

Voter Demobilization: Estimating the Impact in Multi-district Elections *

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revised April 2015

Abstract. The international community devotes significant resources to encouraging emerging democracies to hold free and fair elections, and monitoring whether they do so. Current statistical methods for determining whether violations of electoral norms have occurred are (with notable exceptions) limited to violations that take place within polling stations, such as ballot stuffing. We introduce a method for detecting violations of electoral norms that occur before votes have been cast. Our method is general in the sense that it does not require a natural experiment (although it can exploit one) and is robust to countrywide shifts in voter support ("swing"). The method can not only identify whether violations occurred, but can also estimate the impact they had on the results. We apply this method to allegations of fraud in a recent Canadian federal election, and estimate that illegal demobilization efforts reduced turnout by an average of 3.9% in 27 affected electoral districts. This finding is robust across several specifications and alternative identification strategies.

Keywords: Vote Suppression, Voter Demobilization, Turnout, Election Fraud

*We wish to thank Chris Auld, Annette Bergemann, Mikael Elinder, Eva Moerk, David Huntley, Kevin Milligan, Volker Nocke, Krishna Pendakur, Torsten Persson, Francesco Trebbi, Marit Rehavi, and other participants of talks at the Canadian Institute for Advanced Research, Mannheim, Victoria and Uppsala for useful comments. Special thanks to Christoph Eder for his real-time research assistance. Financial support from the Canadian Institute of Advanced Research is gratefully acknowledged. Remaining errors are our own.

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1 Introduction

In the past twenty five years, there has been a dramatic increase in interest in the conduct of elections around the world, specifically in determining whether elections are “free and fair”. This has been accompanied by a doubling in the number of countries holding regular elections. However, only half of the most recent national elections held met the standard of being free and fair (Bishop and Hoeffler, 2004). The international community has responded to this gap between standards and performance by devoting significant resources to the monitoring of elections in which problems are anticipated. This typically involves sending election observers to the country in question. Some observers may arrive months before the election, and may assist with planning the election and training election officials, but the bulk arrive close to the election day. They monitor a random sample of polling stations, documenting (and attempting to prevent) irregularities at those stations. This effort culminates with the tabulation of a “quick count” of votes from the observed polling stations where violations of electoral fairness like ballot stuffing were hopefully deterred. The announced results can then be compared to the “fair” quick count.

However, observers have been criticized for being overly focused on what happens on election day, and for not devoting sufficient attention to violations of democratic norms that occur before the election day (see Carothers (1997) and Kelley (2010)). This is despite the fact that Bishop and Hoeffler (ibid) find that undemocratic acts are more likely to take place in the leadup to the election (what they term freeness violations) than during the actual voting and counting process (what they term fairness violations).

These questions of legitimacy are more common in emerging democracies, but they are not unknown to established ones. For example, States in the American South put institutions such as poll taxes and literacy requirements in place to foster African American disenfranchisement in the late 19th and early 20th century. A more contemporaneous example is that 19 U.S. states have enacted bills that require voters to prove citizenship or show a photo ID upon casting their ballot, or reduce early voting periods since 2011. These measures were seen by many as a strategy to demobilize parts of the electorate (Bentele and O’Brien, 2013). Other recent instances of vote suppression encompassed deliberate misinformation, often through automated phone calls (“robocalls”). In the 2008 elections, for example, Democrats in Nevada received robocalls informing them that they could vote on November 5 – a day

after the election – to avoid long lines. At the same time, Hispanic voters in Nevada received messages saying that they could vote by phone. Similarly, in the 2010 gubernatorial election, around 50,000 registered Democrats in Maryland were encouraged through automated phone calls to “relax and stay at home” although the polls were still open.¹ A 2006 investigation in Birmingham England uncovered systematic manipulations of a postal voting scheme in municipal elections, and Fukumoto and Horiuchi (2011) document widespread fraud in voter registration in Japanese municipal elections.

The aim of the paper is to present a general method for estimating the impact of specific violations of electoral freeness. This method is applicable to electoral systems where a) parties compete over multiple districts in the same election, b) districts are divided into polls, c) poll boundaries are either persistent over time or their boundaries are marked in electronic maps, and d) election results are available for at least two elections at the poll level. We also require that there is significant variation, both spatial and temporal, in reports of violations at the district level. That is, we need it to be true that for some districts, there were no reports of treatment in one election, and then there were in the subsequent election, or vice versa.² In order to do this, we need to overcome the two main challenges to identifying the impact of a particular violation of electoral freeness. The first is that since suppression is generally illegal (or at least unsavory), those engaging in it typically make attempts to hide their activities. As a result, data on the extent of voter suppression activities is almost always incomplete. It typically consists of aggregated reports of irregularities by voters, observers, or the media. Because the reports typically capture a small fraction of the total number of instances of treatment, the data will suffer from classical measurement error, which introduces attenuation bias. The second issue is one of omitted variable bias. It is natural to assume that crucial variables affecting both turnout *and* the selection of districts for voter suppression (e.g., anticipated closeness of the race) are not recorded in the data. Again, this introduces bias in estimating the impact of freeness violations.

We circumvent these problems by developing a novel empirical strategy, which allows us to account for district-level fixed effects (although treatment is at the district level) and instead uses *within district variation* for identification. Specifically, we use the variation of outcomes (turnout, vote shares) at the level of the polling station within a district.

Consider the following hypothetical: a country is divided into a number of districts which are them-

¹*Washington Post*, November 5 2010, [Election-night robocall tied to employee of political operative working for Ehrlich](#), retrieved January 11 2014. See also Barton (2011) for a review of the wide range of demobilization tactics that have been observed in the U.S., and further examples.

²We require an additional condition for identification of the effect, discussed below.

selves divided into polling stations. In the absence of treatment, election results in polling station i in district j are:

$$Y_{ij} = \alpha OppVoteShare_{ij} + X_{ij}\beta + \gamma_j + \nu_j + \epsilon_{ij}$$

where Y_{ij} is the log of turnout, X_{ij} is a vector of controls (e.g. lagged turnout), γ_j is a district specific fixed effect, and ν_j and ϵ_{ij} are errors. This relationship between opposition vote share and turnout is not intended to be interpreted causally. The incumbent violates electoral freeness (treats) T districts in a way that reduces the turnout of opposition voters to a fraction δ of what it was (for simplicity, we suppose this is the first election in which violations occur). This affects turnout in the treated districts:

$$Y_{ij} = \alpha OppVoteShare_{ij}^{\circ} - \delta * T * OppVoteShare_{ij}^{\circ} + X_{ij}\beta + \epsilon_{ij}$$

It also affects opposition vote share in those districts, so $OppVoteShare_{ij}^{\circ}$ is what opposition vote share would have been without treatment. Assuming treatment is observed, the simplest way to detect this would be to test whether there is a negative correlation between opposition support and treatment. However, this test requires that the likelihood of a district being targeted is independent of the level of opposition support, effectively ruling out strategic targeting choices on the part of the incumbent. To address this, one could test whether within treated districts, there is a negative relationship at the polling station level between opposition support (as measured in the past election) and turnout in the current election. However, this assumes that in the absence of targeting, the correlation would be zero. Since many variables that influence turnout, such as age, education, and income also influence partisan support, this assumption is untenable in most cases. To address this, we test whether the within district relationship between turnout and past opposition support is more negative in treated districts than it is in non-treated districts. This requires that within a treated district, the probability of an opposition voter being treated is independent of the past level of opposition support in that voter's poll. The justification for this assumption is that the incumbent only cares about winning the district as a whole - all voters at all polling stations are equally pivotal.

Since opposition vote share is also impacted by treatment, using observed opposition vote share instead of counterfactual opposition vote share biases the coefficient estimates obtained by the above method. However, since the above equation is correctly specified if the true value of δ is zero, the p-values will be correct. To obtain more reliable coefficients, we proxy for opposition vote share using the lag of opposition vote share. The resulting coefficients will be biased, but the direction of the bias is clear: the estimate of δ will be pushed towards zero.

This addresses issues of targeting, but it does not address the problem that treatment is not perfectly observed. This is especially likely to be true if the freeness violations are illegal. We discuss the impact this may have on estimates below.

The remainder of the paper is organized as follows: section 1.1 presents the example we use to test this method and reviews related literature, section 2 discusses the data and our strategy, section 3 presents the results, section 4 presents robustness tests, and section 5 concludes.

