URBAN SEGREGATION: IMPLICATIONS FOR PRODUCTIVITY AND PUBLIC CHOICE

by

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Urban Segregation
Implications for Productivity and Public Choice

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Abstract

URBAN SEGREGATION: IMPLICATIONS FOR PRODUCTIVITY AND PUBLIC CHOICE

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In this dissertation, I explore the causes and consequences of segregation through a public choice and urban economics framework. In Chapters 1 and 2, I examine whether municipalities adopt zoning regulations in order to exclude low-income and minority households. In the third chapter, I develop and calibrate a model of the productivity consequences of labor market segregation in Northern Ireland.

The first chapter, "Land-Use Regulations and Exclusion in Metropolitan Statistical Areas," is co-authored with Garett Jones and Alex Tabarrok. In this chapter we analyze the public choice behind land-use regulation, specifically testing for an exclusionary motive, within metropolitan areas in the United States. We use an existing dataset of land-use regulation intensity among over 2,000 municipalities in the United States and Census demographic data. In general, we find support for a modest motive to exclude low-income households. A one-standard-deviation increase in the Gini coefficient is

correlated with land-use regulations increased by approximately a tenth of a standard deviation. We find little to no support for a motive to exclude minority households. Income alone explains a large amount of variation in land-use regulations, with a one-standard-deviation increase in income correlating with approximately a third of a standard deviation higher land-use regulations. This finding is robust across specifications, and is consistent with motives other than exclusion for the adoption of land-use regulations.

In the second chapter, “Land-use Regulations as Exclusion: A GIS Analysis,” I extend the study of exclusionary land-use regulations to all municipalities in the contiguous United States. I use Geographic Information Systems (GIS) methods to identify the demographics within a set radius of each municipality for which land-use regulation data are available. If the desire to exclude low-income or minority households from a municipality drives the demand for land-use regulation, then larger demographic differences between a municipality and its surrounding areas would increase the incentive to adopt greater land-use regulations. I find evidence that greater income inequality in surrounding areas is correlated with higher land-use regulations. However, I do not find a robust correlation between racial or ethnic dissimilarity and land-use regulations. As a robustness check, I use a dataset of school district desegregation court orders as indicators of a municipality's exclusionary preferences. I find that these court orders are not robustly correlated with higher land-use regulation.

In the third chapter, “Modeling and Measuring Gains from Labor Market Desegregation in Northern Ireland,” I examine the impact of labor market segregation on productivity. Urban economic theories of agglomeration predict that productivity is higher in larger cities, due to better matching between employees and firms. This theory implies that in segregated economies, the gains from matching will be reduced. Over the past several decades, relations between Catholics and Protestants in Northern Ireland have gradually improved, leading to decreased labor market segregation. I develop a model of the impact of labor force segregation on the agglomeration benefits of matching. Then, I use firm-level data to measure changes in employment segregation in Northern Ireland from 2001 to 2013. Finally, I use these estimates to calibrate the agglomeration model to estimate changes in output, wages, and number of firms from desegregation. The calibrated model estimates that a one percentage point decrease in segregation increases net output by 0.04 percent to 0.29 percent – approximately £5.3 million to £37.9 million.

Land-Use Regulations and Exclusion in Metropolitan Statistical Areas[1]

Introduction

Why do zoning rules differ across localities? One factor may be that some voters prefer stringent zoning rules in order to deter low-income or minority households from moving into the area. In the previous literature, this is referred to as the "exclusionary motive." This motive to exclude is a function both of local attitudes and the size of the perceived threat to the community's future demographics.

We model a municipality as facing a greater incentive to exclude the more disparate the municipality's demographics are from those in the surrounding metropolitan statistical area (MSA). While voters in a municipality may in principle wish to exclude any and all outsiders from anywhere in the country, nearby communities will likely be the most salient. Further, potential in-movers overwhelmingly come from the surrounding area: In every five-year period since 1965, well over 50 percent of all household moves have taken place within the same county (Ihrke and Faber, 2012).

Our measure of land-use regulation comes from an existing index from a survey of municipalities. While some land-use restrictions are adopted at the state level, most

[1] This chapter is co-authored with Garett Jones and Alex Tabarrok.

regulations are adopted at the municipality level. We make the plausible assumption that local governments respond strongly to local voter preferences, so land-use regulations will be adopted according to the desires of the municipality's voters. Thus, in communities with a greater incentive to exclude, we expect higher land-use regulations.

In general, we find support for a modest motive to exclude low-income households. A one-standard-deviation increase in the Gini coefficient is correlated with land-use regulations increased by approximately a tenth of a standard deviation. We find little to no support for a motive to exclude minority households. Income alone explains a large amount of variation in land-use regulations, with a one-standard-deviation increase in income correlating with approximately a third of a standard deviation higher land-use regulations. This finding is robust across specifications, and is consistent with non-exclusionary motives for the adoption of land-use regulations. State fixed effects ensure that our reported results are not merely driven by broad regional differences in zoning rules that may correlate with our other variables.

Related literature

In his literature review in 2004, Ihlanfeldt identifies four possible motivations for imposing land-use regulations: reducing negative externalities, reducing unit costs of public goods, preserving "rural" or similar character of the town, and exclusion of minorities or low-income households (Ihlanfeldt 2004). He notes that the exclusionary motive itself has several possible origins, from outright prejudice to fear of increased crime or falling property values; these motives are hard to disentangle empirically.

Investigating the causes of land-use regulations, Rolleston (1987) uses a sample of nine counties in New Jersey to estimate the influence of a variety of motives for zoning; she finds evidence of racial exclusion but not exclusion of low-income households. Pogodzinski and Sass (1994) build on Rolleston's empirics, taking into account the endogeneity of zoning regulations. They use home sales in Santa Clara County, California, and find that wealthier areas have higher land-use restrictions -- evidence consistent with the fiscal and exclusionary motives. Bates and Santerre (1994) attempt to identify the impact of the proportion in poverty in 132 Connecticut communities and the poverty level in the nearest central city; they find exclusion motivates land-use restrictions but not minimum lot size requirements.

Glaeser and Ward (2009) examine the causes and consequences of land-use restrictions in the greater Boston area. They conclude "that the bulk of these [land-use] rules seem moderately random and unrelated to most obvious explanatory variables." Looking at historical demographic data, they find "weak evidence for the view that high minimum lot sizes where (sic) used by white natives to restrict homes built for blacks and foreigners."

Modeling demand for land use regulations are Fernandez and Rogerson (1997), who use a two-community model to find the equilibrium results of zoning regulations on public education provision. Under their model, increases and decreases of utility occur among various segments within both the rich and the poor communities. Calabrese, Epple, and

Romano (2006) also model zoning; they find the equilibrium of zoning regulation leads to heterogeneous communities and creates non-Pareto efficiency gains.

Among those looking at impacts of land restrictions are Rothwell and Massey; they study how density zoning leads to income segregation (2010) and racial segregation (2009). For their analysis, they use a nationwide survey of restrictive zoning laws from Pendall, Puentes, and Martin (2006). They use Gyourko, Saiz, and Summers's Wharton index (2008) as a robustness test; using the alternative index does not change their findings (Rothwell and Massey 2009).

Land-use regulation: Examples and recent history

Researchers have used the terms land-use regulation and zoning in a variety of ways. Generally, zoning is a subset of land-use regulations concerned with appropriate use of specific lots or areas. Minimum lot sizes and limitations on multifamily housing are examples of zoning regulations. Other land-use regulations not generally considered zoning may be wetland restrictions, infrastructure cost requirements, and permit caps. For our purposes, we will use the categorizations from Gyourko, et al. (2008), as we use their measures of land-use regulations for our study. Their Wharton Residential Land-Use Regulations Index (WRLURI) consists of 11 subindices of land-use regulation. These subindices fall into three categories: The political process of adopting or changing land-use regulations; the local land-use rules themselves; and the actual administration of those rules, including the layers of approval and average delays for permits. They find strong correlations between all their subindices.

The date of each municipality's adoption of land-use regulation is not available. However, some general historical trends emerge. William Fischel has examined the origin and history of zoning, and describes comprehensive zoning as arising in the 1910s, and becoming increasingly exclusionary in the 1970s (Fischel 2004). According to Glaeser and Ward's 2009 study of land-use regulations in the greater Boston area, "[T]here was an initial wave of zoning in the 1920s followed by a much greater wave after World War II." They note the difficulty of ascertaining a specific time of adoption of minimum lot size restrictions – a major component of zoning. They can trace cluster zoning and septic and wetland restrictions, however, as increasing sharply in the late 1970s in the greater Boston area.

Following Fischel (2016), we look to Google's ngrams to help ascertain the timing of the rise of zoning. This feature tracks usage of words in printed books over time, as a percentage of total words. Thus the results are not skewed by larger numbers of digitized recent books. As shown in the graph below, American English usage of zoning and related terms rises throughout the 20th century. This does not directly measure the passage of land-use regulations in the United States, and may lag or lead their adoption, but gives some insight into the general trends in popularity of these ideas.

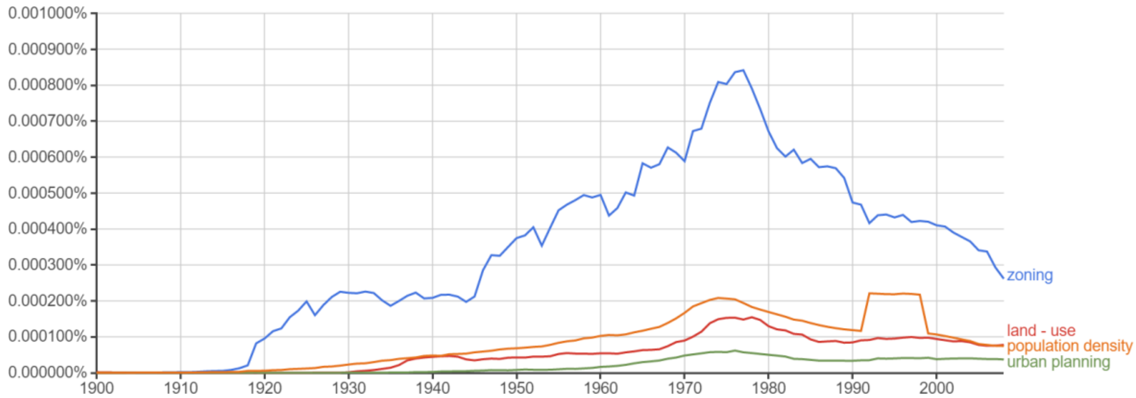

Figure 1: Google ngrams for zoning terminology since 1900

Source: Google ngrams. The frequency of use of "zoning," "land use," "population density," and "urban planning" in digitized books in American English from 1900 to 2008. Frequency of appearance is reported as a percentage of all words from Google's digitized books in that year.

The exclusionary motive

We hypothesize that the incentive to exclude potential neighbors increases with the heterogeneity between a municipality and its surrounding populations. In an entirely homogeneous MSA, none of the municipalities would care to exclude any households. When in a heterogeneous MSA, however, a municipality's voters may wish to prevent osmosis from nearby populations changing the municipality's ethnic or economic mix.

An assumption behind our analysis is that land-use regulations are enacted in response to local demand for them. That is, if communities wish to exclude certain households, their representatives will enact policies in pursuit of those goals. In support of this public-choice approach, Gyourko, Saiz, and Summers find “more intensely involved local and state pressure groups in the more highly-regulated places” (2008). We also assume that if the exclusionary motive is at play, higher income and racial disparities within an MSA would be associated with higher land-use regulations.

Data and methods

We use Gyourko, Saiz, and Summers’s Wharton Index, a national index of municipalities’ land-use regulation levels (2008). The comprehensive score combines a variety of measures, from minimum lot sizes to time required to receive a building permit. Higher values indicate more regulation. For our analysis, we look only at municipalities within metropolitan statistical areas (MSAs) for which all independent variables are available. Thus, our sample includes 1741 observations. Gyourko, et al., standardized the index to have a mean of 0 and a standard deviation of 1. To aid interpretation, we re-standardized the index for our restricted sample. Summary statistics for the regulation variable and other variables are included in Table 1.

Current income and demographic data are from the U.S. Census’ American Community Survey, using the 2005-2009 5-year average when available.

We use the MSA-level Gini coefficient as a proxy for income inequality within the MSA. The Gini values run from 0 to 1, with 0 indicating perfect income equality, and 1 indicating complete inequality. That is, higher Gini values correspond with greater inequality.

Local residents may prefer their locality to maintain its current demographic makeup, and they may in addition (or instead) prefer more residents from the culturally dominant ethnic group. To proxy the difference between the demographic makeup of one's own locality and that of the broader MSA, we calculate the sum of the squared differences in group shares between the municipality and the average for the MSA:

$$Ethnicdelta_{ij} = \sum_{g=1}^{n} (s_{gi} - s_{gj})^2$$

Where *n* is the number of ethnic groups *g*, and *s* is the share of ethnic group *g* in municipality *i* or MSA *j*. We break Census data into eight ethnic groups: all Hispanic, and the non-Hispanic shares of: white, black or African American, American Indian and Alaska Native, Asian, Native Hawaiian and other Pacific Islander, some other race, and two or more races. About half of the municipalities in our sample have 7 or more of these groups represented, and three-quarters have 6 or more. We calculate this measure for each municipality, MSA, and county using Census data. To facilitate interpretation of this variable, we center and standardize it by subtracting the mean ethnic delta for our sample and divide by the standard deviation. The ethnic delta variable measures one possible set of drivers of the municipality's incentive to exclude. Since we do not construct a leave-out measure of the ethnic makeup of the MSA without the municipality of interest, the municipality's ethnic makeup is included in the MSA's ethnic makeup. Thus, we may slightly underestimate the disparity between the municipality and the MSA, potentially leading to overestimates of the impact of disparities upon land-use regulation levels.

The second measure of ethnic disparity is the difference in percentage white:

$$Whitedelta_{ij} = \max[pctwhite_i - pctwhite_j, 0]$$

Where $pctwhite_i$ is the percentage white in municipality *i* and $pctwhite_j$ is the fraction white in MSA *j*. This measure is positive for municipalities that have a greater fraction of whites than the surrounding MSA, and zero otherwise.

