Sidestepping Primary Reform: Political Action in Response to Institutional Change[*]

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Abstract: Many believe primary elections distort representation in American legislatures because unrepresentative actors nominate extremist candidates. Advocates have reformed primaries to broaden voter participation and increase representation. Empirical evidence, however, is quite variable on the effects of reform. I argue that when institutional reform narrows one pathway of political influence, aggrieved actors take political action elsewhere to circumvent reform. I use a difference-in-differences design in the American states and find that although changing primary rules increases primary turnout, campaign contributions also increase with reform. Implementing nonpartisan primaries and reforming partisan primaries lead to estimated 9 and 21 percent increases in individual campaign contributions per cycle. This suggests actors substitute action across avenues of political influence to limit effects of institutional reform.

Keywords: primary elections; campaign finance; turnout; difference-in-differences; political institutions.

From 1787 through the present, a recurring theme in the United States is reform of political institutions to promote self-government. One recent focus of reform has been institutions of nomination. For example, in 2004 the voters of Washington State passed Initiative 872 to implement a *top-two* primary. The reform eliminated separate party ballots and allowed all primary voters to select candidates of any party in most county, state, and federal offices. Proponents argued the more inclusive rules would "increase participation" and allow voters to elect "people over party labels."[1] With proponents making similar arguments, California adopted the top-two primary with Proposition 14 in 2010. In recent decades, other states have implemented nomination reforms such as blanket primaries, allowing crossover voting, or making the choice of primary ballot private to the voter.

Primary reform should increase participation and promote representation because, it is argued, primary elections with more stringent rules of participation cause fewer and different voters to participate than would a system with easier access. Voters willing to incur the costs of more stringent rules of participation are thought to be those with preferences farther from the mainstream. If a voting electorate with out-of-mainstream preferences votes for candidates with out-of-mainstream preferences, stringent rules generate candidates less representative of the electorate as a whole.

Top-two primary elections join a long list of reforms to American political institutions adopted with the goal of changing representation by increasing citizen participation in and proximity to political decision-making (Cain, 2015). The 20th Century began with Progressive reforms such as the direct primary, nonpartisan elections, the initiative and recall, women's suffrage, direct election of senators, and civil service protections. Reform continued mid-century with the McGovern-Fraser Commission and suffrage for 18-year olds, and closed with the Bipartisan Campaign Reform Act. Each reform was either aimed directly at weakening formal political parties and redistributing political power, or did so indirectly by

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[1] See the 2004 Washington State Voters' Pamphlet in Appendix Figure A3). Reforms to primary institutions are sometimes enacted by legislatures, but legislatures are also sometimes circumvented by voter initiative. The Washington reform was not implemented until 2008 after upheld by the Supreme Court.

extending participation to erstwhile excluded groups. Similar goals underly primary reform.

Despite the momentous changes to American political institutions in the 20th Century, calls for institutional change have not abated in the 21st. Reformers currently target the Electoral College, term limits, districting, membership size of the House, election administration, and even plurality elections. Are political institutions less consequential than reformers and political scientists believe? Scholarly evidence on the consequences of primary election reform has not clarified our understanding of the consequences of electoral institutions. Evidence on the top-two primary finds effects on turnout and polarization that vary from modest to near zero (e.g., Hill and Kousser, 2016; Kousser, 2015; Kousser, Phillips, and Shor, 2018; McGhee and Shor, 2017). These variable effects are consistent with scholarship on the influence of primary election rules more broadly, which finds either politically meaningful effects of primary rules (e.g., Bullock and Clinton, 2011; Gerber and Morton, 1998) or fails to detect much effect at all (e.g., Hill, 2015; Hirano et al., 2010; McGhee et al., 2014).[2]

One explanation for empirical evidence on institutional reforms varying across studies and data sets is that reforms can have multifaceted, sometimes countervailing consequences that vary across settings. In response to some reforms, for example, political actors negatively affected by reform may undertake actions to mitigate their losses. Because the political system allows multiple pathways of influence, reform to one pathway may lead to countervailing effort elsewhere. When alternative pathways exist, actors may make efforts of influence so that, in some settings, they are able to mitigate the consequences of reform, while in other settings they are not. This would lead to the empirical observation that reform sometimes corresponds to important political consequence but sometimes does not. Pathways of influence are not limited to political elites like party leaders. Active citizens who are not part of the party might also sidestep reforms with actions such as campaign donations or political participation.

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[2] Work on the consequences of the *candidate* nominated by primary electorates finds more consistent effects of political importance (e.g., Boatright, 2013; Brady, Han, and Pope, 2007; Hall and Thompson, 2018).

This argument, which I call the theory of sidestepping reform and adopt as my perspective here, may be an explanation for the variable effects measured of reforms to primary elections. The theory that political actors may sometimes circumvent or sidestep reform is echoed in the work of other scholars. Cohen et al. (2008) argue elites responded to reform of party presidential nominations by attempting to mold the field of candidates and build coalitions before the electorate becomes involved. When campaign finance is restricted, Issacharoff and Karlan (1999) argue donors find alternative routes for pecuniary effort (the "hydraulic principle"). In the American states, reforms aimed at limiting the power of political parties have been circumvented by extra-legislative organization, candidate recruitment, and the production and dissemination of information to influence electoral competition (Masket, 2016).

Strong empirical evidence about sidestepping reform is somewhat limited, however, perhaps because actors are averse to having their efforts of influence observed. Some scholarship presents logical argument with descriptive evidence (Cain, 1995; Issacharoff and Karlan, 1999) while Cohen et al. (2008) and Masket (2016) draw on journalistic and historical accounts along with some quantitative analysis.

In this essay, I set out to understand the intended and unintended consequences of change to primary institutions and to provide evidence of sidestepping reform with more data and a plausible strategy of causal identification. I focus on changes to rules for sub-presidential primary elections such as open, blanket, and top-two primaries. The theory of sidestepping reform says that if more stringent primary election rules benefit certain political actors, broadening access to primary elections causes those actors to increase action in other realms. Though other realms could include a variety of pathways (such as those in Cohen et al., 2008; Masket, 2016), I consider campaign finance. Campaign finance is a natural alternative pathway of influence as donations can be quickly and flexibly made in response to reform. Imagine a primary voter whose closed primary is reformed to a top-two system. They may want to maintain support for partisan or ideological candidates like

those they had supported in the closed primary and so substitute (or complement) primary turnout under the new rules with new monetary donation. While I do not establish the mechanism conclusively, one interpretation of an increase in donations following reform is that political actors sidestep reform.

I extend the data of and use the same research design as McGhee et al. (2014) to estimate a difference-in-differences design of the effect of changes to primary election rules. Although McGhee et al. (2014) find that primary reforms do not lessen state legislative polarization, I show these same reforms seem to have one intended effect, increasing turnout in primary elections by 1.5 to 6 percentage points. Reforms also had effects in other realms of political action, however. In states where party primaries were reformed to ease access to the ballot, contributions from individuals increased per party-cycle by about $16 million relative to states without reform.

