Slums, Income Segregation and Political Behavior: Evidence from Brazil

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Abstract

This chapter studies the effect of income segregation (operationalized as two-group scaled isolation between poor and non-poor) on political behavior. By increasing the level of within income-group interaction, decreasing the level of across income-groups interaction, and changing the spatial distribution of average neighborhood amenities and attributes, income segregation might affect individual political behavior, though the direction of this relationship is theoretically ambiguous. We empirically estimate the effect of income segregation on political behavior using the percentage of the city land covered by ridges and valleys to instrument *spatial variation* in segregation. The instrument builds on the observation that in Brazil slums tend to form in areas of steep slope. I find that that income segregation has a large significant positive effect on the vote-share of the main left-wing party in Brazil and a large significant negative effect on turnout. However, the unusually high magnitude of the second-stage coefficients and the reduced-form estimates with the opposite sign of the second-stage estimates cast doubt on the validity of the exclusion restriction. We propose two feasible and more reliable empirical methods to try to improve identification. First, we will use an original panel data of segregation measures that are comparable across censuses to instrument *temporal variation* in income segregation with heterogeneous responses to (plausibly) exogenous leave-one-out wage-shocks. Second, we will map segregation measures to observable individual-level characteristics at the polling-booth level to estimate the effects of income segregation using plausibly exogenous *within polling place variation* in average segregation experimented by voters *across polling-booths*.

Keywords: slums, income segregation, ideology, turnout, instrumental variables.

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Introduction

We live in an increasingly urban planet. Today, 54% of the planet's population lives in urban areas, a proportion that is expected to increase to 66% per cent by 2050. Projections show that almost 90% of this increase to be concentrated in Asian and African developing countries. Evidence shows that voting behavior is spatially dependent, but has "done surprisingly little to inform modern positive political economy (Rodden, 2010)". The spatial distribution is also a relevant determinant of economic outcomes like corruption and governance (Campante and Do, 2014; Campante, Do, and Guimarães, 2017). Thus, the fact that spatial distribution of voters is a relevant but overlooked topic by political-economics makes causal evidence on the consequences of changes in the spatial distribution of voters of first order importance to understand the economic and political changes induced by the urbanization in developing countries.

Can the spatial distribution of different groups affect their political behavior? The empirical literature has examined how the spatial distribution of ethnic groups affect political choices (Luttmer 2002; Ananat and Washington, 2009; Ichino and Nathan, 2013; Enos, 2015; Sands and de Kadt 2016), but it has being silent when groups are defined by social classes. The objective of this paper is to help to fill this gap by estimating the effect of income segregation on political choices.

The knowledge of the relationship between income segregation can help governments to draw urban policies that empower the poor by alleviating the political constraints generated by their isolation. For example, urban planners can regulate the use of land to increase political participation, descriptive representation, and substantive representation among the poor and, consequently, improve the supply of local public goods in poor neighbors.

Brazil is a suitable context to study the consequences of income segregation. First, the relevant identity for segregation is plausibly socioeconomic instead of racial in Brazil because of limited racial tensions and mixed-race population. second, census evidence shows that slums are commonly located around water basins and topographical accidents. Third, as pointed out by Alves (2017), the housing market for low-income individuals is segmented between *serviced-* and *unserviced-housing* (a less strict definition of slum).

Following the exposition of Graham (2017), we interpret the effects of income segregation on political behavior as a composite of two effects: *peer-effects* and *effects of place*.[1] *Peer-effects*

[1]We do not try to identify only *peer-effects* using instrumental variables because we do not believe on the

are the changes in political behavior caused by different levels of within and across groups interactions. *Effects of place* are the changes in political behavior caused by shifts in the spatial distribution of average neighborhood amenities and neighborhood attributes induced by the reallocation of individuals in the urban space.

The relationship between income segregation and political behavior is ambiguous. First, *peer-effects* have an ambiguous effect on political choices because the mechanisms that mediate the effects of increasing within group interactions induced by the increase in segregation might be different and have opposite effects from the mechanisms that mediate the effects of decreasing across groups interactions induced by the increase in segregation. Second, *effects of place* have ambiguous effects because changes in segregation induce changes in the spatial distribution of a *bundle* of amenities and neighborhood attributes that might have different effects on political behavior. Third, *peer-effects* and *effects of place* might have different signs. Finally, all mechanisms might be heterogeneous by income-groups, making the average effects ambiguous.

Identifying causal effects of income segregation is a challenging inferential problem. Segregation is likely endogenous to political behavior because of simultaneity and omitted variable issues. Simultaneity is a concern because segregation is plausibly a cause and a consequence of political behavior. Omitted variable bias is a concern because across city neighborhood sorting might depend on unobserved preference parameters that are correlated with political preferences. Therefore, to identify the causal effect of income segregation on political behavior we need to find an exogenous source of variation in income segregation.

We propose two different methods to leverage exogenous variation in income segregation. We try to instrument the *spatial variation* in income segregation using the percentage of the city area that is covered by small ridges and valleys, geographic features that predict the presence of slums. This strategy rely on the strong assumption that the presence of small ridges and valleys only affects political preferences trough income segregation. Then, to rely on more plausible exclusion restrictions, we try to instrument the *temporal variation* in income segregation using heterogeneous responses to percentile-specific leave-one-out income-shocks predicted by

plausibility of the identification assumptions in this case. We believe the composite effects is a policy relevant quantity because most counter-factual policies that we can think generate effects of place when group-identities are defined by income-percentiles. First, even assuming income-ordering is fixed, only a restricted set of reallocation do not not generate *effects of place*. Second, the income-ordering is endogenous to the spatial distribution of the population.

the percentage of the city covered by small ridges and valleys conditioning in the income-shock and these geographic variables.

