THE HOME FRONT: RENT CONTROL AND THE RAPID WARTIME INCREASE
IN HOME OWNERSHIP

Daniel K. Fetter

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ABSTRACT

The US home ownership rate rose by 10 percentage points between 1940 and 1945, about half the size of the net change over the 20th century, despite severe restrictions on construction during World War II. I present evidence that wartime rent control -- which covered over 80 percent of the 1940 U.S. rental housing stock -- played an important role in this shift, as suggested by Friedman and Stigler (1946). The empirical test exploits features of the central authority's method of imposing rent control, which generated variation in the size of rent reductions for cities that had seen similar increases in rents prior to control. Greater rent reductions were associated with greater increases in home ownership over the first half of the 1940's. This relationship does not appear to be driven by differential trends in housing demand or other unobserved factors potentially correlated with variation in rent reductions. The estimates suggest that rent control may explain 65 percent of the urban increase in home ownership over the first half of the 1940's.

Daniel K. Fetter
Department of Economics
Wellesley College
106 Central Street
Wellesley, MA 02481
and NBER
dfetter@wellesley.edu

1   Introduction

> *...many a landlord is deciding that it is better to sell at the inflated market price than to rent at a fixed ceiling price. The ceiling on rents, therefore, means that an increasing fraction of all housing is being put on the market for owner-occupancy, and that rentals are becoming almost impossible to find, at least at the legal rents.*

> – Milton Friedman and George J. Stigler, *Roofs or Ceilings?* (1946)

The mid-20th century increase in home ownership in the United States is often framed as a post-World War II change, associated with the construction of new housing in suburban areas (Jackson, 1985). The years from 1945 to 1960 indeed saw home ownership rise rapidly. Yet intercensal survey estimates suggest that the home ownership rate increased by 10 percentage points between 1940 and 1945 – slightly more than the postwar rise from 1945 to 1960 – despite severe restrictions on residential construction during the war. This paper focuses on one of the largest government interventions in housing markets during the 1940's – wartime and postwar rent control – and attempts to illuminate its role in this "wartime" rise in home ownership.

World War II saw the most widespread imposition of rent control in the history of the United States. Roughly 80 percent of the 1940 rental housing stock lay in areas that the federal government put under rent control between 1941 and 1946. At the same time, sales prices were not controlled. Many observers at the time, including Friedman and Stigler (1946), argued that rent control led to higher rates of home ownership as landlords chose to sell at uncontrolled prices rather than continuing to rent out their properties at the controlled price. Consistent with this story, a large share of the wartime increase in home ownership came from the same dwellings shifting from renter-to owner-occupancy.[1] But most contemporary arguments on the importance of rent control were based solely on aggregate national trends, even though many other factors – such as rising income and savings or increasing marginal tax rates – could have played a role in the observed shift in housing tenure.

An empirical test of the hypothesis that wartime rent control increased home ownership is

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[1] Given the link between tenure and structure type – Glaeser and Shapiro (2003) show that in modern data most single family detached units are owner-occupied and most multifamily units are renter-occupied – it is useful to note that in 1940 about 43 percent of single-family detached units were renter-occupied (by 1950, only 27 percent of single family detached units were renter-occupied). It was certainly true, however, that most owner-occupier households in 1940 – about 84 percent – lived in single family detached housing.

complicated by the fact that nearly all urban areas were placed under rent control during the war. To overcome this challenge, I leverage local-level variation in the severity of rent control that was generated by the central authority's method of determining rent reductions. The empirical test compares cities with similar increases in rents prior to control, but different reductions in rent when control was imposed. The idea is illustrated in Figure 1. On the date when rent control was imposed in each city (the right vertical line), the maximum legal rent for each dwelling was set at its level on the city's 'base' date (the left vertical line). The spirit of the test is to draw comparisons like that between Baltimore and Buffalo and to avoid comparisons like that between Baltimore and Boston, since Boston's low severity of control was most likely due to lesser growth of housing demand. Conditioning on the degree of rent appreciation before control, I use the implied reduction in rents from their pre-control levels to their base date levels as variation in the severity of rent control at the time of imposition, and test whether cities in which rent control was more severe at imposition saw greater increases in home ownership over the first half of the 1940's.

To implement this test, I compile a dataset of 51 cities for which the National Industrial Conference Board (NICB) had begun publishing a monthly rent index by March 1940, and link the data on rents to newly digitized records from intercensal housing surveys carried out by the Census Bureau and the Bureau of Labor Statistics (BLS) between 1944 and 1946. The results provide evidence that rent control did increase home ownership. A 2.5 percentage point greater reduction in rents relative to their maximum pre-control levels – roughly a standard deviation in the sample cities – was associated with roughly a 1 percentage point greater increase in home ownership between 1940 and the city's survey date.

The identifying assumption in the main specification is that conditional on pre-control rent appreciation, the rent reduction at the time of control was unrelated to unobserved city trends also driving differential changes in home ownership. Three complementary approaches support the validity of this assumption. The first approach isolates more transparent variation in how rent reductions were determined. Conditional on the degree of rent appreciation prior to control, the implied rent reduction for a city depended on the combination of the central authority's choice of base date and the timing of rent increases in that city. But while base dates in the initial wave of

rent control imposition were typically city-specific, most areas controlled in the second wave were set to a "default" base date, thus generating variation in severity associated only with the precise timing of rent increases. I show that the severity result holds in the subsample of second-wave cities as well, and further show using "placebo" base dates that conditional on rent appreciation prior to control, the timing of rent increases is unlikely to have been related to underlying trends driving home ownership. The second approach is to introduce additional controls related to trends in other factors that may have increased home ownership. I show that the main results cannot be explained by cities with initially more severe rent control having greater increases in income or savings, greater increases in benefits from the tax treatment of owner-occupied housing, different expectations of future city growth, or greater declines in home ownership over the 1930's. The final approach supports the argument by using an entirely different source of variation – the timing of imposition of control across cities – to assess whether it also offers evidence consistent with rent control encouraging home ownership. In a new dataset on newspaper advertisements, I show suggestive evidence that the imposition of control led to a differential increase in the number of ads for home sales, with little evidence of differential pre-control trends.

The main results focus on a measure of the severity of rent control at the time of imposition, and only imperfectly reflect the incentives that property owners faced in the mid-1940's, when tenure status is observed. For a subsample of 35 cities I use new data on home asking price appreciation between April 1940 and September 1945, obtained from records at the National Archives, to show that house prices rose substantially even as rents stayed fixed. Moreover, specifications similar to those used for the main results suggest that greater initial severity of rent control translated into even greater subsequent severity by raising house prices, for which a natural explanation is the shifting of excess demand into the market for owner-occupancy. I take the difference between house price appreciation as of 1945 and the legal degree of rent appreciation as a measure of 'actual' severity to scale the estimates of the effect of the initial severity of control on home ownership. Using initial severity as an instrument for this measure of actual severity, I find that a 10 percentage point greater difference between house price and allowed rent appreciation translated into an increase in home ownership that was 0.78 percentage points greater. A rough calculation using this estimate

suggests that rent control can explain 65 percent of the increase in home ownership in urban areas over the first half of the 1940's.

A first contribution of this paper is to our understanding of the rise of home ownership in the United States in the mid-20th century. Most work on this shift has focused on forces that increased home ownership in the postwar era, in which the metropolitan population increasingly suburbanized and new housing was supplied elastically.[2] The recent economics literature has not generally noted the increase in home ownership before the end of World War II. Unlike the postwar increase, the wartime increase in home ownership requires an explanation that takes into account the inelastic supply of structures during the war, and hence accounts for the decisions of the erstwhile landlords (or other property owners) who chose to supply housing for owner-occupancy. The focus of this paper is on understanding the role of rent control in the wartime shift, but it is likely that the sudden creation of a large group of home owners during the war and the corresponding disinvestment in rental housing – as well as the continuation of rent controls in many areas in the postwar decade – influenced postwar development as well. Testing for such effects is beyond the scope of this paper, but in the conclusion I offer some speculation on the possible longer-run influences of World War II-era rent control.

Secondly, this paper contributes to the economic history literature on the US economy during World War II. There has been much work on labor markets during the war, the macroeconomic effects of war spending, and other dimensions of the wartime economy – Goldin (1991), Goldin and Margo (1992), Higgs (1992), Mulligan (1998), Collins (2001), Acemoglu, Autor and Lyle (2004), Field (2008), Cullen and Fishback (2012), and Rockoff (2012) are but a few examples from the last two decades. But the modern economics literature has thus far said comparatively little about the operation of wartime housing markets, which is an important gap to fill given how prominently the housing shortage figured at the time.[3]

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[2] Baum-Snow (2007) and Boustan and Shertzer (2013) discuss trends in suburban residence; Glaeser (2013) notes the highly elastic supply of housing in the postwar years. With elastic supply, several factors may have contributed to rising home ownership after the war. Past work has investigated the roles of rising incomes (Chevan, 1989), non-neutralities in the income tax code (Rosen and Rosen, 1980), and expansions of mortgage finance (Fetter, 2013; Chambers, Garriga and Schlagenhauf, forthcoming).

[3] As evidence on this last point, a casual internet search turns up several films premised on the wartime housing shortage, such as "The More the Merrier" (1942) and "Standing Room Only" (1944). (Special thanks go to Stanley Engerman for further movie recommendations on the wartime housing shortage.)

Rent control is a classic economic issue, and this paper also contributes to the literature on rent control in multiple ways. Although World War II has been recognized as an important period of rent control, to the best of my knowledge there has been no work using modern empirical methods to examine it. Also important is that a large share of the rent control literature has focused on single-market case studies, and on cases in which local jurisdictions themselves impose rent control.[4] Centralized administration of wartime rent control generates more transparent variation in rent control regulations, and its wide geographic extent allows the construction of market-level rather than unit-level counterfactuals. Finally, while textbook models predict that rent control will reduce the supply of rental housing, in their review of the vast empirical literature on rent control Turner and Malpezzi (2003) note that there are remarkably few direct empirical tests of this prediction; Sims (2007) is a more recent exception.[5] Isolating a supply relationship for rental housing is not the goal of this paper, but the results are consistent with the prediction that rent control reduces rental housing supply.

The next section provides some background on rent control and wartime housing markets. Section 3 describes the data and empirical strategy and shows the main, reduced-form estimates. Section 4 provides support for a causal interpretation of the estimates. Section 5 offers a plausible scaling of the reduced-form estimates and carries out a rough calculation of the contribution of rent control to the overall increase in home ownership. Section 6 concludes.

2 Rent control and wartime housing markets

2.1 Federal rent control

The expansion of military production in the early 1940's brought large population inflows to many urban areas, with the civilian population more than doubling between 1940 and 1943 in some cities.

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[4]The latter point is why it is important in both Sims (2007) and Autor, Palmer and Pathak (2012) that the removal of rent control in Cambridge and other Eastern Massachusetts cities was determined by a state-wide ballot initiative rather than by local decisions. Central administration of wartime rent control is similarly helpful in my empirical setting.

