Assessing the Potential Effects of *Citizens United*: Evidence from the States

Timothy Werner
Assistant Professor
Department of Business, Government & Society
McCombs School of Business
University of Texas at Austin
1 University Station, B6000
Austin, Texas 78712
(512) 232-6844
timothy.werner@mccombs.utexas.edu

John J. Coleman
Professor
Department of Political Science
University of Wisconsin–Madison
110 North Hall
1050 Bascom Mall
Madison, Wisconsin 53706
(608) 265-3680
coleman@polisci.wisc.edu

August 2012

Draft; before citing or circulating, please contact the authors for the latest version. Prepared for the 2012 meeting of the American Political Science Association, New Orleans, La., August 30–September 2. We would like to thank Nathan J. Kelly, Jeffrey Milyo, David Primo, and Christopher Witko for generously making available their data.

Abstract

The U.S. Supreme Court's 2010 decision in *Citizens United v. FEC* was one of its most controversial in decades. Critics of the decision argued it would lead to a flood of corporate and union cash that would warp electoral and policy outcomes. We test for this possibility by exploiting the variation in campaign finance laws at the state level from 1977 through 2006. Through an analysis of three policy outcomes of interest to businesses and unions over three decades— the degree of liberalism among state elected officials; the relative state minimum wage compared to the federal minimum wage; and the degree of pre-transfer income inequality in a state—we find minimal effects for the overall campaign finance regulatory regimes in general and corporate and union independent expenditures specifically on public policy outcomes. These findings suggest that critics' fears about the possible effects of *Citizens United* at the national level may be overstated.

The Supreme Court's 2010 ruling in *Citizens United v. Federal Election Commission* set off a firestorm of criticism, largely centered on the ruling's provisions that allowed corporations and unions to make unlimited independent expenditures to influence electoral outcomes. Unlike money contributed directly to candidates or political parties, the Court's majority opinion reasoned that such independent expenditures did not lead to the appearance or presence of corruption, meaning government had no constitutionally compelling justification for limiting this spending.[1] Subsequent decisions by lower federal courts and the Federal Election Commission (FEC) have faced similar criticism for allowing these same actors, as well as individuals, to contribute unlimited sums of money to independent expenditure-only committees, a category that includes so-called Super Political Action Committees (Super PACs), 527 organizations, and 501(c) organizations. Depending on their chosen route of engagement, these activities by firms and others may go undisclosed to the public and to corporate shareholders or may be disclosed only long after they occur and then, only if they meet a certain financial threshold. The end result of these court rulings and administrative actions is that corporations, unions, organizations, and individuals have all been granted more latitude to engage in electoral politics financially than at any time since the 1940s (Briffault 2012).

In popular discourse and among its critics, *Citizens United* is linked to the notion that American politics is experiencing a "corporate takeover" that will result in public policy favoring the desires of business and its allies and that, more fundamentally, threatens core democratic precepts of equal opportunity to influence government (see, e.g., Bai 2012; Dworkin 2010). Proposals to address the fallout of the decision have ranged from requiring more comprehensive disclosure by government contractors and publicly held firms, to amending the Constitution to

[1] *Citizens United v. Federal Election Commission*, 130 S. Ct. 876, 899–913 (2010).

overturn the Court's equivalence of money with speech and of corporate persons as having the same spending rights as individuals.

What is missing from both the case law and the popular discourse on this point is hard evidence about the policy impacts of campaign finance laws. Not only do reformers rest their claims of untoward business influence on conjecture, but so too did the Supreme Court in the case, when it asserted as a matter of fact that independent expenditures do not lead to corruption or the appearance of it. In part, this result is a failure of social science, as little existing empirical work in political science, law, economics, or management addresses the question of how campaign finance regimes shape public policy outcomes of interest to business and labor at a systemic level.

Given how recent a decision *Citizens United* is, it is near impossible to address this shortcoming by conducting a rigorous examination of the decision's effects on national politics, particularly on public policymaking. However, federalism allows us to gain some traction on the potential policy impacts of *Citizens United*. Over the last century, the states have experimented with various forms of campaign finance reform, including bans on corporate and union expenditures. Our goal in this paper is to exploit this variation in expenditure bans to provide a better sense of what we can expect, in terms of policy outcomes, at the national level. In particular, we are interested in outcomes of interest to corporations and unions – specifically, left-party power, relative minimum wage rates, and economic inequality – and whether or not these outcomes vary systematically at the state level with the manner in which states allow these actors to participate in electoral politics.

This paper proceeds in four parts. First, we provide a brief background on the power of business and unions in American politics, focusing on the effects of campaign contributions.

Second, we examine how historical variation in campaign finance regimes at the state-level provides us with leverage to assess the potential policy effects of *Citizens United* at the national level. Third, we articulate expectations regarding how campaign finance laws might affect public policymaking in three domains of concern to business and labor. Fourth, we empirically test these hypotheses through analysis of state-level panel data covering the period from 1977–2006. We find that campaign finance laws have minimal effects on aggregate political outcomes.

Political Power and Campaign Finance

Concerns over business's influence on, and advantages in, the public policymaking process in the United States have been ever present. However, since the counter-mobilization of business in the wake of the public interest movements of the 1960s and 1970s (Vogel 1989) and the concurrent and continuing decline of labor unions (Francia 2006), many scholars, politicians, and popular writers have intensified their criticisms. In particular, these critics have focused on what they perceived as the outsized instrumental power of business, as exercised through campaign contributions, lobbying, and corporate charity. The logic of the argument is that policymakers in the elected branches are so dependent on the resources that capital can provide them – in funding their campaigns, in providing information as part of the policy process, and in supporting the creation and maintenance of quasi-public goods, such as cultural institutions and universities – that they are reluctant to enact policies that are perceived as unfriendly to business or pro-labor for fear of electoral reprisal or a lack or electoral support.

Some writers in this vein contend that business' instrumental power, especially in comparison to labor, has become so great that the United States is devolving from a democracy into an oligarchy (Winters and Page 2009). For example, Hacker and Pierson (2010) argue that

since the early 1970s members of Congress have enacted regulatory and tax policies that overwhelmingly favor capital over labor due to campaign contributions from the former and the declining political power of the latter, and Johnson and Kwak (2010) claim that the growing instrumental power of business, especially that of the financial sector, from the 1980s onward produced not only the regulatory policies that allowed for the recent financial crisis but also the allegedly toothless responses to that crisis, including the Dodd–Frank Act.

The empirical literature on campaign contributions and lobbying casts doubt on these contentions, however. At the macro- and micro-levels, respectively, Smith (2000) and Hall and Wayman (1990) both find that when the interests of business clash with public opinion on salient policy issues, it is the public's preferences that tend to prevail. This dynamic is in part a result of business being a far more diverse and divided category of interests than many critics acknowledge (Werner and Wilson 2010). Differences exist not only between large and small firms but also across sectors, between labor and capital-intensive firms, between market leaders and upstarts, and between export-oriented and non-export-oriented firms. Perhaps even more important, the strength of public preferences is also due to the relatively small role corporate and, more generally, organizational contributions have played in electoral politics (Ansolabehere, Snowberg, and Snyder 2005), and consequently, the small amount of leverage that political action committees have had in the overall donor pool in comparison to individuals (Ansolabehere, de Figueiredo, and Snyder 2003). Another perspective arguing that corporate electoral involvement generally has small effects contends that rather than seeking power through campaign contributions, firms are playing a game of defense and view themselves as vulnerable to "shakedown" efforts conducted by office seekers (Sitkoff 2003).

