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

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Abstract

We examine whether corporate money in politics benefits capital and hurts labor using the 2010 Supreme Court ruling *Citizens United*, which rendered bans on political election spending unconstitutional. In difference-in-difference analyses, states with newly overturned bans experience increases in both capital *and* labor income relative to states without bans. We find evidence consistent with increased political spending spurring political competition. This leads to policies that benefit a broader set of constituents compared to alternate forms of political influence like lobbying. Affected states see increased political turnover and reduced regulatory burdens. The economic effects are stronger among ex-ante politically inactive firms.

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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

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 (Zingales, 2017). 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] Simultaneously, labor's share of output has gone down while corporate profits have likely risen (see, e.g., Syverson 2019).[2] Critics of corporate money in politics have connected these trends and frequently argue that money allows firms to capture the political process. Their arguments are often framed as a dichotomy in which firms (and their capital providers, henceforth capital), through political activity, obtain economic benefits at the expense of workers or other stakeholders.[3] 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).

[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 1 shows that the share of political contributions coming from labor interests has been in decline.

[2]Seminal work that documents these trends include Elsby et al. (2013); Karabarbounis and Neiman (2014); Gutiérrez and Philippon (2017); Hall (2018); Grullon et al. (2019); Autor et al. (2020); De Loecker et al. (2020); and Barkai (2020), among others. An active area of debate in the macro literature concerns whether the declining labor share of output represents a change in the "fair" return to capital providers of firms or an increase in their abnormal profits stemming from their increased market power. While this debate is important, our analysis abstracts from the difficult problem of measuring the two types of profits and focuses on *total* returns to capital providers, which we refer to as payments to capital in our analysis.

[3]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`.

However, it is also possible that the sharp increase of money in politics *in aggregate* does not benefit capital at the expense of labor. Rather, increasing money in politics may result in policies that benefit *both* capital and labor. 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, new constituents may enter the political process, thereby increasing political competition.[4] Increased political competition may then lead politicians to cater to the interests of a wider set of constituents. Catering to a wider set of constituents potentially grows the metaphorical economic pie rather than simply dividing it differently to benefit narrow, politically connected interests. Thus, whether the firm-level results (that firm political activity increases firm value) can be aggregated to the total factor level of income to capital and labor depends on whether increased money in politics can strengthen or displace "politically entrenched" interests in equilibrium.

In this paper, we ask whether increasing money in politics—specifically spending in elections—leads to better economic outcomes for capital at the expense of labor or to better economic outcomes for *both* capital and labor. We ask this question in the context of *Citizens United*,[5] 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 like lobbying.[6] *Citizens United* led to a huge increase in political spending in elections. We use this event as a natural experiment in a difference-in-difference design to examine how the economic outcomes of workers and capital providers change 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).

Using state-level economic data on factor incomes 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) *increased* by between 3–4% in states affected by *Citizens United* in the years following the decision. These gains accrue primarily to labor: labor income increases by approximately 4% in treated states, and the effect persists for up to six years after the event. We find positive but statistically insignificant effects for capital income, which is measured with more noise, with increases of roughly 2–3% following the decision.[7] Thus, the labor share of income is largely unchanged. These results suggest that money in politics increases aggregate economic output and that labor (and likely) capital share in the gains.

[4]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.

[5]558 U.S. 50 (2010).

[6]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 corporate aspect of *Citizens United*.

[7]N.B., we do not attempt to distinguish between "fair" returns to capital and abnormal profits stemming from firm market power that accrue to capital providers in our analysis and focus instead on *total* returns to capital providers.

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: they have a similar 2008 Obama vote shares, populations, GDP, labor and capital income, education levels, and unemployment levels. Treated and control states do differ in some respects: treated states are slightly more likely to have a Democratic governor prior to the case, and control states had slightly more exposure to the Financial Crisis (e.g., the magnitude of house price changes pre-crisis). To tackle these identification concerns, we ensure that our results are robust to dynamically controlling for the party of the governor pre-*Citizens United* and for exposure to the Financial Crisis. 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*. Finally, our results are robust to implementing a synthetic controls estimation, which explicitly address potential concerns about pre-trends.

We provide evidence that the mechanism driving our results is that increased money in politics leads to a wider political participation, which in turn, leads to greater political competition, and results in the adoption of more growth-friendly policies. That is, money in politics has a relatively low entry cost relative to other methods of exerting political influence, such as lobbying, personal connections, and revolving door connections. When few firms are able to exert political influence via these higher entry costs methods, they push for rent-seeking, growth-reducing policies. With a lower cost of political entry facilitated by increased money in politics, more firms can push for their political preferences, with the net effects being reflected in elected politicians that 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 caused firms with a history of political activism to lose value.

In support of this increased political competition mechanism, we first 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 are 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 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 that the number of occupations with licensing requirements decreases in treated states, allowing for lower-cost entry of workers in previously restricted occupations. Additionally, we find that 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. We also find consistent and economically large (though typically not statistically significant) evidence for reductions in corporate and personal income tax rates in treated states.

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 much 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 and citizens 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 that labor-friendly policies such as the minimum wage, changed. The second possibility is that increased economic output could be driven by increased government spending and its macroeconomic multiplier effect. However, we examine whether state-level government expenditures increased in treated states after the court ruling and find that, 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 factors of production benefit from increased money in politics: capital or labor. 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 a first-best outcome would be to have a reduced scope for political influence of all forms, including lobbying or hiring from the revolving door, but that once some groups have access to politicians it might be economically beneficial to increase the ability of all groups to have access to politicians.

The paper proceeds as follows. Section 1 reviews the literature. Section 2 describes the data. Sections 3 and 4 examine the political and economic outcomes. Section 5 examines mechanisms. Section 6 concludes.

1 Related Literature

Our results contribute to several areas of the literature. 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 (measured in various ways) 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 literature 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; Brogaard et al., 2021), and weaken regulatory enforcement (e.g., Correia, 2014; Mehta and Zhao, 2020; Mehta et al., 2020; Tenekedjieva, 2021; Akey et al., 2021; Bourveau et al., 2021; Baker 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). Our paper contributes to this literature by highlighting that increased corporate political activity does not necessarily only advance the interests of shareholders but can also have positive effects on the income of firms' workers.

We also contribute to the growing literature on the interactions between labor and finance, which studies the real effects of corporate decisions on workers.[8] 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

[8] 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).

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 increased ability of corporations to spend money on elections leads to an increase in income of both capital and labor providers.

Our paper also contributes to the ongoing research on the secular evolution of factor shares in the macroeconomic literature. Much research documents a decline in the share of GDP going to labor in many industries and nations over recent decades (e.g., Elsby et al. 2013; Karabarbounis and Neiman 2014). However, there is less consensus on what are the causes of the decline in the labor share.[9] A number of researchers have been sounding an alarm about the growth of the market power of large firms in the US economy (Philippon, 2019; De Loecker et al., 2020) as well as their influence over the political process and policies being implemented that benefit those large, incumbent firms (Zingales, 2017). Some scholars have argued that weak antitrust enforcement could exacerbate increased corporate market power (Philippon, 2019), and recent empirical evidence suggests that tougher antitrust enforcement leads to better worker outcomes in terms of employment and labor income (Babina et al., 2022). However, empirical evidence is scarce on whether money in politics allows incumbent firms to benefit at the expense of labor. We contribute to this debate by examining whether the distribution of economic gains to labor versus capital providers was affected by increased money in politics due to the 2010 Supreme Court decision *Citizens United*, which represents one of the biggest changes to election campaign finance rules in the post-World War II era. We find that labor income actually increases following this decision in the affected states, with more muted increases to capital income, and that this labor income increase is particularly large among young firms.

Finally, our paper 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; Denes et al., 2022; Slattery et al., 2022). 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). While there is no consensus on the effect of *Citizens United* on equity returns, most papers find that abnormal returns around *Citizens United* were negative for firms that had made large political contributions before *Citizens United*. Finally, a few studies have examined how the likelihood of specific policies being adopted by states has changed as a result of *Citizens United* (e.g., Werner and Coleman, 2015; Niczyporuk, 2020;

[9] Many possible explanations for the decline in the labor share include increase in corporate profits (e.g., Gutiérrez and Philippon 2017; Hall 2018; Barkai 2020; De Loecker et al. 2020), capital-augmenting technological change (e.g., Zeira 1998; Acemoglu 2003; Brynjolfsson and McAfee 2014; Aghion et al. 2019), a decrease in the price of capital (e.g., Jones et al. 2003; Karabarbounis and Neiman 2014), capital accumulation (e.g., Piketty and Zucman 2014), globalization (e.g., Elsby et al. 2013), weakening bargaining power of labor (e.g., Blanchard and Giavazzi 2003; Stansbury and Summers 2020), increase in business owner's income (Smith et al. 2019), and increase in intangible capital (e.g., Crouzet and Eberly 2019).

Slattery et al., 2022). Our paper contributes to this literature by highlighting that states that were directly affected by *Citizens United* experienced an increase in political competition, particularly at the executive level where seats were more likely to flip from Democrat to Republican and vice versa, as well as a decrease in polarization among elected legislators.[10] Moreover, to the best of our knowledge, we are the first to examine how the economic outcomes of labor and capital providers were affected by the increase in political spending ushered in by *Citizens United*.

2 Institutional Background, Data, and Empirical Strategy

2.1 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 a communication (e.g., political advertisement) that expressly advocates for the election or defeat of a clearly identified candidate and that is not made in coordination with any candidate or her authorized agents. 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 the court ruling.[11] 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.

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

[10] More generally our results also compliment the literature that has found inconclusive evidence of the effect of restrictions on campaign spending on electoral competition (e.g., Gross et al., 2002; Lott, 2006; Primo et al., 2006; Stratmann and Aparicio-Castillo, 2006; Hall, 2016; Butcher and Milyo, 2020).

[11] 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.

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.[12] 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.

In a 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, it ruled that laws requiring "social welfare" non-profit funders, like Citizens United, to disclose their donors were unconstitutional.[13] 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 enactment of individual state-level bans was not caused or affected by the BCRA rules themselves. Figure 1, Panel A shows which states were affected.

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 individuals, corporations, or unions 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).[14] The number of Super PACs grew quickly following *Citizens United*. As Figure 1, Panel B shows, in the next election cycle following *Citizens United*—2012—conservative-aligned Super PACs spent nearly \$500 million and liberal-aligned Super PACs speant nearly \$250 million. This number has dramatically increased since then.

While *Citizens United* impacted both corporations and unions, union political contributions have been a relatively small share of total political contributions at least since 2004, where they comprised roughly 10% of total outside political spending. (See Figure 1, Panel C.) This share has only decreased since *Citizens United*, and following the decision, outside 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.

[12]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).

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

[14]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*.

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 citizens, firms, unions, and non-profits to spend money in politics with substantially less disclosure, which, as we will show later, led to an increase in election-related advertising and donations.

2.2 Data

We combine data from a variety of sources for our analysis. Our sample spans 2004–2018 where possible.[15] 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 the economic variables on an annual basis. Table 1, Panel A provides summary statistics on the variables described below.

2.2.1 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.[16] Panel A of Figure 1 presents a map that shows 23 states that had those bans declared unconstitutional.[17] The states that had a ban on independent political expenditures and were therefore affected by the *Citizens United* decision are treated states, 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 56% and that Republicans control the state Senate and House legislative chamber is 52% and 51%, respectively (see Table 1, Panel A).

Polarization: We use political polarization measures of state's legislative chambers estimated

[15] 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.

[16] 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.

[17] 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.

by Shor and McCarty (2011).[18] 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, the more bipartisan the ideology of its members. Positive values reflect conservative ideology (Republican tilt), while negative ones reflect liberal ideology (Democratic tilt). As shown in Table 1, Panel A, the average state House and Senate in the period has a slightly Republican tilt, but not significantly so.

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 organization 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.[19]

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 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 index.

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),[20] 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. Note that not all states contain a media market, so advertising data are missing for some states.

2.2.2 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

[18] 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).

[19] `https://www.followthemoney.org/`

[20] 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).

level, data on state gross domestic product (GDP), which are further disaggregated into employee compensation and gross operating surplus.[21] We take employee compensation as our measure of labor income and gross operating surplus as our measure of capital income. The chief advantage of the BEA data for our purposes is that income is apportioned according to where the underlying economic activity takes place. As shown in Table 1, our measured labor share is 54% averaged across states.

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, interest income, dividend income (ordinary and qualified), 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, capital income, labor income, and labor share, as follows: we proxy GDP with AGI; we proxy labor income with salary and wage income; we proxy capital income as AGI less salary and wage income; we proxy labor share with salary and wage income divided by AGI.

There are a few drawbacks of the IRS income relative to other measures that particularly impact the measure of capital 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 by a factor of production. Consistent with these issues, average AGI from the IRS is lower than GDP from the BEA, and this is primarily driven by differences in capital income.

Quarterly Workforce Indicators (QWI): As additional robustness, we further 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-quarter level. Additionally, the QWI shows heterogeneity by firm and employee characteristics, such as employer size and age. Thus, the QWI provides year-state-firm heterogeneity panel data reporting employment and payments to employees that supplement our main BEA dataset.

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,

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

leverage, cash, and Tobin's q. We complement these data with historical headquarters data from the Loughran/MacDonald database.[22]

2.2.3 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.

Minimum wage data: In Gopalan et al. (2021), the authors hand-collect data on each state's minimum wage in a given year. The authors shared these data with us. The average state minimum wage in our sample period was \$6.70, and the average annual growth in the minimum wage was 2.8%

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 uses state 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 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 mean 0 and standard deviation of 0.13, 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.

