PhD THESIS DECLARATION
(use a pc to fill it out)

I, the undersigned

FAMILY NAME	Cavgias Martins Fraga
NAME	Alexsandros
Student ID no.	1759794

Thesis title:

Essays on Political Economy

PhD in	Economics
Cycle	29th
Student's Advisor	Eliana La Ferrara
Calendar year of thesis defence	2019

DECLARE

under my responsibility:

1) that, according to Italian Republic Presidential Decree no. 445, 28th December 2000, mendacious declarations, falsifying records and the use of false records are punishable under the Italian penal code and related special laws. Should any of the above prove true, all benefits included in this declaration and those of the temporary "embargo" are automatically forfeited from the beginning;

2) that the University has the obligation, according to art. 6, par. 11, Ministerial Decree no. 224, 30th April 1999, to keep a copy of the thesis on deposit at the "Biblioteche Nazionali Centrali" (Italian National Libraries) in Rome and Florence, where consultation will be permitted, unless there is a temporary "embargo" protecting the rights of external bodies and the industrial/commercial exploitation of the thesis;

3) that the Bocconi Library will file the thesis in its "Archivio Istituzionale ad Accesso Aperto" (Institutional Registry) which permits online consultation of the complete text (except in cases of temporary "embargo");

4) that, in order to file the thesis at the Bocconi Library, the University requires that the thesis be submitted online by the student in unalterable format to Società NORMADEC (acting on behalf of the University), and that NORMADEC will indicate in each footnote the following information:

- PhD thesis (*write thesis title*) **Essays on Political Economy;**
- by (S*tudent's family name and name)* **Cavgias Martins Fraga**, **Alexsandros**;

- defended at Università Commerciale “Luigi Bocconi” – Milano in the year (year of defence) …2019….. ;
- the thesis is protected by the regulations governing copyright (Italian law no. 633, 22nd April 1941 and subsequent modifications). The exception is the right of Università Commerciale “Luigi Bocconi” to reproduce the same, quoting the source, for research and teaching purposes;

- **only when a separate “Embargo” Request has been undersigned**: the thesis is subject to “embargo” for (indicate duration of the “embargo”) …… months;

5) that the copy of the thesis submitted online to Normadec is identical to the copies handed in/sent to the members of the Thesis Board and to any other paper or digital copy deposited at the University offices, and, as a consequence, the University is absolved from any responsibility regarding errors, inaccuracy or omissions in the contents of the thesis;

6) that the contents and organization of the thesis is an original work carried out by the undersigned and does not in any way compromise the rights of third parties (Italian law, no. 633, 22nd April 1941 and subsequent integrations and modifications), including those regarding security of personal details; therefore the University is in any case absolved from any responsibility whatsoever, civil, administrative or penal, and shall be exempt from any requests or claims from third parties;

7) **choose 7a or 7b**:

7a) that the thesis is not subject to “embargo”, i.e. that it is not the result of work included in the regulations governing industrial property; it was not written as part of a project financed by public or private bodies with restrictions on the diffusion of the results; is not subject to patent or protection registrations.

or

7b) that the PhD thesis is subject to “embargo” as per the separate undersigned “PhD Thesis Temporary ″Embargo″ Request”.

Date __December_10th_of_2018_____

Abstract

This thesis answers three policy-relevant and heterogeneous questions related to the political economy of development that, to the best of my knowledge, have been overlooked by the empirical literature.

Chapter 1 studies the consequences of the prominent and publicly acknowledged case of Globo's slanted coverage of the last debate of the 1989 Brazilian presidential election to investigate if a media organization can affect the vote-shares of a presidential election by deliberately manipulating the news coverage of one political debate. Using a differences-in-differences estimator comparing changes in vote-shares before and after the manipulated coverage in municipalities with Globo - the treatment group - and without Globo - the control group, we show that Lula - the candidate harmed by the manipulated coverage - lost around 1.6 percentage points in vote share - around one-million of votes - due to Globo's unfavorable coverage.

Chapter 2 study the unusually high levels of prejudice towards homosexuals in Africa to understand why some people display high levels of prejudice against individuals with different life choices, while others are indifferent or even like them. The big picture of our findings suggests that religious affiliations and beliefs are relevant determinants of prejudice towards homosexuals. We document that individuals with Muslim/new Protestant affiliations have the highest levels of prejudice towards homosexuals even if compared to individuals in the same sub-national administrative units. We also find evidence against the hypothesis that prejudice towards homosexuals was "exported" to Africa by Christian missionaries: exposure to missionary activity at the village level has no significant relationship with individual level prejudice towards homosexuals and, if anything, exposure to missionary activity at the ethnic group level has a negative relationship with individual level prejudice towards homosexuals.

Chapter 3 studies the effect of income segregation on demand for redistribution. Panel data estimates using *within city temporal variation* show a large and significant negative relationship between income segregation and demand for redistribution and reveal a large positive bias in the OLS estimates using *across cities variation* caused by time-invariant city-level characteristics.

Dedications

To my girlfriend and future wife Yekaterina for being so patient with both my academic career and our cultural differences. To my parents and three brothers for supporting my entrepreneurial career. To my “life-brothers” Raphael, Daniel and Taiana for keeping friendship even when I am distant.

To all my Bocconi professors for the priceless knowledge and skills accumulated, in especial Eliana La Ferrara, Guido Tabellini, Diego Ubfal, and Selim Gulesci. A more than special thanks to my adviser Eliana La Ferrara for all the patience and time invested.

To all my classmates, Ph.D. colleagues, and Bocconi staff for the happy time I am having in Italy, especially Francesco, Tiziano, Santiago, Katya, Carrie, Andrea, Jean, Giampaolo, and Alessandra. A more than special thanks to Francesco, Tiziano, and Santiago for being my family abroad and for the mutual help based on skill-complementarities.

To my co-authors Santiago, Luis, Raphael, and Lucas for the pleasure of attacking interesting questions together. A more than special thanks to Luis for the brotherhood and priceless present and future business partnership in this entrepreneurial career.

Alexsandros Cavgias Martins Fraga thanks Coordination of Superior Level Staff Improvement (CAPES) for 48 months of outstanding Ph.D. funding.

Introduction

This thesis answers three policy-relevant and heterogeneous questions related to the political economy of development that, to the best of my knowledge, have been overlooked by the empirical literature.

Chapter 1 answers the following question: Can a media organization affect the vote-shares of a presidential election by deliberately manipulating the news coverage of one political debate? This chapter answers this question by estimating the effects of the prominent and publicly acknowledged case of Globo's slanted coverage of the last debate of the 1989 Brazilian presidential election on the vote-share of Lula - the candidate harmed by the slanted coverage. We use a differences-in-differences estimator comparing changes in Lula's vote-shares (voting intentions) before and after Globo's manipulated coverage in municipalities with Globo - the treatment group - and municipalities without Globo - the control group - in a sample where all municipalities have at least one TV broadcaster. Baseline estimates using electoral data show that Lula lost around 1.6 percentage points in vote share - around one-million of votes - due to Globo's unfavorable second-round coverage. An increase in the vote-shares of the candidate favored by the manipulated coverage explains most of the effect. Baseline estimates using electoral data imply very high persuasion rates - around 14% - in comparison to other information treatments in the persuasion literature. To the best of our knowledge, this is the first study to identify the effects of *media coverage of political debates* in a non-controlled setting. Moreover, in contrast with the recent trend in the media bias literature of focusing in episodes of *long-run slanted coverage*, we shift the focus to the *very short-run effects of slanted coverage* by showing that a one-day episode of media manipulation of news coverage of political debates significantly affected the vote-shares of a presidential election. This chapter is joint work with Raphael Corbi, Luis Meloni, and Lucas Novaes.

Chapter 2 answers the following question: Why some people display high levels of prejudice against individuals with different life choices, while others are indifferent or even like them? We study the unusually high levels of prejudice towards homosexuals in Africa to answer this question. We provide original descriptive evidence on the determinants of prejudice towards homosexuals in the African continent. The higher level of prejudice towards homosexuals among individuals with Muslim/new Protestant affiliations is consistent with the hypothesis that less intrinsic/more extrinsic forms of religiosity and more extreme interpretations from the religious texts from individuals of both groups are important explanations for differences across individuals in prejudice towards homosexuals. We also test the hypothesis that exposure to Christian missions increased individual-level prejudice towards homosexuals in contemporary Africa. We find evidence against this hypothesis: exposure to missionary activity at the village level has no significant relationship with individual level prejudice towards homosexuals and, if anything, exposure to missionary activity at the ethnic group level has a negative relationship with individual level prejudice towards homosexuals. Mechanisms varying within groups of religious, education, and religious practices explain most of the relationship between exposure to missions at the ethnic group level and prejudice towards homosexuals. We improve the existing evidence on the determinants of social intolerance by estimating correlations between individual-level measures of prejudice towards homosexuals - a proxy of social intolerance - and a larger set of potential determinants using fixed effects for sub-national administrative regions and data from more than 30 countries of a continent where prejudice towards homosexuals is a salient phenom. We also contribute to the empirical literature studying the long-run effects of colonial institutions by being the first to test if the historical presence of missions at the ethnic group and village levels is related to contemporary levels of prejudice towards homosexuals. This chapter is joint work with Santiago Pérez Vincent.

Chapter 3 studies the effect of income segregation (measured by the Theil (1972) segregation index of households above the 90th percentile of the within city income distribution) on demand for redistribution (proxied by the vote-share of the main left-wing party in Brazil). 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 demand for

redistribution, though the direction of this relationship is theoretically ambiguous. We use two different empirical strategies to deal with the endogeneity of segregation. First, we use the percentage of the city land in margins of water basins to instrument the *across cities spatial variation* in income segregation. Second, we use an original panel data of segregation measures that are comparable across censuses to estimate the effects of income segregation using only its *within city temporal variation*. Instrumental variable estimates show non-significant effects of income segregation on demand for redistribution. Panel data estimates show a large negative bias in the OLS estimates caused by city level time-invariant characteristics and a large and significant negative relationship between income segregation and demand for redistribution.

Chapter 1

Edited Democracy? Evidence of Media Slant in the Coverage of Presidential Debates

"Ronald [Carvalho], Globo's editor of politics, entered the editing island and said: You have to make a montage with the best of Collor and the worst Lula."
(Octavio Tostes, Jornal Nacional text-editor)

Introduction

Empirical evidence shows that political debates, a common feature of many democracies, affect beliefs about candidates attributes and electoral choices (Bidwell et al., 2018; Bowles and Larreguy, 2018; Brierley et al., 2018; Fridkin et al., 2007; Fujiwara and Wantchekon, 2013; Fujiwara et al., 2017). Many voters do not have time to watch long debates and rely on their media coverage to absorb the relevant information. That said, media organizations are not necessarily neutral messengers, and their private interests may make them manipulate the news coverage of political debates to influence voters. Therefore, a clear understanding of the electoral consequences of episodes of media manipulation of news coverage of political debates is crucial to design press coverage rules that prevent influences in the democratic process.

The media-bias literature has studied the effects of medium-run and long-run slanted

coverage on electoral choices (e.g., Barone et al., 2015; Martin and Yurukoglu, 2017; Pinotti and Tesei, 2017) but, to the best of our knowledge, has been relatively silent about the electoral consequences of episodes of *very short-run slanted coverage*, such as the daily coverage of a political debate. We try filling this knowledge gap by answering the following question: Can a media organization affect the vote-shares of a presidential election by deliberately manipulating the news coverage of one political debate?

We answer this question by studying a prominent and publicly acknowledged case of media manipulation of news coverage of political debates held during the 1989 Brazilian presidential election. Three days before the second round run-off, when the opinion polls indicated a technical tie, the two candidates agreed to participate in a nationally televised debate. The dispute was widely watched, but in addition to the debate millions of voters also tuned in to watch the newscast from the country's dominant TV channel, Globo's Jornal Nacional. However, Globo, by its own later admission, favored the Collor when condensing the debate for its viewers.

Measuring the impact of the manipulated coverage of presidential debates net of the direct effect of the debate is particularly hard as they tend to be almost simultaneously broadcast by different media outlets with varying reach across regions. We take advantage of a unique natural experiment to separately identify the effect of the manipulated coverage: since the last presidential debate was jointly broadcast by all TV broadcasters, all municipalities with at least one TV broadcaster could watch the debate because but only municipalities with Globo could watch the manipulated coverage. We use a differences-in-differences estimator that exploits this natural experiment by comparing changes in outcomes before and after the media manipulation episode in municipalities with Globo - the treatment group - and municipalities without Globo some alternative TV broadcaster - the control group - in the sample where all municipalities have at least one TV broadcaster.

Our first baseline specification compares changes in Lula's vote-share across the two election rounds. This specification estimate the effect of *all* Globo's second-round coverage on Lula's vote-shares. Estimates show that Lula lost around 1.6 percentage points in vote-share - around one-million of votes - due to Globo's unfavorable second-round coverage. These estimates imply very high persuasion rates - around 14% - in comparison to other

information treatments in the persuasion literature.

The effect of Globo's coverage during *all* second-round coverage is plausibly a lower bound for the effect of Globo's manipulated coverage of the last debate for two reasons. First, Globo's coverage was more balanced during the second-round than in the first-round because its direction of journalism established guidelines of fair division of news coverage time that hold until the night of the manipulated coverage.[1] Second, given the increasing trend in Lula's vote-shares during the second-round, there were be more voters to switch votes from Lula to Collor at the end of the second-round than in the first-round.

We use electoral data to investigate the mechanisms behind the negative effect of Globo's manipulated coverage of the last debate on Lula's vote-share. Around .9% of the 1.6% drop in Lula's vote-share caused by Globo's manipulated coverage is explained by a shift in voter preferences in favor of Collor - the candidate favored by the manipulated coverage. Most of the remaining part of the effect is explained by an increase of around 0.6% in null votes. In contrast with the media bias literature but in line with an election with high mobilization in a country with mandatory voting, we document small and insignificant effects of the manipulated coverage on political participation.

Our second baseline specification compares Lula's voting intentions in the 3 days before and 1 day after the slanted coverage in metropolitan areas with and without Globo. This specification estimates the effect of Globo's coverage in the last two days of the second-round, that should closely reflect the impact of the manipulated coverage of the last debate. In line with our priors, the effects of Globo's coverage in the specification using voting intentions is twice larger than the one in the specification using electoral data in the sample of metropolitan areas.

The context of our case study combines features that are common both to weak and consolidated democracies, such as large media corporations (e.g., Fox News in US, Dogan Media in Turkey) and presidential debates, with others that are present in weak democracies, such as a large number of unknown politicians and recently created parties. Then, in general, our estimates should provide a reasonable proxy for the effect of a similar treat-

[1] "*The guideline of fair division of time between the two candidates in all the news programs, decided by the company's management and by the Central Globo of Journalism summit at the beginning of the second round, was the orientation followed by Vianey Pinheiro [Jornal Hoje's editor] in the condensation for the [Jornal] Hoje. Obeying this determination, he tried to give each candidate three minutes.*" (Conti, 1999)

ment in weak democracies with politically motivated media organizations (e.g., Mexico, Argentina, Turkey, Russia) and an upper bound for the effects of a similar treatment in consolidates democracies.[2] However, the recent rise of populist parties (e.g., Five-star movement in Italy) and political outsiders (e.g., Emmanuel Macron in France, Donald Trump in the US) in consolidated democracies suggest that our estimates may provide a reasonable proxy for the effect of a similar treatment for some elections in consolidated democracies.[3]

This paper contributes to two strands of literature. First, it contributes to the extensive literature studying the effects of mass media on political choices (Barone et al., 2015; DellaVigna and Kaplan, 2007; Enikolopov et al., 2011; Martin and Yurukoglu, 2017; Pinotti and Tesei, 2017). The more recent contributions of this literature (Barone et al., 2015; Martin and Yurukoglu, 2017; Pinotti and Tesei, 2017) have focused on the *long run effects of slanted coverage*. We contrast from them by shifting the focus to the *very short-run effects of slanted coverage* by showing that a one-day episode of media manipulation of news coverage of a political debate significantly affected the vote-shares of a presidential election. Our large persuasion rates also provide a message to the media bias literature: highly persuasive slanted coverage may be more plausible in the very short-run than in the long-run because even voters who discount the bias from the news source entirely may not have enough time to learn about the excess of bias in the manipulated coverage.

Second, it contributes to the growing literature investigating how electoral debates impact electoral and policy outcomes (e.g., Bidwell et al., 2018; Bowles and Larreguy, 2018; Brierley et al., 2018; Fridkin et al., 2007; Fujiwara and Wantchekon, 2013; Fujiwara et al., 2017; Wald and Lupfer, 1978). The most credible estimates of this literature use field and laboratory experiments to identify the effects of exposure to debates. Despite vast evidence about the *consequences of electoral debates*, the literature has been relatively silent about the *consequences of media coverage of electoral debates*, with the notable exception of Fridkin et al. 2007. Our main contribution to this literature is to credibly identify the electoral effects of an episode of manipulated coverage of a presidential debate

[2]In consolidated democracies, voters are more educated and informed in average and, consequently, have more precise priors about candidates' and parties' attributes and policy positions.

[3]In these elections, even educated and informed voters have limited information about candidates' and parties' attributes and policy positions.

net of the direct effect of the debate using electoral data from a large democracy.

The most related study to ours is Fridkin et al. (2007), who designs a field experiment with multiple treatment arms to separately identify the effect of the debate from the effect of its immediate media coverage. We differentiate from them in two dimensions. First, since we use electoral data that is representative of the electoral population of a large democratic country, our estimates have higher external validity than theirs. Second, the design of our treatment is plausibly capturing spillover effects across voters from the debate coverage that are not captured by their laboratory experiment.[4]

1.1 Institutional Background

1.1.1 Why Organizations Globo favored Collor in the 1989 election?

In July of 1989, Roberto Marinho - Globo's owner - publicly endorsed Collor's candidacy.[5] Not surprisingly, Globo's coverage of the first-round campaign was slanted in favor of Collor and against its main competitors.[6][7] We are not aware, however, of any public endorsement from Organizations Globo.[8] In April of 1992, Marinho acknowledged that Organizations Globo *"promoted the election of Collor"*.[9] In the next paragraphs, we outline the more plausible motivations behind the publicly acknowledged media slant in favor of

[4]Our ideal experiment randomizes news coverage of debates at the city-level one day after the debate and compares outcomes of control and treatment municipalities two days after the debate coverage. Fridkin et al. (2007)'s experiment randomizes exposure to debate coverage at the individual level just after the debate and compares outcomes of control and treatment immediately after the end of the experiment. Thus, our treatment is plausibly capturing additional spillover effects from discussions about the debate coverage that are not captured by their treatment.

[5]"*I'm going to influence in his favor as much as I can. I will try to be an advisor, to build a better Brazil*" (Folha de Sao Paulo, 07-27-1989).

[6]The proportion of news time dedicated to each candidate in the first round was the following: in July, Collor, 63 percent; Brizola, 6 percent; Lula, 31 percent, and, in September-October, Collor, 49 percent; Brizola, 31 percent; Lula, 20 percent (de Lima, 1990; Rubim, 1989).

[7]Several academics and journalists interviewed in the summer of 1989 concluded that Globo news coverage was clearly slanted against Brizola, Lula and Maluf, as evidenced by taking remarks out of context, editing, and looking for damaging remarks (Shidlo, 1990).

[8]The absence of endorsement from the organization was plausibly to avoid internal conflicts and criticisms. Many of Globo's journalists publicly supported Lula or Brizola, and *telenovela* actors protested their employer's coverage of the debate (Conti, 1999).

[9]"*Yes, we promoted the election of Collor and I had the best reasons for great enthusiasm and great hope that he would make an extraordinary government.*" (Folha de Sao Paulo, April 1992).

Collor, which are less clear from the historical accounts.

Globo organizations had reasons to fear economic losses in case of the election of some of Collor's main competitors. The two strongest left-wing candidates, Leonel Brizola and Lula, confessed repeatedly during the campaign about their intentions to control and regulate the media, often making direct attacks to Globo.[10] In contrast, Organizations Globo had reasons to expect economic gains in the case of Collor's election. Collor was an advocate of a pro-business agenda that would benefit Globo and its main advertisers. He also had economic and familiar ties with Organizations Globo: his family controlled TV Gazeta, an affiliate of Rede Globo in the state of Alagoas, and his first wife was the daughter of a Marinho's family business partner.[11]

1.1.2 Brazil in 1989: a fertile ground to persuade voters

Brazil in 1989 fits the picture of weak parties. The Brazilian party system was fragmented (Mainwaring, 1999), attested by the 22 candidates in the ballot, many of whom were previously unknown to the national electorate. Moreover, only four candidates received more than 10% of the votes in the first round. With one exception, all parties had less than ten years of age.

