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DOES AIR QUALITY MATTER? EVIDENCE FROM THE HOUSING MARKET

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ABSTRACT

This study exploits the quasi-random assignment of air pollution changes across counties induced by federally mandated air pollution regulations to identify the impact of particulate matter on property values. Two striking empirical regularities emerge from the analysis. First, particulate matter declined substantially more in regulated than in unregulated counties during the 1970s and 1980s. At the same time, housing prices rose more in regulated counties. The evidence suggests that this approach identifies two causal effects: 1) the impact of regulation on air quality improvements, and 2) the impact of regulation on economic gains for home-owners. In addition, the results highlight the importance of choosing regulatory instruments that are orthogonal to unobserved housing price shocks that vary by county over long time horizons.

It appears that using regulation-induced changes in particulate matter leads to more reliable estimates of the capitalization of air quality into property values. Whereas the conventional cross-sectional and "fixed effects" estimates are unstable and indeterminate across specifications, the instrumental variables estimates are much larger, insensitive to specification of the model, and appear to purge the biases in the conventional estimates. The estimates imply that a one-unit reduction in suspended particulates results in a 0.7-1.5 percent increase in home values. In addition, it appears that air pollution regulations resulted in real economic benefits to home-owners in regulated counties.

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Introduction

Efforts to regulate air pollution in the United Sates have been among the most controversial interventions mandated by the federal government. Due to the lack of convincing empirical evidence on the costs and benefits of environmental regulation, contentious debates about the value of environmental policy are often based on conjecture and hyperbole. Consequently, credibly measuring the economic value of clean air to individuals is a topic of considerable interest to both economists and policy makers.

The traditional approach to estimating the economic benefits of air quality is to use the housing market to infer the implicit price function for this non-market amenity. Using simple regression analysis, researchers have estimated the relationship between location-specific property values and air pollution, as measured by suspended particulate matter, adjusted for differences across locations in observable characteristics. After almost thirty years of research, the cross-sectional relationship between particulate matter and housing prices appears relatively weak. In particular, Smith and Huang's (1995) meta-analysis of estimates from thirty-seven cross-sectional studies suggests that a one unit (in mg/m^3) decrease in total suspended particulates (TSPs) results in a 0.05-0.10 percent increase in property values.

However, estimating whether housing prices rise with air quality may be a much more difficult problem than previously recognized in the literature. In particular, the "true" relationship may be obscured in cross-sectional analyses by unobserved determinants of housing prices that covary with air pollution. Consequently, it appears that credible estimation of the hedonic housing price-air quality locus requires the random assignment of air quality across localities. Not surprisingly, exogenous differences in air quality are extremely difficult to isolate. The goal of this paper is to try to identify the "causal" relationship between particulate matter and property values and answer the question: Do housing prices rise with air quality?

This study exploits the quasi-random assignment of air pollution changes across counties induced by federally mandated environmental regulation to identify the effects of particulate matter on housing values. In the 1970s, the federal government made a revolutionary foray into the regulation of air pollution with the passage of the 1970 and 1977 Clean Air Act Amendments (CAAAs). The centerpiece of the CAAAs was the EPA's assignment of each county to either non-attainment or attainment status based on whether the federally-determined maximum allowable pollution concentration was or was not exceeded, respectively. Polluters in non-attainment counties faced greater federal regulatory oversight than those in attainment counties. An advantage of this quasi-experimental design is that it potentially controls for confounding determinants of property values such as permanent differences in county characteristics and economic conditions and local housing demand and supply shocks. In addition, the discreteness of the regulations reinforces the credibility of this approach.

To implement our approach, we bring together a variety of rarely-used and comprehensive microdata covering the 1970-1990 period. We compiled data on the attainment/non-attainment status of each county for every year from the *Code of Federal Regulations*. Although this information is central to federal environmental law, surprisingly, this is the first time that either a researcher or the EPA has collected this data. The county air pollution data comes from the Air Quality Subsystem Database and was obtained by filing a Freedom of Information Act request. Finally, the data on property values and county characteristics comes from the 1972, 1983, and 1994 County and City Data Books and the 1980 and 1990 Census five-percent PUMS files.

Two striking empirical regularities emerge from the analysis. First, much of the observed differential reductions in total suspended particulates across counties can be attributed to differences in regulatory pressure. Particulate matter declined 7-10 mg/m^3 and 4-5 mg/m^3 more in non-attainment than in attainment counties during the 1970s and 1980s, respectively. Second, housing prices rose more in non-attainment than in attainment counties during the same period (by about 5 percent and 4 percent in the 1970s and 1980s). The results highlight the importance of carefully choosing regulatory instruments which will be orthogonal to unobserved housing price shocks that may vary by county over long time horizons. In addition, the discontinuous nature of the pollution regulations supports the interpretation that the estimated effects of regulation on changes in air pollution and housing price are causal.

It appears that using <u>regulation-induced changes in TSPs</u> to isolate exogenous shifts in the supply of air quality leads to more reliable estimates of the capitalization of air quality into property values. The conventional cross-sectional and "fixed effects" estimates are very sensitive to specification and provide almost no evidence of a strong, systematic relationship between pollution and housing prices. In addition, due to confounding economic shocks, the fixed-effects analysis for the 1980s results in the "perverse" finding that declines in pollution lead to lower property values. The instrumental variables estimates, on the other hand, are about ten times larger than the largest "conventional" estimates and the estimates predominantly found in the literature. Just as importantly, they are relatively stable across specifications and appear to purge biases induced by differential non-stationarities in prices. The price gradients also vary little by region and demographic group. The evidence suggests that a one-unit reduction in average TSPs results in about a 0.7-1.5 percent increase in home values. It appears that environmental regulations reduced air pollution and that these reductions were capitalized into property values. In addition, air pollution regulations resulted in substantial monetary gains for home-owners in regulated counties.

Hedonic Theory and Previous Findings on Air Quality Capitalization

How to infer the economic value of environmental amenities such as air quality is not immediately obvious since there does not exist an explicit market for good air. Using the framework of hedonic price theory (e.g., Rosen 1974), the traditional approach to this problem has been to use the housing market to infer the implicit prices of this non-market good.[1] Under standard assumptions of perfect competition, information and mobility and the maximization of well-behaved preferences, hedonic theory unambiguously predicts that the implicit price function relating housing prices to the level of the non-market amenity (air quality) will be positively sloped, all else equal.[2] Locations with high levels of pollution must have a lower associated housing price in order to attract individuals. The hedonic price function represents the equilibrium relationship between property values and air quality. The gradient of the implicit price function gives the equilibrium differential that allocates individuals across locations and compensates individuals who face higher pollution levels.

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[1] See Chapter 11 of Freeman (1993) for a comprehensive review of property value models for measuring the value of environmental amenities. Other methods that have been used to measure the value of non-market amenities include contingent valuation, conjoint analysis, and discrete choice models. See Smith (1996) for a review and comparison of these methods.

[2] This also presumes that pollution levels are not systematically related to the productivity of the property as an input in a firm production process. Although this is a nontrivial assumption for agricultural and commercial land, it seems a foregone conclusion for the properties containing private homes examined in this study.

Consequently, hedonic theory predicts that, holding all other determinants of property values constant, housing prices will rise as pollution levels fall. The main goal of this study is to empirically assess this theoretical prediction and answer the question: Do housing prices rise with air quality? In addition, if air quality does get capitalized into property values, then the housing market can potentially be used to measure the economic benefits of pollution regulations that clean the air.

Based on the theory of compensating differentials, researchers have used simple regression analysis to estimate the relationship between location-specific property values and air pollution, as measured by suspended particulate matter, adjusted for differences across locations in observable characteristics. The particulate matter regression coefficient from the cross-sectional housing price equation is then interpreted as the implicit price schedule for air quality. After almost thirty years of research, the cross-sectional relationship between particulate matter and housing prices appears relatively weak. In particular, Smith and Huang's (1995) meta-analysis of 86 estimates from thirty-seven cross-sectional studies suggests that a one unit (in mg/m^3) decrease in total suspended particulates (TSPs) results in a 0.05-0.10 percent increase in property values. When interpreted as taste parameters, the estimates from the surveyed studies imply a median marginal willingness-to-pay (WTP) of $22.40 (in $1982-84) for a one-unit reduction in TSPs and an interquartile range of $0-98.52.[3] Using the median MWTP, if particulate levels decline from Pittsburgh's 1970 average of 133 mg/m^3 to Santa Barbara's 1970 average of 65 mg/m^3, then property values would increase by only $1,500.

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[3] The surveyed studies were conducted from 1967-1988, and most used data from the 1960s and 1970s. For example, the original Ridker and Henning (1967) study and Harrison and Rubinfeld (1978) examined 1960 and 1970 census tract data for the St. Louis and Boston metropolitan areas, respectively. Palmquist (1984) used property sales data in seven SMSAs from the Federal Housing Administration. Blomquist, Berger, and Hoehn (1988) examined 253 counties using 1980 Census data. The recent study by Gyourko and Tracy (1991) examined 130 cities using the 1980 Census. The Ridker and Henning and Harrison and Rubinfeld studies are the most disaggregate since they examine census tract-level information. However, there is little useful additional information provided by examining census tracts since there are relatively few air pollution monitors in a given area. For example, Harrison and Rubinfeld's analysis of 506 census tracts used only 18 TSP monitor observations. As noted by Moulton (1986), treating these correlated observations as providing independent information can lead to misleading inferences. Consequently, we do not believe that our study suffers from aggregation biases by focusing on county-level information, especially since the pollution regulations are enforced at the county level. Examination of county maps (available from the authors) showing the location of pollution monitors and the demographics of the population at the census tract-level confirms this.

However, estimating whether air quality gets capitalized into property values may be a much more difficult problem than previously recognized in the literature. Hedonic theory predicts that housing prices rise with air quality, all else equal. Previous empirical estimates, on the other hand, may be relying on comparisons across locations in which not all else is held constant. For example, areas with high levels of TSPs tend to have better-educated populations and higher per-capita income and population densities. Since air pollution levels are not randomly assigned across locations, the "true" relationship may be obscured in cross-sectional analyses by unobserved determinants of housing prices that covary with air pollution, and the "conventional" estimates may be biased.[4]

It appears that credible estimation of the hedonic housing price-air quality locus requires the random assignment of air quality across localities. Not surprisingly, exogenous differences in air quality are extremely difficult to isolate. The goal of this paper is to attempt to identify the "causal" relationship between particulate matter and property values using the unique research design provided by federally mandated air pollution regulations. Specifically, we will use regulation-induced changes in pollution under the assumption that they are orthogonal to unmeasured determinants of housing price changes.

A final point should be noted before proceeding. The main focus of this study is credible estimation of the hedonic price function for air quality. Another identification issue that has arisen in the literature is using the estimated price function in a second-stage simultaneous equations demand and supply model to recover individual preferences and to make welfare calculations (see Freeman 1993:387-401 for a summary). A multitude of additional econometric identification problems arise in this case (Brown and Rosen 1982, Epple 1987, and Bartik 1987). We will revisit this issue below when we interpret the results.

Federal Air Quality Regulations and a New Research Design

[4] Analogous problems arise in the hedonic wage literature estimating compensating differentials for undesirable job characteristics such as risk of injury or death. In particular, the observed relationship between wages and many job amenities is weak and often has counterintuitive signs (Smith 1979). Brown (1980) suggests that this is due to unobserved permanent differences across individuals and therefore focuses on job "changers".

The ideal analysis of the relationship between property values and air quality would involve a controlled experiment in which the researcher randomly assigns air pollution across localities and then observes differences in housing prices. Since this ideal is not attainable, this study uses federally mandated environmental regulations imposed at the county-level to isolate changes in air quality that are orthogonal to unmeasured determinants of housing prices such as county characteristics and economic conditions and housing demand and supply shocks. We describe the legislative background on the 1970 and 1977 Clean Air Act Amendments, and how they provide a unique opportunity for credibly identifying the hedonic relationship between housing prices and clean air.

What Were the Statutory Requirements of the Amendments?

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. In the absence of federal legislation, few states found it in their interest to impose strict regulations on polluters within their jurisdictions. Disappointed with the persistently high levels of pollution in some areas of the country, Congress passed the 1970 Clean Air Act Amendment (CAAA). The centerpiece of this legislation was the establishment of federal air quality standards that applied at the county-level for carbon monoxide, ozone, sulfur dioxide, and total suspended particulates (TSPs).[5] The stated goal of the CAAA was to bring all counties into compliance with these standards by reducing local air pollution concentrations. Under the purview of the law, the EPA assigned each county to either non-attainment or attainment status based on whether the federal maximum allowable pollution concentrations were or were not exceeded, respectively.

This legislation directed the fifty states to develop and enforce pollution abatement programs that would bring each of their counties into compliance with the standards by 1975. A required component of these programs was that states were to submit State Implementation Plans (SIPs) which precisely detailed the pollution abatement activities that all sources of pollution were required to undertake in "dirty" non-

[5] For the period of this study, the clean air regulations targeted total suspended particulates (TSPs). In 1987 the EPA, concluding that smaller particles that the human body cannot easily filter were more of a health threat, switched the focus of regulation from all particulates to the smaller PM_{10}s; which have an aerodynamic diameter equal to or less than 10 micro meters (μm). In 1997, the focus was changed to particles less than 2.5 μm, or $PM_{2.5}$s.

attainment counties. In such counties, new and modified sources were required to obtain pollution permits and existing major stationary sources were given emissions limits.

By 1975 many areas of the country continued to have pollution levels above the federal standards. In response, Congress enacted the 1977 CAAA which reinforced the system of differential regulation based on county pollution levels. The new legislation required states to publish annually the names of all counties that were not in compliance with the standards. Additionally, states were obligated to draft stricter implementation plans.

In non-attainment counties, new investment by polluters had to be accompanied by the adoption of state-of-the-art pollution abatement equipment under the revised Amendments.[6] Moreover, the increase in emissions from new investment had to be "offset" by a reduction in emissions from another source.[7] In "clean" attainment counties, the regulations were less stringent. Large entrants were allowed to adopt less expensive (and less effective) abatement equipment and "offsets" for emissions from new investment were not required. Smaller entrants and existing plants were essentially unregulated. To ensure that polluters in both non-attainment and attainment counties adhered to these rules, states initiated inspection programs and fined non-compliers.

Were the Regulations Enforced Throughout the U.S.?

In order to avoid differential regulation across states, the CAAAs gave the EPA a number of enforcement tools. Specifically, the legislation required the EPA to review and approve all SIPs to ensure that they were consistent with reducing air pollution concentrations below the federal standards. If a state failed to make requested changes to its plan, the EPA could impose construction bans, enact its own implementation plan for that state, and/or withhold some federal monies. In late 1979, there were construction bans in at least parts of every state, except Wyoming, reflecting the EPA's dissatisfaction with

[6] The EPA set the required abatement technologies at the industry level and states were to prescribe their usage as they established and administered their implementation plans. The technologies for new sources were both more expensive and more effective at abating pollution than the equipment mandated for existing sources.

[7] Offsets could be generated by tighter controls on existing operations at the same site, an enforceable agreement with a different facility to reduce emissions, or the purchase of another facility and reduction of emissions there through the installation of control equipment or by shutting it down (Vesilind, Peirce, and Weiner 1988). In practice, the vast majority of offsets were handled internally (from different facilities within a company). There are some examples, however, of external offsets-- across companies or involving governments. These external exchanges have usually involved in-kind considerations, but in rare cases involved cash (Lave and Omenn 1981).

the states' efforts (Lave and Omenn 1981). To prevent the imposition of stricter penalties, all states eventually submitted acceptable plans.

A significant feature of the 1977 CAAA was that it made the SIPs both state and federal law. This feature enabled the EPA to use the judicial system to pursue its own interpretation of the SIPs. This new statutory power was a practical enforcement tool because the states and the EPA often disputed the interpretation of the implementation plans.[8] The 1977 legislation also required the EPA to initiate administrative orders, monetary penalties, civil actions, or criminal actions against individual sources not in compliance with its state's implementation plan by 1979.

A number of studies have documented the effectiveness of these regulatory actions (Russell 1990, Deily and Gray 1991, Nadeau 1997). Although the enforcement of the CAAAs was not flawless (Crandall 1983), polluters in counties exceeding the standards were subject to considerably greater oversight than similar sources in counties with concentrations below the standards. In addition, Greenstone (1998) provides evidence that the regulations substantially reduced employment, investment, and the total value of shipments of manufacturers located in non-attainment counties.

A New Research Design

Given the above discussion, the legislation, and the tools available for its enforcement, suggest a new research design to infer the effects of particulates pollution on housing values. In particular, federal environmental regulations may provide a valid and powerful "first-stage" for predicting differential changes in air pollution across counties. The conceptual idea underlying this study is to use the attainment/non-attainment status of counties as "instruments" for county-level changes in air quality over the 1970s and 1980s. We use this quasi-experiment to relate regulation-induced changes in particulate matter to coinciding changes in housing prices.

