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Negative traffic externalities and infant health:

The role of income heterogeneity and residential sorting

Dede Long (dede.long@csulb.edu)

David J. Lewis (lewisda@oregonstate.edu)

Christian Langpap (langpapc@oregonstate.edu)

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Abstract

Road traffic is associated with a variety of negative externalities such as air pollution and environmental noise, with significant short- and long-run health impacts on infants. This paper empirically quantifies the effects of traffic negative externalities on infant health, focusing especially on how they differ across income groups. We assemble a rich micro-dataset of infant birth outcomes, parental demographics, and neighborhood characteristics to specify and estimate a health model and an equilibrium sorting model. Our results demonstrate that traffic negative externalities reduce birth weights over two times greater in the lowest-income than in the highest-income families. In addition, the effect of change in income from the poorest to the richest group lifts average birth weights by 0.56% among exposed families, while the income change has no impact on unexposed families. We also find that policies affecting traffic pollution exposure lead to residential sorting. The self-selection of the sorting process causes the lowest-income families to allocate away from neighborhoods experiencing policy-driven environmental improvement, negating the positive benefits of environmental policy. The magnitude of the sorting effect, however, is relatively small.

Keywords: Birth weight, Income heterogeneity, Propensity score matching, Sorting model, Traffic externalities

Author affiliations: Long is assistant professor in the Department of Economics, California State University Long Beach. Lewis and Langpap are professors in the Department of Applied Economics, Oregon State University.

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1. Introduction

A rich literature in public health and environmental economics has documented the effect of highway traffic on a variety of negative externalities, including air pollution and noise, with impacts on environmental quality and health. In particular, traffic-related air pollution can have significant short- and long-term health consequences for infants and children (Chay & Greenstone, 2003a, 2003b; Currie, 2013; Currie et al., 2014; DeCicca & Malak, 2020; Knittel & Sandler, 2013).[1] Extensive documentation has been assembled on noise pollution's negative health effects (Basner et al., 2014; Gehring, Tamburic, Sbihi, Davies, & Brauer 2014; WHO 2011). Although its specific relationship to infant health is far from settled (Smith et al. 2011), pregnant women do appear vulnerable; noise pollution has been linked to low birth weights (LBW) and high incidence of small-for-gestational-age (SGA) (Nieuwenhuijsen et al., 2017). Previous studies also show the traffic-related pollution effects likely are heterogeneous across income groups (Currie 2011; Nilsson 2009; Jans, Johansson, & Nilsson 2018). Damages can be cumulative through a child's life cycle (Case, Lubotsky, & Paxson 2001), affecting later educational attainment, labor market performance, and associated socioeconomic disparities (Currie & Rossin-Slater 2015).

In this paper, we use a rich micro dataset of infant birth outcomes, parental demographics, and neighborhood characteristics to estimate the effects of traffic externalities on

[1] Fetuses and very young children are especially vulnerable to air pollution because of their immature immune systems, high minute ventilation (Buka, Koranteng, & Osornio-Vargas 2006), and underdeveloped lung function (Pinkerton & Joad 2000).

infant health, measured as weight at birth, in the Portland Metropolitan area of Oregon, USA. We estimate the heterogeneity of this effect across socioeconomic groups. Identifying a causal relationship between traffic-related externalities and infant health is challenging because effects can be highly heterogeneous across income groups. Exposure to traffic and pollution is not randomly assigned, and socioeconomic factors are particularly important confounders (Currie & Neidell 2005). Higher-income, better-educated households are likely to be more conscious of the adverse impacts of traffic-related pollution, have more resources to mitigate pollution's health effects (Yang & Chou 2018), have greater preferences for cleaner air and a quieter environment, and are better able and thus more likely to move away from heavily polluted areas (Currie 2011). Moreover, given the positive correlation between socioeconomic status and birth outcomes (Currie & Moretti 2007), the residential sorting process itself affects infant health, further contributing to the heterogeneous effects of traffic-related pollution between different socioeconomic groups. That is, part of the change in average infant health in a given neighborhood is not a direct health effect from on-road traffic changes, but rather a result of sorting driven by these changes. Ignoring this endogenous sorting may overestimate the relationship between traffic externalities and infant health.

Earlier literature has addressed income heterogeneities in health effects, and the potential bias in estimating them induced by socioeconomic confounders, in a variety of ways. Some studies have been conducted at the county level with county-level per-capita incomes (Chay & Greenstone 2003a, 2003b). Others control for income at an aggregated level such as the census block (Banzhaf & Walsh, 2008; Currie, Neidell, & Schmieder 2009). The literature on individual health outcomes has thus far lacked proxies for the *corresponding* individual incomes. In contrast, we are able to include such a proxy by using birth records containing the exact

addresses of our sample of mothers, permitting us to link each mother to publicly available data on the real-market value of her residence and offering, in turn, a fine-scale proxy of family incomes. We then can explicitly control for the effect of family income on infant health, as well as assess how the effect of traffic exposure varies with income.

Virtually all earlier studies recognize the potential for bias in the pollution exposure effect induced by unobserved confounders and sorting. Several use a quasi-experimental approach and rely on air and noise pollution shocks caused by exogenous factors (Chay & Greenstone 2003a; Currie & Walker 2011; Currie et al. 2015; Yang & Chou 2018; Argys et al., 2019). Others take advantage of panel data and use air quality monitor and mother fixed effects (Currie et al. 2009; Currie & Walker 2011), or rely on pollution changes caused by weather events (Jans et al. 2018; Knittel et al. 2015) for identification. To further mitigate bias from socioeconomic confounders, we draw on a rich set of parental- and neighborhood-level controls. Propensity score matching is used to construct a sample of mothers balanced across the household income proxy (i.e. real-market housing value) and other observable characteristics.

Matching exposed and unexposed mothers based on observed characteristics including the income proxy returns a balanced estimation sample. Then, we use this sample in a post-matching regression analysis that controls for spatial and temporal unobservables through the use of fixed effects (Ferraro & Miranda 2017). If sorting contributes to the heterogeneous effects of pollution, it is important to further assess the magnitude of these second-order health impacts induced by sorting. To do so, we separately estimate a sorting model to explain the household's residential choice. We then use this model in a simulation procedure to predict how neighborhood income shifts when residents sort themselves into new locations, in response to adjustments in the spatial patterns of the city's amenities arising from policy-induced traffic

volumes or pollution exposure changes. Through the estimation of the sorting model, the income proxy becomes an estimated function of pollution exposure, which reveals the overall effect of pollution exposure when sorting is considered. Finally, we use the forecasted income and the estimated health model to predict birth outcomes, and thereby obtain the overall health effects.

We acknowledge two main limitations of our analysis. First, the effect of traffic pollution exposure on property values induces a "bad control" problem. Since traffic pollution hurts property values, which in turn has a positive impact on infant birth weight, our estimation results are likely to have an upward bias. That is, the real effect of pollution exposure is "more negative" than our estimates in the health model. Other independent variables in the health model, like race and age compositions, are also potentially "bad controls" due to sorting. Second, given that we have pooled cross-sectional data instead of a panel, in the health model we can only control for neighborhood fixed effects instead of individual fixed effects. Therefore, the causal identification in the health model relies on the assumption that there is no (unobservable) individual preference-based residential sorting, which weakens the causal claim.

Our analysis contributes to several strands of literature. First, we use a household's real-market housing value as a fine-scale proxy for its income by linking its birth records with the property's publicly available tax-lot data. This enables us, in a contribution to the income and infant health literature, to measure not only how income affects birth weight (Conley & Bennett 2001; Conley & Bennett 2000; Currie & Moretti 2007) but also for how income interacts with exposure to traffic-related pollution – that is, whether income offers mothers the ability to mitigate the negative direct or first-round effects of traffic-related pollution with other health investments.

Second, unlike previous literature that ignores neighborhood attributes, we control for covariates at the neighborhood level, including the observable built environment and demographic attributes, and a set of fixed effects controlling for unobserved amenities. Third, this paper contributes to the literature examining traffic reduction policies (e.g. gas or carbon taxes) by explicitly evaluating the health benefits created from reductions in traffic volume. Past work in economics has focused on the environmental and economic impacts of policies such as a carbon tax (Davis & Kilian 2011), yet little systematic study of health effects has taken place.

Our results suggest traffic externality exposure significantly impairs birth weights. There is considerable heterogeneity in this effect across income levels, as birth weight reductions due to pollution exposure are over two times larger in the poorest families than in the highest-income families. The suggestion is that traffic pollution itself is a vector of economic inequality. We also provide evidence of income's direct health effects. On average, the effect of change in income from the poorest to the richest families lifts birth weights by 0.56% among families exposed to pollution. Finally, the neighborhood-level benefits of reducing traffic exposure are, particularly among the lowest income groups, affected by the residential sorting consequent to the policy and urban amenity pattern change. The magnitude of the sorting effect, however, is relatively small.

Environmental policy is generally perceived to be an effective tool for environmental quality improvement and income inequality reduction because both significantly improve the lives of the poor. However, the self-selection of the sorting process forces the lowest income families to allocate away from neighborhoods experiencing policy-driven environmental improvement, negating a part of the positive benefits of environmental policy. This finding sheds light on implications for economic efficiency, social equality, and environmental justice.

The rest of this paper is organized as follows. Section two reports the study area and data. Section three describes our identification strategy and empirical approach including the empirical health model and the sorting model. Section four presents the estimation and simulation results. Section five summarizes and offers the principal conclusions.

2. Study Area and Data

2.1 Study Area

Our study area is the Portland Metropolitan area in Oregon, including Clackamas, Multnomah, and Washington Counties. This is the largest urban area in the state, with a population of approximately 2.3 million. Growth in population, automobile use, and consequent traffic-related pollution are a concern, especially for those near major roads with busy traffic. Portland was recently ranked as one of the most gentrified cities in the U.S. (Bates 2013), suggesting that income-based residential sorting has been particularly pronounced.

2.2 Data

2.2.1 Birth Outcomes and Parental Characteristics

We obtain data on birth weights and individual-level covariates of birth parents from Vital Statistics records provided by the Oregon Health Authority for the Portland Metropolitan area for the years 2000 to 2014. After removing observations with missing or unknown values and wrong addresses, the final dataset includes a total of 292,357 births.[2] Vital Statistics

[2] We removed subjects with birth weight below 300 grams (0.03%) and above 6000 grams (0.01%) as well as those with multiple births (3.49%) and missing or unknown values for mother age (0.003%), maternal complications (0.32%), and delivery weight (1.23%) and pregnancy weight (0.58%). Additionally, we include a category for

provides a rich source of information. They include not only babies' health at birth, but also detailed demographic information on their parents, including age, race, education, and marital status, as well as the exact address of the mother at the time of birth. Pregnancy and delivery conditions such as maternal drinking or smoking, attendant type during delivery, and the place of delivery are also available. We geo-coded the mothers' addresses and merged the Vital Statistic record data with other spatial data layers.

2.2.2 Housing Transactions Data

We obtained information on 512,766 arms-length single-family detached residential transactions for the period 2000 - 2014 from tax-lot data in Portland Metro's Regional Land Information System (RLIS), in turn, drawn from county assessment and tax records. Figure 1 plots all the transactions in our study area. The tax-lot features include total property value[3], real-market housing value, building value, land value, and square footage, land-use type, sales price, and sales date. Most importantly, all include exact longitude and latitude and thus can be joined spatially with the Vital Statistic records. As a result, we have the real-market value (RMV) for the residence of each mother in our data.[4] Given that the Vital Statistics Record data

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missing data for each categorical variable. Finally, subjects with adjusted real-market housing values below $5,000 (2.26%) and above 99th percentile (0.96% of births) are also excluded (2014 is the base year when we adjust for the inflation).

[3] Total property value is the total of building value and land value.

[4] Note that the RMV is distinct from "assessed value", which is used for the purpose of calculating property taxes. In Oregon, county assessors are required to estimate RMV for each parcel, which is comparable to the competitive market price at which a land parcel would be exchanged in an arms-length transaction. The advantage of using

has a time and spatial dimension, we match it with other data layers based on both location and time. For example, mothers are matched with the closest houses in a particular birth year. Summary statistics for birth outcomes, mother demographics, maternal characteristics, and housing value are shown in Table 1. Additional summary statistics for household, housing, and neighborhood characteristics are shown in Appendix Table A1.

