LEAD AND JUVENILE DELINQUENCY: NEW EVIDENCE FROM LINKED BIRTH, SCHOOL, AND JUVENILE DETENTION RECORDS

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Abstract—Using a unique data set linking preschool blood lead levels, birth, school, and detention records for 125,000 children born between 1990 and 2004 in Rhode Island, we estimate the impact of lead on school suspension and juvenile detention. Sibling fixed-effect models suggest that omitted variables related to family disadvantage do not bias OLS estimates. However, measurement error does. We use IV methods that exploit local (within-neighborhood), variation in lead exposure deriving from road proximity and the deleading of gasoline. For boys, a 1 unit increase in lead increased the probability of suspension from school by 6% and detention by 57%.

I. Introduction

LEAD exposure in early childhood has been linked to diminished cognition, poor impulse control, inattention, and aggressive behavior (Needleman et al., 1990; Needleman & Gatsonis, 1991; Banks, Ferretti, & Shucar, 1997). Studies seek to identify the effects of childhood lead exposure on crime by exploiting the deleading of gasoline (Gronquist, Nilsson, & Robling, 2014; Mielke & Zahran, 2012; Reyes, 2007, 2015; Nevin, 2000, 2007).[1] These studies suggest that reductions in exposure to lead in early childhood could explain up to 90% of the sharp downward trend in crime in the United State that started in the mid-1990s. However, there are many competing hypotheses for the large decreases in crime, including abortion legalization (Donohue & Levitt, 2001), the decline of the crack-cocaine epidemic, changes in the availability of handguns, demographic shifts, changes in the demand for unskilled labor, improvements in policing, and increases in the prison population (see Blumstein & Wallman, 2000, for a discussion). Because so many of these factors covary, it can be difficult to distinguish the independent effect of any individual factor in a cohort analysis.

In this paper, we shift away from an exclusive focus on crime, a rare child outcome, by examining school disciplinary problems in addition to juvenile detention. Disciplinary problems are of interest in their own right as a precursor to school failure and dropout, as an outcome which can be observed in even relatively young children and as an indicator that may be predictive of future criminal behavior (Bernberg & Krohn, 2003; Mowen & Brent, 2016; Snyder & Sickmund, 2006). In our data, for example, children who have been suspended from school are 10 times more likely to be detained or incarcerated as adolescents or young adults. Juvenile incarceration has its own negative impact on the trajectories of affected adolescents, reducing the probability of graduating from high school and increasing the likelihood of recidivating as adults (Aizer & Doyle, 2015). By examining school disciplinary problems as well as juvenile detention, we provide a more nuanced picture of the effect of lead on antisocial behavior.

A second contribution is the use of unique individual-level data on 125,000 children born in Rhode Island between 1990 and 2004. Preschool blood lead levels (BLLs) are linked to birth records, school disciplinary records, and data on juvenile detentions at the level of the individual child. Even with individual-level data, the relationship between lead exposure and juvenile antisocial behavior may be confounded by both omitted variables and measurement errors in BLLs. Individual-level data on child BLLs and future behavior using within family and within neighborhood variation in exposure can help to address this limitation.

We develop multiple identification strategies, including both sibling fixed effects (FE) and instrumental variables (IV) techniques. While the sibling fixed effects address potential omitted variables related to family disadvantage, they do not address measurement error (and likely exacerbate it). To assess the degree of measurement error in BLLs, we exploit the fact that there are multiple measures of blood lead for the same child for 70% to 80% of our sample, or sometimes for multiple children in a family. Moreover, some types of measures (venous) are known to produce more accurate measures than others (capillary). Hence, we are able to compare estimates generated from better and worse measures of lead and also to instrument for one blood lead measure with another. We document that measurement error results in significant downward bias in ordinary least squares (OLS) estimation.

In order to address both omitted variable bias and the downward bias due to measurement error, we develop an IV strategy that exploits variation in lead exposure that derives from both variation in proximity to busy roads within a neighborhood and the deleading of gasoline. Most of the lead in car exhaust fell within 50 meters of the road, such that the soil near roads was historically more contaminated with lead than soil farther from roads (Milberg et al., 1980; Fu et al., 1989). For children born in the early 1990s, there is a strong relationship between a child's BLL and traffic on roads within 50 meters of the child's home. However, the amount of lead in soil has declined over time following the deleading of gasoline between 1979 and 1986, and so has the relationship

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[1] Other work has exploited the addition of lead pipes to municipal water systems (Feigenbaum & Muller, 2016; Ferrie, Rolf, & Troesken, 2012) as a source of variation in lead exposure to estimate the impact of lead exposure in early childhood on homicide rates and adult IQ, respectively.

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between road proximity and child BLLs. Among those born in 2004, BLLs vary little with proximity to traffic. Our instrument is an interaction between birth year and proximity to high-traffic roads within a neighborhood. This IV approach exploits the greater decline over time in child lead levels for those living in areas with more initial exposure to traffic. We show that over this time period, there was little change in the background characteristics of children living near high-traffic roads, providing some support for the exogeneity of the instrument.

We also show that over the sample time frame, air pollution in Rhode Island, (including that generated by automobile exhaust) changed very little, providing additional evidence that the effects we are estimating are not driven by changes in pollution more generally, but rather specifically reflect the changes in exposure to lead that we document in the BLLs. Finally, conditions do not appear to be improving more generally for those living near busy roads.

Both school disciplinary infractions and juvenile detentions rise with preschool BLLs. Since lead exposure is associated with family disadvantage, one might have expected OLS estimates to overstate lead's effects. However, our sibling FE estimates suggest relatively little bias due to omitted variables in OLS models. But measurement error in lead does appear to bias OLS (and sibling FE) estimates toward 0. As a result, IV estimates that address both confounding and measurement error yield larger estimates than OLS or sibling FE models. Thus, our results point to mismeasurement of lead exposure as an important source of bias that has been largely ignored in previous research.

The IV estimates suggest that a 1 unit increase in BLLs increased the probability of suspension from school by roughly 6% for boys. For girls, there are no statistically significant effects when either sibling FE or the IV method is used. For detentions, we again find significant effects for boys across all specifications. Our results are consistent with the large estimated effects of lead on crime in earlier cohort-based studies and support the hypothesis that reductions in BLLs may indeed have been responsible for a significant part of the observed decrease in antisocial behavior among youths and young adults in recent decades.

II. Background

Children absorb lead via ingestion or inhalation. Once the lead is absorbed, it impairs brain development and disrupts neurotransmitter function in ways that negatively affect cognition, attention, and short-term memory and reduce impulse control. Children are more susceptible to lead than adults. Among adults, nearly all lead is excreted within weeks, while among children, only one-third of the lead taken in will leave the body (U.S. Department of Health and Human Services, 2007).

Once lead is either ingested or inhaled, it enters the bloodstream and is deposited in soft tissues and organs, including the brain. It does not remain in the blood for long (the half-life of lead in blood is 36 days), suggesting that if exposure is episodic, a single blood lead measure could fail to capture all but recent exposures.

Historically, the two main sources of lead in the environment were gasoline and household paint. By 1971, mounting evidence of the negative health effects of lead culminated in the U.S. surgeon general's pronouncement that lead and childhood lead poisoning was a public health hazard (U.S. Surgeon General, 1972). In response, the federal government banned the addition of lead to household paint starting in 1978 and required phasing-out lead from gasoline over the period 1979 to 1986. Within a few years the amount of new lead in the environment fell back to levels observed in the 1930s (Laidlow & Filipelli, 2008).

