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. Abatement mandates reduce the rate of elevated blood lead levels by 29%. Moreover, the mandates decrease the rate of enrollment in special education in exposed cohorts by 8.1%, indicating a reduction in the number of children with disabilities. A back of the envelope calculation suggests that this decrease in the rate of enrollment in special education induces savings between $17.5 and $111 million per state-cohort on average, while the increased lifetime earnings from the reduction in blood lead levels could lead to increased tax revenues in the order of $2.8 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

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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 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, do the mandates improve children health? In particular, do the mandates improve infant health? And do they reduce the number of children requiring special education? The requirement for special education is indicative of a child's disability. Third, do the mandates have fiscal benefits? Improvements in children's health could translate in increased lifetime earnings and hence in increased tax revenues. In addition, a reduction in in the number of children served under the Individuals with Disability Education Act of 1975 (IDEA) would imply savings for the government.

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-

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

[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, 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). Third, I use Vital Statistics data from the CDC to estimate the effects of the mandates on birth outcomes, infant mortality, and fertility.

Correspondingly, I find three sets of results. First, I provide evidence that the mandates reduce the rate of elevated blood lead levels (EBLLs) by 29%, which is equivalent to a decline in the number of children with EBLLs, estimated imprecisely, on the order of 170 children per state-year. 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 translate into expected benefits of the mandates worth $768 per child and into potential additional fiscal revenues of $16,460 per child, assuming an income tax rate of 15%. Second, among cohorts born up to six years prior to the introduction of a mandate, the rate of special education enrollment falls by 8.1%, 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 17.7% decrease in the number of children on special education per cohort-grade in elementary school, off an average enrollment of 8,382 students, and a 4.8% decrease in the number of children on special education per cohort-grade in middle school, off an average enrollment of 9,624 students per grade. Chambers et al. (2002) estimate that, in 2000, the additional spending to educate a student with a disability amounted to $5,918 per year. Therefore, the mandates could induce savings in special education programs that total between $17.5 million and $111 million per state-cohort on average. However, the reduction in the number of children served under IDEA does not appear to translate into higher test scores on average. Third, the mandates appear to worsen infant health, particularly so for offsprings of mothers of low socioeconomic status. 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, this tightening in the budget constraint of disadvantaged mothers affects their stress level and the health of their babies. However, more research is warranted to validate this hypothesis.

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 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 discusses the impact of the mandates on child disability and special education expenditures, while Section 6 investigates the impact of the mandates on educational attainment and disciplinary actions. Section 7 estimates the impact of the mandates on infant health outcomes and fertility. 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 7 and 8). 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, child disability, educational attainment, infant health, and fertility.

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. Some states, such as Massachusetts and Rhode Island, require universal testing of children below six years of age, while other states prefer to target resources at children most at risk on the basis of the neighborhood of residence or the family's income. Furthermore, there is a mandatory requirement of screening for children on Medicaid. However, using data from the National

[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.

Health and Nutrition Survey (NHANES), GAO (1998) found that two thirds of children on Medicaid had not been screened for lead poisoning prior to the NHANES examination. In my empirical analysis I check that screening rates were not affected by the introduction of abatement mandates. 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.

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.[13] 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 or counts for lead poisoning (Appendix Tables A.2 and A.3), 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. In my empirical analysis I allow for the effects of the mandates to differ across elementary and middle school.

States started reporting the number of children aged six to 21 served under IDEA by disability and race

[13] 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.

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).[14]

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.[15] 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 (Linn 2005).

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

Birth outcomes, infant mortality, and fertility. To analyze the effects of the mandates on infant

[14] 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.

[15] 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.

health and fertility, 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.[16] 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 control for fixed county characteristics, such as the 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:

$$Y_{st} = \beta Mandate_{st} + \gamma_s + \delta_t + \epsilon_{st} \tag{1}$$

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, and γ_s and δ_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 ϵ_{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, α_y, and lags, β_y, of the mandates' effect on old houses:

$$Y_{st} = \sum_{y=1}^{T_{min}} \alpha_y Pre_{t-y,s} + \sum_{y=0}^{T_{max}} \beta_y Post_{t+y,s} + \gamma_s + \delta_t + \epsilon_{st} \tag{2}$$

[16] 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.

Table 3 and Appendix Table A.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, Table 3 and Appendix Table A.4 show similar declines in the incidence of BLLs above $10\mu g/dL$, ranging between 21% and 29% when measured in rates and 10% and 17% when measured in counts. All these estimates are estimated imprecisely and are at most significant at the 10% level. Appendix Tables A.2 and A.3 show no evidence that the mandates affect the rate or number of children screened for lead poisoning. Therefore, it is equivalent to look at rates or counts of elevated blood lead levels. As the assumption of constant effects across states is more likely to hold when the outcome is expressed in rates, I will focus on Table 3.

Figure 3 investigates the validity of the DD design in a balanced sample.[17] The left plot shows estimates from the baseline DD regression in equation 2, while the right plot introduces differential linear trends for implementing states 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 3 and 6 of Table 3 show the relative DD estimates. My preferred specification suggests that introducing a mandate decreases the rate of BLLs above $10\mu g/dL$ by 29% per year on average, or 170 children, and above $20\mu g/dL$ by 26%, or 25 children.

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.5 computes the

[17] To balance the sample, 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.

[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.5), which I scale up by the share of families with small children who live in old houses before the mandates are implemented, 82.5% .

average benefits of preventing one BLL above $10\mu g/dL$ in terms of lifetime earnings to be $110,000. This 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 29% decrease in the rate EBLLs translates into a decrease in the probability that a child has an elevated blood lead level of 0.007 from a baseline rate of 2.1%, I obtain an expected benefit from the introduction of a mandate of $768 per child. The estimation error, however, is such that I cannot reject a benefit as high as $1,600. 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 in the same order of magnitude 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.5).

5 Child Disability Effects

The previous section suggests that the mandates decrease blood lead levels. Thus, it seems natural to ask whether the reduction in blood lead levels translates into a reduction in child disability rates. Using a DD framework, I contrast the number of children requiring special education among cohorts exposed to the mandates by estimating variations of the following specification:

$$Y_{bst} = \beta Mandate_{st} + \phi Exposed_{bst} + \pi X_{st} + \gamma_s + \delta_t + \eta_b + \epsilon_{bst} \tag{3}$$

where Y_{bst} is the logarithm of the rate of enrollment in special education 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 γ_s, δ_t, and η_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

[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.

standards for special education screenings and these standards might change over time.[20] I consider the effects of the mandates on children aged 6-13 years old, and I allow the results to differ across elementary and middle school.

