PREJUDICE, STRATEGIC DISCRIMINATION, AND THE ELECTORAL CONNECTION: EVIDENCE FROM A PAIR OF FIELD EXPERIMENTS IN BRAZIL

Can electoral incentives mitigate racial and class prejudices toward underrepresented groups? We use a pair of large-scale field experiments to investigate the responsiveness of Brazilian legislative candidates to information requests from ficitious voters before and after the 2010 legislative elections. Our panel study design allows us to examine how politicians' electoral incentives and prejudices jointly affect their responsiveness to voters with randomly assigned socioeconomic and partisan characteristics. Distinguishing between prejudiced and strategic discrimination in responsiveness, we find that the most competitive candidates—the socioeconomically privileged with a viable chance of winning a seat—are more responsive to lower class voters in advance of the election, yet less responsive once in office. We take this as evidence of sophisticated behavior by aspiring politicians, who strategically mask their prejudice in pursuit of elected office.

> *Ah! What you are seeing is election season. Everyone speaks ill of the rich and well of the poor ... it's a golden moment. If this was the stock market, the poor voters' stock would be sky rocketing now. Yet as soon as the elections end, the poor bastards aren't invited for coffee, to lunch, to dinner. [And then] four more years without seeing a champion of the poor. Until of course, when another election comes, then it starts all over again.*[1]

Can politicians' electoral incentives mitigate prejudice against underrepresented groups? Despite unprecedented diversity across electorates around the world, office-holding remains an elite and exclusive affair. In established democracies and newly democratic nations alike, legislators tend to be members of the dominant racial, religious, and linguistic groups. Moreover, legislators are typically male, wealthy, and better educated than the vast majority of their constituents. This pattern not only has important implications for descriptive representation (Pitkin 1967); recent research also stresses its substantive consequences by demonstrating how legislators' ascriptive characteristics shape their responsiveness to voters (Kingdon 1981; Carroll 1994; Burden 2007; Butler and Brookman 2011; Grose 2011, 2014; Broockman 2013; Carnes 2013; Butler 2014). At the same time, elections can create incentives for aspiring legislators to be responsive to underrepresented and marginalized groups, especially when electoral institutions favor the political participation of these marginalized classes. In sum, the electoral connection may motivate office-seeking politicians to be responsive to voters they would otherwise ignore.

We provide insights into this question by reporting the results of a pair of field experiments in Brazil. Our two field experiments involved contacting more than 7,000 politicians competing at the state and federal level immediately before the October 2010 elections, and again about six months later. When contacting politicians, we used fictitious aliases and randomized their socioeconomic status, race, and gender (as well as vote intention and past voting behavior) to examine the conditions under which racial and class discrim-

[1] Brazilian President Luiz Inácio Lula da Silva, October 21, 2010, 10 days prior to the 2010 federal and state elections (da Silva 2010).

ination reveals itself during politicians' pursuit of elected office. We then repeated our experiment (with some modifications) six months after the elections, which allows us to compare politicians' responsiveness during election time to their behavior at a time when they are no longer seeking immediate election. As we will argue in greater detail, discrimination (either in favor of or against a particular group of voters) is plausibly strategic in nature before the elections, but not after. To the best of our knowledge, our paper is the first to conduct a pair of experiments on the *same* set of politicians observed in very different electoral contexts, to examine how politicians' electoral incentives and prejudices jointly affect their responsiveness to voters.

While we find evidence of class and racial prejudices amongst most candidates pursuing office, we also find evidence of strategic discrimination amongst the most privileged and competitive candidates in our sample. We partition our sample of candidates based on their electoral viability and find that socioeconomically privileged candidates with a viable chance at winning discriminated *in favor* of lower-class voters in advance of the elections, yet were indifferent to them after the fact. Moreover, socioeconomically privileged candidates who were eventually elected to office discriminated *against* both lower-class and Amerindian voters after the elections. This pattern suggests that their attentiveness to lower-class voters during the election campaign is caused by strategic considerations.

Next, we briefly review the literature on discrimination in political representation, in particular recent field experimental work. In the third section, we introduce the Brazilian case and discuss our experimental designs. In the fourth section of the paper, we present the empirical results from the October 2010 and March 2011 experiments. We close with a discussion of the implications of our findings for electoral competition, political representation, and Brazilian democracy writ large.

I. Politicians' prejudices and electoral incentives

The concept of descriptive representation highlights the importance of elected representatives who reflect their constituents in politically meaningful ways (Pitkin 1967). Contributing to the debate about descriptive and substantive representation, recent field experimental work examines if and why elected representatives and politicians running for public office are less responsive to voters who are not like themselves (Butler and Broockman 2011; Carnes and Holbein 2011; McClendon 2015; Broockman 2013; Butler 2014; Carnes and Lupu 2016). Butler and Brookman (2011) show that when confronted with requests for assistance with voter registration, U.S. state legislators are less responsive to out-group voters than to voters whose race is the same as their own. Broockman (2013) finds that African-American state legislators expend considerably more effort to improve the welfare of black voters than white state legislators, irrespective of whether the voters reside in their districts. McClendon (2015) presents a field experiment involving South African politicians, finds that politicians were more likely to respond to co-ethnic constituents than to out-group constituents. Butler (2014) suggests that voters' socioeconomic status may impact representation in ways similar to race. He shows that legislators judge the opinions of constituents with higher socioeconomic status as more informed of the issues, although there is no evidence that voters' socioeconomic status directly impacts legislators' responsiveness (see also Carnes and Holbein 2011). In his review of this literature, Grose (2014: 366) concludes that "political elites exhibit discrimination and bias when making decisions," implying that politicians' behavior in office is shaped by both intrinsic and electoral motivations.

While recent experimental work has generated novel and normatively compelling findings, it suffers from several limitations. First, with the exception of McClendon (2015), most of the evidence for discrimination in representation has been generated through the study of U.S. politicians. It is unclear the extent to which these findings are generalizable

to other contexts, different electoral systems or different institutionalized incentives for politicians' responsiveness to in- and out-group voters.

