The Impact of Money in Politics on Labor and Capital: Evidence from *Citizens United v. FEC* *

Pat Akey Tania Babina Greg Buchak Ana-Maria Tenekedjieva

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Abstract

We examine whether corporate money in politics benefits or hurts labor using the 2010 Supreme Court ruling *Citizens United*, which rendered bans on political election spending unconstitutional. In difference-in-difference analyses, affected states experience increases in both capital *and* labor income relative to unaffected states. We find evidence consistent with increased political spending spurring political competition and the adoption of pro-growth policies. These policies benefit a broader set of constituents as we find a broad-based increase in labor income. Affected states see increased political turnover and reduced regulatory burdens. The economic effects are stronger among ex-ante politically inactive and younger firms.

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Keywords: Labor and finance, Citizens United, money in politics, political spending, labor income, wages, earnings, capital income, profits, political power

* Akey: INSEAD and University of Toronto; pat.akey@rotman.utoronto.ca. Babina (corresponding author): Columbia University and NBER; tania.babina@gsb.columbia.edu. Buchak: Stanford University; buchak@stanford.edu. Tenekedjieva: Federal Reserve Board of Governors; ana-maria.k.tenekedjieva@frb.gov. We thank Emanuele Colonnelli, Michael Gilbert (discussant), Marco Grotteria (discussant), Nandini Gupta (discussant), Jessica Jeffers, Stefan Lewellen, Max Miller, Marc Painter (discussant), Vincent Pons (discussant), Denis Sosyura (discussant), David Sovich (discussant), Ebonya Washington, and Kairong Xiao for their comments, as well as seminar and conference participants at the AFA, the American Law & Economics Association Conference, the ASU Sonoran Winter Conference, Cheung Kong Graduate School of Business, the CICF, the Columbia Women Economists Seminar, Columbia GSB, the EFA, Erasmus University in Rotterdam, Entrepreneurship Junior Group Online Seminars, Gerzensee Summer Symposium, Jackson Hole Finance Conference, Labor and Finance Online Seminar, Maastricht University, the NYU Law School, Stanford GSB, the WFA (Early Career Women in Finance), the WFA, the University of British Columbia, the University of Bristol; the University of Lugano, the University of Luxembourg, and the 19th Annual WashU Finance Conference. The views in this paper should not be interpreted as reflecting the views of the Board of Governors of the Federal Reserve System or any other person associated with the Federal Reserve System.

With all due deference to separation of powers, last week the Supreme Court reversed a century of law that I believe will open the floodgates for special interests ... to spend without limit in our elections. I don't think American elections should be bankrolled by America's most powerful interests.

—Barack Obama (former US President)

In truth, the Court's ruling will have little impact on the typical Fortune 500 company, which can already afford to spend millions of dollars on lobbying and on building PACs with enough employees to fund them and campaign-finance lawyers to operate them. ... What Citizens United actually does is empower small and midsize corporations ... to make its voice heard in campaigns without hiring an army of lawyers or asking the FEC how it may speak.

—Bradley A. Smith (former FEC Commissioner)

Over the last several decades, firms have devoted increasing monetary resources toward political engagement. Indeed, beyond traditional forms of political influence, such as lobbying or the revolving door, the amount of money spent in federal elections has risen from $3.1 billion in the 2000 election cycle to $14.4 billion in the 2020 election cycle, much of it coming from corporate interests.[1] This increase in election spending has been largely attributed to the deregulation of American campaign finance laws and has attracted much attention among policymakers and academics. Most of this related research has focused on the electoral consequences of deregulating spending in elections but has not examined whether deregulating campaign finance has had real economic effects.[2] In this paper, we fill this gap by studying whether allowing more money in politics (i.e., spending in elections) affects the economic outcomes of firms, workers, and total output.

We examine changes in the income flowing to firms—and especially their employees—for several reasons. First, little is known empirically about how workers fare when corporations have a larger influence over the political process.[3] Second, while one can expect firms to benefit from an increased ability to spend money in elections, it is theoretically unclear whether workers would benefit or be harmed. On the one hand, an increase in money in politics may allow firms to obtain economic benefits at the expense of workers or other stakeholders. Indeed, allowing more money in politics may disproportionately benefit special interests, such as politically connected firms, at the expense of politically disengaged groups, such as their workforce, by allowing firms to exert more control over the political process and leading to rent-seeking and distortive policies, as predicted by models

[1] For spending in federal elections, see `https://www.opensecrets.org/elections-overview/cost-of-election?cycle=2020&display=T&infl=N`. Panel (c) of Figure 2 shows that the share of independent political contributions coming from labor interests has been small and declining.

[2] See, among others, Klumpp et al. (2016), Spencer and Wood (2014), and Denes et al. (2022) for research on the political consequences of the deregulation of money in politics. We discuss this literature in Section 1.

[3] Recent research has examined related questions about the importance of managers' political affiliations for employees (e.g., Colonnelli et al., 2022; Babenko et al., 2020), but to our knowledge has not examined how money in politics affects employee income, which is the focus of this paper.

of special interests such as Grossman and Helpman (1996, 2001).[4] On the other hand, increased money in politics can result in greater political competition and pro-growth policies that benefit *both* firms and their employees. In contrast to other forms of political influence such as lobbying or revolving door connections, direct spending in elections offers constituents a way to advocate for their political interests with lower fixed costs of entry. Facing lower entry costs, previously politically unengaged groups, such as smaller or younger firms, may enter the market for political influence, thereby increasing political competition.[5] Increased political competition should lead politicians to cater to a wider set of constituents and to enact policies that grow the metaphorical economic pie rather than simply dividing it differently to benefit narrow, politically connected incumbent interests (e.g., Besley et al., 2010).

We study this question in the context of *Citizens United*,[6] a 2010 US Supreme Court decision that represented one of the largest changes to election campaign finance rules in the post-World War II era. In a surprise 5-4 decision, the court invalidated federal- and state-level laws that placed restrictions on corporate and union spending in elections, which we argue is a means for exercising political influence with a lower entry cost than previously permitted means such as lobbying. These restrictions had limited corporate donors from contributing to organizations that engaged in explicit advocacy in elections. Additionally, compliance with restrictions involved establishing complex structures like Political Action Committees (PACs), which required disclosure of donor identity and incurred high legal and administrative costs. Thus, the removal of these restrictions allowed corporations to participate in political advocacy at a much lower cost.[7] As a result, *Citizens United* led to a huge increase in corporate political spending in elections.

We use this ruling as a natural experiment: in a difference-in-difference design, we examine how the income of employees and firms (their capital providers and business owners, henceforth "capital income") changes in states that had these restrictions in state-level elections invalidated (i.e., the treated states) relative to states that did not have the restrictions in place (i.e., the control states).[8]

[4]For example in a 2018 speech Sen. Elizabeth Warren said, "One of the principal tools rich and powerful people use is dark money. They have created an evasive enemy that slithers out of sight, with only a glimpse here or there. But make no mistake, this dark money has helped shape the anti-teacher, anti-worker agenda that undermines our democracy. For decades, billionaires have been pouring unlimited, secret money into the hands of carefully picked candidates who will do their bidding. We often talk about the influence of dark money and what it has right here in Washington, but the truth is, the real battle is being fought out on the state and local level." https://www.warren.senate.gov/newsroom/press-releases/senator-warren-delivers-floor-speech-condemning-dark-money-in-politics. Consistent with these arguments, existing firm-level studies find that political activity increases firm value (e.g., Cooper et al., 2010; Akey, 2015; Borisov et al., 2016; Bertrand et al., 2020) and that politically active firms may seek to enact laws that reduce competition in labor or product markets (e.g., Faccio and Zingales, 2021; Cowgill et al., 2022; Lancieri et al., 2022).

[5]For example, Blanes i Vidal et al. (2012) and Bertrand et al. (2014) find that lobbyists are able to charge clients a substantial premium to leverage their relationships with influential politicians.

[6]558 U.S. 50 (2010).

[7]While the decision cleared the way for both corporate *and* union engagement, labor unions' share of political spending has been small and fell further following *Citizens United*. Thus, in this paper, we emphasize the deregulation of corporate spending on elections by *Citizens United*.

[8]It is also worth mentioning that we do not attempt to distinguish between "fair" returns to capital and abnormal profits stemming from firm market power that accrues to capital providers in our analysis and focus instead on *total* income flowing to firms' owners and capital providers.

Our main contribution is to show that the large increase in political election spending unleashed by *Citizens United* resulted in increased labor income, which is most consistent with the mechanism of greater political competition and pro-growth policies.

Using state-level income data from the Bureau of Economic Analysis (BEA) and the Internal Revenue Service (IRS), we show that total income (measured either as state-level GDP or adjusted gross income) *increases* by about 2% in states affected by *Citizens United* in the years following the decision. These gains accrue primarily to workers: labor income increases by up to 3% in treated states, and the effect persists for up to six years after the event. We find positive effects of similar magnitude for capital income, but which are often statistically insignificant.[9] These results suggest that money in politics increases aggregate economic output and employment, and that employees (and likely firm owners and capital providers) share in the gains.[10]

An event-study analysis suggests that our results are unlikely to be due to a preexisting differential trend in treated states. Moreover, treated and control states are largely similar in many respects ex-ante: they have similar 2008 Obama vote share, population, GDP, labor and capital income, education, and unemployment levels. Treated and control states do differ in some respects: treated states are slightly more likely to have had a Democratic governor prior to the decision, and control states had somewhat larger exposure to the Financial Crisis (e.g., the magnitude of house price changes pre-crisis). To tackle these differences, we ensure that our results are robust to dynamically controlling for the party of the governor pre-*Citizens United* and for the exposure to the Financial Crisis, as well as for controlling for pre-event state-level GDP growth—all interacted with year fixed effects. Additionally, a propensity score matching approach, which matches treated and control states on the basis of the aforementioned covariates, eliminates these ex-ante differences yet finds almost identical economic effects of *Citizens United*. Moreover, our results are unchanged when we exclude economic outcomes related to oil and gas sector, suggesting that our results are not driven by the boom in shale oil and gas production that occurred following 2010. Finally, our results are robust to implementing a synthetic controls estimation, which explicitly addresses potential concerns about pre-trends.

To offer intuition for our main economic effects, we provide a theoretical framework that shows how the decrease in the costs of political participation afforded by the *Citizens United* to firms can lead to improved economic conditions of politically inactive groups, such as labor. Our model predicts that lowering the cost of entry into the market for political influence will increase the number of constituents who enter political activism, increasing the competition for redistribution. This increased competition for political influence, in turn, drives down the amount of rent that is extracted in equilibrium because redistribution decreases total output, and is, therefore, costly.

[9] Capital income increase might be insignificant for a number of reasons. First, as we show in our theoretical framework in Section 2, it is not clear whether total capital income would increase after *Citizens United* since some newly politically active firms would benefit from the reallocation of rents, while those firms that have been politically active ex-ante might be worse off. Second, capital income is measured with more noise because it is difficult to allocate capital income to states.

[10] We use data from the Quarterly Workforce Indicators and show that both employment and average earnings increase.

The reduction in rent, in turn, increases total output which benefits both the constituents that have now entered the market for influence by giving them a small share of rents from a larger pie, as well as the unconnected constituents (labor) because the pie has become large enough that they gain from its increase.

We then provide empirical evidence that the mechanism highlighted by our model and driving our results is that increased money in politics leads to wider political participation, which in turn results in greater political competition and the adoption of more pro-growth policies. That is, when few firms are able to exert political influence via higher entry costs methods like lobbying, personal connections, or revolving door connections, they push for rent-seeking, growth-reducing policies. With a lower cost of political entry facilitated by increased money in politics, more firms—especially younger and smaller firms—can push for their political preferences, with the net effects being reflected in elected politicians who represent a wider range of constituents and policies that are broadly better for growth. Consistent with this increased political competition interpretation, existing research suggests that *Citizens United* was a large shock to the political status quo. For example, Albuquerque et al. (2020) and Coates IV (2012) argue that *Citizens United* crowded out existing methods of political activism, such as lobbying and revolving door connections, by allowing a wider set of actors to make political expenditures. These authors find that the court decision led firms with a history of political activism to lose value.

In support of this increased political competition and pro-growth policies mechanism, we show that direct political contributions increase in treated states among a broad set of constituents, including small-money donors, rather than being concentrated in historically politically active firms or industries, such as real estate or finance. In response, we find that political turnover—a direct measure of ex-post political competition—among governors and state legislators increases. These changes are not, as is commonly viewed, Republicans taking Democrats' seats. Rather, there is increased across- and within-party turnover among both Democrats and Republicans.

We also find evidence that state legislatures in treated states become less polarized after *Citizens United*, suggesting that newly elected politicians vote in favor of more centrist policies that are likely to appeal to a broader segment of the voter base. These results suggest that well-connected political incumbents are driven out in favor of newcomers who have broader political support. We find evidence that once these newcomers are elected, they enact policies that encourage economic growth. Previous studies have shown that regulations are costly (Kalmenovitz, 2023, e.g.,) and that a lesser regulatory burden may incentivize higher economic activity and lead to higher growth (see e.g., Djankov et al., 2006). Consistent with this idea, we find a more firm-friendly regulatory environment: there are fewer state-level enforcement actions against violations of labor or consumer protection laws in treated states. More broadly, a composite measure of state-level regulatory burden decreases. Moreover, we do not find any changes in adverse worker health outcomes, suggesting that workers do not bear larger non-economic costs of improved economic conditions.

Closing the loop on the increased political competition mechanism, we find that the effects on workers—increased income and hiring—are concentrated in firms that were not political incumbents: the economic effects are larger for firms that were least likely to be politically active before *Citizens United* allowed more money into politics. In particular, we find that younger firms, which are less likely to have been able to form political connections through lobbying and revolving door connections, see greater growth in labor income. Additionally, Compustat firms with no pre-*Citizens United* record of making political contributions or lobbying see the greatest employment growth. Finally, we observe increased labor income in a large cross-section of industries rather than a concentration of growth in politically powerful industries. Taken together, these results support the mechanism that the increased ability of previously politically inactive firms to make political contributions had the effect of increasing political competition, thereby leading to increased political turnover, economic policies that represent the policy preferences of a broader class of economic agents, and ultimately economic growth that accrues to workers, particularly for firms that were least able to participate politically in other ways prior to *Citizens United*.

We consider (and reject) two alternative explanations for our main results. First, since *Citizens United* also removed restrictions on unions' ability to engage in political spending in some states, it is possible that the improved worker outcomes could be driven by unions' increased ability to advocate for pro-worker policies. However, we find that the increase in labor income is similar in states that did or did not have a ban on spending by labor unions (in addition to a ban on corporate spending), suggesting that our results are not due to an increase in unions' political power. Moreover, we find no evidence of a change in labor-friendly policies, such as the minimum wage. The second possibility is that increased economic output could be driven by increased government spending and its macroeconomic multiplier effect. While there was a modest increase in capital outlay in treated states, the effect is far too small to explain our main results without assuming an implausibly large multiplier.

In summary, our paper brings data to the question of which stakeholders benefit from increased money in politics: labor or capital. Our results highlight that the economic outcomes of political policies are not necessarily zero-sum. Increased money in politics can bring a broader set of interests to the table through easier access to political influence, increasing political competition, and bringing new politicians who enact broadly beneficial policies. However, this paper does not provide welfare analysis of increased money in politics, and, hence, one cannot conclude from our analysis that more money in politics is unilaterally better for labor and capital providers or that more money in politics is socially optimal. It is possible that the first-best outcome would be to have a reduced scope for political influence of all forms, including lobbying or hiring via the revolving door, but once some groups have access to politicians it might be economically beneficial to increase the ability of all groups to have access to politicians.

1 Related Literature

Our results contribute to several strands of literature. We contribute to the growing literature on the interactions between labor and finance, which studies the real effects of corporate decisions on workers.[11] Most closely related to our paper is the nascent part of this literature that studies how workers are affected by the actions taken by firms and their managers to promote corporate interests via political influence. For example, managers can pressure workers to contribute to politicians that advance shareholders' interests (Babenko et al., 2020), and individual political views can shape firm behavior and labor market outcomes (Colonnelli et al., 2022). We contribute by documenting that the increased ability of firms to spend money on elections can actually increase labor income.

A large literature examines the value of political connections and studies the various ways in which political connections can benefit firms or foster corruption. One branch of the literature studies the market value of political connections and generally finds that political connections are associated with higher firm values (e.g., Fisman, 2001; Faccio, 2006; Faccio and Parsley, 2009; Goldman et al., 2009; Cooper et al., 2010; Aggarwal et al., 2012; Akey, 2015; Borisov et al., 2016; Brown and Huang, 2020). Another branch of the literature studies the mechanisms through which political connections can benefit firms. Existing work suggests that political connections can help firms secure bailouts (e.g., Brown and Dinc, 2005; Faccio et al., 2006; Duchin and Sosyura, 2012; Behn et al., 2015), enable firms to better access government resources (e.g., Claessens et al., 2008; Goldman et al., 2013), and weaken regulatory enforcement (e.g., Mehta and Zhao, 2020; Tenekedjieva, 2021; Akey et al., 2021; Bourveau et al., 2021; Heitz et al., 2021). Another branch of the literature studies the reasons for and consequences of corruption in government (e.g., Shleifer and Vishny, 1993, 1994; Glaeser and Saks, 2006; Fisman and Miguel, 2007; Smith, 2016; Zeume, 2017; Ellis et al., 2020; Colonnelli et al., 2022; Colonnelli and Prem, 2022).[12] Our paper contributes to this literature by highlighting that increased corporate political activity does not just advance the interests of shareholders, but can also have positive effects on the income of firms' workers.

Our paper also contributes to the literature in law, economics, and political science that studies the various effects of *Citizens United* on political outcomes or firms' responses. A number of papers examine how *Citizens United* affected campaign contributions, electoral outcomes, and policy responses (e.g., Spencer and Wood, 2014; Klumpp et al., 2016; Tenekedjieva, 2020; Gilens et al., 2021; Denes et al., 2022; Slattery et al., 2023). Yet other studies examine the stock price reactions of firms around the date that *Citizens United* was decided (e.g., Werner, 2011; Coates IV, 2012; Burns and Jindra, 2014; Stratmann and Verret, 2015; Albuquerque et al., 2020). To the best of our knowledge, we are the first to examine how the economic outcomes of labor were affected by the increase in political spending ushered by *Citizens United*.

[11]These papers show that corporate decisions on governance, mergers and acquisitions, initial public offerings, diversification, and leverage are important for worker outcomes such as employment, income, and career trajectories (e.g., Atanassov and Kim 2009; Simintzi et al. 2015; Tate and Yang 2015; Brown and Matsa 2016; Mueller et al. 2017; Bai et al. 2018; Graham et al. 2019; Babina 2020; Babina et al. 2020; Baghai et al. 2021). For reviews of this literature see Pagano and Volpin (2008); Matsa (2018); Pagano et al. (2020); Nishesh et al. (2022).

[12]Besley et al. (2010) study how the Voting Rights Act increased political competition by desegregating large parts of the United States and find that long-run economic growth was higher as a result. Our paper differs in several respects. First, we focus on the effects of increased money in politics on the income of employees and capital providers. Second, we study a discrete change in campaign finance rules that had an immediate impact on political competition, rather than the long-run change in political competition that followed desegregation. Finally, we are able to examine a wider set of policies to understand how political competition promotes higher growth.

2 Illustrative Model

In contrast to other forms of political influence such as lobbying or revolving door connections, direct spending in elections by corporations—relaxed by *Citizens United*—offers constituents a way to advocate for their political interests with lower fixed costs of entry. Specifically, the ruling allowed firms to directly spend money on advertisements in elections and to anonymously contribute to groups that advocated for policies of interest to the firms. Spending in such new ways allowed firms to avoid the costs associated with establishing a vehicle like a Political Action Committee, which would also have required them to disclose their political contributions and incur high legal and administrative costs, or to avoid large fixed costs associated with hiring lobbyists and/or establishing revolving door connections. Therefore, we present a stylized model to illustrate that decreasing the costs of entry into the market for political influence can improve the economic outcomes of agents who are not themselves politically connected.

We do so using a simple setting in which two politicians commit to a policy platform about redistribution that influences the consumption of voters. A unit mass of voters indexed by $i \in [0,1]$ vote for one of two politicians. Before voting, each politician, A and B, commits to a redistributive policy $\mathbf{r}^A \equiv \{r_i\}^A$ and $\mathbf{r}^B \equiv \{r_i\}^B$ that determines each voter's share of aggregate output. The policy being redistributive means that a politician can only allocate more towards one voter group by taking away others' consumption, so the sum of the redistribution shares across groups must be zero:

$$\int_i r_i di = 0.$$

We assume that redistribution is costly, and deviations from the *laissez-faire* policy, $r_i \equiv 0 \ \forall i$, cause reductions in aggregate output. The distortion due to policy $\mathbf{r}$ is

$$d(\mathbf{r}) \equiv \int_i \delta(r_i) di.$$

with $\delta(0) = 0$ and $\delta''(r_i) > 0$. The distortion reduces aggregate output to $A(d)$, with $A(0) = A_0$ and $A'(d) < 0$.[13] For notational convenience we henceforth write output directly as a function of the policy, $A(\mathbf{r})$, (rather than $A(d(\mathbf{r}))$. Under policy $\mathbf{r}$, voter i's consumption is equal to $(1 + r_i)A(\mathbf{r})$.

At the start of the game, voters choose whether to pay a fixed cost c to become *politically connected*. Politician A then chooses a policy $\mathbf{r}$ that maximizes the consumption of these politically connected voters. To simplify exposition, we assume that politician B simply commits to implement the *laissez-faire* redistribution policy. Voters then vote for the politician whose policy maximizes their consumption, and the politician with the majority voter share wins (i.e., the politician who received 50% of the eligible votes). Production, redistribution, and consumption then occur in accordance with the winner's redistribution schedule.

[13] For example, $\delta(r_i) = r_i^2$ and $A(d) = A_0(1 - \beta d)$ where β is a constant parameterizing the output cost of the distortion.

We study an equilibrium characterized by the tuple (p, r_p, r_s, r_o), where p is the endogenous measure of voters who choose to become *politically connected*, and r_p, r_s, and r_o define the redistribution policy towards the politically connected, outsider supporters, and outsider opponents, respectively. In this equilibrium, redistributive politician A wins, and three groups of voters form endogenously: (1) a measure p of *politically connected* voters, (2) a measure $1/2 - p$ of *outsider supporters* who are not politically connected but vote for politician A, and (3) the remaining measure $1/2$ of *outsider opponents* who are not politically connected and vote for politician B. We provide a more careful interpretation of these three groups below, but we interpret them roughly as "connected capital providers," e.g., owners of large firms, "unconnected capital providers," e.g., owners of small firms, and "labor." These four equilibrium quantities are pinned down by the solution to politician A's problem and the entry condition for the politically connected. The politician's problem is

$$
\begin{aligned}
&\max_{r_p, r_s, r_o} p(1 + r_p)A(\mathbf{r}) \\
&s.t. \\
A_0 &\leq (1 + r_s)A(\mathbf{r}) && \text{(Outsider supporters back A)} \\
0 &= (p)r_p + (1/2 - p)\, r_s + (1/2)r_o. && \text{(Policy feasibility)}
\end{aligned}
$$

The entry condition is

$$
(r_p - E[r_{\sim p}])\, A(\mathbf{r}) = c.
$$

The requirement that outsider supporters vote for politician A ensures that A wins the election, as the measure p of politically connected voters plus the measure $1/2 - p$ of outsider supporters gives A exactly $1/2$ of the vote share (and we assume that the tie is broken in favor of A, or equivalently that $1/2 - p + \epsilon$ are in the outsider supporter group). The entry condition requires that the expected consumption benefit of becoming politically connected equals the cost of becoming connected. $E[r_{\sim p}]$ is the expected redistribution policy for the non-connected.[14]

The intuition is as follows: politician A redistributes consumption away from half of the voters without regard to their utility. As a result, they (outsider opponents) will vote for B, and additional redistribution would not alter their vote. The redistributive politician A then redistributes towards unconnected supporters to the point where they become exactly indifferent between supporting A and supporting B's *laissez-faire* policy. Politician A then distributes the remainder to his politically connected supporters. Redistribution towards the politically connected is disciplined in two ways: first, additional redistribution directly reduces output, and therefore as redistribution increases, the politically connected receive a larger share of a shrinking pie. Second, as total output shrinks, the politician must divert a growing share of resources towards the non-politically connected supporters

[14] In our numerical examples, we assume that non-connected are assigned randomly into the unconnected supporters and the unconnected opponents according to the population masses, so that $E[r_{\sim p}] = \frac{(1/2-p)r_s+(1/2)r_o}{1-p}$, but this assumption is innocuous for the qualitative takeaways from the model.

to retain their votes, which further reduces the resources available for the politically connected voters. Put differently, as total output shrinks, politician A must redistribute *even more* of the shrinking pie to retain the support of outside supporters since there is higher competition for their support.

Our main comparative static is over the fixed cost of becoming politically connected, c. Figure 1 illustrates c's impact for a particular set of parameters.[15] Subplot (a) shows that as the cost of political connections falls (the horizontal axis labeled "Entry cost"), the additional voters choose to become politically connected (the vertical axis labeled "Participation"). As more voters choose to become politically connected, the equilibrium distortion of output falls (subplot (b)), and output increases (subplot (c); in both subplots, the horizontal axis represents the proportion of politically connected voters, denoted as "Special interest participation"). Finally, the share of output going to the *unconnected* opposition voters *increases* (subplot (d)) because the aggregate output is higher and the distortion is lower.