1.1 Our example:

We test our method on the results of the Canadian Federal election, that was held on May 2, 2011. The incumbent Conservative party won the election, and formed a majority government. On February 23 2012, news broke that *Elections Canada*, the independent agency responsible for administering and monitoring Canadian elections, was investigating complaints about alleged automated or live phone calls that had attempted to suppress votes for opposition candidates. The investigation, as well as the exposed voter demobilization tactics under scrutiny, subsequently received extensive media coverage, and became commonly known as the “Robocall Scandal”. Although *Elections Canada* does not comment on ongoing investigations, it produced a report in 2013 as a response to the scandal which provided some information about the incidents.³ In the report, the agency revealed that it had received numerous complaints from voters having received automated or real-person phone calls, purportedly from *Elections Canada*, falsely informing recipients that their polling station had moved (this message would have been particularly salient because Canadians may only vote at their local polling station on election day).

Other complaints alleged numerous, repetitive, annoying or sometimes aggressive live or automated calls, as well as calls made late at night. Following the disclosure of the investigation in the media, the Elections Commissioner received a total of over 30,000 communications regarding deceptive and harassing phone calls, including reports from 1,394 individual electors who recalled specific instances of having received calls misinforming them with respect to their correct polling station, or harassing messages made on behalf of a contender in the election for local member of parliament.⁴ The calls appear to have targeted supporters of the two largest opposition parties, the Liberals and the New

³See *Elections Canada* (2013), *Preventing Deceptive Communications with Electors Officer of Canada Following the 41st General Election*.

⁴Over 70 % of those voters said the calls directed them to a wrong polling station, and roughly 50 % of voters said that the callers, live or recorded, were claiming to emanate from *Elections Canada*. See *Elections Canada* (2013).

Democrats.

Initially, 18 districts were under investigation, but after the media picked up the story and the public learned about the probe, more voters came forward with complaints and the list of allegedly affected districts grew to 27 by February 26, 2012. By mid-August 2012, complaints had been recorded from voters in over 200 districts, according to court documents filed by the Commissioner of *Elections Canada*.⁵ In response to a legal challenge by a group of voters in six of these districts, a Federal Court found that “widespread election fraud occurred” during the 2011 federal election, stating in its ruling that “there was an orchestrated effort to suppress votes during the 2011 election campaign by a person with access to the [Conservative Party’s] CIMS database”. Overall, however, the Federal court ruled that the evidence was not quantitatively significant enough to warrant overturning the Conservative MPs’ mandated terms.⁶

We have chosen this episode of voter suppression as an application for our model for two reasons. First, this is an issue of significant interest to Canadian public. The scandal has received widespread media attention. But despite the scrutiny, and a two year long investigation, details about what happened have been scant. Elections Canada was able to confirm that 7,600 automated calls directing voters to the wrong polling station were made the district of Guelph, and charged a Conservative Party communications staffer, Michael Sona, in connection with the calls. These calls resulted in only 68 complaints, or 4.8% of the total. Prospects for successful investigations outside of Guelph are poor; *Elections Canada* has acknowledged it is unlikely they will ever be able to produce an accurate account of the events in the days leading up to the 2011 federal election, or that there will be a satisfactory conclusion to the criminal investigation. In sum, it does not appear likely that non-statistical methods will be able to say much more about the extent or impact of this episode of electoral fraud.

Second, this is a demanding test for our model. While the number of voters that received suppressing phone calls may have been high in an absolute sense, the fraudulent activity was not so obvious that it attracted widespread attention as it was occurring. In addition, we do not observe treatment, only reports of treatment. As individuals engaged in fraudulent activity usually attempt discretion, this is likely to be the case for any other situation where this method would be applied.

⁵See *National Post* from August 21, 2012, [Robocall Complaints About the Federal Election Have Doubled Since March: Court Records](#). Retrieved January 11, 2014.

⁶*Mcewing v Canada (Attorney General)* 2013 FC 525 at para 184, 246.

Related Literature

Our paper relates directly to two literatures: a literature on the detection of electoral fraud, and a related literature on the effectiveness of specific methods of influencing voter turnout.

Detection of Electoral Fraud: The literature on detection of electoral fraud can be divided into three areas. The first is observer based methods for detecting fraud. This typically consists of sending observers to monitor elections in countries where it is expected that fraud may occur. Some observers may arrive months before the election, and may even help the local electoral commission plan the election. It may also include collecting reports, media or otherwise, from citizens of the country about electoral problems. These methods have the added benefit that they may act to deter fraud, as well as detect it. There is mixed evidence about the effectiveness of observers in disciplining electoral participants. Hyde (2007) uses the random assignment of election observers in Georgia to show that the presence of election observers at a poll reduced the electoral support for the government, and estimates the size of the reduction at 6%. However, Ichino and Schündeln (2012) implement a field experiment in Ghanaian elections to show that parties may respond to observers by reallocating problematic activities from observed polling stations to unobserved polling stations, and thus a direct comparison of treated and untreated polls may overstate their effectiveness.

In general, there are two problems with these methods. The first is that they are expensive. The second is that they effectively rely on observers being able aggregate all the fraudulent, irregular, or problematic behaviour they have seen into a pass/fail metric. This is especially difficult without estimates of the likely impact of different types of fraud.

Second, there is a relatively new body of work that uses modern statistical techniques to empirically analyze election fraud that occurs at the polling station level. A number of studies have applied tests based on the second-digit Benford's law to detect electoral fraud (see in particular Mebane, 2012, and the references therein). Wand et al (2001) employ multiple methods, including ordinary least squares, to detect anomalies in the vote for Buchanan in the 2000 Presidential Election. The authors conclude that the butterfly ballot used in the county of Palm Beach, FL, caused over 2,000 Democratic voters to mistakenly vote for Pat Buchanan. Klimek et al. (2012) use scatterplots of turnout and incumbent support to analyze recent elections in Russia and Uganda. They show that for both countries, the scatterplot is suspiciously bimodal. In the Russian case, one of the modes is at 100% turnout and

100% support for the incumbent. However, these statistical methods are limited to the detection of problems that occur in the polling station (e.g. ballot stuffing, double voting), and struggle to detect problems that occur leading up to the election (e.g. vote buying, intimidation, misinformation, banning some candidates).

Third, there is a small but directly relevant literature on detecting fraud that is not specific to particular polling stations. Fukumoto and Horiuchi (2011) use a natural experiment to show that fraudulent address changes are common in Japanese municipal elections. Alvarez and Katz (2008) use historical election data in a political forecasting model to investigate fraud allegations in the 2002 federal election in Georgia; they find little evidence the hypothesis that tampering with the voting machines created systematic advantage for one party, however. We contribute to these studies methodologically in that our focus on polling station level information *without* polling station level treatment allows us to circumvent some of the statistical problems associated with modelling elections using multivariate regression analysis. Because of this, our paper is complementary to the observer-based literature, as a way of estimating the impact of different observed behaviours. It is also complementary to the polling station based literature, because it is designed to detect types of violations of electoral norms they cannot, and vice-versa.

Determinants of Voter Turnout: There has been a considerable amount of research on the determinants of voter turnout and voter mobilization.⁷ Following the pioneering work of Gerber and Green (2000), a large body of work studies the efficacy of “Get Out the Vote” (GOTV) campaigns based on randomized field experiments.⁸ One finding that is consistent across a number of mobilization experiments is that only personalized messages, delivered in person through live phone calls [Gerber and Green (2004)] or door-to-door canvassing [Nickerson (2006), Arceneaux and Nickerson (2010)] are effective in mobilizing voters. In contrast, experiments testing impersonal GOTV methods such as mass email [Nickerson (2007)] and robocalls [Green and Karlan (2006) and Ramirez (2005)] find no statistically significant effects on voter turnout.

A second branch of the literature has focused on voter demobilization – the methods and messages used by key players in the electoral process to limit turnout, or to discourage specific (groups of) voters from voting. One main question of this research has been whether or not negative campaigning depresses voter turnout. The evidence here is somewhat mixed. Ansolabehere et al. (1994, 1999)

⁷See Geys (2006) for a comprehensive survey of the literature.

⁸See their book Green and Gerber (2004) for additional information and further references.

find that negative campaigning significantly reduces turnout at the polls, while subsequent studies reach more optimistic conclusions, finding no evidence in support of the demobilization hypothesis [see e.g. Clinton and Lapinski (2004)]. Other work deals with the historical effect of African American disenfranchisement in the South. Jones et al. (2012) measure the impact of both formal laws and informal modes of voter suppression (poll tax, literacy tests, lynching) on African-American political participation, and find that having one more lynching in county in year decreases black turnout by 3 to 6 percentage points. Further evidence in support of voter disenfranchisement is presented in Naidu (2012), who identifies a causal link between disenfranchisement and reductions in state level spending on black schools, as well as Cascio and Washington (2012), who show that the Voting Rights Act of 1965 significantly increased in black voter participation and induced a shift in the distribution of state aid toward localities with large black populations.

