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Abstract Despite the centrality of voting costs to the paradox of voting, little effort has been made to measure these costs accurately, outside of a few spatially limited case studies. In this paper, we apply Geographic Information Systems (GIS) tools to validated national election survey data from New Zealand. We calculate distance and travel time by road from the place of residence to the nearest polling place and combine our time estimate with imputed wages for all sample members. Using this new measure of the opportunity cost of voting to predict turnout at the individual level, we find that small increases in the opportunity costs of time can have large effects in reducing voter turnout.

JEL Classification D7 · R4

Keywords Opportunity cost · Paradox of voting · Travel time

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1 Introduction

For a rational, self-interested voter, the cost of voting normally exceeds any plausible value of expected benefits from the preferred candidate winning.[1] Yet despite these calculations, many people vote. One solution to this paradox of voting, used at least since Riker and Ordeshook (1968), is to assume some additional (consumption) benefit from voting. Thus, a person will choose to vote if the costs (C) are outweighed by the benefits (B) that flow from the preferred candidate winning, discounted by the probability of casting the deciding vote (P), and allowing for the consumption benefit (D) that results from the feeling of satisfaction in performing one's civic duty:

$$C < PB + D.$$

Hence, efforts in this literature have turned to developing complex 'group-based' models that explain why voters might get a consumption benefit from voting (Feddersen 2004).

But without clear information on the costs, C, it is difficult to know whether consumption benefits need to be large or small to salvage the rational voter model (Niemi 1976). Accurate estimates also are needed because, if costs and benefits of voting are small, the decision to vote is sensitive to small variations in either term (Aldrich 1993). In fact, if these small effects are hard to measure, individual voter turnout will seem largely random (Matsusaka and Palda 1999). Yet, most studies struggle to measure the costs of voting at the individual level because of inadequate data and resort to using imperfect proxies. For example, Cebula and Toma (2006) use state-level median family income as their sole proxy for the opportunity cost of voting. In addition to being a crude measure, this ignores the early argument by Frey (1971) that even if people with high income face a high opportunity cost, they may participate more in politics—including voting—because of greater productivity in their use of time when performing political activities.[2]

In this paper, we use validated national election unit record survey data from the New Zealand Election Study (NZES) to examine the impact of the opportunity cost of time on voting behavior. We improve on previous work by exploiting both individual level socioeconomic information that allows us to impute wage rates for all sample members and location information that allows us to use Geographic Information Systems (GIS) tools to calculate distance and travel time to the nearest polling place. Combined, this information gives a new, more accurate, measure of the opportunity cost of voting than is used in previous papers in the literature.

The NZES data have several advantages over the data used in previous studies. First, the survey data on whether each respondent voted is *validated* by comparing the data with the lists of votes cast that are held by the registrar of electors in each electorate. In contrast, many surveys used in the literature do not validate self-reports even though voting is prone to over-reporting since it is seen as a socially desirable activity.[3] Systemic overstatement of

[1] Feddersen (2004) considers a two-candidate election with five million voters and candidate 1's expected vote share of 50.1%; the benefit to a voter who prefers candidate 2 must be more than eight billion times greater than the cost of voting, in order for expected benefits to outweigh costs. Such a ratio of benefits is inconceivable for any voter, with typical stakes in an election (in terms of the amount of compensation needed for indifference between who wins) of only a few thousand dollars (Tollison and Willett 1973).

[2] Tollison and Willett (1973) point out that predictions about whether higher or lower income individuals have a greater incentive to vote depend on both income and substitution effects, and so will be *a priori* unclear.

[3] For example, Matsusaka and Palda (1999) use surveys from four national elections in Canada where the rate of self-reported voting in their samples was 16 percentage points higher, on average, than the actual turnout in each election. Similarly, 22% of non-voters in local government elections in Sweden claimed to have voted when they were surveyed (Karp and Brockington 2005).

voting generates a non-random misclassification error, so that when voting is the dependent variable it induces bias in all of the regression coefficients (Hausman et al. 1998).[4] Second, voter registration is compulsory in New Zealand so that variation in individual turnout is not driven by differences in voter registration, as happens in the United States.[5] Third, under New Zealand's Mixed Member Proportional (MMP) voting system, each voter has the same weight in determining the overall proportionality of parties in the Parliament, irrespective of the marginality of the electorate in which they live.[6] Finally, at a general election in New Zealand, all voters have just two votes—one for a Party and one for a Member of Parliament for their electorate; referendums are extremely rare and there are never other jurisdictional positions to fill. Thus, the cognitive costs of voting should nor vary between precincts, unlike in the United States where some voters face many more choices than others.

New Zealand is also an ideal case study for using the accessibility of polling places as a measure of the opportunity cost of time facing potential voters. First, almost all resident voters cast their vote in person on Election Day, rather than beforehand.[7] In contrast, studies of how accessibility affects turnout in the United States have to deal with the complication that a large fraction of voters cast an early vote or vote by mail (Dyck and Gimpel 2005). Second, Election Day is always a Saturday, so it is reasonable to assume that the average voter travels from their home to the polls, rather than from their workplace.[8] This lets us use the polling place closest to the place of residence when estimating the opportunity cost of time. Third, we can assume that little time is spent waiting at the polling booth due to congestion; relative to other countries, New Zealand appears to have ample polling places per voter. The 69 electorates in New Zealand each have an average of about 50 polling places, and at each an average of just 600 votes are cast in the 10 hours that the polls are open.

The rest of the paper proceeds as follows. In Sect. 2, we review the previous literature on using accessibility to measure the cost of voting. In Sect. 3, we describe the survey data we use and how we construct the measures of distance and time costs of voting. In Sect. 4, the estimation strategies and empirical results are presented. The conclusions are in Sect. 5.

2 Previous literature

Outside of economics there is a small but growing literature that uses Geographic Information Systems (GIS) tools to create measures of voting costs. In one of the first such studies,

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4Even more non-randomness may come from the tendency for false reports of voting to be more likely for some demographic groups than others (Silver et al. 1986).

5Even though registration is compulsory, there is no legal compulsion to vote, unlike the case in neighboring Australia.

6The exception is that a party that would not be represented in the Parliament if it got less than the threshold 5% of the party vote can still have Members of Parliament to match its vote share, if it wins at least one electorate seat. While voters in that particular electorate could have more than average influence, this situation occurs only rarely. In the empirical analysis below, the results are robust to including electorate fixed effects.

