The True Cost of Air Pollution: Evidence from the Housing Market

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Abstract

In this paper, I present evidence that current economics research significantly underestimates the effects of air pollution. This bias exists because a polluter's effect on nearby residents changes dramatically with the direction of the wind, and most popular methods, including geographic diff-in-diffs and monitor-based interpolations, are unable to account for such sharp changes in exposure over short distances. To solve this problem, I use an atmospheric dispersion model, which explicitly accounts for meteorological conditions, to determine the effect of every polluting firm on every house in metropolitan Los Angeles. I then estimate the effect of NO_x emissions on house prices using the exogenous variation in emissions caused by the California Electricity Crisis of 2000 and a cap-and-trade program in greater Los Angeles. The estimated price response is much larger than past estimates and implies that the net social value of the cap-and-trade program is roughly $502 million per year. However, when based on conventional measures of pollution exposure, this estimated valuation is small and statistically indistinguishable from zero.

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House price capitalization is routinely used to measure the social value of local amenities which lack an explicit market. But the case of air pollution presents a puzzle—house prices do not seem to respond very much to air pollution. Smith and Huang (1995) note that improved air quality leads to much smaller increases in house prices than would be expected given the accompanying health benefits. More recent studies, including those using quasi-experimental research designs, have not resolved this puzzle.[1] Deepening the confusion is a large literature that finds house prices to be very responsive to myriad other locational amenities, including school quality (Black 1999; Cellini, Ferreira, and Rothstein 2010); crime risk (Linden and Rockoff 2008; Pope 2008); hurricane risk (Hallstrom and Smith 2005); and local cancer risk of unknown origin (Davis 2004). This raises the question: What is different about air pollution, a disamenity whose negative value has been well established in other contexts?[2]

In this paper, I argue that this puzzle exists because the measures of pollution exposure used by economists lead to biased estimates of pollution's effects, even with a clean instrument or natural experiment. Estimates based on these measures are biased because pollution concentrations can change dramatically over short distances and these measures cannot account for such changes. Pollution levels spike around pollution sources like firms and highways, particularly when moving from upwind to downwind of a source. The most common measures of pollution exposure, such as geographic difference-in-differences and monitor-based interpolations, are too coarse to capture such sharp changes over short distances. For the geographic diff-in-diff, this leads to contaminated treatment and control groups. For monitor interpolations, it leads to severe measurement error which is correlated with most common instruments.

I solve this problem by measuring local exposure to air pollution with an atmospheric dispersion model which can account for sharp changes in exposure across space. The model, AERMOD, was developed by the atmospheric scientists with the

1. For example, Chay and Greenstone (2005) report a marginal willingness to pay to reduce pollution in line with Smith and Huang (1995). See Section 1 below for further discussion.

2. Neidell (2009) and Moretti and Neidell (2011) find that attendance at outdoor attractions drops precipitously in response to pollution alerts. Qin and Zhu (2015) find that interest in emigration in Chinese cities spikes on high pollution days.

American Meteorological Society and the U.S. Environmental Protection Agency (EPA) and uses extensive data on meteorological conditions and pollution sources to describe how a pollutant is dispersed around its source. Because it explicitly accounts for individual pollution sources, AERMOD captures the sharp changes in pollution exposure that happen around each source. With comprehensive data on houses and administrative data on the emissions of every polluting firm in greater Los Angeles, I am able to map the impact of every firm on the air quality of every house in the region.

I use this atmospheric science–based measure of exposure in a quasi-experimental hedonic framework to answer three questions. First, I estimate the effect on house prices of a large decrease in exposure to industrial NO_x emissions and the associated marginal willingness to pay (MWTP) for pollution reduction. Second, what does this imply about the social welfare effect of RECLAIM, a cap-and-trade program in southern California targeting NO_x which forms the basis of my natural experiment. Third, am I able to detect any change in house prices using this same research design with standard methods in the economics literature instead of the atmospheric science–based approach? For my natural experiment I use the California Electricity Crisis of 2000 which precipitated the near collapse of RECLAIM, a then nascent cap-and-trade market for NO_x in southern California, which caused firms in the area to quickly adopt abatement technology, suddenly and permanently lowering their emissions.

I find that house prices are very responsive to air quality, much more so than previous findings would suggest, and that RECLAIM led to substantial welfare gains. The average house in the sample area gained an additional $7,400 in value due to the Crisis. The implied MWTP to reduce exposure to industrial NO_x emissions is $3,200 per unit of reduced exposure, while past estimates have ranged around $200 per unit.[3] Extending this MWTP to all households, not just those in single-family houses, implies that RECLAIM, whose social value has long been contested, generates a net

3. Great care should be taken when comparing these MWTP estimates, which capture the valuation of different pollutants, and the comparison made here is meant only as a heuristic. Section 6.2 includes a more thorough discussion of this point. A far more rigorous way to test for bias in past methodology is to re-estimate MWTP in this sample using those methods, which I do below.

welfare benefit of $464 million *annually*.[4]

However, when using standard methods for measuring pollution exposure, I am unable to detect any effect on house prices from the Crisis. Specifically, I focus on geographic difference-in-differences models with various treatment radii (as in Davis 2004; Currie and Walker 2011; Currie et al. 2015; and others), as well as models assuming uniform radial dispersion (as in Banzhaf and Walsh 2008). I also estimate instrumental variables models, based on the geographic diff-in-diffs, that use interpolations of pollution monitor readings as the endogenous regressor (as in Hanna and Oliva 2015; Schlenker and Walker 2016; and others). None of the estimates are statistically or economically significant, with many being wrongly signed, suggesting that the much larger estimates found with AERMOD are due to methodology and not the sample or natural experiment.

1 The Puzzle of Clean Air's Low Valuation

House prices have long been used to measure the marginal willingness to pay (MWTP) for non-market goods. The MWTP for pollution abatement has been measured this way many times, starting with Ridker and Henning (1967).

The current body of literature suggests that house prices do not respond much to pollution, implying a disparity between the MWTP for pollution reductions and the expected health benefits. In their meta-analysis of OLS estimates of MWTP, Smith and Huang (1995) find that the interquartile range of estimates is \$0 to \$233 per $\mu g/m^3$ of Total Suspended Particulates (TSP) and that the mean estimate only covers 6–33% of VSL-based mortality cost. More recent instrumental variables estimates have not narrowed this disparity. Chay and Greenstone (2005) use the implementation of the National Ambient Air Quality Standards (NAAQS), county-level house prices, and average county pollution monitor readings to estimate a MWTP of \$191 for a 1 $\mu g/m^3$ reduction in TSP, well within Smith and Huang's interquartile range.[5] Bayer, Keohane, and Timmins (2009) also use county-level

4. Details of this calculation, including social costs considered, are given in Section 7. For discussion of past controversy surrounding the value of RECLAIM, see Fowlie, Holland, and Mansur (2012).

5. This estimate is based on the preferred specification in Chay and Greenstone (2005), Table 5A,

data on house prices and pollution. Instrumenting for local pollution with pollution from distant sources, they estimate a MWTP of $131 per μg/m^3 reduction of PM_{10}.[6]

Such a muted price response to air pollution is somewhat unusual since prices readily respond to other location-specific amenities. Cellini, Ferreira, and Rothstein (2010) use house price responses to bond override elections and estimate the average household is willing to spend $1.50 for a $1 increase in school capital expenditures. Linden and Rockoff (2008) find that when a registered sex offender moves into a neighborhood, the value of nearby houses drops by about $7,000, more than the FBI's estimates of victimization costs would suggest. Davis (2004) looks at how prices respond to the appearance of a cancer cluster in Churchill County, Nevada, where the rate of pediatric leukemia suddenly skyrocketed for unknown reasons. The price response there implies the welfare cost of pediatric leukemia is about $7 million, in line with estimates of the value of a statistical life from Aldy and Viscusi (2008).

Given the proclivity of house prices to capitalize the value of nearby amenities, the muted price response to air pollution is made even more puzzling by households' strong revealed preferences for clean air in contexts other than house prices. Neidell (2009) and Moretti and Neidell (2011) find that attendance at outdoor attractions like sporting events and the zoo drops precipitously in response to smog alerts. Qin and Zhu (2015) find that Internet searches for "emigration" spike in Chinese cities on high pollution days.

This contradictory body of evidence suggests that something specific to air pollution is affecting either the reponse of house prices to air pollution or the measurement of that response.

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column 4, deflated to 2014 dollars using the all-items CPI. Unless otherwise stated, all dollar values in the paper are deflated in the same way.

6. This estimate is based on Bayer, Keohane, and Timmins (2009), Table 6, column 2 and assumes costless migration, which is standard in the literature. They also fit a structural model that allows for costly migration, which yields a MWTP estimate of $352.

2 Econometric Problems Behind the Puzzle

As an economic variable, exposure to air pollution is unusual because unlike wages or education there are no large-sample data on individual-level pollution exposure. Instead researchers approximate or infer exposure levels in some way, and two approaches are predominantly used in the economics literature (see Currie et al. 2014). The first and most straightforward approach is to use a geographic difference-in-differences design where people close to a pollution source are assumed to be exposed to the source while those slightly farther away are assumed not to be exposed. The second approach is to use data from the EPA's network of pollution monitors as a proxy for person-, neighborhood-, or county-level exposure, usually by interpolating between monitors.

Unfortunately, both of these methods suffer from the same problem: They are unable to capture sharp changes in pollution across short distances, which biases estimates based on these methods even within a quasi-experimental research design.

2.1 Bias in Geographic Diff-in-diff Estimates

In a geographic difference-in-differences design, people around a polluting firm or other pollution source are assigned to treatment and control groups based on their proximity to the firm. The econometrician chooses radius r_0 around the firm to define the treatment group and radius $r_1 > r_0$ to define the control. With well-defined treatment and control groups, the problem is now a standard diff-in-diff. The difference over time is centered around some shock to the firm's pollution output, such as a policy change or other exogenous shock (e.g., Currie and Walker 2011; Hanna and Oliva 2015; Schlenker and Walker 2016). For practical reasons, geographic diff-in-diffs are often centered around more routine changes in firm behavior, such as the construction and opening of the firm itself (e.g., Davis 2011; Currie et al. 2015). A key advantage of the geographic diff-in-diff design is that it allows for the estimation of reduced-form effects of policy changes when the exposure data necessary for second-stage estimates is lacking, as is often the case (e.g., Currie and Walker 2011; Davis 2011; Currie et al. 2015).

Unfortunately, the use of a geographic diff-in-diff to study air pollution is problematic because air pollution does not disperse from its source uniformly in all directions, nor is it confined to the neighborhood immediately around the firm. Pollution is blown in the direction of the wind and significant amounts can travel dozens of miles downwind. This wind-driven dispersion contaminates the geographic diff-in-diff's circular treatment and control groups, with many "treated" individuals upwind having little to no treatment and many "control" individuals downwind being intensely treated.

To derive the resulting bias, consider a model where the true effect of a polluting firm f on outcome y_{it} is

$$y_{it} = N_{it}\alpha + X_{it}\beta + \varepsilon_{it} \tag{1}$$

where X_{it} is pollution exposure to i at time t, N_{it} is exposure to non-pollution disamenities created by the firm, and $t \in \{0, 1\}$ indexes time, with $t = 0$ preceding some shock to the firm's emission rate and $t = 1$ following the shock. Let r_{if} be the distance of i from f and assume that r_0 is chosen so that $r_{fi} > r_0$ implies $N_{it} = 0$. The reduced-form geographic diff-in-diff estimation equation is

$$y_{it} = \gamma_1 + \text{post}_t \gamma_2 + C_i \gamma_3 + \left(C_i \times \text{post}_t\right) \gamma_{\text{GD}} + \mu_{it} \tag{2}$$

where $C_i = \mathbf{1}\{r_{if} \leq r_0\}$ is a dummy variable for individuals living in the treatment area and $\text{post}_t = \mathbf{1}\{t = 1\}$. If $X_{it} = 0$ for $r_{if} > r_0$, then $\hat{\gamma}_{\text{GD}}$ recovers the average effect of the pollution shock on people living in the treatment area.

