Harvard Environmental Economics Program

DEVELOPING INNOVATIVE ANSWERS TO TODAY'S COMPLEX ENVIRONMENTAL CHALLENGES

May 2016
Discussion Paper 16-69

The True Cost of Air Pollution: Evidence from House Prices and Migration*

Daniel M. Sullivan
Ph.D. 2016, Harvard University, Economics

*This paper was one of two 2016 winners of the Harvard Environmental Economics Program's Enel Endowment Prize for the Best Paper by a Doctoral Student.

HARVARD Kennedy School
JOHN F. KENNEDY SCHOOL OF GOVERNMENT

heep@harvard.edu
http://heep.hks.harvard.edu

The True Cost of Air Pollution: Evidence from House Prices and Migration

Daniel M. Sullivan

Harvard University

The Harvard Environmental Economics Program

The Harvard Environmental Economics Program (HEEP) develops innovative answers to today's complex environmental issues, by providing a venue to bring together faculty and graduate students from across Harvard University engaged in research, teaching, and outreach in environmental, energy, and natural resource economics and related public policy. The program sponsors research projects, convenes workshops, and supports graduate education to further understanding of critical issues in environmental, natural resource, and energy economics and policy around the world. For more information, see the Program's website: http://heep.hks.harvard.edu.

Acknowledgements

The Enel Endowment for Environmental Economics at Harvard University provides major support for HEEP. The Endowment was established in February 2007 by a generous capital gift from Enel SpA, a progressive Italian corporation involved in energy production worldwide. HEEP receives additional support from the affiliated Enel Foundation.

HEEP also receives support from BP, Chevron Services Company, Duke Energy Corporation, and Shell. HEEP enjoys an institutional home in and support from the Mossavar-Rahmani Center for Business and Government at the Harvard Kennedy School. HEEP collaborates closely with the Harvard University Center for the Environment (HUCE). The Center has provided generous material support, and a number of HUCE's Environmental Fellows and Visiting Scholars have made intellectual contributions to HEEP.

HEEP and the closely-affiliated Harvard Project on Climate Agreements are grateful for additional support from the Harvard University Climate Change Solutions Fund; the Belfer Center for Science and International Affairs and the Ash Center for Democratic Governance and Innovation—both located at the Harvard Kennedy School; Christopher P. Kaneb (Harvard AB 1990); and the International Emissions Trading Association (IETA).

Previous sponsors include: the Alfred P. Sloan Foundation, the AVINA Foundation, Bank of America, Castleton Commodities International LLC, ClimateWorks Foundation, Doris Duke Charitable Foundation, the James M. and Cathleen D. Stone Foundation, the National Science Foundation, the U.S. Department of Energy, and the U.S. Environmental Protection Agency.

Citation Information

Sullivan, Daniel. "The True Cost of Air Pollution: Evidence from House Prices and Migration." Discussion Paper 2016-69. Cambridge, Mass.: Harvard Environmental Economics Program, May 2016.

The True Cost of Air Pollution: Evidence from House Prices and Migration

Daniel M. Sullivan*

This draft: March 29, 2016

Abstract

In this paper, I present evidence that current economics research significantly underestimates the effects of air pollution, regardless of the outcome of interest. This bias exists even in quasi-experimental estimates and arises from the way researchers define individual-level pollution exposure. 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 greater Los Angeles. I then estimate the effect of NO_x emissions on house prices and neighborhood composition 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 social value of the cap-and-trade program is roughly $502 million per year, 15 times larger than the associated abatement costs. However, when based on conventional measures of pollution exposure, this estimated valuation is small and statistically indistinguishable from zero. The estimated neighborhood sorting response suggests that, despite the high aggregate value, low-income households may not have benefited much from the improvement in air quality.

*Department of Economics, Harvard University. Email: dsullivan@fas.harvard.edu. I am especially grateful to David Cutler, Edward Glaeser, Lawrence Katz, and Robert Stavins, for their feedback. I also thank John Coglianese, Timothy Layton, Jing Li, Jonathan Libgober, Amanda Pallais, Christopher Palmer, Jisung Park, Daniel Pollmann, and James Stock, as well as seminar participants at Harvard, BYU, Notre Dame, University of Wisconsin-Madison, and Resources for the Future. Funding from the National Institute on Aging, through Grant Number T32-AG000186 to the National Bureau of Economic Research, is gratefully acknowledged.

1 Introduction

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: The house price response to improved air quality is surprisingly weak compared to the expected health benefits (Smith and Huang 1995). Studies since Smith and Huang (1995), including quasi-experimental studies, have not resolved this discrepancy.[1] Even more puzzling, such a discrepancy exists only for pollution—it is absent for school quality (Black 1999; Cellini, Ferreira, and Rothstein 2010), crime risk (Linden and Rockoff 2008; Pope 2008), and local cancer risk (Davis 2004). And it cannot be explained by public ignorance of or indifference to the dangers of pollution (Neidell 2009; Moretti and Neidell 2011).

In this paper, I show that this puzzle is a special case of a larger problem in the economics literature on air pollution: Estimates of pollution's effects are systematically biased by how pollution exposure to individuals is measured. Pollution concentrations spike sharply downwind of pollution sources, with sharp changes near the source itself, e.g., immediately downwind and upwind of a highway. This undermines the most common econometric tools used to study pollution—geographic difference-in-differences and monitor-based interpolation—because they are unable to capture this sharp geographic variation. This leads to contaminated treatment and control groups and/or non-classical measurement error. The result is biased estimates, even with an exogenous shock to pollution exposure and regardless of the outcome being studied.

In turn, these biased estimates lead to understated valuations of air quality regulations and other programs like subsidies to clean energy.

I solve this problem using tools from atmospheric chemistry and show that house prices respond dramatically to changes in air quality and that conventional methods are unable to detect this response. Specifically, I measure local exposure using AERMOD, an atmospheric dispersion model developed by the American Meteorological Society and the EPA. AERMOD uses data on meteorology (e.g., wind and temperature at multiple altitudes, pressure, surface roughness) and firms (e.g., smoke stack height,

1. See Section 2. For a family of two adults and one child, 1 µg/m^3 of particulate matter ($PM_{2.5}$) costs about \$1,600 in increased mortality risk alone, to say nothing of acute illness risk, decreased quality of life, or the costs borne by other family members (see footnote 3). Chay and Greenstone (2005) estimate a MWTP of \$191 per 1 µg/m^3 of TSP reduction. While assuming costless moving, Bayer, Keohane, and Timmins (2009) estimate MWTP of \$130 for 1 µg/m^3 of PM_{10}. Currie and Walker (2011) find no significant effect on prices after a drop in NO_x and CO near highway toll booths.

diameter, gas temperature) to determine where pollutants go after leaving a firm's smoke stack. To resolve the usual concerns about the endogeneity of local pollution exposure and housing decisions, I exploit the California Electricity Crisis of 2000 as a natural experiment. The Crisis unexpectedly and permanently lowered NO_x emissions in southern California by precipitating the near collapse of RECLAIM, a then-nascent cap-and-trade market for NO_x, which hastened and synchronized firms' adoption of abatement technology.

Using AERMOD to determine who benefited from the Crisis reveals that the demand for clean air is high while using conventional methods does not. Using AERMOD, I estimate that the marginal willingness to pay (MWTP) to reduce exposure to NO_x emissions by 1 $\mu g/m^3$ is $3,272, an order of magnitude larger than past estimates and more in line with the expected health benefits. This implies that the benefit of the RECLAIM cap-and-trade program to local residents is roughly $502 million per year, much larger than the estimated abatement costs of $38 million per year.[2] However, when I use methods now standard in the literature, the estimated price response and the implied social valuation of RECLAIM are small or wrongly signed and not statistically different from zero.

AERMOD also makes it possible to address other questions, such as whether people fully respond to invisible pollutants, and whether or not neighborhood demographics change in response to pollution.

Many pollutants are hard to detect without instrumentation, which raises the possibility that buyers with preferences about pollution exposure may suffer from imperfect information or salience effects. I test for this by exploiting the chemical relationship of NO_x and ozone. NO_2, a primary component of NO_x, is highly visible, but under certain atmospheric conditions it transforms into ozone, which is invisible but far more toxic. The conversion rate of NO_x to ozone varies predictably over the course of the year, making it possible to test whether prices depend on long-run expectations based on all currently available information, or whether they are sensitive to foreseeable short-run changes in toxicity and visibility. I find that prices are much more sensitive to visible NO_x than they are to invisible ozone, consistent with a model where buyers suffer from imperfect information, salience effects, or both.

2. This is the total benefit of decreasing NO_x emissions from actual 1995 levels to the RECLAIM cap in 2005, annualized at 3%. Abatement cost is based on SCAQMD's tabulation of firms' actual equipment and the available abatement technology that would need to be installed to meet certain abatement goals. The cost includes installation and ongoing operation of equipment (SCAQMD 2000).

I also find that the change in air quality induced a large demographic sorting response at the neighborhood (block group) level, which has significant implications for incidence. Neighborhoods that enjoyed bigger air quality improvements due to the Crisis also saw their residents become richer and better educated; however, these neighborhoods also saw a *decrease* in total residents and households. These effects appear to be driven by low-education individuals (those without a high school degree) leaving newly clean areas or moving into these areas at a lower rate. Specifically, the results suggest that 60,000 low-education adults left or avoided the sample area after the Crisis, roughly 13% of the area's pre-Crisis low-ed population. Low rates of home-ownership in low-income neighborhoods further suggest that these individuals did not benefit much from the house price windfall before leaving, making it unlikely that they saw a large share of the aggregate benefits. In addition, the large sorting response confirms that house prices responded to a demand shock caused by a real amenity change and not a contemporaneous change in market dynamics, such as an expansion of credit to sub-prime borrowers.

Together, the results suggest that reducing air pollution is a very cost-effective way to improve welfare but that policies may also face a steep trade-off between efficiency and equity. More generally, the failure of standard methods raises the possibility that current estimates of pollution's effects on other outcomes like health and mortality are also significantly too small.

Before presenting the results in detail, I discuss the puzzle of clean air's seemingly low value, what is behind the puzzle, and how to solve it (Section 2). I then provide the theoretical framework I use to draw conclusions about MWTP, how pollutant visibility affects agents' behavior, and how people sort geographically in response to changes in pollution (Section 3). Next, I discuss my research design based on the Electricity Crisis and outline my estimation strategy (Section 4). Finally, I describe the data I use (Section 5), present the results (Section 6), and discuss the possible welfare implications (Section 7).

2 Finding the Value of Clean Air

House prices have long been used to measure the marginal willingness to pay (MWTP) for non-market goods. By varying a single characteristic of a house and observing the associated price change, we can infer the MWTP for that characteristic (see Section 3.1).

The MWTP for pollution abatement has been measured this way many times, starting with Ridker and Henning (1967).

But past work suggests that house prices do not respond much to pollution, implying a disparity between the MWTP for pollution reductions and the expected health benefits (Smith and Huang 1995). For a family of two adults and one child, 1 microgram of particulate matter ($PM_{2.5}$) per cubic meter of air (µg/m^3) costs about $1,600 in increased mortality risk alone, to say nothing of acute illness risk, decreased quality of life, or the costs borne by other family members.[3] However, in their meta-analysis of OLS estimates of MWTP, Smith and Huang (1995) find that the interquartile range of estimated MWTP is $0 to $233 per µg/m^3 TSP and that the mean estimate only covers 6–33% of VSL-based mortality cost.[4]

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 µg/m^3 reduction in TSP, well within Smith and Huang's interquartile range. Bayer, Keohane, and Timmins (2009) also use county-level data and use pollution from distant sources as an instrument for local pollution to estimate a MWTP of $131 per µg/m^3 reduction of PM_{10}.[5]

This disparity appears to be peculiar to air pollution, as 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

3. The mortality value for an adult is $680 and based on the value of a statistical life (VSL) for adults aged 35–44 from Aldy and Viscusi (2008) and adult $PM_{2.5}$ mortality risk from Pope et al. (2002). For a child, the value is $250 using infant $PM_{2.5}$ risk from Woodruff, Parker, and Schoendorf (2006) and the VSL of a 18–24-year-old, the lowest age estimated by Aldy and Viscusi. All monetary values in the paper are denominated in 2014 dollars unless otherwise noted.

4. There are several measures of the class of pollutants called "particulate matter," which are larger solid and liquid particles rather than gaseous molecules. $PM_{2.5}$ is all such particles with a diameter no larger than 2.5 micrometers (µm), while PM_{10} particles have a diameter between 2.5 and 10 µm. Total suspended particulates, or TSP, is a another measure that corresponds to all particles smaller than 25–40 µm, depending on the apparatus collecting samples. Because of the inconsistent apparatus-dependent definition of TSP, the EPA abandoned it as an official measure in 1987 (52 FR 24634).

5. The estimate from Chay and Greenstone (2005) is based on their preferred specification in Table 5A, column 4. The estimate from Bayer, Keohane, and Timmins (2009) is taken from Table 6, column 2. This estimate 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.

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).

The disparity is also not caused by a general ignorance of pollution's health costs or an unwillingness to avoid pollution. For example, it could be that people simply do not know that pollution is dangerous, or that, like junk food, the cost of a marginal dose is not salient enough to elicit a behavioral response. However, Neidell (2009) and Moretti and Neidell (2011) find the opposite. They find that attendance at outdoor attractions like the zoo and sporting events drops precipitously in response to smog alerts, suggesting that people not only know the health risks but are willing to undertake costly avoidance behavior.

This body of conflicting evidence suggests that something specific to air pollution is attenuating house price responses or estimates of those responses.

2.1 Econometric Problems Behind the Puzzle

A likely candidate for attenuation bias is misspecification in who is exposed to pollution (or pollution clean-up) and who is not. This is because, unlike wages or education, there are no data on individual-level pollution exposure, so researchers must approximate exposure in some way. In the economics literature, two approaches are predominantly used.[6] 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 exposure across short distances, which biases estimates based on these methods. It is also important to note that these problems are inherent to pollution exposure generally and thus extend to estimates of pollution's

6. Currie et al. (2014) summarize the methods used in the literature on pollution's effect on children's health.

effect on any outcome.

2.1.1 Bias in Geographic Diff-in-diff Estimates

In a geographic difference-in-differences design, people around a pollution source are assigned to treatment and control groups based on their proximity to the source. The econometrician chooses radius r_0 around the source to define the treatment group and radius $r_1 > r_0$ to define the control. Having defined treatment and control groups, the problem is now a standard diff-in-diff around some shock to the source's pollution emissions. This allows the reduced-form effect of the pollution source to be estimated when data on exposure is unavailable.[7]

When used to study air pollution, however, the geographic diff-in-diff is biased because the wind does not respect the radii chosen by the econometrician and contaminates both the treatment group and the control group. Suppose the true effect of a polluting firm on outcome y_{it} is

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

where X_{it} is pollution exposure to i at time t, N_{it} is exposure to other disamenities created by the firm (e.g., eyesore of a refinery), and $t \in \{0, 1\}$ indexes the pre- and post-shock time periods, respectively. Exposure can be written $X_{it} = m_{ft} \cdot h(r_{fi}, \theta_{fi}; \boldsymbol{S}_f)$ where m_{ft} is firm f's emissions and h is the probability density function that a molecule of emissions ends up at distance r and heading θ relative to the firm. The vector $\boldsymbol{S}_f$ contains variables about the physical characteristics of the firm's polluting equipment (e.g., height of the smoke stack) and local meteorological conditions like wind speed and direction. Assume r_0 is chosen so that $r_{fi} > r_0$ implies $N_{it} = 0$.

The geographic diff-in-diff estimates the reduced form as

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

where $C_i = \mathbf{1}\{r_{if} \leq r_0\}$ is a dummy variable for individuals living in the treatment area. We can write the expected value of y_{it}, conditional on i's treatment assignment,

7. For examples of research focused on reduced-form geographic diff-in-diffs, see Currie and Walker (2011) and Currie et al. (2015).

in terms of the average effects on the treatment group:

$$\mathbb{E}_i[y_{it} \mid C] = \alpha \bar{N}_t^C \cdot C + \beta \bar{X}_t^C \cdot [C + \varphi(1 - C)] \tag{3}$$

where $\bar{X}_t^C = \mathbb{E}_i[X_{it} \mid C = 1]$ and $\varphi = \mathbb{E}_i[X_{it} \mid C = 0]/\bar{X}_t^C$. Figure 1 depicts the geographic diff-in-diff's radii with the true downwind treatment marked by the shaded region and the average effect for each area based on Equation (1). Note that as wind speed increases, the shaded treatment area narrows and extends farther from the source, increasing relative exposure downwind and thus increasing φ.

By construction, the geographic diff-in-diff recovers the following estimate of pollution's effect on y_{it}:

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

Using Equation (3), this reduces to

$$\hat{\gamma}_{\text{GD}} = \underbrace{\alpha \left(\bar{N}_1^C - \bar{N}_0^C\right)}_{\text{Non-pollution Effect}} + \underbrace{(1 - \varphi)}_{\text{Wind bias}} \cdot \underbrace{\beta \left(\bar{X}_1^C - \bar{X}_0^C\right)}_{\text{Pollution Effect}} \tag{4}$$

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, making this term 0.[8] The second term is the change in average exposure to the treatment group, multiplied by the contamination factor $(1 - \varphi)$.