Lead in automobile exhaust, because of its weight, generally fell within 50 meters of the road. Concentrations of lead in soil are highest in urban areas, and within urban areas, the soil is most contaminated near roadways (Batelle Memorial Institute, 1998; Lejano & Ericson, 2005; Filipelli et al., 2005). Children absorb this residual lead from soil mostly via inhalation through roadside resuspension, which is related to the amount of turbulence generated by weather or passing vehicles (Lough et al., 2005). Lead in the soil and air outside homes is responsible for roughly half of the lead found inside homes (Adgate, Rhoads, & Lioy, 1998).

In their review, Laidlaw and Filippelli (2008) conclude that "interior paint AND Pb-enriched soils are both harmful sources of Pb to children, with paint the likely culprit in cases of acute Pb poisoning, and soil an important source of Pb in the myriad examples of chronic Pb poisoning of urban children" (p. 2023). There are many cases of low-level lead exposure in our data and relatively few cases of what appears to be acute lead poisoning, leading us to focus on lead deposited in soil in this analysis.

Although lead itself is stable and does not break down, lead in the soil near roadways gets washed out, blown or tracked away, or covered by paving material or new soil deposits, posing less of a threat to children over time (Ming et al., 2012; Datko-Williams, Wilkie, & Richmond-Bryant, 2014).[2] The decline in bioavailable lead in soil over time provides a source of identifying variation that we will use to estimate the causal impact of lead on future behavior.

There is significant measurement error in BLLs that derives from three sources. First, the CDC sets an acceptable range for measurement error at 4 ug/dL or 10%, whichever is greater (Parsons & Chisolm, 1997). At BLLs below 10 ug/dL, this margin represents a considerable amount of noise. Second, the sample can be contaminated, which is more likely in blood drawn via a finger prick (capillary method) than a

[2]For example, Mielke, Gonzalez, Powell, Mielke (2016) examined thousands of soil samples from New Orleans, and found that Hurricane Katrina reduced the amount of lead in top soil, which resulted in declines in the amount of lead dust resuspended during summer months and dramatic declines in child BLLs. The reduction in lead found in topsoil was due to both runoff and the replacement of the top layer of soil with less contaminated soil.

needle (venous method) (Parsons, Reilly, & Esernio-Jenssen, 1997). Third, the half-life of lead in blood is approximately 36 days, so lead levels in blood indicate mainly recent exposure (National Research Council,1993). As we will show, the evidence suggests that in practice, measurement error leads to a considerable understatement of the effects of lead.

Nearly all of the literature on the relationship between lead and delinquent behavior can be categorized as epidemiological individual-level analyses and aggregate-level cohort analyses. Early epidemiological studies typically rely on small samples (200–400 children) and include controls or use case-control matching to limit confounding. Examples include Needleman et al. (2002), who find higher bone lead density among delinquents; Wright et al. (2008), who document a relationship between a child BLL at age 6 and arrest for violent crime between 19 and 24; and Dietrich et al. (2001), who find strong correlations between lead in early childhood and self-reports of delinquent behavior at ages 17 to 19.

Cohort analyses exploit variation in exposure to lead across space and time induced by policy changes, including the deleading of gasoline or the use of leaded pipes for water. Variation in lead exposure is usually at the level of the country, state, or municipality by year of birth. These studies do not link individual children's lead levels with future behavior, but rather relate potential exposure in an area to future behavior of the group potentially affected. For example, Reyes (2007, 2015) focuses on variations in the use of leaded gasoline across U.S. states and over time, relating them to behavioral problems in young children, teen pregnancy, aggression, and criminal behavior.

Gronquist et al. (2014) link variation in exposure to lead at the level of the neighborhood to long-term outcomes, including GPA, school completion, and crime, exploiting the deleading of gasoline in Sweden in 1980. One difference between their paper and ours is that they do not link individual child blood lead levels to outcomes, but rather exposure to lead as measured by neighborhood moss levels, since moss absorbs ambient lead from the air. They document larger negative effects of exposure to lead at a neighborhood level on all outcomes for boys and low-income children.

Two recent historical analyses have linked the introduction of lead drinking water pipes in municipalities to reduced IQ in young adulthood (Ferrie et al., 2012) or with violent crime (Feigenbaum & Muller, 2016). These studies are based on historical data from the early twentieth century when there was no information on child BLLs, even in the aggregate, so they cannot link lead pipes with increased lead levels in children. Instead, the authors estimate a reduced-form impact of exposure to lead pipes on outcomes.

A major advantage of these cohort-level studies relative to the epidemiological studies is the use of an exogenous source of variation in lead exposures. A disadvantage is that these studies are potentially subject to the problem of ecological fallacy—incorrect inference regarding individual behavior based on relationships observed in aggregate data. This problem may be particularly acute because the deleading of gasoline coincided with so many other factors that might be expected to affect crime.

Our use of individual-level data and variation in lead levels within a neighborhood enables us to overcome this challenge. The study using data most similar to ours is Billings and Schnepel (2018), who use individual-level data linking children's BLLs, school, and arrest records from Charlotte, North Carolina. They compare children with BLLs just above and just below a cutoff for treatment services and show convincing evidence of the effectiveness of medical, educational, and behavioral interventions among children with BLLs around that threshold. However, their analysis is not intended to measure the impact of lead exposure on schooling outcomes per se or to examine the effects of lower BLLs on outcomes.

Finally, Aizer et al. (2018) use similar data linking BLLs to test scores for younger children from Rhode Island and find that even low BLLs are associated with significant reductions in children's test scores, and that African American children are much more likely to be exposed.

III. Data

In order to estimate the relationship between preschool child BLLs and later problem behavior, we have created a unique data set for Rhode Island children born between 1990 and 2004. Data on each child's preschool BLLs from the Rhode Island Department of Health's (RIDOH) lead screening program (1994–2010) are linked to information from the Rhode Island Department of Education (RIDE) on in-school disciplinary infractions that resulted in suspension from school for school years 2007/2008 through 2013/2014. These data are then linked to information from the Rhode Island Training School, the state's juvenile detention facility (which houses juveniles up to age 17), as well as to data from all state correctional institutions (which house individuals age 18 and older). Data on detentions are available for 2004 to 2014.

The state of Rhode Island is ideal for studying the effects of lead on future outcomes because of the state's aggressive lead screening program. Reports from 2001 to 2011 indicate that over 70% of all children had at least one lead screen by 18 months, and the screening rate was roughly constant over this time period, ranging from 71% to 75% (Rhode Island Department of Health, 2011). Children on Medicaid and children living in Providence have slightly higher screening rates.[3]

In contrast, the national screening rate ranged from 22% (in 2002) to 30% (in 2010) (Raymond, Wheler, & Brown, 2014). In many jurisdictions, children are screened only if there is some reason to suspect lead poisoning, so that the sample of children who are tested is not representative of all children in the jurisdiction. Given the high screening rates in

[3] In Providence, nearly 90% of all children have a BLL by kindergarten entry (McClaine et al., 2013) and 80% of children on Medicaid in the state had been screened for lead by 36 months (Vivier et al., 2001).

Rhode Island, the sample with lead tests is much more similar to the statewide population of children. Moreover, because children without obvious symptoms were tested, we have a large sample of children with relatively low lead levels and can examine the effects of these low lead levels on outcomes.