7In the 2005 national election studied here, less than 7% of votes from resident voters were cast before Election Day.

8Evidence for this assumption also comes from the low share of Special Votes, which are for people voting outside of their home electorate on Election Day. In the 2005 election studied here, these were just 6.7% of total votes cast. Moreover, those who work on Saturdays must be allowed to leave their workplace no later than 3 pm for the purpose of voting (or leave for at least two hours if they have essential work that goes after 3 pm) and their employer cannot make deductions from the employee's remuneration for the time taken off. See Sect. 162 of the *Electoral Act*, 1993.

Gimpel and Schuknecht (2003) investigated the impact that distance to polling stations and impedance (the time and effort of the commute) had on voter turnout in three suburban Maryland counties in the 2000 US presidential election, finding a positive impact of ease of access on turnout. In related work, Dyck and Gimpel (2005) studied the choices that voters in Clark County, Nevada (where Las Vegas is located) made between voting in person on Election Day, voting early (but in person), mailing in an absentee ballot, or not voting. This research was extended to a comparison with Bernalillo County, New Mexico (wherein lies Albuquerque) by Gimpel et al. (2006).

One drawback of these early studies is that they did not use an actual road network to calculate distance (or time). Instead they used "Manhattan distance" which assumes a regular grid layout of streets between origin and destination, with no allowance for sinuosity.[9] This assumption improves upon straight-line ('crow-fly') distance but it still induces non-random measurement error that may bias regression coefficients. The non-randomness arises because actual roads will sometimes follow irregular features such as rivers and broken topography, so Manhattan distance always understates actual distance. In contrast, Haspel and Knotts (2005) use GIS-based road distances to study the impact of accessibility on voting in the 2001 mayoral race in Atlanta. Specifically, they calculate the shortest path over city streets between each individual's home address and the polling place to which he or she was assigned by their optimization software (with a criterion of distance minimization). These distances were quite short, with a median value of 0.69 miles (1.1 kilometers), but they still mattered in determining the odds of voting. For householders with cars, the predicted odds of voting dropped from 0.464 at 0.01 miles (20 meters) to 0.385 at the median distance of 0.69 miles.[10]

More recently, political scientists have exploited natural experiments from changes in the accessibility of polling places following the consolidation of voting precincts. In the California recall election of 2003 that replaced Gray Davis with Arnold Schwarzenegger, Los Angeles County reduced the number of polling places from 5,231 used in 2002 to just 1,885. Brady and McNulty (2011) show that this increased the average distance to the nearest polling place from 0.35 to 0.50 miles, and resulted in a three percentage point decline in in-person voting in consolidated voting precincts compared to unconsolidated ones. Most of this decline is attributed to search costs of finding a new polling place rather than to the transportation cost of greater distance, and is partly offset by a rise in absentee voting. At a much finer spatial scale, McNulty et al. (2009) examine the effects of consolidating eight polling places down to five in the 2006 school budget referendum in the Vestal Central School District of New York State. Even among the habitual voters who participate in these local referendums, consolidation and the increase in average distance (from 0.97 to 1.30 miles) to the polls decreased voter turnout by about seven percentage points.

In summary, this literature from political science suggests that small changes in voting costs may have significant impacts on turnout. But the context for each study is spatially limited, usually considering only a single city or a few counties, so there are reasons to be concerned about the external validity of the findings. Moreover, in almost all of the studies the proxy for cost is just a measure of distance—and sometimes a potentially error-prone measure such as Manhattan distance. In no case has there been an attempt to combine the

[9] Manhattan distance is defined as: $d_i = |(x_1 - x_2) + (y_1 - y_2)|$ where x, y are the longitude and latitude coordinates for the origin (1) and the destination (2).

[10] A drawback of their study is that the data are from voter files, so are restricted to those citizens actually registered to vote, which creates a potential sample selection bias, and also limits the availability of predictor variables compared to what would be available in surveys.

estimates of traveling time with individual-specific time valuations, so as to create an overall dollar cost for the amount of time that would be spent going to vote. There also has yet to be a nationwide study using GIS tools to measure voting costs and their impact on turnout, which is the aim of the current study.

3 Data

Since 1990, the New Zealand Election Study (NZES) has surveyed individuals of voting age (18 and over) after each general election (held every three years), using self-completion postal questionnaires and telephone survey top-ups (Vowles 2010). As noted above, the survey self-reports of voting are validated by checking against the lists of votes cast that are held by the registrar of electors. The surveys also provide a variety of information on respondents' demographic details (age, gender, ethnicity), marital, employment and migration status, level of education, household income (in eight brackets), location and home ownership status. In addition, they include factors found in other studies to influence voting, such as participation in religious observance and prior voting history.

In this study, we use data from the 2005 general election, for which the NZES sampled just over 3,000 respondents.[11] Since the NZES is a postal survey there is no clustering used but sampling weights are formed to ensure national representativeness (hence, dealing with any uneven sampling probabilities and non-response bias). All of the analyses reported below use these weights, along with heteroscedasticity-robust measures of sampling error.

Although the NZES is a postal survey, the exact addresses of respondents are not included in the public use data, so as to maintain confidentiality. But a close approximation is provided with the survey reporting the census meshblock within which each respondent's residence lies. There are almost 40,000 meshblocks in New Zealand, each having an average of just 110 residents, so this is a very finely scaled spatial unit.[12] Moreover, a population-weighted centroid for every meshblock is calculated by Statistics New Zealand from confidential Census address data. We use this population-weighted centroid, rather than the geographic center of the meshblock, as the best estimate of the location of the respondent's dwelling when estimating the road distance and travel time to the nearest polling place.

The names of all polling places used in the 2005 general election were obtained from the New Zealand Electoral Commission website. In most cases, these were located in schools, and civic and church halls. All locations were then geocoded using ESRI ArcGIS Geocoder with the Core Records System street address data and Google Maps to provide exact latitude and longitude coordinates. Occasionally, the Street View feature of Google Maps was used to virtually drive along roads and locate polling places that had no street numbers. For several rural locations, we also contacted key informants (such as local government officers and staff at visitor information centers) to verify the correct locations.

The algorithm within Google Maps for choosing shortest road distances was used to measure the distance between the population-weighted centroid of the meshblock that each

[11] In the raw data files, there are almost 3,700 observations but with missing values for some of the covariates used in our models we have a sample of $n = 3{,}005$. The voting rate and the average distance from the polls are the same in the full sample and the estimation sample, supporting the assumption that the observations are missing at random.