Second, while discrimination in representation and responsiveness raises serious normative concerns, empirically demonstrating the existence of intrinsic prejudice is no small task.[2] The difficulty is that behavior driven by prejudice against certain groups is often observationally equivalent to strategic discrimination against these same groups. In their neo-classical treatment of unequal pay in the labor market, Becker (1971) and Arrow (1973) characterize wage discrimination as a problem of workers' ascriptive characteristics affecting their market value, even though these traits are unrelated to worker productivity. They identify two possible explanations for this irregularity: either prejudices lead employers to favor in-group workers at the expense of out-group workers; or the ascriptive characteristics are used as a screening device for some unobservable trait.[3] This latter dynamic, which we call *strategic discrimination*, reflects the behavior of rational actors faced with incomplete information. In the context of elections, perfectly informed election-seeking politicians would identify with certainty the universe of sympathetic voters and focus their campaign energies accordingly. Yet when faced with uncertainty and imperfect information, they rely on noisy signals such as race to predict likely voter turnout and vote choice.

While existing work documents the existence of discrimination in political representation among U.S. politicians, it is harder to draw firm conclusions about whether discrimination is due to prejudice or electoral considerations (c.f. Broockman 2013). Our panel study allows us to differentiate between strategic and prejudiced discrimination by observing the responsiveness of the same set of politicians in contexts where electoral incentives

[2]We use *discrimination* to refer differential treatment of voters' based on their social or racial group membership, regardless of whether discrimination is due to electoral considerations or intrinsic prejudice. As such, discriminatory treatment may manifest in the explicit favoring of one group over others. In keeping with common practice, we define *prejudiced discrimination* to include all discriminatory behaviors not motivated by candidates' pursuit of elected office.

[3]In economics, these two types of discriminatory behavior are known as taste-based and statistical discrimination, respectively.

are either highly salient (shortly before the elections) or comparatively unimportant (about six months following the elections).

II. Case Selection and Experimental design

Brazil is an excellent environment in which to explore questions of discrimination in representation. Brazil is home to the fourth most unequal income distribution in South America and is among the most unequal countries in the world (World Bank 2014). In Brazil, the wealthiest 10% of the population make on average 40 times as much as the poorest 10%, a figure that is all the more striking in light of the size and rapid growth of the Brazilian economy until 2014.[4] Moreover, due to near 400-years of slave trade, Brazil is home to the largest diaspora of people of African decent, with an absolute majority of Brazilians self-identifying as black or mixed race.[5] Along with race, socio-economic status is the oft-cited explanation for persistent inequalities, faulting socio-economic barriers for non-white Brazilians' widespread exclusion from universities, high paying jobs, the health care system, and the public sphere (Machado de Castro 1993; Hasenberg 2005). Although compulsory voting and the introduction of electronic voting have greatly expanded the effective franchise (Hidalgo 2014), recent work by Cepaluni and Hidalgo (2015) show that these reforms have disproportionately improved turnout amongst middle and higher income Brazilians, relative to lower-income voters. While these reforms were adopted to expand the participation and influence of marginalized groups and low-income voters, a cursory glance at

[4]The disparity in income share held by the top and bottom 10% in Brazil (42.1) is nearly 50% larger than in Russia (28.9), India (25.1), or China (28.3) (World Bank 2014).

[5]Brazilians long prided themselves on the absence of institutionalized racism (Skidmore 1974), yet the widely held notion that Brazil was a "racial democracy" obscured objective and ubiquitous evidence of systemic racism (Hanchard 1994; Winddance Twine 1997; Telles 2004). Despite noted improvements (Ferreira, Leite and Litchfield 2006), race in Brazil is still strongly correlated with poverty, illiteracy, and a lack of life opportunities. One study of public attitudes found that 89% of respondents believe racism exists in Brazil, though only 10% admit to holding prejudiced attitudes. At the same time, 87% of respondents demonstrated evidence of indirect bias against afro-Brazilians (Turra & Venturi 1995). This suggests that classism and racism are widely seen as an enduring social problem, though harboring overt prejudice may be socially unacceptable.

the Brazilian representative institutions suggests that they have failed to yield significant advances in terms of descriptive representation.

Table 1 compares the distribution of racial and socioeconomic attributes among Brazilian legislators to the national average. Several representational distortions are readily apparent. The vast majority of deputies at both the national and state level consist of white, upper class, college-educated professionals. Although non-white Brazilians comprise nearly 50% of the population, less than 10% of the deputies in the national assembly are non-white. The same is true for women. The median income for national deputies is more than 1600 times greater than the median income of the average Brazilian family. Arguably the largest disparity exists with respect to education: nearly 90% of deputies elected to the national congress have a college education but less than 10% of Brazilians hold a college degree. In the second half of Table 1, we report the percentage of deputies and of the national public who spontaneously listed key issues as the country's "most important problem" in representative surveys of the public (LAPOP) and legislative elites (Alcántara 2008; Dulfloth et al. 2013). In line with the findings of Carnes and Lupu (2014), these figures suggest that the disparity in descriptive representation may have substantive implications for legislative priorities and policy-making.

[Table 1 about here]

In spite of these objective inequalities, several institutional features of the Brazilian system incentivize responsiveness to potential voters from across the political and socioeconomic spectrum. Voting is compulsory in Brazil for all literate citizens between the ages of 18 and 70, and voluntary for literate 16 and 17 year-olds, citizens older than 70, and illiterate voters. Average turnout since the transition to democracy (1980–2006) has been around 80%, with voluntary abstention by youth and citizens older than 70 accounting for most of the remaining 20% (Nicolau 2002; Power 2009). Moreover, Hidalgo (2014) reports

that the adoption of electronic voting in 1996 expanded the franchise by roughly one third, lowering the informational barriers to electoral participation for millions of lower-class and illiterate voters (c.f. Cepaluni and Hidalgo 2015). Whereas race or class might be an informative heuristic for U.S. politicians' due to non-compulsory voting and low voter turnout, Brazilian politicians can expect full participation across virtually all socio-demographic groups.