Reforms creating non-partisan primaries (blanket or top-two) led to increases of $18 million in contributions relative to state-parties without reform. I show that contributions increased to a greater degree from citizens previously participating in closed primaries at higher rates than from those participating at lower rates. I find suggestive evidence that reform increased the share of campaign receipts collected by incumbents and winners of primary elections, which may indicate a circling of the wagons by those aggrieved by primary reform. I do not, however, find evidence that reform increased electoral competition at the primary.

An alternative theory to sidestepping reform is that institutional reforms always have distributional consequences and therefore unabated calls for reform come from those who did not benefit from previous reforms. I find two results that distinguish sidestepping from this alternative theory. I find *non-institutional* response to reform as actors changed behavior within the reformed institutional environment. I also find evidence that actors substitute effort across pathways of political influence. These results do not mean the aggrieved do not also work towards institutional change – for example, the state parties of California su-

ing to stop the blanket primary – but do show that some actors take action separate from advocating institutional change.

The argument and empirical evidence in this paper speak to scholarship on primary elections, political participation, and campaign finance, lend empirical support to Madison (1787) that "causes of faction cannot be removed," and explain why the net effects of institutional reforms may vary considerably across settings. Further, the evidence implies that political actors substitute effort across domains of political influence in response to institutional changes, an observation not to my knowledge before made. The results also suggest a new factor for observed variation in levels of campaign finance. In addition to features of candidates, donors, rules, and electoral context, the evidence shows that sub-national institutions of elections have causal influence on campaign finance.

Theoretical perspective: Sidestepping reform

Direct primary elections were an important Progressive reform aimed at reducing the power of parties (though see Ware, 2002) by allowing more of the electorate to participate in candidate nominations. The direct primary remains today an important American political institution and is one of the most common in which reformers advocate change. Recently, advocates have succeeded in reforming primaries in some states to ease access to the primary ballot. To make it easier for more voters to participate in nominations, party registration requirements have been relaxed or eliminated and ballot restrictions loosened. One goal of primary reform is to increase representativeness of nominees by decreasing the relative influence of ideologues and partisans (see the 2004 Washington State Voters' Pamphlet in Appendix Figure A3).

The reasoning of advocates and scholars that easing restrictions on participation in primaries will increase participation and cause increased candidate moderation is not always clearly stated. My impression is that the theory underlying these beliefs follows from three assumptions about the dynamics of primary elections. I summarize these assumptions in

Figure 1. First, that easing restrictions on which citizens are eligible to vote in primary elections should increase turnout (pathway A→B) because costs to vote deter participation. This assumption connects to reforms that relax restrictions on which party ballot voters may select (open primaries) and to reforms that allow voters to select different party candidates in different offices (non-partisan primaries).[3]

Figure 1: Motivation for primary reform: Presumed causal pathways from reform to legislative moderation
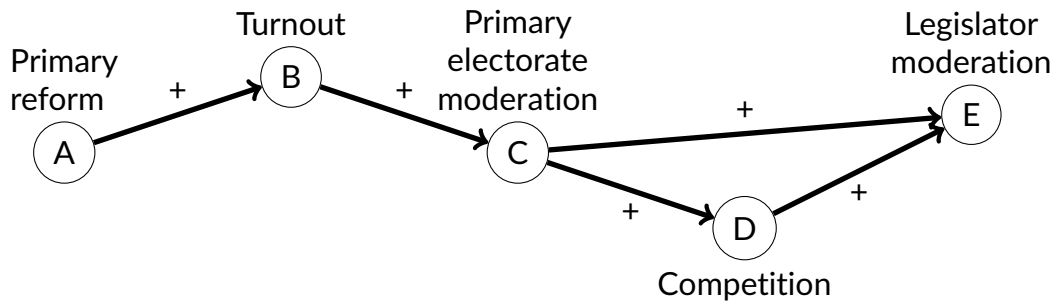
Second, it is assumed that under restrictive primary rules only the most partisan or ideological voters are willing to incur the costs of participating and of voting a full ballot. Therefore, increasing turnout leads to a more centrist primary electorate for each office on the ballot (pathway B→C). Third, with more centrist voters turning out or voting down the ballot, more centrist candidates are more likely to contest and more often win primary elections (C→E). Thus, in primaries with restrictive rules, we should expect more ideological and partisan candidates nominated. It is also sometimes suggested that a more diverse primary electorate may lead to greater competition at primary elections, which also creates a moderating influence on candidates (C→D→E).[4]

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[3] An additional pathway could be that giving voters more choice about which candidates they may select in different offices increases votes cast on down-ballot offices through a decrease in "rolloff." To my knowledge, no research has addressed this question. In the difference-in-differences models of turnout below, both increasing turnout and decreasing rolloff can lead to more ballots cast for primary candidates to the U.S. House.

[4] Of course, primary electors with non-centrist preferences who strategically consider need to win election before the more centrist general electorate would be better off nominating a more centrist candidate at the primary (Aranson and Ordeshook, 1972; Coleman, 1972). The theory summarized in Figure 1 implicitly assumes either that primary voters are not fully strategic, or that the general electorate would not be more likely to elect a centrist candidate.

Existing research has considered many of the mechanisms represented by the pathways in Figure 1. While I cannot do justice to the full literature on primary elections here, I provide an example set of findings in Table 1. The research uses a variety of designs, time periods, and legislative-electoral settings to estimate the relationships of different pathways of the theory. Reform to primary elections is the usual explanatory factor and designs often skip over intermediate edges (e.g., looking at the relationship between rules [node A] and candidate moderation [node E], skipping over B, C, and D). While some research provides empirical support to pathways of the theory, estimates are quite variable. Some find relationships of magnitudes that suggest primary rules have important political consequences, while others find magnitudes near zero suggesting rules are not particularly relevant. Galderisi, Ezra, and Lyons (2001) has a set of empirical chapters that find evidence sometimes in support and sometimes in contrast to that in this table. The final rows of Table 1 present the three main empirical contributions of this essay.