Figure 1 subplot (e) illustrates the key intuition in going from a high (red) to a low (blue) entry cost equilibrium. The politically connected voters can increase their consumption in two ways: either through more redistribution (essentially getting a "sufficiently large" piece of a smaller pie) or through an increase in output, driven by a less aggressive redistribution policy (essentially getting a small piece of a "sufficiently large" pie). When only a small share of voters are politically connected, political insiders prefer additional redistribution because redistribution is shared among a relatively small voter group; while in contrast, additional aggregate output must be shared among all voters. As politically connected voters comprise a larger share of the economy, additional redistribution must be shared by a larger group, while less aggregate economic output is "lost" to outsiders. As output grows, even politically unconnected voters benefit. As more voters become politically connected, politically connected voters' preferences more closely align with those of society at large. In sum, the plot illustrates the key result of our model: the switch from higher to lower entry costs for political influence can result in the increased consumption of the politically inactive group of voters. Finally, in the limit as *all* voters become politically connected, the optimal redistributive policy approaches the *laissez-faire* policy that maximizes aggregate output. The dashed lines in Figure 1 subplots (b)–(d) indicate the *laissez-faire* levels of distortion and output.

We map several aspects of this framework to our empirical setting. Most fundamentally, we use the *Citizens United* ruling as a negative shock to the cost of becoming politically connected, c, and study a variety of political and economic outcomes that change as a result of the plausibly exogenous decrease in c. As we will discuss in further detail in Section 3, *Citizens United* allowed for a wider set of forms of political spending to be undertaken, which were lower cost than existing forms of influence seeking such as hiring from the revolving door or engaging professional lobbyists, and as such, reasonably approximates a decline in c. Our first set of tests examines the outcomes of different groups of constituents after the *Citizens United* decision. We focus on the economic

[15] $\delta(r_i) = r_i^2$, and $A(d) = (1 - 4d)$. Our results here are intended to communicate the intuition of our mechanism rather than fully characterizing the results in the most general setting.

outcomes of labor as an empirical proxy for what we believe to be the starkest prediction of this model: that decreasing c can improve the economic outcomes of politically *unconnected* constituents. In practice, it is difficult to precisely identify the entire set of constituents that would have been politically connected prior to *Citizens United* since there are many possible ways of establishing political connections such as campaign contributions, spending money on lobbyists, or hiring from the "revolving door." Therefore, we focus our analysis in Section 3.2.1 on the economic outcomes of workers since it is likely that, as a group, the employees of firms are unlikely to have been politically connected.[16] We find substantial empirical evidence using a variety of databases that labor income increased.

We interpret some capital providers as having been politically connected before *Citizens United*, while others became politically connected as a result of *Citizens United*. In this case, those capital providers who became politically connected would benefit from *Citizens United*, while those who were already politically connected would be worse off. Precisely identifying those capital providers that entered the market for political influence for the first time is not feasible since not all types of political activity are observable to the econometrician, particularly for state-level politics, which is the focus of this paper. On net, we expect capital income to not decrease and potentially to increase. Consistent with this conjecture, we find some evidence that capital income is higher, although the evidence is statistically weak. Finally, our model predicts higher economic output with increased political participation and a reduction in distortions: consistent with this, we find higher total output.

The model also illustrates two qualitative takeaways about the mechanism through which a decrease in c can increase the economic outcomes of the politically unconnected constituents: an increase in political participation (and therefore an increase in competition for redistribution) and a change in policies. Intuitively, if *Citizens United* decreased the fixed cost of political activism, our model would predict that there would be an increase in political participation coming from new entry into the market for political influence. We examine this prediction in Section 5, where we find that political contributions increase among a wide variety of interests, not simply among those that were likely to have been politically active before *Citizen United*. In this analysis, we find that the largest percentage increase in contributions comes from small or first-time donors, which is consistent with *Citizens United* spurring first-time entry into the market for political influence. Consistent with this increased political participation, we find that the improvement in economic outcomes is larger among ex-ante politically inactive firms.

The second qualitative takeaway illustrates that lowering the cost of entry for political influence can change policies to be less distortive and more growth-oriented. We examine whether economic policy changed after *Citizens United* in Section 5. While specifically identifying policies that are more or less rent-seeking in nature is not feasible, we do show that state-level economic policies change in states affected by *Citizens United*, and in particular, result in lower regulatory burden.

[16] While labor unions are politically active, we specifically examine whether increased political power of unions could explain our result and find no evidence that this is the case.

Existing work provides some evidence that regulations are costly (e.g., Kalmenovitz, 2023) and often benefit politically connected incumbent firms (e.g., Benmelech and Moskowitz, 2010; Faccio and Zingales, 2021; Neretina, 2019). Thus, the decline in regulatory burden, increase in political participation, higher labor income and economic growth are broadly consistent with the mechanism that we illustrate in our model.[17]

In sum, our model predicts that when the fixed cost of entry into the market for political influence or rent-seeking is lower, there is an increase in the number of constituents who enter this market, increasing the competition for rent or redistribution. This increased competition for political influence, in turn, drives down the amount of rent that is extracted in equilibrium because redistribution decreases total output, and is, therefore, costly. The reduction in rent, in turn, increases total output which benefits both the constituents that have now entered the market for influence by giving them a small share of rents from a larger pie, as well as the unconnected constituents because the pie has become large enough that they gain from its increase.

3 Institutional Background, Data, and Empirical Strategy

In this section, we provide information about the institutional background of *Citizens United* and why we believe it is an appropriate setting to study a decrease in the cost of entry into the market for political influence. We then introduce the data that we use for our study and discuss our empirical design.

3.1 Institutional Background

Money in politics in the United States is regulated at the federal, state, and, in some cases, the municipal level by a variety of government agencies. At the federal level, the Federal Elections Commission (FEC) is responsible for the enforcement of campaign finance restrictions regarding candidates for federal elections, while the body or bodies responsible for enforcing state-level restrictions on candidates for state elections depend on the particular state. The federal government has limited ability to regulate state-level elections, and individual state legislatures can implement restrictions on campaign financing in their states, provided that these laws do not infringe on rights that are articulated by their state constitutions or by the US Constitution.

Our empirical setting focuses on the effect of the *Citizens United v. Federal Election Commission* decision which was handed down on January 21, 2010 by the US Supreme Court. The Court ruled that restrictions on independent political expenditures by corporations (including nonprofit corporations) and labor unions are unconstitutional. The Federal Elections Commission defines independent political expenditure as that used for communication (e.g., political advertisement) that expressly advocates for the election or defeat of a clearly identified candidate ("electioneering") and that is not made in coordination with any candidate or her authorized agents.

[17] A decline in regulation could have a direct and negative effect on labor through worse working conditions. However, we examine a variety of non-pecuniary outcomes that employees are likely to be concerned with and find no evidence that they are worse off along these non-monetary dimensions.

Practically, this decision had two important consequences on the regulation of money in politics. The decision directly struck down two provisions of the Bipartisan Campaign Reform Act of 2002 (BCRA), a federal campaign finance law, and indirectly rendered 23 individual state-level campaign finance restrictions unconstitutional because of the broadness of the court ruling.[18] The empirical design of this paper focuses on the second of these consequences—the unexpected removal of individual state restrictions on independent political spending on state-level political campaigns. By striking down these restrictions, the Supreme Court removed much of the uncertainty around what constitutes electioneering and made it much easier for companies to spend money on political campaigns. That is why within the framework that we propose in Section 2, we can interpret *Citizens United* as a negative shock to the cost of being politically connected, offering firms the ability to advocate for their political interests with lower fixed costs of entry as compared to lobbying that requires large costs to set up lobbying operations by hiring lobbyists and lawyers.

The question at the heart of *Citizens United v. FEC* was whether Citizens United, a conservative non-profit, should have been allowed to advertise and broadcast a political documentary that it had created with the support of corporate donors that was critical of Hillary Clinton, without disclosing its donors. The BCRA prohibited corporations and unions from using funds from their general treasuries to fund "electioneering communication" (e.g., political advertisement) 30 days before a primary or 60 days before a general election and required that donors who funded this type of advertisement be disclosed.[19] Citizens United had been prevented from advertising and airing the documentary as it wished due to these provisions of the BCRA, so it sued the Federal Elections Commission, and the case was eventually heard by the Supreme Court of the United States. This case also illustrates how blurry the line of what constitutes electioneering were, and how careful corporations had to be to ensure they did not violate the FEC rules.

In an unanticipated 5-4 decision that was unexpectedly broad, the justices determined that electioneering communication was protected under the First Amendment of the US Constitution, and that the BCRA provisions that prohibited corporations and unions from using funds to fund these types of advertisements were unconstitutional. Moreover, although the Court upheld the BCRA provisions that require for-profit corporations and union funders to be disclosed, the ruling did not require that funders of "social welfare" non-profits, like Citizens United, be disclosed.[20] Since many states had enacted state-level restrictions for state elections that were similar to these provisions of the BCRA—which only applied to federal elections—the *Citizens United* decision effectively ruled that the state-level bans were also unconstitutional. It is worth noting that most states had enacted these bans a long time before *Citizens United*. Indeed, the first ban was enacted in 1908, the most recent ban was enacted in 2007, and the median ban was enacted in 1978; thus, the

[18] There are still a number of restrictions on the ability of individuals or corporations to make campaign contributions *directly* to politicians. Rules about *direct* contributions (i.e., not independent) either to federal politicians or to state politicians were not affected by the *Citizens United* decision.

[19] Electioneering communication was defined as (1) a broadcast advertisement on television or radio that (2) refers to a federal candidate that (3) airs within thirty days of a primary election or 60 days of a general election and that (4) can reach an audience of 50,000 or more (Spencer and Wood, 2014).

[20] "Social welfare" non profits are typically organized as an IRS 501(c)4 organization.

enactment of individual state-level bans was not caused or affected by the BCRA rules themselves. Figure 2, Panel (a) shows which states were affected by the ruling.

This ruling had the immediate effect of establishing a new vehicle for political spending—the "Super PAC" or independent-expenditure-only political action committee (PAC). Super PACs are entities that can receive unlimited amounts of money from corporations, unions, or individuals and can spend this money advocating for or against specific political candidates, but which must remain independent of the PAC of a politician that she endorses (politicians can endorse a specific PAC as their preferred PAC, and such preferred PACs are often run by former advisors of the politician that they support).[21] The number of Super PACs grew quickly following *Citizens United*. As Figure 2, Panel (b) shows, starting with 2010 and in the next full election cycle following *Citizens United*—2012—conservative-aligned Super PACs spent nearly $500 million and liberal-aligned Super PACs spent nearly $250 million. This number has dramatically increased since then.

Citizens United also led to the emergence of non-profit political activism by "social welfare" non-profits. While non-profits are prohibited from engaging in political activity as a substantial portion of their activities, they have become an important force in issue-based advertising on topics that are politically charged (e.g, abortion rights, gun ownership rights) (e.g., Chand, 2014). Social welfare organizations (as with all other non-profits) are not required to disclose their donors or members. Put simply, *Citizens United* allowed for new ways for firms, unions, non-profits, and individuals to spend money in politics with substantially less disclosure.[22]

3.2 Data

We combine data from a variety of sources for our analysis. Our sample spans 2005–2016 where possible.[23] Additionally, we collapse the data for political variables (e.g., advertising spending or the identity of the governor) into two-year election-cycle time periods, while we analyze economic variables on an annual basis. Table 1, Panel A provides summary statistics on the variables described below, and Appendix A provides variable definitions.

3.2.1 Economic Variables

The Bureau of Economic Analysis (BEA): Our main economic outcomes come from the Bureau of Economic Analysis's Regional Economic Accounts. The BEA provides, at the state-year level, data on state gross domestic product (GDP), which are further disaggregated into

[21] Technically, the rules establishing Super PACs were formalized after a DC Circuit Court of Appeals case, *Speechnow.org v FEC*. However, the Speechnow.org case effectively formalized the legal rulings of *Citizens United*.

[22] While *Citizens United* impacted both corporations and unions, union political contributions have been a relatively small share of independent political contributions in Federal elections at least since 2004, where they comprised roughly 10% of total independent political spending. (See Figure 2, Panel (c).) This share has only decreased since *Citizens United*, and following the decision, independent political contributions by unions have comprised roughly 5% or less. Thus, while technically *Citizens United* was a shock to both corporate and union spending, for this paper we focus primarily on the corporate aspect.

[23] Some datasets begin later. Additionally, some datasets have incomplete coverage across all 50 states during the entire sample period. These two factors are reflected in the varying number of observations in subsequent tables.

employee compensation (includes wages, salaries, and other benefits earned by employees) and gross operating surplus (includes income earned by corporations as well as individual or joint business entrepreneurs; also includes other income earned by capital providers.).[24] Since we are primarily interested in the outcomes of employees, we take employee compensation as our measure of labor income ("Labor income" variable in all subsequent analyses), and gross operating surplus as our measure of capital providers/business ownership income ("Capital income" variable for short). The first key advantage of the BEA data is that employee compensation captures only the labor income we are primarily interested in—that of employees—and excludes the income of entrepreneurs, who represent business owners.[25] The second advantage is that income is apportioned according to where the underlying economic activity takes place.

Internal Revenue Service (IRS): For robustness, we supplement the BEA data with the IRS's published Statistics of Income (SOI). The SOI reports, at the aggregated zip code-year level, various components of taxable income, including adjusted gross income (AGI), salary and wage income (the most relevant variable to our study focused on the outcomes of employees), interest income, dividend income, business income, and capital gains. We aggregate the data to the state-year level. From the IRS data, we calculate analogs to the BEA data on total income, labor income, and capital/business ownership income, as follows: we proxy GDP with AGI; we use salary and wage income to measure employee income ("SW" variable in all subsequent analyses); we proxy capital/business ownership income as AGI less salary and wage income ("AGI - SW" variable).

There are a few drawbacks of the IRS income relative to other measures that particularly impact the measure of capital providers/business ownership income. First, the tax base is generally smaller than the actual income earned by various factors of production. This is due to, for example, carried forward losses and other exemptions. Second, income is apportioned according to where the taxpayer lives rather than where the economic activity leading to the income occurs, which will matter if, for example, a filer owns the stock of a company operating in a different state. Third, the timing of realized capital gains may differ from when income was actually earned.[26]

Quarterly Workforce Indicators (QWI): As additional robustness, we use the US Census's Quarterly Workforce Indicators dataset, which is a publicly available aggregation of the longitudinal firm-worker matched microdata covering roughly 95% of US private sector jobs. The QWI reports, among other things, employment counts, average monthly earnings, and total payrolls at the state- and state-industry levels. Additionally, the QWI shows heterogeneity by firm characteristics, such as firm size and age. Thus, the QWI provides year-state panel on employment and payments to employees (overall payroll and average wages) by firm characteristics and industry, supplementing our main BEA dataset.

[24] The BEA's calculation methodology is described here: https://www.bea.gov/sites/default/files/methodologies/0417_GDP_by_State_Methodology.pdf.

[25] Since we are primarily interested in the income of salaried employees and use it as our key dependent variable, the methodological issues of precisely estimating the labor share of income are less relevant here than in other work focusing on labor share.

[26] Consistent with these issues, the average AGI from the IRS is lower than the average GDP from the BEA, and this is primarily driven by differences in capital income.

Government spending data: We obtain data on state tax receipts and spending from the Annual Survey of State and Local Government Finances provided by the US Census.

Compustat data: While most tests rely on aggregated economic data to ensure that we are capturing the effect of *Citizens United* on both public and private firms, we use data on publicly traded firms from Compustat for some cross-sectional tests. We obtain data on employment, size, leverage, cash, and Tobin's q. We complement these data with historical headquarters data from the Loughran/MacDonald database.[27]

3.2.2 Political Variables

Independent expenditure bans: We identify states that had bans on state-level election campaigns pertaining to corporate and/or union independent expenditures that were ruled unconstitutional by *Citizens United* using the information provided by the National Conference of State Legislatures.[28] Panel (a) of Figure 2 presents a map that shows 23 states that had those bans declared unconstitutional.[29] The states that had a ban on independent political expenditures and were therefore affected by the *Citizens United* decision are treated states (variable called "Treated"), while those states that did not have a ban serve as control states.

Party control and elections: We hand-collect data at the state-year level on the party that controls the governor's seat, the lower legislative chamber (typically called the state House of Representatives), and the upper legislative chamber (typically called the state Senate) from several sources: the National Conference of State Legislatures, states' election websites, and Wikipedia. In a given state and two-year election cycle observation, the likelihood that Republicans control the governor's seat is 54% and that Republicans control the state Senate and House legislative chamber is 51% and 50%, respectively (see Table 1, Panel A).

Polarization: We use political polarization measures of state's legislative chambers estimated by Shor and McCarty (2011).[30] The authors construct ideology scores for individual state legislators using data on politicians' votes on bills and their responses to surveys about political ideology using an "ideal point" estimation to capture each legislator's political preferences. Each politician is given a numerical score that indicates how far to the "left" or "right" they are given their observed voting behavior. This allows us to compare polarization across states and years. The closer a legislative chamber's polarization measure is to 0 ("House differences" and "Senate differences" as reported in Table 1), the more bipartisan the ideology of its members.

[27] `https://sraf.nd.edu/`

[28] Klumpp et al. (2016) use the same information source. It can be accessed at `https://www.ncsl.org/research/elections-and-campaigns/citizens-united-and-the-states.aspx`. As in Klumpp et al. (2016), we do not classify Alabama as treated because the ban only applied to state referenda.

[29] These states are Alaska, Arizona, Colorado, Connecticut, Iowa, Kentucky, Massachusetts, Michigan, Minnesota, Montana, New Hampshire, North Carolina, North Dakota, Ohio, Oklahoma, Pennsylvania, Rhode Island, South Dakota, Tennessee, Texas, West Virginia, Wisconsin, and Wyoming.

[30] A long tradition in political science has used ideal point estimation. Seminal papers include Poole and Rosenthal (1985), Poole and Rosenthal (1991), and Poole and Rosenthal (2000). Recent research in financial economics has adopted the methods that underlie the approach to estimate the voting ideology of institutional investors (Bolton et al., 2020).

Independent political expenditures: Many states do not have disclosure requirements for independent political expenditures. To show that *Citizens United* affected independent political expenditures, we collect data on independent expenditures in federal elections from the Center for Responsive Politics (Open Secrets), a non-profit that provides data about money in federal politics.

State-level political contributions: We obtain data about direct political campaign contributions to candidates for state-level political offices from the National Institute for Money in State Politics.[31]

Federal-level lobbying expenditure and political contributions: We obtain data about firms' political contributions and lobbying expenditures at the federal level from the Center for Responsive Politics (Open Secrets). To study the differential effects of *Citizens United* on politically active versus inactive firms, we match these expenses to the set of firms included in the S&P 500.

Political advertising: We obtain data on political advertising from Ad$pender. Ad$pender tracks advertising expenditures across media avenues (e.g., television, radio, magazines, internet advertising, and others),[32] media markets, and years. Ad$pender reports data at the media-market level, which corresponds approximately to a city or an MSA. We aggregate market-level political ad spending to the state level (variable "Ad spending ($M)"). Note that not all states contain a media market, so advertising data are missing for some states.

3.2.3 Other Data

Violation and subsidy data: We obtain data on violations of state and federal laws as well as data on federal and state subsidies from *Good Jobs First,* a non-profit advocacy group that compiles a number of databases related to corporate and government activities. Violation data come from the organization's Violation Tracer database which contains enforcement actions from both federal and state enforcement agencies on topics related primarily to the following: banking; consumer protection; environmental; wage and hour violations; unfair labor practices; health and safety; and workplace discrimination. Subsidy data come from the organization's Subsidy Tracker database, which aggregates data from numerous federal, state, and local government websites. The database includes more than 500,000 state- and local-level subsidies that aggregate to nearly $300 billion dollars during our sample period. These subsidies take a variety of forms including government grants, tax incentives, and cost reimbursements.

Minimum wage data: We obtain minimum wage data used in Gopalan et al. (2021). The authors hand-collected data on each state's minimum wage in a given year to construct the sample.[33]

Tax rate data: We obtain a variety of state tax rates (e.g., sales tax, corporate tax, top income tax, property tax, and the presence of an estate tax) from Baker et al. (2021). These data use state

[31] https://www.followthemoney.org/

[32] Television includes network, cable, spot, Spanish-language network, and syndicated expenditures; radio includes network, national spot, and local expenditures; magazines includes consumer, business-to-business, local, Sunday, and Spanish-language expenditures; newspapers includes national, local, and Spanish-language expenditures; internet expenditures; outdoor expenditures (e.g., billboards).

[33] We thank the authors for sharing these data with us.

and county data to arrive at effective tax rates for residents in a state.

Licensure data: We obtain occupational licensing data from Sorens et al. (2008), who create occupation-year-state licensing data on 39 occupations. The authors use national BEA estimates of the number of employees in a given occupation to estimate state-year employment-weighted licensure requirements, where higher values of the index correspond to more burdensome regulation.

Regulatory freedom data: We obtain the Cato Institute's index of regulatory freedom, which is based on over 50 legal and regulatory observable indicators collected by Sorens et al. (2008) and aggregated by the Cato Institute in their publication *Freedom in the 50 States*. This index covers state-level policies on the freedom of seven categories: land use, labor market, non-federal health insurance, cable and telecom, occupational licensing, lawsuit environment, and miscellaneous. Each policy is weighted by estimates of its cost, and the final index has a mean close to 0 and a standard deviation of 0.13 (variable "Regulatory freedom"), with higher values of the index corresponding to a lower regulatory burden in each state.

Demographic and other data: We obtain demographic data on population, median household income, education, and unemployment from the 2010 Census. We obtain house price changes from the FHFA. We obtain mortgage delinquencies from Corelogic LLMA.

3.3 Empirical Strategy

We implement a standard differences-in-differences estimation using the following equation:

$$Outcome_{st} = \beta Post_t \times Treated_s + \gamma_s + \gamma_{tg} + \gamma_{tp} + \epsilon_{st}. \tag{1}$$

where s indexes state, and t indexes time; $Outcome_{st}$ represents an economic, political, or policy outcome for state s in time period t. $Post_t$ is an indicator variable that takes the value of one in time periods when the *Citizens United* decision could have affected an economic, political, or policy outcome, and is zero otherwise. Since the decision took place on January 21, 2010 (at the beginning of an election cycle with the highest proportion of state races), this indicator variable will "switch on" in 2010 for political variables such as campaign contributions. Since there is a lag between political elections and policies being put in place, this indicator variable will "switch on" in 2011 for policy outcomes, such as regulatory outcomes or licensing requirements. Since economic outcomes such as hiring and investment decisions are made by agents that should be forward-looking, the variable will "switch on" in 2010 for economic outcomes, since the court decision was early in the year. As such, the effect that we estimate will include responses that are driven by *actual* policy changes and *expectations* about policy changes.[34] $Treated_s$ is an indicator that takes the value of one for the 23 states that had previously adopted a ban on independent political expenditures in state-level political elections—a ban that was invalidated by the *Citizens United*, and is zero otherwise.

We include state fixed effects (γ_s). Additionally, we include γ_{tg}, or the fixed effects for state-level

[34] Our estimation results are similar if we instead "switch on" the effect in 2011 for all outcomes.

growth pre-*Citizens United* (from 2005 to 2009) interacted with time fixed effects (label "Year $\times$ Pre-CU GDP Growth FE" in all tables)—these interacted fixed effects allow states with different growth patterns prior to *Citizens United* to follow differential time trends and also absorb standard time fixed effects; given the CU ruling became binding following the Financial Crisis, adding these fixed effects allows to address a concern that treated and control states differ in their response to the Financial Crisis, and the post-CU growth is driven by this response and not by the CU ruling itself. γ_{tp} are year-by-party fixed effects (label "Year $\times$ Pre-CU Gov Party FE") that allow states that had governors of different political parties in the election cycle prior to *Citizens United* to follow different time trends;[35] these fixed effects allow to control for post-CU changes in state economic outcomes to be driven by different types of policies associated with each political party and implemented prior to the CU ruling. Note these latter two sets of fixed effects are conservative in that they specifically rule out the possibility that a few states that happened to have bans overturned by *Citizens United* and continued along a higher growth path, or that states with different political pre-*Citizens United* executive branches were growing at different rates. However, these two sets of fixed effects are not critical to our estimation: we obtain very similar results if we include only state and year fixed effects, as is commonly done in state-level differences-in-differences analysis. Our difference-in-difference sample runs from 2005 through 2016, data permitting. We cluster standard errors by state in all of our analyses.[36]

We also use standard event-study analysis to estimate the effect of the *Citizens United* case dynamically over time as follows:

$$Outcome_{st} = \sum_{\tau=2005}^{2016} \beta_\tau (I_{t,\tau} \times Treated_s) + \gamma_s + \gamma_{tg} + \gamma_{tp} + \epsilon_{st}. \tag{2}$$

where all variables are defined as in Equation (1). In this estimation, β_τ measures changes in the outcome variable in the treated states as compared to the control states year by year. The omitted time period is generally 2009, the last year in which *Citizens United* would have had no political effect. Compared to Equation (1), this specification allows us to examine both the possible existence of pre-trends as well as the timing of the changes after the *Citizens United* decision.

