

Berkhout, Emilie; Maulana, Sandy; Molato, Rhea; Park, Albert; Suryadarma, Daniel

Working Paper

Lead exposure and cognitive skills in a developing country: Evidence from toxic sites in Indonesia

ADB Economics Working Paper Series, No. 774

Provided in Cooperation with:

Asian Development Bank (ADB), Manila

Suggested Citation: Berkhout, Emilie; Maulana, Sandy; Molato, Rhea; Park, Albert; Suryadarma, Daniel (2025) : Lead exposure and cognitive skills in a developing country: Evidence from toxic sites in Indonesia, ADB Economics Working Paper Series, No. 774, Asian Development Bank (ADB), Manila,
<https://doi.org/10.22617/WPS250115-2>

This Version is available at:

<https://hdl.handle.net/10419/322342>

Standard-Nutzungsbedingungen:

Die Dokumente auf EconStor dürfen zu eigenen wissenschaftlichen Zwecken und zum Privatgebrauch gespeichert und kopiert werden.

Sie dürfen die Dokumente nicht für öffentliche oder kommerzielle Zwecke vervielfältigen, öffentlich ausstellen, öffentlich zugänglich machen, vertreiben oder anderweitig nutzen.

Sofern die Verfasser die Dokumente unter Open-Content-Lizenzen (insbesondere CC-Lizenzen) zur Verfügung gestellt haben sollten, gelten abweichend von diesen Nutzungsbedingungen die in der dort genannten Lizenz gewährten Nutzungsrechte.

Terms of use:

Documents in EconStor may be saved and copied for your personal and scholarly purposes.

You are not to copy documents for public or commercial purposes, to exhibit the documents publicly, to make them publicly available on the internet, or to distribute or otherwise use the documents in public.

If the documents have been made available under an Open Content Licence (especially Creative Commons Licences), you may exercise further usage rights as specified in the indicated licence.



<https://creativecommons.org/licenses/by/3.0/igo/>

LEAD EXPOSURE AND COGNITIVE SKILLS IN A DEVELOPING COUNTRY

EVIDENCE FROM TOXIC SITES IN INDONESIA

Emilie Berkhout, Sandy Maulana, Rhea Molato-Gayares, Albert Park, and Daniel Suryadarma

NO. 774

April 2025

**ADB ECONOMICS
WORKING PAPER SERIES**

ADB Economics Working Paper Series

Lead Exposure and Cognitive Skills in a Developing Country: Evidence from Toxic Sites in Indonesia

Emilie Berkhout, Sandy Maulana,
Rhea Molato-Gayares, Albert Park,
and Daniel Suryadarma

No. 774 | April 2025

The *ADB Economics Working Paper Series* presents research in progress to elicit comments and encourage debate on development issues in Asia and the Pacific. The views expressed are those of the authors and do not necessarily reflect the views and policies of ADB or its Board of Governors or the governments they represent.

Emilie Berkhout (eberkhout@youth-impact.org) is a research associate at Youth Impact. Sandy Maulana (smaulana@adbi.org) is a consultant and Daniel Suryadarma (dsuryadarma@adbi.org) is a senior research fellow at the Asian Development Bank Institute. Rhea Molato-Gayares (rmolato@adb.org) is an economics officer and Albert Park (afpark@adb.org) is the Chief Economist and Director General of the Economic Research and Development Impact Department, Asian Development Bank.



Creative Commons Attribution 3.0 IGO license (CC BY 3.0 IGO)

© 2025 Asian Development Bank
6 ADB Avenue, Mandaluyong City, 1550 Metro Manila, Philippines
Tel +63 2 8632 4444; Fax +63 2 8636 2444
www.adb.org

Some rights reserved. Published in 2025.

ISSN 2313-6537 (print), 2313-6545 (PDF)
Publication Stock No. WPS250115-2
DOI: <http://dx.doi.org/10.22617/WPS250115-2>

The views expressed in this publication are those of the authors and do not necessarily reflect the views and policies of the Asian Development Bank (ADB) or its Board of Governors or the governments they represent.

ADB does not guarantee the accuracy of the data included in this publication and accepts no responsibility for any consequence of their use. The mention of specific companies or products of manufacturers does not imply that they are endorsed or recommended by ADB in preference to others of a similar nature that are not mentioned.

By making any designation of or reference to a particular territory or geographic area in this document, ADB does not intend to make any judgments as to the legal or other status of any territory or area.

This publication is available under the Creative Commons Attribution 3.0 IGO license (CC BY 3.0 IGO) <https://creativecommons.org/licenses/by/3.0/igo/>. By using the content of this publication, you agree to be bound by the terms of this license. For attribution, translations, adaptations, and permissions, please read the provisions and terms of use at <https://www.adb.org/terms-use#openaccess>.

This CC license does not apply to non-ADB copyright materials in this publication. If the material is attributed to another source, please contact the copyright owner or publisher of that source for permission to reproduce it. ADB cannot be held liable for any claims that arise as a result of your use of the material.

Please contact pubsmarketing@adb.org if you have questions or comments with respect to content, or if you wish to obtain copyright permission for your intended use that does not fall within these terms, or for permission to use the ADB logo.

Corrigenda to ADB publications may be found at <http://www.adb.org/publications/corrigenda>.

Note:

ADB recognizes “China” as the People’s Republic of China and “Vietnam” as Viet Nam.

ABSTRACT

We provide causal evidence from a developing country context that children exposed to lead at a young age exhibit worse cognitive outcomes. We exploit variation in exposure to lead-contaminated toxic sites in Indonesia and estimate a two-way fixed effects model using variation in age at first exposure and variation in distance to the site. We find that children who were exposed in utero or up to 6 years old scored -0.48 s.d. lower in a test of numeracy and -0.36 s.d. lower in a test of general cognitive ability if they lived within 3 kilometers of the toxic site compared with those who lived beyond 6 kilometers. Confirming that the impact of lead exposure depends on distance, we find that the impact is halved for children who lived 3–6 kilometers from the site and were exposed before 7 years old. The learning penalty in numeracy is equivalent to about 3 years of primary schooling in Indonesia for those who lived within 3 kilometers and 1.4 years for those who lived 3–6 kilometers from the toxic site. The effect size indicates the need to prevent lead exposure, especially at younger ages.

Keywords: pollution, hazardous waste, skills, human capital, learning

JEL codes: Q53, J24, I21

I. INTRODUCTION

According to the World Health Organization (WHO), even low levels of lead exposure are associated with adverse outcomes in cognitive development, behavior, and physical growth (UNICEF and Pure Earth 2020). The causal effects of lead exposure on cognitive skills have been established in developed countries. For example, the prohibition and removal of lead from paint in the United States led to higher standardized test scores among primary school students (Reyes 2015, Aizer et al. 2018, Sorenson et al. 2019), indicating that lead exposure had previously impaired cognitive outcomes. Declines in lead exposure owing to the phaseout of leaded gasoline in Sweden improved cognitive and non-cognitive skills as well as high school completion rates (Grönqvist, Nilsson, and Robling 2020). A growing body of literature based on developed countries has shown that children exposed to lead at a young age exhibit worse cognitive and behavioral outcomes later (Grönqvist, Nilsson, and Robling 2020; Persico, Figlio, and Roth 2020).

It is important to investigate the relationship between lead exposure and cognitive skills in developing countries, for two reasons. First, cognitive outcomes in developing countries lag far behind those in developed countries, as evidenced by internationally comparable standardized tests such as the Program for International Student Assessment (PISA) and Trends in International Mathematics and Science Study (TIMSS). A meta-analysis of 47 studies, 18 of which are observational studies from low- or middle-income countries, estimates that lead exposure accounts for over one-fifth of the gap in learning outcomes between rich and poor countries (Crawfurd et al. 2024).

Second, the main sources of lead exposure are different between developed and developing countries. In high-income countries, lead was most commonly found in paint, petroleum, and water pipes, where lead has been banned for decades now. By contrast, lead in developing countries can be found in a variety of other sources, including foodware, spices, toys, and hazardous waste sites (Ericson et al. 2021; UNICEF and Pure Earth 2020). These sources of lead exposure have only recently started coming to light (Sargsyan et al. 2024). Understanding the effects of lead exposure in developing countries entails investigating the effects of sources other than those found in developed countries.

This paper makes two main contributions to the literature. First, it provides evidence from a developing country context on the causal impact of lead exposure on cognitive skills. Previous studies investigating this causal link were all set in developed countries (Reyes 2015; Aizer et al. 2018; Sorenson et al. 2019; Grönqvist, Nilsson, and Robling 2020; Persico 2022). The meta-analysis by Crawfurd et al. (2024) includes 18 studies from developing countries, but all are observational. Our study is made possible by a near-universal mapping of toxic sites done by Pure Earth in Indonesia. Such data is rare in other developing countries. Second, it investigates the impact of a common source of lead exposure in developing countries, lead-contaminated toxic sites. A few studies have estimated the effects of toxic sites on cognitive outcomes, but those sites are not lead-specific and are located in developed countries (Rau, Urzúa, and Reyes et al. 2015; Persico, Figlio, and Roth 2020).

Lead-acid batteries, commonly used in cars and other automotive vehicles, account for 85% of lead consumed worldwide (WHO 2017). Recycling operations for used lead-acid batteries (ULABs), together with metal mining and processing and other hazardous waste sites, constitute lead toxic sites. Many countries lack adequate environmental controls and monitoring, resulting in informal, unregulated ULAB recycling activities. These practices contaminate their

surroundings by spilling lead dust and lead-containing electrolytes onto the ground and by emitting toxic fumes when smelting lead in open-air furnaces (UNICEF and Pure Earth 2020). Environmental contamination and human exposure can also occur from formal, registered recycling operations when controls are not adequate or when regulations are not sufficiently enforced (WHO 2017). Airborne lead concentrations around battery manufacturing and recycling plants in 37 developing countries were found to be seven times higher, on average, than the permissible exposure limit in the United States (Gottesfeld and Pokhrel 2011).

When these recycling operations are conducted near homes and schools, children and adults are exposed to harmful levels of lead. Indeed, workers and children who live near recycling sites, particularly in developing countries, have a blood lead level (BLL) exceeding the guideline level set by the United States Centers for Disease Control and Prevention (Falk 2003; Noguchi et al. 2013; van der Kuijp, Huang, and Cherry 2013; Sanders et al. 2014).¹ Children living near formal recycling plants in developing countries have a BLL about 13 times higher than those of children in the United States (Gottesfeld and Pokhrel 2011). Even after recycling operations have stopped, elevated lead levels continue to be observed in children who live near the sites (Daniell et al. 2015).

We estimate the impact of lead exposure on learning outcomes in Indonesia, where ULAB recycling began in the 1970s (Irawati et al. 2022). Around 8 million children (over 10% of all children) in the country are estimated to have elevated a BLL (Tang et al. 2022). Children living near ULAB sites across Indonesia are observed to have a BLL between 4.9 µg/dl (Prihartono et al. 2019) and 25.8 µg/dl (Gunawan and Masloman 2014)—well above the 3.5 µg/dl threshold identified by WHO.² Contextually, Indonesia has long struggled with low and stagnant levels of learning outcomes (Beatty et al. 2021; OECD 2023).

We combine the Indonesian Family Life Survey, a long-spanning household and individual longitudinal dataset, with a near-universal mapping of lead toxic sites in the country collected by Pure Earth. We exploit three types of variation in a two-way fixed effects (TWFE) model to identify a causal impact from exposure to toxic sites: variation in the starting year of these sites, variation in the distance of the household to the site, and variation in the age of first exposure.