[12] In terms of physical area, a typical urban meshblock would, if perfectly square, have dimensions of just over 200 metres (0.13 miles).

respondent lived in and the nearest polling booth.[13] When calculating travel times, we assume that the mode of transport is by motor vehicle. New Zealand has the second highest car ownership rate in the world (behind Luxembourg), at almost 600 cars per 1000 population. This ownership rate is much higher than would be predicted by income levels (IMF, 2008), due to the low population density and underdeveloped public transport network. It is therefore likely that the typical journey to and from the polls was made by motor vehicle.

In order to convert the travel time estimates into a measure of opportunity costs of time, we follow an approach used in Skinner (1987) to impute estimates for a variable that is absent from one survey using a regression estimated from another survey that contains a richer set of variables.[14] Since the NZES has no information available on wages and earnings, we use data from the 2005 New Zealand Income Survey (NZIS),[15] which collects very detailed labor market data, including wages, for a sample of $n = 13{,}182$ working individuals of voting age, to impute wages in NZES. While the NZES does not collect wage data, it does collect information on personal and household income in brackets, which are strong predictors of individual wage rates in the NZIS. Following Skinner, we estimate a wage regression using the NZIS controlling for all variables common to both surveys and the coefficients from this regression are then used to predict hourly wage rates for all respondents in the NZES.[16]

Since elections in New Zealand take place on a Saturday, our decision to use an imputed wage to value time also means that wages place a value on leisure time in our analysis. There are both legal and economic reasons for this choice. In New Zealand, employees can trade up to one week of their annual holidays for cash, with the week of foregone leisure valued at the higher of the workers' ordinary weekly pay rate at the time of the trade or their average weekly earnings in the previous 12 months.[17] Hence the principle of valuing leisure time according to the wage rate of an individual is well established. In other settings, econometric studies show that the wage rate represents the scarcity value of leisure time (Larson 1993), especially if employees are supplying their equilibrium hours of work.[18] The major deregulation of the New Zealand labor market in the early 1990s introduced considerable flexibility into employment contracts and removed penal rates, where employers have to pay more per hour for overtime or non-standard work hours. Consequently, New Zealand typically ranks near the top of global surveys of countries with the most flexible work arrangements.[19] It is therefore likely that many New Zealand workers supply their equilibrium hours of work,

[13] These were batch processed using the *traveltime* command written for *Stata* by Ozimek and Miles (2011).

[14] Skinner used a regression estimated on Consumer Expenditure Survey data to impute consumption estimates for households in the Panel Study on Income Dynamics.

[15] This survey is run as a supplement to New Zealand's main labor market survey (the Household Labour Force Survey) and hence is equivalent to the March CPS in the United States.

[16] The following controls are included: age, gender, the personal and household income bracket, employment status, two-digit industry and occupation, highest level of education, ethnicity, urbanity and region. The R-squared from this regression is 0.386, indicating that bracketed income along with other socioeconomic characteristics are strong predictors of wage rates.

[17] Details on this policy are available at: http://www.beehive.govt.nz/release/holidays-act-changes-announced (accessed October 1, 2011).

[18] To allow for workers to be either under- or over-employed (hours less than or exceeding the equilibrium) it is necessary to use survey data that captures workers' willingness to trade wages for leisure, and then use switching regression approaches. See Lee and Kim (2005) for an example. Such data are not available in New Zealand.

[19] See for example, http://www.yourbalance.com.au/flexible-work-survey-highlights-worker-friendly-countries/ (accessed October 2, 2011).

implying that the wage rate is the correct measure of the shadow value of their leisure time and the correct implicit price to use when calculating opportunity costs of the time taken to vote.

4 Empirical analysis

4.1 Descriptive statistics

Table 1 presents means and standard deviations of the variables we use in our analysis. Because many rural areas in New Zealand are quite isolated, we present estimates throughout that both include and exclude them. The voting rate was 81.5% in both the national sample and the urban sub-sample (all estimates use population sampling weights). The average distance by road to the nearest polling place is 1.5 kilometers, or just under one mile, while in urban areas it is one kilometer (0.6 miles). This is comparable to the distances found in previous US studies. Based on this distance, the average round trip by car to the nearest polling place would take just five minutes of traveling time, while for urban areas it would take only four minutes. Using the imputed wages, which average just under $28 per hour (US$19.50 at the time of the election), this is equivalent to a time cost of $2.30 (US$1.60) nationally and $1.80 (US$1.25) in urban areas. Obviously, allowing for parking (albeit, often free on Saturdays) and any waiting at the polls would increase this slightly, but there is no information to estimate these costs, unlike the time costs of travel.

The means of the respondent characteristics used in the model predicting turnout are also presented in Table 1. These characteristics include age, gender, and marital status, whether the respondent is an immigrant, and dummy variables for three ethnic groups (Asian and 'other' are the excluded category). Also included in the models are indicators for highest education level, for lack of religious observance, for whether employed, whether a home owner, whether in an urban area, and whether household income is in the bottom three or top three of the eight income brackets used by the survey. Finally, there is an indicator for whether respondents had voted in the local government elections in the previous year, to proxy for their attitude towards the civic duty of voting. All these variables have previously appeared in models predicting individual turnout (see, e.g., Matsusaka and Palda 1999).

4.2 Empirical specification

We use maximum likelihood probit estimation:

$$\Pr(p_j = 1|\mathbf{x}_j) = \Phi(\mathbf{x}_j\beta) \tag{1}$$

where p_j is an indicator variable for whether the jth individual voted in the 2005 general election, Φ is the standard cumulative normal, $\mathbf{x}_j$ is the vector of explanatory variables for individual j and β is the vector of coefficients to be estimated. These probit coefficients are not directly interpretable, but marginal effects for continuous variables can be calculated (at the mean) as:

$$\left.\frac{\partial\Phi(\mathbf{xb})}{\partial x_i}\right|\mathbf{x} = \bar{\mathbf{x}} = \phi(\bar{\mathbf{x}}\mathbf{b})b_i \tag{2}$$

where $\mathbf{b}$ is the vector of estimated coefficients and ϕ is the normal density. For dummy variables, the discrete change in probability when the dummy variable switches from zero to one is calculated as $\Phi(\bar{\mathbf{x}}_1\mathbf{b}) - \Phi(\bar{\mathbf{x}}_0\mathbf{b})$ where $\bar{\mathbf{x}}_1 = \bar{\mathbf{x}}_0 = \bar{\mathbf{x}}$ except that the ith elements of $\bar{\mathbf{x}}_1$ and $\bar{\mathbf{x}}_0$ are set to one and zero, respectively. Only the marginal effects (and z-statistics based on heteroscedasticity-robust standard errors) are reported in the tables that follow.

