Supplementary Material

This appendix presents extensive supporting material for the simulations and data analysis in the main text of the paper. This material is divided into 4 sections:

1. Simulation details
2. Alternative simulations
3. Data and measurement details
4. Alternative data analyses

The results in each of these sections support the substantive conclusions in the paper.

A1. Simulation details

The simulation begins by creating a small-world network with a population of 1000 individuals. The small world network reflects two important characteristics of observed social network. The first is a high level of clustering, such that an individual is much more likely to be friends with her friends' friends than a randomly chosen individual. While observed social networks often exhibit a high level of clustering (or transitivity) they also exhibit small social distances. While these two properties seem to work at cross-purposes, small world networks exhibit both a high level of clustering and low social distance. To create a small world network, the simulation begins by connecting each individual to her nearest four neighbors. This creates a high level of clustering, but also long social distances. To reduce social distances, these connections are randomly re-wired to some other node in the network with a small probability. These re-wired connections provide a bridge across the network that can substantially reduce social distances while having only a small effect on clustering (Watts and Strogatz 1998). Figure A1 shows the main results plotted over different rewiring probabilities. Higher values of the rewiring probability are associated with lower social distances and lower clustering, but had little effect on the results. The graph shown in the main body of the paper used a rewiring probability of 0.1.

Using this network structure, the paper reports two simulations, the first establishes a baseline level of turnout for comparison, and the second models the effect of a change in the registration deadline. In both simulations, individuals are assumed to participate when the proportion of their associates that are voters exceeds the individual's threshold (Siegel 2009). More formally, individuals in the simulation vote if $k_i \leq v_i$. K_i represents an individual's threshold for voting. V_i represents the turnout rate among one's neighborhood. The neighborhood is defined as the set of individuals with whom one shares a connection. That is, there must be a direct link between A and B in order for B to be in A's neighborhood. The k_i values are drawn from a random uniform distribution.

To incorporate voter registration, 80% of the individuals in the model are classified as registered voters (and are thus eligible to vote) and the other 20% are not registered voters. Since registering to vote entails an additional cost to this 20% of the population, the individuals with the highest thresholds are designated as non-registered voters. This makes it relatively less likely that they will vote, corresponding to higher costs of registration. In the first simulation, the deadline is assumed to have passed, and these individuals are not eligible to vote (i.e. k_i is set to a value greater than 1). The v_i values are calculated by finding the proportion of voters in a person's neighborhood. The contagion process is allowed to run for 20 iterations, such that turnout is calculated for each iteration for each individual. After the 20th iteration, the turnout rate is recorded. This is repeated 1000 times and the mean turnout rate of the 1000 simulations is recorded.

The second simulation is identical to the first, except in this case the non-registered voters have an opportunity to vote. The process is the same, except in this model, non-registered voters can become voters if their neighborhood turnout rate exceeds their threshold by the 10th iteration. After the 10th iteration, non-registrants that had not cleared the threshold are no longer eligible to vote (i.e. k_i is set to a value greater than 1). Otherwise the simulations are identical. The simulation of shorter deadlines is thus relatively conservative as the thresholds for non-registrants are very high and there is a short time span in which the threshold can be met. The effect of registration deadlines is shown in the main text. The effects are plotted for different values of the rewiring probability in Figure A1, where each panel represents a different rewiring probability.

