The Spatial Externalities of Integration: A Theory of Interdependence and Public Goods in Cities
Supplemental Materials

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Contents

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A1 Research Ethics and Transparency

I confirm that the human subjects research pursued complies with the *Principles and Guidance for Human Subjects Research*. This study is also pre-registered with EGAP/OSF. I include an anonymized copy of the original preregistration plan for consideration as an additional Supplementary Material. This paper draws on both survey and qualitative data collected from engaging directly with human participants. For the qualitative interviews and focus groups, participants were recruited via email or phone outreach, and preliminary consent was solicited during recruitment before meeting in-person. A subset of participants were also recruited in-person after an encounter at, for example, a neighborhood association or other civic association meeting. For the survey data collection, participants were recruited in-person by survey enumerators who approached their residence, and consent was documented as a survey response to the consent question.

For both forms of data collection, participants were read the terms of consent in their native language (i.e., Portuguese), and the research team ensured they fully understood all the terms, benefits, and risks of participating in the interview. The oral script detailing consent includes 1) a description of the interview procedures, 2) a statement detailing the the activities involved in the research, 3) a statement that participation is voluntary, and d) the principal investigator's name and contact information. No compensation was paid to the human participants. The participant pool is diverse in age, gender, race, and other demographic attributes. Last, the research does not differentially benefit or harm particular groups.

In accordance with Brazil law and customs for research ethics that uses deception or an experimental design, the researcher sought local ethics review in Brazil under the *Comitê de Ética em Pesquisas Envolvendo Seres Humanos* at a local research institution in addition to IRB review at their home institution. In regard to confidentiality, during the process of soliciting informed consent, interview and focus group participants were informed that they may be quoted directly from their interview, and that the qualitative information gathered is identifiable, but were given the option to conduct the interview anonymously if preferred. Most participants chose to be identified. The identities of interviewed participants are, therefore, disclosed in the manuscript. Again, because the interview questions did not solicit sensitive information nor could they pose physical, reputational, or employability risks to participants, the IRB review allowed interview data collection that is identifiable. For the household survey, participants were informed that the research team would collect household address identifiers should they consent to the study. Stored on a password-protected device, household address identifiers were collected solely for the purpose of linking survey responses to census data for calculating measures of segregation. However, the results are only reported at the aggregate levels without revealing each respondent's identity or location. To protect respondent confidentiality, data and information that can potentially reveal respondents' identity or location (i.e., identifiers) were excluded from the replication data and files.

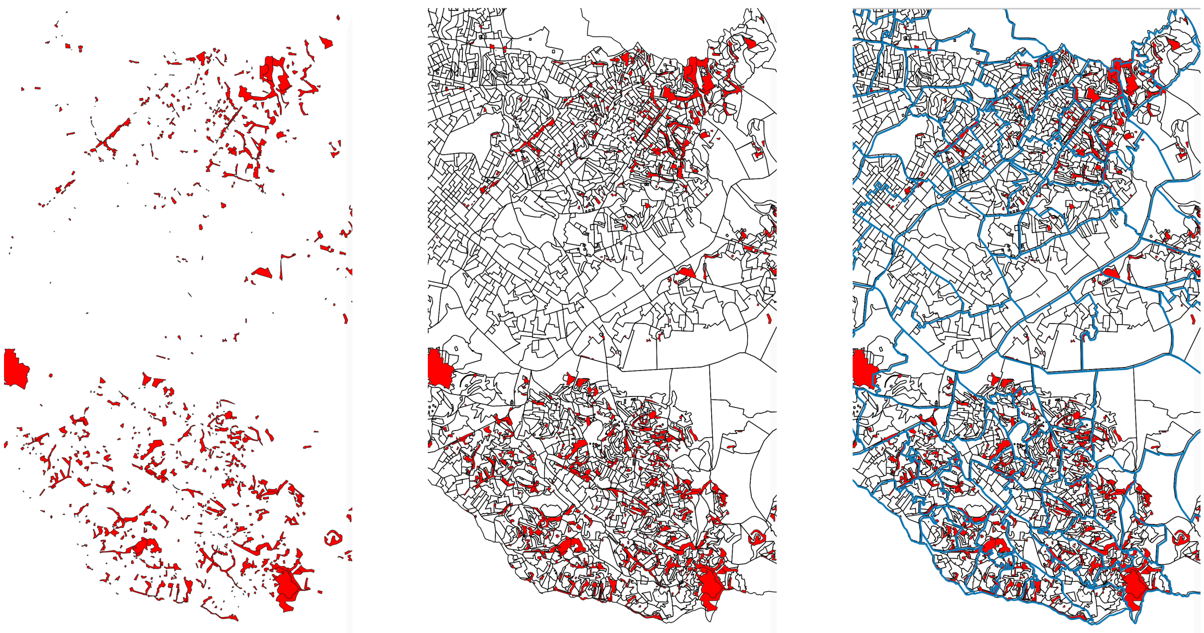
A2 Unit of Analysis

The hypothetical molds with varying layouts of segregation in Figure 1 applies to both the city as well as to neighborhoods as the unit of analysis. I assume that voters living in the most segregated (City A) and integrated (City C) cities also live segregated or integrated from the poor at the neighborhood-level. I also assume that voters form preferences for public goods based largely on the segregation of their neighborhood. However, *provision* occurs at the administrative unit of the municipality (i.e., the city) based on the bird's eye view of the mayor. Since the focus of the paper is on explaining preferences, my main analysis is a large-N one of segregation across neighborhoods within São Paulo to test the micro-level mechanisms that drive preferences for public goods. In addition, I also estimate the effect of city-level segregation on public goods provision in Section 4.3 to confirm the broader implications of the theory. The cross-city analyses offer the analytical advantage of testing the generalizability of the observable implications, whereas the cross-neighborhood research design tests the effect of segregation on voter preferences, in particular, and adjudicates between competing mechanisms that could explain these preferences.

Although my qualitative evidence from focus groups draws on a cross-city comparison of Brasília and Belo Horizonte, the assumption is that, as discussed, voters in these two cities are also correspondingly segregated and integrated from the poor at the neighborhood-level. Therefore, at such extremes of segregation, the comparison of the two cities is also a comparison of their neighborhoods. Alternatively, City B (São Paulo) exhibits the full range of variation in segregation across neighborhoods, variation I exploit in the empirical research design. In City B, voters tend to rely even more on their neighborhood segregation experience as a heuristic, since city-level segregation is less informative. Empirical studies in the political geography literature do note that the difference between the neighborhood and the city is subject to the modifiable areal unit problem (MAUP), although there is no definitive way to address it. Therefore, since city- and neighborhood-level segregation are empirically highly correlated in my sample, I rely on these stylized assumptions to simplify the theory and analysis.

My conception of the neighborhood is equivalent to the census area in the Brazilian census. I focus on the role of geographic space from the perspective of voters, and I clarify that in Brazil, administrative boundaries for neighborhoods and residents' conceived neighborhood boundaries largely overlap. In contrast to the assumptions made by scholars of U.S. politics, the neighborhood is not the lowest unit of aggregation in the Brazilian census, but rather neighborhoods are comprised of census tracts that could each be either a *favela* or a cluster of the middle- or upper-class. Figure A1 below illustrates the relational difference between the *favela* (i.e., census tract or collection of tracts), the neighborhood (i.e., census area), the city, and the state. I focus on the segregation of *favelas* from the rest of the population within neighborhoods.