The present paper is the first study on the impact of deliberate misinformation on voter turnout using data from an actual election. Due to legal and ethical concerns, there have been no field-experiments conducted on whether or not intentionally misleading voters has an effect, and if so, how large that effect is. The only other related contribution we are aware of is Barton (2011), who reports on a framed field experiment where participants in a ‘mock’ gubernatorial election held on a university campus concurrently with the actual gubernatorial election were being intentionally misinformed about the timing of the election. He shows that misinformation regarding election timing reduces voter turnout by 50 percent relative to a control group, but that warning voters of potential misinformation beforehand removes this effect.

2 Data and Empirical Strategy

The empirical analysis is based on a sample that includes the official election results for the 41st Canadian General Election (May 2, 2011), and the results from the previous three general elections as controls, the 40th General Election (October 14, 2008), the 39th General Election (January 23, 2006), and the 38th General Election (June 28, 2004). The data are available on the *Elections Canada* website <http://www.elections.ca/>. For each election and electoral district, we obtained the number of votes for each candidate running in that district, as well as the number of absentee ballots, the number of rejected (invalid) ballots, the total votes cast, and the number of eligible voters. Importantly, those figures are broken down within electoral districts by polling station, which is central to the identification strategy laid out below. There are 308 ridings in total.

Ideally, of course, one would want to indicate ‘treatment’ using the set of ridings where *Elections Canada* confirmed incidences of illegal activities, but the full investigation has not been completed yet, and as mentioned above, *Elections Canada* has a policy of not disclosing information on ongoing investigations. We therefore can only make use of the information on complaints that have been made public in the media. With the exception of some reports that appear to have been initially investigated by *Elections Canada* (in a few ridings, such as Guelph), those complaints have not been officially verified yet. Relying on allegations as reported in the press may lead to considerable measurement error in the data. We do not even have a list of the number of verified complaints by district. For this reason, we confine ourselves to a list of 27 ridings that was made publicly available through various media and party websites in Canada relatively early into the probe (as of February 26, 2012). This list, which was apparently leaked from a source inside *Elections Canada*, is primarily composed of ridings where reports of robocalls were received before this issue became a national news story. Because media reports of ridings where individuals came forward with their recollection of robocalls only a week after the news broke are likely subject to even larger measurement error, this early list is likely to be much more reliable. As we will see below, the estimated effect becomes smaller in value and insignificant if we instead use an extended list of 102 ridings where the media reported alleged robocalls as late as August 2012.⁹

The dependent variable is voter turnout, defined as the percentage of the registered voters who actually cast their vote in the 2011 federal election. Figure 1 shows a geographic map of all 308 electoral districts boundaries, as well as a categorical breakdown of riding-level voter turnout; the 27 districts on our treated list are highlighted in red.

[Figure 1 about here]

The descriptive statistics are summarized in Table 1.

[Table 1 about here]

Apart from concerns pertaining to measurement error, the second challenge when estimating the impact of possible misinformation voter turnout is that the districts that were (allegedly) subjected to the phone calls do not necessarily constitute a random sample. For example, one plausible selection criterion for anyone who deliberately sought to suppress the vote is the expected margin of victory, i.e.,

⁹The complete names of all ridings both the original list of 27, as well as the extended list of 102, can be found in the Appendix.

those districts where the race was expected to be close (and thus the impact of any calls largest) could have been deliberately targeted. The data support this logic: the average winning margin for districts with no robocall-allegations was 10,903 votes or 22.8 percentage points. Ridings where allegations of impropriety have emerged, in contrast, had a margin of victory that was almost 28 percent lower: 8,719 votes or 16.3 percentage points. At the same time, we know from existing work that some form of ‘closeness’ of the race has a significant and positive impact on voter turnout.¹⁰ Moreover, the treated ridings are –by design or by chance – almost all urban in character; average income and education levels of the electorate are thus above those of untreated ridings, again increasing expected turnout. Even if there was no causal effect of robocalls on turnout, we would therefore expect a higher turnout in the affected districts. Indeed, in the 2011 election, turnout in allegation-free ridings was an average of 52.1 percentage points compared to 53.3 percent in robocalled districts.

The primary problem that we need to address is thus one of unobserved variables correlated with the selection of targeted ridings that also impact voter turnout. One strategy that naturally presents given data availability is a difference-in-differences approach. Difference-in-difference estimates use pre-treatment differences in outcomes between treatment and control group to control for pre-existing differences between groups, i.e., they measure the impact of a treatment by the differences between the treated group and the control group in the before-after differences in outcomes. Applied to our context, we would compare the change in voter turnout from the 2008 to the 2011 election in the affected ridings (the treatment group) with the change in voter turnout in the unaffected ridings (the control group), possibly controlling for other observable covariates at the district level such as (lagged) margin of victory and changes in population demographics. This identification method, however, essentially proxies the unobserved expected differences in voter turnout between treatment and control group (absent treatment) with actual voter turnout in the previous election. The exclusion restriction would thus be that between the 2008 and 2011 elections, there was no change in those riding characteristics on which voters and robocall initiators based their voting decisions on, which obviously would be a strong assumption.¹¹

For this reason, we employ a slightly different, and to our knowledge novel, identification strategy: instead of using between-ridings variation to identify the effect of alleged misconduct, we use *within*

¹⁰The estimated size of the effect in the literature is such that an increase in closeness by one standard deviation unit increases turnout rates by approximately 0.58 – 0.69 standard deviation units on average. See Geys (2006) for a comprehensive survey.

¹¹Section 4 below presents, among other robustness checks, the results using a classic DiD approach, which yields a statistically significant effect of robocalls that is statistically significant, and twice as large in magnitude as the one we identify.

riding variation, taking advantage of the fact that *Elections Canada* breaks the results down at the level of the polling station for each riding. Studying individual polling station outcomes within electoral districts has the advantage that we can employ district fixed effects, which will absorb any unobserved heterogeneity at the riding level, including the – unobserved – estimated margin of victory just prior to the election and other riding level characteristics that may have changed between 2008 and 2011.¹²

[Figure 2 about here]

Figure 2 helps to illustrate our basic idea using the Ontario riding of London North, on our treated list, as an example. The figure shows that there is considerable variation at the poll level within each riding. This is true both for voter turnout (the map on the left) and, importantly, also for political affiliations of its residents (the map on the right): some neighbourhoods within a riding tend to lean towards the Conservatives, while others are more inclined to vote for the Liberals, the New Democrats, the Bloc, or the Green Party. Within each district, the voting stations with a higher fraction of non-Conservative voters will typically have a lower turnout rate than usual due to the fact that the Conservatives mobilized their constituency more effectively (it was their best election result since 1988). In Figure 2, districts with dark green shading in the map on the right (indicating more non-Conservative voters), will on average have a lighter blue colour (indicating lower turnout) in the map on the left, and vice versa. We should thus see a negative relationship between the share of non-Conservative voters and voter turnout. Our parameter of interest is the impact of a robocall on the (relative) probability that a non-Conservative voter votes. Now assume that whoever was the potential instigator behind the alleged robocalls randomly called non-Conservative voters in the 27 treated districts we identified. Since the calls were *more* likely to be placed in polling station leaning predominantly non-Conservative, we should see a *more pronounced* drop in turnout of those polls, relative to the district average, in the treated districts as compared to the untreated districts.

Specifically, we estimate a regression model of the form

$$Y_{ij} = \alpha_j + \beta_1 lagY_{ij} + \beta_2 lag_2 Y_{ij} + \gamma lagOppvs_{ij} + \delta(robocall_j \times lagOppvs_{ij}) + \epsilon_{ij}. \quad (1)$$

The dependent variable Y_{ij} is the log of voter turnout (in percent) in the 2011 federal election at polling station i in riding j , where voter turnout is defined as the absolute number of people voting

¹²Naturally, using district fixed effect is also important because polling stations in the same district may be subject to common shocks, so their outcomes are likely correlated. Because treatment (robocall) status is also uniform within a district, the correlation in turnout outcomes may be mistakenly be interpreted as an effect of the being robocalled. The district fixed effects eliminate this concern.

at polling station i in riding j times, divided by the absolute number of registered voters for that polling station and riding.¹³ The right-hand side variables are log voter turnout at the same polling station in the 2008 federal election, $lagY_{ij}$, and the 2006 federal election, lag_2Y_{ij} , the combined vote share of all non-conservative candidates at this polling station in the 2008 election $lagOppvs_{ij}$, and electoral district fixed effects α_j . The coefficient on the interaction term $robocall_j \times lagOppvs_{ij}$ is the parameter of interest.

