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

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change their disclosure laws or practices over the same time period – estimates are indistinguishable from zero and confidence intervals are narrow around zero. Moreover, we do not observe heterogeneous effects for small donors or ideological outliers, despite an assumption in First Amendment jurisprudence that these donors are disproportionately affected by campaign finance regulation. In short, the argument that disclosure aggressively chills speech is not supported by our data.

September 15, 2015

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Abstract

In recent years, the courts have deregulated many areas of campaign finance while simultaneously upholding campaign finance disclosure requirements. Opponents of disclosure claim that it chills speech and deters political participation. We leverage state contribution data and find that the speech-chilling effects of disclosure are negligible. On average, donors to state-level campaigns are no less likely to contribute in subsequent elections in states that increase the public visibility of campaign contributions, relative to donors in states that do not change their disclosure laws or practices over the same time period – estimates are indistinguishable from zero and confidence intervals are narrow around zero. Moreover, we do not observe heterogeneous effects for small donors or ideological outliers, despite an assumption in First Amendment jurisprudence that these donors are disproportionately affected by campaign finance regulation. In short, the argument that disclosure aggressively chills speech is not supported by our data.

Keywords: Campaign finance, disclosure, transparency, state politics.

JEL Codes: C12, D72, D73, K39



As a result of the wave of anticorruption reforms in the 1970s, and technological advancements in the 2000s, governments in the United States make many proceedings, decisions, and information easily accessible to anyone who is interested. As government transparency has increased, mandatory disclosures in many areas have also increased (Ben-Shahar and Schneider, 2014). Political campaigns are no exception. States and the federal government mandate that candidates disclose both personal financial information and the sources of their campaign finances (Briffault, 2010; Corrado, 1997). Opponents decry disclosure's chilling of political speech. Proponents posit that disclosure's benefits outweigh its costs, such that any losses of individual privacy for a contributor or additional reporting costs incurred by candidates as a result of enhanced information disclosure are more than offset by the public's gain in information and corruption deterrence. Courts have usually agreed with the proponents, though data quantifying the costs and benefits of disclosure have emerged more slowly than court opinions upholding disclosure.

Although the courts have grown skeptical of campaign finance limits in recent years, judges almost always uphold disclosure requirements. For example, in *McCConnell v. FEC*, three Justices who disagreed with the Court's opinion on certain regulations of soft money nonetheless voted to uphold disclosure and disclaimer requirements. 540 U.S. 93 at 321 (2003). In *Citizens United*, 558 U.S. 310 (2010), the Court invalidated a federal ban on independent expenditures from a corporation's general treasury by a 5-4 vote, yet agreed 8-1 that disclosure requirements for entities who fund independent electioneering communication are constitutionally valid

under the First Amendment.¹ Even more recently, lower courts have upheld various disclosure laws and practices in state and federal elections, e.g. *Van Hollen v. FEC*, 2014 WL 6657240 (D.D.C. 2014); *Justice v. Hoseman*, 771 F.3d 285 (5th Cir. 2014); *Committee for Justice and Fairness v. Arizona*, 235 Ariz. 347 (Ariz. Ct. App. 2014); *Protect Marriage.com-Yes on 8 v. Bowen*, 752 F.3d 827 (9th Cir. 2014); *Center for Individual Freedom v. Tennant*, 706 F.3d 270 (4th Cir. 2013); *Free Speech v. FEC*, 720 F.3d 788 (10th Cir. 2013); *Center for Individual Freedom v. Madigan*, 697 F.3d 464 (7th Cir. 2012); *The Real Truth About Abortion v. FEC*, 681 F.3d 544 (4th Cir. 2012). This endorsement of disclosure by the courts has prompted interest groups, commissions, academics, and legislators to respond to the deregulation of federal campaign finance rules with calls for stricter disclosure laws ([Hasen, 2012](#); [Briffault, 2012, 2010](#)).²

Despite a willingness to uphold disclosure laws as “a less restrictive alternative to more comprehensive regulations of speech,” *Citizens United*, 558 U.S. at 369, the Supreme Court has expressed a serious concern about laws that have the effect of chilling speech, particularly political speech. In the Court’s view, disclosure generally does not chill speech in a way that violates the First Amendment. As long

¹As in *McConnell*, only Justice Thomas dissented on the point of disclosure, arguing that the First Amendment protects anonymous free speech and that disclosure might lead to retaliation by one’s political nemeses.

²The federal disclosure law proposed in the immediate wake of *Citizens United*, the DISCLOSE Act, aimed at disclosure of independent expenditures, defined as spending that “expressly advocat[es] the election or defeat of a clearly identified candidate that is not made in cooperation, consultation, or concert with, or at the request or suggestion of, a candidate, a candidate’s authorized committee, or their agents, or a political party or its agents.” 11 CFR 100.16(a). We note the important practical and jurisprudential distinction between independent expenditures and direct contributions to candidates. *Citizens United* invalidated a ban on corporate independent expenditures, but it had no effect on the regulation of direct contributions. Indeed, the federal ban on corporate contributions to candidates persists today. As a matter of law, courts apply heightened scrutiny to laws that regulate independent expenditures, but are more lenient with respect to the regulation of direct contributions to candidates. Nevertheless, regulations of both types of political spending have fared poorly in the federal courts recently. The data and analysis that we present is limited to direct contributions to candidates.

as disclosure has a “substantial relation” to a “sufficiently important government interest” it does not abridge the freedom of speech.³ Nevertheless, some argue that disclosure *can* chill political speech (McGeeveran, 2003; Gilbert, 2012, 2013) and others believe it *does* (Samples, 2010; Wang, 2013). Conservative groups have objected to enhanced campaign finance disclosure by arguing that it increases the risk of harassment against those who donate to controversial candidates or causes (Center for Competitive Politics, 2013).⁴ The result, they argue, is that “the amount of people choosing to forgo their rights to add political speech to the marketplace of ideas will shrink, as businesspeople will be forced to choose between their livelihoods and their right to express themselves” (Cantora, 2011). The American Civil Liberties Union is also uncomfortable with campaign finance disclosure, arguing that “the system is not strengthened by chilling free speech and invading the privacy of modest donors to controversial causes.” (ACLU, 2010). Historically, the Supreme Court has been sympathetic to *demonstrated* claims of harassment, or fear of harassment, by donors to controversial candidates and causes. Where disclosure of contributions to political parties will subject contributors to the “reasonable probability” of “threats, harassment, or reprisals”, the First Amendment prohibits the government from compelling disclosures. *Brown v. Socialist Workers Comm.*, 459 U.S. 87, 88 (1982) (quoting *Buckley v. Valeo*). See also *NAACP v. Alabama ex. rel. Patterson*, 357 U.S. 449 (1958) (holding that compelled disclosure of NAACP membership lists would have a repressive effect on the right to associate because of likely harassment against list

³*Citizens United*, 558 U.S. 310, 366-67 (2010) (quoting *Buckley v. Valeo*, 424 U.S. 1, 64 (1976)).

⁴Some legal scholars and political scientists think this claim is overblown. See Hasen (2012), Scholars (2010).

members).

This paper is the first to use contribution data to quantify the impact of disclosure on political participation, and among the first to link empirics to the jurisprudence on campaign finance disclosure. We analyze elections at the state level where variation in disclosure rules and practices over time provide a natural setting to test our hypotheses.⁵ We find that contributors to state-level campaigns opt out to a very small degree in the wake of increased contribution visibility. On average, contributors are about 2-5 percentage points less likely to contribute in subsequent elections in states that increase the public visibility of campaign contributions, relative to contributors in states that do not change their disclosure laws or practices over the same time period, though the estimates are indistinguishable from zero. Perhaps more importantly, contribution size and donor ideology have a minimal impact on willingness to contribute when the contribution is easily discoverable, though our estimates of the differences once we break the data down into ideological cohorts are less precise.

Our paper proceeds in four parts. We first discuss the costs and benefits of campaign finance disclosure. We then describe our data and analyze our hypotheses. Next, we present our findings. We conclude by linking our empirics to the campaign finance jurisprudence and discussing the implications of our findings on the constitutionality of disclosure laws.

