Does Air Quality Matter? Evidence from the Housing Market

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We exploit the structure of the Clean Air Act to provide new evidence on the capitalization of total suspended particulates (TSPs) air pollution into housing values. This legislation imposes strict regulations on polluters in "nonattainment" counties, which are defined by concentrations of TSPs that exceed a federally set ceiling. TSPs nonattainment status is associated with large reductions in TSPs pollution and increases in county-level housing prices. When nonattainment status is used as an instrumental variable for TSPs, we find that the elasticity of housing values with respect to particulates concentrations ranges from −0.20 to −0.35. These estimates of the average marginal willingness to pay for clean air are robust to quasi-experimental regression discontinuity and matching specification tests. Further, they are far less sensitive to model specification than cross-sectional and fixed-effects estimates, which occasionally have the "perverse" sign. We also find modest evidence that the marginal benefit of reductions of TSPs is lower in communities with relatively high pollution levels,

which is consistent with preference-based sorting. Overall, the improvements in air quality induced by the mid-1970s TSPs nonattainment designation are associated with a $45 billion aggregate increase in housing values in nonattainment counties between 1970 and 1980.

I. Introduction

Federal air pollution regulations have been among the most controversial interventions mandated by the U.S. government. Much of this controversy is generated by an absence of convincing empirical evidence on their costs and benefits. Thus the credible estimation of the economic value of clean air to individuals is an important topic to both policy makers and economists.

The hedonic approach to estimating the economic benefits of air quality uses the housing market to infer the implicit price function for this nonmarket amenity. Here, researchers estimate the association between property values and air pollution, usually measured by total suspended particulates (TSPs), regression-adjusted for differences across locations in observable characteristics. After over 30 years of research, the cross-sectional correlation between housing prices and particulates air pollution appears weak. A meta-analysis of 37 cross-sectional studies suggests that a decrease in TSPs of 1 microgram per cubic meter (μg/m^3) results in a 0.05–0.10 percent increase in property values, which implies only a -0.04 to -0.07 elasticity (Smith and Huang 1995). As a result, many conclude that either individuals place a small value on air quality or the hedonic approach cannot produce reliable estimates of the marginal willingness to pay (MWTP) for environmental amenities.

These weak results may be explained by two econometric identification problems that could plague the implementation of the hedonic method. First, it is likely that the estimated housing price–air pollution gradient is severely biased because of omitted variables. We show that the "conventional" cross-sectional and fixed-effects approaches produce estimates of MWTP that are very sensitive to specification and occasionally have the perverse sign, indicating that TSPs and housing prices are *positively* correlated. Second, if there is heterogeneity across individuals in tastes for clean air, then individuals may self-select into locations on the basis of these unobserved differences. In this case, estimates of MWTP may reflect the preferences of subpopulations that, for example, place a relatively low valuation on air quality.

This paper exploits the structure of the Clean Air Act Amendments (CAAAs) to provide new evidence on the capitalization of air quality into housing values. The CAAAs marked an unprecedented attempt by the federal government to mandate lower levels of air pollution. If pol-

lution concentrations in a county exceed the federally determined ceiling, then the Environmental Protection Agency (EPA) designates the county as "nonattainment." Polluters in nonattainment counties face far more stringent regulations than their counterparts in attainment counties.

We use nonattainment status as an instrumental variable for changes in TSPs in first-differenced equations for the 1970–80 change in county-level housing prices. The instrumental variables estimates indicate that the elasticity of housing values with respect to TSPs concentrations ranges from −0.20 to −0.35. These estimates of the average MWTP for clean air are far less sensitive to specification than the cross-sectional and fixed-effects estimates. For example, we find evidence that nonattainment status is uncorrelated with virtually all other observable determinants of changes in housing prices, including economic shocks. Thus it is not surprising that the results are largely insensitive to the choice of controls.

The "reduced-form" relationships between nonattainment status and changes in TSPs and housing prices provide direct estimates of the benefits of this central feature of the CAAAs. We find that TSPs declined by roughly 10 μg/m^3 (12 percent) more in nonattainment than in attainment counties. This finding contradicts recent claims that the Clean Air Act failed to reduce air pollution concentrations (Goklany 1999). Further, the data reveal that housing prices rose by approximately 2.5 percent more in nonattainment counties.

The discrete relationship between regulatory status and the previous year's pollution levels provides two opportunities to gauge the credibility of our results. In principle, the structure of the rule that determines nonattainment status allows for comparisons of nonattainment and attainment counties with almost identical and identical average TSPs levels in the regulation selection year. We construct two sets of robustness tests that utilize the two nonlinearities in the assignment of nonattainment status. The results from these tests are consistent with the reduced-form results and the finding of an important relationship between TSPs and housing values.

Finally, we estimate a random coefficients model that allows for nonrandom sorting. The estimation results provide evidence consistent with the self-selection of individuals across counties based on taste heterogeneity and suggest that the marginal benefit of a reduction in TSPs may be lower in communities with relatively high pollution levels. However, the self-selection bias in estimates of the average MWTP appears to be small relative to the influence of omitted variables.

The analysis is conducted with the most detailed and comprehensive data available on pollution levels, EPA regulations, and housing values at the county level. Through a Freedom of Information Act request, we

obtained annual air pollution concentrations for each county based on the universe of state and national pollution monitors. These data are used to measure counties' prevailing TSPs concentrations and nonattainment status. We use the County and City Data Books data file, which is largely based on the 1970 and 1980 censuses, to obtain measures of housing values and housing and county characteristics.

Taken literally, our estimates indicate that the improvements in air quality induced by the mid-1970s TSPs nonattainment designation are associated with a $45 billion aggregate increase in housing values in nonattainment counties during the 1970s. This gain is large, but the net effect on welfare is unknown because reliable estimates of the social costs of these regulations are not available. The results also demonstrate that the hedonic method can be successfully applied to value environmental amenities.

II.   The Hedonic Method and Econometric Identification Problems

An explicit market for clean air does not exist. The hedonic price method is commonly used to estimate the economic value of this nonmarket amenity to individuals.[1] It is based on the insight that the utility associated with the consumption of a differentiated product, such as housing, is determined by the utility associated with the individual characteristics of the good. For example, hedonic theory predicts that an economic bad, such as air pollution, will be negatively correlated with housing prices, with all other characteristics held constant. Here, we review the method and the econometric identification problems associated with its implementation.

A.   The Hedonic Method

Economists have estimated the association between housing prices and air pollution at least since Ridker (1967) and Ridker and Henning (1967). However, Rosen (1974) was the first to give this correlation an economic interpretation. In the Rosen model, a differentiated good can be described by a vector of its characteristics, $Q = (q_1, q_2, \dots, q_n)$. In the case of a house, these characteristics may include structural attributes (e.g., number of bedrooms), the provision of neighborhood public

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[1] Other methods for the valuation of nonmarket amenities include contingent valuation, conjoint analysis, and discrete choice models. See Smith (1996) for a review and comparison of these methods.

services (e.g., local school quality), and local amenities (e.g., air quality). Thus the price of the ith house can be written as

$$P_i = P(q_1, q_2, \ldots, q_n). \tag{1}$$

The partial derivative of $P(\cdot)$ with respect to the nth characteristic, $\partial P/\partial q_n$, is referred to as the marginal implicit price. It is the marginal price of the nth characteristic implicit in the overall price of the house.

In a competitive market the housing price–housing characteristic locus, or the hedonic price schedule, is determined by the equilibrium interactions of consumers and producers.[2] The hedonic price schedule is the locus of tangencies between consumers' bid functions and suppliers' offer functions. The gradient of the implicit price function with respect to air pollution gives the equilibrium differential that allocates individuals across locations and compensates those who face higher pollution levels. Locations with poor air quality must have lower housing prices in order to attract potential homeowners. Thus, at each point on the hedonic price schedule, the marginal price of a housing characteristic is equal to an individual consumer's MWTP for that characteristic and an individual supplier's marginal cost of producing it. Since the hedonic price schedule reveals the MWTP at a given point, it can be used to infer the welfare effects of a marginal change in a characteristic for a given individual.

In principle, the hedonic method can also be used to recover the entire demand or MWTP function.[3] This would be of tremendous practical importance, because it would allow for the estimation of the welfare effects of nonmarginal changes. Rosen (1974) proposed a two-step approach for estimating the MWTP function, as well as the supply curve.[4] In some recent work, Ekeland, Heckman, and Nesheim (2004) outline the assumptions necessary to identify the demand (and supply) functions in an additive version of the hedonic model with data from a single market. The estimation details are explored in further work.[5]

[2] See Rosen (1974), Palmquist (1991), and Freeman (1993) for details.

[3] Epple and Sieg (1999) develop an alternative approach to value local public goods. Sieg et al. (2000) apply this locational equilibrium approach to value air quality changes in Southern California from 1990 to 1995.

[4] Brown and Rosen (1982), Bartik (1987), and Epple (1987) highlight the strong assumptions necessary to identify the structural parameters with this approach. There is a consensus that empirical applications have not identified a situation in which these assumptions hold and that the second-stage MWTP function for an environmental amenity has never been reliably estimated (Deacon et al. 1998).

[5] Heckman, Matzkin, and Nesheim (2002) examine identification and estimation of nonadditive hedonic models. Heckman et al. (2003) examine the performance of estimation techniques for both types of models.

B. Econometric Identification Problems

This paper's goals are to (1) estimate the hedonic price schedule for clean air and empirically assess whether housing prices rise with air quality and (2) estimate the average MWTP in the population while accounting for preference-based sorting across locations. In some respects, these goals are less ambitious than efforts to estimate primitive preference parameters and, in turn, MWTP functions. However, from a practical perspective, they are of at least equal importance since the consistent estimation of equation (1) is the foundation on which any welfare calculation rests. The reason is that the welfare effects of a marginal change in air quality are obtained directly from the hedonic price schedule. Further, inconsistent estimation of the hedonic price schedule will lead to an inconsistent MWTP function, invalidating any welfare analysis of nonmarginal changes regardless of the method used to recover preference or technology parameters.

Consistent estimation of the hedonic price schedule in equation (1) is extremely difficult since there may be unobserved factors that covary with both air pollution and housing prices.[6] For example, areas with higher levels of TSPs tend to be more urbanized and have higher per capita incomes, population densities, and crime rates. Consequently, cross-sectional estimates of the housing price–air quality gradient may be severely biased because of omitted variables. This is one explanation for the wide variability in hedonic price schedule estimates and the frequent examples of perversely signed estimates from the cross-sectional studies of the last 30 years (Smith and Huang 1995).[7]

The consequences of the misspecification of equation (1) were recognized almost immediately after the original Rosen paper. For example, Small (1975, 107) wrote as follows:

> I have entirely avoided … the important question of whether the empirical difficulties, especially correlation between pollution and unmeasured neighborhood characteristics, are so overwhelming as to render the entire method useless. I hope

[6] The cross-sectional estimation of the hedonic price schedule has exhibited signs of misspecification in a number of settings, including the relationships between land or house prices and school quality, climate variables, and proximity to hazardous waste sites (Black 1999; Deschenes and Greenstone 2004; Greenstone and Gallagher 2004). Similar problems arise when estimating compensating wage differentials for job characteristics, such as the risk of injury or death. The regression-adjusted association between wages and many job amenities is weak and often has a counterintuitive sign (Smith 1979; Brown 1980; Black and Kneisner 2003). Finally, see Halvorsen and Pollakowski (1981) and Cropper, Deck, and McConnell (1988) for discussions of misspecification of the hedonic price schedule due to incorrect choice of functional form for observed covariates.

[7] Smith and Huang (1995) find that a quarter of the reported estimates have perverse signs; i.e., they indicate a *positive* correlation between housing prices and pollution levels.

> that … future work can proceed to solving these practical prob-
> lems. … The degree of attention devoted to this [problem] is
> what will really determine whether the method stands or falls.

In the intervening years, this problem of misspecification has received little attention from empirical researchers,[8] even though Rosen himself recognized it.[9] Thus this paper's first goal is to focus attention on this problem of misspecification and to demonstrate how the structure of the Clean Air Act may provide a quasi-experimental solution in the case of housing prices and TSPs.[10]

Self-selection to locations based on preferences presents a second source of bias in the estimation of the average MWTP for clean air in the population. In particular, if individuals with lower valuations for air quality sort to areas with worse air quality, then estimates of the average MWTP that do not account for this can be biased upward or downward depending on the structure of preferences and the amount of sorting. This is a salient issue for this paper because its identification strategy is based on comparisons across U.S. locations with varying levels of TSPs. If individuals have sorted geographically on the basis of tastes, then the approach may produce estimates of the average MWTP that are based on nonrandom subpopulations. Thus our second goal is to estimate the average MWTP while accounting for self-selection and to probe the heterogeneity in MWTP in the population.

III.   Background on Federal Air Quality Regulations

Before 1970 the federal government did not play a significant role in the regulation of air pollution; that responsibility was left primarily to state governments.[11] In the absence of federal legislation, few states found it in their interest to impose strict regulations on polluters within their jurisdictions. Concerned with the detrimental health effects of persistently high concentrations of TSPs and other air pollutants, Congress passed the Clean Air Act Amendments of 1970.

The centerpiece of the CAAAs is the establishment of separate federal

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[8] Graves et al. (1988) is an exception.

[9] Rosen (1986) wrote that "it is clear that nothing can be learned about the structure of preferences in a single cross-section" (658), and "on the empirical side of these questions, the greatest potential for further progress rests in developing more suitable sources of data on the nature of selection and matching" (688).

[10] In an earlier version of this paper (Chay and Greenstone 2001), we outlined and implemented a method that exploits features of the Clean Air Act to estimate MWTP functions for TSPs. This method requires very strong assumptions that may not be plausible, so this material is not presented here.

