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ABSTRACT

Does Misinformation Demobilize the Electorate? Measuring the Impact of Alleged 'Robocalls' in the 2011 Canadian Election*

The paper presents evidence on the effect of voter demobilization in the context of the Canadian 2011 federal election. Voters in 27 ridings (as of February 26, 2012) allegedly received automated phone calls (`robocalls') that either contained misleading information about the location of their polling station, or were harassing in nature, claiming to originate from a particular candidate in the contest for local Member of Parliament. We use within-riding variation in turnout and vote--share for each party to study how turnout changed from the 2008 to the 2011 election as a function of the predominant party affiliation of voters at a particular polling station. We show that those polling stations with predominantly non-conservative voters experienced a decline in voter turnout from 2008 to 2011, and that this effect was larger in ridings that were allegedly targeted by the fraudulent phone calls. The results thus indicate a statistically significant effect of the alleged demobilization efforts: in those ridings where allegations of robocalls emerged, turnout was an estimated 3 percentage points lower on average. This reduction in turnout translates into roughly 2,500 eligible (registered) voters that did not go to the polls. The 95%-confidence interval gives a lower bound estimate of 1,000 fewer votes cast in robocall ridings, which is still a sizable effect.

JEL Classification: D72
Keywords: Canadian election 2011, vote suppression, voter demobilization, voter turnout

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1 Introduction

In mid February 2012, news broke that Elections Canada, an independent, non-partisan agency with a mandate to monitor compliance and enforce electoral legislation, was investigating complaints about alleged attempts to suppress votes in the 2011 federal elections. The still ongoing probe seeks to identify who was responsible for automated phone calls, so-called "robocalls', that were designed to misdirect voters to the wrong polling stations, or discourage voting for a particular party. The calls appear to have targeted voters with a Liberal Party or New Democratic Party (NDP) affiliation. According to Elections Canada, the calls contained false information regarding changes to voting locations, or harassing messages made on behalf of a contender in the election for local member of parliament.[1]

Initially, the number of ridings under investigation was 18, but after the media had picked up the story and the public had learned about the probe, more voters came forward with complaints and the list of allegedly affected ridings had grown to 27 by February 26, and further grew to over 50 ridings in early March. At this point in time, Elections Canada confirmed that it had received over 30,000 reports about the deceptive and harassing phone calls.[2] A poll that was conducted from March 6 to 8 2011 found that four percent of Canadians – roughly one million individuals – strongly agree that in the last federal election they received a call that deliberately tried to confuse them about the polling station at which they were to vote.[3]

Demobilization efforts using misinformation is a phenomenon that is not confined to Canada: vote suppression techniques have received a considerable amount of media attention in the United States, where these strategies appear to be common and are widely discussed. On

[1]The political leanings of individual voters were apparently identified by earlier calls asking them whether they would support the Conservative candidate. Following a negative answer, voters allegedly received a second call claiming to originate from Elections Canada and falsely informing them about a change in their polling station [Source: "The National" TV broadcast. March 15th, 2012]. In Canada, a person can only vote at a poll where his or her name is included in the list of electors for the respective polling division in which he or she is ordinarily resident. See the Canada Elections Act, http://elections.ca/content.aspx?section=res&dir=loi/fel/cea&document=index&lang=e.

[2]The National Post (March 2nd, 2012). http://news.nationalpost.com/2012/03/02/tory-robocalls-counterattack-backfires-as-elections-canada-confirms-31000-complaints/

[3]See http://www.northumberlandview.ca/index.php?module=news&func=display&sid=13679. The poll was conducted by Ipsos Reid on behalf of Postmedia News and Globan News. For the survey, a representative randomly-selected sample of 1,001 adult Canadians was interviewed by telephone, and 2,153 interviews were conducted online via the Ipsos I-say panel. Ipsos merged the two sample sources and employed weighting to balance demographics and ensure that the samples composition reflected that of the adult population according to Census data. A survey with an unwieghted probability sample of this size and a 100 % response rate would have an estimated margin of error of sample of 2 percentage points.

election night in November 2010, for example, when the polls were still open, around 15,000 registered Democrats in Maryland were informed that "President Obama won the election" and that "the only thing left is to do is watch TV".[4]

The purpose of this paper is twofold. First, we want to assess whether in fact the ridings that were affected by the 'robocalls' had significantly lower turnout – overall or by party affiliations – than those ridings that were not affected. Second, if the answer is affirmative, we seek to provide an estimate of the number of total voters per riding that were discouraged from going to the polls, i.e., we seek to estimate the effectiveness of the misinformation strategy. The problem with identifying any possible causal effect of the robocalls is that a crucial variable that likely affects both turnout *and* the selection of ridings that were targeted is the expected margin of victory, which is unobserved. Instead of proxying for the perceived margin of victory by the actual margin (or the margin in the previous election), we employ a different approach that allows for arbitrary unobservables at the level of the riding and instead uses *within riding variation* for identification.

Specifically, our identification strategy uses the considerable variation of outcomes (turnout, vote shares) at the level of the polling station within a riding: On average, an electoral district has roughly 250 polling stations; in some voters predominantly supported the Conservatives in the 2008 election, while in others, voters predominantly supported the main opposition parties (Liberals, NDP, Bloc). In the 2011 election, turnout at the latter polling stations fell, while turnout at stations with more Conservative leaning voters increased, relative to the district average. This effect could be due to the failure of the main opposition parties to mobilize their supporters as effectively as the Conservatives. Now assume that the instigator behind the robocalls targeted opposition voters evenly within the riding. Because some polling stations have more eligible voters favoring the opposition than others, we would expect those polling stations to have a differentially lower turnout compared to similar polling stations in ridings that were unaffected, assuming the misinformation worked and supporters of the main opposition parties were in fact demobilized. In other words, comparing ridings with robocall complaints to ridings without, the decline in turnout at those polls with more Liberal NDP or Bloc support should be *more pronounced* in the former.