BLLs were measured multiple times for most RI children between birth and the age of 6. Seventy-three percent of children with any BLL have multiple BLLs. The data for each test include the BLL, the age of the child in months, method of collection (capillary versus venous), and the child's address at date of measurement. Blood lead is not normally measured for children over 6 years old, so in all cases, we are examining the relationship between preschool BLLs and later juvenile or young adult outcomes.

To generate a summary measure of BLLs for each child, we calculate the geometric mean over all tests, a procedure that minimizes the influence of outliers and is consistent with the literature. Because we have on average three tests per child, this mean is more informative about lead burden during the child's first 72 months of life than a single measure of blood lead would be.

The data on disciplinary infractions include the reason for the infraction, the type of discipline, a scrambled school identification, year of infraction, student race, gender, ethnicity, and free lunch status for school years 2007/8 to 2013/14.[4] For each child we have, on average, five years of data on infractions. With these data, we construct a single summary outcome measure: whether the child was ever suspended. Roughly 20% of children in our sample were ever suspended compared to 24% who ever had any sort of disciplinary infraction. Our second outcome is whether the child was ever detained in the state's juvenile detention center or state correctional facility. In our sample, 1.1% of all children were ever detained or incarcerated (1.8% of boys). When we examine this measure, we limit our data to children born between 1991 and 1999 because few younger children are detained or incarcerated.

For a subset of these records, those born in the state of Rhode Island after 1996, we also have linked vital statistics natality data (birth certificates). The natality data include child birthweight, birth order, maternal marital status, age, and education at time of birth. They also include a maternal identifier that allows us to identify roughly 28,000 siblings in the data.

Online appendix table 1 provides an overview of our data. There are significant disparities in BLLs by race and income, just as in national data (Raymond et al., 2014). For all children, the average preschool BLL of children in our sample is 3.8 micrograms per deciliter (ug/dl); for white children, the average is 3.4 ug/dl, while black and Hispanic children have levels of 5.3 ug/dl and 4.5 ug/dl, respectively. The best indicator of family income that we have is whether the child was observed to receive a free school lunch, since only children from families with incomes less than 130% of the federal poverty line are eligible. If we categorize the sample by whether children were ever observed to receive a free school lunch, the average BLL of free lunch children is 4.5 ug/dl compared with an average BLL of 3.0 ug/dl among students who always paid for their lunch. Note that BLLs of less than 5 ug/dl (10 in the earlier years of our data) were unlikely to be treated in any way over most of our sample period.

These overall means mask important trends in both lead levels and disparities over time. Over time, both average lead levels and disparities by race and income have declined considerably (see appendix figures 1 and 2). Among children born in 2004, the average lead level had declined by 3.2 ug/dl compared to children born in 1990, the black-white gap between the two cohorts had shrunk from 3.6 to 0.8 ug/dl, and the income gap (free lunch versus paid) had shrunk from 2.7 to 0.9 ug/dl.

Overall in our sample, 20% of children have ever been suspended, though boys are suspended at a higher rate than girls (25% versus 14%). There exists a strong racial and ethnic gradient in suspensions, with African American (36%) and Hispanic (29%) children more likely to be suspended than whites (15%). Children who receive free lunch are also much more likely to be suspended than those who paid for lunch (29% versus 9%).

Disparities in rates of juvenile detention by gender, race, and income are even greater. Rates of juvenile detention are very low in the full sample, at only 1.1%: 1.8% for boys and 0.3% for girls. These rates rise to 3.5% among African Americans, and 5.9% among African American boys. One sees similar gaps in juvenile detention between free lunch and paid lunch children with rates of detention 10 times higher for free lunch boys: 3.2% vs. 0.3%.

The dramatic declines in lead levels over time were accompanied by large declines in disciplinary infractions that were likewise greater for African American children and free lunch children (appendix figures 3A and 3B).[5] Thus, over time in Rhode Island, overall levels of childhood lead exposure, suspensions, and detentions have declined, as have racial- and income-based disparities in these outcomes.

The rest of appendix table 1 traces out the familiar pattern of disadvantage that one might expect. In addition to higher lead levels, suspensions, and detentions, African American and free lunch children tend to have less-educated mothers, younger mothers, single mothers, lower birthweight, and more siblings, as reflected in a higher birth order. These differences highlight the challenges involved in separating the effects of lead from the effects of other disadvantages.

In order to measure a child's potential exposure to residual lead in soil near roads, the data on child blood levels were

[4]For all infractions (infractions leading to suspension), 37% (36%) are for disorderly conduct or disrespectful behavior, 10% (15%) for fighting or assault, 28% (21%) for skipping school or detention, 4% (6%) for threats or harassment, and 1% (2%) for drugs. Fifty-five percent of infractions resulted in suspension.

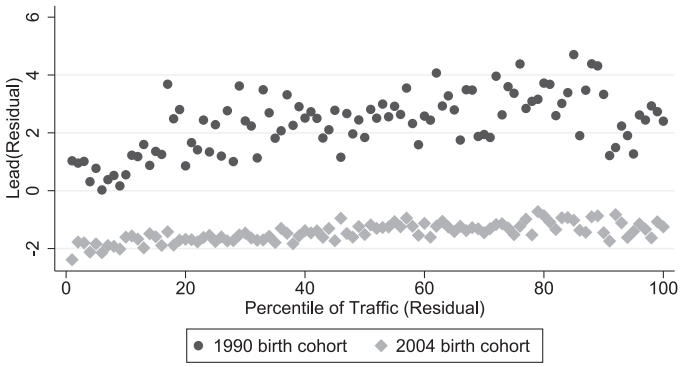
[5]Middle school students have the highest rates of disciplinary infractions, though this may reflect high rates of dropout in higher grades (assuming that the most disruptive students drop out). See appendix figure 3B.

TABLE 1.—ROAD DENSITY AND CHILD DISADVANTAGE

Area of Comparison	State	Municipality	Census Tract	Block Group
Share White				
High-traffic area	0.591	0.701	0.711	0.712
Low-traffic area	0.865	0.755	0.746	0.746
Difference	−0.274	−0.0539	−0.0353	−0.0345
Share paid lunch				
High-traffic area	0.335	0.424	0.438	0.440
Low-traffic area	0.637	0.548	0.534	0.531
Difference	−0.302	−0.124	−0.0961	−0.0908

High (low) traffic is defined as having traffic in the top (bottom) quartile of the distribution of traffic in the state. Neighborhood is defined by municipality (column 2), census tract (column 3), or census block group (column 4). Average levels of advantage are derived from regressions of share white or share paid lunch on an indicator variable for high-traffic area and a constant. The sample consists of children in the lowest and highest quartiles of traffic density in the state. Means presented in column 1 are derived from this simple OLS regression. In columns 2 to 4, we add fixed effects to the OLS regression in the form of municipality FE (column 2), census tract FE (column 3), and census block group FE (column 4).

FIGURE 1.—CHILD LEAD LEVELS AND TRAFFIC, BY BIRTH COHORT



Residuals from separate regressions of lead and traffic on census block group FE for children born in 1990 and 2004.

matched, based on the child's geocoded address at the time of each test, to data on Rhode Island roads from the RIGIS E-911 data for the period 2001 to 2014. For each child and blood test, we measure the number of meters of road by road type within a radius of 25 meters and between 25 and 50 meters of the child's home. We multiply the number of meters of each road type with measures of average traffic per road type based on 1980 national traffic data, with roads within 25 meters receiving twice as much weight as those 25 to 50 meters from the home and roads beyond 50 meters receiving no weight. We then calculate a measure of average traffic over the child's multiple addresses, with each address equally weighted as we do not know how long a child resided at each address. The appendix describes the construction of this measure in greater detail.

