Ludovica Gazze
University of Chicago Energy and Environment Lab
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November 12, 2019
Mark L. Waller
Professor and Acting Department Head
Department of Agricultural Economics
2124 TAMU
College Station, Texas 77843-2124

Attached please find my application for the position of Assistant Professor in Agricultural Marketing and Quantitative Analysis at TAMU. I received my PhD in Economics from MIT in June 2016 and am a Postdoctoral Scholar at the Energy and Environment Lab (E&E Lab) at the University of Chicago.

I have enclosed a copy of my job market paper, "Hassles and Environmental Health Screenings: Evidence from Lead Tests in Illinois". Current lead poisoning prevention programs rely on children's visits to the doctor to identify homes with lead hazards, an ordeal for families. I combine evidence from multiple large administrative datasets with insights from a theoretical model to assess the impacts of ordeals on lead poisoning prevention policies. I exploit doctors' openings and closings and find that travel costs decrease screening and do not improve targeting. Thus, lowering travel costs could increase detection rates and lower social costs of lead exposure.

I have also enclosed a copy of a paper I co-authored with members of the E&E Lab, "Enforcement and Deterrence with Certain Detection: An Experiment in Water Conservation Policy". This paper uses an experiment to study the impact of a technology that allows perfect violation detection and automated enforcement of water conservation regulations. We find that automated enforcement improves compliance, decreasing the pressure on the water utility to develop costly new supply sources. Still, fines increased under automated enforcement causing a surge in complaints. These findings highlight that perfect enforcement might not be politically feasible.

I am excited to be part of a vibrant environment like the Department of Agricultural Economics at TAMU. I genuinely appreciate your consideration. I have arranged for letters of reference by Professors Michael Greenstone, Joshua Angrist, and Benjamin Olken. If you require any additional information, please do not hesitate to contact me. I look forward to hearing from you.

Sincerely yours,
Ludovica Gazze
Postdoctoral Scholar, University of Chicago Energy and Environment Lab
http://home.uchicago.edu/~lgazze/

MIT Economics LUDOVICA GAZZE

OFFICE CONTACT INFORMATION
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Professor Robert Townsend
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MIT PLACEMENT ADMINISTRATOR
Ms. Julia Martyn-Shah
jmshah@mit.edu
617-253-8787

DOCTORAL STUDIES

Massachusetts Institute of Technology (MIT)
PhD, Economics, Completed June 2016
DISSERTATION: "Essays on Housing, Poverty, and Public Health"

DISSERTATION COMMITTEE AND REFERENCES

Professor Michael Greenstone
UChicago Department of Economics
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Professor Benjamin Olken
MIT Department of Economics
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Professor James Poterba
MIT Department of Economics
77 Massachusetts Avenue, E52-444
Cambridge, MA 02142
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RELEVANT POSITIONS

Interim Executive Director 2017-2018
UChicago Energy & Environment Lab

Postdoctoral Scholar 2016-present
UChicago Energy & Environment Lab. Supervisor: Michael Greenstone

PRIOR EDUCATION

Bocconi University, Italy 2010
M.Sc. 110/100 *cum laude* in Economics and Social Sciences

Bocconi University, Italy 2008
B.A. 110/100 *cum laude* in Economics and Social Sciences

CITIZENSHIP Italian **GENDER:** Female

LANGUAGES English (fluent), Italian (native), German (fluent)

FIELDS

Primary Fields: Environmental Economics, Health Economics

Secondary Fields: Labor Economics, Urban Economics

TEACHING EXPERIENCE

Data Analysis for Social Scientists 2016
Research and Communication in Economics: Topics, Methods, and Implementation 2016

FELLOWSHIPS, HONORS, AND AWARDS

BFI Data Acquisition Grant: "Using Satellite Data to Track and Regulate Oil and Gas Methane Emissions in Colorado" *with Thomas Covert, Michael Greenstone, Olga Rostapshova* 2019

J-PAL: "Using Remote Sensing to Reduce Vehicle Emissions in California" *with Fiona Burlig, Michael Greenstone, Olga Rostapshova* 2019

LJAF: "Automated Enforcement of Outdoor Watering Restrictions" *with Oliver Browne, Michael Greenstone* 2018

HUD: "National Evaluation of the Housing and Neighborhood Impact of the HUD Lead-Based Paint Hazard Control Program" *with Steve Billings, Michael Greenstone, Kevin Schnepel* 2018

Joyce Foundation: "Willingness to pay for child health screening: Evidence from lead poisoning prevention in Illinois" 2016

George and Obie Shultz Fund grants 2015
Bonaldo Stringher Fellowship, Bank of Italy 2013
Giovanna Crivelli Fellowship, Unicredit Group 2011
Roberto Franceschi Prize for outstanding research thesis 2010
Merit Award Fellowship, Bocconi University 2008

PROFESSIONAL ACTIVITIES

Referee: *American Economic Journal: Applied Economics, American Economic Journal: Economic Policy, American Economic Review: Insights, Econometrica, Journal of Environmental Economics and Management, Journal of Human Resources*, *Journal of Public Economics, Journal of Policy Analysis and Management, Plos One.*

Talks: TWEEDS, Harvard School of Public Health, Bocconi University, University of Bologna, Marco Fanno Workshop, AERE, ASHEcon, NBER, Indiana University, AFE, Tufts University, RAND 2019

H2D2, ASHEcon, APPAM, Northeastern University, ASSA 2018

Federal Reserve Board of Governors, NBER, Upjohn Institute 2016

EIEF; XVI EU Conference, Fondazione Rodolfo De Benedetti 2013

SELECTED RESEARCH PAPERS

"Hassles and Environmental Health Screenings: Evidence from Lead Tests in Illinois" (Job Market Paper)
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.

"The Price and Allocation Effects of Targeted Mandates: Evidence from Lead Hazards" (R&R Journal of Urban Economics)
Several states require owners to mitigate lead hazards in old houses with children present. I estimate the mandates' effects on housing markets. My empirical strategy exploits differences by state, year, and housing vintage. The mandates decrease the prices of old houses by 7.1 percent, acting as a large tax on owners. Moreover, families with children become 14.6 percent less likely to live in old houses. Increases in rents for family-friendly houses suggest that the mandates have important distributional consequences. These findings are relevant for evaluating similar mandates such as healthy home standards.

"Enforcement and Deterrence with Certain Detection: An Experiment in Water Conservation Policy" *with Oliver Browne, Michael Greenstone, Olga Rostapshova*

New technologies are poised to transform regulatory enforcement by automating costly inspections and driving violation detection rates to 100%. We conduct a randomized field experiment to evaluate the adoption of smart meters for enforcing outdoor water-use regulations in a major US city facing water shortage. We randomize 88,905 households into 12 groups varying enforcement methods (automated or visual inspection) and fine levels. Automated enforcement decreases water use by 3% and violations by 17%. However, due to imperfect deterrence, fines increase by 13,800% and customer service calls increase by 545%, leading to backlash that might make maximum enforcement politically untenable.

"Estimating Health Damages from Lead Pipe Disturbances: Evidence from Chicago" *with Jennifer Heissel*
Water utilities in the United States lose substantial water due to leaks in old water infrastructure. Lead in old service lines that connect homes to water mains may contaminate drinking water. One potential aggravating factor is construction on mains, which shakes the service lines and may remove the protective coating formed by natural sediments. We exploit over 2,500 water main replacements in

Chicago and a unique combination of geocoded data sources to estimate the effects of pipe maintenance on drinking water quality and children's blood levels. By comparing tests in homes in the same neighborhood but at different distances from replaced mains before and after replacement, we find no evidence that water main replacement affects water quality or children's lead levels.

"Correlates of Childhood Lead Exposure at Different Intervention Thresholds: A Geospatial Analysis of Illinois Blood Lead Data 2001-2016" *with Ali Abbasi, Bridget Pals* (R&R American Journal of Public Health)
The threshold defining elevated blood lead levels (EBLLs) has decreased over time. What are the consequences for optimal lead screening policy? We link birth records from 2.37 Illinois children to 4.19 million lead testing records and data on housing age, industrial emissions, and roads. We use multinomial logistic regression to determine predictors of EBLL at different thresholds, controlling for zip code random effects. While pre-1930 housing is associated with over 2-fold increased risk of EBLL at all thresholds, housing built in 1951-1978 is only associated with increased risk of EBLL at the 5µg/dL threshold. These findings suggest screening guidelines may need updating with the new threshold.

SELECTED RESEARCH IN PROGRESS

"On Peer Effects and Pollution: Does Exposure to Lead Affect Everyone in the Classroom?" *with Claudia Persico, Sandra Spirovska*
Lead harms children's cognitive development and behavior. We know substantially less about how one child's lead exposure might affect that child's peers in the classroom. We examine this overlooked social cost of lead exposure: the externality of lead exposure on peers' achievement and behavior in school. We estimate the negative spillovers caused by children with elevated blood lead levels (BLLs) using a novel dataset that links children's BLLs to education data from public schools in North Carolina. By comparing siblings in the same school but with observably different peer cohorts, we also contribute to the peer effects literature by presenting a novel way of estimating the effects of disruptive and low-achieving peers.

"Using Remote Sensing to Reduce Vehicle Emissions in California" *with Fiona Burlig, Michael Greenstone, Olga Rostapshova*
Particulate matter (PM) air pollution presents a substantial threat to human health. The transportation sector, particularly the heavy duty trucking industry, is a major contributor to PM. Yet, enforcing vehicle emissions regulations has proven prohibitively costly. We use new remote sensing technology to detect high emitters at greatly reduced cost. We leverage these data in a randomized trial to determine the impact of remote monitoring on regulatory compliance. Partnering with CARB, we randomly assign high-emitting trucks in California to receive letters that (1) inform fleet owners their vehicle is likely in violation of emissions standards, and (2) specify a penalty for failing to comply.

