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The Downsian voter meets the ecological fallacy*

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Abstract. This paper presents evidence that voter participation does not depend on the probability that one vote is decisive. An extensive summary of the empirical participation literature is provided which shows that most but not all studies have found that turnout in an electoral district is higher when the race is closer. Individual-level vote regressions for the 1979 and 1980 Canadian national elections are estimated using objective measures of closeness (as opposed to self-reported measures). The main finding is that a citizen is no more likely to vote in a close election than in a landslide election. District-level turnout regressions for the same elections are also estimated, and a significant relation between closeness and turnout is observed. This suggests that aggregation bias may generate a spurious closeness-turnout relation in district-level regressions.

1. Introduction

It is safe to say that "Why do people vote?" is one of the most-investigated questions in the social sciences. For example, in a review of the literature from 1970 to 1982, Aldrich and Simon (1986) referenced 128 articles and books. The traditional approach to the study of voting has been to identify personal characteristics which distinguish voters from abstainers; well-known examples are Merriam and Gosnell (1924) and more recently Campbell et al. (1960) and Wolfinger and Rosenstone (1980). Downs (1957) proposed a different approach, a rational voter theory, based on the assumption that a person votes if the benefit of doing so exceeds the cost. As opposed to the traditional approach which asks, "Who votes?", this approach asks, "What are the benefits and costs which make it worthwhile for some to vote and others to abstain?"

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One benefit of voting is the possibility of choosing the winner. Central to the Downsian theory is the idea that when deciding whether to vote or abstain a citizen weighs the chance of casting a decisive ballot and the attendant benefits against the cost of voting. One implication of this theory is what we call the Downsian Closeness Hypothesis (DCH): as a person's probability of casting a vote which swings the election increases, she becomes more likely to vote.[1] There has always been a tension in this theory because the probability that any one vote will affect a national election is essentially zero – how can such an infinitesimal payoff be important?

The most popular way to test the DCH has been to regress the turnout percentage in an electoral district on a measure of election closeness, and test whether the coefficient on closeness is different from zero. Because the DCH is ultimately about what motivates *individuals* to vote, this is an appropriate test only if a correlation between turnout and closeness in the aggregate implies that individuals are responding to election closeness. However, there are reasons to believe it may be a mistake to make inferences about individual behavior from aggregate voting studies, that is, there may be an ecological fallacy.

On a purely statistical level, Cox (1988) noted that because of the way the variables are constructed in these district-level "macro" regressions the closeness coefficients are likely to be biased in favor of the DCH. Glazer and Grofman (forthcoming) gave a number of statistical models where a closeness-turnout correlation can arise in the aggregate even if each voter is not concerned with closeness. In their simplest example, they suppose that each voter has a 50 percent chance of voting for the Democratic candidate and a 50 percent chance of voting for the Republican candidate. As turnout exogenously rises the law of large numbers implies that the victory margin as a percentage of total votes will fall, which induces a spurious closeness-turnout relation. Cox and Munger (1989) argued that close races may attract more campaign spending which in turn spurs turnout. In effect, they proposed that people may be more likely to vote in close elections, not because they expect to alter the outcome, but because of heightened campaign activity in their vicinity. If we try to draw conclusions about individual behavior from aggregate data it is important to evaluate the merits of these objections. We need to determine whether inferences from macro regressions suffer from aggregation bias.

The cleanest way to look for a closeness effect is with regressions using individual-level survey data ("micro" regressions). Two notable micro studies are Riker and Ordeshook (1968) and Ashenfelter and Kelley (1975). Both used self-reported closeness measures: each respondent was asked how close she expected the election to be. Measuring closeness in this way may induce a false relation between closeness and the likelihood of voting if people rationalize their decisions. For example, a person who abstains might explain her action by saying she doesn't expect her vote to matter; conversely, someone who goes to the polls might feel embarassed to admit she knows her vote won't matter.

This study examines the relation between turnout and election closeness in the 1979 and 1980 Canadian national elections, making two significant empirical innovations. First, we estimate micro regressions but construct closeness measures from district level data. Such closeness measures are exogenous to an individual so reduce the danger of observing a spurious closeness-turnout relation. Second, we estimate macro and micro regressions for the same elections and compare the closeness coefficients to shed light on the reliability of macro tests. To the best of our knowledge, neither of these exercises have been performed before.

The paper can be summarized as follows. In Section 2 we provide an extensive summary of the existing literature and report that the macro evidence on the DCH tends to support the theory, but is surprisingly mixed. In Section 3 we estimate macro regressions for the 1979 and 1980 Canadian national elections, and find a significant closeness effect. In Section 4 we estimate micro regressions for these same two elections using the same exogenous closeness measures. Our main finding is that the closeness variables are insignificant in the micro regressions. This argues for rejection of the DCH, and suggests that macro evidence which appears to support the DCH may be spurious, the consequence of aggregation bias. Section 5 concludes.

2. Existing macro evidence on the DCH

This section engages in a "meta-statistical" analysis: we systematically review the empirical literature to determine how much support it provides for the DCH. There are two reasons for providing this survey. First, we think it may be of use to other researchers to present a comprehensive reference list and summary of the relevant literature. Second, we want to show that although many macro studies have found a relation between closeness and turnout, there are many that have not – so many, in fact, that one should hesitate to take these studies at face value.

The most popular way to test the DCH has been with macro regressions. This methodology, first used by Barzel and Silberberg (1973), attempts to estimate how sensitive participation is to election closeness by regressing the percentage of people in a district who voted on the probability that one vote mattered. Formally, macro regressions take the form

$$\mathrm{VPCT}_i = \beta_0 + \beta_1 M_i + \beta_2 S_i + \beta_3 Z_i + \epsilon_i \qquad (1)$$