Thus, even when non-pollution effects are held constant over time, the estimate of the pollution effects is biased because the control group is actually treated as well. And because φ increases with wind speed, the contamination factor $(1 - \varphi)$ and $\hat{\gamma}_{\text{GD}}$ both become more negative as wind speed increases. Furthermore, because the distribution function h need not be monotonic in r, φ need not be less than 1, meaning $\hat{\gamma}_{\text{GD}}$ could have the wrong sign.[9] This contamination problem is common in program evaluation (e.g., Miguel and Kremer 2004) and can be fixed by re-scaling by average treatment

8. This is naturally not the case when the shock to the firm is the construction of the firm itself (as in Banzhaf and Walsh 2008, Davis 2011, and Currie et al. 2015). In such cases, $\bar{N}_1^C > \bar{N}_0^C = 0$. Note also that as the wind gets stronger and $\varphi \to 1$, $\hat{\gamma}_{\text{GD}} \to \alpha \bar{N}_1^C$ 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.

9. An example of the non-monotonicity of exposure with distance is given by Figure 2b.

intensity. But this requires a good measure of treatment intensity, and, as Section 2.1.2 argues, this is a role monitor data are not well suited to play.

Empirically, the dependence of the bias on wind speed is important for two other reasons. First, greater Los Angeles is one of the least windy areas in the United States, so if the wind significantly biases estimates in this sample, it almost certainly biases estimates in other regions with greater wind speeds. Second, when pollution is less influenced by the wind, standard non–wind-based estimates should be less biased. For example, when pollution is emitted at ground level, more of it stays close to the source, keeping φ low. This suggests that the bias in geographic diff-in-diffs around pollution from vehicles on the ground (e.g., Currie and Walker 2011) may not be as severe. However, even car exhaust gets carried by the wind (Hu et al. 2009), and a low φ does not mitigate the separate bias introduced by monitor data.

2.1.2 Bias from Pollution Monitor Interpolation

The most common way monitor data are adapted for use in economics is interpolation, which uses data from pollution monitors to approximate pollution exposure at other locations of interest.[10] To study the effects of pollution exposure on some outcome, we need data on the outcome and pollution exposure, $\{(y_i, x_i)\}_{i=1}^N$, but x_i is never observed. However, we do observe $\{x_m\}_{m=1}^M$, pollution exposure at monitor locations. If values of x are spatially correlated, so $\text{Cov}(x_i, x_j)$ is high if i and j are physically close to one another, then $\{x_m\}$ can be used to construct an approximation $\tilde{x}_i$ for any needed x_i.

The viability of any interpolation method depends critically on the spatial covariance of x. In the most extreme case where $\text{Cov}(x_j, x_k) = 0$ for all $j \neq k$, the interpolated values will obviously be no better than random noise because the monitor data $\{x_m\}$ do not provide any information about x_i. Similarly, if $\text{Cov}(x_j, x_k)$ falls quickly as the distance between j and k grows, then more monitors will be needed at a higher spatial frequency to cover the sample area. For example, if $\text{Cov}(x_j, x_k) \approx 0$ if j and k are more than 1 km apart, but all monitors are 5 km apart, then the interpolated $\tilde{x}_i$ will be no better than noise for large portions of the sample area. The converse also holds and

10. It is also possible to use monitor data by restricting the data sample to people living close to a single monitor. The shortcomings of this method are entirely practical, since reducing the sample radius reduces the measurement error but also reduces the sample itself. This method works well in case studies, like Graff Zivin and Neidell's (2012) analysis of how worker productivity at a single firm covaries with readings from a nearby $PM_{2.5}$ monitor. However, the trade off between sample size and measurement error limits its large-scale use.

helps explain why rainfall data, which is highly correlated across tens of kilometers, has been successfully interpolated in many different contexts.[11]

Unfortunately, air pollution exhibits a much lower degree of correlation across space because of the discrete nature of pollution sources. Unlike rainfall and other continental-scale geologic processes, air pollution is predominantly created by many distinct sources like firms and cars. This makes the local geographic distribution of pollution exposure very idiosyncratic, with sharp changes over very short distances; e.g., pollution levels downwind of a highway are dramatically different from pollution levels upwind. This in turn means that the relationship between any given x_i and a monitor reading x_m depend on many more factors than distance and relative direction. Most importantly, $\text{Cov}(x_i, x_m)$ depends on whether a major pollution source exists between i and m. If m is downwind of the source, x_m varies with the source's emissions but x_i does not, and vice versa.

Evidence confirming this problem can be found in existing literature, even though the problem itself has not been directly raised or addressed. Studies using interpolated values often present a leave-one-out cross-validation as evidence of the interpolation's quality.[12] The value of each monitor reading x_m is interpolated using all remaining monitors and the correlation between x_m and $\tilde{x}_m$ is calculated, with a high correlation coefficient assumed to be evidence of a good interpolation. However, the correlation of x_m and $\tilde{x}_m$ presented in these studies is generally unconditional, which conflates spatial correlation with secular temporal correlation which may equally effect all monitors (e.g., seasonal trends in ozone). Karlsson, Schmitt, and Ziebarth (2015) use German pollution monitors and inverse distance weighting (IDW) to calculate this cross-validation correlation for several pollutants in Germany. The unconditional correlations range from 0.5 to 0.93; however, conditional on year and season effects, the correlations drop precipitously, ranging from 0.15 to 0.47.[13] Likewise, Knittel, Miller, and Sanders (2014) and Lleras-Muney (2010) present evidence of non-classical measurement error in IDW and Kriging interpolations, respectively. In this context, the non-classical measurement error will exacerbate the usual attenuation in OLS estimates from classical and potentially cause wrongly signed estimates.[14]

11. See Pouliot (2015) for a summary of rainfall interpolations.

12. Inverse distance weighting, as well as this cross-validation technique, has been the standard method for with sub-county pollution analyses in the economics literature since Neidell (2004) and Currie and Neidell (2005).

13. See Table F1 of Karlsson, Schmitt, and Ziebarth (2015).

14. This is because the distribution of $\tilde{x}$ is smoother than that of x, so $\text{Var}(\tilde{x}) < \text{Var}(x)$. Noting that

And unlike classical measurement error, quasi-experimental research designs and other IV methods will not necessarily redeem a bad interpolation. Given an instrument z and interpolation error $\eta = \tilde{x} - x$, estimates will only be consistent if z and η must be uncorrelated. This simply follows from the canonical probability limit of the IV estimator:

$$\text{plim}\, \hat{\beta}_{\text{IV}} = \beta \cdot \frac{\text{Cov}(x, z)}{\text{Cov}(x, z) + \text{Cov}(\eta, z)} \tag{5}$$

Note that in this case that $\hat{\beta}_{\text{IV}}$ could be bigger or smaller than β depending on the joint distribution of (x, z, η) which will vary across research designs. Nevertheless, $\hat{\beta}_{\text{IV}}$ can only be consistent when $\text{Cov}(\eta, z) = 0$, which is unlikely to be the case in the most commonly used research designs.

In the case of the geographic diff-in-diff, this condition is very unlikely to hold because firms outnumber monitors by several orders of magnitude. 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. Despite being in one of the most intensively studied areas in the United States, each monitor in greater Los Angeles is outnumbered by hundreds of firms. This disparity is readily apparent in Figure A2, which maps the locations of every polluting firm and pollution monitor in the area. With so few monitors, the distribution of $\tilde{x}$ will be smooth across the sample area of most firms; that is, $\tilde{x}$ will not spike downwind of the firm. But actual exposure x does spike, particularly close to the firm, so η will also spike near the firm and will be correlated with proximity to the firm. And since the instrument z is defined by proximity to the firm, $\text{Cov}(\eta, z) \neq 0$.

In the case of county-level studies using the Clean Air Act (CAA) as a natural experiment, estimates are also likely to be inconsistent because z is mechanically related to $\tilde{x}$. The CAA is often used as a natural experiment because it instituted more stringent regulations on counties whose average monitor reading exceeded a certain threshold. In these county-level studies, the measure of pollution exposure to the county, $\tilde{x}$, is generally the very same monitor average that affects a county's treatment status. The econometric problem this causes is easier to see by noting that Equation (5) can also be written as

$$\text{plim}\, \hat{\beta}_{\text{IV}} = \beta \cdot \frac{\text{Cov}(x, z)}{\text{Cov}(\tilde{x}, z)} \tag{5$'$}$$

Thus, if the treatment impacts monitor readings $\tilde{x}$ more than actual exposure x,

$\tilde{x} = x + \eta$, where η is the interpolation/measurement error, it immediately follows that $\text{Cov}(x, \eta) < 0$.

plim $\hat{\beta}_{\text{IV}} < \beta$ and the estimate will understate the true effect. This would be the case if, as Bento, Freedman, and Lang (2015) find, regulators put more effort into reducing pollution levels at problematic monitors within the county.[15]

2.2 Solving the Puzzle with Atmospheric Dispersion Modeling

The econometric problems described above are rooted in the idiosyncratic nature of pollution exposure across space. Any measure of pollution must capture sudden changes in exposure over short distances in order to be useful in statistical analyses. Atmospheric dispersion models use detailed data on meteorology and firms to accomplish exactly this goal.

A dispersion model uses data on a firm's polluting equipment and the meteorology around the firm (the vector $\boldsymbol{S}_f$ from Section 2.1.1) and predicts the spatial distribution of the firm's pollution (the function h from Section 2.1.1). 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 (e.g., Perry et al. 2005).[16] To account for meteorological conditions, AERMOD requires 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.[17] AERMOD also requires five parameters for the pollution source itself: the smoke stack's height and diameter, the temperature and velocity of the gas exiting the stack, and how much pollution is emitted by the stack.

Using these data, AERMOD yields $\text{aermod}_{ift} = \text{NOx}_{ft} \cdot \tilde{h}(r_{fi}, \theta_{fi}; \boldsymbol{S}_f)$, the pollution exposure to location i at time t due to NO_x emissions from firm f, measured in micrograms per cubic meter of air (µg/m^3). Summing over all firms yields total industrial exposure: $\text{aermod}_{it} = \sum_f \text{aermod}_{ift}$. It is important to note that these

15. There is a more general problem with using the average of a county's monitors: The relationship between $\tilde{x}$ and the true distribution of individual-level exposure is unclear and changes over time because monitors are a sample across *space*, not population. Even if it could be credibly established that $\tilde{x}$ is an unbiased approximation of the mean (or any order statistic) of the true exposure distribution at some point in time, this relationship would quickly be broken as people and firms change their behavior and locations over time.

16. 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. Field tests are generally conducted by placing several dozen monitors around a polluter and adding to its emissions a non-toxic, non-reactive tracer chemical which is not usually present in the area.

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

AERMOD-based measures do not represent NO_x exposure alone. AERMOD uses data on how much NO_x a firm emits, but NO_x will react in the atmosphere to become ozone, thus aermod_{ift} is a composite measure of NO_x and ozone exposure. Section 4.2 below describes how this fact can be used to test how buyers respond differentially to NO_x, which is visible, and ozone, which is not.

Mapping aermod_{ift} (Figure 2) and aermod_{it} (Figure 3) makes clear the problems caused by geographic diff-in-diffs and monitor interpolation, respectively. Figure 2 maps aermod_{ift} for a single firm, the Scatterwood Generating Station. The concentration of NO_x-based pollution is plotted for all 100-meter grid squares. For Figure 2a, this is limited to area less than 20 kilometers of the firm. Figure 3a maps aermod_{it}, total exposure to industrial emissions, across the entire sample area, with monitor locations marked by white dots.[18] Figure 3b shows how exposure would be calculated by interpolating aermod_{it} from actual monitor locations.

Figure 2 shows that the direction and speed of the wind is crucial in knowing who is affected by the firm. It also shows how extensive the contamination of a geographic diff-in-diff can be. In particular, Figure 2b offers a closer look at the exposure around the firm, with circles drawn at one and two miles from the firm for easy comparison to a geographic diff-in-diff's treatment and control groups. Much of the control group sees extreme levels of exposure while the area of lowest exposure in the geo diff-in-diff sample is actually in the treatment group.[19]

Figure 3 shows that there is far too much spatial variation in exposure to be captured by so few monitors. Figure 3a shows how quickly exposure can change over short distances and how unpredictable the exposure distribution can be. The number of local extrema and inflection points far exceeds the number of nearby monitors. Figure 3b makes this problem easier to see by showing the interpolated values of aermod_{it} based on the actual monitor locations. The interpolation follows the literature and is calculated using inverse distance weighting (IDW) with monitors restricted to those with full NO_x coverage over the sample period (1997–2005). Monitors are also given a weight of zero if they are more than 15 km from the point of interest.

Little of the variation seen in Figure 3a remains after interpolation. Most locations' predicted exposure are perfectly correlated with the nearest monitor, and the area that

18. Section 4.3 discusses how this sample region is defined. Details about how AERMOD is implemented in this paper are given in Section 5.5.

19. This non-monotonicity is caused by the height of the firm's smoke stacks (about 300 feet) and the buoyancy of the hot gases they emit. The bulk of the smoke plume travels laterally in the air before touching down.

does have some variation at best vaguely resembles the true distribution. Note that if the 15-km interpolation radius were expanded, this would add no true variation to the data because the sample of monitors would be the same. This would be especially troublesome for the southwestern corner of the region, the Palos Verdes Peninsula, which has very low exposure because it is upwind of all major polluters. Despite being one of the cleanest areas in the sample, it would be assigned a very high-level exposure and be indistinguishable from the truly polluted area near the monitor.

The complex patterns seen in the wind-based exposure distribution is obviously difficult to approximate using concentric circles or other simple methods. The possibility that factors like the wind might affect estimates has been raised occasionally in the literature, but the results have not suggested it is an important issue. Of the economics papers on industrial pollution that have tried to account for the wind, only Hanna and Oliva (2015) find that the wind significantly alters their estimates, and then only in certain specifications.[20] The mixed nature of these past results is likely due to the complexity of the atmospheric dispersion problem, which has been the dedicated focus of many atmospheric scientists for decades (see Cimorelli et al. 2005, Section 1 for a summary). Fortunately, the econometric problems described above can be avoided by taking advantage of their work.

3 Theory and Predictions

This section presents a simple model of locational choice and describes how it can be used to answer the economic questions of interest: what are people willing to pay for clean air; does the market fully capitalize the costs of invisible pollution; and what is the incidence of an air quality improvement.

20. 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. Schlenker and Walker (Forthcoming) measure the change in daily hospital visits due to changes in airport traffic and incorporate wind speed and direction into one of their models. 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.

3.1 House Prices, Hedonics, and MWTP

When choosing a place to live, households weigh a location's amenities and house prices against their own income and preferences. They solve

$$\max_{c,\boldsymbol{g}} u(c, \boldsymbol{g}; \boldsymbol{\alpha}) \quad \text{s.t.} \quad y = c + P(\boldsymbol{g}) \tag{6}$$

where c, the numeraire, is aggregate non-amenity consumption; $\boldsymbol{g}$ is a vector of public and private amenities provided by the chosen neighborhood and house; $P(\boldsymbol{g})$ is the price of a house with amenities $\boldsymbol{g}$; and $(y, \boldsymbol{\alpha})$ are income and a vector of preference parameters, respectively, and together define the household. This differs from a standard consumer problem because many elements of $\boldsymbol{g}$, like air quality or proximity to the ocean, are location specific, so households must physically relocate in order to change their consumption of these amenities. This adds a spatial element to the standard market clearing equilibrium conditions—every household must weakly prefer their current location to all others.

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 marginal price.[21] To see why this is the case, note that for some fixed utility level $\bar{u}$, the solution to Equation (6) can be re-written

$$u\left(y - \theta(\boldsymbol{g}^*; y, \boldsymbol{\alpha}, \bar{u}), \boldsymbol{g}^*; \boldsymbol{\alpha}\right) = \bar{u} \tag{7}$$

where θ is the agent's willingness to pay for $\boldsymbol{g}$, conditional on $(y, \boldsymbol{\alpha}, \bar{u})$. For a single amenity g_k, $\partial\theta/\partial g_k = \theta_{g_k}$ is the marginal willingness to pay for g_k, and P_{g_k} is the marginal price for g_k. If $\theta_{g_k} > P_{g_k}$, then the agent can buy more g_k for less than she would otherwise be willing to pay, and vice versa if $\theta_{g_k} < P_{g_k}$; thus in equilibrium, $\theta_{g_k} = P_{g_k}$ for all g_k at $\boldsymbol{g}^*$.

Estimating the average MWTP, which is difficult to do directly, can therefore be accomplished by estimating P_{g_k} instead, though this requires some assumptions. 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

21. 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 and equilibrium sorting models.

periods and low-frequency data, it is likely to hold when using a short sample period and quarterly data. Another potential problem is that $(P(\boldsymbol{g}^*), \boldsymbol{g}^*)$ is endogenously chosen by the agent, creating a potentially omitted variables problem (Bartik 1987; Epple 1987; Chay and Greenstone 2005). Any attempt to identify P_{g_k} must address this and satisfy the identification assumptions specific to the chosen research design, which I discuss for this paper in Section 4.

3.2 Pollutant Visibility and Prices

Even if people care about pollution, they cannot bid more for houses with cleaner air if they cannot discern clean air from dirty air.

In an efficient market, a house's price should reflect the net present value of expected future utility flows afforded by the house's amenities. If amenities change—or people believe they will change—it should be reflected immediately in the market price of the house. Therefore, any transitory or already foreseen changes in pollution levels, like predictable seasonal variation, should not affect a house's price.