Table 1 Descriptive statistics

	National sample		Urban sub-sample	
	Mean	Std Dev	Mean	Std Dev
Dependent variable = 1 if voted, 0 if not	0.815	0.389	0.815	0.389
Road distance (km) to closest polling booth	1.530	2.500	1.017	1.183
Travel time (mins) for closest polling booth	5.105	6.216	3.908	3.641
Time cost ($) of reaching polling booth	2.322	3.406	1.837	2.190
Imputed wage ($ per hour)	27.759	17.358	27.968	17.377
Age	45.547	16.155	44.804	16.216
Dummy = 1 if Female	0.483	0.500	0.476	0.500
Dummy = 1 if Married	0.682	0.466	0.662	0.473
Dummy = 1 if not born in New Zealand	0.194	0.396	0.200	0.400
Dummy = 1 if < 3 years at current address	0.113	0.316	0.118	0.322
Ethnic group is European/Pakeha	0.845	0.362	0.839	0.367
Ethnic group is Maori	0.128	0.334	0.123	0.329
Ethnic group is Pacific	0.027	0.162	0.030	0.172
Dummy = 1 if no religious observance	0.121	0.326	0.123	0.329
Highest education level is Tertiary	0.516	0.500	0.535	0.499
Employed full- or part-time	0.702	0.457	0.713	0.452
Urban resident	0.832	0.374	1.000	0.000
Homeowner	0.697	0.459	0.682	0.466
Lower household income (< $16,000)	0.063	0.242	0.063	0.243
Higher household income (> $59,000)	0.489	0.500	0.501	0.500
Voted in 2004 local govt elections	0.503	0.500	0.502	0.500
Sample size	3,005		2,452	

Notes: Means and standard deviations are weighted by the population sampling weights

4.3 Estimation results

We estimate probit models with either distance, or time, or cost as the main independent variable, along with 16 control variables to account for potentially relevant determinants of voting that may be correlated with the cost of going to the polls. The reason for not including distance, time and cost in the same equation is because of the high correlation between these variables, which is highest for distance and time ($r = 0.90$) but is also statistically significant for distance and cost ($r = 0.69$). Hence, the results for each probit model illustrate different aspects of the opportunity cost of voting, while reporting all three models allows readers to interpret the role that the modeling assumptions (such as the speed of road travel and the imputation of wages) have on the results.

The results in Table 2 show that distance, time and cost are all statistically significant determinants of individual turnout. The p-values for the null hypotheses that these variables have no effect on the decision to vote range from 0.02 (time) to 0.04 (distance), indicating that accurately measuring aspects of voting costs can give precisely estimated effects on turnout. In terms of magnitudes, each one kilometer (0.6 mile) increase in distance to the nearest polling place reduces turnout by one percentage point; each minute of traveling time reduces turnout by one-half of a percentage point, and each dollar of opportunity cost

Table 2 Marginal effects from probit models of the decision to vote—national sample

	(1)	(2)	(3)
Road distance (km) to closest polling booth	−0.010		
	(2.09)*		
Travel time (mins) for closest polling booth		−0.005	
		(2.37)*	
Time cost ($) of reaching polling booth			−0.008
			(2.25)*
Age	0.002	0.002	0.002
	(2.01)*	(2.01)*	(2.16)*
Dummy = 1 if Female	−0.031	−0.032	−0.024
	(1.29)	(1.33)	(0.99)
Dummy = 1 if Married	−0.012	−0.012	−0.013
	(0.41)	(0.40)	(0.43)
Dummy = 1 if not born in New Zealand	−0.054	−0.055	−0.053
	(1.49)	(1.49)	(1.46)
Dummy = 1 if < 3 years at current address	−0.034	−0.035	−0.031
	(0.83)	(0.86)	(0.78)
Ethnic group is European/Pakeha	0.034	0.036	0.035
	(0.84)	(0.90)	(0.87)
Ethnic group is Maori	−0.152	−0.150	−0.150
	(3.60)**	(3.61)**	(3.58)**
Ethnic group is Pacific	−0.154	−0.159	−0.154
	(1.76)+	(1.80)+	(1.76)+
Dummy = 1 if no religious observance	0.029	0.029	0.031
	(0.87)	(0.85)	(0.93)
Highest education level is Tertiary	0.021	0.022	0.023
	(0.79)	(0.82)	(0.89)
Employed full- or part-time	0.011	0.010	0.019
	(0.39)	(0.34)	(0.64)
Urban resident	−0.027	−0.031	−0.017
	(0.81)	(0.92)	(0.51)
Homeowner	−0.016	−0.014	−0.015
	(0.50)	(0.45)	(0.47)
Lower household income (< $16,000)	−0.010	−0.011	−0.015
	(0.20)	(0.24)	(0.31)
Higher household income (> $59,000)	0.041	0.041	0.050
	(1.50)	(1.50)	(1.80)+
Voted in 2004 local govt elections	0.065	0.065	0.064
	(2.61)**	(2.63)**	(2.55)*
Pseudo-R^2	0.061	0.063	0.061
Wald test: All slopes = 0	80.27**	80.42**	79.08**

Notes: The coefficients give the change in the probability of voting for a one-unit change in each explanatory variable. Numbers in ( ) are z-statistics, calculated from heteroscedasticity-robust standard errors. The sample size is 3,005 and the estimates are weighted by the population sampling weights. The models all include an intercept. + significant at 10%; * significant at 5%; ** significant at 1%

reduces it by 0.8 percentage points. A standard deviation increase in any of these three variables would reduce turnout by three percentage points.

In contrast to the importance of the cost terms, most of the individual characteristics are not statistically significant determinants of the voting decision. Amongst the demographic factors, only age and ethnicity matter; voting rates go up by two percentage points for every ten years of age (the affect appears linear, since a quadratic in age was statistically insignificant), and are approximately 15 percentage points lower for the Maori and Pacific ethnic groups, although only the effect for Maori is precisely estimated. Voting rates are higher for respondents from higher income households, but the effect is imprecisely measured and only attains borderline statistical significance ($p = 0.07$) in model (3).