2.3 Empirical Strategy

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

[22] https://sraf.nd.edu/

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

where s indexes state, and t indexes time; $Outcome_{st}$ represents an economic or political outcome for state s in time period t. $Post_t$ is an indicator variable that takes the value of one for periods following the *Citizens United* case (2011 and after), and is zero otherwise. $Treated_s$ 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. γ_{tp} is a year-by-party fixed effect that allows states that had governors of different political parties in the election cycle prior to *Citizens United* to follow different time trends, which also absorbs standard time fixed effects.[23] γ_s is a state fixed effect. Our difference-in-difference sample runs from 2007 through 2015, data permitting. We cluster standard errors by state in all of our analyses.

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

$$Outcome_{st} = \sum_{\tau=2004}^{2018} \beta_\tau (I_{t,\tau} \times Treated_s) + \gamma_{tp} + \gamma_s + \epsilon_{st}. \tag{2}$$

In this estimation, β_τ measures changes in the outcome variable in the treated states as compared to the control states year by year, where $\tau > 2010$ corresponds to the individual annual treatment effects. The omitted time period is 2010, 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, 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 states with bans of independent expenditures (affected by the court case) had similar a share of voters for Obama in the 2008 presidential election. However, these bans may predominately have been found

[23]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.

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 by 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 average share of the 2008 presidential election that was won by Barack Obama was 49.0% in treated states and 51.8% in control states. 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, 2010 economic outcomes—such as state GDP, labor income, capital income, and labor share—do not very significantly across treated and control states.

Finally, for additional robustness in Appendix A.1, we implement a propensity score matching estimator. The matching procedure fully removes the ex-ante differences in covariates, and we find essentially identical results in our main specifications. Similarly, we show that our results are essentially unchanged when we implement a synthetic control estimation in Figure 5 to explicitly address potential concerns about pre-trends.

3 State-Level Political Consequences of *Citizens United*

We first show that *Citizens United* was an important shock to both the campaign finance landscape and to the outcomes of state-level elections.[24] Our first goal of these analyses is to show that *Citizens United* resulted in increased political spending. Our second goal is to understand whether the removal of campaign finance restrictions caused by *Citizens United* primarily entrenched incumbent political interests and politicians or if the event served as a catalyst for a broader set of interests to begin spending money in politics and help to elect new politicians, increasing political competition.

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.[25] However,

[24] 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). To our knowledge there is less work on the economic effects of *Citizens United* and none that examines our main research question of how this event effected economic outcomes for labor and capital.

[25] A prominent example can be found in Barack Obama's 2010 State of the Union address, in which he ex-

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 a typical Fortune 500 company (which could already afford to spend millions of dollars on lobbying), but rather increase political participation by small and medium-sized firms.[26]

The analysis in this section allows us to examine whether *Citizens United* primarily entrenched incumbent political interests or prompted new interests to spend money in politics and which caused political competition to increase.

3.1 Political Spending

We begin by showing that *Citizens United* led to an increase in political spending. We first plot spending by federal election Super PACs by election cycles in panel (b) of Figure 1.[27] The amount of Super PAC spending was zero in the 2008 election cycle, and grew to a small number in 2010. The number was initially small because *Citizens United* was decided midway through the 2010 election cycle. The case's transformative effect took place starting after 2010, with the amount of Super PAC spending reaching over $600 million in the 2011–2012 election cycle, and over $2 billion in the 2019–2020 election cycle. This timing motivates us to expect changes in political and economic outcomes beginning in 2011, at which point money would begin to influence incumbent politicians' decisions, subsequent elections, and the current and future business environment. Panel (c) shows that total outside spending (which includes Super PACs and other forms of spending made independently of candidates) rarely comes from pro-labor groups: before *Citizens United*, labor-aligned spending comprised roughly 12% of outside spending; this number fell to roughly 4% following the decision.

In addition to prompting the rise of Super PACs, *Citizens United* explicitly allowed non-profit advocacy groups to raise and spend unlimited amounts of money on political advertisements (as we described above) by invalidating the state laws prohibiting such advertisement for state-level elections in 23 states. We therefore examine how the log of total state-level political advertising changed in those states compared to states that did not have such a ban in place using our differences-in-differences framework and data from Ad$pender.[28] Table 2, Panel A shows that political advertising increased by 30% in states treated by *Citizens United* compared to the control

plicitly spoke against the the Court's decision. See https://obamawhitehouse.archives.gov/the-press-office/remarks-president-state-union-address.

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

[27]For much of this political outcome analysis, we group years into two-year election cycles that correspond to the natural pace of election spending. Reliable data on state Super PACs does not exist, so we rely on federal data to illustrate the point that Super PAC spending increased substantially after *Citizens United*.

[28]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 lower bound on the increase in independent expenditures driven by the Supreme Court decision.

states following the decision. Appendix Figure A1 verifies this finding dynamically, showing flat pre-trends up through 2010 and then a spike in ad spending following *Citizens United*.

Our results on Super PAC spending and political advertising show that money in politics has increased after *Citizens United*. Unfortunately, the individuals or corporations who funded this increased spending is largely unknown since *Citizens United* allowed for new forms of anonymous political spending (i.e., so-called "dark money"). For that reason, we next 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 described above, the court ruling did not directly affect state laws related to direct political contributions; they changed laws relating to independent political spending (i.e., political advertising by groups that do not directly contribute to 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 indeed find and describe below), we believe that this analysis sheds some light on which groups may drive the increase of money in politics, however imperfect this test is.

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, 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 2, Panel B presents the results of this analysis. We present the difference-in-differences coefficient from Equation (1) for the full sample (top line labeled "All sectors") and from each sector subsample. We generally find that direct political contributions increased in states affected by *Citizens United* after the ruling compared to controls states, although statistical significance varies by sector. Specifically, we find that that aggregate contributions for all sectors increased by 27%, which is statistically significant at the 10% level. 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 14 of the 15 sectors in states affected by the decision have positive point estimates ranging from 0.17–0.96, with four of these specifications being statistically significant at the 5% or 10% level. 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). The particular sectors that have the largest point estimates are Unitemized Contributions (0.96), Labor (0.55), Lawyers and Lobbyists (0.53), and General Business (0.50) and account for 36% of total contributions. The fact that business groups, labor groups, lobbyists and, in particular, likely first-time or infrequent contributors (proxied by small donations included in the "Unitemized Contributions" category) all increased their political activity 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 included new donors.

3.2 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, it is possible that the expansion of political spending that *Citizens United* caused primarily benefited incumbent politicians and traditionally politically important constituents, which might serve to entrench politicians and reduce 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 influence, 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*.[29]

We begin by examining the effect of *Citizens United* on gubernatorial elections. Figure 2 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) changed in treated states relative to control states after the ruling.[30] As shown in Panel (a) of the figure, the probability that the governorship changed political parties was 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 22 percentage points higher for treated states (as tabulated in Column (1) of Panel A of Table 3), which is roughly 100% of the sample mean. Significantly, the figure shows no pre-trends in political turnover, which supports the identification assumption.

Panel B of Figure 2 splits the political party turnover results by the political party that was in power in 2010 (elected prior to the court ruling). This test provides a systematic way to examine 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

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

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

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 Table 3, Panel A, which shows an increase of 18.7 percentage points in the likelihood of the governor's seat transitioning from Republican to Democrat and an increase of 24.8 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 in Column (3) and marginally statistically significant in Column (4) at the 10% level. 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 4.5% 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 in the form of turnover 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*.[31] Panel B of Table 3 presents the results of this analysis. 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.

We begin by examining turnover in the state Houses of Representatives in Panel B, Columns (1)–(4).[32] 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 2.8 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 10% of the baseline rate. Column (2) shows that the proportion of Republicans is a statistically significant 5.1 percentage points higher. Columns (3) and (4) show that the proportion of new Republicans and new Democrats is 2.7 and 0.1 percentage points higher, though these estimates are not statistically significant at conventional levels. 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 subsequent 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 is defined more broadly. Indeed, we find in this section that political activity across both liberal and conservative groups broadly increased, and that governorships were more likely to turn

[31] 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.

[32] 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.

over both from Democrat to Republican *and* from Republican to Democrat. This is perhaps not surprising given that both conservative- and liberal-aligned Super PACs saw a large increase in spending as shown in Figure 1, Panel (b). Finally, to the extent that one might worry about ex-ante political conditions in states driving our main electoral and economic results, we allow states that had a Democratic governor in power in 2010 to follow different trends around *Citizens United* to alleviate such concerns.

Collectively, the results on turnover in state-level politics suggest that states affected by *Citizens United* had higher turnover of politicians at various levels of government, but that this increased turnover did not uniformly benefit the Republican party. Indeed, we find evidence that governorships were more likely to transition both from Democrat to Republican and vice versa. Additionally, turnover appears to have increased among individual legislators for both Republicans and Democrats. These results provide evidence that increased money in politics likely resulted in higher political competitiveness.

4 State-Level Economic Consequences of *Citizens United*

We now turn to our main question of interest: How does increased political spending affect economic outcomes for capital and labor? The net economic effect on various factors of production is ambiguous. Section 3.2 shows that the increased money in politics spurs political competition, which could lead state governments to adopt policies that improve overall economic conditions and lead to greater economic growth. The benefits of this increased economic growth can then flow to both labor and capital.[33] However, increased political influence of capital could also lead to transfers or capture of these benefits by capital providers at the expense of labor: Panel (c) of Figure 1 shows that the share of outside political expenditures advocating for pro-labor causes dropped dramatically from 12% to 4% after *Citizens United*, suggesting that labor might not benefit from the improved economic conditions. To answer this question, in this section, we examine the impact of *Citizens United* on economic growth and factor incomes.

4.1 Baseline Results

We examine how economic outcomes for labor and capital change after *Citizens United* using data from the BEA and the IRS. The BEA data measure state-level aggregate income as well as payments to labor and capital though various aggregations and imputations. The IRS data do so though aggregating individual-level tax returns, which report both income attributable to labor and income attributable to capital ownership. There is measurement error in both datasets, particularly around state-level capital income, and therefore they complement each other and serve as natural robustness checks. We measure total output as GDP from the BEA and as adjusted gross

[33]Later, we consider whether *Citizens United*, in also declaring restrictions on unions' ability to engage in political advertising unconstitutional, made unions better able to advocate for favorable labor policies. We examine and rule out this channel in subsequent sections.

income (AGI) from the IRS. We measure labor income as total compensation from the BEA and salary and wage income from the IRS. We measure capital income as operating surplus from the BEA and as AGI minus salary and wage income from the IRS. Note that measuring the payments to capital is more complicated using IRS data than using the BEA data because there can be substantial differences in what is earned in a time period and what is taxable in the same time period. Therefore, in the IRS data, our preferred way to measure payments to capital providers is to assume that all income that is not paid out to labor providers is effectively payment to capital providers. Finally, labor share is measured as the respective labor income measure divided by the respective total income measure. All subsequent analyses put the BEA and IRS measures side-by-side and lead to very consistent findings.

The difference-in-difference results are shown in Table 4, Panels A (BEA) and B (IRS). The event studies are shown in Figure 3, with Panels (a), (c), (e), and (g) showing BEA outcomes and (b), (d), (f) and (h) showing IRS outcomes. Beginning with the difference-in-difference results, Column (1) in Panels A and B of Table 4 show that in treated states following the court ruling, total output increases by three to four percentage points with the BEA and IRS measures, respectively. Column (2) in both panels shows that capital income—noisily measured—increases between 2 and 3.5 percentage points. Column (3) in both panels shows that labor income increases by an economically and statistically significant 3.7 to 4 percentage points in treated states following the court ruling. Consistent with growth in both capital and labor income, the labor share does not change significantly. While it is difficult to assess the expected economic magnitude that *Citizens United* might have had on payments to labor or capital, 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.[34]

Examining the event studies, Figure 3, Panels (e) and (f) show a clear increase in labor income on impact, persisting through the entire sample period. The finding is particularly stark with the IRS data. We find similar patterns in overall output, Panels (a) and (b), though magnitudes are lower and standard errors are higher. Panels (c) and (d) show how capital income changed in treatment and control states. Looking at the period-by-period estimates, we find very weak evidence that capital income increased following the treatment year, although the standard errors of the estimation are large. Finally, in Panels (g) and (h) we find little evidence that labor share changed: there are no obvious patterns in the event study and the coefficient in Column (4) of Panels A and B of Table 4 are close to zero. Collectively, these results suggest that payments to labor increase when political spending is less regulated. Although we cannot conclude that payments to capital increased with precision, we find no evidence that the increase in labor income comes at the expense of income to capital providers.

[34]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.

4.2 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 few of the variables 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 Section A.1 details the approach. Table A3, 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 4 replicates Figure 3 with the propensity matching approach and delivers very similar results, with, if anything, stronger effects and less observable pre-trends. For completeness, we show regression evidence using propensity matching approach for the BEA and IRS economic outcomes in Panels B and C (respectively) of Table A3, which produce estimates that are nearly identical to the baseline specification.

Second, we implement a synthetic controls approach following Xu (2017).[35] 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 5, fully eliminate pre-trends in the economic variables and find essentially identical economic effects post-treatment, suggesting that our results warrant a causal interpretation. Additionally, we perform 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 100 random permutations. Figure A2 shows the actual result and median permuted result and, comfortingly, shows no effect in the permuted result.

Additionally, we redo our main difference-in-difference analysis using the US Census' QWI database, which not only has data on overall payments to labor (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. We show the results of the difference-in-difference regressions in Table A1. Table A1 shows effects that are largely consistent with our previous results on labor outcome: aggregate payroll increases by 4.6 percentage points (Column (4)). Column (1) shows that log employment increases by roughly 2.2 percentage points in treated states following *Citizens United*. Average earnings increase by 2.5 percentage points for all workers (Column (2)) and by nearly 5 percentage points among newly hired workers (Column (3)), suggesting that some of these earnings increases are driven by new hires on the extensive margin. Beyond serving as a robustness check, the employment and earnings results from the QWI data provide additional evidence consistent with the equilibrium economic mechanism: by leading to broadly pro-growth policies, *Citizens United* increases firms' demand for labor. As labor demand shifts outward along an upward-sloping labor supply curve, prices (earnings) and quantities (employment) both increase, with the relative increases in prices and quantities being driven by the labor supply elasticity.