where i indexes the electoral district, VPCT_i is the percentage of eligible

Table 1. Summary of the macro evidence on the DCH

Authors	Elections	Estimate supports DCH?	
		β_1	β_2
Rosenthal-Sen (1973)	French legislature, 1958	yes*	–
	French legislature, 1962	yes*	–
	French legislature, 1967	yes*	–
	French legislature, 1968	yes*	–
Barzel-Silberberg (1973)	Governor, 1962, 1964, 1966, 1968	yes*	yes
Denver-Hands (1974)	British general election, 1959	yes*	–
	British general election, 1964	yes*	–
	British general election, 1966	yes*	–
	British general election, 1970	yes*	–
Silberman-Durden (1975)	Congress, 1962	yes*	yes*
	Congress, 1970	yes*	yes*
Tollison-Crain-Pautler (1975)	Governor, 1970	yes	no
Kau-Rubin (1976)	President, 1964	no	yes*
Seidle-Miller (1976)	British general elections, 1964, 1966	yes*	–
Settle-Abrams (1976)	President, 1868–1972	yes*	–
Crain-Deaton (1977)	President, 1972	yes*	yes
Filer-Kenny (1980)	Local New York referendums, 1949–1976	yes	yes*
Caldeira-Patterson (1982)	Iowa house, 1978	yes*	–
	Iowa senate, 1976, 1978	yes*	–
	California assembly, 1978	yes*	–
	California senate, 1978	yes	–
Chapman-Palda (1983)	British Columbia, 1972	no	–
	British Columbia, 1975	no	–
	Manitoba, 1973	yes*	–
	Manitoba, 1977	yes*	–
	Ontario, 1975	yes*	–
	Ontario, 1977	yes*	–
	Quebec, 1973	yes*	–
	Quebec, 1976	no	–
	Saskatchewan, 1975	yes	–
	Saskatchewan, 1978	yes*	–
Foster (1984)	President, 1968	no*	mixed
	President, 1972	yes*	yes
	President, 1976	mixed	mixed
	President, 1980	mixed	yes
Kenney-Rice (1985)	Presidential primary, 1976, 1980	yes	–
Patterson-Caldeira (1986)	Governor, 1978, 1980	yes*	–
Tucker (1986)	Washington senate, 1976–1982	yes*	–
	Washington house, 1976–1982	yes*	–
Hansen-Palfrey-Rosenthal (1987)	Oregon school districts, 1970–1973	–	yes*
Crain-Leavens-Abbot (1987)	House/Senate, 1982	yes*	–
Durden-Gaynor (1987)	Congress, 1970	yes*	yes*
	Congress, 1982	yes*	yes*

Table 1. Continued.

Authors	Elections	Estimate supports DCH?	
		β_1	β_2
Capron-Kruseman (1988)	Western Nations, 1959–1966	–	yes*
Darvish-Rosenberg (1988)	Israeli municipalities, 1973	–	mixed
	Israeli municipalities, 1978	–	yes*
	Israeli municipalities, 1983	–	yes*
	Israeli Knesset, 1973	–	mixed
	Israeli Knesset, 1977	–	yes
	Israeli Knesset, 1981	–	yes
Cox-Munger (1989)	Congress, 1982	yes*	–
Kirchgässner-Schimmelpfennig (1992)	German general election, 1987	yes*	mixed
	British general election, 1987	yes*	no*
Matsusaka (forthcoming)	Congress, 1966	yes*	–
	California ballot propositions, 1912–1990	mixed	–
Filer-Kenny-Morton (forthcoming)	President, 1948, 1960, 1968, 1980	yes*	–

Note. An asterisk (*) indicates that the estimated parameter was significantly different from zero at the 5 percent level. From regression (1), β_1 is the coefficient on margin and β_2 is the coefficient on district size.

citizens who voted, M_i is the margin of victory, S_i is the district's population, Z_i is a vector of control variables, and ϵ_i is an error term.

The variable M_i represents the probability that one vote is decisive. It is usually operationalized as the difference between the votes received by the winning and losing candidates, or this difference divided by the total number of votes. Although this is an ex post measure of the probability of casting a decisive vote, if people have rational expectations we expect it to be correlated with the ex ante probability. Constituency size, S_i, is included because the votes of people in large constituencies are diluted so they have a small probability of being decisive. With but a few exceptions (for example, Hansen, Palfrey and Rosenthal, 1987), the functional forms and variables are selected on the basis of intuitive plausibility; they do not follow from well-specified models.

Table 1 summarizes the papers which have tested the DCH using some variant of regression (1). It can be seen that a research industry grew up around the Barzel-Silberberg methodology.[2] A few of the articles estimated closeness coefficients but were not specifically addressed to the DCH. We have attempted to be comprehensive – the table reports every macro study with which we are familiar.

When an article reported separate macro regressions for individual elections we present the regression results separately. For example, Silberman and Durden (1975) estimated separate regressions for the 1962 and 1970 congressional elections. Tucker (1986) only reported his regressions for a pooled sample of Washington elections in 1976–1982. A "yes" under β_1 means the estimated margin coefficient was consistent with the DCH; a "yes" under β_2 means the estimated district size coefficient was consistent with the DCH; and "mixed" means the estimated coefficient was consistent with the DCH with some Z_i vectors but inconsistent with others.

Counting all the studies there are 43 independent estimates of the margin coefficient and 21 of the district size coefficient.[3] Of the margin coefficients, 35 (81.4 percent) have the sign predicted by the DCH and 30 (69.8 percent) of these are statistically significant. Of the district size coefficients, the signs of 14 (66.7 percent) are consistent with the DCH and 9 (42.9 percent) significantly so. Of the entire 64 coefficients, only 39 (60.9 percent) support the DCH at conventional levels of statistical significance.

There are enough significant positives to pass most tests of joint significance – for example, a sign test with 49 positives out of 64 rejects the null of zero at better than the 1 percent level – but with nearly 40 percent of the coefficients failing to support the DCH there are grounds for caution. This caution is doubly warranted because we might expect a bias in favor of the DCH in this type of meta-analysis if there is a hesitancy of authors to submit and journals to publish insignificant results. It would seem that either the power of macro tests is very low, or there is a closeness-turnout relation but it only operates in certain elections.

On this last possibility, Matsusaka (forthcoming) suggests the pattern in Table 1 may reflect elite activity in legislative elections. A political party distributes its election resources to candidates across the country, moving money and manpower from uncontested districts to close districts. Because campaign activity stimulates participation, this should generate a closeness-turnout relation in elections for the U.S. Congress, French legislature, British parliament, German *Bundestag*, and state and provincial legislatures, which we see. For presidential elections there is less of a tendency for a party to shift resources to close states because of their unequal values in the electoral college. For example, if the candidates were close in Wyoming it would probably not attract much campaign activity because the state's three electoral votes are unlikely to be important. This can explain why closeness effects are difficult to find for presidential elections. The same reasoning explains the negative results for ballot propositions. The challenge to this explanation is the apparent significant closeness effect for gubernatorial elections.

In any case, Table 1 suggests that macro regressions do not provide a robust method to test the DCH. We should receive the findings of any particular macro study with some caution.

3. Macro regressions for the 1979 and 1980 Canadian national elections

In this section we report macro regressions for the Canadian national elections of 22 May 1979 and 18 February 1980. Canada has a parliamentary system of government. There are 282 districts ("ridings"), each of which is counted as an observation in the macro sample. Elections in each district are winner take all. No legislative seats are allocated on the basis of how well a party does nationally.