Conversely, if buyers suffer from imperfect information or salience effects, then prices may depend on transitory changes in pollutant concentrations or salience. Determining current pollution levels is difficult without equipment because many pollutants, like ozone, are colorless and have no bad smell. Extrapolating pollution's daily, weekly, and yearly patterns after a single viewing is even more difficult. But even with perfect information, people may not respond to pollution if it or its costs are not salient. There is a growing body of evidence that salience and framing can significantly affect even weighty decisions like choosing a house and a neighborhood.[22]

We can distinguish between these cases empirically by testing whether house prices respond to foreseeable changes in the composition of air pollution. NO_x is emitted directly by polluters and becomes ozone at different but predictable rates throughout the year. Thus, with perfect information and rational agents, house prices should not respond to these seasonal changes. If the price response does vary seasonally, the physical characteristics of NO_x and ozone will allow us to identify whether toxicity or

22. Pope, Pope, and Sydnor (2014) show that house prices gravitate toward round numbers like $150,000, suggesting that psychological biases play a large role in major purchases. Busse et al. (2015) find people are more likely to buy a convertible car on a hot or cloud-free day, even if they have already owned a convertible and should know how much utility they get from driving a convertible in the snow. An earlier version of this paper, Busse et al. (2012), also provided evidence that houses with air conditioners and swimming pools fetch higher prices during the summer.

visibility affects buyers more (see Section 4.2).

3.3 Sorting, Home-ownership, and Incidence

Describing the incidence of a pollution reduction is difficult without detailed panel data on households. Nevertheless, differential migration behavior and homeownership rates across socioeconomic groups can shed some light on who benefits most from air quality improvements.

In the canonical two-city spatial equilibrium model, population flows into a city after an amenity improvement.[23] However, with many cities and heterogeneous income, low-income households may flow *away* from amenity improvements if high-income households are willing to pay relatively more for the same amenity improvement.[24] If emigrating households' next best amenity and price bundle is unchanged, they are likely worse off since they are moving to a location they had already turned down and must also pay moving costs. However, if households cannot accurately assess air quality, then they may not have been at their optimal residential choice to begin with, making the welfare change for emigrants potentially ambiguous. Furthermore, emigrants may also have been homeowners who benefited from the house price windfall caused by the improvement and they are simply re-optimizing in response to their new budget constraint.

Incumbent home owners in general will benefit from the sudden increase in house prices, regardless of their income or preferences. In the classic Roback (1982) or Alonso–Muth–Mills framework of spatial equilibrium, every household is marginal and prices perfectly capture households' valuation of the amenity, offsetting the utility gains and making incumbent home owners the exclusive beneficiaries of the amenity improvement. However, this is an extreme case, and heterogeneity in preferences and incomes will almost certainly leave many inframarginal households with positive rents after the price increase. But even with many inframarginal consumers, incumbent home owners will capture a large portion of the welfare gains and it is easy to measure how these gains are spread across the income distribution.

---

23. This is a classic result in local public finance going back to Tiebout (1956) and has been examined many times. See Epple and Sieg (1999) for a general application and Kuminoff, Smith, and Timmins (2013) for an overview of the equilibrium sorting literature. See Banzhaf and Walsh (2008) for a two-city model applied to air pollution.

24. Higher willingness to pay among high-income households, conditional on preferences, follows directly from the standard single crossing assumption on preferences for the numeraire and the amenity.

4 Research Design

In this section, I describes how I use the California Electricity Crisis as a natural experiment (Section 4.1) and how the Crisis shocked both NO_x and ozone, which can be used to identify the affect of pollutant visibility on prices (Section 4.2). Section 4.3 details the econometric models I estimate.

4.1 Electricity Crisis as Natural Experiment

Estimates of pollution's effect on house prices may suffer from omitted variables bias because households endogenously choose their bundle of amenities and many characteristics about the location and the residents themselves are unobservable. To identify the causal effect of pollution exposure on house prices, I use the natural experiment created by the California Electricity Crisis of 2000, which unexpectedly and permanently lowered NO_x emissions through its effect on the RECLAIM cap-and-trade program.

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 (see Fowlie, Holland, and Mansur 2012). At that time, firms were given an initial allocation of RECLAIM Trading Credits (RTCs) which were tied to a specific year. At the end of each year, firms must surrender one current-year 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, the total number of RTCs was set to be higher than total emissions initially and decrease over time, eventually creating a binding cap.

However, the California Electricity Crisis caused the aggregate cap to bind suddenly, which in turn caused firms to suddenly cut their emissions. Through 1999, most firms had more than enough RTCs to cover their emissions, so there was little need to trade or install abatement equipment. Because of the lack of demand, RTC prices were low, and firms expected that they would be able to buy RTCs cheaply when their own private cap became binding. In 2000, demand for electricity unexpectedly outstripped potential supply, beginning the California Electricity Crisis.[25] Electricity generators ramped up production to prevent rolling black outs. However, generators subject to

25. The exact causes of the shortage and the Crisis in general are a source of much debate. See Borenstein (2002) and Weare (2003), especially Section 3.

RECLAIM needed RTCs to cover their increased emissions, which caused the aggregate RTC cap to suddenly bind. RTC prices skyrocketed and non-electric firms cut their emissions in response.

This dramatic change is shown in Figure 4, which plots total NO_x emissions, available RTCs for each year, and monthly RTC prices. With the onset of the Crisis, RTC prices jumped from an average of \$2,800 in 1999 to a peak of \$62,000 at the end of 2000. The resulting drop in emissions is shown in Figure 5, which plots the average of firm emissions by quarter and year, giving firms equal weight by re-scaling a firm's emissions by its own sample maximum. Electric generation firms ramped up emissions somewhat in late 1999 and then in earnest in 2000. Non-electric firms responded by cutting emissions dramatically from the third quarter of 2000 through 2001, with a more modest decline afterwards.

In effect, the Crisis hastened firms' long-run adaptation to a binding cap, causing a sudden and permanent drop in emissions. In general, a firm can reduce its emissions by either lowering production or altering the production process itself, usually by installing equipment which removes NO_x from its combustion exhaust before it reaches the outside air. And while the Crisis was temporary, RECLAIM's binding cap was not, meaning firms had a strong incentive to make long-term adjustments. This is why the temporary Crisis caused the permanent drop in pollution seen in Figures 4 and 5.

This sudden, permanent drop in emissions can be used to construct a set of instruments for local residents' exposure to firms' pollution. When faced with high RTC prices, firms with more emissions had a larger incentive to cut emissions, so the Crisis should have had a larger effect on houses downwind of these firms. We can use a house's pre-Crisis exposure to gauge how the Crisis changed its exposure 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 interaction of aermod_pre_i and δ_y, a dummy variable for year y, captures the differential effect of the Crisis on house i in year y. The full set of these interactions $\text{aermod\_pre}_i \times \delta_y$, which I will refer to as the "annual" set of instruments, captures the differential effect of the Crisis on exposure across space and over time.

Similarly, a single interaction, $\text{post}_t = \mathbf{1}\{y \geq 2001\}$, can be used to form a single instrument that I will refer to as the "post" instrument. This instrument, $\text{aermod\_pre}_i \times \text{post}_t$, is the equivalent of a difference-in-difference estimate with variable treatment intensity. While it is coarser than the set of annual instruments, it allows us to summarize the reduced form and first stage effects of the Crisis conveniently with a single number.

The critical identification assumption behind these instruments is that there are no contemporaneous 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 also might have been more affected by the Crisis. 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. Another potential problem is that the instruments might be correlated with changes NO_x from cars. This would bias second-stage estimates upward if industrial exposure were correlated with car 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; Anderson 2015). Furthermore, traffic data show that no significant change in driving patterns coincided with the Crisis.[26]

4.2 Using the Chemistry of NO_x and Ozone

Several characteristics of NO_x and ozone make them ideal for identifying how much buyers depend on the visibility of pollutants in their decision making.

First, NO_x and ozone serve as good counterfactual chemicals for one another. They are both lung irritants, but NO_x has a reddish-brown color and noxious smell, while ozone is invisible, has no bad odor, and is far more toxic than NO_x.[27] Thus, if people respond more to NO_x, it is likely because of its greater visibility, while if people respond more to ozone, it is likely because of its greater toxicity.

Second, ozone is the product of NO_x-dependent atmospheric reactions, so the Electricity Crisis exogenously shocked people's exposure to both pollutants. NO_x, a catchall term for NO and NO_2, is emitted directly by combustion processes while ozone is created from NO_x-dependent chemical reactions. These chemical reactions also

26. 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.

27. NO_x and ozone are oxidizing agents, which react with and destroy cells in the lining of the lung, making it more difficult for the lungs to clear foreign particles and bacteria (Chitano et al. 1995).

depend on UV radiation from the sun, which varies predictably throughout the year.[28]

Third, the predictable variation in UV radiation across seasons results in different but predictable rates of NO_x-to-ozone conversion throughout the year. Table 1 shows these trends using data from pollution monitors on NO_x, ozone, and the coefficient of haze. Columns 1–3 show unconditional means and columns 4–6 show coefficients from regressions of the given pollution metric on dummy variables for each quarter, controlling for monitor-year fixed effects. Standard errors are clustered by monitor. To make comparisons easier, each variable has been standardized to have a mean of 0 and a standard deviation of 1. The patterns in both sets of statistics are the same, regardless of controls: NO_x peaks in Q4 and bottoms out in Q2, with ozone following the opposite pattern.

Because the seasonal variation in NO_x-to-ozone conversion is predictable, it can be used to test whether or not people respond to transitory changes in pollution levels as discussed in Section 3.2. This can be done by allowing the effect of aermod_{it} to differ across seasons of the year. Recall that AERMOD uses data on firms' NO_x emissions and is agnostic about how those original emissions change in the atmosphere before arriving at their final destination. This makes aermod_{it} a measure of both NO_x and ozone, depending on the season. In Q2 aermod_{it} will be primarily ozone, while in Q4 it will be primarily NO_x. A larger effect in Q4 would suggest that people respond more to the visible NO_x, while equal coefficients in all quarters would suggest that agents are not affected by salience and may have perfect information.

4.3 Estimation Strategy

To measure how prices respond to pollution exposure, I estimate the following equation:

$$\ln p_{it} = \beta \cdot \text{aermod}_{it} + \alpha_i + \delta_t + \sum_k \gamma_{1k} \cdot w_{ik} \cdot t + \sum_k \gamma_{2k} \cdot w_{ik} \cdot t^2 + \varepsilon_{it} \tag{8}$$

28. The relationship of NO_x and ozone can be summarized as two chemical processes:

$$\begin{aligned} hv + NO_2 + O_2 &\rightarrow NO + O_3 \\ NO + O_3 &\rightarrow NO_2 + O_2 \end{aligned}$$

In the first process, an ultraviolet photon (hv), an NO_2 molecule, and free oxygen (O_2) react to form ozone (O_3) and NO. In equilibrium, ozone and NO readily react with each other to reverse this process, leaving no net ozone. Disturbances to this equilibrium, like increasing UV radiation or adding volatile organic compounds (VOCs) that convert NO to NO_2, result in increased ozone levels. See Sillman (1999) for a more detailed discussion of NO_x-ozone reactions.

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.

These controls 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 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.[29]

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 11-year period centered on the Crisis: 1995–2005. RECLAIM and the data collection it required was rolled out across firms during 1994,

29. Given the large size of the sample region, the ideal geographic unit for these trends would be individual 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, shown in Figure A1, which is aligned to preserve as many city boundaries as possible. This grid results in 17 different areas that each get their own quadratic time trend.

so the first full year of reliable emissions data is 1995. I follow Fowlie, Holland, and Mansur (2012) and set the last year of my analysis to 2005. This avoids the peak and subsequent collapse of the housing bubble. However, because I use the first two years of data in constructing my instruments, the actual regression sample is limited to 1997–2005.

I restrict the region of analysis to the southwest region of SCAQMD territory, roughly between Santa Monica and Huntington Beach (see Figure 3a), to minimize measurement error due to geography. All of the major polluters are located in this region and houses farther away from the pollution sources are likely to have less actual exposure from the firms and more noise in the modeling prediction, decreasing the signal-to-noise ratio of the pollution measure. 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.[30]

Because of the previously discussed concern about omitted variables, I estimate Equation (8) using two-stage least squares (2SLS) and limited-information maximum likelihood (LIML). As discussed in Section 4.1, the main set of instruments is $\{\text{aermod\_pre}_i \times \delta_y\}_{y=1998}^{2005}$, which I refer to as the "annual" instruments and which have the following first stage:

$$\begin{aligned}\text{aermod}_{it} = &\sum_{y=1998}^{2005} \left(\text{aermod\_pre}_i \times \delta_y\right) \cdot \pi_y \\ &+ \nu_i + \nu_t + \sum_k \eta_{1k} \cdot w_{ik} \cdot t + \sum_k \eta_{2k} \cdot w_{ik} \cdot t^2 + \mu_{it} \quad (9)\end{aligned}$$

The estimates of π_y can be plotted to verify the common trends assumption that the instruments only affect aermod_{it} and $\ln p_{it}$ through the Crisis. I also use an alternative "post" instrument, $\text{aermod\_pre}_i \times \text{post}_t$ where $\text{post}_t = \mathbf{1}\{t \geq 2001\}$, instead, which treats the Crisis as a difference-in-differences with variable treatment intensity. This provides a convenient summary of the average effect of the Crisis on exposure and prices.

To test for pollutant visibility effects, I allow the effect of aermod_{it} in Equation (8)

30. 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 (see Figure A1).

to vary by quarter of year, as discussed in Section 4.2:

$$\ln p_{it} = \sum_q \beta_q \cdot (\text{aermod}_{it} \times Q_q) + \alpha_i + \delta_t + \sum_k \gamma_{1k} \cdot w_{ik} \cdot t + \sum_k \gamma_{2k} \cdot w_{ik} \cdot t^2 + \varepsilon_{it} \quad (10)$$

where Q_q is an indicator equal to 1 if t is the q-th quarter of the year.

For neighborhood sorting, I use census block groups to represent neighborhoods and adapt Equation (8) for use with the block group data:

$$y_{nt} = \text{aermod}_{nt} + \delta_n + \text{post}_t + \sum_g \delta_g \times \text{post}_t + \sum_k \gamma_k \cdot w_{k,2000} \times \text{post}_t + \varepsilon_{nt} \quad (11)$$

where n indexes block groups and y is one of many possible block group–level characteristics (e.g., log median household income). The block group data are taken from the 2000 Census and the ACS 5-year average for 2005–2009. Because there are only two years of data, the only possible time trends are a single dummy variable for "post." The 10-km grid used for localized trends is the same as described above for houses. The controls for year 2000 demographics are designed to account for possible regression to the mean in the outcomes of interest: population, number of households, log median household income, population per square mile, population over age 25, fraction with no high school diploma, fraction with high school but not college degree, fraction white (non-Hispanic), fraction Hispanic, fraction black. These variables are discussed further in Section 5.2.

5 Data

5.1 Houses

Data on home sales and housing characteristics come from county registrar and assessors' offices and were collected by DataQuick, Inc. The data include any property that has been assessed and most sales, refinances, and foreclosures in California after 1990. Data for each property includes square footage, lot size, number of bedrooms and bathrooms, and the year the property was built. Each sale or refinance 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. Latitude and longitude are also included for each property.

I drop sales that fall outside normal market transactions and which 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. I also drop a sale if the property transacted within the previous 90 days, as many of these transactions are duplicates. The sample is also restricted to homes built before 1995 to preclude direct sales from developers to consumers. The top 0.1% of sales are winsorized.

Table 2 shows average sale price, house hedonics, and quarter of sale broken down by the number of times a house transacted during the sample period. House prices are deflated to real 2014 dollars using the all-items CPI. Houses are are not used in summary statistics or regressions if they fall outside the sample region described in Section 4.3.

5.2 Census Block Groups

Data on Census block group demographics are taken from the 2000 Census and 2005–2009 5-year American Community Survey (ACS) sample. For each block group, these data include total population; white (non-Hispanic) population; Hispanic population; black population; the number of households; median of household income; median rent; and educational attainment for individuals age 25 and older. The data also include the block groups' total land area, which I use to calculate population density (population per square mile). I group educational attainment into three categories: people who did not graduate high school; people who graduated high school but do not have a bachelor's degree; and people who hold at least a bachelor's degree. To reduce noise, I drop block groups that have less than 400 people in 2000, which is roughly the 4th percentile of all block groups and constitutes less than 0.5% of all people in the sample. Table 3 presents summary statistics for both 2000 and 2005–2009.

5.3 Firms

There are several components to the firm-level data, which cover firm emissions over time, the firm's name and location, and physical characteristics of the firm's polluting equipment. The firm data also include information about RECLAIM Trading Credit (RTC) allocations and subsequent trades.

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 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, and the ID numbers of participating firms. 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 A.1 for more details).

AERMOD requires data on the physical characteristics of firms' polluting equipment (smoke stack height and diameter, and temperature and velocity of gas exiting the smoke stack), which I take from the National Emissions Inventory (NEI). 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). However, the National Emissions Inventory (NEI) has these data for many firms along with each firm's name, address, SIC, and the type of combustion process behind each stack. I matched most firms to the NEI by reconstructing the NEI-specific ID number from the SCAQMD ID number and other administrative variables, and I validated these matches using fuzzy string matching on firm names and addresses. Any remaining firms were matched via fuzzy string matching and manually checked. For firms with missing stack data, I impute values using the firm's SIC and the stack's equipment-type code (SCC). Details of the imputation process and the construction of the firm-level data in general are outlined in Appendix A.

Table 4 gives summary statistics by industry (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.

