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FEDERAL RESERVE BANK of CHICAGO

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

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Abstract

In response to private insurers' postwar withdrawal from urban neighborhoods, roughly half of US states developed programs in the late 1960s that offered residual property insurance to property owners denied in the private market. These plans, known as Fair Access to Insurance Requirements (FAIR) plans after 1968, inadvertently encouraged moral hazard through underwriting restrictions, risk pooling, and generous payouts. We use a triple-difference design to estimate FAIR's impact, comparing: (1) pre- and post-FAIR participation periods, (2) neighborhoods likely offered FAIR plans versus those not, and (3) similar contrasts in non-participating states. FAIR plans led to significant housing disinvestment and declines in central neighborhood population and income in the late 1960s and 1970s.

JEL Classification: G52, N92, R31

Keywords: Arson, Housing disinvestment, Moral hazard, Neighborhoods, Property insurance

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1 Introduction

Insurance is essential for well-functioning property 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). This paper examines 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. FAIR plans were modeled after earlier state residual property insurance programs, including the influential Boston plan of 1960 and plans in twelve additional states developed and implemented between 1965 and 1967. To incentivize state participation in FAIR, Congress offered riot re-insurance to participating insurers (Maidenberg 1967). This incentive was partially effective: 17 states and Washington, D.C. offered FAIR plans in 1968, increasing to 26 states and D.C. by 1970 (Ansfield 2021). Take-up was swift, with over 300,000 FAIR Plan policies issued in 1969, rising to 5.7 million by 1977 (United States General Accounting Office 1978; Welsh 1972).

However, FAIR plans came with restrictions that may have encouraged moral hazard. Federal guidelines prohibited considering "environmental hazards" beyond property owners' control, such as crime or fire risks (United States Congress 1968). Nearly all FAIR states required all fire and property insurers to participate, potentially reducing prudent underwriting incentives due to loss pooling (United States General Accounting Office 1978; Works 1977). Also, many FAIR plans offered payouts that exceeded actual market value (Dwyer 1978).

While FAIR plans helped many central city property owners secure insurance, concerns emerged about incentivizing housing disinvestment and "arson-for-profit." As central city neighborhoods experienced declining demand and falling rents in the 1960s, generous payouts and low opportunity costs of abandonment made passive disinvestment or even arson attractive to landlords. Some blamed FAIR plans for the 1970s urban arson wave, though a government report found no conclusive evidence (United States General Accounting Office 1978). Others attributed the rise in arson to broader government policy and disinvestment in fire protection (Flood 2010).

We revisit this controversy, estimating FAIR plans' effects on housing and neighbor-

hoods using a triple-difference design. We compare outcomes before and after state FAIR plan adoption (1968), contrasting neighborhoods likely offered FAIR plans with similar neighborhoods, and comparing early-FAIR states to late-FAIR states that did *not* set up residual plans. To identify neighborhoods where FAIR plans were likely offered, we digitized city directories from 1940 and 1967 for 26 large U.S. cities, documenting private property insurance establishments' exit from central city neighborhoods. Using these data, we measure reduced-access versus stable-access neighborhoods symmetrically across early- and late-FAIR states. Reduced-access neighborhoods are likely those where property owners were eligible for FAIR plans. Thus, we identify intent to treat (ITT) effects. We validate our measure using a 1977 survey of FAIR plans in New York City. 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. We provide evidence supporting this assumption: Neighborhood contrasts before FAIR plan availability evolved similarly in states with and without FAIR plans over the 1940s and 1950s.

Our results show that FAIR plan adoption and offering 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 241 pre-war housing units per census tract, or about 23% of the 1950 stock. These declines are consistent with evidence on building fires and are concentrated in the multi-family and rental sectors, consistent with FAIR plans' lowering opportunity costs of abandonment for landlords. We also find that FAIR-induced housing disinvestment led to neighborhood declines in population and income and increases in the Black population share. These neighborhood effects incorporate both direct effects on property owners' passive disinvestment and arson choices, by increasing the value of abandonment. They also include indirect effects that operate through spillovers to neighboring property owners.

Our study contributes to prior research focusing on descriptive features of FAIR plans (Ansfield 2021; Dwyer 1978; Squires et al. 1979; Works 1977). Compared with previous work, our contribution is to estimate the causal effect of FAIR plans on housing and neighborhoods.

We also contribute to a literature examining central city decline in the middle 20th century (Brooks et al. 2024; Collins and Margo 2007). Compared with previous work, our results highlight the role of historical insurance policy. Another strand of this literature examines housing disinvestment (Feins 1977; Gyourko and Saiz 2004; Hillier et al. 2003;

Raleigh and Galster 2015; Sternlieb et al. 1974). Some prior 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 tax shocks in housing abandonment. Increases in property taxes reduce landlords' current and future net cash flows, reducing the opportunity cost of abandonment. In our setting, the generosity of FAIR plan payouts instead increased the benefits of housing abandonment.

Finally, our analysis adds to the understanding of the market failures that plague insurance markets. Prior work on moral hazard in health insurance has studied whether the availability and generosity of 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). Empirical studies of insurance market failures are generally challenged by the difficulty of disentangling selection from moral hazard; an exception is Weisburd (2015) who estimates in Israel that each $100 in auto insurance coverage leads to 1.7 percent more accidents. Compared with prior work, we offer evidence on how insurance contracts can distort behavior in property versus other markets.

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, particularly following the 1967 urban unrest (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 explicit redlining as a root cause, citing an insurance agent who described "knock-out areas" or "redline districts" where companies refused to

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

Seventeen states and the District of Columbia adopted FAIR plans in 1968, with nine more following 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 (United States General Accounting Office 1978).

Take-up was accelerated in part because between 1960 and 1967, thirteen states had created residual property insurance programs called Urban Area Plans, which later became the blueprint for FAIR plans (Hughes et al. 1968).[2] The various Urban Area Plans differed in their details, but many shared several key features: they 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 officially 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. All of the states that developed Urban Area Plans are also early FAIR states.

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 incentivizing "arson-for-profit" (United States General Accounting Office 1978). Critics identified three main issues stemming from commmon FAIR plan provisions that were required by the federal government.

1. Limited underwriting flexibility: FAIR plans were prohibited from denying insur-

[1] Ironically, reduced urban unrest and the 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.

ance based on neighborhood conditions, leading to high acceptance rates and potential vulnerability to arson (United States General Accounting Office 1978). Many FAIR plan officials reported feeling constrained in their ability to deny coverage, including to properties near abandoned buildings or to 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 even required insurers to pay the face value of the policy, regardless of market value (United States General Accounting Office 1978). In one-third of states, owners could request coverage that was beyond the market value of the property (United States General Accounting Office 1978).

In postwar 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.

Contemporary controversy and evidence. While the GAO found no conclusive evidence that FAIR plans encouraged arson more than other insurance plans, they documented that most FAIR plans suffered losses. As of September 1977, only five of 27 plans had earned an underwriting profit since their launch (United States General Accounting Office 1978). Demerjian et al. (2001) estimated aggregate statutory underwriting losses for all FAIR plans 1970–1998 totaled $1.5 billion.