Brazilian voters in 1989 were inexperienced and had limited partisanship. All individuals who turned 18 after 1960, as well as twenty million illiterate citizens enfranchised by the 1988 Constitution, voted for president for the first time. In the days before the election, an opinion poll showed that almost half of the voters declared having no particular affection to any party.[12]

Most theories predict that the persuasive effects of slanted coverage should be magnified in a context with weak parties, limited partisanship, and inexperienced voters. In such a context, voters have imprecise priors about candidates' quality and platforms and

[10] In its 1989 campaign manifesto, the PT declared that new telecommunications regulations were *"urgent"* and that *"with political will, mobilization and civil society organization it is possible to request the repeal of [broadcasting] concessions of those that insist to violate the fundamental democratic principle: the right of the public to be informed in the most objective manner and without distortions."*. This jab is directly aimed at Globo, whose coverage of the democratization process years before was deemed "fraudulent" in the same party document (available at `http://csbh.fpabramo.org.br/uploads/democracia.pdf`, accessed in 2-28-2018.)

[11] Lilibeth Monteiro de Carvalho, Collor's first wife and mother of his first two children was the daughter of Joaquim Monteiro de Carvalho, a business partner of Marinho's family.

[12] IBOPE National Vote XIII Survey, October 1989. Available at `https://www.cesop.unicamp.br`

overweight the relevance of the information absorbed by the media when computing posteriors, what magnifies the influence of the information provided by the slanted coverage. Large persuasion rates of slanted coverage estimated using data from post-communist Russia (Enikolopov et al., 2011; Garcia-Arenas, 2015), a context that is similar to Brazil in 1989, confirm this prediction.

1.1.3 Organizations Globo: a giant with media power

Television dominated the media market by a wide margin at the end of the 1980 decade. Around 72 percent of Brazilian households had televisions sets, and an estimated 94 percent of the population watched television regularly. Television was not only the main commercial media in Brazil but also the primary source of political information in the country.[13] In contrast, newspapers were a much less relevant source of political information.[14]

Globo was the dominant television network. In 1989, 92% of all Brazilian municipalities received Globo's signal, and the network was the unique broadcaster in nearly one-fourth of the country. Its national audience was consistently above 59 percent during the 1989 campaign and could reach up to 84 percent during prime time (de Lima, 1990). *Jornal Nacional* - Globo's prime time newscast - was national leader of audience in the segment.

Globo's market power puts the network in a position to influence its audience. The ability to influence the audience in politics is what Prat (2017) refers to as *media power* of a news organization, or the *"ability to induce voters to make electoral decisions they would not make if reporting were unbiased"*. Under the assumption that all voters are naive and cannot identify news manipulation, the author derives an index that measures the upper bound for the media power of a news organization. According to this index, Globo's *maximal* media power is increasing in the time viewers watch Globo's news coverage and decreasing in the time Globo's viewers watch news coverage from competing news organizations in other media platforms.

[13] 86 to 89 percent of the population considered television their most important source of political information (de Lima, 1990; Porto, 1985)

[14] Brazil has one of the lowest rates of newspaper penetration in the planet: 42 newspaper copies per 1000 inhabitants (Porto, 1985)

TV Globo likely had considerable media power during the 1989 election. First, the competition in the Brazilian news market was mainly across TV broadcasters because of the dominance of TV as a source of information and the very low consumption of newspaper. Second, a good fraction of the viewers watched only Globo's newscasts because of its monopoly in one-forth of the municipalities and dominance regarding audience. Thus, in the context of the 1989 election, it is likely that most viewers watched only Globo's TV news coverage without consuming news from other media platforms, what magnified Globo's media power.

In 2015, Globo's dominance in the media market was still high according to Kennedy and Prat (2017), who ranked Globo News - the news arm of Globo corporation - as one the three most powerful private media organizations in the world. Yet, it is likely that at the time of the 1989 elections the *maximal media power* of Globo was even higher than in 2015, because in between these years the country experienced a significant increase in literacy, witnessed the introduction of cable television and the spread of internet news sources, and today social media is widespread.[15]

1.1.4 The electoral arena of the 1989 election

Despite the high number of candidates, the elections presented a clear left-right cleavage. Voters in the center-right chose Collor, from the recently-created National Reconstruction Party (PRN), and whose campaign confronted the incumbent's inability to rein in inflation and overall public mismanagement. The left divided between Lula, from the Workers' Party (PT), and Leonel Brizola, from the Democratic Labor Party (PDT).

During the campaign, debates were often held to present candidates to voters. Six debates inviting all the candidates were held during the first-round campaign. Curiously none was organized by Globo, and Collor did not participate in any at that stage of the race. All major TV networks jointly broadcast live the debates during the second-round campaign: the first on December 3^{rd} and the second on December 14^{th}. Figure 1.1 reports the timeline of the election and second-round debates.

[15]Kennedy and Prat (2017) uses a sample that is representative of the population of internet users at national level in 2015. This population is composed of more educated individuals who plausibly watch less news coverage of Globo and read more newspaper together with TV newscasts. In this case, the *maximal media power* of Globo would be smaller than the *maximal media power* for the whole population.

Figure 1.1: Timeline of the Election and Second-Round Debates

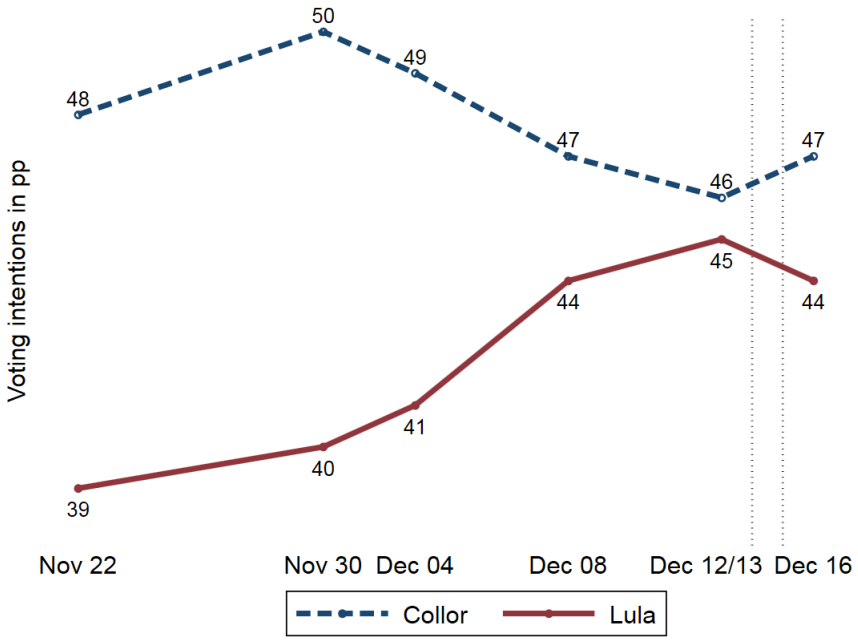
Aggregate poll data indicates that after the debate the proclivity towards candidates changed. The final debate of December 14^{th} had an audience rating of 66 points, which is equivalent to 32 million viewers from all TV stations. Figure 1.2 shows trends in voting from opinion polls about Lula and Collor across the second round. Lula reduced the difference with Collor by 8 percentage points between November 22^{nd} and December 13^{th}. Lula and Collor reached a technical tie on December 13^{th}, one day before the final debate. The reversal of trends at the end of the second-round suggests that the final debate on December 14^{th} and its media coverage on December 15^{th} and 16^{th} changed the trajectory of voters' preferences.

On December 15^{th}, Globo aired in its prime time newscast - the *Jornal Nacional* - a version of the final debate's highlights that favored Collor and harmed Lula. The newscast was viewed by an audience almost as big as the debate itself, receiving a 61 point rating, equivalent to 29 million viewers. Globo publicly acknowledged that the broadcast favored Collor, and justify its decisions in a series of interviews.[16] The consensus among Lula's staff was that Collor performed better at the debate but the edited highlights created the perception of an overperformance of Collor and an underperformance of Lula (Conti, 1999).[17]

[16]The editor for the newscast at the time, Octavio Tostes accuses a higher-ranking editor, Ronald Carvalho, of ordering to change the balanced edition of the debate's highlights aired in the afternoon's newscast and to produce an edition with "*the best of Collor and the worse of Lula*". For more details, see http://memoriaglobo.globo.com/erros/debate-collor-x-lula.htm.

[17] Ricardo Kotscho - Lula's press officer during the 1989 election - argued that "*everyone left the debate with that feeling [of Collor winning the debate], but Globo's edition changed the result of the match. It turned the victory into a massacre.* See Folha de São Paulo, 12-11-1999.

Figure 1.2: A reversal of trends after the last presidential debate and its media coverage.



Adapted by the authors from Pacheco and Alberico (2012)

1.1.5 The manipulated coverage

The partial account of the debate is clear from the segment footage. The edited highlights shows Collor for 72 seconds longer: 3 minutes and 34 seconds versus 2 minutes and 22 seconds (Conti, 1999; Porto, 1985). The content of the coverage is also considerably detrimental to Lula, accentuating candidates gaffes and insinuating that the candidate stated that Northeastern Brazilians were a sub-race. The coverage also showed the candidate stammering and confused. For most part, Collor appeared to be in the offensive and Lula in defensive, such as when Collor accused Lula of being tolerant with Brizola'a accusations that the vice-president in Lula's ticket was corrupt.

Many arguments support the view that Globo's coverage of the final debate was *relatively* more biased in favor of Collor than the competing broadcasters, SBT and Bandeirantes. First, in contrast with the fact that there has been enormous academic and non-academic attention about how Globo's covered the last debate, we are not aware of any work discussing the relevance of the two other networks, SBT's and Bandeirantes'. Second, Globo's owner publicly endorsed the candidacy of Collor and personally worked

to influence Globo's coverage in his favor.[18] Third, Globo's coverage was slanted in favor of Collor during the first round of the election. Forth, Collor's family had commercial ties with Rede Globo and, thus, was a potential competitor of SBT and Bandeirantes.

Globo's manipulated coverage of the last presidential debate was also relatively more biased than the remaining second-round news coverage. In the beginning of the second-round, Globo's direction of journalism established a guideline enforcing a fair division of news coverage time between Lula and Collor that was respected until the night of the manipulated coverage.[19]

1.2 Data and Summary Statistics

The Brazilian electoral authority, the *Tribunal Superior Eleitoral* (TSE) provides data at the municipality level for the 1^{st} and 2^{nd} rounds of the 1989 presidential elections were obtained from the electoral authority at the Ipeadata website.[20] The TSE is an independent branch of the federal judiciary. Established by the 1988 Constitution, it regulates electoral procedures, including most administrative, planning and monitoring procedures of the elections. Municipal demographics and characteristics come from the 1991 Brazilian Population Census of the Brazilian Statistical Bureau (*IBGE*), and Electoral Survey data were obtained at the Center for Studies on Public Opinion (*CESOP/UNICAMP*).

We have information regarding the starting date and location of the main TV broadcasters in Brazil at that time - namely Globo, Bandeirantes, and SBT - including retransmitting antennas as well as their radial signal reach.[21] By georeferencing the location of each antenna, we can determine which municipalities received the signal from a particular broadcaster during the 1989 election.[22]

[18] In August of 1989, Marinho was heard asking Collor which Globo stations were not supporting his candidacy (de Lima, 1990).

[19] "*The guideline of fair division of time between the two candidates in all the news programs, decided by the company's management and by the Central Globo of Journalism summit at the beginning of the second round, was the orientation followed by Vianey Pinheiro [Jornal Hoje's editor] in the condensation for the [Jornal] Hoje. Obeying this determination, he tried to give each candidate three minutes.*" (Conti, 1999)

[20] Data available at http://www.ipeadata.gov.br.

[21] The major broadcasters are responsible for above 90% of all TV audience in Brazil at that time.

[22] We are indebted to Alberto Chong and Eliana La Ferrara for kindly providing us with the TV signal data. Since the geographic borders of municipalities have changed over time, we use Minimal Comparable Areas (AMCs) as our spatial unit of analysis as in Chong and Ferrara (2009). AMCs are the smallest

Figure 1.3: Geographical Distribution of TV Signal per Broadcaster in 1989.

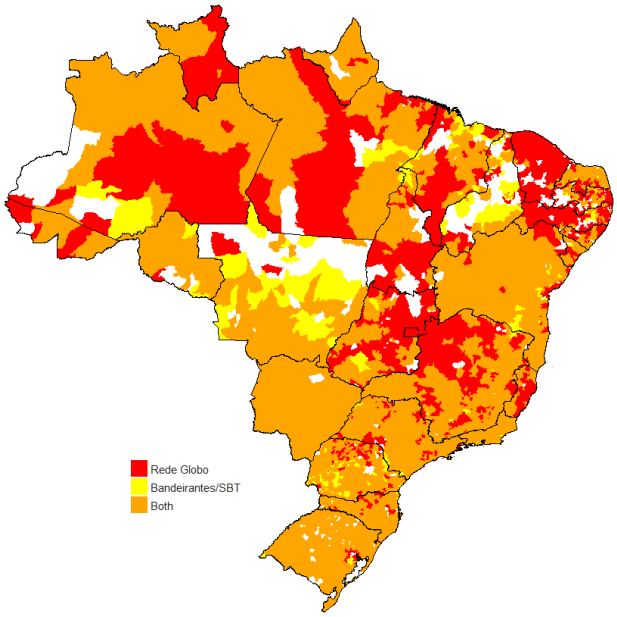
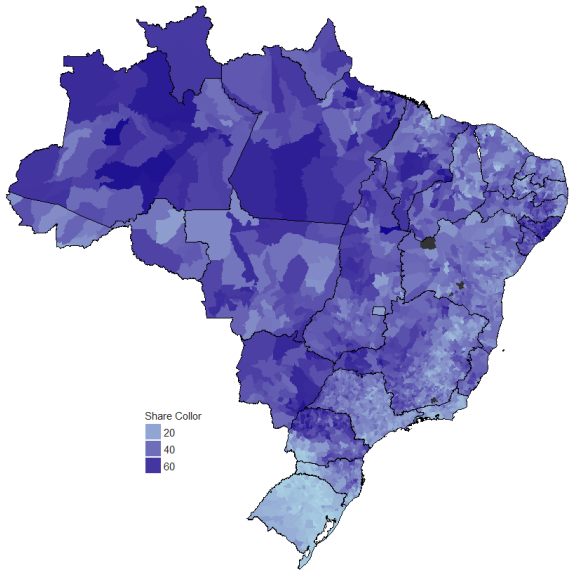


Table 1.1 illustrates the richness of the experiment. By labelling municipalities according to treatment status, row 1 shows that 1,105 out of 4,297 (25%) received TV signal exclusively from Globo during the 1989 election (*Treatment 1*) while 3,054 municipalities received signal from Globo as well as at least one other major TV broadcaster (*Treatment 2*). Our control group is defined as the 138 municipalities that received signal only from Bandeirantes or SBT. Figure 1.3 shows the 1989 geographical distribution of TV signal per broadcaster. Areas in red correspond to municipalities in *Treatment 1* with Globo signal only whereas areas in orange correspond to *Treatment 2* with the signal from another broadcaster as well. Areas in yellow represent our control group and areas in white receive no signal and are excluded from our sample. It is important to point out that treated units exist in all states in the country and control units spread across 17 out of 27 states, which comprise approximately 85% of the overall population.

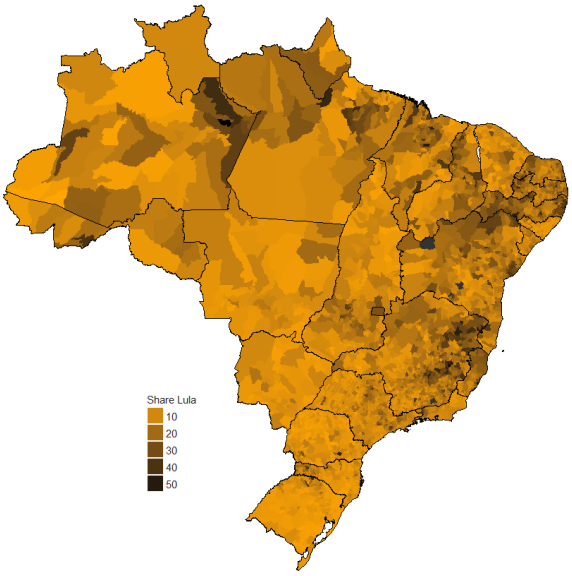
Table 1.2 reports summary statistics for municipal characteristics according to the 1991 Brazilian Census, Broadcaster TV signal and electoral outcomes across treatment status. Municipalities in the treatment group are larger and more urban. Its inhabitants are more

defined geographic areas provided by the *IBGE* that can be compared over time. Throughout the text we use the terms municipalities as a synonym to AMCs.

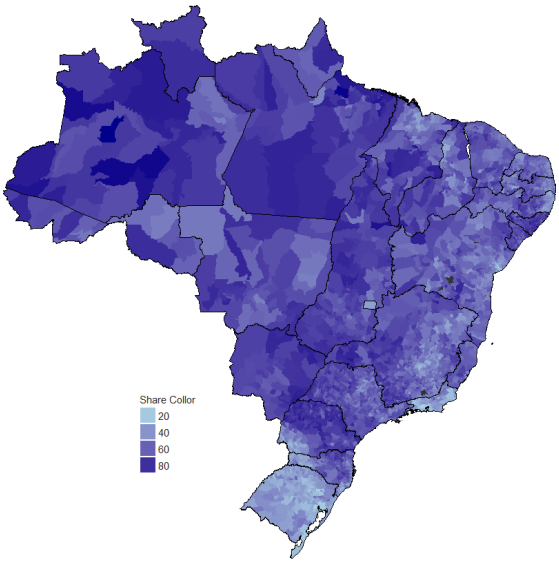
Figure 1.4: Geographical Distribution of 2^{nd} round vote share



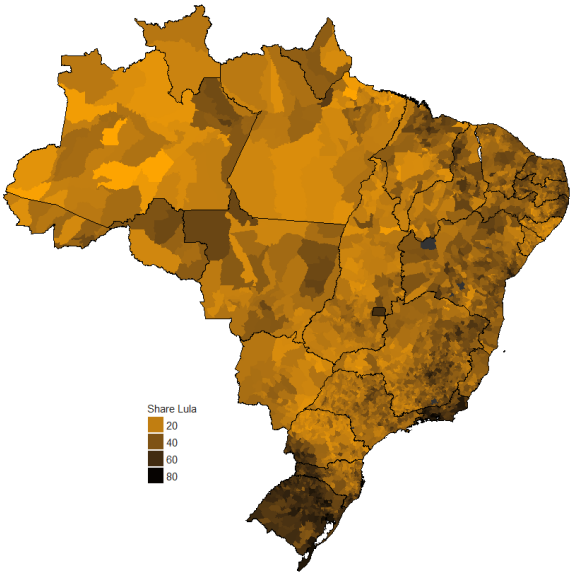
(a) Collor



(b) Lula



(c) Collor



(d) Lula

educated and have higher income, as reflected by a higher tv and radio ownership. On the other hand, Lula's and Collor's average vote share are similar across groups. Figure 2 shows the distribution of votes by candidate and round. At first glance, two patterns stand out. First, Lula received a higher share of votes in larger cities and more populated areas along the coast in the Northeast and Southeast regions. Second, neither Lula nor Collor receive a relatively high share of votes in the populous states of *Rio Grande do Sul*, *Rio de Janeiro*, and *São Paulo*. This is because a former governor of both of the first two states, Leonel Brizola, was also a 1989 presidential candidate and ranked top in both states in the 1^{st} round. A similar situation took place in *São Paulo* with a former governor, Paulo Maluf, and senator, Mario Covas. Interestingly, in *Rio Grande do Sul* and *Rio de Janeiro* most of the votes of Leonel Brizola were transferred to Lula in the 2^{nd} round whereas in *São Paulo* Collor increased by a larger margin.[23]

1.3 Empirical Strategy

In this section, we describe the differences-in-differences strategy that allows us to isolate the effect of Globo's edited coverage of the 1989 presidential debate on the election outcome. By exploring detailed municipality-level broadcaster-specific coverage data, we can disentangle the effect of the edited coverage on voting behavior from the debate itself.

Our empirical strategy tests if the coverage of the debate by Globo was relatively more biased against *Lula* than the coverage of other broadcasters. We investigate if Lula's vote share in municipalities exposed to the debate and Globo's coverage increased *less* than in municipalities exposed to the debate and the coverage of the broadcasters. In this sense, it is a relative estimate of the potential bias of Globo net of the effect of other major TV channels. As previously discussed, qualitative evidence shows that Globo's coverage was relatively more biased in favor of Collor than other networks.

