The Tiebout Model at Fifty

Essays in Public Economics in Honor of Wallace Oates

EDITED BY

William A. Fischel

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Footloose at Fifty: An Introduction to the Tiebout Anniversary Essays

William A. Fischel

A conceit of scholars has it that only an author's writings, not his or her personal life, are worthy of study. I have nonetheless found that there is a ready audience for what I have been able to learn about the life of Charles Tiebout. This inquiry in turn spurred me to ask why Tiebout's article languished for 13 years until Wallace Oates published his 1969 empirical work. My answer is "external events." The 1950s were the low point for local government in the United States, and current interest in the Tiebout model reflects the growing influence of localism. The balance of this chapter attempts to put the rest of the articles in this volume in the context of the ever-growing literature on the Tiebout model.

Who Was Charles Tiebout?

Charles Mills Tiebout was born on October 12, 1924, and grew up in Greenwich, Connecticut. Despite his fame among social scientists, Charles is not the most famous Tiebout. His father, Harry, was a successful psychiatrist who treated, among other disorders, alcoholism. One of his patients was "Bill W," a cofounder of Alcoholics Anonymous. Unlike other contemporary psychiatrists, Harry Tiebout thought there might be something worthwhile in the methods of AA, and after investigating them, he became a strong proponent of the method. Harry is revered in AA circles as one of the fathers of the movement for this reason.

Charles followed in the footsteps of his father and older brother when he entered Wesleyan University in 1941. According to his son Bruce, Tiebout was not initially happy there, and he dropped out in 1942 to join the Navy, in which he served until the end of World War II. In 1946 Charles returned to Wesleyan with his bride, the former Elizabeth Gray, and newfound motivation. He became an honors student of economics and a campus leader in a number of organizations. He began his graduate studies at the University of Michigan months after he received his Wesleyan B.A. in 1950. It is unclear why he sought an academic career, but the inspiration may have been his older brother, Harry Jr., who had obtained a Ph.D. in philosophy at Columbia and was beginning a long career at the University of Illinois as Charles was finishing Wesleyan.

At Michigan, Tiebout made public finance one of his fields of concentration. Richard Musgrave, who was then working on his canonical *The Theory of Public Finance* (1959), offered a seminar on the subject for graduate students. Tiebout took this seminar in 1951 or 1952. In a conversation in 1994, Musgrave told me that Tiebout had in this seminar orally responded to his proposition about preference revelation for public goods. The problem is that if consumers cannot be excluded from public goods, would-be providers cannot expect to be paid for them, and government inquiries about willingness to pay will be met with strategic evasiveness. Hence Musgrave concluded that the only mechanism for determining the level of public goods would have to be political. Tiebout proposed a nonpolitical alternative involving local public goods and consumers shopping around for the community that best fit their demands. Musgrave recalled, however, that Tiebout had proposed his alternative in a joking style, so it was not clear how seriously he meant it.

Tiebout's sense of humor was the first characteristic that almost all of his contemporaries recalled. Even his three children—Charlie, Bruce, and Carol—thought he was funny, an accomplishment any parent can envy. (Bruce, who minds the family history, has generously supplied me with details of his life as well as the photograph of his father in this volume.) He also enjoyed practical jokes. Several of his acquaintances related the story of Tiebout and a friend being stuck in a midwestern city because of airport delays. They went downtown to pass the time, and Tiebout convinced some local dignitaries that they were scouting for business locations, which earned them a luxurious bacchanal at Chamber of Commerce expense. Sad to say, the facts are less gaudy. Charles Leven was the friend who was with Tiebout on that occasion, and Leven told me that they had simply cadged some free drinks from a gullible local banker during a few hours' delay. It is likely, however, that Tiebout did little to discourage the more rewarding account.

Tiebout's Michigan doctoral thesis was not an elaboration of his vote-with-your-feet idea that he had first expressed in Musgrave's class, and Musgrave was not on his thesis committee. The thesis was an empirical inquiry into local multiplier effects. Tiebout estimated the influence on employment and spending of an exogenous increase in spending in small midwestern cities. This application of Keynesian multiplier theory was completed in 1957, three years after he had taken a job at Northwestern University. The unsurprising answer to Tiebout's question was that the local multiplier effect was vanishingly small (that is, hardly greater than unity), as most of the secondary spending leaked out beyond the borders of the locality. Daniel Suits, who chaired Tiebout's thesis committee at Michigan, told me a few years ago that it was not regarded as a strong dissertation.

Tiebout wrote "A Pure Theory of Local Expenditures" (1956) when he was at Northwestern. Charles Leven was a graduate student there and became a close friend of the new assistant professor. Leven recalled that a Northwestern colleague, Meyer Burstein, had complained to Tiebout about the excessive cost of the local services in Evanston, Illinois. Tiebout asked him why he didn't just move to another community. Whether this caused Tiebout to recall his Michigan idea was not clear, but shortly thereafter he told Leven that he was sending an article to the *Journal of Political Economy* at Chicago.[1] Leven reported that Tiebout thought that the Chicago-school editors, regarded by their uptown rivals as excessively fond of free markets, would just have to publish it, since he had shown that governments could be just as efficient as private firms. (Albert Rees, who was department chair when I was a graduate student at Princeton in the early 1970s, once mentioned to me that he was proud to have been the *JPE* editor who accepted the article in 1956.)