We find large negative impacts on numeracy and general cognitive ability when the respondent was exposed in utero or during early childhood and lived less than 6 kilometers from the toxic site. Children who were exposed in utero or up to 6 years old scored -0.48 s.d. lower in a test of numeracy and -0.36 s.d. lower in a test of general cognitive ability if they lived within 3 kilometers of the toxic site compared with those who lived 6–10 kilometers away. Children who lived 3–6 kilometers from the site and were exposed before 7 years old scored -0.24 s.d. lower in numeracy and -0.22 s.d. lower in general cognitive ability. The learning penalty in numeracy is equivalent to

¹ Although there is no known safe level of lead exposure, a BLL above 3.5 µg/dl is considered dangerous (WHO 2021). A BLL of less than 5 µg/dl has been associated with lower intelligence quotient and academic achievement, and behavioral problems (Lanphear et al. 2005; Tang et al. 2022), while a BLL above 10 µg/dl is associated with physical damage such as anemia and stunting (Irawati et al. 2022; Tang et al. 2022). Above 105 µg/dl is deadly.

² Children 1–5 years old living around ULAB sites in Cinangka village, Bogor, where battery recycling started in 1978, had an average BLL of 17 µg/dl (Irawati et al. 2022). Children of the same age living in three neighborhoods in Greater Jakarta had an average BLL of 4.9 µg/dl (Prihartono et al. 2019). Children aged 7–13 in Tangerang, Bogor, Bekasi, and Depok had an average BLL of 11.9 µg/dl (Rachmat, Kusnopranto, and Supriyanto 2019). In Talawaan and Wenang in Sulawesi, children ages 6–8 had an average BLL of 25.8 and 11.4 µg/dl, respectively (Gunawan and Masloman 2014). A sample of adults living near ULAB sites in Pesarean village, Tegal, had a mean BLL of 39.3 µg/dl (Haryanto 2016).

about 3 years of primary schooling in Indonesia for those who lived within 3 kilometers and 1.4 years for those who lived 3–6 kilometers from the toxic site. These estimates are robust to various robustness checks.

The rest of the paper proceeds as follows. The next section discusses the data and Section III provides descriptive findings. Section IV presents the empirical methodology. Section V shows the results, and Section VI concludes.

II. DATA

A. Data

We combine two data sources. The first dataset, to measure lead exposure, comes from the Toxic Sites Identification Program (TSIP) of the nongovernment organization (NGO) Pure Earth.³ The data includes 150 lead-contaminated toxic sites, a near-universal mapping of lead toxic sites in the country. Nearly all are located on the islands of Java and Sumatra (only three are located on other islands). It contains information on the location of the site, lead measurements in either soil or water, a description of the environment around the toxic site, and a risk estimation of contamination by the investigator. Information on the starting year is available for a subset of these sites. Five sites have information on the closing year.

The second dataset is the Indonesian Family Life Survey (IFLS).⁴ The IFLS is a household longitudinal survey representative of about 83% of the Indonesian population. It has had five waves: 1993, 1997, 2000, 2007, and 2014. The IFLS randomly selects enumeration areas (EAs) in each province from a nationally representative sampling frame used in the 1993 SUSENAS, a socioeconomic survey designed by the Indonesian central bureau of statistics.⁵ Within each EA, households are randomly selected from the 1993 SUSENAS listings (Frankenberg and Karoly 1995). Between 1993 and 2014, the IFLS had a re-contact rate of 90.5%, including split-off households (Strauss, Witoelar, and Sikoki 2016a).

Our main outcome variables to measure cognitive skills are scores in a numeracy test and a Raven's Progressive Matrices test.⁶ The former reflects learning in school whereas the latter is a measure of general cognitive ability (Raven 2000). Thorsen, Gustafsson, and Cliffordson (2014) find that general cognitive ability plays an important role in how much an individual can learn in school. In our data, the correlation between the two variables is 0.3 (statistically significant).

The numeracy test was included in the last three rounds of IFLS. The 2000 IFLS tested all respondents between 7 and 24 years old, the 2007 IFLS tested all 7- to 24-year-old respondents and respondents who took the test in 2000, and the 2014 IFLS tested all respondents between 7 and 59 years old. With this setup, 19% of individuals took the identical test three times, 21.7% twice, and the rest once. The test consisted of five basic numeracy questions that the respondents are expected to learn to respond to in grades 1 to 5, or by the age of 10. There are two different versions of the test with one overlapping item. The more difficult version was asked to respondents 15 years or older at the time of the test. We apply a three-parameter logistic (3PL)

³ This data is publicly available at www.contaminatedsites.org/

⁴ This data is publicly available at www.rand.org/well-being/social-and-behavioral-policy/data/FLS/IFLS.html

⁵ The IFLS oversampled rural enumeration areas and enumeration areas in smaller provinces to facilitate urban–rural and Javanese–non-Javanese comparisons.

⁶ General cognitive ability consists of deductive ability—the ability to make meaning and handle complexity—and reproductive ability—the ability to absorb, recall, and reproduce information (Raven 2000).

item response theory (IRT) model to generate a single score for all respondents (see Beatty et al. 2021 for details on the method). A shortened version of Raven's Progressive Matrices was also included starting in the 2000 IFLS. The test comprised a series of images, and the respondent was asked to point to an image that follows logically from the series. We take the number of correct answers out of 12 and standardize it over the whole sample of test takers to have a mean of 0 and a standard deviation of 1.

To construct the treatment variable, the literature suggests that important determinants of risky lead exposure are lead levels in soil or water, the distance between the household and the polluted area, and age when exposed to lead. Clay et al. (2019) find larger negative effects on test scores and behavior from larger lead levels in the topsoil. The Environmental Protection Agency (EPA) developed an Integrated Exposure Uptake Biokinetic (IEUBK) model to predict BLL based on soil levels of lead (Caravanos et al. 2012), which suggests a concave relationship between soil lead and BLL. Studies that use the distance to a toxic site to measure the impacts of lead exposure find negative impacts for households within 3 kilometers of the site (Rau, Urzúa, and Reyes 2015; Persico, Figlio, and Roth 2020; Persico 2022). Rau, Urzúa, and Reyes (2015) find stronger effects within 1.5 kilometers and no statistically significant effects between 3 and 4.5 kilometers.

Young children, especially those aged 5 years or younger, are most susceptible to damage from lead exposure. First, during early childhood, the brain develops up to 90% of its adult volume by the age of 6 years (Brown and Jernigan 2012). In addition, the blood–brain barrier is not yet fully developed (WHO 2021). Second, young children are more likely to take in lead because they tend to put their hands on objects that may be contaminated and into their mouths. They also take in more per unit of body weight (ibid.). Most of the impact studies described previously focus their analysis on exposure either in utero or during early childhood, and therefore do not show differences in impacts by age of exposure. Rau, Urzúa, and Reyes (2015) find stronger effect for students who were 6 or 7 years old when first exposed than for those who were 9 years old.

We merge the IFLS and toxic site data based on household and toxic site locations. The IFLS provides the location of the center of the EA of the household, not of the household itself. However, the EA covers a small area and thus is a reasonable proxy for household location. On average, a village is divided into two EAs. An urban village has a mean radius of 1.4 kilometers (mean+1 s.d. radius is 3.3 km) and a rural village has a mean radius of 3.2 kilometers (mean+1 s.d. radius is 7.9 km). Most of our working sample are located in urban areas (Table 3). As studies previously mentioned find that toxic sites negatively affect children's cognitive development up to 3 kilometers, the center of the EA provides a precise enough approximation to identify households residing within a dangerous radius from a toxic site.

Ideally, we need to know the location of the household during the early childhood of the respondent. However, households may move. Since the IFLS traces household migration, we were able to accurately identify the age of first exposure.⁷ We include respondents who lived near the toxic site at any point between 1993 and 2014. The age of first exposure is determined by the start of the toxic site or the year in which the household moved toward the toxic site.⁸ Moving

⁷ We have to assume that respondents older than 28 in the 2014 IFLS did not move before 1993, because they were older than 6 in 1993. This is relevant for seven sites that started before 1993.

⁸ We do not know the exact year in which the household moved. We only know that the household moved in between two survey rounds. We assume that the household moved halfway between the two survey rounds.

away from a toxic site does not affect the age of first exposure to lead but affects the duration of exposure. The latter is defined as years between the age of first exposure and the age an individual migrated. For those who did not migrate, duration is the difference between age at the time of the survey and age of first exposure.

B. Site Selection

Our analysis depends on the distance between the sampled IFLS EAs and the toxic sites. We limit our analysis to sites where the lead concentration in the soil or water was above the maximum recommended level as determined according to WHO and the EPA.⁹ This is 10 parts per billion (ppb) for water and 400 parts per million (ppm) for soil.¹⁰ We select these sites to increase the precision of our estimates. We further exclude one site that started after 2014, because the respondents in the IFLS data had not been exposed to the site yet.

Table 1 shows the average number of IFLS respondents who lived around the toxic sites by different distance radiuses. Only 19 sites, nine of which have the information on starting date, have any IFLS respondents within a 1-kilometer radius. On average, eight respondents in all sites and seven respondents in those sites with information on starting date lived within this radius. At larger distances, more IFLS respondents are captured within the radius. Nearly all toxic sites have IFLS respondents within 10 kilometers.

We focus on toxic sites with any IFLS respondents living within a 3-kilometer radius, and we limit our analysis to respondents living within a radius of 10 kilometers around these sites. These sites have a substantial sample living within a dangerous distance as defined in the literature. We expect the IFLS test takers to have a similar socioeconomic background within this range, while ensuring a sufficient sample size for statistical power. In addition, we limit our analysis to toxic sites with a known starting date. As the previous subsection explained, age at the time of lead exposure is relevant for the impact on cognitive development, and we need the starting date to determine this. This strategy provides us with 14 sites. We relax some of these decisions and assumptions in the robustness check section.

⁹ Appendix Table A1 shows that the impact coefficients of sites with lead concentration below the maximum recommended level is smaller than our main findings, which is reasonable and lends credibility to our results.

¹⁰ See Pure Earth Maximum Recommended Levels at www.pureearth.org/wp-content/uploads/2021/03/Maximum-Recommended-Levels-Sheet1.pdf

Table 1: Average Number of Indonesian Family Life Survey Respondents by Radius Around Toxic Sites

	(1) All Sites Number of sites with any respondents	(2) Average number of respondents around each site	(3) Sites with Starting Date Number of sites with any respondents	(4) Average number of respondents around each site
< 1 km	19	8.26 (9.50)	9	6.56 (5.79)
< 3 km	59	66.42 (111.61)	14	92.57 (169.78)
< 6 km	82	179.24 (259.82)	18	178.11 (263.74)
< 10 km	89	430.52 (516.64)	21	382.00 (503.74)
All	93		23	

Note: The table includes only sites where test results were above the maximum recommended level in soil or water as determined by the World Health Organization and the Environmental Protection Agency. The number of respondents includes all respondents who lived within the radius at any point in time between 1993 and 2014. In total, Pure Earth tested 147 sites, of which 45 have a known starting date. Standard deviations are in parentheses.

Source: Authors' calculations.

C. Summary Statistics

Table 2 presents information on the toxic sites from TSIP. We construct the variable measuring the degree of contamination as the times that the test result is above the maximum recommended level. This is done to put the test results on the same metric, since tests were sometimes taken from water and at other times from soil. The TSIP investigators estimate the population at risk based on the number of people living near the site or passing the site on their way to work or school, and the type of contamination risk (i.e., dermal, ingestion, or inhalation). For instance, water contamination near a residential area carries a larger population at risk than is the case for soil contamination in an industrial area.

