The Price of a Safe Home:

Lead Abatement Mandates and the Housing Market

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Abstract

State lead abatement mandates require owners of old houses to mitigate lead hazards in the presence of small children. I estimate the effects of these mandates on the housing market using a triple differences strategy that exploits differences by state, year, and housing vintage. The estimates suggest a large fraction of the abatement costs fall on property owners, with house prices for multi-family properties declining by 6.4% and single-family homes declining 4.3%. These effects persist for at least a decade, consistent with low abatement rates. Families with small children bear part of the mandates' costs, too: after a mandate, these families are 17% less likely to live in old houses, and they pay higher rents for safer homes. These results suggest that the mandates have important real distributional consequences despite evidence of low abatement rates.

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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 (Wheeler & Brown 2013), a condition that is associated with reduced IQ (Ferrie et al. 2015) and educational attainment (Currie et al. 2014, Reyes 2015*b*) and an increased risk of criminal activity (Reyes 2007, 2015*a*, Nilsson 2009, Feigenbaum & Muller 2015).[1] In the first half of the last century, however, lead paint was extensively used for residential purposes: in fact, HUD (2011) estimates that nationwide, lead paint still lingers in 5.5 million houses inhabited by small children, the population most at risk for lead poisoning, resulting in lead hazards in 3.7 million homes inhabited by small children, or 21% of houses with small children (Dewalt et al. 2015). 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 units inhabited by children. However, abatement is expensive: Koppel & Koppel (1994) estimate that it can cost between \$500 and \$40,000 per unit, 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 (HUD 2001, 2011),[2] and families with small children complain that landlords discriminate against them to avoid abatement (Berman et al. 2013, Williams 2010). A such, the mandates, and environmental regulations more generally, might be void, or even have counterproductive effects, if they are not incentive compatible (Duflo et al. 2013).

This paper analyzes the effect of state abatement mandates on the housing market equilibrium, providing a novel and extensive characterization of the incidence of these policies. Prior to the mandates, rich families with small children appear to sort into new houses, suggesting that households know about lead hazards and trade off consumption and health.[3] After the introduction of an abatement mandate, we expect families with children to move into old houses as these houses become lead-safe and appreciate. If, however, compliance rates are low, and/or if owners discriminate against families with small children as a consequence of the mandate, old houses depreciate, and families with small children move into new ones. In this paper, I study which equilibrium prevails in states that implement mandates by asking two questions: Who bears the costs of the mandates, owners or renters? Who lives in houses that are likely to contain lead?

To answer these questions, I combine a rich set of data on housing market outcomes and use a triple

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

[2] The US Department of Housing and Urban Development estimates that their funding and enforcement efforts contributed to making 420,000 houses lead-safe.

[3] In the paper, I use the terms "families" and "households" interchangeably, although I have observations at the household level.

differences approach to compare outcomes for old and new houses within a state before and after a mandate's introduction. 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 use sales data, collected by DataQuick from public deeds, to estimate the effect of the mandates on house values; using building structure as a proxy for tenancy, a choice variable for owners, I investigate the effect of the mandates on units in multi- and single-family buildings separately to allow the mandates to affect rental and owner-occupied properties differently. Second, using data from the American Housing Survey (AHS), I estimate the effects of the mandates on the probability that a family with a small child lives in an old house and on housing expenditures.

I find two sets of results. First, the costs imposed by the mandates are capitalized into lower home values: a unit in an old multi-family building depreciates by 6.4%, i.e., by $4.80 per square foot, or 60% of the average abatement cost, and this depreciation persists up to ten years.[4] Old single-family properties persistently lose 4.3% of their value, and fewer of these units appear to enter the rental market after a mandate. Second, after a mandate, families with small children are 17% less likely to live in old houses than before. Together, these results suggest that owners do not immediately comply with the mandates: if they did, abated houses would appreciate, and families with children would move into these safer homes. While landlords 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: rents increase by 6.4-7.4% for old, family-friendly units, a result that is not consistent with lower willingness to pay for old houses by families after a mandate. These changes in the housing market imply that after a mandate, some families with children face higher rents in old units, while others live in new and more expensive houses: in total, I calculate that families with children spend $400 (or 6.4%) more per year on rent for several years after the introduction of a mandate.

In related work, I find that the mandates decrease the probability of lead poisoning Gazze (2016). 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. Accordingly, considering the impact of the policy on the housing market changes the assessment of the mandates from a beneficial policy into a neutral one, on average, for families with small children. The costs borne by these families seem to indicate a failure of the Coase theorem (Coase 1960): although the mandates supposedly empower families with small children with rights to abatement, lax enforcement and discrimination are such that owners mantain *de facto* control, and tenants pay them transfers to have houses abated or pay higher rents to move out of harm's way.

[4]Because nationwide data on abatement projects are not available, I compute the average cost of abatement on data referring to projects conducted in Massachusetts in 2014 and funded by the US Department of Housing and Urban Development (HUD).

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 (Summers 1989, Gruber 1994, MaCurdy & McIntyre 2001).[5] By analyzing the incidence of the mandates, this paper provides some caveats to the work by Aizer et al. (2015), who show that Rhode Island's abatement mandate successfully decreased lead poisoning among African Americans, thus halving 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 (Currie et al. 2011, Cohen-Cole 2006, Evans 2006). Leventhal & Newman (2010) argue that research "on housing and children's development is still in its relative infancy" in terms of both methodology and theory; indeed, the evidence of the effectiveness of lead abatement is mixed, and its effectiveness depends heavily on the techniques used to abate.

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 use a simple infinite-horizon model of an urban rental housing market to derive two sets of predictions regarding the introduction of abatement mandates. First, the mandates always hurt owners of leaded homes. However, the effect of the mandates on the value of old properties overall depends on the strength of enforcement: the more houses are abated, the more old houses will appreciate. Second, families with children move into old units 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

[5] See also Kuminoff et al. (2013) for a review of the literature on equilibrium sorting models.

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 housing unit 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 $r_0 = 0$. Households might need to reoptimize each period due to an exogenous shock, such as a change in work location. Notably, I assume that households have perfect information about houses' lead status; Section 2.4 discusses how the model predictions change when I drop this assumption.

Households vary across two dimensions: per-period income, $y_i \in [\underline{y}, \bar{y}]$, and family status, $s_i \in \{0, 1\}$ where $s_i = 1$ indicates that the household has a small child. The household per-period maximization problem can be written as follows:

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

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) small children, H_0 is the disutility from the outside option, and $H_0 > \alpha_1 > \alpha_0 > 0$. 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, and perform comparative static analysis on τ.

By the concavity of utility of consumption, poor households sort into leaded houses: although everyone dislikes lead, poor households derive a higher marginal utility from the additional consumption they get by living in a leaded house and saving on rent. Moreover, for each level of income, families with small children are willing to pay more to live in a safe home. Hence, the demand for leaded houses is decreasing in τ.

On the supply side, foreign landlords maximize the net present value of rental income. Assuming that without the mandates, rents are constant over time as supply and demand of houses are constant, the price of a house can be written as follows:

$$p_h = NPV(r_h) = \sum_{t=0}^{\infty} \frac{r_h}{(1+i)^t} = \frac{r_h(1+i)}{i} \quad (2)$$

where i is the market interest rate and without loss of generality, the maintenance costs are negligible. For simplicity, I assume a fixed supply of houses of measure one, a fraction of which have lead paint initially.[6] 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, in which case all landlords want to abate. Hence, in an equilibrium with both leaded and non-leaded

[6]The predictions in this section hold if we relax this assumption to allow for an elastic supply of non-leaded houses. By definition, developers cannot add 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 properties to owner-occupiers.

houses, the two values have to be equal.

2.2 Abatement Mandate

Unexpectedly, the government introduces an abatement mandate: with some probability $\pi > 0$, the owner of a leaded house receives a lead order requiring her to abate at cost $A^M \geq A$, which includes relocation and legal expenses. After she abates, the owner will be able to reap higher rent for a safe home starting with the next period. The parameter π measures enforcement, i.e., the probability of a lead order, which can be smaller than the probability that a child lives in a leaded house. Indicating the equilibrium objects under the mandate with a superscript M, the values of non-leaded and leaded houses can be written as follows:

$$NPV_N^M = \frac{(1+i)}{i} r_N \tag{3}$$

$$NPV_L^M = \tau^M + \frac{1-\pi}{1+i} NPV_L^M + \frac{\pi}{1+i} NPV_N^M - \pi A_M \tag{4}$$

In other words, the value of a leaded house equals its rent today plus its expected future value. With probability $1 - \pi$, the house won't be abated; with probability π, the owner receives a lead order and abates at cost A_M. The assumption that households are perfectly informed about lead hazards prevents the mandate from shifting demand for leaded houses. Normalizing r_N without loss of generality and solving for the value of a leaded house recursively, I obtain:

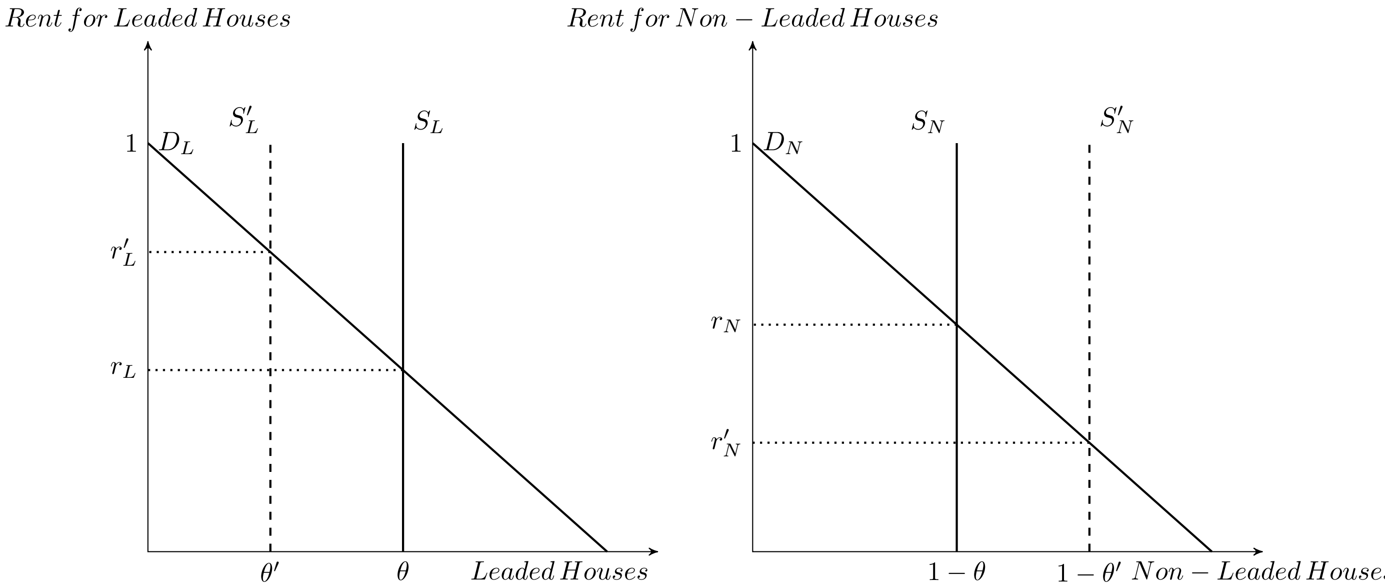
$$NPV_L^M = \frac{(1+i)(i\tau^M + \pi)}{i(i+\pi)} - A_M \frac{(1+i)\pi}{(i+\pi)} \tag{5}$$

where the first term in equation (5) 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. If the drop in the value of leaded houses is big enough that $NPV_L^M < NPV_N - A$, abatement becomes the best solution; if instead the drop in value is small relative to the cost of abatement, owners will wait for law enforcement to require abatement, which yields qualitatively similar, albeit slower, changes. Which equilibrium prevails depends on the elasticity of demand, i.e., on the relative size of each group, μ, and the joint distribution of income and family status. A high share of families with children makes enforcement more likely, incentivizing abatement.

