# Little Lead Soldiers: Lead Poisoning and Public Health

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**Abstract**

Lead poisoning has long-lasting consequences on children's health, as well as on their cognitive and non-cognitive abilities. This paper exploits state-level abatement mandates to study the effects of mitigating lead-paint hazards on several public health outcomes in a difference-in-differences framework. While abatement mandates do not have any significant effect on fertility, birth outcomes, or infant mortality on average, these regulations reduce the number of children with elevated blood lead levels by 26%. Moreover, the mandates decrease the number of children requiring special education in exposed cohorts by 7.3%, indicating a reduction in the number of children with disabilities. A back of the envelope calculation suggests that this decrease in the number of children on special education induces savings between $29 and $176 million per state-cohort on average. However, the reduction in special education enrollment does not appear to be reflected in improvements in educational outcomes, as I find no evidence that average fourth-grade test scores and disciplinary actions change with the mandates.

## 1 Introduction

Lead poisoning has long-lasting consequences on children's health, as well as on their cognitive (Ferrie et al. 2015, Currie et al. 2014, Reyes 2015*b*) and non-cognitive abilities (Reyes 2007, 2015*a*, Nilsson 2009, Feigenbaum & Muller 2015). In particular, children receiving special education are more likely to have elevated blood lead levels (Tarr & Tufts 2009, Miranda et al. 2010), and Nevin (2009) documents a correlation between lead exposure and prevalence of mental retardation. Moreover, because lead can cross the placenta, lead exposure during pregnancy is especially harmful for the fetus and may induce miscarriage (Troesken 2006).[1] In the first half of the last century, however, lead paint was extensively used for residential purposes. In fact, the Department of Housing and Urban Development (HUD) estimates that, nationwide, lead paint

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[1] Indeed, lead was secretly used as a contraceptive in the 19th century.

still lingers in 5.5 million houses (HUD 2011). The Centers for Disease Control and Prevention (CDC) believes the lead paint in the old housing stock to be the main source of exposure for the 535,000 cases of lead poisoning among children born in the US in the 2000s (Wheeler & Brown 2013).[2] Beginning in 1971, a growing recognition of lead hazards motivated an increasing number of states to regulate abatement, i.e., to require control, and, in certain cases, elimination of lead hazards in older units inhabited by children, at costs that can vary between $500 and $40,000 per unit, depending on the extent of the lead hazard (Koppel & Koppel 1994). To fund deleading, HUD has provided over $2 billion in the form of grants and loans for abatement in the past two decades.[3] Hence, it is natural to ask whether the regulations are effective in decreasing children's blood lead levels, and whether the improved children's health results in positive fiscal externalities in terms of reduces expenditure on health care and child disability.

This paper analyzes the effect of state abatement mandates on lead poisoning, fertility, infant health and child disability, providing an estimate of the savings induced by these regulations in terms of health care and educational expenditure. In particular, I ask three questions. First, do the mandates reduce children's blood lead levels? In related work, I find evidence that these mandates do not substantially induce abatement (Gazze 2016), which suggests that the effects of the mandates on children's health might be small due to limited enforcement. However, I also find evidence that families with small children move out of old houses as a result of the regulations, as property owners appear to engage in discriminatory behavior against tenants with small children to avoid complying with the mandates. This reallocation might reduce lead exposure for small children, thus resulting in lower blood lead levels. Second, what are the effects of the mandates on fertility rates and infant health? On the one hand, by reducing lead exposure, the mandates should decrease involuntary abortion rates and improve birth outcomes. On the other hand, if the mandates increase survival rates among otherwise weak fetuses, we might expect birth outcomes to worsen on the margin (Gruber et al. 1999). Third, do the mandates reduce the number of children requiring special education? The requirement for special education is indicative of a child's disability: a decrease in the number of children served under the Individuals with Disability Education Act of 1975 (IDEA) would indicate the mandates' success at improving children's health.

To answer these questions, I combine several data sources on public health outcomes, and I use a difference-in-differences approach to compare outcomes for children exposed and not exposed to the man-

[2]This figure refers to children with blood lead levels (BLL) above $5\mu g/dL$. In 1991, the CDC defined BLLs$\geq 10\mu g/dL$ as the "level of concern" for children aged 1–5 years. However, in May 2012, the CDC accepted the recommendations of its Advisory Committee on Childhood Lead Poisoning Prevention (ACCLPP) that the term "level of concern" be replaced with an upper reference interval value defined as the 97.5th percentile of BLLs in US children aged 1–5 years from two consecutive cycles of National Health and Nutrition Examination Survey (NHANES). In general, the definition of elevated blood lead levels (EBLL) for regulation purposes changes across jurisdictions and over time. In this paper I use the term lead poisoning to refer to cases of elevated blood lead levels.

[3]Source: Author's calculation based on HUD data from 1993 to 2014.

dates across states. This comparison is informative because small children's neurological development is particularly susceptible to neurotoxins (see, e.g., McCabe 1979): hence, children already in school after the introduction of the regulation would benefit less from any decrease in lead exposure potentially induced by the regulation. My empirical analysis proceeds in three steps. First, I investigate the impact of the mandates on blood lead levels using state-level data from the CDC. Second, I use Vital Statistics data from the CDC to estimate the effects of the mandates on fertility, birth outcomes and infant mortality. Third, using data collected by the US Department of Education, I estimate the effect of the mandates on child disability. In addition, I investigate the impact of the mandates on educational attainment using data from the National Assessment of Educational Progress (NAEP).

Correspondingly, I find three sets of results. First, I provide suggestive evidence that the mandates reduce the number of lead-poisoned children by 6,460 per year in implementing states, which is equivalent to a decline in the probability of lead poisoning, estimated imprecisely, on the order of 2.5 per thousand children. Using plausible estimates from the literature on the cognitive effects of lead poisoning, I compute that preventing one case of lead poisoning is worth $110,000 in terms of increased lifetime earnings, which translates into expected benefits of the mandates worth $285 per child and into potential additional fiscal revenues of $16,460 per child, assuming an income tax rate of 15%. Second, the mandates have no significant effect on fertility or infant health on average. Surprisingly, the regulations appear to decrease birth rates in counties with a high share of old houses. Moreover, in these counties birth outcomes do not appear to change after the introduction of a mandate, while birth outcomes deteriorate in the average county. Dettling & Kearney (2014) estimate that fertility is procyclical for property owners and countercyclical for non-owners. Furthermore, in related work, I find that houses depreciate and rental expenditures for families with small children increase after the introduction of the mandates (Gazze 2016). Plausibly, households might respond to the mandates' effects on the housing market by delaying childbearing. Third, among cohorts born up to six years prior to the introduction of a mandate, 7.3% fewer children require special education, and the effects are stronger for speech or language impairments diagnoses and the longer the length of exposure to the mandate. Moreover, the effects are strongest for cohorts that were not born at the time of the introduction of the mandate, implying steady-state effects as large a a 25% decrease in the number of children on special education per cohort-grade, off an average enrollment of 8,706 students in control states, or a 24% fall in the rate of enrollment in special education. Chambers et al. (2002) estimate that, in 2000, the additional spending to educate a student with a disability amounted to $5,918. Therefore, the mandates could induce savings in special education programs that total between $29 million and $176 million per state-cohort on average. Finally, the reduction in the number of children served under IDEA does not translate into higher test scores on average.

While most of the economic literature focuses on the effect of lead policies on crime (Reyes 2007, Nilsson 2009, Feigenbaum & Muller 2015), only a few studies attempt to estimate the effects of these regulations on medical and educational expenditures. Reyes (2014) combines several estimates of the adverse effects of lead poisoning to derive a figure of the social benefits of deleading, but her approach does not rely on counterfactual analysis. In this paper, I provide suggestive evidence that abatement mandates decrease lead poisoning, in line with findings by Aizer et al. (2015) on the effects of Rhode Island lead-safe certification policy for rental units, and by Jones (2012), who compare census tracts with different abatement rates to estimate that in Chicago, abating one unit prevents 2.5 cases of lead poisoning. Other studies have found no evidence that improving home environments and decreasing lead dust through intensive case management lowers blood lead levels (BLLs) (Brown et al. 2006, Campbell et al. 2011) or mitigates behavioral and educational issues for lead-poisoned children more than other early-childhood interventions (Billings & Schnepel 2015). Moreover, I provide novel estimates of the effects of the mandates on fertility, birth outcomes and infant mortality, which complement the existing studies of the effect of lead poisoning on infant mortality and long-term health outcomes in the US in the first half of the last century, when lead exposure was much more severe (Clay et al. 2014, Ferrie et al. 2015). I provide additional evidence of the benefits of the mandates in terms of child disability, in line with the work by Aizer et al. (2015) on the effects of the Rhode Island mandate on the black-white test score gap. However, our results on test scores are strikingly different.[4] On the one hand, the state-level NAEP data does not allow me to investigate whether children from more disadvantaged areas benefit differentially from the policies. On the other hand, if enforcement of the regulations is stricter in Rhode Island than it is in the average state, the impact of the average mandate on test scores would be smaller than the impact estimated for Rhode Island. Therefore, more research is warranted to understand what factors drive the different results. Finally, this paper complements my related work on the costs of the mandates implied by reallocation in the housing market by providing more insight on the health effects and the related positive fiscal externalities of these regulations (Gazze 2016).

The paper proceeds as follows. Section 2 provides background on the health effects of lead poisoning, the mandates studied in this paper, and the regulatory environment concerning special education. Section 3 describes the data I use in my empirical analysis. Section 4 shows the effect of the mandates on children's blood lead levels. Section 5 estimates the impact of the mandates on fertility and infant health outcomes. Section 6 discusses the impact of the mandates on child disability and special education expenditures, while Section 7 investigates the impact of the mandates on educational attainment and disciplinary actions. Section 8 concludes with policy implications.

[4]See also Zhang et al. (2013) for evidence of the correlation between lead poisoning and low test scores.

# 2 Background

## 2.1 Lead Poisoning, Fertility, and Infant Health

When paint surfaces deteriorate in the home, residents, and especially children, are exposed to health hazards from lead-contaminated dust. Lead dust enters the human body through ingestion or inhalation. Once in the bloodstream, lead has several adverse consequences. Lead mimics calcium, which plays a role in many biological processes, including the renal, endocrine, cardiovascular and nervous systems. Lead clogs these processes, with implications ranging from reduced cognitive ability and behavioral problems to infertility and death (DNTP 2012).[5] Moreover, while lead has only a two-week half life in the blood, it has roughly a two-year half life in the brain, and it accumulates in bones (Lidsky & Schneider 2003). The effects of lead poisoning are irreversible, and treatment can only help prevent further accumulation of the toxin (Rogan et al. 2001).

From the perspective of human reproduction, lead is known to cause a number of adverse outcomes in both men and women. In addition to causing infertility in both sexes, effects of lead exposure in women include miscarriage, premature membrane rupture, pre-eclampsia, pregnancy hypertension, and premature delivery (Winder 1992). Indeed, Troesken (2006) reports stories of still births and high rates of infant mortality related to lead-poisoned mothers in the UK in the early years of the Industrial Revolution. In a population study of DC neighborhoods exposed to high levels of lead in the water due to leeching lead pipes, Edwards (2013) finds that areas with high water lead levels see birth rates decrease and fetal death rates increase.

From the perspective of infant health and child development, lead impairs cognitive and non-cognitive ability at levels as low as $1 - 2\mu g/dL$, 80 times lower than the level of concern for iron (DNTP 2012). Lanphear et al. (2005) estimate an IQ loss of 3.9 points when blood lead levels (BLLs) increase from 2.4 to 10 $\mu g/dL$, with lower IQ decrements associated with further BLLs increases. Small children are especially exposed to lead-contaminated dust from paint and windowsills due to normal hand-to-mouth activity, and they might grow accustomed to the sweet taste of lead paint (Fee 1990).[6] Moreover, lead is most damaging to small children: they absorb and retain more lead than adults, and their neurological development is particularly susceptible to neurotoxins (see, e.g., McCabe 1979).

[5] Troesken (2006) discusses how lead pills were used to terminate undesired pregnancies during the late 1800s and early 1900s, sometimes leading to the death of the mothers.

[6] For years, it was argued that only children affected from pica disorder, i.e., the persistent and compulsive craving to eat nonfood items, were subject to eating lead paint, and that careless parents were to blame for their children's lead poisoning. Markowitz & Rosner (2000) quote a letter, dated December 16, 1952, by an official of the Lead Industries Association asserting that childhood "lead poisoning is essentially a problem of slum dwellings and relatively ignorant parents" and that "until we can find means to (a) get rid of our slums and (b) educate the relatively ineducable parent, the problem will continue to plague us."

## 2.2 Lead Abatement Mandates

Lead paint is commonly found in old houses in the US. Starting in the late 19th century, paint manufacturers typically used lead as an additive in residential paint to increase the durability of the paint coat, with paint manufactured prior to 1950 containing up to 50% lead by weight (Reissman et al. 2001). In response to the growing body of evidence of the harm associated with lead, in the late 1950s, some manufacturers voluntarily reduced the lead content of paint to 1%, a level that can still induce severe lead poisoning (Hammitt et al. 1999). Finally, in June 1977, the Consumer Product Safety Commission (CPSC) lowered the allowed level of lead in paint to 0.06%, effectively banning lead paint altogether from 1978 on.[7] Notably, the ban covers new paint, but not the paint present in the pre-existing housing stock (Mushak & Crocetti 1990).

To deal with the potential lead hazards caused by lead-paint in the old housing stock, 19 states have enacted abatement mandates, as summarized in Table 1.[8] Although physicians had warned against the hazards of lead paint since the early 1900s (Ruddock 1924), the first regulation banning the use of lead pigment for interior use in the US was only adopted in 1951 by the Baltimore Commissioner of Health (Fee 1990). Two decades later, in 1970, the US Surgeon General released a policy statement calling for "adequate and speedy removal of lead hazards" from the homes of lead-poisoned children below six years of age (Steinfeld 1971). Massachusetts was the first state to follow suit, introducing in 1971 one of the strictest lead paint regulations in the country, requiring property owners to permanently control lead paint hazards in the home of any child under the age of six.

In related work, I find that old units depreciate persistently after the introduction of the mandates, and fewer old single-family units appear to enter the rental market after a mandate. Moreover, after a mandate, families with small children are less likely to live in old houses than before. Together, these results suggest that owners do not immediately comply with the mandates: if they did, abated houses would appreciate, and families with children would move into these safer homes. Hence, despite little abatement takes place, the mandates have real consequences for families with small children in terms of reallocation across housing units and increased rental expenditures.

## 2.3 Special Education

The Individuals with Disabilities Education Act (IDEA) of 1975 ensures free services to children with disabilities throughout the US.[9] Infants and toddlers with disabilities aged zero to two receive early intervention

[7] Seven years earlier, in 1971, the Lead-Based Poisoning Prevention Act (LBPPA) directed the Secretary of Housing and Urban Development to "prohibit the use of lead-based paint in residential structures constructed or rehabilitated by the Federal Government, or with Federal assistance in any form after January 13, 1971." (LBPPA, 1971)

[8] Regulations were identified with a search through LexisNexis and Westlaw.

[9] Previously known as the Education for All Handicapped Children Act, it was renamed to IDEA in 1990. The latest revisions to the act were signed into law in 2004.

services under IDEA Part C. Children and youth aged 3-21 receive special education and related services under IDEA Part B (US DE). Under IDEA Part B, children receive an Individualized Education Program (IEP), a written statement including an assessment of the child's current levels of academic achievement and functional performance, a list of measurable annual goals to measure the child's progress, and a list of any individual appropriate accommodations for the child. The parameters for defining whether a child qualifies for special education change across states and over time. For instance, the 1990 Supreme Court decision in *Sullivan v. Zebley* led to laxer standards for children to qualify for a mental or emotional disability. In 1996, however, the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) tightened these standards again in an effort to curtail the surge in disability enrollment. By relying on a difference-in-differences methodology, my empirical strategy controls for both time trends and state-level differences in the standards for qualifying for special education under IDEA.

Importantly, IDEA ensures free screening for disabilities to all children enrolled in public schools. For this reason, screening under IDEA is usually the first step for a child to receive Supplemental Security Income (SSI) for a disability. On the other hand, parents might have incentives to avoid screening for their child if they think that being labeled as requiring special education might cause stigma. To the extent that the abatement mandates studied in this paper do not differentially change these incentives for parents of small children, the inclusion of state-year fixed effects in some of my specifications should capture differential generosity across states and periods. In particular, for my estimates to be valid, the mandates need to not be correlated with changes in the relative generosity of SSI benefits with respect to the Temporary Assistance for Needy Families (TANF) and the Aid to Families with Dependent Children (AFDC) programs. Indeed, Cohen (2007) shows that parents respond to changes in the financial gains from receiving SSI benefits when deciding whether to apply for child disability.