The other variable that appears to matter to voting in the general election is whether the individual voted in the local government election of the previous year. The turnout for those who did vote previously is almost seven percentage points higher. One concern with including voting in the local government elections as a covariate might be that this voting could also reflect the accessibility of the polling places. However, in New Zealand, all local government elections are postal ballots, so the correlation between distance to the general election polling places and whether the person voted in the local government election is zero ($r = -0.005$). Thus, there is no change in the magnitude of the distance, time or cost variables (and a slight increase in their statistical significance) if the previous voting behavior is excluded from the model.

The distance to the nearest polling place varies between electorates, since the electorate boundaries are redistricted after every Census to ensure roughly equal populations but the population density is uneven over space. Since turnout also varies between electorates, one concern with the results reported in Table 2 may be that they reflect omitted variable bias. To address this, the models are re-estimated with fixed effects for each electorate (Table 3). While these electorate fixed effects are (jointly) statistically significant, and cause the pseudo-R^2 to double, they have no effect at all on the magnitude and statistical significance of the distance, time and cost variables.

Another possible concern with the results reported in Table 2 could be that the findings of significant distance effects are driven by a small proportion of rural voters who may face a longer drive to the polls.[20] In fact, the opposite is true. The impact of distance, time and cost in reducing voter turnout is stronger in urban areas (Table 4). Specifically, for the urban population, each one kilometer increase in distance to the nearest polling place reduces turnout by two percentage points; each minute of traveling time reduces turnout by 0.8 of a percentage point, and each dollar of opportunity cost reduces it by two percentage points. The effect of the opportunity cost of time in lowering urban turnout is very precisely estimated, with $p = 0.001$.[21]

To illustrate how individual turnout can be affected by the opportunity costs of traveling to the polls, the models in column (3) of Table 2 and Table 4 are used to simulate turnout probabilities as the opportunity cost increases from \$1 to \$10 (US\$0.70 to US\$7). In these simulations, all of the other variables are held at their mean, so the confidence intervals expand as the simulations approach the extremes of the observed costs (Fig. 1). Nevertheless, it is clear that even small increases in the opportunity cost of traveling to vote have large effects on voter turnout. For example, at an opportunity cost of \$10 (two standard deviations

[20] However, even in the rural sector the maximum distances are not especially large, with the 99th percentile of road distances to the closest polling place being 22 kilometers (14 miles).

[21] These results are also robust to including electorate fixed effects.

Table 3 Marginal effects from probit models of the decision to vote—national sample with electorate fixed effects

	(1)	(2)	(3)
Road distance (km) to closest polling booth	−0.010		
	(2.33)*		
Travel time (mins) for closest polling booth		−0.005	
		(2.56)*	
Time cost ($) of reaching polling booth			−0.007
			(2.46)*
Age	0.002	0.002	0.002
	(1.85)+	(1.86)+	(2.03)*
Dummy = 1 if Female	−0.029	−0.030	−0.022
	(1.36)	(1.40)	(1.03)
Dummy = 1 if Married	−0.018	−0.018	−0.019
	(0.74)	(0.74)	(0.77)
Dummy = 1 if not born in New Zealand	−0.068	−0.069	−0.068
	(2.06)*	(2.07)*	(2.04)*
Dummy = 1 if < 3 years at current address	−0.037	−0.038	−0.035
	(1.05)	(1.07)	(0.99)
Ethnic group is European/Pakeha	0.035	0.036	0.036
	(0.97)	(0.98)	(0.98)
Ethnic group is Maori	−0.130	−0.126	−0.126
	(2.26)*	(2.21)*	(2.20)*
Ethnic group is Pacific	−0.121	−0.125	−0.122
	(1.55)	(1.59)	(1.55)
Dummy = 1 if no religious observance	0.031	0.030	0.033
	(1.07)	(1.03)	(1.14)
Highest education level is Tertiary	0.011	0.013	0.014
	(0.47)	(0.54)	(0.60)
Employed full- or part-time	0.014	0.012	0.021
	(0.54)	(0.49)	(0.82)
Urban resident	−0.043	−0.045	−0.033
	(1.46)	(1.54)	(1.08)
Homeowner	−0.010	−0.009	−0.009
	(0.35)	(0.31)	(0.31)
Lower household income (< $16,000)	−0.037	−0.039	−0.043
	(0.81)	(0.87)	(0.95)
Higher household income (> $59,000)	0.043	0.043	0.052
	(1.74)+	(1.76)+	(2.07)*
Voted in 2004 local govt elections	0.075	0.076	0.073
	(3.39)**	(3.43)**	(3.29)**
Electorate fixed effects	Yes	Yes	Yes

Table 3 (*Continued*)

	(1)	(2)	(3)
Pseudo-R^2	0.126	0.127	0.126
Wald test: All slopes = 0	196.32**	200.67**	193.28**

Notes: The coefficients give the change in the probability of voting for a one-unit change in each explanatory variable. Numbers in ( ) are z-statistics, calculated from heteroscedasticity-robust standard errors. The sample size is 3,005 and the estimates are weighted by the population sampling weights. The models all include an intercept and 64 electorate fixed effects. + significant at 10%; * significant at 5%; ** significant at 1%

