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

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Abstract

The perceived increase in corporate political influence has raised concerns that corporations advance policies that benefit capital and harm labor. We examine whether money in politics harms labor using the surprise Supreme Court ruling *Citizens United v. FEC (2010)*, which rendered bans on political spending unconstitutional, affecting roughly half of US states (treated states). In a difference-in-difference analysis, we find that treated states see increased political turnover and, surprisingly, increased labor income. We show evidence that these effects are driven by increased political competition whereby money allows for more political entry from firms that could not exert political influence in other ways. On net, the economic environment becomes more business-friendly and some of these gains are passed on to workers.

Keywords: Citizens United, money in politics, political spending, labor income, capital income, labor share, wages, earnings, minimum wage

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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 [...].

—Barak 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 corporation [...] to make its voice heard in campaigns without hiring an army of lawyers or asking the FEC how it may speak.

—Bradley A. Smith

The last several decades have witnessed two striking trends. First, firms have wielded substantially more political power and devoted increased resources to political engagement (Zingales, 2017). Second, there has been a marked increase in firm concentration and in the share of national output paid to capital as opposed to labor (e.g., Grullon et al., 2019; Autor et al., 2020). It is natural to conclude that these trends might be connected: firms exercise political power to enact policies that benefit them at the expense of workers. To our knowledge, however, no research has attempted to make this link explicit. In this paper, we examine how payments to labor and capital respond to increased corporate political participation in elections following *Citizens United v FEC* (hereafter *Citizens United*), a landmark US Supreme Court case that increased the amount of money in politics, which is often cited as a major catalyst to increased corporate political power.

Increased money in politics may be beneficial or detrimental to workers. Optimistically, more money in politics may make elections more competitive and lead to the adoption of pro-growth policies, thereby increasing overall economic output. As overall economic output rises, the benefits can accrue to both labor and capital. For example, donors may advocate for particular policies that encourage investment or seek to reduce the severity of regulatory infractions (e.g., Akey et al., 2021; Heitz et al., 2021). To the extent that reducing regulatory burden increases overall output and does not directly harm workers, both labor and capital can share the increase in social surplus. In contrast, money in politics may reflect rent seeking: donors may advance policies that transfer resources to them while leaving overall output unchanged. For example, firms may seek to enact policies that reduce competition in labor and product markets (e.g., Faccio and Zingales, 2021; Cowgill et al., 2022), or reduce

the bargaining power of labor. Such policies will increase firm profits but reduce labor income and possibly even total output.

At the individual firm level, most studies have found that indeed, firm political activity increases firm value (e.g., Cooper et al., 2010; Akey, 2015; Borisov et al., 2016; Bertrand et al., 2020).[1] Importantly, however, one cannot conclude from a firm-level analysis how money in politics will impact *factor* income by a simple aggregation. Mechanically, this is because the identification strategies typically rely on absorbing any aggregate variation in the data. More fundamentally, examining firm-level outcomes ignores the general equilibrium impact of *political competition*. If only Firm A has the ability to engage in political influence, it may successfully take steps to extract rents from a competitor, Firm B, at some social cost. If Firm A and Firm B can *both* engage in political influence, their advocacy may cancel out, or even be channeled towards a socially beneficial policy. Alternatively, they may jointly seek transfers from a non-influential party, e.g., workers. A partial equilibrium firm-level analysis would not distinguish these effects. Thus, the central question of this paper is whether money in politics primarily grows the economic pie or simply redistributes output towards particular factors of production.

We examine this important question in the context of *Citizens United*. This 2010 US Supreme Court decision represents 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 regulations that restricted corporations and unions from directly engaging in politically motivated communication.[2] *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 outcomes of workers and capital providers change in states that had these restrictions overturned (i.e., the treatment states) relative the outcomes of workers and capital providers in 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 three and four percent in states affected by *Citizens United* in the years following the decision. These gains accrue primarily to labor: labor income increases by approximately four percent 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 between roughly two and three percent following the decision. Thus, the labor share of

[1] While this is generally the case, some papers suggest that corporate political activity may be indicative of agency problems (e.g., Aggarwal et al., 2012; Coates IV, 2012).

[2] 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*.

income is roughly unchanged. These results suggest that money in politics increases aggregate economic output and that labor (and likely) capital in aggregate share in the gains.

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 share, population, GDP, labor and capital income, education levels, and unemployment levels. Treated and control states do differ in couple of other 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 pre-*Citizens United* governor party and to the Financial Crisis exposures. 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*.

We provide evidence that the mechanism driving our results is that increased money in politics leads to greater political competition, and, subsequently to the adoption of growth-friendly policies. That is, relative to existing methods of exerting political influence, such as lobbying, personal connections, and the revolving door, money in politics has a relatively low entry cost. When few firms are able to exert political influence, they push for rent-seeking, growth-reducing policies. With a lower cost of political entry, more firms can push for their political preferences, the net effect being policies that are broadly better for growth.

In support of this 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 increased political turnover among governors and state legislators. These changes are not, as is commonly viewed, Republicans taking Democrat seats. Rather, there is increased across- and within-party turnover among both Democrats and Republicans. We also find evidence that state legislatures in treatment states are less polarized after *Citizens United*. These results suggest that well-connected political incumbents are driven out in favor of newcomers with a broader political support.

Once newcomers elected, we find evidence of broadly more pro-growth policies. For example, we find that there are fewer state-level enforcement actions of violations of labor or consumer protection laws in treated states. There are no corresponding changes in similar federal enforcements or adverse worker health outcomes, suggesting that these changes arise from a lighter regulatory touch, rather than from changes in underlying firm behavior. We also find consistent and economically large (though typically not statistically significant) evidence for reductions in corporate and personal income tax rates.

Closing the loop, we find that the effects on workers—increased hiring and wages—are

concentrated in the firms that were least likely to be politically connected 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 the revolving door, see greater growth in labor income and average earnings. Additionally, Compustat firms with no pre-*Citizens United* record of making political contributions see the greatest employment growth. Moreover, we observe increased labor income, employment and wage growth 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 ability to make political contributions had the effect of increasing political competition, thereby leading to increased political turnover, political policies that represent the policy preferences of a broader class of economic agents, and ultimately economic growth that accrues to workers, particularly among the 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 advocacy 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 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 money in politics: labor and capital. Our results highlight that the economic outcomes of political choices are not necessarily zero-sum, and that by bringing a broader set of interests to the table through easier access to political influence, labor can benefit from increased corporate money in politics. However, one cannot conclude that more money in politics is unilaterally better for labor and capital providers from our analysis. 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 is socially improving to maximize the ability of all types of agents to have access to politicians.

Section 1 reviews the literature. Section 2 describes the data. Sections 3 and 4 examine the political and economic outcomes. Section 5 examines the mechanism. 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. 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; Agarwal 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), enable firms to better access government resources (e.g., Claessens et al., 2008; Goldman et al., 2013; Brogaard et al., 2021; Colonnelli et al., 2022; Colonnelli and Prem, 2022), 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; Richard B. Baker and Hilt, 2021). A final area of the literature examines the federal campaign contributions of managers and conclude that managers use their personal contributions to advance the interests of shareholders (e.g., Fremeth et al., 2013; Bonica, 2016; Richter and Werner, 2017; Cohen et al., 2019) and in some cases pressure workers to contribute towards politicians that advance the interests shareholders' interests (Babenko et al., 2020). 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 wages of firm workers.

Our paper also contributes 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; Elsby et al. 2013; Autor et al. 2020; Barkai 2020). However, there is less consensus on what are the causes of the decline in the labor share. A number of researchers have been sounding alarm about the growth of the monopoly power of large firms in the US economy (Philippon, 2019) as well as their political 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 monopoly 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 was affected by increased money in politics due to the 2010 Supreme Court decision *Citizens*

United, which represented one of the largest changes to election campaign finance rules in the post World War II era. We find that labor income actually increases following this case in the affected states, with more muted increases to capital income, and that this wage 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 and electoral outcomes (e.g., Spencer and Wood, 2014; Klumpp et al., 2016). 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). Tenekedjieva (2020) suggests that firms decrease their opportunistic use of charitable contributions to influence politicians once they can more freely engage in political spending because of the *Citizens United* ruling. While there is not a 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 amounts of political contributions. 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). Our paper contributes to this literature by examining how economic returns to workers and capital providers were affected by increase in political spending caused 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 of 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 on January 21, 2010 by the US Supreme Court, which 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 the election or defeat of a clearly identified candidate and which 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 court 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.[3] The empirical design of this paper focuses on the second of these consequences, the unexpected removal of individual state restrictions on political independent spending.