Leven's recollection struck me as significant because many critics of the Tiebout model regard it as a manifestation of a libertarian-conservative approach to government. Indeed, an important intention of Tiebout's model was to show that the political process could be dispensed with for a large class of government activity. But Tiebout's own politics were within the mainstream of the Democratic Party. Bruce Tiebout said that his father had been considered for President Kennedy's Council of Economic Advisors on the recommendation of Secretary of Defense Robert McNamara, who was acquainted with Tiebout through his work on the regional economic impacts of defense projects. The anticipated appointment fell through when Lyndon Johnson succeeded Kennedy.

[1] Leven told me this story in 1999, and he elaborated on it in a four-page reminiscence about Tiebout (Leven 2003). Musgrave first told me in 1994 how Tiebout came up with his idea in his Michigan seminar, circa 1951. Leven told me that he had never heard Musgrave's account. Musgrave's remarks on the history of public finance at the 2005 Tiebout conference made it clear that his memory of events in that era remains sharp. I surmise that both stories are true, and Tiebout simply did not mention to Leven that he had thought of this idea at Michigan four or five years earlier. Leven's recollection that Tiebout dashed off a draft in "four or five days" (2003, 236) suggests that Tiebout had been thinking about it well before their encounter with Burstein, the unhappy Evanston resident. I must point out, however, that Leven's statement (2003, 237) that Tiebout never published anything else on local public finance after his 1956 paper overlooks Tiebout (1960; 1961), Tiebout and Houston (1962), and Tiebout and Chinitz (1965), all of which concern local public finance.

Tiebout's friends recall him as progressive-minded on social and political issues, and Bruce Tiebout assured me that his father was a dyed-in-the-wool Democrat and had no use for right-wing politics or economics.

Tiebout left Northwestern for a position at UCLA in 1958, where he was promoted to associate professor of economics with tenure. At UCLA he wrote some articles that followed from his 1956 article. (Wallace Oates discusses three of them in the next chapter.) But these articles did not figure in the later Tiebout literature that followed from Oates's 1969 article, although Ostrom, Tiebout, and Warren (1961) remains influential in political science.

In 1962 Tiebout moved to Seattle for an appointment at the University of Washington as a professor of economics and business. From that time on, he published and consulted on regional-science topics that stemmed from his doctoral dissertation and his later study of input-output models. This work was unrelated to his now-famous 1956 article. He soon gravitated to the University of Washington's geography department, and his research on regional multiplier effects is still influential among geographers.[2] He was president of the Western Regional Science Association in 1965, and the Association has a dissertation prize in his memory.

In Seattle Tiebout became the recipient and disburser of numerous federal grants and contracts, and his government consulting thrived. His family lived in Laurelhurst, a prosperous section of Seattle near the university. His regional-science work was making him well known and well off, and his students and colleagues regarded him with affection and esteem. His life came to a sudden end on January 16, 1968, when he was 43 years old. He suffered a heart attack while he was teaching and died the same day. His widow lived 31 years longer in Seattle, and their children still live in the area. None has pursued an academic career.

Why Did the Tiebout Model Start So Slowly?

Tiebout's now-famous paper did not take the economics profession by storm. Neither Paul Samuelson nor Musgrave, whose work Tiebout specifically addressed, was especially impressed by it. Musgrave (1959, 132) continued throughout his career to think of the Tiebout model as an interesting but special case, one with only limited application to the world of public finance. Samuelson took note of the article in a 1958 comment in the *Review of Economics and Statistics*, but he dismissed it as being both unrealistic and normatively unattractive.

[2] A bibliography of Tiebout's works on economic geography and regional science can be found on a Web site maintained by Gunter Krumme, who was one of Tiebout's students and is now a professor at the University of Washington's Geography Department. http://faculty.washington.edu/~krumme/VIP/Tiebout.html.

Their reactions were similar to that of most other economists who cited the article through the 1950s and 1960s. It was not until Wallace Oates published his 1969 article that connected Tiebout's idea with property tax capitalization that the "Tiebout hypothesis" took off. The annual count of citations to Tiebout more than doubled in the decade after Oates's publication, and economists began using the Tiebout model as a foundation for both theoretical and empirical work in local government.[3]

Oates himself describes how he came upon his idea to test the Tiebout model in the next chapter, and so the question I will pose here concerns fashions in economic models: Why did it take so long for Tiebout's model to be taken seriously? Obscurity of style or excessive mathematics cannot be the cause of its slow start. Tiebout's writing is clear, and the article's mathematics is minimal. Perhaps the converse argument, that it lacked a mathematically expressed model, was the problem. But that, too, seems inadequate as an explanation. Other seminal papers with little or no mathematics have been adopted by the mathematically talented and quickly made their way into the economics mainstream. Coase (1960) and Stigler (1971) come to mind.

An obvious answer to the slow start is that Tiebout himself gave up writing on the subject after he left UCLA in 1962. But that was not without having done several papers that built on his original hypothesis. In one of them, Tiebout even suggests the principle of capitalization: "When you seek good schools for your children, you often find the rents and housing prices are high" (1961, 94). Yet, he did not advance any systematic evidence in support of his idea.

