

Segregation and Spatial Inequality: Enduring Lessons and Unresolved Challenges. John R. Logan in Conversation with Niccolò Morelli

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Abstract

This interview with John R. Logan reflects on more than four decades of contributions to urban sociology, from *Urban Fortunes* to contemporary research on segregation and spatial inequality. Logan retraces the intellectual influences that shaped his work, particularly Immanuel Wallerstein's macro-comparative perspective and Paul Lazarsfeld's quantitative methodology, highlighting how he combined structural theorizing with rigorous empirical analysis. Revisiting the legacy of the Chicago School, he argues that theories should be judged not as right or wrong, but as more or less useful for addressing specific questions about urban life. A central theme of the conversation is the persistence of racial and ethnic segregation, examined through a long-term historical lens that traces contemporary inequalities back to the late 19th century. Logan emphasizes the importance of spatial scale, the interplay between state and market, and the value of mixed methods — especially the integration of ethnography with large-scale data. Ultimately, he defends the enduring relevance of urban sociology, insisting that the discipline advances not through data alone, but through the continual reframing of fundamental questions about inequality and the organization of space.

Keywords: Logan; Segregation; Inequalities; Urban Fortunes; Mixed methods.

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1 Introduction

John R. Logan is Professor of Sociology at the Department of Sociology, Brown University, where he arrived in 2004. He completed a PhD in Sociology at the University of California, Berkeley in 1974. He was a Lecturer and Assistant Professor in the Department of Sociology at SUNY Stony Brook from 1972 to 1980. Before joining Brown, he was a Professor at the University of Albany, where he served as Director of the Lewis Mumford Center for Comparative Urban and Regional Research from 1999 to 2004 and of the Center for Social and Demographic Analysis from 2003 to 2004. From 2004 to 2016, he served as Director of the research initiative on Spatial Structures in the Social Sciences at Brown. In 1987, he published *Urban Fortunes: The Political Economy of Place* with Harvey Molotch, in which they introduced a political-economic approach to urban analysis. The book was re-edited in 2007, on the occasion of its 20th anniversary. John Logan's research interests cover a variety of areas, from diachronic analysis of segregation, with particular interest in migrants and racial minorities, to comparative urban studies. He has also been among the first social scientists to study Chinese cities. Recently, he's been using contemporary and historical census data to study changes in residential patterns, with particular interest in micro longitudinal data.

The interview was informal, and the topics ranged from his first professional inspiration to the impact of *Urban Fortunes* on the sociological debate to current methodological and theoretical challenges in studies of urban segregation.

2 The First Steps of Logan's Career

Niccolò Morelli: So, I'd like to start with your academic path. You began your career as a community organizer and urban planner after completing your master's degree at the university, where your thesis was supervised by Immanuel Wallerstein. How did he influence your work and your approach to research, particularly in relation to long-term comparative studies, diachronic analysis, and your interest in understanding the structure of segregation?

John Logan: It's interesting because there were several people who influenced me, but I think Wallerstein is probably the one who, in terms of the work I've done, has been the most important. At that time, Wallerstein was doing quite straightforward cross-national studies (Wallerstein, 1984). He would collect data on about 50 countries, and then analyze the variables and their relationships (Wallerstein, 1974, 1980 & 1989). That kind of study — quantitative cross-national research — was very common, and I learned a lot from him about how to organize data. I read many articles in Italian, Spanish, and French journals, many of which focused on African countries — Africa was his primary focus at the time (Wallerstein, 1961, 1964 & 1967). I think I learned from him how to do comparative research from that angle.

After that, however, he made a significant shift toward his world-systems approach (Hopkins & Wallerstein, 1967). I stayed in touch with him after I moved back to California. He came to Stanford, to a research center there, for a year, and I remained in close touch with him and became aware of what he was doing. He had a strong influence in pushing me toward a more macro-sociological approach to comparative work, political sociology, and development issues — and that suited me very well. What's curious is that I had already been “infected”, so to speak, by the quantitative cross-national approach, the idea of collecting data and looking for patterns in it. I think I've maintained both of those research styles.