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

Finally, given the observation in Table 1, Panel B, that treated and control states were differentially exposed to house price changes around the Financial Crisis, we bin states into quartiles of pre-crisis (2002–2006) house price changes and include time $\times$ house price change quartile $\times$ 2010 state governor fixed effects to absorb differential time trends across these states. These results, shown in Table A2, are, if anything, somewhat stronger and more precise than our main specification, suggesting that differential exposure to the Financial Crisis is not driving our results. Additionally, industry-level analysis shown in Table 5, Panel A shows that the results are not driven by crisis-related industries, such as real estate or finance, and, instead, are broad-based (e.g. include mining, manufacturing, wholesale trade, and many others). In summary, our results show increases in economic growth, and particularly labor income, that are robust to different data sources, measurement, and specifications.

5 Potential Mechanisms

Our results so far show that *Citizens United* had both political and economic consequences. First, political advertising and direct campaign contributions increase in the treated states. These increases come both from traditionally well-connected groups, such as lobbyists and business interests, but also from labor and traditionally unconnected groups such as small (and potentially first-time) donors. Second, political turnover, particularly among governors, increases in affected states, suggesting that the higher incidence of money in politics can increase political competition. Third, our economic results suggest that overall growth increases in states affected by the court ruling and, in particular, payments to labor increase.

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. Lower barriers to political activism and ensuing wider-spread political participation increase political competition, leading to the adoption of policies benefiting a broader set of constituents and pro-growth policies, rather than rent-seeking policies that benefit a narrower set of 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.

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 to 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 macroeconomic fiscal multiplier. We offer evidence against these alternatives.

5.1 Political Competition and Pro-Growth Economic Policies

Our primary explanation for our main results is that *Citizens United* had the effect of encouraging political spending from a broader set of constituents, which increased political competition and led to a more favorable economic or regulatory environment. We provide evidence in three broad categories of outcomes to support this channel: changes in the composition of legislators, evidence that ex-ante politically *inactive* firms and industries responded as much or more than ex-ante politically *active* firms and industries, and direct evidence that states adopted more favorable economic policies around regulatory enforcement and taxation. Broadly, policy changes appear to reduce administrative and regulatory overhead costs, which leads to increased firm labor demand, output, employment, and wages.

5.1.1 Political Polarization

We begin by examining whether different types of legislators are elected after *Citizens United*. In Section 3.2 we found that the turnover of incumbent politicians was higher at various levels of state governments in both political parties. However, these results do not speak to differences in the actual legislative preferences of the newly elected politicians. If the new politicians are more polarized, they might attempt to enact policies that are more extreme, such as focusing on passing wedge social issues that appeal to the ideological fringe of their parties. Alternatively, if the newly elected politicians are more centrist, policymaking could be more focused on issues that are less partisan and targeted to improve the conditions of a broader set of constituents.

We measure polarization of a state legislative chamber using data provided by Shor and McCarty (2011), which we describe in more detail in Section 2. We use the numerical distance in ideology score between the mean Democrat and Republican in each legislature-year as our measure of polarization, which is the preferred measurement of polarization by those authors. 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 and Table A4 examine how state-level political polarization changes after *Citizens United*. In Figure 6, 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. Numerically, Table A4 confirms the drop in distance of approximately 0.04 units. This drop is economically significant, corresponding to 8.2% of a standard deviation. We find less evidence that polarization changed in the state Senates, which is unsurprising given our earlier finding that state Senate elections were 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 interests. In the following subsection, we look directly at heterogeneity in economic outcomes to examine whether economic growth is similarly broad-based or wither it is concentrated in politically connected firms and industries.

5.1.2 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 responded. If, instead, *Citizens United* primarily provided incumbent, already politically connected agents more tools to influence policy outcomes, we would expect that firms and industries that were the most politically active prior to the decision to respond the most.

We begin by examining how labor-related outcomes responded to *Citizens United* across different 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 26 are statistically significant at the 10% level. Collectively, the industries that have a statistically significant coefficient for at least one of the outcome variables account for 60.65% of total employment in the QWI database. These broad-based economic effects are therefore consistent with a vastly expanded set of constituents participating in political activity and benefiting from it following *Citizens United*.[36]

We next examine whether employment, earnings, or payroll respond 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,[37] and we test whether the labor outcomes' response to *Citizens United* was 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

[36] 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.

[37] 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 fewest.

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 other 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 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 young (5 years old or less) and old firms in terms of log employment, there are much larger differences in terms of worker earnings and total payrolls. Figure 7, Panel (d) shows that workers at younger firms saw their earnings grow significantly more 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 and new hires) increasing by roughly 3.5% more in young firms relative to old firms, and payrolls increasing 4.6% more in young firms relative to old firms. These results suggest that *Citizens United* increased firm labor demand at *all* firms,[38], but particularly more so for young firms that were less likely to be politically connected ex-ante.[39] Thus, our findings underline our primary hypothesis: 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 payments to capital and labor. However, such an aggregate analysis does 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 responses to *Citizens United*.

To complement our aggregate-level analysis, we focus on US public firms from Compustat.[40] We focus on employment because, while employment data are well populated for the sample of Compustat firms, 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

[38] As discussed in Section 4, as firm labor demand increases, prices (wages) and quantities (employment) increase in a manner dictated by the labor supply elasticity across each sector.

[39] 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 that might caused by *Citizens United*.

[40] 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.

federal lobbyist from 2000–2009; and whether a firm had above-median total assets in 2010 as a proxy for size.[41] We classify firms that have made such political donations, have hired federal lobbyists, or are large as ex-ante connected.[42] While none of these proxies are a perfect measure of political incumbency for individual firms, they serve as a useful benchmark.

We estimate firm-level regressions of Equations (1) and (2) and present results in Figure 8 and Table 7.[43] Panel A of Figure 8 presents the event study for the full sample. We find an increase in (log) employment after *Citizens United* for firms headquartered in treatment states compared to firms headquartered in control states. However, as shown in Column (1) of Table 7, the pooled effect is only statistically significant at the 10% level, although the magnitude is strikingly similar to the (statistically significant) effect that we found on log employment using QWI data (0.029 here versus 0.026 in the QWI data).

Turning to the triple-difference estimations, we find little evidence that firms that were likely to have been political incumbents prior to *Citizens United* were the primary drivers of our pooled results. Panels (b) and (c) of Figure 8 present the patterns for groups of firms that had previously made political contributions to federal or state politicians, respectively. We find little evidence that previously politically active firms drive the results, and if anything, Panel (c) suggests that previously politically inactive firms responded most. Similarly, in Panel (d), we find little evidence that firms that lobbied prior to *Citizens United* responded more sharply. Panel (e) of Figure 8 suggests that smaller (public) firms respond more than larger firms. Indeed, as confirmed in Column (5) of Table 7, smaller firms' employment in treatment states increases by 9.8% relative to those in control states, which was largely offset in large firms.[44]

Collectively, the results in this section suggest that firms that were "political incumbents" were not the driving force behind the increased labor income and employment in response to *Citizens United* that we have documented earlier. 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 industries. If anything, smaller and younger firms, and firms with less 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.

[41]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 as a starting place for this analysis since larger firms are more likely to be politically active (e.g., Cooper et al., 2010).

[42]As one would expect, these measures are positively correlated although not perfectly so. The correlations range from 0.36–0.53.

[43]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.

[44]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 had 1,400 employees, so these cross-sectional results are not directly comparable to the firm size results using the QWI data.

5.1.3 Changes in Policies

Last, we consider whether business conditions became more favorable for firms. 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 the rapid 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 treated states becoming more growth-friendly. Specifically, we examine whether treated states experienced changes in the regulatory enforcement of existing laws, fewer occupational licensing regulations, an increase in "regulatory freedom" as measured by the Cato Institute, or a reduced state-level tax burden.

Regulatory enforcement: We begin by examining regulatory enforcement. As we show in Section 3.2, 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 use data from the Violation Tracker database from *Good Jobs First*, a non-profit advocacy group that compiles a number of databases related to corporate and government activities. The database aggregates enforcement actions from both federal and state enforcement agencies on violations related primarily to banking, investor protection, consumer protection, environmental, wage and labor, unfair labor practice, health and safety, and workplace discrimination.

We examine whether the number of state-level and federal enforcement actions change after *Citizens United*. If government regulation of economic activity became more business-friendly, we would expect that the number of state-level enforcement actions decreased, particularly those actions related to 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 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 that they are subject to. More specifically, there are many areas of regulation in which federal and state jurisdictions overlap. 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*. Because enforcement actions are often rare, our outcome variable is log of 1 plus the number of enforcement actions.

Panels (a) and (b) of Figure 9 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).[45] We find that enforcement actions pertaining

[45] 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

to laws protecting labor and consumers fell significantly in treated states following *Citizens United*. In contrast to state-level results, Figure 9, Panel (b), which examines enforcement actions at the federal level, shows that federal enforcement activity did not exhibit any change before or 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 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 consumers decreased by roughly 50% in treated states following *Citizens United* (Column (2)), while state-level and federal-level enforcement actions related to capital protection did not change (Columns (3)–(6)).

The affirmative results for state-level enforcement actions but null-results for federal-level enforcement actions suggest 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 effect in treated states after *Citizens United*.

Collectively, these results suggest that the state-level regulatory environment became 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—costs the firm pays to hire labor but that do not ultimately go to workers. 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 an increased labor demand, leading ultimately to more output, greater employment, and higher wages—exactly what we find in our economic outcomes. Moreover, workers in those states were not worse off along non-financial dimensions.

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 individual 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).[46]

Using data from Sorens et al. (2008), we examine whether state-level licensure requirements

off-label or unapproved promotion of medical products.

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

decline in states affected by *Citizens United* in Table 8, Panel B.[47] In Column (1), we observe reduced regulatory requirements in the treated states as shown by a decrease in employment-weighted licensure of 0.014, which is 10% of the standard deviation. This result is borderline statistically significant at the 10%-level for our standard period of 2007–2015, with a t-value of -1.66 and p-value of 0.104. We also re-estimate this effect on an extended period of 2007–2017, because changes in occupational licensing often must pass through the state legislative branch and changes in legislation might take a long time. Over the longer time horizon, the result is significant at the 10% level (Column (2)). These results suggest that some of the gains for labor in treated states may be coming from easier access to the labor market for 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*. Higher values of the index correspond to a lower regulatory burden in each state.

We examine whether this state freedom index increased in treated states after *Citizens United*. The results of this analysis are presented in Table 8, Panel B, Columns (3) and (4). The overall regulatory environment becomes lighter in treated states, consistent with our previous results. Specifically, regulatory freedom in treated states increases by 0.006, or by 5% of the standard deviation, a result statistically significant at the 10% level. If we extend the observation period to 2017 to account for the slow-moving legislative process, the estimate becomes significant at the 5% level.

Collectively, the results on enforcements, licensing requirements and state freedom index suggest that the regulatory burden decreases in states affected by *Citizens United*. These results support our hypothesis that *Citizens United* resulted in increased political competition, leading to more growth-friendly policies being implemented and to an increase in state-level income for both labor and capital.

Tax changes: Finally, we examine whether state-level tax rates changed in states affected by *Citizens United* relative to those that were not affected. 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 changed differentially in treated and control 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 are not statistically different from zero despite having relatively large economic magnitudes. For example, the point estimates on the corporate

[47]The authors consider an 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 license is required by a contractor but not her employees, the authors record that as a "half license", using value 0.5.

and personal income tax rates are −0.538 and −0.306, 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, which is consistent with our main hypothesis that increased political competition promoted policies that benefited both labor and capital.

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 a mechanism could explain the increase in wages that we observe, it is less likely that this could simultaneously explain increased employment and higher payments to capital. 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 1, Panel (c) suggest that labor's share of political financing, 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, for completeness, 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 unions in addition to banning political advertising by 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 standard empirical specification, in Appendix Table A5, 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 payments to labor in treated states after *Citizens United*, is unlikely to be attributable to 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.[48] 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 changed differentially in states affected by *Citizens United*.

Taken together, our results in this section suggest that our main finding that payments to labor increased when money in politics became less regulated are unlikely to be attributed to changes in policy that would directly effect transfers to labor such as increased union power or minimum wage increases.

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 payments to 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, employment increased employment.

5.3.1 State Fiscal Spending

We begin by studying whether states that were effected 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

[48] Minimum wage data come from Gopalan et al. (2021). We thank the authors for sharing their data.

expenditures and various subcategories. The dependent variable is the log of one plus the expenditure or revenue amount. Columns (1) and (2) present regression estimates and t-statistics for each regression, respectively. The column labeled "pct" provides the percent of total revenue or expenditure for each subcategory in order 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 utility revenues increased, although the category only accounts for about 0.77% of state revenues. On the expenditure side, we find that capital outlays increased by 15% (row 21), which is statistically significant at the conventional levels. While this type of government spending could plausibly have a stimulative effect since it involves direct expenditures for construction, it only accounts for about 6% of government expenditures.[49] We also find that salaries and wages related to highways also increased (row 30), although this category only accounts for 5.8% of state salaries and wages.

In sum, the increases that we find in government spending are too small and too concentrated in particular spending categories to explain the large increase in labor income that we find in our main results without assuming that the fiscal multiplier is implausibly large.[50]

5.3.2 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*, the same non-profit that provides the data on violations that we use in Section 5.1.3. 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. We examine whether the log of one plus the number or dollar-value of state subsidies changed differentially in treated states after *Citizens United*. Table 10, Panel B shows the results. 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 suggesting that *Citizens United* led to an increase in either the number or amount of subsidies—a finding confirmed in recent work by Slattery et al. (2022). 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 9.6% of the standard deviation of the number of subsidies and 5.5% of a standard deviation of the value

[49]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.