On average, sites with a known starting date had lead test results 92 times above the maximum recommended level. Given the mean test result of 38,765 ppm, the IEUBK model of the EPA (Caravanos et al. 2012) predicts BLLs of more than 63.4 µg/dl in 0–4-year-old children and of more than 50.1 µg/dl in 5–9-year-old children living with a distance of 0 kilometers from the toxic site. These are well above the 3.5 µg/dl threshold that is considered dangerous. The TSIP investigators estimate that, on average, 1,611 people were at risk of lead exposure around the toxic site, and they consider ingestion as the main risk of exposure. This could happen, for instance, if water is contaminated with lead or crops and livestock are grown on contaminated soil. Young children may also play on the soil. The average toxic site started in 1993. The oldest started in 1975 and the newest in 2012.

The table also shows that sites with a known starting date are similar to sites without a starting date. A smaller share of sites with a starting date are informal sites. It is likely more difficult to trace the starting date of these sites.

Table 2: Toxic Site Characteristics

	(1) All Sites	(2) Known Starting Date	(3) No Starting Date
Share informal	0.75 (0.44)	0.60 (0.52)	0.81 (0.40)
Soil test result (ppm) ¹	41,273.71 (110,854.65)	38,756.42 (114,519.55)	42,000.93 (111,087.20)
Times test above recommended level	101.91 (274.91)	91.97 (275.68)	105.00 (277.72)
Estimated population at risk	1,948.19 (3,848.72)	1,611.33 (3,420.74)	2,044.43 (3,995.74)
Main risk of exposure			
<i>Dermal</i>	0.02 (0.13)	0.07 (0.27)	0.00 (0.00)
<i>Ingestion</i>	0.85 (0.36)	0.93 (0.27)	0.82 (0.39)
<i>Inhalation</i>	0.13 (0.35)	0.00 (0.00)	0.18 (0.39)
Start year	1993 (10.35)	1993 (10.35)	
Observations	59	14	45

ppm = parts per million.

¹ One site had water tested instead of soil. The test result was 280 parts per billion.

Note: None of these differences is statistically significantly different from 0, possibly because of the small sample size. Mean start year is rounded to the nearest integer.

Source: Authors' calculations.

Table 3 presents the characteristics of respondents living within a 10-kilometer radius from the toxic sites with a known starting date. Close to 90% lived in urban areas. A wealth index based on household assets shows that our sample is somewhat wealthier than the average IFLS respondent, because it is standardized based on all IFLS respondents and larger than zero. The mean standardized test score is also larger than zero.

On average, the respondents lived 5.5 kilometers from the toxic site, and they were 17.8 years old when first exposed. Importantly for our analysis, as is explained later, a fair share of the sample was first exposed to a toxic site in utero (14%) or during early childhood (12%). The average respondent took the numeracy and Raven tests at 24 years of age.

A potential issue for our analysis is that 13% of respondents also have a site nearby without a known starting date. Therefore, we may have miscalculated their age of first exposure to a toxic site. In Section V.B, we show that our results are robust when we exclude these respondents from the analysis.

Because the IFLS tested the same respondents in multiple rounds if their age was within the target age range, we have more observations than respondents in the dataset. We treat each observation as independent in our main analysis, even though it may be the same person tested in different years. However, the results are robust when using only the 2014 IFLS or if we implement a two-way clustering of the standard errors (see Section V.B).

Table 3: Characteristics of Respondents Living Within 10 Kilometers of Toxic Sites with a Starting Date

	(1) Obs	(2) Mean	(3) Std. Dev.	(4) Min.	(5) Max.
Age at time of test (years)	6,206	24.02	12.70	7	59
Male	6,206	0.48	0.50	0	1
Years of school completed	6,195	9.67	4.37	1	21
<i>Completed primary education (share)</i>	<i>6,195</i>	<i>0.81</i>	<i>0.40</i>	<i>0</i>	<i>1</i>
Mother's education (years)	5,903	6.39	4.58	0	19
Father's education (years)	5,757	7.68	4.92	0	96
Standardized wealth index	6,200	0.33	0.97	-3.42	4.57
Lives in urban area (share)	6,206	0.89	0.32	0	1
Age at first exposure to nearest toxic site	6,206	17.74	13.30	-1.00	59.00
<i>In utero (share)</i>	<i>6,206</i>	<i>0.14</i>	<i>0.34</i>	<i>0</i>	<i>1</i>
<i>0–6 years old (share)</i>	<i>6,206</i>	<i>0.12</i>	<i>0.32</i>	<i>0</i>	<i>1</i>
<i>7–12 years old (share)</i>	<i>6,206</i>	<i>0.12</i>	<i>0.33</i>	<i>0</i>	<i>1</i>
<i>13–18 years old (share)</i>	<i>6,206</i>	<i>0.15</i>	<i>0.36</i>	<i>0</i>	<i>1</i>
<i>19–25 years old (share)</i>	<i>6,206</i>	<i>0.18</i>	<i>0.38</i>	<i>0</i>	<i>1</i>
<i>26 years or older (share)</i>	<i>6,206</i>	<i>0.30</i>	<i>0.46</i>	<i>0</i>	<i>1</i>
Distance to nearest toxic site (km)	6,206	5.50	2.52	0.17	9.98
<i>Toxic site within 3 km (share)</i>	<i>6,206</i>	<i>0.20</i>	<i>0.40</i>	<i>0</i>	<i>1</i>
Multiple treatment sites within 3 km (share)	6,206	0.02	0.15	0	1
Site without starting date within 3 km (share)	6,206	0.13	0.33	0	1
Duration of exposure (years)	6,206	11.02	7.74	0	36
Standardized numeracy score	6,206	0.33	1.04	-1.97	2.46
Standardized Raven test score	6,105	0.22	0.91	-2.17	2.14

Note: Age at first exposure of -1 means in utero.

Source: Authors' calculations.

III. DESCRIPTIVE FINDINGS

We first ascertain the characteristics of individuals living close to the toxic sites. Table 4 presents the correlation between distance to the toxic site and several characteristics, controlling for the IFLS wave and toxic site fixed effects to correct for time trends and time-invariant characteristics of the areas around the toxic sites. We find that poorer people lived closer to the toxic site, although the estimates are only weakly statistically significant. Although years of education and wealth can be an outcome of lead exposure, mother's and father's education are not, and show a similar pattern. The table shows that exposure is not random. In Section IV, we specify our strategy to deal with this issue.

Table 4: Correlations Between Respondent Characteristics and Distance to Toxic Site

	(1) Age at Test	(2) Urban	(3) Male	(4) Years of Education	(5) Mother's Education	(6) Father's Education	(7) Wealth Index
ln(Distance)	0.03 (0.27)	0.07 (0.06)	-0.00 (0.01)	0.48*** (0.18)	1.10*** (0.27)	1.30*** (0.32)	0.08* (0.04)
Observations	6,206	6,206	6,206	6,195	5,903	5,757	6,200

Note: Model includes site and Indonesian Family Life Survey wave fixed effects. Standard errors in parentheses and corrected for clustering at the enumeration area level. Significance level: * $p < .10$, ** $p < .05$, *** $p < .01$.

Source: Authors' calculations.

Table 5 shows the correlation between test scores and distance to the toxic site conditional on age, urban status, sex, and mother and father's education. We exclude household wealth from the list of independent variables because it could be affected by the toxic site as well. We also include fixed effects for IFLS waves to correct for the trend in test scores over time. This could be driven by, for example, improvement or deterioration in school quality. In our preferred model, as explained in the next section, we also include site fixed effects to control for site time-invariant unobserved characteristics. However, we cannot use site fixed effects when we split the sample by multiple age groups because we lose too much power as the sample is not evenly divided across sites. The table includes only test results from after the toxic site started.

We find a positive correlation between distance and test scores (columns 1–3, panel A for numeracy and panel B for Raven), indicating that, the further away the household from a toxic site, the higher the score. When we separate the sample by age at first exposure, however, we lose statistical significance, possibly because of the small size of the sample.¹¹

Table 5: Correlation Between Test Score and Distance to Toxic Site by Age of Exposure

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
				Age at First Exposure to Nearest Toxic Site					
	All			In utero	0–6 years	7–12 years	13–18 years	19–25 years	26+ years
<i>Panel A. Dependent variable: numeracy test score</i>									
ln(Distance)	0.05 (0.04)	0.13*** (0.05)	0.09** (0.04)	0.05 (0.08)	0.12 (0.08)	-0.10 (0.08)	-0.11 (0.08)	0.08 (0.07)	0.03 (0.05)
Constant	0.19*** (0.07)	-0.13 (0.15)	-0.35* (0.19)	0.16 (0.27)	-0.81*** (0.25)	0.11 (0.52)	0.82*** (0.23)	-0.15 (0.37)	0.52* (0.29)
Controls	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
IFLS wave FE	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Site FE	No	Yes	Yes	No	No	No	No	No	No
Observations	4,513	4,513	4,513	845	715	646	649	609	1,049

Continued on the next page

¹¹ Appendix Table A2 shows correlations for all sites, but we do not know if the sites started before or after the test. Because we find that age of first exposure is crucial for negative impacts, this may be why we do not find a correlation between test scores and distance to those sites.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
				Age at First Exposure to Nearest Toxic Site					
	All			In utero	0–6 years	7–12 years	13–18 years	19–25 years	26+ years
<i>Panel B. Dependent variable: Raven test score</i>									
ln(Distance)	0.07 (0.04)	0.10*** (0.03)	0.07** (0.03)	0.11 (0.08)	0.07 (0.07)	-0.09* (0.05)	0.05 (0.06)	0.03 (0.03)	-0.00 (0.05)
Constant	-0.03 (0.08)	0.48*** (0.11)	0.24** (0.11)	0.26 (0.23)	0.25 (0.24)	0.15 (0.30)	1.19*** (0.25)	-0.77*** (0.15)	0.01 (0.18)
Controls	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
IFLS wave FE	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Site FE	No	Yes	Yes	No	No	No	No	No	No
Observations	4,639	4,639	4,639	845	713	642	647	604	1,188

FE = fixed effects; IFLS = Indonesian Family Life Survey.

Note: The dependent variable is standardized numeracy (panel A) and Raven score (panel B). The regression includes only observations after being exposed to a toxic site. Controls are age, urban status, sex, and mother and father's education, and interactions with indicators for missing values. Standard errors in parentheses and corrected for clustering at the enumeration area level. Significance level: * $p < .10$, ** $p < .05$, *** $p < .01$.

Source: Authors' calculations.

IV. EMPIRICAL STRATEGY

A. Model and Identification Strategy

Residential location and toxic sites are not random. The potential endogeneity of distance—for instance less educated people generally live closer to toxic sites—means the estimates in Section III cannot be interpreted causally. To disentangle the impact of the toxic sites from other family characteristics, we compare respondents who were exposed during early childhood to cohorts that were exposed only later in life, while also exploiting variation in the year the toxic site started.

We consider two dimensions that determine the treatment intensity: age at first exposure and distance between residence, proxied by the centroid of the EA, and the toxic site. Our model exploits three sources of variation: distance to the toxic site, age of first exposure, and start year of the toxic site. We consider respondents to be exposed to the toxic site if the site was active while the individual was in utero or between ages 0 and 6, and compare them to respondents not exposed to a toxic site before the age of 7.¹² We choose this threshold because studies have shown that lead mainly affects the cognitive development of young children. Also, primary school in Indonesia starts at age 7, so we are measuring the impacts of lead exposure before reaching school-going age.

We estimate the following model for respondent i living near site s , tested in IFLS wave t at age a :

$$Y_{ist} = \beta_1(T_{ist} \times \ln(D_{is})) + \beta_2 T_{ist} + \beta_3 \ln(D_{is}) + \delta_1 X_{ist} + \theta_s + \alpha_a + \gamma_t + \varepsilon_{ist} \quad (1)$$

¹² Note that all the treated cohorts were already exposed at the time of the test, but some of the respondents in the comparison cohort in the 2000 and 2007 IFLS waves had not yet been exposed to a site that started after their test. In the 2014 IFLS wave, all respondents had been exposed to a toxic site.

where Y_{ist} is the test scores, T_{ist} indicates the treated cohort, first exposed during early childhood (in utero or up to 6 years old), D_{is} is the distance in kilometers between the household's EA and the toxic site, and X_{ist} is a vector of control variables. Note that the starting point of our dataset is the toxic site. We assign a time-invariant value of D_{is} based on the earliest IFLS wave in which an individual appears within 10 kilometers from a site. For the sample that eventually moved away from the original site, we assign the original distance.