[5]In addition to Turner and Malpezzi (2003), Glaeser and Gyourko (2008) and Arnott (1995) also offer relatively recent reviews of the broader literature on rent control. Much of the early empirical literature, such as Olsen (1972), estimated the size and distribution of benefits of rent control to tenants in controlled units rather than supply effects.

As early as 1940, rising rents in the areas undergoing industrial expansion drew the attention of the federal government, which feared that rising rents would put upward pressure on wages and reduce labor supply in war production centers.[6] In response to rising prices in many sectors, the Emergency Price Control Act of 1942 established the Office of Price Administration (OPA) as an independent agency, and gave it broad powers to ration goods and to control prices and rents.

Rent control was imposed at the level of 'defense rental areas,' which were designated by the OPA. After a period of 60 days following this designation, the OPA could impose a ceiling on rents. In most cases, defense rental areas were made up of whole counties or groups of counties because in the urgency of wartime the OPA found more precise delineation of areas undergoing rent increases to be too time-consuming. Initially, the OPA requested surveys of rents from the BLS and Work Projects Administration (WPA) to determine areas in which rising rents threatened the defense program, and designated these areas first. In October 1942, however, the entirety of the rest of the country was designated into defense rental areas and was subject to the imposition of control, although in the end not all areas were actually put under control.

The method of rent control was to set a 'maximum rent date' as a base date for freezing rents in each area. The Emergency Price Control Act required that rents in a defense rental area be stabilized at a level prevailing prior to the impact of defense activities. Recognizing that war production affected different areas at different times, the OPA chose a maximum rent date for each defense rental area according to its assessment of when defense production led to rent increases in that area (Porter, 1943). Once a base date was chosen, the OPA required every rental unit in the area to be registered. Forms were sent to the landlord for every rental unit to record the rent and services provided on the base date, with a copy sent to the tenant to verify the landlord's report. The maximum legal rent for each dwelling was then set at its level on the base date.[7] Choice of

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[6]The discussion that follows, except where noted otherwise, is summarized from Harvey C. Mansfield and Associates (1948). In addition to its general concern about inflation, the government appears to have worried that rent increases would lead to greater tenant mobility within cities, reducing their time spent working, and that high rents would dissuade potential new workers from migrating to war production centers.

[7]In cases where landlords made improvements or reduced services provided, the level at which rents were frozen could be adjusted. Similarly, the OPA allowed for adjustment of the maximum rent in cases of "substantial" increase or decrease in occupancy. In cases where a unit existed but was not rented on the base date, the rent was negotiated between the landlord and tenant but was subject to OPA adjustment. New construction projects had to be approved by the Federal Housing Administration and were subject to an FHA-imposed rent ceiling.

the base date was flexible except that it had to be the first of a month. In some cases rents were rolled back by as much as a year and a half, but in general such long rollbacks were found to be problematic. In the first group of cities put under control, in the summer of 1942, a variety of different base dates were used. But by the fall of 1942, a common base date was used for nearly all newly controlled areas. This "default" base date was March 1, 1942, chosen because it was the base date used for other goods under the General Maximum Price Regulation.

Rent control was widespread, and continued to be imposed in new areas even after the end of the war; decontrol did not begin in earnest until the late 1940's.[8] As shown in Table 1, the counties that had been placed under federal rent control by 1946 had held over three quarters of the 1940 total population, and 87 percent of the 1940 nonfarm population.[9] In almost all areas, rent control was imposed and administered by the OPA, rather than local authorities: the few exceptions included Washington, DC, which was covered by its own rent control law passed in December, 1941, and Flint, Michigan, which passed an ordinance in 1942.[10]

Despite widespread federal price and rent control, the OPA did not have the authority to control real estate sales prices or commercial rents.[11] As discussed further below, house prices rose dramatically during the war, giving landlords an incentive to withdraw units from the rental market in order to sell them on the uncontrolled market for owner-occupancy. The OPA recognized this incentive and initially attempted to counteract it by imposing restrictions on eviction. A reportedly more common evasive device was for the landlord to sell the dwelling to the present occupant with

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[8]A handful of areas were decontrolled during or immediately after the war, more were decontrolled in the late 1940's, and most controls that had been imposed during the war or immediately afterwards were eliminated by the mid-1950's, with the famous exception of New York City. Estimating the effects of rent control using variation in decontrol is somewhat more difficult than using variation in the imposition of control. Without the urgency of wartime, geographic boundaries of decontrol were delineated much more precisely than were boundaries of areas put under control; a large share of decontrol actions were also driven by local decisions, unlike the centralized decisions that imposed control.

[9]Data on rent control regulations by county and defense rental area used in this paper, including dates of designation as defense rental areas, dates of control, and maximum rent dates, are from various issues of the *Federal Register*.

[10]Rent control in Washington and Flint was similar to federal rent control: the Flint ordinance was closely modeled on federal rent control, and federal control itself was modeled partly on the DC law.

[11]As noted in Harvey C. Mansfield and Associates (1948), from the beginning the OPA recognized this lack of authority would be a weak point in its rent control program, but decided not to seek the power to control sales prices or commercial rents out of fear that doing so would make the imposition of rent controls politically infeasible. OPA tried on two later occasions to obtain the power to impose price ceilings, but failed. The politics of price control in the housing sector was just one example of the influence of congressional-executive politics on the price control program; Fetter (1974) provides a broader discussion.

a low down-payment, but with regular payments above the maximum rent. In response to this type of evasion, the OPA imposed a restriction in October 1942 that down-payments had to be at least one third of the purchase price (later relaxed to one-fifth) and had to be paid in cash. Nevertheless, conversion of units to owner-occupancy was recognized as a problem throughout the operation of federal rent control.

Consistent with OPA's recognition of this problem, many observers in the postwar decade argued that rent control led to a shift towards higher rates of home ownership. Friedman and Stigler (1946), quoted above, is one example. Humes and Schiro (1946) documented the wartime withdrawal of units from the sample of rental dwellings the BLS used for its cost-of-living index, and claimed that "forced purchases probably constituted a substantial part of these transactions." The history of the OPA in Harvey C. Mansfield and Associates (1948) agreed, noting that "the rent control program undoubtedly contributed to the movement of properties from the rental to the sale market." The Census Bureau's summary of the rapid rise in home ownership over the 1940's (U.S. Bureau of the Census, 1953) also attributed the increase partly to federal rent control. Grebler (1952) emphasized that the movement from renter- to owner-occupancy was particularly important for single-family dwellings, although Harvey C. Mansfield and Associates (1948) noted that there was also some conversion of multifamily dwellings into "cooperatives" during the war. Despite these contemporaneous claims, little evidence has thus far been advanced to support them other than aggregate national trends.

Although these observers typically emphasized actual withdrawals from the rental stock, withdrawals need not have been the only channel through which rent control affected home ownership. The difference in relative returns between renting and selling a property would also have been relevant to the decision of any property owner, whether she had begun as a landlord or not. It is also possible that rent control reduced construction of multifamily dwellings, although with new construction very strictly regulated during the war, this channel may not have been quantitatively important. Given the possibility of multiple supply channels through which rent control may have influenced housing tenure, as well as the lack of data on specific units, the analysis below will focus on the relationship between rent control and the home ownership rate rather than solely on

withdrawals.

2.2 The wartime rise in home ownership

The potential importance of rent control to the rate of home ownership is suggested by Figure 2, which combines owner-occupancy rates from the Decennial Censuses with estimates for 1944, 1945, and 1947 from the Monthly Report on the Labor Force. The increase in home ownership between 1940 and 1945 is remarkable not only for its size – about half that of the overall net change over the 20th century – but also for the fact that it occurred at a time when very little housing was built, as illustrated in Figure 3. If supply of housing for owner-occupancy came only from new construction, the 1940's would present something of a paradox: due to wartime restrictions on residential construction, the years from 1942 to 1945 saw levels of housing starts that were nearly as low as they had been in the depths of the Great Depression. In contrast, increases in home ownership from 1945 to 1950 (and from 1947 to 1950 in particular) were comparatively modest, despite record levels of single-family housing construction.

As these comparisons suggest, a key characteristic of increasing home ownership over the early 1940's was that a large part of its supply side involved conversion of the same dwellings from renter- to owner-occupancy. The BLS recorded 2.3 million nonfarm housing starts in the years from 1940 to 1945, most of them in 1940 and 1941. Yet the number of owner-occupied units increased by 4.8 million from April 1940 to November 1945, and the number of nonfarm owner-occupied units increased by 4.5 million, as documented in Table 2. At the same time, the number of renter-occupied units showed an absolute decline of two million units, from 19.7 to 17.6 million; the number of nonfarm renter-occupied units fell by about 0.9 million.[12] Supporting the interpretation that these changes reflected conversion of renter-occupied units to owner-occupancy rather than

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[12]In part the decrease in the absolute number of units in farm rental housing reflects conversion of dwellings from farm to non farm use over this period, as described in Appendix A of Grebler, Blank and Winnick (1956). The figures in this table also show that home ownership rose substantially for both farm and non farm housing over the 1940's, although the smaller and declining amount of farm housing meant that the contribution of an increasing farm home ownership rate to the overall increase was modest. Consider measuring the contribution of each type of housing as $(own_{d,45}y_{d,45} - own_{d,40}y_{d,40})/(own_{45} - own_{40})$, where own_{dt} represents the owner-occupancy rate in group d in year t and y_{dt} represents d's share of all occupied dwellings at t. By this measure, 99 percent of the increase in home ownership from 1940 to 1945 was associated with non farm as opposed to farm housing. A similar calculation that disaggregates the data into urban, rural non farm, and farm housing attributes 75 percent of the aggregate increase to urban housing (defined as areas with more than 2500 people).

abandonment of deteriorating rental housing, the Census Bureau used cross-tabulations of tenure and year built in the 1950 Census of Housing to estimate that at least 3 million units that were owner-occupied in 1950 had been renter-occupied in 1940 (U.S. Bureau of the Census, 1953). The fact that much of the rise in home ownership involved conversions of the same units is consistent with – although certainly not proof of – the hypothesis that rent control induced landlords to sell their properties for owner-occupancy rather than continuing to rent them out.

In part, the size of the increase in home ownership from 1940 to 1945 was associated with the particularly low level of home ownership in 1940 that followed the foreclosures of the Great Depression. Ratcliff (1944) estimated that there were about 600,000 to 700,000 dwelling units that had been owner-occupied in 1930 but were renter-occupied in 1940 due to foreclosure. A substantial share of these were likely in the hands of financial institutions that may have been unwilling landlords, and hence eager to sell them once prices recovered (Fishback, Rose and Snowden, 2013; Rose, forthcoming). Yet a 'rebound' associated with declines in home ownership over the 1930's does not suffice on its own as an explanation for the rapid increase over the early 1940's, at least from the perspective of understanding the supply of dwellings for owner-occupancy. Illustrating this point, Figure 4 plots the city-level change in home ownership over the first half of the 1940's against the city's change in home ownership between 1930 and 1940, for the sample used in the analysis. As described in more detail below, surveys were carried out at different times between 1944 and 1946, so the figure plots the residuals from regressions of the city-level changes on survey year fixed effects and within-survey-year time trends. There is a slight (and statistically insignificant) negative relationship between the two, but the change in home ownership over the 1930's does little to explain where home ownership increased over the early 1940's: the partial correlation in the sample, after conditioning on survey timing, is -0.067. Similarly, the city-level 1920-30 change in home ownership is, if anything, negatively correlated with the change over the early 1940's, with a partial correlation of -0.227. Hence, a 'return to trend' is also unlikely to serve as an explanation on its own.

