Air Pollution and Economic Opportunity in the United States*

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Abstract

Combining 36 years of satellite derived $PM_{2.5}$ concentrations with individual-level administrative data provided by the U.S. Census Bureau and Internal Revenue Service (IRS), we provide new evidence on the important role that disparities in air pollution exposure play in shaping broader patterns of economic opportunity and inequality in the United States. We first document that early-life exposure to particulate matter is one of the top five predictors of upward mobility in the United States. Second, we exploit regulation-induced reductions in pollution exposure from the 1990 Clean Air Act Amendments to produce new age-specific estimates of pollution-earnings relationship. Combined with individual-level measures of pollution exposure during early childhood, we calculate that disparities in air pollution can account for 17-26 percent of the Black-White earnings gap, 5-27 percent of the Hispanic-White earnings gap, and 6-20 percent of the average neighborhood-earnings effect (Chetty and Hendren, 2018; Chetty, Hendren, and Katz, 2016). Collectively, our findings indicate that environmental inequality is an important contributor to observed patterns of racial economic disparities, income inequality and economic opportunity in the United States.

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1 Introduction

Neighborhoods shape economic opportunity (Chetty et al., 2014; Sharkey and Faber, 2014; Chetty et al., 2016; Galster and Sharkey, 2017; Chetty et al., 2018b; Chetty and Hendren, 2018a,b; Chyn and Katz, 2021). But what it is about neighborhoods that matters for economic opportunity is less clear.

One margin that has received little attention is the role of environmental quality. In the past decade our understanding of the economic consequences of environmental quality has grown substantially. It is now well established that even acute exposure to pollution has both immediate and persistent long-run effects on health, educational attainment, learning, decision-making, productivity, criminal activity, labor force participation, and earnings (Chay and Greenstone, 2003b; Currie and Neidell, 2005; Graff Zivin, J. and Neidell, M., 2012; Schlenker and Walker, 2015; Chang et al., 2016; Ebenstein et al., 2016; Isen et al., 2017; Chang et al., 2018). Higher exposure to particulate matter in early childhood has even been shown to have persistent effects across generations affecting later-life economic outcomes for the children of those that were in-utero exposed (Colmer and Voorheis, 2021). Alongside these causal estimates, it is widely documented that economic and environmental inequality walk hand-in-hand. Disadvantaged communities are disproportionately exposed to higher levels of pollution (Commission for Racial Justice, United Church of Christ, 1987; Mohai et al., 2009; Banzhaf et al., 2019; Colmer et al., 2020; Currie et al., 2020). Taken together, it is natural to consider how much environmental inequality could contribute to systemic disparities in economic opportunity and inequality.

To date, understanding the contribution of environmental quality in shaping economic opportunity has been constrained by data availability. While access to administrative data has driven research on inequality and opportunity into new frontiers, information about demographic characteristics within administrative records and comprehensive historical data on environmental quality has lagged behind. We take advantage of recent advances in the availability of both environmental and administrative data, combining 36 years of satellite-derived, high-resolution data on particulate matter smaller than 2.5 microns ($PM_{2.5}$) concentrations, with U.S. Census Bureau linked survey data and administrative records, providing rich information on individuals' demographic characteristics, residential histories, earnings and economic mobility.

We begin by presenting a new set of stylized facts. Replicating the analysis conducted by Chetty et al. (2014), we show that the spatial distribution of early childhood exposure to particulate matter and the spatial distribution of economic opportunity are strongly correlated. This pattern also holds at the individual level. We also document that early life

$PM_{2.5}$ exposure is one of the top five predictors of upward mobility in the United States.

To explore the contribution of environmental quality to economic opportunity we engage in two sets of empirical exercises. Our first analysis provides new estimates of the relationship between childhood particulate matter exposure on later-life earnings and upward mobility. Exploiting the introduction of the 1990 Clean Air Act Amendments, we estimate that a 1 $\mu g/m^3$ reduction in prenatal $PM_{2.5}$ exposure is associated with a \$1,105 increase in later-life W-2 earnings. This estimate is substantially larger than existing estimates. We argue this difference is driven by both differences in identifying variation, which plausibly result in larger effects, and improvements in data quality, which reduce measurement error. We also estimate that a 1 $\mu g/m^3$ reduction in prenatal $PM_{2.5}$ exposure is associated with a 1.29 percentile rank point increase in upward mobility. For context, the raw correlation between exposure to $PM_{2.5}$ at birth and upward mobility for the 1981 cohort is 0.17 rank points per $\mu g/m^3$ of $PM_{2.5}$. We estimate pollution-earnings relationships for each age of exposure from birth to age 12 and show that the relationship between pollution exposure and earnings is stable up to age 4 and then diminishes quickly. We do not estimate a meaningful relationship between particulate matter exposure and later-life earnings from age 8 onward.

Next, we engage in three decomposition exercises to explore the contribution of pollution to broader patterns of economic opportunity and inequality in the United States. First, we combine our prenatal estimate with individual-level data on pollution exposure at birth to for the population of the United States in 1981 to calculate how much pollution differences might contribute to contemporary differences in black-white earnings and intergenerational income mobility. We calculate that racial gaps in prenatal pollution exposure can account for 26 percent of the contemporary black-white earnings gap for the 1981 cohort, falling to 17 percent for the 1989 cohort. We show that the decline in the share of the gap explained over time is a function of a declining black-white $PM_{2.5}$ gap (23% decline between 1981 and 1989) and an increase in the black-white earnings gap (18% increase between 1981 and 1989).

Our second and third exercises, more directly examine the contribution of environmental quality to the overall "neighborhood effect" on earnings. We do this by revisiting the quasi-experimental and experimental evidence on neighborhood effects presented in Chetty and Hendren (2018a) and Chetty et al. (2016). In our second analysis, we combine our age-specific estimates of the pollution-earnings relationship with the predicted effect of a change in neighborhood mobility on $PM_{2.5}$ exposure, and the overall effect of a change in neighborhood on later-life earnings using the "mover's design" presented in Chetty and Hendren (2018a). Combining estimates, we calculate that the contribution of air pollution to the overall neighborhood effect is concentrated in early childhood. Air pollution accounts for up to 50% of the overall neighborhood effect until age 5, after which the contribution sharply declines. This is driven by the fact that pollution exposure has no effect on later-life earnings after the age of 7. Taking the average of the estimates across all ages, we calculate that $PM_{2.5}$ can account for up to 20% of the average neighborhood-earnings effect by age 12, and 10% by age 24. Our findings indicate that the value of different neighborhood amenities varies over the life cycle. Our third exercise revisits the Moving to Opportunity experiment, run by the U.S. Department of Housing and Urban Development in the mid-1990s. The MTO experiment offered a randomly selected subset of families living in high-poverty housing projects subsidized housing vouchers to move to lower-poverty neighborhoods. This intervention generated exogenous variation in neighborhood environments for otherwise comparable families, providing an opportunity to evaluate the effects of improving neighborhood environments on low-income families (Ludwig et al., 2013; Chetty et al., 2016). Chetty et al. (2016) estimate that the MTO delivered significant increases in later-life earnings for children who moved prior to the age of 13. We present new results showing that treated families experienced persistently lower levels of $PM_{2.5}$ compared to families that did not receive the program. Combining the causal effect of MTO on particulate matter exposure with our estimates of childhood exposure on later life earnings, we calculate that differences in childhood pollution exposure can account for 6% of the overall MTO–earnings effect. We caveat that all of these exercises make strong assumptions about the external validity of our estimated pollution-earnings relationships.

Our findings contribute to the literature on economic inequality and opportunity. Within this literature, the importance of neighborhoods has been established for the economic opportunities of children (Chetty et al., 2016, 2018a; Chyn, 2018; Deutscher, 2019; Chyn and Katz, 2021). However, the particular bundle of characteristics that makes a neighborhood an "opportunity bargain" (Chetty et al., 2018a) has largely remained a black box. We provide new evidence to suggest that environmental quality in early childhood may be an important factor in explaining the overall "neighborhood effect." Given existing evidence that neighborhoods affect earnings after the age of 7, our findings suggest that there may be differences in the value of neighborhood amenities over the life cycle. Environmental quality is particularly important in early childhood, but other neighborhood factors collectively matter more in later childhood. That improvements in place, through reductions in air pollution – a place-based policy – can shape economic opportunity and earnings, provides suggestive evidence that there may be gains from improving place, rather than requiring that people move into higher opportunity neighborhoods (Gaubert et al., 2021).

Second, we contribute to the literature on the economic importance of environmental quality. To date, much of the focus has been on the short and long-term effects of gestational exposure on health and later life labor market outcomes (Chay and Greenstone, 2003b;

Currie et al., 2013; Schlenker and Walker, 2015; Isen et al., 2017; Colmer and Voorheis, 2021). Although this literature has consistently found that "pollution matters", the degree to which pollution effects contribute to aggregate patterns of economic opportunity has not been discussed. We are the first to directly connect pollution exposure with aggregate patterns of economic opportunity and inequality, as well as providing direct evidence of the effect of prenatal pollution exposure on intergenerational income mobility. This evidence connects with recent work showing multigenerational effects of pollution exposure Colmer and Voorheis (2021), deepening our understanding of how environmental quality can have persistent effects on economic circumstances. We also provide new evidence on the pollution-earnings relationship over the life cycle, documenting in line with conventional wisdom that early childhood exposures are especially important in shaping later-life economic outcomes.

Third, we contribute to the literature on race and inequality in the United States (Myrdal, 1944; Duncan, 1968; Black et al., 2015; Margo, 2016; Andrews et al., 2017; Hardy et al., 2018; Connolly et al., 2019; Chetty et al., 2019; Derenoncourt and Montialoux, 2021; Derenoncourt, 2022). Existing work on race and inequality has either been limited by smaller samples in survey data, or lack of information on race in administrative records. We construct new linkages between administrative tax return and Census demographic data, which allow us to systematically document racial disparities in environmental quality and its consequences for economic inequality and opportunity. These new linkages open up many interesting new lines of inquiry to study the intersection of race and inequality in the United States. The focus of this paper has been on understanding racial disparities in air pollution exposure and its consequences for economic opportunity. While a large literature has documented the existence of disparities in exposure across demographic groups (Commission for Racial Justice, United Church of Christ, 1987; Mohai et al., 2009; Banzhaf et al., 2019; Colmer et al., 2020; Currie et al., 2020), less is known about how these disparities have evolved over time, and what the downstream implications of these disparities are. Following Colmer et al. (2020) and Currie et al. (2020) who use satellite data to explore the trends in environmental inequality, we show that pre-existing racial disparities in pollution exposure may account for a non-trivial share of contemporary racial economic disparities.

2 The Correlation Between Environmental Quality and Economic Opportunity

Despite decades of research on racial and economic disparities in pollution exposure, a systematic evaluation of the relationship between environmental quality and economic opportunity has been hindered by data availability. The main issue is that environmental monitoring networks are sparse. Fowlie et al. (2019) document that fewer than 20 percent of counties contain a monitor that is capable of recording fine particulate matter. Hsiang et al. (2018) calculate that only 40 percent have a monitor capable of recording any of the criteria air pollutants regulated under the Clean Air Act.