Controls include urban status, sex, and mother and father's education. As in a standard TWFE effects model, we include site fixed effects, θ_s , to correct for any site-specific time-invariant characteristics, and age-at-test fixed effects, α_a , to correct for birth year cohort trends in test scores.¹³ We also include fixed effects for the IFLS wave, γ_t , to correct for time trends in the test scores across respondents of the same age, as Indonesia experienced a negative trend in the IFLS numeracy score between 2000 and 2014 (Beatty et al. 2021). We prefer to cluster the standard errors at the EA level, but as robustness also show the results using a two-way clustering at the EA and individual levels.

Our coefficients of interest are β_1 and β_2 . While β_2 measures the difference in test scores between the treated and comparison groups living within 1 kilometer from the toxic site, β_1 measures how this difference changes for households that live further away from the toxic site. We expect β_2 to be negative and β_1 to be positive, reflecting a negative impact on test scores when the individual is exposed during early childhood and living close to the toxic site that diminishes as households live further from the site.

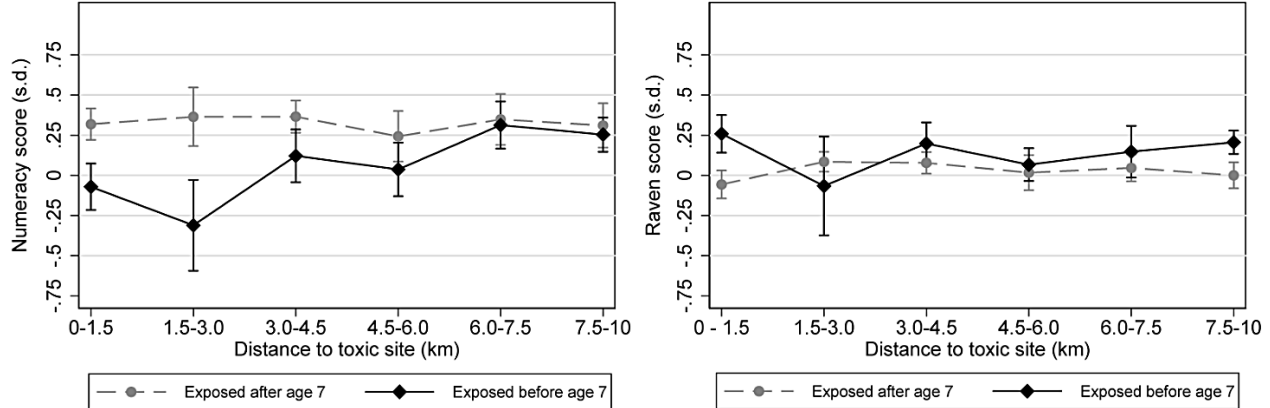
Our model is similar to a staggered Differences-in-Differences (DID) model where all groups are eventually exposed to toxic sites, as there are no “never-opened” sites, but with an additional interaction term with the indicator for the treated group. The staggered start of toxic sites means the birth year range of the treated cohort differs by site. The model compares the difference in test scores between the treated and comparison cohorts in the already-opened toxic sites to the difference between the same birth year cohorts in all not-yet-opened sites.

Our model relies on two identification assumptions. First, the treated and comparison cohorts would have had a similar correlation between test scores and distance without lead exposure. We perform several checks to confirm that this assumption holds. Figure 1 shows that the correlation between numeracy test scores and distance was similar between the groups for distances further than 3 kilometers.¹⁴ This is a version of a parallel pre-trend test but using distance instead of time. Moreover, Appendix Table A3 shows that the correlation between distance and characteristics is not significantly different between the groups for urban, father's education, years of schooling, and wealth. However, we find that the correlation between distance and mother's education is weaker for the younger cohort. In addition, we find a weak correlation difference for the sex of the respondent, but we show in Section V.C that the impacts are similar for boys and girls.

¹³ When only using one IFLS wave, fixed effects for age at the time of the test are the same as birth year fixed effects. Given the correlation between age and test scores, we chose to use the age-at-test fixed effects.

¹⁴ The model we estimate is similar to equation (1) but the distance variable is replaced with indicators for distance group relative to the nearest group (7.5–10 kilometers). Also, we did not perform a TWFE correction for the figure.

Figure 1: Test Scores by Distance to Toxic Site and Age of Exposure



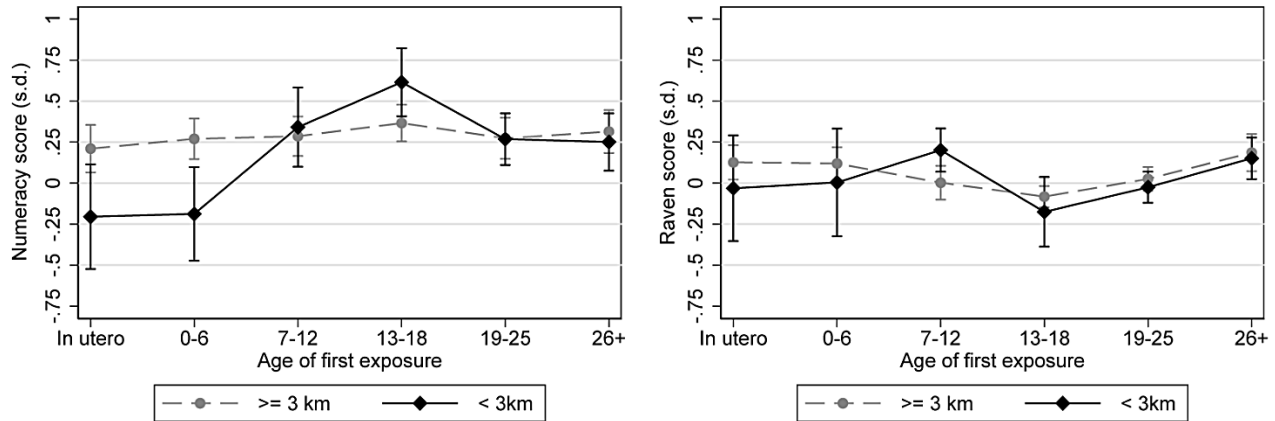
Note: The dependent variable is standardized test scores (left side) and Raven score (right side). Standard errors corrected for clustering at the enumeration area level. Estimates are conditional on control variables for sex, urban status, and parent's education, and year fixed effects. Figures include only tests that were taken after the toxic site started.

Source: Authors' calculations.

The second identification assumption is that, without any lead exposure, the test score trends across birth cohorts would have been the same across toxic sites. Generally, this assumption is tested by specifying the model in terms of an event study, with indicators for each relative year to the start of the treatment, which would be the start year of the toxic site minus 6 years to cover all respondents who were first exposed during early childhood. This would show us how the effect evolves with age at first exposure. However, we have too few observations in each birth year cohort for this analysis to be valid, because we would need respondents of the same age at different distances from the toxic site. Therefore, we group birth year cohort into age groups as used before in Table 5: exposed in utero, age 0–6 (early childhood), age 7–12 (primary school), age 13–18 (secondary school), age 19–25, and age 26 and older, when the brain is fully developed. Figure 2 shows that the four age groups exposed after age 7 show no differences in test score trends, suggesting this trend plausibly holds for the treated cohort had there been no exposure to lead.¹⁵

¹⁵ The model we estimate is similar to equation (1) but the treated cohort indicator T is replaced with indicators for each age group relative to the youngest birth cohort group before treatment (7–9 years old at first exposure).

Figure 2: Test Scores by Age of Exposure and Distance to Toxic Site



Note: The dependent variable is standardized test scores (left side) and Raven score (right side). Standard errors corrected for clustering at the enumeration area level. Estimates are conditional on control variables for sex, urban status, and parent's education, and year fixed effects. Figures include only tests that were taken after the toxic site started.

Source: Authors' calculations.

We also assume no endogenous migration—that is, parents might move when a toxic site starts near their house, or move closer to a toxic site. For instance, our estimates will be biased if wealthy parents are more likely to move when their children are exposed at a young age, or if poorer parents move near a toxic site if the site provides jobs. This assumption is tested in Appendix Table A4, where we find that lead exposure does not influence the decision to move away from a toxic site and the presence of a toxic site does not attract immigrants. These findings are independent of parental education, a proxy for wealth that is exogenous to lead exposure.

We find that 367 respondents moved away after the start of a toxic site while 4,314 respondents moved toward a toxic site. To check for robustness, we estimate equation (1) on the sample that never moved in Section V.B.

Finally, in utero lead exposure could also cause low birthweight, miscarriage, stillbirth, and premature death (WHO, 2021; Tanaka, Teshima, and Verhoogen 2022; Clay, Hollingsworth, and Severnini 2023), in which case we would underestimate the impacts of lead exposure on academic achievement. In Appendix Table A5, we use IFLS data at the mother-pregnancy level to study whether the toxic sites also affected pregnancy outcomes, exploiting variation in exposure to toxic sites across pregnancies of the same women. We find no such effects, perhaps because such outcomes are associated with much higher levels of BLL (Tang et al. 2022). Therefore, miscarriage, stillbirth, and premature death do not significantly affect our analysis.

B. Correction for Two-Way Fixed Effect Identification Problems

Our main identification model is a type of repeated cross-section two-way fixed effect (TWFE) model. Recent literature, starting with Goodman-Bacon (2021), points out issues with these types of models. The estimator is a weighted average of the treatment effects at different sites. This strain of literature finds that, if treatment effects are not homogeneous across sites and over time (i.e., the same for different durations of treatment), some weights may be negative, and the estimator would be biased (de Chaisemartin and D'Haultfoeuille 2022). The reason for this is that the sites that remain treated are also used as controls for new sites that started. We cannot

assume homogeneous treatment effects because treatment sites have different levels of lead and duration of exposure may play a role in the impacts on test scores.¹⁶

To deal with this issue, we use the two-stage correction suggested by Gardner et al. (2024).¹⁷ The correction is done by identifying the correct group and time fixed effects in a first stage using only the comparison cohorts. In the second stage, the average treatment effects are identified by comparing the outcomes of the treated and comparison cohorts after removing these fixed effects. This method follows the original idea of a TWFE model: measuring the difference between a staggered treated and a not-yet-treated or control group corrected for time-invariant differences between the groups and for trends in the outcome variable over time. By estimating these fixed effects only on the not-yet-treated observations, they are not contaminated by heterogeneity in treatment effects across groups or over time.

We prefer this solution because it can be applied to our model, which exploits three sources of variation. Another solution to the TWFE problem is to estimate the effects for each site separately using not-yet-treated as control and to take the average effects (de Chaisemartin and D'Haultfoeuille 2022). However, this estimation focuses only on the switchers around the start of the toxic site. We do not have enough data to generate sufficient statistical power to use these corrections. Moreover, the software developed for these solutions does not allow for an additional interaction term with the treatment indicator.

C. Additional Sites

Our main sample consists of IFLS respondents living around 14 toxic sites with starting dates. To add more sites, we estimate the starting year of the other sites using Google Earth images. This strategy has previously been used to identify village buildings (Guo et al. 2016), urban agriculture spaces (Taylor and Lovell 2012), or map other land uses (Zhao et al. 2021). Using the geographical coordinates of the toxic sites provided by Pure Earth, all available Google Earth images were manually examined. The earliest available and clear images exhibiting a building structure are used to specify the month and year of the post-start date. And then, the earliest images exhibiting the absence of previously found structure are used to indicate the month and year of the pre-start date. Sites for which no Google Earth image is available for the latter are tagged as not having a pre-start date.

