Understanding Housing:
The Intellectual Legacy of John Quigley

by

Edward L. Glaeser
Taubman Center for State and Local Government
Rappaport Institute for Greater Boston
Harvard University

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Edward L. Glaeser

Harvard University and NBER

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Abstract

John Quigley was an important and beloved figure in housing and urban economics who died in 2012, but many non-specialists are only dimly aware of his contributions. This essay surveys his intellectual legacy, beginning with his early work with John Kain. Their research pioneered the application of hedonic price models to housing micro-data, and kicked off a literature that asked whether housing market discrimination led African-Americans to pay more for equivalent units. Over four decades, Quigley continued to develop housing price models, championing a hybrid approach that used both repeat sales and other transactions. Together with Robert Van Order, Quigley developed empirical tests of rational mortgage default and prepayments. Their work found that frictionless models are often rejected by the data. Quigley’s work on green buildings documented their high economic returns; his work on homelessness pointed to high housing prices and overly restrictive supply regulations. Together with Karl Case and Robert Shiller, Quigley demonstrated the strong effects of housing wealth on consumption.

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I. Introduction

In May 2012, the fields of urban and housing economics were diminished when John Quigley died at the age of 70. John’s insights were many; his written research materially enriched our understanding of the connections between the built environment and society as a whole. The great theme of his work is that structures matter: rising housing values increase consumption, housing market discrimination creates economically costly segregation, and housing structures shape our energy consumption. Yet John’s impact went far beyond his published work, through his encouragement of young scholars, his earnest enthusiasm towards new research, and the leadership that he brought to urban and housing economics.

The basic facts of Quigley’s career are straightforward. He received his Ph.D. from Harvard in 1971, taught at Yale from 1972 to 1979, and was at Berkeley from 1979 until his death. He was a major presence both in the Berkeley economics department, where he served as Chair, and in the business school, where he was an intellectual anchor of the real estate program. He was the enduring face of Regional Science and Urban Economics, serving as editor from 1986 to 2003. He had a long list of distinguished students, including Katherine O’Regan, Steven Raphael, and Jesse Rothstein.

He authored 14 books and over 150 articles. Neither the pace nor quality of his academic output showed any signs of slowing over the course of his life. He had two enduring research agendas: the improved measurement of housing prices and formally understanding the behavior of mortgage defaults and prepayments. These were both technical subjects that Quigley furthered with major insights and with work in the weeds. While the Case-Shiller repeat sales index has become famous, Quigley championed a hybrid approach that used both repeat sales and other transactions. He was surely right that in many cases, researchers do not have the luxury of large samples of repeat sales. Quigley’s work on mortgages convinced both himself and the world the hyper-rational repayment models have trouble fitting the data, unless there are very unusual patterns of transaction costs.

Supplementing these two long-standing research agendas, Quigley repeatedly pursued opportunistic attacks on topics of particular policy interest. In the early 1990s, he focused on homelessness and did some of the best work in the area. Quigley emphasized that homelessness is explained in part by high housing prices, as well as the de-institutionalization of the mentally unstable and rising poverty. More recently, Quigley’s work emphasized the role that restrictions on building new housing played in pushing prices up, especially in California.

Together with Katherine O’Regan, Quigley pursued his early interest in racial segregation by looking at the causes and consequences of segregation. They pushed the literature forward by emphasizing that ghettos suffered more from intellectual isolation than from physical distance to jobs. Their largely correlational work seems to have been overturned by the results of the
Moving-to-Opportunity experiment (e.g. Katz, Kling and Liebman, 2001) but, as Quigley himself noted, that treatment may have been random but it was also small. Quigley’s view that ghettos help perpetuate unemployment by isolating children from the larger urban economy may well be vindicated by future research.

Quigley, along with Karl Case and Robert Shiller, wrote the now standard papers documenting that increases in the value of housing seem to have a larger impact on consumption than changes in the value of stocks. These results are hard to understand in a purely neo-classical model. We are all born short housing, for every current owner and future seller who benefits from higher housing prices, there is some future buyer who is losing out. In a sense, homeowners are less gamblers whose fortunes rise and fall with assets values, than hedgers who have insured themselves through ownership against changes in the cost of residential living (Sinai and Souleles, 2005).

Credit constraints provide a natural explanation for their facts, since added wealth that alleviates a credit constraint will impact consumption more strongly than added wealth that does not. If stocks are owned disproportionately by richer people who are not credit constrained, then changes in stock prices may do little to their consumption levels. Homes are owned by the majority of households, for many owners, rising housing values may increase consumption by alleviating credit constraints. Future buyers may also be credit constrained, and as a result, their consumption will not fall even if they know that they will have to pay more for future housing.

Finally, Quigley wrote two temporally distinct bodies of research on housing and energy. In the 1980s, Quigley emphasized that energy should be seen as one component of housing services: structure choices went together with energy choices. More recently, he focused on Green Buildings, emphasizing that they created significant economic rewards.

Many of his papers were marked by a consistent desire to use the tools of economics to understand the day’s pressing social problems, including homelessness, housing bubbles and racial discrimination. Quigley’s considerable talent as an economist was often put in the service of the world.

Almost all of his work highlights the deep connection between the built environment and human existence. Restricted access to housing options made life harder for African-Americans and exacerbated homelessness. Homes are the most important assets for many families, and changes in housing values whip around their consumption levels. Spatial segregation of New Jersey teenagers increases unemployment. The physical structure of society matters.

On a personal level, I knew John Quigley for over 20 years, and was privileged to collaborate with him on two distinct projects: an essay chronicling the achievements of John Kain (jointly authored with Eric Hanushek) and a volume celebrating the life of Karl “Chip” Case. Quigley
generously took over the management of the Case volume’s conference, when my third son was
born on the first day of the conference.

Quigley and I were both mentored by John Kain, and I first attributed Quigley’s kindness to me
as a by-product of our joint connection with Kain. I eventually realized that Quigley was
similarly generous to almost every young scholar. When we were working on Kain’s essay only
a few years ago, I did not imagine that I would be sadly writing a similar essay on Quigley so
soon.

Quigley was as generous as he was thoughtful. He will be sorely missed.

II. Quigley’s Early Years: The Air Force, Harvard, and Yale

Quigley’s career is clearly divisible into two periods: the 34 years that he spent as a mature
scholar at Berkeley and the years before that period. I begin with his early development.

