Online Appendix

In the Shadows of Sunlight: The Effects of Transparency on State Political Campaigns

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1.1 Case Selection

The Campaign Disclosure Project was a collaboration of the UCLA School of Law, the Center for Governmental Studies and the California Voter Foundation. It was supported by The Pew Charitable Trusts. Its list of principal investigators and participants includes some of the most important election law experts in the country, including Daniel Lowenstein (UCLA Law), Jessica Levinson (Loyola Law and Los Angeles Ethics Commission), and Paul S. Ryan (Senior Counsel, Campaign Legal Center).

We use the CDP's state disclosure scores as a proxy for the strength of each state's disclosure regime, to guide our case selection. Disclosure scores are available for the years 2003-2005 and 2007-2008. The scores are calculated using a 300-point system awarded in four categories:

1. *Disclosure laws* (120 points), including disclosure of contributors' occupations and employers, reporting of last minute contributions and independent expenditures, strong enforcement, frequent reporting requirements.

2. *Electronic filing* (30 points), including whether states mandate electronic filing and maintain a searchable database.

3. *Disclosure content accessibility* (75 points), including how easy and inexpensive it is to obtain records from a distance, usually via the Internet, and ways the data could be analyzed online (e.g. searching, filtering, online analysis, and downloadable content).

4. *Online usability* (75 points), an evaluation of the user experience on state disclosure websites,with states earning higher scores for websites that included information about the laws, disclosure requirements, and reporting periods, as well as original content such as the state's own analysis or overviews.

States are assigned letter grades based on this point system, which we convert into an ordinal numeric scale for ease of analysis, 0 for 'F' and 11 for 'A'. Most states improved their scores over time. In 2003 all states scored a 5 (C) or lower, with the modal score being a 0 (F). By 2008 the median score was 6 (B-). The mean score monotonically increased over the time period from 1.4 (between a D- and D) to 4.7 (between a C- and C).

In an ideal world, we would be able to isolate the components of each of the sub-categories that influenced the scores in each subcategory. Indeed, the CDP published a list of the hundred or so variables coded for each state in each year.[1] However, the data for the scoring components is unavailable. Table 4 is our attempt to

[1] See "Grading State Disclosure Criteria" at http://www.campaigndisclosure.org/gradingstate2007/appendix3.html.

capture the changes that we can still observe, a decade later, either because they are in the CDP summaries for the states, or because they were changes enshrined in law. We are unable to create our own measure using a tool like factor analysis, because the only measures available – the four sub-measures – cannot be tested for more than one factor. While the scholars and lawyers involved in the CDP are nationally-recognized experts whose expertise we trust, our restricted ability to look "under the hood" of the measure is unfortunate. The two groups of states we identify fall cleanly into "big change" and "no change" states, but with more fine-grained data, we would have been able to do even more.

Partially as a result of the lack of fine-grained institutional information, in addition to the reality that states generally change a shifting bundle of visibility-related factors over time, we are unable to do two things. First, we are unable to test a "dose" response. We cannot evaluate the relative effect of discrete institutional changes. We can't, for example, say whether online searchability by employer has more of a deterrent effect to political participation than mandatory electronic filing by candidates, which makes disclosure information available more quickly. Further research, in an experimental setting, will be needed to pin down which features of disclosure cause the greatest amount of opting out. We are also unable to rule out that *some* increases in visibility might actually reduce the propensity to opt out, and that what we are observing is the offsetting effects of two kinds of reforms working against each other, resulting in our negligible findings. While we think it highly unlikely that any of the discrete changes observed (e.g., in the price of copies, in the ability to download data, or in the ability to search by name, etc) would actually *increase* donors' propensity to contribute again in a subsequent election, we cannot rule out the possibility, given the nature of the data. Again, we think that conducting follow up research in laboratory experiments would be beneficial. Appendix 1.5 discusses dose response more indepth.

1.2 Calipers

We restrict the populations to areas of overlap, cutting 10 cases of people who gave less than $4 or more than $688,615. We do this to ensure complete coverage for causal inference.[2] We also drop 326 contributors with ideologies that fall outside of the -2 to 2 interval, as they are so politically extreme that estimates based on their behavior might bias our results for the rest of the population. This decision drops 0.1% of the data. (Neither decision affects our results.)

[2]We trim because there is no valid counterfactual for those 10 observations.

