Hassles and Environmental Health Screenings: Evidence from Lead Tests in Illinois

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Abstract

I study the determinants of childhood lead screening using all Illinois birth records (2001-2014), matched to lead testing records and geocoded housing age data. Housing age measures lead risk, as older houses disproportionally have lead paint. Changes in geographic access to providers provide variation in non-monetary costs of testing. Higher costs reduce screening among low- and high-risk households alike. Thus, self-selection based on screening costs does not appear to improve targeting, even though high-risk households are willing to pay $31-419 more than low-risk households for screening. Screening incentives would be cost-effective for reasonable values of lead poisoning externalities.

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1 Introduction

Health screenings are key to enable early detection and treatment of conditions that only present minor symptoms at first. Yet, a growing literature documents both imperfect compliance with screening guidelines for cancer and important selection patterns, with some people seeking and others avoiding screening independently of guidelines and costs (Einav et al. forthcoming, Kim & Lee 2017). Although socioeconomic status correlates positively with health, as well as take-up of screening and health-related information (Jones et al. 2019, Bundorf et al. 2019), little is known about the determinants of screening take-up. I study this issue in the context of screening for childhoold lead poisoning in Illinois, where screening rates are lower than 60 percent even in areas with required universal screening (Figure 1).

Early childhood lead poisoning is associated with reduced IQ (Ferrie et al. 2015) and educational attainment (Aizer et al. 2018, Grönqvist et al. 2020, Reyes 2015*a*), and an increased risk of criminal activity (Aizer & Currie 2019, Feigenbaum & Muller 2016, Reyes 2015*b*, 2007). At current levels, 2.2 percent of Illinois children born in 2014 had lead poisoning (Figure 1).[1] Most exposure happens as children crawl and play in homes with lead paint hazards, and lead exposure disproportionlly affects children of low socioeconomic status (Zartarian et al. 2017). Two thirds of the Illinois housing stock, almost 3.6 million homes, was built prior to the residential lead paint ban in 1978 and may have lead paint.[2] To enable early detection and treatment of lead poisoning, the recommended age for a blood lead test at a doctor's office is between 9-24 months.[3]

This paper investigates how barriers to screening affect screening take-up and for whom. I focus on distance to providers, a non-monetary cost of screening. This sort of barrier to policy uptake is known as a *hassle* or *ordeal*. Do these ordeals improve targeting efficiency, or do they hinder detection and remediation of lead hazards? When only program recipients know their private value of receiving a program, ordeals may reduce inclusion errors. That is, recipients who do not need

[1] During my sample period, the Illinois Department of Public Health (IDPH) referred children to services if they had a blood lead level of $10\mu g/dL$ or higher.

[2] Source: American Community Survey (2017).

[3] Because the effects of lead exposure are worst in small children, in this paper I focus on screening by age two.

it may select out of the program to avoid these costs (Nichols & Zeckhauser 1982). Households may know the state of the paint coat or have their property inspected. However, households with high private values may also face higher costs per ordeal (Alatas et al. 2016), for example because they do not have a car. Then, ordeals may increase exclusion errors: poisoned children may forego screening, leading to high private and social costs.

To study the effect of screening costs on lead poisoning prevention, I link geocoded birth records for the universe of over 2 million children born in Illinois between 2001 and 2014 to blood lead screening records and housing age information from assessor files. Screening data provide ex-post poisoning for screened children, and housing age measures ex-ante observable risk for both screened and unscreened children. First, I estimate the elasticity of screening with respect to travel costs in terms of distance to health care providers. To assuage concerns of endogeneity in households' location relative to providers, I exploit providers' openings and closings and compare children born in the same location in different years who face different sets of providers.[4] The key identifying assumption is that openings and closings of medical doctor offices are orthogonal to trends in lead screening. Second, I study how travel costs affect which households select into screening, in terms of both ex-ante observable and ex-post realized risk. The identifying assumption to study selection is that, while children may obtain other services when they get screening, households with a high or low risk of lead poisoning value these additional services similarly.

First, being 15 minutes farther away from a lead-screening provider (two-way) decreases the likelihood of screening by 9 percent. These results do not appear to be driven by information or increased screening salience following providers' entries and exits. Yet, parents respond to information shocks re-optimizing screening across siblings when one child tests positive for lead exposure. Second, I find no evidence that households who get screening despite facing higher costs have higher observable or unobservable exposure risk. In other words, I find no evidence that ordeals improve targeting efficiency. Third, proximity to providers improves detection of lead

[4] A growing literature leverages closures of health care providers, such as abortion clinics, Social Security Administration field offices, and bank branches to estimate the effect of travel costs on take-up of different programs (Deshpande & Li 2019, Nguyen 2019, Lu & Slusky 2016, 2017, Lindo et al. 2019, Venator & Fletcher 2019).

poisoning, but it does not increase take-up of remediation funding. Thus, removing barriers to screening may not lead to increased remediations, perhaps due to partial compliance with abatement regulations or limited awareness of remediation funding. Moreover, proximity to high-quality providers, as measured either by provider-level screening outcomes or medical school attended, increases screening more than proximity to low-quality providers, suggesting both travel costs and providers' discretion affect screening take-up.

Variation in travel costs allows me to recover households' revealed preference for screening and compare the existing lead screening policy to counterfactual prevention policies. I use travel costs in the logit framework to estimate the willingness-to-pay (WTP) for screening of households with different lead exposure risk. I simulate the impact of four screening policies: travel subsidies, pay-for-performance incentives for providers, an increase in screening locations, and universal screening for children in old homes. I estimate that the average household in the most at-risk homes has a WTP for screening of $7.81 while the average low-risk household has a negative WTP, consistent with the low incidence of lead poisoning, behavioral hazards (Baicker et al. 2015, Chandra et al. 2019, Avery et al. 2019), or non-standard preferences (Kőszegi 2003, Oster et al. 2013). I estimate the difference in WTP between high- and low-risk households to be $31-419. All counterfactual screening policies I examine result in modest benefits for the marginal households. Yet, these policies may be cost-effective when accounting for reductions in lead exposure externalities, consistent with the large impacts of programs targeting disadvantaged children found by Hendren & Sprung-Keyser (2020). By contrast, increasing remediations does not appear to be cost-effective.

This paper contributes to a robust literature that identifies travel costs as an important determinant of take-up of social benefits, including childcare subsidies, disability insurance, small business loans, and health care services (Currie 2006, Rossin-Slater 2013, Herbst & Tekin 2012, Deshpande & Li 2019, Nguyen 2019, Lu & Slusky 2016, 2017, Einav et al. 2016, Lindo et al. 2019, Venator & Fletcher 2019). In the US, information barriers, scheduling challenges, and transportation costs appear to contribute to vaccine delays among disadvantaged families (Brito et al. 1991, Carpenter & Lawler 2019). In India, small financial incentives appear more cost-effective at increasing immunization take-up than improving supply (Banerjee et al. 2010). My paper shows that travel costs decrease detection of lead hazards, potentially imposing a large externality on society.

This paper also contributes to a large literature studying the targeting efficiency of welfare programs (Hanna & Olken 2018). Hoffmann (2018) finds that poor Indian households are very elastic with respect to non-monetary prices, such as travel costs. I find no evidence that high-risk households differentially select into screening at higher distances, suggesting that households at high risk for lead exposure in the US may disproportionally dislike travel hassles, too. My findings suggest that travel costs may have worse targeting properties than bureaucratic ordeals, which have been shown to improve targeting efficiency in the US (Kleven & Kopczuk 2011, Finkelstein & Notowidigdo 2019, Einav et al. forthcoming).

Section 2 provides institutional background and models how travel distance may affect targeting efficiency. Section 3 describes the data I use in this paper. Sections 4 and 5 analyze screening take-up and the impacts of different lead poisoning prevention policies.

2 Background and Theoretical Framework

First, this section provides background on lead screening. Second, it builds on the classical work of Nichols & Zeckhauser (1982) and its extension by Alatas et al. (2016) to show how travel costs affect selection into screening. Third, it discusses the role of lead poisoning externalities in the planner's screening decision.

2.1 Lead Screening Background

Federal guidelines mandate that all children on Medicaid must be screened for lead poisoning at ages one and two. In addition, Illinois requires screening for all children living in high-risk zip codes, defined by housing age and demographic characteristics. Even in these high-risk zip codes, which include the whole city of Chicago, less than two thirds of children born between 2001 and

2014 were screened (Figure 1: Panel B). This low compliance with the screening guidelines raises the questions of who gets screening and what barriers hinder screening.

Appendix Table A.1 shows that children with elevated blood lead levels are more likely to be of low socioeconomic status and to live in old housing than average. These correlations might originate because disadvantaged children are more likely to live in poorly maintained homes. Moreover, parents of low socioeconomic status might be less able to acquire information on the lead status of their residence and to remediate known lead hazards. By requiring screening for children on Medicaid and children living in areas with a high prevalence of old housing, federal and Illinois screening guidelines recognize these higher risks. Indeed, Appendix Table A.1 shows that children of low socioeconomic status and those who live in old housing are more likely to get screened. However, the question remains of whether these policies optimally target screening.

I hypothesize that provider access constitutes a barrier to screening since house visits do not include blood lead screening in Illinois. For comparison, around 80 percent of Illinois children eligible for Family Case Management had three or more well-child visits in the period 2005-2010, even though the American Academy of Pediatrics recommends six visits during the first year of life, to occur at one, two, four, six, nine, and twelve months of age (IDHS, 2010). As the likelihood that a child has a well-child visit decreases with age (Caldwell & Berdahl 2013), missed well-child visits after 9 months of age may explain the gap between well-child visits and lead screening rates.[5] Section 4.4 investigates the alternative explanation that providers' discretion drives the low compliance with the screening guidelines. My empirical findings support the hypothesis that both travel costs and providers' quality affect screening rates.

2.2 The Household's Screening Take-Up Decision

Household i perceives benefit b_i from screening their child only if the child is found to be lead-poisoned: for example, benefits accrue from assignment to case management aimed at reducing

[5]In Illinois, households on Medicaid can choose their primary care provider within a managed care health plan that accepts Medicaid, and they can switch every year.

damages.[6] This modelling choice ignores potential screening benefits accruing from negative tests, such as learning that a home is lead-safe. Parents' perceived screening benefits depend on several factors, including information about exposure risk, degree of risk aversion, degree of altruism towards the child,[7] beliefs about treatment costs and feasibility (which may correlate with home-ownership) as well as recovery probability,[8] and additional benefits from visiting the doctor, such as having a physical examination.[9] My model does not require assumptions on these parameters; the revealed-preference approach in Section 5 allows me to compare willingness-to-pay (WTP) estimates to estimates of screening benefits computed for different parameter values.

The screening cost, c_i, depends on the nominal screening price, p, and the opportunity cost in terms of the parents' wage, w_i and travel time, t_i, which is proportional to distance from the doctor, d_i. I abstract from heterogeneity in p for simplicity, although the cost of a blood lead test in Illinois varies with a child's insurance coverage.[10] Then, child i is screened if and only if

$$b_i \geq c_i = w_i t_i + p. \tag{1}$$

Because $t_i \propto d_i$, this inequality yields a cutoff $\bar{d}_i$ above which a child is not screened:

$$\bar{d}_i = \frac{b_i - p}{w_i}. \tag{2}$$

I assume that benefits increase with risk, that is $b'(r_i) > 0$: the higher the potential exposure,

[6]Interventions at home include education on nutrition and reducing exposure in the home, a home inspection, and referral to lead remediation services, which are generally subsidized for low-income households. Billings & Schnepel (2018) show that such case management fully reverses lead poisoning damages in Charlotte, North Carolina.

[7]The evidence on how much parents value reductions in their children's health risk relative to reductions in their own risk is mixed (see for example, Gerking & Dickie 2013, Gerking et al. 2014)

[8]Myerson et al. 2020 show that increasing treatment access increases screening, evidence of an "ostrich effect", a term coined by Galai & Sade (2006).

[9]Not observing these additional services does not bias the selection analysis if benefits from these additional services are not correlated with screening benefits after controlling for observables.

[10]While lead screening is fully covered for children enrolled in Medicaid or All Kids, nominal prices range between $0-43 for children who are uninsured or on private insurance (https://www.luc.edu/media/lucedu/hhhci/pdf/leadsafeil/LeadSafeILDirectory061_.pdf, accessed in November 2021), with an average venous test costing $31 (Kaplowitz et al. 2012). I discuss how this variation in prices affects my estimates of households' WTP for screening in Section 5.1.

the more likely that screening leads to crucial intervention. Then, riskier children have a higher willingness-to-travel for screening: as in the classic ordeals model (Nichols & Zeckhauser 1982), the cutoff increases with risk:

$$\frac{\partial \bar{d}_i}{\partial r} = \frac{\partial b_i}{\partial r}\frac{1}{w_i} \geq 0. \tag{3}$$

Figure 2 illustrates how risk affects the relationship between screening and distance. High-risk households are less sensitive to distance than low-risk households: their screening rates decline less sharply with distance (left panel). Therefore, the share of screened children that is high-risk increases with distance (right panel).

However, the model's predictions become ambiguous if we consider travel mode, following Alatas et al. (2016). Assume that family assets a_i are negatively correlated with risk, $a'(r_i) < 0$, and that travel time is negatively correlated with assets. For example, travelling by car is faster than walking or using public transit: $t_i(a_i, d_i) \propto \frac{d_i}{a_i}$. Then,

$$\bar{d}_i \propto a_i \frac{b_i - p}{w_i}, \tag{4}$$

$$\frac{\partial \bar{d}_i}{\partial r_i} \propto \underbrace{\frac{\partial a_i}{\partial r}\frac{b_i - p}{w_i}}_{<0} + a_i \underbrace{\frac{\partial b_i}{\partial r}\frac{1}{w_i}}_{>0} \lesseqgtr 0. \tag{5}$$

In a model with assets, cutoffs may either increase or decrease in risk. While the second term in equation (5) is still positive, the first term is negative: riskier households face higher travel times conditional on distance. Thus, the effect of reducing distance to providers on the average riskiness of screened children is an empirical question. In Section 4.2, I exploit providers' openings and closings to answer this question.

2.3 The Planner's Problem

The socially optimal level and targeting of screening may not coincide with the individual optimum due to externalities. First, lead-poisoned children negatively affect their classroom peers (Gazze et al. 2020) and are more likely to engage in risky and criminal behavior (Aizer & Currie 2019, Feigenbaum & Muller 2016, Reyes 2015*b*, 2007). Second, detecting lead hazards may prevent exposure of future residents.

I model the social benefits of screening a child as the sum of three components.[11] First, I consider the private benefit, $b_i - c_i$. Second, I add the averted externality i would have imposed on society if they had not been screened, e_i. Third, I add the discounted value of the avoided externalities from preventing exposure among children $j \in J$ who will live in i's building in the future.[12] Summing over the set of screened children S, this yields

$$B = \sum_{i \in S} (\underbrace{b_i - c_i}_{Private\,Value} + \underbrace{e_i}_{Externality} \underbrace{+\delta \sum_j e_j * \text{Lives in i's building}_j}_{Prevention\,Value}). \tag{6}$$

Thus, some households with low private benefits may have a high social value of screening if they have a large externality or prevention value.

The planner cannot optimally target screening without knowing e_i and e_j. Housing age may proxy for exposure risk at each home. Then, targeting screening based on observable risk may improve upon self-selection on private benefits. I estimate both the average prevention value of screening (Section 4.3) and the societal benefits of different screening policies (Section 5.2).

3 Data

My analysis requires data on children's screening outcomes, travel costs, lead exposure risk, and lead remediations. First, I link birth records to blood lead test data to construct children's screening histories. Second, I geocode children's addresses at birth and lead-screening providers' addresses to measure the distance a child has to travel to get screening. Third, I link these individual-level data to address-level housing age and remediation data to construct unique measures of exposure risk and remediation activity at birth addresses.