Most of what I've published has been in that quantitative mode, but instead of countries, I've looked at cities, neighborhoods, and communities. I continued that line of work, but in the back of my mind, there has always been an interest in understanding the nature of the larger system in which all of this is happening. What's the nature of a whole society, compared to another whole society? To what extent are they similar or different?

So, you put your finger on something I wasn't very aware of myself: I got both of those directions — macro-sociological theorizing and comparative quantitative work — from Immanuel Wallerstein.

NM: And about Lazarsfeld, it seems like another important influence on your quantitative research.

JL: Yes. I took a course with Lazarsfeld during my time at Columbia, and I also worked with him on a project exploring the applications of sociology to real-world questions — essentially, applied sociology. That was his background. The Bureau of Applied Social Research focused on securing funding from the private sector to conduct quantitative research that the private sector would pay for and then utilizing the data to develop methods. I became very interested in quantitative methodology and multivariate analysis. It started with Lazarsfeld's strong emphasis on cross-tabulations and their analysis to get at questions of causality (Lazarsfeld, 1958): you see an association between two things, but why is it there? I've always found that to be a very interesting kind of problem. During my early years at Stony Brook, when I was an assistant professor, I taught graduate courses in statistics and multivariate analysis. I had to teach myself what others were doing by analyzing correlation matrices and other techniques. But at the core, I was always thinking about relationships in a very descriptive way: What's the frequency distribution of this variable? How does it relate to a single other variable? Of course, that's not an adequate approach on its own for a full analysis, but it led me to want to see the data more directly, which is what you do with cross-tabulations. When you run a regression analysis, you obtain a set of coefficients, and you rarely see the actual data. I prefer to see the raw data; that's where the information is for me. The advantage is that I sometimes become aware of questions and patterns that would otherwise remain hidden in a correlation matrix, where a number might not stand out.

3 *Urban Fortunes: An Enduring Legacy*

NM: Focusing now more on your approach and work, you mentioned earlier your interest in macro-sociological theorizing and comparative quantitative work. This brings me back to one of your major works, *Urban Fortunes*, published nearly 40 years ago (Logan & Molotch, 1987). That work, with your co-author Harvey Molotch, was very important for the field and for the discipline on the theoretical side. What I find interesting is that the book was perceived as a challenge to the Chicago School (Clark, 2006), yet you are also very interested in structural analysis. So, how do you relate to the Chicago School approach? How do you see the relationship between your work and the tradition of social ecology?

JL: It's a book that tries to put many ideas together. Although most of what I've published has been based on analysis of data and evidence, in terms of what most motivates me, I think I am fundamentally a theorist. I'm interested in how to interpret things. I'm very happy that *Urban Fortunes* has survived, and that people still read it and find it useful. It was an intervention in a debate within the social sciences, marking a shift from a structural-functionalist to a

political-economy perspective, in response to the demands of the time — the 1970s, 1980s, and 1990s (Gottdiener & Feagin, 1988). Its purpose was to develop a way of thinking applicable to many people's work.

Examining the Chicago School approach, we can then discuss its limitations. In the 1970s, 1980s, and 1990s, many of us argued that the Chicago School overlooked politics and class antagonisms and didn't really know what to do about race (Logan, 1978; Sassen, 2000; Smith, 1985). We said they took capitalism for granted and left out so much. On the other hand, we can ask: would it have been reasonable to expect these issues to be foremost in their minds in Chicago at that time, given what was happening around them? What was the nature of economic development and population shifts? What did the market look like? How did it operate? What was the role of the state in the 1920s, before Fordism? I won't go further into that now, but the point is to put ourselves in that perspective.

Do we have the right to impose a moral standard ourselves? Did the Chicago School have that right? Is that what we think they should have done? Today, we say you must have a normative basis for your work. So, I think that's a provocative way to put the Chicago School and its concerns into a contemporary context, and then ask: what kind of theory did they have? What is the nature of their theory?

They weren't simply "wrong". Fifty years ago, I thought the Chicago School was wrong; now I don't. A theory is not right or wrong; a theory is useful or not. Is it informative or not? It points you in directions that make sense to you and that you can do something with — or it doesn't. That's how I judge a theory now. I see many ways in which human ecology can be applied to help explain certain phenomena. It's not wrong; it's just very incomplete.