Massachusetts Institute of Technology

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Massachusetts Institute of Technology

Academic Transcript

Registrar's Office

77 Massachusetts Avenue
Cambridge, Massachusetts
02139-4307

Ludovica Angela Gazze

MIT ID: 918 179 224

Admitted as a Regular Student for Fall Term 2011-2012
from UNIV COMMERCIALE LUIGI BOCCONI
MILAN, ITALY

Subject	Subject Name	Lvl	Cred	Grade
FALL TERM 2011-2012	COURSE: 14 D	GRADUATE STUDENT		
14.121	Microeconomic Theory I	H	6	A
14.122	Microeconomic Theory II	H	6	B
14.281	Contract Economics	H	12	A
14.451	Dynamic Optimization Methods	H	6	B
14.452	Economic Growth	H	6	A
14.770	Collective Choice Pol Economy	G	12	I/A
	* * *			
SPRING TERM 2011-2012	COURSE: 14 D	GRADUATE STUDENT		
14.126	Game Theory	H	12	A
14.382	Econometrics	H	12	A
14.453	Econmic Fluctuations	H	6	A
14.454	Economic Crises	H	6	A
14.773	Political Econ: Insts & Dev	H	12	A
	* * *			
FALL TERM 2012-2013	COURSE: 14 D	GRADUATE STUDENT		
14.147	Topics in Game Theory	H	12	A
14.385	Nonlinear Econometric Analysis	H	12	A
14.661	Labor Economics I	H	12	A
14.771	Dev Economics: Micro Issues	H	12	A
	* * *			
JANUARY TERM 2012-2013	COURSE: 14 D	GRADUATE STUDENT		
14.389	Econometrics Paper	H	3	A
	* * *			
SPRING TERM 2012-2013	COURSE: 14 D	GRADUATE STUDENT		
14.273	Adv Topics in Industrial Org	H	12	A
14.662	Labor Economics II	H	12	A
14.772	Dev Economics: Macro Issues	H	12	A
	* * *			
SUMMER TERM 2013	COURSE: 14 D	GRADUATE STUDENT		
14.THG	Thesis	H	30	J/SA
	* * *			
FALL TERM 2013-2014	COURSE: 14 D	GRADUATE STUDENT		
14.391	Workshop: Economic Research	H	12	P
14.THG	Thesis	H	36	J/SA
	* * *			
SPRING TERM 2013-2014	COURSE: 14 D	GRADUATE STUDENT		
14.392	Workshop: Economic Research	H	12	P
14.THG	Thesis	H	36	J/SA
	* * *			

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Subject	Subject Name	Lvl	Cred	Grade
FALL TERM 2014-2015	COURSE: 14 D	GRADUATE STUDENT		
14.391	Workshop: Economic Research	H	12	P
14.THG	Thesis	H	36	J/SA
	* * *			
SPRING TERM 2014-2015	COURSE: 14 D	GRADUATE STUDENT		
14.THG	Thesis	H	36	J/SA
	* * *			
FALL TERM 2015-2016	COURSE: 14 D	NON-RESIDENT GRAD		
14.THG	Thesis	G	36	J/SA
	* * *			
SPRING TERM 2015-2016	COURSE: 14 D	GRADUATE STUDENT		
14.392	Workshop: Economic Research	G	12	P
14.THG	Thesis	G	36	SA
	* * *			

29-MAY-2013 Doctoral General Examination completed

03-JUN-2016 Awarded the Degree of Doctor of Philosophy; thesis in the field of Economics: The Economic and Health Consequences of Lead Paint Abatement Regulations

Graduate Cumulative GPA: 4.9 (on a 5.0 scale)

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OFFICIAL TRANSCRIPT: ISSUED 27-MAR-2017
Order #: AVOW:13105914

Issued to

Ludovica Gazze

Unofficial without signature
Mary R. Callahan, Registrar

Mary R. Callahan

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Academic Terms, Student Classification, and Courses
MIT's academic calendar has fifteen-week Fall and Spring Terms including exams, a ten-week Summer Term, and a four-week January Term.

Classification: Undergraduate students (Freshman, Sophomore, Junior, Senior) and Graduate students are matriculated in MIT degree programs; Special students, Exchange students, and Cross-registered students are not. Non-resident graduate students are working on doctoral thesis away from MIT.

Course: The student's Course (degree program) begins with a department or program code as listed below, followed by an option within the department. Undergraduate program options can indicate specialty area. Option codes used in graduate programs starting in Fall 1994 include: M, P, or A, Master's; D, Doctoral; CT, Transportation; RE, Real Estate Development; W, Joint with Woods Hole Oceanographic Institution. Freshmen are not permitted to register in a department. Transfer students generally enter as Sophomores.

Subject, Level, and Credit
Subject: Consists of a department or program code (see list below) followed by a period and a number. *Level* (Lvl): Subjects included in undergraduate cumulative record: **U.** Subjects included in graduate cumulative record: subject approved for (higher) graduate degree credit: **H** (through Summer 2015); other subject accepted for graduate degree credit: **G**; subject in graduate program but not accepted for graduate degree credit: **N.** *Credit:* A credit unit represents one hour of class (lecture/recitation), laboratory/design/fieldwork, or preparation per week for fourteen weeks. Three MIT credit units = one Semester Hour.

Explanation of Grades since 1980

A	Exceptionally good performance, demonstrating a superior understanding of the subject matter, a foundation of extensive knowledge, and a skillful use of concepts and/or materials.
B	Good performance, demonstrating capacity to use the appropriate concepts, a good understanding of the subject matter, and an ability to handle the problems and materials encountered in the subject.
C	Adequate performance, demonstrating an adequate understanding of the subject matter, an ability to handle relatively simple problems, and adequate preparation for moving on to more advanced work in the field.
D	Minimally acceptable performance, demonstrating at least partial familiarity with the subject matter and some capacity to deal with relatively simple problems, but also demonstrating deficiencies serious enough to make it inadvisable to proceed further in the field without additional work.
F	Failed.
J,U	J Satisfactory progress that term. U Progress not satisfactory that term. Final grade in same subject in a later term also covers this term (e.g., J/B or U/A).
P	Prior to Fall 1990: reflects performance at any of the levels A, B, C, or D. Fall 1990 through Summer 1992: for first-year undergraduates reflects performance at any of the levels A, B, or C; for other than freshmen reflects performance at any of the levels A, B, C, or D. Fall 1992 and after: reflects performance at any of the levels A, B, or C, with students graded on a P/D/F basis.
I	Incomplete. When work completed, final grade follows I (e.g., I/B).
O	Absent from the final examination, did not turn in the final paper or project, and/or was absent during the last two weeks of the term. Equivalent to a grade of F.
OX	Absence satisfactorily explained and excused. When work is completed final grade replaces the OX.
SA	Satisfactorily completed doctoral thesis.
S	Credit awarded for work done elsewhere.
URN	Subject in Undergraduate Research Opportunities Program taken for pay or as a volunteer rather than academic credit (the one unit shown does not count for degree credit).
VIS	Research subject taken as a non-degree visiting student.
&	Grade ending in & indicates Advanced Standing Exam (not included in GPA).
#	Grade ending in # indicates ROTC (not included in degree credit; not included in GPA after Summer 1994).
MG	Indicates grade not submitted by instructor.
IP	Indicates subject "in progress" in current term.
PE	Reflects performance at any of the levels A, B, or C, under an emergency closure.
IE	Incomplete. Indicates a portion of the subject requirements has not been fulfilled, due to a major disruption of academic activities. When work completed, final grade follows (e.g., IE/B).

Freshman Grading
Prior to Fall 1990: Freshmen graded on P/F basis with F grade not recorded on transcript. Fall 1990 to Summer 2002: Freshmen graded on P/D/F basis with non passing D and F grades not recorded on transcript. Fall 2002 and after: Freshmen graded in their second semester on A/B/C/D/F basis with non-passing D and F grades not recorded on transcript.

Cumulative Grade Point Averages
Calculated on a 5.0 scale with A = 5, B = 4, C = 3, D = 2, F and O = 0. P, PE, SA, S, URN, MG, and IP, as well as non-passing grades in Freshman year, not included in GPA. J, U, I, IE, and OX grades not included in GPA until completed. Undergraduate Cumulative GPA includes subjects at Level U and Graduate Cumulative GPA includes subjects at Level H, G, and N, and up to a maximum of 24 units of thesis.

Department and Program Codes since 1980

1	Civil and Environmental Engineering (Civil Engineering prior to Fall 1992)
2	Mechanical Engineering
3	Materials Science and Engineering
4	Architecture
5	Chemistry
6	Electrical Engineering and Computer Science
7	Biology
8	Physics
9	Brain and Cognitive Sciences (Psychology prior to Fall 1986)
10	Chemical Engineering
11	Urban Studies and Planning
12	Earth, Atmospheric, and Planetary Sciences (Earth and Planetary Sciences prior to Fall 1984)
13	Ocean Engineering (through Spring 2007)
14	Economics
15	Management
16	Aeronautics and Astronautics
17	Political Science
18	Mathematics
19	Meteorology and Physical Oceanography (through Summer 1983) (Meteorology through Summer 1980)
20	Biological Engineering (Applied Biological Sciences through Summer 2003) (Nutrition and Food Science prior to Fall 1985)
21	Humanities
21A	Anthropology (Anthropology/Archaeology from Summer 1989 through Summer 1996)
21F	Foreign Languages and Literatures (through Summer 2015)
21G	Global Studies and Languages
21H	History
21L	Literature
21M	Music and Theater Arts
21W	Writing and Humanistic Studies (Writing from Summer 1989 through Summer 1991)
22	Nuclear Science and Engineering (Nuclear Engineering through Spring 2005)
24	Linguistics and Philosophy
25	Interdisciplinary Science (to Spring 1983)
BE	Biological Engineering (through Summer 2006) (**BEH** Bioengineering and Environmental Health from Fall 1998 through Summer 2002; **TOX** Toxicology from Spring 1989 through Summer 1998)
CDO	Computation for Design and Optimization
CMS	Comparative Media Studies
CSB	Computational and Systems Biology
EM	Engineering Management
ESD	Engineering Systems Division
HPM	Health Policy and Management (1983-1990)
HST	Harvard-MIT Division of Health Sciences and Technology
IDS	Institute for Data, Systems, and Society
MAS	Media Arts and Sciences
OR	Operations Research
PEP	Professional Education Programs (**ASP** Advanced Study Program through Summer 2006; **CAES** Center for Advanced Educational Services from Spring 1996 through Summer 2003; **EN** Center for Advanced Engineering Study prior to 1995)
RED	Real Estate Development
SCM	Supply Chain Management
SDM	System Design and Management (through Summer 2010)
STS	Science, Technology, and Society
TPP	Technology and Policy Program (through Summer 1999)
UND	Undesignated Sophomore (not yet declared Course)

Used for subjects only: **SEM** Undergraduate Seminar; **CTS** Center for Transportation Studies; **SP** Special Programs; **AS/MS/NS** ROTC; **SRE** Division for Study and Research in Education; **EC** Edgerton Center; **WGS** Women's & Gender Studies. Subjects taken under a Cross-registration arrangement begin with the following school codes: **BU** Boston U; **HA** Harvard U; **MC** Mass College of Art and Design; **SM** School of Museum of Fine Arts; **TU** Tufts U; **W** Wellesley College.