The Air Force, Kain, and Quigley

Perhaps unusually, John Quigley’s life as a scholar began in the Air Force. After receiving his
Ph.D. at Harvard, Quigley became Kain’s student while Kain was teaching at the Air Force
Academy in the early 1960s. The distinguished education scholar Eric Hanushek similarly
entered Kain’s orbit in those years. Quigley served in the Defense Department until 1968,
working as an econometrician, where he managed to publish his first paper, an analysis of the

This paper began Quigley’s enduring interest in Sweden, which would later produce a *Quarterly
Journal of Economics* paper (Quigley, 1972) on the determinants of 19th century Swedish
emigration and a *Journal of Political Economy* essay (Klevmarken, Anders, and Quigley, 1976)
on the returns to age and experience among Swedish engineers. Quigley would later use
Swedish housing data to demonstrate how to improve real estate indices, and test the impact of
universities on local economic development by examining academic decentralization in Sweden
(Englund, Quigley, Redfearn, 1998; and Andersson, Quigley, and Wilhemson, 2004). In 2006,
Quigley became a foreign member of the Royal Swedish Academy of Engineering Sciences and
he received an honorary degree from that country’s Royal Institute of Technology the next year.

In 1968, after finishing his tour of duty, Quigley followed Kain to Harvard to receive his Ph.D.,
where he entered into a maelstrom of urban research. Widespread concern with America’s
urban problems during the 1960s had engendered a flood of research funding by the Ford
Foundation and other well-endowed, well-meaning urban institutions. In the 1950s, John R.
Meyer achieved academic eminence for his pioneering Cliometric study of the economics of
slavery and for his work on railroad competition (Conrad and Meyer, 1958; and Meyer, et al.,
1961). In the 1960s, Meyer turned to the “Urban Transportation Problem,” and when Kain migrated from the Air Force Academy to Harvard, he would enter into Meyer’s orbit.

While Meyer strongly supported urban research, both at Harvard and when he later served as President of the National Bureau of Economic Research, Kain became the lodestar for urban economics at Harvard for decades. Kain’s most cited urban paper connected transportation with job market outcomes through the “Spatial Mismatch Hypothesis,” which began the river of economics research on racial segregation in which Quigley would so productively swim (Kain, 1968). Almost immediately upon Quigley’s arrival at Harvard, he began his productive partnership with Kain.

Their early work was novel both in its topic—segregation was not standard fare for economists during the 1960s—and in its methods. Their 1970 paper, published in the *Journal of the American Statistical Association*, on “Measuring the Value of Housing Quality,” (Kain and Quigley, 1970) was pioneering research in housing price hedonics because it relied on rich house level data instead of the aggregate data that had been used by Nourse (1967) or the tax appraisal records used by Harris, Tolley and Harrell (1968).

The data set is small by modern standards, but richly detailed, produced by three separate surveys of 1,500 households in St. Louis in 1967. Real building inspectors went out to investigate the physical state of each home; at-home interviews produced the rent and home value assessments. The core specification regresses rents on 27 independent variables (some of which are aggregates) for 579 St. Louis renters. Given the large number of independent variables and the modest sample, it is almost remarkable that the results came out as reasonably as they did.

Older structures were cheaper (“a new structure will sell for $3,150 more than an otherwise identical one that is 25 years old”); larger structures were more expensive. Structures with hot water or central heating or appliances rented at higher rates. In a sense, rereading the paper takes you back to the beginning of the housing price hedonic literature, to a heroic moment when Kain and Quigley were inventing a whole new field of research.

Just as in more modern housing price work, it is often the neighborhood level variables that are more interesting than the structural quality variables. Two neighborhood variables have a demonstrable link to rents: schooling and race. Both the median schooling of adults in the area and local public school achievement are positively associated with rents; adult schooling levels are also positively associated with home values. The connection between human capital and area success has been a running theme of urban research for decades since then (Rauch 1993, Glaeser, 1994, Moretti, 2012). Naturally, the literature has moved on from broad correlations to the elegant spatial discontinuity design of Sandra Black’s (1999) work examining the connection between housing prices and attendance districts. All this work ultimately follows from Kain and Quigley.
To many modern readers, their most surprising fact may be that rents were higher, not lower, in areas with larger numbers of African-Americans, at least controlling for the education level of the community. This fact was in line with older work on rents from Chicago finding that African-Americans paid more for similar apartments before World War II. Kain and Quigley explained the finding with the same economic logic used by Becker (1957) in the employment context: cost minimization constrained by segregationist barriers will produce a higher cost than unconstrained cost minimization.

Kain and Quigley would put the St. Louis data to other uses, including measuring neighborhood blight, but their later collaborative work using that data focused primarily on race. They followed up their JASA article with a 1972 paper on racial disparities in homeownership that was published by the American Economic Review. Again, the hyper-detailed St. Louis data was the highlight, but this was supplemented with some cross metropolitan area findings. The St. Louis data’s detail was valuable because rich controls were helpful in making the case that racial differences in homeownership reflected more than just racial differences in income.

They argue again that “housing market discrimination” explains low levels of African-American homeownership and savings: “a limited supply of housing suitable for homeowner-ship in the ghetto and restrictions on Negro purchase outside the ghetto strongly affect the tenure-type of the housing consumed by Negro households as well as its location.” One piece of evidence presented supporting this view is that the level of African-American homeownership was higher in metropolitan areas where a larger number of African-Americans have suburbanized. While it is surely true that discrimination posed barriers to African-American suburbanization, it is also possible that black suburbanization was itself endogenously determined in part by other factors increasing the level of African-American homeownership.

Kain and Quigley’s partnership ultimate produced a book “Housing Markets and Racial Discrimination: A Micro-economic Analysis,” which was published by the NBER in 1975. The book carefully analyzes the many ways in which black and white housing choices differ in the St. Louis area, focusing on structure-type, prices, expenditures, tenure-type and location. By the time the book came out, there was already controversy over whether blacks really paid more, even in St. Louis (Little, 1976).

