# Information Regulation:

# Do the Victims of Externalities Pay Attention?

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Abstract

Individuals living in metropolitan areas are typically exposed to a large number of industrial risks. Information regulation is a new tool to manage such risks. We ask if large-scale information initiatives directed at the general public can affect individual risk perceptions and possibly change the spatial allocation of resources in a metropolitan economy. The answer is affirmative. Using the publication of the Toxic Release Inventory as a case study, we find a decline in predicted prices when new information on pollution became available, indicating that home buyers adjusted their risk perceptions upward. The response to new information on pollution, however, appears to be fairly local. For sources of toxic emissions that are very close by and for sources that are located at a distance of more than one mile, perceptions do not seem to have changed.

Keywords: Information regulation, pollution prevention, Toxic Release Inventory

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## I. The Promise of Information Regulation

The United States pioneered the use of information as a regulatory tool to manage industrial risks in the 1980s. Today, many industrialized nations including members of the European Union and Japan have adopted similar programs, and international organizations such as the OECD and the United Nations recommend that governments develop community-right-to-know initiatives to reduce environmental risks (United Nations, 1992). In the United States, firms are required to provide ever more extensive information about their operations. In a recent effort, more than 15,000 facilities had to disclose their risk management plans, including accident histories for the past five years and analyses of the off-site consequences of disasters. Much of this information is publicly available through the RMP*Info database (Elliot, Kleindorfer and Lowe, 2001). Via Internet, citizens can now track the results of facility inspections by the government, and they are able to monitor regulatory citations and corrective actions that firms have to undertake (see, e.g., the efacts system published by the Pennsylvania Department of Environmental Protection, 2001). At the federal level, the U.S. Environmental Protection Agency (EPA) established expanded mandatory information disclosure as one of ten long-term strategic goals (Case, 2001).

While information regulation initiatives abound, to date, the publication of the Toxic Release Inventory (TRI) by the EPA remains perhaps the best-known effort to use information as a regulatory tool. Established as part of the 1986 Emergency Planning and Community Right-to-Know Act (EPCRA), TRI requires manufacturing firms with more than ten employees to report the release of approximately 650 toxic substances. The EPA then makes this information publicly available.

Information regulation typically complements the use of other regulatory tools such as performance-based standards or price incentives (Kleindorfer and Orts, 1988). The basic idea underlying information regulation is to provide an additional mechanism to influence the behavior of firms and individuals who live close to industrial facilities (Arora and Gangopadhyay, 1995; Kennedy, Laplante and Maxwell, 1994). TRI data allow investors, consumers and nearby homeowners, who care about the environmental performance of businesses, to identify firms with considerable emissions and adjust investment and consumption decisions accordingly. In addition, the available data makes it easier for persons to avoid areas with high aggregate emissions.

Since the first publication of TRI data in June 1989, TRI emissions have fallen by 43%, while industrial activity has risen by 28% over the same time period. A part of this decline in emissions is often credited to the TRI initiative. In a Presidential memo dated August 8, 1995, the Administration asserted that "Right-to-Know protections provide a basic informational tool to encourage informed community-based environmental decision making and provide a strong incentive for businesses to find their own ways of preventing pollution." In a more recent assessment, the EPA concludes that there is a "broad consensus that national publication of the TRI data by the government, followed by analysis by citizens' groups and the news media, led to action by industry to reduce emissions." (EPA, 2000)

There are several mechanisms by which the publication of emissions data can influence firm behavior. A first one operates via capital markets. Hamilton (1995) shows that the initial release of TRI data in 1989 influenced stock market valuations, with reporting firms experiencing negative abnormal returns. In subsequent years, stock market returns were particularly negative for firms whose environmental performance worsened over time relative to other firms (Khanna, Quimio and Bojilova, 1998). The corporations which experienced the largest stock price declines then reduced their on-site emissions to a greater extent than industry peers (Konar and Cohen, 1997).

Neighborhood groups constitute a second channel of influence. These groups use TRI data to directly pressure facilities for change, and they lobby consumers and elected officials for their support of such change (U.S. General Accounting Office, 1994). The EPA has documented more than 40 cases in which right-to-know information was used to improve environmental quality (EPA, 2000: Appendix C).

While we have direct evidence of the reactions of investors, firms and environmental groups to the release of TRI information, little is known whether and how the victims of externalities update their beliefs about nearby environmental risks when new TRI information becomes available. This paper seeks to fill this gap. There are several reasons why the reactions of residents who live in the vicinity of TRI facilities are of interest.

First, economists have long recognized that the damages that victims suffer from externalities provide the necessary incentives for undertaking efficient levels of defensive activities (Olson and Zeckhauser, 1970; Baumol and Oates, 1975). For instance, individuals have incentives to stay away from heavily polluted areas. More information on the size of externalities makes it easier for individuals to protect themselves in an optimal manner. Depending on the direction of the adjustment in risk perceptions, areas in the immediate vicinity of TRI facilities may become more or less attractive for residents. Such changes will be reflected in equilibrium property values, which can then affect future landlord reinvestment behavior (Kutty, 1995). In short, if individuals pay attention to information regulation, this type of regulation can possibly lead to the spatial reallocation of resources in the metropolitan economy.

Second, nearby residents might engage in collective action to reduce pollution levels. A growing body of research shows that community pressure applied by neighborhood organizations can induce firms to cut emissions or change location (Baron, 2001; Pargal and Wheeler, 1996). For instance, information regulation initiatives appear to have motivated chemical manufacturers to transfer wastes to different locations (Khanna et al., 1998). Upon learning the true level of pollution, citizen groups might also lobby regulators and lawmakers for change. While information regulation can place additional constraints on firms and change their desire to locate in particular areas, pressure on polluting firms and the demand for regulation will only increase if the release of information increases risk perceptions. Thus, it is interesting to learn how beliefs about industrial risks changed in response to the publication of the TRI.