However, this key assumption—the control group is not exposed to pollution—is obviously violated if pollution is carried far downwind. The distribution of pollution around its source depends on meteorology and the source's physical characteristics and can be written $X_{it} = m_{ft} \cdot h(r_{fi}, \theta_{fi}; \boldsymbol{S}_f)$; where m_{ft} is firm f's emissions in time t; and h is the probability density function that a unit of emissions ends up at distance r and heading θ relative to the firm. The vector $\boldsymbol{S}_f$ contains variables about the firm's polluting equipment (e.g., height of the smoke stack) and local meteorological conditions like wind speed and direction. If wind speed is high or the smoke stack is tall, a significant amount of pollution can travel well beyond the 1 or 2 miles generally used for the treatment radius r_0.[7]

7. Currie and Walker (2011) use an r_0 of 2 kilometers (approximately 1.24 miles). Davis (2011)

The resulting bias can be derived directly from the geographic diff-in-diff estimator with a bunch of text to fill in the line:

$$\begin{aligned}\hat{\gamma}_{\mathrm{GD}} =& \mathbb{E}[y_{it} \mid C=1, \mathrm{post}=1] - \mathbb{E}[y_{it} \mid C=1, \mathrm{post}=0] \\ & - \big(\mathbb{E}[y_{it} \mid C=0, \mathrm{post}=1] - \mathbb{E}[y_{it} \mid C=0, \mathrm{post}=0]\big)\end{aligned} \tag{3}$$

We can write the expected value of y_{it} conditional on i's treatment assignment in terms of the average exposure to the treatment group:

$$\mathbb{E}_i\left[y_{it} \mid C\right] = C \cdot \bar{N}_t^C \alpha + \left[C + \varphi(1-C)\right] \bar{X}_t^C \beta \tag{4}$$

where $\bar{X}_t^C = \mathbb{E}_i\left[X_{it} \mid C=1\right]$ and $\varphi = \mathbb{E}_i\left[X_{it} \mid C=0\right] / \mathbb{E}_i\left[X_{it} \mid C=1\right]$ is the ratio of average exposure in the control and treatment groups, so average exposure to the control group is $\varphi \bar{X}_t^C$. Substituting Equation (4) into Equation (3) reduces the estimator to

$$\hat{\gamma}_{\mathrm{GD}} = \underbrace{\left(\bar{N}_1^C - \bar{N}_0^C\right)\alpha}_{\text{Non-pollution Effect}} + \underbrace{(1-\varphi)}_{\text{Wind bias}} \cdot \underbrace{\left(\bar{X}_1^C - \bar{X}_0^C\right)\beta}_{\text{Pollution Effect}} \tag{5}$$

The first term captures the firm's non-pollution effects. As β is the coefficient of interest, the ideal research design would hold N_{it} constant over time, eliminating this term.[8] The second term is the pollution effect, multiplied by the contamination factor $(1-\varphi)$.

Thus, even with an ideal natural experiment that holds non-pollution effects constant over time, the estimated pollution effect is biased because the effect on the control group, which is also treated, cancels out some of the effect on the treatment group. The degree of bias is driven by the wind and other factors in $\boldsymbol{S}_f$ that shape the geographic distribution of pollution h. For example, because φ increases with wind speed, the contamination factor $(1-\varphi)$ and $\hat{\gamma}_{\mathrm{GD}}$ both become more negative

uses values of 1 and 2 miles. Currie et al. (2015) use 0.5 and 1 miles. Hanna and Oliva (2015) use 5 kilometers (approximately 3.1 miles). Figure 1, discussed below, compares actual exposure measured by AERMOD to geographic diff-in-diff circles.

8. This is naturally not the case when the shock to the firm is the construction of the firm itself. In such cases, $\bar{N}_1^C > \bar{N}_0^C = 0$. Note also that as the wind gets stronger and $\varphi \to 1$, $\hat{\gamma}_{\mathrm{GD}} \to \alpha \bar{N}_1^C$ and the geographic diff-in-diff recovers the *non*-pollution effects of the firm, including sorting effects for outcomes not directly impacted by non-toxic disamenities.

as wind speed increases. Furthermore, because h need not be monotonic in distance r, φ need not be less than 1, meaning $\hat{\gamma}_{\text{GD}}$ could have the wrong sign.[9]

It is important to note that the dependence of φ on h (and $\boldsymbol{S}_f$ in particular) implies that the bias will vary by pollution source and location. Because bias increases with wind speed and greater Los Angeles is one of the least windy areas in the United States, the bias found in this study is likely to be a lower bound. Similarly, when pollution is emitted close to the ground, more of it stays close to the source, keeping φ low. This suggests that estimates of the effects of cars (e.g., Currie and Walker 2011) may suffer from less bias. However, even car exhaust gets carried by the wind (Hu et al. 2009), and a low φ does not mitigate any separate biases, such as those introduced by monitor data.

Econometrically, this contamination problem is a common issue in program evaluation (e.g., Miguel and Kremer 2004) and can be fixed by re-scaling by average treatment intensity as happens in the second stage of two-stage least squares. However, this re-scaling requires a good measure of treatment intensity. As Section 2.2 below argues, data from geographically sparse pollution monitors, which are used almost universally to measure exposure, are not well suited for this role.

2.2 Bias from Pollution Monitor Interpolation

When data on a spatially correlated variable like rainfall is unavailable for all locations of interest, it is possible to exploit this spatial correlation and interpolate the missing values. Given data on the outcome of interest $\{y_i\}_{i=1}^N$, corresponding data on the spatial variable, $\{x_i\}_{i=1}^N$, are needed but unavailable. However, it is often the case that data at a small set of monitor locations, $\{x_m\}_{m=1}^M$, are available. If x is correlated across space, so that $\text{cov}(x_i, x_m)$ is large when i and m are close to one another, this monitor data can be used to construct an approximation $\tilde{x}_i = \sum_m w_{im} x_m$, where the weights w_{im} are determined by the interpolation technique used. In general, when economists wish to measure pollution exposure to individuals, monitor interpolation is used almost universally, with inverse distance weighting (IDW) being

9. An example of the non-monotonicity of exposure with distance is given by Figure 1, which is caused by the height of the firm's smoke stack and the high temperature/buoyancy of the emitted gases. This also illustrates the importance of variables in $\boldsymbol{S}_f$ other than wind direction.

the predominant method since Neidell (2004) and Currie and Neidell (2005).[10]

Unfortunately, air pollution is poorly suited for interpolation because the correlation between a monitor reading x_m and exposure x_i can be greatly affected by the presence of pollution sources between m and i. Unlike rainfall and other natural phenomena, air pollution is created by many distinct sources like firms and cars. This creates sharp changes in pollution concentrations over very short distances, such as just upwind of a factory versus downwind. This in turn means the correlation between x_i and x_m depends on much more than just distance.

Consider $\text{cov}(x_i, x_m)$ when a polluting firm sits between i and m. If the wind blows toward one, the other will see a much smaller portion of the firm's pollution, creating significant differences between x_i and x_m both cross-sectionally and over time. Thus, $\text{cov}(x_i, x_m)$ depends on the direction of the wind, the height of the firm's smoke stack, and a host of other information not contained in the monitor data alone, making x_m of little use in deducing the value of x_i unless m is extremely close to i. This single-firm example scales to a world with many firms and monitors. The monitors do not pick up any variation in pollution—spatial or temporal—when the source is not upwind of the monitor, and so interpolating between monitors smooths over most of the local spikes in pollution exposure that exist around firms.

The way independent pollution sources segment the distribution of x across space creates a missing information problem whose severity is proportional to the density of the monitors relative to the density of sources. In the extreme case with many monitors around every firm, there will be few instances where i is separated from all monitors by a firm, and sufficient data will exist to accurately describe the distribution of x. This is the approach taken by atmospheric chemists who temporarily lay down dense monitor arrays (e.g., every 100 meters) to study dispersion patterns around particular source (e.g., Perry et al. 2005). On the other extreme, with only a few monitors and many firms, x_i may be completely unrelated to all monitor readings and values interpolated from the monitors will be no better than noise. Empirically, the situation in the United States is much closer to the latter scenario with relatively few monitors. According to the EPA's AirData summary files, the average county had 1.01 monitors in 2005, with almost two-thirds of counties having zero monitors.

10. IDW uses $w_{im} = d_{im}^{-1} / \sum_{m'} d_{im'}^{-1}$ where d_{im} is the distance between i and m.

In the Los Angeles area specifically, which is one of the most studied areas for air pollution in the United States, there are hundreds of firms for every pollution monitor and monitors are spatially sparse, as shown in Figure A3.

Because the problem with interpolation is that the monitor data are lacking in sufficient information, it is not specific to any particular interpolation technique. This includes Kriging, a more advanced interpolation technique and the best linear predictor of x_i when certain assumptions hold (see Cressie 2015). The strength of Kriging is that it uses the monitor data to explicitly estimate a spatial covariance function for x_i to determine the proper interpolation weights w_{im}. However, as argued above, sparse monitor data are insufficient to characterize the spatial covariance of pollution—without accounting for firm locations, meteorology, etc., it is impossible to account for the local extrema in exposure that occur between monitors.

2.2.1 Evidence of Interpolation Bias in Prior Research

The problems with interpolation described above are evidenced in prior literature.

First, the correlation of interpolated exposure and actual exposure appears to be very low after controlling for secular temporal correlation. When using interpolated pollution, it is common practice to assess the quality of the interpolation through a leave-one-out cross-validation technique. Pollution exposure at each monitor is predicted using all other monitors, $\tilde{x}_m = \sum_{m' \neq m} w_{mm'} x_{m'}$. Then the correlation of the predicted and true values, $\text{corr}(\tilde{x}_m, x_m)$, is used to gauge the quality of the interpolation. These correlations are usually quite high, often above 0.9.[11] However, the reported correlation of x_m and $\tilde{x}_m$ presented in these studies is usually unconditional which includes not just spatial but temporal correlation which will be partialed out in regressions. Consider the extreme case with no spatial correlation, where $x_{it} = \delta_t + \varepsilon_{it}$; δ_t is a time shock common to all locations (such as regular seasonal variation); and ε_{it} is a zero-mean i.i.d. white noise term. In this case, $\text{corr}(\tilde{x}_{mt}, x_{mt})$ will be non-zero and potentially large, while the correlation conditional on t—the more relevant value for analyses with time controls—will be zero.[12] This is consis-

11. Currie and Neidell (2005) report cross-validation correlations of 0.92 for ozone, 0.77 for PM_{10}, and 0.78 for CO.

12. See Appendix A for derivations.

tent with Karlsson and Ziebarth (2016), who find that the correlations for pollution IDW interpolations fall precipitously with time controls, from 0.6–0.9 to 0.15–0.4, while weather variables, which are smoother over space, do not.[13]

Second, the smoothing over of local spikes in pollution around pollution sources should lead to non-classical measurement error in interpolated values, with $\tilde{x}_{it}$ being too low for larger x_{it}. Write $\tilde{x}_{it} = x_{it} + \eta_{it}$ where η is the interpolation error. If the interpolation smooths over variation in x, it will be true that $\text{Var}(\tilde{x}_{it}) < \text{Var}(x_{it})$, which implies $\text{cov}(x_{it}, \eta_{it}) < 0$.[14] Knittel, Miller, and Sanders (2016) plot $\hat{\eta}_{mt}$ and x_{mt} from the cross-validation exercise of their IDW interpolation and find that $\tilde{x}_{mt}$ is indeed increasingly too low for higher values of x_{mt}.[15] They also report that the magnitude and sign of the interpolation error is uncorrelated with the distance to the nearest monitor. Karlsson and Ziebarth (2016) complete a similar exercise for temperature, which is smoother over space than pollution, and find that $\hat{\eta}_{mt}$ and x_{mt} are uncorrelated.

2.2.2 Interpolation bias persists in quasi-experimental designs

While attenuation due to measurement error is often resolved by using instrumental variables, this is only true if the measurement error and the instrument are uncorrelated. Let z be an instrument such that $\text{cov}(x,z) \neq 0$ and $\text{cov}(y,z) = 0$ and let $\eta = \tilde{x} - x$ again be the interpolation error. From the canonical probability limit of the IV estimator, we get

$$\text{plim}\,\hat{\beta}_{\text{IV}} = \beta \cdot \frac{\text{cov}(x,z)}{\text{cov}(x,z) + \text{cov}(\eta,z)} = \beta \cdot \frac{\text{cov}(x,z)}{\text{cov}(\tilde{x},z)} \tag{6}$$

Note that the asymptotic bias could be positive or negative depending on the joint distribution of (x, z, η) which will vary across research designs.

13. See Table F1 in Karlsson and Ziebarth (2016).

14. This follows from $\text{Var}(\tilde{x}_{it}) - \text{Var}(x_{it}) < 0$ and the definition of $\tilde{x}_{it}$. See also Wooldridge (2010, p. 82).

15. Lleras-Muney (2010) also presents evidence of non-classical measurement error in Kriging interpolation, showing that the Kriging standard error increases with x_{mt}. However, she does not report whether the error is increasingly positive or negative, though the mechanical nature of the interpolation would suggest the latter.

First, consider the case of a geographic diff-in-diff, which assigns treatment status to those near pollution sources. As discussed above, if pollution sources significantly outnumber monitors, then the pollution spikes caused by many sources will be smoothed over, causing the measurement error to spike near the source. If the treatment variable is an indicator for "near the source", then the treatment variable is clearly correlated with the measurement error and $\hat{\beta}_{\text{IV}}$ is inconsistent. However, the signs of the covariances in Equation (6), which determine the sign of the asymptotic bias, depend heavily on the joint distribution of (x, z, η) which will vary from case to case with the number and location of monitors, the spatial distribution of the study population, and other factors.