Figure 1 shows how the mandate changes the housing market equilibrium. For ease of illustration, I plot supply for leaded and non-leaded houses as vertical, but the they are actually elastic depending on the abatement cost. As abatement reduces the number of leaded houses from θ to θ', fewer people live in such

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.

houses. In particular, households with children move into abated houses, unambiguously increasing the share of children in old houses. In contrast, the effect of the mandates on relative rents is theoretically ambiguous. When landlords abate, the supply of non-leaded houses increases, and their relative rent decreases. Moreover, a market for owner-occupied units would allow landlords to sell their property to a homeowner, potentially at a lower price. Such sales decrease the total supply of rental houses, putting upward pressure on rents. However, outside the scope of this model, a mandate could also decrease rents for leaded houses: for instance, if the enforcement probability depends on tenants' complaints, then owners can "buy" silence by offering discounts.

In summary, this section highlights two facts. First, the mandates always hurt owners of leaded homes. The assumption of a fixed supply of houses makes landlords more inelastic than tenants: even if rents increase due to the increased costs, this rise does not fully compensate owners. Indeed, it is possible for rents and prices to move in opposite directions, as the mandates introduce a wedge between the stream of future rents and the value of a property. Second, because compliance with the mandates and additional abatement procedures make older homes safe, families with small children are attracted to older houses.

2.3 Discrimination

In this section, I derive 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.[7] Under this scenario, subscripted with D, the value of a leaded house is a weighted average of the value of renting to families with small children, $NPV_L^D(1)$, and without small children, $NPV_L^D(0)$, with weights given by the fraction of families with children in the population, μ:

$$NPV_L^D \quad = \quad \mu NPV_L^D(1) + (1-\mu)NPV_L^D(0) - \phi T \tag{6}$$

where ϕT is the expected fine for discriminating. Given that abatement can only be triggered by a child living in the house, landlords face a null probability of abatement by discriminating. Letting $\epsilon = \frac{\pi}{\mu}$ be the probability of a lead order conditional on a child living in a leaded house, I obtain:

$$NPV_L^D(1) \quad = \quad \tau_1 + \frac{1-\epsilon}{1+i}NPV_L^D + \frac{\epsilon}{1+i}NPV_N^D - \epsilon A_M \tag{7}$$

$$NPV_L^D(0) \quad = \quad \tau_0 + \frac{1}{1+i}NPV_L^D \tag{8}$$

where τ_1 and τ_0 are rents paid by families with and without small children, respectively. Plugging (7) and (8) into (6) and solving for the value of a leaded house, I have:

$$NPV_L^D = \frac{(1+i)\left\{i\left[\mu\tau_1 + (1-\mu)\tau_0\right] + \pi\right\}}{i(i+\pi-\mu)} - A_M\frac{(1+i)\pi}{(i+\pi-\mu)} - \phi T \tag{9}$$

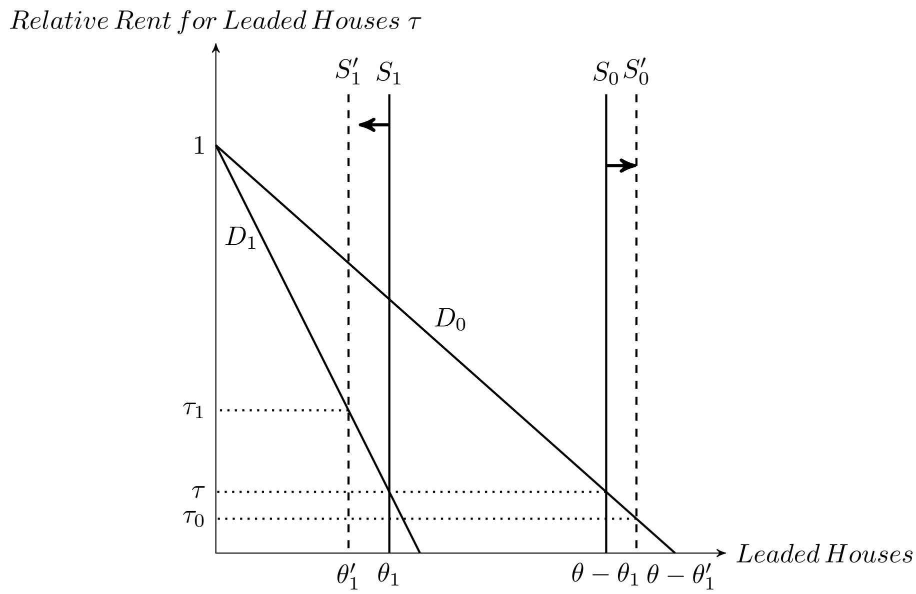
The first term in equation (9) is the net present value of rents, a weighted average of rents paid by families with and without small children. The second term is the expected abatement cost, which depends on enforcement. By a revealed preference argument, the mandate still lowers the value of leaded houses.[8]

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

[7] The results in this section hold in the more general case in which discrimination is possible at all times and in all markets. The mandate increases the cost of providing leaded houses to families with small children, which will be reflected in rents. 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.

[8] For discrimination to be valuable, $i + \pi - \mu > 0$ is required. A standard value for the interest rate, $i = 0.02$, and the population share of household with children, $\mu = 0.15$, yield $\epsilon > 0.87$. 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



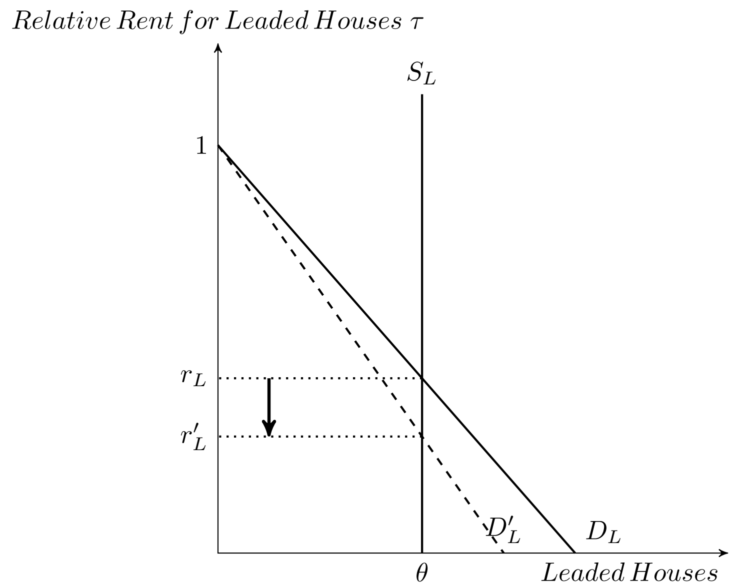
The figure shows the leaded segment of the housing market equilibrium with an abatement mandate and price discrimination. D_1 and S_1 represent demand for and supply of leaded houses for families with small children, while D_0 and S_0 represent demand for and supply of leaded houses for childless families. 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.

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, i.e., $D_1(\tau) + D_0(\tau) = \theta$ and $S_j = \theta_j = D_j(\tau),\ j \in \{0, 1\}$. 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, and consequently, 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.

2.4 Information

In addition to shifting the supply of leaded houses, the mandates might provide information regarding the risks of lead poisoning for small children, which in turn might decrease families' willingness to pay for these houses. Figure 3 depicts the extreme scenario in which the mandates only act through this information

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.

channel, focusing on the leaded segment of the market for ease of illustration: 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 depreciate.[9]

3 Background and Data

3.1 Regulatory History of Lead Paint

Lead paint is commonly found in old houses in the US. Starting in the late 19th century, paint manufacturers typically used lead as an additive in residential paint to increase durability of the paint coat, with paint manufactured prior to 1950 containing up to 50% lead by weight (Reissman et al. 2001). In response to the growing body of evidence of the harm associated with lead, in the late 1950s, some manufacturers voluntarily reduced the lead content of paint to 1%, a level that can still induce severe lead poisoning (Hammitt et al. 1999). Finally, in June 1977, the Consumer Product Safety Commission (CPSC) lowered the allowed level

[9] It is possible that the change in demand and the resulting change in relative prices spurs voluntary abatement. For brevity, I do not discuss this case.

of lead in paint to 0.06%, effectively banning lead paint altogether from 1978 on.[10] Notably, the ban covers new paint, and not the pre-existing housing stock (Mushak & Crocetti 1990). 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% for houses built in the 1970s, to 86% for homes built before 1940 (HUD 2011).

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. Once in the bloodstream, lead impairs cognitive and non-cognitive ability at levels as low as $1 - 2\mu g/dL$, 80 times lower than the level of concern for iron (DNTP 2012): Lanphear et al. (2005) estimate an IQ loss of 3.9 points when BLLs increase from 2.4 to 10 $\mu g/dL$, with lower IQ decrements associated with further BLLs increases. These effects are irreversible, and treatment can only help prevent further accumulation of the toxin (Rogan et al. 2001). Small children are especially exposed to lead-contaminated dust from paint and windowsills due to normal hand-to-mouth activity, and they might grow accustomed to the sweet taste of lead paint (Fee 1990).[11] Moreover, lead is most damaging to small children: they absorb and retain more lead than adults and their neurological development is particularly susceptible to neurotoxins (see, e.g., McCabe 1979).

As of today, 19 states have enacted abatement mandates, as summarized in Table 1.[12] Although physicians had warned against the hazards of lead paint since the early 1900s (Ruddock 1924), the first regulation banning the use of lead pigment for interior use in the US was only adopted in 1951 by the Baltimore Commissioner of Health (Fee 1990). In 1970, the US Surgeon General released a policy statement calling for "adequate and speedy removal of lead hazards" from the homes of lead-poisoned children below six years of age (Steinfeld 1971). Massachusetts was the first state to follow suit, introducing in 1971 one of the strictest lead paint regulations in the country, requiring property owners to permanently control lead paint hazards in the home of any child under the age of six.

In my analysis, I treat all mandates as homogeneous to increase statistical power. In results not reported in the paper, I find little evidence that the impact of the mandates depends on their characteristics. Nonetheless, the mandates differ in terms of their coverage, what triggers a lead order, and type of abatement required. For instance, some regulations cover all properties, whereas others focus on rentals; similarly, some states require abatement as soon as a small child lives in the house, whereas others need a triggering elevated

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

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

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

blood lead level (EBLL) case to issue a lead order, and the definition of EBLL changes across states and over time.[13] Finally, the regulations differ in the extent of mitigation that they require and the protection of liability granted to owners who abate.[14]

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.[15] Data from Maryland indicates that 200,000 units, i.e., only a third of rental properties, have been inspected and certified under the state law that requires all rental properties to be registered.[16] In addition, Appendix Figure A.1 shows that even in states with strict regulations, like Massachusetts, inspections are rare. And deleading projects are even more infrequent: in Massachusetts, the number of abatement projects reported to the Department of Labor since June 2010 totals 7,500 properties, on a total of 2.8 million units, but not all of these projects have been finalized. Finally, 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. Appendix Section A further explores the weak link between the mandates and inspections and abatement.

