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Toxic Assets: How the Housing Market Responds to Environmental Information Shocks*

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Abstract

In 1998, a number of polluting industries, including fossil fuel power plants, were added to the list of firms publicly reporting pollution releases in the Toxics Release Inventory (TRI). This caused a large increase in reported toxic pollution, and a corresponding decrease in median housing prices of 2-3 percent in impacted areas. Contrary to prior findings that TRI information does not influence household actions, I find the additional TRI data caused households to revise priors on ambient pollution levels. This implies that, even with market-based environmental regulation, there remains a role for government as provider of information on environmental conditions.

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"It's not news that they're polluting, but it is news to the extent that they are polluting."

(John F. Sheehan of the Adirondack Council[1])

1 Introduction

Several models in economics use market mechanisms to rectify the problem of environmental externalities. Tiebout (1956) proposes a model where individuals sort in communities with their optimal combination of taxes and amenities. This "voting with your feet" can be applied to an environmental context where, rather than government establishing constraints and regulations, firms are allowed to pollute and households sort based on their preferences for environmental quality. Coase (1960) proposes an alternate solution via private bargaining. Property rights are assigned, and households and firms engage in market transactions to find an agreed-upon level of pollution.

The existence of market solutions has led some to insist government intervention in the market for environmental quality is unnecessary and serves only to create inefficiency and hinder economic growth. But for these models to operate efficiently, households must accurately assess ambient environmental conditions. Households cannot efficiently sort themselves or bargain along their marginal willingness to pay to avoid pollution curve without knowing quantities, and imperfect information will result in an equilibrium that is not socially efficient.

The role of information is equally important for avoidance and mitigation behavior in the face of environmental dangers. Recent research finds when households have information regarding environmental hazards, they adjust behavior in ways that can help offset potential for health consequences. For example, Graff Zivin et al. (2011) find notification of water quality violations leads households to shift consumption from tap to bottled water, and Neidell (2004), Neidell (2009), and Moretti and Neidell (2011) find people adjust behavior to avoid spending time outside on days with dangerous levels of ambient ozone.

This paper furthers the research on environmental information and behavior by considering

[1] Hu (May 12, 2000).

whether or not households living near emitters of toxins are aware of ambient toxin levels, and if not, how they react when they receive more information. I use the Toxics Release Inventory (TRI), an annual report produced by the Environmental Protection Agency (EPA) on the estimated toxins released by firms, as a publically available measure of toxin pollution. I exploit a policy change that added several industries to the list of firms that must report estimated releases, most notably coal and oil power plants, and investigate if and how the housing market capitalized this information through changes in observed housing sales prices. How the market responds to the expanded reporting shows: (1) whether or not households use such information in their decisions, and (2) whether or not households can accurately assess toxins in their surrounding environment on their own. Specifically, any non-zero shift in housing prices is a sign households care about such information, and that they reassessed ambient toxin levels. Changes in price also serve as a hedonic measure of how households value avoiding exposure.

Using data from over 1,000 zip codes across the United States, I use a difference-in-difference model to compare sales prices in zip codes with newly-reporting industries to zip codes with no such changes. I show new pollution information led to price drops in impacted areas of 2-3 percent, with the largest effects occurring in regions with the largest increases in toxin reporting. These effects persist after allowing for a number of different time and region fixed effects, including zip code-specific trends in home prices. Contrary to prior findings that TRI information does not influence home prices, my finding implies households reacted to the information in the TRI, even in zip codes with obviously large polluters such as coal power plants. This carries important implications for market solutions to environmental externalities that require full information to achieve Pareto optimality.

My results also demonstrate the importance of accurate data provision. If households used earlier versions of the TRI to gauge environmental conditions, the earlier omission of substantial polluters may have misinformed households regarding true exposure levels. Further, I build on the earlier work of both Bui and Mayer (2003) and Oberholzer-Gee and Mitsunari (2006), who examine home prices in the earlier years after the initial appearance of the TRI in 1989. To my knowledge, I am the first to consider the impact of the 1998 policy change on home values, and as such the first to examine how households perceive the production of toxins by coal and oil power plants, some of the largest sources of modern toxic pollution in the United States.

The separation between changes in household perceptions of ambient toxins and changes in actual toxin levels also provides a unique opportunity to consider the household willingness to pay to avoid toxin exposure. Most changes in environmental quality are accompanied by other, potentially confounding changes such as recessionary periods, economic development, or changes in regional amenities that might otherwise influence hedonic pricing estimates. No such concerns are present here, as my identification comes from a change in perception rather than a change in exposure.

The remainder of this paper is organized as follows. Section 2 describes the TRI in detail, as well as the relevant policy changes used for identification. Section 3 discusses prior findings on TRI information and home prices within the context of the larger TRI literature. Section 4 describes how the newly released TRI data might influence housing values and the potential mechanisms for information transmission. Section 5 describes the data used in the analysis. Section 6 describes the methodology. Section 7 presents my primary results and explores various robustness checks. Section 8 discusses my findings in context of prior related work. Section 9 concludes.

2 The Toxics Release Inventory

Public Law 99-499 (the "Superfund Amendments and Reauthorization Act of 1986") amended the Comprehensive Environmental Response, Compensation, and Liability Act of 1980 and created the Toxics Release Inventory. Contained within the Act was the requirement that,

> The owner or operator of a facility subject to the requirements of this section shall complete a toxic chemical release form as published under subsection (g) for each toxic chemical listed under subsection (c) that was manufactured, processed, or otherwise used in quantities exceeding the toxic chemical threshold quantity established in

subsection (f) during the preceding calendar year at such facility.

(Public Law 99-499)

The Act applied to facilities that had 10 or more full-time employees, were within SIC codes 2000 through 3999, and produced or released over a threshold level of specifically noted toxins per year.[2] Data are self-reported, collected by the EPA at the end of each calendar year, and later released to the public as the Toxics Release Inventory report. Due to lags between when data are collected and ultimately released to the public, the full TRI data for any given year become public around 18 months after the end of the relevant reporting year.

A number of studies examine the impact of the early TRI data. Closest to this paper, Bui and Mayer (2003) and Oberholzer-Gee and Mitsunari (2006) both consider how home prices respond to the initial 1989 data release. Neither paper finds any consistent change in home prices when the TRI data first appear. I discuss why my results may vary from theirs in Section 8. More recently, Mastromonaco (2012) considers how a later TRI policy change in 2002 influenced housing prices in a number of California cities. Other work explores how the stock market capitalized information on firm toxin emissions. Hamilton (1995) found stock losses for polluters in the days directly following the initial release, and Konar and Cohen (2006), using 1988 TRI data, find both toxic chemical releases and environmental lawsuits to be associated with negative stock returns. Khanna et al. (1998) found repeated release data had lasting effects on firms already known to be large polluters. Less is known, however, about the impacts of the large-scale 1998 adjustment to the reporting requirements of the TRI.

In the 1998 reporting year, seven industries were added to the list of those required to report information in the TRI: electricity production via coal and oil burning (SIC codes 4911, 4931, and 4939), metal and coal mining (SIC codes 10 and 12), solvent recyclers (SIC code 7389), hazardous

[2]In the first reporting year, this threshold was set at 75,000 pounds. This was lowered to 50,000 pounds in the second reporting year, and 25,000 pounds in the third reporting year, and then stabilized for some time. The initial listing of chemicals required to report was a combination of two pre-existing lists of hazardous toxins, the New Jersey Environmental Hazardous Substance List and the Maryland Chemical Inventory Report List. In 1993, the EPA added 23 additional chemicals to the reporting list, with 286 more added in 1994 as the list of who was to report expanded to include all Federal facilities.

waste treatment and disposal facilities (SIC code 4953), chemical distributors (SIC code 5169), and petroleum bulk terminals (SIC code 5171). These industries represented a large share of reported toxin releases, particularly the electricity production sector. As noted in a public statement by then EPA administrator Carol M. Browner upon the release of the new information (emphasis added);

> The new results, when added to the manufacturing sector already reporting, bring the total releases of toxic chemicals reported nationally to 7.3 billion pounds — **nearly triple the previous number**. Americans now will have the best picture ever of the actual amounts of toxic pollution being emitted by industry into local communities [...] **For the record, between 1997 and 1998, total releases of toxic pollution for the manufacturing sector continued to decline** — this time by 90 million pounds. Next year, we'll be able to see how all of the combined sectors will "trend" in terms of total emissions and individually [...] You have been given press kits today similar to previous years. This time, however, **as a result of the new data being presented, you will notice lists of states and facilities in eight different categories. The categories are the traditional manufacturing sector and the seven new sectors**.[3]
>
> (Remarks Prepared for Delivery, TRI Announcement, May 11 2000)

In investigating the impact of the policy change, I focus on land and airborne releases, as they are by far the largest changes due to the policy.[4] From this point forward, unless otherwise noted, the term "new releases" refers to airborne and land releases.

Figure 1 illustrates how newly reported releases from relevant industries compare to releases from earlier reporters. The figure shows all recorded releases in thousands of tons from the 1988 reporting year through 2002 (when the TRI stopped using SIC codes), separated by newly added industries (dashed line) and all other reporting industries (solid line).[5] Total reported releases for the seven impacted industries are effectively zero prior to the 1998 policy change, and after the

[3] Currently available online at `http://yosemite.epa.gov/opa/admpress.nsf/12a744ff56dbff8585257590004750b6/83c9dac72c1425068525701a0052e3dd!OpenDocument`.

[4] An earlier version of this paper (Sanders, 2011) restricted analysis to only airborne toxins to make my results more comparable to prior findings using the TRI: Bui and Mayer (2003) and Oberholzer-Gee and Mitsunari (2006), for example, focus on airborne releases, as do many of the studies on health using the TRI (Currie and Schmieder, 2009; Currie, 2011; Currie et al., 2011). Results using both land and air toxins are consistent with my earlier findings. Very few regions have large changes in water releases, so I omit those here.

[5] Air releases are the sum of stack and fugitive releases, where fugitive releases include equipment leaks, chemical evaporation, etc.

change, releases from these industries are greater than releases for all other industries combined. Figure 2 shows the number of reporting plants by newly reporting industries (dashed line) and all other industries (solid line). Large releases reported for new industries are not due to a large number of newly reporting firms, but to the average amount of toxins for each firm.

Imperfections in data collection make using the TRI an imprecise measure of ambient toxins. Firms appear and disappear due to openings/closings, failure to produce the amount of toxins required to report, etc., which can cause year-to-year changes in both number of firms reporting and total emissions. Reported data are often estimates based on production levels rather than directly measured emissions, and while the EPA does enforce reporting, there is no regular verification of reported versus true toxin releases. de Marchi and Hamilton (2006), for example, show that when pollution monitors can be used to examine ambient toxin levels, drops in emissions reported in the TRI are often smaller than those measured by nearby monitors. They further show the distribution of certain reported emissions fails the "Bedford's Law" test for a distribution of "true" data, and in some cases reported numbers appear to suggest "rule of thumb" uses for reporting rather than direct production numbers.[6]

Such problems mean the TRI data may be an unreliable measure of exact toxic exposure, a problem which led Currie et al. (2011) to develop an instrumental variable strategy using firm openings and closings.[7] I address this issue by focusing on the addition of large-scale newly recorded releases rather than smaller year-to-year marginal effects, and a treatment versus control zip code methodology rather than identifying the marginal effects of a unit of toxins. I discuss this further in Section 6.

Due to an additional policy change in the TRI, I limit analysis to periods prior to May of 2002. In reporting year 2000, the EPA again expanded the toxins on the reporting list, adding

[6] Bedford's Law states that in a distribution of data, the first digit of all values is unevenly distributed across the 1-9 spectrum similar to a logarithmic scale, with 1 being represented approximately 30% of the time and each larger number appearing less and less frequently.