In this paper, we study individuals' reactions to the release of TRI data by analyzing changes in house prices in the Philadelphia region. Following an early contribution by Ridker and Henning (1967), there is now a large literature that studies how environmental amenities are capitalized into residential property values (for a meta-analysis, see Smith and Huang, 1995; Boyle and Kiel (2001) provide a literature review). Building on this work, we seek to identify the effects of releasing environmental information on risk perceptions.[1] The question we ask is whether individual risk perceptions changed sufficiently after the release of TRI information to move house prices. A number of papers found that property values can change significantly if residents learn about changes in the quality of their environment. For instance, Kiel and McClain (1995a) document changes in property values and home appreciation rates (1995b) in the course of the siting process and operation of an incinerator. Gayer, Hamilton and Viscusi (2000) show how the release of EPA Remedial Investigation and Feasibility Studies for seven Superfund sites impacted property values in the greater Grand Rapids area. They find that residents' willingness to pay to avoid risks decreased after the release of environmental reports.

In contrast to these papers, which analyze the effects of a few sources of industrial risk, individuals in our study simultaneously learn about emissions at a large number of facilities. Unlike people living in sparsely populated areas, urban residents typically face a multitude of risks, ranging from dry cleaners and bakeries to chemical facilities. Not much is known about how individuals react to the release of new information which concerns a large number of industrial risks.

[1] Other studies looking for the effects of releasing environmental information include Michaels and Smith (1990), Kohlhase (1991), Kiel (1995), McMillen and Thorsnes (2000). Further studies are discussed below.

Our work is most closely related to Bui and Mayer's (2000) contribution which looks for TRI announcement effects in Massachusetts. They find that the initial release of TRI information had no significant impact on the distribution of house prices. In contrast to their approach, which uses repeat-sale house price indices and aggregate emission levels at the zip code level, we rely on records of individual property transactions in the Philadelphia area. Furthermore, we identify both the number of TRI facilities and their emission levels for incremental quarter-mile distance rings around transacted properties. This level of detail should assist us in identifying the effects of any learning processes that were triggered by the release of the TRI data.

The paper is organized as follows. Section II describes our empirical strategy to identify changes in risk perceptions which are due to the publication of TRI information. In Section III, we present our empirical findings. We discuss our results in Section IV, and Section V offers concluding remarks.

## II. Empirical Strategy and Data

The purchase of a home is one of the most significant economic decisions that households make. Given its importance, it is reasonable to expect that individuals expend considerable resources to acquire information on aspects of house quality they care about. The release of the first TRI data on 19 June 1989 represents an exogenous shift in the cost of acquiring information about toxic emissions. While it was more difficult to learn about emission levels prior to the publication of the TRI database, individuals then gained access to emissions data by facility, release medium, and toxic substances. In 1989, TRI data were posted in the on-line database of the National Library of Medicine, which was accessible through local libraries. Hard copies were available from the EPA at no cost to individuals. Moreover, the press and environmental groups used the opportunity to inform the public about regional emission levels and the most significant polluters. The Philadelphia Inquirer, for instance, published the names of important polluters in Pennsylvania one day after the release of TRI information. Several regional facilities were included in the national press on lists of the "top 100 polluters" in the country.

### 1. Empirical Strategy

We use a simple empirical strategy to identify whether or not risk perceptions shifted as a result of the release of TRI information. Prior to June 1989, individuals had to rely on proxies for emission levels when buying a home. Such proxies possibly included the existence of haze in the metropolitan area, the prevalence of industrial odors in a neighborhood and perhaps negative health outcomes such as respiratory diseases. A proxy is useful if it is correlated with the underlying measure of risk at a location. However, as it is difficult to infer true pollution levels from observables, it is possible that perceived risk at location *l*, $R_l + \nu_{it}(R_l)$, is a biased measure of true risk $R_l$. $\nu_{it}(R_l) \in [-R_l, \infty]$ denotes home buyer *i*'s misperception at time *t*. Misperceptions can depend on the level and type of risk. Once TRI information becomes available, the home buyer can update his estimate of the pollution level.[2] If TRI is effective, $R_l + \nu_{i1}$ will be a more accurate reflection of risk.

A hedonic price function (Rosen, 1974), reflecting the locus of tangencies between offer and bid curves for properties, indicates individuals' marginal willingness to pay for housing characteristics.

(1) $$Price = \beta_0 + \beta_1 (R_l + \nu_{it}) + \beta_2 House + \beta_3 Neighborhood + \beta_4 Time + \varepsilon$$

In our model, house prices are a function of a vector of housing characteristics (*House*), and the type of neighborhood the properties are located in. We control for the character of neighborhoods by including Census Tract fixed effects as well as School District fixed effects in our models. A vector of quarterly dummies captures the time trend in real estate prices.[3]

If risk perceptions change as a result of the release of new information ($R_l + \nu_{i0} \neq R_l + \nu_{i1}$), the estimated coefficient of the effect of pollution on house prices $\beta_1$ will change if the marginal willingness to pay for risk reductions varies with the level of risk. We can test for such changes by allowing the effect of pollution on house prices to vary by period:

[2] Gayer, Hamilton and Viscusi (2000) propose a model where individuals use new information on environmental risks to update their risk beliefs in Bayesian fashion. However, as the authors note, the data used in hedonic pricing studies do not easily lend themselves to a test of different learning models. All that can be inferred is whether and how risk perceptions have changed. This is focus of the present paper.

[3] An alternative to the approach chosen here is the use of nonparametric or semiparametric analysis, which may allow the researcher to better capture nonlinear movements in prices. Meese and Wallace (1991) find no significant differences between nonparametric and flexible parametric specifications. McMillen and Thorsnes (2000) report minor qualitative differences, although they reject even a very flexible OLS specification in favor of their semiparametric model. The sample of prices studied here is large enough to permit a fairly flexible parametric specification including quarterly indicators for time.

$$\text{(2)} \quad \begin{aligned} Price &= \beta_0 + \beta_1 (R_l + \nu_{it}) + \beta_2 House + \beta_3 Neighborhood + \beta_4 Time \\ &+ \beta_5 After + \beta_6 ((R_l + \nu_{it}) \times After) + \varepsilon \end{aligned}$$

*After* is an indicator variable which takes on a value of 1 if the house was sold after the release of new information and zero otherwise. Specification (2) allows the intercept of the price gradient with respect to risk ($\beta_5$) as well as its slope ($\beta_6$) to differ from period to period.