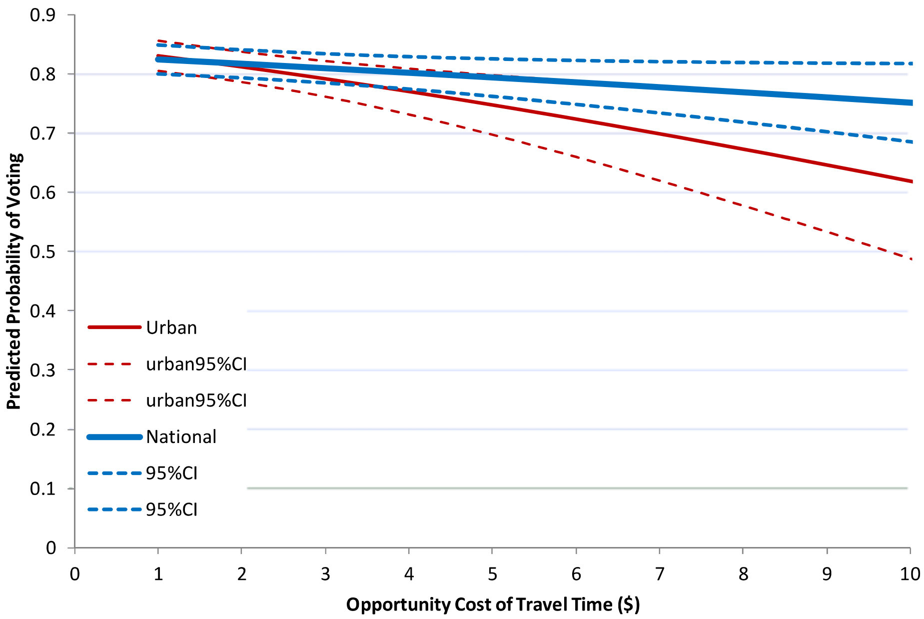

Fig. 1 The effect of the opportunity cost of travel time on predicted turnout

above the national mean), the predicted national turnout would be just 75%, which is down seven percentage points from the mean. In urban areas, the decline is even larger, with a predicted turnout of just 72% when opportunity costs are two standard deviations above the urban mean (approximately $6).

4.4 Endogenous sorting

One potential concern about the validity of our estimates is that perhaps individuals with more public spirit may choose to live closer to the sort of social facilities that are selected as polling locations, since this could minimize their travel to these facilities when serving in churches, volunteering in community halls and so forth. This type of endogenous sorting by location could cause the observed partial correlation between distance and turnout to reflect the confounding effect of public-spiritedness. The ideal way to control for endogenous sorting would be to rely on a natural experiment, such as from the consolidation of polling precincts, that causes exogenous variation in distance to the nearest polling place. But this is

Table 4 Marginal effects from probit models of the decision to vote—urban sample

	(1)	(2)	(3)
Road distance (km) to closest polling booth	−0.019		
	(2.17)*		
Travel time (mins) for closest polling booth		−0.008	
		(2.57)*	
Time cost ($) of reaching polling booth			−0.020
			(3.47)**
Age	0.002	0.002	0.002
	(1.85)+	(1.89)+	(2.01)*
Dummy = 1 if Female	−0.038	−0.038	−0.023
	(1.45)	(1.44)	(0.88)
Dummy = 1 if Married	0.009	0.008	0.009
	(0.26)	(0.24)	(0.29)
Dummy = 1 if not born in New Zealand	−0.045	−0.044	−0.047
	(1.12)	(1.11)	(1.16)
Dummy = 1 if < 3 years at current address	−0.039	−0.038	−0.034
	(0.87)	(0.87)	(0.78)
Ethnic group is European/Pakeha	0.051	0.054	0.054
	(1.16)	(1.22)	(1.23)
Ethnic group is Maori	−0.155	−0.152	−0.154
	(3.43)**	(3.44)**	(3.47)**
Ethnic group is Pacific	−0.171	−0.180	−0.175
	(1.90)+	(1.98)*	(1.93)+
Dummy = 1 if no religious observance	0.017	0.018	0.019
	(0.45)	(0.48)	(0.51)
Highest education level is Tertiary	0.030	0.029	0.035
	(1.02)	(1.00)	(1.22)
Employed full- or part-time	0.001	0.003	0.016
	(0.02)	(0.09)	(0.51)
Homeowner	−0.011	−0.011	−0.007
	(0.32)	(0.31)	(0.20)
Lower household income (< $16,000)	−0.036	−0.037	−0.039
	(0.68)	(0.70)	(0.75)
Higher household income (> $59,000)	0.043	0.042	0.062
	(1.42)	(1.39)	(1.96)+
Voted in 2004 local govt elections	0.058	0.059	0.058
	(2.13)*	(2.18)*	(2.16)*
Pseudo-R^2	0.074	0.076	0.083
Wald test: All slopes = 0	81.92**	82.09**	84.79**

Notes: The coefficients give the change in the probability of voting for a one-unit change in each explanatory variable. Numbers in ( ) are z-statistics, calculated from heteroscedasticity-robust standard errors. The sample size is 2,452 and the estimates are weighted by the population sampling weights. The models all include an intercept. + significant at 10%; * significant at 5%; ** significant at 1%

not possible in New Zealand as the numbers and locations of polling places have largely remained the same since the consolidation of electorates after the adoption of the MMP voting system in 1996.

However, indirect evidence suggests that this type of sorting is unlikely to have a qualitative impact on our estimates. First, our estimated regression models already include a proxy for public-spiritedness: whether the person previously voted in the local government elections. Since this voting is by postal ballot it is orthogonal to polling place distance, but it is highly relevant to turnout, presumably because it captures public-spirited attitudes of the individual. Second, only a subset of schools, churches and community halls are selected as polling locations; specifically, just over one-half of schools and less than one-quarter of churches and community halls are used.[22] So if there is residential sorting along these lines, some publicly spirited individuals who desire to live close to such facilities will locate near to the ones that are *not* used as polling places (but are otherwise perfectly acceptable schools, churches and community halls), ensuring that location relative to the facilities that are used as polling places is approximately random.

We are able to test whether this is, in fact, the case by combining the NZES data with external data on road distances from each meshblock to the nearest school (regardless of whether the school was used as a polling place) and then re-estimating our main regression model also controlling for the distance to the nearest school along with the distance to the nearest polling location. When we estimate this model, we find that the distance to the nearest school is not a statistically significant determinant of voting and has a marginal effect that is less than one-quarter of the marginal effect for distance to the polling place.[23]

4.5 Measurement error and non-linearities

Another potential concern about the validity of our estimates is that either measurement error in the distance to polling locations or non-linearities in the relationship between distance and the likelihood of voting may be biasing our results. For example, the finding that distance, time and cost appear to have larger impacts on urban than rural turnout could reflect measurement error since rural meshblocks are typically larger and hence may have a greater error of approximation from using the population-weighted centroid as the location measure. This sort of measurement error will be random, overstating distance for some and understating for others, so the usual result of attenuation bias due to classical measurement error would be expected to reduce the magnitude of the marginal effects more in the rural areas. Estimates for rural areas will also be more sensitive to any non-linearities in the distance relationship since distances to polling locations are typically larger than in urban locations. To test the sensitivity of the results to these two possibilities, several additional analyses were conducted with the results of these reported in Table 5.