Aside from the state-level mandates I study in this project, both the federal and local governments have regulated lead paint. At the federal level, the Lead-Based Paint Poisoning Prevention Act of 1971 prohibits lead paint in federal homes and provides funds for deleading. In June 1977, the CPSC effectively banned lead paint for residential use in private homes from 1978 on. However, the first federal measure to deal with the existing stock of lead paint in older US homes was the Residential Lead-Based Paint Hazard Reduction Act of 1992 (Title X), which became 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. 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.[17] 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 laws does not

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

[14] For instance, Vermont requires owners only to perform Essential Maintenance Practices, i.e., interim control practices that ensure the paint does not deteriorate. Furthermore, over time the states have changed the definition of abatement and of admissible interim control practices.

[15] See any online forum for home-buyers for posts like the following, accessed on 07/16/2015 at http://www.city-data.com/forum/massachusetts/1875347-ma-lead-law.html: "We hired a lead inspector anyway at the time of the home inspection, and even though I was verbally given results nothing was disclosed to anyone."

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

[17] See, e.g., Philadelphia Lead Paint Disclosure and Certification Law, effective on December 21, 2012, requiring landlords to ensure that property rented to families with children six years and younger is lead-safe. Notably, Pennsylvania has no state-level mandate.

affect the validity of my findings.

Various abatement techniques are available, and abatement costs vary wildly depending on the technique employed and the extent of the lead hazards present in a property. The US Department of Housing and Urban Development (HUD) distinguishes between temporary and permanent removal of lead hazards. As of now, temporary measures are preferred, because they minimize lead dust. As for abatement, in its 2012 *Guidelines for the Evaluation and Control of Lead-Based Paint Hazards in Housing*, HUD defines abatement as "either: the removal of the building component, the removal of the paint itself, or the long-lasting – at least 20 years – enclosure or encapsulation of lead-based paint hazards."[18] On a sample of 2014 HUD grantees from Massachusetts, I estimate that the average cost for lead hazard control interventions is $7,765 per unit, or $6.47 per square foot, which rises to $9,786 per unit ($8.10 per square foot) once relocation and other related rehabilitation work are taken into account.

3.2 Data

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

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.[19] 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. By directly providing the price the buyer paid for a property, sales data provide a more precise estimate of the value of that 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% of housing structures nationwide, although

[18]Encapsulants are coatings or rigid materials that rely on adhesion to a surface painted with lead paint and are not mechanically fastened to the substrate. Enclosures are defined as durable, rigid construction materials that are mechanically fastened to the substrate with screws, nails, or other mechanical fastening systems that can be expected to last at least 20 years under normal conditions (HUD 2012).

[19]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.

different counties enter the sample in different years from 1988, as shown in Figure 4. 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.

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.[20] 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 B.2 show that neither this sample restriction nor the unbalanced nature of the full panel affect the results.

In the empirical analysis, I study the effect of the mandates on house prices separately for rentals and owner-occupied units, 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 units into single- and multi-family buildings according to the assessor's definition. In particular, I include condominiums among the multi-family buildings, as condominium units are typically part of multi-unit buildings and condominium conversion is as much an endogenous choice as tenancy is. Moreover, in my sample, 57% of multi-family properties and 40% of condominiums are rented, while only 21% of single-family properties are rented.

In addition, I complement the property-level data with tract characteristics from the Neighborhood

[20]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% 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.

Change Database (NBCD) provided by Geolytics. The NBCD combines 1970, 1980, 1990, and 2000 Long Form data and the 2010 Summary File 1 and 2010 American Community Survey (ACS) data, creating a synthetic panel at the tract level with details such as: population, household demographics, rental shares, income, poverty status, education level, employment, and housing costs.[21]

A shortcoming of the DataQuick data is the lack of demographic characteristics of buyers and sellers. However, it is possible to complement the transaction data with information on buyers' characteristics made publicly available through the Home Mortgage Disclosure Act (HMDA). Following the literature, I match observations across the two datasets based on census tract, loan amount, and lender names, obtaining a 29% match (Ferreira & Gyourko 2015, DeFusco 2015). I assess potential selection issues with the linked DataQuick-HMDA sample in Section 4.4.

Rents, Housing Choice, and Housing Expenditures. To further analyze the impact of the mandates on rents, occupancy, and households' expenditures, I use the AHS National Sample, years 1985-2009.[22] As the public files of the AHS only disclose identifiers for metropolitan statistical areas (MSAs), 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 information on rents, vacancies, tenancy, and, most importantly, household demographics and tenure duration.[23] Appendix Table B.1 displays the characteristics of the housing stock in my two housing datasets: the DataQuick data repository, including the sample linked to the HMDA data, and the AHS, as well as selected demographic 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.

4 Empirical Analysis: Prices and Allocation

The model in Section 2 links the extent of abatement under a mandate to changes in prices, i.e., home values and rents, and households' allocation for leaded houses relative to non-leaded ones. Absent data on

[21] This is a synthetic panel because tract boundary changes prevent the construction of the actual tract-level panel without the underlying microdata. In this case, variables are imputed according in proportion to area changes.

[22] The AHS was not conducted in 1987. Although data for the year 2011 is available, it uses a different sample, preventing comparisons with previous years.

[23] 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.

abatement, I estimate the effect of the mandates on housing market outcomes directly. 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:

$$Y_{ivst} = \beta Mandate_{st} * Old_v + \pi \mathbf{X_{it}} + \gamma_{sv} + \delta_{tv} + \eta_{st} + \epsilon_{ivst} \tag{10}$$

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. In particular, 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 data availability. 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. Appendix Figure B.1 shows that there is considerable variation in the age of the housing stock even within such small neighborhoods in Wayne County, Michigan. Moreover, the panel nature of the AHS sample allows me to control for property fixed effects, improving the precision of my estimates.[24]

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.[25] The setback of this specification 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

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

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

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. This sparse timeline suggests that states enacted the regulations in an idiosyncratic manner, unrelated to medical research on lead hazards or to other legislation on lead or old houses. To verify that the parallel trends assumption holds in the data, I estimate a year-by-year version of the 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:

$$Y_{ivst} = \sum_{y=1}^{T_{min}} \alpha_y Pre_{t-y,s} * Old_v + \sum_{y=0}^{T_{max}} \beta_y Post_{t+y,s} * Old_v + \pi \mathbf{X_{it}} + \gamma_{sv} + \delta_{tv} + \eta_{st} + \epsilon_{ivst} \qquad (11)$$

In the remainder of this section, I estimate versions of equations (10) and (11) to analyze the effect of the mandates on sale prices (Section 4.1), allowing for the effects of the mandates to vary across states, housing vintages, and neighborhoods (Section 4.2). Then, I relate the change in property values to the effect of the mandates on rents and the decision to rent a unit (Section 4.3). In Section 4.4, 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 4.5)

4.1 Sale Prices

I estimate the effect of the mandates on sale prices in the DataQuick sample separately for multi- and single-family units. Theoretically, the mandates can affect rentals and owner-occupied properties in different ways; for instance, 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 5 plots year-by-year DDD estimates from a version of equation (11) 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 properties (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 properties, 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 estimate for old multi-family properties: after the mandates, these houses depreciate by 6.4% on average (Column 1), a result that is robust to controlling for properties' quality with property fixed effects in Column 4. In particular, Column 2 indicates that older houses transacted up to four years after the mandate lose 3% relative to newer homes in their census tract, and the loss in value is over 8% 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. In particular, some implementing states did not have the capacity to immediately enforce the regulations they enacted, and many states did not collect inspection data prior to enacting lead laws.

Appendix Tables B.2 and B.3 provide a battery of robustness checks that confirm the results in Panel A of Table 2.[26] Columns 1-3 of Table B.3 show that controlling for state-year or county-year instead of tract-year fixed effects significantly increases the estimated medium- to long-run effect of the abatement mandates on the price of old houses. Within a state, vintages are concentrated in different neighborhoods, as illustrated in Appendix Figure B.2, and tracts with many old houses are different from tracts with a lot of new constructions, as shown in Appendix Figure B.3. Thus, if neighborhoods are on different trends that depend on local labor market conditions and are correlated with the introduction of the mandates, not controlling for neighborhood trends produces biased estimates.[27] Indeed, Column 5 of Table B.3 reports estimates from a regression that includes state-year fixed effects and state-vintage-specific linear trends, and these estimates are closer to the results obtained when controlling for tract-year fixed effects.

An abatement mandate can affect single-family properties 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 units from renting out their second properties. Panel B of Table 2 shows that after a mandate, single-family properties depreciate substantially, i.e., by 4.3%, and this point estimate is statistically indistinguishable from the effect of the mandates on multi-family properties. Interestingly, introducing property fixed effects in Column 4 reduces the estimated long-run effects for single-family units. In other words, the mandates appear either to slowly foster abatement the more houses are transacted or to delay the sale of the lowest quality single-family units. While there is no data on abatement rates to test the first hypothesis, Column 2 of Appendix Table B.4 lends little support to the second explanation, as it shows no evidence that the mandates affect the turnover of old single-family houses on average. Sales of multi-family houses, instead, appear to slow

[26] Column 4 of Appendix Table B.2 shows that when limiting the analysis to implementing states only, the effect of the mandates appears to vanish four years after their introduction. However, the sample of implementing states is a highly unbalanced panel of 12 states, and hence it does not allow for a correct estimation of the underlying confounding trends.

[27] Controlling for neighborhood-specific trends reduces the drop in prices of old houses after the mandates. A potential explanation is that falling housing prices result in less maintenance being performed: hence, the risk of lead poisoning increases, potentially triggering the introduction of a mandate.

down after the mandates (Column 1), consistent with a model in which market frictions, such as asymmetric information, deter sales. Section 4.3 examines the mandates' impact on the rental market more closely, looking at both the extensive margin, i.e., the decision to rent out a property, and the intensive margin, i.e., the rents charged for different properties. Finally, Appendix Table B.5 shows that the mandates affect rental and owner-occupied houses in the same way they affect multi- and single-family properties, confirming that it is indeed appropriate to use building structure as a proxy for tenancy.[28]

My estimates of the losses in property values are quite large: prices of old multi-family houses drop by $4.80 per square foot on average, about 60% of the abatement cost. This figure is in line with estimates of the capitalization of the Clean Air Act by Chay & Greenstone (2005); moreover, Billings & Schnepel (2015) find that federally-funded lead remediations that cost on average $7,291 increase home values in Charlotte, NC, by $20,000, with a 179% return on investment.[29] Given the low abatement rates and low enforcement probability observed in reality, even when one considers the high costs 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.[30] To make sense of the estimates in this paper, 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. In practice, relatively large properties with many windows require abatement projects that are much more expensive than average, and it is hard to get a cost estimate until an inspection reveals the extent of the lead hazard.[31] 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 to get a house certified, 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.

