Vote for Pastor Paulo: Religious Ballot Names as Heuristics in Brazil*

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Abstract

When religious leaders run for office, and identify themselves to voters as such, what is the expected effect on electoral results? I address this question through a study of Brazilian clergy—primarily evangelical Christian pastors—running for city council in 2000–2008. Most such candidates use a religious title such as ‘Pastor’ on the ballot, but others do not. Using matching, I compare groups of clergy candidates who differ in their use of religious ballot names but are otherwise similar. On average, failing to advertise one’s religious leadership leads to an 18% reduction in vote share and 28% drop in the probability of being elected. Similar results are obtained when using other techniques, such as difference-in-differences. The estimated effect does not vary with evangelical population, suggesting that it is driven not by believers voting for fellow believers, but rather by positive stereotypes that all Brazilians associate with religious leaders.

1 Introduction

In the mid-1980s, a number of Brazilian evangelical churches abandoned their traditional posture of “believers don’t mess with politics” for a new stance of seeking active involvement in elections and public life. In the years since, self-identified evangelical Christians have claimed 12% of the seats in Brazil’s Congress, been elected governor of states such as Rio de Janeiro, and finished third in two presidential elections. Increasingly, pastors and bishops themselves are seeking elected office, especially at the local level. In the 2000 and 2004 municipal elections, around 1000 candidates for mayor or city council listed their occupation as priest, making religious leaders twice as prevalent among candidates for local office as they are in the general population.

One potential factor in the electoral success of Brazilian evangelicals is the role of religion as a voting cue. Voters in democracies around the world are often asked to choose among relatively unfamiliar options, especially for low-profile elected positions. The problem is compounded in countries with lower levels of education, mandatory voting laws that bring apathetic citizens to the polls, or elections with an excessively large number of candidates. In such circumstances, voters often reach a decision by relying on heuristics or cues, including a candidate’s party affiliation (Conover and Feldman, 1982, 1989), gender (McDermott, 1997), race (McDermott, 1998), occupation (McDermott, 2005), religion (McDermott, 2007, 2009a), and physical attractiveness (Atkinson, Enos and Hill, 2009; Lawson et al., 2010; Lenz and Lawson, 2011). Some heuristics are conveyed via information on the ballot, while others require a minimal level of attention to the campaign, but all are more readily available to the average voter than the details of a candidate’s issue positions or policy proposals.

This paper looks at the effect of a relatively unique heuristic—a candidate’s self-identification as a religious leader, typically an evangelical Christian pastor—on the results of city council elections.

1Brazilian Protestants are generally referred to as evangélicos, regardless of denomination, whereas the equivalent English term denotes specific elements of theology and religious practice, including an emphasis on the conversion experience, missionary efforts, and a literal interpretation of scripture (Freston, 2008). Most politically-active Brazilian Protestants, however, would fit the English definition of “evangelical.” In this paper, I use the term in the same general manner as it is used in Brazil.

2This comparison is based on occupation data from the 2000 census.
in Brazil. Brazilian electoral law allows candidates great leeway to list nicknames on the ballot, and many take advantage of this opportunity; a clergyman named Paulo Rodrigues da Silva might present himself to voters as “Pastor Paulo.” Using matching, I compare groups of clergy candidates for city council in 2000, 2004, and 2008 who differ in their use of religious ballot names but are similar in other respects that might affect electoral results. On average, failing to advertise one’s religious leadership leads to an 18% reduction in vote share and 28% drop in the probability of being elected. Alternative estimation techniques that seek to account for unobserved confounders, including a difference-in-differences design, yield similar results.

The effect of religious ballot names as a heuristic could potentially operate through two distinct mechanisms: group associations between evangelical Christians and their pastors, and positive stereotypes that all Brazilians associate with religious leaders. While inferences about individual behavior based on aggregate data are necessarily somewhat tentative, evidence points toward stereotypes rather than group associations. The magnitude of the treatment effect does not vary with the share of municipal residents who are evangelical Christians, suggesting that the penalty for not using a religious ballot name depends on the behavior of all voters, not just fellow believers. A planned extension to this study—a survey experiment to be conducted during Brazil’s upcoming 2012 municipal elections—will seek to probe these individual-level mechanisms, while also allowing for stronger causal inferences than are possible with the analysis of observational data.

2 Heuristics and Voting Behavior

Research on heuristics and voting behavior has focused on a variety of different cues, but those that have received the most attention are conveyed automatically to voters, independent of a candidate’s campaign strategy. Some such cues, such as party affiliation, are explicitly printed on the ballot. Others, including gender or (in some cases) ethnicity, can be readily inferred from candidates’ names. Still others, such as physical attractiveness, are invisible on the ballot (except in the few elections where candidate photos are included) but are virtually impossible for a candidate to alter
in any significant way, at least during the course of a campaign. Candidates may still choose to emphasize these characteristics to a greater or lesser extent—frequently mentioning one’s party in campaign advertisements, for example. At a baseline level, however, these cues are transmitted to voters in an automatic fashion, independent of candidate strategy.

Other heuristics reach voters, and potentially influence their decisions, only to the extent that they are emphasized during a campaign. Occupation is one such cue. In most elections, candidate occupations do not appear on the ballot; voters are familiar with them only insofar as they are covered by the media or regularly mentioned by candidates themselves. Even where occupation can be listed on the ballot, candidates typically decide how to describe themselves and may opt out entirely. Religion is a second example. Except where it can be inferred from one’s name, a candidate’s religious affiliation—not to mention religiosity—is only public knowledge to the extent that it is emphasized during the campaign. The effect of non-obvious heuristics on voting behavior is thus a political as well as a psychological question. They play a role only insofar as candidates, their opponents, and the media choose to make them an issue in voters’ minds.

