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Density Zoning and Class Segregation in U.S. Metropolitan Areas

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Abstract

Objectives—Socioeconomic segregation rose substantially in U.S. cities during the final decades of the 20th century and we argue zoning regulations are an important cause for this increase.

Methods—We measure neighborhood economic segregation using the Gini Coefficient for neighborhood income inequality and the poor-affluent exposure index. These outcomes are regressed on an index of density zoning developed from the work of Pendall for 50 U.S. metropolitan areas, while controlling for other metropolitan characteristics likely to affect urban housing markets and class segregation.

Results—For both 2000 and changes from 1990 to 2000, OLS estimates reveal a strong relationship between density zoning and income segregation, and replication using 2SLS suggests that the relationship is causal. We also show that zoning is associated with higher inter-jurisdictional inequality.

Conclusions—Metropolitan areas with suburbs that restrict the density of residential construction are more segregated on the basis of income than those with more permissive density zoning regimes. This arrangement perpetuates and exacerbates racial and class inequality in the United States.

Since the mid-1970s, the United States has undergone a dramatic shift in its class structure (Danziger and Gottschalk 1995; Levy 1998). By 2005, inequalities of wealth and income had risen to levels last seen in the 1920s. In broad terms, those in the top fifth of the income distribution saw their income and wealth increase dramatically in real terms; those in the middle three fifths saw their income and wealth stagnate and their indebtedness grow; while those in the bottom fifth not only saw indebtedness grow, but experienced real declines in both wealth and income. Within the top fifth of the distribution, the higher up one goes, the greater the increase in income. Whereas the average income earned by the top 20% of households grew 55% in real terms from 1973 to 2003, the real gain for those in the top 5% was 75%, compared with just 13% among households falling at the median. Over the same period, the Gini Coefficient for household income inequality rose from .397 to .464 (Massey 2007).

As the human ecologist Robert Park (1926) would surely have predicted, rising inequality in the social realm was accompanied by increasing separation in the spatial realm, and after 1970 segregation on the basis of socioeconomic status increased dramatically. Whether measured in terms of income (Jargowsky 1997; Massey and Fischer 2003; Fischer et al.

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2004) or education (Domina 2006), class segregation increased substantially between 1970 and 2000 and both poverty and affluence became more concentrated spatially, especially during the period from 1970 to 1990. Increasingly well-educated and the affluent people seemed intent on segmenting themselves off from the rest of American society, a trend that Reich (1992) labeled “the secession of the successful.”

While social scientists have long studied the causes and consequences of racial-ethnic segregation, the emergence of class segregation is so recent that researchers have done little beyond establishing a basic correlation between social and spatial inequality (see Massey and Eggers 1993; Jargowsky 1997; Watson 2006). Whereas racial-ethnic segregation stems from a complex interplay of group differences in buying power; white prejudices; minority preferences; discrimination in housing and lending markets; zoning, and federal housing policies (Hirsch 1983; Jackson 1985; Massey and Denton 1993; Charles 2003; Rothwell and Massey 2009), class-specific prejudices, preferences, and discrimination are not likely to play as important in determining segregation between income groups. Although, other things equal, affluent families likely prefer to live apart from poor families, the degree of class segregation is not nearly as severe as the degree of racial segregation (White 1987; Massey, Domina, and Rothwell 2009), suggesting that class bias is less powerful than racial bias. It is likely that the combination of racial and class bias has motivated and continues to motivate economic segregation, but the challenge is to find more proximate causes; for why class prejudice may be a ubiquitous cultural norm, there is considerable variation in class segregation across metropolitan areas.

One possible explanation for variation in class segregation focuses on the economics of transportation. One possible explanation focuses on the economics of transportation. Glaeser, Kahn, and Rappaport (2008) identify a large gap in poverty rates between central cities and suburbs, especially in older metropolitan areas with subway systems. They account for this gap in terms of the price of transportation and the opportunity costs of travel, with the low price of public transport attracting the poor to central cities and the high opportunity costs driving the affluent to suburbs. Despite the seeming logic of this account, it nonetheless has certain empirical problems.

Glaeser et al (2008) calculate that a car costs $2,000 a year in maintenance, making it a worthwhile investment if one's time is worth at least $8 per hour; but this rate is below the hourly wage of most workers in central cities. Rapid job growth should also have made suburbs more attractive to the working poor, yet the concentration of poverty was higher in 2000 than in 1970 (Massey and Fischer 2003). Census data for 2000 also indicate that Hispanics are less inclined to use public transportation than blacks, despite having a higher poverty rate. Glaeser et al.'s model also did not account for housing price differentials between cities and suburbs; and it is not entirely clear why the issue is one of central city versus suburbs rather that economic segregation per se, as many suburbs are themselves quite impoverished (Orfield 2002). It is also likely that public transportation systems were endogenously determined by the flight of the affluent to the suburbs, since suburban residents often block the extension of public lines into their municipalities precisely to forestall the entry of poor, minority families from the inner city (see Fogelson 2001, 2005).

Despite these problems, the analysis of Glaeser, Kahn, and Rappaport (2008) does provide an important clue about a significant mechanism underlying economic segregation by showing that poverty rates are lower in the central city and higher in the suburbs of the West and South compared with the Northeast and Midwest. This pattern of regional variation is important because we also know from land use surveys that suburbs in the Northeast and Midwest have more restrictive anti-density regulations than those in the South and West (see Pendall, Puentes, and Martin 2006). Taken together, these two facts suggest that one reason the poor do not live in suburbs is because the construction of affordable housing there is forestalled by density restrictions.