If γ is negative, those polling stations with more non-Conservative voters experienced a drop in voter turnout from the 2008 to the 2011 election, whereas turnout at polling stations with more Conservative voters rose between the 2008 and the 2011 election, relative to the district average, controlling for turnout rates in previous elections we have data for. The coefficient on the interaction term, δ , now measures whether this effect is *stronger* in ridings affected by the robocalls, i.e., whether the robocall indicator detects a differential impact. A negative and significant value of δ thus indicates that the difference between how Conservative voters and voters with a different political orientation turned out at the polls was *larger* in those ridings that were allegedly targeted by calls directed to suppress the (presumably non-Conservative) vote, controlling for voter affiliation and turnout rates in previous years. The identifying assumption in this strategy is that the incidence of robocalls is unrelated to the potential outcomes at polling stations *relative to the district average*, conditional on polling station turnout in the previous elections and the fraction of non-Conservative voters. This assumption implies that in the absence of any misconduct, polls with similar (non-)Conservative vote shares within the same district should have seen a similar deviation from the district average turnout, conditional on past deviations from the district average.

We use the log of turnout in our base specification because the magnitude of the predicted effect should be larger in polling stations with higher turnout (there are more potential voters to deter from going to the polls) and taking logs adjusts for this.

Two treated ridings were dropped from the analysis: the first is Portneuf-Jacques Cartier, where no Conservative ran in 2008. The other district is Saanich Gulf-Islands, where fraudulent robocalling was already reported in 2008. We also dropped all advance, absentee, and mobile polls, where the logic of the identification strategy does not apply. Similarly, we drop a number of polling stations that do not match up from previous elections to 2011 because they have been split up in some way, or rejoined. Another potential problem is that there are substantial differences in the number of polling stations

¹³We take the log of the percent turnout, from 0 to 100, plus one.

in each district. If we weight each observation (polling station) equally, districts with more polling stations would have more influence on the results. We address this problem by weighting the polling stations so that within a district, the weight of a polling station is equal, and that the sum of the weights for the polling stations in a district is the same for all districts.¹⁴

3 Results

Table 2 below reports the resulting parameter estimates. Part A of the table shows the results of a simple cross-section regression, where 2011 voter turnout in riding j is explained by the robocall indicator variable (robocall), controlling for riding characteristics through variables from the previous election(s): voter turnout (lagTurnout, lag₂Turnout), the percentage margin of victory (lagMargin) and the combined percentage share of non-Conservative votes (lagOppvoteshare). Part B present results using our baseline regression 1 that includes electoral district fixed effects.

It is instructive to start with the cross-section regression: we see that the dominant determinant of turnout in riding j in the 2011 election was turnout in the same riding in 2008. Turnout in the 2006 election, however, also explains some of the variation. The coefficient of the winning margin (closeness of the election) in 2011 is negative, as expected. At the district level, political orientation does not matter much, as indicated by the small (negative) and statistically insignificant coefficient of the lagged *Opponentvoteshare*. The coefficient on the robocall indicator, though insignificant, is positive. In other words, even if we proxy the expected margin of victory by its value in the last election, and control for historical turnout, the treatment group still has higher turnout, on average, than the control group. As discussed earlier, however, this observed positive correlation could be driven by a change of characteristics of the riding between 2008 and 2011: it would emerge if, for instance, if treated ridings had an expectation of closer races in 2011 than they had in 2008, and if voters in those ridings were more likely to go to the polls because they felt that their vote mattered more.

[Table 2 about here]

Our identification strategy addresses this issue in a natural way through introducing electoral district fixed effects, which absorb any (change in) unobserved differences at the district level. Part B in Table 2 presents the corresponding estimates. We see that at the level of a polling station, turnout

¹⁴In general, our results were very robust to alternative weighting functions, including dropping weights entirely. See also Section 4 below.

in previous elections still matters most. The combined vote share of the non-Conservative candidates in the prior election is now an important determinant of voter turnout: those polling stations with higher margins for the candidates running against the Conservative candidate in 2008 experienced a drop in turnout in the 2011 election *relative to the district average*. The coefficient on the interaction term signifies that this effect is more severe in ridings with alleged misconduct. In other words, relatively more voters from polling stations that were predominantly non-Conservative stayed home in robocall ridings. The point estimate of the parameter on the interaction term is .070, and has a p-value of 0.022.¹⁵ The estimate implies that if we compared a (hypothetical) polling station with 100 percent non-Conservative votes in a specific riding with another (hypothetical) polling station with 100 percent Conservative votes, *in the same riding*, the former had 7 percentage points less turnout in those ridings where robocalls were reported.

To better assess the magnitude of this effect, we can take the mean lagged combined vote share of all opponents to the Conservative candidate in the affected districts, which was 55.1 percent, and multiply it by the coefficient estimate.¹⁶ The resulting figure of -3.85 gives an estimate of the reduction in voter turnout, measured as a percentage, for the targeted ridings. Using the fact that the average targeted riding had 80,913 registered voters, this translates into an estimated absolute number of roughly fewer 3,000 voters at the ballot, a substantial number. Of those ridings on our list allegedly affected by robocalls, a total of seven had winning margins smaller than that. The lower bound of the 95% confidence interval is still 450 fewer voters that did not vote in robocall ridings on election day, an amount which exceeds the winning margin in one affected district.

To provide a first check on the plausibility of those estimates, it is instructive to look at the riding of Guelph, which is so far is the only riding where the robocall investigation of *Elections Canada* has disclosed quantitative evidence: following numerous complaints about telephone calls regarding voting location changes, *Elections Canada* was able to trace the calls back to a pay-as-you-go cell phone which led the investigators to a voice broadcasting vendor. Records from the vendor show that 7,676 calls were made to Guelph phone numbers within 15 minutes on election day. The calls to electors were transmitted from the voice broadcasting vendor using VoIP (voice over Internet Protocol)

¹⁵We also bootstrapped our standard errors as an alternative (non-parametric) method to measure the accuracy of our estimates. To that end, we use the 279 districts where no robocalls were reported, and randomly draw 27 existing districts (with replacement) to replace the dropped districts, and assign robocalls to those districts in the new sample. Finally we calculate the parameter of interest and the t-statistics of our specification. This procedure is repeated 1000 times to obtain the joint distribution of the t-statistics for both years. The samples were unbiased in both elections. The estimated boundary of the 95 % confidence for the 2011 interval occurs at $t=1.70$, which is considerably lower than the asymptotic value of 1.96. This is possibly because our strategy of clustering errors at the district level is overly conservative.

¹⁶Alternatively, we could use the mean lagged combined voteshare on the polling station level, which is also 55.

calling technology.¹⁷ The list of numbers that were called is consistent with a list of non-supporters of the Conservative Party of Canada, obtained from that party’s database. The vendor charged a total of \$162.10 for these calls¹⁸ which illustrates how cheap the technology is. If we take the confirmed number of 7,676 contacted household as a lower bound for the quantitative incidence of the robocalls in Guelph, we can apply our parameter estimate to obtain an estimate of the maximal ‘success’ rate of voter suppression. The average number of persons in a household in Guelph was 2.5 (2011 Census of the Population). Depending on how many voters were affected per contacted household, this would give us a maximal success rate that lies between 16 percent (all household members) and 39 percent (one voter per household). As an upper bound, this does not seem to unreasonable given Barton (2011) finds a 50 percent success rate for voter misinformation.

4 Robustness Checks

This section present results from a range of sensitivity exercises that we carried out to ensure the robustness of the basic results; it also discusses possible weaknesses in the identification strategy. In addition, to address the concern of whether the exclusion restriction is satisfied, we perform a number of falsification tests.

