The Bronx is Burning: Urban Disinvestment Effects of the Fair Access to Insurance Requirements*

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Abstract

We study the unintended effects of Fair Access to Insurance Requirements (FAIR) plans developed by 26 states in the 1960s to address insurance redlining in urban neighborhoods. FAIR plans' problematic features included prohibitions on considering environmental hazards in underwriting, mandatory insurer participation that diluted underwriting incentives, and payouts exceeding market values in declining areas. Using a triple-difference design comparing pre/post-FAIR periods, neighborhoods with/without likely FAIR access, and participating/non-participating states, we find that FAIR inadvertently led to significant housing disinvestment, accelerated declines in neighborhood population and income, and increases in the Black population share.

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1 Introduction

Insurance is essential for well-functioning real estate markets, enabling loans, repairs, sales, and new construction. The current climate-driven insurance crisis echoes a mid-20th century urban insurance crisis when insurers abandoned American inner cities as affluent white households fled to the suburbs. By the 1960s, central neighborhood insurance policies were expensive, scarce, and vulnerable to cancellation (Aldrich and Reiss Jr 1970; Ansfield 2021). We examine the unintended consequences of a well-meaning policy intervention in this context of insurance scarcity and urban decline.

In response to insurance redlining concerns, Congress authorized Fair Access to Insurance Requirements (FAIR) plans in 1968, providing insurance to property owners denied coverage in the private market. To incentivize state participation in FAIR, Congress offered riot reinsurance to participating insurers (Maidenberg 1967). States quickly responded: 18 states offered FAIR plans in 1968, increasing to 26 by 1970 (Ansfield 2021). Over 300,000 FAIR Plan policies were issued in 1969, rising to 5.7 million by 1977 (US General Accounting Office 1978; Welsh 1972).

While FAIR plans helped many central city property owners secure insurance, concerns emerged that plan features incentivized housing disinvestment and "arson-for-profit." First, federal guidelines prohibited considering "environmental hazards" beyond property owners' control, such as crime or fire risks (US Congress 1968). Second, FAIR states typically required all fire and property insurers to participate, potentially reducing prudent underwriting incentives due to loss pooling (US General Accounting Office 1978; Works 1977). Third, many FAIR plans offered payouts that exceeded actual market value, especially in declining neighborhoods, which made passive disinvestment or even arson attractive to landlords (Dwyer 1978). While some blamed FAIR plans for the 1970s urban arson wave, a government report found no conclusive evidence (US General Account-

ing Office 1978). Others attributed the rise in arson to broader government policy and disinvestment in fire protection (Flood 2010).

We revisit this controversy surrounding FAIR's effects on housing and neighborhoods. A naive analysis would simply compare housing units and neighborhoods with FAIR plans to those without them. However, this approach faces two significant limitations: it is infeasible due to the absence of systematic data on FAIR plan issuance, and it is methodologically flawed because properties and neighborhoods denied private insurance (and subsequently offered FAIR plans) likely differ in numerous observed and unobserved dimensions from others.

Instead, we use a triple-difference design to estimate FAIR's effects. First, we compare outcomes before and after FAIR plan authorization in 1968. Second, we distinguish between neighborhoods likely issued FAIR plans and those without by measuring the withdrawal of private property insurance establishments from central neighborhoods using purpose-digitized city directories from 1940–1967 across 26 major US cities. (We validate our reduced-access measure using a 1977 survey of FAIR plans in New York City.) Third, we compare this within-city neighborhood contrast between states that launched FAIR plans before 1970 and states that did not.

A primary advantage of this design is the symmetric definition of neighborhoods with reduced access to private insurers in both early-FAIR and late-FAIR states. This third contrast between states effectively controls for confounding differences across neighborhoods. Our key identifying assumption is that state-level decisions to offer FAIR plans are unrelated to post-1968 contrasts between central-city neighborhoods that experienced postwar private insurer withdrawal and neighborhoods that did not. To support this assumption, we demonstrate that neighborhood contrasts before FAIR plan availability evolved similarly in states with and without FAIR plans.

An additional strength of our approach, stemming from the lack of microdata on FAIR

plans, is that we avoid inference problems related to endogenous take-up of FAIR plans. Even if FAIR policy-level data were available, property owners who accepted FAIR plans likely differ from those who did not in important ways. Instead, we use the postwar withdrawal of private property insurance establishments as an intention-to-treat, thus estimating intent-to-treat (ITT) effects.

Our results show that FAIR plan availability led to significant housing disinvestment, with affected neighborhoods experiencing declines in pre-war housing stocks between 1960 and 1990. We estimate an average loss of 313 pre-war housing units per census tract for the treated neighborhoods, or about 29.8% of the 1950 stock. These declines are consistent with evidence on building fires and are concentrated in multi-family and rental buildings. We also find that FAIR-induced housing disinvestment led to declines in population and income and increases in the Black population share. These neighborhood effects incorporate both direct effects on property owners' investment choices and indirect effects that operate through spillovers to neighboring property owners.

Prior research describes the history and features of FAIR plans (Ansfield 2021; Dwyer 1978; Squires et al. 1979; Works 1977). Our contribution to this literature is to estimate the causal effect of FAIR plans on housing and neighborhoods.

We also contribute to the literature on central city decline (Brooks et al. 2024; Collins and Margo 2007) and housing disinvestment in the mid-20th century (Feins 1977; Gyourko and Saiz 2004; Hillier et al. 2003; Raleigh and Galster 2015; Sternlieb et al. 1974) by highlighting the role of insurance. Much of this work focuses on the effect of neighborhood decline on housing disinvestment (Cornelissen and Jang-Trettien 2023). We instead identify the other side of the "doom loop": the effect of housing disinvestment on neighborhood decline. Scafidi et al. (1998) and White (1986) analyze the role of property taxes in housing abandonment. Increases in property taxes reduce landlords' expected net cash flows, reducing the opportunity cost of abandonment. We show how the generosity of

FAIR plan payouts increased the benefits of housing abandonment.

Finally, our analysis adds to the understanding of market failures that plague insurance markets. Prior work has studied whether the generosity of health insurance lead people to consume more medical care (holding health constant) due to cost-sharing (Einav and Finkelstein 2018; Zweifel and Manning 2000). It has focused less on whether the availability and generosity of health insurance leads people to behave more recklessly (Finkelstein 2014). Weisburd (2015) estimates that each $100 in auto insurance coverage in Israel leads to 1.7% more accidents. We offer evidence on how insurance contracts can distort behavior in real estate versus other markets. Relatedly, Eriksen and Carson (2017) show that total fires and suspicious fires increase with declines in local house prices, as replacement costs rise above market values. Compared with this work, we identify a distinct channel through property insurance distortions.

2 Historical Background

Context and policy response. In response to growing concerns about insurance redlining in central urban neighborhoods, President Johnson established the National Advisory Panel on Insurance in Riot-Affected Areas (Hughes Panel) in 1967. The panel's report, released in January 1968, highlighted the lack of access to reasonably-priced property insurance in low-income, urban neighborhoods (Dwyer 1978). Their survey revealed that over 40% of businesses and nearly 30% of residents in high-poverty neighborhoods were under-insured due to difficulties obtaining coverage (Hughes et al. 1968, p. 2). Commercial property insurance cancellation rates in areas with urban uprisings were more than double those in unaffected areas (Aldrich and Reiss Jr 1970).

The Hughes report identified redlining as a root cause, citing an insurance agent who described "knock-out areas" or "redline districts" where companies refused to write busi-

ness (Hughes et al. 1968, p. 6). To address this issue, the panel recommended establishing FAIR plans, which Congress quickly authorized. To incentivize adoption, Congress offered federal riot re-insurance to participating insurers (Maidenberg 1967).[1]

Seventeen states and the District of Columbia offered FAIR plans in 1968, with nine more in 1969 and 1970. Take-up was swift: over 300,000 FAIR plan policies were issued in 1969 alone (Welsh 1972), and over 800,000 policies were issued in each of the next three years (Demerjian et al. 2001). By September 1977, FAIR plans had insured over 5.7 million properties (US General Accounting Office 1978).

Take-up was likely accelerated because 13 of the early FAIR states had already created residual property insurance programs called Urban Area Plans, which became the blueprint for FAIR plans (Hughes et al. 1968).[2] These Plans, adopted between 1960 and 1967, limited the use of neighborhood characteristics in underwriting decisions, restricted surcharges even when risks were identified, required insurers to participate in risk-sharing pools, and waived certain property inspections. The structural similarities between Urban Area Plans and subsequent FAIR plans may have introduced moral hazard issues before FAIR plans were authorized in 1968. However, this does not affect our research design, as we compare outcomes in 1960 and earlier—before the first Urban Area Plan—with those in 1970 and later, after all early adopter states had implemented FAIR plans.

Concerns and unintended consequences. Despite good intentions, concerns about perverse incentives soon emerged. In the early 1970s, the Massachusetts FAIR Plan estimated that 60% of its losses were due to arson-related claims (Brady 1983). In 1978, the Senate Subcommittee on Investigations asked the GAO to study whether FAIR plans were incen-

1. Ironically, reduced urban unrest and declining perceived riot risk meant that the federal riot reinsurance program paid few losses and the private reinsurance market eventually recovered (Demerjian et al. 2001).
2. Boston launched the first Urban Area Plan in 1960, establishing practices that many other states would adopt. The Boston plan proved particularly successful, insuring over 20,000 properties by 1967, mostly in the Roxbury neighborhood (Hughes et al. 1968, p. 59). See Appendix C for details.

tivizing "arson-for-profit" (US General Accounting Office 1978). Critics identified three main issues stemming from FAIR plan requirements.

1. Limited underwriting flexibility: FAIR plans were prohibited from denying insurance based on neighborhood conditions, leading to high acceptance rates (US General Accounting Office 1978). Many FAIR plan officials reported feeling constrained in their ability to deny coverage, including to properties near abandoned buildings and owners who had previously been involved in suspicious building fires.[3]

2. Reduced incentives for prudent underwriting: Losses were pooled and shared across all property insurers in a state, diminishing individual insurers' motivation to pressure FAIR Plans for more careful underwriting (Works 1977).

3. Over-insurance: Many FAIR plans offered payouts equal to replacement cost minus depreciation, which often exceeded actual market value in declining neighborhoods (Dwyer 1978). Some states required insurers to pay the face value of the policy, regardless of market value (US General Accounting Office 1978). In one-third of states, owners could request coverage that was beyond the market value of the property (US General Accounting Office 1978).

In post-war central city neighborhoods, property owners were experiencing deteriorating demand and declining net cash flows, reducing their opportunity costs of abandonment. These FAIR plan features generated moral hazard by sharply increasing landlords' benefits of abandonment, passive disinvestment, and even arson. To illustrate this, we develop a simple model of landlord abandonment in Section 5.2.