For MSA-level and county-level regressions, we compute Alesina, et al.'s (2003) index measure of ethnic fractionalization within these areas:

$$FRACT_j = 1 - \sum_{g=1}^{N} s_{gj}^2$$

Where *j* is the MSA or county, *g* is the ethnic group, and *s* is the share of ethnic group *g* in area *j*. Again, to aid interpretation, in the regressions we center and standardize this by subtracting the mean ethnic fractionalization for the MSA or county sample and divide by the standard deviation of that sample; thus, standardized betas are reported.

Table 1: Summary statistics for dependent and independent variables in our main sample

Variables	Observations	Mean	Std Dev	Min	Max
Municipality Dependent					
Regulations	1741	0.000	1.000	-2.261	4.078
Municipality Independent					
MSA Gini	1741	0.453	0.024	0.389	0.537
Ethnic delta	1741	0.000	1.000	-0.571	14.225
Log income	1741	10.907	0.383	9.833	12.351
White delta	1741	0.084	0.093	0.000	0.388
MSA and County Dependent					
MSA regs	233	0.000	1.000	-1.954	4.036
County regs	419	0.000	1.000	-2.567	5.454
MSA and County Independent					
MSA fract.	233	0.000	1.000	-2.040	2.085
County fract.	419	0.000	1.000	-1.710	2.126

Note: Observations are limited to municipalities within metropolitan statistical areas for which all variables are available.

Historical data

In some specifications, we use historical data. Data from this period are not available at the municipality level. From the U.S. Census, county-level median household income is available in 1969. We also use 1979 Gini coefficients for major cities or metropolitan areas, calculated by Madden (2000). We extrapolate these Gini coefficients to the counties in the entire current MSA.

Presidential election voting data by county from 1968 is from CQPress's "Voting and Elections Collection."

Basic model

Our basic model is:

$$Regulations_{ij} = \alpha + \beta_1 DISPARITY_{ij} + \beta_2 INCOME_i + \gamma CONTROLS_{ij} + S_i + \varepsilon_{ij}$$

Where the level of regulations within municipality *i* in metropolitan statistical area (MSA) *j* is measured by the Wharton Index (Gyourko, Saiz, and Summers, 2008). The disparity variable measures either income inequality in MSA *j* or ethnic disparity between municipality *i* and MSA *j*. Income is the natural log of median household income in municipality *i.* The controls include a variety of variables, including the log of total population of municipality *i.* In some specifications we use state fixed effects, denoted by S_i.

Endogeneity is of course a potential confound: Differences in demographics across localities could be effects, not just causes, of land-use regulations. As Rothwell and Massey find, zoning regulations can lead to income and racial segregation within an area (2009, 2010). Land-use regulations may cause sorting, with only the wealthy able to afford to live in stringently zoned areas, rather than initially wealthy populations adopting those high regulations. That said, such sorting is also the *goal* of voters according to the exclusionary motive theory of zoning: If voters encourage individuals with certain demographic characteristics to move to the locality, and individuals with those traits disproportionately move to the locality, then the exclusionary motive is working as the voters planned. It is more plausible that voters (or the local politicians acting as their proxy) know what they are doing when they enact stringent or loose zoning rules rather than that the differences in zoning are overwhelmingly exogenous to voter preferences. Thus our results may overestimate the true causal relationship between voter attitudes and zoning outcomes.

Another potential complication in ascertaining causation arises from sorting based on attitudes. It is plausible that minorities may have settled in areas where they were more accepted. Thus in some areas, higher racial heterogeneity could be a result of more open communities, and those communities also would be less likely to respond to influxes of minorities by instituting land-use regulations. Because we are unable to directly measure municipal attitudes toward minorities, we may *underestimate* the exclusion motive.

While instrumental variables would seem to be the ideal identification strategy, standard instruments for income and racial segregation fail the exclusion restriction or are also correlated with other explanatory variables of interest. For example, lagged income is a common instrument for income inequality, but it is also correlated with income alone, an important explanatory variable itself. Aside from exclusion, other motives, such as public goods provision, are consistent with demand for land-use regulations rising with income.

To address these endogeneity concerns, we use a variety of specifications and robustness checks. To capture varying attitudes toward low-income and minority households, we use state fixed effects; we also break down our sample by region. As a quasi-exogenous shock of diversity that may prompt exclusionary zoning we use influxes of refugees. Finally, to attempt to capture attitudes toward diversity, we look at county-level election returns for George Wallace in 1968. A third-party candidate for president, Wallace earned 10 percent of the vote nationwide and was famous for urging, “Segregation now, segregation forever” (NPR 2013).

Results

Incentive to exclude low-income households

To test for the incentive to exclude low-income households, we use income inequality within the MSA. The log income variable itself is ambiguous – wealthier communities may institute more land-use regulations for reasons other than exclusion. For example, they may adopt more regulation out of the public-goods motive. To test for exclusion, we look at wealth relative to surrounding areas.

Table 2: Land-use regulations and income inequality, with and without state fixed effects

	(1)	(2)	(3)	(4)
VARIABLES	Regulations	Regulations	Regulations	Regulations
MSA Gini	6.100***	3.050***	5.984***	2.964*
	(0.995)	(0.963)	(1.851)	(1.519)
Log income		0.884***		0.679***
		(0.0598)		(0.123)
Log population		0.0239		0.0132
		(0.0178)		(0.0299)
State fixed effects	No	No	Yes	Yes
Observations	1,741	1,741	1,741	1,741
R-squared	0.021	0.131	0.018	0.099
Number of states			49	49

Standard errors in parentheses

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Note: Log income, and log population are municipality-level variables. MSA Gini is an MSA-level variable. Constant not reported.

As shown in Table 2, greater income inequality is statistically significantly associated with higher levels of land-use regulation. Since the Gini values have a small standard deviation of 0.024, we also report the standardized coefficients to aid interpretation of magnitude. This involves transforming each variable to have a mean of 0 and a variance of 1. Doing so allows easy interpretation of the coefficients: a one standard deviation change in the independent variable leads to an estimated standardized beta standard deviations change in the dependent variable. This approach is appropriate for our analysis, due to the large sample size and the lack of intuitive units for many of our explanatory variables. In Table 2, the standardized beta for the Gini in column 2 is 0.073; thus, a one standard deviation change in the Gini coefficient is associated with a 0.073-standard deviation increase in the land-use regulation variable. In column 1, the 6.100 coefficient translates to a 0.145 standard deviation increase in the regulation variable for a one standard deviation increase in the Gini. With state fixed effects in columns 3 and 4, the standardized coefficients are 0.143 and 0.071, respectively.

The coefficients on log income are positive and statistically significant at the 1 percent level. Standardizing the coefficient in column 2 shows that a one standard-deviation increase in log income is associated with a 0.339 standard deviation increase in land-use regulations. The standardized beta for log income in column 4 is 0.260. This strong association between income and land-use regulations is consistent with non-exclusionary motives for land-use regulations discussed above, such as public goods provision.

Incentive to exclude minority households

As one test for exclusion of minority households, we use the ethnic delta variable. The higher the ethnic delta variable, the more the municipality differs ethnically from its surrounding MSA.

Table 3: Land-use regulations and ethnic delta, with and without state fixed effects

VARIABLES	(1) Regulations	(2) Regulations	(1) Regulations	(3) Regulations
Ethnic delta	0.0749***	0.0487**	0.0142	0.00649
	(0.0222)	(0.0209)	(0.0301)	(0.0266)
Log income		0.913***		0.715***
		(0.0587)		(0.130)
Log population		0.0299*		0.0180
		(0.0177)		(0.0307)
State fixed effects	No	No	Yes	Yes
Observations	1,741	1,741	1,741	1,741
R-squared	0.007	0.128	0.000	0.095
Number of states			49	49

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Log income and log population are municipality-level variables. Ethnic delta is calculated with both municipal- and MSA-level data. Constant not reported.

Since the regulations index and the ethnic delta measure were already standardized, these coefficients are easy to interpret. For example, in column 2, a one standard-deviation increase in ethnic delta between the municipality and its surrounding area is associated with 0.049 standard deviations higher land-use regulations.

As with low-income exclusion, including municipality median household income is important, significantly raising the r-squared for the regression. Income is again positive and statistically significant at the 1 percent level. In column 2, the coefficient indicates that a one standard-deviation increase in log income correlates with a 0.350 standard deviation increase in land-use regulations. In column 4, the standardized beta for log income is 0.274.

While approximately 6 percent of the municipalities in our within-MSA sample are under 50 percent white, the literature on exclusion of minorities is largely concerned with white populations seeking to exclude other ethnic groups. It is plausible that non-white ethnic majorities would not use local government to exclude other ethnic groups.[2] We use another measure of ethnic exclusion, the difference in percentage white between the municipality and its surrounding area, to test whether more homogeneously white communities are more likely to institute higher zoning regulations.

[2] For example, some analysts have pointed to underrepresentation of black majorities in local government (see Shanton, 2014).

Table 4: Land-use regulations and white delta, with and without state fixed effects

VARIABLES	(1) Regulations	(2) Regulations	(3) Regulations	(4) Regulations
White delta	1.729***	0.0946	1.573***	0.544**
	(0.206)	(0.250)	(0.423)	(0.251)
Log income		0.908***		0.623***
		(0.0719)		(0.124)
Log population		0.0311*		0.0307
		(0.0186)		(0.0276)
State fixed effects	No	No	Yes	Yes
Observations	1,741	1,741	1,741	1,741
R-squared	0.039	0.126	0.044	0.098
Number of states			49	49

Standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Note: Log income and log population are municipality-level variables. White delta is calculated with both municipal- and MSA-level data. Constant not reported.

The difference in percentage white between the municipality and its surrounding MSA is statistically significantly associated with higher land-use regulations in most specifications. The 1.573 coefficient in column 3 indicates that a one standard deviation increase in the white delta is correlated with 0.180 standard deviation higher regulations. With controls in column 4, the standardized beta for the white delta is 0.062. Again, income is a statistically significant predictor of land-use regulations; the standardized coefficient for column 2 is 0.348. For column 4, a one standard deviation increase in log income is associated with 0.239 standard deviations higher regulations.

Interactions

We also test for interactions between variables related to exclusion.

Table 5: Interaction between ethnic delta and percentage white

VARIABLES	(1) Regulations	(2) Regulations	(3) Regulations	(4) Regulations
Ethnic delta	-0.0906**	-0.0213	-0.0165	-0.0208
	(0.0377)	(0.0358)	(0.0360)	(0.0367)
Percent white	-0.112	-0.556***	-0.506***	-0.543***
	(0.117)	(0.114)	(0.119)	(0.134)
Ethnic delta x Percent white	0.381***	0.0524	0.0242	0.0265
	(0.0644)	(0.0644)	(0.0675)	(0.0676)
Log income		0.997***	0.981***	0.986***
		(0.0663)	(0.0672)	(0.0677)
MSA Gini			1.518	1.498
			(1.094)	(1.094)
Log population				-0.0122
				(0.0199)
Observations	1,741	1,741	1,741	1,741
R-squared	0.027	0.139	0.140	0.140

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Note: Percent white, log income, and log population are municipality-level variables. MSA Gini is an MSA-level variable. Ethnic delta is calculated with both municipal- and MSA-level data. Constant not reported.

Table 5 shows the interaction between the ethnic delta and a municipality's percentage white. With controls included, ethnic delta, percent white, and the interaction between them are all statistically insignificant. Log income is statistically significant at the one percent level.

Table 6: Interaction between percentage white and ethnic delta, with state fixed effects

VARIABLES	(1) Regulations	(2) Regulations	(3) Regulations	(4) Regulations
Ethnic delta	-0.0487	-0.0214	-0.0147	-0.00867
	(0.0497)	(0.0432)	(0.0399)	(0.0400)
Percent white	0.365	-0.106	-0.0219	0.0287
	(0.239)	(0.254)	(0.220)	(0.197)
Ethnic delta x Percent white	0.220	0.0546	0.0185	0.0137
	(0.133)	(0.0929)	(0.0893)	(0.0913)
Log income		0.712***	0.673***	0.667***
		(0.127)	(0.122)	(0.125)
MSA Gini			3.057**	3.024**
			(1.358)	(1.351)
Log population				0.0147
				(0.0273)
State fixed effects	Yes	Yes	Yes	Yes
Observations	1,741	1,741	1,741	1,741
R-squared	0.025	0.094	0.098	0.099
Number of states	49	49	49	49

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Constant not reported.

Table 6 shows the same regressions, but with state fixed effects. Ethnic delta, percent white, and the interaction of the two are statistically insignificant in all specifications.

Log income and the Gini coefficient are statistically significantly positive at the one and five percent levels, respectively.

Table 7: Interaction between rich and Gini coefficient

VARIABLES	(1) Regulations	(2) Regulations	(3) Regulations	(4) Regulations	(5) Regulations
Rich (1 if income higher than MSA median, 0 otherwise)	-2.096**	-0.0155	-0.578	-0.585	0.431
	(0.899)	(0.876)	(1.009)	(0.999)	(0.789)
MSA Gini	2.190	3.088**	3.359**	3.391**	3.466*
	(1.410)	(1.383)	(1.654)	(1.588)	(1.764)
Rich x Gini	5.403***	-0.477	2.111	2.124	-0.799
	(1.980)	(1.950)	(2.265)	(2.245)	(1.700)
Ethnic delta		0.0237		-0.00305	-0.00515
		(0.0221)		(0.0238)	(0.0246)
Log income		1.096***			0.611***
		(0.0864)			(0.136)
State fixed effects	No	No	Yes	Yes	Yes
Observations	1,741	1,741	1,741	1,741	1,741
R-squared	0.056	0.137	0.069	0.069	0.099
Number of states			49	49	49

Standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Note: MSA Gini is an MSA-level variable. Rich and ethnic delta are calculated with both municipal- and MSA-level data. Constant not reported.