[50]See Ramey (2019) for an excellent summary of research on the magnitude of fiscal multiplier in the United States.

of subsidies. As with the results of our government spending tests, one would need to assume that any potential increase in subsidies are 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 increased by roughly 3% in affected states. Labor income increased between 3–4% 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 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 a 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. Taken together, these results suggest that historically politically powerful constituencies did not drive the increased economic growth.

We examine whether increased union labor power or greater government spending could explain our results and find little evidence of these alternative possibilities. We find that payments to labor increased similarly in states that had both a ban on union independent expenditures (as well as corporate independent expenditures) and those states that only had a ban on corporate independent expenditures. Moreover, we find no evidence that labor-friendly policies such as minimum wages changed differentially for treated and control states. Finally, we directly examine whether government spending changed following *Citizens United*, and while we find a small increase in capital outlay spending, that is too small to explain our main results without assuming an implausibly large fiscal multiplier.

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 a 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 that 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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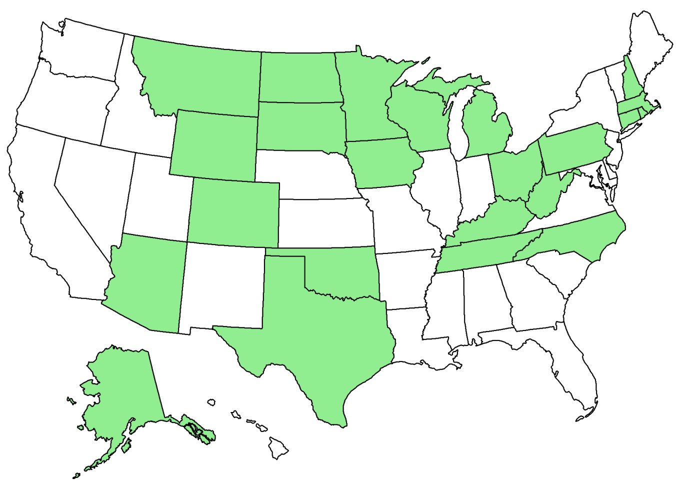
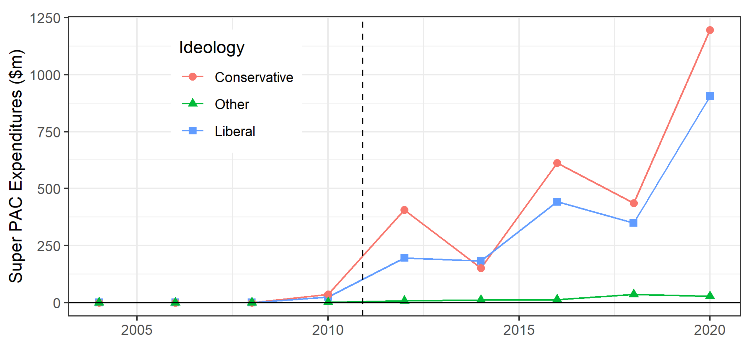
Figure 1: CITIZENS UNITED AND POLITICAL EXPENDITURES

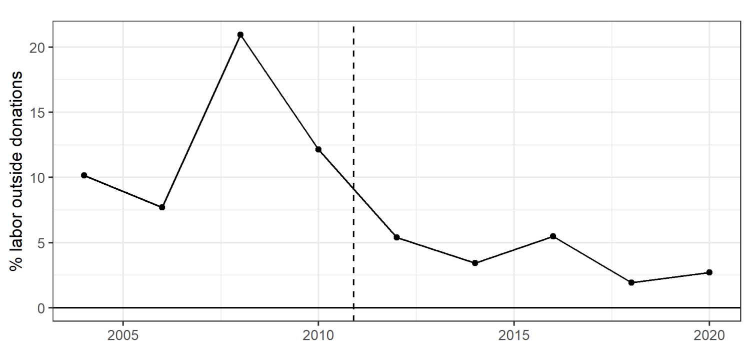
Note: In this figure, Panel (a) shows in green which states banned corporate (including non-profits) or union independent political expenditures before *Citizens United*—the bans that the Supreme Court invalidated. Panel (b) shows total Super PAC spending in federal elections, in millions of dollars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



(a) States with bans pre-*Citizens United*



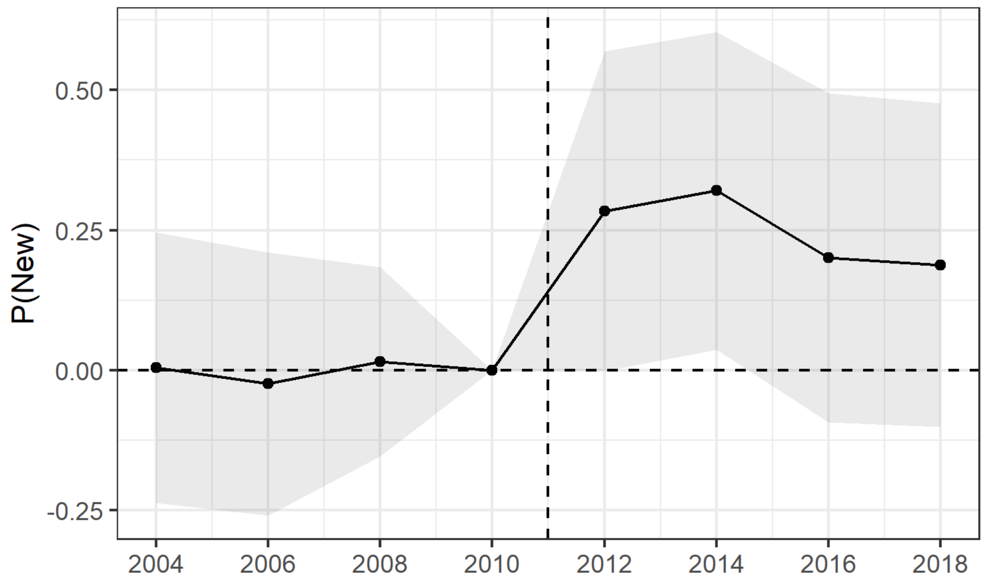
(b) Super PAC Spending



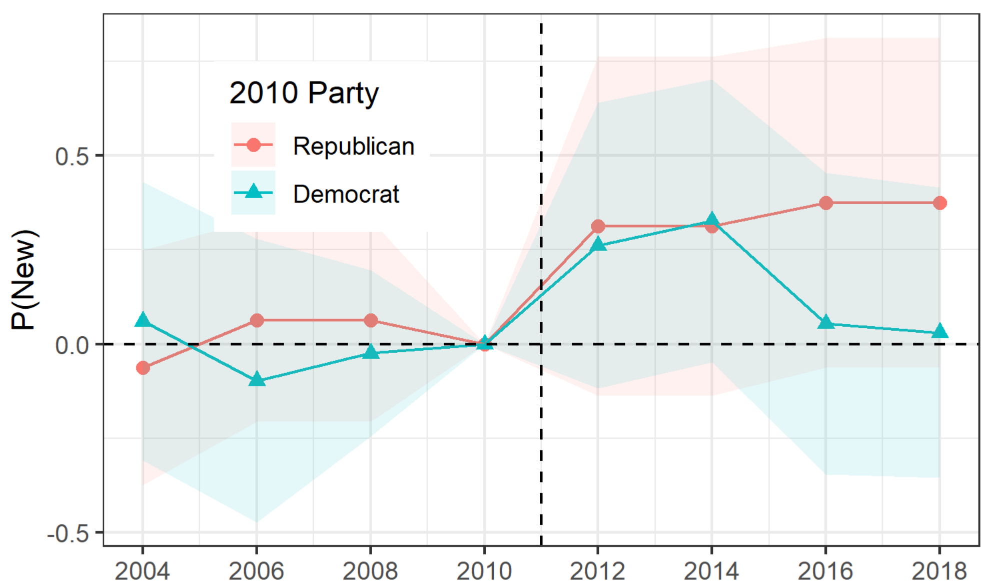
(c) Labor share of outside political spending

Figure 2: 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 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 2010 governor party fixed effects. Standard errors are clustered at the state level.



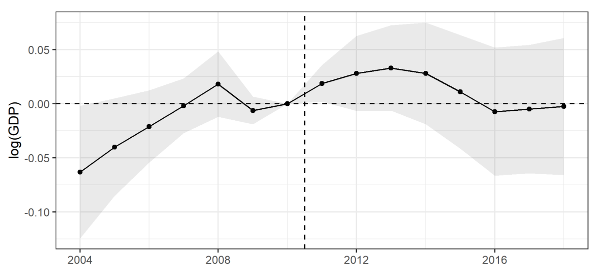
(a) All governors



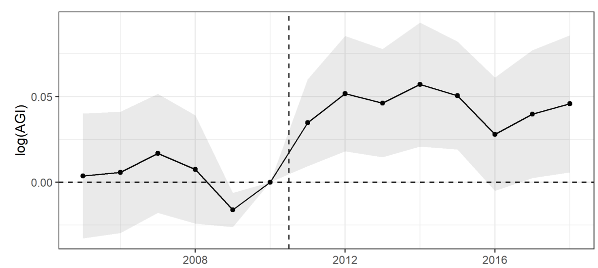
(b) Governor by party before *Citizens United*

Figure 3: Total and Factor Incomes

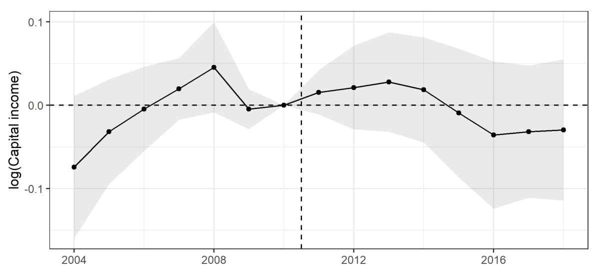
Note: This figure shows changes in state-level economic outcomes around *Citizens United*. States affected by the *Citizens United* case (treated states) are those that had bans on corporate or union independent political expenditures pre-*Citizens United*—the bans that the Supreme Court invalidated.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



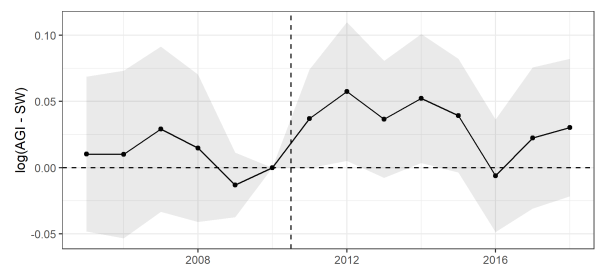
(a) BEA: GDP



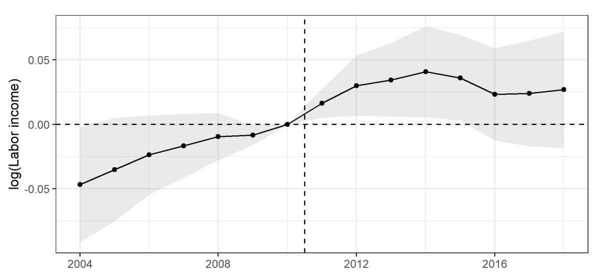
(b) IRS: AGI



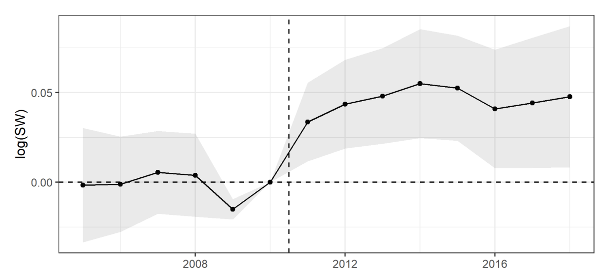
(c) BEA: Capital income



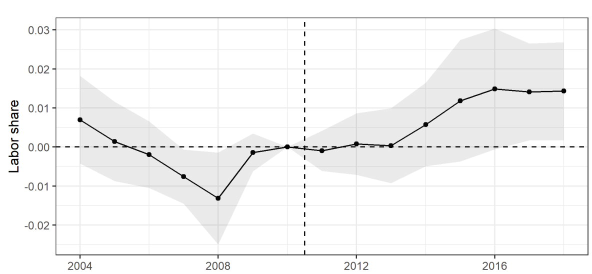
(d) IRS: Non-SW income



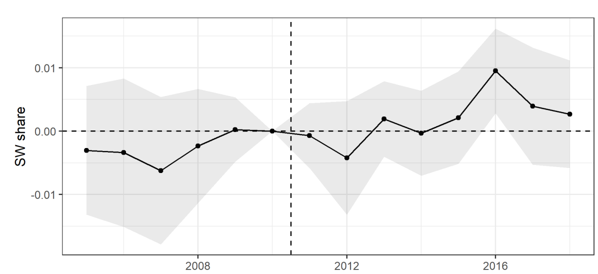
(e) BEA: Labor income



(f) IRS: SW income



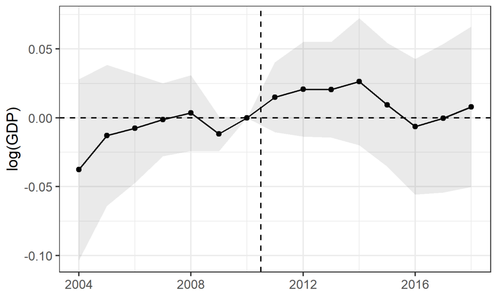
(g) BEA: Labor share



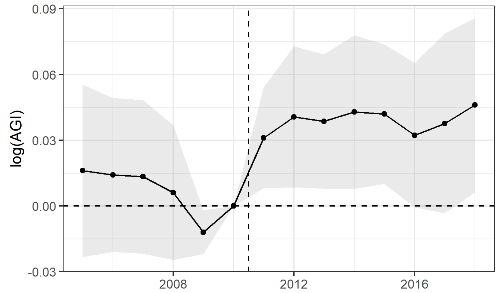
(h) IRS: SW share

Figure 4: Total and Factor Income results with propensity score matching

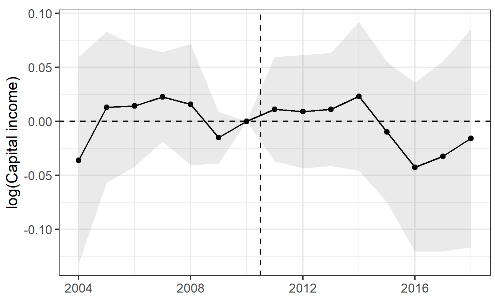
Note: This figure shows changes in economic outcomes around *Citizens United* using a propensity score matching estimator. Treated and control states are matched on the covariates shown in Panel A of Table A3. Panels (a) and (b) show total income; Panels (c) and (d) show capital income. Panels (e) and (f) show labor income. Panels (g) and (h) show labor share. Panels (a), (c), (e), and (g) use the BEA data; Panels (b), (d), (f), and (h) use analogous the 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.