Instrumenting the *spatial variation* in city level in segregation deals with endogeneity issues in two ways. First, since we use variation on income segregation at high levels of aggregation to rule out bias caused by within city sorting. Second, by using only the spatial variation in segregation that is predicted by the geography of the city, we can rule out bias caused by across cities sorting.

Instrumenting the *temporal variation* in city-level variation in segregation deals with endogeneity issues in three ways. First, as in the previous case, we use aggregated indexes of segregation to rule out bias caused by within city sorting. Second, by using segregation measures that are comparable across different censuses, we use city fixed-effects to rule out bias from across cities sorting that is stable over time. Third, by using only the temporal variation that is predicted by heterogeneous responses to leave-one-out income-shocks, we can rule out bias from time-varying across cities sorting.

We measure segregation using the two-group *Normalized Exposure Index*, which is a version of the two-group *Isolation Index* that is independent of the size of the groups. This last property allows us to rely only variation in income segregation that is independent of variation in the size of the income-groups without having to control for them in a regression. Our groups are households whose head earns more-less than half a minimum salary of the base year of each census. We estimate the effect of income segregation on turnout and in the vote-share of the main Brazilian left-wing in presidential elections.

This study is related to with three strands of literature. First, it contributes to the literature that estimate the relationship between segregation and political behavior, a topic that remains understudied. The two notable exceptions are Ananat and Washington (2009) and Sands and de Kandt (2016) who investigate the impact of racial segregation on electoral outcomes in the United States and South Africa, respectively. We differentiate from both by investigating the consequences of income segregation instead of racial segregation. We distinguish from Ananat and Washington (2009) by studying the effects of income segregation in the context of a developing country. In contrast with Sands and de Kandt (2016), who instrument segregation at the electoral ward level, we instrument segregation at more aggregate levels (equivalents of MSA's and commuting zones), an strategy that is robust to within-city sorting bias.

Second, the paper connects with broader literature that estimates causal effects of segregation in economic outcomes (Cutler and Gleaser, 1997; Cutler, Gleaser, and Vigdor, 2007; Ananat and Washington, 2009; and Ananat, 2011). We distinguish from them, who instrument across cities variation in segregation, by using using heterogeneous responses to group specific leave-one-out shocks to instrument *temporal variation* in segregation.

Third, it is related to the economics and political science literature that studies the relationship between local demography and political behavior (Cutler, Elmendorf and Zeckhauser, 1993; and Luttmer, 2002; Ichino and Nathan, 2013; Enos, 2015; and Nathan, 2016). I distinguish from them by investigating the political consequences of local socioeconomic instead of local racial demography. We contrast from Ichino and Nathan (2013) and Nathan (2016) by instrumenting segregation instead of using local demography directly as a regressor, an identification strategy explicitly takes care of the within city sorting.

Section 1 of the paper presents background information about race and identity in Brazil and some features of Brazilian politics. Section 2 discusses how socioeconomic segregation can affect political choices. Section 3 describes the data and the main variables. Section 4 discusses the empirical strategy used in this paper. Section 5 present the results for the models instrumenting spatial variation in segregation. I conclude with a discussion of one possible next steps of this research project.

1 Background

Worker's party voting as a proxy for left-wing preferences. The main left-wing party in Brazil is the *Partido dos Trabalhadores*, popularly known by the acronym PT. The party was created in 1980 by a strong union movement of workers from the state of São Paulo with the help of intellectuals, exiled politicians, and the progressive part of the Catholic church. PT adopted a democratic version of socialist ideas in the first years of its existence, but changed to a more socially democratic agenda praising income redistribution with respect to institutions and contracts in the late 90's. PT is arguably the only party with popular basis and organized militancy in Brazil.

The party had presidential candidates in all federal elections in the new Republic. Luís Inácio da Silva, popularly known as Lula, was the runner-up of 1989, 1994, and 1998 elections and was elected president in 2002 and 2006. Dilma Roussef was PT candidate in 2010 and

2014 and won both elections. We believe the PT vote-shares in Brazilian presidential elections from 1989 to 2014 are a good proxy for the intensity margin of left-wing preferences at local level.

Race and Identity in Brazil. In Brazil, the poor are disproportionally black and mixed-race and the non-poor are disproportionally white. In this context, it is difficult to distinguish socioeconomic segregation from racial segregation.

Despite the difficult to distinguish between income- and racial-segregation in Brazil, four stylized facts suggest that the relevant identity for segregation is socioeconomic instead of racial. First, racial identities are less salient in Brazil because the majority of its African descendant population is mixed-race.[2] Second, historical evidence says that black slaves interacted and lived close to free man in the cities and continued to live after the end of slavery. Third, Brazil has no history of racial segregation policies as the ones observed in South Africa and United States.[3] Finally, Brazil has limited racial tensions in comparison with other multi-ethnic countries.