Only recently has systematic data on air pollution over time and space become available (Di et al., 2016; Van Donkelaar et al., 2016; Meng et al., 2019). These data products combine spatially continuous satellite measurements of pollution correlates (e.g., aerosol optical depth) with other observable pollution correlates—such as emissions inventories, chemical transport models, weather patterns—to provide a high-resolution and consistent understanding of particulate matter concentrations over time and space. We utilize 36 years of annual and monthly $PM_{2.5}$ estimates between 1981 and 2016 for $\sim$8.6 million U.S. grid cells that measure 0.01° by 0.01° (0.9 km by 1.1 km). We spatially intersect this data with Census tract boundary files and link it to individual-level administrative records.

On average, these estimates match up well with the "ground truth" as measured by EPA monitors (Colmer et al., 2020). In-sample measures of fit are very high. However, evidence suggests that satellite-derived measures may deviate from the ground truth in the tails of the pollution distribution (Fowlie et al., 2019). Specifically, estimates tend to be downward biased for high concentrations of $PM_{2.5}$ (Di et al., 2016; Van Donkelaar et al., 2016; Meng et al., 2019). Given existing evidence on the incidence of high pollution, this suggests that prediction errors will attenuate measured disparities, providing a lower bound on true gaps in exposure.

2.1 County-Level Facts

Using this data we explore the correlation between early life pollution exposure and upward mobility at the county-level. In Figure 1 we plot three maps of the United States. Panel (a) plots county-level measures of upward mobility for the individuals born between 1978–1982, first presented by Chetty et al. (2014). Panel (b) plots county-level average daily $PM_{2.5}$ concentrations for the year 1981, aggregated by the authors from new data provided by Meng et al. (2019). Panel (c) plots a heatmap representation of the two measures, presenting a continuous representation of the pollution-mobility relationship. We see that there is substantial spatial heterogeneity in both upward mobility and $PM_{2.5}$ levels. The most striking observation, however, is the strong visual relationship between the two. In Figure 2 we formalize this relationship, presenting the bivariate relationship between the two variables. We estimate a strong negative correlation between early life $PM_{2.5}$ levels and upward mobility. A 1 $\mu g/m^3$ reduction in $PM_{2.5}$ is associated with a 0.64 rank point increase in upward mobility. For context, a 1 $\mu g/m^3$ increase in $PM_{2.5}$ would be equivalent to moving from the 50th to the 75th percentile of the $PM_{2.5}$ distribution in 2016, and a 0.64 point increase in upward mobility is approximately one twentieth the size of the black-white gap in upward mobility from Chetty et al. (2018b).

Second, we document that environmental quality is an important correlate of upward mobility. In Figure 3 we juxtapose the relationship between $PM_{2.5}$ and upward mobility with the bivariate relationships between upward mobility and other neighborhood characteristics, first presented in Chetty et al. (2014). All correlates are standardized for comparability. We observe that $PM_{2.5}$ is one of the top five strongest bivariate predictors of upward mobility in the United States. The association between upward mobility and a one standard deviation increase in $PM_{2.5}$ is comparable in magnitude to the association between upward mobility and a one standard deviation increase in the share of residents that are black, a one standard deviation decrease in the share of workers that live within 15 minutes of work, a one standard deviation increase in the Gini coefficient, a one standard deviation decrease in income-adjusted test scores, a one standard deviation increase in the share of high school dropouts, a one standard deviation decrease in the social capital index, a one standard deviation decrease in the share of households that are married, a one standard deviation increase in the share of single moms, and a one standard deviation decrease in the teenage labor force participation rate. We do not claim causality here. Rather, we highlight the empirical relevance of early-life $PM_{2.5}$ concentrations as a predictor of upward mobility.

2.2 Individual-Level Facts

We also explore the correlation between prenatal $PM_{2.5}$ exposure and individual measures of economic disparities using the Census Bureau's data linkage infrastructure. The Census Bureau's data linkage infrastructure allows us to link data at the address, individual and firm level. The address-based linkages capitalize on the Census Bureau's Master Address File, while the person-based linkages capitalize on a reference file of all individuals who either have a Social Security Number or have filed taxes with an Individual Taxpayer Identification Number (ITIN). The unique anonymized keys that are crucial to this data linkage process – Master Address File Identifiers (MAFIDs) and Protected Identification Keys (PIKs) – are assigned to administrative records, surveys, decennial census and third-party datasets by Census staff using the enterprise Personal Validation System (Wagner and Layne, 2014). Once these keys have been attached to a file, it is then possible to link that file with any other dataset in the Census Bureau's data linkage infrastructure.

For our individual level analyses, we construct a dataset which takes advantage of the Census Bureau's linkage infrastructure to follow individuals over time and identify parent-child relationships. Our individual-level dataset starts from survey responses to the 2001-2019 American Community Surveys (ACS).[1] These surveys provide detailed sociodemographic information – including age, race, sex, education, occupation and family structure – for a very large sample of the U.S. population. We then restrict these individual responses to those born between 1976-1998. From this sample frame we link each birth to their parents based on filing status in the IRS 1040 universe from 1994-1999.[2] We assign the primary tax filer on this tax form as the child's parent.

With these parent-child links in hand, we identify the place of birth for each child and the economic circumstances of each parent at the time of birth. To do this, we link each parent to their 1040 tax returns in the years 1969, 1974, 1979, 1984 and 1989. We then assign place of birth (resolved at the census tract, zip code and county) and parental income information from the form filed in the year closest to the child's birth. Due to the incomplete coverage of tax data held by Census before 1989 we can't rule out measurement error in birth location; however, our results are robust to using place of birth at the county level from the Census Numident and to restricting the sample to those born in the exact filing years.[3]

Finally, we identify later-life economic outcomes for each child. We link all individuals to form W-2s and 1040s between 2010–2018. We then calculate total annual earnings by summing all earnings and deferred compensation across all W-2s received by an individual in a given year. Labor earnings only captures employee compensation. Earnings from independent contractors or self-employed individuals do not appear in this measure. To address this, we also measure Adjusted Gross Income from the form 1040 in which an individual appears as a primary or secondary tax filer. This measure includes all income sources.

Using this data we construct an individual level measure of economic mobility which is similar in spirit to the Chetty et al. (2014) measure used in our county-level analysis. Specifically, we calculate the difference between a child's income rank and their parent's rank in the parental income distribution.[4] This measure captures a relative mobility concept, which we argue is the relevant concept for this time period, as it abstracts from changes in the cross-sectional income distribution and the distribution of income growth which arose during our sample period. In subsequent analysis we will also consider the relationship

[1] $\approx$ 93 percent of individuals in the ACS can be assigned a PIK, the unique linkage key needed to link individuals across datasets.

[2] While the IRS required the reporting of SSNs and other personally identifiable information for dependents after the 1986 tax reforms, this information was not digitally captured until the 1990s.

[3] The Census Numident is an administrative records file derived from Social Security Administration SS-5 forms that is the universe of all individuals who have applied for a Social Security Number.

[4] We use Adjusted Gross Income as our measure of income.

between environmental quality and absolute measures of economic well-being such as labor market earnings.

We show the correlation between individual level upward mobility—the difference between an individual's rank at age 30 and their parent's rank around the child's birth—and an individual's prenatal exposure to $PM_{2.5}$ for a single cohort of individuals born in 1981. Panel B of figure 2 presents the bivariate relationship between these individual-level variables. As with our county-level analysis, we estimate a negative relationship, however there is substantially more heterogeneity. In particular, the non-parametric relationship between individual mobility and $PM_{2.5}$ exposure exhibits more of a U-shaped pattern, with higher levels of upward mobility at high levels of $PM_{2.5}$ exposure. The previous aggregate analysis may have obscured this, as many of the largest counties (e.g. Los Angeles County, CA) are also highly polluted. However, given that this is the unconditional association we are not able to give any clear interpretation to why this pattern arises. Note that the best linear approximation of this non-linear relationship (the line of best fit shown in Figure 2) between early-life air pollution exposure and upward mobility remains negative with a slope of 0.19 in rank points. In the following section, we set out to identify the causal effect of prenatal $PM_{2.5}$ exposure on earnings and our measures of economic opportunity. We then combine these estimates with individual-level measures of environmental and economic disparities to quantify the contribution that air pollution may play in accounting for observed economic disparities in the United States.

3 The Effect of Early-Life $PM_{2.5}$ Exposure on Earnings and Economic Mobility: Evidence from the 1990 Clean Air Act Amendments

To identify the causal effect of particulate matter on earnings we exploit plausibly exogenous variation in prenatal air pollution exposure that arises from the introduction of the 1990s Clean Air Act Amendments (CAAA). By leveraging improvements in the measurement of $PM_{2.5}$ exposure and a more detailed set of administrative records, we are able to refine the approach taken by a number of previous studies (Chay and Greenstone, 2003a; Isen et al., 2017; Voorheis, 2017; Colmer and Voorheis, 2021).

3.1 Data

Our sample frame for this analysis comes from the 2001-2019 American Community Survey (ACS), which we link to longitudinal information from administrative records.

To analyze the effects of the 1990 CAAA, we refine this analysis dataset to a subsample of U.S.-born ACS respondents who were born between 1989-1996, a time period that spans the enactment of the nonattainment designations we leverage in our research design, while ensuring that the youngest cohort will have meaningful labor market activity in our contemporary IRS data (individuals born in 1996 were 23 in tax year 2019).

To measure prenatal exposure to ambient air pollution, we utilize the most detailed geographic information available. The pre-1989 Form 1040 data housed at the Census Bureau contains information on the exact address of parents when they filed their tax returns (street address, city, state and zip code). We first attempt to geocode these addresses to the Census tract level using the Master Address File IDs (MAFIDs) assigned to the 1040s. However, not all cases can be assigned a MAFID, so we additionally use the zip code information in the Form 1040 data to locate individuals (either to assign them to a zipcode tabulation area (ZCTA), or a county). This provides three potential levels of geography to assign pollution exposure: Census tract, ZCTA, or county. We focus on the county level results to be consistent with the descriptive evidence, and present results using alternative exposure definitions in sensitivity analysis.

We measure economic outcomes primarily through income information available in IRS data. We focus on two measures of income: total annual earnings (including deferred compensation) from Form W-2, and adjusted gross income from Form 1040. As we have multiple endpoint observations for individuals (annually from 2016-2019), we create a stacked dataset, with each row corresponding to a year in which income is earned. This will allow us to control for year-of-birth by year-of-income unobservables, accounting for lifecycle earnings patterns (since individuals affected born after the nonattainment designations will always be younger than those born before). We adjust all income amounts to 2012 dollars, which allows for easy direct comparisons with Chetty et al. (2018a) and Isen et al. (2017).

3.2 Research Design

Exposure to air pollution is correlated with many observable and unobservable characteristics that are also correlated with long-run economic and social outcomes. To identify the causal effect of prenatal pollution exposure we need to identify exogenous variation. We do this by exploiting plausibly exogenous, regulation-induced variation in prenatal $PM_{2.5}$ exposure. Specifically, we exploit the introduction of new regulatory particulate matter standards that affected some counties, but not others, following the introduction of the 1990 Clean Air Act Amendments. This style of research design builds on a well-established literature (Chay and Greenstone, 2003a; Isen et al., 2017; Voorheis, 2017; Currie et al., 2020; Colmer and Voorheis, 2021).

