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

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Abstract

Lead paint, a harmful environmental hazard, can still be found in millions of homes in the United States. Due to high inspection and clean-up costs, prevention programs target intervention to the relatively few homes where small children test positive for lead poisoning. Because children have to visit a doctor to get tested, only households willing to undergo this hassle self-select into screening. Is self-selection an effective targeting mechanism? I study screening take-up by analyzing geocoded 2001-2016 lead screening data on 2 million Illinois children. My empirical strategy exploits variation in travel costs due to healthcare providers' openings and closings. I find that travel costs reduce screening among low- and high-risk households alike, without improving targeting. Consistent with low poisoning rates, high-risk households are only willing to pay $4-29 more than low-risk households for screening. Despite poor targeting, screening incentives may be cost-effective because of the externalities of lead exposure.

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1 Introduction

Sources of lead exposure are still pervasive in US homes despite evidence that early childhood poisoning is associated with reduced IQ (Ferrie et al. 2015) and educational attainment (Aizer et al. 2018, Grönqvist et al. 2017, Reyes 2015*a*), and an increased risk of criminal activity (Aizer & Currie forthcoming, Feigenbaum & Muller 2016, Reyes 2015*b*, 2007). 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.[1] Remediating these homes so that children do not ingest or inhale lead dust could cost up to $37.9 billion, and would involve stripping or painting over the lead paint while the home is temporarily vacated.[2] Despite the prevalence of lead paint, poisoning rates are relatively low: at current levels, 2.2 percent of Illinois children born in 2014 had lead poisoning (Figure 1).[3] Thus, it is hard for policy makers to identify homes where clean-up would be socially beneficial, similar to difficulties arising when targeting energy efficiency programs (Boomhower & Davis 2014, Allcott & Greenstone 2017).

To identify homes requiring clean-up, lead poisoning prevention programs in the US rely on early childhood health screenings that reveal lead exposure. Because small children are not systematically in school, this approach hinges on families travelling to their doctor's office for lead screening. This sort of barrier to policy uptake is known as a *hassle* or *ordeal*, and hassles may explain why lead screening rates are lower than 60 percent even in areas where the State of Illinois requires universal screening (Figure 2).

This paper investigates the impact of ordeals on lead poisoning prevention. Specifically, what is the impact of higher screening costs? Do these ordeals improve targeting efficiency, or do they hinder timely 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 it may select out of the program to avoid these ordeals (Nichols & Zeckhauser 1982),

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

[2] Source: Author's calculation based on data from the Department of Housing and Urban Development.

[3] 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. In 2019, IDPH lowered the threshold to $5\mu g/dL$ following Centers for Disease Control and Prevention guidelines that recognize no safe level of lead exposure.

lowering health care costs. Households may have private information on lead hazards in their home if they know how well-preserved the paint coat is or if they have off-the-record property inspection results. However, Alatas et al. (2016) note that households with high potential benefits may also face higher costs per ordeal, for example because they do not have a car and thus must travel for a longer time to visit a doctor. In this case, ordeals may increase exclusion errors: poisoned children may be less likely to see a screening provider, leading to high private and social costs.

To study the effect of ordeals on lead poisoning prevention, I link geocoded administrative data on lead screening for the universe of over 2 million children born in Illinois between 2001 and 2014 to housing age information from assessor files. Screening data include information on realized poisoning risk for the subsample of screened children, and housing age data provide ex-ante observable risk for both screened and unscreened children. First, I estimate the elasticity of screening with respect to travel costs, where travel costs are proxied by distance to health care providers. To assuage concerns of endogeneity in households' location relative to providers, my empirical analysis exploits providers' openings and closings.[4] I compare children born in the same location in different years who face different sets of providers. 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 key identifying assumption needed 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 expect similar benefits from these additional services.

First, being 15 minutes farther away from a lead-screening provider (one-way) decreases the likelihood of screening by 9 percent, on average. Second, I find no evidence that households who get screened 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 timely detection of lead poisoning, but it does not increase take-up of remedia-

[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 forthcoming, Nguyen 2019, Lu & Slusky 2016, 2017, Lindo et al. forthcoming, Venator & Fletcher 2019).

tion 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 screening outcomes or medical school attended, increases screening more than proximity to low-quality providers, suggesting supply-side intervention may also affect screening.

Data on households' revealed preference for screening allow me to estimate the social value of the existing lead screening policy and counterfactual prevention policies. I use travel costs in the logit framework to estimate the willingness-to-pay (WTP) for screening of households in homes 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. Consistent with the low incidence of lead poisoning, I estimate that the average household in the most at-risk homes has a WTP for screening of \$6.14, \$4-29 higher than the the average low-risk household. Such a low WTP results in modest benefits for the marginal households under all counterfactual screening policies I examine. 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 (2019). By contrast, increasing remediations does not appear to be cost-effective.

This paper contributes to three strands of literature. First, a robust body of literature 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 forthcoming, Nguyen 2019, Lu & Slusky 2016, 2017, Einav et al. 2016, Lindo et al. forthcoming, Venator & Fletcher 2019). In the US, limited access to vaccines, including information barriers, scheduling challenges, and transportation costs, appears 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). I use travel costs to elicit households' willingness-to-pay for information about their exposure risks, related to a large environmental economics literature surveyed by Kuwayama & Olmstead (2015) that uses travel costs to estimate the recreational value of environmental amenities. My paper shows that travel costs decrease timely detection of lead hazards, potentially imposing a large externality on society.

Second, a large literature studies the targeting efficiency of welfare programs.[5] Hoffmann (2018) finds that poor Indian households are very elastic with respect to non-monetary prices, such as travel costs. Exploiting providers' openings and closings, I find no evidence of high-risk households differentially selecting into screening at higher distances, suggesting that households at high risk for lead exposure in the US might 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 2018, Einav et al. 2019).

Third, an emerging literature examines the efficacy of environmental regulations. Due to scarce resources, regulators often rely on self-reporting and imperfect monitoring, resulting in rampant non-compliance (Duflo et al. 2013, 2018, Gibson forthcoming, Reynaert & Sallee 2018, Vollaard 2017, Zou 2018). In this context, the ability to target resources for inspections and clean-ups can significantly improve environmental and public health outcomes (Greenstone & Meckel 2019). My paper sheds light on how health screening policies affect the detection of environmental hazards in private homes where universal inspections may be infeasible.

Section 2 discusses how travel distance may affect targeting efficiency in light of a model of households' screening decision. Section 3 describes the data I use in this paper. Sections 4 and 5 analyze screening take-up and the costs and benefits of different lead poisoning prevention policies.

2 Theoretical Framework

The first part of this section discusses how travel costs affect selection into screening, building on the classical work of Nichols & Zeckhauser (1982) and its extension by Alatas et al. (2016). The

[5] See Hanna & Olken (2018) for a review of research in developing countries.

second part discusses how the planner's screening rule may differ from the private optimum due to lead poisoning externalities.

2.1 The Household's Screening Take-Up Decision

I model screening as an insurance mechanism, with benefits if a child is found to be lead-poisoned. In my model, screening benefits derive from assignment of the lead-poisoned child to case management aimed at reducing lead poisoning damages.[6] Thus, I ignore potential benefits from 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 or an immunization shot.[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.

Let b_i be household i's perceived benefit from screening their child for lead exposure. Let the cost of screening child i, c_i, be a function of 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 a healthcare provider, d_i. Here I abstract from heterogeneity in p for simplicity, although the cost of a blood lead test in Illinois varies based on the child's insurance coverage.[10] Then, child i is

[6] Case management occurs mostly at home and includes nutritional education and information about 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 a sample of North Carolina children.

[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. 2018 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.

[10] While lead screening is fully covered for children enrolled in Medicare or All Kids, nominal prices range between \$0-43 for uninsured or private insurance. Source: `http://www.leadsafeillinois.org/uploads/documents/LeadSafeILDirectory061.pdf`. Accessed in June 2019. I discuss how this variation in prices affects my estimates of households' WTP for screening in Section 5.1.

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:

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

If screening benefits are increasing in risk, that is, if $b(r_i)$ and $b'(r_i) > 0$, riskier children will have a higher willingness-to-travel for screening, as predicted by the classic ordeals model (Nichols & Zeckhauser 1982). The higher the potential exposure, the higher the probability that screening detects lead poisoning and leads to timely intervention to remove the exposure source. Then, the cutoff is increasing in risk:

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

Figure 3 illustrates how risk affects the relationship between screening and distance. High-risk households are less sensitive to distance: their screening rates decline less sharply with distance than screening rates for low-risk households (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). Let a_i denote the family's assets, and assume that assets are negatively correlated with risk, $a'(r_i) < 0$, and that travel time is negatively correlated with assets. For example, assume 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, individual distance cutoffs may be either increasing or decreasing 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, and are therefore willing to travel only shorter distances on average. 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.

2.2 The Planner's Problem

The socially optimal level and targeting of screening may not coincide with the individual optimum. Lead exposure has externalities that may not be internalized by households: lead-poisoned children negatively affect their classroom peers (Gazze et al. 2019) and are more likely to engage in risky and criminal behavior (Aizer & Currie forthcoming, Feigenbaum & Muller 2016, Reyes 2015*b*, 2007). Detecting lead hazards following a lead poisoning case might also prevent exposure of future residents.

Thus, 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

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

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 ex-ante the externality each child's undetected poisoning would impose on society. However, the planner observes a proxy for

[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.

exposure risk at each home, namely housing age. In this case, a policy requiring screening based on observable risk may be better than allowing for self-selection based on private benefits. In my empirical analysis I estimate both the average prevention value of screening (Section 4.3) and the societal values of different counterfactual 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. Appendix Table A.1 provides child-level summary statistics for the variables included in the analysis.

3.1 Childhood Lead Screening Measures

The Illinois Department of Public Health (IDPH) collects children's blood lead records from physicians and laboratories. Federal guidelines mandate that all children on Medicaid must be screened for lead poisoning at ages one and two.[13] In addition, IDPH requires screening for all children living in high-risk zip codes, defined by housing age and demographic characteristics.

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). 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-

[13] The effects of lead exposure are worst in small children. Therefore, in the remainder of my paper I focus on screening and exposure by age two. The findings and conclusions carry through in the larger sample.

deceased children not linked to any tests as not screened. Appendix Tables A.2 and A.3 show the number of tests and unique children in my original sample, and the number remaining after each data cleaning and linkage step.

IDPH lead test records include test date, blood lead level (BLL), test type (capillary or venous), provider and laboratory identifiers, and Medicaid status. I compute a child's age at time of testing as test date minus birth date. Capillary tests are prone to false positives due to surface contamination with lead dust. 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 capillary test or a venous test. For each child, I keep the highest venous test when available, or the highest confirmed capillary test when available. Appendix Table A.4 reports the composition of tests in my sample, including 70,000 confirmed EBLLs from over 22,000 children. Laboratories have different minimum reporting limits, meaning BLLs are bottom-censored; I correct for these limits to obtain correct population estimates of lead exposure.[14]

Birth records also include data on 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 below) and Census block group median income from the 2015 American Communities Survey. After geocoding, I obtain a sample of over 2 million 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. In my sample, 37 percent of screened children move by age two.