As discussed above, we use real-market housing value as a proxy for household income. To evaluate the adequacy of this approach, we spatially join tax-lot data with income data at the census tract level and regress median family income on median housing value and census-tract fixed effects.[5] Results shown in Appendix Table A2 indicate that on average a 1% increase in median real-market housing value is associated with a 0.16% increase in income or, alternatively, a 1% income rise is associated with a 6% increase in real-market housing value. The parameter estimate is highly significant. The correlation between census-tract-level median family income and median real-market housing value is 0.5534. Both fixed-effect estimation and correlation results suggest real-market housing value is strongly correlated with income and thus a suitable proxy.

2.2.3 Household and Neighborhood Characteristics

Information on household demographics and neighborhood characteristics come from the U.S. Census. Data are available for 2000 and 2010 at the census block level, which is the finest

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assessor estimates of RMV is that it is available for each parcel every year, which is not the case of sales price since housing transactions take place with a low frequency. We have adjusted the RMV for inflation using 2014 as the base year.

[5] Census-block-group level median income data is only available for year 2000, 2013, and 2014 for the study area.

geographical level available. The study area contains 28,270 census blocks in 2000 and 35,310 in 2010. Blocks vary by sizes, averaging 28 households in a block (excluding those with no houses).

Once the household demographic variables are created, the census-block-level demographics are spatially assigned as household characteristics for mothers located in the corresponding census blocks. The census data also include information on demographic characteristics such as ethnic and age-group composition. However, a potential concern with using census blocks to control for such neighborhood characteristics is that census geography boundaries change over time for reasons that might be correlated with neighborhood characteristics. Additionally, census tracts, census blocks, or census block groups vary greatly in size, rendering aggregation difficult. To overcome these limitations, rather than use the pre-existing census block geography, we follow Banzhaf & Walsh (2008) and Stone, Wu, & Alig (2015) and define a circle with a given size as a neighborhood. Constructing circular neighborhoods in this manner avoids the potential inconsistency and endogeneity problems arising from boundary shifts. Additionally, constructed boundaries provide us with flexibility in merging data with different spatial scales into the same unit of analysis.[6, 7]

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[6] The year 2000 census boundaries shifted in 2010. These boundaries must be the same to ensure accurate results of the linear interpolation. Hence, we aggregate each variable as an area weighted sum from the block level to the circular neighborhood level. The number of elements in the weighted sum varies. Census blocks are small in population dense areas and big in less populated areas. Consequently, more blocks are aggregated into one circular neighborhood in densely populated areas than in remote rural areas.

[7] We spatially join mothers/infants (point) to the closest circular neighborhoods (polygon). A distance field is calculated to the boundary of a polygon to show how close the polygon is. A polygon that the points fall inside is

Neighborhood-level race/ethnicity data include the shares of African American, Asian and Pacific Islander, Native American, and Latino. We also construct variables reflecting densities and shares for the population below 18 and above 65 years old. Density is calculated as the total neighborhood population within each neighborhood, and age group shares are calculated by dividing the total population by the population in the relevant age group (below 18, between 18 and 65, and over 65 years).[8] We estimate demographic data between 2000 and 2010 by linear interpolation. Corresponding 2010 census block data are assigned to years from 2010 to 2014.

2.2.4 Traffic Volume and Exposure to Traffic-related Pollution

We obtain data on a range of variables capturing neighborhood amenities from RLIS, which holds detailed and spatially explicit land-use, street, and transportation information. Specifically, we have information on the distance to the closest park, distance to the closest pedestrian-friendly street, number of bus stops, distance to the city center, and single-family, multi-family, industrial, and commercial zoning area shares.[9] Underlying data layers containing one or more explanatory variables are joined to the study area map. To quantify the explanatory variable in a way that is consistent with our circular neighborhoods, we use ArcGIS to aggregate information from the original geometry each year to the overlaid circular ones.

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treated as being closest to the point (i.e. a distance of 0). When two or more points are at the same distance from the polygon, one of the polygons is randomly selected as the matching feature. The circular neighborhoods are tangent to the neighborhoods around them but they do not overlap with each other.

[8] Since all the neighborhoods are circles with the same size, total population measures the population density.

[9] Parks include all local and state parks, and natural areas that belong to cities, the state, and the federal government in the Portland Metropolitan area.

The RLIS data also provide information on traffic volume, which we use to define exposure to pollution. Road traffic is responsible for a variety of pollutants, for example, noise and air pollution, and the two types of pollution are highly correlated (Stansfeld 2015). Air pollution is difficult to measure, and the literature on how far elevated air pollutant concentrations extend is far from settled. In many cases, the within-city spatial variation in air pollution is greater than the temporal- and between-city variation (Jerrett et al. 2005), and within-city variation is predominantly affected by proximity to transport corridors with heavy traffic flows (Hu et al. 2009). Some previous public health studies have used equivalent continuous sound pressure levels to identify noise exposure, while many use distance from residence to major roads as a proxy measure of noise (Stansfeld 2015).[10] Defining traffic-related pollution exposure, therefore, requires that we define both proximity to roads and which roads are considered to have a high volume of traffic.

Most studies have characterized proximity by focusing on areas 100 – 500 meters from a highway. We use 300 meters as the proximity threshold to define whether a household is exposed to traffic-related pollution.[11] High traffic volume (in number of vehicles) is considered to be anything above the 75th percentile during afternoon peak hours (4 PM-6 PM) in the Portland Metropolitan area.[12] This volume was 1219, 1082, and 897 vehicles on average in the

[10] Continuous Pressure Sound Level (Leq) is a fundamental measurement parameter designed to represent a varying sound source over a given time as a single number.

[11] Alternative cutoffs of 150 m. and 500 m. were used to perform sensitivity analysis and give us similar results.

[12] Traffic volume is measured as the combined auto and truck modeled volume crossing the road. Trucks are considered as passenger car equivalents – 1 truck = 1.7 cars due to the extra capacity they occupy on the system

years 2000, 2005, and 2010, respectively.[13] Considering both traffic volume and proximity, exposure to traffic-related pollution is defined as being located within 300 meters of a high-traffic-volume road. All households in the study area are then assigned into a treatment group (the household is exposed) or a control group (not exposed).

Although modeling traffic pollution exposure with a discrete rather than continuous variable reduces sample variation, we do so because it facilitates the use of propensity score matching to address the endogeneity induced by pollution exposure's association with parental demographics. In addition, findings in the public health literature show that traffic emissions typically diminish to near background levels within 150 to 300 meters of the roadway (Tegan et al. 2013).[14]

3. Empirical Approach

We separately estimate health and sorting models and then combine the results to obtain the overall effect of traffic pollution on infant health. Our approach is summarized in Figure 2. First, we estimate a health model, which is the main model of interest; it measures the effect of traffic pollution exposure and income on infant birth weight. Income is proxied using the real-market housing value of the mother's residence. Second, we estimate a sorting model, which measures the effect of traffic exposure (as a disamenity) and real-market housing value (as

[13] The units are vehicles crossing the road during the afternoon peak two-hour period (4:00 pm - 6:00 pm) in each year (2000, 2005, and 2010).

[14] As a robustness check, we estimate the health model using logged exposure distance as pollution exposure measurement. The result is presented in Appendex Table A3. The coefficient of logged exposure distance is positive and strongly significant, showing birth weight increases when mother live further away from roads with heavy traffic.

housing price) on households' choice of neighborhoods of residence. Third, we use the sorting model to simulate the effect of policies that reduce traffic. Specifically, we lower the traffic exposure level, and use the estimated coefficients obtained from the sorting model to simulate the effect of this change on households' location choices and thereby on housing values. Finally, we use the predicted housing values from the sorting model and the estimated coefficients from the health model to estimate a new set of birth weights. In the following sections, we explain in detail how the health and sorting models are specified and estimated.

3.1 Health Model

To assess the impacts of traffic externalities on infant health and account for heterogeneity in these effects across income groups, we estimate the following regression model using cross-sectional data for births during years 2000 – 2014:

$$\log Birthweight_{ijt} = \alpha_0 + \alpha_1 Exposure_{ijt} \times D\_income1_{ijt} + \alpha_2 Exposure_{ijt} \times D\_income2_{ijt} + \alpha_3 Exposure_{ijt} \times D\_income3_{ijt} + \alpha_4 (1 - Exposure_{ijt}) \times D\_income1_{ijt} + \alpha_5 (1 - Exposure_{ijt}) \times D\_income2_{ijt} + \boldsymbol{\alpha_6 M}_{ijt} + \boldsymbol{\alpha_7 N}_{jt} + \boldsymbol{\alpha_8 Year}_t + \boldsymbol{\alpha_9 Month}_t + a_j + \varepsilon_{ijt} \quad (1)$$

Our measure of infant health is the log-transformed birth weight of child *i* born in neighborhood *j* at time *t*.[15] While the effect of pollution on birth weight is generally negative, we suspect the magnitude of the exposure effect may differ substantially across income groups. For example, high-income families may be less affected by traffic-related pollution exposure than low-income families because they are able to afford defensive actions and avoidance

[15] We logged the dependent variable to capture birth weight outliers (i.e. extremely low and high birth weight).

behaviors against it (Moretti & Neidell 2009; Neidell 2008). We also suspect the magnitude of the income effect may differ across income groups. Birth weight may change less as income increases, that is, income increase has a large impact on poorer families.

To capture these heterogeneous income and exposure effects, we first create a binary variable $Exposure_{ijt}$ to indicate whether birth *i* in neighborhood *j* was exposed to traffic-related pollution in year *t*. We then construct three binary variables $D\_income1_{ijt}$, $D\_income2_{ijt}$, and $D\_income3_{ijt}$ using the income proxy, namely the real-market housing value of the improved physical structure on the mother's property,[16] to indicate the income group birth *i* in neighborhood *j* in year *t* falls into. $D\_income1_{ijt}$ is set equal to one if the birth is in the income group with a real-market housing value below the 5% threshold (66,180 dollars[17]), and to zero otherwise. Similarly, $D\_income2_{ijt}$ ($D\_income3_{ijt}$) equals one if the real-market housing value of birth *i* is between the 5% to 15% (over 15%) threshold. Note that using the real-market housing value of the mother's residence as an income proxy rather than, as in previous literature, controlling for median income in the aggregated census tract or poverty level of the mother's zip-code, substantially enhances the statistical variation available for estimation.[18]

With this model specification, the baseline category is unexposed babies in the highest-

[16] This does not include the value of the land.

[17] We tested 1% thresholds as a robustness check. Results are insensitive to the cutoff changes. As a reference, the poverty threshold in Oregon is 24,008 dollars with two parents and two children in 2014 (Oregon Center for Public Policy, 2015).

[18] We join both single family housing and multi-family housing transactions with the Vital Statistics record and use both to estimate model (1).

income families. As a result, the exposure effect of the highest-income group is captured by the coefficient estimate of $Exposure_{ijt} \times D\_income3_{ijt}$. The difference between the coefficient estimates of $Exposure_{ijt} \times D\_income1_{ijt}$ ($Exposure_{ijt} \times D\_income2_{ijt}$) and $(1 - Exposure_{ijt}) \times D\_income1_{ijt}$ ($(1 - Exposure_{ijt}) \times D\_income2_{ijt}$) captures the exposure effect for the lowest-income (mid-income) group. In terms of income effect, the coefficient of $(1 - Exposure_{ijt}) \times D\_income1_{ijt}$ measures the effect of the change in income from the richest to the poorest for the unexposed infants, whereas the difference between the coefficient estimates of $Exposure_{ijt} \times D\_income1_{ijt}$ and $Exposure_{ijt} \times D\_income3_{ijt}$ represents how the change in income from the highest- to the lowest-income group among the exposed babies.