In my classes, I start the semester talking about Park and Burgess. This semester, I also assigned the first two chapters of *The Gold Coast and the Slum* by Harvey Zorbaugh (1929). That's old material; it isn't central in current conversations, but it's actually a very interesting study of two neighborhoods at opposite ends of the class spectrum. You could say Zorbaugh is really studying neighborhood inequality — just like urban sociologists today. But that's not how he framed it. He was interested in urban disorder in the slum, a concern of the Chicago School.

So, I ask the students: Is that a legitimate way to frame the question of urban sociology? Is moral disorder something we can study? Then I gave my students an assignment for the first essay of the semester, like a midterm essay: "If you were Castells, or David Harvey, or Harvey Molotch, or Neil Brenner, or Saskia Sassen, and Zorbaugh were now your advisee at your university, what would you tell him to do? How would it benefit his work, and how would he respond to your advice?" This raises questions about what he was interested in understanding, what *you* are interested in understanding, and whether there is convergence between these.

In the end, the point of view I try to develop is this: it all depends on the questions about urban life you are trying to understand and on how the theoretical perspective you adopt can help you understand them better.

4 Challenges for Urban Segregation Studies

NM: I'm curious about your reflections on research methodologies, because I read your article where you discuss the pros and cons of the use of census data, the new challenges, and then also reflect on mixed methods, and in particular, you identify urban ethnography as a way to integrate census analysis (Logan, 2018). I'm very interested in this idea of a mixed approach,

especially because, on the European side, I feel there is a rising polarization between quantitative and qualitative sociology, particularly in urban studies. That piece was, and still is, very interesting to me.

JL: Yes, I think we need to move in the opposite direction from that polarization: the two approaches must come closer together. When I was a graduate student at Berkeley, I spent a couple of years as a full-time community organizer around Palo Alto and East Palo Alto, addressing issues such as race relations, school segregation, environmental policy, affordable housing, and local politics. I wasn't "doing a study"; I was in the field every day, talking to people and trying to make a difference. Looking back, it was essentially two years of intensive ethnographic fieldwork, and I've drawn on that intuition and common-sense understanding of local life ever since.

For that reason, phenomena like racial segregation or unequal access to services are not, to me, just variables in a dataset. My basic view — and what I encourage others to do — is to gain some real-world experience, even if you want to focus purely on quantitative analysis. Go into a neighborhood, learn how it works, and then ask yourself: "What do I know from experience that others don't, and how does that change how I see the world compared to the research literature?"

In that sense, the purpose of ethnography is to build a common-sense understanding. That is its competitive advantage: it draws on experience, observation, and even our own biases, which themselves come from experience. Without that, all you have is the correlation matrix — like trying to work with your hands tied behind your back.

NM: I've been fascinated by your approach of using a long-term perspective — for example, starting from one century or one hundred and fifty years ago. I'm interested in understanding what led you to feel the necessity to analyze data from such a long historical perspective, this long diachronic analysis. From your perspective, what are the advantages of using this long-term lens?

JL: What drew me to a long historical perspective is the conviction that what we see today has deep roots (Logan & Stutts, 2011). Space, neighborhoods, and the boundaries between them have long been used to secure advantage, exclude groups, and protect status — sometimes through markets, sometimes through politics. The form varies across places (China, Eastern Europe, the U.S., France, Italy), but the underlying dynamic is remarkably similar. Looking historically lets you see both continuities and context-specific differences.

That's why I look back to the 1880s and 1920s in the very same cities we study today. The experience of earlier immigrant groups is revealing. Italians, for example, arrived with major disadvantages, were near the bottom of the labor market (apart from African Americans), and remained highly segregated and dependent on limited opportunities for a long time. Comparing that trajectory with African Americans is especially instructive, and it raises a central question that remains with us: why did some groups achieve social and residential mobility in the way the Chicago School expected, while African Americans largely did not, and why do some groups (e.g., Mexican Americans or Puerto Ricans) show patterns closer to the African American experience?

