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

Supplementary Information

Contents

I. Supplementary Materials of the Experimental Design

Our first experiment was a full factorial experiment with $3 \times 2 \times 3 \times 4 = 72$ distinct treatment combinations. All emails contained a question about the candidate's platform with respect to unemployment. Table 2 in the main body of the paper details the experimental treatment scheme. Our first factor varied the socio-economic and/or race of the sender, our second design factor varied the gender. These treatments affected the fictitious voter's name, email address as well as gendered pronouns and greetings. The third and fourth factors, meant to capture our fictitious voters' past voting record and future intentions, altered the subject line and body of the email. Example text of an email (corresponding to the mobilized, past supporter treatment condition), and its English translation are shown in Figure A1. As mentioned in the manuscript, while the language in our emails is free from regional colloquialisms or grammatical mistakes, the tenor is informal and simple. Table A1 replicates Table 2 in the manuscript, providing the full Portuguese text of the emails for each of experimental treatments.

Figure A1: Example October Email, Portuguese Text and English Translation

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Subject: Uma pergunta sobre desemprego de um de seus eleitores


Sra. Marli Wensiboski,

Como vai?

Sou do Rio de Janeiro. Já votei anteriormente no PT, apoio a sua candidatura
e você certamente terá meu voto nesta eleição. Tenho uma pergunta sobre
seu programa. A Sra. tem algum plano específico para reduzir o desemprego
em nosso Estado?

Muito Obrigado,
Carlos Chimenes
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Subject: A question about unemployment from one of your supporters

Ms. Marli Wensiboski,

How do you do?

I live in Rio de Janeiro. I have voted for the Worker's Party in the  past. I
support your candidacy and you will certainly receive my vote in this election.
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.
Carlos Chimenes
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Table A1: Factors and factor levels included in the design of the October 2010 (pre-election) experiment

Design factor/level	Portuguese Text for associated treatment
Social class & race	
Lower class	
Amerindian	
No signal	
Gender	
Male	
Female	
Vote intention & turnout propensity	
Mobilized supporter	"Apoio a sua candidatura e você certamente terá meu voto nesta eleição"
Unmobilized supporter	"Apoio a sua candidatura mas talvez justifique meu voto nestas eleições"
Undecided voter	"Ainda não decidi em quem votar desta vez"
No signal	—
Past voting behavior	
Past supporter	"Já votei anteriormente no [name of candidate's party]"
Never supporter	"Nunca votei no [name of candidate's party]"
No signal	—

II. Instrument validity

A. Race/class & gender treatment conditions

Our experiment randomized 72 treatment conditions sent from 24 fictitious voters. The first two factors, corresponding to our racial/class treatment and the gender treatment, altered the ficitious sender's name, email address and respective gendered pronouns. The names, emails and respective race/class and gender treatment conditions are shown in Table A2.

Table A2: Voter characteristics

First name	Last name	Email	Gender	Class & race
Carlos	Chimenes	carloschime@gmail.com	male	no signal
Pedro	Aparecido	pedroapare@gmail.com	male	no signal
Gabriel	Antunes	gabriel.antunes09@gmail.com	male	no signal
Tiago	Bottino	tiabotta@gmail.com	male	no signal
Chardley	da Silva	chardleysilva02@gmail.com	male	lower
Jeorge Washington	Oliveira	jeorgewoliveira@gmail.com	male	lower
Charlington	Moraes	charlingtonmoraes@gmail.com	male	lower
Macmaillan	Barbosa	barbosa.macmaillan7@gmail.com	male	lower
Ibarantã	Ualri	ibaranta.ualri@gmail.com	male	Amerindian
Ubatã	Aikanã	ubaaika@gmail.com	male	Amerindian
Cauré	Guajajara	caure.guajajara@gmail.com	male	Amerindian
Juruna	Acatauassú	juruna.acatauassu@gmail.com	male	Amerindian
Fátima	Cardoso	fatima.cardoso08@gmail.com	female	no signal
Sônia	Texeira	sonia.texeira09@gmail.com	female	no signal
Lúcia	Amorim	lucia.amorim06@gmail.com	female	no signal
Fernanda	Lacerda	fernanda.lacerda09@gmail.com	female	no signal
Lyndiane	Ferreira	lyndiferr@gmail.com	female	lower
Raucilene	Pereira	raucilene.pereira06@gmail.com	female	lower
Britney	Silvério	britneysilv@gmail.com	female	lower
Rozemilce	Bezerra	roze.rra@gmail.com	female	lower
Iacina	Nenã	iacinen@gmail.com	female	Amerindian
Anajá	Obéima	anaobei@gmail.com	female	Amerindian
Guaraciaba	Tibiriçá	guaraciaba.t@gmail.com	female	Amerindian
Jaciaba	Uramutã	jaciaba.u@gmail.com	female	Amerindian

The table displays the names of the 24 fictitious voters used in the experiment as well as their gmail addresses, gender, and social class or race.

To evaluate the validity of the names used in our experimental treatments, we tested our names on a sample of Brazilian university students (N=145). Students were given the list of names and asked to classify each name on the basis of social class and ethnicity. The results of these open-ended surveys of students' perceptions, as well as our intended experimental treatment, are shown in Tables A3 and A4. The students had no trouble picking up on our experimental prompts.

Table A3: Instrument validity: Students' Perceptions of Names and Race, Percentages (%)

Name	Experimental Treatment	Amer-Indian	Black	White
Carlos Chimenes	no signal	7.14	13.49	79.37
Pedro Aparecido	no signal	0.00	46.46	53.54
Gabriel Antunes	no signal	1.54	23.08	75.38
Tiago Bottino	no signal	1.55	13.18	85.27
Chardley da Silva	lower	2.29	70.99	26.72
Jeorge Washington Oliveira	lower	1.56	63.28	35.16
Charlington Moraes	lower	1.55	60.47	37.98
Macmaillan Barbosa	lower	1.57	59.06	39.37
Ibarantã Ualri	Amerindian	85.50	4.58	9.92
Ubatã Aikanã	Amerindian	94.66	2.29	3.05
Cauré Guajajara	Amerindian	87.79	9.92	2.29
Juruna Acatauassú	Amerindian	92.31	6.92	0.77
Fátima Cardoso	no signal	0.00	16.41	83.59
Sônia Texeira	no signal	0.79	27.56	71.65
Lúcia Amorim	no signal	0.79	14.96	84.25
Fernanda Lacerda	no signal	0.77	11.54	87.69
Lyndiane Ferreira	lower	2.36	55.12	42.52
Raucilene Pereira	lower	1.55	70.54	27.91
Britney Silvério	lower	1.56	42.19	56.25
Rozemilce Bezerra	lower	3.12	51.56	45.31
Iacina Nenã	Amerindian	82.31	9.23	8.46
Anajá Obéima	Amerindian	74.62	23.08	2.31
Guaraciaba Tibiriçá	Amerindian	86.82	3.88	9.30
Jaciaba Uramutã	Amerindian	88.46	7.69	3.85

This table reports all names used in the experiment and their intended racial group. The last three columns report the percentage of students who spontaneously classified each name into a particular racial category, which together sum to 100%. Pearson chi-square test = 3.0e+03 with 46 d.f.; p-value < 0.001.

Of particular note are several patterns relating race and social class. 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. The Amerindian treatments were abun-

dantly clear: students easily identified Amerindian names based on non-Portuguese heritage and atypical accentuation, but also interpreted names of Amerindian heritage as a signal of lower class status approximately 66% of the time. While our "lower class" names elicited correct classification 67% of the time, they were also much more likely to be perceived as belonging to individuals of afro-Brazilian descent. Though we cannot disentangle here which dimension is more informative (whether respondents classify the name with respect to race and then infer their social class, or visa versa), the pattern observed in the students' responses are consistent with aggregate correlations of race and social class in Brazil. Though we keep with Brazilian convention and characterize this treatment effect as one which is based on social class, we acknowledge and these results make clear that social class and race are inextricably entwined.

Table A4: Instrument validity: Students' Perceptions of Names and Social Class, Percentages (%)

Name	Experimental Treatment	Lower	Middle	Upper
Carlos Chimenes	no signal	8.00	56.00	36.00
Pedro Aparecido	no signal	22.66	64.84	12.50
Gabriel Antunes	no signal	7.09	43.31	49.61
Tiago Bottino	no signal	4.76	30.95	64.29
Chardley da Silva	lower	82.81	14.06	3.12
Jeorge Washington Oliveira	lower	61.42	31.50	7.09
Charlington Moraes	lower	66.41	25.00	8.59
Macmaillan Barbosa	lower	67.44	23.26	9.30
Ibarantã Ualri	Amerindian	62.70	30.16	7.14
Ubatã Aikanã	Amerindian	64.00	31.20	4.80
Cauré Guajajara	Amerindian	71.77	22.58	5.65
Juruna Acatauassú	Amerindian	66.40	30.40	3.20
Fátima Cardoso	no signal	11.11	57.14	31.75
Sônia Texeira	no signal	9.52	63.49	26.98
Lúcia Amorim	no signal	7.03	31.25	61.72
Fernanda Lacerda	no signal	5.51	29.92	64.57
Lyndiane Ferreira	lower	49.61	44.88	5.51
Raucilene Pereira	lower	66.41	30.47	3.12
Britney Silvério	lower	73.23	21.26	5.51
Rozemilce Bezerra	lower	70.31	28.91	0.78
Iacina Nenã	Amerindian	69.05	29.37	1.59
Anajá Obéima	Amerindian	65.85	31.71	2.44
Guaraciaba Tibiriçá	Amerindian	61.60	32.80	5.60
Jaciaba Uramutã	Amerindian	68.80	26.40	4.80

This table reports all names used in the experiment and their intended social class. The last three columns report the percentage of students who spontaneously classified each name into a particular socio-demographic category, which together sum to 100%. Pearson chi-square test = 1.4e+03 with 46 d.f.; p-value < 0.001.

III. External validity

We evaluate here the extent to which our experimental administration, in which legislative candidates' received emails from putative voters from variable and randomized socioeconomic backgrounds, comports with the reality of contemporary Brazilian politics. Of particular interest is whether the mere fact of receiving an email might have cued candidates to our email senders' socioeconomic status, education, race, location or turnout propensity. This would be problematic if email was an uncommon form of political communication, or if the use of email was so concentrated in citizens of higher socioeconomic status that our candidates would find it impossible to believe that our emails had come from socioeconomically disadvantaged voters. This second concern may be especially pernicious in rural areas, where a much higher concentration of poor Brazilians are known to reside. The following analysis addresses these challenges and concerns.

A. Electoral System Characteristics and Demographic Geography

There are several features of the Brazilian electoral system and demographic geography that lends contextual credibility to our experimental conditions, or minimally would imply that receiving an email from a voter from a lower socioeconomic profile would not cause alarm.

First, all federal and state deputy candidates compete in an open-list PR system for multiple positions in state-wide districts, implying that candidates can earn electoral votes from anywhere in the state (Ames 1995a, Ames 1995b; Nicolau 2007). At $315,399$ km^2, the average Brazilian state is roughly the size of New Mexico or Poland, with the median population akin to that of Connecticut or Iowa (pop.=3.5 million, mean pop. $\approx$ 7 M). In the context of our experiment, our putative voters stated their residence in the candidates' electoral district in the first line of the email, meaning the candidates could infer that said voter lived anywhere within those state-wide territorial limits.

Second, the vast majority of Brazilians now reside in urban environments. The population census of 2010 reports that the rural population of Brazil has been declining steadily since 1970, and that 84.4% of Brazilians resided in urban and metropolitan centers as of 2010 (IBGE 2010).[1] Though urban Brazilians are much more likely to be users of the internet (a fact we explore in more depth below), Brazilians as a whole are also more likely to live in the metropolitan areas.