The underlying assumption of our specification is that the treated and control states would have been on similar trends after the court case in the absence of this treatment. While this assumption is fundamentally untestable, we show below with our dynamic analysis, that the treatment and the control states plausibly follow parallel trends before the treatment. However, one potential concern is that the treated and the control states might have some other characteristics that could send these states on differential trends following the treatment. To examine this, we compare the characteristics of the treated and control states at the time of the court decision to alleviate concerns that the two groups of states are fundamentally different or have low covariate balance,

[35] Specifically, we control for the cycle year-governor's party as of the beginning of 2010, which would have been the last pre-*Citizens United* governor. As of the 2010 cycle, 28 states had a Republican governor, while 22 states had a Democratic governor.

[36] Our results are robust to double clustering by state and year.

as suggested by Atanasov and Black (2021).

Table 1, Panel B compares political, economic, and demographic characteristics of the two groups of states around the time when *Citizens United* was decided. This table shows that the treated states had a similar share of voters for Obama in the 2008 presidential election. However, these bans may predominately have been found in Democratic states (that might have favored such regulations) using other measures, such as the share of Republican governors, which may have different economic fundamentals or demographic characteristics causing them to evolve on different paths following the case. To address this concern, we dynamically control for the party of the state's governor right before the Supreme Court case in all specifications. In practice, however, the addition of this control has no impact on our results.

Additionally, treated and control states differ in their exposure to the Financial Crisis: credit conditions and housing prices are modestly different between the two groups. Housing prices had a higher run-up prior to the Financial Crisis (and a correspondingly higher crash) in control states, along with a higher probability of households being delinquent on loan repayments. However, these differential outcomes are driven by Florida and Nevada, which were the hardest hit by the Subprime Crisis. In unreported results, we remove these states and find similar results. More generally, as we show later, we find similar results when we control for the states' exposure to the Financial Crisis.

Beyond these differences, treated and control states are relatively similar. The demographic characteristics are similar between the two groups of states: on average, states have similar population sizes, median household incomes, and education levels. Unemployment rates do not significantly differ between the two groups. Moreover, pre- *Citizens United* economic outcomes—such as (log) GDP, labor income, and capital income—do not vary significantly across treated and control states, nor there is a significant difference in the growth of these variables prior to the court's case.

Finally, for additional robustness in Appendix B, we implement a propensity score matching estimator. The matching procedure fully removes the ex-ante differences in covariates, and we find largely similar results in our main specifications. Similarly, we show that our results are essentially unchanged when we implement a synthetic control estimation in Figure C.2 to explicitly address potential concerns about pre-trends.

4 Main Results: Economic Consequences of *Citizens United*

We examine how the income of firms and their employees change after *Citizens United* using data from the BEA for our main estimates and the IRS for robustness—both described in Section 3.2.1. The difference-in-difference results are shown in Table 2, Panels A (BEA) and B (IRS). The event studies are shown in Figure 3, with Panels (a), (c), and (e) showing BEA outcomes and (b), (d), and (f) showing IRS outcomes. Beginning with the difference-in-difference results, Column (1) in both panels of Table 2 shows that in treated states following the court ruling, total output increases by about two percentage points, although the result is only statistically significant in the IRS data. Column (2) in both panels shows that capital/business ownership income—noisily measured—increases between 1.4 and 3.7 percentage points. Column (3) in both panels shows that labor income increases by an economically and statistically significant 2 to 3 percentage points in treated states following the court ruling. We note that it is harder to attribute capital/business income than labor income to states, which—combined with a panel of only 50 states—means that the tests that examine capital income may be somewhat underpowered. While it is difficult to assess the expected economic magnitude that *Citizens United* might have had on labor or capital income, the observed effects might seem large. However, it is worth noting that the firm-level literature that examines the returns to political activism generally finds that political connections have large effects on firm outcomes. For example, Brogaard et al. (2021) find that $1.4 trillion in US federal contract renegotiations were preferentially given to politically connected firms from 2001–2012.[37]

Examining the event studies in Figure 3, Panels (e) and (f) show a clear gradual increase in labor income following the court ruling, with the effect persisting through the entire sample period. We find similar patterns in overall output in Panels (a) and (b), though with higher standard errors in some years. Panels (c) and (d) show a noisy increase in capital income.

As we discussed in the section on empirical methodology, we include year times pre-*Citizens United* GDP growth and year times pre-*Citizens United* governor party fixed effects to ensure that our results are not due to a subset of states that are in the treatment group growing faster due to differences in the pre-*Citizens United* growth or political parties. We include these fixed effects to be conservative. However, as shown in Internet Appendix Figure C.1 and Internet Appendix Table C.1, our results are essentially unchanged if we include only year and state fixed effects, as is commonly done with single event differences-in-differences studies.

Collectively, these results suggest that labor income increases when political spending becomes less regulated. Although we cannot conclude with precision that capital income increases, we find no evidence that the increase in labor income comes at the expense of income to capital providers. Moreover, increased labor income is associated with the overall increase in economic activity, as measured by state-level GDP. These results are in line with the main takeaway from our framework in Section 2. We find that the income of the politically *unconnected* group—labor—increases when the cost of entry into the market for political influence decreases, the income of the politically connected group—capital—is non-decreasing, and the overall economic conditions improve.

Robustness Checks and Alternate Specifications: We undertake several additional robustness checks around our main economic results. Although our event study graphs show that labor and capital incomes generally increase in treated states after *Citizens United*, a couple of figures exhibit mild pre-trends. We follow two additional approaches to ensure that our results are not driven by unobservable characteristics that may have changed in treated and control states at the time of *Citizens United*. First, we implement a propensity score matching approach, which matches treated and control states on the basis of the covariates in Table 1, Panel B. Appendix

[37] Outside of the United States, Schoenherr (2019) finds that political connections to South Korean president Lee Myung Bak led to procurement contract misallocation that aggregates up to about 0.41% of GDP.

Section B details the approach. Table B.1, Panel A shows the covariate balance between treated and matched control states and shows that the samples do not differ from one another in any statistically significant way. Figure B.1 replicates Figure 3 with the propensity matching approach and delivers similar results for labor income and total output, but the mixed effects on capital income remain inconclusive. For completeness, we show regression evidence using propensity score matching approach for the BEA and the IRS economic outcomes in Panels B and C (respectively) of Table B.1, which produces estimates that are very close to the baseline specification for labor income and total output, both in magnitude and statistical significance. Consistent with Figure B.1, the results on capital income are inconclusive.

Second, we implement a synthetic controls approach following Xu (2017).[38] This method explicitly addresses any concerns about pre-trends by matching treated states to control states based on ex-ante trends in the dependent variables. Our results, shown in Figure C.2, mostly eliminate pre-trends in the economic variables and find similar increases in labor income and total output in the post-period, particularly in the IRS data, suggesting that our results warrant a causal interpretation. Additionally, we perform a placebo test: a permutation test in the spirit of Abadie et al. (2010) by randomizing assignment into treated and untreated status and recomputing the estimated synthetic controls effect with 50 random permutations. Figure C.3 provides the results of this placebo test and, comfortingly, shows no effects.

Additionally, we redo our main difference-in-difference analysis using the US Census' QWI database, which not only has data on overall payments to workers (measured as total payroll) but also allows us to examine the contributions to the overall labor income increase coming from the growth in total employment and average earnings per worker. Table 3 provides the results of the difference-in-difference regressions and shows the effects that are largely consistent with our previous results on labor outcome: log payroll increases by 3.8 percentage points (Column (4)). Column (1) shows that log employment increases by roughly 1.7 percentage points in treated states following *Citizens United*. Log (average) earnings increase by 2.1 percentage points for all workers (Column (2)) and by 4.1 percentage points among newly hired workers (Column (3)), suggesting that some of these earnings increases are driven by new hires on the extensive margin. Figure 4 plots event study coefficients that show no substantial pretrends in the QWI data.

The QWI data also allows us to rule out a potentially confounding event: the 2010 shale boom. The production of shale gas through hydraulic fracturing (popularly known as "fracking") increased rapidly since 2010. Several of the states in our treatment sample were states in which shale deposits were abundant, and it is possible that the increase in labor outcomes is due to increased employment due to the shale boom rather than to *Citizens United*. However, we think that the shale boom is unlikely to be a plausible driver of the economic effects we document. The "Mining" sector (NAICS2=21), which includes "Oil and Gas" sub-sector, is a relatively small sector with an overage of 0.55% of the total state-level employment for a typical state: it is implausible that this sector would be able to generate (directly or indirectly) a state-level increase in employment

[38] We use the *gsynth* package in R, available here: `https://yiqingxu.org/software/`.

causing a 2% and 3% increase in total state-level payroll. Consistent with this intuition, we obtain almost identical results to the baseline specification in Table 2 when we exclude the "Mining" sector from calculating the state-level employment and payroll in Panel B of Table 3.[39]

Panel B of Table 1 highlights a second potential concern. While unemployment rates are similar in treated and control states, treated states were differentially exposed to house price changes around the Financial Crisis. While these differences are addressed in the propensity score matching analysis above, we go further and control for these characteristics directly in a dynamic way. Specifically, we bin states into quartiles of pre-crisis (2002–2006) house price changes and include year $\times$ house price change quartile $\times$ pre-*Citizens United* state-level growth fixed effects: these fine fixed effects absorb differential time trends across these states due to differential ex-ante economic conditions. The results, shown in Table C.2, are similar to our main results suggesting that differential exposure to the Financial Crisis is not driving our results. Additionally, as we discuss further in Section 5 we perform industry-level analysis and find that the results are not driven by crisis-related industries, such as real estate or finance.

5 Mechanisms

Our results so far show that *Citizens United* was associated with an increase in total output, with a significant increase in labor income and some suggestive evidence of capital income increasing. In this section, we provide evidence of a mechanism that is most consistent with the data: *Citizens United* changed the political-economic equilibrium by lowering barriers to enter the market for political influence. Briefly, it is easier to exert political influence through dollar donations than by building and cultivating political ties, revolving door arrangements, and other "soft," "backroom" forms of influence on politicians. As we illustrate with our framework in Section 2, such a decrease in the cost of political activity will (i) lower barriers to political activism, (ii) encourage broader political participation and increase political competition, and (iii) lead to elected politicians who cater to and adopt policies benefiting a wider set of constituents, rather than implementing rent-seeking policies that benefit a narrower set of politically connected interests. These pro-growth policies increase the economic "pie" available to split between labor and capital, thereby improving economic outcomes for both groups rather than increasing rents to interest groups that were already politically powerful.

We provide a variety of evidence that supports this mechanism. First, states affected by *Citizens United* experienced an increase in political spending across a variety of groups—from both historically politically engaged and unengaged groups—and an increase in political competitiveness as measured by electoral turnover. Second, we find that legislators become less politically polarized and potentially more likely to support centrist, growth-oriented policies. Third, we find that ex-ante politically *inactive* firms and industries respond as much or more than ex-ante politically *active*

[39]Note that in this analysis, the number of observations decreases slightly because the QWI does not have sector-level data available for all states over the sample period.

firms and industries, suggesting that historically entrenched interests are not the primary beneficiaries of the new political equilibrium. Fourth, we provide direct evidence that the affected states adopt more favorable economic policies around regulatory enforcement and taxation. Fifth, we find widespread improvements in economic outcomes across most industry sectors, suggesting that post-*Citizens United* policies benefit a broad set of constituent interests. Overall, policy changes appear to reduce administrative and regulatory costs, which leads to increased firm labor demand, output, employment, and wages.

Beyond offering evidence in support of this mechanism, we consider (and reject) two alternative explanations for our main results. The first alternative is that since *Citizens United* also removed restrictions in some states on unions' ability to engage in political advocacy, it is possible that increases in worker income were driven by unions' increased ability to advocate for pro-worker policies. The second alternative is that increased economic output is driven directly by increased government spending augmented by a fiscal multiplier. We offer evidence against these alternatives.

5.1 Increased Political Competition and Pro-growth Policies

We first show that *Citizens United* is an important shock to both the campaign finance landscape and to the outcomes of state-level elections. We then show evidence on subsequent increase in pro-growth policies.[40]

Political Spending: The dominant narrative surrounding the anticipated effect of *Citizens United* on electoral politics was that it would tilt the playing field in favor of large, incumbent political interests.[41] However, some legal experts argued that even before *Citizens United* the state of US campaign finance law was such that the largest corporations had a sufficient ability to influence the political process, and that the primary consequence of the deregulation of political spending would be to lower entry costs for new players to spend money in politics. For example, Bradley A. Smith, an FEC commissioner from 2000–2005, wrote after the Supreme Court decision that the case would have little impact on large firms (which could already afford to spend millions of dollars on lobbying), but rather increase political participation by small and medium-sized firms.[42]

Previous work has shown that independent political expenditures increase in states that were affected by *Citizens United* (Spencer and Wood, 2014). However, it is unclear who funds these expenditures since the court decision allowed for new forms of anonymous political spending (i.e., so-called "dark money"). For that reason, we use data on direct (i.e., not independent) political contributions to state-level politicians to examine whether the increase in political spending was driven by incumbent interests, first-time political spenders, or a combination of both. As we describe above, the court ruling did not directly affect state laws related to direct political contributions;

[40] We are not the first to study the political consequences of *Citizens United*, as authors in several fields have examined similar questions (e.g., Burns and Jindra, 2014; Spencer and Wood, 2014; Klumpp et al., 2016).

[41] A prominent example can be found in Barack Obama's 2010 State of the Union address, in which he explicitly spoke against the Court's decision. See `https://obamawhitehouse.archives.gov/the-press-office/remarks-president-state-union-address`.

[42] See `https://www.city-journal.org/html/citizens-united-fallout-10686.html`.

they changed laws relating to independent political spending (i.e., political advertising by groups that do not directly contribute to the campaigns of individual politicians). But since the funders of such independent political expenditures are undisclosed, we are forced to rely on direct political contributions, which require that the donor's identity be disclosed. If direct and independent political spending are complements (which we find and describe below), we believe that this analysis sheds light on which groups may drive the increase of money in politics, albeit imperfectly.

We use data from the National Institute for Money in Politics (NIMP) to examine how political contributions from different categories of donors changed after *Citizens United*. The NIMP data codes a "sector" for each donor to indicate the industry or ideological group of a particular contributor. For example, donors can be categorized across traditional economic sectors, such as agriculture or energy; or ideology, such as a single-issue liberal or conservative group, as well as those from labor or business. Moreover, the NIMP classification has a separate category for contributions that are too small to be categorized under campaign contribution laws, which we use as a proxy for donors who are likely to be infrequent or first-time donors.

We aggregate these data to the state-year level by sector and examine how (log) state-level political contributions change after *Citizens United* for different sectors. Table 4 presents the results of this analysis. We present the difference-in-differences β coefficient from Equation (1) for the full sample (top line labeled "All sectors") and for each sector subsample.[43] We generally find that direct political contributions increase after the ruling in all sectors in states affected by *Citizens United* compared to control states, although statistical significance varies by sector. Specifically, we find that aggregate contributions for all sectors increased by 22%, which is not statistically significant at conventional levels. The increase in direct political spending suggests that direct and independent political spending are complements and makes unlikely the possibility that the effect of *Citizens United* was simply to shift campaign finance from one channel to another while keeping total political spending constant.

Examining the results by sector, we find that all sectors (excluding government agencies) in states affected by the decision have positive point estimates ranging between 12% and 98%, with seven of these specifications being statistically significant at the ten-percent level or lower. The overall increase is not concentrated in sectors that are historically very politically active such as finance or energy, or "social issues" sector ("Ideology/Single Issue" category). In fact, the sector with the largest point estimate is "Unitemized Contributions" (0.980), which represents small-check donations. Given that all sectors increased their political activity (e.g., business groups, labor groups, lobbyists, and, in particular, likely first-time or infrequent contributors proxied by small donations included in the "Unitemized Contributions" category), this suggests that the net effect of *Citizens United* on political spending was not an increase in the political spending by incumbent political interests, but rather an increase in political spending by a broad number of political interests that likely include new donors.

[43] In contrast to most of our analysis, we report t-statistics instead of standard errors to facilitate comparison across coefficients.

We confirm that political advertising (from all sources) increase in states affected by *Citizens United* using data from Ad$pender in Appendix Figure C.4. These political advertising data include all types of political advertising spending since it is not possible to separately identify spending on political advertising by political campaigns directly (not affected by *Citizens United*) or advertising as independent expenditures (which is the main type of political spending affected by *Citizens United*). Therefore, this test provides a noisy estimate of the increase in independent expenditures driven by the Supreme Court decision. This figure shows flat pre-trends up through 2010 and then a spike in ad spending following *Citizens United* that is large in implied economic magnitude albeit estimated with low power likely due to data missing for several states. Collectively, the results in this section provide evidence that participation in the market for political influence increased in states affected by *Citizens United*, consistent with one of the key takeaways from the framework we propose in Section 2.

Electoral Outcomes: We next examine the effect of *Citizens United* on the outcomes of both executive and legislative elections to understand how electoral competitiveness changed. On the one hand, the expansion of political spending that *Citizens United* caused might have primarily benefited incumbent politicians and traditionally politically important constituents, resulting in the entrenchment of politicians and reduced political turnover. On the other hand, to the extent that *Citizens United* may have opened up new avenues of political engagement that served to democratize political participation—as our results above on increased political spending across a broad set of constituents suggest—the court ruling may have increased political competition. We study these two possibilities by examining whether the probability of turnover in the governor's political party has changed, as well as whether the proportion of new politicians in state legislative chambers has changed in treated states after *Citizens United*.[44]

We begin by examining the effect of *Citizens United* on gubernatorial elections. Figure 5 examines how the probability that the governor was of a different party than the party in power in 2010 (when the *Citizens United* ruling occurred) changes in treated states relative to control states after the ruling.[45] As shown in Panel (a) of the figure, the probability that the governorship changed political parties is significantly higher in treated states after *Citizens United* relative to control states, both economically and statistically. Indeed, the probability of gubernatorial party turnover was roughly 27 percentage points higher for treated states (as tabulated in Column (1) of Panel A of Appendix Table C.3), which is roughly 100% of the sample mean. The figure shows no pre-trends in political turnover, which supports the identification assumption.

Panel (b) of Figure 5 splits the political party turnover results by the political party that was in power in 2010 (elected prior to the court ruling) and again tests whether the governor in power is different from the 2010 governor. This test provides a systematic way to examine

[44] Klumpp et al. (2016) find that the reelection rates of Republicans in the state Houses increased, but they do not study in detail how the composition of incumbent and new politicians changed.

[45] We examine whether the elected governor's *party* changes rather than whether the *individual* changes based on the idea that governorships are often "passed down" within a party and, for example, that shifts from one Republican governor to the next Republican governor are not policy-change-wise meaningful. Rather, shifts between parties are more likely to represent more fundamental policy change.

the popular belief that *Citizens United* mainly caused Republicans to be elected. We find that there was increased turnover in *both* directions (i.e., Democratic governorships were more likely to transition to Republican control and vice versa) in treated states after the court ruling on the order of roughly 25 percentage points in a given two-year election cycle right after the court ruling across both parties. This is confirmed in Columns (3) and (4) of Appendix Table C.3, Panel A, which shows an increase of 27.3 percentage points in the likelihood of the governor's seat transitioning from Republican to Democrat and an increase of 23.4 percentage points in the likelihood of the governor's seat transitioning from Democrat to Republican, respectively. Though the magnitudes are large, given the smaller sample sizes, the estimates are not statistically significant. Column (2) of Panel A presents the results of a regression that directly estimates the probability of there being a Republican governor in power. We find a small, but not statistically significant increase of 10.3% in treated states after *Citizens United*, largely consistent with the idea that the decision increased political turnover in treated states. These results suggest that executive branch elections became more competitive (as measured by ex-post election outcomes of individual governors and political party in power), but this increase in competitiveness did not solely benefit the Republican party.

We next examine whether *Citizens United* affected political turnover in state legislatures. Specifically, we examine how the proportion of newly elected politicians in state Houses and state Senates changed after *Citizens United*.[46] Given the large number of legislators in each body, rather than looking at changes in political control, we measure turnover as the fraction of legislators that turn over, both overall and within party. Panel B of Appendix Table C.3 presents the results of this analysis. We begin by examining turnover in the state Houses of Representatives in Panel B, Columns (1)–(4).[47] Broadly, the results on state legislatures are weaker than for governorships, although they are in the same direction: Column (1) shows that the proportion of new Representatives is 2.4 percentage points higher in treated states following *Citizens United* relative to the baseline proportion of new Representatives of 27%. This represents a fairly large economic magnitude of 9% of the baseline rate. Column (2) shows that the proportion of Republicans is 3.2 percentage points higher. Column (3) shows that the proportion of *new* Republicans is 2.8 percentage points higher, with the estimate being statistically significant at 10%-level. Column (4) shows that the fraction of new Democrats is unchanged. The effects in the state Senates, shown in Columns (5)–(8) are similar in direction although smaller in effect. Observe that there are fewer state Senate elections in any given year because state Senators' terms are longer and their elections are staggered, which may help to explain some of the weaker statistical significance of these tests.

We emphasize that while *Citizens United* had important state-level electoral consequences, our electoral and economic findings are unlikely to be driven by a "Republican wave" effect. While some research finds that Republican election rates were higher in state Houses affected by *Citizens United* (e.g., Klumpp et al., 2016), we find that there is increased political turnover when turnover

[46] We refer to the lower legislative chamber as the state House of Representatives for consistency, although in some states this chamber is called the state Assembly.

[47] Note that the number of observations in this analysis drops relative to the governor analysis because the legislature data in Shor and McCarty (2011) is not complete for all states.

is defined more broadly. Indeed, we find that political activity across both liberal and conservative groups broadly increases and that governorships are more likely to turn over both from Democrat to Republican *and* vice versa. This is perhaps not surprising since both conservative- and liberal-aligned Super PACs saw a large increase in spending as shown in Figure 2, Panel (b). These results provide evidence that increased money in politics likely resulted in higher political competitiveness.

Political Polarization: We next examine whether voting patterns of legislators change after *Citizens United*. Our political turnover results do not speak to changes in the actual legislative preferences of newly-elected politicians. If politicians become more polarized, they might attempt to enact policies that are more extreme, such as focusing on passing legislation on wedge social issues that appeal to the ideological fringe of their parties. Alternatively, if politicians become more centrist, policy making could be more focused on issues that are less partisan and targeted to improve the conditions of a broader set of constituents. Indeed, the framework that we propose in Section 2 predicts that as more agents enter the market for political influence, politicians' policies consider a larger set of agents and, therefore, tend towards the political center.

We measure the polarization of state legislative chambers using data provided by Shor and McCarty (2011), described in Section 3.2.2. We use the authors' preferred measure of polarization: the numerical distance in ideology score between the mean Democrat and Republican in each legislature-year. Measured ideologies are time-invariant by legislator, meaning that state-level ideologies change due to the turnover of politicians, rather than individual politicians changing their ideology. Thus, we capture only the extensive margin of ideology drift; ideology could change even more as politicians change their preferences.

Figure 6 examines how state-level political polarization changes after *Citizens United*: Panel (a) presents results for the state Houses, while Panel (b) presents results for state Senates. The figure shows, particularly for state Houses, that states affected by *Citizens United* saw a sharp decrease in ideological distance in the first election cycle following the decision. The drop was instantaneous and persistent, with no detectable pre-trends. We find less precise evidence that polarization changed in the state Senates, which is unsurprising given our earlier finding that state Senate elections are not as strongly affected by *Citizens United*, potentially because state Senate elections are more staggered (and senatorial terms tend to be much longer).

Summarizing, we find evidence that political polarization decreased in states affected by *Citizens United*. We conjecture that the less-polarized legislatures are more responsive to the broad interests of their constituents rather than specifically representing concentrated special political interests. In the following subsection, we look directly at heterogeneity in economic outcomes to examine whether economic growth is similarly broad-based or whether it is concentrated in politically connected firms and industries.

Heterogeneity in Economic Outcomes: We next examine how labor-related outcomes vary across industries and firms. If one of the primary effects of *Citizens United* was to expand the set of politically engaged agents, one would expect that a wide cross-section of firms and industries benefited. The framework that we provide in Section 2 predicts that constituents who enter the market for political influence for the first time or those who are not politically active benefit from the reduction in the cost of political activism. As we have discussed, precisely identifying *all* of the groups that had previous ties to politicians is challenging (particularly identifying all ties to state-level politicians). As such, the tests in this section examine whether those firms that are most likely to have had connections to politicians are driving our results. Assuming that *Citizens United* spurred an increase in competition for political influence, we would not expect our results to be driven by these firms.

We begin by examining how labor-related outcomes responded to *Citizens United* across different industry sectors. Panel A of Table 5 presents results for (log) employment, (average) earnings, and payroll for the 20 NAICS sectors in the QWI database using our standard difference-in-difference approach from Equation (1). We find that employment, payroll, and earnings grew in treated states following the court decision across a wide spectrum of industries, suggesting that our main results are driven by a wide cross-section of the economy as opposed to by a few politically connected sectors. In particular, of the 60 possible industry coefficients, (20 sectors $\times$ 3 outcome variables), we find that nearly all have positive point estimates, and 14 are statistically significant at the 10% level.[48] Collectively, the industries that have a statistically significant coefficient for at least one of the outcome variables account for nearly 40% of total employment in the QWI database. These broad-based economic effects are therefore consistent with an expanded set of constituents participating in political activity and benefiting from it following *Citizens United*.[49]

We next examine whether employment, earnings, or payroll responds disproportionately more in sectors that are ex-ante more politically active. We define an industry to be politically active if its total state-level political contributions from 2006 to 2010 were above the median,[50] and we test whether the labor outcomes' response to *Citizens United* is stronger in those industries. Panel B of Table 5 presents the results of this analysis. The main coefficient of interest is the triple interaction term, $Post \times Treated \times Active$. In short, in this table, we find no evidence that labor outcomes respond more in ex-ante more politically active industries. The triple interactions are not statistically significant at conventional levels or economically large, while the main effects are generally in line with the estimates presented in Panel A.