5.4 Meteorology and Pollution Monitor Data

Data on local meteorological conditions come from SCAQMD. Before building new polluting equipment, firms must submit an impact report to SCAQMD using AERMOD to show how the new equipment will impact ambient pollution levels. To facilitate the making of these reports, SCAQMD makes AERMOD-ready meteorological data available on its website.[31]

These data were gathered by 27 meteorological stations throughout SCAQMD.[32]

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

32. The location of these stations is mapped in Figure A2.

The data consist of hourly observations for temperature, mean and standard deviations of wind speed and direction at multiple altitudes, and other variables described in Section 2.2. 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.

Data from air pollution monitoring stations comes from the California Air Resources Board (CARB). They include hourly measures of ozone, NO_x, and the coefficient of haze (COH), which is a measure of visibility interference in the atmosphere. I aggregate the hourly measures to daily and then monthly averages following Schlenker and Walker (Forthcoming).

5.5 AERMOD-based Measure of Exposure

I use AERMOD, which maps firm-level output to house-level exposure, to construct a measure of a house's exposure from all industrial sources. Software for using AERMOD is available on the EPA's website and includes documentation, Fortran source code, and pre-compiled executables for Windows.[33]

As discussed in Section 2.2, house i's exposure to NO_x emissions from firm f at time t can be written $\text{NOx}_{ft} \cdot h(d_{fi}, \theta_{fi}; \boldsymbol{S}_f)$, where $\boldsymbol{S}_f$ contains information on the firm's smoke stacks, as well as local meteorological conditions. The data I use for NOx_{ft} and $\boldsymbol{S}_f$ are described in Sections 5.3 and 5.4. A firm's meteorological data is taken from the nearest meteorology monitor. The values for (d_{fi}, θ_{fi}) are calculated by AERMOD from firms and houses' latitude and longitude. AERMOD then outputs aermod_{ift}, the house's exposure to the firm's emissions. The house's total exposure to industrial NO_x emissions is simply $\text{aermod}_{it} = \sum_f \text{aermod}_{ift}$.

For block group–level exposure, I first calculate exposure at the block level, then calculate the population-weighted average for each block group. At the block level, I use the process described above for houses, substituting house-specific latitude and longitude for the Census-provided internal point for each block.[34] This is a more

33. 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.

34. Analyses using Census geographies like block groups or ZCTA's often use the "centroid" of the geography as its the representative point in space. However, the Census Bureau is very particular to note that because these geographies are not convex, the true centroid may lie outside the geography of interest. As a solution, the Census Bureau calculates "internal points," which are constrained to be inside the geography.

attractive approach than using the block group's internal point because it accounts for heterogeneity in population and exposure across the block group and is a closer approximation to the average exposure to the block group's residents.

Because AERMOD loops over all firms, houses, and meteorological data, it is very computationally intensive for such a large sample, so I impose several restrictions on the data to make calculation more feasible.[35] First, I only calculate a firm's exposure to houses that are within 20 kilometers of the 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 5.4 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 according to the grid square they occupy.

6 Results

6.1 OLS Estimates of Prices and Exposure

I start with simple OLS regressions of log house price on pollution exposure. All regressions are clustered at the 100-m grid used to calculate aermod_{it} (see Section 5.5).

Column 1 of Table 5 shows the naïve univariate regression of prices on exposure. Column 2 adds the year-quarter effects, geographic time trends, and SES time trends from Equation (8), as well as explicit hedonic controls: number of bedrooms, number of bathrooms, square footage, and lot size. Adding time trends and hedonics in Column 2 reduces the effect of aermod on prices by about 75%, suggesting that the coefficient in Column 1 is picking up the fact that houses in polluted areas have different characteristics (e.g., are smaller), and that secular trends affect both pollution and house prices. Column 3 adds block group fixed effects which further reduces the effect of exposure, suggesting that neighborhood-level characteristics are also important. Column 4, the preferred specification defined in Equation (8), trades the block-group effects and hedonics for property-specific fixed effects. The estimate is not dramatically different from Column 3, though it is slightly larger.

35. Even with these restrictions, the model takes approximately 210 CPU days to process all the data.

In aggregate, the results of Table 5 suggest that omitted variables are a potentially serious problem when measuring the effect of pollution exposure.

6.2 Event Studies around the Crisis

For an instrument based on the Crisis to yield consistent estimates of the effect of pollution exposure, the instrument should have no effect before the Crisis began. This common trends assumption requires that, had the exogenous shock not taken place, individuals of varying treatment intensity would have maintained their status quo. The common trends assumption can be assessed by plotting the event study coefficients from Equation (9), the first stage, and the analogous coefficients from the regression of price on the instruments. For the first stage, each coefficient $\hat{\pi}_y$ can be interpreted as the change in relative exposure across areas with different initial exposure levels, aermod_pre_i, relative to the difference across these areas in the omitted year, 2000. For example, if relative exposure does not change between 2000 and 2001, $\hat{\pi}_{2001} = \hat{\pi}_{2000} = 0$, since π_{2000} is omitted and thus constrained to be zero. If, on the other hand, relative exposure decreases in high aermod_pre_i areas, $\hat{\pi}_{2001}$ will be negative ($\hat{\pi}_{2001} < \hat{\pi}_{2000} = 0$).

As Figure 6 shows, it appears that the common trends assumption holds and the Crisis had a large effect on both exposure and prices. For both exposure and prices, Figure 6 shows that aermod_pre_i had no effect before the Crisis, with sharp effects afterwards, suggesting that the Crisis makes a good natural experiment and that house prices respond sharply to exposure levels. As Figure 4 and Figure 5 from Section 4.1 show, the Crisis hit most firms in mid- to late-2000. In Figure 6, we see no significant change in exposure or price between 1997 and 2000. In 2001, relative exposure suddenly drops for high aermod_pre_i properties and continues to decline slightly afterward, consistent with firms' drop in emissions. Similarly, house prices jump at the same time exposure falls and, in a noisy mirror image of the exposure trend, continue to appreciate slightly over time.

The timing of these sharp jumps immediately after the Crisis also suggests that the Crisis, and not coincidental secular trends, is driving these changes.

6.3 Instrumental Variables Estimates of Price Effects

Columns 1–3 of Table 6 present first-stage and reduced-form estimates using the "post" instrument, which provides a more concise summary of the effects of the Crisis. In order to show the main effect of aermod_pre_i on prices, Column 1 uses the same hedonics and block group fixed effects as Table 5, Column 3. The main effect of aermod_pre is -0.0029, suggesting that, on average, houses with 1 additional µg/m^3 of pre-Crisis exposure were valued 0.29% lower than comparable houses. The effect of the "post" instrument, aermod_pre $\times$ post, is 0.0033, suggesting that the value of previously high-pollution houses saw their value equalize with houses that had low pollution throughout the period.

Columns 2 and 3 show the estimated effect of the post instrument on prices and exposure, respectively, using the preferred specification with property fixed effects (see Table A1 for full output). The reduced form estimate in Column 2 is similar to that of Column 1 and shows that house prices increased 0.3% per unit of treatment intensity. At the average value of aermod_pre, this translates to a 1.7% increase in value, or $7,324 for a house of average value in 2000. Similarly, Column 3 shows that exposure decreased by 0.433 µg/m^3 NO_x/ozone for every unit of treatment intensity, or 2.24 µg/m^3 for the average value of aermod_pre_i.

Columns 4 and 5 of Table 6 present the 2SLS results using the "post" instrument, $\text{aermod\_pre}_i \times \text{post}_t$, and the annual set of instruments, $\text{aermod\_pre}_i \times \delta_y$, respectively. (The full results of Column 4 are shown in Table A1.) These results are almost identical and suggest that an additional µg/m^3 of exposure to NO_x emissions decreases the value of a house by about 0.7%. This translates to a MWTP to reduce exposure of $3,272 per µg/m^3. While not directly comparable, this figure more than covers the $PM_{2.5}$ mortality cost of $1,600 per µg/m^3 borne by a family of two adults and one child discussed in Section 2.

The 2SLS estimates do not appear to suffer from weak-instruments bias, as evidenced by the instruments' partial F statistics from the first stage and the LIML results also presented in Table 6. 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.[36] The LIML estimates in column 3 provide further evidence

36. 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 6 for comparison against the usual rule of thumb.

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). If the LIML estimates differ from 2SLS, concerns about weak instruments could be warranted. However, that does not appear to be the case here as the 2SLS and LIML estimates are virtually identical.

These estimates are corroborated by the results of block group–level regressions on monthly rent costs, which are shown in Table 7. The rent regressions are unsurprisingly noisier than those for house prices. Each block group's aermod calculation is based on the aermod value at its constituent blocks' interior points (see Section 5.5), which introduces additional noise. In addition, 20 percent of the block group data have invalid values for monthly rent and were dropped. Nevertheless, the results in Table 7 are very close to those in Table 6 though they are unsurprisingly imprecise.

The 2SLS estimates are also robust to arbitrary spatial correlation across the error terms. This is shown in Table 8 by re-estimating the standard errors from the preferred specification (Table 6, column 1) using the spatial HAC (SHAC) variance-covariance estimator of Conley (1999) and Kelejian and Prucha (2007).[37] 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.

6.4 Comparison to Standard Methods

To verify that the large MWTP estimate found in Section 6.3 is not being driven by a peculiarity of the data or natural experiment, I re-estimate MWTP using non–wind-based instruments standard in the literature. I use two standard ways of constructing an instrument based on the Crisis: geographic difference-in-differences and kernel-based measures of exposure similar to those used by Banzhaf and Walsh (2008).

37. SHAC standard errors can be thought of as an extension of Newey–West standard errors from discrete time to continuous distance. Specifically,

$$\text{Var}(\hat{\beta}) = \left(\sum_i x_i x_i'\right)^{-1} \left(\sum_i \sum_j K(d_{ij}) x_i \hat{\varepsilon}_i \hat{\varepsilon}_j x_j'\right) \left(\sum_i x_i x_i'\right)^{-1}$$

where K is some kernel and d_{ij} is some metric of the distance between units i and j.

6.4.1 Geographic Diff-in-diff and Interpolation

The first standard research design is the geographic diff-in-diff. The equation to estimate is similar to Equation (8), but each pair of house i and firm f is treated as a separate observation so that the same sale price p_{it} can appear with multiple firms:

$$\ln p_{ift} = \text{near}_{if} \times \text{post}_t \cdot \beta + \alpha_{if} + \mathbf{X}_{it}\boldsymbol{\Gamma} + \varepsilon_{ift} \tag{12}$$

where the entity fixed effects are now house-firm effects instead of house effects; $\mathbf{X}_{it}$ includes the same time and demographic controls as Equation (8); and near_{if} is a dummy variable for whether house i is within the set treatment radius of firm f.[38] I estimate this model on the full study sample twice, once with a 1-mile treatment radius and a 2-mile control, and once with a 2-mile treatment and 4-mile control.

The reduced-form estimates, shown in columns 1 and 4 of Table 9, are small, imprecise, and have different signs. For the 1-mile treatment, the average effect of the Crisis on log price is 0.0049, about one third the size of the effect estimated in Table 6, column 2, for a house of average treatment intensity (0.017). The 2-mile estimate implies that houses close to firms lost value because of the Crisis, but this estimate is also imprecise.

The derivation of geographic diff-in-diff bias in Section 2.1 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. To test this, I use 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 6, 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 6 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 10. 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

38. For a similar application, see Currie et al. (2015).

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.

6.4.2 Kernel-based Exposure

The second non–wind-based research design uses radial kernel densities to map firm emissions to local exposure. Specifically, 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. 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. 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}. Once again, the estimation equation is almost identical to Equation (8), except that the exposure measure and instruments are constructed using the relevant kernel density instead of AERMOD. Table 11 presents the results, with the triangle-based regressions in sub-table A and the uniform-based regressions in sub-table B.

The kernel-based estimates, shown in Table 11, 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 6, 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 6 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 6, but is imprecise, while the estimate in column 5 is both economically and statistically insignificant.

6.4.3 Summary and Comparison to Prior Research

Table 12 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} (see footnote 4). 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 10 because the are based on a slightly different geographic sample.

There are several points of interest in Table 12 that support the predictions made in Section 2.1 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 2 that standard methods of measuring exposure are biased, even when quasi-experiments and instrumental variables are used.

6.5 Evidence of Visibility Effects

As discussed in Section 3.2, if buyers suffer from imperfect information or salience effects, they may react to transient or foreseen changes in pollution exposure. If this is case, we should see the effect of aermod_{it} vary seasonally, with a peak in Q2 if toxicity is more important and a peak in Q4 if salience is more important. In contrast, if there are no information or salience problems, then we should see aermod$_{it}$ have a similar effect in every quarter, since exposure at any one time should not matter relative to long-term exposure.

Table 13 estimates Equation (10) which allows the effect of aermod$_{it}$ to vary by quarter. Column 1 reports the 2SLS regression using the annual set of instruments to identify the four endogenous regressors and Column 2 reports the analogous LIML estimates. In both specifications, aermod$_{it}$ has the biggest effect in Q4, consistent with a model where agents use their physical senses to detect pollution and fail to anticipate future pollution exposure when pollution is less salient. However, while the point estimate on Q2 is statistically imprecise, it is still larger than the point estimates in Table 6, which may suggest that even though ozone is not easy to see, it is so toxic that the market may still respond do it, if only partially. The fact that the Q2 effect is about half the size of the Q4 effect suggests that visibility effects dominate toxicity. The small or wrong-signed effects in Q1 and Q3, when both salience and toxicity are middling, further support the conclusion that buyers are incorrectly assessing long-run air quality.

6.6 Geographic Sorting and Home-ownership

As discussed in Section 3.3, differential sorting behavior and home-ownership rates across income groups can shed light on how the gains from the Crisis-induced air quality improvement were distributed. Such behavior would also be evidence that the price effects are driven by real changes in demand for new amenities and not changes in housing market dynamics, e.g., changes in lending standards for low-income households.

Table 14 shows estimates at the block-group level, following Equation (11), for five outcomes: log population, log number of households, population per square mile, log median household income, and the fraction of people over age 25 who did not graduate high school. Sub-table A shows results from univariate regressions of each outcome on aermod$_{nt}$. The effects on income and education are in line with income stratification and Tiebout sorting while the population results are mixed. Sub-table B adds block group fixed effects and the demographic and time controls listed in Section 4.3. These controls absorb most of the variation in local conditions that was previously picked up by exposure and all estimates are small and imprecise.

The reduced-form estimates, shown in sub-table C of Table 14, suggest that the

Crisis caused neighborhoods to become relatively richer and better educated, but less populated than they would have been otherwise. The 2SLS estimates in sub-table D unsurprisingly mirror the reduced-form estimates. This pattern is consistent with either low-income and low-education individuals leaving improved areas or fewer such individuals moving into improved areas than otherwise would have. Because of the inclusion of block group fixed effects, these coefficients can only tell us about relative changes, not absolute ones.

Table 15 shows reduced-form estimates of the Crisis' effect on log population by educational attainment, which shows that the decrease in population was indeed driven by those with low educational attainment. Column 1 shows that block groups gained 2.1% fewer low-ed residents, relative to year 2000 levels, for every unit of treatment intensity, or 13% at the sample average. These regressions, which are weighted by the block group's population in 2000 to be representative of the whole sample, imply that the sample region would have had about 60,000 more low-education residents had the Crisis not occurred. Without knowing the counterfactual net migration, however, it is impossible to say whether this is because the Crisis pushed these people out or prevented them from moving in. It may also be the case that emigrant households were homeowners who simply responded to their new choice set after the home value increased. However, this does not appear to be the case for low-income households.

The Crisis provided a windfall to incumbent homeowners which appears to have accrued primarily to individuals at the top of the income distribution. Figure 7a plots the results of a local linear regression of a block group's home-ownership rate in 2000 on its median household income in 2000, weighted by the block group's population in 2000. Unsurprisingly, the ownership rate increases with income, from around 10% at the bottom to about 95% at the top. However, poorer areas may have also experienced the largest air quality improvements since these areas were, on average, more polluted to begin with.

The dashed line in Figure 7b plots house-price windfall per capita by income, again using a local linear regression, under the assumption that all residents own their own home. As the figure shows, poorer areas did indeed see much larger gains on average: the lowest-income areas see a gain of about $3,500 per person, while the highest-income areas receive roughly $2,000. But this differential is not enough to offset the much wider gap in home-ownership rates, as shown by the solid line in Figure 7b which plots the windfall per capita for local owner-residents only. It is important to note that this does not consider the characteristics of landlords for low-income areas. It could be that the landlords themselves are also low-income, which would make the actual distribution of benefits closer to the dashed line than the solid line.

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 6.4) and also more in line with the expected health benefits (see Section 2). 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.[39]

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. 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.[40] 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.[41] Optimal subsidies for

39. 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.

40. 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). See also Footnote 2.

41. 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.

renewable energy research and electric vehicle take-up are other potential examples.

Vehicle emissions standards are yet another example of potentially undervalued policy. Since coming to light, several back-of-the-envelope estimates of the costs of Volkswagen's cheating on diesel emission tests have been put forward. The Associated Press cite a rough estimate from environmental engineers of \$40–170 million per year due to mortality (Borenstein 2015). The radio magazine *Marketplace* cites economists' rough estimates of health costs of \$80 million per year (Garrison 2015). Back-of-envelope damages based on this paper's results imply the total cheating cost in the United States alone was \$282 million per year.[42]

Consumer welfare is also affected by the fact that people sometimes have a hard time discerning between areas with clean and dirty air, which affects their valuation of homes and where they choose to live. Agents only respond to visible NO_x, not invisible ozone, even though ozone is far more toxic. This could lead to "perverse" sorting, where people with strong preferences for clean air sort into more hazardous areas because ozone-rich air still looks clean. The problem of imperfect information and/or salience could potentially be solved through a cheap informational intervention; providing neighborhood-level information about seasonal and long-term pollution trends for houses on the market could yield large welfare gains per dollar spent.