Contemporary observers, including a 1976 report for the National Fire Prevention and Control Administration (Fisher et al. 1976) and a 1979 Senate report (United States Congress, Senate, Commitee on Government Affairs, Permanent Subcommittee on Investigations 1979), blamed FAIR plans for increasing arson rates. Media investigations linked arson to insurance fraud facilitated by FAIR plans' under-selectivity and excessive

[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 (United States General Accounting Office 1978).

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).

While 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 (United States General Accounting Office 1978). In Illinois, the Metropolitan Chicago Loss Bureau reported that in 1977, 33% of FAIR plan fire claims were arson, totalling $7.7 million. In Massachusetts, one FAIR plan official estimated that 40 percent of all arsons in the State were FAIR plan-related. In Pennsylvania, FAIR plan losses from arson or suspicious fires totalled $1.8 million in 1976 and 1977, and officials noted involvement of organized crime.

Evidence from building fires. We present new evidence suggesting a possible association between FAIR plans and arson, using building fire statistics. These statistics encompass active disinvestment (e.g., arson), passive disinvestment (e.g., neglected maintenance), and accidental causes. Analyzing building fires, rather than specifically classified arson cases, circumvents issues related to proving arson or variations in detection rates across jurisdictions.

Our data come from two sources: National Fire Protection Association (NFPA) reports (1938-1969) (n.a. 1939) and a 1978 U.S. Department of Justice (DOJ) survey (Webster and Matthews Jr. 1979). Both sources collected data from fire departments, ensuring methodological consistency. We manually annotated records from selected cities and years in

[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).

both 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. Despite these constraints, the data offer suggestive evidence of a substantial increase in building fires in cities located in states with FAIR plans.

Figure 1 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 in the 1960s (Carter 2020), suggesting that FAIR plans may have had comparable effects if private insurers reacted similarly to riots in both cities.

To expand this analysis, we examine building fires in 42 cities (with population $\geq$ 250,000 in 1964) across 7 years: 1942, 1948, 1953, 1959, 1964, 1969, and 1978. We estimate the following regression:

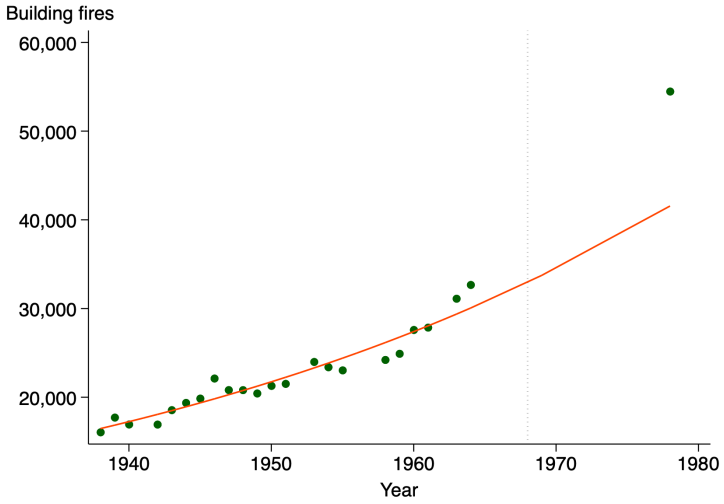
$$\log \text{fires}_{ct} = \delta_c + \delta_t + \alpha_c t + \beta 1(t = 1978) \times 1(\text{early FAIR}) + \epsilon_{ct}$$

where δ_c and δ_t are city and year fixed effects, α_c is a city-specific trend, and β is the coefficient of interest on the interaction between early FAIR plan cities and the 1978, post-FAIR, observation.

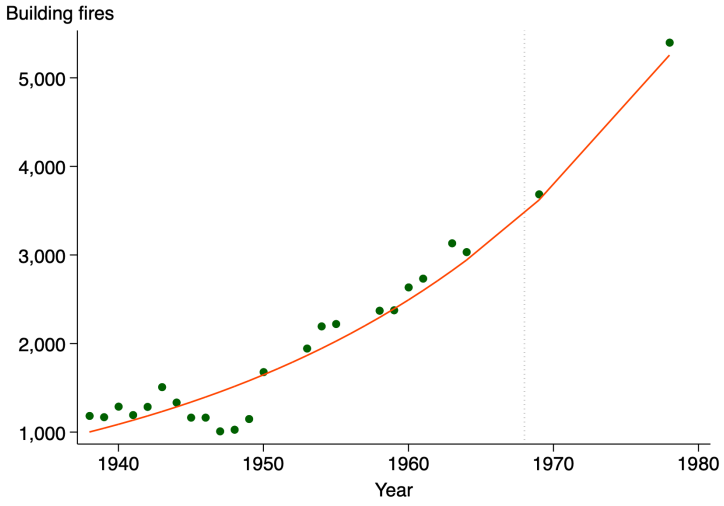
We estimate $\hat{\beta} = 0.278$ (robust s.e. $= 0.115$), indicating that cities in early FAIR plan states experienced 32% more building fires in 1978 compared to non-FAIR cities, conditioned on fixed effects and city-specific trends. This result provides preliminary evidence supporting contemporary commentary that early FAIR plans may have contributed to arson and building disinvestment.

Notably, early FAIR cities showed slightly *slower* fire growth trajectories prior to 1968 (early FAIR average $\hat{\alpha}_c = -0.044$, robust s.e. $= 0.013$), suggesting that the post-FAIR

(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 1: Building fires in New York City and Memphis, 1938–1978

acceleration in building fires represents a reversal from pre-FAIR trends.

3 Data and Methods

Measurement. We use a balanced panel of consistent-boundary census tracts from 1950 through 1990 in 26 major U.S. 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 availability of city 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 the Black population share. Our sample includes approximately 6,000 census tracts over five decades (1950–1990), totalling roughly 30,000 tract–year observations.

To identify neighborhoods likely offered FAIR plans, we digitized city directories for 1940 and 1967 in 26 large U.S. cities.[5] This approach allows us to observe private insurer withdrawal in both early- and late-FAIR states, treating them symmetrically and identifying intent-to-treat (ITT) effects.

