THE ECONOMIC EFFECTS OF SLUM CLEARANCE AND URBAN RENEWAL IN THE UNITED STATES

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Abstract: The Housing Act of 1949 established a federally subsidized program that helped cities clear areas of buildings for redevelopment, rehabilitate deteriorating structures, complete comprehensive city plans, and enforce building codes. The program ended in 1974, but not before financing over 2,100 projects and generating great controversy. We use an instrumental variable strategy to estimate the program's effects on city-level outcomes. The estimated effects are generally positive and economically significant and are not driven by differential changes in cities' demographic composition. We find no evidence of detrimental effects on residential segregation by race.

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In Title I of the Housing Act of 1949, the United States Congress launched a far-reaching, 25-year effort to revitalize central-city economies. With federal financial assistance and state-delegated powers of eminent domain, local governments were able to assemble, clear, and then sell parcels of land in "blighted" urban areas for redevelopment. They also received support for city planning, code enforcement, and the rehabilitation of structures and neighborhoods. By the time new funding ended in 1974, local authorities had been awarded federal support for more than 2,100 distinct urban renewal projects with grants totaling approximately $53 billion (in 2009 dollars), as well as smaller sums for related activities (U.S. Department of Housing and Urban Development [HUD] 1974a, p. 15).[1]

Although the urban renewal program ended nearly 40 years ago, the economic problems that it attempted to address, the basic policy tools that were employed, and the ensuing political conflict over its implementation are not specific to American cities in this period.[2] Governments in many countries struggle to manage urban growth and modernization while balancing demands for property rights, historic preservation, and housing for the poor (Priemus and Metselaar 1993, Mukhija 2001, Zhang and Fang 2004, Field and Kremer 2005, Wines and Ansfield 2010). The use of eminent domain and public subsidies to facilitate private redevelopment is still highly controversial in the United States, as demonstrated by the sharp response to the U.S. Supreme Court's 2005 decision in *Kelo v. City of New London* (Somin 2009).[3] Given that arguments in favor of such interventions typically rest on claims that the local economy would benefit broadly, a better understanding of the effects of the extensive U.S. urban renewal program would be valuable. Although much has been written about the program, surprisingly few studies have collected and analyzed pertinent data.[4]

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[1] The sum of federal grants for projects understates the magnitude of resources associated with the program's operation for several reasons: the public funds typically just cleared the way for re-development, which entailed an unknown (but large) amount of subsequent investment; the grants covered only a portion of the costs for planning, acquiring, and clearing land; and the range of urban renewal "programs" was broader than specific "projects." After 1974, federal funds were channeled to cities under the Community Development Block Grant (CDBG) program. See Galster et al. (2004).

[2] The program was originally characterized as "slum clearance" and "urban redevelopment," but for brevity and consistency with later terminology, we refer to it as "urban renewal." The Housing Act of 1937 initiated federal funding for public housing and encouraged slum clearance. It did not come close to matching the scale of urban renewal efforts under the Housing Act of 1949.

[3] The *Kelo* decision facilitated redevelopment in New London, Connecticut.

[4] Bingham (1975) is closest in spirit to our work. He uses city-level data from 1960 and 1970 to describe the distribution of urban renewal funds and correlations between housing market variables and urban renewal grants. More recently, Rossi-Hansberg, Sarte, and Owens (2010) identified sizable land value

This paper exploits the substantial degree of cross-place variation in urban renewal activity to estimate the program's effects on city-level economic outcomes. It makes four main advances toward a better understanding of the program's effects. First, because cities planned and undertook the projects, selection issues are likely to confound ordinary least squares estimates; therefore, our empirical strategy features an instrumental variable that legally constrained cities' ability to participate in the program. Second, we compile and examine a new dataset for all cities with more than 25,000 residents in 1950 and 1980, thereby spanning the entire period during which the program operated and the recipients of the vast majority of urban renewal funding. Since urban renewal projects often took several years to plan and execute, spanning the full period is important. Third, to the extent that urban renewal appears to have had effects on city-level outcomes, we examine whether such effects worked primarily through the displacement of residents with relatively low levels of human capital (i.e., changes in population composition) or, alternatively, through channels consistent with economic growth. Fourth, we directly assess whether the urban renewal program systematically affected the degree of residential segregation in central cities and African Americans' labor market and housing outcomes. Such claims are common in the literature about urban renewal, but they are anecdotal and rarely formulated with a counterfactual outcome in mind.

The results suggest a less dismal legacy for the U.S. urban renewal program than is commonly portrayed. It appears that cities that were allowed to engage more actively in urban renewal posted better outcomes in 1980 than they otherwise would have in terms of property value, income, and population growth. These results were not achieved by merely pushing residents with low human capital levels out of the city. Moreover, we do not find evidence that urban renewal exacerbated residential segregation or negatively affected African Americans' population size, labor market outcomes, or housing outcomes. The results do not imply that Title I was the best way to provide aid to central cities by any criterion (economic or otherwise), nor do they imply that the dislocation costs for displaced residents and businesses were unimportant.[5] In fact, these costs and their perceived unfairness were fundamental to the program's demise.

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effects from Richmond's "Neighborhoods in Bloom" program, which, like urban renewal, targeted specific areas for concentrated program interventions.

[5] A full cost-benefit analysis of the program is beyond the scope of this paper. See Rothenberg (1967) for a discussion of the difficulties such an undertaking would entail. Glaeser and Gottlieb (2008) address the potential pitfalls of public policies that target specific places.

1. Background

A Brief History

In the aftermath of the Great Depression and World War II, housing issues rose to the top of the domestic policy agenda, and the elimination of slums and redevelopment of central cities became prominent objectives (Gelfand 1975, Teaford 1990, Fogelson 2001). In 1940, the Urban Land Institute, which was closely connected to the National Association of Real Estate Boards, began publishing city-specific studies that recommended areas for redevelopment (Weiss 1980). As early as 1941, the Federal Housing Administration (FHA) and economists Guy Greer and Alvin Hansen published plans for federally-aided slum clearance and urban redevelopment with many similarities to subsequent legislative initiatives (Foard and Fefferman 1960). Proponents of urban renewal believed that blight was rooted in powerful externalities and was therefore "contagious." They also argued that transaction costs inhibited the effective assembly and redevelopment of land in central cities by private enterprise, that city governments had neither the legal nor the financial resources to undertake large-scale clearance and renewal efforts, and that the problems associated with slums extended beyond city boundaries (Slayton 1966).

Under Title I of the Housing Act of 1949, Congress authorized the Housing and Home Finance Agency (HHFA) to assist locally planned urban renewal projects with grants of two-thirds (or in some cases four-fifths) of the net project cost, where the net cost was defined as the difference between the total cost of acquiring and clearing properties and the income received from re-selling the cleared land. The original 1949 Act emphasized slum clearance and redevelopment of a "predominantly residential" character, but subsequent legislation widened the program's scope to include more rehabilitation and conservation efforts, made exceptions for projects that were not predominantly residential (including hospital and university expansions), and added emphasis on city-wide planning and code enforcement.

The grant application and project execution processes changed over time, but a typical chronology would start with the creation of a Local Public Agency (LPA) that was "enabled" under state legislation to undertake urban renewal activities and to exercise eminent domain powers. The LPA would identify an "urban renewal area" (characterized by "blight" or signs of deterioration), hold public hearings, seek approval from the local government (e.g., city council), and then seek approval from HHFA (or later HUD) to proceed with specific project planning in that area. The project plans would include detailed information on current and proposed land

use, changes in streets and utilities, how displaced residents and businesses would be aided, and estimates of the costs entailed. Once approved, a combination of federal loans and grants would allow the project to proceed.[6] Projects often took several years to complete, and the slow pace of progress was a continual source of frustration.

As of June 30, 1966, the last date on which detailed data are available, approved projects had cleared (or intended to clear) over 400,000 housing units, forcing the relocation of over 300,000 families, just over half of whom were nonwhite.[7] The proposed clearance areas included nearly 57,000 total acres (90 square miles), of which about 35 percent was proposed for residential redevelopment, 27 percent for streets and public rights-of-way, 15 percent for industrial use, 13 percent for commercial use, and 11 percent for public or "semi-public" use (HUD 1966, p. 9).

The urban renewal program began with fairly broad political support, but it became increasingly controversial with time. Critics decried the disproportionate impact on poor residents, the use of eminent domain to trump private property rights, the destruction of cohesive neighborhoods, the loss of historic buildings, and the aesthetics of the new structures (Jacobs 1961, Anderson 1964, Gans 1965, Wilson 1966, and von Hoffman 2000, Gotham 2001). Although national publications, such as *Time Magazine* (Nov. 6, 1964), trumpeted the program's accomplishments well into the 1960s, and proponents responded strongly to the critics and clarified the program's goals and lessons-learned (Groberg 1965, Abrams 1966, Slayton 1966), political support for the program eroded. New funding halted in 1974.