Third, as of 2010, fewer Brazilians lived in poverty than ever before. Massive macro-economic expansion coupled with aggressive poverty alleviation policies implemented under President Lula (2003-2011) lifted nearly 30 million Brazilians out of abject poverty since the turn of the century (Neri 2010). Though the IBGE and other government agencies did not begin using a standardized definition of social class until 2012, the independent agency of the Brazilian Association of Research Institutes, or ABEP, classified households into one of 5 social categories based on their monthly salary and the ownership of a variety of consumer goods (ABEP 2012; Neri 2010). The lowest social

[1]The percentage of urban dwellers easily surpasses 90% in the Southeast region of country, home to both São Paolo and Rio de Janeiro (95.9% and 96.7% urban, respectively), though the urban population still represents a strong absolute majority in the least urbanized regions of the country, in states like Maranhão (63.1%), Piauí (65.8%) and Pará (68.5%) (IBGE 2010).

tiers, Classes D & E, account for approximately 16% of all Brazilians and are those living beneath the state defined poverty line.[2] Next is Class C, which has been dubbed by some as the "new middle class," which constituted a very near majority of Brazilians in 2010 (49.5%). The upper middle class, Class B, accounts for the next 30.4%, while the highest social class (Class A, or "elites") represented only 4.1% of all Brazilians in 2010. Though the elevation of millions of Brazilians out of misery is unequivocally positive, critics charge it is a misnomer to label those recently exiting poverty the "middle class," as most do not enjoy the economic security, occupational prestige or social mobility that is usually associated with the term.[3] Instead, many argue that members of Class C are are closer to what in the United States we would consider the "working poor" (The Economist 2012; Pearson 2015). In the context of our experiment (and as we detail below), the recent elevation of so many out of poverty implies a broad cross-section of Brazilians could have credibly believed to have authored our emails.

B. Internet and Email Access across the Socioeconomic Spectrum

Coinciding with the rapid ascent of many Brazilians out of poverty is the broad proliferation of internet access to a broad and increasingly diverse cross-section of the Brazilian population. A survey of households conducted by the Brazilian Institute of Geography and Statistics, (or IBGE) describes that 46.5% of Brazilians reported having used the internet in 2011, an increase of 144% over 2005 (IBGE PNAD 2011). Though personal computer ownership remains a mark of economic privilege, it was lower-class Brazilians who made the largest gains: internet access by those making up to one minimum salary more than tripled, from 9.1% in 2005 to 30.3% in 2011 (IBGE PNAD 2011). The Brazilian Internet Steering Committee (or CGIB) attributes this increase by members of the lower class and working poor to the proliferation of internet cafés (or LanHouses) throughout the country, which offer access to computers with internet access for free or a very small cost (ICT 2010).[4] The report underscores the importance of cyber-cafes and LanHouses to both rural and lower-class Brazilians: as of 2009, an absolute majority of internet users (54%) in rural areas reported accessing the internet in a LanHouse, and LanHouses were the primary location of internet access for a broad majority if internet users from the lowest socioeconomic strata (Classes D & E, 58%). As we explore in more detail below, the increasing prevalence of internet access throughout Brazil, and especially among Brazilians from lower social classes in particular, lends external validity to our experimental design.

The engagement of millions of Brazilians into the *world wide web* has quickly transformed electoral campaigning, with email and social media serving as a critical medium through which candidates recruit and mobilize supporters, and through which voters gather information on candidates and elected officials. Since the internet fueled rise of Barack Obama in 2008, political candidates the world over have sought to emulate his success, building extensive rosters of emails,

[2]According to the 2010 census, the population of Brazil was 190.8 million people (IBGE 2010).

[3]Kerstenstzky et al. (2015) show that more than a third of all Brazilians fall into the lowest third of Class C who were more similar to Classes D & E in their housing conditions, consumption patterns, and social mobility, than to other members of the "new middle class."

[4]The ICT (2010) LanHouses Survey included face-to-face structured and unstructured interviews with a a representative sample of the owners and managers of LanHouses throughout the country.

Facebook and Twitter followers, and curating their own online presence to directly mobilize grassroots support (Aggio 2008). Braga (2011) reports that 69.2% of Brazilian gubernatorial and federal senate candidates in 2010 registered their campaign domain, with most candidates' websites advertising their party and candidate number, their platforms and priorities, as well as their personal contact information. Though Braga et al. (2011) suggest that social media platforms and text messages are candidates' preferred method of en masse communication (2011), focus groups with Brazilian internet users reveal that voters view email as a complementary, more intimate tool for communicating with select politicians, through which candidates foster a closer connection to their constituents (Alde 2011). Brandão (2008) considered constituent emails received by the top two presidential candidates of 2006 (then-President Lula da Silva and opposition Gerardo Alckmin), reporting the candidates received upwards of 500 emails per day throughout the campaign (p. 146, 2008). The largest proportion of emails directed to the opposition candidate Alckmin were questions about the candidate's platform and plans for the future (38%), with another 25% of the emails offering policy and campaign suggestions, many of whom declared that their decision in the election would hinge on the candidates' response (155, Brandão 2008).[5] This widespread use of the internet and email by both candidates and constituents gives us confidence that our emails would not have been seen as atypical, or caused our candidates undue alarm. A sample of the text of the emails we received from candidates is shown in Figure 2A.

To evaluate the realism of our emails more concretely, we collected information on the frequency of internet and email usage, based on self-reported behaviors from internet users throughout Brazil. Our data comes from the 2011 ICT Households and Enterprises Survey, which includes information not only on computer usage and internet access, but also the use of email communications (ICT 2012).[6] The results of the 2011 ICT Survey are reported in Table A5. The first two columns are the results from the nationally representative household survey of Brazilians over the age of 10, which queried respondents' past internet access.The final two columns describe the percentage of internet users (those who accessed the internet in the past three months) who use the internet to communicate or to send and/or receive email.

Overall, the ICT survey reveals that the sorts of Brazilians who access the internet is both broad in scope and increasingly diversified, an absolute majority of Brazilians reported having previously accessed the internet. Though internet use is widespread, internet access correlates strongly with social class and socioeconomic privilege: access is extremely common amongst those with a college degree (91%), amongst those from the highest rung of the social ladder (Class A, 92%), while an overwhelming majority of Brazilians who are illiterate or from the lowest socioeconomic classes, have never accessed the internet (96% and 83%, respectively). Geographically speaking, internet access is more common in the relatively affluent and urbanized states of the South, Southeast and Center-West, as opposed to the relatively impoverished and isolated North and Northeast regions of the country. Likewise, though an absolute majority of urban dwellers have reportedly accessed the internet, a large majority of rural dwellers (75%) have never done so.

[5]For the Incumbent President Lula da Silva, 56% of the emails received were criticisms of his previous administration and policies.

[6]Though the 2010 version of the H&E Survey more closely corresponds to the time period of our study, said survey did not include the battery of questions on email usage. The aggregate proportions of internet usage across groups were similar to those reported here.

Table A5: Self Reported Internet Access & Use of Internet/Email to Communicate

Percentage %		Previously Accessed the Internet	Never Accessed	Use internet to Communicate†	Use email to Send/Receive Messages†
Social Class	A	94	6	98	96
	B	83	17	94	87
	C	54	46	89	73
	D/E	21	79	83	58
Education	Illiterate	4	96	81	60
	Elementary	36	64	83	63
	Secondary	80	20	92	81
	Tertiary	94	6	98	96
Region	Southeast	61	39	91	80
	Northeast	39	61	90	74
	South	58	42	91	82
	North	43	57	88	76
	Center-West	60	40	92	74
Area	Urban	58	42	91	79
	Rural	25	75	84	66

Percentage of the total Brazilian population, based on a nationally representative survey of 25,000 interviewees. Reproduction of Tables C2, C5 & C6 in the ICT Households and Enterprises Survey (ICT 2012). †Percentages based on the total number of respondents reporting having used the internet in the last three months, which was a total of 11336 interviewees.

Despite these marked differences across-socioeconomic classes, respondents from all strata report surprising consistency in their use of the internet: 90% of internet users report using the internet as a means of communication, with an average of 76% of them having used email to do so. In other words, though Brazilian users of the internet are considerably diverse in their socioeconomic background, they appear surprisingly unified in their intent and means of engagement.

To better evaluate the external validity of our experimental administration in light of these aggregate patterns, we can incorporate information on the aggregate distribution of Brazilians by social class, education, and urbanization with the rates of internet access across said groups. More pointedly, we can use Bayes' rule to quantify the candidates' probabilistic guess of the true identity of the email sender, conditional on having received an email.[7] This will, in effect, allow us to simulate candidates' inferences about the real identity of our putative voters.

[7]Bayes' theorem, or the Law of Inverse probability, allows for the updating of inferences about the probability of a hypothesis (A), in the face of new or given information (X). This approach derives the conditional probability of an event (the posterior, $Pr(A|X)$) from the "inverse" conditional probability (the likelihood, $Pr(X|A)$) and the unconditional probability of X (the prior). Whereas Bayes' rule defines how inferences and beliefs may be updated in light of new evidence, it is often invoked as a way to characterize individuals' probabilistic beliefs.

In our application, we can define the various components of Bayes' rule in the following way:

- Pr(A)=The probability of being from a particular sociodemographic profile.
- Pr(X|A)= The probability of receiving an email (X) from a voter from a particular sociodemographic profile. This is the chance of a true positive in our email 'test.'
- Pr(-A)=The probability of *not* being from a particular sociodemographic profile.
- Pr(X|-A)=The probability of an email (X), for a given voter *not* from a particular sociodemographic profile. This is the chance of a false positive in our email 'test.'
- Pr(A|X)= The probability of being a voter of a particular sociodemographic profile, conditional on having received an email (X).

With these probabilities in mind, we can evaluate the probability of having received an email from a putative voter from a particular sociodemographic profile, per Bayes' rule:

$$Pr(A|X) = \frac{Pr(X|A)Pr(A)}{Pr(X|A)Pr(A) + Pr(X|-A)Pr(-A)} \tag{A.1}$$

Table A6 reports the component parts of Bayes' rule, as well as the resulting quantity of theoretical interest, which is the probability that a voter from a given sociodemographic profile is the true author of the email, conditional on having received one (Pr(A|X)). The Table combines information on the national and regional distribution of urban and rural populations, social class and educational levels (parameters Pr(A) and Pr(-A) described above), while the probability of having received an email from a person from a particular socioeconomic background (Pr(X|A) and Pr(X|-A)) are taken from the ICT Household and Enterprises survey results reported above in Table A5 (ICT 2012).[8]

Table A6 gives clearer insight into the sorts of inferences candidates' might have made about the identity of our email senders, assuming they had some knowledge of the frequency of internet usage amongst Brazilians. The probabilities listed in the far right column support our claim that our emails from putatively lower-class Brazilians (Class C) would have been taken as credible email submissions. As we mention in the main text of the paper, our candidates would likely have ruled out the possibility that the emails they received were from the most destitute of Brazilian society: the posterior probability of receiving an email from either an illiterate Brazilian with less than a fifth grade education, or from a Brazilian of the lowest socioeconomic strata (Classes D & E) is very small—only about 5% in either case. But the posterior probability of having received an email from a Brazilian from the lower end of the socioeconomic spectrum (Class C) constitutes a wide plurality of the possibilities: absent additional information given by our experimental treatment about the

[8]Data on the level of urbanization and education acquisition taken from the IBGE Population Census survey of 2010 (IBGE 2010). Information on the scoring and distribution of social classes in Brazil taken from the Associação Brasileira de Empresas de Pesquisa, or ABEP (Brazilian Association of Research Institutes), on which the ICT Household surveys social class metric is based (2011).