[11] Lave and Omenn (1981) and Liroff (1986) provide more details on the CAAAs. In addition, see Chay and Greenstone (1998) and Greenstone (2002).

air quality standards, known as the National Ambient Air Quality Standards, for five pollutants. The stated goal of the amendments is to bring all counties into compliance with the standards by reducing local air pollution concentrations. The legislation requires the EPA to assign annually each county to either nonattainment or attainment status for each of the pollutants, on the basis of whether the relevant standard is exceeded. The federal TSPs standard is violated if either of two thresholds is exceeded: (1) the annual geometric mean concentration exceeds 75 μg/m^3 or (2) the second-highest daily concentration exceeds 260 μg/m^3.[12]

The CAAAs direct the 50 states to develop and enforce local air pollution abatement programs that ensure that each of their counties attains the standards. In their nonattainment counties, states are required to develop plant-specific regulations for every major source of pollution.[13] These local rules demand that substantial investments, by either new or existing plants, be accompanied by installation of state-of-the-art pollution abatement equipment and strict emissions ceilings. The 1977 amendments added the requirement that any increase in emissions from new investment be offset by a reduction in emissions from another source within the same county.[14] States are also mandated to set emissions limits on existing plants in nonattainment counties.

In attainment counties, the restrictions on polluters are less stringent. Large-scale investments, such as plant openings and large expansions at existing plants, require less expensive (and less effective) pollution abatement equipment; moreover, offsets are not necessary. Smaller plants and existing plants are essentially unregulated.

Both the states and the federal EPA are given substantial enforcement powers to ensure that the CAAAs' statutes are met. For instance, the federal EPA must approve all state regulation programs in order to limit the variance in regulatory intensity across states. On the compliance side, states run their own inspection programs and frequently fine noncompliers. The 1977 legislation made the plant-specific regulations both federal and state law, which gives the EPA legal standing to impose penalties on states that do not aggressively enforce the regulations and on plants that do not adhere to them.

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[12] This TSPs standard prevailed from 1971 until 1987, when, instead of regulating all particulates with an aerodynamic diameter less than 100 micrometers (μm), the EPA shifted its focus to fine particles. The regulations were changed to apply only to emissions of PM-10s (particles with an aerodynamic diameter of at most 10 μm) in 1987 and to emissions of PM-2.5s (i.e., smaller than 2.5 μm) in 1997.

[13] The sources of TSPs include industrial processes, smelters, automobiles, the burning of industrial fuels, wood smoke, dust from paved and unpaved roads, construction, and agricultural ground breaking.

[14] Offsets could be purchased from a different facility or could be generated by tighter controls on existing operations at the same site (Vesilind, Peirce, and Weiner 1988).

Nadeau (1997) and Cohen (1998) document the effectiveness of these regulatory actions at the plant level. Henderson (1996) provides direct evidence that the regulations are successfully enforced. He finds that ozone concentrations declined more in counties that were nonattainment for ozone than in attainment counties. Greenstone (2004) finds that sulfur dioxide nonattainment status is associated with modest reductions in sulfur dioxide concentrations. In this paper and Chay and Greenstone (2003a), we find striking evidence that TSPs levels fell substantially more in TSPs nonattainment counties than in attainment counties during the 1970s.[15]

IV.   Data Sources and Descriptive Statistics

To implement the analysis, we compiled the most detailed and comprehensive data available on pollution levels, EPA regulations, and housing values for the 1970s. Here, we describe the data sources and provide some descriptive statistics. More details are provided in the Data Appendix.

A.   Data Sources

TSPs pollution data and national trends.—The TSPs data were obtained by filing a Freedom of Information Act request with the EPA that yielded the Quick Look Report file, which comes from the EPA's Air Quality Subsystem database. This file contains annual information on the location of and readings from every TSPs monitor in operation in the United States since 1967. Since the EPA regulations are applied at the county level, we calculated the annual geometric mean TSPs concentration for each county from the monitor-level data. For counties with more than one monitor, the county mean is a weighted average of the monitor-specific geometric means, with the number of observations per monitor used as weights. The file also reports the four highest daily monitor readings.

Our 1970 and 1980 county-level measures of TSPs are calculated with data from multiple years. In particular, the 1970 (1980) level of TSPs is the simple average over a county's nonmissing annual averages in the years 1969–72 (1977–80). These formulas reduce the degree of measurement error in measured TSPs. Further, the EPA's network of TSPs

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[15] Greenstone (2002) provides further evidence on the effectiveness of the regulations. He finds that nonattainment status is associated with reductions in the employment, investment, and shipments of polluting manufacturers. Interestingly, the regulation of TSPs has little association with changes in employment. Instead, the declines in employment are driven mostly by the regulation of other air pollutants.

monitors was dramatically growing in the late 1960s and early 1970s, so the 1969–72 definition allows for a larger sample.

There are two primary reasons for our exclusive focus on TSPs rather than on other forms of air pollution. First, TSPs are the most visible form of air pollution and have the most pernicious health effects of all the pollutants regulated by the CAAAs.[16] Second, the EPA's monitoring network for the other air pollutants was in its nascent stages in the early 1970s, and the inclusion of these pollutants in our models severely restricts the sample size.[17]

TSPs attainment/nonattainment designations.—The EPA did not begin to publicly release the annual list of TSPs nonattainment counties until 1978. We contacted the EPA but were informed that records from the early 1970s "no longer exist." Consequently, we used the TSPs monitor data to determine which counties exceeded either of the federal ceilings and assigned these counties to the nonattainment category; all other counties are designated attainment. We allowed these designations to vary by year and based them on the previous year's concentrations. This is likely to be a reasonable approximation to the EPA's actual selection rule because it is based on the same information that was available to the EPA. The Data Appendix provides more details on our assignment rule.

Housing values and county characteristics.—The property value and county characteristics data come from the 1972 and 1983 County and City Data Books (CCDB). The CCDBs are comprehensive, are reliable, and contain a wealth of information for every U.S. county. Much of the data is derived from the 1970 and 1980 Censuses of Population and Housing.

Our primary outcome variable is the log median value of owner-occupied housing units in the county. The control variables include demographic and socioeconomic characteristics (population density, race, education, age, per capita income, poverty and unemployment rates, and fraction in urban area), neighborhood characteristics (crime rates, doctors, and hospital beds per capita), fiscal/tax variables (per capita taxes, government revenue, expenditures, and fraction spent on

[16] See Dockery et al. (1993), Ransom and Pope (1995), Chay, Dobkin, and Greenstone (2003), and Chay and Greenstone (2003*a*, 2003*b*) for evidence on the effects of TSPs on adult and infant health.

[17] Only 34 of the 988 counties in our sample were monitored for all the other primary pollutants regulated by the 1970 CAAAs at the beginning and end of the 1970s. Alternatively, when the sample is limited to counties monitored for TSPs and one other pollutant, the sample sizes are 135 (carbon monoxide), 49 (ozone), and 144 (sulfur dioxide). We separately examined the relationship between housing values and levels of ozone, sulfur dioxide, and carbon monoxide in the 1970s and found a weak association. Chay and Greenstone (1998) find modest evidence that changes in ozone concentrations during the 1980s were capitalized into housing prices.

education, welfare, health, and police), and housing characteristics (e.g., year structure was built and whether there is indoor plumbing). The Data Appendix contains a complete set of the controls used in the subsequent analysis.

The census data contain fewer variables on the characteristics of homes and neighborhoods than is ideal. For example, these data do not contain information on square feet of living space, garages, air conditioning, lot size, crime statistics, or schooling expenditures per student. We explain our identification strategy in greater detail below, but we believe that it overcomes some of the limitations of the census data. In particular, we include county fixed effects to control for permanent, unobserved variables and use the indicator for nonattainment status as an instrumental variable in an effort to isolate changes in TSPs that are orthogonal to changes in the unobserved determinants of housing prices.

We note that a number of studies have used census tract–level data or even house-level price data and focused on local markets (e.g., Ridker and Henning 1967; Harrison and Rubinfeld 1978; Palmquist 1984). In contrast, the unit of observation in our data is the county. Two practical reasons for the use of these data are that TSPs regulations are enforced at the county level and census tracts are difficult to match between the 1970 and 1980 censuses.

The use of county-level data raises a few issues. First, in the absence of arbitrary assumptions about which counties constitute separate markets, it is necessary to assume that there is a national housing market. The benefit of this is that our estimates of MWTP will reflect the preferences of the entire U.S. population rather than the subpopulation that lives in a particular city or local market. The cost is that we are unable to explore the degree of within-county taste heterogeneity and sorting. If taste heterogeneity and sorting are greater within counties than between counties, as is likely the case, then the subsequent results will understate the individual-level dispersion in MWTP.

Second, the hedonic approach as originally conceived is an individual-level model, and aggregation to the county level may induce some biases. For example, if the individual relationship is nonlinear, the aggregation will obscure the true relationship. We suspect that the aggregation to the county level may not be an important source of bias. Notably, our cross-sectional estimates from the county-level data are very similar to the estimates in the previous literature that rely on more disaggregated data and are summarized in Smith and Huang (1995).

Further, the aggregation does not lead to the loss of substantial variation in TSPs. Using the availability of readings from multiple monitors in most counties, we find that only 25 percent of the total variation in 1970–80 TSPs changes is attributable to within-county variation, with

the rest due to between-county variation. Finally, since there are substantially fewer monitors than census tracts (or houses), a census tract–level (or individual house–level) analysis introduces inference problems that a county-level analysis avoids.[18]

B. Descriptive Statistics

Figure 1 presents trends from 1969–90 in average particulates levels across the counties with monitor readings in each year.[19] Air quality improved dramatically over the period, with TSPs levels falling from an average of 85 $\mu g/m^3$ in 1969 to 55 $\mu g/m^3$ in 1990. Most of the overall reduction of TSPs occurred in two punctuated periods. While the declines in the 1970s correspond with the implementation of the 1970 CAAAs, the remaining improvements occurred during the 1981–82 recession. As heavily polluting manufacturing plants in the Rust Belt permanently closed as a result of the recession, air quality in these areas improved substantially (Kahn 1999; Chay and Greenstone 2003b). This implies that local economic shocks could drive both declines in TSPs and declines in housing prices. Below, we find that fixed-effects estimates of the hedonic price schedule may be seriously biased by these shocks.

Table 1 presents summary statistics on the variables that we control for in the subsequent regressions. The means are calculated as the average across the 988 counties with nonmissing data on TSPs concentrations in 1970, 1980, and 1974 or 1975, as well as nonmissing housing price data in 1970 and 1980. These counties form the primary sample, and they account for approximately 80 percent of the U.S. population. All monetary figures are denoted in 1982–84 dollars. During the 1970s the mean of the counties' median housing price increased from roughly $40,300 to $53,168, whereas TSPs declined by 8 $\mu g/m^3$. Per capita incomes rose by approximately 15 percent, and unemployment rates were 2.2 percentage points higher at the end of the decade. The increase in educational attainment during this period is also evident. The population density and fraction of people residing in urbanized areas are roughly constant at the beginning and the end of the decade.[20]

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[18] For example, Harrison and Rubinfeld's (1978) analysis of 506 census tracts relies on only 18 TSPs monitors. As noted by Moulton (1986), the treatment of these correlated observations as independent can lead to incorrect inferences.

[19] These are weighed averages of the county means, with the counties' populations in 1980 used as weights. The sample consists of 169 counties with a combined population of 84.4 million in 1980. The unweighted figure is qualitatively similar.

[20] Since the definition of the vacancy variables changes over time, it is impossible to include the first difference of these variables in the subsequent regressions. Consequently, the regressions control separately for the 1970 and 1980 levels of these variables.
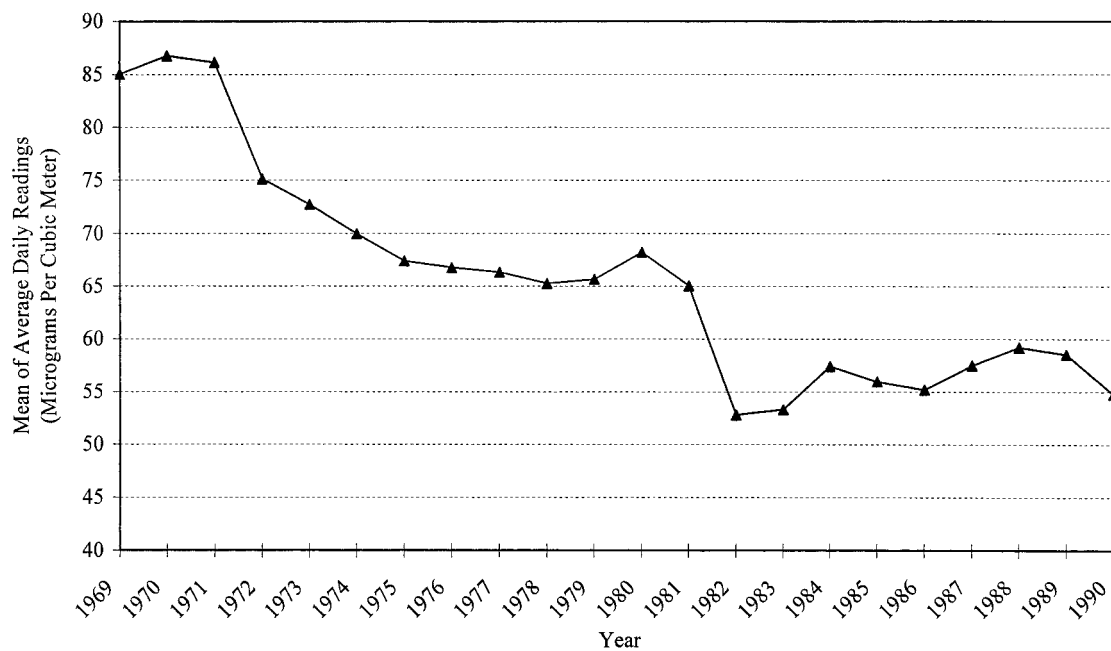
FIG. 1.—National trends in TSPs pollution, 1969–90. The data points are derived from the 169 counties that are continuously monitored in this period. These counties had a total population of approximately 84.4 million in 1980. The annual county means were calculated as the weighted average of the monitor-specific geometric means, where the weight is the number of monitor observations. The year-specific average is calculated as the weighted average of the county-specific means, where the weight is the 1980 population.