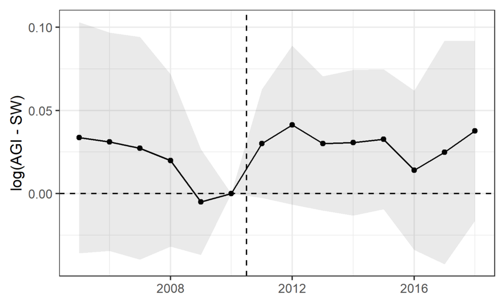
(a) BEA: GDP



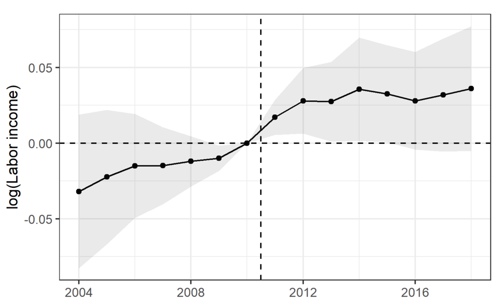
(b) IRS: AGI



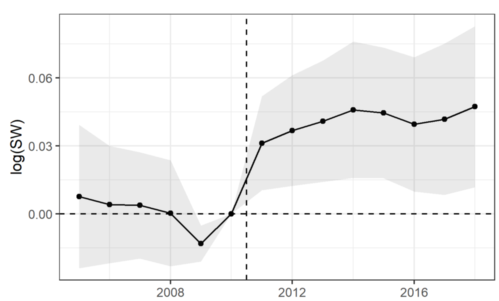
(c) BEA: Capital income



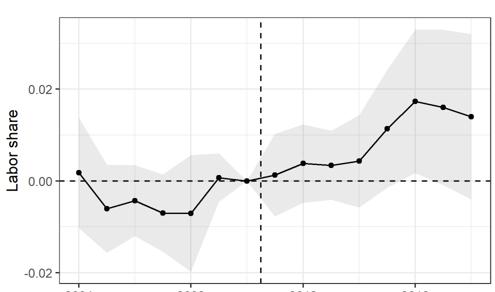
(d) IRS: Non-SW income



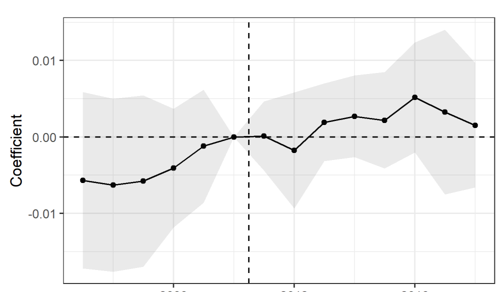
(e) BEA: Labor income



(f) IRS: SW income



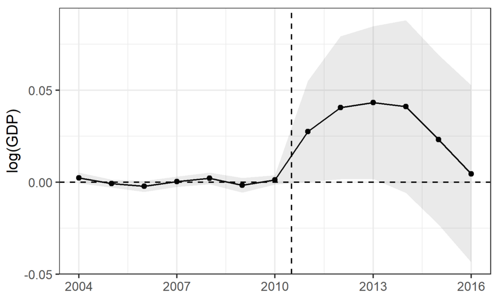
(g) BEA: Labor share



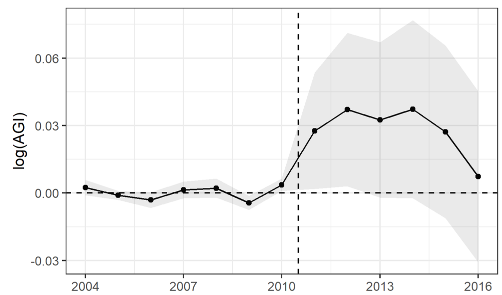
(h) IRS: SW share

Figure 5: Total and Factor Income results with Synthetic Controls

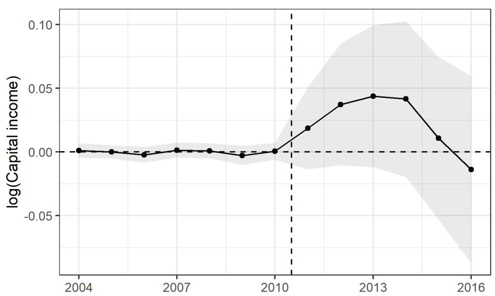
Note: This figure shows changes in economic outcomes around *Citizens United* using a synthetic controls estimator. Panels (a) and (b) show total income; Panels (c) and (d) show capital income. Panels (e) and (f) show labor income. Panels (g) and (h) show labor share. Panels (a), (c), (e), and (g) use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



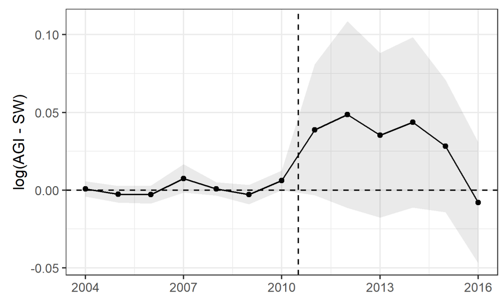
(a) BEA: GDP



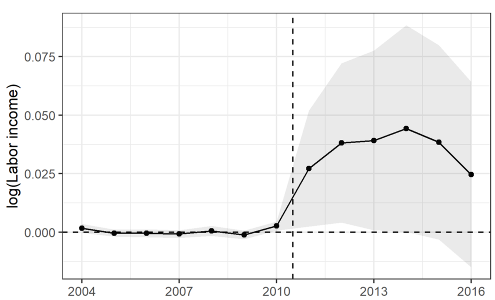
(b) IRS: AGI



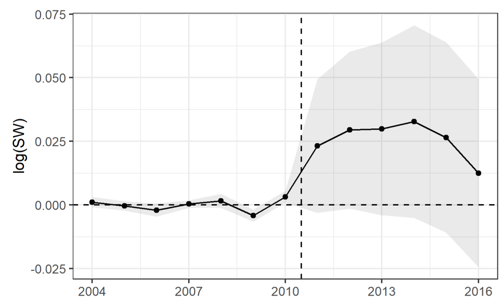
(c) BEA: Capital income



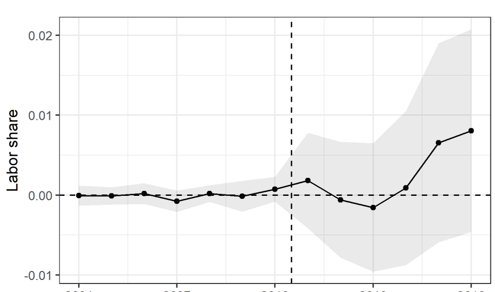
(d) IRS: Non-SW income



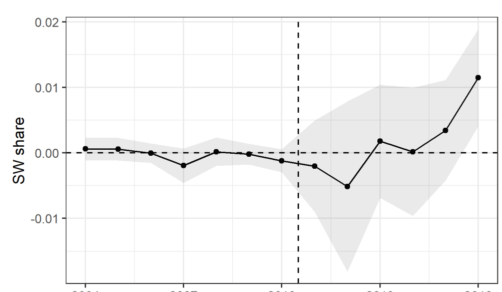
(e) BEA: Labor income



(f) IRS: SW income



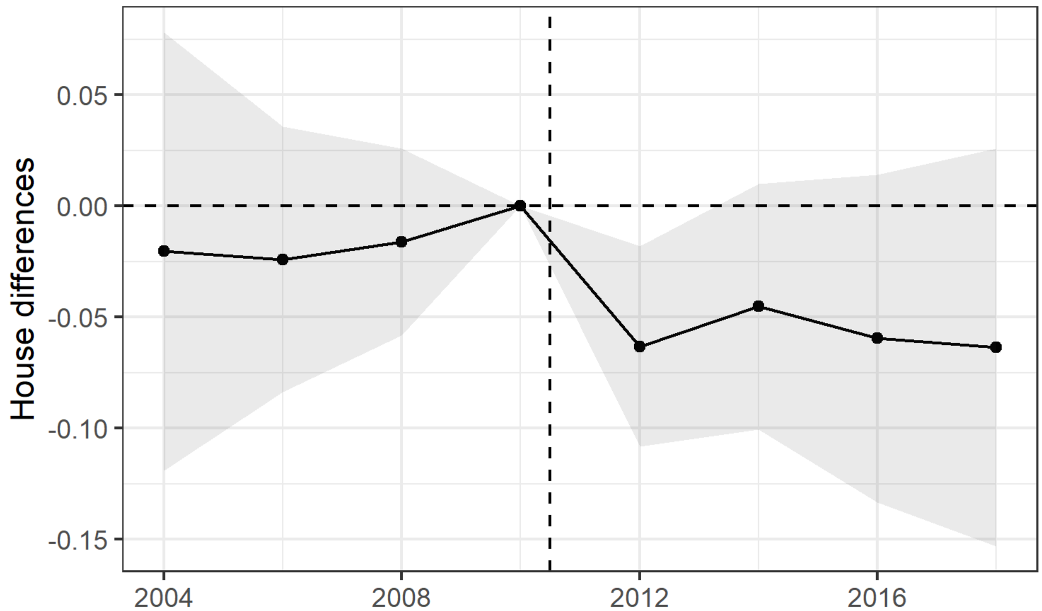
(g) BEA: Labor share



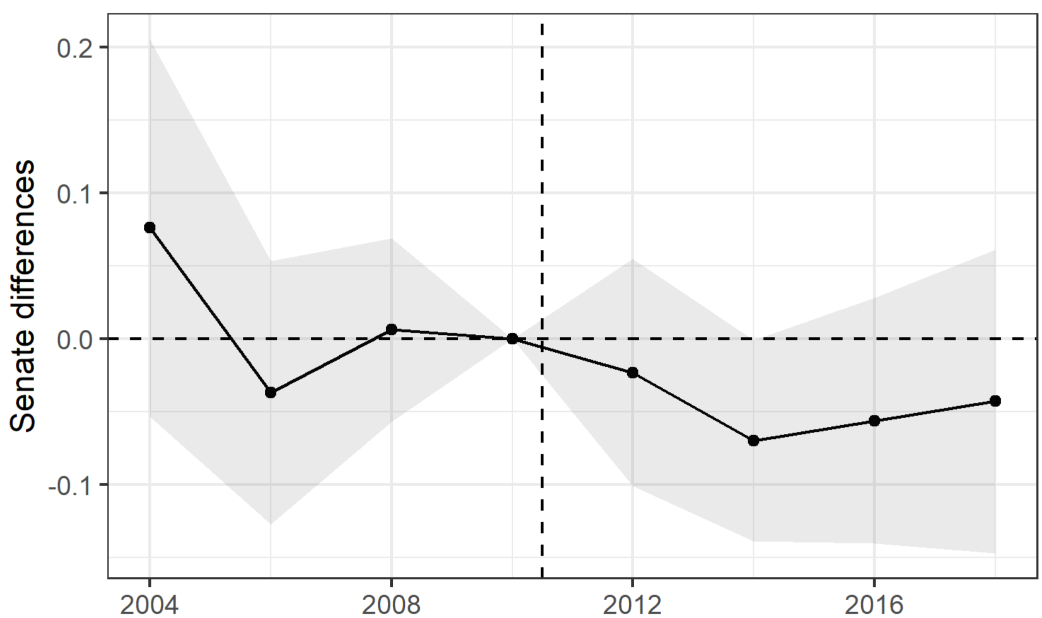
(h) IRS: SW share

Figure 6: Political Polarization

Note: This figure shows changes in political polarization 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 in the lower state House (Panel (a)) or the state Senate (Panel (b)). All specifications include state and year times 2010 governor party fixed effects. Standard errors are clustered at the state level.



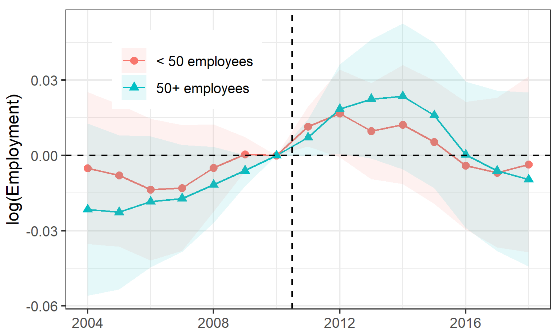
(a) Mean state House distance



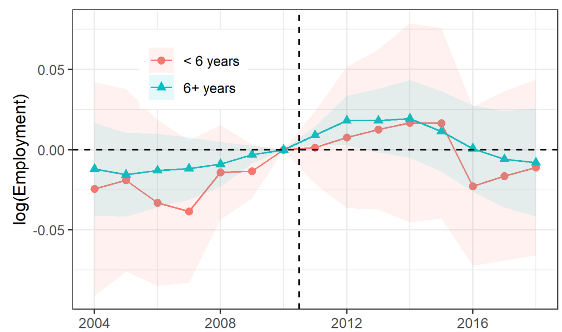
(b) Mean state Senate distance

Figure 7: ECONOMIC OUTCOMES BY FIRM SIZE AND AGE USING QWI DATA

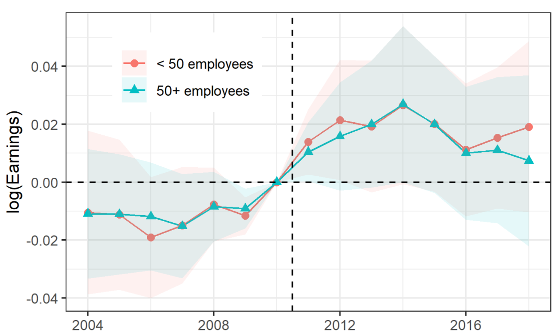
Note: This figure shows changes in state-level total employment, (average) earnings, and total payroll aggregated by firm age and size around *Citizens United*. States affected by the *Citizens United* case (treated states) are those that had bans on corporate or union independent political expenditures 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 worker employment; (c) and (d) show log weekly 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 and year times 2010 governor party fixed effects. Standard errors are clustered at the state level.