Kain always argued that their finding was so difficult to duplicate because other data sources lacked their extremely rich measures of neighborhood and structure quality. Black-white differences in housing quality have been enormous and that it is very difficult to interpret the raw gap in rental payments made by blacks and whites. I also suspect that Kain and Quigley’s finding were essentially looking backward towards a pre-1965 urban America, which was segregated largely because of hard discriminatory barriers. Little’s (1976) competing study of St. Louis may have been looking forward to the post-1975 America, where segregation was more likely to reflect the tastes of whites, who were willing to pay more than blacks to have white neighbors, rather than hard barriers. In that post-1975 world, the whites, not the blacks, are
essentially cost minimizing subjects to a constraint (in this case their tastes) and as a result, economics suggests that the whites consequently have to pay more (Cutler, Glaeser, and Vigdor, 1999).

Their St. Louis volume was one of several NBER urban studies of the time that focused on different cities, and Quigley wrote a second one of those studies during his time at Harvard, which concentrated on Pittsburgh (Quigley, 1976). If discrimination was the great theme of the St. Louis work, then multiple centers of employment, within a metropolitan area, is the running motif of Quigley’s Pittsburgh work. This surely partially reflects Kain’s influence, for Kain was a long-time critic of monocentric models. He had been investigating dispersed employment within metropolitan areas since 1961 (Kain, 1962). Quigley’s work on Pittsburgh illustrates both the strengths and weaknesses of the polycentric approach. The analysis is richer and more realistic than monocentric models of housing markets, but there is a considerable loss of simplicity and elegance.

Eclectic Creativity at Yale

At Harvard, Quigley was the junior partner of a senior urban researcher with a well-defined and successful research agenda. In his later years at Berkeley, Quigley would become a distinguished urban scholar with his own well defined agenda in housing policy and urban economics. The intervening Yale years were far more eclectic. Reading Quigley’s output from those years gives the impression of an extremely bright, intellectually fertile young scholar who is enjoying the freedom to wander before eventually settling close to home.

The breadth of Quigley’s output over that short period is dramatic. He produced a book with Peter Kemper on the economics of refuse collection. He investigated the returns to experience among Swedish engineers. Together with Sharon Oster, he published a paper in the Bell Journal on how the spread of construction innovations were stymied by building codes. He co-edited a book with John Meyer on fiscally-squeezed localities. Together with another former Kain student, Eric Hanushek, he wrote three significant papers on housing demand and household mobility.

While all of his work from this period is interesting, the work with Hanushek and Oster are both the most important contributions from this time period, and the work that fits most clearly in with his research earlier at Harvard and later at Berkeley. The Hanushek papers generally use data created by the “Housing Allowance Demand Experiment” (HADE) component of the “Experimental Housing Allowance Program,” or EHAP, a massive social science inspired experiment conducted by the Department of Housing and Urban Development during the 1970s.

The proliferation of low cost randomized economic experiments, especially in the developing world, has expanded during recent years, but there is nothing new about large scale experiments. Indeed, by modern standards, the scale of EHAP is staggering: $170 million in the 1970s (perhaps $700 million today), more than the entire social science budget for the National Science
Foundation over the time period. The *Journal of Economic Literature* described the project at the time as “the most expensive attempt to originate and quantify social science data through a controlled experiment” (Bradbury and Downs, 1982). The experiment may have reflected the Nixon administration’s desire to replace public housing projects with vouchers, but it remains remarkable that the U.S. government tried to answer policy questions with an experiment.

Quigley was not a central figure in the experiment. Abt associates handled much of the early evaluation, but he and Hanushek did write three of the more interesting papers that used the data that was created. Their first two papers, published in the *Journal of Urban Economics* and *Land Economics*, respectively, emphasize the costs of adjusting housing consumption. Household conditions may change—children are born and leave the house, incomes rise and fall—but it may be costly to move and for many, housing remains the same.

Hanushek and Quigley use the HADE sample of recent movers to estimate the mapping between demographics and housing demand. For their entire sample, they can then use this estimated function to estimate the true housing demand, the housing level that would be consumed absent adjustment costs. They then ask how well the difference between current consumption and preferred consumption explains changes in housing consumption— in the *Journal of Urban Economics*— and mobility— in *Land Economics* (Hanushek, 1979; and Hanushek and Quigley, 1978). In the *Journal of Urban Economics*, they find that on average, households close about one-fourth of the difference between actual housing consumption and ideal consumption each year. In *Land Economics*, they find that mobility rates are higher for households whose housing consumption is currently mismatched to the demand predicted by the recent movers’ demand estimates.

Quigley would have an enduring interest in mobility and stock adjustment models. In later years, consumer demand in such markets would be modeled with option models, and Quigley himself was a pioneer in using such models in the context of mortgages. Yet in the 1970s, Hanushek and Quigley clearly saw the problems of assuming that consumption of housing could adjust seamlessly to changes in demand, either at the individual or the community level.

In their 1980 *Review of Economics and Statistics* paper, again using the HADE data, they brought this insight to estimates of price elasticities (Hanushek and Quigley, 1980). This was the first of their papers that actually used the experimental element to the data. HADE randomly gave some members of the treatment group effective reductions in rent ranging from 20 to 60 percent. This type of true randomization is the ideal so rarely observed in estimating price elasticities of demand. They estimated the price elasticity in the dynamic structure that they had used for their earlier work, which recognized that the impact of a cut in price may influence consumption only slowly over time.

For example, their Pittsburgh based estimates suggest that a 10 percent reduction in price will cause housing consumption to rise 1.2 percent the first year, 4.2 percent after five years, and 6.4
percent eventually, if housing supply is perfectly elastic. Perfect supply elasticity is the appropriate assumption if the subsidy is given to a small subset of the population, but not if the entire market is saturated with the subsidy. When housing supply is less elastic, then the long run impact of price subsidies on housing consumption will be lower and closer in magnitude to the short run impact of subsidies on consumption.

Quigley’s burgeoning interest in dynamics also appears in his work with Sharon Oster on regulation and the diffusion of construction innovations. Since 1980, the gap between housing prices and construction costs has risen enormously. Construction cost variation can only explain a small part of the housing price variation across areas today (Gyourko and Saiz, 2006), which is why researchers, including Quigley, have increasingly focused on land use regulations as a cause of high housing prices. But during the 1970s, the gap between construction costs and prices was smaller, and researchers concerned with affordability were more likely to wonder whether regulations concerning construction materials significantly impacted prices.