	1970	1980
Mean housing value	40,290	53,166
Mean TSPs	64.1	56.3
Economic condition variables:		
Income per capita (1982–84 dollars)	7,122	8,186
Total population	161,889,646	177,192,574
Unemployment rate	.046	.068
% employment in manufacturing	.249	.226
Demographic and socioeconomic variables:		
Population density	608	585
% ≥ high school graduate	.504	.646
% ≥ college graduate	.097	.147
% urban	.576	.593
% poverty	.124	.097
% white	.901	.877
% senior citizens	.100	.113
Housing variables:		
% of houses built in last 10 years	…	.285
% of houses built 10–20 years ago	…	.187
% overall vacancy rate	…	.078
% vacancy rate owners' units	.014	…
% vacancy rate renters' units	.077	…
% owner-occupied	.676	.620
% of houses built before 1939	…	.267
% of houses without plumbing (× 100)	.003	.028
Tax and expenditure variables:		
Per capita government revenue	747	1,098
Per capita total taxes	…	422
Per capita property taxes	170	354
Per capita general expenditures	768	1,072
% of spending on education	.548	.509
% of spending on highways	.091	.070
% of spending on welfare	.046	.037
% of spending on health	.048	.067
% of spending on police	…	.043
Neighborhood variables:		
Crime rate per 100,000	…	4,619
Physicians per 100,000	…	125
Hospital beds per 100,000	…	642

NOTE.—Calculations are based on the 988 counties with data on TSPs concentrations in 1970, 1980, and 1974 or 1975. The housing and overall consumer price index series are used to deflate all monetary entries to 1982–84 dollars. The TSPs data are derived from the EPA's network of pollution monitors. The 1970 (1980) mean TSPs concentration is the average across all counties' mean TSPs concentration from 1969 to 1972 (1977 to 1980). Each county's annual mean TSPs concentration is calculated as the weighted average of the geometric mean concentrations of each monitor in the county, using the number of observations per monitor as weights. The county-level mean across multiple years (e.g., 1969–72) is the average of the annual means. The other entries are derived from the 1972 and 1983 County and City Data Books. An entry with ellipses means that the variable was not collected in the relevant year. See the text and Data Appendix for further details.

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V.   Econometric Models for the Hedonic Price Schedule and Average MWTP

This section discusses the econometric models used to estimate the hedonic price schedule. First, we focus on the constant coefficients version of these models. We then discuss a random coefficients model that under certain assumptions identifies the average MWTP in the population and provides a simple statistical test of sorting based on preferences for clean air.

A.   Estimation of the Hedonic Price Schedule Gradient

The cross-sectional model predominantly used in the literature is

$$y_{c70} = X'_{c70}\beta + \theta T_{c70} + \epsilon_{c70}, \quad \epsilon_{c70} = \alpha_c + u_{c70}, \tag{2}$$

and

$$T_{c70} = X'_{c70}\Pi + \eta_{c70}, \quad \eta_{c70} = \lambda_c + v_{c70}, \tag{3}$$

where y_{c70} is the log of the median property value in county c in 1970, X_{c70} is a vector of observed characteristics, T_{c70} is the geometric mean of TSPs across all monitors in the county, and ϵ_{c70} and η_{c70} are the unobservable determinants of housing prices and TSPs levels, respectively. The coefficient θ is the "true" effect of TSPs on property values and is interpreted as the average gradient of the hedonic price schedule. For consistent estimation, the least-squares estimator of θ requires $E[\epsilon_{c70}\eta_{c70}] = 0$. If there are omitted permanent (α_c and λ_c) or transitory (u_{c70} and v_{c70}) factors that covary with both TSPs and housing prices, then the cross-sectional estimator will be biased.

With repeated observations over time, a "fixed-effects" model implies that first-differencing the data will absorb the county permanent effects, α_c and λ_c. This leads to

$$y_{c80} - y_{c70} = (X_{c80} - X_{c70})'\beta + \theta(T_{c80} - T_{c70}) + (u_{c80} - u_{c70}) \tag{4}$$

and

$$T_{c80} - T_{c70} = (X_{c80} - X_{c70})'\Pi + (v_{c80} - v_{c70}). \tag{5}$$

For identification, the least-squares estimator of θ requires $E[(u_{c80} - u_{c70})(v_{c80} - v_{c70})] = 0$. That is, there are no unobserved shocks to pollution levels that covary with unobserved shocks to housing prices.

Suppose that there is an instrumental variable, Z_c, that causes changes in TSPs without having a direct effect on changes in housing prices. One plausible instrument is mid-1970s TSPs regulation, measured by

the attainment-nonattainment status of a county. Here, equation (5) becomes

$$T_{c80} - T_{c70} = (\boldsymbol{X}_{c80} - \boldsymbol{X}_{c70})'\boldsymbol{\Pi}_{TX} + Z_{c75}\Pi_{TZ} + (v_{c80} - v_{c70})^{\circ} \qquad (6)$$

and

$$Z_{c75} = 1(T_{c74} > \overline{T}) = 1(v_{c74} > \overline{T} - \boldsymbol{X}'_{c74}\boldsymbol{\Pi} - \lambda_c), \qquad (7)$$

where Z_{c75} is the regulatory status of county c in 1975, $1(\cdot)$ is an indicator function equal to one if the enclosed statement is true, and $\overline{T}$ is the maximum concentration of TSPs allowed by the federal regulations.[21] Nonattainment status in 1975 is a discrete function of TSPs concentrations in 1974. In particular, if T_{c74}^{avg} and T_{c74}^{max} are the annual geometric mean and second-highest daily TSPs concentrations, respectively, then the actual regulatory instrument used is $1(T_{c74}^{\text{avg}} > 75 \ \mu g/m^3$ or $T_{c74}^{\text{max}} > 260 \ \mu g/m^3)$.

An attractive feature of this approach is that the reduced-form relations are policy relevant. In particular, Π_{TZ} from equation (6) measures the change in TSPs concentrations in nonattainment counties relative to attainment ones. In the other reduced-form equation,

$$y_{c80} - y_{c70} = (\boldsymbol{X}_{c80} - \boldsymbol{X}_{c70})'\boldsymbol{\Pi}_{yX} + Z_{c75}\Pi_{yZ} + (u_{c80} - u_{c70})^{\circ}, \qquad (8)$$

Π_{yZ} captures the relative change in log housing prices. Since the instrumental variables estimator, θ_{IV}, is exactly identified, it is a simple ratio of the two reduced-form parameters, that is, $\theta_{\text{IV}} = \Pi_{yZ}/\Pi_{TZ}$.

The paper's primary results come from the estimation of θ_{IV} in our full sample, where the mid-decade nonattainment indicator is the instrumental variable. Two sufficient conditions for the instrumental variables estimator to provide a consistent estimate of the hedonic price schedule gradient are $\Pi_{TZ} \neq 0$ and $E[v_{c74}(u_{c80} - u_{c70})] = 0$. We show that the first condition clearly holds. The second condition requires that unobserved price shocks from 1970–80 are orthogonal to transitory shocks to 1974 TSPs levels. In the simplest case, the instrumental variables estimator is consistent if $E[Z_{c75}(u_{c80} - u_{c70})] = 0$.

We also calculate instrumental variables estimates in two other ways that allow for the possibility that $E[v_{c74}(u_{c80} - u_{c70})] \neq 0$ over the entire sample. The first leverages the regression discontinuity design implicit in the $1(\cdot)$ function that determines nonattainment status (Cook and Campbell 1979). For example, if $E[v_{c74}(u_{c80} - u_{c70})] = 0$ in the neighborhood of the annual regulatory ceiling (i.e., 75 $\mu g/m^3$), then a comparison of changes in nonattainment and attainment counties in this neighborhood will control for all omitted variables. In the case in which

[21] In practice, our preferred instrument equals one if a county is nonattainment in 1975 or 1976 and zero otherwise. In this section, we denote it with Z_{c75} for ease of exposition.

this assumption is invalid but the relationship between v_{c74} and $u_{c80} - u_{c70}$ is sufficiently smooth, causal inference is possible by including smooth functions of T_{c74} in the vector of covariates.[22]

The second approach exploits the matching design that is feasible because of the $T_{c74}^{\max} > 260 \ \mu g/m^3$ discontinuity in the regulation selection rule. Here, we estimate θ_{IV} from the sample of counties with "identical" T_{c74}^{avg}. We implement this by limiting the sample to the counties with T_{c74}^{avg} between 50 $\mu g/m^3$ and 75 $\mu g/m^3$ and continuing to use Z_{c75} as an instrumental variable. In this subsample, the nonattainment counties exceed the "bad day" rule but not the annual standard. We further "nonparametrically" control for T_{c74}^{avg} with a series of indicators so that the comparisons between nonattainment and attainment counties are within narrow ranges of preregulation TSPs levels. This approach produces consistent estimates of θ_{IV} if the number of "bad" days in a county does not independently affect housing price *changes*, with T_{c74}^{avg} held constant. This assumption seems plausible.

Before we proceed, it is worth noting that in most applications of Rosen's model, the vector of controls, denoted by **X**, is limited to housing and neighborhood characteristics. Income and other similar variables are generally excluded on the grounds that they are "demand shifters" and are needed to obtain consistent estimates of the MWTP function. However, if individuals believe that there are spillovers, then the presence of wealthy individuals or high levels of economic activity is an amenity and the exclusion restriction is invalid. In our analysis, we are agnostic about which variables belong in the **X** vector and instead show unadjusted estimates, as well as estimates adjusted for all the variables listed in table 1. Importantly, our instrumental variables estimates are largely insensitive to the choice of control variables.

B.   Random Coefficients, Self-Selection, and the Average MWTP

Each point on the hedonic price schedule provides a consumer's WTP for a marginal change in TSPs. If individual tastes for clean air are identical, then the average gradient of the hedonic price schedule, θ, gives the average marginal rate of substitution of wealth for TSPs for all consumers. However, if there is sorting arising from taste dispersion, then θ may differ from the average MWTP in the population. We use

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[22] In some contexts, leveraging a discontinuity design may *accentuate* selection biases if economic agents know about the discontinuity point and change their behavior as a result. Given the wide variety of factors that determine local TSPs concentrations ranging from wind patterns to industrial output, we suspect that counties were unable to engage in nonrandom sorting near the TSPs regulatory ceiling. Similarly, we suspect that during the 1970s individual homeowners were unaware of the proximity of their county's TSPs concentration to the annual threshold, making it unlikely that they would move as a consequence.

a random coefficients model to illustrate this and derive a test for a negative assortative matching equilibrium.

Suppose that preferences for air quality can be summarized at the county level. Simplifying notation, define $y_i \equiv y_{c70}$, $\Delta y_{it} \equiv y_{c80} - y_{c70}$, $\Delta T_{it} \equiv T_{c80} - T_{c70}$, and $Z_i \equiv Z_{c75}$. When we ignore the observables, the random coefficients version of equation (2) is $y_i = \theta_i T_i + \epsilon_i$, where θ_i represents heterogeneity in the MWTP across individuals/counties and $E(\theta_i) = \bar{\theta}$ is the average MWTP in the population. Here, the least-squares estimator of $\bar{\theta}$ will be biased if either $E(\epsilon_i T_i) \neq 0$ as a result of omitted variables or $E(\theta_i T_i) \neq 0$ as a result of self-selection. If individuals sort across counties on the basis of their tastes for air quality, then $E(\theta_i T_i) > 0$; that is, individuals with a high valuation for clean air select to counties with low TSPs levels.

If θ_i is stationary over time, then the random coefficients analogues of equations (4) and (6) are

$$\Delta y_{it} = \bar{\theta}\Delta T_{it} + (\theta_i - \bar{\theta})\Delta T_{it} + \Delta u_{it} \tag{9}$$

and

$$\Delta T_{it} = \Pi Z_i + \Delta v_{it}. \tag{10}$$

Suppose that ΔT_{it} is monotonically related to T_i, in that the size of 1970–80 TSPs reductions is (weakly) increasing in 1970 TSPs levels. (We show below that this is approximately the case.) Then θ_i and ΔT_{it} may be correlated through either a nonconstant marginal utility or self-selection due to taste heterogeneity.

In the presence of correlated random coefficients, identification of $\bar{\theta}$ requires stronger assumptions than the orthogonality conditions above. Wooldridge (1997) and Heckman and Vytlacil (1998) specify conditions under which the inclusion of the first-stage residuals, Δv_{it}, as a control variable in the outcome equation will purge both the omitted variables and selection biases. Since this is numerically identical to two-stage least squares (2SLS), θ_{2SLS} will be consistent. The key condition in each of these papers turns on whether θ_i is correlated with Z_i. In our case, Z_i is a function of TSPs concentrations; and if individuals with a high valuation for clean air select to counties with low TSPs levels, then $E(\theta_{2SLS}) \neq \bar{\theta}$. In this situation, 2SLS may identify the average MWTP for a nonrandom subpopulation.

There is an alternative two-step approach to estimating $\bar{\theta}$ that allows for separate "control functions" for the omitted variables and self-

selection biases. This procedure also provides a simple statistical test for sorting based on tastes.[23] Consider the following assumptions.

