Real Effects of Judicial Selection: The Role of Campaign Finance

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Abstract

This paper studies the real effects of campaign finance and judicial selection. Using exogenous variation in campaign finance laws due to the Supreme Court's surprise verdict in the Citizens United v. FEC case in 2010, I document that relaxing campaign restrictions led to a 67% ($200,000) increase in the average electoral expenditure of judicial candidates and increased competition in State Supreme Court judge elections. The judicial bench also becomes more business-friendly. Since the state courts decide the majority of labor, contract, and administrative law disputes, and the State Supreme Court has the power to set legal precedents, the changes in the judicial bench and competition in expensive elections affect the legal environment and the production choices of firms. I document that labor productivity measured as value added per worker increased by 8% in states with judicial elections. This increase in productivity is mainly driven by sectors that are more exposed to the risk of incomplete contracts and, hence, more reliant on quality legal institutions. These results suggest that removing constraints on campaign finance increases competition and scrutiny in judicial elections and improves productivity in sectors exposed to incomplete contracts risk.

Keywords: Political Finance, Factor Productivity, Money in Politics, Judicial Elections, Contract Enforcement

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Introduction

Campaign finance restrictions on firms, labor unions, and organized interest groups are quite common, e.g., the Bipartisan Campaign Reform Act (BCRA 2002) in the United States and the Political Parties, Elections, and Referendum Act (PPERA 2000) in the United Kingdom. The conventional wisdom in instating such policies is to prevent institutional capture by moneyed interests. Moreover, concerns about the corrupting influence of campaign finance are further elevated if important public officials, such as the judges, are selected through popular elections. The reliance of judges on financing to compete in elections may lead to adverse selection, i.e., corrupt or privately wealthy candidates entering and winning the elections or moral hazard, i.e., judges decide cases to ensure access to financing for future elections. The quality of judges is a key determinant of the quality of legal institutions because judges interpret the law and set precedents. Therefore, judge selection and the quality of legal institutions is important for efficient resource allocation, North et al. (1990), Acemoglu, Gallego and Robinson (2014), La Porta, Shleifer and Vishny (2002). Hence, restrictions on campaign finance will prevent the corruption of the judiciary and improve resource allocation when judges are elected.

On the other hand, campaign finance restrictions may limit judicial candidates' access to finance and constrain the entry of suitable but financially constrained judges. The lack of political competition may weaken the accountability associated with elections where low-performing officials can be voted out. Additionally, in an economy where interest groups form political connections through alternative means such as lobbying and personal acquaintances, campaign finance restrictions may confer an undue advantage to such politically connected agents. Any policy that lowers the cost of forming such connections, for example, easing campaign finance restrictions, will increase competition and improve resource allocation. Therefore, whether less restrictive campaign finance worsens or improves resource allocation is an empirical question.

In this paper, I examine the real effects of campaign finance when judges are elected instead of appointed. I study this in the context of the State Supreme Courts (or High Courts) in the United States.[1] I exploit the heterogeneity in judge selection procedures

[1]High Court and State Supreme Courts, both terms are used interchangeably to refer to the highest

across states where 22 states elect their State Supreme Court judges in popular elections. According to the National Centre for State Courts (NCSC, S. Gibson and Robinson (2023)), 90-95% of all litigation is handled in the state courts, including the majority of contract-related disputes. I restrict attention to the State Supreme Court elections because the State Supreme Court is the highest appellate body within the state's legal system; the State Supreme Court also exerts procedural and budgetary control over the conduct of lower courts and the decisions of the State Supreme Court bench become state law. Therefore, the State Supreme Court strongly influences the legal environment within the state.

To causally examine the effect of campaign finance laws, I exploit the surprise Supreme Court ruling in the Citizens United v. Federal Election Commission (FEC) case in 2010. The decision came in response to an appeal by Citizens United, a conservative non-profit, after a lower court prohibited the broadcast of a documentary attacking Hillary Clinton in 2009. In January 2010, the Supreme Court gave its final decision with 5 judges ruling in favor of and 4 against Citizens United. One salient consequence of the Citizens United ruling was that it rendered restrictions on independent electoral expenditures unconstitutional. Independent expenditures are incurred in support of or against the opponent of a candidate without explicit coordination with the candidate, as opposed to direct expenditures, which come from direct contributions to the candidate's campaign fund. Some states had imposed a ban on independent expenditures. After Citizens United, such bans on independent expenditures were invalidated. Out of the 22 states that rely on judicial elections for the selection of judges to the State Supreme Courts, 11 had instated such bans. I employ a difference-in-differences research design to estimate the political and real effects of campaign finance. I consider the states where the bans on independent expenditure were lifted because of Citizens United as the treated states and states without such bans that remained unaffected as control states.

In the first part of the paper, I examine the effects of campaign finance on expenditures and competition in judicial elections. I document two significant ways in which the Citizens United ruling affected judicial elections. First, I show that when bans on independent expenditure are removed, the average independent expenditure in judicial elections

courts in the state judicial system. In the remainder of the paper, I will use the term, State Supreme Courts to refer to the highest appellate body within the state court system.

increases by around $\$300,000$. Interestingly, although the Citizens United ruling did not concern restrictions around direct expenditures, I find that direct expenditures increase by $\$220,000$. This represents a 61% increase over the pre-treatment average of $\$370,000$. I posit that the increase in direct expenditures is a competitive response to higher independent expenditures in judicial elections. I also examine the sources of such an increase in direct expenditure. I find that direct campaign contributions from businesses and political parties are driving the increase, whereas unions do not increase their direct contribution. Interestingly, contributions from lawyers, who are the dominant source of direct financing, did not change after the Citizens United ruling. This is mainly because the lawyers were not constrained by the erstwhile bans on independent expenditures. Overall, the Citizens United ruling not only affected independent expenditures but also led to an increase in direct expenditures mainly driven by contributions from businesses and political parties.

Second, I explore whether the higher expenditure in elections is associated with increased electoral competition. I find that, on average, challengers seem to rely more on independent expenditures, while incumbents on direct expenditures. The Citizens United ruling eased access to independent expenditure, and therefore, it is expected to aid the entry of challengers in the spirit of Osborne and Slivinski (1996) citizen-candidate model of electoral entry. I find that more challengers enter election races with the number of candidates per seat increasing by 35%. The electoral races become more closely contested, and the incumbent judges are 20% more likely to be voted out. These effects are mainly concentrated in states with non-partisan elections, i.e., there is no political party affiliation with the judges on the ballot. Prior political science research Hall (2001) and Hall (2007) has documented that the lack of party affiliation of judges reduces the information available to voters, and financially-intensive elections with widespread advertisements remedy this lack of information. The observed in-sample increase in voter turnout in judicial elections after Citizens United lends further credibility to this hypothesis. Therefore, the Citizens United ruling seems to have heightened the scrutiny of incumbents and reinforced the threat of getting voted out of office. However, it still raises the question of whether the ease of access to independent expenditure also affects the types of judges who are retained or elected.

To examine the effect of Citizens United on the selection of judges, I employ CFscore from Bonica and Woodruff (2015) as a measure of judge ideology.[2] Considering that the State Supreme Court cases are decided by the bench of judges, I examine the effect on the average ideological score of the bench. I find that the average ideological leaning of the judges tends to become more right-leaning or business-friendly in states with judicial elections where the bans were removed. This suggests that the Citizens United ruling affects not only the incentives of judges when in office because of the greater threat of replacement in competitive elections but also their selection.

In the second part of the paper, I begin by sketching out a conceptual framework to illustrate the electoral incentives of judges. Judges seek ego-rents by judging the cases based on their ideological interpretation of the law and not according to merit. I assume that ideological decision-making is inaccurate which imposes a deadweight cost on the firms' factor of production. For example, inaccurate judgments in contractual disputes between the buyers and suppliers may impose a deadweight cost on the use of material inputs. This cost stems from the asymmetry in punitive costs as they are generally imposed on the defendant but rarely on an unsuccessful plaintiff. Note that this cost will be higher if contractual disputes are more likely. I call this cost the legal friction. The higher the inaccuracy, the greater the legal friction. Voters' information and competition in elections strengthen the accountability of judges and thereby prevent rent-extraction by judges.

To summarize, ideological decision-making due to adverse selection increases the deadweight cost, while accountability pushes in the opposite direction. Which effect dominates is an empirical question that I explore in the remainder of the paper. In the appendix, I show that a reduction in the average friction on a factor, such as material inputs, increases the labor productivity, employment, and entry of firms. The framework also aids in formulating a test for the mechanism by using an empirical analog of the likelihood of contractual disputes.

I empirically examine the effect of Citizens United on labor productivity and establishment entry. First, I test whether the Citizens United ruling affects labor productivity. Using data from the Annual Survey of Manufactures, I measure labor productivity as the

[2]Prior research has documented that the ideological leanings of the judges predict their decisions Bonica and Woodruff (2015), Windett, Harden and Hall (2015)

value added per worker at the state-sector level.[3] I find that labor productivity is 5% higher for treated states and that these effects are mainly driven by states with judicial elections where the labor productivity is 8% higher. I do not find statistically significant improvement in productivity in states where the judges are not elected. These results survive a battery of robustness checks. First, the results are robust to an alternative measure of labor productivity as USD value added per hour of labor. Second, the results are robust to the inclusion of sector-by-year fixed effects to account for time trends that affect the sectors differentially. Third, the effect on productivity is also robust to the inclusion of state-specific time trends to account for demand or industrial policy trends specific to each state.

Higher labor productivity has two competing explanations: Citizens United increased the firms' bargaining power relative to unions. In such a case, unproductive workers may be fired, or the wage rates may decline. On the other hand, if the legal friction faced by firms declines after Citizens United, then employment will increase as firms increase production. I find evidence in favor of reduced friction as employment growth rates are higher (5%), along with investment in physical capital (6%) in states where Citizens United invalidated bans on independent expenditure. Moreover, there is no statistically or economically significant change in the average wage.

Second, I test whether the change in the legal environment encourages entry. I find that establishment entry increases by 5% in treated states with judicial elections, while Citizens United has no statistically significant effect on entry in states without judicial elections. The job reallocation rate, a measure of dynamism in the labor market, increases by 4%. The evidence on labor productivity, employment, and establishment entry indicates that the Citizens United ruling has improved resource allocation in states with judicial elections where the bans on independent expenditures were invalidated.

If the functioning of the courts and the legal environment are indeed the driving force behind higher productivity, then these effects must be more pronounced for sectors more exposed to the legal system. For example, if the production process is complex or requires inputs from several sectors, then the likelihood of a contractual dispute will be higher. Prior

[3]The state-sector level aggregate measure is less prone to noise from plant-specific productivity shocks and measurement errors.

research in international trade, Nunn (2005) and Levchenko (2007) has highlighted the connection between product complexity and incomplete contracts risk. When a buyer and supplier enter a trading relationship, they write contracts to overcome hold up. However, contracts are incomplete, and disputes may still arise. The more diverse the inputs to the production process, the greater the likelihood of contractual disputes, thereby increasing the dependence on the quality of legal institutions for production. Employing the input-output data from the Bureau of Economic Analysis (BEA), I construct a measure of incomplete contracts risk or institutional reliance based on the inverse of the Herfindahl-Hirschman Index (HHI) of inputs to the production process. I find that productivity improvements are driven by the sectors in the top tercile of this incomplete contracts risk exposure. No significant effect on productivity is observed for sectors exposed to incomplete contract risk in states without judicial elections after the Citizens United ruling.

Finally, I consider alternative explanations unrelated to judicial selection and campaign finance that may cause the observed real effects. To account for historical, cultural, and geographic factors that affect the sorting of industries across states, I estimate a two-way fixed effects model with state-sector fixed effects and rule out such cross-sectional differences across states as a likely cause. Another concern may be that other events occuring around 2010 have affected productivity in treated states. The effect of these events could differ across states depending on the judge selection procedure or the bans on independent expenditure. This could bias my estimates of the effect of Citizens United on labor productivity and other real outcomes. There are two events with a significant economic impact that may bias the results. First, the global financial crisis (GFC) of 2008-09. Second, the technological developments around 2010 improved the feasibility of hydraulic fracturing and led to a boost in Shale production. I find no evidence that the results are driven by these concurrent events. I also replicate my findings on labor productivity by using establishment level information from the National Establishment Time Series (NETS) data and find that the labor productivity improvement is not associated with greater dispersion in productivity, a measure of factor misallocation.

To summarize, this paper documents the novel channel of judge selection through which campaign finance affects productivity. Particularly, following the removal of bans on in-

dependent expenditures, the electoral expenditure and competition in judicial elections increased, which strengthened the accountability of elected judges. The concurrent improvement in labor productivity, employment, and entry exclusively in states with judicial elections suggests a reduction in legal friction. Although I abstract from the welfare consequences of removing campaign finance restrictions, I provide evidence that removing campaign finance restrictions may increase the accountability of elected judges, particularly when voters have limited information about their performance. The resulting improvement in the legal environment translates into more productive factor allocation.

Related Literature: I contribute to the literature that studies the effect of institutions on growth and productivity, Acemoglu, Johnson and Robinson (2005), Porta et al. (1998), Michalopoulos and Papaioannou (2014), and Haselmann, Pistor and Vig (2010). I highlight the importance of judicial selection procedures and campaign finance laws for the selection of judges and productivity. Other papers in this literature have focused on the contract-enforcement intensity and its effect on financial development, Brown, Cookson and Heimer (2017), and Cookson (2018). I exploit a similar variation in institutional intensity. However, the difference arises due to the election of judges and the importance of electoral finance in competitive elections. I illustrate a novel channel, i.e., the election of judiciary accompanied by less constrained campaign finance laws, through which the productivity of institutionally dependent sectors is positively affected. This finding is in line with prior work that documents a link between within-country contract-enforcement intensity and the choice of production process Boehm and Oberfield (2020), and cross-country legal reform and labor productivity Chemin (2020).