More importantly, the electoral and party institutions of the Brazilian system strongly incentivize candidate responsiveness (Ames 1995a, 1995b; Baker, Ames and Rennó 2006; Desposato 2013).[6] The Brazilian party system is known for its lack of a clear distinction between left and right parties, oversized and ideologically heterogeneous electoral and governing coalitions, frequent legislative party-switching, and very weak party identity in the electorate (Ames 2001; Baker, Ames and Rennó, Baker et al. Forthcoming). This extreme multipartism combines with open list proportional representation to create an electoral environment in which legislative candidates compete against many co-partisans as well as many candidates from other parties, and typically face several hundred competitors.[7] This implies that not only do candidates lack an ideologically coherent party label to attract like-minded voters, candidates may only win their legislative seat by beating out many other candidates from their same party. Research shows that the most successful candidates are those who build geographically targeted bases of electoral support, who ride the coattails of executive branch candidates (Ames 1995a, 1995b; Samuels 2000, 2003) or who stand out in an extremely crowded electoral arena, based largely on personal (as opposed

[6]Brazilian state and federal elections, which are held every four years, are highly competitive. The national Chamber of Deputies has 513 members, who compete in 27 multi-member electoral districts, corresponding to Brazil's 26 states and its capital district. The number of deputies for each district is determined by population, subject to the restriction that no district can have fewer than 8 or more than 70 representatives. State elections are conducted under the same electoral rules as federal elections.

[7]The district magnitude ranges from 8 to 70, and parties are permitted to nominate 1.5 times that many candidates. Electoral coalitions, which are common, are permitted to nominate up to 3 times as many candidates. The mean number of federal candidates per district competing in 2010 was 513, with a median of 280 and a maximum of nearly 1,300.

to party-based) appeals (Carey and Shugart 1995; Desposato 2013).[8] While candidates pursuing legislative office in the U.S. might use race or class to target or attract voters who are likely partisan supporters (or minimally, persuadable), Brazilian candidates can rarely make a similar claim: insofar that a voter of a particular race, class or ethnicity might affiliate with a particular party, this fact confers no natural advantage to one particular candidate over any of her copartisans who are competing for a seat.

Though the legacies of classism and racism make Brazil an interesting case in which to study descriptive representation and politicians' responsiveness, the institutional context in which Brazilian legislators compete for elected office ought to mitigate the tendencies towards prejudiced discrimination. The electoral environment is cacaphonous and competitive, and the most successful candidates are those that stand out from the crowd. Faced with fierce intra-party competition, every marginal vote counts to improve one's ranking within the party list. Though classism and racism may pervade social life, the electoral environment in which legislative candidates compete for office incentivizes responsiveness to voters, irrespective of race or socioeconomic class.

Based on the ongoing discussion of class, race, and candidates' incentives in Brazil, we have the following expectations. We expect politicians of higher socio-economic status to engage in prejudiced discrimination against lower-class voters. Lacking information on politicians' wealth or income, we measure politicians' socio-economic status by whether they have completed a college degree. College education is strongly correlated with income and social class: Brazilian youth from the top 20% income bracket are six times as likely to attend college as youth from the bottom quintile, with the socio-economic elite heavily overrepresented at the most prestigious universities (Azevedo and Salgado 2012). In our

[8]Most candidates and small parties band together to form electoral coalitions, to which legislative seats are allocated in proportion to their vote share. Often, these coalitions are lead and brokered by candidates pursuing subnational executive offices, such as gubernatorial or mayoral posts (Samuels 2000).

sample, 54% of aspiring legislative deputies have college degrees.[9] We anticipate that those candidates with a college degree will be more likely to discriminate against lower class voters.

Irrespective of candidate socioeconomic status, we also expect candidates to engage in prejudiced discrimination against Amerindian voters. Though we would expect candidates to be more responsive to voters who are racially or ethnically like themselves (Broockman 2013), we lack information about politicians' race. However, less than 1% of the Brazilian population is Amerindian, and the number of Amerindian politicians in our sample is also negligible.[10] In spite of their distinct cultural heritage and the geographic concentration of potential voters, Amerindian ethnicity is rarely evoked as a means of electoral mobilization, which elites might exploit for their own political gain. For this reason, we anticipate all candidates will be *less* responsive to Amerindian voters.

At the same time, we acknowledge that institutionalized electoral incentives may mitigate the tendencies towards prejudiced discrimination. We explore the effect of electoral incentives in two ways: first by differentiating between candidates on the basis of their viability, and by comparing politicians' responsiveness both during and after the elections. First, we expect that more competitive politicians to refrain from prejudiced discrimination against either lower-class or Amerindian voters when they have electoral incentives to do so. Less competitive candidates might engage in prejudiced discrimination either because they do not care about or are unaware of its electoral costs. We distinguish among candidates based on their electoral *viability*—the likelihood that a given candidate has a reasonable chance at winning a legislative seat. In practice, the vast majority of legislative candidates

[9]This measure is imperfect, as access to college education has improved over time for underprivileged Brazilians. However, incorrectly classifying some politicians as privileged would tend to bias our estimates downwards. Moreover, most politicians in our sample are not young enough to have benefitted from the relatively recent expansion of educational opportunities for lower-class Brazilians.

[10]Though Amerindians comprise a small proportion of the population, Brazil is home to more than 300 indigenous ethnicities, who are culturally and linguistically distinct, and enjoy protected governmental status (IBGE 2011).

competing in a given year have very little chance of being elected to office, owing to the permissive candidate entry rules, very high district magnitude and weakened nomination and ballot control exerted by most Brazilian parties.[11] Under the open list proportional representation with D'hondt allocation formula, legislative seats are allocated to pre-electoral coalitions in proportion to the aggregated party vote to all coalitions meeting the district quota, and seats are then allocated based on candidates' individual vote totals for the candidates in our study. In practice, only the most popular candidates from sufficiently large parties/coalitions are elected to office, excluding very popular candidates from very small parties, as well as most candidates from larger parties who fail to garner sufficient individual votes.[12] We anticipate that those with *Viable* candidacies will be sensitive to the electoral costs of prejudiced discrimination, such that we may observe prejudiced discrimination after the elections, though not before. For non-*Viable* candidates by contrast, we expect prejudiced discrimination to be evident both before and after the elections.

Given these considerations, we expect prejudiced discrimination against lower-class voters to be more common among higher status politicians than among politicians who are socioeconomically disadvantaged. We expect prejudiced discrimination against Amerindian voters irrespective of candidate's social status, though only amongst those candidates who are not electorally *Viable*. For these politicians, we expect prejudiced discrimination to be prevalent after the elections, though largely absent before them.