3.2 Provider Access Measures

IDPH collects the name and address of providers who perform lead tests. Screening providers can be individuals, small groups of doctors, or hospitals. Appendix Table A.5 shows that 24 percent of

[14] I determine the cutoff for each laboratory based on the distribution of test results for that laboratory by both test type and year. Appendix Figure A.2 shows an example of a laboratory with a very apparent cutoff at $5\mu g/dL$. 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.

providers in my sample are individuals. 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 the market each year and 4.8 percent exit. Appendix Figure A.3 displays how providers' locations change from the beginning to the end of my sample.

To construct a measure of travel costs for all children in my sample, I calculate the distance "as the crow flies" between the child's birth residence and the closest provider open during the child's birth year.[15] While the median child has a provider within 1.2 kilometers (Appendix Figure A.4), households may not visit their closest provider due to preference for continued care after a move (Raval & Rosenbaum 2018) or insurance network constraints. The sample of screened children allows me to assess the relationship between distance to closest provider and distance to provider of choice. Appendix Figure A.5 shows that over 90 percent of children do not visit their closest provider, and the median household travels 5 kilometers for screening. Still, Figure 4 shows that distance to closest provider predicts actual distance travelled: if the closest provider is 1 kilometer farther away, a household travels on average an extra 3.6 kilometers (Appendix Table A.6).

The impact of nearby providers may depend on the quality of the available providers. I use the 2019 USNews ranking of the medical school the provider attended as one measure of quality, which has been shown to affect opiod prescription rates (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.[16,17] 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. For each provider, I compute their screening rate and their compliance rate with screening 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 house-

[15]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.

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

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

holds travel to see that provider, and I weigh unscreened children by the inverse of their distance.[18] 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 at 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.[19] I then aggregate screening rate and compliance rates with screening age and follow-up guidelines into a summary quality index. Finally, I consider a provider's ability to perform capillary tests as an indicator of quality, because capillary testing may reduce the barrier to screening if households are averse to venous blood draws.

Providers' screening-based quality measures and providers' medical schools may capture different aspects of a provider's practice. Indeed, Appendix Figure A.7 shows that these different measures are imperfectly correlated. One explanation is that a provider's screening record is influenced by their patient base: providers in neighborhoods with high shares of disadvantaged children have higher screening rates (Appendix Figure A.8).[20] Moreover, more educated households visit providers of higher observable quality, such as providers who attended higher-ranked medical schools, but may be less able to sort based on unobservable screening rates (Appendix Table A.7). My empirical analysis is robust to using different quality measures.

3.3 Childhood Lead Exposure Pathways

Although children can be exposed to lead through several channels, deteriorating lead paint, which was used in homes until 1978, is the most common source of lead exposure in Illinois (Abbasi et al. forthcoming). In this paper, I use a house's construction year to proxy for a child's observable risk of lead exposure. To do so, I link birth addresses to parcel-level housing data in the Zillow Transaction and Assessment Dataset that includes information on when each house was built.[21]

I define children living in homes built before 1930 as high-risk. Older homes have a higher

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

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

[20]Appendix Figure A.9 shows the location of providers of different quality in Illinois.

[21]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.

risk of lead paint hazards: HUD estimates 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). In related work using IDPH data, Abbasi et al. (2019) find that children living in homes built prior to 1930 have the highest BLLs, after controlling for children's demographic characteristics, zip code, and birth year fixed effects. Appendix Table A.8 replicates these estimates with binned construction year indicators and different sets of neighborhood fixed effects.

3.4 Lead Hazard Remediations

To measure lead hazard abatement following EBLL detection, I use data on addresses that receive remediation funding under HUD's lead hazard control programs.[22] 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: Travel Costs and Child Lead Screening

This section builds on the model in Section 2 to investigate how travel costs affect screening. First, I estimate the elasticity of screening with respect to travel costs. Second, I study how travel costs affect selection into screening. Third, I estimate the effect of travel costs on timely EBLL detection and the likelihood of hazard remediation. Fourth, I investigate how the quality of nearby providers affects screening.

To study the relationship between screening take-up and travel costs, I exploit changes in distance to providers over time due to providers' openings and closings. As providers open and close, children born at the same location but in different years face different sets of providers. This ap-

[22]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.

proach is internally valid if the timing of openings and closings is exogenous to trends in screening rates over time. This condition would be violated if providers open in areas where public health officials target campaigns to increase screening rates, or if providers open in richer, low-risk areas with decreasing screening rates. 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 relevant 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. Figure 5 plots the β_τ and γ_τ coefficients from estimating equation (7) at both the Census tract and block level. My estimates suggest that providers' entries and exits are not correlated with pre-existing trends in screening rates. Moreover, Appendix Table A.9 shows no correlation patterns between openings and closings and lagged neighborhood characteristics at the Census tract or block level.

Figure 5 suggests that providers' openings and closings provide exogenous variation in travel costs over time. I leverage this variation in a linear probability model that compares children born in the same location in different years, controlling for location and birth year fixed effects, by estimating the following equation:

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

where Y_{igy} is an outcome for child i born in neighborhood g in year y, d_i measures a child's distance to the closest open provider during 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. I cluster standard errors at the zip code level to allow for arbitrary correlation in exposure sources and screening behavior.

The next sections examine the effect of distance on different outcomes. First, I estimate the effect of travel costs on screening by looking at an indicator for whether a child is screened by age two. Second, I study selection by examining 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. Third, I estimate the effect of travel costs on timeliness of poisoning detection and remediation activity by looking at age at test and an indicator for a HUD-funded remediation at the address within three years.

4.1 Do Travel Costs Decrease Screening?

Children born in homes closer to providers have higher screening rates on average, and this pattern holds after controlling for location fixed effects (Figure 6). In the raw data, this relationship does not hold for children farther than ten kilometers from providers, but 93 percent of the children in my sample live within ten kilometers of a provider.[23] In my main analysis, I drop the 31,178 children who are farther than 20 kilometers from a provider (2.6 percent of the original sample), as they are likely very different from the rest of the sample. Columns 1–2 of Appendix Table A.10 show that including these outliers attenuates the estimated elasticity of screening with respect to travel costs, because these outliers have a lower elasticity.

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, implying an elasticity of -0.03. Because 1 kilometer to the closest provider translates into an extra 3.6 kilometers travelled to get screening (Appendix Table A.6: Column 4), it may be appropriate to divide this elasticity by 3.6, obtaining a value of -0.01. Einav et al. (2016) estimate that doubling the distance to a radiation facility reduces take-up of cancer treatment by 2 percent, implying an elasticity of -0.02, while Herbst & Tekin (2012) estimate an elasticity of -0.13 for take-up of childcare subsidy.

[23] Appendix Figure A.4 shows that on average, a child is 3.3 kilometers away from the closest provider, and the distribution is right skewed.

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 for reference. By car, it takes 2 minutes to travel one kilometer in Chicago and 1–1.5 minutes elsewhere in the state, on average (Agbodo & Nuss 2017).[24] Lead screening requires a single appointment, that is a two-way trip to the doctor. Therefore the estimates in Table 1 imply that a $12.50 increase in travel costs (a thirty-minute two-way trip at 1.5 minute per kilometer, 10 kilometers each way, and $25 hourly wage), decreases screening take-up by 9 percent.[25] These estimates are based on distance "as the crow flies" which is smaller than the distance implied by the road network suggesting travel costs per kilometer may be higher.

4.1.1 Robustness Checks

These estimates are robust to different specifications and alternative distance measures, functional forms, sample selection criteria, and outcome definitions. Table 1 shows that these estimates are robust to controlling for different sets of location fixed effects, suggesting that the location of providers' openings and closings is not correlated with children's characteristics that also affect their likelihood of screening. Estimates that control for building fixed effects, which are more stringent and reduce the sample, are not statistically different from those in my preferred specification with block fixed effects. Moreover, Panel B of Table 1 shows no evidence that the screening gradient with respect to distance is nonlinear.

Appendix Table A.10 explores different specifications. Columns 3 and 6–7 include child-level controls and Census block group trends. Controlling for neighborhood trends helps assuage concerns that neighborhood changes over time, such as gentrification, are driving the estimated relationship between screening rates and distance to providers. Columns 4 and 5 use different measures of distance. Column 4 estimates the elasticity of screening relative to the average distance from the closest five providers, to take into account that households do not always visit the closest

[24] Appendix Table A.11 shows that households in Chicago are more sensitive to distance, suggesting that transit availability does not mitigate ordeals in this case.

[25] Source: Bureau of Labor Statistics.

provider. The coefficient on this variable is attenuated with respect to my preferred estimate, but still negative and significant. Column 5 uses distance from the Census block centroid to remove distance variation due to children living in different buildings within the same block, yielding estimates that are not statistically distinguishable from my preferred estimate. Appendix Table A.12 shows that proximity to providers who accept new patients and patients on Medicaid matters most for screening take up. Appendix Table A.11 shows that travel costs affect screening similarly for first-born and younger children, suggesting that knowledge acquired by screening the first child does not change the elasticity to travel costs.

Appendix Table A.13 shows that logistic and ordinary-least-square regressions that include regressors' block-level means but omit block fixed effects yield similar findings to my preferred linear probability model. 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 group fixed effects (Mundlak 1978, Chamberlain 1984, Bafumi & Gelman 2016). This equivalence is important because Section 5.1 uses the logit framework to estimate the differential willingness-to-pay of different households for screening. Moreover, this table shows that my choice of focusing on screening by age two is without loss of generality, as I find similar effects of distance on screening by different ages, likely because most screening happens by age two (Appendix Figure A.10).

4.2 Do Travel Costs Affect Selection into Screening?

The previous section finds that travel costs decrease screening take-up. Section 2 discusses how the marginal child who opts into screening may change as costs increase. On the one hand, families with low exposure risk will not be willing to pay the higher travel cost. On the other hand, children facing high travel costs, who may also be at high risk, might forego screening. Thus, the effect of travel costs on selection is theoretically ambiguous. This section estimates how the composition of screened children changes with travel costs.

I estimate equation (8) on the sample of screened children, with children's characteristics as the dependent variable. I include ex-ante observable and unobservable exposure risk, as measured by housing age and lead levels. Consider two children living next to each other, one in an old house and one in a new house. There is a clinic 250 meters away, and both get screened. Years later, two new families with 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. Among the screened children in this example, the probability that a 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 3.