4.1 Sensitivity Analysis

Table 3 displays the estimated coefficient of the robocall interaction term using variations of our baseline specification from Table 2, Part B. For ease of reference, row (1) repeats the results. As a first robustness check, we add an additional lag. The estimated equation is

$$Y_{ij} = \alpha_j + \beta_1 lagY_{ij} + \beta_2 lag_2 Y_{ij} + \gamma_1 lagOppvs_{ij} + \gamma_2 lag_2 Oppvs_{ij} + \delta_1 (robocall_j \times lagOppvs_{ij}) + \delta_2 (robocall_j \times lag_2 Oppvs_{ij}) + \epsilon_{ij} \quad (2)$$

This specification uses two lags of opposition vote share as a proxy for contemporaneous counterfactual opposition vote share instead of one. For ease of comparison, we report the sum of δ_1 and δ_2 , but calculate significance using the F-test that they are jointly zero, which is slightly more

¹⁷VoIP calling is computer-generated calling over the Internet to recipients telephones. This technology allows a voice broadcasting vendor to program into the call process any calling number its client wishes to be displayed on a recipient’s call display. That number would have nothing to do with the actual call made by the vendor.

¹⁸See the *Elections Canada* report (2013). A former Conservative campaign worker is facing a criminal charges under the Canada Elections Act. The charge is listed under section 491(3)d of the Canadian Elections act, which prohibits preventing or trying to prevent a voter from casting a ballot. The maximum penalty is a \$5,000 fine and five years in prison.

conservative ($F = 3.65$, $p = 0.02$). Removing lags, as we do in row (3), leaves the magnitude of the coefficient largely unchanged, but standard errors do increase slightly. These stability of the coefficient estimates indicates that serial correlation in the error terms should not be a major concern.¹⁹

[Table 3 about here]

Next, we consider a modified difference-in-differences specification, estimating

$$Y_{ij,t} = \alpha_{j,t} + \gamma Oppvs_{ij,t-1} + \delta(robocall_{jt} \times Oppvs_{ij,t-1}) + \epsilon_{ij,t}, \quad t = 2011, 2008, 2006. \quad (3)$$

This equation states that in the absence of treatment, expected log turnout of poll i in riding j at time t is determined by a time-variant riding effect $\alpha_{j,t}$, as well as the composition of the electorate in that poll ij at time t , as proxied by the corresponding non-Conservative vote share of the previous election (time $t-1$). Note that since this specification includes riding time-varying riding fixed effects, the treatment effect only emerges through the interaction term. As can be seen from row (4), the corresponding estimate of the treatment effect almost doubles, and remains significant at the 10% level.²⁰

The specification in row (5) adjusts the weight given to each poll, so the sum of the weights of the observations from each district is equal, while within a district polls are weighted in proportion to their number of voters. Either is consistent with our strategy of clustering errors at the district level; we are treating each district as a single observation. Alternatively, we could continue to weight each district equally, but assign more weight to polling stations with more votes cast, as the larger sample size in these polls reduces the variance of outcomes.²¹

The next two rows (6) and (7) address the possibility that the effect we measure picks up a trend that is specific to ridings in Ontario, or to ridings where the race was close, defined as belonging to the lowest quantile of winning margins (below 5,000 votes in total). For instance, it could have been the case that the provincial arm of the a federal party in opposition was involved in a scandal between the 2008 and 2011 election, which may have prompted more non-Conservative voters to stay at home

¹⁹Also, the estimated coefficients on the lagged turnout from both the 2008 and the 2006 elections are precisely measured and different from each other at the 1 percent level, suggesting that the underlying data-generating process does not correspond to a difference-in-difference model. We also instrumented the 2011 opposition vote share and the interaction with the 2008 opposition vote share and the interaction in our original regression. The point estimate became even more negative and was significant at the 1% level. Further tests led us to reject both a fixed effects and AR(1) specifications for the evolution of turnout and opposition vote share.

²⁰In contrast to our baseline regression (1), the difference-in-differences specification does not allow for time-dependent variation of turnout within a riding other than through the political affiliation of the electorate.

²¹Dropping weights altogether also does not alter our findings. See the previous version of this paper for details.

on election day in Ontario relative to other provinces. Since most ridings we identify as treated are in Ontario, comparing the outcome in those ridings to electoral districts in other parts of the country would falsely attribute part of this effect to our robocall measure. Similarly, close races could have discouraged opposition leaning individuals from going to the polls. By restricting the entire sample to ridings with those respective characteristics, we ensure that the untreated observations are comparable in this dimension, at the cost of losing observations.²²

Row (6) shows that the magnitude of the effect is slightly lower in the Ontario, and the coefficient is no longer significant. This is not unexpected, because treatment is imperfectly observed. By isolating our comparison to districts that were geographically close to treated districts, we are both decreasing the sample size and increasing the likelihood that our comparison districts were treated, but it was not initially reported. In the restricted sample with close races shown in (7), the absolute value of the estimated effect increases and is still significant at the 5% level.²³

However, the same is not true if we use the latest (extended) list of ridings that had reported robocalling, as can be seen in row (8). In late March, the media had compiled a list of 102 affected electoral districts. These complaints came from electors directly contacting the media, since *Elections Canada* would not disclose any information pertaining to the scandal. As discussed earlier, therefore, one would naturally presume to be considerably more measurement error in these later reports, and this outcome is thus somewhat expected.²⁴

Row (9), row (10), and row(11) present alternative variables and alternative functional forms. Row(9) reports the results of proxying for counterfactual opposition vote share using actual opposition vote share. Because current opposition vote share is affected by treatment, the resulting coefficient will be biased if the true effect is non-zero (and the direction of the bias is not clear). However, the test that the effect of treatment is zero will have the correct size under the hypothesis that there is no effect, and we see that the result is significant. Row (10) replaces logged turnout with turnout on both sides of the equation and reports the OLS results, while row (11) makes the same substitution but reports the result of using a generalized least squares model with a logit link, a specification that

²²In Appendix A to this study, we document the results from using a somewhat more sophisticated method, namely propensity score matching, to address this problem more generally. The objective was to determine whether the findings still hold when we mandate that the control group be as similar as possible to the treatment group, where ‘similar’ was determined by a propensity score.

²³Indeed, a closer look at ridings with close races reveals that opposition voters turned out in greater numbers there, *ceteris paribus*. This is of course consistent with the increase in δ in absolute value when restricting the sample to ridings with small winning margins. If close races induce a bias, it is likely to be positive and we would thus be underestimating the effect.

²⁴Obtained from The Sixth Estate on March 29, 2012, [Irregularities Reported in 200 Ridings During 2011 Election](#), retrieved April 15, 2012.

is appropriate for data where the dependent variable is a proportion. The results remain significant, and the coefficient estimates are the same or larger in magnitude.²⁵

4.2 Campaign Intensity

One possible concern with our estimation strategy is that that regressions may be picking up some form of unobserved campaigning ‘intensity’. In particular, suppose the electoral districts that were allegedly targeted by illegal phone calls were also experiencing more campaigning efforts by legal means. As mentioned earlier, the studies that have tried to link (negative) campaigning with turnout have been inconclusive overall, so there is little sound evidence by which to go on. Generally, though, it is conceivable that more voters with affiliations to the Liberal Party or the New Democratic Party were discouraged from going to the polls in districts where the Conservative candidate spent more on campaign advertising and canvassing etc. If the latter – for whatever reason – is correlated with the robocall indicator, estimates based on a model that does not include campaign finance data would be biased upward.

For this reason, we reran our main specification with an interaction term that allows turnout to decline more in polling stations with a larger opposition vote share as a function of campaign spending. The results are listed in Table 4. We see that controlling for campaign spending of Conservative candidates leaves both the magnitude and the significance of the coefficient on the robocall indicator unaffected, which is reassuring. There is also no detectable differential effect of spending on how many (opposition leaning) voters were discouraged from going to the polls. The coefficient on the interaction term of the share of non-Conservative votes in the 2008 and campaign spending is very small and not significantly different from zero.²⁶

²⁵ As an additional robustness check, we verified that our results are not being driven by districts that are geographically distant from those that we flagged as being robocalled. Since approximately 8.9% of polls are in robocalled districts in our sample, we retain in the sample only polls in robocalled districts and the 9% of polls that have the shortest distance to these robocalled districts. Distance between polls is measured as the number of polls one would have to cross to get to a robocalled poll (queen distance). We use this distance measure because geodesic distance is confounded by urban/rural differences. This measure is negative within districts that were robocalled, and positive outside of those districts. There are 5161 polls within districts that were robocalled (we had to use the subset of polls that match to available shape files). This reduced sample gives a point estimate of -0.046 with a standard error of 0.0158 (the t-statistic is -2.93) for the parameter of interest in our main regression. Since there were 5160 polls within 3 polls of the border to a robocalled district, we restrict the sample to all polls within robocalled districts *and* those polls outside robocalled districts within 3 polls of the border. The results are a point estimate of -0.051 with a standard error of 0.026 (the t-statistic is -2.33), which is significant at the 5 % level. The standard errors are thus slightly larger, which is not surprising considering the procedure removes 4/5th of the sample.