The Clean Air Act was first implemented in 1963, but limited federal oversight of state efforts led to disappointing results. It wasn't until Congress enacted the Clean Air Act Amendments of 1970 and established the EPA, dramatically increasing federal powers to address air pollution, that the regulation started to have an effect. The 1970 Amendments relied on "command and control" regulations, using criteria that focused on the health benefits of cleaner air without consideration of the economic costs. The legislation was instigated through the national ambient air quality standards (NAAQS), which set the maximum allowable levels of "criteria air pollutants" – sulfur dioxide, carbon monoxide, nitrogen dixoide, lead, particulates, and ozone. Based on these standards the EPA determines the set of counties that are in "nonattainment". The consequences of nonattainment are severe. State governments have to implement a pollutant-specific plan describing how nonattainment counties will be brought into compliance. If a state does not act or develops an inadequate plan, then federal funding for the state air pollution control program, highway construction, and sewage treatment plants can be withheld. The EPA can also ban permits required for new or modified constructions that could source pollution, or impose its own federal plan on nonattainment counties. These powers are sufficiently broad that even the threat of regulatory action has been associated with reductions in pollution Keohane et al. (2009).

Since the 1970 amendments, there have been several other major amendments, alongside hundreds of additional policy designations as scientific consensus about the harms of pollution and feasible compliance technologies have evolved. Our focus is on the 1990 Clean Air Act Amendments, which updated the national ambient air quality standards, broadened the enforcement powers of the EPA, and created new market-based mechanisms, such as the sulfur dioxide allowance-trading program to address acid rain. The 1990 amendments also resulted in the regulation of "toxic" air pollutants. 189 hazardous air pollutants were identified and emission standards were implemented that provided "an ample margin of safety to protect publish health," by minimizing the amount of toxic pollution that was released into the air.

Our identifying variation comes from the updating of the NAAQS standards, which affects some counties but not others through nonattainment designations.[5] New standards were introduced for particulates smaller than 10 microns (PM_{10}) and for nitrogen oxides (NO_x). Note that these standards did not directly target the fine particulates measured in

[5]The other changes that arose from the 1990 CAAA were common across all counties.

our data ($PM_{2.5}$). Rather, these regulations affected all particles smaller than 10 microns and NO_x an important precursor to the formation of fine particles (NO_x reacts with other atmospheric chemicals to create fine particulates). The introduction of these new standards resulted in new counties falling into nonattainment, providing regulation-induced variation in particulate matter exposure.

We estimate the effect of these new nonattainment designations on prenatal $PM_{2.5}$ exposure using a difference-in-differences research design. We define an indicator variable to be equal to one if an individual's county of birth becomes subject to the new nonattainment designations (zero otherwise) and interact this with an indicator variable each cohort.[6] Treated individual's are those that were conceived in nonattainment counties following the introduction of the 1990 CAA.

We estimate the following specification,

$$\begin{aligned} PM2.5_{i,c,s,m,t} &= \alpha_1(Nonattainment_{c,1990} \times \mathbb{1}[t > 1991]) \\ &\quad + \alpha_c + \alpha_{s,t} + \alpha_m + \alpha_{t,y} + \phi X_i' + \delta X_c' t + \nu_{i,c,s,t} \end{aligned} \tag{1}$$

where i indexes each individual, c indexes the county of birth, s indexes the state of birth, m indexes the month of birth, and t indexes the year of birth, i.e., the cohort.

Prenatal exposure to $PM_{2.5}$ is measured for each individual i, where $PM2.5_{i,c,s,m,t}$ is the average particulate matter concentration that individual i was exposed to in county of birth c in month m and year t. $PM_{2.5}$ is measured in $\mu g/m^3$. We regress this measure of exposure on a time-invariant county indicator equal to 1 if a county is designated in nonattainment of the updated 1990 PM_{10} and NO_x standards, $Nonattainment_{c,1990}$, and interact this term with an indicator equal to 1 for the years after the 1990 CAA amendments went into affect, $1[t > 1991]$. The interaction term is therefore equal to 1 for individuals born in nonattainment counties following the implementation of the 1990 CAAA. The parameter of interest is α_1, which under the assumption of parallel trends and non-interference, provides an estimate of the average treatment effect on the treated for nonattainment designation on prenatal TSP exposure in the years after CAAA regulations went into effect. We include county-of-birth fixed effects to control for time-invariant unobserved determinants of prenatal pollution exposure and state-of-birth $\times$ year fixed effects to control for time-varying determinants of prenatal pollution exposure that are common across all individuals born in state s in year t. We also include month-of-year fixed effects to control for seasonality in exposure.

[6]We observe that nonattainment counties are either in nonattainment of the PM_{10} standard or both the PM_{10} and NO_x standard.

Year-of-birth by tax year fixed effects, $\alpha_{t,y}$ are included to account for lifecycle earnings effects in the second-stage of our analysis, these fixed effects have no effect in the first-stage analysis, but are stated here to be consistent across the first-stage and second-stage empirical specification. Following the existing nonattainment designation literature we also include additional controls: X_j' is a vector of individual characteristics, including age, race, and sex, as well as prenatal exposure to temperature and rainfall. $X_c' t$ is a vector of county-level characteristics, measured in 1980, interacted with linear and quadratic time trends. Across all specifications we cluster our standard errors by the an individual's county of birth—the level at which we measure exposure.

Consistent with previous research exploring the 1970 and 2005 Clean Air Act Amendments we show that prenatal exposure to the new nonattainment designations is associated with substantial and persistent reductions in prenatal $PM_{2.5}$ exposure. Following the introduction of the 1990 CAA we estimate that prenatal exposure to $PM_{2.5}$ concentrations in nonattainment counties fell by 1.32 $\mu g/m^3$ (Table 1). This reduction is similar in magnitude to the declines in prenatal TSP exposure following the 1970 Clean Air Act Amendments.[7] Further, we note that the nonattainment designations did not affect ground level Ozone in regressions using modelled O_3 data from Kim et al. (2020), as shown in column 5 of Table 1, and had only marginal effects on ground level NO_2 (column 4). This is consistent with the overlapping nonattainment designations reducing nitrate or ammonium particulates via precursor chemicals in a way that did not affect other fates of these precursors (e.g. combining with VOCs to form ozone).

Figure 6 presents cohort-specific estimates from a distributed-lag model. We see that before the new regulations, individuals in nonattainment counties were not differentially exposed to $PM_{2.5}$, providing support for the parallel trends assumption. Following the implementation of the 1990 CAA, we estimate a sharp and persistent drop in prenatal $PM_{2.5}$ exposure. The reductions are driven by counties that are in non-attainment of both the PM_{10} and NO_x standard. This does not mean that the PM_{10} nonattainment by itself wasn't effective, just that it wasn't targeted to reduce levels of $PM_{2.5}$, a more granular measure of particulates.

We use this plausibly exogenous variation as an instrument to identify the effects of prenatal $PM_{2.5}$ exposure on later-life economic outcomes. We estimate the following specification,

[7] TSP concentrations fell in nonattainment counties by $\approx$10 $\mu g/m^3$. The crude ratio between $PM_{2.5}$ and TSP is 0.22.

$$
\begin{aligned}
Y_{i,c,s,m,t,y} &= \beta\widehat{PM2.5}_{i,c,s,m,t} \\
&\quad +\alpha_c + \alpha_{s,t} + \alpha_m + \alpha_{t,y} + \phi X_i' + \delta X_c' t + \epsilon_{i,c,s,m,t,y}
\end{aligned} \tag{2}
$$

We consider three main outcomes: 1) individual labor market earnings as measured on form W-2; 2) tax unit adjusted dross income (AGI, which we will abuse notation and refer to as family income) as measured by form 1040; and 3) a measure of upward economic mobility – the difference in AGI ranks between an individual around age 30 and their parent (at the time of the individual's birth).

We have shown that the first-stage is relevant and that the relationship between nonattainment and $PM_{2.5}$ exposure is plausibly identified, assuming parallel trends. If we assume that the exclusion restriction holds, the coefficient of interest, β, identifies the effect of a one-unit increase in CAAA-driven prenatal $PM_{2.5}$ exposure on later-life earnings.

The exclusion restriction assumption – that the 1990 CAAA only affected later-life outcomes through reductions in prenatal $PM_{2.5}$ exposure may not hold. It is possible that nonattainment designations affected outcomes in ways other than the estimated reductions in pollution. Isen et al. (2017) and Colmer and Voorheis (2021) make the point that nonattainment designations could affect economic competitiveness (Greenstone, 2002; Greenstone et al., 2012; Walker, 2011, 2013). However, existing evidence suggests that the effects on the broader local economy are small, affecting less than 0.7 percent of the total workforce (Walker, 2013). By contrast, the reduction in pollution benefited everyone in non-attainment counties. While we can't rule out that the 1990 CAAA contributed to a decline in economic conditions in nonattainment counties, we are able to control for parental Adjusted Gross Income in the year of birth. To the degree that this is insufficient we argue that since effects on competitiveness would be expected to have the opposite effect on health to reductions in pollution exposure, it is plausible that the 2SLS estimates understate the effects of prenatal $PM_{2.5}$ exposure. The reduced form effect of nonattainment remains valid and is interpreted as the net effect of the nonattainment designations on later-life outcomes. Our reduced form and corresponding 2SLS estimates produce very similar results, suggesting that violations of the exclusion restriction are unlikely to be a first-order concern. Another possible violation of the exclusion restriction, is that $PM_{2.5}$ is also correlated with other pollutants that also affect health and development. In this case our estimates reflect the compound effect of the correlated pollutants, rather than just $PM_{2.5}$. While we can't rule out this concern, Table 1 shows that the effect of nonattainment on $PM_{2.5}$ is driven by the combination of PM_{10} and NO_X nonattainment designations (column 1). NO_X is an important precursor for $PM_{2.5}$ but is less important in shaping PM_{10}. Consistent with this we do not estimate a statistically significant effect of the additional NO_X designation on PM_{10} concentrations, despite the fact that PM_{10} and $PM_{2.5}$ concentrations are likely to be strongly correlated (column 2). In addition, we do not estimate meaningful effects of these nonattainment designations on other pollutants, such as NO_2 (column 5) and Ozone (column 6).

3.3 Results

Table 2 summarises our estimates of the effect of regulation-induced decreases in $PM_{2.5}$ on later-life economic outcomes. In column 1 we see that a 1 $\mu g/m^3$ reduction in prenatal $PM_{2.5}$ exposure is associated with a \$1,105 increase in later life W-2 earnings; the reduced form effect of prenatal exposure to nonattainment is associated with a \$1,553 increase in later life W-2 earnings.[8] In column 2 we observe a similar estimate for the relationship between prenatal $PM_{2.5}$ exposure and later-life AGI, however, it is less precisely estimated – a 1 $\mu g/m^3$ reduction in prenatal $PM_{2.5}$ exposure is associated with a \$1,313 reduction in annual AGI. Column 3, presents the association between prenatal $PM_{2.5}$ exposure and our measure of upward mobility. We estimate that a 1 $\mu g/m^3$ reduction in prenatal $PM_{2.5}$ exposure is associated with a 1.28 rank point increase in upward mobility, approximately one-tenth of the size of the black-white mobility gap in Chetty et al. (2018b).

Our results are quantitatively and qualitatively robust to a large array of sensitivity analyses, including changes to the spatial resolution of pollution exposure (Table A1) and to alternative transformations of the outcome variables (Table A2). We also present cohort-specific estimates of the reduced form relationship. As with the first stage distributed-lag estimates, there are no statistically significant or economically meaningful differences between individuals born in treatment and control counties before the nonattainment designations went into effect, providing additional support for the parallel trends assumption. Consistent with the overall post-treatment estimates presented in Table 2, we see that all cohorts born in nonattainment counties following the introduction of the 1990 CAAA have higher later-life earnings, relative to those born in attainment counties. We observe a similar pattern for our cohort-specific estimates of nonattainment on AGI (Panel c of Figure 6) and upward mobility (Panel d of Figure 6). Consistent, with the existing literature on the Clean Air Act, cohort-specific estimates are less precisely estimated (Isen et al., 2017; Colmer and Voorheis, 2021).