Of the 107 sites without a starting date, only 24 could be tagged as having both pre-start and post-start dates and 48 as having only post-start dates; 34 could not be tagged with pre- or post-start dates. When we verify this method using the sites with start dates provided by Pure Earth, the correlation appears to be low. Therefore, we use the additional sites only as a robustness check and retain the original 14 toxic sites as our main sample. After keeping the sites whose lead content is above the WHO requirements and that have a sufficient IFLS sample, we were able to add 11 sites. The results are in Section V.B.

¹⁶ The latter may be less of an issue, as we do not find significantly different impact estimates between the whole sample and the sample that did not migrate. Further discussion is in Section V.

¹⁷ Borusyak, Jaravel, and Speiss (2022) and Liu, Wang, and Xu (2022) suggest the same correction.

V. RESULTS

A. Main Results

In our main results, we include only toxic sites for which the start year is known and whose lead content is higher than the WHO limit. Table 6 presents the estimates where we apply the two-stage TWFE correction.

We find a consistent negative impact on test scores associated with living near a toxic site for respondents who were first exposed during their early childhood. Column 1 shows that the impact of early exposure for those who lived within 1 kilometer of the toxic site is -0.35 s.d. lower numeracy score. This negative effect becomes smaller the further away from the toxic site the household is, supporting the descriptive findings in Section III and the literature.

In column 2, we estimate the model with indicators for different distance ranges to the toxic site instead of a continuous variable, for interpretation purposes. We find that, compared with those living 6–10 kilometers away from the toxic site, respondents scored -0.48 s.d. lower if they lived within 3 kilometers of the site and -0.24 s.d. lower if they lived 3–6 kilometers from the toxic site and were exposed in utero or during early childhood. Comparing these estimates with existing studies mentioned in the Introduction is difficult, given the differences in the treatment variable. To put the impact into perspective, Beatty et al. (2021) show that Indonesian children gain 1 s.d. in this numeracy test in 6 years of primary school. Therefore, the impact is equivalent to 1.4 to 2.9 years of schooling. The correlation between distance and test scores does not exist for respondents exposed to toxic sites after age 7, which is shown by the coefficients close to zero for the distance indicators.

Column 3 includes multiple birth cohort groups to test for parallel trends across older cohorts and to specifically estimate the impact of exposure in utero versus exposure at 0–6 years of age. We find large negative effects of exposure in utero or up to 6 years old relative to being exposed at 7–12 years, significant at 10%, which decline by distance. The impact on those exposed in utero is significantly larger than for those exposed at 0–6 years old. Column 3 also confirms that there were similar trends in test scores for those exposed at 7 years old or later. Therefore, we argue that these cohorts provide a correct comparison group for our identification. As discussed in the previous section, we prefer to combine these age cohorts into one treated and one comparison group, as shown in column 1. This increases our power, which we need when estimating heterogeneous effects further below.

On the Raven test results (columns 4–6), we generally find similar impacts. The only difference is that exposure in utero appears to have the most negative impact, while the effects of being exposed in other age groups appear to be much smaller (column 6). This corroborates studies in the literature that show that exposure to stress and adverse conditions in utero significantly reduce general cognitive ability (e.g., Beshir and Maystadt 2020; Chang, Favara, and Novella 2022).

Table 6: Impact of Lead Exposure on Numeracy and General Cognitive Skills

	(1)	(2)	(3)	(4)	(5)	(6)
	Numeracy Test			Raven Test		
Treated	-0.35** (0.15)	0.25** (0.11)		-0.31** (0.14)	0.15*** (0.06)	
ln(Distance)	0.00 (0.00)		-0.03* (0.02)	0.00 (0.00)		-0.02 (0.01)
Treated*ln(Distance)	0.28*** (0.10)			0.21*** (0.08)		
Within 3 km		-0.04 (0.03)			-0.03 (0.02)	
Within 3 to 6 km		-0.01 (0.02)			-0.01 (0.02)	
Treated*Within 3 km		-0.48*** (0.15)			-0.36*** (0.13)	
Treated*Within 3 to 6 km		-0.24* (0.12)			-0.22*** (0.07)	
Exposed in utero			-0.41* (0.24)			-0.49** (0.19)
Exposed at 0–6 y.o.			-0.30* (0.16)			-0.21 (0.15)
Exposed at 13–18 y.o.			0.03 (0.08)			-0.10 (0.07)
Exposed at 19–25 y.o.			-0.00 (0.06)			-0.00 (0.05)
Exposed at 26 and older			-0.05 (0.06)			-0.04 (0.03)
Exposed in utero*ln(Distance)			0.37** (0.16)			0.34*** (0.11)
Exposed at 0–6 y.o.*ln(Distance)			0.25*** (0.09)			0.15** (0.07)
Exposed at 13–18 y.o.*ln(Distance)			0.01 (0.05)			0.08* (0.04)
Exposed at 19–25 y.o.*ln(Distance)			0.02 (0.05)			0.02 (0.03)
Exposed at 26 and older*ln(Distance)			0.08* (0.05)			0.05* (0.03)
Observations	6,206	6,206	6,206	6,331	6,331	6,331

Note: The dependent variable is standardized numeracy score for columns 1–3 and standardized Raven score for columns 4–6. The omitted category in columns 2 and 5 is “within 6 to 10 km” and in columns 3 and 6 it is “exposed at between 7 and 12 y.o.” All models include survey year-round, toxic site, and age fixed effects, and control variables for urban status, sex, and parent’s education. All models are estimated using a correction of a repeated cross-section two-way fixed effect model. Standard errors in parentheses and corrected for clustering at the enumeration area level. Significance level: * $p < .10$, ** $p < .05$, *** $p < .01$.

Source: Authors’ calculations.

B. Robustness Checks

We implement several robustness checks to see if some stylized facts potentially bias our results. We also check for robustness by relaxing some assumptions and changing sampling decisions. All these results are in Table 7, with panel A (B) showing the results for numeracy (Raven).

First, as shown earlier, in Table 3, 13% of respondents also had a toxic site within 3 kilometers without a known starting date. Therefore, the age of exposure may be incorrect for this group. In column 1 of Table 7, we show that our impact estimates are virtually identical when we exclude these respondents.

Second, by definition, the comparison cohort is older than the treated cohort at the time of the test. Age at the time of the test is correlated with the test score, which increases during school-going age and decreases again after the respondent is above 18 years old (Beatty et al. 2021). We have already shown, in Table 4, that age at the time of the test is not correlated with distance. We perform an additional test to show that the difference in age at the time of the test does not confound our analysis. Because respondents were tested in multiple rounds, we can take an earlier test result for the comparison cohort than for the treated cohort. When we match the treated and comparison cohorts on age at the test, we find similar signs, as shown in column 2, but with a much larger estimate of early exposure. Appendix Table A6 shows that the match reduced the age difference between exposed and comparison cohorts.

Table 7: Robustness Checks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Excluding Observations with Multiple Sites Nearby	Treated and Comparison Matched on Age at Test	Stayers Based on Individual Migration History	Stayers Based on Household Location Between IFLS Rounds	Including Additional Sites with Starting Date	Control for Wind Direction	Including District Fixed Effects	Multiway Clustering of Enumeration Area and Individual
<i>Panel A. Dependent variable: numeracy test score</i>								
Treated	-0.35** (0.17)	-0.60*** (0.19)	-0.32 (0.22)	-0.47* (0.28)	-0.20* (0.12)	-0.38** (0.16)	-0.25 (0.15)	-0.35* (0.19)
ln(Distance)	0.00 (0.00)	-0.00 (0.01)	-0.01 (0.01)	0.01 (0.01)	0.00 (0.00)	0.01 (0.00)	-0.00 (0.00)	0.00 (0.00)
Treated*ln(Distance)	0.29** (0.12)	0.33*** (0.11)	0.40*** (0.13)	0.40** (0.17)	0.17** (0.08)	0.29** (0.11)	0.22** (0.11)	0.28** (0.12)
Observations	5,430	2,045	1,543	2,797	7,046	5,758	6,206	6,206
Mean dep. var.	0.36	0.34	0.40	0.29	0.30	0.33	0.33	0.34
<i>Panel B. Dependent variable: Raven test score</i>								
Treated	-0.31** (0.15)	-0.15 (0.17)	-0.25 (0.21)	-0.51* (0.29)	-0.34*** (0.12)	-0.28* (0.15)	-0.24* (0.14)	-0.31* (0.18)
ln(Distance)	0.00 (0.00)	0.00 (0.00)	0.01 (0.01)	0.01* (0.01)	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)
Treated*ln(Distance)	0.22** (0.08)	0.13 (0.09)	0.22* (0.12)	0.34** (0.17)	0.16** (0.07)	0.20** (0.08)	0.17** (0.07)	0.21** (0.10)
Observations	5,534	2,044	1,558	2,905	8,400	5,884	6,331	6,331
Mean dep. var.	0.20	0.39	0.14	0.09	0.16	0.18	0.18	0.18

IFLS = Indonesian Family Life Survey.

Notes: The dependent variable in panel A (B) is standardized numeracy score (Raven score). Column 1 excludes individuals without a starting date within 3 kilometers. Column 2 matches the exposed test takers and control individuals based on their age at the time of the test across test rounds. Column 3 limits the sample to non-movers after they are exposed to the toxic site, according to the IFLS migration module. Column 4 is similar to Column 3, but migration is measured based on household location between IFLS waves, identifying movers by checking for changes in residence. Column 5 includes additional exposed individuals based on expanded toxic site data and starting dates from Google Earth. Column 6 includes wind direction as a covariate. In Columns 7 and 8, we apply district fixed effects and two-way clustering of standard errors at the individual and enumeration area levels, respectively. All models are estimated using a correction of a repeated cross-section two-way fixed effect model. Standard errors in parentheses and corrected for clustering at the enumeration area level, except for column 8. Significance level: * $p < .10$, ** $p < .05$, *** $p < .01$.

Source: Authors' calculations.

Third, implicit in our main analysis is the assumption that an individual remains exposed to the toxic site once he or she is treated, which does not hold true for those who moved away from a toxic site. Similarly, we set distance to toxic site the first time a sample shows up within 10 kilometers of a toxic site, and do not change the distance if the sample migrates. In this sense, our main impact estimates can be considered as intent-to-treat effects (ITT). To check if the results are robust to this decision, we re-estimate the model only on the sample of non-movers. Column 3 shows the results of this robustness check among stayers based on individual migration history in the IFLS migration module, while column 4 tests for robustness if migration is measured based on household location between IFLS waves. Both tests confirm robustness of our main impact estimates.¹⁸

Fourth, as we discuss in Section 4.3, we add more sites by inferring their possible start date using Google Earth. In column 5, we estimate equation (1) by adding the sample from these additional sites to the original sample. The point estimates are smaller, but the differences are not statistically significant.

Our fifth robustness check is by controlling for the location of the sample's house relative to the toxic site. The main motivation is to differentiate between living upwind and downwind from the toxic site, which would affect how much of the airborne lead is inhaled. Although column 2 of Table 2 indicates that none of the toxic sites we use carry a risk exposure through inhalation, for completeness we still control for the location, following Rau, Urzúa, and Reyes (2015). As shown in column 6 of Table 7, the results are nearly identical to our main results.

Sixth, we replace the site fixed effects with district fixed effects to control for unobserved characteristics of the larger geographical area around the sites. This is a less restrictive set of fixed effects. Finally, we allow for a two-way clustering of the standard errors to take into account the fact that a subsample consists of the same individuals taking the test multiple times. Columns 7 and 8 show that our results are robust to these aspects, although the standard errors of the estimates are larger.