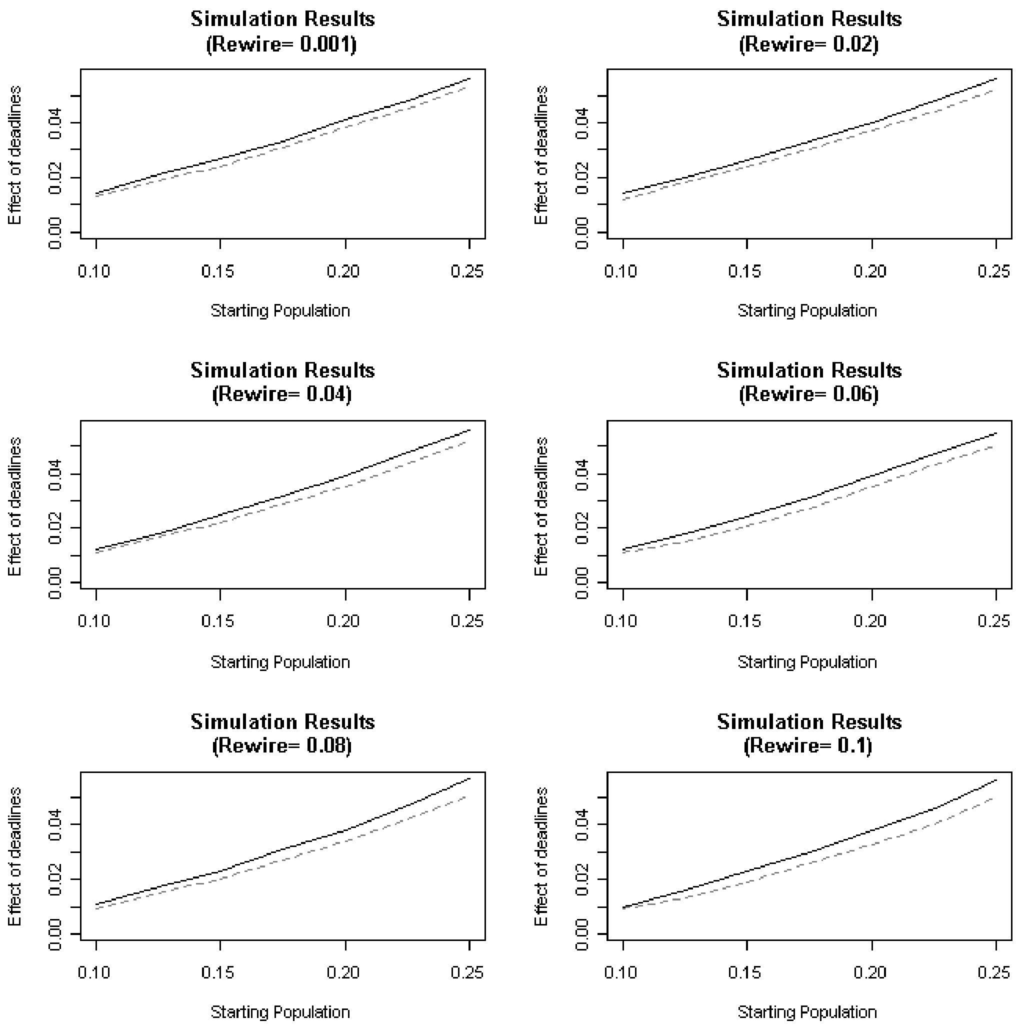
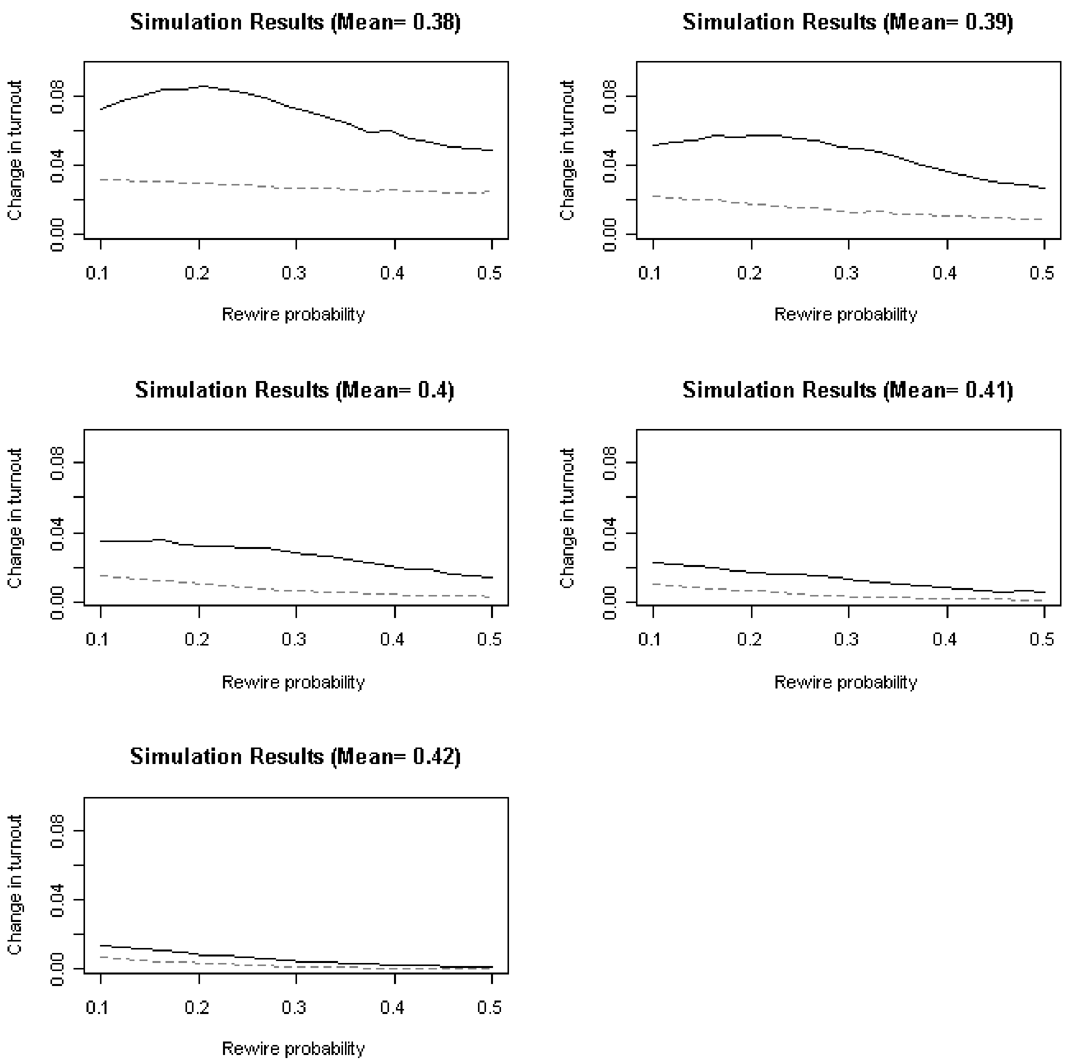


Figure A1: Simulation results. This graph show the effect of shorter deadlines plotted against the size of the starting population. Each panel represents a different rewiring probability used to create the small world network. As the rewiring probability increases over this range, there is a decrease in average social distance and a more gradual decrease in clustering.