Table 2 does not support the hypothesis that the marginal child who is screened at farther distances has higher observable or unobservable exposure risk. In fact, children screened at higher distances have slightly lower BLLs and are less likely to live in a home built prior to 1930, although the BLL result is only significant when controlling for Census tract fixed effects. Consistent with ability to pay being a barrier to screening, 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. Appendix Table A.14 shows that these findings are largely robust to including time-varying neighborhood controls.

4.3 Does Proximity to Providers Improve Children's Outcomes?

The finding that travel costs decrease screening for high- and low-risk children alike suggests that increased travel costs may hinder detection of lead-poisoning cases. If lower detection rates lead to lower remediation rates in affected homes, future residents may face increased poisoning risk, too. This section investigates how travel costs affect the likelihood and timeliness of detecting an EBLL, as well as the likelihood of remediations and future EBLLs at the same location.

Column 1 of Table 3 shows that children who live one kilometer closer to a provider are 3.3 percent more likely to be diagnosed with an EBLL. Moreover, Columns 2 and 3 show that children one kilometer closer to providers are screened six days earlier on average, and are younger when their highest BLL is recorded. Early detection may improve long-term outcomes by reducing exposure (Billings & Schnepel 2018). Column 4 investigates the relationship between travel costs and HUD-funded remediation at a child's home. To allow enough time for remediation to happen after poisoning detection I examine the likelihood of remediations within three years of birth. I find no evidence that proximity to providers is associated with higher remediation activity. Consistent with the lack of impact of travel costs on remediations, Column 5 shows no evidence of lower future EBLL rates for homes closer to providers.[26] Limited effectiveness over time of remediation activity could also explain the lack of impact of travel costs on future poisoning rates.

This section studies the impact of travel costs on poisoning detection and poisoning prevention activities at a child's home. My findings suggest that travel costs may affect outcomes for poisoned children, but do not have significant spillovers on future residents. These results question the prevention value of screening policies, which I investigate in Section 5.

4.4 Does Providers' Quality Affect Screening?

One interpretation of the findings in this section is that after a provider exits, children have less access to health care in general, and forego lead screening as well as other health treatments. However, Illinois children appear to have frequent interactions with providers as measured by measles immunization rates, which are above 97 percent.[27] The first dose of the measles-mumps-rubella vaccine needs to be administered at age one, the same age Medicaid recomments a first lead screening. Although immunization shots are available also at mobile clinics and local health departments, the disparity in immunization and screening rates suggests that providers and/or families exercise more discretion for screening decisions than they do for immunization decisions. Indeed, an extensive literature documents large disparities in providers's practice styles (Mullainathan & Obermeyer 2019, Kwok 2019, Fadlon & Van Parys 2019, Silver 2019, Currie et al. 2016, Van Parys 2016, Fletcher et al. 2014, Epstein & Nicholson 2009).

[26]Remediations and repeated EBLLs in the same home are rare, although my sample includes over 2,000 of these events. Appendix Table A.15 shows that the null effects are robust to limiting the sample to children with a higher incidence of these events, as well as to different techniques that correct for small sample bias.

[27]Source: Illinois School Board of Education. https://www.isbe.net/Documents/Immunization_17-18.xlsx accessed on 2019/08/17.

Here, I ask whether access to high-quality providers affects screening take-up. Appendix Table A.7 shows that highly-educated households sort into high-quality providers, which may confound the estimates of the effect of provider quality. Parents may more easily observe a providers' alma mater and select on that, than providers' screening-based quality. Thus, I test for sorting by investigating whether proximity to high-quality providers as defined by screening-based measures has additional explanatory power over proximity to providers who attended top 20 medical schools. Screening-based quality measures include whether providers offer less-invasive capillary tests, adherence to screening guidelines, and screening rates. I regress a child's screening indicator on indicators for providers' presence within concentric areas of a child's birth address as well as indicators for the presence of high-quality providers:

$$Y_{igy} = \sum_k \beta_k \text{AnyProviderInK}_i + \sum_k \gamma_k \text{HighScreeningQualityInK}_i + \sum_k \delta_k \text{Top20MedSchoolInK}_i + \eta_g + \xi_y + \varepsilon_i, \tag{9}$$

where $k \in < 1km, 2-5km, 5-10km, 10-20km$.

Figure 7 shows that children closer to providers have higher screening rates, and the more so if they are closer to high-quality providers. Convenient access to providers appears to get families "in the door"; once families travel to a provider, high-quality providers disproportionally increase screening rates, as measured by all quality variables. 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 selecting to visit providers with better education. Thus, provider training may increase screening.

5 Benefits of Counterfactual Prevention Policies

The previous section finds that travel costs decrease screening take-up and timely poisoning detection and do not improve targeting. Could policies that increase screening improve outcomes for poisoned children and society at large? This section exploits variation in travel costs to estimate households' willingness-to-pay (WTP) for screening and simulates the impact of five counterfactual policies aimed at increasing screening and/or remediations.

5.1 Exposure Risk and Willingness-to-Pay for Screening

This section estimates the WTP for screening of households with different observable characteristics. Figure 8 illustrates that children living in homes built prior to 1978 are five percentage points (11 percent) more likely to be screened than children living in newer and less risky homes, after controlling for block fixed effects (see Appendix Table A.10). Are households in older homes also less sensitive to travel costs? To answer this question, Table 4 presents results from both the linear probability model in equation (8) and an equivalent logit model. Column 1 reports estimates for the whole sample, while other columns report estimates for subsamples, obtained by interacting a household's distance to the closest provider with indicators for household characteristics.

To derive the WTP for screening, I follow Einav et al. (2016) and I define the 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 household i's opportunity cost of travel time, p is the nominal price of a screening test, and α_i and β_i are preference parameters. 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, household i's WTP for screening is $\frac{\alpha_i}{\beta_i} - \theta_i d_i - p$. As discussed in Section 4.1.1, to avoid the incidental parameters problem (Neyman & Scott 1948) while still being able to recover α_i, I include block-level means of relevant regressors but omit block fixed effects.

Table 4 shows that the average household has a negative WTP for screening and that households in riskier homes have the highest WTP. The average household in a home built prior to 1930 is willing to pay \$6.14 for screening. 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 4 do not show large differences in the elasticity to travel costs, β_i, these different WTPs suggest households have different valuations of screening benefits, α_i.

If all households face the same price for a test, the estimates in Table 4 imply that households in pre-1930 homes are willing to pay up to \$29.16 more than households in newer homes. If, instead, households living in pre-1930 homes have no co-pay while low-risk households pay full price (\$43), the difference in WTP between high- and low-risk households becomes negative. Conversely, the difference widens to \$72.16 if riskier households pay full price due to lack of insurance. Still, my definition of travel costs likely overestimates WTP. First, high-risk households are less likely to drive meaning they need more time to travel a given distance.[28] Second, households often travel to providers that are farther away than their closest provider. To address the second concern, I can divide the WTP estimates by the average relationships between minimum and actual distance in the whole and pre-1930 homes sample, 3.6 kilometers or 6.6 kilometers respectively (Appendix Table A.6: Column 4), yielding a difference in WTP of \$8.10 or \$4.42, respectively.

To interpret the magnitude of these WTP estimates, I need a measure of screening benefits.[29] Section 2 discusses how under risk-neutrality and perfect information, perceived benefits are the converse of the expected costs of lead poisoning. By contrast, perceived benefits exceed expected poisoning costs under risk aversion and fall short of them if households underestimate treatment effectiveness or overestimate treatment costs. Households in pre-1930 homes have a 0.8 percentage point higher likelihood of an EBLL than households in new homes (Column 4, Appendix Table A.8), but estimates of the cost of an EBLL vary widely. On the one hand, Gazze et al. (2019) find that children with EBLLs have test scores that are 0.031 standard deviations lower than their siblings, implying a net present value of lifetime earnings lost to lead poisoning of \$5,616 and an expected lifetime cost of living in a pre-1930 home relative to a new home of \$45.[30] On the

[28] 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.

[29] Using data on chelation treatment for severe lead poisoning, (Agee & Crocker 1996) estimate that parents are willing to pay \$16.11 to reduce their child's lead levels by one percent.

[30] I use estimates by Chetty et al. (2014) that a one-standard-deviation-decrease in test scores is associated with a 12 percent decrease in earnings at 28 and 2018 Current Population Survey data to compute a lifetime earnings profile, assuming a growth rate of real labor productivity of 1.9 percent and a discount rate of 3.38 (that is, the 30-year Treasury bond rate).

other hand, 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), but this estimate does not account for unobserved innate ability correlated with lead exposure. Most parameter values for benefits and WTP indicate parents undervalue screening, although neither estimate includes the opportunity cost of the additional time parents spend caring for a poisoned child.

5.2 Policy Counterfactuals

This section simulates the societal benefits of different policies aimed at increasing screening and remediations in the 2014 cohort as modeled in equation (6). I consider four screening policies. 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.

Table 5 reports the number of additional children screened and additional poisoning cases detected under each policy. I compute additional detection rates under each policy 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 the prevention benefits from the screening policies are zero based on the lack of evidence that proximity to providers reduces future exposure (Section 4.3). Finally, I compare these policies to subsidizing full remediation for addresses with EBLLs. The majority of these policies' monetary costs involve transfers to health care or insurance providers. While I report these costs as benchmarks, 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, I use a value of

[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.

$5,617. This figure is based on estimates in Gazze et al. (2019) that a lead-poisoned child decreases all of their peers' test scores by 0.01 standard deviations per grade.[32] Because this value omits the crime costs of lead poisoning, it likely underestimates the total externality of lead poisoning.[33] All the screening policies I study appear to be cost-effective for externality values lower than $5,617.

First, I simulate the effect of giving households incentives for screening, following a large literature on immunization incentives (Banerjee et al. 2010, Bronchetti et al. 2015). I assign variable 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 5 shows that this policy may benefit the marginal households, although this term is not statistically significant and it is lower than the incentives disbursed as many inframarginal households receive subsidies.

Second, I consider a pay-for-performance incentive for low-performing providers. Although pay-for-performance programs among physicians have had mixed success (Li et al. 2014), physicians appear to respond to increased payments (Alexander & Schnell 2019). For providers in high-risk zip codes with screening rates lower than 50 percent, I assume the policy leads them to screen an additional random 10 percent of children in their catchment area. Column 2 of Table 5 shows that this policy would lead to screening around four times more children than the household incentive, but achieve a similar, and similarly statistically insignificant, private benefit, due to poorer targeting. Dividing the policy's private benefits among the 216 providers affected yields an incentive of $1,230, or $5.24 per additional child.

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, the single largest point of access to health-related services for low-income

[32] I use estimates by Chetty et al. (2014) that a one-standard-deviation decrease in test scores is associated with a 12 percent decrease in earnings at 28 and 2018 Current Population Survey data to compute a lifetime earnings profile, assuming a growth rate of real labor productivity of 1.9 percent and a discount rate of 3.38 (that is, the 30-year Treasury bond rate).