It is our belief that this research design may provide a more credible basis for evaluating the relationship between TSPs and housing prices than previous cross-sectional analyses. First, the regulations are federally mandated, and, therefore, are less likely to be related to differences in tastes, characteristics,

[8] In *Chevron, U.S.A., Inc. v. NRDC*, 467 U.S. 837 (1984), the Supreme Court affirmed the EPA's right to enforce its own interpretation of the SIPs.

or underlying economic conditions across counties. Similarly, the federal imposition of the law is presumably orthogonal to the local political process determining the supply of local non-market amenities. In addition, we will show that the regulations appear to purge differential local demand and supply shocks in the housing market during the period examined which seem to contaminate inferences based on "fixed-effects" analyses.[9]

Finally, since the regulation instrument is discrete, this study has the feature of a quasi-experimental discontinuity design (e.g., Angrist and Lavy 1997). For example, we can compare changes in housing prices and pollution for non-attainment counties just above the federal standard for TSP concentration to changes for attainment counties just below this discontinuity. These comparisons should control for other potential confounding factors which may be related to differences in pollution levels across counties. In particular, discrete differences between attainment and non-attainment counties in changes in pollution and property values near the discontinuity are probably fully attributable to the regulations.

Data and Overview of Changes in Air Pollution

We synthesized a never before used database to implement our evaluation strategy. In particular, we brought together a variety of comprehensive and detailed data on county-level air pollution, the federally imposed environmental regulations, county characteristics, and property values for the 1970-1990 period. Here, we describe the data used in this study and present an overview of the changes in particulate emissions that occurred during the period. More details are provided in the Data Appendix.

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[9] Scientific evidence provides additional support for the credibility of regulation instruments that depend on pollution levels. In particular, Cleveland, et. al. (1976) and Cleveland and Graedel (1979) find evidence that while county-level air pollution depends on a number of factors, local economic activity may be a relatively small one in many counties. For instance, the research found that wind patterns often transport air pollution hundreds of miles and that the ozone concentration of air entering the New York region in the 1970s often exceeded the federal standards. Additionally, a region's topographical features can substantially affect pollution concentrations. Counties located in valleys (e.g., Los Angeles, Phoenix, Denver, and the Utah Valley) are prone to weather inversions, which lead to prolonged periods of high TSP concentrations. Consequently, there are several factors which contributed to county pollution levels and the determination of a county's regulatory status that are unrelated to local economic activity.

The TSPs Pollution Data and National Trends

Particulate matter is predominantly released into the air as a by-product of a number of industrial processes, driving on paved and unpaved roads, and ground breaking associated with construction and agricultural activities (see Appendix Table 1). Industry reduces its emissions via "bag" filters and "wet" scrubbers, while the other sources are often addressed with increased irrigation. Of the pollutants regulated under the CAAAs, it is thought that TSPs are the most detrimental to human health. In particular, recent empirical research has linked particulate pollution to increased rates of morbidity and mortality (e.g., Dockery, et. al., 1993 and Ransom and Pope 1995). A priori, it is reasonable to presume that of the regulated pollutants, TSPs are the most likely to be capitalized into housing values since they are the most visible and have the most pernicious health effects.[10]

We compiled a county-level database containing TSPs concentrations for the years 1971-1990. The underlying data was obtained by filing a Freedom of Information Act request with the EPA that yielded the *Quick Look Report* data file, which comes from the EPA's *Air Quality Subsystem* (AQS) database. It provides the location of each state and national TSPs pollution monitor as well as annual statistics on the number of recorded observations, the number exceeding the federal standards, the annual geometric mean reading, and the 70th, 90th, 95th, and 99th percentile readings.

Since the attainment/non-attainment designations were made at the county level, we group the monitor data at the same level of aggregation. In the subsequent analysis, we use the county-level mean concentration of TSPs, which is the weighted average of the monitor-specific geometric means in a county with weights equal to the number of monitor observations. Since regulations are often intended to reduce extreme values, we also examine an extreme value measure of particulates pollution, which is the maximum of the 99th percentile readings from all of the monitors within a county. The Data Appendix summarizes the reliability of the TSPs pollution data and contains a detailed description of the criteria for the siting of TSPs monitors within a county.

[10] As we discuss later, our analysis generally supports this presumption. One potential exception is the modest evidence that changes in concentrations of ozone were capitalized into housing prices in the 1980s.

Figure 1 presents national trends in average total particulates pollution across counties for the years 1971-90. From the figure, it is clear that air quality improved dramatically in the 1970s and 1980s with particulate emissions falling from an average of above 75 mg/m^3 to less than 50 mg/m^3. However, there is also strong evidence that TSP emissions may be cyclically sensitive. While over half of the overall decline in TSPs occurred during the 1970s, it appears that the rest of the improvements occurred during the 1981-82 recession. It seems plausible that as heavily polluting manufacturing plants shut down in the Rust Belt during the 1981-82 recession, air quality in these areas improved substantially (Kahn 1997). Consequently, as well as being a driving force in the determination of property values, local economic shocks may also underlie changes in particulate emissions.

Non-attainment Status and its Effect on Air Quality

As mentioned above, the CAAAs directed the EPA to create federal pollution standards, also known as the National Ambient Air Quality Standards (NAAQS), to be applied at the county-level for all of the regulated pollutants. In the case of TSPs (see Appendix Table 1), if a county's emissions exceeded an annual geometric mean of 75 mg/m^3 or had a twenty-four hour concentration of greater than 260 mg/m^3 more than once in a given year, the county would be designated non-attainment for the emissions of TSPs in the following year. If a county's emissions were below these ceilings, then it would be classified as an attainment county for TSPs. The Data Appendix details how the *Code of Federal Regulations* and the TSPs pollution data were used to determine attainment/non-attainment status annually from 1971-1990 for each of the 3,063 U.S. counties.

Figures 2A and 2B provide a graphical overview of which counties were regulated during the 1970s and 1980s, respectively. In each of the figures, a county's shading indicates the number of times it was designated non-attainment in the years 1971-1978 (1981-1988); light gray for 0 times, charcoal gray for 1-4 times, black for 5-8 times, and white for the counties without any TSP pollution monitors (consequently, these counties are excluded from the subsequent analysis). Figure 2A reveals that a substantial proportion of the country was non-attainment status either 1-4 or 5-8 times in the 1970s. Interestingly, several other counties were regulated in addition to the "traditional" counties in the Rust Belt and in the South Coast Air Basin around Los Angeles. Importantly, the counties that were unregulated but contained pollution monitors (the gray areas) are in close proximity to the regulated counties. Consequently, the "control" counties are predominantly from the same parts of the country as the non-attainment counties.

Figure 2B shows that the country was much less regulated from 1981-88, reflecting the substantial improvements in air quality that had already occurred in the 1970s. It is more apparent that for the 1980s, the analysis based on the regulatory instruments will compare counties from different areas that may have experienced differential economic shocks during the period.

Table 1 presents summary information on the TSPs monitor data file by county attainment/non-attainment status for the aggregated years of this study. Although only about 1,000 to 1,400 counties were monitored during the 1970s and 1980s, these counties consistently accounted for about 80-85 percent of the U.S. population. Consequently, our study provides a fairly comprehensive analysis. Also, it appears that both average and extreme value TSP readings, while falling in both attainment and non-attainment counties, declined more in the highly regulated non-attainment counties. Interestingly, while the average and 90th percentile readings in non-attainment counties fell more during the 1970s, the 99th percentile readings fell substantially more during the 1980s, possibly reflecting the EPA's focus on extreme observations during that decade.[11]

Figure 3 presents more detailed annual information on changes in TSP pollution by regulatory status. It graphs average annual TSP emissions by attainment/non-attainment status in the same year. This is a fairly myopic presentation of the data since it is likely that there is a lag adjustment relationship between the pollution regulations and changes in pollution levels. For example, air pollution declines in high regulation counties will occur after firms install pollution abatement equipment or obtain offsets, which may take several years. Regardless, TSPs fell about 7 mg/m^3 more in non-attainment counties than in attainment counties during the 1970s, and there was no convergence in pollution levels during the 1980s. Interestingly, it seems that pollution levels fell more than 5 units more in non-attainment counties than in attainment counties during the 1981-82 recession, and that there may have been some differential

[11] Henderson (1996) found that during the 1980s ozone regulations were most effective at reducing extreme value readings. It should be noted that the composition of counties in the attainment and non-attainment groups in Table 1 is changing over time.

"reversion to the mean" in the post-82 period. However, it is worth noting that the composition of counties in the two categories is changing in this analysis.

To more carefully examine the lag relationship between the pollution regulations and TSP changes, Figure 4A graphs average TSP levels by each county's regulatory status in 1972 for the fixed set of counties with TSP monitor readings in every year. Several points are worth noting. First, even the unregulated counties in 1972 are relatively dirty, emitting almost 70 mg/m^3, on average. This is just five units below the federal standards for average annual emissions and implies that the unregulated counties may provide a reasonable comparison group for the regulated counties. Secondly, the TSP levels of non-attainment counties declined by about 10 units relative to attainment counties during the 1970s with most of the convergence occurring within three years of the initial regulations, by which time several of the counties had switched regulatory status.[12]

After this period, however, the pollution levels of the two sets of counties track each other remarkably well. In particular, the non-attainment and attainment counties responded almost identically to the 1981-82 recession and in the post-82 period. To the extent, that these post-regulation changes provide an accurate description of what may have occurred pre-1972, it appears that unregulated counties provide a good counterfactual for what may have occurred in the regulated counties in the absence of the regulations. The figure provides evidence that the regulations may be causally related to TSP changes and resulted in about a ten unit greater reduction in emissions in non-attainment than in attainment counties during the 1970s, on average.

Figure 4B attempts to further explore the impact of pollution regulations on TSP emissions by graphing average TSP levels from 1971-79 by whether or not the county was ever non-attainment in the 1974-75 period for the set of counties with monitor readings in both the beginning and end of the 1970s. By focusing on the mid-decade regulations, the graph allows for the examination of pre-regulation trends in TSP pollution by regulatory status. In addition, it provides a visual representation of the "first-stage"

[12] The calculations are based on the 201 attainment counties and the 111 non-attainment counties in 1972 with monitor readings in every year. By 1975, 40 of the 1972 attainment counties were non-attainment and only 53 of the 1972 non-attainment counties were still non-attainment. By 1990, 13 and 36 of the 1972 attainment and non-attainment counties, respectively, were non-attainment.

equation that will be used below for the 1970s. In the 1971-73 pre-regulation period, the pollution level changes in attainment and non-attainment counties were identical, with TSP levels declining 8 units in both sets of counties, on average.[13] In the post-regulation period, however, TSP levels declined another 10 mg/m^3 in the regulated counties, while they declined by only 4 units in unregulated counties.

We conclude that although regulatory status is determined by pollution levels in the previous year, it seems to be purged of potential biases induced by unobserved pollution shocks that differ by attainment status in the selection year and dynamic reversion to the mean in TSP levels. Regardless, potential biases in the first-stage induced by dynamic "selection biases" will be less of an issue since this study focuses on changes in air pollution and housing prices over 10-year horizons. Overall, it appears that TSP regulations caused a 6-10 mg/m^3 reduction in average particulate pollution during the 1970s. The above points will be revisited below.[14]

Housing and County Characteristics Data

The main data source on property values and county and housing characteristics analyzed in this study comes from the 1972, 1983, and 1994 County and City Data Books (CCDB). The CCDBs contain a wealth of information for every county in the U.S. The Data Appendix contains a detailed description of the CCDB data sets and the variables extracted and used. Since much of the information in the CCDBs comes from the 1970, 1980, and 1990 Censuses of Population and Housing, we view the data as being comprehensive and reliable. The outcome variable used in the analysis is the log-median value of owner occupied housing units which comes from the Censuses. The extracted variables used as controls in the below analyses include demographic and socioeconomic variables (population density, race, education, age, per-capita income, poverty rate, unemployment rate, fraction employed in different industries),

[13] The figure is based on the 642 and 347 counties that were never non-attainment or non-attainment at least once during 1974-75, respectively. The secular decline in TSPs during the 1971-73 pre-regulation period can be explained by two factors: 1) 96 and 92 of the 1974-75 attainment counties were actually non-attainment in 1972 and 1973, while 186 and 212 of the 1974-75 non-attainment counties were also non-attainment in 1972 and 1973; and 2) in Figures 3 and 4A, it appears that there was a secular decline in TSPs from 1972-74. The uniform decline in TSPs in the 1977-79 period can be explained by the fact that 106 and 165 of the 1974-75 attainment and non-attainment counties, respectively, were non-attainment in 1977.

[14] When using similar figures to examine the impact of regulations on changes in pollution during the 1980s, it is extremely difficult to visually distinguish differential reductions in TSPs attributable to regulatory status from different responses to the 1981-82 recession. This important point will be examined in more detail below.

neighborhood characteristics (crime rates and doctors and hospital beds per-capita), fiscal and tax variables (per-capita taxes, per-capita government revenue, per-capita expenditures, fraction of expenditures spent on education, welfare, health, police), and structural housing characteristics (e.g., year that the structure was built).

We also constructed and examined a data extract of the 1980 and 1990 Census 5-percent PUMS microdata. The Data Appendix describes how the extracts were created and matched to the regulation and pollution data and data from the CCDBs.[15] One advantage of the PUMS data is that it allows us to disaggregate the analysis and estimate different hedonic housing price gradients for different demographic groups. This is described in more detail below. In addition, the PUMS contains additional housing variables unavailable in the CCDBs (number of rooms, number of bedrooms, acreage, etc.). However, much of the CCDB data comes from the Censuses, and we find that the results based on the PUMS mirror the CCDB results. In the case of both extracts, it is noteworthy that we have collected the full range of control variables used in all of the previous empirical housing market studies combined. The Data Appendix contains the full list of control variables used.

Table 2 presents some means of county characteristics in 1970 and 1980 by whether or not the county was ever non-attainment or never non-attainment during the 1970s and 1980s. At the beginning of both decades, counties that were ever regulated during the decade had higher housing values, income per-capita, population densities, fraction well-educated, and crime rates than counties that were never regulated. In addition, regulated counties tend to have slightly higher unemployment rates and to be more non-white. Surprisingly, it appears that a smaller fraction of individuals are employed in manufacturing in regulated than in unregulated counties.

Table 2 confirms that the TSP regulations were not randomly assigned with respect to the level of county characteristics at the beginning of each decade. This is not too surprising given the many

[15] The most detailed level of geographic identification available in the 1980 and 1990 5% PUMS is the PUMA (a.k.a., "county group") that the individual lives in. A PUMA is an area containing at least 100,000 individuals. Consequently, we developed a scheme for matching PUMAs to the FIPS county identifiers contained in the pollution, regulation, and CCDB data sets. This scheme is described in the Data Appendix. The finest level of geographic information available in the two one-percent County Group public use samples of the 1970 Census are areas containing at least 250,000 individuals. Consequently, we did not use the 1970 Census microdata in the analyses, although the 1972 CCDB contains 1970 Census data aggregated to the county level.

determinants of pollution levels. However, below we find strong evidence that TSP regulation does appear to be orthogonal to changes in observed county characteristics that are related to housing price changes. This point is even more noteworthy since changes in the observables predict much of the variation in housing price changes.

In addition, Table 2 masks the considerable heterogeneity in shocks that was occurring to the relevant variables in several counties during the period, particularly in the Rust Belt. For example, in Allegheny County in Pennsylvania (the county containing Pittsburgh), average TSP levels declined dramatically (by 55 mg/m^3) during the 1980s. At the same time, both the fraction of employment in manufacturing and housing values fell substantially. Similar patterns emerge in areas such as Lake County, Indiana (containing Gary). Consequently, the relatively large economic shocks experienced by counties in the Rust Belt during the 1981-82 recession may have caused both pollution declines and housing price declines leading to a spurious positive relationship between pollution and property value changes.[16] The potential size and persistence of the shocks to housing prices caused by the 1981-82 recession implies that unbiased identification of the air quality capitalization rate will be particularly difficult for the 1980s.[17]

Econometric Specification and Identifying Assumptions

The previous discussion underscores the potential biases that may plague "conventional" estimates of the hedonic price function based on simple cross-sectional (between) and "fixed-effects" (within) comparisons. Here, we lay out the cross-sectional, fixed-effects, and instrumental variables econometric models that will be estimated below, and the identifying assumptions required in each model to consistently

[16] When examining annual changes in average TSP levels, it appears that all of the 55 mg/m^3 decline in TSPs in Allegheny County during the 1980s had occurred by the end of 1982. Similarly, all of the 21 mg/m^3 decline in Lake County during the 1980s had occurred by the end of 1982, with TSP levels actually rising at the end of the decade.

[17] Using impulse response analysis, Blanchard and Katz (1992) document that housing prices are particularly sensitive to local economic downturns. In response to a one-percent negative employment shock, median housing prices at the city-level appear to fall by over two-percent within five years, and do not rebound until 12 years after the initial shock. Even 8 years after the initial shock, housing prices are still 1.25 percent lower than they would have been in the absence of the shock. In addition, Blanchard and Katz conclude that housing price changes play the key role in overall changes in local price levels.

estimate the air quality capitalization rate. It is shown that the model which uses federal pollution regulations as instruments requires assumptions that have the most intuitive validity. To fix ideas, we assume for now that the effect of particulate pollution on housing prices is homogeneous across counties (time and individuals). We will relax this assumption below.

The cross-sectional model examined in this study, and used predominantly in the previous literature, can be considered using a simple two equation model of housing prices and pollution levels:

(1) $y_{c70} = X_{c70}'\beta + \theta T_{c70} + \varepsilon_{c70}$, $\qquad \varepsilon_{c70} = \alpha_c + u_{c70}$

(2) $T_{c70} = X_{c70}'\Pi + \eta_{c70}$, $\qquad \eta_{c70} = \lambda_c + v_{c70}$,

where y_{c70} is the natural log of average property values in county c in 1970, X_{c70} is a vector of observed county characteristics (defined in the Data Appendix), T_{c70} is the average particulate pollution reading across all monitors in the county, and ε_{c70} and η_{c70} are the unobserved components of housing prices and pollution levels, respectively.[18] The coefficient θ is the "true" effect of TSPs on property values. It is interpreted as the gradient of the hedonic price function and represents the expected decrease in housing prices that would occur if an additional unit of pollution was randomly assigned to a county.