Oster and Quigley were interested in whether regulatory barriers were preventing innovation in the U.S. building trades. Was construction a backward industry because new techniques were being banned? To examine this question, they looked at four innovations, including non-metallic cable and pre-assembled plumbing that diffused across the U.S. during the middle decades of the 20th century. They plot the spread of localities that permit the use of these materials and then examine the cross-section determinants of which communities allowed each new material.

One plausible categorization of theories about regulation is that they fall into three buckets: (1) the benevolent regulation theory, (2) the captured regulatory theory and (3) the imitative stupidity theory of regulation. The benevolent regulation theory argues that rules are devised to minimize adverse Pigouvian externalities. The capture theory is that either industry or labor groups are using regulation to further their own pecuniary interests (Stigler, 1973). The imitative stupidity theory is that regulators generally imitate each other and then occasionally innovate in ways that are essentially purely random.

These rules restricting building materials do not appear to be particularly optimal. Non-metallic cable and pre-fabricated plumbing are extremely common today and do not create terrible risks. Moreover, if regulations were crafted to maximize social welfare, then the barriers to new construction would presumably be lowered in areas where housing demand was more robust, yet that does not seem to have been true. But the rules also do not seem to reflect capture. Local industry structure or unionization variables, including the union connections of the top building official, explain little of the heterogeneity in permitted innovations.

The one reliable fact is that better educated building officials were friendlier to innovation. This is an interesting fact which seems compatible with Welch-Schultz hypothesis that educated
farmers were more open to innovations. It also seems most compatible with the view that these rules were more likely to reflect imitation and ignorance rather than any clear conspiracy.

Perhaps, the high water mark for Quigley’s eclecticism at Yale is his work on homicide with Henry Hansmann. This paper uses international data to test the hypothesis that more ethnically heterogeneous societies are more violent. Since that time, economists have grown increasingly comfortable seeing papers on ethno-linguistic fragmentation and crime, but Hansmann and Quigley (1982) appeared in a sociology journal (Social Forces) rather than a conventional economic outlet. Moreover, it focused on social forces and paid scant attention to the costs and benefits of crime that are the hallmark of the Beckerian (1968) approach.

Hansmann and Quigley’s results are difficult to interpret because they simultaneously estimate effects of ethnic fragmentation, linguistic fragmentation, and religious heterogeneity, and find that while ethnic fragmentation increases crime, linguistic fragmentation decreases crime. The authors have a coherent and plausible explanation of this phenomenon—conflict occurs when different people interact, and linguistic differences deter interactions—but the high degree of correlation between these two variables has meant that later researchers were typically too frightened of collinearity to include both forms of heterogeneity in the same regression.

The paper is quite prescient in its focus on ethnic heterogeneity as subsequently economists have found links between ethnolinguistic fragmentation and corruption (Mauro, 1995), riots (DiPasquale and Glaeser, 1997) and social welfare spending (Alesina and Glaeser, 2004). I cannot speak for the other authors of these later papers, but I was embarrassingly ignorant of Quigley’s pioneering work in this area. One distinguished scholar with far more knowledge about this paper is Michael Woodford, whose work as a young research assistant on this paper helped persuade him to become an economist.

III. Price Indices and Mortgage Defaults: Solving Intellectual Puzzles at Berkeley

Berkeley became Quigley’s permanent academic home after 1979, where he became a fixture of both the economics department and the business school. Almost every economist nurtures two desires. The first is to write the difficult technical papers that are admired by academic peers and published in top tier journals. The second is to influence the wider world. In fields such as monetary economics, where policy-makers clearly feel a need for academic guidance, the two desires can be closely linked. The scientific papers of Lucas, Prescott and Sargent shaped the training of a generation of economics Ph.D. students and those students have helped lead monetary policy.

This is not how urban policy in the U.S. operates, especially below the Federal level. As I have already noted, academics do influence HUD policy, and at times, HUD has tied itself tightly to
science, as in the case of the EHAP experiment or the later work on Moving to Opportunity. But even at HUD, politics trumps social science, and economists do not enjoy a monopoly over academic advice to HUD. At the local level, the appetite for academic wisdom is even more limited. While many accept that monetary policy is an arcane topic that benefits from academic insight, few big city mayors feel like they need to consult a Berkeley economist to improve trash collection, even if that economist, like Quigley, has written a monograph on the topic.

If urban economists want to influence policy, they often choose topics and writing styles that will be more accessible to a wider audience. Quigley’s work in his years at Berkeley can be divided between technical work that appealed to housing specialists and work that spoke to broader policy concerns. As in the case of most economists, the policy-relevant work generally increased as a share of his portfolio as Quigley aged. But unlike most of us, Quigley showed no signs of slowing down. Indeed, by almost any measure, the second half of his time at Berkeley was more productive than the first half of his time at Berkeley.

Housing Prices Indices

Throughout his career, Quigley continued to make contributions to the classic housing price index problem—how to aggregate up dispersed data into a figure that accurately captures the path of the housing prices in an area. Quigley’s early work with Kain made him practically “present at the creation” of housing price indices, but he continually pushed the methodology beyond basic hedonics.

Quigley published at least eight papers between 1989 and 2006 that are closely related to improving the quality of housing price indices. This work is largely technical, and not immediately related to any obvious policy question, but given the widespread need for decent measures of housing prices, Quigley’s work in this area represents the type of technical work that can significantly improve the quality of information available to policy-makers.

At Berkeley, Quigley’s primary contribution in this field involves finding a middle ground between purely hedonic indices, which largely followed his own earlier work, and repeat sales price indices. Case and Shiller had become the intellectual champions of repeat sales indices, originally invented by Bailey, Muth and Nourse (1963), and indeed, the regular announcement of their own repeat sales index is now global news. Case and Shiller had originally discussed using a hedonic or hybrid index, but they concluded that the complexity of such measures would make them far less attractive to the market. Shiller, of course, hoped that their index would be the basis for a trading instrument that would allow homeowners to hedge some of their trading risk.

The appeal of a repeat sales price index is that by looking only at changes in prices, the bulk of the idiosyncratic house quality is essentially first differenced out. One downside of repeat sales indices is that in most data sets repeat sales are only a small sample of total sales. A second downside is that homes may have been modified between sales. While adhering to a transparent repeat sales framework may have been sensible when Case and Shiller were trying to establish a
simple standard index, surely academics would like indices that take advantage of all the available data.