Table A6: Probability of having received an email from a putative voter from a particular socioeconomic background, per Bayes' Rule

Demographic Profile of Theoretical Interest	Pr(A) Pr(Profile)	Pr(X|A) Pr(Email|Profile)	Pr(-A) Pr(-Profile)	Pr(X|-A) Pr(Email|-Profile)	Pr(A|X) Pr(Profile|Email)
Very Poor (D/E Classes)	.16	.21	.84	.77	.05
Lower Class (Class C)†	.60	.54	.40	.89	.48
Upper Class (Class B)†	.36	.83	.84	.74	.39
Very Wealthy (Class A)†	.04	.94	.96	.68	.05
Illiterate/Pre-School	.49	.04	.51	.70	.05
Elementary Education†	.29	.36	.71	.87	.14
Secondary Education†	.49	.80	.51	.65	.54
College Educated†	.22	.94	.78	.58	.31
Urban	.85	.50	.15	.25	.93

Distribution of sociodemographic population taken from the ABEP report "The Criteria of Brazilian Economic Classification" (ABEP 2012). Distribution of self-reported internet access taken from the ICT Households and Businesses Survey of 2011, on the use of information and communication technologies in Brazil (ICT 2012). †Lowest strata (illiterate/preschool or very poor) excluded.

race and/or class of our putative voters, candidates might have inferred that a member of the working poor (Class C) was the true author of the email with probability of .48. Though this clearly leaves open considerable probability that the emails' authors' were from a more privileged socioeconomic position, it certainly wouldn't have rendered this latter conclusion foregone. Likewise, in terms of the likely education level of email users in Brazil, our candidates' would have attributed .54 probability to having received an email from a high school graduate, though considerable probability that the email was sent by someone with less than a high-school education (.15). The final row of A6 shows that our candidates would have attributed an overwhelming probability to the chance that the author of the emails in question were from an urbanized region of their state, given the high rate of internet access in cities, and that 85% of Brazilians now live in urban and metropolitan environments.

A final check on this framework evaluates these inferences across regional context. Brazil is vast, with considerable socioeconomic disparities across regions.[9] Taking this into consideration, our comparisons are even starker.[10] In the North and Northeast regions of the country, where the upper classes (Class B & A) constitute only about 12% of the population, candidates would have attributed a whopping 72 percent probability to the chance that our email was sent by a member

[9]We acknowledge here the ideal data for this analysis would be state or regional summaries of internet usage across social classes. Unfortunately, the ICT surveys did not provide subnational summary statistics of their survey data. As such, we must rely on the national averages of internet use, while varying the proportion of the population that falls into social classes across the regions.

[10]Here we rely on the distribution of social classes across states as reported in the national press (Veras Mota 2014), as it most closely corresponded to the distinctions as defined in the ABEP reports. Said figures defined Classes D & E as "Poor" and Classes B & A as "Upper Class".

of the lower class or working poor (Class C).[11] In the South, where upper class citizens constitute a larger proportion of the greater population, this posterior probability falls to 52%. This means that even in areas of the country with a relatively high concentration of upper class Brazilians, the relative weight of the lower class weighs disproportionately into the likelihood that the email sender is not from a position of socioeconomic privilege.

C. Amerindians and the digital divide

The ICT Households and Enterprises survey did not directly address the prevalence of internet or email access across Brazilians from differing ethnic or racial profiles. This is an especially important consideration in light of our experimental treatment which contained putative authors' names from distinctively Amerindian backgrounds. Amerindians constitute a very small proportion of Brazilians, only about .47% of the population as a whole. Clearly, it is highly unlikely that our emails had originated from any of the most isolated indigenous communities, or by an Amerindian who lack literacy in the Portuguese language. Likewise, our candidates would probably infer the indigenous authors of our emails were more likely urban residents, and likely on the younger end of the spectrum, in light of the broader patterns of internet access and usage throughout Brazil more generally. The candidates might also infer that the indigenous person contacting them was more likely a community leader, or otherwise more directly involved in political leadership or activism than your average Brazilian voter.

With these reservations aside, there are also a number of features of the Brazilian Amerindian population in relation to our political candidates, as well as their likely access to the internet, that lends credibility to our experimental treatment. First, the 2010 census confirms that Amerindians reside in all 26 states including the capital district (IBGE 2010). Fifty-eight percent of indigenous Brazilians live on protected Amerindian communities (akin to reservations in the United States), and 63% inhabit rural areas, but a sizable proportion of indigenous Brazilians (36%) live in urban environments as well. For this reason, and owing to the fact that candidates compete in state-wide districts, all state and federal representatives represent at least some indigenous constituents to whom they could respond.

Second, in tandem with the broader expansion of internet access throughout Brazil, government and non-governmental programs at both the state and national level have prioritized the integration of indigenous communities into the *world wide web*, advancing programs of "digital inclusion" to indigenous communities throughout Brazil (Souza & Tomizawa 2014; Klein & Renesse 2014). The programs often leverage the communitarian structure of shared common resources to expand digital access throughout indigenous communities, providing a access to the internet in a centralized location such as the community center or public school. Preliminary reports from these efforts suggest that at least some members of the indigenous community, especially the young and politically active, view the internet as a valuable opportunity to increased engagement with the broader Brazilian and worldwide community (Souza & Tomizawa 2014; Klein & Renesse 2014). Though systematic information on these efforts is not available, as of 2017, the Indigenous Peoples

[11] As above, this calculation assumes candidates would rule out the probability of receiving an email from the very poor (Classes D & E).

of Brazil (PIB) NGO identifies nearly 70 indigenous websites maintained by Amerindian communities and activist networks,[12] who use the internet and related technologies to educate others about their culture and tradition, and increase awareness of their broader political priorities and projects.

IV. Candidates with and without email addresses

Table A7 reports demographic information on the universe of Brazilian candidates pursuing a position as a state or federal deputy in 2010 (N=20,085), partitioned on whether or not the candidate registered an email with the TSE. On average, candidates with email addresses were more often male, educated, from the left end of the ideological spectrum.[13] Candidates heralding from the North or Northeast were less likely to report an email address, while candidates from the South or Southeast were more likely to report one.[14] For each bivariate relation, we report the Pearson's $\chi^2(1)$ test statistic (or in the case of *Age*, the t-statistic of the difference of means), to evaluate the extent to which observed differences across the candidates with and without email addresses might arise by chance. As seen in column 3, with the exception of candidates' marital status ($p = .14$), the probability of observing test statistics this extreme under the null hypothesis condition of no relationship is extremely small ($p = .00$).

Table A7: Bivariate distributions of observable traits among Brazilian legislative candidates with and without email addresses

	Email	No Email	Pearson $\chi^2(1)$
Male	80.2%	74.6%	90.1
Married	58.5%	57.5%	2.1
Age (Mean)†	46.8%	47.3%	3.1
College education	52.8%	37.0%	516.4
North/Northeast	33.3%	46.0%	344.3
South/Southeast	60.3%	44.2%	527.6
Leftists	46.4%	42.1%	24.9

†The t-test statistic for a difference in means.

To evaluate the extent to which these differences between candidates who did and did not report an email address to the TSE might impact our results, we estimated a Heckman Probit model. Our selection model estimates the effect of candidate's gender, marital status, age, education and

[12]Please see https://pib.socioambiental.org/pt/c/iniciativas-indigenas/autoria-indigena/sites-indigenas

[13]Ideological data coded based on the national party ideological scores reported in Nunes (2012), and Lucas & Samuels (2010). Ideological information was available for only 16 of the 27 parties, based on their national prominence and national representation and reduces the sample size considerably (N=13,549). In keeping with these authors, we classify parties using a trichotomous indicator of ideology (L, C, R).

[14]The constituencies of the North and Northeast tend to be poorer, rural, with a higher concentration of Brazilians of African and Amerindian descent. The South and Southeast, home to the major metropolitan cities of Rio de Janiero and São Paolo, are wealthier and more urban. Please note that we use regional summaries only here for exploratory purposes, and use state level indicators for all analyses reported in the main text.

region on the likelihood of reporting an email to the TSE. These same individual level covariates are then included as predictors of an email response to our experiment (*Reply*=1), along with our measures of electoral viability and incumbency.[15] If, for example, only the most communicative and motivated candidates reported an email in the first place, then this would drastically reduce the scope of candidates (or elected politicians) to which our experimental results might apply.

Table A8: Heckman Probit model with sample selection

Registered Email	Coefficient (s.e.)	**Email Reply**	Coefficient (s.e.)
Male	−0.258*		0.087
	(0.022)		(0.048)
Married	−0.022*		−0.003
	(0.005)		(0.010)
Age	−0.007*		0.004*
	(0.001)		(0.001)
College education	0.440*		−0.144*
	(0.033)		(0.071)
North/Northeast	−0.007		
	(0.077)		
South/Southeast	0.408*		
	(0.150)		
Electoral Viability			−0.160*
			(0.045)
Incumbent			−.058
			(0.051)
Constant	−0.039		0.176*
	(0.129)		(0.081)
Rho	−0.869		
	(0.078)		
$\chi^2(1)$(Rho=0)	17.22		
Prob > χ^2	0.00		
Censored/Uncensored Obs	9840		8755

* denotes statistical significance at 0.05 level.

The results of said selection model are reported in Table A8, shown above.[16] As reported above, candidates reporting their email addresses to the TSE were more often male, younger, college educated and from the Southern part of the country.[17] When we evaluate the effect of these covariates on the likelihood of replying to our email, each of the individual level covariates

[15] As we only collected incumbency and viability data for the candidates who were part of our experiment, we cannot include these two covariates in our selection model.

[16] The Heckman probit selection model includes robust standard errors clustered on party. The results are substantively very similar if we instead estimate clustered standard errors on state, or leave out clustered standard errors altogether.

[17] We do not include ideological orientation as a predictor, as it reduces the sample of candidates by nearly one third.

works in the opposite direction. Female candidates were more responsive to our emails, as were older candidates. Whereas college educated candidates were appreciably more likely to report an email to the TSE, the college educated candidates in our sample were *less* likely to respond to our emails, as were candidates we later identified as *viable* based on their nominal vote rank within their electoral coalition. The parameter ρ characterizes the how the errors in each equation relate to one another; the negative correlation (ρ =-0.869) indicates that the process of reporting and email and replying to said email are inversely related. 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.

V. Candidate resources and responsiveness

We argue in the main paper that candidates' responsiveness to putative voters and constituents is driven both by candidates' social class and their electoral viability. A reasonable alternative hypothesis would posit that candidates' willingness to respond to a voter may depend instead upon her campaign resources, which would allow her to hire staff, or purchase technology that would reduce the costs of email responsiveness. If college education or candidate viability were correlated to candidates' resources, we may be misattributing responsiveness on the basis of social class or electoral viability to what is actually a matter of financial resources.

To evaluate this alternative hypothesis, we collected data on candidates' declared *Fundraising* activities and donations received during the course of their electoral campaign as reported to the TSE (TSE, 2010). Evaluating candidates' *Fundraising* relative to candidates' *College* education, we find a modest correlation between *College* and *Fundraising* (ρ = .12, ρ = .19 with logged *Fundraising*). *Fundraising* is more strongly correlated with electoral *Viability*: ρ = .37 for candidates' absolute *Fundraising* (ρ = .58 for logged *Fundraising*). Even with these somewhat stronger statistical correlations, candidates' self-reported resources do not appear to be very strong proxies for either *College* education or candidates' electoral *Viability*.

Table A9: Effect of Candidate Resources on Email Responsiveness in October 2010 and in March 2011 Experiments

	(1) Pre-election	(2) Post-election	(3) Pre-election	(4) Post-election
Fundraising	-0.041***	-0.035**		
	(0.010)	(0.012)		
Log(Fund.)			-0.094***	-0.026
			(0.013)	(0.015)
Observations	7186	7186	7186	7186

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Turning now to the effects of candidates' campaign resources on email responsiveness, we

regress the responsiveness to our emails both in October 2010 (pre-election) and in March 2011 (post-election) on our measure *Fundraising* using a logistic regression. We also report the effects of logged *Fundraising*, to correct for very strong skew in our data on candidates' campaign resources.[18] We report the results of these regressions in Table A9. Table A9 shows that campaign resources appear to be inversely related to responsiveness to our email. The coefficients for candidates' *Fundraising* (models 1-4) exceed conventional thresholds of statistical significance ($p < .05$), though the negative relationship between these two variables suggests that the most successful fundraisers were actually *less* responsive to our emails overall. Moreover, the substantive effect of these variables is tiny: increasing candidates' fundraising by one million Reais (approximately US$ 600,240 in Oct 2010), decreases the probability of receiving an pre-election email by on average 0.66%.This gives us confidence we are not capturing the effect of campaign resources in the interpretation of our results.