[4]As reported in the Washington Post (November 5, 2010). The message, delivered through an automated phone call, was apparently paid for by Universal Elections, a firm with links to the Republican gubernatorial candidate. See Barton (2011) for a list of the wide range of demobilization tactics that have been observed in the U.S., and further examples.

The results suggest that this is indeed the case: those electoral districts that were allegedly targeted by robocalls experienced a (relative) drop in voter turnout. On average, voter turnout was 3 percentage points lower in those ridings from which complaints had been received as opposed to ridings from which no such complaints had been received. Using the average such riding as a benchmark, this translates into roughly 2,500 fewer voters at the polls in each riding on our robocall list.

It is important to note that our findings in cannot 'prove' whether misconduct or an illegal act has occurred. First, since we can only rely on self-reported incidences of misleading or harassing calls, which are made available through the Canadian media as my data source, the data might be a subject to a considerable amount of measurement error as the actual occurrence of misconduct is obviously not observed. That is, we can only estimate the effect of (the average level of) robocalling in a riding, conditional on the calls being reported, relative to the effect of (the average level of) robocalling in districts where the calls, if any, have *not* been reported and consequently do not appear on the robocall list. Second, the findings only apply to the artificial construct of an "average" riding, i.e., the interpretation of the results necessitates an electoral district with average characteristics (voter turnout, margin of victory, etc.) which does not actually exist. For this reason, we wish to emphasize that the analysis and the corresponding results are not suited to bringing the outcome in a particular riding into question.

The data we use are for the past three Canadian federal elections (2011, 2008, 2006), and can be downloaded from the Elections Canada website. We obtained the list of those ridings that were allegedly targeted for voter demobilization using automated phone calls from the popular press and from the websites of the Liberal Party and the New Democratic Party. As mentioned above, the list is growing over time. For reasons laid out below, the main analysis employs the list as it was compiled a week into the robocall-scandal, with 27 electoral districts affected, dated February 26 2012.[5] The current count of affected ridings is 77, and it is not unlikely that it will continue to increase.

There has been a fair amount of research on the determinants of voter turnout and voter mobilization, both in the economics and political science literature.[6] Following the pioneering work of Gerber and Green (2000), one large strand of the literature studies the efficacy

[5]See Appendix B for details.
[6]See Geys (2006) for a comprehensive survey of the literature.

of "Get Out the Vote" (GOTV) campaigns based on randomized field experiments[7] 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.[8] In contrast, experiments testing impersonal GOTV methods such as mass email [Nickerson (2007)] and robo calls [Green and Karlan (2006) and Ramirez (2005)] find no statistically significant effects on voter turnout.

The second and smaller 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. The 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) in a field experiment and in an aggregate study of the U.S. Senate elections in 1992 find that negative campaigning significantly reduces turnout at the polls. Subsequent studies reached more optimistic conclusions, finding no evidence in support of the demobilization hypothesis [see e.g. Finkel and Geer (1998), Freedman and Goldstein (2002) and Clinton, and Lapinski (2004)].[9] For obvious reasons, though, this literature is confined to legal means in campaign advertising and to messages that are not openly intended to suppress turnout. To our knowledge, there are no hard data on the effect of intentional demobilization on turnout. 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 yes, 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.

The rest of this paper is structured as follows. The next section describes the data and the

[7]See their book Green and Gerber (2004) for additional information and further references.

[8]The study by Arceneaux and Nickerson (2010) also tests for the difference between the effects of a negative tone in the message and a positive tone in the message, and find no systematic difference between the two.

[9]A meta-analysis conducted by Lau et al (1999) concluded that there were no clear effects of campaign tone on turnout.

identification strategy. Section 3 presents the estimation results, while Section 4 presents some robustness checks and discusses possible caveats. A final Section 5 concludes.

2 Data and Empirical Strategy

The empirical analysis is based on a sample that includes the official election results for the 41st General Election (May 2, 2011), and the results from the previous two general elections as controls, the 40th General Election (October 14, 2008) and the 39th General Election (January 23, 2006). The data are available on the Elections Canada website http://www.elections.ca/. For each election and electoral district, the dataset includes 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 is a total of 308 ridings. We flag each riding with an indicator variable that equals one if media sources reported misleading or harassing phone calls in that riding, and zero otherwise. Ideally, of course, one would want to use the set of ridings where Elections Canada confirmed incidences of illegal activities. We might have more information on the alleged misconduct at later points in time, but at the moment we 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), 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. Not only do we not observe whether fraudulent calls actually occurred, we don't even observe whether or not a complaint has been made since Elections Canada does not disclose any information regarding an ongoing investigation. 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 is primarily composed of ridings that were at the time reported to be under investigation by Elections Canada and that was leaked to the press. 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, an early list (which was apparently leaked from a source inside

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.

Elections Canada) is much more reliable. As we will see below, the estimated effect becomes much smaller in value and insignificant if one instead uses lists that emerged at later points in time.[10]

The dependent variable is voter turnout, defined as the percentage of the registered voters who actually cast their vote in the 2011 federal election, by electoral district and polling station. That is, we take the absolute number of people voting at polling station i in riding j and divide it by the absolute number of registered voters for that polling station and riding.

