The Price of a Safe Home: Lead Abatement Mandates and the Housing Market

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Abstract

State mandates require mitigation of old houses that expose a child to lead hazards. I estimate the mandates' effects on the housing market exploiting differences by state, year, and housing vintage. After a mandate, prices of old houses decline by 4.3-6.4 percent, consistent with abatement costs being higher than willingness-to-pay. Families with children become 17 percent less likely to live in old houses. However, rents for old houses and rental expenditures for these families increase, suggesting that increased awareness does not drive families away from old houses. As such, the mandates' weak enforcement appears to have important distributional consequences.

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1 Introduction

The Centers for Disease Control and Prevention (CDC) estimates that 535,000 children born in the US in the 2000s suffered from lead poisoning (**?**), a condition that is associated with reduced IQ (**?**) and educational attainment (**??**) and an increased risk of criminal activity (**????**).[1] What's more these effects develop at blood lead levels as low as $1-2\mu g/dL$, 80 times lower than the level of concern for iron (**??**). Indeed, the Secretary of the Department of Health and Human Services characterized lead poisoning as the "number one" environmental threat to children's health in the US (**?**). While having no biological value and posing such a threat to human health, lead's physical properties make it particularly suited for use in plumbing, paint, storage batteries, and as an additive to gasoline.

Since the deleading of gasoline between 1973 and 1995, lead paint is the major source of lead exposure in the United States: **?** estimates that nationwide, lead paint lingers in 5.5 million houses inhabited by small children, the population most at risk for lead poisoning, resulting in lead hazards in 21 percent of houses with small children, i.e. 3.7 million homes (**?**). In fact, lead paint was extensively used for residential purposes in the first half of the last century, until, beginning in 1971, a growing recognition of lead hazards motivated an increasing number of states to mandate abatement, i.e., control, and, in certain cases, elimination of lead hazards in older houses inhabited by children. However, abatement is expensive: **?** estimate that it can cost between \$500 and \$40,000, depending on the extent of the lead hazard. Unsurprisingly, not all owners comply with the mandates: 1.5 million houses were abated between 1999 and 2006 (**??**), and families with small children complain that landlords discriminate against them to avoid abatement (**??**). A such, the mandates might be void, or even have counterproductive effects.

This paper presents the first large-scale evidence on the effect of state abatement mandates on the housing market, thus providing the first incidence analysis of these

[1] This figure refers to children with blood lead levels (BLLs) above $5\mu g/dL$. Between 1991 and 2012, the CDC defined BLLs$\geq$ $10\mu g/dL$ as the level of concern for children aged 1–5 years. Since 2012, the term "level of concern" has been replaced with an upper reference interval value defined as the 97.5^{th} percentile of BLLs in US children aged 1–5 years from two consecutive cycles of National Health and Nutrition Examination Survey (NHANES), currently at $5\mu g/dL$.

policies. I compare outcomes for old and new houses within a state before and after a mandate's introduction, in a triple differences framework. This comparison is informative because lead regulations specifically target old houses, which are more likely to have lead hazards. My empirical analysis proceeds in two steps: first, I focus on property values; then I analyze households' allocation across houses and their housing expenditure. Together, my findings on prices and allocation shed light on owners' behavioral responses to the tightening of housing standards concerning a specific subpopulation, families with small children. These insights are especially relevant to evaluate the mandates given the scarcity of data on actual abatement at a granular level.

To estimate the effect of the mandates on house values, I use sales data, collected by DataQuick from public deeds. In particular, I investigate the effect of the mandates on rental and owner-occupied homes separately, using building structure as a proxy for tenancy, a choice variable for owners. My analysis shows that the costs imposed by the mandates are capitalized into lower home values: multi-family houses fall in value by 6.4 percent, i.e., by $4.80 per square foot, or 60 percent of the average abatement cost, and this fall in value persists up to ten years.[2] Old single-family homes persistently lose 4.3 percent of their value, and fewer of these houses appear to enter the rental market after a mandate. Arguably, earlier mandates are more likely to increase the salience of lead hazards than mandates enacted after national informational campaigns and federal regulations concerning lead hazards came into place. Nonetheless, I do not find evidence that earlier mandates cause larger decreases in the value of old houses than later mandates, suggesting that information is not the main channel that can explain the effect of the mandates on the housing market.

Under the null hypothesis of perfect information, this first set of results is not consistent with high rates of abatement because old houses should increase in value, relative to new ones, as they are made lead-safe. Hence, in the second part of my analysis, I use data from the American Housing Survey (AHS) to assess how households sort into old and new houses before and after the mandates.[3] Prior to

[2] I compute the average cost of abatement on 2014 Massachusetts data for projects funded by the US Department of Housing and Urban Development (HUD).

[3] In this paper I refer to dwellings as houses. In the analysis, the transaction data are at the

the mandates, high-income families with small children appear to disproportionally choose new houses, confirming that households know about lead hazards and trade off consumption and health.[4] After a mandate, families with small children are 17 percent less likely to live in old houses than before. This finding is also in line with low compliance rates: after a mandate, families with children would move into old houses if these houses were made lead-safe.

My analysis shows that the mandates decrease the value of old houses relative to new ones, but the mandates do not appear to decrease demand of old houses by increasing information about lead hazards. Then, why are families with small children less likely to live in old houses after a mandates? By requiring abatement only in the presence of small children, the mandates make it more costly to have small children in old houses. In this paper, I define discrimination as the restriction in the supply of houses available to families with small children at a given price. To test for discrimination, I estimate the effect of the mandates on rents for old houses with family-friendly characteristics. Consistent with owners discriminating against families with small children, rents for old family-friendly houses appear to increase after a mandate, while rents in newer family-friendly houses and in less family-friendly houses remain stable.

Thus, the mandates have real consequences even with low abatement rates: while owners bear part of the mandates' costs in terms of lower house prices, they pass along a portion of these costs to tenants with small children. These changes in the housing market imply that after a mandate, some families with children face higher rents in old houses, while others live in new and more expensive houses: in total, I calculate that families with children spend \$400 (or 6.4 percent) more per year on rent for several years after the introduction of a mandate. In the case of rental houses, we can think of the mandates as assigning the right to live in a lead-safe home to families with small children. Absent transaction costs, the Coase theorem applies, implying optimal abatement rates and transfers from landlords to renters with small children (**?**). However, the costs borne by these families seem to indicate a failure of the Coase theorem due to lax enforcement and discrimination.

property level, while in the AHS each unit constitutes an observation.

[4]In the paper, I use the term families to refer to households.

If households were sorting efficiently before the introduction of the mandates, then my findings suggest that the mandates decrease welfare. However, the existing data do not allow for a comprehensive welfare calculation. For example, I observe neither maintenance and abatement costs incurred by landlords nor commuting costs incurred by tenants. Moreover, households might not internalize the full costs of lead poisoning borne by society, in which case government intervention is needed to reduce lead exposure. Indeed, in related work, I uncover substantial costs related to lead poisoning in the special education sector (**?**).

My findings suggest that it is important to characterize how abatement mandates change the housing market equilibrium in order to compute the net impact of these policies, in line with the vast literature on government mandates and their unintended consequences (**???**). Furthermore, I provide another example of the principal-agent problem inherent in the landlord-tenant relation and its effects on environmental and public health issues (**?**). In related work, I find that the mandates decrease the probability of lead poisoning (**?**). However, the higher housing expenditures, spread over several years, appear to be of the same order of magnitude as the mandates' benefits on average for the families these regulations are intended to protect. By analyzing the incidence of the mandates, this paper changes the assessment of the mandates from a beneficial policy into a neutral one, on average, for families with small children. Similarly, I provide some caveats to the work by **?**, who show that Rhode Island's abatement mandate successfully decreased lead poisoning among African Americans, thus contributing to reducing the black-white test score gap in the state. Moreover, I contribute to a broad literature that explores the health effects of pollutants and neurotoxins commonly found in homes (**???**).

The paper proceeds as follows. Section 2 outlines a model to show that the impact of a mandate on prices and allocation depends on the strength of enforcement and on the extent of owners' discriminatory behavior. Section 3 provides background on lead poisoning as well as on the regulations studied in this paper, describing the data I use. Section 4 estimates the impact of the mandates on house prices and the allocation of households across houses. Sections 5 discusses the impact of the mandates on families' expenditures. Section 6 concludes with policy implications.

2 A Model of Abatement

I derive two sets of predictions regarding the introduction of abatement mandates in an urban rental housing market. First, the mandates always hurt owners of leaded homes. However, the effect of the mandates on property values is ambiguous: the more houses are abated, the more old houses will increase in value. Second, families with children move into old houses as they are abated, trading off consumption and health. However, if owners discriminate against families with small children, a mandate lowers the share of families with children in old houses and increases their housing expenditure. Similarly, a mandate lowers the share of families with children in old houses if it increases knowledge about lead hazards.

2.1 Set-up

Every period, a set of households of measure one optimize their consumption of a composite good, c, produced with a perfectly elastic supply at price $p_c = 1$, and of housing services, h. Households do not save or borrow and have no other assets; therefore, their consumption is equal to their income net of the housing expenditure. Houses differ only in the presence of lead paint, and each household rents one house at cost r_h, where $h \in \{L, N, 0\}$. The outside option, 0, can be interpreted as living with another household; its rent can be normalized to cost $r_0 = 0$. Notably, I assume that households have perfect information about houses' lead status; Section 2.4 drops this assumption.

Households vary across two dimensions: per-period income, $y_i \in [\underline{y}, \bar{y}]$, and child presence, $s_i \in \{0, 1\}$. Households maximise the following per-period utility:

$$max_h U(h; y_i, s_i, \alpha, r_L, r_N) = log(y_i - r_h) - \mathbb{1}(h = L)[\alpha_1 s_i + \alpha_0 (1 - s_i)] - \mathbb{1}(h = 0) H_0 \tag{1}$$

where $\mathbb{1}(h = L)$ is an indicator for leaded houses, α_1 (α_0) is the cost of lead poisoning to a family with(out) a small child, and $H_0 > \alpha_1 > \alpha_0 > 0$ is the disutility from the outside option. Although no one chooses the outside option, it pins down the rent *levels* in equilibrium. Hence, we can define $\tau = \frac{r_L}{r_N}$, the rental price of leaded houses, L, relative to safe ones, N.

By the concavity of utility of consumption, low-income households sort into leaded houses: although everyone dislikes lead, low-income households derive a high marginal utility from the additional consumption they get by living in a leaded house ($\tau \leq 1$ in equilibrium). Hence, the demand for leaded houses is decreasing in τ.

On the supply side, landlords maximize the net present value of rental income. For simplicity, I assume a fixed supply of houses of measure one, a fraction of which have lead paint initially.[5] An abatement technology turns leaded houses into safe homes at cost A, homogeneous across owners without loss of generality. Abatement is profitable if A is lower than the present value of the markup charged for safe houses. Hence, in an equilibrium with both leaded and non-leaded houses, the two values have to be equal.

2.2 Abatement Mandate

Unexpectedly, the government introduces an abatement mandate: with some enforcement probability $\pi > 0$, a leaded house needs to be abated at cost $A^M \geq A$. After she abates, the owner can charge the rent for a safe home. Assuming that households are perfectly informed about lead hazards, demand for leaded houses is unchanged. Normalizing r_N and with interest rate i , the value of a leaded houses under a mandate can be written as follows:

$$NPV_L^M = \frac{(1+i)(i\tau^M + \pi)}{i(i+\pi)} - A_M \frac{(1+i)\pi}{(i+\pi)} \tag{2}$$

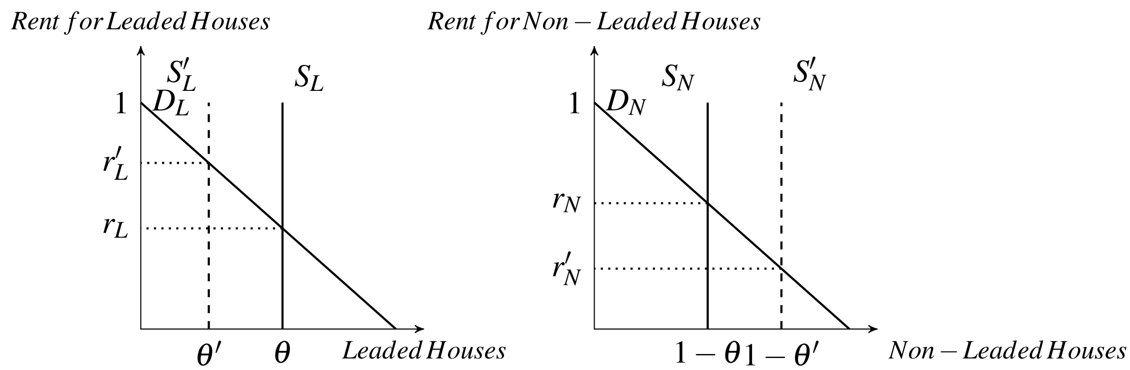
where the first term in equation (2) is the expected stream of rents from a currently leaded house and the second term is the present value of abatement cost.

By a revealed preference argument, $NPV_L^M < NPV_L$: the mandate lowers the value of a leaded house by introducing an additional cost with positive probability π. The assumption of a fixed supply of houses makes landlords more inelastic than

[5] The predictions in this section hold if I allow for an elastic supply of non-leaded houses. By definition, developers cannot build old houses, and I assume that no demolition or renovation takes place. Below, I discuss how the model's intuition carries through if we allow owners to sell rental houses to owner-occupiers.

tenants: even if rents increase due to the increased costs, this rise does not fully compensate owners. Indeed,the mandates introduce a wedge between the stream of future rents and the value of a house.