3. In Illinois, only one percent of applicants were denied coverage. Other officials complained about the 30-day notice required before a policy could be terminated, during which buildings were particularly vulnerable to arson (US General Accounting Office 1978).

Contemporary controversy and evidence. While the GAO found no conclusive evidence that FAIR plans encouraged arson, other contemporary reports (Fisher et al. 1976; US Congress 1979) blamed FAIR plans for increasing arson rates. Media investigations linked arson to insurance fraud facilitated by FAIR plans' under-selectivity and excessive generosity. A 1973 investigation by the *Chicago Tribune* (Young et al. 1973) found that "FAIR Plan [sic] must insure a hovel in the worst neighborhood in Chicago for the same amount it would be insured for if it were located in Kenilworth. Records show slumlords have taken advantage of this regulation, sometimes insuring buildings for 20 to 30 times what they paid, when 'they aren't worth anything unless you burn them.'"[4] A 1978 *Wall Street Journal* editorial complained about under-selectivity: "The problem is the FAIR plans can't just turn away applicants. Along with the vast majority of legitimate applicants have come a few 'torchers' who make a nice profit from burning down worthless buildings for insurance" (Wall Street Journal 1978).

Although arson statistics from this period are scarce, some estimates suggest significant increases. Boudreau et al. (1977) estimated nearly 200,000 "incendiary" fires causing $1.2 billion in damage in 1974, a 270% increase since 1964. The New York City Fire Department reported arson incidents nearly tripled from 1967 to 1976 (Frawley et al. 1986). Partial statistics from three states with FAIR plans indicated substantial arson-related losses and suspicious fire claims (US General Accounting Office 1978). In Illinois, 33% of FAIR plan fire claims were arson in 1977, totaling $7.7 million. In Massachusetts, one FAIR plan official estimated that 40% of all arsons in the State were FAIR plan-related. In Pennsylvania, FAIR plan losses from arson or suspicious fires totaled $1.8 million in 1976 and 1977, and officials noted involvement of organized crime.

Most FAIR Plans lost money. As of September 1977, only five of 27 plans had earned an

4. This investigation also illustrated the difficulty that police and insurers had in proving arson. Despite the fact that "[w]itnesses told police[...] that some containers of flammable liquid were carried into the [Spector-owned] Calumet Avenue building by 'painters' three days before the fire [...] Spector's $24,922 insurance claim was paid without question" (Young et al. 1973).

underwriting profit since their launch (US General Accounting Office 1978). Demerjian et al. (2001) estimated aggregate statutory underwriting losses for all FAIR plans 1970–1998 totaled $1.5 billion.

3 Data and Methods

Measurement. We use a balanced panel of consistent-boundary census tracts from 1950 through 1990 in 26 major US cities from Lee and Lin (2018) (See Appendix B for details). Cities were selected based on the availability of 1950 Census tract data (Manson et al. 2023) and directory data in 1940 and 1967.

Our main outcome of interest is the number of pre-war housing units (built prior to 1940) in each tract–year, focusing on the housing disinvestment margin. We also examine additional neighborhood outcomes, including total housing units, average rents, average household income, average education, and Black population share. Our sample contains about 6,000 census tracts over five decades (1950–1990), or 30,000 tract–year observations.

To identify neighborhoods likely offered FAIR plans, we digitized city directories for 1940 and 1967 in 26 large US cities.[5] This approach allows us to observe private insurer withdrawal in both early- and late-FAIR states. An important virtue of not relying on actual FAIR plan issuance is that take-up was likely endogenous to property and landlord characteristics. Relying on insurer withdrawal allows us to treat early- and late-FAIR states symmetrically and estimate intent-to-treat effects.

Insurer market access M for each tract i in city c and year $t \in \{1940, 1967\}$ is

$$M_{ict} = (1/G_{ct}) \sum_{j=1}^{J_{ct}} e^{-\gamma d_{ij}}, \tag{1}$$

5. For New York City, we digitized directories for Manhattan, Brooklyn, and the Bronx. See Appendix D for details.

where G_{ct} is the geo-coding rate, d_{ij} is the distance from the centroid of tract i to insurer j; J_{ct} is the total number of geo-coded property and casualty insurers in city c in year t; and γ is a spatial decay parameter that determines how quickly access drops off as distance increases. We scale our index by the geo-coding rate for each city–year to account for variation in our success in geo-locating addresses.[6] We set $\gamma = 4$ which implies 95% decay at a distance of 0.75 miles. (Our results are robust to alternative measures; see Appendix Table G.3.)

We define a binary treatment I_{ics} indicating decreased market access to property and casualty insurers, 1940–1967 ($I_{ics} = 1[M_{ic1967} < M_{ic1940}]$). Sixteen percent of sample tracts are classified as treated (reduced access in early-FAIR states), compared with 65% as control (stable access) in early-FAIR states. In late-FAIR states, reduced-access and stable-access tracts represent 4% and 15%, respectively, of our sample (see Table F.1).

Example and validation. Figure 1 illustrates our methodology using New York City as an example. Panel (a) shows the locations of private property and casualty insurance establishments in Manhattan, Brooklyn, and the Bronx. Panel (b) shows that after World War II, private insurers withdrew from many New York City neighborhoods, particularly in the South Bronx and Northern Brooklyn. These neighborhoods likely saw increased FAIR plan offerings and constitute our intent-to-treat areas. Other neighborhoods maintained or improved access to private insurers. Panel (c) displays our main outcome, change in pre-war housing units, as a measure of disinvestment. Notably, areas experiencing insurer withdrawal, especially in the South Bronx and Northern Brooklyn, show significant housing unit losses.[7]

While data on FAIR plan take-up is scarce (and likely endogenous), partial informa-

6. The implicit assumption in this scaling is that the non-geocoded establishments would have the same spatial distribution as the geo-coded addresses.
7. Green shades indicate growth in pre-war housing units, possibly due to conversions of commercial/industrial buildings or unit subdivisions.

tion shows that post-war insurer withdrawal was strongly associated with subsequent FAIR Plan offering and take-up. In 1977, the Federal Insurance Administration (FIA) sampled New York FAIR plan policies (Nwokolo 2023; US Congress 1978), tabulating coverage by neighborhood. Despite representing a single snapshot nearly a decade into FAIR implementation, these data correlate strongly with our treatment measure. Tract-level correlation coefficients are 0.50 and 0.39 for Brooklyn and the Bronx, respectively. Appendix Figure E.1 shows a chloropleth map of 1977 FAIR plan policies, which compares well to our treatment definition in Figure 1b.

Triple difference design. Our main analysis uses a triple-difference design, comparing:

1. Changes in outcomes before and after 1968 for neighborhoods likely offered FAIR plans (reduced-access neighborhoods).

2. These changes against similar neighborhoods in the same city with less FAIR plan prevalence (stable-access neighborhoods).

3. The within-city neighborhood contrast in early-FAIR states versus states that did not offer residual plans.[8]

Our identifying assumption is that the within-city contrast between reduced-access and stable-access neighborhoods in states that did *not* offer FAIR plans is an appropriate counterfactual for the contrast in states that *did* offer FAIR plans. Supporting this assumption, it seems plausible that state-level FAIR adoption decisions did not depend on within-city contrasts in declines in access to private insurers. The main threat to identification is unobserved divergence of neighborhood factors in reduced-access versus stable-access

8. Some states have never participated in the FAIR program; nonetheless we refer to them as "late-FAIR states" to emphasize the symmetry of the contrast. The earliest participation date among the late-FAIR states was West Virginia in 1986, followed by Hawaii and Florida in 1991 and 1993, respectively. Arkansas and Mississippi adopted limited plans that were available in rural areas only (Demerjian et al. 2001).

neighborhoods in early-FAIR but not late-FAIR states. This potential confounder would require, for example, differential insurer withdrawal responses to rising neighborhood risks in late- compared with early-FAIR states. Such unobserved demand factors are not supported by our pre-trends analyses in Section 4 and Appendix F.

Our simplest specification is

$$y_{icst} = \beta_1 F_s I_{ics} \text{Post}_t + \beta_2 F_s \text{Post}_t + \beta_3 I_{ics} \text{Post}_t + \beta_4 F_s I_{ics} + \beta_5 F_s + \beta_6 I_{ics} + \beta_7 \text{Post}_t + \epsilon_{icst}, \quad (2)$$

where F_s is a binary variable indicating whether state s adopted FAIR plans by 1970, I_{ics} is a binary variable indicating that market access to property and casualty insurers declined 1940–1967 ($I_{ics} = 1[M_{ic1967} < M_{ic1940}]$), Post_t is a binary variable indicating year 1970 and later, and ϵ_{icst} is an error term. β_1 is the triple-difference coefficient of interest.

We use two additional specifications. The first augments equation 2 by adding census tract fixed effects to control for level differences in outcomes across neighborhoods. The second instead allows for observed fixed neighborhood characteristics to have time-varying effects. Based on our analysis of pre-trends in Appendix F, we include, as controls, the interaction of Census year indicators with (i) the 1950–1960 change in the Black population share and (ii) the 1950–1960 change in log tract total population. We prefer this specification because it allows us to flexibly control for pre-trends in housing demand and racial composition.[9] This addresses concerns that our results could simply be picking up the continuation of changes that were already happening in neighborhoods where insurance became less accessible.

We construct a placebo treatment using changes in access to law firms to address concerns about unobserved factors affecting general commercial activity, using the same ap-

9. An earlier version of this paper reports results with additional controls for differential trends according to distance to city center and pre-trends in education and housing units, with quantitatively similar results.

proach to defining market access used for property insurers.

Finally, we also run several robustness tests (see Appendix G). We estimate our regressions using different measures of treatment. We test for influential observations at the city level. We estimate heterogeneous effects by structure type. We drop cities with lower geo-coding rates. We use alternative functional forms.

4 Results

4.1 Pre-war housing units

Figure 2a shows pre-war housing units (built 1940 or earlier) in treatment and comparison groups across early and late FAIR states. Pre-trends between 1950 and 1960 appear comparable across groups in both early and late FAIR cities, supporting the parallel trends assumption. All groups experienced declines after 1960, consistent with disinvestment and population loss in US central cities. However, significant differences emerged in early FAIR states.

In early FAIR cities, neighborhoods with declining private insurance access between 1940 and 1967 showed a larger decrease in the pre-war housing stock compared to those with stable access. This effect became apparent during the 1960s and was pronounced in the 1970s, aligning with FAIR plan implementation.[10] Strinkingly, in late-FAIR states, there is little difference between treatment and control groups.