Voter registration in Canada is automatic at the age of 18 so the empirical complications associated with U.S. registration do not arise. In addition, a ballot cast in a Canadian national election pertains only to that national election. As opposed to U.S. elections, where a ballot contains the names of tens of individuals running for numerous races, we can be sure a Canadian voter is at the polling place specifically to vote on the national election. We chose to study Canadians because of these particularly amenable characteristics of Canadian national elections. We have no reason to expect Canadians are fundamentally different than Americans in their attitudes toward voting, or that Canadian institutions otherwise bias our results, so we believe our central findings on the DCH will apply to voting in the United States as well. But it should be kept in mind that there may be important differences of which we are not aware.

Election data were drawn from the *Report of the Chief Electoral Officer Respecting Election Returns, 1979, 1980*. Expenditure data were taken from the *Report of the Chief Electoral Officer Respecting Election Expenses, 1979, 1980*. Demographic data came from the 1981 Canadian census; they are defined in Appendix 1. Summary statistics are presented in Table 2.

Table 3 reports an initial set of estimates of equation (1). Each column is a regression; the first three are for 1979 and the last three for 1980. The dependent variable in all regressions is the number of votes cast as a percentage of eligible voters, VPCT. Coefficients on closeness measures – predicted by the DCH to be negative – are reported above the horizontal line. We also estimated log-of-the-odds models, experimented with functional forms, ran the regressions on subsets of the variables, and included the closeness of the third party, none of which changed the substance of the results. In short, the findings on closeness reported in this section are quite robust.

Three different closeness measures are used. The ideal measure would be survey predictions from opinion polls taken the day before the election; we use ex post measures under the conventional assumption that voters have unbiased expectations. The first measure is

$$C_i = VOTES_i(1) - VOTES_i(2)$$

where $VOTES_i(1)$ is the number of votes cast in district i for the winning can-

Table 2. Summary statistics for the aggregate data

Variable	Mean	S.D.	Minimum	Maximum
VPCT79	75.56	5.57	52.00	87.00
Total votes, 1979	40,926	10,289	5,235	81,610
VPCT80	69.36	5.69	47.00	85.00
Total votes, 1980	38,394	9,582	5,687	81,456
C 1979	10,080	8,712	15	40,480
M 1979	29.37	22.53	0.05	84.40
Registered voters, 1979	54,024	12,320	8,060	98,132
C 1980	9,734	8,894	19	38,487
M 1980	29.97	25.30	0.22	84.10
Registered voters, 1980	55,401	12,900	8,488	107,179
$\hat{C}$	9,735	7,885	0	40,811
$\hat{M}$	29.97	21.98	3.30	96.90
% Educated, 1979	74.12	10.71	6.55	93.60
% Educated, 1980	74.62	10.63	15.02	93.90
% In labor force, 1979	78.09	13.92	56.23	144.20
% In labor force, 1980	76.01	12.24	51.17	131.10
% Catholic	47.65	28.27	9.15	97.60
% Born in Canada	84.09	12.28	35.07	99.40
% French-speaking	26.73	37.17	0.10	99.20
Population growth rate in percent	5.16	10.23	− 16.72	46.20
Average income ($)	12,277	2,187	8,099	21,362
Total campaign expenditures ($), 1979	56,367	12,740	23,201	94,128
Expenditures per capita ($), 1979	1.10	0.37	0.48	3.16
Total campaign expenditures ($), 1980	53,763	16,827	12,188	89,050
Expenditures per capita ($), 1980	1.02	0.41	0.22	3.56

Note. Variables are defined in Appendix 1. Some percentages may exceed 100 due to the choice of deflators. There are 282 observations for each variable.

didate and $VOTES_i(2)$ is the votes of the runner-up. We call this measure "closeness", following Cox and Munger (1989), although it is actually a measure of the distance between parties.

The probability of casting a decisive vote may depend not on the absolute vote difference but the vote difference as a percentage of the total votes cast. Following previous research, we define the "margin" measure as

$$M_i = 100 \times \left[\frac{VOTES_i(1) - VOTES_i(2)}{VOTES_i(1) + VOTES_i(2)} \right].$$

This measure adjusts for the variance in district sizes. For example, in 1979 there were 98,132 registered voters in the York-Scarborough riding near Toronto and only 8,060 in the Nunatsiaq riding in the Northwest Territories. One might expect that a 100 vote difference in York-Scarborough was a closer election than a 100 vote difference in Nunatsiaq.

Table 3. Macro regressions of VPCT on closeness, 1979 and 1980

Variable	1979	1979	1979	1980	1980	1980
C in 10,000	−0.473	—	—	−2.569**	—	—
	(0.410)			(0.510)		
M	—	−0.050**	—	—	−0.131**	—
		(0.015)			(0.017)	
Registered voters	—	—	−0.654+	—	—	−0.986*
in 10,000			(0.360)			(0.390)
Constant	84.04**	84.06**	88.39**	86.57**	87.86**	96.20**
	(7.64)	(7.50)	(7.93)	(7.83)	(7.35)	(8.35)
% Educated	0.166**	0.155**	0.206**	0.053	0.014	0.123*
	(0.047)	(0.046)	(0.052)	(0.052)	(0.049)	(0.060)
% In labor force	−0.105*	−0.093*	−0.120*	−0.170**	−0.139**	−0.220**
	(0.048)	(0.047)	(0.048)	(0.052)	(0.049)	(0.054)
% Catholic	0.087*	0.079**	0.094**	0.063+	0.052+	0.063+
	(0.031)	(0.030)	(0.030)	(0.032)	(0.030)	(0.033)
% Born in Canada	−0.219**	−0.212**	−0.255**	−0.142**	−0.131**	−0.201**
	(0.050)	(0.049)	(0.054)	(0.050)	(0.047)	(0.056)
% French-speaking	−0.019	−0.005	−0.019	−0.001	0.022	−0.030
	(0.024)	(0.024)	(0.024)	(0.026)	(0.025)	(0.026)
Population growth	0.008	0.003	0.039	0.131**	0.102*	0.169**
rate in percent	(0.046)	(0.045)	(0.050)	(0.046)	(0.043)	(0.052)
Average income	0.216	0.239	0.173	0.206	0.194	0.003
in $1,000	(0.249)	(0.242)	(0.245)	(0.266)	(0.249)	(0.272)
R^2	0.213	0.241	0.219	0.149	0.243	0.091
$\bar{R}^2$	0.190	0.218	0.196	0.124	0.221	0.064

Note. Each column is a regression. The dependent variable is VPCT79 in the first three columns and VPCT80 in the last three columns. Variables are defined in the text and Appendix 1. Standard errors are in parentheses. Significance is indicated as follows: '+' is significant at 10%, '*' is significant at 5%, and '**' is significant at 1%. Each regression has 282 observations.