A common problem when testing for the effects of *new* information is that we cannot directly estimate (2) because we do not observe the perceived levels of risk prior to the publication of TRI ($R_l + \nu_{i0}$). However, we can still test whether or not the release of TRI information influenced risk perceptions by substituting true emission levels $R_l$ into equation (2).

$$\text{(3)} \quad \begin{aligned} Price &= \beta_0 + \beta_1' R_l + \beta_2 House + \beta_3 Neighborhood + \beta_4 Time \\ &+ \beta_5' After + \beta_6' (R_l \times After) + \varepsilon \end{aligned}$$

If risk perceptions prior to TRI are biased, $\beta_1'$ does not generally reflect the marginal willingness to pay for risk reductions because true risk $R_l$ is a (perhaps non-linear) transformation of perceived risk $R_l + \nu_{i0}$. Nevertheless (3) allows us to identify if and how risk perceptions changed as a result of TRI. As in the standard hedonic price model, observed price changes allow the identification of changes in risk perceptions. In period $t$=0, unobserved risk perceptions determine a set of market prices for properties with known risk characteristics. In period $t$=1, these risk characteristics remain unchanged and we would predict market prices to remain unchanged as well. However, if the underlying risk perceptions change, this will affect prices and alter the estimated relation between prices and true risk. In other words, we will observe no change in the effect of pollution on house prices ($\beta'_5 = \beta'_6 = 0$) if and only if risk perceptions remain unchanged ($R_l + \nu_{i0} = R_l + \nu_{i1}$).

Figure 1 illustrates the analysis. The bold line represents an individual's willingness to pay a higher price for a property with lower levels of pollution. The figure shows two transactions of two different properties. For property *m*, $R_m$ is the true risk level. In period *t*=0, the house buyer underestimates the true risk, believing that it is $R_m - \nu_m$. Given this misperception, he is willing to pay $P_{m,t=0}$. Similarly, the risk for property *n* with true risk $R_n$ is also underestimated ($R_n - \nu_n$) and, as a result, *n* trades at $P_{n,t=0}$. As $R_m - \nu_m$ and $R_n - \nu_n$ are unobserved, (3) relates the observed prices to true levels of risk. The estimated slope of the price gradient is $\beta'_1$.[4]

In period *t*=1, new risk information becomes available and homebuyers update their beliefs. Figure 1 assumes that misperceptions completely disappear. At risk levels $R_m$ and $R_n$ with transaction prices $P_{m,t=1}$ and $P_{n,t=1}$, the estimated marginal effect of risk on housing prices is $\beta'_1 + \beta'_6$. (If individuals are aware of the true risk, we have $\beta'_1 + \beta'_6 = \beta_1 + \beta_6$). However, irrespective of whether or not homeowners' misperceptions completely disappear, any update in risk beliefs will change the estimated impact of risk on housing prices after the release of new information. Moreover, the direction in which

[4] Note that, a special case aside, $\beta'_1$ does not reflect the true willingness to pay for a risk reduction. The bias in $\beta'_1$ depends on the distribution of $\nu_{i0}$. In figure 1, $\beta'_1 = \beta_1$ if the misperception was a constant.

housing prices adjust is indicative of the direction in which risk perceptions move. In the figure, housing prices fall because individuals initially underestimated the true level of risk. Our null hypothesis is that the release of TRI information had no impact on individual perceptions. For this case, we predict $\beta'_5 = \beta'_6 = 0$.

Before we turn to our results, we describe how we measure risk. We characterize the level of objective risk at a particular location with the sum of all toxic air releases in several quarter-mile distance rings around the property. The first ring aggregates toxic releases from all TRI facilities located between 0 and .25 miles from the property. The second ring measures releases between .25 and .5 miles. All in all, we have data for 20 such rings around each transacted property for a distance of up to 5 miles. While the TRI publishes information on releases into different media (air emissions, water discharges, underground and land disposal releases), we will focus on air emissions (the sum of fugitive air releases and stack air releases) because the relationship between the level of risk and the distance from the emission source is fairly straightforward for this type of release. In contrast, it is less clear whether residents who live a mile from a TRI facility should worry about its water discharges.

## 2. Data

Our data come from two sources. TRI information on emissions, the number, and the location of facilities was taken from the Right-to-Know Net (www.rtk.net). Data on property transactions, which include information on house characteristics, were obtained from Realist, a professional provider of real estate data. Our data covers all residential transactions in the period of June 1988 to June 1990, that is one year prior to and one year after the publication of the TRI. The set includes real estate transactions in five counties in the Philadelphia region: Bucks (N=5,691), Chester (N=4,594), Delaware (N=6,362), Montgomery (N=8,969) and Philadelphia (N=15,247). We geocoded all real estate transactions and computed emission levels for the 20 quarter-mile distance rings around each transaction. Geographic information systems (GIS) software allows us to see if emissions originating closer to a home have a larger impact on house prices than more distant pollution sources, as one would expect. Also, using GIS, we can measure emissions more accurately than would be possible if we relied on aggregate data for jurisdictions. For instance, three counties in our sample border the Delaware River. For homes close to the river, important sources of pollution are often located on the opposite bank of the Delaware in New Jersey. While our property data come from 5 counties in Pennsylvania, emissions data were taken from 18 counties located in four states. The only criterion we use to exclude emission sources from our data is their physical distance from the transacted properties; jurisdictional borders are of no relevance.