A. Experimental designs

Approximately two weeks before the elections, we sent more than 7,000 personalized emails to candidates competing in the October 3, 2010 federal or state elections. Besides requesting

[11] Supra. 7

[12] For example, in our data, only 10% of the candidates were elected to office. Nearly 20% of the sample of candidates were classified as "not elected," indicating their party failed to meet the minimal quota in their district. Seventy percent were designated as alternates, or 'suplentes,' who could be called on to fill the seat in the case of a legislator's death in office, resignation, or impeachment.

information about candidates' platforms, our emails contained randomly assigned signals about the socioeconomic and partisan characteristics of the fictitious senders, allowing us to test whether candidates' responsiveness is affected by these signals. We then observed whether each candidate sent at least one reply email or not. In order to better distinguish between prejudiced and strategic discrimination, we repeated our experiment (with some modifications, discussed below) *after* the elections, in March 2011, when electoral incentives to reply to our emails were largely absent. We now describe our experimental designs in more detail.

We obtained candidate information through Brazil's Superior Electoral Court (*Tribunal Superior Eleitoral*, or TSE), which supervises all elections and collects candidate information. Of the more than 22,000 candidates competing in the 2010 state and federal elections, slightly more than half provided valid, unique email addresses.[13] In addition to candidates' email addresses, first and last names, and ballot names, we know the state and type of election in which each candidate competed, as well as candidates' gender, age, marriage status, level of education, party label, and incumbency status. Following the election, we collected information on each candidate's vote totals. Retaining all politicians pursuing state and federal lower house positions with complete covariate profiles left us with a sample of 7,687 politicians. Unsurprisingly, the lion's share of our sample consists of candidates for the state assemblies (5,214 candidates), with the remaining 32% accounting for aspirants to the national Chamber of Deputies (2,473 candidates).

[Table 2 about here]

The first design factor varies the social class and race of the fictitious voter. We manipulated social class by exploiting the fact that in Brazil certain first names, especially names borrowed from American pop culture such as Britney or Chardley (Charles), are

[13]Below, we report on the differences between candidates who did and did not report an email, as well as potential sources of bias this might introduce. Full analysis is shown in Tables A5 and A6 in the appendix.

generally considered lower-class names (Barbosa 1984; see also Mehrabian and Piercy 1993 and Laham et al. 2012). Moreover, since descendants of slaves are still among the poorest segments of Brazilian society, lower-class first names were paired with last names that were historically given to slaves.[14] We created fictitious minority voters by using names that are easily recognizable as Amerindian (native Brazilian). Finally, we used names that are common in Brazil without being associated with any specific class or race to simulate constituents whose names offer no clear information about class or race.[15] The second design factor varies the gender of the voter, as revealed by his or her first name. We used a total of 24 names, 12 male and 12 female (see TableA2 in the online appendix).[16] We used block randomization to improve both efficiency and balance of observed and unobserved covariates across treatment conditions, blocking on party label, incumbency status, the type of election, and state GDP per capita.[17]

Our October 2010 experiment is a full factorial experiment with $3 \times 2 \times 3 \times 4 = 72$ distinct treatment combinations, which are discussed below. Each candidate in our sample was sent one email, with no follow-up in case of non-response. All emails contained a

[14]President Lula da Silva described this widely recognized social cue the following way: "This society is divided between those with opportunity and those without—and we all know who is who by a person's last name..." (President Lula da Silva, October 21, 2010 (da Silva, 2010).

[15]We tested the names used in our study with a sample of 145 Brazilian university students, who had no trouble picking up the implied class and race signals. Our names that were selected as the "no treatment" signals were almost never classified as Amerindian, and most often spontaneously classified as "white". Though there is more variance with respect to social class, our "no treatment" names were also more likely to be perceived as upper or middle class names, and only rarely taken as an indicator of lower class status. See Tables A2, A3 and A4 for additional information.

[16]We included two additional design factors that convey information about a voter's intended vote choice and past voting behavior. Though we do not report the full model here in the interest of space, results from the full experimental design are shown in Table A7 of the appendix. These design factors allow us to rule out all possibilities that the candidates' responses were made in reaction to partisanship or other political calculus.

[17]We evaluated our randomization procedure by fitting multinomial logit models, one for each design factor, as well as bivariate differences of means tests across many observables in our study. In all cases, we find virtually no evidence of differences of means between our control and treated groups. See the appendix for more details (Tables A14-A17).

question about the candidate's platform with respect to unemployment.[18] We chose this question because we assumed that candidates would find it relatively easy to respond given the high unemployment rates that Brazil had experienced throughout the 2000s. The emails also contained randomly assigned signals about the voter's sociodemographic characteristics, vote intentions, and previous voting behavior (see Table 2).[19]

Several methodological issues regarding the external and internal validity of our study warrant mention. First, if candidates who reported their email to the TSE are unrepresentative of the universe of Brazilian legislative candidates, our experimental sample may be of limited external validity. We acknowledge the dimensions along which this may be true: candidates reporting their email were more often male, college-educated, ideologically liberal, and more commonly from the south or southeastern regions (which are more urban, with higher GDP per capita). Yet, as we show in greater detail in the appendix, these same covariates were associated with lower than average responsiveness to our email. Though this implies we cannot claim to generalize our experimental results to the universe of Brazilian candidates without caveat, we can be confident that our email-based approach did not inadvertently target only the most responsive candidates or politicians. Second, the email addresses of candidates are readily found on parties' websites and candidates' personal websites. Official websites maintained by Brazilian states and the TSE also com-

[18]The full text of the emails was the following: *Dear* [politician's title and name], *How do you do? I live in* [politician's state]. [vote intention & turnout propensity treatment] [past voting behavior treatment] *I have a question about your platform. Do you have a specific plan to reduce unemployment in our state? I thank you for your kind attention.* [voter name]. We report English translations of the Portuguese text of our emails and experimental treatments in Figure A1 and Table A1 of the appendix.