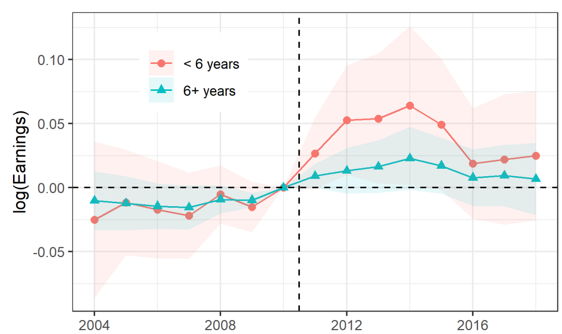
(a) Employment by firm size



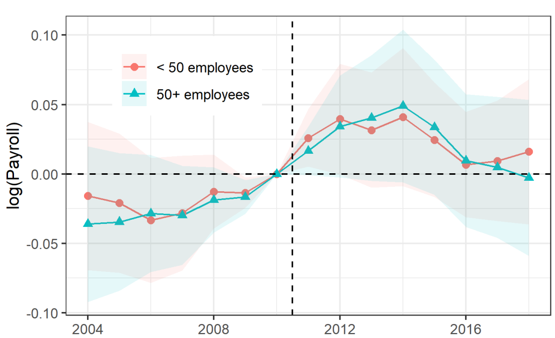
(b) Employment by firm age



(c) Earnings by firm size



(d) Earnings by firm age

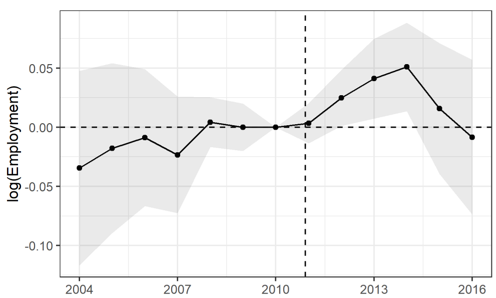


(e) Payroll by firm size

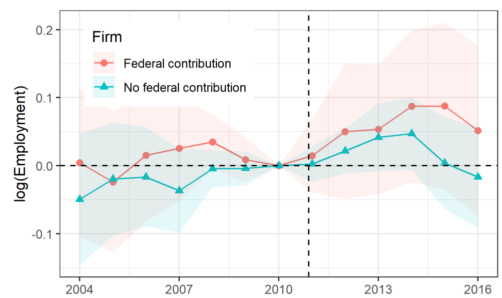
(f) Payroll by firm age

Figure 8: Firm-level employment by Ex-Ante political activity

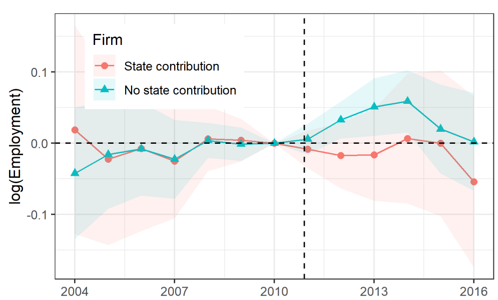
Note: This figure shows changes in firm-level employment around *Citizens United* by ex-ante firm political activity. States affected by the *Citizens United* case (treated states) are those that had bans on corporate or union independent political expenditures pre-*Citizens United*—the bans that the Supreme Court invalidated. The figures show the yearly coefficients and 95% confidence intervals from regressions estimated using Equation (2) where the outcome is the log of firm-level employment among US public firms. Panel (a) presents results for the full sample. Panel (b) divides firms by whether they made campaign contributions to federal election candidates in the 2004, 2006, 2008, or 2010 election cycles. Panel (c) divides firms by whether they were a firm in the S&P 500 Index that made campaign contributions to state election candidates in the 2004, 2006, 2008, or 2010 election cycles. Panel (d) divides firms by whether they are S&P 500 firms that engaged in reportable federal lobbying before *Citizens United*. Panel (e) divides firms by whether they were above or below median size (as measured by total assets) in 2010. Firm employment and financial data come from Compustat; political data come from the Federal Elections Commission and the The National Institute on Money in Politics. All specifications include state and year times 2010 governor party fixed effects. Standard errors are clustered at the state level.



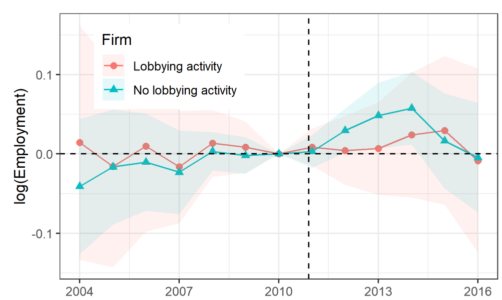
(a) All firms



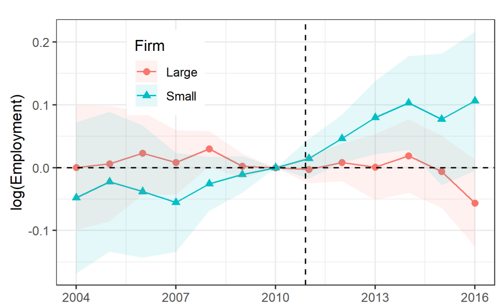
(b) Active (federal contributions)



(c) Active (state contributions)



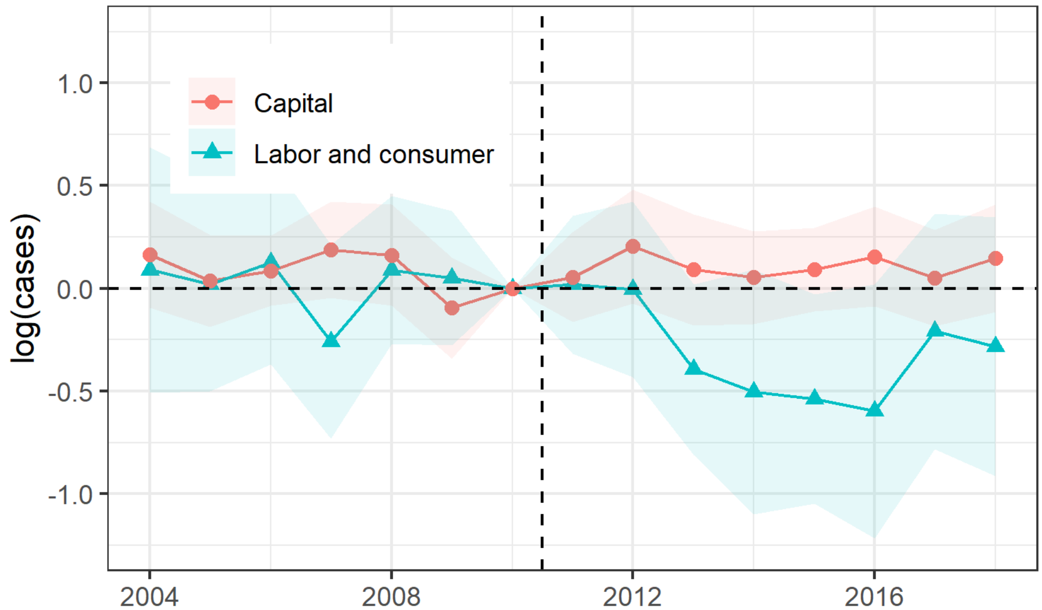
(d) Active (lobbying)



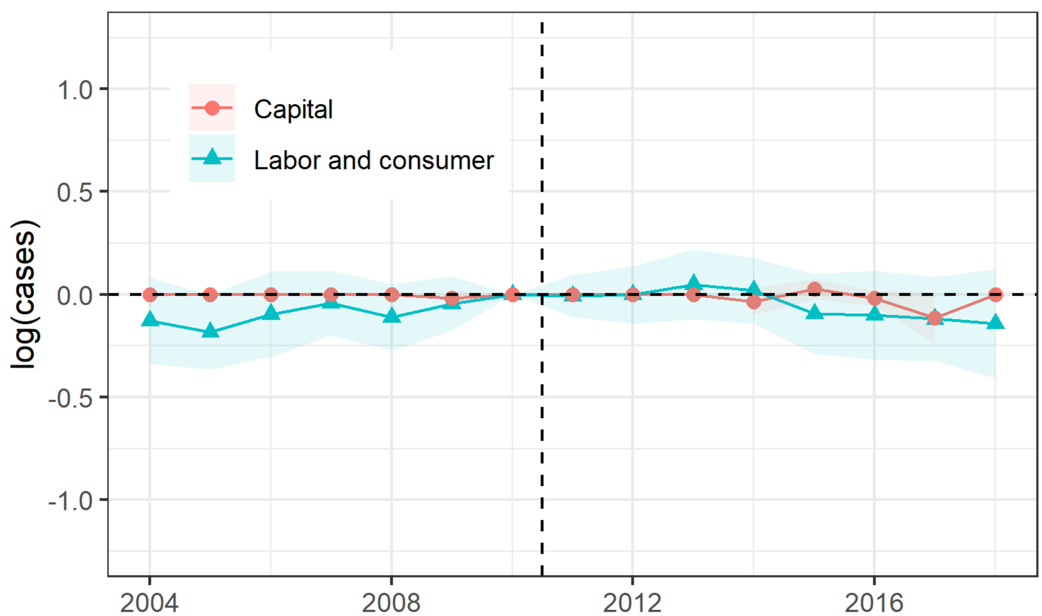
(e) Size

Figure 9: 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). We use $log(1+Y)$ because the data include a few zeros. Panel (b) shows the equivalent for federal enforcement. Enforcement action data comes from *Good Jobs First*. All specifications include state and year times 2010 governor party 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, and legal enforcement. Note that in contrast to other variables, most political outcomes are measured every two years. Panel B shows means of variables for treated and control states as well as the p-value for the difference in means.

Panel A: Summary statistics

Statistic	N	Mean	St. Dev.	Pctl(25)	Median	Pctl(75)
CU (Treated)	50	0.47	0.50	0.00	0.00	1.00
Republican house	280	0.51	0.16	0.39	0.51	0.63
New house	280	0.27	0.11	0.18	0.25	0.35
New republican house	280	0.15	0.09	0.08	0.13	0.20
New democrat house	280	0.12	0.07	0.07	0.11	0.15
Republican senate	259	0.52	0.18	0.40	0.52	0.63
New senate	259	0.23	0.13	0.14	0.22	0.31
New republican senate	259	0.13	0.10	0.05	0.12	0.20
New democrat senate	259	0.10	0.07	0.04	0.09	0.14
New governor	350	0.29	0.46	0.00	0.00	1.00
Republican governor	350	0.56	0.50	0.00	1.00	1.00
GDP ($B, BEA)	750	316.93	398.24	75.81	195.44	387.98
Labor income ($B, BEA)	750	169.97	207.50	40.61	102.23	216.06
Capital income ($B, BEA)	750	125.44	164.64	30.99	78.30	146.66
Labor share (BEA)	750	0.54	0.04	0.52	0.54	0.56
Employment (m, QWI)	705	2.66	2.90	0.69	1.74	3.38
Earnings ($B, QWI)	705	3,756.06	659.99	3,300.75	3,654.75	4,100.50
Payroll ($B, QWI)	705	127.64	161.62	28.71	70.87	156.54
AGI ($B, IRS)	700	180.32	222.10	42.26	105.00	223.91
Salary/wage income ($B, IRS)	700	124.71	150.40	29.62	73.91	160.33
House differences	708	1.56	0.49	1.23	1.50	1.87
Senate differences	713	1.53	0.49	1.17	1.53	1.83
Ad spending ($M)	570	15.76	25.50	0.86	6.41	18.23
Violations (state, aggregate)	750	23.74	81.16	2	4	11
Violations (federal, aggregate)	750	393.01	417.82	125	246.5	494.5
Violations (state, labor and consumer)	750	21.30	80.06	1	3	8.8
Violations (federal, labor and consumer)	750	353.22	384.98	109	213	434
Violations (state, capital)	750	1.20	0.87	1	1	1
Violations (federal, capital)	750	1.01	0.11	1	1	1
Occupational Licensure	550	0.54	0.11	0.48	0.55	0.63
Regulatory freedom index	550	0.00	0.13	−0.08	0.04	0.09

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 (2010)	11.98	12.16	0.55
Log labor income (2010)	11.33	11.52	0.53
Log capital income (2010)	11.08	11.25	0.58
Labor share (2010)	0.52	0.53	0.51
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

Table 2: Political Advertising and Contributions After *Citizens United*

Note: This table shows changes in total political advertising expenditures and 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 estimated using Equation (1) where the dependent variables are political outcomes at the state-year level. *Post* is an indicator for 2011 or later (after the *Citizens United* decision). Panel A shows the results where the dependent variable is the natural logarithm of political advertising spending from Ad$pender. Panel B shows the results where the dependent variables are the natural logarithm of direct contributions to political campaigns at the state-sector level in a given year. 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). All regressions include state and year times 2010 governor party 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 advertising