The third measure of closeness is the number of registered voters. The probability of casting a decisive vote is greater in a district with 10 registered voters than in a district with 10,000 registered voters (Chamberlain and Rothschild, 1981). When few are expected to vote, any one person's chance of being the kingmaker increases.

We are interested in the closeness coefficients so to keep the paper a manageable length we simply report the estimates for the Z_i vector and do not discuss them. Education, income, and employment are standard demographic controls, in general positively related to turnout. The number of Catholics and French-speakers are additional demographic cleavage factors particularly relevant for Canada; we have no theoretical expectation of the sign of their effects. The number of native Canadians and the population growth rate primarily capture the presence of immigrants. One might expect immigrants to an electoral district to take some time to acclimate themselves and learn the local political terrain before participating in elections (Merriam and Gosnell, 1924). On the other hand, immigrant communities may be more closely knit and able to motivate their members.

The estimates in Table 3 appear to be fairly consistent with the estimates reported in Table 1. All closeness coefficients are negative as predicted by the DCH, and all but the C measure in 1979 are statistically significant. Cox (1988) noted that M and registered voters may be negatively correlated with VPCT by construction: the denominator of M is roughly equal to the number of votes cast, which is the numerator of VPCT; the number of registered voters is the denominator of VPCT. He suggested the C measure be used instead as it would not be subject to such biases. There is some support for this contention in 1979 – when M and registered voters are used there is a significant closeness effect, while there is not when C is used. Built-in biases in M and registered voters cannot be the whole story, however, for the closeness effect remains in 1980 even with the C measure.

Although the coefficients on C and M go in the predicted direction, it appears their magnitudes are trivial. In 1980 where the effects are strongest, according to C the difference in turnout between an election where 10,000 votes (a little more than the mean) separated the top two finishers and one where they tied was only 2.6 percent. According to M the difference in turnout between an election with a 30 percent margin (roughly the mean) and one which was dead even was 3.9 percent. The R^2's are somewhat lower than in comparable studies and are primarily driven by the controls not the closeness measures.

Cox and Munger (1988) and Matsusaka (forthcoming) present evidence that the closeness effect in macro regressions may be induced by higher spending in more competitive races. Campaign spending is expected to increase participation by providing low cost information to prospective voters. To look for this we re-estimate the regressions in Table 3 adding per capita campaign expenditures as an explanatory variable. There may be simultaneity problems in these regressions – expenditures might increase turnout and at the same time high turnout districts might attract expenditures (Palda, 1975; Jacobson, 1978). With this caveat in mind we present the regressions in Table 4.

All closeness coefficients become less negative when district campaign spending is included. This implies that some of the observed closeness effects are induced by increased spending in close elections.[4] It appears the effect of the district size variable is completely due to high per capita expenditures in small districts. However, the expenditure variable does not completely remove the closeness effect. The M coefficients remain significantly negative for 1979 and 1980, and the C coefficient remains significantly negative for 1980. Our regressions are consistent with other macro DCH studies which included expenditures as an explanatory variable, for example, Settle and Abrams (1976), Chapman and Palda (1983), and Cox and Munger (1989).

Table 4. Macro regressions of VPCT on closeness and expenditures, 1979 and 1980

Variable	1979	1979	1979	1980	1980	1980
C in 10,000	0.301	—	—	−1.299*	—	—
	(0.460)			(0.640)		
M	—	−0.032+	—	—	−0.112**	—
		(0.017)			(0.021)	
Registered voters	—	—	0.142	—	—	0.230
in 10,000			(0.440)			(0.440)
Constant	82.97**	83.20**	81.97**	86.59**	87.39**	86.34**
	(7.50)	(7.46)	(8.11)	(7.69)	(7.34)	(8.24)
% Educated	0.209**	0.187**	0.199**	0.089+	0.035	0.092
	(0.048)	(0.048)	(0.051)	(0.052)	(0.051)	(0.058)
% In labor force	−0.126**	−0.110*	−0.122*	−0.199**	−0.154**	−0.218**
	(0.047)	(0.048)	(0.048)	(0.052)	(0.050)	(0.051)
% Catholic	0.076*	0.073*	0.074*	0.047	0.047	0.037
	(0.030)	(0.030)	(0.031)	(0.032)	(0.030)	(0.032)
% Born in Canada	−0.259**	−0.240**	−0.250**	−0.189**	−0.153**	−0.199**
	(0.050)	(0.051)	(0.053)	(0.051)	(0.049)	(0.054)
% French-speaking	−0.006	0.004	−0.003	0.014	0.028	0.007
	(0.024)	(0.024)	(0.024)	(0.026)	(0.025)	(0.026)
Population growth	0.027	0.020	0.021	0.153**	0.117**	0.146**
rate in percent	(0.045)	(0.045)	(0.049)	(0.045)	(0.044)	(0.050)
Average income	0.080	0.173	0.109	0.095	0.161	−0.009
in $1,000	(0.247)	(0.243)	(0.243)	(0.263)	(0.249)	(0.260)
Per capita campaign	3.509**	2.226*	3.390**	3.725**	1.634	5.489**
expenditures ($)	(1.043)	(1.029)	(1.134)	(1.152)	(1.085)	(1.089)
R^2	0.245	0.253	0.244	0.180	0.249	0.169
$\bar{R}^2$	0.220	0.229	0.219	0.153	0.225	0.141

Note. Each column is a regression. The dependent variable is VPCT79 in the first three columns and VPCT80 in the last three columns. Variables are defined in the text and Appendix 1. Standard errors are in parentheses. Significance is indicated as follows: '+' is significant at 10%, '*' is significant at 5%, and '**' is significant at 1%. Each regression has 282 observations.

Elite mobilization appears to account for at least part of the aggregate closeness effect. It may explain all of the closeness effect – there may be unobserved campaign expenditures which drive the rest of the closeness coefficients, for example, expenditures by non-candidates and in-kind expenditures like volunteer labor.