[33] As a reference, Heckman et al. (2010) estimate that 38–66 percent of the value of preschool programs is attributable to crime reductions.

preschool children in the US (General Accounting Office 1999). Alternatively, pharmacies could acquire lead screening kits at a cost of $382 for 48 tests. Column 3 of Table 5 shows that this policy would only screen 882 more children, consistent with households viewing even small distances as hassles. Although insignificant, the benefits for these marginal children could be higher than the program's cost because capillary screening kits are cheap.

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 5 shows that, compared to the screening incentive in Column 1, this policy yields fewer additional screenings and lower private 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 social benefits of five screening and remediation policies. Overall, I find that policies increasing screening rates have modest and statistically insignificant 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 pay-for-performance (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.

6 Conclusion

Lead paint in millions of US homes potentially endangers children' health. Lead poisoning prevention programs rely on childhood blood lead screening to identify these hazards, but screening may create hassles for families with small children. This paper examines screening take-up in Illinois and evaluates counterfactual prevention policies. I find that travel costs decrease screening rates but do not affect selection into screening based on either observable or unobservable exposure risk. The relatively low incidence of lead poisoning implies that households have a low average willingness-to-pay for screening. Thus, policies incentivizing screening have low private benefits, yet may be cost-effective when accounting for total societal benefits from averted poisoning externalities.

My findings suggest that decreasing travel costs, for example through subsidies, could increase screening without reducing targeting efficiency. This paper leaves a few open questions for further research. First, because provider quality affects screening, provider training may cost-effectively increase screening. Second, increased provider access appears to improve timely detection of lead poisoning but is not associated with higher remediation activity, casting doubt on the effectiveness of case management. Third, my analysis compares my estimates of the willingness-to-pay for screening to back-of-the-envelope estimates of screening benefits. I am collecting education and behavioral outcome data from the Chicago Public Schools to directly estimate the benefits of screening and of early poisoning detection.

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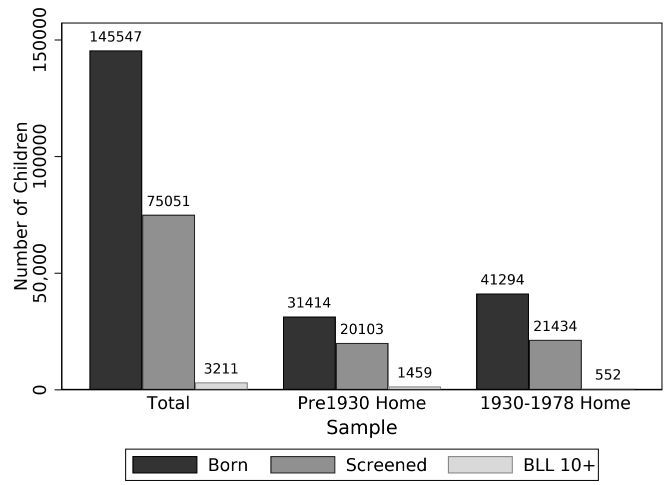
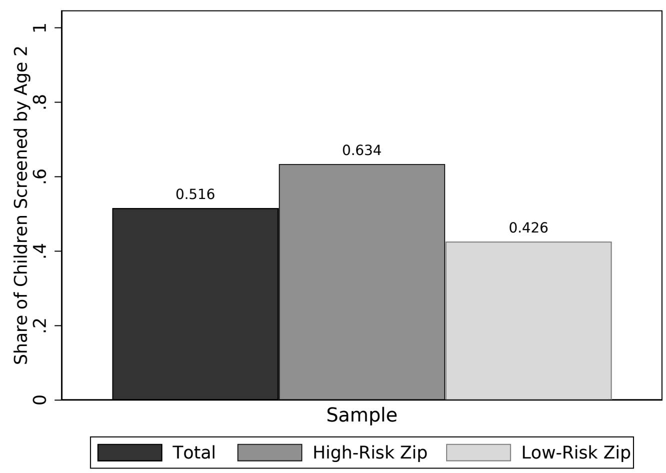
Figure 1: Number of Children Born, Screened, and with BLLs 10+, 2014 Cohort



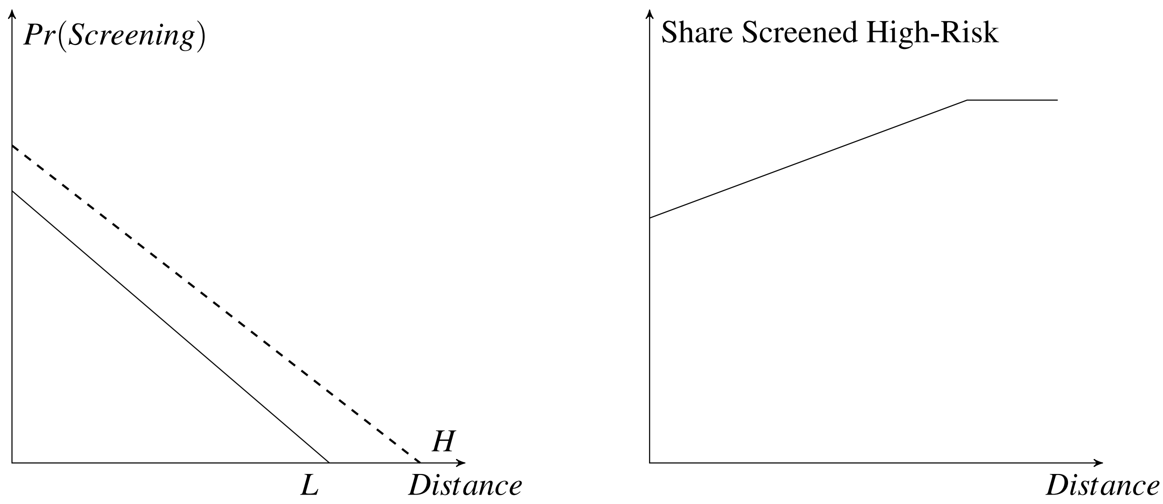
Notes: The figure plots the number of children born, screened, and with blood lead levels 10+ in the 2014 cohort in the whole sample and for the sample of children in pre-1930 and 1930-1978 homes.

Figure 2: Screening Rates by Zip Code Risk, 2014 Cohort



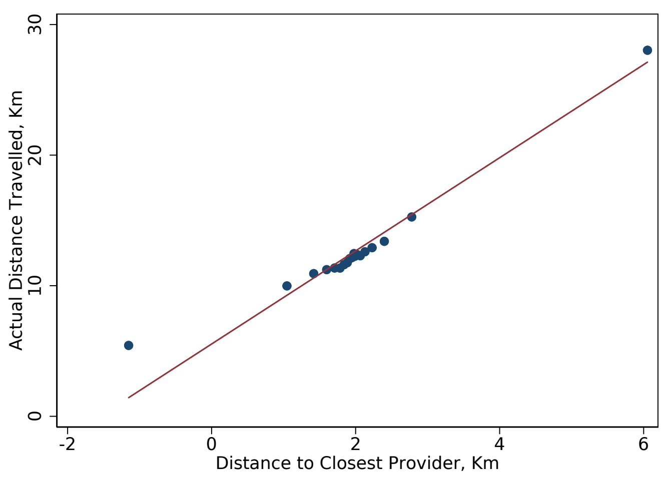
Notes: The figure plots screening rates by age two in the 2014 Illinois birth cohort by risk-level in the birth zip code.

Figure 3: 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.

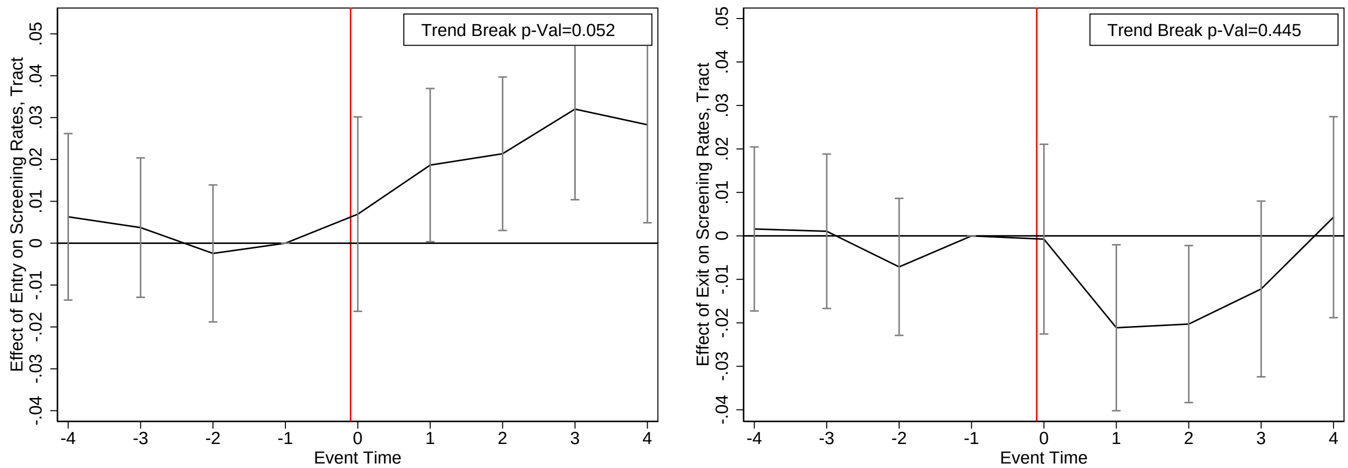
Figure 4: Distance to Closest Provider Predicts Distance Travelled



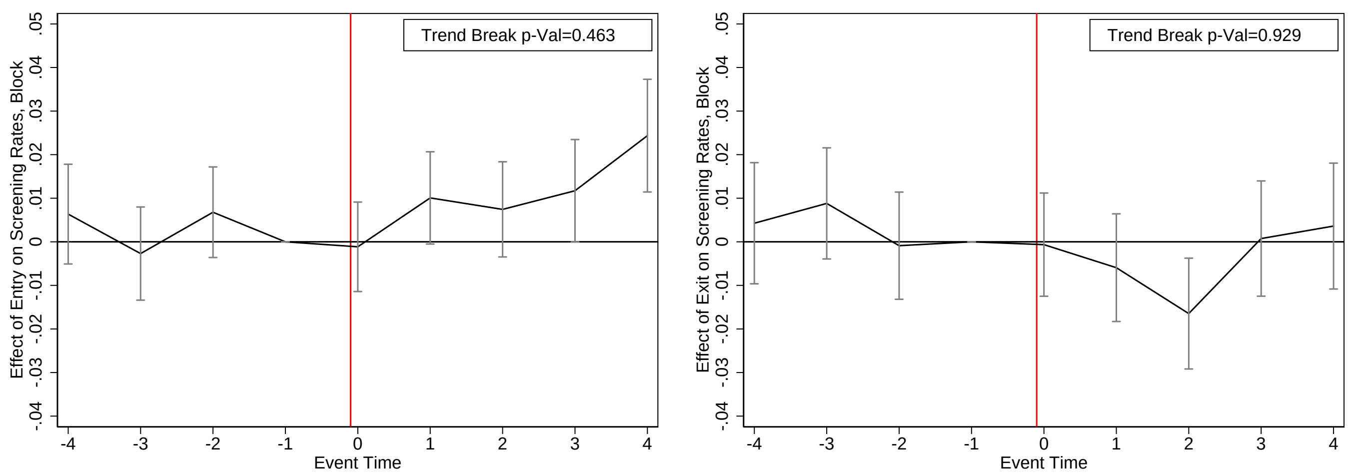
Notes: The figure plots average distance travelled to see a provider, in kilometers, for each vintile of distance between address at test and closest provider, in kilometers (blue dots) as well as the fitted line after partialling out block and year fixed effects.