A2. Alternative simulations

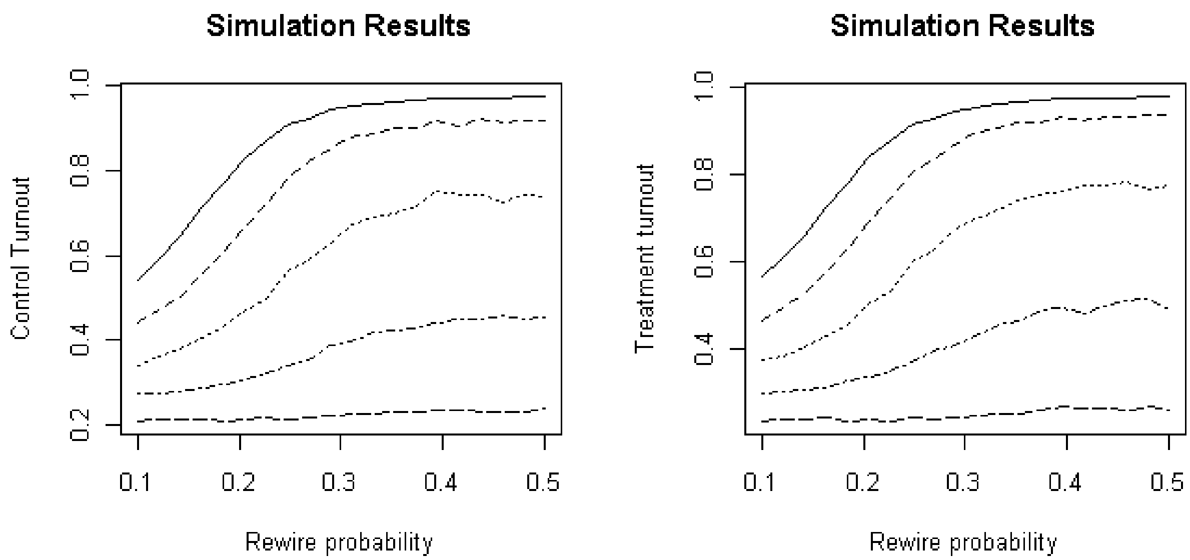
Several alternative simulations were performed. The first used the same parameters as the simulation presented in the main body of the paper, except the thresholds were drawn from a normal distribution (with varying means and a standard deviation of .25), representing thresholds that more closely centered around a common mean. Since the normal distribution is not bounded by 0, any individual with a threshold of 0 or lower is self-mobilizing and this is not further adjusted in the simulation. Rather, the simulation was performed for different mean values of the thresholds, which has the same effect of adjusting the number of self-mobilizing voters as the simulation presented in the main text. The results are shown in Figure A2. From this figure, we can see that the substantive implications of the simulation are the same as the simulation in the main text.

Figure A2: Simulation of shorter deadlines (normally distributed thresholds)



Several other simulations were also conducted. Instead of conducting the simulations for 20 iterations, a separate set of simulations were allowed to run until no individual changed their behavior for 50 consecutive iterations (Siegel 2009). A graph of the results is shown below (Figure A3), which is very similar to the results from the simulation using 20 iterations (Figure A2).

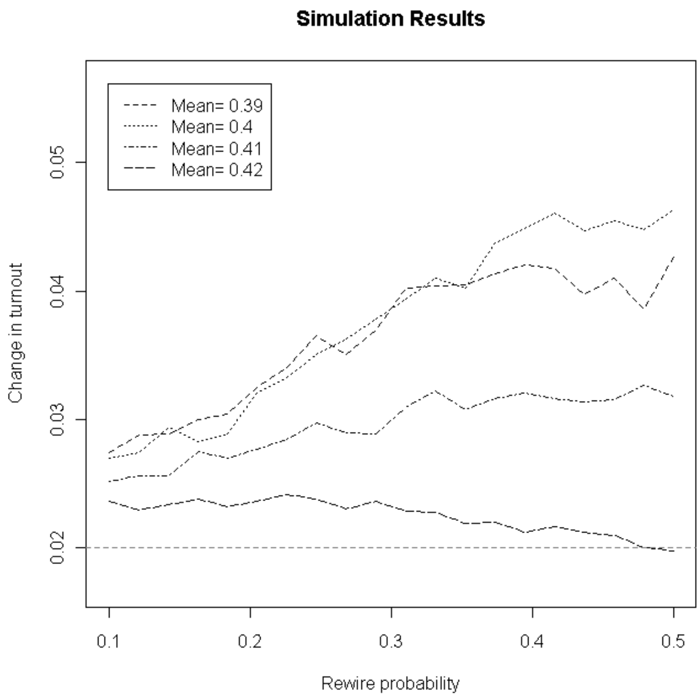
Figure A3: Simulations with longer iterations



Simulation with a fixed direct effect on turnout

While existing research reviewed in the paper suggests that voter turnout is contagious, it is not known whether voter registration is itself contagious. To account for the possibility that registration is not contagious, several simulations were performed in which the direct effect of registration deadlines was exogenously set to 2%. That is, shorter registration deadlines lead to a direct 2% increase in voter turnout, and potentially greater effects through the indirect contagion process. In this simulation, deadlines were assumed to have a direct 2% effect on turnout. This was done by determining the 2% of individuals that had thresholds close to but greater than 0. These individuals have low thresholds and are thus are already likely to vote. The simulation results are shown in Figure A4.