Tiebout probably stopped writing about local public finance for career reasons. Most economists develop more than one line of inquiry and gravitate toward that which is best received by the profession. It seems likely that the response Tiebout was getting to his 1956 article was not encouraging enough to keep him or his graduate students interested in it, especially when his regional-science research was doing quite well.

Why was the response so tepid? It is unlikely that Tiebout's model was dismissed as an "everyone knows this" story. It really was different and original. It was not simply a restatement of federalism, fiscal or otherwise. Tiebout gave

[3] The time lag between Tiebout and Oates, who never met, may explain why most economists pronounce his name as "tee-BOO," as it would be in French, while his family and friends insist on "TEE-boe," rhyming with oboe. I surmised that the name was Dutch, but Dutch friends tell me that the final *t* would be pronounced in that case. The proprietor of the Tiebout Tavern, just off Tiebout Avenue in the Bronx in New York City, answered the phone with the pronunciation favored by the Tiebout family. A treatise on street names in the Bronx offers this background: "An ancient Dutch family of Huguenot extraction, the Tiebout name is found in the 1689 and 1703 Lists of Inhabitants, with sometimes the spelling 'Thibaud' " (McNamara 1991). The name "Thibaud" is pronounced "TEE-boe" in French. Bruce Tiebout confirmed that his ancestors were Huguenots who left France for the Netherlands and then emigrated to New York in the 1600s.

little attention to government organization, and Oates's original theory of fiscal federalism did not require that people move around. Residential mobility was the critical force in Tiebout's model. Everyone knew that Americans were mobile, but no economist had previously connected mobility with demand for the services of local government. The only contemporary parallel to Tiebout's model was an informal paper by George Stigler (1957), who pointed out that taxpayer mobility undermined local government's ability to redistribute wealth. But neither Stigler nor any of his students developed this insight further. Stigler's writing remained more obscure than Tiebout's, with not a single journal citation until Oates noted it in a 1968 article.

The more likely reason for the Tiebout model's slow start is that it was not right for its time. As Oates (1999) and Wickwar (1970) have demonstrated, interest in local government is subject to long historical cycles. The peak of one cycle in the United States appears to have been the two decades before World War I, when cities were growing rapidly, most government spending was local, and important people took an interest in both the governance and study of local institutions.

The 1940s and 1950s were pretty much the nadir of regard for local government. When I mentioned to my colleague Andrew Samwick that Tiebout was known for his sense of humor, Andrew quipped that he would have *needed* one to have come up with his model in the 1950s. Political attention at the time was on the international problems entailed by the Cold War. The communist half of the world organized all economic activity from the center, and many in the noncommunist world regarded centralization as the wave of the future.[4] Macroeconomic issues were at the forefront of economics, and Keynesianism had little use for local government. (Keep in mind that it was not until 1959 that Musgrave established the independence of the stabilization, allocation, and redistributive functions of government, which economists—and apparently no other social scientists—now regard as second nature.)

Before Tiebout and Oates, the treatment of local public finance in American economics textbooks alternated between hostility and condescension (Hansen and Perloff 1944; Samuelson 1948). When local government was in the news, it was almost always in a bad light. Journalists decried local resistance to desegregation, incompetent city governments, and the complacent conformity of the

[4] One datum in evidence of the era's concern with the Cold War was the lead article in the issue of the *Journal of Political Economy* in which Tiebout's 1956 article appeared. "Iron and Steel in the Balance of World Power," by John B. Parrish, described with some alarm how the Soviet Union might soon exceed American steel-making capacity. (This turned out to be an astute projection, though one whose consequences were less dire than Parrish assumed.) Professor Parrish, hardly a proponent of national planning, was moved to editorialize: "In my opinion, the maintenance of the relative position of the United States as a steel power is not a matter that can be left solely up to private industry" (1956, 384).

suburbs. Most academics were contemptuous of municipalities if they thought about them at all, and consolidation of school districts was always regarded as progress.

The changes in American life since the 1960s have not so much elevated general regard for local government—the aforementioned complaints can be heard today, too—as they have diminished regard for the national government. By the 1970s, the Keynesian consensus fell apart, people who favored central planning found less and less to admire in countries that did it, and national defense consumed a decreasing fraction of the federal budget. Americans responding to opinion polls expressed much less trust in national leaders than they had in the two decades following World War II. Within this setting, a model that suggested that politics was unimportant as long as one could choose among various governments started to look more attractive to enterprising scholars.

One point of this perspective is to suggest that some of the conditions that have increased the Tiebout model's salience since the 1960s may be diminishing. Household mobility has decreased in recent decades (Jacoby and Finkin 2004), so there are fewer occasions for migrants to think about the quality of local public services. (I would, however, still remind Tiebout skeptics, who wonder why people would move just because the sidewalks are deteriorating, that it is nearly costless to inquire about the quality of public services once one has decided to relocate for other reasons.) Funding for public schools, once largely a local responsibility, has become more uniform and centralized (at the state level) as the result of widespread litigation. (See chapter 3 by Brunner and Sonstelie in this volume.) Federal involvement in public education—generated by the political quarter that once championed local autonomy—has become more extensive than at any other time since the nineteenth century, when land grant policies helped to establish schools and colleges. National defense has come back to the front page, and federal officials are being held accountable for local problems such as disaster preparedness and environmental quality, even in cases that have negligible spillover effects.