[7] When investigating the impact of toxins on infant health, they find no significant effects with OLS and large, significant effects with IV, suggesting measurement error in TRI data is a problem.

new persistent bioaccumulative toxin (PBT) chemicals and lowering the reporting threshold for certain toxins already on the list, including metals such as lead (100 pound threshold) and mercury (10 pound threshold). Certain dioxins were given low reporting thresholds of anything greater than 0.1 gram of releases.[8] This policy change impacts a number of the same industries. For example, power plants are a large source of both lead and mercury. It impacted a good deal of other dioxin-producing factories as well, which makes comparison between original treatment and control groups less valid across this period.[9] Mastromonaco (2012) considers this alternate treatment in greater detail.

3 Environmental Hedonic Pricing and Prior Evidence From the TRI

A number of studies use changes in the value of homes as a hedonic measure of how households value environmental amenities. [10] Most similar to this work, Bui and Mayer (2003), Oberholzer-Gee and Mitsunari (2006), and more recently Mastromonaco (2012) examine the impact of toxins on home prices using the TRI. Oberholzer-Gee and Mitsunari (2006) use sales records from homes across five Philadelphia counties to investigate how observed prices near TRI facilities changed with the first-ever release of TRI data in 1989. They find home prices decreased across the time of the data release and interpret this change as a revision of the risk expectations of households who, prior to the TRI data release, had underestimated true toxin exposure, though this may be background trends in home prices independent of the TRI period. They also find results are highly

[8]For an in depth list of the PBT listing and threshold changes in reporting year 2000, see Chapter 3 of the 2001 Toxics Release Inventory Public Data Release.

[9]Earlier versions of this paper considered how already treated zip codes saw changes under further treatment. While some negative effects were present, the lack of good controls causes me to omit these results here.

[10]For example, Greenstone and Gallagher (2008) examine housing prices near Superfund sites both before and after cleanup, and Gamper-Rabindran et al. (2011) consider how the results in Greenstone and Gallagher (2008) vary with levels of geographic aggregation. Chay and Greenstone (2005) use changes in pollution resulting from the Clean Air Act to show improved air quality was associated with increases in home prices in the impacted regions, and Bento et al. (2011), using the more recent Clean Air Act Amendments, show impacts of air quality improvement vary by spatial aggregation as well. Leggett and Bockstael (2000), using variation in water quality in the Chesapeake Bay, show a positive willingness to pay to avoid exposure to fecal coliform. Studies specifically investigating how power plants influence housing prices include Blomquist (1974), Nelson (1981), Gamble and Downing (1982), and Davis (2011).

sensitive to distance from a TRI facility, with perceptions being revised only in households a quarter to a half-mile away (they find zero effect for homes closer to TRI sites). Bui and Mayer (2003) use 231 zip codes in Massachusetts and examine both the impact of the initial data release as well as short-run changes in reported toxins in the years that follow. In both cases, they find no detectable impact on home prices, even in communities with high newspaper readership (as measured by the Audit Bureau of Circulations) taken as a proxy for access to information. And while they find reported releases declined substantially after the first reporting years, the declines did not seem related to political economy, neighborhood influence, or price changes.

Despite no consistent evidence housing prices adjust in response to earlier TRI information, other research finds changes in behaviors we expected would be correlated with home values. Banzhaf and Walsh (2008) find people "vote with their feet" for environmental quality, using air releases from the TRI as a measure of toxin levels. Using the 2000 policy change, Currie (2011) finds compositional changes in the characteristics of mothers nearby TRI factories when additional information on toxins is provided, and Mastromonaco (2012) finds households in California see a decrease in value. Other work on environmental information further supports that households use such information when it is made available, with accompanying changes in home values. Davis (2004) shows that the proliferation of information on elevated cancer rates in a Nevada county caused a decrease in home prices of almost 16 percent, and Gayer et al. (2000) find the release of risk information about Superfund sites caused households to revise their expected cancer risks.

4 Toxins Information and Prior Beliefs

Due to the 1998 revision, in this analysis changes in information on toxins were independent of changes in actual levels. Pollution changes and information changes often move together: a toxic event bringing firms to public attention, such as the incident at Three Mile Island (Nelson, 1981; Gamble and Downing, 1982), or newly constructed power plants moving into neighborhoods (Davis, 2011). My design avoids potential contamination from other factors that move along with changes in environmental quality, such as plants openings/closings, economic development, migration patterns, and emissions regulation.

In order for households to respond to the change in information, they must somehow incorporate newly available data into their knowledge set, which can vary based on the framework of household knowledge and learning. I define household perception of toxin exposure in zip code z and time t as

$$E[Tox_{z,t}] = E[Tox_{z,t}(O_{z,t}, I_{z,t}, D_{z,t})], \tag{1}$$

where O is observed exposure (e.g., smoke from factories), I is provided information on toxins (e.g., the TRI or newspaper articles), and D is the perceived danger caused by exposure (e.g., how likely households believe any one pound of toxin is to cause cancer). Households view exposure not just on levels of toxin, but on perceived dangers caused by those levels. Given the presence of uncertainty, the household expectation is drawn from a distribution of possible $Tox_{z,t}$, with variance $Var[Tox]$. In such a model, information such as the TRI can alter household behavior either by shifting the mean expected exposure rate or by reducing the variance in the believed exposure (or both). If, for example, the TRI served only to inform households that their potential exposure was more tightly gathered around the perceived mean, more risk-averse households would sort to regions with more information available, all else held constant.

Increasing TRI information, however, need not mean households move priors closer to the *true* level. The TRI is only an approximation of true toxin levels which is also subject to error (see Section 5). Households might under- or over-react to information, in which case using new information on the TRI might cause them to re-adjust priors incorrectly. In the discussion that follows, I assume households are rational in that D is correctly specified given household preferences.

Households may not use the TRI at all in the construction of their expectations regarding toxin exposure, in which case the policy would have no effects ($\partial Tox/\partial I = 0$). I would find a similar effect if households simply do not know the TRI exists. I show in Section 4 I can relax this assumption, as the media focused on TRI-related stories at the time of each new data release, particularly around the releases impacted by the 1998 policy change. It need not be that households actively sought TRI data, but instead responded to the data provided by the media, or even learned from neighbors who had learned from the media, etc.

As a framework for the role of toxin information in housing prices, I consider a simple model of housing where the price of a house is a function of expected exposure to environmental toxins $Tox_{z,t}$ (specified in equation (1)), all past information on exposure levels TRI_t^{old}, newly incorporated information as estimated by the lagged TRI release TRI_{new}, and other characteristics Γ_t, which includes home characteristics, regional amenities, etc.[11] As is common in the hedonic literature, I use the natural log of house price as the representation of value;

$$ln(homeprice_t) = f(Tox_{z,t}(O_{z,t}, I_{z,t}(TRI_{z,t}^{old}, TRI_{z,t}^{new}), D_{z,t}), \Gamma_{z,t}). \tag{2}$$

I assume, all else held constant, individuals place a negative value on perceived toxins such that $\frac{\partial ln(homeprice_{z,t})}{\partial Tox_{z,t}} < 0$. There is no prior on the sign of the partial derivative of price with respect to new TRI information. If $\frac{\partial ln(homeprice_t)}{\partial TRI^{new}} \neq 0$, households adjust exposure beliefs with the new data, suggesting they responded to the TRI information. $\frac{\partial ln(homeprice_t)}{\partial TRI^{new}} = 0$ indicates that, for any number of reasons, households have no response to the TRI. This could be because they had believed toxin levels were equal to the levels reported in the TRI (prior to the data update), they did not believe the information provided by the TRI, or they were not aware of the information. Note that, as the function is written, anything that alters the belief of danger D can also cause changes in home price. This is important if, for example, a media focus on toxins around the time of the release causes households to become newly aware of the dangers of exposure. The sign of $\frac{\partial ln(homeprice_t)}{\partial TRI_{new}}$ tells us nothing about the rationality of households or their ability to correctly assess

[11]For an example of a more complicated model on the integration of new environmental information and Bayesian updating on priors, see Gayer et al. (2000).

health risks associated with toxin exposure levels.

Interpreting price changes around the time of the TRI release as the result of information requires no other factors correlated with treatment changed due to the policy. For example, if firms that are newly required to report adjust production or employment as a result, there could be economic impacts that, in turn, influence housing prices. Similarly, if firms actively reduce pollution as a result of the policy, information and true pollution levels change simultaneously, making it impossible to separate specific impacts of information disclosure and complicating the estimation of the willingness to pay to avoid toxins.[12] The lag between when toxins are produced, when data are gathered, and when data become publicly available helps me separate the impact of the information shock from any changes caused within the firms in response to the new reporting regulations. That is, if the policy change itself impacts home prices, changes should occur during the year of toxin production (1998).[13]

4.1 Third-Party Sources of TRI Information

TRI data can only change behavior if information is accessed and used in the household decision process. Early TRI data were available in hard copy from the EPA, and eventually on compact data disc. Later, a number of sources made data publicly available online, through venues such as the EPA website or the Right-to-Know network (www.rtknet.org). For a period in the late 1990s and early 2000s, the website Scorecard (scorecard.goodguide.com) provided rankings of the worst polluters by area, which Schlenker and Scorse (2011) use to identify the effects of being a "Top 10" polluter. All require active decisions to seek out data. Atlas (2007) found that in a survey of approximately 1,300 people, few individuals knew about the TRI or the names of TRI facilities in their area, and a report by the United States General Accounting Office found that "more than half

[12]Active attempts were made by some firms to reduce emissions after the initial TRI release in the form of the "33/50" plan, where a number of producers aimed to reduce toxin emissions by 33% in 1992 and 50% in 1995 (Environmental Protection Agency, 1999).

[13]As an alternative, there could be a substantial lag between the adjustment actions of the firm in 1998 and the eventual economic effects 18 months later.

of the residents in three counties with high levels of emissions were unaware that the data were available to the public" (General Accounting Office, 1991). This raises questions for this and any analysis considering the response to specific TRI information.

One potential information vector is the popular media, which brought the TRI to the attention of households with each new data release. The media paid particular attention when power plants were added to the list of reporters. A survey of news stories from LexisNexis®, finds stories with "Toxics Release Inventory" in the headline or opening paragraphs occur with high frequency every year around when the EPA releases new data. One of the largest spikes occurs in May of 2000, when the EPA press release specifically notes press packets have information on new, highly polluting sectors. As examples of how the media relayed this information, I include in the Appendix text from three articles released on May 12th, 2000, that note specific locations recently targeted as high polluters. As early as March of 2000, state-level EPA departments had begun producing press releases and public reports on some of the worst newly reporting polluters.

As a measure of when and to what extent the TRI is discussed in the media, Panel A of Figure 3 shows, by month, the number of articles on LexisNexis® that mention the TRI in the headline or leading paragraph. Panel B shows counts for occurrences of the words coal, oil, and electricity within the TRI articles. Dashed lines indicate the annual Federal EPA data releases. Almost all stories on the TRI occur just around the annual releases, and a particularly large number of articles appear in 2000 and 2002, the releases corresponding to the data impacted by the 1998 and 2000 reporting policy changes, respectively. The coal/oil/electricity graph shows the substantial increase in articles regarding power plants after 1998. The increase in 1999 is a combination of news articles discussing that the 1998 data were now gathered and 2000 data will include coal power plants, and some plants making data public information on their own just before providing it to the EPA in July of 1999, which may have sparked media interest.

In summary, it need not be that households actively seek out and use all available TRI information in their preference decisions, but respond when media outlets pay more attention to the

TRI.