C. Heterogeneity Analysis

Table 8 presents gender heterogeneity in our impact findings. The literature shows that males are affected more (Clay, Portnykh, and Severnini 2019; Grönqvist, Nilsson, and Robling 2022). In our context, we do not find any differences by sex, as the coefficients are not statistically different between groups. Therefore, lead exposure affects both sexes equally negatively.

Table 9 presents impact heterogeneity by duration of exposure. We test whether a longer duration of lead exposure has stronger effects by splitting duration of exposure into three categories: bottom 30% (0–4 years of exposure), middle 40% (5–14 years), and top 30% (15–36 years). Although the point estimates appear to be different in magnitude and are statistically significant, we do not find statistically significant differences in the effects of exposure by duration. Thus, age at first exposure matters more than duration of exposure.

¹⁸ Consistency in the magnitude of effect sizes estimated in columns 3 and 4 support the consistency of migration measures based on household location and individual migration history. Household location between IFLS waves is tracked down to the EA level, whereas individual location based on the migration history module is tracked down to subdistrict level only. The average radius of subdistricts is 10 kilometers, not precise enough for measuring distance to toxic sites.

Table 8: Impact Heterogeneity by Sex

	(1) Male	(2) Female	(3) Male	(4) Female
	Numeracy Test		Raven Test	
Treated	-0.27 (0.19)	-0.48*** (0.16)	-0.35* (0.19)	-0.29** (0.15)
ln(Distance)	0.01 (0.00)	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)
Treated*ln(Distance)	0.22* (0.12)	0.34*** (0.10)	0.29*** (0.11)	0.14* (0.07)
Observations	2,998	3,208	3,060	3,271
Mean dep. var.	0.29	0.38	0.24	0.11
<i>p-value (male – female)</i>	0.42		0.26	

Notes: The dependent variable in columns 1–2 (3–4) is standardized numeracy score (Raven score). All models are estimated using a correction of a repeated cross-section two-way fixed effect model. Standard errors in parentheses and corrected for clustering at the enumeration area level. Significance level: * $p < .10$, ** $p < .05$, *** $p < .01$.

Source: Authors' calculations.

Table 9: Impact Heterogeneity by Duration of Exposure

	(1) Bottom 30% (0–4 years)	(2) Mid 40% (5–14 years)	(3) Top 30% (15–36 years)	(4) Bottom 30% (0–4 years)	(5) Mid 40% (5–14 years)	(6) Top 30% (15–36 years)
	Numeracy Test			Raven Test		
Treated	-0.86*** (0.21)	-0.50*** (0.19)	-0.25 (0.60)	1.37*** (0.28)	-0.71*** (0.17)	-0.50 (0.32)
ln(Distance)	0.02** (0.01)	0.00 (0.01)	0.00 (0.00)	-0.00 (0.00)	0.01** (0.01)	0.00 (0.00)
Treated*ln(Distance)	0.11 (0.13)	0.29*** (0.09)	0.46* (0.27)	0.13 (0.16)	0.10 (0.07)	0.47*** (0.16)
Observations	1,366	1,993	1,042	1,392	2,052	1,083
Mean dep. var.	0.26	0.29	0.24	0.08	0.22	-0.20
<i>p-value (difference relative to reference category)</i>	ref 0.232 0.114	0.416 ref 0.149	0.281 0.440 ref	ref 0.636 0.414	0.981 ref 0.375	0.426 0.711 ref

ref = reference category.

The dependent variable in columns 1–3 (4–6) is standardized numeracy score (Raven score). All models are estimated using a correction of a repeated cross-section two-way fixed effect model. Standard errors in parentheses and corrected for clustering at the enumeration area level. Significance level: * $p < .10$, ** $p < .05$, *** $p < .01$.

Source: Authors' calculations.

D. Panel Individual Analysis on Numeracy

As the IFLS is a longitudinal survey, we can estimate impacts on test score gains over time for the same individuals. We identify individuals who took the numeracy test at between 7 and 12 years old, and retok it 7 years later. This could be between the 2000 and 2007 rounds, or between the 2007 and 2014 rounds. We select this age group because respondents should still be in primary school during the first survey, and they are supposed to learn the contents of the numeracy test during that time. Meanwhile, 7 years later, when they are between 14 and 19 years old, they are expected to know the answers to the numeracy questions. We find 680 respondents who meet the inclusion criteria for this analysis.

We estimate a triple differences model using a similar treatment definition as in the previous analyses. The first difference is between respondents exposed in utero or during early childhood and those exposed at age 7 or older. The second difference is between respondents living within 6 kilometers of the toxic site and those living between 6 and 10 kilometers from the site. This threshold is based on our previous findings (Figures 1 and 2; Table 6) and the need for a sufficient sample size to have statistical power. We use a binary indicator instead of the continuous distance variable because of the smaller sample size. The third difference is over time. Appendix Table A7 shows how the panel sample is divided across these groups. The treatment group of interest lives within 6 kilometers from a toxic site and was exposed during early childhood, consisting of 210 respondents.

We estimate the following model for individual i who first took the numeracy test in IFLS wave t :

$$Y_{ist} = \beta_1(T_{ist} \times C_{ist} \times P_{t+7}) + \beta_2(T_{ist} \times P_{t+7}) + \beta_3(C_{ist} \times P_{t+7}) + \beta_4(T_{ist} \times C_{ist}) + \beta_5 T_{ist} + \beta_6 C_{ist} + \beta_7 P_{t+7} + \delta_1 X_{ist} + \gamma_w + \varepsilon_{ist} \quad (2)$$

where Y_{ist} is the numeracy score, T_{ist} is an indicator for those exposed during early childhood, C_{ist} is an indicator for living within 6 kilometers of the toxic site in year t (2000 or 2007), P_{t+7} indicates the survey round 7 years later, X_{ist} is a vector of control variables for urban status, sex and parent's education, γ_w are survey round fixed effects to correct for a time trend, and ε_i is the error term. Note that we do not include toxic site fixed effects. We also did not put age-at-test fixed effects, owing to the small sample size. Because we study test score gains of the same respondents, these do not affect the results. We do not estimate this model using Raven test scores, as we selected the sample to see progression through primary school.

The findings are presented in Table 10. Columns 1 and 2 show the double difference estimates. Column 1 includes only respondents who were exposed early, and column 2 includes only respondents who lived within 6 kilometers of the toxic site. We find a consistent negative effect on test scores gains between primary school age and secondary school age. In the double difference, students gained about a full standard deviation on the test during these 7 years, and the treated respondents scored -0.26 to -0.27 s.d. lower (columns 1 and 2, respectively).

For a triple difference analysis, only one parallel trend assumption needs to hold. Column 3 shows that the four groups classified in Appendix Table A7 had similar test scores in the baseline year, suggesting that the respondents exposed to a toxic site during early childhood had not fallen behind yet early in primary school. However, it is more likely that all respondents scored low on

the test at that stage. The similarity in baseline scores suggests that we could also expect similar trends in scores over time. In the triple difference results (column 3), we find that the treated respondents barely learned any of the skills tested, as the average gain in test scores was 0.68 s.d. while the treatment effect is -0.61 s.d.¹⁹

Although we do not aim to exploit the staggered start of toxic sites in this analysis, this model does do that. However, it should not be an issue because we use only two time periods for each panel group. Nevertheless, we also show the model estimated on each panel group separately to confirm that results are fairly homogeneous in columns 4 and 5, although we lose precision owing to the smaller sample size.

Table 10: Impacts of Early Lead Exposure on Numeracy Test Score Gains, Individual Panel Data

	(1) All Panel Sample	(2) All Panel Sample	(3) All Panel Sample	(4) 2007– 2014	(5) 2000– 2007
Within 6 km	-0.02 (0.10)		-0.12 (0.09)	-0.05 (0.13)	-0.18 (0.13)
7 years later	1.02*** (0.18)	0.99*** (0.12)	0.68*** (0.13)	0.30 (0.24)	0.26* (0.14)
Within 6 km * 7 years later	-0.26* (0.15)		0.36** (0.15)	0.11 (0.28)	0.50*** (0.17)
Treated		0.07 (0.09)	0.05 (0.11)	-0.01 (0.14)	0.18 (0.15)
Treated * 7 years later		-0.27* (0.14)	0.25 (0.18)	0.20 (0.28)	0.08 (0.31)
Treated * Within 6 km			0.07 (0.14)	0.08 (0.18)	0.04 (0.19)
Treated * Within 6 km * 7 years later			-0.61*** (0.22)	-0.58 (0.36)	-0.19 (0.36)
Observations	544	716	1,360	682	678
Mean dep. var. baseline	0.21	0.17	0.23	0.14	0.32

Note: The dependent variable is standardized numeracy score. Standard errors in parentheses and corrected for clustering at the enumeration area level. All models include year fixed effects and control variables for urban status, sex, and parent's education. Column 1 includes only respondents who were exposed early, and column 2 only respondents who lived within 6 kilometers of the toxic site. Significance: * $p < .10$, ** $p < .05$, *** $p < .01$.

Source: Authors' calculations.

¹⁹ For completeness, we show the estimation results for the Raven test score in Appendix Table A8. Since general cognitive ability is formed early in life, we do not expect any effects as the sample covers 7-12-year-old individuals at baseline. The results generally reflect this, showing mostly statistically insignificant estimates except Column 2, where the interaction term coefficient shows a positive impact among exposed children seven years later.

VI. CONCLUSION

We estimate the impacts of exposure to lead toxic sites, mostly used lead-acid battery (ULAB) recycling, on cognitive skills in a developing country. With the recycling of these batteries, which are mostly used in vehicles, lead leaks into the environment. Previous studies have shown negative impacts of lead exposure on children's academic achievement in developed countries, but only a few have focused on the impacts of toxic sites, and none has measured the impacts in a developing country context.

Exploiting variation in the start of these toxic sites, distance to the toxic site, and age at the start of the toxic site, we find extensive negative effects on cognitive ability. Respondents who were exposed to the toxic site before the age of 7 and who lived within 3 kilometers of a toxic site saw the largest decline on numeracy score of 0.48 s.d., and those living within 3 to 6 kilometers saw a decline of 0.24 s.d. When we study test score gains over 7 years of a subsample of panel respondents who were primary school age at the baseline, we find an even larger negative effect of 0.6 s.d. for those living within 6 kilometers of the sites. We find similar impacts using the Raven test score as the outcome variable, which measures general cognitive ability. Since this type of ability is mainly formed in utero and early life, the negative impact is permanent. General cognitive ability is significantly lower only among the sample exposed to lead in utero. These results are robust against several threats to identification.

To put the magnitude of these effects into perspective, 0.5 s.d. is half of what students learn during primary school (Beatty et al. 2021), and effects above 0.15 s.d. are generally considered large in the education literature (Evans and Yuan 2022).

Post-2010, the Government of Indonesia made significant progress in environmental management by introducing targeted regulations to address lead pollution and incorporating ULAB management into technical guidelines. These efforts represented a substantial advancement in tackling environmental challenges and improving regulatory frameworks. After 2020, however, the Omnibus Law reforms shifted priorities toward facilitating business operations by streamlining environmental permit processes and enhancing regulatory clarity.

With the negative and large impact of lead exposure on cognitive ability, this paper emphasizes the need for stronger regulation or more stringent enforcement around toxic sites and for cleaning up the environment, to prevent more children from being harmed. Several papers have shown that regulations to limit lead exposure, such as removing lead-based paint (e.g. Sorenson et al. 2019), phasing out leaded gasoline (Grönqvist, Nilsson, and Robling 2020), and cleaning up Superfund sites (Persico, Figlio, and Roth 2020), significantly improved cognitive ability again. However, so far, these efforts have taken place mainly in high-income countries. Protecting children in low- and middle-income countries from lead exposure should also be urgently prioritized.