We propose to identify the causal effect of the edited coverage of the debate on the election by comparing changes in the vote share of the two main candidates from the 1^{st} to the 2^{nd} round to the timing of the debate and the broadcaster-specific geographical

[23]This heterogeneity according to political preferences per candidate and how votes move from one candidate to another across rounds stressed the importance to account for the vote share of the remaining candidates in our analysis. See the next section for a more formal argument.

distribution of TV signal availability across municipalities. Let us first consider whether the 1^{st} and 2^{nd} rounds are comparable to convince ourselves that they can be used in a before-after type of comparison. There were 22 candidates in the 1^{st} round of the election with five candidates receiving above 9% of valid votes.[24] On the other hand, only the top two candidates, Lula and Collor, participated in the 2^{nd} round. As the distribution of votes across candidates varies considerably across regions, it is likely that the vote share of other candidates in the 1^{st} round are transferred to Lula and Collor in the 2^{nd} round in such a way that correlates with determinants of TV signal availability.[25] Such a pattern would violate the standard parallel trend assumption of our strategy. In order to account for that, we condition our analysis on the 1^{st} round vote share of all 20 remaining candidates. Second, it is crucial to our analysis that the presidential debate was aired by Globo as well as by the other two major national TV channels at the time, Bandeirantes and SBT. Hence we can compare before and after vote shares in municipalities with exposure to the debate exclusively to municipalities with access to the broadcast of the debate as well as Globo's edited coverage in the following day. Thus, such difference-in-difference analysis captures the relative change in vote shares due to debate coverage net of the direct effect of the debate itself.

More formally, our basic empirical strategy consists in estimating the following equation:

$$y_{jt} = \beta^{cov} globo_j \cdot \delta_t + \delta_j + \delta_t + (\gamma\, sh^c_{j,t=1} + \rho X_j)t + \epsilon_{jt} \tag{1.1}$$

where y_{jt} is Lula's vote share in municipality j and round $t = 1, 2$. δ_j are municipality fixed effects that capture all time-invariant municipal factors that may affect the dependent variable, δ_t is a time dummy that equals 1 if $t = 2$ and $globo_j$ indicates whether municipality j received Globo's signal and 0 otherwise. Hence the coefficient of interest β^{cov} captures the change in vote share in municipalities with access to the debate and Globo's coverage with respect to those with only access to the debate. Additionally, we include $sh^c_{j,t=1}$ that controls vote share of all 20 remaining candidates to allow for dif-

[24] They were Collor, Lula, Brizola, Mário Covas (PSDB), and Paulo Maluf (PDS)

[25] The geographical allocation of TV antennas followed commercial and possibly political criteria (Chong and Ferrara, 2009).

ferential trends for municipalities with different preferences across candidates in the 1^{st} round. X_j represent municipal socioeconomic characteristics such as population size, average levels of schooling and income, access to water and electricity and tv ownership in order to account for municipality-specific trends across cities with different pre-existing levels of development. This is particularly important as these factors are likely to play a significant role in determining the effect of the debate and debate media coverage.[26]

Another potential concern with our identification strategy is that the 1^{st} and 2^{nd} rounds of the 1989 presidential election took place 32 days apart. More specifically, our baseline estimates using real election data capture the effect of all content broadcast by Globo in this period relative to the other networks and not only the effect of the edited debate coverage. Hence it is possible that our approach captures the effect of other developments that took place between rounds and were relatively more emphasized by Globo. In order to circumvent this, we complement our analysis by employing a similar empirical strategy to election survey data that were collected daily between 3 days before and 1 day after the debate coverage, thus considerably closing the gap between *before* and *after* treatment dates. Moreover, instead of comparing vote share in the 1^{st} and 2^{nd} rounds we use data from a survey question regarding intent to vote in the 2^{nd} round that is identical and hence fully comparable across all four polling days. Our survey data contain observations from all 27 state-capital metropolitan areas. In 1989 the city of *São Luís* was the only state capital city without Globo's signal. As it received the signal from other broadcasters, its inhabitants were still able to watch the debate but not Globo's coverage of the debate. Hence we use it as a control for all other capital cities.[27]

Given the features of the survey data described above, we specify the following regression model for the impact of the coverage on vote intention

$$\nu_{ijt} = \beta^{poll} globo_j \cdot \mu_t + \mu_j + \mu_t + \omega W_{ijt} + \xi_{ijt} \tag{1.2}$$

where ν_{ijt} equals one if individual i reports intention to vote for Lula in the 2^{nd} round at time $t = 1, 2$, μ_j and μ_t are set of capital city and time effects. W_{ijt} controls for

[26] Table 1.2 shows how different treatment and control units are across these dimensions.

[27] See Pischke (2007) for a similar identification strategy with one control and many treated units applied to a different setting.

individual characteristics such as gender, education and age and β^{poll} captures the change in vote share in other municipalities relative to *São Luís*. We report heteroskedasticity-robust standard errors clustered at the municipality level in order to account for residual auto-correlation.

1.4 Results

1.4.1 Evidence from Electoral Data

This section presents baseline estimates on the effect of Globo's edited coverage of the last presidential debate on electoral outcomes in the 1989 Brazilian presidential election. We begin by reporting estimates of the average effect on Lula's and Collor's second-round vote share in Table 1.3. Columns (1)-(3) report the results for Lula's vote share. The estimate associated with Globo's coverage in column (1) is positive and insignificant and becomes negative and significant once we control for the first-round vote shares of the other candidates. We interpret this as direct evidence that conditioning on the distribution of votes is critical to ensure that first and second-round vote shares are comparable. By not conditioning our analysis, it is likely that the vote share of other candidates in the first round are transferred to Lula and Collor in the second round in such a way that correlates with determinants of TV signal availability.[28] In column (3) we include as additional controls municipal socioeconomic characteristics interacted with time in order to account for municipality-specific trends across cities with different pre-existing levels of development. Our estimates reported in column (3) show that Globo's coverage is associated with a decrease of around 1.6 p.p in Lula's vote-share. Column (4) allows treatment status to vary according to coverage of other TV broadcasters. The first treatment group is comprised of municipalities that receive signal by Globo but not SBT/Bandeirantes while the second consists of municipalities covered by both. We argue that voters in the first group are more likely to have watched Globo's coverage in the day following the debate as it was the only TV broadcaster available and hence more likely to be affected by it. Our estimates indicate that the effect of Globo's coverage is stronger for the first

[28] See section 2 for a more detailed description of the distribution of vote shares across regions and election rounds.

group ($\approx$ 2 p.p) relative to the second ($\approx$ 1.4 p.p.).

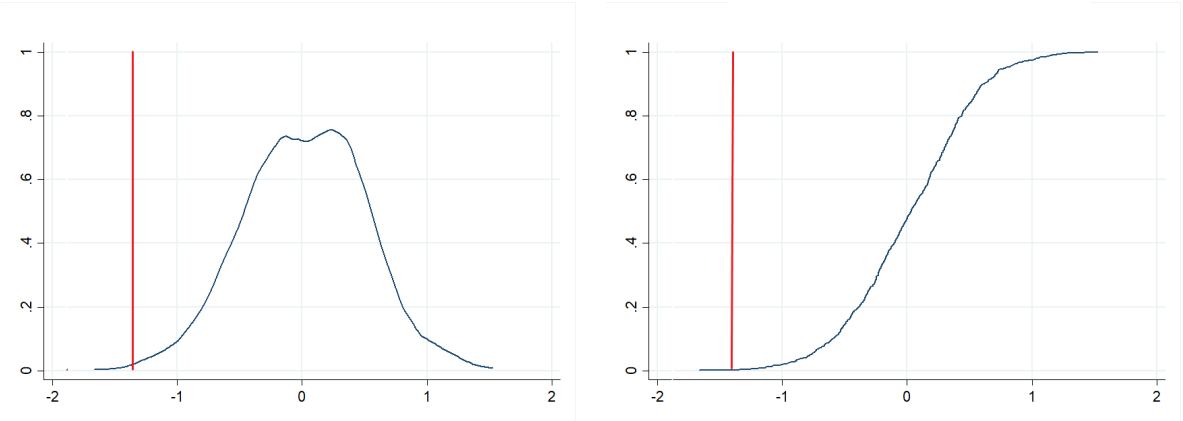
Columns (5) reports the results of our benchmark specification for Collor's vote share. Our estimates indicate that Globo's edited coverage had a positive and significant at 10% effect ($\approx$ .9 p.p.) albeit in smaller magnitude relative to Lula's. Similarly to the pattern found in column (4), the estimates in column (6) show that the effect is stronger in municipalities covered only by Globo.

Table 1.4 reports regression estimates of Globo's biased coverage on non-valid votes and turnout. Columns (1) and (2) report estimates of the treatment effect on the share of blank votes using our benchmark specification. Even though the point estimate is positive, it is not significant at any usual level of statistical confidence. When we split the treatment group according to other TV broadcasters' coverage, we find that the effect the point estimates remain unchanged. In columns (3) and (4) the effects are stronger and more precisely estimated than those in columns (1) and (2). It is worth emphasizing that the sum of the estimated effect on the share of blank and null votes is 0.64, precisely the difference between the effect on Lula's and Collor's vote-share. Finally, the effect on voter turnout reported in columns (5) and (6) is insignificant. This is consistent with the claim that a share of Lula's voters affected by the debate coverage changed their votes to Collor in the second-round, while others invalidated their votes.

Placebo Analysis

In order to test the validity of our results, we conduct a placebo analysis. Since the variation from our identification comes from the geographic distribution of TV broadcast across municipalities, we conduct an analysis randomly assigning treatment status to the municipalities. Considering there is a large variation in the treatment assignment across regions, we randomize the treatment within each region, keeping the number of treated and control municipalities fixed in each region. We then reestimate our benchmark model using the false treatment variable and store the estimates. We repeat the exercise 1,000 times. Figure 1.5 shows the empirical cumulative distribution function and density of the estimated coefficients on Globo's coverage. The distribution of the estimated coefficients on the placebo treatment variable is centered around zero, as expected, and our benchmark estimate from column (4) of Table 1.3, indicated by a vertical line clearly lies on the left-

Figure 1.5: Placebo Analysis



(a) Placebo Density Distribution (b) Placebo Cumulative Distribution

extreme of the range of coefficients estimated in our simulation exercise.

Heterogeneous Effects

We now proceed to analyze the heterogeneity of the estimated effects. In all results that follow, unless explicitly mentioned, we will use our benchmark specification, including municipality and time fixed effects, first-round vote shares and municipal socioeconomic characteristics interacted with time. Table 1.5 presents the result splitting the treatment effect into two different groups, above and below the median according to TV and Radio ownership. Columns (1) and (4) report the estimates for TV. The effect both on Lula's and on Collor's vote share are stronger in municipalities above the median, suggestive evidence that our results are indeed related to Globo's coverage of the debate.

In column (2) and (5) we report the estimates for radio ownership. Similarly to what happens with TVs, the estimates reported in columns (2) and (5) suggest that the effect is stronger according to radio ownership. It is likely, however, that the share of households with radio and TVs is highly correlated. In order to try to disentangle these effects, we split municipalities into four groups. We first split municipalities above/below the median of TV ownership. Then, within these groups, we further split observations according to radio ownership above/below the median. The estimates reported in columns (3) and (6) indicate that the effect of the biased debate coverage is stronger in municipalities with a higher share of households with TVs regardless if the share of households with radios is

above/below the median.

Table 1.6 reports regression estimates of the effect of the edited coverage on Lula's second-round vote share according to observations above and bellow sample median income per capita, education level, population size, and rural population. Column (1) and (2) shows that the effect is significantly higher in municipalities with higher income per capita and higher levels of education. Columns (3) and (4) present the results by population size and by the rural population. For those variables, the difference between groups above/below the median is less stark. It is worth emphasizing that the pattern observed in the heterogeneity of the estimated effects broadly follows Lula's electoral results as he received significantly more votes in wealthier, more educated and more urban state capitals.

Persuasion rates

Persuasion rates estimate the percentage of receivers that change the behavior among those that receive a message and are not already persuaded. They capture the effect of the persuasion treatment on the relevant behavior $(y_t - y_C)$, adjusting for exposure to the message $(e_T - e_C)$ and for the size of the population left to be convinced $(1 - y_C)$.

We calculate Persuasion rates as defined by DellaVigna and Gentzkow (2010)

$$f = \frac{y_T - y_C}{(e_T - e_C)(1 - y_C)}$$

where $y_T - y_C$ is the coefficient of Lula's vote-share in column (3) of Table 1.3 plus the coefficient of Collor's vote-share in column (5) of Table 1.3, and y_C is the share of votes for Collor in the control group. We assume that all households in the treatment group and no household in the control group watched the manipulated coverage of the debate. In this case, $e_T - e_C = e_T$ is the share of households with TVs in the treatment group.

We compute persuasion rates of $\frac{-1.570\%+0.927\%}{35.95\%\cdot(100\%-38.94\%)} \approx 14\%$. According to DellaVigna and Gentzkow (2010), this persuasion rate is very high in comparison to other information treatments in the persuasion literature, showing that the manipulated coverage of the last presidential debate is a very persuasive treatment.

1.4.2 Evidence from Survey Data

We complement our analysis by employing a similar empirical strategy to election survey data. This analysis is important for two reasons. First, the survey data discloses voter preferences daily between 3 days before and 1 day after the debate coverage, providing a close window between *before* and *after* treatment dates.[29] Second, instead of comparing vote share in the 1^{st} and 2^{nd} rounds we use data from a survey question regarding vote intentions in the 2^{nd} round, thus fully comparable across all four polling days. Our survey data contain observations from all 27 state-capital metropolitan areas. In 1989 the city of *São Luís* was the only state capital city without Globo's signal. As it received the signal from other broadcasters, its inhabitants were still able to watch the debate but not Globo's coverage of the debate. We use São Luís as a control for all other capital cities. In order to make our survey data estimates comparable to our baseline estimates, we replicate our baseline estimate restricting the sample to municipalities located in metropolitan areas and redefining as treated municipalities all municipalities located in metropolitan areas except to those located near São Luís. Table 1.7 describes both survey data and the restricted sample electoral/census data. In both datasets, Lula's vote share is considerably higher. This is expected as he received significantly more votes in the state capitals.

Table 1.8 reports our estimates. Columns (1) and (2) report the estimates of the effect of Globo's coverage using survey data. Our estimates point to a large and highly significant effect of Globo's coverage in both Lula's and Collor's vote share. Column (3) and (4) report the estimates of our electoral data baseline specification when we restrict the sample to metropolitan areas and the control group to São Luis. Consistent with the survey results, using our benchmark specification we find larger effects associated - around twice the magnitude - with Globo's when we restrict our analysis to this subset of municipalities.

1.5 Excluding competing histories

In this session, we outline alternative mechanisms - other than the persuasion effects of the manipulated coverage - that could explain the negative and significant β^{cov} and discuss

[29] The 1^{st} and 2^{nd} rounds of the 1989 presidential election took place 32 days apart.

why they are unlikely that to explain our results.

1.5.1 Differences in debate's audience-rates across control and treatment

β^{cov} may be estimating the effect of the debate on Lula's vote-share instead of the effect of Globo's news coverage of the debate on Lula's vote-share if debate audience-rates were higher in municipalities with Globo and the information provided by the debate coverage had a similar content than the one provided by Globo's news coverage of the debate.[30] In this case, β^{cov} would be negative and significant even if Globo's debate coverage had no effect on Lula's vote-share.

It is unlikely that β^{cov} is all explained by differences in audience across municipalities with and without Globo. First, the last presidential debate had a large audience in all municipalities where audience numbers are observable, which likely minimized the across municipalities variation in audience rates all over the country. Second and crucially, we estimate stronger effects on Lula's vote-shares in the municipalities where Globo is the only broadcaster.[31]

Even if the debate audience was considerably higher in municipalities with Globo, our demanding specification may be controlling for these differences. Differences in audience-rates across municipalities should depend on media consumption habits that are arguably stable in the very short-run. Thus, the inclusion of $\mathbf{X}_j \cdot t$ in our baseline specification is controlling for the differences in vote-shares' trends across municipalities with and without Globo that is explained by differences in audience-rates that can be predicted by the socioeconomic variables in $\mathbf{X}_j$.

[30]The qualitative evidence suggests that Collor performed better than Lula in the last presidential debate but that Globo's debate coverage showed an over-performance of Collor and an under-performance of Lula.

[31]Since all TV broadcasters broadcasted the last presidential debate, debate audience should be mechanically higher in municipalities with Globo and some alternative broadcaster. Moreover, the average night audience-rates and the average number of TV per household should be higher in municipalities with Globo and some alternative broadcaster.

1.5.2 Differences in baseline prior means across control and treatment.

β^{cov} may be capturing a heterogeneity in the effect of the debate on Lula's vote-share instead of the effect of Globo's debate coverage on Lula's vote-share if priors about Collor's relative quality have lower means in municipalities with Globo. All else equal, if priors about Collor's relative quality have lower means, there will be a larger mass of Lula's voters to switch to Collor or null votes for any given realization of the information shock generated by the debate. In this case, β^{cov} would be negative and significant even if Globo's debate coverage did not affect Lula's vote-share.

However, it is unlikely that municipalities with Globo had priors about Collor's relative quality with lower means than in municipalities without Globo. First, Globo devoted more time to Collor during all the first-round round news coverage. Second, the content of Globo's first-round campaign coverage favored Collor and harmed some of his competitors, like Lionel Brizola and Paulo Maluf.

Even if municipalities with Globo had priors with lower means about Collor's relative quality, our demanding specification may be controlling for part of these differences. First-round vote-shares should explain part of the variation in prior means about Collor's relative quality. Then, the inclusion of $\mathbf{sh}_{j,t-1} \cdot t$ in our baseline specification is controlling for the differences in vote-shares' trends across municipalities with and without Globo which is explained by differences in priors means about Collor's relative quality that can be predicted by the first-round vote-shares $\mathbf{sh}_{j,t-1}$.

1.5.3 Differences in baseline prior precision across across control and treatment.

β^{cov} may be estimating a heterogeneity in the effect of the debate on Lula's vote-share instead of the effect of Globo's manipulated debate coverage on Lula's vote-share if priors about Collor's relative quality had lower precision in municipalities with Globo. All else equal, if priors about Collor's relative quality have lower precision in municipalities with Globo, the signal generated by the debate is over-weighted in the posterior mean and, consequently, a higher mass of voters believe that Collor is a better candidate after the

manipulation. In this case, β^{cov} would be negative and significant even if Globo's debate coverage did not affect Lula's vote-share.

However, it is unlikely that municipalities with Globo had priors about Collor's relative quality with lower precision than in municipalities without Globo. Priors about Collor's absolute and relative quality should be more precise in municipalities with Globo because of Globo's news coverage favored Collor during all first-round.

1.6 Discussion about differences in magnitudes across samples

In this session, we argue that differences in the magnitudes of $globo_j \cdot t$ across the two samples reported in Table 1.8 reflect differences in the Bayesian updating process explained by each sample.

Estimates from electoral data quantify the effects of a Bayesian updating process held two days after Globo's manipulated debate coverage with priors from one-month before the manipulation. We call this process *update 1*. Estimates from voting intentions data quantify the effects of a Bayesian updating process held one day after the Globo's manipulated debate coverage with priors from three days before the manipulation. We call this process *update 2*. Different theoretical arguments may explain why *update 2* generates stronger effects on Lula's vote-shares than *update 1*.

First, magnitudes may be larger in *update 2* because priors about Collor's relative quality were less precise at the end of the campaign. If priors of *update 2* are less precise, the signal generated by Globo's manipulated debate coverage is over-weighted in the posterior mean and, consequently, a higher mass of voters believe that Collor is a better candidate after the manipulation. However, since voters should have more information about both candidates at the end of the campaign, it is unlikely that priors in *update 2* are less precise than in *update 1*.

Second, magnitudes may be larger in *update 2* because priors about Collor's relative quality had lower mean at the end of the campaign. All else equal, if priors of *update 2* have lower mean about Collor's relative quality, more voters believe that Lula is a better candidate before the manipulated coverage and, consequently, more voters are persuaded

to switch votes to Collor after the manipulated coverage.[32] Given the increasing trend of Lula's voting intentions during the second round, it is likely that priors about Lula's absolute quality had higher mean at the end of the campaign and, consequently, priors about Collor's relative quality had lower means at the end of the campaign.

Third, magnitudes may be larger in *update 2* because voters update information about the relative quality of the candidates in a faster timing than they update information about the bias of the manipulated debate coverage. In this case, estimates from opinion poll data would show stronger effects because the weight given by voters to the information provided by the Globo's debate coverage would vanish over time.

Finally, magnitudes may be larger in *update 2* because the information provided in the last two days of Globo's coverage was more biased in favor of Collor and more precise than during the remaining thirty days of the second-round. In this case, voters give higher weight to a stronger information signal in favor of Collor's relative quality when computing posteriors and, consequently, more voters are persuaded to switch votes to Collor in *update 2*. We believe this is a likely scenario because, as mentioned before, Globo's direction of journalism established guidelines of fair division of news coverage time that hold until the afternoon newscast of the manipulated coverage day.

1.7 Conclusion

In this paper, we test how the TV coverage of presidential debates can affect elections using the most notorious case of slanted reporting to date: Globo's coverage of the last debate between Brazilian presidential candidates in 1989. In a polarized election between left and right, Globo's favoring the right-wing outsider cost around one-million votes to the candidate on the left (or 1.6 percentage points), an effect mostly explained by a shift of votes in favor of the right-wing outsider.