We analyze these patterns by estimating Equation 2 using OLS. Table 1 displays results with different controls. The dependent variable is pre-war housing units. Column 1 includes no controls, Column 2 adds tract fixed effects, and Column 3 controls for the two tract characteristics described in Section 3 interacted with year fixed effects.These

10. Admistrative evidence suggests immediate high application volume, with hundreds of thousands of policies written before 1970 (Demerjian et al. 2001; Welsh 1972). Eighty-nine percent of treated tracts are in states that offered FAIR plans in 1968.

controls allow for differential dynamics associated with these neighborhood characteristics (see Appendix F for details). Notably, the robustness of our estimates to including these controls shows that the variation in insurer withdrawal that is driving our results is orthogonal to the observed evolution of the demographic structure of post-war cities.

Across columns, the estimated triple difference is negative, statistically significant, and consistent in magnitude. In our preferred specification (Column 3), the ITT effect of FAIR plans was a loss of about 313 pre-war housing units per census tract in the treated neighborhoods, accounting for 29.8% of the 1950 sample mean. For inference, the table shows robust standard errors clustered at the city level. Alternatively, using the wild cluster bootstrap, we reject the null hypothesis that the triple-difference coefficient is zero at the 1% level ($p = 0.004$).

Figure 2b and Appendix Figure G.1 show event study estimates, confirming parallel pre-treatment trends and revealing the timing of effects. About one-third of the total dynamic effect appears by 1970, with nearly 90% by 1980. Regulatory reforms in 1980 allowed for stricter FAIR plan underwriting standards (Demerjian et al. 2001).

Owner-occupiers face higher opportunity costs of disinvestment than landlords, and thus we would not expect FAIR plan availability to have as large an effect on owner-occupied properties. Appendix Table G.1 confirms this, showing small and insignificant effects on owner-occupied and single-family units, but significantly negative and large effects on rental and multi-family units, consistent with differential abandonment incentives for landlords versus owner-occupiers.

Robustness. We explore robustness through alternative specifications and treatment definitions. Table G.2 allows for asymmetric continuous effects of post-war changes in access to private insurers. Table G.3 considers alternative measures of market access. Figure G.2 shows results leaving out one city at a time. Table G.4 drops cities with the lowest

geo-coding rates. Our results remain robust to these alternatives.

Placebo test using changes in access to lawyers. Table 1's final column presents a placebo test replicating our preferred specification (Column 3) with an alternative treatment variable based on reduced access to law firms. This placebo treatment shows no effect on pre-war housing units, suggesting our results do not capture unobserved factors associated with general withdrawal of professional services from treated neighborhoods.

The lawyer specification uses a smaller sample due to limited geocoding of historical law firm locations. Column 4 shows our insurance access results remain consistent with this sample, confirming that the difference between declining access to property insurers and law firms is not due to sample variation.

4.2 Neighborhood outcomes

We estimate the ITT effect of FAIR plans on neighborhood outcomes. These incorporate *direct* effects on property owners' passive disinvestment and arson choices, and *indirect* effects through spillovers to neighboring properties.

Table 2 displays results using our preferred specification in Table 1, Column 3. The regressions are weighted by initial tract population. The key coefficient of interest is the triple interaction term in the first row.

Reduced access leads to significant declines in a tract's total population, white population, and nonwhite population (Columns 1, 3, and 4). Intent-to-treat results in a 8.6 percentage point increase in the Black population share (Column 5). We see little impact on rents, perhaps because the reduction in supply is countered by a reduction in housing quality and demand. But we see negative and significant impacts on average income.

These findings suggest FAIR plans substantially impacted neighborhood composition and economic conditions, potentially accelerating white flight and decreasing neighbor-

hood economic status in treated areas. The results highlight the complex interplay between insurance policy, housing markets, and neighborhood dynamics, revealing unintended consequences of FAIR plans.

5 Discussion

We explore why the pre-war housing stock declined sharply after FAIR plans were introduced through two auxiliary analyses. First, cross-city evidence on building fires is consistent with FAIR effects through active disinvestment or arson. Second, a simple theory of landlord abandonment identifies neighborhood decline and over-insurance as key factors in understanding our results.

5.1 Evidence from building fires

We present evidence suggesting a possible association between FAIR plans and building fires, which encompass active disinvestment (e.g., arson), passive disinvestment (e.g., neglected maintenance), and accidental causes.

Our data come from National Fire Protection Association (NFPA) reports (1938-1969) (n.a. 1939), a 1978 US Department of Justice (DOJ) survey (Webster and Matthews Jr. 1979), and the National Fire Incident Reporting System (NFIRS, 1980–1988). These sources are survey or administrative data reported by local fire departments, ensuring methodological consistency.

We examine building fires in 43 cities (with population $\geq$ 250,000 in 1964) across 16 years: 1942, 1948, 1953, 1959, 1964, 1969, 1978, and annual data from 1980–1988. We estimate the following regression:

$$\log \text{fires}_{ct} = \delta_c + \delta_t + \alpha_c t + \gamma \text{Post}_t \times F_s + \epsilon_{ct} \tag{3}$$

where δ_c and δ_t are city and year fixed effects, α_c is a city-specific trend, and γ is the coefficient of interest on the interaction between early-FAIR states and post-FAIR observations.

We estimate $\hat{\gamma} = 0.277$ (robust s.e. $= 0.122$), indicating that cities in early FAIR plan states experienced 32% more building fires after 1968 compared with non-FAIR cities, conditioned on city and year fixed effects and city-specific trends. This result provides evidence supporting contemporary commentary that early FAIR plans may have contributed to arson and building disinvestment.

Notably, conditioned on city and year fixed effects and city trends, early FAIR cities experienced slightly fewer fires through 1964, (-0.062, robust s.e. $= 0.113$). In addition, the effects on building fires appear to be isolated to the period between 1968 and 1980, when FAIR reforms reduced the problem of over-insurance (Demerjian et al. 2001). Estimating separate coefficients γ_1 for $t \in [1968, 1980]$ and γ_2 for $t > 1980$ yields $\hat{\gamma}_1 = 0.268$ (robust s.e. $= 0.117$) and a statistically-insignificant $\hat{\gamma}_2 = 0.141$ (robust s.e. $= 0.208$).

We present back-of-the-envelope calculations comparing the magnitude of these results to our estimates of FAIR plan impacts. In Manhattan, the Bronx, and Brooklyn, we identify 286 census tracts as having declining insurance access and thus likely to have seen high FAIR plan offerings. Our estimates above imply 13,000 excess fires in New York City in 1978 relative to the predicted value from the estimates of Equation 3 had New York not been an early-FAIR state; assuming 9,000 excess fires (13,000 excess fires $\times$ the 70% of New York City buildings located in Manhattan, the Bronx, or Brooklyn) were concentrated in these tracts, this implies 32 building fires per tract in that year. Similarly, Philadelphia experienced about 500 excess fires in 1978, or 23 building fires per tract across 22 declining insurance access tracts. Extrapolating these annual rates to a decennial rate of housing unit loss yields estimates comparable to our main results.

5.2 Theory

We construct a simple theoretical model to rationalize the estimated impacts of FAIR plans on housing stock and building fires. A landlord owns a housing unit i in neighborhood j, with value $V_i = V(B_{j(i)}, Q_i)$, where $B_{j(i)}$ represents neighborhood factors and Q_i represents housing unit factors. The partial derivatives are $V_B > 0$ and $V_Q > 0$.

The housing unit is already constructed; thus, the initial amount and quality of housing Q is given. Moreover, neighborhood quality B has declined significantly since the building was constructed, matching the well-documented decline of post-war US central neighborhoods. In this context, low rents make rebuilding or renovation undesirable. In other words, replacement costs far exceed the discounted value of potential future rents. Instead, the main choice faced by the landlord is routine maintenance (thus keeping the unit habitable and available for rent) or abandonment.[11]

In each period $t \geq 0$, the landlord decides whether or not to stop maintenance and abandon the property. If not, the landlord may conduct routine maintenance and pays a fixed cost $f(t)$. These fixed costs could include, e.g., property taxes and mortgage payments. In each period that the landlord has *not* abandoned the property, they earn rent net of routine maintenance expenditures $p(t) > 0$. The housing unit, and thus net rent, depreciates at an exogenous rate stemming from the aging of the structure.

To sum up, the landlord chooses the period of abandonment t' to maximize discounted future profits under discount rate r.

$$\max_{t'} \int_0^{t'} e^{-rt}[p(t) - f(t)]dt \tag{4}$$

11. This structure accords with prior work on post-war housing markets in central neighborhoods. "[E]conomists who have studied 'slum' housing markets suggest that ownership abandonment is not a random or unexpected event, but a planned occurrence. These markets are said to attract specialized entrepreneurs for whom ownership abandonment is but the last step in a planned process of deferring maintenance, dropping services, and generally trading off immediate profits against a lengthening of the owned lifetime of the building" (White 1986, pp. 312–3).

To simplify the problem further, assume that abandonment immediately results in zero future rents. This assumption implies that net rents do not depend on the level of routine maintenance, except on the extensive margin, and abandonment and routine maintenance are a single choice.[12]

Conditioned on neighborhood quality, housing depreciates so that net rents decline exponentially at rate δ: $p_t = e^{-\delta t} p_0$. Further, fixed costs are constant over time, so that $f_t = f$. This simplified problem is depicted in Figure 3. The total shaded area V^0 corresponds to the landlord's discounted future profits in initial period t_0, or, in other words, the property value. The landlord continues to invest in routine maintenance and rent out the property as long as gross rents exceed the sum of routine maintenance and fixed costs. That is, the landlord's optimum is to abandon the property in period t^*.

Now consider the introduction of FAIR plans, which were restricted from considering neighborhood factors and offered payouts exceeding the market value of insured properties in neighborhoods that had experienced significant declines (Dwyer 1978). Together, these features imply that a typical FAIR plan at period t' offered an insured value of $V_i^{FAIR} = V(Q_i) > V(B_{j(i)}, Q_i)$ greater than the discounted value of profits, or an amount greater than that corresponding to the smaller shaded purple triangle labeled $\mathbf{V}'$. This problem of over-insurance was heavily cited in the US General Accounting Office (1978) report on arson in central cities. It follows that a landlord would immediately choose to abandon their property if offered a FAIR payout $V^{FAIR} > \mathbf{V}'$ at time t'.

While this framework is highly stylized, it captures some key features. As noted, FAIR plans were offered in the context of neighborhood decline and low rents near the abandonment threshold. As seen in Figure 2a, the stock of pre-war housing units gradually declined in all four of our groups throughout the sample period. As emphasized by US

12. This assumption has the flavor of active disinvestment or arson. Contemporary news coverage often observed that suspicious fires occurred even as the buildings were still occupied (Laing 1970; Young et al. 1973).