APPENDIX TABLES

Table A1: Impact Estimate for Toxic Sites with Lead Concentration Below the Maximum Recommended Level

	(1) Numeracy	(2) Raven
Treated	-0.51* (0.27)	-0.18 (0.12)
ln(Distance)	0.03 (0.02)	0.00 (0.01)
Treated*ln(Distance)	0.01 (0.15)	0.07 (0.08)
Observations	739	1835

Note: The dependent variable is standardized numeracy score. Analysis includes respondents living within a 10-kilometer radius of any of six sites with a lead test result below the maximum recommended level and any respondents within 3 kilometers. Standard errors in parentheses and corrected for clustering at the enumeration area level. Significance level: * $p < .10$, ** $p < .05$, *** $p < .01$.
Authors' calculations.

Table A2: Correlation Between Numeracy Score and Distance to Toxic Site for All Toxic Sites

	(1) All Sample	(2) All Sample	(3) All Sample	(4) Only Age ≥ 18
<i>Panel A. Dependent variable: numeracy score (std.)</i>				
ln(Distance)	-0.07*** (0.01)	-0.04*** (0.01)	0.02 (0.02)	-0.01 (0.03)
Controls	No	Yes	Yes	Yes
Year fixed effects	No	Yes	Yes	Yes
Site fixed effects	No	No	Yes	Yes
Observations	14,470	14,470	14,470	1,117
Mean dep. var.	-0.04	-0.04	-0.04	0.09

<i>Panel B. Dependent variable: Raven score (std.)</i>				
ln(Distance)	-0.07*** (0.01)	-0.04*** (0.01)	0.02 (0.02)	-0.01 (0.03)
Controls	No	Yes	Yes	Yes
Year fixed effects	No	Yes	Yes	Yes
Site fixed effects	No	No	Yes	Yes
Observations	14470	14470	14470	1117
Mean dep. var.	-0.04	-0.04	-0.04	0.09

Note: The dependent variable is standardized numeracy score for panel A and standardized Raven score for panel B. Table uses household location at the time of the test to determine distance to toxic site. Analysis includes all 59 sites with any

Continued on the next page

IFLS respondents within 3 kilometer as reported in Table 1. Standard errors in parentheses and corrected for clustering at the enumeration area level. Significance level: * $p < .10$, ** $p < .05$, *** $p < .01$.
Authors' calculations.

Table A3: Correlation Between Distance and Characteristics by Treatment Cohort

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Age at Test	Urban	Male	Mother's Education	Father's Education	Years of Education at School	Wealth Index
ln(Distance)	-20.53*** (1.37)	0.12* (0.07)	-0.05 (0.05)	-6.86*** (0.62)	3.73*** (0.73)	2.51*** (0.85)	0.08 (0.11)
Treated	0.03 (0.43)	0.08 (0.06)	-0.01 (0.01)	0.33* (0.18)	1.11*** (0.30)	1.28*** (0.34)	0.07* (0.04)
Treated*ln(Distance)	2.30*** (0.76)	-0.05 (0.04)	0.05* (0.03)	1.62*** (0.43)	-0.50 (0.45)	-0.20 (0.47)	0.02 (0.06)
Observations	6,206	6,206	6,206	6,195	5,903	5,757	6,200

Note: All models include age-at-test, year-of-test, and site fixed effects. Only column 1 does not include age-at-test fixed effects. Standard errors in parentheses and corrected for clustering at the enumeration area level. Significance level: * $p < .10$, ** $p < .05$, *** $p < .01$.

Authors' calculations.

Table A4: Test for Endogeneity of Migration Decisions

	(1) All Sample	(2) Mother Completed More Than Primary Education	(3) Mother Completed Primary Education or Less
<i>Panel A. Dependent variable: moving away from a toxic site</i>			
Treated	-0.025 (0.030)	-0.008 (0.030)	-0.047 (0.057)
ln(Distance)	0.001 (0.001)	0.000 (0.001)	0.001 (0.001)
Treated*ln(Distance)	0.044* (0.025)	0.037 (0.025)	0.048 (0.051)
Observations	2,123	797	1,196
Mean dep. var.	0.027	0.029	0.022

Continued on the next page

	(1) All Sample	(2) Mother Completed More Than Primary Education	(3) Mother Completed Primary Education or Less
<i>Panel B. Dependent variable: moving toward a toxic site</i>			
Treated	-0.527*** (0.167)	-0.519*** (0.188)	-0.497*** (0.158)
ln(Distance)	-0.001 (0.001)	0.000 (0.001)	-0.001 (0.001)
Treated*ln(Distance)	-0.023 (0.090)	-0.036 (0.100)	-0.002 (0.096)
Observations	2,123	797	1,196
Mean dep. var.	0.890	0.765	0.951

Note: The dependent variable for Panel A is a dummy for moving away from a toxic site. The dependent variable for panel B is a dummy for moving toward a toxic site. For those who moved toward a toxic site, distance and age at first exposure are measured at the time of move. All models include age-at-test, year-of-test, and site fixed effects and controls. Standard errors in parentheses and corrected for clustering at the enumeration area level. Significance level: * $p < .10$, ** $p < .05$, *** $p < .01$.

Authors' calculations.

Table A5: Effect of Toxic Site Exposure on Pregnancy Outcomes

	(1) Pregnancy Ended in Miscarriage or Stillbirth	(2) Two or more pregnancies	(3) Child Born Alive But Died Before Age 7	(4) Two or more pregnancies	(5) Birthweight (kg)	(6) Two or more pregnancies
	All		All		All	
Pregnancy exposed to toxic site	0.002 (0.002)	0.000 (0.004)	-0.007 (0.012)	-0.030 (0.030)	-0.114** (0.046)	0.088 (0.129)
Within 6 km	0.001 (0.003)	0.002 (0.003)	-0.018 (0.017)	-0.039 (0.036)	-0.014 (0.057)	-0.000 (0.288)
Pregnancy exposed*Within 6 km	-0.001 (0.003)	-0.001 (0.001)	0.021 (0.019)	0.020 (0.038)	0.013 (0.071)	-0.124 (0.153)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Mother fixed effects	No	Yes	No	Yes	No	Yes
Observations	2,824	2,544	2,824	2,544	1,002	788
Number of mothers	1120	869	1120	869	794	521
Mean dep. var.	0.122	0.132	0.005	0.007	3.091	3.109

Note: The mother fixed effects model relies on variation in exposure to toxic sites across pregnancies within the same women, who are 264 women of whom 132 lived within 6 kilometers of a site at the end of the pregnancy period. Only about half of the babies were weighed at the time of birth. Controls include decade of the pregnancy to correct for trends in wealth and maternal care standards, age of the mother at the time of birth, and the baby's sex. Standard errors in parentheses and corrected for clustering at the enumeration area level. Significance level: * $p < .10$, ** $p < .05$, *** $p < .01$.

Authors' calculations.

Table A6: Difference Between Treated and Comparison Cohort in Characteristics

	All	Matched on Age at Test
Age	-21.46*** (0.63)	-7.94*** (0.57)
Urban	0.03*** (0.01)	-0.07*** (0.02)
Male	0.03 (0.02)	0.03 (0.03)
Mother's education (years)	3.12*** (0.26)	0.75** (0.36)
Father's education (years)	2.38*** (0.23)	0.65* (0.35)

Note: All models include year-of-test and site fixed effects. Standard errors in parentheses and corrected for clustering at the enumeration area level.
Significance level: * $p < .10$, ** $p < .05$, *** $p < .01$.
Authors' calculations.

Table A7: Sample Size by Treatment Group for Individual Panel Analysis

	Within 6 km	6–10 km	Total
Exposed early (treated)	210	168	378
Exposed after age 7	114	190	304
	324	358	682

Authors' calculations.

Table A8: Impacts of Early Lead Exposure on Raven Test Score Gains, Individual Panel Data

	(1) All Panel Sample	(2) All Panel Sample	(3) All Panel Sample	(4) 2007– 2014	(5) 2000– 2007
Within 6 km	-0.15 (0.10)		-0.05 (0.08)	-0.03 (0.11)	-0.07 (0.10)
7 years later	-0.67*** (0.12)	-0.77*** (0.08)	-0.78*** (0.10)	-1.05*** (0.17)	-0.48*** (0.12)
Within 6 km * 7 years later	0.07 (0.13)		0.01 (0.11)	-0.12 (0.21)	0.09 (0.14)
Treated		-0.13* (0.08)	0.01 (0.12)	-0.08 (0.12)	0.10 (0.19)
Treated * 7 years later		0.24** (0.11)	0.13 (0.16)	0.17 (0.20)	0.01 (0.26)
Treated * Within 6 km			-0.13 (0.14)	-0.14 (0.17)	-0.07 (0.21)

Continued on the next page

	(1) All Panel Sample	(2) All Panel Sample	(3) All Panel Sample	(4) 2007– 2014	(5) 2000– 2007
Treated * Within 6 km * 7 years later			0.07 (0.19)	0.11 (0.27)	0.15 (0.30)
Observations	543	715	1358	680	678
Mean dep. var. baseline	1.02	0.94	0.99	1.19	0.79

Note: The dependent variable is standardized raven score. Standard errors in parentheses and corrected for clustering at the enumeration area level. All models include year fixed effects and control variables for urban status, sex, and parent's education. Column 1 includes only respondents who were exposed early, and column 2 only respondents who lived within 6 kilometers of the toxic site. Significance: * $p < .10$, ** $p < .05$, *** $p < .01$. Authors' calculations.

REFERENCES

- Aizer, Anna, Janet Currie, Peter Simon, and Patrick Vivier. 2018. "Do Low Levels of Blood Lead Reduce Children's Future Test Scores?" *American Economic Journal: Applied Economics* 10 (1): 307–341.
- Beatty, Amanda, Emilie Berkhout, Luhur Bima, Menno Pradhan, and Daniel Suryadarma. 2021. "Schooling Progress, Learning Reversal: Indonesia's Learning Profiles Between 2000 and 2014." *International Journal of Educational Development* 85: 102436.
- Beshir, Habtamu, and Jean-François Maystadt. 2020. "In Utero Seasonal Food Insecurity and Cognitive Development: Evidence on Gender Imbalances from Ethiopia." *Journal of African Economies* 29 (4): 412–431.
- Borusyak, Kirill, Xavier Jaravel, and Jann Spiess. 2022. "Revisiting Event Study Designs: Robust and Efficient Estimation." Working Paper CWP11/22. London: Centre for Microdata Methods and Practice,
- Brown, Timothy, and Terry Jernigan. 2012. "Brain Development During the Preschool Years." *Neuropsychology Review* 22 (4): 313–333.
- Caravanos, Jack, Kevin Chatham-Stephens, Bret Ericson, Philip Landrigan, and Richard Fuller. 2013. "The Burden of Disease from Pediatric Lead Exposure at Hazardous Waste Sites in 7 Asian Countries." *Environmental Research* 120: 119–125.
- Chang, Grace, Marta Favara, and Rafael Novella. 2022. "The Origins of Cognitive Skill and Non-Cognitive Skills: The Long-Term Effect of In-Utero Rainfall Shocks in India." *Economics & Human Biology* 44: 101089. <https://doi.org/10.1016/j.ehb.2021.101089>
- Clay, Karen, Margarita Portnykh, and Edson Severnini. 2019. "The Legacy Lead Deposition in Soils and Its Impact on Cognitive Function in Preschool-Aged Children in the United States." *Economics & Human Biology* 33: 181–192.
- Clay, Karen, Alex Hollingsworth, and Edson Severnini. 2023. "The Impact of Lead Exposure on Fertility, Infant Mortality, and Infant Birth Outcomes." Working Paper 31379. Cambridge, MA: National Bureau of Economic Research.
- Crawford, Lee, Rory Todd, Susannah Hares, Justin Sandefur, and Rachel Bonnifield. 2024. "The Effect of Lead Exposure on Children's Learning in the Developing World: A Meta-Analysis." *The World Bank Research Observer*, lkae010. <https://doi.org/10.1093/wbro/lkae010>
- Daniell, William, Lo Van Tung, Ryan Wallace, Deborah Havens, Catherine Karr, Nguyen Bich Diep, Gerry Croteau, Nancy Beaudet, and Nguyen Duy Bao. 2015. "Childhood Lead Exposure from Battery Recycling in Vietnam." *BioMed Research International* 193715. <https://doi.org/10.1155/2015/193715>
- De Chaisemartin, Clément, and Xavier d'Haultfoeuille. 2022. "Two-Way Fixed Effects and Differences-in-Differences with Heterogeneous Treatment Effects: A Survey". Working Paper 29691. Cambridge, MA: National Bureau of Economic Research.