Privacy
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Revised July 2016

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MICHAEL GREENSTONE
MILTON FRIEDMAN DISTINGUISHED SERVICE PROFESSOR IN ECONOMICS
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October 25, 2019

A Recommendation for Ludovica Gazze

Dear Colleagues:

This letter is in support of Ludovica Gazze's application for a junior position at your institution. Gazze received her PhD from MIT in June 2016. Since then, she has been working as a Postdoctoral Scholar at the University of Chicago's Energy and Environment (E&E) Lab that I direct giving us the opportunity to work together closely.

I am delighted to write on her behalf and to strongly recommend her.

Gazze is an environmental and health economist with an exciting research agenda that has two parts. First, she is writing papers that are greatly expanding understanding about the causes and consequences of lead poisoning, as well as ways to mitigate its damages. This work is of high research and social value and she is uncovering the central role that economics plays in answering these questions. Put plainly, she is a rising leader on the economics of lead exposure and regulation.

Second, she is on the forefront of an emerging approach to energy and environment research that I call "co-generation," which is the lifeblood of the E&E Lab's approach. Its basis is that there is an intersection between the research frontier and policy impact that is a largely unoccupied space but is the best path forward for the wonk's nirvana of evidence-based policymaking. The power of collaborating with policymakers on policy experiments is that the reforms are pre-vetted for local political feasibility and have policymaker buy-in, meaning that the otherwise very difficult step to policy impact is made much easier. Additionally, it turns out that by working with policymakers it is possible to test quite novel reforms with meaningful economic content. Gazze is involved in a series of projects including completed ones in Fresno, CA and ongoing ones with the states of California, Colorado, New York, and Illinois.

Gazze's job market paper, "Hassles and Environmental Health Screenings: Evidence from Lead Tests in Illinois," illustrates how to efficiently assign screenings to determine eligibility for lead remediation of one's home. The government's challenge is that millions of homes have lead paint (3.6 million or two-thirds of homes in Illinois were built prior to the lead paint ban in 1978) that poses a health risk, especially, for children and there aren't sufficient resources to identify and then remediate all of them. The most common allocation rule is to target remediation to homes based on children's blood lead level.

Gazze examines how current approaches to screening children affect targeting efficiency. That is, do they draw in the children with the highest risk of testing positive for lead poisoning? In the presence of limited resources, even for testing, this question is central to the development of efficient policy. The focus is on how screening costs affect targeting efficiency and is a novel test of the "ordeals" model laid out by Nichols and Zeckhauser (1982). Gazze recognizes that travel costs to health care providers can serve as ordeals and whether they lead to take-up by families with higher-risk or lower-risk children depends on the correlation between travel costs and the benefits in terms of identifying a child with elevated blood lead levels. The challenge for policymakers is that the potential recipients are likely to have private information about whether their home needs remediation. So in the presence of this private information, can government ensure that the people at the highest risk of lead poisoning get screened?

To examine these issues, Gazze sets out a structural model of blood lead testing take-up, motivated by clarifying theoretical models of the individual's and planner's optimization problems, respectively. She takes this model to very rich administrative data from the state of Illinois that includes the complete lead screening histories for over 2 million children born in Illinois between 2001 and 2014. These data are merged to a data set that has housing age information from assessor files. The focus in her estimation is the elasticity of screening with respect to distance from health care providers, which is her measure of travel costs. To confront the possibility that risk varies with distance from health providers, Gazze turns to an increasingly popular identification strategy of relying on providers' openings and closings to generate variation in distance from a provider. This approach is combined with very fine-grained location fixed effects (i.e., census tract and census block) so that the comparisons are based on children born in the same neighborhood in different years who have different provider access, after adjustment for year fixed effects.

There are two core empirical findings. First, an extra 15 minutes in one-way distance from a provider decreases the likelihood of screening by 9 percent and proximity to high-quality providers increases take-up. Second, there is no evidence that households who live further distances have higher lead exposure risk. This finding runs counter to the standard ordeals model that those with the highest benefits should travel the farthest; thus, distance does not improve targeting efficiency. Put another way, distance deters testing but has no beneficial targeting impact.

Taking advantage of her structural model, Gazze then conducts a series of policy counterfactuals. A starting point for this exercise is that households in the oldest, and thus riskiest, households are willing to pay (WTP) $6.14 for screening, which seems low (although incidence rates are also low). This low WTP means that there are only modest private benefits from a series of interventions that provide incentives for households/providers, increases in providers, and a mandate for universal screening for children in old homes. Gazze notes that when one accounts for the externalities from lead poisoning (e.g., crime, disruptions in classrooms, etc.) such counterfactual policies may be socially beneficial.

Gazze has also authored "The Price and Allocation Effects of Targeted Mandates: Evidence from Lead Hazards" (R&R, *Journal of Urban Economics*) that estimates the impact of state-level policies to remediate lead hazards in old houses with small children. I say "old" because lead was used extensively in paint until 1978 when it was banned. Nationwide, it is estimated that about 5.5 million children live in homes with lead paint. These policies vary in the timing of their implementation and their exact form but generally require that owners of properties that are rented to families with children remove lead from the facility (with costs ranging between $500 and $40,000). The idea of these regulations was to induce owners of these houses to remove or abate lead-based paint, but there has also been a concern that they operate like a tax on buyers of old homes. Of course, the housing market equilibrium that results need not exactly match what policymakers intended.

The paper presents the first large-scale evidence on the effects of these state abatement mandates on the housing market. The heart of the empirical approach is a difference-in-differences style estimator that exploits the timing of the state policies and whether a house was constructed before or after 1978. Gazze finds that the mandates decrease the prices of old (i.e., those constructed prior to 1978) houses by 7.1 percent, acting as a large tax on owners. This result is confirmed from some powerful event-study style figures. Additionally, after a mandate's imposition, families with children are 14.6 percent less likely to live in old houses. Taken together, these results are consistent with the possibility that mandates do not greatly affect abatement rates. Further, it is evident that they impose large costs on owners of old houses and seemingly on families with children too. In many respects, this paper is an important example of how demand and supply forces can make it challenging for policymakers to achieve their goals.

There are several appealing features of these lead papers that are indicative of her future. First, both papers take an important issue of public health and brings economics to bear on how to make progress on it and what the welfare consequences of doing so are. In this respect, they are important advances on the public health lead literatures and make clear that it is impossible to ignore the economics when devising environmental, health, or really almost any policy. Second, the papers reflect a tremendous amount of work in pulling together administrative and other big data sets and merging them with outside data sets. While not glamorous, such efforts are the lifeblood of compelling microeconomics these days. All in all, it is apparent that between these two papers and the several other lead papers that she is working on (see her CV), she is becoming a leader on the economics of lead exposure and regulation.

I now want to turn to her work at the Energy & Environment Lab, where she has been engaged in all facets of the "co-generation" process that requires a very wide set of skills. A key skill is to gain and keep the trust of policymakers who often only see risk when academics come knocking because their objectives can differ from academics' in fundamental ways. Gazze has now become adept at all stages of these relationships and indeed has nurtured some from their very earliest phase to actual projects. Of course, these projects also require creativity and insight in identifying frontier questions and methods to answer them, like all excellent academic research but with the added twist that it has to be politically feasible.

The most advanced of these projects has produced the paper, "Enforcement and Deterrence with Certain Detection: An Experiment in Water Conservation Policy" (joint with Browne, Greenstone, and Rostapshova), which was conducted with the city of Fresno, CA. This paper is part of a new wave of research that asks how the economics of environmental regulations will be affected when the costs of detecting violations become tiny or even approaches zero. Much of the terrific theoretical literatures on regulation and private information may soon not apply to much of the real world. This change is also relevant to the older but canonical Becker (1968) model of crime that envisioned the costs of catching violators to be meaningful.

This project specifically examines whether smart meters can be used to improve water conservation in a part of the US that is subject to frequent droughts. At the time that we began conversations with Fresno, they had installed about 100,000 smart meters but had no real plan on how to take advantage of the technology. Indeed, they were determining which households were violating their watering rules by having "water cops" drive around the city looking for households who were watering their lawns on the wrong days—a very expensive approach, especially when the meters were recording the information at zero marginal cost. Gazze was central in convincing Fresno to experiment with using smart meters to test the effects of automated detection, varying fines, and varying thresholds. She additionally played a similarly central role in helping to design the experiment.

The paper reports on a randomized field experiment that assigned 88,905 households into 12 groups varying enforcement method (automated via smart meters or manual inspection), violation threshold, and fine levels. Automated enforcement decreases water use by 3% and violations by 17%. However, fines increase by 13,800% and customer service calls and complaints increase by 545%. The backlash underscores the political costs of 100% detection, suggesting the adoption of new detection technologies may be limited by politics.

Through her work at the Energy & Environment Lab, Gazze and I are collaborating (often along with others) on a series of other co-generation style projects that aim to advance the research frontier and affect policy. These include projects that aim to test novel ways to improve enforcement of heavy-duty truck emissions in California, detect oil and gas sites that are violating methane emissions rules in Colorado (this one combines satellite and administrative regulatory data), and improve traffic safety in New York City. While these projects are at various stages of development, they all currently involve randomized control trials and have high potential for policy impact. I'm confident that Gazze will continue to conduct this style of research.