Although the focus on rent control in this analysis puts most of the emphasis on the supply side of the market, it may be helpful to note how these home purchases might have been financed.

With incomes rising during the war and little to spend them on, rising personal savings would have helped finance home purchase over the early 1940's. Mortgage lending for home purchase was also active during the war, as aggregate data on home mortgages from the annual reports of the Federal Home Loan Bank Administration from 1941 through 1945 suggest (Federal Home Loan Bank Administration, various years). While overall lending by savings and loans, banks, and trust companies decreased in fiscal years 1942 and 1943, individual mortgagees were a substantial share of lending over this period (in fiscal year 1941, they represented 24 percent of the number of mortgage loans and 16 percent of their dollar volume) and mortgage lending by individuals increased with each fiscal year from 1941 through 1945, except for a slight decline in fiscal year 1943. Moreover, the dollar volume of loans given by S&L's for home purchase increased in absolute terms in each fiscal year from 1941 through 1945, despite a decrease in the total volume of S&L lending driven by declines in loans for construction, refinancing, and reconditioning.

3 Reduced-form estimates

3.1 Data

The results below are based on a sample of 51 cities in which the National Industrial Conference Board (NICB) had begun monthly rental surveys by March 1940, for construction of the rent component of its cost-of-living index.[13] The NICB's choice of sample cities based on characteristics determined prior to 1940 alleviates concerns that the sample would be skewed towards cities experiencing unusual housing conditions during the war. Most of the cities in the NICB sample were relatively large, with all but three having a 1940 population of 100,000 or more. I obtained the values of the NICB rental indices for these cities from National Industrial Conference Board (1941) and from various issues of the Conference Board *Economic Record* and *Management Record*. In 1943 the NICB published revisions to its indices for many of the sample cities; in these cases I use

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[13]There were 56 cities in which the NICB had begun tracking rents by March 1940. For four of these 56 cities there was no tenure data available for the mid-1940's. These were Lansing, Michigan; Lynn, Massachusetts; Oakland, California; and Wausau, Wisconsin (Lynn and Oakland were in fact surveyed, but only as part of much larger areas, without reporting of data at a more disaggregated level). Minneapolis and Saint Paul, Minnesota had separate NICB rent indices but a combined tenure survey; I combine them for my analysis.

the revised values. Throughout the paper, the index is scaled to measure changes in rents relative to their March 1940 level.

City-level data on housing tenure in the mid-1940's come from a set of newly digitized housing surveys carried out by the Census Bureau and the Bureau of Labor Statistics (BLS) between 1944 and early 1947 (Humes and Schiro, 1946; U.S. Bureau of the Census, 1947*a*, 1948). Surveys were carried out in most large cities, as well as smaller cities that were thought to be experiencing serious housing shortages (Humes and Schiro, 1946). Cities were surveyed at different times; most of the cities in my sample were surveyed in 1945 or 1946, and a few in 1944.[14] The measure of home ownership used below is the estimated share of occupied, privately-financed dwelling units that were owner-occupied as of the survey date.[15]

For the main results and the robustness checks that follow, I link information on rents and changes in tenure to various characteristics of cities before and during the 1940's. The newly collected data includes information the characteristics of war housing built, reported in the September 1945 *Consolidated Directory of the Disposition of War Housing* (National Housing Agency, 1945*a*), and the growth in county bank deposits from 1943-44, from the *Distribution of Bank Deposits by Counties* (U.S. Department of the Treasury, 1945). An additional variable of particular interest, which I have for 35 of the 51 NICB cities, is a measure of the degree of house price appreciation between April 1940 and September 1945. This measure comes from unpublished results of a study carried out by the National Housing Agency in the late 1940's (National Housing Agency, 1947), which I obtained from the National Archives. This study is the source of the monthly house price series covering Washington, DC from 1918 to 1947 that was reproduced in Fisher (1951) and used in the 5-city median house price index of Shiller (2005). In addition to the monthly series for Washington, DC, the NHA calculated median asking prices for single-family houses that were not

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[14]In most cases surveys were done within city limits, but in other cases they covered counties or 'areas' combining the city with surrounding communities or other large cities nearby. Levels of home ownership for the same (or very close to the same) geographic area in 1940 are either provided in the surveys, or can be reconstructed using 1940 Census data. The few minor geographic inconsistencies appear not to be a concern. Further details are given in the data appendix.

[15]It excludes public housing projects, commercial rooming houses, trailers, tourist cabins, transient hotels, dormitories and institutions (U.S. Bureau of the Census, 1948). War housing projects were an important part of changing housing markets in urban areas during World War II, but those that were publicly financed are excluded from my measure of housing tenure.

newly built and had no commercial features in approximately 80 cities in April 1940, September 1945, and several months in late 1946.[16] Most of the other data below come from Haines (2010); when possible I use variables reported at the city level, but when necessary I use variables reported for the corresponding county or counties.

Table 3 reports summary statistics on the 51 cities in the NICB sample. To assess the comparability of the NICB sample with other large cities, I also show statistics for the set of all 92 cities that had a 1940 population of at least 100,000. The cities of the NICB sample tended to have greater population in 1940 than those in the larger set of cities and had slightly less rented single-family housing, but they were quite similar in other respects, suggesting that it is reasonable to expect that the effects found in the NICB sample generalize to a broader class of cities. The table also offers a first look at changes in housing markets in the sample cities over the first half of the 1940's. On average, home ownership rates increased by 9.6 percentage points between the 1940 Census and the city's first survey date. This increase followed an average 6.2 percentage point decline over the 1930's and an average 6.6 percentage point increase over the 1920's. Prior to control, the average city had seen a 6.4 percent rise in rents, and with the imposition of control the average reduction in rents was about 2 percent. Average appreciation in home asking prices suggests that the initial reduction in rents understates the true degree of rent control severity, however: in the 35 cities for which a measure is available, the median asking price in September 1945 was, on average, 56 percent higher in nominal terms than it was in April 1940.

3.2   Empirical test and reduced-form results

Since essentially all U.S. cities were placed under rent control during World War II, estimating the effects of rent control on changes in tenure by comparing controlled to uncontrolled areas is not a feasible empirical approach in this setting. Instead I use variation across cities in the initial severity of rent control: the degree to which rent control forced down rents at the time it was imposed. The

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[16]Use of an asking-price series to calculate home price appreciation has obvious limitations. Asking prices may differ from selling prices, and this deviation may vary over the business cycle; houses advertised for sale may also differ systematically from other houses. But Williams (1954), who constructs an asking-price series for Los Angeles from 1940 to 1953, compares his index to one based on actual sales prices and finds that rates of *appreciation* based on asking prices are very similar to those based on actual sales prices, especially over the 1940's.

test takes advantage of the fact that even across cities in which rents rose to similar degrees prior to control – and hence, are likely to have had similar housing demand shocks early in the 1940's – the combination of the OPA's choice of base date and the precise timing of prior rent increases meant that rent control reduced rents by different amounts in different cities.[17] To recapitulate, the idea is illustrated in Figure 1. For each city, the left vertical line marks the base date, and the right vertical line marks the date on which rent control was imposed. The obvious difficulty in comparing Baltimore to Boston is that while rent control had much less initial impact on rents in Boston, this was because Boston had only minimal increases in rents prior to control, and therefore had either a much smaller demand shock or a much more elastic supply of housing than Baltimore. In contrast, the rent indices for Baltimore and Buffalo show similarly large increases in rents from March 1940 to mid-1942. Yet in Baltimore, the choice of base date brought down rents substantially, whereas the choice of base date in Buffalo meant that rent control had much less initial impact on rents. The spirit of the test is to draw comparisons like that between Baltimore and Buffalo, in order to avoid comparisons in which variation in the severity of control was due, for example, to differences in housing demand. In practice, the main specifications implement this idea by controlling for the maximum degree of rent appreciation prior to control.

The specific measure of severity is the percent decline in rents that would be implied by a reduction in rent from its maximum pre-control level to its level on the base date. That is, it is the difference between the pre-control maximum value of the NICB rent index and its value in the base date month, normalized by the pre-control maximum value.[18] Using only rental market data as of the date of imposition of control has the advantage that the measure is less influenced by endogenous decisions that potential sellers, tenants, or buyers make in response to rent control. But since the initial reduction in rents will not fully reflect the incentives faced by market participants over the course of the war, the main estimates should be interpreted as reduced-form estimates of

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[17]It relies on the assumption that the OPA's choice of base date, in combination with the precise timing of rent increases, were not correlated with unobserved factors also driving greater or lesser growth in home ownership. In Section 4, I use both the "default" base date used in the second wave of rent control and a range of placebo base dates to argue that this assumption is reasonable, or that there is if anything a downward bias in the results.

[18]In Figure 1 it is evident that the rent indices did not always fall to the base date values at the time of imposition of rent control: this may be due to unit-specific adjustments to the maximum legal rent or a change in the composition of units in the NICB sample. I use the value of the rent index in the base month rather than after control to avoid contaminating the severity measure with potential responses to rent control.

the effect of rent control. In Section 5, I discuss their interpretation further and offer a plausible scaling of the reduced-form estimates.

The basic regression specification for city i, with first housing survey in year t, is

$$\Delta_{t,40}(\text{home ownership rate})_i = \alpha(t) + \beta(\text{severity})_i + \gamma' \mathbf{X}_i + \varepsilon_i \tag{1}$$

where $\Delta_{t,40}(\text{home ownership rate})_i$ is the percentage point change in home ownership between April 1940 and the first survey date for city i, carried out in month t (although the notation is suppressed in the equation above, t is a function of i). Since the dependent variable is specified as a difference, this specification implicitly controls for time-invariant city characteristics: the right-hand-side controls in the vector $\mathbf{X}_i$ allow for differential trends according to city characteristics. Below, I first show the relationship between severity and home ownership conditional only on survey time effects, which account for the fact that cities were surveyed at different times. Except where specified otherwise, the function controlling for common time effects, $\alpha(t)$, is allowed to be piecewise linear: I include a set of fixed effects for survey year, and linear trends by survey month within each survey year, allowed to be flexible across years. After showing the raw relationship, I estimate specifications that include various covariates in $\mathbf{X}_i$. Among the key controls is the maximum pre-control rent appreciation in city i. By including it I draw comparisons between cities that had similar degrees of rent appreciation prior to control but rent rollbacks of differing magnitude. Other controls included in the baseline specifications are the city's change in home ownership from 1930 to 1940 as well as its change from 1920 to 1930, and three separate controls for the share of housing units in 1940 that were single-family and owned, rented, and vacant. These variables help to capture the possibility that some cities had greater potential for increases in home ownership than others; put differently, they condition on factors in the pre-treatment period likely to be correlated with the elasticity of supply of housing for owner-occupancy.