Potential Effects

Once a local urban renewal program was undertaken, how might it have affected city-level outcomes? The most direct impact would be on the areas targeted for clearance, redevelopment, or rehabilitation. By knocking down relatively low-quality housing and commercial buildings, the left-hand tail of the distribution of building quality might be thinned out, and the means and medians of various city-wide measures might rise mechanically. The

[6] This paragraph relies primarily on Slayton (1966) and Groberg (1968). Also see Foard and Fefferman (1960) and Sogg and Wertheimer (1959). We proceed as if the supply of grants was essentially elastic for projects that met HHFA or HUD requirements. In fact, funding was subject to Congressional authorization and therefore to year-to-year changes in funding constraints.

[7] Approximately 54 percent of the displaced families were nonwhite (HUD 1966). For perspective, the 1950 Census of Housing characterized approximately 1.3 million units in metropolitan areas as "dilapidated" (defined in notes to table 1).

implicit model held by proponents of urban renewal, however, emphasized the role of substantial spillovers within the city.[8] Blight was considered geographically contagious, highly detrimental to the well-being of people living in or near such areas, a growing drain on public resources, and both a cause and consequence of middle-class flight and local governments' fiscal problems. It was argued that reversing the fortunes of specific areas would benefit the city through a virtuous circle (e.g., less blight, less outmigration, and higher property values across the city), or at least by short-circuiting the process of deterioration. In this context, recourse to eminent domain was important because assembling sizable areas of urban land through individual negotiations with multiple property owners was costly and subject to holdouts (Davis and Whinston 1960). In addition effects emanating from specific projects, the program's emphasis on code enforcement, city-wide planning, and rehabilitation might have had broader impacts (Carey 2001).

In essence, urban renewal attempted to make central-city locations more attractive to both residents and businesses. In an inter-city spatial equilibrium model with mobile workers and capital and tradable goods, as described by Roback (1982), a higher level of local amenities that are valued by both workers and firms tends to raise equilibrium property values (because for any given wage level, both workers and firms are willing to pay more rent) and to have an ambiguous effect on wages (because for any given rent level, workers are willing to accept lower wages but firms are willing to pay higher wages). The attraction of firms and workers would tend to increase the city's population, which in the presence of agglomeration economies could amplify effects on local productivity.[9] In an intra-city model that features externalities in property upkeep, Schall (1976) shows that renewal-like efforts to raise local housing quality may be unsustainable when the competitive equilibrium is at a relatively low level of quality, in which case targeted local improvements have short-lived effects. But the possibility of multiple equilibria in Schall's model does allow scope for a sustained, positive urban renewal effect. Whether the urban renewal program actually had economically significant effects on American cities remains an open and important empirical question.

[8] Along these lines, Ioannides (2002) finds spillover effects in neighbors' behavior regarding property upkeep in American Housing Survey data from the 1980s. Rossi-Hansberg, Sarte, and Owens (2010) find evidence of spillover effects on property values after targeted neighborhood improvements in Richmond, Virginia. See Brueckner and Helsley (2009) for a model where market failures lead to urban blight and suburban sprawl.

[9] See Glaeser (2008) and Albouy (2010) for recent extensions and applications of the Roback model.

2. Empirical Strategy and the Distribution of Urban Renewal Funds

Data and Framework

The goals of Title I were broad and predicated on the belief that targeted improvements within a city could have positive effects for the city as a whole. Because the goals and hypothesized effects were city-wide in scope and because the program was carried out by city governments, we examine the link between urban renewal activity and city-level economic outcomes reported in the 1980 *Census of Population and Housing*.[10] We collected information on urban renewal activity from the Department of Housing and Urban Development's *Urban Renewal Directory*, which was last published in 1974. For each project in each city, the *Directory* lists the value of federal grants approved and disbursed up to the publication date. This city-level sum includes both urban renewal *projects* and funds for smaller initiatives that were added under the urban renewal *programs* umbrella. The close connection between federal funding and urban renewal expenditure ensures that variation across cities in the volume of federal grants is a reliable indicator of variation in urban renewal activity. We scale the "grants approved" figure by the population of each city in 1950, and all regressions control for city-specific characteristics that might confound the interpretation of differences in grant levels across cities (e.g., pre-existing differences in property values). The instrumental-variable strategy, discussed below, addresses concerns regarding the endogeneity of funding and measurement error in the true intensity of urban renewal activity.

The central empirical question in this paper is whether more intensive urban renewal programs (UR_{ij}, where i represents the city and j the census division) led to noticeably better economic outcomes in 1980 (Y_{ij80}), conditional on each city's economic and population characteristics at the time of the federal program's implementation ($\mathbf{X}_{ij50}$) and eight census-division indicator variables (δ_j). That is, do estimates of β_1 in equation 1 suggest a favorable effect of urban renewal efforts?

$$Y_{ij80} = \alpha + \beta_1 UR_{ij} + \mathbf{X}'_{ij50}\boldsymbol{\beta}_2 + \delta_j + u_{ij80} \quad (1)$$

The main outcome variables of interest are the log of median family income, the log of median value of owner occupied property, the employment rate, and the poverty rate. Subsequent

[10] Detailed city-specific case studies would be a valuable complement to this paper's analysis. See White (1980) for an example; unfortunately, the study's underlying project-location data could not be recovered (personal communication with author). Our search at the National Archives and inquiries to HUD did not uncover a systematic collection of city-specific plans, and so the scope for this approach might be limited.

analyses examine additional outcomes, including population and housing stock variables. The extensive set of pre-program control variables (**X**) includes: *housing stock characteristics in 1950* (the percentage of housing units built before 1920, the percentage that were dilapidated, the percentage that lacked indoor plumbing, the percentage that were crowded, the percentage that were owner-occupied, and the log median value of owner-occupied units); *population characteristics in 1950* (the nonwhite percentage of the population, median educational attainment of those over age 24, and the log of the city's total population); and *economic characteristics in 1950* (log median family income, the employment rate, the percentage of employment in manufacturing, and the percentage of families with income below $2,000 [a proxy for poverty]). The list of control variables always includes the 1950 value of the outcome variable. Summary statistics are in appendix table 1. Sources are described in the data appendix.

The main econometric problem in interpreting an estimate of β_1 as a program effect is that urban renewal projects were initiated, planned, and carried out at the local level. Even with a rich set of city-level control variables and census-division indicators, unobservable city-level differences may be correlated with both the intensity of program participation and subsequent economic outcomes. Cities that were deteriorating relative to others in ways that are not captured by the control variables might have pursued a large volume of urban renewal projects; such cities might have ended up with worse economic outcomes than other cities in 1980 *but* with better outcomes than if they had gone without the funding (the OLS coefficient on funding would understate the true impact of UR funding). Or the opposite case could hold: cities with profitable opportunities might have enthusiastically pursued urban renewal projects. Such places might have posted relatively strong outcomes in 1980 even if the renewal program had no effect.

We address this problem by finding plausibly exogenous variation in cities' urban renewal funding that is due to differences in the timing of state-enacted enabling legislation. Enabling legislation permitted and set legal parameters for the creation of local public agencies (LPAs) that could exercise eminent domain to acquire property for private redevelopment, a prerequisite to undertaking urban renewal projects. This legislation was crucial to the implementation of federally funded urban renewal projects and is often cited in the early social science literature that considers cross-city differences in funding (e.g., Straits 1965, Plott 1968, and Bingham 1975), in historical accounts of urban renewal initiatives (e.g., Teaford 1990, Fairbanks 2002, 2006), in considerations of the program's legal aspects (e.g., Sogg and

Wertheimer 1959, Pritchett 2003), and in urban planning publications (e.g., see Greer and Hansen 1941 and issues of the *Journal of Housing*). Archival material indicates that HHFA (HUD's predecessor) closely monitored the development of enabling legislation because it determined cities' ability to participate in the program.[11]

Given that federally funded urban renewal could not be undertaken without state enabling legislation, that Title I funding ended in 1974, that political opposition to the program increased with time, and that there was learning-by-doing in formulating project proposals, a delay in enabling legislation would effectively narrow a city's window of opportunity for participation. There is historical evidence that state legislative delays constrained the participation of cities that wanted to undertake urban renewal.[12] *A priori*, such constraints might seem most likely to bind for smaller cities with comparatively little political influence, but even large cities were affected by delays in enabling legislation. This interpretation is emphasized in studies by historian Robert Fairbanks, who notes that "When Congress passed the Housing Act of 1949, Dallas and Phoenix civic leaders applauded the new law as an important aid in their fight against bad housing and downtown blight" (2006, p. 309). However, he notes that, "The delay in state-enabling legislation deeply inhibited Dallas's ability to participate in slum clearance and redevelopment during the 1950s and 1960s" (p. 310) and that "Despite the initial burst of enthusiasm, Phoenix, like Dallas, had to wait until state legislators passed the necessary enabling legislation" (p. 315).[13] In other work, Fairbanks argues that "No urban redevelopment would be allowed in Texas until the state passed enabling legislation specifically permitting cities to establish redevelopment authorities. The delay would have dire consequences for many Texas

[11] For example, a memo from C.L. Farris (Chief of Field Operations) to H.S. Keith (Director of Slum Clearance and Urban Redevelopment) dated December 5, 1950 discusses cities that are interested in Title I funds pending the passage of appropriate state enabling legislation. A memo from A. Foard (Assistant General Counsel) to Charles Horan (Area 4 Supervisor) dated May 10, 1951 describes the status of enabling legislation in the West (National Archives, College Park, Maryland, Record Group 207, Urban Renewal Administration, General Subject Files).