Table 1 provides summary statistics for our sample. We observe all 40,863 arms-length transactions in the five counties which took place in the year prior to and after the publication of the TRI (19 June 1988 to 19 June 1990). Within a quarter mile of the properties in our sample, facilities emitted on average 2,020 pounds of toxic chemicals into the air per year. In the second distance ring, emissions are 2,500 pounds. The volume of transactions across the two years is fairly similar with 50.3% of all home sales taking place after the publication of the TRI.

## III. Results

Estimation results for equation (3) are reported in Table 2. We adopt a semilogarithmic form of the hedonic price function with the log of house prices as our dependent variable. In a basic model (I), we find that the estimated impact of air pollution on property values changes after the release of TRI information. The coefficient on the indicator for the second period ($\beta'_5$) is statistically significant with a value of -0.005. Also, the estimated risk-price tradeoff is less steep after the publication of the TRI ($\beta'_6>0$), a further indication that risk perceptions changed. Interestingly, we cannot reject the hypothesis that perceptions remain the same for pollution sources located at a distance of less than a quarter mile, while the changes are statistically significant for sources in the second ring.[5] For pollution sources that are located at a distance of more than one mile (rings 5 through 20), we never find an economically or statistically significant risk-price relationship either before or after the publication of the TRI. Consequently, pollution in these rings is omitted from our models. All models in Table 2 control for a basic set of house characteristics (number of stories, frontage, and three indicators for the architectural style of the house), the time trend in real estate prices (seven quarterly indicators), and the general character of the neighborhood (School District and Census Tracts fixed effects).

We test the robustness of our findings by including the number of TRI facilities in the four distance rings (model II). That is, we ask whether risk perceptions changed controlling for the industrial character of a neighborhood. Again, we find a decline in

[5] *p*-values for *F*-tests are 0.22 and 0.03 respectively.

predicted prices and changes in the estimated risk-price relationship. For not quite half the properties in our sample, we have more detailed information on house characteristics.[6] These include the number of rooms, the number of full baths and the year of construction. Models (III) and (IV) in Table 2 report these results. We find the same pattern of predicted price changes as before. Similarly, our results are not sensitive to the choice of study period. Recall that one of the assumptions underlying our analysis is that the true level of risk $R_l$ remains the same before and after the release of new information. This requirement is less likely to be met for longer study periods. For this reason, Table 4 in the Appendix reports TRI announcement effects for study periods of six and four quarters, respectively. The results are qualitatively similar to the ones reported in Table 2. For all specifications in Table 2 and the Appendix, F-tests reject our null that the estimated impact of pollution on house prices remains the same before and after the publication of TRI. The results thus suggest that homebuyers updated their risk perceptions once new information became available. Moreover, the sign of the predicted price changes indicates that perceptions of risk increased. This result does not hold for pollution sources that are located within a quarter mile of the transacted properties. For these sources, perceptions remain unchanged.

[6] Our data are based on county-level transactions records. For three counties in our sample, these records are more extensive, allowing the inclusion of additional house characteristics.

## 1. Matched Sample Results

It is well understood that the non-random nature of property transactions can significantly bias estimates of changes in house prices (Gatzlaff and Haurin, 1998). Thus, one concern with the above results is that the sample of homes sold in the year prior to the release of TRI information might constitute a poor control for the set of houses brought to market after June 1989. We can only identify TRI announcement effects if, conditional on observed house characteristics, the "treated" properties (those sold at a time when additional environmental information was available) and the control group would have sold at similar prices had the TRI not existed. A closer look at our data reveals that this may not be the case. The homes sold prior to June 1989 tend to be located in areas with fewer TRI facilities and somewhat lower pollution levels.[7] Such differences can bias our estimates if, for instance, the marginal effect of air pollution on house prices is non-linear. To see this, suppose that the marginal effect of emissions on prices is smaller in high-pollution areas than in locations with low levels of emissions. This non-linearity could be due to residents in high-pollution areas thinking that the adverse health effects of pollution are particularly small. As the "treated" properties in our sample tend to be located in higher-pollution areas, we would find a more benign effect of emissions on house prices in the second half of our study period. This finding, however, does not represent a true TRI announcement effect.[8]

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[7] There are .040 and .046 facilities in the first distance ring in the control and in the "treated" group, respectively. In a t-test, the equality of these means is rejected with a significance level of 0.18%. The equality of means of total air releases originating in the first distance ring is rejected with a significance level of 7.1%.

[8] The example given above is not the only source of bias. As the houses that were sold after June 1989 are non-randomly selected, the indicator variable *After* might be correlated with elements of the vector of

The bias described here is commonly referred to as selection on observables. Matching estimators (Heckman, Ichimura and Todd, 1998) allow us to rid the analysis of bias as long as we can assume that

(4) $\ln Price \perp After \,|X,$

where *X* are the observed house and neighborhood characteristics from equation (1). The property given in (4) is known as conditional (mean) independence (Rosenbaum and Rubin, 1985). It states that, given *X*, the prices of homes in the control group are what the prices of homes sold after June 1989 would have been had they been sold in the first half of the study period.

Matching involves the creation of a control group, which allows the estimation of the unobservable counterfactual. Every transaction, which took place after the release of TRI information, is paired with a transaction in the control group that has the same *X*-realization. As *X* is multidimensional, matching would in general be quite difficult. Fortunately, we can rely on Rosenbaum and Rubin's (1983 and 1984) result that conditional independence remains valid even if *X* is replaced by the propensity to sell the house after TRI information becomes public, $X : P(X_i) = \Pr(After = 1|X_i)$.

We thus estimate propensity scores by running probits of the indicator variable *After* on the basic and the extended set of house and neighborhood characteristics,

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house and neighborhood characteristics *X*. For instance, in our sample, the "treated" properties have fewer rooms, and they are built later. If *After* and *X* are correlated, omitted non-linear effects of elements of *X* on prices could be picked up by *After*, again leading to biased estimates.

respectively.[9] Each property sold after June 1989 is then matched with the property in the control group that has the nearest propensity score. If a control is the nearest match for more than one transaction, it is used several times. The mean differences in propensity scores of matched pairs are one indicator for the quality of the matches. These means (standard deviations) are .0016 (.0984) for the basic and .000023 (.1406) for the extended set of house characteristics.