1.3 Misreported zip codes

In addition to non-reporting of zip codes in New Jersey and Kansas for 2000, more than 10% of contributor zip codes in Arkansas, New Mexico, South Carolina, Dakota, Vermont, and Wyoming were misreported as well. It seems unlikely that this high of a percentage of misreporting could have been initiated by the contributors, given that the rest of the states have much lower rates of misreporting, most below 4%. Moreover, the misreporting decreased over time. For example, Arkansas has 704 misreported zip codes in 2000 but only 91 in 2004. Iowa had 635 in 2000 and 69 in 2004. Other states had even more drastic reductions: Arizona, Minnesota, North Dakota, Nebraska, Oregon, Virginia, Vermont, and Wyoming all reduced misreporting by over 90% between 2000 and 2004. The size of the reductions strikes the researchers as related more to technological improvements than a drastic change in the level of trust among contributors. Furthermore, among contributors whose zip codes are misreported in 2000 or 2004, 1699 of them who contributed in both elections only have an incorrect zip code in one of the elections in which they contributed, and they were equally likely to have an incorrect address reported in 2000 and later corrected as they were to report correctly in 2000 and later misreport in 2004. All of this points technological or random errors more than to contributor concerns about privacy. Therefore, we are likely excluding many randomly-misreported zip codes, out of an abundance of caution.

There is no statistically-distinguishable difference between the amounts given by those whose zip codes are incorrect (mean $733) and those whose zip codes are correct (mean $ 701, $p = 0.61$).[3]

If the incorrect zip codes in the NIMSP data correlate to ideology, then our estimate could misstate the scale of the influence of ideology on opting out. Those whose zip codes were wrongly reported in the pre-period are slightly to the right, ideologically from those whose zip codes were not wrongly reported (0.17 vs. 0.11, $p = 0$). However the distance between them is $1/10$ of a standard deviation. When we look among misreporting in treatment and control states, we see that the ideologies of contributors with misreported zipcodes in treatment and control (0.18 and 0.15, $p = 0.38$) states are closer than the ideologies of those with properly-reported zip codes (0.08 and 0.54, $p = 0$). Among those who misreported only in 2008, treatment states had a 0.2% misreporting rate and control states had a 0.5% misreporting rate. It therefore seems that our results are missing zip code information for a small number of fairly moderate contributors, which, if anything, will cause us to overstate the effect we observe. While overstating is generally worrisome, here, we argue for a negligible effect, so erring on the side of overstatement is the more conservative approach.

[3] Both groups have a median of $200. The lack of difference persists when we look within group at misreporters and non-misreporters. Among treatment group contributors, misreporters gave a mean of $637 and those without misreported zip codes gave a mean of $662 ($p = 0.55$). Among control contributors, the numbers are $1098 and $1294, respectively ($p = 0.45$).

1.4 Clustered Bootstrap Methodology

Clustered bootstrapping allows us to circumvent a known challenge with combining a treatment dummy and fixed effects in the same regression. Fixed effects regressions omit one of the fixed effect categories as a reference category. There exists a commonly-acknowledged quirk of using a treatment dummy with fixed effect dummies in regression, which, to our knowledge, no literature currently informs. The use of the treatment dummy means that the fixed effects require a reference category – here, a reference state – from *both* treatment and control groups. Our statistical software `R` always drops the alphabetically-last state from each randomly-selected group of states in the cluster bootstrap process. As a result, states like Wyoming and Washington have a much higher probability of being omitted from analysis through the resampling process, which biases our estimates. We therefore add a step to the resampling process. We first require that two treatment and two control states be selected randomly, without replacement. (We require two, otherwise the selection would also be used as the reference category and the run would fail because the treatment dummy would be either all 1s or all 0s, with no ability to detect a 1-to-0 difference.) Then we randomly selected one of the treatment and one of the control states to be the reference category. Then we drew, with replacement, 19 more states from the full list of 23 states. (If any of the second draw matched the states already chosen as the reference category, we labeled them as reference as well.) This two-step sampling process allowed us to equalize, in expectation, the probability that any given state would be the reference category within the treatment and control groups over the 1,000 replications.

1.5 Is there a dose response?

We cannot detect a dose response, given the nature of the data. Different configurations of institutions and data availability combine to create the same scores and same magnitudes of improvement. The aggregated nature of the data do not permit us to say whether, for examples, effects of a three-point increase in disclosure score is more impactful from a lower starting point.

We present below the raw repeater drop off for each state, along with the 2004 and 2008 disclosure scores.

Figure 1 displays the information in Table 1 in a way that might help us to detect a dose response. Each point corresponds to a state and is located at the intersection of the 2004 disclosure score for the state and the magnitude of the 2008 improvement. The size of the point indicates the average drop off for that state. If starting with very little disclosure and increasing data availability at all causes bigger effects than starting with some amount of disclosure and increasing data availability, we would expect to see larger points on the left side of the Figure, which correspond to the lower disclosure scores. We do not. The average effect for states with a score of 0 in 2004 is -0.13, and the average effect for states with a score greater than 0 in 2004 is -0.123. The average effect for states with a score of 0 or 2 in 2004 is also -0.13, and the average effect for states with a score greater than 2 in 2004 is -0.127.