Pendall's earlier (2000) work also suggests that density zoning helps to explain economic segregation by showing that municipalities with low-density zoning regimes experienced 5% slower housing growth and a 0.6 point decrease in multifamily housing units from 1980 to 1990, both statistically significant effects. Here we build on Pendall's analysis to argue that density zoning in the municipalities surrounding major cities constitutes an important cause of class segregation, holding constant other factors associated with metropolitan economic opportunity, population growth, local finance, and motivations to sort by income. In addition to standard regression methods, we apply two-sage least squares to control for the potential endogeneity of zoning policies and class segregation. We find that the relationship is robust to the use of instrumental variables and that, if anything, two-stage least squares increase the size of zoning's apparent effect.

Data and Measures

Our data on density zoning come from Pendall, Puentes, and Martin's (2006) survey of local land use regulations, which asked municipal representatives to report the maximum allowable density permitted for residential construction in their jurisdiction. Upon request, Rolf Pendall supplied us with the coded responses at the local level. The survey, which was conducted in 2003, was organized so as to acquire a representative sample, according to different population categories, of jurisdictions in the 50 largest metropolitan areas in the United States In practice, this covered roughly 33% of all jurisdictions in those MSAs.

Metropolitan areas are defined by the Office of Management and Budget according to commuting patterns between a large central city and surrounding counties and therefore are the best approximation of a regional housing market. As explained in Pendall et al (2006), where the initial response rate was less than 50% of the MSA's population or covered less than 50% of its land area, the surveyors followed up with a second round. Overall, the average MSA response rate was 70%. In total, this means that roughly 29% of all jurisdictions in the 50 largest MSAs responded, yielding a highly representative sample with roughly 1677 jurisdictions of more than 10,000 residents.[1]

As for the land use survey questionnaire, the choices of maximum permitted density were categorical and ranged from under 4 units per acre to greater than 30 units per acre, with the categories of 4-7 units per acre, 8-15 units per acre, and 16-30 units per acre in-between. We assigned these categories ordinal rankings yielding a simple scale of maximum allowable density going from 1 to 5. Other coding strategies yielded identical results.[2] Within each of the 50 metropolitan areas covered by Pendall's survey, we computed the average permitted density score across suburban jurisdictions.

[1]Some MSAs, such as San Antonio, with very few jurisdictions had almost complete coverage despite only three observations, while others with a great many, such as New York and northern New Jersey, had much less complete coverage. We refer readers to Rothwell (2009) for a more detailed technical discussion of how various assumptions about aggregating to the metropolitan level could affect the results. In general, however, his analysis shows that different aggregation strategies have no substantial effect on the results and that aggregated measures of zoning that exclude central cities, or weigh by land area or response rates are all highly correlated.

[2]We tried coding the categories according to maximum units per acre, using the mid-point of each response and 50 for the most permissive. We also tried using actual categorical breakdowns used by Portland Oregon's planning department (i.e. 2.4, 6.2, 11.6, 21.8, 71.5 for the most permissive). The correlations with our ordinal index were 0.98 and 0.95 respectively. See the City of Portland Bureau of Planning and Sustainability, http://www.portlandonline.com/bps/index.cfm?c=31612. Finally, we used our ordinal measure but excluded central cities. This also yielded a high correlation with the original index (0.91) and did not change the results in our models when tested in Tables 2 and 3.

The survey also contains some questions about what zoning was like ten years earlier; and in prior work, we found that density zoning regulations changed little from 1990 to 2000, and that the metropolitan-wide averages of the Pendall scores validly approximate density zoning throughout the period (Rothwell and Massey 2009). In that work, variables indicating the share of jurisdictions in metropolitan areas that changed their density zoning by 10% or more were insignificant. Here we simply assume zoning to be constant in our longitudinal analyses.

The Pendall et al (2006) survey also contained information on other kinds of land use regulation, such as ordinances restricting new development unless developers paid for school infrastructure; growth control statutes to limit permits; containment regulations to reduce sprawl and make development more dense; and pro-development incentives such as affordable housing bonuses, density bonuses, or expedited permitting for affordable housing construction. In earlier work, however, we found that these regulations had weak and inconsistent effects and that they predicted neither metropolitan housing supply (Rothwell 2009) nor racial segregation (Rothwell and Massey 2009). Preliminary models estimated here found the same thing, and we also found no systematic effects when we included more comprehensive indices of zoning developed by Malpezzi (1996) and Gyourko, Saiz, and Summers (2008). Moreover, there is no significant correlation between the use of density restrictions and alternative regulations at the metropolitan level.

Consequently we focus on density regulation as our leading zoning indicator, what Pendall at el (2006) call “traditional” zoning, and argue that it is more fundamental than other regulatory forms because most of the latter are superfluous once high densities are prohibited. As Table 1 shows, the mean density score across the 50 metropolitan areas was 3.39, with a standard deviation of 0.68 and a range from 2.17 to 4.67. Table 1 also reveals considerable variation in the prevalence of affluence and poverty among households.Following Massey and Fischer (2003), poor households are defined as those earning under the federal poverty threshold for a family of four and affluent households are those with incomes at least four times this amount. When the income brackets reported by the census fell in between the measures of poverty and affluence, we counted everyone in that income bracket as poor or affluent. Under this definition, the average poverty rate was 0.19, with a range from 0.13 to 0.29 and a standard deviation of 0.03. The range of affluence was even wider, with an average rate of 0.25, a standard deviation of 0.05, and a range from 0.17 to 0.41.