Table 3 presents the results for the matched data set. As before, we find significant changes in the effect of pollution on house prices before and after the release of TRI information. The marginal impact of pollution on prices is estimated to be less negative in the second period of our study, though the change from the first period is somewhat smaller in the matched sample. We re-estimated the models in Table 3 shortening the window to six and four quarters of data (Table 5 in the Appendix). The point estimates are again of similar magnitude.

Matching methods pose a basic trade-off between the quality of the match and the common support of the treatment and the control group for which the treatment effect can be identified. We re-estimated the models in Table 3, replacing the nearest-neighbor matching method with caliper matching (Cochran and Rubin, 1973). The latter leaves an observation unmatched if none of the scores in the control group are within $\delta$ of the score of a "treated" observation. That is, caliper matching improves the quality of the matches at the cost of further reducing the sample. We chose values for $\delta$ of .00035 and .00008 for the basic set of house characteristics, and values of .0013 and .00030 for the extended

[9] These results are available upon request. We are unable to include all Census Tracts as indicator variables. Thus, we replace the Census Tract fixed effects with Zip Code indicators in this analysis.

set. These values correspond to the 99th and the 95th percentile of the distribution of mean differences in propensity scores obtained by nearest-neighbor matching. The results remain qualitatively very similar to the ones reported in Table 3.[10] The findings of our matching models provide reasonable assurance that the measured impact of the release of environmental information on property values is not driven by selection on observables.

## 2. Repeat-sale analysis

Conditional independence does not guarantee unbiased estimates if the selection of homes that are put on the market is driven by unobservables. We can address this concern for a subsample by re-estimating our model using repeated sales of the same houses. Assuming that unobserved characteristics do not change during the period of two years, the comparison of repeat sales allows us to identify the TRI announcement effect for this group of homes. As is to be expected, our sample size falls quite dramatically, from more than 40,000 observations to 1,549 transactions.[11]

We implement the repeat-sales analysis using property fixed effects (Table 3, Model V). These control for time-invariant characteristics of the homes. The pattern of announcement effects remains as before. Predicted prices in the second period are lower

[10] These results are available upon request.
[11] Of course, houses that turn over very quickly are unlikely to be a random sample (see Table 1). We think of these results as indicative of the impact of TRI on the lower-end of the real estate market.

and emission sources that are close by (<.5 miles) are estimated to have less of an adverse effect on house prices once the TRI becomes public information.

## IV. Discussion

The present study asks if large-scale information initiatives directed at the general public such as the publication of the TRI are powerful enough to change risk perceptions. The answer is affirmative: The predicted effect of pollution on home values in the Philadelphia area did change after the release of TRI data, a finding that is consistent with changes in the underlying risk perceptions. On average, the predicted prices declined indicating that home buyers revised risk perceptions upward. These learning effects are fairly local. Our evidence indicates that risk perceptions remained the same for sources that are very close by, and pollution does not appear to impact property values if the sources are at a distance of more than a mile. The latter finding is consistent with Bui and Mayer who investigate average effects for larger geographic areas (2000).

Note that the estimated changes in prices are not true price changes in the market for real estate. As risk perceptions are unobserved, our estimates for period $t$=0 do not represent the individual willingness to pay for a house with given risk characteristics. Consequently, the predicted changes must not be interpreted as changes in property values that are due to TRI. Our methodology allows us to identify shifts in perceptions, not shifts in prices.

If one is willing to assume that homebuyers' pollution estimates correspond to the true level of toxic emissions once risk information is published – an assumption underlying many hedonic risk and hedonic wage studies – the estimates for period $t$=1 measure the marginal impact of pollution on property values. Our results indicate that this effect is fairly local. For most specifications, $F$-tests cannot reject that $\beta_1' + \beta_6' = 0$ for pollution sources at a distance of more than one-half mile. For sources within a distance of one-quarter mile, a one-standard deviation increase in toxic emissions typically leads to a decline in prices of about 1%.

In the policy debate surrounding information regulation and community-right-to-know laws, it is common to find expectations that the publication of risk information will lead to greater public pressure on firms to reduce pollution levels or move away from densely populated areas (EPA, 2000). However, this will only happen if individuals underestimate pollution levels prior to the release of environmental information. As the predicted decline in prices indicates, home buyers in the Philadelphia area who live in moderate distance to pollution sources appear to have underestimated levels of pollution prior the publication of the TRI. Hence, our results are consistent with case study reports which show that additional information can put pressure on firms to reduce pollution levels.

## V. Conclusions

Information regulation can change the behavior of firms and individuals in a number of ways. This paper asks if the victims of environmental externalities, those who live close to industrial facilities emitting toxic substances, take note of information initiatives. Using the publication of the TRI as an example, we find that some homebuyers did adjust risk perceptions when new environmental information became available. On average, prices declined, indicating that TRI was "bad" news.

Information regulation is now widely seen as a valuable tool for managing industrial risks and environmental health. Proponents of this type of regulation see this approach as a "potent weapon for TRI communities in the effort to force pollution prevention" (Foreman, 1998: 41). While we have evidence that capital markets can induce firms to reduce pollution or change location, the present study suggests that information regulation can also have significant neighborhood effects. As our results indicate, "neighborhood" needs to be taken quite literally in our context as shifts in risk perceptions only occurred for pollution sources at an intermediate distance. Our findings thus leave open the question if large-scale information initiatives are a cost-effective means to improve the public's risk perceptions.