Figure A1: Unit of Analysis



Favelas | *Favelas* and Census Tracts | Within Neighborhoods (blue)

A3 Types of Public Goods

I make a distinction between preferences for an "externality good" and a "non-externality good." I also classify a set of "altruistic goods," a specific type of "non-externality good." A public "externality good" is one that even when geographically targeted towards slums also provide the additional social welfare benefit of reducing externalities to non-slum locations. While most public goods address urban externalities between *individuals*, my definition of public "externality goods" concerns only those that address the externalities of inequality between socioeconomic class *groups* (see the Theory section on the concept in main text). Overall, the three definitional attributes of the concept of the externalities of inequality can be used to classify "externality goods:" these externalities stem from there being relative deprivation, a concentration of poverty (i.e., group-based effects), and spatial proximity. In contrast, a "non-externality" or "altruistic good" is one that directly benefits residents in slums without conferring ancillary benefits to residents beyond slum borders. These goods only improve the welfare of the poor or they address urban externalities between individual members of the poor instead of between the middle class and the poor.

Examples of "externality goods" include different forms of public security, such as public patrolling or the provision of streetlights. To the extent that these goods reduce the magnitude of especially organized crime in *favelas*, they also reduce the potential that such activity spills over across *favela* borders to the neighboring middle-class. Another prime example discussed in-text is the provision of sewage collection and treatment services. Historically, given their status of informality, *favelas* tend to be deficient in such services, thus, sewage run-off flows from these settlements. The extension of sewer lines to these settlements directly addresses the externality of residing near open sewerage or of being downstream from a *favela*. This difference between "externality goods" and "non-externality goods" can be made clearer with an example of the latter: because it does not result from these three definitional conditions, congestion is not an externality of inequality. Congestion stems purely from high population density; inequality is not the source of congestion. Congestion also occurs even in the absence of a concentration of poverty. Last, congestion is an externality that is more so between individuals than one between class groups. There is not one identifiable group that is more or less responsible for congestion across space and time, and because every urban resident experiences it (i.e., is affected), yet also contributes to congestion (i.e., is a producer of it), it is an externality that is internalized. Because road congestion is not an externality *of inequality*, a public good that addresses it –i.e., public transit– is a "non-externality" good. Considering the whole battery of urban public goods types (e.g., public transit, public schools, public hospitals, public security, and sanitation services), we can then classify all these public goods types according to whether they meet these three definitional criteria and, therefore, address an externality of inequality.

Examples of "altruistic goods," a type of "non-externality good," include state provision of soup kitchens or of daycare services for infants and toddlers. In developing countries, the middle and upper class largely rely on private daycares. Regardless of whether they are provided directly in *favelas* or in the neighborhood, overall, such state-sponsored services only help the poor without providing indirect benefits of addressing an externality to the middle-class who share the neighborhood. Although it is the case that any good or service that improves the lives of the poor could also reduce the likelihood they beg in the streets or engage in organized crime, these spillover benefits for the middle-class are perceived as being limited. Middle-class focus group attendees perceive soup kitchens and public daycares to be almost exclusively for the poor. Preferences for these goods, therefore, capture only a degree of social affinity or altruism towards the poor, whereas those for "externality goods" include self-interest motivations for addressing externalities. This is the main distinction between preferences for "externality goods" and "non-externality" or "altruistic goods."

A3.1 Results for Other Public Goods Types

Besides the results for the main "externality goods" presented in-text in Table 1, I also provide below the results for preferences for all other types of public goods collected in the survey. "Broken windows theory" in Criminology maintains that visible signs of dilapidation in the urban environment in the form of vandalism, broken windows, or loitering, among other examples, further encourage crime by creating an atmosphere of unlawfulness and disorder.[1] The paving of roads and

[1] Critics of "broken windows theory," however, argue that the logic that punishing low-level offenses can prevent more serious crimes is flawed. It precludes a focus on solutions for more serious crimes and often results in the

sidewalks reduce the appearance of the dilapidation of the neighborhood and can discourage crime by psychologically imposing a sense of order. However, as the results presented below indicate, I do not find that integration has a statistically significant effect on preferences for paving streets.

However, I do find that class-based integration generates preferences for public schools. I classify investments in public education, especially public primary and secondary schools, as a type of externalities-correcting public good. Although Brazil's middle class usually enroll their own in private schools, in focus group discussions in City C, they often still support public education services. The integrated middle class recognize how the idle youth in *favelas* drives recruitment by organized criminal forces. A major source of organized crime is deficient education opportunities. In addition, the middle class in integrated Belo Horizonte also discuss how pickpocketing and petty street theft instigated by *favela* residents tend to be on the part of the idle youth. Therefore, crime is an externality of inequality that also drives middle-class support for public schools even when they do not use them themselves. In contrast, integration has no effect on preferences for hospitals and mass transit, types of "non-externality goods" discussed earlier.

Table A1: Results for Other Public Goods Types

		Second-Stage:			
	(1) First-Stage: Socioeconomic Integration	(2) Hospitals	(3) Public Schools	(4) Paved Streets	(5) Public Transit
Migration SSIV	0.662***				
	(0.113)				
Socioeconomic Integration		0.480	1.282**	0.799	-0.068
		(0.471)	(0.631)	(0.682)	(0.401)
Observations	3,222	3,217	3,217	3,217	3,217
Neighborhood Clusters	378	378	378	378	378
Outcome Mean	0.640	6.370	5.764	5.481	6.289
F-stat	34.547				

Standard errors in parentheses

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

Notes: 2SLS models of the estimated effect of integration on preferences for all other types of public goods collected in the survey. Paved streets and public schools are classified as "externality goods," while hospitals and public transit are "non-externality goods."

A3.2 Measuring Preferences for "Inclusionary" and "Exclusionary" Security

In Latin America, as well as in many other parts of the world, public policing is often viewed as a form of repression of marginalized groups. It is common for the mainstream media to report accounts of police brutality and overt violence in Brazil's *favelas*. I clarify that I differentiate between preferences for "exclusionary" and "inclusionary" public security, and only focus on the latter. "Inclusionary" public security includes non-punitive street patrolling as a preventative measure to discourage criminal activity and also preventative infrastructure, such as the provision of streetlights. Empirically, I capture this distinction in several different ways. First, respondents

aggressive over-policing of minority communities (Sampson and Raudenbush 2004).

are surveyed about the extent to which they prefer these different types of public security, ranging from the provision of streetlights to that of punitive policing that uses "mano dura" tactics. This strategy makes clear to respondents exactly which type of security they are asked to register their preferences for, allowing me to isolate those for non-punitive patrolling. Second, I include an additional survey question that asks respondents the extent to which they view policing, overall, to be a form of repression of the poor –measured on Likert scale from 1 to 7. Respondents can also provide open-ended responses to accompany their numerical response to this question. I use this question to control for exactly what respondents mean when they prefer more or less policing.

A4 Main Results Controlling for Respondent-Level Controls

Table A2: Main Results with Respondent Control Variables

		Second-Stage:			
	(1) First-Stage: Socioeconomic Integration	(2) Streetlights	(3) Policing	(4) Sewage Collection	(5) Private Security
Migration SSIV	0.662***				
	(0.113)				
Socioeconomic Integration		1.919***	2.548***	1.627**	-0.560**
		(0.705)	(0.614)	(0.719)	(0.238)
Race		0.036	0.035*	0.027	-0.010
		(0.026)	(0.020)	(0.025)	(0.009)
Gender		0.397***	0.325***	0.168*	0.039
		(0.087)	(0.078)	(0.089)	(0.033)
Age (Years)		0.002	0.002	-0.009***	-0.002
		(0.003)	(0.002)	(0.003)	(0.001)
Education		-0.056**	0.020	-0.035	-0.028***
		(0.026)	(0.019)	(0.024)	(0.008)
HH Income		0.008	-0.003	0.051**	-0.004
		(0.032)	(0.025)	(0.026)	(0.012)
Years of Residence		-0.006**	0.001	-0.007**	-0.001
		(0.003)	(0.002)	(0.003)	(0.001)
Political Ideology		0.038**	0.086***	-0.002	0.007
		(0.017)	(0.015)	(0.017)	(0.007)
Observations	3,222	1,840	1,840	1,840	1,840
Neighborhood Clusters	328	328	328	328	328
Outcome Mean	0.640	4.419	5.813	4.630	0.736
F-Statistic	38.083				

Standard errors in parentheses

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

Notes: Main results using 2SLS estimation of the effect of class-based integration on for preferences for public and private goods, estimated with the respondent-level control variables.