The question at the heart of *Citizens United v. FEC* was to determine whether Citizens United, a conservative non-profit, should have been allowed to advertise a political documentary against Hillary Clinton that it had created with the support of corporate donors without disclosing its donors. The BCRA prohibited corporations and unions from using funds from their general treasuries to fund "electioneering communication" (e.g., political advertisement) within 30 days before a primary or 60 days before a general election and required that donors who funded this type of advertisement be disclosed.[4] Citizens United had been prevented from advertising the documentary as it wished due to these provisions of the BCRA, so Citizens United 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, while the court upheld that provisions of the BCRA requiring the disclosure of the funder were still valid for for-profit corporations and unions, the requirement that "social welfare" non-profits, like Citizens United, disclose their donors were unconstitutional.[5] 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 these state-level bans were also unconstitutional. It is worth noting that most states had enacted these bans a long time prior to *Citizens United*. The median year of passage was 1978, thus the enactment of individual state-level bans were not caused or affected by the BCRA rules themselves. Figure 1 Panel A shows which states were affected.

[3] There still exist 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.

[4] 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).

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

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).[6] 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. This is shown in Figure 1 Panel C. This share has only decreased since *Citizens United*, and following the decision, union outside political contributions have comprised roughly 5% or less. Thus, while technically *Citizens United* was a shock to both corporate and union spending, for the sake of this paper we focus primarily on the corporate aspect.

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, and 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.[7] Additionally, for political variables (e.g., advertising spending or the identity of the governor) we collapse the data into two-year election-cycle time periods, while for economic variables, we analyze the data on an annual basis. Table 1 Panel A provides

[6] Technically, the rules establishing Super PACs were formalized after a DC Circuit appeals case, *Speechnow.org v FEC*, however this case effectively formalized the legal ruling put out in *Citizens United*.

[7] 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 number of observations in subsequent tables.

summary statistics on the variables described below.

2.2.1 Political Variables

Independent expenditure bans: We identify those states that had bans on corporate and/or union independent expenditures that were overturned by *Citizens United* using the information provided by the National Conference of State Legislatures.[8] Panel (a) of Figure 1 presents a map that shows 23 states that had those bans overturned.[9] Those states that have had a ban on independent political expenditures were "treated" by the *Citizens United* decision, while those states that did not have a ban serve as control states.

Party control and elections: We hand collect state-year level data on the party which controls the governorship seat, the lower legislative chamber (typically the state house of representatives), and the upper legislative chamber (typically the state senate) from several sources: National Conference of State Legislatures, states' election websites, and Wikipedia. In a given state-year, the likelihood that republicans control the governor seat is 56%, and the upper/lower legislative chamber is 55% and 52%/51%, respectively (see Table 1 Panel A). As we discuss in greater detail below, there do not seem to be any obvious political patterns to the states that did or did not have a ban, and practically all bans were passed decades before *Citizens United*.

Polarization: We use political polarization measures of state's legislative chambers estimated by Shor and McCarty (2011).[10] 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 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 are reflective of conservative ideology, while negative ones of liberal ideology. As shown in Table 1 Panel A, the average state house and senate in the period has slightly republican tilt, but not significantly so. However, within parties, the average democrat score in both chambers is slightly more partisan.

Independent political expenditures: Many states do not have disclosure requirements

[8] 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.

[9] 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.

[10] 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).

for independent political expenditures. To show that *Citizens United* affected independent political expenditures, we collect data on independent expenditures in federal elections from 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.[11]

Political advertising: We obtain data on political advertising from Ad$pender. Ad$pender tracks advertising expenditures across media (e.g., TV, Magazines, Internet advertising, and others)[12], topics, media markets, and years. Ad$pender reports data at the media market level, which corresponds approximately to a city or MSA. We aggregate market-level political ad spending to the state level. Note that not all states contain a media market, and 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 level, data on state gross domestic product (GDP), further disaggregated into employee compensation and gross operating surplus.[13] 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) between 2005 and 2018. The SOI reports, at the aggregated zip-year level, various components of taxable income, including among other categories, 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 capital income as AGI less salary and wage income; we proxy labor income with 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

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

[12] Television, including network, cable, spot, Spanish-language network, and syndicated; Radio, including network, national spot, and local; Magazines, including consumer, business-to-business, local, Sunday, and Spanish-language; Newspapers, including national, local, and Spanish-language; Internet; Outdoor (e.g., billboards).

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

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, e.g., 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 itself 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 a 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 of tests rely on aggreaged economic data in order to ensure that we are capturing the effect of *Citizens United* on both public and private firms, we use data on publicly traded firms from Compustat for some cross-sectional tests. We obtain data on employment, size, leverage, cash, and Tobin's q. We compliment these data with historical headquarters data from Loughran/MacDonald database.[14]

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 comes from the organization's Violation Tracer database which contains enforcement actions from both federal and state enforcement agencies on topics related primarily to banking, consumer protection, environmental, wage and hour violations, unfair labor practice, health and safety, and workplace discrimination. Subsidy data comes 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

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

level of state minimum wage in our sample period was $6.70, and the average annual growth in 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.

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 FHFA. We obtain mortgage delinquencies from Corelogic LLMA.

2.3 Empirical Strategy

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

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

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 that was overturned by the court decision 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.[15] 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 analysis.

We also use standard event-study analysis to estimate the effect of *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 the changes in the outcome in treated and 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 changes after the *Citizens United*

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

decision.

The underlying assumption of our specification is that the treated and the 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 states that had bans overturned by *Citizens United* to those that did not 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—treated and control—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 share of voters for Obama in the 2008 presidential elections. However, these bans may predominately have been found in Democratic states (that might have favored such regulations) as measured by the low 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 control in all specifications dynamically for the party of the state's governor holding this position right before the Supreme Court case. In practice, however, the addition of this control have no impact on our results. While we cannot fully refute this potential concern, we provide evidence that the two groups of states are fairly comparable.

Additionally, treated and control states differ on 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 for households to be 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.

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 capital 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.

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.[16] Our first goal of these analyses is to show that *Citizens United* resulted in increased political spending. Our second goal is to understand whether removal of campaign finance restrictions caused by *Citizens United* primarily benefited incumbent politicians and political interests by entrenching their interests or if the event served as a catalyst for broader set of interests, including new political participants, to begin spending money in politics and help elect new politicians.

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.[17] However, some legal experts argued that even before the *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 entrants to spend money in politics. For example, Bradley A. Smith, an FEC commissioner from 2000–2005, wrote the following following the Supreme Court decision.[18]

> *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—and every incorporated mom-and-pop falafel joint, local firefighters' union, and environmental group—to make its voice heard in campaigns without hiring an army of lawyers or asking the FEC how it may speak.*

The analysis in this section allows us to examine whether *Citizens United* primarily bene-

[16] We are not the first to study the political consequences of *Citizens United*, 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.

[17] A prominent example can be found in Barack Obama's 2010 State of the Union address, when he explicitly spoke against the the court decision. "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—including foreign corporations—to spend without limit in our elections. I don't think American elections should be bankrolled by America's most powerful interests, or worse, by foreign entities. They should be decided by the American people." https://obamawhitehouse.archives.gov/the-press-office/remarks-president-state-union-address

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

fited incumbent political interests or prompted new interests to spend money in politics and increase political competition.

3.1 Political Spending

We begin by showing that *Citizens United* led to an increase in political spending. We begin by plotting the spending by Federal Super PACs by election cycles[19] in panel B of Figure 1.[20] 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 late in the 2010 election cycle. The case's transformative effect took place starting after 2010, with the amount of Super PAC spending reaching over $600 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 contributions made independently from 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), 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.[21] Table 2 Panel A shows that political advertising increased in states treated by *Citizens United* compared to the control states following the decision. More specifically, political advertising in treated states increased by 30% in political cycles after 2010. 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 increases after *Citizens United*. Unfortunately, the individuals or corporations who funded this increased spending is largely unknown since *Citizens United* allowed for new forms of

[19] For much of this analysis, we group years into two-year election cycles that correspond to the natural pace of election spending.

[20] 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*.

[21] These political advertising data includes all types of political advertising spending since it is not possible to separately identify political advertising by political campaigns (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.