Parents are not the only agents to respond to financial incentives in their decision to have their child screened for disabilities. State governments and schools face different trade-offs that might lead to overclassification or to underprovision of services. On the one hand, the states have incentives to move people from the AFDC/TANF program, which is state-funded, to SSI, which is completely federally funded (Kubik 1999). In my empirical strategy, I control for both parents' and states' financial incentives by including in some specifications the maximum benefit that a family of three, i.e., a single parent and two children, is entitled to in a given state and year under the TANF/AFDC programs. On the other hand, schools choose what services to provide taking into account the responses of parents of both disabled and non-disabled children (Cullen & Rivkin 2007). For example, having a high share of disabled children in the school might discourage less expensive and better achieving nondisabled students. Depending on the state financing system, however, schools have the incentive to overclassify students as disabled in order to procure additional resources. In

particular, under a weighted special education funding system, state special education aid is allocated on a per student basis, which might encourage overclassification. Moreover, wherever weights differ according to the severity of the disability, incentives for misclassification arise. In contrast, funding systems that are based on flat grants or on actual expenditures are less prone to misclassification.[10] Appendix Table A.1 reports changes in the funding systems for special education services across states. These changes do not appear to be correlated with the timing of the mandates illustrated in Table 1. Hence, to the extent that the abatement mandates studied in this paper do not differentially change schools' incentives to misclassify students in special education, the inclusion of state-year fixed effects in some of my specifications should capture differential funding for students requiring special education across states and periods.

Historically, minorities have been overrepresented in special education status (Losen & Orfield 2002). Many factors contribute to racial disparites in special education status, and both environmental factors and socioeconomic status play an important role (Oswald et al. 2002). Figure 1 illustrates this fact by plotting the average share of white and black children aged six to 21 served under IDEA across states over time. In particular, overrepresentation is worse for disabilities that can be related to lead poisoning, which are mostly "soft disabilities", i.e., disabilities that allow for a higher degree of subjectivity in the award decision.[11][12] Furthermore, Figure 1 shows a narrowing in the black-white representation gap over time, that parallels the documented narrowing in the black-white test score gap (see Figures 6 and 7). Aizer et al. (2015) show that a policy that required deleading of rental units in Rhode Island significantly reduced the black-white test score gap in the state. Hence, in this paper I investigate whether the average abatement mandate helped reduce the racial disparity in special education.

# 3 Data

In this project, I combine data from five sources in order to analyze the impact of lead abatement mandates on lead poisoning, fertility, infant health, child disability and educational attainment.

**Lead Poisoning.** To evaluate the health benefits of the mandates, I use yearly state-level blood lead level (BLL) data from 1997 to 2012 from the CDC. Appendix Figure A.1 shows a steep decreasing trend in BLLs in the US since 1997. Far from claiming that it can be attributed to the mandates studied in this paper, I discuss methods to separate my estimates from this secular trend in Section 4.

**Fertility, birth outcomes, and infant mortality.** To analyze the effects of the mandates on fertility

[10] The basis of allocation of funds adds another dimension of heterogeneity across special education funding systems. See O'Reilly (1989, 1993), Parish et al. (1997), Parrish et al. (2003), Ahearn (2010) for a thorough discussion of these issues.

[11] Mental retardation, emotional disturbance and specific learning disabilities present the highest overrepresentation rates. See, e.g., Fierros & Conroy (2002) for a discussion of this phenomenon.

[12] See Section 3 below for a thorough discussion of the classification of disabilities into those that are and does that are not related to lead poisoning.

and infant health, I use the Vital Statistics data provided by the National Center for Health Statistics (NCHS) at CDC. I employ the restricted Birth Cohort Linked with Infant Death sample for the years 1989-1991 and 1995-2010, where year 2004 is missing.[13] This sample links information from individual birth certificates, including birth outcomes and mother's demographics, with information from the death certificate of infants who died within a year from birth, including cause of death. The cause of death code is crucial to discard deaths by external cause which are not related to lead poisoning. The restricted data have county identifiers that allow me to estimate heterogeneous effects of the mandates depending on the age of the housing stock.

**Child disability.** To analyze the effects of the mandates on long-run child health, I focus on special education needs as a proxy for child disability. I use state-level counts of the number of children served under IDEA, collected by the Office of Special Education Programs at the US Department of Education (US ED, OSEP). States are required to report the number of children they serve under the IDEA to the US ED. In Table 2, I classify disabilities into lead-related and not lead-related, based on the medical and public health literature on lead poisoning as well as on conversations with pediatricians and health practitioners (Kaiser et al. 2008, Rau et al. 2013). While this is not a perfect classification, it takes into account issues such as reverse causality, by which autistic children are more likely to be lead poisoned because autism induces riskier behavior (Woolf et al. 2007). Figure 2 shows that the most common disabilities change with age: while developmental delay is most relevant in preschool-aged children, speech or language impairments are the lion's share of disabilities both in preschool- and elementary-school-aged children.[14] Later on, specific learning disabilities become most prevalent among secondary-school-aged children. Indeed, Hanushek et al. (1998) argue that different disabilities have different rates of entry into and exit from special education status. Unfortunately, the state-level counts of children served for different disabilities provide information on the net stock of children requiring special education, but not on the flows. In my empirical analysis, I focus on children between six and 13 years of age for two reasons. First, I do not include preschool age cohorts to prevent selection bias due to changes in the composition of the group of children screened for disabilities around the announcement of the mandates, as access to preschool and health care depends on the family's socioeconomic status (Walters 2015, Cascio & Schanzenbach 2013). Moreover, many children are screened for disabilities after a diagnosis of lead poisoning during infancy. Although I do not find that the mandates affect screening rates for lead poisoning (Appendix Table A.2), I mitigate concerns of sample selection by focusing on older children. Second, to avoid issues of sample selection related to dropouts, I limit my sample to children 13 years old or younger. Table A.8 shows that my results are robust to focusing

[13] In results not reported in the data I find that my state-level estimates are robust to including 1983-1988 data for which county identifiers are not available.

[14] Congress widened the definition of disabled under IDEA in 1997 to include the population of developmentally delayed children ages three to nine. Controlling for year fixed effects accounts for changes in the inclusion criteria over time.

on elementary-school-aged children.

States started reporting the number of children aged six to 21 served under IDEA by disability and race in 1998. Hence, to study whether the mandates differentially affect child disability by race, I construct an exposure variable equal to the share of children aged six to 21 that was exposed to the mandate based on birth cohort. Figure 1 plots the average share of white and black children aged six to 21 served under IDEA across states over time, splitting the sample according to whether the disability can be related to lead poisoning.

To validate my findings on child disability, in further work, I plan to obtain data on children receiving disability benefits from the Social Security Administration (SSA) and supplement my analysis using data from the Survey of Income and Program Participation (SIPP).[15]

**Educational Attainment.** To study state-level educational attainment, I use fourth grade mathematics and reading test-score data from NAEP for the years 1992-2013.[16] NAEP selects a random sample of students from about 100 schools that is representative at the state level and that allows for comparisons of educational attainment across states. Relevant to this project, school personnel decide whether to include students with disabilities in NAEP assessments based on information in a student's Individualized Education Program (IEP). Generally, children who are included in the state or local testing program are included in NAEP, if they are selected. Nonetheless, there should be no internal pressure to select certain students or schools for assessment because NAEP does not report scores for individual schools or their students. However, NAEP is voluntary, and there are no incentives for students to perform well. To the extent that these incentives do not change with the introduction of an abatement mandate, this source of measurement error will not bias my estimates of the effects of the mandates on educational attainment.

As a next step, I plan to obtain district-level data on both the number of children served under IDEA and standardized test scores from mandatory state assessments. Although state assessments are not comparable across states, controlling for state fixed effects will allow me to difference out state-level averages and compare changes in the whole test score distribution over time. Moreover, scholars have worked on converting state proficiency standards to NAEP scores.

**Students' Behavior.** Finally, I study whether the mandates improve students' behavior in school using data on disciplinary actions from the Civil Rights Data Collection by US DE for the years 2000, 2004, 2006, 2009-10, and 2011-12. Currently, I analyze data at the state-level, but I plan to geocode data at the school district level in order to compare outcomes for schools with catchment areas that might be differentially

[15] See Duggan & Kearney (2007) and Cohen (2007) for examples of projects on child disability and/or special education requirements that use SIPP and SSA data, respectively.

[16] Mathematics test-score data are available for the years 1992, 1996, 2000, 2003, 2005, 2007, 2009, 2011, and 2013. Reading test-score data are available for the years 1992, 1994, 1998, 2002, 2003, 2005, 2007, 2009, 2011, and 2013.

affected by the mandates due to the different age of the housing stock.

# 4 Lead Poisoning Effects

As the mandates' goal is to reduce lead poisoning caused by exposure to lead paint in old houses, it is natural to wonder whether these policies achieve their goal and decrease blood lead levels. I investigate this question in a difference-in-differences (DD) framework by comparing blood lead levels (BLLs) in implementing and non-implementing states before and after the introduction of the mandates, as outlined in the following equation:

$$Y_{st} = \beta Mandate_{st} + \pi Log(\#Children < 6)_{st} + \gamma_s + \delta_t + \epsilon_{st} \tag{1}$$

where $Y_{st}$ is a measure of lead poisoning in state $s$ at year $t$, $Mandate_{st}$ is an indicator for year $t$ being the year of the mandate's introduction in state $s$ or any year thereafter, $\#Children < 6$ is state's population of children below 72 months of age, and $\gamma_s$ and $\delta_t$ are state and time fixed effects.

The internal validity of the DD framework hinges on the assumption that implementing states are on parallel trends prior to the mandates, i.e., the assumption that the timing of the mandates is uncorrelated with the error term $\epsilon_{st}$, conditional on the control variables. This would be violated, for instance, if states implemented the mandates in response to abnormal blood lead levels. The first mandates were introduced in 1971 and the latest in 2005. This sparse timeline suggests that states enacted the regulations in an idiosyncratic manner, unrelated to medical research on lead hazards or to other legislation on lead or old houses. To verify that the parallel trends assumption holds in the data, I estimate a year-by-year version of the DD, as in the following equation, and present plots of the leads, $\alpha_y$, and lags, $\beta_y$, of the mandates' effect on old houses:

$$Y_{st} = \sum_{y=1}^{T_{min}} \alpha_y Pre_{t-y,s} + \sum_{y=0}^{T_{max}} \beta_y Post_{t+y,s} + \pi Log(\#Children < 6)_{st} + \gamma_s + \delta_t + \epsilon_{st} \tag{2}$$

Tables 3 and 4 present DD estimates of the effect of the mandates on the rates of lead poisoning and on the number of children who test positive for BLLs above $10\mu g/dL$ (Columns 1-3) and above $20\mu g/dL$ (Columns 4-6), over the period 1997-2013. Qualitatively, Tables 3 and 4 show similar declines in the incidence of BLLs above $10\mu g/dL$, ranging between 10% and 26% when measured in counts and 16% and 40% when measured in rates. In many states, screening efforts target children most at risk for lead poisoning, so to compute the benefits of the mandates it is more appropriate to focus on counts of lead-poisoned children, even though Appendix Table A.2 shows no evidence that the mandates affect the number of children screened for lead poisoning. However, when I consider the full sample of BLL data, I cannot reject the null hypothesis that

the mandates have no effect on EBLLs (Panel A of Table 4) due to large standard errors.

In an effort to improve the precision of the estimates of the health effects of the mandates, in Panel B of Table 4, I restrict the sample of implementing states to those that implement the mandates during the period captured in the data, i.e., Georgia, Michigan, Ohio and Rhode Island. In an unbalanced panel, heterogeneous treatment effects make the estimates less precise and interpretable, and I suspect that the mandates have differential health impacts across states, given the evidence of differential enforcement in Gazze (2016). Figure 3 investigates the validity of the DD design in this sample: the left figure shows estimates from the baseline DD regression in equation 2, while the right figure introduces differential linear trends based on initial lead poisoning rates to account for the secular downward trend in the number of confirmed cases of EBLLs in my sample period (Appendix Figure A.1). This latter specification seems to eliminate existing pre-trends, and therefore, it is my preferred specification: Columns 2 and 5 of Table 4 show the relative DD estimates, while Columns 3 and 6 introduce differential linear trends for implementing states. In this restricted sample, my preferred specification suggests that introducing a mandate decreases the number of children who test positive for BLL above $10\mu g/dL$ by 26% per year on average, or 340 children, and above $20\mu g/dL$ by 32%, or 76 children.[17]

Extrapolating these results to all 19 implementing states, a back of the envelope calculation attributes to the mandates 6,460 averted cases of BLLs above $10\mu g/dL$ per year on average, and 1,444 cases of BLLs above $20\mu g/dL$, over a total population of 7.5 million children below 72 months of age in these states. Given the issues with the estimation, I consider these numbers to be only suggestive of the impact of the mandates across different states, and likely to be an upper bound. Because I found little evidence of high abatement rates in Gazze (2016), I argue that much of the mandates' effect on lead poisoning rates is due to the reallocation of families with small children into new houses. Here, I compute the implied rate of lead poisoning among these "movers" if we attribute all of the reduction in EBLLs to the reallocation estimated in Gazze (2016). Before the mandates are introduced, 82.5% of the 6.2 million families with small children live in old homes. After a mandate, 4% fewer families with children, i.e., around 215,000, live in old homes. This figure implies a lead poisoning rate among the movers of 3% if each household had only one child under six, a plausible rate given that the probability of lead poisoning conditional on living in an old house is approximately 1% at baseline.[18]

Drawing on results from the public health literature, Column 4 of Appendix Table A.3 computes the average benefits of preventing one BLL above $10\mu g/dL$ in terms of lifetime earnings to be \$110,000. This

[17] Appendix Figure A.2 confirms the validity of my preferred specification also when the outcome variables are lead poisoning rates and counts of children screened for lead poisoning.

[18] At baseline, the total probability of lead poisoning among children below six years of age is 0.008 (Column 2 of Appendix Table A.3), which I scale up by the share of families with small children who live in old houses before the mandates are implemented, 82.5% .

figure underestimates the benefits of preventing lead poisoning to families because it does not include the improvements in parents' well-being and decision-making due to decreased stress, which Mani et al. (2013) argue can be substantial.[19] In particular, I first assign a probability to different degrees of lead poisoning based on the baseline CDC data (Column 2). Then, I calculate gains in PDV of lifetime earnings (in 2006 USD) by multiplying the loss in IQ points associated with different degrees of lead poisoning from Lanphear et al. (2005) by $17,815 (Gould 2009, Schwartz 1994). Since my estimate of a 26% decrease in the number of EBLLs translates into a decrease in the probability that a child has an elevated blood lead level of 0.0026 from a baseline rate of 1%, I obtain an expected benefit from the introduction of a mandate of $285 per child. Given the irreversible consequences of lead poisoning, I consider this to be a one-time gain. Hence, I conclude that for families with small children, the mandates' expected health benefits are lower than the mandates' costs in terms of the persistent increase in rent expenditures of $400 per year that I estimate in Gazze (2016). In addition, these increased lifetime earnings would translate into increased tax revenues of about $16,460 per child based on an income tax rate of 15% (Column 6 of Appendix Table A.3).

# 5 Fertility and Infant Health Effects

The medical literature on lead poisoning suggests that lead exposure can have large adverse effects on fertility, birth outcomes, and infant mortality. To test whether the decrease in BLLs due to the mandates estimated in the previous section translates into improvements in fertility and infant health, I contrast outcomes for cohorts conceived before and after the introduction of these regulations in a DD framework. In other words, I fit equations of the form:

$$Y_{cst} = \beta_0 Mandate_{st} + \beta_1 Mandate_{st} * OldHousing_c + \pi \mathbf{X}_{ct} + \gamma_s + \delta_t + \epsilon_{cst} \tag{3}$$

where $Y_{cst}$ is an outcome, such as logarithm of the number of births in county $c$ in state $s$ and year $t$; $Mandate_{st}$ is an indicator for year $t$ being the year the mandate is introduced in a given state $s$ or any year after that date; $OldHousing_c$ is an indicator for counties that have above median shares of houses built prior to 1978 according to the 2000 Census;[20] and $\gamma_s$ and $\delta_t$ are state and time fixed effects. In some specifications, I include county fixed effects, state-specific trends, or trends for counties with a high share of old housing, and either county-level population or state-level controls from the 1980 Census interacted

[19] Cattaneo et al. (2009) find that replacing dirt floors with cement floors in Mexico improves both children's health and adult welfare, as measured by increased satisfaction with their housing and quality of life, as well as by lower scores on depression and perceived stress scales.

[20] Appendix Tables A.5 and A.7 show that the effects of the mandates on fertility and infant health in counties with a high share of old housing are robust to defining $OldHousing_c$ as equal to one for counties that have an above median share of houses built prior to 1950.

with year dummies. The controls are: share of high school dropouts, share of high school graduates, share of individuals with less than four years of college, share of blacks, share of hispanics, unemployment rate, and share of people below the poverty line. Furthermore, availability of individual-level data allows me to split the sample based on mother characteristics.