We measure economic segregation in terms of two dimensions—evenness and exposure (see Massey and Denton 1988). Evenness is the degree to which social groups—in this case income classes—are evenly distributed over any set of geographic units—here census tracts. We chose to measure the evenness dimension using the Gini Coefficient for neighborhood income inequality. Just as the Gini may be used to measure income inequality between people in a population, it can be used to measure income inequality between neighborhoods in a political geography. Using the Census Bureau's Summary File 3 for 1990 and 2000 (accessed hrough Social Explorer), we computed Gini Coefficient using the formula of Deaton (1997: 139):

$$Neighborhood\ Gini=\frac{N+1}{N-1}-\frac{2}{N(N-1)_u}\times\left(\sum_{i=1}^{n}P_iX_i\right) \quad (1)$$

where N is the number of neighborhoods in metropolitan area,: is the average median income of all neighborhoods, X_i is the median income of neighborhood I, and P_i is the rank of neighborhood i such that P=1 when X_i is greatest.

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The principal advantage of this formula is its sensitivity to the bottom of the distribution, such that any transfer in median income from a wealthier neighborhood to a poorer one lowers the Gini score, and this improvement in the score increases as the income disparity between the two neighborhoods increases. A second advantage that makes it preferable to a ratio of the top x% to the bottom y% is that it covers the entire distribution, not just two components, which are arbitrarily defined. Finally, it is insensitive to the average median income of the metropolitan area. In other words, it automatically adjusts for geographical differences in the cost of living (between say New York City and San Antonio), in so far as compensation responds to those differences equally across income groups.

Since we are applying the Gini coefficient to neighborhoods and not individuals, we also adjust for the uneven distribution of people between neighborhoods to avoid giving undue weight to a census tract with just a few households. Specifically, we multiplied the rank P_i by the fraction of the metropolitan area's people who lived in that specific neighborhood to derive a population-weighted Gini. As seen in Table 1, the average Gini for neighborhood income inequality was 0.26, with a standard deviation of 0.06 and a range from 0.12 to 0.36. In practice, this measure was highly correlated with the non-weighted measure.

As for our other measure of economic segregation, exposure is the degree to which members of different social groups are exposed to one another within neighborhoods. Of particular interest to us is the exposure of the poor to the affluent within census tracts, which constitutes a basic indicator of the degree to which the poor have access to whatever resources— social, economic, political, or cultural—possessed by the affluent. We posit that, in general, the affluent seek to minimize contact with the poor, and that they collectively accomplish this goal through density regulations that preclude the construction of affordable, multifamily housing in suburban areas. The poor-affluent exposure index is defined as:

$$\textit{Exposure of Poor to Affluent}=\frac{\sum_{i=1}^{n}\left[\frac{P_i}{T_i}\times\frac{A_i}{T_i}\right]}{\sum_{i=1}^{n}\left[\frac{P_i}{T_i}\right]} \qquad (2)$$

where Pi is the number of poor in census tract i, Ti is the total population of tract i, and Ai is the number of affluent households in the tract. This formula yields the probability that a randomly selected neighbor in the census tract of a poor person will be affluent (Lieberson 1981). As shown in Table 1, we observe a wide range of exposures across the 50 metropolitan areas. The poor-affluent exposure index averaged 0.16 and ranged from 0.10 to 0.30 with a standard deviation of 0.04. The standard deviation of the neighborhood Gini coefficient was higher at 0.06, with a mean of 0.26. Thus we observe sufficient variation across metropolitan areas in both structural conditions and neighborhood outcomes to sustain the analysis.

Our main theoretical explanation for how zoning causes neighborhood class segregation is through its effect on inter-jurisdictional class segregation. To test this formally, we create a measure of spatial inequality between jurisdictions. There are a number of potential ways to measure this inequality. A Gini coefficient could be used, but at the scale of a jurisdiction, with populations ranging into the millions but only a few observations, a single measure of poverty or affluence (such as median income) would miss a great deal and raise complications in terms of population weights. On the other hand, an exposure index at the level of a jurisdiction does not make much sense either. Instead we use the dissimilarity index, which is most frequently used as a measure of racial segregation to measure the evenness of populations over neighborhoods. Since we are interested in the segregation of the poor in this paper, we use a jurisdiction's poverty rate as the basis of our analysis. The dissimilarity index applied to the jurisdictional poverty rate measures the evenness with which the poor are dispersed by jurisdiction throughout the metropolitan area. The formula is calculated as:

$$\text{Inter-jurisdictional Poverty Dissimilarity Index}=\frac{\sum_{j=1}^{n}\left(r_j\left|p_j-P\right|\right)}{2RP\,(1-P)} \quad (3)$$

where the subscript *j* refers to the jurisdiction; *r* refers to the number of residents in the jurisdiction; *p* refers to the poverty rate in the jurisdiction; P refers to the poverty rate of the metropolitan area, and R refers to the population of the metropolitan area. The index ranges from 0 to 1 and can be thought of as the percentage of poor people who would have to change jurisdictions to even out the distribution proportionally across the metropolitan area (Massey and Denton 1993). This captures our theoretical expectation of how density zoning affects economic segregation by creating enclaves of poverty and affluence across jurisdictions. To construct this, we use 1990 and 2000 data from the U.S. Department of Housing and Urban Development's State of the Cities Database.