Figure 1: Equilibrium with Abatement



The figure shows the equilibrium in the housing market after a mandate induces abatement. The left panel depicts supply and demand of leaded houses. Abatement reduces supply of leaded houses to S'_L. As leaded houses become more scarce, their rent increases from r_L to r'_L. In contrast, abatement increases supply of non-leaded houses to S'_N (right panel). As non-leaded houses become more abundant, their rent decreases from r_N to r'_N.

Figure 1 shows how the mandate changes the housing market equilibrium under inelastic supply of leaded and non-leaded houses. As abatement reduces the number of leaded houses from θ to θ', households with children move into abated houses as they are made safe, increasing the share of children in old houses.

2.3 Discrimination

In this section, I illustrate the effect of a mandate when owners discriminate against families with small children by charging them higher rents to account for the mandate's costs. Under discrimination, a mandate lowers the share of families with children in old houses. Technically, price discrimination only refers to markets for homogeneous goods, and houses are hardly homogeneous, but I use this term in its legal interpretation. For simplicity, I allow discrimination only under the mandate and only in the leaded segment of the market.[6] Letting $\frac{\pi}{\mu}$ be the probability of a lead order conditional on a child living in a leaded house, I obtain:

$$NPV_L^D = \frac{(1+i)\{i[\mu\tau_1+(1-\mu)\tau_0]+\pi\}}{i(i+\pi-\mu)} - A_M\frac{(1+i)\pi}{(i+\pi-\mu)} - \phi T \tag{3}$$

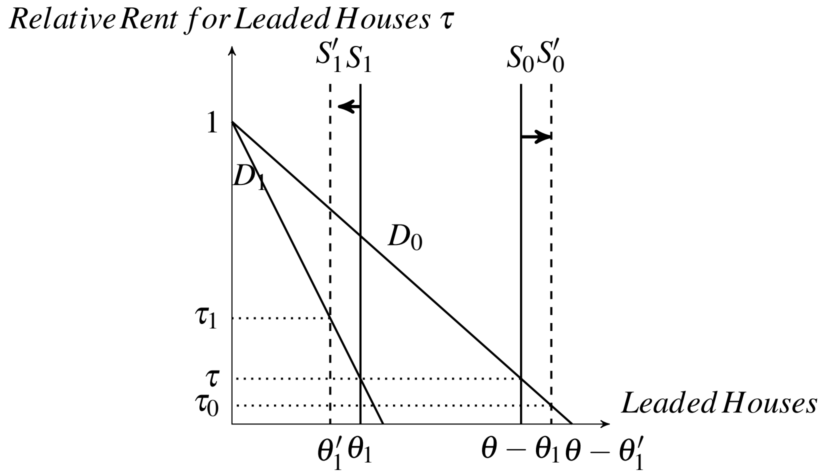
where ϕT is the expected fine for discriminating and τ_1 and τ_0 are rents paid by families with and without small children, respectively. The first term in equation (3) is the net present value of rents, a weighted average of rents paid by families with and without small children. The second term is the expected abatement cost, which depends on enforcement. By a revealed preference argument, the mandate still lowers the value of leaded houses.[7]

If $NPV_L^D > NPV_L^M$, the mandate induces discrimination and lowers the share of families with children in leaded houses. Moreover, under discrimination, the mandate increases the housing expenditures of families with children because they either move to safer and more expensive houses or pay higher rents for the same homes. Figure 2 illustrates the market for leaded houses under this scenario. Let D_1 and D_0 be the demand functions for leaded houses of households with and without small children, respectively. The solid lines S_1 and S_0 are the quantities supplied to families with and without children when mandates are in place but price discrimination is not possible: in this case, τ is such that the market for leaded houses clears. Under discrimination, owners effectively limit supply to families with small children by increasing their rents: the dashed line $S_1^{'}$ shifts in. Conversely, to attract childless households, owners offer them discounts: supply to these households, the dashed line $S_0^{'}$, shifts out. Hence, the effect of the mandates on average rents depends on the relative size of the two groups, parametrized by μ.

[6] The results in this section hold in the more general case in which discrimination is possible at all times and in all markets. Landlords in the non-leaded sector take advantage of the increased demand for safe homes by families with children and raise rents for these households as well. Hence, the total change in the relative rent of leaded houses will be dampened, but the direction of the change is the same.

[7] Discrimination is valuable if $i+\pi-\mu > 0$. A standard value for the interest rate, $i = 0.02$, and the population share of household with children, $\mu = 0.15$, yield $\varepsilon > 0.87$. Such a high enforcement probability is unusual, but it is conditional on the presence of lead hazards in the house.

Figure 2: Equilibrium with Discrimination, Market for Leaded Houses



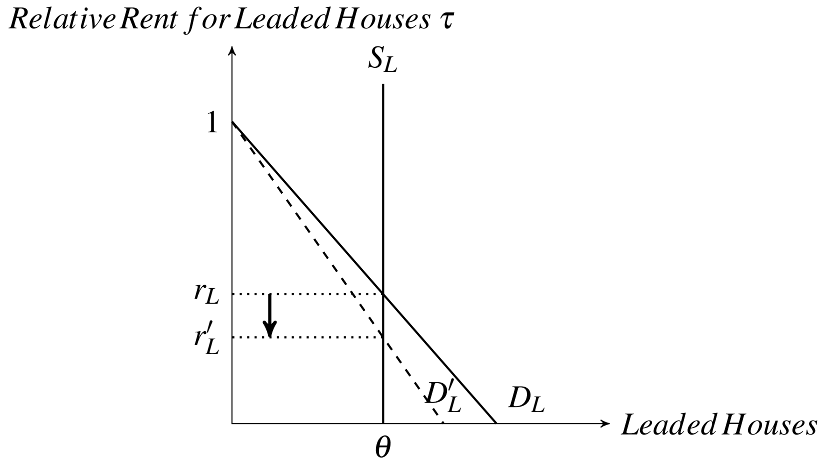
The figure shows the leaded segment of the housing market equilibrium with an abatement mandate and price discrimination. D_1 (D_0) and S_1 (S_0) represent demand for and supply of leaded houses for families with(out) small children. τ is the relative price of leaded houses that would prevail without discrimination, given by the intersection of the demand curves and the solid supply lines. Dashed supply lines S'_1 and S'_0 illustrate the equilibrium with price discrimination, where rent for families with and without children are given by τ_1 and τ_0, respectively.

2.4 Information

The mandates might provide information regarding the risks of lead poisoning for small children, decreasing families' willingness to pay for these houses. Figure 3 depicts the leaded segment of the market under this scenario. D_L represents the demand for leaded houses before the mandate. The mandate changes the perceived cost of lead poisoning for families with children to $\alpha_1 > \alpha_0$, making D'_L steeper. As a result, families with children move out of old houses, causing excess supply, and rent for old houses decreases until the market clears. As no abatement happens, there is no wedge between rents and home values, and old houses fall in value.[8]

[8] It is possible that the change in demand and the resulting change in relative prices spur voluntary abatement.

Figure 3: Equilibrium with Information on Lead Risks



The figure shows the leaded segment of the market when information changes the demand for leaded houses. Information lowers demand for leaded houses to D'_L, decreasing their rent r_L to r'_L.

3 Background and Data

3.1 Regulatory History of Lead Paint

Starting in the late 19th century, manufacturers typically added up to 50 percent lead by weight to paint to increase its durability (**?**). 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 percent, a level that can still induce severe lead poisoning (**?**). Finally, in June 1977, the Consumer Product Safety Commission (CPSC) lowered the allowed level of lead in paint to 0.06 percent, effectively banning lead paint altogether from 1978 on. Notably, the ban covers new paint, and not the pre-existing housing stock (**?**). Moreover, unless the paint coat containing lead is removed, lead remains in a house indefinitely. As a result, the incidence of lead paint in the current housing stock increases with structures' age, from 8 percent for houses built in the 1970s, to 86 percent for homes built before 1940 (**?**).

When paint surfaces deteriorate, residents, and especially children, are exposed to health hazards from lead-contaminated dust. Lead dust enters the human system through ingestion or inhalation. Small children are especially exposed to lead-contaminated dust from paint and windowsills due to normal hand-to-mouth activity (**?**). 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., **?**).

As of today, 19 states have enacted abatement mandates, as summarized in Table 1.[9] In my analysis, I treat all mandates as homogeneous to increase statistical power, although the mandates differ in terms of their coverage, what triggers a lead order, and type of abatement required. In results not reported in the paper, I find little evidence that the impact of the mandates depends on their characteristics.[10]

Anecdotally, enforcement of these mandates is lax, and abatement is slow. Unfortunately, there is little data on inspections, lead orders, and lead-safe certificates, and the existing figures are plagued by misreporting, as off-the-books voluntary lead inspections at sale are the norm in regulated states. Data from Maryland indicates that 200,000 houses, i.e., only a third of rental houses, have been inspected and certified under the state law that requires all rental homes to be registered.[11] In addition, Appendix Section A discusses that even in states with strict regulations, like Massachusetts, inspections are rare. Deleading projects are even more infrequent: in Massachusetts, only 28 percent of houses are abated after a lead hazard is identified. Ultimately, it appears that the lack of enforcement of these regulations can be harmful for public health, as data from Maryland, Massachusetts, New Jersey, and North Carolina show that 13 percent of houses with lead hazards present a new hazard later on.

At a more localized level, city governments also deal with issues related to lead paint and may enact regulations that are stricter than their state's requirements. To the extent that the timing of these city-level regulations is not correlated with the introduction of the state-level mandates, the lack of systematic information on local

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

[10] Furthermore, only a few states, such as Massachusetts, mandate universal blood lead screenings for children. In states where lead inspections are triggered only by elevated blood lead levels, the inspection and abatement rates will depend on screening.

[11] Source: Author's calculation on data from the Maryland Department of the Environment.

laws does not affect the validity of my findings.

3.2 Data

In this project, I combine data from two sources in order to analyze the impact of the mandates on house prices and housing choices.

Housing Prices. To assess the impact of the mandates on home values, I analyze price data at the transaction level obtained from the DataQuick data repository.[12] This is a dataset of public records of property sales (e.g., price, date, mortgage type) from 1988 until 2012 and of property characteristics collected from the most recent publicly available tax assessment and deeds records from municipalities across the US. The assessor file includes details on the physical characteristics (e.g., square footage, number of bathrooms, number of stories, year built), use type (e.g., residential, commercial, single-family, condominium, tenancy), and street address for every property in the covered counties. In the empirical analysis, I exploit the granularity of these data by including census tract fixed effects that restrict the comparison of outcomes across houses in the same neighborhood. Sales data provide a more precise estimate of the value of a property than assessed values; however, if the mandates affect the rate at which old houses are transacted, the estimates of mandates' effect on prices will suffer from selection bias. Because my results are robust to the inclusion of property fixed effects, I conclude that selection bias is not a concern in this context.

Based on the assessor file, the data cover approximately 90 percent of housing structures nationwide, although different counties enter the sample in different years from 1988, as shown in Figure B.1 in the Data Appendix. A comparison of Columns 2 and 3 of Table 1 shows that six implementing states are covered both before and after they introduce a mandate, namely, Connecticut, Georgia, Michigan, North Carolina, Ohio, and Rhode Island. The 3.5 million transactions in these states provide the identifying variation for the empirical analysis, while the other implementing states help estimate trends. In the empirical section, I thoroughly discuss how I establish that my findings are robust to using such an unbalanced panel.

[12] I accessed the data repository, housed at the Taubman Center for State and Local Government at the Harvard Kennedy School, during a visiting period under the Exchange Scholar Program.

In the empirical analysis, I study the effect of the mandates on house prices separately for rentals and owner-occupied properties, discussing the different mechanisms at play in these two segments of the market. In the assessor file, I infer that a house is owner-occupied if the owner's mailing address is the same as the property address. However, tenancy decisions are likely endogenous. Hence, I perform the analysis splitting the sample on a fixed characteristic of the house, i.e., I separate single- and multi-family homes.[13]

Rents, Housing Choice, and Housing Expenditures. To analyze the impact of the mandates on rents, occupancy, and households' expenditures, I use the AHS National Sample, years 1985-2009.[14] I drop observations in MSAs that cross state boundaries, since the mandates are state-level policies, resulting in 368,720 observations in 36 states. Column 4 of Table 1 reports which implementing states are in the AHS sample. Among those states, the ones that implement a mandate after 1985 provide the identifying source of variation for the empirical analysis. Notably, the AHS is a biennial panel of housing units, i.e., surveyors visit the same houses in each wave and do not follow movers; moreover, the data include a vast array of property characteristics, as well as household demographics and tenure duration.[15]

4 Empirical Analysis: Prices and Allocation

The model in Section 2 links the extent of abatement under a mandate to changes in prices and households' allocation for leaded houses relative to non-leaded ones. Using a house's vintage as a proxy for its lead status, I contrast outcomes for old and new houses within a state before and after a mandate in a triple differences framework (DDD). In other words, I estimate the effect of abatement mandates on prices and allocation, by fitting equations of the form:

$$Y_{ivst} = \beta Mandate_{st} * Old_v + \pi \mathbf{X_{it}} + \gamma_{sv} + \delta_{tv} + \eta_{st} + \varepsilon_{ivst} \tag{4}$$

[13] Appendix Table C.3 shows similar results when splitting the sample based on tenancy.