VI. Alternative measures of candidate viability

Our measure of candidate *Viability* is meant to capture candidates' competitiveness, or the objective likelihood a given individual could credibly win a legislative seat. This differentiation is important given the very large pool of candidates in any given district, and the fact that the vast majority of candidates (90%) are not elected to office. We collected the vote totals of all the candidates in our study, identified those who had been elected to office, and then ranked all remaining candidates within their electoral coalition on the basis of their respective nominal vote total. The measure reported in the main body of the paper classifies a candidate as *Viable* if he or she was elected to office, or if (s)he was ranked among the top ten 'losers' within her district-specific electoral coalition or party.

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. Please recall that the vast majority of candidates (70%) are designated as "suplentes", or legislative alternates, whose party won sufficient number of votes to earn at least one legislative seat, but whose individual vote totals did not rank them sufficiently high within the party list. Suplentes may be called to serve in the legislature in the case of vacancies before the end of the legislative term, due to a leave of absence or outright resignation. In his survey of legislative careers of federal deputies, Samuels suggests that since the transition to democracy is 1986, approximately 20% of federal deputies elected to the Chamber leave that position prior to the end of the legislative term, and up to 40% attempt to leave in pursuit of a different elected office (50, 2003). Whereas a candidate's rank on the list of suplentes is determined by his or her rank on the electoral list, our measure of *Viability* used in the manuscript counts all candidates elected to office, as well as the top ten suplentes who would fill a position in case of vacancy.

Here, we report on the results from our two experiments using alternative measures, based on more stringent metrics of candidate *Viability*. As in the main body of the paper, each table reports the simulated treatment effect estimates (first differences), differences in treatment effect

[18] To facilitate interpretation, we divided *Fundraising* by one million.

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*, an indicator for college degree and all controls. Robust standard errors are shown in parentheses.

A. A scaled measure of candidate viability, based on effective party magnitude

Tables A10 and A11 show the results of our analysis when we employ a measure of viability which is scaled to account for variance in party size and the expected level of turnover before the end of the legislative term. To create this measure, we focus on the 30% of federal deputies who were elected in 2010 but resigned or abandoned their posts before the end of the legislative term.[19] We take this 30% as a proxy for the likelihood that a legislative alternate (suplente) would be called to serve in the legislature, based on the probability of vacancy left open by one or more of his coalition members who first assumed office. To scale our measure of *Viability*, we counted the number of legislative posts each electoral coalition won in the 2010 elections, which we then multiplied by 1.3 to determine the number of 'suplente' candidates who would be called to serve in the legislature, or would be classified in our rubric as *Viable*. This has the attractive quality of scaling the list of *Viable* candidates based on the size of the electoral coalition; in a coalition where only one person was elected, the total number of *Viable* candidates would be 2 (1 elected & the top suplente), in an electoral coalition where 6 members were elected, the total number of viable candidates would total 8 (6 & the top 2 candidates). Whereas a wide majority of our legislative candidates competed as part of an electoral coalition where only a single candidate earned office, this more stringent measure of *Scaled Viability* has the effect of diminishing the number of these candidates which we classify as electorally *Viable*.

Table A10: Treatment effect estimates in October 2010 (pre-election) experiment

	Non-Viable (Scaled)			Viable (Scaled)			
College degree	no	yes	DD	no	yes	DD	DDD
Treatments	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Lower class	−0.011	−0.026	0.015	−0.013	0.024	−0.037	0.052
	(0.016)	(0.018)	(0.024)	(0.051)	(0.030)	(0.060)	(0.064)
Amerindian	−0.003	−0.056**	0.053*	0.008	0.043	−0.035	0.088
	(0.016)	(0.017)	(0.024)	(0.052)	(0.033)	(0.060)	(0.065)
Female	0.022*	−0.006	0.029	−0.026	0.033	−0.059	0.088*
	(0.013)	(0.014)	(0.019)	(0.042)	(0.026)	(0.050)	(0.053)
N	3155	3361	–	315	856	–	7,687

The results of our analysis using this more stringent measure of *Scaled Viability* are shown in Tables A10 and A11. Comparing the most electorally successful candidates by this measure to all

[19] Of the 513 national deputies that were elected in 2010, 163 (or 32%) prematurely abandoned their post.

others in the race, we find results are similar to those reported in the paper, though we lack statistical precision on several coefficients. In the preelection experiment, we find evidence of prejudiced discrimination against putatively Lower-class and Amerindian voters from candidates with higher socioeconomic status, though this effect is moderated by candidates' electoral *Viability*. The negative coefficients suggest that *Non-viable* candidates were generally less responsive to Lower-class and Amerindian voters, though the magnitude of these coefficients is considerably larger amongst candidates who come from a position of socioeconomic privilege. Though we lack statistical power to differentiate these coefficients from zero, the coefficient for Amerindian is statistically significant at the $p < .01$ level. As reported in the paper, this trend ostensibly reverses amongst electorally *Viable* candidates: though we cannot differentiate these coefficients from zero, the positive coefficient sign suggests socioeconomically privileged *Viable* candidates may have actually been more responsive to voters from putatively lower-class or Amerindian descent.

Table A11: Treatment effect estimates in March 2011 (post-election) experiment

	Viable (Scaled), Not-Elected			Viable (Scaled), Elected			
College degree	no	yes	DD	no	yes	DD	DDD
Treatments	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Lower class	0.018	−0.044	0.062	0.026	−0.070*	0.096	−0.034
	(0.084)	(0.057)	(0.101)	(0.056)	(0.032)	(0.064)	(0.119)
Amerindian	0.083	−0.069	0.151	0.021	−0.051	0.072	0.080
	(0.103)	(0.055)	(0.116)	(0.055)	(0.032)	(0.064)	(0.132)
Female	−0.053	−0.046	−0.007	−0.049	0.046	−0.095	0.089
	(0.079)	(0.047)	(0.094)	(0.045)	(0.026)	(0.053)	(0.106)
N	95	225	–	220	631	–	1,171

Turning now to the results from the post-election experiment, we find the same general trends as those reported in the main paper. Though *Viable* candidates from socioeconomic privilege appeared more favorably inclined to lower-class and Amerindian voters when pursuing office, this trend reverses once in office. Specifically, while as candidates they appeared to favor (or minimally not discriminate against) putatively lower-class voters, legislators with socioeconomically privileged backgrounds were approximately 7% less responsive to putatively lower class constituents once in office. This effect is statistically significant at a $p < .05$ level.

B. Winners and top three alternates

Table A12 and A13 show the results from our two experiments where candidates are classified as *Viable* if they were elected to office, or if they were one of the top three suplentes. As with the measure of *Scaled Viability*, the magnitude and direction of our coefficients are largely unchanged, though we lack statistical precision in a number of instances. Among candidates classified as *Non-viable*, the coefficients for our race/class treatments are in the expected, negative direction, and the coefficient for Amerindian is significant at the 0.01 level. As with the results reported in the main

text, this discriminatory effect is pronounced for those candidates with a college degree, which is suggestive of racism or classism amongst those pursuing office. Among *Viable (top 3)* candidates, who would win or be at the very top of the suplentes list, this discriminatory behavior is not apparent. Though the coefficients for lower class and Amerindian voters are positively signed, they do not approximate conventional levels of statistically significant. Instead, the most competitive (and electorally successful) candidates responded equally to all fictitious voters.

Table A12: Treatment effect estimates in October 2010 (pre-election) experiment

	Non-Viable (3)			Viable (3)			
College degree	no	yes	DD	no	yes	DD	DDD
Treatments	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Lower class	−0.010	−0.027	0.017	−0.022	0.020	−0.042	0.059
	(0.016)	(0.019)	(0.025)	(0.041)	(0.028)	(0.050)	(0.056)
Amerindian	−0.001	−0.049**	0.048	−0.011	0.005	−0.016	0.064
	(0.016)	(0.018)	(0.025)	(0.041)	(0.028)	(0.051)	(0.057)
Female	0.026	−0.008	0.035	−0.044	0.034	−0.078	0.113*
	(0.014)	(0.015)	(0.020)	(0.034)	(0.023)	(0.041)	(0.046)
N	3033	3149	–	436	1069	–	7,687

Turning to the results of the post-election experiment, which we again partition based on the viable candidates' eventual election, the results are very similar to those shown in the paper. Generally speaking, *Viable (top 3)* candidates, those who had previously responded at equal rates to all fictitious voters when pursuing office, were systematically unresponsive to lower class voters once elected. Consistent with our claim made in the main body of the paper, this effect is pronounced amongst legislators holding a college degree, and statistically differentiable from zero at the $p < .05$ level.

Table A13: Treatment effect estimates in March 2011 (post-election) experiment

	Viable (3), Not-Elected			Viable (3), Elected			
College degree	no	yes	DD	no	yes	DD	DDD
Treatments	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Lower class	−0.008	−0.036	0.028	0.018	−0.067*	0.085	−0.056
	(0.056)	(0.045)	(0.072)	(0.056)	(0.032)	(0.063)	(0.095)
Amerindian	−0.026	−0.087*	0.060	0.026	−0.049	0.075	−0.015
	(0.053)	(0.041)	(0.069)	(0.056)	(0.032)	(0.064)	(0.091)
Female	−0.041	0.018	−0.058	−0.056	0.053*	−0.108*	0.050
	(0.045)	(0.033)	(0.056)	(0.045)	(0.025)	(0.051)	(0.076)
N	215	436	–	211	633	–	1,505

C. Winners and top five alternates

Tables A14 and A15 expand the classified pool of *Viable (top 5)* candidates to those candidates who won office, or were in the top five 'suplente' lists. As was the case of the *Viable (top 3)* candidates listed in Table A12, the positive coefficient on our lower class treatment factor suggests that candidates with a college degree appear slightly more inclined to respond to lower class voters, but as before, we cannot statistically discriminate this estimate from zero. As for candidates classified as *Non-Viable* under this rubric, those with a college degree appear to discriminate against lower class and Amerindian voters. Both of these coefficients are in the negative direction, and both achieve conventional levels of statistical significance.

Table A14: Treatment effect estimates in October 2010 (pre-election) experiment

	Non-Viable (5)			Viable (5)			
College degree	no	yes	DD	no	yes	DD	DDD
Treatments	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Lower class	−0.007	−0.033	0.027	−0.035	0.024	−0.058	0.085
	(0.017)	(0.020)	(0.026)	(0.037)	(0.026)	(0.045)	(0.052)
Amerindian	0.002	−0.060**	0.062*	−0.030	0.018	−0.049	0.110*
	(0.017)	(0.019)	(0.026)	(0.037)	(0.026)	(0.044)	(0.051)
Female	0.023	−0.010	0.033	−0.011	0.031	−0.041	0.075
	(0.014)	(0.016)	(0.021)	(0.029)	(0.021)	(0.036)	(0.041)
N	2888	2876	–	581	1342	–	7,687

As for our post-election experiment results, which we again partition on candidates' eventual election, the results are unchanged and consistent with those reported in the paper. *Viable (top 5)* candidates with a college education respond at equal rates to all voters in advance of the election, but are systematically less responsive to lower class voters once elected.

Table A15: Treatment effect estimates in March 2011 (post-election) experiment

	Viable (5), Not-Elected			Viable (5), Elected			
College degree	no	yes	DD	no	yes	DD	DDD
Treatments	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Lower class	−0.012	−0.020	0.009	0.025	−0.070*	0.095	−0.086
	(0.039)	(0.036)	(0.053)	(0.055)	(0.032)	(0.063)	(0.082)
Amerindian	−0.024	−0.056	0.032	0.028	−0.050	0.078	−0.046
	(0.036)	(0.032)	(0.050)	(0.055)	(0.033)	(0.064)	(0.080)
Female	−0.060*	−0.004	−0.055	−0.056	0.054*	−0.110*	0.054
	(0.030)	(0.027)	(0.041)	(0.045)	(0.026)	(0.052)	(0.066)
N	360	709	–	221	633	–	1,923

D. Winners and top seven alternates

As a final check, we expand our classification of *Viable (top 7)* candidates to include those who were elected or ranked in the top seven suplente positions. The results are the same as reported before, for both experiments.