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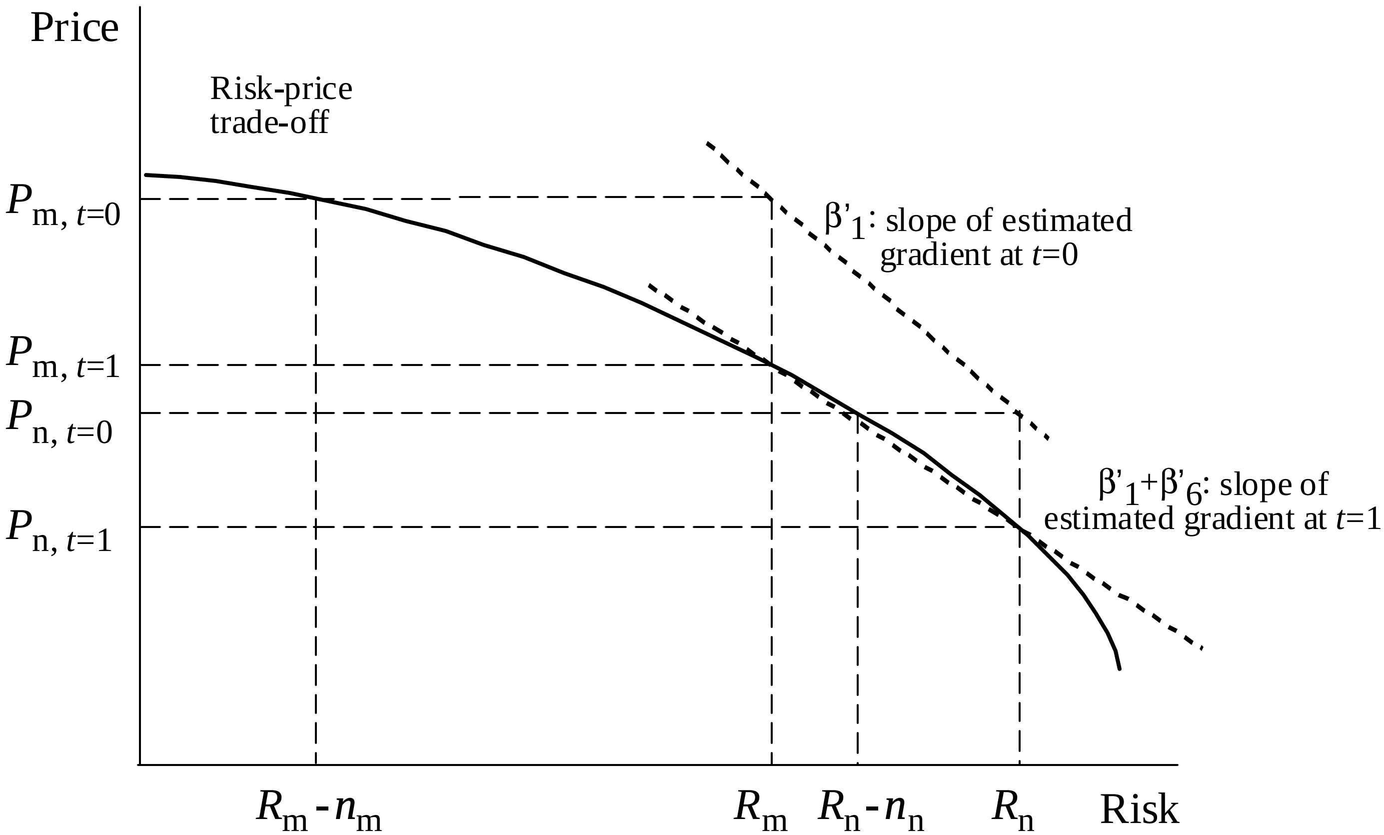


Figure 1 -
Misperceived Risk for Properties *m* and *n*

*Table 1 – Summary Statistics*

| | Full sample | | Matched sample | | Repeat-sale sample | |
|---|---|---|---|---|---|---|
| Variable | number of observations | mean (std dev) | number of observations | mean (std dev) | number of observations | mean (std dev) |
| Air releases in the 1st distance ring (100,000 pounds) | 40863 | 0.020<br>(0.148) | 22744 | 0.020<br>(0.148) | 1549 | 0.020<br>(0.163) |
| Air releases 2nd ring | 40863 | 0.025<br>(0.153) | 22744 | 0.025<br>(0.151) | 1549 | 0.034<br>(0.181) |
| Air releases 3rd ring | 40863 | 0.140<br>(0.996) | 22744 | 0.141<br>(0.997) | 1549 | 0.178<br>(1.174) |
| Air releases 4th ring | 40863 | 0.346<br>(1.730) | 22744 | 0.338<br>(1.705) | 1549 | 0.459<br>(2.063) |
| # of facilities 1st ring | 40863 | 0.043<br>(0.222) | 22744 | 0.045<br>(0.228) | 1549 | 0.055<br>(0.252) |
| # facilities 2nd ring | 40863 | 0.182<br>(0.522) | 22744 | 0.176<br>(0.508) | 1549 | 0.267<br>(0.63) |
| # facilities 3rd ring | 40863 | 0.355<br>(0.772) | 22744 | 0.351<br>(0.762) | 1549 | 0.518<br>(0.914) |
| # facilities 4th ring | 40863 | 0.627<br>(1.151) | 22744 | 0.607<br>(1.135) | 1549 | 0.873<br>(1.32) |
| ln Price | 40863 | 11.386<br>(0.806) | 22744 | 11.441<br>(0.799) | 1549 | 10.923<br>(0.703) |
| Frontage (feet) | 40863 | 57.582<br>(224.476) | 22744 | 64.672<br>(212.557) | 1549 | 24.991<br>(20.658) |
| # of stories | 40863 | 1.909<br>(0.483) | 22744 | 1.887<br>(0.508) | 1549 | 1.999<br>(0.347) |
| # of rooms | 16453 | 7.096<br>(1.480) | 22744 | 7.151<br>(1.509) | 519 | 6.597<br>(1.254) |
| # of full baths | 16776 | 1.665<br>(0.603) | 9668 | 1.663<br>(0.616) | 528 | 1.496<br>(0.541) |
| Year of construction | 16775 | 1960.859<br>(31.421) | 9866 | 1957.788<br>(32.457) | 527 | 1962.345<br>(27.552) |
| After (Indicator: 1= yes) | 40863 | 0.503<br>(0.500) | 22744 | 0.678<br>(0.467) | 1549 | 0.500<br>(0.500) |