Empirical Models

As noted earlier, we estimate the effect of zoning on economic segregation using both ordinary least squares (OLS) and two-stage least squares (2SLS) regression methods, adopting them in a two-step approach. Since we only have zoning data for 50 MSAs at one point in time, we are not able to use metropolitan level fixed effects or year effects models, as would be possible in a panel time series. Fortunately, a considerable amount of data are available at the metropolitan level; so as a next best method, we estimate an OLS and 2SLS regressions with exhaustive controls designed to hold constant variables that might be correlated with economic segregation or zoning density.

Although the use of so many control variables with just 50 observations introduces a potential for over-identification, we address this issue by also specifying a more parsimonious structural model. Our strategy was first to estimate an OLS regression with exhaustive controls and then retain those that were most significant in this analysis and move on to specify a more parsimonious 2SLS model. The general form of the OLS equation is:

$$S_{mt}=\alpha+\beta_1 Z_m+\beta_2 D_{mt}+\beta_3 X_{mt}+\mu_t, \quad (3)$$

where S_{mt} indicates the level of economic segregation in metropolitan area m at time t; Z_m is the density zoning regime for that metropolis which we assume is constant over time; D_{mt} is a vector of demographic variables for area m at time t; and X_{mt} is a vector of metropolitan social, political, and economic factors that potentially affect housing market conditions and which may also be correlated with the area's prevailing economic opportunities.

In the full model, the demographic vector includes the percentage black and Latino, the number of recent in-migrants, and population density; the socioeconomic vector includes the poverty rate, affluence rate, the share of the adult without a diploma, the adult college attainment rate, the manufacturing share of employment, union membership, union membership interacted with the manufacturing sector, the share of local revenue from local sources, per capita state taxes, median household income, the unemployment rate, the ratio of suburban to central city housing, the share of rural housing in the metropolitan area, and the share of commuters with long commute times, as well average January temperature from 1971-2000 (from the National Climatic Data Center), which prior work has shown to predict population growth (Glaeser and Tobio 2007).

We hypothesize that the variables described above are associated with income inequality and economic opportunity. In an alternative model, we rely on a smaller set of variables that capture this effect more directly. One of these variables is the Gini coefficient for household incomes, which we calculate from a 1% sample of the 2000 and 1990 U.S. Census Bureau's Decennial Census, with data provided by IPUMS (Ruggles et al 2008).[3] This index absorbs much of the variation in economic opportunity and skill level as it varies by income group. The formula was identical to the one above expect we used household level data with the household sample weights, as provided by IPUMS. The other control variables used in the parsimonious model are the rates of affluence and poverty, the share of the population that is black or Latino, and the number of jurisdictions, which according to Tiebout's (1956) public goods theory, should produce greater spatial sorting between jurisdictions.

Equation (3) will be used for the cross-section analysis of 2000 data, but we also proposed to study the determinants of change in economic segregation from 1990 to 2000. To do so, we bring S_{mt} to the right-hand side of the equation and use S_{mt+10} on the left-hand side, thereby enabling us to predict 2000 outcomes from baseline conditions in 1990, including the lagged measure of economic segregation, thereby avoiding bias from having variables that are jointly determined.

Despite the plausibility of these specifications, it is still possible that the error term in the model (μ_{τ}) will be correlated with zoning density (Z_m), thereby violating the assumptions of OLS estimation and biasing estimates. This outcome would occur if omitted factors associated with economic sorting are also correlated with zoning, or if past economic sorting is itself a cause of present zoning restrictions.

To control for potential endogeneity, we apply 2SLS; and following Rothwell and Massey (2009), we use year of statehood and population density in 1910 as instrumental variables[4]. The year 1910 is used because zoning was quite rare until Herbert Hoover, as Secretary of Commerce, encouraged the passing of the Zoning Enabling Act in 1920 (Knack, Meck, and Stollman 1996). These two variables should be uncorrelated with (μ_{τ}), but they are strongly correlated with permissive zoning. The correlation between permissive zoning and year of statehood is 0.64, and it is -0.51 for population density in 1910. A regression of permissive zoning on both yields an r^2 of 0.42 and an F-statistic above 20.

The literature provides some limited guidance for understanding the relationship between rural settlements and exclusionary zoning. Olson (1982) argues that political institutions stagnate over time because interest groups with a stake in those institutions form factions to choke off change. In the present case, metropolitan areas in older states are more likely to stagnate because they have had a longer time to establish rural settlements in which anti-development coalitions are likely to form. The reasons they form in rural settlements inside metropolitan areas is recognized by the literature but not well established theoretically (Glaeser and Ward 2006, Schuetz 2008).

[3]For New Haven in 2000, we had to use a separate 5% sample of the city of New Haven, because the New Haven-Meridian, CT MSA was dropped by the Office of Management and Budget between 1990 and 2000.

[4]We used U.S. Decennial Census data from the website Social Explorer at the county level to get the metropolitan measures of population density in 1910. We used the latest definitions of MSAs and their county components to do this. Year of statehood data was taken from the U.S. Mint, which reports the data as part of their state quarters program.

Alesina, Baqir, and Hoxby (2004) show that many of these rural jurisdictions were created as whites fled central cities during a time when foreign immigrants and blacks were moving in. Boustan (2010) also identifies white flight as an important cause of post-WWII suburbanization. Economic incentives stemming from declining transportation costs and federal housing subsidies were also responsible (see Glaeser and Kahn 2004; Jackson 1985; Gyourko and Voith 1997). Once established, rural places inside metropolitan areas faced diseconomies of scale in the provision of public goods as Ladd (1992, 1994) has shown. As a result, residents in low density jurisdictions set up barriers to maintain low taxes, or in the words of Fogelson (2005), a semblance of permanence and stability.