[11] Here, I abstract from the medical sector costs of increasing screening.

[12] e_j will depend on the riskiness of each building, and may be zero.

3.1 Childhood Lead Screening Measures

The Illinois Department of Public Health (IDPH) provided birth and death certificates for almost 4.5 million children born in Illinois between 1991 and 2016. These records include each child's name and birth date, allowing me to link these data to the universe of 5.4 million blood lead tests performed in Illinois between 1997 and 2016, with a match rate of 86 percent (Appendix Figure A.1). Matched and unmatched tests have similar observable characteristics (Appendix Table A.2). Because lead test records are incomplete prior to 2001, I limit my analysis to children born after 2000. I also limit the analysis to children born before 2015 to ensure I observe each child's outcome by age two. I classify non-deceased children not linked to any tests as not screened. Appendix Tables A.3 and A.4 show the number of tests and unique children in my original sample, and the number remaining after each data cleaning and linkage step.

IDPH collects children's blood lead records from physicians and laboratories. These records include test date, blood lead level (BLL), test type (capillary or venous), provider and laboratory identifiers, and Medicaid status (albeit incomplete). Capillary tests are prone to false positives. Thus, capillary tests that show elevated blood lead levels (EBLLs), defined as blood lead levels above 9 micrograms per deciliter of blood ($\mu g/dL$), need to be confirmed by another test. For each child, I keep the highest venous test when available, or the highest confirmed capillary test when available. My sample includes 70,000 confirmed EBLLs from over 22,000 children (Appendix Table A.5). Some laboratories have minimum reporting limits, meaning BLLs are bottom-censored; I correct for these limits to obtain correct population estimates of lead exposure.[13]

Birth records also include family characteristics, such as mother's marital status, age, education, and race, as well as child's address at birth. I geocode these addresses to link the blood lead data to housing age information (see Section 3.3) and Census block group median income from the 2015 American Communities Survey. After geocoding, I obtain a sample of over 2 million

[13] I determine the cutoff for each laboratory based on the distribution of test results for that laboratory by both test type and year. Some laboratories have a thin left tail of test results below the estimated cutoff: I reassign those test results to the cutoff value. For each cutoff-year-type cell, I use laboratories without cutoffs to compute the average BLL for tests below that cutoff and I reassign all test results at the cutoff to this average value.

children and over 2.9 million tests linked to these children. I use birth address rather than address at testing time because I only observe subsequent addresses conditional on a child being screened for lead. Appendix Table A.6 shows that even if a third of households in my sample move within a two-year period, most households remain in homes and zip codes with the same exposure risk.

3.2 Provider Access Measures

IDPH collects the name and address of providers who perform lead tests. A quarter of providers are individuals, while the rest include small groups of doctors and hospitals. I code a provider as entering or exiting the sample the first or last year that I observe them ordering tests, respectively. On average, 4.5 percent of providers enter each year and 4.8 percent exit (4.1 and 4.4 percent, respectively, when excluding providers performing fewer than ten tests per year).[14] These numbers are consistent with statistics on physicians' churning: for example, 9-13 percent of physicians report plans to retire within three years, and about 2.5 percent new students graduate from medical school each year (Walker et al. 2016, Young et al. 2015). Providers who enter or exit throughout my sample are generally similar to the average provider (Appendix Table A.7).[15]

To construct a measure of travel costs, I calculate the distance "as the crow flies" between a child's birth residence and the closest provider open during the child's birth year.[16] The median child has a provider within 1.2 kilometers (Appendix Figure A.4). Over 90 percent of screened children did not get tested at their closest provider (Appendix Figure A.5), likely due to preference for continued care after a move (Raval & Rosenbaum 2018, Sabety 2021) or insurance network constraints. Section 4.1 investigates the relationship between distance to closest provider and distance travelled.

The impact of access to providers may depend on their propensity to screen, which might

[14] The median provider performs 11 tests in a year (Appendix Figure A.2).

[15] Appendix Figure A.3 shows the distribution of providers across neighborhoods and years.

[16] For computational reasons, to identify closest providers I use a search algorithm that conditions on the median catchment distance of each provider, which may overstate distance for children farther away than the median, thus biasing the estimated effect of distance downward. In the sample of screened children, this procedure assigns 7.09 percent of tests to a minimum distance that is higher than the actual distance travelled to obtain the test.

be correlated with quality (Vivier et al. 2001). I use the 2019 USNews ranking of the medical school the provider attended as one measure of quality (Schnell & Currie 2018). I obtain medical school attended by linking providers to the 2019 Medicare Physician Compare File (MPCF) through name, address, and practice name.[17,18] I also consider measures of quality that directly capture a provider's lead screening behavior: I define providers as higher quality if they screen more children and/or screen them at the right times according to federal and state guidelines as follows. Because I do not observe a child's provider if the child is not screened, I calculate a provider's screening rate as the screening rate for children born within the median distance households travel to see that provider, and I weigh unscreened children by the inverse of their distance.[19] Because federal guidelines mandate that all children on Medicaid must be screened for lead poisoning at ages one and two, I compute the share of Medicaid children a provider screened by age one who have a second test by age two. I also compute the share of EBLLs detected by each provider with a required follow-up within 90 days.[20] I then aggregate these screening-based measures into a summary quality index. Because these screening-based quality measures might reflect demand-side preferences, I consider an indicator for performing capillary tests a more objective measure of providers' propensity to screen,, because capillary testing requires a machine and may reduce the barrier to screening if households are averse to venous blood draws.[21]

Providers' screening-based quality measures and providers' medical schools capture different provider's characteristics and are indeed imperfectly correlated (Appendix Figure A.8). My empirical analysis is robust to using different quality measures.

3.3 Childhood Lead Exposure Pathways and Lead Hazard Remediations

Children in homes built prior to 1930 have the highest BLLs in Illinois, after controlling for demographic characteristics and zip code fixed effects (Abbasi et al. 2020). Indeed, HUD estimates

[17] For organizations with multiple providers, I average the rankings.

[18] Only one percent of providers in the MPCF are pediatricians.

[19] For most providers, the median child's address is within 7 kilometers of their provider's address.

[20] Appendix Figure A.6 shows that only around 50 percent of EBLLs have a follow-up test.

[21] Appendix Figure A.7 shows the location of providers of different quality in Illinois.

that 87 percent of houses built before 1940 in the US have lead paint, compared to 69 percent of houses built between 1940 and 1959 and 24 percent of houses built between 1960 and 1977 (HUD, 2011). Thus, I define children born in homes built before 1930 as high-risk, using parcel-level data on construction year in the Zillow Transaction and Assessment Dataset.[22]

To measure lead hazard abatement following EBLL detection, I use data on addresses that receive remediation funding under HUD's lead hazard control programs.[23] Because these funds are targeted to low-income property owners, these data do not cover the universe of lead hazard remediations. Yet, they provide a useful picture of case management following EBLL detection in the absence of more complete data.

4 Empirical Analysis: Barriers to Child Lead Screening

This section investigates screening barriers. First, I estimate the elasticity of screening with respect to distance to providers and risk salience, including information shocks across siblings. Second, I study how costs affect selection into screening. Third, I estimate the effect of screening costs on EBLL detection and hazard remediation. Fourth, I investigate how the quality of nearby providers affects screening.

To study how screening costs affect take-up, I exploit changes in distance to providers over time due to providers opening and closing controlling for neighborhood fixed effects. As providers open and close, children born at the same location but in different years face different travel costs. This approach is internally valid if the timing of openings and closings is exogenous to trends in screening rates. This condition would be violated if providers open in areas targeted by campaigns to increase screening rates, or if providers open in low-risk areas with decreasing screening rates.

[22] More information on accessing the data can be found at http://www.zillow.com/ztrax. The results and opinions are those of the author and do not reflect the position of Zillow Group.

[23] The data were collected for a project with Stephen Billings, Michael Greenstone, and Kevin Schnepel, titled "National Evaluation of the Housing and Neighborhood Impact of the HUD Lead-Based Paint Hazard Control Program, 1993-2016" and funded by HUD.

To investigate the plausibility of this assumption, I estimate the following regression:

$$ScreeningRate_{gy} = \sum_{\tau} \beta_{\tau} Entry_{g,y-\tau} + \sum_{\tau} \gamma_{\tau} Exit_{g,y-\tau} + \eta_g + \xi_y + \varepsilon_i, \tag{7}$$

where $ScreeningRate_{gy}$ is the screening rate in neighborhood g and birth cohort y; $Entry_{g,y-\tau}$ and $Exit_{g,y-\tau}$ are leads and lags of providers' entries and exits, defined as changes in the distance between the neighborhood centroid and the closest provider; η_g is a set of neighborhood fixed effects and ξ_y is a set of birth cohort fixed effects. By plotting the β_{τ} and γ_{τ} coefficients from estimating equation (7) at the level of Census block and tract, Figure 3 suggests that providers' entries and exits are not correlated with pre-existing trends in screening rates. Estimates at the block level appear more precise due to the more granular data, yet smaller likely due to smaller variation in distance induced by entries and exits measured at this level. Moreover, I do not find evidence of asymmetric responses to openings or closings, suggesting that long-term doctor-patient relationships might not be as relevant for pediatric visits as they have been estimated to be for Medicare patients (Sabety 2021). Finally, Appendix Table A.8 shows no correlation between openings and closings and lagged neighborhood characteristics.

I leverage this plausibly exogenous variation in screening costs by comparing children born in the same location in different years, controlling for location and birth year fixed effects:

$$Y_{igy} = \beta d_i + \eta_g + \xi_y + \varepsilon_i, \tag{8}$$

where Y_{igy} is an outcome for child i born in location g in year y, d_i measures a child's distance to the closest provider open in their birth year, η_g is a set of location fixed effects, and ξ_y is a set of birth year fixed effects. My preferred specification defines location as Census block, but my results are robust to considering zip code, tract, block group, or address. My preferred specification omits individual-level controls as I test for selection into screening using child characteristics as outcomes in Section 4.2. Providers' entries and exits might affect screening by changing information and salience in a neighborhood: I introduce neighbors' screening rates and screening outcomes as controls to disentangle these channels. I cluster standard errors at the zip code level to allow for correlation in exposure sources and screening behavior, and my main results are robust to clustering at the county level (Appendix Table A.9).

The next sections estimate the effect of distance on different outcomes Y_{igy}. Section 4.1 uses an indicator for whether a child is screened by age two. Section 4.2 studies selection using indicators for a screened child having certain characteristics, such as living in a home built prior to 1930, being black or hispanic, or having a single, teen, or low-education mother. Section 4.3 estimates the effect of screening costs on timely poisoning detection and remediation by looking at age at test and an indicator for a HUD-funded remediation at the address within three years.

4.1 Does Distance to Providers Decrease Screening and Why?

While the relationship between screening rates and distance to providers in Illinois is U-shaped in the raw data, it becomes closer to linear after controlling for neighborhood fixed effects (Figure 4). In my main analysis, I drop the 31,178 children who are farther than 20 kilometers from a provider (2.6 percent of the sample), as they are very different from the rest of the sample.[24] Indeed, these outliers have a lower elasticity of screening with respect to travel costs (Appendix Table A.10).

Panel A of Table 1 estimates that being one kilometer farther away from a lead-screening provider, a 30 percent increase over the mean distance, decreases the likelihood that a child is screened by age two by 0.4 percentage points, or 0.9 percent relative to the mean. These estimates imply an elasticity of screening with respect to distance to the closest provider of -0.03. Einav et al. (2016) estimate an elasticity of -0.02 for take-up of cancer treatment, while Herbst & Tekin (2012) estimate an elasticity of -0.13 for take-up of childcare subsidy. Non-linear estimates in the right panel of Figure 4 imply that the screening rate in Illinois would have been 2 percentage points higher (4.35 percent) if every child in my sample had a provider within one kilometer.

Because most households do not visit their closest provider, I also estimate the effect of distance from the provider of choice using a two-sample two-stage-least-squares (2SLS) model. Using

[24]On average, the closest provider is at 3.3 kilometers and the distribution is right skewed (Appendix Figure A.4).

the sample of screened children, Panel B of Table 1 estimates that being 1,000 meters farther from the closest provider translates into an extra 75-280 meters travelled to get a child's first lead test, providing a strong first stage in all but the most stringent specification with house fixed effects. Boostrapping this first stage relationship to predict distance from provider of choice for all children, Panel C estimates that an increase in distance to the provider of choice of 1,000 meters (13.4 percent) reduces screening by 1.3-7.2 percentage points, yielding an elasticity of -0.21 to -1.01. Importantly, 2SLS scales the reduced form estimates by the share of compliers, that is the households who change provider following openings and closings.[25]

Interpreting the magnitude of the effect of distance on screening take-up requires data on households' transportation mode, which I do not observe. Thus, I use car travel times, at 1–1.5 minutes per kilometer in Illinois (Agbodo & Nuss 2017). The estimates in Table 1 imply that a $6.25 increase in travel costs (a fifteen-minute two-way trip to the doctor at 7.5 kilometers each way and $25 hourly wage), decreases screening take-up by 9 percent.[26]

Proximity to health care providers might affect screening not just by enabling more timely visits to the doctor, but also by changing the information set available to families. Indeed, a provider opening (or closing) affects an entire neighborhood: more families now have more direct access to a doctor and might learn about lead screening, potentially spreading information and making lead exposure more salient. To examine the role of information and salience, I first focus on families with multiple offsprings where at least one child is screened, and I estimate the effect of that child

[25]While households who get tested at the closest provider are more disadvantaged than average, they are not closer to providers in general or providers who graduated from a top 20 medical school, but are closer to high-quality providers as defined by screening practices (Appendix Table A.1).

[26]Alternatively, using the HERE API to compute travel times for a 12 percent random subsample of the data, Columns 1-3 of Appendix Table A.11 estimates a reduction in screening likelihood per minute of travel time of 0.4 percentage points. This estimate is statistically indistinguishable from the estimate using distance in kilometers in Table 1, replicated on this subsample in Panel A ofAppendix Table A.11. I am limited to using 12 percent of my sample for this exercise by constraints in the free version of the API. For a subset of this 12 percent sample, the HERE API also computed travel times by public transit. Focussing on households with estimated transit travel times shorter than two hours, I find that households appear more elastic with respect to distance, but half as elastic with respect to time, as public transit dilutes travel times. Importantly, the subset of households with computable travel times is more likely to be urban, as indicated by the higher average screening rates. Appendix Table A.12 shows that households in Chicago are more sensitive to distance, suggesting that transit availability does not mitigate ordeals in this case. Indeed, Appendix Figure A.9 shows that households in tracts with low car ownership rates see larger effects of provider distance on screening rates.

having a BLL of $10-14\mu g/dL$ relative to $5-9\mu g/dL$ on the siblings' screening decision. This exercise isolates the effect of the information shock that one child has an EBLL on the decision to screen other children within the family who might also be at risk of lead exposure. Second, I explore how the effect of distance to providers changes with proxies for lead risk information at the neighborhood and family levels.

Table 2 shows that siblings of a child with EBLL are 27 percent more likely to be screened in the 30 days immediately after the EBLL is diagnosed, over a mean of three percent, and are still five percent more likely to be screened after 2 years than siblings of children with BLLs $5-9\mu g/dL$. Moreover, these effects appear to be symmetric for older and younger siblings, albeit larger for older siblings who are less likely to be screened as they age. These findings highlight two facts. First, there is ample variation in screening decisions even within families, suggesting that parents re-optimize based on information shocks. Second, screening costs are nonzero, or else parents would not respond to information changing the expected benefits of screening. Section 5.1 estimates parents' willingness to pay for screening.