*Table 2 – Tests for Changes in the Estimated Effect of Air Emissions on Property Values before and after TRI*

| | (I) | (II) | (III) additional housing info | (IV) additional housing info |
|---|---|---|---|---|
| Air releases in the | 0.083 | 0.084 | 0.072 | 0.073 |
| 1st ring × After | (0.025)** | (0.025)** | (0.029)** | (0.029)** |
| Air releases | 0.018 | 0.018 | 0.035 | 0.035 |
| 2nd ring × After | (0.002)** | (0.002)** | (0.005)** | (0.005)** |
| Air releases | 0.000 | 0.000 | 0.001 | 0.001 |
| 3rd ring × After | (0.000) | (0.000) | (0.001) | (0.001) |
| Air releases | 0.000 | 0.000 | 0.000 | 0.000 |
| 4th ring × After | (0.000) | (0.000) | (0.000) | (0.000) |
| Air releases in the | -0.222 | -0.228 | -0.101 | -0.106 |
| 1st distance ring | (0.025)** | (0.029)** | (0.029)** | (0.037)** |
| Air releases | -0.016 | -0.014 | -0.019 | -0.018 |
| 2nd ring | (0.002)** | (0.003)** | (0.004)** | (0.004)** |
| Air releases | -0.001 | -0.001 | -0.001 | -0.001 |
| 3rd ring | (0.001)* | (0.001) | (0.001) | (0.001) |
| Air releases | 0.000 | 0.000 | 0.000 | 0.000 |
| 4th ring | (0.000) | (0.000) | (0.000) | (0.000) |
| # of facilities | | 0.011 | | 0.005 |
| 1st ring | | (0.019) | | (0.029) |
| # facilities | | -0.032 | | -0.012 |
| 2nd ring | | (0.010)** | | (0.014) |
| # facilities | | -0.018 | | 0.003 |
| 3rd ring | | (0.007)** | | (0.009) |
| # facilities | | 0.007 | | 0.011 |
| 4th ring | | (0.005) | | (0.007) |
| Period after release of | -0.005 | -0.005 | -0.010 | -0.010 |
| information | (0.001)** | (0.001)** | (0.002)** | (0.002)** |
| House Characteristics? | Yes | Yes | Yes | Yes |
| Quarterly Indicators? | Yes | Yes | Yes | Yes |
| School district fixed effects? | Yes | Yes | Yes | Yes |
| Census Tract fixed effects? | Yes | Yes | Yes | Yes |
| Constant | 10.946 | 11.356 | 6.895 | 6.899 |
| | (0.121)** | (0.111)** | (0.336)** | (0.336)** |
| Adj. $R^2$ | 0.53 | 0.57 | 0.53 | 0.53 |
| Observations | 40863 | 40863 | 16453 | 16453 |

** significant at the 1% level * significant at the 5% level

OLS estimates. The dependent variable is the log of sales price. Standard errors are given in parentheses. Each distance ring is a quarter-mile ring around the transacted property. The first ring aggregates toxic air emissions from all TRI facilities located between 0 and .25 miles from the property. The second ring is air emissions between .25 and .5 miles. Releases into the air are measured in 100,000 pounds. House characteristics are the number of stories, frontage size and architectural style (3 indicators). The set of additional house characteristics contains information on the number of rooms, the number of baths and the year of construction.

*Table 3 – Tests for Changes in the Estimated Effect of Air Emissions on Property Values before and after TRI: Nearest-Neighbor-Matched and Repeat-Sale Results*

| | (I) | (II) | (III) additional housing info | (IV) additional housing info | (VII) repeat sales |
|---|---|---|---|---|---|
| Air releases in the | 0.068 | 0.074 | 0.066 | 0.068 | 0.093 |
| $1^{st}$ ring × After | (0.036) | (0.036)* | (0.033)* | (0.033)* | (0.046)* |
| Air releases | 0.020 | 0.019 | 0.033 | 0.033 | 0.012 |
| $2^{nd}$ ring × After | (0.003)** | (0.003)** | (0.005)** | (0.005)** | (0.005)** |
| Air releases | 0.001 | 0.000 | 0.001 | 0.001 | 0.000 |
| $3^{rd}$ ring × After | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) |
| Air releases | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| $4^{th}$ ring × After | (0.000) | (0.000) | (0.001) | (0.001) | (0.000) |
| Air releases in the | -0.183 | -0.131 | -0.138 | -0.134 | |
| $1^{st}$ distance ring | (0.027)** | (0.030)** | (0.026)** | (0.034)** | |
| Air releases | -0.019 | -0.010 | -0.020 | -0.011 | |
| $2^{nd}$ ring | (0.002)** | (0.003)** | (0.004)** | (0.004)** | |
| Air releases | -0.002 | -0.000 | -0.001 | 0.000 | |
| $3^{rd}$ ring | (0.000)** | (0.000) | (0.001) | (0.001) | |
| Air releases | 0.000 | 0.001 | 0.000 | 0.000 | |
| $4^{th}$ ring | (0.000) | (0.000)** | (0.000) | (0.000) | |
| # of facilities | | -0.018 | | 0.034 | |
| $1^{st}$ ring | | (0.016) | | (0.027) | |
| # facilities | | -0.047 | | -0.047 | |
| $2^{nd}$ ring | | (0.007)** | | (0.012)** | |
| # facilities | | -0.070 | | -0.043 | |
| $3^{rd}$ ring | | (0.005)** | | (0.007)** | |
| # facilities | | -0.048 | | -0.004 | |
| $4^{th}$ ring | | (0.004)** | | (0.006) | |
| Period after release | -0.006 | -0.006 | -0.009 | -0.009 | -0.007 |
| of information | (0.002)** | (0.002)** | (0.002)** | (0.002)** | (0.002)** |
| House Characteristics? | Yes | Yes | Yes | Yes | No |
| Quarterly Indicators? | Yes | Yes | Yes | Yes | No |
| School fixed effects? | Yes | Yes | Yes | Yes | No |
| Zip Code fixed effects? | Yes | Yes | Yes | Yes | No |
| Property fixed effects? | No | No | No | No | Yes |
| Constant | 11.501 | 11.532 | 3.611 | 3.546 | |
| | (0.467)** | (0.464)** | (0.410)** | (0.407)** | |
| Adj. $R^2$ | 0.70 | 0.70 | 0.56 | 0.56 | 0.55 |
| Observations | 22744 | 22744 | 11079 | 11079 | 1549 |