In Table 7, we analyze the interaction between income inequality, measured by the MSA Gini coefficient, and relative income, measured by a dummy variable that is 1 if a municipality's median household income is above the MSA average, and 0 otherwise. The results without state fixed effects indicate that higher-income communities are more likely to adopt higher land-use regulations when there is greater income inequality in their MSA. These results are statistically significant at the one percent level. With state fixed effects, however, these estimates are no longer statistically significant. The level of income inequality within the MSA is statistically associated with higher regulations in most specifications.

Table 8: Interactions with all variables of interest, with and without state fixed effects

VARIABLES	(1) Regulations	(2) Regulations State FE
Log income	1.396	3.406**
	(1.124)	(1.293)
MSA Gini	11.39	67.69**
	(26.73)	(31.15)
Log income x MSA Gini	-0.903	-5.942**
	(2.442)	(2.860)
Ethnic delta	-0.0167	-0.0191
	(0.0360)	(0.0407)
Percent white	-0.500***	-0.00453
	(0.120)	(0.218)
Ethnic delta x Percent white	0.0283	0.0488
	(0.0684)	(0.0889)
Observations	1,741	1,741
R-squared	0.140	0.102
Number of states		49

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Log income and percent white are municipality-level variables. MSA Gini is an MSA-level variable. Ethnic delta is calculated with both municipal- and MSA-level data.

Table 8 includes income interactions and ethnicity interactions. Without state fixed effects, no variables or interactions are statistically significant. With state fixed effects, log income is positive and statistically significant at the five percent level, and the Gini coefficient is positive and statistically significant at the five percent level. The interaction between log income and the Gini is negative and statistically significant at the five percent level, indicating that municipalities with higher incomes are less inclined to adopt higher land-use regulations in response to higher income inequality in their MSAs. The ethnic variables are all statistically insignificant in both specifications.

By region

We also test whether attitudes toward low-income and minority households vary by region. Since this would involve the coefficient, rather than the intercept, varying across regions, we do not use regional fixed effects but rather run the regression for each region individually. We break down our data into the nine Census divisions. Table 9 lists the Census divisions and the states included within each.

Table 9: Census divisions and states

Division	**States**
1: New England	CT, ME, MA, NH, RI, VT
2: Middle Atlantic	NJ, NY, PA
3: Midwest	IN, IL, MI, OH, WI
4: West North Central	IA, KS, MN, MO, NE, ND, SD
5: South Atlantic	DE, DC, FL, GA, MD, NC, SC, VA, WV
6: East South Central	AL, KY, MS, TN
7: West South Central	AR, LA, OK, TX
8: Mountain	AZ, CO, ID, NM, MO, UT, NV, WY
9: Pacific	AK, CA, HI, OR, WA

Table 10 shows the test of low-income exclusion broken down by region, with income quintiles indicating a municipality's income quintile within its MSA. Including log income measures the raw effect of higher income on desire for land-use regulations. The income quintiles measure the relative affluence of a municipality compared to its surrounding area. Table 11 does the same with state fixed effects. Table 12 and Table 13 test for racial exclusion, without and with state fixed effects.

Table 10: Regional breakdown of land-use regulations and income inequality

VARIABLES	(New England) Regulation	(Middle Atlantic) Regulation	(Midwest) Regulation	(West North Central) Regulation	(South Atlantic) Regulation	(East South Central) Regulation	(West South Central) Regulation	(Mountain) Regulation	(Pacific) Regulation
MSA Gini	-0.677	4.368*	0.673	-3.154	7.162***	11.63**	8.741**	15.67***	-0.817
	(5.542)	(2.234)	(2.421)	(5.289)	(2.415)	(4.734)	(4.159)	(3.822)	(2.476)
Log income	0.609	0.752***	1.094***	2.654***	1.562***	0.860*	0.496**	1.377***	0.121
	(0.580)	(0.233)	(0.191)	(0.291)	(0.230)	(0.442)	(0.216)	(0.380)	(0.214)
2nd quintile	-0.0397	0.0333	-0.142	-0.491**	-0.344**	-0.399	0.253	-0.418	-0.0104
	(0.373)	(0.170)	(0.137)	(0.198)	(0.170)	(0.260)	(0.214)	(0.292)	(0.161)
3rd quintile	0.311	0.359*	-0.0890	-0.592***	-0.521***	0.135	-0.279	-0.255	0.219
	(0.422)	(0.197)	(0.150)	(0.215)	(0.184)	(0.299)	(0.226)	(0.313)	(0.177)
4th quintile	1.054**	-0.111	-0.0924	-0.961***	-0.418**	-0.125	0.0847	-0.427	0.212
	(0.497)	(0.206)	(0.157)	(0.221)	(0.203)	(0.314)	(0.248)	(0.345)	(0.199)
5th quintile	-0.0660	-0.00282	-0.108	-1.634***	-1.142***	0.0324	-0.183	-0.486	0.152
	(0.619)	(0.254)	(0.199)	(0.279)	(0.263)	(0.492)	(0.297)	(0.403)	(0.239)
Observations	103	252	395	165	234	72	170	112	238
R-squared	0.169	0.226	0.213	0.380	0.235	0.336	0.117	0.245	0.026

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Note: Log income is a municipality-level variable. MSA Gini is an MSA-level variable. Quintiles indicate the municipality's income quintile within its MSA, with the 5th quintile indicating the top quintile with the highest median household income. Constant not reported.

Table 11: Regional breakdown of land-use regulations and income inequality, with state fixed effects

VARIABLES	(New England) Regulation State FE	(Middle Atlantic) Regulation State FE	(Midwest) Regulation State FE	(West North Central) Regulation State FE	(South Atlantic) Regulation State FE	(East South Central) Regulation State FE	(West South Central) Regulation State FE	(Mountain) Regulation State FE	(Pacific) Regulation State FE
MSA Gini	-5.265	2.251	6.649**	1.774	4.396**	16.07***	8.216**	-0.668	-0.191
	(3.296)	(2.564)	(1.906)	(2.172)	(1.777)	(1.872)	(1.520)	(3.017)	(0.374)
Log income	1.007**	0.839	1.050***	1.367***	0.551***	0.518	0.322**	1.497***	0.0607
	(0.284)	(0.332)	(0.199)	(0.239)	(0.147)	(0.447)	(0.0605)	(0.227)	(0.195)
2nd quintile	0.0320	0.0740	-0.163	-0.193*	-0.145	-0.317	0.273	-0.514*	0.0473
	(0.220)	(0.156)	(0.179)	(0.0986)	(0.0769)	(0.329)	(0.172)	(0.243)	(0.272)
3rd quintile	0.286	0.329	-0.105	-0.226	-0.177	0.272	-0.166	-0.343	0.240
	(0.318)	(0.372)	(0.120)	(0.131)	(0.129)	(0.329)	(0.0710)	(0.413)	(0.139)
4th quintile	0.849*	-0.133	-0.109	-0.432**	0.0788	0.0528	0.170	-0.561*	0.282**
	(0.370)	(0.343)	(0.156)	(0.134)	(0.153)	(0.292)	(0.124)	(0.267)	(0.0689)
5th quintile	-0.275	-0.0673	-0.115	-0.633**	-0.165	0.520	-0.0326	-0.597**	0.218
	(0.305)	(0.652)	(0.133)	(0.216)	(0.276)	(0.515)	(0.154)	(0.218)	(0.188)
Observations	103	252	395	165	234	72	170	112	238
R-squared	0.198	0.177	0.226	0.153	0.089	0.412	0.083	0.214	0.026
Number of states	6	3	5	7	8	4	4	8	4

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Log income is a municipality-level variable. MSA Gini is an MSA-level variable. Quintiles indicate the municipality's income quintile within its MSA, with the 5th quintile indicating the top quintile with the highest median household income.

Table 12: Regional breakdown of land-use regulations and ethnic delta

VARIABLES	(New England) Regulation	(Middle Atlantic) Regulation	(Midwest) Regulation	(West North Central) Regulation	(South Atlantic) Regulation	(East South Central) Regulation	(West South Central) Regulation	(Mountain) Regulation	(Pacific) Regulation
Ethnic delta	-0.161	0.106**	0.0419	0.0239	0.165***	0.140*	0.0749	0.207*	-0.0545
	(0.133)	(0.0495)	(0.0430)	(0.0444)	(0.0479)	(0.0800)	(0.0667)	(0.106)	(0.0461)
Log income	0.704**	0.832***	1.052***	1.351***	0.903***	1.008***	0.192	1.137***	0.267*
	(0.295)	(0.120)	(0.104)	(0.196)	(0.160)	(0.226)	(0.165)	(0.278)	(0.142)
Log population	-0.335***	-0.0725	0.0501	0.0123	0.142***	-0.101	0.156***	0.0489	0.0430
	(0.0842)	(0.0447)	(0.0347)	(0.0534)	(0.0422)	(0.0793)	(0.0443)	(0.0663)	(0.0432)
Observations	103	252	395	165	234	72	170	112	238
R-squared	0.221	0.211	0.217	0.234	0.201	0.270	0.086	0.140	0.024

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Constant not reported.

Table 13: Regional breakdown of land-use regulations and ethnic delta, with state fixed effects

VARIABLES	(New England) Regulation State FE	(Middle Atlantic) Regulation State FE	(Midwest) Regulation State FE	(West North Central) Regulation State FE	(South Atlantic) Regulation State FE	(East South Central) Regulation State FE	(West South Central) Regulation State FE	(Mountain) Regulation State FE	(Pacific) Regulation State FE
Ethnic delta	-0.234	0.0506	0.0377*	0.0439***	0.0726*	0.169	0.0630	0.00555	-0.0467***
	(0.138)	(0.130)	(0.0140)	(0.00651)	(0.0323)	(0.231)	(0.0450)	(0.0535)	(0.00655)
Log income	0.700*	0.790	1.101***	0.772***	0.545**	1.017***	0.0831	1.147***	0.248
	(0.298)	(0.290)	(0.198)	(0.185)	(0.158)	(0.158)	(0.109)	(0.154)	(0.134)
Log population	-0.411**	-0.0585	0.0625*	0.0352	0.120***	-0.0697	0.148*	0.0496	0.0482
	(0.103)	(0.0580)	(0.0290)	(0.0674)	(0.0267)	(0.0303)	(0.0495)	(0.0674)	(0.0213)
Observations	103	252	395	165	234	72	170	112	238
R-squared	0.340	0.157	0.218	0.131	0.100	0.295	0.070	0.185	0.023
Number of states	6	3	5	7	8	4	4	8	4

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Constant not reported.

We find that the magnitudes and even signs of the estimated coefficients vary greatly across the United States.

Table 10 shows the effect of income inequality and relative income on land-use regulations. In five of nine regions, the coefficient on the Gini is positive and statistically significant. These regions are: Middle Atlantic, South Atlantic, East South Central, West South Central, and Mountain. Only in the New England region does a higher income relative to the surrounding MSA appear to correlate with higher land-use regulations, with the coefficient for the 4^{th} income quintile being positive and statistically significant at the five percent level. In seven of nine regions, log income is positive and statistically significantly correlated with higher land-use regulations.

Table 11 shows the same regressions with state fixed effects. In this specification, four regions have income inequality that is statistically significantly correlated with higher land-use regulations. These regions are: Midwest, South Atlantic, East South Central, West South Central. In the New England and Pacific regions, the 4^{th} income quintile is positively and statistically significantly correlated with higher land-use regulations; other quintiles and other regions are negative or not statistically significant. With fixed effects, log income is positive and statistically significant in six of nine regions.

Moving onto racial exclusion in Table 12, in four regions the coefficients on the ethnic delta are positive and statistically significant. These regions are Middle Atlantic, South Atlantic, East South Central, and Mountain. Income is positive and statistically significant in all but one of the regions.

In Table 13, the specification with state fixed effects, the ethnic delta is statistically significantly correlated with higher land-use regulations in the Midwest, West North Central, and South Atlantic regions. It is statistically significantly negatively correlated with land-use regulations in the Pacific region. The coefficient on income is statistically significantly positive in six of the nine regions.

Alternative proxies for racial exclusion

As one alternative specification, we use refugees as a quasi-exogenous introduction of minority populations that may prompt demand for exclusionary land-use regulations. This is only quasi-exogenous, as two-thirds of refugees are placed near friends or family already living in the United States (Singer and Wilson, 2006). Yet in a ranking by number of refugees accepted, Singer and Wilson find, "[T]he refugee-receiving metro areas do not fall into the expected order based on their total foreign-born populations" (2007). We use Singer and Wilson's data on total number of refugees initially settled in an MSA from 1983-2004, and divide that by total current population to get refugees as a percentage of total population. This does not capture whether refugees have since moved away from their initial placement.

Again, this regression must be done at the MSA level, using the MSA median household income and the mean land-use index score, weighted by land area. We also use the MSA-level measure of ethnic fractionalization.

Table 14: Refugees and land-use regulations

VARIABLES	(1) MSA Regulations	(2) MSA Regulations	(3) MSA Regulations
Percentage refugees	19.89	-7.775	-7.215
	(12.16)	(11.28)	(11.42)
Log MSA income		2.936***	2.751***
		(0.363)	(0.387)
MSA ethnic fractionalization			0.100
			(0.0638)
MSA log population			5.63e-10
			(3.48e-08)
Observations	233	233	233
R-squared	0.011	0.231	0.240

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Constant not reported.

The sample size is restricted due to the MSA being the unit of observation. The influx of refugees does not appear to affect the adoption of land-use regulations once controlling for MSA income. This result is consistent with minority exclusion not being a strong motive behind land-use regulations, but could also be due instead to the placement of refugees not being sufficiently exogenous, and refugees making up at most 3 percent of the host MSA.

In another alternative measure of the desire to exclude, we use county-level election returns for the segregationist presidential candidate George Wallace in 1968. The dependent variable is the mean Wharton Index score for the county, weighted by land area. County-level log median household income and ethnic fractionalization are also included.