Figure A4: Simulation results with a fixed direct effect on turnout



Two other simulations were performed by incorporating the treatment effect at different points in the process. One included the 2% increase in the middle of the simulation (iteration 10). After this stage, the 2% of non-voters that were the closest to meeting the threshold were switched to voters. Another simulation phased in the increase over iterations, such that there was a 0.1% increase for each iteration (2% total effect). Results from these models are shown in Figures A5 and A6. As anticipated, these models generally show stronger effects of the treatment, as the 2% increase affects a larger number of marginal voters whereas in the initial stage (as presented in the paper) the reform increases turnout among individuals that are already likely to vote. These simulations might more naturally model shorter registration deadlines as more voters enter the electorate throughout the campaign. The body of the paper reports results from a more conservative simulation, and the alternative models suggest substantively similar conclusions.

Figure A5: Change in turnout when increase occurs at the midpoint

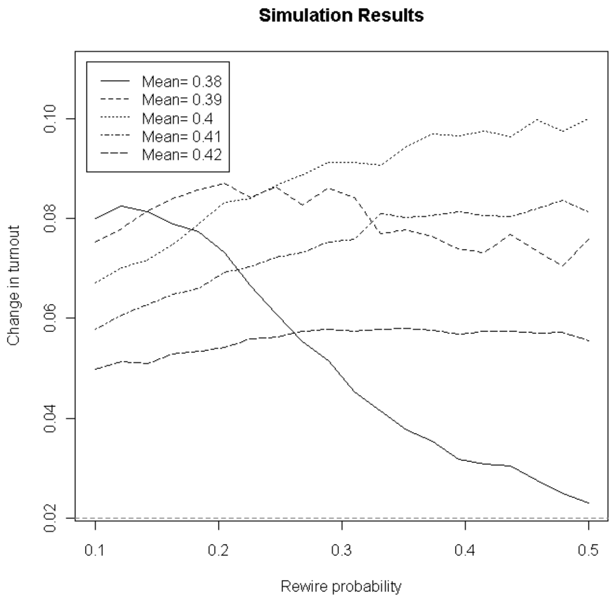
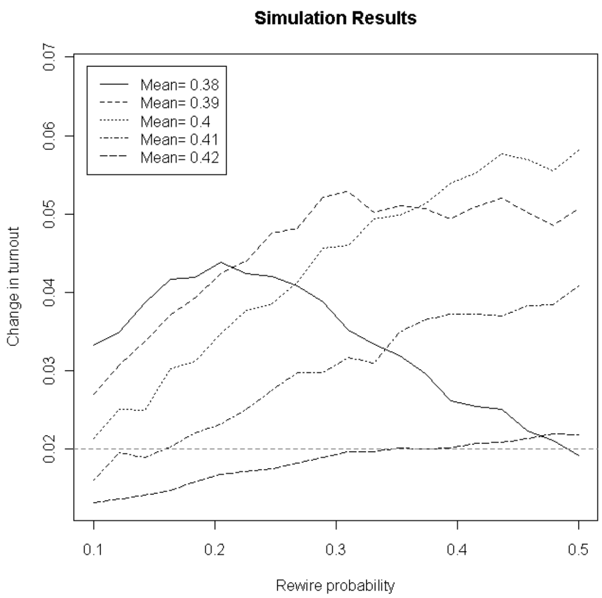


Figure A6: Change in turnout, when increase occurs gradually



A3. Data and Measures

The aggregate-level data include observations at the county level. The data span all regular general elections from 1992 to 2004 when there was at least one statewide-office on the ballot. The spatial domain is all counties or other local governments that have jurisdiction over the administration of elections (such as some townships in Virginia). All states are included except for Alaska, which administers elections at the borough level. These data were primarily gathered from official sources that were either available from Secretaries' of State websites or by request. When these sources were insufficient, other data were coded from the *America Votes* series issued biennially by CQ Press. From these sources, voter turnout data was obtained for 98.8% of the possible cases, and voter registration data was obtained for 90.3% of the cases. The only missing observations for voter turnout are for elections in which there were no statewide races on the ballot. There are more cases with missing voter registration figures due mainly to data availability and the exclusion of North Dakota and Wisconsin. These two states are excluded as North Dakota does not have a registration requirement and Wisconsin did not require registration for residents of certain municipalities with less than 5,000 people (until January 1, 2006).

A similar measure was created for rates of voter registration using data on the number of registered voters. Voter registration numbers can be somewhat inaccurate if they contain individuals that have since moved away from the jurisdiction or that are recently deceased. In some instances, voters can also be erroneously removed from registration lists, either through a prolonged period of inactivity or perhaps through administrative errors, though incorrectly high registration counts seem to be a more common source of error.