We calculate insurer market access M for each tract i in city c and year $t \in \{1940, 1967\}$ as

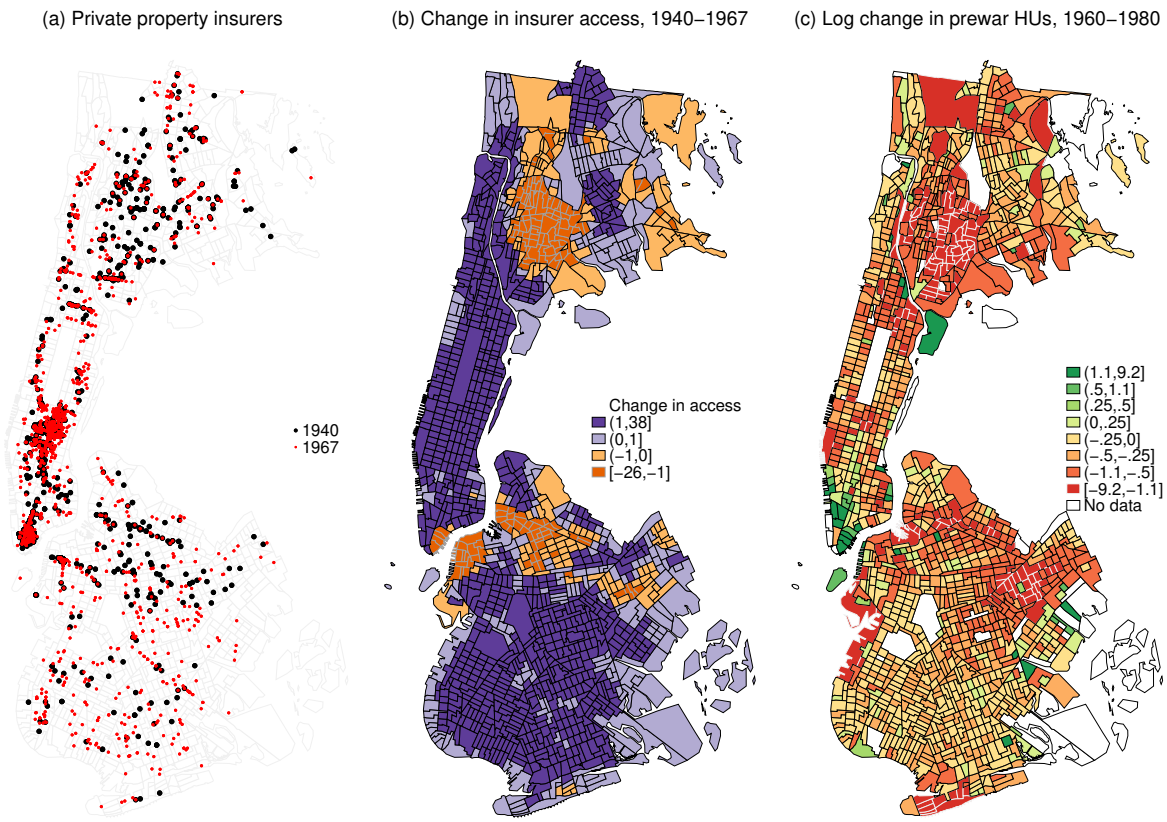
$$M_{ict} = (1/G_{ct}) \sum_{j=1}^{J_{ct}} e^{-\gamma d_{ij}} \tag{1}$$

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 reciprocal of 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 three quarters of a mile. (Our results are robust to alternative measures; see Appendix Table G.3.)

We define a binary variable I_{ics} indicating decreased market access to property and casualty insurers from 1940 to 1967 ($I_{ics} = 1[M_{ic1967} < M_{ic1940}]$). This defines treatment

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

[6]The implicit assumption in this scaling is that the non-geocoded establishments would have the same spatial distribution as the geo-coded addresses.



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

Figure 2: 1940–1967 changes in access to private property insurers

and control groups by change in access and FAIR state adoption status. In our tract sample, approximately 16% 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 account for 4% and 15%, respectively, of our sample (see Table F.1).

Example and validation. Figure 2 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 considerable variation in treatment status I_{ics} across neighborhoods. After World War II, private insurers withdrew from many New York City neighborhoods, particularly the South Bronx and

Bedford-Stuyvesant in 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 prewar housing units, as a measure of disinvestment. Notably, areas experiencing insurer withdrawal, especially in the South Bronx and Bedford-Stuyvesant, show significant housing unit losses.[7]

While comprehensive FAIR plan data is scarce, partial information is available from tabulations presented in hearings before the U.S. Senate in 1978. In 1977, the Federal Insurance Administration (FIA) sampled New York FAIR plan policies (Nwokolo 2023; United States Congress, Senate, Committee on the Judiciary, Subcommitee on Citizens and Shareholders Rights and Remedies 1978), tabulating coverage by neighborhood. Despite representing a single time point 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 2b.

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 late-FAIR states that did not adopt or 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 the *did* offer FAIR plans. The main threat to identification is unobserved neighborhood factors in reduced-access versus stable-access

[7]Green shades indicate growth in prewar housing units, possibly due to conversions of commercial/industrial buildings or unit subdivisions.

[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 that diverge in early-FAIR states but don't diverge in late-FAIR states. Importantly, it seems plausible that state-level FAIR adoption decisions did not depend on within-city contrasts in declines in access to private insurers. We provide evidence supporting this assumption in Appendix F. Conditioned on controls, these neighborhood contrasts evolved similarly over the pre-treatment period 1950–1960, and for a subsample during 1940–1950. We discuss control variables, placebo tests, and robustness checks below.

Our simplest specification is

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

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 decreased from 1940 to 1967 ($I_{ics} = 1[M_{ic1967} < M_{ic1940}]$), $Post_t$ is a binary variable indicating a Census 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 each of four tract characteristics with Census year indicators. The four characteristics are: (i) distance to city center (entered as within-metro decile indicators),[9] (ii) the 1950–1960 change in the number of pre-war housing units, (iii) the 1950–1960 change in the Black population share, and (iv) the 1950–1960 change in average years of educational attainment. We prefer this specification because it allows us to flexibly control for both proximity to the central business district, as well as pre-trends in pre-war housing units, racial composition, and education. 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]Defined by the 1982 Census of Retail Trade (Fee and Hartley 2012). Holian (2019) finds that this measure compares well to others.

proach to defining market access that we use 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 3a shows pre-war housing units (built 1940 or earlier) in treatment and comparison groups across early and late FAIR states. All groups experienced declines, consistent with disinvestment and population loss in U.S. central cities. However, significant differences emerged in early FAIR states.

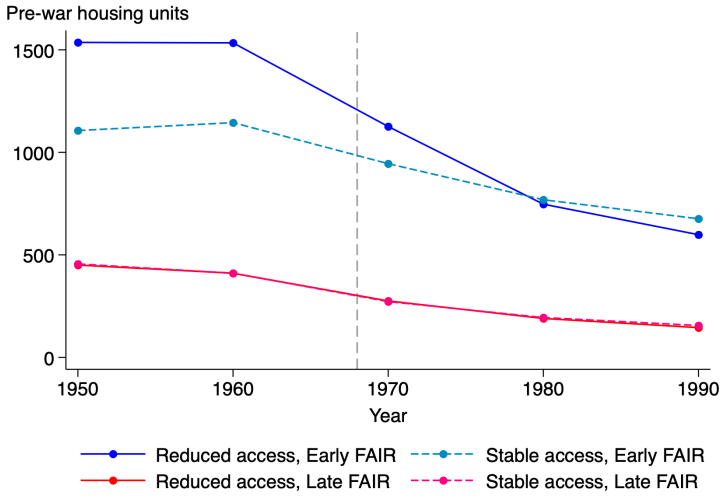
In early FAIR cities, neighborhoods with declining private insurance access between 1940 and 1967 showed a larger decrease in the pre-existing housing stock compared to those with stable access. This effect became apparent during the 1960s and was pronounced in the 1970s, aligning with FAIR plan implementation. Anecdotal evidence suggests immediate high volumes of applications, 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.