Table 15: Land-use regulations and percentage voting for George Wallace in 1968, county-level

VARIABLES	(1) County regulations	(2) County regulations	(3) County Regulations	(4) County regulations	(5) County regulations
Percent voting for Wallace, 1968	-0.963*** (0.316)	-0.758** (0.367)	-0.915** (0.382)	-0.812** (0.371)	-0.950** (0.385)
Log income, 1969		0.275 (0.250)	0.374 (0.258)	0.252 (0.251)	0.349 (0.260)
Gini, 1979			3.299 (2.266)		3.091 (2.284)
County ethnic fractionalization				0.0457 (0.0490)	0.0378 (0.0494)
Observations	419	419	419	419	419
R-squared	0.022	0.025	0.030	0.027	0.031

Standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Note: Constant not reported.

As shown in Table 15, returns for George Wallace are negatively correlated with land-use regulations, even when including controls. These results remain when including state fixed effects. These results indicate that populations that have exhibited segregationist preferences do not tend to adopt land-use regulations in order to exclude.

Zoning only

Finally, we investigate whether our results are sensitive to looking only at zoning, rather than all the elements of the land-use measure. Thus, we standardize and sum the three zoning elements of the index: Open Space, Density Restrictions, and Supply Restrictions, then re-standardize so the resulting index has a mean of 0 and a standard deviation of 1. Table 16 regresses this zoning score against the measures of the incentive to exclude low-income and minority households.

Table 16: Zoning and the incentive to exclude low-income and minority households

VARIABLES	(1) Zoning	(2) Zoning	(3) Zoning	(4) Zoning
MSA Gini	-0.208			-1.041
	(1.013)			(1.101)
Log income	0.502***	0.497***	0.555***	0.573***
	(0.0629)	(0.0617)	(0.0665)	(0.0690)
Log population	-0.0210	-0.0212	-0.0441**	-0.0443**
	(0.0187)	(0.0186)	(0.0210)	(0.0210)
Ethnic delta		0.0108	-0.0170	-0.0121
		(0.0219)	(0.0249)	(0.0254)
Percent white			-0.313**	-0.342**
			(0.133)	(0.136)
Observations	1,741	1,741	1,741	1,741
R-squared	0.038	0.038	0.041	0.041

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Constant not reported.

Income is statistically significant at the 1 percent level. Estimated coefficients for the low-income and minority measures are negative or very small, and statistically insignificant. Table 17Table 17 shows similar results occur with state fixed effects.

Table 17: Zoning and the incentive to exclude low-income and minority households, with state fixed effects

VARIABLES	(1) Zoning	(2) Zoning	(3) Zoning	(4) Zoning
MSA Gini	-0.112			-0.350
	(1.912)			(1.596)
Log income	0.446***	0.444***	0.476***	0.481***
	(0.119)	(0.124)	(0.141)	(0.135)
Log population	-0.0269	-0.0267	-0.0346	-0.0343
	(0.0275)	(0.0284)	(0.0283)	(0.0280)
Ethnic delta		0.00624	-0.00280	-0.00175
		(0.0364)	(0.0286)	(0.0255)
Percent white			-0.144	-0.149
			(0.219)	(0.217)
State fixed effects	Yes	Yes	Yes	Yes
Observations	1,741	1,741	1,741	1,741
R-squared	0.027	0.027	0.028	0.028
Number of states	49	49	49	49

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Constant not reported.

Discussion and further work

The most robust finding is that income is positively correlated with land-use regulations. This is consistent with non-exclusionary motives, such as public goods provision. It seems that by most measures, the desire to exclude has a negligible impact on land-use regulations. The results for exclusion of low-income households were robust but small in magnitude. Our results did not reveal a motive to exclude minority households, despite using state fixed effects, regional breakdowns, and multiple measures of and proxies for motive to exclude minorities to attempt to isolate this effect.

Thus far, we have studied the incentive to exclude presented by nearby populations of minority or low-income households. The other element of the exclusionary motive is, of course, local attitudes. We may wish to try to measure and control for racism. Racial violence may provide a proxy for local attitudes, depending on reliability of the data. For current attitudes, internet searches and tweets have been categorized according to racist content. Most of these measures appear at the state level, but some of these measures may be available at the city level.

Land-Use Regulations as Exclusion: A GIS Analysis

Introduction and literature review

As residential areas in the United States are segregated by race and wealth (Iceland and Weinberg, 2002), concerns have arisen about the sources and motivations behind that segregation. The exclusionary aspect of zoning regulations is a poignant issue. A strong theme in the zoning literature is that municipalities adopt zoning regulations in order to exclude low-income and minority households (see, for example, Pendall, 2000, and Massey and Denton, 1993). Fogelson (2007) chronicles instances of racially motivated restrictive zoning, describing fear of outsiders as a motivation for restrictive covenants in the early twentieth century. As explicitly racial zoning was restricted, Pendall (2000) reports that local governments used minimum lot sizes and other land-use regulations to accomplish racial exclusion. Clingermayer (2004) posits that smaller, more homogeneous municipalities adopt more exclusionary zoning, even if local politicians use other rhetorical justifications (see also Ellickson, 1977). In 2015, the U.S. Supreme Court held that motives do not matter; zoning regulations that have an exclusionary effect violate the Fair Housing Act, regardless of the intent of the regulations (Texas Department of Housing and Community Affairs v. The Inclusive Communities Project, Inc. 2015).

Many studies have focused on quantifying the impacts of zoning on housing prices and segregation. In a series of articles, Rothwell and Massey find that higher zoning regulations are associated with greater racial and income segregation in urban areas (2009, 2010). Lens and Monkkonen (2016) concur that land-use regulations contribute to residential segregation between rich and poor in metropolitan areas.

This is the first nationwide study to test for the presence of the exclusionary motive. Several prior analyses have used localized case studies to evaluate the motives behind adoption of zoning. Rolleston (1987) finds evidence of racial exclusion in part of New Jersey, but does not find evidence of exclusion of low-income households. In a study of communities in Connecticut, Bates and Santerre (1994) find support for exclusion of households in poverty. Pogodzinski and Sass (1994) study Santa Clara County, California, and discover the percentage of non-Hispanic whites is positively correlated with higher land-use regulations. Results from Clingermeyer's 1996 study of nine metropolitan areas suggests that greater state legislative oversight reduces exclusionary zoning. Considering the rationale behind exclusion, Cervero and Duncan (2004) and Lynch and Rasmussen (2004) find that fewer low-income households and minority households correlate with higher home values.

Ihlanfeldt (2004) identifies four potential goals of land-use regulation: Reducing negative externalities, establishing equitable contribution to public goods, preserving certain land-use patterns,[3] and excluding low-income or minority households. This paper explores the impact of surrounding demography on a municipality's tendency to adopt higher land-use regulations.

For the purpose of this study, land-use regulations encompass any barriers to building development. This includes zoning by use, permitting processes, and requirements such as minimum lot sizes.[4] I employ Geographic Information System (GIS) methods to identify the demographics within a set radius of each municipality. The exclusionary motive predicts that wealthier and more homogenous municipalities with more nearby low-income or minority populations tend to adopt more land-use restrictions. The results offer some evidence of exclusion of low-income households, but largely fail to provide empirical evidence of racial exclusion.

As a robustness check, this study uses school district desegregation orders as indicators of a municipality's incentive to use land-use regulations to accomplish segregation. I also employ a geographic instrumental variable to address spatial autocorrelation of ethnic settlement patterns.

Conceptual model and hypotheses

An assumption behind this research is that adoption of land-use regulation is responsive to local demand. This assumption is consistent with analysis by Fischel (2004); Ellickson

[3] Downzoning, a subset of zoning that restricts subdivision of undeveloped land, is largely used to preserve open space, often farm land. In a New Jersey case study, Adelaja et al. (2009) find that adoption of downzoning increases with declining farm population and rising non-farm populations.

[4] The attributes of land-use are adopted from Gyourko et al. (2007), as I use their comprehensive index of land-use regulations in approximately 2,600 municipalities in the United States.

(1977); and Been et al. (2014). The desire to exclude is a potential motivation for adopting land-use regulation. The exclusionary motive may be a function of both attitudes and surrounding demographics. The combination of these internal and external factors constitutes the exclusionary motive. Without attitudes opposing heterogeneity of incomes or race, a municipality's residents would not pursue exclusion through regulation, including through zoning. Without a perceived threat to the homogeneity of the municipality from surrounding populations, a municipality's residents would have no incentive to pursue exclusionary zoning.

This verbal model gives rise to two hypotheses. First, ceteris paribus, greater internal or external pressure for land-use regulations will lead to stricter regulations adopted. Second, when other methods of exclusion are removed, municipalities with a desire to exclude will substitute into land-use regulations.

Data and methods

Data sources

The index of land-use regulations is from the Wharton Residential Land Use Regulation Index compiled by Gyourko et al. (2008). As with any analysis based on survey responses, selection bias may have been a factor. The index authors note that, compared to the average U.S. municipality, responding municipalities have larger populations, a greater share of the elderly and young, higher median levels of education, higher housing values, a lower share of non-Hispanic whites and less owner-occupied housing (Gyourko et al., 2008). The index contains 2,611 observations, and is normalized to have a mean of 0 and standard deviation of 1. Values run from a minimum of -2.2 to a maximum of 4.8.

For this study, I restrict this sample to the contiguous United States, that is, omitting Hawaii and Alaska. The omitted states contain a total of 8 survey respondents. Further, I restrict my sample to the 1,439 municipalities for which all explanatory variables are available. For this subsample, the regulation index has a mean of -0.33 and a standard deviation of 0.84, with a minimum value of -2.15 and a maximum of 3.46.

Demographic data is from the U.S. Census Bureau's American Community Survey. For most data, 5-year averages from 2005-2009 are used. Density data is from 2010. All Census data used is at the level Census-designated place, that is, incorporated municipalities. There are approximately 26,000 of these municipalities in each Census dataset. Geocoded maps are from the U.S. Census Bureau's Topologically Integrated Geographic Encoding and Referencing (TIGER) database, using U.S. Census 2000 boundary definitions.

Data on court-ordered desegregation in U.S. school districts comes from Reardon et al. (2012). The authors used a variety of sources to construct a comprehensive list of school districts ever under a federal court order to desegregate.

Reduced-form model

I assume motivations regarding negative externalities and public goods provision are randomly distributed within a state. There may be state-level variation in state or federal contributions to public goods, and certain state-level land-use regulations may be in effect in some states. Employing state fixed effects removes all state-level variation on these and other margins. The basic specification is:

$$Regulations_{ij} = \alpha + \beta_1 DemographicDelta_{ij} + \beta_2 Income_i +$$

$$\beta_3 Controls_i + \gamma F_i + \epsilon_{ij}$$

Regulations for municipality *i* are measured by the Wharton Index. Income for municipality *i* is the median household income for that Census-designated place. Controls include a vector of control characteristics, including municipality population.[5] F_i indicates states fixed effects. To control for potential state-level variation in land-use regulations, [6] I include state fixed effects specifications of all regressions, and all fixed effects regressions have standard errors clustered at the state level.

DemographicDelta indicates the income or ethnic difference between municipality *i* and the municipalities within a buffer zone *j*. The buffer zone is a geographic zone of a fixed radius around each survey-responding municipality *i*; in this study, both a 10- and 20-mile radius are used.[7] The income delta is measured by the population-weighted Gini coefficient among the municipalities within buffer *j*.[8]

[5] In a Chicago-area case study, McDonald and McMillen (2004) find that larger suburbs are more likely to utilize growth controls.
[6] Clingermayer (1996) analyzes the presence of state-level legislation curbing local governments' ability to adopt ad-hoc zoning.
[7] These radii were selected because the majority of household moves are within the same county (Ihrke and Faber 2012), and median county size for the contiguous United States is 645 square miles (Census 2010), which would correspond to a 14-mile radius if counties were perfectly circular.
[8] The Gini is calculated using the Ginidesc module for Stata (Aliaga et al., 1999).

Ethnic difference is captured in two ways. First, ethnic difference between each municipality and the surrounding municipalities in the specified buffer zone is measured as:

$$Ethnic_{ij} = \frac{1}{n}\sum\left(s_{gi} = s_{gj}\right)^2$$

where n is the number of ethnic groups g, and s is the share of ethnic group g in municipality i or buffer zone j. The second measure of ethnic dissimilarity is

$$WhiteDelta_{ij} = \max[pctwhite_i - pctwhite_j, 0]$$

where pctwhite is the percentage of non-Hispanic white residents in municipality *i* or buffer zone *j*. WhiteDelta measures the difference in percentage white between a municipality and its neighboring municipalities. This variable is equal to that difference for municipalities that have a higher percentage white than their neighbors, and is zero otherwise. If the exclusion motive primarily affects largely white populations, this measure will capture the exclusion motive more precisely than the Ethnic measure.

Due to the cross-sectional nature of the regulatory index, causal claims are difficult to make.[9] For this reason, I use a variety of robustness checks.

[9] While some authors have analyzed determinants broad zoning shifts, e.g., from residential to commercial (see Cho et al., 2012, and Munneke, 2005), nationwide data on the tightening or loosening of residential requirements such as minimum lot size does not appear to be available.

Court-ordered desegregation

In the decades following *Brown v. Board of Education*, many school districts came under federal court orders to desegregate (Reardon et al., 2011). If the exclusionary motive is a determinant of land-use regulations, we would expect higher land-use regulations in municipalities whose school districts received court desegregation orders. First, segregated districts may indicate local attitudes in favor of segregation, which are expected to be positively correlated with exclusionary zoning. Second, court desegregation orders removed a potential method of excluding; municipalities barred from school segregation may have substituted into land-use regulation in order to achieve exclusion.

As mentioned above, this study uses the dataset compiled by Reardon et al. (2012) of all districts ever under a court order to desegregate. For this study, a municipality is coded as having a court order equal to 1 if any of its school districts were under such an order, and 0 otherwise.