Table 4 shows the effect of the mandates on the rate of enrollment in special education. Column 1 shows no change in the average rate of enrollment in special education after a mandate is introduced. However, Columns 2-4 show that two opposing mechanisms are at play after the introduction of a mandate. On the one hand, the rate of enrollment in 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 5, accounts for state-level policy changes that affect the whole risk set for special education classification. In contrast, the mandates decrease the rate of enrollment in special education among those cohorts that were young enough to benefit from a safer home environment. My preferred estimate indicates a decrease in the rate of enrollment in special education under IDEA of 8.1%, a result that is robust across many specifications.[21] In particular, Column 4 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.[22] Indeed, screening for special education requirements is free 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 4 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 4 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. In addition, the effects of the mandates appear to be roughly equal for different cohorts born after the introduction of the regulations. This suggests that

[20] 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.

[21] Column 6 of Appendix Table A.6 shows that the mandates decrease the number of children requiring special education by 7.3%.

[22] Following (Duggan & Kearney 2007), the AFDC/TANF data were obtained from various years of the publications Ways and Means Committee Overview of Entitlement Programs (Green Book).

the stronger effects for younger children are driven by their longer exposure to the mandate, rather than by the fact that the mandates have been in place for longer, meaning that more houses have been abated.[23] The right panel of Figure 4 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 4.[24] Indeed, Table 5 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. Indeed, the effect of a mandate on cohorts that were not born at the time of the introduction of the regulation does not appear to vary substantially by cohort, as depicted in the left panel of Figure 4. Table 6 shows that rates of enrollment in special education fall by about 21.9%, with the largest declines among children in elementary school age. Column 2 of Appendix Table A.6 reports that, on average, 17.7% fewer children born after a mandate require special education in a given elementary school grade, with a 5% confidence interval ranging between 7% and 37%, off an average population of children requiring special education in elementary school of 8,382 children per grade. Column 3 of Appendix Table A.6 reports that, on average, 4.8% fewer children born after a mandate require special education in a given middle school grade, with a 5% confidence interval ranging between 0 and 11%, off an average population of children requiring special education in middle school of 9,624 children per grade. 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 $3.5 million and $18.4 million per state-grade on average in elementary schools and savings up to $6.3 million per state-grade on average in middle schools.[25] 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 $17.5 million and $111 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.

[23] Figure 3 showed stronger effects of the mandates on BLLs over time, hence I cannot completely rule out that increasing enforcement over time explains some of the decrease in special education enrollment.

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

[25] The point estimates are $8.8 million and $2.7 million, respectively.

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:

$$Y_{rst} = \alpha Black_r + \beta_0 Exposure_{st} + \beta_1 Exposure_{st} * Black_r + \pi X_{rst} + \gamma_s + \delta_t + \epsilon_{st} \tag{4}$$

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 γ_s and δ_t are state and time fixed effects. Table 7 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).

6 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 (1) in a difference-in-differences framework, with NAEP scores in mathematics and reading as outcome variables.

Tables 8 and 9 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 5, 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 7 and 8 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. Specifically, I plan to obtain district-level data on both the number of children served under IDEA and standardized test scores from mandatory state assessments.

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 10 finds no effect of the mandates on the number of children receiving corporal punishment, suspensions, or expulsions at the state level.

7 Infant Health and Fertility 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:

$$Y_{ist} = \beta_0 Mandate_{st} + \pi \mathbf{X}_{it} + \gamma_s + \delta_t + \eta Implementing_s * t + \epsilon_{ist} \tag{5}$$

where Y_{ist} is an outcome, such as the logarithm of the birth weight of child i 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; $\mathbf{X}_{it}$ are maternal characteristics; and γ_s and δ_t are state and time fixed effects. In some specifications, I include county fixed effects or differential trends for implementing states. Furthermore, availability of individual-level data allows me to split the sample based on mother characteristics. To correct for multiple hypotheses testing, I construct a standardized health index by averaging standardized measures of the inverse probability of infant death by internal cause, logarithm of birth weight, apgar score at five minutes and gestational age at birth.

Surprisingly, Panel A of Table 11 shows that the mandates weakly worsen infant health, and Figure A.2 corroborates this evidence by showing that these findings are not driven by pretrends. As lead mimics calcium, the lead accumulated in mothers' bones is released in the mother's bloodstream during pregnancy and in breastmilk at lactation (Gulson et al. 2003). This fact, however, cannot explain a negative effect of the mandates on infant health. The worsening of infant health after a mandate is larger for offsprings of mothers of low socioeconomic status (Appendix Table A.7). 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 addition, the medical literature 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).

By allowing the effect of the mandates to vary with the age of the housing stock in each county, Panel

B of Table 11 suggests that the mandates' effects on infant health are highly heterogeneous. Indeed, the mandates appear to have weakly negative effects in counties where the housing stock is newer. In contrast, in counties with a high share of old houses the mandates appear to have no net effect.[26] These findings are puzzling, and further research is warranted to understand their drivers. Specifically, I plan to analyze migration patterns by exploiting the spatial distribution of counties in different commuting zones.

These changes in birth outcomes might also be related to changes in fertility decisions, with disadvantaged mothers being differentially affected (Gruber et al. 1999). I test this hypothesis by estimating the effects of the mandates on county level fertility and on the characteristic of the mothers in my sample. Looking at the fertility margin, Figure 6 plots year-by-year DD estimates from a leads and lags equation in the spirit of equation 2, showing no evidence of an effect of the mandates on the number of births, a result that is confirmed in Columns 1-3 of Table 12 and that holds both in the full sample and in the subsamples of mothers of low socioeconomic status (Appendix Table A.8). Furthermore, Columns 4 and 5 show no evidence of differential effects based on the county housing stock.[27] Similarly, Table 13 shows no effect of the mandates on the characteristics of the women giving birth.

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. The mandates reduce the rate of elevated blood lead levels by 29%, resulting in a decrease in the rate of enrollment in special education of 8.1%. A back of the envelope calculation suggests that this decrease in child disability results in savings for the government that total between $17 million and $111 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 $25\mu g/dL$. In addition, plausible estimates from the public health literature suggest that the lower rates of elevated blood lead levels are associated with increased lifetime earnings and increased tax revenues in the order of $2.8 million per state-cohort on average. In contrast, Vital Statistics data suggest that the mandates might have weakly negative effects on infant health, especially for children of disadvantaged mothers. Further research is warranted to understand the underlying mechanisms of this perverse effect. Nonetheless, the magnitudes of these effects are small, suggesting that the negative

[26] Moreover, results not reported in the paper show that these effects are robust to specifying old houses as houses built prior to 1978 or prior to 1950.

[27] Appendix Table A.9 shows that the null result is robust to specifying old houses as houses built prior to 1978 or prior to 1950.

consequences in terms of increased health care expenditure are not large.

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 appear to 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.5 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.5 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, there appear to 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 focused manner that minimizes adverse housing market effects and adverse effects on mental health and households' budget constraint.

