Supplementary Materials
Sidestepping Primary Reform: Political Action in Response to Institutional Change
Political Science Research and Methods

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A Primary classification

Table A1: Categorization of primary institutions by costliness

Costs	Closed	Partial closed	Partial open	Open to unaffiliated	Open	Top-two, Blanket
Register prior to election?	Y	Y	?	?	N	N
Publicly affirm party?	Y	?	Y	?	N	N
Choose party ballot?	Y	Y	Y	Y	Y	N
Complex crossover incentives?	N	N	N	N	N	Y
Classification	Costly	Costly	Costly	Less costly	Less costly	Non-partisan, ambiguous cost

B Details on contribution aggregation from DIME database

To create sums of individual contributions in each state, party, and election cycle, I select all individual donations from DIME's contribution database (Bonica, 2019) with transaction codes 15, 15E, 16J, 22Y, 15S, or 15L excluding refunds greater than $2,500 from elections 1992 through 2014. I aggregate these individual transactions to the party of recipient, state of contributor, and election cycle. For candidate receipts, I use the DIME recipient database and aggregate recipient receipts to the party of recipient, state of recipient, and election cycle.

C Inference using Conley and Taber (2011) correction

Conley and Taber (2011) argue that with a small number of treated groups, the DID estimator is unbiased but inconsistent and inference using standard approaches can be misleading.[1] The basic problem is that with a fixed and small number of treated units, there can be no appeal to residual errors averaging to zero asymptotically. If the strongest OLS assumptions of normally-distributed homoscedastic errors are met, the DID estimator is consistent (p 113), however if the distribution of errors departs from homoscedastic normal, the estimator is inconsistent.

The data set has a relatively small number of treated units so I use the approach of Conley and Taber (2011) to make inference in the presence of inconsistency. I was unable to locate an existing statistical implementation of the Conley and Taber (2011) method so I created a bootstrap procedure for inference following their approach. Their solution is to use the large number of control units to estimate the full error distribution, then use this estimated distribution for inference on treatment effects. This procedure is consistent if the distribution of errors for the control units is equal to the distribution of errors for the treated units. Importantly, consistency holds even if that distribution is either not normal or not homoscedastic.

I estimate the procedure separately for each dependent variable (or functional form) Y. Following Conley and Taber (2011), I estimate the error distribution of control units by regressing Y on

[1] Inconsistency assumes that if we increased sample size to infinity in this setting the number of treated units would not also increase, which is of course not known.

state and election fixed effects using only control units. The residuals from this regression serve as the estimated distribution of errors, i.e. in the presence of no treatment. I then use the coefficients from this regression to calculate the expected value of Y for each treated unit.

I then execute a bootstrap. On each iteration, I sample with replacement one residual from the control error distribution for each treated unit in each time period. For ever-treated units, $\hat{Y}$ is the expected value of Y – as calculated above – plus the error residual sampled on that iteration. For always-control states, $\hat{Y} = Y$. I then estimate the full DID model using $\hat{Y}$ as dependent variable.