Late FAIR states showed little difference between treatment and control groups. Pretrends between 1950 and 1960 appear comparable across groups in both early and late FAIR cities, supporting the parallel trends assumption.

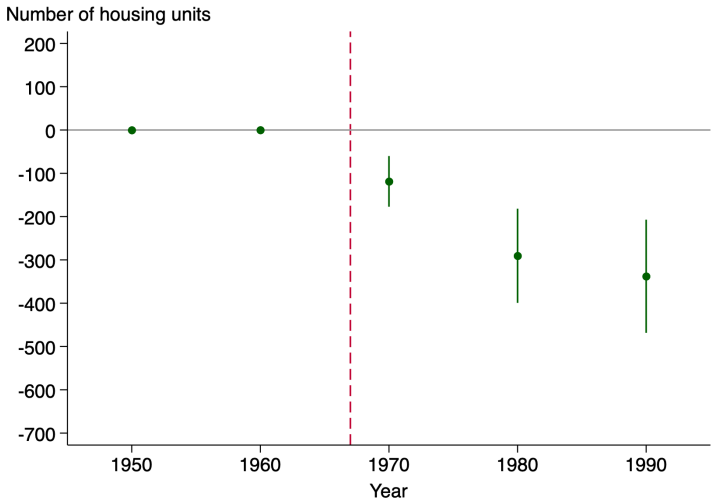
We analyze these patterns by estimating Equation 2 using OLS. Table 1 displays results with different controls. The dependent variable is pre-war housing units. Column 1 includes no controls, Column 2 adds tract fixed effects, and Column 3 controls for four tract factors interacted with year fixed effects: (i) the distance to the city center, entered as within-metro decile indicators, (ii) 1950–1960 change in Black population share, (iii) 1950–1960 change in pre-war housing units, and (iv) 1950–1960 change in average years of educational attainment. These controls allow for differential dynamics associated with these neighborhood characteristics (see Appendix F for details).

Across columns, the estimated triple difference is negative, statistically significant, and consistent in magnitude. In our preferred specification (Column 3), the ITT effect of

(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 3: Pre-war housing units by year

FAIR plans was a loss of about 241 pre-war housing units per census tract, accounting for 22.8% 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 10% level ($p = 0.06$).

Figure 3b and Appendix Figure G.1 show event study estimates, confirming parallel pre-treatment trends and revealing the timing of effects. 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).

FAIR plans may have created moral hazard, making abandonment attractive to landlords. Owner-occupiers, however, may have had higher opportunity costs of disinvestment. Appendix Table G.1 estimates FAIR plans' effect on various housing types. Results show modestly positive effects on owner-occupied and single-family units, but significant negative 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 postwar 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.

Comparison with building fire results. We present back-of-the-envelope calculations comparing these findings to our earlier building fire analysis. In Manhattan, the Bronx, and Brooklyn, we identify 286 census tracts likely to have seen high FAIR plan offerings. Assuming 9,000 excess fires in 1978 (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 that year.[10] Extrapolating these annual rates to a decennial rate of housing unit loss yields estimates comparable to our main results.

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

[10]Similarly, Philadelphia experienced about 500 excess fires in 1978, or 23 building fires per tract across 22 tracts.

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

	(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***	-240.8***	-275.8**	-22.6
*1(>=1970)	(71.6)	(71.1)	(48.7)	(115.4)	(97.7)
1(early FAIR)*1(>=1970)	-104.4	-108.0*	-150.7***	-154.5***	-199.6***
	(61.9)	(61.4)	(34.8)	(47.3)	(37.5)
1(Reduced Access)*1(>=1970)	-1.2	-2.0	28.4	56.6	24.7
	(46.2)	(45.3)	(33.0)	(106.5)	(84.4)
1(Reduced Access)*1(early FAIR)	412.0**		163.8	308.3	112.0
	(154.5)		(102.4)	(190.1)	(177.5)
1(Reduced Access)	-2.5		-55.2	-216.7	-87.7
	(90.7)		(57.2)	(142.7)	(129.0)
1(early FAIR)	692.8***		744.6***	791.2***	782.8***
	(121.6)		(84.2)	(106.2)	(79.8)
1(>=1970)	-225.8***	-225.0***	-1,382.8***	-1,232.0***	-1,215.7***
	(59.5)	(58.7)	(201.0)	(192.6)	(209.2)
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	0.390	0.369	0.366

This table reports OLS estimates of equation 2. Each observation is a census tract $\times$ 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, the 1950–1960 change in average years of education interacted with year fixed effects, the 1950–1960 change in pre-war housing units interacted with year fixed effects, and within-metro distance to central city decile interacted 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 dummy 1(Reduced Access) is defined based on access to lawyers. Robust standard errors are clustered at the city level. ***—$p < 0.01$, **—$p < 0.05$, *—$p < 0.1$.

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 FAIR plans' effects on neighborhood outcomes, representing total ITT effects on housing and neighborhoods. These incorporate *direct* effects on property owners' passive disinvestment and arson choices, and *indirect* effects through spillovers to neighboring properties. For example, if there are negative externalities from abandoned properties to nearby properties, then a FAIR plan holder's choice to abandon their own property may reduce demand and rental income for neighboring property owners, thus further increasing neighborhood disinvestment.

Table 2 displays results using the same specification as Table 1, Column 3, which includes interactions between pre-determined tract factors and year dummies. The regressions are weighted by initial tract population. The key coefficient of interest is the triple interaction term $1(ReducedAccess) \times 1(earlyFAIR) \times 1(>= 1970)$, which captures the differential impact of FAIR plans on neighborhoods with reduced insurance access in early- versus late-adopting states after 1970.

We see reduced access leads to significant declines in a tract's total population, white population, and nonwhite population (Columns 1, 3, and 4). We also see treatment resulting in a 6.9 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. We also see a positive effect on years of education among adults but it is not statistically significant.

These findings suggest FAIR plans substantially impacted neighborhood composition and economic conditions, potentially accelerating white flight and decreasing neighborhood economic status in treated areas. The results highlight the complex interplay between mid-century insurance policy, housing markets, and neighborhood dynamics, revealing unintended consequences of FAIR plans.