[12] In states that did not pass enabling legislation quickly, the *Journal of Housing* reports on repeated efforts, spearheaded by cities, to introduce and pass the legislation. Separately, several cities are listed in the *Congressional Quarterly Almanac* (81st Congress, Volume V, p. 286) as "Communities for S 1070 and HR 4009" (the Housing Act of 1949) that are in states that passed enabling legislation relatively late, implying that they were constrained in their program participation. Bingham (1975, p. 84) reports that later enabling legislation tended to be more restrictive in terms of requirements for referenda on urban renewal programs and projects.

[13] Fairbanks (2006) goes on to note that although representatives from Dallas, Fort Worth, and San Antonio pushed for the state-enabling legislation, representatives from rural areas and various industry groups (e.g., Texas Real Estate Board and Texas Association of Home Builders) worked to defeat it.

cities since it halted implementation of the program in the state and allowed opponents of urban renewal to organize an effective lobby" (2002, p. 186).

In Georgia, South Carolina, and Florida, rulings by state courts forestalled and curtailed the implementation of urban renewal programs. Whereas other state courts interpreted the "public use" qualification for eminent domain fairly broadly when assessing the program's validity, specifically allowing for private redevelopment under Title I, the courts in these three states did not. Later in the paper, we test whether an IV based solely on these judicial decisions confirms the basic results that are based on differences in the timing of effective legislation.

If delays of enabling legislation (1) affected city-level urban renewal participation, (2) did not influence city-level outcomes in 1980 through other channels, and (3) are not correlated with unobserved factors that did influence outcomes in 1980, then the timing of enabling laws may serve as a credible instrumental variable for urban renewal funding.[14] In the paper's next subsection, we focus on examining the first condition—whether enabling legislation affected city-level funding levels. The specific nature of the enabling legislation makes it unlikely to have influenced 1980 outcomes through channels other than program participation (condition 2). Potential confounding relationships (condition 3), such as contemporaneous social and urban programs, secular economic trends, differences in political conservatism, and relevant state policies are assessed in section 3.

Urban Renewal Funding

Table 1 reports ordinary-least-squares regression estimates of equation 2, where UR_{ij} is urban renewal funding per capita (in city i and census division j) and L_{ij} is each city's "years of potential participation" in the federal urban renewal program (defined as the difference between 1974 and the year in which enabling legislation was passed).

$$UR_{ij} = \gamma + \tau_1 L_{ij} + \mathbf{X}'_{ij50}\boldsymbol{\tau}_2 + \lambda_j + e_{ij80} \tag{2}$$

Similar to equation 1, $\mathbf{X}$ is a set of city-level characteristics in 1950, and λ is a set of census-division fixed effects (states are grouped in to nine census divisions). For consistency with the IV regressions that follow, we use a sample of 458 cities with populations of at least 25,000 in

[14] In our main analysis, we use the timing of legislation as reported in Aiken and Alford (1972). This maintains consistency with earlier studies and assures readers that judgment calls in coding have not been influenced by our intention to use the laws in subsequent IV regressions.

1950 and 1980 without missing data on outcome or control variables.[15] If enabling legislation constrained cities' ability to plan, apply for, and receive federal grants, then estimates of τ_1 should be positive.

Table 1's first regression specification (column 1) includes only the census-division fixed effects and L_{ij} (not $\mathbf{X}_{ij50}$).[16] Heteroskedasticity-robust standard errors are adjusted for correlation within states. The results indicate that an additional year of eligibility for participation is associated with 10.55 additional dollars of grants per capita; the standard error is 2.88. In column 2, the estimate of τ_1 is virtually unaffected by adding the full set of 13 control variables for observable city characteristics ($\mathbf{X}_{ij50}$) that one might expect to underpin local demand for urban renewal projects. Rather than being undercut, the estimate of τ_1 increases slightly (from 10.55 to 11.15) and remains statistically significant. *This is consistent with the timing of state enabling legislation affecting local urban renewal funding in a manner that is essentially independent of local characteristics*. This specification corresponds to the first-stage of the baseline IV estimates presented in the paper's next section, where we address concerns about the instrument's excludability from the second-stage at length.

We have tested the robustness of the first-stage relationship extensively. Additional control variables for local political conservatism (proxied by county votes for Barry Goldwater in 1964's presidential election), city tax revenue per capita in 1950, state income per capita, and new federal highway mileage have little influence on the estimate of τ_1.[17] The correlation between funding and enabling legislation also remains strong when omitting cities with over 500,000 residents or the largest city in each state, or when dropping each census division from the sample in turn. Thus, the first-stage correlation reported in table 1, column 2, is not driven by otherwise unobserved differences in political orientation, willingness or ability to tax, cross-state income differences, the sample's most powerful cities, or an idiosyncratic census division.

In summary, legal requirements, historical studies, and archival information indicate that the timing of state enabling legislation constrained cities' ability to participate in the urban

[15] The population threshold reflects the availability of city-level census data in Haines (2004).
[16] Tobit regressions also suggest strong and statistically significant correlations. We focus on OLS results for consistency with the first-stage regressions of the IV estimates that follow.
[17] Goldwater opposed urban renewal programs, and 1964 was the first year the Republican Party's platform criticized urban renewal, so this is a relevant gauge of local conservatism. Nate Baum-Snow kindly supplied highway data from his paper on suburbanization (2007). We lose almost half of the sample when controlling for the change in central-city federal highway miles, but in the reduced sample estimates of τ_1 are similar with and without the highway control variable (12.6 compared to 12.8).

renewal program. Delays in access to the program apparently narrowed the window of opportunity for planning, funding, and carrying out urban renewal projects. This interpretation is supported statistically by a strong link between the quantity of local urban renewal funding and the timing of state legislation, even when controlling for many relevant, observable city characteristics. The first-stage regression results are consistent with a pattern of quasi-random assignment of enabling legislation across cities within census divisions. We further explore the instrument's validity below.

3. Urban Renewal Effects

Basic Results

We estimate the effect of urban renewal programs in equation 1 by instrumenting for funding (*UR*) with the timing of enabling legislation (*L*). In all the regressions we include the set of city-level control variables ($\mathbf{X}_{ij50}$) described above, as well as census-division dummy variables. The implicit first-stage regression results are the same as those in table 1, column 2. The F-statistic for the instrumental variable in the first-stage regression is 13.1, a fairly strong relationship that should mitigate biases associated with weak instruments (Bound, Jaeger, and Baker 1995; Staiger and Stock 1997; Stock, Wright, and Yogo 2002) and with deviations from the assumed exclusion restriction (Conley, Hansen, and Rossi 2008).

The base-specification estimates of β_1 are reported in row 1 of table 2, where each table entry is from a separate regression. The results suggest that urban renewal programs led to higher median income and higher median property values in 1980 at a 5 percent level of statistical significance. The estimated effects on the employment rate and percentage of families in poverty are less precisely estimated, but they are consistent with favorable effects.

For a program that is widely held in low regard, the basic results in table 2 are striking: a $100 per capita difference in grant funding is associated with a 2.6 percent difference in 1980 median income and a 7.7 percent difference in 1980 median property value. The median city in our dataset received $122 per capita in funding, and so the coefficient estimates imply an economically significant impact. Because total investment (a combination of private and public resources) in the wake of federal urban renewal funding is an unknown multiple of the federal grant and because these resources might have been invested elsewhere in the absence of urban renewal programs (e.g., more suburban development), the coefficients from these city-level regressions should be interpreted with care. They are not estimates of the social returns on

investment or local fiscal multipliers. In reduced-form regressions (of Y on L, controls, and census division fixed effects), an extra 5 years of enabling legislation is associated with a 4.3 percent relative rise in property values and 1.4 percent relative rise in median family income. The coefficient on L is significant at the 1 percent level in both regressions.