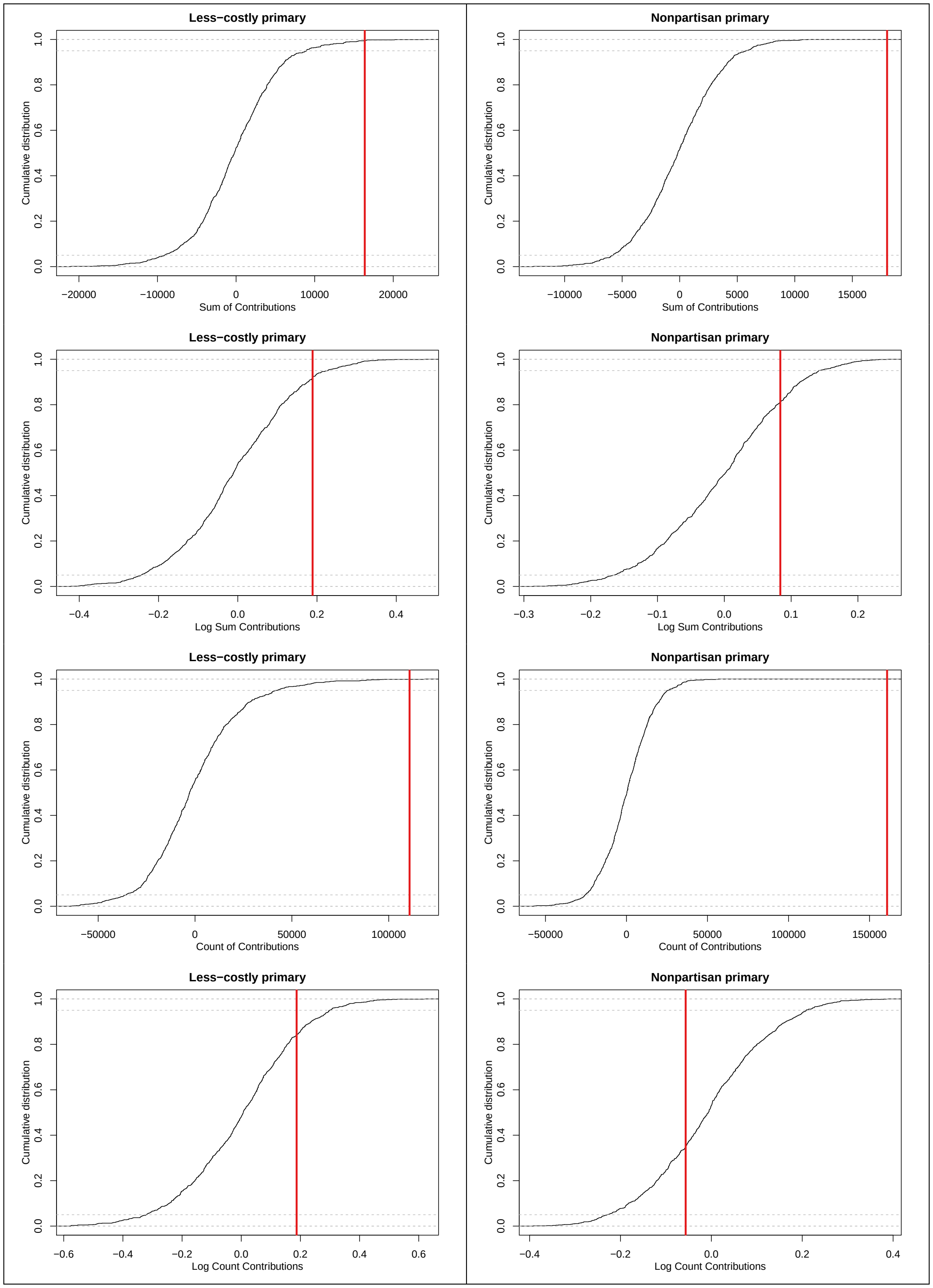
Across bootstrap samples, the distribution of estimated DID coefficients is a consistent estimator for the null distribution when the treatment effect is zero (similar to a permutation test). I then compare the coefficients estimated from the actual data and DID models to the distribution of null effects to make inference about how likely the actual estimate is to have arisen from a sampling distribution with the null hypothesis of no treatment effect.

I plot empirical cumulative distribution functions (ECDF) of the null coefficient distributions in Appendix Figures A1 and A2. An ECDF takes an observed (empirical) distribution of a random variable, sorts the values from low to high, then calculates for each value the proportion of values less than or equal to that value. On the ECDF, the x-axis is the value and the y-axis is the proportion of values less than or equal to that x-value. The ECDF approximates the full cumulative distribution function of the random variable.

Each frame presents one of the two treatments (less-costly primary in left column, nonpartisan primary in right column) for one of the seven dependent variables in Table 4. The vertical lines note the location of the true DID estimate relative to the ECDF. Estimate values that are in the tail of the ECDF are "unusual" and thus less likely to have arisen by chance, while values in the middle of the ECDF are more common in the null distribution and thus more likely to have arisen by chance.

