

BOOK REVIEWS

Arnold R. Hirsch: *Making the Second Ghetto: Race and Housing in Chicago, 1940-1960*. Interdisciplinary Perspectives on Modern History, Cambridge University Press, 1983, 362 pages.

Another case study of the most scrutinized city in America: Chicago! But this time it is neither a University of Chicago sociologist like Robert Park or economist like Richard Muth, but a young historian who focuses his *magnum opus* on the "City of Big Shoulders." The subject treated is of great interest to urban geographers, sociologists, political scientists, economists and planners alike: the mechanism by which radial residential ghettos are created and preserved. And the subject is lavishly researched, pain-stakingly documented, and presented in a clear, compelling, and often poignant manner. Yet, it is precisely the purely historical approach that renders this book somewhat unsatisfying, I would suspect, to most social scientists.

Hirsch describes the confluence of factors leading to the creation of the "second" black ghetto in Chicago from 1940-1960 (the "first" being formed during World War I in the "Black Belt"). During World War II, Chicago, like many Northern industrial cities, was confronted with unprecedented immigration of rural Southern blacks, concomitant housing shortages, and escalating interracial tensions as traditional ecological patterns were threatened. Hirsch documents in detail (often with fascinating supporting photographs) the resulting processes of suburban flight, discrimination, neighborhood succession, and attendant racial violence. None of this description is particularly remarkable or original, however. But Hirsch begins to strike some interesting sparks when he turns to his main thesis about why such *general* forces were translated into such a *particularly* exaggerated pattern of segregation — a pattern that as of 1980 continued to render Chicago as the most segregated major American city.

First, large "downtown" interests found themselves confronted by threats to their economic survival spawned by a decaying central city. Unable to flee the city, they enlisted the political and legal power of the city in a successful effort to "renew" the urban core. This "positive" exercise of power was cloaked in the innocuous rhetoric of the "public interest," belying its self-serving nature and implicit racism. "Slum renewal" became the facade for "black removal" and subsequent relocation in the "public ghettos" of the Chicago Housing Authority (CHA). This action by the Chicago business elite, according to Hirsch, "represented the successful adaptation of the 'business-creed' to post-New Deal America" (p. 133).

Second, those who vehemently resisted the pressures of black immigration into all-white neighborhoods became alienated from and suspicious of both the city government and more "liberal" groups espousing neighborhood interracial reconciliation and "integration management" (e.g., the University of Chicago in Hyde Park). Unable to wield as much clout as their elite white antagonists, these local neighborhood groups exercised a reactionary, "negative" power on policy. Suburban white communities succeeded in preventing scattered-site public housing in their jurisdiction, thereby impeding

desegregation. Urban ethnic enclaves often expressed their power through violence against blacks and black-occupied dwellings, thereby molding CHA site selection and tenant selection policies. In such manner, a programmatic force holding the potential for desegregation was converted into a bulwark for perpetrating status quo segregation.

This is all very interesting. But to social scientists interested in theories and their empirical testing, *Making the Second Ghetto* will seem like a collection of facts badly in need of a unifying conceptual framework. While revealing a general personal bias against history abstracted from theoretical context, the above criticism is particularly apropos here because of the great unfulfilled potential of this work. I suspect that Hirsch's copious scholarship could provide some potent evidence for testing a wide variety of urban social science theories, yet this potential is largely unrealized in the book. For example, what does all this Chicago detail tell us about our theoretical understanding of: How residential mobility is influenced by racial considerations? How prejudices are formed and abetted? How power is wielded in a city and by whom? How urban housing markets operate in an environment of racism and public housing construction? How planners are influenced by public and private pressure tactics? Without such substantiated generalizable theories we have no way of answering the most important questions implicit in this work: What could have been done to make things turn out differently, and what can be done in the future to avoid making "second ghettos" elsewhere?

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Roger Friedland: *Power and Crisis in the City: Corporations, Unions, and Urban Policy*, New York: Schocken Books, 1983, xxviii, 248 p.

Contemporary cities are in trouble. At the same time that there is a need for both massive capital expenditures and consumption subsidies in central cities, the fiscal capacity of cities to meet these needs has been decimated. Is this dilemma the result of an impersonal market system, wherein investment has flowed naturally toward suburbs and low-wage regions in the South and West? If the market mechanism is responsible for the movement of productive resources out of central cities, then the financial crisis of these cities should not be blamed on human actors. Roger Friedland, however, finds the sources of modern urban problems in the conscious decisions of large corporations and labor unions. The spillover impact of the market power of these two institutions on political decisions to spend and tax is the focus of his analysis.

Friedland's work is stimulated by his apparent distaste for models of urban political power that presume manifestations of power from participation of corporate and union leaders in lobbying and election campaigns. This personalized view of power as a reflection of the political activity of elite individu-

als in unappealing to Friedland. Although he is not specific about the rationale for his dissatisfaction with the participatory model of urban political power, he implies that such a model fails to explain why certain elites have a greater impact on urban policy than do competing elites.