The IV regression results are substantially more positive than comparable OLS results, which are reported in appendix table 2.[18] Assuming for now that the instrument is valid, this suggests that urban renewal funding was correlated with unobserved *negative* shocks or trends, which lowers the OLS estimates of urban renewal effects.

Robustness to Additional Controls

We first consider scenarios in which the basic estimates of urban renewal effects might be confounded by omitted variables. Essentially, these are circumstances under which the instrument is hypothetically correlated with the error term in equation 1 (violating "condition 3" described above). Later, we test the sensitivity of the results to a substantial recoding of the enabling legislation instrumental variable and to using an instrumental variable that is based on state court rulings that delayed program participation.

First, it is possible that government programs that were coincident with urban renewal influenced city-level outcomes. If participation in these programs were uncorrelated with the timing of state-enabling legislation for urban renewal, then the IV estimates would still be valid. But since some programs were related to urban renewal (e.g., established in related legislation or motivated by similar concerns), we have run regressions that include control variables for the number of public housing units per capita built under Housing Act of 1949; whether the city filed a first-round application for the Johnson Administration's "Model Cities" program; and city-level spending per capita on poverty programs circa 1966. If the IV regression results were simply picking up a positive influence from these other programs, then the additional control variables should diminish the coefficient on urban renewal funding. Instead, the results in row 2 of table 2 show that including the "other program" variables in the regressions tends to increase the magnitude of the coefficient on the urban renewal variable.

Second, variation in participation in public housing, Model Cities, and other programs raises a distinct and potentially confounding issue—the unobservable quality of local

[18] The null hypothesis of exogenous urban renewal funding is rejected at the 5 percent level in the regressions for income, property value, and employment rate.

government. High-quality local governments might be more adept at applying for federal grants, getting the necessary enabling legislation passed, and carrying out other functions that affect city-level outcomes. This interpretation seems inconsistent with the results in row 2, at least to the extent that participation in public housing and Model Cities proxy for local government initiative or administrative capability. It also seems inconsistent with the OLS results, where one might expect to see a positive link between outcomes and urban renewal funding if good local governments were coincidentally good grant writers and effective lobbyists. We have undertaken additional investigation of the government-quality hypothesis by collecting Moody's city bond ratings for 1950 (Porter 1950). The ratings should reflect forward-looking views of cities' fiscal soundness, which in turn depend on the quality of local management and underlying economic prospects. Not all cities had ratings available, and we assigned cities to one of three categories for analysis: relatively high ratings (Aaa to A ratings), relatively low ratings (Baa and below), and "rating not available." The results are similar to those in row 1.[19] We have uncovered no evidence that the quality or characteristics of local government drive the results.

Third, it is possible that differences in the timing of state enabling legislation are correlated with cross-state differences in support for cities. If differences in such support contributed to differences in city-level outcomes in 1980, then the estimates above would exaggerate the effects of urban renewal. Therefore, we collected information on state aid directed to city governments in 1952 from a Bureau of Census publication (U.S. Department of Commerce 1954b). Expressed relative to the urban population's size in each state, this provides a state-level variable that controls directly for differences in state government policy with respect to cities. We also included a control variable for cross-state differences in political conservatism: the state-level percentage of votes for Goldwater in the 1964 presidential election. Row 3 of table 2 reports the IV coefficients on urban renewal funding from regressions that include both of these state-level control variables. Again, the overall results are similar to those in row 1, with somewhat larger coefficients and standard errors.[20] Results are also similar if

[19] The coefficients and standard errors are as follows, where the subscript denotes the regression's dependent variable: $\beta_{value} = 0.000776$ (0.000316); $\beta_{income} = 0.000255$ (0.000113); $\beta_{employment} = 0.00337$ (0.00208); $\beta_{poverty} = -0.00624$ (0.00512). Adding controls for bond ratings in 1982 do not alter the results, nor does controlling for the type of local government (e.g., mayor-council, city manager, nonpartisan elections).

[20] For a slightly reduced sample of cities we can add a city-specific control variable for per capita "aid received" from other governments in 1950 and/or per capita "intergovernmental transfer revenue" in

analogous variables for state-aid to cities in 1962 or 1972 are added.

Fourth, secular shifts in the US economy may have favored some places relative to others. If these differential trends are correlated with differences in the timing of enabling legislation within census division, the estimated urban renewal effects would be invalid. The city-level control variables for economic and population characteristics, including the percentage of employment in manufacturing, should narrow the scope of this problem. We can further address the issue by including a control variable that interacts detailed (three-digit) state-level industrial composition in 1950 with national-level industry-specific growth rates between 1950 and 1980 (Bartik 1991), which we constructed using information from the Integrated Public Use Microdata Series (IPUMS, Ruggles et al. 2008).[21] The census division controls should capture much of the secular rise of the "sunbelt," but we have also included a control variable for mean January temperature which others have found to be a strong predictor of city growth in this period (Glaeser and Tobio 2008). The results are reported in row 4 of table 2 and are similar to the base results in row 1.

Finally, we dropped the largest city in each state from the sample and re-ran the basic IV regressions. This leaves us with a sample of cities within each state that are less politically influential and more likely to find the timing of enabling legislation exogenous to their circumstances and demands. The results are reported in row 5. They are similar to the base results, and in most cases are marginally stronger in magnitude and statistical significance.

None of the robustness checks above suggests that the basic results are driven by omitted factors that are correlated with the instrument and influence the outcomes of interest directly (condition 3). Although it is not possible to completely rule out contamination from unobservable shocks that are correlated with the instrument, such shocks would have to operate differentially across cities within census divisions in a manner that is not captured by the extensive set of base-specification control variables and the subsequent robustness checks.

Robustness to Changing Instruments

We start by replacing the original instrumental variable based on the timing of enabling legislation with two alternative codings. Although we have confidence in the original variable's

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1964-65 (Haines 2004). The results are similar to those without the additional controls. An HHFA document from 1962 listed five states known to provide some direct financial support for urban renewal; adding a dummy variable for this group of states had little effect on the estimates of interest.

[21] The 1950 census microdata are not sufficiently detailed to construct this variable at the city level.

coding reported in Aiken and Alford (1972), we discovered that the underlying sources are not well documented and that the coding used by Bingham (1975) is neither reported in his book nor retrievable (personal communication, July 2008). Therefore, we independently reconstructed the timeline of state enabling laws from a variety of sources, including many issues of the *Journal of Housing*, *Book of the States*, *Municipal Year Book*, archived reports from the Housing and Home Finance Agency (the predecessor of the Department of Housing and Urban Development), and academic articles. Reassuringly, our coding turned out to be similar to that in Aiken and Alford (1972), albeit not exactly the same. A separate concern is that a handful of states passed legislation for a specific city early in the program and then later passed legislation permitting broader participation. This may introduce some within-state variation in the instrument that is a function of city-specific demand. We created two alternative codings that are based on our reading of the legislative history and incorporate adjustments so that all cities in each state are assigned the same "years of exposure" value.

Row 6 of table 2 reports results when all cities in each state are assigned the earliest date of state enabling legislation, and row 7 reports results when all cities in each state are assigned the latest date. Due to differences in our base coding relative to Aiken and Alford and to the shifting of assignments in some states to either the earliest or latest date of state legislation, about 20 percent of cities have a different "years of exposure" value in rows 6 and 7 than in previous rows. Nonetheless, the pattern of regression results is similar to previous rows.

We have also run the regressions replacing the original instrument with an indicator variable that equals one if the state supreme court invalidated enabling legislation due to a relatively narrow interpretation of what constitutes a valid use of eminent domain (Pritchett 2003). This happened in South Carolina, Georgia, and Florida. With this instrument, variation in predicted funding that enters the second stage is driven by differences in judicial interpretations that delayed program participation and created uncertainty about the scope and nature of legally viable projects. The results are reported in row 8. The first-stage relationship between the indicator and funding is strongly negative (F-statistic = 32.2), and the second-stage results are generally similar to those in row 1. When we restrict the sample to southern cities, we get results that are consistent with those in row 8, but with somewhat larger point estimates and

standard errors.[22]

Using both years-of-potential-participation and state court reversals as instruments in the two-stage-least-squares regressions yields estimates that are close to the base results.[23] Both instruments are strongly correlated with funding in the first-stage regression. With more than one instrumental variable, an overidentification test can assess whether the instruments are correlated with the regression errors, which would raise concerns about instrument validity. In each regression, the overidentification test cannot reject the null hypothesis that the instruments are valid; the p-values of the test statistic are generally quite high.[24] In combination with the other robustness checks, this lends additional confidence regarding the credibility of the empirical strategy.