There are offsets to these trends, of course. Ownership of a home in a well-run community is an important goal for most Americans, and home equity is the chief form of savings for the great majority. Even as enrollments drop, school-district quality continues to be a robust determinant of home values. Land use regulation, a lynchpin of later versions of the Tiebout model, remains largely local and nearly impervious to intellectual carping about its shortcomings and excesses. Local governments are still well represented in American statehouses and in Congress—most legislators started with a local office and remain beholden to city and county organizations for support—so preemption of local powers is not undertaken lightly. Whether scholars in the next half-century will still find the Tiebout model interesting and useful remains to be seen.

The Current State of Tiebout Scholarship

While the political trends that underlie interest in the Tiebout model may not be clear, the scholarly trends are clearly expansive. Papers in George Zodrow's (1983) twenty-fifth anniversary conference focused almost exclusively on the Tiebout model as the touchstone of the economics of local government. It has remained that, but its influence has expanded beyond economics and beyond the local public sector. The chapters in this book may be taken as indicators of this expansionary trend.

As anticipated, the essays for the conference were a mix of overview and new research. **Wallace Oates** offers an overview in chapter 2, "The Many Faces of the Tiebout Model." When an article becomes so widely cited in various fields of social science, its original context is sometimes lost. Oates's essay closely reviews the original article.

Economists have been divided about Tiebout's intention. Was his article primarily a theoretical exercise whose objective was to solve the preference-revelation problem? Or did Tiebout propose his model as a useful description of how fragmented metropolitan governments work? Oates's answer to these questions is . . . yes. Tiebout had both a theoretical point in mind—his answer to Musgrave and Samuelson—and the practical point that American metropolitan areas met the conditions for this theory. (Robert Inman's commentary on the Oates chapter offers an excellent review of the empirical tests of Tiebout's model, and Inman concludes that the evidence for its applicability to actual local governments is reasonably strong.)

The other large issue that Oates addresses is how to characterize the Tiebout model in this century. There are two important distinctions. The easier one is between what Tiebout originally said and what later scholars added to the model. It is clear that Tiebout neglected property taxation and said little about the power that municipalities care most about: land use regulation. Oates and Hamilton added those two crucial dimensions to the original model that are widely agreed to be necessary for its operation. Others added a profit motive for Tiebout's shadowy municipal managers (Sonstelie and Portney 1978; Epple and Zelenitz 1981) and provided evidence that the managers were attentive to voters (Borcherding and Deacon 1972; Bergstrom and Goodman 1973).

Many critiques of the Tiebout model overlook these crucial additions or treat them as if they were not part of the real model. It seems essential for any kind of scientific progress, however, to deal with a properly amended model than simply to complain about omissions in the original paper. The Keynesian model did not spring full grown from the brow of John Maynard Keynes, and little credence would be given to a critique of the model that dwelt solely on what was written in *The General Theory* in 1936.

Oates's other issue is whether the Tiebout model can be grafted onto other areas of economic inquiry beyond local public finance. The mobility of firms and households among states, regions, and countries is an important phenomenon, and to some extent these moves can be said to be shopping for better public services. American tradition (and probably that of other nations) holds that immigrants came from other countries partly to get a better shake from the public sector, broadly conceived. But Oates warns that extending the efficiency findings of the Tiebout model to this larger stage may not be warranted. The choice set for migrants is smaller, the instruments of taxation and regulation are less easily targeted, and mobility is more limited by cultural and family ties. Choosing between Budapest and Berlin is quite different from choosing between Grosse Pointe Farms and Grosse Pointe Woods.

School Finance and School Choice

The next two chapters concern the local government service that people consider most important: public schools. It is no exaggeration to say that economic analysis of American public education has been transformed by the Tiebout-Oates approach. It is equally evident that American courts of law have been oblivious to this literature. In chapter 3, "California's School Finance Reform: An Experiment in Fiscal Federalism," **Eric Brunner** and **Jon Sonstelie** review the aftermath of the twentieth century's most dramatic local-government event: the California Supreme Court's *Serrano* decision and the tax-revolt initiative, Proposition 13. (I say "event" because I think that the two are causally linked [Fischel 2004], but one does not have to accept that to appreciate Brunner and Sonstelie's masterly essay.) Following the California Supreme Court's December 1976 ruling that local school expenditures must be equalized—thus eliminating the fiscal basis for Tiebout sorting—California voters, in June 1978, cut the property tax in half and imposed assessment constraints that have kept the effective rate well below 1 percent.

California overnight became an almost natural experiment on the Tiebout model. Prior to *Serrano* and Proposition 13, California schools were financed like those of most other states, with a mixture of locally controlled property taxes supplemented by state-generated revenues. A Tiebout household could, if it were dissatisfied with the schools and school-related taxes in one community, relocate to another that better fit its demands. (One such foot-voter was plaintiff John Serrano Jr., as I document in Fischel [2004].)