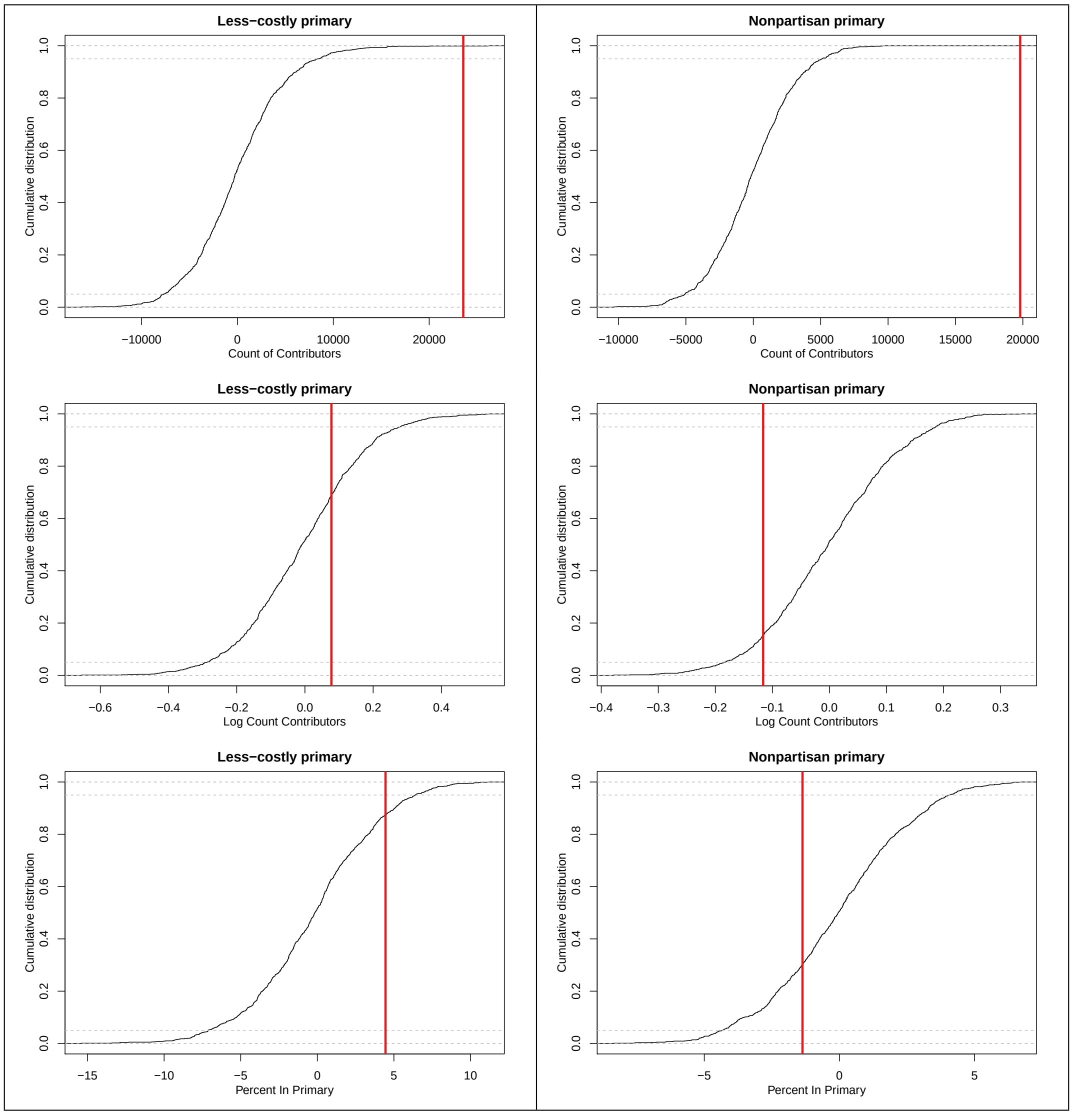
Table 4 has five results that can reject a null hypothesis of zero at $p < 0.05$. The Conley and Taber (2011) correction presented in Figures A1 and A2 moves two effects (less-costly primary on log contributions and on percent of contribution in primary) outside of statistical significance but moves three into significance (both effects on contributions, nonpartisan effect on contributors). Adjusting for inconsistency, in sum, has ambiguous effects on inference but, in total, does not lessen overall confidence in rejecting null hypotheses of zero.

Figure A1: Coefficient estimates versus bootstrap null distributions



Note: Each frame plots the empirical cumulative distribution function for a bootstrap null distribution for difference-in-differences with the Conley and Taber (2011) correction for small number of treated units. Horizontal solid lines represent the actual coefficient estimate. Dashed lines at the 0.05 and 0.95 quantiles of the estimated null distribution. Frames plotted in order of specifications in Table 4.