Next, we examine how labor responses vary by firm size and age in the QWI data. We regard both size and age as proxies of ex-ante political connectedness. Our hypothesis that *Citizens United* expanded the set of politically engaged firms suggests that it should be young firms in particular—those that have not existed long enough to build political connections—that should be most affected by the decision. However, it would undermine our hypothesis if labor outcomes increased more dramatically in the larger or older firms that are more likely to be ex-ante politically

[48] In contrast to most of our analysis, we report t-statistics instead of standard errors to facilitate comparison across coefficients.

[49] Moreover, the fact that differences are not concentrated in industries related to the Financial Crisis, such as real estate or finance, further alleviates identification concerns that the results are driven by spurious crisis-related correlations.

[50] We find that public administration, services, finance, healthcare, and construction account for the largest proportion of contributions, while waste, food services, education, and agriculture account for the smallest proportion.

connected. We explore these outcomes in Figure 7 and Table 6.

We begin with firm size. Panels (a), (c) and (e) of Figure 7 show that (log) employment, earnings, and payroll increase at roughly similar rates for both smaller (fewer than 50 employees) and larger firms, and Table 6, Panel A confirms this finding. These results suggest that firms that were more politically connected ex-ante, at least as proxied by firm size, do not exhibit a greater response to *Citizens United*.

Our findings are more stark with respect to firm age. While Figure 7, Panel (b), and Table 6, Panel B, Column (1) show little difference between younger (5 years old or younger) and older firms in terms of log employment, there are much larger differences in terms of worker average earnings and total payrolls. Figure 7, Panel (d) show that workers at younger firms saw their earnings grow by nearly twice as much in response to *Citizens United* than workers at older firms. Panel (f) confirms a similar finding for total payrolls. Table 6, Panel B, Columns (2)–(4) confirm these results, with worker earnings (all workers and new hires) increasing by roughly 2.1% more in younger firms relative to older firms, new worker earnings increasing by 3.3% more in younger firms relative to older firms, and payrolls increasing 3% more in younger firms relative to older firms, although these differences are only statistically significant for newly-hired worker earnings. These results suggest that *Citizens United* increased firm labor demand at *all* firms,[51] but particularly more so for young firms that were less likely to be politically connected ex-ante.[52] Thus, these findings support our primary mechanism: post-*Citizens United* policies represent a broader set of constituent interests and result in widespread improvements in economic outcomes.

Our analysis above focused on economic outcomes using aggregated state-year or state-industry-year data. The advantage of these data is that they allow us to measure the total change in labor income. However, these aggregate analyses do not allow us to measure outcomes at specific firms, which is particularly useful if one seeks to measure ex-ante political connectedness at the firm level. Thus, we move from aggregate data to firm-level data to more directly examine the relationship between ex-ante firm political connectedness and firm outcomes after *Citizens United*. Specifically, we focus on US public firms from Compustat.[53] We focus on firm employment because, while employment data are well populated, payroll information is most often missing. For these firms, we measure political activity in several ways: whether a firm made campaign contributions to a federal PAC in the political cycles over 2004–2010; whether a firm in the S&P index made political contributions to state politicians in the political cycles over 2004–2010; whether an S&P 500 firm hired a registered federal lobbyist from 2000–2009;[54] and whether a firm had above-median total

[51] As firm labor demand increases, prices (wages) and quantities (employment) increase in a manner dictated by the labor supply elasticity across each sector.

[52] Since young firms are also more financially constrained (Babina et al., 2019, 2020), they are also more likely to respond to more favorable economic conditions due to *Citizens United*.

[53] As is commonly done in studies of corporate policies, we exclude financial firms (e.g., Almeida et al., 2017). Our results are similar if we include financial firms.

[54] Disclosure of political contributions to state politicians is substantially less standardized than disclosure of political contributions to federal politicians. We have identified state-level political activity for firms that were ever members of the S&P 500 stock market index for this analysis since larger firms are more likely to be politically active (e.g., Cooper et al., 2010).

assets in 2009 as a proxy for size.[55] While none of these proxies are a perfect measure of political incumbency for individual firms, they serve as a useful indication.

We estimate firm-level regressions of Equations (1) and present results in Table 7.[56] We find an increase in employment after *Citizens United* for firms headquartered in treatment states compared to firms headquartered in control states. Column (1) of Table 7 presents the results for all firms. Employment increases by 4% with a p-value of 0.052. Turning to the triple-difference estimations, we find little evidence that firms that were likely to have been politically active prior to *Citizens United* were the primary drivers of our results. Columns (2)–(5) of Table 7 confirm that politically *in*active firm responded most to *Citizens United*. The main effect of each regression (capturing the response of ex-ante politically inactive firms) is statistically significant at the ten-percent level or better in each specification and the interaction effect—indicating a firm that is more likely to have been politically active before *Citizens United*—is typically negative and generally insignificant, suggesting that ex-ante politically active firms do not drive increased employment.[57]

Collectively, the results in this section suggest that ex-ante politically active firms are not the driving force behind the increased labor income and employment in response to *Citizens United*. Such a result is largely consistent with the framework that we propose in Section 2 where we show that constituents that were not politically active should respond positively to a decrease in the cost of political activism. On the contrary, we find consistent evidence that labor outcomes were positively affected by *Citizens United* across a wide variety of industries, not just by ex-ante politically active firms or industries. If anything, smaller and younger firms, and firms with weaker ex-ante political activity saw equal or even greater responses to the court decision. Together, our findings consistently support our conjecture that *Citizens United* did not primarily benefit entrenched political interests but rather broadened the set of firms able to exercise political influence and benefit from it. Indeed, while it is difficult to precisely identify all of the firms that would comprise the "politically incumbent" constituents in our framework, the firms that we identify as politically connected in our firm-level tests generally reduce employment when they are headquartered in states affected by *Citizens United*, although this finding is only statistically significant with one measure of political activity. We interpret this finding as broadly supportive of a mechanism of increased competition in the market for influence.

Changes in Policies: Last, we consider whether business conditions became more favorable for firms. Our model predicts that policies are less distortionary in nature after a decrease in the cost of entry into the market for influence. We provide several measurable examples of changes in policies that could boost growth in states affected by *Citizens United*. We believe it is unlikely that

[55] As one would expect, these measures are positively correlated although not perfectly so. The correlations range between 0.30–0.76.

[56] We assign firms to treatment or control states based on the location of their headquarters in 2010. Since Compustat backfills headquarters state location, we use the data provided by Bill MacDonald at https://sraf.nd.edu/ to identify the historical headquarters state.

[57] We note that these results might seem different than our results on firm size using the QWI data which includes data on both public and private firms. However, the median Compustat firm has 1,400 employees, so these cross-sectional results are not directly comparable to the firm size results using the QWI data.

the rapid and broad-based growth in treated states was due to a single policy change and identifying all such changes is beyond the scope of the paper. Instead, we provide examples consistent with the overall environment in the affected states becoming more growth-friendly. Specifically, we examine whether the affected states experience changes in the regulatory enforcement of existing laws, fewer occupational licensing regulations, an improvement in a business-friendly environment, or a reduced state-level tax burden. Existing work suggests that regulations can be quite costly (e.g., Kalmenovitz, 2023) and often designed to benefit politically connected incumbent firms (e.g., Benmelech and Moskowitz, 2010; Faccio and Zingales, 2021; Neretina, 2019). We view this as a useful test of the last qualitative prediction of our model: following a decrease in the cost of political activity, policies become less distortionary.

Regulatory enforcement: We begin by examining changes in regulatory enforcement. Earlier we find evidence that turnover in the executive branch significantly increased in treated states after *Citizens United*, and since state governors are particularly important in establishing regulatory priorities in their states, regulatory outcomes are a likely place to find evidence of a change in economic priorities. We examine whether the number of state- and federal-level enforcement actions change after *Citizens United*, using data from the Violation Tracker database described in Section 3.2.2. If state government regulation of economic activity becomes more business-friendly, we expect that the number of state-level enforcement actions decreased, particularly those actions related to protecting employees or consumers. We use the number of federal-level enforcement actions for similar types of regulated activity as a placebo test to verify that a lower number of enforcement actions by state regulators does not reflect an underlying change in the behavior of firms, which itself could independently lead to a change in the number of enforcement actions. State-level executive agencies, such as a state attorney general (who is appointed by the governor in most states), in states with bans on political spending would have been differentially affected by *Citizens United*, whereas federal regulators would not have changed their regulatory scrutiny of firms in different states before or after *Citizens United*. The dependent variables are the log of one plus the number of corresponding enforcement actions.

Panels (a) and (b) of Figure 8 present the results of our analysis. Panel (a) shows the total number of state-level enforcement actions in which the primary offense type is related to violations against labor and consumers (red) and capital (blue).[58] We find that enforcement actions pertaining to laws protecting labor and consumers fell significantly in treated states following *Citizens United*. In contrast to state-level results, Figure 8, Panel (b)—which examines labor- and consumer-related enforcement actions at the federal level—shows that federal enforcement activity did not exhibit any change after *Citizens United* in treated states relative to control states. Eased enforcement appears primarily to focus on laws concerning labor and consumer protection, as opposed to laws

[58] We define capital protection cases as those for which the primary offense type is defined as investor protection violation or accounting fraud or deficiencies. We define labor and consumer protection cases for which the primary offense type is defined as a wage and hour violation, employment discrimination, workplace safety or health violation, labor relations violation, benefit plan administrator violation, employment screening violation, consumer protection violation, environmental violation, privacy violation, price-fixing or anti-competitive practices, mortgage abuses, or off-label or unapproved promotion of medical products.

specifically geared toward protecting capital providers. Labor and consumer protection laws are much more likely to involve costs in the actual day-to-day operation of a business as opposed to laws concerning investor protection, which primarily address financial reporting and fraud. When examining enforcement actions that are related to capital protection, we find no consistent patterns for either state or federal enforcement actions.

Table 8, Panel A quantifies these results in the difference-in-difference framework and shows that state-level enforcement actions related to labor and consumer regulation violations decreased by roughly 53% in treated states following *Citizens United* (Column (2)), while state- and federal-level enforcement actions related to capital protection did not change (Columns (3) and (6)).

The affirmative results for state-level enforcement actions (and null results for federal-level enforcement actions) suggest that the enforcement patterns changed as opposed to the underlying firm behavior: if firms were committing fewer violations, one would have expected federal-level enforcement to fall as well. To further make the case that reduced enforcement was unrelated to differences in non-monetary worker outcomes, in unreported results, we examine whether reduced regulatory enforcement led to worse non-financial outcomes for workers. Across a wide variety of non-financial outcomes—workplace deaths, foreclosures, evictions, mortality rates, cancer deaths, and denial rate for unemployment claims—we find no changes in treated states after *Citizens United*.

Occupational licensing: Next, we examine if the regulatory burden, as measured by mandatory occupational licensing, decreases in treated states after *Citizens United*. Many states mandate that individuals who want to perform certain types of work must obtain regulatory permission. These state-level regulations have been shown to have significant effects on the labor market, including lower employment growth (Kleiner, 2006).[59]

Using data from Sorens et al. (2008), we examine whether state-level licensure requirements decline in states affected by *Citizens United* in Table 8, Panel B.[60] In Column (1), we observe reduced regulatory requirements in the treated states as shown by a decrease in employment-weighted licensure of 0.017, which is 12% of the standard deviation, although the resulting t-statistic is only 1.55. These results suggest that some of the gains for labor in treated states may be coming from easier access to the labor market for a wider number of workers.

Regulatory freedom: Our previous results showing that treated states experience fewer regulatory enforcements and required fewer occupational licenses suggest an easing of regulatory burden in treated states. We next use a state-level index of regulatory freedom to explicitly measure changes in the overall regulatory burden around *Citizens United*.

We examine whether this state regulatory freedom index increases in treated states after *Citizens*

[59] Kleiner and Vorotnikov (2017) estimate that in the average state, 22% of the workforce requires an occupational license.

[60] The authors consider the occupation to be licensed (value 1) only if it "virtually prohibits a person from practicing the occupation without first obtaining permission, which in turn depends on either the discretion of a government body or certain training or educational requirements". Excluded are "title protection laws", or laws that ban the use of a certain title without meeting requirements. For example, "a law prohibiting an uncertified person from calling herself a "certified interior designer" would not count, but a law prohibiting the same person from [...] advertising that she practices "interior design" would count" (Sorens et al., 2008). In addition, if a license is required by a contractor but not her employees, the authors record that as a "half license", using the value of 0.5.

United. The results of this analysis are presented in Table 8, Panel B, Column (2). The overall regulatory environment becomes lighter in treated states, consistent with our previous results. Specifically, regulatory freedom in treated states increases by 0.012, or by 10% of the standard deviation, a result statistically significant at the 1% level.

Tax changes: Finally, we examine whether state-level tax rates changed in states affected by *Citizens United*. We obtain data on corporate tax rates (in percent) from Baker et al. (2021) and examine whether the level of the top marginal corporate, personal, sales, or estate tax rates change differentially in the affected states after *Citizens United*. Table 9, Panel A presents empirical results for the level of the various tax rates. We find negative point estimates for all categories of tax rates with the exception of property taxes, which is effectively zero. Although most of the estimates have relatively large economic magnitudes, most of them are not statistically different from zero. For example, the point estimates on the corporate and personal income tax rates are -0.551 and -0.359, respectively, which correspond to 8% and 6% of the sample means. We interpret this as suggestive evidence that business conditions are becoming more conducive to economic growth.

Collectively, the results suggest that the state-level regulatory environment becomes more favorable toward firms located in states that were affected by *Citizens United*. In particular, these policy changes appear to reduce overhead and administrative labor costs. These results provide further evidence that the increased economic gains to labor and capital come from improved economic conditions that increase the surplus available to split between labor and capital. Ultimately, such a reduction in costs would lead to increased labor demand, leading ultimately to more output, greater employment, and higher wages—exactly what we find in our main economic outcomes. Moreover, workers in those states were not worse off along non-financial dimensions.

5.2 Pro-Labor Policies

We next examine the first alternative explanation for our main results and study whether *Citizens United* led to more favorable policy changes specifically for workers. While the most widely discussed effect of the court ruling was to invalidate bans on corporate independent expenditures, a number of states had previously enacted bans on union independent expenditures that were also invalidated. It is possible that unions in those states had an increase in political power that allowed them to better bargain on behalf of their members or to more effectively advocate for general pro-labor policies such as a higher minimum wage.

While this type of mechanism could explain the increase in wages that we observe, it is less likely that this could simultaneously explain increased employment. For example, one would expect that an increase in minimum wages or other labor-friendly policies would decrease demand for labor in equilibrium resulting in lower employment levels, which is the opposite of what we observe. Additionally, summary statistics in Figure 2, Panel (c) suggest that labor's share of political spending, if anything, decreased following *Citizens United*. Increased union political power is also unlikely to drive our effects given we observe increased labor income in practically all sectors outside manufacturing—the sectors where US unionized labor concentrates. Nevertheless, we examine whether unions' increased political power could be an important channel for our results.

First, we examine whether the increase in labor income can be explained by increased political power of unions. In order to do so, we first test whether there is higher growth in labor income in the set of states that had previously banned political advertising by both unions and corporations compared to states with no bans. In other words, treatment states must have had corporate *and* union bans, and control states must have had *no bans*. If increased union power were a factor in the observed economic growth, we would expect that the growth in labor income should be stronger in the states where unions gained the most political power.

We present these results, which follow our main empirical specification, in Appendix Table C.4, with Panel A showing the BEA results and Panel B showing the analogous IRS results. As before, there is a borderline significant increase in overall output and capital income and a statistically significant increase in labor income. However, we cannot reject that these results are different from the baseline results that include all treated states.

Additionally, in unreported results, we formally analyze differences between states with (i) corporate bans and (ii) corporate *and* union bans by considering treated states as those with corporate and union bans, and control states as those with corporate bans only. In this analysis, after treatment, corporations gain political influence in both treated and control states, but unions only gain power in treated states. Thus, unions have relatively more power in treated states following *Citizens United*. We find no statistically significant impact of *Citizens United* in this analysis across all economic outcomes and data sources. Collectively, these results suggest that our main result, the increased labor income in treated states after *Citizens United*, is unlikely to be attributable to the increased political power of unions.

Second, we examine whether the effective minimum wage increased in treated states after the court decision. Since *Citizen United* displaced a number of politicians, their replacements could have directly advocated for pro-labor laws, such as an increased minimum wage. An increase in minimum wages could have directly led to the increase in wages paid that we have shown. We examine whether minimum wages increase in states that were affected by *Citizens United* in Table 9, Panel B. We examine potential changes in minimum wages using two different outcome variables: the dollar level of the minimum wage and the percent of annual growth of the minimum wage over the last year. Across both measures, we find no evidence that minimum wages change differentially in states affected by *Citizens United*.

Taken together, our results in this section suggest that our main finding—of increased labor income after money in politics becomes less regulated—is unlikely to be attributed to changes in policy that would directly affect transfers to labor.

5.3 Increased Spending by State Governments

Next, we examine whether increased government spending can explain the increased income growth that we have documented. We focus on two plausible ways that government spending could explain our main economic results: increased economic growth due to a fiscal multiplier associated with increased government spending or an increase in state subsidies to firms. Indeed, it is possible that newly elected politicians in states affected by *Citizens United* were more likely to support broad-based fiscal spending, which could have direct or indirect effects on state-level income of labor or capital. Moreover, it is also possible that firms were better able to negotiate for favorable subsidy deals such as preferential taxation for specific investments when they were able to spend more money in politics, and, as a result, state-level employment increased.

State Fiscal Spending: We begin by studying whether states that were affected by *Citizens United* substantially increased their government expenditures or revenues using data from the Annual Survey of State Government Finances. We present the results of Equation (1) for all categories of state expenditures and revenues in Table 10, Panel A. The top set of lines presents estimates for total government revenues and various subcategories, while the bottom set of lines presents estimates for government expenditures and various subcategories. The dependent variable is the log of one plus the expenditure or revenue amount. Columns (2) and (3) present regression estimates and t-statistics for each regression, respectively. Column (1), labeled "Pct of total", provides the percent of total revenue or expenditure for each subcategory to facilitate assessing the economic importance of each category.

Overall, we find little evidence that total state revenues or total state expenditures significantly changed (rows 1 and 16). While the point estimates are positive for most specifications, few categories are statistically significant. On the revenue side, we find some evidence that general sales tax revenue increased, although the category only accounts for about 12% of state revenues. On the expenditure side, we find a statistically significant increase in capital outlays, highway, police, and liquor store expenditures. The largest category of increased expenditures is capital outlay, and while this type of government spending could plausibly have a stimulative effect , it only accounts for about 13.5% of government expenditures.[61]

In sum, the increases in government spending are too small and too concentrated in particular spending categories to explain the large increase in labor income that we document without assuming a fiscal multiplier is implausibly large.

State Subsidies: We next study whether subsidies provided by state governments to firms were higher in states affected by *Citizens United* using subsidy data from *Good Jobs First*, described in Section 3.2.3. We examine whether the log of one plus the number or the dollar-value of state subsidies change differentially in treated states after *Citizens United*. Table 10, Panel B shows the results from using Equation (1). For completeness, we examine specifications that combine state and local subsidies as well as specifications that examine each type of subsidy separately.

Across all measurements, we find no clear patterns that would suggest that *Citizens United* led to an increase in either the number or amount of subsidies—a finding confirmed in recent

[61] Specifically, capital outlay is defined as: "Direct expenditure for purchase or construction, by contract or government employee, construction of buildings and other improvements; for purchase of land, equipment, and existing structures; and for payments on capital leases. Construction: Production, additions, replacements, or major structural alterations to fixed works, undertaken either on a contractual basis by private contractors or through a government's own staff." See `https://www2.census.gov/govs/pubs/classification/2006_classification_manual.pdf`.

work by Slattery et al. (2023). We find that there are generally positive point estimates on the Post $\times$ Treated coefficient, but none of the point estimates are statistically significant. Focusing on Columns (3) and (6), which examine total subsidies, the point estimates represent a potential increase of 0.035% of the standard deviation of the number of subsidies and 7.6% of the standard deviation of the value of subsidies. As with the results of our government spending tests, one would need to assume that any potential increase in subsidies is implausibly effective in aggregate at stimulating firm growth or employment to explain our main results.

6 Conclusion

We examine how payments to labor and capital providers changed in states affected by the 2010 Supreme Court decision *Citizens United*, which prompted the largest increase in political spending in the post-World War II era. We exploit the fact that the *Citizens United* ruling invalidated bans on independent expenditures in some states but not others and use the event as a natural experiment to identify the causal effect of increased money in politics on the economic outcomes of labor and capital. Using state-level economic data from the BEA and the IRS, we first find that output increases by roughly 2% in affected states. Labor income increases between 2–3% for up to six years following the event, and increases in capital income were economically large, though not always statistically significant. These results are robust to alternate data sources and specifications, and are unlikely to be due to differential trends between treated and control states. At a high level, these results suggest that labor outcomes improve when there is more money in politics and that this improvement does not come at the expense of capital providers.

We provide evidence that *Citizens United* increased political competition, which led politicians to adopt more growth-friendly economic policies. We do so by first showing that political activity increased from a broad variety of interests (and in particular amongst the smallest donors) in treated states after *Citizens United*, rather than increasing only in sectors that were historically politically influential. Furthermore, we find that the turnover of political incumbents increased more in treated states, and contrary to the common view, was not only driven by Republican politicians replacing Democratic politicians. Indeed, we find increased within-party and across-party turnover both in the executive and legislative branches of state governments. Finally, we find that political polarization is *lower* in treated states after *Citizens United*, suggesting that newly elected politicians vote in favor of policies relevant for a broader set of constituents.

Once elected, we find that politicians appear to enact pro-growth policies. For example, we find evidence that the regulatory burden on firms is lower. There are fewer state-level enforcement actions (but not fewer federal enforcement actions for similar activities), suggesting that newly elected governors reduce regulatory burdens rather than that firms change their underlying behavior. This reduced regulatory burden does not come at the expense of workers, since we find no evidence of poorer health outcomes for employees. We find some evidence that tax rates are lower, although despite having large economic magnitudes, these taxation results are not generally statistically significant.

Consistent with the increased political competition mechanism, we find that these economic effects—increased hiring and wages—are not concentrated in sectors or firms to be the most politically engaged prior to *Citizens United*. Indeed, we find that firms across many industries that comprise a large cross-section of the economy responded. Moreover, we find no evidence that firms that were more likely to have been politically active prior to *Citizens United* responded more. To the contrary, we find that there were no differences in the change in growth rates of employment, wages, or payroll for firms in industries that historically made the most political contributions. Using Compustat data on publicly traded firms, we find no evidence that firms that were known to have been politically active by making campaign contributions or engaging in federal lobbying before *Citizens United* increased employment more than other firms. Overall, these results suggest that historically politically powerful constituencies did not drive the increased economic growth.

Finally, we examine whether increased union labor power or greater government spending could explain our results and find little evidence of these alternative possibilities.

In summary, our paper empirically studies which factors of production benefit from money in politics: labor or capital. Our results suggest that the economic outcomes of political choices are not necessarily zero-sum and that increasing the ease of political engagement can bring a broader set of interests to the table, which itself can benefit the interests of both labor and capital. However, an important caveat to our results is that one cannot conclude that more money in politics is unilaterally better for labor and capital providers from our analysis, or that it is socially optimal to deregulate money in politics. This paper does not examine the welfare consequences of increased money in politics triggered by *Citizens United*. Finally, it is possible that the first-best outcome would be to have a reduced scope for political influence of all forms such as lobbying or hiring from the revolving door, but once some groups have access to politicians it might be beneficial to maximize the ability of all types of agents to have access to politicians. We look forward to future research on this topic.

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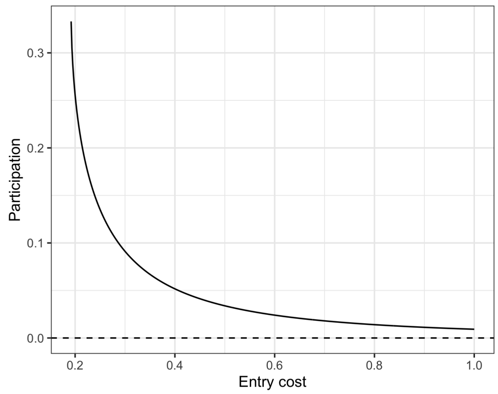
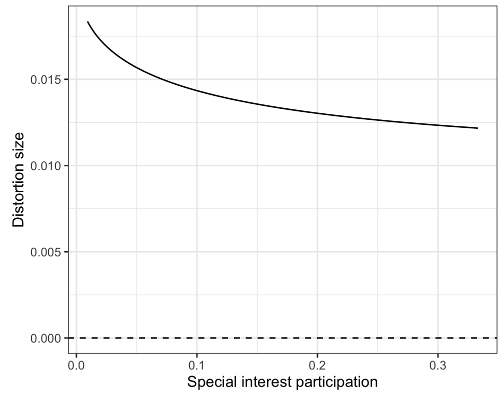
Figure 1: Model: Outcomes versus Costs of Participation

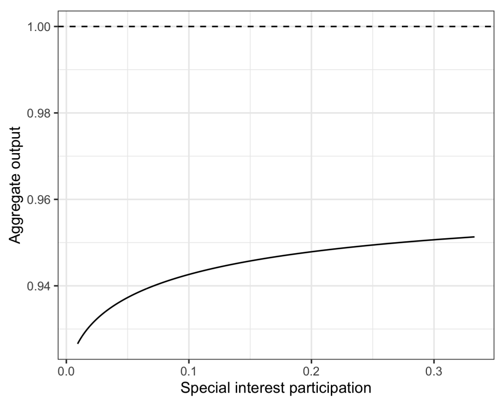
Note: This figure shows comparative statics of the political connection model as we alter the fixed cost c of becoming connected. Subplot (a) plots the fraction of connected voters versus c. Subplot (b)–(d) show the distortion $d(\mathbf{r})$, aggregate output $A(\mathbf{r})$, and income for politically unconnected opponents of the policy $(1+r_o)A(\mathbf{r})$ versus the equilibrium number of politically connected voters. Dashed lines represent the *laissez-faire* level of participation, distortion, and output. Subplot (e) shows equilibrium consumption across voter types for high (red) and low (blue) cost-of-political-entry economies.