Further, many of the accounts critical of business's power fail to distinguish between campaign contributions and traditional D.C.-focused lobbying as separate tactics firms and other actors might employ. Too often critics lump these different tactics together, under the assumption that they serve the same ends and have roughly the same degree of efficacy. In the case of campaign contributions, however, the literature has largely coalesced around the belief that firms and other interests contribute to campaigns not to directly influence policy but rather to have access to relevant policymakers. That is, campaign contributions are more akin to a classic consumption good than an investment (Ansolabehere, de Figueiredo, and Snyder 2003). At most, the existing literature views contributions as akin to paying an ante that all players seeking to influence policy must contribute with no expected return beyond being viewed as serious.

In contrast, lobbying appears to pay greater dividends in that it is aimed not at affecting the composition of policymaking bodies but rather the composition of the policy agenda and policy outcomes. Evidence suggests that the advantages that business has over labor in lobbying does have a payoff for corporate interests, in that firms play a substantial role in shaping the policy agenda (Baumgartner et. al. 2009), and some outcomes of interest to business, such as effective tax rates (Richter, Samphantharak, and Timmons 2009) and contract awards (Goldman, Rocholl, and So 2009), vary significantly with lobbying effort.

The question we explore is whether or not *Citizens United* and the legal and regulatory decisions that followed it might alter our understanding of the power of both business and labor at the federal level, given the newfound ability of corporations, unions, individuals, and other groups to spend directly without limits or to donate unlimited amounts to independent-expenditure committees. We seek to understand whether and how the Supreme Court's decision

and the aftermath of complementary decisions and rule interpretations might affect public policymaking.

To date, empirical explorations of *Citizens United*'s consequences for candidate and group behavior (Franz 2011), for electoral outcomes (Coleman 2010; LaRaja and Shaffner 2012)[2], and in the financial markets (Werner 2011) have found little to no effect. Yet, since we can view *Citizens United* as fundamentally altering the leverage or potential leverage of both businesses and unions in the pool of campaign funds, the policy effects of the decision may be more implicit than explicit—because candidates/policymakers may fear that independent expenditures can dwarf their own campaigns' spending, they might be reluctant to make policy contrary to these interests' desires, regardless of whether or not businesses or unions actually take advantage of the new forms of instrumental power granted to them. Thus, the fear of massive spending could be as powerful an influence on policymakers as the reality of massive spending. As mentioned above, with so little time having passed since the Supreme Court issued its decision, we do not have the ability to examine policy outcomes systematically at the national level. We do, however, have the ability to exploit variation at the state level, as prior to the decision in *Citizens United*, 27 states already had campaign finance regimes similar to that ushered in by the decision. We explore this variation in the development of states' campaign finance regimes further in the next section before turning to our empirical examination.

[2] This result parallels that found by Gross and Goidel (2003) in examining campaign finance regulations generally in the states. Focusing on contribution limits (corporate, union, individual, PAC, family member, and candidate) and public financing (to parties, to candidates), they find minimal impact of these regulations on partisan electoral outcomes.

Variation in State Campaign Finance Regimes

Campaign finance laws and regulations in the states changed dramatically across 1977-2006, the period of our analysis. In the period following Watergate, states, like the federal government (Parker and Coleman 2004), added a range of new restrictions on candidates, parties, interest groups, and individual contributors.

Malbin and Gais's (1998) and Gross and Goidel's (2003) overviews of state campaign finance shows how significantly these regimes have changed and explain some of the motivations and interests behind these changes. A common pattern was for a sharp increase in the number of states adopting a regulation in the 1970s and gradual growth in the number after that. According to Malbin and Gais (1998), in 1972 only 2 states limited individual contributions to state candidates. That number jumped to 21 states by 1980. And by 1996, 35 states did so. The number of states limiting PAC contributions doubled from jumped from 9 in 1976 to 32 in 1996, while limits on contributions from parties were in place in 3 states in 1976 and 19 states by 1996. Four states had public funding of elections in 1974; more than 5 times as many did in 1996. Across the states, therefore, there was a general trend toward adding new restrictions, but the basket of restrictions in place across the states varied widely. And not only did states vary by the presence or absence of various regulations, but they also varied substantially in the nature of these regulations, for example the allowable dollar limits of individual contributions. One of the few areas that did not see substantial change in the number of states with regulations in place was campaign finance disclosure. By 1970, 45 states already had reporting and disclosure requirements in place for state candidates (Gross and Goidel 2003). The exact requirements for disclosure, however, varied.

As Milyo and Primo (2005) suggest, at this point we may be at the "mature" stage of campaign finance regulation, in that certain restrictions and requirements have become the norm around the country. Individual contribution limits and disclosure would be examples of these common restrictions and requirements, respectively. The recent actions of the Supreme Court and the federal courts, however, have led to some disturbance in these mature regimes. In June 2012, the Court struck down restrictions on corporate independent expenditures in Montana, making it clear the majority on the Court believed the logic of *Citizens United* applied to state-level elections, and that the particular circumstances within states—Montana pointed to a history of corruption that preceded the ban on corporate spending—did not override the First Amendment considerations at the root of *Citizens United*. The conviction of the Court majority that aspects of the federal and state campaign finance regimes intrude on free speech and associational rights may disrupt other aspects of campaign finance regulation in future years.

Theoretical Expectations & Policy Implications

Given the myriad campaign finance regimes across states, we can turn to an analysis of these regimes to explore what effects *Citizens United* may have on businesses' and unions' interests in the public policymaking process. To do so, we examine three outcomes of interest to businesses, unions, and society more broadly: left party power in government, state minimum wage rates relative to the federal minimum wage, and pre-transfer market inequality. As discussed above, the extant literature would suggest that these indicators will not vary systematically by campaign finance regimes. That is, because the perceived "return on investment" for individual firms' and unions' political contributions is negative or, at most, zero, few corporations and unions ought to take advantage of looser campaign finance regimes and

participate more heavily in electoral politics. As a result, at the aggregate level, public policies may bear little relationship to the campaign finance regimes of the states that produce them.

Nonetheless, as the rhetoric surrounding *Citizens United* suggests, there are reasonable claims that may lead one to expect a relationship between campaign finance laws and public policy outcomes. For example, if reformers and others worried about the prospects unleashed by *Citizens United* are correct, we would expect that in those states with a *Citizens United*-like environment, these outcomes would be more favorable toward business. We would expect left party power to be lower, the state minimum wage rate to be lower relative to the federal rate, and pre-transfer market inequality to be higher. This expectation rests on the frequently made contention that corporate independent expenditures would dwarf similar efforts by labor unions.

Of course, these possibilities are not the only reasonable expectations regarding the effects of *Citizens United*. We could see a non-effect if neither unions nor businesses engage in independent expenditures or if they both do but cancel each other out. Or, we could even potentially see a positive relationship between the absences of an independent expenditure bans and these policy outcomes, should businesses sit on the sidelines while unions spend, or if an independent expenditure ban applied only to corporations.

In our next section, we test for all of these potential effects using a time-series–cross-sectional framework that includes not just various measures of the political power of business and labor but also controls for covariates that are relevant for each of the three outcomes we model. As hypotheses, we adopt the following naïve expectations, which stem from popular perceptions of the *Citizens United* decision:

> **H1**: The absence of independent expenditure bans on corporations will lead to policy outcomes more favorable to capital, in the form of lower left party power, lower relative minimum wage rates, and higher inequality.