	Dependent variable:
	log(Ad spending)
Post × Treated	0.304**
	(0.149)
State FE	Y
Year × Gov. Party FE	Y
Observations	342
Adjusted R^2	0.700

Panel B: Political contributions by sector

NIMP Sector	Coefficient	t-value		% All Contributions
All sectors	0.27	1.69	*	100.00
Agriculture	0.34	0.87		2.54
Communications and Electronics	0.20	0.93		3.02
Construction	0.41	1.16		3.42
Defense	0.32	0.69		0.07
Energy and Natural Resources	0.44	1.41		5.65
Finance, Insurance and Real Estate	0.30	1.21		13.24
General Business	0.50	1.75	*	12.56
Government Agencies/Education/Other	-0.05	-0.11		5.08
Health	0.23	1.05		6.35
Ideology/Single Issue	0.26	0.50		8.93
Labor	0.55	2.25	**	12.70
Lawyers and Lobbyists	0.53	2.19	**	6.71
Transportation	0.17	0.51		1.52
Uncoded	0.25	0.94		14.06
Unitemized Contributions	0.96	2.01	**	4.16

Table 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). All columns include state and year times 2010 governor party fixed effects. Data include two pre- and two post-CU political cycles and are bucketed into political cycle (two-year) frequency. Panel B shows 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). Standards 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 vs. 2010	Republican governor	New | R	New | D
	(1)	(2)	(3)	(4)
Post × Treated	0.221**	0.049	0.187	0.248*
	(0.108)	(0.114)	(0.166)	(0.145)
State FE	Y	Y	Y	Y
Year x 2010 State Governor Party FE	Y	Y	Y	Y
Observations	200	200	96	104
Adjusted R^2	0.277	0.500	0.150	0.368

Panel B: Political turnover in the state legislatures

	Dependent variable:							
	State house				State senate			
	New	Rep	New Rep	New Dem	New	Rep	New Rep	New Dem
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post × Treated	0.028	0.051**	0.027	0.0005	0.008	0.043	0.004	0.007
	(0.029)	(0.024)	(0.024)	(0.016)	(0.042)	(0.035)	(0.034)	(0.019)
State FE	Y	Y	Y	Y	Y	Y	Y	Y
Year x 2010 State Governor Party FE	Y	Y	Y	Y	Y	Y	Y	Y
Observations	160	160	160	160	148	148	148	148
Adjusted R^2	0.522	0.883	0.532	0.479	0.274	0.862	0.355	0.199

Table 4: Total and factor incomes

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 2011 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 2007 through 2015. 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 (capital income for BEA; AGI less salary and wage (SW) income for IRS). Column (3) is a measure of labor income (labor income 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 and year times 2010 governor party 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)	Labor share
	(1)	(2)	(3)	(4)
Post × Treated	0.030	0.019	0.040**	0.005
	(0.023)	(0.031)	(0.019)	(0.005)
State FE	Y	Y	Y	Y
Year x 2010 State Governor Party FE	Y	Y	Y	Y
Observations	450	450	450	450
Adjusted R^2	0.998	0.996	0.999	0.890

Panel B: IRS data

	Dependent variable:			
	log(AGI)	log(AGI - SW)	log(SW)	SW share
	(1)	(2)	(3)	(4)
Post × Treated	0.037**	0.035	0.037**	−0.00003
	(0.018)	(0.022)	(0.016)	(0.003)
State FE	Y	Y	Y	Y
Year x 2010 State Governor Party FE	Y	Y	Y	Y
Observations	450	450	450	450
Adjusted R^2	0.999	0.997	0.999	0.933

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 2011 or later (after the *Citizens United* decision). Data are from the US Census's QWI database and run from 2007 through 2015. Employment is the number of employees averaged over the four quarters of each year. Earnings is average monthly 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*, and its corresponding *t*-values and statistical significance. The last column ("% All Employment") shows the percentage of employees in each sector over the whole period to facilitate 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 industry-level indicator equal to one if the aggregate industry political contributions between 2006 and 2009 to states are above median. Observations are weighted by the proportion of employees in each sector as of 2010. All specifications in Panel A include state and year times 2010 governor party fixed effects. All specifications in Panel B includes state time active industry and year times 2010 governor party times politically active industry 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 industry

	Log Employment			Log Earnings			Log Payroll			
NAICS sector	Coef	t-value		Coef	t-value		Coef	t-value		% Employment
All sectors	0.03	2.16	**	0.02	2.06	**	0.05	2.13	**	100.00
Agriculture	0.04	1.40		0.03	2.37	**	0.07	2.07	**	1.19
Mining	0.21	1.99	**	0.02	1.02		0.14	0.29		0.55
Utilities	-0.03	-1.19		0.00	-0.29		0.25	1.08		0.54
Construction	0.09	2.66	***	0.03	2.10	**	0.12	2.61	***	5.22
Manufacturing	0.05	3.43	***	0.01	1.39		0.06	3.30	***	8.91
Wholesale Trade	0.05	2.58	***	0.02	1.65	*	0.07	2.42	**	4.18
Retail Trade	0.01	1.03		0.01	1.38		0.02	1.10		11.70
Transportation	0.05	1.18		0.01	0.41		0.05	0.90		3.44
Information	0.01	0.58		0.03	1.71	*	0.03	1.03		2.27
Finance	0.01	0.59		0.01	1.12		0.02	1.03		4.10
Real Estate	0.05	2.30	**	0.05	2.18	**	0.10	2.19	**	1.56
Professional Services	0.02	1.58		0.02	1.70	*	0.04	1.76	*	5.91
Management	-0.04	-0.22		0.01	0.67		-0.07	-0.37		1.54
Waste	0.01	0.76		0.01	0.91		0.02	0.96		7.38
Education	-0.02	-0.92		0.01	1.18		0.00	0.14		8.75
Health	0.00	0.00		0.02	1.89	*	0.02	1.18		13.32
Arts and Recreation	-0.01	-0.71		0.00	0.12		-0.02	-0.53		1.98
Food Services	0.01	1.00		0.03	1.94	*	0.03	1.70	*	9.92
Public Administration	0.02	1.22		0.02	1.82	*	0.39	0.93		3.94
Other	0.03	2.24	**	0.01	0.41		0.04	2.18	**	3.39

Panel B: Effects by politically engaged industries

	Dependent variable:		
	log(Employment)	log(Earnings)	log(Payroll)
	(1)	(2)	(3)
Post × Treated	0.022*	0.014	0.035*
	(0.011)	(0.010)	(0.019)
Post × Treated × Active	0.005	0.003	0.008
	(0.006)	(0.004)	(0.008)
State × Active FE	Yes	Yes	Yes
Year × 2010 Gov. Party × Active FE	Yes	Yes	Yes
Observations	8,456	8,456	8,456
Adjusted R^2	0.627	0.367	0.684

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 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 2011 or later (after the *Citizens United* decision). Economic data are from the US Census's QWI and run from 2007 through 2015. Employment is the beginning-of-quarter number of employees. Earnings is average monthly 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 times firm type fixed effects and year times 2010 governor party times firm type 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	0.026*	0.027**	0.046*	0.051*
	(0.014)	(0.013)	(0.026)	(0.027)
Post × Treated × Small	−0.011	0.002	0.008	−0.005
	(0.008)	(0.006)	(0.008)	(0.008)
State × Size FE	Y	Y	Y	Y
Year × 2010 Gov. Party × Size FE	Y	Y	Y	Y
Observations	864	864	864	864
Adjusted R^2	0.999	0.989	0.958	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	0.021*	0.024**	0.043*	0.044*
	(0.012)	(0.012)	(0.024)	(0.023)
Post × Treated × Young	0.006	0.036*	0.035**	0.046
	(0.024)	(0.020)	(0.017)	(0.036)
State × Age FE	Y	Y	Y	Y
Year × 2010 Gov. Party × Age FE	Y	Y	Y	Y
Observations	864	864	864	864
Adjusted R^2	0.999	0.975	0.922	0.998

Table 7: Firm-level employment

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 2011 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 Lobbying 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 2010, and is zero otherwise. These three variables form the *Characteristic* variable noted in the regression table below and is interacted with *Post* in their respective specification to fully specify the triple-difference model. Employment and financial data comes from Compustat, while political data comes from the Federal Elections Commission and the The National Institute on Money in Politics. Firm employment and financial data come from Compustat; political data come from the Federal Elections Commission, the The National Institute on Money in Politics and Center for Responsible Politics (Open Secrets). The sample runs from 2007 through 2015. All specifications include state and year times 2010 governor party fixed effects. Specifications also include 2010 values of various 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. Standard errors, in parentheses, are clustered at the state level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels respectively.

	Log(Employment)				
	(1)	(2)	(3)	(4)	(5)
Post × Treated	0.0291*	0.0324	0.0311*	0.0274*	0.0814***
	(0.0153)	(0.0198)	(0.0177)	(0.0163)	(0.0258)
Post × Treated × Federal Contributions		-0.0167			
		(0.0529)			
Post × Treated × S&P 500 State Contributions			-0.0160		
			(0.0420)		
Post × Treated × S&P 500 Federal Lobbying				0.00395	
				(0.0345)	
Post × Treated × Large					-0.0923**
					(0.0366)
Firm FE	Yes	Yes	Yes	Yes	Yes
Year × 2010 State Governor Party FE	Yes	Yes	Yes	Yes	Yes
Post × 2010 Controls	Yes	Yes	Yes	Yes	Yes
Post × Characteristic	No	Yes	Yes	Yes	Yes
Observations	21716	21716	21716	21716	21716
Adjusted R^2	0.970	0.971	0.971	0.971	0.970

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 2007 through 2015. Panel B shows changes in occupational licensing and regulatory freedom within each state. Columns (1)–(2) show changes in the employment-weighted occupational license requirements in a given state-year. Columns (3)–(4) show the estimated changes in the regulatory freedom, where a more positive sign means lesser regulatory burden. Columns (1) and (3) limit the sample to the main period used in the paper (2007 to 2015); (2) and (4) limit the sample period to 2007 to 2017. 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. Standards 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 + enforcement actions)					
	State			*Federal*		
	All	Labor/consumer	Capital	All	Labor/consumer	Capital
	(1)	(2)	(3)	(4)	(5)	(6)
Post × Treated	−0.314	-0.461^{**}	−0.007	0.027	0.025	0.0003
	(0.203)	(0.190)	(0.052)	(0.057)	(0.061)	(0.014)
State FE	Y	Y	Y	Y	Y	Y
Year x 2010 State Governor Party FE	Y	Y	Y	Y	Y	Y
Observations	450	450	450	450	450	450
Adjusted R^2	0.837	0.859	0.519	0.962	0.958	−0.011

Panel B: Occupational Licensing and Regulatory Freedom

	Dependent variable:			
	Occupational Licensure		Regulatory Freedom	
	(1)	(2)	(3)	(4)
Post × Treated	−0.014	-0.015^{*}	0.006^{*}	0.011^{**}
	(0.008)	(0.009)	(0.004)	(0.004)
Period: 2007 to	2015	2017	2015	2017
State FE	Y	Y	Y	Y
Year × 2010 State Governor Party FE	Y	Y	Y	Y
Observations	450	550	450	550
Adjusted R^2	0.952	0.949	0.993	0.989

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 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, and Column (2) uses changes in minimum wage. All specifications include state and year times 2010 governor party fixed effects. Standards 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	−0.013	−0.538	−0.306	0.0002	-0.159^{*}
	(0.071)	(0.352)	(0.322)	(0.002)	(0.087)
State FE	Y	Y	Y	Y	Y
Year x 2010 State Governor Party FE	Y	Y	Y	Y	Y
Observations	450	450	450	450	450
Adjusted R^2	0.989	0.930	0.963	1.000	0.774

Panel B: Minimum wage

	Dependent variable:	
	Minimum wage	Δ minimum wage
	(1)	(2)
Post × Treated	−0.006	−0.564
	(0.080)	(1.141)
State FE	Y	Y
Year x 2010 State Governor Party FE	Y	Y
Observations	450	450
Adjusted R^2	0.837	0.233

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). Data run from 2007 through 2015. Panel A examines state government revenues and expenditures. Column "pct" shows the revenue (expenditure) share coming from each category as a percentage of all revenues (expenditures). Columns (1), (2), and (3) 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; and columns (3) and (6) for combined state and local government subsidies. All specifications include state fixed effects and year times 2010 governor party 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)	(2)	(3)
		pct	coef	tval	significance
Revenues					
1	Total Revenue	100.00	-0.01	-0.32	
2	–General revenue	82.91	0.01	0.37	
3	—-Intergovernmental revenue	26.91	0.02	0.64	
4	—-Taxes	40.58	0.02	0.50	
5	——General sales	12.71	0.06	1.31	
6	——Selective sales	6.57	0.02	0.57	
7	——License taxes	2.56	0.01	0.34	
8	——Individual income tax	14.33	0.02	0.70	
9	——Corporate income tax	2.29	-0.27	-1.18	
10	——Other taxes	2.11	0.01	0.08	
11	—-Current charges	8.89	-0.03	-0.87	
12	—-Miscellaneous general revenue	6.54	-0.01	-0.30	
13	–Utility revenue	0.77	2.37	2.03	**
14	–Liquor store revenue	0.35	1.32	1.04	
15	Insurance trust revenue	15.96	-0.03	-0.41	
Expenditures					
16	Total expenditure	100.00	0.01	0.47	
17	–General expenditure	84.39	0.01	0.45	
18	—-Intergovernmental expenditure	25.46	0.02	0.44	
19	—-Direct expenditure	74.54	0.01	0.50	
20	——Current operation	50.03	0.00	-0.01	
21	——Capital outlay	6.06	0.15	2.87	***
22	——Insurance benefits and repayments	13.98	-0.01	-0.29	
23	——Assistance and subsidies	2.03	-0.01	-0.19	
24	——Interest on debt	2.43	0.04	0.68	
25	Exhibit: Salaries and wages	12.42	0.05	1.29	
26	——Education	30.13	0.00	0.07	
27	——Public welfare	25.13	-0.03	-1.13	
28	——Hospitals	3.29	0.00	0.03	
29	——Health	3.07	-0.03	-0.35	
30	——Highways	5.80	0.13	2.05	**
31	——Police protection	0.73	0.06	1.05	
32	——Correction	2.54	0.03	1.26	
33	——Natural resources	1.12	0.04	0.86	
34	——Parks and recreation	0.30	0.11	1.36	
35	——Government administration	2.93	0.01	0.20	
36	——Interest on general debt	2.31	0.04	0.67	
37	——Other and unallocable	5.96	0.03	0.29	
38	–Utility expenditure	1.60	0.16	0.10	
39	–Liquor store expenditure	0.28	2.33	1.61	
40	–Insurance trust expenditure	13.98	-0.01	-0.29	

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	0.300	−0.112	0.158	0.564	0.154	0.287
	(0.369)	(0.213)	(0.296)	(1.408)	(0.982)	(1.120)
State FE	Y	Y	Y	Y	Y	Y
Year × Gov. Party FE	Y	Y	Y	Y	Y	Y
Observations	435	435	435	435	435	435
Adjusted R^2	0.619	0.882	0.683	0.523	0.772	0.609

Appendix

A Additional Tables and Figures

Figure A1: 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. The specification includes state and year times 2010 governor party fixed effects. Standard errors are clustered at the state level.