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 education persons 25+	(7) log of avg. contract rent	(8) log of avg. income
1(Reduced Access)*1(early FAIR)*1(>=1970)	-0.442**	-0.184	-0.334***	-0.212***	0.069**	0.255	-0.016	-0.064**
	(0.178)	(0.127)	(0.104)	(0.073)	(0.026)	(0.181)	(0.057)	(0.030)
1(early FAIR)*1(>=1970)	-0.313**	-0.186	-0.173	-0.283***	0.000	0.776***	0.126**	0.016
	(0.151)	(0.133)	(0.126)	(0.080)	(0.021)	(0.174)	(0.061)	(0.032)
1(Reduced Access)*1(>=1970)	0.122	0.074	0.179*	0.090	-0.044*	-0.438**	-0.036	-0.018
	(0.167)	(0.118)	(0.091)	(0.065)	(0.022)	(0.166)	(0.050)	(0.029)
1(Reduced Access)*1(early FAIR)	-0.056	0.048	0.049	-0.013	0.047	-0.560**	-0.137**	-0.145**
	(0.097)	(0.277)	(0.279)	(0.124)	(0.050)	(0.246)	(0.066)	(0.055)
1(Reduced Access)	0.051	0.142	0.164	0.058	0.016	0.019	0.105	0.088
	(0.086)	(0.239)	(0.241)	(0.106)	(0.050)	(0.265)	(0.066)	(0.057)
1(early FAIR)	0.473***	0.234	0.197	0.372***	-0.023	-0.455	0.002	0.062
	(0.069)	(0.235)	(0.233)	(0.101)	(0.049)	(0.272)	(0.074)	(0.056)
1(>=1970)	-0.541**	-2.976***	-2.218***	-0.583***	-0.133*	3.680***	2.151***	2.784***
	(0.230)	(0.464)	(0.463)	(0.200)	(0.069)	(0.522)	(0.147)	(0.103)
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	0.497	0.159	0.151	0.149	0.384	0.304	0.862	0.843

This table reports OLS estimates of equation 2. Each observation is a census tract $\times$ year. 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, the 1950–1960 change in the average years of education interacted with year fixed effects, the 1950–1960 change in the number of pre-war housing units interacted with year fixed effects, and within-metro distance to central city decile interacted with year-fixed effects. Robust standard errors are clustered at the city level. ***—$p < 0.01$, **—$p < 0.05$, *—$p < 0.1$.

5 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 the original design of FAIR plans resulted in over-insured properties which created moral hazard and accelerated 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 illustrate the challenges in designing policy responses to address unraveling property insurance markets.

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Appendix A Underwriting profits for state FAIR plans through September 1977

A 1978 report (United States General Accounting Office 1978) documented that state FAIR plans generally suffered losses. Table A.1 reproduces this report, which tabulates 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 United States 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.

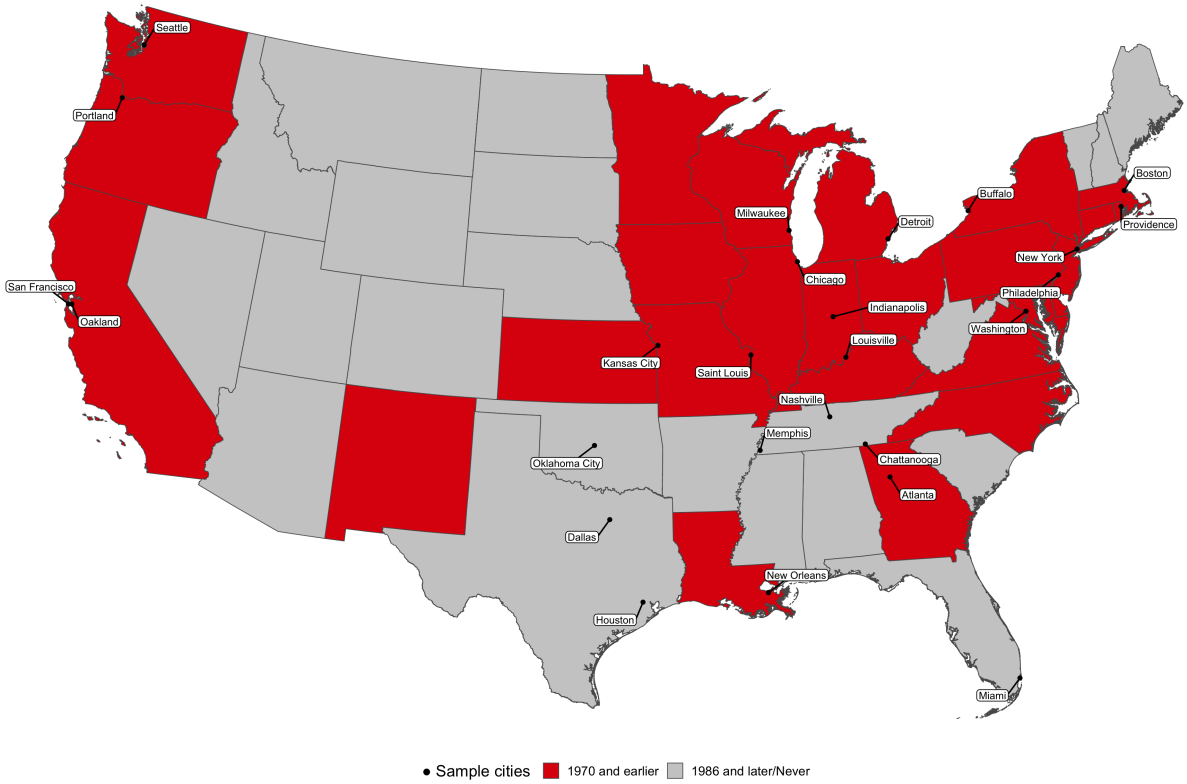
While the plans differed in details, key pieces of their overall structure were basically the same, and they influenced the development of the later FAIR plans. The success of these Urban Area Plans varied, too; the earliest and most successful appears to be the Boston plan, which insured over 20,000 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 et al. 1968, p. 57). Other states, such as New York, Pennsylvania, and Delaware, patterned their Urban Area Plans after Boston had similar provisions.