Regarding voter registration rates, some states have sought to address the issue of voters remaining on the registration lists after they have moved or are deceased by creating an inactive voter list (also sometimes called a suspension list). The rules vary by state, but typically a voter will be placed on an inactive voter list after they have not participated in around two consecutive statewide elections. When present, the total number of active registered voters was recorded, and otherwise data on the total number of registered voters was recorded. Using the data on the number of registered voters, a measure of registration rates was created by taking the ratio of registered voters to VAP.

The main independent variable of interest is the closing date. Registration closing dates are typically set at the state level. The registration deadline variable is recorded as the number of days between Election Day and the last day in which individuals could register. These data are recorded from state codes obtained from Lexis-Nexis. Lexis-Nexis also indexes changes in state codes, so that changes in registration laws were also recorded between 1992 and 2006.

Some states have different closing dates for different modes of registration. For example, Washington state requires registration by mail 30 days prior to an election, but allows individuals to register in person up to 15 days prior to an election. In these instances (which are a minority of cases), the later date is coded (e.g. Washington's deadline is recorded as 15 days). Illinois also has different closing dates for counties with more than 500,000 residents than for those with less than 500,000. The date for counties with more than 500,000 residents is recorded, and is always within one day of the closing date for the other counties throughout the analysis period. Some states also set a fixed number of days prior to an election, such as 30, while other state codes describe the closing date, such as the fifth Monday before an election as in Georgia. For those states that did not provide an explicit date, the closing variable is simply calculated from the date of the election. Connecticut also did not exactly fit the definition in two regards. First, Connecticut allows Election Day registrants to vote in presidential races, but for no other office on the ballot. Second, it also allows certain individuals to register between the ordinary closing date (14 days) and the day before the election (e.g. new citizens or individuals that just turned 18). Connecticut was coded as having no deadline in presidential elections and a 14 day deadline in midterm elections. Scatterplots of the data are shown in Figures A7 and A8, with a small amount of random noise added to the x-axis.

Figure A7: Scatterplot of deadlines and turnout

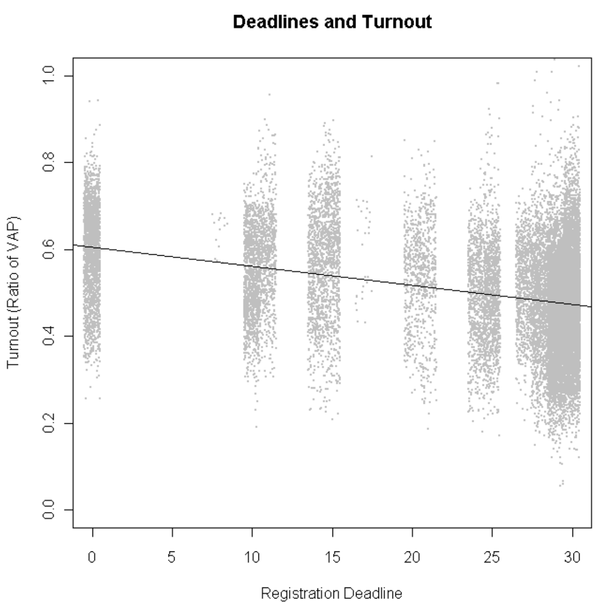
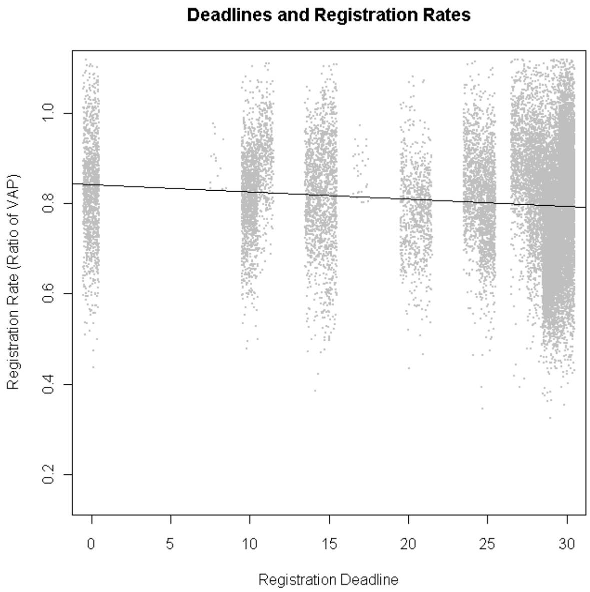


Figure A8: Scatterplot of registration rates and deadlines



A4. Alternative analyses

A number of additional estimates were also obtained using the county-level data. Tables A1 and A2 show the estimates for closing deadlines in alternative models of turnout. Table A1 shows estimates when different subsets of the control variables are included. Table A2 shows results from models using citizen voting age population (McDonald and Popkin 2001), robust standard errors, clustered standard errors, dropping observations with the highest and lowest 1% voter turnout, and dropping observations with the highest and lowest registration deadlines (EDR and 30 days). Similar estimates were obtained for the models of registration which are shown in Table A3-A4. The one departure across analyses is a model including varying-state intercepts shown in Table A5. The estimated coefficients in the hierarchical models are weaker than in the fixed effects models, and positive for the model of rates of registration. With this one notable exception, the empirical results otherwise appear robust over model specification, case restrictions, and methods of analysis.