The relatively small size of the NICB sample makes the downward bias in the usual Eicker-Huber-White robust standard errors a concern. Accordingly, I follow Imbens and Kolesar (2012) and report HC2 standard errors, along with p-values that correspond to a t-distribution with the degrees of freedom adjustment proposed by Bell and McCaffrey (2002).

Table 4 confirms that cities with greater severity had larger increases in home ownership. Column (1) shows the simple relationship between the size of the rent reduction and the change in home ownership, corrected for survey timing (Appendix Figure A1 shows this relationship graphically). The coefficient suggests that an increase in severity of 2.5 percentage points – roughly a standard deviation in the NICB sample – was associated with roughly a 1.1 percentage point greater increase in home ownership during the early 1940's, with a p-value of 0.023.

The main test of the rent control hypothesis is the specification in column (2), which adds a control for the maximum pre-control rent appreciation, as well as the baseline set of controls described above. The relationship between severity and increased home ownership does not appear to be driven by the degree of pre-control rent appreciation (and hence does not merely reflect, for example, differential housing demand growth). The coefficient on severity in fact increases slightly in this specification, and remains significant at a reasonable level, with a p-value of 0.082. The point estimate suggests that severity greater by 2.5 percentage points led to roughly a 1.2 percentage point greater increase in home ownership over the early 1940's (Appendix Figure A2 illustrates this relationship). Finally, in Column (3) I add additional controls to allow differential trends by Census region and by other 1940 city characteristics – house values and rents in 1940 and the nonwhite share of the population. Doing so has little effect on the size or statistical significance of the coefficient on rent control severity.

4 Robustness to alternative explanations

The identifying assumption in the main specifications is that conditional on the maximum pre-control rent appreciation, the reduction in rents induced by rent control was unrelated to underlying city trends that were also associated with differential trends in home ownership. In this section, I offer three complementary pieces of evidence supporting a causal interpretation of the estimates.

4.1 Exploiting the 'default' base date

The variation in the initial severity of rent control in the main specifications comes from the combination of the OPA's choice of base date and the timing of increases in rent. My first approach

to assessing the identification assumption is to use the OPA's "default" base date to isolate variation in rent control severity that does not rely on city-specific choices of the OPA, but instead relies only on variation in the timing of rent increases across cities. I then assess whether the variation in timing of rent increases is likely to be related to unobserved factors that also led to greater growth in home ownership.

The OPA devoted much attention to setting city-specific base dates in the initial wave of rent control imposition in the summer of 1942, and a number of different base dates were used. By October 1942, however, nearly all newly controlled counties were assigned the default base date of March 1, 1942: 92 of 97 counties controlled in October, 197 of 198 counties controlled in November, and 111 of 112 counties controlled in December. Twenty cities in my sample were put under control over these three months, and all shared the common base date. I re-estimate a variant on my main specification on the sample of cities controlled between October and December 1942, making it slightly more parsimonious given the smaller size of the sample.

By controlling for maximum pre-control rent appreciation and testing for differences in increases in home ownership in cities that had sharper initial rent reductions when rolled back to March 1942, this specification exploits variation in the precise timing of rent increases. The spirit of this test is to draw comparisons between two cities in which rents rose by similar amounts prior to control at the end of 1942, but in which rents had risen by different amounts as of March 1942. A city where most of the rent increase happened by March 1942 would have lower severity, and one where most of the increase happened after March 1942 would have greater severity. The identification assumption is that given that cities eventually had similar rent increases before control, the degree of rent appreciation by March 1942 was unrelated to unobserved factors that drove differential trends in home ownership. Below I present evidence supporting this assumption.

The point estimates in Table 5 for this smaller sample suggest that the possible correlation between the OPA's choice of base dates and unobserved factors increasing home ownership does not induce upward bias in the main estimates. The coefficient in column (3), which controls for the maximum pre-control rent appreciation and survey timing in the same way as the main specifications, suggests that a severity greater by 1 percentage point was associated with a 1.6-percentage

point greater increase in home ownership over the 1940's. The point estimate is somewhat larger than those in the main specifications, but given the wide confidence intervals in column (3) the difference in point estimates may not be very meaningful.[19]

The identifying assumption for this robustness check is that conditional on the eventual degree of pre-control rent appreciation, the precise timing of the appreciation was not correlated with unobserved factors that themselves led to greater increases in home ownership between 1940 and the mid-1940's. To evaluate the plausibility of this assumption, I re-estimate the specification in column (3) of Table 5 using months from March 1941 to February 1942 as placebo base dates. If the relationship between rent control severity and home ownership is due to unobserved factors associated with early rent increases, then it is likely that it should not matter whether "early" means prior to March 1942, or instead means prior to September 1941 (to choose one example). That is, calculating rent control severity as the implied reduction to base dates other than March 1942 should produce similar results. Note that since rents are serially correlated, as the placebo dates approach the true base date we should expect them to deliver larger and more significant effects – in the extreme, a "placebo" date one day prior to the actual base date would deliver estimates essentially identical to those from using the actual base date, not really being a "placebo" at all. In the absence of a clear criterion for determining which dates are too close to the true base date to be considered "placebo" base dates, I include results using placebo dates close to the true base date, but the results below should be interpreted with this caveat in mind.

I show the coefficients and confidence intervals for each placebo date as well as the true one in Figure 5. The results support the identifying assumption. Conditional on the eventual degree of rent appreciation, "later" rent appreciation does not appear to be correlated with greater increases in home ownership except when it coincides with greater rent control severity. Using any of the months from March through December 1941 as a placebo base date delivers estimates that are not statistically significantly different from zero, and for which the point estimates themselves are

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[19]While the imprecision of this estimate suggests caution in interpreting its magnitude, it may be that the OPA's choice of city-specific base dates induces a downward bias in the main results. For example, suppose that defense production stimulates demand for rental housing more than for owner-occupied housing because it is expected to be a temporary labor demand shock. If the OPA chose base dates to reduce rents more in cities where rent increases were associated with defense activities, as the institutional background in Section 2 suggests, then greater rent control severity should have been related to greater demand shocks for rental, not owner-occupied, housing.

relatively close to zero. Only when the placebo base date approaches the true base date do the estimates begin to come close to the estimate in Table 5. As noted above, due to serial correlation in rents we would expect to begin seeing larger and more significant effects as the placebo dates approach the true one, which indeed is what is observed in the data.

4.2 Introducing additional controls

There are several other candidate explanations for unusually sharp increases in home ownership rates during the early 1940's, and one might be concerned that these are correlated with the variation in rent control severity even conditional on the degree of rent appreciation prior to control. One possibility is that rising incomes and savings encouraged a shift towards ownership as a form of housing tenure.[20] Another is that the wartime rise in marginal income tax rates made the non-neutralities in the tax code between owner- and renter-occupied housing more relevant than they had been before, driving a shift towards owner-occupancy (Rosen and Rosen, 1980). The abnormally low level of home ownership in 1940, after the foreclosures of the 1930's, may have led to an unusually fast increase as the 'overhang' of properties was sold off. Finally, the war economy may have raised expectations of future growth and encouraged home purchase. As a second approach to supporting a causal interpretation of the main estimates, I assess whether or not these alternative explanations are likely to explain the main result by introducing additional controls that should provide measures of geographic variation in these alternatives. Many of these variables could plausibly be affected by rent control itself, so the coefficient estimates in these specifications should be interpreted with caution; the spirit of this exercise is to see whether inclusion of these variables leads to a substantial reduction in the severity coefficient.

The results shown above, in fact, already address two of these alternative hypotheses. Column (1) of Table 6 reproduces the final column from Table 4. This specification already controls for the city's 1930-40 and 1920-30 changes in home ownership, making it unlikely that the rent control result is driven by depressed levels of home ownership in 1940. It also addresses the possibility that

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[20]As with some of the other explanations listed below, rising income and savings and rent control are not mutually exclusive explanations; the effect of rent control should be interpreted against the backdrop of rising housing demand. The question here is whether the main specifications adequately control for variation in housing demand.

rising marginal tax rates increased home ownership during the war. Work studying geographic variation in housing tax benefits over the 1980's and 1990's (Gyourko and Sinai, 2003, 2004) uses more extensive data than are available for my sample, but as a rough approximation, differential changes in the tax benefit to owner-occupied housing as income tax rates rose during the 1940's should be correlated with city housing values in 1940. Column (1) includes a control for the median owner-occupied housing value in 1940, and as seen in Table 4, inclusion of this control did little to change the size of the severity coefficient. Hence, it is unlikely that spatial variation in the tax benefit to owner-occupied housing explains the rent control result.

I address a range of other concerns in columns (2) through (6), adding controls that are likely to be correlated with income, savings, and expectations of future city growth (in Appendix Table A2 I introduce each individually). There is no data directly measuring city- or county-level personal incomes over this period, so I use two alternative measures: the change in log retail sales per capita from 1939-1948, following Cullen and Fishback (2012), and the change in the log total dollar value of manufacturing wages from 1939 to 1947. As a first proxy for savings, I use the 1943-44 trend in log bank deposits. Although the change in bank deposits relative to 1940 would be preferable, the 1943-44 change should at least provide a measure of the trend in savings growth over the war years.[21] I also include a control for county E-bond sales per capita. Both of these types of savings could of course substitute for saving in the form of housing, but it may be that differential shocks to the ability to save translated into greater saving in all forms. Two final controls in column (2) are the change in log county civilian population from 1940 to 1943 and total county World War II spending from 1940-1945 (normalized by the 1940 population). These variables are meant to serve as additional controls for the size of the housing demand shock across cities. To proxy for expectations of future growth, I control for the share of war housing that was privately financed. The rationale for doing so is that when future demand for housing was uncertain, war housing tended to be publicly financed (National Housing Agency, 1945b). An alternative, which focuses

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[21]As an alternative to using the 1943-44 change in total bank deposits, I have also tried using an estimate of the 1936-44 change in per capita bank deposits, with the 1936 value obtained from Federal Deposit Insurance Corporation (2001). Relative to using the 1943-44 change, the 1936-44 change introduces the complication that population must be imputed for both 1936 and 1944 (I did so by linearly interpolating population for 1936 and using the estimated 1943 civilian population for 1944). The coefficient on rent control severity is similar (and not significantly different) using the alternative measure of bank deposits.

on changes in expectations prior to restrictions on construction, is to use data on city-level building permits before the end of 1941 as a measure of anticipated housing demand at the beginning of the 1940's. I use the number of new housing units authorized in 1940 and 1941, from U.S. Bureau of the Census (1966), normalized by the number of dwelling units in 1940.

The results suggest that the rent control result is not driven by correlated trends in incomes, savings, or housing demand. Including controls for income, savings, county war spending, and population change, in fact, slightly increases the coefficient on severity from .465 to .545. Data on financing of war housing are unavailable for two cities in my sample, so in column (3) I re-estimate the baseline specification omitting these two, and add a control for financing of war housing in column (4). The coefficient of .548 is quite similar to the 0.465 that I estimate in the baseline specification. Data on permits are unavailable for six cities in the sample, so in column (5) I re-estimate the baseline specification omitting these six, and control for permits in 1940 and 1941 in column (6). Although less precisely estimated, the coefficient of 0.412 is also quite similar to the 0.465 of the baseline specification. Altogether, the evidence in Table 6 suggests that these alternative stories do not explain the relationship between rent control severity and increased home ownership.