The left panel of Figure 4 plots year-by-year DD estimates from a leads and lags equation in the spirit of equation 2, showing no significant effect of the mandates on total fertility, a result that is confirmed in Columns 1-3 of Table 5 and that holds both in the full sample and in the subsamples of mothers of low socioeconomic status (Appendix Table A.4). By allowing the effect of the mandates to vary with the age of the housing stock in each county, the right panel of Figure 4 suggests that the mandates' effects on fertility are highly heterogeneous. In particular, births appear to decrease in counties with a high share of houses built prior to the ban of lead paint in 1978. However, this effect vanishes six years after the introduction of a regulation, and the point estimate on the average effect of an abatement mandate on fertility is not statistically significant in Column 5 of Table 5. Nonetheless, it is worth speculating why the mandates might have a negative effect on fertility precisely in those counties where the regulations have more bite due to the age of the housing stock. As these specifications control for county population, the results cannot be explained by migration that might be induced by the mandates.[21] Dettling & Kearney (2014) estimate that birth rates respond to changes in house prices, and in particular they find fertility to be procyclical for owners, whose fertility increases by 5% when houses appreciate by $10,000, while fertility appears to be countercyclical for non-owners. Gazze (2016) finds that the abatement mandates are capitalized into property values that fall by between 4.3% and 6.4%, or $4.80 per square foot. Given that the average house in the sample measures about 1,600 square feet, the average loss in property value totals $7,680. If the fertility effects of property values are linear, the estimates by Dettling & Kearney (2014) translate into a decrease in fertility of about 3.8% among property owners due to the depreciation of old houses induced by the mandates, a value that is not too far from the estimate in Column 5 of Panel A of Table 5, albeit the latter is not statistically significant. In addition, Gazze (2016) finds that rental expenditure for families with children increases, which could also be associated with both the decision to delay childbearing and with stress.

Panel A of Table 6 presents no evidence that the mandates affect infant mortality and birth outcomes on average, a result that is robust across different specifications and among mothers of low socioeconomic status (Appendix Table A.6). One plausible explanation for this result is that the lead accumulated in mothers' bones is released in the mother's bloodstream during pregnancy and in breastmilk at lactation, as

[21] In an analysis not included in the paper, I find no effects of the mandates on state-level migration measured with Census data.

lead mimics calcium (Gulson et al. 2003). As a result, fetuses of mothers with a history of lead exposure are themselves exposed to lead, independently of the mother's environment during pregnancy.

By allowing the effect of the mandates to vary with the age of the housing stock in each county, Panel B of Table 6 suggests that the mandates' effects on infant health are highly heterogeneous, like the mandates' effects on fertility. Indeed, the mandates appear to have weakly negative effects in counties where the housing stock is newer.[22] This pattern could indicate that the mandates increase stress levels for disadvantaged mothers during pregnancy, as maternal stress is associated both with low birth weight and with preterm birth (Wadhwa et al. 1993). In related work, I find that the mandates increase rental expenditures for families with small children and induce discriminatory behavior by property owners, which might decrease housing stability for disadvantaged mothers. To corroborate this hypothesis, in further work, I plan to analyze data from the National Health and Nutrition Examination Survey to estimate the effect of the mandates on households' budget constraints for food and other necessary expenditures. In contrast, in counties with a high share of old houses the mandates appear to have no net effect. In particular, the coefficient on the period after a mandate in counties with a high share of old houses is positive for the Apgar score at five minutes after birth, a measure of the wellness of the newborn. These improvements in birth outcomes with respect to the average county in the state might be due either to the mandates preventing lead exposure for pregnant mothers and their fetuses, or to selection in the marginal born child associated with the reduction in fertility shown in the right panel of Figure 4 (Gruber et al. 1999).[23]

The estimates in Panel B of Table 6 suggest that the mandates increase birth weight and gestational age at birth in counties with a high share of old houses, but these results are not statistically significant by conventional standards. This is in line with the medical literature that finds inconclusive evidence regarding the correlation between low-level lead exposure of mothers during pregnancy and both pre-term birth and birth weight (Zhu et al. 2010). Using estimates of the effect of low birth weights on health care spending by Almond et al. (2005), my findings imply negligible savings from the mandates due to improved birth outcomes.

# 6 Child Disability Effects

The previous sections suggest that the mandates decrease blood lead levels and affect fertility and infant health outcomes depending on the age of the housing stock in the child's environment. Thus, I now proceed to estimate the effects of the mandates on child disability in a DD framework. In particular, I contrast

[22] Appendix Figure A.3 suggests that the mandates effects on the Apgar score at five minutes are not driven by pretrends.

[23] Appendix Table A.6 and Appendix Figure A.4 show that these results hold also and in the subsamples of mothers of low socioeconomic status.

the number of children requiring special education among cohorts exposed to the mandates by estimating variations of the following specification:

$$Y_{bst} = \beta Mandate_{st} + \phi Exposed_{bst} + \pi X_{st} + \gamma_s + \delta_t + \eta_b + \epsilon_{bst} \tag{4}$$

where $Y_{bst}$ is the logarithm of the number of children served under IDEA part B of birth cohort $b$ in year $t$ in state $s$, $Mandate_{st}$ is an indicator for year $t$ being the year the mandate is introduced in a given state $s$ or any year after that date, $Exposed_{bst}$ is an indicator for cohort $b$ being born up to six years prior to the introduction of the mandate in state $s$ in year $t$, $X_{st}$ is state's population of children below 72 months of age, and $\gamma_s$, $\delta_t$, and $\eta_b$ are state, time, and cohort fixed effects. In addition, in my preferred specification, I control for state-year fixed effects by exploiting variation in the length of exposure to the mandates given by the age of the child at the introduction of the regulation. This is important because states set their own standards for special education screenings and these standards might change over time.[24] I consider the effects of the mandates on children aged 6-13 years old, but my results are robust to focusing only on elementary-school-aged children (Table A.8).

Table 7 shows the effect of the mandates on the number of children receiving special education. Column 1 shows no change in the average number of children requiring special education after a mandate is introduced. However, Columns 2-5 show that two opposing mechanisms are at play after the introduction of a mandate. On the one hand, the number of children requiring special education increases after the mandates among those cohorts that were above six years of age when the mandate was introduced. In other words, the mandates seem to increase awareness of disturbances and disabilities and to induce more parents to have their children screened for disabilities. To the best of my knowledge, the introduction of the abatement mandates does not coincide with other state-level campaigns that affect the likelihood of children being classified as requiring special education. Moreover, controlling for state-year fixed effects in Column 6, accounts for state-level policy changes that affect the whole risk set for special education classification. In contrast, the mandates decrease the number of children requiring special education among those cohorts that were young enough to benefit from a safer home environment. My preferred estimate indicates a decrease in the number of children served under IDEA of 7.3%, a result that is robust across many specifications.[25] In particular, Column 5 controls for the maximum size of the benefit that a family of three, i.e., a single parent and two children, is entitled to in a given state and year under the Temporary Assistance for Needy Families and the Aid to Families with Dependent Children programs.[26] Indeed, screening for special education requirements is free

[24] Cullen (2003) implies that funding rules, and hence schools' incentives to classify students as requiring special education, might vary across districts within a state. Data at a finer geographic level would allow me to control for this.

[25] Column 6 of Appendix Table A.9 shows that the mandates decrease the rate of special education enrollment by 8%.

[26] Following (Duggan & Kearney 2007), the AFDC/TANF data were obtained from various years of the publications Ways

of charge in public schools, and children are often screened for special education needs before applying for child disability from the SSA. Since Cohen (2007) shows that larger benefits from these state-administered programs reduce incentives to apply for child disability from the SSA, we would expect the coefficient on the benefit variable to be significantly negative. However, given that the outcome variable in Table 7 does not capture the flow of children into special education services, the variation in benefit levels has low predictive power.

The left panel of Figure 5 confirms that cohorts that are exposed to the mandates for longer periods experience the highest reduction in special education needs, while children who are already above six years of age when the mandate is introduced do not benefit at all. Notably, I cannot disentangle the effect of time elapsed since the introduction of the mandate and the pure effect of exposure: younger cohorts are more exposed to the mandates, and when they are born the mandates have been in place for longer, meaning that more houses have been abated. The right panel of Figure 5 shows that the mandates affect only the rates of disabilities that are related to lead poisoning, as shown in Columns 7 and 8 of Table 7.[27] Indeed, Table 8 shows that the mandates mainly reduce the number of children with speech or language impairments, a disability that affects the largest share of children in elementary school.

To learn about the fiscal savings on special education spending generated by the mandates, one would need to know the steady-state effects of these regulations. The effects of the mandates on the cohorts that were not born at the time of the introduction of these regulations provide an estimate of these steady-state effects. Figure 5 shows that, on average, 25% fewer children born after a mandate require special education in a given grade, with confidence interval ranging between 7% and 43%, off an average population of children requiring special education in control states of 8,706 children. Chambers et al. (2002) estimate that, in 2000, the additional spending to educate a student with a disability amounted to $5,918: therefore, the mandates induce savings in special education programs that total between $3.7 million and $22 million per state-grade on average. Assuming constant effects, this back of the envelope calculation suggests that the mandates can generate savings on special education for elementary and middle school between $29 million and $176 million per state-cohort on average. Moreover, school districts in areas where lead poisoning is more prevalent are likely to save the most from the decreased expenditure on special education, as districts have been increasingly burdened by the rising enrollment in special education services, while federal and state-level funding has not kept up with the increased needs (Parrish 2001). Hence, the mandates might make more resources available for general education expenditures for control states with a housing stock that is similar to the housing stock in the implementing states.

and Means Committee Overview of Entitlement Programs (Green Book).

[27] In these regressions, each disability is weighted by the number of children affected by it.

Figure 1 shows a narrowing of the racial disparity in special education over time both for disabilities that can be related to lead poisoning and for other disabilities. Hence, it seems natural to ask whether the abatement mandates contributed to this trend. Unfortunately, US DE only publishes counts of children on special education by disability and race aggregating children aged six to 21. Hence, to answer this question I estimate variations of the following specification:

$$Y_{rst} = \alpha Black_r + \beta_0 Exposure_{st} + \beta_1 Exposure_{st} * Black_r + \pi X_{rst} + \gamma_s + \delta_t + \epsilon_{st} \tag{5}$$

where $Y_{rst}$ is the logarithm of the number of children served under IDEA part B of race $r \in \{Black, White\}$ in year $t$ in state $s$, $Exposure_{st}$ is the share of children aged six to 21 in state $s$ in year $t$ that were exposed to the mandate, $X_{rst}$ is state's population of children below 72 months of age of race $r$, and $\gamma_s$ and $\delta_t$ are state and time fixed effects. Table 9 shows that classification in special education does not appear to change differentially by race with exposure to the mandates. Unfortunately, the lack of data disaggregated both by race and by birth cohort provides low statistical power to this test. Nonetheless, one potential explanation for these results is that the racial bias in the assessment process for special education dominates the relative gains for black students from the mandates (Harry et al. 2002).

# 7 Education Effects

The previous section estimates large reductions in the number of children served under IDEA after the introduction of lead abatement mandates. It is therefore natural to ask whether the reduced number of children requiring special education translates into improved educational outcomes on average. The mandates may affect educational attainment in at least three ways. First, by reducing lead poisoning, the regulations increase IQ in the whole population. Moreover, there might be spillover effects for children without disabilities if teachers can cover more materials. Third, by freeing resources from funding for special education, the mandates may improve schooling provision for the average student. To test these hypotheses, I estimate equation (3) in a difference-in-differences framework, with NAEP scores in mathematics and reading as outcome variables.

Tables 10 and 11 show no evidence that the mandates improve test scores in either mathematics or reading. As discussed in Section 3, school personnel decide whether to include students with disabilities in NAEP assessments based on information in a student's Individualized Education Program (IEP). In Section 6, I find that the mandates reduce the number of children requiring special education for speech or language impairments, a relatively mild form of disability that is not likely to prevent a child from being included in

the NAEP sample. However, if the mandates affect the ability of the marginal child who takes the exam, test scores might decrease for cohorts exposed to the mandates.

Aizer et al. (2015) hypothesize that environmental inequality can explain the observed inequality in educational attainments. Indeed, they show that a policy that required deleading of rental units in Rhode Island significantly reduced the black-white test score gap in the state. Hence, here I investigate whether the average mandate differentially improves test scores for black students. Figures 6 and 7 show that the black-white test score gap for both mathematics and reading has decreased over time (left panel). However, there is no evidence that this gap decreased differentially for cohorts exposed to the abatement mandates. Moreover, the mandates appear to decrease test scores among black students in specifications that do not allow for state and year fixed effects to differ for black and white students. This is in sharp contrast with the findings in Aizer et al. (2015), who estimate that the Rhode Island mandate explains between 37% and 76% of the decrease in the black-white test score gap in the state, although the large standard errors in my estimates cannot rule out the effects found by Aizer et al. (2015). This discrepancy might be due to several reasons. On the one hand, Aizer et al. (2015) compare test scores for black and white students at the tract-level, gaining identification from differential abatement rates across census tracts, while the state-level NAEP data does not allow me to investigate whether children of more disadvantaged areas benefit differentially from the policies. Moreover, the more granular Rhode Island data allow for identification of the effects of the decreased lead poisoning rates on the whole distribution of test scores. On the other hand, if enforcement of the regulations is stricter in Rhode Island than it is in the average state, the impact of the average mandate on test scores would be smaller than the impact estimated for Rhode Island. Therefore, more research is warranted to understand what factors drive the different results.

Reyes (2015*b*) estimates that lead poisoning leads to adverse behavioral outcomes, such as behavior problems as a child, pregnancy and aggression as a teen, and criminal behavior as a young adult. However, to the best of my knowledge, the impact of lead poisoning on disciplinary actions is relatively understudied. Indeed, classroom behavior is another channel through which lead poisoning can affect educational achievement. However, Table 12 finds no effect of the mandates on the number of children receiving corporal punishment, suspensions, or expulsions at the state level.

# 8 Conclusion

This paper exploits the variation in timing among state-level lead abatement mandates to estimate the policies' impact on children's health and medical and educational expenditures in a DD framework. Although the mandates do not appear to significantly affect fertility and infant health outcomes on average, they appear

to decrease birth rates in counties with a high share of old houses. This is consistent with families delaying childbearing due to the loss in property values and the higher rental expenditure induced by the mandates (Gazze 2016). In counties with a high share of old houses, birth oucomes appear to be better than in the average county after a mandate is introduced, consistent with a change in the marginal child born and with beneficial effects of the mandates in terms of lower lead exposure. Indeed, the mandates reduce the number of children with elevated blood lead levels by 26%, resulting in a decrease in the number of children requiring special education by 7.3%. A back of the envelope calculation suggests that this decrease in child disability results in savings for the government that total between $29 million and $176 million per state-cohort on average. This figure updates the existing figure that one in five children with BLL above $25\mu g/dL$ requires three years of special education at a cost $38,199 (Korfmacher 2003), which implies savings from the mandates of about $650,000 per state on average. In particular, my calculations take into account that lead poisoning causes child disabilities at levels lower that $25\mu g/dL$.

It is more complicated to estimate the costs of the mandates for state governments. Enforcement requires resources in terms of inspectors, contractors and administrative tools to track compliant units. In addition, several states provide subsidies for abatement in the form of subsidized loans or grants for qualified units. For instance, since 1997 Massachusetts has made available almost $94 million through its "Get the Lead Out" program, of which almost $86 million have been utilized. Hence, the savings in spending on special education services might outweigh the costs of subsidized deleading.

In related work, I estimate that the average loss in home values per lead poisoning averted for current property owners totals $1.4 million, while the transfer from families with small children to property owners in terms of higher rents totals $150,000 (Gazze 2016). The social welfare benefits from the mandates, however, are more difficult to compute, as the benefits from reduced crime or improved mental health of parents are hard to quantify. Excluding these figures, the total benefits from the mandates, taking into account the updated figure for special education, reach $138,000 per lead poisoning averted. In fact, Column 3 of Appendix Table A.3 reports savings in health care expenditure from Gould (2009). Moreover, the mandates benefit society by increasing tax revenues by $16,460 per child from the increased lifetime earnings, calculated in Column 6 of Appendix Table A.3 assuming an income tax rate of 15% (Gould 2009). However, these figures do not include the gains from reduced criminal behavior and reduced morbidity for lack of data. As such, this paper cannot provide a definitive answer concerning the impact of an abatement mandate on social welfare; nonetheless, it appears that the mandates in their current format do not pay for themselves. However, there might be benefits to using the savings induced from the reduced number of lead poisoned children to fund the abatement of at-risk properties in a more focused manner that minimizes adverse housing market effects and adverse effects on mental health and households' budget constraint.

