1980 Econ & Demog Vars	No	No	No	Yes	Yes	Yes	Yes
State Fixed Effects	No	No	No	Yes	Yes	Yes	Yes
Quadratic in 1982 HRS Score	No	No	No	No	Yes	No	No
Control for Pathway Scores	No	No	No	No	No	Yes	No
Reg Discontinuity Sample	No	No	No	No	No	No	Yes

Table 3.5: Quasi-Experimental Estimates of Stages of Superfund Clean-ups on Housing Rental Rates, Sample of 2-Mile Radius Circles Around 1982 HRS Sample Sites

			RD-Style Estimators		
	(1)	(2)	(3)	(4)	(5)
1(NPL Only)	0.126	-0.018	-0.040	-0.054	-0.043
[115 Sites, Mean HRS = 40.2]	(0.046)	(0.033)	(0.049)	(0.037)	(0.051)
1(ROD & Incomplete Remediation)	0.106	-0.017	-0.045	-0.059	-0.075
[329 Sites, Mean HRS = 44.3]	(0.030)	(0.022)	(0.041)	(0.028)	(0.032)
1(Const Complete or NPL Deletion)	0.062	0.002	-0.023	-0.036	-0.034
[214 Sites, Mean HRS = 41.6]	(0.032)	(0.021)	(0.041)	(0.028)	(0.031)
P-Value from F-Test of Equality	0.22	0.59	0.51	0.47	0.37
1980 Rental Rate	Yes	Yes	Yes	Yes	Yes
1980 Housing Characteristics of Rental Units	No	Yes	Yes	Yes	Yes
1980 Economic and Demographic Variables	No	Yes	Yes	Yes	Yes
State Fixed Effects	No	Yes	Yes	Yes	Yes
Quadratic in 1982 HRS Score	No	No	Yes	No	No
Control for Pathway Scores	No	No	No	Yes	No
Regression Discontinuity Sample	No	No	No	No	Yes

Table 3.6: Quasi-Experimental Estimates of 2000 NPL Status on 2000 Demand Shifters, Sample of 2-Mile Radius Circles Around 1982 HRS Sample Sites

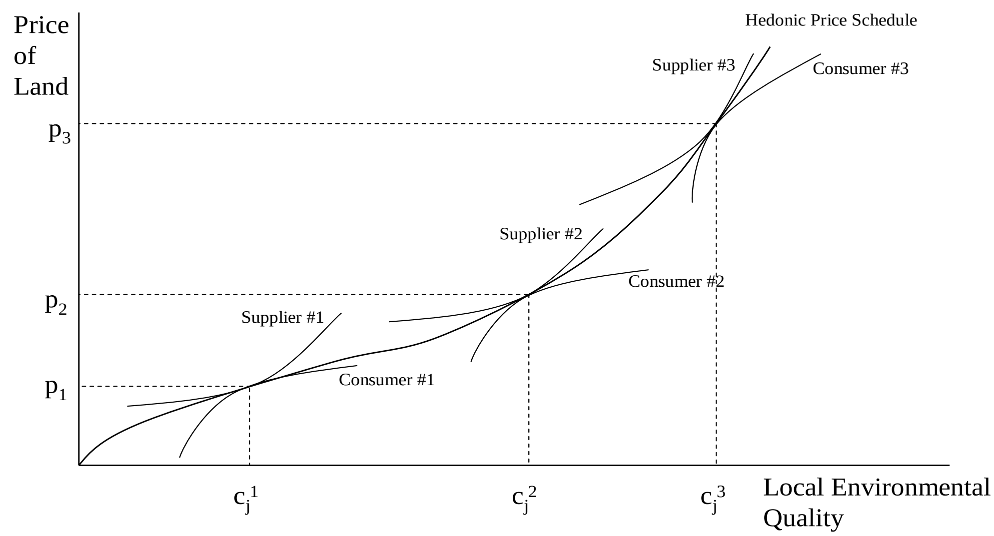
			RD-Style Estimators		
	(1)	(2)	(3)	(4)	(5)
A. Income and Wealth					
Household Income	2,698	1,431	-1,232	123	-593
[1980 Mean: 42,506; 2000 – 1980 Mean: 14,301]	(1,237)	(1,302)	(3,130)	(1,900)	(2,227)
% Public Assistance	-0.007	-0.005	0.008	0.003	0.004
[1980 Mean: 0.078; 2000 -1980 Mean: 0.000]	(0.003)	(0.003)	(0.007)	(0.004)	(0.005)
% College Graduates	0.001	-0.001	-0.009	-0.005	-0.010
[1980 Mean:0.134; 2000 -1980 Mean: 0.082]	(0.007)	(0.007)	(0.019)	(0.011)	(0.013)
B. Demographics Demand Shifters					
% Population Under Age 6	0.000	-0.000	0.002	0.000	0.001
[1980 Mean: 0.086; 2000 -1980 Mean: -0.019]	(0.001)	(0.001)	(0.003)	(0.002)	(0.002)
% Population Over Age 65	-0.000	-0.003	-0.014	-0.007	-0.005
[1980 Mean: 0.106; 2000 -1980 Mean: 0.019]	(0.004)	(0.004)	(0.009)	(0.005)	(0.005)
% Black	-0.015	-0.016	-0.007	-0.012	-0.008
[1980 Mean: 0.088; 2000 -1980 Mean:0.026]	(0.008)	(0.007)	(0.018)	(0.010)	(0.009)
C. Total Population					
Total Population	1,864	514	-2,342	-23	-289
[1980 Mean: 18,038; 2000 – 1980 Mean: 1,226]	(526)	(522)	(1,556)	(809)	(811)
1980 Dependent Variable	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	No	Yes	Yes	Yes	Yes
Quadratic in 1982 HRS Score	No	No	Yes	No	No
Control for Pathway Scores	No	No	No	Yes	No
Regression Discontinuity Sample	No	No	No	No	Yes