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 the 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 coming from labor or businesses. 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 small donors or those donors who are likely to be infrequent 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 labelled "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 ten-percent level. The increase in direct political spending suggests that direct and independent political spending are complements and makes it 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 court case have positive point estimates ranging from 0.17–0.96, with the four of these specifications being statistically significant at the five- or one-percent 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 36.13% of total contributions. The fact that business groups, labor groups, lobbyists and, in particular, likely first-time contributors (proxied by small size donations included in the "Unitemized Contributions" category) all increased their political activity suggests that the net effect of *Citizens United* on politically spending was not an increase in the political spending of 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 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 alternatively have increased political competition. We study these two possibilities by examining whether the probability of turnover in the Governor's political party changes, as well as whether the proportion of new politicians changes in treated states after *Citizens United*.[22]

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.[23] As shown in Panel A of the figure, the probability that the control of 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. Importantly, the figure shows no pre-trends in political turnover, supporting 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

[22] Klumpp et al. (2016) find that the reelection rates of Republicans in the state houses increases, but do not study in detail the how the composition of incumbent and new politicians change.

[23] We examine whether *party* changes rather than *individual* with the idea governorships are often "passed down" within party and that shifts from one, e.g., Republican governor to the next Republican governor are not politically meaningful. Rather, shifts across party are more likely to represent more fundamental political change.

were more likely to transition to Republican control and vice versa) in treated states after the court ruling on the order of roughly 25 percentage points in 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 transitioning from republican to democrat and 24.8 percentage points in the likelihood of 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 ten-percent level. Column (2) of Panel A presents the results of a regression that estimates directly the probability of there being a Republican governor in power. We find a small, but not statistically significant increase in treated states of 4.5% after *Citizens United*, largely consistent with the idea that the decision increased turnover in treated states, and more treated states were ex-ante controlled by democratic governors. 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 the state House of Representatives and state Senate changes after *Citizens United*.[24] 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).[25] 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 porportion 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 though 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

[24] 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.

[25] 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.

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 more broadly defined. Indeed, our results in this section that political activity across both liberal and conservative groups broadly increased, and that governorships were more likely to turnover both from Democratic to Republican *and* 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 the 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. Broadly, these results provide evidence that increased money in politics likely resulted in higher electoral competitiveness.

4 Economic Consequences of Money in Politics

We now turn to our main question of interest: how does increased political spending affect economic outcomes for capital and labor? As we discussed in Section 3.1, we find that, while liberal and conservative super PAC spending both increased, conservative spending increased relatively more. Additionally, beyond the traditional conservative/liberal dichotomy, the share of outside political money advocating for pro-labor causes decreased from roughly 12% to roughly 4% after *Citizens United*. Moreover, Section 3.2 shows that the increased money appears to have had a political effect: political turnover increases in treated states.

As discussed previously, the net economic effect on various factors of production is ambiguous. On one hand, increased political influence of capital could lead to transfers to capital at the expense of labor. On the other hand, by increasing political competition, state governments may be persuaded to adopt policies that lead to greater economic growth rather than policies that benefit a narrower set of interests. The benefits of this increased economic growth can then flow to both labor and capital.[26] In this section, we examine the impact of

[26]Later, we consider whether *Citizens United*, in also overturning restrictions on unions' ability to engage

Citizens United on economic growth and factor incomes in order to address this question.

4.1 Baseline Results

We examine how economic outcomes to labor and capital change after *Citizens United* using data from the BEA and the IRS. The BEA data measures state-level aggregate income as well as payments to labor and capital though various aggregations and imputations. The IRS data does 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 data sets, 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 adjusted gross 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 AGI less 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 are effective payments to capital providers. Finally, labor share is measured as the respective labor income measure divided by the respective total income measure. All subsequent analysis 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 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 show that capital income—noisily measured—increases between 2 and 3.5 percentage points. Column (3) in both panels show that labor income increases by a 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 magnitude that *Citizens United* might have had on payments to labor or capital, 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

in political advertising, made unions better able to advocate for favorable labor policies. We examine and rule out this channel in subsequent sections.

were given to politically connected firms from 2001–2012.[27]

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, with relatively little evidence of a strong pre-trend. 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. Given that there do not appear to be substantial pre-trends, this suggests that the change can be attributed to the *Citizens United* ruling, rather than a latent trend that happened to effect states that had enacted political spending bans and coincided with *Citizens United*.

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, we find little evidence that labor share changed in Panels G and H: 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 and while 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 returns to capital providers.

4.2 Robustness Checks and Alternate Specifications

We undertake several robustness checks around our main economic results. Beyond using both BEA and IRS data, we also use the US Census' QWI database, which does not only have 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 outcomes. 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 much of these earnings increases are driven by new hires on the extensive margin. Finally, aggregate payroll, shown in Column (4), increases by 4.6 percentage points. The event studies confirm an overall lack of pre-trends following by a sharp increase following the decision.

Beyond serving as a robustness check, the employment and earnings results from the QWI

[27] Outside of the US, 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.

shed light on the equilibrium economic mechanism. In short, our findings are consistent with *Citizens United* leading to increased labor productivity: to the extent that political outcomes from the decision remove legal or compliance costs, or increased demand for firm products more generally, firms' demand for labor should increase. As firm labor demand shifts outwards along an upward-sloping labor supply curve, prices (earnings) and quantites (employment) increase, with the allocation to price and quantity increases depending on the labor supply elasticity.

Next, 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 in to 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 A5 Panel A shows that results are not driven only by crisis-related industries like real estate or finance, and instead are broad-based and include, e.g., mining, manufacturing, wholesale trade, and many others.

Finally, 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 controls and shows that the samples do not differ from one another in any statistically significant way. Panels B and C show the BEA and IRS economic outcomes, which are nearly identical to the baseline specification. Figure A2 replicates Figure 3 with the matching approach and delivers very similar results, with, if anything, stronger effects and less observable pre-trends. In summary, our results appear to show increases in economic growth and particularly labor income that are robust to different sources of data, 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 treated states. These increases come both from traditionally well-connected groups such as business and labor associations, but also from 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 increased political competition. Third, our economic results suggest that overall growth increased in states affected by the court ruling and, in particular, payments to labor increased. These results

are robust to alternative measurements, a robust set of dynamic controls, and alternate empirical specifications.

In this section we provide evidence of a mechanism and attempt to rule out several alternatives. Our main hypothesis is that *Citizens United* changed the political-economic equilibrium by lowering barriers to political entry. Briefly, it is easier to exert political influence through dollar donations than it is by building and cultivating social ties, revolving door arrangements, and other "soft," "backroom" forms of influence on politicians. These lower barriers to political activism thus increased political competition, leading to the adoption of policies with broader, pro-growth benefits rather than rent-seeking policies that benefit a narrower set of interests. These pro-growth policies increased the "pie" available to split between labor and capital, and increased economic outcomes for both groups, rather than increasing rents to interest groups that were already politically powerful.

Beyond offering evidence in support of this hypothesis, we consider (and reject) two alternative explanations for our main results. The first alternative is that since *Citizens United also* removed restrictions on unions' ability to engage in political advocacy in some states, 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 ruling out these alternatives.

5.1 Political Competition and Pro-Growth Economic Policies

Our primary explanation for our 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 in hiring labor, 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. For example, moderate incumbents could be replaced with newly elected partisans with

extreme social or economic views. Increased polarization could impact the ease of passing new legislation or the types of bills that are introduced. More specifically, if the new politicians are more polarized they might attempt to enact policies that are more extreme in nature such as focusing on passing wedge social issues that appeal to the ideological extreme of their parties. Alternatively, if the newly elected politicians were more centrist in nature, policy making could be more focused on issues that are less partisan in nature, such as reducing the regulatory burden on small businesses.

We measure polarization of a state legislative chamber using the 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 the 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 ideologies. Thus, we capture only the extensive margin of ideology drift; ideology could be changing even more as politicians change their preferences.

Figure 4 and Table A4 examines how state-level political polarization changes after *Citizens United*. In Figure 4, 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* see a sharp decrease in ideological distance in the first election cycle following the decision. The drop is 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 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 similarily 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, *Cit-*

izens 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 court case to respond the most.

We begin by examining how labor-related outcomes responded to *Citizens United* across different sectors. Panel A of Table A5 presents results for (log) employment, payroll, and earnings 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 grow 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 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 ten-percent 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 political actors following *Citizens United*.[28]

We next examine whether employment, payroll, or earnings responded 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[29] and test whether the labor and economic response to *Citizens United* was stronger in those industries. Panel B of Table A5 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 do not exhibit consistent signs and are not statistically significant at conventional levels, while the main effects are generally in line with the estimates presented in Panel A.

Next, we examine how labor responses vary by firm size and age in the QWI data. We regard both size and age as proxies of ex-ante political connectedness. Our hypothesis that *Citizens United* expanded the set of politically engaged firms suggests that it should be young firms in particular—those that have not existed long enough to build other political connections—that should be most affected by the decision. On the other hand, 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 5 and Table 5.

We begin with firm size. Panels A, C, and E of Figure 5 show that (log) employment,

[28] Moreover, the fact that differences are not concentrated in financial-crisis related industries like real estate or finance further alleviates identification concerns that the results are driven by spurious crisis-related correlations.