²⁶We only report our baseline specification here, but this finding was very robust with regard to various alternatives models.

[Table 4 about here]

4.3 Falsification Tests

Assuming that the alleged incidences of robocalling actually took place, it seems likely that they were targeted in some way. Due to the riding specific fixed effects, our estimation strategy allows for the fact that whoever was behind the calls could have been directing the misinformation towards (opposition) voters in particular ridings. In principle, it also can accommodate a selection of targeted voters that reside in particular polls within a riding, provided that this selection was not based on a poll-specific characteristic that is correlated with the error term. More generally, our identification strategy provides an unbiased estimate of the treatment effect only if in the absence of any robocall allegations, polling stations across all districts would have experienced similar turnout changes as a function of their observed characteristics from the 2008 (and 2006) elections, *relative to the district average*. One possibility that could invalidate this assumption is that polling stations were targeted based on characteristics correlated with turnout. For example, suppose voters differ in their propensity to go to the polls on election day, and that this characteristic varies across the electorate of different polling stations (e.g., because it varies across voters with different demographics). If the instigator of those robocalls somehow targeted those polling stations,²⁷ the robocall indicator would pick up this effect - we would conclude that robocalls cause a drop in turnout relative to the riding average, when in fact the drop would have occurred even in the absence of robocalls because those polling stations happen to have an electorate that is of the easily-to-demobilize kind, and they were selected for that very reason.

To gain some insight on whether or not our robocall variable may be correlated with unobserved, time-invariant characteristics of polling stations (as opposed to districts), we reran the baseline regression in Table 2, using turnout from the 2008 and 2006 general election as our main dependent variables as a falsification test. The estimated coefficients are reported in Table 5.

[Table 5 about here]

The findings do not support evidence of a statistically significant relationship between the turnout at

²⁷During the scandal, there were suggestions in the media that elderly people were specifically targeted. See *The Toronto Star* from March 09, 2012, [Robocalls: Older voters targeted by election day phone calls, Elections Canada believes](#), retrieved April 11 2014.

specific polls and the robocall identifier in previous elections. The robocall variable does not touch on statistical significance in either year, and while the point estimate is still negative in the 2008 election, it reverses its sign in the 2006 election, suggesting that there are no systematic trends in voter turnout that are correlated with our main right-hand side variable of interest. Separate regression relating changes in turnout from the 2008 to the 2011 election to the corresponding changes in turnout from the 2006 to the 2008 election also indicate that there is no (time-invariant) time trend in turnout.²⁸

5 Conclusion

This paper has investigated allegations of attempted voter demobilization in the context of the Canadian 2011 federal election. In 27 of the 308 districts, voters allegedly received automated phone calls containing false information on the location of their election site, or harassing them in the name of one of the contestants. The results suggest that, on average, voter turnout in those districts affected by the demobilization efforts was significantly lower than in the ridings where no automated phone calls have been reported. The point estimate gives a decrease of 7 percentage points in the number of opposition voters turning out. While this may be a reasonable estimate of the effect of misinformation in the 2011 Canadian Federal Election, care should be interpreted in applying the specific value to other contexts, because the effectiveness of voter misinformation is strongly negatively related to the degree to which voters expect it (Barton (2011)).

On the other hand, we believe the method outlined in this paper for detecting these effects has broad applicability to other contexts and other violations of democratic norms, provided that elections are contested in small districts, poll level results are available and useful, there is a benchmark of past election results to compare to, and there is sufficient variation in reports of norms violations. However, this does not imply that the determination of fraud can be done automatically, or on the basis of a rule. Care must be taken to evaluate reports of violations, and discard those that are not credible or that were made in response to media attention about the violations instead of the violations themselves; a failure to do this in our case would have led to a null finding. Researchers

²⁸See our discussion paper for a more in-depth discussion as well as additional robustness checks using alternate control groups determined by propensity score matching. In summary, the estimated effects are not fully robust to the details of the matching procedure, and the pre-selection of the sample. The main problems with matching estimators in our context are that a) our sample of only 27 affected ridings out of 306 is quite small, implying that whether a particular observation is included (matched) or excluded (not matched) can change the coefficient and standard errors significantly, and b) we do not observe “treatment” with certainty. The latter means that if we use a binary variable — whether or not a riding appears on a list of robocalled electoral districts that was published early in the scandal — as a proxy for actual treatment, the probit regressions we use to calculate a riding’s propensity score provide an *alternative* estimate of the probability of being treated.

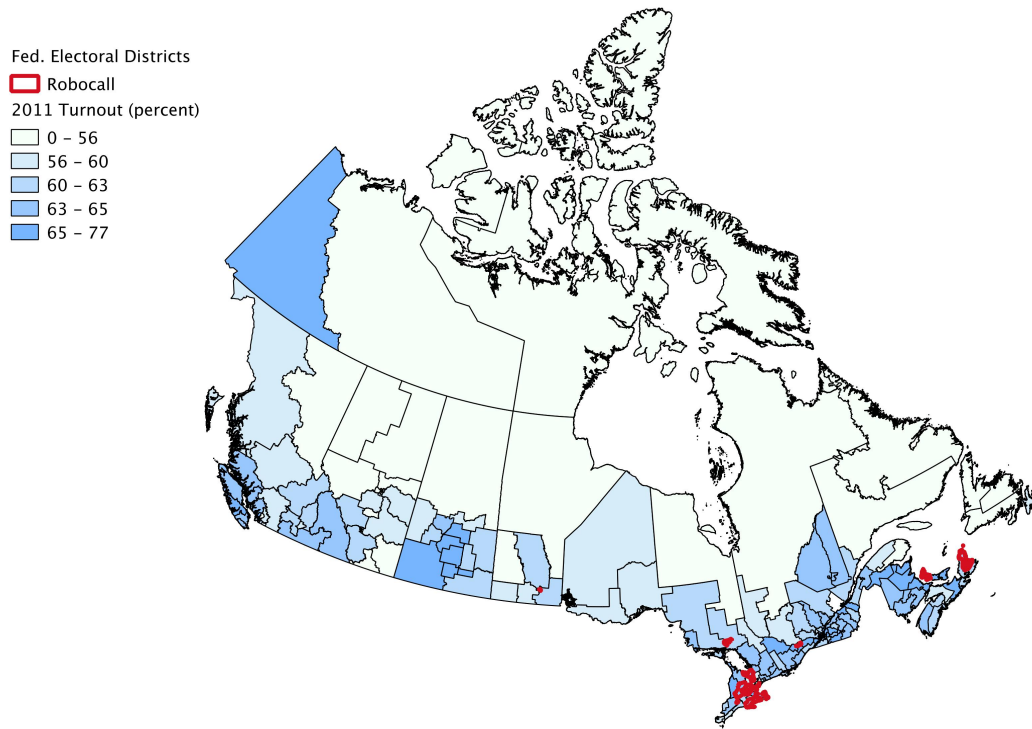
must also have detailed information about the specific example they are investigating. In our case, our results depend on a constant treatment probability among opposition voters within treated districts (a plausible assumption when voters are contacted by phone), but other violations may have a different distribution of treatment. However, if researchers understand what these impacts are likely to be, it should be possible to tailor our method to other circumstances.

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Figure 1: Canadian Federal Electoral Districts



*

Table 1: Summary Statistics

Variable	Obs.	Min	Max	Mean	Median	St. Dev
registered voters	308	17,349	153,438	81,389.77	82,558	16,398.77
total votes cast	308	7,931	90890	49,866.84	50,584	16,389.15
winning margin (votes)	308	9	41,691	11,195.14	9,711	8,546.23
robocall identifier	308	0	1	.08982	0	.2859
number of polling stations	308	54	413	232.52	233	38.45
opponent vote share 2008 (%)	308	18.85	128.28	72.72	70.74	23.80

^a **Note:** Summary statistics of key variables at the electoral district level in the 2011 federal election.

Opponent vote share is the combined number of votes for parties other than the Conservatives, divided by the total number of votes cast.