The closeness measures in Tables 3 and 4 are only observed when the election is over. In using them we implicitly assume that on average districts with close races ex post were known to be close ex ante. The merits of this assumption can be addressed by comparing the results with estimates using closeness measures constructed from information which was publicly available prior to the election. In Table 5 we report estimates of regression (1) using ex ante closeness

Table 5. Macro regressions of VPCT on ex ante closeness, 1980

Variable	Closeness using 1979 measures		Closeness using estimated measures	
$\hat{C}$ in 10,000	−0.798	—	−0.627	—
	(0.561)		(0.633)	
$\hat{M}$	—	−0.084**	—	−0.078**
		(0.020)		(0.022)
Constant	86.80**	86.47**	87.47**	87.50**
	(7.73)	(7.49)	(7.72)	(7.55)
% Educated	0.096+	0.058	0.098+	0.063
	(0.053)	(0.052)	(0.053)	(0.052)
% In labor force	−0.205**	−0.164**	−0.212**	−0.180**
	(0.052)	(0.051)	(0.052)	(0.051)
% Catholic	0.038	0.028	0.039	0.032
	(0.032)	(0.031)	(0.032)	(0.031)
% Born in Canada	−0.190**	−0.147**	−0.198**	−0.160**
	(0.052)	(0.051)	(0.052)	(0.051)
% French-speaking	0.010	0.016	0.009	0.015
	(0.026)	(0.025)	(0.026)	(0.026)
Population growth rate in percent	0.149**	0.121**	0.152**	0.126**
	(0.046)	(0.045)	(0.046)	(0.045)
Average income ($1,000)	0.059	0.102	0.040	0.115
	(0.264)	(0.253)	(0.265)	(0.257)
Per capita campaign expenditures ($)	4.101**	2.240*	4.413**	2.509*
	(1.184)	(1.123)	(1.196)	(1.162)
R^2	0.174	0.219	0.171	0.205
$\bar{R}^2$	0.147	0.194	0.143	0.179

Note. Each column is a regression. The dependent variable is VPCT80. The first two regressions use the corresponding closeness measures for 1979. The last two regressions use OLS projections from 1979 closeness. Variables are defined in the text and Appendix 1. Standard errors are in parentheses. Significance is indicated as follows: '+' is significant at 10%, '*' is significant at 5%, and '**' is significant at 1%. Each regression has 282 observations.

measures, indicated with hats over the variables. We only do this for 1980 because we use the 1979 results as predictors of 1980 closeness.

One way people might predict how close a race will be is by looking at how close it was in the preceding election. This is plausible for the 1980 elections which took place only nine months after the 1979 elections. In the first two regressions the 1979 closeness measures are used for $\hat{C}_i$ and $\hat{M}_i$. In the second two regressions the closeness measures are constructed by first regressing closeness (margin) in 1980 on closeness (margin) in 1979. This gives a reduced-form relation between the years (Appendix 2). Then we forecast 1980 closeness in each district using the 1979 numbers and the estimated model.[5] Despite the instability of the Canadian political environment at the time, closeness and margin appear to be good predictors of themselves: regressions of C for 1980 on C for 1979 yield R^2's on the order of 0.800; R^2's for the autocorrelated regressions of M are about 0.700. As above, the table is formatted so the DCH predicts all coefficients above the horizontal line are negative.

The results using ex ante measures are essentially the same as when ex post measures are used. Turnout increased as the race between the top two candidates became closer. The overall fit of the models in Table 5 is worse than in Tables 3 and 4 as judged by R^2.

To summarize, this section reports a number of macro regressions in the DCH tradition. We believe the estimates show first that there is a relation between closeness and turnout in the aggregate. Second, part of the relation appears to be spurious, induced by the way closeness measures are constructed, as suggested by Cox (1988), and by elite mobilization, as suggested by Cox and Munger (1989). Finally, we hope by presenting a number of different estimates and noting how the closeness coefficients can be significant sometimes and insignificant other times to indicate to the reader that the waters of DCH macro regressions can be rather treacherous in general.

4. Micro regressions for the 1979 and 1980 Canadian national elections

The preceding section shows a closeness-turnout relation in the aggregate for the 1979 and 1980 Canadian national elections. In this section we use survey data to estimate micro regressions for the same elections. If it is the case that closeness caused people to vote then closeness will have explanatory power in the micro regressions. If we observe no closeness effect, then the macro regressions are misleading, suffering from aggregation bias.

The survey data were taken from the *1974–1979–1980 Canadian National Elections and Quebec Referendum Panel Study* (ICPSR 8079) compiled by Harold Clarke, Jane Jenson, Lawrence LeDuc, and John Pammet. The study consists of survey responses from 2,744 Canadians following the national elections of 1974, 1979, and 1980. We matched the aggregate data to each individual's district. Thus our closeness measures are exogenous from the individual's point of view which, as we discuss in the introduction, is one of the key innovations of the study. By using merged aggregate and survey data we eliminate the possibility of aggregation bias and self-reported closeness biases. Summary statistics are provided in Table 6.

The difference between macro and micro regressions, as noted in the introduction, is that in the former the unit of observation is an electoral district while in the latter it is an individual. Before presenting the estimates a brief discussion of the pros and cons of micro regressions is in order. On the positive side, because the DCH is couched in terms of what motivates an individual,

Table 6. Summary statistics for the survey data

Variable	Mean	S.D.	Min	Max	Number
Dummy = 1 if voted, 1979	89.41	30.77	0	1	2,607
Dummy = 1 if voted, 1980	88.22	32.25	0	1	1,664
Education in years	11.13	3.45	1	30	2,583
Age in years	44.31	17.78	18	93	2,627
Income, 1979 ($1,000)	15.88	13.21	1.5	40.0	2,570
Income, 1980 ($1,000)	16.83	13.64	1.5	40.0	1,664
Dummy = 1 if married	0.694	0.461	0	1	2,648
Dummy = 1 if male	0.480	0.500	0	1	2,649
Religiousness (scale 0–2)	0.978	0.661	0	2	2,470
Frequency of church attendance (scale 0–4)	2.087	1.534	0	4	2,471
Dummy = 1 if Catholic	0.454	0.498	0	1	2,630
Dummy = 1 if born in Canada	0.861	0.346	0	1	2,624
Dummy = 1 if union member	0.412	0.492	0	1	2,645
Dummy = 1 if French-speaker	0.251	0.433	0	1	2,649
Duration of current residence (scale 1–4)	1.512	0.756	1	4	2,618
Dummy = 1 if unemployed	0.024	0.154	0	1	2,646
Dummy = 1 if retired	0.107	0.310	0	1	2,646
Dummy = 1 if student	0.044	0.206	0	1	2,646
Dummy = 1 if farmer	0.035	0.184	0	1	2.646
Dummy = 1 if professional	0.109	0.312	0	1	2,646
Dummy = 1 if laborer	0.217	0.412	0	1	2,646
Community size (scale 1–9)	4.873	2.722	1	9	2,649
Dummy = 1 if contacted by campaign, 1979	0.426	0.495	0	1	1,298
Dummy = 1 if contacted by mail, 1979	0.768	0.423	0	1	1,286
Dummy = 1 if contacted by phone, 1979	0.170	0.375	0	1	1,286
Dummy = 1 if contacted by campaign, 1980	0.330	0.470	0	1	795
Dummy = 1 if contacted by mail, 1980	0.768	0.423	0	1	788
Dummy = 1 if contacted by phone, 1980	0.156	0.363	0	1	788

Note. Variables are defined in Appendix 1.

micro regressions are the most direct way to evaluate it. Micro regressions do not run the risk of aggregation biases.