⁵Our paper joins only a few existing empirical analyses of state electoral institutions. For any subject in public law the general trend has been to focus on the federal system. [Primo and Milyo \(2006\)](#) is a rare exception though their focus is on public opinion about government performance, rather than political participation. Recent books cover several aspects of state-level electoral institutions, but they leave disclosure laws unexamined ([Cain, Donovan and Tolbert, 2008](#); [Powell, 2012](#)).

The Benefits and Costs of Disclosure

Participation in campaign finance is an individual-level economic decision, which, in theory, is no different than other decisions we make with our money ([Ansolabehere, De Figueiredo and Snyder, 2003](#); [Gerber and Lupia, 1995](#)). Campaign contributions are thus a function of expected costs and expected benefits of the act. The benefits in this realm are diffuse and accrue to the public, while the costs are more personal to the individual and candidate.

While the case for disclosure is “almost certainly overstated” ([Briffault, 2003](#)), its potential advantages have allure. The two main governmental interests contemplated by *Buckley* and its progeny, disseminating information and combating corruption, are considered to accrue to society at large. In the words of the Supreme Court,

disclosure provides the electorate with information ‘as to where political campaign money comes from and how it is spent by the candidate’ in order to aid the voters in evaluating those who seek federal office. It allows voters to place each candidate in the political spectrum more precisely than is often possible solely on the basis of party labels and campaign speeches. The sources of a candidate’s financial support also alert the voter to the interests to which a candidate is most likely to be responsive, and thus facilitate predictions of future performance in office. . . disclosure requirements deter actual corruption and avoid the appearance of corruption by exposing large contributions and expenditures to the light of publicity. This exposure may discourage those who would use money for improper purposes either before or after the election. *Buckley*, 424 U.S. at 66-67.

Voters might use campaign finance information as a heuristic, an informational shortcut that allows low-information voters to vote as if they had more “encyclopedic” knowledge ([Lupia, 1994](#)). Quantifying the information benefit is difficult

([Fortier and Malbin, 2013](#); [Carpenter, 2009](#)), though a few scholars are making headway. Experimental evidence shows that voters punish anonymity, revealing a demand for disclosure and that disclosure of contributors after an attack ad does inform voters, because it neutralizes the effects of the attack ([Dowling and Wichowsky 2015, 2013](#), cf. [Primo 2013](#)). Several authors note that information about large contributions is likely a better heuristic than information about smaller contributions ([Briffault, 2010](#); [La Raja, 2007](#); [Fung, Graham and Weil, 2007](#)) in part because small-time lack informational and anticorruption value—small donors are not considered beneficiaries of *quid pro quo* corruption ([Briffault, 2012](#); [Hasen, 2010](#)). Moreover, the information about large contributions can get lost in a deluge of data generated by small contributions ([Sullivan, 1998](#)), though though the presence of publicly-available filtering and searching functions in campaign finance databases likely mitigates this problem. Finally, the anti-corruption benefit might not matter much because donors give only a small fraction of their income to politics suggesting that contributions are really just a form of political participation rather than an investment in policy outcomes ([Ansolabehere, 2007](#)).

Disclosure also enables contributors to credibly signal their alignment with a candidate or platform ([Gilbert, 2013](#)), particularly when evidence of the contribution can later be used to gain access to the elected official ([Kalla and Broockman, 2014](#)). The availability of the signaling benefit varies with the strength of the disclosure regime, though candidates can always know who their contributors are,⁶ regardless

⁶[Ackerman and Ayres \(2002\)](#) have proposed to anonymize political donations. In theory, anonymous contributions would render moot the informational asymmetry between candidates and the public and also combat *quid pro quo* corruption as officeholders would not know who supported

of whether they must disclose information about their contributors to the public.⁷

The costs of disclosure can be large and can vary with the strength of the disclosure regime. In particular, disclosure imposes privacy costs on individual contributors (La Raja, 2014).⁸ They might worry that exposure could hurt their business, that the information collected could result in junk mail, or that they might be harassed for their political opinions (McGeveran, 2003; Mayer, 2010). Candidates are reluctant to remind contributors that their information will be disclosed (Carpenter et al., 2014), potentially because they estimate contributor privacy concerns to be large. Furthermore, candidates face fixed administrative costs of reporting contributions, and as reporting thresholds are lowered, the number of contributions subject to reporting increases.

In sum, the traditional view of campaign finance disclosure pits specific, individual-level burdens against diffuse public benefits. From the perspective of an individual, the benefits of disclosure are relatively fixed regardless of the disclosure regime, whereas the costs of disclosure vary depending on the specific rules governing the contribution itself.

We note that the contributor herself does not need to be aware of the disclosure rules governing the candidate she supports. In places with strong disclosure rules

them and thus would not be able to reward large donors.

⁷Candidates might also benefit from disclosure, insofar as they might like to publicize the composition of their donor pools – for populist claims – or use the deluge of information to hide less desirable contributions. But voluntary disclosure is always possible for candidates – indeed, around 17% of our data involves disclosure of contributions below the legal thresholds. So a legal change should not prevent candidates from disclosing, if they think this benefit outweighs the costs described below.

⁸The public faces costs, too. Ramping up disclosure requires fiscal expenditures to support data conversion and storage, which is likely a one-time cost that becomes negligible over time. However, public costs cannot explain behavioral impacts of disclosure laws.

and good data accessibility, all she has to do is happen upon the information online, perhaps on her newspaper's website. Or she might find it incidentally, in the course of looking up an employer, a name, or street, all of which are linked to databases of campaign contributions in some states. When Proposition 8, the referendum opposing same sex marriage in California was pending, enterprising activists searched contribution data for those who supported Proposition 8 and linked it to the addresses of the donors, producing a geo-tagged and interactive map that circulated online and eventually was the source of harassment for some Proposition 8 supporters. California's disclosure requirements made the map possible. One did not have to be aware of the laws and regulations themselves to know that contributions are highly visible in California. From this logic we develop our first hypothesis.

Hypothesis 1: *Disclosure Causes Donors to Stop Contributing*

Ceteris paribus, past contributors will be less likely to make future contributions in states that increase the visibility of contributions relative to states that do not increase the visibility of contributions over the same time period.

Asymmetries of Benefits and Costs

As the Supreme Court conceded in *Buckley*, "It is undoubtedly true that public disclosure of contributions to candidates and political parties will deter *some* individuals who otherwise might contribute" 424 U.S. at 58 (emphasis added). In other words, the costs and benefits of disclosure may not fall on all actors equally because *some* contributors likely have a different elasticity of demand for participation.

Thus, strong disclosure laws are likely to decrease campaign giving among those who would otherwise contribute were it not for the increased cost of participating

brought about by disclosure. In *Buckley* the Court hypothesized that smaller contributors would be more elastic to disclosure rules: “[c]ontributors of relatively small amounts are likely to be especially sensitive to recording or disclosure of their political preferences. These strict requirements may well discourage participation by some citizens in the political process, a result that Congress hardly could have intended.” 424 U.S. at 83. This dovetails with the ACLU’s concern about “modest” contributors being edged out by disclosure laws.

There are two mechanisms for disclosure laws to create this outcome: one for candidates and one for contributors. First, the administrative cost to candidates can be so burdensome that they forgo the solicitation of small-money donors in favor of rich donors where the net gain – money received minus the administrative costs related to disclosure – is highest. Second, the privacy costs to smaller contributors might cause them to opt out of giving above the disclosure threshold or from giving altogether. In either case, smaller contributors are more likely to drop out of the donor pool as disclosure rules are strengthened.⁹

Hypothesis 2: *Small Contributors Will Drop Out of Donor Pool at a Higher Rate Compared to donors in states that do not strengthen disclosure of campaign contributions, repeat contributions will decrease among small contributors in states that strengthen their disclosure laws.*

Would-be contributors who opt out might do so for at least two reasons related to privacy. The first reason would be to avoid unwanted email or physical mail. The second reason, as explained by [La Raja \(2014\)](#) is related to homophily ([Mutz,](#)

⁹We operationalize our second hypothesis by identifying individuals who make smaller contributions in successive elections; comparing the percentage of donors during election cycle EC_{t-1} who give again in election cycle EC_t . Equally important is the effect of disclosure on *potential* small contributors who decide not to give in the wake of disclosure rules, regardless of past behavior. By definition, these individuals are unobserved and our findings do not speak to their behavior directly.