	State	Score 04	Score 08	Score Diff	Repeater drop 04-08
1	AR	0	3	3	-0.09
2	AZ	3	7	4	-0.12
3	CO	4	8	4	-0.11
4	IA	0	3	3	-0.14
5	KS	0	3	3	-0.14
6	MN	2	7	5	-0.08
7	NC	3	7	4	-0.14
8	NJ	5	8	3	-0.11
9	NY	2	8	6	-0.13
10	OK	4	7	3	-0.16
11	OR	2	9	7	-0.14
12	SC	0	5	5	-0.12
13	VA	3	9	6	-0.12
14	WV	0	6	6	-0.16

Table 1: Raw data on state disclosure scores and repeat contributor participation decreases from 2004-2008.

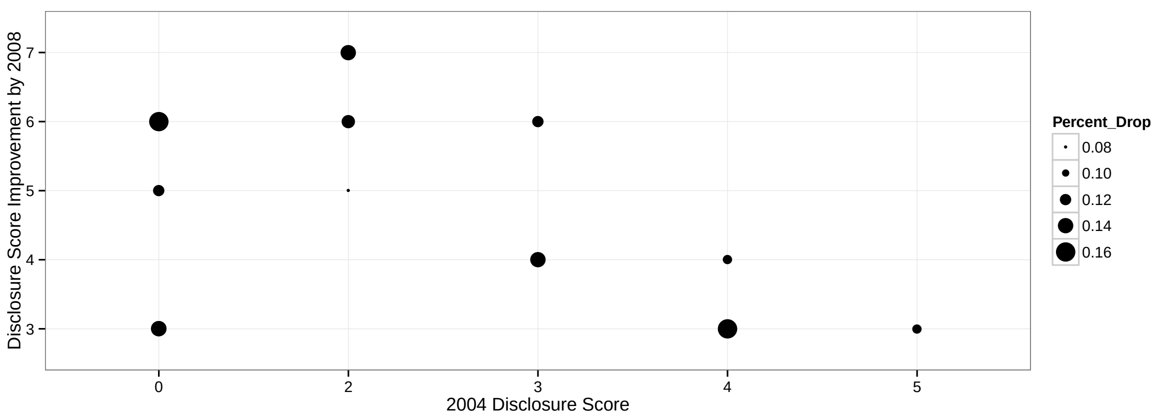

Figure 1: Each treatment state's 2004 Disclosure Score plotted against its improvement over time. The size of each point reflects the overall contributor drop-off rate for the state making the improvement, with larger dots indicating larger drop-offs.

2 Tables to Support Figures 2 through 4

	\$100 or less	\$101 to \$249	\$250 to \$499	\$500 to \$999	>\$999
Intercept	0.14	0.14	0.17	0.24	0.38
	[0.1 , 0.24]	[0.1 , 0.24]	[0.1 , 0.34]	[0.14 , 0.56]	[0.22 , 0.79]
Treatment	0.02	0.02	0.06	0.06	0.01
	[-0.12 , 0.26]	[-0.12 , 0.26]	[-0.15 , 0.31]	[-0.23 , 0.29]	[-0.41 , 0.31]
Post	-0.04	-0.04	-0.04	-0.06	-0.1
	[-0.06 , -0.01]	[-0.06 , -0.01]	[-0.08 , -0.01]	[-0.1 , -0.02]	[-0.11 , -0.05]
Treatment ∗ Post	**-0.03**	**-0.03**	**-0.03**	**-0.03**	**-0.01**
	[-0.07 , 0.01]	**[-0.07 , 0.01]**	**[-0.08 , 0.02]**	**[-0.08 , 0.02]**	**[-0.07 , 0.03]**
State Fixed Effects	yes	yes	yes	yes	yes
Median N. Obs.	63450	28488	29169	20556	24945

Table 2: Repeat contributions among 2000 contributors in 14 treatment states and 9 control states, analyzed based on the amount contributed. Dependent variable is whether the contributors gave again in a subsequent cycle (2004 or 2008). Difference-in-differences estimates of the difference in contribution percentages in 2008 and 2004 for treatment and control groups are shown in boldface. Confidence intervals (90%) are provided below the estimates. They are generated using a cluster bootstrap with 1000 replications. These estimates are used to construct Figure 2.