These facts support the hypothesis that the spatial separation of blacks and whites in Brazil is a byproduct of inherited initial economic conditions that perpetuated over time instead of deliberated policies of racial separation or willingness to overpay to live in racially homogeneous communities.[4] In other words, in Brazil, racial segregation is plausibly a consequence of socioeconomic segregation not a cause.

2 How income segregation affect political choices?

We follow the exposition of Graham (2017) and interpret the effects of income segregation affects on political choices as a composite of *peer-effects* and *effects of place*. *Peer-effects* are changes in political behavior caused by different levels of within and across groups interactions. *Effects of place* are changes in political behavior caused by shifts in the spatial distribution of average neighborhood amenities and neighborhood attributes induced by the reallocation of individuals

[2]According to the 2010 census, 43.1% of the population declared themselves as mixed-race while only 7.6% themselves declared themselves as black.

[3]Scholars coined the term "racial democracy" to describe the (exaggerated) idea that Brazil is a society without a "color line" and without legal barriers to people of color rising to official posts or positions of wealth and prestige. The origin of the term is attributed to Wagley (1952): "Brazil is renowned in the world for its racial democracy".

[4]Our interpretation do not exclude the possibility that Brazilians are willing to pay to live in economically homogeneous communities.

in the urban space. This section is organized around this classification: the first sub-section list plausible theoretical mechanisms that would mediate *effects of place* on political participation and ideology; the second sub-section does the same for *peer-effects*.

2.1 Effects of place

Negative externalities. The presence of negative neighborhood amenities and undesirable neighborhood attributes (e.g., lack of sewage, light and garbage collection) is correlated with usually correlated with the presence of poor households in the neighborhood. For instance, only 56.33% of households in illegal settlements are connected to the sewage system in Brazil. Poor households might also have illegal connections to the electric and water networks, what generates material disutility to the legal clients.

The degree by which the negative externalities and material disutility generated by the poor affect the political behavior of the non-poor depends on the degree of spatial segregation of the two income-groups. If, non-poor individuals live in segregated cities, they have, in average, limited exposition to the negative neighborhood amenities and neighborhood attributes generated by the poor and, consequently, smaller incentive to demand public investments and distributive policies that alleviate the losses from generated by negative externalities and material disutility.

Crime. We expect crime to be a major mediator of *effects of place* because of the high levels of crime in Brazil.[5] The *effects of place* mediated by crime have ambiguous effects on voting behavior. First, the relationship between income segregation and average level of crime is unclear. Second, the relation between exposure to crime and political ideology is plausibly ambiguous. More exposure to crime might increase demand for redistribution, a left-wing agenda, but also increase demand for punishment, usually a right-wing agenda.[6]

Third, income segregation might affect the distribution of exposure to crime even without

[5] In 2012, the United Nations Office on Drugs and Crime ranked Brazil as the number one country worldwide in absolute number of homicides, with over 50,000 cases, and as the 18th place in terms of homicide rates, with 25.2 homicides per 100,000 inhabitants.

[6] An ongoing debate about the responsibility age for violent crimes shed light on how exposure to criminality generates demand for punishment. According to recent opinion polls, 87% of the Brazilians support the law project that reduces the responsibility age for violent crimes from 18 to 16 years despite lack of evidence about the efficacy of the policy.

changing average levels of crime. The average political will change in this case if income-groups respond differently to exposure to crime. Finally, income segregation might change political behavior even without changing any feature of the distribution of crime. For a given level of crime, segregation might change demand for punishment by changing beliefs about the reasons behind criminal behavior. More interaction with the poor might increase the perception of the non-poor that crime is a consequence of scarcity and increase demand for redistribution. Alternatively, it might increase the perception that crime is a consequence of personality traits and, thus, increase demand for punishment.

2.2 Peer-effects

Biased beliefs Classic models predict that demand for redistribution depends on individuals' position in the income ordering (Romer, 1975; Meltzer and Richards, 1981), income inequality (Alesina and Giuliano, 2010), and social mobility (Benabou and Ok, 2001). An implicit assumption of these models is that people hold correct information about their position in the income distribution and about features of the income generating process. However, this implicit hypothesis is often rejected empirically.[7] Part of the same evidence shows that the individual demand for redistribution correlate with the *perceived moments* of the income instead the *observed moments*. This evidence suggests that income segregation should affect demand for redistribution if it change the *perceived moments* of the income distribution.

Cruces, Perez-Truglia, and Tetaz (2013) suggest a mechanism by which income segregation affect beliefs about moments of the income distribution: inferences based on reference groups that differ from the whole population. Therefore, income segregation should affect the *perceived moments* of the income-distribution by making individuals choose reference groups that differ from the whole population.

If the non-poor have preferences for interacting with in-group peers and form reference groups based on their income without adjusting for the biased information set, they will consider themselves poorer than in reality and, consequently, demand more redistribution. Then, the demand of redistribution of the non-poor will be increasing in their level of segregation. By the

[7] Research shows that individuals have biased beliefs about its own position on the ranking (Cruces, Perez-Truglia, and Tetaz 2013; Engelhardt and Wagener, 2014; Gimpelson and Treisman, 2015; Karadja, Mollerstrom, and Seim, 2016), income inequality (Norton and Ariley, 2011; Gimpelson and Treisman, 2015), average income (Chambers, Swan, and Heesacker, 2014), and relative income (Grigorieff and Roth, 2016)

same logic, the poor will consider themselves richer than in reality and, consequently, demand less redistribution

More within group and less across groups interaction might also reduce information about the income of out-group members and induce people to believe that the rest of society is similar to them. In this case, more segregated poor and non-poor individuals would underestimate the true level of income inequality and, consequently, demand less redistribution if they are inequality averse.