These findings attest that one single news segment can have a substantial effect on voters. We provide a message to the media bias literature: large persuasion ares of slanted coverage may be more plausible in the *very short-run* than in the *long-run* because even voters who discount the bias from the news source entirely may not have enough time to

[32] We are implicitly assuming that the popularity shock has an uni-modal symmetric distribution.

learn about the excess of bias in the manipulated coverage. We also provide a message to media regulators: regulating and monitoring the daily news coverage of campaign events in the proximity of the the election is essential to prevent media interference in the democratic process.

Table 1.1: TV Broadcast Coverage

Treatment status	Broadcaster	Municipalities
Treatment 1	Globo=1 and SBT/Bandeirantes=0	1,105
Treatment 2	Globo=1 and SBT/Bandeirantes=1	3,054
Control	Globo=0 and SBT/Bandeirantes=1	138

The Table reports the number of municipalities according to TV Broadcaster signal availability. We define our treatment group as municipalities for which Globo was available during the 1989 Presidential election. Furthermore, we separate treated observations into municipalities without (Treatment 1) and with (Treatment 2) access to SBT/Bandeirantes. Our control group is comprised by municipalities with access to SBT/Bandeirantes but not Globo. Municipalities without access to any major broadcaster were not included in the sample. For more details on how signal availability variables were constructed, see section 3.

Table 1.2: Descriptive Statistics

	Treatment		Control	
	Mean	Std. Dev.	Mean	Std. Dev.
Population	33,624	194,546	22,921	64,001
Years of schooling	3.20	1.25	2.55	1.20
Income per capita	0.74	0.43	0.59	0.40
Share of households with TV	53.67	26.39	35.95	22.33
Share of households with radio	75.18	15.06	65.83	13.61
Share of pop. in rural areas	44.37	22.48	56.80	21.46
Globo	1.00	0.00	0.00	0.00
SBT or Bandeirantes	0.73	0.44	1.00	0.00
Lula's vote share (1st round)	12.94	8.75	13.29	9.32
Collor's vote share (1st round)	36.93	15.46	38.94	14.37
Blank and Null votes (1st round)	8.43	4.44	10.78	6.06
Lula's vote share (2nd round)	33.81	14.45	32.83	12.60
Collor's vote share (2nd round)	60.60	15.03	61.41	13.69
Blank and Null votes (2nd round)	5.59	1.86	5.76	2.41
Number of municipalities	4159	-	138	-

The Table reports summary statistics of the main variables included in the analysis such as municipal characteristics from the 1991 Census, TV Broadcaster signal availability and electoral outcomes. Income per capita indicates the average municipal income in terms of Brazilian minimum wage in 1991 (equivalent to USD 83,00 in 2018). The sample includes all municipality-round observations covering all Brazilian municipalities for which at least one major broadcaster was present during the 1989 Presidential election. For more details on how signal availability variables were constructed, see section 3.

Table 1.3: Edited Debate Coverage and Vote shares

	Lula's vote share				Collor's vote share	
	(1)	(2)	(3)	(4)	(5)	(6)
Globo=1	1.337	-1.088	-1.570		0.927	
	[0.965]	[0.505]**	[0.520]***		[0.497]*	
Globo=1 and SBT/Band=1				-1.378		0.713
				[0.523]***		[0.499]
Globo=1 and SBT/Band=0				-2.001		1.405
				[0.536]***		[0.514]***
Mean dep. var	23.36	23.36	23.36	23.36	48.95	48.95
Political controls	No	Yes	Yes	Yes	Yes	Yes
Socieconomic controls	No	No	Yes	Yes	Yes	Yes
Observations	8594	8594	8594	8594	8594	8594
R-squared	0.70	0.96	0.96	0.96	0.96	0.96

The table reports regression estimates of the effect of Globo's edited coverage on second round's vote shares in 1989 presidential elections. Columns (1)-(4) reports OLS baseline estimates on Lula's vote share. The specification used in Column (1) includes municipality and time fixed effects. Column (2) includes first-round vote shares as additional controls. Column (3) includes municipal socioeconomic characteristics (population size, average levels of schooling and income, access to water and electricity, and tv ownership) interacted with time. Column (4) splits treatment status according to coverage of other TV broadcasters. Columns (5) and (6) repeat columns' (3) and (4) specification and estimate the effect of the edited coverage on Collor's vote share. Heteroskedasticity-adjusted standard errors clustered at the municipality level are reported in below the coefficients. Significantly different from zero at 99% (***), 95% (**) and 90% (*) confidence level.

Table 1.4: Edited Debate Coverage and Other Electoral Outcomes

	Blank votes		Null votes		Turnout	
	(1)	(2)	(3)	(4)	(5)	(6)
Globo=1	0.097		0.546		-0.273	
	[0.108]		[0.197]***		[0.231]	
Globo=1 and SBT/Band=1		0.097		0.568		-0.253
		[0.108]		[0.198]***		[0.238]
Globo=1 and SBT/Band=0		0.097		0.498		-0.320
		[0.113]		[0.204]**		[0.250]
Observations	8594	8594	8594	8594	8594	8594
R-squared	0.64	0.64	0.81	0.81	0.23	0.23

The table reports regression estimates of the effect of Globo's edited coverage on the share of blank and null votes and turnout. Columns (1)-(2) and (3)-(4) report OLS baseline estimates on the effect on the share of blank and null votes. Column (5)-(6) report estimates of the effect on voter turnout, proxied by the number of votes per capta. All specifications include municipality and time fixed effects, first-round vote shares and municipal socioeconomic characteristics (population size, average levels of schooling and income, access to water and electricity, and tv ownership) interacted with time. Heteroskedasticity-adjusted standard errors clustered at the municipality level are reported in below the coefficients. Significantly different from zero at 99% (***), 95% (**) and 90% (*) confidence level.

Table 1.5: Heterogeneous Effect according to and TV and Radio Ownership

	Lula			Collor		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment × TV High	-1.813			0.972		
	[0.539]***			[0.510]*		
Treatment × TV Low	-1.483			0.911		
	[0.545]***			[0.521]*		
Treatment × Radio High		-1.731			0.927	
		[0.529]***			[0.503]*	
Treatment × Radio Low		-1.488			0.927	
		[0.537]***			[0.514]*	
Treatment × TV High, Radio High			-1.810			0.953
			[0.558]***			[0.527]*
Treatment × TV High, Radio Low			-1.759			1.037
			[0.544]***			[0.514]**
Treatment × TV Low, Radio High			-1.409			0.996
			[0.559]**			[0.535]*
Treatment × TV Low, Radio Low			-1.570			0.818
			[0.578]***			[0.555]
Observations	8594	8594	8594	8594	8594	8594
R-squared	0.96	0.96	0.96	0.96	0.96	0.96

The table reports regression estimates of the effect of Globo's edited coverage on Lula's and Collor's second-round vote shares according to TV and radio ownership. Columns (1) and (4) report the effects for municipalities above/bellow sample median TV ownership. Columns (2) and (5) report the effects for municipalities above/bellow sample median radio ownership. Columns (3) and (6) report the heterogeneous effect according to TV and radio ownership. All specifications include municipality and time fixed effects, first-round vote shares and municipal socioeconomic characteristics (population size, average levels of schooling and income, access to water and electricity, and tv ownership) interacted with time. Heteroskedasticity-adjusted standard errors clustered at the municipality level are reported in below the coefficients. Significantly different from zero at 99% (***), 95% (**) and 90% (*) confidence level.

Table 1.6: Heterogeneous Effect according to Municipal Characteristics

	Income per capta	Education	Population	Pop. in rural areas
	(1)	(2)	(3)	(4)
Globo=1 * Above median	-2.437	-2.512	-1.699	-2.086
	[0.508]***	[0.518]***	[0.508]***	[0.508]***
Globo=1 * Below median	-1.350	-1.362	-2.085	-1.424
	[0.546]**	[0.544]**	[0.514]***	[0.507]***
Municipality FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Political controls	Yes	Yes	Yes	Yes
Munic. characteristics × time	Yes	Yes	Yes	Yes
Observations	8349	8349	8349	8349
R-squared	0.96	0.96	0.96	0.96

The table reports regression estimates of the effect of Globo's edited coverage on Lula's second-round vote share according to observations above and bellow sample median income per capta, education level, population size and rural population. All specifications include municipality and time fixed-effects, first round's vote-shares and municipal socioeconomic characteristics (population size, average levels of schooling and income, access to water and electricity, and tv ownership) interacted with time. Heteroskedasticity-adjusted standard errors clustered at the municipality level are reported in below the coefficients. Significantly different from zero at 99% (***), 95% (**) and 90% (*) confidence level.

Table 1.7: Restricted Sample Descriptive Statistics

	Survey		Electoral/Census
	Before	After	
Age	20.18	20.12	26.97
Male	0.51	0.51	0.52
Years of schooling	8.16	8.71	5.33
2^{nd} Round Lula's vote share	49.88	51.33	52.85
2^{nd} Round Collor's vote share	33.13	32.83	41.09
Observations	4,325	4,747	375

The Table gives summary statistics for survey data and comparable restricted sample of the 2nd-round actual election data. Globo's edited coverage of the debate aired on the late evening of December 15th 1989. Our survey data were collected in December 12th-15th and 16th, before and after coverage respectively. Survey voting variables indicate 2nd-round vote intention for Lula and Collor. We restrict our election data sample to include only municipalities located in state-capital cities metropolitan areas as to mimic the sampling of our survey data.

Table 1.8: Edited Debate Coverage and Voteshare: Restricted Sample

	Survey data		Electoral data	
	(1)	(2)	(3)	(4)
	Lula	Collor	Lula	Collor
Treatment	-0.101	0.134	-5.053	3.306
	[0.019]***	[0.019]***	[0.774]***	[0.620]***
Observations	9072	9072	740	740
R-squared	0.10	0.09	0.98	0.98

The table reports regression estimates of the effect of Globo's edited coverage on Lula's and Collor's vote shares. Columns (1) and (2) report estimates using survey data. Globo's edited coverage of the debate aired on the late evening of December 15th 1989. Our survey data were collected in December 12th-15th and 16th, before and after coverage respectively. Survey voting variables indicate 2nd-round vote intention for Lula and Collor. Columns (3) and (4) report the estimates using actual electoral data. They include 1st-round vote shares of other candidates as controls and municipality characteristics interacted with time. We restrict our election data sample to include only municipalities located in state-capital cities metropolitan areas as to mimic the sampling of our survey data. Heteroskedasticity-adjusted standard errors clustered at the state level for the survey data estimates and at the municipality level for the electoral estimates are reported in below the coefficients. Significantly different from zero at 99% (***), 95% (**) and 90% (*) confidence level.

Chapter 2

The Origins of Prejudice towards Homosexuals in Africa

"The moral thing I should wish to say to them is very simple: I should say, love is wise, hatred is foolish. In this world which is getting more and more closely interconnected, we have to learn to tolerate each other, we have to learn to put up with the fact that some people say things that we don't like. We can only live together in that way, and, if we are to live together and not die together, we must learn a kind of charity and a kind of tolerance, which is absolutely vital to the continuation of human life on this planet." (quote obtained from minute 07:38)[1]

(Bertrand Russel, British Philosopher and Mathematician)

Introduction

Why some people display high levels of prejudice against individuals with certain life choices, while others are indifferent or even like them?[2] Can this type of prejudice be explained by socioeconomic conditions (e.g., poverty and illiteracy) or historical factors (e.g., exposure to foreign cultures)? Is it fueled by specific interest groups pushing their agenda (e.g., politicians or extremist religious groups)? The economics literature is relatively silent about the determinants of prejudice and social intolerance and, despite advances by other academic disciplines, such as political science and sociology, there is still not a clear

[1] See the full video of the interview to Bertrand Russel here

[2] Implicit in our research question is the view that individuals choose between disliking or being indifferent (and liking) individuals with choices to which they disagree. The recent literature about motivated beliefs (see Bénabou and Tirole (2016), for a survey) supports this view.

understanding of them.[3][4] The objective of this paper is to provide novel evidence to help to filling this knowledge gap.

The interaction between individuals with different life-choices is becoming a common event in modern days. For example, millions of Syrian refugees live in Germany now, a place where many considered unacceptable or even illegal in Syria (e.g., homosexuality, aesthetic conventions) are acceptable. Prejudice is a necessary condition for actions that generate economic losses (e.g., intolerance and discrimination). Moreover, prejudice can generate welfare losses to targeted individuals even without actions of intolerance or discrimination.[5] More broadly, an environment of low prejudice usually have higher levels of subjective well being.[6] Therefore, providing evidence on why some individuals choose to dislike individuals with choices to which they disagree is of first-order to learn how to reduce prejudice and increase welfare in a world of increasing interactions between individuals with heterogeneous preferences. For example, policy makers and non-governmental organizations can use this evidence to develop and target effective anti-prejudice propaganda.

We study the determinants of prejudice towards homosexuals in the context of contemporary Africa. More specifically, we want to understand which, if any, are the colonial and contemporary influences that shape the current within-country distribution of prejudice towards homosexuals in Africa. We measure prejudice towards homosexuals using a new question included in the Afrobarometer (AB) Wave 6 that asks how much respondents would dislike having homosexuals as neighbors.[7] Our main outcome is a dummy variable

[3]Corneo and Jeanne (2009) make this point clear in their motivation: "Economists are perhaps the only social scientists who have been silent about the nature of tolerance."

[4]In his recent survey about political scientists' research on intolerance, Gibson (2015) indicate: "No theory to date has propounded a differentiated explanation of the origins of social and political intolerance, and thus no unified theory of the etiology of intolerance exists."

[5]Contemporary research in social psychology suggests that targeted individuals respond strategically to prejudice (Major and Vick, 2005) and under-perform when stereotypes are salient (see Steele et al., 2002, for a review).

[6]Trends in social tolerance are strongly positively correlated with trends in measures of subjective well-being, happiness and life satisfaction (Inglehart et al. , 2009).

[7]This question was meant to be a measure of tolerance: "This approach to studying tolerance most closely matches the "least-liked" approach." (Afrobarometer Dispatch No. 74, p.p. 3). However, a closer look at the *concept of intolerance* reveal one inconsistency. In philosophy, *intolerance* is defined as: "a conjunction of a *negative motive* ... and a *negative act*, wherein the latter may range from smirks to insults to discrimination to physical abuse to extermination" (Preston King, *Toleration*, p.p. 9). We believe this question captures the *negative motive* from the respondent but not necessarily a *negative act*. Therefore, we decided to interpret it as an intensity margin measure of *prejudice* towards homosexuals instead of *intolerance* against homosexuals.

that takes value one if the respondent says she would *strongly dislike* or *somewhat dislike* having homosexuals as neighbors and value zero if the respondent says she would *not care*, *somewhat like*, or *strongly like*. We refer to this measure as *prejudice towards homosexuals* from now on.

We rely on the classic definition of prejudice: *"An antipathy based on faulty and inflexible generalization. It may be felt or expressed. It may be directed towards a group or an individual of that group."* (Allport, 1954, p.p. 9). We can interpret the degree of prejudice towards homosexuals of a population as an indicator of *prejudice towards diversity* under the assumption that, apart from moral constraints, individual sexual preferences do not directly affect third parts' utility.[8] In line with the interpretation of prejudice towards homosexuals as a measure of prejudice towards diversity, Inglehart (2003, 2005) has said that openness towards to gay and lesbian population is the best indicator of the general tolerance of nations.

We focus on prejudice towards homosexuals for three reasons. First, it is the most salient phenomenon in Africa: the percentage of respondents that would dislike having homosexual neighbors is much higher than the percentage that would dislike having a neighbor with a different religion, different ethnicity, born in a different country, or with HIV. [9] Second, expressing dislike for these other four groups might be capturing factors other than prejudice like access to club goods, fear of competition for rival goods, or fear of contagion. Third, the measure of prejudice towards homosexuals is almost uncorrelated with the measures of prejudice towards other four groups.[10]

We think Africa is the ideal laboratory to investigate both the historical and contempo-

[8]We believe this is a reasonable assumption in the African context. First, since the African continent is experiencing a demographic boom, a high reproduction rate is not more important for the well being of the communities, by the contrary. Second, the gay movement is insignificant or even forbidden in most of the African continent. Then, it is very implausible that gays have an agenda for club goods or impose any threat to political groups without such type of organization. Third, same-sex relations are not the main vector of transmission of HIV in Africa.

[9]78.2% of the respondents mentioned they would "strongly dislike" or "somewhat dislike" having homosexuals as neighbors against 11.6% for individuals from a different religion, 8.6% for individuals from a different ethnicity, 18.8% for immigrants and foreigner workers, 28.8% and for people who have HIV.

[10]The highest correlation between our measure and any of the other four measures is .16 while the correlations between the other four measures range from .32 and .71. These low correlations are essential to motivate the use of question 89C as a measure of *prejudice over third part choices*. Question 89C could be capturing a general type of prejudice/intolerance - *prejudice towards third part choices and innate traits* - if the correlations were high.

raneous determinants of prejudice towards homosexuals for three reasons. First, Africans show a high level of prejudice towards homosexuals in comparison to other continents.[11] Second, historical accounts indicate that same-sex relationships were not rare in several African tribes (Murray and Roscoe, 1998; Epprecht, 2008). Third, on the methodological side, the African continent provides potential sources of exogenous variation in institutions generated by the external intervention of other countries (e.g., religious missions, borders, colonial laws).

We provide new descriptive evidence on the determinants of prejudice towards homosexuals. Male individuals have a higher prejudice towards homosexuals on average. Older individuals have higher prejudice towards homosexuals on average. Individuals in better material conditions have lower prejudice towards homosexuals on average. Post-secondary education is negatively related prejudice towards homosexuals. Surprisingly, primary education is positively related with prejudice towards homosexuals. Muslim religious affiliations and a group with mainly new Protestant religious affiliations have a higher prejudice towards homosexuals on average. Individuals with weaker religiosity - measured by frequency of church attendance - have a lower prejudice towards homosexuals.

The negative and significant relationships between youth, post-secondary education, and better material conditions are consistent with the long-debated *modernization theory* hypothesis. The higher level of prejudice towards homosexuals among individuals with Muslim/new Protestant affiliations is consistent with the hypothesis that less intrinsic/more extrinsic forms of religiosity and more extreme interpretations from the religious texts from individuals of both groups are important explanations for differences across individuals in prejudice towards homosexuals.

We also test the hypothesis that exposure to Christian missions increased individual-level of prejudice towards homosexuals in contemporary Africa. We find evidence against this hypothesis: exposure to missionary activity at the village level has no significant relationship with individual level prejudice towards homosexuals and, if anything, exposure

[11]The World Value Survey has a question that asks (spontaneously) which type of individuals the respondent would not like to have as neighbors. The percentage of respondents who mentioned homosexuals is around 25% in the Latin American (and European) countries in the sample and 67% in the African countries in the sample.

to missionary activity at the ethnic group level has a negative relationship with individual level prejudice towards homosexuals. We also document that mechanisms varying within groups of religious, education, and religious practices explain most of the relationship between exposure to missions at the ethnic group level and prejudice towards homosexuals. One plausible rationalization for these findings is that individuals of ethnic groups exposed to traditional Christian affiliations (e.g., Catholic, traditional Protestant) developed more intrinsic/less extrinsic forms of religiosity and less extreme interpretations from the religious texts which persisted over time.

This research is related to two different strands of literature. First, this chapter is related to the economic literature on social intolerance (Corneo and Jeanne, 2009; Berggren and Nilsson, 2010, 2014, 2015).[12] The existing evidence is scarce. The existing causal evidence (Berggren and Nilsson 2010, 2014, 2015) uses aggregate measures of social intolerance and rely on identification strategies based on fixed-effect models or instrumental variables with cross-country data. The existing descriptive evidence Corneo and Jeanne (2009) uses microdata from 23 countries in World Value Surveys to test specific predictions from a theoretical model. We improve the existing evidence by estimating correlations between individual-level measures of prejudice towards homosexuals and a larger set of potential determinants using fixed effects for sub-national administrative regions and data from more than 30 countries of a continent where prejudice towards homosexuals is a salient phenom. We also differ from the literature by providing evidence in the context of developing countries, where legal protection to minorities is weaker.

Second, we relate with the empirical literature studying the long-run effects of colonial institutions on economic variables (Nunn, 2010; Nunn and Wantchekon, 2011; Michaloupolos and Papaioannou, 2014; Fenske, 2015; Cagé and Rueda, 2016, 2017). Nunn (2010) uses the Roome (1924)'s map to create indicators of exposure to Christian missions at the ethnic and village level and find that exposure to missions at both levels increase conversion to Christian affiliations. Cagé and Rueda (2016) use data from The Geography Atlas of Protestant Missions (1903) to measure distance to missions with a printing press and show that locations close to mission with a printing press have higher newspaper

[12]Our definition of prejudice towards diversity is closely related to the concept of *social intolerance* used by these papers.

readership, trust, education, and political participation. Cagé and Rueda (2016) show that regions close to historical mission settlements are more likely to have a high HIV prevalence today and that this relationship is driven by negative perceptions of condom use in regions close to missions without health investments. We contribute to this literature by testing if the historical presence of missions at the ethnic group and village levels is related to contemporary levels of prejudice towards homosexuals at the individual level. To the best of our knowledge, we are the first to test this hypothesis.