	< -0.5	-0.5 to -0.01	0 to 0.49	> 0.5
Intercept	0.14	0.31	0.15	0.31
	[0.09 , 0.34]	[0.16 , 0.62]	[0.08 , 0.32]	[0.14 , 0.45]
Treatment	0.04	-0.03	0.02	0.01
	[-0.16 , 0.2]	[-0.34 , 0.18]	[-0.16 , 0.18]	[-0.18 , 0.19]
Post	-0.03	-0.12	-0.03	-0.1
	[-0.06 , 0.01]	[-0.22 , -0.05]	[-0.05 , 0.02]	[-0.12 , -0.06]
Treatment $*$ Post	**0.01**	**0.04**	**-0.03**	**-0.01**
	[-0.04 , 0.05]	**[-0.05 , 0.15]**	**[-0.07 , 0]**	**[-0.06 , 0.02]**
State Fixed Effects	yes	yes	yes	yes
Median N. Obs.	31546	26441	40043	31541

Table 3: Repeat contributions among 2000 contributors in 14 treatment states and 9 control states, analyzed based on the ideological distance from the average contributor in one's zip code. A positive ideological distance means the contributor is to the right of the average contributor in the zip code. A negative distance means the contributor is to the left of the average contributor in the zip code. Dependent variable is whether the contributors gave again in a subsequent cycle (2004 or 2008). Difference-in-differences estimates of the difference in contribution percentages in 2008 and 2004 for treatment and control groups are shown in boldface. Confidence intervals (90%) are provided below the estimates. They are generated using a cluster bootstrap with 1000 replications. These estimates are used to construct Figure 3.

	< -1	-1 to -0.5	-0.49 to -0.01	0 to 0.49	0.5 to 0.99	1 and above
Intercept	0.16	0.18	0.16	0.28	0.25	0.17
	[0.01 , 0.33]	[0.12 , 0.38]	[0.09 , 0.39]	[0.18 , 0.41]	[0.13 , 0.55]	[0.04 , 0.37]
Treatment	0.08	0.06	0	0	-0.01	-0.05
	[-0.14 , 0.27]	[-0.17 , 0.23]	[-0.26 , 0.21]	[-0.19 , 0.23]	[-0.33 , 0.16]	[-0.24 , 0.15]
Post	-0.05	-0.03	-0.08	-0.13	-0.06	-0.05
	[-0.09 , 0.04]	[-0.06 , 0]	[-0.12 , 0]	[-0.23 , -0.01]	[-0.1 , -0.01]	[-0.08 , -0.03]
Treatment $*$ Post	**-0.01**	**-0.02**	**0.04**	**0.02**	**-0.01**	**0.02**
	[-0.1 , 0.05]	**[-0.06 , 0.03]**	**[-0.05 , 0.09]**	**[-0.1 , 0.12]**	**[-0.07 , 0.03]**	**[-0.01 , 0.04]**
State Fixed Effects	yes	yes	yes	yes	yes	yes
Median N. Obs	11122	23454	23023	16355	40093	14129

Table 4: Repeat contributions among 2000 contributors in 14 treatment states and 9 control states, analyzed based on the measure of raw ideology, or conservatism score. Dependent variable is whether the contributors gave again in a subsequent cycle (2004 or 2008). Difference-in-differences estimates of the difference in contribution percentages in 2008 and 2004 for treatment and control groups are shown in boldface. Confidence intervals (90%) are provided below the estimates. They are generated using a cluster bootstrap with 1000 replications. These estimates are used to construct Figure 4.

3 Robustness Checks

3.1 Results using a two point change threshold

To test our identification assumption that a disclosure score improvement of three points or higher constituted 'treatment', we relaxed the assumption to a two-point change constituting treatment. This increased the size of the treatment group by 5 states (Hawaii, Montana, New Hampshire, Texas, Wisconsin) and 40,024 contributors. We present below the main results using this new treatment group. The point estimate that results is 0, with a maximum negative effect of -0.03. If anything, using a 2 point threshold would strengthen our argument that disclosure has negligible effects here. In the interest of social scientific integrity (to avoid data mining), we stick with the three-point threshold from our initial research design.

	Model 1
Intercept	0.18
	[0.15 , 0.35]
Treatment	0.02
	[-0.14 , 0.13]
Post	-0.07
	[-0.08 , -0.05]
Treatment Post	**0.0005**
	[-0.03 , 0.02]
Fixed Effects	yes

Table 5: Average effects of increased disclosure among 215,668 contributors in 19 treatment states and 9 control states, where the threshold of determining whether a state is in the treatment group is relaxed to a 2 point improvement in disclosure scores. All members of the sample contributed in the year 2000. Dependent variable is whether the contributors gave again in a subsequent cycle (2004 or 2008). Difference-in-differences estimates of the difference in contribution percentages in 2008 and 2004 for treatment and control groups is shown in boldface. Confidence intervals (90%) are provided below the estimates. They are generated with clustered bootstrapping (1000 replications).

3.2 Placebo Test with Federal Data

In this section, we present a placebo test with federal contribution data. The tables and figures here echo the tables and figures in the main text; the only difference is that the data we used was federal contributions to candidates for the U.S. House of Representatives from a given state.