It is also worth noting the variability of coefficients across these measures points to a non-monotonic relationship between electoral viability and candidate responsiveness, though one which is consistent with the logic herein described. Whereas we anticipate the most competitive candidates will not discriminate in the pre-electoral period, we acknowledge the limit to this logic: faced with almost certain election, "safe" candidates may behave as they please with confidence that their electoral prospects are secure. Indeed, the coefficients for responsiveness to both lower-class and Amerindian voters increase as our measure of *Viability* becomes less and less restrictive (so as to include candidates farther down on the alternate (suplente) candidate list).

Table A16: Treatment effect estimates in October 2010 (pre-election) experiment

	Non-Viable (7)			Viable (7)			
College degree	no	yes	DD	no	yes	DD	DDD
Treatments	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Lower class	−0.008	−0.038	0.030	−0.022	0.023	−0.045	0.074
	(0.017)	(0.020)	(0.027)	(0.033)	(0.024)	(0.041)	(0.049)
Amerindian	0.005	−0.063**	0.068*	−0.036	0.012	−0.048	0.115*
	(0.017)	(0.020)	(0.027)	(0.032)	(0.024)	(0.040)	(0.048)
Female	0.022	−0.007	0.029	0.007	0.018	−0.011	0.040
	(0.014)	(0.016)	(0.022)	(0.026)	(0.020)	(0.033)	(0.039)
N	2740	2642	–	729	1576	–	7,687

Table A17: Treatment effect estimates in March 2011 (post-election) experiment

	Viable (7), Not-Elected			Viable (7), Elected			
College degree	no	yes	DD	no	yes	DD	DDD
Treatments	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Lower class	0.015	−0.012	0.027	0.023	−0.074*	0.097	−0.069
	(0.034)	(0.030)	(0.045)	(0.055)	(0.032)	(0.064)	(0.078)
Amerindian	−0.007	−0.049	0.042	0.024	−0.057	0.081	−0.039
	(0.030)	(0.027)	(0.040)	(0.054)	(0.033)	(0.064)	(0.076)
Female	−0.040	0.000	−0.041	−0.056	0.051*	−0.107*	0.066
	(0.027)	(0.023)	(0.035)	(0.045)	(0.025)	(0.051)	(0.062)
N	508	943	–	221	633	–	2,305

VII. Balance Tests

As described in the paper, the randomization of our experimental treatment involved blocking on party label, incumbency status, type of election (state vs. federal) and state GDP per capita. Here, we evaluate the efficacy of our randomization strategy. Tables A18-A21 report select summary statistics of observable covariates across our treatment and control groups. For each experimental treatment, we report the covariate mean for the candidates in our control group, whose email contained "no signal" of the fictitious voters' race, ethnicity, class or gender (columns 1, 5 & 9). Next, we report the sample mean for the candidates receiving each experimental treatment, as well as the p-value from the two tailed t-test of the differences in means (columns 2 & 3, 6 & 7 and 10 & 11). The last statistic reported for each treatment is variance ratio of the treatment over control, where a ratio equal to 1 indicates perfect sample balance (columns 4, 8 and 12) (Sekhon 2011). The observable covariates we report include election type (state/federal), college education, incumbency, election, gender, marital status, candidate age, our measure of intra-party competition (copartisan ratio), as well as indicators for both region and party.[20]

Scanning down the columns of p-values and variance ratios, we only rarely find evidence of differences between the control and treatment groups. The variance ratios almost always approximate 1, which we would expect if covariate balance was achieved. In the instances where the variance ratio departs 1, or the p-value is less than the critical value of $\alpha < .05$, it is often in the case of a specific party where there were very few partisans in the sample to begin with. All of the major parties (PSDB, PMDB and the PT, for example) are balanced across the control and treatment groups.

We also checked the randomization with respect to observables by fitting multinomial logit models, one for each design factor, to check whether assignment to factor levels were predicted by type of election, state and party dummies, a measure of intra-party competition, and candidates' age, marital status, incumbency status, education, and gender. Owing to the very large number of covariate predictors and the multinomial design for three of our four experimental conditions, we do not provide the full results here. However, the χ^2 test statistic values from LR tests of the null hypothesis the joint inclusion of these covariates does not improve model fit are 1.00, 1.00, 0.86, and 1.00, demonstrating that the block randomization procedure was successful in balancing these observables. As for the multinomial models for the second experiment (March 2011, post-election), the χ^2 p-values from LR tests are 0.96 and 0.98, which is what one would expect if treatment effects were randomly assigned.

[20] In the interest of space, we elected to report the balance statistics based on region as opposed to state (N=26 + the federal district). Our comparisons of the sample means between the treated and control groups across states did not yield any statistically significant differences. Please note that dichotomous state indicators are included in all of the analyses reported in the paper.

Table A18: October 2010 Balance Tests, first and second experimental factors

	Lower Class/Afro-Brazilian				**Amerindian**				**Gender**			
	Control Mean	Treatment Mean	T-test p value	Var Ratio	Control Mean	Treatment Mean	T-test p value	Var Ratio	Control Mean	Treatment Mean	T-test p value	Var Ratio
Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
State Candidate	0.672	0.678	0.636	0.991	0.672	0.675	0.766	0.994	0.674	0.675	0.924	0.998
Federal Candidate	0.328	0.322	0.636	0.991	0.328	0.325	0.766	0.994	0.326	0.325	0.924	0.998
College	0.549	0.540	0.463	1.000	0.549	0.532	0.192	1.010	0.547	0.534	0.206	1.000
Incumbent	0.086	0.073	0.086	0.867	0.086	0.078	0.294	0.918	0.080	0.078	0.722	0.976
Elected	0.110	0.104	0.521	0.957	0.110	0.090	0.016	0.842	0.103	0.100	0.706	0.979
Male	0.808	0.804	0.681	1.020	0.808	0.800	0.453	1.030	0.802	0.807	0.564	0.981
Married	0.404	0.391	0.347	0.999	0.404	0.426	0.093	1.020	0.402	0.412	0.318	1.010
Candidate Age	47.00	46.70	0.284	0.976	47.00	46.70	0.264	1.010	46.70	46.90	0.347	1.020
Copartisan Ratio	19.80	20.00	0.650	1.010	19.80	19.80	0.972	1.000	19.70	20.00	0.437	1.040
North	0.128	0.134	0.524	1.040	0.128	0.126	0.846	0.989	0.128	0.131	0.699	1.020
North East	0.203	0.205	0.863	1.010	0.203	0.199	0.725	0.986	0.206	0.198	0.386	0.972
South	0.146	0.151	0.571	1.030	0.146	0.169	0.018	1.130	0.158	0.152	0.440	0.969
SouthEast	0.456	0.451	0.737	0.998	0.456	0.439	0.204	0.993	0.447	0.451	0.682	1.000
PC do B	0.045	0.046	0.896	1.020	0.045	0.046	0.961	1.010	0.045	0.047	0.728	1.030
PCB	0.002	0.003	0.405	1.600	0.002	0.002	0.992	1.010	0.002	0.002	0.642	0.803
PCO	0.000	0.000	0.317	Inf	0.000	0.001	0.083	Inf	0.000	0.001	0.316	3.010
PDT	0.070	0.071	0.956	1.000	0.070	0.067	0.680	0.963	0.069	0.070	0.964	1.000
PHS	0.019	0.021	0.701	1.070	0.019	0.019	0.975	1.010	0.021	0.019	0.546	0.913
PMDB	0.057	0.056	0.820	0.977	0.057	0.055	0.726	0.965	0.056	0.056	0.968	1.000
PMN	0.032	0.031	0.941	0.989	0.032	0.030	0.789	0.962	0.031	0.031	0.977	1.000
PP	0.048	0.048	0.948	1.010	0.048	0.048	0.959	1.010	0.048	0.048	0.890	1.010
PPS	0.051	0.048	0.581	0.941	0.051	0.046	0.413	0.912	0.048	0.049	0.810	1.020
PR	0.026	0.029	0.462	1.120	0.026	0.028	0.713	1.060	0.025	0.030	0.220	1.170
PRB	0.026	0.027	0.932	1.010	0.026	0.025	0.829	0.966	0.027	0.026	0.805	0.969
PRP	0.014	0.014	0.821	0.951	0.014	0.013	0.663	0.907	0.014	0.014	0.985	1.000
PRTB	0.022	0.021	0.786	0.954	0.022	0.022	0.955	0.990	0.024	0.020	0.275	0.856
PSB	0.057	0.057	0.911	0.989	0.057	0.055	0.727	0.965	0.057	0.057	0.994	0.999
PSC	0.038	0.038	0.998	1.000	0.038	0.038	0.964	1.010	0.037	0.039	0.586	1.060
PSDB	0.067	0.066	0.918	0.990	0.067	0.069	0.785	1.030	0.069	0.066	0.544	0.955
PSDC	0.010	0.013	0.314	1.280	0.010	0.012	0.505	1.180	0.011	0.012	0.746	1.070
PSL	0.024	0.025	0.794	1.040	0.024	0.024	0.902	1.020	0.024	0.025	0.705	1.050
PSOL	0.038	0.038	0.942	1.010	0.038	0.038	0.964	1.010	0.040	0.035	0.200	0.873
PSTU	0.006	0.006	0.865	1.060	0.006	0.005	0.601	0.829	0.006	0.005	0.676	0.888
PT	0.090	0.088	0.855	0.986	0.090	0.089	0.945	0.995	0.091	0.088	0.629	0.970
PT do B	0.022	0.021	0.783	0.952	0.022	0.023	0.693	1.070	0.023	0.021	0.672	0.941
PTB	0.056	0.056	0.905	1.010	0.056	0.055	0.858	0.982	0.057	0.054	0.563	0.952
PTC	0.027	0.027	0.936	0.988	0.027	0.032	0.289	1.170	0.026	0.031	0.163	1.190
PTN	0.023	0.020	0.521	0.893	0.023	0.023	0.956	0.991	0.021	0.023	0.637	1.070
PV	0.091	0.091	0.997	1.000	0.091	0.093	0.797	1.020	0.089	0.094	0.378	1.060