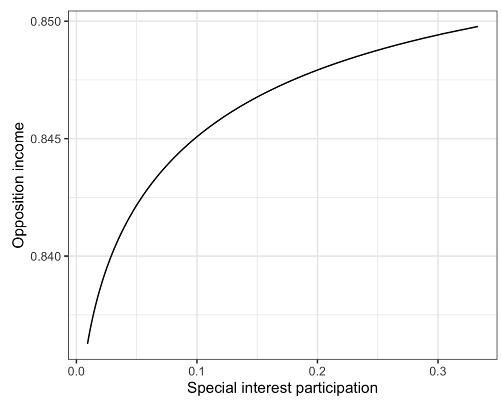
(a) Participation vs. entry cost



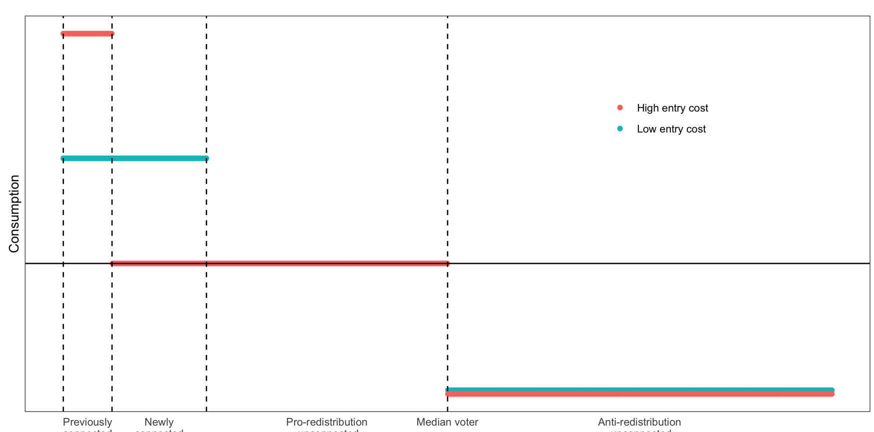
(b) Distortion



(c) Aggregate output



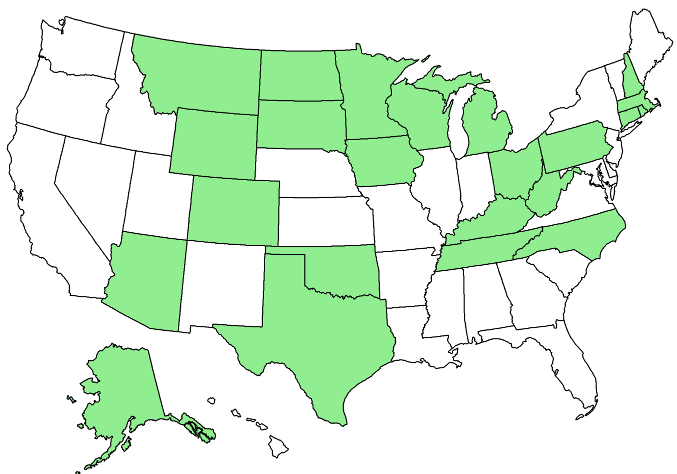
(d) Unconnected opponents' income



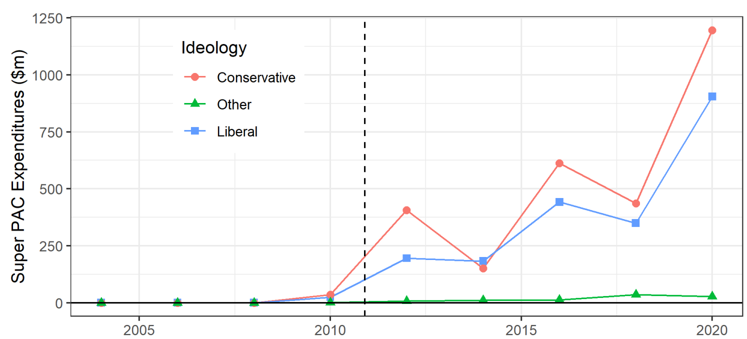
(e) High versus low cost of entry

Figure 2: Citizens United and Political Expenditures

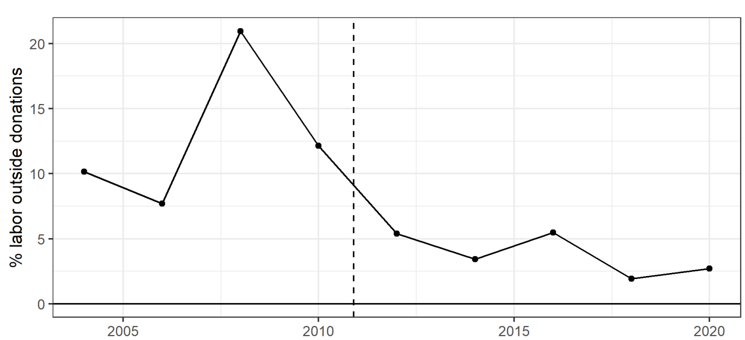
Note: In this figure, Panel (a) shows in green which states banned corporate (including non-profits) or union independent political expenditures before *Citizens United*—the bans that the Supreme Court invalidated. Panel (b) shows total Super PAC spending in federal elections, in millions of dollars in two-year (election-cycle length) increments from groups with conservative, liberal, and other ideologies. Panel (c) shows the fraction of total independent political spending (spending by political groups independent from candidates that include but are not limited to, Super PAC spending) in federal elections coming from labor-supporting organizations. Spending data are from OpenSecrets.org.



(a) States affected by *Citizens United*



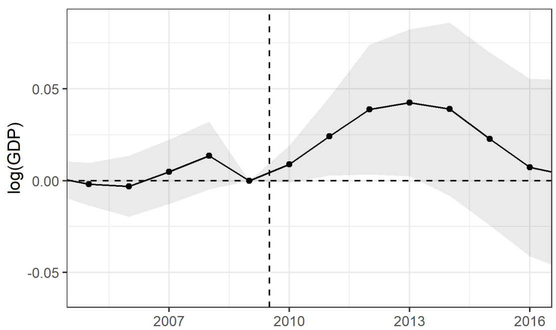
(b) Super PAC Spending



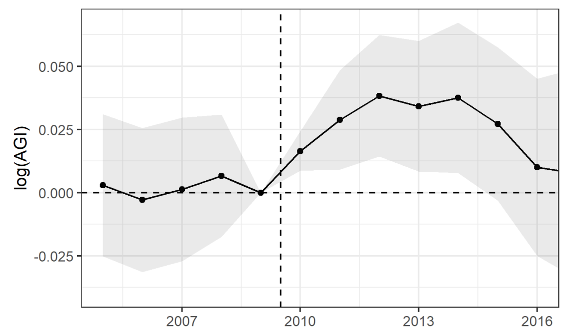
(c) Labor share of independent political spending

Figure 3: Income: Total, Capital, and Labor

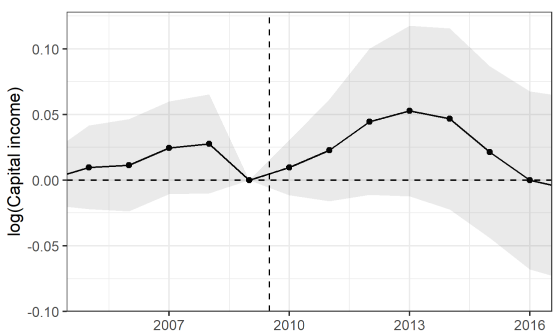
Note: This figure shows changes in state-level economic outcomes around *Citizens United*. States affected by the *Citizens United* case (treated states) are those that had bans on corporate or union independent political expenditures pre-*Citizens United*—the bans that the Supreme Court invalidated. The figures show the annual coefficients and 95% confidence intervals from regressions estimated using Equation (2) where the outcomes are total, capital, or labor incomes from the BEA and the IRS. Panels (a) and (b) show total income (GDP for BEA; AGI for IRS). Panels (c) and (d) show capital income (gross operating surplus for BEA; AGI less salary and wage income for IRS). Panels (e) and (f) show labor income (employee compensation for BEA; salary and wage (SW) income for IRS). Panels (a), (c), and (e) use BEA data; Panels (b), (d), and (f) use analogous IRS data. All specifications include state fixed effects, the fixed effects for state-level growth pre-*Citizens United* (from 2005 to 2009) interacted with year fixed effects, and the fixed effects for pre-*Citizens United* governor party (2010) interacted with year fixed effects. Standard errors are clustered at the state level.



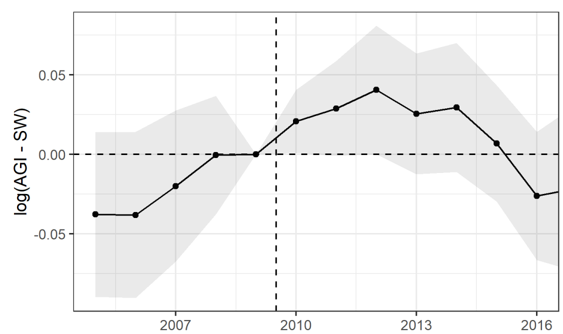
(a) BEA: GDP



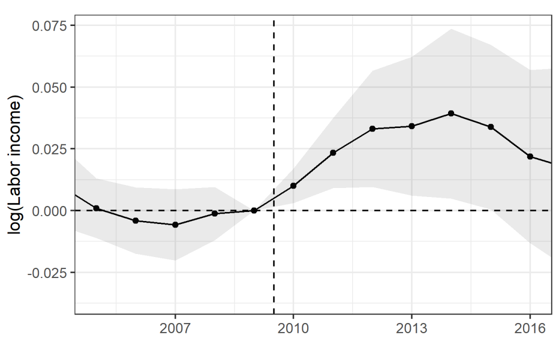
(b) IRS: AGI



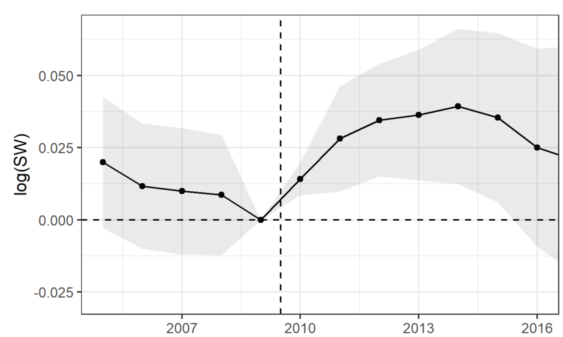
(c) BEA: Capital income



(d) IRS: Non-SW income



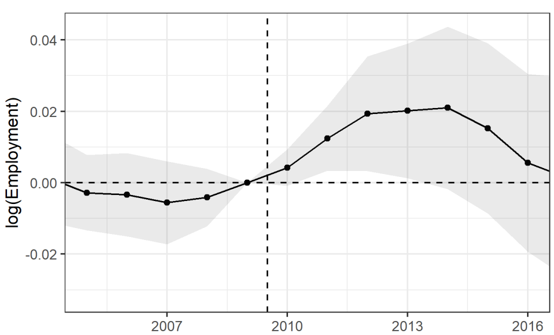
(e) BEA: Labor income



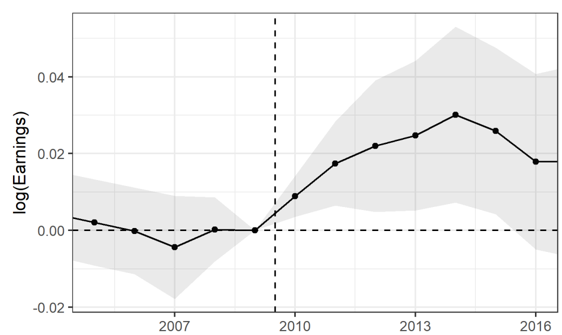
(f) IRS: SW income

Figure 4: ECONOMIC OUTCOMES USING QWI DATA

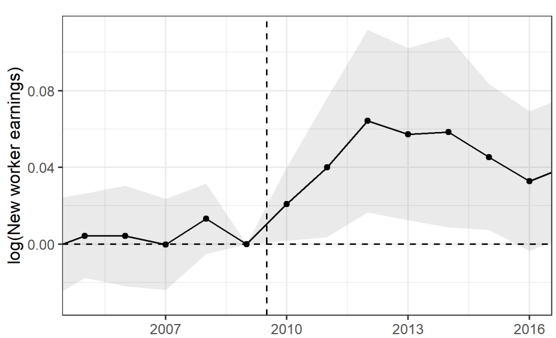
Note: This figure shows changes in state-level total employment, (average) earnings, (average) earnings by new workers, and total payroll around *Citizens United*. States affected by the *Citizens United* case (treated states) are those that had bans on corporate or union independent political expenditures pre-*Citizens United*—the bans that the Supreme Court invalidated. The figures show the annual coefficients and 95% confidence intervals from regressions estimated using Equation (2) where the outcomes are state-level economic outcomes from the US Census's QWI dataset. Employment is the number of employees. Earnings are average employee earnings: the figue in panel (b) includes all workers; the figure in panel (c) includes only newly-hired workers. Payroll is total payroll. All variables are aggregated to the annual level from quarterly data and then logged. All specifications include state fixed effects, the fixed effects for state-level growth pre-*Citizens United* (from 2005 to 2009) interacted with year fixed effects, and the fixed effects for pre-*Citizens United* governor party (2010) interacted with year fixed effects. Standard errors are clustered at the state level.



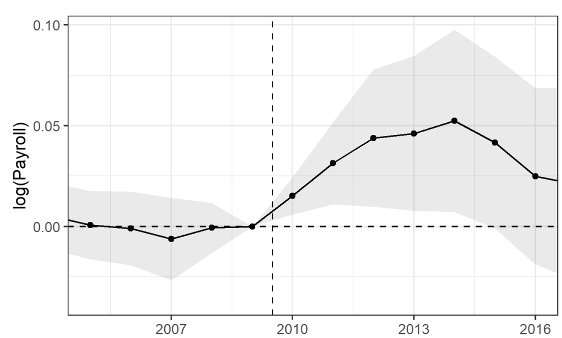
(a) Employment



(b) Earnings



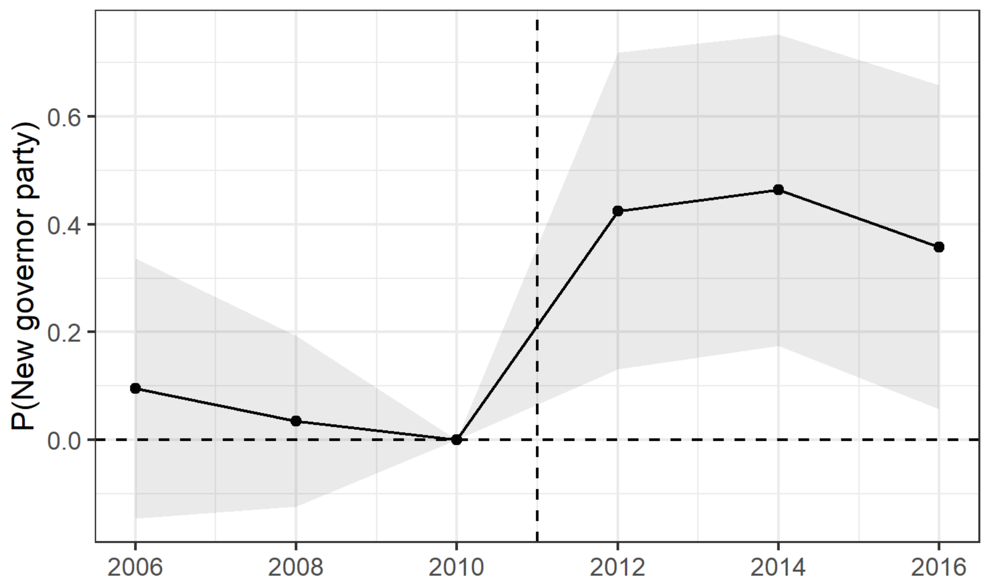
(c) New worker earnings



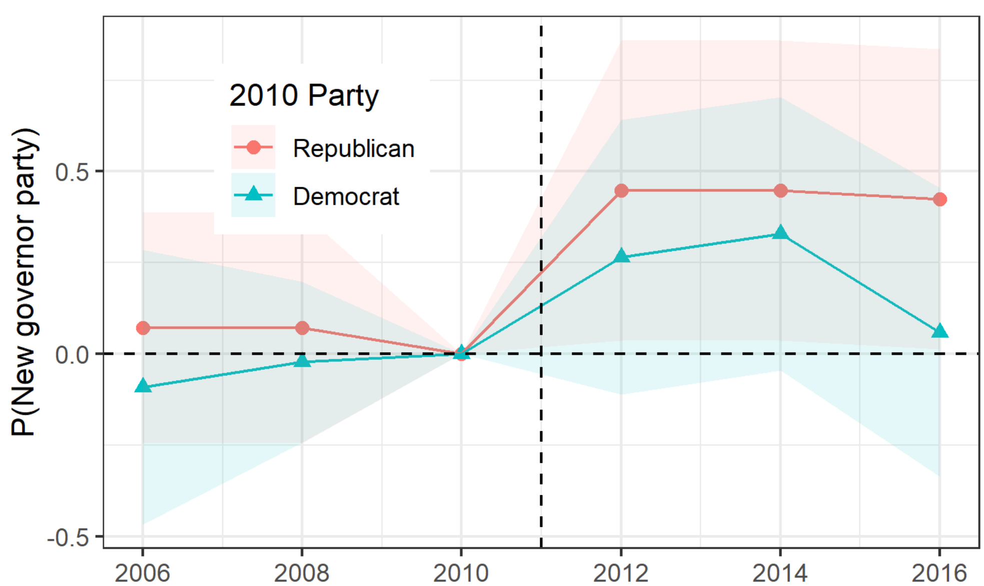
(d) Payroll

Figure 5: GUBERNATORIAL TURNOVER

Note: This figure shows changes in governor party around *Citizens United*. States affected by the *Citizens United* case (treated states) are those that had bans on corporate or union independent political expenditures pre-*Citizens United*—the bans that the Supreme Court invalidated. Each figure shows whether the governor's party in control is different from the party in control as of 2010 when the case was decided. Panel (a) shows the combined estimate. Panel (b) separately considers states with Republican or Democratic governors as of 2010. Specifically, each figure shows the time series coefficients from regressions estimated using Equation (2) where the dependent variable is whether the current governor party is the same as the 2010 governor party. The dots represent the coefficient estimates (with two-year, election cycle length increments) and the shaded region is the 95% confidence interval. All specifications include state and year times pre-*Citizens United* state-level state-level GDP growth (from 2005 to 2009) fixed effects. Standard errors are clustered at the state level.



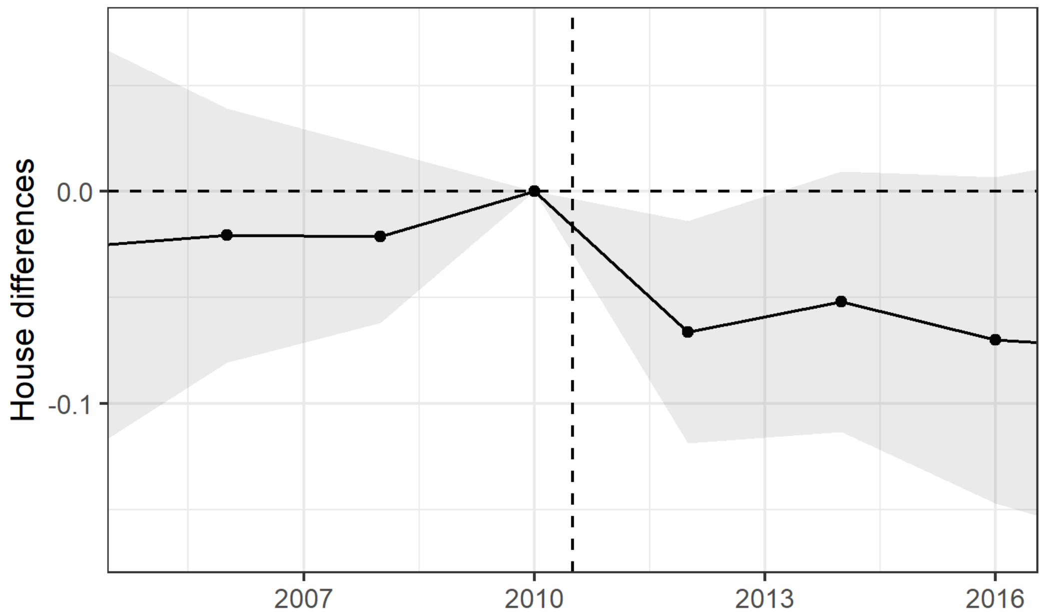
(a) All governors



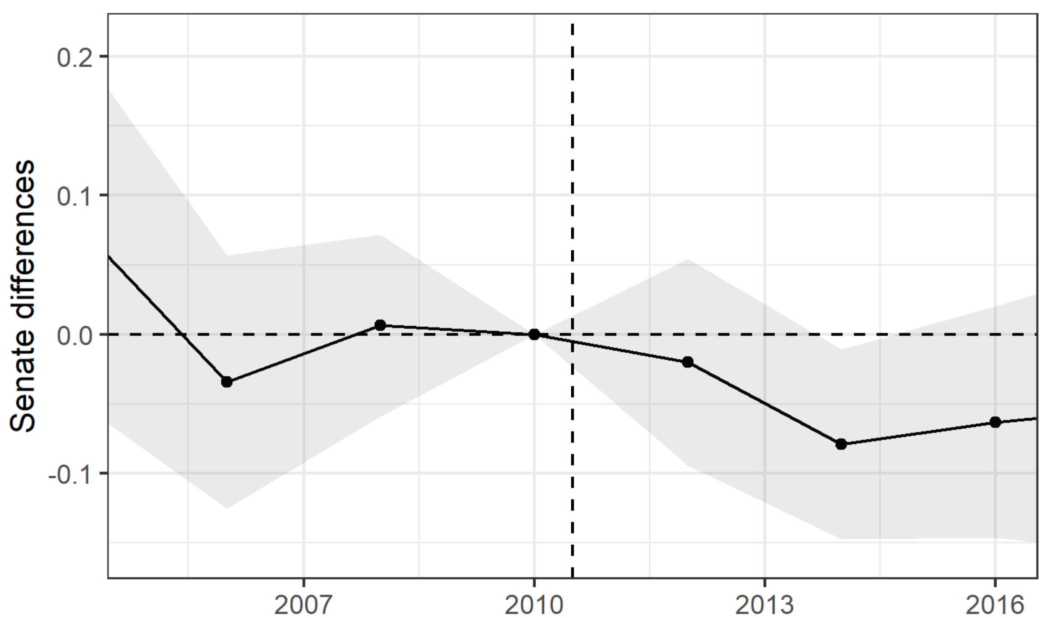
(b) Governor by party before *Citizens United*

Figure 6: Political Polarization

Note: This figure shows changes in political polarization of state legislatures around *Citizens United*. States affected by the *Citizens United* case (treated states) are those that had bans on corporate or union independent political expenditures pre-*Citizens United*—the bans that the Supreme Court invalidated. The figures show the coefficients (with two-year, election-cycle-length increments) and 95% confidence intervals of Equation (2) where the outcome is the mean political distance between legislators in the lower state House (Panel (a)) or the state Senate (Panel (b)). All specifications include state fixed effects, the fixed effects for state-level growth pre-*Citizens United* (from 2005 to 2009) interacted with year fixed effects, and the fixed effects for pre-*Citizens United* governor party (2010) interacted with year fixed effects. Standard errors are clustered at the state level.