H2: The absence of independent expenditure bans on unions will lead to policy outcomes more favorable to labor, in the form of greater left party power, higher relative minimum wage rates, and lower inequality.

Models and Variables

To assess the potential impact of *Citizens United*, we ran separate time-series–cross-sectional models that examined whether or not variation in state-level campaign finance laws affected left-party power, relative minimum wage rates, and pre-transfer inequality levels in the 50 states over the 30-year period from 1977 through 2006. For all three of these outcomes, we ran an error correction model (ECM) using Prais-Winsten regression with panel corrected standard errors. Our dependent variables, defined further below, were the one-year differenced value of the outcome, and our independent variables were the one-year lag of the dependent variable, as well as the one-year differenced and lagged values for all of our independent variables.

Error correction modeling is a time-series approach that can easily be applied to a panel framework (Beck 2001; Kelly and Witko 2012). Chief among its benefits are its lack of imposed restrictions and its use of the first-differenced value of the dependent variable, which assures us that our panels are of stationary processes. To assess statistical significance in an ECM, we examine both the lagged and differenced values for each independent variable, and if either is statistically significant, we can state that there is a significant association between that variable and the dependent variable (De Boef and Keele 2008). In terms of their substantive interpretation, the differenced measure captures the variable's immediate impact, while the lagged term, in combination with the lagged value for the dependent variable captures the error correction component of the variable or its long-term impact.

Following Wilson and Butler (2007), we conducted various diagnostic tests to arrive at our final models. Although the ECM captures the dynamics of our data, it does not take account whether or not unit effects, autocorrelation, or heteroskedasticity might need addressing. First, through a series of F-tests, we concluded that it was necessary to include unit effects for the 50 states, which we did by including dummy variables for all states but one. In our tables below we omit these coefficients from our results, but these unit effects have the benefit of capturing non-time varying or very slow-moving differences across the states (e.g., political culture), even if they are not theoretically or substantively of direct interest themselves. Second, despite differencing our dependent variable, tests for serial autocorrelation revealed its presence in all of our models; we successfully corrected for it through the inclusion of a common AR(1) process. Finally, tests also revealed that our panel-corrected standard errors were heteroskedastic; we corrected for this violation as well.

As a final note on our modeling approach, in an attempt to better identify the intervention effect of a corporate or union spending ban being put into place or repealed, in second specifications of each of our models, we eliminated those states that had such bans in place prior to the start of the relevant time period for that model. By eliminating these censored, early adopters, we do lose observations and must limit our inferences to the remaining states. However, this procedure allows us to more cleanly identify statistically the impact of the enactment of independent expenditure bans.

Key Independent Variables

Our key independent variables are common across all of our models. Our two key independent variables capture whether or not in each state-year dyad a ban on corporate

independent expenditures and union independent expenditures was present (1) or absent (0). These data came from the National Conference of State Legislatures. Consistent with our naïve hypotheses above, we would expect that the coefficient for corporate and union independent expenditures would be statistically significant, if these bans affect policymaking. In the case of a corporate ban, we would expect it to be positively associated with the relative minimum wage and negatively associated with left party power and pre-transfer market inequality; we would expect the opposite signs for a union ban.

Of course, these campaign finance variables are unlikely to capture all of the relevant variation in attempts by interested actors to affect these outcomes. To control for other campaign finance effects, we include an index of campaign finance laws developed by Primo and Milyo (2006) that we supplement. Primo and Milyo construct a 0 to 5 index that is a sum of the presence/absence of various campaign finance reforms by state-year and includes measures tapping donation limits, disclosure laws, and public funding. Using Primo and Milyo's underlying data, as well as data gathered by Werner and Mayer (2012), we can expand the 0 to 5 scale to a 0 to 11 scale by separating out public funding programs by partial versus full public funding and by whether or not such programs were for legislative or gubernatorial candidates. We can also separate out whether or not limits on expenditures in the pre-*Buckley v. Valeo* era applied to legislative or gubernatorial candidates. The 11 items in our campaign finance index are: 1) disclosure law; 2) limits on individuals' donations to candidates; 3) limits on organizations' donations to candidates; 4) a ban on corporate contributions to candidates; 5) a ban on union contributions to candidates; 6) partial public funding for legislative candidates; 7) full public funding for legislative candidates; 8) public funding for gubernatorial candidates; 9)

expenditure limits for legislative candidates; 10) expenditure limits for gubernatorial candidates; and 11) a soft money ban.

In 1976, states averaged 2.74 laws (standard deviation 1.27) and in 2006 states averaged 3.70 laws (standard deviation 1.46). The median number of reforms in place rose from 3 to 4 over the period, with the maximum score rising from 5 to 7 over that time.

Additionally, as noted above, businesses and unions attempt to influence electoral and policy outcomes via non-campaign finance routes. To capture these efforts, we include the number of business associations existing in each state-year using data gathered by Spillman (2003) from the *Encyclopedia of Associations*, *National Trade and Professional Association Directory*, and *Associations Yellowbook*, as well as the percentage of each state-cycle's non farm-based workforce that is unionized (using data collected by Hirsch and Macpherson 2003 and updates on their website).[3] This measure is a raw count of associations for each state by year. Although it might seem appropriate to adjust this count for state population, doing so could in fact be misleading. Nearly all states will have single associations representing their major industries and sectors, rather than some multiple of these associations as a state enlarges. There will be one state bar association representing attorneys, for example. Economically more diverse states will likely generate more associations, and we believe the raw count of associations best reflects their potential impact on policy outcomes.

Our working assumptions with the business association and union density variables are twofold. First, a state that is more densely populated with business associations will have more

[3] Spillman collected these data from recent editions of these sources only, which suggests that there may be a mortality bias in our use of her data. That is, although we calculate our counts for each year based upon the year the various organizations listed as their founding, if an organization from the 1970s did not survive into the 2000s, then it would not be captured in the Spillman data, and thus, there may be a slight undercount of associations.

business-friendly lobbying efforts directed at state legislators. Second, elected officials in a state whose workforce is relatively highly unionized will, either because of lobbying efforts or the number of votes accounted for by union members, be more receptive to a labor-friendly message.

Dependent Variables

Our first dependent variable is left party power in each state-year. As Kelly (2009) and Kelly and Witko (2012) note, the concept of left party power originates in the power resource theory literature in comparative public policy (see, e.g., Huber and Stephens 2001) and attempts to capture how low income groups will fare in terms of governmental policymaking. As Kelly and Witko note, even though American Democrats would not be considered a "left" party in comparative context, in every state, the Democratic Party is to the left of its complementary Republican Party (see, Gelman et al. 2008). Further, common space measures pegged to national politics developed by Berry et al. (1998) allow us to adjust for the considerable ideological variation across state parties, recognizing, for example, that Democrats in Alabama are ideologically of a different stripe than Democrats in Vermont. Thus, to measure left party power, we used the NOMINATE version of Berry et al.'s institutional ideology score by state-year. This measure computes the relative liberalism and conservatism of the Democartic and Republican parties in each state, and then weights these scores by the degree of Democratic control of the state legislature and the governorship. The logic is that the ability of unions to spend should push a state's Democratic politicians further to the left when taking into account other variables that might affect the degree to which they lean to the left. Corporate spending should have the opposite effect.