Next, consider county-level studies using the Clean Air Act (CAA) as a natural experiment. In these studies, $\tilde{x}_{it}$ is generally the average of a county's monitors and is assumed to represent average exposure in the county. The CAA established limits (National Ambient Air Quality Standards or NAAQS) on county-level pollution as measured by the county's average monitor readings, making the regulatory metric identical to $\tilde{x}_{it}$. If a county's $\tilde{x}_{it}$ exceeds the NAAQS it is in "non-attainment" and local regulators are given additional authority to limit local emissions to lower $\tilde{x}_{it}$. Thus, the onset of the NAAQS resulted in exogenous changes in local pollution as non-attainment counties suddenly faced additional regulatory pressure while the remainder did not.[16]

Such a research design is likely biased downward because the instrument is more closely related to $\tilde{x}$ than x because regulators specifically target $\tilde{x}$ rather than x. Monitors are not sited within a county to form a representative sample of population exposure, and there is evidence that local regulators strategically site monitors to reduce the likelihood of their county violating the NAAQS (Grainger, Schreiber, and Chang 2016).[17] In addition, Auffhammer, Bento, and Lowe (2009) find that regulators put more effort into reducing pollution levels at problematic monitors

16. See, e.g., Chay, Dobkin, and Greenstone (2003) and Chay and Greenstone (2003, 2005).

17. This disconnect between monitor readings and population exposure is a problem more general than the one described here about the CAA-based instrument. When the monitor is initially placed, its readings may represent the average of the population's exposure distribution, or they may represent any other order statistic. As the location and behavior of individuals and polluting firms changes over time, the monitor remains fixed in space and its relationship to the population exposure distribution will change, making monitor readings over time a poor proxy for population exposure over time.

within a county, resulting in uneven treatment across monitors and the county. This means that the CAA policy shock affects $\tilde{x}$ more than x which, as Equation (6) shows, leads to downward biased estimates.

3 Measuring Exposure with a Dispersion Model

Atmospheric dispersion models solve the problems described above by explicitly accounting for the sudden changes in pollution exposure around firms and the way pollution is distributed by meteorological forces.

A dispersion model uses data on a polluting firm and the meteorology around it to predict the impact of the firm's pollution on air quality at nearby locations. Recall from Section 2.1 that exposure at location i to firm f's pollution can be written $x_{ift} = m_{ft} \cdot h(r_{fi}, \theta_{fi}; \boldsymbol{S}_f)$, where h is a probability density function for over locations (r, θ) for pollution emitted by f. This distribution over space depends on $\boldsymbol{S}_f$, the firm's characteristics (e.g., stack height) and surrounding meteorology. An atmospheric dispersion model is a model of h developed by atmospheric chemists and validated with controlled experiments.[18] With knowledge of h and data on m_{ft} and $\boldsymbol{S}_f$, x_{ift} can be calculated for any arbitrary location (r_{if}, θ_{if}) as can total exposure, $x_{it} = \sum_f x_{ift}$. Most importantly, by explicitly accounting for the local distribution of pollution around every firm, exposure based on a dispersion model does not suffer interpolation's missing information problem.

In this paper, I use AERMOD, the EPA's legally preferred model for short-range applications. This preference is based on the model's high accuracy as established by peer-reviewed field tests (Perry et al. 2005).[19] To account for meteorological conditions, AERMOD uses hourly data on temperature, wind speed, and wind direction at multiple elevations; the standard deviation of vertical wind speed; the convectively and mechanically driven mixing heights; and other parameters.[20]

18. Validation experiments are conducted by placing a dense network of several dozen monitors around a firm, releasing a rare, non-reactive tracer chemical, then comparing model predictions to monitor readings. For example, see Perry et al. (2005).

19. Regulatory preference is stated in 40 CFR pt. 51, app. W (2004). See Cimorelli et al. (2005) for a rigorous development of the model itself.

20. A full list of the variables used is found in the AERMOD user manual or Cimorelli et al. (2005).

AERMOD also accounts for each smoke stack's height and diameter, the temperature and velocity of the gas exiting the stack, and the rate at which the pollutant in question is emitted from the stack (mass per unit time). Given this data, the model outputs the concentration of pollution at a location in micrograms per cubic meter of air, $\mu g/m^3$.

Calculating location-specific exposure using AERMOD and plotting it for the analysis sample in metro Los Angeles makes the problems described in Section 2 more apparent.[21]

Figure 1 shows that ignoring the complex distribution of pollution around a firm causes geographic diff-in-diffs to have contaminated control samples and to miss the exposure effects for large portions of the popluation. It shows the average exposure to NO_x emitted by the Scatterwood Generation Station in Los Angeles in 1999, with circles drawn at 1 mile and 2 miles to represent the geographic diff-in-diff treatment and control radii described in Section 2.1. Pollution exposure is significantly higher to the northeast, the direction of prevailing winds, with high concentrations at 5 and even 10 miles downwind, well beyond the 2-mile control restriction. Furthermore, the area with the lowest exposure in the 2-mile sample area is actually in the "treatment" area, right next to the firm.[22]

Figure 2 shows how quickly pollution levels can change over short distances, undermining the usefulness of monitor data. It plots exposure to NO_x from all major firms across the sample area in metro Los Angeles, as well as the locations of pollution monitors. This map shows a great deal of variation in pollution, with far more spikes in local exposure than monitors available to measure them.

Figure 3 further highlights the over-smoothing problem that results from interpolation by taking the exposure values in Figure 2 and interpolating between the marked monitor locations. Panel A uses inverse distance weighting (IDW) and Panel B uses the more advanced Kriging procedure.[23] In both cases, almost all of the

21. These data beyond these plots and exactly how I implement the AERMOD model described in Section 5.

22. This is because hot gases are bouyant and can travel considerable horizontal distance before reaching the ground, especially when released from a tall smoke stack.

23. The inverse distance weighting used here imposes zero weight on monitors farther than 15 km from the point being interpolated. Such a restriction is common in the literature to prevent interpolated values based exclusively on far away monitors (see, e.g., Hanna and Oliva 2015). The Kriging procedure used here is simple Kriging with an exponential variogram.

spatial heterogeneity is gone, and areas that differ by an order of magnitude are assumed to have the same exposure.

Together, these figures help explain the difficulties in measuring pollution exposure and help explain some contradictory results in the current literature regarding the importance of wind. Of the economics papers to address the question of wind and industrial pollution, only one, Hanna and Oliva (2015), finds that wind significantly alters their estimates, and then only in certain specifications.[24] This is likely due to the complexity of the atmospheric dispersion problem, which depends on a number of factors besides the wind and affects both the direction pollution travels as well as the distance.

Despite its rigorous evaluations in the atmospheric chemistry literature, I also validate AERMOD's predictions against contemporaneous monitor readings in Figure 4. Panel A plots the AERMOD-predicted exposure to NO_x over time at the northern monitor in the sample area shown in Figure 2 as well as the actual monitor readings from that monitor. Panel B plots the same for the southern monitor. The plotted values are averages from the fourth quarter of each year because the AERMOD and monitor readings are most comparible at this time due to the decreased number of atmospheric chemical reactions during this time of year; these reactions are discussed in more detail below.[25] Figure 4 shows a strong similarity in AERMOD and monitor

24. Hanna and Oliva (2015) look at how labor supply in Mexico City responded to a drop in pollution after the closure of a large refinery. They include the local elevation and a linear measure of degrees downwind in some specifications. Davis (2011) estimates the effect of plant openings on nearby house values and includes dummy variables for "upwind" and "downwind" in a robustness check. Contrary to expectations, he finds that houses upwind of plants have slightly lower prices. Schlenker and Walker (2016) measure the change in daily hospital visits due to changes in airport traffic and incorporate wind speed and direction into one of their models, with no substantive difference in results. Luechinger (2014) compares county-level infant health before and after the mandated desulfurization of power plants in Germany. He calls a county "downwind" of the power plant if it falls in the same 30-degree arc as the prevailing wind direction and includes downwind dummies in all his specifications.

25. It should also be noted that each variable is measured in different units. Because firm-level monitoring trakes mass of NO_x emitted (total grams of NO and NO_2) AERMOD measures loca exposure in units of mass per volume of air (μg/m^3). In contrast, monitors measure the number of NO and NO_2 molecules relative to other molecules in the air (parts per million). It is generally possible to convert between these two units using the ideal gas law. However, RECLAIM's monitoring systems do not differentiate between NO and NO_2 and the relative ratio of these chemicals is crucial to converting between μg/m^3 and ppm due to their different molecular masses. Given this limitation of the data, and the fact that the NO/NO_2 mix varies both across firms and across time within firms, it is

patterns over time. While there is some variance between the AERMOD and the monitors, most likely due to other sources of NO_x like cars, atmospheric chemistry, or limitations of the meteorological data discussed in Section 5.4, the figure is strong evidence that AERMOD is accurately measuring pollution exposure.

A final caveat about this measure of exposure is that it does not account for chemical transformations of the emitted NO_x. Pollutants often react with other chemicals in the atmosphere after being emitted. In particular, NO_x can combine with free oxygen to form ozone which is not emitted directly by polluters and is only present at ground level as a product of NO_x-based reactions. Though AERMOD and other models are capable of modeling this chemical process, it requires high-quality data on pre-existing levels of many other pollutants.[26] Because of the lack of such data, I am unable to confidently model the NO_x–ozone process. This means AERMOD predicts “exposure to NO_x emissions”, which potentially includes ozone, rather than “exposure to NO_x.” While this makes interpreting AERMOD more difficult from a biochemical point of view, this actually makes it a more comprehensive and policy-relevant metric, as NO_x emissions are what is regulated at the firm level.

4 Theory and Research Design

4.1 House Prices and Willingness to Pay

I use hedonic valuation to test whether households value clean air. When choosing a place to live, households weigh a location’s amenities against the bundled price of those amenities, $P(\boldsymbol{g})$, where $\boldsymbol{g}$ is a vector of amenities. Rosen (1974) noted that utility-maximizing agents will choose a bundle of amenities and prices $(P(\boldsymbol{g}^*), \boldsymbol{g}^*)$ so that their marginal willingness to pay for each $g_k \in \boldsymbol{g}$ is equal to the corresponding marginal price, P_{g_k}.[27] Estimating average MWTP, which is difficult to do directly,

best to compare the AERMOD predictions and monitor readings as is.

26. While UV light is a main part of the NO_x–ozone reactions, they also depend on a class of chemicals called volatile organic compounds, or VOC’s. The rate of NO_x–ozone conversion also depends on the relative ratios of NO, NO_2, and ozone. See Sillman (1999).

27. There are a number of theoretical frameworks that can be used to estimate MWTP. See Palmquist (2005) and Kuminoff, Smith, and Timmins (2013) for summaries of valuation using hedonic pricing

can thus be accomplished by estimating P_{g_k}.

Using capitalization effects to estimate marginal prices and MWTP requires some assumptions. First, in order to identify P_{g_k} using intertemporal variation in house prices, the shape of P, which is endogenously determined in equilibrium, must be constant over the sample period (Kuminoff and Pope 2014). While this assumption is less palatable for longer sample periods and low-frequency data, it is likely to hold when using a short sample period and data frequency. Second, agents choose $(P(\boldsymbol{g}^*), \boldsymbol{g}^*)$ is endogenously chosen by the agent, creating a potentially omitted variables problem (Bartik 1987; Epple 1987). Any attempt to identify P_{g_k} must address this and satisfy the identification assumptions specific to the chosen research design. To address this issue, I use the California Electricity Crisis of 2000 as a natural experiment and outline the necessary assumptions below.

4.2 Electricity Crisis as Natural Experiment

The California Electricity Crisis of 2000 precipitated the near collapse of RECLAIM, a cap-and-trade market for NO_x in southern California, which lead to a dramatic decrease in NO_x emissions. This unanticipated shock to air pollution, which varied across neighborhoods, can be used to identify the causal effects of pollution exposure on house prices.

4.2.1 Electricity Crisis of 2000 and Subsequent Decrease in Pollution

In 1994, the South Coast Air Quality Management District (SCAQMD), which regulates air pollution in Los Angeles, Orange, San Bernardino, and Riverside Counties, instituted a cap-and-trade program for NO_x emissions called RECLAIM.[28] Firms were given an initial allocation of year-specific RTCs for each of the upcoming years. At the end of each year, firms must surrender one current-year RECLAIM Trading Credit (RTC) for every pound of NO_x emitted. Excess RTCs can be sold to other firms but not banked for future years. To ease firms' transition into the program, SCAQMD set the initial number of RTCs to be far higher than total emissions. The

and equilibrium sorting models.