Rural Counties and the Instrumental Variable

If the timing of state enabling legislation for urban renewal strongly predicted property values, income, employment, poverty, and population growth in *rural* areas, it would suggest that the main instrumental variable is correlated with unobserved determinants of economic outcomes, and therefore an invalid basis for making causal inferences (assuming that positive spillovers from urban renewal are not strongly felt in rural areas). To investigate this possibility, we constructed a dataset of rural counties using Haines (2004). Table 3 reports reduced-form regressions of outcomes (Y from equation 1) on enabling legislation instruments (L from equation 2) and control variables for rural counties, defined as counties with less than 25 percent urban population in 1950 (approximately 1,500 counties in 46 states, about half of all US counties). For comparison, in the first row of table 3 we report similar reduced-form regressions for the cities examined above, where there is strong evidence of a correlation between outcomes and years of potential participation in urban renewal. In subsequent rows (county data), the results show no evidence of a strong relationship between urban renewal enabling legislation and

[22] The coefficients and standard errors are as follows, where the subscript denotes the regression's dependent variable: $\beta_{value} = 0.00125$ (0.000499); $\beta_{income} = 0.000370$ (0.000251); $\beta_{employment} = 0.00696$ (0.00375); $\beta_{poverty} = -0.00734$ (0.00684). There are 117 southern cities in the regressions. Standard errors cannot be clustered by state in this case but are similar if clustered by metropolitan area or county codes.
[23] The coefficient estimates (and standard errors) are: 0.00081 (0.00030) for property value; 0.00025 (0.000089) for income; 0.0044 (0.0022) for employment rate; and -0.0068 (0.0044) for poverty rate.
[24] The p-value on the Hansen J-statistic (overidentification test) is: 0.83 in the property-value regression; 0.97 in the income regression; 0.39 in the employment regression; and 0.79 in the poverty regression.

outcomes in rural counties.[25] The rural county estimates never approach statistical significance, and they are often small and have the opposite sign relative to the city-level results.

Channels of Influence

If the urban renewal program affected city-level economic outcomes, as suggested above, how were these results achieved? As mentioned earlier, one can imagine urban renewal's effect on city-level variables working through a narrow, mechanical, and perhaps even perverse channel—essentially lopping off the left-hand tail of the housing-quality distribution and driving people with low levels of human capital and earnings out of the city. We will refer to this as the "displacement channel," which works by altering the composition of the city's population. Of course, a mechanism that merely displaces the poor from the city is quite different from the mechanism touted by proponents of urban renewal, who argued that it could impart a virtuous circle of renewal and growth, or at least dampen an ongoing circle of deterioration. For short, we will refer to this as the "renewal and growth channel." These two channels of influence are not mutually exclusive. Displacement of the poor, for example, could occur as a byproduct of rising property values, which in turn are anchored by gains in local amenities or productivity.

We shed light on these issues in two ways. First, in IV regressions that are similar to those above, we directly examine whether urban renewal funding affected city-level proxies for the "displacement" or "renewal and growth" channels (table 4). Then, to see if the basic results in table 2 are driven (partially or wholly) by changes in population characteristics, as the displacement hypothesis would suggest, we add control variables for potentially endogenous population characteristics (observed in 1980) to the base regressions from table 2. If the displacement channel were the primary means by which urban renewal affected outcomes, we would expect to see a sharp diminution of the point estimates on urban renewal funding (table 5).

The first two columns of table 4 estimate the effect of urban renewal on the median schooling level of the adult population and the black percentage of the population. If urban renewal worked by disproportionately displacing from the city those with low levels of human capital or minority residents, one would expect the urban renewal coefficient in the schooling regression to be strongly positive or the coefficient in the black-share-of-population regression to be strongly negative. We find no support for these hypotheses. The estimated effect on

[25] The county-level control variables are similar to those in our city-level regressions, but not exactly the same due to the availability of variables in Haines (2004). See table 3 notes.

schooling is small and imprecise, and there is no reduction in the black population (the point estimate is positive), even though black residents were disproportionately displaced from renewal areas *within* cities.[26]

The next four columns of table 4 assess aspects of the "renewal and growth" channel. The last two columns, under "City Growth," report estimates of the urban renewal effect on the overall size of the population and housing stock in 1980, controlling for their size in 1950, changes in land area, and the base set of 1950 control variables (**X**). The estimates suggest that the urban renewal positively affected city population and housing units; a $100 per capita increase in urban renewal funding is associated with about a 9 percent increase in population and housing in the base specifications. The point estimates are notably smaller, however, when only the court reversal instrument is used (row 8). In the middle two columns, under "Housing Stock Characteristics," urban renewal is associated with a lower proportion of old housing units in 1980 (i.e., units built before 1940). The coefficient suggests that an additional $100 per capita in funding decreased the share of old housing by 3 percentage points. The estimate of the effect on the percentage of units without full plumbing is also negative, but it is relatively imprecise.[27]

The weight of evidence in table 4—the absence of a discernable effect on the population's composition in terms of race or education level, the relative increase in city population size and housing stock, and the relative decline in the share of old housing units—is not consistent with a simple story in which urban renewal perversely raised city-wide outcome variables through displacement of those with relatively low levels of human capital. They are, however, consistent with the operation of the alternative "renewal and growth" mechanism. In particular, higher wages, property values, and populations are consistent with higher local productivity in the Roback model.

In table 5, we re-run the base regressions from table 2 and add control variables that proxy for displacement: the racial and educational composition of the city in 1980. The additional control variables may be endogenous to urban renewal, and so the resulting coefficients on urban renewal funding are no longer interpreted as estimates of the program's

[26] In a similar regression for the proportion of adults (age 25 and up) with less than four years of high school completion (rather than median schooling), we also get a small, imprecise point estimate.
[27] The census did not classify housing units as "dilapidated" in 1980, but in 1950 the correlation between the percent of units dilapidated and the percent of units without full plumbing was 0.77. In results that are omitted for brevity, we find no effect on the percent of housing units that were owner-occupied.

overall effect. Rather, the point is to see whether changes in city population characteristics underpin the coefficients estimated in table 2's baseline regressions. The first row of table 5 simply replicates the original results from table 2 for easier comparison; the table's second row adds controls for the percentage black in 1980 and median schooling in 1980. The coefficients on urban renewal are slightly *larger* in the augmented regressions, and so it seems highly unlikely that the positive estimated effects of urban renewal in table 2 are driven by changes in the city's basic population characteristics.

4. Race and Urban Renewal

Urban Renewal's Effect on Residential Segregation

It is possible that the urban renewal program had positive effects on average city-level outcomes but simultaneously had negative effects on specific population groups. African Americans made up about 17 percent of central-city residents in 1960 (U.S. Department of Commerce 1963, p. 1), but they made up slightly more than half of the families displaced by urban renewal projects that were approved by 1966 (HUD 1966, p. 9). Stories of predominantly black neighborhoods that were transformed or even dismantled by urban renewal are not difficult to find (Saunders and Shackelford 1998, Carey 2001, Schuyler 2002), and the colloquial likening of urban renewal to "negro removal" is both evidence of and fuel for the view that the program had pronounced negative effects on African Americans.[28]

Relatively early recognition of race-specific issues pertaining to urban renewal, including a hypothesized link to heightened residential segregation, appears in the 1959 *Report* of the U.S. Commission on Civil Rights, which states that: "The clearance of slums occupied largely by Negro residents and their replacement with housing accommodations beyond the means of most Negroes gives rise to the question whether slum clearance is being used for 'Negro clearance.' Small areas occupied by Negroes may be selected for urban renewal, forcing them to move into other areas that are predominantly Negro, thereby reinforcing or perhaps establishing for the first time strict patterns of residential segregation" (p. 488). The *Report* did not develop or explore evidence on the question, nor has the subsequent literature that tends to take the connection for granted and to bundle urban renewal with public housing, redlining, and various other policies in

[28] Writer James Baldwin famously used the phrase "negro removal" in an interview in 1963, but the phrase predates that interview. It was used in a New York Times article about urban renewal on August 30, 1962 and in reference to the National Recovery Act in the 1930s (Arnold 1962, Moreno 2007).

a broad assignment of blame for residential segregation in the post-war era.[29] The hypothesis is particularly important given evidence of the negative effects of residential segregation on African-Americans' socioeconomic outcomes (Massey and Denton 1993, Cutler and Glaeser 1997, Collins and Margo 2000, Ananat 2009).

We investigate whether there is an empirical link between the extent of urban renewal activity and the level of residential segregation in 1980 as measured by the city-level dissimilarity index. The dissimilarity index, described by Duncan and Duncan (1955), is widely used in quantitative studies of segregation (Massey and Denton 1995; Cutler, Glaeser, and Vigdor 1999). It ranges between zero and one, and it can be interpreted as the proportion of the black population that would have to move to a different neighborhood in the city to achieve a balanced distribution of blacks and non-blacks across neighborhoods. We calculated segregation levels for each city using the original tract-level information for 1980 produced by the U.S. Census (1982, Summary Tape File 3A).