Table A1: Additional models of turnout

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Turnout	Turnout	Turnout	Turnout	Turnout	Turnout
Deadline	-0.0044	-0.0037	-0.0033	-0.0031	-0.0030	-0.0027
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Age		0.0098	0.0071	0.0092	0.0091	0.0088
		(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Education		0.3092	0.2872	0.6304	0.6390	0.6726
		(0.0109)	(0.0104)	(0.0170)	(0.0171)	(0.0157)
Income				-0.0000	-0.0000	-0.0000
				(0.0000)	(0.0000)	(0.0000)
Anglo			0.1133	0.1193	0.1136	0.1333
			(0.0046)	(0.0046)	(0.0046)	(0.0043)
Population					-0.0000	-0.0000
					(0.0000)	(0.0000)
Competitiveness						0.4507
						(0.0072)
Constant	0.6046	0.1820	0.1751	0.1474	0.1526	-0.0525
	(0.0021)	(0.0077)	(0.0076)	(0.0076)	(0.0076)	(0.0077)
Observations	21759	21743	21071	21071	21071	21071

Standard errors in parentheses

Table A2: Models of turnout with 1) CVAP, 2) Robust SE, 3) Clustered SE, 4) Excludes highest and lowest 1% of turnout, 5) Excludes EDR and 30 day deadline states

	(1)	(2)	(3)	(4)	(5)
VARIABLES	Turnout	Turnout	Turnout	Turnout	Turnout
Deadline	-0.003	-0.003	-0.003	-0.003	-0.003
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Age	0.009	0.009	0.009	0.008	0.010
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Education	0.683	0.648	0.648	0.584	0.652
	(0.013)	(0.015)	(0.033)	(0.012)	(0.015)
Income	-0.000	-0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Anglo	0.096	0.124	0.124	0.122	0.122
	(0.003)	(0.004)	(0.009)	(0.003)	(0.004)
Population	-0.000	-0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Competitiveness	0.158	0.153	0.153	0.128	0.172
	(0.006)	(0.007)	(0.008)	(0.006)	(0.007)
Presidential	0.130	0.128	0.128	0.126	0.127
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Constant	0.036	0.003	0.003	0.046	-0.053
	(0.006)	(0.008)	(0.016)	(0.006)	(0.008)
Observations	21071	21071	21071	20747	12796

Standard errors in parentheses

Table A3: Additional models of registration

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Regis.	Regis.	Regis.	Regis.	Regis.	Regis.
Deadline	-0.0014	-0.0009	-0.0005	-0.0004	-0.0004	-0.0003
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Age		0.0111	0.0089	0.0101	0.0098	0.0098
		(0.0002)	(0.0002)	(0.0003)	(0.0003)	(0.0003)
Education		0.0969	0.0797	0.2760	0.2904	0.2919
		(0.0125)	(0.0120)	(0.0200)	(0.0199)	(0.0199)
Income				-0.0000	-0.0000	-0.0000
				(0.0000)	(0.0000)	(0.0000)
Anglo			0.0904	0.0935	0.0822	0.0833
			(0.0054)	(0.0054)	(0.0055)	(0.0055)
Population					-0.0000	-0.0000
					(0.0000)	(0.0000)
Competitiveness						0.0210
						(0.0095)
Constant	0.8403	0.4059	0.4034	0.3866	0.3974	0.3877
	(0.0027)	(0.0091)	(0.0090)	(0.0091)	(0.0091)	(0.0101)
Observations	19878	19863	19327	19327	19327	19327

Standard errors in parentheses

Table A4: Models of registration with 1) CVAP, 2) Robust SE, 3) Clustered SE, 4) Excludes highest and lowest 1% of turnout, 5) Excludes EDR and 30 day deadline states

	(1)	(2)	(3)	(4)	(5)
VARIABLES	Regis.	Regis.	Regis.	Regis.	Regis.
Deadline	-0.0005	-0.0004	-0.0004	-0.0004	-0.001
	(0.0001)	(0.0001)	(0.0002)	(0.0001)	(0.012)
Age	0.0089	0.0098	0.0098	0.0082	0.011
	(0.0003)	(0.0003)	(0.0007)	(0.0002)	(0.002)
Educ	0.3296	0.2881	0.2881	0.2246	0.228
	(0.0200)	(0.0235)	(0.0527)	(0.0186)	(0.011)
Income	-0.0000	-0.0000	-0.0000	-0.0000	0.000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.000)
Anglo	0.0226	0.0809	0.0809	0.0993	0.159
	(0.0055)	(0.0072)	(0.0168)	(0.0052)	(0.006)
Population	-0.0000	-0.0000	-0.0000	-0.0000	0.000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.000)
Competitiveness	-0.0326	-0.0323	-0.0323	-0.0178	-0.002
	(0.0105)	(0.0116)	(0.0117)	(0.0097)	(0.022)
Presidential	0.0230	0.0226	0.0226	0.0200	0.023
	(0.0019)	(0.0019)	(0.0011)	(0.0018)	(0.000)
Constant	0.4748	0.3983	0.3983	0.4349	0.306
	(0.0102)	(0.0128)	(0.0276)	(0.0095)	(0.000)
Observations	19327	19327	19327	18973	12635