In three distinct papers (Case and Quigley, 1991; Quigley, 1995; and Englund, Quigley, and Redfearn, 1998), Quigley made the pragmatic case that researchers should use all of their data. His statistical procedures used generalized least squares to estimate the error associated with each observation. If a property is a repeat sale, then there is greater ability to predict the new price, and that property weighs more heavily in the index. If the repeat sale has changed its physical characteristics, then the predictive power is weaker, and its place in the index is downgraded. Quigley refined these techniques by using them on Hawaiian and then Swedish data.

While this hybrid price approach may be Quigley’s largest contribution to the price index problem, he had a number of other interest insights. Mason and Quigley (1996) presented a semi-parametric approach to the problem that focused on using the “Generalized Additive Model.” Englund, Quigley, and Redfearn (1999) examine time-based aggregation, demonstrating the advantages of ensuring that indices are available at high frequencies. Yet working with shorter time periods makes the number of repeat sales smaller, and increases the advantages of using all the available data. Clapham, Englund, Quigley, and Redfearn (2006) and Deng and Quigley (2008) look at the problem of ex post index revision, which may cause much mischief when contracts are index related, and re-examine option index design in that light.

Almost all housing price indices have trouble dealing with the selection of homes that are sold during a given time period. The prevailing wisdom is that price indices understate true price falls during early periods of price decline, because the only sales that occur are those when the seller has done extremely well. Quigley did not solve this problem but Hwang and Quigley (2004) represent a significant step forward in this challenging area.

Because of Quigley’s interest in housing price indices, he was naturally interested in the related task of appraisal. Indeed, Quan and Quigley (1989) is a paper about appraisals that also contains insights that will find their way into Quigley’s later work on price indices. Quan and Quigley (1991) present a fully specified model of the home purchase procedure in which the role of appraisers is clear and socially valuable.

The development of better price indices is clearly related to the measurement of the returns and variance of housing as an asset (Quan and Quigley, 1989). For example, Quigley’s work on Swedish pricing led him to conclude, like Case and Shiller (1989) that housing price movements show substantial serial correlation and are not a random walk. Quigley’s investigation into more finance-related issues also led him to consider homeownership’s role as a hedge. We are, of course, all born short housing, which suggest that homeownership has some value hedging housing cost risk, but for individuals with short time horizons the value of this hedge is limited. Quigley concluded that while housing is an appropriate part of longer term portfolios, shorter
term portfolios should essentially be housing free. This suggested the possible value of instruments or policies that allowed short-term owners to diversify the risk of housing value fluctuations (Englund, Hwang, and Quigley, 2002).

*Mortgages Defaults and Prepayments: Quigley, Van Order, and Deng*

The second large and technical research agenda that Quigley followed concerns mortgage defaults and prepayments. Much of this work was joint with Robert Van Order. Quigley’s 2000 *Econometrica* paper with Yongheng Deng and Robert Van Order may be the most famous example of Quigley’s work on mortgages. This paper is now seen as a classic empirical analysis of mortgage prepayment and defaults that recognizes that these are competing risks that must be modeled jointly. As of March 2014, this was Quigley third most cited paper on Google Scholar.

But this paper is really a culmination of a large body of Quigley’s contributions on mortgages and defaults, which ultimately build on Quigley’s earlier work with Hanushek that considered the sticky dynamics of housing consumption. His earlier focus was on changing homes, rather than prepaying or foreclosing on mortgages, but the consistent interest in frictions and dynamics is clear. Moreover, prepayment also occurs when individuals sell, so there is an even more direct link to his earlier work on household mobility.

Quigley (1987) makes the connection between mobility and mortgage explicit by estimating proportional hazard models of mobility, where the size of the outstanding mortgage is a key independent variable. He finds that mobility is less common when families owe more, which partially helps explain low levels of geographic mobility during our more recent great recession (Ferreira, Gyourko, and Tracy, 2011). He also finds that the impact of outstanding mortgage size is larger in states which enforce mortgage rules that require full payment at time of sale.

Quigley’s partnership with Van Order begins with two papers on mortgage defaults published in 1990 and 1991 in *Real Estate Economics* and the *Journal of Public Economics*. Van Order had been thinking about mortgage defaults for many years, after all he worked for the Federal Home Loan Mortgage Corporation (Freddie Mac), and he had published a path-breaking paper applying options theory to the mortgage defaults in 1985 (Foster and Van Order, 1985). Their 1990 paper is an early application of proportional hazard models to mortgage prepayments. The paper uses excellent mortgage data from Freddie Mac and establishes that while the option model has considerable empirical value, people do not prepay “ruthlessly” enough. Individuals fail to exercise their option when it would be in their financial interest, and simple models of transaction costs do not really make sense of the facts either. Quigley would be interested in irrational borrowers for years, as well as being quite open to irrationality in other markets (Deng and Quigley, 2004; and Quigley, 2006).

Quigley and Van Order’s second paper in this vein used a similar methodology—proportional hazard models applied to mortgage—to estimate default risk and calculate appropriate capital requirements for Savings and Loan institutions. The data for this exercise again came from.
Freddie Mac, which owned 300,000 mortgages bought between 1976 and 1980 that it did not insure. The subsequent behavior of these mortgages enabled the authors to calculate the default risk facing institutions heavily invested in residential mortgages. They found that a 3 percent capital requirement would be adequate even for institutions holding the risk of high loan-to-value loans.

The authors are quite clear that their results are specific to that time period, and default risks might be higher in subsequent years. Unfortunately, the general view that default risks were likely to be small became part of the prevailing wisdom. That wisdom led many economists, including myself, to underestimate the default risks facing the GSEs and many lenders in 2006, and instead worry more about interest rate related risks in the retained mortgage portfolios of the GSEs.

Lekkas, Quigley, and Van Order (1993) examine the connection between defaults and loan loss severity, again finding that the “frictionless model does not do well” empirically, pushing towards transaction costs or semi-rational models of mortgage default. Quigley and Van Order (1995) again focus on default using proportional hazard models to test the options model of defaults, assuming zero transaction costs. Perhaps the most striking fact in the paper is that default rates for high loan-to-value loans are much lower in reality than is predicted by a rational model. People do not seem to be sufficiently ruthless in their default behavior when they have negative equity. Sensibly, they take the view that most of their facts can be reconciled with transaction costs that differ across buyers, and that are particularly low for distressed owners or owners looking to move.