[19]We acknowledge that our experiment measures candidates' responsiveness only indirectly, as it is possible that some replies were drafted by others with access to candidates' email accounts, such as staff or family members. Although we cannot be absolutely certain about the authorship of the reply emails, we received almost no emails signed by staff or family members. Moreover, even if not all reply emails were written by the candidates themselves, we think it reasonable to assume that the individuals responding to our emails did so on behalf of the candidates and in accordance with the candidates' wishes and preferences. Previous research has made the same assumption (e.g., Butler and Brookman 2011).

monly provide lists of candidates and their email addresses.[20] As such, it would not be difficult for the average internet user to obtain the information we employed in this study.

As to internal validity, the very fact that our fictitious voters contacted candidates via email revealed some information about their socioeconomic status. Although almost 40% of the Brazilians had access to Internet during the time of our experiment, candidates might have inferred that the voter did not belong to the poorest sections of Brazilian society and had a basic level of education. Even so, internet usage is common amongst Brazilians across the socioeconomic spectrum.[21] Second, we decided against incorporating slang or grammatical mistakes, due to the wide regional variation in grammar structure and colloquialisms. Though our emails may seem unrealistically correct to have been drafted by a voter with very little formal education, the text of our emails are simple, informal and short, and could easily have been drafted by a voter with basic education. Finally, that fact that our putative voter contacted the candidate with a public policy question revealed some interest in public affairs. These limitations would make it harder for our study to detect evidence of class discrimination, which makes our findings all the more striking.

In order to better distinguish between prejudiced and strategic discrimination, we repeated our experiment approximately six months after the 2010 elections and two months after elected candidates had taken office (in March 2011), when incentives to be responsive to voters were very much muted in comparison to the election campaign.[22] This approach is a distinct advantage of our study, as other recent field experimental research on representation considers either responsiveness before (e.g., Butler and Broockman 2011) or after

[20]Among the candidates who sent reply emails, only a small number expressed curiosity as to how we had obtained their email addresses. Almost all of these cases involved candidates who had dropped out of the race long before the election but were still listed as candidates by the TSE.

[21]In 2009, about 37% of the Brazilian population was using the internet, according to the CIA's World Factbook.

[22]Electoral incentives to respond to our emails were undoubtedly much higher in the days before the hotly contested 2010 election than six months after the election, even for those candidates who were eventually elected to office. The significantly lower response rate in the March 2011 experiment described below is also in line with the assumption that electoral incentives to be responsive to voters were much lower after the election than during the election campaign.

(e.g., Broockman 2013) an election, but not both.

To evaluate the presence of strategic discrimination based on relative candidate competitiveness, we partition our sample based on candidate viability. Here, we classified candidates as "viable" based on their relative rank within the electoral coalition in which they competed. Following the elections, we collected candidates' individual vote totals from the 2010 elections, allowing us to identify those candidates who were elected to office based on their individual level vote totals, as well as the relative ranking of all unelected alternate candidates within each district-specific electoral coalition.[23] We classify a candidate as viable if s/he was elected, or was one of the top ten ranked vote-getters within their electoral coalition.[24] With this classification, approximately one-third of the candidates (2,816) were classified as *Viable*.

In this second experiment, we sent another round of emails to the 7,687 elected and unelected politicians in our sample.[25] These emails informed politicians that the sender had voted for them back in October and once again asked them about their views on unemployment.[26] We randomly assigned social class, race, and gender using the same set of fictitious voters.[27] Note that in the second experiment, the only randomly assigned

[23]This approach accounts not only for the electoral success of candidates relative to their copartisans, but also accounts for the very real possibility that a former candidate could be called to serve in the legislature at some future point in the legislative term. Candidates whose party clears the electoral threshold but who are not directly elected to office are designated "suplentes," or alternates, who may be called to serve in the legislature in the case of premature vacancy by an elected deputy. Samuels reports that up to 40% of deputies elected to the Chamber take a leave of absence during their term (50, 2001). This implies that our candidates at the top of the 'suplente' list also have a viable chance of being called to serve as an alternate.

[24]We report in the appendix the results from our analysis using more stringent definitions of *Viable*. The results do not change.

[25]Among the candidates in our sample, 557 had won seats in state legislatures and 297 had won seats at the federal level.

[26]*Dear* [politician's title and name], *How are you? I voted for you in the last election because I like your platform and political views. I would like to know more about your ideas on unemployment. What can our government do to fight it? I have heard different opinions and am quite confused. Sincerely* [voter name]. The full Portuguese text of the experimental treatment, as well as its English translation, is provided in the Appendix.

[27]The randomization procedure was identical to the one described above except that we made sure that no candidate was contacted twice by the same voter. We report in the appendix (Tables A9-A12) the differences between the treated and control groups across a wide variety of observables. The differences of means tests are also uniformly consistent with randomized assignment of treatment conditions.

treatment was the fictitious voter's name, which we used to signal race, class, and gender.

III. Results

We start by presenting the results of the October 2010 experiment, before turning to the results of the March 2011 experiment.

A. October 2010 experiment

Our outcome of interest is whether a candidate had sent at least one reply email by election day.[28] Of the 7,687 candidates in our sample, 1,510 (or 19.6%) sent at least one reply.[29]

Based on the expectations formulated earlier, we allow treatment effects to vary between viable and non-viable candidates and candidates with and without college degrees, our measure of candidates' social class. Table 3 reports simulated treatment effect estimates from a probit model containing three-way interactions between the treatment indicators, an indicator for candidate viability, and an indicator for college degree.[30] The outcome variable takes the value 1 if a candidate sent at least one reply email. The model controls for state dummies, party dummies, intra-party competition, and candidates' age, marital status, gender, and incumbency (before the 2010 elections).[31] Treatment effects are first differences in the probability of receiving a reply email as we switch from one factor level to another level (with the remaining design factors and covariate values held at their observed values). For the gender factor, the difference is moving from male to female voter. Robust

[28] Reply emails without any personalized content—out-of-office messages, candidate newsletters, or flyers—were coded as non-response, as were emails received after the elections.

[29] Among non-viable candidates, the response rate was 22.8%; among viable candidates, 14.2%.

[30] All simulations use the observed values approach (Hanmer and Kalkan (2013)).