ASSUMPTION 1. $E(\Delta u_{it}|Z_i) = E(\theta_i|Z_i) = 0.$

ASSUMPTION 2. $E(\Delta u_{it}|\Delta T_{it}, Z_i) = \lambda_T \Delta T_{it} + \lambda_Z Z_i.$

ASSUMPTION 3. $E(\theta_i|\Delta T_{it}, Z_i) = \psi_T \Delta T_{it} + \psi_Z Z_i.$

Assumptions 2 and 3 allow the conditional expectations of both Δu_{it} and θ_i to depend linearly on ΔT_{it} and Z_i. When these assumptions are combined with assumption 1, they imply $E(\Delta u_{it}|\Delta T_{it}, Z_i) = \lambda_T \Delta v_{it}$ and $E(\theta_i|\Delta T_{it}, Z_i) = \psi_T \Delta v_{it}$.

This results in the regression model

$$\Delta y_{it} = \bar{\theta}\Delta T_{it} + \lambda_T \Delta \hat{v}_{it} + \psi_T \Delta T_{it} \cdot \Delta \hat{v}_{it} + \Delta \epsilon_{it}, \tag{11}$$

where $\Delta \hat{v}_{it}$, the estimated residuals from the first-stage equation, is the potentially endogenous component of TSPs changes. This model is less restrictive than 2SLS, which effectively allows for only the first control function, $\lambda_T \Delta \hat{v}_{it}$. Further, below we test the robustness of the model by including polynomials in the two control functions.

The estimation of equation (11) has several attractive features. First, under assumptions 1–3, least-squares fitting of (11) will produce a consistent estimate of the average MWTP in the population. Second, $\lambda_T = \text{Cov}(\Delta u_{it}, \Delta v_{it})/\text{Var}(\Delta v_{it})$, so it provides a convenient measure of the importance of omitted variables bias in the conventional fixed-effects estimator (i.e., eq. [4]). The cyclical nature of TSPs concentrations implies that the estimated λ_T will be positive.

Third, the coefficient $\psi_T = \text{Cov}(\theta_i, \Delta v_{it})/\text{Var}(\Delta v_{it})$, so it measures the importance of selection bias due to heterogeneity in the MWTP. The sign and significance of the estimated ψ_T provide a test of sorting. First, note that homogeneous preferences and marginal utilities that do not increase in air quality imply $\psi_T \leq 0$. Only if there is taste heterogeneity *and* individuals sort across counties on the basis of this heterogeneity can ψ_T be greater than zero (i.e., individuals who prefer clean air sort into low-pollution counties). As a result, an estimated $\psi_T > 0$ is consistent with negative assortative matching under the weak restriction of nonincreasing marginal utilities.

This test may have important implications for the optimal design of regulatory policy. If tastes are homogeneous, then a diminishing marginal utility implies that the marginal benefit of a reduction in pollution is greater in communities with higher preregulation TSPs levels. This is consistent with the CAAAs' annual threshold of 75 $\mu g/m^3$. However,

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[23] The control function approach outlined here follows from Garen (1984), who examined selectivity bias in the returns to education. See Heckman and Vytlacil (1999), Blundell and Powell (2000), Florens et al. (2000), Manski and Pepper (2000), and Vytlacil (2000) for a discussion of these and related issues.

if there is taste heterogeneity and sorting based on this heterogeneity, then those with a greater distaste for pollution will sort to areas with lower TSPs levels. Here, the welfare gain from a reduction in TSPs may be greater in communities with *lower* pollution levels, a possibility that the current design of the CAAAs effectively ignores.

VI. Initial Evidence on the Validity of the CAAAs as a Quasi Experiment

This section provides initial evidence on the validity of mid-1970s non-attainment status as an instrumental variable. First, we demonstrate that TSPs nonattainment status is strongly correlated with declines in TSPs concentrations and increases in housing prices. These findings are robust to exploiting the discreteness of the rule that determines TSPs nonattainment status. Second, we provide theoretical and empirical rationales for using mid-1970s TSPs nonattainment status as an instrument, instead of nonattainment status at the beginning of the decade. We also highlight several problems with the conventional cross-sectional and fixed-effects estimation strategies.

A. *TSPs Nonattainment Status and Changes in TSPs Concentrations and Housing Prices*

Figure 2 examines the initial impact of the 1970 CAAAs on TSPs concentrations. The counties with continuous monitor readings from 1967 to 1975 are stratified by their regulatory status in 1972, which is the first year in which the CAAAs were in force.[24] The horizontal line at 75 μg/m^3 is the federal annual standard, and the vertical line separates the preregulation years (1967–71) from the years in which the regulations were enforced. The exact TSPs concentration is reported at each data point.

Before the CAAAs, TSPs concentrations are approximately 35 μg/m^3 higher in the nonattainment counties. The preregulation time-series patterns of the two groups are virtually identical. From 1971 to 1975, however, the set of 1972 nonattainment counties had a stunning 22 μg/m^3 reduction in TSPs, whereas TSPs fell by only 6 μg/m^3 in attainment counties, continuing their pre-1972 trend. This implies that virtually the entire national decline in TSPs from 1971 to 1975 in figure 1 is attributable to the regulations.

Figure 3 demonstrates that mid-1970s nonattainment status is also associated with reductions in TSPs concentrations. Here, counties are divided into those that are nonattainment in either or both 1975 and

[24] The sample consists of 228 counties with a total population of 89 million in 1970.

FIG. 2.—1967–75 trends in TSPs concentrations, by 1972 attainment status. The data points are derived from the 228 counties that were continuously monitored in this period. The 116 attainment counties had a 1970 population of approximately 25.8 million people, whereas about 63.4 million people lived in the 112 nonattainment counties in the same year. Each data point is the unweighted mean across all counties in the relevant regulatory category.
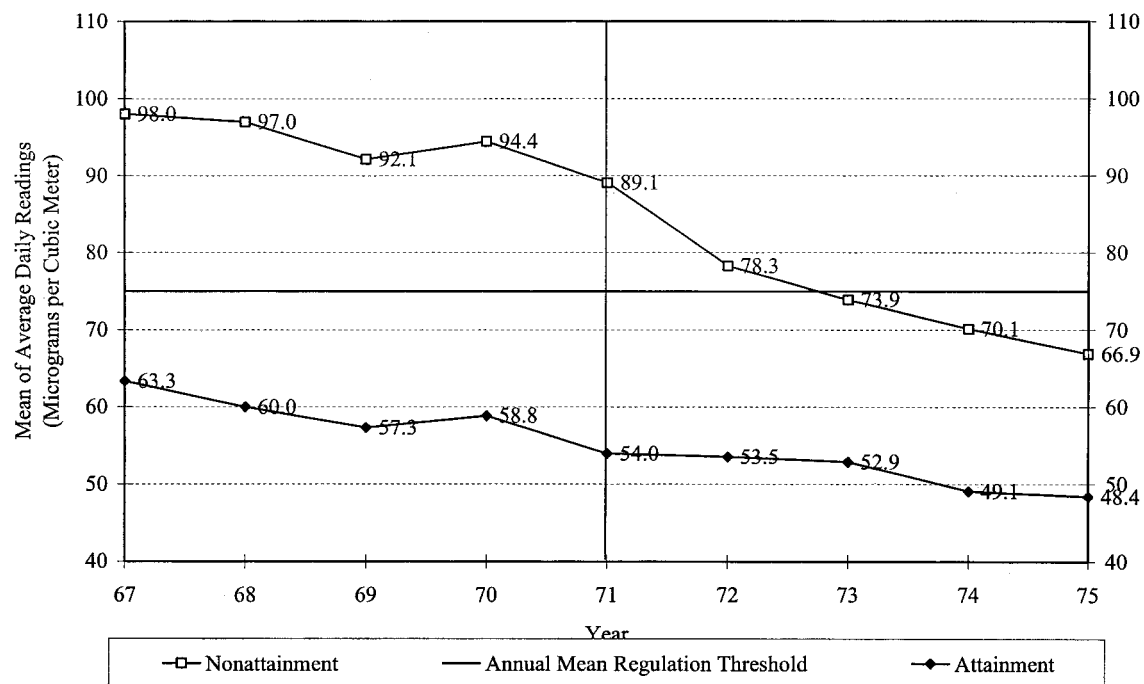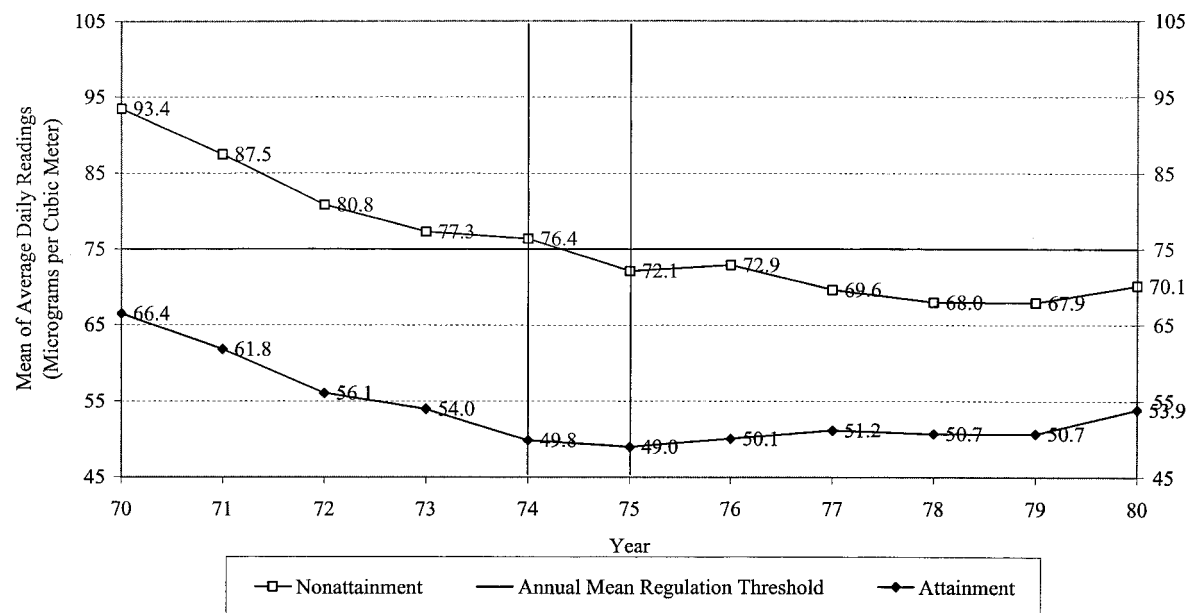
Fig. 3.—1970–80 trends in TSPs concentrations, by 1975–76 nonattainment status. The data points are derived from the 414 counties that were continuously monitored in this period. The 265 attainment counties had a 1970 population of approximately 56.1 million people, whereas roughly 67.5 million people lived in the 149 nonattainment counties in the same year.

1976 and those that are attainment in both years. TSPs concentrations are plotted for the 414 counties with monitor readings in every year from 1970 to 1980. Average TSPs concentrations decline by approximately 17 $\mu g/m^3$ in both sets of counties between 1970 and 1974. While the 1975–76 nonattainment counties are more likely to also be nonattainment in 1972, this suggests that, at least as it relates to pre-existing trends, the attainment counties may form a valid counterfactual for the nonattainment ones.[25] For example, mean reversion and differential trends are not likely sources of bias. Between 1974 and 1980, mean TSPs concentrations declined by 6.3 $\mu g/m^3$ in nonattainment counties and increased by 4.1 $\mu g/m^3$ in attainment counties. Consequently, mid-decade nonattainment status is associated with roughly a 10 $\mu g/m^3$ relative improvement in TSPs.[26]

Figures 4 and 5 provide additional graphical evidence of the validity of mid-decade TSPs nonattainment status as an instrument. Separately for the 1975 attainment and nonattainment counties, the figure graphs the bivariate relation between the 1970–80 changes in mean TSPs (fig. 4) and log housing prices (fig. 5) and the geometric mean of TSPs levels in 1974. Recall that this is the selection year for the 1975 nonattainment designation.

The plots come from the estimation of nonparametric regressions that use a uniform kernel density regression smoother; thus they represent a moving average of the changes across 1974 TSPs levels, unadjusted for any covariates.[27] The difference in the plots for attainment and nonattainment counties can be interpreted as the impact of 1975 nonattainment status. Notably, these differences can be estimated separately for the nonattainment counties that exceed the annual threshold and those that exceed only the daily concentration threshold (i.e., counties with 1974 TSPs concentrations below 75 $\mu g/m^3$). Thus the figure also provides an opportunity to visually analyze the comparisons underlying the regression discontinuity and "bad day" tests of robustness.

[25] Seventy-three of the 265 (117 of the 149) 1975–76 TSPs attainment (nonattainment) counties were TSPs nonattainment in 1972. Thus over 25 percent of the counties switched their nonattainment status between the beginning and the middle of the decade.

[26] The results are qualitatively similar when fig. 3 is based on the 988 counties in our primary sample. When a figure similar to fig. 3 is constructed for the 1980s, the reduction in TSPs attributable to mid-decade regulations cannot be distinguished from differential responses to the 1981–82 recession. This finding is not surprising given the geographic variation in the effect of the 1981–82 recession (Chay and Greenstone 2003*b*) and the termination of the TSPs regulatory program in 1987. For these and others reasons, this study focuses solely on the 1970s. Chay and Greenstone (1998) provide a fuller discussion of these issues and present the results from strategies that address the problems in estimating the hedonic price schedule for the 1980s.