For consistent identification of the air quality capitalization rate, the least squares estimator based on cross-sectional comparisons requires that $E[\varepsilon_{c70}\eta_{c70}]=0$. The potential biases that may arise in this estimator are made clearer by decomposing the two residual error terms into two components: permanent unobserved determinants of property values and TSP levels that vary by county (α_c and λ_c) and transitory shocks to the two processes (u_{c70} and v_{c70}). If there are fixed differences between counties in unmeasured characteristics that are related to both TSPs and housing prices or if there are differences in economic conditions that drive both processes, then the cross-sectional estimator will be biased. From the previous discussion it is unlikely that pollution levels are randomly assigned conditional on the observables. Table 2 suggests that the cross-sectional estimator will be biased towards finding a positive association between TSPs and property values.[19]

[18] In the analysis below, T_{c70} is the average of the TSP mean readings over all monitors in a county from 1971-72. T_{c80} and T_{c90} are based on the years 1977-79 and 1987-89, respectively. Averaging over more than one year reduces the impact of temporary perturbations on our measures of pollution.

[19] Actually, equations (1) and (2) require random assignment conditional on a linear combination of the observables, X. Allowing the observables to enter the housing price equation with a nonparametric form, $g(X_{c70})$,

With repeated observations on counties over time, a "fixed-effects" regression model can be used to control for the contribution of permanent unobserved components to biases in the conventional cross-sectional estimates. Using equations identical to (1) and (2) for 1980, one can "first-difference" the data to absorb the county fixed effects (α_c and λ_c), and estimate a regression relating changes in log-property values to changes in TSP levels across counties:

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

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

For identification, this "within" estimator requires that $E[(u_{c80} - u_{c70})(v_{c80} - v_{c70})]=0$. In other words, there are no shocks to pollution levels that coincide with changes in property values that cannot be explained by changes in the observables. However, we have already documented above that both TSPs and housing prices are very sensitive to economic shocks. For example, from 1980-82 both pollution levels and property values fell more in areas of the country that were greatly affected by the 1981-82 downturn (e.g., the Rust Belt). To the extent that differential local economic shocks cause differential reductions in both pollution and prices, the fixed-effects estimator will be positively biased. If economic shocks are a large component of variation in both pollution and price changes, then this bias can be quite large. In this study, it appears that estimates of the price gradient based on comparisons of changes across counties can actually accentuate the bias in the conventional cross-sectional estimator.[20]

Now consider the availability of an instrumental variable, Z_c, that causes changes in pollution levels but does not have a direct effect on housing price changes. The proposed instrument is mid-decade TSP pollution regulations as measured by the attainment/non-attainment status of a county. In this case, equation (4) becomes:

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is more robust under the assumption of random assignment conditional on X. Consequently, below we allow quadratics, cubics, and interactions of the X's to enter the housing price regression. In addition, since the treatment variable is observed at the county level, we can allow for state effects (γ_s) in the regressions to control for fixed unobserved differences across states. Allowing for a flexible functional form and making within-state comparisons is in the spirit of multivariate matching.

[20] It is worth noting that the below analysis controls for state-level differences in pollution and price changes by allowing for unrestricted state-time effects (γ_{st}). This is possible since both TSPs and the regulations are observed at the county level. See Blanchard and Katz (1992) for a summary of the importance of state-level economic shocks.

(5) $T_{c80} - T_{c70} = (X_{c80} - X_{c70})'\Pi_{TX} + Z_{c75}\Pi_{TZ} + (v_{c80} - v_{c70})°$, and

(6) $Z_{c75} = 1(T_{c75} > \bar{T}) = 1(v_{c75} > \bar{T} - X_{c75}'\Pi - \lambda_c)$,

where Z_{c75} is the regulatory status of county c in the middle of the decade (e.g., 1975), 1(•) is an indicator function equal to one if the enclosed statement is true, and $\bar{T}$ is the maximum allowable concentration of TSPs specified by the federal pollution regulations.[21] In principle, the attainment status instrumental variable is a discrete function of unobserved determinants of mid-decade pollution levels.

Two conditions <u>sufficient</u> for the IV estimator (θ_{IV}) to be consistent are that $\Pi_{TZ} \neq 0$ and that $E[v_{c75}(u_{c80} - u_{c70})] = 0$. Given the "face validity" of pollution regulations and the previous discussion, the first condition is trivial. The second condition requires that decade-long changes in housing prices that cannot be predicted by changes in the observed covariates are orthogonal to stochastic shocks to the level of TSPs pollution in 1975. More precisely, variation across counties in unpredictable shocks to housing prices from 1970 to 1980 is mean independent of variation across counties in 1970-80 changes in TSPs pollution <u>attributable to differential shocks</u> to TSP levels in 1975, the regulation selection year. If attainment status in 1975 is determined by permanent or predictable differences across counties in TSP levels in that year, then the IV estimator will be consistent by definition.

In fact, due to the regression discontinuity design implicit in the indicator function, 1(•), determining regulatory status, the above conditions are stronger than those necessary to consistently identify the air quality capitalization rate. Since the regulation selection rule is a discrete function, there exists a discontinuity in the relation between the instrument and pollution shocks in 1975. As long as the relationship between these shocks and the unpredictable changes in pollution and housing prices is not discrete at the same discontinuity point ($\bar{T}$) where the probability of regulation "jumps", the observed changes can be causally related to the regulations. Even if v_{c75} is correlated with both $(v_{c80} - v_{c70})$ and $(u_{c80} - u_{c70})$, it is unlikely that these relations would be as discrete as its relation to the probability of regulation through the transformation function 1(•). Comparing regulated and unregulated counties near the

[21] The notation used for the instrumental variable is for ease of exposition. According to the law, the regulatory status of counties in 1975 is determined by TSP levels in 1974. In addition, the instrument we use is an indicator equal to one if the county was non-attainment in either 1974 or 1975 (1984-85 for the 1980s).

discontinuity "works" as long as the independent effect of 1975 pollution shocks on 1970-80 changes in TSPs and housing prices is sufficiently "smooth" (e.g., Angrist and Lavy 1997).[22]

The results below suggest that there is no systematic relationship between v_{c75}, (v_{c80} - v_{c70}), and (u_{c80} - u_{c70}) to begin with. Consequently, we believe that our study identifies two causal effects: 1) the impact of TSP regulations on changes in TSP pollution, and 2) the impact of regulation on changes in housing prices. This is the optimal situation for instrumental variables estimation and allows us to infer both the air quality benefits and the economic benefits of federal pollution regulations from 1970-1990.

Before proceeding, it is useful to discuss the reasons why our study uses mid-decade attainment/non-attainment status to form our instrumental variables. Since there were substantial economic shocks during the examined period, careful choice of regulatory instruments which are unrelated to nonstationarities in housing prices and particulate pollution is imperative. This point is easiest to understand in the context of the 1980s. Since the 1981-82 regulatory status of counties was determined precisely when the recession occurred, one might presume, a priori, that these instruments will not be purged of differential shocks to TSPs and prices and will suffer from biases at least as large as those in the fixed effects estimates (see footnotes 16 and 17). Many counties that were regulated in 1981 due to high pollution levels may have experienced relatively large declines in both pollution and housing prices during the decade due to the downturn (e.g., Pittsburgh and Gary).

The 1979-80 regulations, on the other hand, were determined before the economic downturn ("pre-determined") but are unlikely to be strongly related to property value changes from 1980-90. Most significantly, since local housing markets are likely to be integrated over long time horizons, it is possible that any housing price gains attributable to 1979-80 regulations would dissipate by 1990 (Blanchard and Katz 1992). In addition, 1980-90 is a long window to require stationarity of pollution levels and prices with respect to TSPs levels in 1979.[23]

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[22] It is possible to think of scenarios in which a regression discontinuity design could accentuate biases if economic agents change their behavior due to knowledge of the discontinuity (e.g., "avoidance" behavior). In our study, it is doubtful that counties had fine enough control over pollution levels to engage in non-random behavior near the discontinuity (especially given the large sunk costs associated with pollution such as the substantial fixed investment required to reduce industrial emissions).

[23] It is noteworthy that our identification strategy for the effect of regulation on housing price changes is based on the idea that the supply-side of the housing market does not adjust instantaneously to air quality improvements,

The 1984-85 regulations were determined after the downturn ("post-determined"). They are also attractive since 1980 and 1990 housing prices form a "symmetric" window around these regulations giving some time for any non-neutral shocks associated with the middle of the decade to dissipate on either end of the decade. Finally, Figures 4A and B suggest that it takes about 2-3 years for pollution to respond fully to regulation. Using mid-decade regulations as instruments allows for enough time for pollution to respond and for these responses to be capitalized into 1990 property values. In addition, it is unlikely that the supply-side of local housing markets is elastic enough in the short-run for housing price gains to be arbitraged away by 1990.[24] The above discussion illustrates the potential complexity of an evaluation problem which involves two different dynamic, non-stationary processes; pollution and housing prices. These points will be raised again when we discuss the results.[25]

"Conventional" Estimates of the Implicit Price of Air Quality

Table 3 presents the "conventional" cross-sectional estimates of the relationship between property values and particulate pollution levels at the county-level for 1970, 1980, and 1990. These estimates provide a useful benchmark since they are based on regression specifications typically used in the previous literature and adjust for similar observable covariates. For each year, the first column gives the unadjusted correlation between TSPs and housing prices; the second column allows the observables to enter linearly; the third column includes unrestricted state effects; and the fourth column includes quadratic and cubic terms of the control variables and their interactions.

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and that housing stock is fixed in the short-run and adjusts in the long-run. The below results support this view of housing markets. These same points arise with respect to using 1971-72 regulations as instruments for the 1970-80 analyses. This issue will be revisited when we interpret the results.

[24] Interestingly, when 1970-80 changes in TSPs pollution and changes in housing prices are regressed on all of the 1971-1990 TSP regulations, only 1974-75 non-attainment status has significant effects on both pollution and housing price changes. Other than the 1974-75 regulations, only 1971-72 non-attainment status has a significant impact on the 1970-80 change in TSPs. The 1980-90 TSP regulations are statistically unrelated to changes in TSPs and housing prices in the 1970s.

[25] Similar issues of "feedback", mean reversion bias, dynamic selection bias, and bias attributable to non-stationarities also arise in the dynamic panel data models used in the training evaluation literature (e.g., Ashenfelter and Card 1985, Chamberlain 1993). The below analysis controls for unrestricted forms of nonstationarity or dynamic mean reversion at the state-level by allowing for state-time effects.

For 1970 the unadjusted relation between housing values and pollution is weak and has a counterintuitive sign. Adjusting for a linear combination of the other observables leads to the statistically significant result that a one-unit decline in pollution leads to a 0.07 percent increase in housing prices. This finding is in the middle of the range of estimates summarized in the Smith and Huang (1995) meta-analysis. It is also noteworthy since it pertains to the time period examined in the bulk of the previous studies and is based on a similar linear regression specification. To give an idea of its magnitude, the coefficient estimate implies that if Allegheny county reduced its 1970 average TSP levels by 50 percent (a 65 mg/m^3 reduction), housing prices would rise by less than 5 percent (about $2,000), all else equal. Consequently, we have replicated the result found previously that the cross-sectional relationship between property values and pollution levels is weak at best. In addition, this finding is based on data that is more comprehensive and detailed than the databases previously used.

The next two columns of the 1970 regression results show that one can reduce this correlation even more by controlling for differences across states in housing prices and allowing the covariates to enter with a flexible functional form. Notably, over 90 percent of the variation in housing prices across counties is explained by the covariates in these regressions. Overall, it appears that air quality is not capitalized into property values.

The results for 1980 and 1990 provide an even more dubious picture of the reliability of estimates based on cross-sectional comparisons. In both cases, allowing the covariates to enter linearly and ignoring state-level differences in housing prices leads to the "perverse" result that a one-unit reduction in average TSPs is associated with a 0.09-0.16 percent decrease in housing prices. This result is even more disturbing since the coefficients are precisely estimated and the regressions have an R-squared of about 0.85. In both cases, controlling for state effects and allowing for non-linearities in the control variable leads to results closer to those previously found. However, one is left with the distinct impression that the cross-sectional relation between pollution and property values is weak, unstable, and indeterminate. In fact, it appears that any result is achievable depending on the specification used.[26]

[26] It is worth noting that sulfur dioxide appears to be unrelated to housing prices in 1970 and 1980. Ozone, on the other hand, has a stronger relation to property values in 1990 than in 1980. However, controlling for these pollutants, while based on a much smaller set of counties, had little effect on the estimated TSP coefficients. Also,

One reason that the cross-sectional analyses may lead to counterintuitive or weak results is that it fails to control for unobserved permanent differences across counties. To address this issue, we estimated a simple "fixed effects" model relating changes in housing prices to changes in pollution levels over the two decades. For consistency, this approach requires that there were no differential unobserved shocks to housing prices in heavily polluted counties relative to less polluted counties over ten year horizons. This assumption seems particularly strong for the 1980s in which underlying economic shocks associated with the 1981-82 recession may have caused both air pollution and housing prices to decline more in counties with higher pollution levels and greater industrial concentration in heavily polluting manufacturing plants.

Regardless, Table 4 presents the fixed effects estimates of the effect of pollution on log-property values. For the 1970-80 period, the first row provides estimates that are in the middle of the range of estimates previously found and similar to the 1970 cross-sectional estimates. Since pollution levels are observed at the county level, we can also control for unrestricted state-specific trends in housing prices over the decade. The second row shows that when one controls for state-time effects in property values, the estimated air quality capitalization rate is reduced and cannot be statistically distinguished from a zero-capitalization interpretation.

The results for the 1980-90 period are more troublesome. They imply that a one-unit decrease in mean particulate pollution results in a 0.1 percent decrease in housing prices; a result that holds up even after including state-time indicators to absorb differential price shocks at the state-level. Although clearly counterintuitive, this finding was foreshadowed by the above discussion. Specifically, it appears that the counties during the 1980s that experienced large declines in pollution due to the 1981-82 economic downturn coincidentally experienced declines in property values. In this study, myopically using within county variation in pollution levels over time leads to results that may be more biased than using between variation in pollution across counties. Observed within-county changes in TSPs could be the result of the same driving forces underlying changes in property values. Similar to the cross-sectional results, the fixed

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in regressions that allowed for both mean and 99th percentile TSP to enter, the effect of mean TSP "dominated" the 99th-percentile TSP effect providing evidence of collinearity. Finally, allowing TSPs to enter nonlinearly using a cubic and spline functions had little effect on the results. It appears that above low levels of TSP emissions (e.g., the 1st quartile), the effect of TSP on property values is close to linear in 1970, 1980, and 1990.

effects results leave one with the impression that the effect of pollution on housing prices is unstable and indeterminate.[27]

Instrumental Variables Estimates of Air Quality Capitalization

The above results highlight the problems associated with the conventional estimates of air quality capitalization which use non-random sources of air pollution variation. Here we present the results of analyses that use the quasi-random assignment of air pollution changes across counties induced by federal environmental regulations to identify the housing price-air quality relationship.

Reduced-Form Relationship of Regulation, TSPs, and Housing Prices

The instrumental variables estimate of the effect of TSPs on property values is a simple function of two reduced-form relationships: the effect of TSPs regulation on changes in TSPs pollution and the effect of regulation on changes in log-housing prices:

(7) $T_{c80} - T_{c70} = (X_{c80} - X_{c70})'\Pi_{TX} + Z_{c75}\Pi_{TZ} + (v_{c80} - v_{c70})°$, and

(8) $y_{c80} - y_{c70} = (X_{c80} - X_{c70})'\Pi_{yX} + Z_{c75}\Pi_{yZ} + (u_{c80} - u_{c70})°$,

where $\theta_{IV} = \Pi_{yZ}/\Pi_{TZ}$. Here, we make the case that our estimates of both Π_{yZ} and Π_{TZ} are unbiased.

Table 5 presents the regression results from estimating equations (7) and (8), where Z_{c75} is an indicator equal to one if the county was non-attainment in either 1974 or 1975 (1984 or 1985 for the 1980s). The two sets of columns present the results for 1970-80 and 1980-90, separately. To address the issue of instrument selection brought up above, we also present the coefficient estimates from reduced-form equations which use 1971-72 (81-82) non-attainment status as the regulation variable of interest. The coefficient estimates are from regressions that include changes in the observed covariates as controls, while the regression diagnostics are from regressions that include unrestricted state-time effects. The estimated relation between regulation and changes in the maximum of the 99^{th}-percentile TSPs readings is also shown.

[27] Although based on only the small subset of counties with ozone monitors (less than 300), the 1980-90 fixed effects analysis provides modest evidence that ozone levels are negatively associated with property values. This is interesting since ozone became a major focus of federal pollution regulations and the national debate on pollution during the 1980s.

For 1970-1980, it appears that TSPs regulation led to air quality improvements. In particular, particulate matter declined 7 mg/m^3 more in counties that were non-attainment from 1974-75 relative to the attainment counties. TSPs fell 10 units more in counties that were regulated from 1971-72 relative to counties that were unregulated at the beginning of the decade. These results mirror the patterns depicted in Figures 4A and 4B. For 1980-1990, the estimated effects of regulation on mean TSPs reductions are not as large (about 4 mg/m^3), but are still statistically significant. Regulation also caused reductions in extreme value readings in both decades.