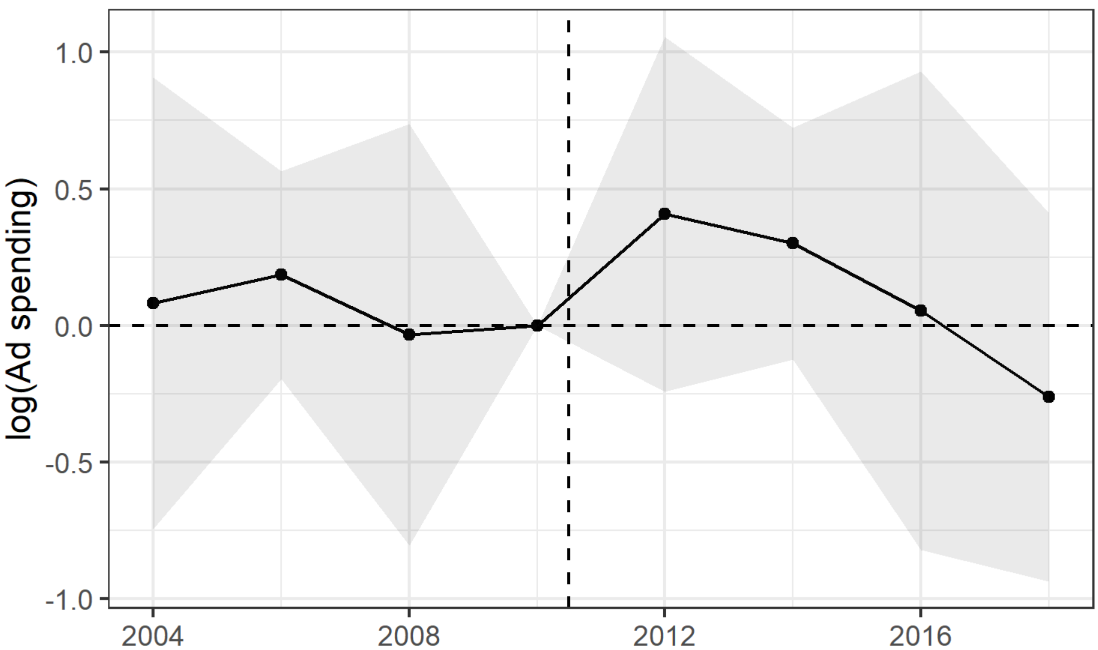


Table A1: Economic outcomes using QWI data

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 2011 or later (after the *Citizens United* decision). Data are from the US Census's QWI and run from 2007 through 2015. Employment is the beginning-of-quarter number of employees. Earnings is average monthly 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 and year times 2010 governor party 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)	log(Earnings)	log(New worker earnings)	log(Payroll)
	(1)	(2)	(3)	(4)
Post × Treated	0.022*	0.025**	0.049*	0.046*
	(0.012)	(0.012)	(0.025)	(0.024)
State FE	Y	Y	Y	Y
Year x 2010 State Governor Party FE	Y	Y	Y	Y
Observations	432	432	432	432
Adjusted R^2	1.000	0.982	0.931	0.999

Table A2: Total and factor incomes with house price change controls

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 2011 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 2007 through 2015. 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 (capital Income for BEA; AGI less salary and wage income for IRS). Column (3) is a measure of labor income (labor income 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 and year times 2010 governor party times quartiles of house price changes 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)	Labor share
	(1)	(2)	(3)	(4)
Post × Treated	0.038	0.016	0.055**	0.009
	(0.027)	(0.036)	(0.022)	(0.005)
State FE	Y	Y	Y	Y
Year × Gov. Party $\times \Delta HP_{2002,2006}$ FE	Y	Y	Y	Y
Observations	900	900	900	900
Adjusted R^2	0.997	0.994	0.998	0.861

Panel B: IRS data

	Dependent variable:			
	log(AGI)	log(AGI - SW)	log(SW)	SW share
	(1)	(2)	(3)	(4)
Post × Treated	0.046***	0.037*	0.048***	0.002
	(0.017)	(0.022)	(0.014)	(0.003)
State FE	Y	Y	Y	Y
Year × Gov. Party $\times \Delta HP_{2002,2006}$ FE	Y	Y	Y	Y
Observations	450	450	450	450
Adjusted R^2	0.999	0.998	0.999	0.938

A.1 Robustness with propensity score matching

As a robustness check, we redo our main analysis on economic outcomes (factor 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 A3, Panel A shows the differences between the treated and the matched control sample (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 4 shows the corresponding event studies. Again, the results are qualitatively similar, although the matching estimator somewhat reduces the presence of pre-trends, especially in the BEA data. 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 A3: Economic 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 and matched control samples 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
2008 Obama vote share	-0.01	0.87
Republican governor	0	1
Population	-0.07	0.29
Median household income	-0.43	0.25
Log GDP	-0.44	0.24
Log labor income	-0.47	0.22
Log capital income	-0.40	0.27
Labor share	-0.02	0.24
Fraction with bachelors	-0.03	0.17
Unemployment (2010)	0.001	0.90
90+ days mortgage delinquency (2010)	-0.003	0.53
House price change 2002-2006	-0.04	0.51
House price change 2007-2010	0.01	0.64

Panel B: BEA data

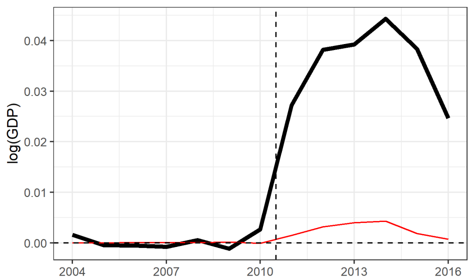
	Dependent variable:			
	log(GDP)	log(Capital income)	log(Labor income)	Labor share
	(1)	(2)	(3)	(4)
Post × Treated	0.021	0.003	0.037**	0.008
	(0.023)	(0.034)	(0.017)	(0.006)
State FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Observations	306	306	306	306
Adjusted R^2	0.998	0.995	0.999	0.868

Panel C: IRS data

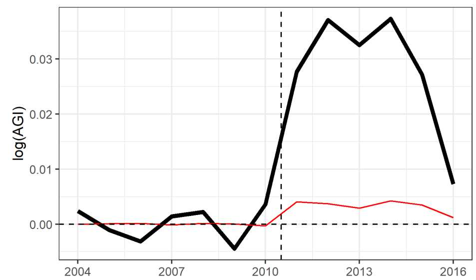
	Dependent variable:			
	log(AGI)	log(AGI - SW)	log(SW)	SW share
	(1)	(2)	(3)	(4)
Post × Treated	0.037**	0.022	0.042***	0.004
	(0.018)	(0.025)	(0.015)	(0.003)
State FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Observations	306	306	306	306
Adjusted R^2	0.999	0.997	0.999	0.923

Figure A2: Total and Factor Income results with Synthetic Controls

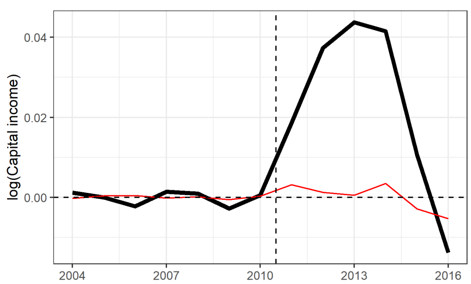
Note: This figure shows changes in economic outcomes around *Citizens United* using a synthetic controls estimator. The results mirror those in Figure 5, with the addition of a permutation test. The solid black line is the actual estimated treatment effect. The lighter red line is the median treatment effect among 100 permutations where treatment is randomly assigned to states. Panels (a) and (b) show total income; Panels (c) and (d) show capital income. Panels (e) and (f) show labor income. Panels (g) and (h) show labor share. Panels (a), (c), (e), and (g) use the BEA data; Panels (b), (d), (f), and (h) use analogous IRS data.



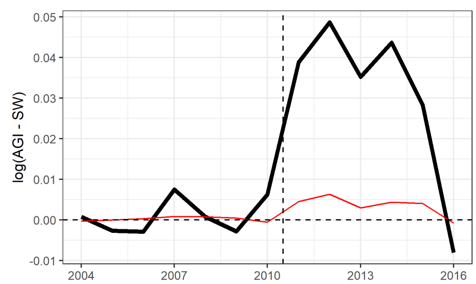
(a) BEA: GDP



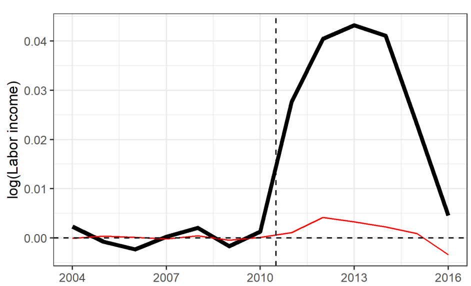
(b) IRS: AGI



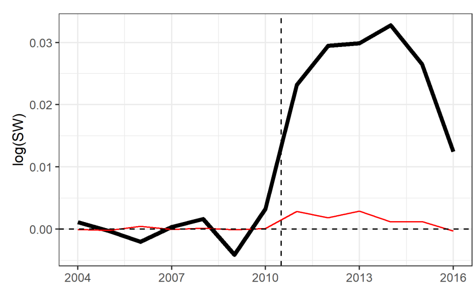
(c) BEA: Capital income



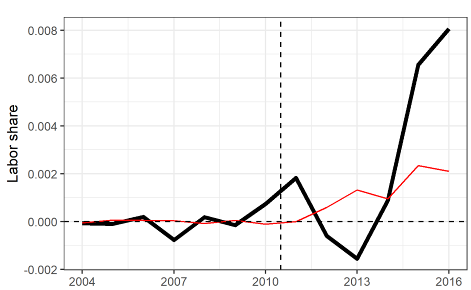
(d) IRS: Non-SW income



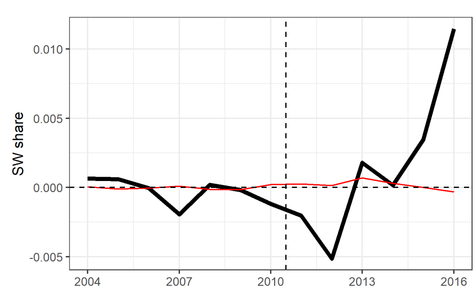
(e) BEA: Labor income



(f) IRS: SW income



(g) BEA: Labor share



(h) IRS: SW share

Table A4: Polarization

Note: This table shows changes in polarization of state politicians 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 are polarization of state-level politicians. *Post* is an indicator for 2011 or later (after the *Citizens United* decision). Data come from Shor and McCarty (2011) from 2007 through 2015. In Column (1), the polarization is measured at the state House, and in Column (2)—at the state Senate. The estimate of polarization comes from a combination of (i) ideological ideal point (how often legislators vote with other legislators on a common set of roll calls) and (ii) a recurring survey of state legislative candidates to allow comparisons across time, chambers, and states. All specifications include state fixed effects and year times 2010 governor party 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:	
	House differences	Senate differences
	(1)	(2)
Post × Treated	−0.044	−0.041
	(0.028)	(0.032)
State FE	Y	Y
Year x 2010 State Governor Party FE	Y	Y
Observations	419	420
Adjusted R^2	0.982	0.966

Table A5: TOTAL AND FACTOR INCOMES FOR 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 4 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 2011 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 2007 through 2015. 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 (capital income for BEA; AGI less salary and wage income for IRS). Column (3) is a measure of labor income (labor income 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 and year times 2010 governor party 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)	Labor share
	(1)	(2)	(3)	(4)
Post × Treated	0.036	0.024	0.050**	0.006
	(0.030)	(0.038)	(0.024)	(0.006)
State FE	Y	Y	Y	Y
Year x 2010 State Governor Party FE	Y	Y	Y	Y
Observations	378	378	378	378
Adjusted R^2	0.998	0.996	0.999	0.889

Panel B: IRS data

	Dependent variable:			
	log(AGI)	log(AGI - SW)	log(SW)	SW share
	(1)	(2)	(3)	(4)
Post × Treated	0.050**	0.048*	0.048**	−0.0005
	(0.022)	(0.028)	(0.019)	(0.003)
State FE	Y	Y	Y	Y
Year x 2010 State Governor Party FE	Y	Y	Y	Y
Observations	378	378	378	378
Adjusted R^2	0.999	0.998	0.999	0.930