Why might primary reform have consequences that vary from politically important to null (Table 1) despite conventional understanding (Figure 1)? The theory of sidestepping reform provides an explanation. Changes to primary rules that encourage broader participation change expectations actors have about political outcomes of the system. Change to expected outcomes may change incentives for political actors who prefer status quo to the new system. Changed incentives can induce behavior not previously taken on other pathways of influence – effort to sidestep reform. Activists, party leaders, or power brokers who had more influence over nominations in closed primary elections or party conventions might react to reforms democratizing nominations with effort to maintain influence (e.g., Cohen et al., 2008). Campaign donors who previously made large soft money contributions to national party committees may respond to restrictions on those direct contributions by increasing independent expenditures (Issacharoff and Karlan, 1999). In fact, even actors silent in the previous system may become newly involved in opposition to the new status

Table 1: Sample of research findings on mechanisms and consequences of primary reform

|	Outcome (Fig. 1 path)	Explanatory variable	Research design	Quantitative relationship
Geras and Crespin (2018)	Congressional primary turnout (A→B)	Primary election laws	PCS	varies across models, higher turnout in open primaries
Rogowski and Langella (2015)	Primary candidate ideology (A→E)	Primary election laws	DID, PCS	about zero
Hill (2015)	Congressional primary voter ideology (A→C)	California top-two primary	ITS	about zero
Norrander and Wendland (2016)	Presidential primary voter ideology and party identification (A→C)	Primary election laws	2008 CS	about zero
Gerber and Morton (1998)	Voter-legislator congruence (A→E)	Primary election laws	PCS	3 to 9 points ADA scores
Hirano et al. (2010)	Congressional polarization (A→E, B→E, C→E, D→E)	Implementation of direct primary; Level of primary turnout; Primary competition	DID, IV	about zero
Bullock and Clinton (2011)	California congressional polarization (A→E)	Implementation of blanket primary	ITS	around 9% moderation in congressional rank, only in less partisan districts
McGhee et al. (2014)	State legislature polarization (A→E)	Primary election laws	DID	about zero
McGhee and Shor (2017)	State legislature polarization (A→E)	Implementation of top-two primaries	ITS, M, DID	ambiguous[a]
Kousser, Phillips, and Shor (2018)	Voter-legislator congruence (A→E)	Primary election laws	ITS	about zero
(This essay)	Congressional primary turnout (A→B)	Primary election laws	DID	2 to 6 points rate of turnout
(This essay)	Congressional primary competition (A→D)	Primary election laws	DID	about zero
(This essay)	Campaign contributions and receipts (A→F Fig. 2)	Primary election laws	DID	9 to 50 percent increase in campaign receipts

Note: DID=difference-in-differences; CS=cross-section; PCS=pooled cross-section; IV=instrumental variables; ITS=Interrupted time-series; M=Matching.

[a]Results vary across designs, specifications, and parties.

quo.[5] Others have also alluded to the possibility of sidestepping reform using different language (Cain, 2015; Issacharoff and Karlan, 1999).

Figure 2 adds sidestepping response to the causal pathways of primary reform. Node F might include donations, volunteering, lobbying, activism, greater coordination by party leaders, or different candidates running for office. Alternative pathways of influence allow motivated political actors to try to circumvent change to the status quo either directly or by limiting competition.

Figure 2: Sidestepping reform: Causal pathways from reform to unclear consequence
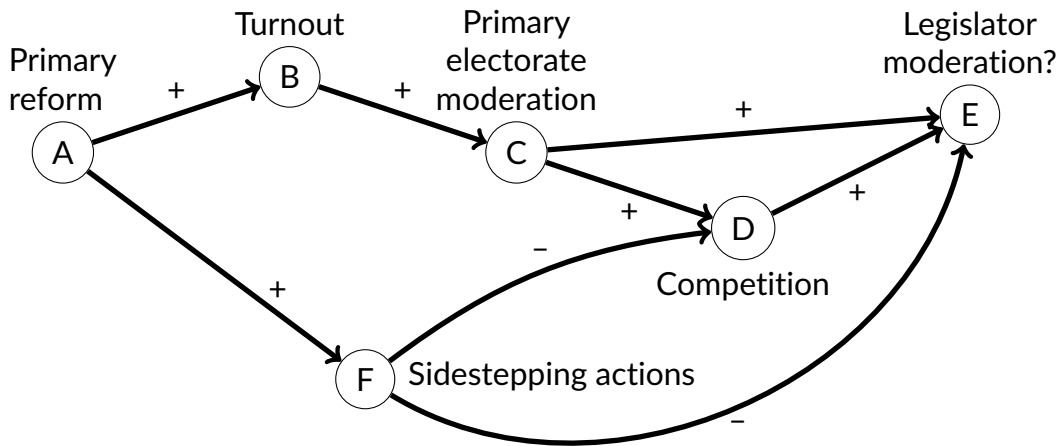
Applying Figure 2 to interpret the variable empirical evidence on the effects of primary reform from Table 1 suggests that, in some cases, effort to sidestep has been successful. Actors who anticipated the consequences of reform changed their behavior to influence outcomes away from the new status quo leading in some cases to limited or vitiated consequences of reform. However, in other settings it may be that the consequences of reform are too large or the alternative pathways of influence too narrow to successfully sidestep. Sidestepping won't vitiate consequences of reform when the paths from F to E are smaller

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[5] I acknowledge that there are likely models of this strategic situation that could lead to no observed equilibrium behavioral change in response to institutional reform. Though outside of the scope of this paper note that, under such a model, observing no systemic response to institutional reform would not be evidence that the reform was not consequential (see Gordon and Hafer, 2005).

in magnitude than the paths from C to E.[6]

Evidence for sidestepping reform: Data, measurement, and statistical model

I turn now to evidence of sidestepping primary reform in operation. I do not prove the existence of every node in Figure 2. Rather, I use the theory to generate hypotheses about the likely consequences of primary reforms. My empirical design then tests these hypotheses and, finding evidence in support, suggests sidestepping reform a plausible theory to explain larger patterns.

A desirable design would induce institutional reforms – i.e., change expectations of the status quo – and measure changes in action taken on alternative pathways of influence. Consider campaign finance. If campaign finance is a pathway of influence, actors might substitute campaign contributions or fund-raising effort in response to reforms.[7] Campaign finance as pathway to influence political outcomes away from the status quo could be generated by different actors. Motivated individual citizens (Barber, Canes-Wrone, and Thrower, 2017; Brown, Hedges, and Powell, 1980; Brown, Powell, and Wilcox, 1995; Francia et al., 2003; Hill and Huber, 2017; Magleby, Goodliffe, and Olsen, 2018) or Political Action Committees (PACs) who care about policy might pro-actively give without candidate solicitation. Alternatively, motivated parties or candidates who care about elections might exert new effort to raise funds, or candidates who would not otherwise run for office may enter the contest and begin to raise new money. Any or all of these actors may use campaign finance and other pathways of political influence (volunteering, production of information, lobbying, etc.) to influence post-reform outcomes.

I estimate the effect of primary reform on campaign finance. I look at changes over time in primary rules in each state and party and classify each change by the effect on costs for voter participation. If stringency of access to nominating elections has consequences as in

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[6]For some evidence in related realms consistent with sidestepping, see Olson and Rogowski (2019) and Hassell (2015).

[7] For evidence that campaign finance reform can influence electoral outcomes, see Hall (2016).

Figure 1, sidestepping reform suggests that as state-parties move from more to less stringent rules, loss of political influence from stringent primary rules should increase magnitude of campaign finance (Figure 2).