[14] The AHS was not conducted in 1987. Starting from 2011, the AHS uses a different sample, preventing comparisons with previous years.

[15] The AHS provides assessed home values for owned houses only, hence I do not use this variable. Moreover, the AHS only provides construction year in bins.

where Y_{ivst} is the outcome of interest for house i of vintage v, in state s and 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, Old_v is an indicator for houses targeted by the mandates, $\mathbf{X_{it}}$ is a vector of house characteristics that are potentially time-varying, and δ_{tv}, γ_{sv}, and η_{st} are time-vintage, state-vintage, and state-year fixed effects respectively. Specifically, Old_v equals one for houses built before 1950 in Maryland and 1978 elsewhere, and vintage refers to century of construction for houses built in the 1700s and 1800s and to decade for the 1900s. The controls included in $\mathbf{X_{it}}$ vary depending on the sample. In particular, the granularity of the transaction sample allows me to include tract-year and tract-vintage fixed effects that replace the respective state-level interactions. The introduction of tract fixed effects restricts the analysis to the comparison of old and new houses within a small area with a population of less than 10,000 individuals.[16] In addition, controlling for tract-vintage fixed effects allows me to control for local variation in the characteristics of the housing stock built at different times. For instance,the variation in the local availibility of natural gas at a given point in time is an important factor in determining the heating fuel of houses built at that time (**??**).[17] Finally, the panel nature of the AHS sample allows me to control for unit fixed effects, improving the precision of my estimates.[18]

By introducing state-year or tract-year fixed effects, I control non-parametrically for state-specific or tract-specific trends in the housing market, which might be correlated with the introduction of the mandates. Such correlation would arise, for instance, if urban flight and urban decay, which are associated with decreasing house values, lead to poorly maintained houses, and hence higher lead hazards and a stronger push to enact preventative regulations.[19] The setback of this specification

[16] Appendix Figure C.1 shows that there is considerable variation in the age of the housing stock even within such small neighborhoods for the case of Wayne County, Michigan.

[17] Appendix Figure C.3 shows that there is no sharp discontinuity in the shares of houses that are gas-heated or oil-heated around the year 1978. In particular, these shares are mostly constant in the 1970s and the early 1980s. Appendix Table C.4 shows that my results are robust to focusing on houses built in a small window of years around 1978, confirming that my findings are not driven by spurious fluctuations in fuel prices.

[18] In some specifications, I also include fixed effects for number of units, stories and rooms in the property.

[19] In results not reported in the paper, I find that while the estimates of the mandates' impact on the

is that I cannot estimate the effect of the mandates on the *level* of prices, i.e., the potential spillovers of the policies on new houses. Notably, the model outlined in Section 2 yields predictions on the relative prices of older and newer houses, as well as on the shares of households of a certain type living there, and not on the price of newer homes. Thus, the DDD framework is the correct approach to analyze the impact of these policies on the housing market.

The internal validity of the DDD framework hinges on the assumption that old and new houses 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_{ivst} conditional on the control variables. This would be violated, for instance, if local governments systematically introduced revitalization programs targeted differentially at old houses alongside the mandates. The first mandates were introduced in 1971 and the latest in 2005, suggesting that the regulations are idiosyncratic. To verify that the parallel trends assumption holds in the data, I estimate a year-by-year version of the DDD, as in the following equation, and present plots of the leads, α_y, and lags, β_y, of the mandates' effect on old houses:

$$Y_{ivst} = \sum_{y=1}^{T_{min}} \alpha_y Pre_{t-y,s} * Old_v + \sum_{y=0}^{T_{max}} \beta_y Post_{t+y,s} * Old_v + \pi \mathbf{X_{it}} + \gamma_{sv} + \delta_{tv} + \eta_{st} + \varepsilon_{ivst} \tag{5}$$

In the remainder of this section, I first analyze the effect of the mandates on sale prices (Section 4.1). Then, I relate the change in house values to the effect of the mandates on rents and the decision to rent a house (Section 4.2). In Section 4.3, I study how the housing market allocation changes as a result of the changes in prices and rents. Finally, I provide suggestive evidence on the mechanisms responsible for the estimated changes in prices, rents and allocation after the introduction of a mandate (Section 4.4)

price of old houses are robust to the exclusion of state-year FE, the estimates of the mandates' impact on new houses are not robust to different specifications of secular trends. Hence, I conclude that controlling non-parametrically for underlying secular trends is the best approach to obtain unbiased estimates of the mandates' impact on the housing market.

4.1 Sale Prices

I estimate the effect of the mandates on sale prices in the DataQuick sample separately for multi- and single-family homes, as enforcement might be stricter for rentals. The model in Section 2 shows that an abatement mandate increases the value of old houses that are remediated but reduces the price of old homes when abatement rates are low. Thus, this exercise sheds light on abatement rates even without data on abatement decisions.

Figure 4 plots year-by-year DDD estimates from a version of equation (5) that controls for tract-year fixed effects: abatement mandates erode the value of older homes relative to newer ones, both for multi-family (left panel) and single-family houses (right panel). In both panels, the relative price of old houses is constant up to several years prior to the mandates, although early leads are estimated somewhat imprecisely for multi-family houses, and it starts falling as soon as the mandate is announced. Moreover, the price drop persists for up to ten years after the mandates, a finding that excludes high abatement rates in response to the regulations. Panel A of Table 2 presents the corresponding point estimates for old multi-family houses: after the mandates, these houses fall in value by 6.4 percent on average (Column 1), a result that is robust to controlling for house fixed effects in Column 4. In particular, Column 2 indicates that older houses transacted up to four years after the mandate lose 3 percent relative to newer homes in their census tract, and the loss in value is over 8 percent in later years. This lagged effect is surprising: in a world of perfect information, owners should immediately internalize the costs induced by the mandate. However, uncertainty about the severity of enforcement at enactment can explain a delayed reaction by owners.[20]

An abatement mandate can affect single-family houses through two different mechanisms. First, some mandates require buyers to abate if the change in ownership results in a child entering a leaded house. Second, the abatement requirement for rentals might discourage owners of multiple single-family homes from renting out their second house. Panel B of Table 2 shows that after a mandate, single-family homes fall substantially in value, i.e., by 4.3 percent, and this point estimate is sta-

[20] Appendix Tables C.1 and C.2 provide a battery of robustness checks that confirm the results in Panel A of Table 2.

tistically indistinguishable from the effect of the mandates on multi-family houses. Interestingly, introducing house fixed effects in Column 4 reduces the estimated long-run effects for single-family homes. In other words, the mandates appear to slowly foster abatement the more houses are transacted, but the lack of data on abatement rates prevents a direct test of this hypothesis. Section 4.2 examines the mandates' impact on the rental market more closely, looking at both the extensive margin, i.e., the decision to rent out a house, and the intensive margin, i.e., the rents charged for different houses.

Until now, I have considered the impact of the mandates on all old houses indiscriminately, but the use of lead paint peaked before WWII and decreased gradually after 1950.[21] Hence, the probability of lead hazards is higher for houses built in the first half of the 20^{th} century, a fact that should be reflected in bigger value losses for very old houses. Pooling together multi- and single-family houses for issues of statistical power, Figure 5 shows that the effect of the mandate is indeed stronger for older vintages.[22] After the mandates, houses built in the 1990s appear to increase in value relative to houses built in the 1980s, most likely due to substitution patterns between houses of different vintages generated by building and demolition patterns in each neighborhood.[23]

My estimates of the losses in house values are quite large: prices of old multi-family houses drop by $4.80 per square foot on average, about 60 percent of the abatement cost. This figure is in line with estimates of the capitalization of the Clean Air Act by **?**; moreover, **?** find that federally-funded lead remediations that cost on average $7,291 increase home values in Charlotte, NC, by $20,000, with a 179 percent return on investment.[24] Given the low abatement rates and low enforcement probability observed in reality, even when one considers the high costs

[21]HUD estimates that 87 percent of houses built before 1940 in the US have lead paint, compared to 69 percent for houses built between 1940 and 1959 and 24 percent for houses built between 1960 and 1977 (**?**).

[22]Moreover, a comparison Columns 1 and 3 with Columns 2 and 4 of Appendix Table C.4 provides evidence that when limiting the analysis to vintages that are built within 10 years of each other, the difference in the estimates with and without tract-year fixed effects vanishes.

[23]The relative point estimates are shown in Appendix Table C.5.

[24]My estimates are also in line with the literature on the capitalization of pollution and school investments (**???????**).

associated with lead poisoning lawsuits, both their estimates and the large response of house prices to the mandate I estimate in this section are a puzzle. Nonetheless, it is worth noting that the observed average cost is an underestimate of the true abatement cost, for at least two reasons. On the one hand, we only observe abatement costs conditional on abatement; when costs are heterogeneous, only owners with relatively low costs will abate, meaning that observed costs belong to the lowest tail of the cost distribution. On the other hand, the observed abatement cost does not take into account the cost of funding for abatement projects, the psychic costs of interacting with government bureaucracy, or the opportunity cost of rent missed during abatement. Moreover, the mandates might foster maintenance and costly avoidance behavior, explaining why the loss in value is such a high fraction of the abatement cost: indeed, as the mandates specify requirements for the renovation of leaded houses, they impose a liability on these homes even when they do not get abated.

4.2 Rents

The results in the previous section suggest that the mandates lower the value of both rental and owner-occupied homes that are likely to have lead hazards. In this section, I ask (1) whether the mandates deter owners from participating in the rental market and (2) whether owners are able to shift part of the burden of the mandates to tenants. To study the effects of the regulations on the decision to rent and on rents, I estimate equation (4) in the AHS sample of multi- and single-family homes separately, introducing unit fixed effects as controls. The estimated effect of the mandates on the rental market is strikingly different for multi- and single-family houses. Columns 1 and 2 of Table 3 show no effect of the mandates on the owner's decision to rent an old unit in a multi-family house. This is not surprising: owners can only live in one of the units. Moreover, I find no statistically significant impact of the mandates on rents for old multi-family houses (Column 3), consistent with owners bearing most of the costs of the regulations.

Instead, Table 3 suggests that the mandates deter owners of old single-family houses from renting them out, although the estimates are quite imprecise. Column 6 further suggests that the contraction in the supply of old rental single-family houses exerts a temporary upward pressure on rents for these houses. Seven years after the introduction of a mandate, rents for old single-family homes relative to new ones appear to return to their pre-mandate levels, when new constructions might adjust to substitute for the old houses that are taken out of the rental market.

4.3 Allocation

The price effects of the mandates on both owner-occupied and rental homes and the simultaneous change in the pool of rental homes found in the previous sections raise the question of how abatement mandates affect housing allocation. Figure 6 shows that prior to the mandates, high-income families with small children are less likely to live in old houses. The same graph, indicates that after the mandates, even fewer low- and middle-income families with small children live in old homes.[25]

To confirm that these patterns are indeed caused by the mandates, I compare household characteristics in old and new houses before and after a mandate by estimating a version of equation (4) with unit fixed effects on the AHS sample. Column 1 of Table 4 finds 17 percent fewer families with small children in old houses. Plotting period-by-period estimates from equation (5), Figure 7 suggests that this effect is transitory, fading after six years. A plausible explanation is that the salience of lead hazards brought about by the introduction of a mandate decreases over time, reducing the reallocation effect to only its supply-side component as time passes. Indeed, Appendix Table C.8 shows that this pattern is more pronounced for single-family houses, where discrimination on the supply side is likely to be less important. Moreover, as children age and lead hazards become less threatening, inertia keeps families from moving back to leaded houses.[26] Indeed, Column 2 of Table 4 shows that children aged six to eleven are less likely to live in old homes four to ten years after a mandate. On the contrary, Column 3 of Table 4 shows that people over 59 years of age are no less likely to live in old houses: if anything, they replace families with small children, as the point estimate is actually positive.

[25] Appendix Table C.6 illustrates the average allocation of households in old and new houses before and after mandate.

[26] The average household in the AHS sample spends six years in the same rented house.

Finally, Columns 4-6 of Table 4 show no change in the demographic composition of households that live in old houses along income or racial lines. Appendix Table C.7 shows that the results in this section are robust to different specifications.

These findings indicate that the mandates keep households with small children away from old houses, which is inconsistent with voluntary abatement or compliance. If old houses were abated and made safer for families with children, these households should move into older homes. Moreover, Appendix Table C.8 shows that the mandates affect occupancy in multi-family houses the most: in other words, if any abatement takes place, it seems that it takes place differentially in single-family houses. Notably, the findings in this section do not require households to move at higher rates after the mandates. Indeed, in an analysis not reported in the paper, I find no evidence that the mandates induce higher turnover. In the AHS, on average over 50 percent of households with small children move in each wave: the mandates appear to steer some of these movers to new houses rather than old ones.

4.4 Mechanisms

The previous sections find that abatement mandates decrease the value of old houses and push families with small children into newer and safer homes. In this section, I test two alternative explanations for the reallocation of families with children out of old houses following the mandates: discrimination and information. As I don't find evidence that rejects the null hypothesis of perfect information and efficient sorting prior to the mandates, the findings in the previous sections suggest that the mandates decrease welfare due to lax enforcement and discrimination.

4.4.1 Discrimination

Anecdotally, some owners refuse to rent to families with small children.[27] Moreover, in a randomized audit study in Greater Boston, **?** show that to this day, landlords discriminate against prospective tenants with small children.