General Accounting Office (1978), FAIR payouts often exceeded the value of discounted future rents net of expenses. Once offered, FAIR policies were rapidly adopted. Finally, our evidence is consistent with relatively rapid abandonment of rental properties.

This framework also connects to our empirical design. The within-city contrast compares housing units in reduced-access versus stable-access neighborhoods in *both* late-FAIR and early-FAIR states. A FAIR plan offering V^{FAIR} where $V' < V^{FAIR} < V^0$ would result in abandonment in the reduced-access neighborhood but not in the stable-access neighborhood. Instead, abandonment takes place later, and gradually, as depreciation eventually pushes rent revenue below expenses. The within-city contrast between stable-access and reduced-access housing units might correspond to a comparison between two housing units at different points on the depreciation curve. However, the cross-state contrast makes the same comparison, except that in late-FAIR states, reduced-access neighborhoods are *not* offered a FAIR plan.

That said, this framework lacks some channels that might be relevant. It omits any detection or punishment for landlords that choose arson. Our simple framework also omits any spillovers from abandonment to the value of neighboring properties, which might amplify the effects of FAIR plan offering on abandonment. Finally, in our simple framework disinvestment and abandonment occur simultaneously. If the landlord continues to receive rental income for some time after lapses in maintenance, the equilibrium is somewhat different, but the difference-in-differences follows through. See White (1986) for an analysis of a similar problem.

Despite these limitations, the framework helps to rationalize our results and clarifies certain identification assumptions and challenges. Neighborhood decline and over-insurance are key factors in understanding our results.

6 Conclusion

Our results suggest that residual property insurance plans adopted in the 1960s reduced incentives to invest in or maintain housing, which led to significant declines in the stock of pre-war housing units. This is consistent with FAIR plan features that created moral hazard and increased the benefits of abandonment by landlords. Our results also suggest that FAIR plans led to significant neighborhood change. Neighborhoods that were likely to be offered FAIR plans saw relative declines in population and income and increases in the Black population share. In sum, our results provide new evidence that FAIR plans over-insured properties, creating moral hazard and accelerating housing disinvestment in mid-century US central cities. Further, our results provide evidence for the role of housing disinvestment—whether through arson or more subtle neglect—in neighborhood decline.

Our results do not imply that any public intervention in insurance markets will have the same effect. If FAIR Plan policies had been granted more discretion to consider legitimate environmental risks (such as proximity to fire hazards and fire history of property owners), and insurance payouts had been limited to market values, these public-private plans may not have triggered the same levels of arson and disinvestment. That said, the unintended consequences of FAIR plans in the late 1960s and 1970s highlight the challenges in designing policy responses to address unraveling property insurance markets.

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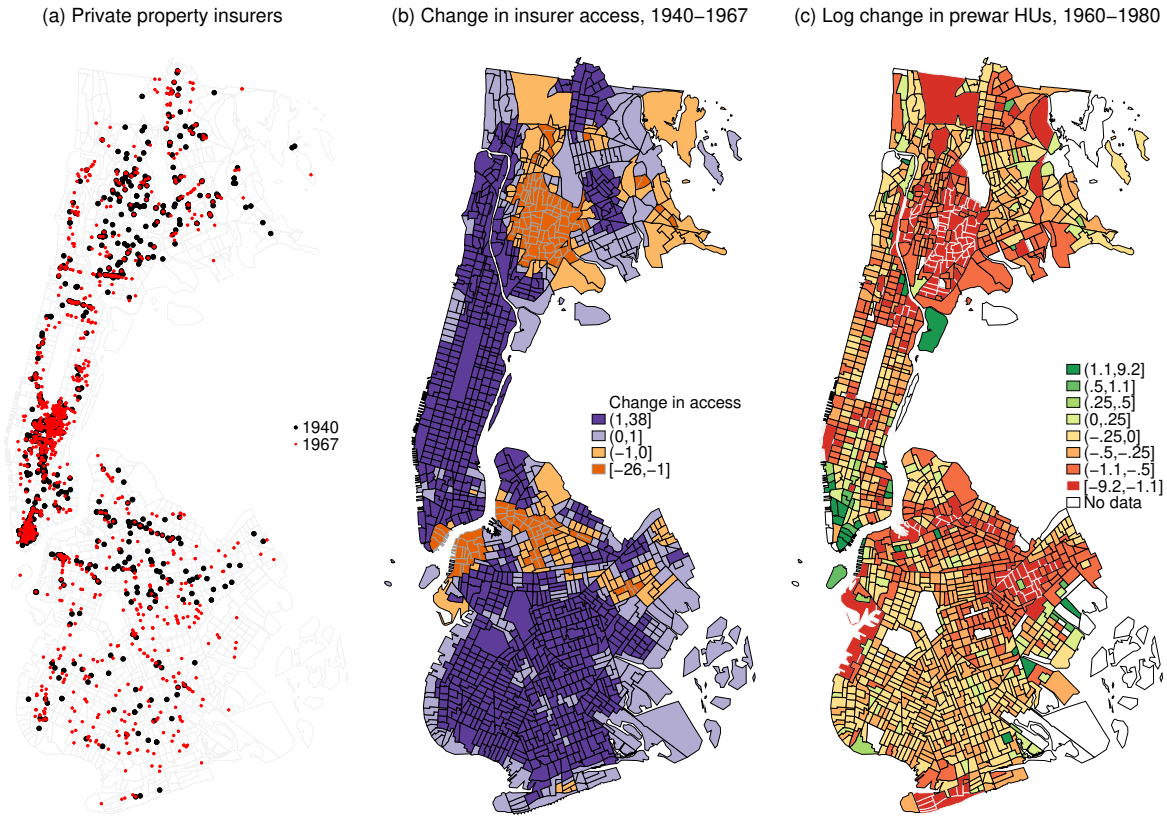
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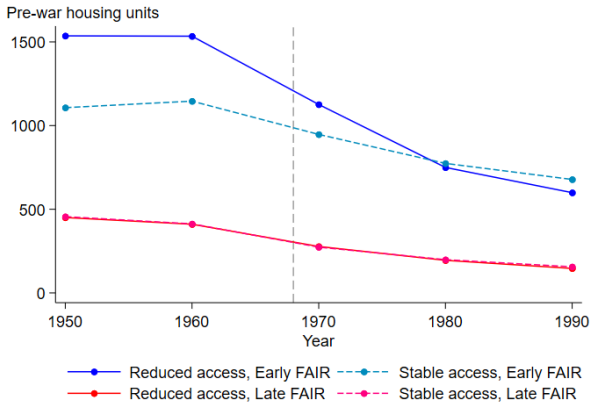
These maps show (a) the location of private property and casualty insurance establishments from city directories for Manhattan, Brooklyn, and the Bronx circa 1940 and 1967 and (b) changes in a market access measure for 2010 US Census tract geographies. Panel (c) shows the log change in pre-war housing units, i.e., housing units built 1940 or earlier.

Figure 1: Insurer withdrawal and post-FAIR housing disinvestment

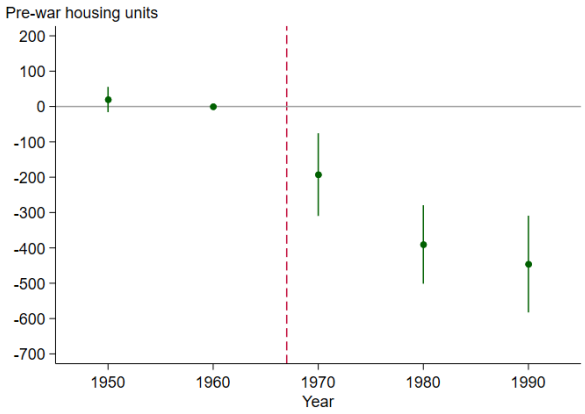
Appendix

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(a) Group averages

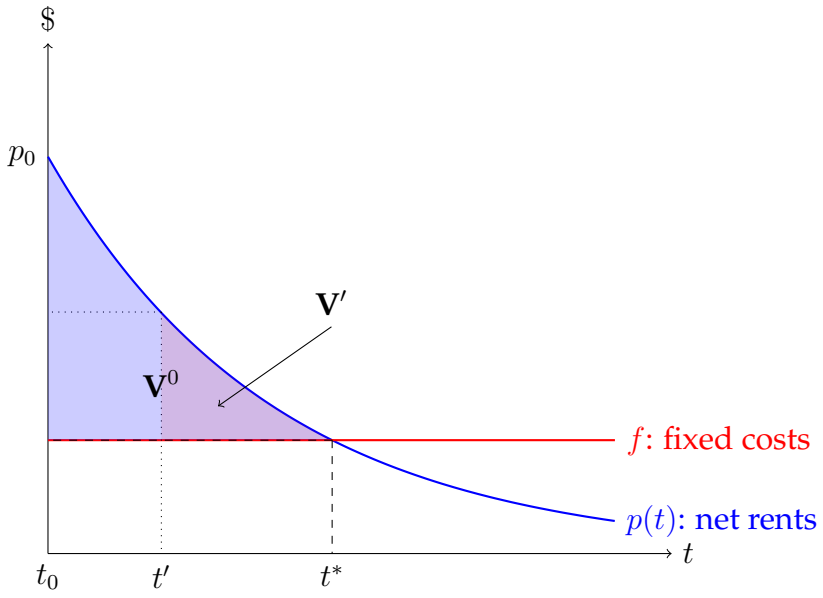


(b) Triple-difference event study



Panel (a) shows average pre-war housing units by year for four groups of consistent-boundary Census tracts across 26 cities classified by State FAIR adoption and tract change in access to private insurers. Pre-war housing units are housing units built 1940 and earlier. Early FAIR states are those that offered FAIR plans by 1970. Reduced-access tracts are those that saw declining market access to private property insurers, 1940–1967. Vertical line denotes the authorization of FAIR plans in 1968. Panel (b) shows ITT estimates using the event study version of the triple-difference specification in Table 1, Column 3.

Figure 2: Pre-war housing units by year and comparison group



This figure depicts the landlord abandonment problem under low and declining rents. The shaded region $\mathbf{V^0}$ corresponds to the landlord's discounted future profits in initial period t_0. At time t', the landlord may be offered an FAIR policy with payout exceeding the amount corresponding to $\mathbf{V}'$, leading to abandonment.