This paper is organized as follows. Section 1 describes different determinants of prejudice towards homosexuals (social intolerance) highlighted by social scientists. Section 2 describes the data sets used in the empirical analysis. Section 3 describes the new descriptive evidence on the determinants of prejudice towards homosexuals. Section 4 describes the relationship between exposure to Christian missions and the within-country distribution of prejudice towards homosexuals in contemporary Africa.

2.1 Theory

This section describes the main determinants of prejudice towards homosexuals (preference for diversity) proposed by social scientists and social psychologists.

Material conditions. Inglehart and Welzel (2005) provide a recent interpretation of the so-called *modernization theory*:

> *"Socioeconomic modernization reduces the external constraints on human choice by increasing people's material, cognitive, and social resources. This brings growing mass emphasis on self-expression values, which in turn leads to growing public demands for civil and political liberties, gender equality, and responsive government, helping to establish and sustain the institutions best suited to maximize human choice - in a word, democracy."*

According to this theory, greater development leads to the praise of self-expression, which should favor the recognition of diversity and reduce prejudice towards individuals with different ways of living, including homosexuals. Hence, a priori, we expect better material conditions to have a negative impact on prejudice towards homosexuals.

Education. Education should reduce prejudice through several different mechanisms. First, it increases lifetime income, which reduces prejudice towards diversity according to the *modernization theory*. Second, it increases the *cognitive ability* of individuals, which avoids literal and extremist readings of religious texts and allow contextualization of religious the messages. Third, by increasing chances of working in market activities instead of subsistence activities, it increases chances of *non-threatening contact* with individuals having different ways of living, which should reduce prejudice according to Gordon Allport's *contact hypothesis*.

However, in certain contexts, higher educational attainment is associated with indoctrination from political or religious groups (e.g., missionary schools, military schools), which might promote prejudice. Hence, a priori, the expected effect of education on sexual prejudice is ambiguous.

Religion and Religious practices. In a seminal work about prejudice, Allport (1954) predicts an ambiguous relationship between religion and prejudice. On the one hand, religious messages teach tolerance, compassion, and altruism. On the other hand, religion is sometimes a central element on conflicts and wars. Social psychology evidence (Batson, Schoenrade, and Ventis, 1993; Hunsberger and Jackson, 2005; Hall, Matz, and Wood, 2010) shows that religiosity is related with prejudice, but the sign of this relation is heterogeneous (positive/negative) across studies.

The main proposed explanation for these ambiguous findings is that religious orientations are *heterogeneous on their motivations and practices*. Allport and Ross (1967) conceptualize two types of religious orientations. *Intrinsic orientation* are mature and motivated by genuine faith. *Extrinsic orientations* are immature, motivated by selfish reasons (e.g., social status, access to club goods), and use religion in an instrumental way. Allport and Ross (1967) also hypothesize that extrinsic religious orientations should have more prejudice, and intrinsic religious orientations should have more tolerance. Later research partially confirmed the two hypothesis (see Batson et al., 1993, Batson and Stocks, 2005; Hunsberger and Jackson, 2005): extrinsic orientations usually have a positive relationship with prejudice in the data, but intrinsic orientations showed inconsistent results.

Relevant to our question, religions also differ in the presence of passages interpreted as a condemning of homosexual relationships in their religious texts. For example, Christianity and Islam explicitly condemn homosexuality while Hinduism and African Candomblé do not. Hence, conversion from religions that do not condemn homosexuality to Christianity and Islam should increase sexual prejudice. Finally, sexual prejudice can also change within religious affiliation depending on the intensity of the religiosity across individuals and the interpretation of the religious texts. Hence, given the multiplicity of potential mechanisms and conflicting evidence, the expected effect of education on sexual prejudice is a priori ambiguous.

2.2 Data

We use data from the Afrobarometer (AB) Wave 6 released in December of 2016. For the first time, AB included a measure of individual prejudice attitudes for more than 30 African countries. Our prejudice measure is based question 89C of the AB Wave 6: *"For each of the following types of people, please tell me whether you would like having people from this group [homosexuals] as neighbors, dislike it, or not care."*. Our main outcome is a dummy variable that takes value one if the respondent says he would *strongly dislike* or *somewhat dislike* having homosexuals as neighbors. Our outcome can distinguish between individuals with *negative feelings* towards homosexuals (diversity) and individuals with *neutral or positive feelings* towards homosexuals (diversity), what is in line with the classic definition of prejudice.

We geolocalize respondents using low-level geographical references obtained by request and internet gazetteers. Information about the location of religious missions is taken from Nunn (2010). We merged respondents to mission treatment variables according to their coordinates.

We obtain information from the pre-colonial cultural traits of ethnic groups in the Murdock's Ethnographic Atlas (1967). We manually merge self-declared AB's information on ethnicity with Murdock's classification. We obtain control variables from the replication packages of the main papers in the institutions' literature using African data. When choosing controls, we tried to follow the more recent set of controls used in published

papers.

We use three samples in our analysis. The *W6 Full sample* contains all 48030 Wave 6 AB's respondents with non-missing information about individual level controls and sub-national geographical units. The *Full sample* contains all 33981 Wave 6 AB's respondents with non-missing information about individual level controls, sub-national geographical units, and the ethnic group identified in Murdock's classification. *R3 sample* contains all 25549 Wave 6 AB's on *Full sample* whose country was present in the AB Wave 3.

2.3 Descriptive Evidence

We start the analysis by asking the data what is the partial correlation between prejudice towards homosexuals and the main determinants of sexual prejudice described in the previous section. We estimate the regression model using the observations in *W6 Full sample*

$$prejudice\_towards\_homosexuals_i = \alpha_d + \beta determinant_i + \gamma \mathbf{X}_i + \varepsilon_i$$

where $prejudice\_towards\_homosexuals_i$ is dummy variable that is equal to 1 when the respondent i answered she *strongly dislike* or *somewhat dislike* having homosexuals as neighbors; $determinant_i$ is the determinant of prejudice in a given table (e.g., gender, education category, etc); α_d are district fixed-effects; and $\mathbf{X}_i$ is a vector of individual level controls. The specification in the last column of each table presents a regression model including all determinants and all controls.

All specifications use standard errors clustered at the district-level. $\mathbf{X}_i$ include a set of controls obtained from AB Wave 6: a dummy variable indicating if respondent resides in an urban setting; occupation dummies for the different categories coded in the AB; and village controls, such as access to electricity, piped water, or sewage system.

Gender. Table 2.1 shows that female individuals have lower levels of prejudice towards homosexuals than male individuals. In the specification with individual controls and district fixed-effects in column (1), female respondents have 0.7% lower prejudice towards

homosexuals than male respondents. The specifications in columns (2) to (5) include, sequentially, village controls, education fixed-effects, religion fixed-effects and religious practice fixed-effects to the specification of column (1). The partial correlations in columns (2) to (5) are very similar, if not stronger, to the one in column (1).

The negative partial correlation between female gender and prejudice towards homosexuals is in line with the predictions of Corneo and Jeanne (2009)'s model of endogenous social tolerance. They argue that parents teach more social tolerance to female children because the marginal return of the investment in social tolerance is higher for female children than for male ones (e.g., tolerant female might have higher chance to find a tolerant husband, intolerant male individuals might have more professional success).

Age. Table 2.1 also shows a positive relationship between age and prejudice towards homosexuals. The reference group is the set of respondents with 35-44 years. The specification in column (1) shows that individuals between 18-24 and 25-34 years have, respectively, 2% and 1.4% lower prejudice towards homosexuals than individuals in the reference group. The specification in column (1) also shows that individuals between 45-54 and with more than 55 years have, respectively, 1.8% and 3.3% higher prejudice towards homosexuals than individuals in the reference group.

Overall, the estimates indicate that age is an important predictor of prejudice: the youngest in the sample (between 18-24 years) have 5.3% lower prejudice than the oldest (more than 55 years), which represents $\frac{5.3\%}{78.2\%} = 6.8\%$ of the sample average. The specifications in columns (2) to (5) include, sequentially, village controls, education fixed-effects, religion fixed-effects and religious practice fixed-effects to the specification of column (1). As can be seen in the specification of columns (2) to (5), the pattern of coefficients in the specification in column (1) is robust to the inclusion of additional controls and fixed-effects.

Material conditions. Table 2.2 shows a negative partial correlation between respondents' living conditions and prejudice towards homosexuals. The reference group is the set of respondents with *neither good nor bad* material conditions. The specification in column (1) shows that individuals living in *very bad* and *fairly bad* material conditions

have, respectively, 1.8% and 1.2% higher prejudice towards homosexuals than individuals in the reference group. The specification in column (1) also shows that individual in *very good* material conditions have 2.1% lower prejudice towards homosexuals than the reference group. The specifications in columns (2) to (5) show that the pattern of coefficients in the specification in column (1) is robust to the inclusion of village controls, education fixed-effects, religion fixed-effects, and religious practice fixed-effects.

Overall the estimates indicate that living conditions are important predictors of prejudice: improving the living conditions of an individual from *very bad* to *very good* would decrease her prejudice in 3.9%, which represents $\frac{3.9\%}{78.2\%} = 5\%$ of the sample average. These partial correlations are in line with the predictions from *modernization-theory*, which states that better living conditions lead to praise of self-expression and, consequently, recognition of diversity and reduce prejudice towards different ways of living.

Education. Table 2.3 shows that education has a heterogeneous relationship with prejudice towards homosexuals. The reference group is the set of respondents with *no formal* education. The specification in column (1) shows that individuals with *primary* education have 1.2% higher prejudice than individuals in the reference group, which represents $\frac{1.2\%}{78.2\%} = 1.5\%$ of the sample average. In contrast, the specification in column (1) also shows that individuals with *post-secondary* education have 4.4% lower prejudice than the reference group, which represents $\frac{4.4\%}{78.2\%} = 5.6\%$ of the sample average. As can be seen in the specification of columns (2) to (5), the pattern of coefficients in the specification in column (1) is robust to the inclusion of additional controls and fixed-effects.

This heterogeneous relationship is in line with multiple mechanisms mediating the effect of education on prejudice towards homosexuals. The large negative partial correlation between *post-secondary* education and prejudice towards homosexuals is consistent with the effects of *modernization theory* and increasing *non-threatening contact* with individuals having different ways of living. In contrast, the positive partial correlation between *primary* education and prejudice is consistent with indoctrination from political or religious groups (e.g., missionary schools, military schools) with a high level of prejudice towards homosexuals.

Religion. Table 2.4 shows that individuals with *Muslim* and *Other* religious affiliations have higher levels of prejudice towards homosexuals than individuals with *Catholic*, *Protestants*, and *Ethnic or No* religion. The reference group is the set of respondents with *Ethnic or No* religion. *Other* affiliations is mainly constituted by many small new Protestant churches and Hindus. The specification in column (1) shows that respondents with *Muslim* religious affiliations have 3.4% higher prejudice towards than individuals in the reference group, which represents $\frac{3.4\%}{78.2\%} = 4.3\%$ of the sample average, and that individuals with *Other* religious affiliations have 3.0% lower prejudice than the reference group, which represents $\frac{3.0\%}{78.2\%} = 3.8\%$ of the sample average. We cannot reject the hypothesis that individual with *Catholic* and *Protestant* affiliations have equal average levels of prejudice. As can be seen in the specifications in columns (2) to (4), the pattern of coefficients is robust to the inclusion of village controls, education fixed-effects, and religion fixed-effects.

The higher level of prejudice towards homosexuals among individuals with Muslim/new Protestant affiliations is consistent with the hypothesis that more intrinsic/less extrinsic forms of religiosity and more extreme interpretations from the religious texts from individuals of both groups are important explanations for differences across individuals in prejudice towards homosexuals. Once we include religious practice fixed-effects in the specification in column (5), the coefficients of *Muslim* and *Other* affiliations drop around 50% and become insignificant, suggesting that differences in prejudice towards homosexuals are driven by individuals of the same religious affiliations but with different levels of church attendance. These results are consistent the hypothesis that within *Muslim* and *Other* religious affiliations, individuals with different levels of church attendance have less intrinsic/more extrinsic forms of religiosity and more extreme interpretations from the religious texts and, consequently, higher levels of prejudice towards homosexuals.

Religious practices. Table 2.5 shows that respondents with lower levels of church attendance have a lower prejudice towards homosexuals on average. The reference group is the set of individuals who attend church *once per week*. Column (1) shows that respondents who *never* attend church and attend church *once per month or less* have, respectively, 1.9% and 2.4% lower prejudice towards homosexuals than the reference group. There are no significant differences in prejudice towards homosexuals between individuals who

attend church *once per day* or *more than once per day* and the reference group. As can be seen in the specifications in columns (2) to (5), the pattern of coefficients remains unchanged once we include village controls, education fixed-effects, religion fixed-effects and religious practice fixed-effects. These results are consistent the hypothesis that individuals who *never* attend church or attend church *once per month or less* have more intrinsic/less extrinsic religious practices and-or less extreme interpretations of the same religious message than individuals with other levels of religious attendance.

2.4 Exposure to Missionary Activity

In this section, we test the hypothesis that the hypothesis that historical exposure to Christian missions increased prejudice towards homosexuals of individuals in contemporary Africa. We estimate the regression model

$$prejudice\_towards\_homosexuals_{i,e,v,c} = \alpha_c + \beta_1 M_e + \beta_2 M_v + \gamma_1 \mathbf{X}_e + \gamma_2 \mathbf{X}_v + \gamma_3 \mathbf{X}_i + \varepsilon_i$$

where $prejudice\_towards\_homosexuals_{i,e,v,c}$ is a dummy variable equaling one when the individual i of ethnic group e living in village v in country c answered that she would *strongly dislike* or *somewhat dislike* having homosexuals as neighbors; α_c is a country fixed effect; M_e is an intensity margin measure of exposure to missionary activity of ethnicity e; M_v is an intensive margin measure of exposure to missionary activity of village v. X_e, X_v, X_i are, respectively, vectors of controls at the ethic group, village, and individual level.

The definition of the treatment variables M_e and M_v follows Nunn (2010)[13]. M_e is the logarithm of one plus the normalized (per 1000 square kilometers) number of missions inside the polygon of ethnic group e in the Murdock (1959)'s map. M_v is the logarithm of one plus the number of missions stations inside a 25-kilometer radius around the coordinates of village v. M_e is a measure of historic exposure of ancestors of respondent i to missionary activity and M_v is a measure of historic presence of mission stations in a given location.

[13]We follow the footnote 3 of Nunn (2010): *"Because the measure is left skewed, I take the natural log of one plus the normalized number of missions."*. Then, we define the mission treatment at the ethnic group level $M_e = Log(1 + (num\_missions_e/area_e))$. By analogy, we define the mission treatment at village level as $M_v = Log(1 + (num\_missions_e/3.14 \cdot 25^2))$.

When choosing the set of controls, we follow Nunn (2010, 2014) as close as possible. X_i is a vector of individual-level control variables including a gender indicator variable, age group dummies, occupation fixed effects, quality of living condition fixed effects, and an indicator variable that equals one if the respondent lives in an urban location. X_e is a vector of ethnicity level control variables including: an indicator that equals one if early European explorers contacted the ethnic group e; an indicator variable equaling one if the ethnic group was connected to the colonial railway network, the fraction of the ethnic group's land suitable for cultivation, the fraction of land within ten kilometers of a water source, and a measure of the ancestral exposure of the ethnic group to the trans-Atlantic and Indian Ocean slave trades. X_v includes the same set of controls listed in X_e but measured at the village level. We also include two additional controls used by Cagé and Rueda (2016): the average of the Malaria Ecology Index at the ethnic and village level.

β_1 and β_2 have a causal interpretation if, conditioning on X_e and X_v, selection of missions stations into locations is as good as random. This assumption implies that X_e and X_v includes all relevant variables at the village and ethnic group level explaining both the location of mission stations and prejudice towards homosexuals. Since we believe this is a strong assumption, we prefer to interpret our coefficients as partial correlations. However, given the extensive set of controls of variables measuring factors that predict the location of the missions, it is not unlikely that the partial correlation is close to the actual causal effect of both missions treatments.

Table 2.6 shows estimates of β_1 and β_2 in two different samples. The *Full Sample* includes the 25 countries in the AB round 6 for which we could identify the ethnic groups in Murdock's classification. The *R3 Sample* includes the 16 countries in the *Full Sample* also included in the AB round 3 and used by Nunn (2010) and Nunn (2014). Specifications in columns (1) to (3) show estimates for the R3 Sample and specifications in columns (4) to (6) for the Full Sample. We cluster standard errors at the ethnic group level in columns (1) and (4), at the village level in columns (2) and (5), and within ethnic groups and within districts in columns (3) and (6) using the procedure of Cameron, Gelbach, and Miller (2006). The specification in column (3) shows a negative and significant at 1% partial correlation between the mission treatment at the ethnic group level and prejudice towards homosexuals in the R3 Sample. β_1 is of small magnitude in R3 Sample: an

increase of one standard deviation in the exposure to missions at the ethnic group level decreases prejudice towards homosexuals in -1.9%, which represents -1.51% of the sample average. In contrast, the same specification shows a non-significant zero partial correlation between exposure to missionary activity at the village level and prejudice towards homosexuals. The specification in column (6) shows a negative and significant at 10% partial correlation between the mission treatment at the ethnic group level and prejudice towards homosexuals in the Full Sample. In contrast, the same specification shows a non-significant at 10% partial correlation between exposure to missionary activity at the village level and prejudice towards homosexuals. β_1 has a smaller magnitude in the Full Sample than in the R3 Sample: an increase of one standard deviation in the exposure to missions at the ethnic group level decreases prejudice towards homosexuals in around .9%, which represents .73% of the sample average. Overall, the pattern of coefficients in the Full Sample is similar to the one observed in the R3 Sample but with smaller magnitudes.

The presence of historical missions at the ethnic group and village levels should affect the contemporary levels of prejudice towards homosexuals by changing the distribution of its main determinants (e.g., religious affiliation, religiosity, and education). Tables 2.7 and 2.8 in the Appendix replicate the exercises of Nunn (2010) and Nunn (2014), who estimate the effects of exposure to historical missionary activity at the ethnic group and village level on conversion to Christian religions and education, respectively. We replicate both exercises to validate exercises and to test if two relevant mechanisms (religious conversion and education) are affected by the missions treatments in our data.

The specification in column (3) of Table 2.7 shows evidence that missionary activity at the ethnic group level is related to conversion to Christian affiliations but no evidence that exposure to missionary activity at the village level is related to conversion to Christian affiliations.[14] Overall, the evidence that exposure to missionary activity at the ethnic group level and village level is weaker in our data than in Nunn (2010)'s data.

The specification in column (3) of Table 2.8 corroborate the findings that exposure to missionary activity at the ethnic group level and village level is positively related to educational attainment in contemporary Africa. The estimates of β_1 and β_2 in Table 2.8

[14] Nunn (2010) uses a logit while we use a linear probability model. The lack of correlation between the mission treatment at the village and the indicator variable for Christian respondents remain if we use a logit.

are always positive and significant at 10% both in the R3 Sample and the Full Sample. In R3 Sample, the standardized effect of β_1 is .046, against .047 of Nunn (2014), and the standardized β_2 is .015, against .036 of Nunn (2014).

Table 2.9 repeat the benchmark specification in Table 2.6 but sequentially adding group fixed effects for religion, religious practices, and education. If one of these variables mediate the effect of exposure to missions at the ethnic group level, we should see a reduction in the coefficients of β_1 and β_2 when we include the corresponding set of fixed effects in the regression. In the specification in column (1), β_1 drops $(.069 - .065)/.069 = 5.7\%$ when we include the religious groups' dummies in the regression in sample 1. Similarly, in the specification in column (4), β_1 drops $(.035 - .032)/.035 = 8.5\%$ when we include the religious groups' dummies in the regression in sample 2. Both results suggest that conversion to Christian religions plays a small role in explaining the effect of exposure to missions at the ethnic group level on prejudice towards homosexuals. We do not observe similar changes in β_1 when we include religious practices and education fixed effects in the regressions in the R3 Sample and the Full Sample, which suggest that both mechanisms play no role in explaining the effect of exposure to missions at the ethnic group level on prejudice towards homosexuals. Altogether the results in Table 2.9 suggest that most of the mechanisms explaining the relationship between exposure to missions at the ethnic group level and prejudice towards homosexuals vary within religious, education, and religious practices groups. One plausible rationalization for these findings is that individuals of ethnic groups exposed to traditional Christian affiliations (e.g., Catholic, traditional Protestant) developed more intrinsic/less extrinsic forms of religiosity and less extreme interpretations from the religious texts which persisted over time.

Conclusion

In this chapter, we studied the unusually high levels of prejudice towards homosexuals in contemporary Africa to understand why some individuals display high levels of prejudice against individuals with choices to which they disagree while others are indifferent or even like them.