Table 3 investigates whether providers' openings and closings increase screening primarily by increasing its salience. Columns 1-3 estimate equation (8) controlling for screening rates at the neighborhood-cohort level: these estimates are virtually indistinguishable from those in Panel A of Table 1, suggesting that information cannot fully explain the effect of travel costs. Next, I study how the travel elasticity of screening changes with information. Column 5 focuses on siblings, and interacts distance to provider with indicators for younger siblings of tested children as well as younger siblings of children with EBLLs.[27] Travel costs have larger effects for younger siblings with screened older siblings, unless the older sibling has an EBLL. I interpret these coefficients as suggesting that a negative test for one child decreases the expected benefits from screening their sibling, thus making parents more cost-sensitive. Viceversa, a positive test reduces the sensitivity to travel distance. Column 6 repeats this exercise but interacting distance to providers with an indicator for a child living in the same building having an EBLL and finds similar patterns.

[27] Column 4 verifies that the main result holds in this sibling sample.

4.1.1 Robustness Checks

The estimated effect of screening costs is robust to different specifications, samples, travel costs measures, functional forms, and outcome definitions. Table 1 shows robustness to controlling for different location fixed effects, suggesting that spurious correlation does not drive my findings. Moreover, I estimate similar elasticities for children at different distances from providers, although absolute changes appear to have larger effects at smaller distances (Appendix Table A.13).

Appendix Table A.10 explores different specifications and distance measures. Column 3 controls for Census block group trends to assuage concerns that neighborhood changes, such as gentrification, drive the estimated relationship between screening rates and distance to providers. Column 4 uses average distance from the closest five providers as households do not always visit the closest provider. While attenuated with respect to my preferred estimate, the coefficient on this variable is negative and significant. Column 5 uses distance from the Census block centroid to remove variation in travel costs due to children living in different buildings within the same block, yielding estimates that are not statistically distinguishable from my preferred estimate. Related, Appendix Table A.14 includes both distance to the closest provider and distance to the five closest providers: because distance to closest provider has a higher explanatory power, provider density does not appear to drive my findings.

Logistic and ordinary-least-square regressions that include regressors' block-level means but omit block fixed effects yield similar results to my preferred linear probability model (Appendix Table A.15). This approach avoids the incidental parameters problem (Neyman & Scott 1948) and is equivalent to the linear fixed effects model if there is no correlation between the relevant regressors and the fixed effects (Mundlak 1978, Chamberlain 1984, Bafumi & Gelman 2016). This equivalence is important because Section 5.1 uses the logit framework to estimate households' willingness-to-pay for screening. This table also shows robustness to measuring screening at different ages, consistent with most children being screened by age two (Appendix Figure A.10).

4.2 Does Distance to Providers Affect Selection into Screening?

Distance to providers decreases screening take-up, but for whom is theoretically ambiguous. On one hand, families with low exposure risk will not be willing to travel farther. On the other hand, children facing high travel costs, who may also be at high risk, may forego screening.

To assess how the composition of screened children changes with distance, I estimate equation (8) on the sample of screened children, with children's characteristics as outcomes. I examine ex-ante observable and unobservable exposure risk, as measured by housing age and lead levels. Consider two children, one in an old house and one in an adjacent new house. There is a clinic 250 meters away, and both get screened. Years later, two new children move in; the clinic is closed and the closest provider is now a kilometer away. Only the child in the old house gets screened. In this example, the probability that a screened child lives in an old home increases with distance: it is 0.5 at 250 meters and 1 at one kilometer. Data from this example would suggest that hassles improve targeting based on observable risk, as illustrated in Figure 2.

In contrast to this example, Table 4 does not support the hypothesis that children screened at farther distances have higher observable or unobservable risk: they are less likely to live in a home built prior to 1930, and have slightly lower BLLs (only significant when controlling for Census tract fixed effects). Children screened at higher distances are also slightly less likely to be black or hispanic, with significant estimates only when controlling for tract fixed effects. These findings are largely robust to including time-varying neighborhood controls (Appendix Table A.16).[28]

4.3 Does Proximity to Providers Improve Children's Outcomes?

Because distance decreases screening for high- and low-risk children alike, decreased provider access may hinder detection of lead-poisoning. If lower detection rates lead to fewer remediations, future residents may face increased risk. I investigate how distance affects prevention outcomes by estimating equation (8) with the following outcome variables: an indicator for EBLL detection

[28] Columns 4-9 of Appendix Table A.12 shows that these selection patterns are also visible in Chicago, where households appear more sensitive to distance.

(equal to 0 if the child is either not screened or has a BLL lower than $10\mu g/dL$), age at first and highest test, as well as indicators for remediations and subsequent EBLLs at the same location.

Table 5 shows that children who live one kilometer closer to a provider are 3.3 percent more likely to be diagnosed with an EBLL (Column 1). Because screening increases by 0.9 percent per kilometer (Table 1), the higher EBLL detection rate is likely due to both the extensive margin and selection on the intensive margin. Moreover, these children are screened six days earlier, and are younger when their highest BLL is recorded (Columns 2-3). Early detection may improve long-term outcomes by reducing exposure and enabling access to treatments, that Billings & Schnepel (2018) show can improve outcomes for lead-exposed children.

Next, I ask whether distance affects prevention and outcomes beyond the first lead-exposed child in a house. Columns 4 and 5 find no evidence that proximity to providers is associated with higher HUD-funded remediation activity at a child's home within three years of birth or with lower future EBLL rates at that home.[29] Column 5 rules out effects in the same order of magnitude as the direct detection effects. Summing up, these findings suggest that proximity to providers improves detection of lead exposure, potentially improving access to treatment for directly exposed children but without beneficial externalities on future residents through increased remediations. Potential explanations include further barriers to remediations, which might be stronger for high-risk families who tend to be of low socioeconomic status, as well as temporary remediations whose effectiveness might fade over time.

4.4 Does Providers' Quality Affect Screening?

The disparity between well-child visits and screening rates discussed in Section 2.1 suggests that providers may exercise discretion in screening. As providers' practices differ greatly (Mullainathan & Obermeyer 2019, Kwok 2019, Fadlon & Van Parys 2020, Silver 2020, Currie et al. 2016,

[29] I observe over 2,000 remediations and repeated EBLLs in the same home. My findings are robust to limiting the sample to children with a higher incidence of these events, as well as to correcting for small sample bias (Appendix Table A.17). My sample size allows for detection of a 7 percent effect on remediations, meaning 37 percent of the EBLL cases detected due to reduced distance would have to take up remediations for this analysis to be powered.

Van Parys 2016, Fletcher et al. 2014, Epstein & Nicholson 2009), I ask how access to providers of different quality affects screening take-up.

I regress a child's screening indicator on indicators for the presence of any provider and of a high-quality provider within given distances of a child's birth address. Screening quality measures include whether providers offer less-invasive capillary tests, adherence to screening guidelines, and screening rates. Importantly, while a provider's screening rate might reflect demand-side preferences, I consider the indicator for providers offering capillary testing as more directly capturing a provider's propensity to screen. Moreover, I test for sorting by investigating whether proximity to a provider with a good screening record has additional explanatory power over proximity to a provider who attended a top 20 medical school, because parents may more easily observe a provider's alma mater and select on that, than screening record. I estimate

$$Y_{igy} = \sum_k \beta_k \text{ProviderInK}_i + \sum_k \gamma_k \text{HighScreenQualityInK}_i + \sum_k \delta_k \text{Top20SchoolInK}_i + \pi X_{igy} + \eta_g + \xi_y + \varepsilon_i, \tag{9}$$

where $k \in \{< 1km, 2-5km, 5-10km, 10-20km\}$ and X_{igy} are individual and neighborhood level characteristics that may correlate with parents' and providers' screening propensity. Variation in proximity to providers of different quality in this equation is given by providers entries and exits as well as differences in children's addresses within a geography, as in equation (8).

Figure 5 shows that proximity to providers increases screening, and the more so if the providers are of high quality, using any quality variable. For example, screening increases by 2.5 percentage points when there is a provider within one kilometer, and further by 6.5 percentage points when that provider offers capillary testing. Figure 4 estimates that having a provider within one kilometer increases screening by 4 percentage points on average. The effect of general proximity is 29-58 percent the effect of supply-side providers' quality, depending on the quality measure. Because I estimate similar effects of proximity to high-quality providers when using capillary testing ability and screening-based quality, I interpret my findings as suggestive that providers' discretion matters beyond demand-side preferences. Moreover, screening-based quality measures have additional predictive power beyond a provider's alma mater, suggesting that these results are not driven by households with a higher propensity to screen choosing providers with better education.

5 Willingness-to-Pay for Screening and Policy Counterfactuals

Distance to providers decreases screening and poisoning detection without improving targeting. Could policies that increase screening improve outcomes for poisoned children and society? I exploit variation in travel costs to estimate households' willingness-to-pay (WTP) for screening and simulate the impact of five counterfactual policies increasing screening or remediations.

5.1 Exposure Risk and Willingness-to-Pay for Screening

I derive the WTP for screening by defining household i's utility from screening as

$$u_i = \alpha_i - \beta_i(\theta_i d_i + p), \tag{10}$$

where d_i is distance from provider, θ_i is opportunity cost of travel time, p is the price of a test, and α_i and β_i are preference parameters (Einav et al. 2016). Assuming that $\alpha_i = \delta^{\alpha} X_i + \varepsilon_i$, $\beta_i = \delta^{\beta} X_i$ and that ε_i follows a Type I Extreme value distribution, i's WTP for screening is $\frac{\alpha_i}{\beta_i} - \theta_i d_i - p$.

Table 6 estimates the marginal effect of distance on screening take-up of households with different characteristics using both the linear probability model in equation (8) and a logit model. To recover α_i while avoiding the incidental parameters problem (Neyman & Scott 1948), I include block-level means of controls. I then compute WTP using the logit estimates. Column 1 reports estimates for the whole sample, while other columns report estimates for subsamples, obtained by interacting distance to the closest provider with indicators for household characteristics.

The average household has a negative WTP for screening, yet households in the riskiest homes, those built prior to 1930, are willing to pay \$7.81 for screening on average (Table 6). Similarly, households with low socioeconomic status have a higher WTP for screening than better off households, consistent with their heightened risk even after controlling for housing age. Because Panels A and B of Table 6 do not show large differences in the elasticity to travel costs, β_i, across housing vintage, race, and ethnicity, these different WTPs suggest households have different valuations of screening benefits, α_i.

Because I do not observe the price each household pays for a test, which could vary with insurance status, I perform a bounding exercise. If all households face the same price, Table 6 implies that households in pre-1930 homes are willing to pay up to \$31.44 more than households in newer homes, as the test price would cancel out of the difference. If, instead, households in pre-1930 homes have no co-pay while low-risk households pay the maximum full price indicated in outreach materials in Illinois (\$43, as discussed in Section 2.2), the difference in WTP between high- and low-risk households becomes negative. Conversely, the difference widens to \$74.44 if riskier households pay full price (\$43) due to lack of insurance while low-risk households do not pay. Because households often do not visit their closest provider, I can further divide the WTP estimates by the average ratio between minimum and actual distance, 75-281 meters per kilometer (Table 1), yielding a difference in WTP of \$111.89-419.20. Still, my definition of travel costs likely overestimates WTP as high-risk households are less likely to drive meaning they need more time to travel a given distance.[30]

To interpret the magnitude of these WTP estimates, I need a measure of screening benefits. Under risk-neutrality and perfect information, benefits are the converse of the expected costs of lead poisoning as screening enables detection (Table 5) and potential treatment. Yet, the literature lacks rigorous and comprehensive estimates of the cost of an EBLL. The correlation between IQ losses and BLLs implies an expected lifetime cost of living in a pre-1930 home relative to a new home of \$910 (Schwartz 1994). While, this estimate does not account for unobserved innate ability correlated with lead exposure, it also omits the opportunity cost of the additional time parents spend caring for a poisoned child and additional damages not measured by test scores.

[30] Appendix Figure A.11 shows a negative correlation between car ownership rates and the share of homes built prior to 1930 for Census tracts with fewer than 50 percent of homes built prior to 1930.

5.2 Policy Counterfactuals

This section simulates the impact on EBLL detection of four screening policies and one remediation policy in the 2014 cohort as modeled in equation (6). First, I look at incentives for households and providers. Then, I look at a policy opening screening locations in each zip code. Finally, I evaluate a 100 percent screening requirement for children in homes built prior to 1930. Moreover, I compare these policies to subsidizing full remediation for addresses with EBLLs.

Table 7 reports the number of additional children screened and additional poisoning cases detected under each policy. I compute additional detection rates assuming that marginal children have the average poisoning rate in the 2014 cohort, based on my finding that hassles do not improve targeting (Section 4.2). When evaluating the screening mandate for old homes, I use the poisoning probability among children living in old homes. I compute the private benefits of each policy by summing the WTP for screening of the marginal households, $b_i - c_i$, estimated in Section 5.1.[31] I assume no prevention benefits from screening because Section 4.3 finds no evidence that proximity to providers reduces exposure of future residents. Examining the opportunity cost of using public funds for these policies is outside the scope of this paper.

Because estimates of the externality of lead exposure e_i are not available, for each policy I compute the per-child difference between the policy's private benefits and its costs. This difference indicates the minimum value of the average externality that would make each policy cost-effective. All the screening policies I study appear to be cost-effective for externality values lower than \$15,976, the estimated spillover effect that a lead-poisoned child imposes on their school peers Gazze et al. (2020). As this value omits the crime costs of lead poisoning, it underestimates its total externality, further implying the cost-effectiveness of the screening policies examined.[32]

[31] The reported private benefits estimates are not rescaled by the relationship between actual and closest distance discussed in the previous section, which would imply smaller private benefits for each policy.

[32] Heckman et al. (2010) estimate that 38–66 percent of the value of preschool programs is attributable to crime reductions. Specific to lead poisoning, Aizer & Currie (2019) find that a one-unit increase in BLLs is associated with an increase in the probability of detention for boys of 1.3 percentage points on a mean of 1.8 percent, while Grönqvist et al. (2020) find an increase in the probability of conviction by age 24 of 1.8 percentage points on a mean of 16.4 percent for an increase of roughly 1.6 units of BLLs, with effects manifesting for lead exposure levels of $7\mu g/dL$ or above. I perform a back-of-the-envelope calculation using juvenile detention costs alone for the US of \$588 per day (Justice Policy Institute 2020). Thus, the increase in the probability of detention/conviction suggests that the expected

First, I study the effect of incentivizing households for screening, following a large literature on immunization incentives (Banerjee et al. 2010, Bronchetti et al. 2015). To simulate a travel voucher households could receive upon screening, I assign incentives based on the zip code average realized travel distance, valued at 1.2 minutes per kilometer and $25 per hour ($10.5 on average). I identify the marginal children screened under this policy as those whose WTP turns from negative to positive under the counterfactual policy, weighting by the realized probability of screening for a given WTP. Column 1 of Table 7 shows that this policy's private benefits are positive but lower than the incentives disbursed as many inframarginal households receive subsidies.

Second, I consider a pay-for-performance (PFP) incentive for low-performing providers, a policy with mixed success (Li et al. 2014, Alexander & Schnell 2019). Under PFP, I assume that providers in high-risk zip codes with screening rates lower than 50 percent screen an additional 10 percent of random children in their catchment area. Column 2 of Table 7 shows that PFP would lead to screening around four times more children than the household incentive, but achieve a similar private benefit, due to poorer targeting.

Third, I simulate a provider opening at the centroid of each zip code without providers in 2014. In the past, lead screening was offered at the Special Supplemental Nutrition Program for Women, Infants, and Children (WIC), the single largest point of access to health-related services for low-income preschool children in the US (General Accounting Office 1999), and WIC status appears associated with increased screening (Vaidyanathan et al. 2009). Alternatively, pharmacies could be equipped with capillary screening kits at the cost $7.96 per test. While this policy would only screen 882 more children, the benefits for these marginal children appear to outweigh the program's cost (Column 3 of Table 7).