More importantly, the sorting results suggest that high-income households enjoyed more of the albeit large welfare gain than low-income ones. The large outmigration of low-education residents, coupled with their low rates of home-ownership, raises the possibility that they were even made worse off by the improvement in air quality and would have preferred it never have happened. This would imply a steep trade off between equity and efficiency, however large the efficiency gains may be.

42. To get this number, I assume that the extra Volkswagen NO_x emissions were emitted uniformly by SCAQMD firms, then multiply MWTP (\$3,272) with the resulting exposure and the households exposed.

Appendix

A Firm Data Construction

A.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]

A.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 A.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

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.

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.

A.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 A.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.

References

Aldy, Jospeh E. and W. Kip Viscusi. 2008. "Adjusting the Value of a Statistical Life for Age and Cohort Effects." *Review of Economics and Statistics* 90 (3): 573–581.

Anderson, Michael L. 2015. "As the Wind Blows: The Effects of Long-term Exposure to Air Pollution on Mortality." NBER Working Paper #21578.

Banzhaf, H. Spencer and Randall P. Walsh. 2008. "Do People Vote with Their Feet? An Empirical Test of Tiebout's Mechanism." *American Economic Review* 98 (3): 843–863.

Bartik, Timothy J. 1987. "The Estimation of Demand Parameters in Hedonic Price Models." *Journal of Political Economy* 95 (1): 81–88.

Bayer, Patrick, Nathaniel Keohane, and Christopher Timmins. 2009. "Migration and hedonic valuation: The case of air quality." *Journal of Environmental Economics and Management* 58:1–14.

Bento, Antonio, Matthew Freedman, and Corey Lang. 2015. "Who Benefits from Environmental Regulation? Evidence from the Clean Air Act Amendments." *Review of Economics and Statistics* 97 (3): 610–622.

Black, Sandra E. 1999. "Do Better Schools Matter? Parental Valuation of Elementary Education." *Quarterly Journal of Economics* 114 (2): 577–599.

Borenstein, Seth. 2015. "AP analysis: Dozens of deaths likely from VW pollution dodge." October 3. http://bigstory.ap.org/article/a6925f0af82e44aaa1a1ed4b55d030f6, *Associated Press.*

Borenstein, Severin. 2002. "The Trouble with Electricity Markets: Understanding California's Restructuring Disaster." *Journal of Economic Perspectives* 16 (1): 191–211.

Busse, Meghan R., Devin G. Pope, Jaren C. Pope, and Jorge Silva-Risso. 2012. "Projection Bias in the Car and Housing Markets." NBER Working Paper #18212.

———. 2015. "The Psychological Effect of Weather on Car Purchases." *Quarterly Journal of Economics* 130 (1): 371–414.

Cellini, Stephanie Riegg, Fernando Ferreira, and Jesse Rothstein. 2010. "The Value of School Facility Investments: Evidence from a Dynamic Regression Discontinuity Design." *Quarterly Journal of Economics* 125 (1): 215–261.

Chay, Kenneth Y. and Michael Greenstone. 2005. "Does Air Quality Matter? Evidence from the Housing Market." *Journal of Political Economy* 113 (2): 376–424.

Chitano, P., J.J. Hosselet, C.E. Mapp, and L.M. Fabbri. 1995. "Effect of oxidant air pollutants on the respiratory system: insights from experimental animal research." *European Respiratory Journal* 8:1357–1371.

Cimorelli, Alan J., Steven G. Perry, Akula Venkatram, Jeffrey C. Weil, Robert J. Paine, Robert B. Wilson, Russell F. Lee, Warren D. Peters, and Roger W. Brode. 2005. "AERMOD: A dispersion model for industrial source applications. Part I: General model formulation and boundary layer characterization." *Journal of Applied Meteorology* 44 (5): 682–693.

Coglianese, John, Lucas W. Davis, Lutz Kilian, and James H. Stock. 2015. "Anticipation, Tax Avoidance, and the Price Elasticity of Gasoline Demand." NBER Working Paper #20980.

Conley, Timothy G. 1999. "GMM estimation with cross sectional dependence." *Journal of Econometrics* 92:1–45.

Currie, Janet, Lucas Davis, Michael Greenstone, and Reed Walker. 2015. "Do Housing Prices Reflect Environmental Health Risks? Evidence from More than 1600 Toxic Plant Openings and Closings." *American Economic Review* 105 (2): 678–709.

Currie, Janet, Joshua Graff Zivin, Jamie Mullins, and Matthew Neidell. 2014. "What Do We Know About Short- and Long-Term Effects of Early-Life Exposure to Pollution?" *Annual Review of Resource Economics* 6 (1): 217–247.

Currie, Janet and Matthew Neidell. 2005. "Air pollution and infant health: What can we learn from California's recent experience?" *Quarterly Journal of Economics* 120 (3): 1003–1030.

Currie, Janet and Reed Walker. 2011. "Traffic Congestion and Infant Health: Evidence from EZPass." *American Economic Journal: Applied Economics* 3 (1): 65–90.

Davis, Lucas. 2004. "The Effect of Health Risk on Housing Values: Evidence from a Cancer Cluster." *American Economic Review* 94 (5): 1693–1704.

———. 2011. "The Effect of Power Plants on Local Housing Values and Rents." *The Review of Economics and Statistics* 93 (4): 1391–1402.

Epple, Dennis. 1987. "Hedonic Prices and Implicit Markets: Estimating Demand and Supply Functions for Differentiated Products." *Journal of Political Economy* 95 (1): 59–80.

Epple, Dennis and Holger Sieg. 1999. "Estimating Equilibrium Models of Local Jurisdictions." *Journal of Political Economy* 107 (4): 645–81.

Fowlie, Meredith, Stephen P. Holland, and Erin T. Mansur. 2012. "What Do Emissions Markets Deliver and to Whom? Evidence from Southern California's NOx Trading Program." *American Economic Review* 102 (2): 965–993.

Garrison, Mark. 2015. "What if it's not just Volkswagen?," *Marketplace.* http://www.marketplace.org/topics/sustainability/what-if-its-not-just-volkswagen. September 24.

Graff Zivin, Joshua and Matthew Neidell. 2012. "The Impact of Pollution on Worker Productivity." *American Economic Review* 102 (7): 3652–3673.

Hanna, Rema and Paulina Oliva. 2015. "The Effect of Pollution on Labor Supply: Evidence from a Natural Experiment in Mexico City." *Journal of Public Economics* 122:68–79.

Hu, Shishan, Scott Fruin, Kathleen Kozawa, Steve Mara, Suzanne E. Paulson, and Arthur M. Winer. 2009. "A wide area of air of air pollutant impact downwind of a freeway during pre-sunrise hours." *Atmospheric Environment* 43 (16): 2541–49.

Karlsson, Martin, Maike Schmitt, and Nicolas R. Ziebarth. 2015. "The Short-term Population Health Effects of Weather and Pollution." Working Paper.

Karner, Alex A., Douglas S. Eisinger, and Deb A. Niemeier. 2010. "Near-Roadway Air Quality: Synthesizing the Findings from Real-World Data." *Environmental Science & Technology* 44 (14): 5334–5344.

Kelejian, Harry H. and Ingmar R. Prucha. 2007. "HAC estimation in a spatial framework." *Journal of Econometrics* 140:131–154.

Knittel, Christopher R., Douglas L. Miller, and Nicholas J. Sanders. 2014. "Caution, drivers! Children present: Traffic, pollution, and infant health." Working Paper.

Kuminoff, Nicolai V. and Jaren C. Pope. 2014. "Do "Capitalization Effects" for Public Goods Reveal the Public's Willingness to Pay?" *International Economic Review* 55 (4): 1227–50.

Kuminoff, Nicolai V., V. Kerry Smith, and Christopher Timmins. 2013. "The New Economics of Equilibrium Sorting and Policy Evaluation Using Housing Markets." *Journal of Economic Literature* 51 (4): 1007–1062.

Linden, Leigh and Jonah E. Rockoff. 2008. "Estimates of the Impact of Crime Risk on Property Values from Megan's Laws." *American Economic Review* 98 (3): 1103–1127.

Lleras-Muney, Adriana. 2010. "The Needs of the Army: Using Compulsory Relocation of the Military to Estmate the Effect of Air Pollutants on Children's Health." *Journal of Human Resources* 45 (3): 549–590.

Luechinger, Simon. 2014. "Air pollution and infant mortality: A natural experiment from power plan desulfurization." *Journal of Health Economics* 37:219–231.

Mian, Atif and Amir Sufi. 2009. "The Consequences of Mortgage Credit Expansion: Evidence from the US Mortgage Default Crisis." *Quarterly Journal of Economics* 124 (4): 1449–1496.

Miguel, Edward and Michael Kremer. 2004. "Worms: Identifying Impacts on Education and Health in the Presence of Treatment Externalities." *Econometrica* 72 (1): 159–217.

Montiel Olea, Jose Luis and Carolin Pflueger. 2013. "A Robust Test for Weak Instruments." *Journal of Business and Economic Statistics* 31 (3): 358–369.

Moretti, Enrico and Matthew Neidell. 2011. "Pollution, Health, and Avoidance Behavior: Evidence from the Ports of Los Angeles." *Journal of Human Resources* 46 (1): 154–75.

Neidell, Matthew. 2009. "Information, Avoidance Behavior, and Health: The Effect of Ozone on Asthma Hospitalizations." *Journal of Human Resources* 44 (2): 450–478.

Neidell, Matthew J. 2004. "Air pollution, health, and socio-economic status: the effect of outdoor air quality on childhood asthma." *Journal of Health Economics* 23 (6): 1209–1236.

Palmquist, Raymond B. 2005. "Property Value Models." Chap. 16 in *Handbook of Environmental Economics,* edited by Karl-Göran Mäler and Jeffery R. Vincent, 2:763–819. Amsterdam: North Holland.

Perry, Steven G., Alan J. Cimorelli, Robert J. Paine, Roger W. Brode, Jeffrey C. Weil, Akula Venkatram, Robert B. Wilson, Russell F. Lee, and Warren D. Peters. 2005. "AERMOD: A dispersion model for industrial source applications. Part II: Model performance against 17 field study databases." *Journal of Applied Meteorology* 44 (5): 694–708.

Pope, C. Arden, Richard T. Burnett, Michael J. Thun, Eugenia E. Calle, Daniel Krewski, Kazuhiko Ito, and George D. Thurston. 2002. "Lung Cancer, Cardiopulmonary Mortality, and Long-term Exposure to Fine Particulate air Pollution." *Journal of the American Medical Association* 287 (6): 1132–1141.

Pope, Devin G., Jaren C. Pope, and Justin R. Sydnor. 2014. "Focal Points and Bargaining in Housing Markets." Working paper.

Pope, Jaren C. 2008. "Fear of crime and housing prices: Household reactions to sex offender registries." *Journal of Urban Economics* 64:601–614.

Pouliot, Guillame. 2015. "Spatial Econometrics for Misaligned Data." Working Paper.

Ridker, Ronald G. and John A. Henning. 1967. "The Determinants of Residential Property Values with Special Reference to Air Pollution." *Review of Economics and Statistics* 49 (2): 256–257.

Roback, Jennifer. 1982. "Wages, Rents, and the Quality of Life." *Journal of Political Economy* 90:1257–78.

Rosen, Sherwin. 1974. "Hedonic Prices and Implicit Markets: Product Differentiation in Pure Competition." *Journal of Political Economy* 82 (1): 34–55.

Schlenker, Wolfram and W. Reed Walker. Forthcoming. "Airports, Air Pollution, and Contemporaneous Health." *Review of Economic Studies.*

Sillman, Sanford. 1999. "The relation between ozone, NO_x, and hydrocarbons in urban and polluted rural environments." *Atmospheric Environment* 33:1821–1845.

Smith, V. Kerry and Ju-Chin Huang. 1995. "Can Markets Value Air Quality? A Meta-Analysis of Hedonic Property Value Models." *Journal of Political Economy* 103 (1): 209–227.

South Coast Air Quality Management District. 2000. *Review of RECLAIM Findings.* Internal Report, October 20.

———. 2015a. Public Records Requests #80085 and #80086.

———. 2015b. Correspondence in reply to Public Records Request #80089.

Stock, James H., Jonathan H. Wright, and Motohiro Yogo. 2002. "A Survey of Weak Instruments and Weak Identification in Generalized Method of Moments." *Journal of Business and Economic Statistics* 20 (4): 518–529.

Stock, James H. and Motohiro Yogo. 2002. "Testing for Weak Instruments in Linear IV Regression." NBER Technical Working Paper #284.

Tiebout, Charles M. 1956. "A Pure Theory of Local Expenditures." *Journal of Political Economy* 64 (5): 416–424.

U.S. National Archives and Records Administration. 2004. *Code of Federal Regulations.* Title 40. Protection of Environment.

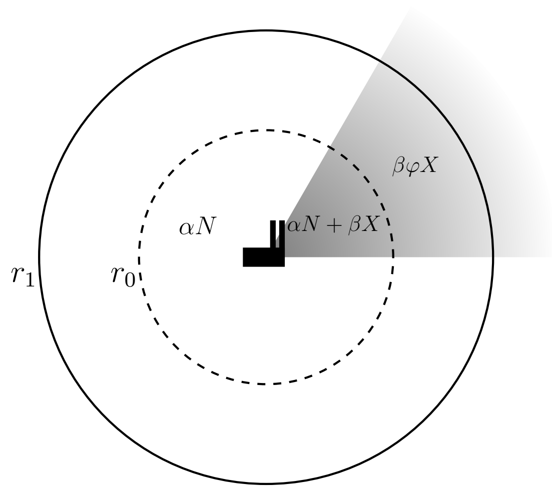
Walker, W. Reed. 2013. "The Transitional Costs of Sectoral Reallocation: Evidence from the Clean Air Act and the Workforce." *Quarterly Journal of Economics* 128 (4): 1787–1835.

Weare, Christopher. 2003. *The California Electricity Crisis: Causes and Policy Options.* San Fransisco, CA: Public Policy Institute of California.

Woodruff, Tracey J., Jennifer D. Parker, and Kenneth C. Schoendorf. 2006. "Fine Particulate Matter ($PM_{2.5}$) Air Pollution and Selected Causes of Postneonatal Infant Mortality in California." *Environmental Health Perspectives* 114 (5): 786–790.

Figures and Tables

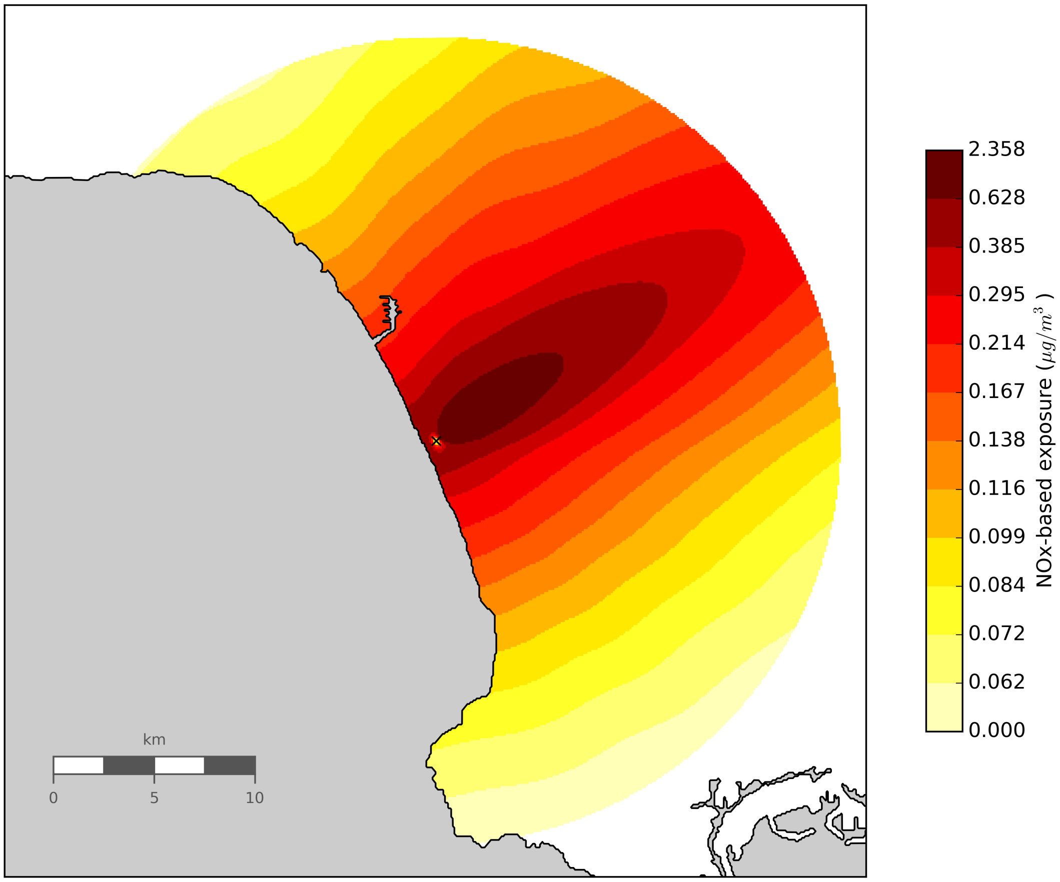
Figure 1: Geographic Diff-in-diff with Wind



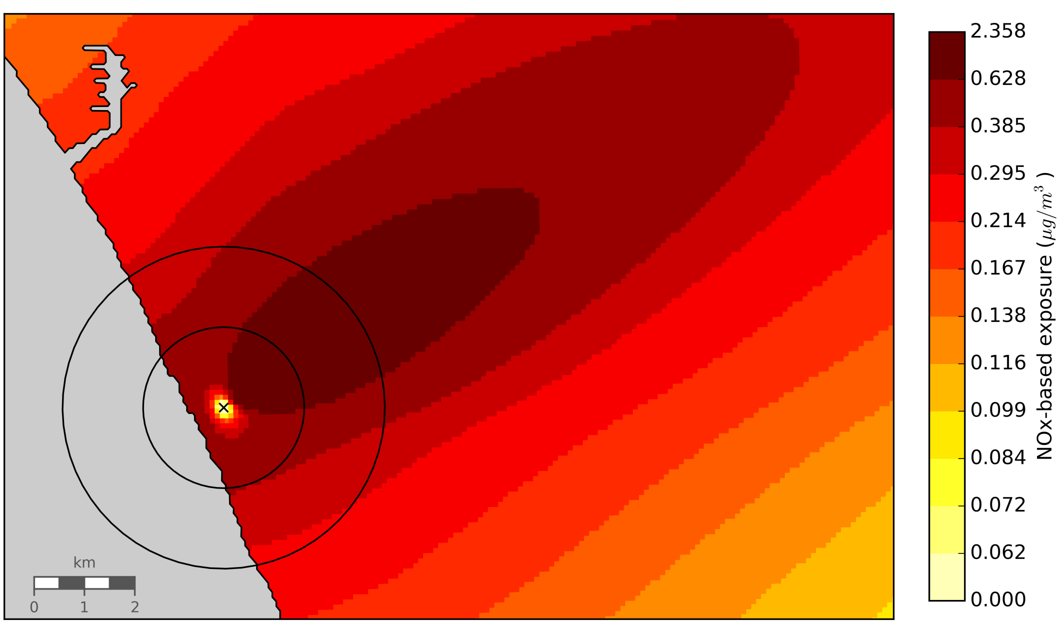
Notes: Dashed circle denotes boundary of geographic diff-in-diff's treatment group, solid circle denotes boundary of control group and sample. Shaded area is the true treatment area downwind. Values r_0 and r_1 are treatment and control radii. Other values are reduced form effects of the firm, see Equation (1) and Section 2.1.