The vector $\boldsymbol{M}_{ijt}$ contains individual-level variables, including babies' gender, mothers' demographic characteristics such as age, marital status, education status, race, and a broad range of maternal/pregnancy characteristics like maternal weight gain, number of clinic visits, maternal smoking, maternal illness and complications, delivery place, payment type, and attendant type.[19] $\boldsymbol{N}_{jt}$ is a vector containing neighborhood amenities and characteristics, such as public transportation and greenspace access at time *t*, distance to the city center, as well as other social attributes at the neighborhood level, including share of the population by race (white, African American, Native American and other minorities, Asian or Pacific Islander, and Latino) and by

[19] Maternal illness and complications include breech birth, meconium passage, premature rupture, induction of labor, augmentation of labor, precipitous labor, prolonged labor, fetal intolerance, pregnancy/chronic diabetes, gestational diabetes, pregnancy hypertension, gestational hypertension, eclampsia hypertension, previous preterm birth, and uterine bleeding.

age (children, working adults, and retirees).

$\boldsymbol{Year_t}$ is a vector of year fixed effects to control for time-variant unobserved factors correlated with both traffic externalities exposure and birth weight, as well as for the decreasing trend in automobile emissions due to improvements in vehicle emission abatement technology.[20] We also include month fixed effects, $\boldsymbol{Month_t}$, which specify the month in which infants were born, to control for monthly fluctuations in birth weight.

The term a_j captures neighborhood fixed effects to control for neighborhood-level unobservables that affect birth weight but are time-invariant. Neighborhood fixed effects effectively eliminate the correlation between pollution exposure and unobserved time-invariant factors. To test whether including individual-level fixed effects rather than neighborhood fixed effects would affect the estimation result for pollution exposure, we construct a panel sub-sample using repeated births from the same mother and compare the panel sub-sample results with the full sample results. However, the panel sub-sample did not retain enough variability to yield significant results.

Finally, ε_{ijt} is the idiosyncratic error term. While we use real-market value as a proxy to explicitly control for the effect of household income on infant health, there remains a potential for bias due to other unobserved socioeconomic confounders. Additionally, the residential sorting process determines certain neighborhood characteristics where households reside,

[20] Note that linear interpolation of some neighborhood characteristics does not affect interpretation of year fixed effects. Interpolation means that, for some years, these characteristics change at a constant rate, which varies across neighborhoods. Year fixed effects, on the other hand, capture changes over time in unobservables that are the same for all neighborhoods, and because there is a dummy for every year these unobservables are allowed to vary at a non-constant rate over time.

including traffic, which also generates a potential bias in estimating the impact of traffic pollution exposure. Indeed, Table 1 suggests that there are significant differences in observed characteristics across the treatment and control subsamples, possibly resulting from the sorting process. As expected, education and racial composition differ between these groups. The share of minorities is greater in exposed (high-pollution) areas than in the unexposed group. Households far from high-traffic roads (the control group) have consistently higher levels of education. The average house value is much higher in exposed areas than in control areas.[21]

We use propensity score matching to control for differences in the observed characteristics between treatment and control groups. This approach is widely used in a variety of fields, including public health. It enables researchers to address a non-randomized/observational setting by simulating the crucial characteristics of a randomized controlled trial (Dehejia & Wahba 1998). Conditional on the propensity score – namely the probability of being assigned to the treatment group – the distribution of the baseline covariates of the treated group will be similar to the covariates of the control group. Mothers who are exposed to traffic-related pollution are matched to a comparable set of mothers who are not exposed, and the resulting matched dataset is balanced on the distributions of covariates across treated and control mothers. In other words, after matching, covariates that may be correlated with exposure assignment and birth weight will have treatment group distributions that are observationally similar to those in the control group.

We estimate the propensity score using a logit model (Stuart 2010). Based on theory and

[21] While one might anticipate a negative correlation between exposure and propeorty values, in our data areas with high traffic-related pollution are also close to the city center and have high house values, likely due to presence of other amenities. Distance to city center is included as a controlled variable.

evidence about variables simultaneously associated with pollution exposure and birth outcome, as well as data available to us, we include mother's age, race, education level, marital status, maternal tobacco use, maternal weight gain, number of prenatal visits, delivery attendant type, and delivery location in the model.[22] Most importantly, we match on family income, measured by the real-market housing value of the mother's residence, which is one of the potential confounders (Currie and Neidell 2005).

The main objective of propensity score estimation is to obtain a balanced sample. Hence, the criterion used to determine the final inclusion/exclusion of covariates is matching quality (Caliendo & Kopeinig 2005; Stuart 2010). We first estimate the propensity score by including a small set of covariates known to be correlated with birth weight.[23] If any variables remain unbalanced after matching, they are then included as covariates in the re-estimated propensity score model. In particular, a variable is added as a covariate if the standardized difference between the mean of the treated and untreated groups in the matched sample obtained from the first-round propensity score estimation is above 0.25 (Stuart, 2010).[24] This matching quality selection method allows us to include a range of neighborhood-level variables, including the number of public transportation stops, distance to the city center, neighborhood racial composition (i.e. share of Africa Americans) and age composition (i.e. share of population below 18 years old), and share of multi-family zoning areas. Hence, the matching process eliminates

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[22] Father's race, age, and education level are not matched because there are too many missing values.

[23] These variables are mother age, marital status, mother's education, mother's race, maternal smoking, and real-market value of mother's property.

[24] The rule of thumb is that a standardized difference in means above 0.25 can cause bias in regression estimates (Imbens and Wooldridge 2009).

systematic differences in observable neighborhood characteristics potentially generated by the sorting process.

We use nearest-neighbor propensity score matching with replacement as the matching algorithm.[25, 26] After matching, we perform a common support (overlap) test for the propensity score estimator.[27] Figure A1 shows the density distribution of the propensity score before and after matching. It indicates that the common support condition is met, so that balance greatly improves after matching. We also calculate the standardized difference in means to ensure that balance is achieved on all covariates (Caliendo and Kopeinig 2005).[28] Table 2 presents the standardized mean difference and the p-values from a difference in means test across treatment (exposed) and control (unexposed) groups before and after matching.[29] We want to make sure that the treatment and the matched sample differ in terms of exposure to traffic, the average distance to the high-traffic-volume road in the treatment and control groups before and after

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[25] We tested 1, 2, 3, and 4 nearest neighbors with replacement and one-to-one exact matching with replacement; results are generally robust to matching algorithms and number of neighbors.

[26] Radius matching was also used as a sensitivity test and gives similar results.

[27] Common support condition (or overlap) ensures that $0 < P(D = 1|X) < 1$. That is, observations with the same covariate values have a positive probability of being both exposed and unexposed.

[28] The standardized difference is defined as the difference of sample means in the treated and control subgroups as a percentage of the square root of the average of sample variances in both groups: $\frac{(\overline{X_1}-\overline{X_0})}{\sqrt{\frac{V_1(X)+V_0(X)}{2}}}$ (Caliendo and Kopeinig 2005).

[29] We dropped 74,470 observations after matching. Every observation in the treatment (exposed) group is matched with observations in the control (unexposed) group. This is known as trimming the sample.

matching confirms that it is the case. Results suggest that the matching procedure effectively returns a balanced sample.[30] The treatment and matched sample also differ substantially in terms of exposure to traffic, as reflected by the distance to high traffic volume road.

We use this balanced sample to estimate model (1). Past literature has shown that non-experimental designs replicate randomized experimental estimates when matching and fixed effects are combined (Ferraro & Miranda, 2014, 2017). Propensity score matching corrects the self-selection problem generated from systematic differences in observed characteristics, while fixed effects control for potential bias arising from unobserved time-invariant factors at the neighborhood level. Note that, while fixed effects mean that we exploit within-neighborhood variation, this does not imply that we need to restrict matches to be within neighborhoods. Matching means that the sample is balanced across observable characteristics, including some time-varying neighborhood characteristics. Fixed effects then wipe out any neighborhood-level time-invariant unobservables that could be correlated with included covariates to mitigate the potential bias.

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[30] Since we use one-to-one nearest neighbor matching with replacement, individuals from the control group are used more than once as a match. Allowing replacement substantially decreases bias and leads to high matching quality. This, however, reduces the number of distinct observations (sample size), which leads to low p-values. Past literature indicates that using statistical significance testing to evaluate covariate balance is discouraged because it is sensitive to sample size (Austin, 2011; Imai, King, & Stuart, 2008). The statistical difference in means between the treatment and control groups does not affect the balance of our matched sample. According to Caliendo and Kopeinig (2008), if standardized bias is reduced to below 5% after matching, the matching method is considered effective in balancing the distributions of the covariate. As shown in Table 2, the standardized difference is mostly below 5% after matching with significant bias reduction, indicating that our matched sample is well balanced.

We want to point out again that combining the propensity score matching method with fixed effects does not necessarily address the selection bias due to preference-based residential sorting. In particular, matching exposed and unexposed mothers based on income proxy does not eliminate the issue that income proxy is a "bad control". In addition, we are not able to control for unobserved individual characteristics through the propensity score matching method with neighborhood fixed effects. The exposure effect measured in model (1) is therefore likely to have an upward bias.

3.2. Sorting Model

The health model (1) identifies the direct effects of changes in traffic externalities from pollution but not the overall (direct and indirect) effects. In particular, the health model (1) does not take account of locational re-sorting due to the policy-driven changes in traffic exposure. Yet in the medium- to long-term, these changes will likely induce sorting by altering neighborhood-level housing demand (Hamilton & Phaneuf 2015). When supply is exogenous, households with heterogeneous preferences for traffic-related pollution re-sort themselves across neighborhoods in response to the policy change, creating a new market-demand relationship and altering neighborhood income distributions. Housing prices adjust and the market clears as the prices of housing types in excess demand rise and the prices of those in excess supply fall.

However, since health model (1) is estimated at the individual level, its use to quantify the health impacts of a localized change in traffic pollution requires individual data from the localized distributions proxied by housing prices from equation (1). If individuals sort in response to a localized pollution change, the corresponding housing price adjustment implies a change in the localized income distribution. This in turn alters the estimated neighborhood

health impacts of the pollution change, since the treatment effect of traffic pollution exposure in (1) depends on income.

To account for potential changes in localized income distributions due to a pollution shock, we estimate an equilibrium sorting model that models the sorting process in response to changes in traffic pollution exposure, simulate a new income distribution after the policy change, then calculate the post-sorting neighborhood average birth weight using the health model estimation results. The purpose of the sorting model is seen in Figure A2, which illustrates our hypothesis of the link between a reduction in traffic pollution exposure and neighborhood income distributions. Our sorting model tests for, and quantifies, the hypothesized change depicted in Figure A2.

This integrated analysis offers two major benefits unattainable by estimating only the reduced-form health model (1). First, the sorting model estimation results are used to simulate changes in the income distribution in neighborhoods resulting from changes in traffic-related pollution. As shown in the example in Figure A2, if a traffic reduction is seen as an amenity that induces sorting, housing demand will rise, lifting housing prices and shifting the neighborhood's income distribution. Since exposure to noise and air pollution differentially affects health across low- and high-income groups, changes in neighborhood income distribution affect the overall health benefits of any reduction in neighborhood traffic. Evaluating the average health benefits of a traffic reduction requires information on our individual-level income proxy. Second, comparing the health model with and without the simulated policy-induced sorting price adjustment highlights the fact that health impact evaluations that ignore indirect sorting (price) effects likely overestimate the overall health benefits from pollution reduction.

Sorting models have their foundation in Tiebout's (1956) seminal study, which argues that households "vote with their feet", sorting into their most preferred community. The sorting process reveals household preferences and demands for public goods. A rich empirical literature has been devoted to residential sorting and equilibrium in the housing market (e.g. Bayer & Timmins 2007; Ellickson 1971; Epple & Platt 1998; Epple & Romer 1991; Westhoff 1977). In our framework, households choose locations based on their personal characteristics and preferences for housing and neighborhood attributes.