Table A19: October 2010 Balance Tests, third experimental factor

	Mobilized				Unmobilized				Undecided			
Variable	Control Mean (1)	Treatment Mean (2)	T-test p value (3)	Var Ratio (4)	Control Mean (5)	Treatment Mean (6)	T-test p value (7)	Var Ratio (8)	Control Mean (9)	Treatment Mean (10)	T-test p value (11)	Var Ratio (12)
State Candidate	.678	.666	.296	1.020	.676	.673	.850	.100	.671	.688	.128	0.971
Federal Candidate	.322	.334	.296	1.020	.324	.327	.850	.100	.329	.312	.128	0.971
College	.539	.544	.683	.999	.542	.537	.686	.100	.542	.537	.729	1.000
Incumbent	.079	.079	.999	.100	.077	.084	.329	1.080	.081	.073	.254	0.914
Elected	.103	.098	.486	.955	.099	.109	.203	1.090	.103	.097	.400	0.946
Male	.804	.803	.899	1.010	.804	.805	.938	.997	.807	.797	.329	1.040
Not Married	.407	.408	.937	.100	.406	.411	.675	.100	.407	.406	.953	1.000
Age	46.80	46.80	.914	.951	46.80	46.60	.483	.976	46.80	46.80	.972	1.060
Copartisan Ratio	19.90	19.90	.881	.980	19.80	20.00	.838	1.040	19.80	20.00	.767	0.991
Northeast	.201	.205	.724	1.010	.205	.192	.194	.953	.201	.205	.695	1.020
North	.128	.133	.586	1.030	.129	.131	.737	1.020	.129	.129	.998	0.100
Southeast	.450	.446	.792	.999	.449	.449	.993	.100	.449	.447	.864	0.999
South	.156	.153	.756	.986	.154	.157	.734	1.020	.155	.155	.974	0.999
PC do B	.046	.044	.619	.947	.045	.047	.781	1.030	.045	.048	.593	1.060
PCB	.002	.001	.346	.596	.002	.001	.352	.599	.002	.003	.460	1.500
PCO	.001	.000	.046	.000	.000	.001	.372	2.990	.000	.000	1.000	0.100
PDT	.070	.067	.662	.964	.070	.069	.940	.994	.069	.071	.730	1.030
PHS	.020	.020	.823	1.040	.020	.018	.506	.891	.020	.020	.927	1.020
PMDB	.057	.054	.602	.951	.056	.057	.762	1.030	.056	.055	.827	0.979
PMN	.030	.033	.471	1.100	.032	.027	.251	.855	.031	.031	.941	1.010
PP	.049	.046	.625	.950	.049	.047	.751	.967	.048	.050	.683	1.040
PPS	.048	.048	.989	.999	.049	.047	.751	.967	.048	.049	.860	1.020
PR	.027	.030	.487	1.110	.027	.029	.661	1.070	.028	.026	.530	0.913
PRB	.027	.025	.593	.923	.026	.027	.887	1.020	.026	.028	.641	1.070
PRP	.013	.017	.199	1.310	.014	.013	.904	.975	.015	.010	.073	0.681
PRTB	.022	.023	.677	1.070	.022	.022	.909	1.020	.021	.024	.530	1.110
PSB	.056	.059	.590	1.050	.057	.055	.788	.975	.057	.055	.807	0.977
PSC	.037	.040	.582	1.070	.038	.039	.735	1.040	.039	.035	.400	0.903
PSDB	.069	.064	.409	.931	.067	.070	.655	1.040	.067	.069	.783	1.020
PSDC	.012	.011	.925	.979	.012	.010	.577	.879	.011	.013	.399	1.220
PSL	.024	.024	.927	.986	.025	.024	.825	.967	.024	.026	.549	1.100
PSOL	.038	.039	.817	1.030	.037	.040	.510	1.080	.038	.036	.617	0.941
PSTU	.006	.004	.226	.671	.006	.006	.813	1.080	.006	.007	.580	1.200
PT	.088	.091	.735	1.030	.088	.091	.683	1.030	.090	.085	.476	0.950
PT do B	.023	.020	.449	.883	.022	.020	.588	.915	.021	.024	.502	1.120
PTB	.054	.059	.389	1.090	.055	.056	.866	1.020	.057	.052	.355	0.914
PTC	.029	.028	.866	.977	.029	.029	.983	.100	.029	.028	.791	0.964
PTN	.022	.021	.691	.937	.022	.021	.716	.942	.021	.025	.321	1.180
PV	.092	.090	.782	.981	.090	.095	.566	1.040	.092	.091	.863	0.988

Table A20: October 2010 (pre-election) Balance Tests, fourth experimental factor

	Previous Voter				Never Voted			
Variable	Control Mean (1)	Treatment Mean (2)	T-test p value (3)	Var Ratio (4)	Control Mean (5)	Treatment Mean (6)	T-test p value (7)	Var Ratio (8)
State Candidate	0.679	0.668	0.321	1.020	0.669	0.686	0.127	0.974
Federal Candidate	0.321	0.332	0.321	1.020	0.331	0.314	0.127	0.974
College	0.536	0.550	0.211	0.995	0.546	0.530	0.174	1.000
Incumbent	0.079	0.080	0.809	1.020	0.078	0.081	0.724	1.030
Email Reply	0.185	0.190	0.572	1.020	0.181	0.199	0.040	1.080
Elected	0.103	0.098	0.517	0.961	0.098	0.107	0.196	1.080
Male	0.798	0.817	0.041	0.930	0.810	0.792	0.053	1.070
Not Married	0.413	0.396	0.132	0.987	0.402	0.417	0.186	1.010
Age	46.900	46.500	0.131	0.974	46.700	46.900	0.547	1.070
Copartisan Ratio	19.700	20.200	0.284	1.050	19.800	20.100	0.475	1.020
Northeast	0.204	0.198	0.519	0.978	0.198	0.211	0.155	1.050
North	0.129	0.129	0.970	0.998	0.132	0.124	0.334	0.952
Southeast	0.441	0.465	0.040	1.010	0.453	0.440	0.245	0.994
South	0.161	0.144	0.046	0.915	0.153	0.160	0.376	1.040
PC do B	0.045	0.047	0.808	1.020	0.046	0.046	0.943	1.010
PCB	0.002	0.001	0.282	0.578	0.002	0.002	0.987	0.992
PCO	0.001	0.000	0.713	0.674	0.000	0.001	0.531	1.980
PDT	0.070	0.069	0.926	0.993	0.069	0.070	0.812	1.020
PHS	0.019	0.020	0.736	1.060	0.021	0.018	0.338	0.858
PMDB	0.057	0.053	0.399	0.927	0.055	0.057	0.777	1.030
PMN	0.030	0.033	0.575	1.070	0.031	0.031	0.915	0.987
PP	0.048	0.049	0.865	1.020	0.048	0.049	0.835	1.020
PPS	0.049	0.046	0.554	0.944	0.047	0.050	0.673	1.040
PR	0.027	0.029	0.559	1.080	0.028	0.027	0.805	0.968
PRB	0.027	0.025	0.572	0.925	0.026	0.026	0.916	0.986
PRP	0.013	0.015	0.523	1.130	0.014	0.013	0.655	0.917
PRTB	0.022	0.023	0.784	1.040	0.021	0.023	0.535	1.100
PSB	0.056	0.057	0.915	1.010	0.056	0.057	0.830	1.020
PSC	0.039	0.037	0.690	0.956	0.037	0.040	0.597	1.060
PSDB	0.067	0.068	0.882	1.010	0.067	0.068	0.903	1.010
PSDC	0.011	0.013	0.540	1.140	0.013	0.010	0.214	0.768
PSL	0.025	0.024	0.777	0.960	0.024	0.024	0.964	1.010
PSOL	0.036	0.041	0.318	1.120	0.038	0.038	0.990	1.000
PSTU	0.005	0.006	0.598	1.170	0.006	0.005	0.273	0.718
PT	0.089	0.089	0.937	0.995	0.091	0.086	0.463	0.952
PT do B	0.023	0.020	0.337	0.864	0.021	0.023	0.603	1.080
PTB	0.056	0.055	0.897	0.989	0.056	0.054	0.673	0.963
PTC	0.029	0.028	0.840	0.974	0.028	0.030	0.725	1.050
PTN	0.021	0.024	0.430	1.130	0.023	0.019	0.157	0.807
PV	0.094	0.087	0.307	0.935	0.089	0.097	0.199	1.090

Table A21: March 2011 Balance Tests, first and second experimental factors

	Lower Class/Afro-Brazilian				**Amerindian**				**Gender**			
	Control Mean	Treatment Mean	T-test p value	Var Ratio	Control Mean	Treatment Mean	T-test p value	Var Ratio	Control Mean	Treatment Mean	T-test p value	Var Ratio
Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
State Candidate	0.676	0.678	0.875	1.000	0.671	0.678	0.596	1.010	0.673	0.677	0.722	1.010
Federal Candidate	0.324	0.322	0.875	1.000	0.329	0.322	0.596	1.010	0.327	0.323	0.722	1.010
College	0.538	0.535	0.836	0.999	0.548	0.535	0.359	0.996	0.534	0.547	0.200	1.000
Incumbent	0.079	0.086	0.332	0.925	0.072	0.086	0.052	0.851	0.076	0.083	0.223	0.921
Elected	0.103	0.101	0.762	1.020	0.100	0.101	0.913	0.992	0.099	0.104	0.516	0.963
Male	0.813	0.797	0.117	0.938	0.803	0.797	0.550	0.976	0.806	0.803	0.712	0.988
Not Married	0.386	0.418	0.013	0.974	0.416	0.418	0.867	0.999	0.018	0.016	0.471	1.120
Candidate Age	46.70	47.10	0.178	0.956	46.60	47.10	0.115	0.987	46.50	47.10	0.017	0.989
Copartisan Ratio	20.00	20.10	0.790	0.994	19.50	20.10	0.221	0.966	19.90	19.80	0.900	1.030
North	0.131	0.130	0.928	1.010	0.126	0.130	0.655	0.974	0.101	0.106	0.471	0.960
North East	0.203	0.194	0.413	1.030	0.210	0.194	0.135	1.060	0.416	0.398	0.081	1.010
South	0.155	0.156	0.897	0.993	0.154	0.156	0.796	0.987	0.262	0.247	0.116	1.040
South East	0.447	0.457	0.482	0.996	0.442	0.457	0.264	0.994	0.035	0.028	0.086	1.220
PC do B	0.049	0.041	0.154	1.180	0.048	0.041	0.196	1.170	0.046	0.046	0.933	0.992
PCB	0.003	0.001	0.237	2.030	0.002	0.001	0.511	1.520	0.002	0.002	0.609	0.786
PCO	0.000	0.001	0.573	0.509	0.000	0.001	0.572	0.508	0.000	0.001	0.311	0.327
PDT	0.067	0.069	0.801	0.977	0.073	0.069	0.527	1.060	0.069	0.070	0.881	0.989
PHS	0.019	0.018	0.776	1.060	0.021	0.018	0.396	1.170	0.021	0.019	0.508	1.110
PMDB	0.050	0.056	0.300	0.896	0.061	0.056	0.408	1.090	0.060	0.052	0.134	1.130
PMN	0.035	0.026	0.041	1.350	0.032	0.026	0.196	1.210	0.030	0.032	0.512	0.926
PP	0.050	0.045	0.339	1.110	0.050	0.045	0.375	1.110	0.053	0.044	0.052	1.200
PPS	0.048	0.046	0.838	1.020	0.050	0.046	0.488	1.080	0.049	0.048	0.831	1.020
PR	0.028	0.026	0.549	1.100	0.029	0.026	0.501	1.110	0.027	0.028	0.681	0.950
PRB	0.029	0.028	0.788	1.040	0.021	0.028	0.071	0.742	0.027	0.025	0.497	1.090
PRP	0.016	0.013	0.339	1.230	0.011	0.013	0.363	0.804	0.013	0.014	0.633	0.916
PRTB	0.022	0.023	0.863	0.971	0.021	0.023	0.583	0.909	0.022	0.022	0.839	0.972
PSB	0.060	0.054	0.309	1.110	0.055	0.054	0.886	1.020	0.055	0.058	0.526	0.948
PSC	0.041	0.038	0.499	1.090	0.035	0.038	0.652	0.942	0.038	0.038	0.910	1.010
PSDB	0.065	0.075	0.150	0.879	0.061	0.075	0.040	0.829	0.064	0.071	0.188	0.906
PSDC	0.011	0.012	0.855	0.956	0.012	0.012	0.948	1.020	0.012	0.011	0.408	1.180
PSL	0.023	0.025	0.549	0.905	0.025	0.025	0.923	1.020	0.023	0.026	0.292	0.868
PSOL	0.035	0.039	0.432	0.903	0.039	0.039	0.877	0.981	0.036	0.039	0.494	0.930
PSTU	0.008	0.004	0.078	1.860	0.005	0.004	0.536	1.270	0.008	0.004	0.008	2.210
PT	0.092	0.094	0.764	0.978	0.082	0.094	0.109	0.883	0.087	0.092	0.422	0.951
PT do B	0.019	0.023	0.402	0.861	0.024	0.023	0.788	1.050	0.024	0.019	0.130	1.240
PTB	0.051	0.061	0.089	0.839	0.055	0.061	0.295	0.900	0.058	0.053	0.376	1.080
PTC	0.028	0.032	0.407	0.885	0.027	0.032	0.279	0.851	0.027	0.030	0.388	0.899
PTN	0.024	0.021	0.585	1.100	0.020	0.021	0.776	0.950	0.021	0.023	0.670	0.941
PV	0.084	0.093	0.247	0.915	0.097	0.093	0.641	1.030	0.092	0.090	0.740	1.020

VIII. Full experimental design results

A. *Treatment effects of voters' stated vote intention or past voting behavior, October (pre-election) experiment*

As mentioned in the manuscript, our field experiment included two additional design factors that conveyed information about the voter's intended vote choice and past voting behavior. Taking inspiration from the research on electoral strategies and vote buying, we hypothesized that candidates may face incentives to discriminate in favor of supporters (core voters) or undecided (swing) voters (Cox and McCubbins 1986; Lindbeck and Weibull 1987). Our experimental treatment included 4 factor levels: a voter who has decided to support the candidate and will turn out (mobilized supporter), a voter who has decided to support the candidate but might not turn out (unmobilized supporter), an undecided voter, and no signal about the voter's vote and turnout intentions (no signal). The final design factor varies a voter's past voting behavior, to account for the possibility that candidates allocate resources to voters who have supported their parties in the past (Nichter 2008: 27). There are 3 factor levels: a voter who has supported the candidate's party in the past (past supporter), a voter who has never supported the candidate's party (never supporter), and no signal about the voter's past voting behavior (no signal). Please see Table 2 in the main text, or Table 2A (above) for more details.