There are several reasons why we believe that these estimates represent the true impact of the regulations, especially for the 1970-80 period. First, Figures 4A and 4B suggest that attainment counties provide a reliable counterfactual for non-attainment counties. Second, the estimated effects are insensitive to the method used for controlling for the observed covariates. For example, when we experimented with propensity score methods to control for bias due to selection on the observables and also when we allowed the covariates to enter with a flexible form, the estimated effects remained unchanged.[28] In addition, controlling for state-time effects had no impact on the results (e.g., the effect of 1974-75 and 1984-85 non-attainment status on changes in mean TSP became –5.87 and –3.80, respectively). Consequently, it is unlikely that differential pollution shocks in the regulation selection year (e.g., 1974) by non-attainment status and subsequent mean reversion are a source of bias. Finally, the effect of the "predetermined" 1979-80 TSPs regulations on 1980-90 pollution changes (-4.93 with 0.83 standard error) is close to the results based on the 1984-85 and 1981-82 regulations.

Table 5 shows another striking empirical regularity. TSP non-attainment status in 1974-75 is associated with a 4.8 percent gain in home values in the 1970s, and 1984-85 regulations are associated with a 3.9 percent gain in the 1980s. Taken literally, this result suggests that federal pollution regulations resulted in substantial monetary benefits for home-owners in regulated counties. In addition, it provides the first estimates of the economic value of federal environment policy which are based on the housing market.

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[28] This is not surprising given that changes in TSPs pollution are relatively unpredictable (e.g., low R-squared in Table 5). However, it is also true that the estimated probability distributions of regulation conditional on the observables for the attainment and non-attainment counties exhibited substantial overlap and that the distributions of the control variables also overlapped substantially for the two sets of counties.

There is no systematic relationship between 1971-72 non-attainment status and 1970-80 changes in property values. This is not surprising given the potential adjustment of housing markets in the long-run and the above discussion on sensible selection of regulatory instruments. Notably, housing prices declined 4 percent more in counties that were non-attainment in 1981-82 than in attainment counties during the 1980s. This result was also suggested by the previous discussion on the importance of non-neutral housing price shocks during the 1980s. The 1981-82 regulations were determined during the recession. The regulated counties are those whose 1980-81 TSP levels were high and who subsequently experienced large declines in both pollution levels and property values due to the recession. Consequently, IV estimates for the 1980s based on 1981-82 non-attainment status are effectively making comparisons across counties which accentuate the positive bias exhibited in the fixed effects estimates. Although the 1984-85 TSP regulations appear to purge this bias, we view the 1970-80 IV estimates as being more reliable than the 1980-90 estimates due to the substantial non-stationarities in the 1980s.

The regression diagnostics in Table 5 also provide relevant information. Examining the F-statistics, it is clear that mid-decade non-attainment status explains a substantial fraction of the variation in changes in TSPs across counties in both decades, while the control variables and the state-time effects are only weakly associated with pollution changes.[29] It appears that the most important predictor of changes in pollution is environmental regulation. On the other hand, the observed covariates and the state-time effects explain a substantial portion of the variation in housing price changes, especially in the 1980s. In fact, about 90 percent of the overall variation in changes in property values can be explained by the observables. In addition, the observables predict more of the variation in pollution changes in the 1980s than in the 1970s, potentially due to the greater import of "observable" shocks in the 1980s.

Figures 5A, B, and C provide a more detailed view of the two reduced-form relationships of interest for the 1970s. Figure 5A presents the estimated probability of a county being non-attainment, adjusted for changes in observable characteristics, by the county's mean TSPs reading in 1974.[30] The

[29] Interestingly, the observables do a much better job of predicting pollution levels in 1970, 1980, and 1990 than 1970-80 and 1980-90 changes in pollution.
[30] Attainment status in 1978 is used since it is based on the published CFR as opposed to the actual monitor readings that determine 1975 attainment status. Although this should be accurate given the high serial correlation in non-attainment status, Figure 5A probably understates the "true jumps" in probability of regulation that occur at

counties are aggregated into groups by TSP levels (given on the x-axis), with about 10-15 percent of the counties in each group. At the mid-point of each pollution group, the graph plots the mean probability of regulation for the counties and its sampling error band. The dotted vertical line is at the federal threshold for mean TSPs regulation of 75 mg/m^3.

From the figure, there are two large discontinuities in the probability of regulation. The most obvious is at the regulation threshold for mean TSPs (pollution groups 66-75 and 75-109). At the threshold point, the probability of regulation increases almost 30 percent.[31] The adjusted probability of regulation increases another 15 percent in the 110-198 pollution group. Interestingly, there is another nonlinearity in the probability of regulation as one moves from the 51-58 pollution group to the 58-66 group (about 23 percent). This occurs because several counties in the 58-66 group are non-attainment for exceeding the maximum daily concentration threshold of 260 mg/m^3.[32] Consequently, there are two different "points" at which we can infer the causality of regulation's impact, since the probability of regulation has a discrete relation to the selection year mean TSPs at these points. In particular, if we observe large "jumps" in changes in TSPs and housing prices at the same discontinuity points, it is very unlikely that the jumps can be attributed to any factor other than the regulations themselves.

Figures 5B and 5C show the regression-adjusted 1970-80 change in mean TSPs and log-property values by mean TSPs in the regulation selection year using the same groups as in Figure 5A. Focusing on Figure 5B, it appears that there are discrete increases in pollution reductions at the two discontinuity points. Particulate matter declines are 3 mg/m^3 greater in the 58-66 group than in the 51-58 group, and almost 4 and 12.5 units greater in the 75-109 and 110-198 groups than in the 66-75 group, respectively. These sharp reductions cannot be explained by reversion to the mean in pollution. In fact, there appears to

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the discontinuity points. In addition, the figures based on the "unadjusted" probability of regulation and "unadjusted" changes in housing prices and pollution levels look almost identical to the "regression-adjusted" figures presented.

[31] The unadjusted probability of regulation at this point increases from 30 percent to 69 percent. The unadjusted probability of regulation in the 110-198 pollution group is 0.9. The main reason why the probability of regulation is non-zero in all of the groups is that some counties that do not exceed the annual mean concentration threshold are regulated for exceeding the maximum daily concentration threshold of 260 mg/m^3.

[32] At this point, the unadjusted probability of regulation increases from 11 percent to 36 percent. The unadjusted probability of a county having a maximum 99% TSPs reading exceeding 260 mg/m^3 increases from 4.5% in the 7-58 mg/m^3 group to 26.4% in the 58-75 mg/m^3 group. 62.3 percent of the counties with 1974 mean TSP exceeding 75 mg/m^3 had maximum 99% TSPs readings exceeding 260 mg/m^3.

be no systematic relationship between 1974 TSP levels and the 1970-80 change in TSPs except at the discontinuity points.

Figure 5C exhibits analogous patterns. Property values increased almost 3.5 percent more in the 58-66 group than in the 51-58 group during the 1970s. At the mean TSP regulation threshold, housing prices increased 4 and 5 percent more in the 75-109 and 110-198 groups than in the 66-75 group. These results are even more remarkable given that housing prices appear to decline more during the 1970s as 1974 TSP levels increase, with the only exceptions being at the discontinuity points.

In terms of equations (3), (5), and (6), it appears that there is no confounding correlation between v_{c74} and $(v_{c80} - v_{c70})$ and $(u_{c80} - u_{c70})$. If anything, v_{c74} and $(u_{c80} - u_{c70})$ are negatively correlated, which implies that the resulting IV estimates may be slightly biased toward finding no air quality capitalization. The occurrence of jumps in pollution declines and housing price increases at both discontinuity points represents a preponderance of striking evidence that two causal effects are being identified: the impact of regulation on air quality improvements, and the impact of regulation on gains to home-owners. In addition, the results look identical without regression adjustment, which implies that the mid-decade regulations are orthogonal to observable determinants of 1970-80 changes in pollution and prices.[33]

1970-1980 IV Results

Table 6 presents the various instrumental variables estimates of the air quality capitalization rate for the period 1970-1980. The first column of the table contains the Wald estimator, based on two bivariate regression models of pollution and housing price changes on 1974-75 non-attainment status. It is equivalent to $\theta_{Wald} = \Pi_{yZ}/\Pi_{TZ}$ from regressions with no covariates. The other columns present the two-

[33] To exploit the other regulation rule based on the maximum daily concentration threshold, we examined the relation between the maximum of 99% TSPs in 1974 with the probability of regulation and 1970-80 changes in pollution and housing prices. The findings were strikingly similar to the above results. In particular, the only noticeable discontinuity in the probability of regulation occurs when one moves from counties with 99% readings between 220-260 units to counties with 99% readings between 260-400 units (an unadjusted difference of about 25%). Except at this discontinuity, there appears to be no relation between 99% TSPs in 1974 and 1970-80 changes in mean TSPs and housing prices. At the discontinuity, mean TSPs fell 4.5 units more and housing prices rose 4.5% more in the 260-400 group than in the 220-260 group during the 1970s. Since mean TSPs and extreme value TSP readings are very highly correlated, it is not possible to separately identify the impact of the two different regulation selection rules on changes in pollution and property values.

stage least squares estimates of the effect of pollution controlling for changes in the obervables and additionally for state-time effects, respectively.

The first row of the table presents the estimates for the full sample of counties. The capitalization rate increases from 0.4 to 0.7 to a high of 1.5 percent as more variables are included in the analysis. In all three cases, the estimates are statistically significant and have the intuitive sign. Comparing the Wald estimator to the unadjusted correlations in Table 3, it is apparent that just using changes in pollution attributable to the regulations is enough to reverse the "perverse" results from the conventional comparisons. Non-attainment status defines treatment and control groups that purge biases in the other estimates and accentuate the capitalization of air quality into housing prices.

The next three rows of the table present the IV estimates for different subsets of counties based on the regression discontinuity categories in Figure 5A. When examining counties whose 1974 TSP levels were no less than 58 mg/m^3, the Wald estimator is within a sampling error of the other two estimates, suggesting a capitalization rate of between 0.53-0.89 percent. This is even more remarkable given that less than one percent of the variation in 1970-80 price changes is explained in the bivariate regression model, whereas over 90 percent is predicted in the model that allows for covariates and state-time effects. Apparently, 1974-75 non-attainment status is orthogonal to virtually all of the other determinants of changes in property values.

The next row "refines" the comparisons even more by focusing only on those counties just below and just above the mean TSPs regulation threshold (1974 mean TSPs between 58 and 109 units). Although the inferences are relatively imprecise given the reduced sample size, the Wald estimator is again within a sampling error of the other two estimates, and the capitalization rate varies between 0.7 and 1.2 percent. Combined with the evidence from Figures 5A, B, and C, a strong case can be made that 1974-75 regulations are randomly assigned with respect to both changes in pollution and changes in housing prices during the 1970s.

The final row uses the discontinuity at 58 mg/m^3 to refine the treatment and control counties (1974 TSPs between 42 and 75 units). Here, the sampling errors are quite large. However, the results are still informative. While the Wald estimator and the estimate that adjusts only for the observables are in the same range as the other estimates, the estimate that adjusts for state-time effects is substantially larger (capitalization rate of 3.3 percent). This finding was foreshadowed by the patterns exhibited in Figure 5C. In particular, the state-time effects are absorbing the negative "trend" in changes in housing prices that occurs when moving from 42 to 58 mg/m^3. This "detrending" accentuates the capitalization effect when moving from 58 units to 75 units. This also explains the size of the "full sample" estimate that controls for state-time effects. Since it is not clear whether "detrending" housing price changes results in more valid comparisons, we conclude from this table that a one-unit reduction in particulate matter results in about a 0.7-1.5 percent increase in housing values during the 1970s. This range is about ten times larger than the estimates summarized in Smith and Huang.[34]

1980-1990 IV Results

Table 7 presents the instrumental variables estimates of pollution's effect on housing prices for the 1980-1990 period. As mentioned above, due to the extremely large confounding shocks that occurred during the 1980s, the results for this decade may be less reliable than the results for the 1970s.[35] Regardless, the first row of the table presents the two-stage least squares estimates which use 1984-85 non-attainment status as an instrument. Similar to the results for the 1970s, a one-unit decline in mean TSPs results in a 0.6-1.0 percent increase in property values. The estimates are noticeably more imprecise than the estimates for the 1970s, due to the weaker first-stage relationship between regulations and pollution changes. Interestingly, the IV estimate which uses 1981-82 non-attainment status as the instrument and controls for state-time effects is 0.26 (0.16 standard error), while the IV estimate that uses 1979-80 regulation as the instrument is 0.02 (0.16 standard error). As expected, using regulations determined during the 1981-82 recession result in estimates that are even more "perverse" than the fixed-effects estimates. Using the "pre-determined" 1979-80 regulations mitigates some of the bias, but is less than

[34] There are other notable points for the 1970-80 analysis. First, the reduced-forms for the Wald estimator based on the restricted samples in rows 2, 3, and 4 of Table 6 are (coefficient (sampling error) for pollution changes and housing price changes, respectively): row 2, –8.23 (1.51) and 0.04 (0.016); row 3, -5.79 (1.65) and 0.04 (0.021); row 4, -3.14 (1.37) and 0.02 (0.016). In addition, the IV estimate that uses 1971-72 non-attainment status as an instrument and controls for state-time effects is –0.64 (0.12). The IV estimate that uses all of the 1971-78 non-attainment indicators as instruments and controls for state-time effects is -0.58 (0.10).

[35] For example, the Wald estimator which uses the 1984-85 regulations as the instrument results in an extremely imprecise non-negative coefficient estimate.

satisfactory due to the weak relationship between pre-1980s regulations and 1980-90 changes in property values.

In another attempt to purge the biases associated with non-neutral shocks to air quality and housing prices during the 1980s, we used 1984-85 attainment/non-attainment status for ozone emissions as an instrument for changes in TSP emissions. There are several reasons why ozone regulation may provide an attractive alternative set of instruments. First, when examining annual changes in ozone emissions, it is clear that ozone exhibits much less cyclical sensitivity over time and did not respond to the 1981-82 recession. Second, Greenstone (1998) documents that ozone regulations may have had "cross-effects" on outcomes in TSP emitting industries due to interrelations in the production processes. Finally, ozone regulations should be unrelated to the level of TSPs pollution in the regulation selection year, which is the source of bias in the IV estimates based on TSP regulations. Consequently, we hypothesized that ozone regulation may cause reductions in TSPs which are orthogonal to the reductions in TSPs that were caused by the 1981-82 recession.[36]

The second row in Table 7 presents the IV estimates which use 1984-85 ozone non-attainment status as the instrumental variable. Remarkably, the estimated capitalization rates are greater than the rates based on TSP regulation. Although the sampling errors are large due to the weaker relation between ozone regulation and TSP changes, the estimates imply a 1.4-1.5 percent capitalization rate.[37] Overall, identical to the results for the 1970s, we find that a one-unit decrease in TSPs resulted in a 0.6-1.5 percent increase in property values during the 1980s.

Heterogeneity in the Hedonic Housing Price Function

The previous analysis has presumed that TSPs pollution has a constant effect on housing prices for all counties, individuals, and time periods. Tables 6 and 7 provide some evidence that the estimated effects are stable across different sets of counties and in two different decades. However, it is useful to examine

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[36] The fact that ozone regulation was a major focus of the EPA during the 1980s reinforces the possibility that it may have a nontrivial first-stage relationship with reductions in TSPs.

[37] The reduced-forms for the "ozone" IV estimate which controls for the observable covariates (coefficient (sampling error) for pollution changes and housing price changes, respectively) are –3.30 (1.02) and 0.05 (0.012). The IV estimates that use 1979-80 and 1981-82 ozone regulations and control for state-time effects are both noisy and statistically indistinguishable from zero.

this assumption more closely. Regardless, it is interesting to know whether capitalization rates varied across different parts of the country or across different demographic and socioeconomic groups.

Table 8 disaggregates the instrumental variables estimates in Tables 6 and 7 by region. In the first set of rows, it is clear that excluding California from the analysis has little effect on the estimates. In fact, it appears that outside of California during the 1980s, the estimated capitalization rates are higher and also less sensitive to the inclusion of state-time effects as controls.

The next two sets of rows present the two-stage least squares estimates separately for the non-Rust Belt and Rust Belt states. It appears that capitalization rates may have been slightly lower in the Rust Belt states than in the rest of the country. Interestingly, while the impact of the regulations on pollution reduction was slightly greater in the Rust Belt during the 1970s (-7.3 units versus –5.9 units without state-time effects), the effect of regulation on housing price increases was lower in the Rust Belt (2.5 percent versus 4.7 percent). This provides weak evidence that the economic benefits of regulation were smaller in the Rust Belt states..[38] However, it should be noted that the sampling errors of the estimates preclude any definitive conclusions.

Table 9 presents fixed effects and instrumental variables estimates of capitalization rates by demographic and socioeconomic groups using the 1980 and 1990 5-percent Census PUMS microdata. The Data Appendix describes how we created the extract and matched it to the pollution and regulation data. In addition, it details the control variables used in the analysis. The estimates are based on separate regressions for each group which include state-time effects and are analogous to the results one would obtain from a "fully interacted" regression. The results presented are for the "100-percent matches" (described in the Appendix), but are insensitive to the matching algorithm used.