Our empirical model is based upon the theoretical equilibrium sorting framework in Bayer et al. (2004, 2007), Bayer and Timmins (2007), and Klaiber and Phaneuf (2010) and built on the discrete choice random-utility maximization (RUM) framework discussed in McFadden (1977) and Berry (1994). In the random utility model, each neighborhood provides a bundle of amenities, and each family chooses where to live based on the amenity bundles offered in different neighborhoods. Instead of using single housing transactions as analysis units, we aggregate housing transactions into housing types (Klaiber & Phaneuf 2010).[31] A housing type *h*

[31] Following Klaiber and Phaneuf (2010), choice sets in the sorting model are constructed from three components – house location, house size, and transaction time. Neighborhoods where houses are located form the choice sets' location component and are defined in the same manner as in the health model. The second component considered in choice set construction is house size. House size is divided into three categories – small, medium, and large. Tertiles are used to define house sizes. The lowest tertile (i.e. 0%-33%) corresponds to small houses; the second tertile (i.e. 33%-66%) defines medium houses; the highest tertile (i.e. 67% - 100%) contains large houses. The final component of the choice sets is the time of the housing transaction. To match time period with traffic volume data, we define three aggregate time periods for the 15 years of the study period: 2000-2004, 2005-2009, 2010-2014 corresponding to the traffic volume data available for 2000, 2005, and 2010. There are a total of 8,391 housing types. Transactions are assigned to a house type given its neighborhood, time, and house-size combinations. On

is constructed with three components: house location, j, house size, k, and transaction time, t. Household i's indirect utility from choosing housing type h={j, k, t}is:

$$U_{jkt}^{i} = U_{h}^{i} = V(\boldsymbol{H}_h, \boldsymbol{A}_h, \boldsymbol{S}_h, p_h, \xi_h) + \epsilon_h^i. \quad (2)$$

Equilibrium is reached when every household resides at its utility-maximizing location and can be represented in the utility form:

$$U_h^i \geq U_{h'}^i \; \forall \; i, h, h'. \quad (3)$$

Following conventional RUM theory, utility is comprised of observed (housing attributes $\boldsymbol{H}_h$, neighborhood environmental characteristics $\boldsymbol{A}_h$, and neighborhood social characteristics, $\boldsymbol{S}_h$) and unobserved components. The unobserved components include a house-type-specific unobservable ξ_h and an idiosyncratic term ϵ_h^i unique to each individual i and house type h. ξ_h captures the mean indirect utility specific to each house type. The empirically estimated indirect utility function can be written as:

$$U_h^i = \beta_h^i \boldsymbol{H}_h + \beta_a^i \boldsymbol{A}_h + \beta_s^i \boldsymbol{S}_h + \beta_p p_h + \xi_h + \epsilon_h^i, \quad (4)$$

where β_x^i, x = {h, a, s} is a vector of structural preference parameters consisting of a mean parameter $\beta_{0,X}$ common across all households and an individually specific component $\beta_{r,X}$ that varies by households such that:

$$\beta_X^i = \beta_{0,X} + \sum_{r=1}^{R} \beta_{r,X} d_r^i, \quad (5)$$

Where R is the total number of household characteristics d_r^i (e.g., household size and the number of children). This model specification captures heterogeneous household preferences for

average a house type contains approximately 48 housing transactions, providing substantial statistical variation. Aggregating houses into groups also reduces the number of choice alternatives, facilitating estimation. We therefore have three time equilibrium in each time period.

different neighborhood amenities (such as air quality and noise level) since household demographics d_r^i vary with neighborhood characteristics. Given this specification, the mean indirect utility ξ_h includes mean parameter $\beta_{0,X}$.

Parameters in equations (4) and (5) are estimated following the two-stage econometric implementation strategy discussed in Bayer and Timmins (2007), Berry et al. (2004), and Klaiber and Phaneuf (2010). In the first stage, interaction parameters $\beta_{r,X}$ and mean indirect utility ξ_h are recovered by maximum likelihood. In particular, if we assume ϵ_h^i in equation (4) follows an *i.i.d* Type I extreme value distribution, the conditional probability of household *i* choosing a house type *h* is:

$$Pr_h^i = \frac{e^{\beta_X^i X_h + \beta_p p_h}}{\sum_j e^{\beta_X^j X_h + \beta_p p_h}}, \tag{6}$$

where $X_h = \{\boldsymbol{H}_h,\ \boldsymbol{A}_h, \boldsymbol{S}_h\}$.[32] The expected share of households choosing a house type *h* can be obtained by aggregating (6) to generate an aggregated expected demand for house type *h*. Under the market equilibrium, the aggregated housing demand equals the exogenous housing supply for each house type *h*.

The loglikelihood function can be written as:

$$ll = \sum_i \sum_h Y_h^i ln(Pr_h^i), \tag{7}$$

where Y_h^i =1 if individual *i* chooses housing type *h*, 0 otherwise.

Given our large dataset and the resulting computational constraint, we leverage the independence of irrelevant alternatives (IIA) assumption maintained in the logit model and use

[32] Due to computational difficulty, we did not allow housing prices to be heterogeneous across housing types. In other words, p_h is not interacted with individual demographics d_r^i.

the method proposed by McFadden (1977) to estimate parameters in the first stage estimation. Specifically, individuals' chosen alternatives and a subset of non-chosen alternatives are used. In addition, the indirect utility, ξ_h for each housing type is estimated by the contraction mapping method developed by Berry, Levinsohn, and Pakes (BLP) (Berry 1994; Berry et al. 1995).[33]

In the second stage, the mean indirect utilities ξ_h for housing types recovered in the first stage are regressed on neighborhood characteristics, $\boldsymbol{A}_h$ and $\boldsymbol{S}_h$, housing characteristics $\boldsymbol{H}_h$, and housing price p_h:

$$\xi_h = \beta_{0,H}\boldsymbol{H}_h + \beta_{0,A}\boldsymbol{A}_h + \beta_{0,S}\boldsymbol{S}_h + \beta_p p_h + \mu_h \tag{8}$$

We estimate equation (8) with 2SLS estimation since housing prices are endogenous. In particular, unobserved house type characteristics are likely correlated with the observed price, which is an explanatory variable. Correlation occurs because better locations, characterized in part by the unobserved attributes, are likely to command a higher price, inducing a relationship between unobservable factors and prices. We use the characteristics of neighborhoods within a 4-mile distance to construct an instrument for each neighborhood's housing price, following methods discussed in Bayer et al. (2004). The instrument arises from the market-clearing condition given the spatial nature of a housing market. Specifically, the neighborhood characteristics in a distant neighborhood are correlated with a local price, while the unobserved attributes of a local housing type are not likely to be correlated with the distant housing type's amenities or other exogenous attributes. This suggests the exogenous attributes of distant neighborhoods are desirable instruments for the price variable.

[33] Given that single-house transactions are aggregated into housing types, housing characteristics and sales prices are medians for all housing transactions within a housing type.

Following Klaiber and Phaneuf (2010), the 2SLS estimation is carried out as follows. First, we rearrange equation (8) and add housing price instruments. The rearranged equation with additional independent variables can be written as:

$$\xi_h - \beta_p^* p_h = \beta_{0,H}\boldsymbol{H}_h + \beta_{0,\tilde{A}}\widetilde{\boldsymbol{A}_h} + \beta_{0,\tilde{S}}\widetilde{\boldsymbol{S}_h} + \mu_h, \tag{9}$$

where β_p^* is an initial guess for the price coefficient. $\widetilde{\boldsymbol{A}_h}$ and $\widetilde{\boldsymbol{S}_h}$ include the neighborhood characteristics for the current house type and additional neighborhood amenities and social attributes of neighborhoods within a 4-mile distance as housing price instruments. We then estimate equation (9) using OLS and obtain the predicted price $\widehat{p_h}$ that meets the market clearing condition:

$$\boldsymbol{Housing\ Supply}_h = \frac{1}{N}\sum_{i=1}^{N}\frac{e^{\widehat{\beta_X^J}X_h}}{\sum_j e^{\widehat{\beta_X^J}X_h}} \quad \forall h = 1,\ldots,J\,, \tag{10}$$

where $\widehat{\beta_X^J}$ are the parameter estimates obtained from the first-stage estimation. Finally, we re-estimate equation (9) using the predicted price as the initial guess for the price coefficient until the coefficient estimate does not change to ensure the stability of our estimation results. This iteration ensures our estimates do not depend on the initial guess of prices.

With the first and second stage estimation results in the sorting model, we evaluate the impact of policies that reduce traffic flows on residential sorting. Specifically, we examine a range of hypothetical policy shocks sufficient to reduce exposure to traffic-related pollution, measured by traffic volume within a neighborhood, by 5%, 10%, and 20%. Carbon pricing is one example of a policy that could generate such a traffic change through higher gas prices.[34] If

[34] A study has shown that traffic flow elasticity to gasoline price changes is -0.050 on highways without a high-occupancy vehicle (HOV) lane (Bento et al. 2012). In that way, a $1.84 gas tax would lead to a 5% traffic volume

these shocks affect traffic-related pollution near housing type *h*, the housing demand for *h* increases if households have preferences for living near less traffic. Consequently, house prices are bid up in neighborhoods with lower levels of traffic externalities and the market reaches a new equilibrium as new housing demand is equated to the assumed exogenous housing supply.

The sorting model returns a price vector under the new market equilibrium, which is a function of the new traffic volume (and pollution) level.[35] The new housing price vector proxying for the new distribution of neighborhood incomes is the key variable that will be used to predict post-sorting average birth weight in a neighborhood with the health model estimation results. Previous literature suggests that birth weight among low-income households is more responsive to changes in traffic externalities; that is, the health benefits of pollution reduction are greater in low- than in high-income houses (Maciag 2015). The sorting process - displacement of low-income households away from locations with reduced air pollution - mitigates the direct benefits of the air pollution reduction policy. Hence, the post-sorting health effects of the reduction in traffic externalities would be more modest than the pre-sorting ones. Finally, we incorporate the new housing price results from the sorting model into the health model to compute the overall (post-sorting) health effects of a reduction in traffic externalities.

In sum, we use a two-step process to obtain the overall birth weight effects of a change in traffic-related externalities. First, we estimate the health model (1) directly in a post-matching regression. A direct effect of a reduction in traffic is determined from the health model (1) while

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decline, given that the current Oregon average price of gasoline was $1.84 per gallon during 2000 - 2010 (EIA 2017).

[35] Note that housing price in the sorting model is not transaction price for an individual house, but the median price of a housing type.

holding the individual income proxy variable fixed at initial values. Second, we simulate how real-market housing values adjust to traffic externality changes through the sorting model and re-evaluate (1) by substituting the post-sorting real-market housing values to obtain corresponding birth weights. Therefore, the effect of externality reductions on birth weight can be decomposed into a direct and an indirect effect. The overall effect of these reductions is determined from the health model (1) after the income proxy variable has been adjusted to reflect the sorting model's estimate in response to a change in pollution. The indirect effect of traffic reductions on birth weight is the difference between the overall effect and the direct effect.

4. Estimation Results

4.1 Health Model Results

The health model is estimated with a balanced sample obtained by one-to-one nearest-neighbor matching with replacement. Estimated coefficients of the main variables of interest are shown in Table 3 and the full coefficient set in Appendix Table A5. Note that since the empirical model is in the log-linear form, the coefficients of the dummy variables need to be rescaled to reflect their relative effects on birth weight. The estimation results indicate that the marginal effect of traffic-related pollution exposure on birth weight differs substantially by income. On average, pollution exposure, defined by distance to busy roads with high traffic volume, reduces birth weight by 1.27% among the lowest-income mothers and 0.50% in the highest-income group, whereas it has no statistically significant impact on infant birth weights in

the mid-income group.[36] The exposure effect diminishes with increasing income and is more than two times larger in the lowest- than the highest-income families. To provide perspective on the relative magnitude of these effects, we note (from Table A5) that maternal smoking reduces birth weight by 4.67% on average.[37] In the lowest-income group, therefore, the health effect of traffic-related pollution exposure is about 27.19% of the effect of maternal smoking, while in the highest-income group it is 10.71% of the effect of smoking, a result that contributes to the mounting evidence that income exacerbates health disparities. The results also demonstrate that income has a positive impact on birth weight only when families are exposed to pollution. Among the exposed babies, a change in income from the lowest- to the highest-income group lifts the birth weight by 0.56% on average.[38] This positive income effect, however, disappears among unexposed families. This may be that the income effect is already captured by the fixed effects or other control variables.

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[36] The percent impact of pollution exposure status change from 0 to 1 on birthweight is (exp($\alpha_1 - \alpha_4$) − 1)*100% = (exp(-0.0127)-1)*100% = -1.27% in the lowest-income group. Similar calculation is done to compute the exposure effects among the mid- and higher-income families.