The basic substructure of Friedland's model of urban political power consists of two behavioral assumptions. First, national corporations have since 1964 sought to influence local tax and budget decisions toward sustained economic growth in central city cores. This growth has been shaped primarily by corporate needs for land and public services conducive to a transformation of central cities from production centers to office economies. The second assumption is that these same national corporations and major labor unions found it necessary for purposes of social control to encourage public expenditures that would pacify the low income inhabitants who were paying the major costs of the tax subsidies and expanded use of eminent domain which corporations obtained. What unifies these assumptions is the assertion that both corporations and unions purposefully use their economic muscle to channel market and political decisions toward their twin goals of economic growth and social control. They do not rely on impersonal equilibrating forces to attain their objectives.

The participation of union and corporate officials in political behavior is treated by Friedland as a consequence rather than a cause of their political power. Instead of individual participation in the political process, the source of power for both organizations consists of their control of resources on which the political process depends. Specifically, corporations have the power to reallocate their resources outside the taxing jurisdiction of any city administration with whom they are negotiating. When these corporations seek tax breaks, zoning preferences, and relaxation of regulatory standards, their record of success stems from the ominous nature of their threats to move to more friendly urban areas.

The political power of labor unions originates from a different source. Their geographical location is a function of decisions made by employers. Thus, unions cannot duplicate the threat to move used by national corporations when they pursue their urban political goals. Unions have political power because they can mobilize masses of people to do the work required for successful referenda efforts or political campaigns. The typically Democratic mayors of major urban areas depend extensively on unions to inform masses of voters and to motivate them to participate in the political process. Unions have political clout not because of their control over material resources but because they can promise a large number of committee human resources to those political officials who champion their urban goals.

Based on (1) his two behavioral assumptions about the objectives of national corporations and unions and (2) his assertions about the source of their urban political power, Friedland attempts to explain selected urban political decisions as a reflection of the density of union and corporate organizational resources in particular central cities. The local power of national corporations is estimated by comparing the number of corporate head-quarters of the largest 1000 industrial corporations located in various large cities; union power is represented by the number of national unions in

central cities in 1960. He attempts to measure the policy impact of union and corporate power in the 130 cities that had a population of 100,000 or more in 1960. His methodology consists of establishing correlations between high union or corporate density and particular urban policies. Then he provides a logic that attempts to transform the correlation into causation.

The core of Friedland's book consists of his attempts to explain the allocation of urban renewal and poverty expenditures in terms of their contribution to corporate and union organizational objectives. He found that urban renewal expenditures were particularly high in those cities where corporate and union power was the greatest. He attributes this finding to the useful role played by urban renewal in restructuring downtown areas, facilitating office growth in central cities, providing luxurious housing for white-collar workers, protecting the value of real estate investments, and (for the unions) expanding employment in the construction trades.

War on Poverty funds provided, according to Friedland, a form of riot insurance. While this concept of poverty expenditures as a social control device is hardly original with Friedland, he does provide an interesting integration of poverty expenditures and the effects of urban renewal. To accept his analysis of poverty expenditures, one must see poverty funds as a response to perceived threats from those who lost homes and jobs as a result of urban renewal.

From this perspective the new poverty agencies prevented the mobilization of the victims of economic growth in the cities against the private and public investment decisions that were altering the city's landscape. Organizations created to administer poverty funds served many useful purposes: coopting community activists through new forms of political patronage, diverting the ire of inner city residents away from urban renewal and toward the poverty bureaucracy, and subdividing lower-income ethnic groups into competitors for poverty funds. Friedland's primary empirical finding with respect to poverty expenditures is consistent with this interpretation of the function of those expenditures. Where corporate and union power was strongest, poverty expenditures were high, especially for nonwhites. Racial unity provided a cohesive force which gave Blacks a political potential that was particularly threatening to those who wanted continued expansion of the office economy in downtown areas.

Friedland's book is strong at the conceptual level, but the empirical content tends to detract from, rather than strengthen his analysis. Since so much of his empirical analysis is dependent on the identification of those cities with high and low corporate power, his measure of that power is critical to the credibility of his findings. Friedland categorizes a city as high corporate power city if it has two or more corporate headquarters and as a high union power city if fifty or more unions are located in the city. Friedland does not share his rationale for this categorization format, but it appears to be based much more on computational convenience than on any plausible logic.

Another major problem with Friedland's work is his claim that he is analyzing the organizational power of corporations *and* unions. Very little of the book discusses the role played by unions in urban tax and expenditure decisions. When union behavior is analyzed, the discussion is typically brief and dis-

joined. Despite the fact that unions are largely ignored in his book, Friedland repeatedly generalizes about the impact of corporate and union power. The emphasis on corporate power weakens Friedland's claim that he is formulating a model of organizational power.