There is considerable variation in road density and traffic volume across the state even within a neighborhood. The median coefficient of variation in traffic within a census block group is 0.64 and ranges from 0.29 to 2.3. Appendix figure 4 shows a map of a single Rhode Island census tract made up of five census block groups to illustrate how this measure of traffic exposure varies even across homes within a census block group. In the figure, each circle represents a home. Circle radius indicates traffic volume within 50 meters of the home, with larger circles indicating greater traffic volume. The traffic volume for each home is a function of the number of meters of road within 50 meters and the type of road, with highways generating the most traffic, town streets the least, and state and county roads somewhere in between.

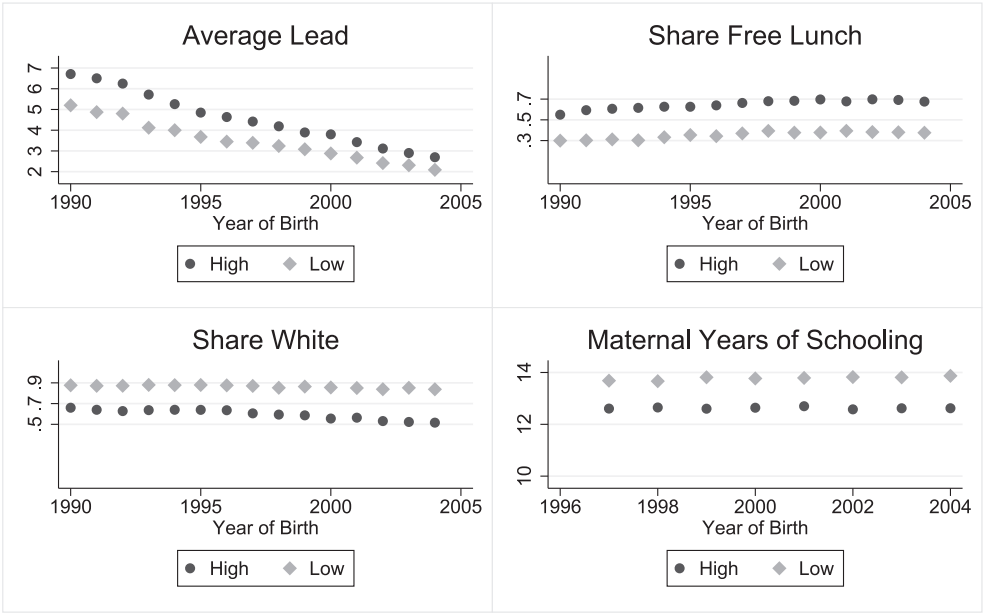
Table 1 shows that disadvantaged children are more likely to live in high-traffic neighborhoods. When we compare the race and free lunch status of children who live in high-traffic (top quartile) versus low-traffic (bottom quartile) census block groups, we find that high-traffic areas are 59% white and 33% paid lunch relative to low-traffic areas, which are 87% white and 64% paid lunch, a difference of 28 (white) and 31 (paid lunch) percentage points (table 1, column 1). Much of this difference is due to the fact that disadvantaged children live in more urban parts of the state. When we condition on municipality (of which there are 39 in the state), the difference in racial and income composition by traffic exposure declines from 28 and 31 percentage points, respectively, to 6 and 13 percentage points (table 1, column 2). If we condition on census tract or census block group, the differences decline further still, to 3 and 9 percentage points, respectively, as shown in columns 3 and 4. Clearly, conditioning on neighborhood significantly reduces differences in child characteristics across high- and low-road density areas, though it does not entirely eliminate the differences.

Because conditioning on neighborhood does not eliminate average differences in child characteristics between high- and low-traffic exposure homes and because of the potential for endogenous sorting between high- and low-traffic areas, we do not simply use spatial variation in traffic exposure within neighborhoods for identification. Rather, we exploit changes over time in lead exposure in high- versus low-traffic areas within a neighborhood. More specifically, the identifying assumption is that traffic exposure is more predictive of a child's BLLs early in the sample (i.e., for those born in the early 1990s) than for those born later in the sample (in the early 2000s).

Figure 1 shows the relationship between traffic exposure (measured in percentiles) and child preschool BLLs for children born in 1990 and those born in 2004. In order to focus on differences in traffic exposure within a neighborhood (defined as a census block group), we first regress lead levels and traffic measures on neighborhood fixed effects and then plot the residuals from these two regressions. The residuals capture variation in both lead levels and traffic within a neighborhood. The relationship between within-neighborhood traffic volume and child preschool BLLs (the slope) is steeper for the older children than the younger children in our sample. In other words, child BLLs increase significantly with road proximity for those born in the early 1990s, while for those born in 2004, the relationship between road proximity and BLLs is much weaker. This weakening relationship over time is consistent with the hypothesis that initially high lead levels in areas near roads have declined over time.

For the IV exclusion restriction to hold, it must be the case that the variation in lead exposure over time within

FIGURE 2.—TRENDS IN CHILD CHARACTERISTICS, BY TRAFFIC DENSITY



High(low) traffic defined as top(bottom) quartile of traffic volume in the state. Maternal education available only for children born after 1996.

high-traffic areas is uncorrelated with underlying changes in the characteristics of children living in these areas that might also affect outcomes. In other words, the declines in children's BLLs cannot be correlated with changes in the underlying composition of the children that would predict probabilities of suspension or detention.

Figure 2 plots time trends in lead levels and child characteristics by birth cohort and traffic volume (top quartile versus bottom quartile of traffic volume). The differences in the average lead levels of children with the highest versus lowest traffic exposure are initially large but converge over time (top left panel of figure 2). In contrast, while the underlying characteristics of children (race, income, and maternal education) differ in high-traffic areas relative to low-traffic areas, both the levels and the differences between high- and low-traffic areas are fairly constant over time (top right and bottom panels of figure 2). We conclude that the convergence in lead levels for children living in high-versus low-traffic areas is not driven by changes in the underlying characteristics of the children living in these areas.

Another possible explanation for the steeper declines in lead levels among children living in high-traffic areas is that conditions are simply improving generally for disadvantaged children, who happen to live in high-traffic areas of the state. We explore this idea in appendix figure 5, where we show that even among the most advantaged children in the state, those in high-traffic areas start with higher levels of blood lead, which converge over time to the BLLs of advantaged children living in low-traffic areas.[6] Thus, our results are not driven by contemporaneous policies mainly benefiting disadvantaged children.

Finally, could the declines in lead levels that we observe simply reflect more general improvements in environmental conditions over this period? To rule out this possibility, we provide two pieces of evidence. First, appendix figure 6 presents trends in air quality in Rhode Island over time (1990–2013) as measured by the Environmental Protection Agency's Air Quality Index (AQI), a composite of measures of ozone, carbon monoxide, particulates, nitrous oxides, and sulfur dioxide taken from air quality monitors in the state. These data are available for three of the state's five counties: Providence (65% of all children in Rhode Island), Kent (14%), and Washington (10%), covering 89% of the state's population of children. An AQI below 50 is considered "good" air quality by the EPA. The figure shows quite clearly that the median AQI has always been below 50 in Rhode Island and does not show any noticeable time trend over this period for the state overall and for Providence County specifically—the most populous county in the state and the one with the most traffic. In addition, we show that birthweight, which is sensitive to other types of air pollution (Aizer & Currie, 2014), does not differentially improve over time for children near roads. While in theory, maternal exposure to lead can affect newborn health, in practice most epidemiological evidence suggests little to no relationship (see Taylor, Golding, & Edmond, 2015), table S2, for a summary of the existing findings).