This paper is also related to the literature that relies on the Citizens United v. FEC ruling to identify the effect of increased political expenditure on various economic variables of interest. This paper studies an alternative channel that affects the selection of state judges and the legal environment within a state. I also explore the implications for productivity and establishment entry, which depends on the quality of legal institutions. The papers in this literature most closely related are Akey et al. (2022), Denes, Scanlon and Schulz (2022), and Klumpp, Mialon and Williams (2016). Akey et al. (2022) highlights the democratizing effect of the ruling and how broader political participation leads to higher

labor income. Denes, Scanlon and Schulz (2022) highlight the rise of dark money pools following the ruling, Klumpp, Mialon and Williams (2016) highlight how the ruling has led to higher turnover and increased expenditure in political races. This paper replicates some of these facts in the context of judicial races to show that political expenditures have increased, and electoral races have become more competitive, accompanied by shifts in the ideology of the judicial bench. The key takeaway from this paper has the flavor of Gilens, Patterson and Haines (2021), where the authors document a business-friendly shift in the laws in the aftermath of more relaxed campaign finance laws. However, the results in this paper offer an alternative explanation that is only operative in states with judicial elections.

The paper also contributes to the literature that studies resource misallocation due to political frictions Fisman (2001), Faccio, Masulis and McConnell (2006), Haselmann, Schoenherr and Vig (2018). In this paper, I focus on the judicial frictions that may affect firms reliant on contract enforcement for their production process. I find that removing campaign finance restrictions increases political expenditure, electoral competition, and labor productivity, particularly in states where the judiciary is elected. Moreover, the within-sector dispersion of productivity, a measure of factor misallocation (in the spirit of Hsieh and Klenow (2009), and Larrain and Stumpner (2017)) does not increase. I use a difference-in-differences approach to quantify the misallocation as in Sraer and Thesmar (2023) and document that improved productivity is not associated with higher factor misallocation.

The paper is organized as follows: I begin by discussing the institutional background and research design and arguing the plausible exogeneity of treatment assignment. The following couple of sections document Citizens United's effect on electoral expenditures and competition in judicial elections. I then present Citizens United's effect on real variables such as labor productivity, employment, and establishment entry. I also examine the plausible mechanism and test the robustness of the results before concluding.

1 Institutional Background

State courts decide the majority of cases related to criminal, civil (including contract law), and administrative law. State court judge selection procedures differ across states in the United States. In this paper, I abstract from the judicial selection in the lower state courts and focus on the selection procedure of the state Supreme Court judges (also referred to as high courts). The State Supreme Courts are the highest appellate body within the state court system, and decisions of the Supreme Court determine state law. In addition to hearing appeals and revising decisions of the lower courts, the state Supreme Court bench exercises control over the lower courts through periodic reviews, imposing case disposal protocols to enhance court efficiency and budgetary discretion. Therefore, the selection of state Supreme Court judges is crucial in determining the legal environment within a state.

Overall, 22 states undertake elections to select the bench of their Supreme Courts. Figure (1) illustrates the heterogeneity of selection procedures and how certain forms of selection are not localized in a particular region. This provides some degree of relief against endogenous selection into different procedures for selecting judges. The remaining 38 states have adopted some form of appointment. The judges are appointed either by the governor or the state legislatures or through a merit plan in which a governor-nominated commission appoints the judges, who are then approved by the legislature and the governor. Table (32) in the Appendix provides more details regarding the selection procedures for judges across different states.

The judge tenures differ across states. The judges' tenure in some states may last from 6 to 10 years, while some State Supreme Court judges may even serve until retirement. The judicial elections are either for an open seat, i.e., elections without incumbents where the incumbent has retired or there may be a general election with incumbents and challengers. A majority of the states conduct elections in even-numbered years. In contrast, a few states, such as Pennsylvania (exclusively in odd-numbered years), Louisiana, and Wisconsin, may conduct elections in odd-numbered years.

The judicial candidates may raise funding for their election campaign from individual donors, special interest groups, or rely on personal wealth. Candidates use the funding for advertisements and paying staff involved in the electoral campaign. This type of expen-

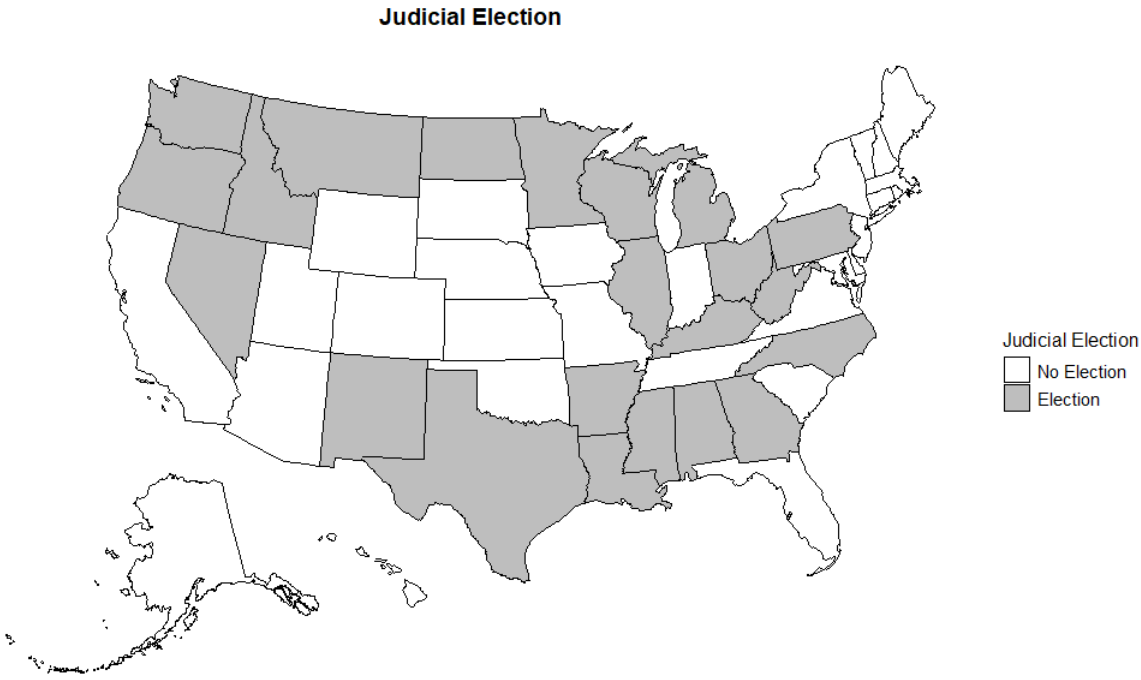


Figure 1: States with elections for the Supreme Courts in Grey. 22 states have judicial elections.

diture is referred to as the direct expenditure. Additionally, independent advertisements run on behalf of the candidate highlight the candidate's ideological position or provide more information about the judges' track record. This type of expenditure incurred on behalf of the candidate without direct contact with him is referred to as independent expenditure. Figure (21) provides an illustration of an advertisement favoring candidate Janet Protasiewicz by Planned Parenthood, an ideological group in the Wisconsin State Supreme Court elections of 2023. Although there are disclosure requirements for direct and independent expenditures, the disclosure laws for independent expenditures are quite lax. The anonymity of donors makes independent expenditure the most preferred conduit for the flow of dark money in elections. The limitless flow of such dark money could lead to institutional capture.

To prevent capture by moneyed interests, some states had imposed bans on independent expenditures by corporations and/or unions in any state election. However, in March 2009, Citizens United, a non-profit conservative special interest group, appealed the decision of the District Court of Columbia to the Supreme Court of the United States. Later, in January 2010, the Supreme Court, in a surprise 5-4 ruling, rendered such bans on independent expenditure unconstitutional. The ruling was met with strong and conflicting reactions from various political actors. Most were concerned with the possibility of institutional capture due to the increasing flow of dark money into the elections. Certainly, the ruling allowed interest groups to donate more freely and anonymously through super PACs (Political Action Committees). Figure (2) illustrates the selection procedures, along with the states that had imposed a ban on independent expenditures. After the Citizens United ruling, these bans were invalidated, relaxing the campaign finance constraints for firms and interest groups.

Judicial elections differ from legislative elections in one other dimension. While some states allow the party of the judge to be on the ballot, others do not. The former are termed partisan election states, and the latter are the non-partisan election states. Legal and Political science scholars such as Kang and Shepherd (2015), Lim (2013), and Ash and MacLeod (2021) have shown that this distinction is important in determining the political expenditures and quality of judges. Figure (10) in the Online Appendix provides

information about the states with partisan and non-partisan judicial elections. Overall, out of the 22 states with judicial elections, 11 states have partisan elections.

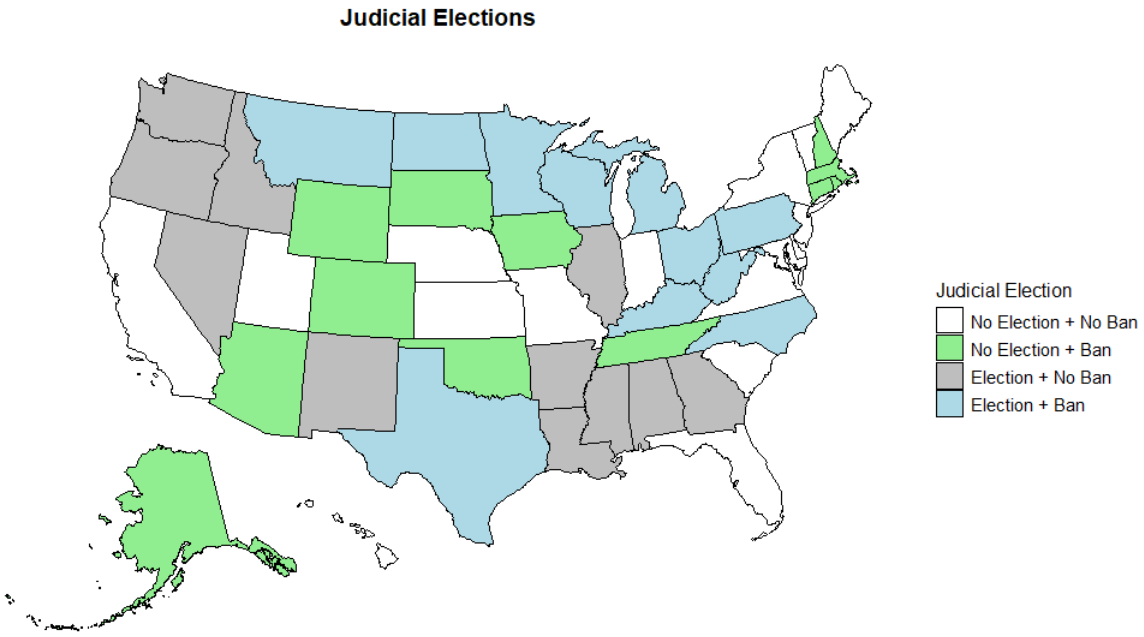


Figure 2: States that imposed some form of ban on independent expenditure are highlighted in blue and green. States with judicial elections, but no bans are in grey. States without judicial elections and no bans are in white.

2 Data and Identification Strategy

I combine data from several sources to draw a connection between campaign finance laws, actual political expenditure in judicial elections, outcomes in judicial elections, and productivity of the real sector. In this section, I will describe the sources of data along with the identification strategy. More details regarding the sample construction are in the Online Appendix.

2.1 Political Expenditures Data

Political expenditure data is from the National Institute on Money in State Politics (NIMSP, OpenSecrets.org). The sample period for the direct expenditure is $2000 - 2022$. There is data for some state election races going back up to 1989. However, NIMSP started collecting data for all 50 states only since 2000.[4] Overall, we have data covering the direct expenditure of 907 judicial candidates for 23 years, with 1235 candidate-year observations. Figure 24 in the online appendix illustrates the lack of funding data availability before 2000, where it is clear that over 50% of the states going for state Supreme Court judge elections are not covered in the data. The independent expenditure data has limited coverage due to poor disclosure quality. This data is available for 13 states with robust disclosure requirements from 2006-2022. Of the 13 states covered, 9 had imposed a ban on independent expenditures by incorporated entities before 2010.

2.1.1 Judicial Elections and Judge Ideology Data

Several judicial scholars have painstakingly collected and compiled data on judicial elections. In this paper, I use the most up-to-date and comprehensive source of this information from Kritzer (2015). This dataset provides information on all judicial elections from 1946 until 2020. The original dataset contains the identity of the candidates, their incumbency status, the votes received, the type of election, and the number of seats being contested. I use the sample starting from the year 2000 and collapse the data at the level of elections, which gives me 675 election-year observations.

In Table (1), we can see that the average margin of victory is 39%. For reference, the average margin of victory in the House of Representatives and the US Senate races in 2022 was 28% and 19.6% respectively.[5] Therefore, the judge election races are not as closely

[4]See the disclosure from NIMSP available at `https://www.followthemoney.org/help/q-and-a` : "The institute has contributions data for candidates running for state office in all 50 states since 2000 (though data for some state races extends back to 1989). I began collecting ballot measure contributions data in 2004, and independent spending data for some state elections in 2006. I recently added contributions data for candidates running for federal office and some local offices beginning in 2011-2012."

[5]Source:`https://ballotpedia.org/Election_results,_2022:_Congressional_margin_of_victory_analysis`

Table 1: Summary Statistics: Judge Elections

	Mean	SD	25th percentile	Median	75th percentile	N
vote margin	0.39	0.38	0.10	0.20	0.84	638
no. of seats	1.03	0.16	1.00	1.00	1.00	675
no. of cand.	2.20	1.20	2.00	2.00	2.00	675
cands. per seat	2.13	1.10	2.00	2.00	2.00	675
incumb. win	0.23	0.42	0.00	0.00	0.00	675

contested as the legislative elections, mainly because a majority of the election races are uncontested. The incumbents win with a probability of 32%, and there are incumbents contesting an election in 445 out of the 675 races. Therefore, conditional on an incumbent competing in the election, the turnover is high relative to the legislative elections where the incumbents are re-elected with a probability of around 80%.

Judge Ideology Political scientists have designed several spatial measures for capturing the ideology of judges. Of important note are three measures of judge ideology. The first, Public Assisted Judge Ideology (PAJID) from Brace, Langer and Hall (2000) relies on the electorate's ideological position at the time of election, the common-space CFScore compiled by Bonica and Woodruff (2015) relies on the political donation by judges, and the Windett, Harden, and Hall (WHH) score which combines elements of the common-space approach with judicial decisions Windett, Harden and Hall (2015). In this paper, I employ the replication data from Wilhelm, Vining and Hughes (2023) which computes the PAJID measures from 1979-2020. They also compile the CFScore from Bonica and Woodruff (2015) which is available until 2015.