After the *Serrano*–Proposition 13 event(s), this system was no longer operative. Brunner and Sonstelie, who have authored a host of studies related to this topic, give a balanced and fairminded account of what happens when one of the foundations of the Tiebout model is overturned. One clear finding is that, although spending has been equalized to a degree greater than almost any other state, test scores have remained as unequal as before. Their most puzzling findings are that the state government has been unable to fully replace locally generated spending (as *Serrano* advocates had hoped) and California households still evidently care as much about their school districts now as they did before *Serrano* and Prop 13. One would have thought that California voters would be eager to embrace vouchers after school-funding equalization, but the two voter initiatives that proposed a voucher system were soundly rejected. Brunner and Sonstelie find that voters are unwilling to give up on their local public schools despite the loss of local fiscal control. The Tiebout model appears to work in ways that transcend its fiscal requirements.

The other Tiebout-inspired essay about schools is chapter 4 by **Jack Buckley** and **Mark Schneider**. "School Choice, Parental Information, and Tiebout Sorting: Evidence from Washington, DC" reports previously unpublished research based on surveys of Washington parents who chose charter schools over public schools. Buckley and Schneider are political scientists, and their work here reflects a dimension of the Tiebout model that economists often overlook: How much knowledge do people actually have about the local public sector?

Buckley and Schneider's survey of parents who recently chose charter schools suggests a pessimistic answer. Parents who made this choice actually had more erroneous information about the quality of their schools than those who did not. This reinforces a long line of research by political scientists that suggests that the term *informed voter* is an oxymoron. If this were all we knew about public-sector choices, the Tiebout model would have long ago been dismissed as laughably irrelevant.

Yet, it is *not* dismissed, just as Richard Lester's (1946) evidence that business executives are interested in almost anything but maximizing profits did not deter economists from using that vital assumption. The obvious reply to the political science critique is evidence from the housing market. Someone must know about intercommunity differences in schools, since they are priced pretty accurately in housing values. And most economic tests of the median-voter model support the idea that voters in all but the largest school districts can evaluate spending programs (Inman 1978; Romer, Rosenthal, and Munley 1992). Yet, the Buckley and Schneider evidence should be a useful irritant to economists. More research needs to be done to understand just how the public-sector market works.

Government Structure in Metropolitan Areas

Tiebout himself intended his model not to have a political sector. The whole idea was to show that an economic decision—choice of residence—could determine an efficient set of local services. This may be why the model is more highly regarded by economists than by political scientists and law professors, whose stock in trade is the political sector. (As the presence of representatives of those disciplines in this volume suggests, however, the Tiebout model cannot be conveniently ignored.) Most economic research since the 1970s, however, has conceded that some sort of political structure must be assumed in order to generate any supply of public goods. The structure most frequently imposed has been the median voter model, which assumes the majority of voters get their way, regardless of the political structure. Coupled with the assumptions that communities are numerous and residents are mobile, the efficiency conditions of the model continue to hold.

William Hoyt's chapter 5 reexamines this sanguine conclusion. In "Imperfect Competition Between Communities, Politics, and Capitalization," he points out that many American metropolitan areas are not characterized by numerous, small localities. Some areas have only a few local governments, and others have many small suburbs plus one or two large cities. Using the tools of public choice and microeconomic theory, Hoyt explores the implications of these imperfectly competitive local government structures. One important possibility is that very large local governments might not be responsive to the majority of voters. They may instead be captured by development interests or entrenched bureaucracies.

Hoyt's most provocative finding is that in large municipalities, in which bureaucrats are more likely to rule, the benefits and burdens of taxes and spending will not be fully capitalized in the city's housing prices. Some of the burdens will be exported to other jurisdictions. As a result, housing prices become less accurate as a measure of public service benefits throughout the metropolitan area. While his empirical evidence on this is not conclusive, Hoyt's theory may explain why the Tiebout model tests best in the suburbs rather than in central cities.

Zoning, Exclusion, and Segregation

Aside from education expenditures and property taxes, the Tiebout-relevant aspect of local government that most concerns social scientists is zoning. Tiebout mentioned zoning as a means of limiting population growth in a community in order to achieve optimal size. However, the modern application of zoning to the model did not arrive in economics until Bruce Hamilton (1975) introduced it. His modification of the Tiebout model required localities to use zoning not so much to limit population as to limit "free-riders" who would pay less in property tax than they consumed in services. This came to be known as "fiscal zoning." I have argued that American zoning is perfectly capable of doing what Hamilton assumed it could do (Fischel 1992), and the main reason municipalities do not use it to its full effect is because of state and federal constitutional constraints (Fischel 1995).

Chapters 6 and 7 address a more sophisticated—some would say "insidious"—role for zoning. In chapter 6, "Exclusion's Attraction: Land Use Controls in Tieboutian Perspective," **Lee Anne Fennell** surveys and analyzes the role of zoning as a means of excluding people. At one level, the term *exclusionary zoning* is redundant. Zoning necessarily excludes something if it has any effect at all, and excluding undesirable uses of property is at its foundation. But Fennell, a prolific legal scholar, explores how zoning can be used to shape the human personality of the community as well as its fiscal and environmental qualities.