Yongheng Deng joined this team for their next paper—a 1996 piece in *Regional Science and Urban Economics*, which considers the competing risks of default and prepayment. Bringing both risks together in a single paper—default and prepayment—essentially merges Quigley and Van Order (1990) and Quigley and Van Order (1995). The paper estimates a competing risk proportional hazard model, but it did not explicitly solve for optimal borrower behavior with those competing risks. This first application was primarily an empirical exercise in predicting defaults used for the public policy-related purpose: predicting the impact of reducing down-payment rates to zero. Somewhat presciently, given the events of the past decade, the trio concluded that the social costs of subsidized no down payment loans would be significant.

This team’s 2000 *Econometrica* paper then extends this analysis by developing empirical methods that allowed for competing risks and unobserved individual heterogeneity. This represents a formalization of ideas about heterogeneous transaction costs that Quigley had been discussing verbally for a decade. Taken together, this body of work shows a slow, methodical approach to an important technical problem: understanding the behavior of mortgages.

Just as in the case of housing prices, Quigley developed an early interest and then kept at it, intermittently, year after year. Ultimately, in both cases, his research provided major insights
and helped shape our modern understanding of the topic. But for younger scholars hoping that their work will yield a sudden explosion of insight that will change the world, Quigley’s career offers a somewhat different picture of the life of the scholar, where years of diligence, rather than a single bright flash, is the path towards knowledge.

IV. Public Policy and the Built Environment

Quigley’s work on price indices and mortgage defaults both represent years of work on difficult technical problems. These papers certainly had policy implications, but they were typically somewhat removed from hot areas of policy interest. Even more importantly, Quigley’s work in those areas gives the impression of being motivated primarily by the intellectual challenge of solving an important social science problem. In other areas, however, Quigley jumped into hot topics and also—more often than not—was able to produce significant contributions. Yet his interventions were not random. The built environment runs through almost all of his policy-related work from segregation to homelessness to the environment.

Energy and the Environment

Quigley’s work on energy use appeared during two very distinct periods separated by almost twenty years. The early phase lasted from 1984 to 1991 and the latter phase ran from 2009 until after his death. The primary theme of the early work is the connection between housing structures and energy use, and that raising prices on energy will also cause shifts in housing demand. The primary theme of the latter body of work is that builders who made energy efficient choices were also making financially prudent choices, since the economic returns to green building were high enough to offset the costs.

Quigley’s 1984 paper on residential energy conservation emphasizes the role that energy plays as part of the flow of housing services. Naturally, then, changes in the cost of energy will tend to shape the choices of home-builders and buyers. Higher energy costs may lead to more energy efficient building, and perhaps to smaller houses overall. Quigley then uses a simple housing model to infer the impact that rising energy prices will have on the costs of housing services and the overall level of housing and residential energy demand. He also argues that subsidies for “Green Building” are quantitatively reasonable, as long as the social cost of energy is 50 percent higher than its cost to consumers.

Quigley (1984b) uses similar methods to evaluate energy efficiency standards across California, and concludes that they too seem relatively low cost. But Quigley remained clear that higher prices were also effective in reducing energy use and Quigley (1991) estimates that a doubling of energy prices would reduce residential energy use by 25 percent.
Quigley persisted in his approach of viewing energy use within the wider lens of housing services. Quigley and Rubinfeld (1989) focus on dwelling unit comfort produced by climate and purchased inputs, such as air conditioning and central heating. Using a clever hedonic approach, they find that warm and cold places specialize in “purchased comfort,” while coastal California does not, but that overall dwelling comfort services were still higher in California. When we consider carbon emissions across the United States, it is clear that natural production of housing comfort results in far lower levels of carbon use than purchased housing comfort (Glaeser and Kahn, 2010).

Quigley’s second burst of research on energy in buildings was remarkably prolific: nine papers appeared in just five years. Such output would be impressive in a hungry young scholar eager to earn tenure, but from someone as senior as Quigley, it is truly remarkable. Four of these papers, three of which were jointly authored with Piet Echholtz and Nils Kok, can be grouped together as building the case that green – low energy use— generate significant economic benefits.

Eichholtz, Kok, and Quigley (2010) examine Leed and Energy Star buildings within the U.S. and find that rents in such buildings are typically three percent higher than rents in comparable non-green buildings. Occupancy rates are also higher in green buildings, so the impact of green ratings on total earnings per square foot is over seven percent. The impact on building sales price appears to be as large as 16 percent. These remarkably large returns to green ratings appear to be too large to reflect purely the energy cost savings, which suggests that there is some corporate cachet that comes from inhabiting green space.

Eichholtz, Kok, and Quigley (2013) consider a larger sample of buildings in the U.S. and find broadly similar results to their earlier work. Eichholtz, Kok, and Quigley (2009) perform a similar exercise in the United Kingdom and estimate rent premia above 20 percent for green certified buildings. Deng, Li, and Quigley (2012) document that green ratings are also associated with higher office rents in Singapore.

The high economic benefits that developers appear to reap from green building naturally led Quigley to wonder about what factors slowed the adoption of energy conserving structures in Kok, McGraw, and Quigley (2011). In a sense, this work harkens back to his early work on the diffusion of construction technology with Sharon Oster. Confirming the role that human capital plays on supporting innovation found in that earlier work, Kok, McGraw, and Quigley (2011) finds that diffusion is faster in high income areas and slower in areas with higher unemployment rates. The proportion of LEED certified professionals strongly increases the share of LEED certified buildings, and reduces the share of Energy Star building. The two forms of certification do seem to be substitutes. High energy prices also prod diffusion of energy saving methods.

Eichholtz, Kok, and Quigley (2009) ask what induces companies to rent green building space. Oil and finance firms are particularly likely to rent green. These results can also be interpreted
as suggesting something about knowledge, since finance is a particularly high human capital industry and energy firms are likely to know something about energy costs. Other interpretations are certainly possible, and the fact that government-related entities are more likely to rent green presumably indicates something about a desire to broadcast “corporate social responsibility.”

Brounen, Kok, and Quigley (2012) tackle the issue of knowledge directly, by examining an energy-related survey of Dutch consumers. They find that 56 percent of respondents do not know their home energy costs and that 40 percent were not able to properly evaluate the benefits of conservation-related investments. Ignorance may indeed be a barrier to wider diffusion of greener living spaces.