Table 1 above summarizes the descriptive statistics:

The primary challenge when estimating the impact of possible misinformation and harassment on voter turnout is that the districts that were subject to the phone calls do not necessarily constitute a random (representative) sample. In particular, one plausible selection criterion for anyone who deliberately sought to suppress the vote is the expected margin of victory, i.e., those districts where the race was expected to be close (and thus the impact of any calls likely largest) could have been deliberately targeted. The data are in support of 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, there is ample evidence in the economic and political science literature that some form of 'closeness' of the election, usually measured by the (percentage) vote gap

[10] See Appendix A. This is in line with the suspicion that observations from ridings which were added to the list at later points in time are somewhat less trustworthy.

between the first and second candidate in the race, has a significant and positive impact on voter turnout. This holds regardless of whether the studies use the actual election result as a basis of the closeness variable or proxy the estimated closeness of the election by using previous election results, opinion polls, or newspaper reports.The 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.[11]

The observed correlation between incidence of robocalls and closeness of the race on the one hand, and the known link between closeness of the race and electoral turnout on the other hand, implies for instance that if there was no causal effect of robocalls on turnout, we would expect a *higher* turnout in those districts affected. Indeed, this pattern is present in the data: in the 2011 election, turnout in allegation-free ridings was an average of 48,079 votes or 52.1 percentage points. In contrast, in robocall ridings an average of 53,014 went to the polls, or 53.3 percent of registered voters.

Thus, the problem is one of selection bias: an unobserved variable that likely determined the selection of targeted ridings, namely the estimated or perceived margin of victory, also affects voter turnout. One natural way to address the selection bias in this context given the available data 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 co-variates such as (lagged) margin of victory and changes is population demographics. This identification method, however, essentially proxies the unobserved estimated voter turnout with actual voter turnout in the past election, which constitutes only an imperfect measure of what voters and robocall initiators presumably based their decisions on.

For this reason, we employ a slightly different strategy: instead of using between-ridings

[11] See Geys (2006). for a comprehensive survey of the literature. Besides closeness of the election, other significant determinants of voter turnout are the amount spent on political advertising, the degree of polarization, demographic characteristics of the electorate, as well as institutional variables such as electoral system, compulsory voting laws, or registration requirements.

variation to identify the effect of alleged misconduct, we use *within* 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] Specifically, we estimate a regression model of the form

$$Y_{ij} = \gamma_j + \beta_1 lagY_{ij} + \beta_2 lagoppvoteshare_{ij} + \beta_3\big(robocall_i \times lagoppvoteshare_{ij}\big) + \epsilon_{ij} \quad (1)$$

where Y_{ij} is voter turnout (in percent) in the 2011 federal election at polling station i in riding j, $lagY_{ij}$ is voter turnout at the same polling station in the 2008 federal election, $lagoppvoteshare_{ij}$ is the combined vote share of all non-conservative candidates at this polling station in the 2008 election relative to total votes cast at this station, and γ_j are electoral district fixed effects[13]

The coefficient on the interaction term $robocall_i \times lagoppvoteshare_{ij}$ is the parameter of interest. To understand the idea behind the strategy intuitively, note that the share of combined votes that non-Conservative candidates can secure in an election will vary from polling station to polling station: some neighborhoods 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. If β_2 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. The coefficient on the interaction term, β_3, 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 β_3 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.

[12] Naturally, 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.

[13] There is a number of polling stations that do not match up from 2008 to 2011 because they are split up in some way, or rejoined. Those are dropped in the estimations that follow.

Note that this strategy, too, represents a difference-in-differences approach, where we compare the relative outcomes at polling stations within a district from the 2008 and the 2011 election (the first difference) across districts that reported receiving robocalls and those that did not (the second difference). 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* in 2011. This assumption implies that in the absence of any misconduct, polls with similar (non-)Conservative margins within the same district should have seen a similar change in turnout from the 2008 to the 2011 election.

Two 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 robocalling was already reported in 2008. We also dropped all absentee and mobile polls, where the logic of the identification strategy should not apply. Another potential problem is that there are substantial differences in the number of polling stations 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 proportional to the number of votes cast, 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. The heteroskedasticity-robust standard errors (in parenthesis) are clustered at the electoral district level. 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 from in the 2008 election: voter turnout (lagturnout), the percentage margin of victory (lagmargin) and the combined percentage share of non-Conservative votes (lagoppvoteshare). Part B present the difference-in-differences estimates with additional controls, including electoral district fixed effects.

It is instructive to start with the cross-section regression: we see that the dominant determi-

[14]See Appendix A. for the results without the weighting function. The point estimates are even higher than in the weighted regressions.

Table 2: Voter Turnout and Robocalls in 2011

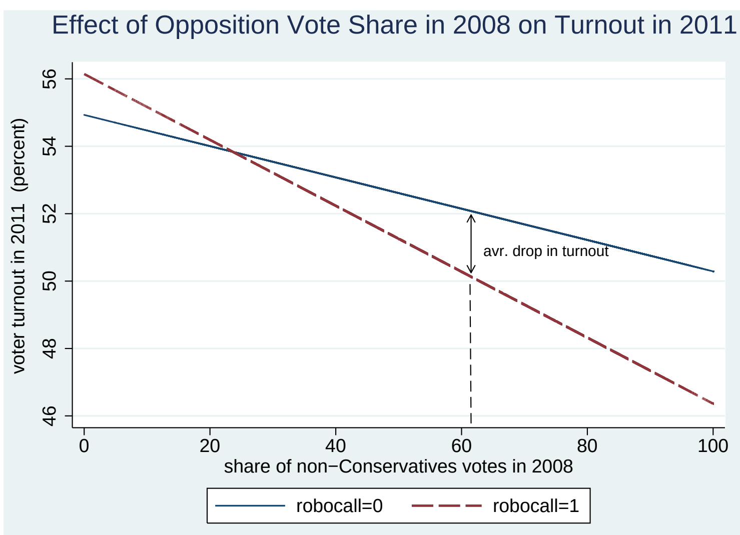
	coefficient	standard error
A. Cross-Section Regression on District Level		
lagturnout	.88**	(.031)
lagmargin	−.000463**	(.0000172)
lagoppvoteshare	−.019**	(.0063)
robocall	1.06**	(.402)
number of observations		272
B. Within District Difference-in-Difference Estimates		
lagturnout	.734**	(.0119)
lagoppvoteshare	−.046**	(.007)
robocall×lagoppvoteshare	−.051**	(.0154)
riding fixed effects		yes
number of observations		59,373