Figure 3: Landlord abandonment problem

Table 1: ITT effects of FAIR plans on pre-war housing units

	(1)	(2)	(3)	(4)	(5)
Outcome variable:	Housing units built 1940 or earlier				
Treatment definition:	Insurers	Insurers	Insurers	Insurers Lawyer sample	Lawyers
1(Reduced Access)*1(early FAIR)	-382.3***	-382.9***	-312.5***	-349.3**	-84.2
*1(>=1970)	(71.6)	(71.1)	(73.1)	(147.9)	-104.8
1(early FAIR)*1(>=1970)	-104.4	-108.0*	3.9	73.7	34.2
	(61.9)	(61.4)	(63.2)	(44.1)	-50.4
1(Reduced Access)*1(>=1970)	-1.2	-2.0	-8.0	24.8	-5.9
	(46.2)	(45.3)	(57.2)	(138.4)	-82.2
1(Reduced Access)*1(early FAIR)	412.0**		295.2*	486.1**	208.1
	(154.5)		(161.8)	(178.9)	-179.2
1(Reduced Access)	-2.5		9.0	-204.8	-47.2
	(90.7)		(117.8)	(135.3)	-101.7
1(early FAIR)	692.8***		473.5***	360.5***	367.3***
	(121.6)		(122.8)	(89.1)	-98.4
1(>=1970)	-225.8***	-225.0***	-463.2**	-464.3*	-467.9*
	(59.5)	(58.7)	(217.8)	(246.1)	-246.3
Tract Fixed Effects	NO	YES	NO	NO	NO
Tract Changes*Year Fixed Effects	NO	NO	YES	YES	YES
Observations	29,726	29,726	29,726	24,024	24,024
R-squared	0.167	0.843	0.248	0.216	0.212

This table reports OLS estimates of equation 2. Each observation is a census tract×year. The dependent variable is the number of pre-war housing units, or housing units built 1940 and earlier. 1(Reduced Access) is a dummy for change in market access between 1940 and 1967 being less than 0. Column 1 does not include any control variables and corresponds exactly to equation (1). Column 2 includes tract fixed effects. Columns 3–5 control for the 1950–1960 change in the black population share in the tract interacted with year fixed effects, and the 1950–1960 log change in population interacted with year fixed effects. Column 4 uses the same specification as Column 3 except using the same sample as Column 5. In Column 5, the treatment 1(Reduced Access) is defined based on access to lawyers. Robust standard errors are clustered at the city level. ***—$p < 0.01$, **—$p < 0.05$, *—$p < 0.1$.

Table 2: ITT effects of FAIR plans on neighborhoods

Outcome variables:	(1) log of white population	(2) log of black population	(3) log of nonwhite population	(4) log of tract population	(5) share black	(6) Years of educ. persons 25+	(7) log of avg. contract rent	(8) log of avg. income
1(Reduced Access)*1(early FAIR)	-0.494***	-0.181	-0.333***	-0.188**	0.086***	0.081	-0.030	-0.096**
*1(>=1970)	(0.174)	(0.122)	(0.110)	(0.075)	(0.030)	(0.234)	(0.057)	(0.044)
1(early FAIR)*1(>=1970)	-0.121	0.174	0.193**	0.003	0.039	0.496**	0.111*	0.010
	(0.153)	(0.106)	(0.084)	(0.063)	(0.027)	(0.224)	(0.063)	(0.032)
1(Reduced Access)*1(>=1970)	0.168	-0.002	0.104	0.008	-0.078***	-0.184	-0.021	0.017
	(0.151)	(0.099)	(0.067)	(0.064)	(0.018)	(0.196)	(0.049)	(0.024)
1(Reduced Access)*1(early FAIR)	-0.053	0.149	0.159	-0.009	0.051	-0.665***	-0.162**	-0.149***
	(0.101)	(0.312)	(0.313)	(0.127)	(0.049)	(0.234)	(0.070)	(0.048)
1(Reduced Access)	0.099	0.171	0.179	0.131	0.041	-0.071	0.077	0.031
	(0.089)	(0.249)	(0.253)	(0.109)	(0.044)	(0.193)	(0.064)	(0.042)
1(early FAIR)	0.324***	0.005	-0.033	0.155*	-0.110**	0.183	0.143*	0.233***
	(0.051)	(0.224)	(0.224)	(0.085)	(0.047)	(0.266)	(0.080)	(0.051)
1(>=1970)	-0.436**	-0.020	0.016	-0.390***	0.005	3.041***	1.831***	2.244***
	(0.157)	(0.191)	(0.194)	(0.069)	(0.033)	(0.274)	(0.075)	(0.036)
Tract Changes*Year Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
Observations	29,670	28,658	29,504	29,687	29,679	29,236	28,558	29,136
R-squared	0.486	0.121	0.106	0.155	0.357	0.220	0.841	0.817

This table reports OLS estimates of equation 2. Each observation is a census tract$\times$year. Dependent variables are noted in column headings. Columns 1, 2, and 3 are weighted by the 1950 white population, black population, and nonwhite population, respectively. Columns 4–8 are weighted by the 1950 population. 1(Reduced Access) is a dummy for change in market access between 1940 and 1967 being less than 0. All columns control for the 1950–1960 tract change in black population share interacted with year fixed effects, and the 1950–1960 log change in population interacted with year fixed effects. Robust standard errors are clustered at the city level. ***—$p < 0.01$, **—$p < 0.05$, *—$p < 0.1$.

Appendix A Underwriting profits for state FAIR plans

A 1978 report (US General Accounting Office 1978) documented that state FAIR plans generally suffered losses. Table A.1 reproduces these tabulations of the total number of policies and the total underwriting profit or loss through September 1977. As of September 1977, only five of 27 plans had earned a profit since their launch.

Table A.1: Underwriting profit/loss for state FAIR plans through September 1977

State	Policies	Profit	Profit per policy
CT	87,323	-15,174,000	-173.77
MA	329,008	-52,419,000	-159.32
OR	3,990	-565,000	-141.60
MN	19,352	-2,479,000	-128.10
RI	57,927	-7,270,000	-125.50
IL	405,929	-41,638,000	-102.57
NJ	366,545	-34,684,000	-94.62
OH	163,012	-12,615,000	-77.39
MI	820,269	-60,498,000	-73.75
NC	64,159	-4,425,000	-68.97
KY	68,594	-4,229,000	-61.65
NY	1,187,962	-68,537,000	-57.69
IA	11,963	-586,000	-48.98
MO	258,853	-12,315,000	-47.58
WI	71,467	-2,555,000	-35.75
KS	36,282	-1,135,000	-31.28
PA	448926	-13,058,000	-29.09
WA	17,889	-446,000	-24.93
VA	121607	-2,831,000	-23.28
DE	39779	-911,000	-22.90
MD	349803	-4,642,000	-13.27
DC	136932	-6,210	-4.54
CA	655117	652,000	1.00
IN	21145	256,000	12.11
GA	25730	431,000	16.75
NM	2282	236,000	103.42
PR	1316	298,000	226.44

Reproduction of table in US General Accounting Office (1978).

Appendix B Tract data

Our neighborhood data starts with the consistent-boundary census tract panel developed by Lee and Lin (2018). This database reports characteristics of Census tracts from decennial Censuses 1940–1990. Because tract boundaries change over time, statistics are adjusted using areal weights to 2010 census tract geographies.

We use the geo-coded addresses from the city directories for property and casualty insurers and lawyers in 1940 and 1967 to compute changes in market access according to equation (1) for each tract. We also compute some alternative measures of changes in insurer access (see Table G.3).

We keep only census tracts that exist and have nonzero census housing tabulations in 1950. This results in a balanced panel of consistent-boundary census tracts, 1950–1990.

Appendix C State residual property insurance plans

Figure C.1 shows our 26 sample cities by state FAIR plan adoption year. States that adopted FAIR plans in 1970 or earlier are colored red. States that adopted FAIR plans 1986 or later or that never adopted FAIR plans are colored gray. Adoption dates from Demerjian et al. (2001).

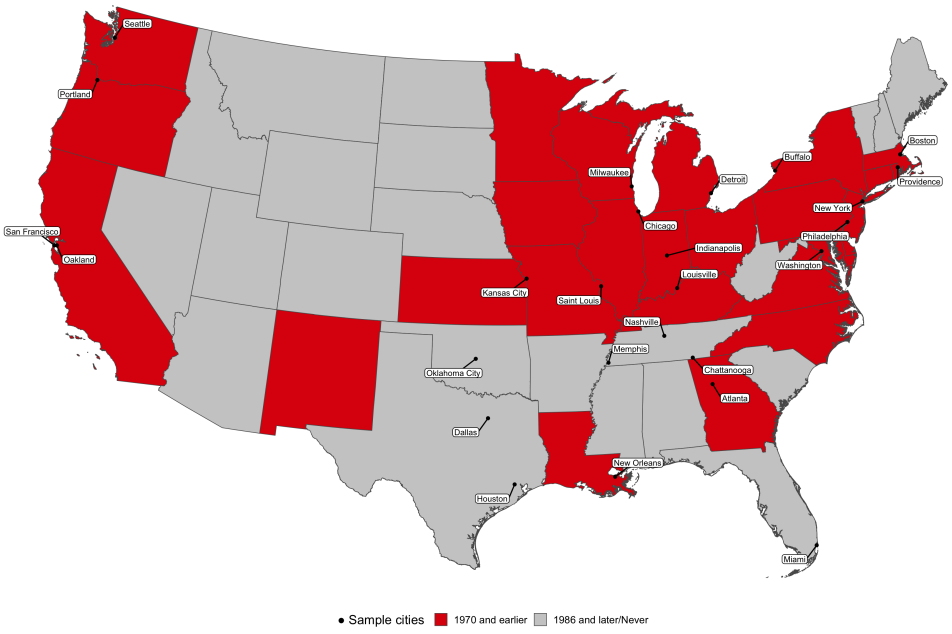
Figure C.2 shows year of earliest state residual property insurance plan offering, including both FAIR plans and earlier Urban Area Plans. Between 1960 and 1967, 13 states set up formal or informal residual property insurance programs (Hughes et al. 1968). These were typically patterned after the first one developed for Boston in 1960.

While the plans differed in details, key pieces of their overall structure were basically the same, and they influenced the development of the later FAIR plans. The success of these Urban Area Plans varied, too; the earliest and most successful appears to be the

Boston plan, which insured over 20,000 properties, primarily in the Roxbury neighborhood, over 1960–1967 (Hughes et al. 1968, p. 59).

First, many plans explicitly restricted the use of neighborhood factors from underwriting decisions. Under the Boston Plan, "no company writing fire insurance [...] should reject a risk solely because of the area in which it was located" (Hughes et al. 1968, p. 57). Few risks were rejected; between 1962 and August 1967, just seven percent of applications were declined (Hughes et al. 1968, p. 58).