Our estimated effects are substantially larger than previous estimates of the long-term

[8]The first-stage estimate predicts a 1.383 $\mu g/m^3$ reduction in $PM_{2.5}$, which combined with our second-stage estimate would predict a \$1,528 effect of pollution reductions from nonattainment. This suggests that any violations of the exclusion restriction are unlikely to be a first-order concern.

effect of prenatal pollution exposure. Isen et al. (2017) estimate that a 10 $\mu g/m^3$ reduction in Total Suspended Particulates, induced by the 1970 Clean Air Act Amendments was associated with a \$352 increase in earnings. Total Suspended Particulates – defined as the total mass of particles smaller than 100 microns – are much coarser than $PM_{2.5}$. Consequently, we need to re-scale existing estimates to make a proper comparison. Using all EPA monitor observations from monitor sites that had co-located active $PM_{2.5}$ and TSP monitors, we calculate a crude scaling factor between TSP concentrations and $PM_{2.5}$ concentrations as 4.35. A 10 $\mu g/m^3$ reduction in TSP corresponds to a 2.29 $\mu g/m^3$ decrease in $PM_{2.5}$. As such, the Isen et al. (2017) estimate is consistent with a \$153.60 increase in earnings per $\mu g/m^3$ of $PM_{2.5}$.[9] Our baseline estimate on W-2 earnings is 7 times larger.

There are a number of plausible origins for the increase in magnitude. We argue that our estimates differ due to differences in the policy variation used — the EPA's regulations after the 1990 Clean Air Act focus on finer particulates than the regulations after the 1970 Clean Air Act. Since finer particles are more damaging to health, the 1990s nonattainment designations may have had a much larger effect on health than the 1970s nonattainment designations. While the crude reduction in particles is similar across the two policies, the actual reduction in $PM_{2.5}$ from the 1990 CAAA is likely much larger than the reductions in 1970 as it would have been easier and lower cost to reduce coarser particulates. One possible confounder to this interpretation is that we use a different assignment of place of birth to Isen et al. (2017) and Colmer and Voorheis (2021) — we use information on the location an individual's parent filed taxes rather than the place of birth reported to the Social Security Administration. We believe that using tax data locations may more accurately capture exposure, since SSA locations may correspond to the hospital a child was born in rather than their residence. Any classical measurement error in exposure will have attenuated previous estimates. In Table A3 we show that, if anything, estimates are larger when using the Numident place of birth. Another possibility is that our data on exposure is different from the previous literature. As noted earlier, the satellite-derived data product performs similarly to the ground-based monitors in areas where the monitor network has coverage. Importantly, however, the satellite derived data product allows us to observe exposure for all counties, including those not monitored. This in turn means that our sample is closer to being nationally representative (since it includes individuals born in all counties, not a selected sample born in monitored counties). Comparisons between column 1 and column 3 of Table A3 show that our results are similar in magnitude when we restrict to monitor counties. In addition, we continue to estimate larger effects than Isen et al. (2017) and Colmer and Voorheis (2021) when using monitor data on PM_{10} concentrations (Table A3, columns

[9] \$351.74/2.29 = \$153.60.

5 and 6). The estimates on PM_{10} are in between our estimates using $PM_{2.5}$ concentrations and the Isen et al. (2017) estimates using total suspended particles, further supporting the claim that differences in our estimates arise from the reduction in smaller, more damaging particulates. Finally, we note that our results are robust to omitting individuals in counties directly neighboring nonattainment counties (see Table A4), which suggests spillovers do not play an meaningful role in driving our results.

3.4 Age-Specific Estimates of the $PM_{2.5}$-Earnings Relationship

Thus far, all our results have focused on the effect of exposure to particulate matter in utero as a determinant of later life outcomes. In our research design, we operationalize this by comparing individuals who received one additional year of cleaner air at age 0 (children born in nonattainment counties) to those who did not receive this improvement (children born in counties not in nonattainment). However, given our rich data, it is also possible to examine the impacts of exposure to pollution at later ages on later life outcomes. To do this, we compare children living in nonattainment counties at a given age to those in attainment counties, isolating the effect of an additional year of clean air *at age 1,2,3...*. To do this, we modify our research design to estimate the following equation:

$$\begin{aligned} Y_{i,c,s,m,t_k,y} &= \beta_k \widehat{PM2.5}_{i,c,s,m,t_k} \\ &\quad + \alpha_c + \alpha_{s,t_k} + \alpha_m + \alpha_{t,y} + \phi X_i' + \delta X_c' t_k + \epsilon_{i,c,s,m,t_k,y} \end{aligned} \tag{3}$$

which is now indexed by t_k, the year in which a child i turns age k.

We estimate these IV regressions for each age 1,2,....12, and trace out the age-specific effects (β^k) of pollution exposure on later life earnings. These age specific effects are shown graphically in Figure 5, along with the age 0 (in utero) effect from Table 2. As with the baseline results, the age-specific results measure $PM_{2.5}$ exposure at the county level. Exposure to an additional 1 $\mu g/m^3$ of $PM_{2.5}$ has slighter smaller effects on adult earnings than exposure in utero through age 4, after which estimated effect sizes decrease dramatically. By age 8, we do not estimate any meaningful effect of $PM_{2.5}$ on adult earnings, with a high degree of precision. This pattern provides direct evidence on the conventional wisdom that exposure to environmental hazards in early childhood is of particular importance.

4 Exploring the Contribution of Air Pollution to Economic Opportunity

To better understand how much broader patterns of economic opportunity are explained by variation in air quality, we engage in three quantitative thought experiments. First, we combine our causal estimates of the effect of early life $PM_{2.5}$ exposure on earnings and economic mobility with observed patterns of individual-level pollution and economic disparities. The objective of this exercise is to calculate how much early-life pollution exposure can account for racial earnings gaps. Second, we leverage plausibly exogenous variation in early life pollution exposure, arising from a mover's design – exploiting differences in the age in which children moved – to explore how much of the overall "neighborhood earnings effect" could be accounted for by air quality during childhood. Finally, we exploit exogenous variation in early pollution exposure from the Moving to Opportunity randomized experiment, comparing those that move to those that don't, as an alternative lens through which to calculate how much of the overall "neighborhood earnings effect" could be accounted for by air quality in early childhood

4.1 How Much Does Prenatal Pollution Exposure Contribute to Black-White Earnings Gaps?

Our first analysis combines our estimates of the long-run economic effects of prenatal pollution exposure with cohort-specific disparities in $PM_{2.5}$ exposure. With these measures, we provide an estimate of the role that disparities in air quality at birth play in contributing to later-life economic disparities. Specifically, we consider how much racial gaps in pollution exposure at birth contribute to contemporary gaps in the level of income.

We use our linked dataset to estimate the cohort-specific Black-White and and Hispanic-White gaps in $PM_{2.5}$ exposure at birth – for each cohort between 1981 and 2016. The Black-White prenatal $PM_{2.5}$ gap has fallen from 2.91 $\mu g/m^3$ in 1981 to 1.48 $\mu g/m^3$ in 2016 (Figure 6a). The Hispanic-White prenatal $PM_{2.5}$ gap has fallen from 1.43 $\mu g/m^3$ in 1981 to 0.72 $\mu g/m^3$ in 2016 (Figure 6b); however, unlike the Black-White gap, which shows a stable decline over time, the Hispanic-White gap has fluctuated a lot more across cohorts and does not appear to follow a declining trend.

We then use our linked dataset to calculate the cohort-specific racial earnings gaps at age 30 using Form W-2 data.[10] We calculate that the Black-White earnings gap has steadily

[10]We average all non-missing annual W-2 observations for an individual for the year in which they turned 30.

increased from \$11,277 for the 1981 cohort to 12,107 for the 1989 cohort (Figure 6d). We calculate that the Hispanic-White earnings gap has decreased from \$6,871 for the 1981 cohort to \$6,303 for the 1989 cohort (Figure 6d).

Using our central estimate of the effect of $PM_{2.5}$ exposure on earnings, assuming constant marginal damages for each race group and cohort, we calculate that \$3,215 (29 percent) of the \$11,277 Black-White earnings gap and \$1,580 (18 percent) of the \$6,871 Hispanic-White income can be accounted for by $PM_{2.5}$ disparities at birth.[11] Across cohorts between 1981 and 1981, we can explain 20-28 percent of the Black-White earnings gap (Figure 6e) and 5-38 percent of the Hispanic-White earnings gap Figure (6f). The share of the Black-White earnings gap that can be accounted for by $PM_{2.5}$ disparities at birth has declined steadily over time – the earnings gap has increased over time. By contrast, the share of the Hispanic-White earnings gap that can be accounted for by $PM_{2.5}$ disparities at birth has remained more stable. In recent years, the Hispanic-White income gap has shrunk which, combined with a more stable Hispanic-White prental $PM_{2.5}$ gap, has led to a greater share of the income gap being allocated to prenatal $PM_{2.5}$.

We note caveats. These calculations combine non-marginal changes in pollution exposure with an out-of-sample estimate of the marginal pollution-earnings relationship. We also assume constant marginal damages, i.e., a linear dose response function. If the dose response function is convex, marginal damages will decrease as the pollution gap shrinks. In this case, our calculations will overstate the contribution of early life pollution exposure. If the dose response function is concave, marginal damages will increase as the pollution gap shrinks. In this case, our calculations will understate the contribution of early life pollution exposure. Given the size of the pollution gaps, we do not think that assuming linearity in the dose response function is plausible over this range. Existing evidence on the shape of the dose response function based on credible research designs has not uncovered strong evidence of non-linearities.

4.2 How Much Does Air Pollution Contribute to the Effect of "Neighborhood" on Earnings?

In addition to providing evidence on the contribution of air pollution to contemporary racial economic disparities, we also explore the contribution of air pollution to the overall effect of "neighborhood" on later-life earnings. Unlike our analysis of racial disparities, which exploited descriptive differences in exposure, we exploit quasi-experimental and experimental

[11] If we use the upper and lower bounds of the 95 percent confidence interval for our earnings estimate, we can account for between \$401 (3.6 percent) and \$6,026 (53 percent) of the Black-white earnings gap and between \$197 (2.87 percent) and \$2,691 (43 percent) of the Hispanic-White earnings gap.

variation in pollution exposure, resulting from movements between neighborhoods. This allows us to combine marginal changes in pollution exposure with our marginal estimate of the pollution-earnings relationship.

4.2.1 Evidence from a Mover Design

Our first approach combines our age-specific estimate of the pollution-earnings relationship with mover design inspired by Chetty and Hendren (2018a). This approach exploits variation in the timing of children who moved to better/worse neighborhoods at different ages. Approaches that compare individuals that move to those that don't face strong identification assumptions given selection into moving. By contrast, the mover design relies on a less stringent identification assumption. Instead of having to assume away selection effects, we instead have to assume that any selection effects associated with moving don't vary with the age of the child when the family moved. Chetty and Hendren (2018a) and others have provided evidence to support this assumption.