A5 Main Results Controlling for Baseline Controls

Table A3: Controlling for Pre-Existing Levels of Public Goods

		Second-Stage:			
	(1) First-Stage: Socioeconomic Integration	(2) Streetlights	(3) Policing	(4) Sewage Collection	(5) Private Security
Migration SSIV	0.662***				
	(0.113)				
Socioeconomic Integration		2.317***	2.743***	2.168**	-0.486*
		(0.821)	(0.719)	(0.891)	(0.277)
Baseline Public Goods		-7.198	0.362	11.515	-0.863
		(8.057)	(6.311)	(8.272)	(2.258)
Observations	3,218	3,217	3,217	3,217	3,217
Neighborhood Clusters	378	378	378	378	378
Outcome Mean	0.640	4.419	5.813	4.630	0.736
F-statistic	26.068				

Standard errors in parentheses

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

Notes: Main results for preferences for public and private goods, estimated with the pre-existing share of each neighborhood's poor population with access to public goods as a baseline control.

A6 Causal Mediation Analysis of the Mechanisms

Next, beyond the results for the direct effects of integration on each of the mechanisms already discussed in Section 4.3, I isolate the causal mediation effect of each mechanism. As discussed in Section 3.4, I estimate the combined effect of the red arrows (dashed) and blue arrows (solid) by using the randomized campaign of a hypothetical political candidate to prime each respondent to think about a specific mechanism before they answer a set of questions measuring their preferences (see Figure 4 in main text). In other words, I observe how the instrumented effects of socioeconomic integration on preferences differs for respondents who were randomly assigned a vignette about organized crime compared to one about racial tolerance or one about social affinity towards the poor. The results are presented in Figure A1 below.

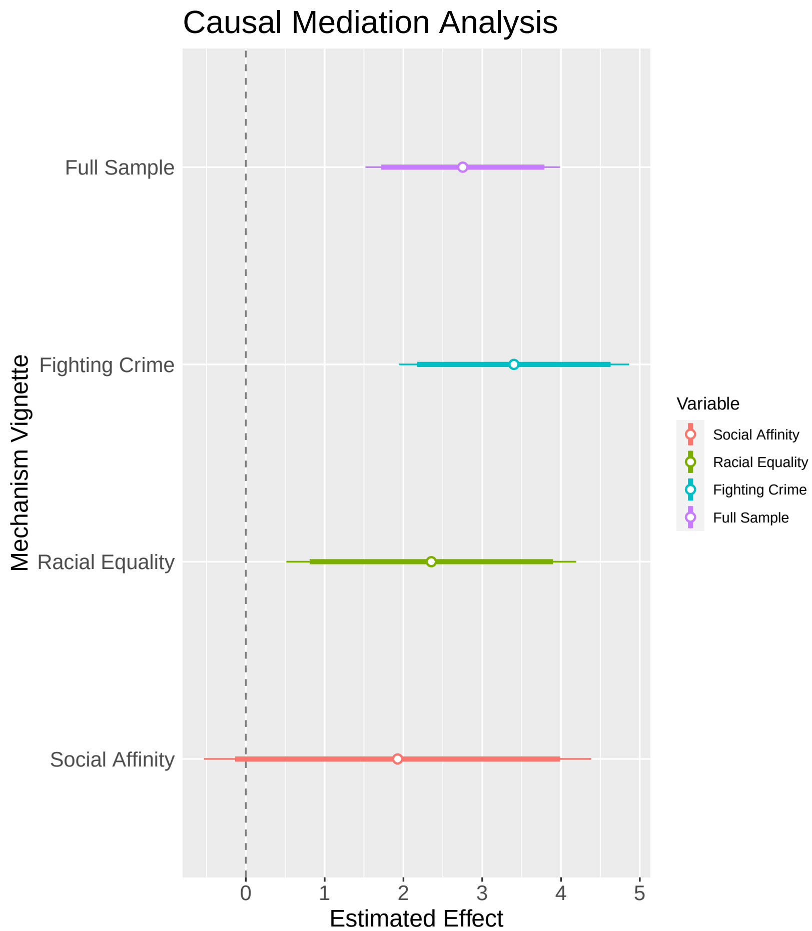


Figure A2: Causal mediation analysis. See Table A23 for results in table form.

I include the estimated effect of socioeconomic integration for the full sample of respondents (i.e., inclusive of all respondents assigned to all three treatment campaigns as well as those in the reference group campaign) as a point of comparison at the top. As already discussed in Section 4.1, integration has an, overall, positive effect on preferences for policing in the full sample. Deconstructing these effects in the full sample, we can see that the effect size of socioeconomic integration varies according to the mechanism respondents are primed to consider. For the subset of respondents assigned to the spatial externalities campaign, the causal effect of integration on preferences for policing is much larger than that for the full sample. In contrast, for the subset of respondents primed to think about racial tolerance instead, the effect size is smaller. For those assigned to the social affinity treatment, integration still has a positive effect on preferences for policing; however, the effect is no longer statistically significant. Overall, this set of results show that the ***spatial externalities effect*** is a much stronger mechanism –or at least has a much larger mediation effect– on preferences for an "externality good" than the alternative mechanisms discussed in the literature.

A7 Frequency of Encountering the Poor and Preferences for Private Security

Table A4: Frequency of Encountering the Poor and Relative Preferences for Private Security

	Dependent variable:
	Preferences for Private Security
Model	(1)
Frequency Encounter Poor	-0.035**
	(0.014)
Racial Segregation	0.200
	(0.241)
Gender	-0.007
	(0.032)
Age (Years)	-0.002
	(0.001)
Education	-0.005*
	(0.003)
Household Income	-0.003
	(0.027)
Race (Skin Color Indicators)	✓
Survey Enumerator Fixed-Effects	✓
Observations	2,462
Neighborhood Clusters	344
Note:	*p<0.1; **p<0.05; ***p<0.01

Notes: OLS estimation includes cluster-robust standard errors at the neighborhood-level and demographic controls. Skin color indicator variables range from 1-11, with 1 indicating fairest skin.

A8 Constructing Measures of Segregation

I use 2000 and 2010 Brazilian Census tract data (IBGE 2010) and shapefiles of municipal and census tract boundaries to code spatial and a-spatial versions of four different measures of socioeconomic segregation: the *spatial dissimilarity*, the *spatial information theory*, the *interaction*, and the *isolation* for each neighborhood in São Paulo and also for each of Brazil's over 5,500 municipalities. These four measures vary in the extent to which they measure the two main conceptual dimensions of segregation: 1) spatial evenness (or spatial clustering), and 2) spatial exposure (or spatial isolation). To ensure robustness, I code each of these indices using 8 different thresholds of income (i.e., those above and below 1MW, 2MW, 3MW, etc.) for demarcating segregation between two socioeconomic groups. I construct measures of segregation based on the methodologies detailed in Massey and Denton (1988), Reardon and O'Sullivan (2004) and (Garcia-López and Monreno-Monroy 2018), and my discussion here of these segregation indices credits their work. Measures of segregation that capture evenness measure the extent to which the two income groups above and below the threshold are evenly distributed across space. Using the *Seg* package in R, I calculate the *spatial dissimilarity*, the most commonly used index of evenness (Reardon and O'Sullivan 2004):

$$\tilde{D} = \sum_{i=1}^{T} \pi_p \tilde{D}_p \tag{1}$$

$$\tilde{D}_p = \sum_{m=1}^{M} \frac{1}{2I} \mid \tilde{\pi}_{m|p} - \pi_m \mid \tag{2}$$

And where I in the above is an interaction index that proxies for population diversity:

$$I = \sum_{m=1}^{M} (\pi_m)(1 - \pi_m) \tag{3}$$

The *spatial dissimilarity index* captures the percentage of the population of one group that would need to change their residence for each neighborhood to have the same corresponding percentage of that group as that for the municipality as a whole (Massey and Denton 1988).