Second, many plans placed restrictions on surcharges and rate adjustments, even when hazards were identified. Under the Boston plan, surcharges in the Roxbury neighborhood were limited to five to fifteen cents per hundred dollars of coverage (Hughes



This map shows our 26 sample cities by State FAIR plan adoption year. States that adopted FAIR plans in 1968–1970 are colored red. States that adopted FAIR plans 1986 or later or that never adopted FAIR plans are colored gray. Adoption dates from Demerjian et al. (2001).

Figure C.1: Sample cities by state FAIR plan adoption year

et al. 1968, p. 57). Other states, such as New York, Pennsylvania, and Delaware, patterned their Urban Area Plans after Boston, and had similar provisions.

Third, mandatory participation pools required companies to write policies and share risks among all insurers. For example, in Detroit, all property and liability insurance companies doing business in the state signed pledges to participate (Hughes et al. 1968, p. 61). Under the Watts plan in Los Angeles, 110 companies pooled premium income, expenses, and losses, even those that were unfamiliar with the area (Hughes et al. 1968, p. 75). Some plans required companies to participate in writing policies in high-risk areas as a condition of doing business in the state.

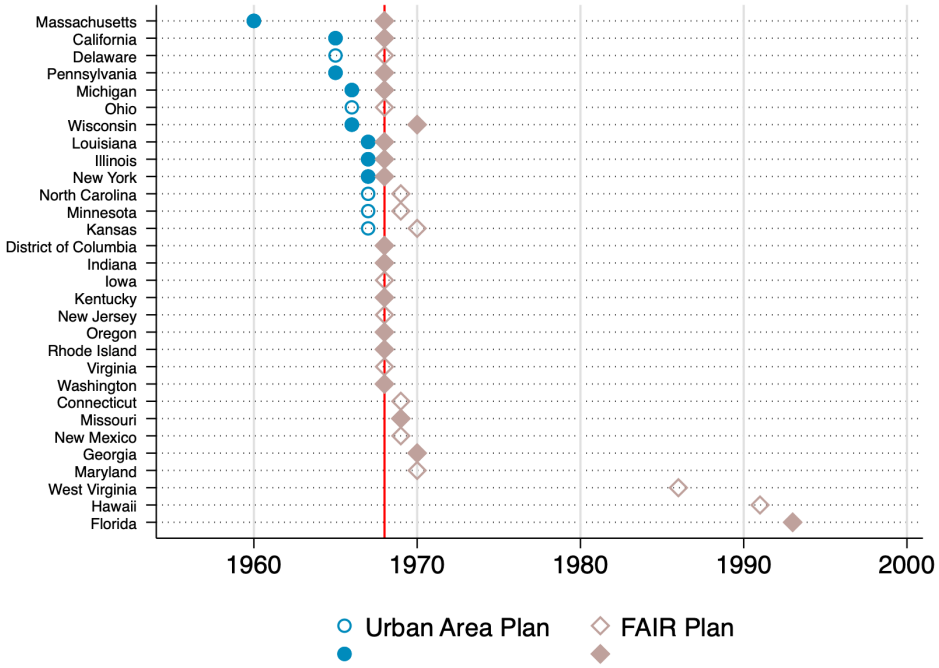
Fourth, free inspections were typically provided to property owners, paid for by the plan. Some plans (e.g., Michigan), allowed properties to bypass inspections.

Appendix D City directories

We sourced city directories from the Chicago Public Library, the New York Public Library, and internet resources. We focused on directories circa 1940 and 1967. In a small number of cases, we were unable to locate directories from those exact years, so we chose a directory from the nearest available year. See Table D.1 for the precise years by city that we used.

Circa 1940, these directories were titled *Telephone Directory* or *Classified Telephone Directory*. Circa 1960, these directories were titled *Telephone Directory* or the *Yellow Pages*. For both eras, these directories were published by the local Bell Operating Companies that were subsidiaries of AT&T. For example, Southern Bell Telephone and Telegraph Company published directories for their service area, which covered our sample cities of Atlanta, Chattanooga, Louisville, Nashville, and New Orleans.

Each directory contained classified listings, such as "Insurance" or "Attorneys." In a



This figure shows year of earliest state residual property insurance plan offering. Blue circles indicate Urban Area Plans and brown diamonds indicate FAIR Plans. The vertical red line indicates federal authorization of FAIR plans in 1968. Filled markers indicate states in our sample. West Virginia, Hawaii, and Florida are "Late FAIR states." Twenty-one additional states are in this category. Five states (Arkansas, Alabama, Mississippi, South Carolina, and Texas) only offered rural or beach FAIR plans, outside cities. Sixteen states never offered FAIR plans. Sources: Demerjian et al. (2001) and Hughes et al. (1968).

Figure C.2: Year of earliest state residual property insurance plan

small number of cases, the directories did not appear to have a separate classification for attorneys or lawyers. These are noted in Table D.1.

Figure D.1 shows an example page from the 1940 Chicago directory. Each listing contains name of establishment, address, and phone number. For example, the first listing is for A-B-C Insurance Agency, located at 224 South Michigan Avenue, phone number WAB-2934.

We selected Property and Casualty Insurers where they were listed separately. Other-

wise, we selected all insurers but dropped insurers with names containing "life" to avoid firms that primarily sold life insurance policies. We exclude large panel advertisements.

Then, we used ArcGIS to geo-code the addresses using a locator file built from ArcGIS 2012 StreetMap(TM) North America. We used human annotators to verify the geo-coder output and fill in missing values that the geo-coder was unable to locate. Based on these annotations, we found that missing geo-locations were usually building names without street addresses (e.g., "The Monadnock Building" versus 53 West Jackson Avenue). These named buildings tend to be concentrated in the central business district. Fortunately, there were usually many other establishments in the central business district that we were able to geo-code successfully. Our classification of treatment is based on market access. Thus, because missing addresses tend to be co-located with other addresses that we are able to successfully geo-code, these missing values should have minimal effects on our



Figure D.1: Example page from 1940 Chicago directory

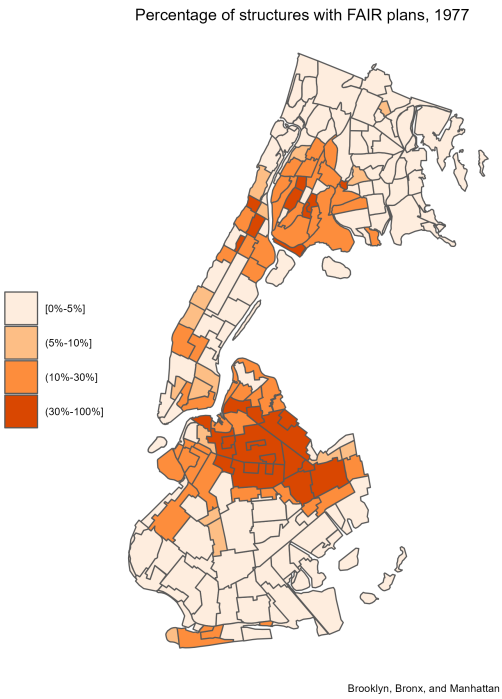
Table D.1: City directories: Geo-coding rates by city and year

City	Year 1	Insurers	Lawyers	Year 2	Insurers	Lawyers
Atlanta	1940	76%	47%	1967	58%	67%
Boston	1934	77	78	1966	35	35
Buffalo	1945	25	13	1967-1968	29	30
Chattanooga	1940	26	91	1967	49	33
Chicago	1940	97	85	1967	95	87
Dallas	1941	39	15	1961	45	28
Detroit	1940	21	9	1967	29	10
Houston	1940	74	n/a	1967	73	n/a
Indianapolis	1940	17	5	1967	77	63
Kansas City	1940	27	n/a	1967	52	n/a
Louisville	1940	20	n/a	1967	54	n/a
Memphis	1940	83	27	1967	73	48
Miami	1940	18	6	1967	73	36
Milwaukee	1940	93	77	1967	73	60
Nashville	1940	95	13	1967	62	35
New Orleans	1951	97	n/a	1967	87	n/a
New York (Bronx)	1957	57	73	1967	45	46
New York (Brooklyn)	1944	77	85	1967-1968	66	86
New York (Manhattan)	1940	89	86	1967	79	85
Oakland	1940	28	6	1967	89	5
Oklahoma City	1940	80	86	1967	58	80
Philadelphia	1938	65	25	1960	59	37
Portland, OR	1940	18	n/a	1967*	50	n/a
Providence	1942	78	n/a	1962	42	n/a
Saint Louis	1951	100	100	1967	100	100
San Francisco	1940	35	5	1967	73	57
Seattle	1941	6	4	1960	30	6
Washington	1941	95	n/a	1962-1963	56	n/a

This table shows source years for city directories and geocoding rates by city. Geocoding rates expressed as percentage points. *—For Portland, insurer data from the 1966–1967 directory and lawyer data from the 1967–1968 directory. "n/a" indicates we were unable to locate directories in both years separately classifying lawyers or attorneys.

classification and our results. See Figures 1 and E.1 for further validation of our directory data.

Appendix E Validation using 1977 FIA sample



This map displays data from US Congress (1978). Spatial units are as defined as in the original source.

Figure E.1: Share of structures covered by FAIR plans in 1977 by neighborhood.

Appendix F Summary statistics and balance tests

Table F.1 presents tabulations of sample census tracts by comparison group. In our sample, 16% of sample tracts are classified as treated (reduced access in early-FAIR states), and 65% as control in early-FAIR states. In late-FAIR states, reduced-access and stable-access tracts represent 4% and 15%, respectively.

Table F.2 presents tests of the differences in the pre-trends of 11 variables for 1950–1960, before and after controlling for pre-existing (1950–1960) trends in the Black popu-

Table F.1: Treatment and comparison groups

Treatment/Control groups	Sample Size	Percent
Reduced access, early FAIR	965	16.15
Stable access, early FAIR	3,899	65.24
Reduced access, late FAIR	240	4.02
Stable access, late FAIR	872	14.59
Total	5,976	100

This table presents the distribution of the sample size across the four treatment and comparison groups. Each unit is a census tract with 2010 geometries. We use a balanced panel of census tracts from 1950 to 1990. The statistics refer to one year.

lation share and log population.[13] These estimates are from an event-study specification corresponding to equation 2. We report coefficient estimates on the triple interaction between reduced/stable access, state FAIR status, and the indicator for 1950 (column 1). The base year is 1960, so a positive coefficient indicates that the outcome fell more quickly in the double difference (reduced-access neighborhoods vs. stable-access neighborhoods in early-FAIR states vs. late-FAIR states). For example, reduced-access tracts experienced slower (but statistically insignificant) growth in pre-war housing units compared with stable-access tracts, in early-FAIR versus late-FAIR states, by about 22 housing units. Column 2 reports robust standard errors clustered at the city level.