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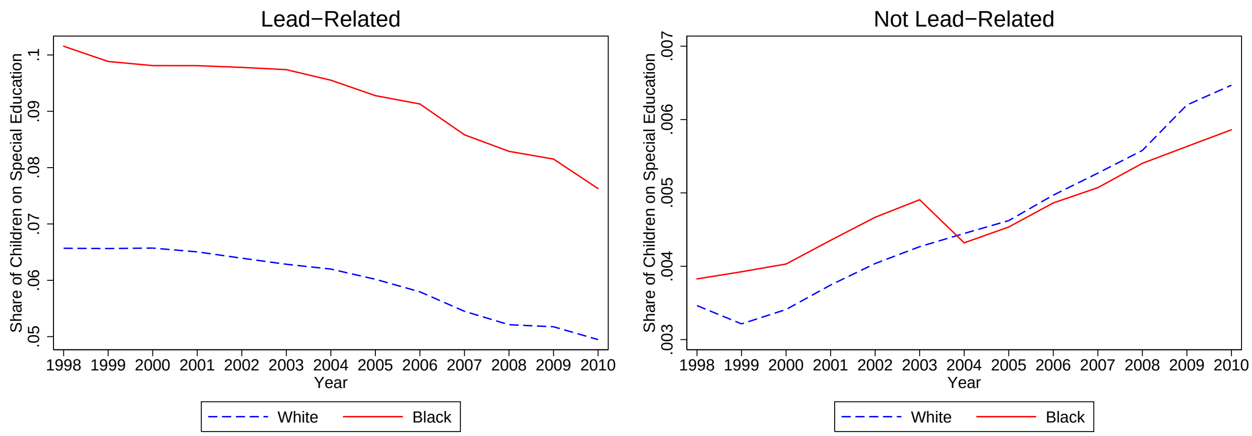
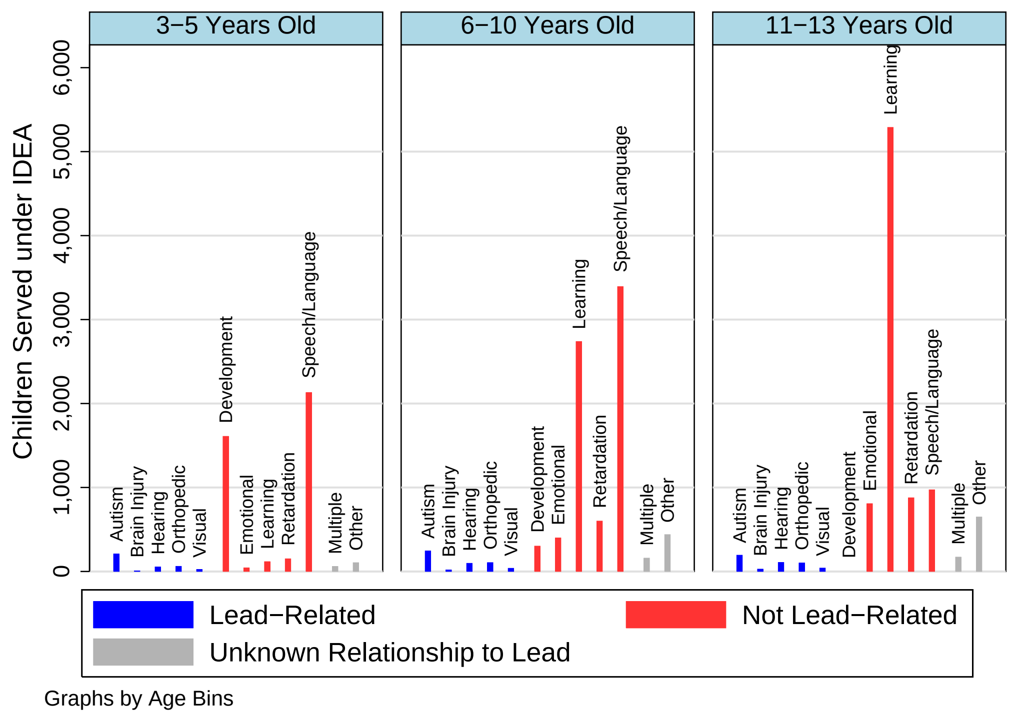
Figure 1: Distribution of Children Served under IDEA by Age Group and Disability



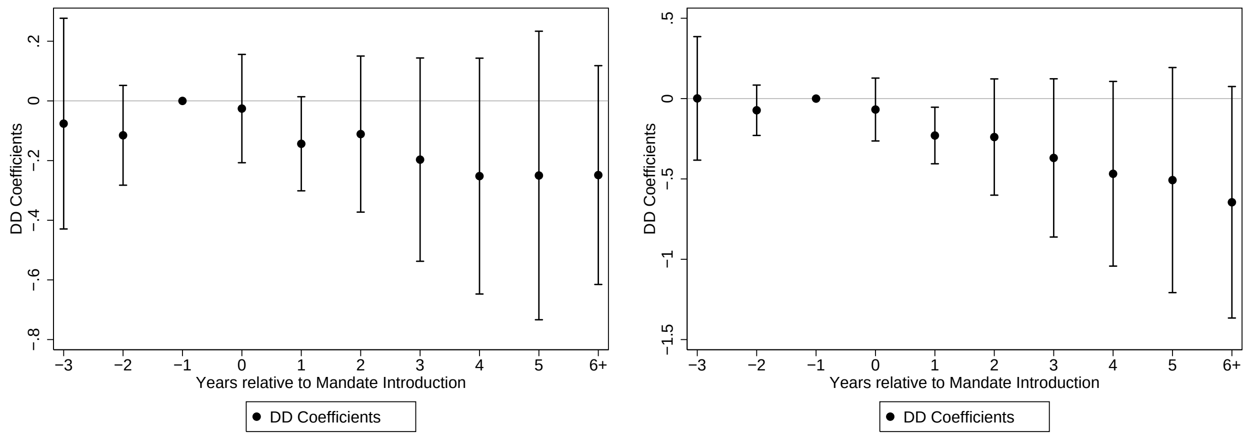
Source: US ED, OSEP. The figure plots the average number of children aged six to 21 served by a state each year under IDEA in each disability category by race for the years 1998-2010. I classify disabilities as being potentially related to lead poisoning (left panel) and not related to lead (right panel) 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