Fourth, I consider a mandate to screen all children in homes built prior to 1930, which leverages observable exposure risk to target screening. Column 4 of Table 7 shows that, compared to the screening incentive in Column 1, this policy yields fewer additional screenings and lower pri-

crime cost (for boys) of lead poisoning ranges between $7.64-10.58 per day of detention. With average detention lengths for severe offenses of 11.4 months (Loughran et al. 2009), this adds up to $2,613-3,618. It is noteworthy that this calculation is again an underestimate as it excludes victimization costs, for example.

vate benefits, but similar rates of poisoning detection. This result is consistent with the finding in Section 4.2 that households do not self-select into screening based on better information about unobservable risk. Thus, the social planner may be able to target screening based only on observable risk. However, it may be prohibitively costly to screen all children in old homes.

Fifth, I consider a policy that keeps screening constant but assumes perfect remediation after EBLL detection, preventing new lead poisoning cases at homes with previous cases. In the 2014 cohort, 638 homes had an EBLL. Because 10.3 percent of addresses with EBLLs in the 2001–2003 cohorts have another child with EBLLs within 10 years, I assume that remediating these 638 homes would prevent 66 new cases. The average remediation cost in the HUD data for the 2010–2016 period is \$10,646, suggesting lead poisoning externalities need to be on the order of \$100,000 for remediations to be cost-effective in terms of prevention benefits only. Importantly, I do not have estimates of averted case management costs that would factor in prevention benefits.

This section evaluates the impact on EBLL detection of five screening and remediation policies. Overall, policies increasing screening rates have modest private benefits for marginal children, but may be cost-effective after taking into account lead-poisoning externalities as small as \$3,500. Specifically, I consider a screening subsidy, which allows households with the highest WTP at the margin to select into screening, and find that even this policy has small private benefits. Then, I consider supply-side policies such as a PFP incentive and an increase in provider locations, and find that while both have worse targeting outcomes than the screening subsidy, PFP leads to higher screening rates and thus higher poisoning detection rates. To better study targeting, I next consider a screening mandate in old homes, and find that it leads to similar poisoning detection rates as the subsidy, suggesting that households do not have private information on unobservable risks. Finally, I examine perfect remediation and find it not to be cost-effective because of the uncertainty in turnover of residents at each address. Importantly, this section does not consider additional welfare implications of changing provider access, for example stemming from other health outcomes.

6 Conclusion

This paper examines barriers to take-up of child blood lead screening in Illinois and evaluates counterfactual prevention policies. I find that distance to providers decreases screening rates but does not affect selection into screening based on either observable or unobservable exposure risk. Policies incentivizing screening have low private benefits, yet may be cost-effective when considering averted poisoning externalities.

My findings suggest that removing barriers to provider access, for example through travel subsidies, could increase screening and lead poisoning detection without reducing targeting efficiency. However, increased provider access is not associated with higher remediation activity, suggesting case management may need improvement. Outside the scope of this paper, provider training may also increase screening, as provider quality affects screening.

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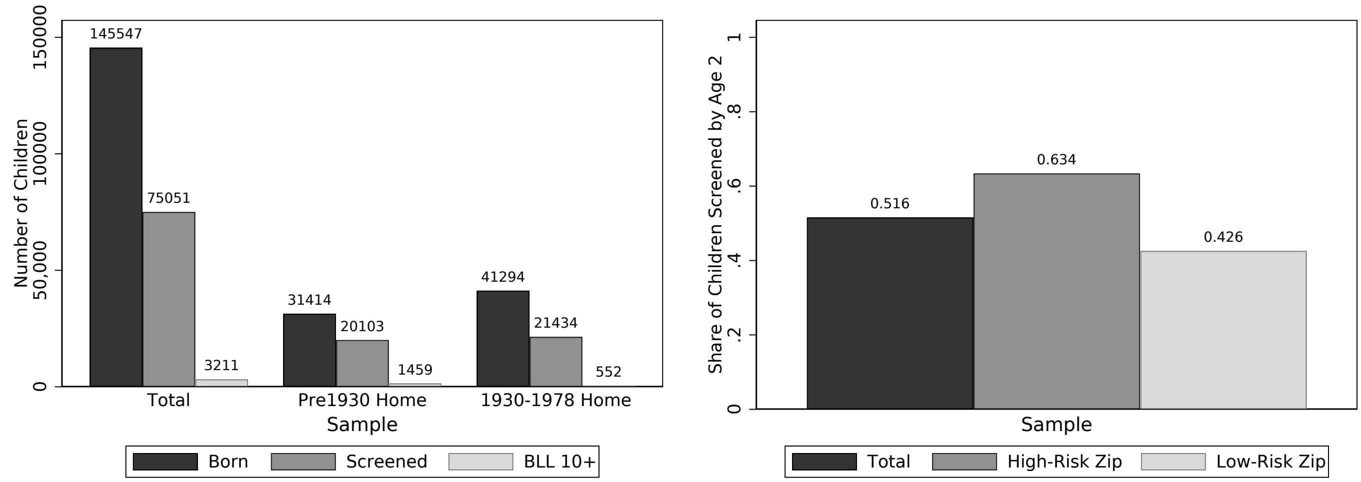
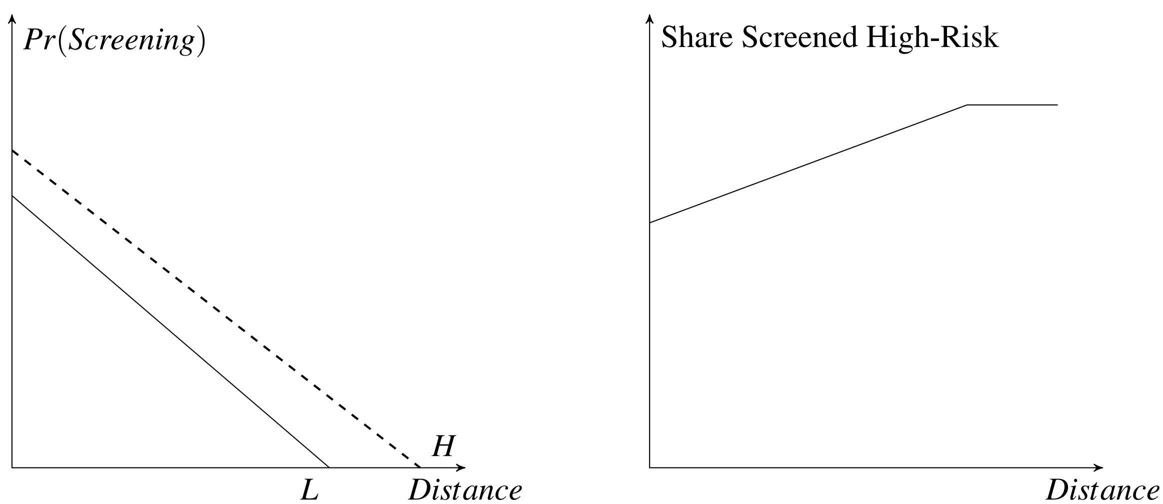
Figure 1: Lead Screening and Exposure Rates in the Illinois 2014 Cohort

(a) Number of Children Born, Screened, and with BLLs 10+ (b) Screening Rates by Zip Code Risk



Notes: The figure plots screening and exposure rates for children born in Illinois in 2014. Panel A plots the number of children born, screened, and with blood lead levels (BLLs) $\geq 10\mu g/dL$ in the whole sample and for the sample of children in pre-1930 and 1930-1978 homes. Panel B plots screening rates by age two by risk-level of the birth zip code.

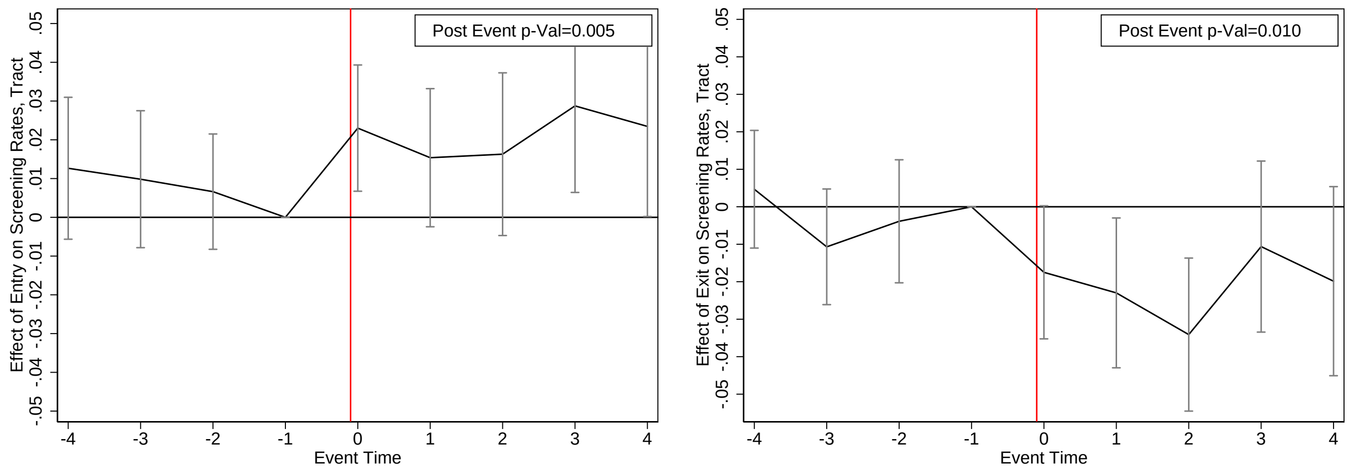
Figure 2: Relationship between Distance and Screening Rates, by Risk



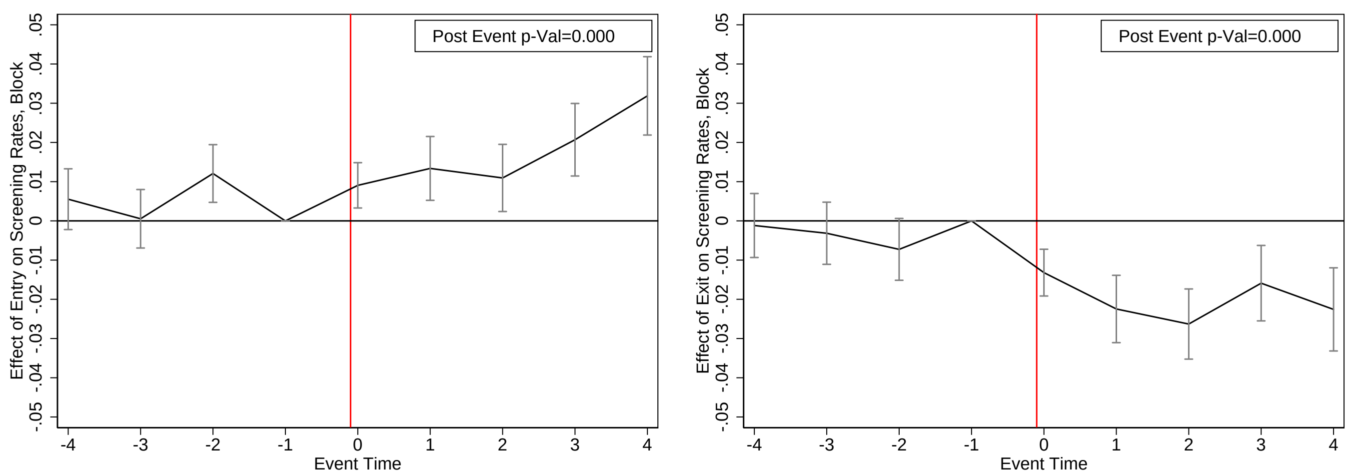
Notes: The figure illustrates the screening predictions from the ordeals model. The left panel plots hypothetical screening rates by distance for low risk (L) and high risk (H) households. The right panel plots the share of screened children who are high risk by distance as implied by the relationships plotted in the left panel.

Figure 3: Year-by-Year Effects of Openings and Closings

(a) Tract Level

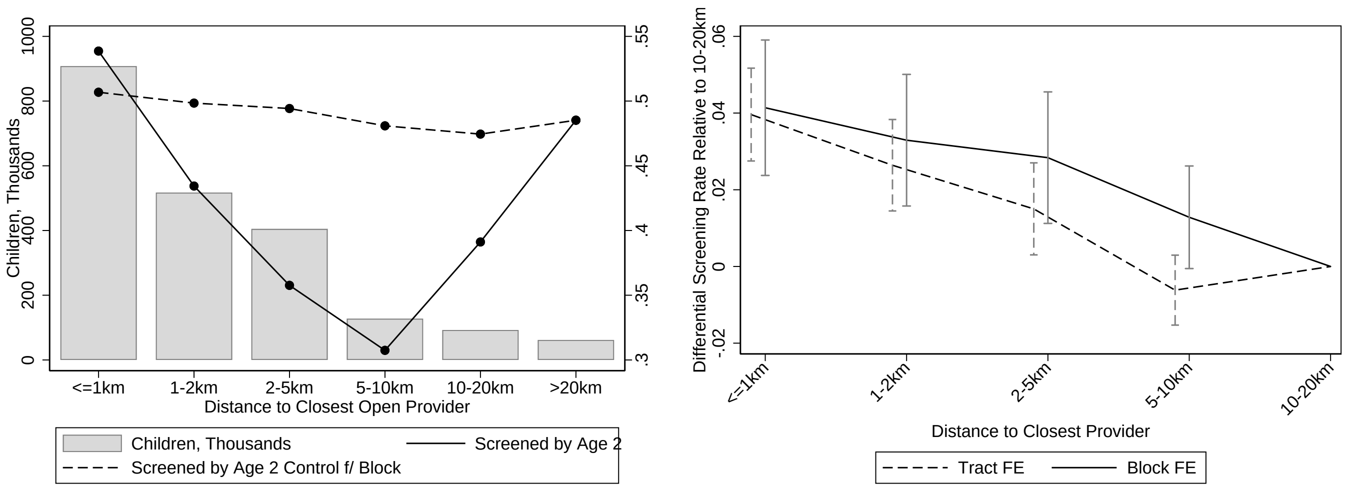


(b) Block Level



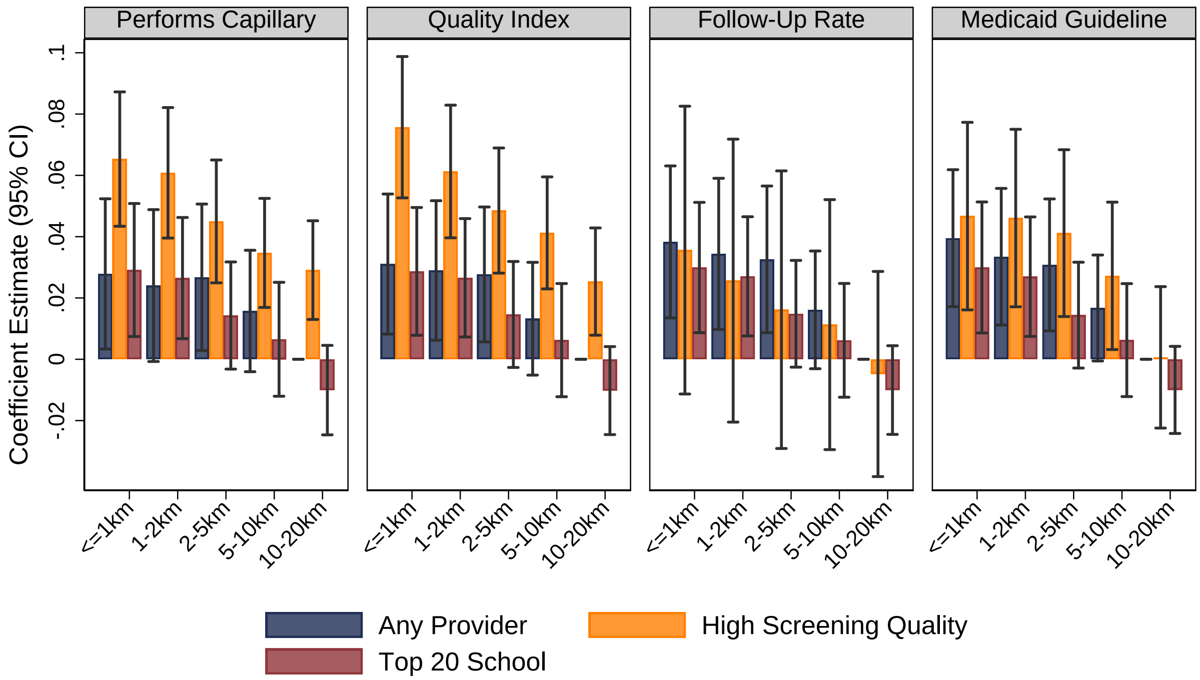
Notes: The figure plots DD coefficients on year-by-year entry and exit dummies, at the tract (Panel A) level, and block level (Panel B). Entries and exits are defined as changes in distance from the area centroid to the closest provider. The sample only includes areas with one entry and/or one exit over time. The outcome variable is the screening rate of children born in each area-year. Coefficients on entry and exit in each panel are estimated in a single regression. The vertical line indicates the entry or exit period. Year and area fixed effects are included. T-1 is the omitted category. The vertical bars are 95 percent confidence intervals. Standard errors are clustered at the tract and block level, respectively. Each graph reports the p-value for a test that the post-event coefficient is 0 from a related regression including only post-entry and post-exit indicators.