We can reject for seven outcomes that pre-trends are equal at the 5% level. The estimates in Column 1 also show economically significant differences in pre-trends.

For this reason, we adopt a control variable approach. Column 3 reveals that controlling for Black population share and log population changes greatly reduces the economic significance of the differences in trends of the remaining 1950–1960 variables that were not controlled for. (The top two rows are mechanically zero.) Furthermore, nearly all

13. We focus on rent rather than home values as the median share of units reporting home values is only about 9% in 1960 in the 965 census tracts in our reduced insurance access early FAIR state sample, reflecting low owner-occupancy rates in these areas.

Table F.2: Balance of pre-trends

	(1)	(2)	(3)	(4)
			Controlled	
	Coef.	std. err.	Coef..	std. err.
1960–1950 Changes in:				
Black Population Share	-0.0616***	(0.0185)	0	0
Population (log)	0.117***	(0.0386)	0	0
White Population (log)	0.266***	(0.0881)	-0.0106	(0.0111)
Black Population (log)	0.177**	(0.0699)	-0.0229	(0.0194)
Nonwhite Population (log)	0.185**	(0.0665)	-0.00231	(0.0203)
Pre-war Housing Units	22.11	(15.42)	13.52	(17.76)
Years of Education	0.0899**	(0.0365)	0.0395	(0.0559)
Contract Rent (log)	-0.0529*	(0.0271)	-0.0519*	(0.0287)
Income (log)	0.0333	(0.0238)	0.0204	(0.0258)
Total Housing Units (log)	0.0659***	(0.0169)	-0.0222	(0.0251)
College Share (25+)	0.00456	(0.00292)	0.00342	(0.00339)

This table displays balance tests of pre-trends of our outcome variables 1960–1950 from an event-study specification corresponding to equation 2. Column 1 reports differences in differences with 1960 as the base year. Column 2 reports robust standard errors clustered at the city level. Column 3 reports the same tests as Column 1 but on the residuals from regressing changes in the outcome variable on 1950–1960 changes in the first two variables (Black population share and population). Columns 2 and Column 4 report the standard errors of the coefficients. ***—$p < 0.01$, **—$p < 0.05$, *—$p < 0.1$.

of these differences are no longer statistically significant at the 5% level. Contract rents are the only exception, and the sign of the estimate indicates that reduced-access tracts saw conditionally *faster* rent growth in the 1950s. Thus, our post-FAIR results represent a reversal of fortune.

Appendix G Additional results and robustness

Appendix G.1 Event study estimates

In Figure G.1, we show event study estimates. The vertical axis indicates the coefficient estimate of the triple interaction term—β_1 in equation 2—interacted with year dummies.

Figure G.1a reports the estimated coefficients without additional controls (corresponding to Column 1 in Table 1), while Figure G.1b (reproducing Figure 2b from the main text) reports the coefficients with neighborhood characteristics×time fixed effects as controls (corresponding to Column 3 in Table 1).

We also report estimates for a somewhat smaller sample of cities with available tract data starting in 1940. This balanced panel contains 5,291 tracts in 24 cities. Figure G.1c is the same as Figure G.1a except using a balanced panel from 1940 to 1990. Figure G.1d is the same as Figure G.1b except using a balanced panel from 1940 to 1990. In all four panels, the estimated effect is small and close to zero before 1970. In the period immediately after FAIR authorization, the negative effect on pre-war housing stock emerged and became even more negative during the 1970s and the 1980s.

Appendix G.2 Results by structure type and tenure

Table G.1 shows estimates of equation 2 for total housing units, by tenure (owner versus renter-occupied housing units), and by structure type (housing units in single-unit structures versus multi-unit structures.

Appendix G.3 Robustness checks

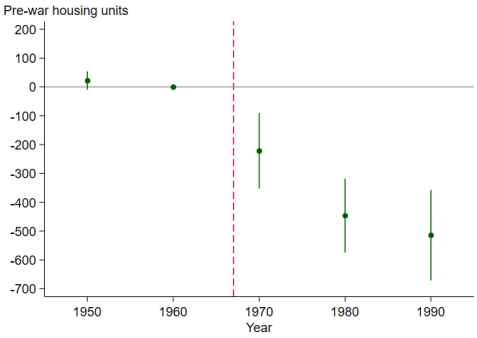
Remaining exhibits in this section show results described in the main text.

Table G.1: FAIR effects by structure type and tenure

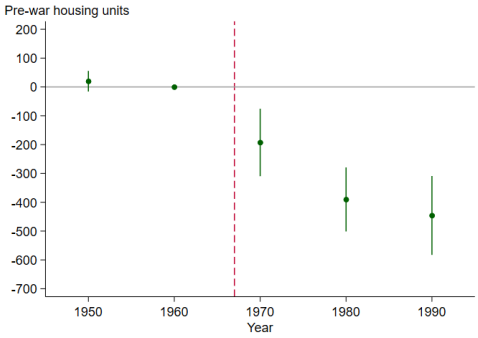
Outcome variable: Treatment definition:	(1) Housing units (HUs)	(2) Owner-occupied HUs	(3) Renter-occupied HUs	(4) Single-family HUs	(5) Multi-family HUs
1(Reduced Access)*1(early FAIR)	-439.6***	-15.2	-426.1***	34.4	-474.9***
*1(>=1970)	(125.3)	(52.6)	(83.4)	(62.5)	(78.8)
1(early FAIR)*1(>=1970)	-199.6	-39.4	-158.9**	-46.0	-152.9**
	(121.3)	(57.0)	(72.3)	(70.6)	(65.3)
1(Reduced Access)*1(>=1970)	74.0	-51.0	125.3*	-60.4	135.5*
	(110.7)	(48.0)	(69.5)	(51.4)	(67.1)
1(Reduced Access)*1(early FAIR)	186.7	-81.4	268.1*	-172.7	359.5**
	(237.5)	(123.9)	(155.1)	(166.1)	(143.3)
1(Reduced Access)	46.4	-48.0	94.4	5.0	41.0
	(208.5)	(101.6)	(107.8)	(134.3)	(75.9)
1(early FAIR)	379.0**	58.0	321.0**	-131.2	509.9***
	(174.7)	(123.6)	(141.2)	(154.4)	(149.0)
1(>=1970)	693.6***	455.2**	237.7	521.9**	171.2
	(240.7)	(190.2)	(178.4)	(234.7)	(208.6)
Tract Fixed Effects	NO	NO	NO	NO	NO
Tract Changes*Year Fixed Effects	YES	YES	YES	YES	YES
Observations	29,777	29,752	29,751	29,784	29,775
R-squared	0.102	0.067	0.105	0.081	0.117

This table reports the estimation results of specification 2. Each observation is a census-tract-year. $1(ReducedAccess)$ is a dummy for change in market access between 1940 and 1967 being less than 0. All columns control for the 1950-1960 change in the black population share interacted with year-fixed effects, and the 1950-1960 log change in population interacted with year-fixed effects. Robust standard errors are clustered at the city level. ***—$p < 0.01$, **—$p < 0.05$, *—$p < 0.1$.

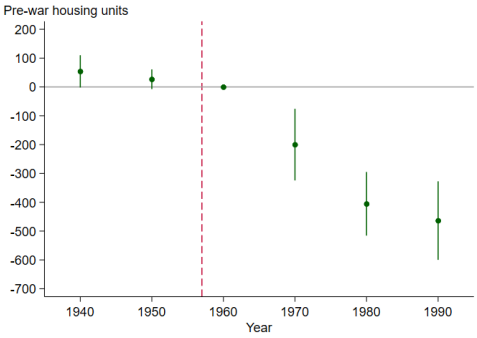
(a) No controls



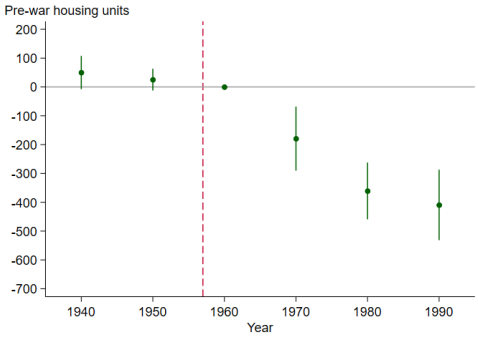
(b) Regression adjustment



(c) No controls, 1940 balanced panel

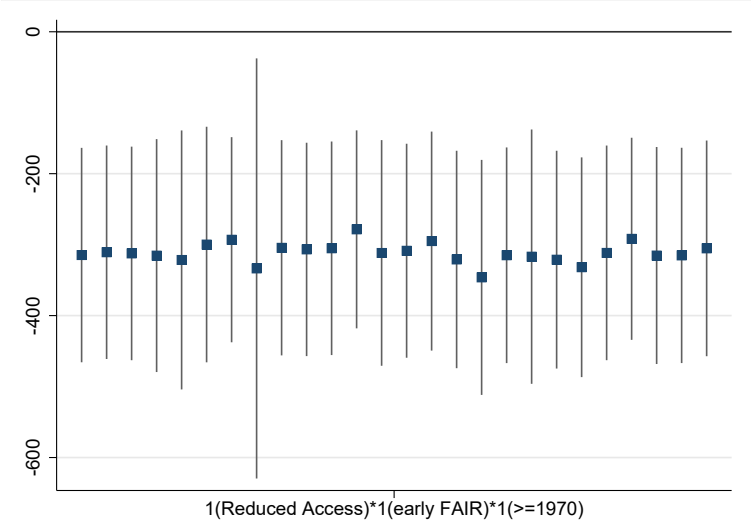


(d) Regression adjustment, 1940 balanced panel



The vertical axis indicates the coefficient of the triple interaction term (β_1 in equation 2) interacted with year dummies. Figure G.1a reports the estimated coefficients without additional controls (corresponding to Column 1 in Table 1), while Figure G.1b reports the coefficients with neighborhood characteristics*time fixed effects as controls (corresponding to Column 3 in Table 1). Figure G.1c is the same as Figure G.1a except using a balanced panel from 1940 to 1990. Figure G.1d is the same as Figure G.1b except using a balanced panel from 1940 to 1990.

Figure G.1: Event study



This figure shows the estimated coefficients on the triple-interaction term of Column 3 in Table 1 using samples leaving one city out at a time. The vertical axis measures the coefficient estimate.