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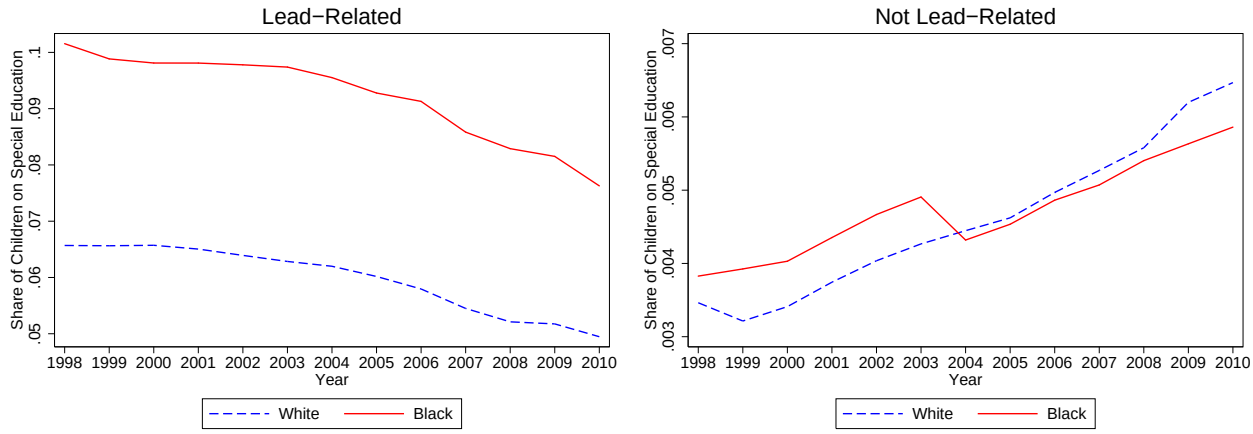
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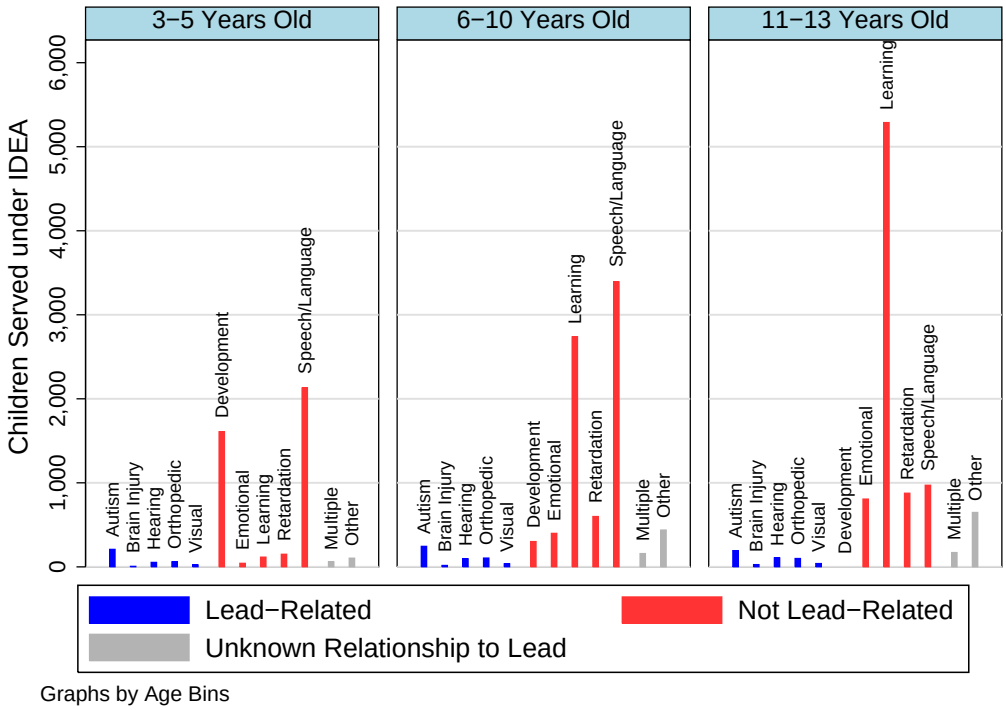
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Figure 1: Distribution of Children Served under IDEA by Age Group and Disability



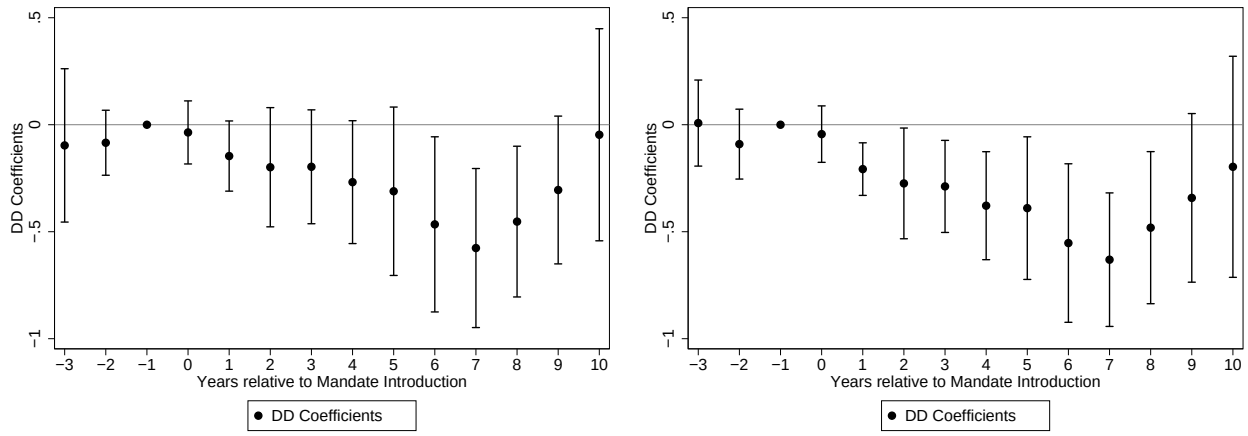

Source: US ED, OSEP. The figure plots the average number of children aged six to 21 served by a state each year under IDEA in each disability category by race for the years 1998-2010. I classify disabilities as being potentially related to lead poisoning (left panel) and not related to lead (right panel) according to the public health literature (see, e.g., Kaiser et al. (2008)) and conversations with medical professionals with years of experience in working with lead-poisoned children.

Figure 2: Distribution of Children Served under IDEA by Age Group and Disability



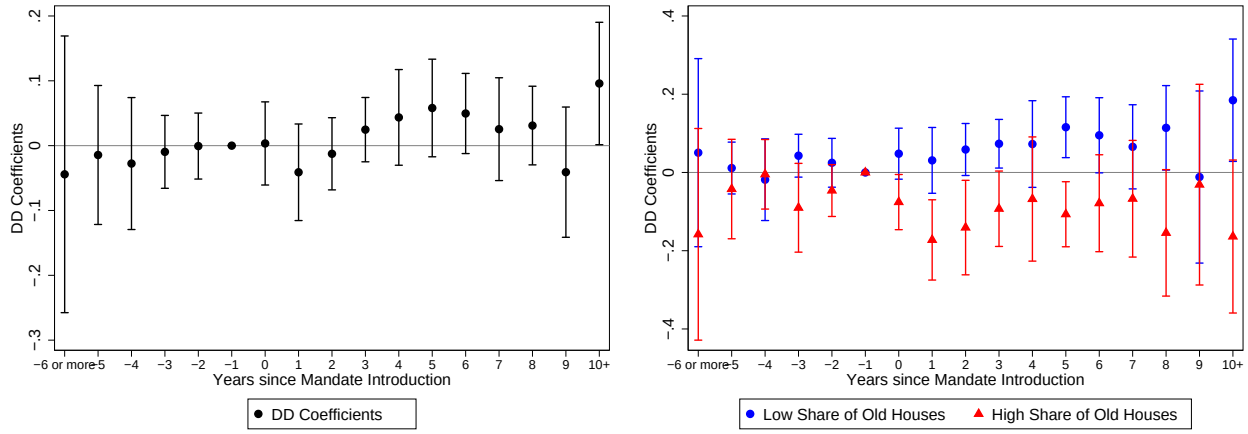

Source: US ED, OSEP. The figure plots the average number of children served by a state each year under IDEA in each disability category by school-age groups for the years 1990-2010. I classify disabilities as being potentially related to lead poisoning (red bars), not related to lead (blue bars), or unclear (gray bars) according to the public health literature (see, e.g., Kaiser et al. (2008)) and conversations with medical professionals with years of experience in working with lead-poisoned children.

Figure 3: Lead Poisoning Effects, Counts of BLLs above $10\mu g/dL$, Alternative Specifications



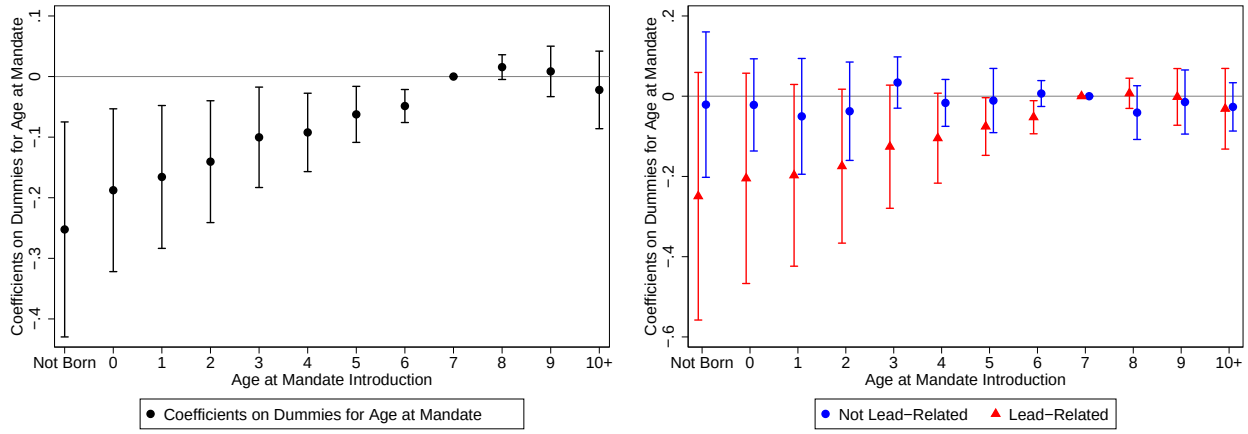

The figure plots DD coefficients on year-by-year mandate dummies estimated on the CDC sample for the years 1997-2012, restricted to all non-implementing states and states that implement lead-paint abatement mandates after 1997. The outcome variable is the logarithm of number of children with BLL above $10\mu g/dL$. Logarithm of state population below 72 months of age and state and year fixed effects are included in both panels. In addition, the right figure includes differential trends for states with high initial rates of lead poisoning. $T = 0$ is the year the mandate was introduced. $T = -1$ is the omitted category. The vertical bars are lower and upper bounds of 95% confidence interval. Standard errors are clustered at the state level (24 clusters).

Figure 4: Effect of the Mandates on Fertility



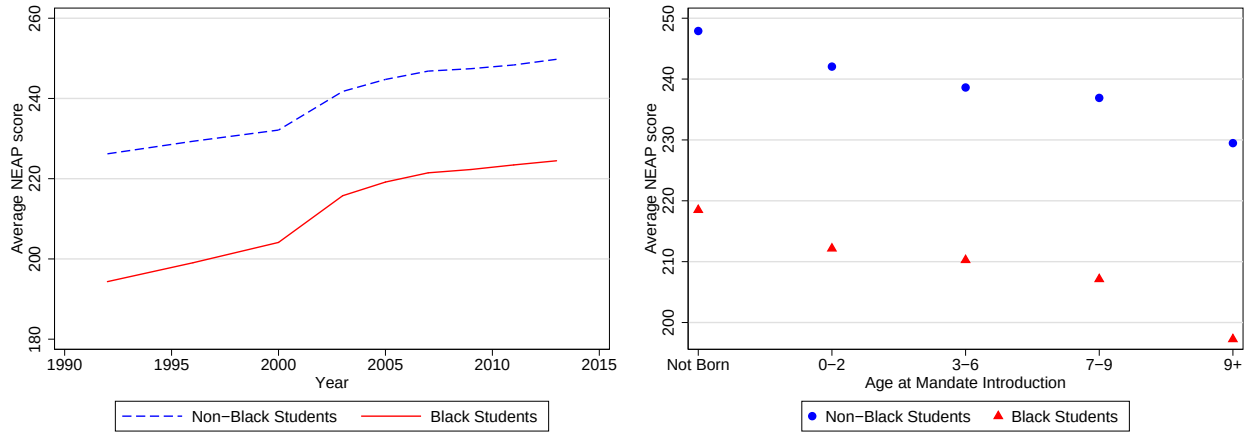

Source: Source: NCHS. The figure plots the year-by-year DD coefficients on the Birth Cohort Linked Birth and Infant Death Data of the National Vital Statistics System, 1989-1991 and 1995-2010 (2004 is missing). The dependent variable is the logarithm of the number of births in each state and year. The logarithm of county population and county and year fixed effects are included. The left panel plots the coefficients separately for counties with above (red triangles) and below (blue dots) median share of houses built prior to 1978. $T = 0$ is the year the mandate was introduced. $T = -1$ is the omitted category. The bars indicate confidence intervals at the 95% level with standard errors clustered at state level.

Figure 5: Effect of the Mandates on Special Education, by Age at Mandate



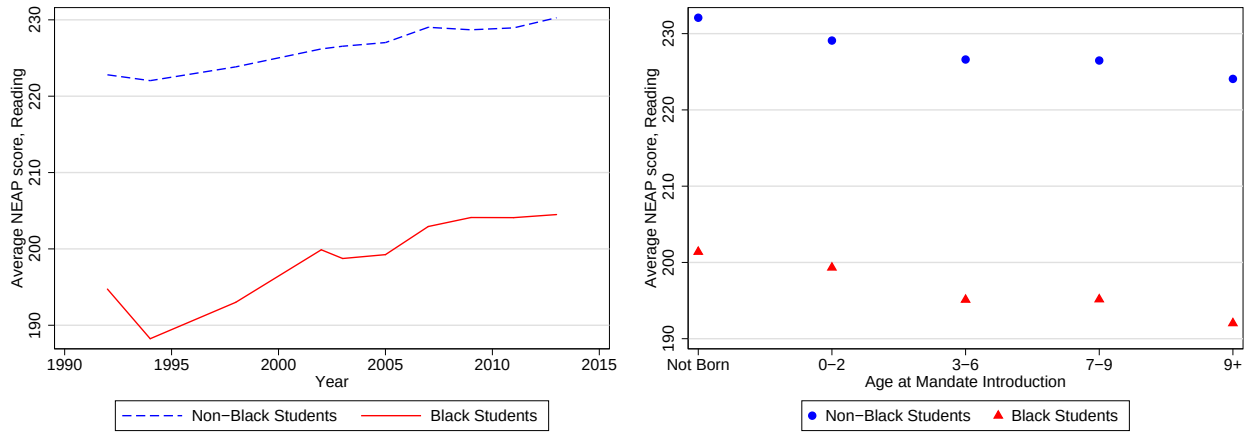

Source: US ED, OSEP. The figure plots the DD coefficients by exposure to the mandate on the IDEA sample of children aged 6-13, for the years 1990-2010. State-year and cohort fixed effects are included. Age 7 at the introduction of the mandate is the omitted category. The bars indicate confidence intervals at the 95% level with standard errors clustered at state level.

Figure 6: The Black-White Test Score Gap over Time, Mathematics




Source: NAEP. The figure plots the average mathematics test scores for black (red) and white (blue) students over time (left panel) and by exposure to the abatement mandates (right panel) for fourth graders over the years 1992-2013.

Figure 7: The Black-White Test Score Gap over Time, Reading




Source: NAEP. The figure plots the average reading test scores for black (red) and white (blue) students over time (left panel) and by exposure to the abatement mandates (right panel) for fourth graders over the years 1992-2013.

Table 1: State-Level Abatement Mandates

| State | Enactment Year | Rentals Only | Trigger | Coverage |
|---|---|---|---|---|
| (1) | (2) | (3) | (4) | (5) |
| CT | 1992 | No | <6 Year-old | All |
| DC | 1983 | No | <8 Year-old | All |
| GA | 2000 | Yes | <6 Year-old with EBLL | Multifamily >12 units |
| IL | 1992 | No | Children | All |
| KY | 1974 | No | Children | All |
| LA | 1988 | No | <6 Year-old | All |
| MA | 1971 | No | <6 Year-old | All |
| MD | 1995 | Yes | N/A | All |
| ME | 1991 | No | <6 Year-old | All |
| MI | 2005 | Yes | N/A | All |
| MN | 1991 | No | Child with EBLL | All |
| MO | 1993 | No | <6 Year-old | All |
| NC | 1989 | No | <6 Year-old with EBLL | All |
| NH | 1993 | Yes | <6 Year-old with EBLL | All |
| NJ | 1971 | No | Children | All |
| OH | 2003 | No | <6 Year-old with EBLL | All |
| RI | 2002 | Yes | N/A | All |
| SC | 1979 | No | Children | All |
| VT | 1996 | Yes | N/A | All |

The table displays the timeline of the introduction of abatement mandates in the 19 implementing states together with the main characteristics of each mandate. Column 2 reports the mandates' enactment year. Columns 3-5 characterize whether the mandate covers only rental properties, what triggers a lead order, and whether the type of properties covered by the mandate.