2.1.2 Manufacturing Census Data

The state Supreme Court judges affect the legal environment within the state. An individual plant operating in a given state may be exposed to various shocks including shocks to its productivity. Additionally, there may be measurement errors at the plant level. Such plant-specific shocks and measurement errors wash out if we aggregate the measures of real

activity at the industry level within the jurisdiction. This reduces noise in the real output data. I employ the sector-state level aggregates compiled by the Census Bureau for the manufacturing sector as part of the Annual Survey of Manufactures. In this dataset, we observe the capital expenditure, shipments, wage-bill, capital expenditure, and value-added at the 4-digit NAICS level by state at an annual frequency. Relying on the API for pulling the data, the sample period is $2003 - 2021$. There are 84 unique 4-digit NAICS sectors, resulting in a sample of $53,975$ state-sector-year observations. The panel is unbalanced and some state-sector pairs appear only after the treatment year 2010. To address this concern, I restrict the sample to include state-sector pairs having at least 1 pre-treatment year observation. This results in a sample of $33,620$ state-sector-year observations.

Table 2: Summary Statistics: Manufacturing Production

	N	Mean	SD	25th percentile	Median	75th percentile
Revenue (000 USD)	36,404	2,305,091	6,497,902	420,147.5	979,983.5	2,259,563
CapEx (000 USD)	36,316	64,372.8	182,708.2	7,806.5	23,506.5	60,965.5
Emp. (000)	36,404	3,824.4	5,495.8	1,097	2,108	4,197
Value Added (000 USD)	36,363	1,007,763	2,118,776	194,804	463,797	1,057,952
Lab Prod (000 USD/emp)	36,363	273.6	302.7	132.0	192.9	307.9
Rev/Mat Cost	33,727	2.1	0.7	1.7	2.0	2.4
wage (000 USD/emp)	36,404	41.5	14.2	33.0	39.9	48.1

Table (2) shows that the distribution of all variables is skewed except the average wage of production workers. The average labor productivity computed as the ratio of value added per production worker, is USD $280,000$. A given sector on average employs around $4,000$ production workers and incurs an annual capital expenditure worth USD $68,000$ in a state. The average wage of the workers is USD $40,000$.

In addition to the Annual Survey of Manufactures, I also employ the business dynamics statistics data released for 19, 2-digit NAICS sectors for all 50 states annually for 22 years. The sample consists of $21,318$ state-sector-year observations. This data allows me to observe the number of firms, establishments, employment, and other measures of business dynamism such as entry, exit, job creation, and job destruction rates.

2.1.3 Other Data

I also make use of other public sources of data such as state election commissions for gubernatorial election races, input-output data from the Bureau of Economic Analysis to compile the measure of sector-level institutional dependence. For robustness of results to disaggregation at the establishment level, I use the National Establishment Time Series (NETS) to provide establishment-level evidence on average productivity, and resource misallocation due to the relaxation of campaign finance restrictions.

2.2 Research Design

As discussed earlier, the United States (US) offers an interesting setting to study the effect of campaign finance restrictions on judge selection and sector-level productivity. In total 22 states in the US employ elections to select judges to the highest state courts. The remaining 28 use some form of governor, or state-legislature appointment. The forces that determine the form of selection depend on the historical context of the particular state, and different states have adopted the selection procedure over a long span. For example, Virginia instated the current legislative election method of selection in 1779, whereas, Illinois switched to partisan elections in 1962. The common reasoning behind employing elections is to give more power to the citizens and prevent a possible capture of all branches of the government by a minority elite.

The Bipartisan Campaign Reform Act (BCRA) of 2002 imposed two key restrictions. First, it imposed limits on parties raising and spending in election campaigns. Second, it restricted issue-advocacy advertisements run by incorporated entities and issue-based special interest groups for example, Right to Life. The 2010 ruling by the SCOTUS in the Citizens United v. FEC case, ruled that any campaign finance restriction imposed on organized interest groups is unconstitutional. This also applied particularly to bans on independent expenditure imposed by 23 states. The Supreme Court's decision was unexpected and was a $5-4$ split among the bench. This episode provides a natural experiment that immediately and unexpectedly eased the campaign finance restrictions in these 23 states. I consider these states as the treated states in a difference-in-difference framework.

For simplicity consider two periods $t \in \{0, 1\}$ and two states $s \in \{T, C\}$ with multiple sectors operating within each state. There is an intervention that affects state T, the treated states. The other state is the control state C which is unaffected by the treatment. Define the across time and within state-sector differences by Δ_t, so that

$$\Delta_t(y_{js}) = (y_{js1} - y_{js0})$$

I consider states directly affected by the Citizens United ruling, as the treated states. The period following 2010 is the post-period in a simple 2×2 difference-in-differences (DD) research design. In this paper, I focus on the heterogeneous effect of campaign finance restrictions conditional on the type of judge selection procedure.

I estimate the following two-way fixed effects model,

$$\begin{aligned} y_{jst} = \delta_s + \delta_t + \beta_{ep} \cdot (Election \times Post) + \beta_{bp} \cdot (Ban \times Post) \\ + \beta ebp \cdot (Election \times Ban \times Post) + \varepsilon_{jst} \end{aligned} \tag{1}$$

y_{jst} is any dependent variable of interest for unit j in state s at time t. δ_s are state fixed-effects that account for state-specific time-invariant geographic, historical, or cultural characteristics. Controlling for these allows us to isolate the historical or cultural determinants of selection into the decision to elect the judges, or impose bans on independent expenditures. δ_t is the time fixed-effect that accounts for election-cycle or year-specific shocks that affect the variable of interest across all states. I examine the heterogeneous treatment effects for states with judicial elections ($Election = 1$), and states without. Standard errors are clustered at the level of treatment assignment, i.e., at the level of states Abadie et al. (2023). The estimate of the average treatment effect conditional on the judicial selection procedure is,

$$E[\Delta_t(y_{js})|Election = 1] = \beta_{ebp} + \beta_{bp} \ , \ E[\Delta_t(y_{js})|Election = 0] = \beta_{bp}$$

To causally identify the effect of the relaxation of campaign finance restrictions, the outcome variable must satisfy the assumption of parallel trends. This assumption is likely to hold if the states that had imposed a ban on independent expenditure do not differ significantly on observables relative to states that had not imposed such bans conditional on the selection procedure. In Table (3), I provide regression evidence that the treatment

assignment, i.e. lifting of bans ($Ban = 1$) or the lifting of bans in states with judicial elections ($Election \times Ban = 1$) is uncorrelated with the scale of operations of firms, i.e. capital expenditure and employment. Further, I show that the average productivity-related measures, such as wage and labor productivity, are also not significantly different across treated and control groups. Table (17) in the Appendix provides further evidence that the treatment assignment conditional on judicial election is uncorrelated to electoral variables such as democratic governor and president vote share, median income, and percentage of rich and poor households. Note that there are no statistically significant differences across states for productivity variables; there may be differences in investment and employment across states with bans on independent expenditures with judicial elections. I account for some of these differences by controlling for state, and state-sector fixed effects as well as state-specific time trends. The parallel trends assumption is later validated in the dynamic event-study specifications discussed along with the two-way fixed-effects regression evidence.

Table 3: Pre-Treatment Difference

Note: This table presents the evidence for the absence of pre-treatment selection. The table shows the regression of economic outcome variables on the indicators for states with judicial elections for the state Supreme Court judges, and states with bans on independent expenditure invalidated by the 2010 Citizens United v. FEC ruling and their interaction for the pre-treatment period 2003-2009. All regressions include year fixed effects to account for aggregate economic shocks and 4-digit NAICS sector fixed effects. Standard errors are clustered at the state level.

	(1)	(2)	(3)	(4)	(5)
	log(CapEx)	log(Emp)	wage (USD/emp)	Labor Productivity (USD/emp)	Labor Productivity (USD/hr)
Election	0.19	0.16	-0.16	14.26	5.99
	(0.15)	(0.14)	(0.78)	(17.71)	(7.98)
Ban	0.05	-0.00	0.76	14.47	5.82
	(0.15)	(0.14)	(0.83)	(9.00)	(4.21)
Election × Ban	0.24	0.26	-0.16	-20.91	-8.25
	(0.21)	(0.19)	(1.11)	(20.34)	(9.15)
Year FE	Y	Y	Y	Y	Y
Sector FE	Y	Y	Y	Y	Y
N	12,744	12,744	12,744	12,744	12,701
R-sq.	0.44	0.45	0.61	0.46	0.47

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

3 Judicial Elections and Competition

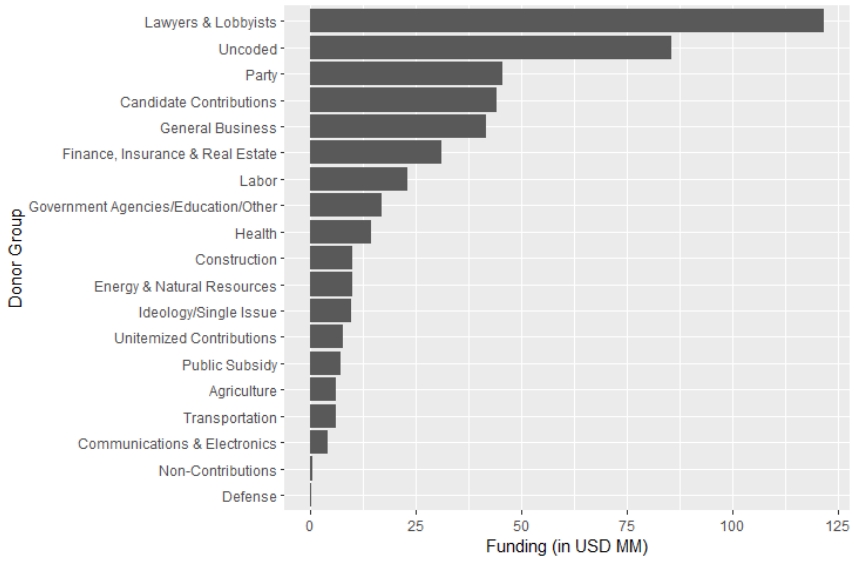
3.1 Political Expenditure in Judicial Elections

The National Institute on Money in State Politics (NIMSP) compiles expenditures disclosed by judicial candidates, and campaign finance donors from all 50 states. First, we focus on direct expenditure due to better coverage and transparent disclosures across states. The NIMSP classifies the donations into different categories depending on the information in each disclosure. These categories include contributions from lawyers, businesses, unions, candidate self-funding, or political parties.

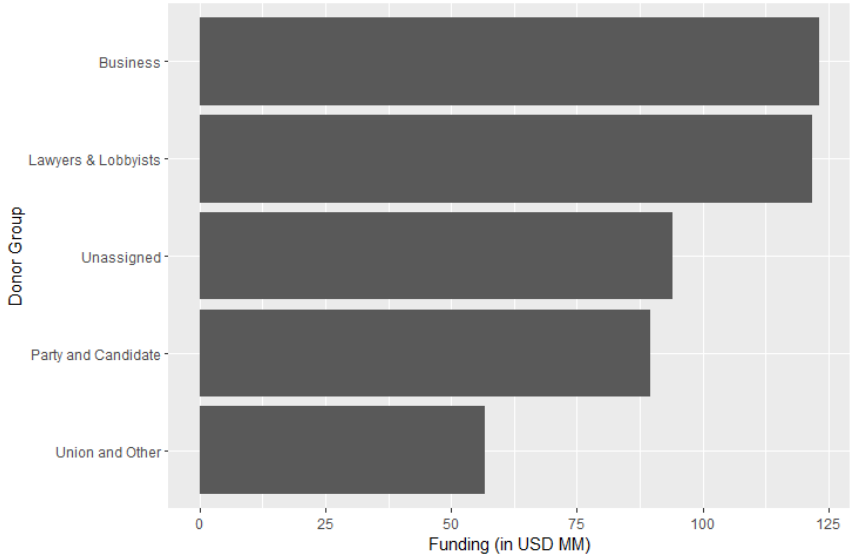
First, I consider the aggregate campaign contributions from different sources over the sample period, $2000 - 2021$. In Figure (3), Panel (a) I show the categories, as classified by NIMSP. In Panel (b), I combine these into the broad categories of Business, Lawyers, Party, and Unions. Small itemized contributions and donations where the source could not be attributed are classified as unassigned. 20% of the donations are unassigned.[6] Lawyers and lobbyists are the largest contributors to judicial elections. Their donations amount to 120 MM which is around 27% of all direct expenditure in these elections. Political parties have donated around \$50 MM (11%). When I consolidate the donations from different business interests, such donations are the leading source of campaign finance for judges competing in elections and amount to a total of \$125 MM (28%). The unions and ideological groups have donated around \$50 MM. In summary, the business interests and lawyers are more active than the unions when contributing directly to judicial candidates. Table (4) replicates some of these patterns and highlights how politically active different groups are.

The Supreme Court ruling lifted bans imposed on independent expenditure. Therefore, it is possible that direct expenditure remained unaffected. On the other hand, if the Citizens United ruling eased access to independent expenditure for any candidate, the competing candidate may increase her direct expenditure. In this case, we would expect that the direct expenditure increases after the ruling in states where the bans were lifted relative to the control states, without such bans. In Figure (17) we compare the average direct expenditure

[6]Note that the two categories most strongly correlated with the unassigned contributions are contributions from parties and self-funding of candidates, and the donations from business groups. This hints that unassigned contributions must be closely related to these donating groups.