Because there were no changes in federal disclosure laws over the 2004-2008 time period (and because, even if there were changes, they would affect contributors from all states equally), we should not observe any differences between treatment and control states. Estimates should be close to zero. If estimates with the federal data are less negative (more positive) than estimates with the state data in the main text, then the triple difference would imply that whatever trend was happening at the federal level, the more pronounced difference between treatment and control states at the state level would indicate that there could actually be treatment effect of enhanced disclosure among those contributing to state races. But what we see, almost across the board, is that federal-level estimates are more negative at the federal level. Moreover, for the most part, the lower bound on the 90% confidence intervals is lower for the estimates of federal data than state data.

These results help support our argument that the effect at the state level is negligible: estimates on the state contributor data are the same as, or closer to zero than, the effects we observe where there was no treatment at all, among federal contributors.

	Model 1	Model 2	Model 3
Intercept	0.25	0.71	-0.73
	[0.11 , 0.5]	[0.5 , 1]	[-0.99 , -0.48]
Treatment	0.02	0.05	-0.02
	[-0.25 , 0.20]	[-0.2 , 0.29]	[-0.24 , 0.16]
Post	-0.01	-0.01	-0.04
	[-0.06 , 0.06]	[-0.06 , 0.06]	[-0.06 , -0.01]
Treatment $*$ Post	**-0.04**	**-0.04**	**-0.03**
	[-0.1 , 0.03]	**[-0.1 , 0.03]**	**[-0.04 , -0.02]**
Ideology		-0.02	0.16
		[-0.04 , -0.01]	[0.13 , 0.19]
log(Amount)]			-0.02
			[-0.12 , 0.14]
log(Rel. Amount)		0.07	
		[0.06 , 0.09]	
State Fixed Effect	yes	yes	yes

Table 6: Placebo test of 2004 and 2008 repeat federal contributions among 2000 federal contributors in 14 treatment states and 9 control states. Dependent variable is whether the contributors gave again in a subsequent cycle (2004 or 2008). Difference-in-differences estimates of the difference in contribution percentages in 2008 and 2004 for treatment and control groups is shown in boldface. Confidence intervals (90%) are provided below the estimates. They are generated with a cluster bootstrap (1000 replications).

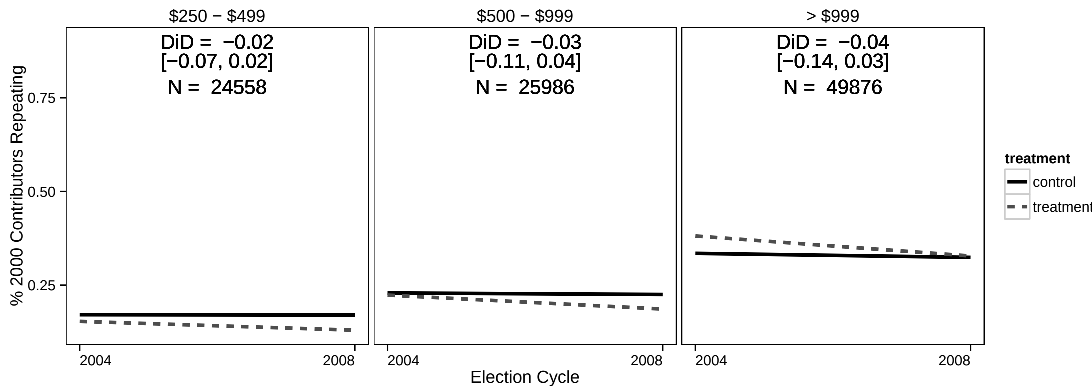

Figure 2: Repeat contributions in a given federal election cycle by amount contributed in 2000 to federal elections, calculated with 1000 bootstrapped difference-in-differences regressions. The repeating percentage decreases in control states (solid black line) and decreases slightly more in treatment states (dashed, medium gray line) in the wake of enhanced visibility. Same division of amounts as in main text, though at the federal level, disclosure only occurs for amounts $250 and over.