Figure A2: Coefficient estimates versus bootstrap null distributions (continued)



Note: Each frame plots the empirical cumulative distribution function for a bootstrap null distribution for difference-in-differences with the Conley and Taber (2011) correction for small number of treated units. Horizontal solid lines represent the actual coefficient estimate. Dashed lines at the 0.05 and 0.95 quantiles of the estimated null distribution. Frames plotted in order of specifications in Table 4.

D Evaluation of parallel trends

One concern with any observational study aiming to uncover causal relationships is if treatment and control groups have unobserved heterogeneity. In the DID context, the assumption necessary for identification is parallel trends. In this case, we want to believe that states that implement reform were not trending differently in turnout, contributions, or political competition such that differences in state political environments, rather than primary reform itself, led to changes in outcomes.

Evaluating parallel trends is challenging in the context of this study for two reasons. First, there are few regime changes in the McGhee et al. (2014) data set and only 11 elections even in my extension of their data, limiting statistical power. Second, state-parties move into and out of treatment at different times and I include two different treatment variables, complicating any simple graphical evaluation. (McGhee et al. (2014) do not evaluate parallel trends)

I follow the recommendation of Angrist and Pischke (2009, p 237) and include lag and lead of treatment in the DID regression model. The idea of the test is non-parallel trends correlated with treatment assignment would show up in an indicator that reform is implemented in the next election (treatment at $t-1$). The lag term is of substantive interest to see if any initial effect decays or increases in the election following the first election under reform (treatment at $t + 1$), but does not evaluate parallel trends per se (Angrist and Pischke, 2009, p 237).

In Tables A2, A3, and A4, I reproduce Tables 3, 4, and 6 with one lead and one lag for each primary reform variable. Sample size does not provide extensive statistical power – and cases are lost due to lag and lead values outside of 1992 and 2014.

For the analysis of turnout in Table A2, the indicators that the observation is one election prior to reform ($t - 1$) are all small and estimated with uncertainty except for the coefficient for Democratic votes cast as percent of eligible. This coefficient suggests some concern about parallel trends for the less costly reform, but given that the other seven $t-1$ coefficients do not show similar patterns, it may also be sampling variability. The $t + 1$ coefficients suggest there is some reversion effect to the increase in turnout in the second election of a nonpartisan primary reform.

For the analysis of contributions in Table A3, none of the $t-1$ coefficients suggest clear violation of parallel trends. In contrast to suggestive evidence of reversion in turnout in the second election held under reform, a few of the $t + 1$ coefficients here suggest increasing rather than reverting patterns of contributions. This could be evidence of violation in parallel trends.

The lag/lead models of competition in Table A4 present results similar to those for turnout in Table A2. There is additionally also some indication of a lessening of competition in the election following the first held under the nonpartisan and less costly reforms (negative coefficients on the $t + 1$ variables).

Table A2: Difference-in-differences effects of primary reform on turnout in House primary elections, 1992 to 2014 with dynamic lead and lag

	(1) Total primary Votes cast as Percent of Voting Eligible	(2) Major party Votes cast as Percent of Voting Eligible	(3) Democratic Votes cast as Percent of Voting Eligible	(4) Republican Votes cast as Percent of Voting Eligible
Less costly nominating institution	2.5	-0.08	-1.5	1.1
	(4.6)	(1.5)	(2.5)	(1.7)
Nonpartisan nominating institution	9.5**	3.2**	2.7*	3.3*
	(2.9)	(0.9)	(1.3)	(1.3)
Nonpartisan nominating institution lag t-1	-1.1	-0.05	-0.06	-0.05
	(0.6)	(1.0)	(1.2)	(1.9)
Less costly nominating institution lag t-1	1.2	1.7	3.3*	0.1
	(1.5)	(1.0)	(1.5)	(1.4)
Nonpartisan nominating institution lead t+1	-5.1**	-1.7	-1.0	-2.5*
	(1.0)	(0.9)	(1.2)	(1.1)
Less costly nominating institution lead t+1	-2.2	-0.07	0.7	-0.9
	(3.5)	(1.7)	(1.5)	(3.3)
Observations	489	1,100	550	550
R-squared	0.103	0.044	0.150	0.110
Number of Party_State	50	100	50	50
State FEs	Yes			
Election cycle FEs	Yes	Yes	Yes	Yes
Party-state FEs		Yes	Yes	Yes

** p<0.01, * p<0.05

OLS coefficients with robust standard errors clustered on state-party in parentheses.