[27] The smoothed scatter plots are qualitatively similar for several different choices of bandwidth, e.g., bandwidths that use between 10 and 20 percent of the data to calculate local means.
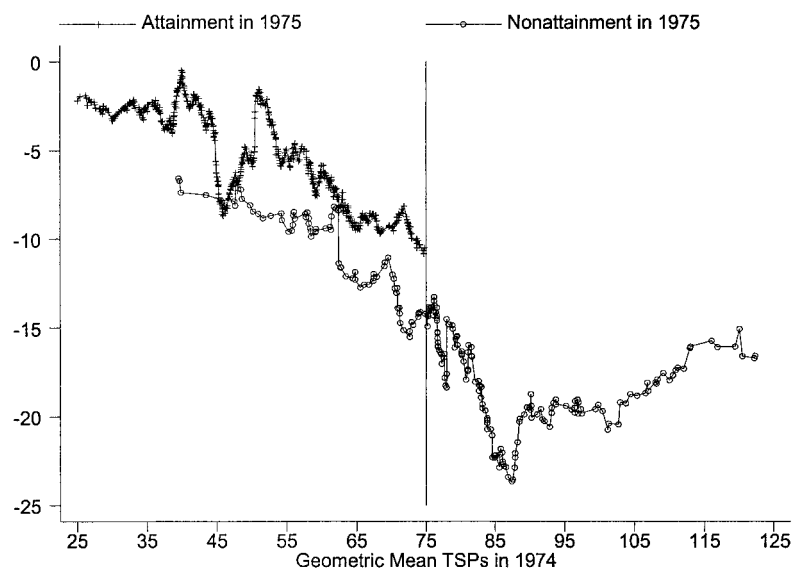
Fig. 4.—1970–80 change in mean TSPs by 1975 nonattainment status and the geometric mean of TSPs in 1974.
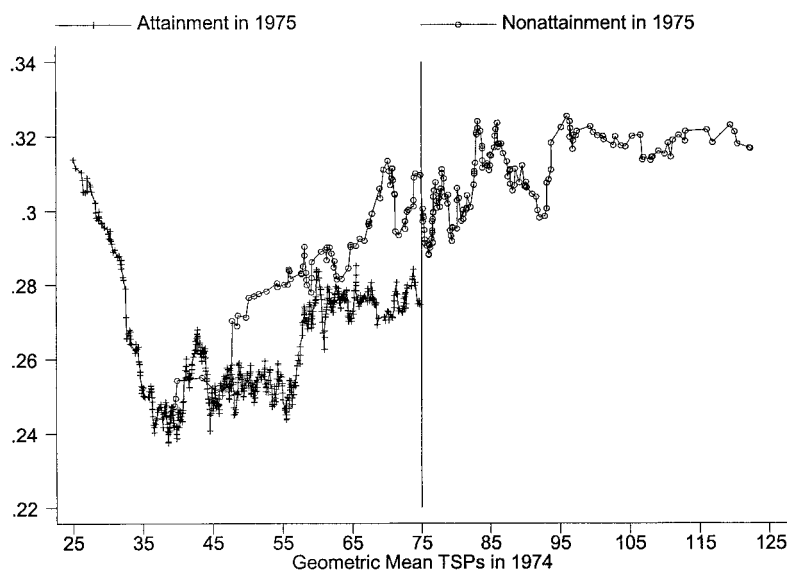

Fig. 5.—1970–80 change in log housing values by 1975 nonattainment status and the geometric mean of TSPs in 1974.

Figure 4 presents evidence on the effect of TSPs nonattainment status on changes in air quality. First, one can compare the nonattainment counties with selection year TSPs concentrations just above 75 $\mu g/m^3$ (demarcated by the vertical line in the graph) and attainment counties just below this threshold. This comparison reveals that at this regulatory threshold, nonattainment counties had an approximately 5 $\mu g/m^3$ larger decline in TSPs than attainment counties. Comparisons of the counties with selection year TSPs concentrations in the 65–85 $\mu g/m^3$ range show that the nonattainment counties had anywhere from a 10 to a 14 $\mu g/m^3$ greater reduction in mean TSPs. The size of the TSPs reductions falls for counties with 1974 mean concentrations greater than 90 $\mu g/m^3$. The slight downward slope in the plot for attainment counties is consistent with some reversion in TSPs.

Second, consider the counties with annual mean concentrations below 75 $\mu g/m^3$. Here, the nonattainment counties received this designation for having as few as two bad days (there are 67 such counties). A comparison of counties with selection year mean TSPs concentrations in the same range suggests that nonattainment status leads to a five-unit greater reduction in mean TSPs over the decade.

Figure 5 illustrates the effect of mid-decade nonattainment status on housing price changes. Both sets of comparisons indicate a clear association between 1975 nonattainment status and greater increases in property values over the decade.[28] They suggest that nonattainment counties had about a 0.02–0.04 log point relative increase in housing prices.

The discrete differences between nonattainment and attainment counties in TSPs and housing price changes shown in figures 4 and 5 support a claim of causal relationships between 1975 TSPs nonattainment status and these outcomes. The finding that nonattainment status results in reductions in TSPs contradicts recent claims that the Clean Air Act failed to reduce air pollution concentrations (Goklany 1999).

Figures 4 and 5 foreshadow the instrumental variables results on the relationship between housing prices and TSPs. In particular, the strong correspondence between the patterns in both panels suggests a causal relationship between air pollution and property values through the mechanism of regulation. The figure also implies that the MWTP may vary with the level of TSPs concentrations. Specifically, the ratio of the nonattainment-attainment difference in increases in housing prices to the difference in mean declines in TSPs is lower in magnitude in dirtier

[28] There are no nonattainment counties with 1974 geometric mean TSPs concentrations below 40 $\mu g/m^3$. The counties with mean TSPs concentrations below 40 $\mu g/m^3$ have much smaller populations and noticeably different characteristics than the counties with higher TSPs concentrations. Therefore, these counties may not be comparable to the other monitored counties.

counties (i.e., those with 1974 TSPs concentrations above 75 $\mu g/m^3$). One explanation for this difference is that individuals with strong preferences for clean air systematically sorted to the relatively clean areas. Section VII reports results from the estimation of equation (11) to test for this possibility more formally.

B.   Mid-Decade TSPs Nonattainment Status as an Instrument

In our context, there are theoretical and empirical reasons why mid-decade nonattainment status is a better candidate as an instrument than the beginning of decade nonattainment designation. First, we consider the theoretical rationale. Since county-level housing price data are unavailable annually, this study uses 1970 and 1980 census data. Figure 2 suggests a two- to three-year lag before the full effects of TSPs nonattainment status on pollution levels are realized. Thus a focus on counties that are attainment and nonattainment in the early 1970s would leave five to six years for households to move to new locations and for housing supply to respond to the resultant air quality changes before housing values in these counties are observed again in 1980.

This potential behavioral response implies that instrumental variables estimates that use early-decade nonattainment status as an instrument will not provide the MWTP for a fixed set of people or a representative agent. Consistent with this possibility, we find evidence that the relative composition of people in the early-decade nonattainment counties changed between 1970 and 1980. We also find evidence that the relative housing stock changed in early-decade nonattainment counties, leaving open the possibility that housing composition changed in unobservable ways as well. Thus, to the extent that local housing markets are integrated over this time horizon, it is invalid to use 1970–80 housing price changes to measure the MWTP for a reduction in TSPs in the early 1970s.[29]

The use of mid-decade nonattainment status as an instrument may mitigate this problem. Specifically, changes in air quality due to mid-decade regulation are not evident until the end of the 1970s, which corresponds roughly with the timing of the 1980 census data on housing values. Thus there is a smaller window of time for general equilibrium responses to affect the composition of households and houses in this set of attainment and nonattainment counties. The nearly identical change in TSPs levels in these counties before 1975 shown in figure 3

[29] For example, Blanchard and Katz (1992) find that local housing prices fall in the first five years following a negative local employment shock but rebound fully within about a decade of the shock. This rebound in prices can be interpreted as the general equilibrium responses of consumers and suppliers of housing.

suggests little impetus for an air quality–induced housing market response from 1970 to 1974 as well.

The second rationale for our focus on mid-decade TSPs nonattainment status is that it is uncorrelated with most observable determinants of housing prices, including economic shocks. This is not true of early-1970s nonattainment status or of TSPs levels and first differences.

Table 2 shows the association of TSPs levels, TSPs changes, and TSPs nonattainment status with numerous determinants of housing prices. The entries in each column are the differences in the means of the variables across two sets of counties and the standard errors of the differences (in parentheses). In columns 1–4, the sample is our base set of 988 counties. The results in column 5 are derived from the "regression discontinuity" sample of 475 counties that are near the annual regulatory threshold, that is, nonattainment counties that have 1974 geometric mean TSPs concentrations between 75 and 100 $\mu g/m^3$ and attainment counties with 1974 mean concentrations between 50 and 75 $\mu g/m^3$. Column 6 presents results for the bad day sample, that is, the 419 attainment and nonattainment counties with 1974 mean concentrations between 50 and 75 $\mu g/m^3$.

Column 1 presents the mean difference in the 1970 values of the covariates between counties with 1970 TSPs concentrations greater and less than the median 1970 county-level TSPs concentration. If TSPs levels were randomly assigned across counties, one would expect very few significant differences. However, there are significant differences between the two sets of counties for several key variables, including income per capita, population, population density, urbanization rate, the poverty rate, the fraction of houses that are owner-occupied, and the share of government spending on education. Mean housing values are *higher* in the dirtier counties, although this difference is not statistically significant at conventional levels. Although it is not presented here, an analogous examination of the means in 1980 leads to similar conclusions. Overall, these findings suggest that "conventional" cross-sectional estimates may be biased because of omitted variables or incorrect specification of the functional form of the observable variables in the regression equation.

Column 2 performs a similar analysis for the 1970–80 changes in TSPs. Here, the entries are the mean difference in the change in the covariates between counties with a change in TSPs that is less than (i.e., larger declines) and greater than the median change in TSPs. Reductions in TSPs are highly correlated with economic shocks. The counties with large declines in pollution experienced substantially less growth in per capita income, smaller population growth, a bigger increase in unemployment rates, a larger decline in manufacturing employment, and less new home construction. It appears that TSPs levels are procyclical.

Unless the regression model can perfectly control for the economic cycle, the fixed-effects estimator of the hedonic price schedule will have a positive bias. For example, the second row shows that housing values grew more in the counties that had a relative *increase* in TSPs levels.

Column 3 compares the 1970–80 changes in the variables of counties that are nonattainment and attainment in the early 1970s. Here, a county is designated nonattainment if it exceeds the federal standards in any of the years 1970, 1971, or 1972; all other counties are in the attainment category. The nonattainment counties had a smaller increase in per capita income and larger declines in population, manufacturing employment, new home construction, and population density than the attainment counties. We suspect that the population flows reflect a substantial worsening of economic conditions in nonattainment counties, possibly because of nonneutral impacts of the 1974–75 recession and/or the economic effects of the regulations themselves (e.g., Greenstone 2002). These entries suggest that estimates that rely on early-decade TSPs nonattainment status as an instrument will be positively biased.[30]

Column 4 repeats this analysis among TSPs nonattainment and attainment counties in 1975–76 and finds that the observables are better balanced across these counties. Importantly, the mid-decade nonattainment instrument purges the nonneutral economic shocks apparent above. For example, the differences in the changes in per capita income, total population, unemployment rates, manufacturing employment, and new home construction among the two sets of counties are all smaller in magnitude than in the other columns and statistically indistinguishable from zero. Also, nonattainment and attainment counties had nearly identical changes in urbanization rates during the 1970s, suggesting that differential "urban sprawl" within counties is not a source of bias.[31] Notably, these nonattainment counties had both a greater reduction in TSPs and a greater increase in housing values from 1970 to 1980, foreshadowing our instrumental variables results.

Finally, columns 5 and 6 compare the covariates across 1975 TSPs nonattainment and attainment counties for the regression discontinuity and bad day samples. It is evident that the observables are well balanced by nonattainment status. In fact, none of the mean differences in col-

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[30] Counties that were nonattainment for TSPs in 1973–74 also had statistically significant larger declines in population and increases in the poverty rate.

[31] While there are some significant differences between nonattainment and attainment counties in 1970 values of the variables, the hypothesis of equal population densities in 1970 cannot be rejected at conventional levels.

TABLE 2
DIFFERENCES IN SAMPLE MEANS BETWEEN GROUPS OF COUNTIES, DEFINED BY TSPS LEVELS, CHANGES, OR NONATTAINMENT STATUS

			TSPs NONATTAINMENT			
	CROSS SECTION 1970 (1)	FIRST DIFFERENCE 1980−1970 (2)	In 1970, 1971, or 1972 (3)	In 1975 or 1976 (4)	In 1975 Regression Discontinuity Sample (5)	In 1975 Bad Day Sample (6)
Total counties (nonattainment)	988	988	988 (380)	988 (280)	475 (123)	419 (67)
Housing value	1,092 (918)	−3,237** (713)	−517 (726)	2,609** (806)	2,007 (1,193)	2,503 (1,585)
Mean TSPs	39.2** (1.2)	−30.9** (1.0)	−19.6** (1.4)	−10.0** (1.8)	−12.3** (2.4)	−4.8 (2.9)
Economic condition variables:						
Income per capita (1982–84 dollars)	377.7** (94.7)	−159.9** (40.7)	−81.6* (41.2)	48.6 (46.4)	47.2 (65.1)	−37.2 (94.1)
Total population (% change)	142,016** (24,279)	−.058** (.013)	−.046** (.013)	−.001 (.017)	.005 (.028)	.015 (.030)
Unemployment rate ($\times 100$)	−.144 (.120)	.519** (.129)	.200 (.132)	.043 (.152)	.305 (.215)	−.032 (.274)
% employment in manufacturing ($\times 10$)	.098 (.083)	−.119** (.026)	−.081** (.026)	−.005 (.028)	−.057 (.042)	−.066 (.051)
Demographic and socioeconomic variables:						