Figure 4: Determinants of Screening: Distance to Providers



Notes: The figure plots the average likelihood of a child being screened by age two by distance to closest open provider. The bars in the left panel show the number of children in each distance bin on the left y-axis, and the lines represents their screening rates on the right y-axis: the solid line plots raw means, while the dashed line plots residualized screening rates after controlling for Census block fixed effects. The right panel plots screening rates for each distance bin relative to children born 10 to 20 kilometers away from open providers controlling for tract fixed effects (dashed line) and block fixed effects (solid line), with vertical bars indicating 95% confidence intervals based on standard errors clustered at the zip code level.

Figure 5: Determinants of Screening: Provider Quality



Notes: Each panel plots coefficients from a regression estimating the effect of having any provider (blue bars), a high-quality provider based on the definition in each panel (orange bars), and a provider who attended a top 20 medical school (maroon bars) within each concentric buffer indicated on the x-axis on screening take-up. The quality index includes screening rates in a provider's catchment area, as well as a provider's rate of follow up within 90 days on cases of EBLLs and a provider's rate of adherence to Medicaid guidelines, that is the rate at which children on Medicaid screened by that provider at age one have a second test at age two. Providers' catchment areas are computed based on the median distance of children to their screening providers in my sample. Within catchement areas, I compute provider-level screening rates by weighting unscreened children by the inverse of their distance to the provider. The sample includes all geocoded children born 2001-2014 whose closest provider is within 20 kilometers. Each regression includes child-level controls, as well as birth year and block fixed effects. Vertical bars display 95% confidence intervals based on standard errors clustered at the zip code level.

Table 1: Determinants of Screening: Distance to Provider

	(1)	(2)	(3)	(4)	(5)
Panel A: Reduced Form. Dependent Variable: Screened by Age 2					
Distance to Closest Open Provider	-0.008***	-0.004***	-0.004***	-0.004***	-0.003***
	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)
Mean Outcome Variable	0.46	0.46	0.46	0.46	0.47
N	2050536	2050553	2050533	2018383	1463352
Panel B: First Stage. Dependent Variable: Distance Travelled					
Distance to Closest Open Provider	0.366***	0.281***	0.166***	0.075**	0.082*
	(0.035)	(0.029)	(0.028)	(0.032)	(0.048)
F-statistic	111.31	92.65	35.58	5.38	2.92
Mean Outcome Variable	7.74	7.74	7.74	7.54	7.25
N	585587	585638	585544	543689	312669
Panel C: Second Stage. Dependent Variable: Screened by Age 2					
Predicted Distance to Choice Provider	-0.023***	-0.013***	-0.022***	-0.073***	-0.147
	(0.001)	(0.001)	(0.002)	(0.019)	(0.265)
Mean Outcome Variable	0.46	0.46	0.46	0.53	0.76
N	2047415	2048444	2040705	1527937	697536
Zip Code FE	X				
Tract FE		X			
Block Group FE			X		
Block FE				X	
Home FE					X

Notes: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. The table displays the impact of distance to providers on the likelihood of a child being screened by age two. The sample includes all geocoded children born 2001-2014 whose closest provider is within 20 kilometers. Panel A reports the effect of distance to the closest provider open during a child's birth year in kilometers. Panel B estimates the impact of distance to the closest provider open during the year of a test on the actual distance travelled to get the child's first test for the sample of screened children. Panel C estimates the effect of predicted distance from provider of choice on the likelihood a child is screened in the whole sample, where each column predicts distance from provider of choice using the first stage in Panel B in that column. Each column includes birth year fixed effects and a set of location fixed effects as indicated at the bottom of each column. Panels A and B include standard errors clustered at the zip code level in parentheses. Panel C includes standard errors from predictions bootstrapped in 500 iterations.

Table 2: Determinants of Screening: Spillovers Across Siblings

Dependent Variable Screened within:	30 Days (1)	90 Days (2)	180 Days (3)	360 Days (4)	720 Days (5)
Panel A: Effects on Any Siblings					
Sibling with BLL 10+	0.008***	0.018***	0.026***	0.027***	0.025***
	(0.002)	(0.004)	(0.004)	(0.005)	(0.005)
Mean Outcome Variable	0.03	0.09	0.17	0.33	0.50
N	195503	195503	195503	195503	195503
Panel B: Effects on Younger Siblings					
Sibling with BLL 10+	0.006***	0.016***	0.026***	0.032***	0.032***
	(0.002)	(0.003)	(0.004)	(0.005)	(0.005)
Mean Outcome Variable	0.02	0.07	0.14	0.29	0.51
N	132926	132936	132970	133064	133192
Panel C: Effects on Older Siblings					
Sibling with BLL 10+	0.019***	0.031***	0.039***	0.029***	0.025***
	(0.004)	(0.007)	(0.008)	(0.008)	(0.007)
Mean Outcome Variable	0.04	0.09	0.16	0.29	0.36
N	81022	81022	81022	81022	81022
Mother FE	X	X	X	X	X
Test Year FE	X	X	X	X	X
Test Month FE	X	X	X	X	X
Birth Order FE	X	X	X	X	X

Notes: $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$. The table displays the impact of a sibling having a BLL of 10-14$\mu g/dL$ relative to 5-9$\mu g/dL$ on the probability that a child is tested within 1, 3, 6, 12, or 24 months of the sibling's test as indicated at the top of each column. Panel A reports the effect on any sibling, independently of birth order. Panel B estimates the effects on younger siblings, and Panel C on older siblings. Each regression includes mother, birth order, year, and month of test fixed effects. Standard errors clustered at the zip code level are in parentheses.

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Table 3: Determinants of Screening: Information

Dependent Variable:	Screened by Age 2					
	(1)	(2)	(3)	(4)	(5)	(6)
Distance to Closest Open Provider	-0.003***	-0.003***	-0.005***	-0.003***	-0.002***	-0.004***
	(0.000)	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)
Share Tested in Neighborhood-Cohort	0.427***	0.171***	-0.109***			
	(0.017)	(0.011)	(0.005)			
Distance to Closest Open Provider X Born after Tested Sibling					-0.006***	
					(0.002)	
Distance to Closest Open Provider X Born after Sibling with BLL 10+					0.007***	
					(0.003)	
Born after Tested Sibling					-0.553***	
					(0.009)	
Born after Sibling with BLL 10+					-0.022*	
					(0.012)	
Distance to Closest Open Provider X BLL 10+ within a Year of Birth within 15m						0.012***
						(0.001)
BLL 10+ within a Year of Birth within 15m						0.058***
						(0.004)
Mean Outcome Variable	0.46	0.46	0.46	0.46	0.46	0.46
N	2050429	2049377	1404918	1052305	1052305	2018383
Children in Neighborhood-Cohort	69.02	26.00	4.95			
Tract FE	X					
Block Group FE		X				
Block FE			X			X
Mother FE				X	X	

Notes: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. The table displays the impact of distance to the closest provider open during a child birth year on the likelihood a child is screened by age two. Columns 1-3 control for the share of screened children born in a child's neighborhood-cohort, with neighborhood defined at the bottom of each column. Columns 4-5 limit the sample to children with siblings and control for mother and birth order fixed effects. Column 6 controls for neighboring cases of blood lead level (BLL) above the intervention threshold of $10\mu g/dL$ Standard errors clustered at the zip code level in parentheses.

Table 4: Selection into Screening Conditional on Distance

Dependent Variable:	BLL 10+ by Age 2	Max BLL by Age 2	Home Pre1930	Black	Hispanic	Single Mother	Mother 20 or Younger	Mother High School or Less
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Tract and Year FE								
Distance to Closest Open Provider	-0.0003**	-0.0052*	-0.0047***	-0.0023***	-0.0021***	-0.0019***	-0.0002	-0.0008
	(0.000)	(0.003)	(0.001)	(0.000)	(0.001)	(0.001)	(0.000)	(0.001)
Panel B: Block and Year FE								
Distance to Closest Open Provider	-0.0004	-0.0084	-0.0012**	-0.0004	-0.0002	0.0006	0.0002	-0.0004
	(0.000)	(0.007)	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)
Mean Outcome Variable	0.02	2.93	0.46	0.23	0.33	0.49	0.12	0.16
N	890091	890091	645177	890091	890091	890091	890091	890091

Notes: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. The table displays the impact of distance to the closest provider open during a child's birth year on selection into screening by age 2. The sample includes all geocoded children born 2001-2014 whose closest provider is within 20 kilometers and who are screened. Outcome variables are indicated in each column. BLL is short for blood lead level. Panel A reports the effects controlling for the child's birth tract, Panel B controls for child's birth block. Each regression includes birth year fixed effects. Standard errors clustered at the zip code level in parentheses.

TABLES

Table 5: Effect of Proximity to Providers on EBLL Detection, Detection Timing, and Prevention

Dependent Variable:	BLL 10+ Detected (1)	Age at First Test (2)	Age at Highest Test (3)	Remediation within 3 Years (4)	Future BLL 10+ Detected (5)
Distance to Closest Open Provider	-0.000269***	0.193423***	0.181126***	-0.000007	0.000024
	(0.000093)	(0.050784)	(0.049486)	(0.000023)	(0.000204)
Mean Outcome Variable	0.009	20.434	21.325	0.001	0.017
N	2018383	1194748	1194748	2018383	421920
Block FE	X	X	X	X	X

Notes: $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$. The table displays the impact of distance to the closest provider open during a child birth year on the outcome indicated in each column. BLL is short for blood lead level. The sample includes all geocoded children born 2001-2014 whose closest provider is within 20 kilometers. Each column includes birth year and block fixed effects. Standard errors clustered at the zip code level in parentheses.

TABLES

Table 6: Heterogeneity in Willingness to Pay for Screening

Sample:	All	Home Vintage			Black		Hispanic		Single Mother		Mother 20 or Younger	
		Pre1930	1930-1978	Post1978	No	Yes	No	Yes	No	Yes	No	Yes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Logit Marginal Effects												
Distance to Closest	-0.010***	-0.008***	-0.009***	-0.008***	-0.010***	-0.005**	-0.010***	-0.004*	-0.014***	0.001	-0.011***	0.002
Open Provider	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Panel B: OLS Coefficients												
Distance to Closest	-0.006***	-0.005***	-0.005***	-0.006***	-0.006***	-0.005***	-0.006***	-0.005***	-0.008***	0.000	-0.006***	0.001
Open Provider	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Panel C: Average Willingess to Pay												
Average WTP ($)	-5.258***	7.808***	-5.140***	-23.632***	-5.892***	5.017***	-6.468***	5.101***	-5.362***	1.218***	-3.436***	2.721***
	(0.001)	(0.002)	(0.001)	(0.027)	(0.001)	(0.002)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)
P-value Equality w/ Ref		0.000	0.000		0.000		0.000		0.000		0.000	
Mean Outcome Variable	0.463	0.600	0.453	0.288	0.438	0.572	0.406	0.604	0.391	0.585	0.449	0.602
N	1451137	505167	578901	367069	1189347	261790	1036904	414233	916396	534741	1323733	127404

Notes: $^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$. The table displays the marginal effects of distance to providers on the likelihood of a child being screened by age two from logit (Panel A) and OLS (Panel B) models on different subsamples indicated in each column. Estimates for each set of columns, that is home vintages (Columns 2-4), race (Columns 5-6), ethnicity (Columns 7-8), mother's marriage status (Columns 9-10), and mother's age (Columns 11-12), are estimated in a single regression that interacts distance with the characteristic indicator in each column. Panel C reports average willingness-to-pay for screening for the average household in each subsample as estimated by the logit model in Panel A. The sample includes all geocoded children born 2001-2014 whose birth address matched a parcel record, and whose closest provider is within 20 kilometers and either opened or closed during their birth year. Each column includes birth year indicators, child-level demographic controls, and block-level averages of all included regressors. Standard errors clustered at the zip code level in parentheses.

TABLES

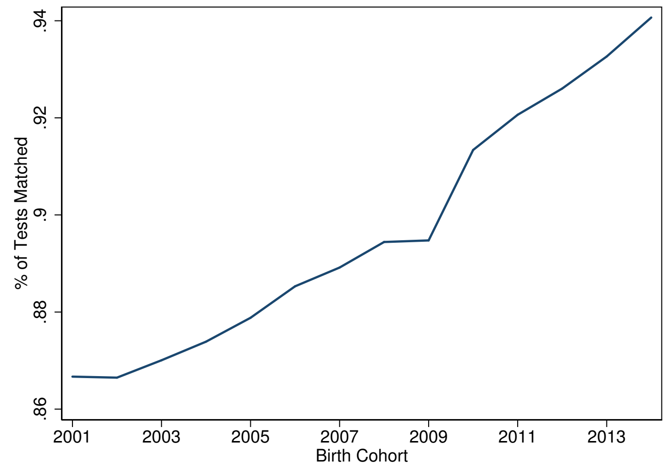
Table 7: Policy Counterfactuals

Policy:	Household Incentive (1)	Provider Incentive (2)	Diffused Screening (3)	Pre1930 Screening Mandate (4)	Remediation Follow-Through (5)
Additional Children Screened, 1,000	15.91	50.70	0.88	11.31	
Additional BLLs 10+ Detected, 1,000	0.14	0.43	0.01	0.15	
Change in Private Welfare, $1,000	370.09***	265.70***	9.92***	194.83***	
	(3.55)	(3.83)	(0.34)	(2.35)	
Prevention, $1,000					393.72***
					(4.70)
Cost, $1,000	434.71	1774.47	7.02		6792.15
Private Benefit - Cost, Per Child with BLL 10+, $1,000	-0.48	-3.50	0.39		

Notes: $^{*}p<0.10,^{**}p<0.05,^{***}p<0.01$. The table displays the impact of the counterfactual policies in each column on the 2014 cohort. Additional cases detected are the product of additional children screened and the poisoning probability in the 2014 cohort (0.0085) except in Column 4 which uses the poisoning probability conditional on living in an old home (0.0131). The sum of the additional children's willingness-to-pay (WTP) yields the private benefits of each policy. WTP is estimated in a logit model that includes demographic and block-group level controls. Household incentives average $10.5. Columns 1 and 3 count children whose WTP turns positive under the policy as additionally screened. Column 2 simulates increases in screening rates for low-screening providers in high-risk zip codes of 10 percentage points, at a cost of $35 per screening, the median increase in Medicaid payments analyzed by Alexander & Schnell (2019). Column 3 simulates providers opening at the zip code centroid for each zipcode-year cell without open providers, at $7.96 per test. Column 4 assumes remediations in 638 homes with blood lead levels (BLLs) $\geq 10\mu g/dL$ in 2014 prevent 66 new cases in the following ten years, at the baseline re-poisoning rate of 10.3 percent. Average remediation cost are $10,646 per house.