2002). Individuals may fear that disclosure will expose their political allegiances to neighbors, colleagues, and friends (McClurg, 2006). This fear is likely greater where individuals are ideologically dissimilar from their neighbors and friends, as La Raja showed in a recent survey experiment. The ACLU's concern about deterring contributions to controversial candidates, and conservatives' concerns about businesses being hurt by revelation of political activities, both speak to homophily and privacy concerns.

Hypothesis 3: *Ideological Outliers Will Drop Out of Donor Pool at a Higher Rate*
In states that strengthen disclosure of campaign contributions, contributors who are ideologically distant from their neighbors will opt out of the donor pool at a higher rate than contributors in states that do not change their laws.

Empirical Analysis of State Campaign Finance Disclosure

We test our three hypotheses by analyzing individual contributions to state gubernatorial and legislative campaigns. Over the past ten years, some states increased the strength of their disclosure requirements and some states did not. We use previously-published state disclosure scores as a proxy for strong versus weak disclosure regimes, which enables us to compare contributions in states that moved to a strong regime to contributions in states that remained in a weak regime, over time. We also compare the political ideology of individual donors to the ideology of other contributors from the same zip code.

Data

We focus our analysis on state elections between 2000 and 2008. In particular, we compare the pool of campaign contributors in states that strengthened their disclosure rules and practices between 2004 and 2008 (our “treatment” states), to the pool of campaign contributors in states that did not change their disclosure rules and practices (our “control” states). We limit our analysis to states that improved their disclosure regime substantially between 2004 and 2008, and states that failed to improve over the time period, retaining their weak disclosure regimes throughout. There are fourteen “treatment” states and nine “control” states. See Figure 1. By construction, no treatment states had very high disclosure scores at the beginning of the time period or very low disclosure scores at the end of the time period.

We rely on state scores produced by the Campaign Disclosure Project (CDP), an annual report authored by subject-area experts from both academia and law.¹⁰ The CDP evaluated every state’s campaign finance laws and data accessibility for the years 2003-2005 and 2007-2008. Many disclosure improvements highlighted by the CDP involved access to information, and many of the changes were not accomplished via new legislation or administrative rules but instead by simple improvements to data accessibility, such as improving the user interface for state websites and making campaign contribution data searchable and downloadable. Some states enabled searching by name, geographical location (address, zip code, etc.), or em-

¹⁰Our use of expert-informed data follows a tradition of using similar data in both political science and economics. See, e.g., [Clinton and Lewis \(2008\)](#) and [Fisman and Miguel \(2007\)](#). More fine-grained data would allow us to make more precise claims about which aspects of disclosure impose the most costs, as we discuss in the online appendix.

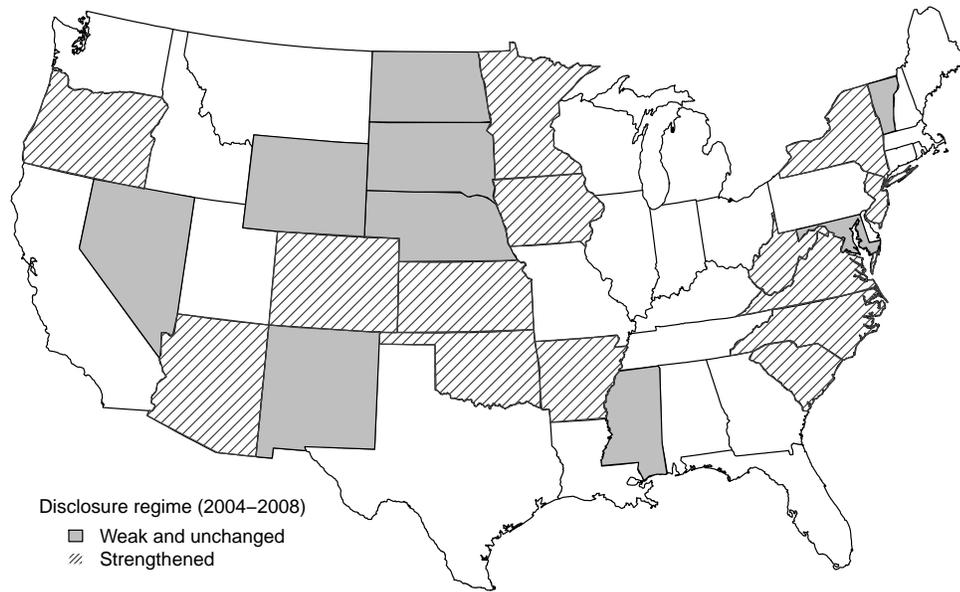


Figure 1: States with no change comprise our control group (n=9). States that strengthened their disclosure rules and practices comprise our treatment group (n=14). Note that Kansas and Vermont are dropped for the ideology analysis for lack of zip code data.

ployer (usually for larger amounts). Others mandated electronic filing between 2004 and 2008, which greatly improved the searchability of data. Reporting thresholds were unchanged in all treatment states except North Carolina, which slightly decreased its threshold, from \$100 to \$50, and New Jersey, which decreased from \$400 to \$300. We include a table of changes in treatment and control states from 2003 - 2008, as well as more information about the CDP’s methodology, in the appendix.¹¹

Our data on campaign contributions and political ideology are drawn from the Database on Ideology, Money in Politics, and Elections (DIME) (Bonica, 2013) which

¹¹The CDP grades states on four dimensions: (1) de jure language; (2) electronic filing; (3) content accessibility; and (4) user-friendliness. Each dimension comprises a handful of measures, which are further sub-divided into dozens of sub-measures, coded by campaign finance experts. Our identification of treatment and control states is based on the aggregate score of all measures in all four dimensions for each state. Aspects of these (largely technologically-driven) changes appear in each dimension, so we also cannot isolate the discrete impact of small changes. We describe this limitation in more detail in the online appendix.

includes contributions data from the National Institute on Money in State Politics.¹² NIMSP collects the contributor's zip code, recipient's name, recipient's state, recipient's party, target seat, amount contributed, and the date of the contribution.

Among the 23 states in our sample, more than one million individuals contributed half a billion dollars to 15,995 candidates for 5,553 contested seats for statewide office in 2000, 2004, and 2008. We subset the data to the 200,880 individuals who made a contribution during the 2000 election cycle, and analyze the behavior of this panel in subsequent statewide elections. We track the raw amount contributed by the panel as well as a relative measure of each contributor's impact, which divides the individual's contribution(s) by the total contributions to the seat in question.¹³

We use DIME ideology scores to identify the ideology of each contributor. DIME uses common contributors to state and federal races to bridge ideology estimations across different types of races in a method that improves upon other well-known ideology estimation efforts, such as NOMINATE scores (Bonica, 2014; McCarty, Poole and Rosenthal, 2006). We use the DIME estimates to generate a measure of the absolute value of the distance between each contributor's ideology and the average ideology of his or her zip code, using the most recent pre-treatment measure of ideology.¹⁴

¹²NIMSP data can be downloaded at <http://www.transparencydata.com/bulk/>. This data is licensed under the Creative Commons Attribution-Noncommercial-Share Alike 3.0 United States License by the National Institute on Money in State Politics.

¹³For 2000 contributors who gave again in 2004, we use the maximum of their relative contributions in 2004 (if they gave to multiple seats) as a measure of their relative importance. If contributors skipped the 2004 election, we use maximum of their relative contributions for the year 2000 as their measure of relative importance in that election.

¹⁴If a contributor gave in 2000 but not 2004, we use the 2000 ideology score. If they gave in both

We note one important caveat about our process of using zip codes in the DIME data. The data contains both missing and misreported zip codes. After omitting two states that did not report zip codes in 2000, 3.7% of contributor zip codes are misreported in 2000 (6,163 of 164,278). The number drops sharply when we examine misreported zip codes among the 2000 contributors who gave in 2004 (974 of 41,374 contributors to both elections) and 2008 (502 of 30,071 repeat contributors). Despite several attempts, we have not found a way that our results are confounded by the missing data. We explain more in the online appendix.