[28] An additional difference between single- and multi-family houses is that the latter are more likely to be owned by corporate investors. Results by Genesove & Mayer (2001) suggest that due to loss aversion, drops in expected market values lead to smaller effects on list and sales prices for houses listed by homeowners.

[29] My estimates are also in line with the literature on the capitalization of pollution and school investments (Currie et al. 2015, Greenstone & Gallagher 2008, Gamper-Rabindran & Timmins 2013, Davis 2011, Muehlenbachs et al. forthcoming, Bartik et al. n.d., Cellini et al. 2010)

[30] A recent trial in Baltimore awarded $5 million to two sisters poisoned in the 1990s (Wheeler & Brown 2013).

[31] Source: author's discussions with contractors from the Greater Boston area.

4.2 Heterogeneous Price Effects

The model in Section 2 emphasizes that the impact of the mandates depends on both enforcement and the elasticity of demand for leaded houses. Regarding enforcement, I investigate (1) whether mandates that focus on rental properties, as described in Table 1, have a different impact on the housing market than mandates with a broader focus, and (2) whether the mandates have a bigger effect on the oldest units among old houses, which are more likely to present a lead hazard. As for the elasticity of demand for leaded houses, I investigate what neighborhood characteristics are correlated with stronger price responses to the introduction of a mandate.

Table 3 suggests that mandates that target only rental properties affect the housing market in a way that is very similar to mandates with a broader focus. Indeed, the point estimates are close to the estimates from the full sample, reported in Table 2, although the sample restriction makes the estimates statistically insignificant. To shed light on the degree of heterogeneity in the effect of the mandates across states, I plot the difference between the price of new and old houses in North Carolina, Ohio and Rhode Island in Appendix Figure B.4. The price dynamics vary greatly across these three states: the introduction of a mandate in Ohio coincides with a steep increase in the price gap between new and old houses, while the housing markets in Rhode Island and North Carolina seem to respond more slowly. Moreover, the differential responses do not appear to be related with the type of the mandate, as Rhode Island targets only rental properties while North Carolina and Ohio target both rental and owner-occupied properties. On the contrary, these different trajectories hint at the crucial role of enforcement in mediating the letter of the abatement mandates; however, only detailed data on enforcement can provide definitive evidence on this.

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.[32] 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 properties for issues of statistical power, Figure 6 shows that the effect of the mandate is indeed stronger for older vintages. Nonetheless, Appendix Table B.6 shows that after a mandate, even houses built in the 1970s see their price decrease by about 1% relative to houses built in the 1980s.[33] After the mandates, houses built in the 1990s appear to appreciate 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.[34]

[32] HUD estimates that 87% of housing units built before 1940 in the US have lead paint, compared to 69% for houses built between 1940 and 1959 and 24% for houses built between 1960 and 1977 (HUD 2011). Use of lead paint declined earlier and faster in the South and West of the country.

[33] Moreover, a comparison Columns 1 and 3 with Columns 2 and 4 of Appendix Table B.6 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.

[34] The relative point estimates are shown in Appendix Table B.7.

Next, I analyze how demand conditions shape the impact of the mandates in different neighborhoods by estimating equation (11) on subsamples of tracts based on their 1980 characteristics, again pooling multi- and single-family properties together for statistical power. First, I divide tracts according to the average ratio of land prices to home values: interpreting high land values as indicators of the high quality of local amenities, I hypothesize that abatement mandates will not play an important role in determining price dynamics in tracts that are in high demand for other reasons. Indeed, Columns 1-2 of Table 4 show that the mandates have close to no impact in places where land prices are very high. Moreover, Columns 3-4 show that old houses suffer less from the mandates in tracts with high poverty rates. This is consistent with an equilibrium such that households who respond have a relatively high willingness to pay for safe homes. Indeed, Figure 7 suggests that the highest reduction in the share of families with children living in old houses occurs for households in the fourth to sixth decile of the income distribution; Section 4.4 discusses households' allocation in greater detail. Finally, Columns 5-6 of Table 4 show that tracts with a high share of small children present the biggest drop in the relative price of old houses, consistent with the mandates targeting children's residences.

4.3 Rents

The results in the previous sections suggest that the mandates lower the value of both rental and owner-occupied properties that are likely to have lead hazards. In this section, I ask (1) whether the mandates deter owners from entering into or encourage them to exit from 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 (10) in the AHS sample of multi- and single-family properties separately, introducing property fixed effects as controls.[35] The estimated effect of the mandates on the rental market is strikingly different for multi- and single-family properties. Columns 1 and 2 of Table 5 show no effect of the mandates on the owner's decision to rent an old unit in a multi-family structure.[36] This is not surprising: owners of triple-deckers might live in one of the units, but will usually rent out the other ones. Moreover, I find no statistically significant impact of the mandates on rents for old multi-family properties (Column 3), consistent with owners bearing most of the costs of the regulations.

Instead, the mandates appear to deter owners of old single-family properties from renting them out: Column 4 of Table 5 estimates that over 70% fewer old single-family properties transition into the rental market each year, from an initial entry rate of 2.1%. However, large standard errors cannot reject smaller effects. Column 6 suggests that the contraction in the supply of old rental single-family properties exerts a

[35] In this sample, the oldest vintage corresponds to houses built prior to 1910, and the following correspond to each decade thereafter.

[36] In results not reported in the paper, I find no effects of the mandates on vacancy rates, home values, or on turnover.

temporary upward pressure on rents for these properties. Seven years after the introduction of a mandate, rents for old single-family properties relative to new ones appear to return to their pre-mandate levels, suggesting that new constructions might adjust to substitute for the old properties that are taken out of the rental market.

4.4 Allocation

The big 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. Appendix Table B.8 illustrates the allocation of households in old and new houses before and after mandate, showing little evidence of changes on average. However, Figure 7 shows that prior to the mandates, rich families with small children are less likely to live in old houses, consistent with poorer households with children trading off a healthier home for consumption. The same graph, indicates that after the mandates, even fewer low- and middle-income families with small children live in old homes.

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 (10) with property fixed effects on the AHS sample. Column 1 of Table 6 confirms that after the mandates, there are 2.6 percentage points fewer families with small children in old houses, a reduction of 17%. Plotting period-by-period estimates from equation (11), Figure 8 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 a supply-side component as time passes. Indeed, Appendix Table B.10 shows that this pattern is more pronounced for single-family houses, where the discrimination component 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.[37] Indeed, Column 2 of Table 6 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 6 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. Finally, Columns 4-6 of Table 6 show no change in the demographic composition of households that live in old houses along income or racial lines. Only 15% of households who live in old houses have small children, so if the mandates have any effect on race or income, it is probably too small to be captured on average. Appendix Table B.9 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,

[37] The average household in the AHS sample spends six years in the same rented house, suggesting that moving costs might be substantial.

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 B.10 shows that the mandates affect occupancy in multi-family properties the most: in other words, if any abatement takes place, it seems that it takes place differentially in single-family houses. Notably, these findings 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% 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.

I perform a similar allocation analysis on the subsample of transactions for multi- and single-family properties, pooled together for statistical power, linked to the HMDA dataset, which provides information on income and race of the buyer of a house, as well as on occupancy. Column 1 of Table 7 verifies that the price results reported in Section 4.1 hold in the linked subsample. More interestingly, Column 2 shows that after the mandates, buyers of old houses have 2.3-3.5% lower income than buyers of newer homes, a persistent difference illustrated in Figure 9 as well. Surprisingly, older houses in this subsample appear to be more likely to be rented after a mandate is introduced (Column 3 of Table 7), the more so over time (Appendix Figure B.5). As the mandates increase the cost of renting out an old house, this result is counterintuitive. One potential explanation is that houses in this sample are of higher quality: indeed, Appendix Table B.1 reports that these houses transact at a higher price per square foot. Moreover, I infer rental status in HMDA data from an indicator for primary residence. Hence, this result might be confounded by secondary residences that are not for rental.

4.5 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, while the regulations have no statistically significant effect on rents for old multi-family homes. These findings are not consistent with high compliance rates: we expect to find more families with children in old units after a mandate if the mandate induces higher abatement rates. 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.

4.5.1 Discrimination

Anecdotally, some owners refuse to rent to families with small children, while others quote different prices to households of different races.[38] Moreover, in a randomized audit study in Greater Boston, Berman et al. (2013) show that to this day, landlords discriminate against prospective tenants with small children: they find strong evidence of discrimination in 50% of 36 tests and find no evidence of discrimination only in 11% of tests. However, it is illegal to treat customers differently on the basis of their membership in a protected class, and owners trade-off the savings from postponing abatement with the probability of being fined for the discriminatory behavior.

Unfortunately, we lack systematic audit data across states to directly test whether landlords discriminate against families with children as a result of the mandates. 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 8 show that after a mandate, rents for old houses with two or more bedrooms increase by 6.4-7.4%. Estimates of changes in rents for new houses are not statistically significant when controlling for property 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 results for units that housed a small child in 1985, but these estimates are less precise, especially when introducing property fixed effects.

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.

4.5.2 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. However, to assess the role of information in shaping the impact of the mandates, Table 9 investigates whether mandates implemented before and after Title X have differential effects on the value of old houses. The federal disclosure mandate arguably increased the salience of lead poisoning, and we expect mandates enacted before Title X to have a stronger impact than those implemented afterwards if the primary effect of

[38] Sources: Attorney General's Office, Civil Rights Division, Massachusetts and San Antonio Fair Housing Council in Yinger (1998).

a mandate is to decrease demand for leaded houses by parents.[39] On the contrary, mandates implemented after Title X have a bigger impact on home values, suggesting that the mandates and lead salience are complements. In other words, old houses depreciate more 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. Only 15% of households in the US have small children (see Appendix Table B.1), so the general equilibrium effects are likely to be small. Hence, I interpret the results in this section as indicating that several years after the introduction of a mandate, families with children spend \$400 more per year on rents than they did before the mandate was enacted.

Table 10 presents estimates from fitting the following equation:

$$Y_{ijst} = \beta_0 Mandate_{st} * SeniorHH_j + \beta_1 Mandate_{st} * SmallKid_j + \gamma_{sj} + \delta_{tj} + \eta_{st} + \epsilon_{ijst} \tag{12}$$

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

Columns 1-2 of Table 10 reiterate that after a mandate, families with small children are less likely to live in old houses. Moreover, although some of these families leave the rental market, this result is not robust to controlling for income, consistent with richer families anticipating the purchase of a home (Columns 3-4).[40] For those who still rent, this reallocation results in higher housing expenditures: Column 6 of Table 10

[39] Using data from the AHS, Bae (2012) finds that the disclosure mandate increases buyers' testing at sale and reduces purchases of old homes among families with small children. Surprisingly, in a follow up study, Bae (2015) finds no effect of the mandate on the value of old houses.