The key point is that contemporary disadvantage is not a recent development: it is produced and reproduced over time. In fact, African Americans were already highly segregated in Northern cities even when they were only a tiny share of the population (Carlson et al., 2025; Logan et al., 2024). This suggests segregation did not "begin" with 1920s zoning, restrictive covenants, or 1930s redlining. If we want to understand responsibility, we must go back fur-

ther — at least to 1880 — and confront segregation as an expression of the broader social order and its entrenched racism.

NM: I saw that article in *City & Community* last year (Logan et al., 2024), and it was interesting because you also introduced the question of the *scale* of segregation. At times, the relevant scale was the neighborhood; at other times, the street. That's interesting, because we often treat neighborhoods as monolithic, but sometimes, for analyzing segregation, other units of analysis are better suited.

JL: I'm certainly a strong advocate of understanding spatial scale and spatial patterns. It's not just what you can measure with a segregation index; it's *where* people are — in relation to other people — and how that matters. I've been especially interested in the housing market, access to housing (see Alba & Logan, 1992), and, to some extent, segregation. Sometimes segregation is practiced at a very large scale: for example, in the 1950s, African Americans were largely excluded from suburban residential developments in the United States (Logan et al., 2023). In 1910 and 1920, in Chicago, I found (as others have) that African Americans were living in most parts of the city, in various locations (Shertzer et al., 2016). But wherever they were, they lived in small pockets; they were not spread out. They lived in the worst buildings on the block. Landlords who had better buildings wouldn't rent to them. On that spatial scale, their ability to find decent housing at a fair price was severely constrained.

We organize access to quality services with these kinds of boundaries, and that's not at the neighborhood scale — it's larger. We also organize politics at various spatial scales. Currently, we frequently discuss the gerrymandering of congressional districts and how shifting boundaries can disproportionately advantage some groups and disadvantage others. That's being done at a very large scale, too.

All these scales matter, but in different ways. We need spatial data and spatial analysis methods to address these variations.

NM: In relation to your contribution to the study of spatial analysis, you were among the first to work on Chinese cities. What can we learn, comparatively, from the study of Chinese cities?

JL: There was an entire society about which I knew nothing, and I didn't think anybody knew much. In the 1980s, it became suddenly possible to travel there and learn more. There were so many questions one could ask by opening this big "black box" of China.

From a theoretical standpoint, the key question was whether a society with such a strong central role for the state would have significantly different relationships among people and groups, a distinct urban structure, and a different relevance of neighborhoods, among other things (Bian & Logan, 1996). Could it possibly be like what we know from a market society in the U.S. or Western Europe? How different is it really? Are there commonalities? What is different, and why?

When China began to introduce market mechanisms, I found this particularly intriguing because I had long argued against a purely market interpretation of American society and urban development (Bian & Logan, 1996). I had argued that the state, politics, power, and collective action are very important in American urban life (*ibidem*). Now, if the state *introduces* the market, can we gain a better understanding of the relationship between the state and the market?

That brings us back to classical sociological theorists on the relationship between state and market. As I've worked on China, that question has always been in my mind: What am I

learning about the relationship between state and market in the United States by seeing how it evolves in China, where we know the market is a creation of the state? Over time, I've found many similarities and differences stemming from the different contexts. I've had the opportunity to get to know many people, and since urban social science was largely absent in China when I began, I've been able to contribute to its development and support young scholars there.

I think I've made more of a contribution to the development of new generations of scholars in China than I have in the United States, actually. That contribution is sort of invisible from an American perspective, but in terms of my impact, I'm very happy with it.

NM: What about European cities?

JL: I've always had a strong connection with Western Europe. However, in terms of the questions that interested me, studying Spain and Portugal (Logan, 1977) led me to comparisons with South America — specifically, underdevelopment and dependent development. So, I was drawn in that direction.

Europe is becoming increasingly challenging and interesting due to new developments, such as the rise of the far right in many European countries. Now I think about comparing that to the U.S. Mass migrations — from North Africa, the Middle East, Eastern to Western Europe — also raise new issues that I see as good comparisons to the American case. So, Europe is becoming more interesting from that perspective.