Geographic instrumental variable

I use a geographic instrumental variable, following a process similar to that used by Johnson and Koyama (2016). The location of minority populations may be endogenous to welcoming attitudes, which would also be correlated with lower exclusionary regulations. That is, initial minority populations may settle in places relatively non-hostile toward them, and further influxes of that minority may tend to locate near existing populations of the minority. To address this, I use the demographics of the ring of municipalities outside of the buffer zone to instrument for the demographics of the municipalities within the buffer around the municipality of interest. For the 10-mile buffer, this study uses the municipalities between 10 miles and 20 miles from the municipality of interest; for the 20-mile buffer, the demographics of the 20- to 40-mile ring are used as an instrument. The outer ring is set to be sufficiently far from the core municipality to avoid the neighborhood effects of specific racial groups locating in and close to towns more open to diversity.

As an example, Figure 2 shows the 10-mile and 20-mile radii around Falls Church, Virginia, a suburb of Washington, D.C. Falls Church, highlighted in yellow, answered the Wharton survey, and is municipality i for this example. The 10-mile buffer zone is denoted in orange, and includes such nearby municipalities as Bethesda, Maryland; Annandale, Virginia; and Washington, D.C. The purple ring includes all municipalities between 10 and 20 miles from Falls Church.[10] It includes such municipalities as Rockville, Maryland; Woodbridge, Virginia; and Centreville, Virginia. The average demographics of the purple ring is used to instrument for the average demographics of the orange buffer zone.[11]

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[10] Municipalities partially intersecting the buffer zone or outer ring are included; in Figure 2, for example, Washington, D.C. is included in both the buffer zone and the outer ring for calculation purposes.
[11] 8Demographics of the orange buffer zone and purple outer ring are gathered by averaging the demographics within all the municipalities that at least partially intersect the relevant radius.

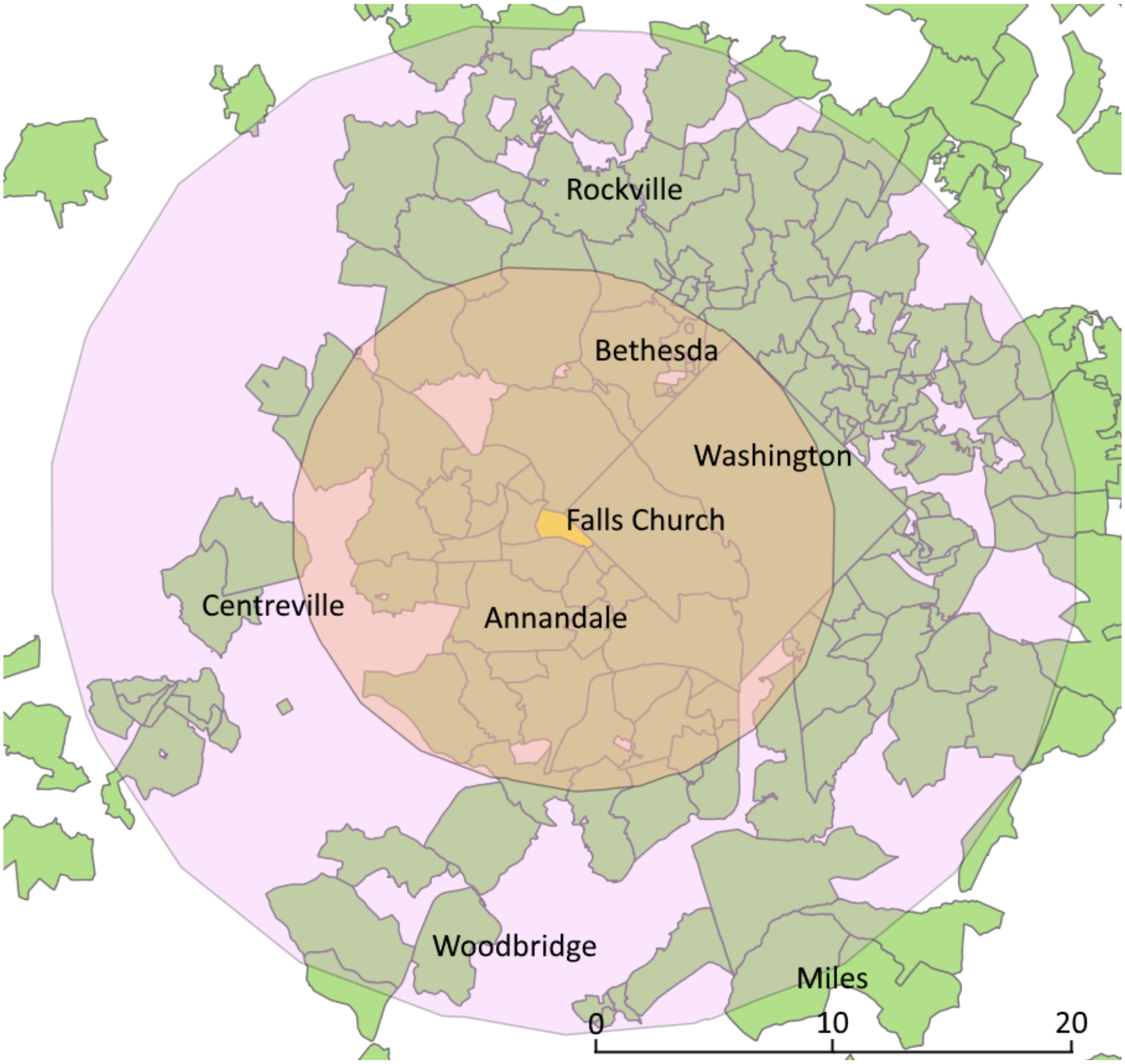

Figure 2: An example of a 10-mile buffer and 10-mile to 20-mile outer ring constructed around Falls Church, Virginia, a municipality that responded to the land-use survey

Results

Neighboring demographics and zoning

In the most basic specification, the difference in ethnic make-up between the municipality and the surrounding area is positively correlated with higher land-use regulations. However, once introducing controls for income and housing density, the magnitude of the coefficient falls and loses statistical significance. A plot of this regression, with those controls as well as state fixed effects, is shown in Figure 3. As the figure indicates, there is minimal relationship between ethnic difference and land-use regulations, and the relationship is not statistically significant. The effect of log income is statistically significant at the 1 percent level. Housing density and the log of total population are also positively correlated with land-use regulations, also statistically significant at the 1 percent level.

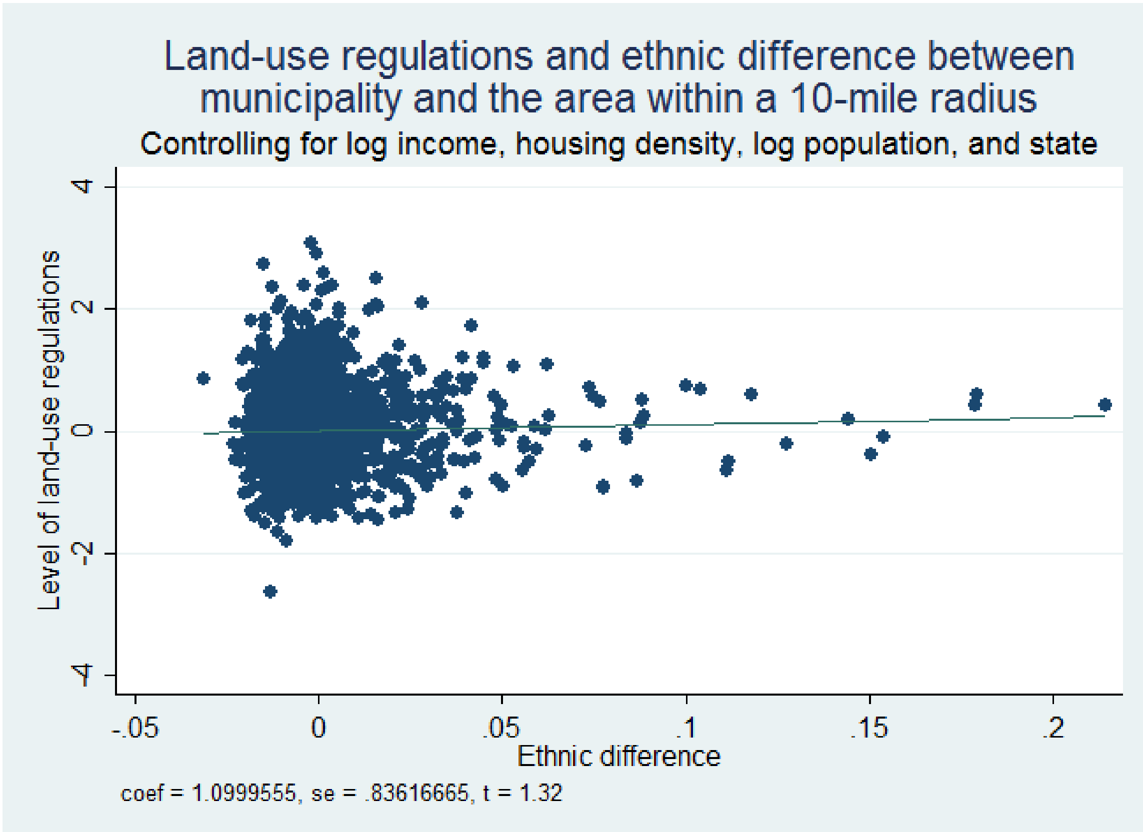

Figure 3: A plot of land-use regulations and the ethnic difference between each municipality and those within a 10-mile radius, with controls for log income, housing density, log population, and state

Table 18 shows the relationship between land-use regulations and the ethnic difference between each municipality and the communities within a 10-mile radius of it. The first three columns are without state fixed effects; the last three include state fixed effects. In no specification is the measure of ethnic difference between a municipality and those in the surrounding 10 miles statistically significant. As controls are added, the magnitude of the estimated coefficient falls from 2.271 to -1.964 in the specifications without fixed effects. The 2.271 coefficient would imply that a one-standard-deviation increase in the ethnic dissimilarity would correspond with a 0.03-standard-deviation increase in land-use regulations.

Table 18: Land-use regulations and ethnic delta among 10-mile buffer, with and without state fixed effects

VARIABLES	(1) Regulations	(2) Regulations	(3) Regulations	(4) Regulations	(5) Regulations	(6) Regulations
Ethnic (10-mile buffer)	2.271	-1.448	-1.964	2.148	-1.077	-0.702
	(1.640)	(1.526)	(1.525)	(1.633)	(1.729)	(1.617)
Log income		0.930***	0.904***		0.753***	0.729***
		(0.0574)	(0.0576)		(0.0930)	(0.0944)
Housing density			0.0356			-0.134***
			(0.0288)			(0.0454)
Log population			0.0579***			0.0682***
			(0.0176)			(0.0206)
State fixed effects	No	No	No	Yes	Yes	Yes
Observations	1,439	1,439	1,439	1,439	1,439	1,439
R-squared	0.001	0.156	0.165	0.002	0.132	0.149
Number of states				48	48	48

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Note: Constant not reported. With fixed effects, standard errors are clustered by state.

Both with and without state fixed effects, log income and log population are positively correlated with higher land-use regulations, and these estimates are statistically significant at the 1 percent level. The coefficient of 0.930 for income in column 2 implies a one-standard-deviation increase in income correlates with a 0.40-standard-deviation increase in land-use regulations. In column 6, the standardized coefficient is 0.31 for log income. In columns 3 and 6, the standardized betas for log population are 0.835 and 0.098, respectively. With state fixed effects, housing density is statistically significantly and negatively correlated with land-use regulations; the coefficient of -0.134 corresponds with a one-standard-deviation increase in housing density being correlated with 0.118-standard deviations lower land-use regulations.

Table 19 shows the same regressions using the ethnic difference between each municipality and those within a 20-mile radius. Without controls, the ethnic variable is positive and statistically significantly correlated with higher land-use regulations. Column 1 reports an estimated coefficient of 4.952, significant at the 1 percent level, without controls and without state fixed effects. This magnitude implies that a one-standard-deviation increase in the ethnic difference would correspond to a 0.084-standard deviation increase in land-use regulations. When controlling just for income, however, the estimate drops to nearly 0, and is not statistically significant. With state fixed effects, there is a similar pattern for both magnitude and statistical significance. As with the smaller buffer zone radius in Table 18, log income and log population are statistically significantly correlated with higher land-use regulations at the one percent level. With state fixed effects, housing density is negatively correlated with land-use regulations, with the estimate statistically significant at the one percent level. The coefficients for these controls are similar in magnitude to those in Table 18.

Table 19: Land-use regulations and ethnic delta among 20-mile buffer, with and without state fixed effects

VARIABLES	(1) Regulations	(2) Regulations	(3) Regulations	(4) Regulations	(5) Regulations	(6) Regulations
Ethnic (20-mile buffer)	4.952***	0.0449	-0.304	3.776*	-0.175	0.144
	(1.545)	(1.459)	(1.455)	(1.883)	(1.400)	(1.388)
Log income		0.921***	0.896***		0.748***	0.723***
		(0.0581)	(0.0583)		(0.0905)	(0.0921)
Housing density			0.0339			-0.136***
			(0.0288)			(0.0448)
Log population			0.0568***			0.0681***
			(0.0176)			(0.0206)
State fixed effects	No	No	No	Yes	Yes	Yes
Observations	1,439	1,439	1,439	1,439	1,439	1,439
R-squared	0.007	0.155	0.164	0.006	0.132	0.149
Number of states				48	48	48

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Constant not reported. With fixed effects, standard errors are clustered by state.

Table 20 shows the estimated impact of the difference, or delta, between the percentage white of the municipality and the percentage white in municipalities within 10 miles. Without controls, that difference appears to have a positive and statistically significant impact on land-use regulations, in specifications both with and without state fixed effects. The coefficient in column 1, 1.342, implies that a one-standard-deviation increase in the difference in percentage white would correspond to a 0.17-standard-deviation increase in land-use regulations. When controlling for log income alone, the sign reverses and the coefficient of -0.530 is statistically significant at the five percent level without state fixed effects. With state fixed effects, the estimate is statistically insignificant. Controlling for log income, housing density, and log population, the estimated impact of difference in percentage white is -0.390, which is statistically significant at the ten percent level in the specification without state fixed effects. With state fixed effects, the estimate is close to zero and is statistically insignificant. Again, log income and log population are positively correlated with higher land-use regulations, with these estimates significant at the one percent level both with and without fixed effects. With state fixed effects, housing density is also negatively and statistically significantly correlated with land-use regulations.