Indeed, year of statehood is highly correlated (−0.68) with the density of rural housing units in 1990 (the number of rural units in a MSA divided by its land mass). In the absence of a strong agricultural base, rural land was more likely to be developed into small towns in the Northeast and Midwest, and these municipalities were then more likely to employ density zoning to protect themselves against competing land uses, as our data shows (see Pendall et al 2003 for regional patterns in exclusionary zoning).

This fiscal motivation seems to be conflated and compounded by a bias against the poor, who are often minorities. They are viewed as uniquely burdensome fiscally because their contribution in property taxes is thought to be less than their consumption of public goods (Downs 1973). These motivations compel many low density towns and municipalities to use anti-density zoning. Very little research has been done in trying to explain the sociological motivations for zoning in terms of prejudice. Downs (1973) and Fischel (1985) both argue that class prejudice is a significant factor in zoning, though neither formally test that hypothesis. Using detailed survey data in Los Angeles, Bobo and Zubrinsky (1996) find that racial bias explains variation in attitudes towards racial integration much more than class bias. Massey and Denton (1993) similarly find evidence that racial bias dominates class bias when it comes to integration.

Zoning and Economic Segregation in 2000

The top panel of Table 2 summarizes the effect of density zoning on class segregation by using OLS to regress the zoning score on the Gini Coefficient and the poor-affluent exposure index while controlling for a range of variables likely to affect either zoning or class segregation. To conserve space, we only show the coefficient for the zoning variable and simply list the controls at the bottom of the table, but full equations will be sent upon request. Standard errors are estimated to be robust with respect to heteroskedasticity and geographic clustering at the metropolitan level. Recall that the density is measured on a 1 to 5 scale where larger values indicate a more permissive zoning regime—i.e. higher allowable densities. Thus, the higher the score the greater the average density allowed and, according to our reasoning, the more likely affordable housing is to be built in suburban or affluent areas, leading to lower levels of income segregation.

As Table 2 shows, this is exactly what we find. A metropolitan area's average density score is significantly and negatively associated with the degree of income segregation—the higher the score, the lower the degree of sorting by income. The observed range in the zoning index is from 2.17 to 4.67, a shift of 2.5 points. When the coefficient of -0.041 is applied to this value the product is -0.07, meaning that shifting from the most restrictive to the most expansive zoning regime would reduce the Gini Coefficient by almost two standard deviations (see Table1), which is a very large effect. We also find that density zoning has significant influence on the poor-affluent exposure index, raising it by 0.017 points for each point on the zoning scale. A hypothetical change in density zoning from the most restrictive to the least restrictive would increase inter-class exposure by slightly more than one standard deviation.

The second panel in Table 2 repeats the analysis using 2SLS and a subset of controls designed to maximize the adjusted r-squared and conserve degrees of freedom. Considering the neighborhood Gini, we see that the estimated effect of zoning on class segregation is robust to instrumental variable estimation and the effect is somewhat strengthened by using 2SLS, shifting upward in absolute value from 0.041 to 0.054. The instrumented effect of zoning on the poor's exposure to the affluent is also significant, but in this case, the effect was slightly smaller, suggesting that the endogeneity bias slightly over-stated the relationship between zoning and economic segregation. Nonetheless, in both cases the hypothesized effect persists under instrumental variable estimation. This implies that the effect of zoning on economic segregation is indeed causal. The standard tests for instrumental variables confirm this. The Anderson coefficient rejects the null hypothesis that the instruments (year of statehood and historic population density) are not predictive of zoning, and the Hansen J-statistic fails to reject the null hypothesis that the instruments are truly exogenous.

Some readers may still be concerned that we are using a large number of control variables to estimate associations between just 50 observations. To address this, Table 3 reruns these regressions using a sparse concentrated model. Here, the factors thought to be structurally correlated with income inequality between households and jurisdictions are replaced by direct measures of inequality on those dimensions. The results are very similar in magnitude and significance across the four regressions. Using the instrumental variable coefficients, a change in permitted zoning from the most restrictive to the least would close 50% of the observed gap between the most unequal metropolitan area and the least, in terms of neighborhood inequality. This is equal to two standard deviations. The instrumented effect of permitted density zoning on inter-class exposure is smaller at just under one standard deviation. The only substantial difference when comparing these class exposure results to those in Table 2 is that now the effect of zoning is only significant at the 10% level in the 2SLS regressions.

Accounting for Change 1990-2000

Overall, then, the evidence is consistent with the idea that restrictive density regulations prevent the construction of high-density, multi-family housing, and thereby limit the supply of affordable housing by increasing the average price of units in affluent neighborhoods to the exclusion of lower income people. In this section, we analyze the marginal effect of density zoning on changes in economic segregation between 1990 and 2000. As discussed above, we take a conservative econometric strategy in an attempt to avoid endogeneity by using 1990 control variables to predict 2000 levels of economic segregation conditional on 1990 levels of segregation. The results of this analysis are presented in Table 4, with the fully specified OLS estimates shown in the top panel and the more refined 2SLS model shown in the bottom panel.

The OLS estimates reinforce and extend the earlier cross sectional findings, clearly showing that more permissive density zoning significantly mitigates shifts toward greater economic segregation over time and promotes greater contact between the poor and the affluent. Each ordinal increase in density zoning reduces the shift toward greater segregation by -0.026 points and increases poor-affluent exposure by 0.019 points. When the analysis is repeated under 2SLS estimation, the coefficients change as they did before, with respective values of -0.35 and 0.015, indicating that the endogeneity bias attenuates the association of zoning with overall neighborhood inequality but slightly inflates its association with inter-class exposure.