Results from the fully specified model that includes all four experimental treatments (class & race, gender, vote intention and previous voting) are shown in Table A22, below. The results for the first two design factors (class/race and gender) are identical to those reported in the main body of the manuscript (Table 3). As for the second two design factors, the coefficients represent the difference from moving to the "no signal" treatment to the level specified in Table A22. The generally small and statistically insignificant impact estimates in the lower half of Table A22 suggest that candidates were largely unaffected by voters' stated vote intentions or past voting behavior. We cannot reject the null hypothesis of no effect when we jointly test the significance of these two design factors.[21] This is evidence that our results are not driven by ideological or partisan motivations.

[21] LR test: $\chi^2_{10} = 10.32$, $p = 0.41$.

Table A22: 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)
Mobilized supporter	0.038	0.012	0.026	0.009	−0.024	0.033	−0.007
	(0.020)	(0.025)	(0.032)	(0.032)	(0.026)	(0.042)	(0.052)
Unmobilized supporter	0.026	0.013	0.012	0.073*	−0.037	0.110*	−0.097
	(0.020)	(0.025)	(0.032)	(0.035)	(0.025)	(0.044)	(0.054)
Undecided voter	0.032	−0.008	0.041	0.026	−0.027	0.053	−0.012
	(0.021)	(0.024)	(0.031)	(0.031)	(0.027)	(0.041)	(0.052)
Past supporter	−0.009	0.030	−0.039	0.051	0.027	0.024	−0.062
	(0.018)	(0.021)	(0.028)	(0.029)	(0.022)	(0.037)	(0.045)
Never supporter	0.032	0.029	0.003	0.021	0.038	−0.016	0.020
	(0.018)	(0.021)	(0.028)	(0.028)	(0.022)	(0.036)	(0.045)
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. 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 0.05 level. ** denotes statistical significance at 0.01 level.

IX. Pre-electoral Experiment, Partitioned on Election

To maximize transparency and comparability of our results across the two experiments, we report here the results of the pre-electoral experiment for *Viable* candidates, partitioned on their eventual election. Though we lack the statistical precision to make meaningful differentiations across subgroups here, the direction in coefficients presented below stand in stark contrast to these groups' responsiveness in the post-election experiment. Though college educated *Viable* candidates who would go onto elected office were generally equally (if not more) responsive to putative voters from lower-class background, they were systematically less responsive to them once in office.

Table A23: Treatment effect estimates in October 2010 (pre-election) experiment, 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	0.004	0.039	−0.034	−0.032	0.028	−0.060	0.025
	(0.032)	(0.026)	(0.041)	(0.055)	(0.033)	(0.064)	(0.076)
Amerindian	−0.053	0.000	−0.053	−0.001	0.042	−0.043	−0.010
	(0.029)	(0.025)	(0.039)	(0.060)	(0.037)	(0.069)	(0.079)
Female	0.051*	−0.001	0.052	−0.075	0.057*	−0.132*	0.184**
	(0.025)	(0.021)	(0.033)	(0.048)	(0.029)	(0.056)	(0.065)
Mobilized supporter	−0.016	−0.029	0.012	0.096	−0.012	0.108	−0.096
	(0.033)	(0.031)	(0.045)	(0.065)	(0.042)	(0.079)	(0.090)
Unmobilized supporter	0.054	−0.030	0.084	0.110	−0.054	0.163*	−0.080
	(0.040)	(0.029)	(0.049)	(0.060)	(0.040)	(0.072)	(0.087)
Undecided voter	0.004	−0.032	0.037	0.083	−0.023	0.105	−0.069
	(0.033)	(0.031)	(0.045)	(0.060)	(0.043)	(0.075)	(0.087)
Past supporter	0.043	0.027	0.016	0.020	0.029	−0.009	0.026
	(0.031)	(0.025)	(0.039)	(0.062)	(0.035)	(0.071)	(0.082)
Never supporter	0.017	0.029	−0.012	0.013	0.050	−0.037	0.025
	(0.030)	(0.026)	(0.040)	(0.057)	(0.035)	(0.067)	(0.078)
	N	711	1251	221	633		

N = 7,687. The table displays untransformed estimates (first differences), differences in treatment effect estimates (second differences, DD), and differences in differences in treatment effect estimates (third differences, DDD) from a ordinary least squares 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 0.05 level. ** denotes statistical significance at 0.01 level.

X. Robustness Checks

A. Robustness to controls

Here we report another piece of evidence that is consistent with randomly assigned treatment, demonstrating that our results are impervious to the inclusion or exclusion of our covariate controls. Table A24 reports the heterogeneous treatment effects for candidates with and without a college degree, which we have partitioned on the basis of their electoral *Viability*. We summarize here the results of five different model specifications, each of which includes different covariate controls, which are listed in the first column. The results shown at the top of the table correspond directly to the results shown in the main body of the paper, in Table 3, in which we control for state and party fixed effects, as well as intra-party competition, and candidates' age, marital status, gender, and incumbency. The subsequent models exclude some or all of these controls. We find no appreciable difference across models in terms of substantive effects or statistical significance, which would also suggest successful randomization. Table A25 displays similar information, corresponding to the March 2011 (post-election) experiment, and a similar conclusion is reached.

Table A24: October 2010 (pre-election) experiment, Robustness to controls

		Not Viable		Viable	
		No College	College	No College	College
Controls	Treatments	(1)	(2)	(3)	(4)
All controls	Lower class	−0.012	−0.056**	−0.007	0.040
		(0.018)	(0.022)	(0.030)	(0.022)
	Amerindian	0.011	−0.076**	−0.047	0.016
		(0.018)	(0.021)	(0.029)	(0.022)
	Female	0.018	−0.011	0.024	0.020
		(0.015)	(0.017)	(0.024)	(0.018)
No state	Lower class	−0.014	−0.057*	−0.009	0.040
		(0.019)	(0.022)	(0.028)	(0.021)
	Amerindian	0.011	−0.076**	−0.041	0.014
		(0.019)	(0.022)	(0.028)	(0.021)
	Female	0.017	−0.014	0.022	0.021
		(0.015)	(0.018)	(0.022)	(0.017)
No party	Lower class	−0.011	−0.057**	−0.009	0.037
		(0.018)	(0.022)	(0.030)	(0.022)
	Amerindian	0.013	−0.078**	−0.048*	0.015
		(0.018)	(0.021)	(0.028)	(0.022)
	Female	0.016	−0.011	0.021	0.019
		(0.015)	(0.017)	(0.023)	(0.018)
No individual	Lower class	−0.010	−0.057**	−0.006	0.041
		(0.018)	(0.021)	(0.030)	(0.023)
	Amerindian	0.012	−0.077**	−0.047	0.017
		(0.018)	(0.021)	(0.029)	(0.022)
	Female	0.017	−0.010	0.025	0.019
		(0.015)	(0.017)	(0.024)	(0.018)
No Controls	Lower class	−0.012	−0.062**	−0.005	0.040*
		(0.019)	(0.023)	(0.027)	(0.020)
	Amerindian	0.011	−0.081**	−0.037	0.014
		(0.019)	(0.022)	(0.026)	(0.020)
	Female	0.016	−0.011	0.019	0.018
		(0.016)	(0.018)	(0.021)	(0.017)
	N	2537	2334	932	1884

N = 7,687. The table displays simulated treatment effect estimates (first differences) from a probit model containing three-way interactions between the treatment indicators, an indicator for candidate viability, and an indicator for college degree across five different models with variable controls included. The different models additionally contain 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 0.05 level. ** denotes statistical significance at 0.01 level.

Table A25: March 2011 (post-election) experiment, Robustness to controls

		Viable, Not-Elected		Viable, Elected	
		No College	College	No College	College
Controls	Treatments	(1)	(2)	(3)	(4)
All controls	Lower class	−0.003	−0.005	0.019	−0.075*
		(0.030)	(0.026)	(0.056)	(0.034)
	Amerindian	−0.037	−0.048*	0.024	−0.061
		(0.026)	(0.024)	(0.056)	(0.034)
	Female	−0.013	0.014	−0.062	0.057*
		(0.023)	(0.020)	(0.045)	(0.027)
No state	Lower class	−0.012	−0.005	0.020	−0.073*
		(0.028)	(0.025)	(0.055)	(0.034)
	Amerindian	−0.041	−0.045	0.029	−0.064
		(0.026)	(0.024)	(0.056)	(0.033)
	Female	−0.013	0.010	−0.061	0.052*
		(0.022)	(0.020)	(0.046)	(0.026)
No party	Lower class	−0.008	−0.006	0.008	−0.074*
		(0.030)	(0.025)	(0.055)	(0.034)
	Amerindian	−0.041	−0.046*	0.017	−0.061
		(0.027)	(0.023)	(0.056)	(0.034)
	Female	−0.011	0.014	−0.058	0.056*
		(0.023)	(0.019)	(0.046)	(0.027)
No individual	Lower class	−0.001	−0.007	0.020	−0.071*
		(0.031)	(0.027)	(0.052)	(0.031)
	Amerindian	−0.036	−0.046	0.022	−0.053
		(0.027)	(0.025)	(0.052)	(0.031)
	Female	−0.012	0.016	−0.062	0.048*
		(0.024)	(0.021)	(0.043)	(0.024)
No Controls	Lower class	−0.016	−0.008	0.019	−0.066*
		(0.030)	(0.026)	(0.050)	(0.030)
	Amerindian	−0.047	−0.043	0.020	−0.051
		(0.027)	(0.024)	(0.051)	(0.031)
	Female	−0.012	0.015	−0.055	0.042
		(0.023)	(0.020)	(0.042)	(0.023)
	N	711	1251	221	633

$N = 2{,}816$. The table displays simulated treatment effect estimates (first differences) from a probit model containing three-way interactions between the treatment indicators, an indicator for candidate viability, and an indicator for college degree across five different models with variable controls included. The different models additionally contain 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 0.05 level. ** denotes statistical significance at 0.01 level.

B. Untransformed (ordinary least squares) estimates

As second robustness check, we provide the untransformed coefficients from an ordinary least squares model to ensure our results are not driven by the functional form of the model specification. As shown in Table A26, our substantive results are identical to the inferences gleaned from the probit model.

Table A26: 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)
Low class	−0.016	−0.059**	0.042	−0.002	0.041*	−0.044	0.086
	(0.019)	(0.023)	(0.029)	(0.026)	(0.020)	(0.033)	(0.044)
Amerindian	0.012	−0.078**	0.090**	−0.034	0.015	−0.048	0.138**
	(0.019)	(0.022)	(0.029)	(0.025)	(0.020)	(0.032)	(0.043)
Female	0.020	−0.012	0.032	0.007	0.018	−0.011	0.043
	(0.015)	(0.018)	(0.024)	(0.021)	(0.016)	(0.027)	(0.036)
N	2537	2334	–	932	1884	–	–
Adj. R^2	0.038	0.025	–	0.032	0.017	–	–

N = 7,687. The table displays untransformed estimates (first differences), differences in treatment effect estimates (second differences, DD), and differences in differences in treatment effect estimates (third differences, DDD) from a ordinary least squares 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 0.05 level. ** denotes statistical significance at 0.01 level.