Balance Checks

The cleanest designs for causal inference demonstrate balance between two groups and then use a simple difference in means as a causal estimate. Where groups lack balance, we are forced to rely on model specification. Here, there are small but statistically significant differences between contributors in treatment and control states. We therefore use fixed effects probability models and controls to correct for imbalance.¹⁵

Table 1 examines balance on key covariates using pre-treatment data. We assess individual level balance on (1) amount contributed in 2000 among contributors, normalized by population and the number of seats contested, (2) relative measures of amount contributed per seat and per state, and (3) contributor ideology in 2000.

2000 and 2004, we use the 2004 ideology score. For measures of ideological distance, we omit zip codes that have only one contributor, which removes 0.1% of contributors from the data.

¹⁵The primary imbalance is from the sheer size of the two groups – approximately 94% of the contributors are in treatment states. Nevertheless, the groups overlap almost completely for the key individual-level covariates, and we trim modestly (10 cases with outlier amounts) to ensure complete overlap. We also drop 326 contributors with ideologies that fall outside of the -2 to 2 interval.

We find that the differences for amounts are substantively fairly small, but they are highly statistically significant, as a result of the number of observations we have. Contributors in control states give less money per capita per seat (one cent, compared to six cents in treatment states), but the relative importance of each contributor is higher in control states than treatment states, whether measured relative to other contributors to the race, or relative to all contributors in the state.

One area of possibly important substantive imbalance lies in ideology. The range of political ideology in both groups of states is similar, though contributors in control states are more conservative. The differences are reflected in the interquartile ranges. The interquartile range of contributor ideology is -0.44 to 0.79 for treatment states and -0.09 to 1.12 in control states. Contributors in treatment states have a mean ideology score of 0.12 (st.dev 0.77), and candidates in control states average 0.49 (st.dev 0.86).

The mean differences are not large, but the distribution of ideologies are clearly not equivalent. We therefore control for ideology in some specifications below.

Ideally, states in the two groups are balanced on the features that affect a state's propensity to enter into "treatment" (i.e., to improve disclosure) before it occurs and similar in characteristics that could explain away any effect we observe *after* treatment. Political competition might affect a state's probability of strengthening campaign finance disclosure requirements, as incumbents attempt to erect barriers to entry for challengers. The way we would normally expect to observe these changes is through legislation, but many of the disclosure changes that we observe here seem to have been generated in the state bureaucracy (some perhaps by moti-

| Characteristic | Treatment | Control | p-value (t-test) |
|---|----------------|----------------|------------------|
| <i>Individual-level characteristics</i> | | | |
| Amount per capita per seat in 2000 state elections | \$0.06 | \$0.01 | 0 |
| Relative amount contributed per seat in prior election | 0.008 | 0.05 | 0 |
| Relative amount contributed per state in prior election | 0.00008 | 0.0007 | 0 |
| Average contributor ideology score, 2000 (Std. dev.) | 0.12 (0.77) | 0.49 (0.86) | 0.00 – |
| <i>State-level characteristics</i> | | | |
| Share of state population contributing to same-state candidates in 2000 | 0.008 | 0.0008 | 0.03 |
| Voter turnout 2004 | 0.63 | 0.64 | 0.65 |
| Presence of divided government, 1994-2004 | 0.66 | 0.51 | 0.02 |
| Size of legislative min. party under divided government, 1994-2004 | 0.40 | 0.38 | 0.3 |
| Average number of seats up for election | 115.6 | 60.0 | 0.02 |
| Numeric Disclosure Scores in pre-period | 1.8 | 0.8 | 0.19 |

Table 1: Balance between treatment and control states on measures of political competition, per capita political spending, and average ideology of contributor.

vated bureaucrats with web-design skills). So the route from political competition to enhanced disclosure is indirect here, and imbalance between groups might not doom our inference. Nevertheless, we examine balance on various measures of political competition, including the presence or absence of divided government, the size of the minority party, and the number of seats being challenged in an election.

We construct our measures of political competition using data from 1994-2004. As the table indicates, the two groups of states are not balanced on all of the characteristics. There are more seats up for election in treatment states than in control states, for example. In both groups of states, less than 1% of the population con-

tributes to state campaigns for election.¹⁶ However, in treatment states, the share is ten times greater than the share in control states. Voter turnout is similar across treatment and control states. States in both groups are under divided government for similar amounts of time leading up to the period covered by our data. The legislative minority party under divided government holds a similar percentage of seats in both groups of states, though, depending on supermajority requirements, minority parties could be slightly stronger in treatment states over the time period.

Finally, we include the disclosure scores for the pre-period as a balance check. Before 2004, the two groups were fairly similar in average disclosure scores, with treatment states averaging 1.8 and control states just below 1. The difference between the two groups is just over half of a standard deviation (*st.dev* = 1.8), but substantively, it is not a big difference.

Because we are studying effects that occur during the same time that same sex marriage was appearing on state ballots, we also investigated to see whether treatment states were more likely to have same sex marriage on ballots in 2004 or 2008. They were. Gay marriage was on the ballot in Arizona and Arkansas, both treatment states, and two states that were not in our sample (California and Florida). We do not measure contributions for ballot initiatives. Nevertheless, there is a chance that contributors in Arizona and Arkansas were either more (policy-oriented) or less (privacy-concerned) likely to support candidates with strong stances on gay marriage in the post-period, which our data could pick up. Arizona and Arkansas

¹⁶We normalize the number of contributors by population because the range is so large, from 1,028 in North Dakota to 65,493 in New York.

comprise 10% of the treatment state contributors in the year 2000 and 7% of the repeat contributors in 2008, but the attrition in those states (85% in Arkansas and 90% in Arizona) was equal to or higher than the average attrition across treatment states (85% for all states, 84% not including Arizona and Arkansas). Therefore, any imbalance introduced by gay marriage initiatives is marginal and biased toward a larger effect.

The balance statistics provide some foundation for our inferences about the effect of disclosure. While most differences between treatment and control states are substantively rather small, they are almost all statistically significant. Our use of individual-level controls for ideology and amount, combined with techniques that account for the clustered nature of the data and fact that treatment occurs at the state level, strengthen the validity of our findings.¹⁷

Methods and Findings

In this section, we describe our methods and present our findings. We search average effects first, then we turn to heterogeneous treatment effects among small contributors and ideological outliers. We leverage the difference in state disclosure regimes over time to create a difference-in-differences design. Because we have 23 states in the sample, we estimate the difference-in-differences using a state fixed effect. We run 1000 replications of each estimate, bootstrapping data at the state level,

¹⁷Following [Rosenbaum \(1999\)](#) we note that in an observational study of this nature, generalizability is a secondary concern to making the cleanest possible causal inference, though with 46% of the states in the country under analysis, we think generalizability is fairly strong here.

and reporting 90% confidence intervals on the median estimate from each of the 1000 resamplings. This design choice takes the clustered nature of the data at the state level seriously, while simultaneously allowing us to randomize which states are included (and excluded, by use of the treatment dummy) from the analysis. We generally find that not only are estimates of average effects quite small, but we can usually rule out non-negligible effects because the lower bound of our confidence interval is still close to zero (Rainey, 2014).

Average Effects

We first examine whether contributors are deterred by disclosure, on average. We subset the data to the people in the sample who contributed to state candidates in the year 2000. We then analyze whether they continued to contribute in 2004, when no states in the sample had big changes in disclosure, and 2008, by which time the treatment states had improved disclosure. The unit of analysis is contributor-cycle for 2004 and 2008 cycles. If contributors are deterred by enhanced disclosure, then we should observe a divergence in the probability of repeating after the treatment states make contributions more visible.

We restrict the populations to areas of overlap, cutting 10 cases of people who gave less than \$4 or more than \$688,615.¹⁸

¹⁸We also restrict the data to people with ideology scores between -2 and 2, which trims the data by 326 contributors.