Table 5 confirms the strength of these relationships using the parsimonious models described above for both the OLS and 2SLS estimation. As with the 2000 regressions, the estimated effects of higher permitted density are noticeably higher in terms of the evenness of the neighborhood median income distributions, but again, they are just slightly lower in terms of inter-class exposure. For both Tables 4 and 5, the instrumented estimates and their evaluation statistics (i.e. the Anderson and Hansen coefficients) suggest that the effect of density zoning on changes in class segregation is indeed causal.

An important theoretical consideration is whether zoning operates on neighborhood segregation through the neighborhood, through the jurisdiction, or some combination of the two. Unfortunately, we only have zoning data at the level of jurisdictions and not within jurisdictions. Yet, neighborhood-level differences in zoning are not likely to matter as much as inter-jurisdictional differences. Once a jurisdiction agrees to allow high density development—whether to accommodate work-force needs or because of outside political pressure—it does not matter, from a fiscal standpoint, where the development is located. It is only out of prejudice, cultural preference, or the related concerns over crime and property values that would motivate residents to use zoning to delimit neighborhoods in the same jurisdiction. In other words, the economic and political incentives are not as sharp. The major exception to this logic is that jurisdictions with multiple school districts boundaries will still create fiscal incentives for affluent people to segregate themselves from the poor via zoning, a point that should be the subject of further research to try to pinpoint the role of class prejudice as opposed to fiscal prejudice.

In this section, we simply test whether or not zoning works through inter-jurisdictional segregation using the dissimilarity index for the jurisdictional poverty rate. Inter-jurisdictional segregation is likely to both cause and be caused by neighborhood segregation so regressing the former on the latter is not particularly illuminating. Our strategy here is to use permitted density zoning as an instrumental variable to try to tease out the ways in which jurisdictional segregation causes neighborhood segregation through zoning only and not other factors associated with sorting. Zoning is an imperfect instrument because zoning may also cause neighborhood segregation in other ways than through jurisdictions.[5] However, if we find no relationship between instrumented jurisdictional segregation and neighborhood segregation, then we can rule out jurisdictional segregation as an explanation for zoning's effect.

Table 6 reports the results, which are consistent with our theoretical prediction. The first stage establishes that zoning is indeed strongly associated with inter-jurisdictional segregation, even conditional on overall income inequality, the poverty rate, the affluence rate, and the minority population rate. As the second stage shows, the portion of jurisdiction-wide segregation associated with zoning has a very strong positive effect on neighborhood inequality and a very strong negative effect on the exposure of the poor to the affluent.[6] Indeed, changes across the range of variation in inter-jurisdictional segregation can account

[5]There is no way to definitely prove that this is not a problem, but using two instruments allows one to run regressions with one instrument at a time and effectively test if the excluded instrument is correlated with the error term (as in the Hansen statistic, which is calculated automatically in STATA). We experimented with this by using an alternative measure of density regulation as a second instrument, using data from Gyourko et al (2008). When we did this, the Hansen statistic and the Anderson correlation could not reject the null hypothesis that both instruments are valid and relevant, respectively. We report the results only using density zoning to avoid complicating the analysis unnecessarily.

[6]The results are robust to controlling for population density.

for the full range of observed neighborhood inequality, as measured by the Gini coefficient, and half of the full range of observed variation in the poor's exposure to the affluent.

In considering these results, we focus on three potential explanations. It is well established in the zoning literature that richer jurisdictions are much more likely to use exclusionary zoning (Glaeser and Ward 2006, Schuetz 2008, Fischel 1985), and given the results in Table 6, this is the most likely explanation for our results: exclusionary zoning exacerbates neighborhood inequality by segregating low income people by jurisdiction. Affluent jurisdictions exclude the poor by blocking developers from building moderate to high density housing. Their motivation is most likely driven by a combination of fiscal concerns and class bias.

A secondary explanation concerns the tenure and stock of housing built under the different regimes and does not rely on inter-jurisdictional effects. It is easier to accommodate low income people in a given neighborhood if townhouses and apartments are present, then if there are only large single-family homes (see Mitchell 2004 for a discussion of the relationship between zoning and housing choice). Housing units become affordable more easily when supply exceeds demand, or when housing agencies make explicit arrangements with developers.

Finally, a third explanation for our results is that exclusionary zoning between neighborhoods may be more likely if exclusionary zoning is used to distinguish jurisdictions, and we believe this will be driven by prejudice against low income people more than fiscal incentives.

The analysis presented above also distinguishes between a static equilibrium effect of zoning on segregation, illustrated by the 2000 cross-sectional results, and a dynamic one, illustrated by changes from 1990 to 2000. We believe that the static relationship between zoning and neighborhood class segregation is mostly driven by inter-jurisdictional class segregation, whereas the dynamic effect arises because residents of impoverished neighborhoods find it hard to exit these areas when the choices are either enclaves of affluence or other poor neighborhoods. Our zoning data do not vary over time, so we cannot estimate a panel model or difference out unobserved fixed effects of metropolitan areas; but we believe that our large set of controls, various specifications, and use of instrumental variables guard against potential biases.

Conclusion

In the 1990s, researchers documented an increase in class segregation within U.S. metropolitan areas but did not address its causes other than to point out the obvious fact that more income inequality creates more potential for segregation (Massey and Eggers 1993; Jargowsky 1997; Watson 2006). However, a new data set on land use regulations created by Pendall allows us to examine the degree to which the political regulation of housing production contributes to income segregation measured both in terms of evenness and exposure. To the extent that housing units differ in price, in a competitive market people will sort themselves into different homes based on the ability to pay. If high priced housing units are located in different neighborhoods than low-priced housing units, then economic segregation will inevitably occur.