Table 2: Links between Disabilities and Lead Poisoning

| Disability (1) | Relationship to Lead Poisoning (2) |
|---|---|
| Autism | Not Lead-Related |
| Deaf-Blindness | Not Lead-Related |
| Hearing Impairments | Not Lead-Related |
| Orthopedic Impairments | Not Lead-Related |
| Traumatic Brain Injury | Not Lead-Related |
| Visual Impairments | Not Lead-Related |
| Developmental Delay [(a)] | Lead-Related |
| Emotional Disturbance | Lead-Related |
| Mental Retardation | Lead-Related |
| Speech or Language Impairments | Lead-Related |
| Specific Learning Disabilities | Lead-Related |
| Multiple Disabilities | Unknown Relationship to Lead |
| Other Health Impairments | Unknown Relationship to Lead |

Source: Author's classification. I classify disabilities as being potentially related to lead poisoning, not related to lead, or unclear according to the public health literature (see, e.g., Kaiser et al. (2008)) and conversations with medical professionals with years of experience in working with lead-poisoned children. [(a)] The Developmental Delay definition was introduced in 1997.

Table 3: Lead Poisoning Effects, Rates of Elevated Blood Lead Levels

| Dependent Variable: | Log Rates of Children with BLL>10ug/dL | | | Log Rates of Children with BLL>20ug/dL | | |
|---|---|---|---|---|---|---|
| | State, Year FE | Differential Trends by Baseline BLL | Differential Trends for Implementing States | State, Year FE | Differential Trends by Baseline BLL | Differential Trends for Implementing States |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *Panel A: Full Sample* | | | | | | |
| Post-Mandate | -0.383** | -0.405** | -0.403** | -0.385*** | -0.421*** | -0.376** |
| | (0.152) | (0.165) | (0.165) | (0.134) | (0.142) | (0.151) |
| Log Children <72 Months | -1.866* | -2.808*** | -1.763* | -1.734* | -2.558** | -1.782* |
| | (1.030) | (1.067) | (0.998) | (1.003) | (1.090) | (0.981) |
| N | 561 | 543 | 561 | 560 | 543 | 560 |
| Avg EBLL Rates, Non-Implementing States | 0.021 | 0.021 | 0.021 | 0.005 | 0.005 | 0.005 |
| Avg EBLL Rates, Implementing States Pre-Period | 0.023 | 0.023 | 0.023 | 0.006 | 0.006 | 0.006 |
| *Panel B: Mandates after 1996 Only (24 States)* | | | | | | |
| Post-Mandate | -0.259*** | -0.316*** | -0.163 | -0.310*** | -0.373*** | -0.216* |
| | (0.086) | (0.090) | (0.120) | (0.115) | (0.122) | (0.110) |
| Log Children <72 Months | -2.668** | -3.521*** | -2.702** | -2.347 | -2.834** | -2.380 |
| | (1.348) | (1.293) | (1.338) | (1.521) | (1.272) | (1.511) |
| N | 345 | 327 | 345 | 344 | 327 | 344 |
| Avg EBLL Rates, Non-Implementing States | 0.021 | 0.021 | 0.021 | 0.005 | 0.005 | 0.005 |
| Avg EBLL Rates, Implementing States Pre-Period | 0.015 | 0.015 | 0.015 | 0.002 | 0.002 | 0.002 |

Notes: *** p<0.01, ** p<0.05, * p<0.1. The table presents DD estimates on the BLL sample from CDC for the years 1997-2012. The dependent variable is the logarithm of the rate BLLs above $10\mu g/dL$ over the number of children screened for lead poisoning in Columns 1-3 and the logarithm of the rate BLLs above $20\mu g/dL$ over the number of children screened for lead poisoning in Columns 4-6. The logarithm of state population below 72 months of age and state and year fixed effects are included in all columns. In addition, Columns 2 and 5 include differential linear trends for states with initial outcome levels below and above the median, while Columns 3 and 6 include differential linear time trends for implementing states. Panel A reports estimates on the full sample of states, Panel B reports estimates on the subsample of states that implement lead-paint abatement mandates after 1997 and all non-implementing states. Average rates of BLLs above $10\mu g/dL$ or $20\mu g/dL$ in non-implementing and implementing states (pre-period only) is shown at the bottom of each column. Standard errors clustered at the state level are shown in parentheses.

Table 4: Lead Poisoning Effects, Counts of Elevated Blood Lead Levels

| Dependent Variable: | Log Number of Children with BLL>10ug/dL | | | Log Number of Children with BLL>20ug/dL | | |
|---|---|---|---|---|---|---|
| | State, Year FE | Differential Trends by Baseline BLL | Differential Trends for Implementing States | State, Year FE | Differential Trends by Baseline BLL | Differential Trends for Implementing States |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *Panel A: Full Sample* | | | | | | |
| Post-Mandate | -0.177 | -0.177 | -0.102 | -0.184 | -0.192 | -0.081 |
| | (0.158) | (0.110) | (0.163) | (0.179) | (0.130) | (0.181) |
| Log Children <72 Months | 2.303*** | 0.341 | 1.909*** | 2.381*** | 0.591 | 1.842** |
| | (0.727) | (0.539) | (0.706) | (0.768) | (0.603) | (0.770) |
| N | 561 | 543 | 561 | 560 | 543 | 560 |
| Avg Number of EBLL, Non-Implementing States | 1372 | 1433 | 1372 | 253 | 262 | 253 |
| Avg Number of EBLL, Implementing States Pre-Period | 1692 | 1692 | 1692 | 310 | 310 | 310 |
| *Panel B: Mandates after 1996 Only (24 States)* | | | | | | |
| Post-Mandate | -0.190 | -0.263** | -0.159 | -0.244* | -0.320** | -0.222 |
| | (0.135) | (0.126) | (0.169) | (0.144) | (0.132) | (0.162) |
| Log Children <72 Months | 3.142*** | 0.833 | 3.131*** | 3.383*** | 1.519 | 3.375*** |
| | (0.997) | (1.102) | (0.983) | (1.049) | (0.997) | (1.032) |
| N | 345 | 327 | 345 | 344 | 327 | 344 |
| Avg Number of EBLL, Non-Implementing States | 1372 | 1433 | 1372 | 253 | 262 | 253 |
| Avg Number of EBLL, Implementing States Pre-Period | 1287 | 1287 | 1287 | 236 | 236 | 236 |

Notes: *** p<0.01, ** p<0.05, * p<0.1. The table presents DD estimates on the BLL sample from CDC for the years 1997-2012. The dependent variable is the logarithm of number of children with BLL above $10\mu g/dL$ in Columns 1-3 and the logarithm of number of children with BLL above $20\mu g/dL$ in Columns 4-6. The logarithm of state population below 72 months of age and state and year fixed effects are included in all columns. In addition, Columns 2 and 5 include differential linear trends for states with initial outcome levels below and above the median, while Columns 3 and 6 include differential linear time trends for implementing states. Panel A reports estimates on the full sample of states, Panel B reports estimates on the subsample of states that implement lead-paint abatement mandates after 1997 and all non-implementing states. Average number of children with BLL above $10\mu g/dL$ or $20\mu g/dL$ in non-implementing and implementing states (pre-period only) is shown at the bottom of each column. Standard errors clustered at the state level are shown in parentheses.

Table 5: Effects of the Mandates on Fertility

| Dependent Variable: | Log Number of Births | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Post-Mandate | 0.017 | -0.040 | 0.046 | 0.038 | 0.053 |
| | (0.041) | (0.029) | (0.053) | (0.078) | (0.081) |
| Post-Mandate, Counties with Old Housing Stock | | | | 0.014 | -0.022 |
| | | | | (0.081) | (0.082) |
| N | 47283 | 47283 | 47238 | 47238 | 47238 |
| Avg Births, Control Counties | 1555 | 1555 | 1555 | 1555 | 1555 |
| Avg Births, Treated Counties Pre-Mandate | 1532 | 1532 | 1532 | 1532 | 1532 |
| State FE | X | X | | | |
| Baseline Controls Interacted with Year FE | | X | | | |
| County FE | | | X | X | X |
| Trend for High-Pre1978-Share Counties | | | | | X |

Notes: *** p<0.01, ** p<0.05, * p<0.1. Source: NCHS. The table presents DD estimates on the Birth Cohort Linked Birth and Infant Death Data of the National Vital Statistics System for the years 1989-1991 and 1995-2010 (2004 is missing). The dependent variable is the logarithm of the number of births in each county and year. Counties with old housing stock are counties with above median share of houses built prior to 1978. Year fixed effects and the logarithm of the county population in each year are included in all columns. Column 2 includes state-level controls from the 1980 Census interacted with year dummies (share of high school dropouts, share of high school graduates, share of individuals with less than four years of college, share of blacks, share of Hispanics, unemployment rate, and share of people below the poverty line). Columns 3-5 include county fixed effects. Column 5 includes a trend for counties with old housing stock. The average number of births in control and pre-mandate treated states is shown at the bottom of each column. Standard errors clustered at state level are shown in parentheses.

Table 6: Effects of the Mandates on Infant Health

| Dependent Variable: | Probability of Death by Internal Cause (*1,000) (1) | Log Birth Weight (2) | Probability of Birth Weight ≤2500 (3) | Probability of Birth Weight ≤1500 (4) | Apgar Score at 5 Minutes (5) | Number of Gestational Weeks (6) |
|---|---|---|---|---|---|---|
| *Panel A: Average Effects* | | | | | | |
| Post-Mandate | -0.038 | -0.002 | 0.001 | 0.001** | -0.021 | -0.043 |
| | (0.2357) | (0.001) | (0.001) | (0.000) | (0.021) | (0.031) |
| N | 72913680 | 72877496 | 73241480 | 73241480 | 61004580 | 72683304 |
| Mean Outcome, Control Counties | 5.700 | 3299.767 | 0.076 | 0.014 | 8.896 | 38.772 |
| Mean Outcome, Treated Counties Pre-Mandate | 8.004 | 3325.824 | 0.078 | 0.015 | 8.910 | 38.948 |
| *Panel B: Effects by Age of the Housing Stock* | | | | | | |
| Post-Mandate | -0.002 | -0.002 | 0.002 | 0.001 | -0.046** | -0.069** |
| | (0.4876) | (0.002) | (0.002) | (0.001) | (0.020) | (0.032) |
| Post-Mandate, Counties with Old Housing Stock | 0.003 | 0.001 | -0.001 | 0.000 | 0.044* | 0.033 |
| | (0.4876) | (0.003) | (0.003) | (0.001) | (0.026) | (0.040) |
| N | 72913680 | 72877496 | 72877496 | 72877496 | 60641356 | 72319584 |
| Mean Outcome, Control Counties | 5.700 | 3299.767 | 0.076 | 0.014 | 8.896 | 38.772 |
| Mean Outcome, Treated Counties Pre-Mandate | 8.004 | 3325.824 | 0.078 | 0.015 | 8.910 | 38.948 |

Notes: *** p<0.01, ** p<0.05, * p<0.1. Source: NCHS. The table presents DD estimates on the Birth Cohort Linked Birth and Infant Death Data of the National Vital Statistics System for the years 1989-1991 and 1995-2010 (2004 is missing). The dependent variable is indicated in each column. Counties with old housing stock are counties with above median share of houses built prior to 1978. County and year fixed effects, and a trend for counties with old housing stock are included in all columns. The mean outcome (in levels) in control and pre-mandate treated states is shown at the bottom of each column. Standard errors clustered at state level are shown in parentheses.

Table 7: Effects of the Mandates on Special Education Needs

| Dependent Variable: | Log Number of Children Aged 6-13 Receiving Special Education | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| Sample: | All Disabilities Combined | | | | | | Lead-Related | Not Lead-Related |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Post-Mandate | 0.000 | 0.089*** | 0.042* | 0.044*** | 0.042* | | | |
| | (0.032) | (0.023) | (0.024) | (0.015) | (0.024) | | | |
| Exposed | | -0.144*** | -0.069** | -0.066*** | -0.068** | -0.073** | -0.081* | 0.016 |
| | | (0.034) | (0.030) | (0.025) | (0.030) | (0.036) | (0.041) | (0.029) |
| Maximum TANF/AFDC Benefit for Family of 3 | | | | | -0.0002 | | | |
| | | | | | (0.0003) | | | |
| Log Number of Children | 0.683*** | 0.672*** | 0.387** | 0.465*** | 0.372* | 0.011 | -0.078 | 0.551*** |
| | (0.152) | (0.150) | (0.178) | (0.115) | (0.191) | (0.303) | (0.332) | (0.178) |
| N | 8552 | 8552 | 8552 | 8552 | 8552 | 8552 | 8552 | 8536 |
| Average Number of Kids on SpEd, Non-Exposed Cohorts | 8706 | 8706 | 8706 | 8706 | 8706 | 8706 | 7618 | 464 |
| Average Number of Kids on SpEd, Exposed Cohorts, Treated States | 8572 | 8572 | 8572 | 8572 | 8572 | 8572 | 7194 | 482 |
| State, Year FE | X | X | X | X | X | | | |
| Cohort FE | | | X | X | X | X | X | X |
| State-specific Linear Trends | | | | X | | | | |
| State-Year FE | | | | | | X | X | X |

Notes: *** p<0.01, ** p<0.05, * p<0.1. Source: US ED, OSEP. Population data by age, state and year was obtained from SEER. The table presents DD estimates on the IDEA sample of children aged 6-13 for the years 1990-2010. Each observation is a state-year-age cell. The dependent variable is the logarithm of number of children receiving special education under IDEA, Part B, for any disability (Columns 1-6), for lead-related disabilities only (Column 7) and for disabilities not related to lead poisoning (Column 8). The fixed effects included in each specifications are indicated at the bottom of each column. The average number of children receiving special education under IDEA, Part B for exposed (post-period only) and non-exposed cohorts is shown at the bottom of each column. Standard errors clustered at state level are shown in parentheses.

Table 8: Effects of the Mandates on Special Education Needs, by Disability

| Dependent Variable: | Log Number of Children Aged 6-13 Receiving Special Education, by Disability | | | |
|---|---|---|---|---|
| Sample: | Mental Retardation | Speech or Language Impairments | Emotional Disturbance | Specific Learning Disabilities |
| | (1) | (2) | (3) | (4) |
| Exposed | 0.013 | -0.224** | 0.013 | -0.022 |
| | (0.047) | (0.112) | (0.054) | (0.082) |
| Log Number of Children | 0.855** | 0.295 | 0.854*** | 1.037** |
| | (0.379) | (0.560) | (0.281) | (0.525) |
| N | 8521 | 8482 | 8505 | 8545 |
| Average Number of Kids on SpEd, Non-Exposed Cohorts | 706 | 2447 | 560 | 3856 |
| Average Number of Kids on SpEd, Exposed Cohorts, Treated States | 705 | 2614 | 539 | 3197 |

Notes: *** p<0.01, ** p<0.05, * p<0.1. Source: US ED, OSEP. Population data by age, state and year was obtained from SEER. The table presents DD estimates on the IDEA sample of children aged 6-13 for the years 1990-2010. Each observation is a state-year-age cell. The dependent variable is the logarithm of number of children receiving special education under IDEA, Part B, for the particular disability indicated in each column. State-year and cohort fixed effects are included. The average number of children receiving special education under IDEA, Part B for exposed (post-period only) and non-exposed cohorts is shown at the bottom of each column for each disability. Standard errors clustered at state level are shown in parentheses.

Table 9: Effects of the Mandates on Special Education Needs, By Race

| Dependent Variable: | Log Number of Children Aged 6-21 Receiving Special Education | | | | | |
|---|---|---|---|---|---|---|
| Sample: | Lead Related | | | Not Lead-Related | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Exposure | -0.018 | -0.019 | | 0.410*** | 0.409*** | |
| | (0.160) | (0.158) | | (0.137) | (0.137) | |
| Black | 0.776*** | 0.776*** | 0.780*** | 0.283** | 0.283** | 0.287* |
| | (0.143) | (0.143) | (0.198) | (0.117) | (0.117) | (0.159) |
| Exposure*Black | 0.097 | 0.097 | 0.096 | -0.007 | -0.007 | -0.026 |
| | (0.221) | (0.221) | (0.304) | (0.145) | (0.145) | (0.189) |
| Maximum TANF/AFDC Benefit for Family of 3 | | 0.0001 | | | 0.0001 | |
| | | (0.0002) | | | (0.0004) | |
| Log Number of Children | 1.154*** | 1.154*** | 1.156*** | 1.105*** | 1.105*** | 1.108*** |
| | (0.060) | (0.060) | (0.083) | (0.049) | (0.049) | (0.066) |
| N | 1322 | 5345 | 5345 | 5345 | 5345 | 5345 |
| Average Number of Kids on SpEd, Non-Exposed Cohorts | 34924 | 8353 | 8353 | 8353 | 8353 | 8353 |
| Average Number of Kids on SpEd, Exposed Cohorts, Treated States | 57039 | 8342 | 8342 | 8342 | 8342 | 8342 |
| State, Year FE | X | X | | X | X | |
| State-Year FE | | | X | | | X |

Notes: *** p<0.01, ** p<0.05, * p<0.1. Source: US ED, OSEP. Population data by age, race, state and year was obtained from SEER. The table presents DD estimates on the IDEA sample of children aged 6-21 for the years 1998-2010. Each observation is a state-year-race cell. The dependent variable is the logarithm of number of children receiving special education under IDEA, Part B, for lead-related disabilities only (Columns 1-3) and for disabilities not related to lead poisoning (Columns 4-6). The fixed effects included in each specifications are indicated at the bottom of each column. The average number of children receiving special education under IDEA, Part B for control states and treated states (post-mandate only) is shown at the bottom of each column. Standard errors clustered at state level are shown in parentheses.