Standard errors in parentheses

Table A5: Models with varying state intercepts

Variable	Turnout		Registration	
	Coef.	**Std. Error**	**Coef.**	**Std. Error**
Deadline	-0.0006	0.0003	0.001	0.0004
Age	0.008	0.000	0.011	0.000
Education	0.252	0.008	0.238	0.010
Anglo	0.067	0.004	-0.048	0.005
Population	-0.000	0.000	-0.000	0.000
Competitiveness	0.155	0.006	-0.013	0.008
Presidential	0.128	0.001	0.020	0.001
	N= 21071		N=18973	

Table A6: Additional turnout models of the survey data

VARIABLES	(1) Turnout	(2) Turnout	(3) Turnout
Deadline	-0.011	-0.010	-0.010
	(0.000)	(0.000)	(0.000)
Female		0.084	0.086
		(0.006)	(0.006)
Age		0.038	0.039
		(0.000)	(0.000)
Educ		0.394	0.404
		(0.003)	(0.003)
Income		0.091	0.092
		(0.001)	(0.001)
Anglo		0.034	0.048
		(0.008)	(0.008)
Presidential			0.673
			(0.007)
Competitiveness			0.870
			(0.032)
Constant	0.620	-3.215	-3.953
	(0.007)	(0.017)	(0.023)
Observations	566548	499225	499225

Standard errors in parentheses

Table A7: Additional registration models of the survey data

VARIABLES	(1) Regis	(2) Regis	(3) Regis
Deadline	-0.012	-0.010	-0.010
	(0.000)	(0.000)	(0.000)
Female		0.155	0.155
		(0.007)	(0.007)
Age		0.035	0.036
		(0.000)	(0.000)
Educ		0.437	0.438
		(0.003)	(0.003)
Income		0.081	0.081
		(0.001)	(0.001)
Anglo		0.058	0.060
		(0.009)	(0.009)
Presidential			0.281
			(0.008)
Competitiveness			-0.067
			(0.036)
Constant	1.462	-2.222	-2.311
	(0.008)	(0.019)	(0.024)
Observations	564151	497437	497437

Standard errors in parentheses

Matched sample analysis

An additional analysis of the survey data was performed using a matching procedure. One aspect of these data that departs from most previous applications of matching methods is that the treatment is not dichotomous. Closing deadlines range from a maximum of 30 days to Election Day registration. With many possible values, traditional propensity scores cannot be calculated. Instead, I used a generalized propensity score matching procedure developed by Lu, Zanutto, Hornik, and Rosenbaum (2001). The procedure involves calculating a linear propensity score and matching cases that are a similar as possible on the linear propensity score and as different as possible on the treatments received. To implement this procedure I randomly selected 5,000 observations to match to a corresponding case. The results from the balance assessment are shown in Table A8. This balance assessment shows the correlation between registration and the control variables (Lu et al. 2001). Several matched samples were drawn, one of which showed the best improvement in balance. This matched sample was used to estimate logit models of registration and turnout presented in tables A9-A11. These results are consistent with the analysis of the full data.

Table A8: Balance summary

	Full data	**Match 1**	**Match 2**	**Match 3**	**Match 4**
Female	0.007	-0.006	0.014	0.012	.015
Age	-0.003	0.031	-0.001	-0.009	-0.014
Education	-0.035	-0.024	-0.041	-0.042	-0.039
Income	-0.031	0.006	-0.025	-0.024	-0.020
Anglo	-0.144	-0.000	-0.140	-0.126	-0.129
Presidential	0.014	-0.037	0.010	0.006	-0.003
Competitiveness	-0.027	0.003	-0.035	-0.034	-0.042

Table A9: Logit model of turnout, matched sample

Variable	Coef.	Std. Error	p-value	90% Interval	
				Lower	Upper
Deadline	-0.010	0.002	0.000	-0.012	-0.006
Female	0.086	0.046	0.060	0.011	0.162
Age	0.038	0.001	0.000	0.036	0.041
Education	0.385	0.019	0.000	0.354	0.417
Income	0.090	0.007	0.000	0.079	0.101
Anglo	0.219	0.059	0.000	0.122	0.314
Presidential	0.692	0.050	0.000	0.609	0.775
Competitiveness	0.239	0.222	0.280	-0.125	0.604
Constant	-3.693	0.148	0.000	-3.936	-3.450
N= 9915					
Pseudo-R^2=.14					

Table A10: Logit model of registration, matched sample

Variable	Coef.	Std. Error	p-value	90% Interval	
				Lower	Upper
Deadline	-0.011	0.002	0.000	-0.014	-0.008
Female	0.156	0.052	0.003	0.070	0.241
Age	0.034	0.002	0.000	0.032	0.037
Education	0.433	0.023	0.000	0.395	0.471
Income	0.073	0.007	0.000	0.061	0.085
Anglo	0.151	0.063	0.016	0.048	0.254
Presidential	0.305	0.056	0.000	0.212	0.398
Competitiveness	-0.619	0.265	0.019	-1.055	-0.184
Constant	-2.014	0.159	0.000	-2.276	-1.752
N= 10000					
Pseudo-R^2=.12					