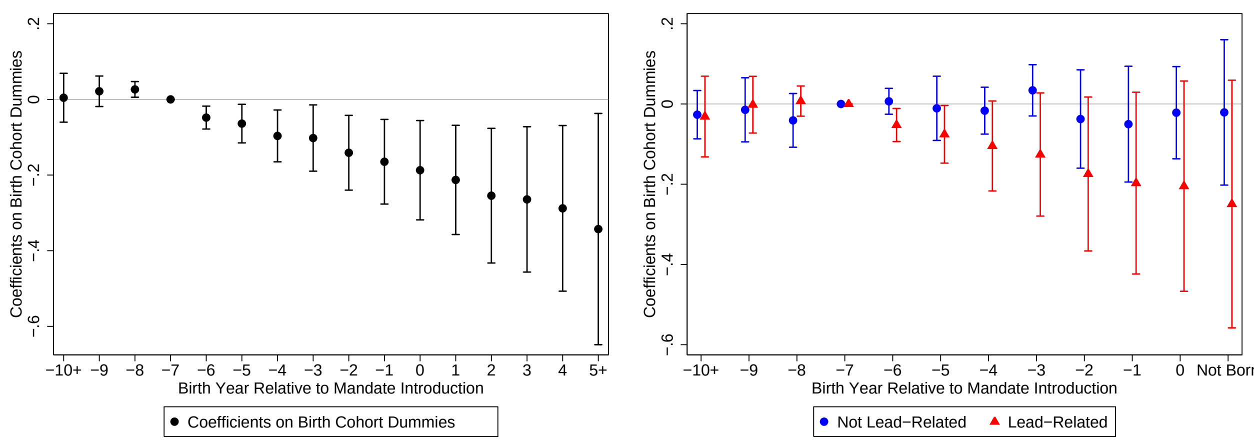
Source: US ED, OSEP. The figure plots the average number of children served by a state each year under IDEA in each disability category by school-age groups for the years 1990-2010. I classify disabilities as being potentially related to lead poisoning (red bars), not related to lead (blue bars), or 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, Rates of BLLs above $10\mu g/dL$, Alternative Specifications



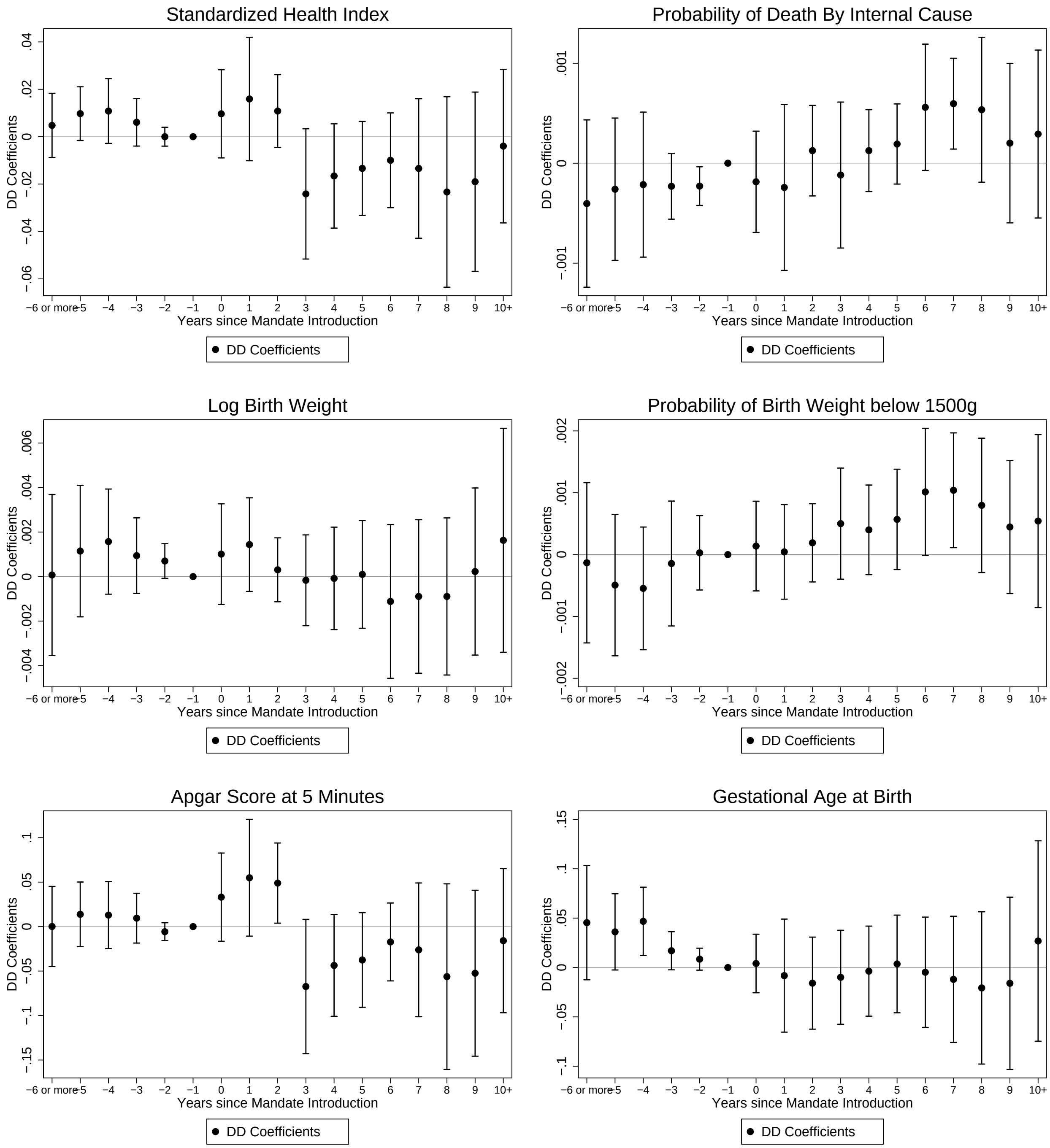
The figure plots DD coefficients on year-by-year mandate dummies estimated on the CDC sample for the years 1997-2012, on the subsample of states that implement lead-paint abatement mandates after 1997 and all non-implementing states. The outcome variable is the logarithm of number of children with BLL above $10\mu g/dL$. State and year fixed effects are included in both panels. In addition, the right figure includes differential trends for implementing states. $T = 0$ 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.

Figure 4: Effect of the Mandates on Special Education, by Age at Mandate



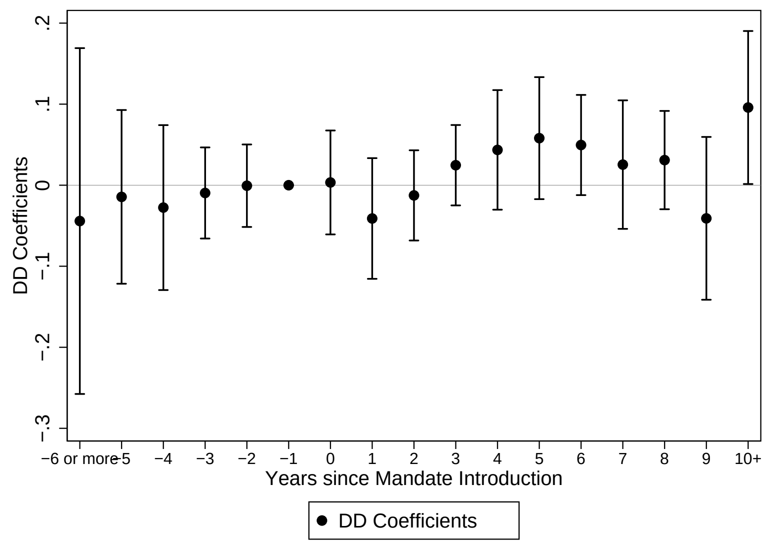
Source: US ED, OSEP. The figure plots the DD coefficients by exposure to the mandate on the IDEA sample of children aged 6-13, for the years 1990-2010. State-year and cohort fixed effects are included. Cohorts born seven years prior to the introduction are the omitted category. The bars indicate confidence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