In summary, Gazze is an empirical economist working in the fields of environment and health. She uses economics to guide her analyses and on the empirical side has a wide set of tools running from the full gamut of reduced-form evaluation approaches to designing and conducting randomized control trials. She is using these tools in the service of an ambitious research agenda that marks her as a leader on the economics of lead exposure and regulation and places her at the frontier of the emerging area of "co-generation" that aims to simultaneously produce frontier research and affect policy. As if all of this were not enough, she also has a great deal of experience writing successful grants.

I strongly recommend that Economics departments, Business schools, Policy schools, and Schools of Public Health with an interest in Environmental and Health economics give Gazze careful consideration for an appointment. She is terrific.

If I can provide further information, please do not hesitate to contact me.

Best Regards,

Michael Greenstone

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October 3, 2019

To whom it may concern:

I am writing on behalf of **Ludovica (Ludo) Gazze,** an **MIT** Ph.D. student who received her doctorate in 2016 and is looking for *an Assistant Professor or other research position* for the Fall of 2020. Ludo is an applied microeconomist specializing in environmental economics. At MIT, Ludo was advised by Ben Olken, Jim Poterba, and me. Upon graduation, she took a post-doc position at the University of Chicago Energy and Environment Lab, where she works closely with Michael Greenstone.

Ludo left MIT with strong training in Labor and Development and a deep interest in environmental issues. She has used her time at the Energy & Environment Lab to build on these skills and interests, developing a full-blown environmental agenda. A major fruit of this work is an exciting new (solo-authored) job market paper on the effects of travel costs on lead exposure screening take-up and subsequent remediation. Lead screening takes time and money. How important are the costs of detecting lead exposure, especially among poor children? Using a massive and previously unexploited administrative data set on children in Illinois, Ludo explores the efficiency and welfare consequences of lead exposure screening. Because travel costs are not randomly assigned, she develops a compelling identification strategy that exploits openings and closings of screening providers. The results are striking: 15 minutes of additional travel reduces screening by 9 points. At the same time, her analysis uncovers no evidence that travel (or "hassle") costs act as a favorable selection device where those at highest risk of lead exposure self-select into screening.

Ludo's job market paper provides credible and timely evidence on a stubborn public health problem. But her work goes beyond credible impact evaluation with an insightful welfare analysis that takes account of the fact that lead poisoning is rare in the general population. The results of this analysis suggest a high willingness-to-pay for screening, yet screening is a blunt instrument for reducing lead-related harms. Obvious policy strategies such as increasing the density of screening providers are unlikely to be fruitful because screening rarely leads to homeowner or landlord remediation. Overall, Ludo provides a nuanced and sophisticated economic analysis that offers thought-provoking findings but no easy answers to an important public health problem. I expect this paper to find its way into a top journal.

Ludo's job market paper is emblematic of the steady stream of sophisticated environmental and public health economics we can expect from her in the years to come. Her rapidly-expanding research portfolio includes an impressive thesis chapter examining the effect of US states' lead paint abatement mandates on the housing market. This paper (now R&R at the *Journal of Urban Economics*) focuses on the consequences of lead abatement mandates for the cost and allocation of housing. Ludo uses a simple model of the housing market to guide her interpretation of a range of fascinating empirical results. In addition to the housing market, the paper also looks at the health consequences of abatement mandates and presents a thought-provoking welfare analysis of abatement policy. The identification strategy is transparent (a triple differences strategy exploiting abatement variation by state, year and home construction vintage), and Ludo's empirical analysis is exceptionally meticulous and convincing. This project also incorporates a fascinating exploration of the distributional consequences of abatement mandates.

Ludo has many other exciting empirical projects in draft form or in the kitchen. All promise to be relevant and interesting, and seem likely to yield major publications. For example, her manuscript with Jennifer Heissel focuses on the effects of pipe maintenance on lead exposure, exploiting the obscure but important fact that routine water main replacement can actually aggravate lead exposure. This creative and exceptionally topical project (in view of the problems in Flint, MI) leverages new data sources and uses econometric tools to produce a timely state-of-the-art causal analysis of an important question.

Since leaving MIT, Ludo has hit her stride. She has proven to be a creative and tireless scholar and is headed for a stellar research career. Ludo's impressive combination of strong research skills, collegiality, and capacity for hard work will make her a welcome addition to any economics department looking to make an outstanding applied micro hire. She is an exceptionally strong candidate for any school or program looking specifically to hire someone with interests in environmental economics or in real estate and housing markets. Ludo is an excellent presenter and will likely make a strong teacher. Finally, I expect you will find Ludo to be a valued colleague and outstanding sounding board for your own work. I am very happy to enthusiastically recommend Ludo to you.

Please don't hesitate to contact me if I can provide further information.

Sincerely,

Josh Angrist
Ford Professor of Economics

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

Ludovica Gazze*

October 2019

Job Market Paper

Latest version available at http://economics.mit.edu/files/18371

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.

*Department of Economics, University of Chicago. Email: lgazze@uchicago.edu. I am indebted to Michael Greenstone for his mentorship throughout my postdoctoral scholarship. Ali Abbasi, Marcella Alsan, Alex Bartik, Fiona Burlig, Thomas Covert, Catie Hausman, Michael Kofoed, David Meltzer, Rebecca Meyerson, Jack Mountjoy, Tommaso Sonno, Dan Waldinger, and seminar and conference participants at EPIC, Indiana University, Urban Labs, APPAM, ASHEcon, the H2D2 Research Day, and the 4th Marco Fanno Alumni Workshop provided helpful comments and suggestions. I am also extremely grateful to the staff at the Illinois Department of Public Health for sharing the data for this analysis as well as their insights and expertise in interpreting the results. The conclusions, opinions, and recommendations in this paper are not necessarily the conclusions, opinions, or recommendations of IDPH. This project would not have been possible without the generous support of the Joyce Foundation. Bridget Pals and Xiyue (Iris) Song provided excellent research assistance. All remaining errors are my own.

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

[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.

1982). 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 complete lead screening histories for the universe of over 2 million children born in Illinois between 2001 and 2014 to housing age information from assessor files. Screening data includes information on realized poisoning risk for the subsample of screened children, and housing age data provides 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. 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 have similar expected 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 remediation funding. Thus, removing barriers to screening may not lead to increased remediations, perhaps due to partial compliance with abatement regulations or limited awareness of remediation funding. Moreover, proximity to high-quality providers, as measured either by 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 average WTP for screening among households in the most at-risk homes to be \$6.14, \$4-29 higher than the WTP of low-risk households. Such a low average 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.[4] 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 models households' screening decision and discusses the impact of travel distance on targeting efficiency and prevention. 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

[4] 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, thus ruling out benefits from learning that a home is lead-safe. Specifically, screening benefits derive from assignment of the lead-poisoned child to case management aimed at reducing lead poisoning damages.[5] Parents' perceived screening benefits depend on several factors, including information about exposure risk, degree of risk aversion, degree of altruism towards the child,[6] beliefs about treatment costs and recovery probability,[7] and additional benefits from visiting the doctor, such as having a physical examination or an immunization shot.[8] 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.[9] Then, child i is

[5] 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.

[6] 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)

[7] Myerson et al. 2018 show that increasing treatment access increases screening, evidence of an "ostrich effect".

[8] Not observing these additional services does not bias the selection analysis if benefits from these additional services are not correlated with screening benefits.

[9] 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:

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

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,

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

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

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.[10] 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 who will live in i's building in the future. Summing over the set of screened children S, this yields

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

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

The planner cannot optimally target screening without knowing ex-ante the externality each child's undetected poisoning would impose on society. However, the planner observes a proxy for exposure risk at each home, namely housing age. In this case, a policy requiring screening based

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

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.[11] 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-deceased children not linked to any tests as not screened. Appendix Tables A.2 and A.3 show the

[11] 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.

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 construct a child's age at time of testing based on test date and 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 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 elevated blood lead levels (EBLLs), defined as blood lead levels above $9\mu g/dL$, from over 22,000 children. Laboratories have different minimum reporting limits, which vary over time, meaning BLLs are bottom-censored; I correct for these limits to obtain correct population estimates of lead exposure.[12]

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. Because my analysis focuses on outcomes by age two, when lead exposure is most damaging, mobility is not likely to severely bias my estimates of the effects of distance.

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

[12] 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.[13] 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 consider 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.[14,15] 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-

[13]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.

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

[15]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.[16] 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.[17] I also compute the share of EBLLs detected by each provider with a required follow-up within 90 days.[18] 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).[19] 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. 2019*b*). In this paper, I use a house's construction year to proxy for the child's observable risk of lead exposure. To do so, I link exact birth addresses to parcel-level housing data in the Zillow Transaction and Assessment Dataset that includes information on when each house was built.[20]

[16]For most providers, the median child's address is within 7 kilometers of their provider's address.
[17]I only observe Medicaid status for screened children.
[18]Appendix Figure A.6 shows that only around 50 percent of EBLLs have a follow-up test.
[19]Appendix Figure A.9 shows the location of providers of different quality in Illinois.
[20]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.

I define children living in homes built before 1930 as high-risk. Older homes have a higher 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*a*) 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. HUD awards grants to local agencies for individual remediation projects.[21] 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 dis-

[21] 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.

tance 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 approach 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 does not show 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.[22] 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. For reference, Herbst & Tekin (2012) estimate an

[22] 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.

elasticity of -0.13 for take-up of childcare subsidy.

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).[23] 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.[24] 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

[23] 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.

[24] Source: Bureau of Labor Statistics.

from the closest five providers, to take into account that households do not always visit the closest 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 way 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). Appendix Table A.14 shows that my estimates are robust to including only children born within two kilometers of a provider's entry or exit during their birth year, suggesting that the results in the larger sample are not driven by omitted correlates of provider location.

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.15 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-poisoned children. If lower detection rates lead to lower remediation rates in affected homes, future residents may face increased poisoning risk, too. This section investigates the extent to which changes in distance affect the likelihood and timeliness of detecting an EBLL, as well as the likelihood of remediations and the likelihood of 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 a blood lead level of $10\mu g/dL$ or above. 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.[25]

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.[26] 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 pharmacies, mobile clinics, and local health departments, the disparity in immunization and screening rates suggests that providers

[25]Remediations and repeated EBLLs in the same home are rare, although my sample includes over 2,000 of these events. Appendix Table A.16 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.