[37] Again, since smoking is a binary variable, the relative effect of smoking is (exp(-0.0478)*100% = - 4.67%

[38] The coefficient of the mid-income group is not statistically significant. As a robustness check, we test different income group thresholds and whether the income effect is statistically different across the mid- and highest-income groups. While there is a very small difference between the income effects across the two income groups in our original specification (*p-val* = 0.073), the results disappear in other specifications and are not robust. Therefore, our conclusion that there is heterogeneity in the income effect on birth weight, and the income effect goes away for income levels beyond that of the lowest-income groups still holds.

Estimation results for other covariates are consistent with expectations (see Table A5). Male infants tend to have higher birth weights than females. Babies born to married couples tend to have higher birth weights than those born to unmarried couples. Maternal smoking reduces birth weight by 4.67%, consistent with past studies showing maternal smoking is one of the most important determinants of impaired birth weight (Zdravkovic, Genbacev, McMaster, & Fisher 2005). Mothers visiting prenatal care more frequently have heavier babies. Parity, indicating a female has been pregnant and carried the pregnancy to a viable gestational age, is associated with higher birth weights. This finding agrees with previous research showing birth weight tends to rise with parity (Hinkle et al. 2014; Wilcox et al. 1996). As expected, maternal weight gain and birth weight are positively associated. Mothers with higher pregnancy weight gain are more likely to deliver heavier babies.

4.2 Sorting Model Results

First-stage maximum-likelihood estimation recovers, along with the mean indirect utility of each housing type, estimates of the interaction parameters reflecting preferences for neighborhood and housing characteristics. The first-stage estimates divide the overall population into groups reflecting households' heterogeneous preferences for a variety of neighborhood and housing attributes. Estimates presented in Table 4 are consistent with expectations and, for the most part, with findings in the earlier literature. Most importantly, the interaction between traffic volume and the number of children is negative and highly significant (1% level) among families with children, supporting our hypothesis of the importance of sorting away from high-traffic regions. Overall, household preferences for neighborhood amenities vary strongly by family structure and are estimated to be highly heterogeneous. Our second-stage estimates decompose mean indirect utility into observable and unobservable components. Results reported in Table 4

based on the instrumental variable for house price have expected signs and magnitudes. For example, all else equal, households prefer houses that are lower in price. The second and third-period estimation results are similar to the first-period results, indicating individual preferences for the housing market are constant across time.

4.3 Simulation Results

Estimation results of the 5%, 10%, and 20% traffic reduction policy simulation are presented in Table 5. Figure 3 highlights the initial high-traffic-volume roads before the traffic reduction. The simulation analysis provides several insights unavailable from examining the health model alone. The sorting model indicates that a decrease in traffic volume boosts the real-market housing value. Specifically, the larger the traffic reductions are, the greater the housing prices increase. As shown in Figure 4, neighborhoods with a 5% projected housing price increase are those with the most traffic. Thus, the income distributions of neighborhoods suffering less traffic-related pollution have shifted in a manner consistent with our hypothetical example in Figure A2. This shift in the income distribution is driven by higher-income households moving into the neighborhoods with reduced traffic and corresponding pollution, which in turn bids up property prices in those neighborhoods. Traffic reduction health effects will shift with this new income distribution.

As shown in Table 5, consequent upon the simulated 5%, 10%, and 20% traffic reductions, the number of high-traffic-volume roads decreased from 25.01% to 20.03%, and the share of exposed individuals declines from 53.95% to 47.79%. Given our use of a binary exposure variable in the health model (1), we focus our analysis of health outcomes on infants who are no longer exposed on account of the traffic-related pollution reduction. A policy shock

reducing traffic pollution by 5%, 10%, and 20% leads to 0.52%, 0.50%, and 0.50% higher average birth weights in these families, respectively.

The results demonstrate that reductions in traffic lead to increases in birth weights, however, the marginal effect of pollution reduction is decreasing. Two major causes contribute to these findings. First, traffic reduction lifts birth weights because households exposed before the simulated policy changes are no longer exposed after the traffic reduction, thus directly inducing an increase in birth weight. Second, a diminishing marginal effect is observed as an indirect effect of sorting. The sorting model simulation reflects a shift in the neighborhood's income distribution, that is, when higher-income households who are not severely affected by the traffic-related pollution move into neighborhoods with less traffic, while the lowest-income families who benefit the most from the traffic reduction reallocate away from these neighborhoods. This sorting process and the associated income distribution shift thus lead to the decreasing marginal effect of pollution reduction. Given the limitation of the sorting model, we are not able to track where individual mothers move to. Individual and neighborhood characteristics and month and year fixed effects should change after sorting. However, we are not able to predict such changes in the sorting model and therefore assume they are the same as the pre-sorting level in the birth weight prediction.

Results from the health model show that the direct effect of reducing traffic by 5%, 10%, or 20% is to increase birth weights by 0.51% for these individuals who experience a change in exposure status.[39] In other words, in the absence of sorting, the pollution reduction resulting in a reduction in traffic directly raises birth weight by an average of 0.51% of the affected

[39] This is the aggregated average of birth weight percentage change among individuals in all three income groups and are no longer exposed at the post sorting stage.

individuals. Subtracting the direct effect from this total effect shows that sorting increases average birth weight in neighborhoods where traffic decreased by 5% by 0.01%, while it decreases average birth weight in neighborhoods with 10% and 20% traffic reduction by 0.01%. It might seem surprising that the sorting process lowers average birth weight in the long run following a traffic reduction policy. This is because, as explained above, higher-income households experiencing little pollution effects move into neighborhoods with less traffic, while low-income families who experience substantial exposure effects reallocate away from lower pollution areas after the policy shock. Sorting thus negates some of the positive health impacts generated from a pollution reduction policy. This indirect sorting effect becomes greater as traffic reduction increases and when cleaner neighborhoods attract a larger number of higher-income families.

If we only examine families in the lowest income group, the overall effect of eliminating traffic-related pollution exposure by 5%, 10%, or 20% is to boost birth weight by 1.65%, 1.60%, and 1.53%, while the direct effect is to increase birth weight by 1.27% among these families. Sorting alone, therefore, lifts average birth weight in the poorest neighborhoods experiencing a 5%, 10%, and 20% traffic reduction by 0.38% and 0.33%, and 0.26%. The exposure effect diminishes with greater traffic reduction when more and more lowest-income families allocate away from areas with improved pollution. The impact of residential sorting on birth weight, however, is relatively a small effect based on the simulation results.

5. Conclusions

There is considerable evidence that traffic-related pollution affects infant health, and this effect is likely heterogeneous across income groups. In particular, income and other

socioeconomic confounders, along with the related sorting process in response to traffic pollution change, complicate estimation of traffic externalities' health effects. In this paper we take advantage of a rich micro dataset of infant birth outcomes, including the exact address for each mother, to create a fine-scale proxy for family income. This allows us to examine the heterogenous health impacts across income categories at a considerably finer resolution than in previous literature. We also combine propensity score matching to construct a balanced estimation sample with neighborhood fixed effects to estimate the effects of traffic-related pollution.

Our results indicate that traffic-related pollution significantly reduces birth weights, and this effect is highly heterogeneous across incomes. In particular, our estimates suggest the impaired birth weights from traffic-related externality exposure are over two times higher in the lowest- than in the highest-income families. In addition, among exposed families, the income effect of the change in income from the poorest families to the richest ones improves birth weight by 0.56% on average. These income group differences highlight the importance of conditioning on a fine income scale when measuring traffic-related pollution's health effects. While we are not able to control directly for household income, we condition on a fine-scale proxy that is highly correlated with it. Omitting income or such proxies, or controlling for aggregate income only, likely masks meaningful variation at the household level, hence failing to adequately capture these important socioeconomic gradients.

There are two main limitations with our approach in the health model. First, as an income proxy, property value is a "bad control" because it is an outcome of traffic pollution exposure. Given that property value has a positive impact on birth weight and is negatively affected by pollution exposure, our pollution exposure estimate may have an upward bias.

Second, we are not able to control for individual fixed-effects and thus implicitly assume no unobservable preference-based residential sorting exists, which compromises the casual claim.

This paper also uses a simulation approach that combines a health model and a sorting model to examine the effects of a policy that lowers traffic on infant health when residents are allowed to resort themselves in response. The simulation procedure allows us to explicitly assess the impact of the household self-selection process, one of the most important systematic ways in which a family can affect its traffic-related pollution exposure. We introduce a range of policy shocks of 5%, 10%, and 20% reductions in traffic and use the sorting model to predict the new housing value distributions this policy causes. This in turn allows us to simulate the birth weights contingent on residential sorting consequent to the policy, and hence disentangle the direct and overall (direct and sorting-induced indirect) health effects of lowering traffic.

Results from this simulation procedure suggest that when residential sorting is introduced, the benefits of traffic reductions diminish as traffic reductions increase. This is because heterogeneous households sort in response to reduced traffic on busy roads, altering neighborhood compositions accordingly. That is, households with higher income who are not severely affected by pollution move into neighborhoods with less traffic, whereas lowest-income families experiencing the largest pollution mitigation benefits are forced to reallocate away. Our analysis offers fresh insights, and the associated policy implications, on how urban environment improvement policies affect infant health. Given that the lowest-income families receive greater benefits from pollution reductions, locating traffic reductions in poorer neighborhoods could bring an improvement in health impacts. However, the sorting process negates a part of these benefits as the poorest families are forced to move out of neighborhoods with reduced pollution levels.

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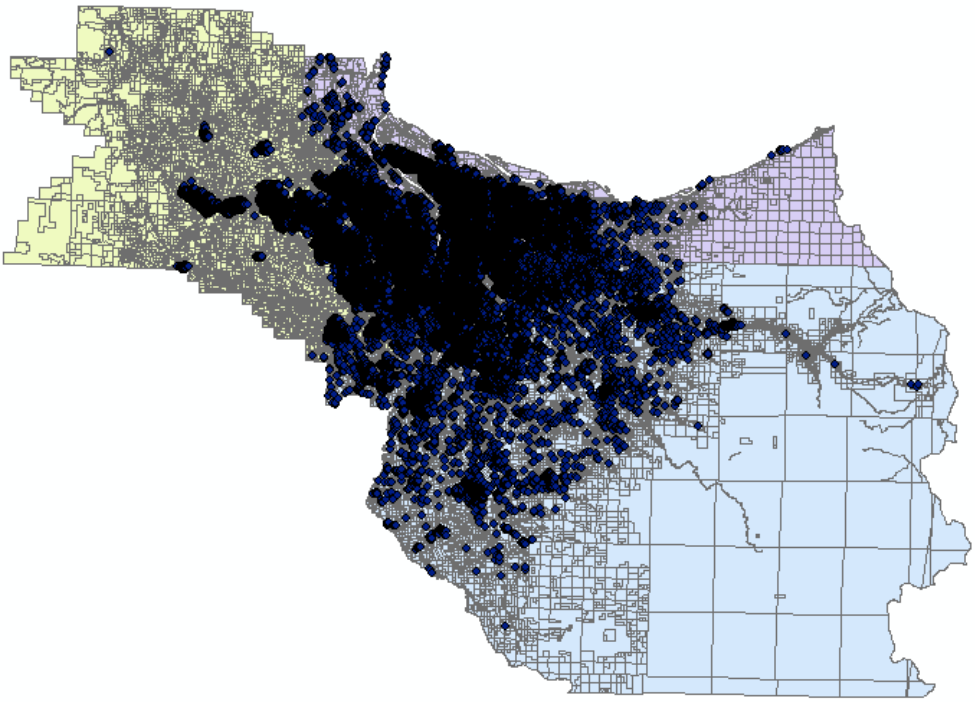
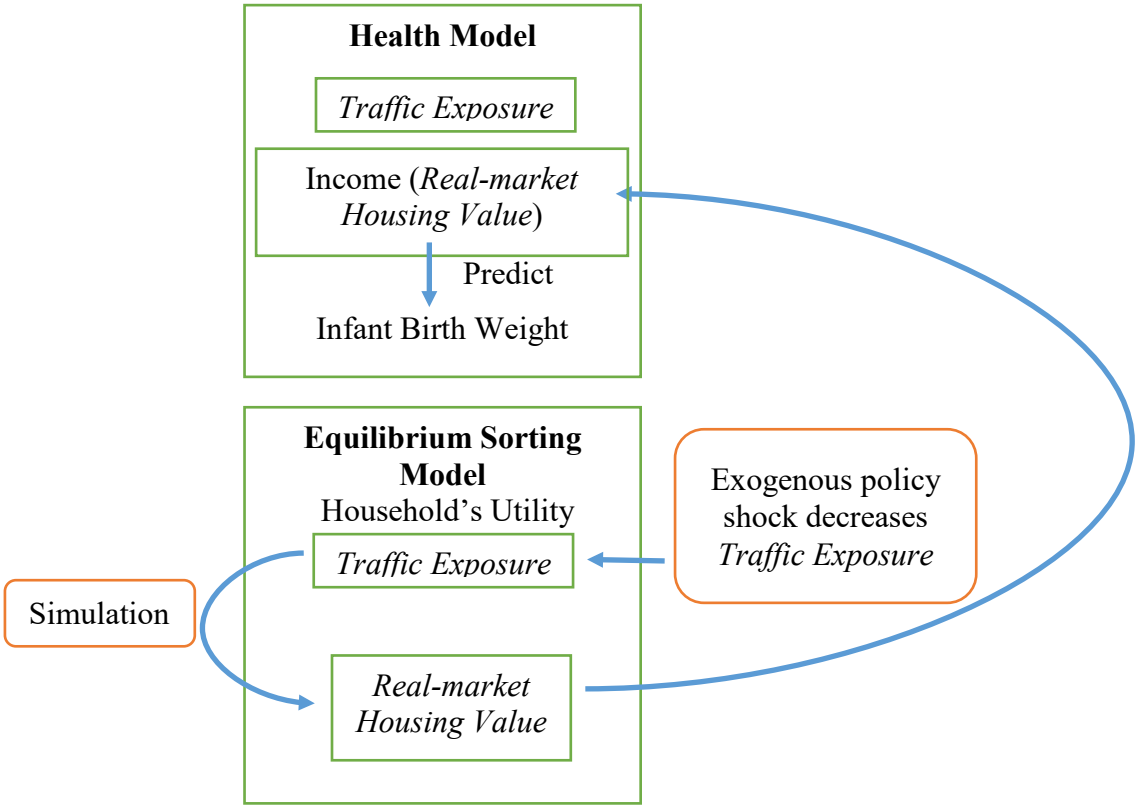
Figure 1. Single-Family Housing Transactions in Our Study Area