** significant at the 1% level * significant at the 5% level

Weighted-least-squares estimates in models (I) through (IV). In these specifications, the control observations are weighted by the number of times they are used in the nearest-neighbor matching process. The dependent variable is the log of sales price. Standard errors are given in parentheses.

# Appendix

*Table 4 – Tests for Changes in the Estimated Effect of Air Emissions on Property Values before and after TRI – Results for Shorter Windows of Time*

| | (I) | (II) | (III) additional housing info | (IV) additional housing info |
|---|---|---|---|---|
| | | Window of 6 quarters | | |
| Air releases in the | 0.081 | 0.083 | 0.068 | 0.070 |
| 1st ring × After | (0.029)** | (0.029)** | (0.033)* | (0.033)* |
| Air releases | 0.021 | 0.021 | 0.040 | 0.040 |
| 2nd ring × After | (0.003)** | (0.003)** | (0.006)** | (0.006)** |
| Air releases | 0.000 | 0.000 | 0.000 | 0.000 |
| 3rd ring × After | (0.000) | (0.000) | (0.001) | (0.001) |
| Air releases | 0.000 | 0.000 | 0.000 | 0.000 |
| 4th ring × After | (0.000) | (0.000) | (0.001) | (0.001) |
| After | -0.008 | -0.007 | -0.011 | -0.011 |
| | (0.001)** | (0.001)** | (0.001)** | (0.001)** |
| Observations | 29632 | 29632 | 11765 | 11765 |
| | | Window of 4 quarters | | |
| Air releases in the | 0.099 | 0.099 | 0.066 | 0.067 |
| 1st ring × After | (0.036)** | (0.036)** | (0.040) | (0.040) |
| Air releases | 0.020 | 0.020 | 0.040 | 0.040 |
| 2nd ring × After | (0.003)** | (0.003)** | (0.007)** | (0.007)** |
| Air releases | 0.000 | 0.000 | 0.001 | 0.001 |
| 3rd ring × After | (0.001) | (0.001) | (0.001) | (0.001) |
| Air releases | 0.000 | 0.000 | 0.000 | 0.000 |
| 4th ring × After | (0.000) | (0.000) | (0.001) | (0.001) |
| After | -0.009 | -0.009 | -0.008 | -0.010 |
| | (0.001)** | (0.001)** | (0.001)** | (0.001)** |
| Observations | 20184 | 20184 | 8106 | 8106 |

** significant at the 1% level * significant at the 5% level
This Table reports announcement effects only. However, the specifications are the same as the ones for models (I) through (IV) in Table 2. Detailed results are available upon request.

*Table 5 – Tests for Changes in the Estimated Effect of Air Emissions on Property Values before and after TRI – Matched Sample Results for Shorter Windows of Time*

| | (I) | (II) | (III) additional housing info | (IV) additional housing info |
|---|---|---|---|---|
| | | Window of 6 quarters | | |
| Air releases in the | 0.095 | 0.099 | 0.077 | 0.079 |
| $1^{st}$ ring × After | (0.041)* | (0.041)* | (0.036)* | (0.036)* |
| Air releases | 0.012 | 0.013 | 0.043 | 0.044 |
| $2^{nd}$ ring × After | (0.004)** | (0.004)** | (0.006)** | (0.006)** |
| Air releases | 0.000 | 0.000 | 0.000 | 0.000 |
| $3^{rd}$ ring × After | (0.001) | (0.001) | (0.001) | (0.001) |
| Air releases | 0.000 | 0.000 | 0.000 | 0.000 |
| $4^{th}$ ring × After | (0.000) | (0.000) | (0.001) | (0.001) |
| | -0.008 | -0.008 | -0.011 | -0.011 |
| After | (0.001)** | (0.001)** | (0.001)** | (0.001)** |
| Observations | 17207 | 17207 | 8265 | 8265 |
| | | Window of 4 quarters | | |
| Air releases in the | 0.061 | 0.068 | 0.065 | 0.080 |
| $1^{st}$ ring × After | (0.057) | (0.056) | (0.043) | (0.043) |
| Air releases | 0.013 | 0.012 | 0.039 | 0.038 |
| $2^{nd}$ ring × After | (0.005)** | (0.005)** | (0.007)** | (0.007)** |
| Air releases | 0.001 | 0.001 | 0.001 | 0.001 |
| $3^{rd}$ ring × After | (0.001) | (0.001) | (0.001) | (0.001) |
| Air releases | 0.001 | 0.001 | -0.001 | -0.001 |
| $4^{th}$ ring × After | (0.000) | (0.000) | (0.001) | (0.001) |
| | -0.009 | -0.009 | -0.010 | -0.010 |
| After | (0.001)** | (0.001)** | (0.001)** | (0.001)** |
| Observations | 12307 | 12307 | 6038 | 6038 |

** significant at the 1% level * significant at the 5% level
This Table reports announcement effects only. However, the specifications are the same as the ones for models (I) through (IV) in Table 3. Detailed results are available upon request.