(a) Funding Disaggregated



(b) Funding Aggregated

Figure 3: Funding of Judicial Candidates from different Donor Categories

Table 4: Summary Statistics Judge Direct Expenditures

	N	Mean	SD	25th percentile	Median	75th percentile
Fund (in MM)	1,081	0.45	0.76	0.05	0.21	0.56
Business	1,081	0.11	0.25	0.00	0.02	0.11
Lawyers and Lobbyists	1,081	0.11	0.20	0.00	0.04	0.14
Unassigned	1,081	0.09	0.18	0.01	0.03	0.09
Party and Cand.	1,081	0.08	0.35	0.00	0.01	0.05
Union and other	1,081	0.05	0.17	0.00	0.01	0.04

in 2-year election cycles for treated and control states. The average direct expenditure rises sharply after 2010, in treated states. Also note that the pre-period trends in direct expenditure for treated and control states track each other quite well. This inspires some confidence that the assumption of parallel trends likely holds. Note that this evidence does not account for state-specific factors such as history, culture, natural endowments, state income, and traditional partisan position that may influence the expenditure in judicial elections. In the following sections, I adopt the difference-in-differences (DD) framework to analyze the effect of the Supreme Court ruling on direct and indirect election expenditure, electoral competition, labor productivity, and investment.

3.2 Political Expenditure: Effect of Citizens United

First, I test whether lifting bans on independent expenditure increased independent expenditure in judicial elections. If this true, then it confirms that the constraints on independent expenditure were binding in judicial elections. Akey et al. (2022), Denes, Scanlon and Schulz (2022), and Spencer and Wood (2014) have already documented this pattern for other political races. Second, I examine if there is a competitive response that increases direct expenditure. To test for these hypotheses, I estimate regression equation.

$$y_{ist} = \delta_s + \lambda_t + \beta \cdot Ban_s \times Post_t + \varepsilon_{ist} \tag{2}$$

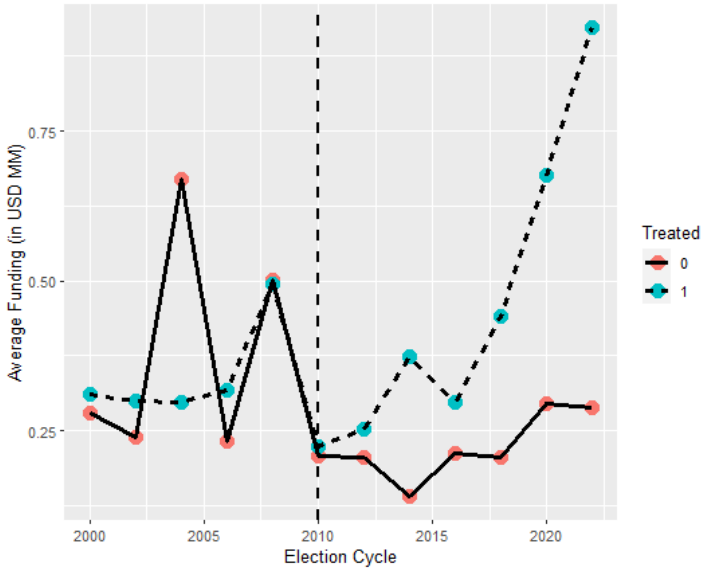


Figure 4: Average funding in USD for judicial elections in Treated vs. Control States

in the regression specification $y_{ist} \in \{\text{Direct Expenditure}_{ist}, \text{Independent Expenditure}_{ist}\}$ for candidate i, in state s and time t, $Treat_s = \mathbb{1}(\text{State imposed ban on independent expenditure})$, $Post = \mathbb{1}(t >= 2010)$. $\{\delta_s, \lambda_t\}$ are state and time fixed-effect. The state fixed-effects account for state-specific time-invariant characteristics such as history, culture, and partisan electoral preferences. I present the DD estimates for the average treatment effect on the funding and independent expenditures in Table (5). The average increase in direct expenditure is nearly $\$220,000$, and the independent expenditure increases by $\$300,000$.

Next, I test for the assumption of parallel trends in direct expenditure and examine the dynamic effects of the Supreme Court ruling on the funding of representatives in elections.[7] I estimate the following regression equation,

$$y_{ist} = \delta_s + \lambda_t + \sum_{\tau=-5}^{6} \beta_\tau \cdot Ban_s \times D_{t-\tau} + \varepsilon_{ist} \tag{3}$$

I set $\beta_0 = 0$. The comparisons in the change in funding across treated and control states take the 2010 cycle as the baseline. Figure (5) shows that the direct expenditure

[7]The indirect expenditure data compiled by NIMSP has limited coverage for 13 states starting from 2006 due to poor disclosure.

Table 5: Effect on Political Finance of Judges

Note: This table presents the estimation results of Equation (2). The dependent variable is the total expenditure by a candidate in the judicial elections, measured in USD MM. Variable *Ban* indicates the states that had imposed a ban on independent expenditures by unions or corporations, which were rendered unconstitutional by the Supreme Court in 2010. The specifications in each column vary depending on the inclusion of state, and year fixed effects. Standard errors are clustered at the state level.

	(1)	(2)	(3)	(4)
	Fund (in MM)	Fund (in MM)	Ind. Exp. (in MM)	Ind. Exp. (in MM)
Post	-0.39***		-0.15	
	(0.11)		(0.26)	
Ban	-0.02		-0.07	
	(0.15)		(0.08)	
Ban × Post	0.18*	0.22**	-0.16	0.30**
	(0.10)	(0.10)	(0.34)	(0.13)
Election Cycle FE	N	Y	N	Y
State FE	N	Y	N	Y
N	1,227	1,227	251	251
R-sq.	0.08	0.27	0.13	0.36

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

in treated and control states follow parallel trends in the pre-period. However, after the Supreme Court ruling the direct expenditure in treated states increased compared to the control states.

In Table (6), I further dissect the sources of increase in direct expenditure. We estimate (1), with the direct contributions from different funding sources as the dependent variable. The increase in political funding is mainly driven by funding from Business, and political parties (and unassigned groups). Interestingly, lawyers and lobbyists, one of the prominent sources of financing, were not directly affected by the ruling. The change in direct expenditure is neither economically nor statistically significant for this group. Moreover, unions and other ideological groups do not seem to be affected by the Supreme Court ruling either. This is somewhat puzzling, but this alludes to the fact that restrictions on expenditure were not binding for unions. This may be because they have other means of exerting influence, or they are financially constrained. Figure (6) provides further credible evidence regarding the change in funding from different sources. Again, there is a statistically and economically significant increase in expenditure from Business, however, there is no such increase for Unions. In the appendix, I exploit the heterogeneity in the pre-existing

Note: This figure presents the estimation results of Equation (3). The dependent variable is the political expenditure by a judicial candidate, measured in USD MM. The figures indicate the coefficients and 95% confidence intervals that illustrate the dynamic effects on labor productivity due to the lifting of the bans imposed on independent expenditures by unions or corporations, which were rendered unconstitutional by the Supreme Court in 2010. Event Time, 0 is the year 2010. All estimations include state and year fixed effects. Standard errors are clustered at the level of the state.

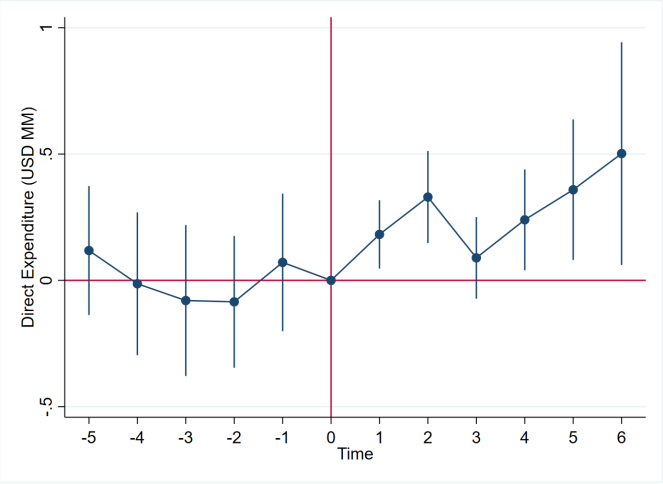


Figure 5: Event study plots. Event time is the 2009-2010 election cycle.

bans, i.e. corporations-only bans vs. corporations and unions bans to show how a higher competition among various interest groups is a likely reason behind the higher political expenditure in judicial elections after the 2010 Supreme Court ruling.

Table 6: Effect on Direct Expenditures (Categorized)

Note: This table presents the estimation results of Equation (2). The dependent variable is the total expenditure by a candidate in the judicial elections, measured in USD MM. Different columns shows results for estimation with the dependent variable as the political expenditure in an election by a candidate from a particular source, such as business, unions, or political parties. Column (5) shows the results for expenditure items that could not be assigned to a particular source. Variable *Ban* indicates the states that had imposed a ban on independent expenditures by unions or corporations, which were rendered unconstitutional by the Supreme Court in 2010. The specifications in each column vary depending on the inclusion of state, and year fixed effects. Standard errors are clustered at the state level.

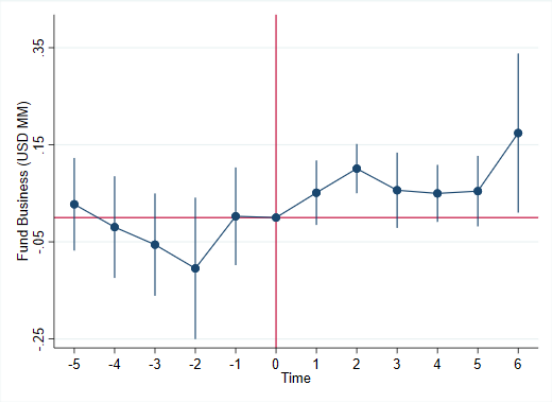
	(1)	(2)	(3)	(4)	(5)
	Business	Party and Cand.	Union and other	Lawyers and Lobbyists	Unassigned
Ban × Post	0.08*	0.08**	-0.00	-0.04	0.07**
	(0.05)	(0.03)	(0.02)	(0.03)	(0.03)
Election Cycle FE	Y	Y	Y	Y	Y
State FE	Y	Y	Y	Y	Y
N	1,070	1,070	1,070	1,070	1,070
R-sq.	0.21	0.16	0.17	0.21	0.32

Standard errors in parentheses

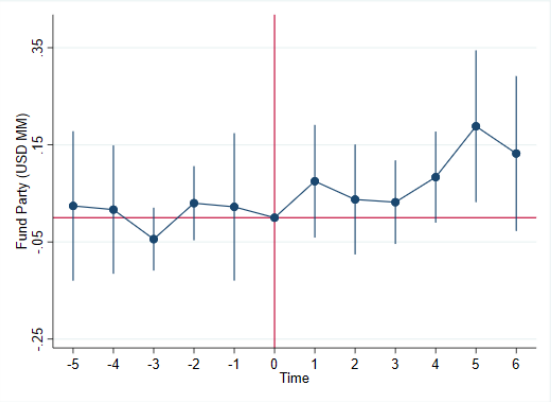
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Discussion Earlier in this section, I alluded to a possible explanation for why direct expenditure may increase when restrictions on indirect expenditure are relaxed. I now provide evidence that suggests that incumbents have easier access to direct funding, and challengers seem to rely more on indirect expenditure. In Table (7), I consider the average indirect and direct expenditure for candidates who are challengers, incumbents, or competing for an open seat. I compare the challengers and the incumbents. In states with bans, the pre-2010 independent expenditures are quite low compared to direct expenditure, and the incumbents enjoy a clear financing advantage relative to challengers. During the same period, the challengers in states without such bans (Table 8) rely more on indirect expenditure, while incumbents spend more directly. After 2010, the challengers and incumbents both

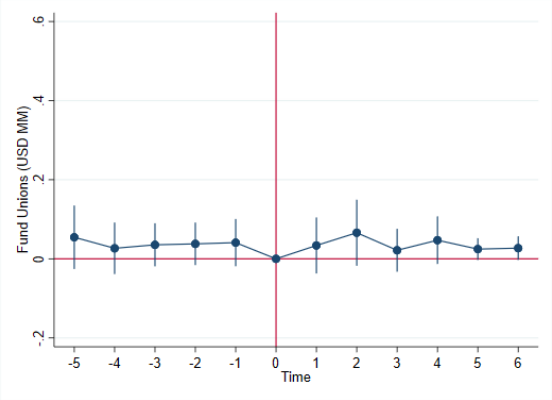
Note: This figure presents the estimation results of Equation (3). The dependent variable is the political expenditure by a judicial candidate, measured in USD MM from various sources such as Business groups in Panel (a), Party and self-funding from candidates in Panel (b), and funding from unions and other special interest groups in Panel (c). The figures indicate the coefficients and 95% confidence intervals that illustrate the dynamic effects on labor productivity due to the lifting of the bans imposed on independent expenditures by unions or corporations, which were rendered unconstitutional by the Supreme Court in 2010. Event Time, 0 is the year 2010. All estimations include state and year fixed effects. Standard errors are clustered at the level of the state.



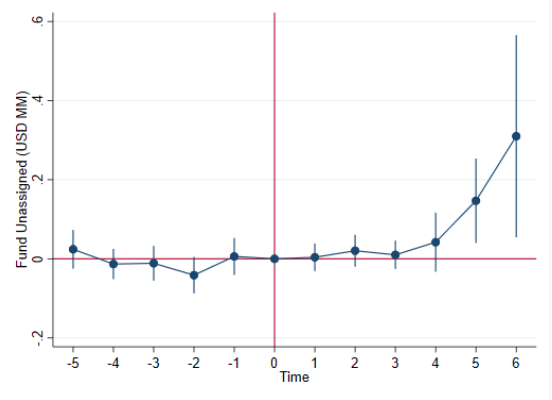
(a) Funding Business



(b) Funding Party



(c) Funding Unions



(d) Funding Unassigned

Figure 6: Event Study of Funding of Judicial Candidates from Different Donor Categories

increase their indirect expenditure in treated states (Table 7). Their direct expenditure also increases. However, it is clear that challengers seem to rely more heavily on indirect expenditure and experience a stronger gain in treated states relative to control states where there was no change in campaign finance restrictions. As the evidence suggests, newer entrants (challengers) rely more on indirect expenditure to compete. Therefore, reducing the cost of access to such funding should increase the political entry in the spirit of Osborne and Slivinski (1996) citizen-candidate model of political competition. Political entry should increase competition in judicial elections and increase political turnover. We test for this effect on electoral competition in the following section.