Unfortunately, we lack systematic data across states to directly assess whether landlords discriminate against families with children as a result of the mandates.

[27] Sources: Attorney General's Office, Civil Rights Division, Massachusetts.

As an indirect test, I exploit fixed house characteristics to identify homes that are attractive to families with small children, such as the number of bedrooms or the presence of a small child at baseline (year 1985). Columns 1-2 of Table 5 show that after a mandate, rents for old houses with two or more bedrooms increase by 6.4-7.4 percent. Estimates of changes in rents for new houses are not statistically significant when controlling for unit fixed effects. Furthermore, the mandates have no effect on rents for houses with less than two bedrooms, and if anything, the point estimate is negative. Columns 3-4 show similar but less precise results for houses inhabited by a small child in 1985.

These results alone do not prove that landlords discriminate based on family status. An alternative explanation is that family-friendly houses constitute a *de facto* separate segment of the market: the mandates act as a tax on these houses, which is reflected in higher rents. However, this is not consistent with the fact that rents for new family-friendly houses do not seem to increase.

4.4.2 Information

Another explanation for the decrease in the share of families with children living in old houses is that the mandates decrease families' willingness to pay for a leaded house by providing information regarding the risks of lead poisoning for small children. As discussed in Section 2.4, providing information about lead hazards decreases rents for old houses, which is inconsistent with my findings on family-friendly homes. To assess the role of information in shaping the impact of the mandates, I exploit the Residential Lead-Based Paint Hazard Reduction Act of 1992 (Title X), effective on December 12, 1996. The act mandates disclosure of known information on lead hazards before the sale or lease of houses built prior to 1978. The disclosure mandate arguably increased the salience of lead poisoning, and I expect mandates enacted before Title X to have a stronger impact than those implemented afterwards if the primary effect of a mandate is increase information on lead hazards.[28] On the contrary, Table 6 shows that mandates implemented after

[28]Using data from the AHS, **?** finds that the disclosure mandate increases buyers' testing at sale and reduces purchases of old homes among families with small children, and this is not differential by socioeconomic status. Surprisingly, in a follow up study, **?** finds no effect of the mandate on the

Title X have a bigger impact on home values, suggesting that the mandates and lead salience are complements. In other words, old houses lose more value after a mandate when households perceive the cost of lead poisoning to be higher.

5 Empirical Analysis: Families' Expenditures

The previous section provides evidence that the mandates displace families with children from old houses and that rents and prices respond in a manner that is consistent with a discriminatory equilibrium with low abatement rates. Hence, it is natural to ask how the mandates affect families' housing expenditures. To answer this question, I employ a framework in which the household is the unit of observation. In this setting, the mandates directly affect households with small children; other households are affected only indirectly through the adjustment in the housing market equilibrium. Hence, the DDD approach is valid for estimating the change in expenditures of households with and without children as long as the two groups are on parallel trends prior to the introduction of the regulations. Reassuringly, only 15 percent of households in the US have small children (see Appendix Table B.1), so the general equilibrium effects are likely to be small.

Table 7 presents estimates from fitting the following equation:

$$Y_{ijst} = \beta_0 Mandate_{st} * SeniorHH_j + \beta_1 Mandate_{st} * SmallKid_j + \gamma_{sj} + \delta_{tj} + \eta_{st} + \varepsilon_{ijst} \quad (6)$$

where Y_{ijst} is an outcome for household i of type j in state s in year t; types are given by the indicators $SmallKid_j$ for households with children aged six or below and $SeniorHH_j$ for households consisting only of members aged 60 or above; $Mandate_{st}$ is an indicator for year t being the year of the mandate's introduction in state s or any year thereafter; γ_{sj}, δ_{tj} and η_{st} are state-type, time-type and state-year fixed effects.

Columns 1-2 of Table 7 reiterate that after a mandate, families with small chil-

value of old houses, a finding that is at odds with the results in this paper on abatement mandates. The triple differences design in this paper allows me to compute the net effect of the abatement mandates on house prices and allocation accounting for potentially endogenous maintenance decisions, which **?** controls for, instead.

dren are less likely to live in old houses.[29] Moreover, although some of these families leave the rental market, this result is not robust to controlling for income, consistent with high-income families anticipating the purchase of a home (Columns 3-4). For those who still rent, this reallocation results in higher housing expenditures: Column 6 of Table 7 suggests that after a mandate, families with small children pay 6.6 percent higher rents on average, i.e., $400 more per year (in 2006 USD). This estimate is statistically significant at the 10 percent level, and the rent increase appears to be persistent over time (Figure 8). Notably, in an analysis not reported in the paper, I find no evidence that families with small children live in bigger or better homes after the mandates; hence, it appears that the higher housing expenditures these families incur are not compensated by better amenities. There is no evidence that senior households, who face lower risks of lead poisoning, pay higher rents after the mandates: if anything, the point estimate is negative. Finally, it is worth emphasizing that while these results hold for renters, I have no measure of housing expenditures for homeowners.

6 Conclusion

This paper exploits the variation in the timing of state-level lead abatement mandates, as well as the regulations' focus on old houses and families with small children, to estimate the policies' impact on the housing market equilibrium in a DDD framework. I show that 60 percent of the costs imposed on property owners by the mandates are capitalized into lower home values. Moreover, landlords shift a third of the burden of future abatement costs to families with small children who incur higher housing expenditures.

My findings from related work suggest that the mandates' costs and benefits are of the same order of magnitude for families with small children on average (**?**). However, some families might actually lose if the mandates result in a large displacement or a large increase in rental expenditure relative to their counterfac-

[29]Column 1 of Appendix Table C.9 suggests that families in the second and third quartile of the income distribution respond more strongly than both the lowest-income and the highest-income families. This is consistent with the model prediction that middle-income households are those at the margin.

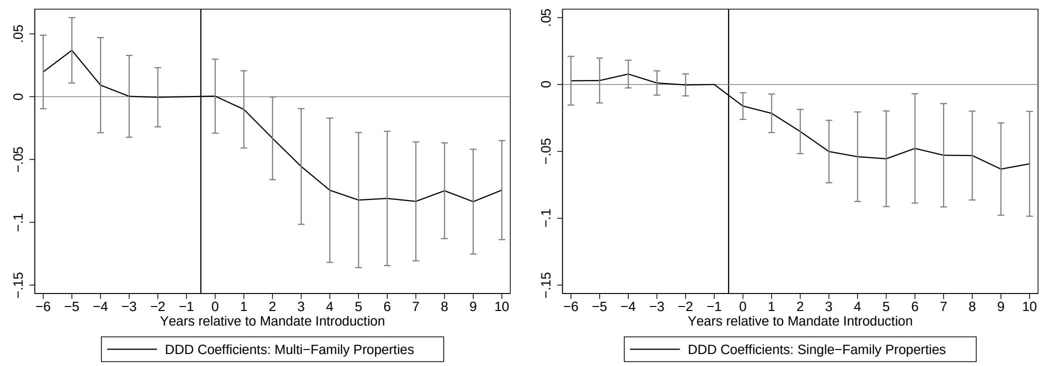
tual. Such a situation could arise both under the null hypothesis of efficient sorting, which the data do not reject, and under the null hypothesis of market failures that prevent low-income families from abating lead hazards in owner-occupied and rental homes. Indeed, the mandates appear to cause unintended consequences for the citizens they are supposed to protect due to discriminatory behavior. Stricter enforcement of control rights would ensure that the mandates do not result in transfers from tenants to property owners.

A different–and more complicated–question concerns the impact of an abatement mandate on social welfare. If the mandates generate misallocation, households might commute longer to work, for instance.[30] In addition, frictions in the housing market might waste resources, increasing the time needed to match households to houses. Finally, as families with small children represent a small fraction of the population, it is neither cost-effective nor feasible to require abatement of the entire US housing stock at once. However, as time passes, more and more paint deteriorates to the point of becoming a health hazard. Hence, the inability of the mandates to stimulate abatement is shifting the burden of lead poisoning to the future generations.

[30] In results not reported in the paper, I find suggestive evidence that families with small children indeed commute longer after the mandates, although I cannot reject the existence of pre-trends.

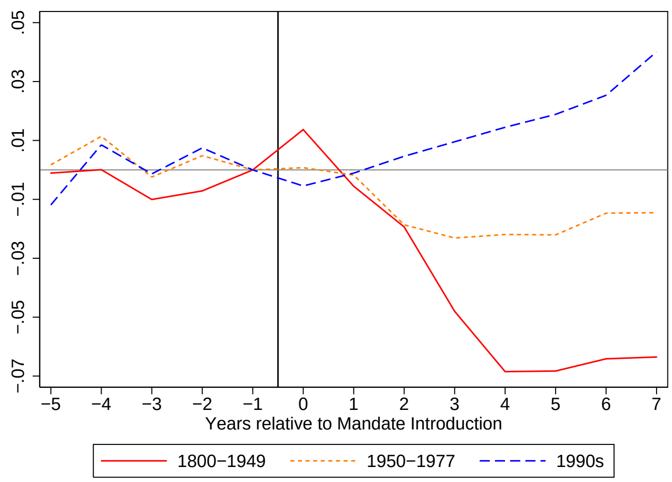
References

Figure 4: Price Effects



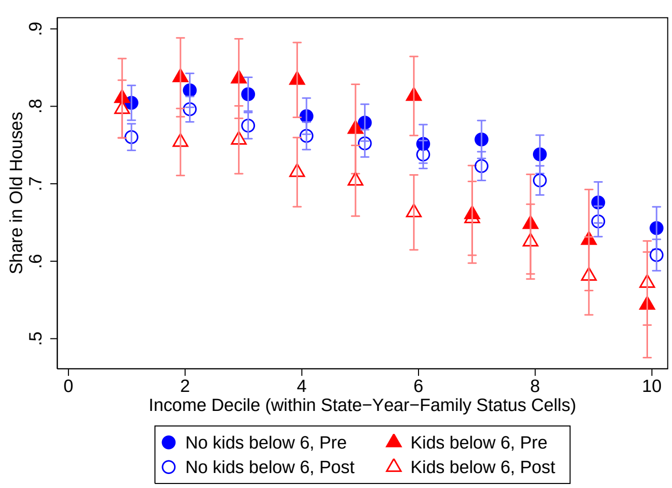
The figure plots DDD coefficients on year-by-year mandate dummies, estimated on the DataQuick samples (1988-2012) of multi- (left panel) and single-family (right panel) houses. Each census tract is weighted by 1980 population. The outcome variable is the logarithm of the price per square foot. The vertical line indicates the introduction of the mandate. For implementing states, the sample is limited to a [-6,10] window around the introduction of the mandates. Tract-year, tract-vintage and vintage-year fixed effects are included. T-1 is the omitted category. The vertical bars are 95 percent confidence intervals. Standard errors are clustered at the state level (42 clusters).

Figure 5: Price Effects, By Year of Construction



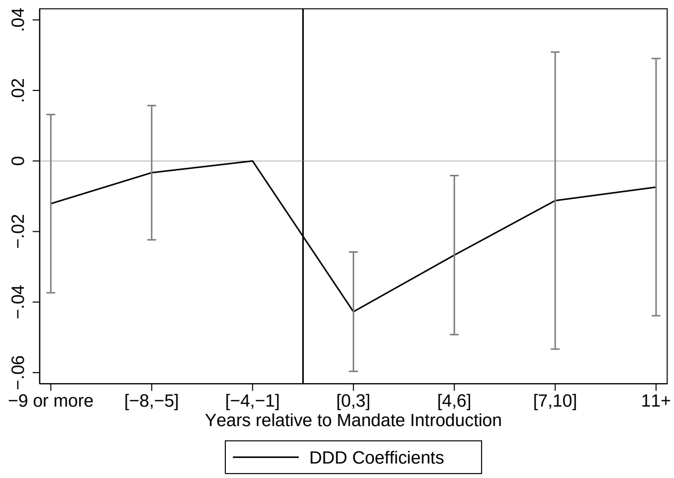
Notes: The figure plots DDD coefficients on year-by-year vintage dummies (1978-1989 is the omitted category), estimated on the DataQuick sample (1988-2012). Each census tract is weighted by 1980 population. The outcome variable is the logarithm of the price per square foot. The vertical line indicates the introduction of the mandate. T-1 is the omitted category. Tract-year, tract-vintage and vintage-year FE are included.

Figure 6: Sorting into Old Houses, By Income and Family Status



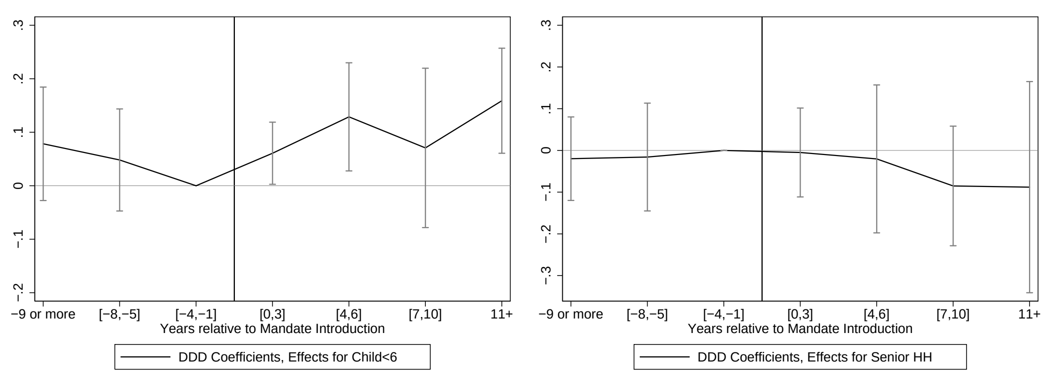
Notes: The figure plots the share of families in the AHS sample with (red triangles) and without (blue dots) children living in old houses in implementing states before (solid) and after (empty) the introduction of the mandates, by income decile. The vertical bars are 95 percent confidence intervals. The sample is limited to houses built between 1950 and 1999.