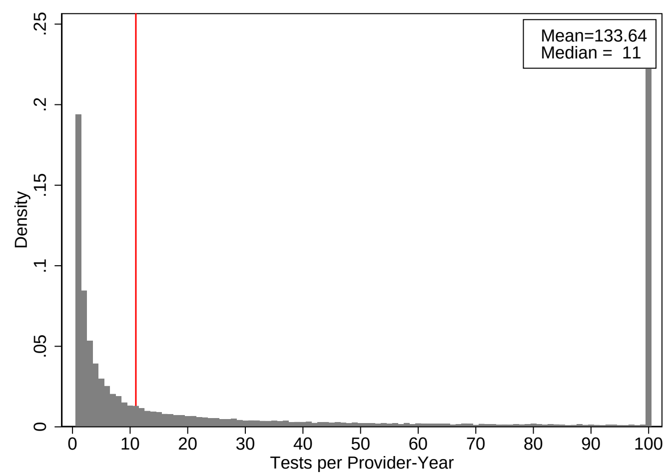
TABLES

Figure A.1: Match Rate between Blood Lead Levels and Birth Records



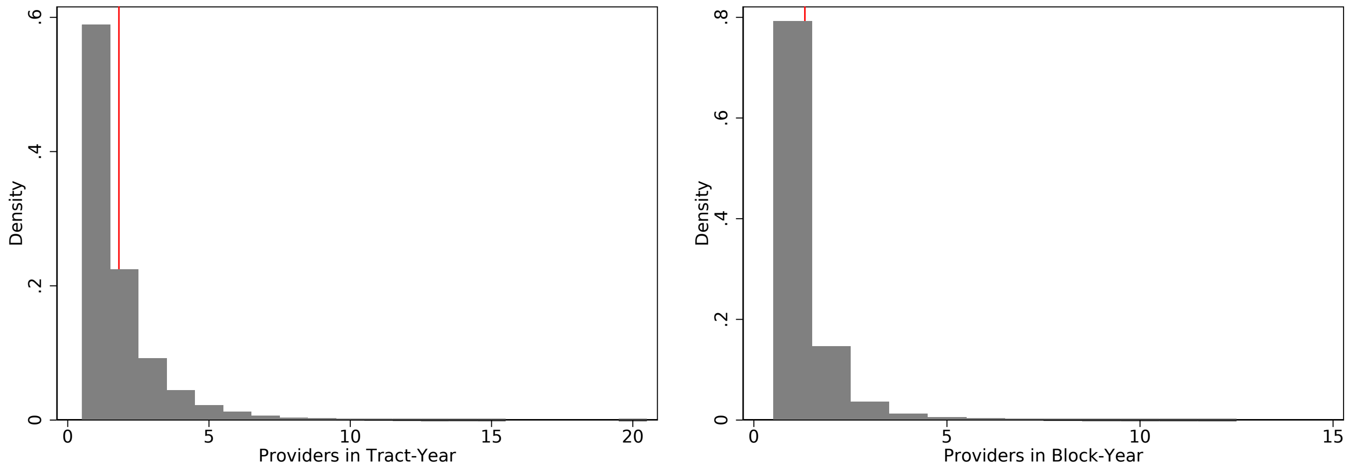
Notes: The figure plots the percent of tests successfully linked to birth records by birth cohort as recorded in the test data.

Figure A.2: Distribution of Tests per Provider-Year



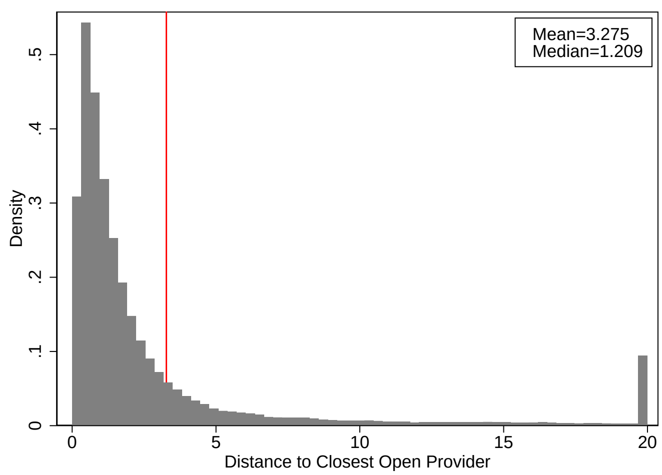
Notes: The figure plots the distribution of number of tests performed by providers in a year. The number of tests is censored at 100 for ease of visualization. The red vertical line indicates the median of the variable in the uncensored data.

Figure A.3: Distribution of Providers within Neighborhoods



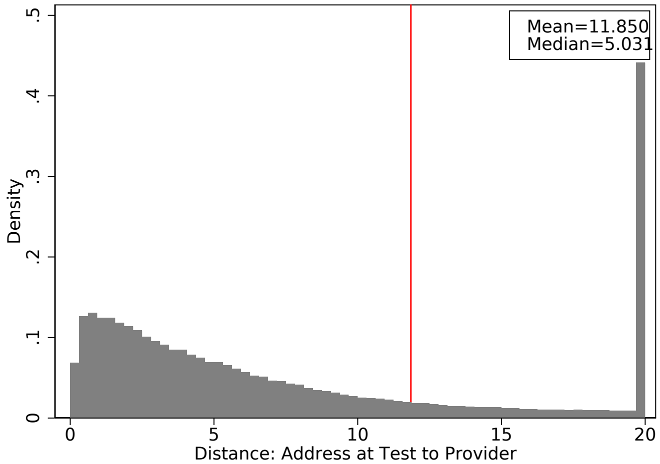
Notes: The figure plots the distribution of open providers at the tract-year (left panel) and block-year (right panel) level in Illinois, conditional on a neihgborhood having a provider. The vertical red line indicates the mean number of provider in a neighborhood-year.

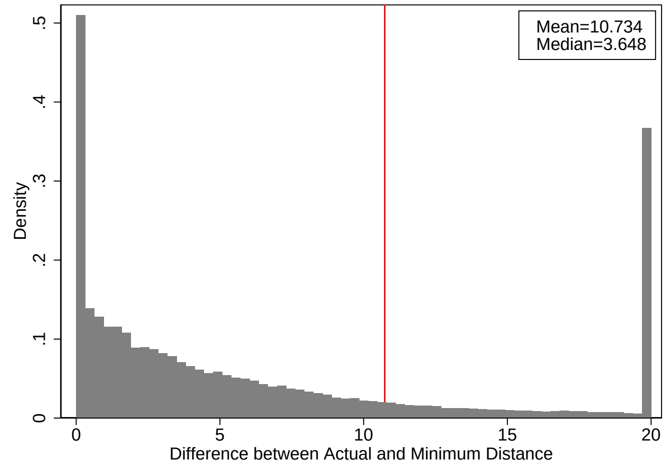
Figure A.4: Distance to Closest Providers



Notes: The figure plots the distribution of distance in kilometers from children's birth address to the closest provider open during the child's birth year. Distance is censored at 20km for ease of visualization. The red vertical line indicates the mean of the variable in the uncensored data.

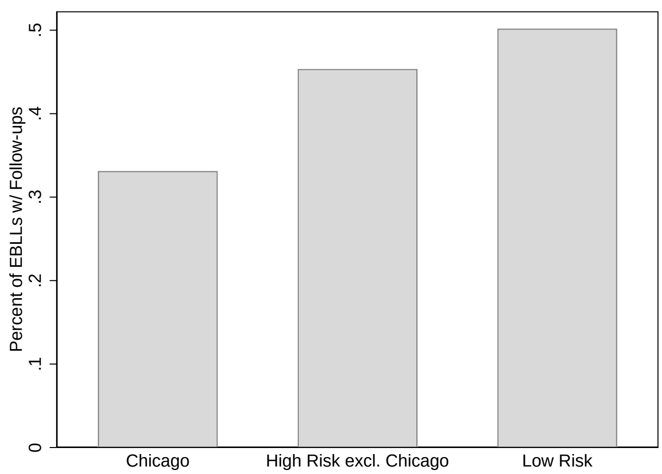
Figure A.5: Distance to Providers





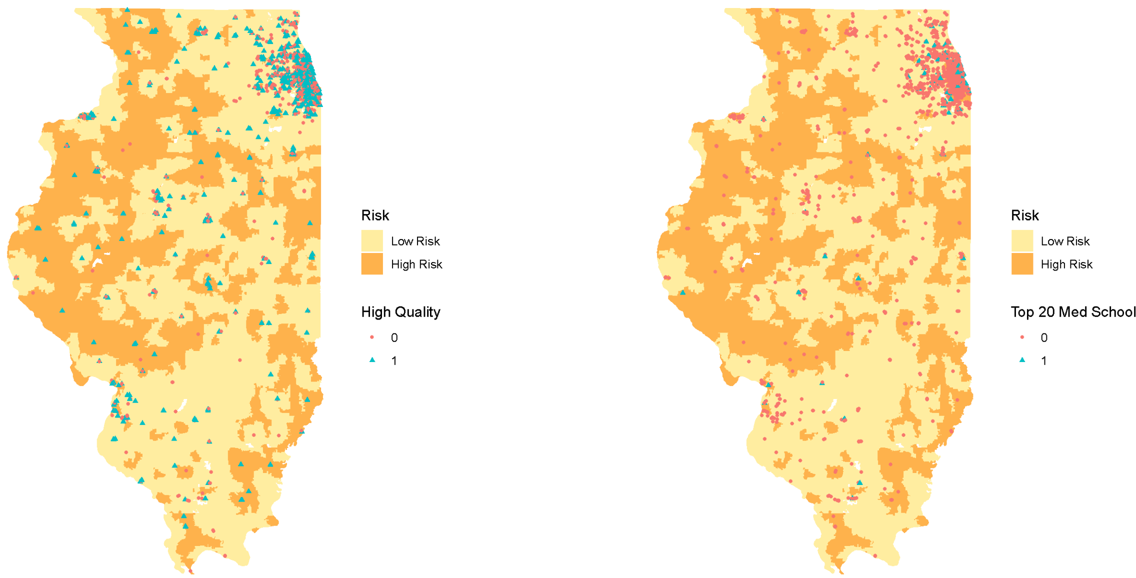
Notes: The left panel plots the distribution of distance in kilometers between children's address at test and the provider associated with the test. The right panel plots the distribution of the difference in kilometers between distance traveled at test and minimum distance between address at test and the closest active provider during the test's year. In both graphs, distance is censored at 20km for ease of visualization. The red vertical line indicates the mean of the variable in the uncensored data.

Figure A.6: Follow-up Rates



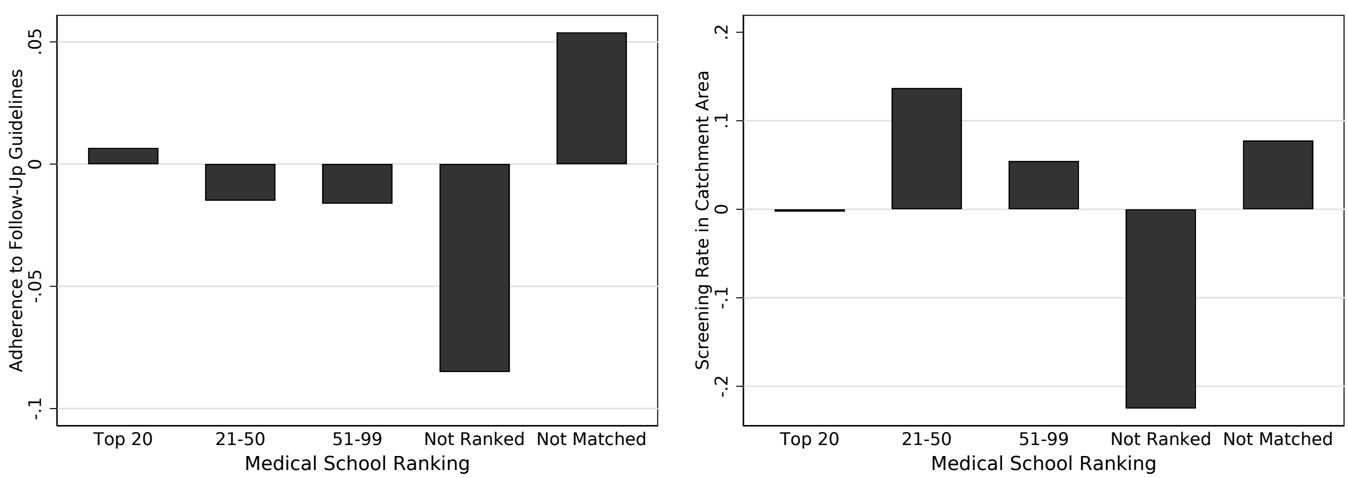
Notes: The figure plots follow-up rates in IL for tests that identify an elevated blood lead level (EBLL) by risk-level in birth zip code.

Figure A.7: Location of Providers, by Quality



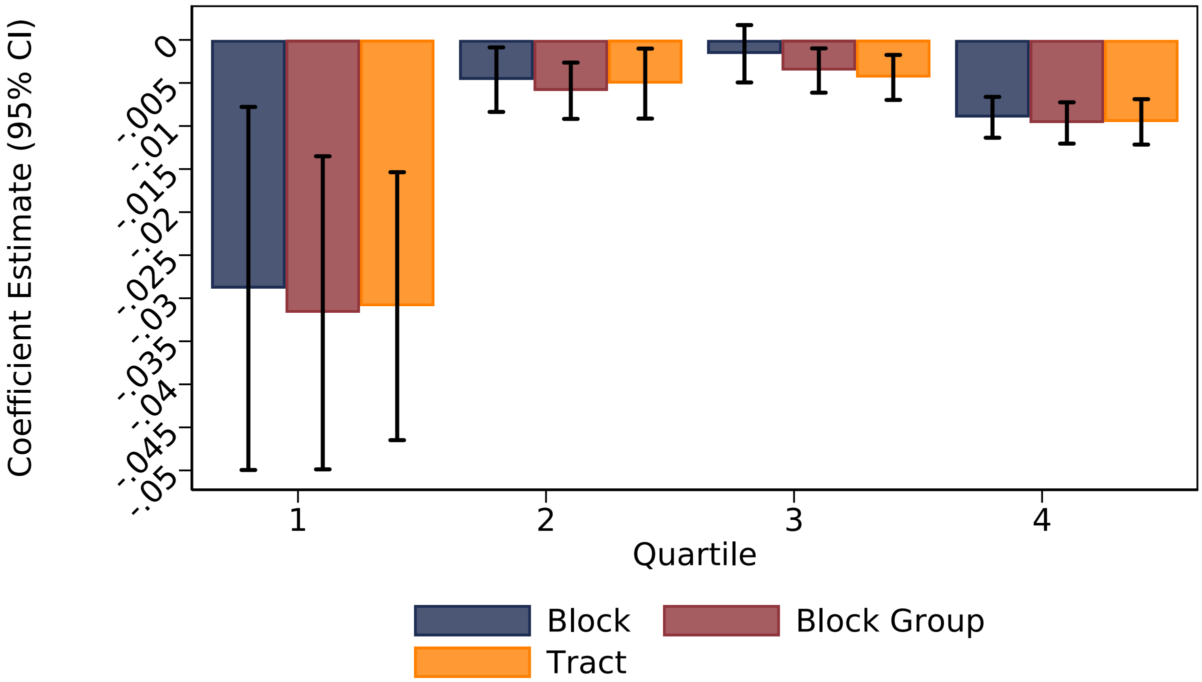
Notes: The figure plots the distribution of open providers by quality (left panel) and ranking of medical school of record (right panel) in Illinois in high and low risk zip codes over the years 2001-2014. High-quality providers are defined as having a quality index above median.