To calculate the contribution of air quality to the overall effect of neighborhood on earnings we need three components. First, we need to estimate the age-specific relationship between predicted neighborhood mobility and earnings. Second, we need to estimate the relationship between predicted neighborhood mobility and $PM_{2.5}$ concentrations, providing the expected change in pollution from a change in neighborhood mobility. Finally, we need age-specific estimates of the relationship between $PM_{2.5}$ and later-life earnings, which we described in section 3.4.

Our approach mirrors Chetty and Hendren (2018a). Using linked IRS 1040 records, we track residential histories and income histories for individuals born between 1981 and 1995 from ages 0 through 24. We then extract three pieces of information: 1) individual family income and earnings at age 24, 2) county of birth, and 3) the first county of residence we observe for a child that differs from their county of birth through age 12. Using this information, we calculate cohort-specific income ranks in adulthood, and define two sets of children: permanent residents of a county (defined as individuals who still live in the their county of birth at age 12), and movers (individuals who move once between age 0 and 12).[12] Using these ranks, we calculate the average income rank for permanent residents of each county in the United States for each cohort. This allows us to construct a key measure: the predicted economic mobility of an origin-destination county pair, $\Delta_{ods} = \bar{r}_{ds} - \bar{r}_{os}$, where o indexes origin counties, d indexes destination counties, and s indexes cohorts. Following Chetty and Hendren (2018a), we take this to be a summary measure of the opportunity of

[12]We set 12 as the age defining permanent residents to match the age profile effects of pollution exposure, which we estimate through age 12.

a neighborhood.

The movers design estimates the relationship between neighborhood opportunity and later life outcomes by estimating the following regression for movers:

$$Earnings_i = \alpha_{qodsk} + \sum_{k=0}^{24} \gamma_k \Delta_{ods} + \epsilon_i \tag{4}$$

where i indexes individuals, q indexes parent income quintiles at birth, o and d index origin and destination counties, s indexes cohorts and k indexes age at first move. α_{qodsk} are parent income quintile-by-cohort-by-age-by-county pair fixed effects. Estimating this equation produces age specific effects of neighborhood opportunity on earnings in adulthood – the γ_k coefficients. Figure 7a visualizes these estimated coefficients for ages 0 through 24. Moving to a higher opportunity neighborhood has declining impacts on adult earnings through around age 12, after which effects are flat and close to zero.

To understand how much of these neighborhood opportunity effects might be driven by pollution exposure, we need to understand the relationship between neighborhood opportunity as measured here and fine particulate matter. Figure 7b presents a binned scatterplot and linear association between the average $PM_{2.5}$ difference between origin and destination counties for each cohort and the Δ_{ods} estimates from the movers design. A 1 percentile point increase in predicted neighborhood opportunity is associated with a 0.08 $\mu g/m^3$ decrease in $PM_{2.5}$.[13]

We combine estimates of these two parameters, with our estimates of the age-specific effect of $PM_{2.5}$ on adult earnings (β_k from equation 3, shown in Figure 5) to calculate the share of the movers design effect that can be attributed to $PM_{2.5}$ for each age group,

$$PM_{2.5}\ Share = \frac{\hat{\beta}^k \times \hat{\Delta} PM_{2.5}}{\hat{\gamma}^k} \tag{5}$$

Figure 7c presents this share fore each age between 0 and 12. For very young children, pollution exposure can account for as much as 50 percent of the overall neighborhood opportunity effect. After age 4, there are sharp declines in the fraction of neighborhood effects explained. None of the variation in neighborhood effects for children older than 8 can be attributed to pollution. If we average across all children through age 12, we can attribute

[13] We do not estimate a full mover's design specification with $PM_{2.5}$ as the outcome because there shouldn't be any meaningful difference in the level of $PM_{2.5}$ exposure for someone who moves earlier to someone who moves a bit later (other than through changes in trends. By using the cross-sectional relationship between predicted mobility and $PM_{2.5}$ we capture differences in exposure that arise from the number of years an individual is exposed to higher/lower pollution. Our estimates in Figure 5 indicate that air quality in early childhood matters more than later childhood.

20% of the movers design effects to pollution. In addition to highlighting the contribution of air pollution to the overall effect of neighborhood on earnings, this finding indicates that there should be differences in willingness-to-pay for different neighborhood amenities over the life-cycle.

4.2.2 Evidence from the Moving to Opportunity Experiment

Our second approach exploits experimental variation in pollution exposure from the Moving to Opportunity (MTO) randomized experiment. The MTO experiment was conducted by the U.S. Department of Housing and Urban Development (HUD) in the mid 1990s. The objective was to examine whether moving public housing recipients to lower poverty neighborhoods improved the economic and social outcomes of adults. Families were tracked over time, and HUD collected outcomes for both children and adults at the end of the experiment.

The MTO experiment randomized recipients into three groups: the treatment group received a voucher that could only be used in a low poverty neighborhood; the Section 8 group received a voucher that could be used anywhere; the control group did not receive a voucher.

Evaluations during and after the experiment found little evidence of improvements in the economic circumstances for the treatment groups (Kling et al., 2007; Sanbonmatsu L et al., 2011). However, more recent work documents that children in the treatment group who were younger than 13 when they moved experienced higher incomes as adults (Chetty et al., 2016).

We calculate the extent to which improvements in air quality may have contributed to this earnings effect. We do this by estimating whether voucher-induced movements resulted in lower exposure to $PM_{2.5}$ and combine estimates of the change in pollution with our estimates of the $PM_{2.5}$–earnings relationship.

Data We use data from HUD on the individuals that participated in the Moving to Opportunity experiment. We focus on those that were younger than 13 years old at time of randomization. Following Chetty et al. (2016), we restrict the sample to those older than 23 in tax years 2008 - 2012.

We identify demographic information, survey responses, and survey weights from the MTO Final Analysis dataset provided by HUD. We construct quarterly address history over the duration of the MTO experiment (1994 - 2010) for all participants using the MTO Final Evaluation Residential Address History dataset. This dataset provides the census tract that every MTO participant lived in during the experiment.

We merge the MTO participants' residential histories to the Census tract level measures of $PM_{2.5}$ concentrations discussed above. We define an individual's pollution exposure as the duration weighted average of each quarter's $PM_{2.5}$ exposure up to the age of 18, or calculate annual average pollution exposure for each year through age 18.

We also merge the MTO participants to income information from IRS tax data to measure their economic outcomes. We follow Chetty et al. (2016) and focus on two outcomes: individual earnings, which we measure using annual average wage income from Form W-2s, and tax unit level total income, which we measure as the adjusted gross income reported on form 1040. For comparability with previous literature, we measure this income information from the years 2008–2012.

Research Design We estimate OLS regressions of the form:

$$Y_{i,s} = \alpha + \beta_1 Exp_{i,s} + \beta_2 S8_{i,s} + \delta_s + \epsilon_{i,s} \tag{6}$$

where $Y_{i,s}$ denotes outcomes for individual i in randomization site s. The outcomes we focus on are time-weighted $PM_{2.5}$ pollution exposure, wage income, and adjusted gross income. Exp_i and $S8_i$ are whether the individual was assigned to the experimental or Section 8 groups and δ_s is a set of randomization site fixed effects. We weight regressions using the standard MTO final analysis weights, which adjust for differences in sampling probabilities across sites and over time. We cluster standard errors by family, the level at which randomization occurred.

Randomization site fixed effects account for inherent differences between the five randomization sites (Baltimore, Boston, Chicago, New York, and Los Angeles), which is particularly important in this context because of differing baseline pollution levels between cities.

β_1 and β_2, respectively, provide estimates of the association between being offered the experimental voucher or the Section 8 voucher and our outcomes of interest, relative to the control group. Because some families do not use the vouchers, the estimates capture the intent to treat effect.

MTO Results Table 3 presents estimates of the relationship between take-up of vouchers and income, for individuals whose families received the voucher before the age of 13. In column 1, we estimate that children whose parents were part of the experimental group have annual W-2 earnings that are \$2,790 higher than the control group. We estimate no statistically significant effects of assignment to the Section 8 group. These findings very closely match the estimates in Chetty et al. (2016).

In column 2, we turn to the effects of MTO randomization group assignment on adjusted gross income (AGI). Wage earnings are a component of AGI, though they come from different

IRS datasets. The AGI results match the W-2 results: children whose parents were part of the experimental group have annual AGI earnings that are \$4,298 higher than the control group. Likewise, we estimate no significant effect of Section 8 vouchers on annual AGI earnings.

In column 3, we estimate the relationship between take-up and post-treatment $PM_{2.5}$ exposure. We estimate that being offered the experimental voucher is associated with a 0.407 $\mu g/m^3$ reduction in $PM_{2.5}$ exposure, relative to the control group. This is a 3 percent reduction in exposure relative to the mean. Section 8 voucher recipients do not appear to experience significant reductions in exposure relative to the control group.

How Much of the MTO-Earnings Effect can be explained by MTO-induced reductions $PM_{2.5}$ Exposure? We have shown that receiving MTO low poverty vouchers reduced children's lifetime pollution exposure and increased earnings. The MTO experiment increased earnings by \$2,790 for children whose family were offered the voucher before the age of 13; and decreased exposure to $PM_{2.5}$ by 0.4 $\mu g/m3$. Combining our estimate of the reduction in pollution with the average effect of $PM_{2.5}$ on earnings between birth and age 12, we calculate that, on average, \$167 (6 percent) of the earnings effect can be accounted for by reductions in childhood $PM_{2.5}$ exposure.

5 Conclusion

We have shown, across datasets and research designs, that exposure to ambient air pollution is closely related to economic opportunity in the United States. We document that early life exposure to fine particulate matter is one of the top five predictors of intergenerational income mobility in the United States. We argue that this strong correlation, at least in part, reflects a causal relationship between air quality and economic opportunity. We provide evidence for this claim in two parts: first, we present new evidence that plausibly exogenous shocks to early life pollution exposure arising from nonattainment designations enacted following the 1990 Clean Air Act Amendments have large effects on later life economic outcomes. These estimates are much larger than existing estimates of the pollution-earnings relationship, which we argue is largely driven by the smaller particulates under study, which are more damaging to health than the larger particles studied in previous analyses (Isen et al., 2017; Colmer and Voorheis, 2021). Taking advantage of our rich longitudinal data, we also present new estimates on the pollution-earnings relationship from birth through to age 12, rather than focusing solely on prenatal exposure. We provide direct evidence that pollution exposure in early childhood is much more damaging than later childhood exposure.

Second, we combine our estimates of the pollution earnings relationship with individual-level information on race, residence, and later-life earnings to explore the degree to which differences in exposure to pollution by race and neighborhood contribute towards contemporary racial economic disparities and the overall effect of neighborhood on economic opportunity. We show that racial gaps in prenatal $PM_{2.5}$ exposure can account for a meaningful share of contemporary racial economic disparities and are a non-trivial contributor to the overall effect of neighborhood on later-life earnings. Collectively, our results suggest that exposure to environmental hazards in early childhood are an important determinant of later life economic opportunity in the United States.

These results underline the importance of understanding disparities in pollution exposure: environmental inequality exacerbates economic inequality. However, these results also provide hope. We know very little about how to reduce racial disparities and ensure economic opportunity. By contrast, evidence suggests that we have been very successful in reducing air pollution over the last few decades (Figure 6a, Colmer et al. (2020), and Currie et al. (2020)). Our findings provides evidence that improving air quality meaningfully improves economic opportunity and reduces contemporary racial disparities in the United States.