We produced two pieces of evidence to shed light on this question. First, we docu-

mented original descriptive evidence showing partial correlations between prejudice towards homosexuals and its main determinants. Second, we also test the hypothesis that exposure to Christian missions increased prejudice towards homosexuals across individuals in contemporary Africa.

The negative and significant relationships between youth, post-secondary education, and better material conditions are consistent with the long-debated *modernization theory* hypothesis. The higher level of prejudice towards homosexuals among individuals with Muslim/new Protestant affiliations is consistent with the hypothesis that less intrinsic/more extrinsic forms of religiosity and more extreme interpretations from the religious texts from individuals of both groups are important explanations for differences across individuals in prejudice towards homosexuals.

We find evidence against the hypothesis that exposure to Christian missions increased prejudice towards homosexuals in Africa. Exposure to missionary activity at the village level is uncorrelated with individual level prejudice towards homosexuals and, if anything, exposure to missionary activity at the ethnic group level has a negative relationship with individual level prejudice towards homosexuals. We also document that mechanisms varying within groups of religious, education, and religious practices explain most of the relationship between exposure to missions at the ethnic group level and prejudice towards homosexuals. One plausible rationalization for these findings is that individuals of ethnic groups exposed to traditional Christian affiliations (e.g., Catholic, traditional Protestant) developed more intrinsic/less extrinsic forms of religiosity and less extreme interpretations from the religious texts which persisted over time.

The big picture of our results suggests two messages. First, differences in religious practices and interpretations across and within religious groups are essential to explain why some individual display more in prejudice towards diversity (towards homosexuals) than others. Second, ancestral exposure to missionary activity of traditional Christian affiliations (e.g., traditional Protestant and Catholics) did not "export" prejudice towards diversity (towards homosexuals) to Africa but helped to moderate it.

Table 2.1: Correlates of Prejudice: Age and Gender

	(1) b/se	(2) b/se	(3) b/se	(4) b/se	(5) b/se
Female	-0.007*	-0.007*	-0.009**	-0.009**	-0.011***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Age [18-24]	-0.020***	-0.021***	-0.020***	-0.021***	-0.020***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Age [25-34]	-0.014**	-0.014***	-0.012**	-0.012**	-0.012**
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Age [45-54]	0.018***	0.018***	0.016***	0.016***	0.016***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Age [55+]	0.033***	0.033***	0.029***	0.029***	0.029***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Individual Controls	Yes	Yes	Yes	Yes	Yes
Village Controls	No	Yes	Yes	Yes	Yes
Education FE	No	No	Yes	Yes	Yes
Religion FE	No	No	No	Yes	Yes
Practice FE	No	No	No	No	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Observations	48030	48030	48030	48030	48030
Clusters	2170	2170	2170	2170	2170
R-Squared	0.408	0.408	0.409	0.409	0.410

Clustered standard errors (DISTRICT) reported between parentheses. FE: UNIQUEEA
Dep. Variable: DUM_PREJUD, **Reference Cat: AGE [35-44]**

Table 2.2: Correlates of Prejudice: Material Conditions

	(1) b/se	(2) b/se	(3) b/se	(4) b/se	(5) b/se
Living Cond. [Very Bad]	0.018***	0.018***	0.017**	0.017**	0.017**
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Living Cond. [Fairly Bad]	0.012*	0.012*	0.011*	0.011*	0.011*
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Living Cond. [Fairly Good]	-0.007	-0.007	-0.007	-0.007	-0.007
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Living Cond. [Very Good]	-0.021*	-0.021*	-0.020*	-0.021*	-0.020*
	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)
Individual Controls	Yes	Yes	Yes	Yes	Yes
Village Controls	No	Yes	Yes	Yes	Yes
Education FE	No	No	Yes	Yes	Yes
Religion FE	No	No	No	Yes	Yes
Practice FE	No	No	No	No	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Observations	48030	48030	48030	48030	48030
Clusters	2170	2170	2170	2170	2170
R-Squared	0.408	0.408	0.409	0.409	0.410

Clustered standard errors (DISTRICT) reported between parentheses. FE: UNIQUEEA
Dep. Variable: DUM_PREJUD, **Reference Cat: Neither good nor bad**

Table 2.3: Correlates of Prejudice: Education

	(1) b/se	(2) b/se	(3) b/se	(4) b/se	(5) b/se
Education [Primary]	0.012*	0.012*	0.012*	0.012**	0.012*
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Education [Secondary]	-0.010	-0.010	-0.009	-0.008	-0.010
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Education [Post-secondary]	-0.044***	-0.044***	-0.042***	-0.041***	-0.042***
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
Individual Controls	Yes	Yes	Yes	Yes	Yes
Village Controls	No	Yes	Yes	Yes	Yes
Living Cond. FE	No	No	Yes	Yes	Yes
Religion FE	No	No	No	Yes	Yes
Practice FE	No	No	No	No	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Observations	48030	48030	48030	48030	48030
Clusters	2170	2170	2170	2170	2170
R-Squared	0.408	0.409	0.409	0.409	0.410

Clustered standard errors (DISTRICT) reported between parentheses. FE: UNIQUEEA
Dep. Variable: DUM_PREJUD, **Reference Cat: No formal education**

Table 2.4: Correlates of Prejudice: Religion

	(1) b/se	(2) b/se	(3) b/se	(4) b/se	(5) b/se
Religion [Catholic]	0.008	0.008	0.009	0.009	-0.004
	(0.011)	(0.011)	(0.011)	(0.011)	(0.013)
Religion [Protestant]	0.008	0.008	0.009	0.010	-0.005
	(0.011)	(0.011)	(0.011)	(0.011)	(0.012)
Religion [Muslim]	0.034***	0.034***	0.034**	0.034***	0.019
	(0.013)	(0.013)	(0.013)	(0.013)	(0.015)
Religion [Other]	0.030*	0.029*	0.029*	0.031**	0.016
	(0.016)	(0.016)	(0.015)	(0.015)	(0.017)
Individual Controls	Yes	Yes	Yes	Yes	Yes
Village Controls	No	Yes	Yes	Yes	Yes
Education FE	No	No	Yes	Yes	Yes
Living Cond. FE	No	No	No	Yes	Yes
Practice FE	No	No	No	No	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Observations	48030	48030	48030	48030	48030
Clusters	2170	2170	2170	2170	2170
R-Squared	0.408	0.408	0.409	0.409	0.410

Clustered standard errors (DISTRICT) reported between parentheses. FE: UNIQUEEA
Dep. Variable: DUM_PREJUD, **Reference Cat: None or Ethnic Religion**

Table 2.5: Correlates of Prejudice: Religious Practice

	(1) b/se	(2) b/se	(3) b/se	(4) b/se	(5) b/se
Rel. Practice [Never]	-0.019**	-0.019**	-0.019**	-0.020**	-0.021**
	(0.009)	(0.009)	(0.009)	(0.009)	(0.011)
Rel. Practice [≤ Once Month]	-0.024***	-0.024***	-0.024***	-0.024***	-0.025***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Rel. Practice [≤ Once Day]	0.003	0.003	0.003	0.003	0.003
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Rel. Practice [> Once Day]	0.004	0.004	0.004	0.005	0.000
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Individual Controls	Yes	Yes	Yes	Yes	Yes
Village Controls	No	Yes	Yes	Yes	Yes
Education FE	No	No	Yes	Yes	Yes
Living Cond. FE	No	No	No	Yes	Yes
Religion FE	No	No	No	No	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Observations	48030	48030	48030	48030	48030
Clusters	2170	2170	2170	2170	2170
R-Squared	0.408	0.408	0.409	0.409	0.410

Clustered standard errors (DISTRICT) reported between parentheses. FE: UNIQUEEA
Dep. Variable: DUM_PREJUD, **Reference Cat: Once a week**

Table 2.6: Christian Missions and Sexual Prejudice

	R3 Sample (16 countries)			Full Sample (25 countries)		
	(1)	(2)	(3)	(4)	(5)	(6)
	b/se/p	b/se/p	b/se/p	b/se/p	b/se/p	b/se/p
Missions (Ethnicity)	-0.072***		-0.069***	-0.040**		-0.035*
	(0.022)		(0.021)	(0.019)		(0.019)
	[0.001]		[0.001]	[0.041]		[0.060]
Missions (Village)		-0.003	0.003		-0.001	0.002
		(0.009)	(0.009)		(0.008)	(0.008)
		[0.693]	[0.706]		[0.898]	[0.778]
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes
Ethnicity Controls	Yes	No	Yes	Yes	No	Yes
Village Controls	No	Yes	Yes	No	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	25549	25549	25549	33981	33981	33981
Clusters	191	1331	.	242	1647	.
R-Squared	0.24	0.24	0.25	0.22	0.22	0.22

Clustered standard errors (p-values) reported between parentheses (brackets).
ETHNIC class: ethnic, GEO class: aiddata25, Dep. Variable: DUM_PREJUD

Table 2.7: Christian Missions and Conversion

	R3 Sample (16 countries)			Full Sample (25 countries)		
	(1)	(2)	(3)	(4)	(5)	(6)
	b/se/p	b/se/p	b/se/p	b/se/p	b/se/p	b/se/p
Missions (Ethnicity)	0.166*		0.153*	0.125		0.113
	(0.095)		(0.089)	(0.085)		(0.080)
	[0.082]		[0.086]	[0.143]		[0.159]
Missions (Village)		0.025	-0.000		0.019	0.005
		(0.020)	(0.020)		(0.016)	(0.017)
		[0.210]	[0.983]		[0.237]	[0.768]
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes
Ethnicity Controls	Yes	No	Yes	Yes	No	Yes
Village Controls	No	Yes	Yes	No	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	25549	25549	25549	33981	33981	33981
Clusters	191	1331	.	242	1647	.
R-Squared	0.32	0.30	0.33	0.39	0.38	0.40

Clustered standard errors (p-values) reported between parentheses (brackets).
ETHNIC class: ethnic, GEO class: aiddata25, Dep. Variable: DUM_CHRIST

Table 2.8: Christian Missions and Educational Attainment

	R3 Sample (16 countries)			Full Sample (25 countries)		
	(1)	(2)	(3)	(4)	(5)	(6)
	b/se/p	b/se/p	b/se/p	b/se/p	b/se/p	b/se/p
Missions (Ethnicity)	0.562***		0.420***	0.381**		0.230*
	(0.165)		(0.144)	(0.151)		(0.131)
	[0.001]		[0.004]	[0.012]		[0.079]
Missions (Village)		0.182***	0.138***		0.206***	0.185***
		(0.043)	(0.047)		(0.039)	(0.047)
		[0.000]	[0.003]		[0.000]	[0.000]
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes
Ethnicity Controls	Yes	No	Yes	Yes	No	Yes
Village Controls	No	Yes	Yes	No	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	25549	25549	25549	33981	33981	33981
Clusters	191	1331	.	242	1647	.
R-Squared	0.46	0.48	0.48	0.48	0.50	0.50

Clustered standard errors (p-values) reported between parentheses (brackets).
ETHNIC class: ethnic, GEO class: aiddata25, Dep. Variable: Q97

Table 2.9: Christian Missions and Sexual Prejudice (Channels)

	R3 Sample (16 countries)			Full Sample (25 countries)		
	(1)	(2)	(3)	(4)	(5)	(6)
	b/se/p	b/se/p	b/se/p	b/se/p	b/se/p	b/se/p
Missions (Ethnicity)	-0.065***	-0.064***	-0.063***	-0.033*	-0.032*	-0.032*
	(0.020)	(0.021)	(0.021)	(0.019)	(0.018)	(0.019)
	[0.001]	[0.002]	[0.002]	[0.077]	[0.084]	[0.089]
Missions (Village)	0.003	0.004	0.005	0.002	0.003	0.005
	(0.009)	(0.009)	(0.009)	(0.008)	(0.008)	(0.008)
	[0.700]	[0.675]	[0.537]	[0.771]	[0.721]	[0.552]
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes
Ethnicity Controls	Yes	Yes	Yes	Yes	Yes	Yes
Village Controls	Yes	Yes	Yes	Yes	Yes	Yes
Religion FE	Yes	Yes	Yes	Yes	Yes	Yes
Practice FE	No	Yes	Yes	No	Yes	Yes
Education FE	No	No	Yes	No	No	Yes
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	25549	25549	25549	33981	33981	33981
Clusters	.	.	.	.	.	.
R-Squared	0.25	0.25	0.25	0.23	0.23	0.23

Clustered standard errors (p-values) reported between parentheses (brackets).
ETHNIC class: ethnic, GEO class: aiddata25, Dep. Variable: DUM_PREJUD

Chapter 3

Income Segregation and Demand for Redistribution: Evidence from Brazil

"From the hill you can see it so cool
What happens there on your coast
We like everything, we want is more
From the top of the city to the edge of the pier
More than a nice tan
We want to be by your side."

(From the song "*Nós vamos invidir a sua praia*"
of the Brazilian rock band *ultraje a rigor*, a brilliant
criticism to socioeconomic segregation in Brazil.)

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% by 2050. Projections show that almost 90% of this increase will 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 such as corruption and governance (Campante and Do, 2014; Campante, Do, and Guimarães, 2017).

Thus, the fact that the spatial distribution of voters is a relevant but overlooked topic by political-economists 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 demand for redistribution? 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, to the best of my knowledge, it has been relatively silent when social classes define groups. The objective of this paper is to help to fill this gap by estimating the effect of affluent segregation on demand for redistribution.

We follow the sociology literature and measure affluent segregation using a two-group Theil (1972) segregation index of individuals above/below the 90th percentile of the within city income distribution. We refer to income segregation as *affluent segregation* hereinafter. We use the vote-share of the main Brazilian left-wing party in presidential elections as a proxy for the *intensive margin* of demand for redistribution.

The knowledge of the relationship between affluent segregation and demand for redistribution can help governments to understand which features of the population spatial distribution induce political choices that increase the supply of public goods for the poor and reduce the within city income inequality. For example, urban planners can use land regulation as a distributive policy by reducing the segregation of the affluent.

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

Brazil is a suitable context to study the consequences of affluent segregation. First, the relevant identity for segregation is plausibly socioeconomic instead of racial in Brazil because of a mixed-race population and limited racial tensions. Second, Brazil is a large continental country with a predominantly urban population, what produces a reasonable

number of large urban areas with different geographic features.

The relationship between affluent segregation and the demand for redistribution is theoretically ambiguous. First, *peer-effects* have an ambiguous effect on demand for redistribution because the mechanisms that mediate the effects of increasing within income 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. For example, affluent individuals that interact less with affluent peers can demand less redistribution because of a more accurate perception about her income percentile but demand more redistribution because of a more accurate perception about the overall income inequality. 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 demand for redistribution. For example, affluent individuals might demand more redistribution if exposed to negative externalities generated by nearby slums but demand less redistribution if exposed to crime from the same slums. Third, *peer-effects* and *effects of place* might have different signs. For example, affluent individuals might demand less redistribution because of negative out-group feelings generated by violent interactions with slum dwellers but demand more redistribution to reduce negative externalities generated by the same slums. Finally, all mechanisms might be heterogeneous by income-groups, making the average effects ambiguous. For example, less segregation can increase perceived levels of inequality among all income groups but only affect the ones that are inequality averse.

Identifying the causal effect of affluent segregation on demand for redistribution 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 related to political preferences. Thus, to identify the causal effect of income segregation on demand for redistribution we need to find a source of exogenous variation in affluent segregation.

We use two different methods to leverage exogenous variation in income segregation.

Our first strategy leverages exogenous *spatial variation* in affluent segregation that is predicted by the percentage of the city land in margins of water basins. This identification strategy rules out bias caused by across cities sorting. The rationale behind this instrument is based on two arguments. First, the instrument is capturing the presence of internal rivers that facilitate the spatial separation of income groups. Second, the instrument is capturing the presence of positive amenities that make affluent individuals cluster in neighborhoods.

Our second strategy exploits an original panel data of income segregation measures that are comparable across censuses to estimate the effects of affluent segregation using only its *within city temporal variation*. This identification strategy rules out bias from across cities sorting that is stable over time.

Instrumental variable estimates show non-significant effects of affluent segregation on demand for redistribution. In contrast, panel data estimates show a strong and significant negative relationship between income segregation and demand for redistribution and reveal a large negative bias in the OLS estimates caused by city level time-invariant characteristics.

This study is related to three strands of literature. First, it adds one original piece of evidence to the literature that studies the relationship between segregation and political behavior, a topic that remains understudied to the best of our knowledge. The two notable exceptions are Ananat and Washington (2009) and Sands and de Kadt (2016). Ananat and Washington (2009) use an index based on the shape of railroads before US Great Migration to instrument a black-white dissimilarity index and find that more racially segregated cities elect fewer representatives who vote liberally and in favor of legislation that is favored by the blacks. Sands and de Kandt (2016) use the spatial distribution of topographic features that facilitate the spatial separation of ethnic groups to instrument white isolation and find that more segregated electoral wards have higher white racial voting in South Africa. We distinguish from both by investigating the consequences of income segregation instead of racial segregation in the context of a developing country with high inequality and limited racial tensions.

Second, it relates to the literature studying the co-determination between the demand for redistribution (taxation) and sorting decisions that determine income segrega-

tion (Schmidheiny, 2006, 2007; Windsteiger, 2018). Windsteiger (2018) develop a framework in which individuals sort by income and show that *"if people are segregated according to income, there will be less demand for redistribution in society"*. We contribute to this literature by testing the hypothesis that income segregation has a causal effect on the demand for redistribution. To the best of our knowledge, we are the first study trying to identify the causal effects of income segregation on demand for redistribution. Finally, we contribute to the sociology literature that documents levels and trends in income segregation (Watson 2009; Reardon and Bischoff, 2011) by producing original facts about trends in income segregation in a developing country for 30 years.

This chapter is organized as follows. Section 1 of the paper presents background information. Section 2 discusses how affluent segregation can affect demand for redistribution. Section 3 describes the data and how the main variables are computed. Section 4 provides descriptive evidence about income in Brazil. Section 5 discusses both identification strategies. Section 6 presents the results.

3.1 Background

Worker's party voting as a proxy for left-wing preferences. The Worker's Party - *Partido dos Trabalhadores* (PT) - is the most important left-wing party in Brazil. Created in 1980 by a union of industrial workers from São Paulo, PT adopted a democratic version of socialist ideas in its first years of existence but changed to a more social democratic agenda in the late '90s.

PT had presidential candidates in all federal elections since the start of the new Republic in 1989. Luís Inácio da Silva - popularly known as *Lula* - was the runner-up of the 1989, 1994, and 1998 elections and won the 2002 and 2006 elections. Dilma Roussef - Lula's successor - won the 2010 and 2014 elections.

We believe the PT vote-share in presidential elections is a good proxy for the *intensity margin* of demand for redistribution in Brazil for several reasons. First, estimates of parties' ideology (Power and Zucco, 2009; and Benoit and Wiesehomeier, 2009) show that PT is among the more leftist parties in Brazil. Second, PT is the party with more affiliates and the highest level of party identification in Brazil (Samuels and Zucco, 2013).

Third, PT's government focused on policies of income redistribution (e.g., real increases in minimum wage, cash transfers to the poor) instead of an agenda liberalization of values (e.g., drugs liberalization, reproductive right).

Race and Identity in Brazil. In Brazil, the poor are disproportionally black and mixed-race, and the affluent are disproportionally white. Despite the difficulty of distinguishing race and income in Brazil, some stylized facts suggest that the relevant identity for segregation is socioeconomic instead of racial in Brazil. First, racial identities are less salient because the majority of its African descent population is of mixed-race. According to the 2010 census, 43.1% of the population declared themselves as mixed-race while only 7.6% themselves declared themselves as black. Second, historical evidence says that black slaves interacted and lived close to the 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 the United States. Finally, Brazil has limited racial tensions in comparison with other multi-ethnic countries. These stylized facts support the hypothesis that the spatial separation of blacks and whites in Brazil is a byproduct of inherited initial economic conditions that persisted over time instead of deliberated policies of racial separation or willingness to overpay to live in racially homogeneous communities.

We choose our segregation measure to capture variation on top of racial segregation. Since the white are 47.7% of the Brazilian population and we define the affluent as only the 10% richer of the within city income distribution, our measure will capture segregation between affluent whites and non-affluent whites even if all affluent are white and all black are non-affluent.

3.2 How income segregation affect demand for redistribution?

We follow the exposition of Graham (2017) and interpret the effects of income segregation on demand for redistribution as a composite of *peer-effects* and *effects of place*. *Peer-effects* are changes in the demand for redistribution caused by different levels of within and across groups interactions. *Effects of place* are changes in the demand for redistribution caused

by shifts in the spatial distribution of average neighborhood amenities and neighborhood attributes induced by the reallocation of individuals in the urban space.

This section is organized around this classification: the first sub-section list plausible theoretical mechanisms that can mediate *effects of place* on demand for redistribution; the second sub-section does the same for *peer-effects*.