[31] Our measure of intra-party competition is the ratio of the number of candidates per party in each district to the previous number of seats held by that particular party in the previous legislative session, with the denominator set to 1 if no co-partisans won seats previously (see Crisp, Jensen and Shomer 2007). Intra-party competition is a much more important predictor of candidate responsiveness in the October 2010 experiment than in the March 2011 experiment, which supports our argument about electoral incentives to respond being greater in the pre-election experiment than the post-election experiment. Additional information is available upon request.

standard errors are shown in parentheses. The third column of Table 3 shows second differences for the impact estimates in columns 1 and 2. The sixth column of Table 3 shows second differences for the impact estimates in columns 4 and 5. Column 7 shows third differences for columns 3 and 6.

[Table 3 about here]

Several results are noteworthy. First, candidates from privileged socioeconomic background evinced strong responses to our treatment for both lower-class and Amerindian voters, but the direction of the effect is conditioned by *Viability*. Compared to the "no signal" condition, responsiveness among non-viable candidates coming from a privileged social class is about 5.6 and 7.6 percentage points *lower* for lower-class and Amerindian voters, respectively (column 2 of Table 3). To put the magnitude of these estimates into context, we can compare them to the baseline response rate of non-viable candidates with college degrees (26.4%). The treatment effect for lower-class names, -5.6 percentage points, then translates into a $-5.6 \div 26.4 \approx -21$ percent change in responsiveness. For Amerindian names, the change corresponds to a -28.7 percentage decline in responsiveness.

Yet the patterns of treatment effect heterogeneity is starkly different amongst viable candidates. Among viable candidates—those who would go on to either win a legislative seat or be in a reasonable position to do so —candidates from relatively privileged social position are much *more* responsive to lower-class voters (compared to the "no signal" condition), with an estimated difference in effect sizes of about 4.0 percentage points (column 5). This represents a roughly 26% increase in responsiveness, taking into account the average response rate for viable, college-educated candidates (15.8%). Viable candidates from higher social class appear more responsive to voters from Amerindian decent, though this treatment effect does not approximate conventional levels of statistical significance. Column 7 of Table 3 contrasts the estimated second differences (columns 3 and 6). Both of the

estimated third differences are substantively large and statistically significant, indicating that social class of the candidate moderates the impact of the class and race treatments in opposite ways for viable and non-viable candidates. In sum, electoral viability, as classified by relative success within their electoral coalition, is correlated with candidates' responsiveness to voters from backgrounds different from their own.

By contrast, non-viable candidates from lower socioeconomic background were generally unaffected by our treatments, responding equally to all voter types, irrespective of race or class. Though we hypothesized that all candidates would discriminate against Amerindian voters, irrespective of candidate social class, we find no evidence of discrimination by candidates of lower socioeconomic status. All discriminatory effects with respect to the Amerindian treatment stemmed from candidates from higher socioeconomic status, a finding that ran counter to our hypotheses but is consistent with the classist dynamics widely thought to exist in Brazil. Finally, we have no evidence that candidates of any type were more or less responsive to voters of different genders.[32]

In the next section, we first present the results of the March 2011 post-election experiment and then discuss the results of the two experiments in light of our hypotheses about strategic and prejudiced discrimination.

B. March 2011 experiment

Of the 7,687 politicians in our sample, 1,024 (or 13.3%) sent at least one reply email in the March 2011 experiment. This reply rate is substantially lower than the rate for the October 2010 experiment ($p < 0.001$), strongly suggesting that electoral incentives had

[32]In the interest of space we do not report the results of the third and fourth factors of our full experimental design, which randomized the putative voters' vote intent and past voting history. We find there to be no consistent effects of either experimental treatment. The full results are reported in Table A7 of the appendix. In Table A21, we reestimate the model shown here holding the other two factors as the "no signal" category, to rule out any possible contamination effects across treatments. The results are unchanged.

been an important motivation for candidates to reply to our pre-election emails.[33]

In Table 4, we report the results of our analysis for the second experiment based on the responses from the full pool of former candidates. As would be expected, candidates who were not considered *Viable* (defined as being one of the top-10 nominal vote earners in their electoral coalition or party) were generally unresponsive to our requests for information in the post-electoral period. This should come as no surprise, as they now lack both the electoral or representational incentives to respond to our putative voters. For what it is worth, amongst those former candidates who *did* reply to our email, we find evidence of discrimination against lower-class and Amerindian voters by all candidates, irrespective of viability or social class. Amongst those with viable candidacies, the negative coefficients might suggest these general patterns of discrimination hold, but we lack statistical precision to make a more general claim. This lack of precision is likely due to the fact that among the 2,816 we classifed as *Viable*, only a third ($N = 874$) were eventually elected to office. As such, nearly 70% lacked incentives to respond at all.

[Table 4 about here]

The non-election of a vast majority of our candidates substantially changes their incentives to respond and may confound our results. Conversely, it may also be the case that those elected to office would be incentivized to respond, in light of their duties as a representative or to curry favor for future electoral contests. In order to account for this possible confounding effect, Table 5 reports the results from the second experiment, partitioning the candidates on their eventual election. As before, we allow treatment effects

[33]One candidate not only replied to our email but also forwarded her reply to all other members of her party (PPS, *Partido Popular Socialista*). Another party member then replied, warning his copartisans that he had received an identical email and that these emails must be a scam. Fortunately, only 4% of the politicians in our sample belong to the PPS. We decided to leave PPS members in our sample, but dropping them from the analysis has no effect on our results. We carefully wrote the email messages using a grammatically correct but colloquial and neutral language. Among the candidates who sent reply emails, none of them showed concern that the emails were false. Therefore, we believe that the spillover problem with the PPS was an isolated and minor event.

to vary between candidates with and without college degrees (as a measure of candidates' social class), but now report the treatment effects for only those candidates we identified as *Viable* (partitioned on election). In so doing, we can isolate the effect of our treatments before and after the elections on the sub-population of candidates of most interest: those candidates who had a real chance of winning a seat in the election and those who were eventually elected to office. We control for the same set of covariates as before.

Columns 1 and 2 of Table 5 shows that in March 2011, when electoral considerations are absent for those viable but un-elected candidates, socially privileged aspiring politicians still discriminate against Amerindian voters. The post-election treatment effect estimates are of smaller size as the pre-election treatment effect estimates, and this effect is only statistically different from no-signal in the case of the Amerindian treatment. However, given that these former candidates lack incentives to respond at all, it is perhaps unsurprising that they were, on the whole, unresponsive to our fictitious voters, irrespective of class or race.