4.3 Evidence from the timing of imposition

Use of an entirely different source of variation – the timing of rent control imposition across cities – provides a final approach to supporting the interpretation of the main estimates. There is no data on housing tenure at a sufficiently high temporal frequency to use this test to examine shifts into home ownership directly, but changes in the number of newspaper advertisements offering houses for sale is likely to provide a rough measure of the degree to which property owners chose to attempt to sell in response to the imposition of rent control.[22] For one newspaper from each of nine cities, I have collected data on the number of ads offering houses for sale on the first Sunday of each March, June, September, and December from 1939 to 1946. Details on the sample and the data

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[22]A caveat is that in general, more ads need not always mean that more homes are being put on the market, since it is also consistent with homes staying on the market for longer. But in light of the results on severity shown above, this seems a less plausible interpretation of the results.

construction are given in the data appendix. Although the sample is small, there is a reasonable degree of variation in timing of rent control across the nine cities: one was controlled in 1941q4, two in 1942q2, three in 1942q3, two in 1942q4, and one in 1943q4.

The basic specification is

$$\ln(\# \text{ sale ads})_{ct} = \alpha_c + \beta_t + \sum_{\tau=-8+}^{8+} \gamma_\tau \mathbf{1}(\text{control imposed at time } t - \tau)_{ct} + \varepsilon_{ct} \qquad (2)$$

for city c observed in quarter t. This specification controls flexibly for fixed city characteristics and time shocks common across cities. It also allows an assessment of the identifying assumption by testing whether cities appeared to be on different trends in sales prior to the imposition of control. I estimate a separate coefficient for each relative quarter from 7 quarters prior to 7 quarters following control, plus a common coefficient for all quarters 8 or more before and one for 8 or more after control. The omitted category is the quarter immediately preceding the quarter in which rent control was imposed. In the preferred specifications below, I augment equation (2) by controlling for city-specific linear time trends and city-specific season (quarter of year) fixed effects. The motivation for including the latter is pronounced seasonality in advertisements that appears to have been specific to a few cities in the sample. I estimate standard errors clustered by city; following Imbens and Kolesar (2012), I use the Bell and McCaffrey (2002) adjustment for both standard errors and hypothesis testing to account for the small number of clusters (nine).

Figure 6 plots the estimates of γ_τ for the log number of sale advertisements, from a specification that augments equation (2) with city-specific trends and city-season fixed effects. These coefficients and those from alternative specifications are also shown in Appendix Table A3. There is little indication of a differential trend in sale ads pre-dating the imposition of rent control. The coefficients for the first period of control and afterwards indicate a differential uptick in the number of sale ads immediately after the imposition of rent control, with a number of sale advertisements about 0.2 log points higher about a year after the imposition of control. The results are quite imprecise, as would be expected with only nine clusters, so caution is clearly merited in interpreting the estimates. But taken together, they provide suggestive evidence supporting the interpretation of

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the main estimates, that rent control induced property owners to put their properties on the market for owner-occupancy.

5   Discussion

5.1   The effect of rent control on house prices

The main estimates use a measure of rent control severity that reflects the incentives that landlords faced at the time of rent control imposition, which may not necessarily reflect the incentives they faced at the end of the war, when levels of home ownership are observed. As seen in Table 3, house price appreciation was substantial over the first half of the 1940's, raising the return to selling rather than renting out housing. Hence, investigating the possible effects of rent control on house prices is helpful for interpreting the main estimates, and is also of independent interest.

In general it is ambiguous whether rent control should increase or decrease prices in the uncontrolled market for owner-occupied dwellings. To the extent that excess demand from unsatisfied consumers in the rental market spills over into the owner-occupied market, there will be upward pressure on house prices, but if landlords respond by shifting units from rental to owner-occupancy the net effect on prices is unclear.[23] Several other factors may play a role as well. For example, Autor, Palmer and Pathak (2012) note that control of rental units may affect house prices through externalities related to maintenance and residential sorting; they find that the removal of rent control in Cambridge in the mid-1990's raised values of uncontrolled housing.

To assess the effect of rent control on house prices in this setting, I estimate specifications similar to equation (1) with home asking price appreciation from April 1940 to September 1945 as the dependent variable. The measure of house price appreciation is available for 35 of the sample cities. Unfortunately, the NHA's criteria for choosing a sample of cities to survey for house prices were not clearly specified, but it can be said that these 35 cities had similar average growth in

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[23]Gould and Henry (1967) point out that even in the absence of a supply response, the effect of price controls on the price of an uncontrolled substitute depends on the rationing mechanism in the controlled market. Fallis and Smith (1984), however, question the applicability of their analysis to the housing market, noting that the most natural assumptions on how controlled housing is allocated imply that rent control would raise the price of uncontrolled substitutes. More recent discussions relevant to the effect of rent controls on the price of uncontrolled substitutes include Hubert (1993) and Häckner and Nyberg (2000).

home ownership to the full NICB sample of 51 cities (9.6 percentage points for the full set of 51 and 9.4 percentage points for the subsample with price appreciation data), and also that rent control severity is not a statistically significant predictor of the presence of price data either in an unconditional specification or conditional on the main set of controls used above.[24]

The results in Table 7 suggest that cities with greater initial rent control severity saw greater house price appreciation from 1940 to 1945.[25] In particular, the specification in column (3), which contains the full set of controls, gives a coefficient of 3.984 (and a p-value of 0.030), suggesting that an initial level of severity greater by 2.5 percentage points (a standard deviation in the full sample) was associated with appreciation in house prices that was greater by 10 percentage points. In the context of World-War II era housing markets, it appears that rent control tended to increase prices for owner-occupancy. Moreover, the results suggest that because of their effect on house prices, differences in initial severity translated into much greater differences in landlords' relative returns to selling their properties or continuing to rent them out.

5.2 Scaling the reduced-form estimates

Since home ownership is observed around 1945 for each city, the difference between house price appreciation from early 1940 to late 1945 and the appreciation allowed under rent control – that is, the frozen level of rents – provides one possible measure of 'later severity' from the period corresponding to my observation of home ownership. Taking the specification in column (2) of Table 4 and using 'initial severity' (the degree to which rent control reduced rents initially) as an instrument for 'later severity' (the difference between house price appreciation to September 1945 and the frozen level of rent appreciation) yields a coefficient of 0.078, with a standard error of 0.048.[26] That is, a ten percentage point differential appreciation in house prices relative to rents due to the initial severity of rent control was associated with an increase in home ownership that

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[24]For example, conditional on the controls used in column (2) of Table 4, except for survey time effects, the coefficient on severity is 0.539 with an HC2 standard error of 4.091.

[25]Since house price data are for the same time period for all cities, I do not include survey time controls as in the tenure results.

[26]This is calculated on a sample of 34 cities rather than 35, with one city omitted because it lacked data on the number of occupied dwellings on the tenure survey date (not used for this result, but necessary for the back-of-the-envelope calculation in the following section). The first-stage F-statistic for the excluded instrument is 9.05.

was greater by 0.78 percentage points.

If the only channel through which initial rent control severity influenced home ownership was through this measure of the severity of control in September 1945, this scaling is the correct one. One caveat to assuming this exclusion restriction is the possibility that greater initial rent rollbacks allowed tenants to accumulate greater savings, which translated into higher levels of home ownership; however, given the large average increases in house prices and the many other factors inducing greater saving during the war, this channel seems unlikely to have been quantitatively important. Another caveat is that the level of home ownership in the mid-1940's is a stock that reflects the full time path of severity, as opposed to just the severity as of September 1945; if house price appreciation relative to early 1940 was serially correlated within cities over time, however, this may also not be a serious concern. Overall, this scaling of the reduced form estimates seems a plausible one, although other channels through which initial severity may have affected home ownership cannot be ruled out completely.

5.3   How much of the increase in home ownership can rent control explain?

These estimates allow a back-of-the-envelope calculation of how much of the overall increase in home ownership rent control may explain. There are 34 cities for which price appreciation data is available to calculate the scaled estimate and which have estimates of the number of occupied dwellings on the tenure survey date (necessary to calculate a home ownership rate for the cities as a group). In these cities as a group, weighting by each city's share of all occupied dwellings, the increase in home ownership was 7.7 percentage points. Using the coefficient of 0.078 estimated above, the predicted increase in home ownership if the 'severity' of control had been zero in all cities is 2.7 percentage points. This calculation suggests that rent control may explain 65 percent of the increase in home ownership in the sample cities over the early 1940's.

These results suggest that rent control played an important role in increased home ownership over the early 1940's. Although the estimates are specific to this sample, Table 3 shows that the sample cities were in many respects very similar to other large urban areas in 1940, so rent control is likely to have played a similar role in the broader set of cities as well. Extending these results to

smaller urban areas or to rural areas under rent control requires somewhat more caution, although rent control may have had important effects there as well.

6   Conclusion

World War II coincided with extraordinary changes in urban housing markets, of which one of the most notable was the rapid increase in home ownership. While other periods of rising home ownership over the last century have been associated with construction of new housing, the extremely limited amount of construction during the war meant that a substantial share of the increase in home ownership over the early 1940's came from shifting existing structures from renter- to owner-occupancy. This fact shifts the focus to the incentives facing property owners to sell their properties for owner-occupancy rather than to rent them out.

In a new dataset on city-level changes in tenure, rents, and house prices during the first half of the 1940's, I show that conditional on the degree of pre-control rent appreciation, cities in which rent control was more severe at the time of control – as measured by the degree to which control was meant to lower rents – had greater increases in home ownership. This relationship is stable across specifications that allow for differential trends by a variety of pre-1940 characteristics, and is not explained by differential changes in income, savings, tax benefits to home owners, expectations across cities, or endogenous choice of base dates. The estimates suggest that rent control may explain 65 percent of the urban increase in home ownership over the early 1940's.

An open question is what role rent control played in the longer-run increase in home ownership. It is not unreasonable to suppose that it may have had persistent effects: landlords' experience under rent control, for instance, may have discouraged subsequent investment in rental housing. A large increase in home ownership rates as a result of temporary factors may also have affected the political economy of home ownership. For example, home purchases among lower-income groups in the immediate postwar period, when rent control was still in effect in many areas, were cited as one of the reasons behind the introduction of the exclusion of capital gains on the sale of a principal residence in 1951 (U.S. Congress, 1951).

More broadly, the attentiveness of home owners to local politics (Fischel, 2001) suggests that

the rapid creation of a large new group of home owners may have exerted substantial influence on local political decisions well into the postwar period. It would be interesting for future research to explore the effects of the rapid wartime increase in home ownership on local government or on the course of postwar suburbanization.