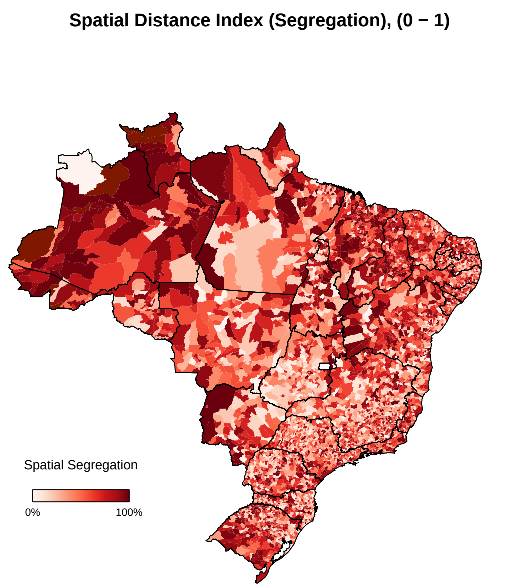


Figure A3: The Spatial Dissimilarity Index. Calculated using 2010 Brazil Census tract data from Instituto Brasileiro de Geografia e Estatística (IBGE 2010).

Next, I also calculate a second measure of evenness called the *spatial information theory* index, denoted $\tilde{H}$, first proposed by Theil (1972):

$$\tilde{H} = \sum_{i=1}^{T} \pi_p (\frac{E - \tilde{E}_p}{E}) \tag{4}$$

where E (i.e., different from $\tilde{E}_p$) is defined as the regional entropy of the total population as follows:

$$E = -\sum_{m=1}^{M}(\pi_m)log_M(\pi_m), \tag{5}$$

The spatial information theory index is a measure of the extent to which the local environment of p (i.e., in this context, the unit of the Brazilian census tract) is, on average, less socioeconomically diverse than the total population of the region R (i.e., the corresponding Brazilian municipality). E is maximized if individuals are evenly distributed among the M groups (Reardon and O'Sullivan 2004).

In addition, I also construct segregation indices that measure the spatial exposure (or spatial isolation) of groups. Exposure directly captures the degree of possible contact –i.e., of physical interaction– between groups (Massey and Denton 1988). Indices of exposure, therefore, directly depend on the extent to which the groups share residential area. To measure exposure (and isolation), I calculate the *interaction* index as follows:

$$\sum_{i=1}^{n} = [(\frac{x_i}{X})(\frac{y_i}{t_i})] \tag{6}$$

And I also calculate the *isolation* index:

$$\sum_{i=1}^{n} = [(\frac{x_i}{X})(\frac{x_i}{t_i})] \tag{7}$$

Last, I create a simple nearest average distance to slum measure. The index takes each non-slum census tract and calculates its distance (km) to the nearest informal settlements. The distance for each of these tracts is then averaged at the level of the neighborhood (i.e., census area) (456).

Figure A4: The measure takes each majority poor census (e.g., slums in red), finds the nearest majority middle-class census tract, and spits out that distance

A9 Constructing Predicted In-Migration Shift-Share Instrumental Variable

I briefly give a concrete example of the type of variation that the predicted migration instrument exploits. Both São Paulo in Southeast Brazil and the capital city of Brasília in the Central-West region were major destinations for poor rural migrants in 1990. However, poor migrants arriving in these cities historically before 2000 came from parts of the country that experienced very different patterns of outmigration compared to patterns from the subsequent decades (2000-2010, 2010-2020). While São Paulo experienced the largest influx of migration from the Northeast, Brasília drew the majority from the Central region. Poor migrants from the drought-ridden Northeast region tend to come from counties that specialized in the production of cocoa, tropical fruits, and forest products (e.g., timber). By contrast, Central Brazil –historically considered unsuitable for agricultural production–focuses mostly on livestock and the production of soy. While the incidence of droughts in the 1990's and 2000's spurred out-migration from the Northeast, the adoption of genetically modified soy in 2003 instead lowered outmigrants from Central Brazil. Extensive variation in rural municipality of origin also exists across neighborhoods within the same city.

The empirical strategy makes use of the fact that poor migrants from the Brazilian countryside settled where previous migrants from their communities had moved. Drawing on the empirical strategy proposed by Boustan (2010) for estimating the Great Migration of blacks in the U.S., the steps for constructing the shift-share instrument are as follows. First, I calculate net out-migration from rural municipalities between 2000 and 2010, using the two rounds of census data available from IBGE (2010). Second, I prepare the "push" and "keep factor" variables, which are measured at the beginning of the decade (2000) using census data. Then, to predict migration of the poor from each rural municipality, I estimate the net poor out-migration rates at the municipal level as a function of "push" and "keep factors" as follows:

$$\text{Migration rate}_{m,t-(t+10)} = \alpha + \gamma(\text{push factors})_{m,t} + \varepsilon_{m,t} \tag{8}$$

I use different combinations of following set of rural municipalities characteristics measured at the start of the decade to predict migration over the course of the subsequent decade:[2] changes in land inequality, share of residents cultivating cassava, share cultivating soy, share cultivating coffee, share cultivating sugarcane, share cultivating cocoa, share cultivating corn, share cultivating cotton, share cultivating wheat, adoption of genetically modified (GM) soy, adoption of genetically modified maize, adoption of new wave of land reforms (1988-2013), and the incidence of drought (i.e., measured using either the Vegetation Health Index (VHI), Vegetation Condition Index (VCI), Temperature Condition Index (TCI), or the Smoothed Brightness Temperature (SMT)).

I use several different measures of drought (see Appendix A7) available from NOAA STAR. The data set is available here. I spatially join the GEO TIFF files with rural municipality boundaries, and calculate measures of mean, range, maximum, and minimum drought by municipality. I use data on adoption of genetically modified crops from Bustos, Caprettini, and Ponticelli (2016), and data on land reform programs from Albertus, Brambor, and Ceneviva (2018). Tables A16 and A17 presents the coefficients regressing net migration rates on these characteristics of rural municipalities to illustrate the strength of some of these "push" or "keep factors."

[2] I calculate different versions of the same instrumental variable using these different combinations of "push factors."

Table A5: Strength of Migration "Push Factors:" Brazilian Cash Crops

	(1) Net Out-Migration of the Rural Poor (2000-2010)
Share Cassava	-110.336***
	(19.170)
Share Coffee	-49.823***
	(12.475)
Share Soybeans	52.981*
	(27.926)
Share Cocoa	-211.242***
	(54.186)
Share Rice	43.591**
	(19.618)
Share Cotton	-142.136*
	(76.179)
Observations	4,056

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

Table A5: Agricultural "push factors" for predicting out-migration of the rural poor.

Table A6: Strength of Migration "Push Factors:" GM Crops, Land Inequality, Land Reforms

	(1) Net Out-Migration of the Rural Poor (2000-2010)
GM Soy	6.577***
	(1.298)
GM Maize	-2.147***
	(0.598)
Change in Land Inequality (1990-2000)	-14.702*
	(8.784)
Cumulative Land Reforms	-2.353***
	(0.395)
Vegetation Condition Index (Drought)	-0.002**
	(0.001)
Observations	2,584

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

Table A6: Adoption of genetically modified (GM) soy, GM maize, previous period (1990-2000) change in land inequality, land reforms, and incidence of drought as rural "push factors."