Figure 5: Year-by-Year Effects of Openings and Closings

(a) Tract Level

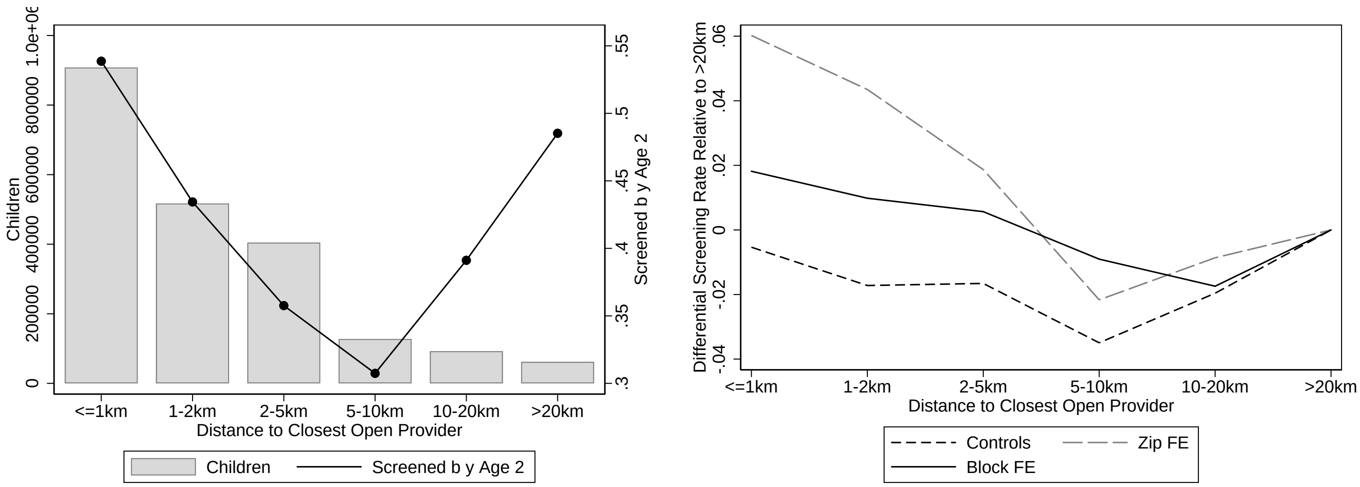


(b) Block Level



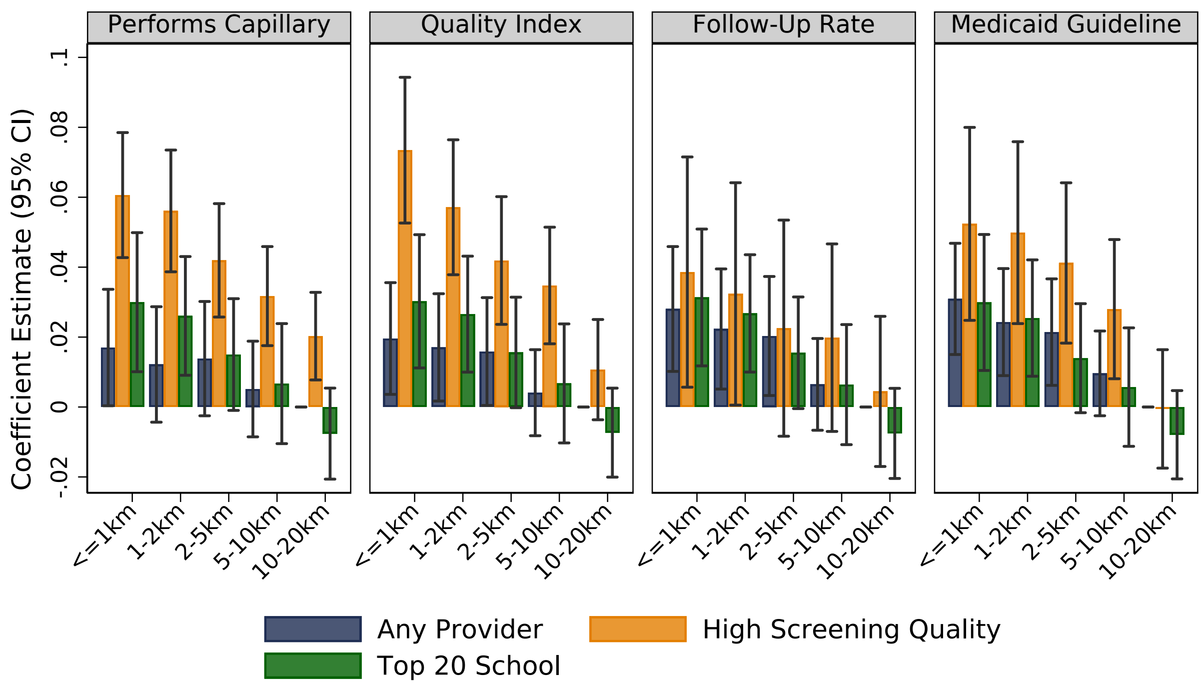
Notes: The figure plots DD coefficients on year-by-year entry and exit dummies, at the tract (Panel A) and block (Panel B) level. The outcome variable is the screening rate. Coefficients on entry and exit in each panel are estimated in a single regression. The vertical line indicates the entry or exit period. For neighborhoods with entries or exits the sample is limited to a balanced panel in the [-4,4] window around the entry or exit. Neighborhood and year fixed effects are included. T-1 is the omitted category. The vertical bars are 95 percent confidence intervals. Standard errors are clustered at the neighborhood level.

Figure 6: Determinants of Screening: Distance to Providers



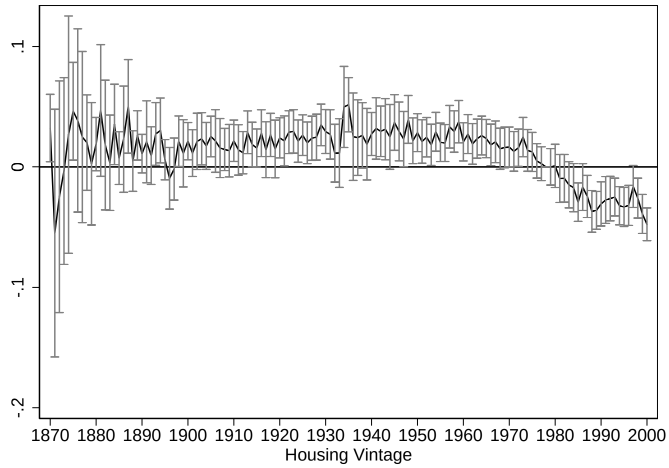
Notes: The figure plots the average likelihood of a child being screened by age two by distance to closest open provider. The bars in the left panel show the number of children in each distance bin on the left y-axis, and the line represents their screening rates on the right y-axis. The right panel plots screening rates for each distance bin relative to children born 20 kilometers or further from open providers controlling for children and home characteristics (short-dash line), zip fixed effects (grey long-dash line), and block fixed effects (black line).

Figure 7: Determinants of Screening: Provider Quality



Notes: The figure plots 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 (green 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 birth year and block fixed effects. Vertical bars display 95% confidence intervals based on standard errors clustered at the zip code level.

Figure 8: Determinants of Screening: Latent Exposure



Notes: The figure plots coefficients of a linear probability model on the likelihood of a child being screened by age two. The figure shows the vintage-by-vintage impact on screening of living in a home built in a particular year relative to homes built in 1978. The regression includes block and birth year fixed effects, as well as demographic controls. Vertical bars display 95% confidence intervals based on standard errors clustered at the zip code level.

Table 1: Determinants of Screening: Provider Distance

Dependent Variable:	Screened by Age 2				
	(1)	(2)	(3)	(4)	(5)
Panel A: Continuous Distance					
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)
Panel B: Binned Distance					
Closest Open Provider within 1Km	0.072***	0.040***	0.040***	0.041***	0.032***
	(0.008)	(0.006)	(0.007)	(0.009)	(0.012)
Closest Open Provider 1-2Km	0.055***	0.026***	0.028***	0.033***	0.023*
	(0.007)	(0.006)	(0.007)	(0.009)	(0.012)
Closest Open Provider 2-5Km	0.030***	0.015**	0.017**	0.028***	0.012
	(0.007)	(0.006)	(0.007)	(0.009)	(0.012)
Closest Open Provider 5-10Km	-0.011*	-0.006	0.001	0.013*	0.006
	(0.006)	(0.005)	(0.005)	(0.007)	(0.010)
Mean Outcome Variable	0.46	0.46	0.46	0.46	0.47
N	2050536	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 distance to the closest provider open during a child birth year on the likelihood of a child being screened by age two. Panel A reports the effect of a continuous distance measure in kilometers, while Panel B reports the effect of binned distance indicators. 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 for the location indicated at the bottom of each column. Standard errors clustered at the zip code level in parentheses.

Table 2: Selection into Screening Conditional on Distance

Dependent Variable:	BLL 10+ By Age 2 (1)	BLL By Age 2 (2)	Home Pre1930 (3)	Black (4)	Hispanic (5)	Single Mother (6)	Mother 20 or Younger (7)	Mother High School or Less (8)
Panel A: Tract and Year FE								
Distance to Closest	-0.0003**	-0.0052*	-0.0047***	-0.0023***	-0.0021***	-0.0019***	-0.0002	-0.0008
Open Provider	(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	-0.0004	-0.0084	-0.0012**	-0.0004	-0.0002	0.0006	0.0002	-0.0004
Open Provider	(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 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. 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.

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Table 3: 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	-0.0003***	0.1934***	0.1811***	0.0000	-0.0002
	(0.000)	(0.051)	(0.050)	(0.000)	(0.000)
Mean Outcome Variable	0.009	20.434	21.325	0.001	0.016
N	2018383	1194748	1194748	2018383	476357
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. 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.