Table 7: Summary Statistics: Judge Election Expenditure in States with Bans on Independent Expenditure (Treated)

	(Pre-2010)		(Post-2010)	
	Independent Expenditure (USD MM)	Direct Expenditure (USD MM)	Independent Expenditure (USD MM)	Direct Expenditure (USD MM)
Challenger	0.05	0.28	0.27	0.46
Incumbent	0.12	0.68	0.28	0.81
Open	0.25	0.44	0.57	1.08
Total	0.12	0.51	0.34	0.73
Observations	34		171	

Table 8: Summary Statistics: Judge Election Expenditure in States without Bans on Independent Expenditure (Control)

	(Pre-2010)		(Post-2010)	
	Independent Expenditure (USD MM)	Direct Expenditure (USD MM)	Independent Expenditure (USD MM)	Direct Expenditure (USD MM)
Challenger	0.30	0.15	0.34	0.15
Incumbent	0.17	0.24	0.43	0.57
Open	-	-	0.92	0.97
Total	0.24	0.19	0.59	0.64
Observations	11		38	

3.3 Electoral Competition

The 2010 Supreme Court ruling eased access to indirect expenditure by allowing a broader set of constituents to donate freely and anonymously. As discussed in the previous section, new entrants or challengers seemingly benefit from this form of financing. In this section, I

first test if removing constraints on indirect expenditure increased entry in judicial elections. Next, I test if the entry of candidates is also associated with closer competition in electoral races. Finally, I test if increased entry and competition lead to higher political turnover for incumbent judges.

I estimate the same regression equation (2) replacing y_{ist} with election level dependent variable in each election i in state s in election cycle t. The dependent variables are the number of candidates competing in an election, the vote margin of victors, and an indicator for the incumbent winning the race. Since there could be many races in which only one candidate (incumbent) runs for election and some elections where there are no incumbents (elections for open seats), I control for the election type fixed effect to account for such election-specific differences. In Table (9), Columns (1) and (2), the average treatment effect of relaxing campaign finance restrictions on the number of challengers in an election race is positive, i.e., when the bans on independent expenditure are removed, more challengers enter the race along the lines of Osborne and Slivinski (1996). In particular, if before 2010, there were, on average, 3 (1.5 candidates per seat) candidates competing for 2 judicial seats, after the removal of bans, there are 2 candidates per seat.

The second piece of evidence in Table (9), Column (3) implies that there is a 15% reduction in the vote margin of the victors. Figure (7) illustrates this finding graphically. I document a clear decline in the average vote margin in treated states relative to the control states after the 2010 Supreme Court decision. However, controlling for whether there are two or more candidates per seat (in Column (5)), the coefficient estimate of interest becomes insignificant. This indicates that the primary driving force behind lower vote margins is the entry of challengers in election races. In Column(4), I consider the distinction between partisan and non-partisan elections. I find the increase in competition is concentrated in states with non-partisan elections. In non-partisan elections, voters have less information about the candidates' partisan preferences because the party is not on the ballot. Therefore, it seems plausible that the higher electoral spending eased this information friction and increased competition. The third piece of evidence, included in the appendix, deals with the competitive advantage of incumbents in judicial elections. The incumbency advantage, measured as the likelihood of the incumbent winning a re-election

Table 9: Electoral Competition

Note: This table presents the estimation results of Equation (2). The dependent variable in Columns (3) and (4) is the percentage difference in votes of the winner and the closest losing rival. Columns (1) and (2) show the results with the number of candidates per seat in judicial elections as the dependent variable. Variable *Ban* indicates the states that had imposed a ban on independent expenditures by unions or corporations, which were rendered unconstitutional by the Supreme Court in 2010. The specifications in each column vary depending on the inclusion of state, and year fixed effects. Standard errors are clustered at the state level.

	(1)	(2)	(3)	(4)	(5)
	no. of cand.	no. of cand.	vote margin	vote margin	vote margin
# Seats	2.28***	1.41**	-0.06	-0.15***	0.69***
	(0.55)	(0.60)	(0.05)	(0.04)	(0.03)
Ban × Post	0.57**	0.57**	-0.15**	-0.24***	-0.03
	(0.26)	(0.27)	(0.07)	(0.06)	(0.02)
Ban × Post × Partisan				0.20	
				(0.14)	
$\mathbb{1}\{\#\text{ Candidates} > \#\text{ Seats}\}$					-0.80***
					(0.01)
$\mathbb{1}\{\#\text{ Candidates} > \#\text{ Seats}\} \times \text{Ban} \times \text{Post}$					-0.00
					(0.02)
Election Cycle FE	N	Y	Y	Y	N
State FE	N	Y	Y	Y	N
Election type FE	Y	Y	Y	Y	Y
Observations	675	675	638	638	638
Rsq.	0.29	0.41	0.33	0.34	0.85

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

bid, also declines after 2010. In Table (23), (in Appendix), I exhibit evidence that the incumbent's likelihood of winning the re-election declines by 20 pp. from the baseline of 55% in treated states after 2010. Overall, I find evidence consistent with Akey et al. (2022) that the 2010 decision of the Supreme Court had a democratizing effect. To my knowledge, this is the first paper that documents this effect on the judicial election races.

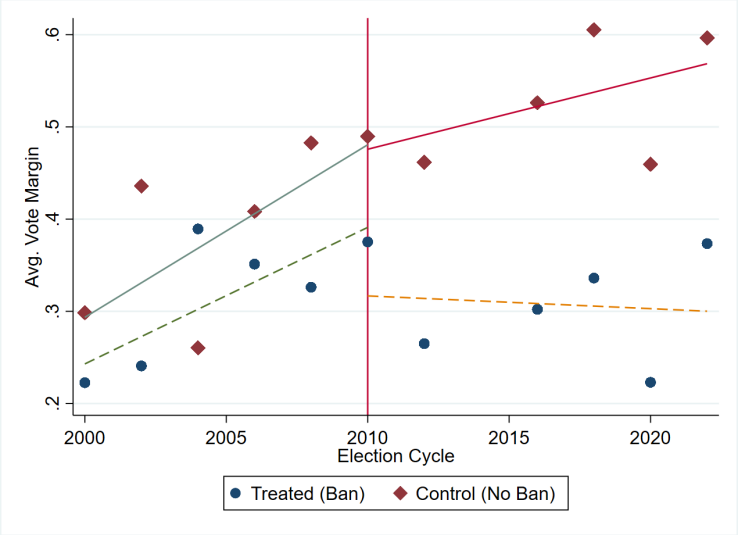


Figure 7: Vote Margin in Treated v. Control States

3.4 Effect on Bench Composition

The State Supreme Court cases are decided by a bench of judges, not by the jury or an individual judge. Although judges intend to interpret the state law in its spirit and based on precedent, their personal preferences or interpretation of law frequently affects their final decision as documented by Windett, Harden and Hall (2015), Brace, Langer and Hall (2000). These authors proxy for judge preferences using a spatial ideology measure for each judge. As we have seen, removing restrictions on campaign finance increased the flow of money and competition in judicial elections. It is important to understand the effect of such changes in elections on the ideology of the State Supreme Court benches. In this section, I provide evidence that the average ideology of the judicial bench shifts in response to the 2010 Citizens United decision by the Supreme Court.

I restrict attention to the Common Space ideology score (CFScore) from Bonica and

Woodruff (2015). This score relies on revealed ideological preferences by assigning an ideological score depending on the political donations by the judicial candidate. Bonica and Woodruff (2015) document that ideology scores constructed using this methodology predict the votes of judges, and therefore, the ideological leaning of the judges affects how cases may be decided.

$$\begin{aligned} y_{st} = \delta_s + \delta_t + \beta_{ep} \cdot (Election \times Post) + \beta_{bp} \cdot (Ban \times Post) \\ + \beta ebp \cdot (Election \times Ban \times Post) + \varepsilon_{st} \end{aligned} \tag{4}$$

I estimate the regression specification in Equation (4) with mean bench ideology and the standard deviation of the bench ideology (a measure of diversity) as the dependent variable. The ideology score lies in the range of $[-2, 2]$, with a higher number associated with more right-leaning or business-friendly judges. The results are in Table (10). There is a shift in the ideology of the bench, with both the mean and the standard deviation of ideology increasing for the states with ex-ante bans and judicial elections. An opposite pattern holds for states without judicial elections. This pattern of right-leaning bench holds in an event study design with 2010 as the baseline year. The right-shifting pattern of mean bench ideology is illustrated in Figure (8). One must note that this evidence is suggestive because Bonica and Woodruff (2015) compute these measures elected for the first time until 2012. Therefore, the ideology of first-time elected judges after 2012 is missing. However, as long as there is no systematic selection of judges contesting re-elections based on their ideology (which seems plausible), the pattern documented suggests that right-leaning judges are more likely to retain their seats on the bench than left-leaning judges in states with judicial elections where the bans on independent expenditure were invalidated by the Citizens United ruling in 2010.

4 Productivity and Judiciary

4.1 Conceptual Framework

Consider a one factor and one good economy. Consumers derive utility from consuming the final good. The final good's production requires the factor f, which can also be considered an intermediate good. The price of the final good is the numeraire. The final

Table 10: Effect on Ideology (Common Space CFscore)

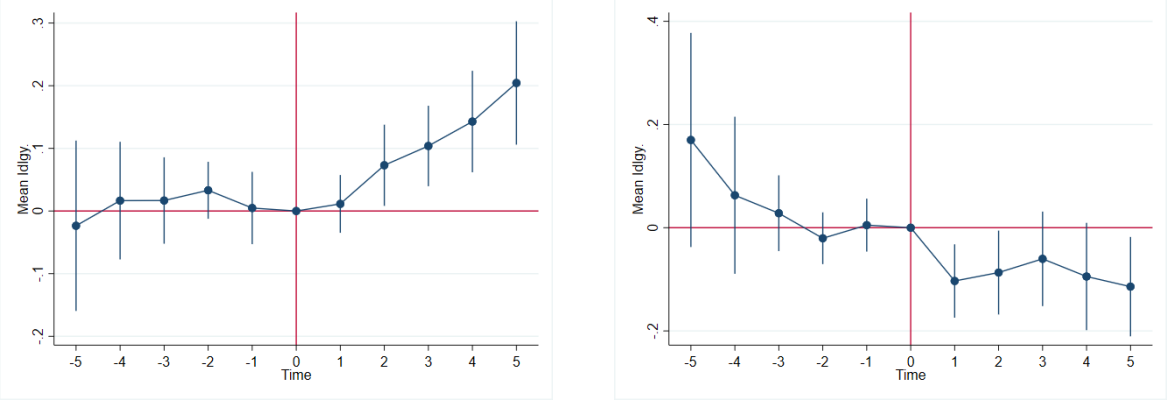
Note: This table presents the estimation results of equation (1). Columns (1)-(2) show results with median ideology (CFscore) of the judicial bench, which takes values in $(0, 100)$, with higher values indicating a more liberal ideology of a given state s in year t as the dependent variables. Columns (3)-(4) show results with the standard deviation of the ideology (CFscore) of the judicial bench, which takes values in $(0, 100)$, with higher values indicating a more liberal ideology of a given state s in year t as the dependent variable. Variables *Elect* indicates states with judicial elections for state supreme court judges, *Ban* indicates the states that had imposed a ban on independent expenditures by unions or corporations, which were rendered unconstitutional by the Supreme Court in 2010. Variable *Contract* indicates sectors with high reliance on contract enforcement. All regressions include state and year fixed effects. Standard errors are clustered at the state level.

	(1) Mean Ideology	(2) Mean Ideology	(3) SD Ideology	(4) SD Ideology
Ban × Post	-0.15	-0.25**	-0.04	-0.01
	(0.16)	(0.13)	(0.09)	(0.09)
Election × Ban × Post	0.26	0.37**	0.09	0.06
	(0.15)	(0.15)	(0.11)	(0.11)
State FE	N	Y	N	Y
Year FE	N	Y	N	Y
N	993	993	972	972
R-sq.	0.06	0.84	0.10	0.73

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: This figure presents the estimation results of equation (3). The dependent variable is the mean ideology (CFscore) of the judicial bench, which takes values in $[-2, 2]$, with higher values indicating a more right-leaning ideology for a given state s in year t. Panel (a) shows results for states with judicial elections for state Supreme Court judges, and Panel (b) for states that use some form of appointment. The figures indicate the coefficients and 90% confidence intervals that illustrate the dynamic effects on ideology due to the lifting of the bans imposed on independent expenditures by unions or corporations, which were rendered unconstitutional by the Supreme Court in 2010. Event Time, 0 is the year 2010. All estimations include state and year fixed effects. Standard errors are clustered at the state level.



(a) States With Judicial Elections

(b) States Without Judicial Elections

Figure 8: Effect on Mean Judicial Bench Ideology (CFscore) Bonica and Woodruff (2015)

good is produced in a perfectly competitive final goods market according to the production function,

$$y = f^{\alpha}$$

The price of the factor is p_f. I assume that the inputs to the final good are relationship-specific and susceptible to hold-up. In other words, the producer of the final good and the supplier of the intermediate good write contracts to overcome the hold-up problem as in Grossman-Hart-Moore. However, contracts are incomplete, and a contractual dispute may arise with probability $\pi \in [0, 1]$. Conditional on a dispute, the final goods producer is at fault with probability $q = 0.5$, and the court's accuracy is $p \in [0, 1]$. A higher p signifies a well-functioning court. We assume that the cost of an incorrect court decision is asymmetric and the final goods producer incurs a cost $cp_f f > 0$ (or net cost).[8] Such an assumption is reasonable if we assume that the supplier of the factor faces limited liability or that courts impose punitive damages on the defendant but rarely on the plaintiff, even if the decision is in favor of the defendant. Therefore, the final goods sector chooses the input f to maximize,

$$\Pi = \max_f f^{\alpha} - p_f f \cdot \underbrace{\left(1 + \frac{\pi}{2}(1-p)c\right)}_{\tau}$$

The distortion τ disappears if contracts are complete $\pi \to 0$ or the courts are accurate $p \to 1$. The production of the final good

$$f^* = \left(\frac{\alpha}{p_f \tau}\right)^{\frac{1}{1-\alpha}}$$

Consumers' indirect utility from the consumption of the final good decreases if the distortion τ increases.

$$\log(f^*) = \frac{1}{1-\alpha}\left(\log\left(\frac{\alpha}{p_f}\right) - \log(\tau)\right) \propto -\log(\tau)$$

4.1.1 Electoral Accountability

The distortion in input choice is due to biased courts. Production efficiency will improve if such distortion is lower. Next, I outline the role of elections and campaign finance in the

[8] Consider that the cost on the producer is c^p and that on the supplier is $c^f < c^p$, then $\tau = 0.5\pi(1 - p)(c^p - c^f)$.

reduction of distortion due to the inferior quality of courts. Prior evidence from Bonneau and Hall (2009), Hall (2007), and Hall (2001) suggests that attack ads and competitive election campaigns increase the voters' scrutiny. I argue that less restrictive campaign finance improves access to information and voters' sensitivity to court conduct, which makes the judges more accountable. This naturally leads to better judicial conduct and reduces distortions in the production process. In this subsection, I demonstrate this accountability mechanism through a simple conceptual framework based on Persson and Tabellini (2002) model of electoral accountability where voters observe a noisy signal about the incumbent's action.