In addition to including the lagged and differenced versions of the three campaign finance variables of interest (the corporate and union bans and the index), as well as the percentage of the population in unions, to predict the differenced value of left party power, we included its lag and the lags and differences for a series of variables tapping how left-leaning a state's government is likely to be. These variables included a revealed measure of the state's electorate's preferences (the Democratic percentage of the two-party vote in the most recent presidential election), an expressed measure of the state's electorate's preferences (the citizen ideology measure from Berry et al. 1998), and various measures of the state's demographics: the percentages of the population that live in urban areas, that is non-white, that is over 65 years of age, that has at least a college degree, and that is employed in manufacturing. Combined with the unit effects, we believe these covariates effectively tap competing explanations for elites' liberalism.

Our second dependent variable analyzes state minimum wage rates relative to the federal rate. That is, for each state-year observation, we calculate its relative minimum wage using the following equation:

$$\frac{State\ Minimum\ Wage_{it} - Federal\ Minimum\ Wage_{t}}{Federal\ Minimum\ Wage_{t}}$$

The quotients resulting from these calculations then served as the dependent variable in our analysis. To predict the relative minimum wage, in addition to its own lag and the lags and differences of our key campaign finance, business lobbying, and union density variables, we used the lagged and differenced values of control variables selected from a review of the existing literature on state minimum and living wage rates (Ford, Minor, and Owens 2012; Gallet 2004;

Waltman and Pittman 2002), as well as the more general literature on social policy competition between the states (e.g., Berry, Fording, and Hanson 2003). First, to account for national influences on state decisions, we counted the number of years since the last federal minimum wage increase and also accounted for the national inflation rate, as several state minimum wages are pegged to it. Second, to capture state-level economic conditions, we included the percentage of the adult population employed in the state, as well as the real (in thousands of 2006 Illinois dollars, adjusted following the approach developed by Berry, Fording, and Hanson 2000) disposable income per capita. Third, to account for additional political forces that might pressure states to adjust their relative minimum wage, we included the percentage of workers in farm-based employment, citizen ideology, institutional ideology (the dependent variable in our left party power model), the percentage of the population between 18 and 24 years of age, and the percentage of the population that is non-white. Finally, to control for dynamics due to neighboring states' decisions, we included the population-weighted relative minimum wage in neighboring states.[4]

Our final model examines pre-transfer market inequality as its dependent variable and adds our three campaign finance and our business associations indicators to the analysis of this policy outcome by Kelly and Witko (2012) (their analysis includes the union density measure present in our models). As in that article and Kelly's earlier work (2009), we are particularly interested in how the ways in which states "condition" their market environments (for example, through their labor or regulatory regimes), affect inequality. Our focus on this pre-transfer outcome is justified, given states' limited discretion in transfer programs and their demonstrated inability to address post-transfer inequality unilaterally (see, e.g., Barrilleaux and Davis 2003).

[4] We employed California as Hawaii's neighbor and Washington State as Alaska's neighbor when calculating this indicator.

To measure inequality, we use a by state-year Gini index calculated by Kelly and Witko from U.S. Census data. Following Kelly and Witko and based upon their findings, we split our sample into two periods: 1977-1994 and 1995-2006. As these authors conclude and as others have argued, the importance of the states in addressing both pre- and post-transfer inequality grew in importance following the Republican Revolution of 1994 and subsequent shifts in federal economic and social policy. Splitting our analysis by time period most efficiently captures this dynamic, to see whether the relationships between our key variables and inequality were any different in these two eras. Kelly and Witko show through Chow tests conducted using seemingly unrelated regression results that this approach is statistically more appropriate than including a dummy variable for years pre- and post-1994. The significance and magnitude of the effect of state-level variables on inequality are greater in Kelly and Witko's study in the latter period. Our analysis extends this logic to analyzing the effect of campaign finance regimes and independent expenditure regulations.

Results and Discussion

Left party power: We turn first to an analysis of left party power, with results presented in Table 1. Focusing first on the variables of chief theoretical and empirical interest, the campaign finance variables, only the lagged term for the campaign finance index is statistically significant, and it is significant in both specifications of our model. There are no significant effects for the binary indicators capturing corporate and union spending bans, nor is there an effect for union density: bans on corporate and union independent expenditures and a higher level of unionization in a state are not associated with the degree of liberalism among state elected officials.

[Insert Table 1 about here.]

The signs on the campaign finance index coefficients are negative, a finding that is in tension with popular perceptions of campaign finance reform's likely effects. The call for additional campaign finance reform is most commonly heard from Democratic officials and liberal activists, who argue that campaign finance in the absence of various reforms tilts the electoral playing field too heavily to the advantage of Republican and conservative candidates. We might therefore expect that such reforms would lead to greater electoral success for the party—presuming that Democrats weigh their self-interest at least to some degree on such issues and that they do act purely on principle. Our findings, however, show that the adoption of campaign finance reforms is associated with a decline in the subsequent level of left party power.

The substantive impact of this effect is substantial. The mean of the lagged campaign finance index is 3.14, which after setting all other variables at their means in our first specification in Table 1, produces a predicted left party power level of -0.002 (effectively zero). If we adjust this lagged index upwardly by one standard deviation (effectively, 1.53 reforms), however, we shift the predicted ideological makeup of a state's government from one that is essentially evenly split between liberalism and conservatism to one that is seven times more conservative, with a predicted left party power value of -0.014. This change is captured in Figure 1, which plots predicted left party power over the observed range of the lagged campaign finance index variable. Over the entire range of the lagged index, predicted left party power decreases from 0.024 to -0.033. The intersection of the fitted prediction line with the horizontal line at zero reveals that, all else equal, the tipping point between having a state government that is slightly liberal versus one that is slightly conservative occurs at a fairly low level of campaign finance reform (between the enactment of two and three reforms out of 11 possible).

Table 1: Error Correction Panel Model of State Left Party Power, 1977-2006

		All States		Pre-1977 Bans Excluded	
Independent Variables		**Coeff.**	**Std. Error**	**Coeff.**	**Std. Error**
Left Party Power	L	-0.274	(0.019)	-0.271	(0.021)
% Union	L	0.045	(0.146)	-0.004	(0.165)
	D	0.100	(0.191)	0.011	(0.202)
Campaign Finance Index	L	-0.008	(0.005)	-0.008	(0.005)
	D	-0.001	(0.009)	-0.003	(0.010)
Corporate Spending Ban	L	-0.013	(0.026)	-0.009	(0.026)
	D	-0.058	(0.053)	-0.061	(0.053)
Union Spending Ban	L	-0.014	(0.029)	-0.019	(0.029)
	D	0.006	(0.061)	0.008	(0.061)
% Democratic Two-Party Presidential Vote	L	-0.006	(0.052)	0.027	(0.055)
	D	0.012	(0.073)	0.003	(0.079)
Citizen Ideology	L	0.383	(0.051)	0.367	(0.056)
	D	0.934	(0.078)	0.931	(0.051)
% Urban	L	-0.100	(0.131)	-0.291	(0.143)
	D	0.154	(0.312)	0.028	(0.338)
% Nonwhite	L	-0.049	(0.099)	-0.032	(0.101)
	D	0.127	(0.354)	0.164	(0.357)
% 65+	L	-0.416	(0.456)	-0.284	(0.471)
	D	3.469	(2.672)	2.080	(2.763)
% College +	L	0.047	(0.155)	0.037	(0.167)
	D	-0.181	(0.250)	-0.208	(0.275)
% Manufacturing	L	-0.074	(0.087)	-0.065	(0.093)
	D	0.243	(0.207)	0.310	(0.222)
ρ		0.028		0.032	
n (observations)		1550		1271	
n (states)		50		31	
r^2		0.318		0.322	

L = Lag term; D = Difference term

Error correction model with heteroskedastic panel corrected standard errors and a common AR(1) process. Dependent variable is the differenced left party power by state-year; state fixed effects were estimated but are suppressed for space.