In the absence of land use regulation, however, it is not immediately apparent that low-priced units necessarily must be separated spatially from high priced units. Although land may be more expensive in some areas than others, if there is a reason for low income people to want to live there (better access to employment and services) then entrepreneurs have an incentive to provide low-cost housing by using the land more intensively, building more units per acre to amortize the higher costs among a larger number of consumers.

Land markets, of course, are not unregulated, although the degree of regulation varies from place to place. We hypothesize that one kind of land use regulation—the setting of maximum allowable densities for residential construction—plays a key role in determining the degree of class segregation that prevails in a metropolitan area. By limiting the ability of developers to produce affordable, multi-family housing projects, restrictive density zoning promotes income segregation by channeling low-income households to systematically different locations in the urban geography than high-income households.

We tested this hypothesis by assembling a data set that measured the average restrictiveness of density zoning in the suburbs of 50 metropolitan areas using a five point ordinal scale and then regressed this measure on two measures of income segregation—the Gini Coefficient for neighborhood income inequality and the poor-affluent exposure index—while controlling for a variety of factors likely to influence the nature and distribution of metropolitan housing. Our estimates consistently showed that density zoning had strong and significant effects, both in determining the level of class segregation prevailing in a metropolitan area at any point in time, and in determining the change in class segregation over time. Measured over the range of zoning, the effects were quite large, accounting for over two standard deviations in the Gini Coefficient for neighborhood income inequality and one standard deviation in the exposure of the poor to the affluent in the year 2000. Replication of these results using two-stage least squares suggest the effect of density zoning on class segregation is indeed causal.

Zoning originally developed in the 1920s in rural settlements on the outskirts growing cities, and became more prominent as industrialization, black migration, and immigration increased the density of central cities. Residents of suburban jurisdictions had strong fiscal incentives, buttressed by racial and class prejudice, to maintain the character of their towns by blocking dense residential development. As a result, poverty became concentrated in dense areas with affordable housing, mostly in central cities, and surrounding suburbs became enclaves of low-density affluence. In sum, class segregation is as much a product of politics as of markets. Although markets allocate people to housing based on income and price, political decisions allocate housing of different prices to different neighborhoods and thereby turn the market into a mechanism for class segregation.

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Table 1

Summary of key variables in analysis of density zoning and economic sorting within the 50 Largest U.S. metropolitan areas, 2000.

Variable	Mean	Standard Deviation	Minimum	Maximum
Structural Conditions				
Permitted Density Zoning	3.39	0.68	2.17	4.67
Poverty Rate	0.19	0.03	0.13	0.29
Affluence Rate	0.25	0.05	0.17	0.41
Household Income Gini	0.43	0.03	0.37	0.50
Neighborhood Outcomes				
Neighborhood Income Gini	0.26	0.06	0.12	0.36
Poor-Affluent Exposure	0.16	0.04	0.10	0.30
Inter-Jurisdiction Poverty Segregation	0.26	0.09	0.10	0.42

Table 2

Extensive Model of OLS and 2SLS regressions of density zoning on economic segregation and neighborhood exposure in 50 U.S. metropolitan areas in 2000 (robust, clustered, standard errors in parentheses)

Independent Variables	Neighborhood Gini	Poor-Affluent Exposure
Ordinary Least Squares		
Permitted Density Zoning	−0.041**	0.017**
	(0.013)	(0.006)
Constant	0.190	0.046
	(0.202)	(0.108)
Adjusted R^2	0.552	0.875
Two Stage Least Squares		
Permitted Density Zoning	−0.054**	0.014*
	(0.015)	(0.006)
Constant	0.219	−0.051
	(0.177)	(0.073)
Adjusted R^2	0.469	0.842
Anderson Correlation	33.134	33.134
p-value	0.000	0.000
Hansen J Statistic	0.222	0.019
p-value	0.637	0.891

** p<.01;

* p<.05; Year of statehood and metropolitan population density in 1910 are the instruments. Anderson tests null hypothesis that instruments are irrelevant; Hansen tests null that they are uncorrelated with the error term.

OLS Controls: 2000 values of Poverty Rate, Affluence Rate, Percentage of Residents who Rent, Percentage Aged 25+ with a College Degree, Percentage Black or Latino, Population Density, Manufacturing Share of Workforce, Agricultural Share of Workforce, Median Household Income, Number Persons Who Arrived 1995-2000, Unemployment Rate, Average January Temperature, Rural Share of Housing Units, Percentage of Commuters with Commute Times Over 35 Minutes, Suburban Share of Housing, Per Capita State Tax Burden, Share of Local Tax Revenue from Property Taxes, Share of local government expenditure from local revenue, Union Share of Workforce, Interaction of Union share and Manufacturing Share, Percentage Aged 25+ with No High School Diploma

2SLS Controls: 2000 values of Poverty Rate, Affluence Rate, Percentage Black or Latino, Population Density, Manufacturing Share of Workforce, Union Share of Workforce, Interaction of Union share and Manufacturing Share, Share of Adults 25+ Without High School Diploma, Per Capita State Tax Burden, Number Residents Who Moved into MSA 1995-2000.

Table 3

Parsimonious Model of OLS and 2SLS regressions of density zoning on economic segregation and neighborhood exposure in 50 U.S. metropolitan areas in 2000 (robust, clustered, standard errors in parentheses).