Figure A.8: Providers: Correlation in Quality Measures



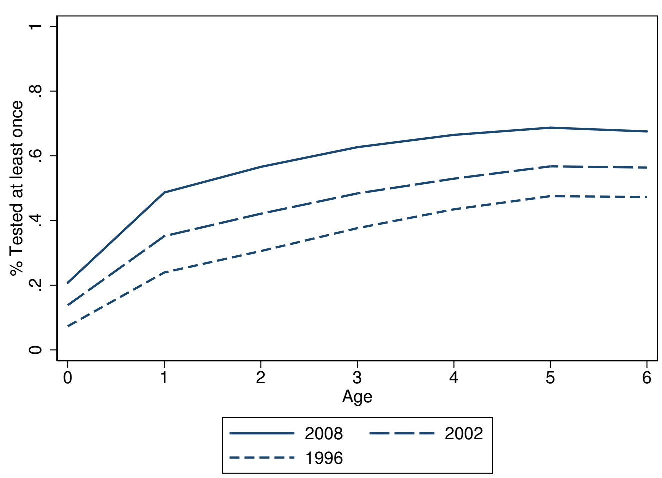
Notes: The figure plots on the y-axis the average z-scores of adherence to follow-up guideline (left panel) and screening rate (right panel) by ranking of the medical school each provider earned their degrees at on the x-axis.

Figure A.9: Determinants of Screening: Providers Distance, by Car Ownership



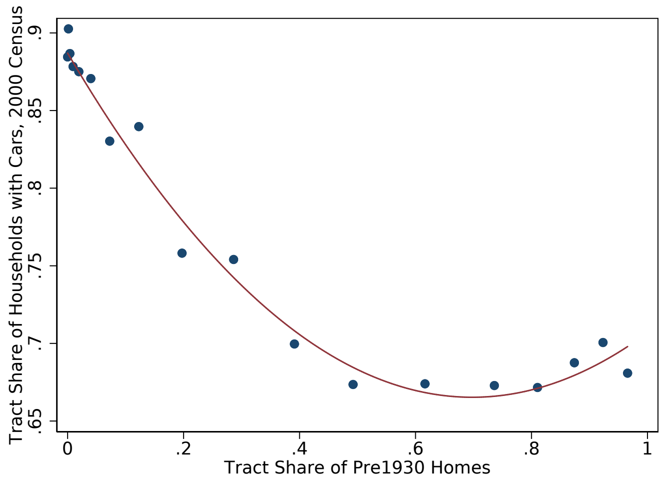
Notes: The figure plots the regression estimates of the effect of distance to closest provider on the likelihood that a child is screened by age two by quartile of car ownership rates in the child's Census tract, controlling for tract, block group, or block fixed effects. Tract-level car ownership in 2000 is measured in Census data. Vertical bars indicate 95% confidence intervals based on standard errors clustered at the zip code leve.

Figure A.10: Cumulative Distribution of Age at First Blood Lead Test



Notes: The figure plots the cumulative distribution of age of first test in Illinois over time.

Figure A.11: Correlation between Car Ownership and Housing Age



Notes: The figure plots the average car ownership rates by quantiles of share of pre-1930 homes in Census tract using 2000 Census data, and fits a quadratic line.

Table A.1: Summary Statistics: Children

Sample:	Whole Sample (1)	Screened Children (2)	Children with BLL 10+ (3)	Children Switching Providers (4)	Siblings Switching Providers (5)	Tested at Closest Provider (6)
Home Pre1930	0.348	0.451	0.765	0.539	0.508	0.483
	(0.476)	(0.498)	(0.424)	(0.498)	(0.500)	(0.500)
Home 1930-1977	0.399	0.391	0.196	0.362	0.379	0.378
	(0.490)	(0.488)	(0.397)	(0.481)	(0.485)	(0.485)
Low Income	0.278	0.366	0.561	0.464	0.436	0.433
	(0.448)	(0.482)	(0.496)	(0.499)	(0.496)	(0.496)
Black	0.179	0.226	0.360	0.318	0.307	0.196
	(0.383)	(0.418)	(0.480)	(0.466)	(0.461)	(0.397)
Hispanic	0.246	0.319	0.329	0.407	0.366	0.257
	(0.431)	(0.466)	(0.470)	(0.491)	(0.482)	(0.437)
Single Mother	0.384	0.490	0.632	0.617	0.585	0.487
	(0.486)	(0.500)	(0.482)	(0.486)	(0.493)	(0.500)
Mother 20 or Younger	0.091	0.119	0.184	0.170	0.135	0.122
	(0.287)	(0.324)	(0.387)	(0.376)	(0.342)	(0.327)
Mother Less than High School	0.012	0.019	0.012	0.020	0.018	0.023
	(0.109)	(0.137)	(0.110)	(0.139)	(0.131)	(0.150)
Mother High School, No Diploma	0.103	0.135	0.212	0.183	0.173	0.144
	(0.304)	(0.342)	(0.409)	(0.386)	(0.378)	(0.351)
BLL 10+ within a Year of Birth within 15m	0.054	0.079	0.690	0.118	0.102	0.084
	(0.226)	(0.269)	(0.463)	(0.322)	(0.302)	(0.278)
BLL 10+ within a Year of Birth 15-100m	0.104	0.139	0.099	0.193	0.174	0.117
	(0.305)	(0.346)	(0.298)	(0.395)	(0.379)	(0.322)
Chicago Born	0.283	0.380	0.487	0.446	0.413	0.242
	(0.450)	(0.485)	(0.500)	(0.497)	(0.492)	(0.428)
High Risk Zip excl. Chicago	0.169	0.204	0.282	0.213	0.216	0.307
	(0.375)	(0.403)	(0.450)	(0.409)	(0.411)	(0.461)
Screened by Age 2	0.456	1.000	1.000	0.865	0.713	1.000
	(0.498)	(0.000)	(0.000)	(0.341)	(0.452)	(0.000)
Highest BLL by Age 2	2.919	2.919	15.335	3.408	3.243	3.001
	(2.596)	(2.596)	(7.707)	(3.279)	(3.029)	(2.756)
BLL 10+ by Age 2	0.020	0.020	1.000	0.035	0.030	0.025
	(0.140)	(0.140)	(0.000)	(0.184)	(0.169)	(0.155)
Distance to Closest Open Provider	2.279	1.934	1.611	1.627	1.754	2.379
	(3.195)	(3.004)	(2.930)	(2.576)	(2.757)	(3.555)
Has Provider w/ Capillary in 1Km	0.308	0.382	0.455	0.424	0.400	0.420
	(0.462)	(0.486)	(0.498)	(0.494)	(0.490)	(0.494)
Has High Quality Provider in 1Km	0.295	0.374	0.478	0.424	0.398	0.429
	(0.456)	(0.484)	(0.500)	(0.494)	(0.489)	(0.495)
Has Provider w/ Top 20 Degree in 1Km	0.033	0.039	0.047	0.040	0.040	0.020
	(0.178)	(0.193)	(0.211)	(0.197)	(0.195)	(0.139)
N	2050536	934099	18779	391985	705976	39725

Notes: The table displays summary statistics for the covariates in the sample. BLL is short for blood lead level. Column 1 includes all geocoded children whose birth address matched a parcel record for birth cohorts 2001-2014. Column 2 limits the sample to screened children while Column 3 limits the sample to children with at least one BLL at or above $10\mu g/dL$. Column 4 limits the sample to children with multiple tests who switch providers across tests. Column 5 limits the sample to children in households with multiple siblings who switch providers across siblings. Column 6 limits the sample to children who are tested at their closest provider.

Table A.2: Summary Statistics: Matched and Unmatched Blood Lead Tests

Sample:	Unmatched		Matched	
	Mean	Standard Deviation	Mean	Standard Deviation
	(1)	(2)	(3)	(4)
Geocoded	0.729	0.445	0.762	0.426
Home Pre1930	0.540	0.498	0.545	0.498
Home 1930-1977	0.350	0.477	0.349	0.477
Low Income Block Group	0.458	0.498	0.457	0.498
Share Black in Tract	0.262	0.352	0.283	0.372
Share Hispanic in Tract	0.226	0.291	0.243	0.300
Fraction Less than High School	0.561	0.184	0.577	0.176
Chicago Residence	0.428	0.495	0.468	0.499
N	715273		4707326	

Notes: The table displays summary statistics for the unmatched (Columns 1-2) and matched (Columsn 3-4) tests in the sample. Housing age and Census characteristics of block group and tracts are based on the child's address at time of test.

Table A.3: Sample Size and Linkages

	Tests Linked to Test Address		Test Linked to Birth Address		Children with Birth Records
	# Tests (1)	# Children (2)	# Tests (3)	# Children (4)	# Children (5)
Total	5,403,722	2,653,402	5,403,722	2,653,402	4,465,487
Matched to Birth Record	4,692,618	2,166,694	4,685,569	2,160,081	4,465,487
Geocoded	3,587,020	1,820,517	4,167,897	1,903,385	3,847,728
Born between 2001-2014	2,664,302	1,392,758	2,935,018	1,281,933	2,123,496
Linked to Parcel Data	1,926,388	1,007,129	2,144,859	890,637	1,466,015
Drop follow-up	1,851,106	1,004,026	2,064,753	890,637	1,466,015
Linkage with Census Block Data	1,850,783	1,003,859	1,722,482	780,980	1,465,336

Notes: The table displays the number of tests and unique children in my original sample (first row) and those remaining after each data cleaning and linkage step.

Table A.4: Screening Rates and Average Blood Lead Levels

	Illinois		Chicago	
	Geocoded	Non-Geocoded	Geocoded	Non-Geocoded
Screening Rate (%)	60%	58%	76%	74%
Avg. Blood Lead Level (ug/dL)	2.55	2.52	2.40	2.39

Notes: The table displays the screening rates and average blood lead levels in Illinois and Chicago, respectively, in the sample of geocoded (Columns 1 and 3) and non-geocoded (Columns 2 and 4) births (for screening rates) and tests (for average blood lead levels).

Table A.5: Sample Size and Extent of Lead Exposure

	Number of Tests, Excl. Follow-Up (1)	Number of Tests, Excl. Follow-Up, Linked to Covariates (2)	Number of Children (3)
Panel A: Any Test Type			
Total	2,557,184	1,594,313	953,749
Elevated (>10ug/dL)	77,919	37,310	27,175
Confirmed Elevated	70,171	32,319	22,579
Panel B: Capillary Tests			
Total	990,734	729,945	512,185
Elevated (>10ug/dL)	25,463	15,384	14,125
Confirmed Elevated	17,715	10,393	11,305
Panel C: Venous Tests			
Total	1,566,449	864,367	538,225
Elevated (>10ug/dL)	52,456	21,926	14,827

Notes: The table displays the number of tests (Column 1), number of tests excluding those that are within 90 days of a previous test (Column 2), and the number of children (Column 3) in my sample (Total) and those that display elevated levels, for any test (Panel A), capillary (Panel B), and venous (Panel C). I show separately the number of confirmed capillary tests, that is capillary tests that are followed up by another elevated level within 90 days, be it venous or capillary.

Table A.6: Estimates of Fraction of Movers in Sample

Sample:	Siblings (1)	Tested Child (2)
Moved	0.356	0.331
Moved to House with Different Risk	0.154	0.150
Moved to Zip Code with Different Risk	0.082	0.069
N	480865	883816

Notes: The table displays the share of households estimated to move within a two year period in my sample. Column 1 indentifies movers among households with multiple children as those with a change in birth address between births. Column 2 indentifies movers among households with a tested child as those whose residence address at time of test differs from the birth address. Houses are defined as having different risk if one is built before 1930 and one after.

Table A.7: Summary Statistics: Providers

	All Providers (1)	Opening Providers (2)	Closing Providers
Years Open	8.172	4.648	11.274
	(6.051)	(3.697)	(6.391)
Individual Provider	0.242	0.220	0.180
	(0.428)	(0.414)	(0.384)
Top 20 Degree	0.029	0.028	0.029
	(0.168)	(0.165)	(0.169)
Top 21-50 Degree	0.174	0.151	0.190
	(0.379)	(0.359)	(0.392)
Unranked Degree	0.685	0.709	0.649
	(0.465)	(0.454)	(0.477)
Performs Capillary	0.636	0.541	0.780
	(0.481)	(0.498)	(0.414)
Fraction Years Accepting New Patients	0.500	0.408	0.608
	(0.404)	(0.406)	(0.369)
High Quality	0.703	0.758	0.667
	(0.457)	(0.429)	(0.471)
N	4542	2060	2189

Notes: The table displays summary statistics for the providers in the whole sample (Column 1) and for providers who enter or exit between 2001 and 2014 (Columns 2-3, respectively).

Table A.8: Lagged Determinants of Providers' Entry and Exit, Neighborhood Level

Dependent Variable:	Entry	Exit	Distance To Closest Provider	Entry	Exit	Distance To Closest Provider
Neighborhood Level		Tract			Block	
	(1)	(2)	(3)	(4)	(5)	(6)
Number of Providers	-0.0445***	0.1794***	-0.1424***	-0.0509**	0.2575***	-0.2172***
	(0.009)	(0.012)	(0.036)	(0.022)	(0.029)	(0.080)
Number of Births	0.0001	0.0001	-0.0024	0.0000	0.0000	0.0014
	(0.000)	(0.000)	(0.002)	(0.000)	(0.000)	(0.001)
Share Screened	0.0158	0.0126	-0.3001	0.0001	-0.0003	-0.0063
	(0.012)	(0.013)	(0.191)	(0.000)	(0.000)	(0.012)
Average BLL	0.0005	0.0009	-0.0598**	0.0000	0.0000	-0.0017
	0.001	(0.002)	(0.027)	(0.000)	(0.000)	(0.001)
Share Homes Pre-1930	0.0152	-0.0052	-0.2432	-0.0003	0.0000	0.0135
	(0.012)	(0.014)	(0.303)	(0.000)	(0.000)	(0.017)
Share Black	0.0122	0.0732***	0.1365	0.0003	0.0003	0.0024
	(0.025)	(0.028)	(0.243)	(0.000)	(0.000)	(0.013)
Share Hispanic	0.0227	0.0087	-0.1596	-0.0001	0.0001	-0.0129
	(0.021)	(0.023)	(0.203)	(0.000)	(0.000)	(0.009)
Share Single Mothers	0.0006	0.0150	-0.4121	0.0002*	0.0000	-0.0232**
	(0.015)	(0.016)	(0.342)	(0.000)	(0.000)	(0.011)
Share Mothers 20	-0.0486**	-0.0159	0.3604	-0.0003**	-0.0001	0.0139
or Younger	(0.020)	(0.026)	(0.392)	(0.000)	(0.000)	(0.013)
Share Mothers High School	0.0368**	0.0368**	0.0280	0.0000	0.0003	-0.0159
or Less	(0.019)	(0.018)	(0.247)	(0.000)	(0.000)	(0.011)
Mean Outcome Variable	0.0398	0.0535	2.8021	0.0005	0.0008	1.6101
N	32019	32019	32019	361900	361900	361830

Notes: $^{*}p < 0.10$,$^{**}p < 0.05$,$^{***}p < 0.01$. The table displays the correlates of the likelihood that a provider opens (Columns 1,4) or closes (Columns 2,5) and average distance to providers (Columns 3,6) in a given year at different neighborhood levels. Observations in Columns 1-3 are at the tract-year level and in Columns 4-6 at the block-year level. Characteristics are lagged by one year, and all reflect births except for blood lead levels (BLLs) and number of providers. Each column includes year fixed effects and the neighborhood fixed effects indicated at the top of each column. Standard errors clustered at the neighborhood level in parentheses.