To measure stringency of access to primary elections, I use McGhee et al.'s (2014) compilation of state-party primary election rule changes from 1992 to 2008. I extend their time-series to 2014 through personal correspondence with the authors and with documentation of state election laws provided by the National Conference of State Legislatures.

Because I am interested in how actors respond to reforms that ease access, I categorize primary rules by considering how burdensome each rule is for individual voter participation. In my category "Costly," voters must formally register with a party in order to participate in that party's primary, often with some level of restriction on that registration. I assign Open and Open-to-unaffiliated primary systems to the category "Lower Cost" as any voter may participate in any party primary without restriction but must still choose a party ballot. I classify Top-Two and Blanket primaries "Nonpartisan" because costs to participate are ambiguous relative to Open but likely less costly than the various versions of closed (see Appendix Table A1). Readers should interpret the effects I estimate as average responses to rule changes that increase or decrease costs for voter participation in primary elections. My categories abstract away from nuances of primary election rules.[8]

For turnout and political competition, I extend Hirano et al.'s (2010) time-series of U.S. House primary election data through 2014 with results from the Federal Election Commission. Turnout is measured by the number of votes cast for House candidates in each state and election, excluding votes for write-ins, divided by Voting Eligible Population from McDonald (2019). For contribution records, I use the Database on Ideology, Money in Politics, and Elections (DIME, Bonica, 2013, 2019), which compiles individual campaign donations from state and federal campaign filings.

Because states (and sometimes parties within states) choose their own rules of primary

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[8] In Appendix Section E, I present results with McGhee et al.'s (2014) five-category classification.

elections, I use a two-way fixed effects difference-in-differences design (DID) to estimate causal effects of primary reform on turnout, political competition, and campaign finance. The model measures the effects of within-state(-party) changes in primary institutions over time, holding constant all time-invariant features of the state and state-party such as party balance, party organization institutions, geography, average policy views, and legislative institutions. Effects are identified when a state-party changes its institution, which occurs in the time period of this panel with movement both into and out of less costly rules for participation.

I summarize legal and statutory changes that generate identification in Table 2. One concern with this identification strategy is that institutional change is endogenous to features of the electoral environment and thus the required DID assumption of parallel trends is violated. Table 2 presents the cause of each change, which varies across states and times from judicial rulings to legislative action to voter initiatives. That etiology varies across settings provides some comfort that there is not some single omitted factor that always causes reform and would lead to spurious estimate of the effect of reform. Of additional support to the causal interpretation is the section on heterogeneity in treatment effects below with empirical evidence of the causal mechanism in Figure 2. Individuals who previously participated in closed primaries at higher rates most increased their donations in response to reform.

That said, these statutory changes are not a large number and so readers should be cautious in interpretation of this evidence. However, this is the set of natural experiments we have and my goal is to learn from them as much as we can. The DID design provides a plausible path for doing so. I present results by applying a Conley and Taber (2011) correction for small number of treated units below.

The unit of observation is the state-party-year with statistical model

$$y_{ijt} = \alpha_{ij} + \gamma_t + \beta x_{ijt} + \varepsilon_{ijt}, \tag{1}$$

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Table 2: Changes in primary institutions

State	Year	Party	Switch	Cause
AK	1996	Rep	Became nonpartisan	State supreme court approves statewide blanket primary
UT	1996	Dem	Left less costly	State legislature moved primaries to closed (HB 359)
UT	1996	Rep	Left less costly	State legislature moved primaries to closed (HB 359)
CA	1998	Dem	Became nonpartisan	Proposition moved primaries to blanket from closed (Prop 198)
CA	1998	Rep	Became nonpartisan	Proposition moved primaries to blanket from closed (Prop 198)
AK	2002	Rep	Left nonpartisan	Supreme Court strikes down blanket primary, Alaska moves to semi-closed
AK	2002	Dem	Left nonpartisan	Supreme Court strikes down blanket primary, Alaska moves to semi-closed
CA	2002	Dem	Left nonpartisan	Supreme Court strikes down blanket primary, California moves to semi-closed
CA	2002	Rep	Left nonpartisan	Supreme Court strikes down blanket primary, California moves to semi-closed
WA	2004	Rep	Became less costly	Supreme Court declares state's blanket primary unconstitutional
WA	2004	Dem	Became less costly	Supreme Court declares state's blanket primary unconstitutional
LA	2008	Rep	Left nonpartisan	State legislature moved primaries to closed (SB 18, Act 560)
LA	2008	Dem	Left nonpartisan	State legislature moved primaries to closed (SB 18, Act 560)
WA	2008	Rep	Left less costly	Voter initiative approves top-two primary in 2004 (I 872); not implemented until 2008 following Supreme Court approval
WA	2008	Dem	Left less costly	Voter initiative approves top-two primary in 2004 (I 872); not implemented until 2008 following Supreme Court approval
CA	2010	Rep	Became nonpartisan	Proposition moved primaries to nonpartisan (State legislature-referred Prop 14)
CA	2010	Dem	Became nonpartisan	Proposition moved primaries to nonpartisan (State legislature-referred Prop 14)
LA	2010	Dem	Became nonpartisan	State legislature moved primaries to nonpartisan blanket (HB 292)
LA	2010	Rep	Became nonpartisan	State legislature moved primaries to nonpartisan blanket (HB 292)

where y is the outcome of interest in state i for party j in election cycle t, α is a state-party fixed effect, γ is an election cycle fixed effect, β is the coefficient of interest on x measuring a less-costly or nonpartisan primary election institution, and ε is a random disturbance.[9] Some models are estimated at the level of state-year, aggregating across parties. As with all DID designs, the model captures the causal effect of x on y if a parallel trends assumption holds. The variation in etiology of reform in Table 2 is my strongest evidence in support of ignorability. I present an empirical evaluation of parallel trends in Appendix Section D.

Results: Political consequences of primary reform

I first use the DID design to evaluate if more open rules of access to primary elections increases voter participation. The columns of Table 3 estimate the effect of primary reform on turnout in primary elections to the U.S. House aggregated to the state-election. The dependent variable in the first column is the number of votes cast in all House primary elections for each state and election cycle divided by the Voting Eligible Population in that state and year. All standard errors are clustered on the state-party. Point estimates suggest an increase in turnout of 1.5 percentage points in open primaries and 6.1 percentage points in nonpartisan primaries. Although these estimates have large standard errors, the second magnitude is of political importance suggesting that nonpartisan primaries do serve the goal of increasing participation. The second column estimates the effect on votes cast for major party candidates only, with point estimates of 0.4 and 1.8. The third and fourth columns present effects for Democratic and Republican primary candidates separately, with little heterogeneity by party. Table 3 in whole suggests easing restrictions on voting in primary elections increases participation in nominating contests, but uncertainty about the magnitude remains given sampling variability.

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[9] Results are robust to using party-cycle rather than cycle fixed effects γ.