Figure 5: Effects of the Mandates on Infant Health



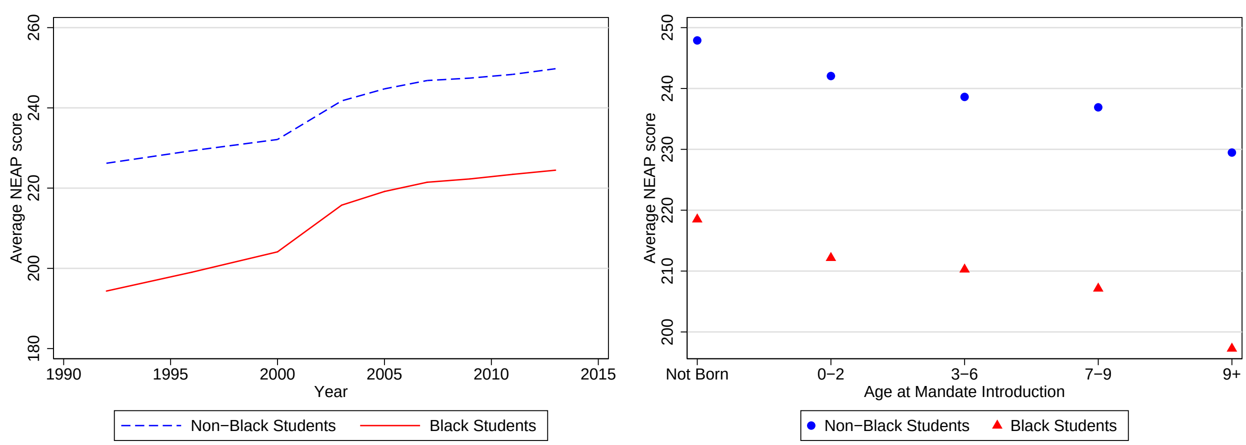
The figures plot DD coefficients on year-by-year mandate dummies estimated on the Birth Cohort Linked Birth and Infant Death Data of the National Vital Statistics System for the years 1989-1991 and 1995-2010 (2004 is missing). The dependent variable is indicated in each figure. The standardized health index is the average of the standardized probability of internal death, the log birht weight, the apgar score and the gestational age at birth. Counties with old housing stock are counties with above median share of houses built prior to 1978. County and year fixed effects, controls for mother's education, race and marital status, and a trend for implementing states are included. $T = 0$ 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 6: Effect of the Mandates on Fertility



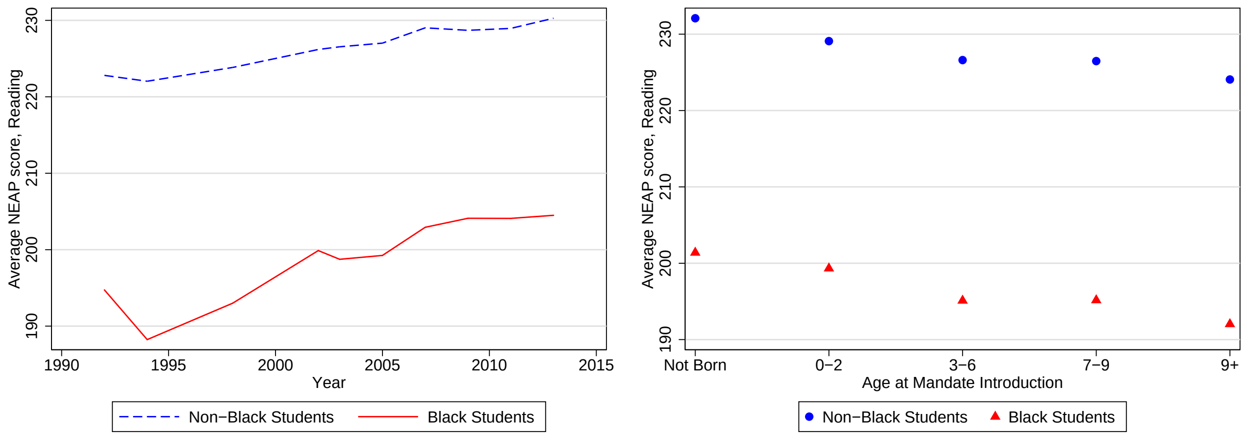
Source: Source: NCHS. The figure plots the year-by-year DD coefficients on the Birth Cohort Linked Birth and Infant Death Data of the National Vital Statistics System, 1989-1991 and 1995-2010 (2004 is missing). The dependent variable is the logarithm of the number of births in each state and year. The logarithm of county population and county and year fixed effects are included. $T = 0$ 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 7: 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 8: 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 (1)	Enactment Year (2)	Rentals Only (3)	Trigger (4)	Coverage (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	-0.217	-0.225*	-0.290*	-0.231**	-0.257**	-0.262*
	(0.136)	(0.132)	(0.151)	(0.110)	(0.103)	(0.134)
N	561	543	561	560	543	560
Average EBLL Rate, Non-Implementing States	0.021	0.021	0.021	0.005	0.005	0.005
Average EBLL Rate, Implementing States Pre-	0.023	0.023	0.023	0.006	0.006	0.006
Panel B: Mandates after 1996 Only (24 States)						
Post-Mandate	-0.137	-0.177*	-0.074	-0.202*	-0.261***	-0.137
	(0.124)	(0.102)	(0.110)	(0.110)	(0.099)	(0.096)
N	345	327	345	344	327	344
Average EBLL Rate, Non-Implementing States	0.021	0.021	0.021	0.005	0.005	0.005
Average EBLL Rate, Implementing States Pre-	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: Effects of the Mandates on Special Education Needs

Dependent Variable:	Log Rates 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	0.001	0.042	0.033**	0.042				
	(0.035)	(0.027)	(0.015)	(0.027)				
Exposed		-0.067**	-0.068**	-0.067**	-0.081**	-0.092**	-0.090**	0.012
		(0.034)	(0.027)	(0.034)	(0.038)	(0.038)	(0.044)	(0.026)
Maximum TANF/AFDC Benefit for Family of 3				0.00002				
				(0.00025)				
Exposed, Aged 11-13						0.058		
						(0.065)		
Aged 11-13						-0.221***		
						(0.022)		
N	8552	8552	8552	8552	8552	8552	8552	8536
Average SpEd Rates, Non-Exposed Cohorts	0.112	0.112	0.112	0.112	0.112	0.112	0.099	0.005
Average SpEd Rates, Exposed Cohorts, Treated States	0.121	0.121	0.121	0.121	0.121	0.121	0.101	0.007
State, Year FE	X	X	X	X				
Cohort FE		X	X	X	X	X	X	X
State-specific Linear Trends			X					
State-Year FE					X	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 the rate of 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 rate of 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 5: Effects of the Mandates on Special Education Needs, by Disability