Occupational and religious heuristics have received less scholarly attention than other types, but prior research has demonstrated sizable effects in a variety of different low-information contexts. Studies of religious cues, such as Catholicism or evangelicalism, have relied primarily on survey questions about hypothetical candidates, thus approximating an election where voters have little background knowledge of choices on the ballot (McDermott, 2007, 2009a). For their part, occupational cues have been shown to affect voting behavior in a variety of lower-profile races in California, including elections to the Los Angeles Junior College Board of Trustees (Mueller, 1970), Democratic and Republican Party county committees (Byrne and Pueschel, 1974), state Superior Court judgeships (Dubois, 1984), and positions such as Treasurer and Attorney General (McDermott, 2005). Outside of the United States, studies have demonstrated particularly large occupational cuing effects in open-list proportional representation elections for local councils in the German state of Baden Württemberg (Mechtel, 2011) and in Barcelona, Spain (Sajons, 2011).

3California allows candidates to list occupation on the ballot.
4Spain uses closed-list proportional representation, but Sajons (2011) conducted an exit poll asking voters how
Existing studies have proposed two main mechanisms to explain the effects of occupational and religious cues: group associations and stereotypes. When informed of a candidate’s religion or occupation, voters who share this characteristic might be particularly likely to favor him or her (McDermott 2009b). The largest effects of occupational cues in Sajons’s (2011) study were for candidate’s whose occupation matched that of the respondent, regardless of whether it was high- or low-skilled. Alternatively, voters might be influenced by assumptions about a candidate’s attributes based on stereotypes attached to his or her religion or occupation (McDermott 2005, 2007, 2009a). An educator, for instance, might be thought of as more qualified for the local school board than a taxi driver, while an evangelical Christian might be considered more conservative than a non-religious candidate.

3 Religious Cues in Brazil’s City Council Elections

Contests for city council in Brazilian municipalities are a classic example of a low-information election. All legislative elections in Brazil use open-list proportional representation, and city council elections have the maximum possible district magnitude, a single municipal district. Each coalition is allowed to present twice as many candidates as there are available seats, and Brazil’s highly fragmented party system means that there will be numerous lists to choose from. In the 2000, 2004, and 2008 elections, the median municipality had 44–47 total candidates, from 4–5 coalitions, running for 9 city council seats. Making a well-informed choice among so many options would be difficult in any democracy, let alone a middle-income country with many low-education voters. Moreover, voting in Brazil is mandatory, and abstention is punished with a fine, ensuring that many politically apathetic citizens will nonetheless cast a ballot. Those with opinions about municipal politics are most likely to care about the mayor’s race, choosing among the vast number of contenders for city council only as an afterthought.

\[\text{they would have voted under open list rules, a change that was being debated at time.}\]

\[\text{5Parties not running in a coalition may present 1.5 times as many candidates as there are seats (Tribunal Superior Eleitoral 2010 324).}\]
Given the low-information nature of elections to city council, the effect of heuristics on voting behavior offers a potential explanation for religious leaders’ success at the polls. If effectively communicated to voters, a candidate’s status as pastor or priest serves as both a religious and an occupational cue. As with other heuristics of this sort, group associations and stereotypes can both play a potential role in its effects.

A first possibility is that religious Brazilians choose clergy candidates for city council because they want to put fellow believers in office. Evangelical Christians are a steadily growing share of Brazil’s population, from XX% in the XX census to 15% in 2000. Many churches have encouraged the faithful to elect like-minded politicians, a stance epitomized by the slogan “Brother Votes for Brother” adopted by the Assemblies of God in the 1980s. Evidence from presidential elections suggests that evangelicals’ voting behavior is strongly influenced by group associations. In the 2002 and 2010 elections, the third-place candidates were evangelical Christians (albeit not pastors) who spoke openly about their faith during the campaign. Evangelical voters were disproportionately likely to favor these fellow believers in both elections (Bohn, 2004; Smith, 2011). However, their voting behavior was indistinguishable from that of Catholics in 2006, when no evangelicals were running (Bohn, 2007). In lower-level contests, where clergy are more likely to run, voting might follow a similar logic, with evangelicals and other Christians favoring their religious leaders because they want to elect other members of this identity group.

A separate explanation for evangelical pastors’ electoral success concerns the characteristics or stereotypes that all Brazilians—not just fellow believers—associate with them. By definition, those in charge of a religious congregation have leadership experience, something that is often important to voters when choosing elected representatives. Voters may also be more likely to attribute positive qualities such as honesty or integrity to evangelical pastors than to candidates with other occupations. In the 2010 AmericasBarometer survey, 32% of Brazilians expressed the highest level of confidence in the evangelical church—well ahead of most political institutions, and second only to the Catholic Church (36%), whose leaders run for office much less frequently. Voters may well transfer these positive opinions of evangelicals in general to specific candidates if
made aware of their status as religious leaders.