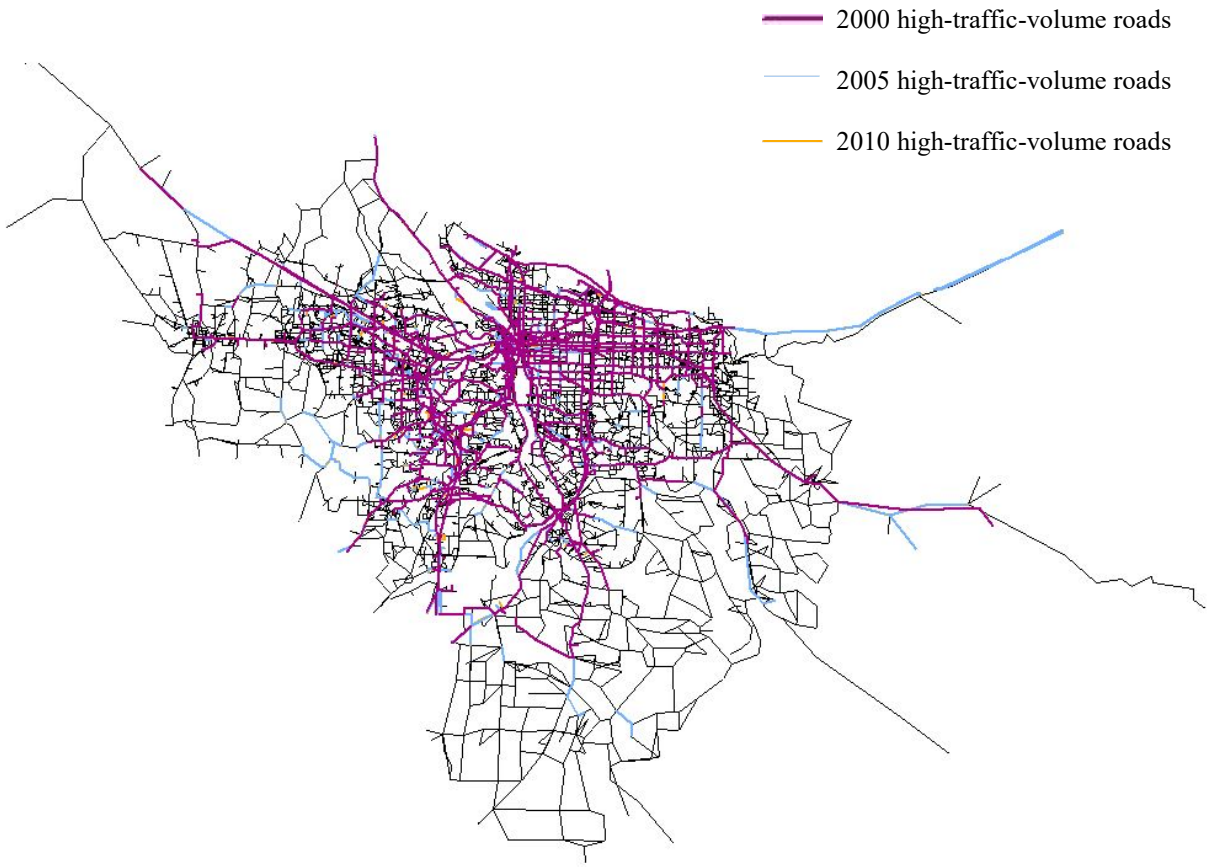
Notes: Figure 1 plots the single-family housing transactions (dark blue dots) in the Portland Metropolitan Area, including Clackamas (blue area), Multnomah (purple area), and Washington (yellow area) Counties from 2000 to 2014. The data comes from Portland Metro's Regional Land Information System (RLIS).

Figure 2. Empirical Approach Diagram



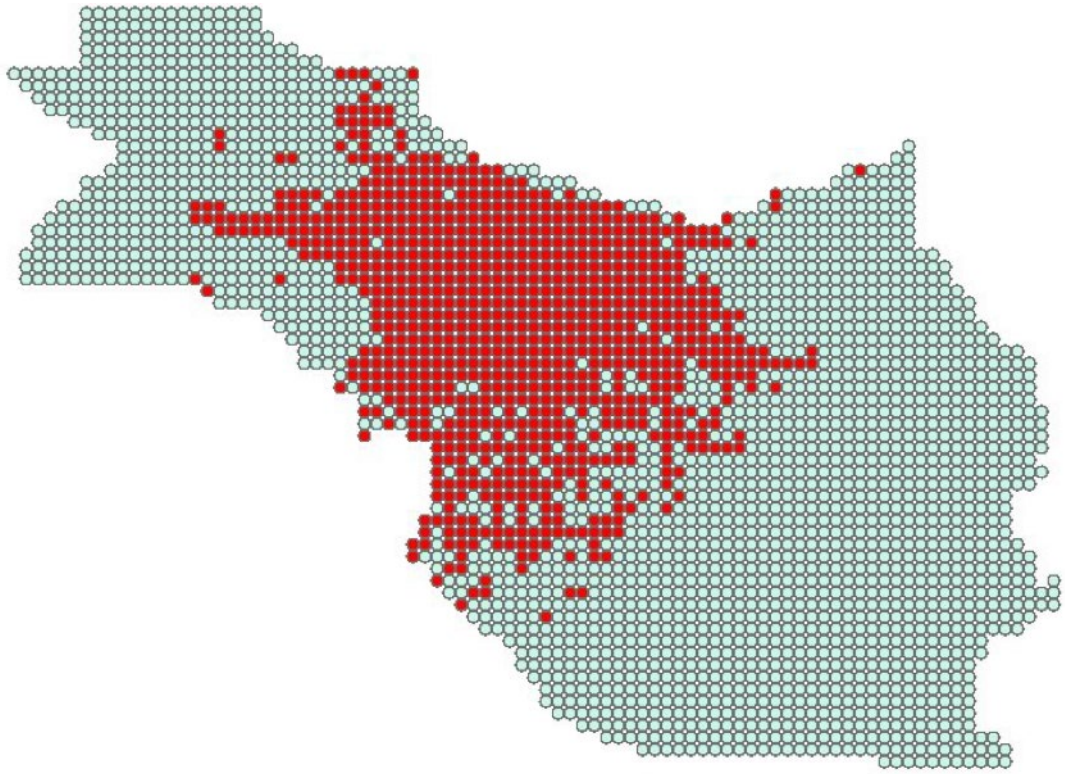
Notes: Figure 2 shows our estimation procedure. We separately estimate health and sorting models and then combine the results using the key variable, real-market housing value, to obtain the overall effect of traffic pollution on infant health.

Figure 3. Current Level of Traffic in 2000, 2005, and 2010 with Highlighted High-traffic Roads



Notes: Figure 3 highlights the high-traffic-volume roads in the years 2000, 2005, and 2010 in the Portland Metropolitan area. Exposed mothers are the ones who live within 300 meters from the highlighted high-traffic-volume roads.

Figure 4. Neighborhoods with Increased Housing Price after a 5% Pollution Reduction



Notes: Figure 4 shows neighborhoods (in red) with a projected housing price increase after a 5% decrease in traffic volume.

Table 1. Characteristics of the Portland Metropolitan Area, Oregon birth cohort 2000 - 2014 [in (%) or mean ± standard deviation], overall and according to exposure status

	All	Treatment	Control
Study population (n)	292,357	154,411	137,860
Birth outcomes			
Birth weight (gram)	3,376.88±575.38	3,367.77±576.27	3,387.10±574.21
Covariates			
Mother age	28.84±5.95	28.23±5.97	29.53±5.86
Baby Gender			
Female	131,749 (48.63)	76,120 (48.68)	55,629 (48.56)
Male	139,159 (51.37)	80,238 (51.32)	58,921 (51.44)
Marital Status			
Married	207,907 (71.11)	100,553 (65.95)	104,721 (76.91)
Missing	23 (0.01)	11 (0.01)	10 (0.01)
Mother education			
<= 8th grade	16,249 (5.56)	10,814 (7.09)	5,317 (3.90)
> 8th grade and up to high school	95,565 (32.69)	56,251 (36.89)	38,355 (28.17)
Some college (w/ Associate Degree)	73,049 (24.99)	37,254 (24.43)	34,649 (25.45)
College and up	104,535 (35.76)	46,539 (30.52)	56,518 (41.51)
Missing	2,959 (1.01)	1,614 (1.06)	1,322 (0.97)
Mother race			
White	193,929 (66.33)	93,489 (61.32)	97,972 (71.95)
Africa American	11,305 (3.87)	7,439 (4.88)	3,722 (2.73)
American Indian	2,090 (0.71)	1,232 (0.81)	832 (0.61)
Asian, Native Hawaian, and Pacific Islander	25,822 (8.83)	13,304 (8.73)	12,190 (8.95)
Hispanic	51,023 (17.45)	32,294 (21.18)	18,160 (13.34)
Other	889 (0.30)	493 (0.32)	359 (0.26)
Missing	7,299 (2.50)	4,221 (2.77)	2,926 (2.15)
Maternal smoking during pregnancy			
Non-smoking	266,190 (91.05)	136,916 (89.80)	125,884 (92.45)
Smoking	24,610 (8.42)	14,694 (9.64)	9,626 (7.07)
Missing	1,557 (0.53)	862 (0.57)	651 (0.48)
Maternal drinking during pregnancy			
Non-smoking	272,467 (93.2)	142,601 (93.53)	126,465 (92.88)
Smoking	17,688 (6.05)	8,697 (5.70)	8,668 (6.37)
Missing	2,202 (0.75)	1,174 (0.77)	1,028 (0.75)
Pregnancy/Chronic Diabetes	1,823 (0.62)	1,023 (0.67)	772 (0.57)
Gestational Diabetes	16,342 (5.59)	8,622 (5.65)	7,449 (5.47)
Pregnancy Hypertension	3,455 (1.18)	1,813 (1.19)	1,589 (1.17)