Consider an incumbent judge I, seeking re-election. The judge inherently wants to decide cases based on his ideological leanings, which decreases accuracy and increases distortion τ. This distortion affects the input choice. Using insights from the previous section, I model the voter's preferences using the indirect utility derived earlier. Mainly, voters prefer less distortion. The judge, through his conduct, wants to maximize his utility, which depends on rents from ideological decision-making $r \leq \overline{r}$ and ego rents from political office summarized in $R > 0$. There are 3 periods to model the accountability mechanism of elections. The judge has CRRA preferences with risk-aversion parameter $\gamma = 2$, i.e. $u(r) = -1/r$. The judge maximizes the expected utility,

$$\max_{r_1, r_2} -\frac{1}{r_1} + Pr(I \text{ wins}) \cdot \left(-\frac{1}{r_2 + R} \right)$$

The timing of the game is as follows:

1. $t = 0$, the judge I chooses r_1.

2. $t = 1$, the political alignment variable η^I is realized, the challenger enters the race, the voters observe a noisy measure of the distortion

$$\tilde{\tau} = \underbrace{\eta^I r_1}_{\tau} \cdot \underbrace{\nu}_{\text{noise}}$$

. A lower value of alignment η^I signifies a stronger alignment and lower distortion despite a high r. They vote to maximize their utility, taking the average alignment of the challenger as given.

3. $t = 2$, the election results are out, the winning judge chooses r_2, voters earn their utility, and the game ends.

The voters' utility is given by

$$w(\tau) = -\log(\tau).$$

Note that at the time of choosing r_1, the candidate does not know the alignment η^I. However, he anticipates that the voters do not observe η^I but infer the level of rents from the observed distortion $\tilde{\tau}$ and vote. η^I and ν are independent and drawn from the log-normal distribution.

$$\log(\eta) \sim \mathcal{N}(\mu_\eta, \sigma_\eta^2) \ , \ \log(\nu) \sim \mathcal{N}(0, \sigma_\nu^2).$$

I solve the model by backward induction. At $t = 2$, a judge of any type chooses the highest possible rent $r_2 = \overline{r} = r_2^I = r_2^C$. Let the second-period payoff of the judge be $\tilde{R} = -1/(R + \overline{r})$. The probability that the incumbent wins is,

$$\begin{aligned} Pr(I \text{ wins}) &= Pr(E[w(\tau_2^I|\tilde{\tau}_1)] > E[w(\tau_2^C|\eta^C)]) \\ &= Pr(E[\log(\eta^I)|\tilde{\tau}_1] < E[\log(\eta^C)]) \\ &= Pr(E[\log(\eta^I)|\tilde{\tau}_1] < \mu_\eta) \end{aligned}$$

The voters draw an inference about the candidate's ability (or alignment) based on the observed signal $\tilde{\tau}$ and vote for the incumbent if he promises a higher utility than the unconditional expected alignment with the challenger C. Note that the voters update their beliefs about η^I conditional on observing the signal $\tilde{\tau}$. They learn in a Bayesian manner as in DeGroot (1974). Therefore,

$$\log(\eta^I)|\tilde{\tau} \sim \mathcal{N}(\bar{\eta}(\tilde{\tau}_1), \bar{\sigma}^2(\tilde{\tau}_1))$$

where,

$$\bar{\eta}(\tilde{\tau}_1) = (\log(\tilde{\tau}_1) - E[\log(r_1)]) \cdot \frac{\sigma_\eta^2}{\sigma_\eta^2 + \sigma_\nu^2} + \mu_\eta \cdot \frac{\sigma_\nu^2}{\sigma_\eta^2 + \sigma_\nu^2}$$

The incumbent takes this belief as given and chooses r_1 to maximize the payoff at $t = 0$. The probability that the incumbent wins conditional on choosing r_1 is

$$Pr(I \text{ wins}) = Pr\left((\log(r_1) + \log(\eta\nu) - E[\log(r_1)] - \mu_\eta) < 0\right)$$
$$= \Phi\left(\frac{\log(r_1) - E[\log(r_1)]}{\sqrt{\sigma_\eta^2 + \sigma_\nu^2}}\right)$$

The incumbent chooses r_1 conditional on the probability of winning. The first order condition with respect to r_1 and the rational expectations, $\log(r_1^*) = E[\log(r_1^*)]$ results

$$r_1^* = \frac{1}{\phi\left(\frac{\log(r_1^*) - E[\log(r_1^*)]}{\sqrt{\sigma_\eta^2+\sigma_\nu^2}}\right)\tilde{R}} = \frac{\sqrt{2\pi(\sigma_\eta^2 + \sigma_\nu^2)}}{\tilde{R}}$$

Making campaign finance less restrictive increases competition by improving the information of the voters so that σ_ν is lower. This makes the voters more sensitive to the incumbent's choice and induces the incumbent to choose lower rent r_1^*, which in turn improves the expected payoff of voters in equilibrium $\mu_\eta - \log(r_1^*)$. Thus, more competitive judicial elections increase the accountability of judges and prevent them from judging cases against the voters' interests.

In appendix D, I adapt the Hopenhayn (1992) model to a static setting and show how a reduction in average distortion in input choice leads to higher average labor productivity, higher employment, and increased entry. In the next subsection, I test whether less restrictive campaign finance leads to higher productivity and employment. I also test whether the effects are stronger for sectors where incomplete contract risk is higher, such as more downstream sectors that employ a variety of inputs from different sectors and are more dependent on contract enforcement for production.

4.2 Productivity and Campaign finance

In this section, I examine the effect of less restrictive campaign finance due to the Citizens United ruling on labor productivity. I measure labor productivity as the total value added in USD per worker. I estimate the following regression specification and highlight the heterogeneous effect on productivity conditional on the judicial selection procedure, i.e.

election v. non-election.

$$y_{jst} = \beta_{eb} \cdot (\text{Election}_s \times \text{Post}_t) + \beta_{bp} \cdot (\text{Ban}_s \times \text{Post}_t) + \beta_{ebp} \cdot (\text{Election}_s \times \text{Ban}_s \times \text{Post}_t) \\ \delta_{sj} + \delta_{jt} + \beta_{ST} \cdot (s \times t) + \varepsilon_{jst} \quad (5)$$

where y_{jst} is the dependent variable of interest for the 4-digit NAICS sector j in state s at time t. I control for state-sector fixed effects. Therefore, I control for factors such as the suitability of geographic conditions, administrative organization, historical factors that may lead to differences in productivity of a sector in a given state. I also allow for state and sector-specific time trends. β_{bp} and $\beta_{bp} + \beta_{ebp}$ are the coefficients of interest.

Table (11) illustrates the effect of the Citizens United ruling on labor productivity. Column (1) shows that labor productivity increases by $\$13,000$ per worker in treated states. Columns (2) and (3) show the heterogeneous effect in the judicial election and non-election states and that within-sector productivity increases significantly in states with judicial elections after the removal of bans on independent expenditures. Column (3) tests for the significance of coefficient estimates ($\beta_{bp} + \beta_{ebp}$), which represents the effect on productivity in states with judicial elections. Productivity increases by $\$21,000$, which corresponds to an 8% increase in productivity over the pre-period average. Column (4) shows that the results are robust to controlling for state-by-sector fixed effects. Column (5) illustrates that the effect on productivity is robust to state-specific linear time trends, indicating that the effect on productivity does not pick up differential productivity trends across the states.

I verify that the increase in labor productivity is not driven by the more intensive utilization of the labor force. In Table (12), I show that the increase in productivity measured as value added per hour of labor also increases by 9% which is in line with evidence in Table (11) for value-added per worker.

4.2.1 Dynamic Effect on Labor Productivity

The effect on labor productivity could be due to differential trends in the pre-period. The effect of the removal of bans on independent expenditure, estimated in Equation (5), will be biased if there are pre-period differential trends in labor productivity. To test whether there are pre-period trends, I estimate the following dynamic event-study specification where D_t

Table 11: Effect on Labor Productivity (USD/emp)

Note: This table presents the estimation results of equation (5) The dependent variable is labor productivity, measured as value added in USD 1000s per worker. Variables *Elect* indicates states with judicial elections for state supreme court judges, *Ban* indicates the states that had imposed a ban on independent expenditures by unions or corporations, which were rendered unconstitutional by the Supreme Court in 2010. The specifications in each column vary depending on the inclusion of state-by-sector, and sector-by-year fixed effects. Sector is defined at the 4-digit NAICS code level. Standard errors are clustered at the state level.

	(1)	(2)	(3)	(4)	(5)
	Labor Productivity	Labor Productivity	Labor Productivity	Labor Productivity	Labor Productivity
Ban × Post	11.08*	-7.27			
	(6.21)	(10.58)			
Election × Ban × Post		28.75*	21.46**	19.67**	24.51**
		(14.72)	(10.22)	(8.98)	(10.79)
State FE	-	Y	Y	-	-
State × Sector FE	Y	N	N	Y	Y
Sector ×Year FE	Y	Y	Y	Y	Y
State Time Trend	N	N	N	N	Y
N	36,324	36,348	36,348	36,324	36,324
R-sq.	0.75	0.47	0.47	0.75	0.75

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 12: Effect on Labor Productivity (USD/hr)

Note: This table presents the estimation results of equation (5) The dependent variable is labor productivity, measured in USD value added per hour. Variables *Elect* indicates states with judicial elections for state supreme court judges, *Ban* indicates the states that had imposed a ban on independent expenditures by unions or corporations, which were rendered unconstitutional by the Supreme Court in 2010. The specifications in each column vary depending on the inclusion of state-by-sector, and sector-by-year fixed effects. Sector is defined at the 4-digit NAICS code level. Standard errors are clustered at the state level.

	(1)	(2)	(3)	(4)	(5)
	Lab Prod (USD/hr)	Lab Prod (USD/hr)	Lab Prod (USD/hr)	Lab Prod (USD/hr)	Lab Prod (USD/hr)
Ban × Post	6.62**	-3.00			
	(3.15)	(5.21)			
Election × Ban × Post		15.61**	12.60**	11.13**	12.30**
		(7.65)	(5.58)	(4.79)	(5.29)
State FE	-	Y	Y	-	-
State × Sector FE	Y	N	N	Y	Y
Sector ×Year FE	Y	Y	Y	Y	Y
State Time Trend	N	N	N	N	Y
N	36,670	36,687	36,687	36,670	36,670
R-sq.	0.73	0.46	0.46	0.73	0.73

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

is the indicator for the year relative to 2010, the year of the Citizens United ruling. Figure (9) provides evidence that there are no pre-period trends in labor productivity and the difference between the productivity across states where the bans were lifted and where the ruling had no change in campaign finance laws is not significantly different than the difference in the year 2010. Panel (a) illustrates the increase in productivity for states with judicial elections after Citizens United, and Panel (b) shows that there is no effect on labor productivity in states where judges to the high court are not elected. Figure (19) in the Appendix shows that the results also hold for productivity measured as value added per hour of labor.

$$y_{jst} = \delta_{sj} + \delta_{jt} + \sum_{\tau=-5}^{T} \beta_\tau \cdot Ban_s \times D_{t-\tau} + \varepsilon_{jst} \tag{6}$$

4.3 Effect on Investment and Employment

The increase in labor productivity may be due to the firms facing lower employee termination costs, possibly following a decline in the collective bargaining power of workers. This should be reflected in a decline in the wages of workers or lower employment as unproductive workers are fired. On the other hand, the productivity improvement could be due to the reduction in legal friction and more efficient utilization of factors. If such a reduction of legal or contractual frictions is at play, then we should observe a rise in the human and physical capital growth rates, i.e., the growth rate of capital expenditure and production workers should be higher. The revenue per unit cost of material may also be higher, indicating more productive use of inputs. We test for these by estimating the equation (5), and the results are illustrated in Table (13).

The coefficient estimates in Table (13) are for the effect on the dependent variable conditional on the state electing their supreme court judges. In Columns (1) through (3), I provide evidence that the physical and human capital growth rates and the revenue per material cost are higher in states with judicial elections. Moreover, in Column (4) I illustrate the effect on average wage, i.e. the total wage bill divided by the number of employees. If the collective bargaining power of the workers is lower, we expect a significant decline in the wage rates. There is a 1% decline in wages; however, the coefficient

Note: This figure presents the estimation results of equation (10). The dependent variable is labor productivity, measured as value added in USD 1000 per employee. Panel (a) shows results for states with judicial elections for state Supreme Court judges, and Panel (b) for states that use some form of appointment. The figures indicate the coefficients and 90% confidence intervals that illustrate the dynamic effects on labor productivity due to the lifting of the bans imposed on independent expenditures by unions or corporations, which were rendered unconstitutional by the Supreme Court in 2010. Event Time, 0, is the year 2010. All estimations include state and sector-by-year fixed effects. A sector is defined at the 4-digit NAICS code level. Standard errors are clustered at the state level.