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

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).

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 than 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, policies improving providers' screening-related quality, such as 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 average estimates in 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 most households have a negative WTP for screening and that households in riskier homes have the highest WTP. Households in homes built prior to 1930 are willing to pay \$6.14 for screening. Similarly, households with low socioeconomic status have a higher WTP for screening than better off households, reflecting their heightened risk. 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.[27] 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.[28] 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

[27] 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.

[28] 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.

an expected lifetime cost of living in a pre-1930 home relative to a new home of \$45.[29] On the 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.[30] 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. While I report the costs of these policies although they involve transfers, examining the opportunity cost

[29] 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).

[30] 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.

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 \$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.[31] Because this value omits the crime costs of lead poisoning, it likely underestimates the total externality of lead poisoning.[32] 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.

[31] 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).

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

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 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 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 societal benefits from averted poisoning externalities.

My findings suggest that decreasing travel costs, for example through subsidies, could increase screening without reducing targeting efficiency. Yet, 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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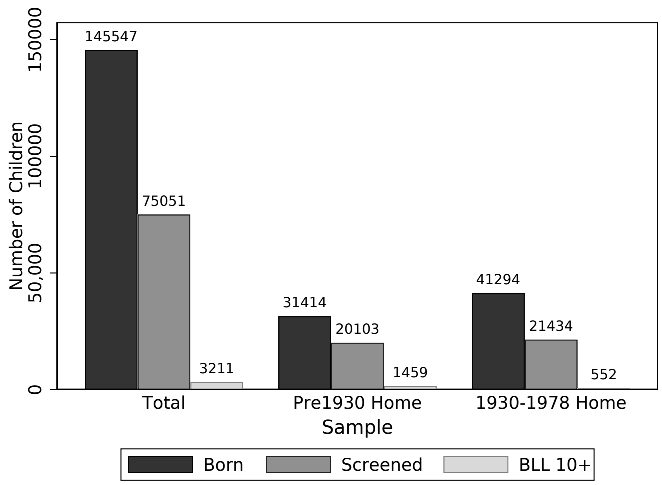
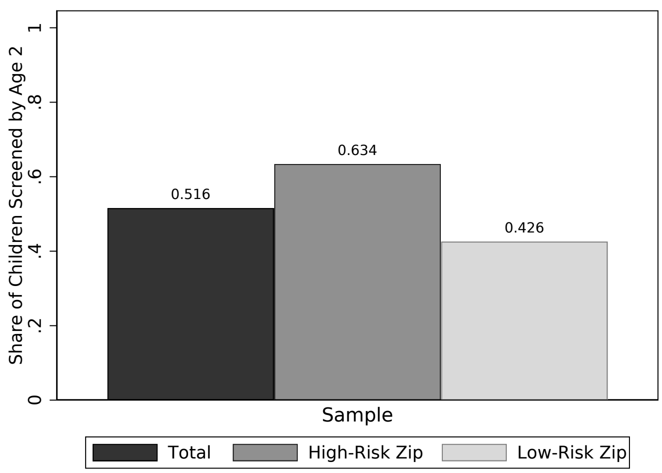
Figure 1: Number of Children Born, Screened, and with BLLs 10+, 2014 Cohort



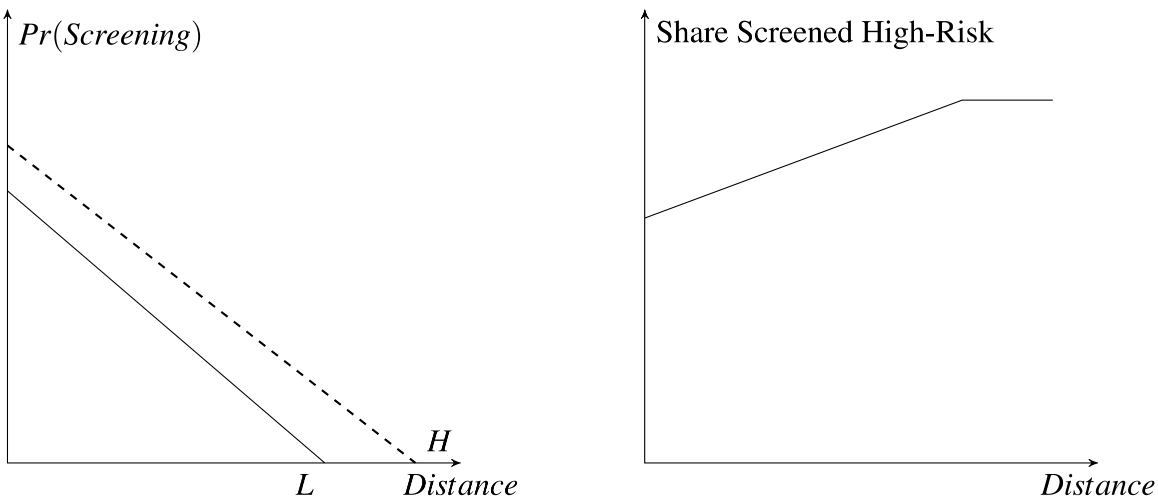
Notes: The figure plots the number of children born, screened, and with blood lead levels 10+ in the 2014 cohort in the whole sample and for the sample of children in pre-1930 and 1930-1978 homes.

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



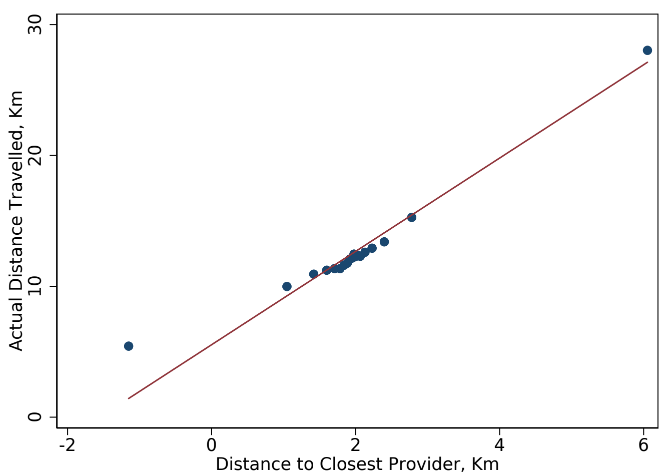
Notes: The figure plots screening rates by age two in the 2014 Illinois birth cohort by risk-level in the birth zip code.

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.

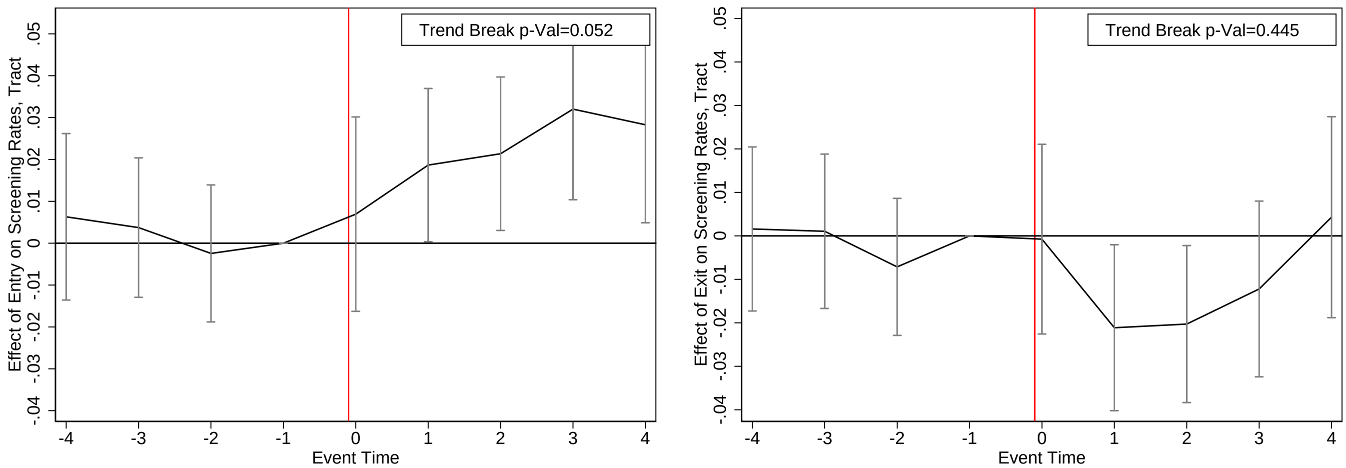
Figure 4: Distance to Closest Provider Predicts Distance Travelled



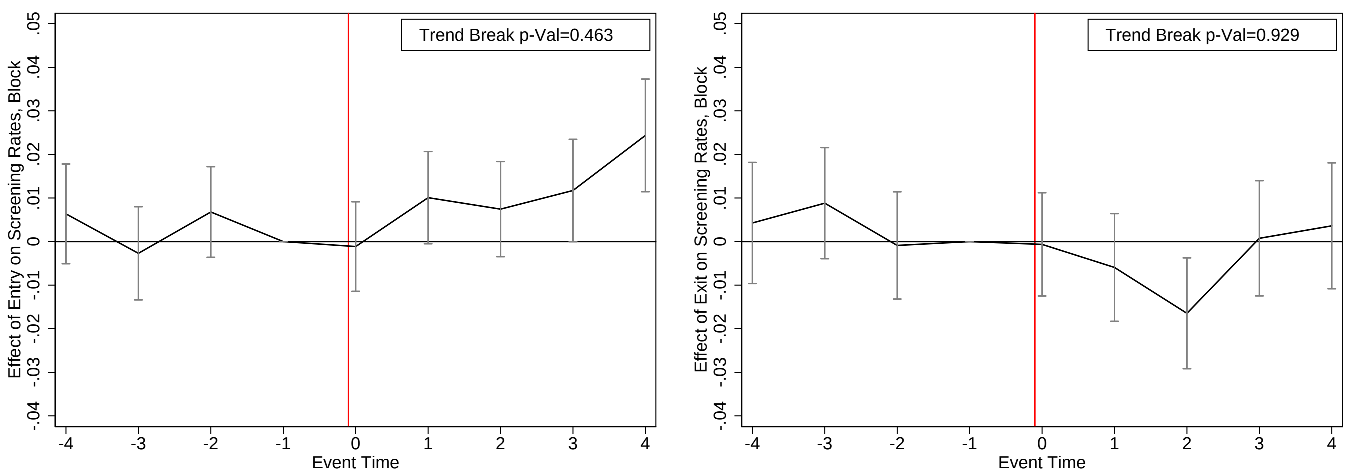
Notes: The figure plots average distance travelled to see a provider, in kilometers, for each vintile of distance between address at test and closest provider, in kilometers (blue dots) as well as the fitted line after partialling out block and year fixed effects.