The first two rows of the table are based on the full sample and replicate the previous results for 1980-1990 based on the CCDB data. Again, the fixed effects estimates result in counterintuitive

[38] These results suggest that the IV estimates do not overstate the true capitalization rate. For example, it is unlikely that the regulations induced structural changes which improved economic conditions in local economies that were previously heavily concentrated in manufacturing (e.g., a "race to the top" in areas like Pittsburgh).

inferences.[39] The IV estimates, on the other hand, imply that a one-unit reduction in TSPs resulted in a 0.89 percent increase in mean log-property values and a 0.95 percent increase in median log-values.

The next set of rows present the housing price gradients by race, education group, and household income quartile (see the Appendix for details on the cells). First, note that the fixed-effects estimates always have the counterintuitive sign and vary little across the different groups. The IV estimates, on the other hand, almost always reverse the sign of the "conventional" estimates. Interestingly, it appears that the price gradients vary only slightly by race. In addition, there does not appear to be a systematic relationship between the gradients and household income quartile. However, examination of the education group gradients reveals that home-owners with less than a college degree, especially high school dropouts, had higher air quality capitalization rates than college-educated home-owners. In fact, for the well-educated, it appears that air quality improvements were not capitalized.[40]

One potential explanation for this is that air quality varies within a county and there is some segmentation in the housing market. If so, then it is possible that college-educated home-owners living in counties that were dirty, on average, and subsequently regulated were actually living in the parts of the county that had relatively good air and where there were less regulation-induced air quality improvements. For these individuals, the first-stage pollution change equation may be misleading.[41]

Tables 8 and 9 are suggestive at best. Given the apparent validity of the regulation instruments, it seems useful to examine whether the estimated effects vary along observable dimensions. However, disaggregating the analysis at this fine a level is asking a lot from the data, as can be seen by the large

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[39] One reason why these fixed-effects estimates may be even more positive than the estimates in Table 4 is that the analysis in Table 9 is focused on large counties (with 400,000 individuals, on average), which may have experienced relatively large economic shocks during the 1981-82 recession.

[40] When examining the reduced-form equations, it appears that regulation-induced air quality improvements were slightly greater for blacks than whites during the 1980s. There are no noticeable differences in pollution reductions by household income quartile. Although the sampling errors are quite large, it appears that air quality improvements were slightly greater for the college-educated than for high school dropouts. However, regulations had no discernable impact on the values of homes owned by the well-educated.

[41] To address this possibility, we examined 1990 county maps from the Landview geographic software with census tract-level information on the location of pollution monitors, major sources of pollution, and the demographics of the population. It appears that in many major counties (e.g., Allegheny, PA and Cook, IL) the monitors are located where the major sources of pollution are and that these sources are predominantly in or around census tracts that have less-educated, more non-white individuals with lower incomes. This is the subject of future research.

sampling errors. We conclude that the effect of TSPs on property values is relatively homogeneous.[42] However, the impreciseness of the estimates suggests that a convincing analysis would require detailed information on pollution, housing values, and demographics below the county-level (e.g., census tracts). This is the subject of future research.

Potential Interpretation of the Results and Welfare Calculations

We conclude from the above analysis that TSP pollution regulations are causally related to both pollution reductions and housing price increases during the 1970s and 1980s. In addition, it seems reasonable to presume that regulation-induced changes in pollution represent an exogenous shift in the supply of clean air driven by the federal government. Consequently, one may be able to impose a more structural interpretation on the estimated hedonic price function. In particular, under the standard assumptions of hedonic price theory, if preferences are identical for the "marginal" home-buyer across counties, then our estimates are identifying the "average" slope of the marginal individual's indifference curve. If in addition preferences are linear with respect to air quality, then we are identifying the marginal individual's willingness-to-pay for the air quality improvement.[43] Since we have valid exclusion restrictions in the air quality demand equation, we can circumvent many of the econometric identification problems that arise when trying to infer demand parameters from the estimated price function (see Brown and Rosen 1982, Bartik 1987, and Epple 1987).

The potential dynamic response of housing prices to the regulation-induced air quality changes can be examined in a simple demand and supply framework. For now, focus on the response of pollution and housing prices to the 1974-75 TSP regulations. We found that mid-decade regulations caused air pollution to decline in regulated counties over the next 2-3 years. Suppose that the supply-side of the housing market is inelastic in the short-run and perfectly elastic in the long-run. Then as the demand for housing in regulated counties increases in response to the pollution reductions, housing prices will be immediately bid

[42] Similar results were found for the effect of 99% TSP readings.

[43] A demand equation that is homogeneous and linear in air quality implies individual preferences that are also homogeneous and linear. The previous results are consistent with a view that preferences are approximately homogeneous and linear in air quality.

up and the air quality improvements will be fully capitalized into housing. According to capitalization theory, this initial jump in property values represents the present discounted value of the future stream of benefits from the amenity change to home-owners in regulated counties. In the long-run, the supply-side of the housing market adjusts fully to the demand shock and housing prices will fall. The previous results suggest that the housing market fully adjusted within about five to six years of the initial housing price increase caused by the improvements in air quality.

Under the above assumptions, we can calculate the economic benefits of the mid-decade TSP regulations to individuals in the United States during the 1970s and 1980s. In the 1970s, there was about a 7-unit reduction in mean TSPs associated with 1974-75 regulations. In addition, air quality improvements were capitalized into property values at the rate of about one-percent. There were about 40 million homeowners in the United States in the late 1970s (based on rough calculations from the PUMS Census counts), and about 45% of the U.S. population lived in regulated counties (see Table 1). Finally, the average value of homes in regulated counties in the mid-1970s was about $65,000-70,000 in $1982-84 (see Table 2). According to these figures, the aggregate economic benefits of the 1974-75 TSP pollution regulations to home-owners in regulated counties were over $80 billion (in $1982-84). Based on similar calculations, the aggregate economic benefits of the 1984-85 TSP regulations were about $50 billion. The estimated direct effect of regulation on changes in housing prices implies aggregate benefits estimates of the same magnitude.

There are several reasons why the above benefits estimates may understate the true overall benefits of the pollution regulations. First, ideally we would like to observe the immediate response of housing prices to the pollution reductions caused by the mid-decade regulations, since this represents the full value of the regulations in the year of capitalization. However, we only observe prices at the end of each decade. To the extent that housing markets have adjusted to the initial price increase by the end of the decade, our estimates of the capitalization will understate the true value of the mid-decade regulations. Given that the pollution reductions induced by the 1974-75 regulations were probably fully realized by about 1978, the size of the understatement may not be large. Second, we are only estimating the benefits of the mid-decade regulations since the housing price effects of the regulations in other years are not measurable given the data constraints. Finally, any benefits accruing to renters in regulated counties are not picked up in the above calculations.[44]

Of course, there are caveats with the above interpretation of the results. First, if preferences for air quality vary, then individuals will self-select to the locations that provide the housing price/air quality bundle that provides the most utility (Tiebout 1956 and Rosen 1974). Due to this "perfect" sorting by tastes, the hedonic housing price-air quality locus maps out the outer envelope of the indifference curves of the marginal consumers in each market/county. As a result, it becomes nearly impossible to transform the estimated equilibrium price differentials into primitive taste parameters for clean air even if the pollution shocks are exogenous. In addition, the results in Table 9 suggest that high school dropouts benefited more from pollution regulations in the 1980s than college graduates. However, market segmentation within counties provides another potential explanation for the differences in price gradients by education group. Future research will attempt to examine these issues in more detail using an analysis of census tract data.

Conclusion

This study exploited the quasi-random assignment of air pollution changes across counties induced by federally mandated air pollution regulations to identify the impact of particulate matter on property values. Two striking empirical regularities emerged from the analysis. First, particulate matter declined substantially more in regulated than in unregulated counties during the 1970s and 1980s. At the same time, housing prices rose more in regulated counties. The evidence suggests that this approach identifies two causal effects: 1) the impact of regulation on air quality improvements, and 2) the impact of regulation on economic gains for home-owners. In addition, the results highlight the importance of choosing regulatory instruments which will be orthogonal to unobserved housing price shocks that vary by county over long time horizons.

[44] Appendix Table 2 presents the IV estimates of the effect of changes in TSPs on changes in rental prices for 1970-1990. The ratio of the rental price gradient to the housing price gradient varies between 0.25-0.85 for the 1970s and between 0.3-0.7 for the 1980s. If renters and home-owners have identical tastes and renters only value pollution reductions in the current year, then these ratios approximate the time discount rate of home-owners. Otherwise, there is little information contained in this statistic.

It appears that using regulation-induced changes in particulate matter leads to more reliable estimates of the capitalization of air quality into property values. Whereas the conventional cross-sectional and "fixed effects" estimates are unstable and indeterminate across specifications, the instrumental variables estimates are much larger, insensitive to specification of the model, and appear to purge the biases in the conventional estimates. The "causal" estimates imply that a one-unit reduction in suspended particulates results in a 0.7-1.5 percent increase in home values. In addition, one set of calculations suggests that the pollution regulations may have resulted in over $80 billion (in $1982-84) of aggregate economic gains for home-owners in regulated counties during the 1970s and $50 billion of benefits in the 1980s.

The results from this study show the potential promise of using hedonic analysis of the housing market to measure the value of environmental resources and the monetary benefits of federal pollution regulations. We believe that additional research on this topic is called for and imperative. For example, future research will use a disaggregate analysis of census tract-level information to examine which individuals experienced the regulation-induced air quality improvements and, consequently, who benefited the most from the pollution regulations.

DATA APPENDIX

Determining Attainment/Non-attainment Status at the County Level

The ability to accurately determine the EPA's assignment of counties to attainment/non-attainment status for TSPs is crucial for implementing our research design. In the 1972-1977 period, the EPA did not publicly release the names of the counties that were designated non-attainment. 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 used our pollution data to replicate the EPA's selection rule. Counties with monitor readings exceeding the NAAQS for TSPs were assigned non-attainment status; all other counties were designated attainment.

Beginning in 1978, the *Code of Federal Regulations* (Title 40, Part 80) published annually the names of counties whose pollution levels exceeded the federal standards. For each of the regulated pollutants, the CFR lists every county as, "does not meet primary standards," "does not meet secondary standards," "cannot be classified," "better than national standards," or "cannot be classified or better than national standards." In addition, the CFR occasionally indicates that only part of a county did not meet the primary standards. For the years 1978 through 1990, we assigned a county to the TSP non-attainment category if all or part of it failed to meet the "primary standards" for TSPs in that year; otherwise, it was assigned to TSP attainment. These annual county-level designations were collected for each of the 3,063 U.S. counties. Comparing the 1978 published attainment status to the attainment status generated from the EPA's selection rule using the 1977 pollution data suggests that our regulation data for 1972-77 is accurate. The results in Figure 5A confirm this interpretation.

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

Central to the credibility of this quasi-experiment 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 non-attainment status, it is possible that the 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.[45] In particular, the legislation forbids states from siting a monitor in a location that does not meet one of the scientific criteria outlined for monitors.[46]

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 SIPs. Since the SIPs 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

[45] The substance of this discussion results from the Code of Federal Regulations (CFR) 1995, title 40, part 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.

[46] 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. The CFR, moreover, 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 the above.

often require photographs of sites to verify a monitor's placement. Fourth, it is illegal to move many of the monitors. For the monitors that can be moved, the relocation can only be done 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."[47]

Matching the Census PUMS Extracts to the TSPs Pollution and Regulation Data

First, we created two extracts of the Census 5-percent PUMS in which we calculated the mean and median of log-property values (as well as the means of all of the control variables described below) by year, PUMA/County Group, and demographic cells. For one data extract, six demographic cells were created from a black-white racial indicator and three education group indicators: less than a high school degree, high school degree to some college, college degree or more. For the other extract, four cells were created corresponding to household income quartiles.[48] To ensure a single reported property value for each property, only the property values reported by the heads of household were used to calculate the means and medians.[49]

Next, we wrote code for three different schemes for matching the PUMA/County Group identifier provided in the PUMS to the FIPS county codes.[50] The schemes matched PUMAs to counties which accounted for 100%, at least 75%, and at least 50% of the populations in the PUMAs.[51] Finally, we matched the merged cell-level data from all household heads in the 1980 and 1990 5-percent PUMS to the TSP pollution and regulation data and the CCDB data by the FIPS county identifiers. The results in Table 9 were insensitive to the choice of matching algorithms.

Variables from the 1972, 1983, and 1994 County and City Data Books

The following are the list of variables taken from the 1972, 1983, and 1994 County and City Data Books (CCDB) and used in the property value regressions. Most of the information comes from the 1970, 1980, and 1990 Censuses of Population and Housing. The crime data comes from the U.S. Federal Bureau of Investigation; the medical data comes from the American Hospital Association and the American Medical Association; 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.

[47] It should also be noted that the county measures of mean TSP pollution levels used in the analysis are based on averaging the annual geometric mean reading of every monitor in the county over 2-3 years. Consequently, any idiosyncratic shocks to pollution levels in a county in a short time span will not pose any problems.

[48] For 1980 the household income quartiles were defined as follows (in nominal dollars): quartile 1, household income less than or equal to $9,000; quartile 2, household income between $9,000 and $17,450; quartile 3, income between $17,450 and $27,600; quartile 4, income greater than $27,600. For 1990 the quartile cut-off points were less than $17,200, between $17,200 and $33,000, between $33,000 and $54,500, and greater than $54,500. The quartiles were based on households in the Mid-Atlantic region.

[49] For the demographic cells extract, there are a total of 6,636 and 6,646 cells for the 1980 and 1990 PUMS, respectively. For the household income quartile extract, there are a total of 4,616 and 4,580 cells for the 1980 and 1990 PUMS.

[50] The code came from a combination of the MABLE/GEOCORR Geographic Correspondence Engine Version 2.5 for the 1990 Census and the 1980 County Group Equivalency File for the 1980 Census.

[51] The following information gives a sense of the number and comprehensiveness of the various matches. First, the total U.S. populations in 1980 and 1990 were 226,545,805 and 248,709,873, respectively. For the matching requiring that a county account for 100% of the PUMA population: 603 PUMAs accounting for 369 counties with a population of 143,462,851 could be matched for 1980, while 604 PUMAs accounting for 404 counties with a population of 168,576,749 could be matched for 1990. For matches in which the county accounts for at least 50% of the PUMA population, the numbers were 831 PUMAs, 561 counties, and 178,872,025 people for 1980 and 836 PUMAs, 616 counties, and 203,467,422 people for 1990. Consequently, it appears that the PUMAs/counties that can be matched are relatively large and that the analysis of the Census PUMS extracts is fairly comprehensive.

We start with the variables used in the 1980 analysis from the 1983 CCDB and then describe which variables were either unavailable for or added to the analysis for the other years.

<u>outcome variable</u>
log-median value of owner occupied housing units in 1980
(deflated to $1982-84 by the total shelter component of the CPI)

<u>population and demographic variables</u>
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 degree in 1980
% of population over 25 with at least a college degree in 1980

<u>socioeconomic variables</u>
% of employment in manufacturing in 1980
civilian labor force (aged 16 or older) unemployment rate
per-capita money income in 1979
% of families below the poverty level in 1979
% of population in urban area

<u>housing variables</u>
% 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

<u>neighborhood variables</u>
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

<u>spending and tax variables</u>
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

<u>For 1970 the following variables were unavailable</u>:

% 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 1990 the following variables were unavailable:
% of population female
% of population in urban area
% of occupied housing units lacking complete plumbing
property crimes per 100,000
per-capita property taxes
% of spending on education
% of spending for police protection
% of spending on public welfare
% of spending on health
% of spending on highways

For 1990 the following variable was added:
violent crimes per 100,000 in 1991

For 1970-80 and 1980-90 Fixed Effects and Instrumental Variables Regressions
"First differences" in all of the variables that are in both CCDB's for the years examined were included as control variables.