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

nant of turnout in riding j in the 2011 election was turnout in the same riding in 2008. The coefficient of the winning margin (closeness of the election) in 2011 is negative, as expected, but small in absolute value. What matters more is the political orientation of a riding: as indicated by the negative and significant coefficient of the lagged *opponentvoteshare*, the larger the share of non-Conservative votes in 2008, the larger the drop in turnout in 2011. The coefficient on the robocall dummy, in contrast, is positive and highly significant, i.e., electoral districts that experienced robocalls had higher turnout than those which didn't. Read in this way, robocalls increased voter turnout. As discussed earlier, however, this observed positive correlation could be driven by the estimated winning margin just prior to the election: it would emerge if, for instance, the instigator of the calls picked the ridings that were expected to be close races, and if voters in those ridings were more likely to go to the polls because they felt that their vote matters more. Controlling for the lagged winning margin does not fully address this problem because the margin of the winning candidate in the 2008 election does not correspond perfectly to the perceived (estimated) margin prior to the 2011 election.

The identification strategy addresses this issue in a natural way through introducing electoral district fixed effects, which absorb any unobserved differences at the district level. Looking at Table 1 part B, we see that at the level of a polling station, previous turnout still matters most. The combined vote share of the non-Conservative candidates in the prior election also continues to determine voter turnout: as at the riding level, 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. The coefficient on the interaction term signifies that this effect is more severe in ridings with alleged misconduct. In other words, 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 .051, which 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 5.1 percentage points less turnout in those ridings where robocalls were reported. The effect is illustrated in Figure 1.

Figure 1: Differential Impact of Robocalls in Polls depending on Opposition Vote Share

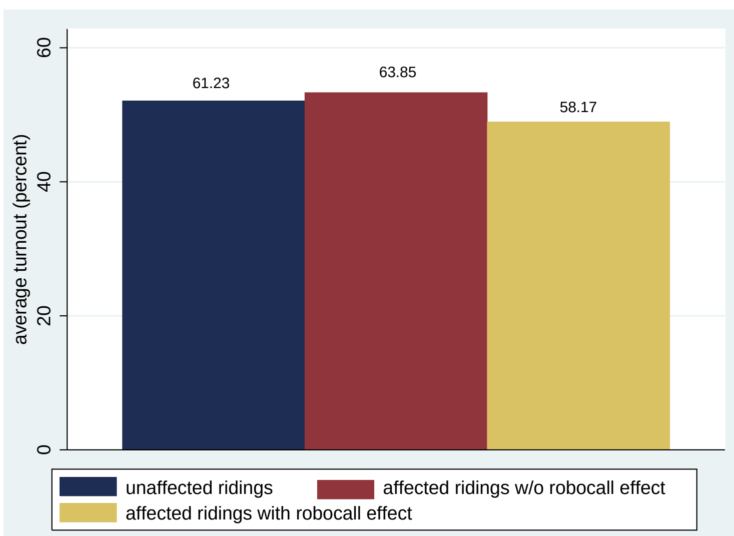


To better assess the magnitude of this effect, we can take the mean lagged combined vote share of all opponents to the Conservative candidate in the affected districts, which was

60 percent, and multiply it by the coefficient estimate.[15] The resulting figure of -3.0 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 83,268 registered voters, this translates into an estimated absolute number of roughly fewer 2,500 voters showing up at the polls. This is substantial. Of those ridings on our list allegedly affected by robocalls, a total of 6 had winning margins smaller than that. The lower bound of the 95% confidence interval is 1,032 voters that did not vote in robocall ridings on election day, an amount which is still larger than the winning margin in three ridings (two of which were won by a Conservative candidate).[16]

Figure 2 provides an illustration. We see that if we exclude the measured impact of robocalling in those ridings that were affected, their turnout is higher than in those ridings where robocalling was not reported, which reiterates the findings in Panel A of Table 2. Taking the measured effect of alleged robocalls into account, however, leads to a decline in turnout.

Figure 2: Effect of Reported Robocalls on Voter Turnout in Percentage Points



[15] Alternatively, we could use the mean lagged combined vote–share on the polling station level, which is considerably lower, namely 55.2. Arguments can be made for using either figure. If we use the polling station level as benchmark, we'd weigh each polling station equally, which obviously is not correct as the number of polling stations and the number of voters per polling station varies considerably. District averages avoid this problem.

[16] The point estimate is -0.051. The lagged Opposition vote share at the district level has a mean of 60% and a standard deviation of 15.4 (95% confidence t-stat is 1.97).

4 Robustness Checks

This section present results from sensitivity analysis exercises carried out to check the robustness of the basic results, and discusses possible weaknesses in the identification strategy.

4.1 Sensitivity Analysis

Table 3 displays the estimated coefficient of the robocall-interaction term using variations on the basic specification of Panel B in Table 2.