Population density	602.3**	−66.9**	−100.5**	−18.0	1.0	42.6
	(192.6)	(24.8)	(31.4)	(24.9)	(48.0)	(49.7)
% urban (×10)	1.413**	−.051	−.087	−.009	−.021	.124
	(.168)	(.051)	(.048)	(.053)	(.062)	(.088)
% poverty (×10)	−.118**	.107**	.154**	.139**	.029	.173**
	(.046)	(.024)	(.024)	(.024)	(.040)	(.034)
% white (×10)	.119	−.072**	−.224**	−.195**	−.086	−.124
	(.083)	(.031)	(.032)	(.036)	(.054)	(.066)
Housing stock variables:						
% of houses built in last 10 years		−.034**	−.025**	−.006	−.006	.007
		(.007)	(.007)	(.008)	(.012)	(.016)
% owner-occupied (×10)	−.127*	.081*	.127**	.082*	.046	−.109
	(.055)	(.036)	(.033)	(.037)	(.044)	(.064)
% houses no plumbing	−.005**	−.055**	−.073**	−.075**	.013	−.077**
(×1,000)	(.001)	(.017)	(.018)	(.018)	(.031)	(.019)
Tax and expenditure variables:						
Per capita government revenue	23.8	77.3*	60.2	44.6	10.2	101.9
	(24.7)	(34.2)	(42.6)	(30.2)	(49.0)	(68.6)
Per capita property taxes	8.5	26.0**	7.2	−1.1	−1.7	14.6
	(11.7)	(9.6)	(10.3)	(9.4)	(12.9)	(19.2)
% of spending on education	−.030**	−.006	−.009	.012	.009	−.020
	(.008)	(.006)	(.006)	(.007)	(.009)	(.012)

NOTE.—See the note to table 1. The entries in each column are the differences in the means of the variables across two sets of counties and the standard errors of the differences (in parentheses), which allow for heteroskedasticity. Col. 1 presents the mean difference in the 1970 values of the covariates between counties with 1970 TSPs concentrations greater and less than the median 1970 county-level TSPs concentration, respectively. Col. 2 reports the analogous calculations for 1970–80 changes in TSPs; i.e., the entries are the mean difference in the change in the covariates between counties with a change in TSPs that is less than and greater than the median change in TSPs. The entries in cols. 3 and 4 are the mean difference of the 1970–80 change in the covariates between 1971–72 and 1975–76 nonattainment and attainment counties, respectively. The entries in cols. 5 and 6 compare 1970–80 changes for 1975 nonattainment and attainment counties; the samples are restricted to the regression discontinuity and bad day test samples as described in the text. See the text for more details.

* Significant at the 5 percent level.

** Significant at the 1 percent level.

umn 5 are statistically different from zero, and only two in column 6 are.[32]

Although a direct test of the validity of the exclusion restriction is unavailable, it is reassuring that our instrumental variable is largely uncorrelated with observable determinants of housing prices. Overall, the results in this table suggest that using mid-decade nonattainment status as an instrumental variable has important advantages over "conventional" estimation strategies, as well as over the use of beginning-of-decade nonattainment status as an instrumental variable. In particular, the instrument appears to purge the local demand and supply shocks that may contaminate estimates based on first differences. Finally, since the regulations are federally mandated, their imposition is presumably orthogonal to the local political process determining the supply of nonmarket amenities.[33]

Before we proceed, figure 6 provides a graphical overview of the location of the 1975–76 TSPs nonattainment and attainment counties. A county's shading indicates its regulatory status: light gray for attainment, black for nonattainment, and white for the counties without TSPs pollution monitors. The pervasiveness of the regulatory program is evident. For example, 45 of the 50 states had at least one county designated nonattainment. Below, this allows us to control for region-specific determinants of the change in housing prices between 1970 and 1980.

VII.   Regression-Adjusted Estimates of the Hedonic Price Schedule Gradient and the Average MWTP

Here, we present the estimates of the hedonic price schedule gradient and the average MWTP from the econometric models discussed above. There are three main findings. First, conventional regression analysis produces unreliable estimates of the hedonic price schedule gradient. Second, the 1975–76 TSPs nonattainment instrumental variable produces robust estimates that imply that individuals place greater value on clean air than previously recognized. Finally, the results from the random coefficients model provide evidence of taste-based sorting in

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[32] Further, the differences in the 1970 levels of the variables are also smaller in these restricted samples. This is especially true in the regression discontinuity sample, where the hypothesis of equal means cannot be rejected for all the covariates except "% urban" (p-value = .037).

[33] Scientific evidence provides additional support for the credibility of regulation instruments that depend on pollution levels. Cleveland et al. (1976) and Cleveland and Graedel (1979) document that wind patterns often transport air pollution hundreds of miles and that the ozone concentration of air *entering* into the New York region in the 1970s often exceeded the federal standards. A region's topographical features can also affect pollution concentrations. Counties located in valleys (e.g., Los Angeles, Phoenix, Denver, and the Utah Valley) are prone to weather inversions that lead to prolonged periods of high TSPs concentrations.

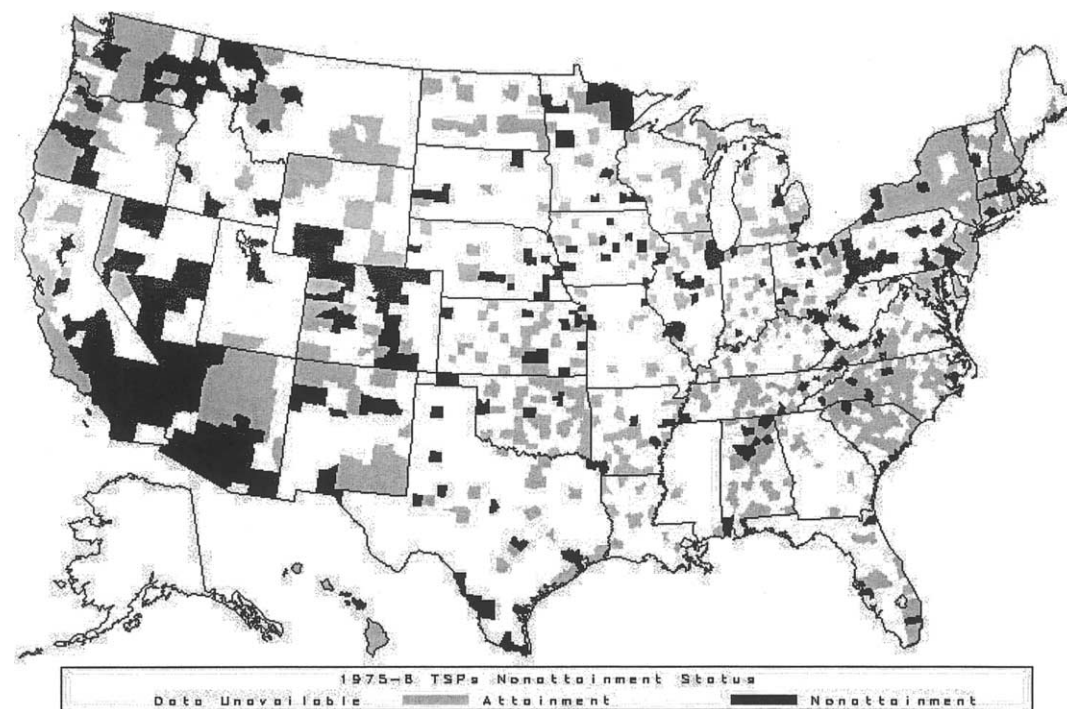FIG. 6.—Incidence of 1975–76 TSPs nonattainment status. In the primary sample of 988 counties, there are 280 nonattainment and 708 attainment counties. They are pictured in black and gray, respectively. The 2,169 counties without complete data are depicted in white. The nonattainment designations are determined from the EPA's Air Quality Subsystem Database. See the Data Appendix for further details.

TABLE 3
Cross-Sectional and First-Difference Estimates of the Effect of TSPs
Pollution on Log Housing Values

	(1)	(2)	(3)	(4)
	A. 1970 Cross Section			
Mean TSPs (1/100)	.032	−.062	−.040	−.024
	(.038)	(.018)	(.017)	(.017)
R^2	.00	.79	.84	.85
Sample size	988	987	987	987
	B. 1980 Cross Section			
Mean TSPs (1/100)	.093	.096	.076	.027
	(.066)	(.031)	(.030)	(.028)
R^2	.00	.82	.89	.89
Sample size	988	984	984	984
	C. 1970–80 (First Differences)			
Mean TSPs (1/100)	.102	.024	.004	−.006
	(.032)	(.020)	(.016)	(.014)
R^2	.02	.55	.65	.73
Sample size	988	983	983	983
County Data Book covariates	no	yes	yes	yes
Flexible form of county covariates	no	no	yes	yes
Region fixed effects	no	no	no	yes

Note.—See the notes to tables 1 and 2. For 1970 and 1980, the mean TSPs variable is the 1969–72 and 1977–80 average of the annual geometric mean concentrations, respectively. See the Data Appendix for a full list of the control variables. The flexible functional form includes quadratics, cubics, and interactions of the variables as controls. The mean of the natural log of 1970 housing prices is 10.55. The means of the dependent variables in panels B and C are 10.82 and 0.27, respectively. Standard errors (in parentheses) are estimated using the Eicker-White formula to correct for heteroskedasticity.

==**equilibrium, but they also suggest that the overall variation in county-level MWTP is not large.**==

A.   Conventional Estimates of the Hedonic Price Schedule Gradient

Table 3 presents "conventional" estimates of the capitalization of TSPs into property values from the 1970 and 1980 cross sections and the 1970–80 first differences. These estimates provide a useful benchmark since they are based on regression specifications typically used in the literature. For the 1970 and 1980 cross sections, column 1 gives the unadjusted correlation, column 2 allows the observables to enter linearly, column 3 adds cubic polynomials and interactions of the control variables, and column 4 adds unrestricted region effects for each of the nine Census Bureau divisions so that the identification comes from within-region comparisons of counties. These four specifications are used throughout the remainder of the analysis.

For 1970 the unadjusted correlation between housing prices and TSPs has a counterintuitive sign but is statistically insignificant. However, the

correlation adjusted for a linear combination of the observables suggests that a one-unit decline in TSPs leads to an approximately 0.06 percent increase in housing values. While the implied elasticity is only -0.04, the estimate would be judged statistically significant at conventional levels. Also, it is in the middle of the range of estimates summarized in the Smith and Huang (1995) meta-analysis. This is noteworthy since it is based on a time period and regression specification similar to those used in the bulk of the previous research.

The estimate implies that if Allegheny County, which is in Pittsburgh, reduced its 1970 TSPs levels by 50 percent (a 65 $\mu g/m^3$ reduction), housing prices would increase by only 4 percent or about \$3,200 (2001 dollars), all else equal. Further, the estimate is reduced when the analysis adjusts for a flexible functional form and interactions for the covariates in column 3 and becomes even smaller and statistically insignificant when a full set of region indicators are included in column 4. Notably, the fit of the regressions in columns 2–4 is quite good.

The 1980 results also raise questions about the reliability of the cross-sectional approach. Here, a linear covariate adjustment leads to the perverse result that a one-unit reduction in TSPs is associated with a 0.10 percent *decrease* in housing prices. This is particularly disturbing given the estimate's precision and the excellent fit of the regression equation ($R^2 = 0.82$). Further, this is the same specification that produced an estimate similar to the "meta-estimate" from the previous literature. Controlling for nonlinearities and interactions in the covariates and unrestricted region effects reduces the magnitude of the estimate, but it is still perversely signed.

Panel C of the table contains the 1970–80 first-differenced results. First-differencing the data eliminates the bias in the cross-sectional estimates attributable to permanent differences across counties. However, this approach will be biased if it is not possible to adequately control for shocks that drive both pollution and price changes. In column 1, the unadjusted correlation between changes in housing prices and TSPs has the perverse positive sign and is highly significant. This finding was foreshadowed by the results in column 2 of table 2. Adjustment for the covariates in columns 2–4 causes the estimate to become economically small and statistically indistinguishable from zero.

These results represent our effort to replicate the previous literature's approach. Overall, with county-level data on almost 1,000 counties, the cross-sectional and fixed-effects correlation between TSPs and property values is weak and very sensitive to the choice of specification. On the basis of these conventional estimation procedures, we conclude that either individuals place a small value on air quality or the hedonic price schedule gradient is plagued by substantial omitted variables bias.

TABLE 4
ESTIMATES OF THE IMPACT OF MID-DECADE TSPs NONATTAINMENT ON 1970–80
CHANGES IN TSPs POLLUTION AND LOG HOUSING VALUES

	(1)	(2)	(3)	(4)
	A. Mean TSPs Changes			
TSPs nonattainment in 1975 or 1976	−9.96	−10.41	−9.57	−9.40
	(1.78)	(1.90)	(1.94)	(2.02)
F-statistic TSPs nonattainment*	31.3	29.9	24.4	21.5
	(1)	(1)	(1)	(1)
R^2	.04	.10	.19	.20
	B. Log Housing Changes			
TSPs nonattainment in 1975 or 1976	.036	.022	.026	.019
	(.012)	(.009)	(.008)	(.008)
F-statistic TSPs nonattainment*	8.5	6.2	9.3	6.4
	(1)	(1)	(1)	(1)
R^2	.01	.56	.66	.73
County Data Book covariates	no	yes	yes	yes
Flexible form of county covariates	no	no	yes	yes
Region fixed effects	no	no	no	yes
Sample size	988	983	983	983

NOTE.—See the notes to previous tables. In panel A the dependent variable is the difference between the 1977–80 and 1969–72 averages of mean TSPs concentrations. The mean is −7.82 $\mu g/m^3$. In panel B the dependent variable is the difference between 1980 and 1970 log housing values, and its mean is 0.27. Standard errors (in parentheses) are estimated using the Eicker-White formula to correct for heteroskedasticity.

* Numbers in parentheses in rows with F-statistics are numerator degrees of freedom.

B.   Instrumental Variables Estimates of the Hedonic Price Schedule Gradient

Reduced-form relations.—Table 4 reports the results from estimating equations (6) and (8). The regulation variable is an indicator equal to one if the county was nonattainment in either 1975 or 1976 (or both years). Column 1 presents the unadjusted estimates, and columns 2–4 present the estimates from the same specifications as in table 3.