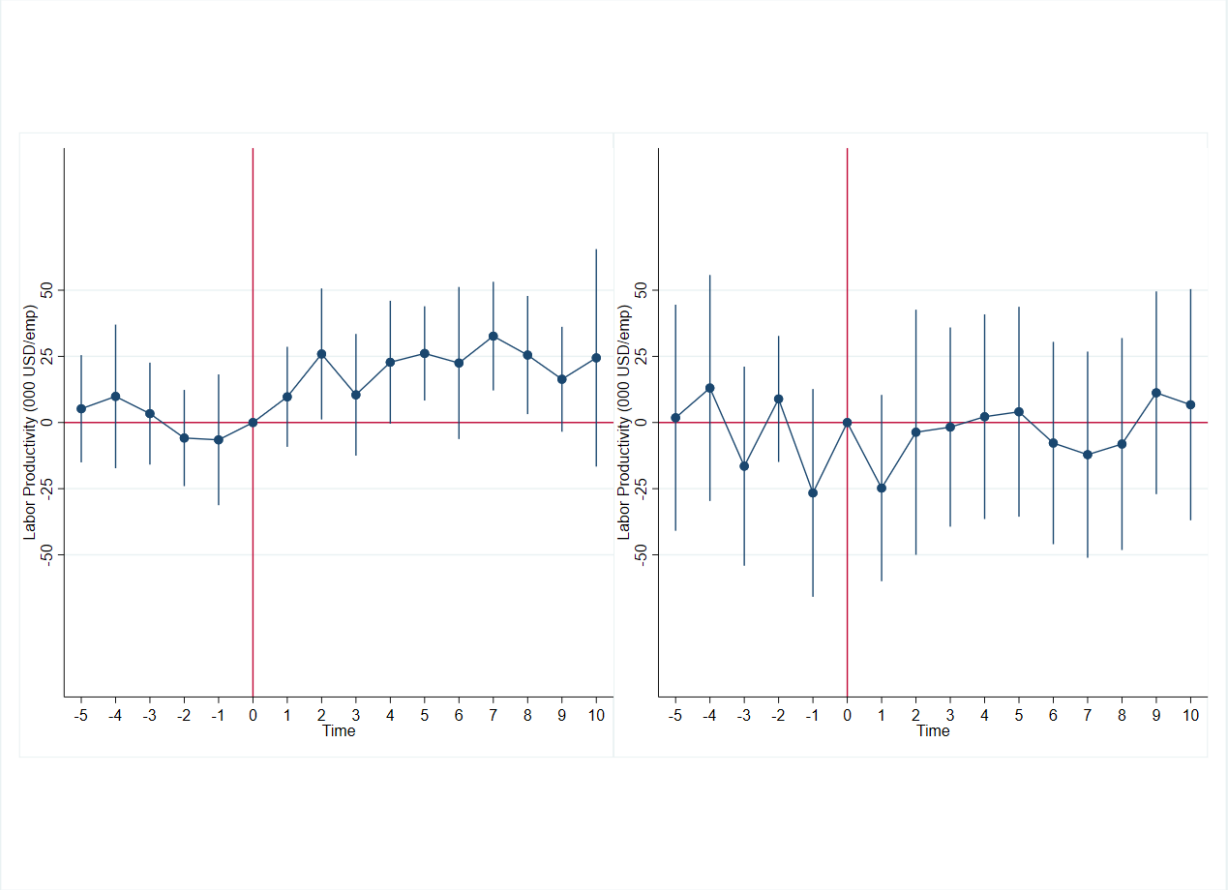


Figure 9: Effect on Labor Productivity in '000 USD /emp: States with Judicial Elections (Left), and without judicial elections (Right)

estimate is not statistically significant. All results in Table (13) are robust to controlling for state-specific time trends. In Appendix G, I estimate the effect on labor productivity and employment conditional on the state having Right-to-Work (RTW) laws. States with Right-to-Work laws have lower unionization rates and financially constrained labor unions Fortin, Lemieux and Lloyd (2023), i.e., the unions are weaker in states with RTW laws. I find that labor productivity and employment are higher in states with weaker unions after Citizens United. This suggests that Citizens United did not curtail the bargaining power of unions but allowed firms to take advantage of reduced legal friction in states where unions were weaker ex-ante. Therefore, the evidence seems more consistent with the efficient utilization of factors stemming from a reduction in legal friction than explanations related to a decline in the bargaining power of workers relative to firms. In Online Appendix (B), I show that after Citizens United, the rate of civil cases (mainly contract-related) from the State Supreme Courts appealed to the United States Supreme Court is lower for treated states with judicial elections. This provides suggestive evidence of the improvement in decision quality in State Supreme Courts with judicial elections where bans on independent expenditure were invalidated after Citizens United.

Table 13: Effect on Capital Expenditure, Employment, and Wages

Note: This table presents the estimation results of equation (5) The dependent variable is the Capital Expenditure, measured in USD 1000s, Employment, Revenue per dollar of material input costs and Wage measured in USD 1000 per worker. Variables *Elect* indicates states with judicial elections for state supreme court judges, *Ban* indicates the states that had imposed a ban on independent expenditures by unions or corporations, which were rendered unconstitutional by the Supreme Court in 2010. All regressions include state-by-sector, sector-by-year fixed effects, and state-specific linear time trends. Sector is defined at the 4-digit NAICS code level. Standard errors are clustered at the state level.

	(1)	(2)	(3)	(4)
	log(CapEx)	log(Emp)	Rev/Mat Cost	wage (000 USD)
Election × Ban × Post	0.07**	0.05*	0.07**	-0.43
	(0.03)	(0.03)	(0.03)	(0.50)
State × Sector FE	Y	Y	Y	Y
Sector × Year FE	Y	Y	Y	Y
N	32,743	36,324	33,671	36,324
R-sq.	0.88	0.96	0.69	0.80

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

4.4 Establishment Entry and Reallocation

If the change in campaign finance laws leads to more productive factor allocation, will such effects also translate to entry and exit? For example, does a more conducive legal environment with reduced contractual frictions affect the choices of entry and exit of establishments and the job reallocation rates? In this section, we test whether the changes in the campaign finance laws affect establishment entry, exit, and job reallocation. I employ a different dataset compiled by the Census Bureau to answer this question. The publicly available sample of the Annual Survey of Manufactures data does not provide the number of establishments or firms at the state-sector level. Hence, I use the business statistics and dynamics data, which provides establishment entry, exit, and job reallocation rates at the 2-digit NAICS level by states at an annual frequency. In Table (14), I document a 0.55 pp. increase in establishment entry in states with judicial elections. This represents an approximately 5% increase in establishment entry. On the other hand, there is neither an economically (0.6%) nor statistically significant effect on establishment exit rates. Table (15) illustrates the increase in job reallocation rates of 1.14 pp.(4% over the pre-period mean of 30.3%), mainly driven by job creation rates 0.6 pp. (4% over the pre-period mean of 15.8%). Around 50% of this increase in reallocation is due to the continuing establishments, and the remaining is due to the net effect of new entrants and the establishments that die off. The evidence suggests that a change in the legal environment seems to foster business dynamism. In Appendix D, I show that the reduction in contractual or legal frictions encourages entry as it becomes more profitable for firms to operate.

Table 14: Effect on Establishment Entry, Exit, and Employment

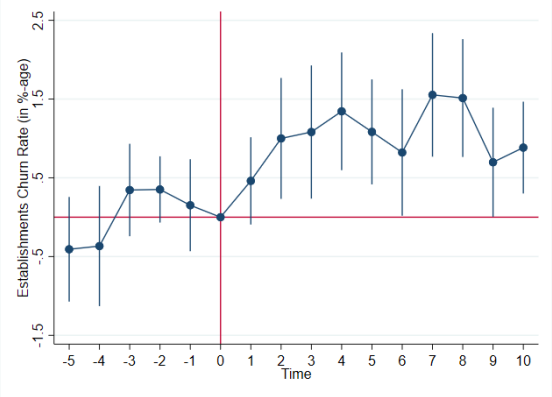
Note: This table presents the estimation results of equation (5). The dependent variables in Columns(1)-(3) are the logarithm of the number of firms, employees, establishments respectively. Columns (4) and (5) show results for establishment entry and exit rates (in %-age) from the Business Statistics Table of the US Census Bureau. Variables *Elect* indicates states with judicial elections for state supreme court judges, *Ban* indicates the states that had imposed a ban on independent expenditures by unions or corporations, which were rendered unconstitutional by the Supreme Court in 2010. The specifications in each column vary depending on the inclusion of state-by-sector, and sector-by-year fixed effects. Sector is defined at the 2-digit NAICS code level. Standard errors are clustered at the state level.

	(1)	(2)	(3)	(4)	(5)
	log(Firms)	log(Emp.)	log(Estab.)	Estab. Entry Rate	Estab. Exit Rate
Election × Ban × Post	0.03**	0.05***	0.03**	0.54**	0.07
	(0.01)	(0.02)	(0.01)	(0.22)	(0.25)
State FE	Y	Y	Y	Y	Y
Sector ×Year FE	Y	Y	Y	Y	Y
State Time Trend	Y	Y	Y	Y	Y
N	20,900	20,900	20,900	20,714	20,685
R-sq.	0.90	0.90	0.90	0.53	0.70

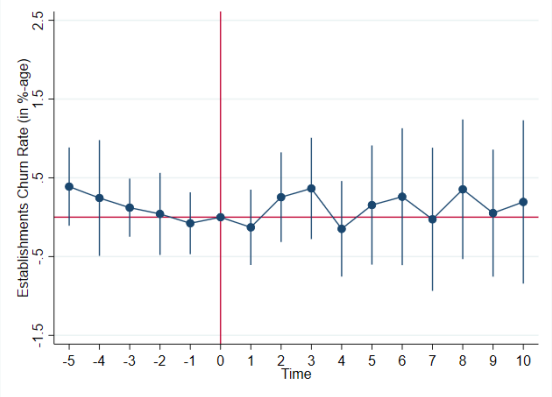
Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: This figure presents the estimation results of equation (10). The dependent variable is the logarithm of labor productivity, measured as USD sales worker. Panel (a) shows results for sectors with high contract reliance and Panel (b) for sectors with high contract reliance. The figures indicate the coefficients and 95% confidence intervals that illustrate the dynamic effects on labor productivity due to the lifting of the bans imposed on independent expenditures by unions or corporations, which were rendered unconstitutional by the Supreme Court in 2010. Event Time, 0, is the year 2010. All estimations include state and sector-by-year fixed effects. The sector is defined at the 4-digit NAICS code level. Standard errors are clustered at the state level.



(a) States with Judicial Elections



(b) States without Judicial Elections

Figure 10: Dynamic effect on the Business Churn rate, sum of entry and exit rates of establishments.

Table 15: Job Reallocation Rates

Note: This table presents the estimation results of equation (5). The dependent variables in Columns(1)-(3) are the job creation, destruction, and the sum of the creation and destruction rates, the reallocation rate. Columns (4) captures the job reallocation rate from continued establishments. All rates are in %-age. Data comes from the Business Statistics Table of the US Census Bureau. Variables *Elect* indicates states with judicial elections for state supreme court judges, *Ban* indicates the states that had imposed a ban on independent expenditures by unions or corporations, which were rendered unconstitutional by the Supreme Court in 2010. Sector is defined at the 2-digit NAICS code level. Standard errors are clustered at the state level.

	(1)	(2)	(3)	(4)
	Creatn. Rate	Destructn. Rate	Reallocn. Rate	Reallocn. Rate (Contd. Estab.)
Election × Ban × Post	0.60***	0.53	1.14**	0.55**
	(0.19)	(0.37)	(0.43)	(0.27)
State FE	Y	Y	Y	Y
Sector ×Year FE	Y	Y	Y	Y
N	20,900	20,900	20,900	20,571
R-sq.	0.43	0.43	0.51	0.50

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

5 Institutional Reliance: Contract Enforcement

I have documented that the average sector-level labor productivity increases in states where there were bans on independent expenditure after the Citizens United ruling in 2010. This effect is driven by establishments operating in states with judicial elections. I have abstracted from the heterogeneous treatment effect across sectors. However, if productivity improvements are indeed driven by changes in the legal environment, then the sectors that are more reliant on legal institutions, such as those facing incomplete contracts risk, must experience a more pronounced effect on productivity. I test this hypothesis in this section.

Recall from the discussion in section 4.1 that the legal distortion due to inaccurate and biased decision-making by the judiciary is stronger for sectors where a contractual dispute is more likely or where the inputs are relationship-specific and susceptible to hold up. This idea of more reliance on contracts based on relationship-specificity of inputs or product complexity is based on the international trade literature, Levchenko (2007) and Nunn (2005). The key idea is that if the product is complex, i.e., it requires inputs from several sectors, the producers of this complex good are more exposed to incomplete contracts risk. Such sectors are more susceptible to hold-up than others with fewer inputs or a less complex production process and, therefore, are more reliant on legal institutions for production.

The measure of institutional reliance is the inverse of the Herfindahl-Hirschman Index (HHI) of inputs in the spirit of Levchenko (2007). Reliance on dispersed suppliers indicates a more complex production process. I compute the measure using the input-output matrices compiled by the Bureau of Economic Analysis (BEA) for the year 2000 (before the start of the sample). More details about the measure, its computation, and comparison with Levchenko (2007) are in Appendix B.

$$inputhhi_i = \sum_{j=1}^{N} \left(\frac{E_{ij}}{E_i} \right)^2 \text{ where } E_i = \sum_{j=1}^{N} E_{ij}$$

where E_{ij} is the amount of input sourced by industry i from industry j. The industry distinction is at the 4-digit NAICS level. Higher HHI of inputs corresponds to firms with lower incomplete contracts risk. Therefore, for a measure of institutional reliance, I trans-

form the measure as $contint = -\log(inputhhi)$ and an indicator variable for the top tercile of institutional reliance, $Contract = \mathbb{1}(contint > \tau_{\{2/3\}})$ (top tercile) where $\tau_{\{2/3\}}$ denotes the 66-th percentile of *contint*.