5 Data

5.1 Home Prices

Housing data were downloaded from the Zillow® website.[14] Values reported are month-by-year-by-zip code medians for observed sales, and are a weighted average of true observed sales prices for the prior three months, with the most recent month weighted most heavily.[15] Given data are available in only regions with a large amount of sales data, when using a balanced panel in my period of interest I have a primary sample of almost 1,100 zip codes.[16] I restrict my main analysis to zip codes with at least one observed sale per month during the period, though I later relax this constraint to include all zip codes with available home price data.[17] The majority of zip codes do not have available data until 1998, which I use as my starting point for the analysis. I focus on values for detached, single-family homes as my measure of housing value and outcome of interest. All prices are inflated to 2010 dollars using the monthly CPI.

The advantage of the Zillow® data is the large number of observations, both across geographic space and in the frequency of observation. Given the use of the median observed sales price, the disadvantage is the inability to control for household specific factors such as home or lot square footage or number of bedrooms. This could be problematic if the policy change influences the type of home sold across zip codes.[18] As a robustness check, I repeat the analysis using the Zillow® Home Value Index (HVI) as the outcome of interest. The HVI is the zip code-level median estimated housing value for all similar homes (not just those sold), as calculated by Zillow's® propri-

[14] I thank Thomas Blake for sharing the downloaded data.

[15] This weighting structure is described on the Zillow® website, but no exact weighting metric is provided.

[16] The number of zip codes in the United States is regularly changing with urban development. There are currently over 40,000.

[17] In Sanders (2011), I used data collapsed to the quarterly level to reduce noise — results were consistent with my current findings.

[18] For example, those who are most averse to toxins might live in less-expensive homes on average. Sales of such homes would represent a potential change in the demographics of the region, but no change in housing value per-se. Given the persistence of the effect, this seems unlikely.

etary calculation methods. The exact formula used by Zillow® to make these calculations (called "Zestimates") is proprietary, though a partial description is available on the company website.[19] The calculations use a large amount of publicly available data, much of it from county records, and, in some cases, from user reported housing characteristics. Values are based on observed sales and re-weighted according to characteristics contained within the proprietary formula and all the known housing stock in the region.[20] A benefit of the HVI is that the median value is estimated using all available homes for which Zillow® has data in the area rather than only those that are sold. This means changes in values should be less sensitive to selective home-type sales.

Given the time period of interest, I omit all data from the state of California from my primary estimation. Very few plants in California saw any changes from this policy, meaning almost all of the California data would fall into the control category. In addition, during the early 2000s California saw a number of potentially confounding factors; housing speculation, large swings in the NASDAQ index, and electricity deregulation all have the potential to create differential responses among treatment and control groups. I later show my results are robust to the inclusion of California data, but I consider results omitting California to be more reliable.

Table 1 shows the annual median housing sales price and HVI for the primary zip codes included in my analysis as well as the within zip code, between zip code, and overall standard deviations.

5.2 TRI

Toxin data are from the TRI Basic Data Files on the EPA website, which are annually aggregated by facility and toxin and include information on facility name and location, toxins released, and on-

[19]http://www.zillow.com/wikipages/What-is-a-Zestimate/.

[20]Using their formula, Zillow® retroactively estimated historical values. Zillow® specifically notes estimates should not be considered a substitution for true sales price and/or appraisal. However, when compared to known sales and valuation information, the estimates are relatively accurate. Nationally, 75% of estimated values fell within 20% of a true sales price, with a median error of 8.5% (http://www.zillow.com/howto/DataCoverageZestimateAccuracy.htm). There have been historical changes to the estimation strategy, but all such changes were to made to the entirety of the data considered in my analysis, and thus should not influence consistency over time.

and off-site releases for land, water, and air. All data are recorded in pounds until reporting year 2000. After 2000, the majority of data remain in pounds, though dioxins are reported in grams. Also included are SIC classification codes for each reporting producer, which I use to identify polluters impacted by the policy change. To aggregate toxins data to the zip code level, I sum all land and air releases across all producers, resulting in a zip code-specific annual total pounds of toxins released by medium. Any zip code in which no TRI data are reported is assigned a total release level of zero. Prior work on the TRI and home values has separated toxins by categories of potential health damage to test for differences across assessed health risk, and found none (Bui and Mayer, 2003). As such, I do note separate by toxicity. Table 1 shows average zip code-level TRI releases over time, and between, within, and overall standard deviations.

6 Methodology

I use a difference-in-difference framework, comparing zip codes impacted by the policy change to non-impacted zip codes, before and after the 2000 data release of the 1998 toxins report. I match TRI facilities to zip codes, and consider zip codes to be "treated" if they have at least one TRI-reporting firm with a primary SIC code associated with the newly reporting industries. There is some question as to whether or not zip code is the correct level of aggregation for effects. What, after all, is the appropriate geographic distance by which to consider polluting industries relevant to a neighborhood? Schlenker and Scorse (2011) note that much of the online availability for TRI data was accessed using zip codes, which supports my current method. Further, Davis (2011), Gamper-Rabindran et al. (2011), and Mastromonaco (2012) all find housing effects for environmental bads are highly localized, with effects fading within 2-3 kilometers. Finally, zip codes serve as a likely proxy for "neighborhoods", and households may be more likely to make decisions based on neighborhood rather than absolute distance.

A number of SIC groups were added in 1998, not all of which were large polluters. In order to test for differential effects across general ranges of the emissions distribution, I allow for four different treatment groups by splitting new emissions by quartile. The indicator for being in quartile q is;

$$TRI_{Q^q} = \begin{cases} 1 & \text{if } TRI^{new}_{z,1998} \text{ in quartile } q \\ 0 & \text{otherwise} \end{cases} \tag{3}$$

By allowing the effect to vary across the distribution of new information, I can (1) test for non-linearities in the effects, and (2) further test if households are truly responding to new information. If so, my *ex ante* expectation is that effects should be largest for the regions that saw the largest increase in reported toxins.

One concern is that treatment zip codes are fundamentally different from control zip codes. If, for example, the housing stock in regions with power plants follows a different growth rate, or people self-select into regions with more or less visible pollution, zip codes without any newly reporting industries may serve as incorrect counterfactuals. Table 2 shows average income, demographic, and housing information by control group and treatment quartiles, as taken from the 2000 census. No surprisingly, there are some differences. Median income is higher in control zip codes, as is the owner occupancy rate and fraction of new homes that were built recently. Education levels are also higher, and poverty rates lower.

In the absence of zip code-level time-varying covariates to control for other factors that might influence home prices (e.g., wages, employment rates and school quality) I allow for time controls at varied levels of aggregation. In my preferred specification, I include month and year effects as well as linear zip code-level time trends to control for unobservable trending differences:

$$\begin{aligned} ln(homeprice_{z,m,y}) = \alpha post_{m,y} + & \Sigma_1^4 \beta_q TRI_{Q^q} + \Sigma_1^4 \gamma_q post X TRI_{Q^q} \\ & + \tau_y + \mu_m + \psi_z + \psi_z * t + \epsilon_{z,m,y}, \end{aligned} \tag{4}$$

where z indicates zip code, m month, and y year. The term $post$ is an indicator variable for any period after the first recorded appearance of state EPA reports and news articles using the updated 1998 data (March of 2000), and TRI_{Q^q} is an indicator variable defined in equation (3). τ_y and μ_m are common year and month effects, ψ_z are zip code fixed effects, and $\epsilon_{z,m,y}$ is an error term. The time interaction t with the zip code fixed effects allows for different linear trends by zip code. The inclusion of zip code-specific time trends should to some help capture any differences in pre-treatment price growth across groups. In alternate specifications, I allow for more flexible time trends at the higher regional aggregation of state. The coefficients of interest are γ_1 through γ_4, the difference-in-difference estimates of the policy change for each part of the distribution. In order to display the geographic coverage of the data, Figure 4 shows all zip codes used in the primary analysis, shaded gray. Figure 5 shows the treatment zip codes by quartile group.

7 Results

Before showing difference-in-difference results, it is useful to consider the size of the treatment differences across quartiles. Table 3 compares means of housing prices and total air toxin releases between 1998 and 2002, for control group and treatment quartiles (note this table shows toxins in the year in which they were reported, not the year in which they were produced). Treatment zip codes begin with higher levels of reported pollution, even before the addition of new industries in 1998, and treatment zip codes have lower median housing sales prices as well. Both groups have increasing housing values. Panel A of Figure 6 shows all home prices over time. Panel B plots HVI data. For both observed sales prices and the HVI, values are steadily increasing over time.

Prior to reporting year 2000, both treatment and control groups see annual reductions in reported toxins, which follows the general trend seen in the historical TRI data. For the control group, this trend continues. For most treatment zip codes, however, average zip code-level reported toxins increase in 1998, though those in quartile 1 actually see a continued reduction, and those in quartile 2 see little increase. The zip codes in quartile 3 see a large jump in 2001 — a mining facility in Arizona had a usually high dump of copper deposits that year. While levels drop again in 2002, they remain more than double 2000 levels. The impact is most staggering in quartile 4, where the 2000 reporting levels are almost 700 percent above those in prior years.

Figure 7 splits housing price trends into three groups: control zip codes, zip codes in treatment quartiles 1 and 2, and zip codes in treatment quartiles 3 and 4. In order to make trends more comparable, I plot residuals after controlling for constant treatment group differences. The three groups follow similar trends, which supports the use of a diff-in-diff model, but the 3rd and 4th quartile group dips in the months just around the treatment period. Figure 8 better illustrates this change using an event study framework. The figure displays the difference in home values of 3-4 quartile homes (called "impacted" zip codes) and all other homes. Each point is a coefficient on an interaction between treatment and a month-by-year fixed effect,

$$homeprice = \sum_{t=1998m1}^{2002m4} \theta_t + \sum_{t=1998m1}^{2002m4} \delta\theta_t X impacted + \epsilon. \tag{5}$$

I plot the estimate for the vector of coefficients contained in δ, making it the analog of an event study with the common treatment point of March of 2000, the period of earliest news articles I found referencing the 1998 data. Common pre-trends are apparent, as differences are constant for the two years prior to treatment. There is a substantial decrease in relative value of treatment zip code homes around the time of the treatment. The capitalization of information appears rapid and permanent. Prices return to trend and permanently lower levels for at least the following two years.

In order to interpret these findings as the effect of the TRI data, however, it must be that no other fundamental changes occurred just around the treatment period which impact treatment and control groups differently. Despite substantial investigation, I have found no suggestion of any such effects. One possibility is the NASDAQ crash, which began in the months just around the treatment period (see Figure A-1 in the Appendix), one of the primary reasons I omit California from my analysis (where effects were likely the largest due to the tech industry). In some specifications, I include controls for the monthly closing value of the NASDAQ. The use of a diff-in-diff means that to be confounding it must be that treatment and control regions responded to this differently. To allow for that possibility, I interact the NASDAQ close with state-specific effects and MSA-specific effects, and in both cases results are consistent. It is also important that the ramp up in the NASDAQ prior to the crash does not appear to alter pre-trends, and the housing prices after the crash do not continue to drop as the index does.

Panel A of Table 4 shows results of regressions based on equation (4). All standard errors are clustered at the zip code level to allow for common shocks and to help avoid autocorrelation problems in calculation of the standard errors. Coefficients are changes in logs (multiplied by 100) and interpretable as percentage changes. Column 1 includes controls for month-by-year time effects (zip code specific effects are averaged in the inclusion of a treatment group indicator and are thus omitted for simplicity). Though effects are only marginally significant, the values suggest differential effects by treatment quartiles. No effects are present for quartiles 1 through 3, but estimates suggest households in the 4th quartile saw reductions of 2.3 percent. Column 2 allows time effects to vary more flexibly by state. Homes in quartile 1 see positive but statistically insignificant effects, quartile 2 zero effect, quartile 3 a small negative effect, and quartile 4 again has decreases of 2.3 percent.