[40] See, e.g., Henderson & Ioannides (1983) for a discussion of homeownership over the life cycle.

suggests that after a mandate, families with small children pay 6.6% higher rents on average, i.e., $400 more per year (in 2006 USD). This estimate is statistically significant at the 10% level, and the rent increase appears to be persistent over time (Figure 10). There is no evidence that senior households pay higher rents after the mandates: if anything, the point estimate is negative. It is worth emphasizing that while these results hold for renters, I have no measure of housing expenditures for homeowners. In implementing states, 6.2 million households have small children, and 40% of them live in rental homes.[41] Hence, extrapolating from Column 6 of Table 10, the additional housing expenditure that I attribute to the mandates totals $645 million per year, with a 90% confidence interval spanning from $60 to $1230 million. 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.

The model in Section 2 predicts that middle-income households are those at the margin: the richest households had already sorted out of leaded houses (Figure 7), and the poorest ones cannot afford to live in a newer, safer homes. Indeed, Column 1 of Appendix Table B.11 suggests that families in the second and third quartile of the income distribution respond more strongly than both the poorest and the richest families. In contrast, Column 3 shows no evidence of a differential impact of the mandates on rents paid by households of different socioeconomic status, but the estimates are very imprecise.

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% of the costs imposed on property owners by the mandates are capitalized into lower home values: a back of the envelope calculation indicates that the average loss in home values per lead poisoning averted totals $1.4 million.[42] Moreover, landlords shift a third of the burden of future abatement costs to families with small children, charging them higher rents. As a result, parents who can afford it choose to live in newer and safer homes, incurring 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 (Gazze 2016). As such, some disadvantaged families might be worse off under a mandate. It is worth asking whether this is in line with the equity considerations

[41] Source: Census Bureau.

[42] Section 4.1 estimates lower home values by $4.8 per square foot, i.e., $6500 for an average house. As there are a over 1.41 million old properties in the implementing states in my sample, the total loss per lead poisoning averted borne by property owners totals $1.39 million.

that are at the basis of other pieces of legislation, such as the Fair Housing Act of 1968. Indeed, the current mandates appear to cause unintended consequences for the citizens they are supposed to protect due to discriminatory behavior: the Coase theorem suggests that stricter enforcement of control rights and empowerment of the most disadvantaged households would ensure that a broader population benefits from the regulations. In addition, the probability of lead poisoning conditional on living in a leaded house likely decreases with income, as costly avoidance behavior becomes available.[43] However, middle-income families seem to respond more and more significantly to the mandates than low-income families. Dewalt et al. (2015) estimate that 20% of households with small children earning less than $30,000 per year live in houses with lead hazards. Hence, ensuring that the poorest households live in safer homes might have a bigger effect on children's health than the current regulations. Finally, 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.[44] Moreover, frictions in the housing market might waste resources, increasing the time needed to match households to houses.

Further research is needed to shed light on the observed low abatement rates. An analysis of HUD data on grants and loans for abatement can inform policy discussion on owners' costs and liquidity constraints. If the distribution of abatement costs is skewed to the right, it would be extremely costly to foster additional abatement. Moreover, as families with small children represent a small fraction of the population, it is neither efficient 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.

[43] See, for example, Moretti & Neidell (2011) on avoidance behavior in relation to air pollution.

[44] 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.

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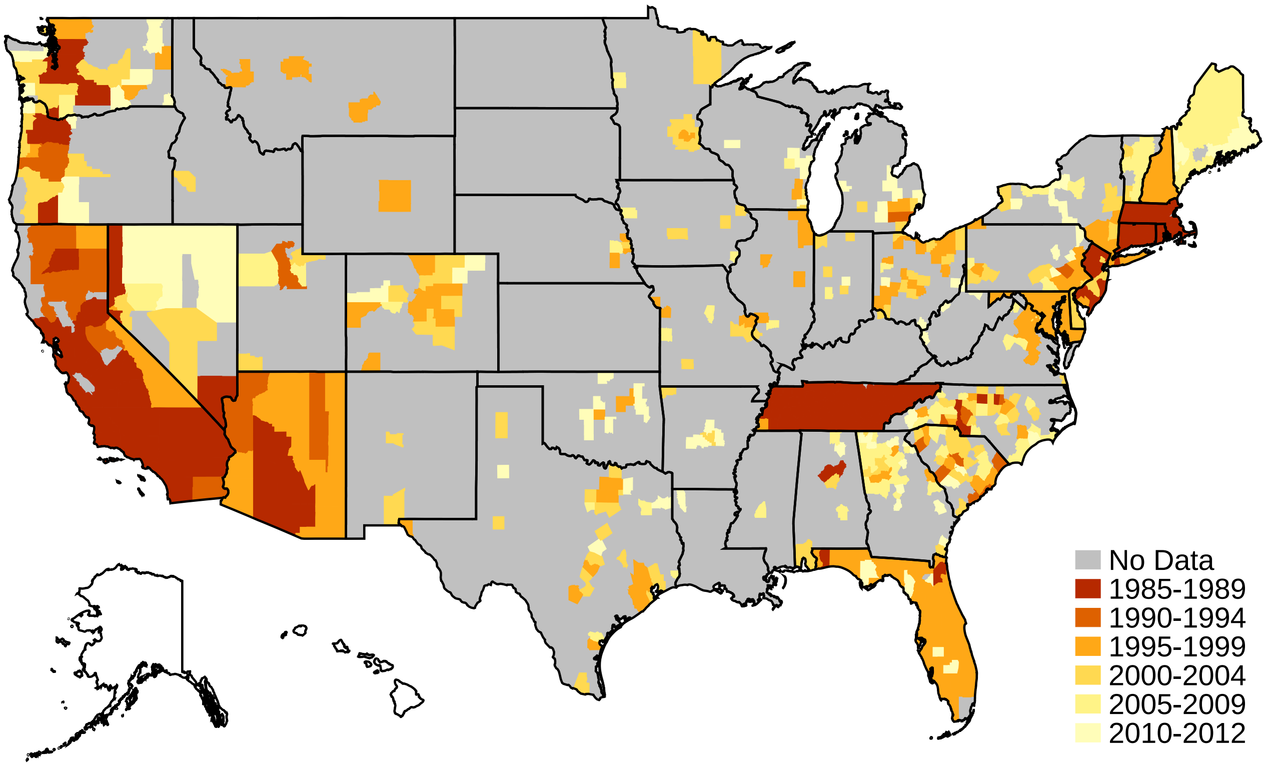
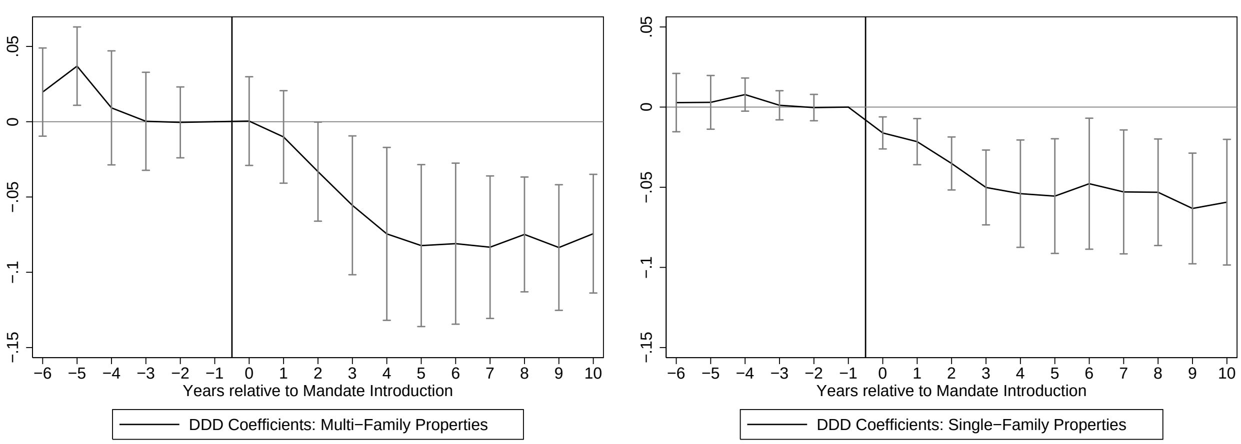
Figure 4: Transaction Data Coverage



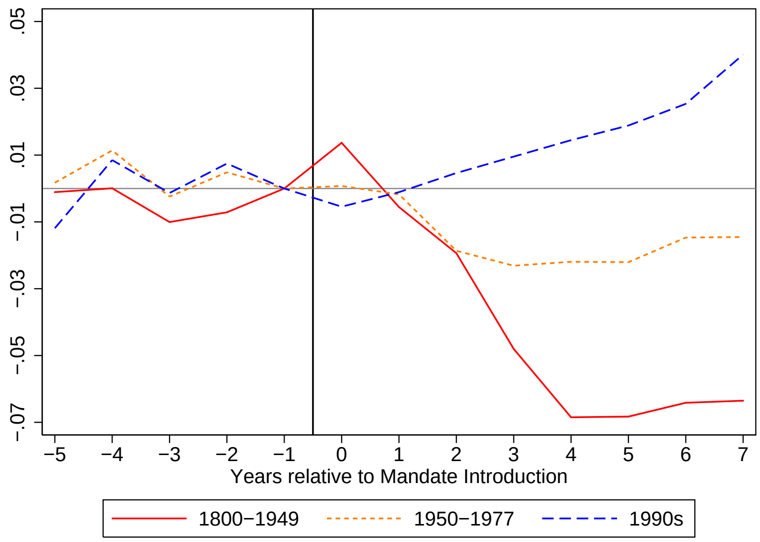
The figure shows a heat map of the coverage of the DataQuick data repository, by county and initial coverage year. Darker shades indicate counties that have been in the database the longest.

Figure 5: Price Effects



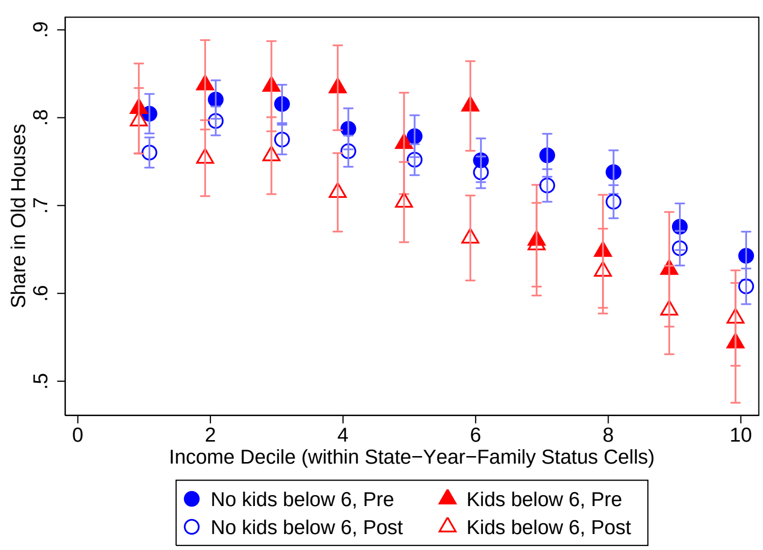
The figure plots DDD coefficients on year-by-year mandate dummies, estimated on the DataQuick samples of multi- (left panel) and single-family (right panel) properties for the years 1988-2012, where each census tract is weighted by population in 1980. The outcome variable is the logarithm of the transaction price divided by square footage of the unit. The vertical line at $t = 0$ 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 dashed lines are lower and upper bounds of 95% confidence interval. Standard errors are clustered at the state level (42 clusters).