Biased beliefs can also affect the decision of voting. In the calculus of voting framework of Downs (1957), Tullock (1967), and Riker and Ordeshook (1968), a citizen has probability p of being the pivotal voter in the election, a benefit B if his preferred candidate wins the election, a direct utility D from the act of voting (e.g., adhering to an ethical standard), and a cost C from voting (e.g., time and transportation cost). A citizen votes if and only if $pB + D > C$, that is, if the expected benefits of voting exceed its costs. An implicit assumption in this model is that people hold correct information about the cost-benefit parameters. p, B, D, and C. Therefore, income segregation will affect political participation if it affects the *perceived* values of p, B, D, and C.

Out-group feelings. Allport's (1979) seminal work on *contact* and *threat* explains how increasing across groups interactions affect inter-group feelings. Repeated *contact* under certain conditions with a new group may improve an individual's disposition toward members of the new group. However, a sense of *threat* might emerge with exposure to the new group, translating into a negative disposition toward its members.

Therefore, increasing across group interaction might increase or decrease the sense of group identity depending on weather individuals individuals feel threatened or not with the out-group contact. Lower income segregation of the poor should generate positive out-group feelings in the non-poor and increase their demand for redistribution in contexts where *non-threatening* (e.g., cities with low crime rates) is more likely.

Group identification. Recent papers (Shayo, 2009; Klor and Shayo, 2010) show that support for redistribution depends on which group individuals choose to identify with and extract utility from having similar outcomes. These models predict that the poor will demand more redistribution if they choose to identify with the other poor (namely, if they choose *class identification*) and demand less redistribution if they choose to identify with the whole country (namely, if they choose *nation identification*). Based on these models, income segregation of the poor will change their demand for redistribution if it changes their decisions of *group identification*.

Situational identity theory (Posner, 2004; Eifert et al., 2010) suggests that identities (e.g., ethnic, racial, social) are subject to contextual activation, that is, similar individuals might identify with different groups depending on the context. Given the well documented empirical support for this theory, it is plausible that income segregation affects the salience of socioeconomic identities by changing the local context, which might influence decisions of *group identification* and, in turn, change demand from redistribution.

Peer pressure. DellaVigna et. al. (2016) propose a simple explanation for why individuals vote: it is because others will ask. If individuals care about what others think of them, they may derive pride from telling others that they voted and fell shame or guilt from admitting that they did not vote.

This mechanism might be amplified by increasing across income-groups interactions. If the non-poor individuals care more about the political participation than the non-poor and the non-poor care about what non-poor say about them, more interaction between different income-groups will increase peer pressure from the non-poor to the poor and increase their average political participation. Based on this logic, cities with higher income segregation should have lower turnout rates.

3 Data

Our sample. Our unit of analysis is the *Minimum Comparable Area (MCA)*, which consist of sets of municipalities whose borders were constant over the study period. We use the MCAs of 1991, the first year of our analysis. We restrict the analysis to MCAs of 1991 that have at least 50 thousand inhabitants in 2010. Our sample has 647 observations, but we are temporarily restricted to 641 observations.[8]

We compute segregation indexes, controls, and income shocks using data form the 1991,

[8]Some geographical variable have long computational time for some large AMCs.

2000, and 2010 censuses. Geographical variables are computed from raster files of land elevation using the polygons of the 1991 AMCs. We use electoral data from 7 presidential elections from 1989-2014.

Making census units comparable across time. The smallest unit of observation with observable income-group counts for the universe of the Brazilian population is the *Enumeration Area (EA)*.[9] An EA is a set of nearby housing units to be visited by a census interviewer.

Unfortunately, we cannot compute segregation indexes that are comparable across time using EAs as input unit because EAs are not fixed across censuses. To solve this issue, Mation (2010) developed an innovative method based on graph-theory to produce *Minimum Comparable Areas for Census Enumeration Areas (MCAEAs)*. The input unit of our segregation indexes is the MCAEA of the 1991 census produced by the method of Mation (2010).

Measuring city level income segregation. We measure income segregation using population census data with income-group counts of number of household heads inside observable income-bins. The only variable that is available for 1991, 2000, and 2010 has income-bins based on the household-head income measured in nominal minimum wage of the census reference-month. This variable is admittedly a problematic measure household income because it does not take into variation in economic conditions coming from household-size and female participation in the labor market. We plan to deal with these issues later on by controlling for these variables in the sensibility analysis of results.

In our analysis, the minority group are poor-households whose head earns less than half a minimum salary of the base year of each census. We measure income segregation using the η^2-*Index*, which is given by

$$N = \frac{I - Q}{1 - Q}$$

where

$$I = \sum_{j=1}^{J} \frac{\#poor_j}{\#poor} \cdot \frac{\#poor_j}{\#poor_j + \#non\_poor_j}$$

is the two-group *Isolation Index*, $Q = \frac{\#poor}{\#poor + \#non\_poor}$ is the fraction of poor households in the city, $\#poor = \sum_{j=1}^{J} \#poor_j$ and $\#non\_poor = \sum_{j=1}^{J} \#non\_poor_j$ are, respectively, the total

[9](In Portuguese, *Setor Censitário*. To put this unit of analysis in perspective, the EAs of the Brazilian census are smaller than the census tracts but larger than the census blocks of the USA census.

number of poor and non-poor households in the city, $\#poor_j$ and $\#non\_poor_j$ are, respectively, the number of poor and non-poor households in EA j.