Table 3: Difference-in-differences effects of primary reform on turnout in House primary elections, 1992 to 2014

	(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	1.5	0.4	-0.07	0.7
	(2.6)	(0.7)	(1.7)	(1.0)
Nonpartisan nominating institution	6.1*	1.8*	1.8*	1.6
	(3.0)	(0.8)	(0.8)	(1.5)
Observations	550	1,200	600	600
R-squared	0.078	0.043	0.150	0.092
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.

Contributions and receipts increase with primary reform

Table 4 presents DID estimates of the effect of changing primary rules on campaign contributions from individual donors. Dependent variables sum individual contributions to recipients of the two major parties in each cycle, with each observation a state-party-cycle. I also include logged versions of each count dependent variable given the different sizes of states.

The first column is total contributions where the best estimate is that less-costly primaries increased contributions by about $16.4 million and nonpartisan primaries by about $18 million to candidates of each party in each state with reform. The loglinear specification in column two indicates a 21 percent increase in contributions for less-costly primaries and 9 percent increase for nonpartisan.

The third and fourth columns consider effects of reform on counts of contributions and

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Table 4: Difference-in-differences effects of primary reform on individual contributions, all offices

	(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	16,376**	0.19**	110,747	0.19	23,554**	0.078	4.45*
	(3,935)	(0.06)	(70,809)	(0.14)	(8,745)	(0.11)	(2.08)
Nonpartisan nominating institution	18,019**	0.084	160,832	-0.057	19,804	-0.12	-1.37
	(6,607)	(0.11)	(119,489)	(0.12)	(13,000)	(0.10)	(1.55)
Observations	1,200	1,200	1,200	1,200	1,200	1,200	1,200
R-squared	0.302	0.826	0.262	0.787	0.356	0.741	0.578
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.

fifth and sixth on counts of contributors. Estimates have larger standard errors on coefficients, but point estimates suggest reform increases both the count of contributions reported and the count of unique contributors. Magnitudes are on the order of 100,000 new contributions and 20,000 new contributors with reform. The loglinear models (columns four and six) suggest -5 (non-partisan) to 21 (less-costly) percent increase in number contributions but -11 (non-partisan) to 8 (less-costly) percent increase in number contributors, all estimated with notable sampling variability.

The seventh column addresses the destination of increased donations. Results suggest reform increases percentage of contributions classified for the primary election (in open primaries) by around 4.5 points, but fewer primary contributions in nonpartisan primaries relative to contributions in the general.

Table 4 estimates effects on individual donor actions. Table 5 presents DID effects with the dependent variable receipts for candidates of each major party, state, and election cycle. These sums differ from those in Table 4 by including contributions from non-individuals like PACs and from individual contributions not itemized (small donations).[10] The first column presents the effect of reform on candidate receipts, with estimates that moving to less-costly and nonpartisan primaries increases receipts by about $19 and $18.5 million. The loglinear model (column two) estimates increases of 56 and 17 percent. The third and fourth columns show that the count of contributors increases with less costly reform and is uncertain with nonpartisan reform.

If primary reform increases the heterogeneity of the primary electorate, one response to reform might be for donors to increase support for status quo incumbents or for parties to increase coordination on preferred candidates (Cohen et al., 2008; Hassell, 2015). Columns five and six present suggestive evidence of both pathways. In states with reform, best estimates are that percent of all candidate receipts to incumbents (in races with an incumbent) increase between one and six points. Across all contests, percent of receipts to winners in-

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[10] I aggregated the data for Table 4 from the individual contribution files from DIME. Bonica (2019) tabulates sums by recipient as a separate summary file. See Appendix Section B for details on aggregation choices.

Table 5: Difference-in-differences effects of primary reform on candidate receipts, all offices

	(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
Less costly nominating institution	18,937** (4,625)	0.45* (0.21)	43,160** (12,251)	0.30** (0.09)	1.10 (6.0)	5.14 (3.4)
Nonpartisan nominating institution	18,567* (8,827)	0.16 (0.13)	11,889 (12,593)	-0.054 (0.10)	5.95 (3.7)	2.49 (3.7)
Observations	1,200	1,200	1,200	1,200	1,168	1,200
R-squared	0.318	0.503	0.337	0.583	0.011	0.617
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 institutions most costly for individual participation.

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creases on the order of two to five points. These results are uncertain, but suggest reform does lead to changes in which candidates obtain the larger share of donations.

In sum, primary reforms appear to increase turnout but also increase campaign donations from individuals and receipts to candidates. The magnitude of these estimates is of political importance, with turnout increases of up to six points and campaign finance increasing between 9 and 55 percent. These findings are consistent with (a) primary reforms changing the set of eligible voters who vote in primaries [the increase in turnout] as in pathway A→B from Figure 1, but (b) political actors sidestepping reform through alternative pathways of influence [the changed level and patterns of campaign finance], pathways F→D and F→E in Figure 2.

Are increased donations due to increased competition?

An alternative explanation of these findings is that increases in campaign finance follow from increased competition in primary elections. Indeed, competition was one of the arguments for Initiative 872 in Washington ("More competitive primaries and general elections"). This alternative mechanism, however, is inconsistent with the theory of sidestepping reform because increased competition is not an alternative pathway of political influence as clearly as is campaign finance.

In Table 6, I present DID results on three measures of political competition. Column one estimates the effect of primary reform on the percentage of House seats for each state-party with at least two (non-write-in) candidates, i.e. a contested seat. Column two estimates the effect on number of primary candidates, and three the log of number candidates. Column four estimates the average margin over second place of the winning candidate. Increasing competition would suggest positive effects in columns one, two, and three, and a negative effect in column four. Point estimates in columns one, two, and three are near zero with five of six in the direction suggesting decreased competition. Confidence intervals exclude effects of reform on percent primaries contested greater than 8 percent. Coefficient

Table 6: Difference-in-differences effects of primary reform on competition in House primary elections, 1992 to 2014

	(1) Percentage House Primaries Contested	(2) Number House Primary Candidates	(3) Log Primary Candidates	(4) Average Winning Margin (Percent)
Less costly nominating institution	-4.9	-2.9*	-0.2	12
	[-15 - 5.2]	[-5.5 - -0.4]	[-0.5 - 0.02]	[-0.2 - 24]
Nonpartisan nominating institution	0.9	-2.1	-0.08	14*
	[-6.4 - 8.2]	[-5.0 - 0.9]	[-0.2 - 0.02]	[2.7 - 25]
Observations	1,093	1,093	1,093	969
R-squared	0.069	0.109	0.086	0.039
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.

estimates in column four are politically important *in the wrong direction*, suggesting primary reform increases the average winning margin. In total, Table 6 suggests against primary reform increasing political competition at primary elections and thus against competition as the factor driving increased donations.