Figure 6: Price Effects, By Year of Construction



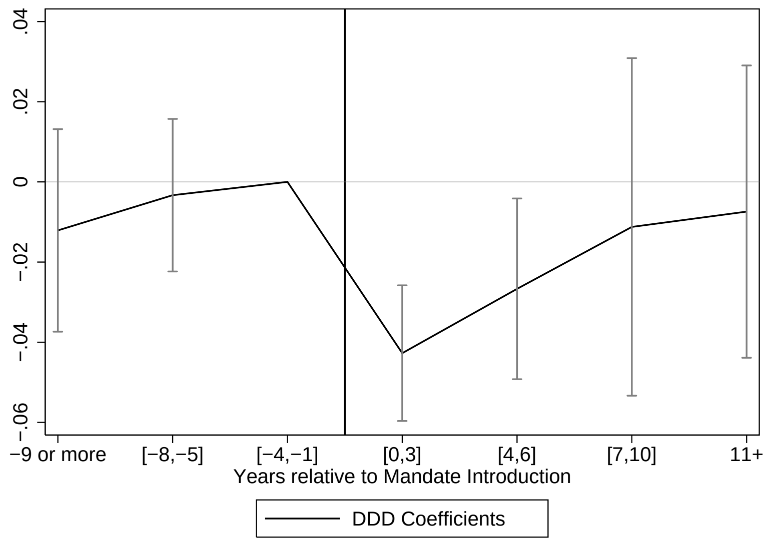
Notes: The figure plots DDD coefficients on year-by-year vintage dummies (1800-1949, 1950-1977, 1990s: 1978-1989 is the omitted category, 1700s and 2000s are dropped) for the years 1988-2012, estimated on the DataQuick sample where each census tract is weighted by population in 1980. The vertical line at $t = 0$ indicates the introduction of the mandate. The sample is restricted to tracts that have similar shares of houses built in the 1980s and 1990s. T-1 is the omitted category. Tract-year, tract-vintage and vintage-year FE are included.

Figure 7: Sorting into Old Houses, By Income and Family Status



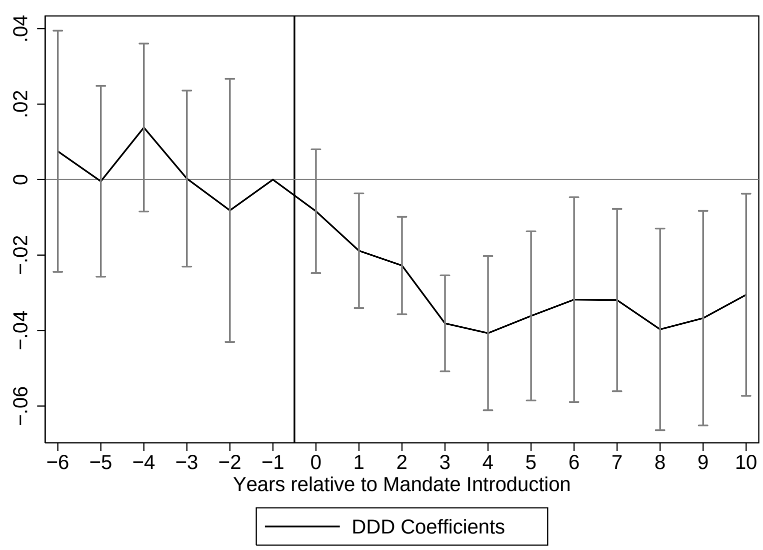
Notes: The figure plots the share of families in the AHS sample with (red triangles) and without (blue dots) children living in old houses in implementing states before (solid) and after (empty) the introduction of the mandates, by income decile. The vertical bars represent 95% confidence intervals.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

Figure 8: Allocation Effects: Child Under Six



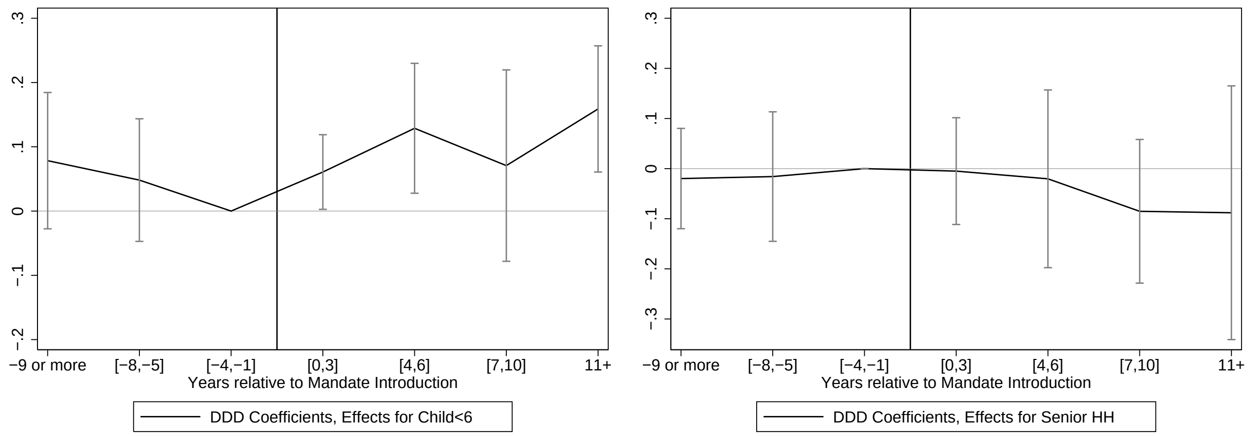
The figure plots DDD coefficients on four-year mandate dummies, estimated on the AHS sample for the years 1985-2009. The outcome variable is a dummy for the household having a child below six years of age. State-year, year-vintage, month of interview and property fixed effects are included. The vertical line at $t = 0$ indicates the introduction of the mandate. $T \in [-4, -1]$ is the omitted category. The dashed lines are lower and upper bounds of 95% confidence interval. Standard errors are clustered at the state level (36 clusters).

Figure 9: Allocation Effects: Buyer's Income



The figure plots DDD coefficients on year-by-year mandate dummies for the years 1990-2012,, estimated on the linked DataQuick-HMDA sample where each census tract is weighted by population in 1980. The outcome variable is the logarithm of buyer's income. The vertical line at $t = 0$ 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 dashed lines are lower and upper bounds of 95% confidence interval. Standard errors are clustered at the state level (42 clusters).

Figure 10: 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 at $t = 0$ indicates the introduction of the mandate. $T \in [-4, -1]$ is the omitted category. The dashed lines are lower and upper bounds of 95% confidence interval. 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 properties, what triggers a lead order, and whether the type of properties covered by the mandate.

Table 2: Price Effects

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

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. The table presents DDD estimates on the transaction sample for multi- (Panel A) and single-family units (Panel B) 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 unit. Tract-year, tract-vintage and vintage-year fixed effects are included in all columns. In addition, Column 4 includes house-specific controls (i.e., fixed effects for number of units, stories, and rooms in the property) and Column 5 includes property 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: Price Effects, Mandates Targeting Only Rental Properties

Dependent Variable:	Log Price per Square Foot	
Sample:	Multi-Family	Single-Family
	(1)	(2)
Mandate Effects on Old Houses:		
0-3 Years After Mandate	-0.022	-0.029*
	(0.029)	(0.018)
4-6 Years After Mandate	-0.088	-0.061
	(0.069)	(0.039)
7-10 Years After Mandate	-0.087	-0.057
	(0.053)	(0.041)
N	3438117	14578408
Price Per SqFt, New Homes, Pre-Period	133.452	128.619
Price Per SqFt, Old Homes, Pre-Period	79.862	123.744

Notes: $***$ p<0.01, $**$ p<0.05, $*$ p<0.1. The table presents DDD estimates on the transaction sample of multi- (Column 1) and single-family properties (Column 2) 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 unit. Tract-year, tract-vintage and vintage-year fixed effects are included. For implementing states, the sample is limited to mandates that focus on rental properties, and 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 in each subsample at the bottom of each column. Standard errors clustered at the state level (42 clusters) are shown in parentheses.

Table 4: Price Effects by Tracts' Characteristics

Dependent Variable:	Log Price per Square Foot					
	By Land to House Price Ratio		By Poverty Rate		By Share of Children under 5	
Sample:	Low	High	Low	High	Low	High
	(1)	(2)	(3)	(4)	(5)	(6)
Mandate Effects on Old Houses:						
0-3 Years After	-0.016**	0.000	-0.021***	-0.017	-0.013	-0.029***
Mandate	(0.006)	(0.011)	(0.007)	(0.015)	(0.009)	(0.009)
4-6 Years After	-0.040***	-0.023*	-0.058***	-0.030**	-0.040***	-0.060***
Mandate	(0.007)	(0.013)	(0.014)	(0.014)	(0.012)	(0.018)
7-10 Years After	-0.055***	-0.011	-0.068***	-0.031**	-0.043***	-0.073***
Mandate	(0.008)	(0.015)	(0.011)	(0.016)	(0.012)	(0.015)
N	14024492	2188926	11776346	9910081	11160959	10396623
Price Per SqFt, New Homes, Pre-Period	93.129	136.560	111.175	88.981	110.193	105.245
Price Per SqFt, Old Homes, Pre-Period	82.708	130.207	103.896	71.869	101.333	84.811

Notes: $***$ p<0.01, $**$ p<0.05, $*$ p<0.1. 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 unit. Columns 1-2 show estimates for tracts below and above the 90th percentile of average land-to-value ratios, Columns 3-4 for tracts with below and above median poverty rate, and Columns 5-6 for tracts with below and above median shares of children under five years of age. Tract characteristics are defined from 1980 NBCD data. 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 in each subsample at the bottom of each column. Standard errors clustered at the state level (42 clusters) are shown in parentheses.