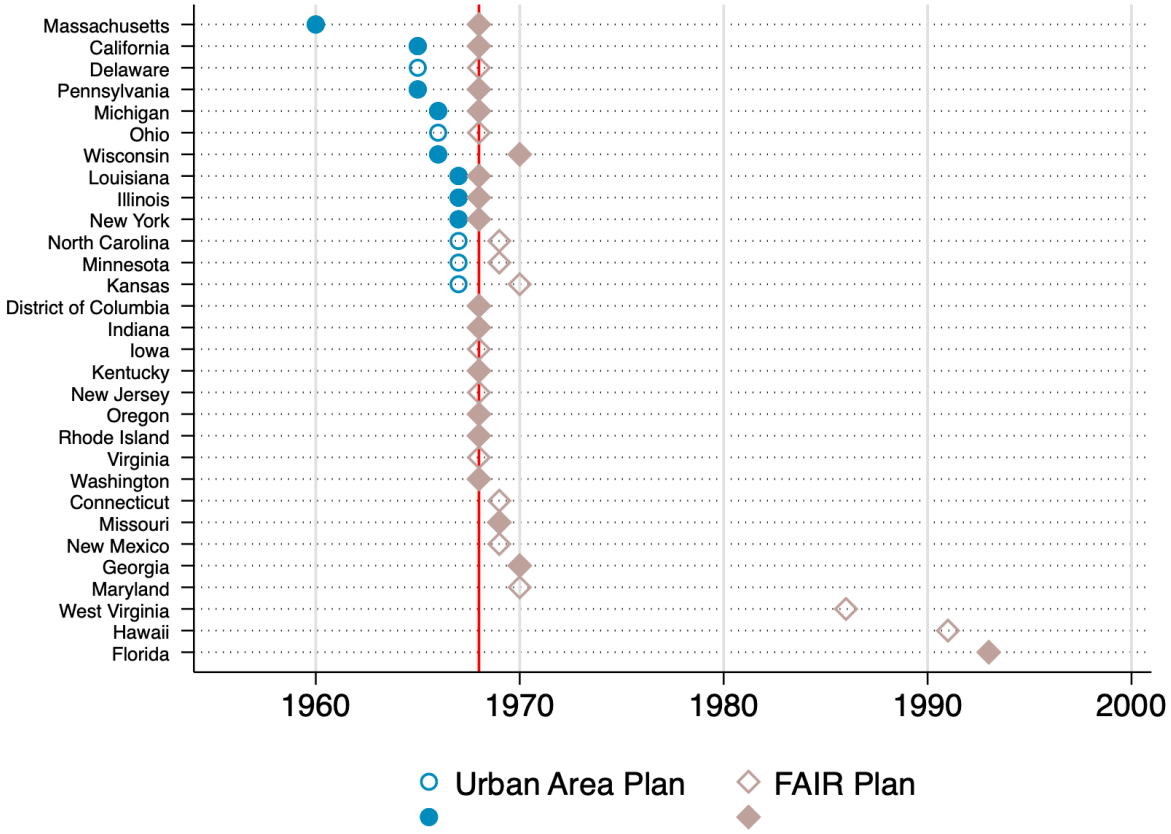
Third, mandatory participation pools required companies to write policies and share risks among all insurers. For example, in Detroit, all property and liability insurance companies doing business in the state signed pledges to participate (Hughes et al. 1968, p. 61). Under the Watts plan in Los Angeles, 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.



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

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



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

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 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.

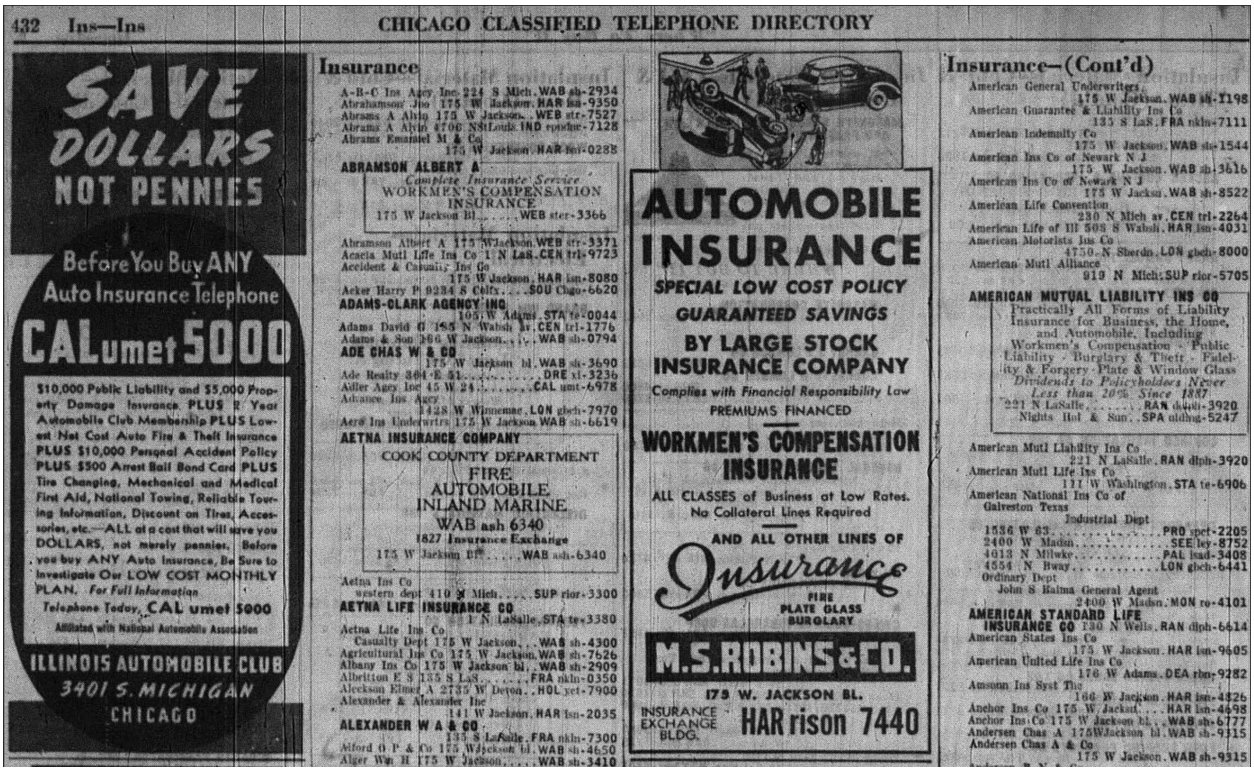


Figure D.1: Example page from 1940 Chicago directory

We selected Property and Casualty Insurers where they were listed separately, otherwise we selected all insurers but dropped insurers with names containing "life" to avoid using firms that primarily sell life insurance policies. We ignored large panel advertisements.

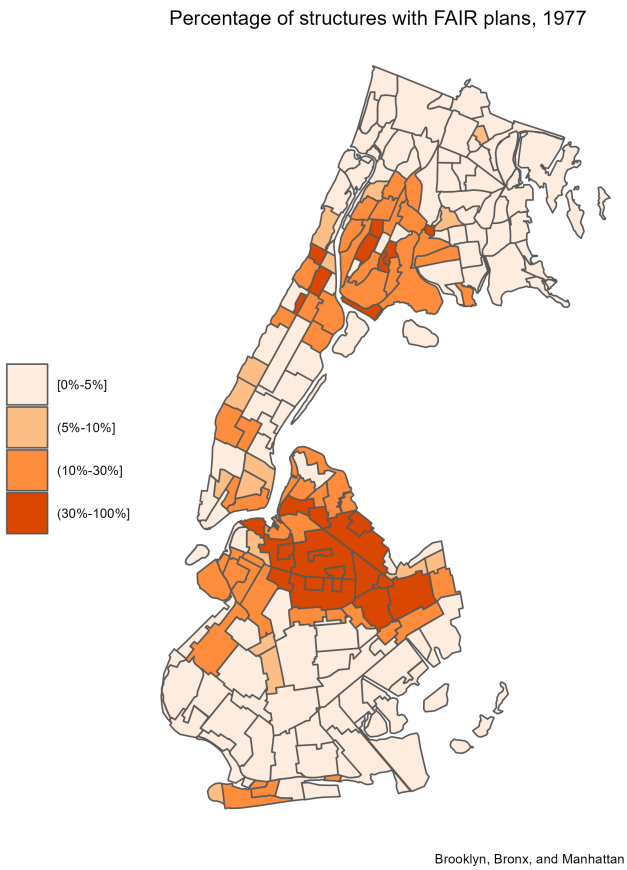
Then, we used ArcGIS to geo-code the addresses using a locator file built from ArcGIS 2012 StreetMap(TM) North America. We used human annotators to verify the geo-coder output and fill in missing values that the geo-coder was unable to locate. Based on these annotations, we found that missing geo-locations 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 classification and our results. See Figures 2 and E.1 for further validation of our directory data.