28. For additional details about RECLAIM, see Fowlie, Holland, and Mansur (2012).

aggregate RTC cap was scheduled to decline each year, and it was anticipated that without firm adjustment total emissions would equal or exceed total RTCs around 1999.

However, firms did not adequately plan for the eventual binding of the RTC cap. For total emissions to stay under the RTC cap, some firms would need to lower NO_x emissions, either by installing abatement equipment to remove NO_x from their emitted smoke or by decreasing production. But RTC prices were so low there was little short-run incentive to install abatement equipment. SCAQMD publicly reported in mid-1998 that abatement installations were lagging behind what was necessary to avoid the coming "cross-over point" when emissions would equal or exceed permits. Some firms had even canceled orders for abatement equipment that had been made prior to RECLAIM. Firm managers later reported that they believed other "companies were reducing their emissions or were going to begin installing [abatement equipment], and as a result believed that they would be able to buy credits. . . [and] that long-term RTC prices would continue to stay low or would at least gradually rise to the cross-over point" (EPA 2002, p. 24).

This failure to anticipate increased RTC prices caused the cap-and-trade market to nearly collapse at the onset of the California Electricity Crisis in mid-2000. The heart of the Crisis was that electricity demand theatened to exceed potential supply.[29] To prevent rolling black-outs, many electricity producers significantly increased generation and, as a result, their NO_x emissions. This caused the aggregate RTC cap to finally bind which in turn caused a dramatic spike in RTC prices, from \$2,800 per ton in 1999 to \$62,000 by the end of 2000 (see Appendix Figure A1).

Firms not generating electricity responded by finally installing abatement equipment, ultimately leading to a permanent decrease in the average firm's emissions by almost 40%. This sudden drop is shown by the solid lines in Figure 5 which plots the quarterly and annual average of firm emissions scaled by the firm's own sample maximum. The dashed lines show that emissions for electricity generators also decreased by roughly 50% relative to pre-Crisis levels once the Crisis had subsided.

29. The exact causes of the Crisis, such as the derelguation of wholesale electricity markets and market manipulation by certain actors, remain a source of debate. See Borenstein (2002) and Weare (2003), especially Section 3.

The permanence of these pollution reductions, despite the temporary nature of the Crisis, is due to the permanence of the RECLAIM cap-and-trade market. RECLAIM's aggregate emissions cap was the true driving force behind the pollution reductions. The cap, which firms had failed to anticipate, became permanently binding during the Crisis. Had firms adapted to the future binding of the cap—as each firm believed all other firms were doing—there may not have been any permanent change in pollution due to the Crisis. Instead, the Crisis synchronized and hastened the long-term adaptation to the Crisis that should have already happened.

4.2.2 Using the Crisis to Construct Instrumental Variables

The sudden, permanent drop in emissions that followed the Crisis can be used to construct a set of instruments for local residents' exposure to firms' pollution. When faced with high RTC prices, high-emission firms had a larger incentive to cut emissions so the Crisis should have had a larger effect on houses downwind of these firms. A house's pre-Crisis exposure to emissions can thus be used to gauge its exposure to the effects of the Crisis relative to other houses.

Using aermod_{it}, the AERMOD-predicted exposure to house i in time t, I define pre-Crisis exposure aermod_pre_i as the average exposure across all 8 quarters in 1995 and 1996, the first two years of firm-level emissions data. With aermod_pre_i as a measure of treatment exposure, a variable intensity diff-in-diff instrument can be constructed: $\text{aermod\_pre}_i \times \text{post}_t$ where $\text{post}_t = \mathbf{1}\{t \geq 2001\}$ is an indicator variable for post-Crisis years. The corresponding event study instruments, $\text{aermod\_pre}_i \times \delta_y$ (where δ_y is a dummy variable for year y), capture the differential effects of the Crisis on house i in year y relative to the omitted year. These can be used to test the common trends assumption underlying the diff-in-diff.

The identification assumption behind these instruments is that there are no coincidental changes in house prices or non-industrial pollution exposure that are correlated with the instruments, conditional on the other covariates. For example, the housing bubble might have induced more appreciation in poorer neighborhoods which may be relatively more polluted before the Crisis due to residential sorting. Fortunately, we can explicitly control for time trends in such risk variables, and the build up of the bubble was not a discrete event like the Crisis was, so this assumption can be assessed using the event study. Another potential problem is that the instruments might be correlated with changes in NO_x from cars. This would bias second-stage estimates upward if industrial exposure were correlated with automobile exposure *and* the Crisis also caused a sudden and permanent drop in car usage in the area. The former condition is unlikely given the large area that firms affect, while highways rarely have a significant impact beyond 500 meters (Karner, Eisinger, and Niemeier 2010). Furthermore, traffic data show that no significant change in driving patterns coincided with the Crisis.[30]

4.3 Estimation Strategy

I estimate the marginal price of pollution exposure using the following econometric model:

$$\ln p_{it} = \beta \cdot \text{aermod}_{it} + \alpha_i + \delta_t + \sum_k \gamma_{1k} \cdot w_{ik} \cdot t + \sum_k \gamma_{2k} \cdot w_{ik} \cdot t^2 + \varepsilon_{it} \tag{7}$$

where p_{it} is the price of house i in quarter t; aermod_{it} is exposure to industrial NO_x-based pollution; α_i are house fixed effects; δ_t are time (quarter-year) effects; $(\gamma_{1k}, \gamma_{2k})$ are coefficients of quadratic time trends for local geographies, defined by a 10-km grid, and local economic conditions that might affect house prices (discussed below); and ε_{it} is the usual residual term. I estimate this equation using two-stage least squares (2SLS), with the primary specification using $\text{aermod\_pre}_i \times \text{post}_t$ to instrument for aermod_{it}, as detailed in the previous section.

The additional controls included in Equation (7) account for a number of factors that may confound estimates of β, such as amenities not included in the available data and differential trends across local housing markets. The house fixed effects, α_i, capture of all time-invariant characteristics about the house like square footage, number of bedrooms, proximity to the beach, etc. The time effects, δ_t, account for general trends in the housing market over time, as well as seasonal trends within

30. Unreported regressions show traffic patterns had no significant break from trend through the period of the Crisis. I use data from the California Department of Transportation's Freeway Performance Management System (PeMS) for the Bay Area (region 11), 1999–2005. The Bay Area is used because data for Los Angeles only go back to 2001.

each year (e.g., if houses consistently sell for more during the summer). The local geographic trends allow different parts of the metropolitan area to have different secular trends.[31]

The local trends by economic variables are specifically targeted at concerns related to the housing bubble, which differentially impacted neighborhoods with poor credit. Mian and Sufi (2009) find that zip codes with lower incomes and credit scores were affected more by the expansion of sub-prime credit. If these areas also experienced relatively bigger air quality improvements thanks to the Crisis, the coefficient on aermod_{it} could pick up any increase in house prices due to the expansion of sub-prime credit. To prevent this, I interact the following variables with quadratic time trends: the average loan-to-value ratio for houses sold in the house's census tract in 2000; the average predicted interest rate for mortgages taken out in the house's census tract in 2000; and the median household income in the house's census block group in 2000. The first two variables are averages at the tract level, rather than block group, because they are based on transacted properties in that year, making the smaller block-group sample too noisy. The predicted mortgage interest rate data was calculated by DataQuick using proprietary methods and is included in the house data described in Section 5.1.

I restrict the analysis to the period 1997–2005. RECLAIM's first full year of emissions data collection was 1995, but data from 1995 and 1996 are used to construct aermod_pre_i. Following Fowlie, Holland, and Mansur (2012), I set the last year of my analysis to 2005. This avoids the peak and subsequent collapse of the housing bubble.

I restrict the region of analysis to the southwest part of SCAQMD territory, roughly between Santa Monica and Huntington Beach (see Figure 2), to minimize measurement error due to geography. Most major polluters are in this region and locations farther away from the pollution sources are likely to have less actual

31. Given the large size of the sample region, it would be natural for local trends to be defined by cities, which have economically meaningful boundaries (unlike zip codes) and are generally small but not so small as to be computationally burdensome (unlike tracts and zip codes). Unfortunately, many houses do not have a city listed in the data, and the cities of Los Angeles and Long Beach cover a large portion of the sample region while also having a great deal of within-city heterogeneity. To overcome these issues, I use a 10-km grid which is aligned to preserve as many city boundaries as possible. This grid results in 17 different areas that get their own quadratic time trend.

exposure from the firms and a lower signal-to-noise ratio in aermod$_{it}$. Predicting the pollution distribution is also more complicated farther inland because of the San Gabriel and Santa Ana Mountains, which can act like a dam, collecting pollution blown from the coasts. To avoid these problems, I restrict my sample to houses within 10 kilometers of a major electric firm in Los Angeles or Orange County.[32]

5 Data

5.1 Houses

Housing data come from county registrar and assessors' offices via DataQuick, Inc. The data include any property that has been assessed and most sales in California since 1990. Data for each property includes square footage, lot size, number of bedrooms and bathrooms, the year the property was built. Each sale includes the value of the mortgage and any additional loans taken against the property, as well as interest rates as estimated by DataQuick using proprietary methods. The median income of each house's Census block group is taken from the 2000 Census.

Sales outside normal market transactions are dropped since they may not accurately reflect the market's valuation of the house. Specifically, all transactions must be arms-length, non-distressed sales (i.e., no foreclosure sales or short sales) with a price of at least \$10,000. Extremely high-value sales (the top 0.1%) are dropped. I also drop sales that occur within 90 days of a previous transaction, as many of these are duplications. The sample is also restricted to homes built before 1995 to preclude direct sales from developers to consumers. Table A1 shows summary statistics for houses in the sample, including sale price, property characteristics, number of times sold, etc. House prices are deflated to real 2014 dollars using the all-items CPI.

5.2 Firms

Most of the data come from SCAQMD via public records requests (SCAQMD 2015a). These data include each firm's name, address, SCAQMD-assigned ID number, the

32. I also include in this group the southwestern most firm in the area in order to include the Palos Verdes Peninsula in the regression sample.

mass of NO_x the firm emitted every quarter from 1994 to 2014, and all relevant RTC data, including initial allocation of RTCs, the quantity, price, and vintage of exchanged RTCs. Firms' operating addresses were geocoded to get latitude and longitude to represent the location of the firm's smoke stacks, which are required by AERMOD and other location-based calculations (see Appendix B.1 for more details). Firms' SIC info is taken from Fowlie, Holland, and Mansur (2012). Data on firms' physical characteristics (smoke stack height and diameter, and temperature and velocity of gas exiting the smoke stack), come from the National Emissions Inventory (NEI).[33] Firms were matched to the NEI using SCAQMD ID number, and firm name and address. Full details of the construction of the firm-level data are given in Appendix B. Table A2 gives summary statistics by 4-digit SIC on emissions, smoke stack parameters, electric-generator status, average distance to the nearest meteorological station, and the number of firms in each industry group.

5.3 Meteorology and Pollution Monitor Data

Data on local meteorological conditions come from SCAQMD and were gathered by 27 meteorological stations throughout the region.[34] The data include hourly observations for temperature, mean and standard deviations of wind speed and direction at multiple altitudes, and other variables described in Section 3. Each station provides at least three years of data between 2006 and 2012. While these stations were not in operation at the time of the Crisis, wind patterns at the given locations are very stable over time.

Air pollution monitor data come from the California Air Resources Board (CARB) and includes hourly readings for NO_x and ozone in parts per million (ppm). I aggregate the hourly measures to daily and then monthly averages following Schlenker and Walker (2016). I exclude monitors that did not operate for the entire 1997–2005 sample period. The location of each meteorology and pollution monitor is shown in Figure A3.

33. Regulators often collect these data specifically to run atmospheric dispersion models like AERMOD, but the data collected by SCAQMD could not be made available (SCAQMD 2015b).

34. The data are most easily accessible via the SCAQMD website: http://www.aqmd.gov/home/library/air-quality-data-studies/meteorological-data/data-for-aermod

5.4 AERMOD-based Measure of Exposure

I use AERMOD to construct a measure of a house's exposure from all industrial sources. Software implementing AERMOD is available on the EPA's website.[35]

As discussed in Section 3, house i's exposure to NO_x emissions from firm f at time t can be written $x_{ift} = \text{NOx}_{ft} \cdot h(d_{fi}, \theta_{fi}; \boldsymbol{S}_f)$, where $\boldsymbol{S}_f$ contains information on the firm's smoke stacks and its surrounding meteorology. The data for NOx_{ft} and $\boldsymbol{S}_f$ are described in Sections 5.2 and 5.3. A firm's meteorological data is taken from the nearest meteorology monitor. Given these data and a house's location, AERMOD outputs aermod_{ift}, the house's exposure to the firm's emissions. The house's total exposure to industrial NO_x emissions is simply $\text{aermod}_{it} = \sum_f \text{aermod}_{ift}$.