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Figure 1: Rent index in three cities before and under rent control
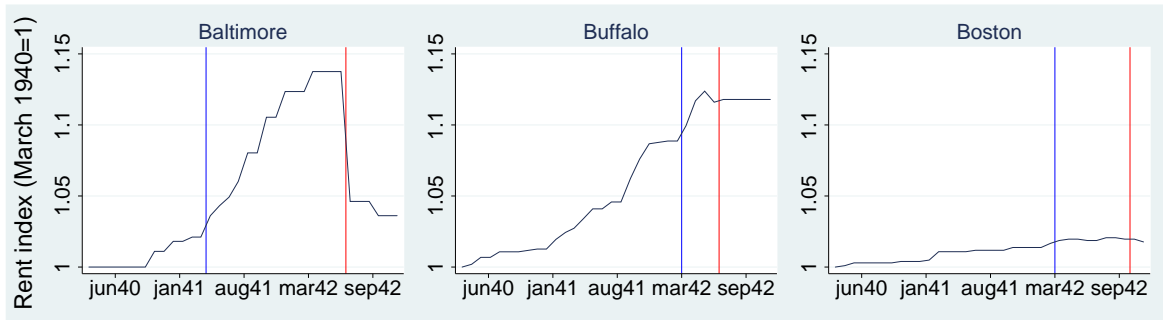
Notes: Rent indices are those published by the National Industrial Conference Board. The left vertical line indicates the 'base' date, the right vertical line indicates the date control was imposed.

Figure 2: Rate of owner-occupancy over the 20th century
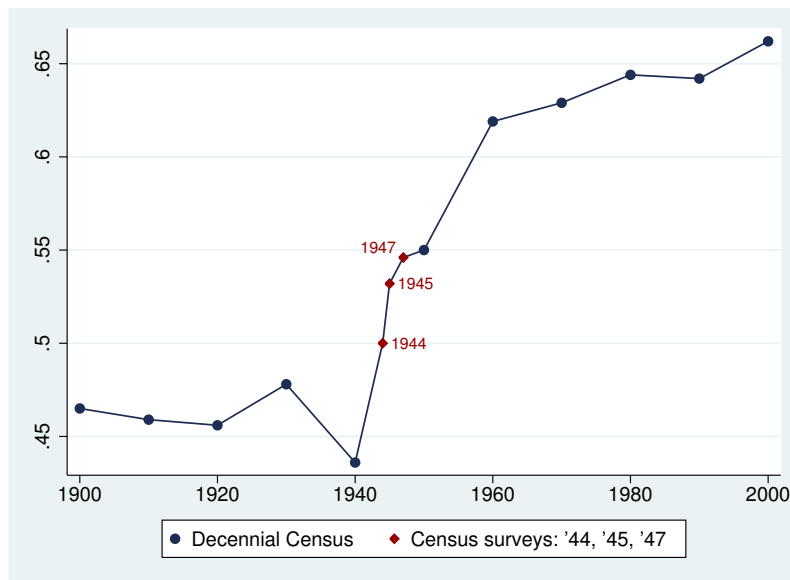
Notes: Figure shows share of occupied dwelling units that are owner-occupied. Data come from the Decennial Census and the October 1944, November 1945, and April 1947 sample surveys for the Monthly Report on the Labor Force (U.S. Bureau of the Census, 1945, 1946, 1947b).

Figure 3: BLS series on number of private nonfarm housing starts, 1920-1950
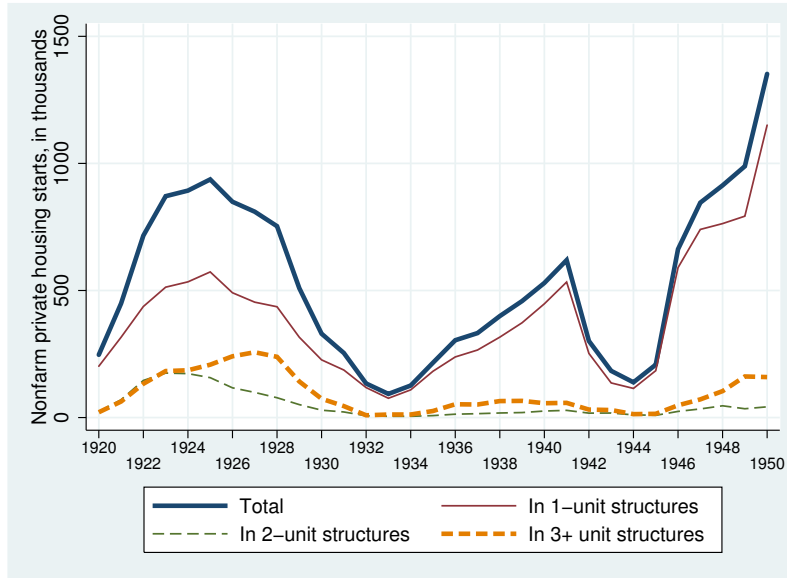
Notes: Figure shows number of private nonfarm dwelling units started by structure type and year. Source: Snowden (2006).

Figure 4: Change in home ownership in the 1930's and the early 1940's
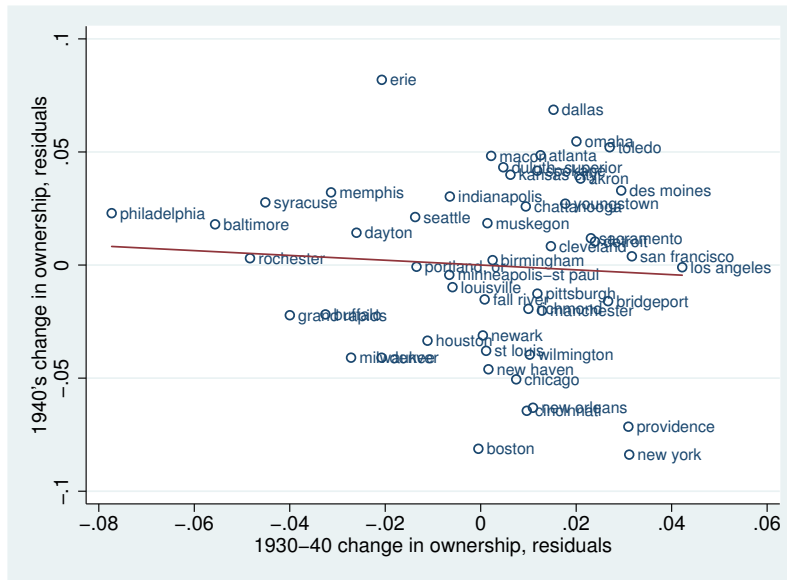
Notes: Figure shows change in home ownership between the 1940 Census and each city's first housing survey against the city's 1930-40 change in home ownership, accounting for different survey timing by plotting residuals from regressing each change on survey year indicators and within-year survey time trends (all housing surveys were carried out between 1944 and 1946). Partial correlation: -0.067.

Figure 5: Coefficients and 90%, 95% confidence intervals using 'placebo' base dates
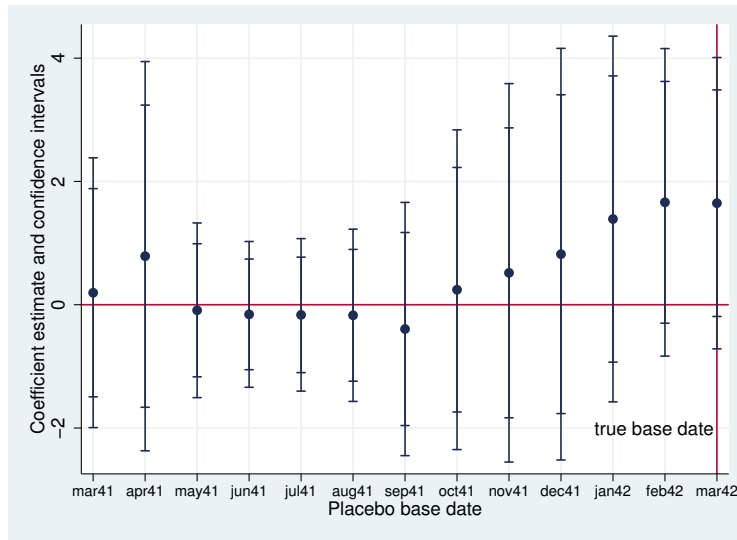
Notes: Graph shows point estimates and 90% and 95% confidence intervals, from estimating equation (1) on the late-controlled sample, using each month on the x-axis as a 'placebo' base date. For all cities, March 1942 was the true base date. Confidence intervals are based on HC2 standard errors and a t distribution with Bell and McCaffrey (2002) degrees of freedom adjustment.

Figure 6: ln(# sale ads) relative to quarter prior to control
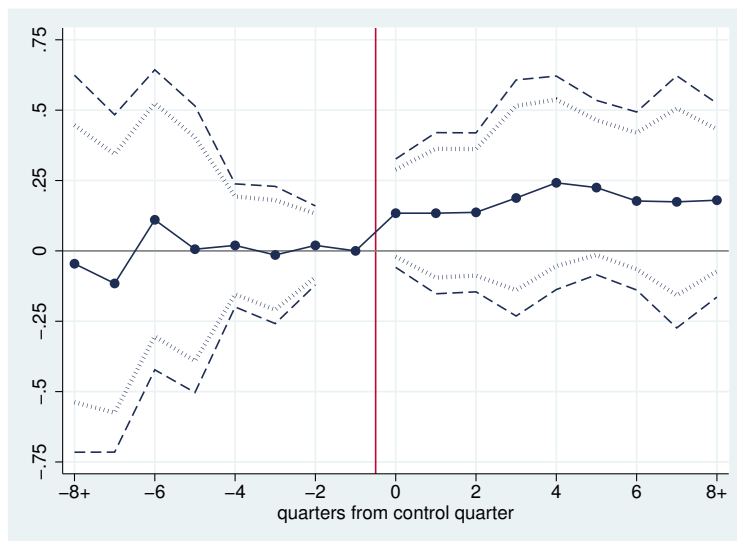
Notes: Graph shows γ_τ coefficients from estimating equation (2) with dependent variable ln(#sale ads), augmented with city-specific linear time trends and city-quarter (season) fixed effects. Omitted quarter is the quarter before control was imposed. Vertical line separates last quarter before control from quarter in which control was imposed. Bands represent 90% and 95% confidence intervals, using Bell and McCaffrey (2002) standard errors and degrees of freedom clustered by city.

Table 1: Counties under rent control, 1942-46

	# counties ever under rent control	share of 1940 population	share of 1940 dwelling units	share of 1940 nonfarm pop
1942	763	0.579	0.587	0.662
1943	848	0.662	0.674	0.762
1944	922	0.690	0.702	0.789
1945	1000	0.712	0.724	0.810
1946	1232	0.775	0.784	0.866

Notes: Rent control information is from the *Federal Register*; county data is from Haines (2010).

Table 2: Changes in tenure, 1940-1950

	all occupied units (millions)			nonfarm occupied units (millions)			home ownership (all)
	total	tenant	owner	total	tenant	owner	
1940	34.9	19.7	15.2	27.7	16.3	11.4	.436
1944	37.2	18.6	18.6	30.8	16.2	14.6	.5
1945	37.6	17.6	20.0	31.3	15.4	15.9	.532
1947	39.0	17.7	21.3	32.4	15.3	17	.546
1950	42.8	19.2	23.6	37.1	17.3	19.8	.551

Notes: 'Home ownership' is share of total occupied units that are owner-occupied. Figures from 1940 and 1950 come from Census of Housing. Intercensal figures are from U.S. Bureau of the Census (1945, 1946, 1947b).