A limitation of micro regressions is that in survey data self-reported turnout rates exceed actual turnout rates. In our sample the actual rate for the 1979 election was about 76 percent while the sample self-reported rate was about 89 percent. It may be the survey oversampled voters; people in transition are difficult to interview and less likely to vote. It is also possible respondents forgot whether or not they voted or lied so it they wouldn't appear to be irresponsible citizens.

Vote validation studies for the United States indicate that false voters differ from the population at large; in particular, they tend to be more educated and older (Silver, Anderson and Abramson, 1986). The main concern with non-representative sample respondents and false responses is that they may bias regression coefficients. However, a number of researchers have concluded they do not substantially affect most analyses of voting (Sigelman, 1982; Anderson and Silver, 1986). We do not have comparable evidence for Canadians.

Because the dependent variable is discrete (vote or abstain), ordinary least squares estimation is inappropriate. Following standard procedure, we instead estimate logit regressions. Discriminant analysis is not pursued due to the non-normality of most of our dependent variables.

Table 7 reports the logit estimates. As before, each column is a regression. The first three for 1979 are analogous to the first three regressions in Table 4. The second three for 1980 are analogous to the last three regressions in Table 4. The reported estimates are the derivatives of the logistic probability function evaluated at the mean, not the raw logit coefficients which are difficult to interpret (if P is the mean probability of voting and β_i is the logit coefficient, then we report $\beta_i P(1-P)$.) For example, in the first regression if an average person had one more year of education her probability of voting increased by 1.230 percent. In parentheses beneath each coefficient is the p-value for the χ^2 statistic associated with omitting the variable from the model. Once again, the regressions are presented so the DCH predicts coefficients above the horizontal line are negative. At the end of each regression is the number of observations and the model χ^2.

The main thing to note is the absence of support for the DCH. The sign of the coefficient on closeness between the winner and the runner-up is inconsistent with the DCH in four or six cases. None of the closeness coefficients even approach statistical significance. In addition, the estimates are still quantitatively trivial. The strongest negative closeness effect, for the 1980 M estimate of -0.014, implies that an average person in the closest district ($M=0.22$) was only 0.859 percent more likely to vote than the average person in the least close district ($M=84.10$).

The three dummy variables indicating whether a person was contacted by a campaign worker, by mail, or by phone are available for only half the sample. When we include them in the regressions we are forced to drop half the observations. To see if a larger sample size would increase the significance of the closeness effects we re-estimated the micro regressions without the contact variables. We also left out campaign expenditures so there were no obvious proxies for elite activities in the regressions. This gives the best chance to observe a DCH effect.

The closeness coefficients from these regressions are presented in Table 8. The table is identical to Table 7 except that to conserve space we do not report estimates for the parameters below the horizontal line. Thus, the first column reports the same regression as the first column in Table 7 except that campaign spending and the campaign contact variables are omitted.

Even in these regressions, which should be favorable for the DCH, there is

Table 7. Micro logit regressions, 1979 and 1980

Variable	1979	1979	1979	1980	1980	1980
C in 10,000	0.489	—	—	0.725	—	—
	(0.736)			(0.779)		
M	—	−0.007	—	—	−0.014	
		(0.894)			(0.877)	
Registered voters in 10,000	—	—	0.423	—	—	0.318
			(0.703)			(0.812)
Constant	−30.24**	−28.66**	−32.18*	−20.01	−17.74	−21.03
	(0.007)	(0.010)	(0.015)	(0.189)	(0.251)	(0.230)
Education in years	1.230**	1.243**	1.220**	1.097*	1.102*	1.096*
	(0.002)	(0.001)	(0.002)	(0.027)	(0.026)	(0.027)
Age in years	0.843**	0.850**	0.840**	0.576	0.575	0.571
	(0.008)	(0.007)	(0.008)	(0.200)	(0.202)	(0.206)
$Age^2 \times 10^{-2}$	−0.719*	−0.723*	−0.715*	−0.456	−0.454	−0.449
	(0.031)	(0.030)	(0.032)	(0.339)	(0.343)	(0.349)
Income ($1,000)	0.084	0.167	0.084	0.041	0.043	0.042
	(0.166)	(0.146)	(0.171)	(0.727)	(0.711)	(0.720)
Dummy = 1 if married	2.016	2.032	2.061	1.862	1.893	1.899
	(0.365)	(0.362)	(0.355)	(0.555)	(0.548)	(0.547)
Dummy = 1 if male	2.997	3.011	3.079	5.802*	5.768*	5.815*
	(0.167)	(0.165)	(0.158)	(0.048)	(0.049)	(0.047)
Religiousness (scale 0–2)	1.713	1.724	1.697	−0.612	−0.559	−0.575
	(0.290)	(0.287)	(0.295)	(0.782)	(0.801)	(0.795)
Frequency of church attendance (scale 0–4)	1.186	1.146	1.164	0.002	−0.019	−0.008
	(0.105)	(0.117)	(0.109)	(0.998)	(0.984)	(0.993)
Dummy = 1 if Catholic	0.753	0.895	0.886	1.198	1.258	1.291
	(0.796)	(0.758)	(0.759)	(0.742)	(0.729)	(0.723)
Dummy = 1 if born in Canada	−1.088	−1.160	−1.040	1.083	1.078	1.183
	(0.765)	(0.750)	(0.776)	(0.830)	(0.831)	(0.816)
Dummy = 1 if union member	3.249	3.181	3.137	−0.412	−0.429	−0.452
	(0.119)	(0.126)	(0.132)	(0.878)	(0.873)	(0.867)
Dummy = 1 if French-speaker	−4.642	−4.335	−4.416	3.811	4.684	4.529
	(0.136)	(0.160)	(0.146)	(0.403)	(0.300)	(0.272)
Duration of current residence (scale 1–4)	−1.713	−1.713	−1.717	−0.311	−0.279	−0.280
	(0.278)	(0.279)	(0.278)	(0.882)	(0.894)	(0.894)
Dummy = 1 if unemployed	7.764	7.611	7.456	−0.881	−0.948	−1.100
	(0.395)	(0.403)	(0.414)	(0.934)	(0.929)	(0.918)
Dummy = 1 if retired	−3.222	−3.293	−3.300	−9.556+	−9.542+	−9.629+
	(0.434)	(0.424)	(0.423)	(0.070)	(0.070)	(0.068)
Dummy = 1 if student	−6.176	−6.132	−6.137	−6.678	−6.489	−6.624
	(0.126)	(0.129)	(0.128)	(0.274)	(0.288)	(0.277)
Dummy = 1 if farmer	−5.556	−5.672	−5.633	−5.944	−5.846	−6.039
	(0.256)	(0.245)	(0.248)	(0.379)	(0.387)	(0.373)
Dummy = 1 if professional	−4.543	−4.547	−4.404	−1.331	−1.365	−1.391
	(0.257)	(0.257)	(0.275)	(0.785)	(0.780)	(0.776)
Dummy = 1 if laborer	−5.035+	−5.040+	−5.045+	−3.844	−3.788	−3.841
	(0.055)	(0.054)	(0.054)	(0.294)	(0.301)	(0.294)

Table 7. Continued.