Table 10: Effects of the Mandates on Test Scores, Mathematics

| Dependent Variable: | Standardized Test Scores, Mathematics | | | | | | |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Exposed | 0.024 | 0.029 | 0.026 | 0.120 | 0.107 | 0.101 | -0.011 |
| | (0.076) | (0.086) | (0.084) | (0.104) | (0.120) | (0.093) | (0.086) |
| Not Born At Mandate | | | 0.029 | | | 0.012 | |
| | | | (0.060) | | | (0.112) | |
| Exposed, Black | | | | -0.227 | -0.228 | -0.218** | 0.053 |
| | | | | (0.167) | (0.175) | (0.098) | (0.050) |
| Black | | | | -1.496*** | -1.493*** | -1.493*** | |
| | | | | (0.051) | (0.054) | (0.054) | |
| Not Born At Mandate, Black | | | | | | -0.012 | |
| | | | | | | (0.206) | |
| Log Number of Children | 0.459* | 0.198 | 0.205 | 0.338* | 0.376 | 0.377 | 0.213 |
| | (0.266) | (0.284) | (0.284) | (0.178) | (0.296) | (0.295) | (0.273) |
| N | 801 | 801 | 801 | 801 | 801 | 801 | 801 |
| Mean Test Score, Control States | -0.027 | -0.027 | -0.027 | -0.027 | -0.027 | -0.027 | -0.027 |
| Mean Test Scores, Non-Exposed Cohorts, Treated States | -0.642 | -0.642 | -0.642 | -0.642 | -0.642 | -0.642 | -0.642 |
| State, Year FE | X | X | X | X | X | X | |
| State-specific Linear Trends | | X | X | | X | X | X |
| State-Black, Year-Black FE | | | | | | | X |

Notes: *** p<0.01, ** p<0.05, * p<0.1. Source: NEAP. Population data from SEER. The table presents DD estimates on the sample of fourth graders for the years 1992-2013. Each observation is a state-year cell. The dependent variable is the standardized NEAP mathematics score. State and year fixed effects are included in each column except for Column 7, which includes state-black and year-black fixed effects. In addition, Columns 2, 3, 5, and 6 include state-specific trends. The average standardized test score in control states and treated states (pre-mandate) is shown at the bottom of each column. Standard errors clustered at state level are shown in parentheses.

Table 11: Effects of the Mandates on Test Scores, Reading

| Dependent Variable: | Standardized Test Scores, Reading | | | | | | |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Exposed | 0.050 | 0.092 | 0.082 | 0.198 | 0.181 | 0.143 | 0.008 |
| | (0.083) | (0.083) | (0.081) | (0.122) | (0.131) | (0.093) | (0.078) |
| Not Born At Mandate | | | 0.064 | | | 0.074 | |
| | | | (0.067) | | | (0.113) | |
| Exposed, Black | | | | -0.306 | -0.312 | -0.245*** | 0.081 |
| | | | | (0.215) | (0.222) | (0.090) | (0.062) |
| Black | | | | -1.679*** | -1.673*** | -1.673*** | |
| | | | | (0.057) | (0.059) | (0.060) | |
| Not Born At Mandate, Black | | | | | | -0.078 | |
| | | | | | | (0.248) | |
| Log Number of Children | 0.842** | -0.227 | -0.211 | 0.524* | -0.162 | -0.148 | -0.172 |
| | (0.409) | (0.305) | (0.301) | (0.289) | (0.317) | (0.309) | (0.297) |
| N | 877 | 877 | 877 | 877 | 877 | 877 | 877 |
| Mean Test Score, Control States | -0.049 | -0.049 | -0.049 | -0.049 | -0.049 | -0.049 | -0.049 |
| Mean Test Scores, Non-Exposed Cohorts, Treated States | -0.267 | -0.267 | -0.267 | -0.267 | -0.267 | -0.267 | -0.267 |
| State, Year FE | X | X | X | X | X | X | |
| State-specific Linear Trends | | X | X | | X | X | X |
| State-Black, Year-Black FE | | | | | | | X |

Notes: *** p<0.01, ** p<0.05, * p<0.1. Source: NEAP. Population data from SEER. The table presents DD estimates on the sample of fourth graders for the years 1992-2013. Each observation is a state-year cell. The dependent variable is the standardized NEAP reading score. State and year fixed effects are included in each column except for Column 7, which includes state-black and year-black fixed effects. In addition, Columns 2, 3, 5, and 6 include state-specific trends. The average standardized test score in control states and treated states (pre-mandate) is shown at the bottom of each column. Standard errors clustered at state level are shown in parentheses.

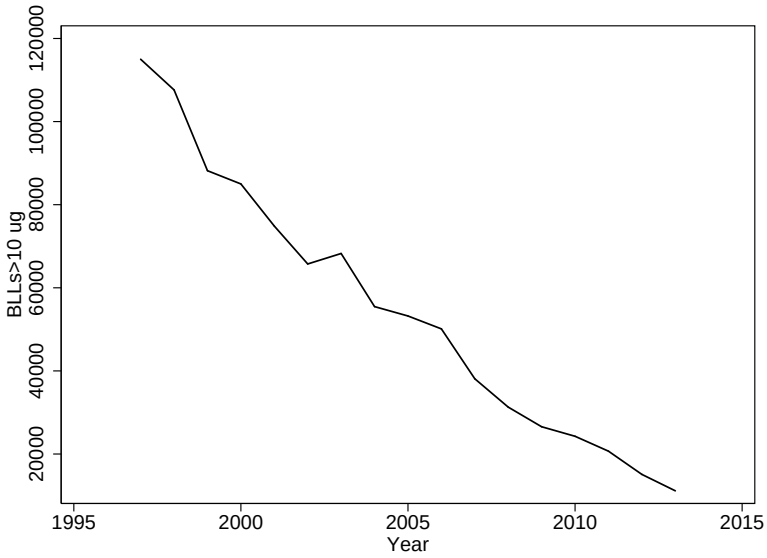
Table 12: Effects of the Mandates on Disciplinary Actions

| Dependent Variable: | Log Number of Children Receiving Disciplinary Actions | | | | | |
|---|---|---|---|---|---|---|
| | Corporal Punishment | | Suspensions | | Expulsions | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Exposed | 0.259 | 0.003 | 0.061 | 0.024 | -0.277 | 0.246 |
| | (0.359) | (0.884) | (0.140) | (0.106) | (0.315) | (0.803) |
| Log Number of Children | -0.316 | 6.952 | 0.480 | -1.134 | 2.185** | 2.809 |
| | (3.248) | (9.529) | (1.002) | (1.763) | (1.085) | (2.548) |
| N | 129 | 129 | 253 | 253 | 246 | 246 |
| Average Number of Kids Receiving Disciplinary Actions, Control States | 11354 | 11354 | 58669 | 58669 | 2125 | 2125 |
| Average Number of Kids Receiving Disciplinary Actions, Treated States, Pre-Mandate | 9370 | 9370 | 86212 | 86212 | 2770 | 2770 |
| State, Year FE | X | X | X | X | X | X |
| State-specific Linear Trends | | X | | X | | X |

Notes: *** p<0.01, ** p<0.05, * p<0.1. Source: US ED, CRDC. Population data from SEER. The table presents DD estimates on the sample of elementary and secondary school students without disability for the years 2000-2012. Each observation is a state-year cell. The dependent variable is the logarithm of number of children receiving the disciplinary action indicated in each column. State and year fixed effects are included in each column. In addition, Columns 2, 4, and 6 include state-specific trends. The average number of children receiving each disciplinary action in control states and treated states (pre-mandate) is shown at the bottom of each column. Standard errors clustered at state level are shown in parentheses.

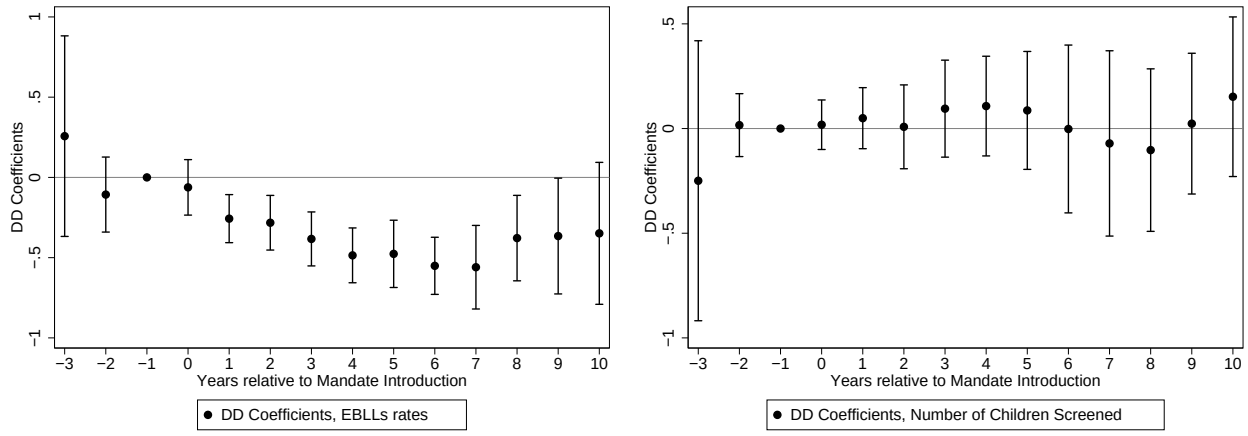
# A Additional Figures and Tables

Figure A.1: Number of Children below 72 Months of Age with Elevated Blood Lead Levels



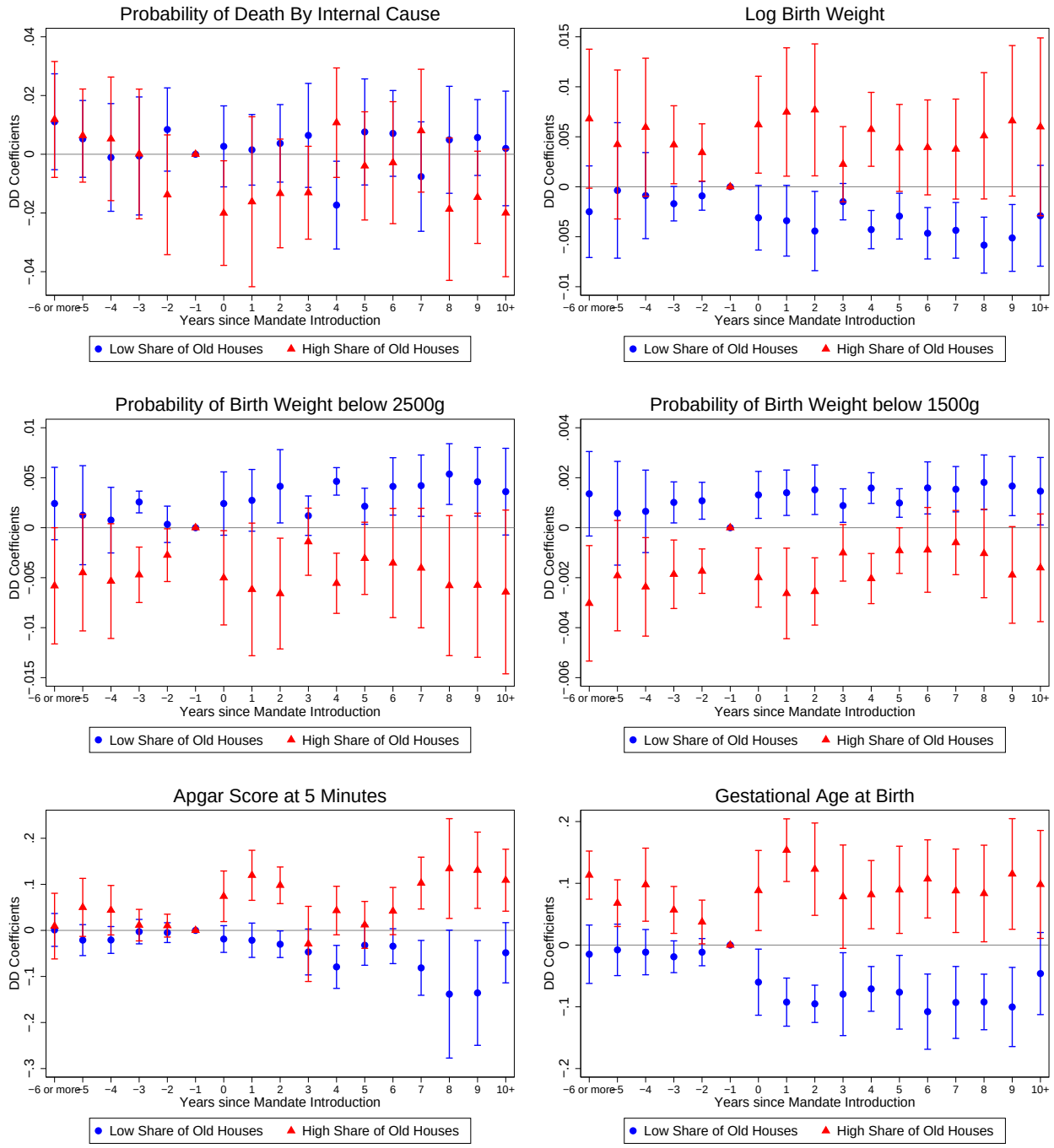

Source: CDC, 1997-2012. The figure plots the number of children with BLLs above $10\mu gd/L$ in the US over calendar time.

Figure A.2: Lead Poisoning Effects, Rates and Screening Counts



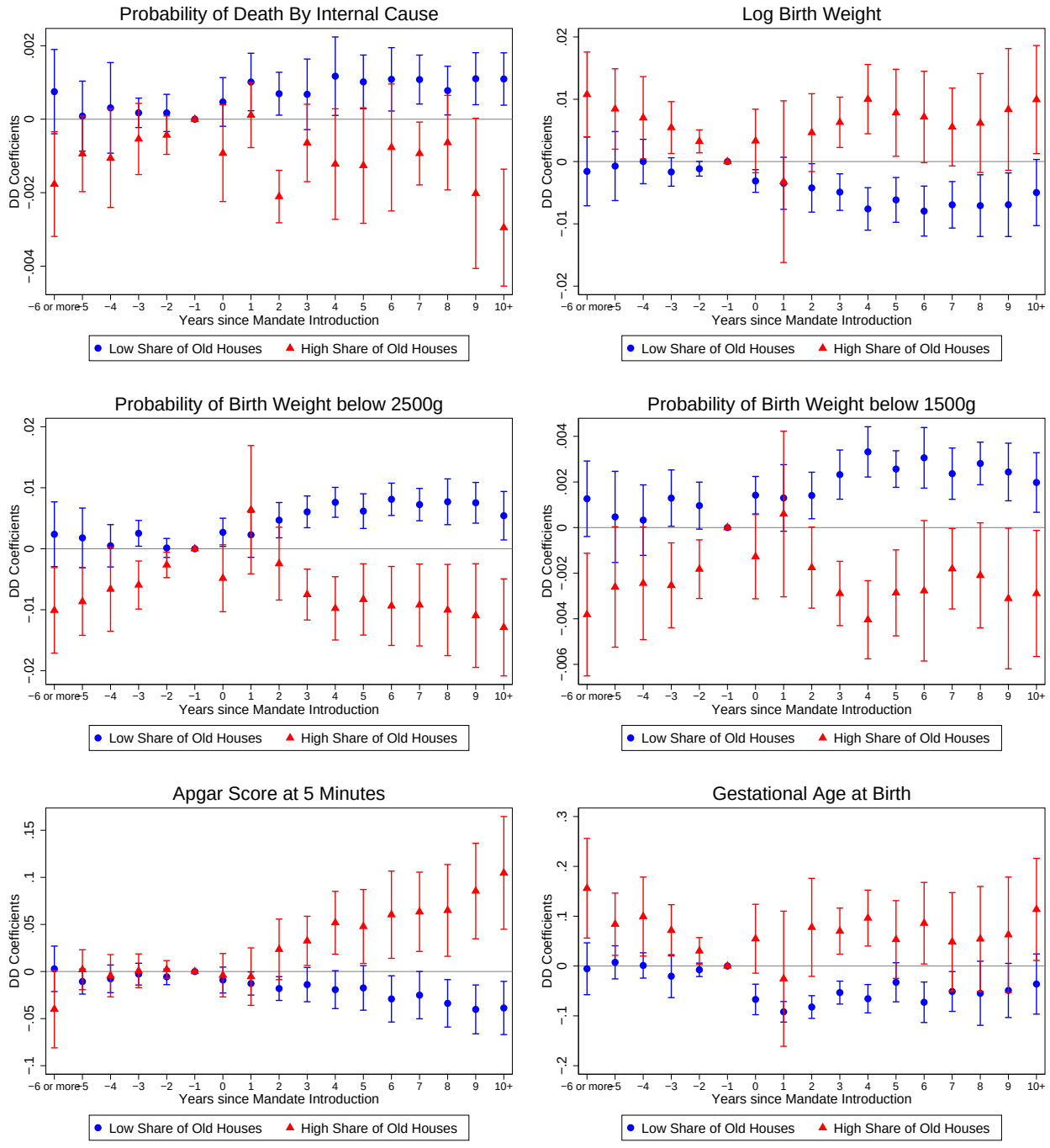

The figure plots DD coefficients on year-by-year mandate dummies estimated on the CDC sample for the years 1997-2012, restricted to all non-implementing states and states that implement lead-paint abatement mandates after 1997. The outcome variable is the logarithm of the rate of children with BLL above $10\mu g/dL$ over the number of children screened for lead poisoning in the left panel and the logarithm of the number of children screened for lead poisoning in the right panel. Logarithm of state population below 72 months of age, state and year fixed effects, and differential trends for states with high initial rates of lead poisoning are included in both panels. $T = 0$ is the year the mandate was introduced. $T = -1$ is the omitted category. The vertical lines are lower and upper bounds of 95% confidence interval. Standard errors are clustered at the state level (24 clusters).