Figure G.2: Sensitivity to influential cities

Table G.2: Continuous effects of 1940–1967 changes in access to private insurers

	(1)	(2)	(3)
Outcome variable:	Housing units built 1940 or earlier		
1(early FAIR)*1(>=1970)*chg MA	21.3***	-5.3*	18.8**
	(7.4)	(2.7)	(6.9)
1(early FAIR)*1(>=1970)*chg MA*1(chg MA<0)	-24.0***	5.8	-20.7***
	(7.4)	(5.3)	(7.0)
1(early FAIR)*1(>=1970)	-111.7*	-106.2*	4.0
	(62.8)	(61.5)	(64.5)
1(early FAIR)*1(>=1970)*1(chg MA<0)	-417.8***	-374.4***	-342.7***
	(70.6)	(71.2)	(72.8)
1(Reduced Access)*1(>=1970)	-1.2	-2.0	-7.6
	(46.2)	(45.3)	(57.9)
1(Reduced Access)*1(early FAIR)	412.0**		295.2*
	(154.5)		(161.8)
1(Reduced Access)	-2.5		9.0
	(90.7)		(117.8)
1(early FAIR)	692.8***		473.5***
	(121.6)		(122.8)
1(>=1970)	-225.8***	-225.0***	-443.9*
	(59.5)	(58.7)	(221.9)
Tract Fixed Effects	NO	YES	NO
Tract Changes*Year Fixed Effects	NO	NO	YES
Observations	29,726	29,726	29,726
R-squared	0.174	0.843	0.254

This table reports the estimation results of a variant of specification 2 by introducing asymmetric continuous effects of changes in access to private insurers. Each observation is a census-tract-year. 1(Reduced Access) is a dummy for change in market access between 1940 and 1967 being less than 0. ΔMA is a continuous measure of changes in market access between 1940 and 1967, which is demeaned in both the $(-\infty, 0)$ and $(0, +\infty)$ ranges. Column 1 does not include any control variables and corresponds exactly to specification (1). Column 2 includes tract fixed effects. Column 3 controls for the 1950-1960 change in the black population share interacted with year-fixed effects, and the 1950-1960 log change in population interacted with year-fixed effects. Robust standard errors are clustered at the city level. ***—$p < 0.01$, **—$p < 0.05$, *—$p < 0.1$.

Table G.3: Robustness to different treatment definitions

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome variable:	Housing units built 1940 or earlier					
Treatment definition:	change in market access decay parameter=8			change in distance to nearest 5 insurers		
1(Reduced Access)*1(early FAIR)	-240.8***	-242.2***	-216.3***	-474.1***	-473.1***	-385.2***
*1(>=1970)	(73.3)	(72.7)	(68.1)	(63.4)	(63.1)	(53.2)
1(early FAIR)*1(>=1970)	-123.8*	-127.2**	-6.3	-65.0	-69.3	35.2
	(60.4)	(59.9)	(59.3)	(42.0)	(40.8)	(44.4)
1(Reduced Access)*1(>=1970)	-67.3*	-68.0*	-36.7	125.3***	124.1***	104.7***
	(38.0)	(37.1)	(43.4)	(41.9)	(40.5)	(36.5)
1(Reduced Access)*1(early FAIR)	235.5		201.9	658.4***		507.9***
	(142.8)		(142.4)	(120.3)		(107.5)
1(Reduced Access)	116.3**		58.1	-240.4***		-206.6***
	(53.2)		(85.4)	(59.6)		(60.0)
1(early FAIR)	717.0***		483.2***	603.8***		399.0***
	(119.1)		(116.7)	(81.5)		(84.4)
1(>=1970)	-214.3***	-213.7***	-455.3**	-272.0***	-270.8***	-517.6**
	(57.5)	(56.7)	(214.6)	(37.5)	(36.0)	(213.9)
Tract Fixed Effects	NO	YES	NO	NO	YES	NO
Tract Changes*Year Fixed Effects	NO	NO	YES	NO	NO	YES
Observations	29,726	29,726	29,726	29,726	29,726	29,726
R-squared	0.164	0.841	0.247	0.170	0.842	0.250

This table tests the robustness of the baseline results (Columns 1-3 in Table 1) to different treatment definitions. As mentioned in Section 3, we calculated market access for each tract as distance weighted average number of property and casualty insurers with distance decay parameter $\gamma = 4$. From Columns 1–3 in this table, we calculate market access by setting $\gamma = 8$. As an alternative, in Columns 4–6, we calculate the average distance to the nearest five insurers in 1940 and 1967 for each tract and then define the dummy "reduced access" as being equal to 1 if the 1940-1967 change in this distance is greater than 0.

Table G.4: Robustness to dropping low geo-coding rate cities

	(1)	(2)	(3)
Outcome variable:	Housing units built 1940 or earlier		
1(Reduced Access)*1(early FAIR)	-395.3***	-396.2***	-326.8***
*1(>=1970)	(79.1)	(78.6)	(79.7)
1(early FAIR)*1(>=1970)	-125.1*	-129.2*	-1.7
	(65.9)	(65.1)	(72.1)
1(Reduced Access)*1(>=1970)	-5.9	-6.7	-8.3
	(53.5)	(52.5)	(62.3)
1(Reduced Access)*1(early FAIR)	393.3**		283.1
	(167.6)		(171.4)
1(Reduced Access)	9.2		11.2
	(102.3)		(124.0)
1(early FAIR)	744.1***		499.9***
	(126.0)		(138.2)
1(>=1970)	-216.7***	-215.9***	-487.0**
	(63.9)	(63.0)	(227.5)
Tract Fixed Effects	NO	YES	NO
Tract Changes*Year Fixed Effects	NO	NO	YES
Observations	27,152	27,152	27,152
R-squared	0.173	0.843	0.245

This table tests the robustness of the baseline results (Columns 1–3 in Table 1) to dropping 4 cities with low-geo-coding rates: Seattle, Miami, Indianapolis, and Portland (see Table D.1). Column 1 does not include any control variables and corresponds exactly to equation 2. Column 2 includes tract fixed effects. Column 3 controls for the 1950-1960 change in the black population share interacted with year-fixed effects, and the 1950-1960 log change in population interacted with year-fixed effects. Robust standard errors are clustered at the city level. ***—$p < 0.01$, **—$p < 0.05$, *—$p < 0.1$.

Appendix H Building fires

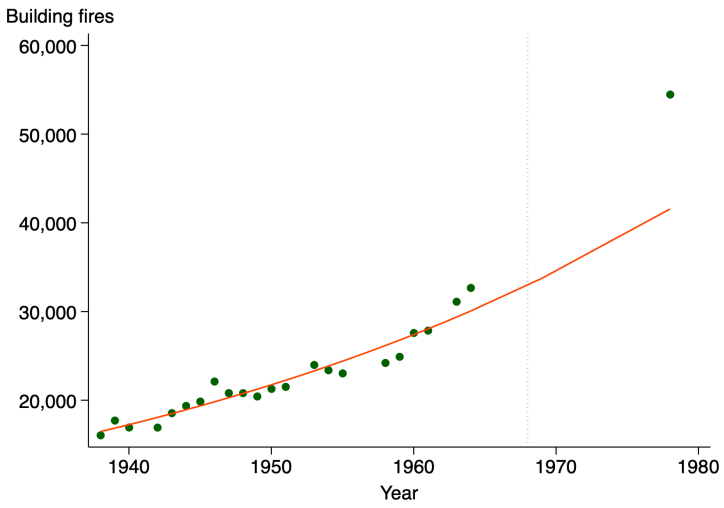
Our data on building fires come from National Fire Protection Association (NFPA) reports (1938-1969) (n.a. 1939), a 1978 US Department of Justice (DOJ) survey (Webster and Matthews Jr. 1979), and the National Fire Incident Reporting System (NFIRS, 1980–1988). These sources are survey or administrative data reported by local fire departments, ensuring methodological consistency. We manually annotated records from selected cities and years in both the NFPA and DOJ datasets.

The NFPA series, which ends just as the first FAIR plans were adopted, has some limitations due to missing data for certain cities or years. The combined dataset provides city-level statistics without distinguishing between residential and nonresidential fires or detailing the extent of damage. We also cannot observe the value of damage or subsequent repairs. Our panel is mostly balanced through 1978, with about 41 city observations per year. Rollout of NFIRS reporting was slower, with about 22 city observations per year. Despite these constraints, the data offer suggestive evidence of a substantial increase in building fires in cities located in states with FAIR plans.

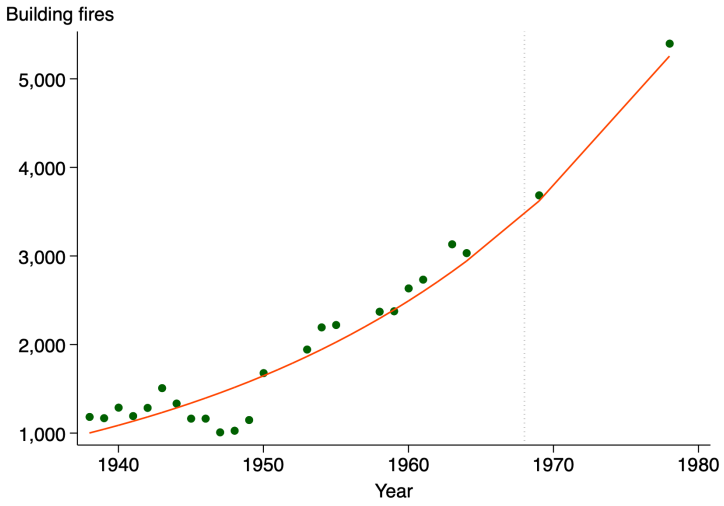
Figure H.3 illustrates the evolution of building fires in New York City (an early FAIR plan adopter) and Memphis (non-adopter), using data from NFPA and 1978 DOJ surveys. Both cities show an upward trend in building fires through 1964, likely due to aging housing stock and deteriorating urban demand conditions. An exponential trend fitted to 1938–1964 is shown, which fits the data well. A vertical line denotes the 1968 Federal authorization of FAIR plans and New York State's adoption. (New York adopted an Urban Area Plan in 1967.)

New York City experienced approximately 13,000 "excess" fires in 1978 compared to the pre-FAIR trend, while Memphis showed little deviation from its 1938–1964 trend. Despite other differences between the cities, they experienced similar civil unrest severity

(a) New York City



(b) Memphis



These figures show annual building fires for New York City and Memphis reported in publications of the National Fire Protection Association (NFPA) 1938–1969 and Webster and Matthews Jr. (1979) in 1978. In some years, the original sources did not report data due to nonresponse. Each city's 1938–1964 exponential trend is shown as a red line. A vertical dotted line denotes Federal authorization of FAIR plans and adoption of the New York State FAIR plan in 1968.

Figure H.3: Building fires in New York City and Memphis, 1938–1978

in the 1960s (Carter 2020), suggesting that FAIR plans may have had comparable effects if private insurers reacted similarly to riots in both cities.

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