Independent Variables	Neighborhood Gini	Poor-Affluent Exposure
Ordinary Least Squares		
Permitted Density Zoning	−0.048 **	0.015 **
	(0.009)	(0.004)
Constant	0.256	−0.080
	(0.139)	(0.057)
Adjusted R^2	0.381	0.824
Two Stage Least Squares		
Permitted Density Zoning	−0.048 **	0.012
	(0.013)	(0.006)
Constant	0.253	−0.056
	(0.160)	(0.073)
Adjusted R^2	0.381	0.820
Anderson Correlation	32.966	32.966
p-value	0.000	0.000
Hansen J Statistic	0.696	0.358
p-value	0.404	0.550

** p<.01;

* p<.05; Year of statehood and metropolitan population density in 1910 are the instruments. Anderson tests null hypothesis that instruments are irrelevant; Hansen tests null that they are uncorrelated with the error term.

OLS & 2SLS Controls: 2000 values: Household Income Gini Coefficient, Poverty Rate, Affluence Rate, Percentage Black or Latino, Number of Jurisdictions

Table 4

Extensive Model of OLS and 2SLS regressions of density zoning on changes in economic segregation in 50 MSAs from 1990-2000 (robust, clustered, standard errors in parentheses).

Independent Variables	Neighborhood Gini	Poor-Affluent Exposure
Ordinary Least Squares		
Permitted Density Zoning	−0.026**	0.019**
	(0.012)	(0.005)
Constant	−0.478	0.134
	(0.312)	(0.091)
Adjusted R^2	0.613	0.819
Two Stage Least Squares		
Permitted Density Zoning	−0.035*	0.015**
	(0.014)	(0.006)
Constant	−0.016	0.012
	(0.129)	(0.055)
Adjusted R^2	0.638	0.815
Anderson Correlation	25.809	38.982
p-value	0.000	0.000
Hansen J Statistic	0.001	0.092
p-value	0.979	0.762

** p<.01;

* p<.05; Year of statehood and metropolitan population density in 1910 are the instruments. Anderson tests null hypothesis that instruments are irrelevant; Hansen tests null that they are uncorrelated with the error term.

OLS Controls: 1990 values of Economic Segregation Index, Poverty Rate, Percentage Black or Latino, Population Density, Manufacturing Share of Workforce, Median Household Income, Agricultural Share of Employment, Share 25+ with College Degrees, Unemployment Rate, Number Residents Who Moved into MSA 1980-1990, Average January Temperature, Rural Share of Housing Units, Suburban Share of Housing Units, Share of Commuter with Commute Times Over 35 Minutes, Per Capita State Tax Burden, Share of Local Tax Revenue from Property Taxes, Union Share of Workforce, Interaction of Union share and Manufacturing Share.

2SLS Controls: 1990 values of Economic Segregation Index, Poverty Rate, Percentage Black, Population Density, Manufacturing Share of Workforce, Union Share of Workforce, Interaction of Union share and Manufacturing Share, Number Residents Who Moved into MSA 1980-1990, Share 25+ with a College Degree.

Table 5

Parsimonious Model of OLS and 2SLS regressions of density zoning on changes in economic segregation in 50 MSAs from 1990-2000 (robust, clustered, standard errors in parentheses).

Independent Variables	Neighborhood Gini	Poor-Affluent Exposure
Ordinary Least Squares		
Permitted Density Zoning	−0.032 **	0.015 **
	(0.010)	(0.004)
Constant	.247 **	0.042
	(0.085)	(0.054)
Adjusted R^2	0.574	0.805
Two Stage Least Squares		
Permitted Density Zoning	−0.034 *	0.013 *
	(0.014)	(0.006)
Constant	.256 **	0.051
	(0.098)	(0.056)
Adjusted R^2	0.574	0.804
Anderson Correlation	32.715	42.291
p-value	0.000	0.000
Hansen J Statistic	0.010	1.104
p-value	0.919	0.293

** p<.01;

* p<.05; Year of statehood and metropolitan population density in 1910 are the instruments. Anderson tests null hypothesis that instruments are irrelevant; Hansen tests null that they are uncorrelated with the error term.

OLS & 2SLS Controls: 1990 values: Economic Segregation Index, Household Income Gini Coefficient, Poverty Rate, Affluence Rate, Percentage Black or Latino, Number of Jurisdictions

Table 6

Model of 2SLS Regressions of Inter-jurisdictional Segregation, instrumented with Density Zoning, on Neighborhood Segregation in 50 MSAs in 2000 (robust, clustered, standard errors in parentheses).

Independent Variables	Regression Models	
First Stage Regression	Inter-jurisdictional Segregation	
Permitted Density Zoning	−0.058 **	
	(0.016)	
Constant	0.128	
	(0.195)	
Adjusted R^2	0.570	
F-Statistic	13.4	
Second Stage Regressions	Neighborhood Gini	Poor-Affluent Exposure
Inter-Jurisdictional Segregation	0.830 **	0.268 **
	(0.258)	(0.085)
Constant	0.153	− 0.054
	(0.184)	(0.056)
Adjusted R^2	0.585	0.610
Anderson Correlation	12.121	12.121
p-value	0.000	0.000

** p<.01;

* p<.05; Permitted density is the instrumental variable, with top panel showing the first stage regression. The Anderson correlation tests the null hypothesis that instruments are irrelevant; the Hansen tests is not applicable because there is only one instrumental variable.

Controls for first and second stage: 2000 values: Household Income Gini Coefficient, Poverty Rate, Affluence Rate, Percentage Black or Latino, Number of Jurisdictions