Table 5: 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	Exit from Rental	Log Monthly Rent
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Mandate Effects on Old Houses, Single Post-Period						
Mandate Effects on	0.112	0.016	0.024	-0.015**	0.080	0.174***
Old Houses	(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	0.037	0.008	0.026	-0.006	0.021	0.306***
Mandate	(0.074)	(0.016)	(0.035)	(0.007)	(0.043)	(0.058)
4-6 Years After	0.272*	0.033**	0.089	-0.023**	0.116	0.114**
Mandate	(0.146)	(0.017)	(0.067)	(0.010)	(0.079)	(0.050)
7-10 Years After	0.166	0.017	-0.078	-0.027***	0.151	-0.011
Mandate	(0.116)	(0.011)	(0.096)	(0.010)	(0.094)	(0.136)
10+ Years After	0.058	0.014	0.047	-0.027**	0.149	0.045
Mandate	(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: *** p<0.01, ** p<0.05, * p<0.1. The table presents DDD estimates on the AHS sample of multi- (Columns 1-3) and single-family properties (Columns 4-6) for the years 1985-2009. Outcome variables are defined in each column. State-year, year-vintage, month of interview and property 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 6: 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 Old Houses	-0.026***	-0.023	0.025	-0.040	-0.005	-0.017
	(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 Mandate	-0.039***	0.002	0.013	-0.050	0.001	-0.015
	(0.008)	(0.017)	(0.015)	(0.047)	(0.013)	(0.013)
4-6 Years After Mandate	-0.023**	-0.043*	0.036**	-0.009	-0.023	-0.005
	(0.010)	(0.023)	(0.017)	(0.056)	(0.022)	(0.012)
7-10 Years After Mandate	-0.009	-0.051**	0.015	-0.072	0.029	-0.013
	(0.020)	(0.025)	(0.026)	(0.062)	(0.024)	(0.018)
10+ Years After Mandate	-0.005	-0.039	0.080***	-0.009	-0.050	-0.064*
	(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: *** p<0.01, ** p<0.05, * p<0.1. 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 property 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 7: Allocation Effects, DataQuick-HMDA Sample

Dependent Variable:	Log Price per SqFt	Log Income	Minority HH Head	House Rented
	(1)	(2)	(3)	(4)
Mandate Effects on Old Houses:				
0-3 Years After Mandate	-0.016***	-0.023***	-0.005	0.005**
	(0.005)	(0.008)	(0.003)	(0.002)
4-6 Years After Mandate	-0.033***	-0.036***	-0.002	0.007***
	(0.007)	(0.006)	(0.002)	(0.003)
7-10 Years After Mandate	-0.034***	-0.035***	0.002	0.013***
	(0.006)	(0.005)	(0.002)	(0.004)
N	6355362	6161748	5828740	6342439
Mean Outcome, New Homes, Pre-Period	108.193	11.105	0.064	0.033
Mean Outcome, Old Homes, Pre-Period	94.252	10.842	0.112	0.063

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. The table presents DDD estimates on the DataQuick-HMDA sample for the years 1990-2012, where each observation is weighted by population in 1980. Outcome variables are defined in each column. 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 (Column 1) and average outcomes 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 (42 clusters) are shown in parentheses.

Table 8: 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, Post Mandate			0.010	0.044
			(0.310)	(0.260)
N	13110	13762	3876	3960
Outcome Mean, 0/1 Bedrooms, Pre-Mandate	5.933	5.939		
Outcome Mean, No Child at Baseline, Pre-Mandate			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, Post Mandate			0.089***	0.049
			(0.033)	(0.032)
N	53225	57871	32382	34455
Outcome Mean, 0/1 Bedrooms, Pre-Mandate	5.765	5.730		
Outcome Mean, No Child at Baseline, Pre-Mandate			5.801	5.763
Controls	X		X	
Property FE		X		X

Notes: *** p<0.01, ** p<0.05, * p<0.1. The table presents DDD estimates on the AHS samples of new (Panel A) and old houses (Panel B) for the years 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 property, #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 property, 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 9: 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	-0.003	-0.025***
Mandate	(0.012)	(0.009)
4-6 Years After	-0.037***	-0.052***
Mandate	(0.012)	(0.019)
7-10 Years After	-0.040***	-0.063***
Mandate	(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: *** p<0.01, ** p<0.05, * p<0.1. 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 unit. 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 10: 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

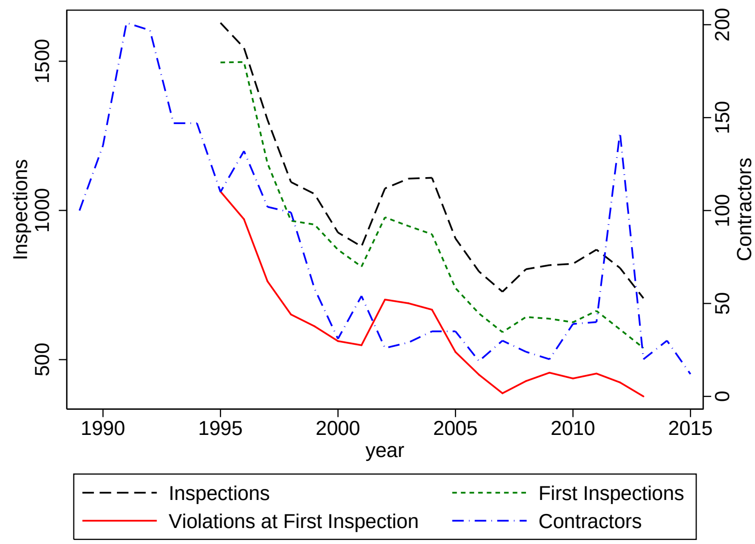
Notes: *** p<0.01, ** p<0.05, * p<0.1. The table presents DDD estimates on the AHS sample for the years 1985-2009. Post-mandate dummies are interacted with dummies for "Child below six years of age" and "Youngest HH Member above 59 years of age". Outcome variables are defined in each column. Rent is 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.

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.

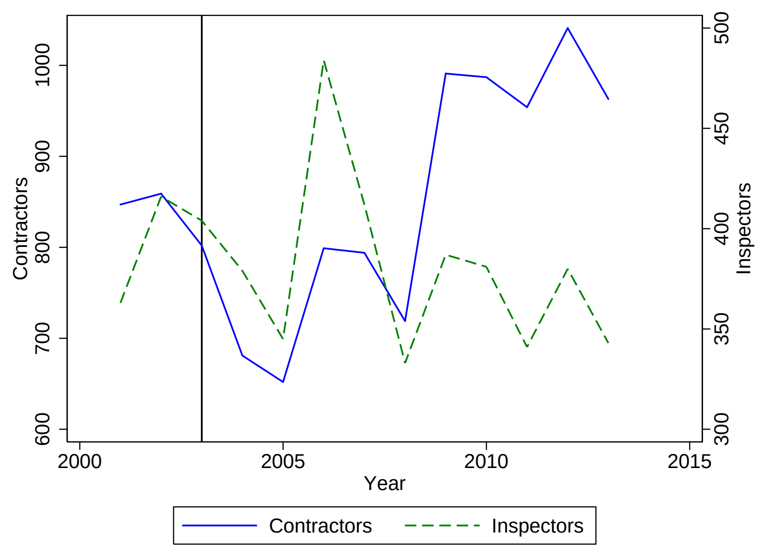
Here, I compare an early adopting state, Massachusetts, which introduced the lead mandate in 1971, with Ohio, which introduced it in 2004. Figure A.1 shows that, to this day, the state performs only 700 inspections per month, despite the fact that Massachusetts contains over 2.1 million houses built 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.2: 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 property (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.

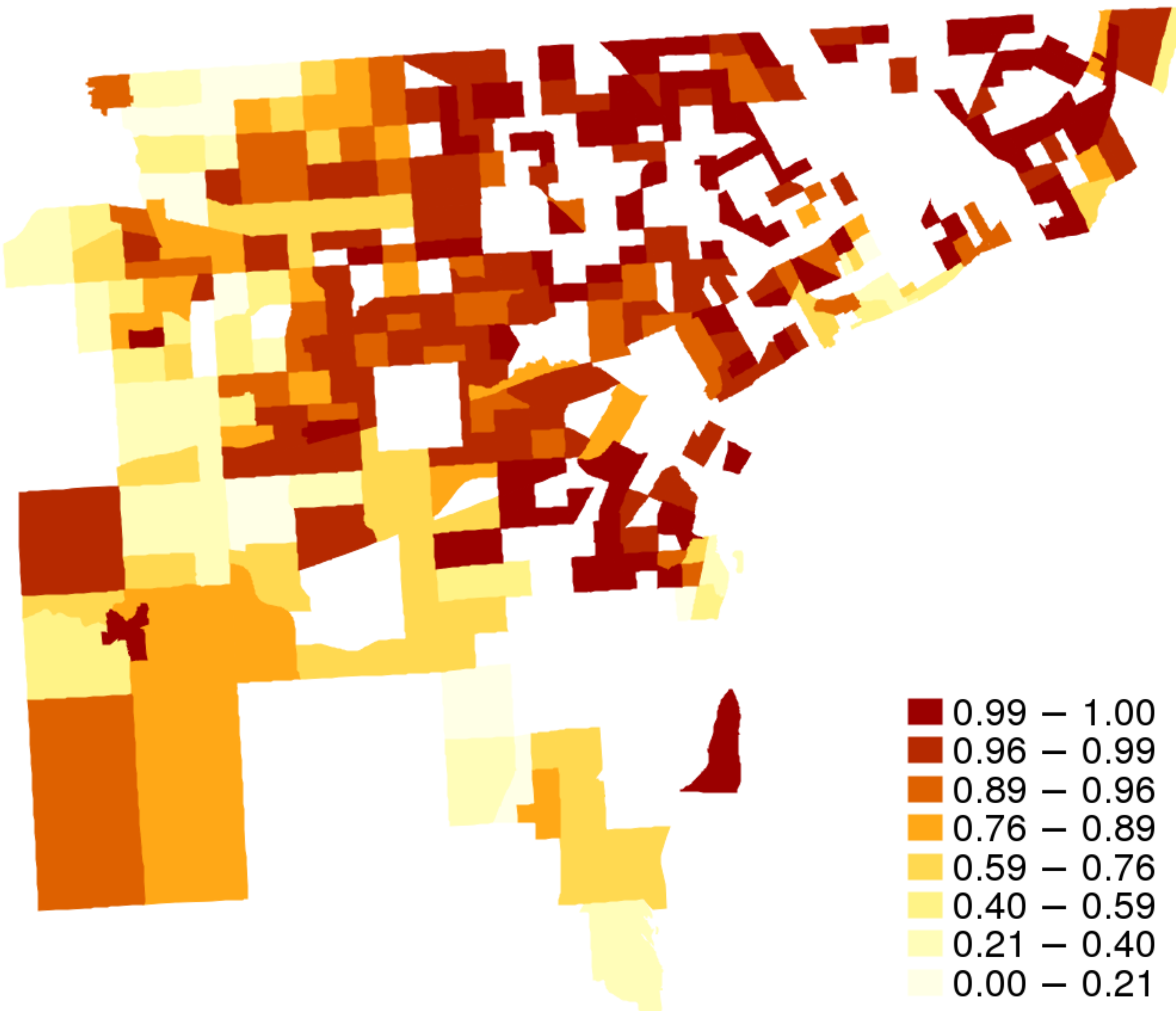
Figure A.2: Enforcement, OH



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.