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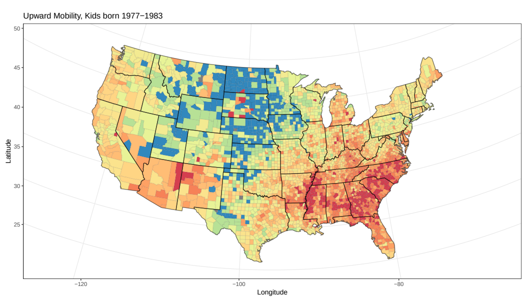
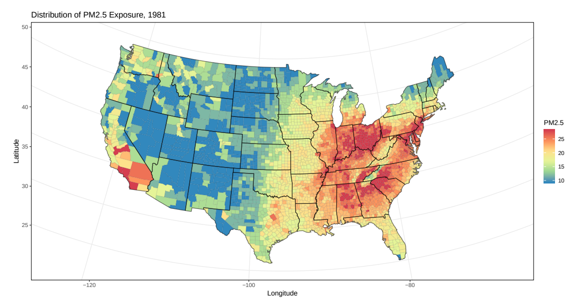
Tables and Figures

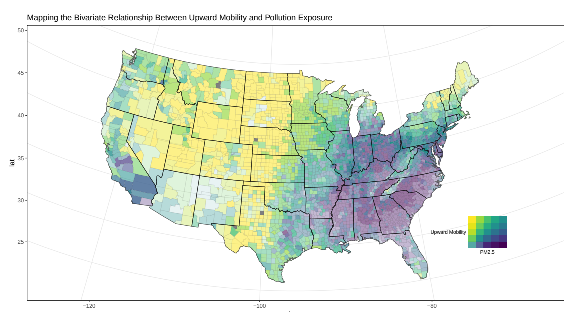
Figure 1: Spatial Variation in Upward Mobility and Environmental Quality



(a) Upward Mobility



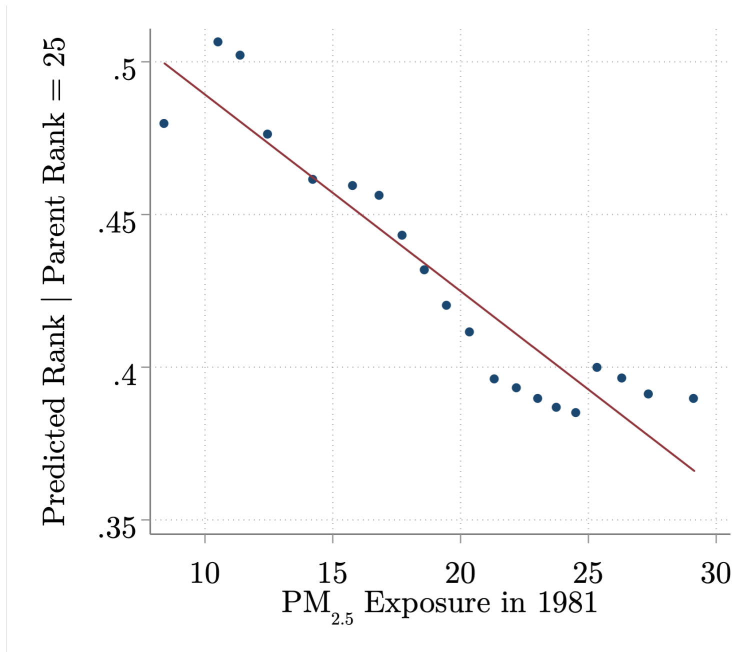
(b) $PM_{2.5}$



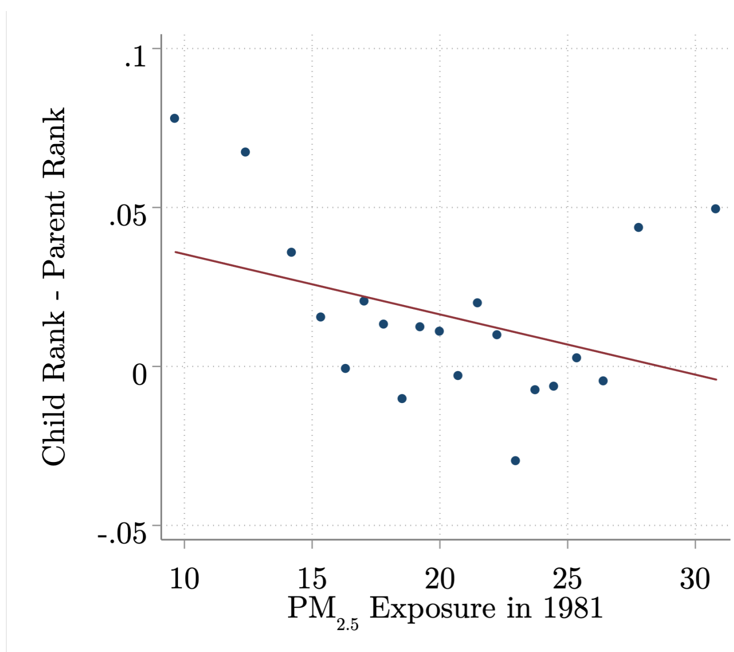
(c) Pollution-Mobility Matrix

Source: Author's calculations using data from Meng et al. (2019) and Chetty et al. (2014). The maps summarize the county-level distribution of upward mobility and pollution exposure. The top panel maps the county-level measures of upward mobility (the predicted rank for a child born to parents at the 25th percentile) from Chetty et al. (2014). The middle panel maps county-level annual average $PM_{2.5}$ concentrations in 1981. County-level averages are calculated by intersecting the gridded data from Meng et al. (2019) with Census tracts and then calculating a tract population weighted average for each county. The bottom panel maps the two county-level variables together using a bivariate color palette.

Figure 2: The Bivariate Relationship between early life $PM_{2.5}$ Exposure and Upward Mobility



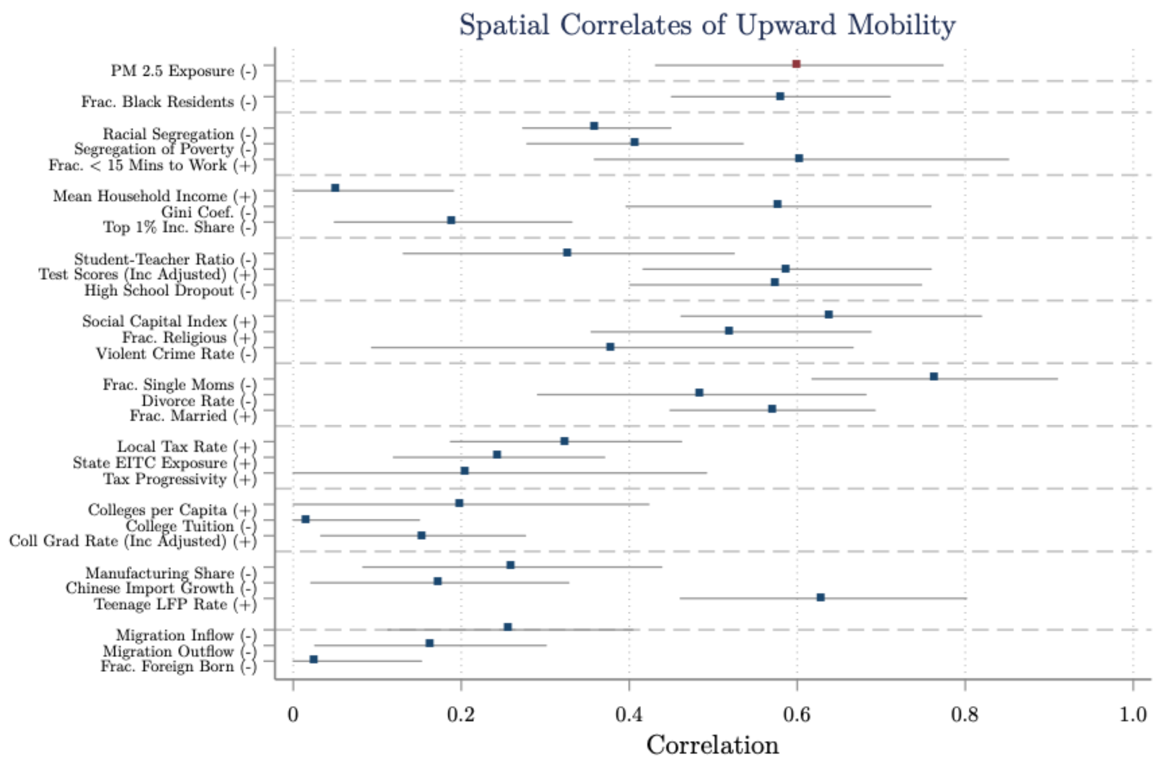
(a) County-level



(b) Individual-level

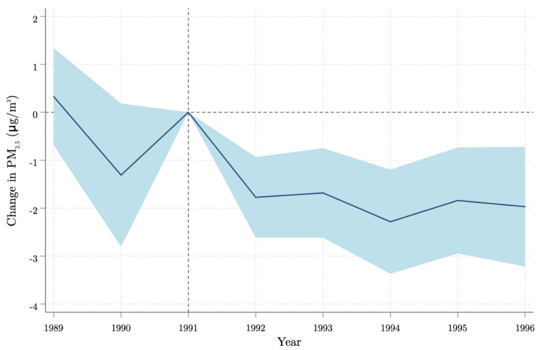
Source: Author's calculations using data from Meng et al. (2019), Chetty et al. (2014), IRS 1040s, ACS 2001-2019. Panel a) summarizes the bivariate relationship between county-level $PM_{2.5}$ and county-level predicted upward mobility (child rank - parent rank). Panel b) summarizes the bivariate relationship between individual-level $PM_{2.5}$ exposure compared to individual level upward mobility (child rank - parent rank). Each point reflects the average upward mobility and $PM_{2.5}$ within each vigintile bin of the $PM_{2.5}$ distribution. Error bars reflect the 95 percent confidence intervals calculated with robust standard errors clustered at the county of birth level.

Figure 3: The Relative Importance of $PM_{2.5}$ as a Correlate of Upward Mobility

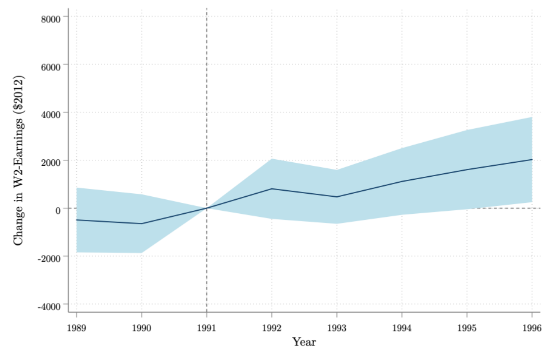


Source: Author's calculations using data from Meng et al. (2019) and Chetty et al. (2014). See figure 1 for more details. This figure shows bivariate correlations between county-level upward mobility and county-level $PM_{2.5}$, as well as correlations between upward mobility and other county-level characteristics from Chetty et al. (2014).