Because AERMOD loops over all firms, houses, and hours of meteorological data, it is very computationally intensive for such a large sample and so I impose several restrictions on the model to make calculation more feasible.[36] First, I only calculate exposure to houses that are within 20 kilometers of a given firm and set exposure outside this radius to zero. Second, I use one year of meteorological data, 2009, which is also the only year during which all of the meteorological stations described in Section 5.3 were operating. Third, I construct an arbitrary 100-meter grid by rounding each house's UTM coordinates to the nearest 100 meters and calculate the exposure value at the center of each grid square. Houses are then assigned exposure values according to the grid square they occupy.

6 Results

6.1 Event Study of the Crisis's Effects

Figure 6 plots the effects of the Crisis over time on both house prices and pollution exposure to provide a visual test of the common trends assumption and provide credibility to the natural experiment. Specifically, it plots the estimated $\hat{\pi}_y$ coefficients

35. Fortran source code and pre-compiled executables for Windows are available. See http://www.epa.gov/scram001/dispersion_prefrec.htm. I use AERMOD version 13350 compiled using Intel Fortran Compiler 15.0 for Linux and run on the Odyssey cluster supported by the FAS Division of Science, Research Computing Group at Harvard University.

36. Even with these restrictions, the model takes approximately 210 CPU days to run.

from the equation

$$Y_{it} = \sum_{y \neq 2000} \left(\text{aermod\_pre}_i \times \delta_y\right) \pi_y + \alpha_i + \delta_t + \sum_k \gamma_{1k} \cdot w_{ik} \cdot t + \sum_k \gamma_{2k} \cdot w_{ik} \cdot t^2 + \varepsilon_{it}$$

where π_y is the effect of exposure to the Crisis's effects in year y; Y_{it} is either $\ln p_{it}$ (the reduced form) or aermod_{it} (the first stage); and all other variables are the same as in Equation (7). Each π_y captures the effect of the Crisis on house prices or pollution exposure relative to the omitted year, 2000. With a valid natural experiment, we should see no effect before the Crisis ($\hat{\pi}_y \approx 0$ for $y < 2000$) with a sharp change immediately following it ($\hat{\pi}_{2001}$ negative for pollution exposure and positive for house prices).

Figure 6 is strong evidence that the Crisis is a valid natural experiment, with little effect on exposure and prices before the exogenous shock of the Crisis and sharp effects immediately following the shock. The effect on house-level pollution exposure over time (the dashed line) unsurprisingly mimics the behavior of firm emissions shown in Figure 5, with a flat profile before the Crisis, a sudden drop right after the Crisis, and a slight negative trend going forward as firms complete their abatement solutions. The effect on house prices (the solid line) is a mirror image of the exposure effect, showing a flat profile before the Crisis followed by a sudden jump in value of houses with improved air quality. This suggests that the instrument based on aermod_pre_i is indeed capturing the effects of the Crisis-induced reduction in exposure rather than other secular changes. For example, if there were no pollution effect and the instrument was instead picking up the beginning of the housing bubble which increased prices differentially in poorer neighborhoods, the Figure 6 would instead show a smooth, exponential-like increase in prices.

6.2 Instrumental Variables Estimates

Table 1 presents estimates of the causal effects of pollution exposure on house prices. The reduced-form estimates in columns 1 and 2 show that the Crisis-induced pollution reduction significantly increased house prices. Column 1 is the preferred specification based on Equation (7) with house-level fixed effects, year-quarter effects, and trends in local geographic and demographic characteristics. The coefficient of 0.0032 implies that every unit of initial exposure (i.e., treatment intensity in the Crisis) increased the sale price of a home by roughly 0.32%. This coefficient is also precisely estimated, with a t statistic of approximately 4 (p-value less than 0.0001). Multiplying this estimate by the average treatment intensity, 5.172, gives the effect of the Crisis on the average house's value, equal to 1.7% or $7,324 for the average home sold in 2000.

Column 2 is a robustness check for the preferred specification in column 1 which relaxes the house-level fixed effects in favor of block group fixed effects and explicit controls for house quality: interior square feet, lot size, number of bedrooms, and number of bathrooms. While unable to control for all time-invariant house characteristics, this specification allows for the inclusion of properties only sold once during the sample period and it allows estimation of the aermod_pre_i main effect which is otherwise subsumed by the house-level effects. The effect of the Crisis estimate in Column 2, 0.0033, is essentially the same as in column 1, and the coefficient on aermod_pre_i, -0.0029, is negative, confirming that properties initially more exposed to pollution were worth less.[37]

Column 3 presents the first-stage estimate that corresponds to the reduced form presented in column 1. The estimated coefficient of -0.4328 implies that for every unit of exposure to NO_x emissions in 1995–1996 (the basis for aermod_pre_i), roughly 43% of that exposure was removed by the Crisis and RECLAIM. This is consistent with the firm-level behavior shown in Figure 5, which shows a decrease in firm-level emissions of a similar magnitude. This relationship between *firm*-level emissions and *house*-level exposure is non-trivial because it depends on the geographic distributions of firms and houses and the differential behavior of firms. For example, it could be that while the average firm reduced emissions, those firms that did reduce emissions are located far from population centers, while firms close to population did not significantly reduce emissions. This estimate shows that the exposure to actual houses dramatically and significantly changed due to the decrease in firm emissions.

37. The correlation of initial pollution exposure and neighborhood characteristics and how neighborhoods changed demographically in response to the air quality improvement following the Crisis is the focus of Sullivan (2016).

Column 4 presents two-stage least squares (2SLS) estimates which show the causal effect of pollution exposure on house prices is statistically and economically significant. This estimate essentially combines those of columns 1 and 3, with aermod_{it} as the endogenous regressor and $\text{aermod\_pre}_i \times \text{post}_t$ as the excluded instrument. The estimate of -0.0073 is again precisely estimated (t-stat 3.1, p-value 0.002) and implies that an additional unit of exposure to NO_x emissions decreases a house's value by 0.73%. Using the average sale price of homes in 2000, this translates to a MWTP to reduce exposure of $3,272.

While this estimate is roughly 15 times larger than past estimates of MWTP, great care should be taken when comparing these figures because they represent estimates of MWTP to reduce different pollutants. This study estimates MWTP to reduce exposure to NO_x emissions, which primarily take the form of NO_x or ozone, while past studies focus on particulate matter, either total suspended particulates (TSP) as in Smith and Huang (1995) and Chay and Greenstone (2005), or PM_{10} as in Bayer, Keohane, and Timmins (2009). However, the relative toxicity of NO_x emissions and particulate matter suggest that the biological harm of particulate matter is at least that NO_x emissions, if not dramatically greater (see, e.g., Muller and Mendelsohn 2009). In addition, when NO_x emissions take the form of ozone, they are significantly less visible than most particulate matter. These facts suggest that the MWTP here is, if anything, a lower bound for the MWTP to reduce particulate matter. Nevertheless, it is difficult to compare this estimate with past ones, and a better way to test for bias in past methodology is to re-estimate MWTP in this sample using those methods, as I do below in Section 6.3.

Columns 5 and 6 are robustness checks on the preferred 2SLS estimate and show that it is robust to both the choice of instruments and the IV method used. Both columns replace the variable intensity diff-in-diff instrument with the full set of event study instruments used in Figure 6 and Section 6.1, specifically, $\text{aermod\_pre}_i \times \delta_y$ with year effects δ_y. These estimates are both remarkably close to the preferred estimate in column 4, suggesting that neither the choice of instruments nor the choice of IV method is driving the results.

Table 1 also provides evidence that the estimates do not suffer from significant weak-instrument bias. First, the partial F statistics for both sets of instruments are included in their respective columns. Following Stock and Yogo (2002) and Stock, Wright, and Yogo (2002), I use the instruments' partial F statistic in the first stage to assess whether the instruments are weak. The F statistics, assuming spherical errors, for the post and annual instruments are 6,323 and 923, respectively, leaving little worry about a weak instruments problem.[38] The LIML estimates in column 6 provide further evidence against weak instruments because the LIML estimator is median-unbiased and thus more reliable than 2SLS when instruments are weak (Stock, Wright, and Yogo 2002). The similarity of the 2SLS results in column 5 and the LIML results does not raise any concern about weak instrument bias.

The 2SLS estimates are also robust to arbitrary spatial correlation across the error terms, as shown in **??**. This table re-estimates the standard errors from the preferred specification (Table 1, column 4) using the spatial HAC (SHAC) variance-covariance estimator of Conley (1999) and Kelejian and Prucha (2007). I use a triangle kernel with bandwidths from 200 meters to 1600 meters (1 mile) and list the standard error and corresponding p-value for each bandwidth. The p-value at each bandwidth is less than 0.05, suggesting that the estimates are indeed statistically significant. The standard errors also increase with bandwidth at a decreasing rate, further suggesting that the estimates are credibly precise.

These estimates are further corroborated by the results of Sullivan (2016) which estimates the effects of the Crisis on block group–level rents and demographics. The estimated reduced-form effect on rents is 0.0031, very similar to the estimate here for house prices. Additionally, the paper finds evidence of a large sorting response following the Crisis, confirming that the change in prices is due to an actual change in amenities rather than contemporaneous secular trends in the housing market.

38. Following Stock and Yogo (2002) and Stock, Wright, and Yogo (2002), it has become standard practice to measure the strength of excluded instruments using the partial F statistic from the first stage. However, the usual rules of thumb from Stock, Wright, and Yogo assume spherical error terms. The correct test statistic for robust first-stage F stats is an open topic of research (see, e.g., Montiel Olea and Pflueger 2013). Therefore, I follow the approach of Coglianese et al. (2015) and report the non-robust F statistics in Table 1 for comparison against the usual rule of thumb.

6.3 Comparison to Standard Methods

Section 2 above argues that standard methods of measuring pollution impact will be biased due to the wind. I test this argument by re-estimating the effect of the Crisis but without using AERMOD. Specifically, I estimate the Crisis's effect on house prices using the standard geographic diff-in-diff and kernel-based measures based on Banzhaf and Walsh (2008). These estimates do not detect any significant effect on house prices from the Crisis.

6.3.1 Geographic Diff-in-diff and Interpolation

To estimate the geographic diff-in-diff I follow Currie et al. (2015) and use an equation similar to Equation (7) where each house-firm pair is treated separately, effectively pooling the various firm-level diff-in-diffs:

$$\ln p_{ift} = \text{near}_{if} \times \text{post}_t \cdot \beta + \alpha_{if} + \boldsymbol{X}_{it}\boldsymbol{\Gamma} + \varepsilon_{ift} \tag{8}$$

where the entity fixed effects are now house-firm effects instead of house effects; $\boldsymbol{X}_{it}$ includes the same time-trend and demographic controls as Equation (7); and near_{if} is a dummy variable for whether house i is within the set treatment radius of firm f. I estimate this model once with a 1-mile treatment radius and 2-mile control radius and again with 2- and 4-mile radii. The results are presented in Table 2.

The reduced-form estimates in columns 1 and 4 of Table 2, are small, imprecise, and have different signs. For the 1-mile treatment, the average effect of the Crisis on log price is 0.0040, less than one fourth the size of the average effect found using AERMOD of 0.017. This estimate is also highly imprecise, with a standard error of 0.0050. The 2-mile estimate in column 4 implies that "treated" houses *lost* value after Crisis and is also imprecisely estimated.

The derivation of geographic diff-in-diff bias in Section 2 predicts that the first-stage and reduced-form estimates should have the same bias and that, with a good measure of exposure, the second-stage estimate should be unbiased, though potentially noisy. I test this using the firm-specific exposure measure aermod_{ift} as the endogenous regressor. For the 1-mile treatment radius, the biases appear to be roughly equal. The reduced-form effect is 32% of the average reduced-form effect found in Table 1, column 2, while the first stage effect is 22% of its AERMOD-based equivalent. Consequently, the second stage coefficient, -0.0098, is similar to the estimates in Table 1 but very imprecise. For the 2-mile treatment, the reduced-form and first-stage estimates recover only 7% and 1% of the wind-based IV estimates, respectively, and all three estimates are imprecise.

For a more direct comparison with prior literature, I also estimate geographic diff-in-diffs using interpolated NO_x and ozone from pollution monitors and present the results in Table A3. As before, the interpolation is calculated using inverse distance weighting using monitors with full NO_x and ozone coverage during the sample period that are no more than 10 km from the point being interpolated. Estimates are very sensitive to the treatment and control radii and the instruments used and are generally imprecise or have the wrong sign. The second-stage estimate of ozone's effect on prices in sub-table B, column 6 is the only second-stage estimate that has the correct sign and is precisely estimated. However, it is not robust to the choice of instruments, and the estimate using the "annual" instrument in column 7 is imprecise with the wrong sign.