Heterogeneous effects by previous primary turnout

The theory of sidestepping reform suggests that when one avenue of influence is closed, actors opposed to the reform pursue alternative avenues. I have presented evidence of this phenomenon at the state-party-level in the context of loosening access to participation in primary elections. However, this state-party-level relationship may follow from different, non-sidestepping mechanisms. For example, if liberalizing access to primaries increased

participation more generally, it might incidentally increase participation outside of voting, such as making donations. A general increase in participation could be an omitted variable for sidestepping reform making the evidence so far presented ambiguous relative to my theoretical argument.

An individual-level empirical implication of the argument suggests itself to evaluate the mechanism. The individuals most affected by primary reform are those who had previously been participants in closed party primaries. An avenue of influence these individuals were previously taking is closed to them. The theory suggests that these individuals should be the most responsive to reform, the most motivated to find new actions so as to sidestep. While the theory does not say absolutely that previous primary participants should increase their donations – there may be other avenues of influence outside of donations – finding such a pattern would reinforce the interpretation of the state-party-level relationship. If it is the actors aggrieved by the reform (previous primary voters) who respond, we should see greater increases in contributions from previous primary voters than from actors who were not previously participating in primary elections. If it is a general increase in engagement causing the increase in contributions, we should not see variation in the increase by previous primary participation.

In this section, I drill down to evaluate if primary reform prompts individuals who had previously participated at higher rates in closed primaries to increase donations more than individuals who had previously participated at lower rates. The ideal design would be to enumerate every eligible voter in each state, match them to their primary turnout history and their contribution history, and run a DID similar to that utilized above at the state level.

Unfortunately, data limitations prevent this analysis. First, state voter files generally retain only a few recent turnout histories and, in some cases, only for currently-active registrants. Using a current voter file to measure primary turnout from many years previous induces extensive missingness. Second, matching voter file records to contribution records is incredibly difficult because there is no unique identifier that matches individuals from

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one to the other. While both data sets have name and address, both are subject to idiosyn-cratic standardizations and the contribution records especially are subject to mis-reporting and entry errors. In preliminary efforts to merge between the two data sets, I found that matching on full name and zip code led to less than 25 percent success in matches.

I implement an alternative toward the ideal DID. To address the first problem of enu-meration of individual turnout histories back in time, I have personally been collecting voter file snapshots from California and Washington since 2006. These collections cover year of reform for these two reform states, which allows me to run a two-state interrupted time-series. The DIME compilation also covers contributions in these two states for this time period.

To address the second challenge of matching individuals with poorly-recorded names and addresses, I implement a partial-aggregation procedure. I assume that surname is the data field least-likely to be recorded with error. On this assumption, I aggregate turnout histories from the voter files and individual contributions from DIME to the surname-state-year. I then merge average turnout in the most recent four primaries for individuals to sums and counts of contributions on surname, state, and election year. For example, in 2008 in California, I sum for each individual registrant turnout in the 2008 presidential, 2008 congressional, 2006 congressional, and 2004 presidential primaries and then take the average of those sums across all registrants with the same surname. From the DIME data, I sum and count contributions made in 2007 and 2008 for each surname with a reported address in California. These two records are then merged to create the combined data set.

I purge surname of all spaces and special characters, which leads to 87.8 percent of surnames in the contributor data matching a surname in the voter files. I locate at least one contribution for the surname of 82.5 percent of registrants in the voter files.

Turnout can vary by large magnitudes in primary elections, so I don't simply interact the reform indicator with average turnout. Instead, for each state-year, I classify previous primary turnout into the top quartile, third quartile, and bottom half. This categorizes state-

surname-election observations into three groups, surnames that participated to the highest degree in closed primaries (top quartile), middling degree (third quartile), or lowest degree (bottom half).

In Table 7, I present ITS models of contributions on primary reform indicators interacted with each of the previous primary turnout categories with level of observation the state-surname-election. Because this data set includes only California and Washington for the subset of years 2006 to 2016, it covers only nonpartisan reforms and there is no less costly reform category. The ITS has state-surname and election fixed effects and I also include turnout category fixed effects.

Coefficients of interest measure how contributions respond to primary reform by previous primary turnout, holding fixed average state-surname contributions, average primary turnout group contributions, and election-specific effects on contributions. All standard errors are clustered on the state-election.

The first column presents the relationship between the sum of contributions by state-surname and nonpartisan primary reform interacted with category of previous primary turnout. The first coefficient estimates that among surnames with the lowest previous primary turnout, total contributions increased by an average $122 after primary reform. In the second turnout category, the coefficient indicates total contributions decreased by $658 and in the third, highest-participating turnout category, increased by $342. Each of these point estimates is subject to large uncertainty such that the standard errors make uncertain whether any of the effects are greater or less than zero.

Coefficients in the remaining three columns are all estimated with greater certainty, allowing us to reject a null hypothesis of an effect less than zero. Column two presents the log-linear model, where the coefficients indicate that contributions increased following reform by 34 percent for surnames in the lowest half of previous primary participation, 57 percent for surnames in the third quartile, and 49 percent for surnames in the fourth

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Table 7: Interrupted-time-series by previous primary turnout

	(1) Sum of Contributions	(2) Log Sum Contributions	(3) Number Contributions	(4) Log Number Contributions
Nonpartisan*Bottom half previous primary turnout	122 (86.6)	0.29** (0.00)	1.70** (0.07)	0.12** (0.00)
Nonpartisan*3rd quartile previous primary turnout	-658 (475.2)	0.45** (0.01)	13.2** (0.30)	0.33** (0.00)
Nonpartisan*4th quartile previous primary turnout	342 (235.2)	0.40** (0.00)	2.87** (0.07)	0.21** (0.00)
Observations	11,120,076	11,118,830	11,120,076	11,120,076
R-squared	0.000	0.015	0.005	0.032
Number of name_state	1,853,346	1,853,346	1,853,346	1,853,346
State-year FEs	Yes	Yes	Yes	Yes
Name-state FEs	Yes	Yes	Yes	Yes
Previous turnout group FEs	Yes	Yes	Yes	Yes

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

OLS coefficients with robust standard errors clustered on state-election in parentheses. Excluded category is closed partisan primaries. Limited to California and Washington.

quartile.[11] A similar pattern manifests for the number of contributions and log number in columns three and four.

Columns two, three, and four provide evidence in support of the theory of sidestepping reform. In each case, registrants who had been previously participating in close primaries in the top half increased their contributions in response to reform at higher rates than those in the bottom half of previous turnout. Surnames with higher primary participation increased the dollar amount of contributions by 23 and 15 percentage points more than those in the lowest category, all else equal.

In sum, the partial-aggregate ITS analysis finds that individual political contributions increased more from those who were participating in closed primaries prior to reform. This provides evidence in support of a sidestepping interpretation to the state-party-level results.

Robustness and alternative explanations

In the Appendix, I address robustness. Appendix Section C applies a correction following Conley and Taber (2011) to adjust inference for a setting of a small number of treated units, finding two results in Table 4 move out of statistical significance while three results move into significance. Adjusting inference for a small number of treated units does not substantively alter overall conclusions.