For our first sensitivity analysis, we calculate the length of the perimeter of each meshblock in which the survey respondents resided and use this as an indicator for potential measurement error. The perimeter is used rather than the meshblock area because it is not only

[22] Approximately 1,400 polling places in 2005 were in schools but the Ministry of Education lists over 2,500 schools in their directory. Approximately 1,100 polling places in 2005 were in churches or community halls but an enumeration in 2004 found over 3,000 places of worship in New Zealand (Statistics New Zealand, 2007) and well over 1,000 town and community halls.

[23] These data on the distance to schools were kindly provided to us by Jaime Pearce and are described in Pearce et al. (2006). The other facilities studied by Pearce et al. (2006) are health- and exercise-related, and so are less relevant to the sorting hypothesis, and do not include churches or community halls. The results of these additional probit models where distance to the nearest school was either added to or used in place of the distance to the nearest polling place variable are available from the authors.

Table 5 Sensitivity analyses

	Distance	Time	Cost
Panel A			
Interaction terms—national sample			
Levels specification	0.018	0.005	0.010
	(1.32)	(1.20)	(1.21)
Logarithmic specification	0.020	−0.000	−0.002
	(0.69)	(0.02)	(0.14)
Interaction terms—urban sample			
Levels specification	0.002	−0.001	−0.004
	(0.10)	(0.12)	(0.36)
Logarithmic specification	0.003	−0.002	−0.007
	(0.10)	(0.16)	(0.59)
Panel B			
Marginal effects—national sample			
Linear specification	−0.010	−0.005	−0.008
	(2.09)*	(2.37)*	(2.25)*
Logarithmic specification	−0.028	−0.010	−0.014
	(1.83)+	(2.02)*	(2.30)*
Quadratic specification—linear term	−0.025	−0.011	−0.024
	(2.33)*	(2.79)**	(2.86)**
Quadratic specification—squared term	0.001	0.001	0.001
	(1.36)	(1.60)	(1.96)+
Marginal effects—urban sample			
Linear specification	−0.019	−0.008	−0.020
	(2.17)*	(2.57)**	(3.47)**
Logarithmic specification	−0.035	−0.011	−0.015
	(2.34)*	(2.10)*	(2.41)*
Quadratic specification—linear term	−0.044	−0.019	−0.025
	(2.20)*	(2.76)**	(2.18)*
Quadratic specification—squared term	0.003	0.001	0.001
	(1.42)	(1.94)+	(0.57)
Panel C			
Predicted probabilities—national sample			
Evaluation point (ca. +2 std deviations)	7 km	18 minutes	$10
Linear specification	0.756	0.747	0.752
	[0.034]	[0.035]	[0.034]
Logarithmic specification	0.747	0.717	0.743
	[0.055]	[0.038]	[0.038]
Predicted probabilities—urban sample			
Evaluation point (ca. +2 std deviations)	4 km	12 minutes	$6
Linear specification	0.755	0.747	0.724
	[0.033]	[0.033]	[0.033]

Table 5 (*Continued*)

	Distance	Time	Cost
Logarithmic specification	0.730	0.742	0.757
	[0.048]	[0.035]	[0.036]

Notes: The models include all other variables described in Table 1 as covariates. Other notes are as per Tables 2, 3, 4. Robust z-statistics in ( ), delta method standard errors in [ ]; $^{+}$ significant at 10%; * at 5%; ** at 1%

the size, but also the irregularity of shape, that could impair the accuracy of the population-weighted centroid as a measure of location for a respondent within a meshblock. Next, a dummy variable identifying the sub-samples with shorter than median or longer than median perimeters was interacted with the distance, time and cost variables and hypothesis tests were conducted of whether the marginal effects were the same across the two sub-samples. Since these tests may also pick up the effect of non-linearities, with marginal effects changing depending on the point where they are evaluated, this analysis was carried out in both levels and logarithms.

The results for the interaction variables reported in Panel A of Table 5 suggest that there is no significant effect of attenuation bias due to potential measurement error. While several of the marginal effects appear slightly less negative for the sub-sample coming from meshblocks with longer perimeters (except in the models using the specification with the logarithms of time and cost), these differences are never statistically significant. Moreover, when this test is repeated just for the urban sample, the same result, of no difference in marginal effects between long and short perimeter meshblocks, is found.

For our second sensitivity analysis, we re-estimated the models reported in Tables 2 and 4 using either the logarithm of distance, time and cost, or quadratics for these variables, in place of the linear, levels specifications reported previously. The results are presented in Panel B of Table 5, with the marginal effects from the linear specification also reported for ease of comparison. The marginal effects from the logarithmic specifications are all statistically significant, in both the national sample and the urban sample. Hence, this specification might therefore be statistically more appropriate. However, it turns out that the choice of levels versus logarithms has little impact on the substantive results. The predicted probabilities of voting are largely the same whether the model is estimated with voting costs in either levels or logarithms. This is illustrated in Panel C of Table 5, using the predicted probabilities at the integer values of distance, time or cost that are closest to two standard deviations above the mean.[24]

In contrast to when we measure costs in logarithms, the use of quadratic specifications for these variables is not supported by the data. In only two of the six combinations of samples and opportunity cost variables are the squared terms statistically significant, while the linear terms in the quadratic always retain their statistical significance in all six combinations. Since the squared terms are generally statistically insignificant, predicted values from the quadratic specification were not calculated. Based on these findings, it appears that the results reported in Tables 2, 3, 4 and Fig. 1 are not sensitive to the assumption of a linear relationship between voting costs and the probit index function.

[24]Tables of predicted values for every integer value of distance, time and cost from zero to two standard deviations above the mean are available from the authors. Closer to the mean there is even less difference between the predictions from the different specifications.

4.6 Controlling for distance versus opportunity cost

One of the main contributions of our paper is that we measure the opportunity cost of voting for individuals in a national election as opposed to just the distance to the nearest polling location. In this section, we test whether controlling for opportunity cost leads to a better model fit than just controlling for distance. The two models with road distance and opportunity costs of travel time as the main independent variable of interest are non-nested, in the sense that it is not possible to impose a set of linear restrictions to derive one model from the other. Forming a 'compound' model with both distance and costs is not advisable, both because of the potential multicollinearity problem described above and also because such an approach is a less powerful statistical procedure than are formal non-nested tests (Pesaran 1982). Instead, we use the Pesaran (1974) version of a Cox likelihood ratio test of the validity of one linear model, H_0 as opposed to its non-nested alternative H_1.