[29] 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.

earnings, and payroll increase at roughly similar rates for both small (fewer than 50 employees) and larger firms, and Table 5 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 5 Panel B, and Table 5 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 5 Panel D shows that workers at younger firms see 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 5 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[30], but particularly more so among young firms that were less likely to be politically connected ex-ante.[31] Thus, our findings underline our primary hypothesis: that post-*Citizens United* policies that represent a broader set of constituent interests.

Our analysis so far has focused on economic outcomes using aggregated state-year or state-industry-year data. The advantage of these data is that it allows 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 political connectedness at a 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.[32] We focus on employment because while employment data is well populated for the sample of Compustat firms, payroll information is most often missing in Compustat. For these firms, we measure political activity in several ways: whether or not a firm made campaign contributions to a Federal Political Action Committee in the political cycles over 2004–2010; whether or not a firm in the S&P index made political contributions to state politicians in the political cycles over 2004–2010; and whether or not a firm had above median total assets in 2010 as a proxy for size.[33] We classify firms, that have made such political donations or are

[30] 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.

[31] 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 ensure *Citizens United*.

[32] 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 financials.

[33] 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

large, as ex-ante connected.[34] 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 6 and Table 6.[35] Panel A of Figure 6 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 6 the pooled effect is only statistically significant at the ten-percent 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 6 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. Panel D of Figure 6 suggests that smaller (public firms) firms respond more than larger firms. Indeed, as confirmed in Column (4) of Table 6, smaller firms' employment in treatment states increases by 9.8% relative to those in control states, which was largely offset in large firms.[36]

Collectively, this section's results 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, and not just those industries with ex-ante political connections. If anything, smaller, younger, and firms with less ex-ante political connections see 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.

5.1.3 Changes in Policies

We finally examine whether business conditions became more favorable for firms by examining whether the enforcement of regulations became less stringent and whether tax rates

are more likely to be politically active (e.g., Cooper et al., 2010).

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

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

[36] We note that these results 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.

changed in states affected by *Citizens United*.

Regulatory enforcement: We begin by examining regulatory enforcements. As we show in Section 3.2, we find evidence that turnover in the executive branch was particularly pronounced 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 compiled by *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 topics related primarily to banking, consumer protection, environmental, wage and labor violations, unfair labor practice, health and safety, and workplace discrimination.

We examine whether the number of state-level and federal enforcements changes 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 that are related to consumers or employees. 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 receive. More specifically, there are many areas of regulation in which there is an overlap in federal and state jurisdiction. 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, out outcome variable is log of 1 plus the number of enforcement actions.

Panels A and B of Figure 7 present the results of our analysis. Panel A shows the total number of state-level enforcement actions whose primary offense type is relating to violations against labor and consumers (red) and capital (blue).[37] We find that enforcement actions around laws protecting labor and consumers fall significantly in treated states following Citizens United. In contrast to state-level results, Figure 7 Panel B, which examines enforcement actions at the federal level, shows that federal enforcement activity did not exhibit any sort of change before or after *Citizens United* in treatment states relative to control states.

Eased enforcement appears primarily concentrated among labor- and consumer-protection

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

laws, as opposed to laws specifically oriented around 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 around investor protection, which primarily concern 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 related to capital protection. Table 7 Panel A quantifies these result in the difference-in-difference framework and shows that state-level labor and consumer-related enforcement actions decrease by roughly 50% in treated states following *Citizens United* (column (2)), while state-level enforcement actions related to capital protection, and federal enforcement actions, did not change (columns (3)–(6)). These tests suggest that while firms did not change their overall behavior, the state regulatory environment became more favorable to firms following *Citizens United*.

The results for state- but not for federal-level enforcement suggests that what changed was enforcement patters as opposed to underlying firm behavior: if firms were committing fewer violations, one would expect 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 rate, cancer deaths, and denial rate of 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 towards 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 are coming 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 (and recall that our earlier results show that financial payments to employees increase).

Tax changes: We finally 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 change differentially in treatment and control firms after *Citizens United*. Table 7 presents empirical results for the level of the various tax rates. We find negative point estimates for all of the 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 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 and 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 overturn bans on corporate independent expenditures, a number of states had previously enacted bans on union independent expenditures that were also overturned. 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*. Nevertheless, for completeness, we examine whether the increased union political power is 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 a 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 ban*. If increased union power were a factor in the 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 A6, 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 treatment states.

Additionally, in unreported results, we formally analyze differences between states with

(a) corporate bans and (b) corporate *and* union bans by considering treatment 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 treatment and control states, but unions only gain power in treatment states. Thus, unions have relatively more power in treatment 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, increased payments to labor in treated states after *Citizens United*, are 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 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 7 Panel C.[38] We examine potential changes in minimum wages using two different outcome variables: the dollar level of the minimum wage and the percent 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 investment when they were able to spend more money in politics, and increased employment as a result.

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

5.3.1 State Fiscal Spending

We begin by studying whether states that were effected by *Citizens United* substantially increased their 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 8 Panel A. The first panel presents estimates for total government revenues and various subcategories while the second panel presents estimates for government expenditures and various subcategories. The dependent variable is the log of one plus the expenditure or revenue amount. Results are qualitatively similar if we use level of revenue or expenditure divided by state-level GDP in 2008. Columns (1) and (2) present coefficients and t-statistics for each regression. The column labeled "pct" provides the percent of total revenue or expenditure for each category 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 13.5% of state revenues. On the expenditure side, we find that capital outlay do increase by 15% (row 21), which is statistically significant at 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.[39] We also find that salaries and wages related to highways also increase (row 30), although this category only account 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.[40]

5.3.2 State Subsidies

Finally, 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

[39]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."

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

government grants, tax incentives, and cost reimbursements. We examine whether the log of one plus the number or dollar-value of state subsidies changes differentially in treated states after *Citizens United*. Table 8 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. We find that there are generally positive point estimates on the Post×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 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 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 overturned 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 BE) and the IRS, we first find that output increased by roughly three percent in affected states. Labor income increased between three to four percent for up to six years following the event, and increases in capital income were economically significant 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 treatment 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*.

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 firms changing 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 significance, our 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 publically traded firms, we find no evidence that firms that were known to have been politically active 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 union labor power or increased 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) to 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 treatment 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 and capital. Our results suggest that the economic outcomes of political choices are not necessarily zero-sum, and that increasing ease of political engagement can bring a broader set of interests to the table, which itself can benefit the interests of labor. 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. 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 is socially improving 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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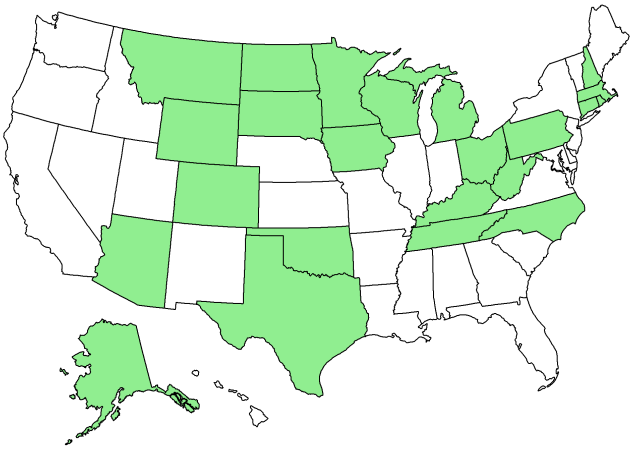
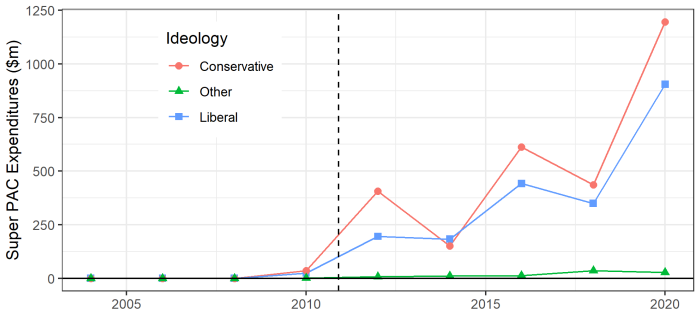
Figure 1: Citizens United and Political Expenditures