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

(a) Tract Level

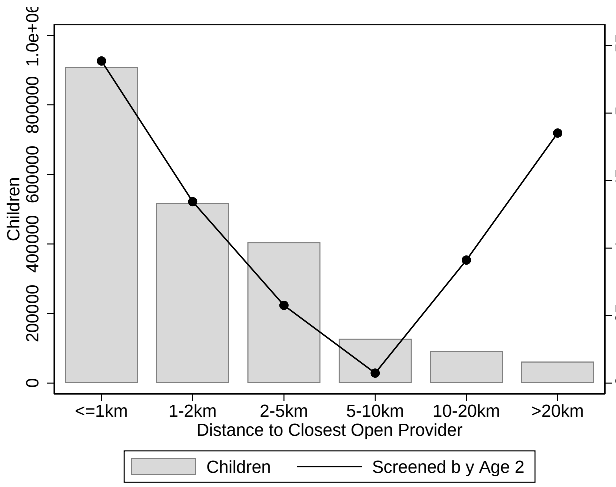


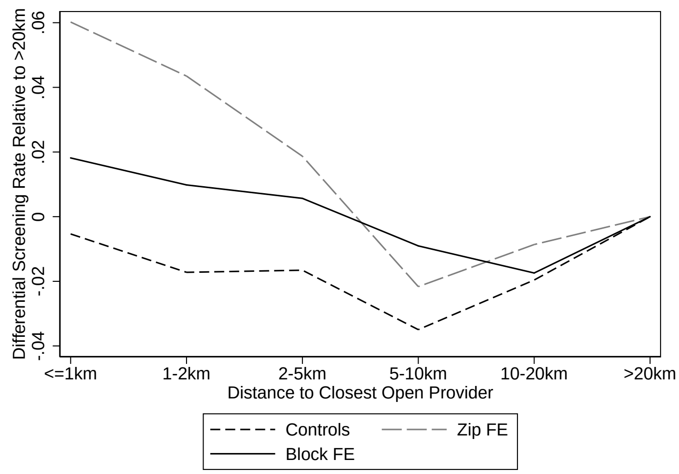
(b) Block Level



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.

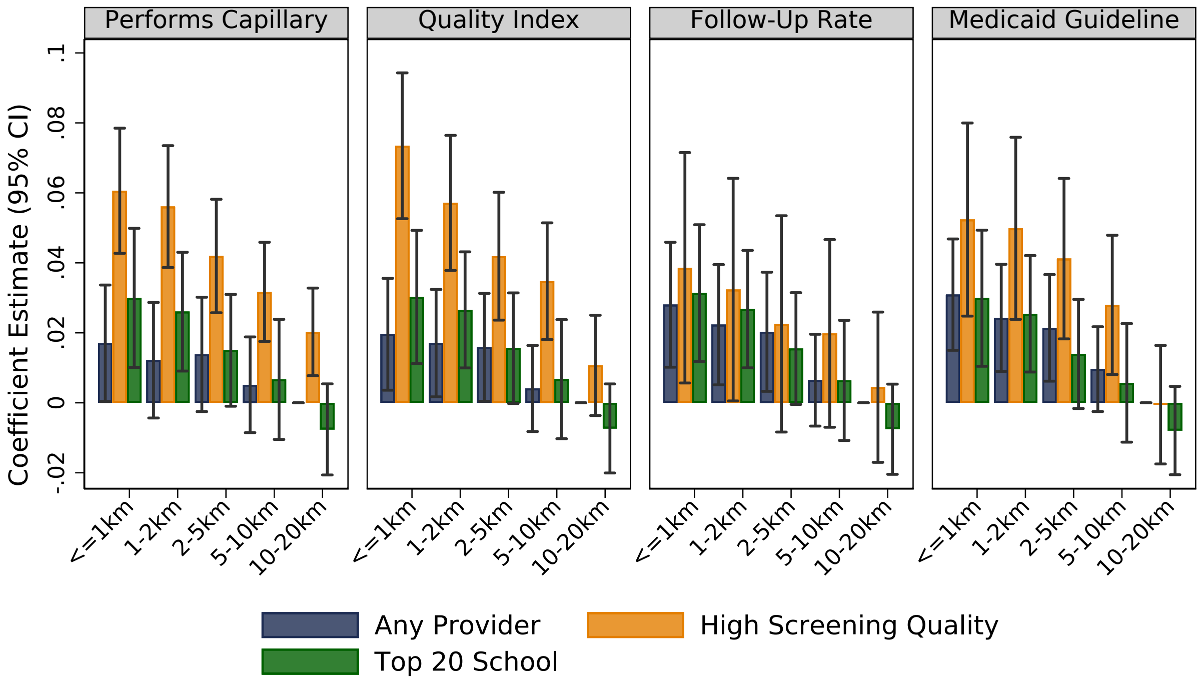
Figure 6: Determinants of Screening: Distance to Providers





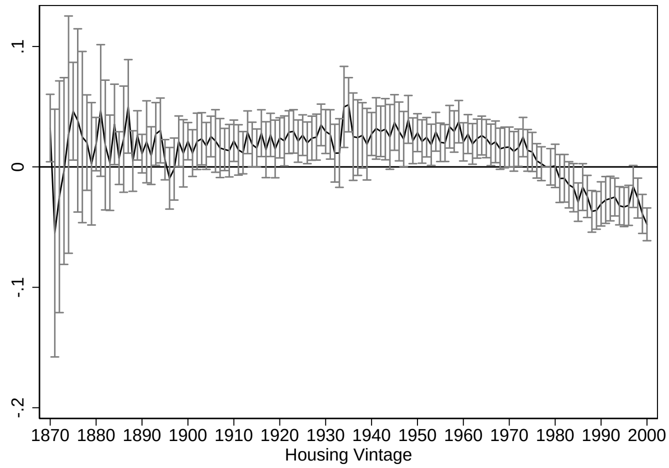
Notes: The figure plots the average likelihood of a child being screened by age two by distance to closest open provider. The bars in the left panel show the number of children in each distance bin on the left y-axis, and the line represents their screening rates on the right y-axis. The right panel 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	-0.008***	-0.004***	-0.004***	-0.004***	-0.003***
Open Provider	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)
Panel B: Binned Distance					
Closest Open Provider	0.072***	0.040***	0.040***	0.041***	0.032***
within 1Km	(0.008)	(0.006)	(0.007)	(0.009)	(0.012)
Closest Open Provider	0.055***	0.026***	0.028***	0.033***	0.023*
1-2Km	(0.007)	(0.006)	(0.007)	(0.009)	(0.012)
Closest Open Provider	0.030***	0.015**	0.017**	0.028***	0.012
2-5Km	(0.007)	(0.006)	(0.007)	(0.009)	(0.012)
Closest Open Provider	-0.011*	-0.006	0.001	0.013*	0.006
5-10Km	(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	BLL By Age 2	Home Pre1930	Black	Hispanic	Single Mother	Mother 20 or Younger	Mother High School or Less
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Tract and Year FE								
Distance to Closest Open Provider	-0.0003**	-0.0052*	-0.0047***	-0.0023***	-0.0021***	-0.0019***	-0.0002	-0.0008
	(0.000)	(0.003)	(0.001)	(0.000)	(0.001)	(0.001)	(0.000)	(0.001)
Panel B: Block and Year FE								
Distance to Closest Open Provider	-0.0004	-0.0084	-0.0012**	-0.0004	-0.0002	0.0006	0.0002	-0.0004
	(0.000)	(0.007)	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)
Mean Outcome Variable	0.02	2.93	0.46	0.23	0.33	0.49	0.12	0.16
N	890091	890091	645177	890091	890091	890091	890091	890091

Notes: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. The table displays the impact of distance to the closest provider open during a child 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.

TABLES

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.

TABLES

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 in each subsample as estimated by the logit model in Panel A. The sample includes all geocoded children born 2001-2014 whose birth address matched a parcel record, and whose closest provider is within 20 kilometers and either opened or closed during their birth year. Each column includes birth year indicators, child-level demographic controls, and block-level averages of all included regressors. Standard errors clustered at the zip code level in parentheses.

TABLES

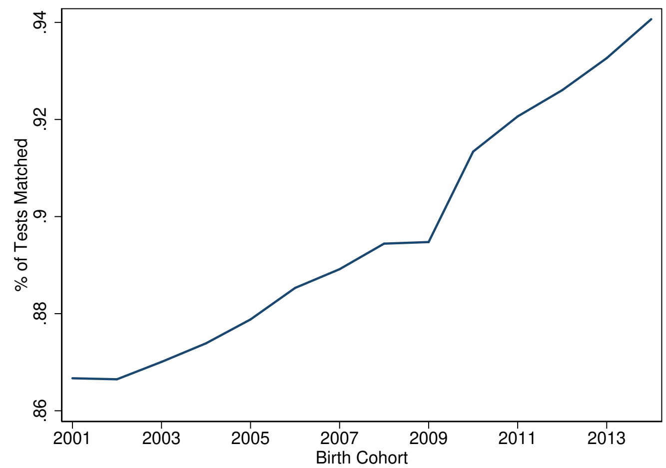
Table 5: Policy Counterfactuals

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.