One of the consistently applied constitutional constraints on zoning is that it cannot discriminate by race or by most other demographic characteristics, such as religion and income. But this does not eliminate the desire to do so. Fennell describes the many indirect ways by which constraints on the use of land could be motivated by their effect on the type of people who might live in the housing development that is subject to local zoning approval.

My own suspicion about zoning motives is that Fennell is often right: The object of NIMBYism is not necessarily the proposed apartment building's blockage of light and air, or even its smallish contribution to the tax base, but the possibility that poorer people will occupy it. And I must add that the super-rationality that Fennell imputes to zoning authorities is not so far from the truth. People motivated by the desire to protect and enhance their own property will think through the long-term implications of land use policies. (This truth, incidentally, undercuts most theories of the "race to the bottom" among local governments, which cast local governments as witless pursuers of industrial tax base, heedless of its adverse effect on community environments.) The difficulty for external overseers of the process, such as courts, is winnowing the legitimate from the illegitimate motives. Suburban officials have learned not to go on record as saying they opposed the development because they feared that minorities would occupy it. Talking only about preserving "small-town character" keeps these small-town characters out of court.

In an extended comment on Fennell's chapter, Robert Ellickson raises a contrarian point. If American suburbs are insufficiently diverse with respect to race, income, and ethnic origin, what is the optimal level of diversity? To use an example that he does not, American universities are almost all committed to "diversity," with the goal being to have the groups of American society proportionately represented in their student bodies and faculties. But if they were to accomplish that, all universities would look alike in this dimension. There would be no diversity from the point of view of those choosing to attend or work at any one of them. Among universities, that may be regarded as a good thing, but at the municipal level, Ellickson reminds us that it could mean no more Chinatowns, artist colonies, student ghettos, or gay neighborhoods. When Ellickson turns to proposed remedies for exclusionary zoning, however, he finds that few critics of the suburbs are willing to impose effective legal obligations on municipalities to become diverse. He concludes from this that, using Fennell's coinage, "we are all Tieboutians now."

Yet, Fennell's criticism of manipulative exclusion does not necessarily have to resolve Ellickson's concern about optimal diversity. The analogy that might be useful is that developed by William Baumol and Wallace Oates in *The Theory of Environmental Policy* (1975). Like most economists, they point out that one could go too far in pursuing environmental goals, which implies that there is some "optimal" level of pollution. But they acknowledge that neither they nor anyone else knows what that level is. Their recommendation, which was the innovative part of their argument, was to approach policy by increments. If you think you have too much pollution, try to reduce it in the most cost-effective way. After you've done that and you still think there is too much pollution, do it again.

Returning to Fennell's point, the remedy for exclusionary zoning might be similarly incremental. If the suburbs don't have enough diversity, leaning on them to allow some lower-cost housing is the right idea. The error to avoid—an error I think was made by the New Jersey Supreme Court's "regional fair-share" remedy for combating exclusionary zoning—is to adopt a grand solution and then wash one's hands of it.[5] Like environmental quality, the desirable mix of housing in a region is a moving target that must constantly be reevaluated.

The other chapter about zoning is that of **Stephen Calabrese, Dennis Epple,** and **Richard Romano**. The method of chapter 7, "Nonfiscal Residential Zoning," could hardly be more different from Fennell's institutional examination. Their article must be seen as a step in a career-long enterprise by Epple and his various coauthors to ground the Tiebout model in a formal economic theory. In particular, they have explored the role of zoning in shaping the character of communities. Among the more interesting is a model in which income-segregation can occur without zoning. The land market coupled with a plausible demand for public goods causes households to spontaneously sort themselves. This forestalls the endless chase of the poor following the rich in order to "free-ride" on public goods.

Building on this highly abstract model, Calabrese, Epple, and Romano explore the possibility that residents of already-sorted communities might want

[5] The *Mount Laurel II* language that exclusive New Jersey municipalities are fond of citing is this: "Finally, once a community has satisfied its fair share obligation, the *Mount Laurel* Doctrine will not restrict other measures, including large-lot and open area zoning, that would maintain its beauty and communal character." *Southern Burlington County NAACP v. Township of Mount Laurel,* 456 A.2d 390, 421 (N.J. 1983).

further sorting. The reason for this desire is that residents realize that there may be spillover benefits from having higher-income people live in their community. The spillover benefits are not directly fiscal in nature, as they would be in the Hamilton-inspired version of zoning. In the Calabrese-Epple-Romano world, residents get some direct benefits from living among the higher-income residents. (If this seems unappealing, the model could be motivated as well by living among the well educated.) In order to take advantage of this version of peer effects, the high-income suburb (or subdivision) has to effectively expel some of its lower-income (but hardly poor) residents. This cannot be done literally—almost all zoning changes "grandfather" previously existing uses—but one could think of a planner who goes through each stage of settlement on paper and then puts the final policy in place.