Figure 7: Allocation Effects: Child Under Six



The figure plots DDD coefficients on four-year mandate dummies, estimated on the AHS sample (1985-2009). The outcome variable is a dummy for the household having a child below six years of age. State-year, year-vintage, month of interview and unit fixed effects are included. The vertical line indicates the introduction of the mandate. $T \in [-4,-1]$ is the omitted category. The vertical bars are 95 percent confidence intervals. Standard errors are clustered at the state level (36 clusters).

Figure 8: Rent Expenditure Effects, by Family Status



The figure plots DDD coefficients on four-year dummies for households with a child below six years of age (left), and households above 59 years of age (right), estimated on the AHS sample for the years 1985-2009. The outcome variable is the logarithm of monthly rent. State-year, year-household characteristic, state-household characteristic, and month of interview fixed effects are included. Controls include second order polynomials of household's income. The vertical line indicates the introduction of the mandate. $T \in [-4,-1]$ is the omitted category. The vertical bars are 95 percent confidence intervals. Standard errors are clustered at the state level (36 clusters).

Table 1: State-Level Abatement Mandates

State (1)	Enactment Year (2)	DataQuick Start Year (3)	In AHS (4)	Rentals Only (5)	Trigger (6)	Coverage (7)
CT	1992	1988	Yes	No	<6 Year-old	All
DC	1983	-	No	No	<8 Year-old	All
GA	2000	1996	Yes	Yes	<6 Year-old with EBLL	Multifamily >12 units
IL	1992	1996	Yes	No	Children	All
KY	1974	2004	Yes	No	Children	All
LA	1988	2012	Yes	No	<6 Year-old	All
MA	1971	1988	Yes	No	<6 Year-old	All
MD	1995	1997	Yes	Yes	N/A	All
ME	1991	2005	No	No	<6 Year-old	All
MI	2005	1991	Yes	Yes	N/A	All
MN	1991	1998	No	No	Child with EBLL	All
MO	1993	1998	No	No	<6 Year-old	All
NC	1989	1988	Yes	No	<6 Year-old with EBLL	All
NH	1993	1996	No	Yes	<6 Year-old with EBLL	All
NJ	1971	1988	Yes	No	Children	All
OH	2003	1996	Yes	No	<6 Year-old with EBLL	All
RI	2002	1988	Yes	Yes	N/A	All
SC	1979	1990	Yes	No	Children	All
VT	1996	2002	No	Yes	N/A	All

The table displays the timeline of the introduction of abatement mandates in the 19 implementing states. Columns 2 and 3 contrast the mandates' enactment year with the year in which the state appears in the DataQuick Sample. Column 4 indicates whether the state is included in the AHS sample. Columns 5, 6, 7 characterize whether the mandate covers only rental homes, what triggers a lead order, and whether the type of buildings covered by the mandate.

TABLES

Table 2: Price Effects

Dependent Variable:	Log Price per Square Foot			
	(1)	(2)	(3)	(4)
Panel A: Mandate Effects, Multi-Family Properties				
0-10 Years After Mandate	-0.064			
	(0.014)			
0-3 Years After Mandate		-0.031	-0.032	-0.049
		(0.010)	(0.010)	(0.018)
4-6 Years After Mandate		-0.086	-0.087	-0.108
		(0.022)	(0.020)	(0.029)
7-10 Years After Mandate		-0.089	-0.091	-0.104
		(0.013)	(0.012)	(0.018)
N	3734665	3734665	3734558	2458772
Price Per SqFt, New Homes	105.483	105.483	105.483	108.670
Price Per SqFt, Old Homes	75.075	75.075	75.075	76.343
Panel B: Mandate Effects, Single-Family Properties				
0-10 Years After Mandate	-0.043			
	(0.011)			
0-3 Years After Mandate		-0.031	-0.031	-0.028
		(0.008)	(0.008)	(0.008)
4-6 Years After Mandate		-0.051	-0.051	-0.045
		(0.015)	(0.015)	(0.018)
7-10 Years After Mandate		-0.058	-0.057	-0.032
		(0.016)	(0.016)	(0.018)
N	16172953	16172953	16172827	9585516
Price Per SqFt, New Homes	105.560	105.560	105.560	107.504
Price Per SqFt, Old Homes	98.570	98.570	98.569	100.184
Controls			X	
Property FE				X

Notes: The table presents DDD estimates on DataQuick samples (1988-2012) of multi- (left panel) and single-family (right panel) houses. Each census tract is weighted by 1980 population. The outcome variable is the logarithm of the price per square foot. Tract-year, tract-vintage and vintage-year fixed effects are included. In addition, Column 3 includes house-specific controls (i.e., fixed effects for number of units, stories, and rooms in the building) and Column 4 includes house FE. For implementing states, the sample is limited to a $[-6, 10]$ window around the introduction of the mandates. Average price per square foot in implementing states before the mandates is shown separately for new and old houses at the bottom of each column. Standard errors clustered at the state level (42 clusters) are shown in parentheses.

Table 3: Rental Market Effects, Extensive and Intensive Margins

Sample:	Multi-Family			Single-Family		
Dependent Variable:	Entry into Rental	Exit from Rental	Log Monthly Rent	Entry into Rental	from Rental	Log Monthly Rent
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Mandate Effects on Old Houses, Single Post-Period						
Mandate Effects on Old Houses	0.112	0.016	0.024	-0.015	0.080	0.174
	(0.102)	(0.011)	(0.038)	(0.007)	(0.049)	(0.042)
Panel B: Mandate Effects on Old Houses, Multiple Post-Periods						
0-3 Years After Mandate	0.037	0.008	0.026	-0.006	0.021	0.306
	(0.074)	(0.016)	(0.035)	(0.007)	(0.043)	(0.058)
4-6 Years After Mandate	0.272	0.033	0.089	-0.023	0.116	0.114
	(0.146)	(0.017)	(0.067)	(0.010)	(0.079)	(0.050)
7-10 Years After Mandate	0.166	0.017	-0.078	-0.027	0.151	-0.011
	(0.116)	(0.011)	(0.096)	(0.010)	(0.094)	(0.136)
10+ Years After Mandate	0.058	0.014	0.047	-0.027	0.149	0.045
	(0.100)	(0.014)	(0.060)	(0.012)	(0.117)	(0.055)
N	45881	4260	30629	75640	8302	12267
Outcome Mean, New Homes	0.099	0.016	6.168	0.013	0.108	6.146
Outcome Mean, Old Homes, Pre-Period	0.073	0.015	5.823	0.021	0.156	5.880

Notes: The table presents DDD estimates on the AHS sample of multi- (Columns 1-3) and single-family houses (Columns 4-6) for the years 1985-2009. Outcome variables are defined in each column. State-year, year-vintage, month of interview and house fixed effects are included. Mean outcome values in implementing states before the mandates are shown separately for new and old houses at the bottom of each column. Standard errors clustered at the state level (36 clusters) are shown in parentheses.

TABLES

TABLES

Table 4: Allocation Effects

Dependent Variable:	HH has child <6	HH has child 6-11	Youngest HH member >59	Log Income	College Education	Black HH Head
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Mandate Effects on Old Houses, Single Post-Period						
Mandate Effects on	-0.026	-0.023	0.025	-0.040	-0.005	-0.017
Old Houses	(0.006)	(0.017)	(0.017)	(0.042)	(0.018)	(0.011)
Panel B: Mandate Effects on Old Houses, Multiple Post-Periods						
0-3 Years After	-0.039	0.002	0.013	-0.050	0.001	-0.015
Mandate	(0.008)	(0.017)	(0.015)	(0.047)	(0.013)	(0.013)
4-6 Years After	-0.023	-0.043	0.036	-0.009	-0.023	-0.005
Mandate	(0.010)	(0.023)	(0.017)	(0.056)	(0.022)	(0.012)
7-10 Years After	-0.009	-0.051	0.015	-0.072	0.029	-0.013
Mandate	(0.020)	(0.025)	(0.026)	(0.062)	(0.024)	(0.018)
10+ Years After	-0.005	-0.039	0.080	-0.009	-0.050	-0.064
Mandate	(0.018)	(0.034)	(0.025)	(0.047)	(0.033)	(0.037)
N	203316	203316	203316	178396	203316	203316
Outcome Mean, New Homes, Pre-Period	0.156	0.152	0.204	10.614	0.482	0.125
Outcome Mean, Old Homes, Pre-Period	0.150	0.153	0.267	10.182	0.357	0.211

Notes: The table presents DDD estimates on the AHS sample for the years 1985-2009. Outcome variables are defined in each column. State-year, year-vintage, month of interview and unit fixed effects are included. Mean outcome values in implementing states before the mandates are shown separately for new and old houses at the bottom of each column. Standard errors clustered at the state level (36 clusters) are shown in parentheses.

Table 5: Rent Effects by Number of Bedrooms and Children's Presence at Baseline

Dependent Variable:	Log Monthly Rent			
	(1)	(2)	(3)	(4)
Panel A: New Homes				
Post Mandate	0.046	0.024	-0.028	-0.015
	(0.039)	(0.045)	(0.030)	(0.033)
2+ Bedrooms, Post Mandate	-0.102	-0.083		
	(0.046)	(0.062)		
House has Child at Baseline,			0.010	0.044
Post Mandate			(0.310)	(0.260)
N	13110	13762	3876	3960
Log Rent, 0-1 Bedrooms	5.933	5.939		
Log Rent, No Child at Baseline			5.725	5.711
Panel B: Old Homes				
Post Mandate	-0.029	-0.040	-0.007	-0.012
	(0.039)	(0.036)	(0.033)	(0.030)
2+ Bedrooms, Post Mandate	0.074	0.064		
	(0.032)	(0.024)		
House has Child at Baseline,			0.089	0.049
Post Mandate			(0.033)	(0.032)
N	53225	57871	32382	34455
Log Rent, 0-1 Bedrooms	5.765	5.730		
Log Rent, No Child at Baseline			5.801	5.763
Controls	X		X	
Property FE		X		X

Notes: The table presents DDD estimates on the AHS samples of new (Panel A) and old houses (Panel B) (1985-2009). Column 1 includes a second order polynomial in square footage and fixed effects for #bedrooms-state, #bedrooms-year, number of units, condominium and month of interview. Column 2 includes unit, #bedrooms-year, and month of interview fixed effects. Column 3 includes a second order polynomial in square footage and fixed effects for state-child at baseline, year-child at baseline, number of units, condominium and month of interview. Column 4 includes unit, year-child at baseline, and month of interview fixed effects. The outcome variable is the logarithm of monthly rent. Mean outcome values in implementing states before the mandates are shown separately for houses with less than two bedrooms and for houses without children at baseline at the bottom of each column. Columns 3-4 exclude implementing states where a mandate was introduced before 1985. Standard errors clustered at the state level (36 clusters) are shown in parentheses.

Table 6: Price Effects Before and After Title X

Dependent Variable:	Log Price per Square Foot	
Sample:	Mandates Pre-Title X	Mandates Post-Title X
	(1)	(2)
Mandate Effects on Old Houses:		
0-3 Years After Mandate	-0.003	-0.025
	(0.012)	(0.009)
4-6 Years After Mandate	-0.037	-0.052
	(0.012)	(0.019)
7-10 Years After Mandate	-0.040	-0.063
	(0.014)	(0.019)
N	19744250	20301036
Price Per SqFt, New Homes, Pre-Period	112.845	106.668
Price Per SqFt, Old Homes, Pre-Period	108.674	92.009

Notes: The table presents DDD estimates on the transaction sample from DataQuick for the years 1988-2012, where each observation is weighted by population in 1980. The outcome variable is the logarithm of the transaction price divided by square footage of the house. For implementing states, the sample is limited to a [-6,10] window around the introduction of the mandates. Among implementing states, Column 1 only includes states that implement mandates before 1997, the year of the enactment of Title X, while Column 2 only includes states that implement mandates after 1997. Tract-year, tract-vintage and vintage-year fixed effects are included. Average price per square foot in implementing states before the mandates is shown separately for new and old houses in each subsample at the bottom of each column. Standard errors clustered at the state level (42 clusters) ares shown in parentheses.

Table 7: Tenancy Effects

Dependent Variable:	Old House		Rented		Log Monthly Rent	
	No Controls	Controls	No Controls	Controls	No Controls	Controls
	(1)	(2)	(3)	(4)	(5)	(6)
Post-Mandate,	-0.039	-0.035	-0.031	-0.013	0.063	0.064
Child <6	(0.014)	(0.014)	(0.015)	(0.016)	(0.038)	(0.036)
Post-Mandate, Youngest	0.015	0.027	-0.006	-0.002	-0.022	-0.028
HH Member >59	(0.013)	(0.015)	(0.016)	(0.019)	(0.036)	(0.061)
Income Controls		X		X		X
N	207427	182336	184021	180067	85235	72563
Outcome Mean, Pre-Period	0.832	0.832	0.349	0.349	6.263	6.263

Notes: The table presents DDD estimates on the AHS sample for the years 1985-2009. Post-mandate dummies are interacted with dummies for "Child below six years of age" and "Youngest HH Member above 59 years of age". Outcome variables are defined in each column. Rent is expressed in 2006 USD. State-year, year-household characteristic, state-household characteristic, and month of interview fixed effects are included. Controls include second order polynomials of household's income. Mean outcome values in implementing states before the mandates. Standard errors clustered at the state level (36 clusters) are shown in parentheses.