Table 3: Means and standard deviations by sample

	NICB sample	All cities ≥100k
1940 population (thousands)	615.249	412.913
	(1131.136)	(869.219)
Change in log county pop, 1940-43	0.028	0.028
	(0.077)	(0.093)
Share owner-occ, sfd: 1940	0.242	0.247
	(0.118)	(0.121)
Share renter-occ, sfd: 1940	0.152	0.161
	(0.081)	(0.090)
Share vacant, sfd: 1940	0.012	0.013
	(0.006)	(0.008)
Change in h.o., 1920-1930	0.066	0.065
	(0.032)	(0.037)
Change in h.o., 1930-1940	-0.062	-0.060
	(0.029)	(0.034)
Home ownership: 1940	0.353	0.358
	(0.096)	(0.095)
Change in h.o., 1940-first survey date	0.096	
	(0.048)	
Maximum pre-control rent index	1.064	
(March 1940=1)	(0.045)	
Reduction in rent, max to freeze	0.020	
	(0.025)	
Home asking price index, Sep 1945	1.562	
(April 1940=1)	(0.193)	

Notes: NICB sample comprises the 51 cities tracked by the NICB prior to March 1940 with data from the Census / BLS tenure surveys. Data on home asking prices are available for only 35 of the NICB cities. There were 92 cities of population greater than 100,000 in 1940. Share owner-occ, sfd is share of all dwelling units that were single-family detached and owner-occupied in 1940, and similarly for share renter-occ, sfd and share vacant, sfd (the residual category is housing that was not single family detached). Rent and price indices are nominal. Reduction in rent is difference between maximum pre-control rent index and the level on the base date, as a share of the maximum pre-control index.

Table 4: Percentage point change in home ownership, 4/40 to survey date, and rent control 'severity'

	(1)	(2)	(3)
Percent reduction in rent from	0.436	0.469	0.465
max pre-control to freeze level	(0.170)	(0.253)	(0.256)
p-value	0.023	0.082	0.087
Max pre-control rent	no	yes	yes
Year FE / trends	yes	yes	yes
Baseline controls	no	yes	yes
Additional 1940 controls	no	no	yes
N	51	51	51

Notes: HC2 standard errors in parentheses. Reported p-values correspond to a t-distribution with degrees of freedom determined by Bell and McCaffrey (2002) adjustment. 'Max pre-control rent' indicates that the specification controls for the degree of rent appreciation from March 1940 to the pre-control maximum. Year fixed effects / trends are fixed effects for survey year and separate linear trends in survey month for each survey year. Baseline controls are 1920-30 change in home ownership; 1930-40 change in home ownership; 1940 share of dwellings single-family and owned, rented, and vacant (each entered separately). Additional controls are log median house value in 1940, log average rent in 1940, nonwhite population share in 1940, and Census region fixed effects (allowing regional trends). For covariate coefficient estimates, see Appendix Table A1.

Table 5: Rent control severity and change in home ownership for cities controlled 10/42-12/42

	(1)	(2)	(3)
Percent reduction in rent from	2.660	2.256	1.647
max pre-control to freeze level	(0.394)	(0.515)	(0.892)
p-value	0.002	0.010	0.130
Max pre-control rent	no	yes	yes
Year fixed effects	yes	yes	yes
Baseline controls	no	yes	yes
Within-year trends	no	no	yes
N	20	20	20

Notes: HC2 standard errors in parentheses. Reported p-values correspond to a t-distribution with degrees-of-freedom determined by Bell and McCaffrey (2002) adjustment. Baseline controls are 1920-30 change in home ownership; 1930-40 change in home ownership; 1940 share of dwellings single-family and owned, rented, and vacant (each entered separately). Within-year trends only estimated for 1945 and 1946 (all 1944 observations were surveyed in September).

Table 6: Robustness to alternative explanations

	(1)	(2)	(3)	(4)	(5)	(6)
Percent reduction in rent from	0.465	0.545	0.510	0.548	0.412	0.412
max pre-control to freeze level	(0.256)	(0.379)	(0.256)	(0.368)	(0.340)	(0.566)
p-value	*0.087*	*0.170*	*0.063*	*0.159*	*0.244*	*0.480*
Change in city home	-0.214	-0.273	-0.135	-0.358	-0.213	-0.275
ownership, 1930-40	(0.269)	(0.255)	(0.273)	(0.259)	(0.272)	(0.347)
Change in city home	-0.228	-0.177	-0.173	-0.313	-0.241	-0.235
ownership, 1920-30	(0.213)	(0.227)	(0.218)	(0.279)	(0.226)	(0.299)
Log median house	-0.080	-0.096	-0.082	-0.102	-0.088	-0.095
value, 1940	(0.054)	(0.060)	(0.054)	(0.057)	(0.056)	(0.073)
Change in log county		0.047		0.076		0.082
retail sales per capita, 1939-48		(0.146)		(0.142)		(0.167)
Change in log county		-0.037		-0.012		-0.052
manufacturing wages, 1939-47		(0.038)		(0.032)		(0.072)
Change in log county		0.189		0.178		0.120
bank deposits, 1943-44		(0.171)		(0.163)		(0.286)
County per capita		0.023		-0.143		-0.043
e-bond sales, 1944		(0.237)		(0.235)		(0.325)
Change in log county		0.131		0.010		0.153
civilian population, 1940-43		(0.152)		(0.155)		(0.216)
County total WWII		-0.005		-0.004		-0.005
spending per capita, 1940-45		(0.004)		(0.005)		(0.006)
Share of H-1 war				-0.064		
housing privately financed				(0.041)		
Permits in 1940-41 as						0.130
share of 1940 dwelling units						(0.622)
N	51	51	49	49	45	45

Notes: HC2 standard errors in parentheses. Reported p-values correspond to a t-distribution with degrees-of-freedom determined by Bell and McCaffrey (2002) adjustment. All specifications control for maximum pre-control rent; 1920-30 change in home ownership; 1930-40 change in home ownership; 1940 share of dwellings single-family and owned, rented, and vacant (each entered separately); log median house value in 1940; log average rent in 1940; nonwhite population share in 1940; region fixed effects; survey year fixed effects; and within-survey year time trends. Per capita E-bond sales and war spending per capita measured in thousands of dollars.

Table 7: Home asking price appreciation, 4/40-9/45, and rent control 'severity'

	(1)	(2)	(3)
Percent reduction in rent from	2.240	4.310	3.984
max pre-control to freeze level	(1.098)	(1.808)	(1.611)
p-value	*0.086*	*0.036*	*0.030*
Max pre-control rent	no	yes	yes
Baseline controls	no	yes	yes
Additional 1940 controls	no	no	yes
N	35	35	35

Notes: HC2 standard errors in parentheses. Reported p-values correspond to a t-distribution with degrees of freedom determined by Bell and McCaffrey (2002) adjustment. 'Max pre-control rent' indicates that the specification controls for the degree of rent appreciation from March 1940 to the pre-control maximum. Baseline controls are 1920-30 change in home ownership; 1930-40 change in home ownership; 1940 share of dwellings single-family and owned, rented, and vacant (each entered separately). Additional controls are log median house value in 1940, log average rent in 1940, nonwhite population share in 1940, and Census region fixed effects (allowing regional trends).

Appendix 1: Data

51-city sample

As described in the text, the sample for the main regressions comprises repeated observations for 51 cities in which the NICB had begun tracking rents by March 1940. By that point the NICB had actually begun tracking rents in 56 cities, but two, Minneapolis and St. Paul, had a combined tenure survey, and four had no tenure survey (these were Lansing, Michigan; Lynn, Massachusetts; Oakland, California; and Wausau, Wisconsin – Lynn and Oakland were, in fact, surveyed, but only as part of much larger Boston area and San Francisco Bay area surveys).

The tenure surveys provide data at various levels of geographic aggregation, and using them to measure changes relative to 1940 requires some care in identifying comparable geographic areas for the 1940 values. Sometimes survey data is provided for the city boundaries, sometimes for county boundaries, and sometimes for "areas" encompassing multiple cities or a city and surrounding areas. When multiple levels of geographic aggregation are available for a city, I choose survey data according to the following rules of precedence. Having chosen the surveys, I then use the earliest tenure survey available for each city.

(1) When surveys following the city boundaries were available, I used those, and excluded any surveys at the area level. This group included 32 of the final 51 cities.

(2) If no survey at the city level was available but there was a survey at the county level, I used those (and excluded any survey at the area level for the same city). This group included three cities: Los Angeles (Los Angeles County), Cleveland (Cuyahoga County), and Newark (Essex County).

(3) If there was no survey for a single city but there was for a pair of cities (without surrounding areas outside of the cities), I used the pair of cities. This group included two pairs of cities: Minneapolis-St. Paul and Duluth-Superior.

(4) If surveys were not available at either the city or county level, I used data at the "area" level (encompassing the primary city and one or more surrounding smaller areas). This group included the remaining 14 cities in the sample.

For 4 of the 14 surveys at the "area" level, the surveys reported 1940 tenure figures for an area that covered the same area as the intercensal survey, and in 5 of the "area"-level surveys the 1940 tenure figures covered between 95 and 100 percent of the intercensal survey area. Of the remaining five surveys at the area level, four have no 1940 data reported with the intercensal surveys and one has data reported for a 1940 area covering only 85 to 95 percent of the intercensal survey area. For these five, I reconstruct 1940 home ownership numbers for a comparable area using the reported area definitions and the minor civil division data from the 1940 Census, referring to the original survey reports to identify the survey areas when necessary.

A related issue is that for cities that annexed land between 1940 and the first tenure survey and which subsequently had tenure surveys reporting figures for the city, it is not always clear whether the boundaries followed were the original ones or those after annexation. In my sample, there are 8 cities that had annexed more than half a square mile of land between 1940 and the year of the first tenure survey (based on information from various issues of the *Municipal Year Book* (Ridley and Nolting, various years)) and whose survey reported figures at the city level.

Ultimately, any area inconsistencies in the tenure measures do not appear to be a concern. There are 13 cities in which area comparability may be an issue: those that (a) were surveyed at the area level but with coverage between 95 and 100 percent, or (b) were surveyed at the city level and annexed more than half a square mile of land before the first tenure survey. Omitting these 13 cities from my sample strengthens the results reported in the text.

Several control variables are measured at the city level, regardless of the geographic extent of the tenure survey. These include the 1920-30 and 1930-40 changes in home ownership, and the variables on occupancy and tenure by structure type. For three observations I combine two cities to calculate these control variables (these are Minneapolis-St. Paul, Duluth-Superior, and the Fall River-New Bedford area). The NICB rent indices are also measured at the city level.

To calculate control variables at the county level, I aggregated data from all counties in which a sample city (or pair of cities, for Minneapolis-St. Paul and Duluth-Superior) had population in 1940. The one exception is Youngstown, Ohio, which had 99.9 percent of its 1940 population in Mahoning County.