Next, to generate the predicted out-migration from each origin municipality, I multiply the fitted migration rate by each municipality's initial population of the poor in 2000. I calculate different versions of the instrument using under 1 minimum wages (MW), under 2MW, and under 3MW as three different thresholds for defining the poor, and the results remain consistent. Next, I use a measure –reported in the 2000 census– of the share of the poor population in each destination municipality or São Paulo neighborhood who had lived in another (rural) municipality in 1995 as the weights for assigning predicted in-migration. Specifically, I assign predicted out-migration mapped above to urban neighborhoods or cities according to these settlement patterns of the rural poor from their communities who migrated during the 1995 to 2000 period. I calculate the number of poor migrants predicted to arrive in each destination municipality (or census area) c at time t as the weighted sum over the over 2,000 rural municipalities with migrants leaving municipality m and settling in municipality m (see Equation 1 in the main text).

The shift-share instrument is, therefore, constructed using two components: predicted migrant out-flows from the countryside, predicted using the "push factors," and historical settlement patterns of the poor leaving these same municipalities between the 1995-2000 period. The instrument calculated using the threshold of 2MW for defining the poor is mapped for São Paulo census areas below.

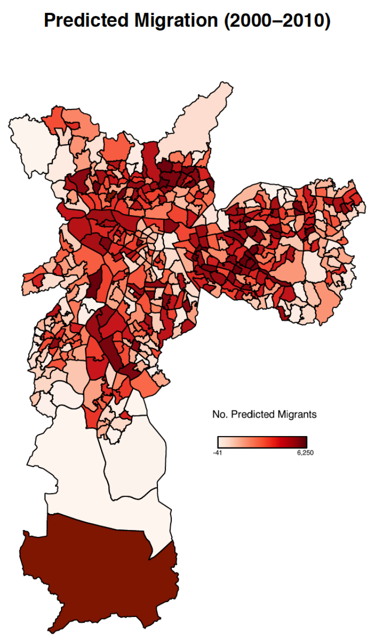


Figure A5: This figure maps in-migration of the poor between the 2000-2010 period as predicted by the shift-share instrument.

A10 Survey Sampling and Design

I worked with the São Paulo division of the Brazilian Census Bureau (IBGE) to ascertain exactly which set of household addresses fall within the boundaries of each neighborhood to be able to feed the list of addresses into the algorithm for random sampling. The sampling strategy for household addresses is as follows. First, at the level of the census area, we surveyed as many households as was feasible. Within each census area, we randomly sampled two census tracts. In addition, within each of the census tracts, we then randomly sampled a household address, producing two household addresses (i.e., one for each census tract). This sampling procedure was repeated for each survey enumerator for every day they were in the field. Each survey enumerator was assigned two different household addresses per day: one to use as their starting point at the start of the day and another for use as a starting point mid-day. Beyond each of these starting points, enumerators counted 5 houses from every household successfully surveyed. This interval count rule ensured randomization of households selected beyond the randomized starting points.

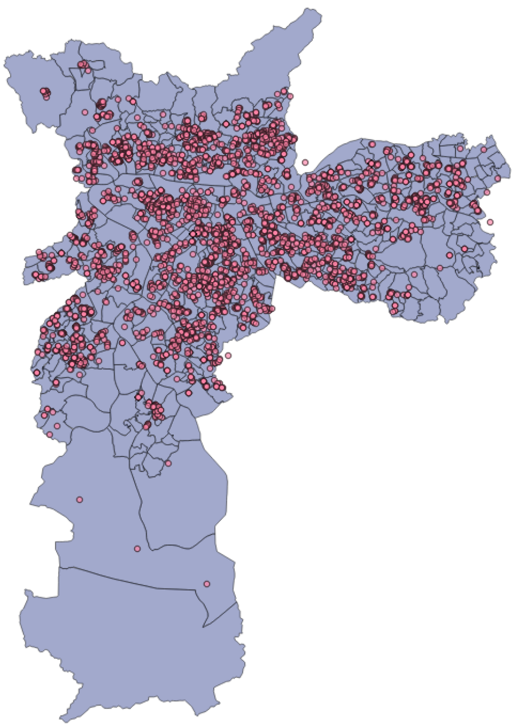


Figure A6: This figure maps the over 4,000 geocoded survey observations across census areas for São Paulo. The survey covers 420 of the 456 total census areas in the megacity. The survey required coordinated planning with real estate management companies to access condominiums, permission from neighborhood associations to enter, and careful survey sampling around areas dominated by the São Paulo-based *Primeiro Comando da Capital* (PCC), the largest criminal organization in Brazil. The census areas that were not surveyed were localities deemed inaccessible or unsafe for entry. A total of 4,204 households were surveyed; however, over 200 addresses are ones that cannot be accurately geocoded.

In total, the face-to-face survey was administered to 4,204 households residing across 420 of the total 456 neighborhoods in São Paulo. Almost all households surveyed reside in neighborhoods designated as formal "urbanized area" (i.e., non-*favela*), since the population of interest is the urban population that does not reside in informal settlements. The response rate is around 37.7%.

A10.1 Exceptions to Sampling Strategy

There were several exceptions to the randomized sampling procedure. Parts of the city are, for example, impenetrable for security reasons. Since the analysis is focused on middle-class residents, the survey largely excludes informal settlements. These areas were also excluded to ensure enumerator safety. Second, the survey sampling excludes areas occupied by the São Paulo-based

Primeiro Comando da Capital (PCC), the largest criminal organization in Brazil. Third, highly commercialized census areas were excluded from the sample, given the dearth of residents in these areas. In addition, condominium-heavy census areas had a different sampling procedure that is not randomized.[3] The surveying of residents living in condominiums required a planned coordination effort with real estate management companies, the *sindicatos* (superintendents), and the residents of each condominium complex.

A10.2 Mechanism Vignette Treatments

In the middle of the survey, respondents were told:

"During a recent municipal election, a politician made the following campaign promise:

> 'Brazil is an extraordinary country, rich in natural resources, and São Paulo is a magnificent and beautiful city. However, the government can do much more to improve our lives. My fight is [**TREATMENT**]. This cause deserves your vote.'

This base text is the same across respondents; however, each respondent randomly receives one of the following four possible treatments in the bolded text above:

> **Control Group 1 (broad economy and well-being)** [to improve our economy and the well-being of our city. Development is key across all of Brazil, and it is especially important here in the largest city in the country: São Paulo. If you elect me, I will fight to improve our economy and sustain our healthy and peaceful democracy. I will fight to improve the quality of life in our beautiful city.]

> **Treatment Group 2 (Social Affinity)**: [for the defense of the interests of all common people. The income distribution in Brazil is among the most unequal in the world. Residents in *favelas* or *comunidades* have low life expectancy and very poor quality of life. Philanthropic programs substantially improve the welfare of residents in these settlements. When there are those in need, there are generous and benevolent desires to help them. If you elect me, I will focus on the rights of citizenship for the city's needy people and communities and on expanding their access to adequate public healthcare.]

> **Treatment Group 3 (Racial Tolerance) :** [for the defense of quotas in public and private universities for blacks. Brazil is one of the most racially diverse countries in the world. More than 50% of the population define themselves as black or mixed race, and wages among white Brazilians are on average twice as high as wages among black Brazilians. The majority of residents in Brazil's *favelas* or *comunidades* are black. If you elect me, I will also fight against racism and discrimination in the labor market and work to promote education about black history in our school curriculum.]

[3]This is not of great concern, because the actual share of condominium-dominant census areas in São Paulo is quite low.

Treatment Group 4 (Externalities): [against the high levels of crime in our city that too often threatens our property and way of life. Ranging from theft to homicides, Brazilian cities experience some of the highest volumes of crime in the world. Organized crime in poorer areas and financially-motivated crimes, such as armed robbery, occur with great frequency, and victims of these crimes are targeted indiscriminately. If you elect me, I will work with the police to crack down on crime, especially street crime and organized criminal violence in *favelas/comunidades* that makes our city feel less safe.]