The η^2-Index measures "the excess isolation of the minority in a city compared to perfect integration relative to the corresponding excess isolation that would be observed in a perfectly segregated city (Graham, 2017)". This index is a *scaled* measure of minority isolation, that is, a measure that do not depend on the size of the minority-group. This property allows us to estimate the coefficients to relying only on variation in income segregation that is independent of variation in the size of the income-groups without having to control for them in a regression.

Geographic instrument. The geographic instruments are computed from the ASTER Global Digital Elevation Model (NASA and METI, 2011), that provides land elevation data with a resolution of 90 meters. We use the land classification method of Weiss (2001) that, given a definition neighborhood, classifies the land according to the standardized terrain position of a raster tile and its land-slope in degrees.[10]

A tile is classified as a *valley* if its standardized-TPI is smaller than one-standard deviation of the across tiles TPI distribution and it is classified as a *ridge* if its standardized-TPI is larger than one-standard deviation of the across tiles TPI distribution. Our instrumental variable for the spatial variation of income segregation is the percentage of the land surface that is classified as a *valley* or a *ridge*. We use only the 8 nearest tiles as neighbors of a tile. Given the 90m resolution of our raster, this neighborhood concept captures the presence of ridges and valleys of around 200 meters of radius. We believe this is an appropriate neighborhood concept because the descriptive evidence shows that slums are located around small scale geographical accidents.

[10]The Terrain Position of tile rc in row c and column r is given by $TP_{rc} = \sum_{i=r-1}^{r+1} \sum_{j=c-1}^{c+1} (e_{rc} - e_{ij})$, where e_{rc} denotes elevation value attached to tile in the r-row and to the c-column. The standardized Terrain Position of tile rc is given by $STP_{rc} = \dfrac{TP_{rc} - E[TP_{rc}]}{E[SD_{rc}]}$.

4 Identification

Consider the following reduced-form model:

$$y_c = \alpha_{state} + \beta segregation_{g,c} + u_c$$

where $y_{c,t}$ is the electoral outcome of city c, $segregation_{g,c}$ is the two-group segregation measure of individuals inside-outside income-group g of city c, α_{state} is a state fixed-effects, and u_c is the error-term of city c. We do not use controls because income segregation is highly endogenous to most demographic variables. To avoid controlling for the size of the income-groups, we use an income segregation measure that does not depend on the size of the minority-group.

We identify the effects of income segregation in electoral outcomes by leveraging exogenous variation from the geographical accidents that reduce the cost of illegal constructions. Our instrument builds on census descriptive evidence showing that slums are usually located around geographical accidents, like water basins and areas of steep slope, such as valleys and ridges. Our instrument, named $perc\_valleys\_ridges_c$ is the percentage of the land area that is covered by valleys or ridges. The first stage is given by

$$segregation_{g,c} = \alpha_{state} + \beta perc\_valleys\_ridges_c + v_c.$$

The exclusion restriction imposes that the presence of small valleys and ridges do not have any direct effect on political behavior that is not trough its effect on income segregation. This is admittedly a strong assumption because geography plausibly affects demography and there is a well documented relationship between demographic variables (e.g., population density, urbanization rates) and political behavior (see Rodden (2010)). We are exploiting variation in land shape in a very small scale (radius of around 200m) in order to minimize any direct impact of geography on political choices.

The *exogeneity assumption* implies that the presence of valleys and ridges is uncorrelated with the part of political preferences that is not explained by segregation. Our instrument is unlikely to be simultaneously determined with political preferences because valleys and ridges are difficult to be changed by human action. Omitted variable bias can be an issue if individuals with different political preferences sort across cities depending on their geographic characteristics. However, it is not evident that migrants are informed about small scale geography of destination cities or that the presence of small ridges and valleys is a relevant variable in this decision.

5 Results

Estimates using across cities variation. Table I shows the estimates of the relationship between income segregation and PT vote-share in presidential elections using *across cities variation* in income segregation. All regressions include state fixed-effects and use robust standard-errors. Column (1) shows a positive and significant at 1% partial correlation between the PT vote-share in the 2010 presidential elections and income segregation. The partial correlation has a magnitude of .145 standard-deviations.

Column (3) shows a negative effect and significant at 1 percent of the percentage of the land cover covered by small valleys and ridges on income segregation. The effect has a magnitude of −.137 standard-deviations and explains 2.5 percent of the within state variation in income segregation. The instrument passes the standard tests of underidentification and weak instruments.[11]

IV estimates in column (4) shows a positive and significant at 10% effect of income segregation in the PT vote-share in the 2010 presidential election. The IV estimates reveal a very large negative bias in the OLS estimates in column (1): the coefficient of income segregation almost triples in magnitude. The effect on column (4) is of very large magnitude: a one standard deviation increase in income segregation generates an increase of .386 on the PT vote-share in the 2010 presidential election.