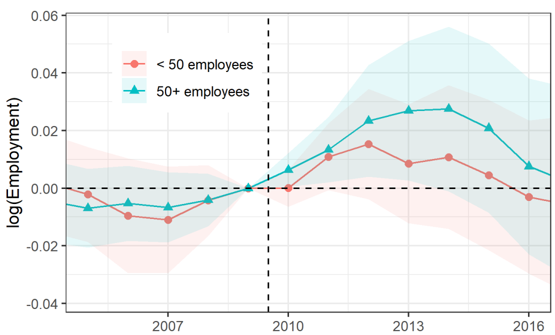
(a) Mean state House distance



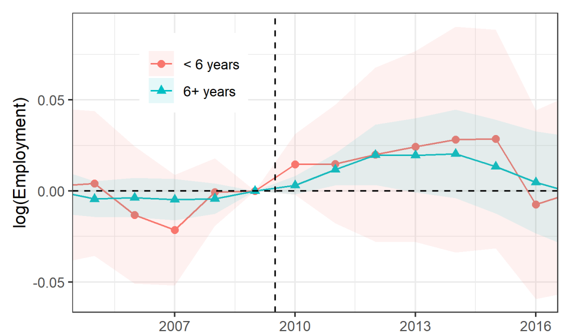
(b) Mean state Senate distance

Figure 7: Economic outcomes by firm size and age using QWI data

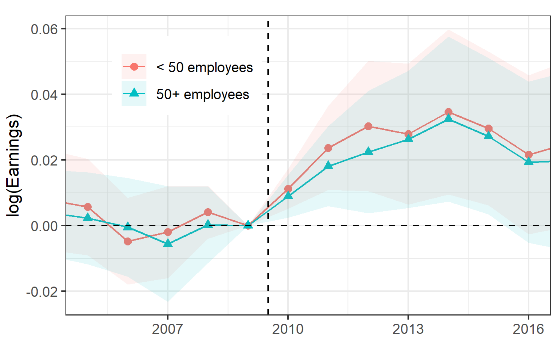
Note: This figure shows changes in state-level total employment, (average) earnings, and total payroll aggregated by firm age and size around *Citizens United*. States affected by the *Citizens United* case (treated states) are those that had bans on corporate or union independent political expenditures pre-*Citizens United*—the bans that the Supreme Court invalidated. The figures show the annual coefficients and 95% confidence intervals from regressions estimated using Equation (2) where the outcomes are state-level economic outcomes from the US Census's QWI dataset. Panels (a) and (b) show log employment; (c) and (d) show log (average) earnings, and (e) and (f) show log payroll. Panels (a), (c), and (e) show heterogeneity by firm size, with the red corresponding to outcomes calculated across smaller firms (firms with fewer than 50 employees) and the blue corresponding to larger firms (firms with more than 50 employees). Panels (b), (d), and (f) show outcomes by firm age, with the red corresponding to outcomes calculated across younger firms (defined as less than six years old) and the blue to older firms (defined as six or more years old). All specifications include state fixed effects, the fixed effects for state-level growth pre-*Citizens United* (from 2005 to 2009) interacted with year fixed effects, and the fixed effects for pre-*Citizens United* governor party (2010) interacted with year fixed effects. Standard errors are clustered at the state level.



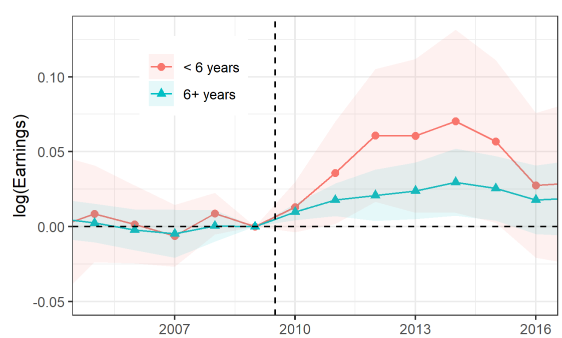
(a) Employment by firm size



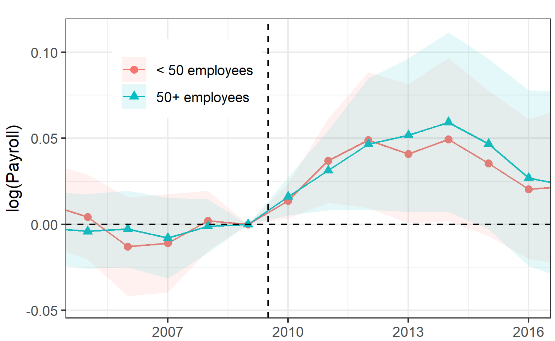
(b) Employment by firm age



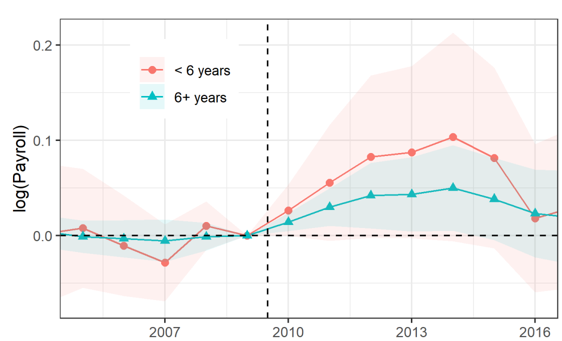
(c) Earnings by firm size



(d) Earnings by firm age



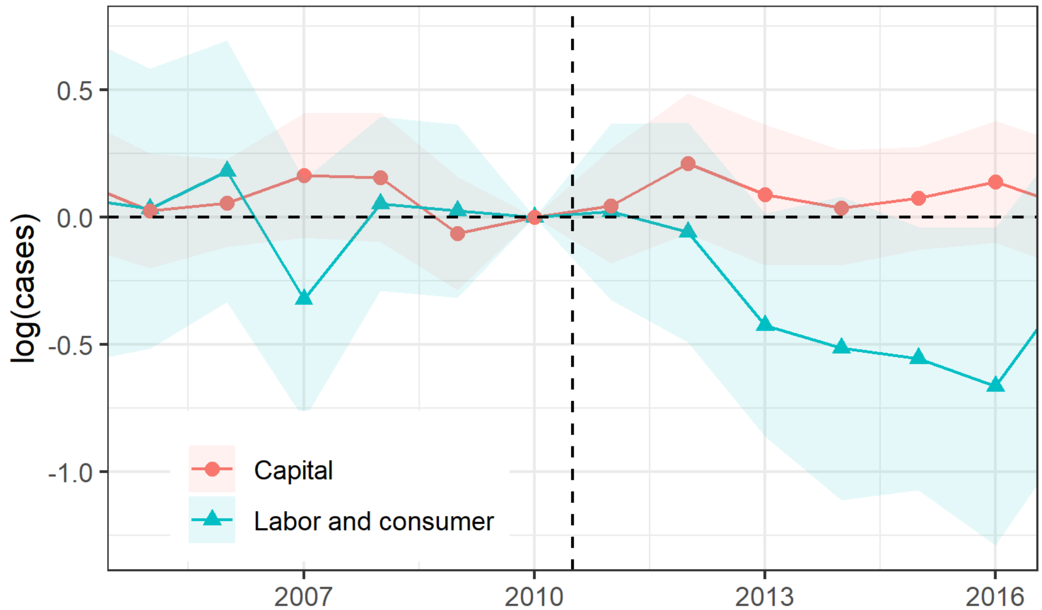
(e) Payroll by firm size



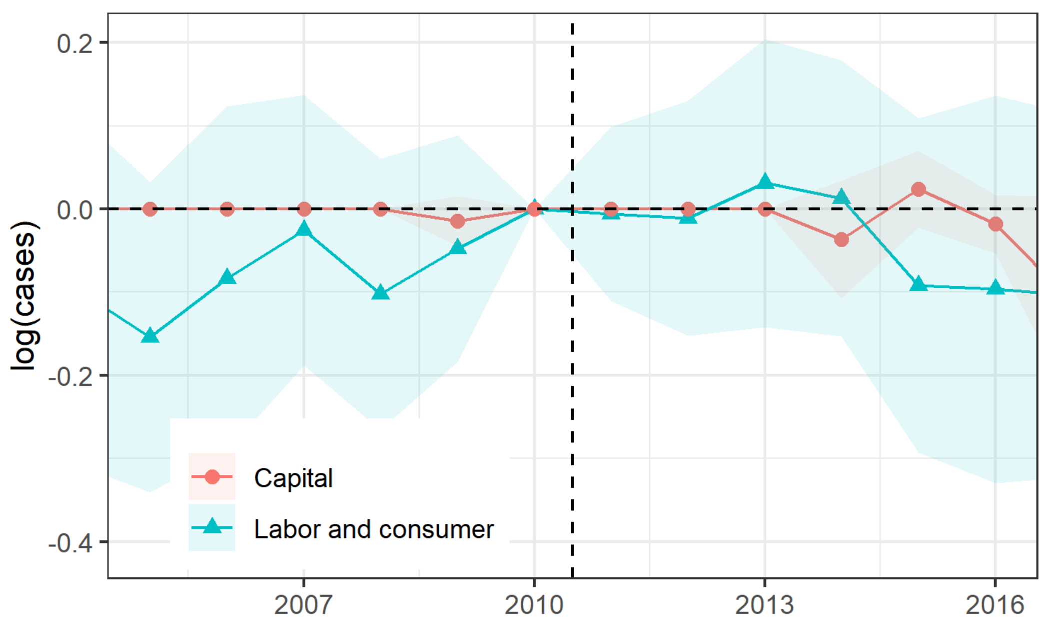
(f) Payroll by firm age

Figure 8: Legal Enforcement

Note: This figure shows changes in government enforcement actions around *Citizens United*. States affected by the *Citizens United* case (treated states) are those that had bans on corporate or union independent political expenditures pre-*Citizens United*—the bans that the Supreme Court invalidated. The figures show the annual coefficients and 95% confidence intervals from regressions estimated using Equation (2) where the outcomes are enforcement actions. Panel (a) shows log of one plus the number of enforcement actions by state governments of labor and consumer protection laws (blue) and capital protection laws (red). Panel (b) shows the equivalent for federal enforcement. Enforcement action data comes from *Good Jobs First*. All specifications include state fixed effects, the fixed effects for state-level growth pre-*Citizens United* (from 2005 to 2009) interacted with year fixed effects, and the fixed effects for pre-*Citizens United* governor party (2010) interacted with year fixed effects. Standard errors are clustered at the state level.



(a) State enforcement



(b) Federal enforcement

Table 1: Summary Statistics and Panel Balance

Note: This table shows summary statistics and panel balance for the main datasets used in the analysis. Panel A shows summary statistics for the data used in the main analysis, including economic outcomes, political outcomes, ad spending, legal enforcement, and policy outcomes. Note that in contrast to economic variables, most political outcomes are measured every two years (standard election cycle-length increments). Panel B shows the means of variables for treated and control states as well as the *p*-value for the difference in means. The data runs from 2005 to 2016, and some variables have fewer observations due to incomplete data coverage for some states. In Panel B, where possible and relevant we use the latest pre-treatment (2009) economic variables. Some variables (e.g., those from the decennial Census) are available only in 2010. In the case of mortgage delinquencies and unemployment, we use 2010 rather than 2009 to better capture differential exposures to the financial crisis.

Panel A: Summary statistics

Statistic	N	Mean	St. Dev.	Pctl(25)	Median	Pctl(75)
Treated	300	0.45	0.50	0	0	1
GDP ($B, BEA)	600	309.98	383.33	73.67	190.38	387.74
Labor income ($B, BEA)	600	166.12	199.83	39.35	100.77	215.26
Capital income ($B, BEA)	600	122.79	158.25	29.90	77.12	144.43
AGI ($B, IRS)	600	172.58	209.37	40.33	100.25	217.64
Salary/wage income ($B, IRS)	600	120.13	143.00	28.55	70.65	157.34
AGI - Salary/wage income ($B, IRS)	600	52.45	67.23	11.65	30.53	60.49
Employment (m, QWI)	564	2.64	2.86	0.69	1.74	3.39
Earnings ($, QWI)	564	3,708.53	602.44	3,295.44	3,604.88	4,014.94
Payroll ($B, QWI)	564	124.58	155.43	28.39	69.52	155.19
Republican house	240	0.50	0.16	0.39	0.50	0.62
New house	240	0.27	0.11	0.19	0.25	0.35
New republican house	240	0.15	0.09	0.08	0.13	0.20
New democrat house	240	0.12	0.07	0.07	0.11	0.16
Republican senate	222	0.51	0.17	0.40	0.51	0.63
New senate	222	0.24	0.13	0.14	0.22	0.30
New republican senate	222	0.13	0.10	0.05	0.12	0.19
New democrat senate	222	0.10	0.07	0.05	0.10	0.14
New governor party	300	0.26	0.44	0	0	1
Republican governor	300	0.54	0.50	0	1	1
House differences	564	1.55	0.49	1.22	1.50	1.85
Senate differences	567	1.51	0.48	1.15	1.50	1.82
Ad spending ($M)	456	14.07	22.52	0.68	5.98	16.30
Total direct political contributions ($M)	600	63.33	164.09	2.86	14.93	54.64
Employment (Compustat)	28,230	12.4	56.8	0.352	1.8	7.4
Federal Contributions	28,230	0.203	0.402	0	0	0
S&P 500 State Contributions	28,230	0.137	0.344	0	0	0
S&P 500 Federal Lobby	28,230	0.14	0.347	0	0	0
Large	28,230	0.531	0.499	0	1	1
Violations (state, aggregate)	600	25.93	88.33	2	4	11
Violations (federal, aggregate)	600	390.23	406.65	127	252	483.5
Violations (state, labor and consumer)	600	23.30	87.33	1	3	9
Violations (federal, labor and consumer)	600	352.35	377.81	109	215.5	424
Violations (state, capital)	600	1.22	0.87	1	1	1
Violations (federal, capital)	600	1.01	0.08	1	1	1
Subsidies (Count, state)	561	381.89	679.15	42	117	410
Subsidies (Count, local)	561	162.45	841.63	0	2	43
Subsidies (Count, total)	561	545.59	1,290.67	53	161	537
Subsidies ($M, state)	561	215.73	665.43	4.19	44.69	165.14
Subsidies ($M, local)	561	51.78	162.62	0.00	0.00	15.56
Subsidies ($M, total)	561	308.33	734.92	11.75	82.34	304.11
Occupational liscensure	600	0.53	0.12	0.47	0.54	0.62
Regulatory freedom	600	−0.003	0.13	−0.09	0.03	0.09
Minimum wage	600	7.03	1.04	6.55	7.25	7.50
Δ Minimum wage	600	0.10	0.25	0.00	0.00	0.00
Sales tax rate	550	4.97	1.94	4.00	5.50	6.00
Corporate tax rate	550	6.56	2.74	6.00	7.00	8.50
Top income tax rate	550	5.46	2.94	4.54	6.00	7.00
Property tax rate	550	0.04	0.18	0.00	0.00	0.00
Estate/Inheritance indicator	550	0.28	0.45	0	0	1

Table 1: Summary Statistics and Panel Balance (continued)

Panel B: Panel balance

Variable	Mean (treated)	Mean (control)	P
2008 Obama vote share	0.49	0.52	0.29
Republican governor (2010)	0.30	0.56	0.08*
Population (millions, 2010)	5.51	6.72	0.54
Median household income (thousands, 2010)	49.64	49.86	0.92
log(GDP) (2009)	11.94	12.13	0.53
log(Labor income) (2009)	11.31	11.51	0.51
log(capital income) (2009)	11.02	11.19	0.56
Fraction with bachelors (2010)	0.31	0.30	0.49
Unemployment (2010)	0.08	0.09	0.28
90+ days mortgage delinquency (2010)	0.03	0.04	0.04**
House price change 2002-2006	0.28	0.43	0.01***
House price change 2007-2010	-0.09	-0.16	0.02**
GDP Change (2004-2009)	0.19	0.17	0.33
Labor income change (2004-2009)	0.16	0.15	0.58
Capital income change (2004-2009)	0.23	0.20	0.39

Table 2: Income: Total, Capital, and Labor

Note: This table shows changes in state-level economic outcomes around *Citizens United*. States affected by the *Citizens United* case (treated states) are those that had bans on corporate or union independent political expenditures pre-*Citizens United*—the bans that the Supreme Court invalidated. This table shows the results from regressions estimated using Equation (1) where the dependent variables are economic outcomes at the state-year level. *Post* is an indicator for 2010 or later (after the *Citizens United* decision). Data in Panel A are from the BEA. Data in Panel B are from the IRS. Both run from 2005 through 2016. In each Panel, Column (1) is a measure of aggregate income (GDP for BEA; AGI for IRS). Column (2) is a measure of capital income (gross operating surplus for BEA; AGI less salary and wage (SW) income for IRS). Column (3) is a measure of labor income (employee compensation for BEA; salary and wage income for IRS). All specifications include state fixed effects, the fixed effects for state-level growth pre-*Citizens United* (from 2005 to 2009) interacted with year fixed effects, and the fixed effects for pre-*Citizens United* governor party (2010) interacted with year fixed effects. Standard errors, in parentheses, are clustered at the state level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

Panel A: BEA data

	Dependent variable:		
	log(GDP)	log(Capital income)	log(Labor income)
	(1)	(2)	(3)
Post × Treated	0.023	0.014	0.030**
	(0.019)	(0.028)	(0.014)
State FE	Y	Y	Y
Year × Pre-CU GDP Growth FE	Y	Y	Y
Year × Pre-CU Gov Party FE	Y	Y	Y
Observations	600	600	600
Adjusted R^2	0.998	0.996	0.999

Panel B: IRS data

	Dependent variable:		
	log(AGI)	log(AGI - SW)	log(SW)
	(1)	(2)	(3)
Post × Treated	0.026*	0.037*	0.020*
	(0.014)	(0.021)	(0.011)
State FE	Y	Y	Y
Year × Pre-CU GDP Growth FE	Y	Y	Y
Year × Pre-CU Gov Party FE	Y	Y	Y
Observations	600	600	600
Adjusted R^2	0.999	0.997	0.999

Table 3: Economic outcomes using QWI data

Note: This table shows changes in state-level total employment, (average) earnings, and total payroll around *Citizens United*. States affected by the *Citizens United* case (treated states) are those that had bans on corporate or union independent political expenditures pre-*Citizens United*—the bans that the Supreme Court invalidated. This table shows the results from regressions estimated using Equation (1) where the dependent variables are labor-related outcomes at the state-year level. *Post* is an indicator for 2010 or later (after the *Citizens United* decision). Data are from the US Census's QWI and run from 2005 through 2016. Employment is the number of employees. Earnings are average employee earnings: Column (2) includes all workers; Column (3) includes only newly-hired workers. Payroll is total payroll. All variables are aggregated to the annual level from quarterly data. All specifications include state fixed effects, the fixed effects for state-level growth pre-*Citizens United* (from 2005 to 2009) interacted with year fixed effects, and the fixed effects for pre-*Citizens United* governor party (2010) interacted with year fixed effects. Panel A includes all sectors, and Panel B excludes the "Mining" Sector (which contains data from oil and gas subsectors) from total calculations. Note that QWI industry-level data is not available for all state-years, so the number of observations is lower. Standard errors, in parentheses, are clustered at the state level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

Panel A: Employee outcomes using QWI Data (all sectors)

	Dependent variable:			
	log(Employment)	log(Earnings)	log(New worker earnings)	log(Payroll)
	(1)	(2)	(3)	(4)
Post × Treated	0.017*	0.021**	0.041*	0.038*
	(0.010)	(0.010)	(0.021)	(0.019)
State FE	Y	Y	Y	Y
Year × Pre-CU GDP Growth FE	Y	Y	Y	Y
Year × Pre-CU Gov Party FE	Y	Y	Y	Y
Observations	576	576	576	576
Adjusted R^2	1.000	0.983	0.934	0.999

Panel B: Employee outcomes using QWI Data (excluding "Mining" sector)

	Dependent variable:			
	log(Employment)	log(Earnings)	log(New worker earnings)	log(Payroll)
	(1)	(2)	(3)	(4)
Post × Treated	0.019*	0.024**	0.037*	0.042**
	(0.010)	(0.011)	(0.021)	(0.020)
State FE	Y	Y	Y	Y
Year × Pre-CU GDP Growth FE	Y	Y	Y	Y
Year × Pre-CU Gov Party FE	Y	Y	Y	Y
Observations	537	537	537	537
Adjusted R^2	0.999	0.978	0.934	0.999

Table 4: Direct Political Campaign Contributions After *Citizens United*

Note: This table shows changes in direct state-level contributions to political campaigns around *Citizens United*. States affected by the *Citizens United* case (treated states) are those that had bans on corporate or union independent political expenditures pre-*Citizens United*—the bans that the Supreme Court invalidated. This table shows the results from regressions (β coefficients and their corresponding t-statistics) estimated using Equation (1) where the dependent variables are the natural logarithm of direct contributions to political campaigns at the state-sector level in a given year with even-odd year fixed effects to account for state election cycles. *Post* is an indicator for 2010 or later. The last column ("% All Contributions") provides the proportion of contributions over the entire sample by each sector to facilitate understanding of the relative size of each sector. Data are provided by the National Institute for Money in State Politics (NIMP) and to maintain a consistent pre- and post-window with our other analyses, run from 2005 through 2016. All specifications include state fixed effects, the fixed effects for state-level growth pre-*Citizens United* (from 2005 to 2009) interacted with year fixed effects, and the fixed effects for pre-*Citizens United* governor party (2010) interacted with year fixed effects, and election cycle (even- and odd-year) fixed effects. We present t-statistics instead of standard errors to facilitate comparisons across sectors. Statistical tests account for clustering at the state level in all tests. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

	NIMP Sector	Coefficient	t-value		% All contributions
1	All sectors	0.223	1.262		100
2	Agriculture	0.450	1.335		2.318
3	Communications & Electronics	0.385	1.881	*	3.397
4	Construction	0.344	0.962		3.417
5	Defense	0.186	0.359		0.061
6	Energy & Natural Resources	0.511	1.733	*	5.669
7	Finance, Insurance & Real Estate	0.303	1.239		12.848
8	General Business	0.210	0.752		12.256
9	Government Agencies/Education/Other	-0.129	-0.330		4.303
10	Health	0.340	1.858	*	7.823
11	Ideology/Single Issue	0.845	2.050	**	9.456
12	Labor	0.547	1.739	*	12.633
13	Lawyers & Lobbyists	0.447	1.819	*	6.235
14	Transportation	0.125	0.488		1.538
15	Uncoded	0.391	1.479		13.822
16	Unitemized Contributions	0.980	2.383	**	4.225

Table 5: Economic outcomes by industry using QWI data

Note: This figure shows changes in state-level total employment, (average) earnings, and total payroll aggregated by industry around *Citizens United*. States affected by the *Citizens United* case (treated states) are those that had bans on corporate or union independent political expenditures pre-*Citizens United*—the bans that the Supreme Court invalidated. This table shows the results from regressions estimated using Equation (1) where the dependent variables are economic outcomes. *Post* is an indicator for 2010 or later (after the *Citizens United* decision). Data are from the US Census's QWI database and run from 2005 through 2016. Employment is the number of employees. Earnings is average employee earnings. Payroll is total payroll. Panel A uses a panel at the state-year level for each industry sector (at 2-digit NAICS level). It shows the effect across all sectors: the coefficient on *Treated* (whether the state had a ban on independent political expenditures before 2010) times *Post*. We present the corresponding *t*-statistics and statistical significance to facilitate comparisons across sectors. The last column ("% All Employment") shows the percentage of employees in each sector over the whole period to facilitate the understanding of the relative size of each sector. Panel B uses a panel at the state-year-NAICS sector level. It shows the effects by whether the industry was ex-ante politically engaged, where Active is an industry-level indicator equal to one if the aggregate industry political contributions between 2006 and 2009 to states are above the median. Observations are weighted by the proportion of employees in each sector as of 2010. All specifications include state fixed effects, the fixed effects for state-level growth pre-*Citizens United* (from 2005 to 2009) interacted with year fixed effects, and the fixed effects for pre-*Citizens United* governor party (2010) interacted with year fixed effects. All specifications in Panel B include state time active industry, and year times pre-*Citizens United* GDP growth times politically active industry fixed effects, and year times pre-*Citizens United* governor party times politically active industry fixed effects. All statistical tests account for clustering at the state level. *, **, and *** denote statistical significance at the 10%, 5%-, and 1% levels respectively.