We run the following linear probability model on individual level data:

$$Y = \alpha + \beta_1 T + \beta_2 P + \beta_3 (T * P) + \gamma_1 C + \gamma_2 F$$

where Y indicates (0, 1) whether the contributor gave to a state candidate in 2004 (or 2008), T indicates whether the contributor is in a treatment state, P indicates the “post” period, or 2008, C is a vector of individual-level controls for amount given in the pre-period and pre-period ideology, and F is a fixed effect for every state in the sample, which is intended to control for any state-specific features that could confound our inference, particularly unobservable ones. The coefficient of interest is the difference in differences reported in β_3 , which estimates the 2008 - 2004 probability of contributing among treatment-state contributors, minus the same probability among control-state contributors. If the estimate is negative, then treatment state contributors have a lower probability of contributing in 2008 than we would expect, using the control groups’ time trend as a counterfactual.

The results of the basic difference-in-differences estimation are presented in Table 2, where Model 1 presents the basic difference-in-differences result using only state fixed effects, and Models 2 and 3 introduce controls for 2000 or 2004 observations of ideology and amount.

The coefficient of interest is the difference-in-differences, from the interaction between Treatment and Post. For all three models, the estimate is very small, either a 2 or 3 percentage point decrease in repeat contributions below the level of contributions we would have expected, taking the time trend for control states as our

| | Model 1 | Model 2 | Model 3 |
|-----------------------|---------------------------------------|---------------------------------------|--|
| Intercept | 0.2 [0.14 , 0.32] | 0.41 [0.3 , 0.94] | -0.37 [-0.51 , -0.27] |
| Treatment | 0.02 [-0.14 , 0.13] | 0.06 [-0.47 , 0.27] | 0.03 [-0.15 , 0.22] |
| Post | -0.06 [-0.07 , -0.03] | -0.06 [-0.11 , -0.02] | -0.07 [-0.09 , -0.05] |
| Treatment Post | -0.02 [-0.04 , 0.01] | -0.02 [-0.06 , 0.03] | -0.03 [-0.04 , -0.01] |
| Ideology | | -0.03 [-0.04 , -0.01] | 0.11 [0.09 , 0.12] |
| log(Relative Amount) | | 0.04 [0.03 , 0.05] | |
| log(Amount) | | | 0.03 [-0.14 , 0.17] |
| State Fixed Effects | yes | yes | yes |

Table 2: Average effects of increased disclosure scores among contributors from the year 2000 in 14 treatment states and 9 control states. Dependent variable is whether the contributors gave again in the year 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 1000 replications (bootstrapped), to allow random exclusion of one treatment and one control state in each iteration.

counterfactual time trend. While the confidence intervals for two of the three estimates cross zero, we can rule out non-negligible negative effects by examining the lower end of the confidence intervals, which are -0.04 and -0.06. In other words, with 1000 replications of the estimate using randomly-selected treatment and control states, the estimated deterrent effect on contributors of increased disclosure is no larger than a 6 percentage point decrease below that which we would have expected in the counterfactual world in which the treatment states did not improve disclosure.

Interpretations of actual levels of giving are slightly noisy given the sensitivity

of the intercept to the small size of the control group relative to the treatment group. Nevertheless, we illustrate our expectations using the coefficients from Model 1. Interpreting the coefficients of Model 1, we observe that 22% of treatment group contributors to state campaigns in the year 2000 contributed again in 2004 ($\alpha + \beta_1$), compared to 20% of control group contributors (α). In 2008, both groups had 14% of contributors from the year 2000 repeat their participation (treatment level is $\alpha + \beta_1 + \beta_2 + \beta_3$, and control level is $\alpha + \beta_2$). The larger drop in participation among the treatment group results in the negative difference-in-differences estimate. Using the control group's time trend of a 6 percentage point decrease, we would have expected 16% of treatment group contributors to participate in 2008. We further note that the smallest estimated level of participation for the treatment group, given the lower bound of the confidence interval (-0.04), is 12%, or two percentage points less than participation by contributors in states with no changes in disclosure.

Heterogeneous Effects

The results in Table 2 show aggregate effects. But, as we discussed above, past cases indicate that a court assessing the impacts of disclosure laws would probably care much more about heterogeneous effects, or whether certain identifiable groups in the contributing population are dissuaded from contributing after their contributions become more visible. We hypothesized that people giving the largest contributions will be less elastic to strict disclosure rules than people giving smaller contributions, who will opt out when the costs of exposure exceed the benefits of

participation (Hypothesis 2). If contributing is about buying access and policy favors, then those giving less money have less of a reason to expose their small contributions, for which their expected policy and access benefits are limited. If, however, contributing, at least for small contributors, is about participation in the process and signaling your involvement to others, we might not see an increased amount of opting out among small contributors.

Figure 2 shows repeat contributions across state elections for donors that gave different amounts in 2000. The image plots the treatment and control groups separately, using results from the following regression, run on each subgroup in the data, by amount:

$$Y_{is} = \alpha + \beta_1 T_s + \beta_2 P + \beta_3 (T * P) + \gamma F_s$$

where the variables and coefficients are interpreted as described above, in the main difference-in-differences analysis. This time, we analyze by subgroup, where we define the subgroup by the amount contributed in 2000. The subgroups are chosen somewhat arbitrarily, with consideration of both the range of the contribution size and the group size.¹⁹

As Figure 2 shows, as the size of the contribution increases, the overall tendency to contribute in a subsequent election increases in both treatment and control states. Simply put, repeat players are more likely to contribute big amounts, and big contributors are likely to be repeat players. The *changes* in repeat contributors, however, tell an interesting story. The percent of repeat contributors decrease in control states

¹⁹Contribution data is clumpy, as contributors tend to give amounts in multiples of \$50. Contributors of \$100 and \$250, for example, make up almost 40% of the data.

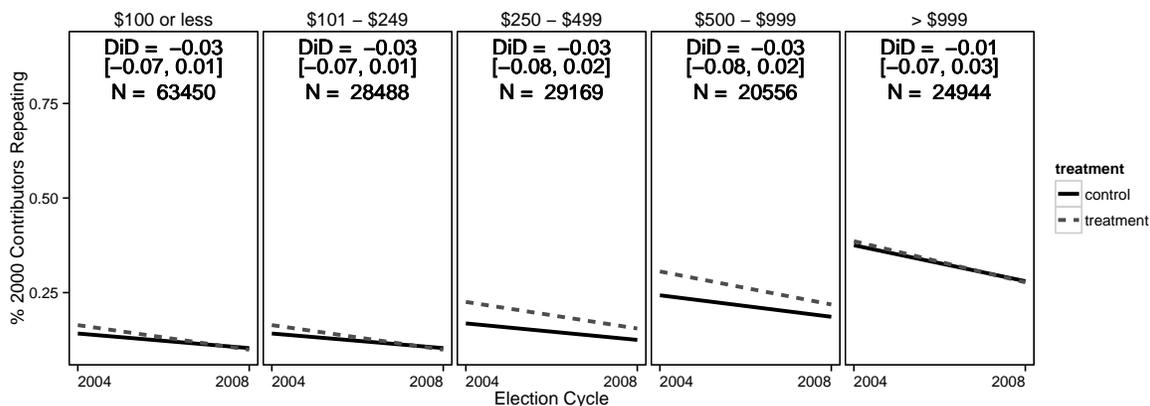


Figure 2: Repeat contributions in a given state election cycle by amount contributed in 2000 to state elections, calculated with 1000 bootstrapped difference-in-differences regressions. The repeating percentage decreases in control states (solid black line) and decreases slightly more in treatment states (dashed, medium gray line) in the wake of enhanced visibility. As above, these effects are not statistically distinguishable from zero, their magnitude is very small, and confidence intervals are similar to those in Table 2. It seems clear that small-time contributors are not disproportionately affected by enhanced disclosure.

between 2004 and 2008, which is natural, given that some attrition is to be expected in a panel study. The percent of repeat contributors in treatment states, where we expected opting out, decreases at a slightly faster rate than in control states over the same time period. The effects are not statistically distinguishable from zero, but confidence intervals are fairly narrow, at [-0.07, 0.02] for the maximum range.