Figure A.3: Effects of the Mandates on Infant Health




The figures plot DD coefficients on year-by-year mandate dummies estimated on the Birth Cohort Linked Birth and Infant Death Data of the National Vital Statistics System for the years 1989-1991 and 1995-2010 (2004 is missing). The dependent variable is indicated in each figure. Counties with old housing stock are counties with above median share of houses built prior to 1978. County and year fixed effects, and a trend for counties with old housing stock are included in all columns. $T = 0$ is the year the mandate was introduced. $T = -1$ is the omitted category. The vertical lines are lower and upper bounds of 95% confidence interval. Standard errors are clustered at the state level.

Figure A.4: Effects of the Mandates on Infant Health, Low Education Mothers




The figures plot DD coefficients on year-by-year mandate dummies estimated on the Birth Cohort Linked Birth and Infant Death Data of the National Vital Statistics System for the years 1989-1991 and 1995-2010 (2004 is missing). The sample is limited to mothers with high school diploma or below. The dependent variable is indicated in each figure. Counties with old housing stock are counties with above median share of houses built prior to 1978. County and year fixed effects, and a trend for counties with old housing stock are included in all columns. $T = 0$ is the year the mandate was introduced. $T = -1$ is the omitted category. The vertical lines are lower and upper bounds of 95% confidence interval. Standard errors are clustered at the state level.

Table A.1: State Financing Systems for Special Education Services

| State | 1988-1989 | 1992-1993 | 1994-1995 | 1999-2000 | 2008-2009 |
|---|---|---|---|---|---|
| (1) | (2) | (3) | (4) | (5) | (6) |
| Alabama | Resource-Based | Resource-Based | Flat Grant | Flat Grant | Flat Grant |
| Alaska | Pupil Weights | Pupil Weights | Pupil Weights | Flat Grant | Flat Grant |
| Arizona | Pupil Weights | Pupil Weights | Pupil Weights | Pupil Weights | Pupil Weights |
| Arkansas | Pupil Weights | Pupil Weights | Pupil Weights | Variable Block Grant | No Separate Special Education Funding |
| California | Resource-Based | Resource-Based | Resource-Based | Flat Grant | Flat Grant |
| Colorado | % Reimbursement | % Reimbursement | Flat Grant | Variable Block Grant | Pupil Weights |
| Connecticut | % Reimbursement | % Reimbursement | % Reimbursement | Flat Grant | No Separate Special Education Funding |
| Delaware | Resource-Based | Resource-Based | Resource-Based | Resource-Based | Resource-Based |
| Florida | Pupil Weights | Pupil Weights | Pupil Weights | Pupil Weights | Pupil Weights |
| Georgia | Pupil Weights | Pupil Weights | Pupil Weights | Pupil Weights | Pupil Weights |
| Idaho | Resource-Based | Resource-Based | % Reimbursement | Flat Grant | Flat Grant |
| Illinois | Resource-Based | Resource-Based | Resource-Based | % Reimbursement | % Reimbursement & Variable Block Grant |
| Indiana | Pupil Weights | Pupil Weights | Pupil Weights | Pupil Weights | Pupil Weights |
| Iowa | Pupil Weights | Pupil Weights | Pupil Weights | Pupil Weights | Pupil Weights |
| Kansas | Resource-Based | Resource-Based | Resource-Based | Resource-Based | Resource-Based |
| Kentucky | Resource-Based | Pupil Weights | Pupil Weights | Pupil Weights | Pupil Weights |
| Louisiana | Resource-Based | % Reimbursement | % Reimbursement | Pupil Weights | Pupil Weights |
| Maine | % Reimbursement | % Reimbursement | % Reimbursement | % Reimbursement | Pupil Weights |
| Maryland | % Reimbursement | % Reimbursement | Flat Grant | Variable Block Grant & Pupil Weights | Flat Grant |
| Massachusetts | Pupil Weights | Pupil Weights | Flat Grant | Flat Grant | Flat Grant |
| Michigan | % Reimbursement | % Reimbursement | % Reimbursement | % Reimbursement | % Reimbursement |
| Minnesota | Resource-Based | Resource-Based | % Reimbursement | Variable Block Grant | % Reimbursement |
| Mississippi | Resource-Based | N/A | Resource-Based | Resource-Based | Resource-Based |

| State | 1988-1989 | 1992-1993 | 1994-1995 | 1999-2000 | 2008-2009 |
|---|---|---|---|---|---|
| (1) | (2) | (3) | (4) | (5) | (6) |
| Missouri | Resource-Based | Resource-Based | Resource-Based | Resource-Based & Flat Grant | No Separate Special Education Funding |
| Montana | % Reimbursement | % Reimbursement | Flat Grant | Flat Grant | Flat Grant |
| Nebraska | % Reimbursement | % Reimbursement | % Reimbursement | % Reimbursement | % Reimbursement |
| Nevada | Resource-Based | Resource-Based | Resource-Based | Resource-Based | Resource-Based |
| New Hampshire | Pupil Weights | Pupil Weights | Pupil Weights | Pupil Weights | Pupil Weights |
| New Jersey | Pupil Weights | Pupil Weights | Pupil Weights | Pupil Weights | Flat Grant |
| New Mexico | Pupil Weights | Pupil Weights | Pupil Weights | Pupil Weights & Resource-Based | Pupil Weights |
| New York | Pupil Weights | Pupil Weights | Pupil Weights | Pupil Weights | Pupil Weights |
| North Carolina | % Reimbursement | Flat Grant | Flat Grant | Flat Grant | Pupil Weights |
| North Dakota | Resource-Based | Resource-Based | Flat Grant | Flat Grant | No Separate Special Education Funding |
| Ohio | Resource-Based | Resource-Based | Resource-Based | Pupil Weights | Pupil Weights |
| Oklahoma | Pupil Weights | Pupil Weights | Pupil Weights | Pupil Weights | Pupil Weights |
| Oregon | % Reimbursement | Pupil Weights | Pupil Weights | Pupil Weights | Pupil Weights |
| Pennsylvania | % Reimbursement | Flat Grant | Flat Grant | Flat Grant | Flat Grant |
| Rhode Island | % Reimbursement | % Reimbursement | % Reimbursement | N/A | No Separate Special Education Funding |
| South Carolina | Pupil Weights | Pupil Weights | Pupil Weights | Pupil Weights | Pupil Weights |
| South Dakota | % Reimbursement | % Reimbursement | % Reimbursement | Flat Grant & Pupil Weights | Flat Grant & Pupil Weights |
| Tennessee | Pupil Weights | Resource-Based | Resource-Based | Resource-Based | Resource-Based |
| Texas | Pupil Weights | Pupil Weights | Pupil Weights | Pupil Weights | Pupil Weights |
| Utah | Pupil Weights | Pupil Weights | Pupil Weights | Variable Block Grant | Variable Block Grant |

| State | 1988-1989 | 1992-1993 | 1994-1995 | 1999-2000 | 2008-2009 |
|---|---|---|---|---|---|
| (1) | (2) | (3) | (4) | (5) | (6) |
| Vermont | % Reimbursement | % Reimbursement | Flat Grant | % Reimbursement & Flat Grant | % Reimbursement |
| Virginia | Resource-Based | Resource-Based | Resource-Based | Resource-Based | Resource-Based |
| Washington | Resource-Based | Resource-Based | Pupil Weights | Pupil Weights | Pupil Weights |
| West Virginia | Resource-Based | Resource-Based | Flat Grant | Pupil Weights | No Separate Special Education Funding |
| Wisconsin | Resource-Based | Resource-Based | % Reimbursement | % Reimbursement | % Reimbursement |
| Wyoming | % Reimbursement | % Reimbursement | % Reimbursement | % Reimbursement | % Reimbursement |

Source: O'Reilly (1989), O'Reilly (1993), Parish et al. (1997), Parrish et al. (2003), Ahearn (2010). A "Pupil Weights" system allocates funds on a per student basis. A "Flat Grant" system allocates funds to districts by dividing total state funding available for special education by population counts. A "Resource-Based" system allocates funds based on payment for specified resources. A "Percent Reimbursement" system allocates funds based on actual expenditures. A "Variable Block Grant" systme allocates funds based on base year allocations, expenditures, and/or enrollment.

Table A.2: Lead Screening Effects

| Dependent Variable: | Log Number of Children Tested | | |
|---|---|---|---|
| | State, Year FE | Differential Trends by Baseline BLL | Differential Trends for Implementing States |
| | (1) | (2) | (3) |
| *Panel A: Full Sample* | | | |
| Post-Mandate | 0.206 | -0.058 | 0.080 |
| | (0.254) | (0.202) | (0.203) |
| Log Children <72 Months | 4.169*** | -0.273 | 0.074 |
| | (1.143) | (0.248) | (0.457) |
| N | 561 | 514 | 576 |
| Avg Number Kids Tested, Non-Implementing States | 97775 | 99658 | 92816 |
| Avg Number Kids Tested, Implementing States Pre-Period | 83728 | 87713 | 83728 |
| *Panel B: Mandates after 1996 Only (24 States)* | | | |
| Post-Mandate | 0.069 | 0.053 | 0.004 |
| | (0.149) | (0.154) | (0.133) |
| Log Children <72 Months | 5.810*** | 4.353*** | 5.833*** |
| | (1.076) | (1.087) | (1.094) |
| N | 345 | 327 | 345 |
| Avg Number Kids Tested, Non-Implementing States | 97775 | 101879 | 97775 |
| Avg Number Kids Tested, Implementing States Pre-Period | 94777 | 94777 | 94777 |

Notes: *** p<0.01, ** p<0.05, * p<0.1. The table presents DD estimates on the BLL sample from CDC for the years 1997-2012. The outcome variable is the logarithm of the number of children tested for lead poisoning. Logarithm of state population below 72 months of age and state and year fixed effects are included in all columns. In addition, Column 2 includes differential linear trends for states with initial outcome levels below and above the median, while Column 3 include differential linear time trends for treated states. Panel A reports estimates on the full sample of states, Panel B reports estimates on the subsample of states that implement lead-paint abatement mandates after 1997 and all non-implementing states. Average number of children tested in non-implementing and implementing states (pre-period only) is shown at the bottom of each column. Standard errors clustered at the state level are shown in parentheses.

Table A.3: Costs to Society associated with EBLLs

| | | | Costs associated to IQ Loss | | | | |
|---|---|---|---|---|---|---|---|
| BLL (ug/dL) | Baseline Probability | Cost of recommended medical action | Average IQ point loss per ug/dL | IQ Loss in PDV of Lifetime Earnings | Lost Tax Revenues | Special Education | Total Cost |
| (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| 10-14 | 0.005 | 74 | 0.19 | 99853 | 14978 | - | 114905 |
| | | | (0.12,0.26) | (96735, 102970) | (14510, 15445) | | (111319, 118490) |
| 15-19 | 0.002 | 74 | 0.19 | 116777 | 17517 | - | 134368 |
| | | | (0.12,0.26) | (107424, 126130) | (16113, 18919) | | (123612, 145123) |
| 20-44 | 0.002 | 1207 | 0.11 | 127199 | 19080 | 1439 | 148925 |
| | | | (0.07,0.15) | (114016, 140382) | (17102, 21057) | | (133764, 164085) |
| 45-69 | 0.000 | 1335 | 0.11 | 174231 | 26135 | 68 | 201768 |
| | | | (0.07,0.15) | (143945, 204516) | (21591, 30677) | | (166939, 236596) |
| >70 | 0.000 | 3444 | 0.11 | 223222 | 33483 | 12 | 260161 |
| | | | (0.07,0.15) | (175121, 271322) | (26268, 40698) | | (204845, 315476) |
| Total | 0.008 | 304 | | 109736 | 16460 | 1519 | 128019 |
| | | | | (102981, 116490) | (15447, 17473) | | (120251, 135786) |
| Source | CDC | Gould (2009) | Lanphear et al. (2005) | | Gould (2009) | Korfmacher (2003) | |
| Method | # Cases / Population <6 | Cost of medical procedure | Multivariate regression | Schwartz (1994) estimate of loss per IQ point adjusted to 2006 values=$17,815 | 15% of PDV of lifetime earnings | Schwartz (1994): 20% of BLL>20ug require special ed for 3 years; total cost: $38199 | |

Notes: Total IQ loss is computed as the average IQ point loss for BLL below $10\mu g/dL$, 5.13 (Lanphear et al. 2005), plus the cumulative IQ point loss for the smaller ranges plus the IQ loss for a child's confirmed range assuming she has the mean value for ranges $10-19\mu g/dL$ and the minimum value for higher ranges. 95% confidence intervals are reported in parentheses when available.

Table A.4: Effects of the Mandates on Fertility, by Mother's Characteristics

| Dependent Variable: | Log Number of Births | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| *Panel A: High School Graduate Mothers* | | | | | |
| Post-Mandate | -0.019 | -0.057 | 0.003 | 0.019 | 0.021 |
| | (0.041) | (0.037) | (0.033) | (0.027) | (0.032) |
| Post-Mandate, Counties with Old Housing Stock | | | | -0.033 | -0.038 |
| | | | | (0.039) | (0.046) |
| N | 27563 | 27563 | 27535 | 27535 | 27535 |
| Avg Births, Control Counties | 893 | 893 | 893 | 893 | 893 |
| Avg Births, Treated Counties Pre-Mandate | 881 | 881 | 881 | 881 | 881 |
| *Panel B: Black Mothers* | | | | | |
| Post-Mandate | -0.046 | 0.038 | -0.018 | -0.071 | 0.041 |
| | (0.092) | (0.067) | (0.115) | (0.167) | (0.170) |
| Post-Mandate, Counties with Old Housing Stock | | | | 0.100 | -0.155 |
| | | | | (0.162) | (0.167) |
| N | 24560 | 24560 | 24558 | 24558 | 24558 |
| Avg Births, Control Counties | 348 | 348 | 348 | 348 | 348 |
| Avg Births, Treated Counties Pre-Mandate | 442 | 442 | 442 | 442 | 442 |
| *Panel C: Single Mothers* | | | | | |
| Post-Mandate | -0.041 | -0.013 | -0.010 | -0.034 | 0.011 |
| | (0.039) | (0.030) | (0.056) | (0.076) | (0.078) |
| Post-Mandate, Counties with Old Housing Stock | | | | 0.044 | -0.058 |
| | | | | (0.069) | (0.074) |
| N | 40777 | 40777 | 40743 | 40743 | 40743 |
| Avg Births, Control Counties | 621 | 621 | 621 | 621 | 621 |
| Avg Births, Treated Counties Pre-Mandate | 554 | 554 | 554 | 554 | 554 |
| State FE | X | X | | | |
| Baseline Controls Interacted with Year FE | | X | | | |
| County FE | | | X | X | X |
| Trend for High-Pre1978-Share Counties | | | | | X |

Notes: *** p<0.01, ** p<0.05, * p<0.1. Source: NCHS. The table presents DD estimates on the Birth Cohort Linked Birth and Infant Death Data of the National Vital Statistics System for the years 1989-1991 and 1995-2010 (2004 is missing). The dependent variable is the logarithm of the number of births in each county and year. Counties with old housing stock are counties with above median share of houses built prior to 1978. Year fixed effects and the logarithm of the county population in each year are included in all columns. Column 2 includes state-level controls from the 1980 Census interacted with year dummies (share of high school dropouts, share of high school graduates, share of individuals with less than four years of college, share of blacks, share of Hispanics, unemployment rate, and share of people below the poverty line). Columns 3-5 include county fixed effects. Column 5 includes a trend for counties with old housing stock. Panels A-C present estimates on the sample of mothers with high school diploma or below, black mothers and single mothers, respectively. The average number of births in control and pre-mandate treated states is shown at the bottom of each column. Standard errors clustered at state level are shown in parentheses.