Panel A shows that mid-decade nonattainment status is associated with a 9–10 $\mu g/m^3$ (11–12 percent) reduction in TSPs. This estimate is insensitive to a wide set of controls, including region fixed effects as in column 4. Further, it is highly significant with an F-statistic ranging between 22 and 31 depending on the specification, suggesting that it is the most important (observable) determinant of 1970–80 changes in TSPs. Thus the first-stage impact of regulation is indeed very powerful.

Panel B reveals another striking empirical regularity. The TSPs nonattainment variable is associated with a 2–3.5 percent relative increase in housing values from 1970 to 1980. These estimates are also highly significant. The adjusted estimates are on the low end of this range, but after the linear adjustment in column 2, further controls have little effect on the estimate, even as there is a large improvement in the regression fit (e.g., $R^2 = 0.73$ in col. 4).

TABLE 5
INSTRUMENTAL VARIABLES ESTIMATES OF THE EFFECT OF 1970–80 CHANGES IN TSPs
POLLUTION ON CHANGES IN LOG HOUSING VALUES

	(1)	(2)	(3)	(4)
	A. TSPs Nonattainment in 1975 or 1976			
Mean TSPs (1/100)	−.362	−.213	−.266	−.202
	(.152)	(.096)	(.104)	(.090)
Sample size	988	983	983	983
	B. TSPs Nonattainment in 1975			
Mean TSPs (1/100)	−.350	−.204	−.228	−.129
	(.150)	(.099)	(.102)	(.084)
Sample size	975	968	968	968
	C. TSPs Nonattainment in 1970, 1971, or 1972			
Mean TSPs (1/100)	.072	−.032	−.050	−.073
	(.058)	(.042)	(.041)	(.035)
Sample size	988	983	983	983
County Data Book covariates	no	yes	yes	yes
Flexible form of county covariates	no	no	yes	yes
Region fixed effects	no	no	no	yes

NOTE.—See the notes to previous tables. The coefficients are estimated using 2SLS. The first row of panels A–C indicates which instrument is used. From panels A to C, the instruments are an indicator equal to one if the county was nonattainment for TSPs in either 1975 or 1976, an indicator equal to one if the county was nonattainment for TSPs in 1975, and an indicator that equals one if the county was nonattainment for TSPs in either 1970, 1971, or 1972, respectively. Standard errors (in parentheses) are estimated using the Eicker-White formula to correct for heteroskedasticity.

Taken literally, these results imply that the federal TSPs nonattainment designation resulted in substantial improvements in air quality and property values in these counties. These findings are important in their own right because they indicate that the Clean Air Act's regulation of TSPs had substantial benefits during the 1970s.

Instrumental variables estimates.—Table 5 contains the instrumental variables estimates of the hedonic price schedule gradient derived from three different instruments. In panel A the instrument is the 1975–76 nonattainment indicator, so the reported θ_{IV} is simply the ratio of the reduced-form estimates in table 4. The estimates suggest that a 1 μg/ m^3 reduction in mean TSPs results in a 0.2–0.4 percent increase in property values, which is a −0.20 to −0.35 elasticity. This is roughly five to eight times larger than the largest cross-sectional estimate in table 3.[34] Further, these estimates are largely insensitive to regression adjust-

[34] The analysis is unweighted. It is natural to consider weighting by the square root of the population for efficiency purposes. In this setting, this weighting procedure increases the standard error on the mean TSPs coefficient by at least a factor of two. Nevertheless, some readers are likely to be interested in the results from this approach that allow larger counties to have a greater influence on the results. When we use the square root of the sum of 1970 and 1980 county-level populations as weights, the 1975–76 TSPs nonattainment instrument and the cols. 1–4 specifications yield estimates (standard errors) of −0.576 (0.526), −0.364 (0.204), −0.498 (0.259), and −0.379 (0.186).

ment, which is not surprising given the findings in table 2.[35] Thus concerns about which of the measured variables belong in the hedonic price schedule equation are unfounded in this setting.[36]

Panel B presents instrumental variables estimates when the instrument is 1975 TSPs nonattainment status. This provides the statistical analogue to the plots in figures 4 and 5. The parameter estimates are very similar to the estimates in panel A. Overall, these results also suggest that there is a robust association between changes in TSPs and housing prices.

Panel C presents instrumental variables results when the instrument is based on 1970–72 TSPs nonattainment status. As Section V.B details, we suspect that these estimates are biased upward, but they are presented for completeness. The unadjusted estimate in column 1 has a perverse sign. As we control for more covariates, the sign reverses. In fact, the column 4 estimates imply that a 1 $\mu g/m^3$ reduction in mean TSPs results in a statistically significant increase of 0.07 percent in property values. This pattern of the coefficients is consistent with our concerns about the validity of the instrument due to nonneutral economic shocks and general equilibrium responses to the regulation-induced changes in air quality documented in table 2.[37]

Robustness tests.—Table 6 presents the estimation results from the regression discontinuity and bad day matching tests of the robustness of the estimates in table 5. The specifications in panel A (regression discontinuity I) use 1975–76 nonattainment status as an instrument but drop from the sample the 95 counties that are nonattainment only

[35] The instrumental variables point estimates are larger when we adjust for the levels of the variables in 1970 rather than the 1970–80 changes. For example, with the 1975–76 TSPs nonattainment instrument, the instrumental variables estimates (standard errors) are −0.481 (0.183) and −0.400 (0.174) when the analysis adjusts linearly for the 1970 levels of the controls (i.e., col. 2) and when the natural log of 1970 housing price is added to this specification, respectively.

[36] We also explored the consequences of limiting the sample to the 428 counties located in standard metropolitan statistical areas (SMSAs). The estimates across the four specifications are −0.297 (0.206), −0.417 (0.213), −0.462 (0.300), and −0.412 (0.269). The standard errors are similar when the variance-covariance matrix allows for correlation within SMSAs rather than the Eicker-White standard errors reported here. There is little variation in nonattainment status within SMSAs, so the inclusion of SMSA fixed effects causes the standard errors to increase dramatically, making meaningful inference impossible. Finally when the sample is limited to the counties that are not part of an SMSA, the estimates across the four specifications are −0.975 (0.513), −0.302 (0.170), −0.222 (0.119), and −0.135 (0.104). In the context of the standard errors, the qualitative findings are similar to those in the SMSA sample.

[37] As n. 25 detailed, 1970–72 TSPs nonattainment status is correlated with 1975–76 TSPs nonattainment status. Owing to this confounding and the possibility that 1970–72 nonattainment status may capture unobserved trends in housing prices, it may be appropriate to include the 1970–72 TSPs nonattainment indicator as a control in the equations in which the 1975–76 nonattainment indicator is the instrumental variable. When this covariate is added to our four primary specifications, the estimates are −2.159 (1.927), −0.612 (0.418), −0.673 (0.425), and −0.466 (0.359). The loss of precision is predictable, but the qualitative results are unchanged.

TABLE 6
ROBUSTNESS OF THE INSTRUMENTAL VARIABLES ESTIMATES OF THE EFFECT OF 1970–80
CHANGES IN TSPs POLLUTION ON CHANGES IN LOG HOUSING VALUES

	(1)	(2)	(3)
	A. Regression Discontinuity I TSPs Non-attainment in 1975 or 1976		
Mean TSPs (1/100)	−.902	−.267	−.449
	(.570)	(.250)	(.383)
Sample size	872	869	869
	B. Regression Discontinuity II TSPs Non-attainment in 1975		
Mean TSPs (1/100)	−.285	−.133	−.122
	(.165)	(.101)	(.119)
Sample size	475	472	472
	C. Bad Day/Matching TSPs Nonattainment in 1975		
Mean TSPs (1/100)	−.498	−.486	−.394
	(.788)	(.580)	(.516)
Sample size	419	416	416
Census covariates	no	yes	yes
Flexible form of census covariates	no	no	yes

NOTE.—See the notes to previous tables. The coefficients are estimated using 2SLS. In panel A, counties that are TSPs nonattainment for the bad day rule only in 1975 and 1976 are dropped from the sample. The instrumental variable is an indicator equal to one if the county was nonattainment for TSPs in either 1975 or 1976. All specifications include quadratics in the 1974 and 1975 county-level geometric means of TSPs and their interaction in order to control for "smooth functions" of the selection variables. In panels B and C, the instrumental variable is an indicator equal to one if the county was nonattainment for TSPs in 1975. In panel B, the sample is limited to counties with 1974 county-level geometric means of TSPs between 50 and 100 μg/m^3; further, counties that are TSPs nonattainment for violating the bad day rule in 1975 but with mean TSPs below the annual threshold are dropped from the sample. In panel C, the sample is restricted to counties with 1974 county-level geometric means of TSPs between 50 and 75 μg/m^3. Standard errors (in parentheses) are estimated using the Eicker-White formula to correct for heteroskedasticity.

because they exceeded the bad day rule; that is, they had geometric mean TSPs below 75 μg/m^3 in 1974 and 1975. To adjust for "smooth" functions of the variables that determine regulatory status, these specifications further control for the geometric means of TSPs in 1974 and 1975, their squares, and their interaction. Thus the TSPs coefficient is identified from discontinuous differences between attainment and nonattainment counties at the annual TSPs regulatory thresholds.

The regression-adjusted instrumental variables estimates in columns 2 and 3 indicate that a one-unit decline in TSPs is associated with a 0.35–0.58 percent increase in housing prices.[38] The estimates have associated t-statistics that are greater than one, but they are not statistically significant at conventional levels. The loss of precision is mostly the consequence of attempting to separately identify the effect of the in-

[38] The parameter estimates are modestly larger in magnitude when the squares of 1974 and 1975 TSPs concentrations are dropped from these specifications. Also, note that the percentage effects are larger than the point estimates since the natural log approximation understates the percentage change.

strument from polynomial functions of 1974 and 1975 TSPs concentrations.

Panel B presents the results from our regression discontinuity II approach. Here, 1975 TSPs nonattainment status is the instrument, and the sample is limited to counties with 1974 TSPs concentrations in the 50–100 $\mu g/m^3$ range. Further, counties that are nonattainment for violating the bad day rule only are dropped, which leaves a sample of 475 counties. The identifying assumption is that the omitted variables are balanced across the counties above and below the annual threshold in this subsample. The estimates imply that a 1 $\mu g/m^3$ reduction in mean TSPs results in a 0.1–0.3 percent increase in housing values, with the adjusted estimates being at the low end of this range.

The results in panel C are also derived from the 1975 instrument, but here the sample is restricted to counties with 1974 TSPs concentrations below the annual regulatory threshold of 75 $\mu g/m^3$ and above 50 $\mu g/m^3$. This test exploits the bad day feature of the regulations. The estimates imply that a 1 $\mu g/m^3$ reduction in mean TSPs results in a 0.50–0.65 percent increase in house prices, although all have an associated t-statistic less than one. The point estimates are nearly identical in specifications that further control for a series of indicator variables for each five-unit interval between 50 and 75 $\mu g/m^3$ of 1974 mean TSPs. This analysis controls flexibly for the annual selection variable, so that identification comes from comparisons of attainment and nonattainment counties with essentially the same annual TSPs levels.

Overall, the results in table 6 support the robustness of the estimates of the hedonic price schedule gradient presented in table 5. This is noteworthy since these tests are derived directly from the structure of the regulations. Thus they are prespecified and rule out ex post rationalizations of unexpected findings. On the other hand, each of these tests is very demanding of the data; consequently, none of the individual estimates would be judged to differ from zero by conventional criteria.

C.   Random Coefficients Estimates of the Average MWTP and Evidence on Taste Sorting

If tastes for clean air are homogeneous, then the 2SLS estimates of the hedonic price schedule gradient in table 5 are consistent estimates of the average MWTP in the population. However, a comparison of the estimates in panels B and C of table 6 (abstracting from the standard errors) indicates that the housing price–TSPs gradient may be steeper at lower concentrations of TSPs. This was also suggested by figures 4 and 5. If there were homogeneous county-level tastes for air quality, this finding would violate the assumption of nonincreasing marginal utility. Consequently, there is at least modest evidence of taste heterogeneity,

TABLE 7
Control Function Estimates of the Capitalization of 1970–80 Changes in TSPs Pollution, with Correction for Selectivity Bias Due to Random Coefficients

	(1)	(2)	(3)	(4)
Mean TSPs (1/100)	−.320	−.196	−.256	−.200
	(.157)	(.101)	(.110)	(.099)
v_i (first-stage residual) (1/100)	.500	.256	.289	.205
	(.164)	(.103)	(.112)	(.100)
v_i × mean TSPs (1/10,000)	.116	.049	.032	.007
	(.043)	(.026)	(.023)	(.021)
Sample size	988	983	983	983
Census covariates	no	yes	yes	yes
Flexible form of census covariates	no	no	yes	yes
Region fixed effects	no	no	no	yes

Note.—The standard errors (in parentheses) are calculated on the basis of 1,000 bootstrap replications of the sequential estimator. See the text for details on the selectivity bias correction when the endogenous variable is continuous. Estimates are insensitive to including polynomials of the arguments of the two control functions.

and the table 5 estimates may represent the average MWTP for a nonrandom subpopulation.

To examine this issue more formally, we estimate the random coefficients regression model specified in equation (11). The model relaxes the single-index restriction of 2SLS and includes separate control functions for the omitted variables and self-selection biases. As long as assumption 1 and the linear conditional expectations restrictions from assumptions 2 and 3 hold, the model will consistently identify the population average MWTP and provide a simple test of county-level taste sorting.