We hypothesized that the cost of disclosure would be particularly burdensome for those who contribute small amounts, but we do not observe that small contributors in treatment states opt out at a higher rate than other treatment state contributors. Among those contributing \$100 or less, treatment state contributors were three percentage points less likely to contribute than they would have been in the absence of the change in disclosure visibility, using the time trend of the control states as a counterfactual. Indeed, across the entire spectrum of contributors, we see that the

usual estimate of chilling participation is only a 3 percentage point decrease below the counterfactual time trend presented by the control states. This is a negligible effect, at best.²⁰ To the extent that contributors opt out in the wake of enhanced disclosure, the magnitude of the effect is very small and does not fall more heavily on small donors.

Because of variations in cost of living across the states, as well as the different state sizes represented by the two groups, we have repeated this analysis using relative amount measures. The difference-in-difference estimates are more likely to be positive, particularly among relatively smaller contributors. The lowest low-end of a confidence interval for any panel is -0.08, though confidence intervals tend to be wider than those reported in Figure 2.²¹

The Supreme Court has disavowed an interest in leveling the playing field in terms of the amounts contributed and spent in politics. Disclosure is an area in which the Court will not observe an uneven playing field – the (already negligible) effects of disclosure do not fall more heavily on small contributors.

²⁰Because only 2 of our 14 treatment states reduced the disclosure threshold, we think that an increased administrative burden cannot explain these results, particularly since they would be expected to affect contributors at the more “modest” end of the contribution spectrum, who are already not repeating their contributions.

²¹This is probably due, in part, to the fact that there is more serious imbalance in terms of the sizes of treatment and control groups in some panels, and the way we constructed the randomization of state fixed effects, we required at least two control and two treatment states in each randomization. We failed to find at least two control states that had enough contributors to identify an effect in panels about 80% of the time, meaning that these estimates are drawn from fewer successful replications (around 200).

Heterogeneous Treatment Effects by Ideology

We also hypothesized that the privacy costs of enhanced visibility would be more salient among contributors who are ideological outliers (Hypothesis 3). In the case of ideology, the motivation to opt out is driven by the desire to avoid revealing one's politics that differ from the surrounding political culture, one's neighbors, or friends. We use a contributor's zip code to proxy local political culture and the political leanings of neighbors and friends.²² As an example, we suspected that a supporter of a socialist candidate would be more likely to opt out in the wake of disclosure enhancements if the contributor lived in a conservative zip code, like 75225 (Dallas, Texas), than if they lived in a relatively liberal zip code, like 94709 (Berkeley, California). Similarly, a contribution to a Republican candidate may be more likely to be suppressed by enhanced disclosure in 94709 than in 75225.

This measure differs from one recently used in an experimental setting ([La Raja, 2014](#)). Our measure of ideological distance is based purely on physical proximity, which can vary depending on the population of the zip code. La Raja asked respondents about whether their political views differed from people in "your family, coworkers, and neighborhood". Family could live anywhere; coworkers might or might not live in the same zip code. So La Raja's measure captures an aspect of homophily that ours does not – the subjective impression of would-be contributors.

²²Zip codes are admittedly an imperfect proxy for geography, though they do reflect several relevant characteristics of neighborhoods, such as ease of travel and the volume of mail. We note some important data-centric challenges, including the missing or incorrect zip codes described above (dropped for this analysis), and some cases (0.2% of the sample) where our subject was the sole contributor from his or her zip code (also dropped for this analysis). Future researchers might be interested to know that the DIME data provides estimated latitude and longitude coordinates for each contributor, which may provide more precise measures of geographic proximity between contributors, to the extent the coordinates are reliable.

On the other hand, our measure offers the advantage of detecting whether the direction of the ideological distance matters – whether a contributor to the right of the zip code is more likely to opt out than one to the left of the zip code.

Figure 3 plots the effects of ideological distance from one’s neighbors for contributors along the political spectrum (negative numbers are less conservative, positive numbers are more conservative), between treatment and control states. Note that values represent the ideological *distance from the mean political ideology of a contributor’s zipcode*, not the absolute ideology measure for each contributor. For example, a conservative living in a fairly conservative district would have a smaller ideological distance than a moderate conservative living in a very liberal district.²³ We also restrict the sample to conservatives who are *more conservative* than the average of their zip code, and liberals who are *more liberal* than the average of their zip code in order to preserve ordering on the x-axis.²⁴ The zero on the x-axis is the normalized mean of the ideology of the contributors for each zip code and each observation represents the location of each contributor on the x-axis relative to the mean of his or her zipcode.

Figure 3 shows a now-common theme on the effects of campaign finance disclosure. Any effects are minimal, and we can rule out non-negligible effects by examining the most extreme negative effects observed (with the lower confidence

²³Further note that we retain the sign for all contributors. Thus, we code contributors as having negative distance if they are classified as negative on the conservatism scale (meaning ideologically more liberal than the national median voter).

²⁴This restriction drops 20% of the sample. We generate the mean zip code values using the full sample, then drop those we cannot properly line up on the x-axis. If we include the full sample, the differing signs for these contributors counteracts the effects of the limited sample and the result is an artificial increase among moderates.

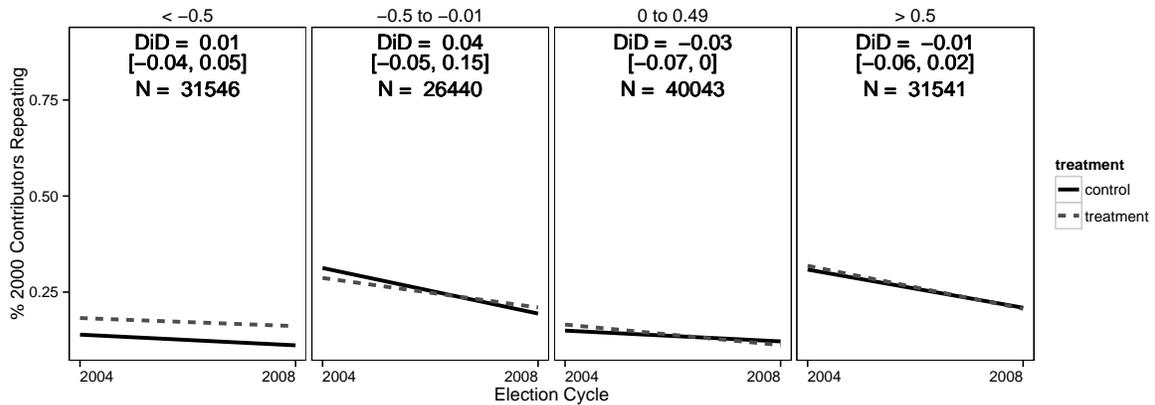


Figure 3: Repeat contributions to same-state candidates by 2000 contributors in the year 2004 and 2008, grouped by each contributor’s ideological distance from others in their zip codes. Difference-in-difference estimates with 90% confidence intervals reported. All confidence intervals cross zero, though none is less than -0.07, and ideological outliers within their zip codes are not affected any more than those who are more aligned with their neighbors.

intervals). Even when we slice the data much more thinly, and even accounting for the clustered structure of the data by bootstrapping, 95% of the replications we ran showed an effect no more negative than -0.07 (-0.06 among those more ideologically distant from their neighbors). This means that, *at most*, there is a 6 percentage point decrease in participation among people who are far to the right of their zip codes in the wake of disclosure enhancements, relative to what we would have expected, using the control group’s time trend as a counterfactual. The more typical impact (the difference in differences estimate) is a much more modest 1 percentage point decrease below the counterfactual expectation.

Figure 3 is measured based on each contributor’s ideological distance from the mean of the zip code, on the assumption that geographic proximity affects contributors’ cost-benefit analyses on whether to participate once their participation is more

visible. But a lot of the enhanced visibility from disclosure improvements is online and publicly available, regardless of the person searching it. Therefore, in Figure 4, we repeat the analysis focusing solely on the contributor's ideology, rather than a contributor's ideological distance from her neighbors. If would-be repeat contributors opt out due to concerns about exposure of their contributions *online*, then we should observe supporters of extreme candidates opting out of contributing in 2008 in treatment states more than in control states.