Table 20: Land-use regulations and white delta among 10-mile buffer, with and without state fixed effects

VARIABLES	(1) Regulations	(2) Regulations	(3) Regulations	(4) Regulations	(5) Regulations	(6) Regulations
White delta (10-mile buffer)	1.342***	-0.530**	-0.390*	1.262***	-0.170	0.0270
	(0.201)	(0.226)	(0.230)	(0.247)	(0.237)	(0.240)
Log income		1.011***	0.961***		0.778***	0.719***
		(0.0684)	(0.0696)		(0.105)	(0.112)
Housing density			0.0360			-0.136***
			(0.0288)			(0.0451)
Log population			0.0504***			0.0687***
			(0.0180)			(0.0207)
State fixed effects	No	No	No	Yes	Yes	Yes
Observations	1,439	1,439	1,439	1,439	1,439	1,439
R-squared	0.030	0.158	0.166	0.038	0.132	0.149
Number of states				48	48	48

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Note: Constant not reported. With fixed effects, standard errors are clustered by state.

Using the 20-mile radius for comparison of percentage white, Table 21 shows similar results to Table 20. Estimated magnitudes and statistical significance levels are very close to those in Table 20, with the exception of the estimate for the white delta in column 3. Column 3 shows that with controls and without state fixed effects, the estimate is close to zero and statistically insignificant.

Table 21: Land-use regulations and white delta among 20-mile buffer, with and without state fixed effects

VARIABLES	(1) Regulations	(2) Regulations	(3) Regulations	(4) Regulations	(5) Regulations	(6) Regulations
White delta (20-mile buffer)	1.573***	-0.197	0.0182	1.421***	0.110	0.345
	(0.197)	(0.225)	(0.232)	(0.268)	(0.257)	(0.250)
Log income		0.956***	0.890***		0.725***	0.657***
		(0.0690)	(0.0709)		(0.0985)	(0.106)
Housing density			0.0338			-0.138***
			(0.0288)			(0.0448)
Log population			0.0570***			0.0764***
			(0.0182)			(0.0201)
State fixed effects	No	No	No	Yes	Yes	Yes
Observations	1,439	1,439	1,439	1,439	1,439	1,439
R-squared	0.043	0.156	0.164	0.049	0.132	0.151
Number of states				48	48	48

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Note: Constant not reported. With fixed effects, standard errors are clustered by state.

Showing the effect of income inequality, Table 22 reports the relationship between the Gini coefficient among municipalities within 10 miles of each municipality tested. With and without state fixed effects, and with and without controls, these estimates are positive and statistically significant at the one percent level. The magnitudes range from 3.058 without controls and without state fixed effects, to 1.175 controlling for income and state. These correspond to standardized betas of 0.20 and 0.0841.

Table 22: Land-use regulations and Gini coefficient among 10-mile buffer, with and without state fixed effects

VARIABLES	(1) Regulations	(2) Regulations	(3) Regulations	(4) Regulations	(5) Regulations	(6) Regulations
Gini (10-mile buffer)	3.058***	1.587***	1.402***	2.426***	1.175***	1.241***
	(0.376)	(0.366)	(0.376)	(0.535)	(0.407)	(0.401)
Log income		0.851***	0.838***		0.693***	0.671***
		(0.0587)	(0.0587)		(0.0852)	(0.0865)
Housing density			0.0107			-0.148***
			(0.0293)			(0.0438)
Log population			0.0522***			0.0615***
			(0.0175)			(0.0200)
State fixed effects	No	No	No	Yes	Yes	Yes
Observations	1,439	1,439	1,439	1,439	1,439	1,439
R-squared	0.044	0.166	0.172	0.035	0.139	0.157
Number of states				48	48	48

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Constant not reported. With fixed effects, standard errors are clustered by state.

Table 23 reports the same regressions, but with the Gini among municipalities within the 20-mile radius of the municipality. Here, the estimated coefficients are slightly higher than in Table 22. All are still statistically significant at the one percent level. In Table 23, the 3.568 coefficient in column 1 implies a one-standard-deviation increase in the Gini coefficient would correspond to a 0.21-standard-deviation increase in land-use regulations. The lowest magnitude is 1.622 in column 5, and that corresponds to a standardized beta of 0.097.

Table 23: Land-use regulations and Gini coefficient among 20-mile buffer, with and without state fixed effects

VARIABLES	(1) Regulations	(2) Regulations	(3) Regulations	(4) Regulations	(5) Regulations	(6) Regulations
Gini (20-mile buffer)	3.568***	1.937***	1.762***	3.232***	1.622***	1.723***
	(0.431)	(0.418)	(0.428)	(0.506)	(0.374)	(0.411)
Log income		0.849***	0.834***		0.678***	0.655***
		(0.0585)	(0.0585)		(0.0879)	(0.0898)
Housing density			0.00921			-0.151***
			(0.0292)			(0.0457)
Log population			0.0533***			0.0614***
			(0.0175)			(0.0198)
State fixed effects	No	No	No	Yes	Yes	Yes
Observations	1,439	1,439	1,439	1,439	1,439	1,439
R-squared	0.046	0.168	0.174	0.044	0.142	0.160
Number of states				48	48	48

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Note: Constant not reported. With fixed effects, standard errors are clustered by state.

In Table 22 and Table 23, log income and log population are positively correlated with higher land-use regulations, statistically significant at the one percent level. These estimates are robust to state fixed effects and are similar in magnitude to those in Tables 18-21. In both Tables 22 and 23, with state fixed effects the coefficient for housing density is negative and statistically significant at the one percent level, with similar magnitudes to those in earlier tables.

Desegregation orders and zoning

As another method of testing for the exclusionary motive, I analyze the impact of school desegregation on land-use regulations. Court-ordered desegregation identifies areas that revealed a preference for segregation through school district policies. These orders also remove one mechanism of achieving segregation, increasing the incentive to use land-use regulations as a substitute policy to exclude. Table 24 shows the relationship between land-use regulations and court-ordered desegregation of schools, with and without state fixed effects. The results show that regulations tend to be lower in municipalities located in school districts that were ever under court orders to desegregate. The coefficients on the dummy variable for a court order are consistently negative. Without state fixed effects, these negative coefficients are statistically significant at the one percent level with and without controls. With state fixed effects, they remain negative but are statistically insignificant. The estimated effects of income, housing density, and population are similar to the previous regressions. With state fixed effects, the coefficient for the housing density coefficient is negative and statistically significant at the one percent level.

Table 24: Land-use regulations and school desegregation orders, with and without state fixed effects

VARIABLES	(1) Regulations	(2) Regulations	(3) Regulations	(4) Regulations
Court orders	-0.212***	-0.152***	-0.0702	-0.0619
	(0.0585)	(0.0554)	(0.0751)	(0.0517)
Log income		0.873***		0.722***
		(0.0574)		(0.0914)
Housing density		0.0257		-0.133***
		(0.0289)		(0.0454)
Log population		0.0673***		0.0706***
		(0.0180)		(0.0213)
State fixed effects	No	No	Yes	Yes
Observations	1,439	1,439	1,439	1,439
R-squared	0.009	0.168	0.001	0.150
Number of states			48	48

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Constant not reported. With fixed effects, standard errors are clustered by state. "Court orders" is 1 if any school district in the municipality was ever under a court desegregation order, 0 otherwise.

Geographic instrumental variable

To address the potential endogeneity of openness and presence of low-income or minority households, I implement a geographic instrumental variable (IV), using two-stage least squares. I use the demographics in the 10-mile to 20-mile ring around each municipality to instrument for the demographics in the 10-mile buffer around the municipality, and the demographics in the 20-mile to 40-mile ring to instrument for the demographics in the 20-mile buffer. In most specifications, these instruments are relevant, as indicated by high F-statistics for the first stage. These F-statistics are reported in the tables, and I also note in the text when F-statistics are below the conventional threshold of 10 for a strong instrument.

Table 25 and Table 26 show the results for ethnic difference using the instrumented 10-mile and 20-mile buffers, respectively. The magnitudes of the coefficients for ethnic difference are larger than in Tables 18 and 19. The highest uncontrolled estimate for the ethnic variable is 41.22 in Table 26, which would correspond with a standardized beta of 0.701. The highest estimate with controls is 25.16 in Table 26, implying a standardized beta of 0.428. These estimates are not statistically significant, however.

Table 25: Land-use regulations and instrumented ethnic delta among 10-mile buffer, with and without state fixed effects

VARIABLES	(1) Regulations	(2) Regulations	(3) Regulations	(4) Regulations	(5) Regulations	(6) Regulations
Ethnic (instrument for	11.72	7.140	5.472	11.34	7.562	7.446
10-mile buffer)	(8.936)	(8.456)	(8.534)	(10.88)	(9.844)	(9.907)
Log income		0.881***	0.865***		0.696***	0.676***
		(0.0747)	(0.0726)		(0.120)	(0.120)
Housing density			0.0289			-0.151***
			(0.0299)			(0.0482)
Log population			0.0530***			0.0678***
			(0.0186)			(0.0209)
State fixed effects	No	No	No	Yes	Yes	Yes
Observations	1,439	1,439	1,439	1,439	1,439	1,439
R-squared		0.137	0.151	-0.029	0.106	0.126
Number of states				48	48	48
First stage F-statistic	51.2331	49.348	47.916	20.439	19.540	18.767

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Note: Constant not reported. With fixed effects, standard errors are clustered by state.

Table 26: Land-use regulations and instrumented ethnic delta among 20-mile buffer, with and without state fixed effects

VARIABLES	(1) Regulations	(2) Regulations	(3) Regulations	(4) Regulations	(5) Regulations	(6) Regulations
Ethnic (instrument for 20-mile buffer)	16.85	18.47	18.04	41.22	30.81	25.16
	(14.21)	(13.30)	(13.16)	(30.29)	(31.81)	(27.97)
Log income		0.766***	0.746***		0.472*	0.501**
		(0.127)	(0.123)		(0.273)	(0.239)
Housing density			0.0273			-0.179**
			(0.0307)			(0.0753)
Log population			0.0455**			0.0694***
			(0.0202)			(0.0240)
State fixed effects	No	No	No	Yes	Yes	Yes
Observations	1,439	1,439	1,439	1,439	1,439	1,439
R-squared		0.061	0.071	-0.554	-0.233	-0.087
Number of states				48	48	48
First stage F-statistic	17.891	19.421	19.667	3.447	2.050	2.791

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Note: Constant not reported. With fixed effects, standard errors are clustered by state.

In Table 25, the coefficients for log income range from 0.676 to 0.881, and those for total population go from 0.053 to 0.068. These are similar in magnitude to those estimated in regressions above, and remain statistically significant at the 1 percent level. In the 10-mile specification with fixed effects, the coefficient for housing density is -0.151, which is statistically significant at the one percent level, and also of magnitude similar to those in previous tables.

In Table 26, the coefficients for income in the fixed effects specifications are slightly smaller than in previous estimates and are statistically significant at the ten and five percent levels in columns 5 and 6, respectively. Estimated coefficients for log population are similar in magnitude to those above, and are statistically significant at the five or one percent level. With fixed effects, the coefficient for housing density is -0.179, statistically significant at the five percent level. Columns 4 through 6 of Table 26 should be interpreted with caution, however, as the F-statistics for the first stage of the two stage least squares are below 4 for these specifications, meaning the instrument is weak.

Table 27 and Table 28 show the IV results with the white delta as the measure of incentive to exclude minority households. As in Tables 20 and 21, without controlling for income the white delta is statistically significant and positive. With the instrumented 10-mile radius comparison group, the estimated coefficient is 1.670 without state fixed effects, and 1.415 with state fixed effects; both of these estimates are statistically significant at the one percent level. Upon controlling for income, however, the estimates fall and become statistically insignificant. Table 28 shows similar estimates occur when using the instrumented 20-mile radius.

Table 27: Land-use regulations and instrumented white delta among 10-mile buffer, with and without state fixed effects

VARIABLES	(1) Regulations	(2) Regulations	(3) Regulations	(4) Regulations	(5) Regulations	(6) Regulations
White delta	1.670***	0.0203	0.335	1.415***	0.121	0.433
(instrument for 10-mile buffer)	(0.367)	(0.469)	(0.493)	(0.381)	(0.516)	(0.574)
Log income		0.918***	0.835***		0.724***	0.641***
		(0.0976)	(0.103)		(0.159)	(0.179)
Housing density			0.0319			-0.140***
			(0.0289)			(0.0440)
Log population			0.0620***			0.0767***
			(0.0193)			(0.0234)
State fixed effects	No	No	No	Yes	Yes	Yes
Observations	1,439	1,439	1,439	1,439	1,439	1,439
R-squared	0.028	0.155	0.160	0.038	0.131	0.147
Number of states				48	48	48
First stage F-statistic	618.337	435.838	401.811	593.854	401.150	343.635

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Note: Constant not reported. With fixed effects, standard errors are clustered by state.

Table 28: Land-use regulations and instrumented white delta among 20-mile buffer, with and without state fixed effects

VARIABLES	(1) Regulations	(2) Regulations	(3) Regulations	(4) Regulations	(5) Regulations	(6) Regulations
White delta	1.464***	0.188	0.576	1.500***	0.350	0.763
(instrument for 20-mile buffer)	(0.393)	(0.480)	(0.518)	(0.456)	(0.510)	(0.535)
Log income		0.889***	0.789***		0.681***	0.576***
		(0.101)	(0.110)		(0.128)	(0.141)
Housing density			0.0314			-0.140***
			(0.0289)			(0.0437)
Log population			0.0686***			0.0864***
			(0.0206)			(0.0219)
State fixed effects	No	No	No	Yes	Yes	Yes
Observations	1,439	1,439	1,439	1,439	1,439	1,439
R-squared	0.042	0.154	0.161	0.049	0.131	0.148
Number of states				48	48	48
First stage F-statistic	481.201	402.380	361.714	571.899	404.808	331.675

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Note: Constant not reported. With fixed effects, standard errors are clustered by state.