Dependent Variable:	Log Rates 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	0.012	-0.230**	0.012	-0.021
	(0.047)	(0.112)	(0.054)	(0.082)
N	8521	8482	8505	8545
Average SpEd Rates, Non-Exposed Cohorts	0.010	0.032	0.007	0.049
Average SpEd Rates, Exposed Cohorts, Treated States	0.010	0.035	0.008	0.045

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 the rate of special education enrollment for the particular disability indicated in each column. State-year and cohort fixed effects are included. The average rate of special education enrollment 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 6: Effects of the Mandates on Special Education Needs, Steady State

Dependent Variable:	Log Rates of Children Aged 6-13 Receiving Special Education		
	All Grades	Elementary School	Middle School
	(1)	(2)	(3)
Unborn at Mandate	-0.219***	-0.171*	-0.044
	(0.035)	(0.027)	(0.015)
Aged 0-3 at Mandate	-0.126***	-0.111*	-0.039
	(0.000)	(0.034)	(0.027)
Aged 4-6 at Mandate	-0.064***	-0.065**	-0.012
	(0.000)	(0.000)	(0.000)
Aged 8-10 at Mandate	0.011	0.009	0.012
	(0.000)	(0.000)	(0.000)
N	8552	5345	3207
Average SpEd Rates, Aged 7 at Mandate	0.118	0.111	0.130
Average SpEd Rates, Control States	0.112	0.110	0.115

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 (Column 1), aged 6-10 (Column 2) and aged 11-13 (Column 3) for the years 1990-2010. Each observation is a state-year-age cell. The dependent variable is the logarithm of the rate of special education enrollment. State-year and cohort fixed effects are included. The average rate of special education enrollment for cohorts aged 7 at the introduction of a mandate and control 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, By Race

Dependent Variable:	Log Rate of Children Aged 6-21 Receiving Special Education					
Sample:	Lead Related			Not Lead-Related		
	(1)	(2)	(3)	(4)	(5)	(6)
Exposure	-0.099	-0.100		0.381**	0.380**	
	(0.188)	(0.186)		(0.152)	(0.152)	
Black	0.419***	0.419***	0.419***	0.044	0.044	0.044
	(0.053)	(0.053)	(0.073)	(0.055)	(0.055)	(0.076)
Exposure*Black	0.241	0.241	0.241	0.079	0.079	0.055
	(0.308)	(0.308)	(0.425)	(0.194)	(0.194)	(0.249)
Maximum TANF/AFDC Benefit for Family of 3		0.00010			0.00007	
		(0.00022)			(0.00040)	
N	1322	1322	1322	1302	1302	1302
Average SpEd Rates, Non-Exposed Cohorts	0.075	0.075	0.075	0.005	0.005	0.005
Average SpEd Rates, Exposed Cohorts, Treated States	0.079	0.079	0.079	0.005	0.005	0.005
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 the rate of special education enrollment 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 rate of special education enrollment 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 8: 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 9: 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 10: 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.

Table 11: Effects of the Mandates on Infant Health

Dependent Variable:	Standardized Health Index	Probability of Death by Internal Cause (*1,000)	Log Birth Weight	Probability of Birth Weight ≤2500	Probability of Birth Weight ≤1500	Apgar Score at 5 Minutes	Number of Gestational Weeks
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Average Effects							
Post-Mandate	-0.009*	0.352	-0.001	0.001	0.001**	-0.008	-0.033
	(0.005)	(0.222)	(0.001)	(0.001)	(0.000)	(0.014)	(0.027)
N	59662808	59662808	59638584	59638584	59638584	48064744	59146996
Mean Outcome, Control Counties	0.005	5.700	3299.767	0.076	0.014	8.896	38.772
Mean Outcome, Treated Counties Pre-Mandate	0.022	8.004	3325.824	0.078	0.015	8.910	38.948
Panel B: Effects by Age of the Housing Stock							
Post-Mandate	-0.022***	0.397	-0.002	0.001	0.0004	-0.038	-0.065**
	(0.007)	(0.469)	(0.002)	(0.002)	(0.0007)	(0.024)	(0.028)
Post-Mandate, Counties	0.020**	-0.074	0.001	-0.001	0.0003	0.049	0.050*
with Old Housing Stock	(0.009)	(0.555)	(0.002)	(0.002)	(0.0008)	(0.032)	(0.029)
N	59300124	59300124	59276064	59276064	59276064	47702968	58784688
Mean Outcome, Control Counties	0.005	5.700	3299.767	0.076	0.014	8.896	38.772
Mean Outcome, Treated Counties Pre-Mandate	0.022	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. The standardized health index is the average of the standardized probability of internal death, the log birht weight, the apgar score and the gestational age at birth. Counties with old housing stock are counties with above median share of houses built prior to 1978. County and year fixed effects, controls for mother's education, race and marital status, and a trend for implementing states 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 12: Effects of the Mandates on Fertility

Dependent Variable:	Log Number of Births				
	(1)	(2)	(3)	(4)	(5)
Post-Mandate	0.017	0.046	-0.031	0.038	-0.037
	(0.041)	(0.053)	(0.036)	(0.078)	(0.060)
Post-Mandate, Counties with Old Housing Stock				0.014	0.011
				(0.081)	(0.080)
N	47283	47238	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				
County FE		X	X	X	X
Differential Trends for Implementing States			X		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. Columns 2-5 include county fixed effects. Column 5 includes a trend for implementing states. The average number of births in control and pre-mandate implementing states is shown at the bottom of each column. Standard errors clustered at state level are shown in parentheses.