[6]To generate a summary measure of disadvantage that captures the elements of family background most relevant to lead exposure, we first use data from 1990/91 to predict average BLLs from children in this cohort based on race, ethnicity, free lunch status, and neighborhood FE. We then use the estimated coefficients to predict a BLL for every child in the sample. "Advantaged" children are those in the bottom quartile of the distribution of predicted lead. These are the children whose family background characteristics make it unlikely for them to have been exposed to high lead levels.

IV. Estimation

We first present OLS estimates of the relationship between preschool BLLs, suspensions, and juvenile detentions based on the following equation:

$$\begin{aligned} Y_i = {} & \beta_0 + \beta_1 Lead_i + \beta_2 X_i^1 + \beta_3 X_i^2 \\ & + \beta_4 Years\ of\ Infraction\ Data_i + \beta_5 Year\ of\ Birth \\ & \times Race_{it} + \beta_6 Year\ of\ Birth^2 \times Race_{it} \\ & + \beta_7 Year\ of\ Birth \times Free\ Lunch_{it} + \beta_8 Year\ of\ Birth^2 \\ & \times Free\ Lunch_{it} + \tau_n + \tau_t + \varepsilon_i \end{aligned} \quad (1)$$

Each observation is a child, and Y is an indicator for ever suspended or juvenile detention or incarceration; *Lead* is the geometric mean of the child's multiple BLLs measured before the start of school; X^1 is a vector of child characteristics that we have for all children (gender, race, ethnicity, and free lunch status); X^2 is a vector of child characteristics that we have only for children born in Rhode Island after 1996 (maternal education, birthweight, birth order, maternal age, and marital status at birth); *Years of Infraction Data* is a vector of indicator variables for the number of years for which we have infraction data for the child, which ranges from 1 to 8, with an average of 5. We also include a vector of neighborhood fixed effects, τ_n, year of birth fixed effects, τ_t, and *Race* and *Free Lunch* quadratic time trends to address the concern that any improvements in outcomes could simply reflect generally improving circumstances over time for the most disadvantaged children. For the juvenile detention outcome, rather than race and free lunch quadratic time trends, we interact race and free lunch state with year-of-birth indicator variables because of the strong national nonlinear trends in crime over this period.

A. *Exploring Bias from Omitted Variables Using Sibling Fixed-Effects Models*

For a subset of our sample, we can include sibling fixed effects. This analysis is limited to the sample of children born in Rhode Island in or after 1997 with at least one sibling also born in the state after 1997—roughly 34,000 children. The characteristics of the sibling subsample are very similar to the overall sample of children born in 1997 or later (see appendix table 1, columns 7–8), suggesting a subsample that it is representative of the general population of children. Discordance in lead levels across siblings is driven by residential moves (two-thirds of the sample moves between children) and the natural decay of lead in the environment over time. In order to keep the maximum number of sibling pairs, we do not require the lead measures to be taken at the same time, so that they could have been drawn at any time over our sample period (subject to the constraint that only children under 6 are tested for lead). In addition to the sibling FE, we also include controls for year of birth, birth order, gender, birthweight, and the number of years of infractions data available for each child.

B. *Exploring Bias from Measurement Error in Blood Lead Levels*

We attempt to assess the direction and degree of bias arising from measurement error by exploiting the fact that we have multiple lead measures for each child, and even for multiple children within a family. We conduct two exercises. First, we compare estimates based on more and less accurate measures of lead. In particular, for a small subset of the children, we have both capillary and venous BLLs, and it is well established that capillary measures are less accurate. For a larger subset of the children, we have multiple measures per child taken at different times over the child's first six years of life. For these children, we can compare estimates based on a single random draw with those based on the average of multiple draws for the same child, with the latter being a considerably less noisy measure of a child's exposure to lead over the first six years of life.

Another means of assessing the direction and magnitude of bias from measurement error is to instrument for one measure of lead with another measure of lead, following the approach of Ashenfelter and Krueger (1994). With our data, we can instrument for a single BLL measure with another single BLL measure for the child, for the capillary measure with the venous, and for one child's average BLL with his or her sibling's average BLL. Comparing the magnitudes of the OLS and IV estimates in these cases provides evidence regarding the direction and magnitude of the bias in OLS estimates. For this, one need not assume that the instrument represents a less noisy measure of the child's lead exposure, only that the measurement errors in the two BLL measures are uncorrelated with the signals and with each other, a reasonable assumption in this case.

C. *Instrumental Variable Estimates*

Our instrumental variables strategy exploits declines in the residual lead near roads after the deleading of gasoline to identify the estimated impact of preschool child BLLs on future behavior. Our instrument is the natural log of traffic volume interacted with a linear year of birth variable. The first-stage equation is:

$$\begin{aligned} Lead_i = {} & \alpha_0 + \alpha_1 X_i^1 + \alpha_2 X_i^2 + \alpha_3 \ln(Traffic\ Volume)_i \\ & + \alpha_4 \ln(Traffic\ Volume) \times Year\ of\ Birth_{it} \\ & + \alpha_5 Years\ of\ Infraction\ Data_i + \alpha_6 Year\ of\ Birth \\ & \times Race_{it} + \alpha_7 Year\ of\ Birth^2 \times Race_{it} \\ & + \alpha_8 Year\ of\ Birth \times Free\ Lunch_{it} \\ & + \alpha_9 Year\ of\ Birth^2 \times Free\ Lunch_{it} \\ & + \tau_n + \tau_t + \mu_i, \end{aligned} \quad (2)$$

TABLE 2.—ORDINARY LEAST SQUARES EFFECTS OF LEAD ON THE PROBABILITY OF SUSPENSION AND OF INCARCERATION

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
					Any Suspension			Any Detention/Incarceration			
Geometric mean of lead	0.0146 [0.0005]	0.0089 [0.0005]	0.0084 [0.0005]	0.0104 [0.0005]	0.0087 [0.0006]	0.0059 [0.0006]	0.0020 [0.0002]	0.0016 [0.0002]	0.0015 [0.0002]	0.0014 [0.0002]	−0.0006 [0.0002]
Lead × male						0.0056 [0.0008]					0.0038 [0.0004]
Male	0.1070 [0.0021]	0.1090 [0.0020]	0.1100 [0.0021]	0.1000 [0.0023]	0.1000 [0.0023]	0.0875 [0.0032]	0.0142 [0.0007]	0.0142 [0.0007]	0.0141 [0.0007]	0.0136 [0.0007]	−0.0025 [0.0015]
Observations	124,579	124,579	121,290	80,079	80,079	124,579	70,681	70,681	70,681	70,533	70,681
R^2	0.128	0.185	0.198	0.182	0.191	0.185	0.019	0.025	0.039	0.047	0.042
FE	Municipality	Block group	Block group and school	Block group	Block group	Block group	Municipality	Municipality	Block group	Block group	Block group
Birth cohorts	1990–2004	1990–2004	1990–2004	1997–2004	1997–2004	1997–2004	1991–1999	1991–1999	1991–2004	1991–2004	1991–2004