But even if we believe that green building is likely to spread, especially if energy prices get higher, Brounen, Kok, and Quigley (2012) suggest that this may not actually lead to lower levels of energy, at least in the Netherlands. This paper compares the impact of demographics on energy use with the impact of structural attributes, and finds that demographics can also be quite powerful. Richer and older people tend to use more energy in the Netherlands, holding structure constant. The elderly are particularly large gas consumers, although they use somewhat less electricity. As Europe becomes progressively older and richer, this may act to somewhat offset the reductions in energy generated by more environmentally sensitive structures.

**Homelessness and Land Use Regulations**

Energy use was a hot policy topic in the 1970s and the 2000s, and Quigley provided scholarly light on the subject. Homelessness became a major issue in the late 1980s and early 1990s, and Quigley, together with Stephen Raphael, did some of the best work in that area (Quigley, Raphael, and Smolensky, 2001). Quigley (1996) also wrote an elegant survey on the topic. Broadly speaking, theories about the rise in homelessness could be divided into two broad classes: people and structures.

The people theories emphasized an increase in the supply of homelessness-prone persons, due either to de-institutionalization of mentally troubled persons or economic woe. The structure theories focus on the limited availability of low cost housing. O’Flaherty (1996), for example, stressed that homelessness may have been increased by regulations destroying the supply of single room occupancy units in New York City.

Quigley (1990) is a particularly early paper of his on the topic, which argues that contrary to certain conservative claims, there was little statistical evidence that rent control increased homelessness. Quigley and Raphael (2004) and Quigley, Raphael, and Smolensky (2001) both assemble impressively broad data sets on homelessness across America. Their data confirms strongly the connection between the supply of low cost rental housing and homelessness. When rental housing is expensive and unavailable, homelessness is far less common. Their work does not ignore the “people” side, for they also find that holding average income constant, homelessness increases with poverty.
Mansur, Quigley, Raphael, and Smolensky (2002) further this research agenda by examining the impact that different housing policies might have on homeownership. While most homelessness work was typically brute empirics, this paper illustrates the power of economic theory to help illuminate even a problem as opaque as homelessness. They again found that sensible housing policies that reduced the cost of rental housing could substantially reduce homelessness.

The connection between homelessness and housing supply helps explain Quigley’s broader interest in housing supply, but Quigley was certainly interested in supply because of his broader interests in the economics of housing. As Quigley himself inhabited an area of the U.S. where prices had risen dramatically during his lifetime, it was surely natural for him to have an interest in why housing had become so unaffordable. Greulich, Quigley, et al. (2004) downplayed the possible role that immigration has played in pushing up rental costs, and Quigley, Raphael, and Rosenthal (2005) instead point towards regulation as a cause of high rental rates. 2

Quigley’s ground-level observation of California unsurprisingly led him to the view that regulation was part of the problem. Some significant regulations, like prevailing wage laws for public projects, increase the physical cost of constructing low income housing (Dunn, Quigley, and Rosenthal, 2005). More often land use regulations severely limit the production of new homes. Quigley saw the autarkic nature of local land use decisions, where residents did not appear to internalize the costs imposed by restrictions on would-be homebuyers outside the area.

Quigley and Raphael (2005) is an important part of the literature that has grown after 2000, illustrating the power that local land-use restrictions exert in shaping housing prices and the geography of American development. They measure the number of growth restrictions and document that areas with more growth restrictions have less new construction of single family homes, higher prices, and higher price increases during the 1990s.

One of the great challenges doing research on land use regulation is the incredible amount of institutional variation across states. As such, much of the work in this area has tended to be within state, and Quigley’s deep knowledge of California allowed him to produce papers that benefitted from rich detail. For example, Quigley, Raphael, and Rosenthal (2009) presents a remarkable rich portrait of regulation around the San Francisco Bay—one of the great epicenters of land regulation in America. Kok, Monkonnen, and Quigley (2011) combines rich institutional data on Bay Area regulations with excellent data on land sales and illustrates how restrictive regulations push up the price of housing. Quigley and Swoboda (2010) emphasize the interaction between land use regulation and durable housing, making the point that when durability limits the ability to adjust density on developed land, the social cost of other land use restrictions may be more severe.

2 In principle, homeowners do internalize some of these costs because the value of their land should fall when subdivision or density are forbidden. Skewed land ownership could easily explain, however, why the median local voter wants limits on land development, as could the existence of any local monopoly power.
Quigley obviously considered the barriers to housing supply to be important, but he was also a steadfast advocate of using demand-side support to ease the housing cost burden for lower income Americans (Quigley and Raphael, 2004; Quigley, 2007; and Quigley, 2011). In Quigley’s view, too many of our housing policies are legacies in an age in which much American housing was sub-standard. By contrast, in modern years, American housing is extremely good by almost any standard, and the primary challenge facing housing policy is to reduce the cost of that housing for lower income Americans.

*Cities and Segregation*

Given Quigley’s early days with John Kain, it is not surprising that he continued to show an interest in residential segregation and on its interaction with transportation and access to jobs. Miller and Quigley (1990) examined racial segregation in San Francisco and found that it had declined since 1970. O’Regan and Quigley (1996a) used access to rich micro-data on New Jersey teenagers, and found that neighborhood has a powerful correlation with youth unemployment and idleness. O’Regan and Quigley (1996b) find similar results use cross-metropolitan area data.

Yet the data is less supportive of Kain’s hypothesis that it is physical access to jobs that matters. Instead, general measures of neighborhood prosperity seem be stronger predictors of teenage employment. This point is made even more strongly in O’Regan and Quigley (1998), who point clearly towards to the dearth of knowledge and information in high poverty areas. O’Regan and Quigley (1993) provide evidence supporting the view that social networks have a powerful impact on employment outcomes. The view that neighborhoods shape what we learn and the disadvantaged provide less learning has become almost a consensus among those who believe that childhood neighborhood influences adult employment outcomes.

In recent years, however, the view that growing up in segregated neighborhoods hurts adult outcomes has been challenged by the results of the Moving-to-Opportunity experiment (Katz, Kling and Liebman, 2001). This remarkable randomized analysis truly enables researchers to compare the life paths of children who did and did not receive vouchers enabling them to move to lower poverty areas. Since the children in voucher recipient households seem to have similar adult outcomes to the control households, it is harder to believe that the old correlation results—showing the connection between poverty and segregation – represent a causal link.