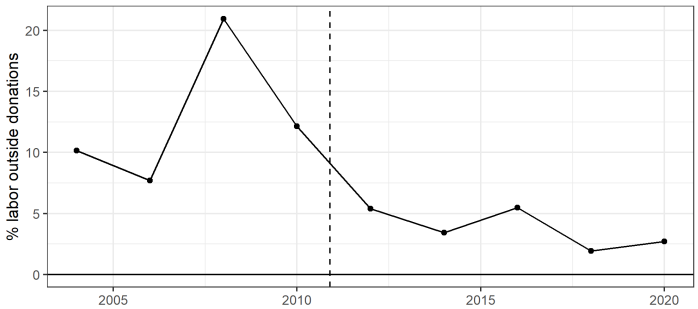
Note: In this figure, Panel (a) shows in green which states banned corporate (including non-profits) or union independent political expenditures before *Citizens United*—the bans that were overturned by the court decision. Panel (b) shows total Super PAC spending in Federal elections, in millions of 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 includes but is 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*



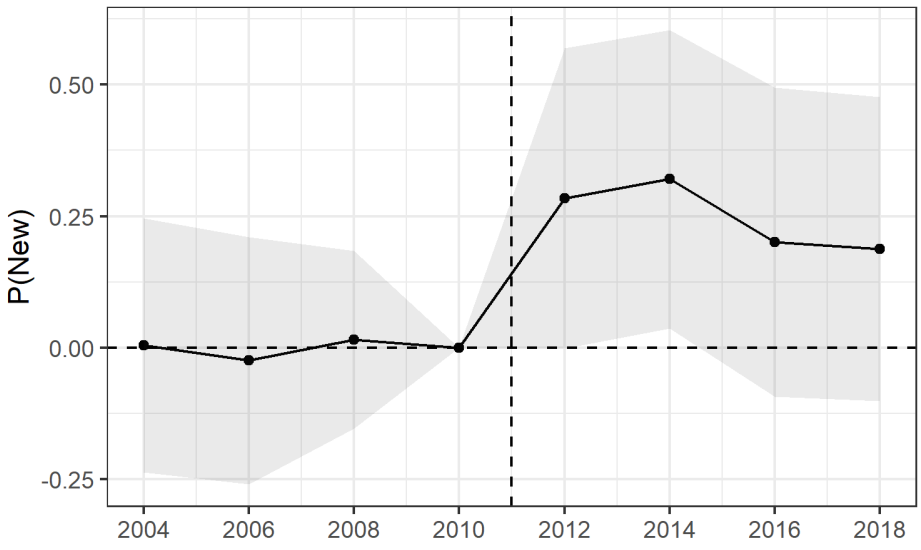
(b) Super PAC Spending



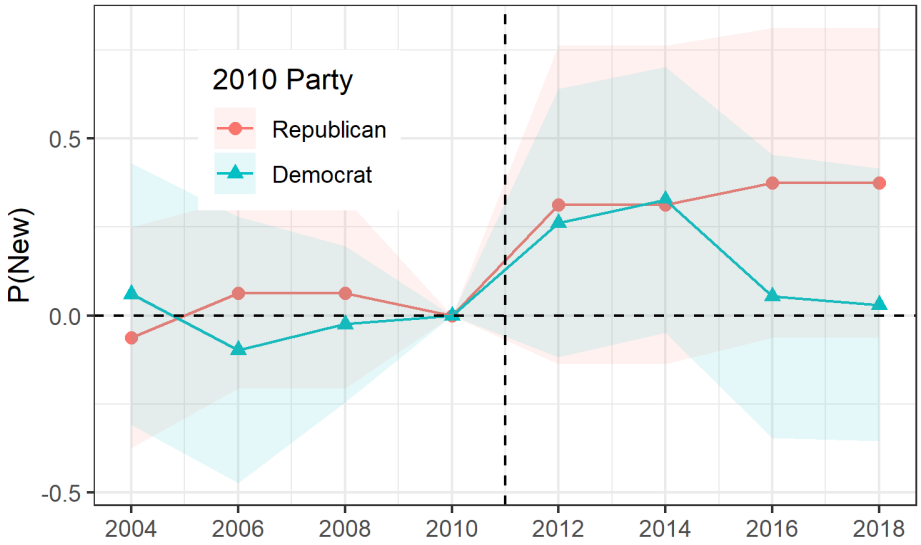
(c) Labor share of outside political spending

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 with bans on corporate or union independent political expenditures pre-*Citizens United*—the bans that were overturned by the court decision. 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.



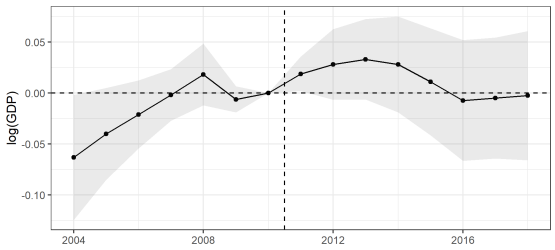
(a) All governors



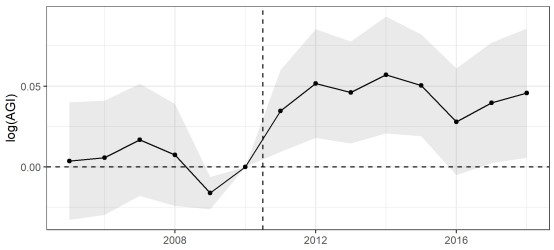
(b) Governor by party before *Citizens United*

Figure 3: Total and Factor Incomes

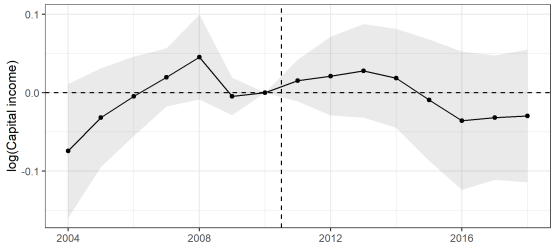
Note: This figure shows changes in state-level economic outcomes around *Citizens United*. States affected by the *Citizens United* case (treated states) are those with bans on corporate or union independent political expenditures pre-*Citizens United*—the bans that were overturned by the court decision. 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.