The interesting thing about this unsavory activity is that in the Calabrese et al. model, the "city" jurisdiction is actually in favor of the suburban expulsion of the not-rich-enough residents. This is because the excluded suburban residents must now live in the city, and they are richer than the other city residents. The involuntary immigrants create a spillover benefit for the city residents, who benefit from proximity to them. It's kind of like the jayvee basketball team benefiting because the varsity has cut some of the shorter players from its roster. Only those who lost their place are upset.

As abstract as this model is, it implicitly offers an explanation for why exclusionary zoning persists. State authorities can, in principle, override local zoning anytime they want. If cities were really that unhappy about suburban exclusion, they could get their state legislative representatives, who typically outnumber those from the most exclusive suburbs, to disallow the practice. In reality, though, exclusion from the suburbs of the rising middle class may be beneficial to the central city in that it stems the "flight to the suburbs" by their middle class.

Extensions of the Tiebout Framework

Chapters 8, 9, and 10 invoke the Tiebout model in settings that go beyond intrametropolitan choices of local services. Each chapter is distinctly different, but they share the expansive view of Tiebout's basic idea that has accounted for its growing influence in recent years. While I share Oates's skepticism about this expansion from the intrametropolitan setting, these chapters do illustrate the attraction of Tiebout's most basic idea: People can choose different locations, and when they do, public-sector considerations play a role in their choices.

Rick Hills is another law professor who has employed Tiebout-style arguments in his scholarship. The title of chapter 8, "Compared to What? Tiebout and the Comparative Merits of Congress and the States in Constitutional Federalism," plays on a theme that Oates raised in chapter 2: How should the

Tiebout model be evaluated normatively? Tiebout's original work sought to show that choosing among localities could mimic the private market. But in the real world, the choice is usually not between local government provision and private provision. The most pressing issues are local governments versus state and national governments. Only a few scholars seriously test the regulation of environmental quality by local governments against its provision by private firms. The environmental question is usually cast in terms of centralized versus local government regulation. Hills thus frames the question for legal scholars as one of federalism—the balance of authority among levels of government—not between government and private actors.

Hills notes in chapter 8 that the Tiebout model was initially disparaged by legal scholars, and he gives a thorough account of what they found wrong with it. Most economists would point out that their objections were based on distributive concerns about excluding the poor, while Tiebout's argument was about efficiency. Even in a society that everyone agreed was egalitarian, one would still expect sorting based on different preferences. And as chapters 2 and 4 by Oates and by Brunner and Sonstelie (in this volume) point out, the so-called homogenous suburbs and their school districts are in reality far more diverse than they are usually assumed to be.

The main efficiency problem that lawyers see in small governments is that they have so much perimeter relative to land area. This makes them likely to externalize costs on their neighbors; the paradigm is the municipal dump deliberately located on the downwind border. This is a problem more often invoked as a hypothetical example than examined empirically. My own investigation of an allegedly "beggar-thy-neighbor" landfill found that there was less to it than meets the nose, given the freight it is made to bear in the literature (Fischel 2001, ch. 8). However, Hills's rhetorical strategy is to concede the criticisms of Tiebout but then turn and ask whether centralization of local services does not have its own problems. The systematic error that Hills finds most relevant is that Congress is structurally inclined to make policies that are too uniform for a nation with as much diversity as the United States. Tiebout's legal critics should not get away with the implicit assumption that more centralized provision of public goods operates costlessly.

Despite a general mistrust of local government, legal scholarship has conceded to the Tiebout-style model in one particular area. The explicit reason for the federalization of environmental policy in the early 1970s was a story about the "race to the bottom" if regulation were left to the states. Borrowing freely from an article by Oates and Schwab (1988), Richard Revesz (1992) argued that the race-to-the-bottom scenario was founded on faulty analysis. States do like to attract businesses for employment and fiscal concerns, but those same citizens who want jobs and lower taxes also want better environments. As long as there is a reliable connection between state decision makers and their citizens—frequent elections would usually do it—it seems unlikely that states would foul their own nests.

Hills believes that Revesz won this argument. The race-to-the-bottom story is now confined to sniping about stateline spillovers and biased representation, not about chasing industry for fiscal gain. But the question here is whether this is a victory for the Tiebout model. The theater of operations is not a metropolitan area but the entire nation, among whose states people vote less often with their feet. Can the Tiebout model be stretched so far from its origins? It is tempting to say that Hills tells no more than a story about fiscal federalism, which does not inherently require mobility. Yet, there is mobility by firms among states; they do shop around for favored jurisdictions for tax reasons in the Oates-Schwab model. It might be best to refer to this class of models as "Tiebout-inspired."

Perhaps because it does stretch the model some, Hills draws a modest normative lesson from the merits of the Tiebout-among-the-states model. He uses it to make the case for judicial forbearance about interstate tax competition. Federal judges have been urged to use the "dormant commerce clause" (see Hills for a good explanation) to stop tax competition for business among the states. Hills suggests that having judges weigh in on this issue might have some unintended consequences. Lacking a Tiebout-inspired understanding of the role this competition plays in allocating industry, a decision to limit it could do more harm than good.