[Insert Figure 1 about here.]

Two mechanisms may be behind this effect. First, as we suggest above, Democrats may willingly pay an electoral cost for enacting campaign finance reforms on principle. An example of such behavior is the federal ban on soft money expenditures passed as part of the Bipartisan Campaign Finance Reform Act in 2003. By the time this ban was enacted, Democrats had achieved parity with Republicans in soft money fundraising (and even surpassed them in some years), but they still voted for the legislation.[5]

A second possible mechanism behind the link of greater fundraising restrictions and lower left party power relates to the growing strength of the Republican Party at the state-legislative level during this time-period. Because many campaign finance reforms are designed to limit behaviors that are easier for incumbents to engage in, such restrictions may aid challengers disproportionately. At the aggregate level such a result would result in the overall "out" party, the Republicans, being advantaged as more restrictions on incumbents were enacted and the campaign finance playing field became more level between incumbents and challengers.

Of the control variables in our model, the most interesting significant result was the positive relationship between citizen ideology and left party power: as both the lagged and differenced value of this variable increased (as citizens became more liberal), so too were elected officials, indicating that there is a strong substantive connection between these two measures. This result provides support for the view that elections promote policy responsiveness.

Minimum wage: The results for the relative minimum wage parallel those for left party power. Table 2 shows that neither the bans on corporate and union spending nor union density have a significant effect on the relative minimum wage. The results diverge from those on left

[5] For soft money fundraising totals, see, OpenSecrets.org, "Party Fundraising Totals by Cycle," at: http://www.opensecrets.org/bigpicture/ptytots.php?cycle=2000#soft

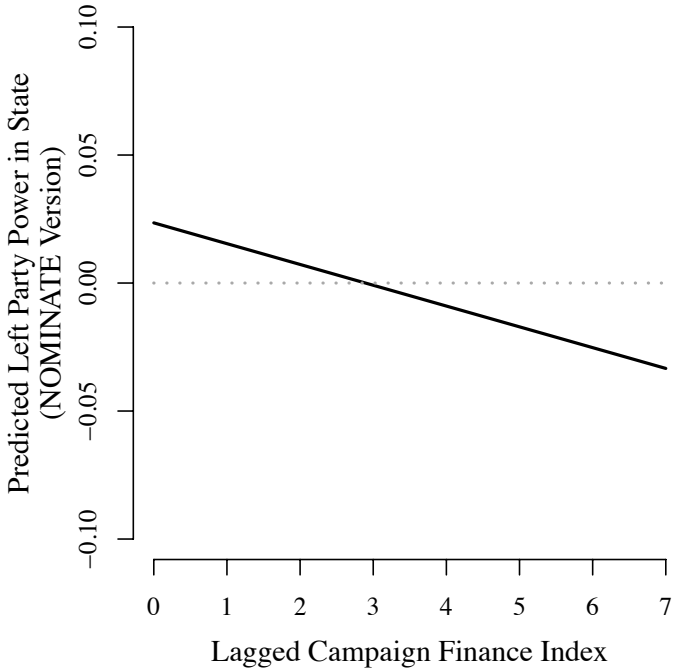

Figure 1: The predicted level of left party power in state government declines when states have enacted additional campaign finance reforms in the previous year. The dashed horizontal line demarcates a perfectly moderate state government, with values above zero indicating greater left party power.

party power in that the campaign finance index has no significant relationship to the minimum wage. Of the variables tapping union and business influence, only the coefficient for the differenced version of the number of business associations is significant. As business associations increase in number, and likely spend more time lobbying relevant decision-makers, the relative minimum wage decreases. To illustrate, when we set all variables in the first specification to their means (the mean difference in the number of business associations is 0.902), the predicted relative minimum wage level is 1.42% above the federal minimum wage. After increasing the differenced number of business associations from its mean by one standard deviation (1.82), the predicted relative minimum wage level decreases substantially, to just 0.53% above than the federal minimum wage, a reduction of nearly two-thirds in a state's relative minimum wage rate.

[Insert Table 2 about here.]

Figure 2 plots the predicted relative minimum wage in a state over the observed range of the differenced number of business associations (i.e., the number of new business associations in a state-year). The plot reveals a steep decline in the relative minimum wage as business steps up its mobilization: the creation of four or more new associations in a year is associated with a state's relative minimum wage turning negative and the state's minimum wage dropping below the federal rate. Substantively, few observations fall at four or higher on the *x*-axis and few occupations are covered solely by a state's minimum wage, but nonetheless this result demonstrates the power of the business lobby and also, in the context of our full minimum wage model, reveals lobbying's effect versus the non-effects of campaign finance.

[Insert Figure 2 about here.]

Table 2: Error Correction Panel Model of State Relative Minimum Wages, 1977-2006

		All States		Pre-1977 Bans Excluded	
Independent Variables		**Coeff.**	**Std. Error**	**Coeff.**	**Std. Error**
Relative Minimum Wage	L	-0.226	(0.024)	-0.212	(0.026)
Total Business Associations Existing	L	-0.003	(0.001)	-0.001	(0.001)
	D	-0.004	(0.002)	-0.005	(0.003)
% Union	L	-0.001	(0.001)	-0.002	(0.002)
	D	0.002	(0.002)	-0.001	(0.002)
Campaign Finance Index	L	0.004	(0.004)	0.004	(0.004)
	D	0.011	(0.008)	0.011	(0.008)
Corporate Spending Ban	L	0.001	(0.022)	-0.002	(0.023)
	D	0.078	(0.053)	0.071	(0.053)
Union Spending Ban	L	-0.025	(0.024)	-0.024	(0.025)
	D	-0.073	(0.054)	-0.078	(0.055)
Years Since Federal Increase	L	0.001	(0.001)	0.001	(0.001)
	D	0.008	(0.002)	0.008	(0.002)
National Inflation Rate	L	0.004	(0.002)	0.004	(0.002)
	D	-0.002	(0.002)	-0.002	(0.002)
% Employed	L	0.002	(0.001)	0.001	(0.002)
	D	0.005	(0.003)	0.003	(0.004)
Real Disposable Income per capita ($1000)	L	0.002	(0.001)	0.001	(0.002)
	D	-0.001	(0.004)	-0.002	(0.004)
% Farm Employment	L	-0.001	(0.003)	-0.001	(0.004)
	D	-0.007	(0.013)	-0.001	(0.140)
Citizen Ideology	L	0.001	(0.001)	0.001	(0.001)
	D	0.001	(0.001)	0.001	(0.001)
Institutional Ideology (Left Party Power)	L	0.001	(0.001)	0.001	(0.001)
	D	-0.001	(0.001)	-0.001	(0.001)
% 18-24	L	-0.003	(0.003)	-0.003	(0.003)
	D	0.003	(0.004)	0.004	(0.005)
% Nonwhite	L	0.001	(0.001)	0.001	(0.001)
	D	-0.002	(0.002)	-0.003	(0.003)
Weighted Relative Minimum Wage in Neighboring States	L	0.031	(0.027)	0.300	(0.029)
	D	0.387	(0.036)	0.347	(0.039)
ρ		0.063		0.066	
n (observations)		1550		1271	
n (states)		50		31	
r^2		0.285		0.252	

L = Lag term; D = Difference term

Error correction model with heteroskedastic panel corrected standard errors and a common AR(1) process. Dependent variable is the differenced relative minimum wage by state-year; state fixed effects were included but are suppressed for space.