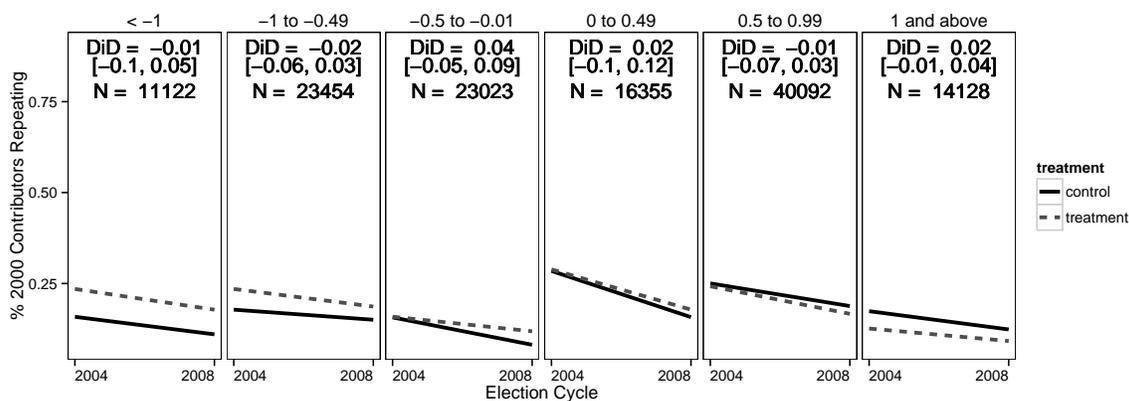


Figure 4: Repeat contributions to same-state candidates by 2000 contributors in the year 2004 and 2008, grouped by ideological ranges. Within-panel difference-in-difference estimates with 90% confidence intervals reported. All confidence intervals cross zero, though none is less than -0.1, and the average impact on ideological outliers is no greater than impacts on moderates.

In Figure 4 we see a similar pattern to those shown previously, of very small impacts, some of which are positive. However, since the range of raw ideological scores is broader than the range of ideological differences shown in Figure 3, we slice the data more thinly, reducing our statistical power. As a result, our confidence intervals are wider. Therefore, while we show that the point estimates show negligible effects, the lower-bound of the confidence intervals approach a 10 percentage

point negative impact, reducing our ability to rule out non-negligible effects here.

Interestingly, the contributor pool in treatment states becomes slightly more liberal after disclosure is strengthened, on the order of -0.08 . At the same time, ideology among control state contributors shifts to the right by 0.1 (from 0.49 to 0.59). In other words, another impact of campaign finance disclosure in treatment states is a shift in the donor pool more to the left than it otherwise would be, using control states as a counterfactual. The raw difference-in-differences is a leftward shift of -0.18 effect is around $1/4$ of a standard deviation.

Combined, Figures 2 and 4 beg the question whether there is a correlation between ideology and the amount contributed. The answer is that they are not strongly correlated. The amount per contributor (logged) and the contributor's ideological score have an overall correlation of 0.06 (0.04 among contributors in the treatment group). This means that for a one-unit increase in a contributor's conservatism score (more than one standard deviation, given the distribution of conservatism scores), a contributor is estimated to spend only 4% more in an election cycle. Since the average total amount contributed (to all recipients) per contributor in the pre period is just under $\$1300$ (median is $\$300$), this is a relatively small increase of $\$170$ in total contribution amount per contributor, in response to a shift in ideology of more than one standard deviation.

To test the robustness of our findings, we ran a placebo test on federal contributions using the same methodology described above. Because federal disclosure rules did not change more in treatment states than in control states over the time period, we expect no effect. Most estimates for heterogeneity for amount and ideology

are similar in magnitude to the findings for state contributions, though confidence intervals are much wider, in general, for estimates of the behavior of the federal contributors. Results are presented in Section C of the online appendix.

Conclusion

The Supreme Court has long supported disclosure laws on the premise that they increase information and combat corruption. Opponents of disclosure laws decry the burdens that they create for contributors. Our findings indicate that disclosure, particularly in the form of increased visibility of contributions, does not have more than a negligible deterrence effect on contributors generally, and any effect does not fall harder on smaller contributors or ideological outliers. The Supreme Court has repeatedly rejected any interest in “leveling the playing field” among political participants.²⁵ Our findings indicate that, when it comes to the effects of disclosure on giving, the playing field may not be as uneven as some fear, though we hasten to add that the rise of “dark money” (expenditures made by groups that do not disclose their donors), particularly after the years covered by our study, means that we cannot see the whole playing field.

Campaign finance disclosure do not seem to have the effect of deterring ideological outliers any more than other participants in the system. The group most likely to opt out has probably varied over time. In the 1950’s and again in the 1980’s, the

²⁵See, for example, the opinion of Chief Justice Roberts in *McCutcheon v. FEC*, 572 U.S., at ___ (slip op., at 18) (“No matter how desirable it may seem, it is not an acceptable governmental objective to “level the playing field” or to “level electoral opportunities,” or to “equalize the financial resources of candidates.” The First Amendment prohibits such legislative attempts to “fine-tune” the electoral process, not matter how well intentioned.”) (internal citations omitted).

court acted to protect more liberal activists and contributors from harassment (*see NAACP v. Alabama ex. rel. Patterson and Brown v. Socialist Workers Comm.*). In current times, conservatives who oppose gay marriage might be most worried about harassment. Nevertheless, this research indicates that conservatives, whether measured in terms of absolute ideology or ideological distance from one's neighbors, are not measurably more deterred than their liberal neighbors and compatriots – indeed, any deterrence effects are negligible, at best.

Of course, we only observe contributions that are above the disclosure threshold. It might be the case that more people are contributing below required disclosure thresholds or reallocating their money from campaign contributions to disclosure-free advocacy organizations ([Issacharoff and Karlan, 1998](#)). Furthermore, because of the bundled nature of state disclosure reforms and the coarseness and limited time series available with available disclosure data, this study cannot test the relative impacts of discrete disclosure changes. The recent move toward experimental evaluation of variations on disclosure and disclaimer regimes is useful in this regard ([La Raja, 2014](#); [Dowling and Wichowsky, 2013](#); [Ridout, Franz and Fowler, 2014](#)).

The federal trend to “deregulate and disclose” federal campaign finance has been accompanied by a similar trend in state governments across the country. The potential costs of this new degree of transparency on political behavior are understudied. We attempt to answer some of the most pressing questions in order fill the gap between campaign finance laws on the books and electoral funding in practice. We think that the criticism of disclosure – from both the right and the left – is overstated.

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Shadows of Sunlight Online Appendix

A 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'. Figure A1 shows overall state grades by year. Most states improve 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, indicated by dashed lines, monotonically increases over the time period from 1.4 (between a D- and D) to 4.7 (between a C- and C).

Using the ordinal numeric scale, we call "treatment" states any states that improved their disclosure scores by 3 or more units between 2004 and 2008, and "control" states any states that had no measured improvement in their disclosure regime over the same time period. Because most improvements pinpointed by the CDP did not, in fact, involve changes to disclosure laws (their most heavily-weighted

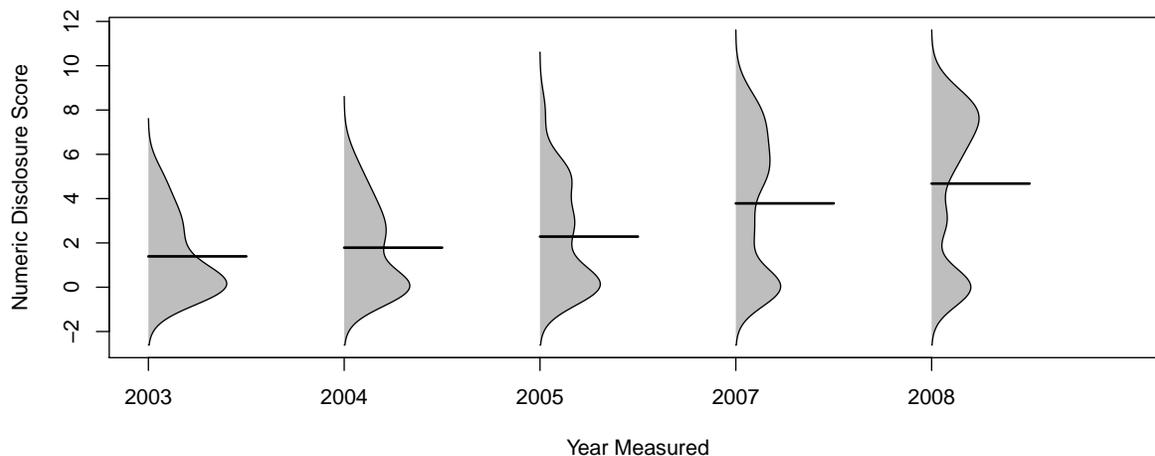


Figure A1: Disclosure scores over time for states in our sample. Scores are converted from the Campaign Disclosure Project’s letter grades, where F=0 and A=11. Mean score for each year indicated by horizontal line.

category), we followed up on each state in the sample, pinpointing the changes that were made in treatment states and verifying that no important changes were made in control states, using a combination of the CDP coders’ summaries, which captured changes in data accessibility and web navigation that are no longer observable, and our independent Westlaw searches to find legal changes over the time period. Table D shows the results of our inquiry.