Column 3 shows my preferred specification, which allows for year and month fixed effects along with zip-specific linear time trends. Effects for the lower treatment groups remain positive but are statistically insignificant. The level, however, is economically significant, and suggests the lowest quartile saw price increases of approximately 2.1 percent. New information for these regions was small, and households may have overestimated how bad small polluters were in comparison to large-scale producers. This may also be a sign of housing demand "spillover", where the decrease in demand for housing in higher-pollution groups leads to an increase in demand for lower pollution treatment groups, which may be a realistic relocation choice. As with the state-by-time models, in the time-trend models effects for quartiles 2, 3, and 4 are negative, with the largest effects occurring for the treatment-heavy regions. Quartile 2 homes see a drop of 0.3 percent, quartile 3 homes of 2 percent, and quartile 4 homes of 2.8 percent. Effects are statistically significant for only group 4.

In Panel B, I restrict the analysis to include only regions that had non-zero reported emissions for other SIC groups. In other words, all regions without *any* 1998 toxins from prior-reporting industries are omitted. This is to investigate if control groups with no prior pollutant information are responsible for the effect, which are least likely to be similar to the treatment groups. All columns follow the same specification as Panel A. Unfortunately, this means I lose approximately 50% of my data. Despite this, results are largely consistent. In the preferred model of Column 3, the same pattern occurs across groups. Effects for group 4 are even more negative at 3.3 percent and are statistically significant at the 5% level. Clearly, my findings are not a result of "unusual" control groups. Figure A-4 in the Appendix shows the event study design for this restricted control group set.

Table A-1 in the Appendix repeats this analysis using the HVI. In Panel A, results are inconsistent with the sales price findings, where all treatment groups see positive but insignificant effects of the policy. Once zip codes are restricted to those with at least some non-zero 1998 toxin data, however, Panel B shows HVI results are similar to those using sales price data, though results are more negative for quartile 3 than 4. As a whole, however, these findings suggest results are not wholly a product of selective sales.

7.1 Expansions

To examine the sensitivity of my results, I consider a number of different specifications and robustness checks. Unless otherwise noted, all regressions include zip code fixed effects, month and year effects, and zip code-specific linear time trends as in Column 3 of Table 4.

I first expand the data set to include California zip codes with available housing data. Results are larger but consistent with my prior findings. As noted above, the real estate market's unusual behavior in California in the early 2000s makes these numbers less reliable — Figure A-2 in the

Appendix repeats Figure 8 with the California data included, and a clear trend in the divergence in housing prices begins in 1999. Column 2 uses an unbalanced panel of all zip codes with any housing sales data available (as long as at least three sales occurred in each year).[21] This increases the number of available zip codes to over 4,300 and results are consistent though slightly smaller. The possibility for different zip codes entering the data as a result of the treatment means the balanced panel estimates are more reliable.

Given the timing of the data release and the use of power plants as a source of identification, deregulation of the electricity industry could play an important role in the outcome. Specifically, deregulated power plants may behave differently in ways that impact home prices. In Column 3 I include an indicator for deregulation, where the indicator is equal to 1 for the time in which a state moved to a deregulated electricity sector (and for all times afterward). Results are unchanged, which is not surprising given the findings for state specific month-by-year effects. In Column 4, I allow for more flexible quadratic zip code-level time trends, which does little to change my results.

The crash in the NASDAQ stock index occurred at approximately the same time as state EPA divisions began releasing the 1998 TRI data to the public. If the loss in asset wealth caused people to sell their homes, this could potentially confound my results. While the difference-in-difference estimation strategy will somewhat address this concern, it is possible treatment zip codes experience the consequences of the crash in a manner different from control zip codes. In Column 5, I include the monthly average closing value of the NASDAQ stock index, interacted with state fixed effects to allow the impacts to vary by state. Results are unchanged, and as is shown in Figure A-3, the pre- and post-treatment difference trends are very similar to the baseline estimates. Column 6 allows for even more region-specific effects by allowing the impact of the NASDAQ index to vary by MSA, and again, results are consistent.

As statistically significant results are present only in groups 3 and 4, in Column 7 I combine

[21] Other specifications relaxed this restriction to the occurrence of any sale — results were similar and are available upon request.

these two groups into a single "treatment" group, which I use for the majority of the remaining analysis for simplicity. In Column 8 I restrict the analysis to only zip codes with at least some non-zero toxins impacted by the policy. Here, I assign groups 3 and 4 as treatment, and call groups 1 and 2 controls. This specification would be most relevant if regions with newly reporting SIC codes (but smaller levels of new information) were better controls that regions without any impacted SIC code facilities. The treatment effect is now almost 3.3 percent, which is not surprising given the prior finding that houses in quartile 1 saw a non-trivial increase in home prices.

In Table 6 I allow results to vary across types of treatment zip codes. Here I again combine quartiles 3 and 4 to construction a single treatment category. I include indicators and interactions for being in the upper 25 percent (among treatment zip codes) of each subgroup considered. The estimation equation is now:

$$\begin{aligned} ln(homeprice) = \alpha + &\beta_1 post + \beta_2 impactedzip + \beta_3 postXimpactedzip \\ &+ \beta_4 subgroup + \beta_5 subgroupXtreatment \\ &+ \beta_6 postXsubgroup + \beta_7 postXtreatmentXsubgroup \\ &+ \tau + \psi + \psi * t + \epsilon, \end{aligned} \tag{6}$$

where all fixed effects are as defined in equation (4) and sub- and super-scripts are omitted for simplicity. Note that all results following this specification should be interpreted as suggestive. True differences might be driven by other omitted zip code characteristics that are strongly correlated with those I consider.

Column 1 allows the effect to vary by zip code education level, proxied by the percentage of the population with a bachelor's degree. Higher education levels might change how households respond to information in either the positive or negative direction. For example, more educated households may be more likely to use TRI information, but they may also be more likely to accurately assess toxin exposure in the absence of TRI data. This interaction of the post-period treatment effect with the indicator for "higher education level" is the difference-in-difference-in-difference estimate of the impact across education levels. The triple interaction is positive, but neither statistically nor economically significant. Column 2 shows no significant differences by median income group, though results are economically significant and suggest the response is smaller in higher income regions. Column 3 shows that results do not appear to vary much by percentage of the units that are owner occupied.

Effects of many toxins are long-term. I expect a greater concern for households with children, and a smaller concern among the elderly population. In Columns 4 and 5, I allow the effect to vary by share of households with children under 18 and share of the population that is over 65. Column 2, which includes an interaction term for "high percentage of households with children under 18", suggests a similar effect in zip codes with many families with children. Column 5 instead includes an interaction term for "high percentage of persons age 65 and up". The interaction term is not statistically significant, but is large and positive at 2.2%, effectively wiping out any response in those zip codes. This suggests the response in zip codes that have a large elderly population is weaker. I note these effects need not be directly caused by the differences in the observed variable per-se. For example, zip codes with a large elderly population may have a different response due to income rather than age (given the large number of elderly in low-income status).[22]

The common treatment period for all treatment zip codes raises concerns if factors other than the new TRI information changed in ways for which time trends cannot control. In Table A-2 of the appendix, I test for the existence of pre-trend differences between treatment and control zip codes. Column 1 includes time indicators for twelve and eighteen months prior to treatment and an interaction between said indicators and the indicator for treatment zip codes. If there were a long-term pre-existing divergence between treatment and control groups then: (1) either or both of these lagged interactions should be economically and statistically significant, and (2) the estimated

[22]I have also re-run this specification omitting Florida, which has a number of zip codes and a large elderly population, to see if this changes the outcome. It does not.

value of the treatment should be absorbed by the inclusion of pre-treatment interactions. Neither pre-term interaction is individually significant, and a test of joint significance yields a p-value of 0.7. Also important, the estimate on the "true" treatment period is only slightly smaller, and remains highly significant. In some sense, this serves as a test for the contemporaneous economic effects of the 1998 policy change. If firms somehow modified their behavior in response to the need to report, the 18-month lagged interaction should be significant. Column 2 repeats this analysis but using interactions for three and six months prior. Again, neither lag is statistically significant, and both are jointly insignificant.

7.2 An Estimate of Information Capitalization

According to the 2000 census, there were 426,580 single-family units in treatment (quartile 3 and 4) zip codes. Zillow® data suggest an average early 2000 home price of approximately $154,000. Column 6 (with the combine quartile 3 and 4 treatment effect) thus suggests a drop in home value of $1.53 billion for homes within my data. Given there are regions with newly reporting TRI facilities that are not contained within my data (due to lack of housing information), the true change in housing value would be even larger (assuming marginal effects are constant across regions). This ignores the positive effect seen in the quartile 1 group. The result is statistically insignificant but not economically insignificant. Using results from Column 3 of Table 4, this suggests those houses saw a gain in value of approximately $680 million. In some sense, this treatment served as a transfer of wealth from zip codes with higher levels of newly reported pollution to those with much smaller information shocks. This further suggests households may have a general perception of how much pollution is generated by a "generic" polluting industry, which was an overestimate in low pollution areas and an underestimate in high pollution areas.

To place my findings within the context of the MWTP to avoid other environmental bads, it is useful to consider prior environmental hedonic estimates using housing values. Davis (2011) finds the construction of new natural gas power plants reduced home values within 2 miles of plants by 4.1-7.1 percent.[23] Chay and Greenstone (2005) find that Clean Air Act total suspended particulate reductions during the 1970s increased home values by 2-3.5 percent, and Bento et al. (2011) find similarly sized county-level results for the later 1990 Amendments. Gamper-Rabindran et al. (2011) find cleanup of Superfund sites raised highly localized housing values by up to 19 percent, though at a different level of aggregation Greenstone and Gallagher (2008) find cleanups to have no effect.

More directly related to the dissemination of environmental information, Davis (2004) finds that the increased information on cancer clusters dropped home values by 14 percent, while Gayer et al. (2000) find increased information on Superfund hazardous waste risk shifted risk expectations downward but still led to a home price decrease of approximately 1 percent.[24,25] As noted in Section 3, most prior works on the TRI and housing values find no consistent change in home prices due to new TRI information (Bui and Mayer, 2003; Oberholzer-Gee and Mitsunari, 2006), and in Section 8 I discuss why my findings may differ from theirs. Most recently, Mastromonaco (2012) finds the 2000 TRI policy change regarding lead and PBTs was associated with a value drop of up to 8.6 percent for nearby homes in California.

8 Discussion

I find the release of additional TRI information, resulting from the addition of certain large-scale polluters to the list of factories that must report, caused a statistically and economically significant change in home sales prices in zip codes with newly reporting polluters. Prior work by Bui and

[23]Davis (2011) shows many new plants opened in 2000, but notes almost all new plants were natural gas plants, which are exempt from reporting to the TRI. For my results to be due to newly-constructed power plants, new natural gas plants would have to have opened in the same zip codes as already existing impacted industries at the same time as the new TRI release. Davis (2011) also finds the effects of being close to a power plant fade within approximately 2 miles, meaning the probability of a treatment zip code in my analysis being close to a treatment area from that analysis is relatively small.

[24]This is calculated using their reported price drop of $661 divided by the mean housing value of $74,176 (in 1996 dollars).

[25]A recent example of the effect of information in non-environmental literature is Linden and Rockoff (2008) who find that releasing information on sexual offenders in the neighborhood lowers home values by approximately 4 percent.

Mayer (2003) and Oberholzer-Gee and Mitsunari (2006) on TRI information and housing prices, however, finds no consistent effects. There are a number of reasons my results might differ. First, I employ a different estimation strategy, with a difference-in-difference model rather than directly investigating marginal effects. If households use a non-marginal decision heuristic such as "do not respond unless the change is larger than X", a difference-in-difference model might be better able to detect effects. Different results by treatment quartile further support this potential response metric. Differential effects by quartile also hint at why prior work may have found zero effect, as assuming linearity would average these marginal effects.