Table 3.7: Quasi-Experimental Estimates of the Effect of 2000 NPL Status on Housing Supply, Samples of 2-Mile and 3-Mile Radii Circles Around 1982 HRS Sample Sites

			RD-Style Estimators		
	(1)	(2)	(3)	(4)	(5)
Total Housing Units					
2 Mile Radius from Hazardous Waste Sites	332	94	-829	-208	-255
[1980 Mean: 6,835; 2000 – 1980 Mean: 853]	(139)	(147)	(349)	(210)	(187)
3 Mile Radius from Hazardous Waste Sites	1,046	292	-903	61	-77
[1980 Mean: 15,657; 2000- 1980 Mean: 1,960]	(317)	(278)	(669)	(408)	(356)
1980 Dependent Variable and Ln House Price	Yes	Yes	Yes	Yes	Yes
1980 Housing Characteristics	No	Yes	Yes	Yes	Yes
1980 Economic and Demographic Variables	No	Yes	Yes	Yes	Yes
State Fixed Effects	No	Yes	Yes	Yes	Yes
Quadratic in 1982 HRS Score	No	No	Yes	No	No
Control for Pathway Scores	No	No	No	Yes	No
Regression Discontinuity Sample	No	No	No	No	Yes

Figure 3.1: Welfare Gains Due to Amenity Improvements

Bid Curves, Offer Curves, and the Equilibrium Hedonic Price Schedule in a Hedonic Market for Local Environmental Quality



Welfare Gains Due to Amenity Improvements with Two Supply Curves

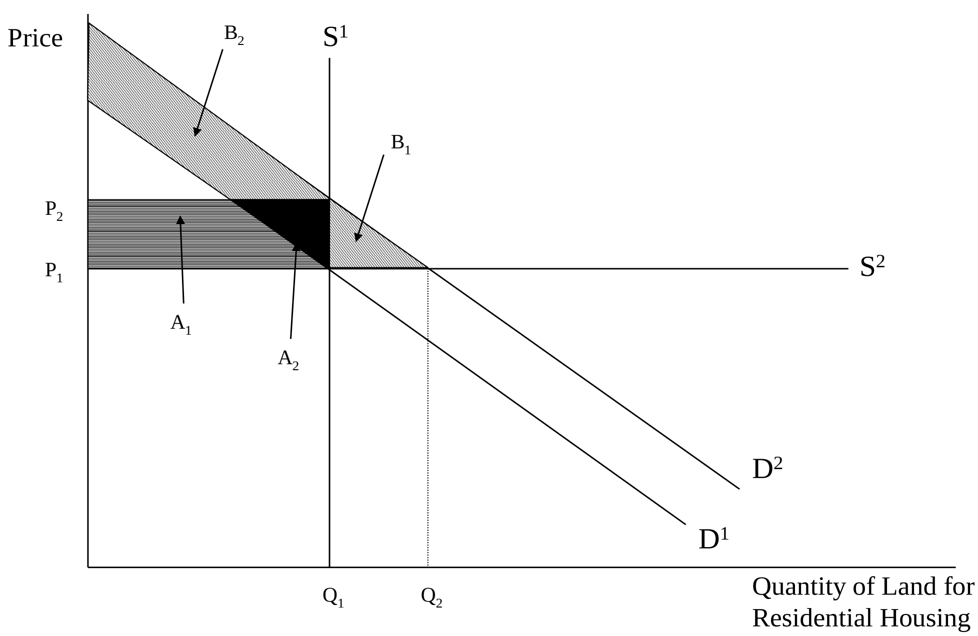


Figure 3.2: Geographic Distribution of Hazardous Waste Sites in the 1982 HRS Sample