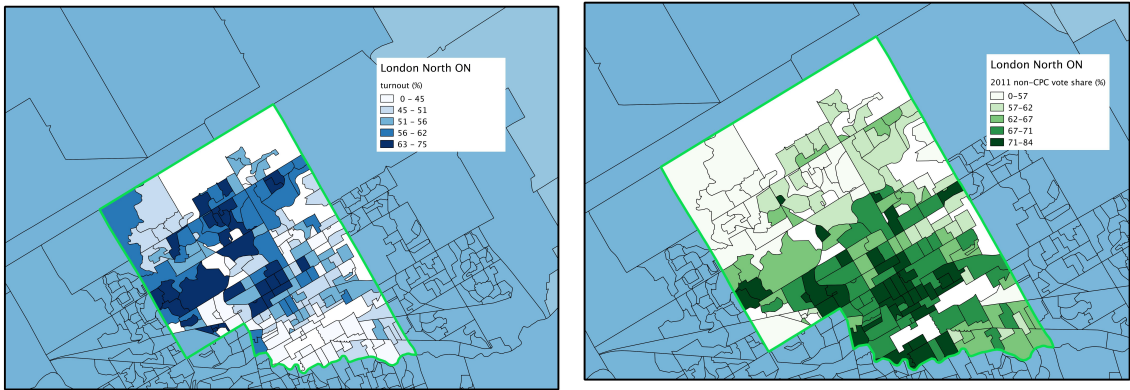


Figure 2: The district of London North, ON

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Table 2: The Effect of Robocalls on Voter Turnout in 2011

	coefficient	robust standard error
A. Cross-Section Regression on District Level		
lagTurnout	.604***	(.054)
lag ₂ Turnout	.308***	(.073)
lagMargin	-.000***	(.000)
lagOppvoteshare	-.017	(.015)
robocall	.006	(.006)
number of observations		306
B. Within District Estimates		
lagTurnout	.574***	(.015)
lag ₂ Turnout	.277***	(.015)
lagOppvoteshare	-.099***	(.015)
robocall×lagOppvoteshare	-.070**	(.030)
district fixed effects		yes
number of observations		44,750

^a **Note:** The standard errors reported in parentheses are clustered at the district level for the regression in B. Superscripts ***, ** and * indicate significance at 1%, 5%, and 10 % respectively.

*

*

Table 3: Sensitivity Analysis

	estimated δ	robust st.err.	# of obs.
(1) original specification	-.070**	(.0307)	44,750
(2) saturated lag structure	-.099**	(.0384)	44,750
(3) one lag of turnout	-.076**	(.0327)	54,080
(4) DiD specification	-.170**	(.071)	144,015
(5) weight polls by size	-.079**	(.016)	44,750
(6) restrict sample to Ontario	-.044	(.0303)	16,483
(7) restrict sample to close races	-.109**	(.0477)	9,731
(8) extended list of 102 ridings	.005	(.0122)	44,750
(9) current opposition vote share	-.091***	(.0393)	44,751
(10) absolute value of turnout	-.038**	(.015)	44,750
(11) gls with logit link	-.162***	(.062)	44,742

^a **Note:** All entries represent estimates from specification in Panel B of Table 2 that includes district fixed effects. The standard errors reported in parentheses are heteroskedasticity-robust, and clustered at the district level. Superscripts ***, ** and * indicate significance at 1%, 5%, and 10 % respectively.

Table 4: Turnout and Campaign Spending

	coefficient	robust standard error
lagTurnout	.575***	(.0150)
lag ₂ Turnout	.279***	(.0146)
lagOppvoteshare	-.101***	(.0159)
robocall×lagOppvoteshare	-.067**	(.0316)
Cspending×lagOppvoteshare	-.00002	(.0007)
district fixed effects		yes
number of observations		44,750

^a **Note:** Cspending is the amount of money spend in the 2011 electoral campaign in district j , as reported by the Conservative candidates themselves to *Elections Canada*, measured in Canadian dollars. The standard errors reported in parentheses are clustered at the district level. Superscripts ***, ** and * indicate significance at 1%, 5%, and 10 % respectively.

Table 5: Falsification Tests Using the 2008 and 2006 Elections

	coefficient	robust standard error
A. 2008 General Election		
lagTurnout	.538***	(.015)
lag ₂ Turnout	.379***	(.014)
lagOppvoteshare	-.084***	(.020)
robocall×lagOppvoteshare	0.007	(.086)
riding fixed effects		yes
number of observations		45,183
B. 2006 General Election		
lagTurnout	.696***	(.011)
lagOppvoteshare	.056***	(.016)
robocall×lagOppvoteshare	.054	(.039)
riding fixed effects		yes
number of observations		45,949

^a **Note:** The standard errors reported in parentheses are clustered at the district level. Superscripts ***, ** and * indicate significance at 1%, 5%, and 10 % respectively.

Appendix

List of 27 electoral districts where *Elections Canada* received reports of false or misleading phone calls during the 2011 General Election, as released by interim Liberal Party leader Bob Rae on February 26 2012.²⁹

1. Sydney-Victoria (N.S.): Winner: Liberals; Margin of victory: 765 votes
2. Egmont (P.E.I.): Winner: Conservatives; Margin of victory: 4,470 votes
3. Eglinton-Lawrence (Ont.): Winner: Conservatives; Margin of victory: 4,062 votes
4. Etobicoke Centre (Ont.): Winner: Conservatives; Margin of victory: 26 votes
5. Guelph (Ont.): Winner: Liberals; Margin of victory: 6,236 votes
6. Cambridge (Ont.): Winner: Conservatives; Margin of victory: 14,156 votes
7. Hamilton East-Stoney Creek (Ont.): Winner: NDP; Margin of victory: 4,364 votes
8. Haldimand-Norfolk (Ont.): Winner: Conservatives; Margin of victory: 13,106 votes
9. Kitchener-Conestoga (Ont.): Winner: Conservatives; Margin of victory: 17,237 votes

²⁹Source: Yahoo news.

See <http://ca.news.yahoo.com/blogs/canada-politics/robocall-scandal-could-lead-elections-202108363.html>

10. Kitchener-Waterloo (Ont.): Winner: Conservatives; Margin of victory: 2,144 votes
11. London North Centre (Ont.): Winner: Conservatives; Margin of victory: 1,665 votes
12. London West (Ont.): Winner: Conservatives; Margin of victory: 11,023 votes
13. Mississauga East-Cooksville (Ont.): Winner: Conservatives; Margin of victory: 676 votes
14. Niagara Falls (Ont.): Winner: Conservatives; Margin of victory: 16,067 votes
15. Oakville (Ont.): Winner: Conservatives; Margin of victory: 12,178 votes
16. Ottawa Orleans (Ont.): Winner: Conservatives; Margin of victory: 3,935 votes
17. Ottawa West-Nepean (Ont.): Winner: Conservatives; Margin of victory: 7,436 votes
18. Parkdale-High Park (Ont.): Winner: NDP; Margin of victory: 7,289 votes
19. Perth-Wellington (Ont.): Winner: Conservatives; Margin of victory: 15,420 votes
20. Simcoe-Grey (Ont.): Winner: Conservatives; Margin of victory: 20,599 votes
21. St. Catharines (Ont.): Winner: Conservatives; Margin of victory: 13,598 votes
22. St. Paul's (Ont.): Winner: Liberals; Margin of victory: 4,545 votes
23. Sudbury (Ont.): Winner: NDP; Margin of victory: 9,803 votes
24. Wellington-Halton Hills (Ont.): Winner: Conservatives; Margin of victory: 26,098 votes
25. Willowdale (Ont.): Winner: Conservatives; Margin of victory: 932 votes
26. Saint Boniface (Man.): Winner: Conservatives; Margin of victory: 8,423 votes
27. Winnipeg South Centre (Man.): Winner: Conservatives; Margin of victory: 8,544 votes

List of 102 electoral districts with some additional information where according to media sources and reports from the Liberal Party of Canada or the New Democratic Party of Canada, voters received misleading or harassing phone calls during the 2011 General Election. Dated March 29, 2012.³⁰

1. Ajax-Pickering, reported by Hill Times.
2. Ancaster-Dundas-Flamborough, reported by National Post.
3. Barrie, reported by National Post (calls impersonating Elections Canada and directing voters to bogus polling locations).
4. Bas-Richelieu-Nicolet-Becancour, reported by National Post.
5. Beaches-East York, reported by Globe & Mail.
6. Beausejour, reported by CBC.
7. Brampton West, reported by National Post
8. Burnaby-Douglas, reported by Burnaby Now (calls impersonated *Elections Canada* and misdirected voters).
9. Burnaby-New Westminster, reported by Burnaby Now.
10. Calgary Centre, reported by Hill Times.
11. Cambridge, reported by private citizen (Postmedia: "harassing phone calls").
12. Cariboo-Prince George, reported by Prince George Citizen (harassment calls).
13. Chilliwack-Fraser Canyon, reported by National Post.
14. Davenport, reported by NDP.
15. Don Valley East, reported by National Post.
16. Dufferin-Caledon, reported by Orangeville Banner.
17. Edmonton Centre, reported by NDP (CBC: phone calls misdirected voters to wrong polling stations).
18. Edmonton East, reported by NDP (fake live calls impersonating *Elections Canada*, misdirecting voters. Postmedia: some live calls originally claimed to be from *Elections Canada*, then when pressed, said they were actually from a Conservative call centre.)
19. Eglinton-Lawrence, reported by Liberals (Fake Liberal calls targeted Jewish voters on Saturdays, and even accidentally phoned the Liberal riding phone bank, which has sworn out an affidavit.)
20. Egmont, reported by Liberals (Postmedia: live callers pretended to represent Liberal candidate, but mispronounced his name).