Variable	1979	1979	1979	1980	1980	1980
Community size	−0.363	−0.382	−0.331	−0.737	−0.716	−0.701
(scale 1–9)	(0.341)	(0.313)	(0.406)	(0.186)	(0.197)	(0.212)
Expenditures per capita	10.882*	9.766*	10.911**	6.693	5.171	6.328
($)	(0.013)	(0.020)	(0.010)	(0.200)	(0.284)	(0.169)
Dummy = 1 if contacted	1.332	1.281	1.326	8.600*	8.476*	8.474*
by a campaign worker	(0.533)	(0.549)	(0.535)	(0.015)	(0.017)	(0.017)
Dummy = 1 if contacted	3.454	3.510+	3.454	5.194+	5.121+	5.106+
by mail	(0.104)	(0.098)	(0.104)	(0.060)	(0.065)	(0.065)
Dummy = 1 if contacted	4.797	4.671	4.717	5.125	5.001	4.963
by phone	(0.155)	(0.166)	(0.161)	(0.327)	(0.339)	(0.343)
Number of observations	1,087	1,087	1,087	682	682	682
Model χ^2	87.65	87.55	87.68	49.29	49.23	49.26

Note. Each column is a regression. The dependent variable is 1 if the person voted and 0 if not. The indicated coefficients are the derivatives of the probability function evaluated at the mean, not the raw logit coefficients. They are multiplied by 100 to convert them into percentages. In parentheses beneath the coefficients are *p*-values: '+' is significant at 10%, '*' is significant at 5%, and '**' is significant at 1%.

Table 8. Closeness coefficients from micro logit regressions without campaign variables

Variable	1979	1979	1979	1980	1980	1980
C in 10,000	0.298	—	—	−0.381	—	—
	(0.708)			(0.740)		
M	—	−0.007	—	—	−0.041	
		(0.808)			(0.256)	
Registered voters	—	—	0.020	—	—	−0.202
in 10,000			(0.971)			(0.764)
Number of observations	2,266	2,266	2,266	1,463	1,463	1,463
Model χ^2	94.81	94.73	94.67	48.33	49.49	48.31

Note. This table presents the closeness coefficients for the regressions in Table 7 omitting campaign spending and contact variables. The dependent variable is 1 if the person voted and 0 if not. The indicated coefficients are the derivatives of the probability function evaluated at the mean, not the raw logit coefficients. They are multiplied by 100 to convert them into percentages. In parentheses beneath the coefficients are *p*-values: '+' is significant at 10%, '*' is significant at 5%, and '**' is significant at 1%.

no evidence that the probability of voting is sensitive to electoral closeness. Four of six coefficients have the correct negative sign but they do not approach significance at conventional levels. Moving from Table 7 to Table 8 we added 1,179 observations for 1979 and 771 observations for 1980 – because the p-values do not improve much it seems doubtful that sample size can explain the absence of a closeness effect. More plausibly, there simply is no closeness effect.

Table 9. Ex ante closeness coefficients from micro logit regressions, 1980

Variable	Expenditures and contact omitted				Expenditures and contact included			
	Closeness using 1979 measures		Closeness using estimated measures		Closeness using 1979 measures		Closeness using estimated measures	
$\hat{C}$ in 10,000	−0.511	—	−0.466	—	0.575	—	0.933	—
	(0.398)		(0.713)		(0.807)		(0.687)	
$\hat{M}$	—	−0.059	—	−0.047	—	−0.042	—	−0.017
		(0.170)		(0.293)		(0.617)		(0.844)
Number of observations	1,463	1,463	1,463	1,463	682	682	682	682
Model χ^2	48.42	50.09	48.35	49.31	49.27	49.46	49.37	49.25

Note. This table reports the closeness coefficients for the 1980 regressions in Table 7 using the ex ante closeness measures in Table 5. The first four regressions omit per capita campaign spending and campaign contact variables. The dependent variable is 1 if the person voted and 0 if not. The indicated coefficients are the derivatives of the probability function evaluated at the mean, not the raw logit coefficients. They are multiplied by 100 to convert them into percentages. In parentheses beneath the coefficients are *p*-values: '+' is significant at 10%, '*' is significant at 5%, and '**' is significant at 1%.

For completeness we also re-estimated the regressions using the ex ante closeness measures from Table 5. Table 9 reports the closeness coefficients from these regressions. As with Table 8 we omit the parameters below the horizontal line. In Table 9 we omit expenditures and campaign contact variables in the first four regressions and include them in the last four regressions. Again the results are uniformly unfavorable for the DCH – none of the coefficients can be statistically distinguished from zero.

Because the macro regressions for these elections exhibit significant closeness effects the failure of the micro regressions to show any sensitivity to closeness is striking. This appears to confirm the ecological fallacy conjecture: individuals who do not care about closeness can, if studied as a group, appear to behave as if they care. On logical grounds the micro regressions are to be preferred as they are direct tests. The demonstrated instability of macro estimates gives reason to prefer the micro regressions on empirical grounds. This leaves us to conclude against the DCH. We also conclude that arguments in favor of the DCH based on macro evidence probably suffer from fallacious ecological reasoning.

We would like to be able to point out what specifically is the source of the aggregation bias, but the obvious candidates can be ruled out. The spurious correlation proposed by Cox (1988) would not seem to be a problem here because closeness remains significant in the macro regressions even when we use his preferred measure, C. The spurious inference of causality identified by Glazer and Grofman (forthcoming) would probably generate a positive relation between closeness and voting at the micro level whenever it generated one at the aggregate level, unless the micro regressions parameterize the variables driving turnout. It may be that the aggregate closeness-turnout relation is driven by a strong correlation in very small districts. Voters in these districts are overweighted in macro regressions and when this is corrected in micro regressions the already weak effect vanishes. Other explanations are possible and it would seem a worthwhile project to pursue.