C. Holding other experimental factors constant (to the "no signal" treatment)

To rule out the possibility of contamination effects across the third and fourth experimental factors (which randomized past voting behavior and future vote intentions), we reestimated the models for the first (pre-electoral) experiment holding the third and fourth design factors to the "no signal" control categories. Doing so ameliorates concerns of possible treatment interactions, and it is this subset that is most closely comparable to the subjects in the post-election experiment where vote intention or past vote behavior were not manipulated. We report the results of our pre-election experiment in Table A27, below. The results do not change.

Table A27: 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	−0.011	−0.053*	0.043	−0.005	0.040	−0.045	0.088*
	(0.016)	(0.021)	(0.026)	(0.024)	(0.022)	(0.033)	(0.041)
Amerindian	0.010	−0.072**	0.082**	−0.035	0.016	−0.051	0.133**
	(0.016)	(0.020)	(0.026)	(0.022)	(0.022)	(0.031)	(0.041)
Female	0.016	−0.011	0.026	0.019	0.020	−0.001	0.027
	(0.013)	(0.016)	(0.021)	(0.019)	(0.019)	(0.026)	(0.034)
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. 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 0.05 level. *** denotes statistical significance at 0.01 level.

D. Alternative measure of incumbency

To assuage possible concerns that our self-reported measure of incumbency would overstate the population of incumbents in our sample, we reestimated our models using a measure of incumbency based on whether a candidate had been elected to office in a previous election. We merged our original candidate records with the candidate lists of all national and subnational (prefects and mayors) elections from 1998 to 2010. The results from our experiments using this alternative measure are shown below, in Tables A28 and A29. The results are the same as those reported in the manuscript. The only substantive difference is seen in the second (post-election) experiment, in which the magnitude of the coefficients for *Viable* candidates who were elected to office is larger than those reported in the main text.

The Tables display simulated treatment effect estimates, differences in treatment effect estimates, and differences in differences in 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. The model additionally contains state dummies, party dummies, intra-party competition, and candidates' age, marital status, gender, and incumbency. Robust standard errors are shown in parentheses.

Table A28: 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	−0.014	−0.056*	0.042	−0.008	0.040	−0.047	0.089
	(0.018)	(0.022)	(0.029)	(0.030)	(0.022)	(0.038)	(0.047)
Amerindian	0.011	−0.076**	0.087**	−0.048	0.016	−0.064	0.151**
	(0.018)	(0.021)	(0.028)	(0.029)	(0.022)	(0.036)	(0.045)
Female	0.016	−0.012	0.029	0.024	0.020	0.004	0.025
	(0.015)	(0.017)	(0.023)	(0.024)	(0.018)	(0.030)	(0.038)

Table A29: Treatment effect estimates in March 2011 (post-election) experiment

	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	−0.004	0.001	0.017	−0.080*	0.097	−0.095
	(0.029)	(0.025)	(0.038)	(0.062)	(0.039)	(0.074)	(0.083)
Amerindian	−0.035	−0.046*	0.011	0.029	−0.067	0.096	−0.085
	(0.026)	(0.023)	(0.034)	(0.064)	(0.039)	(0.075)	(0.082)
Female	−0.012	0.014	−0.026	−0.065	0.057	−0.123*	0.097
	(0.022)	(0.019)	(0.029)	(0.051)	(0.031)	(0.061)	(0.067)

E. Analysis Excluding the PPS

Tables A30 and A31 show the results of our analysis when the small party who expressed skepticism of our email is omitted. The results are unchanged.

Table A30: 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)
tab3a							
Lower class	−0.012	−0.065**	0.053	−0.011	0.039	−0.050	0.104*
	(0.018)	(0.022)	(0.029)	(0.031)	(0.023)	(0.038)	(0.048)
Amerindian	0.016	−0.081**	0.097**	−0.043	0.019	−0.062	0.159**
	(0.019)	(0.022)	(0.029)	(0.029)	(0.023)	(0.037)	(0.047)
Female	0.015	−0.016	0.031	0.022	0.022	0.000	0.031
	(0.015)	(0.018)	(0.023)	(0.024)	(0.019)	(0.031)	(0.039)

Table A31: 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)
tab5a							
Lower class	−0.008	0.003	−0.011	0.023	−0.083*	0.106	−0.117
	(0.030)	(0.026)	(0.040)	(0.056)	(0.036)	(0.066)	(0.077)
Amerindian	−0.037	−0.046	0.009	0.027	−0.070*	0.097	−0.087
	(0.027)	(0.024)	(0.036)	(0.056)	(0.035)	(0.066)	(0.076)
Female	−0.015	0.017	−0.032	−0.067	0.052	−0.119*	0.087
	(0.023)	(0.020)	(0.031)	(0.045)	(0.027)	(0.053)	(0.061)

XI. Response Rates: Subgroups and Comparable Studies

We report here the baseline response rates for each of the subgroups of theoretical interest in our study. In the lower panel of the Table A32, we also report the response rates for comparable experimental studies of elites' behavior. We acknowledge our response rate is substantially lower than similar studies of U.S. politicians, but on par with comparable studies conducted in Brazil or other developing countries, such as South Africa. Though we cannot definitively explain this divergence, we speculate that the lower response rate is due to lower professionalism amongst Brazilian legislative candidates and deputies (Samuels 2003), and because our policy-related inquiry required some substantive response.

Table A32: Response Rates in Field Experiments on Elite Behavior

Author	**Sample**	**Baseline Response Rate**
Anonymous	7,687 Brazilian state & federal legislative candidates	19.6%
	2,816 *Viable* candidates	14.2%
	4,871 *Non-Viable* candidates	22.8%
	4,218 *College* educated candidates	21.4%
	3,469 *Non-College* educated candidates	17.5%
Anonymous	2,816 formerly viable candidates	14.2%
	854 *Elected* candidates	14.2%
	1,962 *Non-Elected* candidates	14.0%
	4,218 *College* educated candidates	15.3%
	3,469 *Non-College* educated candidates	12.0%
Broockman (2013)	5,593 U.S. state legislators	42.3%
Butler and Broockman (2011)	4,859 U.S. state legislators	56.5%
Butler, Karpowitz and Pope (2012)	1,036 letters sent to 489 different legislative offices	51%-28%
Distelhorst and Hou (2016)	1,225 China's local political officials	43.4%
McClendon (2016)	1,229 South African city councilors	20.9%
Spada and Guimarães (2013)	1,008 Brazilian municipal level candidates	28.7%
White, Nathan and Faller (2015)	Over 7,000 county and municipal election officials	71%

XII. Examples of Candidates' Responses

In what follows, we provide the English translation of six candidates' responses to our emails from the pre-election experiment. The emails provided were selected at random, we drew one from each of our experimental subgroups (male no treatment, male lower class, male indigenous, female no treatment, female lower class, female indigenous). Some emails have been lightly edited for the purposes of clarification. Please recall that in order to be included in our study, the email had to contain at minimum some indication of a personalized response—mechanized or automated replies, such as rote additions to the candidates' mailing lists or Facebook network were excluded from the analysis. The following gives an example of the sorts of emails we received.

Figure A2: Random Sample of Candidates' Responses, English Translations

Response to Pedro Aparecido, no treatment male name:

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Subject: RE: A question about unemployment from one of your supporters

Good afternoon.

Pedro, I'm doing well, thank you. How about you?

With regard to the 1st job, we ought to establish technical courses that will lead to
automatic hiring by the companies upon graduation.

With regard to people with low qualifications, we should give a minimum salary
to cover their basic needs and allow them to take job skills training, offering tax
exemptions to employers who hire them.

Regarding people over 40, 50 years old, we should propose a law directed to companies
so that they hire as part of their staff, besides quality young employees, a percentage
of older people too.

Bringing resources will be my biggest goal when elected. My area of action will be:
environment, more jobs, safety, fighting for Constitutional Amendment Proposal 300,
full-time education, health, construction of more infrastructure for first aid facilities
and several medical specialties centers.

I propose a project to include eye exams for elementary school children in order to detect
visual problems that prevent good school performance and I'm also very sensitive about
animals dumped on public roads. I already have been working on this problem.

Hugs,
```

Response to Macmillian Barbosa, lower socioeconomic class male name:

Subject: RE: A question about unemployment from one of your supporters

Dear Macmaillan Barbosa, I'm well and resolute about our endeavor. Thank you for the support and the confidence vote. This will be a vote for structural change and, above all, ethical behavior in Roraima politics.

I answer your question based on the economy of the state, its salaries, and on my own academic training as a public manager, as well as experience in social struggles. Therefore, I support:

1. The fight for investment of the state of Roraima in the technical training directed to economic development activities, like fish farming;
2. Young people and graduates from universities and colleges, with the help from the Promotion Agency of the state of Roraima- AFER, allocating 1% to support collective and individual projects - for example: for physiotherapists to set up clinics; accountants their offices; agronomists and so on;
3. To invest in autonomous projects to generate jobs and income. I refer here to the rubber workers, seamstresses, ice cream vendors, cooks, hairdressers, street vendors, manicures. Note: To advance these projects we will fight for micro credit as an alternative to official banks. The rules should focus on the creation of jobs and income.
4. To defend civil service exams for all areas of the public administration, with hiring of all approved. If I got elected, I'll help to create a committee to carry out these projects.

I hope that my response has helped you to continue to support my candidacy.

Thank you

Response to Cauré Guajajara, indigenous male name:

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Subject: RE: A question about unemployment from an undecided voter

Good night Caure!

I'm happy to receive your message and to see your preoccupation with public policies.

Unemployment must be tackled in a comprehensive way:
--Improving education, ending this policy of automatic grade promotion and implementing
full-time elementary education.
--Expanding technical courses to create better jobs.
--Supporting public policies for motivating family farming and small producers.
--Giving incentives to small and medium entrepreneurs to create more jobs.
--Broadening access to higher education.

Regards,
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Response to Sônia Texeira, no treatment female name:

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Subject: RE: A question about unemployment from one of your supporters

Mrs. Sonia,

Good evening,

Yes, I have a plan. As part of my proposals, one of them, is the creation of commercial
guards, which will generate around 30,000 jobs, with a course for the profession.
Moreover, by reducing poverty, companies will be able to hire more employees.

Together with the next governor, I will provide benefits to street vendors, manicures,
shoemakers, and informal sector workers, to generate new jobs. To incentivize outsourced
supermarket grocery packers' companies for the whole capital district.

I'll be on the streets with the population and community leaders listening to the proposals
from those communities regarding the generation of new jobs.

Thanks for the support, contact me if you want any clarification of these proposals, I'll
be happy to respond.

Thanking you,
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Response to Lyndiane Ferreira, lower socioeconomic class female name:

Subject: RE: A question about unemployment from one of your supporters

My friend Lyndiane,

Thank you very much for your interest.

As a state deputy, I'll push the State Government to give more tax incentives so that companies settle in the state and can create jobs.

In addition, I'm going to develop projects for entrepreneurs from all sectors to invest more in the hiring of young people, with the first job, and also for people between the ages of 40 and 60 who are losing space in the market but who're still productive.

Kind Regards.

Response to Anajá Obeima, indigenous female name:

Subject: RE: A question about unemployment

Hi Anaja Obeima, how are you?

Thank you so much for stopping by and having seen my program. First, every society needs to prepare its citizen to meet its demands, when I speak in helping citizens, I want to say to train them, to qualify the workforce, this is called investment in intellectual capital. And this is done with a good education. Offering a quality school with technical and higher education courses. We miss it in our state, because it's bankrupt at the moment. But while that does not happen, we must believe in the poor people, those who have never had opportunities - the mechanic, the seamstress, the baker, the potter, the ice-cream maker, in sum, all self-employed workers and vendors who do not have access to a line of credit to be able to have a working capital to start their own businesses. All of these that I have mentioned are humble, unemployed people who need opportunities. And this can be a solution for reducing unemployment in our state.

Thanks for the attention and I'm sorry if it did not meet your expectation.

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