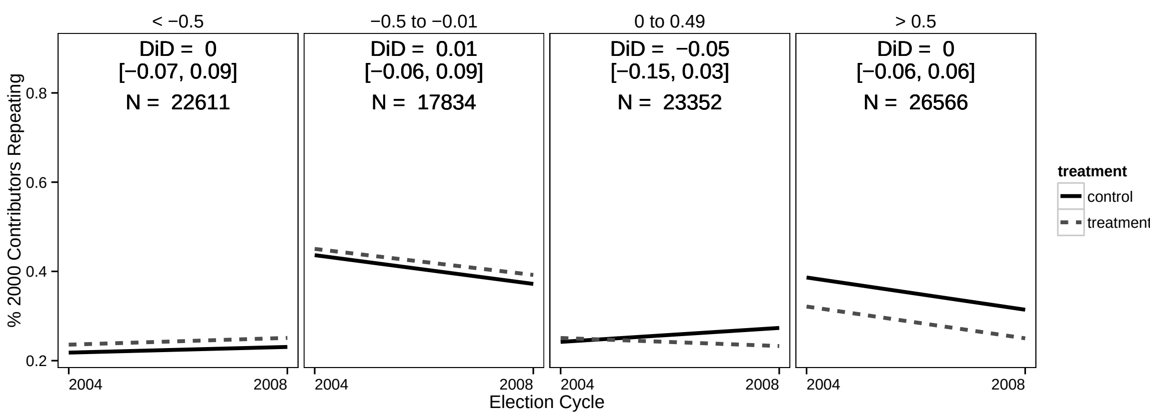

Figure 3: Repeat federal contributions to same-state candidates by 2000 contributors in the year 2004 and 2008, grouped by each contributor's ideological distance from others in their zip codes. Within-panel difference-in-difference estimates with 90% confidence intervals reported.

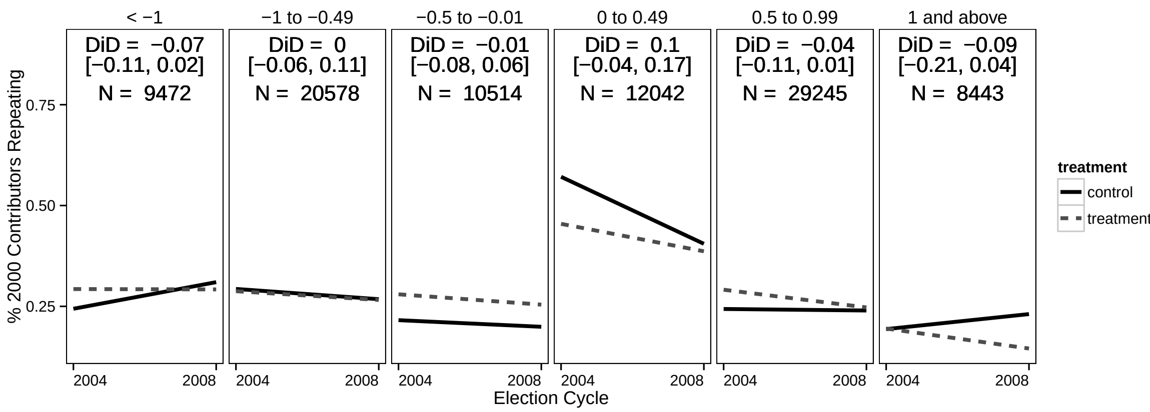

Figure 4: Repeat federal contributions to same-state candidates by 2000 contributors in the year 2004 and 2008, grouped by ideological ranges (without taking ideological distance into account). Within-panel difference-in-difference estimates with 90% confidence intervals reported. Confidence intervals are generated using clustered bootstraps (1000 replications).

4 Legal changes in treatment states

The following multipage table shows legal changes in treatment states over the time period.

State	Group	Web. nav. added	Contributions searchable by donor's... Name	Geography	Employer	Amount	Download data	Electr. filing	Data price ↓	↓ report threshold	Current statutes
Arkansas	Treatment	2005, 2008	2007			2008	2008	2008(v)			A.C.A. § 7-6-201 (amended 2009, 2011, 2013); A.C.A. § 7-6-203 (amended 2009, 2011, 2013); A.C.A. § 7-6-204 -06; A.C.A. § 7-6-207 (amended 2009, 2011, 2013); A.C.A. § 7-6-214; A.C.A. § 7-6-218 (amended 2013); A.C.A. § 7-6-220 (amended 2011)
Arizona	Treatment	2005, 2008	2003	2008				$\leq$ 2003			A.R.S. § 16-901; A.R.S. § 16-902.01 (amended 2008, 2010, 2012); A.R.S. § 16-904; A.R.S. §§ 16-912 - 15; A.R.S. § 16-916 (amended 2008, 2010, 2012); A.R.S. § 16-918 (amended 2008, 2010); A.R.S. § 16-943; A.R.S. § 16-958 (amended 2012)
Colorado	Treatment	2007, 2008	2003			2003		2003(v) 2007(m)			C.R.S.A § 1-45-108; C.R.S.A. § 1-45-109 (amended 2009, 2010); C.R.S.A. Const. Art. 28, §§ 2, 5, 7, 9, 10
Iowa	Treatment	2004, 2005, 2008						2003(v) law passed	2005 paper copies		I.C.A. § 68A.102 (amended 2008, 2010); I.C.A. § 68A.201 (amended 2010); I.C.A. § 68A.203; I.C.A. § 68A.401; I.C.A. § 68A.402 (amended 2008, 2010); I.C.A. § 68A.404 (amended 2008, 2009, 2010); I.C.A. § 68A.405; I.C.A. § 68A.501; I.C.A. § 68B.32A (amended 2008, 2010)
Kansas	Treatment	2004, 2007, 2008	2003	$\leq$ 2008		2007		2008(v)			K.S.A. § 25-4143 – 45; K.S.A. § 25-4147 - 48 (amended 2008, 2009, 2011); K.S.A. § 25-4150; K.S.A. § 25-4153 - 54; K.S.A. § 25-4156 - 57; K.S.A. § 25-4167 – 68; K.S.A. § 25-4173