The remainder of this section briefly discusses the coefficient estimates on the control variables in Table 7, primarily to note that our estimates are in line with the rest of the voting literature (compare, for example, with Wolfinger and Rosenstone, 1980). The first set of controls are demographic variables. Among the personal characteristics for which we control the most consistent predictor is education, a finding which conforms with most other studies. Age had a significant but diminishing effect on participation – the numbers for 1980 are not significant in the reported regressions but are in the full sample. The estimates indicate the effect of age on turnout peaked in a person's late 50's and then became negative. Most recent voting studies have found that income has no effect on propensity to vote once education is controlled; our results concur. Men were more likely to vote than women even though we have controlled for education, income, and occupation, which might be expected to explain sex differences in participation. Laborers were 5 percent less likely to vote than the baseline occupation, clerical workers. In 1980, retirees were more than 9 percent less likely to vote, which is somewhat surprising because labor force participation was negatively related to turnout in the macro regressions.

The second set of variables capture campaign effects. Personal contact increased the likelihood of voting by over 8 percent for 1980. The latter result squares with the finding that the number of people personally contacted by party representatives dropped 10 percent between the 1979 and 1980 elections; as a result it is likely that contact efforts were better targeted in 1980. Mail contact increased turnout about 3.5 percent in 1979 and 5.1 percent in 1980. Spending per capita had a significant positive effect on the probability of voting. The highest estimate indicates an effect of 10.911 percent per dollar.

5. Conclusion

Our main finding is that for the 1979 and 1980 Canadian national elections, the probability a person voted was not sensitive to her chance of casting a pivotal vote. This conclusion is robust to a number of different specifications of closeness. Some will find this unsurprising. Palfrey and Rosenthal (1985), building on the work of Ledyard (1981, 1984), developed a general equilibrium rational voter model and demonstrated that when an electorate is large and citizens have incomplete information about each others' costs there will be no instrumental voters. That is, they give a logical argument why only people who derive a consumption benefit from voting will go to the polls. Our results can be viewed as providing empirical support for their theoretical conjecture.

We also show that when voter turnout at the district level is regressed on election closeness and there is a significant effect, when individual turnout is regressed on election closeness the effect vanishes. We also find evidence that the aggregate relation between turnout and closeness may be partially caused by the tendency of elites to mobilize in close elections (Cox and Munger, 1989). This, and the evidence that closeness estimates from district level regressions vary from study to study, suggest that tests based on macro regressions suffer aggregation problems.

Notes

1. This hypothesis has been given many names including the Instrumental Voter Hypothesis and the Rational Voter Hypothesis. We call it the DCH to make clear our belief that the overall validity of the Downsian rational voter approach neither stands nor falls on the DCH alone, although it is one of the implications of the approach.

2. It is clear from the citations in the papers of Table 1 that the Barzel and Silberberg paper was the seminal paper in the area although it was chronologically preceded in publication by Rosenthal and Sen (1973).
3. In these numbers we omit Seidle and Miller (1976) which was a replication of part of Denver and Hands (1974), and we only count the estimates once for the 1972 presidential election (Crain and Deaton, 1977; Foster, 1984), 1970 congression election (Silberman and Durden, 1975; Durden and Gaynor, 1987), 1982 congressional election (Durden and Gaynor, 1987; Cox and Munger, 1989), and presidential election times series (Settle and Abrams, 1976; Filer, Kenny and Morton, forthcoming).
4. In 1979 the correlation between average expenditures and closeness was -0.556 when measured by C, -0.499 when measured by M, and -0.650 when measured by registered voters. The spending-closeness correlation for 1980 was -0.691 when closeness is measured by C, -0.638 when measured by M, and -0.724 when measured by registered voters. All correlations are significantly different from zero at better than the 1 percent level.
5. Predicted closeness was negative for two districts; we normalized them to zero.

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Appendix 1. Variable definitions

Aggregate variables are defined as follows

% Educated:	The numerator is the number of people in a district who are at least 15 years old and have some high school education according to the 1981 census. The denominator is the number of registered voters in the district in the indicated year.
% In labor force:	The numerator is the number of people in a district who are at least 15 years old and in the labor force according to the 1981 census. The denominator is the number of registered voters in the district in the indicated year.
% Catholic:	Number of Catholics in a district divided by the population of the district, both numbers from the 1981 census.
% Born in Canada:	Number of people in a district who were born in Canada divided by the population of the district, both numbers from the 1981 census.
% French-speaking:	Number of people in a district whose mother tongue is French divided by the population of the district, both numbers from the 1981 census.
Population growth rate:	Difference between the district's population in 1981 and 1976, divided by the 1976 population.
Average income:	Average male income plus the average female income in a district divided by two.
Expenditures:	The sum of campaign expenditures in a district by candidates from the three major parties.

Survey variables are defined as follows

Income:	Family income is divided into eight different ranges. We assign each person the income in the middle of the range, except the top and bottom categories where we assign the minimum and maximum in the range, respectively.
Married:	The person is married or in a common law marriage.
Religiousness:	A self-reported measure of a person's religiousness; 0 is not very religious and 2 is very religious.
Church attendance:	Frequency of church attendance is divided into five categories; 0 is never attends church and 4 is once a week.
Catholic:	The person is Roman Catholic.
Union member:	The person or a member of her family is a union member.
French-speaker:	The person speaks French only or French and English at home.

Duration of current residence: Measure of how long a person has lived in her current province; 1 is all her life, 2 is most of her life, 3 is some of her life, and 4 is only a year a so.

Community size: Measure of the density of a person's community; 1 is over 500,000 people (city core), 2 is over 500,000 (adjacent suburb), 3 is over 500,000 (distant suburb), 4 is 100,000 to 500,000, ..., 7 is 1,000 to 10,000, 8 is rural non-farm, 9 is farm.

Contact: Whether or not the person received the indicated type of contact prior to the election by local candidates or party officials.

Appendix 2. Predicted closeness regressions

The closeness and margin measures used in Table 5 are estimated in following way. C(1,2) and M(1,2) indicate the closeness between the winner and the runner-up. C(1,3) and M(1,3) indicate the closeness between the winner and the third place finisher.

$$\hat{C} = -437 + .6252 \times C(1,2) + .3091 \times C(1,3) + .0000382 \times C(1,2)^2 - .0000351 \times C(1,2) \times C(1,3)$$

$$\hat{M} = 0.02 + 0.25 \times M(1,2) + 0.16 \times M(1,3) + 0.7082 \times M(1,2)^2 + 0.1059 \times M(1,2) \times M(1,3)$$