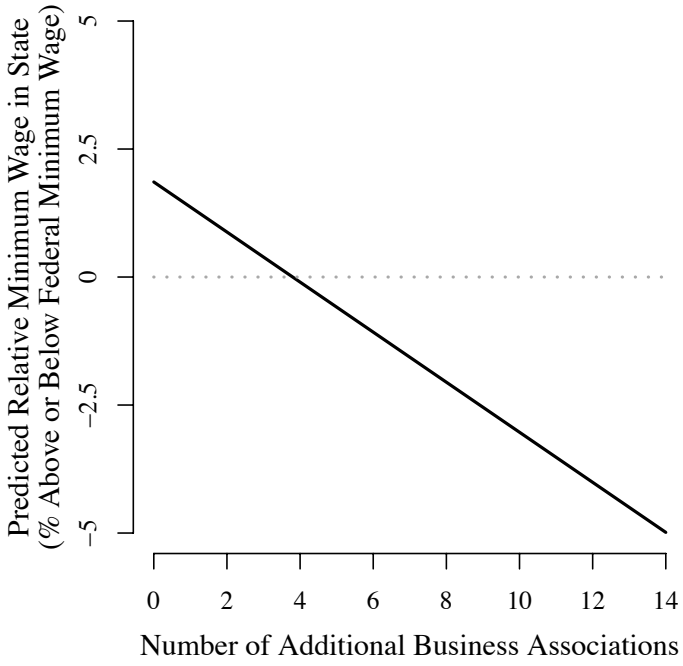

Figure 2: The predicted relative minimum wage in a state, as a percentage of the federal minimum wage, declines sharply as the number of additional business associations existing (year-to-year difference) increases. The dashed horizontal line demarcates the point at which a state's minimum wage equals the federal minimum wage.

The significant results among our control variables in the minimum wage model were signed as expected and in-line with previous research in this area. For economic factors, both the national inflation rate and the population-weighted average of the relative minimum wage in neighboring states were significant and positively signed, indicating that as inflation ate away at the minimum wage's effectiveness and as neighboring states adjusted their minimum wages, a state was more likely to increase its relative minimum wage. Political factors also contributed to minimum-wage decision-making. When the federal government failed to act to adjust the wage and as state governments became more liberal, states were more likely to increase their relative minimum wages.

Income inequality: Finally, we asked whether campaign finance regulations in general have a significant relationship to a state's level of income inequality. And more specifically, do unrestricted corporate and/or union independent expenditures have any effect on this level? Overall, the answer to these questions is no.

The results for our pre-transfer market inequality analyses are in Tables 3 (pre-1995) and 4 (1995 and beyond). In the pre-1995 model, the coefficients for the variables capturing the presence of corporate and union spending bans are insignificant, as are all the coefficients for union density and business associations. The only significant finding among our campaign finance variables is again for the lagged campaign finance index. As in the left party power analysis, the campaign finance index is signed negatively, indicating that as more restrictions are put into place, the higher the level of pre-transfer inequality in a state. Considering that many campaign finance reforms are designed to limit the influence of wealthy individuals and interests, this result may seem puzzling. However, recalling the finding in the left party power analysis that campaign finance reforms decrease left party power, the result is unsurprising.

Because the actors most likely to legislate to decrease inequality are Democrats, should campaign finance reform decrease their strength in politics as Table 1 suggests, it is less likely that public policy will actively address inequality by altering market conditions.

[Insert Tables 3 and 4 about here.]

This finding does not prevail across the time-split in our analysis. Table 4, which analyzes only 1995-2006, shows that the campaign finance index variable's coefficient, like all the key independent variables' coefficients, is insignificant. None of the measures of union or corporate influence at the state level appear to affect pre-transfer market inequality in this era. This result is particularly interesting, given that the influence of states in this realm likely increased after Republicans won House and Senate majorities in 1994 and significant additional power over social policy devolved to the states.

Across both time periods a host of control variables were significantly associated with pre-transfer market inequality and are signed in-line with expectations from the existing literature. With regard to that literature, we note that Kelly and Witko's (2012) central argument, that those variables related to state characteristics have a greater effect on the level of a state's pre-transfer inequality level after 1994, holds after controlling further for corporate and union influence.

Conclusions

The U.S. Supreme Court's 2010 decision in *Citizens United v. FEC* was one of its most controversial in decades. Decried by critics as an assault on democracy and praised by supporters as a strong defense of the free speech rights of individuals assembled in corporate forms as for-profit or non-profit organizations or associations, the decision led to both the fears and hopes that

Table 3: Error Correction Panel Model of State Pre-Transfer Inequality, 1977-1994

		All States		Pre-1977 Bans Excluded	
Independent Variables		**Coeff.**	**Std. Error**	**Coeff.**	**Std. Error**
Pre-Transfer Market Inequality	L	-0.719	(0.035)	-0.770	(0.040)
% Union	L	-0.013	(0.028)	0.017	(0.033)
	D	-0.011	(0.028)	0.017	(0.031)
Total Business Associations Existing	L	-0.001	(0.001)	-0.001	(0.001)
	D	-0.001	(0.001)	-0.001	(0.001)
Campaign Finance Index	L	0.002	(0.001)	0.002	(0.001)
	D	0.001	(0.001)	0.001	(0.001)
Corporate Spending Ban	L	0.001	(0.005)	0.002	(0.005)
	D	0.008	(0.009)	0.011	(0.009)
Union Spending Ban	L	-0.002	(0.005)	-0.003	(0.005)
	D	-0.001	(0.009)	-0.004	(0.009)
Democratic President	L	0.001	(0.002)	0.001	(0.002)
	D	0.002	(0.002)	0.003	(0.003)
% Congress Democratic	L	-0.065	(0.028)	-0.082	(0.031)
	D	-0.076	(0.024)	-0.067	(0.026)
National Policy Liberalism	L	0.001	(0.001)	0.001	(0.001)
	D	0.001	(0.001)	0.001	(0.001)
State Minimum Wage	L	0.001	(0.001)	0.001	(0.001)
	D	0.001	(0.001)	0.001	(0.001)
Left Government Power	L	0.002	(0.003)	0.001	(0.004)
	D	-0.006	(0.004)	-0.006	(0.004)
% Unemployed	L	0.219	(0.037)	0.181	(0.043)
	D	0.260	(0.048)	0.253	(0.055)
% Manufacturing	L	-0.043	(0.020)	-0.051	(0.022)
	D	0.084	(0.043)	0.068	(0.048)
Change in Gross State Product	L	0.049	(0.017)	0.040	(0.018)
	D	-0.089	(0.090)	-0.064	(0.094)
% Nonwhite	L	0.006	(0.015)	0.025	(0.016)
	D	0.043	(0.034)	0.035	(0.035)
% 65+	L	0.624	(0.115)	0.664	(0.129)
	D	0.799	(0.598)	1.454	(0.642)
ρ		0.011		0.024	
n (observations)		900		738	
n (states)		50		41	
r^2		0.416		0.434	

L = Lag term; D = Difference term

Error correction model with heteroskedastic panel corrected standard errors and a common AR(1) process. Dependent variable is the differenced pre-transfer inequality by state-year; state fixed effects were included but are suppressed for space.