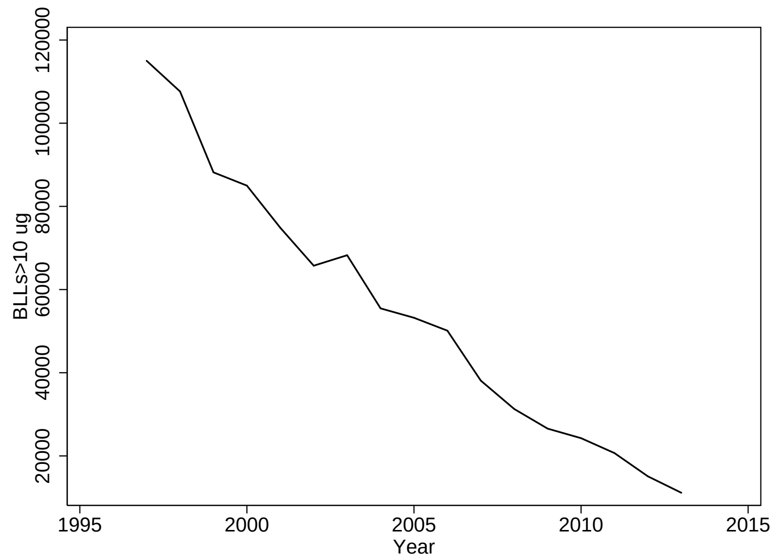
Table 13: Effects of the Mandates on Maternal Characteristics

Dependent Variable: Probability that Mother Is	High School Graduate or Below (1)	Black (2)	Not Married (3)
Panel A: Average Effects			
Post-Mandate	-0.004	0.010	-0.004
	(0.007)	(0.007)	(0.006)
N	71199096	60454952	72632824
Mean Outcome, Control Counties	0.551	0.129	0.654
Mean Outcome, Treated Counties Pre-Mandate	0.559	0.198	0.684
Panel B: Effects by Age of the Housing Stock			
Post-Mandate	0.002	0.019**	-0.006
	(0.010)	(0.008)	(0.006)
Post-Mandate, Counties with Old Housing Stock	-0.010	-0.015	0.004
	(0.014)	(0.011)	(0.009)
N	71198352	60454296	72632080
Mean Outcome, Control Counties	0.551	0.129	0.654
Mean Outcome, Treated Counties Pre-Mandate	0.559	0.198	0.684

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, the logarithm of the county population in each year, and a differential trend for implementing states are included in all columns. The share of mothers of each type in control and pre-mandate implementing states is shown at the bottom of each column. Standard errors clustered at the state level are shown in parentheses.

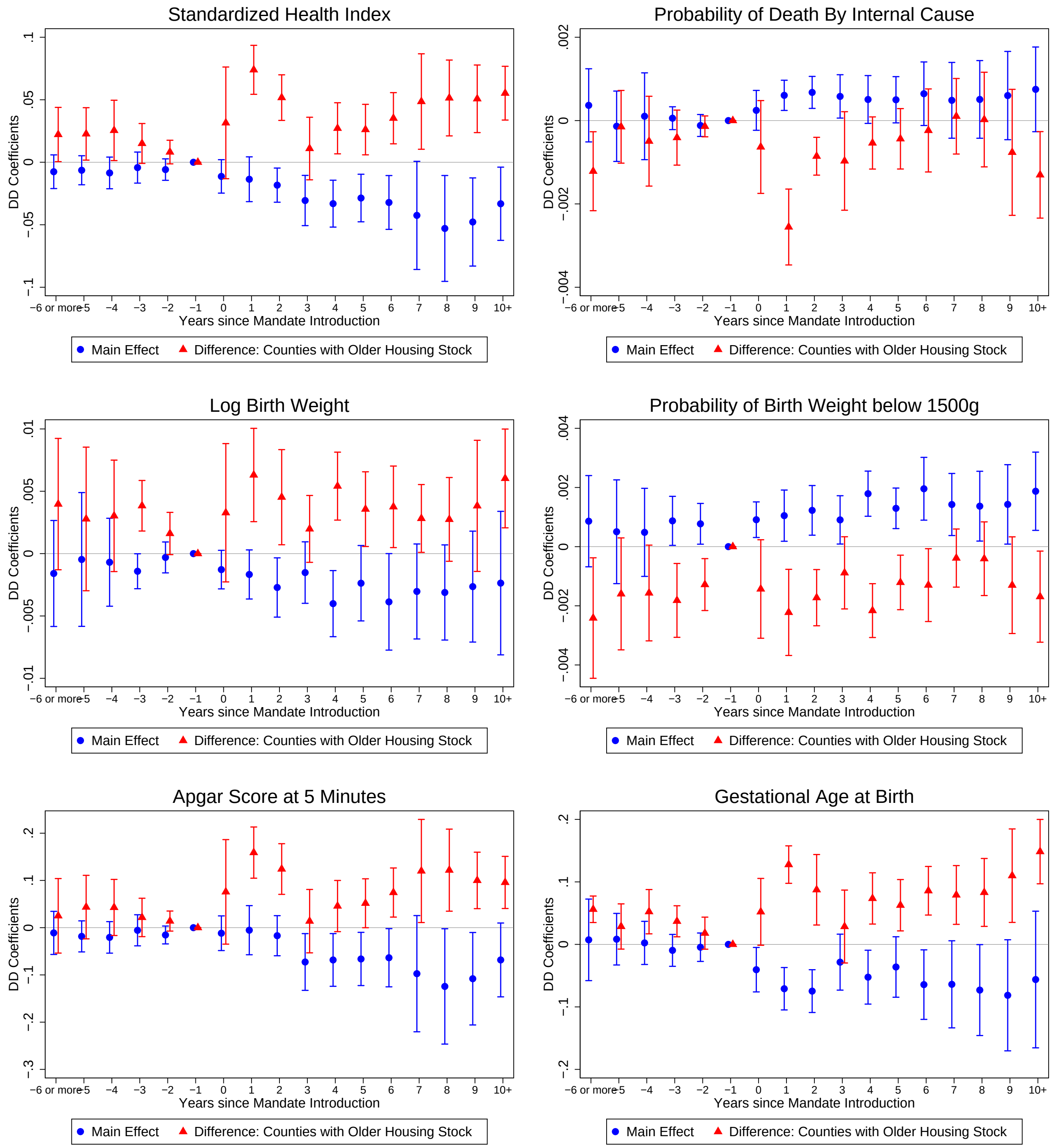
A Additional Figures and Tables

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: 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. The standardized health index is the average of the standardized probability of internal death, the log birht weight, the apgar score and the gestational age at birth. Counties with old housing stock are counties with above median share of houses built prior to 1978. County and year fixed effects, controls for mother's education, race and marital status, and a trend for implementing states are included. $T = 0$ 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 (1)	1988-1989 (2)	1992-1993 (3)	1994-1995 (4)	1999-2000 (5)	2008-2009 (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 (1)	1988-1989 (2)	1992-1993 (3)	1994-1995 (4)	1999-2000 (5)	2008-2009 (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, Rates

Dependent Variable:	Log Screening Rate		
	State, Year FE	Differential Trends by Baseline BLL	Differential Trends for Implementing States
	(1)	(2)	(3)
Panel A: Full Sample			
Post-Mandate	-0.076	0.091	0.131
	(0.227)	(0.200)	(0.209)
N	561	543	561
Average Screening Rate, Non-Implementing States	0.116	0.117	0.116
Average Screening Rate, Implementing States Pre-Period	0.218	0.218	0.218
Panel B: Mandates after 1996 Only (24 States)			
Post-Mandate	-0.152	-0.079	-0.155**
	(0.194)	(0.128)	(0.064)
N	345	327	345
Average Screening Rate, Non-Implementing States	0.116	0.117	0.116
Average Screening Rate, Implementing States Pre-Period	0.220	0.220	0.220