TABLES

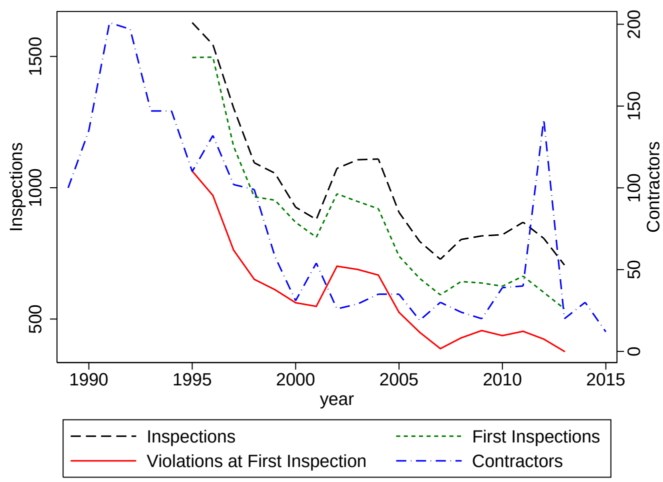
A The first stage on inspections and abatement

I collected data on lead inspectors and certified contractors, as well as on inspections and abatement projects, from selected states. The data are sparse and usually start after the introduction of the mandates, as the states set up registries in compliance with the regulations. Moreover, in general, voluntary inspections are not included. Finally, many of the inspectors' and contractors' licenses are dormant, as renewal costs are low compared to the initial fixed cost of obtaining a new license.

In Michigan, which implemented a mandate in 2005, 584 abatement projects were reported to the Department of Health and Human Services in fiscal year 2015, while there have been an average of over 1,000 projects in the fiscal years 2009-2013 in Ohio, which implemented a mandate in 2003.

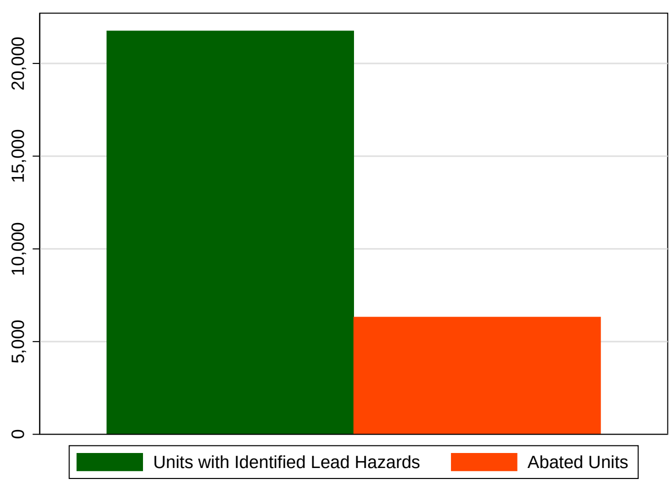
Here, I compare an early adopting state, Massachusetts, which introduced the lead mandate in 1971, with Ohio, which introduced it in 2004. Figure A.1 shows that, to this day, the state performs only 700 inspections per month, despite the fact that Massachusetts contains over 2.1 million houses built before 1978. Two thirds of these inspections visit a house for the first time. These figures have decreased over time, but remarkably, over the majority of first inspections find some lead hazard violations. In line with the trend in inspections, the number of certified lead contractors has also decreased over time. Nonetheless, licensed contractors seem to respond more to the funds available for training than to the changes in the housing stock, as emphasized by the spike starting in 2010, when the American Recovery and Reinvestment Act (ARRA) of 2009 increased local governments' ability to organize training workshops. A similar pattern is visible in Ohio, as shown in Figure A.4: after an initial spike in the number of licensed inspectors in 2006, their number goes back to the pre-mandate level, fluctuating between 700 and 800 active licenses per year. In Ohio, the number of licensed contractors does not respond to regulation, and it increases markedly in 2009, similarly to what happens in Massachusetts (Figure A.1).

Figure A.1: Enforcement, MA



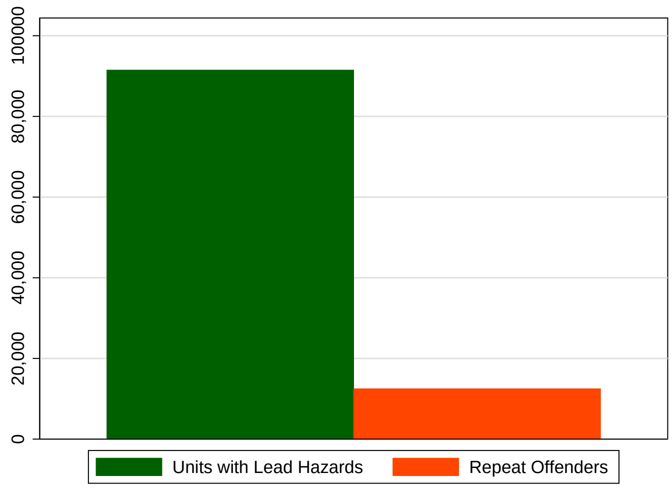
Source: Inspections data from Massachusetts Department of Public Health; lead-licensed contractors from Massachusetts Department of Labor Standards. The figure plots the number of inspections (black dashed), first inspections to a house (green dotted) and first inspections that find violations (red solid) on the left axis, and the number of contractors that are licensed for lead projects (blue dash-dot) on the right axis over calendar time in years.

Figure A.2: Houses with Identified Lead Hazards and Abated Houses, MA



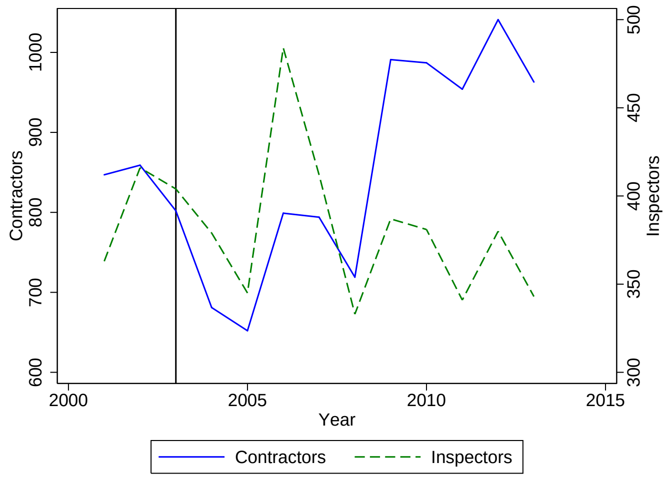
Source: Massachusetts Department of Public Health. The figure plots the number of houses with identified lead hazards (green bar) in Massachusetts for the years 1995-2015. A lead hazard can be identified either by an inspection outcome recording a violation or by an elevated blood lead level. The red bar illustrates how many of these houses are eventually abated.

Figure A.3: Houses with Identified Lead Hazards and Repeat Offender Houses



Sources: Massachusetts Department of Public Health (1995-2015), Maryland Department of the Environment (1995-2015), New Jersey Department of Health (1973-2015), North Carolina Department of Health and Human Services (1993-2015). The figure plots the number of houses with identified lead hazards (green bar) in Massachusetts, Maryland, New Jersey, and North Carolina. A lead hazard can be identified either by an inspection outcome recording a violation or by an elevated blood lead level. The red bar illustrates how many of these houses present a new lead hazard after the first one.

Figure A.4: Enforcement, OH



Source: Ohio Department of Health. The figure plots the number of contractors that are licensed for lead projects (blue solid) on the left axis and the number of lead inspectors on the right axis (green dashed) over calendar time in years. The vertical line indicates the year Ohio introduced a lead abatement mandate, 2003.

B Data Appendix

From the transaction file, I drop properties with missing characteristics and transactions that are not arms-length transfers, such as transactions between family members, to ensure that the sale price reflects the true value of the house.[31] When, according to the assessor file, properties undergo major renovations, I replace construction year with the renovation year because these renovations likely change the lead status of the house and because the renovations are public information available to the buyers. Indeed, in these cases, the assessor deems the original construction year not informative of the value of the house. My results are robust to both dropping these properties and including them with their original vintage. Then, I assign each geocoded property to a census tract according to 2010 boundaries, dropping observations in areas that were not tracted in 1980. To avoid comparing houses in neighborhoods that are fundamentally different in terms of age of the housing stock, I drop all tracts with only new or only old houses. This leaves over 27 million transactions for 18 million properties in 44,170 census tracts. Furthermore, in my preferred specification, I limit the sample for implementing states to observations in a window of $[-6, 10]$ years around the introduction of the policies to obtain a more balanced panel. Columns 1-2 in Appendix Table C.1 show that neither this sample restriction nor the unbalanced nature of the full panel affect the results.

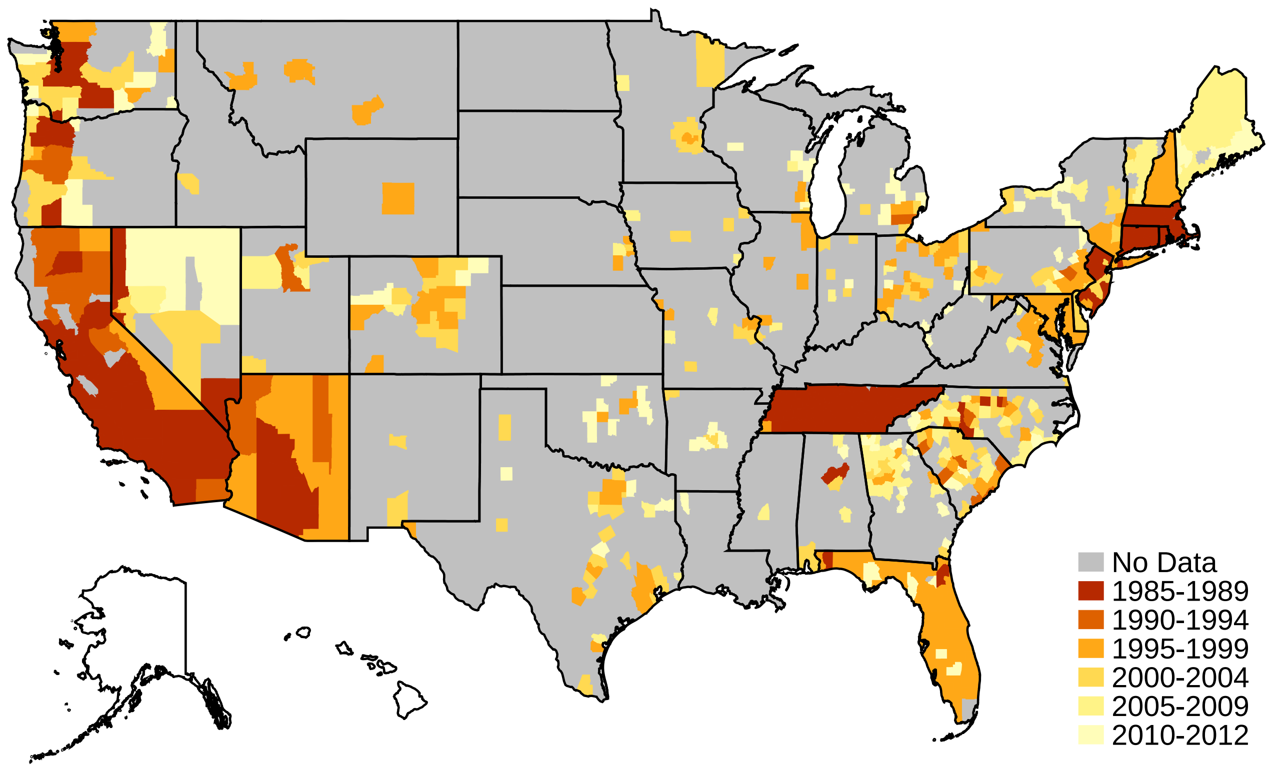
In the empirical analysis I estimate the effect of the mandates on house values separately for single- and multi-family homes. I include condominiums among the multi-family houses, as condominium conversion is as much an endogenous choice as tenancy is. Moreover, in my sample, 57 percent of multi-family properties and 40 percent of condominiums are rented, while only 21 percent of single-family properties are rented.

Table B.1 displays the characteristics of the housing stock in my two housing datasets: the DataQuick data repository and the AHS, as well as selected demo-

[31] Specifically, I drop duplicate transactions, transactions for less than \$10,000, not arms-length transfer, group-property sales, subdivisions and property splits, transactions that include liens or encumbrances or only partial interest in the property, and repeat sales. Moreover, I drop properties where any of the following characteristics is missing, provided that this information is not missing in more than 30 percent of the properties in the county: address, square footage, number of rooms, number of bathrooms, number of bedrooms, lot size, building's square footage, year of construction.

graphic characteristics from the AHS. Although the DataQuick and AHS samples are similar in terms of the size of the housing units, as well as house values and age of the housing stock, houses in the DataQuick sample are somewhat newer, and their average price per square foot is higher, likely reflecting selection in terms of what houses are transacted.