A. Sites with 1982 HRS Scores Exceeding 28.5

B. Sites with 1982 HRS Scores Below 28.5

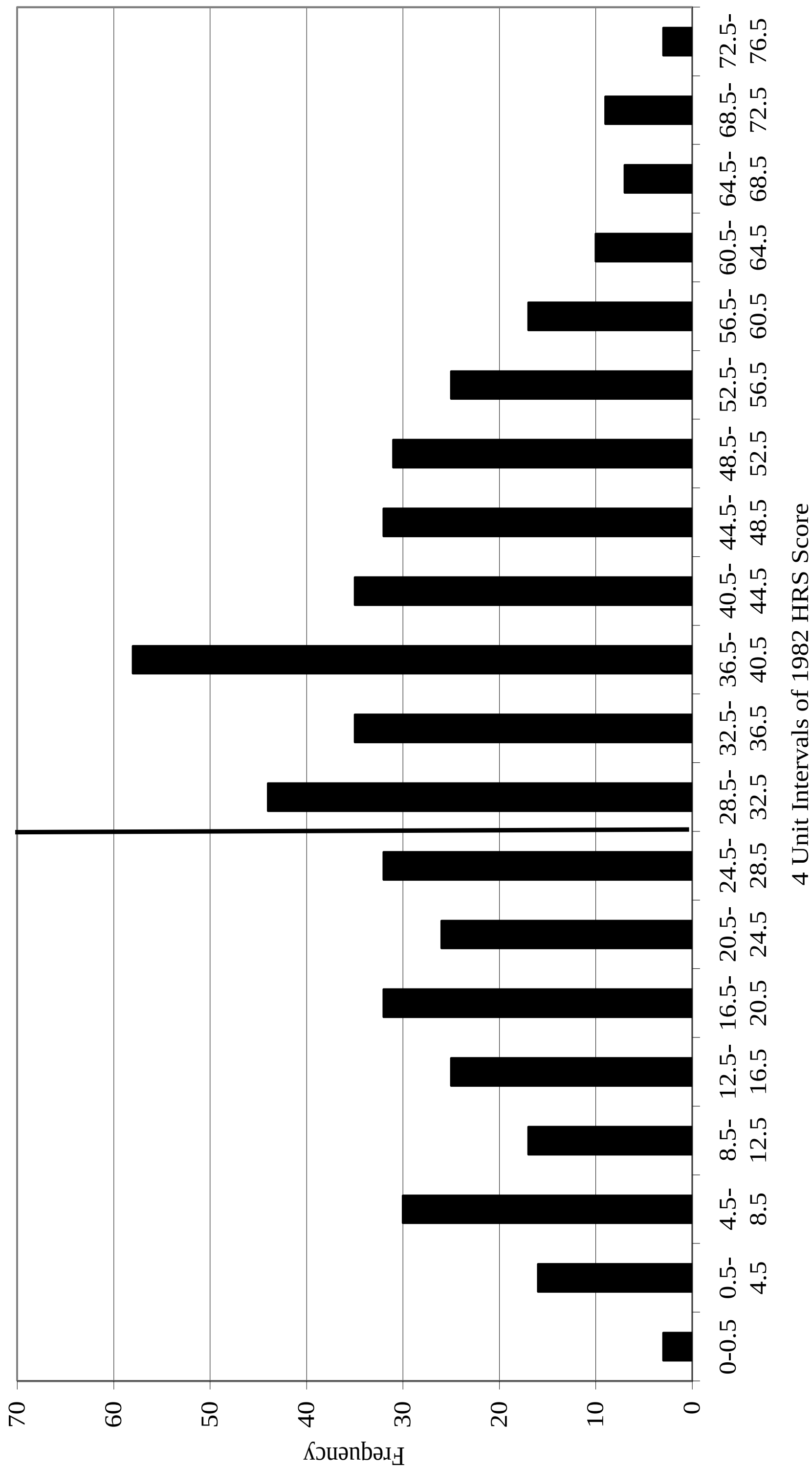


Figure 3.3: Distribution of 1982 HRS Scores in the 1982 HRS Sample

Figure 3.4: Probability of Placement on the NPL by 1982 HRS Score in the 1982 HRS Sample