Newspaper advertisements

The data on advertisements of housing for sale comes from nine cities for which digital images of classified ads were available consistently throughout the sample period, with headings in the classified ad section that allow a straightforward classification of ads into offered sales (as opposed to, for example, houses either for sale or for rent). The nine newspapers are the Chicago Tribune, the Cleveland Plain Dealer, the Dallas Morning News, the Los Angeles Times, the New Orleans Times-Picayune, the New York Times, the Portland Oregonian, the Seattle Times, and the Washington Post. In each city, I use the count of ads appearing on the first Sunday of each March, June, September, and December. I include housing in the city or suburban areas. For the New York Times, I exclude ads for housing in suburban areas that were put under control before or after the first wave of rent control in New York City (for example, counties in New Jersey, Connecticut, and some nearby counties in New York State were put under control either earlier or later than New York City was).

I attempt to exclude certain types of advertisements from the count of sale ads. Among the excluded ads are those for the sale of unimproved land; ads for the sale of "income property," such as apartment buildings or farms; ads for summer homes, beachfront or resort properties; ads for exchange of real estate; ads for home building or financing; and ads offering homes "for rent or

sale." A caveat is that sometimes ads for empty lots are included in the same classification as ads for homes in suburban areas. As a rule, the counts include or exclude entire classifications of ads, rather than trying to distinguish between different types of ads under the same classification. Hence, when ads for empty lots are included under classifications that primarily advertise homes, they are included in the total count. Some ads advertise multiple properties, but since many of these do not specify the exact number available, I use counts of the number of distinct advertisements rather than the number of distinct properties.

Appendix 2: Additional Figures and Tables

Figure A1: Change in home ownership and 'severity' of rent control (net of survey timing)
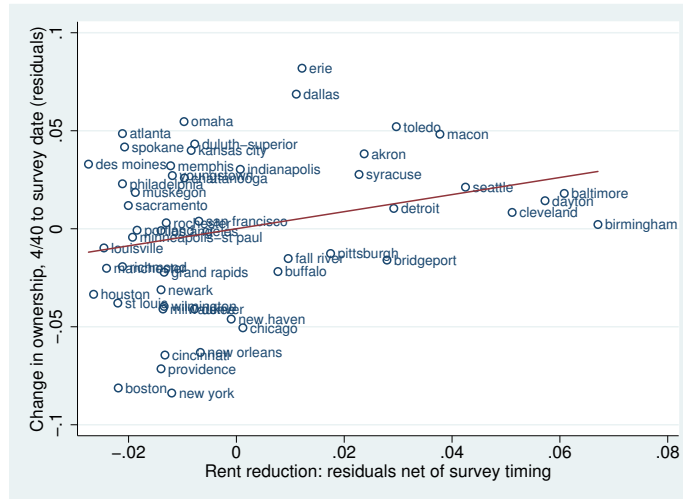
Notes: x-axis shows residuals of percent change in NICB rent index from the maximum pre-control rent to the level on the freeze date, net of survey year fixed effects and linear time trends within survey year. y-axis shows corresponding residuals of percentage-point change in home ownership between the 1940 Census and the first intercensal tenure survey carried out in each city.

Figure A2: Change in home ownership and 'severity' of rent control (net of all baseline controls)
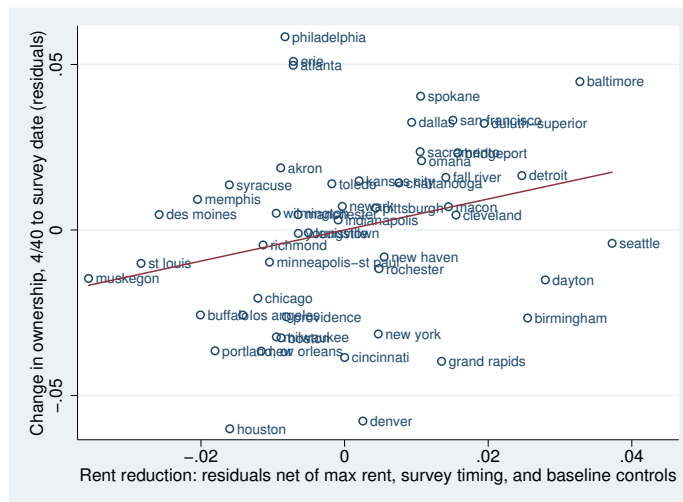
Notes: x-axis shows residuals of percent change in NICB rent index from the maximum pre-control rent to the level on the freeze date, net of all baseline controls. y-axis shows corresponding residuals of percentage-point change in home ownership between the 1940 Census and the first intercensal tenure survey carried out in each city.

Table A1: Change in home ownership, 4/40 to survey date, and rent control 'severity'

	(1)	(2)	(3)
Percent reduction in rent from max pre-control to freeze level	0.436 (0.170)**	0.469 (0.253)*	0.465 (0.256)*
Max pre-control rent appreciation (NICB)		-0.128 (0.158)	-0.134 (0.175)
Change in city h.o., 1930-40		-0.314 (0.278)	-0.214 (0.269)
Change in city h.o., 1920-30		-0.243 $(0.161)^{\dagger}$	-0.228 (0.213)
Share DU's SFD and owned, 1940		0.096 $(0.065)^{\dagger}$	0.178 $(0.109)^{\dagger}$
Share DU's SFD and rented, 1940		0.182 (0.070)**	0.124 (0.136)
Share DU's SFD and vacant, 1940		1.075 (1.062)	0.518 (1.536)
Nonwhite population share, 1940			0.039 (0.105)
Log median house value, 1940			-0.080 $(0.054)^{\dagger}$
Log average monthly rent, 1940			0.056 (0.049)
Year FE / trends	yes	yes	yes
Region FE	no	no	yes
N	51	51	51

Notes: HC2 standard errors in parentheses. P-value stars (†: $p < 0.15$, *: $p < 0.10$, **: $p < 0.05$) correspond to a t-distribution with the conventional degrees-of-freedom ($n - k$). For other notes, see Table 4.

Table A2: Robustness to alternative explanations

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Percent reduction in rent from max pre-control to freeze level	0.465 (0.256)	0.505 (0.304)	0.385 (0.262)	0.578 (0.265)	0.464 (0.267)	0.466 (0.268)	0.476 (0.266)	0.386 (0.267)	0.548 (0.368)	0.373 (0.381)	0.412 (0.566)
p-value	*0.087*	*0.116*	*0.160*	*0.044*	*0.102*	*0.102*	*0.092*	*0.171*	*0.159*	*0.345*	*0.480*
Change in city home ownership, 1930-40	-0.214 (0.269)	-0.224 (0.281)	-0.259 (0.256)	-0.244 (0.268)	-0.214 (0.269)	-0.193 (0.266)	-0.224 (0.272)	-0.299 (0.265)	-0.358 (0.259)	-0.243 (0.312)	-0.275 (0.347)
Change in city home ownership, 1920-30	-0.228 (0.213)	-0.238 (0.222)	-0.261 (0.201)	-0.151 (0.219)	-0.228 (0.215)	-0.225 (0.218)	-0.217 (0.218)	-0.348 (0.232)	-0.313 (0.279)	-0.243 (0.230)	-0.235 (0.299)
Log median house value, 1940	-0.080 (0.054)	-0.073 (0.056)	-0.080 (0.052)	-0.091 (0.050)	-0.080 (0.054)	-0.094 (0.060)	-0.074 (0.051)	-0.107 (0.051)	-0.102 (0.057)	-0.085 (0.057)	-0.095 (0.073)
Change in log county retail sales per capita, 1939-48		0.053 (0.141)							0.076 (0.142)		0.082 (0.167)
Change in log county manufacturing wages, 1939-47			-0.042 (0.032)						-0.012 (0.032)		-0.052 (0.072)
Change in log county bank deposits, 1943-44				0.219 (0.169)					0.178 (0.163)		0.120 (0.286)
County per capita e-bond sales, 1944					0.009 (0.185)				-0.143 (0.235)		-0.043 (0.325)
Change in log county civilian population, 1940-43						0.080 (0.135)			0.010 (0.155)		0.153 (0.216)
County total WWII spending per capita, 1940-45							-0.003 (0.004)		-0.004 (0.005)		-0.005 (0.006)
Share of H-1 war housing privately financed								-0.060 (0.030)	-0.064 (0.041)		
Permits in 1940-41 as share of 1940 dwellings										0.157 (0.419)	0.130 (0.622)
N	51	51	51	51	51	51	51	49	49	45	45

Notes: HC2 standard errors in parentheses. Reported p-values correspond to a t-distribution with degrees-of-freedom determined by Bell and McCaffrey (2002) adjustment. All specifications control for maximum pre-control rent; 1920-30 change in home ownership; 1930-40 change in home ownership; 1940 share of dwellings single-family and owned, rented, and vacant (each entered separately); log median house value in 1940; log average rent in 1940; nonwhite population share in 1940; region fixed effects; survey year fixed effects; and within-survey year time trends. Per capita E-bond sales and war spending per capita measured in thousands of dollars.

Table A3: Newspaper ads: response to imposition of rent control

Dependent variable:	(1) ln(sale ads)	(2) ln(sale ads)	(3) ln(sale ads)
Control imposed:			
8+ quarters later	0.092	-0.044	-0.046
	(0.152)	(0.211)	(0.206)
7 quarters later	-0.038	-0.131	-0.116
	(0.081)	(0.218)	(0.214)
6 quarters later	0.218	0.141	0.110
	$(0.123)^\dagger$	(0.192)	(0.201)
5 quarters later	0.067	0.004	0.006
	(0.121)	(0.197)	(0.194)
4 quarters later	0.069	0.021	0.019
	(0.061)	(0.101)	(0.09)
3 quarters later	0.003	-0.029	-0.015
	(0.077)	(0.104)	(0.102)
2 quarters later	0.068	0.052	0.019
	(0.064)	(0.066)	(0.06)
In that quarter	0.122	0.138	0.134
	$(0.074)^\dagger$	(0.089)	$(0.083)^\dagger$
1 quarter earlier	0.089	0.120	0.134
	(0.109)	(0.132)	(0.12)
2 quarters earlier	0.123	0.170	0.137
	(0.119)	(0.143)	(0.116)
3 quarters earlier	0.127	0.189	0.188
	(0.129)	(0.162)	(0.161)
4 quarters earlier	0.168	0.246	0.242
	(0.119)	(0.155)	(0.145)
5 quarters earlier	0.12	0.211	0.225
	(0.094)	(0.129)	$(0.114)^\dagger$
6 quarters earlier	0.105	0.21	0.177
	(0.097)	(0.133)	(0.113)
7 quarters earlier	0.056	0.176	0.174
	(0.118)	(0.144)	(0.142)
8+ quarters earlier	0.026	0.184	0.180
	(0.117)	(0.118)	(0.105)
City fixed effects	yes	yes	yes
Quarter fixed effects	yes	yes	yes
City-specific trends	no	yes	yes
City-season fixed effects	no	no	yes
N	288	288	288

Notes: Omitted category is quarter immediately preceding imposition of control. Standard errors are clustered at the city level and reported in parentheses. Both standard errors and reported p-values follow Bell and McCaffrey (2002) to adjust for a small number of clusters ($\dagger$: < 0.15, $*$: < 0.10, $**$: < 0.05, $***$: < 0.01).