Table 3: Sensitivity Analysis

	estimated β_3	st.err.	# of obs.
(1) original specification	$-.051^{**}$	(.015)	59,373
(2) absentee, advance, and mobile polls	$-.175$	(.206)	1,695
(3) restricted sample of close races	$-.059^{*}$	(.027)	16,715
(4) incl. pollstation weights	$-.042^{**}$	(.0163)	59,373
(5) extended list of ridings[17]	$-.007$	(.0137)	59,373

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

For ease of reference, row (1) repeats the results from Table 2. The first robustness check is a counterfactual analysis. If the identification strategy is correct, reported robocalls should have no affect on advanced polls and absentee polls. Similarly, the relationship between the share of votes in favor of the opposition in 2008 and and turnout in 2011 should also not be present among mobile polls, where the electorate is not stable over time. The second row (2) presents the results of the main specification restricting the sample to advance polls, mobile polls, and absentee polls. As expected, the observation of a reported robocall incidence has no predictive power for those voters; the corresponding coefficient is negative, but highly insignificant. The next row (3) shows the results of the main specification restricting the sample to close races (the lowest quantile of winning margins). The absolute value changes little and the estimate remains significant, indicating that the results are not driven by outliers. The specification in row (4) includes weighted observations within districts, giving polling stations with more (less) total votes cast a higher (lower) weight in the overall regression. This reduced the estimated effect somewhat, but the coefficient remains large in absolute value and highly significant. However, the same is not true if we use the most current (extended) list of ridings that had reported robocalling, as can be seen in row (5). On March 10 2012, this list was comprised of 77 electoral districts. As discussed earlier, though, this outcome is somewhat expected, as one would naturally presume there to be more measurement error in later reports.

4.2 Were robocall reports correlated with 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 the Conservative candidate spend 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 the 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, where the latter is defined as the total amount of money spent that Conservative candidates reported in their ridings. Again, the data are freely available on the Elections Canada website.

We see that controlling for campaign spending of Conservative candidates leaves the magnitude or 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.

Another approach to check whether the robocall indicator may be picking up campaigning intensity it to look at the determinants of Conservative campaign spending, and verify

Table 4: Turnout and Campaign Spending by Conservative Candidates

	coefficient	standard error
Within District Difference in Differences Estimates		
lagturnout	$.73^{**}$	(.012)
lagoppvoteshare	$-.046^{**}$	(.007)
robocall×lagoppvoteshare	$-.051^{**}$	(.015)
Cspending×lagoppvoteshare	$-.00007$	.0003
district fixed effects		yes
number of observations		59373

[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 heteroskedasticity–robust, and clustered at the riding level. Superscripts **, and * indicate significance at 1% and 5% respectively.

whether the unexplained residual varies with the robocall indicator. The results of the corresponding regressions (on the riding level) are presented in Table 7 in Appendix B. While reported Conservative spending varies with election outcomes from 2008 and 2011 such as the votes share of opposition candidates, expenses of the opponents, and the closeness of the race, among others, the estimated coefficient on the robocall variable is positive but insignificant, which lends further credibility to the hypothesis that the intensity of campaigning effort (as measured by the absolute dollar amount reported) is not related to the robocall variable, once we control for observable characteristics of the districts. Since those are absorbed in the riding fixed effects in our main specification, we can be reasonably confident that unobserved campaigning intensity is not driving our results.

4.3 Did the robocall instigators target specific polling stations?

Assuming for simplicity 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 (e.g, based on turnout), provided that this selection was not based on a poll-specific characteristic that is correlated with the error term [18]

More generally, difference-in-difference estimators provide an unbiased estimate of the treatment effect only if in the absence of treatment, the outcomes in the treatment and control group would have followed similar paths. Translated to our setting where we employ riding fixed effects, this means that 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 election (turnout and their non-Conservative vote share), *relative to the riding average*. One possible logic that could invalidate this assumption is the following. 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 political affiliations). In other words, an underlying unobserved characteristic of some polling stations is their tendency to be more easily demobilized than others. If the instigator of those robocalls somehow targeted those polling stations, the robocall dummy would pick up this effect - we'd conclude that robocalls cause a relative drop in turnout, when if 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 this matter, we reran the regressions in Table 2, but using data from the 2008 and 2006 general elections. The results are presented in Table 5. The findings are surprising. We see the estimate on the robocall variable is roughly of similar magnitude as in the 2011 election and although it is no longer significant at the 5 percent level, the t-statistic is still fairly large. Assuming there were no robocalls in the 2008 election[19] the estimated coefficient of course cannot possibly represent a causal effect. There are two possible explanations. First, there might in fact be underlying time trends in voter turnout based on the propensity to turnout (as discussed above) *and* the instigators specifically targeted polls that had experienced an above-average drop in turnout in the 2008 election. In this case, our research design would overstate the impact of robocalls. Second, robocalling, if it occurred, could have been based on observed turnout in the 2008 election, and thus

[18] Another possibility is that the effect of robocalls on turnout is non-linear in lagged opposition vote share, in which case our estimated parameter would lose economic meaning. We include higher order polynomials of our variable of interest and cannot reject that they are all zero, although the higher-order coefficients are not precisely estimated.

[19] One alleged exception is the riding of Saanich-Gulf Islands, see above.

Table 5: Regressions for the 2008 general election

	coefficient	standard error
Within District Difference in Differences Estimates		
lagturnout	$.72^{**}$	(.015)
lagoppvoteshare	$-.08^{**}$	(.014)
robocall×lagoppvoteshare	$-.061$	(.034)
riding fixed effects		yes
number of observations		52,110

[a] **Note:** The standard errors reported in parentheses are heteroskedasticity–robust, and clustered at the district level. Superscripts ** and * indicate significance at 1% and 5%, respectively.

be endogenous in the regression in Table 5. If the party responsible for the robocalls had picked ridings (for the 2011 election) in which there was an above-average turnout in the 2008 election,[20] the estimates would be biased upward. As a consequence, the results in Table 5 would be unsuited to check the validity of the main regression in part B of Table 2.