Panel A: Effects by industry

	log(Employment)			log(Earnings)			log(Payroll)			
NAICS sector	Coef	t-value		Coef	t-value		Coef	t-value		% Employment
All sectors	0.02	2.19	**	0.02	1.89	*	0.04	2.06	**	100.00
Agriculture	0.04	1.15		0.02	1.11		0.06	1.50		1.19
Mining	0.20	2.00	**	0.01	0.57		0.38	1.40		0.53
Utilities	-0.24	-1.36		-0.01	-1.12		-0.12	-1.21		0.54
Construction	0.09	2.41	**	0.02	1.86	*	0.11	2.37	**	5.40
Manufacturing	0.04	2.61	***	0.00	0.29		0.04	2.35	**	9.07
Wholesale Trade	0.05	2.70	***	0.02	1.63		0.06	2.52	**	4.17
Retail Trade	0.01	0.94		0.01	1.04		0.01	0.78		11.75
Transportation	0.03	1.04		0.00	0.06		0.03	0.60		3.45
Information	0.01	0.44		0.03	1.43		0.02	0.77		2.28
Finance	0.02	1.38		0.02	1.53		0.04	2.16	**	4.10
Real Estate	0.04	2.14	**	0.05	2.60	***	0.08	2.25	**	1.57
Professional Services	0.02	1.18		0.02	1.51		0.03	1.47		5.84
Management	0.04	0.23		0.00	0.10		0.41	1.50		1.51
Waste	0.02	1.29		0.00	0.25		0.02	0.80		7.50
Education	-0.15	-1.30		0.01	1.11		-0.22	-1.10		8.63
Health	0.00	-0.30		0.01	1.32		0.00	0.32		13.10
Arts and Recreation	-0.01	-0.61		0.00	-0.02		-0.02	-0.58		1.97
Food Services	0.01	0.83		0.03	1.93	*	0.03	1.48		9.92
Public Administration	0.03	0.90		0.01	1.26		0.96	1.23		3.90
Other	0.03	1.83	*	0.00	-0.22		0.02	1.55		3.36

Panel B: Effects by ax-ante politically active industries

	log(Employment)	log(Earnings)	log(Payroll)
	(1)	(2)	(3)
Post × Treated	0.021**	0.008	0.026*
	(0.010)	(0.008)	(0.015)
Post × Treated × Active	0.001	0.006	0.006
	(0.006)	(0.005)	(0.006)
State × Active FE	Yes	Yes	Yes
Year × Pre-CU GDP Growth FE	Y	Y	Y
Year × Pre-CU Gov Party FE	Y	Y	Y
Observations	11,251	11,251	11,251
Adjusted R^2	0.630	0.381	0.688

Table 6: Economic outcomes by firm size and age using QWI data

Note: This table shows changes in state-level total employment, (average) earnings, and total payroll aggregated by firm age and size around *Citizens United*. States affected by the *Citizens United* case (treated states) are those that had bans on corporate or union independent political expenditures pre-*Citizens United*—the bans that the Supreme Court invalidated. This table shows the results from regressions estimated using Equation (1) where the dependent variables are economic outcomes at the state-year level. *Post* is an indicator for 2010 or later (after the *Citizens United* decision). Economic data are from the US Census's QWI and run from 2005 through 2016. Employment is the number of employees. Earnings is average employee earnings: Column (2) includes all workers; Column (3) includes only newly hired workers. Payroll is total payroll. All variables are aggregated to the annual level from quarterly data. Panel A shows the effect by firm size, where Small is an indicator that equals one for outcomes aggregated across firms that have fewer than 50 employees. Panel B shows the effect by firm age, where Young is an indicator that equals one for outcomes aggregated across firms that are five or fewer years old. All specifications include state fixed effects, the fixed effects for state-level growth pre-*Citizens United* (from 2005 to 2009) interacted with year fixed effects, and the fixed effects for pre-*Citizens United* governor party (2010) interacted with year fixed effects. Standard errors, in parentheses, are clustered at the state level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

Panel A: Effects by firm size

	Dependent variable:			
	log(Employment)	log(Earnings)	log(New worker earnings)	log(Payroll)
	(1)	(2)	(3)	(4)
Post × Treated	0.024**	0.023*	0.037	0.044**
	(0.011)	(0.011)	(0.022)	(0.021)
Post × Treated × Small	−0.013	0.002	0.009	−0.007
	(0.010)	(0.006)	(0.007)	(0.010)
State × Size FE	Y	Y	Y	Y
Year × Pre-CU GDP Growth FE	Y	Y	Y	Y
Year × Pre-CU Gov Party FE	Y	Y	Y	Y
Observations	1,152	1,152	1,152	1,152
Adjusted R^2	0.999	0.988	0.956	0.999

Panel B: Effects by firm age

	Dependent variable:			
	log(Employment)	log(Earnings)	log(New worker earnings)	log(Payroll)
	(1)	(2)	(3)	(4)
Post × Treated	0.017	0.022*	0.035	0.038*
	(0.010)	(0.012)	(0.021)	(0.020)
Post × Treated × Young	0.006	0.021	0.033***	0.030
	(0.022)	(0.015)	(0.012)	(0.031)
State × Age FE	Y	Y	Y	Y
Year × Pre-CU GDP Growth FE	Y	Y	Y	Y
Year × Pre-CU Gov Party FE	Y	Y	Y	Y
Observations	1,152	1,152	1,152	1,152
Adjusted R^2	0.999	0.975	0.923	0.998

Table 7: Firm-level employment by Ex-Ante political activity

Note: This table shows changes in firm-level employment around *Citizens United* by ex-ante firm political activity. Firms headquartered in states affected by the *Citizens United* case (treated states) are those that had bans on corporate or union independent political expenditures pre-*Citizens United*—the bans that the Supreme Court invalidated. This table shows the results from regressions estimated using Equation (1) where the dependent variable is the log of firm-level employment among US public firms. *Post* is an indicator for 2010 or later (after the *Citizens United* decision). Federal Contributions is an indicator variable that takes the value of one if a firm made campaign contributions to federal candidates in the 2004, 2006, 2008, or 2010 election cycles and is zero otherwise. S&P 500 State Contributions is an indicator variable that takes the value of one if a firm is in the S&P 500 index and made campaign contributions to state-level election candidates in the 2004, 2006, 2008, or 2010 election cycles, and is zero otherwise. S&P 500 Federal Lobby is an indicator variable that takes the value of one if the firm is in the S&P 500 and engaged in reportable Federal lobbying before *Citizens United*, and is zero otherwise. Large is an indicator variable that takes the value of one if a firm had above median assets in 2009, and is zero otherwise. These three variables form the *Characteristic* variable noted in the regression table below and are interacted with *Post* in their respective specification to fully specify the triple-difference model. Employment and financial data come from Compustat, while political data comes from the Federal Elections Commission and the National Institute on Money in Politics (Open Secrets). The sample runs from 2005 through 2016. All specifications also include the 2009 values of various firm-level controls interacted with *Post*; these controls include the natural logarithm of total assets, the natural logarithm of Tobin's q, leverage, cash flow, and cash/total assets. All specifications include firm fixed effects (which absorb state fixed effects), industry interacted with year fixed effects, the fixed effects for state-level growth pre-*Citizens United* (from 2005 to 2009) interacted with year fixed effects, and the fixed effects for pre-*Citizens United* governor party (2010) interacted with year fixed effects. Standard errors, in parentheses, are clustered at the state level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

	Dependent variable:				
	log(Employment)				
	(1)	(2)	(3)	(4)	(5)
Post × Treated	0.0400*	0.0464*	0.0404*	0.0394*	0.0951***
	(0.0201)	(0.0260)	(0.0229)	(0.0217)	(0.0318)
Post × Treated × Federal Contributions		-0.0259			
		(0.0598)			
Post × Treated × S&P 500 State Contributions			-0.00595		
			(0.0472)		
Post × Treated × S&P 500 Federal Lobby				0.00601	
				(0.0436)	
Post × Treated × Large					-0.101**
					(0.0480)
Firm FE	Yes	Yes	Yes	Yes	Yes
Year × Pre-CU GDP Growth FE	Yes	Yes	Yes	Yes	Yes
Year × Pre-CU Gov Party FE	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes
Post × Political Characteristic	No	Yes	Yes	Yes	Yes
Post × Firm Controls (2009)	Yes	Yes	Yes	Yes	Yes
Observations	28,230	28,230	28,230	28,230	28,230
Adjusted R^2	0.969	0.969	0.969	0.969	0.969

Table 8: Policy Responses: Enforcement actions, Occupational Licensing and Regulatory Freedom

Note: This table shows policy changes in enforcement actions, state-level occupational licensing requirements, and state-level regulatory freedom around *Citizens United*. States affected by the *Citizens United* case (treated states) are those that had bans on corporate or union independent political expenditures pre-*Citizens United*—the bans that the Supreme Court invalidated. This table shows the results from regressions estimated using Equation (1) where the dependent variables are state-level policies measured each year. *Post* is an indicator for 2011 or later (after the *Citizens United* decision). Panel A shows enforcement actions brought against corporations within each state. Columns (1)–(3) show the number of violations enforced by state government agencies. Columns (4)–(6) show the number of violations enforced by federal government agencies. Columns (1) and (4) are all types of enforcement actions; (2) and (5) show enforcement actions brought to enforce labor or consumer rights; (3) and (6) show enforcement actions to enforce capital owners' rights. Data are from the *Good Jobs First*'s Violations Tracker and run from 2005 through 2016. Panel B shows changes in occupational licensing and regulatory freedom within each state. Column (1) shows changes in the employment-weighted occupational license requirements in a given state-year. Column (2) shows the estimated changes in the regulatory freedom, where larger values mean lesser regulatory burden. Data on occupational license requirements come from Sorens et al. (2008), and data on regulatory freedom come from the Cato Institute from their *Freedom in the 50 States* publication and run from 2005 through 2016. All specifications include state fixed effects, the fixed effects for state-level growth pre-*Citizens United* (from 2005 to 2009) interacted with year fixed effects, and the fixed effects for pre-*Citizens United* governor party (2010) interacted with year fixed effects. Standard errors are clustered at the state level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

Panel A: Enforcement actions

	Dependent variable:					
	log(1 + violations)					
	State			*Federal*		
	All	Labor/consumer	Capital	All	Labor/consumer	Capital
	(1)	(2)	(3)	(4)	(5)	(6)
Post × Treated	−0.316	−0.442***	0.055	0.037	0.029	−0.004
	(0.197)	(0.175)	(0.061)	(0.061)	(0.064)	(0.009)
State FE	Y	Y	Y	Y	Y	Y
Year × Pre-CU GDP Growth FE	Y	Y	Y	Y	Y	Y
Year × Pre-CU Gov Party FE	Y	Y	Y	Y	Y	Y
Observations	600	600	600	600	600	600
Adjusted R^2	0.817	0.832	0.545	0.954	0.950	−0.048

Panel B: Occupational Licensing and Regulatory Freedom

	Dependent variable:	
	Occupational Licensure	Regulatory Freedom
	(1)	(2)
Post × Treated	−0.017	0.012***
	(0.011)	(0.004)
State FE	Y	Y
Year × Pre-CU GDP Growth FE	Y	Y
Year × Pre-CU Party FE	Y	Y
Observations	600	600
Adjusted R^2	0.932	0.991

Table 9: Policy Responses: Tax rates and Minimum wage

Note: This table shows policy changes in state-level taxes and minimum wage around *Citizens United*. States affected by the *Citizens United* case (treated states) are those that had bans on corporate or union independent political expenditures pre-*Citizens United*—the bans that the Supreme Court invalidated. This table shows the results from regressions estimated using Equation (1) where the dependent variables are state-level policies measured each year. *Post* is an indicator for 2011 or later (after the *Citizens United* decision). Panel A shows the results for state tax rates (in percent) with data from Baker et al. (2021). Panel B shows the results for the state minimum wage with data from Gopalan et al. (2021): Column (1) uses minimum wage levels (in dollars), and Column (2) uses annual changes in minimum wage. All specifications include state fixed effects, the fixed effects for state-level growth pre-*Citizens United* (from 2005 to 2009) interacted with year fixed effects, and the fixed effects for pre-*Citizens United* governor party (2010) interacted with year fixed effects. Standard errors are clustered at the state level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

Panel A: Tax rate

	Dependent variable:				
	Sales rate	Corporate rate	Top income rate	Property rate	Estate/Inheritance tax
	(1)	(2)	(3)	(4)	(5)
Post × Treated	−0.042	−0.551	−0.359	0.0003	−0.171*
	(0.078)	(0.438)	(0.337)	(0.002)	(0.096)
State FE	Y	Y	Y	Y	Y
Year × Pre-CU GDP Growth FE	Y	Y	Y	Y	Y
Year × Pre-CU Gov Party FE	Y	Y	Y	Y	Y
Observations	550	550	550	550	550
Adjusted R^2	0.986	0.925	0.957	0.999	0.722

Panel B: Minimum wage

	Minimum wage	Δ Minimum wage
	(1)	(2)
Post × Treated	0.052	0.050
	(0.104)	(0.042)
State FE	Y	Y
Year × Pre-CU GDP Growth FE	Y	Y
Year × Pre-CU Gov Party FE	Y	Y
Observations	600	600
Adjusted R^2	0.881	0.156

Table 10: State Revenues, Expenditures, and Subsidies

Note: This table shows the results from regressions estimated using Equation (1) where the dependent variables are state governments' revenues and expenditures as well as subsidies at the state-year level. States affected by the *Citizens United* case (treated states) are those that had bans on corporate or union independent political expenditures pre-*Citizens United*—the bans that the Supreme Court invalidated. *Post* is an indicator for whether the year is 2011 or later (after the *Citizens United* decision). run from 2005 through 2016. Panel A examines state government revenues and expenditures. Column (1), "Pct of total", shows the revenue (expenditure) share coming from each category as a percentage of all revenues (expenditures). Columns (2), (3), and (4) show the regression coefficient, *t*-value, and statistical significance. Panel B examines state- and local-subsidies to corporations. Columns (1)–(3) show regressions with the log of one plus the number of subsidies as dependent variables. Columns (4)–(6) show regressions with the log of one plus the value of subsidies as dependent variables. Columns (1) and (4) report analyses for state government subsidies; columns (2) and (5) for local government; columns (3) and (6) for combined state and local government subsidies. All specifications include state fixed effects, the fixed effects for state-level growth pre-*Citizens United* (from 2005 to 2009) interacted with year fixed effects, and the fixed effects for pre-*Citizens United* governor party (2010) interacted with year fixed effects. Standard errors, in parentheses, are clustered at the state level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

Panel A: State revenues and expenditures

		log(Level + 1)			
		(1) Pct of total	(2) Coef	(3) t-value	
	Revenues				
1	Total Revenue	100.000	0.015	0.517	
2	–General revenue	82.855	0.018	0.698	
3	—-Intergovernmental revenue	26.793	0.012	0.420	
4	—-Taxes	40.725	0.037	0.925	
5	——General sales	12.831	0.078	2.039	**
6	——Selective sales	6.539	0.032	0.900	
7	——License taxes	2.558	0.012	0.240	
8	——Individual income tax	14.385	0.001	0.019	
9	——Corporate income tax	2.307	-0.342	-1.355	
10	——Other taxes	2.104	0.041	0.387	
11	—-Current charges	8.807	-0.029	-0.753	
12	—-Miscellaneous general revenue	6.531	-0.018	-0.328	
13	–Utility revenue	0.778	1.473	1.290	
14	–Liquor store revenue	0.347	1.193	1.249	
15	Insurance trust revenue	16.019	0.003	0.024	
	Expenditures				
16	Total expenditure	100.000	0.021	1.105	
17	–General expenditure	25.633	0.020	1.015	
18	—-Intergovernmental expenditure	74.367	0.012	0.312	
19	—-Direct expenditure	50.333	0.021	1.128	
20	——Current operation	6.105	0.020	0.817	
21	——Capital outlay	13.502	0.119	2.375	**
22	——Insurance benefits and repayments	2.027	-0.026	-1.196	
23	——Assistance and subsidies	2.400	0.013	0.167	
24	——Interest on debt	12.442	0.047	0.690	
25	Exhibit: Salaries and wages	84.859	0.038	0.954	
26	——Education	30.215	0.022	1.060	
27	——Public welfare	25.465	-0.025	-0.915	
28	——Hospitals	3.269	-0.043	-0.428	
29	——Health	3.048	-0.024	-0.257	
30	——Highways	5.870	0.111	1.958	*
31	——Police protection	0.734	0.094	1.882	*
32	——Correction	2.544	0.042	1.585	
33	——Natural resources	1.135	0.083	1.449	
34	——Parks and recreation	0.308	0.097	1.028	
35	——Government administration	2.934	0.038	1.019	
36	——Interest on general debt	2.283	0.049	0.704	
37	——Other and unallocable	6.014	0.047	0.484	
38	–Utility expenditure	1.599	-0.118	-0.082	
39	–Liquor store expenditure	0.284	2.008	1.760	*
40	–Insurance trust expenditure	13.502	-0.026	-1.196	

Panel B: Subsidies

	Dependent variable:					
	N (state)	N (local)	N (total)	Value (state)	Value (local)	Value (total)
	(1)	(2)	(3)	(4)	(5)	(6)
Post × Treated	0.290	−0.206	0.171	1.387	−0.660	0.449
	(0.413)	(0.222)	(0.335)	(1.317)	(0.943)	(1.108)
State FE	Y	Y	Y	Y	Y	Y
Year × Pre-CU GDP Growth FE	Y	Y	Y	Y	Y	Y
Year × Pre-CU Gov Party FE	Y	Y	Y	Y	Y	Y
Observations	561	561	561	561	561	561
Adjusted R^2	0.551	0.854	0.632	0.512	0.726	0.563

Appendix

A Variable Definitions

Table A1: Variable definitions and sources

We provide variable definitions and data sources for variables used in our analysis.

Variable	Source	Definition
Main variable for treatment definition		
Treated	Authors' collection	An indicator equal one for the 23 states that had previously adopted a ban on independent political expenditures in state-level political elections—a ban that was invalidated by the *Citizens United*, and is zero otherwise
Aggregate economic outcomes		
GDP	BEA	Current dollar gross domestic output
Labor income	BEA	Current dollar employee compensation
Capital income	BEA	Current dollar gross operating surplus
AGI	IRS	Reported adjusted gross income
Salary/wage income	IRS	Reported income from salaries and wages
AGI - Salary/wage income	IRS	AGI minus salary and wage income
Employment	QWI	Total number of jobs
Earnings	QWI	Average monthly earnings of employees with stable jobs
New worker earnings	QWI	Average monthly earnings of newly hired employees
Payroll	QWI	Total quarterly payroll for all jobs
Additional control variables		
2008 Obama vote share	Wikipedia	2008 share voting for Obama
Population	2010 Census	State population
Median household income	2010 Census	State median household income
Fraction with bachelors	2010 Census	Fraction of population with bachelors degree
Unemployment	2010 Census	Unemployed fraction
90+ days mortgage delinquency	2010 GSE data	Fraction mortgage borrowers 90+ days delinquent
House price change 2002-2006; 2007-2010	HUD	Change in house prices from given years
GDP; Labor income; Capital income change (2004-2009)	BEA	2004-2009 change in GDP, Labor income, Capital income
Political outcomes		
Ad spending	Ad$pender	Millions of dollars spent on all political advertising media
Direct political campaign contributions	The National Institute for Money in State Politics	Millions of dollars spent in state-level individual political contributions
Republican house (senate)		Fraction of republican representatives in lower state house
New house (senate)	Various sources, primarily National Conference of State Legislatures and states' election website	Fraction of representatives that are new as of the time t legislative session in the lower (upper) state house
New republican (democrat) house (senate)	Various sources, primarily National Conference of State Legislatures and states' election website	Fraction of republican (democrat) representatives that are new as of the time t legislative session in the lower (upper) state house
Republican governor	Authors' collection	An indicator for whether the governor is republican
New governor party	Authors' collection	An indicator for whether the governor's party is different from that in 2010
House (senate) differences	Shor and McCarty (2011)	The difference in idealogy score for the median Democrat and Republican in a legislative chamber-cycle

Table A1: Variable definitions and sources (continued)

We provide variable definitions and data sources for variables used in our analysis.

Variable	Source	Definition
Variables used for firm-level analysis		
Employment	Compustat	Total firm employment (variable "emp")
Size	Compustat	Total firm assets (variable "at")
Tobin's q	Compustat	Calculated as (at - ceq + csho×prcc_f)/at
Leverage	Compustat	Calculated as lt/at
Cash	Compustat	Calculated as che/at
Cashflow	Compustat	Calculated as (ni + dp)/at
Federal contributions	Opensecrets	An indicator variable that takes the value of one if a firm made federal political contributions before or in the 2010 election cycle
S&P 500 state contributions	The National Institute for Money in State Politics	An indicator variable that takes the value of one if an S&P 500 firm made state political contributions before or in the 2010 election cycle
S&P 500 federal lobby	The National Institute for Money in State Politics	An indicator variable that takes the value of one if an S&P 500 firm engaged in federal lobbying before or in the 2010 election cycle
Large	Compustat	An indicator if the value of firm assets in 2009 was greater than the median
Policy outcomes		
Violations	GoodJobsFirst	Counts of legal enforcements against firms from state or federal regulators
Subsidies	GoodJobsFirst	Counts or $s of subsidies to firms from the state or local governments
Occupational licensure	Sorens et al. (2008)	Employment-weighted licensing requirements
Regulatory freedom	Sorens et al. (2008)	The Cato Institute's regulatory freedom index
Tax rates	Baker et al. (2021)	State-level tax rates
Minimum wage	Gopalan et al. (2021)	State-level minimum wages
Aggregate state revenues and spending		
State revenues	Annual Survey of State and Local Government Finances provided by the US Census	Various accounts
State expenditures	Annual Survey of State and Local Government Finances provided by the US Census	Various accounts

B Robustness with Propensity Score Matching

As a robustness check, we redo our main analysis on economic outcomes (incomes from the BEA and the IRS) using a propensity score matching approach. In particular, we match treated and control states using the covariates discussed in Table 1, Panel B, and rerun the difference-in-difference and event-study analyses on the matched sample. Recall from this analysis that the treated and control samples differed significantly on the Financial Crisis-related variables: 2010 mortgage delinquencies, house price increases going into the crisis, and house price declines coming out of the crisis. In particular, control states had somewhat greater house price run-ups prior to the Crisis, house price declines, and mortgage delinquencies than treated states during the Crisis. This analysis aims to eliminate this potential Crisis-related confounder.

Table B.1, Panel A shows the (mean) differences between the treated states and the matched control sample of states (second column) and the p-value of the difference in means (third column). The matching approach successfully eliminates all statistically significant differences between the samples. In particular, the potentially concerning differential exposure to the Financial Crisis related variables (mortgage delinquency, house price run-ups pre-crisis, and house price declines post-crisis) are removed in the matched sample. Additionally, the small, though marginally statistically significant difference in the likelihood of treated states having Republican governors is completely eliminated.

Panels B and C show the results for the BEA measures and the IRS measures, respectively. The results are qualitatively and quantitatively unchanged. Figure B.1 shows the corresponding event studies. Again, the results are qualitatively similar. We take these results as additional confirmation of our main findings and as further evidence that our empirical approach is picking up differences caused by the *Citizens United* treatment.

Table B.1: Income: Total, Capital, and Labor. Results with Propensity Score Matching

Note: This figure shows changes in economic outcomes around *Citizens United* using a propensity score matching estimator. Treated and control states are matched on the covariates shown in Panel A, which shows the differences between treated states and the matched control sample of states and the p-values for the differences in means. Panel B shows the difference-in-difference estimates for the BEA measures. Panel C shows the difference-in-difference estimates for the IRS measures. All specifications include year and state fixed effects. Standard errors, in parentheses, are clustered at the state level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

Panel A: Covariate balance

Variable	Treated - Control	p-value
2008 Obama vote share	-0.02	0.43
Republican governor	-0.10	0.53
Population	0.003	0.94
Median household income	-0.24	0.45
log(GDP) (2009)	-0.20	0.53
log(labor income) (2009)	-0.23	0.48
log(capital income) (2009)	-0.16	0.62
Fraction with bachelors (2010)	-0.002	0.90
Unemployment (2010)	-0.0004	0.94
90+ days mortgage delinquency (2010)	-0.002	0.48
House price change 2002-2006	-0.08	0.15
House price change 2007-2010	0.02	0.34
GDP Change (2004-2009)	0.02	0.39
Labor income change (2004-2009)	0.01	0.77
Capital income change (2004-2009)	0.03	0.40

Panel B: BEA data

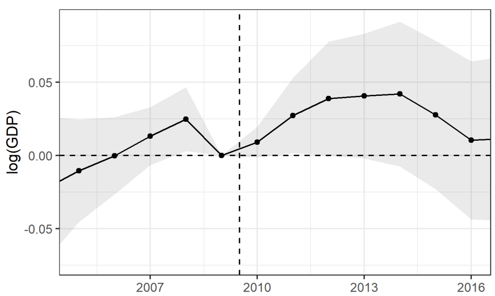
	Dependent variable:		
	log(GDP)	log(capital income)	log(labor income)
	(1)	(2)	(3)
Post × Treated	0.023	0.003	0.035*
	(0.023)	(0.033)	(0.018)
State FE	Y	Y	Y
Year FE	Y	Y	Y
Observations	504	504	504
Adjusted R^2	0.997	0.995	0.999

Panel C: IRS data

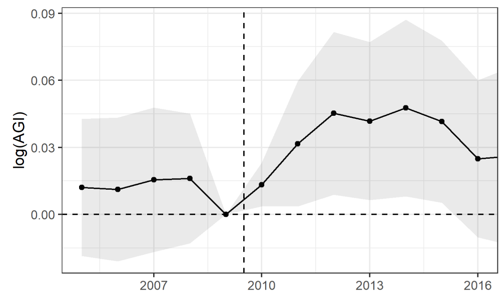
	Dependent variable:		
	log(AGI)	log(AGI - SW)	log(SW)
	(1)	(2)	(3)
Post × Treated	0.024	0.014	0.029*
	(0.017)	(0.023)	(0.016)
State FE	Y	Y	Y
Year FE	Y	Y	Y
Observations	504	504	504
Adjusted R^2	0.998	0.996	0.999

Figure B.1: Income: Total, Capital, and Labor. Results with propensity score matching

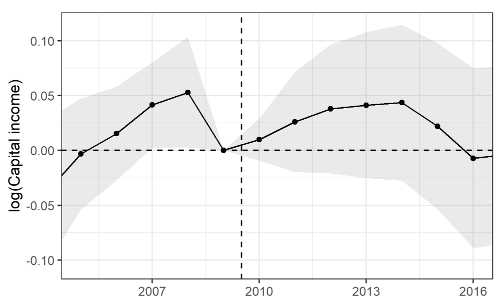
Note: This figure shows changes in economic outcomes around *Citizens United* using a propensity score matching estimator. Treated and control states are matched on the covariates shown in Panel A of Table B.1. Panels (a) and (b) show total income; Panels (c) and (d) show capital income. Panels (e) and (f) show labor income. Panels (a), (c), and (e) use the BEA data; Panels (b), (d), and (f) use the analogous IRS data. The shaded region is the 95% confidence interval. All specifications include year and state fixed effects. Standard errors are clustered at the state level.