Excluded category is institutions most costly for individual participation.

Table A3: Difference-in-differences effects of primary reform on individual contributions with dynamic lead and lag

	(1) Sum of Contributions (1000s)	(2) Log Sum Contributions	(3) Count of Contributions	(4) Log Count Contributions	(5) Count of Contributors	(6) Log Count Contributors	(7) Percent Contributions In Primary
Less costly nominating institution	9,649**	0.26*	44,174*	-0.046	12,271**	-0.12	3.19
	(3,060)	(0.11)	(18,769)	(0.12)	(3,836)	(0.14)	(4.00)
Nonpartisan nominating institution	176	-0.094	22,780	-0.28*	68.4	-0.32*	2.61
	(3,304)	(0.11)	(19,988)	(0.11)	(2,148)	(0.13)	(2.25)
Nonpartisan nominating institution lag t-1	8,533	0.0100	74,590	0.025	10,422	-0.0043	-0.64
	(7,026)	(0.07)	(65,297)	(0.07)	(7,853)	(0.07)	(2.24)
Less costly nominating institution lag t-1	-4,522	-0.14	11,631	0.090	1,781	0.052	4.52
	(3,034)	(0.12)	(15,049)	(0.13)	(4,035)	(0.14)	(3.26)
Nonpartisan nominating institution lead t+1	26,863*	0.31	115,372	0.35*	23,818*	0.32*	-6.32*
	(11,127)	(0.18)	(80,015)	(0.14)	(11,929)	(0.14)	(2.85)
Less costly nominating institution lead t+1	12,584	-0.064	55,223	0.36*	14,806	0.31*	0.27
	(7,830)	(0.16)	(42,680)	(0.14)	(8,142)	(0.14)	(3.90)
Observations	1,100	1,100	1,100	1,100	1,100	1,100	1,100
R-squared	0.321	0.838	0.306	0.815	0.399	0.767	0.590
Number of Party_State	100	100	100	100	100	100	100
Party-state FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election cycle FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes

** $p<0.01$, * $p<0.05$

OLS coefficients with robust standard errors clustered on state-party in parentheses.

Money dependent variables in thousands of dollars.

Excluded category is institutions most costly for individual participation.

Table A4: Difference-in-differences effects of primary reform on competition in House primary elections, 1992 to 2014 with dynamic lead and lag

	(1) Percentage House Primaries Contested	(2) Number House Primary Candidates	(3) Log Primary Candidates	(4) Average Winning Margin (Percent)
Less costly nominating institution	-0.1	-2.5	-0.2	8.7
	[-16 - 16]	[-7.1 - 2.1]	[-0.6 - 0.1]	[-5.3 - 23]
Nonpartisan nominating institution	4.8	-0.5	0.05	13*
	[-7.4 - 17]	[-5.7 - 4.7]	[-0.1 - 0.2]	[2.8 - 23]
Nonpartisan nominating institution lag t-1	-1.8	0.9	-0.006	1.1
	[-13 - 9.8]	[-1.8 - 3.5]	[-0.2 - 0.1]	[-9.3 - 12]
Less costly nominating institution lag t-1	-2.4	2.5	0.2	3.3
	[-22 - 17]	[-0.7 - 5.7]	[-0.05 - 0.4]	[-6.2 - 13]
Nonpartisan nominating institution lead t+1	-4.9	-4.8**	-0.3**	6.0
	[-12 - 2.5]	[-7.6 - -2.1]	[-0.4 - -0.1]	[-6.5 - 19]
Less costly nominating institution lead t+1	-7.8	-3.0*	-0.08	5.9
	[-22 - 6.3]	[-5.4 - -0.5]	[-0.3 - 0.1]	[-8.6 - 20]
Observations	1,007	1,007	1,007	888
R-squared	0.072	0.121	0.098	0.046
Number of Party_State	99	99	99	98
Party-state FEs	Yes	Yes	Yes	Yes
Election cycle FEs	Yes	Yes	Yes	Yes

** p<0.01, * p<0.05
OLS coefficients with robust 95% confidence interval clustered on state-party.
Contested primary defined as more than one non-write-in candidate.
Excluded category is institutions most costly for individual participation.

E Robustness to McGhee et al. coding of primary reform

Tables A5 through A8 reproduce tables from the main body using the original McGhee et al. (2014) coding of primary rules.