Second, my geographic variation is larger, covering over a thousand zip codes across multiple states — Bui and Mayer (2003) use 231 zip codes in one state, and Oberholzer-Gee and Mitsunari (2006) have data from 5 counties. Third, the world in which TRI data were released for the first time is fundamentally different from that in which TRI data are updated. The initial 1989 data release, for example, did not have the advantage of the Internet, and households had to seek out hard copies of the TRI if they wanted information. Data are now available online, news outlets have expanded both in number and scope of coverage, and additional information is more readily available on the dangers of environmental toxins. Communication was more costly in the past, so dissemination of information across households and neighborhoods is now higher. Fourth, households may not have held solid priors before the first TRI data were released, and it may have taken time before people knew how to interpret toxin data. By the time of the 2000 data release, the TRI had been around for over a decade. If households believed that the prior TRI releases were an accurate reflection of true ambient toxins, their priors would have been more solidified, and thus their response would be greater with the 2000 data release.

Finally, there is a large difference in the size of the information shock regarding changes in reported emissions. Figure 9 illustrates this difference by reporting average releases per zip code for impacted zip codes from 1988 through 2002. Conditional on non-zero releases, the average reported releases in the first TRI data were less than 200 tons (mean non-zero releases were around

200 tons in Bui and Mayer (2003) as well). In 2000, however, average non-zero newly reported emissions level for treatment zip codes was almost 1000 tons.[26] In the average location near a polluter, the revealed information in 2000 was around 5 times the average in the 1989 data release. Again, something as simple as households using a "cutoff" response point heuristic could explain the difference in findings.

There remains the question of whether my results are a supply side response (e.g., an increased desire to sell a home in a treated region), a demand side response (e.g., a decreased desire to buy a home in a treated region), or some combination of both. While the implications regarding market efficiency are the same, the interpretation of the result can vary between the two scenarios. A supply side response suggests local households indeed updated their priors. If the response was entirely from the demand side, individuals living within treatment zip codes could be fully aware of pollution levels, but new arrivals were insufficiently informed until the TRI was expanded. A fully demand side response seems unlikely, as it would require a prior equilibrium entirely based on outside misinformation that was never updated, or at least not within the timeframe of my data.

9 Conclusion

This paper seeks to further research on environmental information by re-examining how home prices change in response to information about ambient pollution levels. A 1998 law change meant several highly polluting industries, including coal and oil power plants, had to newly provide estimated toxin emissions to the EPA. This information was added to the pre-existing Toxics Release Inventory, a publically available data set on toxic pollution in the United States. Upon the release of the new data, zip codes with newly reporting industries saw statistically and economically significant changes in home sales prices. By using a source of identification based on changes in information rather than changes in toxin levels, I avoid bias from unobservable effects that might potentially confound other hedonic estimates, e.g., economic development, migratory behavior,

[26]This uses the treatment classification of zip codes in the 3rd and 4th quartiles.

and housing trends. Zip codes with increases in the upper two quartiles pollution information saw a 2.5 percent decrease in observed home sales prices, a total change in housing stock value of approximately $1.6 billion. I also find suggestive evidence that regions with very low new information levels see increases in housing values (around 680 million). This suggests some areas were "pleasantly surprised" as to their true toxin exposure levels, though results are noisy.

My results provide insight into household beliefs regarding toxin exposure. Unlike prior work involving the capitalization of TRI information, I find households revised priors when the expanded information was released, and markets adjusted accordingly. Information on how households view toxic pollution from fossil fuel power plants is important given recent potential expansions of coal power plant regulation expected to reduce mercury releases by approximately 90 percent (approximately 44 tons), and cost $10.9 billion in the year 2016.[27]. My results also speak to the role of market forces in the task of dealing with environmental externalities. Market mechanisms exist that, in theory, achieve socially efficient equilibria, but they require all parties to be fully informed about the size of the externality. At least in the case of environmental toxics, without information gathered and released by the government, there exists asymmetry in information, which makes Pareto optimal free-market solutions unlikely.

[27]From the EPA mercury and air toxics fact sheet available at `http://epa.gov/airquality/powerplanttoxics/pdfs/proposalfactsheet.pdf`.

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Table 1
Home Price and Land and Air Toxic Pollution Across Time

Year	Median Sales Price	Home Value Index	Average Land & Air Emissions
1998	169,226	166,839	59,490
1999	175,923	174,603	66,485
2000	183,728	183,460	106,767
2001	192,760	193,081	144,603
2002	205,304	205,616	124,990
Overall SD	78,329	80,470	730,784
Within Zip SD	69,048	70,503	719,496
Between Zip SD	42,330	44,558	277,732

Notes: Home sales prices and Home Value Index are taken from Zillow® website. Average land and air emissions are calculated using all releases for included zip codes, summed to total pounds across all toxins. For toxins, "year" refers to the year in which the data were released rather than collected.

Table 2
Mean Census Zip Code Characteristics for Controls and Treatment Quartiles

		Impacted Zip Codes			
	Treatment	Quartile 1	Quartile 2	Quartile 3	Quartile 4
Median Income	53,120	46,411	48,057	44,202	46,271
Median Number of Rooms	5.8	5.6	5.7	5.4	5.5
% Vacant	6.1	5.7	6.2	6.7	6.5
% Owner Occupied	73.1	69.8	71.0	66.9	69.5
% Built in 1990s	24.4	20.4	20.2	16.5	18.7
Single Family Units	8457	8565	9071	8457	8595
Education					
% High School Grad	26.6	29.4	30.2	32.4	29.6
% College Grad	19.2	16.0	15.6	13.9	15.1
% Advanced Degree	10.2	8.0	7.7	6.3	7.7
Demographics					
% White	79.5	80.2	76.2	75.9	78.6
% Black	11.8	12.7	13.7	17.1	13.2
% Poverty	5.6	7.7	7.1	7.6	6.9
Age 21 and Older	70.9	70.6	70.1	72.0	70.2
Age 65 and Older	12.5	12.1	12.1	14.1	12.1
Kids under 18 in Household	34.4	33.4	34.8	31.2	34.1
Acres	13,404	13,847	15,373	14,575	14,002
Population Density (per acre)	2.5	2.5	2.57	2.41	2.48
Total Zip Codes	991	25	23	23	27

Notes: All 2000 census variables are taken from the American Fact Finder Census website and reported at the zip code level. "Treatment" is classified by zip code. Zip codes are considered treated if they have non-zero land and/or air releases for toxin reporting firm in SIC codes 10, 12, 4911, 4931, 4939, 4953, 5169, 5171, and 7389. Quartiles are determined using 1998 reporting year releases for relevant industries (see Section 6).

Table 3
Sales Price and Average Land & Air Toxic Pollution Across Time by Treatment Status

		Treatment			
Year	Controls	Quartile 1	Quartile 2	Quartile 3	Quartile 4
Panel A: Median Home Value					
1998	171,267	150,665	152,115	133,091	142,987
1999	178,059	155,566	157,929	140,555	148,639
2000	186,276	159,114	167,013	154,253	157,946
2001	195,370	169,375	177,362	164,549	164,425
2002	208,065	177,751	189,908	177,652	174,439
Panel B: Average Reported Land and Air Toxic Pollution					
1998	47,907	169,934	222,503	201,896	191,206
1999	56,556	168,352	194,950	203,708	161,723
2000	51,800	110,911	165,614	218,420	1,899,283
2001	52,715	77,397	178,543	1,297,221	2,339,044
2002	45,219	68,844	194,206	312,118	2,701,955

Notes: See Table 1. “Treatment” is classified by zip code. Zip codes are considered treated if they have non-zero land and/or air releases for toxin reporting firm in SIC codes 10, 12, 4911, 4931, 4939, 4953, 5169, 5171, and 7389. Quartiles are determined using 1998 reporting year releases for relevant industries (see Section 6).

Table 4
Impact of 1998 TRI Information Release on Log Single Family Home Price

	(1)	(2)	(3)
Panel A: All Balanced Controls			
Post X Quartile 1	0.63	2.05*	2.07
	(1.78)	(1.15)	(1.56)
Post X Quartile 2	0.25	0.16	-0.32
	(1.32)	(1.05)	(0.99)
Post X Quartile 3	0.24	-0.39	-2.04
	(2.08)	(1.39)	(1.28)
Post X Quartile 4	-2.31*	-2.30**	-2.78**
	(1.20)	(1.11)	(1.19)
Fixed Effects			
Zip Code	X	X	X
Month-by-Year	X		
State-by-Month-by-Year		X	
Month and Year			X
Linear Zip Code Trends			X
Zip Codes	1,093	1,093	1,093
Observations	56,836	56,836	56,836
Panel B: Only Non-Zero Toxin Zip Codes			
Post X Quartile 1	1.05	2.58**	2.81*
	(1.88)	(1.13)	(1.54)
Post X Quartile 2	0.13	0.79	-0.38
	(1.59)	(1.19)	(1.24)
Post X Quartile 3	1.05	1.45	-1.12
	(2.09)	(1.07)	(1.24)
Post X Quartile 4	-2.68**	-2.37**	-3.31**
	(1.27)	(1.16)	(1.47)
Fixed Effects			
Zip Code	X	X	X
Month-by-Year	X		
State-by-Month-by-Year		X	
Month and Year			X
Linear Zip Code Trends			X
Zip Codes	534	534	534
Observations	27,768	27,768	27,768

Notes: Standard errors are clustered at the zip code level. Regressions include all zip codes with data from years 1998 through May of 2002. "Post" is for all months after the release of policy-updated Toxics Release Inventory data, which begins in March of 2000. "Treatment" is classified by zip code. Zip codes are considered treated if they have non-zero land and/or air releases for toxin reporting firm in SIC codes 10, 12, 4911, 4931, 4939, 4953, 5169, 5171, and 7389. Quartiles are determined using 1998 reporting year releases for relevant industries (see Section 6).

Table 5

Robustness of Results to Basic Specifications

	(1) Include CA	(2) All	(3) Deregulated	(4) Quadratic Trends	(5) NASDAQ by State	(6) NASDAQ by MSA	(7) Single Treatment	(8) Some 1998 Toxics Only
Post X Quartile 1	1.16	-0.28	2.06	2.07	2.03	2.07		
	(1.35)	(0.99)	(1.56)	(1.57)	(1.54)	(1.54)		
Post X Quartile 2	0.28	-0.83	-0.31	-0.32	-0.33	-0.36		
	(1.21)	(1.01)	(0.99)	(1.00)	(0.99)	(0.99)		
Post X Quartile 3	-1.66	-1.37*	-2.05	-2.04	-2.09	-2.12*		
	(1.02)	(0.73)	(1.29)	(1.30)	(1.28)	(1.28)		
Post X Quartile 4	-3.48***	-2.20**	-2.79**	-2.78**	-2.82**	-2.83**		
	(1.19)	(0.91)	(1.19)	(1.20)	(1.18)	(1.19)		
Post X “Treatment”							-2.48***	-3.27**
							(0.88)	(1.27)
Zip Codes by Group								
Q1	29	64	25	25	25			
Q2	31	58	27	27	27			
Q3	32	85	23	23	23			
Q4	27	66	27	27	27			
Treatment							50	50
Total Zip Codes	1,427	4,318	1,093	1,093	1,093	1,093	1,093	102
Observations	74,204	172,935	56,836	56,836	56,836	56,836	56,836	5,304

Notes: Standard errors are clustered at the zip code level. All regressions include month effects, year effects, zip code fixed effects, and zip code-specific linear trends. See notes from Table 4. Column 1 includes data from California. Column 2 includes all housing data with at least 3 sales per year. Column 3 controls for state-level timing of electricity deregulation. Column 4 allows for more flexible quadratic zip code trends. Column 5 controls for changes in the NASDAQ stock index interacted with state fixed effects to allow effects to differ by state, and Column 6 allows the effect to vary by MSA. Column 7 classifies quartiles 1 and 2 of new releases as part of the control group, and combines quartiles 3 and 4 into as single treatment group. Column 8 follows column 7, but drops all controls not in quartiles 1 or 2.