Variables from the 1980 and 1990 Census 5% PUMS Data

The following are the list of variables taken from the 1980 and 1990 Censuses of Population and Housing 5-percent Public Use Microdata Samples and used in the property value regressions. The codebooks contain detailed explanations of each variable. Crude information on number of bathrooms, air conditioning, and heating equipment is only available in the 1980 Census. Variables from the 1983 and 1994 County and City Data Books described above were matched to the Census PUMS data by county and used as control variables in the analyses. All variables were included as "first differences" in the fixed effects and instrumental variables analyses.

outcome variable
log-mean property value of owner occupied housing units

The original Census variable is in 24 categories for 1980 and 25 categories in 1990. The mid-point for each category was used when calculating the means of log-property values.
(deflated to $1982-84 by the total shelter component of the CPI)

control variables
black-white (indicator and %)
less than high school degree (indicator and %)
high school degree to some college (indicator and %)

college degree or better (indicator and %)
average age
% 65 years or older
% female
household income (indicator and average)
% below poverty line
% in pollution intensive manufacturing
(metal and nonmetal mining; lumber and wood products; wood furniture and fixture; pulp and paper; printing; inorganic and organic chemicals; petroleum refining; rubber and miscellaneous plastic products; stone, clay, glass, and concrete; iron and steel; nonferrous metals; fabricated metals; electronics; motor vehicles, bodies, parts, and accessories; dry cleaning; Source: U.S. EPA)
% in other manufacturing
% outside of manufacturing
average travel time to work
average rooms
average bedrooms
% less than one acre
% ten acres or more
% built 0-5 years ago
% built 5-10 years ago
% built 10-20 years ago
% built 20-40 years ago
% built 40 or more years ago
% with telephone
% moved in last 0-5 years
% moved in 5-10 years ago
% moved in 10-20 years ago
% moved in 20 or more years ago
% commercial establishment on property
% no automobiles
% 2 automobiles
% 3 or more automobiles
% complete plumbing facilities
% complete kitchen facilities
Variables from 1983 and 1994 CCDB's also included
(population density, unemployment rate, vacancy rate, % owner-occupied, overall crime rate per 100,000, serious crimes per 100,000, physicians per 100,000, hospital beds per 100,000, per-capita total taxes, per-capita government revenue, per-capita general expenditures)

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Table 1: Characteristics of Counties with Total Suspended Particulates Monitors

	Attainment Counties (1)		Nonattainment Counties (2)	
Number of Counties Monitored				
1971-2	810		287	
1977-79	1000		444	
1987-9	873		127	
Share of U.S. Population in Monitored Counties				
1971-2	0.414		0.396	
1977-79	0.399		0.463	
1987-9	0.516		0.262	
Mean (Standard Deviation) of County-Specific Average Hourly Reading				
1971-2	64.6	(29.1)	92.3	(30.5)
1977-79	54.8	(17.2)	78.2	(31.1)
1987-9	50.1	(14.5)	68.0	(19.1)
Mean (Standard Deviation) of County-Specific 90th Percentile Hourly Reading				
1971-2	117.8	(82.1)	208.7	(119.1)
1977-79	98.0	(35.7)	168.3	(75.7)
1987-9	89.9	(33.6)	144.9	(59.8)
Mean (Standard Deviation) of County-Specific 99th Percentile Hourly Reading				
1971-2	187.4	(224.1)	415.7	(278.4)
1977-79	160.2	(85.1)	390.8	(404.8)
1987-9	150.9	(95.5)	256.4	(144.1)

Notes: The year-specific monitor readings are calculated as the mean of the county-specific mean (or percentile) In counties with multiple monitors, the county-specific mean is calculated as the weighted average of the monitor-specific means, where the weight is the number of monitor observations. In multiple-monitor counties, the relevant percentile is the maximum of that percentile from all monitors in that county. TSPs are measured in micrograms per cubic meter.

Source: Authors' tabulations from EPA's "Quick Look Reports" data file and Code of Federal Regulations.

Table 2: Means of County Characteristics by Regulation Status

	1970 Characteristics		**1980 Characteristics**	
	Unregulated in 70s	Regulated in 70s	Unregulated in 80s	Regulated in 80s
Property Value ($82-84)	41,940	59,969	56,363	76,610
Pop. Density	332	4,905	524	4,717
% Manufacturing	0.27	0.25	0.23	0.22
% White	0.90	0.86	0.86	0.78
% >= HS Grad.	0.51	0.56	0.67	0.70
% >= Coll. Grad.	0.10	0.12	0.16	0.19
Unemp. Rate	0.043	0.045	0.064	0.068
Income per-capita ($82-84)	7,289	9,028	8,647	9,758
Crime Rate (per 100,000)			5,523	6,739
N	1,188	228	824	158

Notes: County characteristics taken from the 1972 and 1983 County and City Data Books. Unregulated (Regulated) counties are those that were never (ever) non-attainment for TSPs during 1971-78 and 1981-88 for the 1970s and 1980s, respectively. The CPI and total shelter component of the CPI were used to deflate income per-capita and property values. Means calculated using county population sizes as weights.

Table 3: Cross-Sectional Estimates of the Effect of Pollution on Log-Property Values
1970, 1980, and 1990 County Data Books
(estimated standard errors in parentheses)

	1970				1980				1990			
Mean TSP (1/100)	0.021 (0.035)	**-0.066** **(0.015)**	-0.019 (0.014)	-0.011 (0.012)	0.152 (0.048)	**0.085** **(0.022)**	-0.026 (0.018)	-0.014 (0.016)	0.600 (0.092)	**0.164** **(0.044)**	**-0.119** **(0.036)**	-0.062 (0.032)
99% TSP (1/100)	0.007 (0.003)	-0.002 (0.002)	0.002 (0.001)	0.000 (0.001)	0.004 (0.004)	-0.009 (0.002)	-0.001 (0.002)	-0.003 (0.001)	0.023 (0.015)	0.017 (0.007)	-0.011 (0.004)	-0.001 (0.004)
Pop. Density	N	Y	Y	Y	N	Y	Y	Y	N	Y	Y	Y
Pct. Employ Manf.	N	Y	Y	Y	N	Y	Y	Y	N	Y	Y	Y
Demographics	N	Y	Y	Y	N	Y	Y	Y	N	Y	Y	Y
Socioecon. Vars.	N	Y	Y	Y	N	Y	Y	Y	N	Y	Y	Y
Housing Vars.	N	Y	Y	Y	N	Y	Y	Y	N	Y	Y	Y
Fiscal and Tax Vars.	N	Y	Y	Y	N	Y	Y	Y	N	Y	Y	Y
Crime Vars.	N	N	N	N	N	Y	Y	Y	N	Y	Y	Y
Medical Vars.	N	N	N	N	N	Y	Y	Y	N	Y	Y	Y
State Effects	N	N	Y	Y	N	N	Y	Y	N	N	Y	Y
Flexible Form	N	N	N	Y	N	N	N	Y	N	N	N	Y
R-squared (Mean TSP Reg.)	0.00	0.84	0.91	0.95	0.01	0.84	0.94	0.96	0.04	0.85	0.95	0.97

Notes: See notes to Tables 1 and 2. See the text and Data Appendix for the definition of the pollution and control variables used. The 1970, 1980 and 1990 TSP variables are based on the 1971-72, 1977-79, and 1987-89 means of the annual statistics. The Mean TSP and 99% TSP coefficient estimates are based on separate regressions. The R-squared is from the regression using Mean TSP. The flexible functional form included quadratics, cubics, and interactions of the variables as controls. All regressions are weighted by the county population sizes.

The mean of property values (in $1982-84) is $50,384, $61,061, and $67,312 in 1970, 1980 and 1990, respectively. The mean (standard deviation) of Mean TSP is 71.9 (31.9), 62.0 (24.9), and 52.4 (16.3) for 1970, 1980 and 1990. The mean (standard deviation) of 99% TSP is 247.2 (259.5), 231.1 (258.2), and 164.3 (108.7) for 1970, 1980, 1990. The sample sizes of the regressions are 1,073, 1,392, and 983 in 1970, 1980, and 1990.

Table 4: Fixed Effects Estimates of the Effect of Changes in Pollution on Changes in Property Values
1970, 1980, and 1990 County Data Books
(estimated standard errors in parentheses)

	1970-80		**1980-90**	
	Mean TSP (1/100)	99% TSP (1/100)	Mean TSP (1/100)	99% TSP (1/100)
County Effects	**-0.060**	0.002	**0.096**	-0.002
	(0.027)	(0.002)	**(0.044)**	(0.003)
R-squared	0.98	0.98	0.99	0.98
County Effects and State-Year Effects	**-0.027**	0.000	**0.137**	0.003
	(0.017)	(0.001)	**(0.032)**	(0.002)
R-squared	0.99	0.99	0.99	0.99

Notes: See notes to Table 3. The regressions were estimated using the county population sizes as weights. See the Data Appendix for the set of control variables used.

Table 5: Reduced-Form Estimates of the Effect of Air Quality Regulation on Changes in Air Pollution and Changes in Property Values
(estimated standard errors in parentheses)

	1970-80 Change			1980-90 Change		
	Mean TSP	99% TSP	Log-Value	Mean TSP	99% TSP	Log-Value
TSP Non-attainment 1974-75 (84-85)	**-6.84** **(1.40)**	**-49.44** **(19.60)**	**0.048** **(0.008)**	**-3.88** **(1.05)**	**-55.57** **(16.12)**	**0.039** **(0.013)**
TSP Non-attainment 1971-72 (81-82)	-9.66 (1.36)	-157.03 (18.79)	-0.003 (0.009)	-4.32 (0.88)	-50.57 (13.43)	-0.040 (0.011)
Pop. Density		Y			Y	
Pct. Employ Manf.		Y			Y	
Demographics		Y			Y	
Socioecon. Vars.		Y			Y	
Housing Vars.		Y			Y	
Fiscal and Tax Vars.		Y			Y	
Crime Vars.		N			Y	
Medical Vars.		N			Y	
F-stat. Non-attainment (numerator d.o.f.)	24.03 (1)	6.36 (1)	31.27 (1)	13.53 (1)	11.89 (1)	8.90 (1)
F-stat. other variables (numerator d.o.f.)	3.05 (21)	4.14 (21)	48.33 (21)	9.15 (21)	5.33 (21)	83.75 (21)
F-stat. State-year effects (numerator d.o.f.)	3.34 (47)	3.93 (47)	48.42 (47)	4.00 (48)	12.01 (48)	25.19 (48)
R-squared	0.20	0.19	0.89	0.39	0.54	0.93
Mean of Dependent Var.	-13.90	-42.41	1.040	-10.20	-110.08	0.600

Notes: See notes to Table 3. The regulation variables are equal to one if the county was ever regulated for TSP during the specified two years. The estimates presented are for separate regressions of the outcome variable on 1974-75 (84-85) and 1971-72 (81-82) non-attainment status and do not include state-year effects. Including state-year effects had little effect on the point estimates. The regression diagnostics (F-statistic and R-squared) are from regressions that use 1974-75 (84-85) non-attainment status as a explanatory variable and include state-year effects.

Table 6: Instrumental Variables Estimates of the Effect of Changes in Pollution on Changes in Log-Property Values, 1970-1980
(estimated standard errors in parentheses)

	Mean TSP (1/100)			99% TSP (1/100)	
	Wald	W/ Covariates	W/ State-Year	W/ Covariates	W/ State-Year
1974-75 TSP Regulation	-0.400	**-0.698**	**-1.561**	-0.097	-0.160
(N = 986)	(0.170)	**(0.194)**	**(0.338)**	(0.043)	(0.036)
Sample Limited to:					
51 < 1974 Mean TSP < 198	-0.526	-0.890	-0.697		
(N = 701)	(0.239)	(0.272)	(0.153)		
58 < 1974 Mean TSP < 109	-0.690	-1.198	-0.824		
(N = 505)	(0.453)	(0.579)	(0.207)		
42 < 1974 Mean TSP < 75	-0.631	-1.133	-3.369		
(N = 579)	(0.620)	(0.748)	(1.251)		

Notes: The coefficients were estimated using two-stage least squares. The instrumental variable used is an indicator for whether the county was ever non-attainment for TSP in 1974-75. See the Data Appendix for the list of control variables used. The sample sizes of the respective estimated equations are in parentheses.

Table 7: Instrumental Variables Estimates of the Effect of Changes in Pollution on Changes in Log-Property Values, 1980-1990
(estimated standard errors in parentheses)

	Mean TSP (1/100)		99% TSP (1/100)	
	W/ Covariates	W/ State-Year	W/ Covariates	W/ State-Year
1984-85 TSP Regulation	**-1.001**	**-0.602**	-0.070	-0.043
(N = 908)	**(0.448)**	**(0.343)**	(0.031)	(0.015)
1984-85 Ozone Regulation	-1.511	-1.384		
(N = 908)	(0.622)	(0.981)		

Notes: See notes to Table 6. The instrumental variable used is an indicator for whether the county was ever non-attainment for TSP in 1984-85. The other instrumental variable used is an indicator for whether the county was ever non-attainment for ozone in 1984-85.

Table 8: Estimates of the Effect of Pollution on Property Values by Location
(estimated standard errors in parentheses)

	1970-80 Change Mean TSP (1/100)	**1980-90 Change** Mean TSP (1/100)
No California (N=958, 869)		
County Effects	-0.658	-1.473
	(0.211)	(0.726)
County Effects and State-Year Effects	-1.834 (0.590)	-1.471 (0.718)
Non-Rust Belt (N=769, 690)		
County Effects	-0.787	-1.676
	(0.297)	(0.843)
County Effects and State-Year Effects	-2.270 (0.580)	-0.236 (0.338)
Rust Belt Only (N=217, 218)		
County Effects	-0.383	0.013
	(0.212)	(0.913)
County Effects and State-Year Effects	-0.111 (0.174)	-2.239 (2.353)

Notes: The 2SLS estimates presented use only the specified counties (e.g., No California drops all California counties). The sample sizes of the respective estimated equations are in parentheses. The Rust Belt states are Illinois, Indiana, Michigan, New York, Ohio, Pennsylvania, and Wisconsin.

Table 9: Estimates of the Effect of Pollution on Property Values by Demographic Group
1980-1990 Census 5% PUMS
(estimated standard errors in parentheses)

	"Fixed Effects" Mean TSP (1/100)	**Instrumental Variables** Mean TSP (1/100)
Effect on Mean Log-Value	0.349	-0.885
	(0.029)	(0.243)
Effect on Median Log-Value	0.347	-0.951
	(0.030)	(0.264)
Demographic Gradients		
Black	0.465	-1.190
	(0.055)	(0.238)
White	0.334	-0.918
	(0.037)	(0.345)
< 12 yrs. of Education	0.292	-1.763
	(0.050)	(0.650)
12-15 yrs. of Education	0.197	-0.847
	(0.038)	(0.717)
>= 16 yrs. of Education	0.234	0.523
	(0.041)	(0.655)
Household Income Gradients		
Quartile 1	0.342	-0.861
	(0.075)	(0.850)
Quartile 2	0.344	-1.452
	(0.060)	(0.834)
Quartile 3	0.387	-0.190
	(0.057)	(0.710)
Quartile 4	0.380	-0.930
	(0.059)	(0.954)

Notes: See the text and the Data Appendix for the data construction and the control variables used. The specific gradients were estimated each time only on the sample of indicated individuals, analogous to a fully interacted regression equation. All analyses are weighted by the cell sizes. Results are from specifications that include state-year effects.

Appendix Table 1: Regulation, Sources, Control Technologies, and Health Effects of Total Suspended Particulates (TSPs) Pollution

<u>National Ambient Air Quality Standards</u> Maximum Allowable Concentration (Primary Standard):	
Annual Geometric Mean (never to be exceeded)	75 Micrograms per Cubic Meter
Maximum 24 Hour Concentration (not to be exceeded more than once a year)	260 Micrograms per Cubic Meter
<u>Sources</u> Industrial Processes (e.g., Pulp and Paper; Stone, Clay, Glass, and Concrete Products; Iron and Steel), Smelters, Automobiles, Burning Industrial Fuels, Woodsmoke, Dust from Paved and Unpaved Roads, Construction, and Agricultural Ground Breaking.	
<u>Techniques to Control Emissions</u> The control of TSPs is frequently accomplished by directing the polluted air through a "bag" filter, which captures the pollutants or a wet "scrubber" that increases the mass of the particulates, causing their separation from the "clean" air (Vesilind, et al., 1988).	
<u>Health Effects</u> TSPs can affect breathing and respiratory systems, causing increased respiratory disease and lung damage. Children, the elderly and people suffering from heart or lung disease (like asthma) are especially at risk. Recent research has linked particulate pollution to increased mortality rates (Dockery et al, 1993; Ransom and Pope, 1995).	

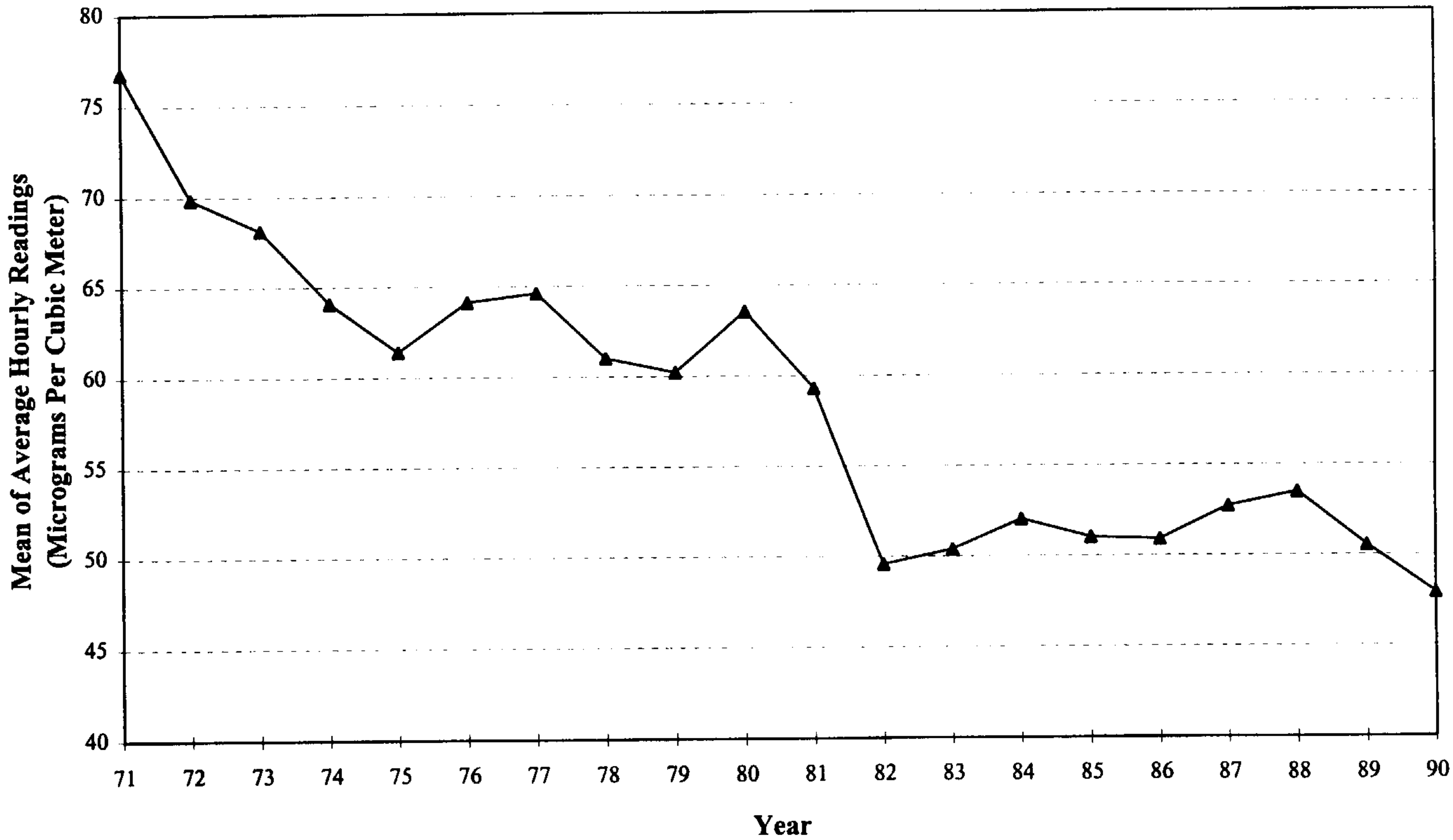
Appendix Table 2: Instrumental Variables Estimates of the Effect of Changes in Pollution on Changes In Log-Rent, 1970-1990
(estimated standard errors in parentheses)

	1970-1980 Change Mean TSP (1/100)		**1980-1990 Change** Mean TSP (1/100)	
	W/ Covariates	W/ State-Year	W/ Covariates	W/ State-Year
1974-75 (84-85) TSP Regulation	-0.173	-1.337	-0.706	-0.292
	(0.086)	(0.287)	(0.265)	(0.225)
1984-85 Ozone Regulation			-0.911	-0.424
			(0.351)	(0.447)

Notes: See notes to Tables 6 and 7. The outcome variable is the log of median gross rent of renter-occupied units in a county.