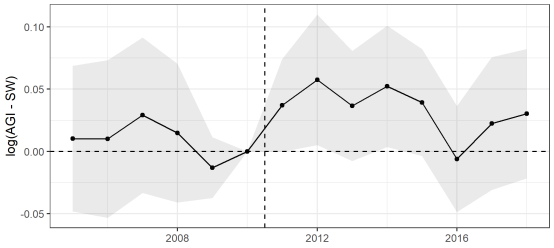
(a) BEA: GDP



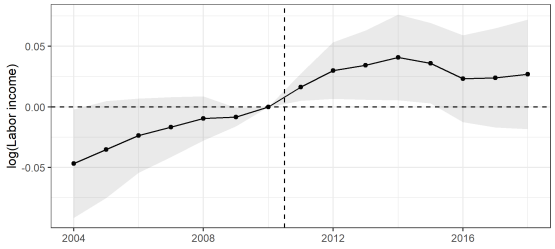
(b) IRS: AGI



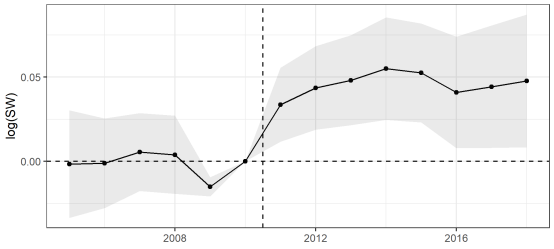
(c) BEA: Capital income



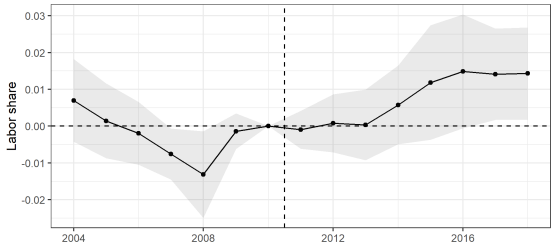
(d) IRS: Non-SW income



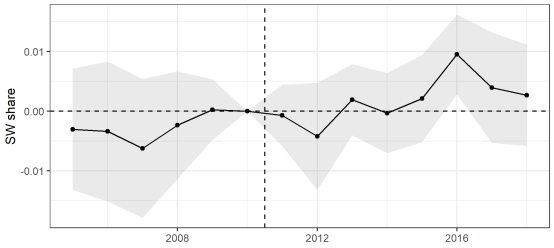
(e) BEA: Labor income



(f) IRS: SW income



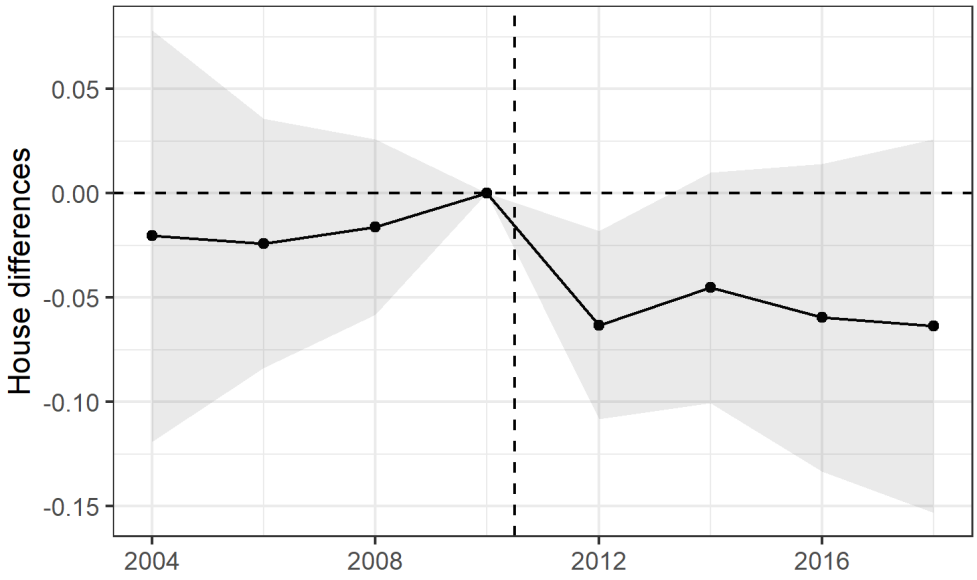
(g) BEA: Labor share



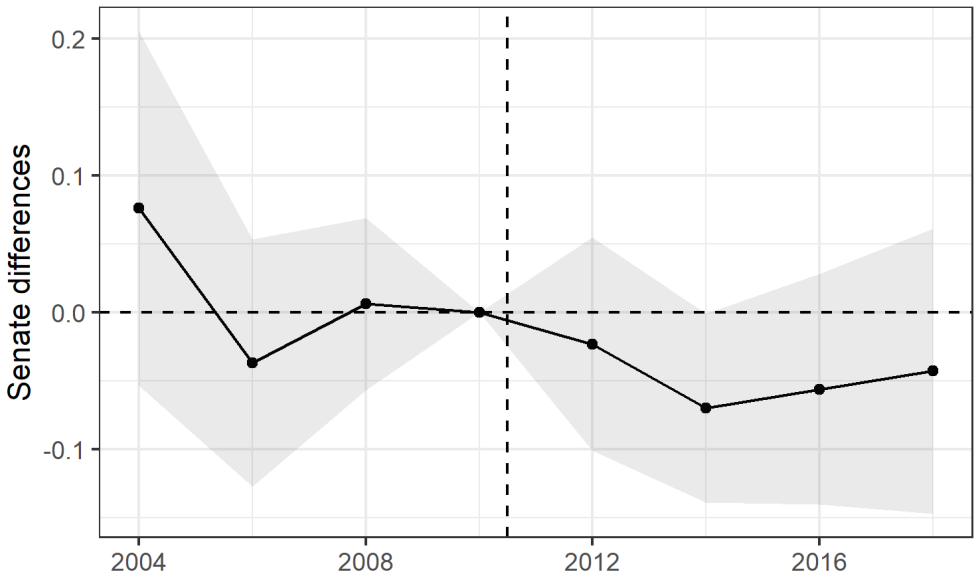
(h) IRS: SW share

Figure 4: Political Polarization

Note: This figure shows changes in political polarization around *Citizens United*. States affected by the *Citizens United* case (treated states) are those with bans on corporate or union independent political expenditures pre-*Citizens United*—the bans that were overturned by the court decision. 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)) and the upper state house or 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

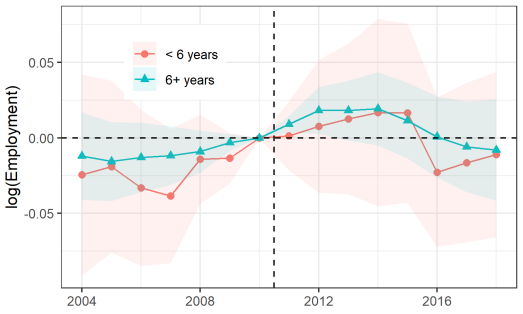


(b) Mean State Senate distance

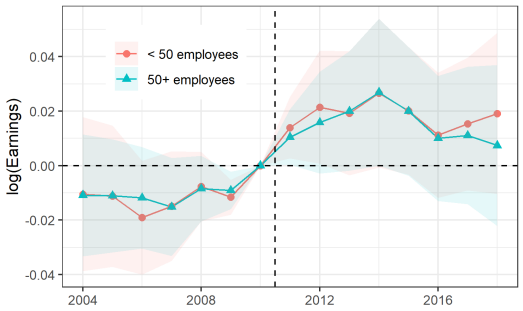
Figure 5: Economic outcomes by firm size and age using QWI data

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 with bans on corporate or union independent political expenditures pre-*Citizens United*—the bans that were overturned by the court decision. 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.

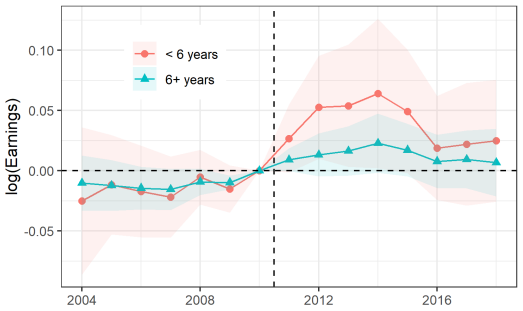
(a) Employment by firm size



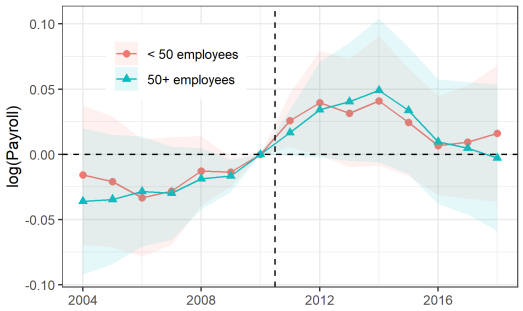
(b) Employment by firm age



(c) Earnings by firm size



(d) Earnings by firm age

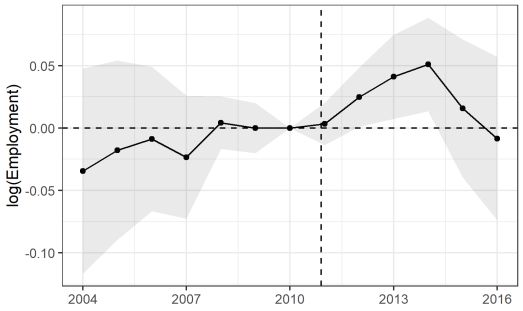


(e) Payroll by firm size

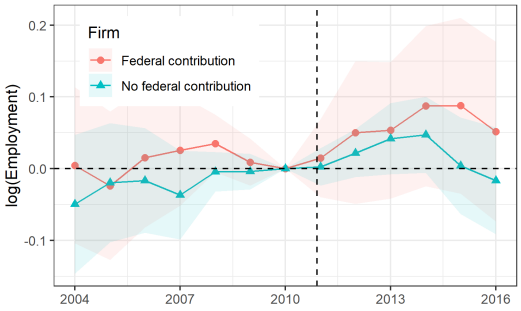
(f) Payroll by firm age

Figure 6: Firm-level employment by Ex-Ante political activity

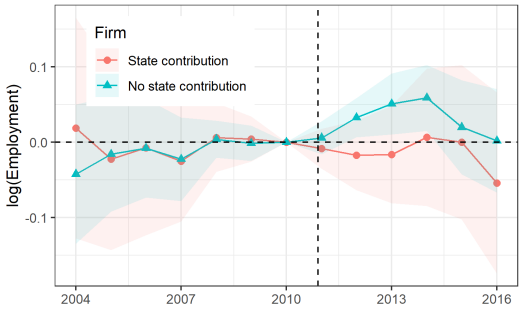
Note: This figure shows changes in firm-level employment around *Citizens United* by ex-ante firm political activity. States affected by the *Citizens United* case (treated states) are those with bans on corporate or union independent political expenditures pre-*Citizens United*—the bans that were overturned by the court decision. 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 or not they made campaign contributions to Federal election candidates in the 2004, 2006, 2008, or 2010 election cycles. Panel (c) divides firms by whether or not 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 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.