Table 4: 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	-0.007***	-0.004**	-0.006***	-0.008***	-0.007***	-0.002	-0.007***	-0.001	-0.011***	0.004***	-0.008***	0.004**
Open Provider	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)
Panel B: OLS Coefficients												
Distance to Closest	-0.005***	-0.003*	-0.004***	-0.006***	-0.005***	-0.003	-0.005***	-0.003	-0.008***	0.004***	-0.006***	0.004***
Open Provider	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Panel C: Average Willingess to Pay												
Average WTP ($)	-6.787**	6.141***	-4.603***	-23.015	-6.588***	5.591	-7.620***	6.615*	-4.979***	0.687***	-3.592***	2.541***
	(3.172)	(1.686)	(0.704)	(18.080)	(2.485)	(3.586)	(2.640)	(3.962)	(0.243)	(0.113)	(0.242)	(0.374)
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.

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Table 5: 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	
	(447.60)	(861.27)	(9.97)	(249.49)	
Externality, $1,000	759.54***	2420.85***	42.10***	833.98***	
	(229.20)	(730.52)	(12.70)	(251.66)	
Prevention, $1,000					391.99***
					(118.29)
Total Benefits, $1,000	1129.63	2686.55	52.02	1028.81	391.99
Cost, $1,000	434.71	1774.47	7.02		6792.15

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 WTP yields the private benefits of each policy. WTP is estimated in a logit model that includes demographic and block-group level controls. The externality of each EBLL case is assumed to be $5,617. Household incentives average $10.5. Columns 1 and 3 count children whose willingness-to-pay (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. 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 EBLLs in 2014 prevent 66 new cases in the following ten years, at the baseline re-poisoning rate of 10.3 percent, for an externality benefit of $8,794 each. Average remediation cost are $10,646 per house.

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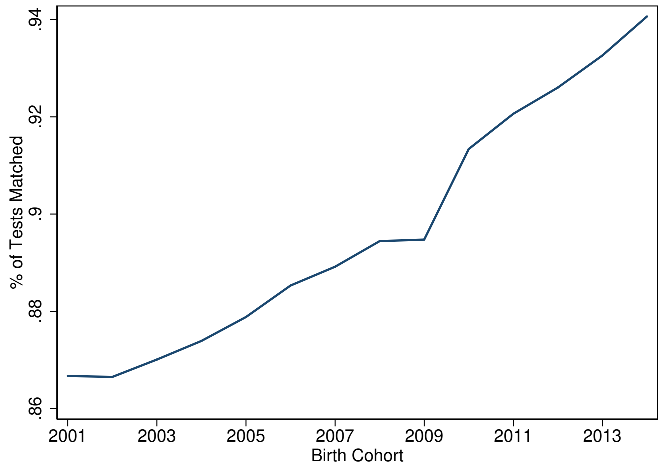
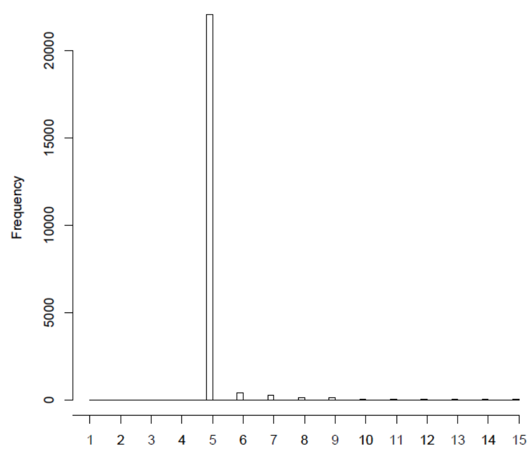
Figure A.1: Match Rate between Blood Lead Levels and Birth Records



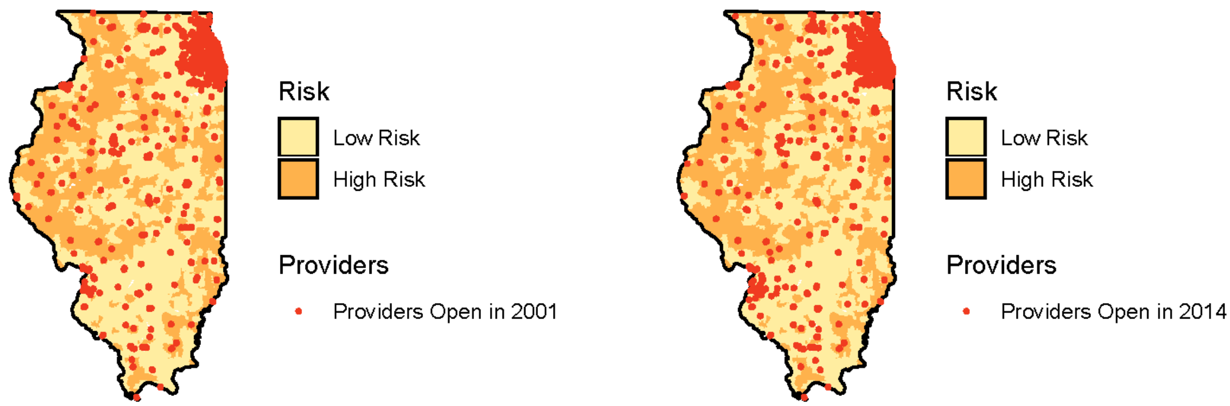
Notes: The figure plots the percent of tests successfully linked to birth records by birth cohort as recorded in the test data.

Figure A.2: Distribution of Test Results of Laboratory with Cutoff at 5 $\mu g/dL$



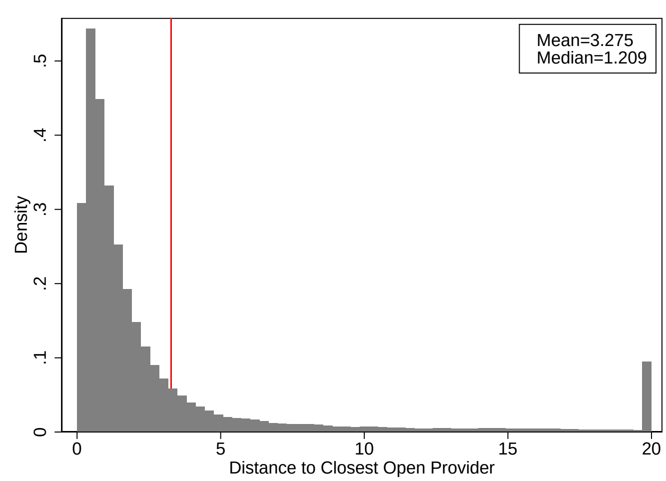
Notes: The figure plots the number of tests on the y-axis by BLL result on the x-axis for one laboratory in our sample.

Figure A.3: Location of Providers Operating in IL in 2001 and 2014



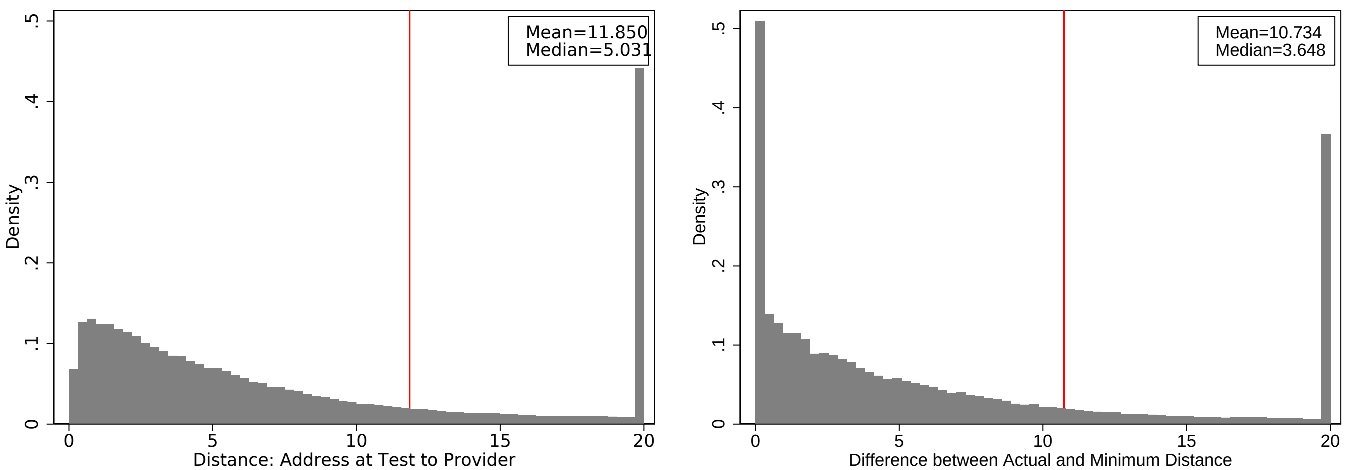
Notes: The figure plots the distribution of open providers in Illinois in high and low risk zip codes in the years 2001 (left) and 2014 (right).

Figure A.4: Distance to Providers



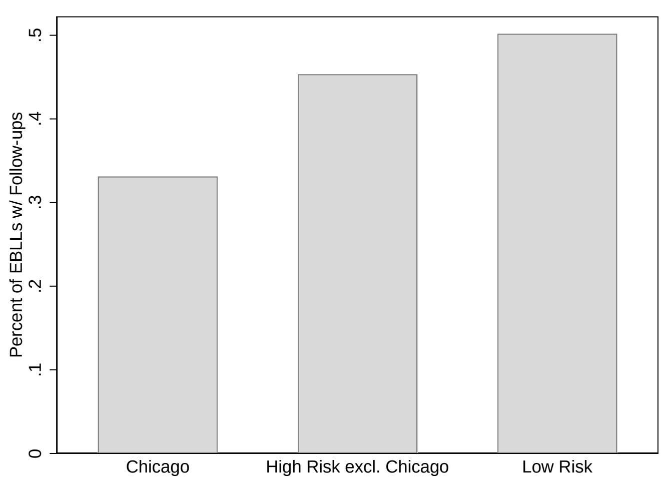
Notes: The figure plots the distribution of distance in kilometers from children's birth address to the closest provider open during the child's birth year. Distance is censored at 20km for ease of visualization. The red vertical line indicates the mean of the variable in the uncensored data.