Respondents are then asked "How likely are you to vote for this candidate?"

Beyond measuring the effects of socioeconomic integration on responses to these campaign endorsement questions (i.e., as outcome measures), each respondent is also unknowingly primed to think about a specific (randomized) mechanism before they consider their preferences for public and private provision of services.

A10.3 Survey Descriptive Statistics

Table A7: Survey Descriptive Statistics

	Mean	Standard Deviation	Minimum	Maximum	Count
Age (Years)	49.260	17.527	18.000	93.000	3394
Race (Black)	0.282	0.450	0.000	1.000	3401
Gender (Female)	0.576	0.494	0.000	1.000	2725
Education (Years)	13.222	5.862	0.000	79.000	3388
Household Income (Alt.)	1.944	0.644	1.000	4.000	3365
Married	0.491	0.500	0.000	1.000	3401
Catholic	0.476	0.500	0.000	1.000	2470
Years of Residence	25.718	19.172	0.000	86.000	3392
Preferences Streetlights	4.418	1.875	1.000	7.000	3392
Preferences Policing	5.814	1.540	1.000	7.000	3392
Preferences Sewage Collection	4.629	1.937	1.000	7.000	3392
Preferences Public Schools	5.764	1.630	1.000	7.000	3392
Preferences Private Security (Absolute)	3.773	2.449	1.000	7.000	3391
Preferences Private Security (Relative)	0.736	0.723	0.143	7.000	3391
Preferences Hospitals	6.371	1.259	1.000	7.000	3392
Preferences Public Transit	6.289	1.274	1.000	7.000	3392
Preferences Paved Streets (in *favelas*)	5.481	1.678	1.000	7.000	3392
Preferences Soup Kitchens (in *favelas*)	5.720	1.591	1.000	7.000	3395
Fear of Crime	2.712	1.080	1.000	4.000	3393
Crime Victimization (12 months)	0.223	0.416	0.000	1.000	3393
Concern for Sewage Pollution	2.441	2.097	1.000	6.000	3396
Racial Tolerance (Marriage)	6.659	0.971	1.000	7.000	3393
Left-Right Ideology (1-10)	5.884	2.746	1.000	10.000	2642

Note: Descriptive statistics of survey respondents in household survey. Household income is measured in terms of minimum wages (MW), and the alternative income measure ranges from 1 to 4, corresponding to the categories "Lower-class," "Lower-middle-class," "Upper-middle-class," and "Upper-class," respectively. Due to a technical glitch in the Qualtrics survey platform, certain demographic variables, such as race and gender, have over 700 observations that were not recorded. I create versions of the data in which I use the *mice* and *naniar* packages in R to impute values for these missing observations. However, the reported results and tests in the main text and Appendix uses the original data without the missing data imputations.

A10.4 Survey Balance on Mechanism Treatments

Table A8: Balance for Externalities Treatment

	Control			Treatment			
	N	Mean	Standard Deviation	N	Mean	Standard Deviation	Difference
Age (Years)	811	49.48	17.44	886	48.97	17.58	-0.594
Race (Black)	811	0.28	0.45	886	0.29	0.46	0.015
Education (Years)	810	13.28	6.18	884	13.25	5.72	0.085
Gender (Female)	642	0.58	0.49	693	0.61	0.49	0.019
Household Income (Alt.)	807	1.95	0.64	880	1.94	0.64	-0.017
Married	811	0.47	0.50	886	0.49	0.50	0.022
Years of Residence	810	25.67	19.30	886	26.22	19.06	0.639

Note: Balance on covariates among respondents in the externalities treatment relative to control group. The "Difference" column presents the coefficients from regressing treatment status on variable, with robust standard errors clustered at the neighborhood-level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A9: Balance for Racial Tolerance Treatment

	Control			Treatment			
	N	Mean	Standard Deviation	N	Mean	Standard Deviation	Difference
Age (Years)	811	49.48	17.44	860	49.32	17.81	-0.406
Race (Black)	811	0.28	0.45	860	0.29	0.45	0.011
Education (Years)	810	13.28	6.18	859	13.26	5.81	-0.081
Gender (Female)	642	0.58	0.49	696	0.56	0.50	-0.030
Household Income (Alt.)	807	1.95	0.64	849	1.97	0.64	0.014
Married	811	0.47	0.50	860	0.50	0.50	0.027
Years of Residence	810	25.67	19.30	860	25.79	19.64	0.130

Note: Balance on covariates among respondents in the racial tolerance treatment relative to control group.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A10: Balance for Social Affinity Treatment

	Control			Treatment			
	N	Mean	Standard Deviation	N	Mean	Standard Deviation	Difference
Age (Years)	811	49.48	17.44	838	49.23	17.36	-0.277
Race (Black)	811	0.28	0.45	838	0.26	0.44	-0.023
Education (Years)	810	13.28	6.18	836	13.09	5.74	-0.204
Gender (Female)	642	0.58	0.49	690	0.57	0.50	-0.008
Household Income (Alt.)	807	1.95	0.64	831	1.92	0.65	-0.033
Married	811	0.47	0.50	838	0.51	0.50	0.042
Years of Residence	810	25.67	19.30	837	25.13	18.70	-0.229

Note: Balance on covariates among respondents in the social affinity towards the poor treatment relative to control group.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A11 Identification Assumptions

A11.1 Exogeneity of Instrument

As discussed in the Data and Research Design in the. main text, I propose an instrumental variable for segregation that can be constructed at either the neighborhood- or the city-level. Recent papers by Goldsmith-Pinkham, Sorkin, and Swift (2020); Adão, Kolesár, and Morales (2019);

Borusyak, Hull, and Jaravel (2020) have extensively explored the validity of this type of shift-share instrument variable (SSIV). Borusyak, Hull, and Jaravel (2020) demonstrate that in the case where the shares are endogenous, the "shifters" can provide exogeneity provided that the shocks to industry –or in this case, rural municipalities– are not correlated with shocks to urban destination localities. To confirm that the results are not driven by this correlation between origin-specific shocks ("push or keep factors") and shocks to destination localities ("pull factors"), I construct two alternative versions of the instrument and conduct an over-identification test. Specifically, I construct a second version of the instrument that uses rural municipality out-migration rates that are first residualized on state fixed-effects. This version of the instrument accounts for correlation in shocks to rural municipalities and to urban destinations. I also construct a third version of the instrument that uses municipality of birth of rural-born poor residents living in urban destination localities in 2000 in place of 1995-2000 settlement patterns as the weights for assigning predicted migration. As described in Derenoncourt (2019), variation in municipality of birth proxies for variation in historical migration trends, allowing for even more variation in the exposure of urban destination localities to rural origin shocks.

In addition, Adão, Kolesár, and Morales (2019) finds that standard inference procedures, such as geographically clustered standard errors, may be invalid in a shift-share design, because observations with similar exposure shares are likely to have correlated residuals. If, for example, a set of origin municipalities have similar importance across various destination localities, there will be correlation at the origin municipality-level across destination localities. To test for this, I follow (Derenoncourt 2019) in running a placebo test that uses instead the interaction of rural poor migrant location choices with, instead, a normally distributed random variable with mean 0 and variance 5. I estimate these OLS regressions, using this placebo measure and socioeconomic integration as the dependent variable 1,000 times, and I note the fraction of times the result indicated significant effects at the 5% level. The coefficient is significant 6.2% of the time at the 5% level. While the standard errors may benefit from the adjustment proposed in Adão, Kolesár, and Morales (2019), the precision of the findings from this placebo test indicate that the effect of rural-to-urban migration of the poor is likely not driven by noise. It would remain highly significant even with Adão, Kolesár, and Morales's (2019) proposed adjustment for standard errors.