Table D.1: City directories: Years and geo-coding rates by city

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.

Appendix E Validation using 1977 FIA sample



This map displays data from United States Congress, Senate, Committee on the Judiciary, Subcommitee on Citizens and Shareholders Rights and Remedies (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: 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. We use a balanced panel of census tracts from 1950 to 1990. The statistics refer to one year.

Table F.2 presents tests of the differences in the pre-trends of seven variables for 1950–1960 (top panel) and six variables for 1940–1950 (bottom panel) before and after controlling for distance to the CBD and pre-existing (1950–1960) trends in pre-war housing units, racial composition, and education. Monthly contract rents and annual incomes are reported in nominal dollars. 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 989 census tracts in our reduced insurance access early FAIR state sample, reflecting low owner-occupancy rates in these areas. Tract data are only available for about an 85% subset of our sample in 1940.

Column 1 of Table F.2 presents a difference in differences (DiD) in mean outcome changes between 1950–1960 (or 1940–1950). The first difference is between "reduced access" tracts and "stable access" tracts. The second difference is between early FAIR states and late FAIR states. For example, the first estimate reports that reduced-access tracts experienced slower growth in prewar housing units compared with stable-access tracts, in early-FAIR versus late-FAIR states, by about 42 housing units.

Column 2 presents p-values from a test of whether these DiD in means are equal to zero. For all 1950–1960 changes in variables and two 1940–1950 changes in variables the p-values show that the tests reject that these DiD in means are equal at the 5 percent level. The values in Column 1 also show economically significant DiD in the pre-trends among the four groups of tracts.

For this reason, we adopt a control variable approach. Column 3 reveals that control-

ling for within-metro deciles of distance to CBD and the changes in the three first-listed factors (1950–1960 changes in pre-war housing units, Black share, and years of education) greatly reduces the economic significance of the differences in trends of the remaining four 1950–1960 variables that were not controlled for. (The differences in the means of the top three variables are mechanically zero.) Furthermore, the p-values of these tests presented in Column 4 reveal that most of these differences are no longer statistically significant at the 5 percent level (income is the exception).

The bottom panel of Table F.2 presents tests of the DiD in the pre-trends of six variables for 1940–1950. (Income is not available in the 1940 Census.) The values in Columns 1 and 3 show economically small DiD for both the raw and residualized variables. The p-values of these tests presented in Column 4 reveal that most of these differences (like the upper panel) are not statistically significant at the 5 percent level. The exceptions are years of education and the owner occupancy rate. (Notably, the 1940–1950 changes in years of education DiD is smaller than half the magnitude and in the opposite direction than for 1950–1960.) Despite these statistically significant DiD in 1940–1950 changes we interpret the sum of the evidence as supporting our assessment that conditional on within-metro distance to CBD deciles and 1950–1960 trends in prewar housing units, Black population share, and years of education, no economically meaningful, and mostly no statistically meaningful DiD pre-trends are apparent in the two decades from 1940 to 1960.

Table F.2: Balance of pre-trends

	(1)	(2)	(3)	(4)
			Residualized	
	DiD in Means	p-value	DiD in Means	p-value
1950-1960 Changes in:				
Pre-war Housing Units	-44.38	0.02	0.00	
Black Population Share	0.08	0.00	0.00	
Years of Education	-1.06	0.00	0.00	
Population	-461.73	0.00	-163.62	0.07
Contract Rent	-0.14	0.89	2.05	0.04
Income	-1,816.32	0.00	-749.79	0.00
Owner Occupancy Rate	-0.04	0.00	0.00	0.57
1940-1950 Changes in:				
Pre-war Housing Units	22.52	0.51	1.65	0.95
Black Population Share	0.01	0.17	0.00	0.64
Years of Education	0.30	0.01	0.36	0.00
Population	-462.89	0.02	-377.80	0.05
Contract Rent	-5.59	0.00	-5.13	0.00
Owner Occupancy Rate	0.03	0.00	0.03	0.00

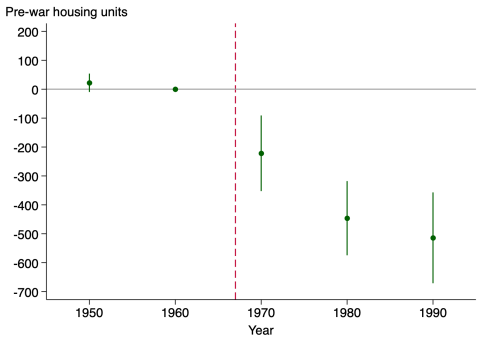
This table presents balance tests of the pre-trends of our outcome variables from 1950-1960 and 1940-1950. Column 1 reports differences in differences in means calculated as (early FAIR, reduced access - early FAIR, stable access) - (late FAIR, reduced access - late FAIR, stable access). Column 2 reports p-values from testing whether these differences in Column 1 are different from zero. Column 3 reports the same tests as Column 1 but on the residuals from regressing changes in the outcome variable on within-metro distance to CBD decile indicators and 1950–1960 changes in the first three variables (prewar housing units, Black population share, and years of education). Column 4 reports p-values from testing whether the differences in Column 3 are different from zero.

Appendix G Additional results and robustness

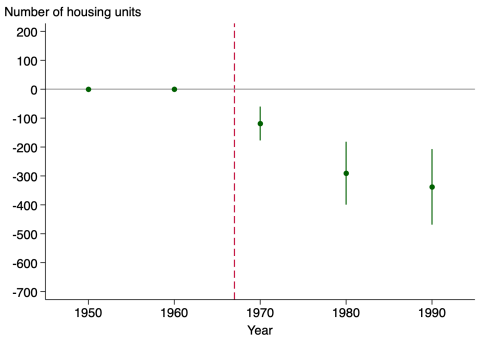
Appendix G.1 Event study estimates

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 3b from the main text) reports the coefficients with neighborhood characteristics*time fixed effects as controls (corresponding to Column 3 in Table 1). In both panels, the estimated effect is small and close to zero before 1970. Figure G.1b shows a precisely zero pre-1970 effect by construction because the regression controls for the pre-treatment outcome change $\times$ year fixed effects. Immediately after the policy, the negative effect on prewar housing stock emerged and became even more negative during the 1970s and the 1980s.