Table 6 reports results from three regressions estimated using the instrumental variable approach described above. The hypothesis is that higher levels of urban renewal activity between 1949 and 1974 resulted in higher levels of residential segregation at the program's conclusion, conditional on 1950 city characteristics and census-division indicators. For the full sample of 458 cities, the point estimate is negative and the standard error is relatively large; there is no statistically significant relationship between urban renewal funding and segregation. Because relatively few cities had census tracts defined in 1950, adding a control variable for pre-program segregation sharply reduces the sample size. In column 2, we exclude the cities that have no segregation index available for 1950, but we use the same regression specification as in column 1. The coefficient changes sign but remains small and statistically insignificant. In column 3, we add the control variable for the level of residential segregation in 1950 from Cutler, Glaeser, and Vigdor (1999). The coefficient on urban renewal funding is similar to that in column 2. In sum, the regression results do not provide support for the view that the degree of residential segregation in cities was strongly influenced by the extent of urban renewal activity, but the relatively large standard errors do not allow us to dismiss the hypothesis completely. For

[29] See Hirsch (2000), Carr and Kutty (2008), and Squires (2008) for recent examples. The claim is also a recurring theme in Pritchett (2003). A report by the Chicago Urban League concludes that "urban renewal, as conducted up to now, in Chicago, is working great and undue hardships on the Negro population, and, on balance, is working more and more harm on the city as a whole" (1958, p. 2).

example, at the upper edge of the 95 percent confidence interval of column 3's regression, a $100 increase in renewal funding per capita would raise the dissimilarity index by 0.06, which is small but not trivial compared to the sample average index value of 0.74 in 1950.[30]

For more perspective on the potential relationship between residential segregation and urban renewal, it is useful to acknowledge two additional facts: 1) the majority of the twentieth-century rise in urban residential segregation occurred between 1920 and 1950, before the urban renewal program started; 2) the average level of residential segregation declined between 1950 and 1980, particularly after 1970 (Cutler, Glaeser, and Vigdor 1999). The timing of the rise and fall of residential segregation and the cross-city econometric analysis in table 6 strongly suggest that the federal urban renewal program contributed little, if anything, to the high and potentially destructive levels of residential segregation in American cities.

Urban Renewal and African Americans' Economic Outcomes

Data limitations make it difficult to study outcomes for persons who were actually displaced by urban renewal projects (Hartman 1964, U.S. Housing and Home Finance Agency 1966). However, we can use race-specific data collected from the published census volumes to assess whether urban renewal activity had a discernable impact on the housing and labor market outcomes of black central-city residents. The regression specifications in table 7 are similar to those in tables 2 and 4, except that the outcome variables in 1980 are now specific to African Americans. The standard errors for some of the estimates are large, but the pattern of results is consistent with earlier findings. On average, more urban renewal funding is associated with higher black-owned property values and family income, faster population growth, fewer old housing units, and (less precisely estimated) higher employment, less poverty, and fewer units with inadequate plumbing. In panel A, the point estimates are larger than the corresponding estimates from table 2. Far more detailed information would be necessary to undertake closer within-city examinations, but one could speculate that the relatively large point estimates reflect the proximity of black residents to redeveloped areas, where effects on property values and

[30] In the 75 city sample, F-statistic on the excluded instrument is 2.3, but the instrument is statistically stronger when specified as the earliest or latest year of state enabling legislation (12.1 and 4.5 respectively), rather than the base coding from Aiken and Alford (1972). In these regressions, the coefficient on urban renewal is smaller in magnitude than that in column 3 of table 6, and the upper edge of the 95 percent confidence interval is between 0.0003 and 0.0004, rather than 0.0006.

employment opportunities may have been concentrated.[31] The difference also partly reflects the change in sample composition—the point estimates from table 2 rise somewhat when restricted to the set of 346 cities available in table 7's sample.

It appears that median schooling levels among African Americans tended to rise slightly with urban renewal. By itself, this does not account for the higher black income associated with urban renewal (i.e., controlling for median black education in 1980 only slightly diminishes the urban renewal coefficient in the income regression). Moreover, we think the rise in education is unlikely to reflect a simple displacement-channel story because it happened in the context of faster overall black population growth. Although the migration patterns of less-educated African Americans cannot be evaluated with city-level census data, we assembled some information using state-of-residence and state-of-birth data from the 1980 IPUMS (5 percent sample).[32]

Regressing the net in-migration rate of low-education blacks on census division dummies and urban renewal funding per urban capita (state level), instrumented with the earliest or latest year of enabling legislation in that state, yields urban renewal coefficients that are positive and statistically significant.[33] That is, within census divisions, plausibly exogenous variation across states in urban renewal activity was positively correlated with in-migration by African Americans with low education levels. This result is weakened but not overturned by controlling for state income per capita in 1949.[34] The evidence is not definitive, but it diminishes the plausibility that outmigration by less-educated African Americans accounts for table 7's results.

For another perspective, we calculated the high-education proportion of blacks residing in each state and the high-education proportion of blacks born in each state; the difference is a

[31] Rossi-Hansberg et al. (2010) provide a detailed examination of recent renewal-type projects in Richmond, where the land value effects diminished with distance from the project. Collins and Smith (2007) find concentrated property value effects of riots in Cleveland. See Bartik (1991), Bound and Holzer (2000) and Hoynes (2000) regarding the relative sensitivity of black labor market outcomes to local economic conditions.

[32] We defined less-skilled black migrants in the IPUMS sample as US-born blacks, age 21 to 59, with less than 12 years of education, and with a different state-of-residence than state-of-birth. When the migrants are counted by birth-state, this gives the number of out-migrants; when counted by state-of-residence, this gives the number of in-migrants. The difference for each state is net migration, which we scale by the number of blacks who were born in the state and had less than 12 years of education.

[33] Urban population in each state is from the 1950 Census of Population, volume II, part 1, table 13. The F-statistic on the excluded instrument in the state-level regressions is large (>20).

[34] Without controlling for state income per capita, the coefficient on urban renewal funding is 0.009 (0.004); controlling for log 1949 income reduces the coefficient to 0.006 (0.004). Based on the 0.006 coefficient, a one-standard-deviation increase in urban renewal funding is associated with a 0.43 standard-deviation increase in the low-education immigration rate.

rough measure of how migration affected the skill mix among African Americans (e.g., 63 percent of the sample's blacks who resided in Pennsylvania had 12+ years of school, but 70 percent of blacks born in Pennsylvania had 12+ years of school; we attribute the difference to net migration). In an IV regression with census division fixed effects and log income per capita in 1949, the estimated effect of urban renewal on the migration-driven change in skill mix is small, negative, and statistically insignificant.[35]

This section's results suggest that urban renewal programs did not have a discernable negative impact on average black labor market outcomes or housing characteristics observed in 1980. We reiterate that the analysis here is informative about the average economic outcomes for a population group that was disproportionately affected by the program's operation, but it is not informative about the costs incurred by those who were actually displaced by urban renewal projects. We remain mindful of Paul Douglas's reflection that "The clearance of slums without the requirement for full replacement…forced poor people to carry an unfair share of the burden of rebuilding America's cities."[36] Additional research on this topic and the distributional effects of urban renewal would be valuable.

5. Conclusion

Title I of the National Housing Act of 1949 funded a widespread effort to confront urban decay and encourage central-city redevelopment. In a 50-year anniversary retrospective on the 1949 Housing Act, Robert E. Lang and Rebecca R. Sohmer wrote, "The consensus is that Title I urban renewal mostly failed, in part because large-scale slum clearance proved a crude and largely unworkable redevelopment method" (2000, p. 296). In the same volume, Jon C. Teaford highlighted the usefulness of lessons learned from urban renewal and examples of well-regarded projects, but he also noted "…the chief product of Title I was a widely held commitment never to have another Title I" (2000, p. 463). The program's operation, particularly the scale of displacement and the pace of redevelopment, sowed the political seeds of its demise. The end was hastened by the impression—punctuated by riots, rising crime, and municipal fiscal crises—that cities continued to deteriorate despite billions of dollars of urban renewal expenditure.

Nonetheless, the fundamental policy issues and tools associated with urban renewal

[35] The coefficient on urban renewal funding is -0.00008 (0.00029).
[36] This quote is from the foreword to Groberg (1968). Senator (and economist) Douglas was writing as the Chairman of the National Commission on Urban Problems.

remain highly relevant, and there is much to be learned about and from the extensive U.S. experience with government-facilitated urban redevelopment. This paper provides new econometric evidence regarding the urban renewal program's effects, which appear to have been significantly more favorable than commonly portrayed, at least by some basic metrics of local economic vitality. From a city-level perspective, our estimates suggest that urban renewal led to higher median incomes and property values, faster population and housing stock growth, a lower proportion of old housing units, and (less precisely estimated) a higher employment rate, lower poverty rate, and lower proportion of housing units without full plumbing. The patterns are consistent with the program spurring more central-city growth than otherwise would have occurred, rather than simply demolishing the left-hand tail of the housing quality distribution and pushing low-earning residents out of the city. The results do not suggest that urban renewal systematically affected residential segregation levels or adversely affected African Americans' economic outcomes. However, many questions remain about the costs borne by displaced persons and businesses.