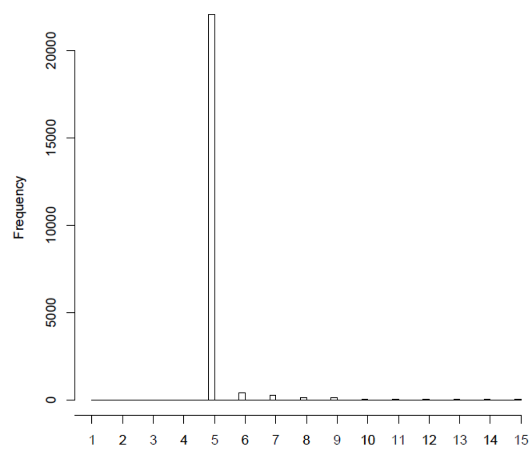
TABLES

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



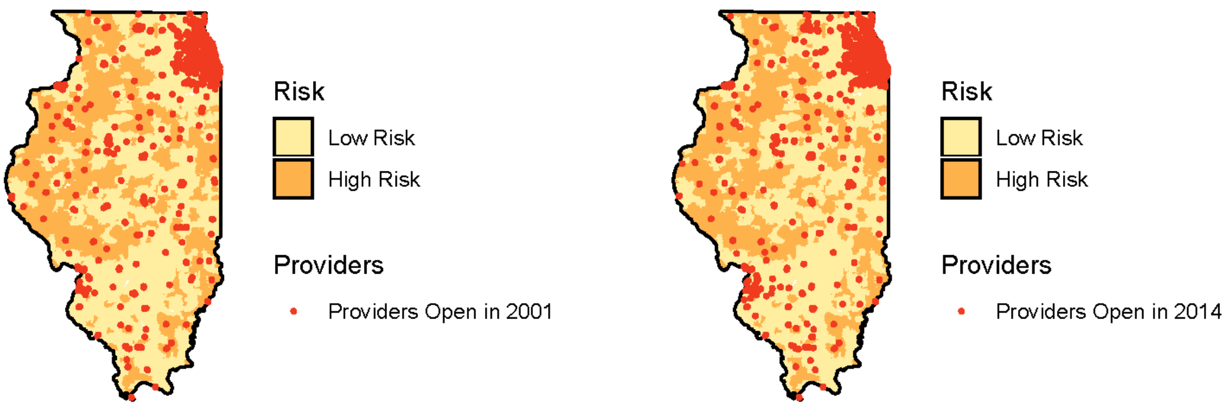
Notes: The figure plots the percent of tests successfully linked to birth records by birth cohort as recorded in the test data.

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



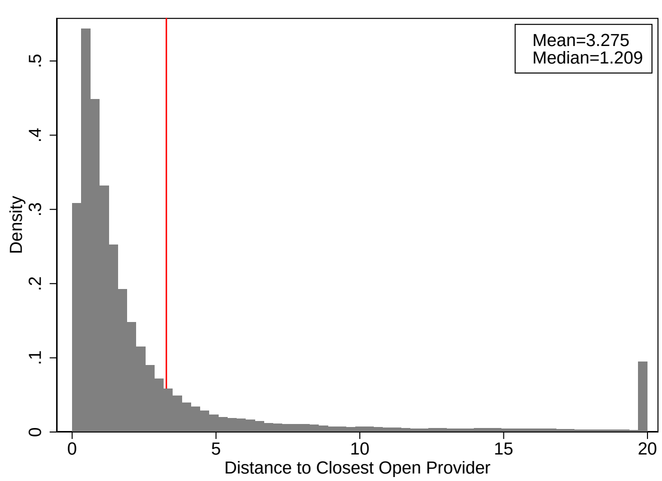
Notes: The figure plots the number of tests on the y axis by BLL result on the x axis for one laboratory in our sample.

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



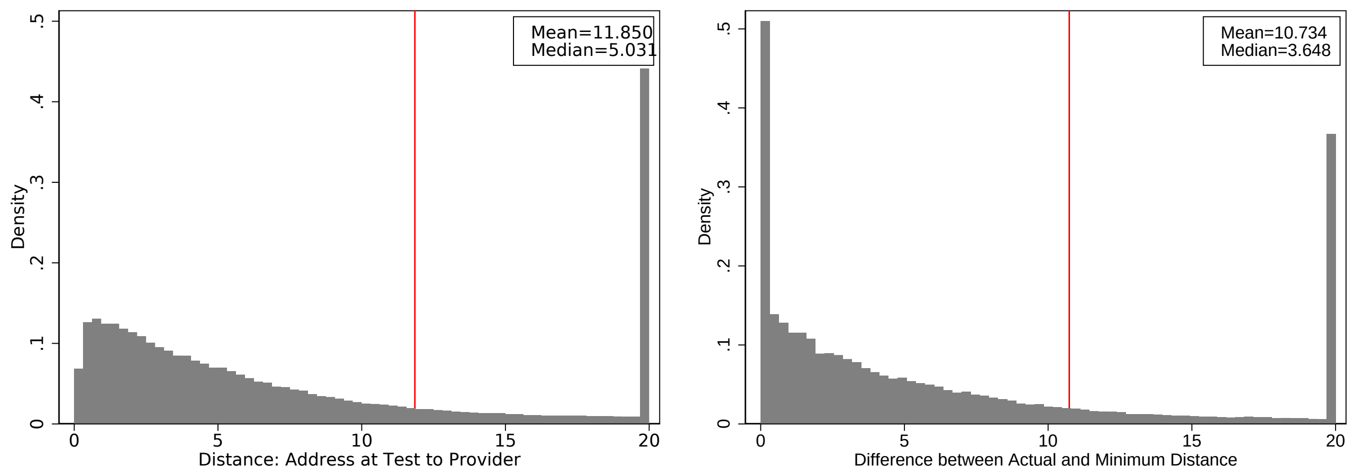
Notes: The figure plots the distribution of open providers in Illinois in high and low risk zip codes in the years 2001 (left) and 2014 (right).

Figure A.4: Distance to Providers



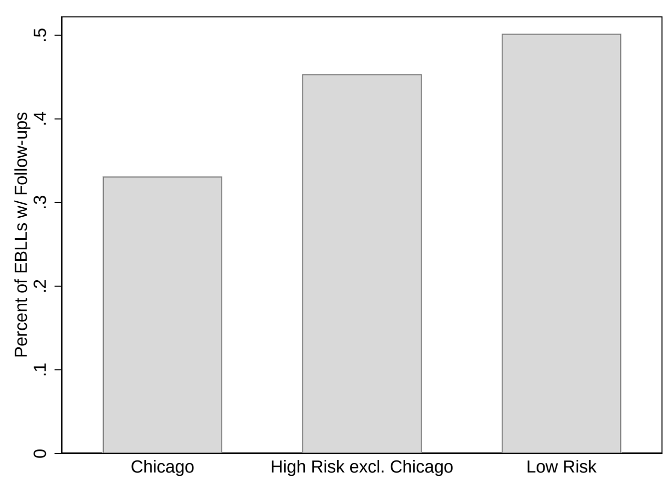
Notes: The figure plots the distribution of distance in kilometers from children's birth address to the closest provider open during the child's birth year. Distance is censored at 20km for ease of visualization. The red vertical line indicates the mean of the variable in the uncensored data.

Figure A.5: Distance to Providers



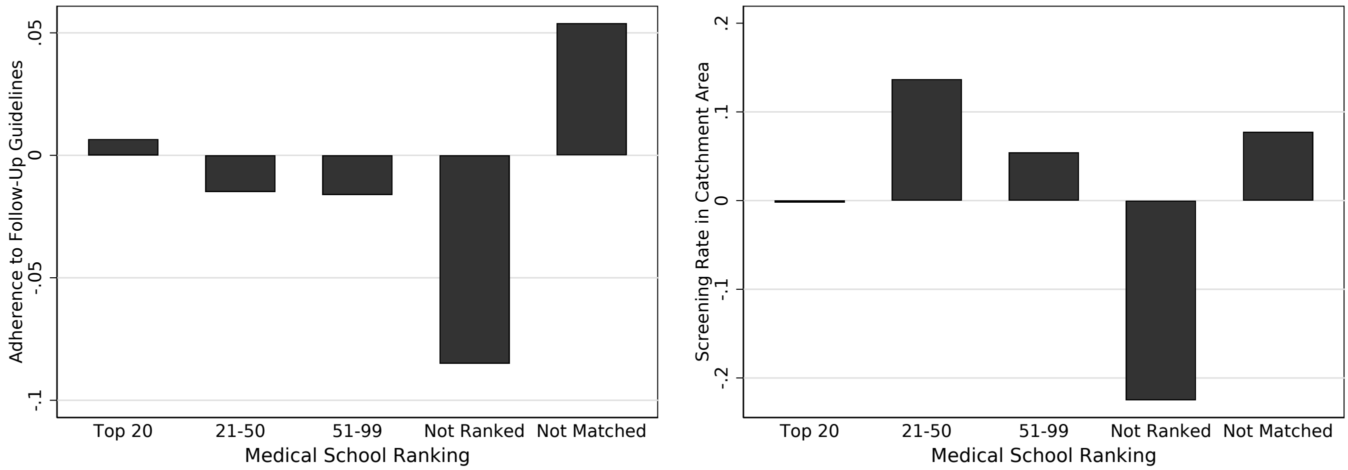
Notes: The left panel plots the distribution of distance in kilometers between children's address at test and the provider associated with the test. The right panel plots the distribution of the difference in kilometers between distance traveled at test and minimum distance between address at test and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



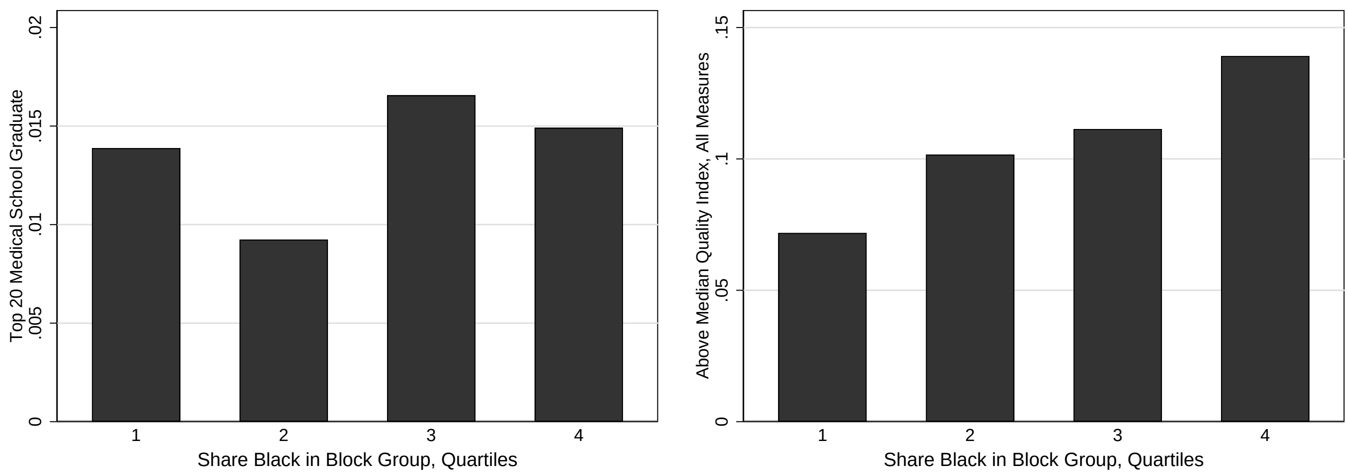
Notes: The figure plots follow-up rates in IL for tests that identify an EBLL by risk-level in birth zip code.