(a) No controls

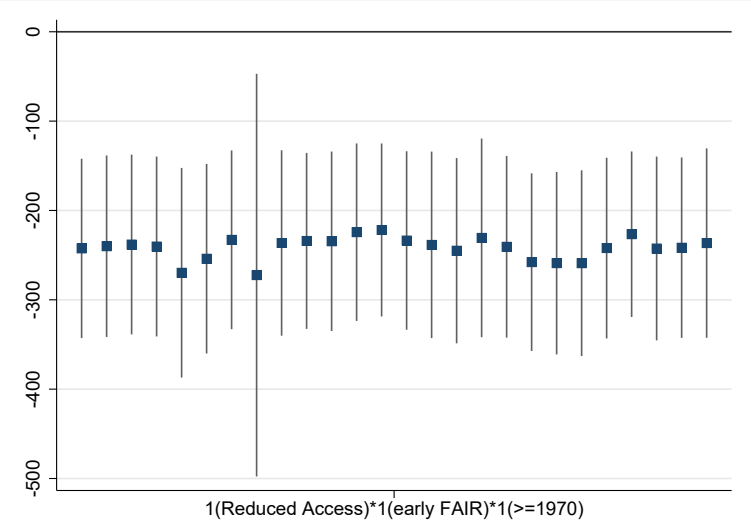


(b) Regression adjustment



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.1: Event study



Note: 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.1: FAIR effects by structure type

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)	-261.7***	68.3**	-332.0***	93.4*	-355.2***
*1(>=1970)	(60.7)	(26.6)	(51.6)	(45.8)	(54.4)
1(early FAIR)*1(>=1970)	-433.6***	-159.7***	-270.4***	-171.1***	-261.9***
	(58.8)	(29.6)	(41.8)	(43.5)	(45.4)
1(Reduced Access)*1(>=1970)	37.3	-89.8***	127.5***	-66.9***	105.5**
	(35.9)	(16.4)	(33.6)	(23.5)	(39.1)
1(Reduced Access)*1(early FAIR)	35.2	-121.6	156.8	-227.5	263.2***
	(152.9)	(104.0)	(95.0)	(147.8)	(89.5)
1(Reduced Access)	-5.6	8.5	-14.2	52.4	-58.5
	(120.1)	(74.3)	(74.2)	(101.2)	(64.5)
1(early FAIR)	628.3***	52.0	575.9***	-118.2	745.9***
	(102.9)	(116.1)	(130.5)	(150.5)	(154.5)
1(>=1970)	145.5	846.2***	-703.3***	784.9***	-639.7**
	(252.3)	(162.6)	(211.9)	(230.5)	(234.1)
Tract Fixed Effects	NO	NO	NO	NO	NO
Tract Changes*Year Fixed Effects	YES	YES	YES	YES	YES
Observations	29,777	29,765	29,738	29,795	29,762
R-squared	0.203	0.128	0.270	0.117	0.255

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 in the tract interacted with year-fixed effects, the 1950-1960 change in average years of education interacted with year-fixed effects, the 1950-1960 change in pre-war housing units interacted with year-fixed effects, and within-metro distance to central city decile interacted the year-fixed effects. Robust standard errors are clustered at the city level. ***—$p < 0.01$, **—$p < 0.05$, *—$p < 0.1$.

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

Outcome variable:	(1)	(2)	(3)
	Housing units built 1940 or earlier		
1(early FAIR)*1(>=1970)*1(chg MA<0)	-417.8***	-374.4***	-233.5***
	(70.6)	(71.2)	(44.4)
1(early FAIR)*1(>=1970)*chg MA	21.3***	-5.3*	4.7
	(7.4)	(2.7)	(6.8)
1(early FAIR)*1(>=1970)*chg MA*1(chg MA<0)	-24.0***	5.8	2.0
	(7.4)	(5.3)	(9.2)
1(early FAIR)*1(>=1970)	-111.7*	-106.2*	-152.4***
	(62.8)	(61.5)	(34.6)
1(Reduced Access)*1(>=1970)	-1.2	-2.0	29.8
	(46.2)	(45.3)	(33.9)
1(Reduced Access)*1(early FAIR)	412.0**		163.8
	(154.5)		(102.4)
1(Reduced Access)	-2.5		-55.2
	(90.7)		(57.2)
1(early FAIR)	692.8***		744.6***
	(121.6)		(84.2)
1(>=1970)	-225.8***	-225.0***	-1,372.3***
	(59.5)	(58.7)	(202.6)
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	0.391

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 in the tract interacted with year-fixed effects, the 1950-1960 change in average years of education interacted with year-fixed effects, the 1950-1960 change in pre-war housing units interacted with year-fixed effects, and within-metro distance to central city decile interacted the 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***	-166.6***	-474.1***	-473.1***	-210.9***
*1(>=1970)	(73.3)	(72.7)	(45.9)	(63.4)	(63.1)	(58.3)
1(early FAIR)*1(>=1970)	-123.8*	-127.2**	-162.7***	-65.0	-69.3	-154.7***
	(60.4)	(59.9)	(33.1)	(42.0)	(40.8)	(41.0)
1(Reduced Access)*1(>=1970)	-67.3*	-68.0*	-10.7	125.3***	124.1***	17.3
	(38.0)	(37.1)	(30.6)	(41.9)	(40.5)	(47.8)
1(Reduced Access)*1(early FAIR)	235.5		69.1	658.4***		142.8
	(142.8)		(97.9)	(120.3)		(103.8)
1(Reduced Access)	116.3**		43.9	-240.4***		-28.1
	(53.2)		(42.2)	(59.6)		(80.5)
1(early FAIR)	717.0***		760.6***	603.8***		745.0***
	(119.1)		(82.7)	(81.5)		(91.2)
1(>=1970)	-214.3***	-213.7***	-1,351.2***	-272.0***	-270.8***	-1,368.2***
	(57.5)	(56.7)	(207.4)	(37.5)	(36.0)	(209.2)
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	0.390	0.170	0.842	0.390

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

Outcome variable:	(1)	(2)	(3)
	Housing units built 1940 or earlier		
1(Reduced Access)*1(early FAIR)	-395.3***	-396.2***	-238.0***
*1(>=1970)	(79.1)	(78.6)	(51.3)
1(early FAIR)*1(>=1970)	-125.1*	-129.2*	-180.2***
	(65.9)	(65.1)	(32.0)
1(Reduced Access)*1(>=1970)	-5.9	-6.7	10.5
	(53.5)	(52.5)	(30.8)
1(Reduced Access)*1(early FAIR)	393.3**		125.0
	(167.6)		(104.5)
1(Reduced Access)	9.2		-23.2
	(102.3)		(47.5)
1(early FAIR)	744.1***		813.3***
	(126.0)		(79.0)
1(>=1970)	-216.7***	-215.9***	-1,376.6***
	(63.9)	(63.0)	(200.7)
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	0.395

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 in the tract interacted with year-fixed effects, the 1950-1960 change in average years of education interacted with year-fixed effects, the 1950-1960 change in pre-war housing units interacted with year-fixed effects, and within-metro distance to central city decile interacted the year-fixed effects. Robust standard errors are clustered at the city level. ***—$p < 0.01$, **—$p < 0.05$, *—$p < 0.1$.