The paper's findings advance the broader understanding of the economic forces and policies that shaped American cities in the twentieth century. The urban renewal program appears to have improved city-level outcomes in certain dimensions, but of course it did not fully offset the factors that drained population and economic vitality from many cities in the period studied (see Cullen and Levitt 1999, Baum-Snow 2007, and Boustan 2010). More generally, the results are consistent with studies that suggest that targeted local investments may have sizable economic impacts, perhaps due to the strength of externalities and spillovers (cf., Rossi-Hansberg et al. 2010; Greenstone et al. 2010). This argument is commonly made in legal and political settings to justify local policy interventions and the use of eminent domain authority, but causal evidence supporting the claim is relatively scarce. Examining similar policies in other settings, undertaking further explorations of the causal pathways, and measuring the distributional consequences of such policies are important avenues for further research.

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Table 1: Urban Renewal Funding and Enabling Legislation

	1	2
Years of potential UR participation	10.548	11.153
	(2.884)	(3.077)
Percent units owner-occupied 1950	-----	-2.276
		(1.591)
Ln median property value 1950	-----	-70.259
		(106.125)
Percent units dilapidated 1950	-----	1.133
		(3.217)
Percent units built before 1920	-----	-0.165
		(1.160)
Percent units w/o full plumbing 1950	-----	-0.437
		(1.642)
Percent units crowded 1950	-----	0.059
		(2.418)
Ln population 1950	-----	-3.446
		(15.575)
Percent population nonwhite 1950	-----	1.354
		(1.443)
Percent employment in manufacturing 1950	-----	-1.126
		(1.611)
Percent labor force employed 1950	-----	-11.355
		(7.292)
Median years schooling 1950	-----	5.850
		(24.697)
Ln median family income 1950	-----	-11.644
		(210.828)
Percent families w/ income <$2k 1950	-----	-1.209
		(4.801)
Census-division dummies	Yes	Yes
Observations	458	458
R-squared	0.104	0.136

Notes: The dependent variable is cumulative Title I grant approvals per capita at the city-level between 1949 and 1974. Heteroskedasticity-robust standard errors corrected for correlation within states are reported in parentheses. A "dilapidated" unit had "serious deficiencies, was rundown or neglected, or was of inadequate original construction, so that it did not provide adequate shelter or protection against the elements or endangered the safety of the occupants….was below the generally accepted minimum standard for housing and should be torn down or extensively repaired or rebuilt" (Bureau of Census, 1954, volume II, part 1, p. XIV). Units without full plumbing are those without "complete plumbing for exclusive use." "Crowded" units are those with more than 1 person per room. Median property value pertains to owner-occupied housing. Summary statistics are in appendix table 1.
Sources: See data appendix.

Table 2: Urban Renewal Effects on Property Value, Income, Employment, and Poverty in 1980

	Ln median property value	Ln median family income	Employment rate	Poverty rate
Panel A: Basic specification				
1: Controls for 1950 characteristics, census division	0.000774 (0.000327)	0.000256 (0.000113)	0.00328 (0.00213)	-0.00619 (0.00513)
Panel B: Additional controls				
2: Add controls for public housing, poverty spending, Model Cities	0.00110 (0.000506)	0.000432 (0.000219)	0.00598 (0.00319)	-0.0128 (0.00940)
3: Add controls for state aid to cities and support for Goldwater	0.00106 (0.000398)	0.000287 (0.000145)	0.00690 (0.00374)	-0.00905 (0.00630)
4: Add controls for labor-demand shift (based on industrial composition) and mean January temperature	0.000787 (0.000325)	0.000277 (0.000124)	0.00396 (0.00244)	-0.00762 (0.00483)
5: Drop largest city in each state	0.000692 (0.000302)	0.000262 (0.000102)	0.00353 (0.00197)	-0.00686 (0.00474)
Panel C: Changing instruments				
6: Alternative legal coding, earliest year of enabling leg.	0.000789 (0.000445)	0.000318 (0.000130)	0.00514 (0.00266)	-0.00895 (0.00615)
7: Alternative legal coding, latest year of enabling leg.	0.000862 (0.000456)	0.000301 (0.000134)	0.00452 (0.00270)	-0.00569 (0.00584)
8: State supreme court reversal of enabling legislation	0.000852 (0.000384)	0.000252 (0.0000974)	0.00598 (0.00314)	-0.00760 (0.00497)

Notes: The top entry of each cell is the estimated coefficient on urban renewal funding per capita from a separate instrumental variable regression. Heteroskedasticity-robust standard errors are adjusted for correlation within states and reported in parentheses below the coefficient. See table 1's notes for variable definitions.
Sources: See data appendix.

Table 3: Comparison of City and Rural County Outcomes,
Reduced-form Regressions of Outcomes on Instrumental Variable

	Ln median property value	Ln median family income	Employment rate	Poverty rate	Ln population
1. Sample of cities, IV based on year of enabling legislation	0.00866	0.00286	0.0366	-0.0691	0.0104
	(0.00329)	(0.00115)	(0.0240)	(0.0595)	(0.00565)
2. Sample of rural counties, IV based on earliest year of state enabling legislation	0.00214	-0.000718	0.00117	-0.00838	-0.00271
	(0.00399)	(0.00164)	(0.0536)	(0.0441)	(0.00471)
3. Sample of rural counties, IV based on latest year of state enabling legislation	0.00148	-0.000952	-0.0261	0.00633	-0.00236
	(0.00412)	(0.00177)	(0.0479)	(0.0529)	(0.00462)

Notes: Each coefficient is from a separate OLS regression of an economic outcome on years of eligibility for urban renewal (based on the timing of state enabling legislation) and control variables. Row 1's sample includes the cities examined in table 2. Rows 2 and 3 include rural counties (less than or equal to 25 percent urban population in 1950, approximately half of all U.S. counties). The underlying control variables for the city and county regressions are very similar but not exactly the same due to data availability in Haines (2004). Differences are as follows: whereas the city regressions control for proportion of housing units built before 1920 (and counted in 1950), the county regressions control for the proportion of units built between 1940 and 1950 (and counted in 1950); whereas the city regressions control separately for the proportion of units without full plumbing and the proportion of units dilapidated, the county regressions control for the proportion with full plumbing and not dilapidated (a single variable); and whereas the city regressions control for the proportion of units with more than 1 person per room, the county regressions control for median number of rooms per unit and median number of persons per unit. Standard errors are clustered by state.
Sources: See data appendix.

Table 4: Urban Renewal Effects on Population and Housing Characteristics in 1980

	Population Characteristics		Housing Stock Characteristics		City Growth	
	Median schooling	Percentage black	Percentage old units	Percentage w/o all plumb.	Ln population	Ln housing units
Panel A: Basic specification						
1: Controls for 1950 characteristics, census division	0.0000960	0.0108	-0.0310	-0.00156	0.000939	0.000953
	(0.000403)	(0.00981)	(0.0131)	(0.00115)	(0.000547)	(0.000539)
Panel B: Additional controls						
2: Add controls for public housing, poverty spending, Model Cities	0.000221	0.00156	-0.0366	-0.00197	0.00140	0.00139
	(0.000562)	(0.0133)	(0.0169)	(0.00144)	(0.000721)	(0.000746)
3: Add controls for state aid to cities and support for Goldwater	0.000207	0.00606	-0.0390	-0.00148	0.00110	0.00112
	(0.000404)	(0.00991)	(0.0158)	(0.00109)	(0.000556)	(0.000553)
4: Add control for labor-demand shift and January temperature	0.000131	0.00792	-0.0295	-0.00164	0.000902	0.000928
	(0.000401)	(0.00892)	(0.0116)	(0.00118)	(0.000456)	(0.000488)
5: Drop largest city in each state	0.000111	0.00402	-0.0308	-0.00149	0.000769	0.000748
	(0.000428)	(0.00849)	(0.0140)	(0.00109)	(0.000506)	(0.000485)
Panel C: Alternative instruments						
6: IV with alternative legal coding, earliest year of enabling leg.	0.000103	0.0120	-0.0417	-0.00115	0.00118	0.00128
	(0.000460)	(0.0110)	(0.0167)	(0.00130)	(0.000764)	(0.000785)
7: IV with alternative legal coding, latest year of enabling leg.	0.000262	0.0121	-0.0314	-0.00140	0.000843	0.000809
	(0.000463)	(0.0122)	(0.0161)	(0.00129)	(0.000683)	(0.000695)
8: State court reversal of enabling legislation	0.000683	0.00749	-0.00969	0.000317	0.000428	0.000126
	(0.000583)	(0.0147)	(0.0160)	(0.00189)	(0.000637)	(0.000667)

Notes: Each cell reports the coefficient estimate on urban renewal funding per capita from a separate IV regression. Heteroskedasticity-robust standard errors are adjusted for correlation within states and reported in parentheses.
Sources: See data appendix.