Columns 1 to 6 present regressions of an indicator for any suspension on the child's geometric mean of BLL before age 6. In column 1, we include only controls for year of birth, municipality, and gender. In columns 2 to 5 we add controls for gender, race, ethnicity, and free lunch status. In column 5, we include additional controls available only for children born in Rhode Island in 1997 or later that derive from birth certificate data and include maternal age, education, marital status, and child birth order and birthweight. In column 4, we do not include those additional controls but do limit the sample to that used in column 5. Columns 7 to 11 present estimates from a regression of an indicator for any detention or incarceration on the same controls but for the sample of children born 1991 to 1999. All regressions include year-of-birth fixed effects, birth order fixed effects, 801 census block group fixed effects, race and free lunch–specific quadratic time trends, and controls for the number of years with infraction data. There are 39 municipalities and 8 of census Block groups. Robust standard errors in brackets are clustered at the level of the census tract.

where *Lead* is the child's average BLL. The excluded instrument is ln(*Traffic Volume*) × *Year of Birth*. The terms representing the main effects of ln(*Traffic Volume*) and *Year of Birth* are included as controls in both the first and second stages. All other controls in equation (2) are as defined as in equation (1).

The second-stage model is given by the following equation:

$$\begin{aligned} Y_i = {} & \gamma_0 + \gamma_1 Lead_i + \gamma_2 Lead \times Male_i + \gamma_3 \boldsymbol{X}_i^1 + \gamma_4 \boldsymbol{X}_i^2 \\ & + \gamma_5 Years\ of\ Infraction\ Data_i + \gamma_6 Year\ of\ Birth \\ & \times Race_{it} + \gamma_7 Year\ of\ Birth^2 \times Race_{it} \\ & + \gamma_8 Year\ of\ Birth \times Free\ Lunch_{it} + \gamma_9 Year\ of\ Birth^2 \\ & \times Free\ Lunch_{it} + \gamma_{10} \ln(Traffic\ Volume)_i \\ & + \tau_n + \tau_t + \vartheta_i, \end{aligned} \tag{3}$$

where all the terms are as defined above and lead is instrumented using the predicted lead level from equation (2). *Lead* × *Male* is instrumented using predicted lead interacted with male. An interaction between lead and gender is included given the large differences in disciplinary infractions and detention by gender (table 1). This strategy of using the predicted lead level from equation 2 interacted with an indicator for male to construct an instrument for *Lead* × *Male* follows Wooldridge (2010).

V. Results

Table 2 shows OLS estimates of equation (1) with increasing numbers of controls as one moves across the columns. In column 1, we include only gender, year of birth, and municipality FE. Column 2 includes fixed effects for census block groups and controls for child characteristics (race and free lunch status and their interactions with quadratic time trends). The estimated coefficient falls by roughly one-third. The estimate of 0.0089 implies that each 1 unit increase in average blood lead increases the probability of a suspension by about 1 percentage point on a baseline of 20%. Column 3 adds fixed effects for schools in addition to census block groups. This addition reduces the estimated effect of lead only slightly. Finally, column 5 shows the effect of adding additional controls for family background and birthweight. This model is estimated using the subsample for whom we have this information (those born 1997 and after in Rhode Island). For this reason, in column 4, we present estimates on this latter sample but without the controls for comparison. The estimate without controls is 0.0104, and the estimate with controls is 0.0087. This represents a decline of 15%, which we consider relatively small given that the characteristics birthweight, birth order, maternal education, and marital status have been found to be highly predictive of child outcomes in previous work. Because of the large disparities in behavior by gender, we ask whether the relationship between lead and behavior varies by gender and find that the relationship is two times stronger for boys than for girls (0.006 versus 0.012, table 2, column 6).

Columns 7 to 11 of table 2 perform the same exercises for the probability of any detention or incarceration on the sample of those born 1991 to 1999. Once again, the results are quite stable when additional sets of controls are added, suggesting that a fairly basic set of background variables may be sufficient to control for potential confounding. The point estimate of 0.0014 (column 10, table 2) on a baseline of 0.01 suggests that an increase of 1 unit in average BLLs increases the probability of detention or incarceration by 15%. As with the suspension results, the relationship between lead and detention differs significantly by gender, though in this case, there is no significant relationship for girls and a stronger relationship for boys (0.004 on a baseline of 1.8%).

The appendix discusses a second way to test for the importance of omitted variable bias in the OLS estimates, which exploits the decline in negative selection into lead exposure over this period. If observables and unobservables are positively

TABLE 3.—COMPARING ESTIMATES THAT ADDRESS BIAS FROM OMITTED VARIABLES AND MEASUREMENT ERROR

	(1) OLS	(2) OLS	(3) Sibling FE	(4) OLS	(5) OLS	(6) IV	(7) IV
A. Any Suspension							
Geometric mean of lead	0.0059	0.0020	−0.0015				−0.0084
	[0.0006]	[0.0013]	[0.0021]				[0.0206]
Lead × male	0.0056	0.0125	0.0122				0.0161
	[0.0008]	[0.0018]	[0.0024]				[0.0022]
First random draw				0.0032	0.0032	0.0096	
				[0.0005]	[0.0005]	[0.0009]	
First random × male				0.0038	0.0036	0.0067	
				[0.0006]	[0.0007]	[0.0010]	
Male	0.0885	0.0535	0.0576	0.0940	0.0963	0.0810	0.0492
	[0.0032]	[0.0059]	[0.0082]	[0.0029]	[0.0034]	[0.0005]	[0.0067]
Observations	124,579	28,040	28,040	124,579	90,700	90,700	124,371
R^2	0.179	0.250	0.663	0.184	0.194		
Effect of 1 ug/dl lead for girls	0.0059	0.0020	0.0000	0.0032	0.0032	0.0096	0.0000
Effect of 1 ug/dl lead for boys	0.0115	0.0145	0.0122	0.0070	0.0068	0.0163	0.0161
B. Juvenile Detention or Incarceration							
Geometric mean of lead	−0.0006						0.0090
	[0.0002]						[0.0111]
Lead × male	0.0038						0.0133
	[0.0004]						[0.0011]
First random draw				−0.0005	−0.0005	−0.0005	
				[0.0001]	[0.0002]	[0.0003]	
First random × male				0.0024	0.0024	0.0053	
				[0.0003]	[0.0003]	[0.0004]	
Male	−0.0025			0.0032	0.0045	−0.0100	−0.0478
	[0.0015]			[0.0013]	[0.0015]	[0.0021]	[0.0033]
Observations	70,681			70,681	50,684	50,684	70,551
R^2	0.042			0.040	0.045		
Effect of 1 ug/dl lead for girls	0.0006			0.0000	0.0000	0.0000	0.0000
Effect of 1 ug/dl lead for boys	0.0032			0.0024	0.0624	0.0053	0.0133
Measure of lead	Geometric mean	Geometric mean	Geometric mean	Single draw	Single draw	Single draw	Single draw
Sample	Full	Siblings	Siblings	Full	>1 BLL	>1 BLL	Full
Additional natality controls		Yes	Yes				
Fixed effect	Block group	Block group	Block group	Block group	Block group	Block group	Block group
Instrument	None	None	None	None	None	Other draw	Traffic × year of birth

Column 1 reproduces column 6 of table 2. Controls in all columns include race, ethnicity, gender, free lunch status, year-of-birth indicators, and race- and free-lunch-specific quadratic trends in the year of birth. Additional controls included in columns 2 and 3 of panel A include maternal age and marital status at birth, birth order, and birthweight.

correlated, then a decline in negative selection on observables over time will imply a decline in selection on unobservables as well. And if negative selection on unobservables is driving OLS estimates of the relationship between lead and suspensions, then the strength of the estimated OLS relationship between lead and suspensions should decline over time. We show that while race and free lunch become less predictive of BLLs over time, consistent with reductions in negative selection into lead exposure over time, the estimated OLS relationship between lead and suspensions remains constant, suggesting that it is not driven by unobservables (appendix table 2).