NM: I agree, and I would add the relationship between government, cities, people, and the market. With the weakening of the welfare state, linked to the rise of the far right, we see an increasing inequality (Hemerijck, 2013). In Western Europe, segregation existed, but welfare policies mitigated its impact (Musterd & Ostendorf, 2013). Now that the welfare state is weakening, segregation is becoming a rising issue (Musterd et al., 2017). Another important issue could be age segregation: Europe is ageing quickly, and we still do not know how this is affecting social dynamics at a fine scale such as neighborhoods.

JL: There is so much happening in Europe now that raises questions we hadn't anticipated. That's part of the thrill of being a social scientist: it's your job to keep an eye on things, feel the pulse of what's happening, and be drawn to new questions. Many of them involve new comparisons: Spain and South America, the U.S. and China, Europe and the U.S., and so on. Because things keep happening, we must be quick on our feet.

NM: In this sense, I'm also interested in the current challenges for studying urban segregation. One issue, in my view, is that in residential segregation studies, we usually look at "night-time segregation" — where people sleep. But now we also have the possibility to study daily mobility flows, for example, studying mobile phone use and other tools. This would enable us to analyze different dimensions of segregation: neighborhoods that are segregated both day and night, or neighborhoods that are mixed during the day but segregated at night. Is this something you've been analyzing?

JL: I think there's a lot of potential there. Some people talk about these as "activity spaces": the kinds of people you are in contact with in the different spaces you move through during the day (Müürisepp et al., 2022; Wong & Shaw, 2011). It's very challenging because, although we can track people using cell phone data or devices and ask them to keep diaries, the methods we have mostly tell us *where* people go and *how long* they stay, not what they do there. We can say: they go to work, we know where the workplace is, and we know they're there for eight hours. But what is their *position* in that space? What kind of relationships do they have with which

other people? We don't know. If we simply say: "This group is less segregated in the workplace than in the neighborhood", we really must ask: what is their position in the workplace? Do they have social contacts with the people whose offices they clean or repair? Are they the bosses? Middle management? Who do they talk to?

We don't have effective methods for collecting large-scale data on that. We know where they are, but we don't know what they are doing or what is happening there. If we truly want to know whether the day/night question is important and in what way, we need ethnography. We need to go where they go and observe what happens. What I suspect is that in mixed-activity spaces, there is not much actual social mixing — though there probably is some, and some of it may be significant. But we don't know. This is why there's a tradition in ethnography of spending time in bars or other public places: to see what people do when they are "hanging out" outside their home environment.

Some people *do* study workplaces ethnographically, though I'm not sure how many do so from this segregation perspective. So yes, if you really want to know about daytime versus nighttime experiences, you have to be there in the daytime and at night and see.

NM: So again, a mixed-methods approach. In response to new challenges and opportunities, we see that mathematicians and physicists are now working on urban segregation and publishing on urban topics. For me, this is interesting because it shows that other disciplines find urban analysis attractive. But at the same time, it raises the question: What is the state of urban sociology as a discipline at this moment? Are we interacting on equal footing with these disciplines, or are we letting other fields take advantage of these data and define the agenda?

JL: I'm happy for other disciplines to take advantage of such data and see what they can come up with. But in general, this broader field of "data science" and this fascination with big data have a fundamental weakness: they don't necessarily have a *question* to answer. They have a lot of data, and data has patterns. A mathematician, or someone who can do DNA sequencing, could also work with cell phone positional data. They can certainly *do something*. But what question are they trying to answer with it?

I have yet to see someone from that perspective who can independently say, "I've thought of a way this could be important for answering a question that social scientists have been grappling with — and now I can help, because my analysis sheds new light". I haven't seen that. A data scientist needs to work together with a historian, a sociologist, a cultural theorist, or someone like that. The social scientist might say: "Here's the kind of question I would frame, and I suspect your information might be useful, but I don't know how to make it useful". Then the data scientist can respond: "Now I understand your question; here's what I might suggest".