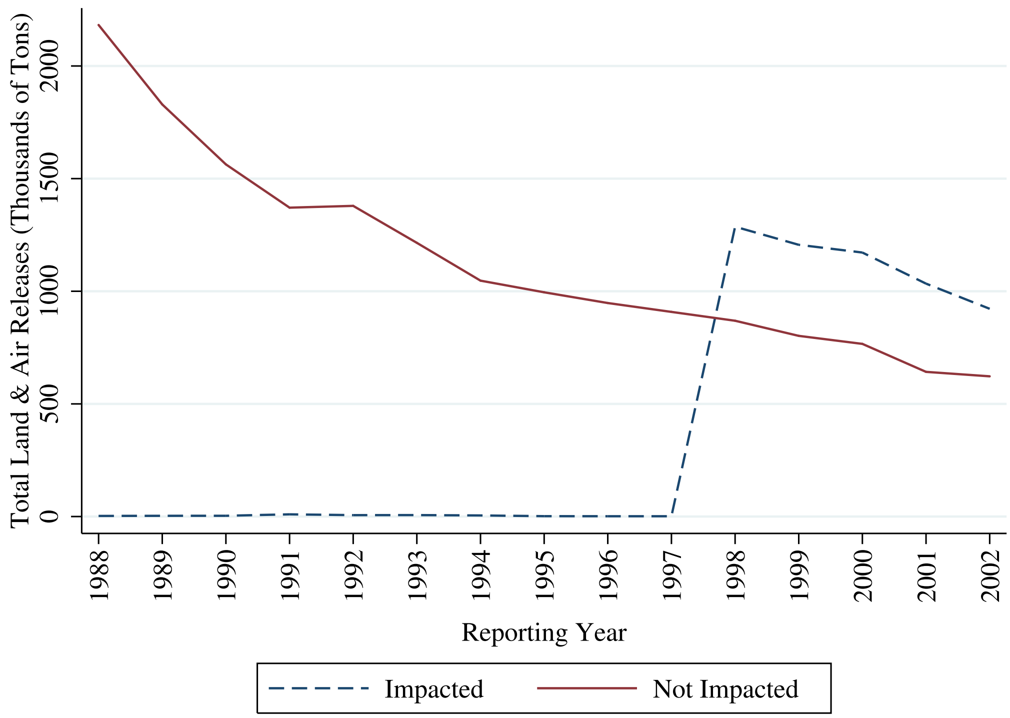
Table 6
Variations in Treatment/Control Groups and Differential Effects by Subgroups

	(1) Avg. Education	(2) Median Income	(3) Homeowners	(4) Kids < 18	(5) Pop. > 65
Post X Treatment	-2.35**	-2.99***	-2.66***	-2.73***	-3.05***
	(1.04)	(1.07)	(0.98)	(1.01)	(1.01)
Post X High Ed	0.78**				
	(0.33)				
Post X Treat X High Ed	0.2				
	(1.96)				
Post X Income		0.19			
		(0.33)			
Post X Treat X Income		2.14			
		(1.80)			
Post X High Homeownership			-0.16		
			(0.33)		
Post X Treat X Owner			0.58		
			(2.19)		
Post X High Share with Kids				1.09	
				(2.04)	
Post X Treat X Kids				-0.07	
				(0.34)	
Post X High Share above 65					0.04
					(0.38)
Post X Treat X Above 65					2.21
					(1.97)
Zip Codes	1,093	1,093	1,093	1,093	1,093
Observations	56,836	56,836	56,836	56,836	56,836

Notes: Standard errors are clustered at the zip code level. All regressions include month effects, year effects, zip code fixed effects, and zip code-specific linear trends. See notes from Table 4. Each regression is a difference-in-difference-in-difference specification, where the additional interaction term allows treatment effects to vary by 2000 census characteristics as described in Section 7. Column 1 allows for differential effects by percentage of the population with a bachelors degree, column 2 by median income, column 3 by share of units that are owner occupied, column 4 by share of households with children under 18, and column 5 by fraction of the population over the age of 65.

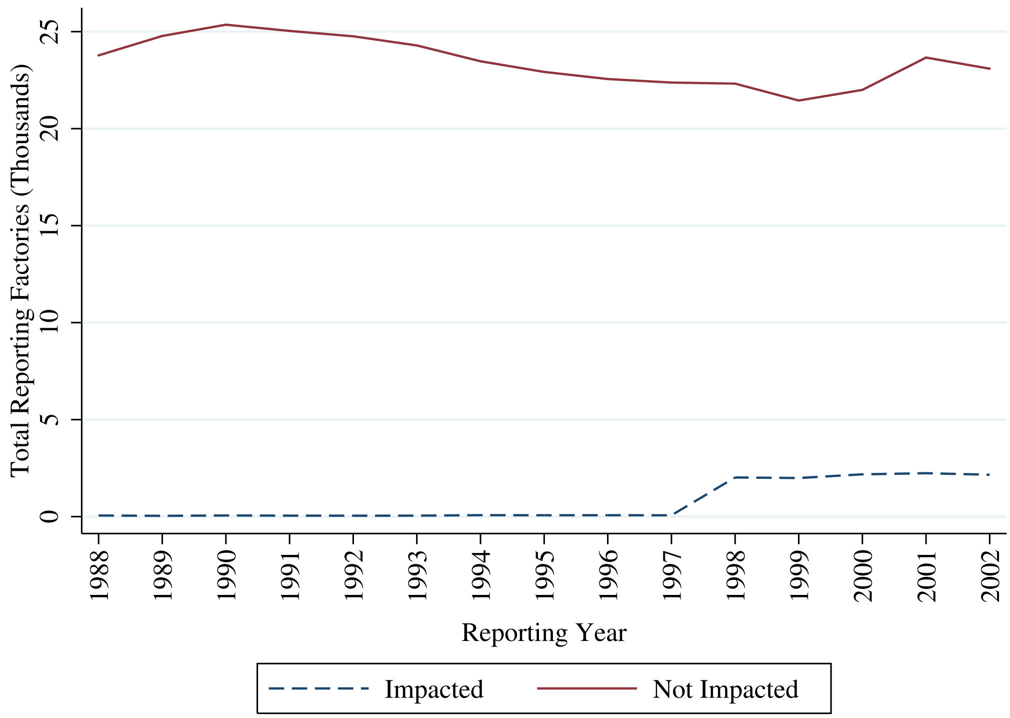
10 FIGURES

Figure 1
Total Toxic Releases Reported for Impacted vs. Not Impacted Industries



Notes: Toxics are the sum of all land and air releases, in thousands of tons, across all toxins recorded as reported in the Toxics Release Inventory. "Impacted" indicates firms classified under SIC codes 10, 12, 4911, 4931, 4939, 4953, 5169, 5171, and 7389 (see Section 2). "Not Impacted" includes all other industries.

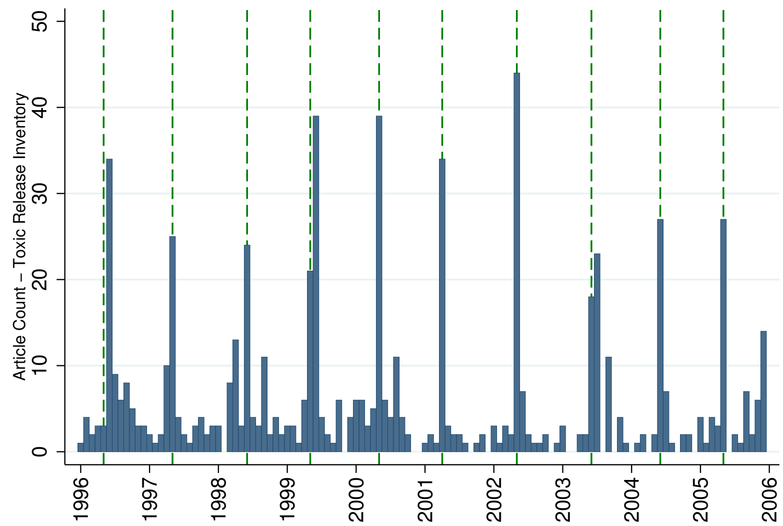
Figure 2
Number of Reporting Facilities for Impacted vs. Not Impacted Industries



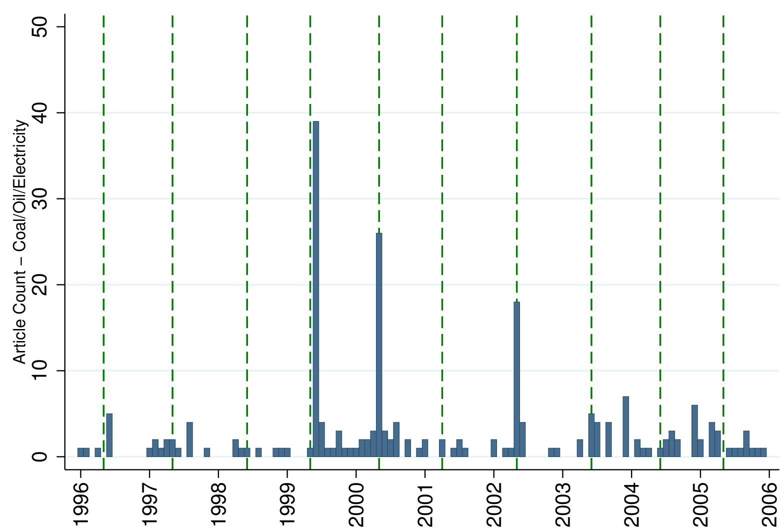
Notes: Count of total reporting firms reporting any non-zero land and air releases to the Toxics Release Inventory. “Impacted” indicates firms classified under SIC codes 10, 12, 4911, 4931, 4939, 4953, 5169, 5171, and 7389 (see Section 2). “Not Impacted” includes all other industries.

Figure 3
Number of Articles Archived on LexisNexis® Containing Selected Keywords

Panel A: Occurrence of “Toxics Release Inventory”

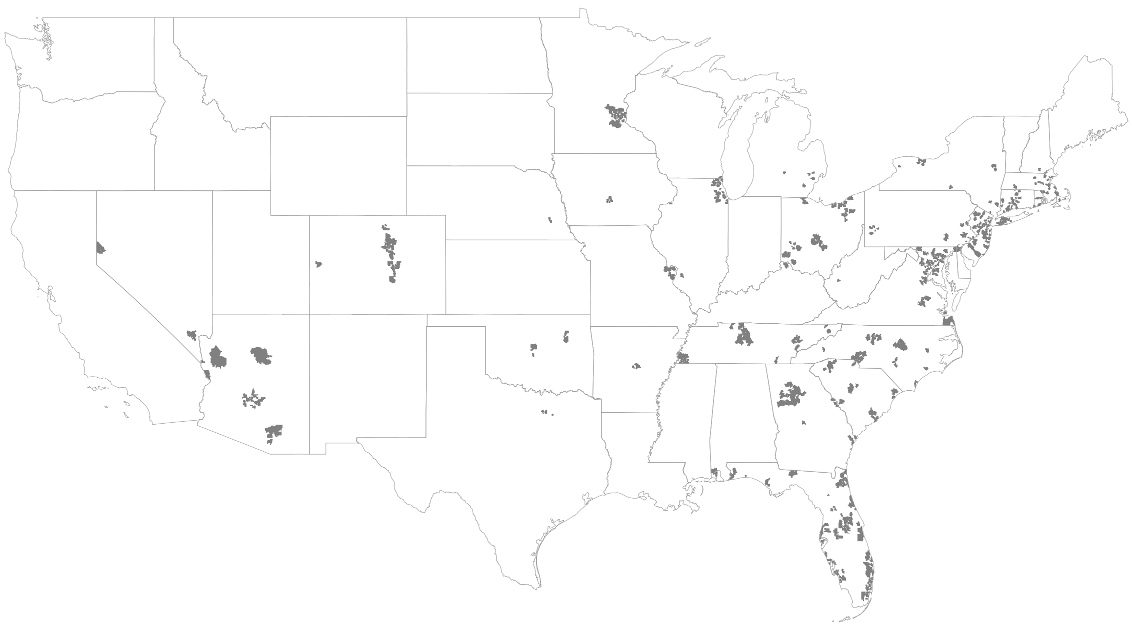


Panel B: Occurrence of “Coal”, “Oil”, and/or “Electricity”