To assess both possibilities further, we ran a separate regression that relates changes in turnout from the 2008 to the 2011 election to the corresponding changes in turnout from the 2006 to the 2006 election, in order to see whether or not there is a (time-invariant) time trend. The answer is no, as illustrated in Table 6, Panel A. We see that the measured effect of reported robocalls is to depress voter turnout relative to the 2008 election, confirming the earlier estimates. More importantly, however, there seems to be no stable time trend in the change in voter turnout on the polling station level. The coefficient on the lagged change in voter turnout is negative and very precisely measured: polling stations that experienced a decline in turnout in the 2008 election (relative to 2006) subsequently saw a turnout *increase* in the 2011 election, controlling for their average party affiliation. Thus, there is no evidence of a time-invariant propensity of particular polling station to be ‘easy-to-demobilize’. To the contrary, the process appears to be one of mean reversion. This implies that if the instigator of the calls had specifically targeted polls with declining turnout in 2008, our research design would actually *underestimate* the true effect the impact of robocalls.

[20] Indeed, since such a strategy would have maximized effectiveness of the call, this logic does not seem implausible.

Table 6: Change in Voter Turnout 2006-2008 and Robocalls in 2011

	coefficient	standard error
A. Dependent Variable is Change in Turnout from 2008 to 2011		
change in turnout from 2006 to 2008	$-.291^{**}$	(.004)
oppvoteshare 2008	$-.019^{**}$	(.003)
robocall×lagoppvoteshare	-0.043^{**}	(.012)
riding fixed effects		yes
number of observations		48,079
B. Dependent Variable is Robocall (probit)		
turnout 2008	$.011^{**}$	(.0008)
change in turnout from 2006 to 2008	$.015^{**}$	(.003)
oppvoteshare 2008	$-.002^{**}$	(.0005)
turnout change×lagoppvoteshare	$-.0004^{**}$	(.00004)
number of observations		47,915

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

We are thus left with the second explanation of the puzzling findings in Table 5, namely, that robocalls were placed strategically based on 2008 election turnout numbers. It is important to note that this problem does *not* invalidate the original regression. It does imply, though, that we cannot use the 2008 election as a falsification test of the estimation strategy due to an endogeneity problem. Panel B in Table 6 sheds light on this issue, by estimating a probit model with the robocall identifier as the dependent variable. The independent variables are the change in voter turnout at the level of polling stations from the 2006 to the 2008 election, the opposition vote share in the 2008 election, and an interaction term that allows differential effect of the turnout change depending on the political orientation of the polling station. All three covariates are highly significant: polling stations where turnout was high in 2008 and had previously increased were more likely to be in a riding with robocall reports in 2011. Importantly, the former effect is less pronounced if the electorate residing in the poll was more leaning toward the opposition candidates. Those polling stations, where the opposition had won relatively more votes in 2008, were less likely to be in an affected riding. Again, if that was indeed a selection criterion this would lead to a downward bias in our results, the estimate in Section 3 would then represent a lower bound of the true effect.[21]

In light of the findings in Panel B of Table 6, we also ran an additional robustness check by using a propensity score matching estimator to determine whether the results still hold when we mandate that the control group be as similar as possible to the treatment group. We create a propensity score by using a rich set of controls to predict the incidence of robocalls, including the winning margin, turnout, and opposition vote share in the previous election, as well as the change in turnout, conservative spending, the standard deviation of income, the fraction of the districts population over 65, and a leave-one-out estimator of the distance to the nearest robocalled district. We then matched treated observations to control observations and reran the main regression in Table 2, part B. While we find substantial common support for the treated group (robocalled ridings) and the matched untreated group (ridings not our list), the estimated effects are not fully robust to the details of the matching procedure. Under the nearest neighbor matching method, the point estimates for our primary parameter of interest range is -0.076 for one neighbour (s.e. = 0.032) and to -0.048 for two neighbours (s.e. = 0.025). If we use an Epanechnikov kernel instead, the point estimate is -0.035, and the standard error is 0.018 (p=0.060). There are two main problems with matching as an alternative to difference-in-differences estimators in our context. First, our sample of only 27 affected ridings out of 306 is quite small. Because of small sample size, whether a particular observation is included (matched) or excluded (not matched) can change the coefficient and standard errors significantly. Second, we do not observe "treatment" with certainty. Instead, 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, which is the exposure to misleading phone calls. If the latter were deliberately targeted, the probit regressions we use to calculate a riding's

[21] As a final 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.

propensity score provide an *alternative* estimate of the probability of being treated, which is based on a wide range of observable variables, including some that are highly significant in the regression (such as spending by the Conservative candidate and the fraction of the population over 65). It is quite possible, then, that the propensity score provides additional information reagarding actual treatment, and may even predict the probability of actual exposure to robocalls better than the binary measure of treatment we employ. In this case, of course, the main identifying assumption of matching estimators would be violated.[22]