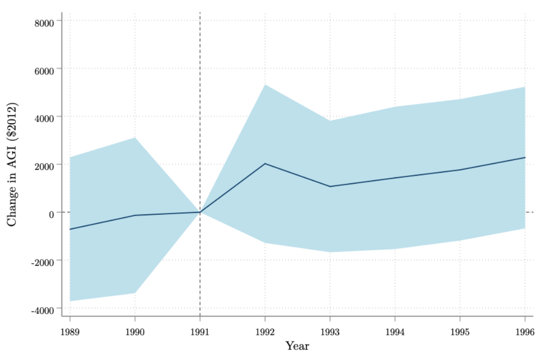
Figure 4: Cohort-Specific Estimates of the Relationship between Prenatal Nonattainment Exposure and Our Main Outcomes.



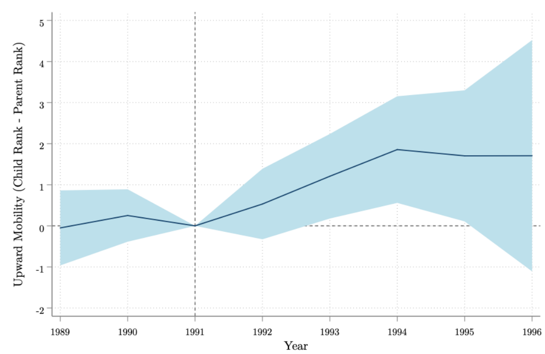
(a) Prenatal $PM_{2.5}$ Exposure



(b) W2 Earnings ($2012)



(c) AGI ($2012)



(d) Upward Mobility

Source: IRS 1040s, IRS W-2s, ACS 2001-2019, Census Numident and author's calculations using data from Meng et al. (2019). These figures present cohort-specific estimates of the association between prenatal exposure to nonattainment designations and our main outcomes of interest. Panel a) presents estimates of the association between prenatal exposure to nonattainment and prenatal $PM_{2.5}$ exposure. This is the first-stage of our analysis. Panel b) presents estimates of the association between prenatal exposure to nonattainment and later-life W2 earnings. Panel c) presents estimates of the association between prenatal exposure to nonattainment and later-life AGI. Panel d) presents estimates of the association between prenatal exposure to nonattainment and later-life upward mobility, measured as the difference in AGI income rank between children and their parents. Error bars reflect the 95 percent confidence intervals calculated with robust standard errors clustered at the county of birth level.

Table 1: The Association between Prenatal Nonattainment Exposure and Pollution Exposure

	(1) $PM_{2.5}$ (at birth)	(2) PM_{10} (at birth)	(3) $PM_{2.5}$ (at age 18)	(5) NO_2 (at birth)	(5) O_3 (at birth)
PM10 Nonattainment	-0.1542	-3.281***	0.084***	-0.1863	-0.4509
	(0.1199)	(0.7765)	(0.02)	(0.2061)	(0.3743)
PM10 and NOx Nonattainment	-1.383***	-2.983	0.058	-0.8837*	-1.038
	(0.3375)	(2.511)	(0.0566)	(0.4651)	(0.8248)
Fixed Effects	Birth County, Birth-State × Year, Birth Month				
Individual Controls	Yes	Yes	Yes	Yes	Yes
County-level Controls	Yes	Yes	Yes	Yes	Yes
Observations	3,570,000	2,278,000	3,118,000	3,570,000	3,570,000
First Stage F-Stat	9.74	10.43	–	–	–

Notes: Significance levels are indicated as * 0.10 ** 0.05 *** 0.01. Source: Source: IRS 1040s, IRS W-2s, ACS 2001-2019, Census Numident and author's calculations using data from Meng et al. (2019). This table shows the first stage effect of prenatal exposure to nonattainment PM_{10} and NO_x designations on $PM_{2.5}$ exposure at birth (column 1), PM_{10} exposure at birth, using a restricted sample of monitor counties (column 2), and $PM_{2.5}$ exposure measured at age 18 (column 3). Columns 4 and 5 use CACES modelled data to do a falsification test for whether the nonattainment designation affected other pollutants (NO_2 and O_3).

Table 2: The Association Between Prenatal $PM_{2.5}$ and Adult Economic Outcomes

	(1) W-2 Earnings	(2) AGI	(3) Upward Mobility
Panel A: IV			
$PM_{2.5}$ ($\mu g/m^3$)	-1105**	-1313*	-0.0128**
	(493.2)	(693.4)	(0.005855)
Panel B: Reduced Form			
Nonattainment × Post	1553***	1922**	0.01103**
	(531.9)	(868.8)	(0.005443)
Fixed Effects	Birth County, Birth Year × Tax Year, Birth State × Birth Year, Birth Month		
Individual Controls	Yes	Yes	Yes
County-level Controls	Yes	Yes	Yes
Observations	10,610,000	13,710,000	13,710,000
Control Mean	$25,490	$35,340	0.66
First Stage F-Stat	9.69	9.74	9.74

Notes: Significance levels are indicated as * 0.10 ** 0.05 *** 0.01. Source: Source: IRS 1040s, IRS W-2s, ACS 2001-2019, Census Numident and author's calculations using data from Meng et al. (2019). This table shows the second stage effect of $PM_{2.5}$ on earnings, AGI and upward mobility in panel A, and the reduced form effect of nonattainment PM_{10} and NO_x designations on on earnings, AGI and upward mobility in panel B. Column 1 uses a sample consisting of individuals born between 1989-1996 who have W-2 earnings between 2016-2019. Columns 2 and 3 use a sample consisting of individuals born between 1989-1996 who are a primary or secondary 1040 filer between 2016-2019. Upward mobility in column 3 is defined as the child's AGI rank in 2016-2019 subtracted from their parent's AGI rank in their year of birth.

Figure 5: Age Specific Effects of $PM_{2.5}$ on Adult Earnings

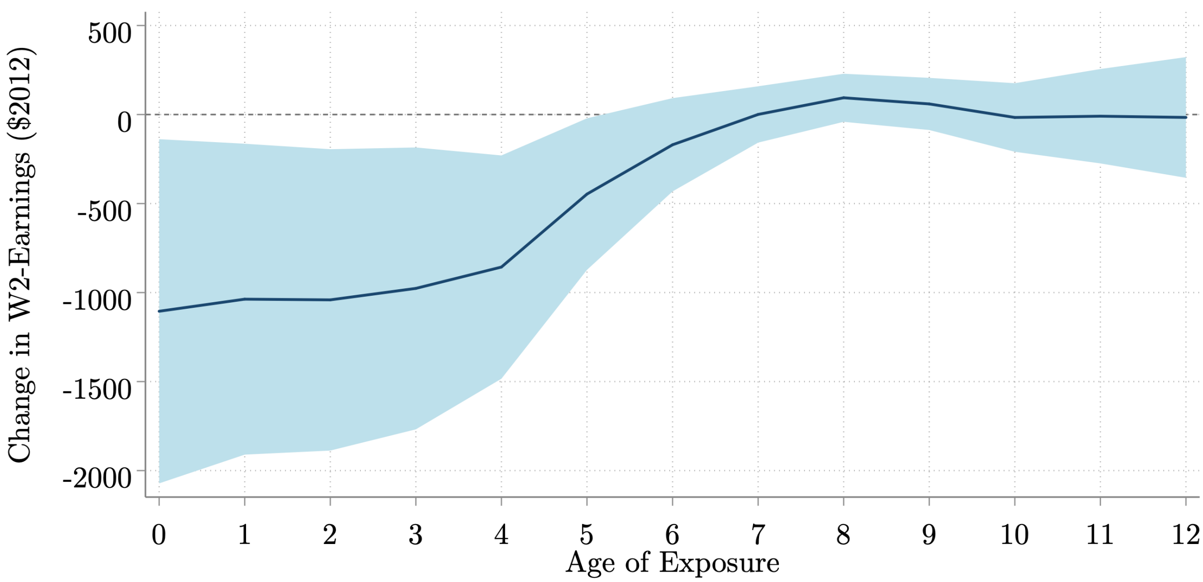
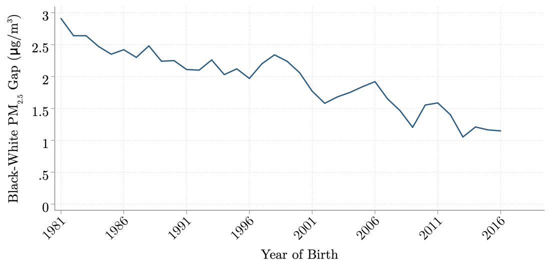
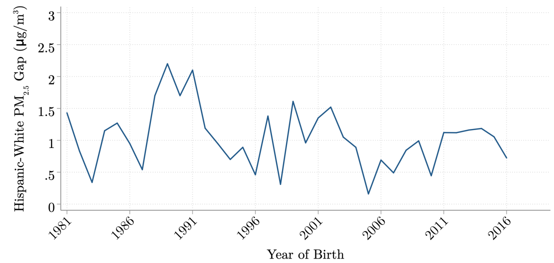


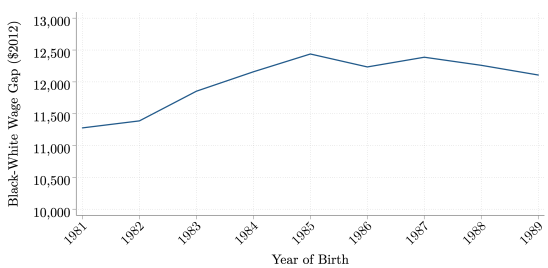
Figure 6: Cohort-Specific Estimates of $PM_{2.5}$ Gaps, Earnings Gaps, and the Share of Earnings Gaps that can be Explained by $PM_{2.5}$ Disparities at Birth.



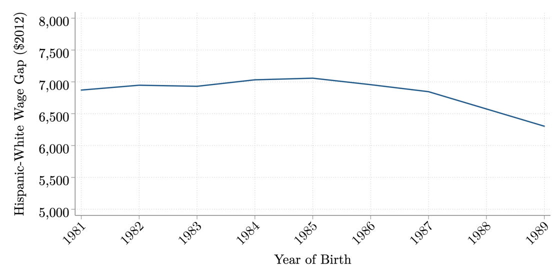
(a) Black-White Prenatal $PM_{2.5}$ Gap



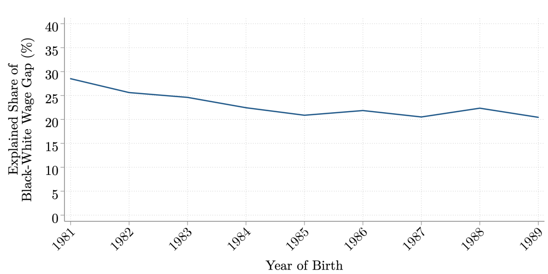
(b) Hispanic-White Prenatal $PM_{2.5}$ Gap



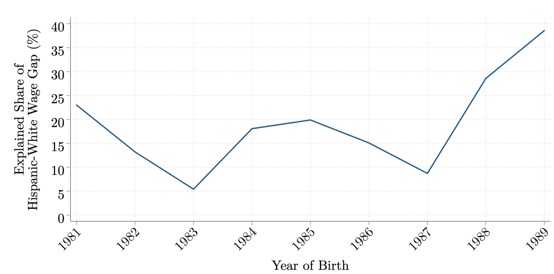
(c) Black-White Income Gap ($2012)



(d) Hispanic-White Income Gap ($2012)