Figure A.5: Distance to Providers



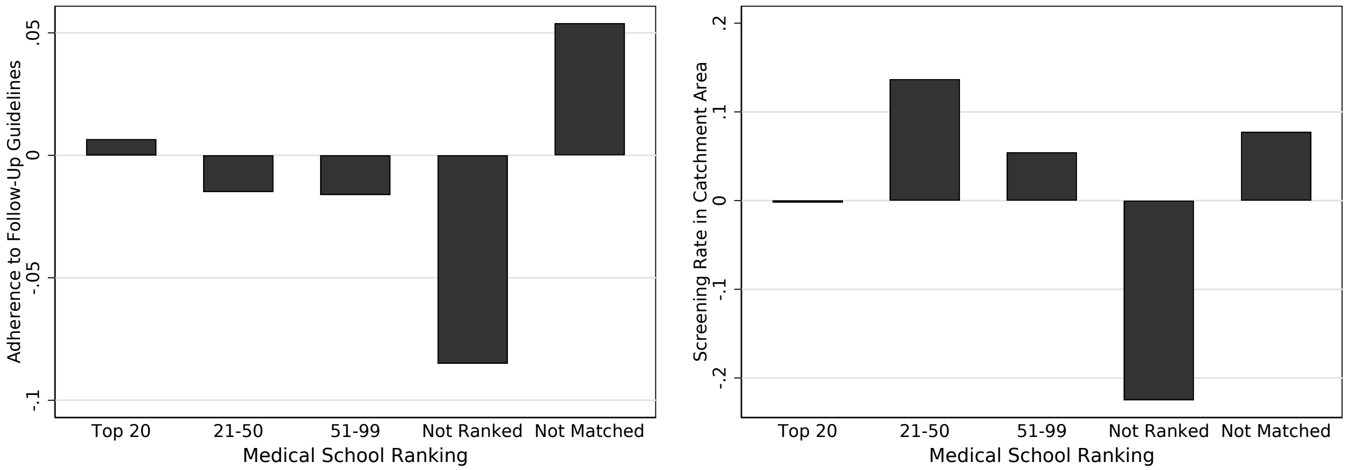
Notes: The left panel plots the distribution of distance in kilometers between children's address at test and the provider associated with the test. The right panel plots the distribution of the difference in kilometers between distance traveled at test and minimum distance between address at test and 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



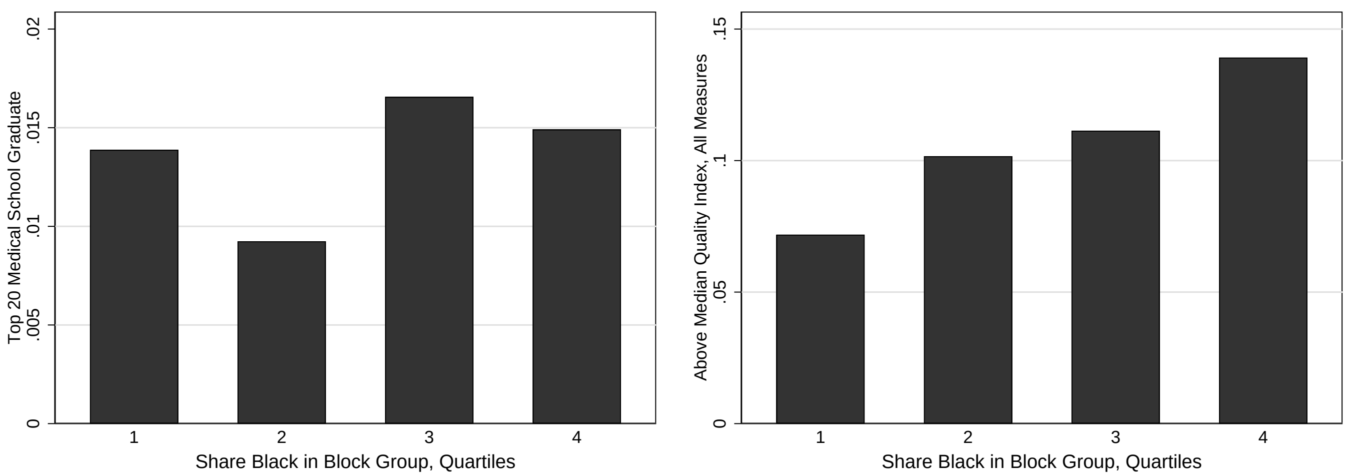
Notes: The figure plots follow-up rates in IL for tests that identify an EBLL by risk-level in birth zip code.

Figure A.7: Providers: Correlation in Quality Measures



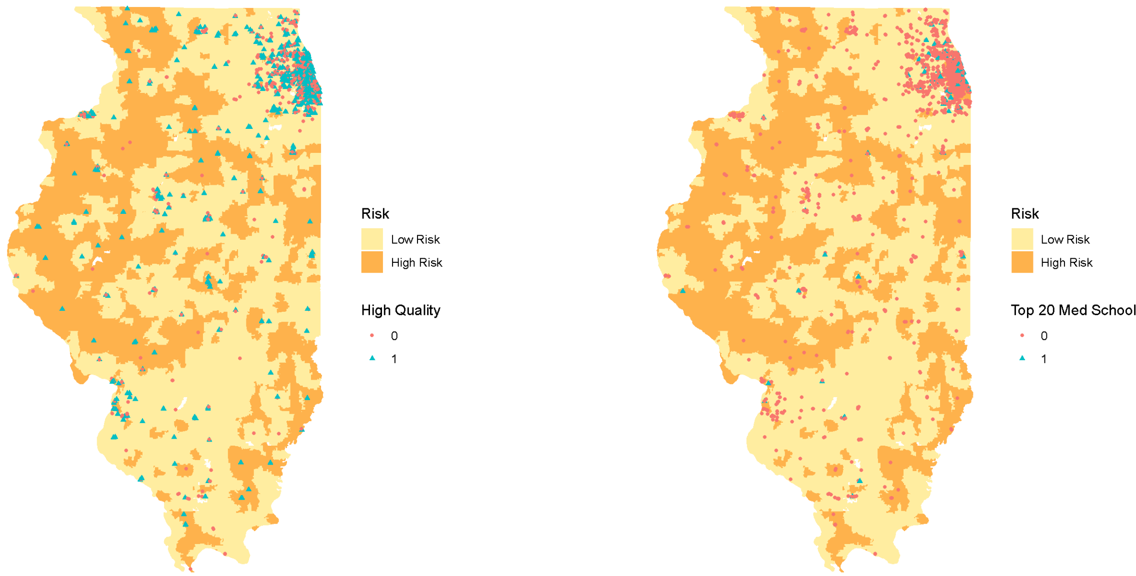
Notes: The figure plots on the y-axis the average z-scores of adherence to follow-up guideline (left panel) and screening rate (right panel) by ranking of the medical school each provider earned their degrees at on the x-axis.

Figure A.8: Providers: Correlation between Provider Quality and Neighborhood Characteristics



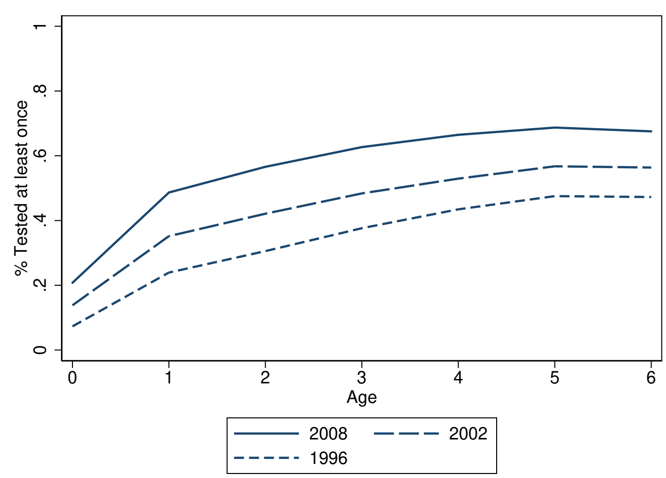
Notes: The figure plots on the y-axis the share of providers who are from top 20 medical schools (left panel) and who have a quality index above median (right panel) by share of black children born in the provider's census block group on the x-axis.

Figure A.9: Location of Providers, by Quality



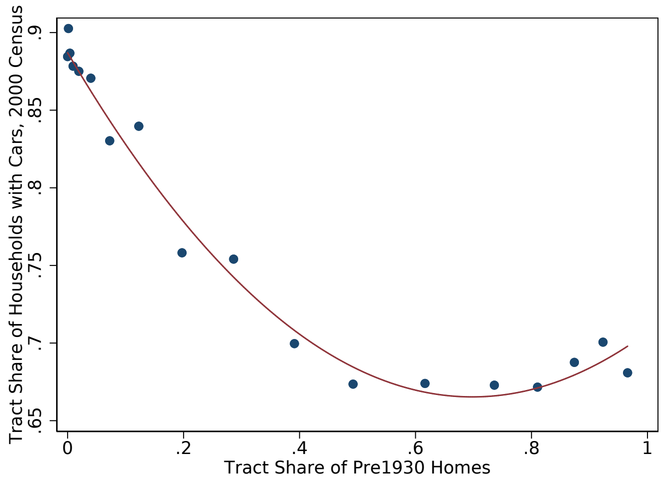
Notes: The figure plots the distribution of open providers by quality (left panel) and ranking of medical school of record (right panel) in Illinois in high and low risk zip codes over the years 2001-2014. High-quality providers are defined as having a quality index above median.

Figure A.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		Screened Children	
	Mean	Standard Deviation	Mean	Standard Deviation
	(1)	(2)	(3)	(4)
Home Pre1930	0.348	0.476	0.451	0.498
Home 1930-1977	0.399	0.490	0.391	0.488
Low Income	0.278	0.448	0.366	0.482
Black	0.179	0.383	0.226	0.418
Hispanic	0.246	0.431	0.319	0.466
Single Mother	0.384	0.486	0.490	0.500
Mother 20 or Younger	0.091	0.287	0.119	0.324
Mother Less than High School	0.012	0.109	0.019	0.137
Mother High School, No Diploma	0.103	0.304	0.135	0.342
EBLL within a Year of Birth within 15m	0.054	0.226	0.079	0.269
EBLL within a Year of Birth 15-100m	0.126	0.332	0.172	0.378
Chicago Born	0.283	0.450	0.380	0.485
High Risk Zip excl. Chicago	0.169	0.375	0.204	0.403
Screened by Age 2	0.456	0.498	1.000	0.000
Highest BLL by Age 2	2.919	2.596	2.919	2.596
BLL 10+ by Age 2	0.020	0.140	0.020	0.140
Distance to Closest Open Provider	2.279	3.195	1.934	3.004
Has Provider w/ Capillary in 1Km	0.308	0.462	0.382	0.486
Has High Quality Provider in 1Km	0.295	0.456	0.374	0.484
Has Provider w/ Top20 Degree in 1Km	0.033	0.178	0.039	0.193
N	2050536		934099	

Notes: The table displays summary statistics for the covariates in the sample. Columns 1-2 include all geocoded children whose birth address matched a parcel record for birth cohorts 2001-2014, while Columns 3-4 limit the sample to children whose birth address is within 2 kilometers of a provider opening or closing during their birth year.