Table 7 presents the results from estimating this model using the 1975–76 TSPs nonattainment variable as the instrument.[39] There are several findings. First, the estimates of the average MWTP are only slightly higher than the 2SLS estimates in table 5. Thus the single control function underlying 2SLS appears to do a reasonable job of absorbing both sources of bias. Second, the estimated coefficient of the first control function, λ_T, is positive and highly significant. This implies that the omitted variables bias in the conventional first-differences estimate is substantial, even after regression adjustment. Third, the selection bias control function also has a positive coefficient estimate ($\psi_T > 0$), which is highly significant in column 1. Under the assumptions of the model, this provides direct statistical evidence of nonrandom taste sorting. The fact that the estimated ψ_T is reduced substantially by regression adjustment implies that much of the county-level taste-sorting behavior can be explained by observable differences across counties.

[39] Some of the regressors in eq. (11) are generated from first-stage estimation. As a result, we calculated the standard errors of this sequential estimator using the bootstrap with 1,000 replications.

These results suggest that negative assortative matching may be a relevant phenomenon in the housing market. However, table 7 suggests that the overall heterogeneity in county-level MWTP across the population is not large. Further, the relative magnitudes of the λ_T and ψ_T estimates imply that omitted variables bias is a much bigger issue than selectivity bias in estimating the hedonic price schedule and MWTP. To probe the robustness of the results to the linear conditional expectations restrictions from assumptions 2 and 3, we also estimated a model that allows polynomials of both control functions to enter the outcome equation. This leads to average MWTP estimates that are identical to those in table 7, suggesting that the linear "approximations" in assumptions 2 and 3 may be robust.

VIII.   Interpretation and Welfare Calculations

We now use the above findings to calculate the economic benefits of the regulations and, more generally, the WTP for air quality.[40] While the gradient of the hedonic price function provides the average MWTP for a one-unit decline in air pollution, a welfare analysis of the non-marginal reductions in TSPs induced by the mid-decade TSPs regulations requires identification of the MWTP *function*.

An ad hoc approach to obtaining this function is to make strong assumptions on its shape. A popular, but likely invalid, assumption is that preferences are homogeneous and linear with respect to air quality, so that the MWTP for clean air is constant (Freeman 1974). In this case, it is straightforward to calculate WTP.[41] The 1975–76 TSPs nonattainment counties had about a 10-unit reduction in mean TSPs, and this

[40] An alternative question is how county managers value the reduction in TSPs concentrations in the long run. This could be measured as the differential change in *aggregate* county-level housing values between nonattainment and attainment counties from 1970 to 1990 (or 2000). Such a measure would attempt to capture the consequences of supply and demand responses to the air quality change rather than to abstract from them. A theoretical limitation of this value is that it cannot be attached to the preferences of any individual consumer(s), so it cannot be readily extrapolated to other contexts. From an empirical standpoint, the estimation of this value would rely on the strong assumption that there are no differential shocks to aggregate property values (e.g., due to economic cycles) that are correlated with the nonattainment designation over long (15–25 years) time horizons.

[41] When the validity of the constant MWTP assumption is set aside, there are some important differences between this measure and an ideal measure of welfare change. First, this measure will tend to *overstate* the welfare gain relative to one derived from a compensated MWTP function that holds utility constant. Second, we assume that consumers and suppliers have not had time to respond to the change in TSPs by moving or changing the supply or quality of the housing stock. However, at the existing hedonic price schedule, some individuals are likely to be made better off by making these changes. Our measure of the welfare change does not account for this type of compensatory behavior and will thus tend to *understate* the true welfare gain. See Bartik (1988) for a clear discussion of these issues.

decline was capitalized into property values at a rate of about 0.28 percent per unit.[42] Since the average value of a house in mid-decade nonattainment counties in 1970 was $86,900 (in 2001 dollars), mean housing values increased by roughly $2,400 in these counties. The Census Bureau's Public-Use Microdata Samples (PUMS) data indicate that there were about 19 million houses in these nonattainment counties. This implies that the WTP for the late 1970s reductions in TSPs is approximately $45 billion.

The $45 billion figure is also an estimate of the increase in local property values attributable to the mid-1970s TSPs regulation. Over longer time periods, it may be reasonable to expect the value of the tax base to increase by even more as supply responds. Nevertheless, this figure is potentially useful for local governments, since it provides a monetary measure of the local benefits of regulation. By this metric, the CAAAs' mid-decade regulation of TSPs provided substantial benefits.

In this paper we follow the previous literature's convention for calculating the value of a one-unit decline in TSPs; however, this convention may be flawed. It assumes that the entire increase in housing prices is due to the change in air quality as of 1980 and that individuals would expect the regulation-induced gains in air quality in nonattainment counties to disappear after 1980. A more realistic view is that nonattainment status changed individuals' expectations about the future path of TSPs concentrations in both sets of counties and that the change in housing prices reflects the expected stream of utility associated with this change in expected TSPs concentrations.

We make two alternative calculations that account for expectations and the long-lived nature of housing assets. First, we assume that individuals expected the relative gain in air quality in nonattainment counties to remain constant at 10 μg/m^3 forever. Under the homogeneous MWTP assumption, this implies that a *permanent* 1 μg/m^3 decline in TSPs concentrations increases housing prices by roughly 0.28 percent or $243 (2001 dollars). Second, by adding an assumption about the discount rate, we can also calculate the value of a 1 μg/m^3 decline in TSPs that lasts only a single year. For example, with a 5 percent discount rate, our results imply that a one-unit decline in TSPs that lasts one year is worth approximately $12. Of course, the valuation calculations for a one-unit reduction in TSPs will vary depending on the discount rate and individuals' expectations on the future path of TSPs.

This study has focused on the effect of TSPs on land values, but according to the canonical Roback (1982) model, the full implicit price of an amenity is the sum of the land price differential plus the negative

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[42] The summary estimate of 0.28 percent is equal to the weighted average of the estimates from panel A of table 5, where the weights are the inverse of the standard errors.

of the wage differential. To address this, we estimated instrumental variables specifications in which the 1970–80 change in log income is the dependent variable. The coefficient estimates for the mean change in TSPs are a precisely estimated zero across all the specifications. Consequently, the above welfare calculations are unchanged by accounting for changes in income.

More broadly, we think that this study has a number of implications for future research. For starters, the results suggest that markets can be used to determine the value of environmental amenities and perhaps other nonmarket goods. Further, the paper demonstrates that quasi-experimental approaches can be effective in estimating parameters derived from economic models (e.g., MWTP). The paper's results also indicate that omitted variables bias is a first-order issue when attempting to estimate structural demand parameters in the absence of a credible instrument. An important avenue for future research is to cross-validate these findings in other settings.

Finally, the ultimate promise of hedonic theory is that it provides a framework to obtain MWTP functions. These functions are of tremendous practical import because they can be used to (1) calculate the welfare effects of nonmarginal changes in goods for which explicit markets are missing and (2) forecast the consequences of alternative policies. Future research should integrate the credible estimation of the hedonic price schedule with strategies to recover MWTP functions.

IX. Conclusion

This study has exploited the air pollution reductions induced by the Clean Air Act Amendments to provide new evidence on the capitalization of air quality into housing values. The evidence suggests that TSPs nonattainment status is causally related to both declines in air pollution and increases in housing prices during the 1970s. Using the county-level regulations as an instrument, we estimate that a 1 $\mu g/m^3$ reduction in TSPs results in a 0.2–0.4 percent increase in mean housing values, which is a −0.20 to −0.35 elasticity. These estimates of the average marginal willingness to pay for clean air are robust to quasi-experimental regression discontinuity and matching specification checks. Further, they are far less sensitive to model specification than cross-sectional and fixed-effects estimates, which occasionally have the "perverse" sign. The estimation of a random coefficients model provides modest evidence that the marginal benefit of reductions in TSPs is lower in communities with relatively high pollution levels, which is consistent with preference-based sorting.

Welfare calculations suggest that the mid-1970s TSPs nonattainment designation provided a $45 billion aggregate gain for homeowners in

nonattainment counties. This gain is large, but the net effect on welfare is unknown since reliable estimates of the social costs of these regulations are not available. Regardless of whether the TSPs nonattainment designations pass or fail a cost-benefit test, this paper's findings suggest that individuals place a higher value on clean air than has previously been recognized.

Data Appendix

Determining Attainment/Nonattainment Status at the County Level

The ability to accurately determine the EPA's assignment of counties to attainment/nonattainment status for TSPs is crucial for implementing this paper's quasi experiment. In the 1972–77 period, the EPA did not publicly release the names of the counties that were designated nonattainment. To learn the identity of these counties, we contacted the EPA but were informed that records from that period "no longer exist." However, the readings from the air pollution monitoring system were used by the EPA and the states to determine which counties were in violation of the federal air quality standards. Consequently, for the years 1972–77, we use our pollution data to replicate the EPA's selection rule. Counties with monitor readings exceeding the National Ambient Air Quality Standards for TSPs were assigned nonattainment status; all other counties were designated attainment.

Beginning in 1978, the *Code of Federal Regulations* (CFR) (title 40, pt. 80) published annually the identity of all nonattainment counties. We collected these annual county-level designations for each of the 3,063 U.S. counties. There is a close correspondence between our "constructed" measure of 1978 TSPs nonattainment status with the actual designations. This suggests that our constructed nonattainment designations are likely to be a good approximation to the counties that the EPA treated as nonattainment in the earlier part of the 1970s.

The Siting of TSPs Monitors and the "Reliability" of the TSPs Pollution Data

Central to the credibility of the analysis is that the pollution concentration readings used accurately reflect the "true" air quality faced by individuals. Since readings from the TSPs monitors are used to determine nonattainment status, it is possible that states or counties strategically placed the monitors to fabricate the appearance of low (or improving) pollution concentrations. To explore the likelihood of this, we examined the CFR and found that the amendments contain very precise criteria that govern the siting of a monitor.[43] In particular, the

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[43] The substance of this discussion results from the 1995 CFR, title 40, pt. 58, and a conversation with Manny Aquilania and Bob Palorino of the EPA's District 9 Regional Office. Using a recent CFR is not a problem, because the hierarchical control over monitor placement specified in the 1995 CFRs is consistent with previous monitor siting guidelines.

legislation forbids states from siting a monitor in a location that does not meet one of the scientific criteria outlined for monitors.[44]

Moreover, the amendments provided the EPA with a number of enforcement tools to ensure that the states complied with the criteria for siting a monitor. First, the part of the CFR that lists the criteria for monitor placements is incorporated into the state implementation plans. Since the plans are both federal and state law, the EPA can sue states for violating federal law. Second, the usual process for siting is that the states propose a monitor network, and the EPA's district office either approves it or suggests alterations. The federal EPA can also review and reject the siting program, resulting in two layers of oversight. Third, the district offices often require photographs of sites to verify a monitor's placement. Fourth, it is illegal to move many of the monitors. The monitors that can be moved can be relocated only to better meet the scientific criteria outlined in the CFR. Finally, the district offices are cognizant of which states do not put resources into their siting programs. One district officer said that in these situations they are willing to "play dictator."[45]

Variables from the 1972 and 1983 County and City Data Books

Below are listed the variables taken from the 1972 and 1983 County and City Data Books and used in the housing value regressions. Most of the information comes from the 1970 and 1980 Censuses of Population and Housing. The crime data come from the U.S. Federal Bureau of Investigation; the medical data come from the American Hospital Association and the American Medical Association; and the spending and tax variables come from the Census of Governments. See "Source Notes and Explanations" in the CCDB for more detailed explanations of the variables and their sources. We start with the variables used in the 1980 analysis from the 1983 CCDB.

Outcome variable:

Log median value of owner-occupied housing units in 1980 (deflated to 1982–84 dollars by the total shelter component of the consumer price index).

Economic conditions variables:

Per capita money income in 1979
Civilian labor force (aged 16 or older) unemployment rate
% of employment in manufacturing in 1980

Demographic and socioeconomic variables:

[44] These criteria require that the monitors be placed so that they determine the highest concentration expected in the area, the representative concentrations in areas of high population density, the impact on ambient pollution levels of significant fixed and mobile categories, and the general background concentration level due to geographic factors. Moreover, the CFR specifically requires that the monitors be a minimum distance from stationary sources of pollution. Using the Landview CD-ROM to examine maps of counties giving the location of pollution monitors, the location of stationary pollution sources, and the location and demographics of the population confirmed this.

[45] The county-level measures of mean TSPs pollution levels used in the analysis are based on averaging the annual geometric mean reading of every monitor in the county over four years. Consequently, any idiosyncratic shocks to pollution levels in a county in a short time span will not pose any problems.

Population per square mile in 1980
% of population white in 1980
% of population female in 1980
% of population aged 65 and over in 1980
% of population over 25 with at least a high school diploma in 1980
% of population over 25 with at least a college degree in 1980
% of population in urban area
% of families below the poverty level in 1979

Housing variables:

% of year-round housing built in last 10 years
% of year-round housing built 10–20 years ago
% of year-round housing built before 1939
% of occupied housing units lacking complete plumbing in 1980
% of housing units vacant in 1980
% of housing units owner-occupied in 1980

Neighborhood variables:

Crime rate per 100,000 in 1981
All serious crimes known to police per 100,000 in 1981
Property crimes per 100,000 in 1981
Physicians per 100,000 in 1980
Hospital beds per 100,000 in 1980

Spending and tax variables:

Per capita government revenue in 1977
Per capita total taxes in 1977
Per capita property taxes in 1977
Per capita general expenditures in 1977
% of spending on education in 1977
% of spending for police protection in 1977
% of spending on public welfare in 1977
% of spending on health in 1977
% of spending on highways in 1977

For 1970 the following variables were unavailable:

% of year-round housing built in last 10 years
% of year-round housing built 10–20 years ago
% of year-round housing built before 1939
Crime rate per 100,000
All serious crimes known to police per 100,000
Property crimes per 100,000
Physicians per 100,000
Hospital beds per 100,000
Per capita total taxes
% of spending for police protection

For the 1980 − 1970 first-differences and instrumental variables regressions, "first differences" in all the variables that are in both the 1972 and 1983 CCDBs are included as control variables.

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