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.** However, the data for the scoring components is unavailable. Table D is our attempt to 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

**See “Grading State Disclosure Criteria” at <http://www.campaigndisclosure.org/gradingstate2007/appendix3.html>.

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.

B More on 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, $p = 0.61$).^{**}

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.

^{**}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 ($p = 0.45$).

C Robustness Checks with Federal Data

In this section, we present robustness checks 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.

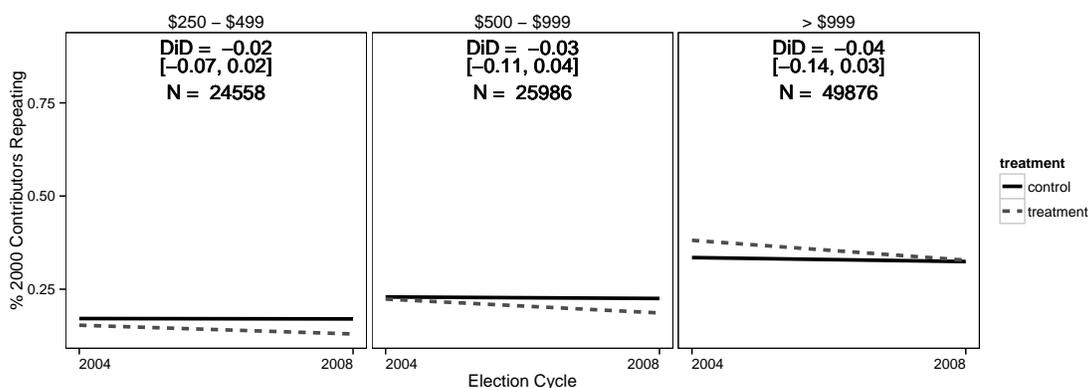


Figure C1: Repeat contributions in a given federal election cycle by amount contributed in 2000 to federal elections, calculated with 1000 bootstrapped difference-in-differences regressions. The repeating percentage decreases in control states (solid black line) and decreases slightly more in treatment 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.

| | Model 1 | Model 2 | Model 3 |
|-------------------------|------------------------------|------------------------------|--------------------------------|
| Intercept | 0.25 [0.11, 0.5] | 0.71 [0.5, 1] | -0.73 [-0.99, -0.48] |
| Treatment | 0.02 [-0.25, 0.20] | 0.05 [-0.2, 0.29] | -0.02 [-0.24, 0.16] |
| Post | -0.01 [-0.06, 0.06] | -0.01 [-0.06, 0.06] | -0.04 [-0.06, -0.01] |
| Treatment * Post | -0.04 [-0.1, 0.03] | -0.04 [-0.1, 0.03] | -0.03 [-0.04, -0.02] |
| Ideology | | -0.02 [-0.04, -0.01] | 0.16 [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 C1: 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 the year 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 1000 replications (bootstrapped), to allow random exclusion of one treatment and one control state in each iteration.

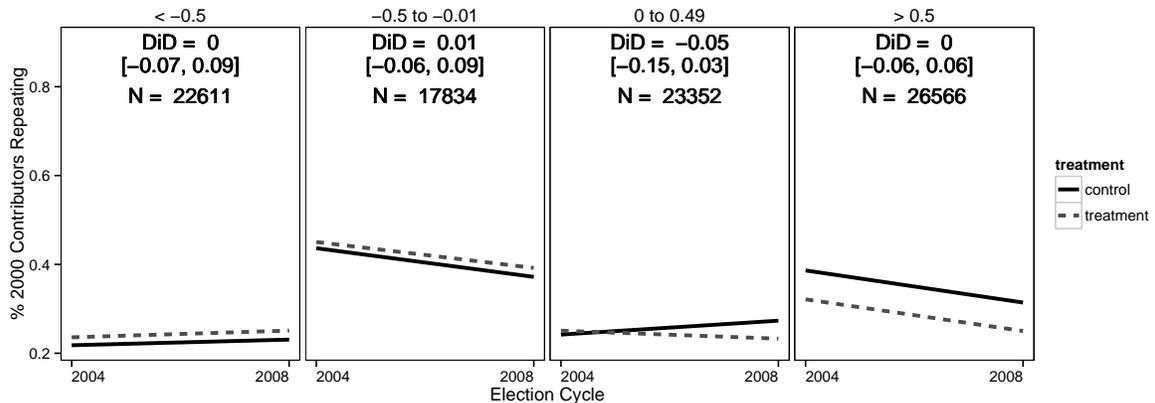


Figure C2: Repeat federal contributions to same-state candidates by 2000 contributors in the year 2004 and 2008, grouped by each contributor's ideological distance from others in their zip codes. Within-panel difference-in-difference estimates with 90% confidence intervals reported.

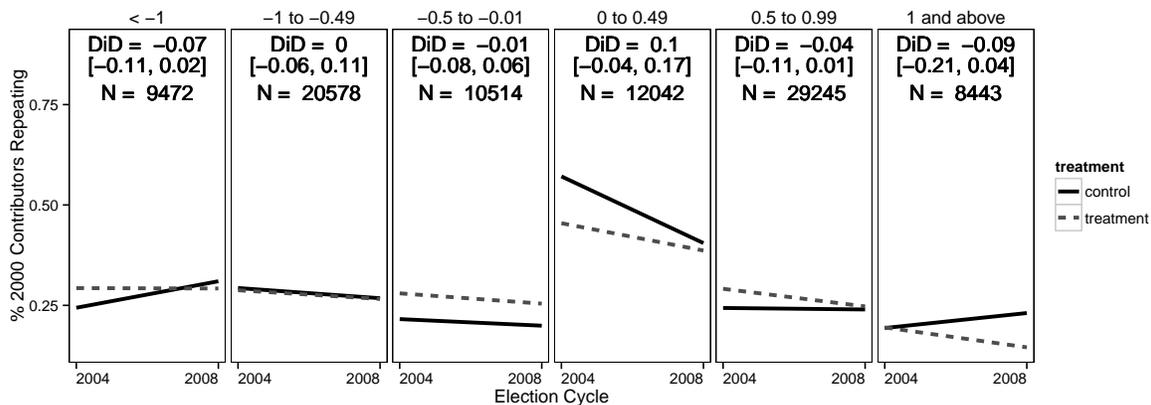


Figure C3: Repeat federal contributions to same-state candidates by 2000 contributors in the year 2004 and 2008, grouped by ideological ranges (without taking ideological distance into account). Within-panel difference-in-difference estimates with 90% confidence intervals reported.

D Legal changes in treatment states

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



Contributions searchable by donor's...

| State | Group | Web. nav. added | 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 | | | | ≤ 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 | ≤ 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 |

Contributions searchable by donor's...

| State | Group | Web. nav. added | Name | Geography | Employer | Amount | Download data | Electr. filing | Data price ↓ | ↓ report threshold | Current statutes |
|----------------|-----------|------------------|--------|-----------|----------|--------|---------------|---|--------------|--------------------------|---|
| Minnesota | Treatment | 2005, 2007 | ≤ 2008 | 2007 | 2007 | 2003 | 2008 | 2003(v) | Data price ↓ | | 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 | ≤ 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 |



Contributions searchable by donor's...

| State | Group | Web. nav. added | 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 | 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 C2: 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.