Table 5: Urban Renewal Coefficients,
Regressions including Controls for Potentially Endogenous Population Characteristics

	Ln median property value	Ln median family income	Employment rate	Poverty rate
1: Basic specification	0.000774	0.000256	0.00328	-0.00619
	(0.000327)	(0.000113)	(0.00213)	(0.00513)
2: Basic specification plus controls for percent black and median education in 1980	0.000875	0.000310	0.00438	-0.00850
	(0.000362)	(0.000128)	(0.00241)	(0.00448)

Notes and sources: See table 2 and data appendix.

Table 6: Urban Renewal Effect on Residential Segregation in 1980

	1	2	3
Coefficient on UR funding	-0.000043	0.000169	0.000157
	(0.000119)	(0.000306)	(0.000251)
Control for 1950 segregation	No	No	Yes
Census division fixed effects	Yes	Yes	Yes
N	458	75	75

Notes: Each coefficient is from a separate instrumental variable regression that includes census division fixed effects and the set of city characteristics described in the text. The dependent variable is city-level residential segregation, measured as a dissimilarity index across central-city census tracts in 1980 based on the original 1980 tract data from Summary Tape File 3A (U.S. Department of Commerce 1982). Column 1 includes all available cities. Column 2 is the same specification as column 1 but excludes the cities for which there is no 1950 segregation measure. Column 3 adds a control variable for segregation in 1950. Heteroskedasticity-robust standard errors are adjusted for correlation within states. In this table's analysis, the Pacific and Mountain census divisions are treated as a single census region category due to the small number of cities in the Mountain region with 1950 segregation measures.
Sources: See data appendix.

Table 7: Urban Renewal Effects on African-American Outcomes in 1980

Panel A: Main outcome variables	Ln median value	Ln median family income	Employment rate	Poverty rate
Basic specification	0.00121	0.00101	0.0106	-0.0176
	(0.000660)	(0.000581)	(0.00937)	(0.0179)
Panel B: Population and housing characteristics	**Percent old housing units**	**Median schooling**	**Percent w/o full plumbing**	**Ln black population**
Basic specification	-0.0357	0.00308	-0.00367	0.00452
	(0.0186)	(0.00175)	(0.00325)	(0.00259)

Notes: Each coefficient is from a separate regression that includes regional fixed effects and the set of city characteristics described in the text. Heteroskedasticity-robust standard errors are adjusted for correlation within states. See table 1's notes for variable definitions. Because some cities lacked black-specific outcome data for 1980, there are only 346 observations.
Sources: See data appendix.

Data Appendix

Housing and Population Data

We manually entered data for the percent of dilapidated housing units, units built before 1920, units without full plumbing, and crowded units for 1950 from the published volumes of the *1950 Housing Census*. Data for dilapidated units and units without full plumbing are from table 18, data for crowded units are from table 19, and data for units built before 1920 are from table 20. We collected data on the percent of families with incomes less than $2,000 from table 11 in the published volumes of the *Characteristics of Population* in the *1950 Population Census*. We collected data on median years of schooling of adults (over age 24) in 1980 from table 119 of the *General Social and Economic Characteristics* volume of the *1980 Census of Population*.

Some of the black-specific data for 1980 were also entered manually. Data for the percent of black owner-occupied units, median schooling, percent employment, and median family income were entered from the *General Social and Economic Characteristics* volume of the *1980 Census of Population*. The percent of owner-occupied units are from table 22, median schooling data are from table 132, percent employment data are from table 134, and median family income data are from table 136. Data for the percent of black units without full plumbing, crowded units, and median property values are from table 3 of the *General Housing Characteristics* volume of the *1980 Census of Housing* and data for the percent of old units are from table 77 of the *Detailed Housing Characteristics* volumes of the *1980 Census of Housing*.

The data for population, land area, housing units, percent employment, median family income, median property values, percent of owner occupied housing, and percent of employment in manufacturing for 1950 and 1980 are from the 1952 and 1983 *City Data Book* files compiled by Haines (2004, file numbers 60 and 66). These files also provide data for the percent of nonwhite residents and median schooling in 1950, and for the percent black, percent of crowded units, occupied units without full plumbing, families in poverty, and old units in 1980. The 1983 *City Data Book* provides mean January temperatures, averaged from 1951 to 1980.

Election, Government Spending, and Enabling Legislation Data

State and county-level votes for Goldwater in the 1964 Presidential Election were entered from David Leip's *Atlas of U.S. Presidential Elections*. Data for state aid to cities were entered from "State Payments to Local Governments in 1952" in *State and Local Government Special Studies, 35*, published by the U.S. Department of Commerce (1954). These data were divided by state urban population from *1950 Characteristics of the Population* ("old definition" of urban).

Urban Renewal state enabling legislation data and data on city applications to the Model Cities program are from *Governmental Units Analysis Data* (Aiken and Alford 1972). Data for units of public housing per capita built under the 1949 Housing Act, spending per capita on poverty programs circa 1966, and applications to the Model Cities program are also from *GUAD*. We collected Urban Renewal funding data from the *Urban Renewal Directory* (HUD 1974). To check the accuracy of the enabling legislation dates in GUAD, we created an alternative coding for the timing of enabling legislation based on our reading of the *Journal of Housing*, *Book of the States*, *Municipal Year Book*, archived reports from the Housing and Home Finance Agency (the predecessor of HUD) and academic articles.

Tract-level Data

We calculated central city segregation levels in 1980 using data from Summary Tape File 3A of the *1980 Census of Population and Housing* (ICPSR 8071). The 1950 segregation levels, which are available for relatively few cities, were originally described in Cutler, Glaeser, and Vigdor (1999) and are posted by Vigdor at: http://trinity.aas.duke.edu/~jvigdor/segregation/index.html.

Appendix Table 1: Summary Statistics

Variable	Mean	Std. Dev.
UR funds per capita (1950 population)	177.043	221.444
Years of potential participation in UR program	22.581	4.266
Ln population 1980	11.297	0.887
Ln median family income 1980	9.827	0.172
Employment rate 1980	92.632	2.925
Percent of families in poverty 1980	11.040	5.143
Median years of schooling 1980	12.467	0.717
Percent black 1980	16.136	17.820
Ln housing units 1980	10.382	0.896
Ln median property value 1980	10.627	0.364
Percent of units built before 1940	37.401	19.070
Percent of units crowded 1980	4.208	3.538
Percent of units w/o full plumbing 1980	1.66	0.907
Percent of units owner-occupied 1980	54.327	11.325
Ln population 1950	11.090	0.879
Ln median family income 1950	8.120	0.175
Employment rate 1950	94.950	2.018
Percent of families with income under $2000 in 1950	21.410	8.997
Median years of schooling 1950	10.310	1.268
Percent nonwhite 1950	9.282	11.816
Ln housing units 1950	9.901	0.885
Ln median property value 1950	9.027	0.293
Percent of 1950 units built before 1920	49.414	21.784
Percent of units dilapidated	6.623	5.578
Percent of units crowded 1950	12.727	6.587
Percent of units w/o full plumbing 1950	21.723	13.412
Percent of units owner-occupied 1950	52.317	11.690
Percent of employment in manufacturing 1950	29.508	15.247
New England	0.120	
Mid Atlantic	0.157	
East North Central	0.245	
West North Central	0.083	
South Atlantic	0.116	
East South Central	0.055	
West South Central	0.085	
Mountain	0.039	
Pacific	0.100	

Notes and sources: See table 1.

Appendix Table 2: OLS Results for Comparison with IV Results (in Tables 2 and 4)

Panel A			
Ln median value	Ln median family income	Employment rate	Poverty rate
-.0000747	-.0000371	-.0014021	.0005116
(.0000467)	(.0000213)	(.0006128)	(.0008834)
Panel B			
Ln population	Median schooling	Percent black	
.0000086	.0001091	.0038598	
(.0000504)	(.0000807)	(.0025046)	
Panel C			
Ln housing units	Percent old	Percent w/o full plumbing	
.0000323	-.0053381	-.0003936	
(.0000479)	(.0023411)	(.0001906)	

Notes and sources: See table 2 and table 4.