Apart from the effects of religious leadership as a voting cue, clergy candidates might be successful at the polls because they use their ministry for political purposes, promoting their own candidacy from the pulpit and enlisting parishioners to campaign on their behalf. In Brazil’s 2010 presidential election, exhortations by pastors and priests to vote (or not vote) for particular candidates had an effect on their followers’ behavior at the polls (Smith, 2011). One could imagine that the effects of clergy as “opinion leaders” would be even larger if they were promoting their own candidacy. Religious leadership as self-promotion is conceptually distinct from religious leadership as a heuristic; the former is relevant for those that share a close personal relationship with the candidate, the latter for those who know little about him or her. Empirically, however, the two might be more difficult to entangle, especially when working at the aggregate level without data on individual voters and their social networks.

Brazilian elections give candidates a relatively unique opportunity to communicate heuristics of their choosing directly to voters via the ballot. Federal electoral law, which governs contests at all levels in Brazil, places few restrictions on the name that candidates list on the ballot. Ballot names may not be offensive, irreverent, or create confusion about the candidate’s identity, but candidates are given free reign otherwise (Tribunal Superior Eleitoral, 2010, 327–328). The use of nicknames is quite common, and titles are also widely used, especially “Doctor” and “Professor” (the latter referring to a teacher at any level of education). Candidates thus have ample opportunity to cue occupation and group membership on the ballot, as well as to engage in blatant electioneering (“Narciso the People’s Councilman”) or simply identify themselves to local voters (“Miguel from the Café”).

Religious leaders running for office typically take advantage of the opportunity to communicate

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6 The legislation reflects the informal nature of names in Brazil, including among politicians, and the lack of any general naming convention. Many Brazilians—up to and including presidents—are routinely referred to by their first names, first name and suffix (e.g., “Pedro Junior”), or a nickname. Others may be referred to only by their last names.

7 In contrast, electoral laws in the U.S. (which vary by state) heavily regulate the use of nicknames, and titles are either outlawed entirely or permitted only in unusual circumstances. See, e.g., http://election.dos.state.fl.us/candidate/pdf/Memo_Nickname_on_Ballot_5-10.pdf and http://www.lrc.ky.gov/krs/118-00/129.PDF.
their status as clergy via their ballot names. Of city council candidates in 2000, 2004, and 2008 who listed their occupation as “priest,” 74% used a religious title in their ballot name. The vast majority of these (65% of all candidates) used “Pastor”—a term that, in Brazil, refers specifically to Protestant (primarily evangelical) clergy. An additional 2% adopted the title “Father,” reflecting the relative scarcity of Catholic priests on candidate lists. The remainder used other titles, including “Bishop,” “Brother,” and “Missionary,” that are generally associated with evangelicals. Those who opted to forego the religious title almost always used their full legal name or an abbreviated version.

The nature of electoral procedures in Brazil means that the effect of ballot heuristics on voting behavior is mediated by the campaign. Since 1998, all voting in Brazil is electronic, and voters do not actually choose from a list of candidates on the ballot. Rather, they enter a candidate’s number and then are shown a confirmation screen containing that candidate’s ballot name, party, and photograph. A list of names and corresponding numbers is available for consultation upon request, though voters are encouraged to make up their minds in advance and come prepared with their choices. However, candidates’ campaign materials, television advertisements, and speeches routinely use the same name that they have selected for the ballot, since voters need to confirm their final selection before it is recorded and could easily become confused if an unfamiliar name appears. The effect of ballot names on Brazilian voters is less of a strict “ballot” effect than in other countries, but names should serve as heuristics regardless.

Based on theoretical expectations about the effect of religious heuristics on Brazilian voters, I advance the following hypotheses. Among religious leaders running for city council, failing to use a religious title on the ballot (the less common occurrence) should reduce one’s vote share and probability of being elected. If such an effect is due primarily to the positive stereotypes, such as honesty, that all Brazilians tend to associate with evangelical pastors, one would not not expect its magnitude to vary systematically across municipalities with different-sized evangelical populations. On the other hand, if group associations are driving the relationship, one would expect larger effects in towns where the electorate is more heavily evangelical, since a greater share of

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8See [http://www.youtube.com/watch?v=MpMV5_XJzGs](http://www.youtube.com/watch?v=MpMV5_XJzGs) for a demonstration of the voting procedure.
voters can potentially respond to the cue.

4 Analysis

To test these hypotheses, I use matching techniques to analyze the results of several Brazilian city council elections. Matching seeks to simulate the context of a random experiment by comparing observations that received a treatment to a subset of those that did not receive it (the control group). This subset is chosen such that the distribution of observed pretreatment covariates—those that cannot be considered a consequence of the treatment itself—is similar across groups. If the matching procedure achieves balance across groups with respect to all covariates that affect both treatment assignment and the outcome, simple mean differences offer an unbiased estimate of the average treatment effect on the treated (ATT). Unlike regression, matching is a non-parametric procedure that does not require any modeling assumption about how these observed covariates are related to the treatment and the outcome of interest.

Like regression, matching does require the assumption of no confounding, or selection on observables—namely, that no unobserved covariate affects treatment assignment as well as the outcome. The dataset used for this analysis contains a wealth of candidate- and municipal-level variables on which one can condition, lessening the severity of this assumption. Nonetheless, key covariates—in particular, propensity to use the pulpit for promoting one’s own candidacy—remain unobserved. As discussed below, I attempt to account for this possibility with alternative estimation procedures, including a difference-in-differences design. However, it is still likely that the estimates contain some degree of bias, as with nearly any analysis of non-experimental data in the social sciences.