Figure 2: Exposure due to Scatterwood Generating Station, 1999

(a) All land within 20 km



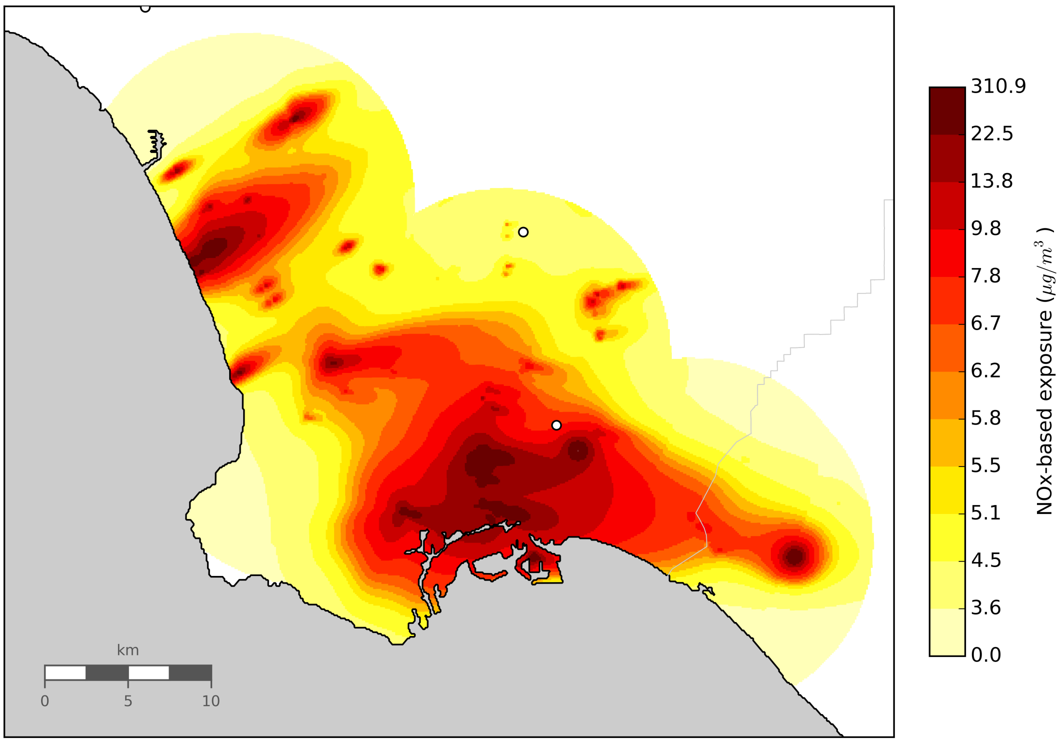
(b) Zoomed



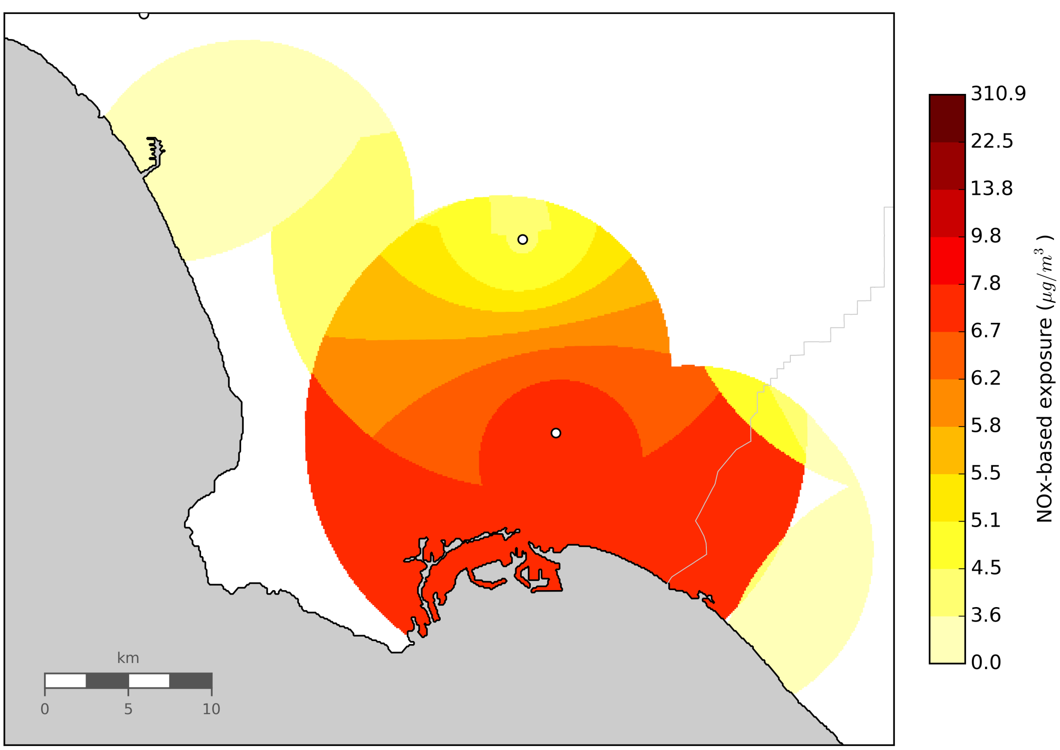
Notes: The color of each square is determined by the average aermod exposure due to the Scatterwood Generating Station in 1999. Each plotted square is 100 meters wide. Breaks in the color scale are set at order statistics of the plotted sample: minimum, 1–9th deciles, the 95th and 99th percentiles, and the maximum.

Figure 3: Total Exposure in Sample Region, 1999

(a) AERMOD-based Exposure

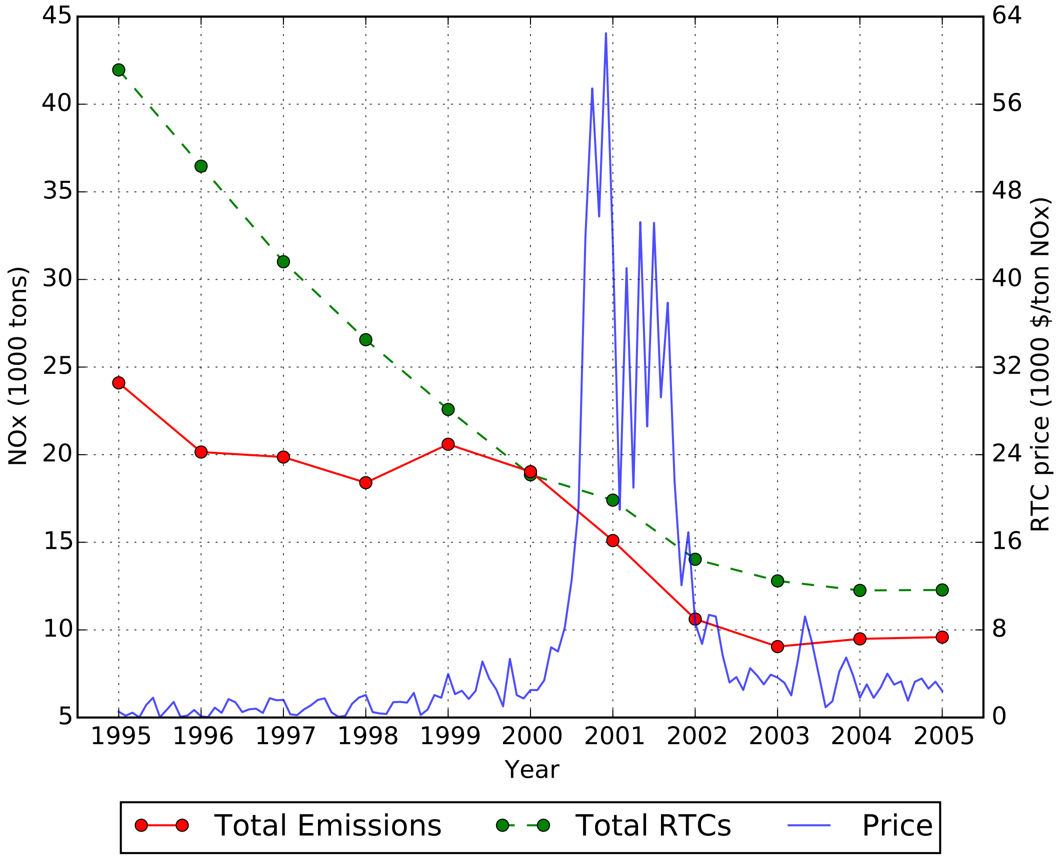


(b) Interpolated (IDW) AERMOD-based Exposure from Monitor Locations



Notes: Sub-figure (a) plots the average exposure due to all firms. Sub-figure (b) plots the average exposure as interpolated from monitor locations marked with black circles. Breaks in the color scale are set at order statistics of the plotted sample in sub-figure (a): minimum, 1–9th deciles, the 95th and 99th percentiles, and the maximum. Each plotted square is 100 meters wide.

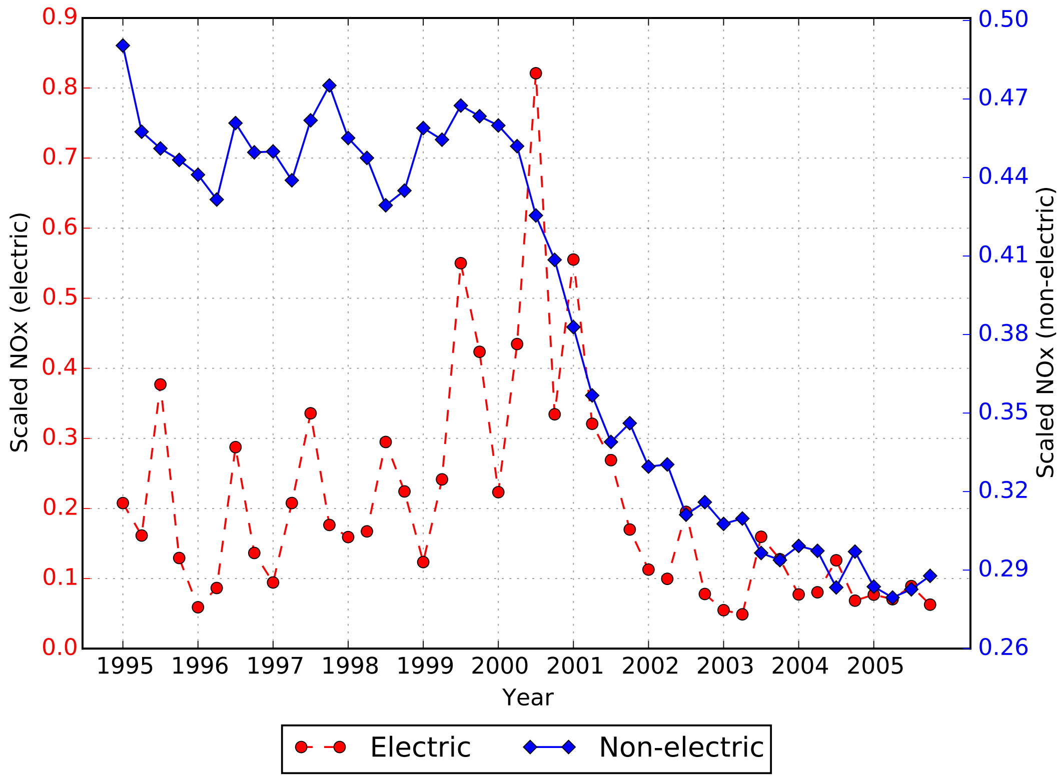
Figure 4: RECLAIM Market



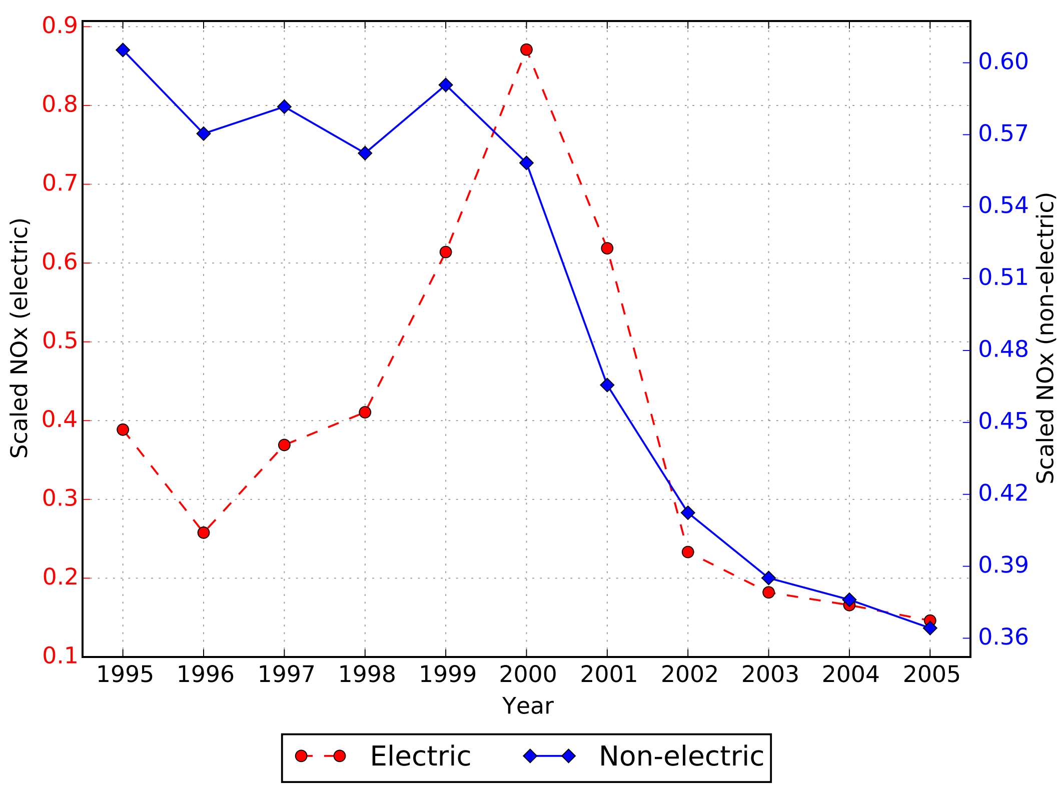
Notes: "Total RTCs" is the number of RTCs expiring in the calendar year. "Price" is the average of all arms-length transactions in a month across all RTC vintages.