³⁰Source: The Sixth Estate. See <http://sixthestate.net/?p=3646>

21. Elmwood-Transcona, reported by NDP (A formal complaint has been sent to *Elections Canada* over phone calls claiming voting locations had changed.)
22. Esquimalt-Juan de Fuca, reported by campaign volunteer to Sixth Estate (overnight calls impersonating the Liberal Party).
23. Essex, reported by NDP (National Post: robocalls misdirected voters).
24. Etobicoke Centre, reported by Liberals (a court case will begin in April to hear allegations that Conservatives temporarily shut down a polling station and harassed Liberal voters. See also Global News).
25. Fredericton, reported by private citizen (CBC: Phone number connected to the Conservative Party attempted to misdirect voters to wrong polling station).
26. Guelph, reported by Liberals. Guelph is the centre of most of the allegations; this riding received widespread reports of both hoax night-time phone calls claiming to be Liberals, and election-day calls claiming voting locations had changed.)
27. Haldimand-Norfolk reported by Liberals (Postmedia: harassing overnight calls impersonated the Liberal Party)
28. Haliburton-Kawartha Lakes-Brock, reported by Liberal candidate.
29. Halton, reported by *Elections Canada*: election-day robocalls misdirected voters.
30. Hamilton Centre, reported by NDP.
31. Hamilton East-Stoney Creek, reported by Liberals
32. Kelowna-Lake Country, reported by Conservatives
33. Kingston and the Islands, reported by Liberals (CBC: Callers impersonating Liberal Party misdirected voters to wrong voting locations on election day.)
34. Kitchener Centre, reported by voting officer ("a lot" of electors were called and told their polling stations had changed).
35. Kitchener Waterloo, reported by *Elections Canada*
36. Kitchener-Conestoga, reported by private citizen (election-day robocalls misdirected voters).
37. Lac Saint Louis, reported by Liberals (Cyberpresse: Voters received misdirection calls.)
38. Lanark-Frontenac-Lennox and Addington, reported by National Post.
39. London North Centre, reported by Liberals (Postmedia: Telephone campaign falsely informed listeners that the Liberal candidate spent half of each year in Africa).
40. London West, reported by Liberals (Local radio: MP3 recording of an alleged hoax robocall

attempting to misdirect a voter).

41. Malpeque, reported by PEI Guardian (misleading calls).
42. Markham-Unionville, reported by NDP, reported by National Post.
43. Mississauga East-Cooksville, reported by Liberals
44. Mississauga-Streetsville, reported by National Post.
45. Mount Royal, reported by Liberals. CBC (election-day robocalls misdirected voters).
46. Nanaimo-Alberni, reported by NDP (Parksville News: phone calls misdirected voters).
47. Nanaimo-Cowichan, reported by Sixth Estate (calls misdirecting voters).
48. New Westminster-Coquitlam, reported by Royal City Record.
49. Niagara Falls, reported by Liberals (Postmedia: overnight callers impersonated Liberal Party)
50. Nipissing Timiskaming, reported by Liberals (CBC: Calls impersonating *Elections Canada* misdirected voters to the wrong locations.)
51. North Vancouver, reported by private citizen (Postmedia: election-day robocalls misdirected voters.)
52. Northumberland-Quinte West, reported by Trentonian (misleading and harassing calls).
53. Oak Ridges-Markham, reported by National Post.
54. Oakville, reported by Liberals (Postmedia: callers with "fake accents" pretended to represent Liberal candidate.)
55. Ottawa Centre, reported by NDP.
56. Ottawa Orleans, reported by Liberals (OpenFile: election-day robocalls impersonated *Elections Canada* and misdirected voters. Ottawa Citizen: fake callers misdirected voters.)
57. Ottawa-Vanier, reported by CBC (misdirection calls and harassment calls).
58. Ottawa West-Nepean, reported by Liberals (Postmedia: election-day calls misdirected voters).
59. Outremont, reported by Sixth Estate.
60. Parkdale-High Park, reported by Liberals and by NDP (Postmedia: overnight callers impersonated the Liberal Party. National Post: robocalls misdirected voters).
61. Perth-Wellington, reported by Liberals.
62. Peterborough, reported by Conservatives.
63. Pierrefonds-Dollard, reported by Liberals (CBC: Election-day calls misdirected voters).
64. Pitt Meadows-Maple Ridge-Coquitlam, reported by private citizen (CBC: Conservative call centre contacted a woman who had previously told them she would be voting NDP, and told her

that her polling station had changed.)

65. Prince George–Peace River, reported by *Elections Canada* (election-day robocalls misdirected voters).
66. Regina-Lumsden-Lake Centre, reported by private citizen (election-day calls misdirected voters).
67. Richmond Hill, reported by Liberal Party (misdirection calls).
68. Saanich-Gulf Islands, reported by Greens (See also Maclean’s. Toronto Star: election-day live calls misdirected voters.)
69. Saint Boniface, reported by Liberals (Postmedia: callers impersonated the Liberal Party).
70. Saint John, reported by private citizen (CBC: calls impersonated *Elections Canada* and misdirected voters).
71. Sarnia-Lambton, reported by Sun Media (RMG telephone calls misdirected voters to the wrong polling station)
72. Saskatoon-Rosetown-Biggar, reported by Council of Canadians.
73. Sault Ste Marie, reported by National Post.
74. Scarborough Southwest, reported by National Post.
75. Scarborough-Rouge River, voting irregularities reported by Conservative Party.
76. Simcoe-Grey, reported by Liberals.
77. South Shore-St. Margaret’s, reported by NDP (Chronicle-Herald: election-day robocalls misdirected voters).
78. St. Catharines Conservatives by 8822 Conservatives by 13,598 Reported by Liberals. National Post: alleges live calls misdirect voters.
79. St. Paul’s, reported by Liberals (National Post: robocalls misdirect voters).
80. Sudbury, reported by Liberals and NDP.
81. Sydney-Victoria, reported by Liberals (Chronicle Herald: fake Liberals and anonymous robocallers misdirected voters).
82. Timmins-James Bay, reported by private citizen.
83. Trinity-Spadina, reported by National Post.
84. Thunder Bay-Superior North (CBC: calls misdirect voters to wrong polling stations).
85. Toronto-Danforth, reported by CBC (misleading calls).
86. Vancouver Centre, reported by private citizen (misleading call).
87. Vancouver East, reported by NDP to *Elections Canada* in June 2011.

88. Vancouver Island North, reported by CHEK TV (election-day calls misdirected self-identified NDP and other voters).
89. Vancouver Kingsway, reported by National Post
90. Vancouver Quadra, reported by Liberals Postmedia: Late-night phone calls impersonated Liberal Party.
91. Vancouver South, reported by Liberals (CBC: overnight phone calls)
92. Vaughan, reported by iPolitics.ca (financial misconduct and campaign irregularities).
93. Wascana, reported by Liberals (Global News: overnight live calls).
94. West Nova, reported by CBC (election-day calls misdirected voters to nonexistent polling locations).
95. Willowdale, reported by Liberals (CBC: Calls impersonated Liberal Party).
96. Windsor West, reported by Liberals (Windsor Star: "similar" phone calls to other ridings).
97. Windsor-Tecumseh, reported by NDP.
98. Winnipeg Centre, reported by private citizens (Winnipeg Free Press: election day robocalls misdirected voters).
99. Winnipeg South, reported by NDP.
100. Winnipeg-South Centre, reported by Liberals (National Post: robocalls and live calls misdirected voters).
101. York Centre, reported by National Post (misleading calls).
102. Yukon, reported by CBC (calls with false information about polling stations).