Matching is also similar to regression in requiring the stable unit treatment value assumption (SUTVA)—namely, that the potential outcomes for any one unit are independent of any other unit’s assignment to treatment or control [Rubin 1978]. This assumption has implications both for the internal validity of the estimated effects and for their interpretation. The main threat to internal
validity would involve contamination between subjects, which might occur if multiple clergy were running against each other in the same municipality, and one’s choice of religious ballot name influenced others’ decisions. This possibility seems remote, however. Of all municipal elections from 2000–2008 with at least one clergy candidate, 76% had exactly one, and 91% had one or two. Moreover, candidate lists are published only after the registration deadline has passed, so no candidate can take this information into account when filing his or her own papers. SUTVA also means that, while our estimates give us the average effect of any one candidate’s choice of ballot name, they cannot tell us what would have happened if every religious leader running for office had acted similarly.

Data for the analysis are drawn two publicly-available sources, Brazil’s Superior Electoral Tribunal (TSE) and the Brazilian Institute of Geography and Statistics (IBGE). I began with the TSE’s results from the city council elections of 2000, 2004, and 2008, along with demographic information on candidates such as their year of birth, education level, and occupation. To augment the candidate-level database, I merged in municipal-level demographic and political data from IBGE, such as percent evangelical, Human Development Index, and vote share for major parties in prior elections.

To identify religious leaders running for office, I reduced the dataset to those candidates who recorded their occupation as “priest or member of a religious order or sect.” This category does not exhaust the list of clergy candidates; part-time pastors, or those with professional backgrounds in a different field, might state a different occupation. Indeed, in each election, more candidates with “pastor” in their ballot name stated their occupation as something other than “priest.” In particular, current members of the city council might list their occupation as “Municipal Public Servant,” so this sample likely undercounts incumbents. However, only for self-declared clergy can we be sure that candidates who did not adopt a religious ballot name could potentially have

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9These data were downloaded from the TSE’s web site, [http://www.tse.gov.br](http://www.tse.gov.br). The files exclude candidates who were legally registered but received zero votes. For 2008, some data were also obtained from [http://eleicoes.uol.com.br/2008/candidatos](http://eleicoes.uol.com.br/2008/candidatos). I also use results of the 1996 election to score variables such as incumbency and prior vote share for candidates in the 2000 election. The prevalence of missing occupation data for 1996 candidates prevented me from including them in the analysis.
done so. The final dataset contains 2321 clergy candidates for city council in the 2000, 2004, and 2008 elections—1729 of which adopted a religious ballot name, and 592 of which did not.

Among clergy candidates, the treatment group is defined as those who did not adopt a religious ballot name. Failing to advertise one’s status as a religious leader is the less common occurrence, and matching tends to achieve better covariate balance when the treatment group is much smaller than the control group, since there are many control observations to choose from. Religious ballot names include “Pastor,” “Father,” and a variety of lesser-used identifiers. As noted above, the vast majority of these candidates appear to be Protestant, but I include those with Catholic ballot names (and even one “Rabbi”) because the treatment group—which cannot be broken down by denomination, due to the lack of ballot names—is also likely to contain some non-Protestant religious leaders. I manually inspected the ballot names of the treatment group to ensure that they contained no religious words other than those that are part of the candidate’s legal name (e.g., “Evangelista”—literally, Evangelist—which is a common surname).

It is important to specify what types of causal inferences can and cannot be made as a result of this matching procedure. Subject to the assumptions outlined above, matching identifies the average treatment effect on the treated. In this instance, therefore, I am looking at candidates who did not adopt a religious ballot name and estimating the average effect of this decision on their electoral results. Thus, I can consider the counterfactual of how one of these candidates would have fared if he or she had run as “Pastor” or used some other religious title. I cannot, however, make direct inferences about respondents who did use a religious ballot name because they constitute a larger and more varied group than those who did not.

To pair candidates from the control group to those in the treatment group, I match on a rich set of covariates that might have influenced their choice of religious ballot name as well as their electoral results. At the municipal level, these include geographical location as well as socioeconomic, religious, and political variables. Specifically, I match on latitude, longitude, and dummy variables for the large states of Minas Gerais and São Paulo; municipal-level Gini coefficient, Human Development Index (HDI), and GDP per capita; the percentage of residents who describe them-
selves as evangelical, and of any Christian denomination; and vote share for the major center-right presidential candidate (from the PSDB) in 1998. At the individual level, I condition on a series of political covariates, including incumbency status, political party, election year, self-declared campaign spending limit, vote share of the candidate’s current party in the last election, an indicator for whether the candidate ran in the last election, and for re-runners, personal vote share in the last election. Finally, I match on a set of individual socio-economic characteristics, including year of birth, education, sex, an indicator for single males (which should correlate with being a Catholic priest), and reported assets. In addition, limiting the dataset to self-identified clergy ensures that treatment and control groups are exactly matched on occupation.

The nonparametric matching procedure I use is Genetic Matching, which algorithmically maximizes balance across treatment and control groups on a given set of covariates. In addition to matching directly on the covariates listed above, I included the linear predictors from a propensity-score model (a logistic regression of treatment assignment on all of the covariates), which tends to improve the performance of the Genetic Matching algorithm. Matching was one-to-one, with replacement. The 592 treatment observations were matched to 449 control observations; 338 controls were used once, 84 were used twice, 21 were used three times, and 6 were used four times.

Covariate balance is substantially improved as a result of the matching procedure, as shown in Figure 1. For the full sample of clergy candidates, the dots indicate the minimum p-value from a t-test of difference in means (two-sample before matching, paired after matching) and, for non-binary variables, a bootstrapped Kolmogorov-Smirnov (KS) test of equality of distributions. Before matching, 14 out of 32 covariates generated p-values of less than .1, and several were less than .001. After matching, the lowest p-values were .098 (year of birth) and .091 (male).