Figure 5: Scaled Emissions by Firm Type

(a) Quarterly

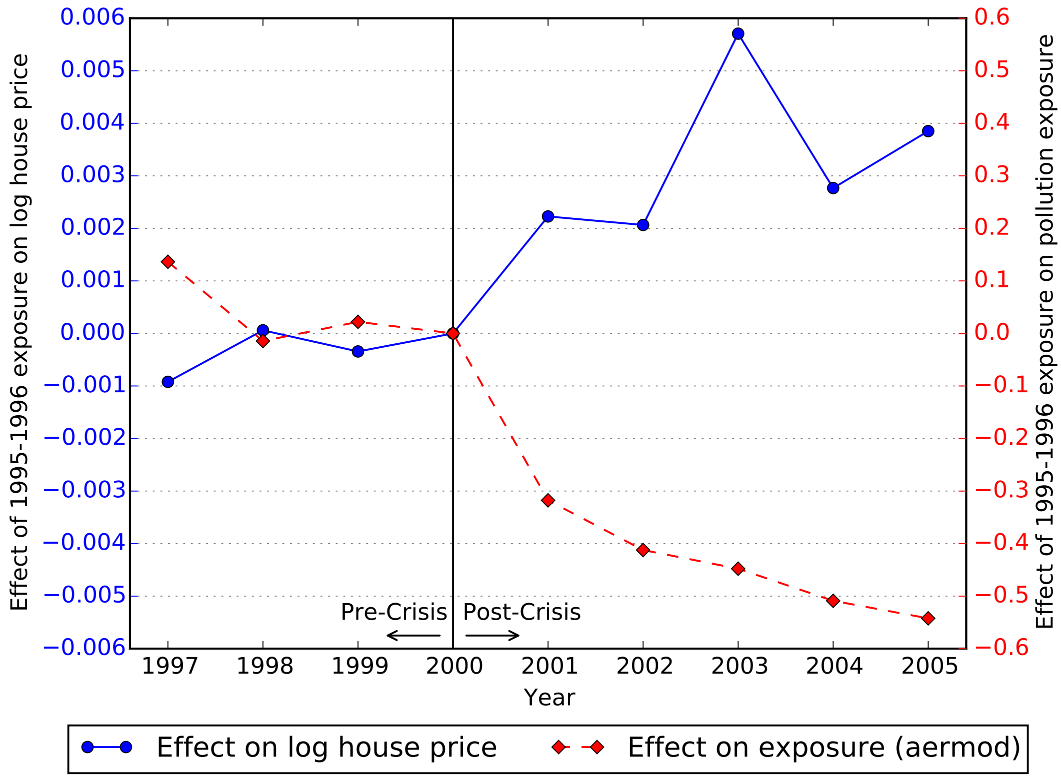


(b) Annual



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

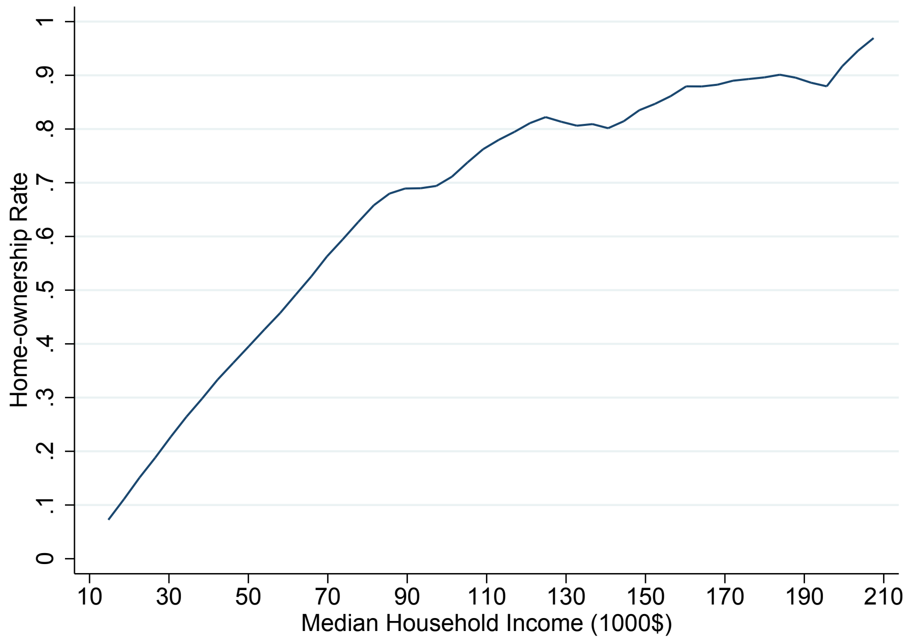
Figure 6: Pollution Exposure and House Prices around the Crisis



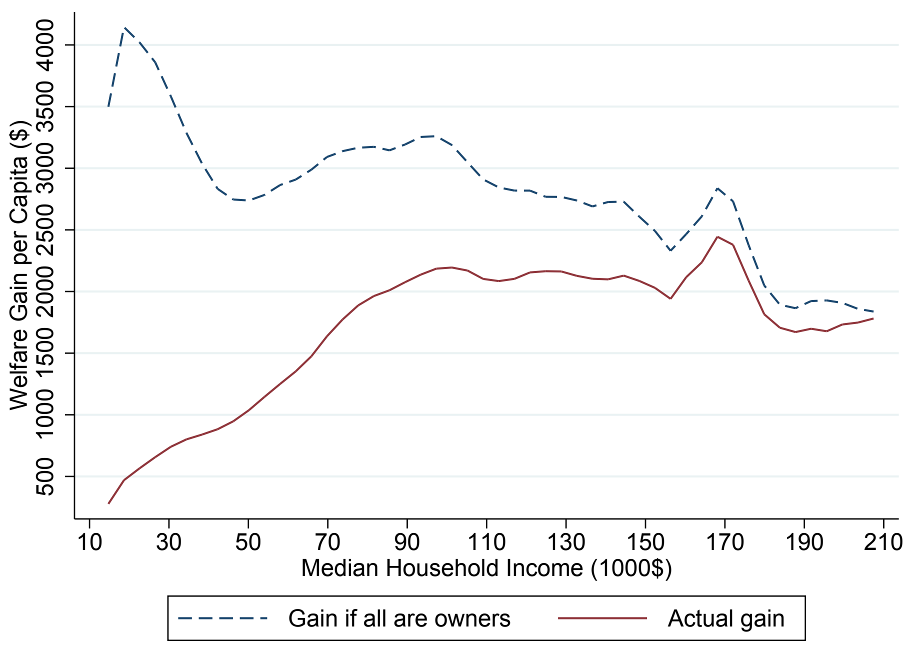
Notes: Plotted points are coefficients from a regression of the specified outcome on the "annual" instruments: aermod_pre interacted with year dummies. See Equation (9). Here, the year of the Crisis, 2000, is the omitted group. Sample and other controls as in Table 6, columns 2–6. aermod_pre is the average of the aermod exposure variable for 1995 and 1996. Average value of aermod_pre is 5.172.

Figure 7: Home-ownership and Price Windfall by Income

(a) Home-ownership rate



(b) House Price Windfall



Notes: Plots are the result of local linear regressions using an Epanechnikov kernel with bandwith of 5. Sample is Census 2000 block groups, weighted by population. In subplot B, the dashed line is the gain to owners of units occupied by a household with the given income, and the solid line is the gain to residents.

Table 1: Seasonal Trends in Pollution

	Unconditional Mean			Regression		
	Haze	NO_x	Ozone	Haze	NO_x	Ozone
Q1	0.200	0.359	-0.626			
Q2	-0.616	-0.561	0.701	-0.805***	-0.904**	1.312***
				[0.188]	[0.292]	[0.166]
Q3	-0.329	-0.448	0.467	-0.522**	-0.797**	1.084**
				[0.183]	[0.285]	[0.306]
Q4	0.748	0.654	-0.545	0.552**	0.295**	0.084
				[0.154]	[0.087]	[0.094]

Notes: N=499. Data are monthly averages of hourly readings from the 6 monitors in and near SCAQMD that had readings for all three pollution measures. Each cell is the raw mean of the measure in a quarter or the conditional mean calculated from a regression of the pollution measure on quarter dummies and monitor-year dummies. Sample period is 1991–1997; all monitors have at least 82 of 84 possible monthly observations. Pollution measures have been standardized to have mean 0, standard deviation 1. "Haze" is the coefficient of haze. Standard errors, clustered by monitor, in brackets: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 2: 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 3: Block Group Summary Statistics

	Total		Mean	
	2000	2005/9	2000	2005/9
Population	2,775,700	2,811,468	1,435	1,454
			(814)	(867)
Households	950,591	952,008	492	492
			(322)	(332)
Pop. Density (pop/mi^2)			13,423	13,518
			(8,389)	(8,815)
Household Income (BG Median)			49,292	64,211
			(23,411)	(32,920)
Population over age 25	1,717,881	1,796,814	888	929
			(505)	(564)
Educational Attainment (count)				
Less than High School	458,399	384,055	237	199
			(221)	(209)
High School Grad	830,050	895,603	429	463
			(269)	(294)
More than High School	429,432	517,156	222	267
			(262)	(312)
Educational Attainment (fraction)				
Less than High School			0.28	0.22
			(0.22)	(0.19)
High School Grad			0.48	0.50
			(0.13)	(0.14)
More than High School			0.24	0.28
			(0.19)	(0.21)
Race/Ethnicity (count)				
White (non-Hispanic)	852,136	787,815	441	407
			(468)	(466)
Hispanic	1,030,236	1,147,634	533	593
			(546)	(601)
Black	507,488	468,462	262	242
			(380)	(378)
Race/Ethnicity (fraction)				
White (non-Hispanic)			0.34	0.32
			(0.31)	(0.30)
Hispanic			0.34	0.38
			(0.26)	(0.28)
Black			0.19	0.17
			(0.25)	(0.24)

Notes: Number of block groups is 1,934. Block groups with fewer than 400 people in 2000 are excluded from regression sample and so are excluded here. Data for 2000 comes from the 2000 Census. Data for 2005/9 comes from the 2005–2009 ACS 5-year sample and is labeled "2005" elsewhere. All educational attainment variables are restricted to people who are at least 25 years old. Income is denominated in nominal dollars. Standard deviations in parentheses.

Table 4: 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 5: Pollution's effect on House Price, OLS

	(1)	(2)	(3)	(4)
Aermod	-0.0507***	-0.0139***	-0.0030***	-0.0033***
	[0.0019]	[0.0009]	[0.0006]	[0.0010]
Controls	N	Y	Y	Y
Fixed Effects	None	None	BG	House
R^2	0.0461	0.7678	0.8649	0.9483
N	118,522	118,522	118,522	41,771

Notes: Outcome variable is ln house price. Controls include year-quarter effects, quadratic time trends by local geography and year 2000 SES variables, and hedonics: lot size, bedrooms, bathrooms, square feet. 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 6: 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	0.0033***	0.0032***	-0.4328***			
	[0.0005]	[0.0008]	[0.0748]			
Aermod_pre	-0.0029**					
	[0.0012]					
Fixed Effects	BG	House	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	0.865	0.948	0.911			
N	118,522	41,771	41,771	41,771	41,771	41,771

Notes: Sample average of aermod_pre is 5.172. In addition to fixed effects, controls include year-quarter effects and quadratic time trends by local geography and year 2000 SES variables (see Section 4.3). For full output of columns 2–4, see Table A1. Column 1 also includes the following hedonic controls: lot size, bedrooms, bathrooms, square feet. "Post" IV is aermod_pre × post, "Annual" IV is aermod_pre interacted with year dummies. First-stage F stat assumes homoskedasticity. 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 7: Effect on Block Group Median Monthly Rent

	(1) ln Rent	(2) ln Rent	(3) Aermod	(4) Aermod	(5) ln Rent	(6) ln Rent
Aermod_pre×post	0.0022	0.0026	-0.2518***	-0.2489***		
	[0.0015]	[0.0018]	[0.0181]	[0.0227]		
Aermod					-0.0089	-0.0106
					[0.0061]	[0.0072]
Method	OLS	OLS	OLS	OLS	2SLS	2SLS
Weighted by Pop.		X		X		X
R^2	0.9163	0.9331	0.9881	0.9883		

Notes: N=3,162. Excluded instrument in 2SLS regressions is aermod_pre × post. Rents with error codes ($0) or top codes ($2,001) are dropped from the sample. Sample and controls are otherwise the same as in Table 14, plus an interaction of median rent in 2000 with post. Sample average of aermod_pre is 6.455. Standard errors, clustered by tract, in brackets: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 8: Robustness to Spatially Correlated Error Terms

	Std. Err.	p-value
Baseline (clustered)	0.0024	0.0022
SHAC by Bandwidth (m)		
200	0.0025	0.0036
400	0.0028	0.0086
600	0.0031	0.0178
800	0.0033	0.0279
1000	0.0035	0.0362
1200	0.0036	0.0414
1400	0.0037	0.0452
1600	0.0037	0.0480

Notes: N=41,771. Each row re-estimates the standard error of aermod in the 2SLS regression in Table 6, column 4 using the non-parametric Spatial HAC (SHAC) method of Conley (1999) and Kelejian and Prucha (2007). Kernel used is a triangle with the listed bandwidth. Clustered standard error from baseline regression is given on the first row.

Table 9: 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	0.0049	-0.4991***		-0.0011	0.0237	
	[0.0049]	[0.0566]		[0.0023]	[0.0222]	
Aermod			-0.0098			-0.0461
			[0.0098]			[0.1029]
Method	OLS	OLS	2SLS	OLS	OLS	2SLS
R^2	0.9453	0.9095		0.9416	0.9104	
N	92,807	92,807	92,807	430,836	430,836	430,836

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 at 100-m grid, in brackets: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 10: Price Effects with Geographic Diff-in-diff and Interpolation

	A. 1-mile treatment, 2-mile control						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	ln Price	NO_x	ln Price	ln Price	Ozone	ln Price	ln Price
Near×post	0.0056	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 subtable is 50,746; 264,234; and 423,945, 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. 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 11: Price Effects with Kernel-defined Instruments and Exposure

	A. Triangle Kernel (5-km band)				
	(1)	(2)	(3)	(4)	(5)
	ln Price	Aermod	Triangle	ln Price	ln Price
Triangle_pre× post	-0.0002	-0.1005***	-0.3830***		
	[0.0007]	[0.0090]	[0.0113]		
Aermod				0.0021	
				[0.0071]	
Triangle					0.0006
					[0.0019]
Method	OLS	OLS	OLS	2SLS	2SLS
R^2	0.948	0.888	0.932		

	B. Uniform Kernel (2-km band)				
	(1)	(2)	(3)	(4)	(5)
	ln Price	Aermod	Uniform	ln Price	ln Price
Uniform_pre× post	0.0001	-0.0479***	-0.4071***		
	[0.0003]	[0.0064]	[0.0212]		
Aermod				-0.0026	
				[0.0071]	
Uniform					-0.0003
					[0.0008]
Method	OLS	OLS	OLS	2SLS	2SLS
R^2	0.948	0.888	0.906		

Notes: N=41,771. Sample average of triangle_pre is 2.303. Sample average of uniform_pre is 1.683. Controls include house effects, year-quarter effects, and local quadratic time trends. 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 12: 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. Significance levels taken from original sources: ** $p < .05$, *** $p < .01$

Row 1: Table 9, col 1
Row 2: Table 9, col 4
Row 3: Table 11A, cols 1 & 5
Row 4: Table 11B, cols 1 & 5
Row 5: Smith and Huang (1995), abstract, meta-analysis
Row 6: Smith and Huang (1995), abstract, meta-analysis
Row 7: Chay and Greenstone (2005), Table 5A, col 4
Row 8: Bayer, Keohane, and Timmins (2009), Table 6, col 2
Row 9: Bayer, Keohane, and Timmins (2009), Table 6, col 4; accounts for moving costs
Row 10: Table 6, cols 2 & 4

Table 13: Exposure's Effect on House Price by Quarter, 2SLS

	(1)	(2)
Aermod×Q1	-0.005	-0.005
	[0.051]	[0.052]
Aermod×Q2	-0.018	-0.019
	[0.014]	[0.014]
Aermod×Q3	0.015	0.015
	[0.012]	[0.012]
Aermod×Q4	-0.033**	-0.033**
	[0.016]	[0.017]
Method	2SLS	LIML
κ	1	1.0001
Test for Equality (p-value)		
Q4=Q1	0.651	0.657
Q4=Q2	0.537	0.541
Q4=Q3	0.063	0.065

Notes: N=41,721. Outcome variable is ln house price. Controls include house effects, year-quarter effects, and local quadratic time trends. Excluded instruments are aermod_pre interacted with year dummies. Standard errors, clustered at 100-m grid, in brackets: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 14: Effect of Pollution on Block Group Demographics

	ln Pop.	ln H-holds	Pop/mi^2	ln Income	% No HS
			A. Naïve OLS		
Aermod	0.003	0.004**	-8.668	-0.101***	0.027***
	[0.002]	[0.002]	[36.865]	[0.008]	[0.003]
			B. OLS with Controls		
Aermod	-0.002	0.001	-50.478	0.003	0.002
	[0.005]	[0.005]	[80.337]	[0.004]	[0.001]
			C. Reduced Form		
Aermod_pre×post	-0.004**	-0.004*	-79.998**	0.004*	-0.003***
	[0.002]	[0.002]	[39.863]	[0.003]	[0.001]
			D. 2SLS		
Aermod	0.017*	0.016*	322.427*	-0.018	0.011***
	[0.009]	[0.010]	[169.796]	[0.011]	[0.004]

Notes: N=3,868. Sample periods are 2000 and 2005–2009 using data from the 2000 Census and 2005–2009 ACS, respectively. Regressions include block group fixed effects and 10-km grid–post dummies. Year-2000 demographic controls, interacted with "post", include: population, number of households, population per square mile, ln median household income, number of people at least 25 years old, fraction without a high school diploma, fraction with diploma but no college, fraction with at least some college, fraction white (non-hispanic), fraction hispanic, fraction black. All educational attainment variables are restricted to the sample of people who are at least 25 years old. Block groups with fewer than 400 people in 2000 are dropped. Sample average of aermod_pre is 6.224. Standard errors, clustered by tract, in brackets: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