Table A5: Difference-in-differences effects of primary reform on turnout in House primary elections, McGhee et al. coding

	(1) Total primary Votes cast as Percent of Voting Eligible	(2) Major party Votes cast as Percent of Voting Eligible	(3) Democratic Votes cast as Percent of Voting Eligible	(4) Republican Votes cast as Percent of Voting Eligible
Semi-Closed	1.4	0.3	0.4	0.9
	(1.9)	(0.7)	(0.8)	(1.1)
Semi-Open	8.1	1.2	3.6**	-1.9
	(4.2)	(1.4)	(1.1)	(1.8)
Open	6.5	1.2	2.6*	-0.1
	(4.1)	(1.4)	(1.3)	(1.7)
Nonpartisan	8.0	2.3	3.6**	1.8
	(4.3)	(1.3)	(0.7)	(1.9)
Observations	550	1,200	600	600
R-squared	0.083	0.043	0.154	0.094
Number of Party_State	50	100	50	50
State FEs	Yes			
Election cycle FEs	Yes	Yes	Yes	Yes
Party-state FEs		Yes	Yes	Yes

** p<0.01, * p<0.05

OLS coefficients with robust standard errors clustered on state-party in parentheses.

Excluded category is closed primary.

Table A6: Difference-in-differences effects of primary reform on individual contributions, McGhee et al. coding

	(1) Sum of Contributions (1000s)	(2) Log Sum Contributions	(3) Count of Contributions	(4) Log Count Contributions	(5) Count of Contributors	(6) Log Count Contributors	(7) Percent Contributions In Primary
Semi-Closed	22,364	0.12	19,713	0.017	7,226	-0.012	-0.48
	(17,211)	(0.10)	(28,214)	(0.15)	(8,140)	(0.14)	(3.21)
Semi-Open	17,910	-0.30	191,385	0.056	22,562	-0.11	7.53*
	(16,375)	(0.19)	(165,840)	(0.24)	(20,629)	(0.21)	(3.09)
Open	32,069	0.012	239,361	0.23	39,590	0.0018	9.29**
	(16,640)	(0.17)	(169,310)	(0.22)	(20,958)	(0.20)	(2.82)
Nonpartisan	32,092	0.034	228,685	-0.032	29,648	-0.16	0.83
	(17,303)	(0.13)	(170,028)	(0.21)	(20,989)	(0.17)	(2.69)
Observations	1,200	1,200	1,200	1,200	1,200	1,200	1,200
R-squared	0.321	0.827	0.271	0.787	0.361	0.741	0.579
Number of Party_State	100	100	100	100	100	100	100
Party-state FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election cycle FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes

** $p<0.01$, * $p<0.05$

OLS coefficients with robust standard errors clustered on state-party in parentheses.

Money dependent variables in thousands of dollars.

Excluded category is closed primary.

Table A7: Difference-in-differences effects of primary reform on candidate receipts, McGhee et al. coding

	(1) Candidate Receipts (1000s)	(2) Log Receipts	(3) Count of Contributors	(4) Log Count Contributors	(5) Percent Receipts To Incumbent	(6) Percent Receipts To Primary Winners
Semi-Closed	15,772	0.022	13,356	0.017	3.52	-0.97
	(13,792)	(0.12)	(12,274)	(0.15)	(4.0)	(3.7)
Semi-Open	12,033	-0.68**	21,706	-0.23	8.89	11.0*
	(18,912)	(0.26)	(23,971)	(0.21)	(7.9)	(4.2)
Open	29,614	0.0073	59,729*	0.15	7.53	12.1**
	(18,767)	(0.23)	(24,009)	(0.19)	(8.7)	(4.1)
Nonpartisan	28,302	-0.051	23,770	-0.12	10.1*	5.59
	(19,114)	(0.20)	(21,719)	(0.18)	(3.9)	(4.0)
Observations	1,200	1,200	1,200	1,200	1,168	1,200
R-squared	0.326	0.505	0.341	0.583	0.011	0.618
Number of Party_State	100	100	100	100	100	100
Party-state FEs	Yes	Yes	Yes	Yes	Yes	Yes
Election cycle FEs	Yes	Yes	Yes	Yes	Yes	Yes

** $p<0.01$, * $p<0.05$

OLS coefficients with robust standard errors clustered on state-party in parentheses.