Unfortunately, reduced-form estimates in column (2) cast some doubt in the validity of the IV estimates in column (4). The percentage of the city covered by valleys and ridges has a

[11]The first diagnostic tool for assessing the strength of identification is based on a Langrange Multiplier (LM) test for underidentification using the Kleibergen and Paap (2006) rk statistic. The null hypothesis being tested means the matrix of correlations between instruments and regressors is rank-deficient, which is equivalent to having an underidentified model. The model of column (3) has a rk LM-statistic of 11.32 and a p-value of 0.0008, allowing us to reject the hypothesis of underidentification.

The second diagnostic is based on the Stock and Yogo (2005) characterization of weak instruments using the the robust analog of the Cragg-Donald Wald statistic, the Kleibergen-Paap rk Wald statistic. The null hypothesis being tested is that the estimator is weakly identified in the sense that it is subject to bias that the investigator finds unacceptably large. The rk Wald F-statistic of the model of column (3) is 11.68, above the "rule of thumb" of Staiger and Stock (1997), which says that the F-statistic should be at least 10 for weak identification not to be considered a problem. The critical value of the Stock and Yogo (2005) for one instrument, one endogenous regressor, and a desired maximal size of 15% for a 5% Wald test is 8.96. The rk Wald F-statistic in column (3) is also above this critical value, providing evidence against the hypothesis of weak instruments.

negative and significant at 1% effect in the PT vote-share in the 2010 presidential elections. The effect has a magnitude of .053 standard deviations. The reduced-form estimates has the opposite sign that we would expect if geography has no direct on political outcomes, suggesting a plausible large direct negative effect of the presence of ridges and valleys on political outcomes.

Table II shows the estimates of the relationship between income segregation and turnout in the 2010 presidential elections using *across cities variation* in income segregation. All regressions include state fixed-effects and use robust standard-errors. Column (1) shows a positive and significant at 1% partial correlation between turnout in the 2010 presidential election and income segregation. The partial correlation is of large magnitute: $-.23$ standard-deviations.

IV estimates in column (4) shows a negative and significant at 10% level effect of income segregation on turnout in 2010 presidential election. The IV estimates reveal a very large negative bias in the OLS estimates in column (1): the coefficient of income segregation is more than six times larger in absolute value. The effect on column (4) is of very large magnitude: a one standard deviation increase in income segregation generates an increase of 1.38 standard-deviations on turnout. Unfortunately, this unusually high magnitude of the second-stage coefficients cast doubt on the validity of the identification assumptions.

Moreover, reduced-form estimates in column (2) also cast some doubt in the validity of the IV estimates in column (4). The percentage of the city covered by valleys and ridges has a positive and significant at 1% effect on turnout. The effect has a magnitude of .191 standard-deviations. The reduced-form estimates has the opposite sign that we would expect if geography has no direct on political outcomes, suggesting a large direct positive effect of the instrument on the outcome.

6 More reliable empirical strategies

6.1 Instrumenting temporal variation in segregation

Consider the following reduced-form model

$$y_{c,t} = \alpha_c + \alpha_t + \beta segregation_{g,c,t} + \gamma \mathbf{X}_{c,t-1} \cdot t + u_{c,t}$$

where $y_{c,t}$ is the electoral outcome of city c at year t, $segregation_{g,c,t}$ is the two-group segregation measure of individuals inside-outside income-group of city c at year t, α_c and α_t are city and year fixed-effects, $\mathbf{X}_{c,t-1} \cdot t$ is a vector of controls of city c at year $t-10$ interacted with a linear trend, and $u_{c,t}$ is the error-term of city c at year t. It is convenient to write the previous model in first-differences:

$$\Delta y_{c,t} = \alpha + \beta \Delta segregation_{g,c,t} + \gamma \mathbf{X}_{c,t-1} + \Delta u_{c,t}$$

This specification deals with the endogeneity of segregation in several ways. First, it rules out within city sorting bias because income segregation income segregation is measured at city level. Second, it includes city fixed-effects to exclude the possibility of bias from across cities sorting that is stable over time. Third, it has time fixed-effects and lagged controls in the trend control for linear and non-linear confounding trends that are correlated with the outcome.

This model, however, do not deal with bias coming from time-variant sorting across cities. To solve this issue, we leverage variation from heterogeneous responses to income-group specific leave-one-out wage-shocks predicted by the presence of geographical accidents that predict the presence of slums, conditioning in the shock and in the source of heterogeneity.

The first-stage for this specification is given by:

$$\Delta segregation_{g,c,t} = \alpha + \beta_1 \Delta W_{g,c,t} + \beta_2 geography_c +$$

$$+\beta_3 \Delta W_{g,c,t} \cdot geography_c + \gamma \mathbf{X}_{c,t-10} + \Delta u_{c,t}$$

where $\Delta W_{g,c,t}$ is the leave-one-out income-shock received by group g in city c between census years t and $t-10$.

Our instrument builds on two pieces of evidence. First, census descriptive evidence shows that slums are usually located around geographical accidents, like water basins (e.g., lakes and ponds), areas of steep-slope (hills and valleys), and transportation lines (e.g, highways and railways). Second, Alves (2017) shows that the housing market for low-income individuals is segmented in *serviced* and *unserviced* housing (a proxy for slums) and that a leave-one-out shocks for low income individuals is a relevant instruments for the unserviced housing demand.