Table A.9: Determinants of Screening: Distance to Provider

Dependent Variable:	Screened by Age 2				
	(1)	(2)	(3)	(4)	(5)
Distance to Closest	-0.008***	-0.004***	-0.004***	-0.004***	-0.003***
Open Provider	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Mean Outcome Variable	0.46	0.46	0.46	0.46	0.47
N	2050535	2050553	2050533	2018383	1463352
Zip Code FE	X				
Tract FE		X			
Block Group FE			X		
Block FE				X	
Home FE					X

Notes: $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$. The table displays the impact of the closest provider open during a child's birth year in kilometers on the likelihood of a child being screened by age two. The sample includes all geocoded children born 2001-2014 whose closest provider is within 20 kilometers. Each column includes birth year fixed effects and a set of location fixed effects as indicated at the bottom of each column. Standard errors clustered at the county level are in parentheses.

Table A.10: Determinants of Screening: Provider Access, Robustness Checks

Dependent Variable: Screened by Age 2	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Distance to Closest Open Provider	-0.0005**	-0.0028***	-0.002***	-0.001***	-0.004***	-0.003***	-0.005***
	(0.000)	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)
Distance to Closest Open Provider X 20+km Away		0.0027***					
		(0.001)					
20+km Away		-0.0239					
		(0.015)					
Black						0.047***	0.051***
						(0.004)	(0.005)
Hispanic						0.110***	0.110***
						(0.005)	(0.005)
Single Mother						0.051***	0.042***
						(0.004)	(0.004)
Mother 20 or Younger						0.016***	0.013***
						(0.002)	(0.002)
Mother High School or Less						0.005	0.006*
						(0.003)	(0.003)
Home Pre1930							0.050***
							(0.006)
Home 1930-1977							0.050***
							(0.004)
BLL 10+ within a Year of Birth within 15m							0.067***
							(0.005)
BLL 10+ within a Year of Birth 15-100m							0.014***
							(0.003)
Mean Outcome Variable	0.46	0.46	0.46	0.46	0.46	0.46	0.46
N	2076225	2076225	2050533	2018383	2018351	2018383	1434900
Block FE	X	X		X	X	X	X
Block Group FE & Trend			X				
Distance Measure: Avg of 5 Closest Providers				X			
Distance Measure: From Block Centroid					X		

Notes: $^{*}p < 0.10$,$^{**}p < 0.05$,$^{***}p < 0.01$. The table displays the impact of distance to the closest provider open during a child birth year on the likelihood of a child being screened by age two. Columns 4 and 5 use different distance measures per the bottom of those columns. The sample includes all geocoded children born 2001-2014 whose birth address matched a parcel record. Columns 3-7 limit the sample to children within 20km of an open provider. BLL is short for blood lead level. Each column includes birth year fixed effects and location fixed effects per the bottom of each column. Standard errors clustered at the zip code level in parentheses.

Table A.11: Determinants of Screening: Car and Transit Travel Times

Dependent Variable:	Screened by Age 2					
Sample:	Car			Public Transit		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Travel Distance						
Distance to Closest	-0.004***	-0.004***	-0.005***	-0.013***	-0.013***	-0.010*
Open Provider	(0.001)	(0.001)	(0.002)	(0.003)	(0.003)	(0.006)
Panel B: Travel Time						
Travel Time to Closest	-0.004***	-0.004***	-0.003**			
Open Provider (Minutes)	(0.001)	(0.001)	(0.001)			
Travel Time to Closest				-0.007***	-0.006***	-0.003
Open Provider (10 Minutes)				(0.001)	(0.001)	(0.002)
Mean Outcome Variable	0.46	0.46	0.46	0.50	0.50	0.51
N	245018	244930	193050	179664	179484	144794
Tract FE	X			X		
Block Group FE		X			X	
Block FE			X			X

Notes: $^{*}p < 0.10$,$^{**}p < 0.05$,$^{***}p < 0.01$. The table examines the relationship between travel times by car (Columns 1-3) and by public transit (Columns 4-6) and screening likelihood in a 12 percent random sample stratified by block group and birth year. For this sample, I used the STATA command georoute (Weber & Peclat 2016), based on the HERE API which limits free requests to 250,000 observations, to estimate travel times by car and public transit to the closest open provider. Public transit times are estimated for Wednesday October 16, 2019 at 10am. Panel A estimates the impact of distance to the closest provider open on the likelihood a child is screened in this subsample, for the routes the algorithm was able to find information for. Panel B estimates the effect of travel time in minutes (10 minutes for Columns 4-6) on the likelihood a child is screened. Columns 4-6 limit the sample to households with estimated travel times smaller than two hours. Each column includes year fixed effects and a set of location fixed effects for location indicated at the bottom of each column. Standard errors clustered at the zip code level in parentheses.

Table A.12: Heterogeneity in Screening and Selection by Zip Code Risk

Dependent Variable:	Screened by Age 2			BLL 10+ by Age 2, Screened			Pre1930 Home, Screened		
Sample:	Chicago	High Risk w/out Chicago	Low Risk	Chicago	High Risk w/out Chicago	Low Risk	Chicago	High Risk w/out Chicago	Low Risk
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Tract and Year FE									
Distance to Closest Open Provider	-0.011**	-0.002*	-0.003***	-0.001	0.000	0.000	-0.025**	-0.014***	-0.002**
	(0.005)	(0.001)	(0.001)	(0.002)	(0.000)	(0.000)	(0.010)	(0.004)	(0.001)
Panel B: Block and Year FE									
Distance to Closest Open Provider	-0.008	-0.001	-0.003***	-0.002	0.000	0.000	0.001	-0.002	-0.001
	(0.006)	(0.002)	(0.001)	(0.002)	(0.000)	(0.000)	(0.003)	(0.002)	(0.001)
Mean Outcome	0.61	0.55	0.34	0.03	0.03	0.01	0.67	0.53	0.13
N	576731	330241	1100179	350784	174208	356550	315276	102421	223041

Notes: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. The table displays the impact of distance to the closest provider open during a child birth year on the likelihood of a child being screened by age two (Columns 1-3), and on the probability that a screened child has a BLL 10+ (Columns 4-6), or lives in a pre1930 home (Columns 7-9) for different subsamples indicated in each column. The sample includes all geocoded children born 2001-2014 whose closest provider is within 20 kilometers. Each column includes birth year and tract (Panel A) or block (Panel B) fixed effects. Standard errors clustered at the zip code level in parentheses.

Table A.13: Determinants of Screening: Provider Access, Different Samples

Dependent Variable:	Screened by Age 2				
Sample:	10KM	5KM	2KM	1KM	0.5KM
	(1)	(2)	(3)	(4)	(5)
Distance to Closest Open Provider	-0.006***	-0.009***	-0.018***	-0.027***	-0.030
	(0.001)	(0.002)	(0.004)	(0.008)	(0.019)
Mean Outcome Variable	0.46	0.47	0.50	0.54	0.57
N	1933096	1809487	1407149	893976	412857
Block FE	X	X	X	X	X

Notes: $^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$. The table displays the impact of distance to the closest provider open during a child birth year on the likelihood of a child being screened by age two. The sample includes all geocoded children born 2001-2014 whose birth address matched a parcel record and who are born within the distance indicated at the top of each column from an open provider. Each column includes birth year and block fixed effects. Standard errors clustered at the zip code level in parentheses.

Table A.14: Determinants of Screening: Provider Access and Density

Dependent Variable:	Screened by Age 2		
	(1)	(2)	(3)
Distance to Closest Open Provider	-0.003***	-0.003***	-0.003***
	(0.001)	(0.001)	(0.001)
Distance to 5 Closest Open Providers	-0.001***	-0.001***	-0.001***
	(0.000)	(0.000)	(0.000)
Mean Outcome Variable	0.46	0.46	0.46
N	2050535	2050515	2018367
Tract FE	X		
Block Group FE		X	
Block FE			X

Notes: $^{*}p < 0.10$,$^{**}p < 0.05$,$^{***}p < 0.01$. The table displays the impact of distance to the closest and the five closest providers open during a child birth year on the likelihood of a child being screened by age two. The sample includes all geocoded children born 2001-2014 whose birth address matched a parcel record within 20km of an open provider. Each column includes birth year fixed effects and location fixed effects per the bottom of each column. Standard errors clustered at the zip code level in parentheses.

Table A.15: Determinants of Screening: Provider Access, Logit Model

Dependent Variable:	Screened by Age 1		Screened by Age 2		Screened by Age 6	
Specification:	OLS	Logit	OLS	Logit	OLS	Logit
	(1)	(2)	(3)	(4)	(5)	(6)
Distance to Closest Open Provider	-0.003***	-0.027***	-0.005***	-0.027***	-0.005***	-0.023***
	(0.001)	(0.006)	(0.001)	(0.006)	(0.001)	(0.005)
Home Pre1930	0.037***	0.197***	0.050***	0.225***	0.063***	0.281***
	(0.004)	(0.023)	(0.006)	(0.026)	(0.006)	(0.029)
Home 1930-1977	0.037***	0.203***	0.050***	0.226***	0.064***	0.280***
	(0.003)	(0.019)	(0.004)	(0.021)	(0.005)	(0.022)
Black	0.024***	0.135***	0.051***	0.219***	0.094***	0.417***
	(0.004)	(0.020)	(0.005)	(0.021)	(0.005)	(0.023)
Hispanic	0.089***	0.428***	0.109***	0.477***	0.127***	0.590***
	(0.005)	(0.022)	(0.005)	(0.023)	(0.005)	(0.024)
Single Mother	0.029***	0.130***	0.042***	0.184***	0.050***	0.256***
	(0.003)	(0.015)	(0.004)	(0.016)	(0.004)	(0.017)
Mother 20 or Younger	0.003	0.017*	0.013***	0.060***	0.019***	0.132***
	(0.002)	(0.010)	(0.002)	(0.009)	(0.002)	(0.012)
Mother Less High School or Less	0.002	-0.009	0.006*	0.024*	0.012***	0.091***
	(0.003)	(0.014)	(0.003)	(0.014)	(0.003)	(0.017)
BLL 10+ within a Year of Birth within 15m	0.049***	0.252***	0.067***	0.311***	0.043***	0.265***
	(0.005)	(0.020)	(0.005)	(0.022)	(0.003)	(0.020)
BLL 10+ within a Year of Birth 15-100m	0.009***	0.069***	0.014***	0.065***	0.013***	0.063***
	(0.003)	(0.013)	(0.003)	(0.013)	(0.002)	(0.012)
Marginal Effect of Distance to Closest Open Provider		-0.006***		-0.007***		-0.005***
		(0.001)		(0.001)		(0.001)
Mean Outcome Variable	0.32	0.32	0.46	0.46	0.61	0.61
N	1451137	1451137	1451137	1451137	1451137	1451137

Notes: $^{*}p < 0.10$,$^{**}p < 0.05$,$^{***}p < 0.01$. The table displays OLS coefficients and coefficients and marginal effects from logit models of the impact of distance to the closest provider operating during a child birth year on the likelihood of a child being screened by age 1 (Column 1-2), age 2 (Column 3-4), and age 6 (Column 5-6). The sample includes all geocoded children born 2001-2014 whose birth address matched a parcel record, and whose closest provider is within 20 kilometers. BLL is short for blood lead level. Each column includes birth year indicators and block-level averages of all included regressors. Standard errors clustered at the zip code level in parentheses.

Table A.16: Selection into Screening Conditional on Distance: Robustness Checks

Dependent Variable:	BLL 10+ By Age 2	BLL By Age 2	Home Pre1930	Black	Hispanic	Single Mother	Mother 20 or Younger	Mother High School or Less
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Tract and Year FE								
Distance to Closest Open Provider	-0.0003**	-0.0044**	-0.0043***	-0.0024***	-0.0026***	-0.0026***	0.0000	-0.0009**
	(0.000)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)
Panel B: Block and Year FE								
Distance to Closest Open Provider	-0.0001	-0.0003	0.0000	0.0001	0.0009**	0.0010*	0.0001	-0.0002
	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)
Mean Outcome Variable	0.02	2.99	0.46	0.24	0.38	0.48	0.12	0.16
N	697482	697482	645177	697482	697482	697482	697482	697482

Notes: $^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$. The table displays the impact of distance to the closest provider open during a child birth year on selection into screening by age two. The sample includes all geocoded children born 2001-2014 whose closest provider is within 20 kilometers and who are screened. Outcome variables are indicated in each column. Panel A reports the effects controlling for the child's birth tract, Panel B controls for child's birth block. Each regression includes birth year fixed effects as well as tract or block level time-varying controls such as average blood lead levels (BLLs) by age 2, share of pre1930 homes, share black, share hispanic, share single mothers, share teen mothers, and share of mothers with high school education or less. Standard errors clustered at the zip code level in parentheses.

Table A.17: Effect of Proximity to Providers on Prevention, Robustness Checks for Rare Events

Specification:	Low Income Block (1)	Block with Remediation (2)	Logit (3)	Penalized Logit (4)
Panel A: Remediation within 3 Years				
Distance to Provider	0.000043	0.000079	-0.009173	-0.008720
	(0.000)	(0.002)	(0.031)	(0.031)
Mean Outcome Variable	0.003	0.052	0.001	0.001
N	563938	54134	1636204	1636204
Panel B: Future BLL 10+ Detected				
Distance to Provider	-0.000717**	-0.003795**	0.008860	0.008919
	(0.000)	(0.002)	(0.011)	(0.010)
Mean Outcome Variable	0.073	0.136	0.035	0.035
N	437433	43008	1199562	1199562

Notes: $^{*}p < 0.10$,$^{**}p < 0.05$,$^{***}p < 0.01$. The table displays the impact of distance to the closest provider open during a child birth year on the likelihood of remediation within three years (Panel A) and of future poisoning (Panel B). BLL is short for blood lead level. The sample includes all geocoded children born 2001-2014 whose closest provider is within 20 kilometers, with further constraints indicated in each column. Standard errors clustered at the zip code level in parentheses, except for Column 4 which reports standard errors under the assumption of homoscedasticity.

Table A.18: Determinants of Lead Exposure

Dependent Variable:	Highest BLL by Age 2		BLL 10+ by Age 2	
	(1)	(2)	(3)	(4)
Home Pre1930	0.415***	0.316***	0.010***	0.008***
	(0.025)	(0.023)	(0.001)	(0.001)
Home 1930-1977	0.030*	0.067***	-0.001*	0.000
	(0.016)	(0.019)	(0.001)	(0.001)
Low Income	0.003		-0.002***	
	(0.014)		(0.001)	
Black	0.255***	0.180***	0.003	0.002**
	(0.050)	(0.026)	(0.002)	(0.001)
Hispanic	-0.161***	-0.115***	-0.007***	-0.004***
	(0.024)	(0.017)	(0.001)	(0.001)
Single Mother	0.025**	0.026**	0.001*	0.001***
	(0.010)	(0.011)	(0.000)	(0.001)
Mother 20 or Younger	0.038***	0.020	0.000	-0.001*
	(0.014)	(0.015)	(0.001)	(0.001)
Mother Less than High School	0.039	0.055*	0.003***	0.005***
	(0.028)	(0.031)	(0.001)	(0.001)
Mother High School, No Diploma	0.154***	0.149***	0.005***	0.005***
	(0.017)	(0.017)	(0.001)	(0.001)
BLL 10+ within a Year of Birth within 15m	2.281***	2.078***	0.167***	0.157***
	(0.133)	(0.135)	(0.010)	(0.010)
BLL 10+ within a Year of Birth 15-100m	0.231***	0.128***	0.008***	0.004**
	(0.024)	(0.028)	(0.001)	(0.002)
Mean Outcome Variable	2.97	2.99	0.02	0.02
N	671156	645177	671156	645177
Zip FE	X		X	
Block FE		X		X

Notes: $^{*}p<0.10$, $^{**}p<0.05$, $^{***}p<0.01$. The table displays estimates of the impact of various variables on a child's maximum blood lead level (Columns 1-2) and likelihood of having an elevated blood lead level (BLL) (Columns 3-4) by age two. The sample includes all geocoded children born 2001-2014 whose birth address matched a parcel record, and whose closest provider is within 20 kilometers. Each column includes birth year and block fixed effects. Standard errors clustered at the zip code level in parentheses.