Estimates from models with sibling fixed effects are shown in table 3. The inclusion of a sibling FE addresses omitted variables correlated with exposure to lead and family disadvantage that have not already been addressed with the controls included in the OLS regressions. However, any bias due to measurement error can be exacerbated in an FE setting, a point to which we return. Siblings can be identified only among those born in 1997 or later, but the sibling subsample is representative of the population of children born in 1997 or later (appendix table 1, columns 7 and 8). In estimates not presented here, we find that OLS estimates based on the sibling subsample are very similar to OLS estimates based on the full sample of children born in 1997 or later.

The first three columns of table 3 compare OLS and sibling FE estimates and include the interaction term *Lead* × *Male*. At the bottom of the table, we calculate the effect sizes for girls and for boys for ease of comparison. Column 1 contains the OLS estimates based on the full sample as reported in table 2, column 6 (and column 11 for the detention or incarceration outcome). Column 2 presents the OLS estimates for the sibling sample, and column 3 presents the FE estimates for the sibling sample. Comparing columns 1 and 2, we see that the OLS estimate declines for girls but remains the same for boys when we conduct OLS analyses but move from the full sample to the sibling sample. This is because the relationship between lead and suspensions for girls declines for all children born in 1997 or later.

When we include the sibling FE in column 3, the estimated effect of lead on suspension for girls falls from an already small number (0.0024) to 0, while the estimate for males remains unchanged (0.012), suggesting that among boys, an increase in average BLL of 1 ug/dL increases the probability

of suspension by roughly 1 percentage point on a mean of 25%, (a roughly 4% increase at the mean.) In estimates not presented here, we also generate sibling FE estimates stratifying by child gender, thereby comparing only children of the same gender within each household; the estimates are similar to those based on the interaction term. We conclude, based on the similarity of the OLS and FE estimates, that unobserved family background factors are not likely to be a large source of bias in our OLS estimates.[7]

While sibling FE can reduce bias from omitted variables, they tend to exacerbate attenuation bias due to measurement error. Hence, the sibling FE estimates may be smaller than the true causal impact. Below, we discuss the measurement issues in greater detail and assess the extent to which measurement errors may be biasing OLS and sibling FE estimates.

A. *Assessing Bias from Measurement Error in BLLs*

We conduct two exercises to assess bias from measurement error. First, we compare OLS estimates of the relationship between lead and our outcomes based on more and less accurate measures of child lead exposure. Second, we compare OLS results with estimates in which we instrument for one measure of lead with another measure of lead. This IV strategy addresses bias from measurement error but not endogeneity (Ashenfelter & Krueger, 1994).

We find that child lead levels based on more accurate measures generate considerably larger estimated effects of lead on both suspensions and detentions. In table 3, we compare estimates based on a single child BLL (column 4) with those based on the more precise average of all of a child's BLLs (column 1). Panel A contains the estimates for suspensions and panel B for detention or incarceration. The estimates suggest that the effect of lead on the probability of suspension is considerably smaller when a noisier measure of lead is used. For both boys and girls, the effects roughly double when the average is used as opposed to the single draw. When we limit the sample to those with multiple BLLs (column 5 for the OLS) and instrument for one single draw using another draw (column 6), the estimated effects triple in size.

Overall, these exercises suggest that the true effect of lead on suspensions and detention is considerably understated by OLS and FE estimates because of measurement error in child BLLs, while omitted variable bias is likely not a great concern. We explore the degree of bias from measurement error in greater detail in appendix table 3, where we also exploit the fact that venous measures of blood lead are more accurate than capillary measures. For a subset of the sample, we also instrument for a child's average BLL with his or her sibling's average BLL. The estimated effects based on the more accurate measure are between 26% and 77% larger than those based on the less accurate measure, depending on specification and gender of the child. In the next section, we present IV estimates based on our *Traffic* × *Birth Cohort* instrument that should address both measurement error and omitted variable bias. The results of this analysis are discussed in further detail in the appendix.

B. *Instrumental Variables Estimates Based on Exposure to Traffic*

Regression models linking traffic volume with child BLLs from equation (2), the first stage, are shown in appendix table 4 column 1. Exposure to traffic is a highly statistically significant determinant of child BLLs, and its effect declines over time (coefficient on ln(*Traffic*) 0.28, SE .0274, coefficient on ln(*Traffic*) × *Year of Birth* −0.014, SE 0.002). As discussed above, our instrument is not traffic volume but the interaction between the traffic volume and a linear time trend. The identifying assumption is that places with high-traffic volumes saw larger reductions in BLLs due to the reduction over time in the amount of residual lead near roadways, and not some other factor that also affected children's probability of being suspended or incarcerated. We showed in figure 2 that the decline in lead BLLs near roads was not driven by changes in the composition of children. Appendix figure 5 suggested that the decline in BLLs is not driven by general improvements in the outcomes of disadvantaged children over this period either. Moreover, there do not appear to be significant changes in other types of air pollution over time in Rhode Island that are known to have a negative effect on health. All three findings suggest that the instrument is capturing declines in exposure to lead over this period, not other factors that might independently affect child outcomes.[8]

Estimates of the second-stage regression in equation (3), including the *Lead* × *Male* interaction and using the traffic instrument, are shown in the last column of table 3. As with the sibling FE estimates, the IV estimates suggest that there is no significant relationship between a girl's BLLs and the probability of suspension. However, for boys, the strong relationship between lead and suspension found in the OLS and sibling FE estimates remains: 1-unit increase in average BLLs increases the probability of any suspension by 1.56 percentage points on a baseline of 25%, suggesting an increase of 6%. This IV estimate is only slightly larger than the OLS and FE estimates, although a Hausman test rejects the null hypothesis of equality with the OLS estimates.

For juvenile detention (table 3, panel B), there is no statistically significant effect for girls in either the OLS or the IV settings. The estimate for boys, however, increases considerably: 1 unit increase in average blood lead increases the

[7] In our sample, 66% of the families move between children. When we stratify the sample by movers and nonmovers, the estimated effects of lead on suspensions are the same for girls and slightly larger for boys in the sample of families who do not move. We conclude that any declines in family resources that might be correlated with a move are not biasing our sibling FE estimates upwards.

[8] As a separate check, we regress an indicator for African American and free lunch status on the measures of *Traffic* and *Traffic* × *Year of Birth* and examine the coefficients on the interaction term. These regressions are discussed in the appendix.