4.4 Bootstrapping Standard Errors

A final concern is that the asymptotic confidence intervals may not be a good indicator for the accuracy of our estimates. We would like to know the likelihood of rejecting the null hypothesis if no robocalls had in fact been reported, in the current sample. To that end, we use the 279 districts where no robocalls were reported to bootstrap the standard errors in order to assess their sampling distribution. Accordingly, we drop the robocalled districts from the sample, 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 for both 2011 and 2008. This procedure is repeated 1000 times to obtain the joint distribution of the t-statistics for both years. These samples were unbiased in 2011 (mean = 0.0002, se = 0.0008), but positive in 2008 (mean = 0.02, se = 0.0002). The 2008 estimate is consistent with a true effect closer to zero, and robocalls removing from the population of non-robocalled districts those districts that would drive the estimate downwards. The estimated boundary of the 95 % confidence interval occurs at t=2.43, which is higher than the asymptotic value of 1.97 but considerably smaller in magnitude than what we observed. Importantly, the correlation between the t-statistics in 2011 and 2008 is close to zero (0.01) and not significant. This implies that even if we observe significant results in the robocalled districts in 2008, we would not be more likely to see significance in 2011 in the absence of treatment. Put differently, even if the incidence of reported robocalls is correlated with time trends in turnout at the polling station level in 2008 (if, for instance, the instigator of the alleged calls was targeting districts with lower turnout of Opposition-leaning voters in the previous election), we would not expect to see those time trends to continue in 2011.

[22]Detailed regression tables are available on request.

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 ridings, 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 ridings affected by the demobilization efforts was significantly lower than in the ridings where no automated phone calls have been reported. The point estimate gives 3 percentage points. As such, the effect is considerably smaller than the 50 percent reduction in turnout that Barton (2011) finds. But since nothing is yet known about the total numbers of voters that actually have received a phone call, if any, those numbers are not comparable. Besides, Barton's results are based on a framed-field experiment with little consequence of failing to go to the polls and it may be difficult to draw inferences regarding actual elections. In either case, Barton also reports that pre-election warnings against possible fraudulent messages inoculates voters against misinformation effects, and generally restores voter turnout. If his findings are taken at face value, the outlook is positive: having been warned, the Canadian electorate should now be guarded against any future attempts at demobilization.

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Appendix A

Table 7: Voter Turnout and Robocalls in 2011: non-weighted regressions

	coefficient	standard error
Within District Difference-in-Difference Estimates		
lagturnout	$.718^{**}$	(.0115)
lagoppvoteshare	$-.056^{**}$	(.006)
robocall×lagoppvoteshare	$-.051^{**}$	(.008)
number of observations		57241

[a] **Note:** This specification is identical to that in Table 2, except for the fact that it does not employ weights that adjust for the relative size of the electorate at different polling stations and ridings. The standard errors reported in parentheses are heteroskedasticity–robust, and clustered at the district level. Superscripts ** and * indicate significance at 1% and 5%, respectively.

Table 8: Voter Turnout and Robocalls in 2011: extended list

	coefficient	standard error
Within District Difference-in-Difference Estimates		
lagturnout	$.711^{**}$	(.0113)
lagoppvoteshare	$-.052^{**}$	(.007)
robocall×lagoppvoteshare	$-.008$	(.012)
number of observations		57241

[a] **Note:** These regressions use the specification of Table 2 for the extended list of 57 ridings (as of February 29th 2012). Standard errors reported in parentheses, and are heteroskedasticity–robust, and clustered at the district level. Superscripts ** and * indicate significance at 1% and 5%, respectively.

Table 9: Determinants of Campaign Spending by CPC candidates 2001

	coefficient	standard error
Cross-Section OLS on Electoral District Level		
CPC candidate expenses in 2008	.452**	(.101)
CPC candidate vote share in 2008	.401**	.219
Opposition expenses in 2011	−.007	.062
Opposition expenses in 2008	−.102	.051
Closeness of race in 2011	−.281	.161
Winning margin in 2008	−.0008***	.0002
increase in turnout from 2006 to 2008	−84.47	62.275
robocall	2.87	2.15
number of observations		270
provincial fixed effects		yes

[a] **Note:** The dependent variable is the total dollar amount a Conservative candidate reported to Elections Canada as his or her expenses in each riding during the 2011 electoral campaign. The co-variates on the level of a riding are: the Conservative voteshare in 2008, the expenses of the Conservative candidate in 2008, the joint expenses of Opposition candidates in 2008, the electoral margin in 2008, the closeness of the election in 2011, and the robocall indicator variable. Standard errors reported in parentheses, and are heteroskedasticity–robust, and clustered at the provincial level. The ridings are weighted by their relative size. Superscripts ** and * indicate significance at 1% and 5%, respectively.

Appendix B

B1. List of Ridings with Alleged Misconduct

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.[23]

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

[23]Source: Yahoo news.
See http://ca.news.yahoo.com/blogs/canada-politics/robocall-scandal-could-lead-elections-202108363.html

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
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

B2. Extended List of Ridings with Alleged Misconduct

List of 77 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 10, 2012.[24]

[24] Source: The Sixth Estate. See http://sixthestate.net/?p=3646

1. Ancaster-Dundas-Flamborough, reported by National Post.
2. Bas-Richelieu-Nicolet-Becancour, reported by National Post.
3. Brampton West, reported by National Post
4. Burnaby-Douglas, reported by Burnaby Now (calls impersonated Elections Canada and misdirected voters).
5. Burnaby-New Westminster, reported by Burnaby Now.
6. Cambridge, reported by private citizen (Postmedia: "harassing phone calls").
7. Chilliwack-Fraser Canyon, reported by National Post.
8. Davenport, reported by NDP.
9. Don Valley East, reported by National Post.
10. Edmonton Centre, reported by NDP (CBC: phone calls misdirected voters to wrong polling stations).
11. 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.)
12. 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.)
13. Egmont, reported by Liberals (Postmedia: live callers pretended to represent Liberal candidate, but mispronounced his name).
14. Elmwood-Transcona, reported by NDP (A formal complaint has been sent to Elections Canada over phone calls claiming voting locations had changed.)
15. Esquimalt-Juan de Fuca, reported by campaign volunteer to Sixth Estate (overnight calls impersonating the Liberal Party).
16. Essex, reported by NDP (National Post: robocalls misdirected voters).
17. Etobicoke Centre, reported by Liberals (a court case will begin in April to hear allegations that Conservatvies temporarily shut down a polling station and harassed Liberal voters. See also Global News).
18. Fredericton, reported by private citizen (CBC: Phone number connected to the Conservative Party attempted to misdirect voters to wrong polling station).
19. 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.)