[Table 5 about here]

However, a more interesting pattern emerges amongst those candidates elected to office. For socioeconomically privileged representatives, we find evidence of discrimination on the basis of both class and race. College educated candidates who were elected to office were about 7.5 percent less responsive to voters from lower-class background as compared to the "no treatment" category, and 6.1 percentage points less responsive to voters of Amerindian descent. Taking into account the baseline response rate amongst socioeconomically privileged representatives (15.5%), this translates to a 48.4% and 39.4% percentage decrease in responsiveness, respectively. Recall that in our first experiment, these two voting constituencies were the object of *increased* responsiveness when these candidates were pursuing office, a trend which ostensibly reverses absent immediate electoral incen-

tives.[34] We acknowledge that these results are best interpreted with caution, as we lack sufficient information to statistically discriminate between groups. Nevertheless, the general unresponsiveness by all former candidates and elected officials to putatively lower-class and Amerindian voters suggests that classism and racism may inform the behavior of the Brazilian political elite.

IV. Discussion

Few national legislatures in the world constitute a descriptively representative reflection of the national population. On the whole, legislative representatives tend to be wealthier, more educated, and whiter than the average voter or constituent. Political scientists have viewed this discrepancy to be of broad political import: heterogeneity of legislative delegation correlates with increased attentiveness to minority populations (Schwindt-Bayer & Mishler 2005), and minority legislators have shown to have systematically different preferences and priorities than their majority group counterparts (Carroll 1994). Recent experimental research identifies one causal mechanism to explain these aggregate trends, highlighting how representatives life experience' and unconscious prejudice inform their responsiveness (Butler & Broockman 2011), outreach efforts (Broockman 2013), and political priorities while in office (Carnes & Lupu 2014, 2016).

We sought to understand the bias in legislators' responsiveness in a system which is highly unequal and racially heterogenous, but whose electoral institutions ought to mitigate politicians' prejudices in responsiveness. In so doing, we are the first to experimentally document discrimination in representation among Brazilian politicians, and among the first to study discrimination in voter representation beyond the U.S case. More importantly, our panel study design allows us to test the hypothesis that electoral incentives temper

[34]The same is true if we compare these results to candidates in the pre-election experiment who received "no signal" with respect to vote intention or previous voting behavior. See Table A21 for more details.

prejudiced discrimination. Discrimination may reflect prejudice, or a deeply held antipathy towards certain groups, but it may also simply be the result of strategic electoral behavior in the face of incomplete information. Strategic discrimination is consistent with politicians' office-seeking ambitions and is in most cases observationally indistinguishable from politicians' prejudice. Our pair of field experiments, conducted on the same set of politicians observed in very different electoral contexts, allows us to differentiate between these very different motivations for discriminatory behavior. To the best of our knowledge, our paper is the first to experimentally examine the intricate interplay between politicians' electoral incentives and prejudices.

We find strong evidence of prejudiced discrimination among the majority of politicians pursuing legislative office, who discriminate against lower-class and Amerindian voters both before and after the elections. This effect is pronounced amongst the most privileged political elites: we found little evidence of discrimination by lower class candidates, irrespective of electoral viability. Instead, we found evidence of prejudiced discrimination against lower-class and Amerindian voters by candidates of relatively high social class.

Even more importantly for Brazilian democracy and the representation of voters from marginalized groups, we find that the most competitive candidates—those who are socioeconomically privileged with a viable chance of winning—mask their prejudice when seeking office, but discriminate against these constituents once elected to office. These results are sobering. Latin America holds the distinction of being one of the most unequal regions of the world, where inequality and systemic poverty are both ubiquitous and persistent. Brazil is the largest Latin American country in terms of population, land mass and size of the economy, and is no exception to these general trends. Although democracy is now the norm and recent reforms have expanded the electoral potency of voters from the lowest socioeconomic strata (Hildalgo 2014), our results suggest that this has not necessarily translated into increased legislative responsiveness to constituents of lower-class backgrounds. Similar

to the work on representative bias in the United States (Butler and Broockman 2011; Butler 2014), this suggests that increased electoral participation of underprivileged voters has not solved the problem of underprivileged voters' lack of substantive representation in the political system. Instead, it would seem that Brazil's representative institutions, remain a largely elite, and possibly elitist, affair.

At the same time, we are encouraged that the electoral connection democracy implies—that which induces attentiveness to voters out of electoral necessity—appear to have muted politicians' prejudices in the preelectoral period. This would seem to suggest that the electoral connection is operating as we hope that it would. Our finding that the most privileged and competitive candidates were responsive to lower class voters in advance of the election is consistent with strategic behavior, wherein their short term electoral ambitions trumped their prejudice against lower class and Amerindian voters. Though reforms that extend the effective franchise are an important first step, institutional changes that may reorient legislators' behavior towards a longer time horizon may be a critical way forward. Extending the shadow of the electoral connection to induce elite responsiveness may have broad implications for the underrepresentation of the lower class, and be a transformative force in Brazilian democracy for many years to come.

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Table 1: Distribution of class, race, gender, and various attitudes among Brazilian federal and state legislators compared to national averages

	Federal deputies	State deputies	National average
Sociodemographic characteristics			
Male	91.8%	88.4%	48.9%
White‡	90.2%		47.3%
Median income	2.4 million R$	∼12,500 R$	1,472 R$
Managerial/professional occupations	74.5%	81.1%	7.3%
Working class occupations (white and blue collar)	13.6%	8.9%	26.8%
Elementary/poorly defined occupations	11.9%	9.1%	14.8%
College education	86.6%	70.1%	9.0%
Average age (years)	53	48	41
Most important problems†			
Lack of health care	0.8%	26.5%	18.0%
Violence	10.5%	7.2%	17.0%
Unemployment	10.5%	5.9%	14.0%
Economic crises	26.0%	7.4%	2.1%
Political crises	25.0%		2.2%
Corruption	13.0%		11.0%

Racial demographics are from Santos (2010); gender composition is from Araújo (2013). The occupational composition of state legislatures is available in Araújo (2013) and Dufloth et al. (2013). The latter also reports the approximate income of state deputies. National demographic information is provided by the IBGE. Opinions of federal deputies are taken from the University of Salamanca's Parliamentary ELITES Survey Data (Alcántara 2008). National public opinion data are from the LAPOP data, administered as part of the Americas Barometer survey series at Vanderbilt University.