Table A.2: 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.3: 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 BLLs).

Table A.4: 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.5: Summary Statistics: Providers

	Mean (1)	Standard Deviation (2)
Years Open	8.172	6.051
Individual Provider	0.242	0.428
Top20 Degree	0.029	0.168
Top 21-50 Degree	0.175	0.380
Unranked Degree	0.685	0.465
Performs Capillary	0.636	0.481
High Quality	0.703	0.457
N	4542	

Notes: The table displays summary statistics for the providers in the sample.

Table A.6: Distance to Closest Provider Predicts Distance Travelled

Dependent Variable:	Actual Distance Travelled			
	(1)	(2)	(3)	(4)
Panel A: Whole Sample				
Distance to Closest	2.572***	2.582***	2.733***	3.566***
Open Provider	(0.188)	(0.198)	(0.216)	(0.324)
Mean Outcome Variable	12.72	12.60	12.60	12.38
N	1046307	985141	985116	947501
Panel B: Households in Pre1930 Homes				
Distance to Closest	4.858***	4.862***	5.152***	6.597***
Open Provider	(0.460)	(0.466)	(0.494)	(0.562)
Mean Outcome Variable	9.95	9.95	9.95	9.83
N	367850	367787	367493	358726
Zip Code FE	X			
Tract FE		X		
Block Group FE			X	
Block FE				X

Notes: The table displays the impact of distance to the closest provider open during the year of a test on the actual distance travelled to get the test. Panel A includes all geocoded children born 2001-2014 whose closest provider is within 20 kilometers. Panel B further limits the sample to children in homes built prior to 1930. 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.7: Provider Choice: Mother's Education and Provider Quality

Dependent Variable:	Distance Traveled over Minimum (1)	Performs Capillary (2)	Follow Up Rate (3)	Medicaid Guidelines (4)	Screening Rate (5)	Top 20 Med School (6)
High School,	0.341***	0.003	-0.014**	-0.048***	-0.024	0.006**
No Diploma	(0.059)	(0.004)	(0.007)	(0.011)	(0.018)	(0.002)
High School	0.599***	0.008*	0.002	-0.075***	-0.030	0.005**
Diploma	(0.075)	(0.004)	(0.007)	(0.013)	(0.020)	(0.002)
Some College	0.961***	0.006	0.020**	-0.132***	-0.086***	0.006**
	(0.078)	(0.004)	(0.008)	(0.016)	(0.022)	(0.002)
College Degree	1.228***	0.019***	0.069***	-0.246***	-0.055	0.009**
(4 Years)	(0.106)	(0.005)	(0.010)	(0.021)	(0.039)	(0.004)
More than College	1.265***	0.021***	0.083***	-0.305***	-0.024	0.013***
	(0.116)	(0.005)	(0.011)	(0.023)	(0.044)	(0.005)
Unknown	0.740***	0.021***	0.015	-0.093***	-0.007	-0.003
	(0.091)	(0.006)	(0.011)	(0.021)	(0.033)	(0.003)
Mean Outcome	4.49	0.89	0.17	0.19	1.73	0.03
N	743207	996858	971138	813208	739903	996858
Block FE	X	X	X	X	X	X

Notes: ${}^{*}p < 0.10, {}^{**}p < 0.05, {}^{***}p < 0.01$. The table displays the correlation between a monther's education and the distance the household travels to visit a provider for lead screening (Column 1) and the quality of the provider visited (Columns 2-6). Outcomes in Columns 3-5 are z-scores. Each column includes birth year and census block fixed effects. Standard errors clustered at the zip code level in parentheses.

Table A.8: Determinants of Lead Exposure

Dependent Variable:	Highest BLL by Age 2		BLL 10+ by Age 2	
	(1)	(2)	(3)	(4)
Home Pre1930	0.422***	0.316***	0.010***	0.008***
	(0.026)	(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	0.009		-0.002***	
	(0.014)		(0.001)	
Black	0.259***	0.181***	0.004*	0.002**
	(0.050)	(0.026)	(0.002)	(0.001)
Hispanic	-0.157***	-0.114***	-0.007***	-0.004***
	(0.023)	(0.017)	(0.001)	(0.001)
Single Mother	0.026**	0.026**	0.001*	0.001***
	(0.010)	(0.011)	(0.000)	(0.001)
Mother 20 or Younger	0.037***	0.021	0.000	-0.001*
	(0.014)	(0.015)	(0.001)	(0.001)
Mother Less than High	0.040	0.053*	0.003***	0.005***
School	(0.028)	(0.031)	(0.001)	(0.001)
Mother High School,	0.155***	0.149***	0.005***	0.005***
No Diploma	(0.017)	(0.017)	(0.001)	(0.001)
EBLL within a Year of Birth	2.186***	2.018***	0.165***	0.155***
within 15m	(0.140)	(0.143)	(0.010)	(0.011)
EBLL within a Year of Birth	0.119***	0.035**	0.000	-0.002**
15-100m	(0.017)	(0.018)	(0.001)	(0.001)
Mean Outcome Variable	2.97	2.99	0.02	0.02
N	671194	645218	671194	645218
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 (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.

Table A.9: 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 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.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)
EBLL within a Year of Birth within 15m							0.061***
							(0.005)
EBLL within a Year of Birth 15-100m							0.010***
							(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			X				
Block Group 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, indicated at 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. 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: Provider Access, by Zip Code Risk and Birth Order

Dependent Variable:	Screened by Age 2				
Sample:	Chicago	High-Risk w/out Chicago	Low-Risk	First Born	Non-First Born
	(1)	(2)	(3)	(4)	(5)
Panel A: Tract and Year FE					
Distance to Closest	-0.011**	-0.002*	-0.003***	-0.003***	-0.004***
Open Provider	(0.005)	(0.001)	(0.001)	(0.001)	(0.001)
Panel B: Block and Year FE					
Distance to Closest	-0.008	-0.001	-0.003***	-0.004***	-0.003***
Open Provider	(0.006)	(0.002)	(0.001)	(0.001)	(0.001)
Mean Outcome Variable	0.61	0.55	0.34	0.45	0.46
N	576731	330241	1100179	1414724	549499

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 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 block fixed effects. Standard errors clustered at the zip code level in parentheses.

Table A.12: Determinants of Screening: Provider Availability

Access Variable:	Accepts New Patients		Accepts Medicaid Patients		Accepts New & Medicaid Patients	
Sample:	All	Low Income	All	Low Income	All	Low Income
	(1)	(2)	(3)	(4)	(5)	(6)
Closest Open Provider	0.018**	0.019	0.025***	0.020	0.031***	0.031
within 1Km	(0.008)	(0.021)	(0.009)	(0.021)	(0.008)	(0.021)
Closest Open Provider	0.014*	0.019	0.021**	0.018	0.026***	0.028
1-2Km	(0.008)	(0.021)	(0.008)	(0.021)	(0.008)	(0.021)
Closest Open Provider	0.015*	0.017	0.020**	0.017	0.024***	0.025
2-5Km	(0.008)	(0.023)	(0.008)	(0.023)	(0.008)	(0.022)
Closest Open Provider	0.007	0.023	0.010	0.020	0.012*	0.021
5-10Km	(0.007)	(0.021)	(0.007)	(0.021)	(0.007)	(0.020)
Closest Open Provider	0.054***	0.051***	0.060***	0.053***	0.051***	0.040***
within 1Km, High Quality	(0.009)	(0.015)	(0.008)	(0.012)	(0.007)	(0.012)
Closest Open Provider	0.046***	0.043***	0.052***	0.046***	0.044***	0.035***
1-2Km, High Quality	(0.008)	(0.015)	(0.007)	(0.011)	(0.006)	(0.011)
Closest Open Provider	0.035***	0.033**	0.041***	0.038***	0.031***	0.025**
2-5Km, High Quality	(0.008)	(0.014)	(0.007)	(0.011)	(0.006)	(0.011)
Closest Open Provider	0.019***	0.005	0.023***	0.011	0.018***	0.005
5-10Km, High Quality	(0.007)	(0.015)	(0.005)	(0.012)	(0.005)	(0.012)
Closest Open Provider	0.009	0.015	0.014**	0.001	0.011**	-0.002
10-20Km, High Quality	(0.006)	(0.015)	(0.005)	(0.013)	(0.005)	(0.012)
Mean Outcome Variable	0.46	0.60	0.46	0.60	0.46	0.60
N	2018383	563938	2018383	563938	2018383	563938
Block FE	X	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 operating during a child birth year and distance to a provider possessing the characteristic indicated in each column 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 (odd columns) or among those, only children living in low-income block groups (even columns). Each column includes birth year and block fixed effects. Standard errors clustered at the zip code level in parentheses.

Table A.13: 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	-0.003***	-0.027***	-0.005***	-0.027***	-0.005***	-0.023***
Open Provider	(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***	0.476***	0.127***	0.589***
	(0.005)	(0.022)	(0.005)	(0.023)	(0.005)	(0.024)
Single Mother	0.029***	0.130***	0.042***	0.183***	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	0.002	-0.009	0.006*	0.024*	0.012***	0.091***
or Less	(0.003)	(0.014)	(0.003)	(0.014)	(0.003)	(0.017)
EBLL within a Year of Birth	0.045***	0.220***	0.061***	0.280***	0.037***	0.239***
within 15m	(0.004)	(0.019)	(0.004)	(0.020)	(0.003)	(0.020)
EBLL within a Year of Birth	0.004	0.050***	0.009***	0.042***	0.010***	0.044***
15-100m	(0.003)	(0.013)	(0.003)	(0.013)	(0.002)	(0.013)
Marginal Effect of Distance		-0.006***		-0.007***		-0.005***
to Closest Open Provider		(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. 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.14: Selection into Screening Conditional on Distance: Robustness Checks

Dependent Variable:	BLL 10+ By Age 2 (1)	BLL By Age 2 (2)	Home Pre1930 (3)	Black (4)	Hispanic (5)	Single Mother (6)	Mother 20 or Younger (7)	Mother High School or Less (8)
Panel A: Tract and Year FE								
Distance to Closest Open Provider	-0.0003** (0.000)	-0.0044** (0.002)	-0.0043*** (0.001)	-0.0024*** (0.001)	-0.0026*** (0.001)	-0.0026*** (0.001)	0.0000 (0.000)	-0.0009** (0.000)
Panel B: Block and Year FE								
Distance to Closest Open Provider	-0.0001 (0.000)	-0.0003 (0.001)	0.0000 (0.000)	0.0001 (0.000)	0.0009** (0.000)	0.0010* (0.001)	0.0001 (0.000)	-0.0002 (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 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.15: 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	0.0000	0.0001	-0.0092	-0.0087
	(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	-0.0007**	-0.0038**	0.0089	0.0089
	(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). 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.