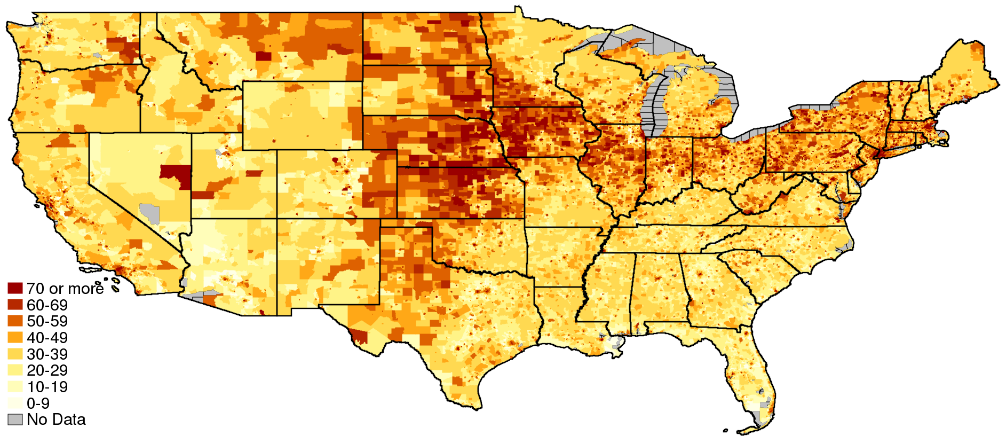
B Additional Figures and Tables

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



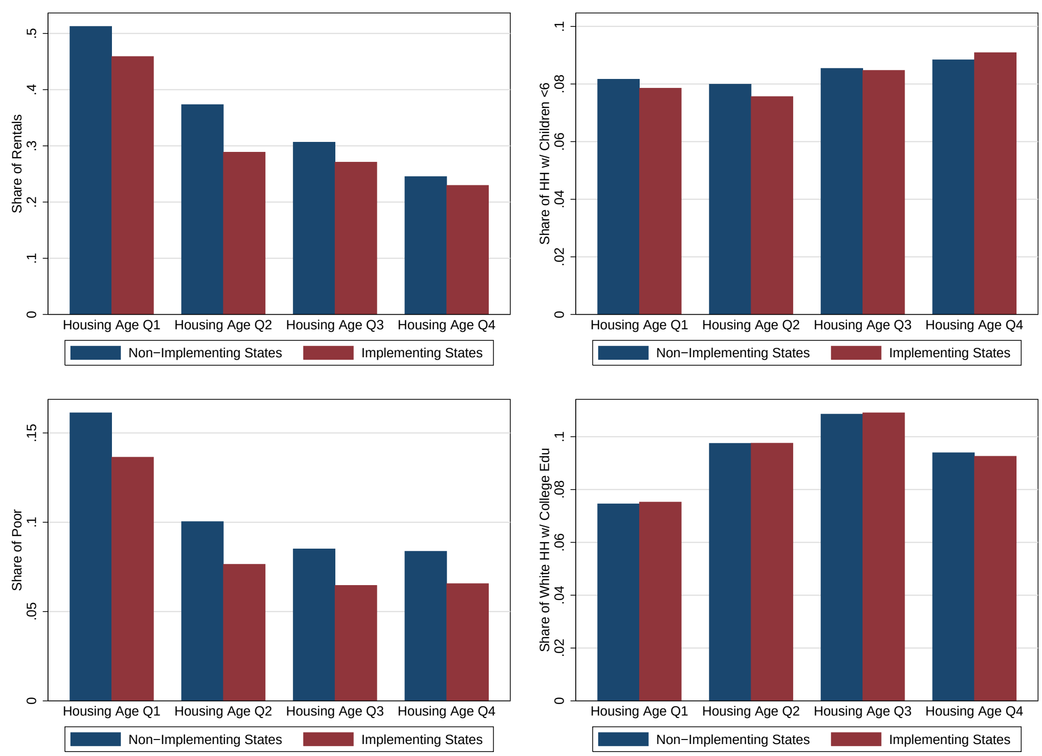
Notes: The figure plots the shares of old houses in census tracts in Wayne county, Michigan, in the DataQuick sample.

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



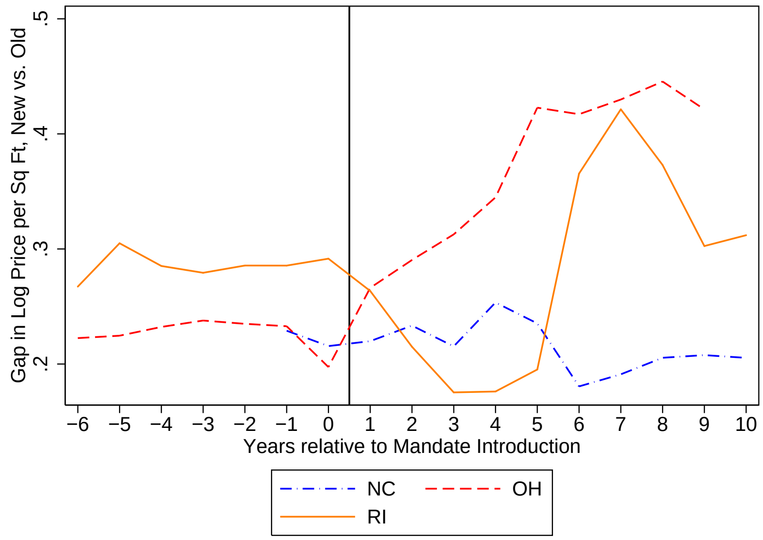
Source: NBCD. The figure plots the distribution of housing stock age in US census tracts, with darker colors assigned to tracts with older houses, i.e., where median age is higher. Age is computed in years.

Figure B.3: Correlation between Age of the Housing Stock and Demographics at the Tract Level



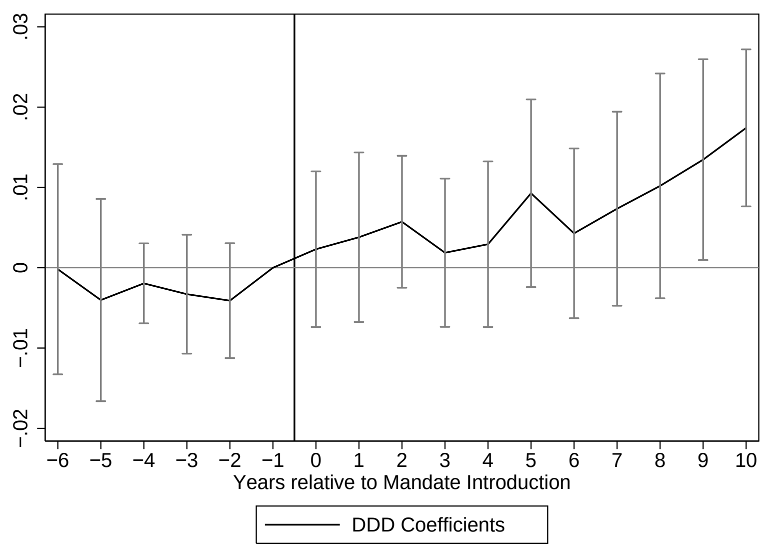
Notes: The figures plot the shares of rental houses, households with children below age five, households below poverty, and white, college-educated households, in each tract by quartile of average construction year (tracts in the first quartile have the oldest housing stock). Blue bars denote non-implementing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

Figure B.4: 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.

Figure B.5: Allocation Effects: Occupancy



The figure plots DDD coefficients on year-by-year mandate dummies, estimated on the linked DataQuick-HMDA sample for the years 1990-2012, where each census tract is weighted by population in 1980. The outcome variable is an indicator for rental property. The vertical line at $t = 0$ 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 dashed lines are lower and upper bounds of 95% confidence interval. Standard errors are clustered at the state level (42 clusters).

Table B.1: Summary Statistics

	DataQuick (1)	DataQuick, HMDA Sample (2)	AHS (3)
Price per Square Foot	144.91	167.16	132.67
	(658.47)	(361.52)	(223.95)
Vintage	1968.52	1965.88	1956.88
	(26.51)	(26.57)	(25.09)
Square Footage	1640.05	1635.06	1698.66
	(683.29)	(661.95)	(1484.63)
Number of Rooms	6.39	6.47	5.39
	(1.87)	(1.82)	(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.

Table B.2: Price Effects for Multi-Family Properties, 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	-0.040***	-0.005	-0.045***	-0.032***	-0.028***
Mandate	(0.008)	(0.019)	(0.014)	(0.011)	(0.010)
4-6 Years After	-0.088***	-0.058***	-0.041	-0.085***	-0.084***
Mandate	(0.016)	(0.015)	(0.026)	(0.022)	(0.022)
7-10 Years After	-0.087***	-0.051***	-0.011	-0.084***	-0.087***
Mandate	(0.010)	(0.013)	(0.029)	(0.014)	(0.012)
11+ Years After	-0.086***				
Mandate	(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: $***$ p<0.01, $**$ p<0.05, $*$ p<0.1. The table presents DDD estimates on the transaction sample for multi-family properties 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 unit. 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 B.3: Price Effects for Multi-Family Properties, 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: $***$ p<0.01, $**$ p<0.05, $*$ p<0.1. The table presents DDD estimates on the transaction sample for multi-family properties 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 unit. 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 B.4: Sale Effects

Dependent Variable:	Probability of Sale (X1000)	
Sample:	Multi-Family	Single-Family
	(1)	(2)
Mandate Effects on Old Houses:		
0-3 Years After Mandate	-0.896	0.133
	(0.556)	(0.332)
4-6 Years After Mandate	-0.861**	-0.042
	(0.355)	(0.455)
7-10 Years After Mandate	-0.358	-0.078
	(0.582)	(0.570)
N	582956	2098910
Probability of SaleX1000, New Homes, Pre-Period	10.611	13.770
Probability of SaleX1000, Old Homes, Pre-Period	6.876	11.085

Notes: *** p<0.01, ** p<0.05, * p<0.1. The table presents DDD estimates on the transaction sample of multi- (Column 1) and single-family properties (Column 2) from DataQuick for the years 1988-2012, where each observation is weighted by population in 1980. Each observation is a tract-year-vintage cell, and the dependent variable is the share of houses in the cell transacted in that year multiplied by 1,000. 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. The probability of sale multiplied by 1,000 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 B.5: 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 Mandate	-0.052***		-0.031***	
	(0.016)		(0.006)	
0-3 Years After Mandate		-0.040***		-0.015**
		(0.010)		(0.006)
4-6 Years After Mandate		-0.060***		-0.043***
		(0.023)		(0.010)
7-10 Years After Mandate		-0.061***		-0.049***
		(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: *** p<0.01, ** p<0.05, * p<0.1. The table presents DDD estimates on the transaction sample of rental (Columns 1-2) and owner-occupied properties (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 unit. 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 B.6: 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: *** p<0.01, ** p<0.05, * p<0.1. 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 unit. 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 B.7: 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: *** p<0.01, ** p<0.05, * p<0.1. 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 B.8: 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 B.9: 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
Outcome Mean, New Homes, Pre-Period	0.156	0.152	0.204	10.614
Outcome Mean, Old Homes, 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: *** p<0.01, ** p<0.05, * p<0.1. 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, property 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 B.10: 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 Old Houses	-0.058*	-0.030	-0.014	-0.024	-0.018	0.034**
	(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 Mandate	-0.082***	-0.003	-0.003	-0.032*	0.010	0.023
	(0.025)	(0.013)	(0.037)	(0.017)	(0.026)	(0.014)
4-6 Years After Mandate	-0.062	-0.054	0.018	-0.015	-0.045	0.032***
	(0.043)	(0.034)	(0.061)	(0.027)	(0.033)	(0.011)
7-10 Years After Mandate	-0.032	-0.062**	-0.062	-0.019	-0.048	0.039
	(0.036)	(0.027)	(0.059)	(0.032)	(0.044)	(0.028)
10+ Years After Mandate	-0.035	-0.031	0.002	-0.009	-0.033	0.093***
	(0.037)	(0.033)	(0.051)	(0.027)	(0.071)	(0.030)
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: *** p<0.01, ** p<0.05, * p<0.1. The table presents DDD estimates on the AHS sample of multi- (Columns 1-3) and single-family properties (Columns 4-6) for the years 1985-2009. Outcome variables are defined in each column. State-year, year-vintage, month of interview and property 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 B.11: 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: *** p<0.01, ** p<0.05, * p<0.1. 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.