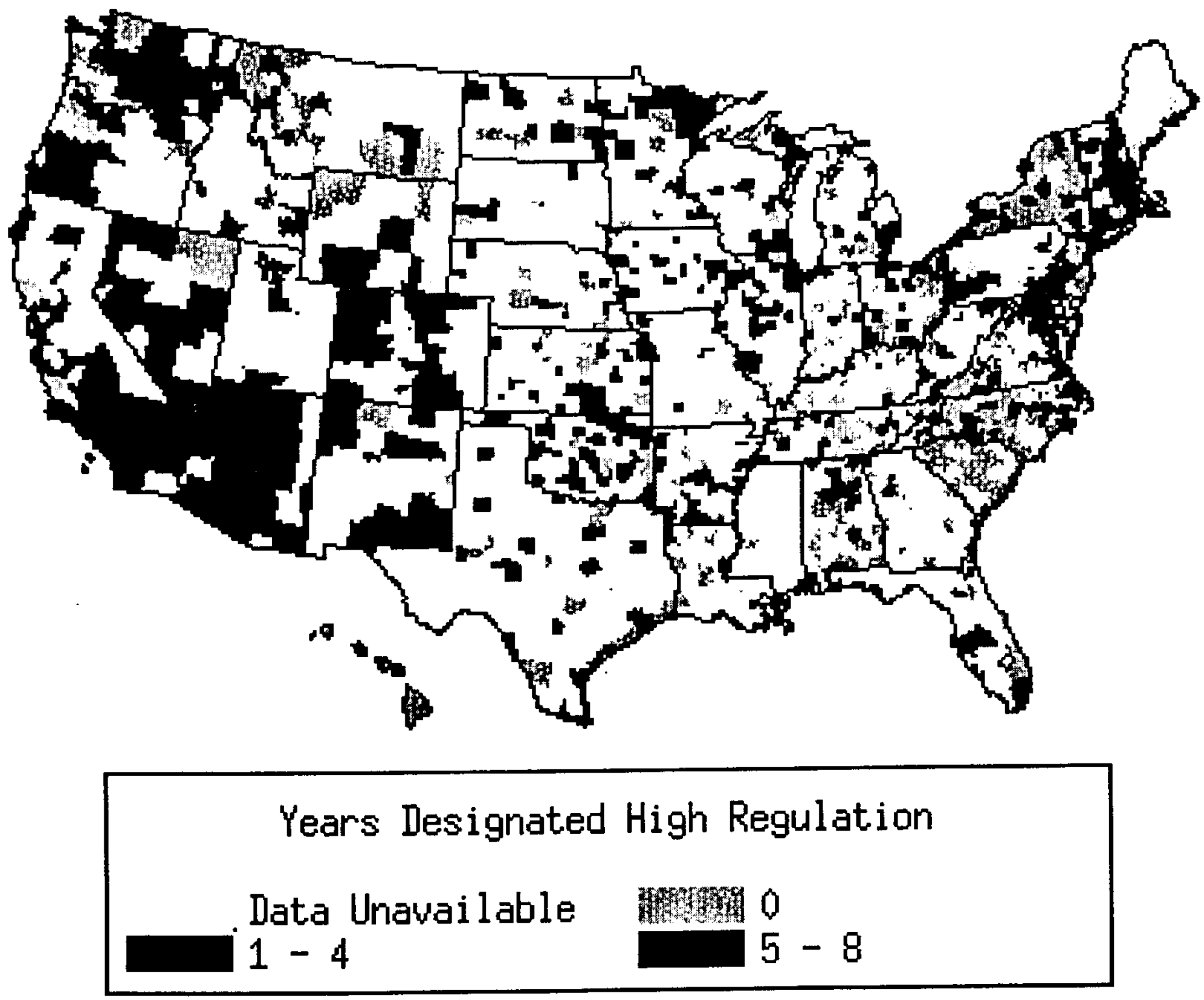
Figure 1

National Trends in Total Suspended Particulates Pollution, 1971-90



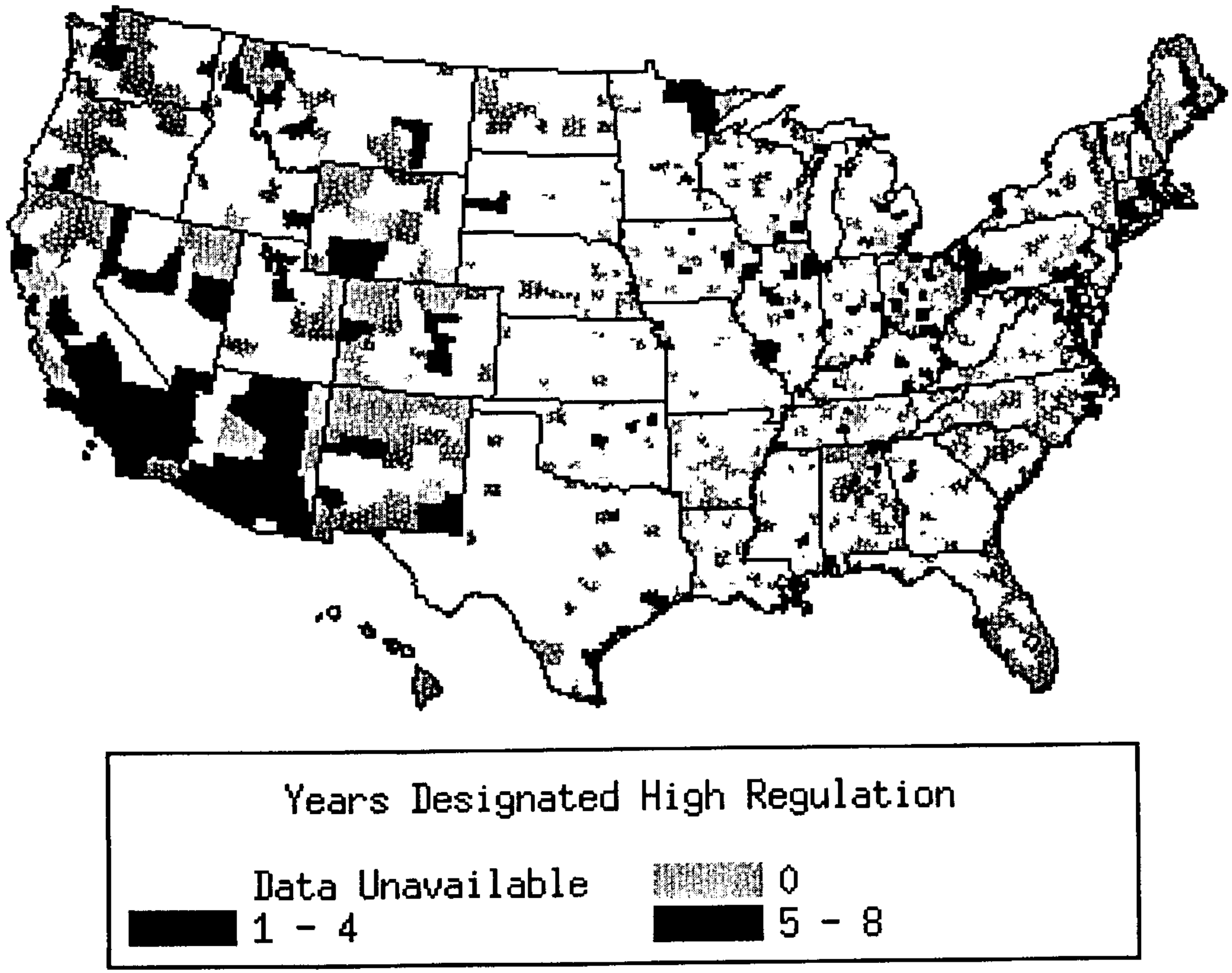
Note: The year-specific average is calculated as the mean of the county-specific means. In counties with multiple monitors, the mean is calculated as the weighted average of the monitor-specific means, where the weight is the number of monitor observations.

Figure 2A: Incidence of High Regulation for TSPs by County, 1971-1978



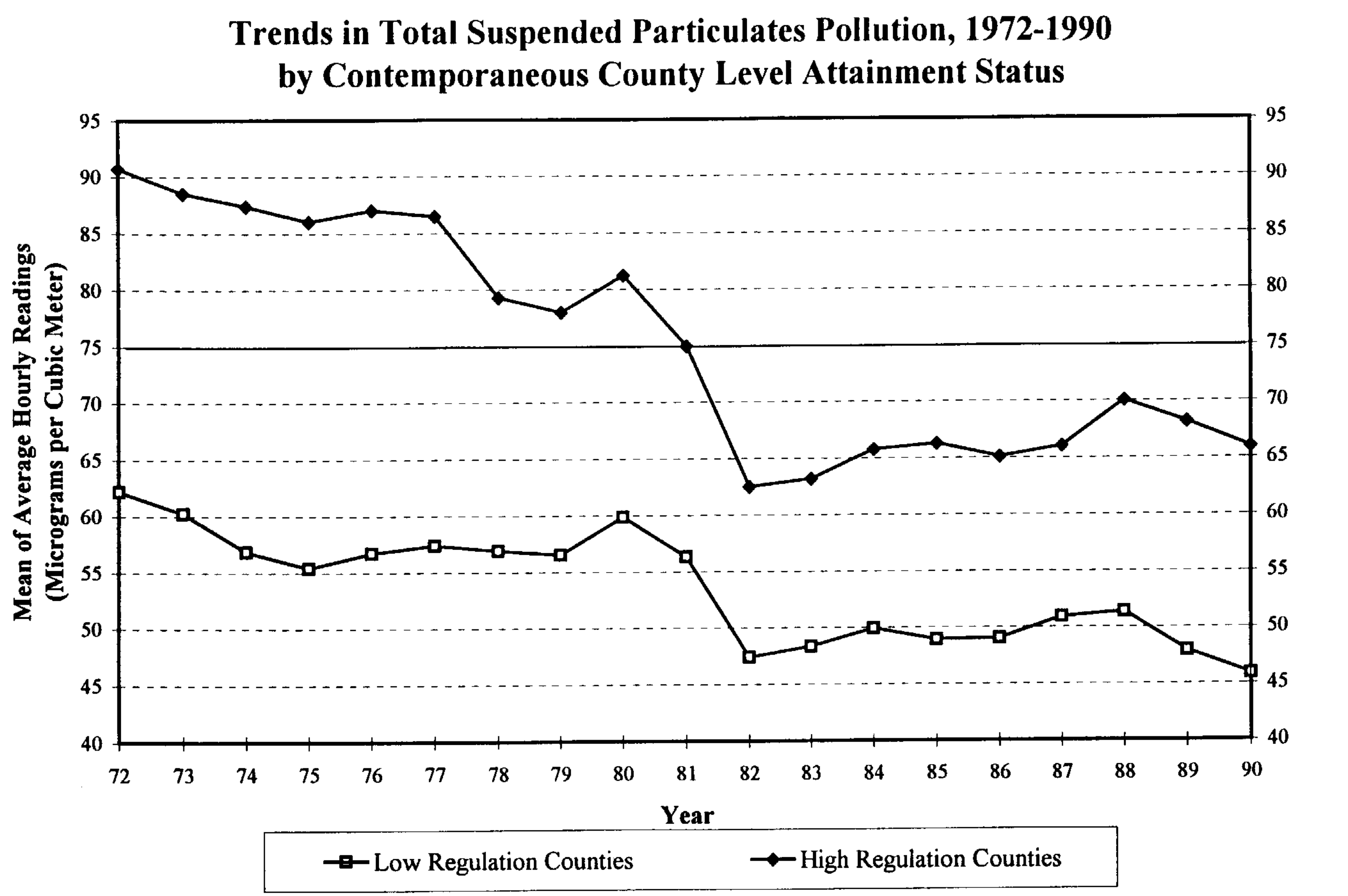
Sources: EPA Air Quality Subsystem Database and Code of Federal Regulations

Figure 2B: Incidence of High Regulation for TSPs by County, 1981-1988



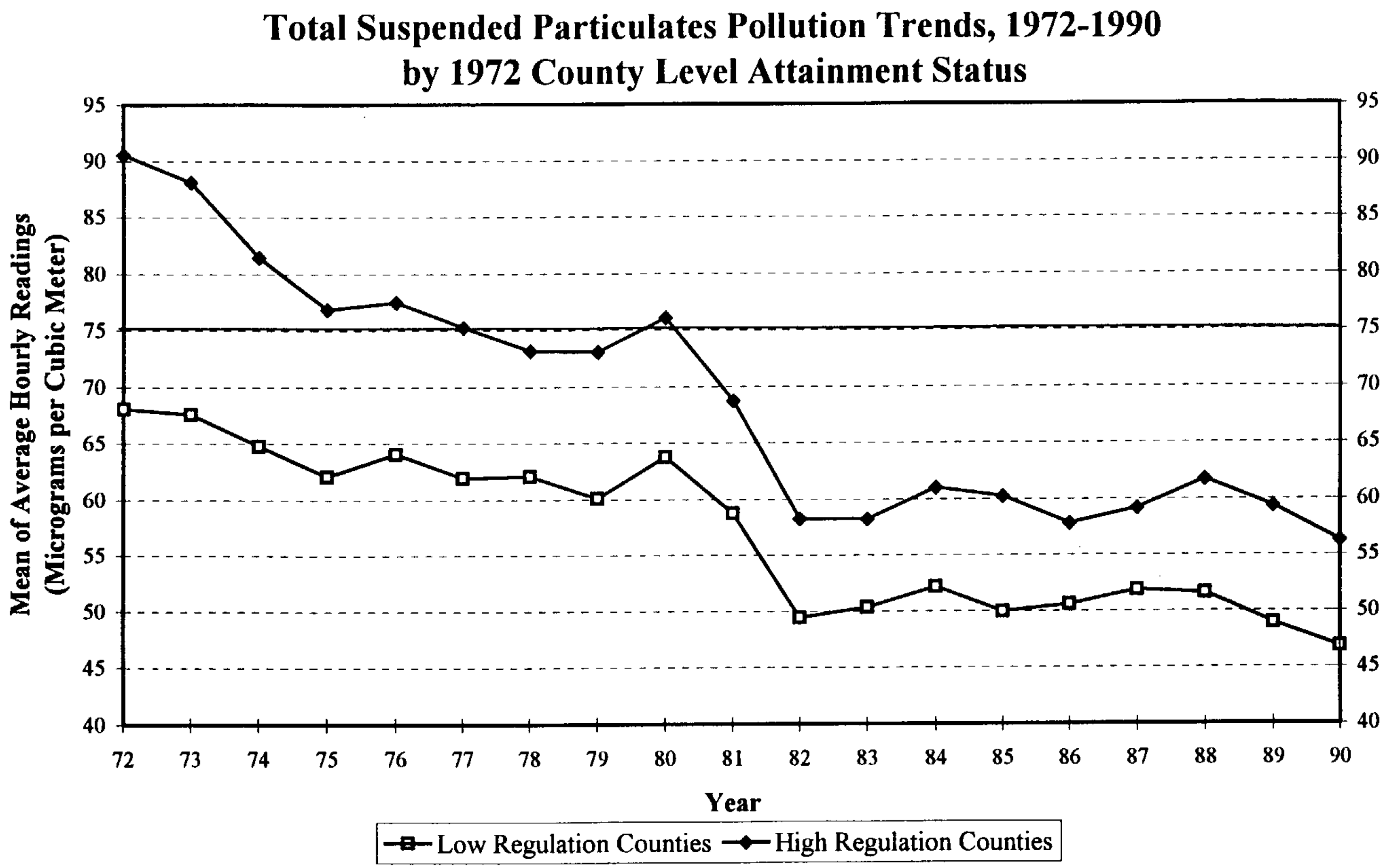
Sources: EPA Air Quality Subsystem Database and Code of Federal Regulations