6.3.2 Kernel-based Exposure

The second non–wind-based research design uses radial kernel densities to map firm emissions to local exposure. I use a triangle kernel with 5-km bandwidth and a uniform kernel with 2-km bandwidth as the proxy for the spatial distribution h instead of AERMOD.[39] In both cases, the sample is restricted to houses within 5 km of a firm. This is similar to the approach taken by Banzhaf and Walsh (2008), who use the equivalent of a uniform kernel with a 1 mile (1600 meter) bandwidth. The kernel approach should be an improvement over the geographic diff-in-diff because it can account for neighboring firms' overlapping treatment areas. Once again, the estimation equation is almost identical to Equation (7), except that the exposure measure and instruments are constructed using the relevant kernel density instead of AERMOD. Table 3 presents the results, with the triangle-based regressions in Panel

39. To make the unit-less kernel-based variables comparable to the AERMOD measure, I re-scale them so that their sample average is the same magnitude as the sample average of aermod_{it}.

A and the uniform-based regressions in Panel B.

The kernel-based estimates, shown in Table 3, are also small and imprecise. Column 1 of each sub-table shows the reduced form estimates, which are small and imprecise, with the triangle-based estimate having the wrong sign. Column 2 shows the first stage using aermod_{it} as the endogenous regressor, which is included to be more comparable to my preferred specification and to overcome the fact that the kernel variables have an arbitrary scale. In all cases the excluded instruments are defined using the kernel-based exposure.

These estimates are imprecise and again imply a much smaller average effect than Table 1, with neither effect being more than 10% of the wind-based result. Column 3 shows the first-stage regressions using the kernel-based exposure measure, which are precise but hard to compare to Table 1 because of the scaling issue. Columns 4 and 5 show the 2SLS estimates using aermod_{it} and kernel-defined exposure, respectively, as the endogenous regressors. When using instruments based on the triangle kernel, the estimates have the wrong sign due to the wrong-signed first stage. When using uniform-based instruments, the estimate in column 4 is almost 50% of the preferred AERMOD-based estimates in Table 1, but is imprecise, while the estimate in column 5 is both economically and statistically insignificant.

6.3.3 Summary and Comparison to Prior Research

Table 4 summarizes all the estimates from above along with previously discussed estimates from the literature. The first column lists the model or paper that generated the estimate; the second column lists the estimated effect of the Crisis on average house prices for models from this paper; and the third column lists the estimated MWTP for a 1 $\mu g/m^3$ reduction in pollution. For the models estimated in this paper, the pollutant is NO_x and/or ozone, while for Smith and Huang (1995) and Chay and Greenstone (2005) it is TSP, and for Bayer, Keohane, and Timmins (2009) it is PM_{10}. For this comparison, I do not combine non–wind-based designs with aermod_{it} in any way, as the point of the comparison is to gauge the importance of the wind. Hence, there are no MWTP estimates from the geographic diff-in-diff models because the geographic diff-in-diff has no independent measure of exposure. I also do not include the interpolated regressions from Table A3 because the are based on a slightly different geographic sample.

There are several points of interest in Table 4 that support the predictions made in Section 2 that standard estimates may be biased downward. First and foremost, the AERMOD-based estimates dwarf all other estimates in magnitude and precision. Second, the uniform kernel estimate, though imprecise, is not dissimilar from prior research. Third, the instrumental variables estimates from prior research (Chay and Greenstone 2005; Bayer, Keohane, and Timmins 2009) are not dramatically different from the prior OLS estimates (Smith and Huang 1995)—the OLS estimates fall between the IV estimates. All of these observations are consistent argument in Section 1 that standard methods of measuring exposure are biased, even when quasi-experiments and instrumental variables are used.

7 Welfare Implications and Conclusion

This paper provides evidence that clean air has a much higher value than previously believed. The estimated MWTP, \$3,272 per $\mu g/m^3$ of exposure to NO_x emissions, is an order of magnitude larger than past estimates (see Section 6.3) and also more in line with the expected health benefits (see Section 1). The distinguishing feature of these estimates is that they rely on atmospheric science to determine who is and is not exposed to pollution, while standard estimates do not. When re-estimated using standard, non–wind-based measures, MWTP is small or wrongly signed and statistically insignificant.

Furthermore, the econometric problems behind this difference are not unique to the housing market, raising the concern that other estimates of pollution's effects, like those on infant health, are also biased. This in turn raises the question of whether the MWTP estimated here does indeed cover the estimated health costs since they may be downward biased themselves and is a topic for future work.[40]

The fact that air pollution is far more costly than previously believed has significant policy implications, as air quality regulations are likely to be undervalued.

40. Most estimates of the mortality and morbidity dose response to pollutants are from the epidemiology literature and may suffer from omitted variables bias as well. Thus, it is not immediately clear whether current estimates of direct health effects are too high or too low.

For example, Fowlie, Holland, and Mansur (2012) note that RECLAIM has been frequently criticized as an ineffective policy. But the results here imply that reducing emissions in SCAQMD from 1995 levels to the 2005 RTC cap is worth roughly $502 million annually, far more than the estimated annual abatement costs of $38 million.[41] The EPA's troubled attempts to tighten ozone standards, which met resistance on cost-benefit grounds, are another possible example of policy that is grossly undervalued.[42] Optimal subsidies for renewable energy research and electric vehicle take-up are other potential examples.

However, the evidence of sorting found by Sullivan (2016) suggests that the large aggregate welfare gains disproportionately went to high-income households. This raises the concern that there is steep trade off between equity and efficiency, however large the efficiency gains may be.

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41. There are naturally many general equilibrium costs to consider as well, like those borne by displaced workers (see Walker 2013). SCAQMD asks firms to report how many jobs are lost or gained due to RECLAIM every year. Through 1999, firms reported a total net employment change of -109 workers which they attributed to RECLAIM (SCAQMD 2000). Abatement costs based on SCAQMD calculations (SCAQMD 2000).

42. See, e.g., "Obama Asks EPA to Pull Ozone Rule," *Wall Street Journal*, September 3, 2011; "EPA Sets New Ozone Standard, Disappointing All Sides," *New York Times*, October 1, 2015.

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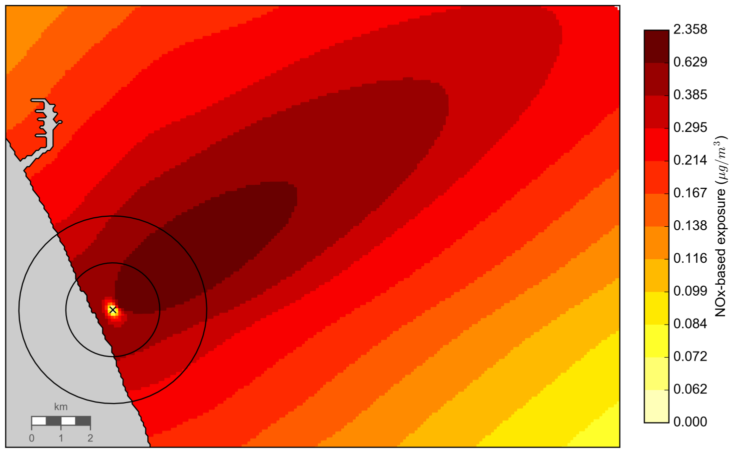

Figure 1: Exposure to NO_x from a Single Firm, 1999

Notes: Colors show average exposure to NO_x emitted by the Scatterwood Generating Stations, Los Angeles, in 1999. Exposure is calculated using AERMOD as described in Section 5.4. Black "X" marks the location of the firm. Circles mark area within 1 and 2 miles from the firm.

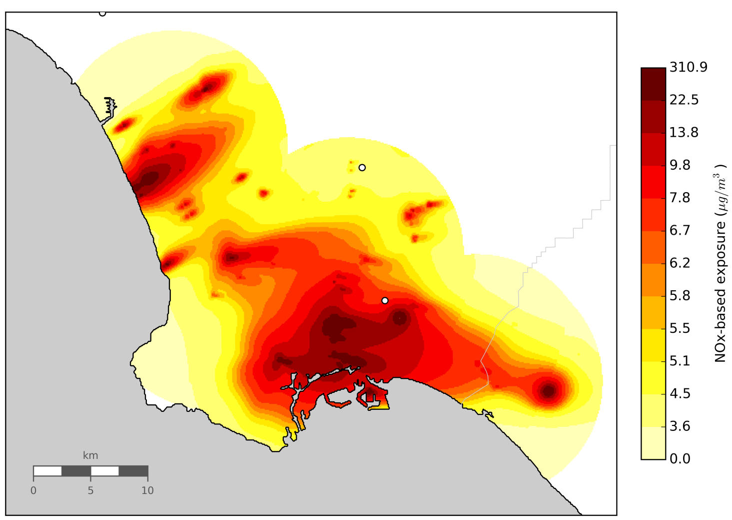

Figure 2: Exposure to Industrial NO_x Emissions, 1999

Notes: Colors show average exposure to NO_x emissions from industrial sources in 1999. White circles mark the location of pollution monitors for NO_x in operation 1997–2005.

(a) Inverse Distance Weighting (b) Kriging

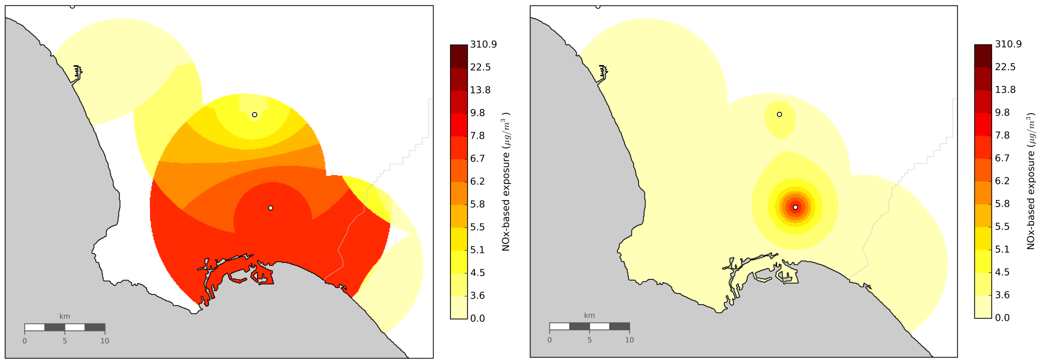

Figure 3: NO_x Exposure as Interpolated from Monitor Locations, 1999

Notes: Figures plot interpolations under the hypothetical that Figure 2 represents true exposure to NO_x emissions but data is only available at monitor locations marked by white dots. These monitors are actual NO_x monitors in operation during sample period (1997–2005) that would be used for interpolation. Color scale for exposure intensity is the same as in Figure 2. Sub-figure (a) plots values interpolated via inverse distance weighting (IDW) with the restriction that monitors are not used (given zero weight) if they are farther than 15 km from the point being interpolated. Sub-figure (b) plots values interpolated via simple Kriging using an exponential variogram.

(a) Northern Monitor (b) Southern Monitor

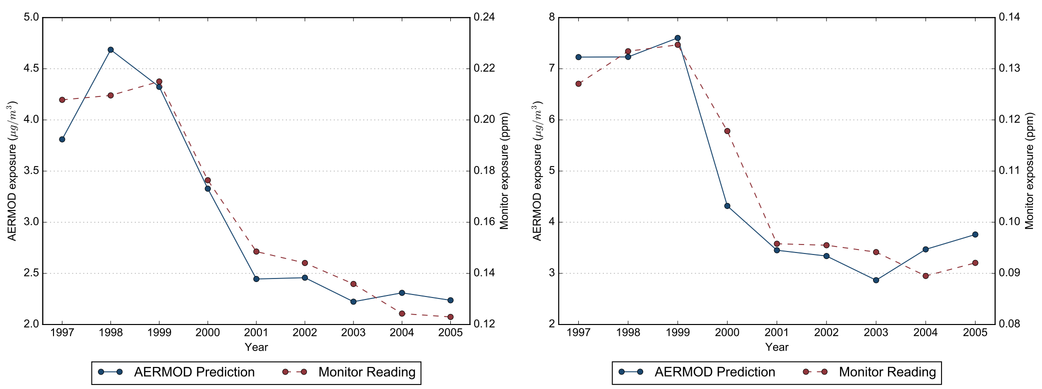

Figure 4: AERMOD and Pollution Monitor Readings Over Time

Notes: Figures plot exposure to NO_x as predicted by AERMOD (solid lines) at the two monitor locations shown in Figure 2, as well as the actual monitor readings for each location (dashed lines). Plotted values are the fourth quarter average for the given year for reasons of atmospheric chemistry, see Section 3.