Figure B.1: Transaction Data Coverage



The figure shows a heat map of the coverage of the DataQuick data repository, by county and initial coverage year. Darker shades indicate counties that have been in the database the longest.

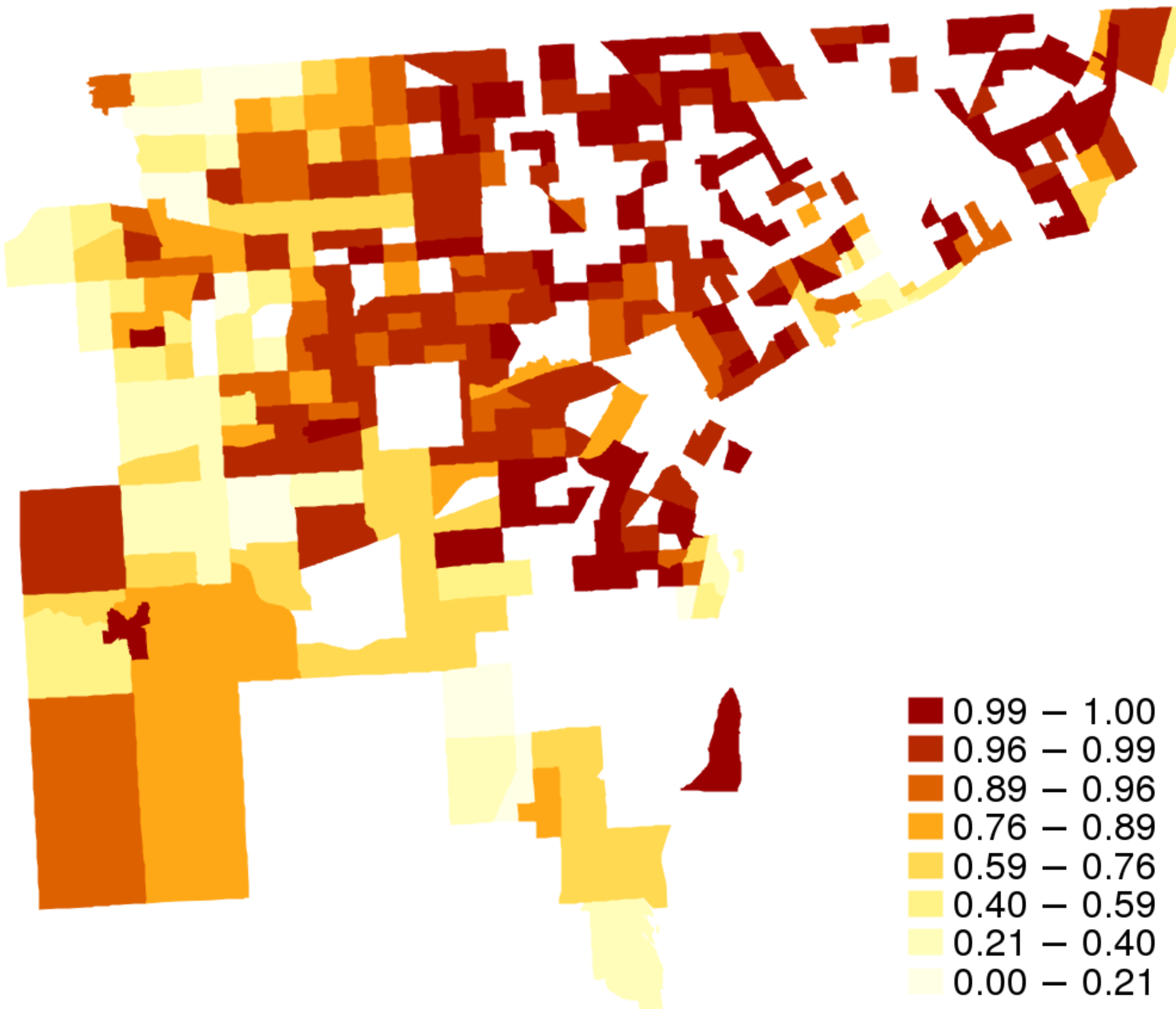
Table B.1: Summary Statistics

	DataQuick (1)	DataQuick, HMDA Sample (2)	AHS (3)
Price per Square Foot	144.91 (658.47)	167.16 (361.52)	132.67 (223.95)
Vintage	1968.52 (26.51)	1965.88 (26.57)	1956.88 (25.09)
Square Footage	1640.05 (683.29)	1635.06 (661.95)	1698.66 (1484.63)
Number of Rooms	6.39 (1.87)	6.47 (1.82)	5.39 (1.98)
HH has Child <6			0.15 (0.36)
HH has Child 6-11			0.15 (0.36)
House is Rented			0.40 (0.49)
Monthly Rent			628.77 (418.53)

Notes: The table reports summary statistics from the DataQuick sample (years 1988-2012) and the AHS sample (years 1985-2009). In the AHS sample, the price variable is the assessed value of home-owned houses and vintage is a 10-year bin starting in 1900 (AHS). Standard deviations are shown in parentheses.

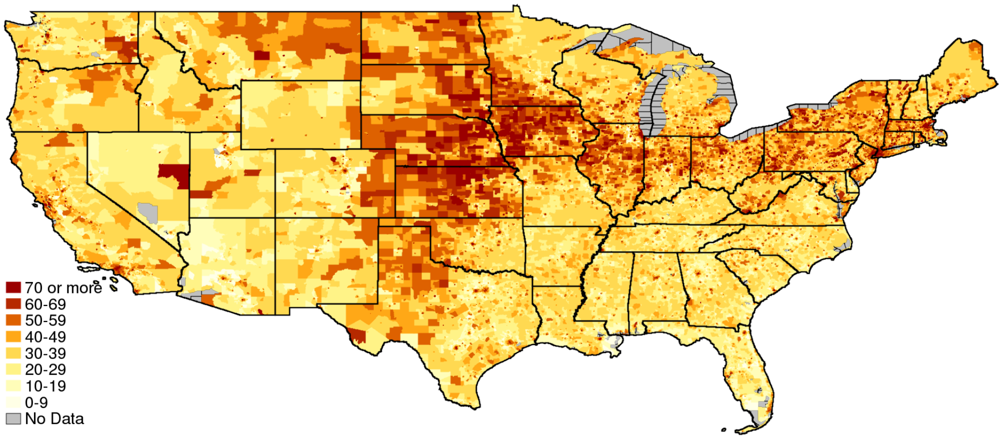
C Additional Figures and Tables

Figure C.1: Share of Old Houses in Wayne County, Michigan



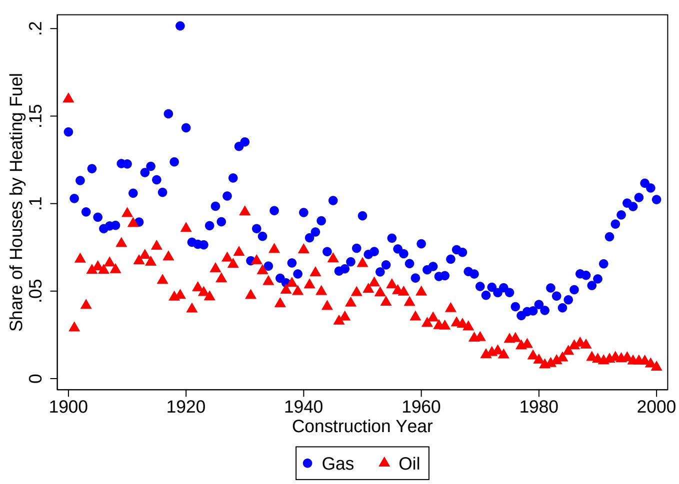
Notes: The figure plots the shares of old houses in census tracts in Wayne county, Michigan, in the DataQuick sample.

Figure C.2: Median Age of the Housing Stock by Tract as of 2010



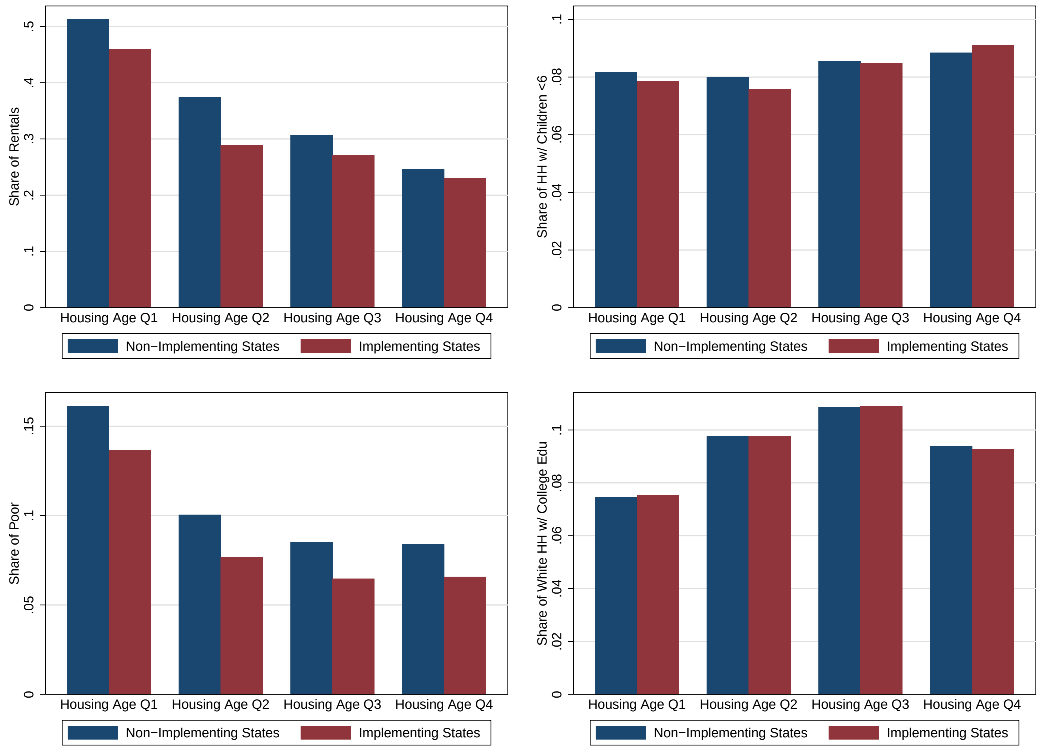
Source: NBCD. The figure plots the distribution of housing stock age in US census tracts, with darker colors assigned to tracts with older houses, i.e., where median age is higher. Age is computed in years.

Figure C.3: Share of Houses Built, by Heating Fuel



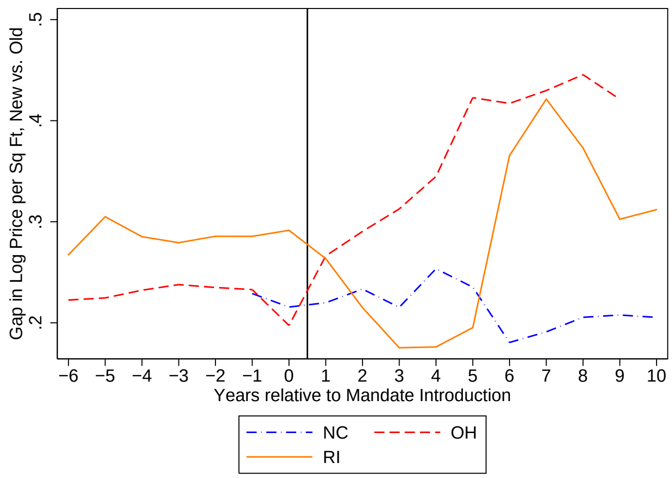
Source: DataQuick. The figure plots the share of houses heated by gas (blue dots) and oil (red triangles) built between the year 1900 and the year 2000.

Figure C.4: Correlation between Age of the Housing Stock and Demographics at the Tract Level



Notes: The figures plot the shares of rental houses, households with children below age five, households below poverty, and white, college-educated households, in each tract by quartile of average construction year (tracts in the first quartile have the oldest housing stock). Blue bars denote non-implementing states and red bars denote implementing states.

Figure C.5: Price Gap between Old and New Houses, By State



Notes: The figure plots the price difference between new and old houses over time relative to the introduction of a mandate in NC, OH, and RI, obtained from the DataQuick sample, 1988-2012. The vertical line at $t = 0$ indicates the introduction of the mandate in each state.