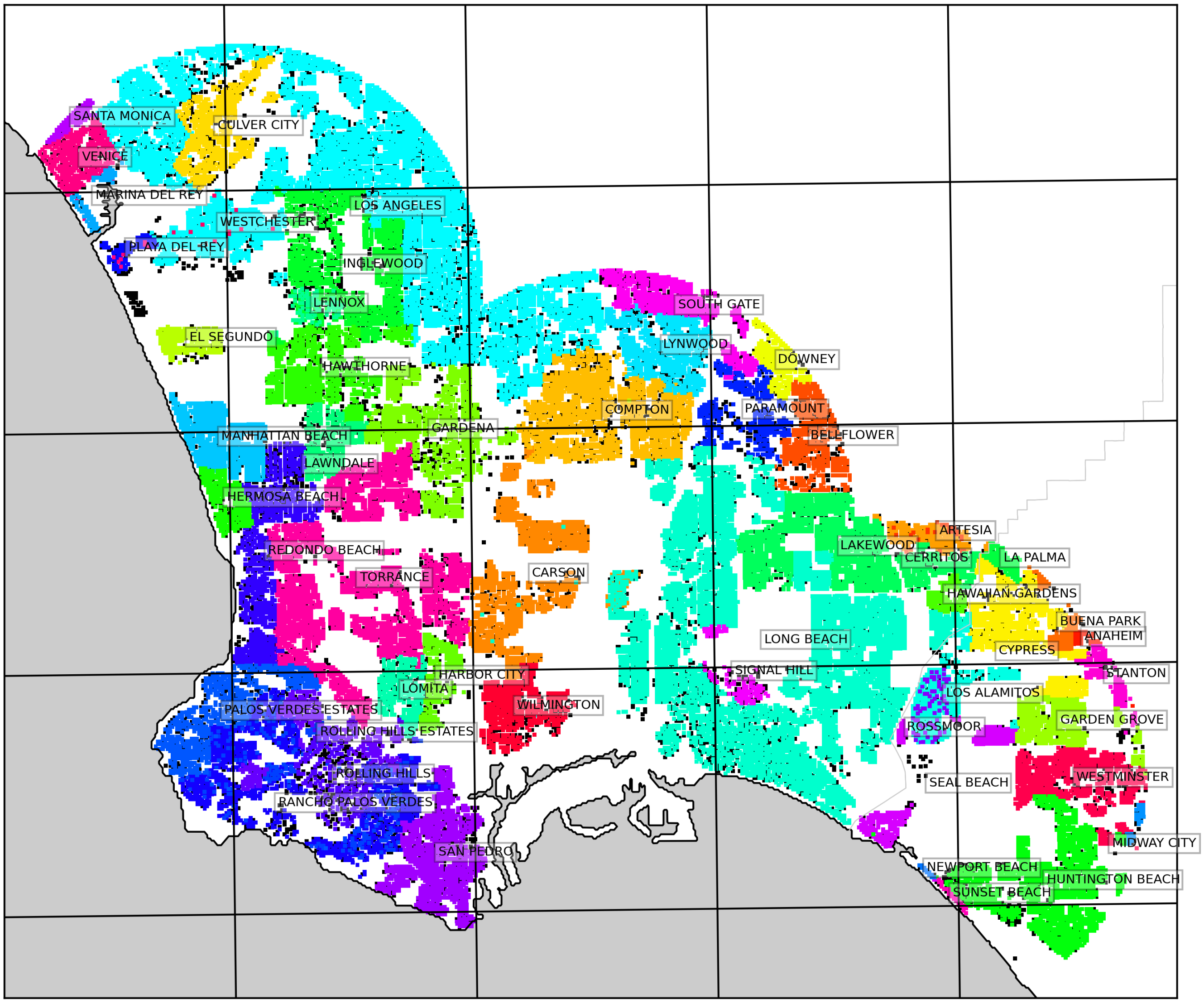
Table 15: Change in Population by Educational Attainment

	(1) ln Less than HS	(2) ln High School	(3) ln More than HS
Aermod_pre×post	-0.021***	0.005*	-0.002
	[0.005]	[0.002]	[0.004]
R^2	0.982	0.995	0.980
N	3,588	3,864	3,714

Notes: Outcome is the log of the number of people with the given educational attainment who are at least 25 years old. Block groups with an undefined logarithm in either year are dropped. Regressions weighted by block group population in 2000. Otherwise, sample and controls are the same as in Table 14. Sample average of aermod_pre is 6.225. Standard errors, clustered by tract, in brackets: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

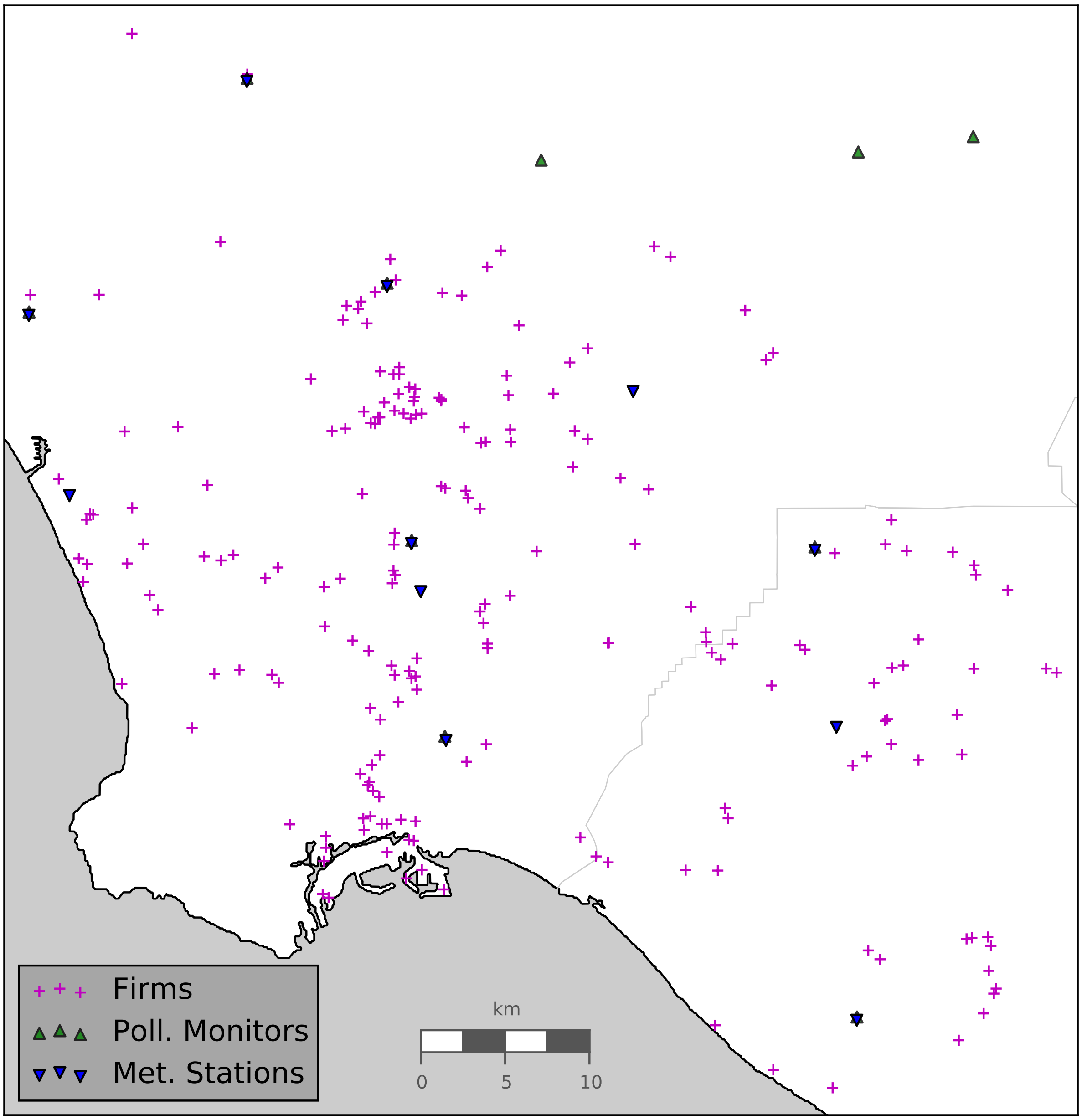
Appendix Figures and Tables

Figure A1: Cities in Sample Area with 10-km Grid



Notes: Colors denote parcels belonging to different cities. Black parcels have no city data.

Figure A2: 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: Pollution and House Prices, Full Diff-in-diff Results

	(1) ln Price	(2) Aermod	(3) ln Price
Aermod_pre×post	0.0032***	-0.4328***	
	[0.0008]	[0.0748]	
Aermod			-0.0073***
			[0.0024]
Year-Quarter Effects			
1997Q2	0.0712	-0.3588	0.0686
	[0.0681]	[0.4045]	[0.0679]
1997Q3	0.1384	0.4692	0.1419
	[0.1310]	[0.7743]	[0.1309]
1997Q4	0.2033	-0.7367	0.1979
	[0.1885]	[1.1304]	[0.1882]
1998Q1	0.2578	-0.5633	0.2536
	[0.2447]	[1.4530]	[0.2444]
1998Q2	0.3393	-2.9441*	0.3177
	[0.2978]	[1.7688]	[0.2973]
1998Q3	0.4252	-0.6891	0.4201
	[0.3466]	[2.0542]	[0.3462]
1998Q4	0.4730	-0.6668	0.4681
	[0.3920]	[2.3262]	[0.3916]
1999Q1	0.5225	-1.9205	0.5084
	[0.4344]	[2.5840]	[0.4339]
1999Q2	0.5827	-2.0290	0.5678
	[0.4728]	[2.8160]	[0.4722]
1999Q3	0.6537	-0.8812	0.6473
	[0.5086]	[3.0291]	[0.5082]
1999Q4	0.6790	-1.1694	0.6704
	[0.5420]	[3.2295]	[0.5416]
2000Q1	0.7352	-2.1188	0.7197
	[0.5700]	[3.4038]	[0.5694]
2000Q2	0.7930	-2.2088	0.7767
	[0.5957]	[3.5583]	[0.5951]
2000Q3	0.8422	-1.7783	0.8291
	[0.6178]	[3.7013]	[0.6172]
2000Q4	0.8985	-2.7325	0.8784
	[0.6366]	[3.8267]	[0.6360]
2001Q1	0.9363	-0.2790	0.9342
	[0.6529]	[3.9004]	[0.6526]
2001Q2	0.9765	-1.0513	0.9687
	[0.6657]	[3.9849]	[0.6654]
2001Q3	1.0221	-0.9022	1.0154
	[0.6758]	[4.0596]	[0.6754]
2001Q4	1.0844	-1.3753	1.0743
	[0.6822]	[4.1135]	[0.6819]

2002Q1	1.1269	-1.0652	1.1191
	[0.6865]	[4.1590]	[0.6862]
2002Q2	1.2030*	-1.3092	1.1934*
	[0.6878]	[4.1862]	[0.6875]
2002Q3	1.2714*	-0.9633	1.2643*
	[0.6870]	[4.2010]	[0.6868]
2002Q4	1.3142*	-0.7296	1.3088*
	[0.6829]	[4.2021]	[0.6828]
2003Q1	1.3676**	-0.5854	1.3633**
	[0.6768]	[4.1923]	[0.6768]
2003Q2	1.4447**	-0.6273	1.4401**
	[0.6689]	[4.1759]	[0.6689]
2003Q3	1.4980**	0.0445	1.4983**
	[0.6587]	[4.1541]	[0.6589]
2003Q4	1.5687**	-0.0061	1.5687**
	[0.6479]	[4.1249]	[0.6481]
2004Q1	1.6395***	0.3494	1.6421***
	[0.6351]	[4.0926]	[0.6355]
2004Q2	1.7394***	0.5440	1.7434***
	[0.6225]	[4.0642]	[0.6230]
2004Q3	1.7928***	1.2637	1.8021***
	[0.6095]	[4.0432]	[0.6101]
2004Q4	1.8183***	1.4946	1.8293***
	[0.5975]	[4.0220]	[0.5982]
2005Q1	1.8874***	2.0931	1.9028***
	[0.5876]	[4.0156]	[0.5884]
2005Q2	1.9504***	2.1437	1.9661***
	[0.5793]	[4.0313]	[0.5801]
2005Q3	1.9984***	2.8258	2.0191***
	[0.5752]	[4.0567]	[0.5761]
2005Q4	2.0246***	3.4054	2.0496***
	[0.5762]	[4.1181]	[0.5771]
Demographic Time Trends			
Loan-to-Value Ratio$\times t$	0.0312	0.2497	0.0330
	[0.0305]	[0.1623]	[0.0304]
Loan-to-Value Ratio$\times t^2$	0.0003	-0.0222	0.0001
	[0.0032]	[0.0170]	[0.0032]
Interest Rate$\times t$	-0.0368	0.2035	-0.0353
	[0.0311]	[0.1916]	[0.0310]
Interest Rate$\times t^2$	0.0058*	-0.0212	0.0057*
	[0.0033]	[0.0202]	[0.0033]
log Median Income$\times t$	0.0068	-0.1038**	0.0061
	[0.0073]	[0.0412]	[0.0073]
log Median Income$\times t^2$	-0.0034***	0.0044	-0.0033***
	[0.0008]	[0.0039]	[0.0008]
Geographic Time Trends			
Grid 1$\times t$	-0.0042	0.5283***	-0.0003

	[0.0175]	[0.0682]	[0.0175]
Grid 1×t^2	-0.0001	-0.0390***	-0.0004
	[0.0017]	[0.0052]	[0.0017]
Grid 2×t	0.0051	0.7910***	0.0109
	[0.0173]	[0.0821]	[0.0172]
Grid 2×t^2	-0.0018	-0.0657***	-0.0023
	[0.0017]	[0.0061]	[0.0017]
Grid 3×t	0.0102	1.0460***	0.0179
	[0.0187]	[0.1474]	[0.0186]
Grid 3×t^2	-0.0029	-0.0806***	-0.0035*
	[0.0019]	[0.0129]	[0.0019]
Grid 4×t	0.0042	0.9010***	0.0109
	[0.0209]	[0.1276]	[0.0209]
Grid 4×t^2	-0.0012	-0.0800***	-0.0018
	[0.0022]	[0.0144]	[0.0022]
Grid 5×t	0.0592***	0.4166***	0.0623***
	[0.0185]	[0.0508]	[0.0185]
Grid 5×t^2	-0.0054***	-0.0307***	-0.0056***
	[0.0018]	[0.0046]	[0.0018]
Grid 6×t	-0.0861***	0.5024***	-0.0824***
	[0.0213]	[0.0488]	[0.0213]
Grid 6×t^2	0.0074***	-0.0364***	0.0071***
	[0.0021]	[0.0045]	[0.0021]
Grid 7×t	-0.0177	0.4356***	-0.0145
	[0.0188]	[0.1151]	[0.0188]
Grid 7×t^2	0.0015	-0.0237***	0.0013
	[0.0019]	[0.0074]	[0.0019]
Grid 8×t	-0.0459**	0.5502***	-0.0418**
	[0.0199]	[0.0675]	[0.0199]
Grid 8×t^2	0.0042**	-0.0311***	0.0040**
	[0.0020]	[0.0060]	[0.0020]
Grid 9×t	-0.0008	0.4396***	0.0024
	[0.0168]	[0.0574]	[0.0168]
Grid 9×t^2	0.0000	-0.0272***	-0.0002
	[0.0016]	[0.0049]	[0.0016]
Grid 10×t	-0.0000	0.4612***	0.0033
	[0.0175]	[0.0526]	[0.0175]
Grid 10×t^2	-0.0004	-0.0341***	-0.0006
	[0.0017]	[0.0050]	[0.0017]
Grid 11×t	0.0111	0.7269**	0.0164
	[0.0236]	[0.3109]	[0.0232]
Grid 11×t^2	-0.0015	-0.0266	-0.0017
	[0.0024]	[0.0196]	[0.0023]
Grid 12×t	0.0301*	0.7649***	0.0357**
	[0.0176]	[0.1242]	[0.0176]
Grid 12×t^2	-0.0027	-0.0540***	-0.0031*
	[0.0017]	[0.0076]	[0.0017]

Grid 13×t	-0.0132	0.7943***	-0.0074
	[0.0180]	[0.0958]	[0.0180]
Grid 13×t^2	-0.0002	-0.0476***	-0.0005
	[0.0018]	[0.0076]	[0.0018]
Grid 14×t	0.1239*	1.3842**	0.1341*
	[0.0730]	[0.6399]	[0.0753]
Grid 14×t^2	-0.0141*	-0.1041	-0.0148*
	[0.0084]	[0.0637]	[0.0086]
Grid 15×t	-0.0038	0.7585***	0.0018
	[0.0205]	[0.0755]	[0.0205]
Grid 15×t^2	-0.0016	-0.0514***	-0.0020
	[0.0021]	[0.0075]	[0.0021]
Grid 16×t	0.0261	0.6306***	0.0307*
	[0.0176]	[0.1031]	[0.0176]
Grid 16×t^2	-0.0024	-0.0758***	-0.0030*
	[0.0017]	[0.0135]	[0.0017]
Grid 17×t	0.0127	0.4856***	0.0163
	[0.0183]	[0.0738]	[0.0183]
Grid 17×t^2	-0.0017	-0.0395***	-0.0020
	[0.0018]	[0.0074]	[0.0018]
Method	OLS	OLS	2SLS

Note: N=41,771. Table presents the full regression output Columns 2–4 of Table 6. Controls also include property fixed effects.