Table A.5: Effects of the Mandates on Fertility, Older Housing Stock

| Dependent Variable: | Log Number of Births | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| *Panel A: Full Sample* | | | | | |
| Post-Mandate | 0.017 | -0.040 | 0.046 | 0.049 | 0.047 |
| | (0.041) | (0.029) | (0.053) | (0.079) | (0.081) |
| Post-Mandate, Counties with | | | | -0.006 | 0.000 |
| Old Housing Stock | | | | (0.081) | (0.082) |
| N | 47283 | 47283 | 47238 | 47238 | 47238 |
| Avg Births, Control Counties | 1555 | 1555 | 1555 | 1555 | 1555 |
| Avg Births, Treated Counties Pre-Mandate | 1532 | 1532 | 1532 | 1532 | 1532 |
| *Panel B: High School Graduate Mothers* | | | | | |
| Post-Mandate | -0.019 | -0.057 | 0.003 | 0.051 | 0.032 |
| | (0.041) | (0.037) | (0.033) | (0.041) | (0.042) |
| Post-Mandate, Counties with | | | | -0.088*** | -0.041 |
| Old Housing Stock | | | | (0.029) | (0.038) |
| N | 27563 | 27563 | 27535 | 27535 | 27535 |
| Avg Births, Control Counties | 893 | 893 | 893 | 893 | 893 |
| Avg Births, Treated Counties Pre-Mandate | 881 | 881 | 881 | 881 | 881 |
| *Panel C: Black Mothers* | | | | | |
| Post-Mandate | -0.046 | 0.038 | -0.018 | -0.127 | 0.012 |
| | (0.092) | (0.067) | (0.115) | (0.144) | (0.144) |
| Post-Mandate, Counties with | | | | 0.202 | -0.144 |
| Old Housing Stock | | | | (0.149) | (0.158) |
| N | 24560 | 24560 | 24558 | 24558 | 24558 |
| Avg Births, Control Counties | 348 | 348 | 348 | 348 | 348 |
| Avg Births, Treated Counties Pre-Mandate | 442 | 442 | 442 | 442 | 442 |
| *Panel D: Single Mothers* | | | | | |
| Post-Mandate | -0.041 | -0.013 | -0.010 | -0.033 | 0.001 |
| | (0.039) | (0.030) | (0.056) | (0.073) | (0.076) |
| Post-Mandate, Counties with | | | | 0.039 | -0.040 |
| Old Housing Stock | | | | (0.070) | (0.078) |
| N | 40777 | 40777 | 40743 | 40743 | 40743 |
| Avg Births, Control Counties | 621 | 621 | 621 | 621 | 621 |
| Avg Births, Treated Counties Pre-Mandate | 554 | 554 | 554 | 554 | 554 |
| State FE | X | X | | | |
| Baseline Controls Interacted with Year FE | | X | | | |
| County FE | | | X | X | X |
| Trend for High-Pre1950-Share Counties | | | | | X |

Notes: *** p<0.01, ** p<0.05, * p<0.1. Source: NCHS. The table presents DD estimates on the Birth Cohort Linked Birth and Infant Death Data of the National Vital Statistics System for the years 1989-1991 and 1995-2010 (2004 is missing). The dependent variable is the logarithm of the number of births in each county and year. Counties with old housing stock are counties with above median share of houses built prior to 1950. Year fixed effects and the logarithm of county population are included in all columns. Column 2 includes state-level controls from the 1980 Census interacted with year dummies (share of high school dropouts, share of high school graduates, share of individuals with less than four years of college, share of blacks, share of Hispanics, unemployment rate, and share of people below the poverty line). Columns 3-5 include county fixed effects. Column 5 includes a trend for counties with old housing stock. Standard errors clustered at state level are shown in parentheses.

Table A.6: Effects of the Mandates on Infant Health, by Mother's Characteristics

| Dependent Variable: | Probability of Death by Internal Cause (1) | Log Birth Weight (2) | Probability of Birth Weight ≤2500 (3) | Probability of Birth Weight ≤1500 (4) | Apgar Score at 5 Minutes (5) | Number of Gestational Weeks (6) |
|---|---|---|---|---|---|---|
| *Panel A: High School Graduate Mothers* | | | | | | |
| Post-Mandate | 0.0005 | -0.004 | 0.004 | 0.001 | -0.012 | -0.067*** |
| | (0.0006) | (0.003) | (0.003) | (0.001) | (0.009) | (0.018) |
| Post-Mandate, Counties with Old Housing Stock | -0.0006 | 0.003 | -0.004 | -0.001 | 0.041** | 0.058 |
| | (0.0009) | (0.005) | (0.005) | (0.002) | (0.018) | (0.050) |
| N | 23926444 | 23912864 | 23912864 | 23912864 | 17711792 | 23644352 |
| Mean Outcome, Control Counties | 0.008 | 3284.370 | 0.079 | 0.014 | 8.931 | 38.885 |
| Mean Outcome, Treated Counties Pre-Mandate | 0.010 | 3270.032 | 0.088 | 0.017 | 8.903 | 38.909 |
| *Panel B: Black Mothers* | | | | | | |
| Post-Mandate | -0.0003 | -0.0007 | 0.0016 | -0.0003 | -0.085** | -0.069*** |
| | (0.0010) | (0.0022) | (0.0019) | (0.0010) | (0.038) | (0.025) |
| Post-Mandate, Counties with Old Housing Stock | 0.0008 | -0.0039 | 0.0018 | 0.0018 | 0.058 | 0.001 |
| | (0.0012) | (0.0030) | (0.0028) | (0.0014) | (0.036) | (0.040) |
| N | 9044778 | 9037626 | 9037626 | 9037626 | 7985061 | 8967080 |
| Mean Outcome, Control Counties | 0.012 | 3104.334 | 0.130 | 0.030 | 8.828 | 38.276 |
| Mean Outcome, Treated Counties Pre-Mandate | 0.015 | 3090.955 | 0.138 | 0.031 | 8.801 | 38.316 |
| *Panel C: Single Mothers* | | | | | | |
| Post-Mandate | 0.0001 | -0.0013 | 0.0015 | 0.0004 | -0.065*** | -0.066*** |
| | (0.0009) | (0.0025) | (0.0024) | (0.0011) | (0.021) | (0.023) |
| Post-Mandate, Counties with Old Housing Stock | 0.00002 | -0.0003 | -0.0005 | 0.0007 | 0.030 | 0.024 |
| | (0.00097) | (0.0037) | (0.0038) | (0.0013) | (0.025) | (0.046) |
| N | 25141886 | 25128464 | 25128464 | 25128464 | 21276152 | 24894196 |
| Mean Outcome, Control Counties | 0.008 | 3205.576 | 0.097 | 0.019 | 8.857 | 38.630 |
| Mean Outcome, Treated Counties Pre-Mandate | 0.012 | 3170.294 | 0.114 | 0.024 | 8.849 | 38.672 |

Notes: *** p<0.01, ** p<0.05, * p<0.1. Source: NCHS. The table presents DD estimates on the Birth Cohort Linked Birth and Infant Death Data of the National Vital Statistics System for the years 1989-1991 and 1995-2010 (2004 is missing). The dependent variable is indicated in each column. Counties with old housing stock are counties with above median share of houses built prior to 1950. County and year fixed effects, and a trend for counties with old housing stock are included in all columns. Panels A-C present estimates on the sample of mothers with high school diploma or below, black mothers and single mothers, respectively. The mean outcome (in levels) in control and pre-mandate treated states is shown at the bottom of each column. Standard errors clustered at state level are shown in parentheses.

Table A.7: Effects of the Mandates on Infant Health, Older Houses

| Dependent Variable: | Probability of Death by Internal Cause (1) | Log Birth Weight (2) | Probability of Birth Weight ≤2500 (3) | Probability of Birth Weight ≤1500 (4) | Apgar Score at 5 Minutes (5) | Number of Gestational Weeks (6) |
|---|---|---|---|---|---|---|
| *Panel A: Full Sample* | | | | | | |
| Post-Mandate | -0.0002 | -0.0026 | 0.0022 | 0.0006 | -0.046** | -0.081** |
| | (0.0004) | (0.0017) | (0.0016) | (0.0005) | (0.021) | (0.034) |
| Post-Mandate, Counties with Old Housing Stock | 0.0004 | 0.0007 | -0.0007 | 0.0003 | 0.045 | 0.048 |
| | (0.0005) | (0.0029) | (0.0027) | (0.0007) | (0.029) | (0.050) |
| N | 72913680 | 72877496 | 72877496 | 72877496 | 60641356 | 72319584 |
| Mean Outcome, Control Counties | 0.006 | 3299.767 | 0.076 | 0.014 | 8.896 | 38.772 |
| Mean Outcome, Treated Counties Pre-Mandate | 0.008 | 3325.824 | 0.078 | 0.015 | 8.910 | 38.948 |
| *Panel B: High School Graduate Mothers* | | | | | | |
| Post-Mandate | 0.0004 | -0.0045 | 0.0033 | 0.0010 | -0.0121 | -0.0928*** |
| | (0.0005) | (0.0036) | (0.0031) | (0.0011) | (0.0095) | (0.0261) |
| Post-Mandate, Counties with Old Housing Stock | -0.0002 | 0.0030 | -0.0027 | -0.0002 | 0.0402** | 0.0851* |
| | (0.0008) | (0.0045) | (0.0045) | (0.0014) | (0.0176) | (0.0489) |
| N | 23926444 | 23912864 | 23912864 | 23912864 | 17711792 | 23644352 |
| Mean Outcome, Control Counties | 0.008 | 3284.370 | 0.079 | 0.014 | 8.931 | 38.885 |
| Mean Outcome, Treated Counties Pre-Mandate | 0.010 | 3270.032 | 0.088 | 0.017 | 8.903 | 38.909 |
| *Panel C: Black Mothers* | | | | | | |
| Post-Mandate | -0.0010 | -0.0002 | 0.0016 | -0.0004 | -0.091** | -0.070** |
| | (0.0006) | (0.0024) | (0.0020) | (0.0009) | (0.037) | (0.032) |
| Post-Mandate, Counties with Old Housing Stock | 0.0021** | -0.0055* | 0.0024 | 0.0023* | 0.067 | -0.002 |
| | (0.0009) | (0.0032) | (0.0026) | (0.0012) | (0.042) | (0.051) |
| N | 9044778 | 9037626 | 9037626 | 9037626 | 7985061 | 8967080 |
| Mean Outcome, Control Counties | 0.012 | 3104.334 | 0.130 | 0.030 | 8.828 | 38.276 |
| Mean Outcome, Treated Counties Pre-Mandate | 0.015 | 3090.955 | 0.138 | 0.031 | 8.801 | 38.316 |
| *Panel D: Single Mothers* | | | | | | |
| Post-Mandate | -0.0005 | -0.0012 | 0.0013 | 0.0003 | -0.075*** | -0.076*** |
| | (0.0006) | (0.0017) | (0.0017) | (0.0007) | (0.017) | (0.023) |
| Post-Mandate, Counties with Old Housing Stock | 0.0010 | -0.0010 | 0.0005 | 0.0010 | 0.044 | 0.035 |
| | (0.0007) | (0.0033) | (0.0034) | (0.0009) | (0.028) | (0.053) |
| N | 25141886 | 25128464 | 25128464 | 25128464 | 21276152 | 24894196 |
| Mean Outcome, Control Counties | 0.008 | 3205.576 | 0.097 | 0.019 | 8.857 | 38.630 |
| Mean Outcome, Treated Counties Pre-Mandate | 0.012 | 3170.294 | 0.114 | 0.024 | 8.849 | 38.672 |

Notes: *** p<0.01, ** p<0.05, * p<0.1. Source: NCHS. The table presents DD estimates on the Birth Cohort Linked Birth and Infant Death Data of the National Vital Statistics System, 1989-1991 and 1995-2010 (2004 is missing). The dependent variable is indicated in each column. Counties with old housing stock are counties with above median share of houses built prior to 1950. County and year fixed effects, and a trend for counties with old housing stock are included in all columns. The mean outcome (in levels) in control and pre-mandate treated states is shown at the bottom of each column. Standard errors clustered at state level are shown in parentheses.

Table A.8: Effects of the Mandates on Special Education Needs, Elementary School Children

| Dependent Variable: | Log Number of Children Aged 6-10 Receiving Special Education | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| Sample: | All Disabilities Combined | | | | | | Lead-Related | Not Lead-Related |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Post-Mandate | -0.007 | 0.132*** | 0.034 | 0.050*** | 0.034 | | | |
| | (0.037) | (0.027) | (0.028) | (0.018) | (0.028) | | | |
| Exposed | | -0.179*** | -0.061* | -0.045* | -0.060* | -0.058 | -0.067 | 0.023 |
| | | (0.036) | (0.033) | (0.025) | (0.032) | (0.036) | (0.041) | (0.037) |
| Maximum TANF/AFDC Benefit for Family of 3 | | | | | -0.0002 | | | |
| | | | | | (0.0003) | | | |
| Log Number of Children | 0.630*** | 0.609*** | 0.243 | 0.327 | 0.225 | -0.894*** | -1.024*** | -0.110 |
| | (0.184) | (0.184) | (0.195) | (0.213) | (0.209) | (0.182) | (0.216) | (0.214) |
| N | 5345 | 5345 | 5345 | 5345 | 5345 | 5345 | 5345 | 5333 |
| Average Number of Kids on SpEd, Non-Exposed Cohorts | 8353 | 8353 | 8353 | 8353 | 8353 | 8353 | 7319 | 477 |
| Average Number of Kids on SpEd, Exposed Cohorts, Treated States | 8342 | 8342 | 8342 | 8342 | 8342 | 8342 | 7098 | 490 |
| State, Year FE | X | X | X | X | X | | | |
| Cohort FE | | | X | X | X | X | X | X |
| State-specific Linear Trends | | | | X | | | | |
| State-Year FE | | | | | | X | X | X |

Notes: *** p<0.01, ** p<0.05, * p<0.1. Source: US ED, OSEP. Population data from SEER. The table presents DD estimates on the IDEA sample of children aged 6-10 for the years 1990-2010. Each observation is a state-year-age cell. The dependent variable is the logarithm of number of children receiving special education under IDEA, Part B, for any disability (Columns 1-6), for lead-related disabilities only (Column 7) and for disabilities not related to lead poisoning (Column 8). The fixed effects included in each specifications are indicated at the bottom of each column. The average number of children receiving special education under IDEA, Part B for exposed (post-period only) and non-exposed cohorts is shown at the bottom of each column. Standard errors clustered at state level are shown in parentheses.

Table A.9: Effects of the Mandates on Special Education Rates

| Dependent Variable: | Log Rates of Children Aged 6-13 Receiving Special Education | | | | | |
|---|---|---|---|---|---|---|
| Sample: | All Disabilities Combined | | | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Post-Mandate | 0.001 | 0.087*** | 0.042* | 0.044*** | 0.042 | |
| | (0.035) | (0.025) | (0.024) | (0.015) | (0.027) | |
| Exposed | | -0.139*** | -0.069** | -0.066*** | -0.067** | -0.081** |
| | | (0.035) | (0.030) | (0.025) | (0.034) | (0.038) |
| Maximum TANF/AFDC Benefit for Family of 3 | | | | | 0.00002 | |
| | | | | | (0.00025) | |
| N | 8552 | 8552 | 8552 | 8552 | 8552 | 8552 |
| Average Number of Kids on SpEd, Non-Exposed Cohorts | 0.112 | 0.112 | 0.112 | 0.112 | 0.112 | 0.112 |
| Average Number of Kids on SpEd, Exposed Cohorts, Treated States | 0.121 | 0.121 | 0.121 | 0.121 | 0.121 | 0.121 |
| State, Year FE | X | X | X | X | X | |
| Cohort FE | | | X | X | X | X |
| State-specific Linear Trends | | | | X | | |
| State-Year FE | | | | | | X |

Notes: *** p<0.01, ** p<0.05, * p<0.1. Source: US ED, OSEP. Population data from SEER. The table presents DD estimates on the IDEA sample of children aged 6-13 for the years 1990-2010. Each observation is a state-year-age cell. The dependent variable is the logarithm of the rate of children receiving special education under IDEA, Part B, over the state population in that age-year cell for any disability. The fixed effects included in each specifications are indicated at the bottom of each column. The average rate of children receiving special education under IDEA, Part B for exposed (post-period only) and non-exposed cohorts is shown at the bottom of each column. Standard errors clustered at state level are shown in parentheses.