The relationship between treatment and control groups after matching shows that clergy candidates who do not use a religious ballot name experience a significant drop in their vote share

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10 Parties used in matching are those with more than 5% of clergy candidates in all three years combined, or more than 10% in any one year.
11 Self-declared spending limit was available only for 2004, and total value of reported assets only for 2008.
12 This is the balance metric maximized by the Genetic Matching algorithm.
Figure 1: Balance Statistics for Federal Deputies
and probability of being elected. As shown in the top panel of Table 1, failing to advertise one’s status as a religious leader lowers valid vote share by 0.22 percentage points (significant at the .001 level) and probability of victory by 0.04 (significant at the .05 level). These numbers might seem small in absolute terms, but the large number of candidates in city council elections means that the baseline values of these variables are extremely low. In the median municipality, if every voter chose randomly, each candidate should expect to receive around 2.2% of the vote and have a 0.2 probability of gaining a seat. Baseline values in the matched control group are even lower: 1.2% of the valid vote and a 0.15 probability of winning. Compared to similar candidates who did use a religious ballot name, therefore, the reduction in electoral prospects is substantial: an 18% decrease in vote share and 28% drop in the probability of victory.

| Table 1: The Effect of Not Using A Religious Ballot Name |
|---|---|---|---|
| **DV** | **ATT** | **SE** | **ATT Baseline** | **N** |
| Full Sample | | | | |
| Vote Share | -0.22*** | 0.06 | -18% | 1184 |
| Elected | -0.04* | 0.02 | -28% | 1184 |
| Valid Section Results | | | | |
| Vote Share | -0.17** | 0.06 | -15% | 1066 |
| Vote Share, Bottom 75% | -0.08* | 0.03 | -15% | 1066 |
| Vote Share, Bottom 50% | -0.04* | 0.02 | -16% | 1066 |

NOTE: Vote share is measured in percent of all valid votes. “Bottom 75%” and “Bottom 50%” give the combined vote share from sections where the candidate got the fewest votes. Standard errors are those proposed by Abadie and Imbens (2006). “Baseline” is the mean value of the dependent variable in the matched control group. *p < .05, **p < .01, ***p < .001.

4.1 Testing Individual-Level Mechanisms

I had hypothesized that ballot name effects might be due to two distinct mechanisms—group associations between pastors and evangelical voters, and positive stereotypes that all Brazilians associate with religious leaders—and that these would have different implications for how effects vary across municipalities. If clergy without religious ballot names are losing votes among fellow
believers, effects should be largest in towns with large evangelical populations. Percent evangelical population in one’s municipality does vary significantly across candidates, as shown in Figure 2. The median is 16.4%, only slightly higher than the national figure of 15.4%, but the range is from zero to 49.6%.

To test whether the effect of not using a religious ballot name varies according to a municipality’s evangelical population, I used the matched dataset to estimate a linear regression of each dependent variable on an interaction of the treatment indicator and percent evangelical. In both cases, the coefficient on the interaction term is nowhere close to statistical significance, and the sign is positive, contrary to expectations that the negative effect would increase in magnitude with larger evangelical populations. Similarly insignificant results are obtained for a treatment interac-
tion with percent Christian (Catholics, evangelicals, and others), which varies much less. Results are shown in Table 2.

**Table 2: Treatment Interactions: Percent Evangelical and Christian**

<table>
<thead>
<tr>
<th>DV</th>
<th>Vote Share</th>
<th>Elected</th>
<th>Vote Share</th>
<th>Elected</th>
</tr>
</thead>
<tbody>
<tr>
<td>Intercept</td>
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<td>0.12*</td>
<td>-1.05</td>
<td>0.02</td>
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<td></td>
<td>(0.21)</td>
<td>(0.05)</td>
<td>(1.18)</td>
<td>(0.13)</td>
</tr>
<tr>
<td>Treatment</td>
<td>-0.28</td>
<td>-0.05</td>
<td>0.38</td>
<td>0.07</td>
</tr>
<tr>
<td></td>
<td>(0.24)</td>
<td>(0.06)</td>
<td>(1.42)</td>
<td>(0.17)</td>
</tr>
<tr>
<td>Pct. Evangelical</td>
<td>-0.04**</td>
<td>0</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.01)</td>
<td>(0)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Treatment × Pct. Evangelical</td>
<td>0</td>
<td>0</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.01)</td>
<td>(0)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Pct. Christian</td>
<td></td>
<td></td>
<td>0.02†</td>
<td>0</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(0.01)</td>
<td>(0)</td>
</tr>
<tr>
<td>Treatment × Pct. Christian</td>
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<td>0</td>
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<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(0.02)</td>
<td>(0)</td>
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<tr>
<td>Weighted N</td>
<td>1184</td>
<td>1184</td>
<td>1184</td>
<td>1184</td>
</tr>
</tbody>
</table>

NOTE: Entries are weighted least squares (using matching weights) regression coefficients with estimated standard errors (clustered on municipality) in parentheses. †p < .1, *p < .05, **p < .01, ***p < .001.

Based on this analysis of aggregate data, therefore, it seems likely that the effect of religious ballot names on electoral results depends not on group associations between evangelicals (or Christians in general) and their clergy, but rather on positive stereotypes that all Brazilians associate with religious leaders. However, a more definitive assessment of these hypotheses about causal mechanisms would need to rely on individual-level data.