Friedland provides very little analysis concerning the significance of his findings. He seems primarily concerned with demonstrating that political participation is not a good proxy for political power. He does offer a convincing argument that organizational power may have significant impacts on urban politics and that political participation by leaders of major economic organizations is a consequence of that power. Those who measure political power by contributions of time and money should pay careful attention to Friedland's organizational model of urban political power. As a contribution to the debate among political scientists and sociologists over the source of power in urban political decisions, Friedland's book is successful.

Unfortunately, Friedland does not explore any broader significance for his analysis. Although he is clearly displeased that corporations and unions possess the power he identifies, he does not suggest particular constraints on the use of that power. This book ends with a call for a sharing of the social costs or urban dislocation, but his own analysis would suggest that the probability of such sharing is remote. The predominantly poor residents of inner cities lack access to the basic investment decisions that shape their tax bills and job prospects. Providing them access would cause a tradeoff between growth and equity that would be displeasing to those with organizational power.

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Robert J. Newman, *Growth in the American South: Changing Regional Employment and Wage Patterns in the 1960's and 1970's*. New York University Press: 1984.

The reader of this book will be exposed to an interesting and insightful blend of analytical tools applied to important issues in regional growth patterns in the United States. Using his Ph.D. dissertation as a starting point, Newman has produced a very carefully constructed analysis of why the South has grown more rapidly than other regions of the United States in the 1960's and 1970's.

Many regional analysts may disagree with conclusions that he reaches. However, few will be able to argue with his methodological approach. The great strengths of this work are: (1) the formulation of refutable hypothesis about the role of various factors in Southern/non-Southern regional growth patterns of the 1960's and 1970's; and (2) the application of appropriate statistical methods to test these hypotheses.

It is a very welcome relief from the mainly polemical approach of earlier works, e.g., Weinstein and Firestone (1978). This book should be required

reading for anyone with an interest in understanding the sunbelt phenomenon of the past twenty years. It will not provide all the answers but does provide the right path to follow for a better understanding of the role of the private and public sector forces in regional growth patterns.

Given the strong recommendation to read the book, several controversial issues need to be considered. First, Newman provides a serious challenge to the findings of Due (1960) and the ACIR (1979) that state and local taxes are not a major issue in determining industry location (and thus regional employment growth differentials). Second, from this reviewer's perspective, there is a potential misspecification bias in the model used to test for the influence of several public policy variables on regional growth differentials.

Newman's analysis of regional growth patterns uses: states as the units of observation; a 1957-79 time period for employment growth differentials (a shift-share type of growth differential measure is employed); and a 1959-78 time period for regional wage rate differentials.

To explain the state differentials in employment growth, Newman "abstracts" from economic forces that others have considered and focuses on the change in (1) corporate income taxes, (2) right-to-work legislation and (3) the degree of union activity in each state. Generally, he finds that states that lowered relative corporate tax rates, had right-to-work legislation ("Business Climate" proxy), and had lowered the relative proportion of the labor force that were union members during this time period experienced faster rates of employment growth than the national average. These states were mainly southern states.

Newman is very careful to introduce interindustry considerations into his analysts because he recognizes (formally) that optimal timing of investment in a new plant (perhaps in a new region because of more favorable "business conditions") will vary by the capital intensity of different industries. Similarly, time lags are explicitly introduced through the use of dummy variables to account for the lag between when a state produces a more favorable "business climate" and when it pays a company to invest in a new plant in this state.

However, the statistical results that are obtained may be biased because relevant explanatory variables are omitted. Including only "tax," "union" and "right-to-work" explanatory variables in a regression on state employment change (relative to the U.S. average), almost endures the introduction of specification bias. At the same time it provides an excellent opportunity for additional research on the robustness of the Newman results. A more theoretically complete model of regional growth differentials could be introduced into the Newman framework. This would provide the "control" variables needed to assess the Newman hypotheses.

The second part of this book looks at the issue of regional wage differentials. It is very well written and provides a valuable historical overview and update on trends through the 1970's. Trends in raceage cohort earnings ratios between regions are analyzed in a very clear and concise manner. Questions analyzed include: Is the narrowing of wage differentials (South/non-South) likely to continue or are there "compensating equilibrium differentials" between regions? Are wages rising faster in the South because of

increases in the demand for southern labor and short-run inelasticities in southern labor supply relative to the non-South?

This section of the book and the summary and conclusions chapter provide many opportunities for further research. Newman has provided regional analysis with an important stepping-stone for the understanding of regional growth in the United States.

In addition to being valuable reference for researchers, students interested in regional science, regional economics, industrial location, economic geography and labor markets will benefit greatly from careful reading of the book. His concise review of previous work and methodological approach will both be of lasting value to students.

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