Appendix Tables A5, A6, and A7 reproduce Tables 3, 4, and 5 using the original McGhee et al. classification of primary election types. Point estimates suggest larger effects of open and non-partisan institutions than of semi-open and semi-closed institutions relative to the baseline category of closed. Semi-open primaries appear to generate more turnout than semi-closed primaries.

Appendix Table A9 considers heterogeneity in the effect of reform on candidate receipts for different offices. Effects are consistent for governor, House, and Senate contests.

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[11] Log contributions include an additional dollar added to each count so that the log is defined. Surnames with negative sums of donations are dropped because the log is undefined.

Discussion

From their inception, one goal of primary elections was to democratize candidate nominations (Merriam and Overacker, 1928). Recent reforms aimed at easing access to the primary ballot have similar goals and I estimate indeed have increased participation. Yet in scholarship with many research designs and varied sample populations, we have estimated inconsistent effects on legislative polarization and primary voter moderation.

The theory of sidestepping reform and evidence here provides an explanation for this variability. Reforms may very well increase participation and provide moderating incentives for candidates. But reforms also influence strategic campaign donations, which compete for influence with a more inclusive primary electorate. The net effect of increased turnout and increased magnitudes of campaign finance on political outcomes then depends upon politicians' demand for each and the mapping into their subsequent legislative behavior. In some settings, candidates may see greater need for donations, in others, greater need for votes from the newly-participating electors, leading to heterogeneity of the consequences of reform.

Reformers might conclude from the evidence here that institutional reforms must be multi-pronged, for example primary reform must be paired with campaign finance reform. Perhaps so. However, the theory of sidestepping reform holds that motivated actors react to reforms by pursuing alternative pathways of influence. If nomination politics and campaign finance pathways are both limited, we should anticipate actors will find other routes. The argument in Cain (2015) is compelling. It may be a fool's errand to try to prevent motivated actors from influencing elections and policy. Instead, reforms ought to promote pluralism and acknowledge "the critical role that intermediaries inevitably play in any large democracy (6)." It is difficult to take the politics out of politics.

The difficulty of taking politics out of politics harkens back to perhaps the first institutionalist of political science, James Madison (see Kernell, 2003), who suggested it futile to try to prevent faction and instead that "relief is only to be sought in the means of control-

ling its effects" (Madison, 1787). He argued for institutions that funneled factional impulse into competition and required compromise through checks and balances. Institutions are consequential, Madison suspected, but cannot prevent political actors who desire influence from taking action.

This essay suggests new inquiries for scholars of both electoral behavior and political institutions. First is the implication that participating in institutions of political choice such as primary elections and making pecuniary donations may be substitutes for or complements to each other, rather than stand-alone acts. Time-series analysis of individual choices in both realms could enlighten the causes of participation (Leighley and Nagler, 2014) and campaign donations (Brown, Hedges, and Powell, 1980; Brown, Powell, and Wilcox, 1995; Magleby, Goodliffe, and Olsen, 2018) and, perhaps, policy consequences. The results here offer a new explanation for why some eligible citizens choose to make candidate donations while others do not.

Second, other institutional reforms not considered here seem to have had more consistent and large consequences. Abolishing cross-filing, moving to a secret ballot, and the Voting Rights Acts all had material consequences on American politics. What was it about these reforms that kept sidestepping effort, to the degree it was present, from vitiating effects of reform? Theoretical and empirical consideration on the scope of sidestepping reform and the parameters of effective institutional reform would be of great value.

Third, that candidate receipts increase following reform shows that candidates do not exhaust the pool of available campaign funds in every election. This means that candidates may be trading off time raising funds with time on other activities. Candidate choices in allocation of effort have consequences for who gives, how much, and to what consequence (Milyo, 2001). Understanding of the dynamics of campaign finance would benefit from insight into how candidates make this trade off and how institutional reforms might influence their choices. It would be unfortunate if reforms aimed at improving representation instead caused politicians to spend more time raising money.

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Finally, these results speak to the effort to understand the consequences of primary elections for party polarization. There is disagreement in the literature as to how consequential are primary *reforms* (e.g., Bullock and Clinton, 2011; Gerber and Morton, 1998; Hill, 2015; Hirano et al., 2010; Kousser, 2015; Kousser, Phillips, and Shor, 2018; McGhee et al., 2014; McGhee and Shor, 2017), even if primary *elections* are consequential (e.g. Boatright, 2013; Brady, Han, and Pope, 2007; Hill and Tausanovitch, 2018). The evidence of sidestepping reform here suggests that to understand party polarization and the consequences of institutional reform requires analysis of the many competing mechanisms of reform together. To the extent different causes of polarization are complements or substitutes, relating over-time or cross-sectional variation in one institution may fail to accurately characterize the consequences of reform.

This essay illustrates benefits to considering the interplay of the many actions available to political actors as they pursue interests within multifaceted institutional contexts. Efforts to reform one facet must consider reaction of actors in others.

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Appendix

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.[12] 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

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[12]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.