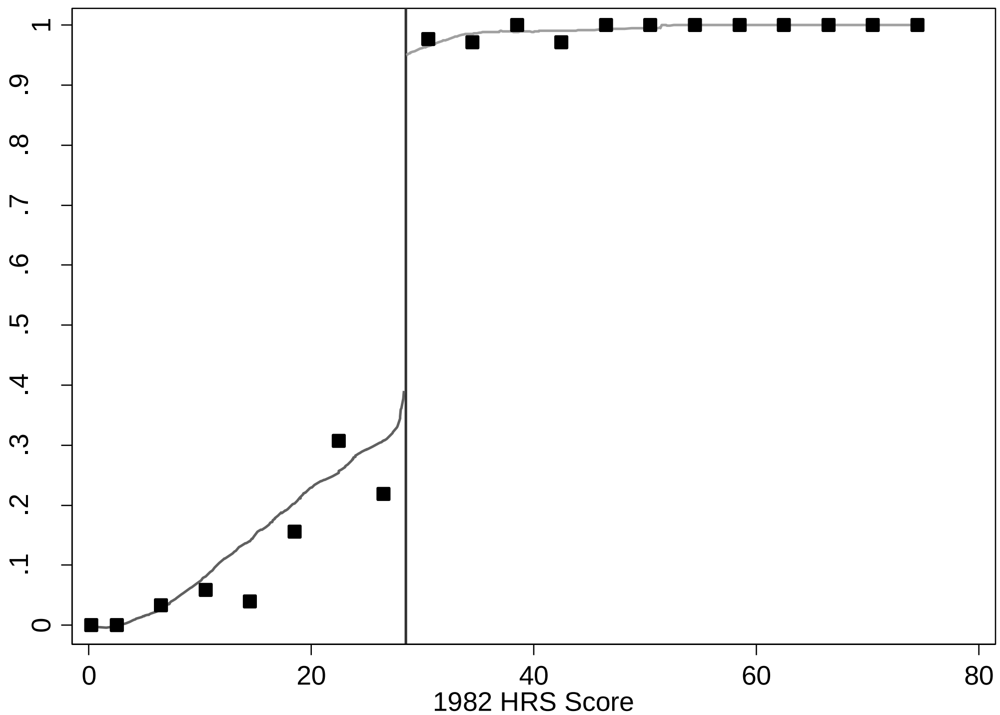
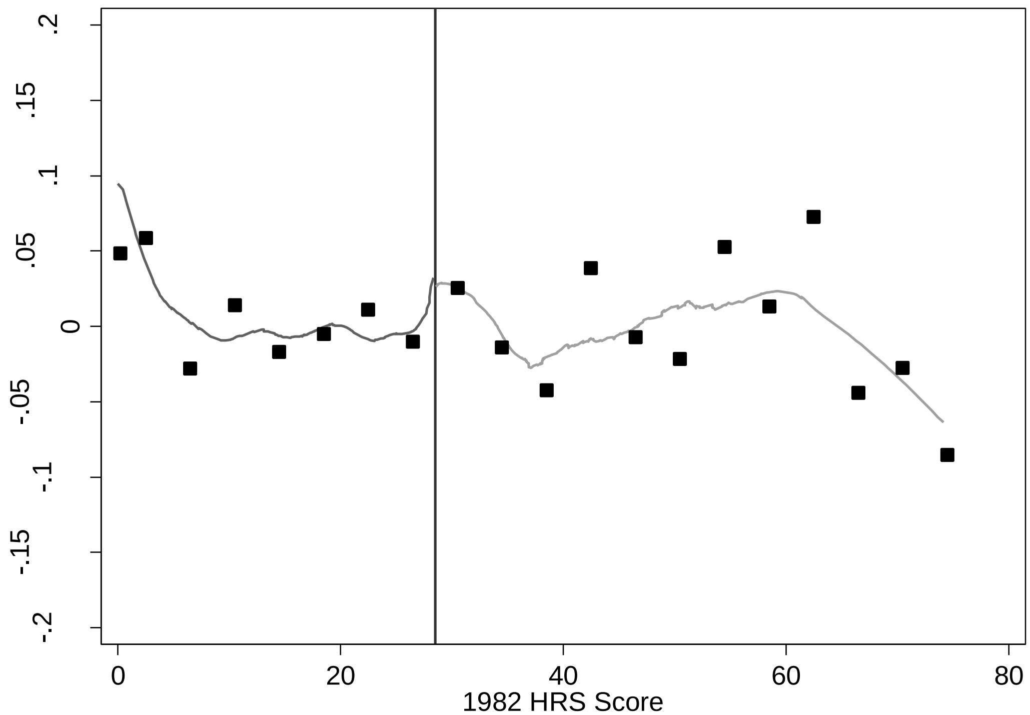


Figure 3.5: 2000 Residential House Prices by 1982 HRS Score, Sample of 2-Mile Radius Circles Around 1982 HRS Sites



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