The first term in our instrumental variable is the Bartik (1991) wage shock for low income individuals used by Alves (2017), defined as

$$\Delta W_{g,c,t} = \sum_{s \in S} (\ln W_{g,s,-c,t} - \ln W_{g,s,-c,t-10}) \cdot \frac{L_{g,s,c,t-10}}{L_{g,c,t-10}}$$

where $W_{g,s,c,t}$ is average wage of income-group g in sector s in city c at year t, $W_{g,s,-c,t}$ is the average wage of income-group g in sector s in the whole country excluding city c at year t,

$L_{g,s,c,t}$ is number of workers of income-group g in sector s in city c at year t, and $L_{g,s,-c,t}$ is the number of workers of income-group g in sector s in the whole country excluding city c at year t. The second term $geography_c$ is the percentage of the total land of city c that is covered by water basins (e.g., rivers and lakes) and areas of steep slope (e.g, valleys and ridges).

The exclusion restriction of geographic variable is more more reliable when instrumenting trends than levels of income segregation because geography is plausibly related to stable demographic features of the city that affect political variables (e.g., population density). Leave-one-out income-shocks are plausibly exogenous when the spatial distribution of economic activities is measured before the shock but plausibly have a direct effect on political choices by changing economic conditions. Therefore, to rely in plausible exclusion restrictions, we will leverage variation only from heterogeneous responses to these shocks explained by within state geographic characteristics of the city.

The intuition behind the instrument is the following: the housing market for low income individuals of nearby cities should respond differently to *the same* low income wage-shock depending on the availability of accidents that predict the presence of slums (and plausibly increase the benefits of this type of construction).

6.2 Exploiting plausibly exogenous within polling place variation in segregation

The previous regression models can be extended to exploit *across income-groups variation* in income segregation. Consider the following extension of the cross sectional regression model:

$$y_{g,c} = \alpha_g + \alpha_c + \beta segregation_{g,c} + u_{g,c}$$

where $y_{g,c}$ is the outcome of income-group g in city c, $segregation_{g,c}$ is the two-group segregation measure of individuals inside-outside income-group g of city c, α_g and α_c are income-group and city fixed-effects, and $u_{g,c}$ is the error-term of income-group g in city c.

This specification deals with the endogeneity issues in several ways. First, it rules out bias from within city sorting because income segregation is measured at aggregate city level. Second, since we include income-group fixed effects, we can exclude the possibility of bias from non-differential sorting across cities (that is, bias from across cities sorting that do not vary within income-groups). Third, it includes city fixed-effects to exclude the possibility of bias from differential across cities sorting that do not vary within city.

This extended specification, however, introduces issues on measurement and endogeneity. First, since we cannot observe the income-group of voters, average outcomes are not observable at the income-group level. Second, even if we would observe the income-group of each voter, the model can only be estimated for turnout measures because individual electoral outcomes are confidential by law. Third, since income-group of each voter depends on the realization of a variable that is endogenous to segregation (household head income), the income-group fixed-effects introduce additional endogeneity to the regression.

Special features of the Brazilian electoral system help us to deal with these additional issues. First, since there is a detailed data set with characteristics of voters and polling-booth identifiers, the income-group of each voter can be predicted out-of-sample using a Mincerian-style regression on the observable controls and used to compute the percentage of voters by income-group in each polling-booth.[12] Second, since there is a detailed data set with electoral outcomes at the polling-booth level, regression models at the polling-booth level can used to estimate parameters of regression model at the individual-level outcomes under certain conditions. Finally, since voters are sequentially assigned to polling-booths within a polling-place by rule, we expect the *within polling-place variation across polling-booths* of the percentage of voters in each income-group to be as good as random.[13]

Consider the previous regression model but with outcomes at the individual level and polling-place fixed effects:

$$y_{i,g,c} = \alpha_p + \alpha_g + \beta segregation_{g,c} + u_{i,g,c}$$

where $y_{i,g,c}$ is the outcome of voter i of income-group g in city c, $segregation_{g,c}$ is the two-group of individuals inside-outside income-group g of city c, α_g and α_p are income-group and polling-place fixed effects.

The N voters are assigned to B observable polling-booths of size N_1, ... , N_B by the electoral authority. Averaging the previous regression model for all voters assigned to polling-booth b we

[12]Polling-booth is associated with the room where each voter goes to vote inside a polling-place and must have, by rule, from 50 to 500 voters in Brazil.

[13]In Brazil, voters can choose the polling-place they prefer within their electoral district of residence, but cannot choose polling-booths within the chosen polling-place. According to the Electoral Supreme Court, voters are sequentially assigned to polling-booths within a polling-place in order to ensure randomness and balance the number of voters between polling-booths within each polling-place.

get

$$\frac{1}{N_b}\sum_{i\in b} y_{i,g,c} = \alpha_{p.p.}\cdot\frac{1}{N_b}\sum_{i\in b}\mathbf{1}(i\in p) + \alpha_g\cdot\frac{1}{N_b}\sum_{i\in b}\mathbf{1}(i\in g) +$$
$$+\beta\cdot\frac{1}{N_b}\sum_{i\in b}\mathbf{1}(i\in g)\cdot segregation_{g,c} + \frac{1}{N_b}\sum_{i\in b} u_{i,g,c}$$

where N_b is the number of voters in polling-boot b, $\mathbf{1}(i\in p)$ is a dummy variable that equals 1 when individual i belongs to polling-place p, and $\mathbf{1}(i\in g)$ is a dummy variable that equals 1 when individual i belongs to income-group g.