Gestational Hypertension	16,152 (5.52)	8,411 (5.52)	7,460 (5.48)
Eclampsia Hypertension	1,473 (0.50)	838 (0.55)	625 (0.46)
Previous Preterm Birth	7,681 (2.63)	4,241 (2.78)	3,314 (2.43)
Uterine Bleeding	3,331 (1.14)	1,797 (1.18)	1,442 (1.06)
Induction of Labor	81,717 (27.95)	42,369 (27.79)	38,466 (28.25)
Augmentation of Labor	52,803 (18.06)	28,315 (18.57)	23,807 (17.48)
Precipitous Labor	12,564 (4.30)	6,505 (4.27)	5,802 (4.26)
Prolonged labor	6,311 (2.16)	3,398 (2.23)	2,803 (2.06)
Fetal Intolerance	16,826 (5.76)	9,245 (6.06)	7,423 (5.45)
Breech Birth	10,641 (3.64)	5,510 (3.61)	5,014 (3.68)
Meconium	13,632 (4.66)	7,445 (4.88)	6,026 (4.43)
Premature Rupture	15,586 (5.33)	8,505 (5.58)	6,778 (4.98)
Number of Prenatal Care Visit	12.45±10.49	12.33±10.65	12.59±10.30
Maternal Weight Gain	30.95±13.87	30.80±14.18	31.12±13.51
Parity	168,954 (57.79)	86,355 (56.64)	80,332 (59.00)
Delivery Place			
Hospital Birth Center	284,943 (97.46)	148,683 (97.51)	132,572 (97.36)
Home (Mother's Residence)	1,989 (0.68)	1,135 (0.74)	833 (0.61)
Doctor's Office	5,224 (1.79)	2,552 (1.67)	2,662 (1.96)
Other	44 (0.02)	22 (0.01)	21 (0.02)
Missing	157 (0.05)	80 (0.05)	77 (0.05)
Attendant Type			
MD	219,457 (75.06)	112,577 (73.83)	103,866 (76.28)
Doctor's Office	9,433 (3.23)	4,996 (3.28)	4,338 (3.19)
Other Medical Personnel	3,538 (1.21)	1,904 (1.25)	1,603 (1.18)
N.D.	1,839 (0.63)	960 (0.63)	877 (0.64)
C.N.M	55,675 (19.04)	30,870 (20.25)	24,230 (17.80)
R.N	698 (0.24)	376 (0.25)	322 (0.24)
Noncertified Midwife	1,162 (0.40)	544 (0.36)	618 (0.45)
Other Specified Person	555 (0.19)	245 (0.16)	307 (0.23)
Payment Type			
Medicaid	95,526 (32.67)	60,364 (39.59)	33,918 (24.91)
Private Insurance	186,726 (63.87)	86,480 (56.72)	97,922 (71.92)
Self-pay	6,972 (2.38)	3,884 (2.55)	3,037 (2.23)
Other	2,467 (0.84)	1,403 (0.92)	1,020 (0.75)
Missing	666 (0.23)	341 (0.22)	264 (0.19)
Birth Month			
January	22,853 (7.82)	12,061 (7.91)	10,524 (7.73)
Feburary	22,199 (7.59)	11,681 (7.66)	10,238 (7.52)
March	24,714 (8.45)	12,895 (8.46)	11,562 (8.49)
April	24,280 (8.30)	12,576 (8.25)	11,366 (8.35)
May	25,814 (8.83)	13,497 (8.85)	11,985 (8.80)
June	25,097 (8.58)	12,988 (8.52)	11,798 (8.66)

July	25,913 (8.86)	13,401 (8.79)	12,169 (8.94)
August	25,601 (8.76)	13,298 (8.72)	11,959 (8.78)
September	24,914 (8.52)	12,952 (8.49)	11,624 (8.54)
October	24,475 (8.37)	12,745 (8.36)	11,383 (8.36)
November	22,868 (7.82)	12,021 (7.88)	10,558 (7.75)
December	23,629 (8.08)	12,357 (8.1)	10,995 (8.07)
Adjusted Building Value (in 1000s)	1466.10±3736.05	1981.48±4258.51	888.52±2940.42
Dist to High-traffic Volume Rd (ft)	1948.46±4516.37	453.71±271.19	3623.61±6153.41

Notes: Table 1 shows the summary statistics of the birth cohort in the Portland Metropolitan area in the years 2000 – 2014 (overall and conditional on exposure status). Real-market housing value statistics are obtained after joining vital stats data obtained from the Oregon Health Authority with the single-family and multi-family real-market value tax-lot data obtained from the Regional Land Information System (RLIS). Real-market housing value is adjusted for inflation using 2014 as the base year.

Table 2. Covariate Balance across Treatment and Control Groups Before and After Matching on Propensity Score for Selected Variables

Variable	Original Sample				Matched Sample		
	Mean Treatment	Mean Control	Standardized Difference (%)	p-value	Mean Control	Standardized Difference (%)	p-value
Mother Age	28.23	29.53	-21.80	0.00	28.21	0.50	0.20
Married	0.66	0.77	-24.50	0.00	0.66	-0.50	0.20
Mother Education							
> 8th grade and up to high school	0.37	0.28	18.80	0.00	0.37	-1.20	0.00
College and up	0.31	0.42	-23.00	0.00	0.30	0.70	0.06
Mother Race							
Africa American	0.05	0.03	11.30	0.00	0.05	0.40	0.29
Hispanic	0.21	0.13	20.90	0.00	0.21	-0.70	0.07
% African American	3.00	2.43	12.50	0.00	3.01	-0.30	0.32
% White	69.66	75.69	-40.80	0.00	69.25	2.70	0.00
% Native American and Other	9.60	8.24	26.70	0.00	9.95	-6.90	0.00
% Asian Pacific	5.54	5.04	11.20	0.00	5.18	8.00	0.00
% Latino	9.67	7.99	24.30	0.00	9.91	-3.40	0.00
% Children	20.01	21.85	-31.00	0.00	19.98	0.60	0.11
% Retirees	10.16	10.68	-11.40	0.00	9.98	4.00	0.00
# Bus Stop	39.17	21.77	50.90	0.00	37.08	6.10	0.00
% Multi-family Land	13.20	7.43	58.40	0.00	13.25	-0.50	0.20
% Commercial Land	5.09	3.55	20.90	0.00	4.55	7.40	0.00
Distance to Pedestrian Street (in 1000s)	0.17	0.24	-23.10	0.00	0.16	2.80	0.00
Distance to City Center (in 1000s)	42.43	56.34	-52.00	0.00	42.32	0.40	0.15
Adjusted Real-market Housing Value (in 1000s)	1,981.50	888.52	29.90	0.00	1,874.40	2.90	0.00
Distance to High-traffic-volume road (ft)[a]	453.71	3,623.61	NA	NA	2,300.16	NA	NA

Notes: Table 2 presents the standardized mean difference across treatment (exposed) and control (unexposed) groups before and after matching. The standardized difference is greatly reduced after matching, suggesting that the matching procedure effectively returns a balanced sample. *Absolute value of mean standardized difference above 10%.

a. Exposure status (treatment status) is defined by infants' distance to high-traffic volume roads. As a result, we cannot match the treated and control groups on the distance to high-traffic-volume roads variable. This is why the standardized difference and p-values are missing. We added the average distance to high-traffic-volume roads here to demonstrate that there is substantial variation across treatment and control groups.

Table 3. Selected Health Model Estimation Results

Dependent Variable	Log(birthweight)		
Independent Variables	Estimate		Robust Std. Err.
inc1*exp	-0.0106	***	0.0026
inc2*exp	-0.0019		0.0017
inc3*exp	-0.0050	***	0.0008
inc1*non-exp	0.0021		0.0022
inc2*non-exp	0.0006		0.0016
Mother Characteristics	Yes		
Pregnancy/Delivery Characteristics	Yes		
Neighborhood Characteristics	Yes		
Month, Year, Neighborhood Fixed Effects	Yes		
Observations	308,900		
R^2	0.1430		

Notes: Table 3 shows the regression of logged birth weight on the interactions between pollution exposure (exposed and non-exposed) and income groups. The baseline is the interaction between income group 3 (highest-income group) and the unexposed. *** p<0.01, ** p<0.05, * p<0.1.

Table 4. First Period Sorting Model Parameter Estimates

First Stage Estimation Results for interaction parameters between individual characteristics and housing and neighborhood attributes

Variables	Estimate	Std. Err.	
Bldg Square Footage-X-Household Size	28.676	0.0947	***
Distance to Park-X-Children	-4.4075	0.4283	***
Distance to Pedestrian Street-X-Children	1.7866	0.3167	***
Traffic Volume-X-Children	-14.6357	0.1191	***
Distance to City Center-X-Adult	16.5077	0.1118	***
% Park Area-X-Senior	2.9874	0.3554	***
Log-likelihood	-672,989.80		
Pseudo R^2	0.1547		

Sorting Model Second Stage Estimation Results Using 2SLS

Variables	Estimate	Std. Err.	
Constant	18.5009	1.3821	***
% African American	-7.4297	3.3088	**
% White	1.9242	1.4322	
% Native American and Other	-15.273	4.3053	***
% Asian Pacific	-3.1838	2.9838	
% Latino	5.0202	3.3447	
% Children	-0.7938	3.3791	
% Retirees	-1.3533	2.727	
Population Density (in 1000s)	-2.2764	0.6804	***
# Transportation Stop (in 100s)	3.9102	1.6232	**
% Park	1.8595	1.0597	*
Distance to Parks (in 1000s)	1.8459	1.0565	*
% Single Family Land	-1.3560	1.1984	
% Multi-family Land	-5.1363	5.8848	
% Industrial Land	10.0786	3.5527	***
% Commercial Land	6.5520	6.9701	
Distance to Pedestrian Street (in 1000s)	0.3452	1.1836	
Distance to City Center (in 1000s)	-20.5206	1.8281	***
Traffic Volume (in 1000s)	-0.2891	0.7251	
Housing Age	-3.9350	0.4925	***
Building Square Footage	29.7786	5.1291	***
Price (in 1000s)	-57.7063	6.8719	***

Notes: Table 4 reports the estimation results of the sorting model. The first-stage maximum-likelihood estimation divides the overall population into groups by household size, number of children, number of seniors, and number of working adults, reflecting households' heterogeneous preferences for a variety of neighborhood and housing attributes. The second-stage 2SLS estimation with an instrumental variable for housing price decomposes the mean indirect utility estimates recovered in the first-stage estimation into observable and unobservable components. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

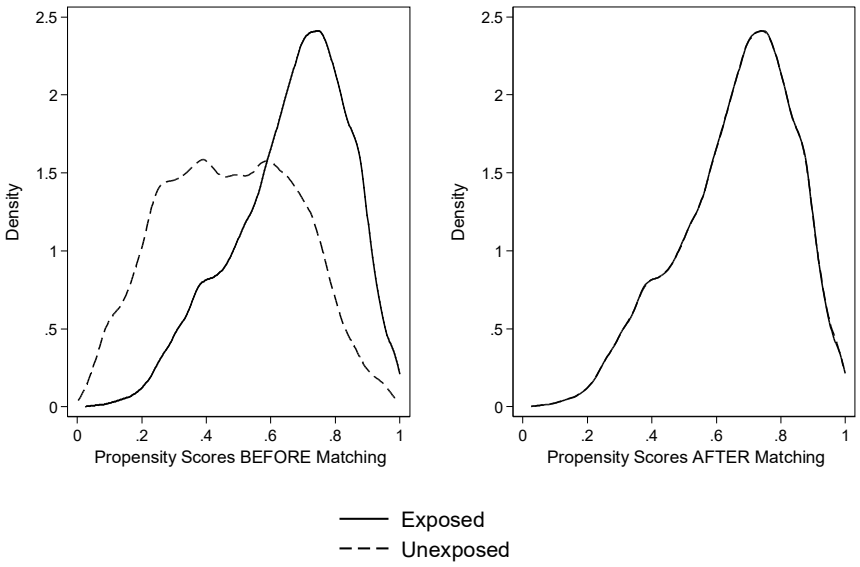
Table 5. 5%, 10%, and 20% Traffic Reduction Policy Simulation Results

Policy Scenario	# (%) of High-traffic-volume Roads	# (%) of Exposed Infants	# of individuals experienced exposure status change	% Change of Housing Price	Birthweight Change (%)
No Traffic Reduction	22,804 (25.01%)	157,712 (53.95%)	NA	NA	NA
5% Traffic Reduction	21,742 (23.85%)	154,497 (52.85%)	3,392	15.60%	0.52%
10% Traffic Reduction	20650 (22.65%)	152,597 (52.20%)	6,695	17.17%	0.50%
20% Traffic Reduction	18,260 (20.03%)	139,705 (47.79%)	14,792	19.06%	0.50%

Notes: Table 5 reports information related to the policy simulation results of the sorting model. High-traffic-volume roads are roads with traffic above 75 percentile. Exposed infants are those living 300 meters from the high-traffic-volume roads. The housing price change is obtained from the sorting model simulation. Birthweight changes are calculated using the predicted birth weight after housing price changes.