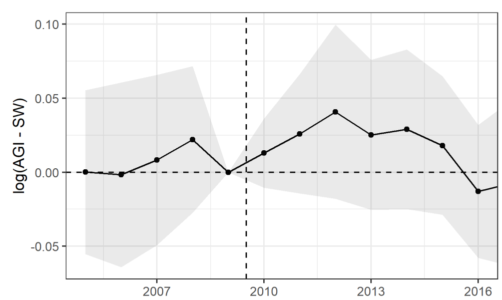
(a) BEA: GDP



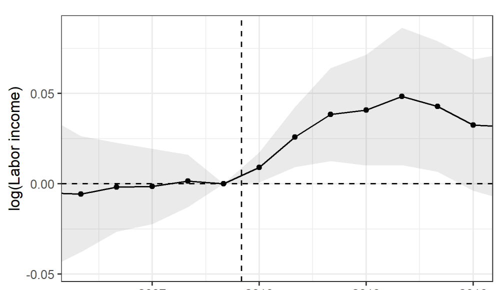
(b) IRS: AGI



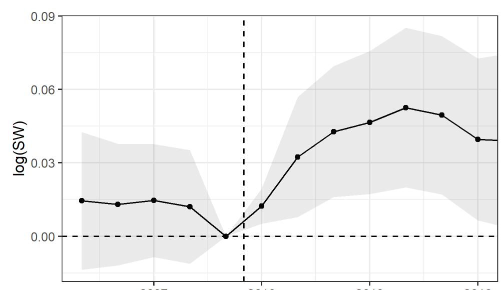
(c) BEA: Capital income



(d) IRS: Non-SW income



(e) BEA: Labor income

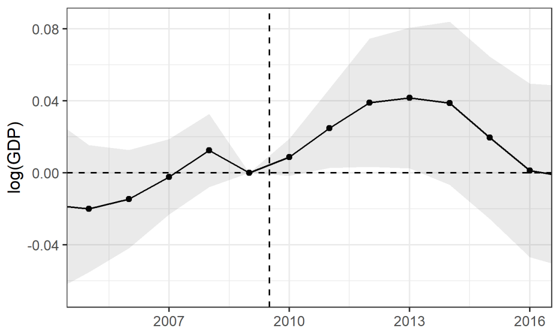


(f) IRS: SW income

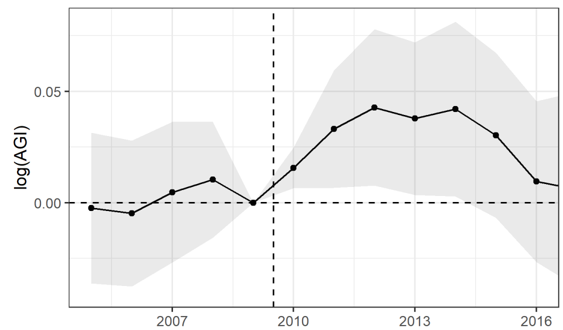
C Additional Tables and Figures

Figure C.1: Income: Total, Capital, and Labor. Results with only state and year fixed effects

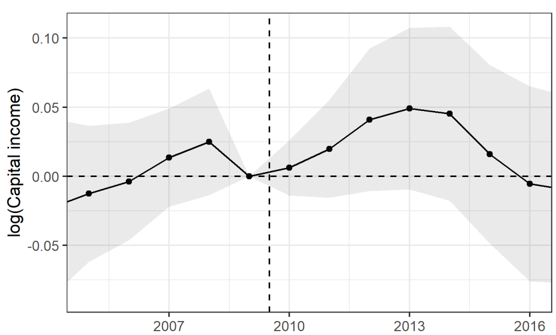
Note: This figure shows changes in state-level economic outcomes around *Citizens United*. States affected by the *Citizens United* case (treated states) are those that had bans on corporate or union independent political expenditures pre-*Citizens United*—the bans that the Supreme Court invalidated. The figures show the annual coefficients and 95% confidence intervals from regressions estimated using a version of Equation (2) which only includes state fixed effects and year fixed effects. The outcomes are total, capital, or labor incomes from the BEA and the IRS. Panels (a) and (b) show total income (GDP for BEA; AGI for IRS). Panels (c) and (d) show capital income (gross operating surplus for BEA; AGI less salary and wage income for IRS). Panels (e) and (f) show labor income (employee compensation for BEA; salary and wage (SW) income for IRS). Panels (a), (c), and (e) use BEA data; Panels (b), (d), and (f) use analogous IRS data. Standard errors are clustered at the state level.



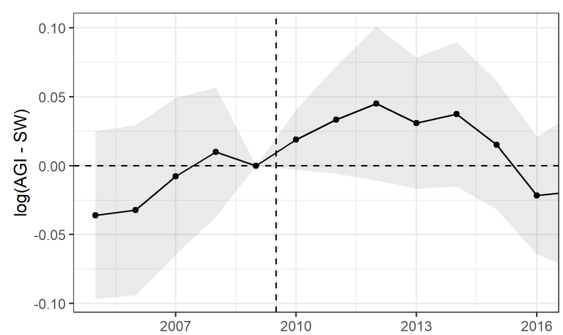
(a) BEA: GDP



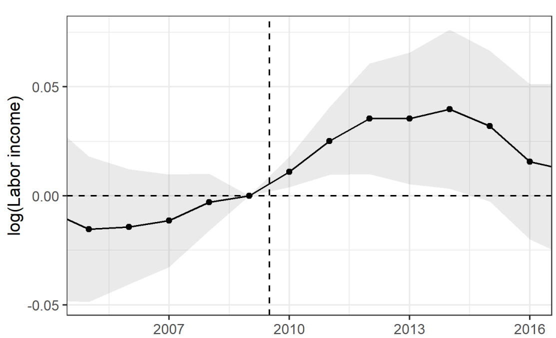
(b) IRS: AGI



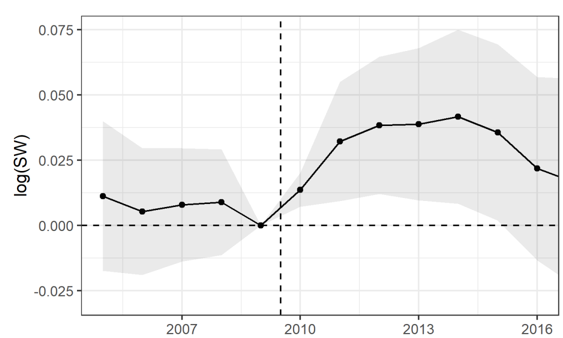
(c) BEA: Capital income



(d) IRS: Non-SW income



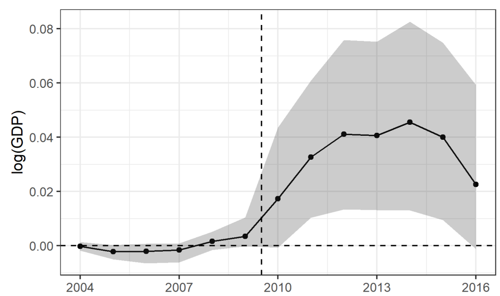
(e) BEA: Labor income



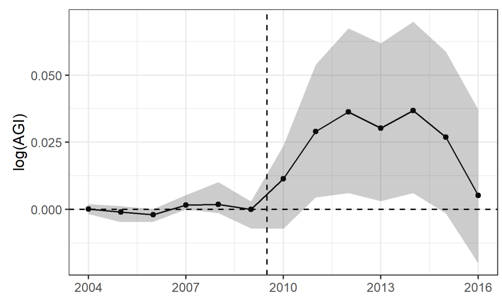
(f) IRS: SW income

Figure C.2: Income: Total, Capital, and Labor. Results with Synthetic Controls

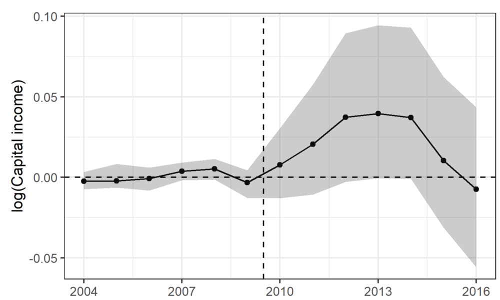
Note: This figure shows changes in economic outcomes around *Citizens United* using a synthetic controls estimator. Panels (a) and (b) show total income; Panels (c) and (d) show capital income. Panels (e) and (f) show labor income. Panels (a), (c), and (e) use the BEA data; Panels (b), (d), and (f) use the analogous IRS data. Shaded regions show bootstrapped 95% confidence intervals.



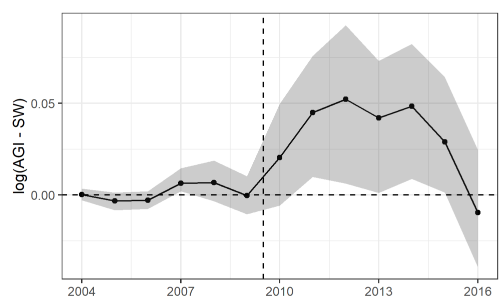
(a) BEA: GDP



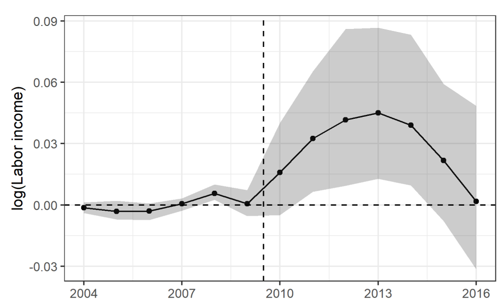
(b) IRS: AGI



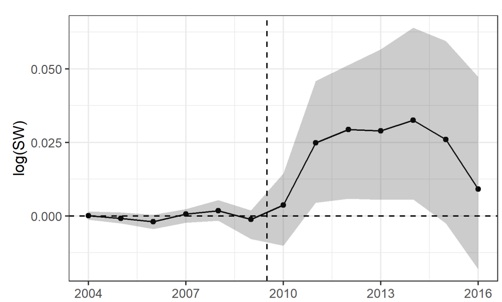
(c) BEA: Capital income



(d) IRS: Non-SW income



(e) BEA: Labor income



(f) IRS: SW income

Table C.1: Income: Total, Capital, and Labor. Results with only state and year fixed effects

Note: This table shows changes in state-level economic outcomes around *Citizens United*. States affected by the *Citizens United* case (treated states) are those that had bans on corporate or union independent political expenditures pre-*Citizens United*—the bans that the Supreme Court invalidated. This table shows the results from regressions estimated using a version of Equation (1) which only includes state fixed effects and year fixed effects. The outcomes are economic outcomes at the state-year level. *Post* is an indicator for 2010 or later (after the *Citizens United* decision). Data in Panel A are from the BEA. Data in Panel B are from the IRS. Both run from 2005 through 2016. In each Panel, Column (1) is a measure of aggregate income (GDP for BEA; AGI for IRS). Column (2) is a measure of capital income (gross operating surplus for BEA; AGI less salary and wage (SW) income for IRS). Column (3) is a measure of labor income (employee compensation for BEA; salary and wage income for IRS). Standard errors, in parentheses, are clustered at the state level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

Panel A: BEA data

	Dependent variable:		
	log(GDP)	log(Capital income)	log(Labor income)
	(1)	(2)	(3)
Post × Treated	0.027	0.015	0.036*
	(0.020)	(0.027)	(0.018)
State FE	Y	Y	Y
Year FE	Y	Y	Y
Observations	600	600	600
Adjusted R^2	0.998	0.995	0.999

Panel B: IRS data

	Dependent variable:		
	log(AGI)	log(AGI - SW)	log(SW)
	(1)	(2)	(3)
Post × Treated	0.029*	0.036	0.025
	(0.017)	(0.022)	(0.015)
State FE	Y	Y	Y
Year FE	Y	Y	Y
Observations	600	600	600
Adjusted R^2	0.998	0.997	0.999

Table C.2: Income: Total, Capital, and Labor. Results Controlling for House Price Changes

Note: This table shows changes in state-level economic outcomes around *Citizens United* while controlling for house price changes prior to the Financial Crisis. States affected by the *Citizens United* case (treated states) are those that had bans on corporate or union independent political expenditures pre-*Citizens United*—the bans that the Supreme Court invalidated. This table shows the results from regressions estimated using Equation (1) where the dependent variables are economic outcomes at the state-year level. *Post* is an indicator for 2010 or later (after the *Citizens United* decision). Data in Panel A are from the BEA. Data in Panel B are from the IRS. Both run from 2005 through 2016. In each panel, Column (1) is a measure of aggregate income (GDP for BEA; AGI for IRS). Column (2) is a measure of capital income (gross operating surplus for BEA; AGI less salary and wage income for IRS). Column (3) is a measure of labor income (employee compensation for BEA; salary and wage income for IRS). Column (4) is a measure of the labor share of income (labor income divided by GDP for BEA; salary and wage income divided by AGI for IRS). All specifications include state fixed effects, year times 2010 governor party fixed effects, and year times pre-*Citizens United* house price growth between 2002 and 2006 fixed effects. Standard errors, in parentheses, are clustered at the state level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

Panel A: BEA data

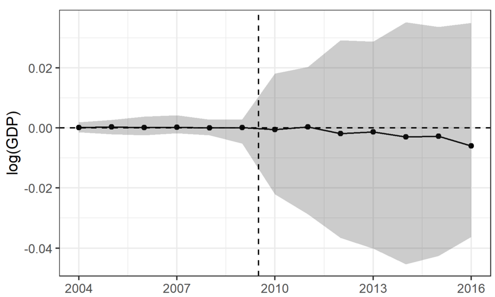
	Dependent variable:		
	log(GDP)	log(Capital income)	log(Labor income)
	(1)	(2)	(3)
Post × Treated	0.022	−0.007	0.043*
	(0.025)	(0.034)	(0.021)
State FE	Y	Y	Y
Year × Pre-CU House Price Growth FE	Y	Y	Y
Year × Pre-CU Gov Party FE	Y	Y	Y
Observations	600	600	600
Adjusted R^2	0.998	0.995	0.999

Panel B: IRS data

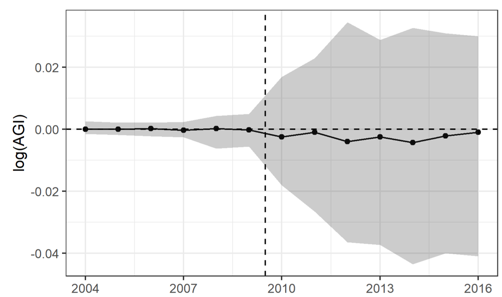
	Dependent variable:		
	log(AGI)	log(AGI - SW)	log(SW)
	(1)	(2)	(3)
Post × Treated	0.032	0.023	0.036**
	(0.020)	(0.026)	(0.018)
State FE	Y	Y	Y
Year × Pre-CU House Price Growth FE	Y	Y	Y
Year × Pre-CU Gov Party FE	Y	Y	Y
Observations	600	600	600
Adjusted R^2	0.999	0.997	0.999

Figure C.3: Income: Total, Capital, and Labor. Synthetic controls placebo test

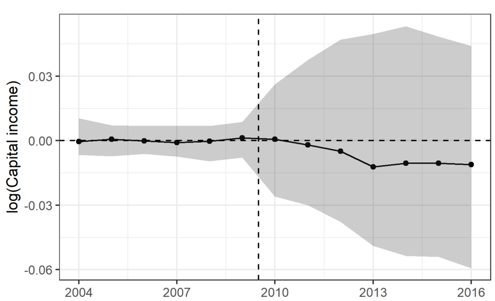
Note: This figure shows changes in economic outcomes around *Citizens United* using a synthetic controls estimator. The results mirror those in Figure C.2, except that treatment is randomly assigned to states 50 times and standard errors are bootstrapped. Panels (a) and (b) show total income; Panels (c) and (d) show capital income. Panels (e) and (f) show labor income. Panels (a), (c), and (e) use the BEA data; Panels (b), (d), and (f) use the analogous IRS data.



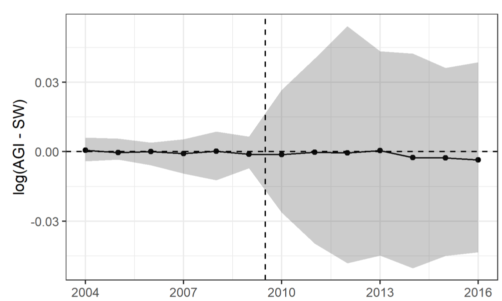
(a) BEA: GDP



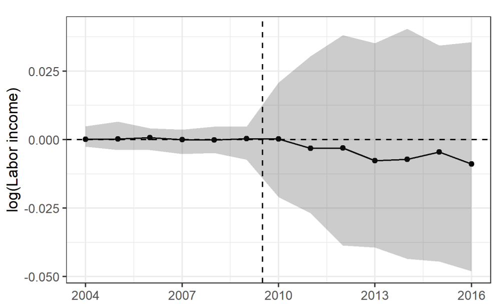
(b) IRS: AGI



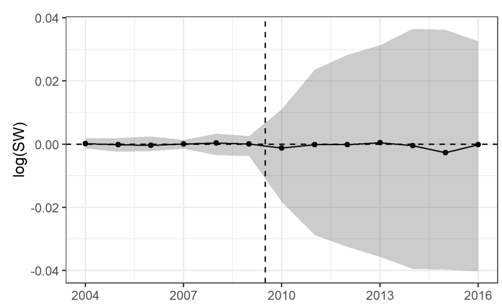
(c) BEA: Capital income



(d) IRS: Non-SW income



(e) BEA: Labor income



(f) IRS: SW income

Figure C.4: Political Advertising Expenditures

Note: This figure shows changes in political advertising expenditures around *Citizens United*. States affected by the *Citizens United* case (treated states) are those that had bans on corporate or union independent political expenditures pre-*Citizens United*—the bans that the Supreme Court invalidated. This figure shows the time series coefficients from regressions estimated using Equation (2) where the outcome is (log) political advertising spending. The dots represent the coefficient estimates (with two-year, election-cycle-length increments) and the shaded region is the 95% confidence interval. Data are from Ad$pender. All specifications include state fixed effects, the fixed effects for state-level growth pre-*Citizens United* (from 2005 to 2009) interacted with year fixed effects, and the fixed effects for pre-*Citizens United* governor party (2010) interacted with year fixed effects. Standard errors are clustered at the state level.

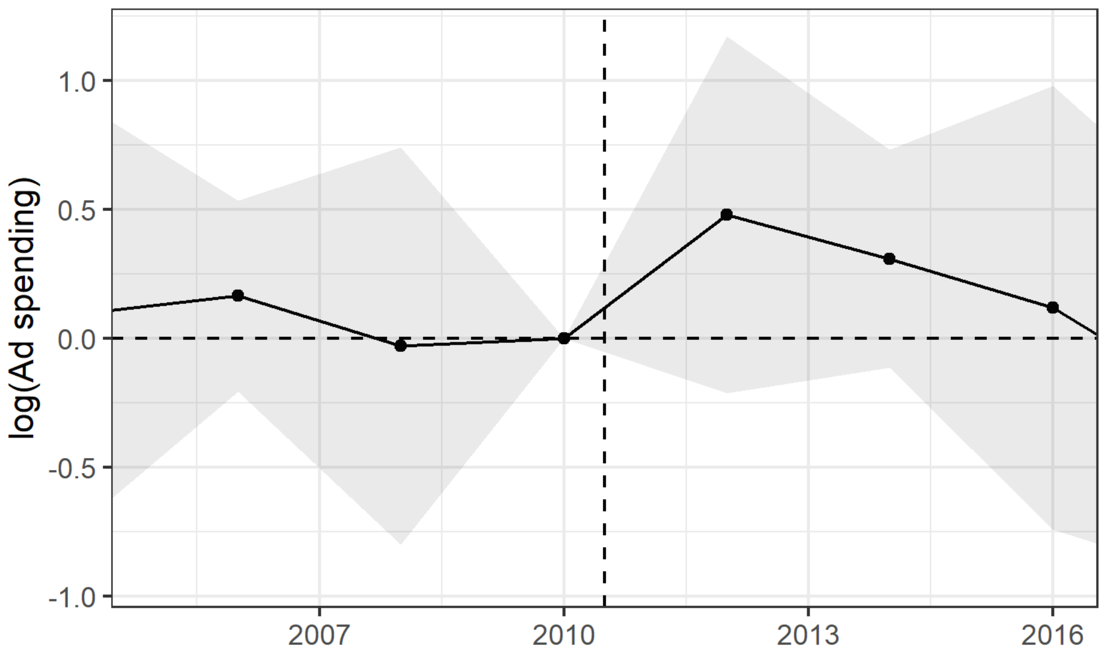


Table C.3: POLITICAL TURNOVER IN STATE-LEVEL RACES

Note: This table shows changes in state-level political turnover outcomes around *Citizens United*. States affected by the *Citizens United* case (treated states) are those that had bans on corporate or union independent political expenditures pre-*Citizens United*—the bans that the Supreme Court invalidated. This table shows the results from regressions estimated using Equation (1) where the dependent variables are political outcomes at the state-election-cycle level. *Post* is an indicator for 2011 or later (after the *Citizens United* decision). Data are bucketed into political cycle (two-year) frequency and include three pre-*Citizens United* periods (2005-2006, 2007-2008, 2009-2010) and three post-*Citizens United* political cycles (2011-2012, 2013-2014, and 2015-2016). Panel A shows turnover outcomes for state governors. New governor party is whether the governor party differs from the 2010 governor party. Republican governor is whether the governor is a republican. New party given R is whether the governor party is different, given that the 2010 party was republican. New party given D is whether the governor party is new, given whether the 2010 party was democrat. Panel B shows turnover outcomes for state Houses (lower state legislatures) (Columns (1)–(4)) and state Senates (upper state legislatures) (Columns (5)–(8)). *New* is the fraction of legislators that are new relative to the previous election cycle. *Rep* is the fraction of Republican legislators. *New Rep* and *New Dem* are the fraction of new Republican and Democratic legislators, respectively. Data are collected by authors (for governors) and by Shor and McCarty (2011) (for legislatures, missing some states). All specifications include state fixed effects, and time interacted with state-level growth pre-*Citizens United* (from 2005 to 2009) fixed effects. Panel B additionally includes the fixed effects for pre-*Citizens United* governor party (2010) interacted with year fixed effects. Standard errors are clustered at the state level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

Panel A: Political turnover of governors

	Dependent variable:			
	New governor party	Republican governor	New party given R	New party given D
	(1)	(2)	(3)	(4)
Post × Treated	0.272**	0.103	0.273	0.234
	(0.125)	(0.149)	(0.211)	(0.165)
State FE	Y	Y	Y	Y
Year × Pre-CU GDP Growth FE	Y	Y	Y	Y
Observations	300	300	144	156
Adjusted R^2	0.232	0.332	0.246	0.207

Panel B: Political turnover in the state legislatures

	Dependent variable:							
	State Houses				State Senates			
	New	Rep	New Rep	New Dem	New	Rep	New Rep	New Dem
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post × Treated	0.024	0.032	0.028*	−0.004	0.026	0.026	0.016	0.012
	(0.017)	(0.020)	(0.017)	(0.012)	(0.025)	(0.030)	(0.019)	(0.014)
State FE	Y	Y	Y	Y	Y	Y	Y	Y
Year × Pre-CU GDP Growth FE	Y	Y	Y	Y	Y	Y	Y	Y
Year × Pre-CU Gov Party FE	Y	Y	Y	Y	Y	Y	Y	Y
Observations	240	240	240	240	222	222	222	222
Adjusted R^2	0.553	0.874	0.562	0.522	0.289	0.839	0.350	0.212

Table C.4: Income: Total, Capital, and Labor. Results for States with Corporate and Union Bans Only

Note: This table shows changes in state-level economic outcomes around *Citizens United*. States affected by the *Citizens United* case (treated states) are those with bans on corporate **and** union independent political expenditures pre-*Citizens United*—the bans that were invalidated by the court decision. This is in contrast to Table 2 in the paper body which examines bans on corporate **or** corporate *and* union expenditures. This table shows the results from regressions estimated using Equation (1) where the dependent variables are economic outcomes at the state-year level. *Post* is an indicator for whether the year is 2010 or later (after the *Citizens United* decision). Data in Panel A are from the BEA. Data in Panel B are from the IRS. Both run from 2005 through 2016. In each panel, Column (1) is a measure of aggregate income (GDP for BEA; AGI for IRS). Column (2) is a measure of capital income (gross operating surplus for BEA; AGI less salary and wage income for IRS). Column (3) is a measure of labor income (employee compensation for BEA; salary and wage income for IRS). All specifications include state fixed effects, the fixed effects for state-level growth pre-*Citizens United* (from 2005 to 2009) interacted with year fixed effects, and the fixed effects for pre-*Citizens United* governor party (2010) interacted with year fixed effects. Standard errors, in parentheses, are clustered at the state level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

Panel A: BEA data

	Dependent variable:		
	log(GDP)	log(Capital income)	log(Labor income)
	(1)	(2)	(3)
Post × Treated	0.032	−0.011	0.036**
	(0.020)	(0.033)	(0.018)
State FE	Y	Y	Y
Year × Pre-CU GDP Growth FE	Y	Y	Y
Year × Pre-CU Gov Party FE	Y	Y	Y
Observations	600	600	600
Adjusted R^2	0.999	0.996	0.999

Panel B: IRS data

	Dependent variable:		
	log(AGI)	log(AGI - SW)	log(SW)
	(1)	(2)	(3)
Post × Treated	0.031*	0.040	0.026**
	(0.017)	(0.027)	(0.013)
State FE	Y	Y	Y
Year × Pre-CU GDP Growth FE	Y	Y	Y
Year × Pre-CU Gov Party FE	Y	Y	Y
Observations	504	504	504
Adjusted R^2	0.999	0.997	0.999