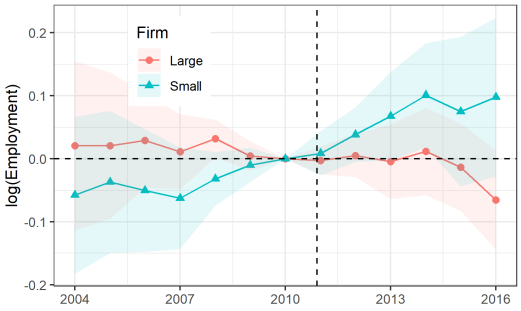
(a) All firms



(b) Active (federal contributions)



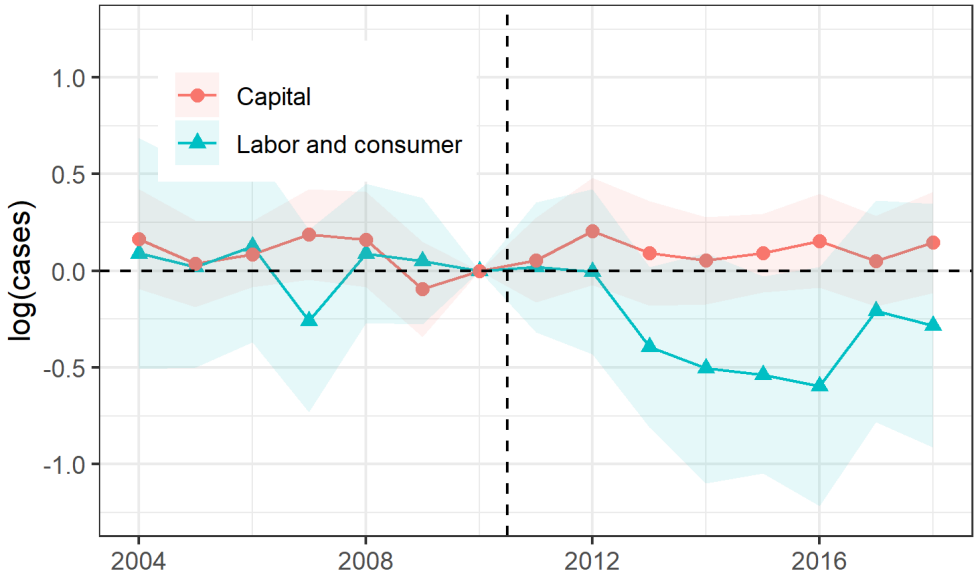
(c) Active (state contributions)



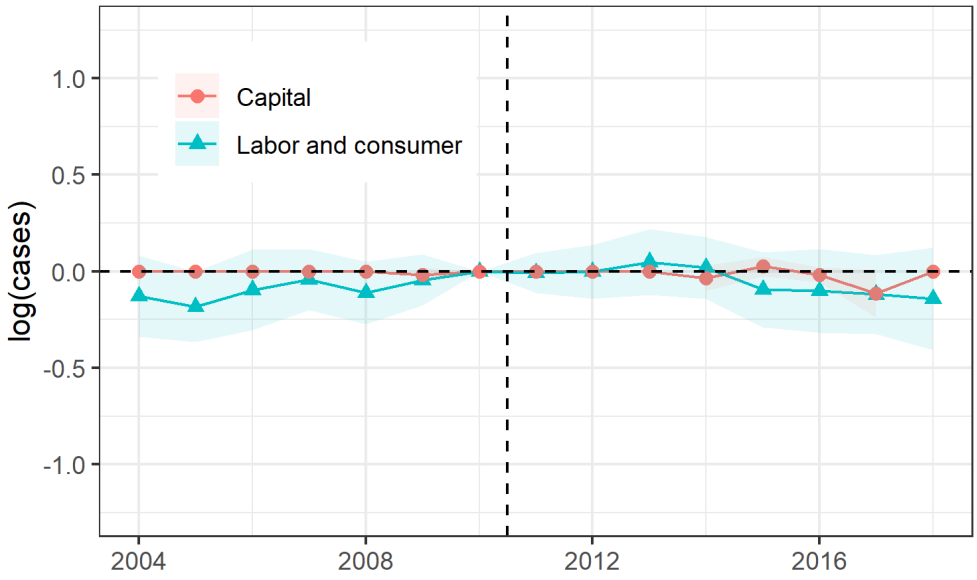
(d) Firm size

Figure 7: 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 with bans on corporate or union independent political expenditures pre-*Citizens United*—the bans that were overturned by the court decision. 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 1+ the number of enforcement actions by state-level government of labor and consumer protection laws (blue) and capital protection laws (red). We use $log(1 + Y)$ because the data include many 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 ($B, IRS)	700	124.71	150.40	29.62	73.91	160.33
Alignment (House dem)	708	−0.83	0.37	−1.09	−0.87	−0.58
Alignment (House rep)	708	0.74	0.34	0.62	0.81	0.95
Alignment (Sen dem)	713	−0.82	0.38	−1.09	−0.84	−0.53
Alignment (Sen rep)	713	0.74	0.32	0.55	0.78	0.96
House differences	708	1.56	0.49	1.23	1.50	1.87
Senate differences	713	1.53	0.49	1.17	1.53	1.83
Ad spending ($M)	570	15.76	25.50	0.86	6.41	18.23
Violations (aggregate)	750	23.74	81.16	2	4	11
Violations (labor and consumer)	750	21.30	80.06	1	3	8.8
Violations (capital)	750	1.20	0.87	1	1	1

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 with bans on corporate or union independent political expenditures pre-*Citizens United*—the bans that were overturned by the court decision. *Post* is an indicator for 2011 or later (after *Citizens United* case). This table shows the results from regressions estimated using Equation (1) where the dependent variables are political outcomes at the state-year level. 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 ten-, five-, and one-percent 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
Note:	*p<0.1; **p<0.05; ***p<0.01

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 with bans on corporate or union independent political expenditures pre-*Citizens United*—the bans that were overturned by the court decision. This table shows the results from regressions estimated using Equation (1) where the dependent variables are political turnover outcomes at the state-year level. All columns include state and year times 2010 governor party fixed effects. Data run from 2007 through 2015 and are bucketed into political cycle (two-year) frequency. *Post* is an indicator for whether the year is 2011 or later (after *Citizens United* case). Panel A shows outcomes for governors. 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 ten-, five-, and one-percent levels respectively.

Panel A: Political turnover of governors

	Dependent variable:			
	New governor party vs. 2010	Republican governor	New given R	New given 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 with bans on corporate or union independent political expenditures pre-*Citizens United*—the bans that were overturned by the court decision. 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 *Citizens United* Case). 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 ten-, five-, and one-percent 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 firm size and age using QWI data

Note: This table shows changes in state-level total employment, (average) earnings, and total payroll aggregated by firm age and size around *Citizens United*. States affected by the *Citizens United* case (treated states) are those with bans on corporate or union independent political expenditures pre-*Citizens United*—the bans that were overturned by the court decision. This table shows the results from regressions estimated using Equation (1) where the dependent variables are economic outcomes at the state-year level interacted with firm characteristics. *Post* is an indicator for whether the year is 2011 or later (after *Citizens United* case). 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 ten-, five-, and one-percent 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 × Age 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 6: Firm-level employment

Note: This table 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 with bans on corporate or union independent political expenditures pre-*Citizens United*—the bans that were overturned by the court decision. 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 whether the year is 2011 or later (after *Citizens United* case). 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 and made campaign contributions to state candidates in the 2004, 2006, 2008, or 2010 election cycles 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 and the The National Institute on Money in Politics. 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 ten-, five-, and one-percent levels respectively.

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

Table 7: Policy Responses: Enforcement actions, Tax rates, and Minimum wage

Note: This table shows policy changes in enforcement actions, state-level taxes, and state-level minimum wages around *Citizens United*. States affected by the *Citizens United* case (treated states) are those with bans on corporate or union independent political expenditures pre-*Citizens United*—the bans that were overturned by the court decision. This table shows the results from regressions estimated using Equation (1) where the dependent variables are outcomes of the state level policies. *Post* is an indicator for whether the year is 2011 or later (after *Citizens United* case). Panel A shows enforcement actions brought against corporations within each state. Columns (1)–(3) are the number of violations enforced by state government agencies. Columns (4)–(6) are the number of violations enforced by federal government agencies. Columns (1) and (4) are all types of enforcement actions; (2) and (5) are enforcement actions brought to enforce labor or consumer rights; (3) and (6) are 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 the results for state tax rates with data from Baker et al. (2021). Panel C 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 ten-, five-, and one-percent 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: 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 C: 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 8: 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. *Post* is an indicator for whether the year is 2011 or later (after *Citizens United* Case). Data run from 2007 through 2015. Panel A examines state government revenues and expenditures. Column "pct" shows the revenue (expenditure) coming from each category as a percentage of all revenues (expenditures). Columns (1), (2) and (3) show the regression coefficient, t-value and significance where the outcome is $log(Y + 1)$. Panel B examines state- and local-subsidies to corporations from 2007–2015. Columns (1)–(3) show regressions with the log 1+ number of subsidies as dependent variables. Columns (4)–(6) show regressions with the log 1+ 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 ten-, five-, and one-percent 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 with bans on corporate or union independent political expenditures pre-*Citizens United*—the bans that were overturned by the court decision. 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.