Money dependent variables in thousands of dollars.

Excluded category is closed primary.

Table A8: Difference-in-differences effects of primary reform on competition in House primary elections, McGhee et al. coding

	(1) Percentage House Primaries Contested	(2) Number House Primary Candidates	(3) Log Primary Candidates	(4) Average Winning Margin (Percent)
Semi-Closed	0.9	-5.5	0.06	-4.8
	[-9.7 - 11]	[-16 - 4.8]	[-0.2 - 0.3]	[-12 - 2.2]
Semi-Open	15**	-3.7	0.2	-8.9
	[4.4 - 26]	[-11 - 3.8]	[-0.02 - 0.5]	[-27 - 9.2]
Open	3.1	-6.3	-0.08	6.1
	[-7.4 - 14]	[-14 - 1.4]	[-0.3 - 0.2]	[-11 - 23]
Nonpartisan	5.6	-5.3	0.009	9.8
	[-3.8 - 15]	[-13 - 2.4]	[-0.2 - 0.2]	[-2.9 - 22]
Observations	1,093	1,093	1,093	969
R-squared	0.070	0.121	0.086	0.041
Number of Party_State	99	99	99	98
Party-state FEs	Yes	Yes	Yes	Yes
Election cycle FEs	Yes	Yes	Yes	Yes

** p<0.01, * p<0.05

OLS coefficients with robust 95% confidence interval clustered on state-party.

Contested primary defined as more than one non-write-in candidate.

Excluded category is closed primary.

Note: Contested primary defined as more than one non-write-in candidate.

F Additional tables and figures

Table A9: Difference-in-differences effects of primary reform on candidate receipts, By office of candidate

	(1) Gov Candidate Receipts (1000s)	(2) Gov Log Receipts	(3) Gov Count of Contributors	(4) House Candidate Receipts (1000s)	(5) House Log Receipts	(6) House Count of Contributors	(7) Oth Candidate Receipts (1000s)	(8) Oth Log Receipts	(9) Oth Count of Contributors	(10) Sen Candidate Receipts (1000s)	(11) Sen Log Receipts	(12) Sen Count of Contributors
Less costly nominating institution	5,557	-0.29	19,536**	5,358*	0.23	8,129	-12,585	-0.20	18,896*	4,801**	1.29**	2,838**
	(6,298)	(0.63)	(5,094)	(2,055)	(0.23)	(4,975)	(7,844)	(0.37)	(9,277)	(659)	(0.44)	(761)
Nonpartisan nominating institution	6,019	-0.091	-1,704	7,812*	0.46	12,031	-10,565	0.054	-10,624	-895	-0.54	-1,457
	(6,034)	(0.54)	(2,542)	(3,160)	(0.37)	(8,078)	(7,618)	(0.34)	(8,611)	(652)	(0.42)	(1,064)
Observations	681	582	681	1,200	1,189	1,200	985	906	985	1,199	1,116	1,199
R-squared	0.157	0.199	0.054	0.330	0.159	0.220	0.315	0.482	0.299	0.085	0.030	0.145
Number of Party_State_office	100	100	100	100	100	100	100	100	100	100	100	100
Party-state FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election cycle FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

** $p<0.01$, * $p<0.05$

OLS coefficients with robust standard errors clustered on state-party in parentheses.

Money dependent variables in thousands of dollars.

Excluded category is institutions most costly for individual participation.

Table A10: Difference-in-differences effects of primary reform on individual contributions, effect of leaving reform

	(1) Sum of Contributions (1000s)	(2) Log Sum Contributions	(3) Count of Contributions	(4) Log Count Contributions	(5) Count of Contributors	(6) Log Count Contributors	(7) Percent Contributions In Primary
Became costly this cycle	-4,462	0.061	-36,119	0.035	-2,939	0.050	-5.97**
	(6,628)	(0.12)	(47,112)	(0.15)	(7,338)	(0.17)	(1.69)
Observations	1,200	1,200	1,200	1,200	1,200	1,200	1,200
R-squared	0.292	0.826	0.233	0.787	0.337	0.741	0.579
Number of Party_State	100	100	100	100	100	100	100
Party-state FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election cycle FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes

** $p<0.01$, * $p<0.05$

OLS coefficients with robust standard errors clustered on state-party in parentheses.

Money dependent variables in thousands of dollars.