State	Group	Web. nav. added	Contributions searchable by donor's... Name	Geography	Employer	Amount	Download data	Electr. filing	Data price ↓	↓ report threshold	Current statutes
Minnesota	Treatment	2005, 2007	$\leq$ 2008	2007	2007	2003	2008	2003(v)			M.S.A. § 10A.01 (amended 2008, 2010, 2013, 2014); M.S.A. § 10A.02; M.S.A. § 10A.20 (amended 2010, 2013, 2014); M.S.A. § 10A.14 (amended 2008, 2010, 2013)
North Carolina	Treatment	2004, 2005, 2008					2004	2003(v) legis. 2003(m) state $5,000+		2006 from $100 to $50	N.C.G.S.A. § 163-278
New Jersey	Treatment	2005, 2007, 2008	2007	2007	2007	2007	2005	2003(v) 2007(m) $100k+		2004 from $400 to $300	N.J.S.A. 19:44A-11; N.J.S.A. 19:44A-16; N.J.S.A. 19:44A-20; N.J.S.A. 19:44A-22.3;
New York	Treatment	2004, 2005	2003	2005		2005	2007	2003(m) $1k+ 2004(m) all			NY ELEC § 14-118; NY ELEC § 14-112; NY ELEC § 14-108; NY ELEC § 14-102; NY ELEC § 14-100; NY ELEC § 14-104; NY ELEC § 14-110; NY ELEC § 14-128; NY ELEC § 14-120; NY ELEC § 14-106; NY ELEC § 14-104
Oklahoma	Treatment	2004, 2005, 2007	2008	2008	2008	$\leq$ 2007	2008	2003(v) 2006(m) $20k+			74 Okl. St. Ann. § 4256; 74 Okl. St. Ann. § 4255; OK ST Ethics Commission 257:10-1-13; Okl. Const. Art. 29, § 3; OK ST Ethics Commission 257:10-1-12; OK ST Ethics Commission 257:1-1-2; OK ST Ethics Commission 257:10-1-11; OK ST Ethics Commission 257:10-1-19; OK ST Ethics Commission 257:10-1-18; OK ST Ethics Commission 257:10-1-15

			Contributions searchable by donor's. . .								
State	Group	Web. nav. added	Name	Geography	Employer	Amount	Download data	Electr. filing	Data price ↓	↓ report threshold	Current statutes
Oregon	Treatment	2004, 2005		2008	2008		2007	2003(m) $50k+ 2007(m) $2k+			O.R.S § 260.055; O.R.S § 260.039; O.R.S § 260.057; O.R.S § 260.005; O.R.S § 260.083; O.R.S § 260.112; O.R.S § 260.043; O.R.S § 260.046; O.R.S § 260.083
South Carolina	Treatment		2006					2006			SC ST § 8-13-1302; SC ST § 8-13-1306; SC ST § 8-13-1308; SC ST § 8-13-1310; SC ST § 8-13-1304; SC ST § 8-13-1368; SC ST § 8-13-1360; SC ST § 8-13-1324; SC ST § 8-13-1300
Virginia	Treatment		2008	2005		2008		2003(m) state 2003(v) legis			VA Code Ann. § 24.2-945.2; VA Code Ann. § 24.2-956; VA Code Ann. § 24.2-957.1; VA Code Ann. § 24.2-958.1; VA Code Ann. § 24.2-959; VA Code Ann. § 24.2-943; VA Code Ann. § 24.2-945.1; VA Code Ann. § 24.2-947.1; VA Code Ann. § 24.2-948.4
West Virginia	Treatment		2008	2008	2008			2004(v) 2007(m) state			W. Va. Code § 3-1B-2; W. Va. Code § 3-8-5; WV ST § 3-8-2; W. Va. Code § 3-8-1a