### 4.2 Effects on Disaggregated Electoral Results

As with any analysis of observational data, the matching results presented above rely on the assumption that no unobserved covariate influences assignment to treatment or control—i.e., the choice of a religious ballot name—and also has a direct effect on the outcome. Despite conditioning on a wide variety of observed covariates, bias due to an unobserved confounder is a distinct
possibility in this analysis. In particular, certain candidates might use their religious leadership to advance their political careers in multiple ways—electioneering from the pulpit, or enlisting church members as campaign volunteers—and also be more likely to use a religious title on the ballot. Part of the negative effect of not adopting a religious ballot name might thus be due to the fact that such candidates refrain from exploiting their status as clergy in general.

If political ambition is acting as an unobserved confounder, one would expect the estimated effect of a religious ballot name on vote share to be geographically concentrated rather than dispersed. Pastors who use their ministry for electioneering purposes are most likely to influence the vote of those who already have a close personal relationship with them, such as members of their congregation. Such individuals should tend to cluster geographically within the municipality; those who live near a church are more likely to attend it than those on the other side of town. In contrast, the effect of a religious ballot name on voting behavior should not depend on geographical proximity to a particular church or religious leader, since voters across the municipality are equally exposed to the same cue.

In order to test for political ambition as an unobserved confounder, I examine disaggregated electoral results. Registered voters in Brazil are assigned on a geographical basis to an electoral section (seção), each of which contains several hundred voters. Sections are then assigned to a polling place for each election (Tribunal Superior Eleitoral 2010, 47–48, 69). If political ambition is acting as an unobserved confounder, one would expect to find a relationship between vote share and use of a religious ballot name only in sections where clergy candidates received the most votes—that is, where they are geographically dominant. Alternatively, if religious ballot names are serving as heuristics, one should find similar effects even in those sections where clergy candidates received fewer votes.

While I was able to obtain a full set of section-level results for the 2008 election, the files from 2004 and 2000 are missing data from some states and municipalities. I have valid results for 533

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13 Ames (2001) has made a similar argument regarding the geographical concentration of votes for evangelical candidates for federal deputy.

14 In the clergy candidate dataset, the number of sections per municipality ranges from 4 to 20,301.
of the 592 treated clergy candidates (those without a religious ballot name) and a similar share of
the pre-matching controls. Proceeding with the analysis of this subset thus requires new matches.
The triangles in Figure 1 plot balance statistics before and after matching, both of which are quite
similar to covariate balance using the full sample.

Results of the section-level analysis, summarized in the bottom panel of Table 1, suggest that the
effect of not using a religious ballot name cannot be attributed to a more general reluctance to use
one’s ministry for political gain. As basis for comparison, I first calculate the effect of not using
a religious ballot name on the fully aggregated vote share for this subsample. The ATT estimate,
while slightly smaller in magnitude than in the full sample (-.17 versus -.22), is similarly signifi-
cant. I then estimate the effect on a candidate’s vote share in the 75% and 50% of sections where
he or she received the fewest votes. Both estimates are negative and significant at the .05 level.
They are smaller in magnitude than the effect on fully aggregated vote share, but one should bear
in mind that the relevant baselines are lower, since these are the sections where candidates received
less support. When expressing the ATT as a percentage of the vote share obtained by candidates in
the matched control group, the three estimates are virtually identical. For the subsample with valid
section results, failing to use a religious ballot name reduces one’s vote share by 15–16%, even in
those sections where the candidate had the weakest performance.

4.3 Difference-in-Differences Analysis

To allow for an even stronger causal inference about the effect of religious ballot names, I conduct a
difference-in-differences analysis for those clergy candidates who ran in two subsequent elections.
The difference-in-differences design is a technique for dealing with unobserved confounders that
do not vary over time (Angrist and Pischke 2008, 229). If the propensity to use one’s ministry for
political gain differs across treatment and control groups, but does not change from one election to
the next, it should serve as a confounder only when analyzing the results of a single election. As a
time-invariant fixed group effect, its influence will disappear when examining the difference in vote
share between two consecutive elections. The same is true of any other unobserved confounder
whose average values for the treatment and control groups are constant over time. Comparing
the change in performance between candidates who dropped or added a religious ballot name and
those who ran twice with similar monikers should thus provide a stronger basis for causal inference
about the effect of this heuristic.

Relatively few clergy candidates ran in two consecutive city council elections, and even fewer
changed their ballot name from one election to the next, so the difference-in-differences design
trades stronger causal inferences for reduced statistical power. There are 154 instances in which a
religious leader ran for city council in the same town in 2000 and 2004 or 2004 and 2008. Thirteen
of these candidates did not use a religious ballot name in either election; 117 did so in both. The
remaining 24 candidates changed their status from one election to the next; 8 dropped the religious
ballot name and 16 added one.

The crucial identifying assumption for the difference-in-differences design is that, in the absence
of treatment, both the treatment and control groups would exhibit “common trends” in the outcome
variable (Angrist and Pischke 2008, 230). In other words, if candidates who dropped or added a
religious ballot name had not done so, they would have experienced the same change in vote share
(on average) as those who did, in fact, run with similar names in both elections. Since candidates
choose their own ballot names, this assumption may not be realistic. For example, a candidate
might add a religious ballot name to compensate for poor campaigning skills that caused her to
lose the first election. Poor campaigning skills in the first election could also directly affect her
change in vote share between the two, perhaps because she learned from her initial mistakes and
was able to correct them. Even if she had not added a religious ballot name, learning from prior
mistakes would probably boost her expected vote share more than for the group of candidates that
experienced no change in ballot name status.