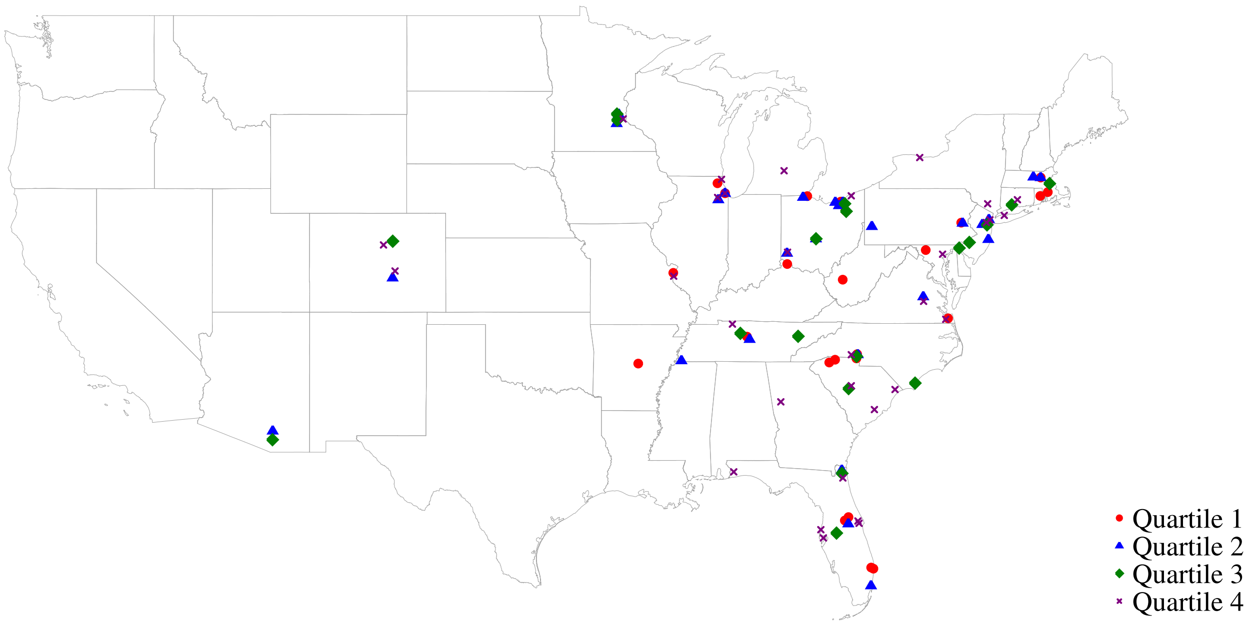
Notes: Counts of news stories archived on LexisNexis® containing particular text in specifically the headline or opening paragraph, by month and year. Relevant texts are shown on y-axis labels. Dashed lines mark the annual Federal public release time of newest TRI data, as specified by the EPA website.

Figure 4
Primary Analysis Zip Codes



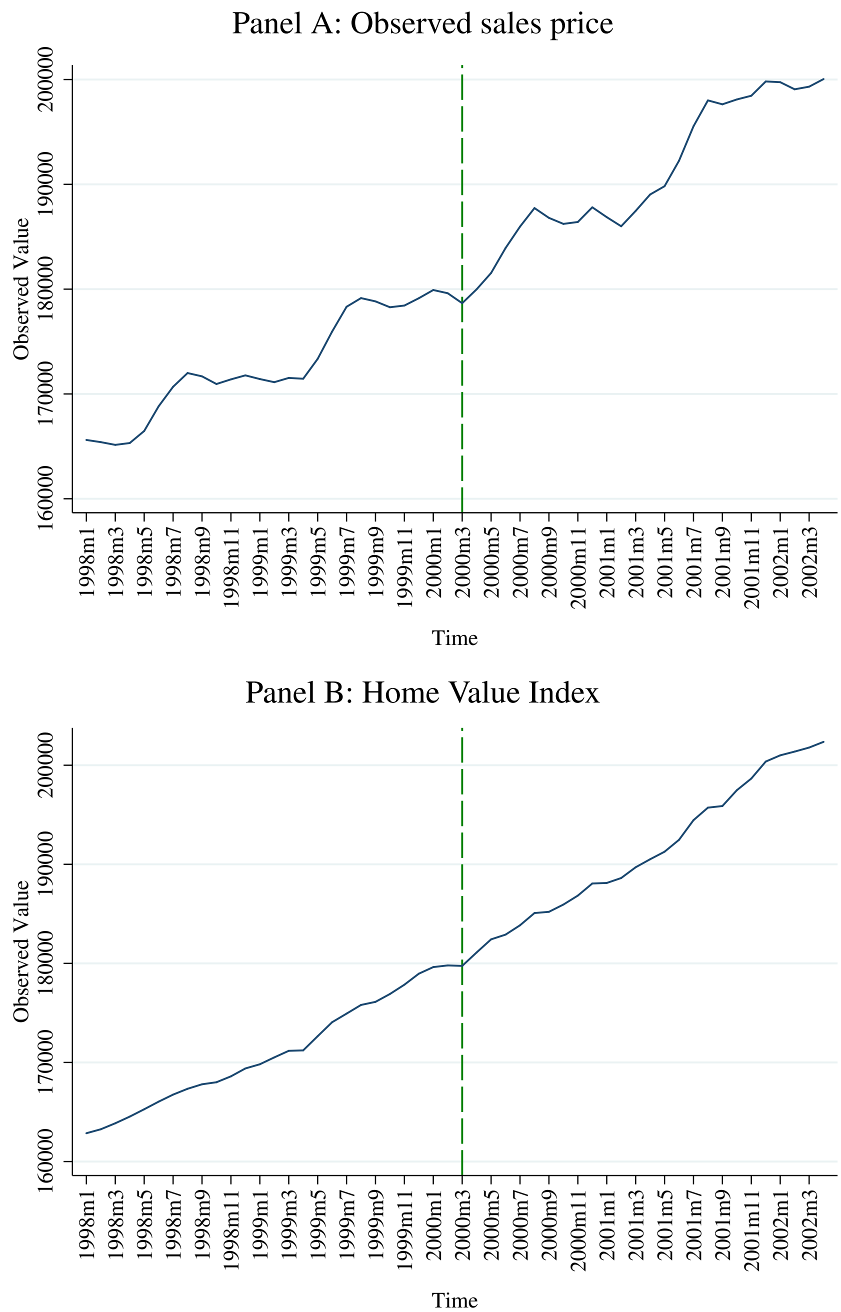
Notes: Primary analysis zip codes, which are restricted to a balanced panel from January of 1998 through May of 2002, with at least one sale per month per zip code. See Section 6 for further details.

Figure 5
Treatment Zip Code Locations



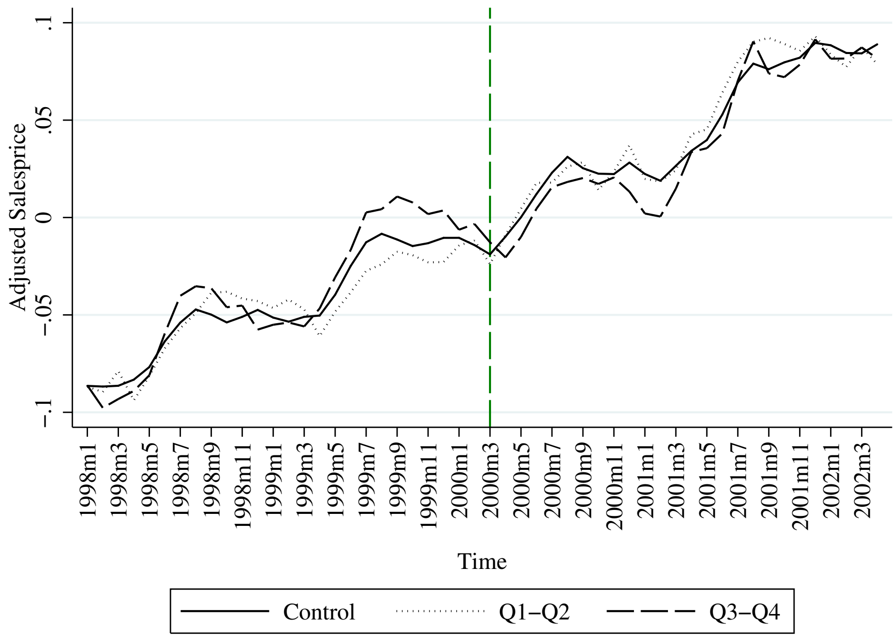
Notes: Primary analysis treatment zip code groups split by quartile of new TRI-reporting information. See Section 6 for further details.

Figure 6
Observed Sales Price and Home Value Index for Primary Analysis Zip Codes



Notes: Sales prices are based on median zip code-level observed month-by-year sales values. Home Value Index is based on the Zillow® proprietary formula for estimating value of homes in a zip code. Vertical line indicates the time period of the 1998 policy impacted Toxics Release Inventory data first becoming publically available (March of 2000).

Figure 7
Event Study Comparison of Sales Price

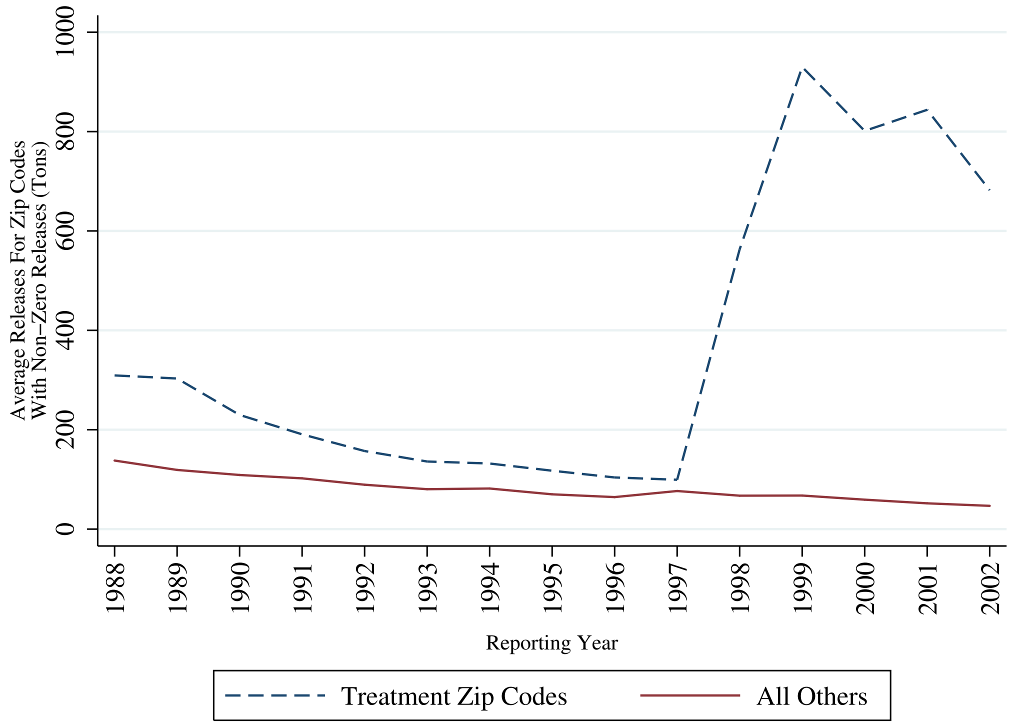


Notes: Sales prices are based on median zip code-level observed month-by-year sales values. Averages are calculated using (1) control group, (2) treatment group quartiles 1 and 2, and (3) treatment group quartiles 3 and 4. Vertical line indicates the time period of the 1998 policy impacted Toxics Release Inventory data first becoming publically available (March of 2000).