Quigley does not seem to have abandoned his view that neighborhood helps shape human capital (I have not either). Instead, Quigley and Raphael (2008) note that the impact of MTO on experienced poverty was actually pretty minor moving households “from neighborhoods at roughly the 96th percent of the neighborhood poverty distribution to neighborhoods at the 88th percentile.” This is a fair comment that has been echoed by other scholars whose pre-MTO work pointed to the importance of neighborhoods. Yet there is no question that the MTO work has
raised the research bar, and if future scholars want to reverse the verdict of MTO, they will need to run experiments of a similar quality.

*The Consumption Effect of Housing*

In 2001, America was experiencing the aftermath of the great burst of the internet bubble. Some feared that declining stock prices would cause consumption to fall and pull America into recession. But others hoped that resilient and then rising housing prices would keep Americans buying.

As a matter of theory, it is not clear whether higher housing prices will cause consumption to rise or fall. We are all born short housing. If a household were infinitely lived and uninterested in moving, then changes in housing prices would not impact their real wealth in any way. Even though we do have limited life spans, altruistic parents still presumably care about the rising costs to their children of higher housing prices.

The theoretical impact of housing prices on consumption must hinge on distributional issues. When prices rise, the price of this necessary commodity has increased, homeowners may have gotten richer but renters have become poorer. It must be that the owners are more likely to spend, perhaps by cashing out housing equity. Renters may not be saving at all, so the impact on their consumption may be negligible. In general, if a society is more liquidity constrained, then its consumption is more likely to respond to changes in housing prices.

Working in the shadow of the internet bust, Case, Quigley, and Shiller (2005) found dramatic evidence, using both cross-state and cross-country evidence, that increases in housing wealth were associated with increases in consumption. This work has since become Quigley’s most cited work according to Google Scholar. This work was originally presented in 2001, and I was admittedly an early skeptic, not because I disputed the empirical facts but because I was unsure of the theoretical framework that would support such large consumption effects.

Yet the events of the past 12 years have only supported the view that housing price changes have particularly powerful consumption effects. America hummed along with the housing bubble from 2001-2006, and then crashed after housing plummeted (Case and Quigley, 2008). Their work has only seemed more valid over time. Case, Quigley, and Shiller (2013) have updated their work and found even stronger evidence supporting the link.

It is possible, even in the face of these findings, to maintain skepticism. Housing price changes are unlikely to be orthogonal to other economic events and may well be forward looking. As such, it is always possible to argue that correlations between consumption and housing prices reflect the influence of other economic variables that drive both series. I am sure that there is some spurious correlation, but I have largely come around to the view that consumption rises with housing prices, and that the effect may be larger for stock prices.
I believe that the most natural interpretation that justifies this view is that housing price increases ease liquidity constraints for many American homeowners, whereas stock price increases do not because the owners of stocks are far less likely to face such constraints than the homeowners. Renters are poorer as a result of rising home prices, but as discussed earlier, they save little anyway.

Still, there is an even larger takeaway from Quigley's housing wealth paper: it is yet another reminder of the importance of the built environment to the larger economy. Quigley (1999) makes that point explicitly, and Quigley (2001) emphasizes the power of the housing sector to drive economic conditions in Asia. In a sense, that theme is really the grand theme of Quigley's entire career.

The Breadth of the Scholar

I have tried to put Quigley’s work in neat buckets with discernible themes. Yet there are many papers that refuse to be so easily categorized. For example, Quigley’s (1998) paper on urban diversity and economic growth is an extremely influential and deeply interesting paper on the nature of agglomeration economies. It taps in to Jane Jacobs’ view that new ideas reflect combinations of old ideas and heterogeneity of old ideas can be an important urban strength.

He used the natural experiment of Sweden’s university decentralization to examine the impact of universities on local economic success (Andersson, Quigley, and Wilhemsson, 2004). This work supports the findings of the broader literature linking education to regional prosperity (e.g. Moretti, 2003). In related work, Quigley documented the distribution of creativity across Sweden, and again found an important role for academic institutions (Andersson, Quigley, and Wilhemsson, 2005). Huffman and Quigley (2002) discuss the role that universities played enabling the growth of Silicon Valley.

Taking Quigley’s interest in housing price indices to a completely different field, Hwang, Quigley, and Woodward (2005) provide an index for the value of venture capital investments. This enables them to gauge the return, variance, and covariance of investments in new start-ups.

Congestion pricing has been loved by urban economists for more than 50 years since William Vickrey presented the idea, but voters have been less enthusiastic. Stockholm first introduced congestion pricing and then had a referendum on the policy after seven months, which gave Harsman and Quigley (2010) an opportunity to examine the forces of the influence demand for congestion pricing. While ideology matters, time savings matters too and they found that the neighborhoods that experienced some of the largest benefits from congestion pricing over the previous seven months voted most assiduously for the policy’s retention.

Together with Dwight Jaffee, Quigley (2013) detailed the messy history of Freddie Mac and Fannie Mae. The two authors brought together a wide body of evidence suggesting that these
agencies today did more harm than good. This work was influential in the recent discussion about the future of these government-sponsored enterprises.

V. Conclusion

John Quigley was an important urban economist, whose contributions continued steadily over the years. His early work with John Kain played a critical role in developing two distinct literatures: the economics of segregation and the economics of housing prices. Over the next thirty years, Quigley worked steadily to advance methods for estimating housing price indices, developing his vision of a hybrid index that used both repeat sales and one-off transactions. Together with Robert Van Order, Quigley pioneered the formal analysis of mortgage prepayments and defaults with many years and many papers.

While these two main bodies of research stretch throughout Quigley’s career, he had four other research areas in which he was a major force. His work on the consumption effects of housing wealth is highly cited and enormously policy relevant. His work on the adverse effects of segregation, generally with Katherine O’Regan, perpetuated a line of research that he had begun with John Kain. His work on homelessness housing regulation, often with Stephen Raphael, provided trenchant analysis of how restricting housing supply could increase the number of people living on the streets. His work on energy conservation makes the case that green buildings can often be economically remunerative as well as ecologically attractive.

Quigley was a scholar of both depth and breadth. But his written contributions to social science do not capture the measure of John as a scholar and friend. He was always enthusiastic about younger scholars, interested in new work, and serious about social relevance. He had accumulated a happy wisdom that could light up a room. He will be greatly missed.

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