Table 4: Error Correction Panel Model of State Pre-Transfer Inequality, 1995-2006

		All States		Pre-1995 Bans Excluded	
Independent Variables		**Coeff**	**Std Error**	**Coeff**	**Std Error**
Pre-Transfer Market Inequality	L	-0.863	(0.046)	-0.873	(0.056)
% Union	L	0.101	(0.091)	-0.012	(0.111)
	D	-0.041	(0.079)	-0.100	(0.096)
Total Business Associations Existing	L	-0.001	(0.001)	-0.001	(0.001)
	D	0.001	(0.001)	0.001	(0.002)
Campaign Finance Index	L	0.001	(0.002)	0.002	(0.002)
	D	-0.001	(0.003)	0.004	(0.003)
Corporate Spending Ban	L	0.002	(0.006)	0.001	(0.007)
	D	-0.006	(0.010)	-0.008	(0.010)
Union Spending Ban	L	-0.009	(0.009)	-0.009	(0.009)
	D	-0.010	(0.013)	-0.009	(0.014)
Democratic President	L	0.009	(0.005)	0.008	(0.007)
	D	0.006	(0.004)	0.002	(0.005)
% Congress Democratic	L	-0.045	(0.127)	-0.021	(0.163)
	D	-0.065	(0.121)	-0.056	(0.155)
National Policy Liberalism	L	-0.001	(0.001)	-0.001	(0.001)
	D	0.001	(0.001)	-0.001	(0.001)
State Minimum Wage	L	0.002	(0.001)	0.002	(0.002)
	D	0.001	(0.001)	-0.002	(0.002)
Left Government Power	L	-0.001	(0.004)	0.002	(0.005)
	D	-0.012	(0.005)	-0.017	(0.006)
% Unemployed	L	0.357	(0.114)	0.329	(0.148)
	D	0.304	(0.150)	0.357	(0.185)
% Manufacturing	L	0.085	(0.039)	0.135	(0.051)
	D	0.670	(0.046)	0.077	(0.056)
Change in Gross State Product	L	0.012	(0.014)	0.002	(0.015)
	D	-0.040	(0.050)	-0.047	(0.054)
% Nonwhite	L	-0.002	(0.078)	-0.008	(0.085)
	D	0.407	(0.177)	0.468	(0.190)
% 65+	L	0.382	(0.323)	0.730	(0.353)
	D	0.898	(0.793)	0.766	(0.896)
ρ		0.030		0.022	
n (observations)		600		396	
n (states)		50		33	
r^2		0.544		0.555	

L = Lag term; D = Difference term

Error correction model with heteroskedastic panel corrected standard errors and a common AR(1) process. Dependent variable is the differenced pre-transfer inequality by state-year; state fixed effects were included but are suppressed for space.

What about the possibility that the effects of the campaign finance regime and corporate and union spending bans might work in tandem rather than independently. That is, perhaps a spending ban added atop a robust campaign finance regulatory regime would achieve results that a ban added on top a modest regime would not. Although not presented above, we tested this possibility by adding interactions for the lags and differences of our key finance independent variables. Our results were unchanged from those reported above.

We also find that the potential for business lobbying, rather than the potential for business independent expenditures in campaigns, has effects on one of our outcomes of interest: the relative state minimum wage. Lobbying did not show a relationship to income inequality and we did not test it with regard to left party power since we did not have a theoretical reason to believe lobbying would be linked to subsequent levels of left party power. With regard to union density, and thus the potential for policy and political outcomes to be pushed in a leftward direction due to a greater presence of union workers in a state, we do not find any relationship between density and our political and policy outcomes.

What might explain the relative lack of connection between independent expenditure bans and political and policy outcomes, considering the fears expressed by critics? In addition to examining other policy areas to see how well the results in this paper travel to those domains, future research could explore several possibilities (Coleman 2010). Perhaps in states where businesses do not face spending restrictions, opponents are more aggressive in this competitive environment. The splits among businesses mentioned earlier might blunt corporate influence. The difference between restrictive and non-restrictive states could have been blurred by election-motivated issue ads in the restrictive states. Lifting bans on corporate spending can benefit multiple corporate forms, including nonprofit corporations and groups (Citizens United itself was

political and policy outcomes might change as a result of the decision and subsequent decisions by lower federal courts and the Federal Election Commission.

With the decision so recent, however, there has been insufficient time to examine its potential impact on national political and policy outcomes. For analytical purposes, the system of federalism in the United States provides an opportunity to assess the possible effects of *Citizens United.* Prior to the Supreme Court's decision, about half the states allowed the kinds of corporate and union independent expenditures in state elections that were given the Court's approval in the *Citizens United* decision concerning federal elections.

Using a time-series, cross-sectional analysis covering all states from 1977 through 2006, we examined the effect of corporate independent expenditure bans, union independent expenditure bans, and the overall nature of state campaign finance regimes to explore the impact of campaign finance rules on three key political and policy outcomes: the degree of liberalism among state elected officials; the relative state minimum wage compared to the federal minimum wage; and the degree of pre-transfer income inequality in a state. We find minimal effects and no support for our naïve hypotheses that tracked conventional wisdom and popular perceptions. The kind of corporate and union independent expenditures permitted by *Citizens United* had no systematic relationship to political and policy outcomes. State campaign finance regimes were significant only for the degree of left party power and for the degree of inequality prior to 1995, but they were significant in a direction likely unexpected by reform advocates: a larger number of reforms were associated with decreased levels of left party power and increased inequality. These findings suggest that the fears expressed by critics of the Supreme Court's decision have not been borne out in the experience of the states.

a nonprofit entity). Lastly, the analysis here does not consider the level of spending in states that allowed corporate and union spending. Although we believe that comparing the presence or absence of bans is the most direct test of the potential effects of *Citizens United*, we do not doubt that examining variations in spending within the "no-ban" groups across time would be of interest. A major, perhaps insurmountable, obstacle would be the absence of reliable spending data at the state-level across time.

In addition to considering the impact of various campaign finance regimes on political and policy outcomes, future research can also shed light on whether there is any link between these regulations, and particularly corporate and union spending bans, and the presence of corruption in a state. The compelling government interest the Supreme Court has identified that allows limits on campaign contributions is not whether policy tilts to the left or right whether one party wins "too many" or "too few" seats. It is the reality or appearance of the potential corrupting effects of contributions received directly by candidates and parties that the Court has concluded provides sufficient justification to restrict contributions. Independent expenditures were less problematic, the Court reasoned, because they are not under the control of a candidate or party. As we noted at the outset, neither the critics of the Court's decision nor the supporters have marshaled systematic evidence to support their view that independent expenditures have or do not have corrupting implications, making this a ripe area for future investigation.