Chapter 9, by **Edward Cartwright, John Conley,** and **Myrna Wooders**, represents another branch of the Tiebout model. This offshoot was started by James Buchanan's economic theory of clubs (1965). Buchanan was aware of Tiebout's work (see his 1961 comment on Tiebout), though he did not cite Tiebout's original paper in his seminal article on clubs. In any case, club theory is more general than Tiebout's model. The clubs do not have to be set up to enjoy public services; their members can jointly consume any service, such as a swimming pool or Jerry Lewis movies. But club theory is also in a sense easier than Tiebout's theory because it does not necessarily involve geographic space. The problems of proximity are less imperative in clubs, since it is in principle easy for people to form their own club. The scarcity of land for housing, employment, and public facilities makes Tiebout models more difficult to solve.

The Cartwright-Conley-Wooders chapter, "The Law of Demand in Tiebout Economics," uses club theory to address a Tiebout-like problem. When potential clubs are scarce, as they are in a Tiebout world, members with different tastes and personal characteristics may be matched up less than perfectly. Starting from such a position, the authors ask what would happen to members' satisfaction (utility) if more members were added. In the conventional Tiebout model, adding more members to a community first lowers average costs to existing residents, but eventually the public service becomes congested, and additional members would reduce the satisfaction of preexisting residents. Cartwright et al. show that in their Tiebout-club model, it may be possible that this would not happen if the additional membership causes the matches of personal characteristics and tastes to improve. In some cases, crowding is offset by what might be called "the more, the merrier" principle.

This is a highly abstract and complicated model, and I would recommend reading Jan Brueckner's commentary as a guide to understanding it. The chapter demonstrates that economists have gone well beyond the issue of simple spatial crowding. They have begun to address the complexities of interactions among members of a group that engages in cooperative endeavors. I would speculate that the finding in chapter 9 suggests an interesting agglomeration economy that arises from club size. This could help in modeling the effects of urban size on productivity and consumer welfare.

Chapter 10, "Tiebout—Stability and Efficiency: The Examples of Australia and South Africa," by **Jeffrey Petchey** and **Perry Shapiro**, invokes yet another extension of the Tiebout model. The issue here is the use of central government fiscal aid to national regions to regulate the migration of population, usually to encourage people in poor regions to stay put. The seminal paper in this literature is Flatters, Henderson, and Mieszkowski (1974), which does not mention Tiebout at all, though subsequent literature has labeled this a Tiebout-style model.

The Flatters et al. model does invoke geographic migration and public goods. However, the jurisdictions are very large, so this might be called the regional-migration variant of the Tiebout model. The difference this makes is that relocation by migrants affects total labor supply and hence the productivity of labor in the region. In the traditional Tiebout model of local public finance, migrating from one community to another has no effect on employment. In Tiebout's original formulation, there is no labor income; in the more realistic variants on it, there are enough small communities within a labor market that community choice can be made without changing jobs.

When regional migration is considered, there is a potential conflict between private and public goods. A regional differential in the provision of public goods might draw labor to the region with the higher-quality services. But this immigration might cause labor productivity to fall in the place receiving the new workers. Petchey and Shapiro apply this model to a characterization of the history of Australia and, more loosely, to South Africa.

Australia, like many countries other than the United States, has long had a policy that transferred fiscal resources from some regions to others. Petchey and Shapiro describe the history of this policy as an attempt to prevent the breakup of the Australian confederation in the early twentieth century. The question they ask is why this policy has persisted long after the danger of secession has passed.

In principle, the Flatters et al. model could be used to justify some form of interregional fiscal transfers. The efficiency rationale would be that improving public goods in areas from which citizens might choose to migrate would dissuade them from migrating and thus maintain equal labor productivity among regions. It appears, however, that neither Australia nor South Africa meets the necessary conditions for the policies to improve the well-being of their citizens.

Conclusion

Like many influential models, Tiebout's has come a long way from its modest origins. It is now the accepted benchmark for local government in metropolitan areas. The economic analysis of American public schools, financed by thousands of local districts, has been transformed by it. Scholarly discussions of land use regulation do not go far before invoking the Tiebout model. Its influence has extended to political science and legal scholarship. It has been applied to nonmetropolitan settings such as club theory, regional fiscal transfers, and interstate competition for business.

Tiebout's influence does not stem from the rigorousness of the original article itself. As a model of local public finance, it is, as Oates points out, seriously incomplete. As a theoretical alternative to the ballot box, it works only under the most restrictive set of assumptions. What, then, explains its robustness? The answer appears to be that Tiebout saw a new way of looking at economic and political problems. Instead of assuming a stationary population in dealing with collective decisions, the world Tiebout envisioned is one of people in motion. His insight provided a new dimension in which economists, and now other social scientists, can rework familiar problems. The Tiebout perspective, rather than the original model, is what is so attractive.

How did he come up with this? Tiebout's theoretical methods were conventional, and his empirical evidence consisted of casual, though astute, observations. I once asked his boyhood friend Ross Worn (who married Tiebout's sister) if their Greenwich, Connecticut, background offered any clues. Mr. Worn could not recall anything, though he, like almost everyone else who knew Tiebout, remarked on his friend and brother-in-law's sense of humor. Maybe that was it. It could be that Charlie's irreverent sense of humor was what liberated him from conventional thinking and gave us the enduring and entirely serious Tiebout model.

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