3.2.1 Effects of place

Negative externalities. The presence of poor households in a neighborhood can generate neighborhood attributes that produce negative externalities (e.g., lack of sewage, lack of garbage collection). The degree by which these negative externalities affect the demand for redistribution of the affluent depends on their degree of spatial segregation. Affluent individuals who live in segregated cities have limited average exposition to these negative externalities and, consequently, less incentive to demand distributive policies that reduce these externalities and improve the living conditions of the poor.

Crime. *Effects of place* mediated by crime have an ambiguous effect on demand for redistribution. If affluent segregation affects the average level of crime, its effect on demand for redistribution will depend on which policy voters believe are effective to reduce crime. Less affluent segregation can increase their exposure to crime and their demand for redistribution if it increases the belief that better living conditions reduce crime or not affect/decrease demand for redistribution if it increases the belief that is the punishment that reduces crime.

Affluent segregation can affect demand for redistribution even without affecting average levels of crime or any other feature of the crime distribution. If affluent segregation affects the exposure of crime across income groups, its effect on demand for redistribution will depend on how each group responds to the change in exposure to crime. If income segregation does not affect any feature of the crime distribution, its effects on demand for redistribution will depend on the *perceived* levels of crime and *perceived* motivations behind criminal behavior. For example, affluent individuals who interact more with the poor might believe more that crime is a consequence of scarcity instead of pre-determined personality traits and, consequently, increase demand for redistribution.

3.2.2 Peer-effects

Biased beliefs. Classic models predict that the individual demand for redistribution depends on his position in the income distribution (Romer, 1975; Meltzer and Richards, 1981), and on the levels of 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 other features of the income generating process.

Vast literature rejects this hypothesis empirically.[1] Part of this literature shows that the individual demand for redistribution correlate with the *perceived* moments of the income distribution instead of its real moments. Then, according to this evidence, income segregation should affect individual demand for redistribution if it changes the *perceived* position of the individual in the income distribution and the *perceived* features of the income generating process.

Cruces, Perez-Truglia, and Tetaz (2013) suggest a mechanism by which affluent segregation can affect demand for redistribution: inferences based on reference groups that differ from the whole population. This mechanism can affect individual demand for redistribution by changing perceptions about his position in the income order. If the affluent have preferences for interacting with in-group peers and form reference groups based on their income, they will consider themselves poorer than in reality and, consequently, demand more redistribution. By the same logic, the non-affluent will consider themselves richer than in reality and, consequently, demand less redistribution. In this case, the demand of redistribution of the affluent is increasing in their level of segregation, and the one of the non-affluent is decreasing in their level of segregation.

Inference based on biased reference groups can also affect the demand for redistribution by changing perceptions about the aggregate level of income inequality. More within group and less across groups interaction might reduce information about the income of out-group members and make individuals believe that the rest of society is similar to them. In this case, more segregated individuals from all income groups underestimate the

[1] Research shows that individuals have biased beliefs about its 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)

true level of income inequality and, consequently, demand less redistribution if they are inequality averse.

Out-group feelings. Demand for redistribution towards out-group individuals should be greater if there are positive feelings towards out-group members. Allport's (1979) seminal work on *contact* and *threat* explains how increasing across groups interactions affect out-group feelings. A sense of *threat* might emerge with exposure to the new group, translating into a negative disposition toward its members. If no sense of *threat* emerges, repeated *contact* with a new group may improve an individual's disposition toward its members. Based on this theory, the demand for redistribution of the affluent should be an increasing function of their segregation in contexts of non-threatening contact (e.g., market interactions) and a decreasing function of their segregation in contexts of threatening contact (e.g., violence from ghetto members).

Endogenous group-identification. Shayo (2009) and Klor and Shayo (2010) show that demand for redistribution depends on choices of *group identification* - which group individuals choose to identify with and extract utility from having similar outcomes. The main prediction of the framework is 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 when choosing to identify with the whole country (namely, if they choose *nation identification*). Based on this mechanism, affluent segregation will change the demand for redistribution of the poor if it affects the trade-off between choices of *group identification* and *nation identification*. *Situational identity theory* (Posner, 2004; Eifert et al., 2010) suggests that identities (e.g., ethnic, racial, social) are subject to contextual activation. In other words, similar individuals might identify with different groups depending on the context. Given the well documented empirical support for this theory, it is plausible that more affluent segregation increase the salience of socioeconomic identities and, consequently, increase class identification and demand for redistribution among the poor.

3.3 Data

Unit of analysis. The official definitions of a municipality and a metropolitan area in Brazil are based on political criteria. For example, Brazil has more than 5.5 thousand municipalities, which vary greatly in size and many of them belong to the same economic or population agglomeration. Then, to have a definition of a city with economic meaning, we follow Da Mata et al. (2007) and use *Urban Agglomeration* (UA) as the unit of observation. A UA is a set of municipalities that work as a unique functional urban area, as explained in IPEA et al. (2002).[2]

Many of the Brazilian municipalities have no consistent border across different censuses. Then, to produce UAs with fixed borders across censuses, we define UAs as groups of *Minimum Comparable Areas* (MCAs) of census municipalities. MCAs are sets of municipalities with borders that are consistent across different censuses based on the borders of one census. Our sample consists of 123 UAs of 1991 census municipalities for the censuses of 1991, 2000, and 2010.

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) - in Portuguese, *Setor Censitário*. An EA is a set of nearby housing units to be visited by the same census interviewer.[3]

It is impossible to compute segregation indexes that are comparable across censuses using observed group counts because EAs are not consistent across censuses. In order to produce EAs that are comparable across censuses, Mation (2010) developed a method based on graph-theory to produce *Minimum Comparable Areas for Enumeration Areas* (MCA-EAs). We compute our segregation indexes from income group counts at the level of 1991 census MCA-EAs produced by the method of Mation (2010).

Measuring income segregation. We compute income segregation indexes using pop-

[2]To the best of our knowledge, UAs are the closest analogous to US metropolitan areas in the Brazilian census. Then, its use as the unit of observation also improve comparability with the international literature.

[3]In perspective, the Brazilian census EAs are smaller than the US census tracts but larger than the US census blocks.

ulation census data with the number of household heads inside income-bins at the level of 1991 census MCA-EAs. We measure segregation at the level of UAs of MCAs of 1991 municipalities.

Following the sociology literature (Reardon and Bischoff, 2011; Bischoff and Reardon, 2014) and a recent application in economics (Chetty et. al.; 2017), we measure income segregation using a two-group Theil (1972) index with groups defined as individuals above/below percentiles of the income distribution.

The Theil index of the p-percentile of the households-head income distribution is defined as

$$H(p) = 1 - \sum_{e \in E} \frac{t_e}{T} \frac{E_e(p)}{E(p)}$$

where

$$E(p) = p \log_2 \frac{1}{p} + (1-p) \log_2 \frac{1}{(1-p)}.$$

T and t_e are, respectively, the population of the city and enumeration-area e. $E(p)$ is the p-percentile entropy of the city. $E_e(p)$ is defined analogously for the enumeration-area e. $H(p)$ measures how individuals whose households-head has income above the p-percentile of the households-head income distribution are segregated from individuals whose households-head has income below the p-percentile of the same distribution.

The only variable available in 1991, 2000, and 2010 censuses have income-bins based on the household-head income measured in fractions of the nominal minimum wage of the census reference-month, which implies that we do not observe income group counts at income percentiles and that income groups vary across censuses. To solve both issues, we use the interpolation procedure proposed by Bischoff and Reardon (2014) and compute segregation at the 10th, 25th, 75th, 90th percentiles. The main dependent variable of our analysis is the 90th income percentile segregation or *affluent segregation*.

Measuring the presence of margins of water basins. Water coverage data is from the Global MODIS Raster Water Mask (Carroll, Townshend, DiMiceli, Noojipady, and Sohlberg, 2009), with a resolution of 250 meters. Consider a raster dataset of a generic city with R rows and C columns divided into RC tiles. $w_{r,c} = 1$ if the tile in the r^{th} row and c^{th} column is covered by water and $w_{r,c} = 0$ if it is covered by land.

Define by $N_{r,c}^K$ the set of K^2-1 neighboring tiles of the tile r, c in the center of a square with K^2 tiles. Given a set of K surrounding tiles, a tile is defined as a margin of a water basin if it is not covered by water and at least one of his surrounding tiles is covered by water. More formally,

$$m_{r,c}^K := \begin{cases} 1, & if\ (w_{r,c}=0) \cap (\exists\ i,j \in N_{r,c}^K; w_{i,j}=1) \\ 0, & if\ (w_{r,c}=1) \cup (\nexists\ i,j \in N_{r,c}^K; w_{i,j}=1). \end{cases}$$

The percentage of the land in margins of water basins (e.g., rivers, lakes, and ponds) is defined as

$$perc\_margin^K := \frac{\sum_{r=1}^{R}\sum_{c=1}^{C} m_{rc}^K}{RC - \sum_{r=1}^{R}\sum_{c=1}^{C} w_{rc}}.$$

We computed $perc\_margin^K$ for a large number of K's and chose the instrument that explains the largest share of the variance of our income segregation measures. We use K=81 in our estimates.

Controls. We use census microdata to compute city level controls. The vector of city level controls includes: logarithm of median household per-capita income, Gini index of household per-capita income, percentage of poor households, percentage of the population living in informal settlements, logarithm of population, population density, percentage of the population that is black or mixed, average age of the population, average years of schooling of the population, an index about the availability of public services (sewerage and garbage collection), percentage of households paying rent, and percentage of individuals that migrated from other municipalities.

3.4 Identification

3.4.1 Identification in the cross section

We identify the effects of income segregation in preference for redistribution in the cross-section by leveraging exogenous variation explained by the presence of margins of water

basins. The rationale behind this instrument is based on two arguments. First, the instrument is capturing the presence of internal rivers that facilitate the spatial separation of income groups, a strategy similar to Cutler and Gleaser (1997)'s stage. Second, the instrument is capturing the presence of positive amenities that anchor neighborhoods to high incomes, an argument that is elaborated and tested by Lee and Lin (2017) using US data.

Consider the following reduced-form model:

$$y_c = \alpha_{region} + \beta segregation_c^a + u_c$$

where $y_{c,t}$ is the electoral outcome of city c, $segregation_c^g$ is the affluent segregation of city c, α_{region} is a region fixed-effect, and u_c is the error-term of city c. Consider the following first stage equation

$$segregation_c^a = \alpha_{region} + \beta perc\_margins_c + v_c.$$

where $perc\_margins_c$ is the percentage of the city in margins of water basins.

Instrumenting the *spatial variation* in city-level affluent segregation deals with endogeneity issues in two ways. First, since we use the variation on affluent segregation at the city level, we can rule out bias caused by within city sorting. Second, by using only the spatial variation in affluent segregation that is predicted by the presence of margins of water basins, we can rule out bias caused by across cities sorting under the assumption that this variable is irrelevant for the sorting decisions.

The *exclusion restriction* imposes that the presence of margins of water basins do not have any direct effect on demand for redistribution that is not through its effect on affluent segregation. This assumption is admittedly strong 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 take two steps to reduce concerns of exclusion restriction violations. First, we compare levels of affluent segregation of UAs inside the same region, which have more similar demographic characteristics and similar presence of large water basins (e.g., Amazon river). Second, our instrument also captures variation in the shape of water

basins, not only in its presence.

The *exogeneity assumption* implies that the presence of margins of water basins is uncorrelated with the part of the demand for redistribution that is not explained by affluent segregation. We believe this is a more plausible assumption that the exclusion restriction. Our instrument is unlikely to be simultaneously determined with political preferences because margins of water basins 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 the presence of margins of water basins. However, it is not evident that the presence of margins of water basins is a relevant variable for this sorting decision and, even if it is the case, migrants plausibly have a noisy prior about the *within city* distribution of margins of water basins of destination cities.

3.4.2 Identification in the panel

We estimate more refined partial correlations between affluent segregation and demand for redistribution by using only its within city temporal variation. Consider the following reduced-form model

$$y_{c,t} = \alpha_c + \alpha_t + \beta segregation^{a}_{c,t} + \gamma \mathbf{X}_{c,t-10} \cdot t + u_{c,t}$$

where $y_{c,t}$ is the electoral outcome of city c at year t, $segregation^{a}_{c,t}$ is the affluent segregation of city c at year t, α_c and α_t are city and year fixed-effects, $\mathbf{X}_{c,t-10} \cdot t$ 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. We do not include any contemporaneous control in the regression models because we believe they are endogenous to affluent segregation. We use a vector of lagged controls interacted with the time trend. The vector of lagged controls is composed by: population, household size, percentage of young population, female participation in the labor market, fertility rate, percentage of the population that is black or mixed, percentage of adult population that is illiterate, household per capita income, household Gini index of inequality, household per-capita income of the affluent. We use income groups of fixed size to rely only on temporal variation in affluent segregation that is independent of the size of the income groups without having to use a contemporaneous variable for

group size in the regressions.

This specification deals with the endogeneity of affluent segregation in three ways. First, since we measure affluent segregation at the city level, it rules out bias from within city sorting. Second, it includes city fixed-effects to exclude the possibility of bias from across cities sorting that is stable over time. Third, it has year fixed-effects and lagged controls interacted with the time trend to control for linear and non-controls for linear confounding trends related to both segregation and preference for redistribution.

3.5 Descriptive facts about income segregation in Brazil

Descriptive facts about segregation in Brazil. Table 3.1 presents descriptive statistics for the Theil (1972) segregation indexes for the 10th, 25th, 75th, and 90th percentiles of the within city total household head income distribution.

Bischoff and Reardon (2014) compute levels of income segregation for US Metropolitan Statistical Areas using the same methodology: segregation of the bottom decile of the income distribution was .146 in 2000 and 0.163 in 2010; segregation of the top decile of the income distribution was .185 in 2000 and 0.200 in 2010. Based on Bischoff and Reardon (2014)'s benchmark, we can compare levels of segregation in the US and Brazil. In contrast, segregation of the bottom income decile in Brazil is substantially lower in Brazil than in the US (in 2000, BR=.063 and US=.146; in 2010, BR=0.068 and US=0.163). Segregation of the top income decile in is very similar to US levels (in 2000, BR=.191 and US=.185; in 2010, BR=0.175 and US=0.200).

Figure 3.1 shows trends in segregation of the 10th, 25th, 75th, and 90th percentiles of the within city total household head income distribution. We document new facts about trends in income segregation in Brazil for the last three censuses. Segregation of the 10th percentile has a non-monotonic behavior between 1991 and 2010: it decreased between 1991 and 2000 but increased between 2000 and 2010. Income segregation of the 25th, 75th, and 90th percentiles declined monotonically between 1991, 2000, and 2010.

3.6 Results

We estimate all regression models using affluent segregation defined as the 90th percentile segregation. Results are very similar if we define affluent segregation defined as the 90th percentile segregation.

Results in the cross section. Table 3.2 shows OLS estimates of the relationship between affluent segregation and the PT vote-share in presidential elections using *across cities spatial variation* in affluent segregation. Columns (1) and (2) show results for 1991, (3) and (4) for 2000, and (5) and (6) for 2010. Columns (1), (3), and (5) show results from regressions with region fixed effects and columns (2), (4), and (6) show results from regressions with region state effects. Columns (1) and (2) show positive and significant at 5% partial correlations between the PT vote-share in presidential elections and affluent segregation in 1991. Columns (3) and (4) also show positive partial correlations between the PT vote-share in presidential elections and affluent segregation in 2000, but only the coefficient in column (4) is significant. The partial correlation increases in magnitudes in columns (2) and (4) when estimated using only within state variation. In contrast to columns (1) to (4), columns (5) and (6) show negative and non-significant partial correlations between the PT vote-share in presidential elections and affluent segregation in 2010.

Table 3.3 shows the first stage regressions of affluent segregation on the instrument. Column (1) shows results for 1991, (2) for 2000, and (3) for 2010. All specifications include region fixed effects and use robust standard errors. Results in columns (1) to (3) support the validity of the relevance assumption. First, the F-statistic of the instrument in the three regressions is 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. Second, the marginal effect of the instrument is of substantial magnitude in each regression model: one standard deviation increase in the percentage of the city land in margins of water basins increase the affluent segregation in around .3 standard deviations. Third, the instrument explains around seven percent of the within region spatial variation in affluent percentile segregation in each regression model.

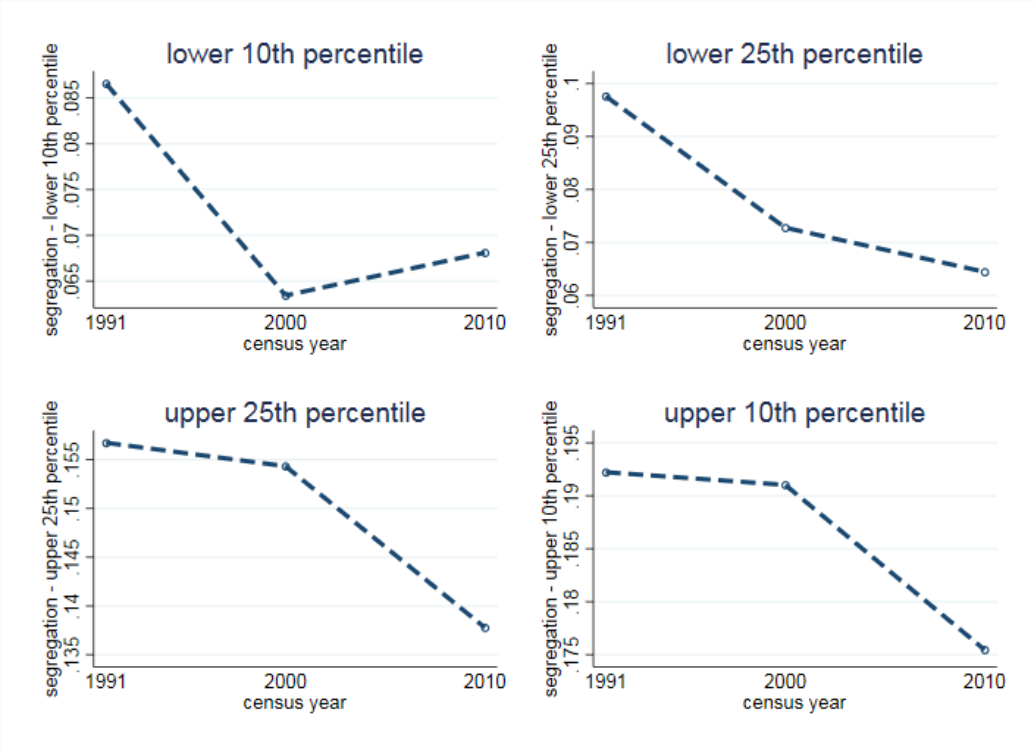
Table 3.4 shows the second stage regressions of PT vote-share on affluent segregation. Column (1) shows results for 1991, (2) for 2000, and (3) for 2010. All specifications include region fixed effects and use robust standard errors. Results in columns (1) to (3) do not support the existence of a relationship between affluent segregation and PT vote-share in presidential elections. All second stage coefficients are non-significant at 10%. Specifications in columns (1) and (2) show positive effects of affluent segregation on the PT vote-share. In contrast, the specification in column (3) shows the negative effects of affluent segregation on the PT vote-share.

Overall, results in the cross section are not conclusive about the existence of a relationship between income segregation and demand for redistribution, the sign of the relationship, and the sign of the OLS bias in the cross-section. Given the impossibility of finding a stronger and more reliable instrument for the spatial variation in affluent segregation, we decided to move the identification to a panel data set.

Results in the panel. Table 3.5 shows OLS estimates of the relationship between affluent segregation and PT vote-share in presidential elections using a panel data set with 123 UAs for three census - 1991, 2000, and 2010. All specifications use robust standard errors. The specification in column (1) does not include fixed effects. The specification in column (2) includes year fixed effects. Consistent with the partial correlations estimated using spatial variation in affluent segregation, the coefficients in columns (1) and (2) show a positive relationship between affluent segregation and the PT vote-share in presidential elections. Only the coefficient in column (2) is significant at 5%.

The inclusion of city fixed effects in column (3) reveals a large positive bias in the OLS coefficient of affluent segregation caused by city level time-invariant omitted variables. The coefficient of affluent segregation becomes negative and significant at 1% when estimated using only its within city temporal variation. The negative relationship in column (3) is of large magnitude: one standard deviation increase in affluent segregation reduces the PT vote-share in .5 standard deviations. We test the robustness of the coefficient in column (3) by re-estimating it in columns (4) to (6) but using a more refined temporal variation. The coefficient of affluent segregation is of smaller magnitude in specifications with more demanding fixed effects and lagged controls interacted with the trend, but

Figure 3.1: Trends in income segregation - 1991-2010



remains negative, significant, and of large magnitude. In our favorite specification in column (6), one standard deviation increase in affluent segregation reduces the PT vote-share in .27 standard deviations.