State	Group	Web. nav. added	Contributions searchable by donor's... Name	Geography	Employer	Amount	Download data	Electr. filing	Data price ↓	↓ report threshold	Current statutes
Maryland	Control	Yes (fields not specified)					2004	2003(m) $5k+			MD Code, Election Law, § 1-101; MD Code, Election Law, § 13-208; MD Code, Election Law, § 13-305; MD Code, Election Law, § 13-309; MD Code, Election Law, § 13-311; MD Code, Election Law, § 13-316; MD Code, Election Law, § 13-304; MD Code, Election Law, § 13-207; MD Code, Election Law, § 13-312; MD Code, Election Law, § 13-222; MD Code, Election Law, § 13-221
Mississippi	Control							NA			Miss. Code Ann. § 23-15-801; Miss. Code Ann. § 23-15-807; Miss. Code Ann. § 23-15-809; Miss. Code Ann. § 23-15-803; Miss. Code Ann. § 23-15-805; Miss. Code Ann. § 23-15-817; Miss. Code Ann. § 23-15-813; Miss. Code Ann. § 23-15-811
North Dakota	Control		2004	2004			2004	NA			NDCC,16.1-08.1-02; NDCC, 16.1-08.1-04; NDCC, 16.1-08.1-01; NDCC, 16.1-08.1-03.3; NDCC, 16.1-08.1-06; NDCC, 16.1-08.1-05; NDCC, 16.1-08.1-07
Nebraska	Control	2007	2005					NA			Neb. Rev. St. § 49-1410; Neb. Rev. St. § 49-1445; Neb. Rev. St. § 49-1449; Neb. Rev. St. § 49-1454; Neb. Rev. St. § 49-1453; Neb. Rev. St. § 49-1470; Neb. Rev. St. 49-1450; Neb. Rev. St. § 49-1462; Neb. Rev. St. § 49-1459; Neb. Rev. St. § 49-1456; Neb. Rev. St. § 49-1478.01; Neb. Rev. St. § 49-1472
New Mexico	Control		2008					2003(v) 2006(m)	2004 copies to 10¢		N. M. S. A. 1978, § 1-19-26; N. M. S. A. 1978, § 1-19-33; N. M. S. A. 1978, § 1-19-26.1; N. M. S. A. 1978, § 1-19-29; N. M. S. A. 1978, § 1-19-27; N. M. S. A. 1978, § 1-19-31

State	Group	Web. nav. added	Contributions searchable by donor's... Name	Geography	Employer	Amount	Download data	Electr. filing	Data price ↓	↓ report threshold	Current statutes
Nevada	Control	2005, 2008						(v)	2005 copies from \$1 to 50¢		N.R.S. 294A.120 (amended 2011, 2013); N.R.S. 294A.380; N.R.S. 294A.400 (amended 2011, 2013); N.R.S. 294A.420 (amended 2011, 2013); N.R.S. 294A.341; N.R.S. 294A.190
South Dakota	Control							NA			SDCL § 12-27-24; SDCL § 12-27-11; SDCL § 12-27-1; SDCL § 12-27-3 (amended 2010, 2012); SDCL § 12-27-22 (amended 2008, 2010); SDCL § 12-27-25 (amended 2008); SDCL § 12-27-29; SDCL § 12-27-6; SDCL § 12-27-28 (amended 2008); SDCL § 12-27-16 (amended 2010); SDCL § 12-27-15; SDCL § 12-27-32; SDCL § 12-27-42 (amended 2008)
Vermont	Control	2005, 2008						NA			All repealed: 17 V.S.A. §§ 2801 – 03; 17 V.S.A. §§ 2805 – 06; 17 V.S.A. § 2810 - 11; 17 V.S.A. § 2882 - 83; 17 V.S.A. §§ 2892 – 93
Wyoming	Control	2007						NA			W.S.1977 § 22-25-101; W.S.1977 § 22-25-102 (amended 2009, 2011, 2015); W.S.1977 § 22-25-106 (amended 2011); W.S.1977 § 22-25-107 (amended 2008, 2013); W.S.1977 § 22-25-108 (amended 2015); W.S.1977 § 22-25-110 (amended 2011); W.S.1977 § 22-25-112

Table D1: Website and data availability data from the Campaign Disclosure Project; legal citations from Westlaw. Years given are the years that the Project reports improvements made. Many columns are self-explanatory, but not all. "Web nav. added" is the years in which the Campaign Disclosure Project mentioned that the website had enhanced navigability. Contributions searchable by "geography" are searchable by zip code or address. The year that data is first made downloadable on the website is in the "Download data" column. For "Electr. filing", (m) indicates mandatory electronic filing; (v) indicates voluntary electronic filing. Some states only included data filed electronically in searchable databases. Others included scanned, handwritten filings as "electronic" filings, which greatly reduces searchability. "Data price ↓" captures the year in which the price of data (usually on paper or via CD) is reduced. "↓ report threshold" describes the year and amount of any reduction in the threshold for reporting a contribution.