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 screening rate for lead poisoning over the population of children below 72 months of age. 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. The average screening rates 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: Lead Screening Effects, Counts

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	-0.165	0.026	0.067
	(0.223)	(0.185)	(0.201)
N	561	543	561
Average Number Kids Tested, Non-Implementing States	97775	101879	97775
Average Number Kids Tested, Implementing States Pre-Period	83728	83728	83728
Panel B: Mandates after 1996 Only (24 States)			
Post-Mandate	-0.197	-0.119	-0.188***
	(0.231)	(0.142)	(0.059)
N	345	327	345
Average Number Kids Tested, Non-Implementing States	97775	101879	97775
Average 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 screened for lead poisoning. 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.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 A.5: 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.6: Effects of the Mandates on Special Education Needs, Counts

Dependent Variable:	Log Number of Children Aged 6-13 Receiving Special Education		
	All Grades	Elementary School	Middle School
	(1)	(2)	(3)
Unborn at Mandate	-0.227***	-0.177*	-0.048*
	(0.078)	(0.091)	(0.029)
Aged 0-3 at Mandate	-0.133***	-0.117*	-0.044*
	(0.047)	(0.065)	(0.026)
Aged 4-6 at Mandate	-0.064***	-0.064**	-0.014
	(0.021)	(0.028)	(0.011)
Aged 8-10 at Mandate	-0.004	0.000	0.003
	(0.019)	(0.022)	(0.008)
N	8552	5345	3207
Average Number of Children on SpEd, Aged 7 at Mandate	8850	8382	9624
Average Number of Children on SpEd, Control States	8573	8209	9179

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 (Column 1), aged 6-10 (Column 2) and aged 11-13 (Column 3) for the years 1990-2010. Each observation is a state-year-age cell. The dependent variable is the logarithm of the number of children on special education. State-year and cohort fixed effects are included. The average number of children on special education for cohorts aged 7 at the introduction of a mandate and control 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, by Mother's Characteristics

Dependent Variable:	Standardized Health Index	Probability of Death by Internal Cause	Log Birth Weight	Probability of Birth Weight ≤2500	Probability of Birth Weight ≤1500	Ap Scor Min
	(1)	(2)	(3)	(4)	(5)	(6
Panel A: High School Graduate						
Post-Mandate	-0.022***	0.0003	-0.003**	0.002	0.001***	-0.0
	(0.006)	(0.0002)	(0.001)	(0.001)	(0.000)	(0.0
N	73277872	73277872	73241480	73241480	73241480	6100
Mean Outcome, Control Counties	0.005	0.006	3299.767	0.076	0.014	8.8
Mean Outcome, Treated Counties Pre-Mandate	0.022	0.008	3325.824	0.078	0.015	8.9
Panel B: Black Mothers						
Post-Mandate	-0.015**	0.0005	-0.002	0.001	0.001	-0.0
	(0.007)	(0.0005)	(0.003)	(0.003)	(0.001)	(0.0
N	9150728	9150728	9143525	9143525	9143525	8090
Mean Outcome, Control Counties	0.011	0.012	3104.334	0.130	0.030	8.8
Mean Outcome, Treated Counties Pre-Mandate	-0.007	0.015	3090.955	0.138	0.031	8.8
Panel C: Single Mothers						
Post-Mandate	-0.032***	0.0004	-0.005**	0.004*	0.001**	-0.0
	(0.008)	(0.0004)	(0.002)	(0.002)	(0.001)	(0.0
N	25285208	25285208	25271726	25271726	25271726	2141
Mean Outcome, Control Counties	0.012	0.008	3205.576	0.097	0.019	8.8
Mean Outcome, Treated Counties Pre-Mandate	-0.018	0.012	3170.294	0.114	0.024	8.8

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. The standardized health index is the average of the standardized probability of internal death, the log birht weight, the apgar score and the gestational age at birth. Counties with old housing stock are counties with above median share of houses built prior to 1978. County and year fixed effects, controls for mother's education, race and marital status, and a trend for implementing states are included in all columns.Counties with old housing stock are counties with above median share of houses built prior to 1950. 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.8: 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	-0.030	0.008	-0.046*	0.021	-0.032
	(0.035)	(0.039)	(0.026)	(0.056)	(0.043)
Post-Mandate, Counties with				-0.023	-0.025
Old Housing Stock				(0.050)	(0.049)
N	44280	44240	44240	44240	44240
Avg Births, Control Counties	897	897	897	897	897
Avg Births, Treated Counties Pre-Mandate	880	880	880	880	880
Panel B: Black Mothers					
Post-Mandate	-0.046	-0.018	0.054	-0.071	0.0004
	(0.092)	(0.115)	(0.152)	(0.167)	(0.189)
Post-Mandate, Counties with				0.100	0.101
Old Housing Stock				(0.162)	(0.162)
N	24560	24558	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	-0.010	-0.024	-0.034	-0.048
	(0.039)	(0.056)	(0.047)	(0.076)	(0.062)
Post-Mandate, Counties with				0.044	0.044
Old Housing Stock				(0.069)	(0.070)
N	40777	40743	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				
County FE		X	X	X	X
Differential Trends for Implementing States			X		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. Columns 2-5 include county fixed effects. Column 5 includes a trend for implementing states. 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.9: 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	0.046	-0.031	0.049	-0.025
	(0.041)	(0.053)	(0.036)	(0.079)	(0.061)
Post-Mandate, Counties with				-0.006	-0.010
Old Housing Stock				(0.081)	(0.081)
N	47283	47238	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	-0.030	0.008	-0.046*	0.031	-0.022
	(0.035)	(0.039)	(0.026)	(0.054)	(0.042)
Post-Mandate, Counties with				-0.039	-0.042
Old Housing Stock				(0.053)	(0.052)
N	44280	44240	44240	44240	44240
Avg Births, Control Counties	897	897	897	897	897
Avg Births, Treated Counties Pre-Mandate	880	880	880	880	880
Panel C: Black Mothers					
Post-Mandate	-0.046	-0.018	0.054	-0.127	-0.056
	(0.092)	(0.115)	(0.152)	(0.144)	(0.172)
Post-Mandate, Counties with				0.202	0.201
Old Housing Stock				(0.149)	(0.149)
N	24560	24558	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	-0.010	-0.024	-0.033	-0.046
	(0.039)	(0.056)	(0.047)	(0.073)	(0.059)
Post-Mandate, Counties with				0.039	0.039
Old Housing Stock				(0.070)	(0.071)
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				
County FE		X	X	X	X
Differential Trends for Implementing States			X		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. Columns 2-5 include county fixed effects. Column 5 includes a trend for implementing states. Standard errors clustered at state level are shown in parentheses.