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 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, non-partisan 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
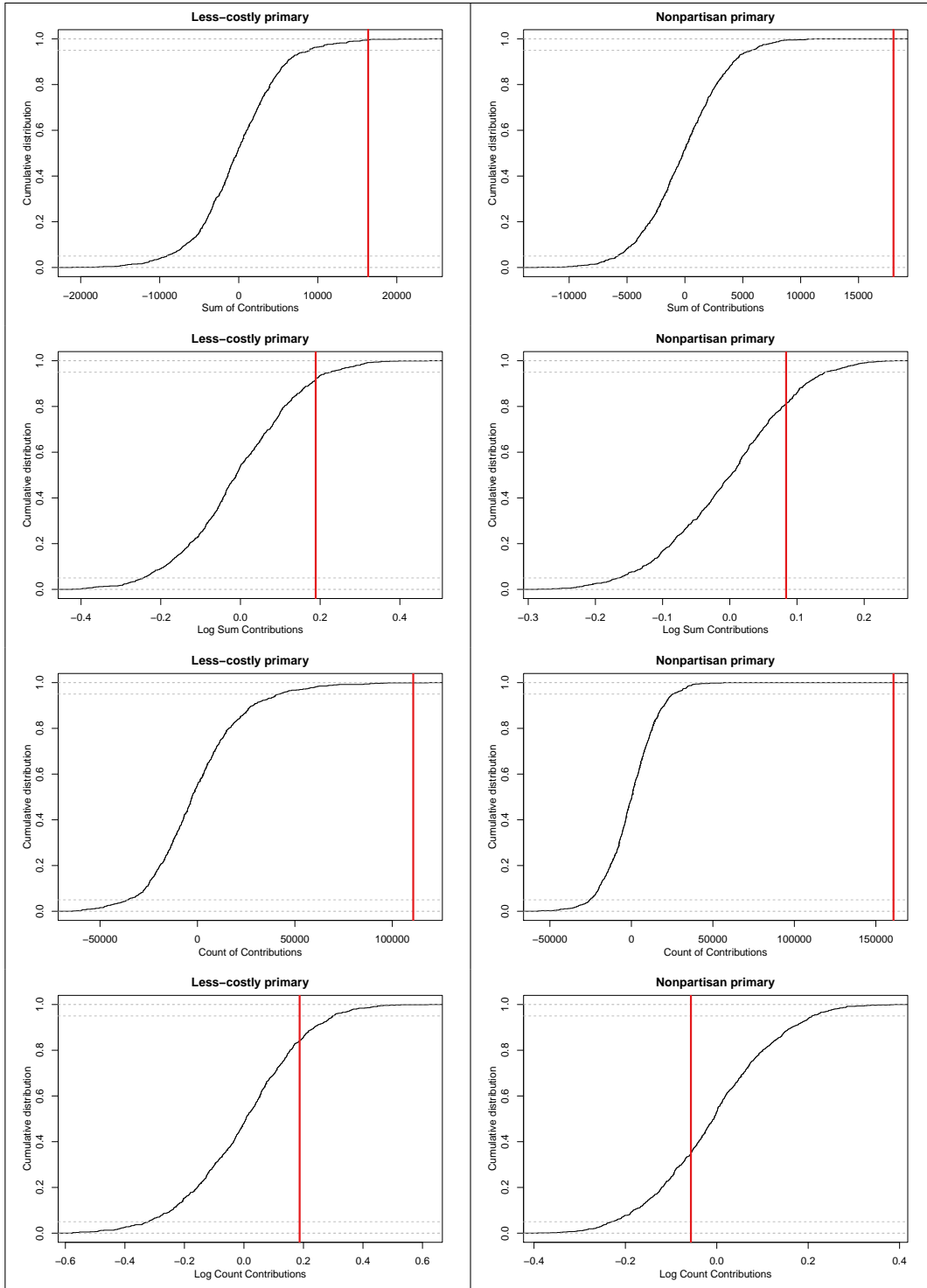
Note: Each frame plots the empirical cumulative distribution function for a bootstrap null distribution for difference-in-differences with the Conley and Taber (2011) correction for small number of treated units. Horizontal solid lines represent the actual coefficient estimate. Dashed lines at the 0.05 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)
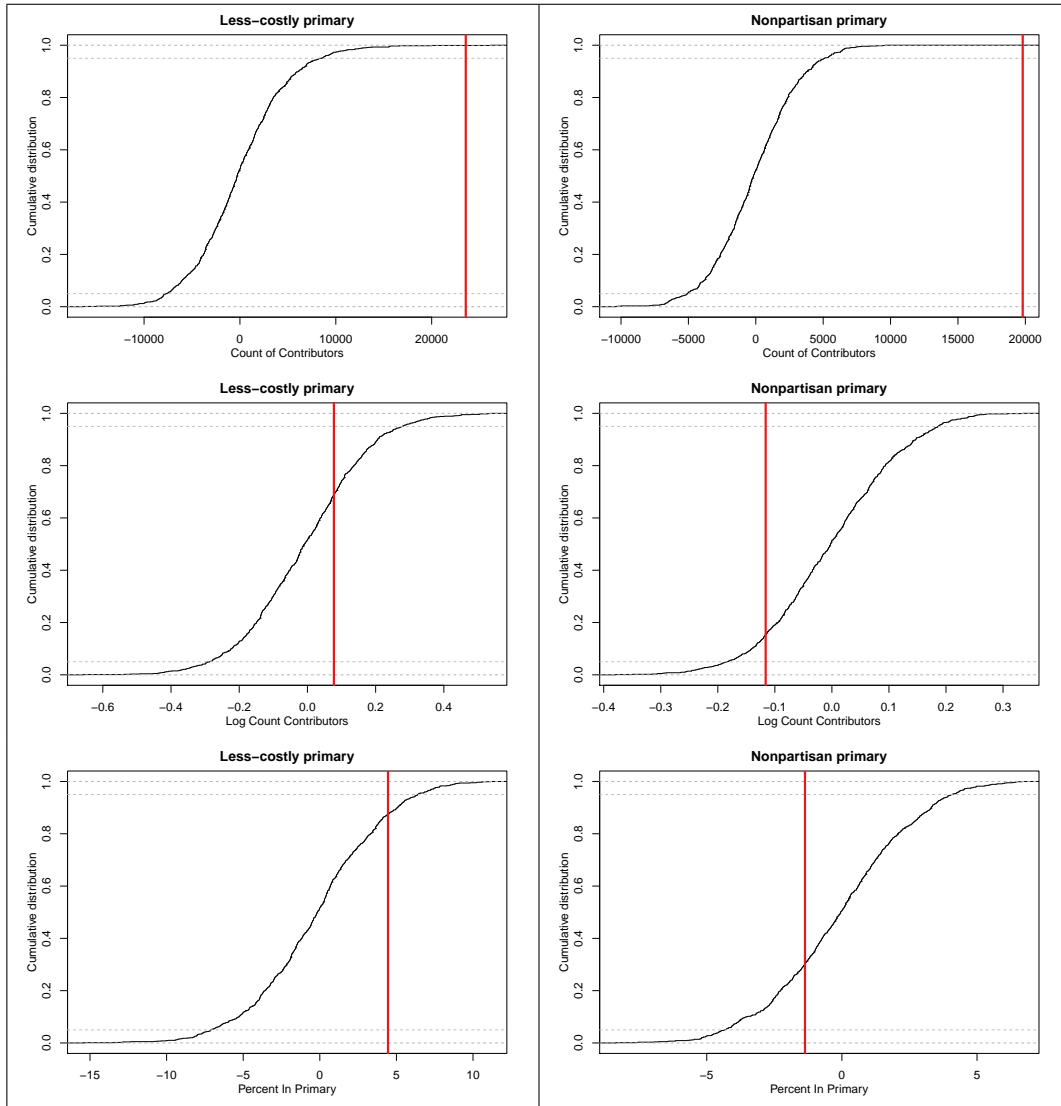
Note: *Each frame plots the empirical cumulative distribution function for a bootstrap null distribution for difference-in-differences with the Conley and Taber (2011) correction for small number of treated units. Horizontal solid lines represent the actual coefficient estimate. Dashed lines at the 0.05 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:

JUDY GOLBERG, Chair, President of Washington League of Women Voters; GARY LOCKE, Governor of the State of Washington, Democrat; KEN EIKENBERRY, former Washington Attorney General, past State Republican Chair; JOCELYN LANGLOIS, acting Chair, Libertarian Party of Washington State; JODY GRAGE HAUG, Membership Chair, Green Party of Washington; JOAN THOMAS, past President Seattle LWV, past President Washington LWV.

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