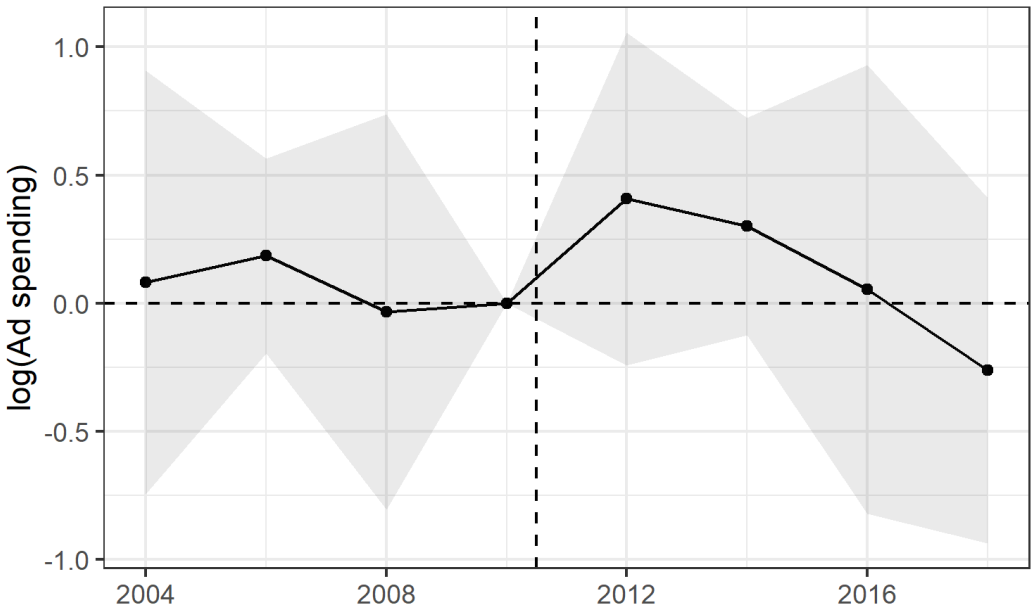


Table A1: Economic outcomes using QWI data

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 with bans on corporate or union independent political expenditures pre-*Citizens United*—the bans that were overturned by the court decision. 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 a zero-one indicator for whether the year is 2011 or later (post *Citizens United* case). 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 ten-, five-, and one-percent 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 with bans on corporate or union independent political expenditures pre-*Citizens United*—the bans that were overturned by the court decision. 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 *Citizens United* Case). 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 ten-, five-, and one-percent 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 in 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 A2 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 the results of 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 ten-, five-, and one-percent 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

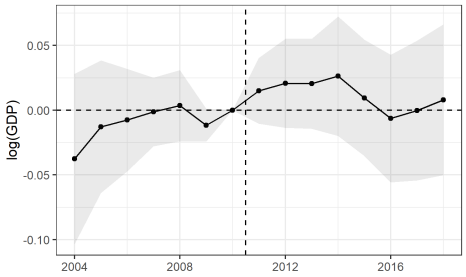
	Dependent variable:			
	log(GDP)	log(Capital income)	log(Labor income)	Labor share
	(1)	(2)	(3)	(4)
Post × Treated	0.021	0.003	0.037**	0.008
	(0.023)	(0.034)	(0.017)	(0.006)
State FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Observations	306	306	306	306
Adjusted R^2	0.998	0.995	0.999	0.868

Panel C: IRS data

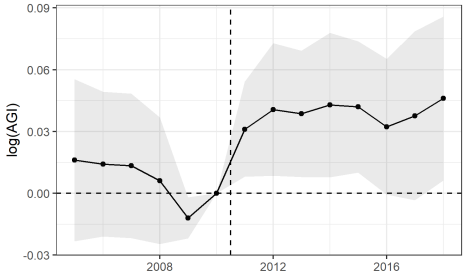
	Dependent variable:			
	log(AGI)	log(AGI - SW)	log(SW)	SW share
	(1)	(2)	(3)	(4)
Post × Treated	0.037**	0.022	0.042***	0.004
	(0.018)	(0.025)	(0.015)	(0.003)
State FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Observations	306	306	306	306
Adjusted R^2	0.999	0.997	0.999	0.923

Figure A2: Economic results with propensity score matching

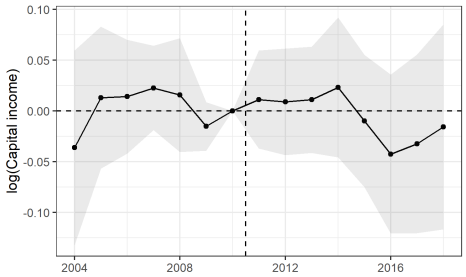
Note: This figure shows changes in economic outcomes around *Citizens United* using a propensity-score matching estimator. Treated and control states are matched on the covariates shown in Panel A of Table A3. Panels (a) and (b) show total income; Panels (c) and (d) show capital income. Panels (e) and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



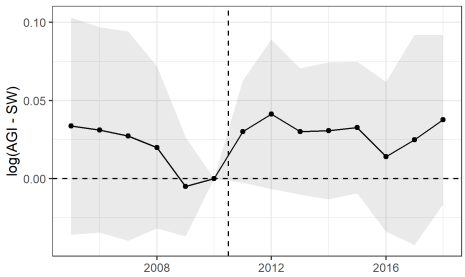
(a) BEA: GDP



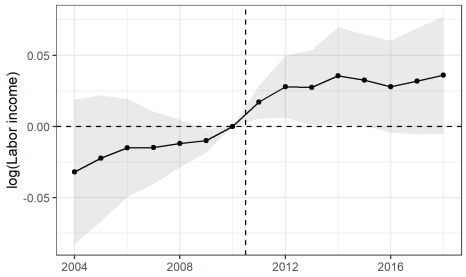
(b) IRS: AGI



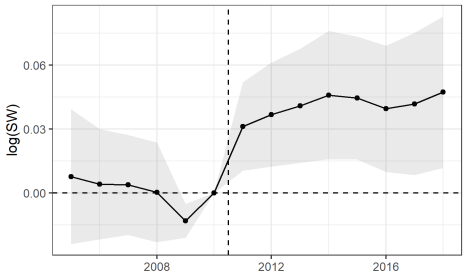
(c) BEA: Capital income



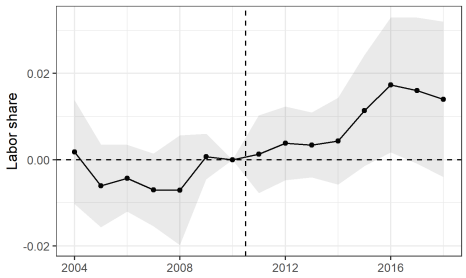
(d) IRS: Non-SW income



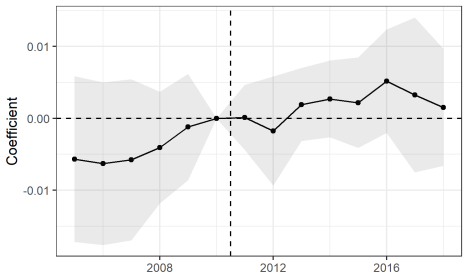
(e) BEA: Labor income



(f) IRS: SW income



(g) BEA: Labor share



(h) IRS: SW share

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 with bans on corporate or union independent political expenditures pre-*Citizens United*—the bans that were overturned by the court decision. This table shows the results from regressions estimated using Equation (1) where the dependent variables are polarization of state-level politicians. *Post* is an indicator for whether the year is 2011 or later (after *Citizens United* Case). 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 (1) ideological ideal point (how often legislators vote with other legislators on a common set of roll calls) and (2) 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 ten-, five-, and one-percent 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: 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 with bans on corporate or union independent political expenditures pre-*Citizens United*—the bans that were overturned by the court decision. This table shows the results from regressions estimated using Equation (1) where the dependent variables are economic outcomes. 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. Post is a zero-one indicator for whether the year is 2011 or later (after *Citizens United* case). 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 indicator, 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 ten-, five-, and one-percent 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 A6: 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 overturned 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 *Citizens United* case). 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 ten-, five-, and one-percent 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