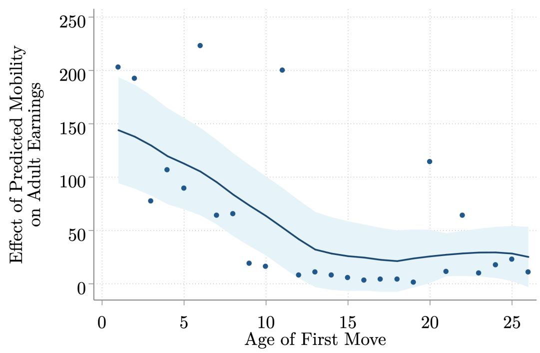
(e) $PM_{2.5}$ Share of Black-White Income Gap (%)



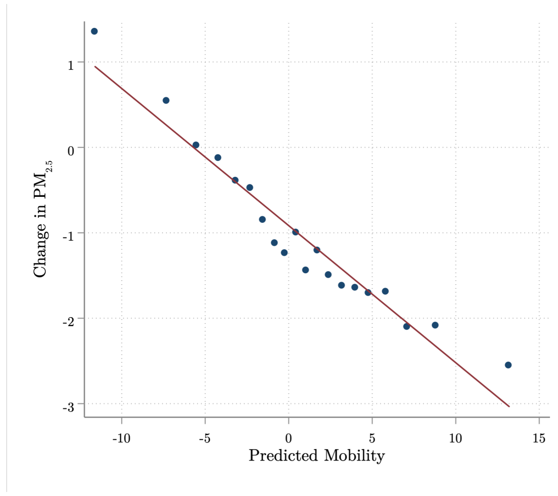
(f) $PM_{2.5}$ Share of Hispanic-White Income Gap (%)

Source: IRS 1040s, IRS W-2s, ACS 2001-2019, Census Numident and author's calculations using data from Meng et al. (2019). Panel a) presents the Black-White gap in prenatal $PM_{2.5}$ concentrations for each birth cohort between 1981 and 2016. Panel b) presents the Hispanic-White gap in prenatal $PM_{2.5}$ concentrations for each birth cohort between 1981 and 2016. Panel c) presents the Black-White Income gap at age 30 in 2012 dollars for each birth cohort between 1981 and 1989. Panel d) presents the Hispanic-White earnings gap at age 30 in 2012 dollars for each birth cohort between 1981 and 1989. Panel e) presents the share of the Black-White earnings gap that can be accounted for by combining the Black-White prenatal $PM_{2.5}$ gap and our central estimate of the relationship between prenatal $PM_{2.5}$ exposure and later-life earnings around age 30. Panel f) presents the share of the Hispanic-White earnings gap that can be accounted for by combining the Hispanic-White prenatal $PM_{2.5}$ gap and our central estimate of the relationship between prenatal $PM_{2.5}$ exposure and later-life earnings around age 30.

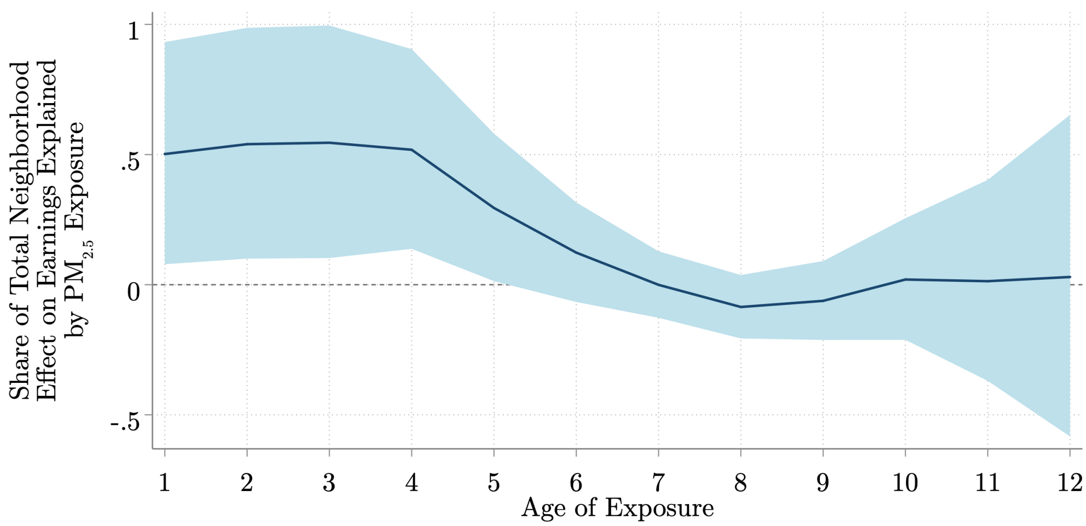
Figure 7: Movers Design Results



(a) Predicted Mobility–Earnings Effect



(b) Predicted Mobility–$PM_{2.5}$ Effect



(c) $PM_{2.5}$ Share of Neighborhood Mobility Effect on Earnings

Source: IRS 1040s, IRS W-2s, ACS 2001-2019, Census Numident and author's calculations using data from Meng et al. (2019). Panel a) presents the relationship between predicted neighborhood mobility and later-life earnings, by age of first move. Panel b) presents the relationship between predicted neighborhood mobility and neighborhood $PM_{2.5}$ concentrations. Panel c) presents the share of the mover design effect on earnings that can be attributed to $PM_{2.5}$ for each age group. This is the result of combining the estimates in panel a) and b) with our age-specific effects of $PM_{2.5}$ on later life earnings in Figure 5. This share is calculated for each age group using equation 5.

Table 3: The Association between MTO Treatment Take-Up, Earnings and $PM_{2.5}$ Exposure

	(1) W-2 Earnings	(2) 1040 AGI	(3) $PM_{2.5}$
Exp Group (TOT)	2790**	4298***	-0.407***
	(1346)	(1582)	(0.137)
S8 Group (TOT)	926.3	1718	0.0467
	(1069)	(1141)	(0.108)
Site FE	Yes	Yes	Yes
Observations	9,500	9,500	9500
Control Mean	$9,598	$11,760	14.83

Notes: Significance levels are indicated as * 0.10 ** 0.05 *** 0.01. Source: HUD MTO, IRS 1040s, IRS W-2s and Meng et al. (2019). Column 1 show effects on earnings as measured as the annual earnings across all Form W-2s, while column 2 shows the effects on AGI income on form 1040s. Column 3 shows the effects on $PM_{2.5}$ exposure.

Online Appendices – Not for Publication

A Additional Results and Robustness Tests

Table A1: Robustness Check: Alternate Spatial Resolutions

	(1) W-2 Earnings	(2) W-2 Earnings	(3) W-2 Earnings
$PM_{2.5}$ ($\mu g/m^3$)	-961.2***	-989.1**	-1105**
	(330)	(403.1)	(493.2)
Observations	8,945,000	10,610,000	10,610,000
First Stage F-Stat	19.47	14.36	9.69
	1040 AGI	1040 AGI	1040 AGI
$PM_{2.5}$ ($\mu g/m^3$)	-1274***	-1162**	-1313*
	(487)	(564.9)	(693.4)
Observations	11,520,000	13,710,000	13,710,000
First Stage F-Stat	19.26	14.44	9.74
Exposure Level	Tract	Zip Code	County
Fixed Effects	Birth County, Birth Year × Tax Year, Birth State × Birth Year, Birth Month		
Individual Controls	Yes	Yes	Yes
County-level Controls	Yes	Yes	Yes

Notes: Significance levels are indicated as * 0.10 ** 0.05 *** 0.01. Source: Source: IRS 1040s, IRS W-2s, ACS 2001-2019, Census Numident and author's calculations using data from Meng et al. (2019). See table 2 for more information. This table shows the relationship between $PM_{2.5}$ and earnings, using different definitions of pollution exposure. Column 1 uses $PM_{2.5}$ exposure resolved to the Census tract level, while columns 2 and 3 use zip code and county level resolution respectively.

Table A2: Robustness Check: Alternative Transformations of the Outcome Variable

	(1) W-2 Earnings	(2) AGI	(3) Earnings	(4) AGI
Panel A: IV				
$PM_{2.5}$ ($\mu g/m^3$)	-0.03004**	-0.0207**	-0.02827**	-0.01375**
	(0.01225)	(0.01037)	(0.01156)	(0.01147)
Transformation	Log	Log	IHS	IHS
Fixed Effects	Birth County, Birth Year × Tax Year, Birth State × Birth Year, Birth Month			
Individual Controls	Yes	Yes	Yes	Yes
County-level Controls	Yes	Yes	Yes	Yes
Observations	10,610,000	13,710,000	10,610,000	13,710,000
Control Mean	$25,490	$35,340	$25,490	$35,340
First Stage F-Stat	9.69	9.74	9.69	9.74

Notes:Significance levels are indicated as * 0.10 ** 0.05 *** 0.01. Source: Source: IRS 1040s, IRS W-2s, ACS 2001-2019, Census Numident and author's calculations using data from Meng et al. (2019). See table 2 for more information. This table shows the effect of $PM_{2.5}$ on earnings and AGI using different transformations of the dependent variable. Columns 1 and 2 use a logarithmic transformation (which implicitly excludes zero and negative values), while columns 3 and 4 use an inverse hyperbolic sine, which allow for zero and negative valued income.

Table A3: Robustness Check: Alternative Birth Designations, Sample Restrictions, and Pollutants

	(1) W-2 Earnings	(2) W-2 Earnings	(3) W-2 Earnings	(4) W-2 Earnings	(5) W-2 Earnings	(6) W-2 Earnings
PM ($\mu g/m^3$)	-1,105**	-1700**	-979**	1,476**	-148	-337**
	(493.2)	(739)	(466)	(677)	(98)	(163)
Birth Designation	IRS	Numident	IRS	Numident	IRS	Numident
Sample	Full Sample	Full Sample	Monitor Counties	Monitor Counties	Monitor Counties	Monitor Counties
Pollutant	$PM_{2.5}$	$PM_{2.5}$	$PM_{2.5}$	$PM_{2.5}$	PM_{10}	PM_{10}
Fixed Effects	Birth County, Birth Year × Tax Year, Birth State × Birth Year, Birth Month					
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes
County-level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	10,610,000	10,470,000	7,525,000	8,238,000	6,791,000	7,554,000
First Stage F-Stat	9.69	8.498	10.02	8.985	10.46	9.122

Table A4: Robustness Check: Omitting Adjacent Counties

	(1) W-2 Earnings	(2) W-2 Earnings	(3) W-2 Earnings
$PM_{2.5}$ ($\mu g/m^3$)	-1105**	-1223**	-816.6*
	(493.2)	(506.8)	(452.1)
Observations	10,610,000	10,420,000	9,123,000
First Stage F-Stat	19.26	23.33	19.66
Sample	Full Sample	Drop NO_X Neighbors	Drop NO_X and PM_{10} Neighbors
Fixed Effects	Birth County, Birth Year × Tax Year, Birth State × Birth Year, Birth Month		
Individual Controls	Yes	Yes	Yes
County-level Controls	Yes	Yes	Yes

Notes: Significance levels are indicated as * 0.10 ** 0.05 *** 0.01. Source: Source: IRS 1040s, IRS W-2s, ACS 2001-2019, Census Numident and author's calculations using data from Meng et al. (2019). See table 2 for more information. This table shows the relationship between $PM_{2.5}$ and earnings, using differing samples to address potential spillovers. Column 1 uses $PM_{2.5}$ reports baseline results from Table 1, Column 2 reports results from an identical regression estimating on a sample that excludes all individuals born in counties that border NO_2 nonattainment counties, while Column 3 repeats this exercise omitting individuals born in counties that border either NO_2 or PM_{10} nonattainment counties.