Conclusion

In this chapter, we study the effects of income segregation on demand for redistribution in Brazil. Our instrumental variable estimates show non-significant effects of income segregation on demand for redistribution. In contrast, our panel data estimates show a large negative bias in the OLS estimates caused by city level time-invariant characteristics and a large and significant negative relationship between income segregation and demand for redistribution. Unfortunately, it is hard to take strong conclusions from these estimates. The large negative partial correlation estimated using only within city variation is suggestive of a negative causal effect of income segregation on demand for redistribution.

Table 3.1: Average levels of income segregation - 1991-2000

income-group	**t**	**mean**	**sd**	**mean/sd**
lower 10%	1991	.087	.044	.509
lower 10%	2000	.063	.031	.495
lower 10%	2010	.068	.033	.483
lower 25%	1991	.098	.048	.493
lower 25%	2000	.073	.032	.438
lower 25%	2010	.064	.033	.519
upper 75%	1991	.157	.063	.402
upper 75%	2000	.154	.066	.431
upper 75%	2010	.138	.065	.473
upper 90%	1991	.192	.075	.389
upper 90%	2000	.191	.081	.422
upper 90%	2010	.175	.077	.430

Table 3.2: OLS estimates in the cross section

Dep var: PT vote-share	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
90th percentile segregation	24.69**	33.14***	6.180	20.95**	-18.61*	-5.956
	(9.788)	(9.093)	(9.352)	(9.619)	(10.77)	(9.346)
Observations	122	122	123	123	123	123
R-squared	0.313	0.498	0.144	0.386	0.453	0.737
Region FE	YES	YES	YES	YES	YES	YES
State FE	NO	YES	NO	YES	NO	YES
Year	1991	1991	2000	2000	2010	2010

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

Table 3.3: First Stage in the cross section

Dep var: 90th percentile segregation	**(1)**	**(2)**	**(3)**
% of land coverage in	0.00269***	0.00294***	0.00265***
margins of water basins	(0.000744)	(0.000821)	(0.000781)
Observations	122	123	123
1st stage F-statistic	13.09	12.77	11.47
1st stage partial R2	0.0719	0.0698	0.0618
Standardized beta	.304	.315	.289
Region FE	YES	YES	YES
Year	1991	2000	2010

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

Table 3.4: Second Stage in the cross section

Dep var: PT vote-share	**(1)**	**(2)**	**(3)**
90th percentile segregation	15.02 (36.97)	52.09 (32.36)	-39.63 (57.05)
Observations	122	123	123
Year	1991	2000	2010
Region FE	YES	YES	YES

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

Table 3.5: OLS estimates in the panel

Dep var: PT vote-share	**(1)**	**(2)**	**(3)**	**(4)**	textbf(5)	**(6)**
90th percentile	11.74	19.16**	-88.95***	-72.42**	-58.62**	-48.36**
segregation	(10.16)	(9.330)	(28.15)	(28.12)	(23.07)	(20.91)
Observations	368	368	368	368	368	368
Clusters	123	123	123	123	123	123
R-squared	0.004	0.475	0.726	0.811	0.897	0.914
Standardized beta	.066	.108	-.502	-.409	-.331	-.273
Year FE	NO	YES	YES	YES	YES	YES
City FE	NO	NO	YES	YES	YES	YES
Region-Year FE	NO	NO	NO	YES	YES	YES
State-Year FE	NO	NO	NO	NO	YES	YES
Lagged Controls*Year	NO	NO	NO	NO	NO	YES

Standard errors clustered at the UA level reported between parentheses.
***** p<0.01, ** p<0.05, * p<0.1**

Bibliography

Adena, Maja, Ruben Enikolopov, Maria Petrova, Veronica Santarosa, and Ekaterina Zhuravskaya, "Radio and the Rise of the Nazis in Prewar Germany," *The Quarterly Journal of Economics*, 2015, *130* (4), 1885–1939.

Alesina, Alberto and Paola Giuliano, *Preferences for Redistribution*, North Holland,

_ , _ , and Nathan Nunn, "On the Origins of Gender Roles: Women and the Plough," *Quarterly Journal of Economics*, 2013, *128* (2), 469–530.

Allport, G.W., *The nature of prejudice*, Addison-Wesley Pub. Co., 1954.

_ , *The individual and his religion: a psychological interpretation* Macmillan Paperbacks, Macmillan, 1960.

Ames, Barry, *The Deadlock of Democracy in Brazil*, Ann Arbor: University of Michigan Press, 2001.

Ananat, Elizabeth O., "The Wrong Side(s) of the Tracks: The Causal Effects of Racial Segregation on Urban Poverty and Inequality," *American Economic Journal: Applied Economics*, 2011, *3* (2), 34–66.

Ananat, Elizabeth Oltmans and Ebonya Washington, "Segregation and Black political efficacy," *Journal of Public Economics*, June 2009, *93* (5-6), 807–822.

Anderson, T. W. and Herman Rubin, "Estimation of the Parameters of a Single Equation in a Complete System of Stochastic Equations," *Ann. Math. Statist.*, 03 1949, *20* (1), 46–63.

Barone, Guglielmo, Francesco D'Acunto, and Gaia Narciso, "Telecracy: Testing for Channels of Persuasion," *American Economic Journal: Economic Policy*, May 2015, *7* (2), 30–60.

Batson, C. Daniel and E. L. Stocks, *Religion and Prejudice*, Blackwell Publishing Ltd,

Benabou, Roland and Efe A. Ok, "Social Mobility and the Demand for Redistribution: The Poum Hypothesis," *The Quarterly Journal of Economics*, 2001, *116* (2), 447–487.

Bentzen, Jeanet Sinding, "Acts of God? Religiosity and Natural Disasters Across Subnational World Districts," Discussion Papers 15-06, University of Copenhagen. Department of Economics April 2015.

Berggren, Niclas and Mikael Elinder, "Is tolerance good or bad for growth?," *Public Choice*, Jan 2012, *150* (1), 283–308.

_ **and Therese Nilsson**, "Globalization and the transmission of social values: The case of tolerance," *Journal of Comparative Economics*, 2015, *43* (2), 371 – 389.

_ **and** _ , "Tolerance in the United States: Does economic freedom transform racial, religious, political and sexual attitudes?," *European Journal of Political Economy*, 2016, *45* (Supplement), 53 – 70. On Institutions and Well Being.

Bhalotra, S., K. Harttgen, and S. Klasen, "The impact of school fees on educational attainment and the intergenerational transmission of education," Education for All Global Monitoring Report, United Nations Education, Scientific and Cultural Organizations 2013.

Bidwell, Kelly, Katherine Casey, and Rachel Glennerster, "Debates: Voting and Expenditure Responses to Political Communication," *Working Paper*, 2018.

Bjerk, David, "Theory and Evidence Regarding the E§ects of Segregation on Crime Rates," 2007.

Boas, Taylor C., "Television and Neopopulism in Latin America: Media Effects in Brazil and Peru," *Latin American Research Review*, 2005, *40* (2), 27 – 49.

Boas, Taylor C, "Mass media and politics in Latin America," *Constructing democratic governance in Latin America*, 2013, pp. 48–77.

Bowles, Jeremy and Horacio Larreguy, "Who Debates,Wins? Experimental Evidence on Debate Participation in a Liberian Election," *Working Paper*, 2018.

Brierley, Sarah, Eric Kramon, and George Ofosu, "The Moderating Effect of Debates on Political Attitudes," *Working Paper*, 2018.

Cage, Julia and Valeria Rueda, "The Long-Term Effects of the Printing Press in Sub-Saharan Africa," *American Economic Journal: Applied Economics*, July 2016, *8* (3), 69–99.

Cavalcanti, Tiago and Daniel Da Mata, "On the Determinants of Slum Formation," March 2014.

Chambers, John R., Lawton K. Swan, and Martin Heesacker, "Better Off Than We Know: Distorted Perceptions of Incomes and Income Inequality in America," *Psychological Science*, 2013.

Chong, Alberto and Eliana La Ferrara, "Television and divorce: Evidence from Brazilian novelas," *Journal of the European Economic Association*, 2009, *7* (2-3), 458–468.

Conley, Timothy, Christian Hansen, and Peter Rossi, "Plausibly Exogenous," *The Review of Economics and Statistics*, 2012, *94* (1), 260–272.

Conti, Mario Sergio, "Notícias do planalto," *São Paulo: Companhia das Letras*, 1999.

Corneo, Giacomo and Olivier Jeanne, "A theory of tolerance," *Journal of Public Economics*, 2009, *93* (5-6), 691–702.

Cruces, Guillermo, Ricardo Perez-Truglia, and Martin Tetaz, "Biased perceptions of income distribution and preferences for redistribution: Evidence from a survey experiment," *Journal of Public Economics*, 2013, *98*, 100 – 112.

Cutler, David M. and Edward L. Glaeser, "Are Ghettos Good or Bad?," *The Quarterly Journal of Economics*, August 1997, *112* (3), 827–72.

_ , **Douglas W. Elmendorf, and Richard J. Zeckhauser**, "Demographic Characteristics and the Public Bundle," NBER Working Papers 4283, National Bureau of Economic Research, Inc February 1993.

_ , **Edward L. Glaeser, and Jacob L. Vigdor**, "When are ghettos bad? Lessons from immigrant segregation in the United States," *Journal of Urban Economics*, May 2008, *63* (3), 759–774.

de Lima, Venicio A., "Televisão e política: Hipótese sobre a eleição presidencial de 1989.," *Comunicação e Política*, 1990, *9* (11), 29–54.

DellaVigna, Stefano and Ethan Kaplan, "The Fox News effect: Media bias and voting," *The Quarterly Journal of Economics*, 2007, *122* (3), 1187–1234.

_ **and Matthew Gentzkow**, "Persuasion: Empirical Evidence," *Annual Review of Economics*, 2010, *2* (1), 643–669.

_ , **Ruben Enikolopov, Vera Mironova, Maria Petrova, and Ekaterina Zhuravskaya**, "Cross-border media and nationalism: Evidence from Serbian radio in Croatia," *American Economic Journal: Applied Economics*, 2014, *6* (3), 103–132.

Downs, Anthony, *An economic theory of democracy*, New York: Harper and Row, 1957.

Druckman, James N., Matthew S. Levendusky, and Audrey McLain, "No Need to Watch: How the Effects of Partisan Media Can Spread via Interpersonal Discussions," *American Journal of Political Science*, 1978, *62* (1), 99–112.

Dulani, B., Sambo G., and K. Y. Dionne, "Good neighbours? Africans express high levels of tolerance for many, but not for all," Afrobarometer Dispatch No. 74, Afrobarometer 2016.

Edwards, Anthony C. and Mike J. Lowis, "The Batson-Schoenrade-Ventis Model of Religious Experience: Critique and Reformulation," *The International Journal for the Psychology of Religion*, 2001, *11* (4), 215–234.

Eifert, Benn, Edward Miguel, and Daniel N. Posner, "Political Competition and Ethnic Identification in Africa," *American Journal of Political Science*, 2010, *54* (2), 494–510.

Engelhardt, Carina and Andreas Wagener, "Biased Perceptions of Income Inequality and Redistribution," June 2014, (4838).

Enikolopov, Ruben, Maria Petrova, and Ekaterina Zhuravskaya, "Media and political persuasion: Evidence from Russia," *The American Economic Review*, 2011, *101* (7), 3253–3285.

Enos, Ryan D., "What the Demolition of Public Housing Teaches Us about the Impact of Racial Threat on Political Behavior," *American Journal of Political Science*, 2016, *60* (1), 123–142.

Epprecht, M., *Heterosexual Africa?: The History of an Idea from the Age of Exploration to the Age of AIDS* New African Histories, Ohio University Press, 2008.

Fenske, James, "African polygamy: Past and present," *Journal of Development Economics*, 2015, *117* (C), 58–73.

Ferrara, Eliana La, Alberto Chong, and Suzanne Duryea, "Soap operas and fertility: Evidence from Brazil," *American Economic Journal: Applied Economics*, 2012, pp. 1–31.

Fridkin, Kim L, Patrick J Kenney, Sarah Allen Gershon, Karen Shafer, and Gina Serignese Woodall, "Capturing the power of a campaign event: The 2004 presidential debate in Tempe," *Journal of Politics*, 2007, *69* (3), 770–785.

Fujiwara, Thomas, "Voting Technology, Political Responsiveness, and Infant Health: Evidence From Brazil," *Econometrica*, 2015, *83* (2), 423–464.

_ **and Leonard Wantchekon**, "Can Informed Public Deliberation Overcome Clientelism? Experimental Evidence from Benin," *American Economic Journal: Applied Economics*, October 2013, *5* (4), 241–55.

_ , _ , Gabriel López-Moctezuma, Cecilia Pe Lero, and Daniel Rubenson, "Policy Deliberation and Voter Persuasion: Experimental Evidence from an Election in the Philippines," *Working Paper*, 2017.

Garcia-Arenas, Javier, "J. Garcia-Arenas. 2015. "The Impact of Free Media on Regime Change: Evidence from Russia."," *Working Paper*, 2015.

Gentzkow, Matthew, "Television and voter turnout," *The Quarterly Journal of Economics*, 2006, pp. 931–972.

Gibson, James L., "Enigmas of Intolerance: Fifty Years after Stouffer's Communism, Conformity, and Civil Liberties," *Perspectives on Politics*, 2006, *4* (1), 21–34.

Gimpelson, Vladimirand and Daniel Treisman, "Misperceiving Inequality," May 2015, (21174).

Grigorieff, Alexis and Christopher Roth, "How does economic status affect social preferences? Experimental evidence from the US.," 2016.

Hagopian, Frances, "Parties and voters in emerging democracies," in "The Oxford Handbook of Comparative Politics" 2007.

Hall, Deborah L., David C. Matz, and Wendy Wood, "Why Don't We Practice What We Preach? A Meta-Analytic Review of Religious Racism," *Personality and Social Psychology Review*, 2010, *14* (1), 126–139. PMID: 20018983.

Han, Enze and Joseph O'Mahoney, "British colonialism and the criminalization of homosexuality," *Cambridge Review of International Affairs*, 2014, *27* (2), 268–288.

Hughes, Sallie and Chappell Lawson, "The barriers to media opening in Latin America," *Political Communication*, 2005, *22* (1), 9–25.

Ichino, Nahomi and Noah L. Nathan, "Crossing the Line: Local Ethnic Geography and Voting in Ghana," *American Political Science Review*, 004 2013, *107* (2), 344–361.

Inglehart, R. and C. Welzel, *Modernization, Cultural Change, and Democracy: The Human Development Sequence*, Cambridge University Press, 2005.

Inglehart, Ronald, Roberto Foa, Christopher Peterson, and Christian Welzel, "Development, Freedom, and Rising Happiness: A Global Perspective (1981–2007)," *Perspectives on Psychological Science*, 2008, *3* (4), 264–285. PMID: 26158947.

Kennedy, P and A. Prat, "Media Power," 2017, (12426).

King, P.T., *Toleration*, Frank Cass, 1976.

Kleibergen, Frank and Richard Paap, "Generalized reduced rank tests using the singular value decomposition," *Journal of Econometrics*, July 2006, *133* (1), 97–126.

Klor, Esteban F. and Moses Shayo, "Social identity and preferences over redistribution," *Journal of Public Economics*, 2010, *94* (3–4), 269 – 278.

Ladd, Jonathan McDonald and Gabriel S Lenz, "Exploiting a rare communication shift to document the persuasive power of the news media," *American Journal of Political Science*, 2009, *53* (2), 394–410.

Lawson, Chappell, *Building the fourth estate: Democratization and the rise of a free press in Mexico*, Univ of California Press, 2002.

_ **and James A McCann**, "Television news, Mexico's 2000 elections and media effects in emerging democracies," *British Journal of Political Science*, 2005, *35* (1), 1–30.

Levitsky, Steven and Lucan A Way, "Why democracy needs a level playing field," *Journal of Democracy*, 2010, *21* (1), 57–68.

Luttmer, Erzo F. P., "Group Loyalty and the Taste for Redistribution," *Journal of Political Economy*, June 2001, *109* (3), 500–528.

M., J. Townshend C. DiMiceli P. Noojipady R. Sohlberg Carroll, "A New Global Raster Water Mask at 250 Meter Resolution," 2099.

Mainwaring, Scott, *Rethinking party systems in the third wave of democratization: the case of Brazil*, Stanford University Press, 1999.

Major, Brenda and S. Brooke Vick, *The Psychological Impact of Prejudice*, Blackwell Publishing Ltd,

Martin, Gregory J. and Ali Yurukoglu, "Bias in Cable News: Persuasion and Polarization," *American Economic Review*, September 2017, *107* (9), 2565–99.

Massey, Douglas S. and Nancy A. Denton, "The Dimensions of Residential Segregation," *Social Forces*, 1988, *67* (2), 281–315.

Meltzer, Allan H. and Scott F. Richard, "A Rational Theory of the Size of Government," *Journal of Political Economy*, October 1981, *89* (5), 914–27.

Mounir, Mollerstrom Johanna Karadja and David Seim, "Richer (and Holier) than Thou? The Effect of RelativeIncome Improvements on Demand for Redistribution."

Murdock, G.P., *Atlas of World Cultures* University of Pittsburgh Press Digital Editions, University of Pittsburgh Press, 1981.

Murray, S.O. and W. Roscoe, *Boy-Wives and Female-Husbands: Studies in African Homosexualities*, Palgrave Macmillan, 2001.

Nathan, Noah L., "Local Ethnic Geography, Expectations of Favoritism, and Voting in Urban Ghana," *Comparative Political Studies*, 2016.

Norton, Michael I. and Dan Ariely, "Building a Better America?One Wealth Quintile at a Time," *Perspectives on Psychological Science*, 2011, *6* (1), 9–12.

Nunn, Nathan, "The Long Term Effects of Africa's Slave Trades," *Quarterly Journal of Economics*, 2008, *123* (1), 139–176.

_ , "Religious Conversion in Colonial Africa," *American Economic Review Papers and Proceedings*, 2010, *100* (2), 147–152.

_ **and Leonard Wantchekon**, "The Slave Trade and the Origins of Mistrust in Africa," *American Economic Review*, 2011, *101* (7), 3221–3252.

Papaioannou, Elias, "National Institutions and Subnational Development in Africa," *The Quarterly Journal of Economics*, 2014, *129* (1), 151–213.

Pinotti, Paolo and Andrea Tesei, "CEP Discussion Paper No 1475 April 2017 The Political Legacy of Entertainment TV Ruben Durante," 2017.

Popkin, Samuel L, *The reasoning voter: Communication and persuasion in presidential campaigns*, University of Chicago Press, 1994.

Porto, Mauro, "Mass media and politics in democratic Brazil," *Brazil since*, 1985, pp. 288–313.

Posner, Daniel N., "The Political Salience of Cultural Difference: Why Chewas and Tumbukas Are Allies in Zambia and Adversaries in Malawi," *American Political Science Review*, *98* (4), 529–545.

Prat, Andrea, "Media Power," 2017, (17-107).

Riker, Peter C. Ordeshook William H., "A Theory of the Calculus of Voting," *The American Political Science Review*, 1968, *62* (1), 25–42.

Rodden, Jonathan, "The Geographic Distribution of Political Preferences," *Annual Review of Political Science*, 2010, *13* (1), 321–340.

Romer, Thomas, "Individual welfare, majority voting, and the properties of a linear income tax," *Journal of Public Economics*, 1975, *4* (2), 163–185.

Rubim, Antonio A. C., *Comunicação, espaço público e eleições presidenciais*, Comunicação e política, 1989.

Sands, Melissa and Daniel de Kadt, "Segregation drives racial voting: New evidence from South Africa," *Working Paper*, February 2016.

Shayo, Moses, "A Model of Social Identity with an Application to Political Economy: Nation, Class, and Redistribution," *American Political Science Review*, 005 2009, *103* (2), 147–174.

Shidlo, Gil, "The Brazilian elections of 1989," *Electoral Studies*, 1990, *9* (3), 251 – 256.

Steele, Claude M., Steven J. Spencer, and Joshua Aronson, *Contending with group image: The psychology of stereotype and social identity threat*, Vol. 34 of *Advances in Experimental Social Psychology*,

Stock, James and Motohiro Yogo, *Testing for Weak Instruments in Linear IV Regression*, New York: Cambridge University Press,

Tullock, Gordon, "The Welfare Costs of Tariffs, Monopolies, and Theft," *Economic Inquiry*, 1967, *5* (3), 224–232.

UNODC, "Global study on homicide," *United Nations Office on Drugs and Crime*, 2013.

Wald, Kenneth D. and Michael B. Lupfer, "The Presidential Debate as a Civics Lesson," *The Public Opinion Quarterly*, 1978, *42* (3), 342–353.

Wiesehomeier, Nina and Kenneth Benoit, "Presidents, Parties, and Policy Competition," *The Journal of Politics*, 10 2009, *71*, 1435–1447.

Zucco, Cesar and Timothy Power, "Data and Estimates: Estimating Ideology of Brazilian Legislative Parties, 1990–2005: A Research Communication," *Latin American Research Review*, 01 2009, *44*, 218–246.