Figure A.7: Providers: Correlation in Quality Measures



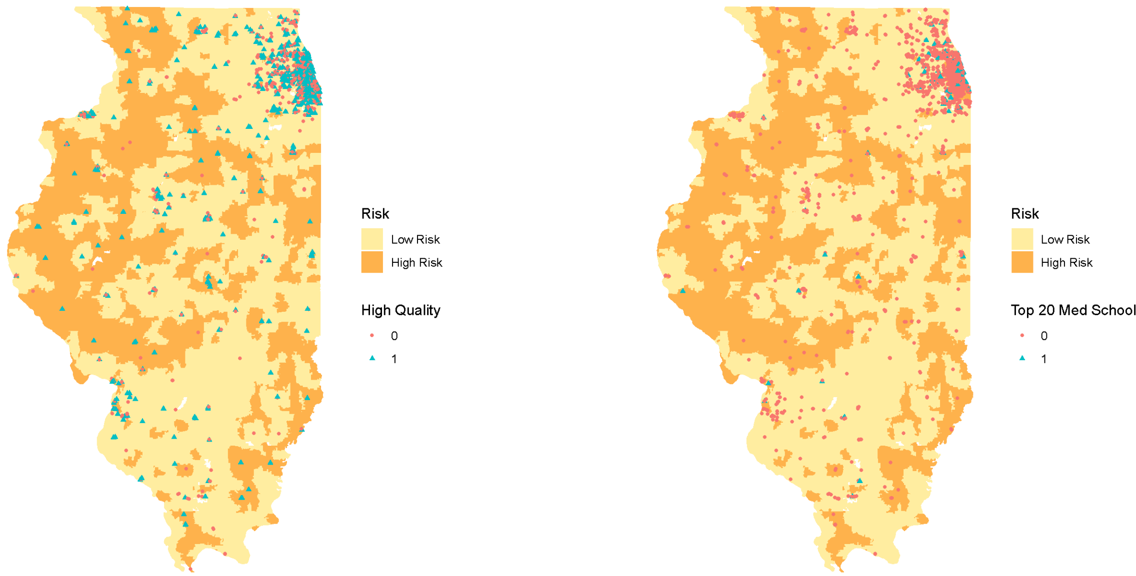
Notes: The figure plots on the y axis the average z-scores of adherence to follow-up guideline (left panel) and screening rate (right panel) by ranking of the medical school each provider earned their degrees at on the x axis.

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



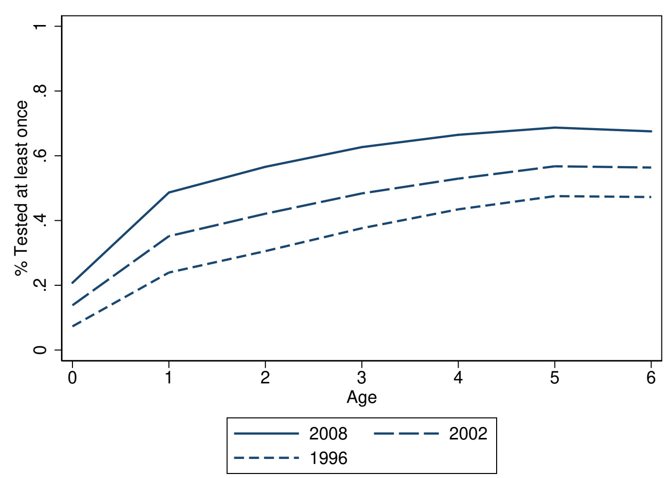
Notes: The figure plots on the y axis the share of providers who are from top 20 medical schools (left panel) and who have a quality index above median (right panel) by share of black children born in the provider's census block group on the x axis.

Figure A.9: Location of Providers, by Quality



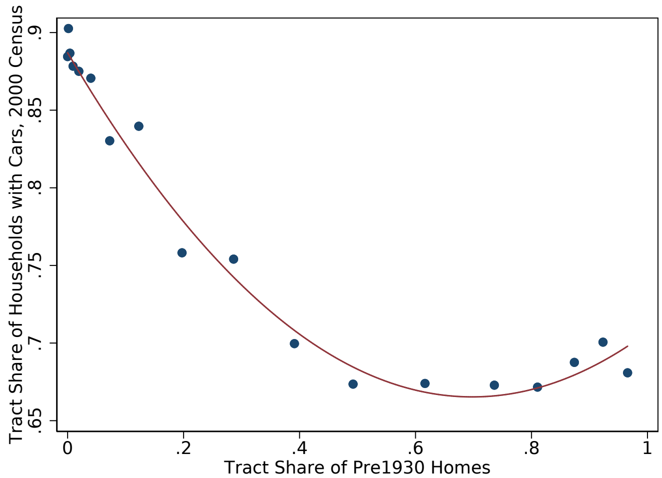
Notes: The figure plots the distribution of open providers by quality (left panel) and ranking of medical school of record (right panel) in Illinois in high and low risk zip codes over the years 2001-2014. High-quality providers are defined as having a quality index above median.

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 pre1930 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	Standard Deviation
	(1)	(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	Performs Capillary	Follow Up Rate	Medicaid Guidelines	Screening Rate	Top 20 Med School
	(1)	(2)	(3)	(4)	(5)	(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						0.005	0.006*
or Less						(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 within 1Km	0.018**	0.019	0.025***	0.020	0.031***	0.031
	(0.008)	(0.021)	(0.009)	(0.021)	(0.008)	(0.021)
Closest Open Provider 1-2Km	0.014*	0.019	0.021**	0.018	0.026***	0.028
	(0.008)	(0.021)	(0.008)	(0.021)	(0.008)	(0.021)
Closest Open Provider 2-5Km	0.015*	0.017	0.020**	0.017	0.024***	0.025
	(0.008)	(0.023)	(0.008)	(0.023)	(0.008)	(0.022)
Closest Open Provider 5-10Km	0.007	0.023	0.010	0.020	0.012*	0.021
	(0.007)	(0.021)	(0.007)	(0.021)	(0.007)	(0.020)
Closest Open Provider within 1Km, High Quality	0.054***	0.051***	0.060***	0.053***	0.051***	0.040***
	(0.009)	(0.015)	(0.008)	(0.012)	(0.007)	(0.012)
Closest Open Provider 1-2Km, High Quality	0.046***	0.043***	0.052***	0.046***	0.044***	0.035***
	(0.008)	(0.015)	(0.007)	(0.011)	(0.006)	(0.011)
Closest Open Provider 2-5Km, High Quality	0.035***	0.033**	0.041***	0.038***	0.031***	0.025**
	(0.008)	(0.014)	(0.007)	(0.011)	(0.006)	(0.011)
Closest Open Provider 5-10Km, High Quality	0.019***	0.005	0.023***	0.011	0.018***	0.005
	(0.007)	(0.015)	(0.005)	(0.012)	(0.005)	(0.012)
Closest Open Provider 10-20Km, High Quality	0.009	0.015	0.014**	0.001	0.011**	-0.002
	(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: Determinants of Screening: Provider Access, Sample Within 2Km of Entry or Exit

	(1)	(2)	(3)	(4)	(5)
Panel A: Continuous Distance					
Distance to Closest	-0.007***	-0.005***	-0.004***	-0.003***	-0.003***
Open Provider	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Panel B: Binned Distance					
Closest Open Provider	0.039***	0.032***	0.029**	0.027**	0.017
within 1Km	(0.012)	(0.012)	(0.013)	(0.014)	(0.015)
Closest Open Provider	0.023*	0.019	0.016	0.018	0.008
1-2Km	(0.012)	(0.012)	(0.013)	(0.014)	(0.015)
Closest Open Provider	0.011	0.010	0.008	0.012	-0.004
2-5Km	(0.012)	(0.012)	(0.013)	(0.014)	(0.016)
Closest Open Provider	-0.009	0.000	0.000	0.005	-0.001
5-10Km	(0.014)	(0.013)	(0.014)	(0.014)	(0.017)
Mean Outcome Variable	0.48	0.48	0.48	0.48	0.49
N	1653139	1653140	1653117	1637275	1222373
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 and who are born within 2 kilometers of a provider entry or exit during their birth year. Each column includes birth year fixed effects and the location fixed effects indicated at the bottom of each column. Standard errors clustered at the zip code level in parentheses.

Table A.15: Selection into Screening Conditional on Distance: Robustness Checks

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

Notes: $^{*}p < 0.10$,$^{**}p < 0.05$,$^{***}p < 0.01$. The table displays the impact of distance to the closest provider open during a child birth year on selection into screening by age two. The sample includes all geocoded children born 2001-2014 whose closest provider is within 20 kilometers and who are screened. Outcome variables are indicated in each column. Panel A reports the effects controlling for the child's birth tract, Panel B controls for child's birth block. Each regression includes birth year fixed effects as well as tract or block level time-varying controls such as average 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.16: Effect of Proximity to Providers on Prevention, Robustness Checks for Rare Events

Specification:	Low Income Block	Block with Remediation	Logit	Penalized Logit
	(1)	(2)	(3)	(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.