The first-stage effect of the instrument is strong with an F-statistic of 34.547. Another main identification assumption for instrumental variables is that of the exogeneity of the instrument. The instrument must not be correlated with the error term of the estimated models. There is a violation of the assumption of exogeneity if, for example, there are pre-treatment period trends and outcomes (i.e., omitted variables) that are correlated with the instrument constructed for the current period. The assumption of exogeneity cannot be directly test, but I estimate four placebo tests to test for potential violations of this assumption. First, I show in Table A11 that while controlling for baseline controls, the predicted migration shift-share instrumental variable (2000-2010) does not predict the lagged net migration rate of the poor prior to 2000 before the decade of the predicted migration changes. It also does not predict the lagged change in neighborhood-level income inequality (1991-2000) (Table A12) nor does it predict other public policies in the pre-treatment period (Table A13). (See also Replication Files.)

A11.2 Exclusion Restriction and Balance on Omitted Control Variables

In addition, another key identification assumption is that of the exclusion restriction, the assumption that the instrument –either the predicted migration SSIV or the interaction of the SSIV with "urban form"– affects the outcome of interest only through the treatment –in this case, socioeconomic segregation. At the neighborhood-level, the predicted migration SSIV arguably captures exogenous increases in the population of the poor based on the logic described in Section 3.2 that shocks to migrants' origin locations ("push factors") are plausibly orthogonal to shocks to destination localities ("pull factors"). Nonetheless, the "uphillness" of destination cities is not random. I borrow from Esarey (2015) to argue and provide evidence that the interaction of the SSIV and "uphillness" is random. Esarey (2015) and Pearl (2000) show that if two exogenous variables v and w have a conditional relationship with x (i.e., segregation), but an unconditional relationship with y (i.e., the outcome, or preferences for public goods), then the interaction term vw can potentially serve as an effective instrument to identify the causal effect of x on y. More specifically, the effect of v on x (segregation) is conditional on the value of w, "urban form," but the effect of v on y does not depend on w.

The exclusion restriction condition cannot be directly tested, but I test for the plausibility that it holds. Specifically, I conduct balance tests on post-treatment measures of neighborhood- and city-level economic and political features as well as respondent-level demographic traits. In other words, I test whether the proposed instrument predicts these other neighborhood- or respondent-level characteristics –besides socioeconomic segregation– that could, in turn, also affect the outcomes. In addition, since I exclude these neighborhood- and respondent-level control variables in the 2SLS estimations to avoid bias from conditioning on concomitant variables (Rosenbaum 1984), these tests also confirm that the instrument tends to be balanced on these measures. For the neighborhood-level instrument, Table A14 presents the balance tests for survey respondent-level variables, while Table A15 presents the tests for neighborhood-level features. Last, to ensure that segregation and not changes in poverty or inequality levels are driving the results, I confirm that the instrument is balanced on neighborhood-level measures of current period poverty and inequality in Table A16. Because there is balance on these variables, they are not driving the results. And although the exclusion restriction can never be directly tested, these tests provide evidence for the plausibility that it holds. I, once again, omit the presentation of these same tests for the city-level to stay within the page count limit (see Replication Files for more).

A11.3 Neighborhood-Level Placebo Tests for Exogeneity of the Instrument

Table A11: Neighborhood-Level Placebo Test: Lagged Net Migration (1991-2000)

	(1) Lagged Net Actual Migration Rate
Migration SSIV	47.496
	(128.642)
Baseline Public Goods	64.007***
	(14.267)
Observations	442

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

Notes: The instrumental variable does not predict the net migration rate in the previous period.

Table A12: Neighborhood-Level Placebo Test: Lagged Change in Income Inequality (1991-2000)

	(1) Lagged Change in Gini Coefficient (1991-2000)
Migration SSIV	0.740
	(0.476)
Baseline Public Goods	0.526***
	(0.057)
Observations	456

Standard errors in parentheses

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

Notes: The instrumental variable does not predict the change in neighborhood-level income inequality in the previous period (i.e., before 2000).

Table A13: Neighborhood-Level Placebo Tests of Exogeneity: Lagged Public Goods Provision

	(1) Lagged Sewage Services	(2) Lagged Change in Sewage Services
Migration SSIV	-0.055	0.023
	(0.060)	(0.396)
Baseline Share Poor	0.222***	0.628
	(0.072)	(0.411)
Observations	456	442

Standard errors in parentheses

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

Notes: The instrumental variable does not predict the level or change in public goods provision in the previous period.

A11.4 Neighborhood-Level Tests of Plausibility of Exclusion Restriction

Table A14: Neighborhood Balance Tests of Exclusion Restriction: Respondent-Level Variables

	(1) Race	(2) Education	(3) Household Income	(4) Gender	(6) Age (Categories)	(7) Political Ideology
Migration SSIV	-2.941	-6.577	1.914	1.409	-3.571*	-5.420
	(4.930)	(6.827)	(4.658)	(1.010)	(1.847)	(10.186)
Baseline Controls	5.005***	-5.794***	-4.971***	0.177	-0.256	1.755*
	(0.645)	(0.602)	(0.602)	(0.138)	(0.202)	(0.982)
Observations	3,155	3,937	3,377	3,139	3,937	3,146
Neighborhood Clusters	377	397	391	376	397	386

Standard errors in parentheses

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

Notes: The instrumental variable is balanced on respondent-level demographic control variables.

Table A15: Neighborhood Balance Tests of Exclusion Restriction: Neighborhood-level Features

	(1) Racial Segregation	(2) Gini Coefficient (2010)	(3) Mean Household Income
Migration SSIV	0.220	0.364	7,579.575
	(0.235)	(0.253)	(5,123.160)
Baseline Public Goods	-0.272***	-0.797***	-14,374.906***
	(0.032)	(0.055)	(1005.448)
Observations	456	456	378

Standard errors in parentheses

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

Notes: The instrumental variable is balanced on neighborhood-level control variables.

Table A16: Neighborhood Balance Tests of Exclusion Restriction: Neighborhood-Level Poverty and Inequality

	(1) Share of Population Poor	(2) Gini Coefficient (2010)
Migration SSIV	-508.294	0.364
	(418.161)	(0.253)
Baseline Public Goods	1,161.895***	-0.797***
	(72.095)	(0.055)
Observations	378	456

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

Notes: The instrumental variable is balanced on neighborhood-level poverty and inequality.

A12 Additional Robustness Tests

A12.1 Controlling for Residential Sorting

As discussed, beyond the proposed identification strategy, I also test the robustness of the results to controlling for residential sorting. I use a survey question that asks respondents how long they have lived in their current neighborhood, and I estimate the main results, using only the subset of respondents who have lived in the neighborhood since the pre-period before the decade of the predicted migration SSIV. The results reported below show that even limiting the analysis to respondents who did not sort, the results remain robust.

Table A17: Robustness: Main Results Controlling for Residential Sorting

		Second-Stage:			
	(1) First-Stage: Socioeconomic Integration	(2) Streetlights	(3) Policing	(4) Sewage Collection	(5) Private Security
Migration SSIV	0.636***				
	(0.118)				
Socioeconomic Integration		2.563***	2.342***	2.151**	-0.838***
		(0.965)	(0.801)	(1.074)	(0.311)
Observations	1,858	1,858	1,858	1,858	1,858
Neighborhood Clusters	351	351	351	351	351
Outcome Mean	0.640	4.419	5.813	4.630	0.736
F-statistic	28.962				

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

Notes: The main results remain robustness even among respondents who have lived in their current neighborhood since before 2000 (i.e., those who did not residentially sort).

A12.2 Controlling for Distance to City Center

In some cities, the centrality in the location of each neighborhood is correlated with how segregated or integrated they are. The distance from the city's center may also be correlated with a series of other omitted variables. Therefore, I calculate a measure of the distance of each neighborhood in São Paulo to the city's center to use a control variable. As a robustness check of the main results, I run an estimation where I include the distance to the city's center as an interaction term to observe whether it could be driving the effect of class-based integration on preferences.