Since, by construction, $\frac{1}{N_b}\sum_{i\in b}\mathbf{1}(i\in p) = 1$, the previous model can be rewritten as

$$y_{b,c} = \alpha_p + \sum_{g\in G}\alpha_g \%(i\in g)_b + \beta segregation_{b,c} + u_{b,c}$$

where $y_{b,c} := \frac{1}{N_b}\sum_{i\in b} y_{i,g,c}$ is the average outcome of polling-booth b in city c, G is a set of income-groups chosen by the the econometrician, $\%(i\in g)_b := \frac{1}{N_b}\sum_{i\in b}\mathbf{1}(i\in g)$ is the fraction of voters of income-group g in polling-booth b, $segregation_{b,c} := \frac{1}{N_b}\sum_{i\in b}\mathbf{1}(i\in g)\cdot segregation_{g,c}$ is the average segregation experienced by voters of polling-booth b, and $u_{b,c} := \frac{1}{N_b}\sum_{i\in b} u_{i,g,c}$ is the error term of the model at the polling-booth level.

This specification deals with additional endogeneity issues. First, since *within polling-place variation across polling-booths* of voters characteristics is plausibly exogenous, we can control for the share of voters in each income-group and other average voters' characteristics without worrying about *bad control problems*. Second, since we include polling-place fixed effects, we can rule out bias from differential across cities sorting that do not vary within polling place. In other words, the model can estimate the causal effects of segregation if differences in non-observable variables across income-groups in a city caused by differential across cities sorting do not vary within polling-place. The interpretation of the effects should also change because polling-place fixed effects net out political effects of income segregation that are mediated by *effects of places* that do not vary within polling-place.[14]

This regression model can be extended even further to control flexibly for income-group characteristics that vary within polling-place. In this case, the specification is given by:

$$y_{b,c} = \sum_{g,p\in G\times P}\alpha_{g,p}\%(i\in g)_{b,p} + \beta segregation_{b,c} + u_{b,c}$$

[14] Feedback on the credibility and potential of this empirical strategy is specially welcome.

where P is the set of all polling-places in Brazil.

However, to use this *within polling-place variation* to estimate the effects of income segregation, we need to map *individual-level variation* in income segregation that depends on individual characteristics that can averaged at the polling-booth level. We currently observe birth date, gender, marital status and education of voters together with the polling-booth identifier.[15] In the previous regression model, segregation varies across individuals according to their income-group status, that is predicted by the observable characteristics and electoral district status.

There are another individual-level characteristics that can predict *individual-level variation* in income segregation. City level also segregation varies over time. Therefore, individuals of the same city but of different age-cohorts are exposed to different levels of segregation *at a given age*. Evidence shows that individual exposure to negative income-shocks and high levels of inequality during the *impressionable years* (between 18 and 25 years old) change the political behavior of individuals in adulthood (Giuliano and Spilimbergo, 2009; Roth and Wohlfart, 2017). Then, one natural solution is assigning each age-cohort to the segregation index of that city measured when the cohort was in the *impressionable years*. In this case, the coefficient of segregation measures the effect of *income segregation during impressionable* years on political outcomes.

Another potential source of variation income segregation is the place of residence. Recent advances on the measurement of segregation (Echenique and Fryer, 2007; Mele, 2012) allow the researchers to measure segregation at the individual level. Mele (2012) also shows evidence from United States that encourages analysis of the effects of segregation at less aggregate units of observation. First, the within city distribution of segregation is highly left-skewed, showing that *high average segregation* are driven by few very segregated units. Second, instrumental variable estimates using *median segregation* instead of *average segregation* are never significant, showing that average effects of segregation are driven by these few very segregated areas.

In our case, we can compute income segregation at the EA level. If we observe the address of each voter, we could geolocalize them and attach each address to an EA and its segregation level.[16] If we also observe the polling-booth identifiers, we could compute the *average small*

[15]Marital status and education of voters are measured with error because they are computed at moment of registration are not updated unless the voter change polling-place.

[16]The website of the National Electoral Catalog (in Portuguese, *Catálogo Nacional de Eleitores*) says the electoral authority has access to the registration address of all voters in the country. We will be applying for this information in the following days. This is arguably confidential data, but they recently gave researchers access to

scale segregation experienced by voters across polling-booths and apply the empirical strategy described in this sub-section.

information that also has confidential status: the birth date of each voter.

Table I: cross sectional estimates, PT vote-shares

	(1) Vote-share PT president	(2) Vote-share PT president	(3) Normalized exposure	(4) Vote-share PT president
Normalized exposure	0.827*** (0.177)			2.191* (1.231)
Percentage ridge or valley		-0.164* (0.0960)	-0.0747*** (0.0219)	
Standardized effect	.145	-.053	-.137	.386
Observations	641	641	641	641
R-squared	0.643	0.631	0.334	
State FE	YES	YES	YES	YES

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table II: cross sectional estimates, turnout

	(1) Turnout	(2) Turnout	(3) Normalized exposure	(4) Turnout
Normalized exposure	-0.396***			-2.342***
	(0.0704)			(0.709)
Perc. ridge or valley		0.175***	-0.0747***	
		(0.0382)	(0.0219)	
Standardized effect	-.235	.191	-.137	-1.388
Observations	641	641	641	641
R-squared	0.35'	0.342	0.334	
State FE	YES	YES	YES	YES

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

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