In order to make the common trends assumption more plausible, I use matching to condition on
observed pretreatment covariates prior to the difference-in-differences estimation. The treatment

\(^{15}\)One can sometimes test this assumption by examining trends during periods prior to or after the treatment was
applied. Unfortunately, only 11 candidates ran in all three elections in the same town, and none changed their ballot
name.
group for this analysis is defined as those candidates who added a religious ballot name from one election to the next. The ideal comparison group would be candidates who ran twice with no religious ballot name—that is, they are exactly matched to the treatment group in terms of their ballot names in election 1—and are also similar on other relevant covariates such as incumbency, party, and prior vote share. However, the fact that only 13 clergy candidates ran twice without a religious ballot name virtually assures that treatment and control groups would differ on a variety of other important variables. As a next-best solution, I compare name adders to candidates who ran twice without a significant change in ballot name—either using a religious moniker in both elections or in neither. The size of this group (N = 130) compared to the 16 treated candidates ensures that one can find a matched control group that is similar on the relevant set of pretreatment covariates.

I use the same matching procedure as with the other two samples and match on the same set of covariates (except for running in the previous election, which is perfectly balanced by design). Party, declared assets, spending limit, and size of the municipal electorate are measured in the second election, while prior vote share and prior party vote share refer to the first. The squares in Figure 1 plot balance statistics before and after matching. As with the other two samples, balance is greatly improved by matching; the minimum p-value goes from virtually zero beforehand to .15 afterwards. In the matched control group, 15 candidates used religious ballot names in both elections, whereas 1 used a religious ballot name in neither. Hence, ballot name in the first election is the main observable way in which the two groups differ.

Results from the difference-in-differences analysis are shown in Figure 3. The difference in trends between the two groups is immediately apparent: those who ran with similar ballot names in both elections tended to lose vote share (an average of .29 percentage points), whereas those who added a religious ballot name tended to gain (an average of .26 percentage points). The dashed

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16 One could also compare name droppers to those who used a religious ballot name in both elections. Since there are 117 candidates in the latter category, one could obtain good matches, so this comparison would provide the strongest basis for the parallel trends assumption. Unfortunately, the treatment group would contain only 8 candidates. Not only would this analysis have extremely low power, but the central limit theorem is unlikely to apply in such small samples, so t-tests would not be reliable.

17 The negative change among matched controls is largely driven by three incumbents, who lost an average of 1.12
Figure 3: The Effect of Using a Religious Ballot Name (Difference-in-Differences Design)

Name adders ($N = 16$) used a religious ballot name in 2004 or 2008, but not in the previous election. Matched controls either used a religious ballot name in both elections ($N = 15$) or in neither ($N = 1$). Abadie and Imbens (2006) standard error in parentheses.

The line illustrates the assumed trend for name adders if they had not changed ballot name status between the two elections. The difference in differences between those who added a religious ballot name and the matched control group is 0.55 percentage points, more than double the estimate percentage points, versus 0.1 percentage points for two-time challengers. Incumbents in Brazilian municipal elections are generally disadvantaged in subsequent contests (Titiunik, 2009; Brambor and Ceneviva, 2011), so this finding is not unexpected. The three incumbents who added a religious ballot name also tended to lose vote share, but only an average of 0.4 percentage points.
from the simple cross-sectional analysis. Though this estimate fails to obtain conventional levels of statistical significance—unsurprising, given the low statistical power—it would be significant at the .1 level for a one-tailed test.

5 Conclusion and Proposed Extensions

The three estimation procedures used in this paper point to similar conclusions: using a religious ballot name helps clergy candidates’ electoral performance, and not using one hurts it. While there are two plausible individual-level mechanisms that could explain this effect—group identifications between evangelical candidates and their pastors, and positive stereotypes that all Brazilians associate with religious leaders—the lack of a significant interaction between the treatment effect and evangelicals’ share of the municipal population suggests that stereotypes are principally responsible for the relationship.

Yet these conclusions are necessarily tentative. When analyzing only aggregate data, it is difficult to make strong inferences about individual-level causal mechanisms. Moreover, causal inferences regarding the aggregate-level effect depend on assumptions, like selection on observables, that may not be met in practice. The difference-in-differences design and analysis of effects on disaggregated vote share seek to deal with the most obvious potential confounders, but they rely on additional assumptions, e.g., about the geographical distribution of support that comes from politicizing one’s ministry.

To further test the role of the positive stereotypes mechanism using the existing dataset, one possibility is to compare the effect of using a religious ballot name to that of other common professional titles such as “Professor” and “Doctor.” There were 7448 city council candidates from 2000–2008 who listed their occupation as medical doctor; 74% used “Doctor” in their ballot name. Teachers ran for office even more frequently—64,348 candidates placed themselves in this category—and 25% used “Professor” in their ballot name. Effects of these ballot names on electoral performance should be primarily attributable to stereotypes.\footnote{While group associations might lead teachers and doctors to vote for their professional colleagues, neither group...}
tached to doctors and teachers will not necessarily be the same as those associated with religious leaders, it seems reasonable to expect that they would be similarly positive. Indeed, among the various occupational categories listed on the ballot in Baden Württemberg’s local elections, Mechtel (2011) found positive effects on electoral results for teachers, professors, physicians, and pastors. Hence, a positive effect on vote share for “Doctor” and “Professor” ballot names should increase confidence that the religious ballot name effect is due to stereotypes, whereas a null effect would suggest that something else is going on.