The test can be described in general terms, as follows:

$$H_0: \quad y = x_0 b_0 + u_0$$
$$H_1: \quad y = x_1 b_1 + u_1$$

where x_0 and x_1 are matrices of n observations on explanatory variables that are not linear combinations of one and other, b_0 and b_1 are corresponding parameter vectors, u_0 and u_1 are random errors with zero mean and variance-covariance matrices $\sigma_0^2 I$ and $\sigma_1^2 I$ (I is an identity matrix of order n). Constructing the test statistic involves six steps, where in what follows we use the notation $M_i = I - x_i(x_i' x_i)^{-1} x_i'$, $i = 0, 1$:

(i) Regress y on x_0 to form $\hat{y} = x_0 \hat{b}_0$
(ii) Regress the fitted values from (i), $x_0\hat{b}_0$, on x_1 to form residuals: $M_1 x_0 \hat{b}_0$
(iii) Calculate the sum of squared residuals from (ii), $\hat{b}_0' x_0' M_1 x_0 \hat{b}_0$
(iv) Regress the residuals from (ii) on x_0
(v) Calculate the sum of squared residuals from (iv), $\hat{b}_0' x_0' M_1 M_0 M_1 x_0 \hat{b}_0$
(vi) Calculate the test statistic $N = \sqrt{s/\hat{v}}$ which is $N(0, 1)$ under H_0, where:

$$s = (n/2)\ln\{\hat{\sigma}_1^2/[\hat{\sigma}_0^2 + (1/n)(\hat{b}_0' x_0' M_1 x_0 \hat{b}_0)]$$
$$\hat{v} = (\hat{\sigma}_0^2 \hat{b}_0' x_0' M_1 M_0 M_1 x_0 \hat{b}_0)/(\hat{\sigma}_0^2 + \hat{b}_0' x_0' M_1 x_0 \hat{b}_0)^2$$

and where $\hat{\sigma}_0^2$, $\hat{\sigma}_1^2$ are SSE_0/n and SSE_1/n, where SSE is sum of squared errors.

The decision procedure for the test is to reject H_0 for negative (since this is a lower tail test) values of $\sqrt{s/\hat{v}}$ exceeding the critical value from the standard normal tables. After testing H_0 versus H_1, the procedure is reversed with H_1 replacing H_0 in the above steps.

To implement the Pesaran non-nested test, the models in columns (1) and (3) of Table 4 were re-run, using Ordinary Least Squares (OLS) so that the interpretation is as a Linear Probability Model (LPM). This is because the likelihood ratio theory for the Pesaran (1974) non-nested test is developed for OLS rather than for Probit models. This change in the estimator to an LPM rather than Probit makes almost no difference compared with what is reported in Table 4, with the point estimates on distance and cost both remaining at −0.02 and the t-statistics being approximately 0.2 lower than the z-statistics reported in Table 4.

When the distance-based model is H_0 it is soundly rejected against H_1, the cost-based model. The test statistic is −2.59, which is statistically significant at the $p < 0.01$ level. However, when the test procedure is reversed, with H_1 replacing H_0 as the model under test, there is no rejection of the cost-based model in favor of the distance-based model. Specifically, the test statistic is only 0.34, which is not statistically significant ($p < 0.37$).

In other words, this direct confrontation of the two models favors the use of an opportunity cost of time variable, rather than just using road distance as a proxy. Thus, the effort to exploit individual-level socioeconomic information so as to impute wages for all sample respondents and then calculate the dollar value of their opportunity cost of travel time is supported by these results. The non-nested testing results imply that this new, more accurate measure of the opportunity cost of voting should be used instead of the simpler, distance-based proxies that previously have been used in the US literature.

5 Conclusions

In this paper, we use validated national election unit record survey data from the New Zealand Election Study (NZES) to examine the impact of the opportunity cost of time on voting behavior. Our analysis finds that small increases in the opportunity costs of time have large effects in reducing voter turnout for a national election in New Zealand. For example, at an opportunity cost of $10, the predicted national turnout would be just 75%, which is down seven percentage points from the mean. Our findings thus confirm the conjecture first made by Niemi (1976: 117) that "if the *B* (benefits) or *PB* (benefits weighted by the probability that a person's vote matters) term is indeed quite small, then a small increase in the cost of voting—such as driving a mile instead of a half-mile to the polls—would significantly reduce turnout."

While a number of previous papers, such as Haspel and Knotts (2005), who examine the 2001 mayoral race in Atlanta, and McNulty et al. (2009), who examine a school budget referendum in the Vestal Central School District of New York State, find similar results, the current paper makes a major contribution to the literature by examining the impact of voting cost on voting behavior in a national election in a country with ample polling places per voter, weekend voting, little road congestion, and little use of absentee ballots. Furthermore, New Zealand does not have state/provincial governments, an upper house of parliament, or an elected executive or judiciary. Hence, the triennial election for national parliament is the only politically important election in New Zealand, as well as the only one that involves in-person voting. Showing that small opportunity costs of voting matter for voter turnout even in this setting extends and corroborates the findings of the previous, more spatially limited, case studies.

There are at least two important implication of our finding that opportunity costs are low, but that turnout is still sensitive to those low costs. First, if these low costs were not able to be accurately measured, then the decision to vote would (erroneously) appear largely to be random, and random voting presents a seemingly difficult challenge for the rational voter model. Second, our results imply that efforts by theorists to maintain the rational voter model by explaining why voters may get a consumption benefit from voting do not have to accommodate large consumption benefits. Since the decision to vote is sensitive to small variations in cost, the consumption benefit of voting also can be quite small.

Our paper also makes a second, methodological, contribution by showing that measuring the opportunity cost of time needed for voting for each individual provides a more accurate measure of voting costs than just measuring the distance to the nearest polling location or the average income of the local population as has been done in previous papers in the literature. In particular, we combine data on individual level socioeconomic information that allows us impute wage rates for all sample members with location information that allows us to use Geographic Information Systems (GIS) tools to calculate distance and travel time by road from each individual's place of residence to the nearest polling place. Non-nested tests provide formal evidence in favor of the greater explanatory power of this cost-based measure, compared with simpler (and more commonly used) distance measures of the opportunity cost of voting. While some previous studies have also used these innovative GIS methods, they have not combined them with labor market or socioeconomic information that allows a more precise valuation of time costs. The findings here suggest that both sources of information are needed to measure the opportunity costs of voting accurately.

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