In both Tables 27 and 28, the estimated coefficients for log income are statistically significantly positive at the one percent level, and the estimates range from 0.576 to 0.918. Log population is statistically significant at the one percent level, with coefficients ranging from 0.062 to 0.0864. With fixed effects, the coefficient for housing density is -0.140 in both tables, significant at the one percent level.

Finally, Table 29 and Table 30 show the IV results for the Gini coefficient. Table 29 uses the Gini among municipalities between 10 and 20 miles from the municipality to instrument for the Gini coefficient within the 10-mile radius of the municipality. In all specifications, this instrumented Gini is statistically significant. Without state fixed effects, the estimated coefficient is 3.299 with controls, statistically significant at the ten percent level. With state fixed effects and controls, the estimate is 5.882, statistically significant at the ten percent level. With state fixed effects, log income and log population are statistically significant at the one and ten percent levels respectively and positively correlated with higher land-use regulations, and housing density is statistically significant at the one percent level and negatively correlated with regulations.

Table 29: Land-use regulations and instrumented Gini coefficient among 10-mile buffer, with and without state fixed effects

VARIABLES	(1) Regulations	(2) Regulations	(3) Regulations	(4) Regulations	(5) Regulations	(6) Regulations
Gini (instrument for	3.685**	3.427**	3.299*	6.492**	5.011*	5.882*
10-mile buffer)	(1.532)	(1.464)	(1.702)	(2.762)	(2.758)	(3.072)
Log income		0.769***	0.763***		0.519***	0.470***
		(0.0865)	(0.0884)		(0.162)	(0.170)
Housing density			-0.0206			-0.197***
			(0.0403)			(0.0537)
Log population			0.0462**			0.0366*
			(0.0184)			(0.0198)
State fixed effects	No	No	No	Yes	Yes	Yes
Observations	1,439	1,439	1,439	1,439	1,439	1,439
R-squared	0.042	0.151	0.157	-0.063	0.059	0.045
Number of states				48	48	48
First stage F-statistic	92.138	97.1345	74.770	42.160	34.800	27.732

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Constant not reported. With fixed effects, standard errors are clustered by state.

Table 30: Land-use regulations and instrumented Gini coefficient among 20-mile buffer, with and without state fixed effects

VARIABLES	(1) Regulations	(2) Regulations	(3) Regulations	(4) Regulations	(5) Regulations	(6) Regulations
Gini (instrument for	12.86***	11.14***	13.87***	2.480	-0.860	0.190
20-mile buffer)	(3.386)	(3.643)	(5.157)	(8.788)	(8.686)	(9.947)
Log income		0.502***	0.425**		0.782**	0.717*
		(0.152)	(0.188)		(0.364)	(0.396)
Housing density			-0.160**			-0.137
			(0.0804)			(0.0940)
Log population			0.0306			0.0674
			(0.0238)			(0.0493)
State fixed effects	No	No	No	Yes	Yes	Yes
Observations	1,439	1,439	1,439	1,439	1,439	1,439
R-squared				0.042	0.118	0.151
Number of states				48	48	48
First stage F-statistic	31.408	25.658	15.506	5.812	4.341	3.720

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Constant not reported. With fixed effects, standard errors are clustered by state.

Table 30 shows the estimates for the Gini among the 20-mile to 40-mile ring instrumenting for the 20-mile radius around the municipality. Without state fixed effects, the estimated coefficients are higher than in Tables 5, 6, and 12; with controls for income, density, and population, the estimate is 13.87, significant at the one percent level. This corresponds to a standardized beta of 0.830. With state fixed effects, the instrumented Gini coefficient is statistically insignificant and small in magnitude. However, the F-statistics for these columns 4 through 6 are below 6, indicating a weak instrument in these specifications.

Conclusion

While cross-sectional data limit causal claims, the data do shed light on the exclusionary motive. The results in this paper are consistent with wealthy communities seeking to exclude low-income households. In most specifications, a one-standard-deviation increase in the Gini coefficient would correspond with approximately 0.10- to 0.20-standard deviations higher land-use regulations.

Regarding exclusion of minorities, however, the data fail to show evidence that greater ethnic dissimilarity between a municipality and its surrounding area prompts adoption of higher land-use regulations.

Income is a robust predictor of land-use regulations, and estimated coefficients are large in magnitude, with a one-standard-deviation increase in log income corresponding with land-use regulations between 0.18 to 0.43 standard deviations higher. This relationship is consistent with non-exclusionary motivations for land-use regulations, such as public goods provision and preservation of lower density.

Municipalities with higher populations also appear to have higher land-use regulations. The robustly positive estimates for the impact of total population on land-use regulations goes against the hypothesis in much of the exclusionary zoning literature that smaller municipalities are more prone to adopt more extensive land-use regulations. The coefficients indicate that a one-standard-deviation increase in log population corresponds with regulations that are stricter by 0.05- to 0.12-standard-deviations.

The estimated correlation between housing density and land-use regulations is also consistent across specifications. The standardized betas for housing density range from -0.12 to -0.17. Housing density could reasonably be a cause of land-use regulations, as in areas motivated by the desire to preserve low density (see Ihlanfeldt 2004), or an effect, as many land-use regulations limit density directly or indirectly.

Modeling and Measuring Gains from Labor Market Desegregation in Northern Ireland

Introduction

The conflict between Protestants and Catholics in Northern Ireland has been centuries in the making, and the spatial and socioeconomic segregation between the two groups endures despite political efforts to establish unity (see Todd and Ruane 2011). In 1998, the Good Friday Agreement largely ended sectarian violence, and established political power-sharing and peace agreements between Catholic republican and Protestant loyalist factions (Shirlow and Coulter 2014). In the economic realm, the 1989 Fair Employment Act strengthened earlier legislation and set in place stronger monitoring and enforcement to improve Catholic representation in the labor market (Gallagher 1992). Workforce equality has improved, and labor market segregation has diminished but not disappeared (ECNI 2015).

Urban economic theories of agglomeration predict that productivity is higher in larger cities, due to better matching between employees and firms (see, for example, Andersson et al 2007). Applying this theory to Northern Ireland, the region is predicted to suffer from lower productivity and lower wages due to segregation. In the extreme case of full segregation, there would be two independent labor markets in which Catholic firms could only draw from the pool of Catholic workers and Protestant firms could only draw from the pool of Protestant workers.

The source of workplace segregation may not be discrimination on the part of employers or employers' desire to prevent workplace strife. Homophily may be at play (see Currarini et al 2009), with locational and social choices leading to segregated networks, naturally leading to segregated workforces. Regardless of the source, however, the observed segregation in the labor market leads to predicted consequences for agglomeration.

Segregation can affect productive efficiency through channels other than matching. Spatial segregation may lead to longer commutes, housing inefficiencies, and duplication of public services and stores to serve each segment of the population (Shirlow and Murtagh 2006; Deloitte 2007). Further, risk of workplace violence may reduce foreign direct investment (Deloitte 2007).

This paper focuses on the matching implications of segregation. I present a model of the impact of labor force segregation on the agglomeration benefits of matching. Then, I provide a thorough estimate of the degree of employment desegregation in Northern Ireland since 2001. Finally, I use these estimates to calibrate the agglomeration model to estimate the productivity gains from that desegregation.

Previous literature

Existing models and theories of a dual labor market, or labor market segmentation, focus on tiers, with an "underclass" at a disadvantage to access the normal labor market with its returns to education and skill (see, for example, Dolado et al 2009 and Hudson 2007). Hsieh et al (2016) analyze the increased labor market participation among blacks and women, particularly in skilled occupations, and find the occupational convergence explains one quarter of the increase in output per person since 1960. Cavalcanti and Tavares (2016) calibrate the effects of the gender wage gap on output.

For much of the 20^{th} century, Northern Ireland resembled this kind of dual labor market. Skilled job participation, labor market success, and public-sector employment were higher among the Protestant population (Borooah 1997, Gallagher 1992, Todd and Ruane 2011). However, by 2014, Catholic economic activity rates, unemployment rates, and representation in all occupational classifications had converged toward Protestant ones, and Catholics had higher levels of skill qualifications (OFMDFM 2014). By 2013, 41 percent of firms had majority Catholic workforces (ECNI 2013). Thus, the current labor market in Northern Ireland less resembles a primary and secondary labor market, and operates more as two semi-isolated labor markets operating in the same physical space.

The Equality Commission of Northern Ireland (ECNI) is tasked with upholding the requirements of the Fair Employment Act, and publishes an annual monitoring report. The report contains the number of employees by religion at all companies with greater than 25 employees, as well as aggregate findings based on these data. The Commission reports that segregation has been decreasing, with workforce participation in both the private and public sectors approaching a mix reflecting the economically active population (ECNI 2015). Some researchers have questioned whether this adequately captures labor market segregation or inequality in Northern Ireland. O'Leary and Li (2006) note while unemployment has decreased among Catholics, Catholic males lag in terms of earnings. Shirlow (2006) argues that the monitoring data understates worksite segregation by reporting company-wide numbers; a firm with a largely Catholic worksite and a largely Protestant one would appear unsegregated in the data. However, to my knowledge, no researchers have provided a comprehensive measure of the change in workforce segregation, other than the aggregate indicators reported in the Equality Commission reports. In this paper, I establish a consistent measure of workforce segregation that utilizes the microdata available in the ECNI reports since 2001, the first year for which this data is readily attainable.

Model

Foundational model

This paper adapts a model by Duranton and Puga (2004). In this model, a firm h's production is

$$y(h) = \beta l(h) - \alpha \tag{1}$$

where β is the marginal product of labor, $l(h)$ is the firm's workforce, and α denotes the firm's fixed cost. Thus, for the total economy,

$$Y = \beta L - \alpha n \tag{2}$$

where Y captures output net of the fixed costs. Workers' skills and firms' skill requirements are modeled as evenly distributed around a unit circle. The skill distance, or mismatch, between a worker and a firm is denoted z. The cost of the mismatch is μz. The labor force L is exogenous. The number of firms, n, is endogenous, and firms are located 1/n distance apart around the unit circle.

The equilibrium conditions derive from firms' competition for workers, yielding

$$w(h) - \mu z = w - \mu(\frac{1}{n} - z) \tag{3}$$

where firm h offers wage $w(h)$, a worker is located at skill distance z from firm h, and the competing firm offers wage w and is $(\frac{1}{n} - z)$ from the worker.

In the symmetric zero-profit equilibrium, $w(h)=w$, and that wage[12] is:

$$w = \beta - \frac{\mu}{n} \tag{4}$$

The equilibrium number of firms is:

$$n = \sqrt{\frac{\mu L}{\alpha}} \tag{5}$$

Net output is a function of the size of the labor force L, the marginal product of labor β, the firm's fixed cost α, and the cost of mismatch between employee and firm, μ:

$$Y = L(\beta - \sqrt{\frac{\mu\alpha}{L}}) \tag{6}$$

[12] Since $\pi(h) = \beta l(h) - wl(h) - \alpha$, in the equilibrium with zero profits, $w = \beta l(h) - \alpha = \frac{Y}{L}$. That is, wage is equal to net output divided by number of workers.

Applying the model to full segregation

Using this model, one can compute the net output loss from a fully segregated labor market, in which the labor force is evenly divided into two types, and these types will only work with others of their type. Let the two types be P and C, and $L_P = L_C = .5L$. Let Y_S denote the net output under full segregation, and Y_U denote the net output with zero segregation.

The net output in the unsegregated market will be the same as that in the original model:

$$Y_U = L(\beta - \sqrt{\frac{\mu\alpha}{L}}) \tag{7}$$

The net output in the fully segregated market will be the sum of the two identical, smaller labor markets:

$$Y_s = Y_p + Y_c = 2Y_p = 2\left(\frac{L}{2}\right)\left(\beta - \sqrt{\frac{\mu\alpha}{\frac{L}{2}}}\right) = L(\beta - \sqrt{\frac{\mu\alpha}{\frac{L}{2}}}) \tag{8}$$

Thus, the difference between the unified and segregated net output will be:

$$Y_u - Y_s = \left(\sqrt{2} - 1\right) * \sqrt{L\mu\alpha} = 0.414 * \sqrt{L\mu\alpha} > 0 \tag{9}$$

Furthermore, the equilibrium number of firms will be higher than in the unsegregated case. In the model, as the labor force increases, the number of firms rises less than proportionately. As proportionately fewer fixed costs are required, output per worker rises. In the segregated case of two smaller workforces, there are inefficiently many firms incurring inefficiently many fixed costs.

$$n_s = 2 * \sqrt{\frac{\mu L}{2\alpha}} > \sqrt{\frac{\mu L}{\alpha}} = n_u \tag{10}$$

The wages in the segregated case are lower than in the unified labor market:

$$w_s = \beta - \frac{\mu}{n_s} < \beta - \frac{\mu}{n_u} = w_u \tag{11}$$

Applying the model to partial segregation

To incorporate partial segregation into the above model, I introduce a segregation factor φ. Higher values of φ indicate greater levels of segregation, and $0 \leq \varphi \leq 1$.

This segregation factor affects the cost of mismatch between workers and firms. In this model, segregation is a cost borne by the worker. A worker is more willing to endure an extra skill distance in order to work for a firm of the same type, or endures a personal cost of working for a firm of the opposite type that is a closer skill match. Specifically, the worker is indifferent between a firm of the same type *h* and a competing firm of the opposite type when:

$$w(h) - (1 + \varphi)\mu z = w - (1 + \varphi)\mu(\frac{1}{n} - z) \tag{12}$$

This leads to the following estimates for net output, number of firms, and wages:

$$Y = L\left(\beta - \sqrt{\frac{(1+\varphi)\mu\alpha}{L}}\right) \tag{13}$$

$$n = \sqrt{\frac{(1+\varphi)\mu L}{\alpha}} \tag{14}$$

$$w = \beta - \frac{(1+\varphi)\mu}{n} \tag{15}$$

Substituting equation 14 into 15, the wage equation in terms of exogenous variables is:

$$w = \beta - \left(\sqrt{\frac{(1+\varphi)\mu\alpha}{L}}\right) \tag{16}$$

It is worth noting that this wage equation is equal to Y/L, which can be obtained from equation (13).