Table A11: Substantive effects

	Mean	SD	90% Confidence Interval	
			Lower	Upper
Matched sample				
Turnout	0.064	0.012	0.044	0.084
Registration	0.050	0.009	0.035	0.064

Analysis of the 1996 Political Network Election Study

The main body of the paper reports an analysis of the 1996 Political Network Election Study, wave 3. There are 4 waves of the survey that were conducted, and wave 3 immediately followed the 1996 Presidential election (interviews were conducted between November 6, 1996 and January 12, 1997). As the data do not include measures of voter registration for respondents' discussion partners, the variable for voter turnout among respondents' discussion partners was analyzed (turnout among main respondents was over 90%). Data on 830 respondents to wave 3 of the survey was analyzed. Of these, 350 were matched to at least one discussion partner in the data for analysis. Using these data, two measures were constructed. One is a count of the number of voters in the respondent's discussion network. The other is the proportion of one's discussion network that voted.

The first part of the analysis considers whether the average number of voters in discussion networks was greater in St. Louis than Indianapolis, as the registration deadline was slightly shorter (27 to 29 days). The average number of voters in the discussion networks was greater, but the difference was not statistically significant, as shown in the t-test results in Table A12. A similar analysis was done using the measure of the proportion of voters in discussion networks. These results are shown in Table A13.

To assess the effects of social networks on turnout, a logit model of turnout in the 1996 presidential election was estimated. The first estimates include a count of the number of voters in one's discussion network, and are shown in Table A14. As shown in the table, across a range of specifications, the coefficient is positive and statistically significant at conventional levels. Table A15 reports estimates for models with the measure of the proportion of the discussion network that are voters. These results similarly show a positive and statistically significant effect on turnout across a range of specifications.

Table A12: T-test of the number of voters in discussion networks

	N	**Mean**	**Std. Error**
Indianapolis (29 days)	181	1.552	0.068
St. Louis (27 days)	169	1.592	0.078
P-value (2-sided)	0.705		

Table A13: T-test of the proportion of voters in discussion networks

	N	Mean	Std. Error
Indianapolis (29 days)	181	0.884	0.021
St. Louis (27 days)	169	0.909	0.020
P-value (2-sided)	0.383		

Table A14: Analysis of the number of voters in respondents' discussion network

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLES	Turnout	Turnout	Turnout	Turnout	Turnout	Turnout	Turnout
Network Vote	1.193***	1.178***	1.130***	1.128***	1.038***	1.015***	0.990**
Count	(0.357)	(0.355)	(0.349)	(0.350)	(0.360)	(0.377)	(0.394)
Female		-0.418	-0.446	-0.452	-0.418	-0.563	-0.200
		(0.481)	(0.484)	(0.485)	(0.488)	(0.502)	(0.529)
Age			0.025	0.025	0.033*	0.031*	0.024
			(0.016)	(0.016)	(0.017)	(0.017)	(0.017)
Non-Anglo				-0.031	0.229	0.232	-0.031
				(0.673)	(0.700)	(0.739)	(0.730)
College					0.995*	0.993*	0.482
					(0.533)	(0.539)	(0.597)
Partisanship						0.626**	0.699***
						(0.250)	(0.258)
Political Knowledge							0.572**
							(0.251)
Constant	1.250***	1.529***	0.349	0.364	-0.365	-2.002	-2.787**
	(0.408)	(0.527)	(0.901)	(0.909)	(1.000)	(1.218)	(1.273)
Observations	349	349	349	348	347	342	342
Pseudo R-squared	0.090	0.095	0.111	0.111	0.134	0.173	0.206

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table A15: Analysis of the proportion of voters in respondents' discussion network

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLES	Turnout	Turnout	Turnout	Turnout	Turnout	Turnout	Turnout
Network Vote	2.107***	2.070***	1.983***	1.970***	1.838***	1.876***	1.924***
Proportion	(0.543)	(0.546)	(0.555)	(0.557)	(0.571)	(0.602)	(0.622)
Female		-0.367	-0.393	-0.402	-0.332	-0.449	-0.084
		(0.484)	(0.486)	(0.487)	(0.493)	(0.509)	(0.533)
Age			0.021	0.021	0.031*	0.029*	0.021
			(0.016)	(0.016)	(0.017)	(0.017)	(0.017)
Non-Anglo				-0.128	0.193	0.290	0.063
				(0.681)	(0.714)	(0.772)	(0.752)
College					1.182**	1.251**	0.698
					(0.534)	(0.539)	(0.596)
Partisanship						0.689***	0.776***
						(0.251)	(0.262)
Political							0.635**
Knowledge							(0.251)
Constant	0.989**	1.249**	0.278	0.312	-0.655	-2.575**	-3.509***
	(0.447)	(0.569)	(0.910)	(0.922)	(1.027)	(1.276)	(1.331)
Observations	349	349	349	348	347	342	342
Pseudo R-squared	0.077	0.080	0.092	0.092	0.124	0.171	0.213

*** p<0.01, ** p<0.05, * p<0.1

Standard errors in parentheses

Supplementary material references

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