So interdisciplinarity is very possible, but as long as data scientists believe they themselves know what to do with the data *because* they can do something with it — "Look, here's a pattern; isn't it wonderful?" — the relevance remains unclear. That's the concern. It's hard for one field, from a very different perspective, to contribute to another unless the fields are really in touch with each other.

5 Asking Enduring Questions

NM: This leads me to another question. Reflecting on your book *Urban Fortunes*, which is now 40 years old and still relevant, and on the fact that this is the centennial of *The City* (Park & Burgess, 1925), and it's still in discussion, are we able to ask good questions today? Are we

lacking books or evidence that can open the discussion for decades? I don't mean we're no longer capable, but maybe the sheer volume of data means we can pose a question and get a yes/no answer the next day. There's less need — or less incentive — for medium- or long-range questions. So, are we still able to formulate good questions that truly advance urban sociology?

JL: I've never thought that was the problem. I have trouble *stopping* myself from starting new projects on new questions or angles. In part, it's because there are so many research opportunities, so many kinds of information becoming accessible even to those of us who are not ethnographers. The question is how to exploit these sources to answer the same questions we've had for a hundred years — and maybe to reframe those questions. I think we're very much midstream. We haven't come close to exhausting the possibilities of urban sociology.

There are many ways to interpret the patterns you see in the world and to tell a narrative about them. I'm always pushing to find better ways to understand things: to frame the questions more clearly, or in a way that fits better with what I see as the important issues in urban sociology, which are all about inequality and the organization of space and community in ways that reinforce social inequality (Logan, 2012). That's what I think is important. So whatever topic I bring up, I look for a way to formulate the question in that direction. Sometimes it's harder to publish a paper if people don't understand your frame of reference — if you're not replicating or directly building on what someone else did, offering just another variation. People understand that more easily. But if you say, "I have a different way of looking at this, and here is where that leads me", usually, reviewers almost always reject the first version of the paper, because I'm not communicating as much as I need to. There's a real need to communicate when you're pushing in a slightly different direction about how to frame a question. Sometimes it's because I don't fully understand the idea myself — it's a bit blurry, I sense there's something there, but I don't yet know how to express it. I feel like I understand something only when I'm finally able to communicate it to journal reviewers or readers in a way that makes them say, "Oh yes, there's something interesting here". At that point, it's because I, myself, now understand my idea. And it's very rarely about a wonderful data set or a strikingly useful application of a statistical method. That's never what makes a paper useful. It's always more about how the question is framed.

NM: Maybe, to rephrase a bit: we publish a lot of articles, in many journals, but very few articles or books truly "remain", in the sense that they open debates that last a long time. *Urban Fortunes* opened such a debate; *The City* did too. But today it seems harder — perhaps because of how research careers are structured. It seems more difficult now, at least to me.

JL: I agree that articles and books serve different purposes. An important book can have a much greater long-term impact than an important article. The same is true for Castells or David Harvey: their books had a big impact on how many people thought about many questions. That's one kind of intellectual purpose.

Another purpose is more mundane but at least as important, and it concerns my view of how science works as a social system. All the people trying to do studies, publish them, build on each other's work, bring out new facets of things we already thought we knew — what shows up in journals is like a conversation among specialists. They are trying to influence each other, to argue that "you should look in this direction, not that one"; that "you should take this to be true; what you thought was true isn't true". It's the back-and-forth of conversation among scientists, trying to move in some direction.

So, the big question is: can science progress? Can it move forward, build up? My view is: yes, but no single one of us does it alone. We do it in connection with *and* in reaction to

everybody else. It's not a perfect system. The decisions journals make about what to publish, foundations about what to fund, departments about whom to hire — it's a highly stratified system, and it's just as flawed as any other social system we are part of. But I think it *does* work — or at least it can work — and in many ways, it has.

When I think about writing a new article, usually I'm responding to something I've been reading. I'm trying to think: what can I add? What should I challenge, what should I reinforce, and how can I do it? I have a lot of experience working with data and putting it together in ways others haven't, drawing conclusions others haven't. It's fun, so I like doing it. But again, the reason to do it is to contribute to a collective enterprise.

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