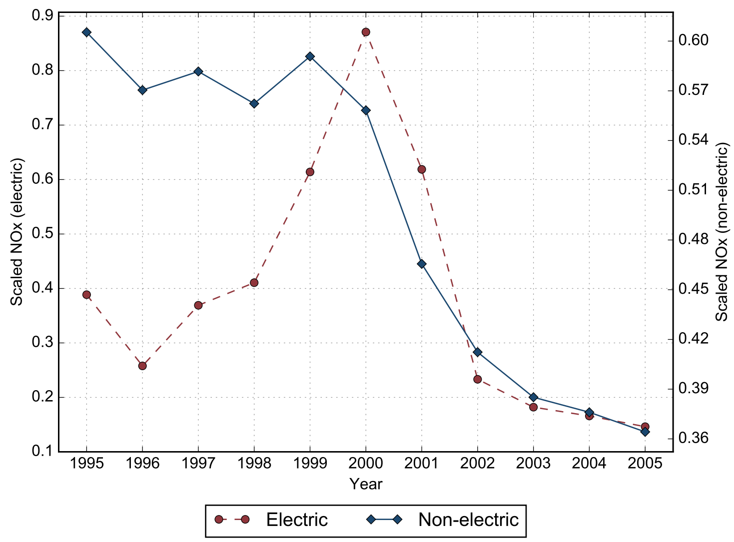

Figure 5: Scaled Firm Emissions of NO_x by Firm Type

Notes: Firm emissions are scaled by firm's own maximum emissions. Sample is restricted to firms that operated in at least 8 quarters.

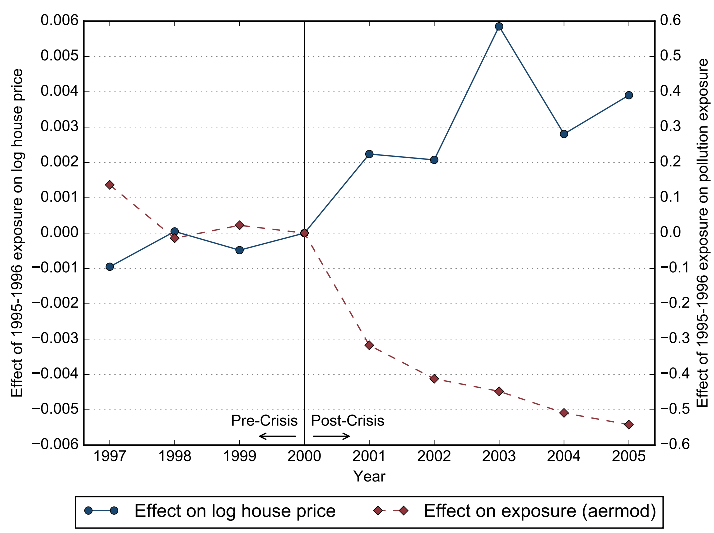

Figure 6: Crisis's Effect on Pollution Exposure and House Prices

Notes: Plotted points are coefficients from a regression of the specified outcome on aermod_pre interacted with year dummies (year 2000 omitted). Sample and other controls as in Table 1, columns 4. Average value of aermod_pre is 5.172.

Table 1: Pollution's effect on House Price, Instrumental Variables

	(1) ln Price	(2) ln Price	(3) Aermod	(4) ln Price	(5) ln Price	(6) ln Price
Aermod				-0.0073***	-0.0073***	-0.0073***
				[0.0024]	[0.0023]	[0.0024]
Aermod_pre×post	0.0032***	0.0033***	-0.4328***			
	[0.0008]	[0.0005]	[0.0748]			
Aermod_pre		-0.0029**				
		[0.0012]				
Fixed Effects	House	BG	House	House	House	House
Method	OLS	OLS	OLS	2SLS	2SLS	LIML
IV set				Post	Annual	Annual
κ				1	1	1.0003
1st Stage F-stat				6388	932	932
R^2	0.948	0.865	0.911			
N	41,771	118,522	41,771	41,771	41,771	41,771

Notes: Controls include listed fixed effects, year-quarter effects and quadratic time trends by local geography and year 2000 SES variables (see Section 4.3). "Post" IV is aermod_pre × post, "Annual" IV is aermod_pre interacted with year dummies. First-stage F stat assumes homoskedasticity. Column 2 also includes controls for lot size, bedrooms, bathrooms, interior square feet. Sample average of aermod_pre is 5.172. Observations absorbed by fixed effects are dropped. Standard errors, clustered at 100-m grid, in brackets: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 2: Price Effects with Geographic Diff-in-diff

	(1)	(2)	(3)	(4)	(5)	(6)
	0–1 vs. 1–2 miles			0–2 vs. 2–4 miles		
	ln Price	Aermod	ln Price	ln Price	Aermod	ln Price
Near×post	0.0040	-0.5155***		-0.0016	0.0201	
	[0.0050]	[0.0578]		[0.0022]	[0.0222]	
Aermod			-0.0077			-0.0817
			[0.0096]			[0.1404]
Method	OLS	OLS	2SLS	OLS	OLS	2SLS
R^2	0.9454	0.9091		0.9417	0.9102	
N	92,901	92,901	92,901	431,634	431,634	431,634

Notes: Unit of observation is house-firm-quarter. Near=1 for houses closer to firm, e.g., 0–x miles as specified. Controls include house-firm effects, year-quarter effects, and local quadratic time trends. Observations absorbed by fixed effects are dropped. Standard errors, clustered by 100-m grid in brackets.

Table 3: Price Effects with Kernel-defined Instruments and Exposure

	(1) ln Price	(2) Triangle	(3) ln Price	(4) ln Price	(5) Uniform	(6) ln Price
Triangle_pre×post	-0.0002	-0.3840***				
	0.0007	0.0112				
Triangle			0.0005			
			0.0019			
Uniform_pre×post				0.0001	-0.4098***	
				0.0003	0.0213	
Uniform						-0.0003
						0.0008
Method	OLS	OLS	2SLS	OLS	OLS	2SLS
R^2	0.948	0.932		0.948	0.906	

Notes: N=41,783. Sample averages of triangle_pre and uniform_pre are 2.301 and 1.678, respectively. Controls include house effects, year-quarter effects, and local quadratic time trends. Observations absorbed by fixed effects are dropped.

Table 4: Comparison of Pollution Estimates Across Models

	Model/Paper	Crisis' Effect on Avg. Price	MWTP
	Standard models		
(1)	Geo DD (1 mile)	\$1,438	
(2)	Geo DD (2 miles)	−\$589	
(3)	Triangle kernel	−\$217	−\$246
(4)	Uniform kernel	\$95	\$138
	Prior Research		
(5)	SH 1995 (3rd q-tile)		\$233**
(6)	SH 1995 (mean)		\$260**
(7)	CG 2005		\$191**
(8)	BKT 2009		\$130***
(9)	BKT 2009 (w/ moving)		\$350**
	Wind-based model		
(10)	Aermod	\$7,324***	\$3,272***

Notes: Each row is taken from a different research design. "Effect of Crisis" is the reduced form effect of the Electricity Crisis calculated at sample averages. For estimates from other papers, the authors' stated preferred estimate is used. Geo DD, Triangle, and Uniform rows use only results specific to those research designs, i.e., no first or second stage using Aermod-based exposure. Sources of estimates as follows. Row (1): Table 2, col 1; (2): Table 2, col 4; (3): Table 3A, cols 1 & 5; (4): Table 3B, cols 1 & 5; (5): Smith and Huang (1995), abstract, meta-analysis; (6): Smith and Huang (1995), abstract, meta-analysis; (7): Chay and Greenstone (2005), Table 5A, col 4; (8): Bayer, Keohane, and Timmins (2009), Table 6, col 2; (9): Bayer, Keohane, and Timmins (2009), Table 6, col 4, accounts for moving costs; (10): Table 1, cols 2 & 4.

Appendix

A Conditional and un-conditional cross-validation correlations

Let $x_{it} = \delta_t + \varepsilon_{it}$, $\text{Var}(\delta_t) = \sigma_\delta^2$, and $\text{Var}(\varepsilon_{it}) = \sigma_\varepsilon^2$ where ε_{it} is mean-zero and i.i.d. Also let $\tilde{x}_{mt} = \sum_{m' \neq m} w_{mm'} x_{m't}$ where the interpolation weights $w_{mm'}$ are constructed such that $\sum_{m'} w_{mm'} = 1$. Then

$$\text{corr}(\tilde{x}_{mt}, x_{mt}) = \frac{\sigma_\delta^2}{\left[\left(\sigma_\delta^2 + \sigma_\varepsilon^2 \sum_{m'} w_{mm'}^2\right)\left(\sigma_\delta^2 + \sigma_\varepsilon^2\right)\right]^{\frac{1}{2}}} > 0$$

Note that $\text{corr}(\tilde{x}_{mt}, x_{mt}) \to 1$ as $\sigma_\varepsilon^2 / \sigma_\delta^2 \to 0$. In a scenario with large within-in year variation, but little cross-year variation in wind patterns or firm behavior, this ratio of variances could be very low.

For the conditional correlation, we have

$$\begin{aligned}
\text{cov}\left(\tilde{x}_{mt}, x_{mt} \mid \delta_t\right) &= \mathbb{E}\left[\left(\tilde{x}_{mt} - \mathbb{E}\left[\tilde{x}_{mt} \mid \delta_t\right]\right)\left(x_{mt} - \mathbb{E}\left[x_{mt} \mid \delta_t\right]\right) \middle| \delta_t\right] \\
&= \mathbb{E}\left[\left(\sum_{m' \neq m} w_{mm'} \varepsilon_{m't}\right) \varepsilon_{mt} \middle| \delta_t\right] = \sum_{m' \neq m} w_{mm'} \mathbb{E}[\varepsilon_{m't} \varepsilon_{mt}] = 0
\end{aligned}$$

B Firm Data Construction

B.1 Geocoding

The accurate geocoding of pollution sources is obviously critical when analyzing the effect these sources have on the surrounding population. Administrative records on the latitude and longitude of each smoke stack operated by the firm would be the ideal data. Regulators often collect this data for the explicit purpose of dispersion modeling, and though SCAQMD does collect this data, they are unavailable for public use (SCAQMD 2015b). In lieu of direct geographic data for each smoke stack, I follow the literature and simply geocode the firms' street addresses, taking care to use the actual operating address of the firm and not a corporate or mailing address which are often listed in databases. For large firms and firms that match to interpolated street addresses instead of parcel centroids, I double-checked the coordinates using satellite photos from Google Maps to make sure the geographic point that represents the firm is reasonably close to the actual smoke stacks.[43]

B.2 Facility ID

SCAQMD assigns each facility an ID number; however, a facility may have more than one ID number in the data, both over time and cross-sectionally. This is primarily a concern when matching firms to the NEI, as described in Appendix B.3. It might also affect the pattern of firm behavior described by Figure 5, though this figure is only descriptive and not used in any calculations.

A facility's ID can change under a number of circumstances: the facility is sold, changes its name, or some part of its address changes. For the most part, these changes occur for superficial reasons, e.g., a zip code or street suffix is changed. To construct unique facility ID's, I flagged every pair of facilities less than 400 meters apart and visually inspected satellite photos and emissions data for every cluster of neighboring facilities. First, firms were merged if they occupied the same or neighboring parcels and shared breaks in their time series of emissions. For example, Facility A emits 25 tons per quarter from 1994 to 1999Q3 and then is missing from the data, while Facility B, located at the same parcel of land as A, enters the data in 1999Q4 and begins emitting 25 tons per quarter. Facilities were also merged if they had similar names and occupied the same or neighboring parcels of land. These merges were verified by checking whether or not the firms appeared separately in the NEI.

43. This is potentially important because the firm's "store-front" address right on the street is often at the edge of the property, far away from the smoke stacks. Using unchecked street addresses can introduce significant errors (1–2 km) for firms that occupy large parcels of land.

B.3 Stack Data from the NEI

Data for each firm's smoke stacks is taken from the National Emissions Inventory (NEI) from 1999 and 2002. Besides the smoke stack parameters, the NEI also has data on firm's name, address, SIC, and the equipment's SSC, and the estimated emissions by pollutant for each stack.[44] It also includes the ID number assigned to the facility by state-level regulators. For SCAQMD firms, this "state ID" consists of a county code, an air basin code, an air district code, and the SCAQMD-assigned facility ID. Using this reconstructed ID, I was able to match most facilities in the SCAQMD emissions data to the NEI using either their own facility ID or an ID from a facility I had previously matched to it as described in section B.2. I used the 2002 NEI data whenever possible, falling back to the 1999 database when necessary. For facilities whose ID's did not match either dataset, I tried to match them using firm address and name. Firms that still did not match were almost all small firms that had ceased to exist before the NEI 1999 data was collected. These firms should have little impact on the overall results and were dropped. For matched facilities, I verified that individual stacks were not duplicated.