To calculate distance to the city center, I use the location of the central business district of the city, made available for São Paulo by (Angel et al. 2010) from the Stanford Digital Repository here: `https://purl.stanford.edu/cx475hg3503`. I used this measure because it fittingly captures the concept of city center as the most busy and crowded spot throughout the day and night. Using ArcMap GIS, I then mark the actual Euclidean center (i.e., "centroid") of the "polygon" (GIS shapefile) that marks the administrative boundary of each neighborhood. Using the central business district and the "centroids" of neighborhoods, I then calculate a distance to city center measure. I replicate the main results, including an interaction of this measure below:

Table A18: Main Results with Interaction with Distance to City Center

	(1) Streetlights	(2) Policing	(3) Sewage Collection	(4) Private Security
Socioeconomic Integration	3.952**	5.597***	3.699*	-0.541
	(2.008)	(1.982)	(2.153)	(0.625)
Integration · Distance to Center	-9.312	-23.848	0.925	0.840
	(18.037)	(15.796)	(20.953)	(5.800)
Distance to Center	2.668	15.164	-6.480	-0.844
	(12.703)	(10.903)	(15.095)	(4.108)
Observations	3,217	3,217	3,217	3,217
Neighborhood Clusters	378	378	378	378

Standard errors in parentheses

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

Notes: The main results remain robustness even when controlling for each neighborhood's distance to the city's center.

A12.3 Addressing Spatial Interdependence using Spatial-2SLS Estimation

As discussed in text, there may be spatial interdependence in the outcome variables (i.e., survey responses), resulting in biased estimates. A complication with the dataset is that the data is hierarchical: examining the effects of neighborhood-level segregation on household survey responses that are nested within neighborhoods. The main analysis uses standard 2SLS estimation with robust standard errors clustered at the neighborhood-level. However, there could be spatial interdependence between households or between proximate neighborhoods that is not accounted for. Betz, Cook, and Hollenbach (2019) demonstrate that such spatial interdependence results in asymptotically biased estimates even when the instrumental variable is randomly assigned. There are several different strategies for addressing this concern. First, I estimate the results while including more flexible geographic controls (e.g., latitude and longitudinal coordinates) (omitted to stay within page count). Second, one could report standard errors corrected for spatial correlation (Conley 1999), or estimate a spatial-lag model.

The main strategy I employ is to estimate a Spatial-2SLS (S-2SLS) model that instruments for both the endogenous predictor and the spatial lag of the outcome variable (see Betz, Cook, and Hollenbach (2019) and Franzese and Hays (2007). The S-2SLS is preferred to alternative methods, because the modified 2SLS nests the standard 2SLS model and the spatial-autoregressive (SAR) model. While the standard 2SLS restricts ρ, the spatial effect, to zero by assumption, this nesting allows for a direct test of this assumption. The nesting ensures that even when no spatial interdependence is present and $\rho = 0$, the S-2SLS model will still recover the same estimates as procured from 2SLS estimation (Betz, Cook, and Hollenbach 2019). Following Betz, Cook, and Hollenbach (2019), I estimate a S-2SLS model, using the spatial lag of the exgenous predictor as the instrument for the spatial lag of the outcome. I use the *spivreg* package in Stata to create a connectivity matrix W based on the geographic proximity (e.g., contiguity) of neighborhoods and estimate the S-2SLS. As Table A19 below shows, with the exception of preferences for streetlights, the main results are robust even when accounting for spatial interdependence using S-2SLS estimation.

Table A19: Robustness: Main Results using Spatial-2SLS Estimation

	(1) Policing	(2) Sewage Collection	(3) Streetlights	(4) Private Security
Socioeconomic Integration	3.008**	4.102*	1.268	-0.856**
	(1.469)	(2.194)	(1.863)	(0.412)
Baseline Public Goods	-0.819	-9.833	-11.477	0.122
	(9.907)	(14.774)	(12.555)	(3.234)
Observations	378	378	378	378
Outcome Mean	5.813	4.630	4.419	0.736

Standard errors in parentheses

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

Notes: Main results for preferences for public and private goods, estimated using spatial-2SLS estimation to correct for spatial interdependence.

A13 Full Results for Figures and Tables

Table A20: Full Results for Table 3: Effects of Integration on Streetlights and Sewer Lines in *Favelas*

	(1) First-Stage	(2) % Streetlights in *Favelas*	(3) % Share Sewer Lines in *Favelas*
Migration SSIV · Uphillness	0.187***		
	(0.045)		
Socioeconomic Integration		0.786**	1.649*
		(0.330)	(0.879)
Migration SSIV	-0.001**	-0.001**	-0.0004
	(0.001)	(0.001)	(0.001)
Uphillness	-1.266***	0.026	8.029***
	(0.171)	(0.375)	(0.867)
Observations	5,492	3,806	5,492
F-stat		13.140	17.104
Outcome Mean	0.187	0.813	0.393

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

Notes: The models control for the two components of the instrument (i.e., the predicted migration SSIV and "hilliness") and include robust standard errors.

Table A21: Full Results for Figure 6: Effects of Integration on the Mechanisms

	(1) Socioeconomic Integration	(2) Concern for Sewage	(3) Fear of Crime	(4) Crime Victimization	(5) Racial Tolerance	(6) Soup Kitchens
Migration SSIV	0.662***					
	(0.113)					
Socioeconomic Integration		2.831**	1.145***	0.317**	0.088	-1.219*
		(1.153)	(0.403)	(0.159)	(0.361)	(0.660)
Observations	3,222	3,218	3,218	3,218	3,218	3,218

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

Notes: The results from Figure 6 in main text presented in table form.

Table A22: Full Results for Figure 7: Campaign Endorsement Questions

	(1) Endorses Candidate (Fear of Crime)	(2) Endorses Candidate (Racial Tolerance)	(3) Endorses Candidate (Social Affinity)
Socioeconomic Integration	1.578**	0.027	-1.327**
	(0.730)	(0.681)	(0.587)
Racial Segregation	1.953*	1.001	-1.264
	(1.102)	(1.046)	(1.010)
Gender	0.095	0.189	0.012
	(0.121)	(0.121)	(0.121)
Age (Years)	0.003	-0.015***	-0.022***
	(0.003)	(0.003)	(0.003)
Education	0.009	0.005	0.046***
	(0.011)	(0.012)	(0.011)
Household Income	-0.004	0.013	0.054
	(0.105)	(0.114)	(0.102)
Race (Skin Color Indicators)	✓	✓	✓
Survey Enumerator Fixed-Effects	✓	✓	✓
Observations	634	622	626

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

Notes: The results from Figure 7 in main text presented in table form. Skin color indicator variables range from 1-11, with 1 indicating fairest skin.

Table A23: Full Results for Figure A3: Causal Mediation Analysis

		Second-Stage:			
	(1) First-Stage: Socioeconomic Integration	(2) Preferences Full Sample	(3) Preferences Spatial Externalities	(4) Preferences Racial Equality	(5) Preferences Social Affinity
Migration SSIV	0.662***				
	(0.113)				
Socioeconomic Integration		2.753***	3.402***	2.354**	1.927
		(0.630)	(0.746)	(0.939)	(1.254)
Observations	3,222	3,217	844	810	793
Neighborhood Clusters	378	378	294	285	287
Outcome Mean	0.640	5.818	5.818	5.818	0.736
F-statistic		34.547	37.139	26.688	23.021

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

Notes: Integration, overall, has a positive effect on preferences for policing in the full sample. Deconstructing this effect in the full sample, the effect size varies according to the randomized mechanism vignette respondents were assigned to. For respondents assigned to the political campaign concerning spatial externalities, the causal effect of integration on preferences for policing is the largest. See Section A6 for more details on the design of the mechanism vignettes.

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