- Ericson, Bret, Howard Hu, Emily Nash, Greg Ferraro, Julia Sinitsky, and Mark Patrick Taylor. 2021. "Blood Lead Levels in Low-Income and Middle-Income Countries: A Systematic Review." *The Lancet Planetary Health* 5 (3): e145–e153.
- Evans, David, and Fei Yuan. 2022. "How Big Are Effect Sizes in International Education Studies?" *Educational Evaluation and Policy Analysis* 44 (3): 532–540.
- Falk, Henry. 2003. "International Environmental Health for the Pediatrician: Case Study of Lead Poisoning." *Pediatrics* 112: 259–264.
- Frankenberg, Elizabeth, and Lynn Karoly, with Paul Gertler, Sulistinah Achmad, I.G. Agung, Sri Hatmadji Hatmadji, and Paramita Sudharto. 1995. "The 1993 Indonesian Family Life Survey: Overview and Field Report." Santa Monica, CA: RAND, Labor and Population.
- Gardner, John, Neil Takhral, Linh Tô, and Luther Yap. 2022. "Two-Stage Differences in Differences." https://jrgcmu.github.io/2sdd_gtty.pdf
- Goodman-Bacon, Andrew. 2021. "Difference-in-Differences with Variation in Treatment Timing." *Journal of Econometrics* 225 (2): 254–277.
- Gottesfeld, Perry, and Amod Pokhrel. 2011. "Lead Exposure in Battery Manufacturing and Recycling in Developing Countries and Among Children in Nearby Communities." *Journal of Occupational and Environmental Hygiene* 8 (9): 520–532.
- Grönqvist, Hans, Peter Nilsson, and Per-Olaf Robling. 2020. "Understanding How Low Levels of Early Lead Exposure Affect Children's Life Trajectories." *Journal of Political Economy* 128 (9): 3376–3433.
- Gunawan, Lilian, and Nurhayati Masloman. 2014. "Correlation of Blood Lead Level and Intelligence Quotient in Children." *Paediatrica Indonesiana* 54 (3): 127–131.
- Guo, Zhiling, Xiaowei Shao, Yongwei Xi, Hiroyuki Miyazaki, Wataru Ohira, and Ryosuke Shibasaki. 2016. "Identification of Village Building via Google Earth Images and Supervised Machine Learning Methods." *Remote Sens* 8 (4): 271. <https://doi.org/10.3390/rs8040271>
- Haryanto, Budi. 2016. "Lead Exposure from Battery Recycling in Indonesia." *Reviews on Environmental Health* 31 (1): 13–16.
- Irawati, Yana, Haryoto Kusnoputranto, Umar Fahmi Achmadi, Ahmad Safrudin, Alfred Sitorus, Rifqi Risandi, Suradi Wangsamuda, Puji Budi Setia Asih, and Din Syafruddin. 2022. "Blood Lead Levels and Lead Toxicity in Children Aged 1-5 Years of Cinangka Village, Bogor Regency." *PLoS ONE* 17 (2): e0264209. <https://doi.org/10.1371/journal.pone.0264209>
- Lanphear, Bruce, Richard Hornung, Jane Khoury, Kimberly Yolton, Peter Baghurst, David Bellinger, Richard Canfield, Kim Dietrich, Robert Bornschein, Tom Greene, Stephen Rothenberg, Herbert Needleman, Lourdes Schnaas, Gail Wasserman, Joseph Graziano, and Russell Roberts. 2005. "Low-Level Environmental Lead Exposure and Children's Intellectual Function: An International Pooled Analysis." *Environmental Health Perspectives* 113 (7): 894–899.

- Liu, Licheng, Yi Wang, and Yiqing Xu. 2022. "A Practical Guide to Counterfactual Estimators for Causal Inference with Time-Series Cross-Sectional Data." *American Journal of Political Science*. <https://doi.org/10.1111/ajps.12723>
- Noguchi, Takako, Takaaki Itai, Nguyen Minh Tue, Tetsuro Agusa, Nguyen Ngoc Ha, Sawako Horai, Trang Pham, Pham Hung Viet, Shin Takahashi, and Yuko Tajima. 2014. "Exposure Assessment of Lead to Workers and Children in the Battery Recycling Craft Village, Dong Mai, Vietnam." *Journal of Material Cycles and Waste Management* 16: 46–51.
- Organisation for Economic Co-operation and Development (OECD). 2023. "PISA 2022 Results: Indonesia Factsheet." Paris: OECD.
- Persico, Claudia, David Figlio, and Jeffrey Roth. 2020. "The Developmental Consequences of Superfund Sites." *Journal of Labor Economics* 38 (4): 1055–1097.
- Persico, Claudia. 2022. "Can Pollution Cause Poverty? The Effects of Pollution on Educational, Health and Economic Outcomes." Working Paper w30559. Cambridge, MA: National Bureau of Economic Research.
- Prihartono, Nurhayati, Ratna Djuwita, Putri Mahmud, Budi Haryanto, Helda Helda, Tri Yunis Miko Wahyono, and Timothy Dignam. 2019. "Prevalence of Blood Lead Among Children Living in Battery Recycling Communities in Greater Jakarta, Indonesia." *International Journal of Environmental Research and Public Health* 16 (7): 1276.
- Rachmat, Basuki, Haryoto Kusnopranto, and Inswiasri Supriyanto 2019. "Anemia in Children Due to Airborne Lead Exposure of Used Lead-Acid Battery Recycling Area in Jabodetabek, Indonesia." *Indian Journal of Public Health Research & Development* 10 (11).
- Rau, Tomás, Sergio Urzúa, and Loreto Reyes. (2015). "Early Exposure to Hazardous Waste and Academic Achievement: Evidence from a Case of Environmental Negligence." *Journal of the Association of Environmental and Resource Economists* 2 (4): 527–563.
- Raven, John. 2000. "The Raven's Progressive Matrices: Change and Stability over Culture and Time." *Cognitive Psychology* 41 (1): 1-48.
- Reyes, Jessica. 2015. "Lead Policy and Academic Performance: Insights from Massachusetts." *Harvard Educational Review* 85 (1): 75–107.
- Sanders, Alison, Sloane Miller, Viet Nguyen, Jonathan Kotch, and Rebecca Fry. 2014. "Toxic Metal Levels in Children Residing in a Smelting Craft Village in Vietnam: A Pilot Biomonitoring Study." *BMC Public Health* 14 (1): 1-8.
- Sargsyan, Aelita, Emily Nash, Gordon Binkhorst, Jenna Forsyth, Barbara Jones, Gabriel Sanchez Ibarra, Sarah Berg, Andrew McCartor, Richard Fuller, and Stephan Bose-O'Reilly. 2024. "Rapid Market Screening to Assess Lead Concentrations in Consumer Products Across 25 Low- and Middle-Income Countries." *Scientific Reports* 14 (1): 9713.

- Sorensen, Lucy, Ashley Fox, Heyjie Jung, and Erika Martin. 2019. "Lead Exposure and Academic Achievement: Evidence from Childhood Lead Poisoning Prevention Efforts." *Journal of Population Economics* 32 (1): 179–218.
- Strauss, John, Firman Witoelar, and Bondan Sikoki. 2016a. "The Fifth Wave of the Indonesia Family Life Survey: Overview and Field Report (WR-675/1-NIA/NICHD)." Santa Monica, CA: RAND, Labor and Population.
- Strauss, John, Firman Witoelar, and Bondan Sikoki. 2016b. "User's Guide for the Indonesia Family Life Survey, Wave 5, Volume 2 (WR-675/2-NIA/NICHD)." Santa Monica, CA: RAND, Labor and Population.
- Tanaka, Shinsuke, Kensuke Teshima, and Eric Verhoogen. 2022. "North-South Displacement Effects of Environmental Regulation: The Case of Battery Recycling." *American Economic Review: Insights* 4 (3): 271–288.
- Tang, Lynn, Aaron Schwid, Dan Kass, and Mutiara Siadari. 2022. "Reduction Childhood Lead Poisoning in Indonesia." Policy Brief. UNICEF, Pure Earth, Vital Strategies, and Ministry of Environment and Forestry of Indonesia.
- Taylor, John, and Sarah Lovell. 2012. "Mapping Public and Private Spaces of Urban Agriculture in Chicago Through the Analysis of High-Resolution Aerial Images in Google Earth." *Landscape and Urban Planning* 108 (1): 57–70.
- Thorsen, Cecilia, Jan-Eric Gustafsson, and Christina Cliffordson. 2014. "The Influence of Fluid and Crystallized Intelligence on the Development of Knowledge and Skills." *British Journal of Educational Psychology* 84: 556–570.
- United Nations Children's Fund (UNICEF) and Pure Earth. 2020. *The Toxic Truth: Children's Exposure to Lead Pollution Undermines a Generation of Future Potential*. www.unicef.org/reports/toxic-truth-childrens-exposure-to-lead-pollution-2020.
- Van der Kuijp, Tsering, Lei Huang, and Christopher Cherry. 2013. "Health Hazards of China's Lead-Acid Battery Industry: A Review of its Market Drivers, Production Processes, and Health Impacts." *Environmental Health* 12: 1–10.
- World Health Organization (WHO). 2017. *Recycling Used Lead-Acid Batteries: Health Considerations*. Geneva: WHO.
- WHO 2021. *Exposure to Lead: A Major Public Health Concern. Preventing Disease Through Healthy Environments*. Second Edition. Geneva: WHO.
- Zhao, Qiang, Le Yu, Xuecao Li, Dailiang Peng, Yongguang Zhang, and Peng Gong. 2021. "Progress and Trends in the Application of Google Earth and Google Earth Engine." *Remote Sens* 13 (8): 3778. <https://doi.org/10.3390/rs13183778>.

Lead Exposure and Cognitive Skills in a Developing Country

Evidence from Toxic Sites in Indonesia

This paper gives causal evidence from a developing country context that children exposed to lead at a young age exhibit worse cognitive outcomes. Using a two-way fixed effects model, it exploits variation in age at first exposure and distance to lead-contaminated toxic sites in Indonesia. It finds a learning penalty in numeracy equivalent to about 3 years of primary schooling for those who lived within 3 kilometers of a toxic site and 1.4 years for those who lived 3–6 kilometers away.

About the Asian Development Bank

ADB is a leading multilateral development bank supporting sustainable, inclusive, and resilient growth across Asia and the Pacific. Working with its members and partners to solve complex challenges together, ADB harnesses innovative financial tools and strategic partnerships to transform lives, build quality infrastructure, and safeguard our planet. Founded in 1966, ADB is owned by 69 members—49 from the region.



ASIAN DEVELOPMENT BANK

6 ADB Avenue, Mandaluyong City

1550 Metro Manila, Philippines

www.adb.org