Figure 3



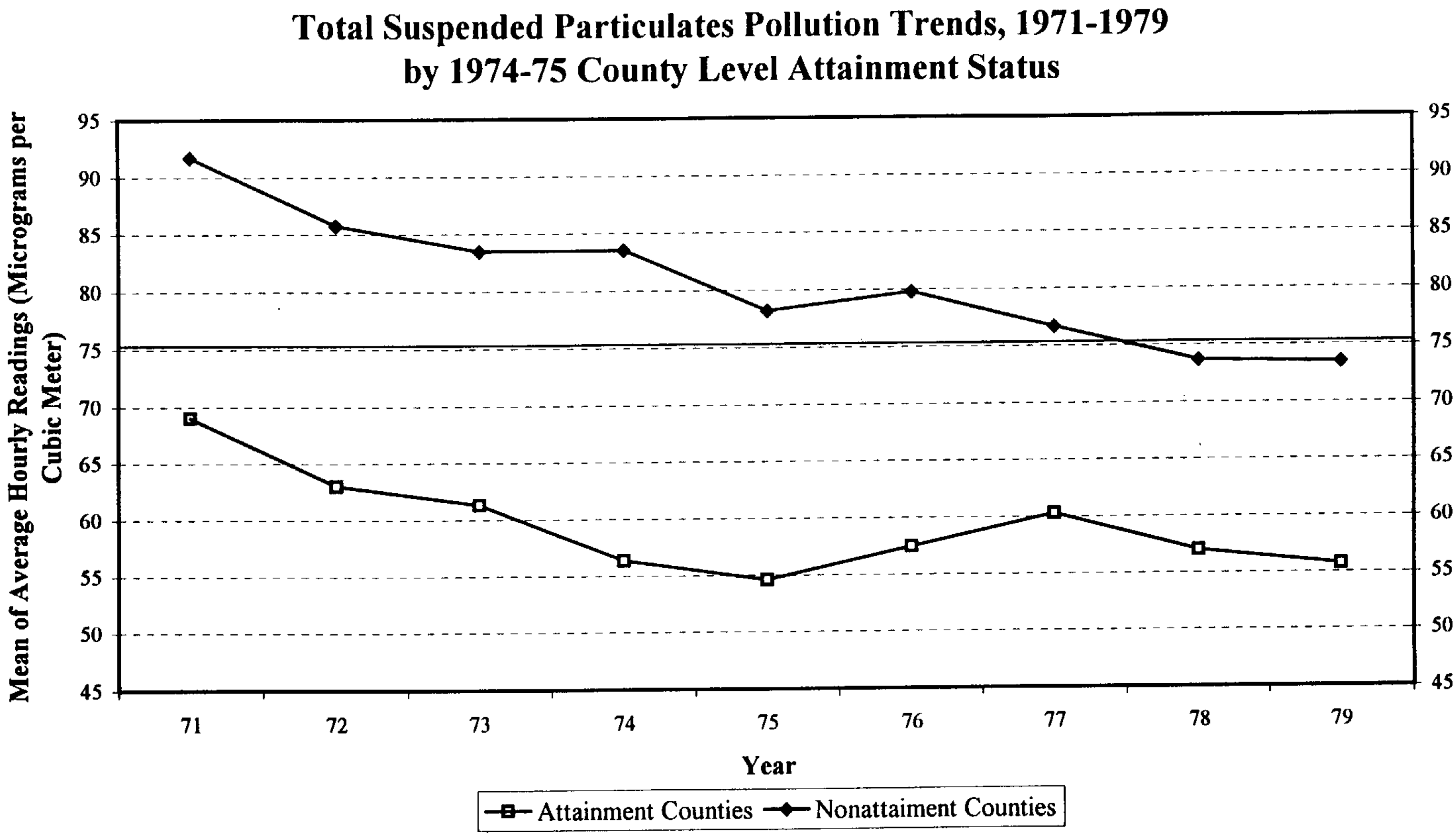
Notes: High (Low) regulation refers to counties with that designation in the year of the observation. Consequently, neither set of counties is fixed in this graph. The dark line at 75 mg / m^3 per cubic meter represents the National Ambient Air Quality Standard for the annual geometric mean of hourly readings.

Figure 4A



Notes: High (low) regulation status refers to counties with that designation in the year of the observation. The sample was limited to counties with TSP monitor readings for every year. The dark line at 75 mg / m3 represents the National Ambient Air Quality Standard for the annual geometric mean of hourly readings.

Figure 4B



Notes: Nonattainment status refers to the counties with that designation in either 1974 or 1975. The sample was limited to counties with TSPS monitor readings in both the 1971-2 and 1977-9 periods. The dark line at 75 mg / m^3 represents that National Ambient Air Quality Standard for the annual geometric mean of hourly readings.

Figure 5A

Regression Adjusted Probability of Nonattainment Status, by Mean TSPs Reading in Selection Year

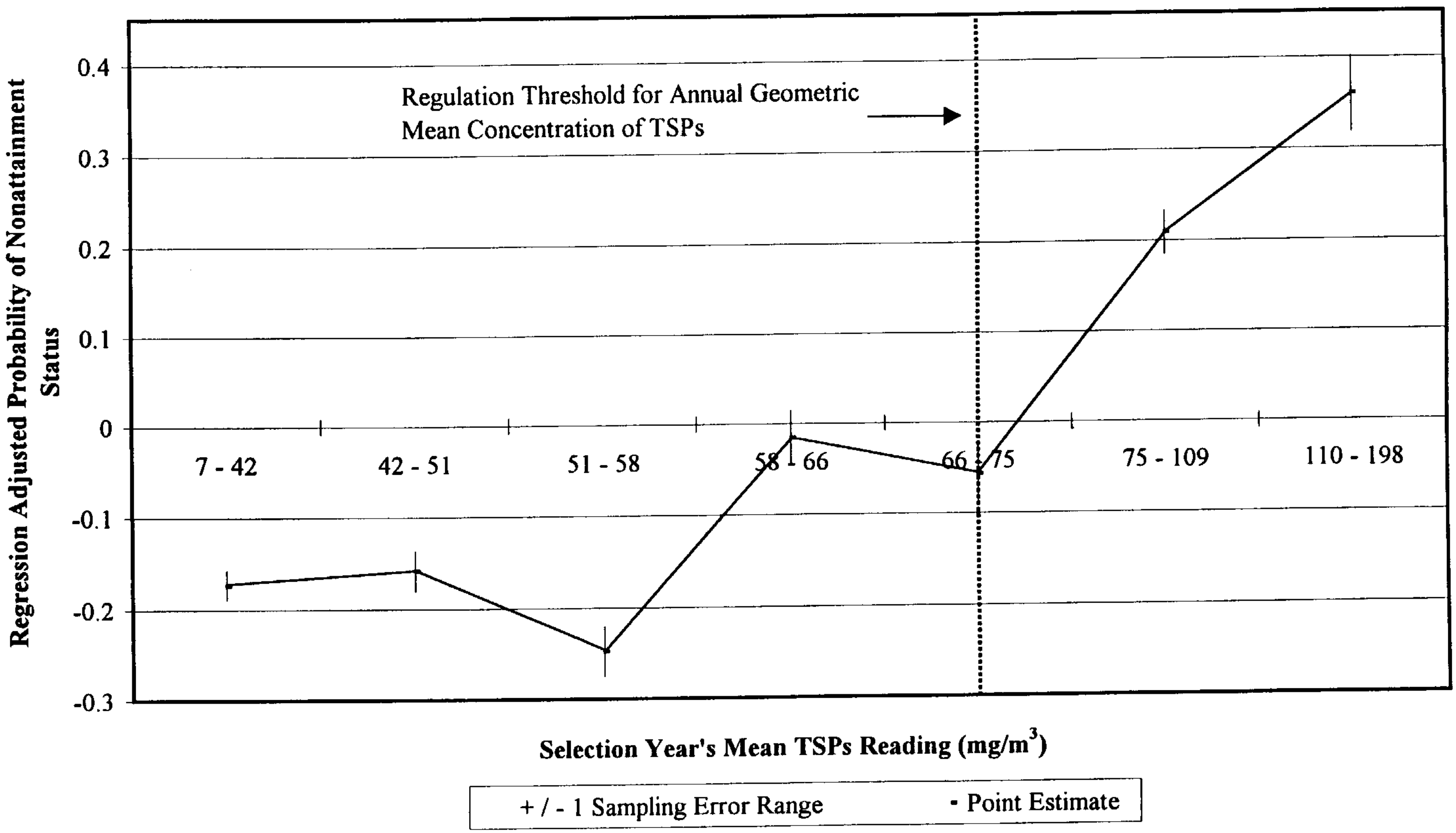


Figure 5B

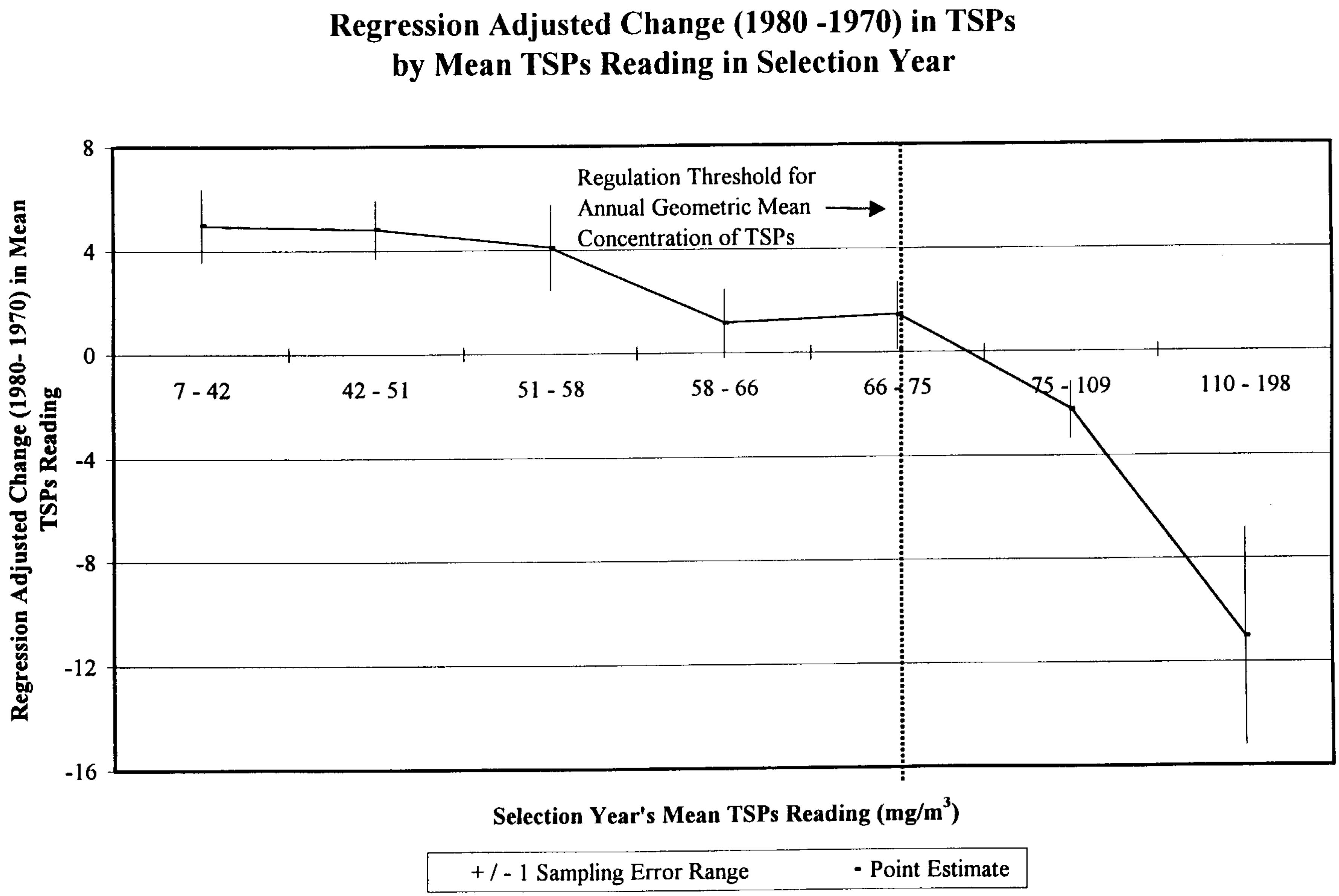
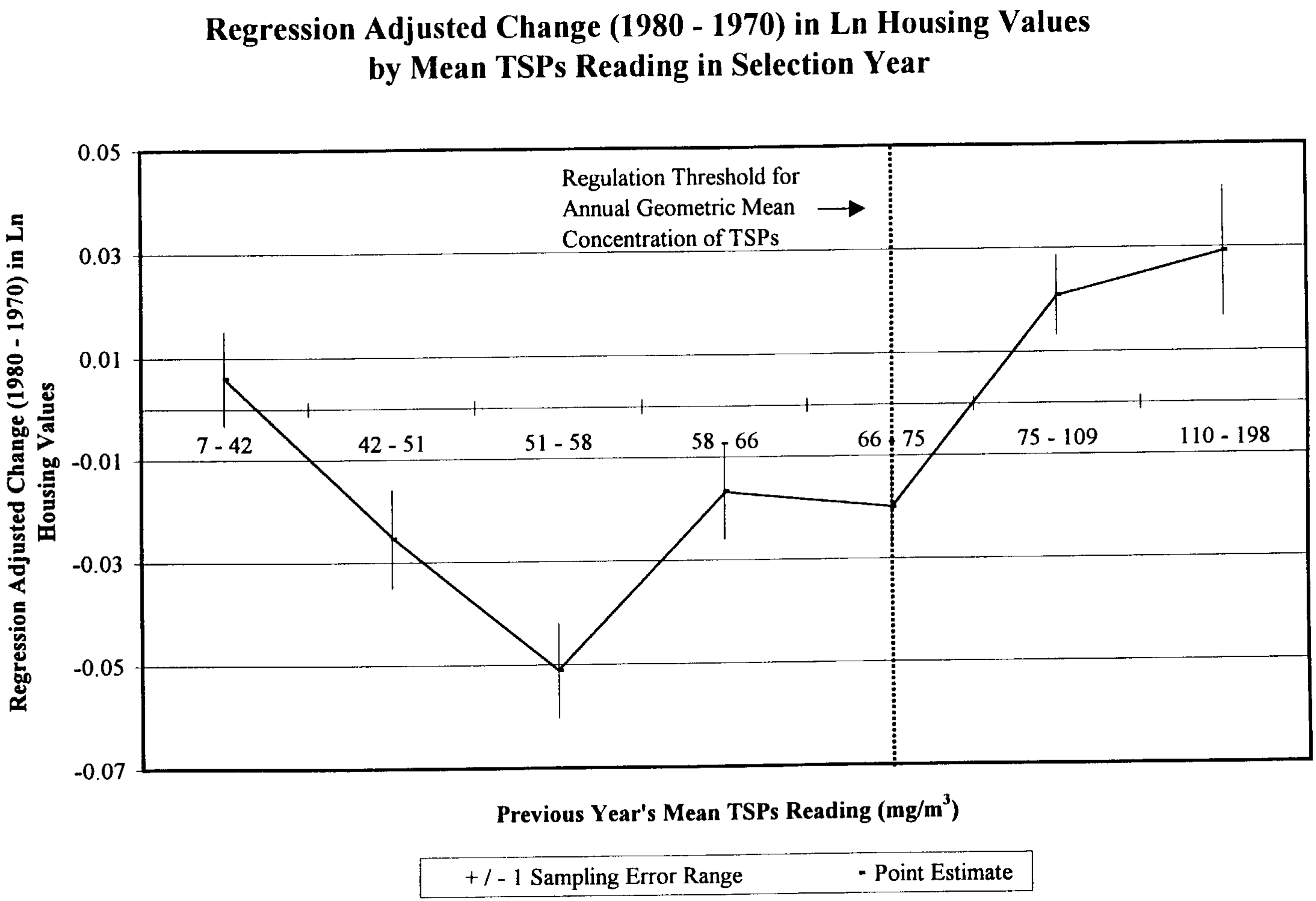

Regression Adjusted Change (1980 -1970) in TSPs
by Mean TSPs Reading in Selection Year
Regression Adjusted Change (1980- 1970) in Mean TSPs Reading
8
4
0
-4
-8
-12
-16
Regulation Threshold for
Annual Geometric Mean
Concentration of TSPs
7 - 42
42 - 51
51 - 58
58 - 66
66 - 75
75 - 109
110 - 198
Selection Year's Mean TSPs Reading (mg/m3)
+ / - 1 Sampling Error Range
- Point Estimate

Figure 5C


Regression Adjusted Change (1980 - 1970) in Ln Housing Values
by Mean TSPs Reading in Selection Year
Regression Adjusted Change (1980 - 1970) in Ln Housing Values
0.05
0.03
0.01
-0.01
-0.03
-0.05
-0.07
Regulation Threshold for Annual Geometric Mean Concentration of TSPs
7 - 42
42 - 51
51 - 58
58 - 66
66 - 75
75 - 109
110 - 198
Previous Year's Mean TSPs Reading (mg/m³)
+ / - 1 Sampling Error Range
- Point Estimate