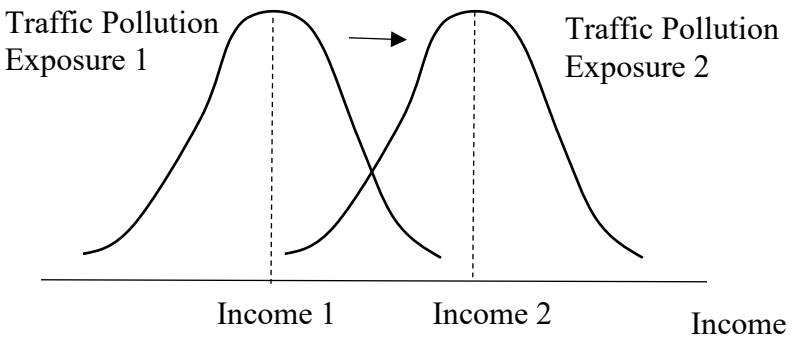
Appendix

Figure A1. Density Distribution of Propensity Score Before and After Matching (Traffic-related Pollution Exposure)



Notes: Figure A1 shows the density distribution of the propensity score for treatment and control groups before and after matching. It indicates that the common support condition is met, so that balance greatly improves after matching.

Figure A2. A Hypothetical Example of How a Neighborhood's Income Distribution is Affected by Sorting Response to a Reduction in Traffic-related Pollution from TRP1 to TRP2



Notes: Figure A2 illustrates our hypothesis of the link between a reduction in traffic pollution exposure and neighborhood income distributions. If individuals sort in response to a localized pollution change (from traffic pollution exposure 1 to traffic pollution exposure 2), the corresponding housing price adjustment implies a change in the localized income distribution from income distribution 1 to distribution 2. This in turn alters the estimated neighborhood health impacts of the pollution change, since the treatment effect of traffic pollution exposure in the health model depends on income.

Table A1. Sorting Model Data Summary Statistics: Household, Housing, and Neighborhood Characteristics of the Portland Metropolitan Area, Oregon 2000-2014

Variable	Mean	Std. Err.	Min	Max
Household Characteristics				
Household Size	2.73	0.51	1	8.50
# Children	0.71	0.34	0	5.60
# Adult	1.65	0.36	0	7.77
# Senior	0.24	0.22	0	5.12
Observations	505,332			
Housing Characteristics				
House Age	46.97	22.99	11.00	116.00
Bldg Square footage	1866.55	701.13	506.00	8949.00
Sale price (in 1000s)	217.88	91.53	25.00	927.50
Observations	505,332			
Neighborhood Characteristics				
% Black	1.08	2.92	0	41.46
% White	82.01	15.66	6.82	99.37
% Native American and Other	5.49	3.82	0	31.40
% Asian, Pacific Islander	2.72	3.26	0	23.36
% Latino	4.62	4.34	0	38.28
% Children	21.46	5.94	0.25	39.79
% Seniors	11.24	4.91	0	57.50
Population Density	162.94	126.23	0.74	991.31
# Bus Stop	14.42	25.98	0	260.33
% Park Area	7.16	11.63	0	98.22
Distance to Park (in 1000s)	4.12	6.69	0	48.22
% Single-Family Housing	1.98	8.48	0	82.17
% Multi-Family Housing	0.32	2.11	0	33.37
% Industrial	0.42	2.89	0	44.72
% Commercial	0.38	1.74	0	20.93
Distance to Pedestrian Street (in 1000s)	0.55	0.62	0	5.33
Distance to City Center (in 1000s)	73.91	38.29	4.23	247.89
Traffic Volume	645.52	661.16	0	3800.85
Observations	5,828			

Notes: Table A1 shows the summary statistics of household, housing, and neighborhood characteristics for births in the Portland Metropolitan area in the years 2000 – 2014. Data for household characteristics are obtained from the U.S. census. Household characteristics are not whole numbers because census block group level statistics are aggregated to the house type level, defined by house size, circular neighborhood location, and transaction time.

Housing characteristics are obtained from the tax-lot data in the Regional Land Information System (RLIS). Neighborhood characteristics are obtained from RLIS's land-use spatial data layers. The number of bus stops is not a whole number because they are aggregated to the house type level.

Table A2. Fixed Effects Regression of Census-tract Median Income on Real-market Housing value

Variables	Estimate	Std. Err.	
Constant	9.1766	0.1015	***
Log (Median Housing Value)	0.1634	0.0087	***
Observations	2,780		

Notes: Table A2 shows the regression results of census-tract level median family income on the median real-market housing value of mother's residence, suggesting real-market housing value and family income are highly correlated.

*** p<0.01, ** p<0.05, * p<0.1.

Table A3. Regression of Logged Birth Weight on Logged Distance to Exposure

Dependent Variable	log(birthweight)	
Independent Variables	Estimate	Robust Std. Err.
log(Distance to Pollution Exposure)	0.1632	0.0364***
Mother Characteristics	Yes	
Pregnancy/Delivery Characteristics	Yes	
Neighborhood Characteristics	Yes	
Real-market Housing Value	Yes	
Neighborhood Fixed Effect	Yes	
Observations	308,822	
Adjusted R^2	0.1444	

Notes: Table A3 shows the regression results of logged birth weight on the logged distance to pollution exposure, suggesting birth weight increases when mothers live further away from heavy traffic. Note we have multiplied the coefficient by 100 to improve readability*** p<0.01, ** p<0.05, * p<0.1.

Table A4. Health Model Results with Individual Fixed Effect and Neighborhood Fixed Effect Using the Panel Sub-sample

Dependent Variable	Log(birthweight)			
	（1）		（2）	
Independent Variables	Estimate	Robust Std. Err.	Estimate	Robust Std. Err.
		Repeated Birth Panel Sample		
Pollution Exposure	0.0002	0.0276	-0.0447	0.0344
Mother Characteristics	Yes		Yes	
Pregnancy/Delivery Characteristics	Yes		Yes	
Neighborhood Characteristics	Yes		Yes	
Real-market Housing Value	Yes		Yes	
Month, Year Fixed Effects	Yes		Yes	
Neighborhood Fixed Effect*s*	No		Yes	
Individual Fixed Effect*s*	Yes		No	
Observations	16,857		16,857	
Overall Adjusted R^2*	0.1432		0.1607	

Notes: Table A4 reports the estimation results of the health model using the repeated births panel subsample with individual fixed effects and neighborhood fixed effects. Column (1) shows the estimation results with the individual fixed effects. Column (2) shows the estimation results with the neighborhood fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table A5. Complete Health Model Estimation Results

Dependent Variable	Log(birthweight)		
Independent Variables	Estimate	Robust Std. Err.	
Constant	7.9493	0.0324	***
Mother Age	0.0003	0.0001	***
Male Baby	0.0309	0.0007	***
Marital Status			
Married	0.0124	0.0009	***
Missing	-0.0368	0.0360	
Mother Education			
<= 8th Grade			
> 8th Grade and Up to High School	-0.0099	0.0015	***
Some College (w/ Associate Degree)	-0.0012	0.0017	
College and Up	0.0011	0.0018	
Missing	0.0019	0.0046	
Mother Race			
White			
Africa American	-0.0528	0.0020	***
American Indian	0.0076	0.0042	*
Asian, Native Hawaiian, and Pacific Islander	-0.0446	0.0013	***
Hispanic	-0.0055	0.0011	***
Other	-0.0035	0.0055	
Missing	-0.0044	0.0021	**
Maternal Smoking			
Not smoking			
Smoking	-0.0478	0.0013	***
Missing	0.0210	0.0059	***
Maternal Drinking			
Not drinking			
Drinking	0.0021	0.0028	
Missing	0.0184	0.0034	***
Pregnancy/Chronic Diabetes	0.0143	0.0058	**
Gestational Diabetes	0.0138	0.0017	***
Pregnancy Hypertension	-0.0858	0.0048	***
Gestational Hypertension	-0.1124	0.0023	***
Eclampsia Hypertension	-0.1728	0.0083	***
Previous Preterm Birth	-0.0930	0.0029	***
Uterine Bleeding	-0.1108	0.0058	***
Induction of Labor	0.0381	0.0007	***
Augmentation of Labor	0.0231	0.0008	***

Precipitous Labor	-0.0303	0.0018	***
Prolonged labor	0.0421	0.0020	***
Fetal Intolerance	-0.0302	0.0018	***
Breech Birth	-0.1169	0.0032	***
Meconium	0.0446	0.0014	***
Premature Rupture	-0.0775	0.0023	***
Number of Prenatal Care Visit	0.0047	0.0001	***
Missing Number of Prenatal Care Visit	-0.4345	0.0125	***
Maternal Weight Gain	0.0025	0.0000	***
Parity	0.0348	0.0009	***
Delivery Place			
Hospital Birth Center			
Home (Mother's Residence)	0.0671	0.0045	***
Doctor's Office	0.0645	0.0041	***
Other	0.0684	0.0150	***
Missing	0.0091	0.0110	
Attendant Type			
MD			
Doctor's Office	0.0153	0.0017	***
Other Medical Personnel	0.0160	0.0041	***
N.D.	0.0055	0.0048	
C.N.M	0.0346	0.0007	***
R.N	-0.0260	0.0071	***
Noncertified Midwife	0.0209	0.0056	***
Other Specified Person	-0.0550	0.0110	***
Payment Type			
Medicaid			
Private Insurance	0.0008	0.0009	
Self-pay	-0.0041	0.0026	
Other	0.0157	0.0034	***
Missing	-0.0401	0.0108	***
% African American	0.0010	0.0004	**
% White	0.0006	0.0002	***
% Native American and Other	0.0003	0.0004	
% Asian Pacific	-0.0001	0.0005	
% Latino	0.0002	0.0004	
% Children	-0.0006	0.0004	
% Retirees	0.0004	0.0004	
Population Density (in 1000s # of individual)	0.0000	0.0000	
# Bus Stop	0.0000	0.0000	
% Single Family Land	0.0000	0.0001	
% Multi-family Land	-0.0004	0.0001	***

% Industrial Land	-0.0003	0.0001	***
% Commercial Land	0.0001	0.0001	
Distance to Pedestrian Street (in 1000s feet)	0.0172	0.0069	**
Distance to City Center (in 1000s feet)	0.0008	0.0002	***
inc1*exp	-0.0106	0.0026	***
inc2*exp	-0.0019	0.0017	
inc3*exp	-0.0050	0.0008	***
inc1*non-exp	0.0021	0.0022	
inc2*non-exp	0.0006	0.0016	
Month, Year, Neighborhood Fixed Effects	Yes	Yes	
Observations	308,900		
R^2	0.1430		

Notes: Table A5 shows the full regression of logged birth weight on various mother demographics including maternal pregnancy attributes, neighborhood characteristics, and the interactions between income group dummy variables and the binary pollution exposure status. The estimation controls for the neighborhood, month, year, and neighborhood fixed effects. *** p<0.01, ** p<0.05, * p<0.1.