Table C.1: Price Effects for Multi-Family Houses, Alternative Specifications

Dependent Variable:	Log Price per Square Foot				
	Full Sample	Balanced Panel	Implementing States Only	No Weights	Exclude 1700s and 2000s Vintages
	(1)	(2)	(3)	(4)	(5)
Mandate Effects on Old Houses:					
0-3 Years After Mandate	-0.040	-0.005	-0.045	-0.032	-0.028
	(0.008)	(0.019)	(0.014)	(0.011)	(0.010)
4-6 Years After Mandate	-0.088	-0.058	-0.041	-0.085	-0.084
	(0.016)	(0.015)	(0.026)	(0.022)	(0.022)
7-10 Years After Mandate	-0.087	-0.051	-0.011	-0.084	-0.087
	(0.010)	(0.013)	(0.029)	(0.014)	(0.012)
11+ Years After Mandate	-0.086				
	(0.013)				
N	5213644	3760997	437869	3800679	3436234
Price Per SqFt, New Homes, Pre-Period	109.499	104.060	105.483	105.483	105.302
Price Per SqFt, Old Homes, Pre-Period	70.349	72.890	75.075	75.075	75.651

Notes: The table presents DDD estimates on the transaction sample for multi-family buildings from DataQuick for the years 1988-2012, where each observation is weighted by population in 1980. The outcome variable is the logarithm of the transaction price divided by square footage of the house. Tract-year, tract-vintage and vintage-year fixed effects are included. For implementing states, the sample is limited to a [-6,10] window around the introduction of the mandates, but for Column 1, which shows the estimates on the full sample. Column 2 presents estimates from a balanced sample that includes all non-implementing states and only those implementing states that have observations for all periods in a window of years around the introduction of the mandates, i.e., CT, GA, MI, NC, OH, RI. Column 3 limits the sample to implementing states only, i.e., CT, GA, IL, MD, MI, MN, MO, NH, NC, OH, RI, VT. Column 5 removes the 1980 tract population weights, and Column 6 drops the oldest (1700s) and most recent (2000s) vintages from the sample. Average price per square foot in implementing states before the mandates is shown separately for new and old houses at the bottom of each column for each estimation sample. Standard errors clustered at state level (42 clusters) are shown in parentheses, with the exception of Column 3 in which standard errors are clustered at state-vintage level.

Table C.2: Price Effects for Multi-Family Houses, Alternative Sets of Fixed Effects

Dependent Variable:	Log Price per Square Foot.			
	State-Year & State-Vintage FE	County-Year & County-Vintage FE	Tract-Year & State-Vintage FE	State-Year FE, State-Vintage Trends
	(1)	(2)	(3)	(4)
Mandate Effects on Old Houses:				
0-3 Years After Mandate	-0.050	-0.051	-0.029	-0.027
	(0.022)	(0.017)	(0.011)	(0.025)
4-6 Years After Mandate	-0.190	-0.180	-0.084	-0.147
	(0.028)	(0.026)	(0.024)	(0.041)
7-10 Years After Mandate	-0.167	-0.165	-0.084	-0.133
	(0.036)	(0.018)	(0.015)	(0.018)
N	3734674	3734674	3734666	0.0391951
Price Per SqFt, New Homes, Pre-Period	105.483	105.483	105.483	105.483
Price Per SqFt, Old Homes, Pre-Period	75.075	75.075	75.075	75.075
State-Vintage	X		X	X
County-Vintage		X		
Tract-Vintage				
State-Vintage Trends				X

Notes: The table presents DDD estimates on the transaction sample for multi-family buildings from DataQuick for the years 1988-2012, where each observation is weighted by population in 1980. The outcome variable is the logarithm of the transaction price divided by square footage of the house. The set of fixed effects included in each specification is defined in each column. Average price per square foot in implementing states before the mandates is shown separately for new and old houses at the bottom of each column for each estimation sample. Standard errors clustered at state level (42 clusters) are shown in parentheses.

Table C.3: Price Effects, by Occupancy

Dependent Variable:	Log Price per Square Foot			
Sample:	Rental		Owner-Occupied	
	(1)	(2)	(3)	(4)
Mandate Effects on Old Houses:				
0-10 Years After	-0.052		-0.031	
Mandate	(0.016)		(0.006)	
0-3 Years After		-0.040		-0.015
Mandate		(0.010)		(0.006)
4-6 Years After		-0.060		-0.043
Mandate		(0.023)		(0.010)
7-10 Years After		-0.061		-0.049
Mandate		(0.022)		(0.008)
N	5786770	5786770	15635580	5786770
Price Per SqFt, New Homes, Pre-Period	107.547	107.547	107.554	107.547
Price Per SqFt, Old Homes, Pre-Period	83.728	83.728	98.374	83.728

Notes: The table presents DDD estimates on the transaction sample of rental (Columns 1-2) and owner-occupied houses (Columns 3-4) from DataQuick for the years 1988-2012, where each observation is weighted by population in 1980. The outcome variable is the logarithm of the transaction price divided by square footage of the house. Tract-year, tract-vintage and vintage-year fixed effects are included. For implementing states, the sample is limited to a $[-6, 10]$ window around the introduction of the mandates. Average price per square foot in implementing states before the mandates is shown separately for new and old houses at the bottom of each column. Standard errors clustered at the state level (42 clusters) are shown in parentheses.

Table C.4: Price Effects, 5- and 10-Year Windows around Mandates

Dependent Variable:	Log Price per Square Foot			
Sample:	1973-1983 Vintages Only		1968-1988 Vintages Only	
	(1)	(2)	(3)	(4)
Mandate Effects on Old Houses:				
0-3 Years After	-0.005	-0.008	-0.010	-0.011
Mandate	(0.003)	(0.005)	(0.005)	(0.004)
4-6 Years After	-0.019	-0.017	-0.036	-0.025
Mandate	(0.007)	(0.007)	(0.012)	(0.011)
7-10 Years After	-0.034	-0.013	-0.049	-0.026
Mandate	(0.012)	(0.006)	(0.016)	(0.010)
N	1663170	1597531	2944413	2911256
Price Per SqFt, New Homes, Pre-Period	101.85	101.85	102.87	102.87
Price Per SqFt, Old Homes, Pre-Period	105.51	105.51	105.70	105.70
State-Year	X		X	
Tract-Year		X		X
Vintage-Year	X	X	X	X
State-Vintage	X		X	
Tract-Vintage		X		X

Notes: The table presents DDD estimates on the transaction sample from DataQuick for the years 1988-2012, where each observation is weighted by population in 1980. The sample is limited to houses built between 1973 and 1983 in Columns 1-2 and between 1968 and 1988 in Columns 3-4. The outcome variable is the logarithm of the transaction price divided by square footage of the house. Tract-year, tract-vintage and vintage-year fixed effects are included, where vintage is construction year. For implementing states, the sample is limited to a [-6,10] window around the introduction of the mandates. Average price per square foot in implementing states before the mandates is shown separately for new and old houses at the bottom of each column. Standard errors clustered at the state level (42 clusters) are shown in parentheses.

Table C.5: Price Effects by Year of Construction

Dependent Variable:	Log Price per Square Foot		
	1800-1949	1950-1977	1990s
	(1)	(2)	(3)
Mandate Effects on Old Houses:			
0-3 Years After Mandate	-0.012	-0.014	0.001
	(0.016)	(0.005)	(0.012)
4-6 Years After Mandate	-0.061	-0.021	0.018
	(0.032)	(0.010)	(0.023)
7-10 Years After Mandate	-0.058	-0.017	0.037
	(0.028)	(0.014)	(0.014)
N		17964990	
Price Per SqFt, 1980s Houses, Pre-Period		112.21	
Price Per SqFt, Relevant Vintage, Pre-Period	75.18	103.19	106.51

Notes: The table presents DDD estimates from a single regression on the transaction sample from DataQuick for the years 1988-2012, where each observation is weighted by population in 1980. The oldest (1700s) and most recent (2000s) vintages are dropped from the sample. Houses built in the 1980s are the omitted category. Tract-year, tract-vintage and vintage-year fixed effects are included. For implementing states, the sample is limited to a [-6,10] window around the introduction of the mandates. Average price per square foot in implementing states before the mandates is shown separately for each vintage at the bottom of each column. Standard errors clustered at the state level (42 clusters) are shown in parentheses.

Table C.6: Allocation Summary Statistics

	Old Houses		New Houses	
	Before Mandate	After Mandate	Before Mandate	After Mandate
	(1)	(2)	(3)	(4)
Child <6	0.17	0.16	0.15	0.14
	(0.37)	(0.36)	(0.36)	(0.34)
Child 6-11	0.16	0.16	0.15	0.14
	(0.37)	(0.37)	(0.36)	(0.35)
HH >59	0.22	0.23	0.27	0.29
	(0.41)	(0.42)	(0.45)	(0.45)
Log Income	10.63	10.73	10.31	10.42
	(1.03)	(1.14)	(1.10)	(1.16)
College Educated	0.57	0.63	0.45	0.51
	(0.49)	(0.48)	(0.50)	(0.50)
Black HH Head	0.10	0.18	0.14	0.19
	(0.30)	(0.38)	(0.35)	(0.39)

Notes: The table reports summary statistics of characteristics of households living in old and new houses before and after a mandate, from the AHS sample (years 1985-2009). Standard deviations are shown in parentheses.

Table C.7: Allocation Effects, Robustness Checks

Dependent Variable:	HH has child <6 (1)	HH has child <6 (2)	Youngest HH member >59 (3)	Youngest HH member >59 (4)
Panel A: Mandate Effects on Old Houses, Single Post-Period				
Mandate Effects on Old Houses	-0.027	-0.044	0.027	-0.034
	(0.006)	(0.011)	(0.016)	(0.015)
Panel B: Mandate Effects on Old Houses, Multiple Post-Periods				
0-3 Years After Mandate	-0.041	-0.046	0.018	-0.027
	(0.011)	(0.012)	(0.016)	(0.016)
4-6 Years After Mandate	-0.028	-0.037	0.036	-0.021
	(0.015)	(0.022)	(0.016)	(0.020)
7-10 Years After Mandate	-0.002	-0.027	0.011	-0.049
	(0.018)	(0.029)	(0.023)	(0.032)
10+ Years After Mandate	-0.005	-0.040	0.088	0.001
	(0.014)	(0.042)	(0.021)	(0.039)
N	203316	203316	203316	178396
Pre-Period	0.156	0.152	0.204	10.614
Pre-Period	0.150	0.153	0.267	10.182
State-Year	X	X	X	X
Vintage-Year	X	X	X	X
State-Vintage	X		X	
Property		X		X
State-Vintage-Specific Trends		X		X

Notes: Table presents DDD estimates on the AHS sample for the years 1985-2009. The outcome variables are defined in each column. State-year, year-vintage, state-vintage and month of interview fixed effects are included in Columns 1 and 3; state-year, year-vintage, month of interview, house fixed effects and state-vintage-specific linear trends are included in Columns 2 and 4. Standard errors clustered at the state level (36 clusters) are shown in parentheses.

Table C.8: Allocation Effects by Housing Structure

Sample:	Multi-Family			Single-Family		
Dependent Variable:	HH has child <6	HH has child 6-11	Youngest HH member >59	HH has child <6	HH has child 6-11	Youngest HH member >59
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Mandate Effects on Old Houses, Single Post-Period						
Mandate Effects on	-0.058	-0.030	-0.014	-0.005	0.024	-0.027
Old Houses	(0.030)	(0.019)	(0.041)	(0.015)	(0.029)	(0.017)
Panel B: Mandate Effects on Old Houses, Multiple Post-Periods						
0-3 Years After	-0.082	-0.003	-0.003	-0.024	0.058	-0.030
Mandate	(0.025)	(0.013)	(0.037)	(0.134)	(0.036)	(0.032)
4-6 Years After	-0.062	-0.054	0.018	0.111	-0.050	0.013
Mandate	(0.043)	(0.034)	(0.061)	(0.103)	(0.033)	(0.037)
7-10 Years After	-0.032	-0.062	-0.062	-0.238	0.040	-0.030
Mandate	(0.036)	(0.027)	(0.059)	(0.140)	(0.055)	(0.031)
10+ Years After	-0.035	-0.031	0.002	0.097	-0.060	-0.107
Mandate	(0.037)	(0.033)	(0.051)	(0.095)	(0.074)	(0.073)
N	54542	54542	54542	108823	108823	108823
Outcome Mean, New Homes, Pre-Period	0.061	0.034	0.369	0.203	0.228	0.129
Outcome Mean, Old Homes, Pre-Period	0.128	0.101	0.309	0.156	0.175	0.249

Notes: The table presents DDD estimates on the AHS sample of multi- (Columns 1-3) and single-family houses (Columns 4-6) for the years 1985-2009. Outcome variables are defined in each column. State-year, year-vintage, month of interview and unit fixed effects are included. Mean outcome values in implementing states before the mandates are shown separately for new and old houses at the bottom of each column. Standard errors clustered at the state level (36 clusters) are shown in parentheses.

Table C.9: Tenancy Effects by Income Quartiles

Dependent Variable:	Old House	Rented	Log Monthly Rent
	(1)	(2)	(3)
Post-Mandate, Child <6	0.003	-0.008	0.052
	(0.025)	(0.019)	(0.116)
Post-Mandate, Youngest HH Member >59	0.025	-0.010	-0.018
	(0.014)	(0.017)	(0.061)
Post-Mandate, Child <6, Lowest Income Quartile	-0.026	-0.052	-0.014
	(0.042)	(0.036)	(0.118)
Post-Mandate, Child <6, 2nd and 3rd Income Quartiles	-0.059	-0.042	0.073
	(0.028)	(0.045)	(0.107)
N	190753	183538	76795
Outcome Mean, Old HH	0.867	0.266	5.865
Outcome Mean, Child <6	0.825	0.409	5.780

Notes: The table presents DDD estimates on the AHS sample for the years 1985-2009. Post-mandate dummies are interacted with dummies for "Youngest HH Member above 59 years of age", "Child below six years of age", "Child below six years of age, first income quartile", and "Child below six years of age second and third income quartile". Income quartiles are computed within state-year-family status cells. Outcome variables are defined in each column. State-year-income quartiles, income quartiles-year-household characteristic, income quartiles-state-household characteristic, and month of interview fixed effects are included. Mean outcome values in implementing states before the mandates are shown separately for households of different family status at the bottom of each column. Standard errors clustered at the state level (36 clusters) are shown in parentheses.