20. Haldimand-Norfolk reported by Liberals (Postmedia: harassing overnight calls impersonated the Liberal Party)
21. Halton, reported by Elections Canada: election-day robocalls misdirected voters.
22. Hamilton East-Stoney Creek, reported by Liberals
23. Kelowna-Lake Country, reported by Conservatives
24. Kingston and the Islands, reported by Liberals (CBC: Callers impersonating Liberal Party misdirected voters to wrong voting locations on election day.)
25. Kitchener Centre, reported by voting officer (”a lot” of electors were called and told their polling stations had changed).
26. Kitchener Waterloo, reported by Elections Canada
27. Kitchener-Conestoga, reported by private citizen (election-day robocalls misdirected voters).
28. Lac Saint Louis, reported by Liberals (Cyberpresse: Voters received misdirection calls.)
29. Lanark-Frontenac-Lennox and Addington, reported by National Post.
30. London North Centre, reported by Liberals (Postmedia: Telephone campaign falsely informed listeners that the Liberal candidate spent half of each year in Africa).
31. London West, reported by Liberals (Local radio: MP3 recording of an alleged hoax robocall attempting to misdirect a voter).
32. Markham-Unionville, reported by NDP, reported by National Post.
33. Mississauga East-Cooksville, reported by Liberals
34. Mississauga-Streetsville, reported by National Post.
35. Mount Royal, reported by Liberals. CBC (election-day robocalls misdirected voters).
36. Nanaimo-Alberni, reported by NDP (Parksville News: phone calls misdirected voters).
37. Niagara Falls, reported by Liberals (Postmedia: overnight callers impersonated Liberal Party)
38. Nipissing Timiskaming, reported by Liberals (CBC: Calls impersonating Elections Canada misdirected voters to the wrong locations.)
39. North Vancouver, reported by private citizen (Postmedia: election-day robocalls misdirected voters.)

40. Oak Ridges-Markham, reported by National Post.
41. Oakville, reported by Liberals (Postmedia: callers with "fake accents" pretended to represent Liberal candidate.)
42. Ottawa Centre, reported by NDP.
43. Ottawa Orleans, reported by Liberals (OpenFile: election-day robocalls impersonated Elections Canada and misdirected voters. Ottawa Citizen: fake callers misdirected voters.)
44. Ottawa West-Nepean, reported by Liberals (Postmedia: election-day calls misdirected voters).
45. Parkdale-High Park, reported by Liberals and by NDP IPostmedia: overnight callers impersonated the Liberal Party. National Post: robocalls misdirected voters).
46. Perth-Wellington, reported by Liberals.
47. Peterborough, reported by Conservatives.
48. Pierrefonds-Dollard, reported by Liberals (CBC: Election-day calls misdirected voters).
49. 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.)
50. Prince George–Peace River, reported by Elections Canada (election-day robocalls misdirected voters).
51. Regina-Lumsden-Lake Centre, reported by private citizen (election-day calls misdirected voters).
52. Saanich-Gulf Islands, reported by Greens (See also Maclean's. Toronto Star: election-day live calls misdirected voters.)
53. Saint Boniface, reported by Liberals (Postmedia: callers impersonated the Liberal Party).
54. Saint John, reported by private citizen (CBC: calls impersonated Elections Canada and misdirected voters).
55. Sarnia-Lambton, reported by Sun Media (RMG telephone calls misdirected voters to the wrong polling station)
56. Sault Ste Marie, reported by National Post.

57. Scarborough Southwest, rported by National Post.
58. Simcoe-Grey, reported by Liberals.
59. South Shore-St. Margaret's, reported by NDP (Chronicle-Herald: election-day robocalls misdirected voters).
60. St. Catharines Conservatives by 8822 Conservatives by 13,598 Reported by Liberals. National Post: alleges live calls misdirect voters.
61. St. Paul's, reported by Liberals (National Post: robocalls misdirect voters).
62. Sudbury, reported by Liberals and NDP.
63. Sydney-Victoria, reported by Liberals (Chronicle Herald: fake Liberals and anonymous robocallers misdirected voters).
64. Thunder Bay-Superior North (CBC: calls misdirect voters to wrong polling stations).
65. Vancouver East, reported by NDP to Elections Canada in June 2011.
66. Vancouver Island North, reported by CHEK TV (election-day calls misdirected self-identified NDP and other voters).
67. Vancouver Kingsway, reported by National Post
68. Vancouver Quadra, reported by Liberals Postmedia: Late-night phone calls impersonated Liberal Party.
69. Vancouver South, reported by Liberals (CBC: overnight phone calls)
70. Wascana, reported by Liberals (Global News: overnight live calls).
71. West Nova, reported by CBC (election-day calls misdirected voters to nonexistent polling locations).
72. Willowdale, reported by Liberals (CBC: Calls impersonated Liberal Party).
73. Windsor West, reported by Liberals (Windsor Star: "similar" phone calls to other ridings).
74. Windsor-Tecumseh, reported by NDP.
75. Winnipeg Centre, reported by private citizens (Winnipeg Free Press: election day robocalls misdirected voters).
76. Winnipeg South, reported by NDP.
77. Winnipeg-South Centre, reported by Liberals (National Post: robocalls and live calls misdirected voters).