References

Ansolabehere, Stephen, John M. de Figueiredo, and James M. Snyder. 2003. "Why is there So Little Money in U.S. Politics?" *Journal of Economic Perspectives* 17 (1): 105–30.

Ansolabehere, Stephen, Erik C. Snowberg, and James M. Snyder, Jr. 2005. "Unrepresentative Information: The Case of Newspaper Reporting on Campaign Finance." *Public Opinion Quarterly* 69 (2): 213–31.

Bai, Matt. 2012. "How Did Political Money Get This Loud?" *New York Times Sunday Magazine*, July 22.

Barrilleaux, Charles, and Belinda Creel Davis. 2003. "Explaining State Variation in Levels and Change in the Distribution of Income in the United States, 1978–1990." *American Politics Research* 31 (3): 280–300.

Baumgartner, Frank R., Jeffrey M. Berry, Marie Hojnacki, and David C. Kimball. 2009. *Lobbying and Policy Change: Who Wins, Who Loses, and Why*. University of Chicago Press.

Beck, Nathaniel. 2001. "Time-Series–Cross Section Data: What Have We Learned in the Past Few Years?" *Annual Review of Political Science* 4: 271–93.

Berry, William D., Evan J. Ringquist, Richard C. Fording, and Russell L. Hanson. 1998. "Measuring Citizen and Government Ideology in the American States, 1960–93." *American Journal of Political Science* 42 (2): 327–48.

Berry, William D., Richard C. Fording, and Russell L. Hanson. 2000. "An Annual Cost of Living Index for the American States, 1960–1995." *Journal of Politics* 62 (2): 550–67.

Berry, William D., Richard C. Fording, and Russell L. Hanson. 2003. "Reassessing the 'Race to the Bottom' in State Welfare Policy." *Journal of Politics* 65 (1): 193–49.

Briffault, Richard. 2012. "Super PACs." *Minnesota Law Review* 96: 1629–78.

Coleman, John J. 2010. "*Citizens United* and Political Outcomes." Manuscript. http://users.polisci.wisc.edu/coleman/CitizensUnitedandPoliticalOutcomes.pdf.

De Boef, Suzanna, and Luke Keele. 2008. "Taking Time Seriously: Dynamic Regression." *American Journal of Political Science* 52 (1): 184–200.

Dworkin, Ronald. 2010. "The Decision That Threatens Democracy." *The New York Review of Books*, May 13.

Francia, Peter L. 2006. *The Future of Organized Labor in American Politics.* New York: Columbia University Press.

Franz, Michael M. 2010. "The *Citizens United* Election? Or Same As It Ever Was?" *The Forum* 8 (4): Article 7.

Ford, William F., Travis Minor, and Mark F. Owens. 2012. "State Minimum Wage Differences: Economic Factors or Political Inclinations?" *Business Economics* 47 (1): 57–67.

Gallet, Craig A. 2004. "The Determinants of Living Wage Rates." *Social Science Journal* 41 (3): 661–66.

Gelman, Andrew, David Park, Boris Shor, Joseph Bafumi, and Jeronimo Cortina. 2008. *Red State, Blue State, Rich State, Poor State: Why Americans Vote the Way They Do.* Princeton, NJ: Princeton University Press.

Goldman, Eitan, Jörg Rocholl, and Jongil So. 2009. "Do Politically Connected Boards Affect Firm Value?" *Review of Financial Studies* 22 (6): 2331–60.

Gross, Donald A., and Robert K. Goidel. 2003. *The States of Campaign Finance Reform*. Columbus: Ohio State University Press.

Hacker, Jacob S., and Paul Pierson. 2010. *Winner-Take-All Politics: How Washington Made the*

Rich Richer and Turned Its Back on the Middle Class. New York: Simon & Schuster.
Hall, Richard L., and Frank W. Wayman. 1990. "Buying Time: Moneyed Interests and the Mobilization of Bias in Congressional Committees." *American Political Science Review* 84 (3): 797–820.
Hirsch, Barry T., and David A. Macpherson. 2005. "Union Membership and Coverage Database from the Current Population Survey: Note." *Industrial and Labor Relations Review* 56 (2): 349–54.
Huber, Evelyne, and John Stephens. 2001. *Development and Crisis of the Welfare State*. Chicago: University of Chicago Press.
Johnson, Simon, and James Kwak. 2010. *13 Bankers: The Wall Street Takeover and the Next Financial Meltdown*. New York: Pantheon.
Kelly, Nathan J. 2009. *The Politics of Income Inequality in the United States*. New York: Cambridge University Press.
Kelly, Nathan J., and Christopher Witko. 2012. "Federalism and American Inequality." *Journal of Politics* 74 (2): 414–26.
La Raja, Raymond J., and Brian F. Schaffner. 2012. "The (Non-)Effects of Campaign Finance Spending Bans on Macro Political Outcomes." Working paper, Department of Political Science, University of Massachusetts, Amherst.
Malbin, Michael J., and Thomas L. Gais. 1998. *The Day After Reform: Sobering Campaign Finance Lessons from the American States*. Albany, NY: Rockefeller Institute Press.
Parker, David C. W., and John J. Coleman. 2004. "Pay to Play: Parties, Interests, and Money in Federal Elections." In Kenneth Goldstein and Patricia Strach, eds., *The Medium and the Message: Television Advertising and American Elections*. Englewood Cliffs, NJ: Prentice Hall.
Primo, David M., and Jeffrey Milyo. 2006. "Campaign Finance Laws and Political Efficacy: Evidence From the States." *Election Law Journal* 5 (1): 23–39.
Richter, Brian Kelleher, Krislert Samphantharak, and Jeffrey F. Timmons. 2009. "Lobbying and Taxes." *American Journal of Political Science* 53 (4): 893–909.
Sitkoff, Robert H. 2003. "Politics and the Business Corporation." *Regulation* 26 (4): 30–36.
Smith, Mark A. 2000. *American Business and Political Power: Public Opinion, Elections, and Democracy*. University of Chicago Press.
Spillman, Lynette. 2003. "National Business Associations, United States, 2003." Ann Arbor, Mich.: Inter-university Consortium for Political and Social Research.
Vogel, David J. 1989. *Fluctuating Fortunes: The Political Power of Business in America*. Washington, D.C.: Beard Books.
Waltman, Jerold, and Sarah Pittman. 2002. "The Determinants of State Minimum Wage Rates: A Public Policy Approach." *Journal of Labor Research* 23 (1): 51–6
Werner, Timothy. 2011. "The Sound, the Fury, and the Nonevent: Business Power and Market Reactions to the *Citizens United* Decision." *American Politics Research* 39 (1): 118–41.
Werner, Timothy, and Kenneth R. Mayer. 2012. "Public Campaign Finance and the Incumbency Advantage." Working paper, Department of Business, Government and Society, University of Texas at Austin.
Werner, Timothy, and Graham K. Wilson. 2010. "Divided but Strong: Business Representation in Washington, D.C." In *The Oxford Handbook of Business and Government*, ed. David Coen, Wyn P. Grant, and Graham K. Wilson. New York: Oxford University Press.

Wilson, Sven E., and Daniel M. Butler. 2007. “A Lot More to Do: The Sensitivity of Time-Series Cross-Section Analyses to Simple Alternative Explanations.” *Political Analysis* 15 (2) 101–23.
Winters, Jeffrey A., and Benjamin I. Page. 2009. “Oligarchy in the United States?” *Perspectives on Politics* 7 (4): 731–51.