Many of the stack parameters in the NEI are flagged as imputed values. The imputation process was not well documented, so I re-imputed them using the median stack parameters from all non-imputed stacks in the SIC and SCC group. Finally, when passing the stack parameters to AERMOD, I weighted each stack according to its reported emissions in the NEI.

44. The Source Classification Codes (SCC) for point pollution sources are a hierarchical index used by the EPA that categorize pollution-generating equipment by combustion type, fuel type, and size. It is analogous to the hierarchical SIC and NAICS industry codes.

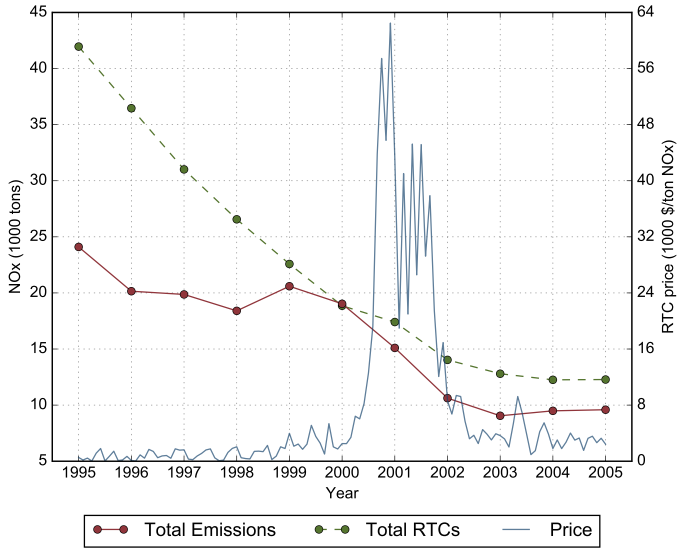

Figure A1: Emissions, Permits, and Permit Price under RECLAIM

Notes: "Total RTCs" is the number of RTCs expiring in the calendar year. "Price" is the average of all arms-length transactions in a month across all RTC vintages.

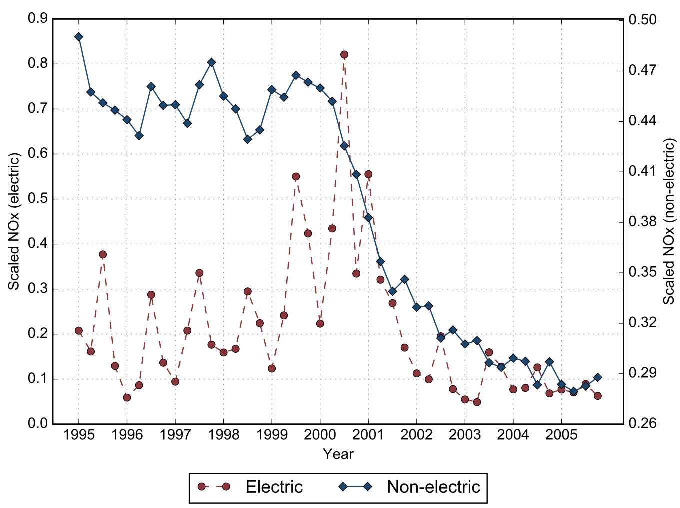

Figure A2: Scaled Firm Emissions of NO_x by Firm Type, Quarterly

Notes: Firm emissions are scaled by firm's own maximum emissions. Sample is restricted to firms that operated in at least 8 quarters.

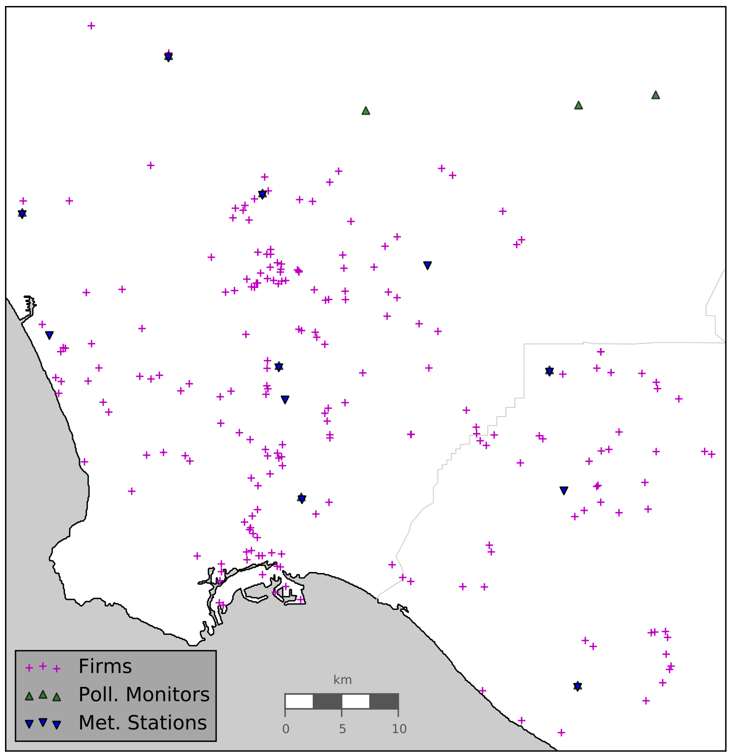

Figure A3: Monitoring Station and Firm Locations

Notes: Firms and meteorology stations are restricted to those that contribute to the main regression sample. Pollution monitors restricted to those with constant NO_x coverage over 1997–2005.

Table A1: House Summary Statistics

	Never Sold	Sold Once		Repeat Sales	
		Pre	Post	Pre	Post
Sale Price		394,621	541,016	420,397	603,089
		(284,495)	(357,224)	(303,028)	(395,970)
Lot Size	19,963	14,831	19,454	19,444	14,650
	(943,394)	(812,098)	(918,742)	(992,280)	(807,084)
Square Feet	1,537	1,611	1,534	1,573	1,491
	(647)	(721)	(689)	(707)	(654)
Year Built	1950	1952	1950	1951	1950
	(15.15)	(15.61)	(15.77)	(16.96)	(16.78)
Bedrooms					
1	0.01	0.01	0.01	0.01	0.02
2	0.23	0.22	0.24	0.25	0.27
3	0.48	0.48	0.49	0.49	0.49
4	0.22	0.23	0.21	0.21	0.19
5+	0.05	0.05	0.05	0.04	0.03
Bathrooms					
1	0.34	0.29	0.33	0.31	0.35
2	0.47	0.47	0.46	0.45	0.45
3	0.13	0.16	0.13	0.15	0.13
4+	0.03	0.04	0.04	0.05	0.04
Sold in Quarter					
1		0.19	0.22	0.20	0.21
2		0.28	0.27	0.29	0.28
3		0.28	0.28	0.28	0.27
4		0.25	0.24	0.24	0.23
Times Sold				2.14	
				(0.38)	
Total Properties	240,110	84,011		19,539	

Notes: Summary statistics from regression sample as described in Section 5.1. Table lists sample means with standard deviations given in parentheses.

Table A2: Firm Summary Statistics by Industry

(a) Tons of NO_x Emitted

Industry	Mean		Median		Share of Total	
	1998	2002	1998	2002	1998	2002
Petroleum Refining	665.20	479.08	818.57	492.87	51.1%	63.4%
Electric Services	213.19	60.14	100.73	48.94	22.9%	11.1%
Glass Containers	199.27	107.82	145.04	77.92	4.6%	4.3%
Crude Petroleum and Natrual Gas	36.43	8.86	5.72	1.42	3.6%	1.5%
Other Petroleum and Coal Products	321.18	301.88	321.18	301.88	2.5%	4.0%
Steam and Air-Conditioning Supply	38.80	5.65	14.48	3.71	1.8%	0.4%
Other Industrial Inorganic Chemicals	39.70	37.01	34.50	43.59	1.2%	1.5%
Secondary Smelting and Refining	50.84	27.34	52.62	27.63	1.2%	1.1%
Flat Glass	116.21	50.52	116.21	50.52	0.9%	0.7%
Gas and other Services	107.87	9.45	107.87	9.45	0.8%	0.1%
Other Industries	9.61	6.70	4.64	2.91	9.3%	11.9%
All firms	71.47	39.99	6.98	4.26	100.0%	100.0%

(b) Physical Characteristics

Industry	Smoke Stack				Dist. to	
	Height (m)	Diameter (m)	Velocity (m/s)	Gas Temp. (K)	Met. Site (km)	Firms
Petroleum Refining	30.10	1.59	11.83	548.83	6.52	10
Electric Services	40.84	3.69	19.49	481.12	7.46	14
Glass Containers	26.09	1.23	13.41	495.60	7.82	3
Crude Petroleum and Natrual Gas	6.85	0.34	13.56	595.11	6.09	13
Other Petroleum and Coal Products	14.66	0.63	14.56	341.32	6.06	1
Steam and Air-Conditioning Supply	19.68	0.83	12.67	468.18	6.76	6
Other Industrial Inorganic Chemicals	35.23	0.97	11.87	540.11	6.09	4
Secondary Smelting and Refining	9.35	0.69	14.11	406.11	5.52	3
Flat Glass	10.97	1.28	13.60	547.04	5.34	1
Gas and other Services	18.29	4.36	22.49	727.59	6.25	1
Other Industries	12.43	0.82	10.01	486.12	6.17	126
All firms	16.14	1.08	11.46	497.27	6.31	182

Table A3: Price Effects with Geographic Diff-in-diff and Interpolation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. 1-mile treatment, 2-mile control							
	ln Price	NO_x	ln Price	ln Price	Ozone	ln Price	ln Price
Near×post	0.0056	0.3398			-0.0846		
	[0.0082]	[0.4708]			[0.1125]		
NO_x			0.0164	-0.0070			
			[0.0328]	[0.0065]			
Ozone						-0.0658	0.0028
						[0.1308]	[0.0225]
Method	OLS	OLS	2SLS	2SLS	OLS	2SLS	2SLS
IV Set			Post	Annual		Post	Annual
1st Stage F-stat			0.9	2.0		0.9	3.2
B. 2-mile treatment, 4-mile control							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	ln Price	NO_x	ln Price	ln Price	Ozone	ln Price	ln Price
Near×post	-0.0083**	-1.0768***			0.2228***		
	[0.0034]	[0.1860]			[0.0452]		
NO_x			0.0077**	0.0018			
			[0.0034]	[0.0023]			
Ozone						-0.0373**	0.0037
						[0.0175]	[0.0051]
Method	OLS	OLS	2SLS	2SLS	OLS	2SLS	2SLS
IV Set			Post	Annual		Post	Annual
1st Stage F-stat			50.9	12.1		38.5	39.8
C. 3-mile treatment, 6-mile control							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	ln Price	NO_x	ln Price	ln Price	Ozone	ln Price	ln Price
Near×post	-0.0017	-0.5263***			0.1365***		
	[0.0022]	[0.1082]			[0.0276]		
NO_x			0.0033	0.0051			
			[0.0043]	[0.0037]			
Ozone						-0.0126	0.0018
						[0.0166]	[0.0088]
Method	OLS	OLS	2SLS	2SLS	OLS	2SLS	2SLS
IV Set			Post	Annual		Post	Annual
1st Stage F-stat			26.4	4.8		32.1	15.0

Notes: N for each panel is 76,757; 367,872; and 896,398, respectively. Unit of observation is house-firm-quarter. NO_x and ozone exposure interpolated from monitors using inverse distance weighting. Near=1 for houses within specified treatment radius. Sample restricted to houses within specified control radius. IV Set "Post" is Near×post. IV Set "Annual" is Near times year dummies. 1st Stage F-stat assumes spherical errors. Controls include house-firm effects, year-quarter effects, and quadratic time trends by local geography and year 2000 SES variables.

Table A4: Robustness Checks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
	Cluster at		Conley Std. Err., w/ Bandwidth				Control for	
	Block Group	Tract	¼ mile	1 mile	2 miles	Uniform emissions	Uniform_pre×year	"Firm w
				Panel A. Reduced Form				
Aermod_pre×post	0.0033***	0.0033***	0.0033***	0.0033***	0.0033***	0.0032***	0.0038***	0.
	[0.0009]	[0.0010]	[0.0009]	[0.0010]	[0.0011]	[0.0009]	[0.0009]	[
	(0.000)	(0.001)	(0.000)	(0.002)	(0.002)			
				Panel B. 2SLS				
Aermod	-0.0075***	-0.0075*	-0.0075***	-0.0075**	-0.0075*	-0.0082***	-0.0084***	-0.
	[0.0029]	[0.0041]	[0.0029]	[0.0038]	[0.0041]	[0.0030]	[0.0029]	[
	(0.009)	(0.067)	(0.009)	(0.049)	(0.069)			