†We acknowledge a slight difference in wording across the surveys: the ELITES survey posed the question in terms of problems faced by "the government" as opposed to the most important problem facing the country. As for the state deputies, the question was framed in terms of highest budgetary priority, though neither political crises nor corruption were included (Dufloth et al. 2013).

‡Though candidates for state or federal office must self-report their level of education and gender, 2014 was the first year in which they were required to self-report their race. For this reason, the racial composition of the state legislatures is not well known.

Table 2: Factors and factor levels included in the design of the October 2010 (pre-election) experiment

Design factor/level	Text for associated treatment
Social class & race	
Lower class	
Amerindian	
No signal	
Gender	
Male	
Female	
Vote intention & turnout propensity	
Mobilized supporter	"I support your candidacy and you will certainly receive my vote in this election"
Unmobilized supporter	"I support your candidacy but might register as a non-voter"
Undecided voter	"I'm not sure whom to vote for"
No signal	—
Past voting behavior	
Past supporter	"I have voted for [name of candidate's party] in the past"
Never supporter	"I have never voted for [name of candidate's party]"
No signal	—

Table 3: Treatment effect estimates in October 2010 (pre-election) experiment

	Non-Viable			Viable			
College degree	no	yes	DD	no	yes	DD	DDD
Treatments	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Lower class	−0.012	−0.056***	0.044	−0.007	0.040*	−0.047	0.091*
	(0.018)	(0.022)	(0.028)	(0.030)	(0.022)	(0.038)	(0.046)
Amerindian	0.011	−0.076***	0.087***	−0.047	0.016	−0.063*	0.150***
	(0.018)	(0.021)	(0.028)	(0.029)	(0.022)	(0.036)	(0.046)
Female	0.018	−0.011	0.029	0.024	0.020	0.005	0.024
	(0.015)	(0.017)	(0.023)	(0.024)	(0.018)	(0.030)	(0.038)
N	2537	2334		932	1884		

N = 7,687. The table displays simulated treatment effect estimates (first differences), differences in treatment effect estimates (second differences, DD), and differences in differences in treatment effect estimates (third differences, DDD) from a probit model containing three-way interactions between the treatment indicators, an indicator for candidate viability, and an indicator for college degree. The model additionally contains state dummies, party dummies, intra-party competition, and candidates' age, marital status, gender, and incumbency (before the 2010 elections), and the remaining experimental treatments. Robust standard errors are shown in parentheses. The third column shows second differences for the impact estimates in columns 1 and 2. The sixth column shows second differences for the impact estimates in columns 4 and 5. Column 7 shows third differences for columns 3 and 6.

* denotes statistical significance at 0.10 level. ** denotes statistical significance at 0.05 level. *** denotes statistical significance at 0.01 level.

Table 4: Treatment effect estimates in March 2011 (post-election) experiment, full candidate sample

	Non-viable			Viable			
College degree	no	yes	DD	no	yes	DD	DDD
Treatments	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Lower class	−0.036**	−0.019	−0.018	0.000	−0.029	0.030	−0.048
	(0.015)	(0.019)	(0.024)	(0.027)	(0.021)	(0.035)	(0.042)
Amerindian	−0.031**	−0.050***	0.020	−0.027	−0.055***	0.028	−0.009
	(0.015)	(0.018)	(0.024)	(0.025)	(0.020)	(0.033)	(0.040)
Female	0.019	0.024*	−0.005	−0.026	0.030*	−0.056**	0.051
	(0.012)	(0.015)	(0.019)	(0.022)	(0.017)	(0.027)	(0.033)
N	2537	2334	–	932	1884	–	–

N = 7,687. The table displays simulated treatment effect estimates (first differences), differences in treatment effect estimates (second differences, DD), and differences in differences in treatment effect estimates (third differences, DDD) from a probit model containing three-way interactions between the treatment indicators, an indicator for candidate viability, and an indicator for college degree. The model additionally contains state dummies, party dummies, intra-party competition, and candidates' age, marital status, gender, and incumbency (before the 2010 elections). Robust standard errors are shown in parentheses.

* denotes statistical significance at 0.10 level. ** denotes statistical significance at 0.05 level. *** denotes statistical significance at 0.01 level.

Table 5: Treatment effect estimates in March 2011 (post-election) experiment, only previously viable candidates, partitioned on election

	Viable, Not-Elected			Viable, Elected			
College degree	no	yes	DD	no	yes	DD	DDD
Treatments	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Lower class	−0.003	−0.005	0.002	0.019	−0.075**	0.094	−0.092
	(0.030)	(0.026)	(0.039)	(0.056)	(0.034)	(0.066)	(0.076)
Amerindian	−0.037	−0.048**	0.011	0.024	−0.061*	0.085	−0.074
	(0.026)	(0.024)	(0.035)	(0.056)	(0.034)	(0.066)	(0.075)
Female	−0.013	0.014	−0.026	−0.062	0.057**	−0.119**	0.093
	(0.023)	(0.020)	(0.030)	(0.045)	(0.027)	(0.053)	(0.061)
N	711	1251		221	633		

N = 2,816. The table displays simulated treatment effect estimates (first differences), differences in treatment effect estimates (second differences, DD), and differences in differences in treatment effect estimates (third differences, DDD) from a probit model containing three-way interactions between the treatment indicators, an indicator for candidate viability, and an indicator for college degree. The model additionally contains state dummies, party dummies, intra-party competition, and candidates' age, marital status, gender, and incumbency (before the 2010 elections). Robust standard errors are shown in parentheses. The third column shows second differences for the impact estimates in columns 1 and 2. The sixth column shows second differences for the impact estimates in columns 4 and 5. Column 7 shows third differences for columns 3 and 6.

* denotes statistical significance at 0.10 level. ** denotes statistical significance at 0.05 level. *** denotes statistical significance at 0.01 level.