TABLE 4.—OLS AND IV ESTIMATES OF LEAD AND SUSPENSIONS, ROBUSTNESS CHECKS

	(1)	(2)	(3)	(4)
A. OLS Estimates				
Geometric mean of lead	0.0059	0.0053	0.0058	0.0064
	[0.0006]	[0.0006]	[0.0006]	[0.0006]
Lead × male	0.0056	0.0055	0.0056	0.0051
	[0.0008]	[0.0008]	[0.0008]	[0.0008]
Observations	124,579	124,578	124,579	114,552
R^2	0.185	0.194	0.185	0.184
Additional controls	None	Infractions/Child in School × Grade	Certificates in tract at birth	None
Sample	Full	Full	Full	Drop tracts w/new roads
B. IV Estimates				
Geometric mean of lead	−0.0084	−0.0144	−0.0194	0.0011
	[0.0206]	[0.0209]	[0.0325]	[0.0229]
Lead × male	0.0161	0.0165	0.0168	0.0138
	[0.0022]	[0.0022]	[0.0028]	[0.0024]
Observations	124,371	124,370	124,371	114,372
Additional controls	None	Infractions/Child in School × Grade	Certificates in tract at birth	None
Sample	Full	Full	Full	Drop tracts with/new roads

All regressions include year-of-birth fixed effects, birth order fixed effects, 801 census block group fixed effects, gender, race, ethnicity, free lunch status, and race- and free-lunch-specific quadratic time trends. Robust standard errors clustered on census tract appear in brackets.

probability of detention by 1.3 percentage points on an overall baseline detention rate of 1.8%. This is a large percent effect, in part because the baseline detention rate in the overall population is so small. Among juveniles detained, 86% are on free lunch, so one interesting thought experiment is to consider the number of children in the free lunch group as the denominator. In the free lunch sample, a 1 unit increase in BLL is estimated to increase the probability of detention by 1.6 percentage points. Given the much higher incidence of detention in the free lunch group, the percentage increase is smaller than in the full sample, at 57%.

IV estimates may be greater than OLS estimates because of measurement error, as we have discussed. Alternatively, if the children most affected by the instrument (i.e., those living near busy roads) have a higher propensity to be suspended or incarcerated than all children, it is possible that we are finding a higher effect of the treatment on the treated.

C. *Robustness*

Results of four robustness checks are shown in table 4. First, we include a control for the average number of infractions per year in the child's *School* × *Birth Cohort* to account for any changes in policy at the *School* × *Cohort* level that might also influence the number of disciplinary infractions (column 2). Second, we add controls to account for the Rhode Island Lead-Safe certificate program analyzed by Aizer et al. (2018). This program aimed to encourage landlords to mitigate lead hazards in homes for rent, and so it could have affected the youngest children in our sample, though it would not have affected many of the children old enough to suffer juvenile detention. Specifically, in column 3, we add controls for the number of certificates that had been issued in the census tract where the child lived as of the time of his or her first lead test (a proxy for the intensity of the lead paint cleanup efforts).

In column 4, we show estimates after dropping all census tracks for which we observe substantial changes (increases) in roads over this period. Sixty-six percent of our sample live in tracts that gained no new roads between 1980 and today. Eight percent of our sample live in tracts that gain a substantial number of new roads. These are mostly suburban subdivisions. No one lives in a tract that loses roads over this period. When we drop the sample that gains a substantial number of new roads, the sample size falls to 114,512 from approximately 125,000 and the estimates remain largely unchanged. The results suggest that the estimates are robust to all these modifications.

Could traffic volume and the interaction between traffic volume and birth cohort simply reflect changes in housing or air quality more generally over this period? If so, one might expect to estimate a significant relationship between traffic volume and birthweight, a measure of newborn health that is very responsive to both maternal circumstances and air pollution (Aizer & Currie, 2014). To consider this possibility, we present reduced-form estimates of the impact of traffic volume and its interaction with year of birth on birth-weight in appendix table 5. We find no relationship between birthweight and traffic volume.

VI. Conclusion

This paper makes several contributions to the literature examining the link between lead poisoning and antisocial behavior among juveniles. First, we broaden our study away from the exclusive focus on crime, to consider school disciplinary problems. Disciplinary problems predict school failure and drop out, are much more common, and can be observed in both boys and girls. Suspensions are also highly predictive of future criminal activity: in our data, children who have been suspended are ten times more likely to spend any time in juvenile detention. We also look at juvenile

detention and incarceration, which is an indicator of crime but has its own negative impacts on juveniles.

A second contribution is to construct and examine rich individual-level data from linked administrative records for all children born in Rhode Island between 1990 and 2004 and to exploit very local variation within census block groups in exposure to lead. The long time span allows us to link preschool BLLs to the outcomes of children in middle school and beyond. By including individual-level controls and also controlling for neighborhoods at the level of the census block group and by including sibling FE, we are able to alleviate concerns about confounding due to omitted variables.

A third contribution is to develop multiple identification strategies that rely on different assumptions. Our sibling FE suggests that OLS estimates are not substantially biased by omitted variables. However, we present evidence that the OLS and sibling FE estimates are attenuated by measurement error in BLLs, an important result given that attenuation due to measurement error has been largely ignored in the previous literature on the effects of lead.

We find that a 1 unit increase in BLLs increased the probability of suspension from school by about 6% for boys, with small and imprecise estimates for girls. For detention, we again find statistically significant effects only for boys. These estimates are less precise and suggest that a 1 unit increase in BLL increases the probability of detention by about 56% for each 1 unit increase in BLLs for low-income children (the overwhelming majority of children in detention in Rhode Island). The estimates in this paper are local average treatment effects that are relevant across the range of pollution exposure in the 1990s.

Our estimates support the hypothesis that reductions in BLLs may have been responsible for a significant part of the observed decrease in antisocial behavior among youths and young adults in recent decades. We calculate that reductions in blood lead can explain one-third of the 72% decline in suspensions over the fifteen-year period of 1990 to 2004. We think it reasonable to conclude that reductions in lead can explain a similar share of the decline in crime witnessed over the period 1994 to 2015. This estimate falls in the middle of the range of elasticities for the effect of lead on risky behaviors presented by Reyes (2015).

One limitation of our work is that we focus on outcomes that are not highly prevalent for girls, which may explain our lack of large or significant effects for girls. Future work should consider estimating the effect of exposure to lead in early childhood on outcomes more relevant for girls, which might include, for example, teen pregnancy, truancy, or substance abuse.

A second limitation is that we do not actually observe measures of lead in soil at various distances from roadways over time. To the best of our knowledge, systematic measures were never taken. Thus, while we believe our story about gradual decay in lead near roadways is a plausible explanation for the differential declines in children's BLLs with distance from roads, other explanations are possible. For instance, if all children played less outdoors over time, then they might be less exposed to resuspended lead particles in the air. In this scenario, children living near roads would still be most affected even if there was no decline in the amount of lead in soil.

What are the policy implications of our findings given that lead levels are already at historic lows in the United States? This work shows that exposure to even low levels of lead in early childhood generates substantial costs for many years after initial exposure. Not only is chronic exposure to low levels of lead still an issue in many older urban communities, but there continue to be instances of acute exposure, such as the case of the contamination of the water supply in Flint, Michigan, in 2015. Moreover, while lead has been known for many years to negatively affect child health and well-being, prompting federal and state authorities to continuously measure children's exposure to lead, many other toxic substances not regularly measured have more recently become known or suspected of negatively affecting child outcomes. Evidence presented here suggests that not only should we consider measuring these substances in children, but that how we measure exposure to toxic substances has important implications in terms of estimating health effects.

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