To allow for stronger causal inferences about the effect of using a religious ballot name, and also to leverage individual-level data for assessing the mechanism underlying this effect, I propose to conduct a survey experiment prior to Brazil’s October 2010 municipal elections. The survey would be conducted in the municipality of Nova Iguaçu, a working-class suburb of Rio de Janeiro. Nova Iguaçu has a relatively large population of evangelicals (29% of residents in the 2000 census), and also of those with no religion (22%), ensuring that these groups will be well represented, along with Catholics, in a random sample. Most importantly, evangelicals have been active in municipal politics. In the past three elections, there have been multiple clergy candidates for city council, two of which are current incumbents. Clergy candidates have run both with and without religious ballot names and will hopefully do so again in 2012.

The survey experiment would ask voters about actual candidates in the city council election, varying their ballot names in order to assess the causal effect of identifying oneself as a religious leader. Outcome variables would include likelihood of voting for the candidate and assessments of his or her honesty and leadership experience—the latter in order to test the hypothesis that voters associate positive stereotypes with clergy candidates. In the treatment condition, respondents would be given the candidate’s religious ballot name; in the control condition, they would be given only first and last name. By employing real-world treatments, this survey experiment should achieve an unusual degree of external validity.

In order to put treatment effects in perspective, and also to disguise the purpose of the survey, constitutes 15% of Brazil’s population, like evangelical Christians.
I plan to include additional questions that test the effects of other heuristics. The same trait evaluation and intended vote questions will be asked about candidates who are teachers or medical doctors, with their “Professor” and “Doctor” ballot names as a treatment condition. Since ballot names that cue occupation also help to simply distinguish the candidate from others, I also plan to include several candidates whose ballot names serve an identification purpose without conveying a high-status occupation (e.g., “Daniel from the Bakery”). Finally, in order to compare these ballot name cues with the most commonly studied heuristic, a separate, orthogonal treatment will also inform respondents of the candidate’s party affiliation.

Table 3: Sample Treatment and Control Names

<table>
<thead>
<tr>
<th>Category</th>
<th>Treatment Name</th>
<th>Control Name</th>
<th>Party</th>
<th>Sex</th>
<th>Incumbent</th>
</tr>
</thead>
<tbody>
<tr>
<td>Clergy</td>
<td>Bishop Marivaldo</td>
<td>Marivaldo Amorim*</td>
<td>PT do B</td>
<td>M</td>
<td>Yes</td>
</tr>
<tr>
<td></td>
<td>Pastor Nice</td>
<td>Elenice Sena</td>
<td>PRB</td>
<td>F</td>
<td>No</td>
</tr>
<tr>
<td>Doctors</td>
<td>Dr. Thiago Portela</td>
<td>Thiago Portela*</td>
<td>PPS</td>
<td>M</td>
<td>Yes</td>
</tr>
<tr>
<td></td>
<td>Dr. Katia</td>
<td>Katia Lima</td>
<td>PMDB</td>
<td>F</td>
<td>No</td>
</tr>
<tr>
<td>Teachers</td>
<td>Professor Serginho</td>
<td>Sergio Felicio</td>
<td>PDT</td>
<td>M</td>
<td>No</td>
</tr>
<tr>
<td></td>
<td>Professor Marli</td>
<td>Marli Freitas</td>
<td>PT</td>
<td>F</td>
<td>Yes</td>
</tr>
<tr>
<td>Other</td>
<td>Daniel from the Bakery</td>
<td>Daniel da Silva</td>
<td>DEM</td>
<td>M</td>
<td>Yes</td>
</tr>
<tr>
<td></td>
<td>Regina from the Market</td>
<td>Regina Nascimento</td>
<td>PSDB</td>
<td>F</td>
<td>No</td>
</tr>
</tbody>
</table>

NOTE: Occupation words in treatment names are translated into English. Incumbent refers to current status; all other data are from the 2008 election. *Used control name, not treatment name, in last election.

Table 3 contains a sample list of the types of candidates and ballot names that would be included in the survey experiment.⁴⁹ In each occupation category, I will seek to include two candidates that differ on sex and incumbency status. These variables—the former invariably cued by the candidate’s first name, the latter affecting the degree to which they are already well known—are likely to interact with the treatments. I also plan to choose candidates that maximize variation across relevant political parties.

A final planned extension involves qualitative interviews with clergy candidates about their strategy for winning votes and, in particular, their choice of ballot names. At present, the selection

⁴⁹Each of these candidates ran in the 2008 election; the final survey instrument would draw from the list of candidates in the upcoming election, to be released in July 2012.
mechanism for religious ballot names is fairly uncertain. One possibility is that certain parties or churches encourage or discourage this practice. Another is that candidates tend to use a religious ballot name when they are already well known in the community by this moniker. Different explanations for this choice are likely to have different implications for the plausibility of the selection on observables assumption; party dictates can be accounted for much more easily than political ambition. Another important question to verify is to what extent candidates actually campaign using their chosen ballot name, religious or not, and avoid using other labels. Prior to the 2012 election in Nova Iguacu, I plan to interview clergy candidates—including one incumbent who ran as “Bishop” in 2000 and dropped the religious ballot name thereafter—about these and other aspects of campaign strategy.
References