Figure 8
Event Study Comparison of Sales Price

Notes: Sales prices are based on median zip code-level observed month-by-year sales values. Each point is a coefficient from the treatment interaction terms in equation (5), with the baseline month of January of 1998. Vertical line indicates the time period of the 1998 policy impacted Toxics Release Inventory data first becoming publically available (March of 2000).

Figure 9
Average Air Toxics Released per Zip Code by Treatment



Notes: Toxics are the sum of all land and air releases, in thousands of tons, across all toxins recorded as reported in the Toxics Release Inventory. "Impacted" indicates firms classified under SIC codes 10, 12, 4911, 4931, 4939, 4953, 5169, 5171, and 7389 (see Section 2). "Not Impacted" includes all other industries. Average per zip is calculated by dividing total releases by number of zip codes in each group.

A Appendix — Example Newspaper Excerpts on New TRI Releases

> Two Baltimore Gas and Electric Co. power plants in Anne Arundel County released 11.5 million pounds of toxic chemicals into the air in 1998, ranking them first in the state and 11th in the nation for toxins, the U.S. Environmental Protection Agency said yesterday.[28] (*The Sun*)

> The heaviest polluters were the 27 power plants in Ohio, which emitted 113.9 million pounds of toxic chemicals in 1998. In comparison, the 34 power plants in New York released 18.7 million pounds, and the 15 power plants in New Jersey released 8 million pounds.[29] (*The New York Times*)

> The report shows that two of the state's coal power plants, Sithe Energy's Keystone plant in Armstrong County and Edison Mission Energy Inc.'s Homer City plant in Indiana County are among the top 20 power plants in the nation, releasing a combined 18.5 million pound (sic) of toxic chemicals into the air, water, and land.[30] (*Pittsburgh Post-Gazette*)

[28] Murray (May 12, 2000)
[29] Hu (May 12, 2000)
[30] Hebert (May 12, 2000)

Table A-1
Impact of 1998 TRI Information Release on Log Single Family Home Price

	(1)	(2)	(3)
Panel A: All Balanced Controls			
Post X Quartile 1	0.06	1.32	0.54
	(1.87)	(1.21)	(0.83)
Post X Quartile 2	-0.64	-0.35	-0.74
	(1.35)	(0.89)	(0.52)
Post X Quartile 3	0.4	-0.63	-0.68
	(1.85)	(1.01)	(0.66)
Post X Quartile 4	1.3	1.22	1.42
	(2.71)	(2.48)	(1.14)
Fixed Effects			
Zip Code	X	X	X
Month-by-Year	X		
State-by-Month-by-Year		X	
Month and Year			X
Linear Zip Code Trends			X
Zip Codes	1,093	1,093	1,093
Observations	56,836	56,836	56,836
Panel B: Only Non-Zero Toxin Zip Codes			
Post X Quartile 1	-0.03	1.38	0.23
	(2.01)	(1.23)	(0.86)
Post X Quartile 2	-0.55	0.65	-0.91
	(1.54)	(0.96)	(0.62)
Post X Quartile 3	-0.22	-0.04	-1.05
	(2.05)	(1.07)	(0.74)
Post X Quartile 4	-1	-0.31	0.3
	(1.34)	(1.27)	(0.70)
Fixed Effects			
Zip Code	X	X	X
Month-by-Year	X		
State-by-Month-by-Year		X	
Month and Year			X
Linear Zip Code Trends			X
Zip Codes	534	534	534
Observations	27,768	27,768	27,768

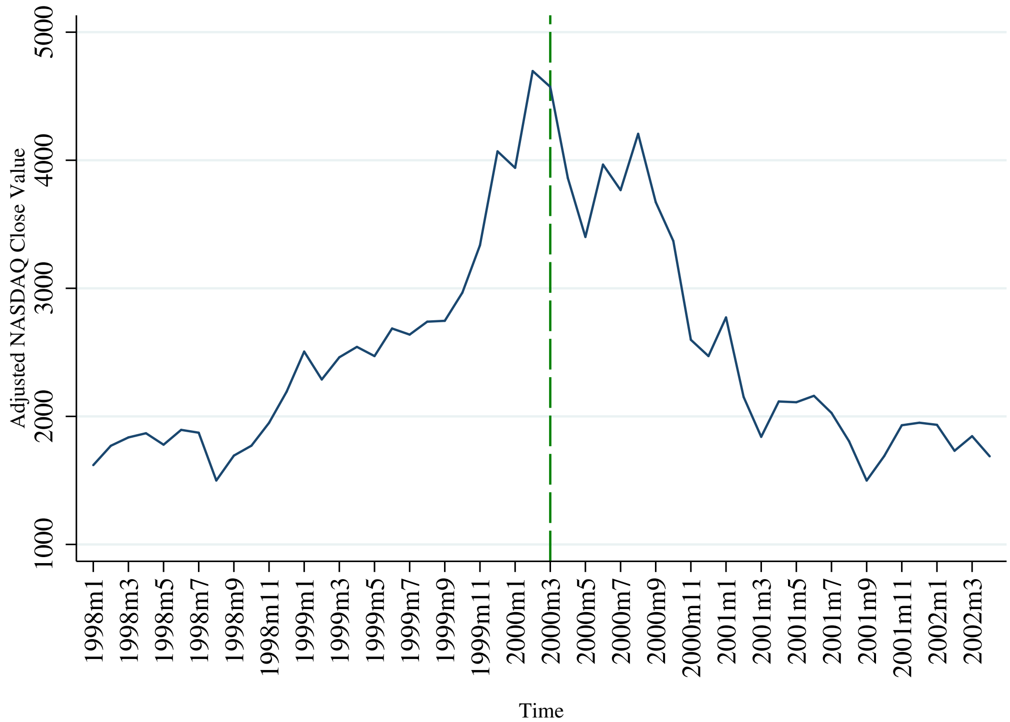
Notes: See Table 4. HVI is the Zillow® estimated median value for all homes in a zip codes, calculated using a proprietary formula.

Table A-2
Including Lead Time-Indicator Interactions

	1 Short Period Lead	2 Long Period Lead
Post X Treatment	-2.35***	-2.11**
	(0.91)	(0.90)
3 Months Pre X Treatment	-1.13	
	(0.86)	
6 Months Pre X Treatment	2.05	
	(1.57)	
12 Months Pre X Treatment		0.67
		(0.76)
18 months Pre X Treatment		-0.1
		(0.74)
Joint F-test	0.3991	1.5543
Joint p-value	0.671	0.2118
Observations	56,836	56,836

Notes: Standard errors are clustered at the zip code level. Regressions include all zip codes with data from years 1998 through May of 2002, and fixed effects for month, year, zip code, and linear zip code time trends. “Post” is for all months after the release of policy-updated Toxics Release Inventory data, which begins in March of 2000. “Treatment” is classified by zip code. Zip codes are considered treated if they have non-zero land and/or air releases for toxin reporting firm in SIC codes 10, 12, 4911, 4931, 4939, 4953, 5169, 5171, and 7389. Quartiles are determined using 1998-reporting year releases for relevant industries (see Section 6). As a test for pre-trend differences, regressions include indicators for lead periods (either 3 and 6 month leads or 12 and 18 month leads) interacted with the treatment indicator (zip codes in quartiles 3 and 4 of new releases). F-test and associated p-values are for tests of joint significance of both lead-period interactions.

Figure A-1
Monthly Closing Value of NASDAQ Stock Index

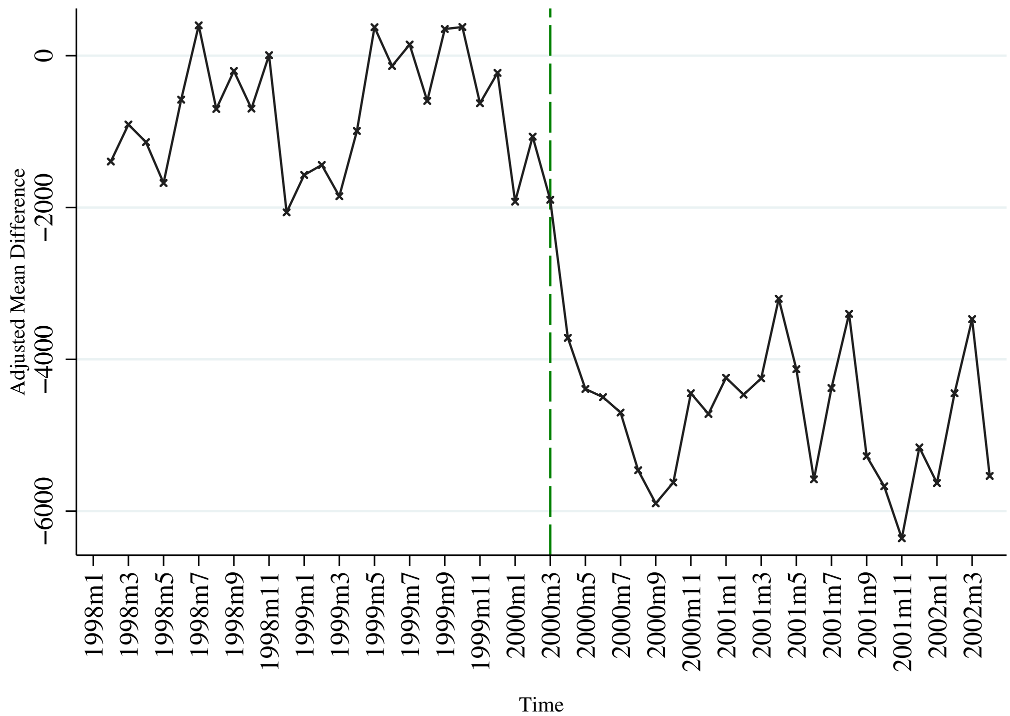


Notes: Data are monthly average NASDAQ closing values as calculated using historical stock market data. Vertical line indicates March of 2000, the earliest period of available new TRI information.

Figure A-2
Difference Between Treatment and Control Group Home Prices – Including Data from California

Notes: Sales prices are based on median zip code-level observed month-by-year sales values. Each point is a coefficient from the treatment interaction terms in equation (5), with the baseline month of January of 1998. Vertical line indicates the time period of the 1998 policy impacted Toxics Release Inventory data first becoming publically available (March of 2000). Data include zip codes from the state of California.

Figure A-3
Difference Between Treatment and Control Group Home Prices Adjusted for State-Specific Effects of NASDAQ Closing



Notes: Sales prices are based on median zip code-level observed month-by-year sales values. Each point is a coefficient from the treatment interaction terms in equation (5), with the baseline month of January of 1998. Vertical line indicates the time period of the 1998 policy impacted Toxics Release Inventory data first becoming publically available (March of 2000). Regressions also control for NASDAQ monthly closing value, interacted with region indicators to allow effects to vary by state.

Figure A-4
Difference Between Treatment and Control Group Home Prices – Only Non-zero Reporting Zip Codes

Notes: Sales prices are based on median zip code-level observed month-by-year sales values. Each point is a coefficient from the treatment interaction terms in equation (5), with the baseline month of January of 1998. Vertical line indicates the time period of the 1998 policy impacted Toxics Release Inventory data first becoming publicly available (March of 2000). Data are restricted to zip codes with at least non-zero emissions reported in 1998.