Unsurprisingly, when φ=1, the results reproduce those of the full segregation results above, and when φ=0, results are identical with those of the basic, unified model results.

This model generates the following predictions:

1) Effect of segregation on net output: $\frac{\partial Y}{\partial \varphi} = -\frac{\sqrt{L\mu\alpha}}{2\sqrt{(1+\varphi)}} < 0$
2) Effect of segregation on number of firms: $\frac{\partial n}{\partial \varphi} = \frac{\sqrt{\mu L}}{2\sqrt{(1+\varphi)\alpha}} > 0$
3) Effect of segregation on wages: $\frac{\partial w}{\partial \varphi} = -\frac{\mu}{2\sqrt{\frac{\mu L(1+\varphi)}{\alpha}}} < 0$

Net output is decreasing in segregation. This follows from the intuition of the agglomeration benefits from matching; segregation leads to worse matching and thus lower productivity. The number of firms is increasing in segregation. This follows from segregation increasing the cost to workers of matching with a firm of opposite type and increasing the skill mismatch of which workers are willing to bear the cost in order to work for a firm of the same type; more firms enter as the higher cost to workers allows firms to reduce wages further below workers' marginal product. Because of the fixed cost to firms, segregation results in inefficiently many firms. Given the exogenous labor force, the number of workers per firm falls as the number of firms rises, meaning that segregation also yields smaller firms than in the unified case. Finally, as mentioned above, wages are falling with segregation, as the greater φ allows firms to pay further below workers' marginal product. Thus, in a Beckerian result, workers' taste-based discrimination is costly to the workers (Becker 1957).

Data

Economic variables

To calibrate the model, I use data from various United Kingdom and Northern Ireland statistical agencies. These are shown in Table 31.

Table 31: Data sources and calculations

Exogenous	**Description**	**Data source**	**Calculation**
α	Fixed cost	Author's calculation	$(1-\epsilon_L)(GVA)$
β	Marginal product of labor	Author's calculation	$\left(\frac{GVA}{L}\right)$
μ	Cost of mismatch	Author's calculation	$\frac{\alpha n^2}{(1+\phi)L}$ (see eq. 13)
φ	Segregation	ECNI (2001-2015)	Dissimilarity index (see eq. 17)
L	Labor force[13]	ELMS (2017)	
Endogenous			
w	Wage	Author's calculation	$\beta-\sqrt{\frac{\alpha\mu}{L}}$
Y	Net output	Author's calculation	$(\epsilon_L)(GVA)$
n	Number of firms	BEIS (2016)	
Other			
GVA	Gross value added	ONS (2016)	Deflated using CPI
CPI	Consumer price index	ONS (2017)	
ϵ_L	Labor share of income	NISRA (2015), Gollin (2002)	0.57 to 0.9

In 2012, the labor share of gross value added in Northern Ireland was 57 percent (NISRA 2015). Because this labor share is calculated using total compensation, and wages are negatively affected by segregation, 0.57 represents the lower bound of ϵ_L. Gollin (2002) estimates that the labor share of most countries lies between 65 percent and 80 percent. I allow the estimates for ϵ_L, the labor share of income, to vary between 0.57 and 0.90, and report results using these values.

[13] Because there is no unemployment in this model, I use the total number of employed workers rather than the number of economically active adults.

Following equation (2), βL constitutes productivity, of which Y captures net output, or labor's share of output, and α can be thought of as capturing capital's share. This interpretation differs slightly from the original model's description of α as the fixed cost to the firm, but it follows mathematically from the model that α is the residual of labor's share. Allowing ϵ_L to vary mitigates inaccuracies in estimates of Y and α.

Segregation

Data on workplace segregation comes from the Equality Commission of Northern Ireland, which annually collects and publishes the number of Catholic, Protestant, and "Non-determined"[14] employees at all firms with greater than 25 employees operating within Northern Ireland. All firms above the specified size are legally required to report these figures (ECNI). For firms with fewer than 10 Catholic or Protestant employees, the data are truncated, with only the total number of employees reported, along with an identifier for "Less than 10 Roman Catholic employees," "Less than 10 Protestant employees," or "Less than 10 Protestant employees and less than 10 Roman Catholic employees,"[15] as the cause of truncation for that firm.

To impute numbers for this censored data, I calculate the average number of non-determined employees within other firms of the same size. I assume that the censored firm has the average number of non-determined employees in its firm size, that the censored number of employees is 9, and that the rest of the employees are in the

[14] All employees are included in one of these categories. Non-determined covers any employee not identifying with either the Catholic or Protestant community.

[15] This truncation only occurs for Catholic and Protestant communities; data is not censored for firms with fewer than 10 Non-determined employees.

remaining category. For example, if a firm with 30 employees is censored as "few Protestant," and the average number of Non-determined employees for uncensored firms with 30 employees is 1, the imputed numbers for this firm are: 20 Catholic, 9 Protestant, and 1 Non-determined. Since it is highly unlikely that all firms with fewer than 10 employees from a particular community will have 9 representatives of that group, this will lead to slightly underestimated levels of segregation. In 2001, 927 out of 2,329 reporting firms were truncated. In 2013, it was 706 out of 2,231 firms.

To measure workplace segregation, I use the standard D dissimilarity index:

$$D = \frac{1}{2}\sum_{i=1}^{n}\left(\left|\frac{l_{RC,i}}{L_{RC}} - \frac{l_{P,i}}{L_P}\right|\right) \tag{17}$$

Where $l_{RC,i}$ ($l_{P,i}$) is the number of Catholics (Protestants) at an individual firm and L_{RC} (L_P) is the proportion of working age Catholics (Protestants) times the total number of workers in the sample. L_{RC} (L_P) is computed in this manner for two reasons. First, due to the truncation of the data, it is not possible to compute the total number of Catholics or Protestants employed in the entire sample. Second, using the proportion of working age individuals captures segregation stemming from differences in workforce participation, as well as workplace segregation among employed individuals. In the absence of segregation, D=0. With full segregation, D=1.

This measure has its critics. Echenique and Fryer (2005) point out that this index of dissimilarity is sensitive to artificial boundaries of the units i, which are often census tracts or other arbitrary boundaries. Since firms are a meaningful unit, however, this weakness is not a reason to reject this index for this paper. Second, they note that the index measures segregation at the level of *i*, rather than the level of the individual. This is a problem for studies of the effect of segregation on individual outcomes; however, this paper focuses on aggregate outcomes, so the index remains appropriate for this study. In fact, this dissimilarity index best captures the notion of workplace segregation by measuring the difference between the makeup of each workplace and the makeup of the working age population from which the workplaces draw.

Aslund and Nordstrom (2007) develop a more sophisticated measure of segregation that incorporates sorting along education and skills. Using extensive individual-level data, they present a non-parametric measure of workplace segregation between native and foreign-born populations in Sweden. Such worker-level data is not available for Northern Ireland. Furthermore, the evenness of Catholic and Protestant representation among occupational classifications and skill levels indicates that human capital differences are not a large factor in the Northern Ireland setting.[16]

Figure 4 shows the change in workplace segregation for all reporting firms and for large firms with greater than 250 employees from 2001 to 2013. Segregation levels have fallen, but by a modest four percentage points. It is possible that workforce segregation experienced a dramatic drop prior to 2001. However, both survey responses (NISRA

[16] In 2014, for example, the Catholic/Protestant split for all standard occupational classifications were within 6 percentage points from the overall split of 48/52 for the whole sample (OFMDFM 2014). The skill qualification gaps were similarly small (Equality and Good Relations Directorate 2017).

2016) and sociological analysis (Shirlow and Coulter 2014) indicate that sectarian relations are improving slowly following the Good Friday Agreement of 1998.

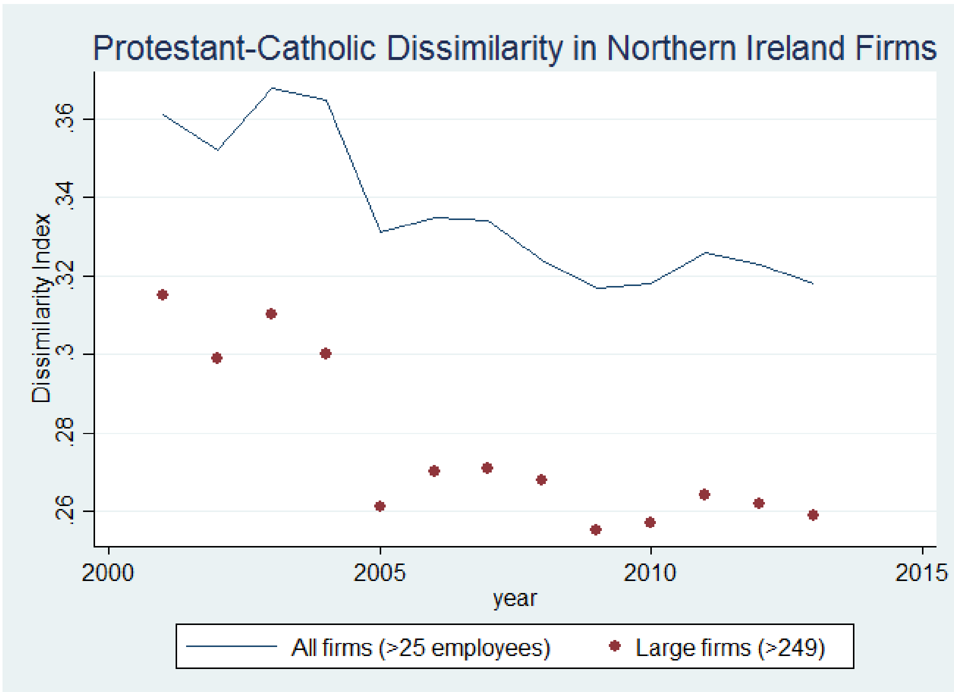

Figure 4: Dissimilarity index for Catholics and Protestants in Northern Ireland workplaces

Censored firms

As noted above, approximately one third of firms have censored data, meaning they have fewer than 10 Protestant or Catholic workers. Especially among firms with smaller workforces, a random draw of employees from the economically active would result in some positive number of firms having employee breakdowns that would be censored. I use a binomial distribution to estimate the probability that the number of censored firms within each firm size decile could have arisen from firms randomly drawing employees from the working age population. For all deciles in all years the number of censored firms is statistically extremely unlikely ($p<0.00001$).

Calibration

Having established estimates for the relevant variables, it is possible to calculate the partial derivatives above and estimate the effects of segregation on output, number of firms, and wages. The years for which all variables are available are 2010 through 2013; I report mean estimates among these years. In Table 32, the partial derivatives, elasticities, and semi-elasticities show the predicted magnitude of segregation's impact on output, number of firms, and wages. The semi-elasticities, denoted by *s*, show the percent change in the variable of interest given a one-percentage-point change in φ. For example, in column (1), the semi-elasticity between number of firms and segregation is 0.378, indicating that a one-percentage-point increase in segregation would lead to a 0.378 percent increase in the number of firms.

Table 32: Estimated relationships between segregation and output, number of firms, and wages

	(1)	(2)	(3)	(4)
	$\epsilon_L = 0.57$	$\epsilon_L = 0.70$	$\epsilon_L = 0.80$	$\epsilon_L = 0.90$
$\frac{\partial Y}{\partial \varphi}$	-£3.86 billion	-£2.69 billion	-£1.79 billion	-£0.897 billion
$\frac{\partial n}{\partial \varphi}$	44,170	44,170	44,170	44,170
$\frac{\partial w}{\partial \varphi}$	-£4,920	-£3,433	-£2,288	-£1,144
$\epsilon_{Y\varphi}$	-0.092	-0.052	-0.030	-0.014
$\epsilon_{n\varphi}$	0.122	0.122	0.122	0.122
$\epsilon_{w\varphi}$	-0.092	-0.052	-0.030	-0.014
$s_{y\phi}$	-0.285	-0.162	-0.095	-0.042
$s_{n\phi}$	0.378	0.378	0.378	0.378
$s_{w\phi}$	-0.285	-0.162	-0.095	-0.042

The estimate in the first row implies that when net output is 57 percent of gross-value-added, moving from a fully segregated to a fully desegregated labor market would increase annual output by approximately £3.86 billion. The estimated difference in annual wages between the fully segregated and unsegregated labor market is £4,920. These are rough estimates, since the partial derivative is calculated at current values of the variables.

The elasticity and semi-elasticity offer more targeted estimates. Since wage and net output per worker are mathematically equivalent in this model, the elasticities are also equal in this calibration. When labor's share of output is higher, the sensitivity of net output and wages to segregation falls. A one percentage-point increase in segregation would lead to an estimated drop in net output and wages by between 0.04 percent and 0.29 percent, depending on the assumed labor share.

Regardless of the value for labor's share of output, the number of firms under the fully segregated market is estimated at 44,170 higher than in the unsegregated case. From current levels, a one percentage-point increase in segregation would be predicted to increase the number of firms by approximately 0.4 percent.

As Duranton and Puga state in the original model (2004), the cost of mismatch can be thought of as training costs. The average mismatch cost is $\frac{\mu}{4n}$, which calibrates to £2,460 when labor's share of output is 0.57 and £572 when ϵ_L is 0.90.

Conclusion

This paper develops a model of a partially bifurcated labor market, and estimates the relationship between changes in segregation and agglomeration benefits from improved matching. I then calibrate this model using data from Northern Ireland, where there is substantial labor market segregation between Catholics and Protestants.

Using firm-level data, I estimate workforce segregation between Catholics and Protestants in Northern Ireland. To my knowledge, this is the first thorough, micro-level estimate of the extent of this phenomenon in Northern Ireland. My data show that over the period from 2001 to 2013, workplace segregation has modestly declined by approximately four percentage points.

The calibrated model estimates that each percentage point decrease in segregation would increase net output by 0.04 percent to 0.29 percent – approximately £5.3 million to £37.9 million. For each percentage-point decrease in segregation, wages would increase by £7 to £51 per year, and the number of firms would fall by 443, improving economies of scale.

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