Excluded category is no change in institution in this cycle.

Figure A3: Original arguments for and against Initiative 872 in Washington State

Statement For Initiative Measure 872

VOTE FOR THE PERSON — NOT THE PARTY

Last year the state party bosses won their lawsuit against the blanket primary, and in 2004 they convinced the Governor to veto legislation allowing voters to continue to vote for any candidate in the primary. Most of us believe this freedom to select any candidate in the primary is a basic right. Don't be forced to choose from only one party's slate of candidates in the primary. *Vote Yes on I-872.*

MORE COMPETITIVE PRIMARIES AND GENERAL ELECTIONS

Under I-872, the two candidates with the most votes in the primary win and go on to the general election ballot. No political party is guaranteed a spot on the general election ballot. Parties will have to recruit candidates with broad public support and run campaigns that appeal to all the voters. That's fair – and that's right.

PROTECT PRIVACY AND INCREASE PARTICIPATION

Under I-872, you will never have to declare party or register by party in order to vote in the primary. In the primaries in 2000, the turnout in Washington was *more than twice as high* as in states with party primaries – because voters in this state could support any candidate on the primary ballot. *Vote Yes on I-872.*

RETURN CONTROL OF THE PRIMARY TO THE VOTERS

The September primary this year gave the state party bosses more control over who appears on our general election ballot at the expense of the average voter. I-872 will restore the kind of choice in the primary that voters enjoyed for seventy years with the blanket primary. Protect Washington's tradition as a state that elects people over party labels. *Vote Yes on I-872.*

For more information, call 1.800.854.1635 or visit www.i872.org .

Rebuttal of Statement Against

I-872 gives voters *more choices* in the primary and *better choices* in the general. *All the voters* will decide who is on the November ballot. Whether it's one Republican and one Democrat, one major and one minor party, or even an Independent — they will be *the candidates the voters want the most.* The primary and general election should be decided by voters, not by exclusive party organizations that might be dominated by special interests!

Voters' Pamphlet Argument Prepared by:

TERRY HUNT, President, Washington State Grange; BILL FINKBEINER, State Senator, Republican; BRIAN HATFIELD, State Representative, Democrat; SAM REED, Secretary of State, Republican; JOHN STANTON, Chairman and CEO, Western Wireless; DARLENE FAIRLEY, State Senator, Democrat.

Statement Against Initiative Measure 872

I-872 REDUCES YOUR ELECTION CHOICES
THE LEAGUE OF WOMEN VOTERS AND OTHER CONCERNED CITIZENS URGE YOU TO MAKE SURE WASHINGTON VOTERS HAVE CHOICES IN NOVEMBER

Vote No on I-872! Don't be fooled. I-872 creates a Louisiana-style primary that would sharply reduce your choices in general elections. Over a third of the statewide and congressional candidates who appeared on the general election ballot in 2000 would have been eliminated in the primary if I-872 had been the law.

Third Parties and Independents Eliminated: If I-872 is passed, third parties, minor parties and even independents will be eliminated from the general election ballot, leaving (in most cases) one Republican and one Democrat. In November 2000, 180,000 voters who voted for third party candidates in the general election would never have had that choice if I-872 had been the law. Insulating the top two political parties from competition is a bad idea.

Single-Party Elections Will Result: Under I-872 many voters will not be able to vote for a candidate that represents their philosophy because the two top vote-getters in a race may be of the same party resulting in only one party being represented on the November ballot. In one-third of the races for Governor in the last twenty-five years, I-872 would have resulted in two general election gubernatorial candidates from the same party. In fact, the voters' ultimate choice for Governor in 1980, John Spellman, would never have appeared on the November ballot.

We urge you to preserve Washington's independent, multi-partisan election system by voting No on I-872.

For more information, call 206.652.8904 or visit www.No872.org .

Rebuttal of Statement For

The League of Women Voters and many others believe I-872 is bad for Washington. I-872 does not "restore the kind of choice" voters had in the past. *It reduces everybody's choice in the general election.*

It decreases general election ballot diversity by eliminating third party candidates and independents. Some November ballots may have choices from only one party for an office.

Support good government and general election choices. *Vote No on I-872.*

Voters' Pamphlet Argument Prepared by:

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12 *The Office of the Secretary of State is not authorized to edit statements, nor is it responsible for their contents.*

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