To test whether the productivity effects are stronger for institutionally reliant sectors, I estimate the following regression equation,

$$\begin{aligned} y_{jst} = {} & \beta_{bp} \cdot (Ban \times Post) + \beta_{ebp} \cdot (Election \times Ban \times Post) \\ & + \beta_{bhp} \cdot (Ban \times Contract \times Post) + \beta_{ebhp} \cdot (Election \times Ban \times Contract \times Post) \\ & + \mathcal{I}(Election, Ban, Contract, Post) + \delta_{sj} + \delta_{jt} + \varepsilon_{jst} \end{aligned} \tag{7}$$

where $\mathcal{I}(Election, Ban, Contract, Post)$ includes all the 2 and 3 term interactions between the variables, except for those already specified in Equation (7). The coefficients of the variables (and different combinations of their sums), $\{Ban \times Post,\ Election \times Ban \times Post,\ Ban \times Post \times Contract,\ Election \times Ban \times Post \times Contract\}$ allow us to quantify the heterogeneous effects on productivity depending on whether the sector is more or less institutionally-reliant. Note that the above is not a triple difference-in-differences design. Sectors with greater and smaller reliance on legal institutions may follow different time trends. However, the regression specification estimates the heterogeneous effects of relaxing campaign finance laws on different sectors in a single estimation.

Table (20) in the Appendix (B) provides the estimation results for the above specification. However, it is more informative to compute the treatment effects by summing coefficients on the variables of interest. First, I define the different quantities of interest, and in Table (16), I test whether the effect of laxer campaign finance has heterogeneous effects on sectors with more or less reliance on legal institutions.

I am interested in the effect conditional on the selection procedure and the degree of institutional reliance of the sector, $\boldsymbol{X} = (Elect, Contract)$,

$$\begin{aligned} D(\boldsymbol{X}) = {} & (E[y_{kst}|Ban = 1, Post = 1, \boldsymbol{X}] - E[y_{kst}|Ban = 1, Post = 0, \boldsymbol{X}]) \\ & - (E[y_{kst}|Ban = 0, Post = 0, \boldsymbol{X}] - E[y_{kst}|Ban = 0, Post = 0, \boldsymbol{X}]) \end{aligned}$$

For example, the average treatment effect on industries with higher reliance on institutions

($Contract = 1$) in states without judicial elections is

$$D(\text{no election, Institutionally Reliant}) = D(Election = 0, Contract = 1) = \beta_{bhp} + \beta_{bp}$$

Table (16) illustrates how the labor productivity increase is statistically and economically significant for sectors with greater reliance on legal institutions (or exposure to incomplete contracts risk). Moreover, this increase is only observed for states that hold judicial elections for the State Supreme Court judges. The effects on physical capital and employment growth rates are also higher for more institutionally-reliant sectors. Column (5) estimates of the effect on wages display no such heterogeneous treatment effects for sectors depending on their institutional reliance. This suggests that a change in the workers' bargaining power is not the plausible cause for higher labor productivity. Figure (11) estimates Equation (10) on two different sub-samples, one with high institutional reliance sectors and the other with low institutional reliance sectors, to highlight the dynamic effect of the change in campaign finance laws on labor productivity. Panels (a) and (b) show the effect on states with judicial elections. The productivity improvement seems to be concentrated in institutionally-reliant sectors (Panel (a)). Panels (c) and (d) reaffirm the results from previous sections that the effect on labor productivity is mainly driven by states that hold judicial elections for the State Supreme Court.

Table 16: Treatment Effect Heterogeneity due to reliance on Contract Enforcement

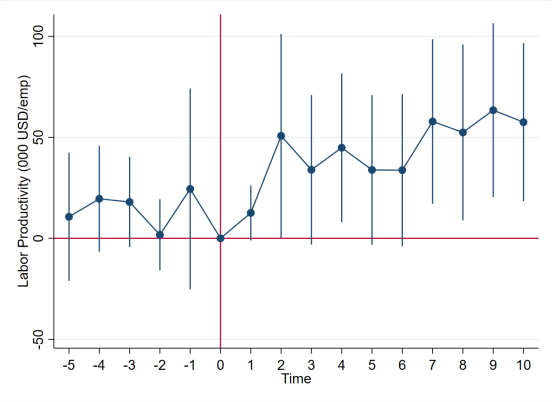
Note: This table presents the estimation results of equation (7) and the corresponding treatment effects for different sub-populations. Columns (1)-(4) show results with Labor Productivity in 1000 USD per worker, logarithm of Capital Expenditure, measured in USD 1000s, logarithm of Employment, and Wage measured in USD 1000 per worker as dependent variables. Variables *Elect* indicates states with judicial elections for state supreme court judges, *Ban* indicates the states that had imposed a ban on independent expenditures by unions or corporations, which were rendered unconstitutional by the Supreme Court in 2010. Variable *Contract* indicates sectors with high reliance on contract enforcement. All regressions include state, sector-by-year fixed effects, and state-specific linear time trends. Sector is defined at the 4-digit NAICS code level. Standard errors are clustered at the state level.

	(1)	(2)	(3)	(4)	(5)
	Labour Productivity	log(CapEx)	log(Emp)	Rev/Mat. Cost	Wage (USD/emp)
D(Contract = 1, Elect = 0)	5.73	-0.03	-0.03	-0.01	-0.15
$\beta_{bhp} + \beta_{bp}$	(0.44)	(0.66)	(0.40)	(0.90)	(0.80)
D(Contract = 0, Elect = 0)	3.04	0.01	0.01	0.00	0.34
β_{bp}	(0.85)	(0.86)	(0.69)	(0.92)	(0.55)
D(Contract = 1, Elect = 1)	32.27***	0.07	0.03	0.13*	-0.03
$\beta_{ebhp} + \beta_{bhp} + \beta_{ebp} + \beta_{bp}$	(0.00)	(0.15)	(0.41)	(0.09)	(0.97)
D(Contract = 0, Elect = 1)	4.90	0.06	0.05**	0.05**	-0.7
$\beta_{ebp} + \beta_{bp}$	(0.66)	(0.13)	(0.04)	(0.04)	(0.30)

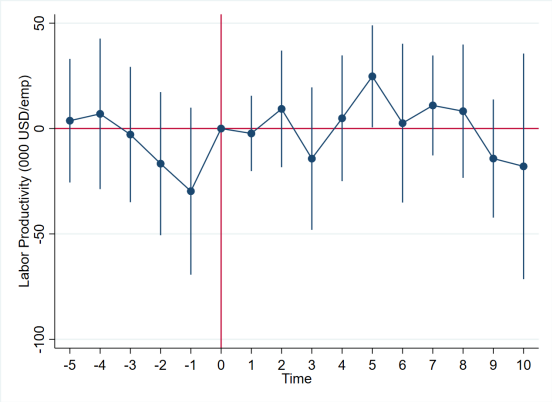
p-values in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

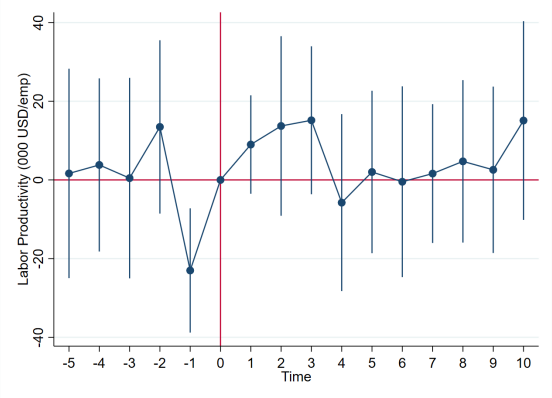
Note: This figure presents the estimation results of equation (10). The dependent variable is labor productivity, measured as USD value added per emp. Panels (a) and (b) show results for states with judicial elections for State Supreme Court judges and Panels (c) and (d) for states without judicial elections of the State Supreme Court. Panels (a) and (c) restrict the sample to sectors with high reliance on institutions, while Panels (b) and (d) restrict the sample to sectors with low reliance on institutions. The figures indicate the coefficients and 90% confidence intervals that illustrate the dynamic effects on labor productivity due to the lifting of the bans imposed on independent expenditures by unions or corporations, which were rendered unconstitutional by the Supreme Court in 2010. Event Time, 0 is the year 2010. All estimations include state and sector-by-year fixed effects. The sector is defined at the 4-digit NAICS code level. Standard errors are clustered at the state level.



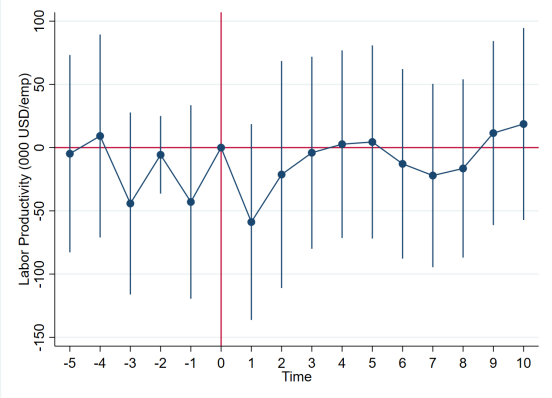
(a) High Incomplete Contracts Risk Sectors in States with Judicial Elections



(b) Low Incomplete Contracts Risk Sectors in States with Judicial Elections



(c) High Incomplete Contracts Risk Sectors in States without Judicial Elections



(d) Low Incomplete Contracts Risk Sectors in States without Judicial Elections

Figure 11: Effect on Labor Productivity in USD /emp

6 Conclusion

In this paper, I show that less restrictive campaign finance laws may increase expenditure and competition in judicial elections. The State Supreme Court judges influence the legal environment in many ways, particularly through the predictability and consistency in enforcing contract and administrative law in their state. Expensive and competitive elections strengthen the accountability mechanism of elections, which improves judicial performance and alleviates legal frictions faced by firms. I exploit the surprise ruling by the United States Supreme Court in the Citizens United v. FEC case for variation in campaign finance laws across states. The ruling invalidated bans on independent expenditures imposed by 23 states. After Citizens United, such states experienced more financially intensive elections, including judicial elections. First, I document the increased expenditure of judicial candidates in state supreme court elections. This increase was mainly driven by funding from businesses and political parties. Second, I document that expensive judicial election races are associated with increased competition. The number of candidates per judicial seat increased along with the decline in the vote margin of the winners. The incumbency advantage in judicial races also declined significantly. Finally, I show that the Citizens United ruling is associated with higher labor productivity, exclusively in states that elect their state supreme court judges. I establish that productivity improvement is due to the change in the legal environment following less restrictive campaign finance laws. I show that labor productivity improvement is concentrated in sectors more exposed to incomplete contracts risk and hence more reliant on the legal institutions for their production process. This paper provides evidence that allowing firms to donate more freely in elections affects the selection of judges, alleviates legal friction, improves productivity and resource allocation.

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A Appendix: Exogenous Treatment Assignment

Below I document, how the removal of bans is uncorrelated with crucial state-level characteristics and the dependent variables in the pre-period.

Table 17: Covariate Balance

	Mean (Treated)	Mean (Control)	Diff.	p-value
Pres. Total Votes	3,486,586	2,109,254	-1,377,333	(0.14)
Pres. Dem. Vote Share	49.3	48.4	-.98	(0.77)
Gov. Dem. Vote Share	49.3	43.5	-5.8	(0.29)
Median Ideology (PAJID)	32.99	48.06	-15.07	(0.16)
Mean Ideology (PAJID)	40.39	45.71	-5.31	(0.48)
Real GDP (in USD MM)	379,940	235,787	-144,153	(0.27)
Labor Income (in USD MM)	191,610	118,616	-72,994	(0.24)
Mean Income (HH)	60,662	60,593	-70	(0.98)
Median Income (HH)	45,983	45,119	-863	(0.72)
Fraction above 200k	2.55	2.69	.14	(0.70)
Fraction below 10k	8.07	8.82	.75	(0.27)
No. of HH	3,014,342	1,852,805	-1,161,537	(0.19)
Population above 18	6,035,970	3,754,700	-2,281,270	(0.22)
Prcnt HS grad	31.6	29.4	-2.2	(0.21)
Prcnt Bachelors	23.4	22.4	-1.1	(0.51)

B Appendix: Contract Reliance

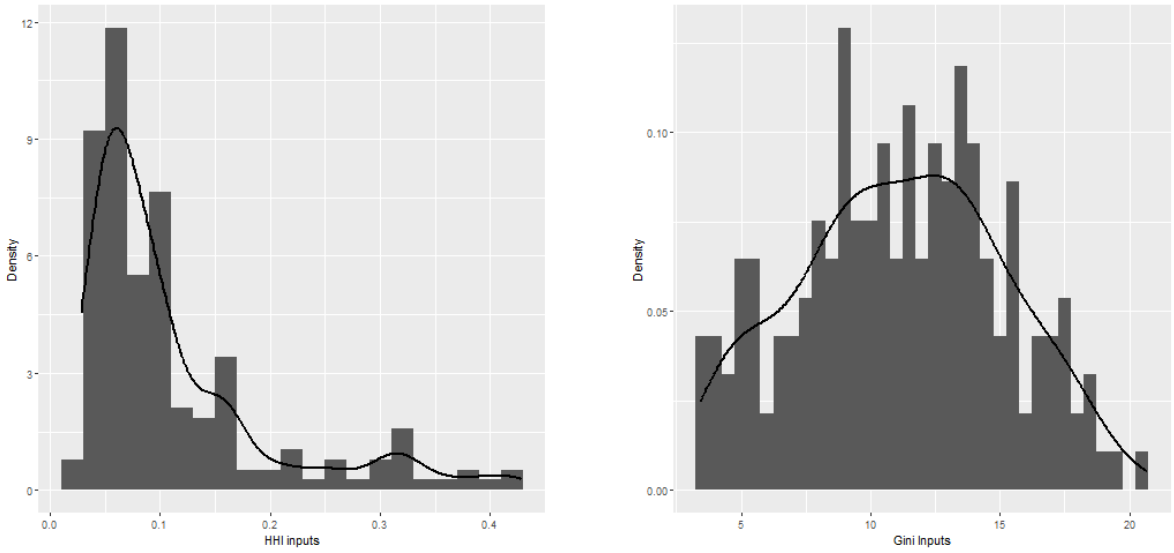
In this section, I show the results of the computations of the contract reliance measure described in the main text. I supplement the measure with another measure, the input Gini. All the results in the main text are robust to both measures. The *inputgini* is defined as,

$$inputgini_i = \frac{2}{N+1} \sum_{j=1}^{N} \sum_{k=1}^{j} \frac{E_{ik}}{E_i}$$

where E_{ik} are arranged in an ascending order and $E_i = \sum_{k=1}^{N} E_{ik}$. The higher the gini, the higher is the input specificity.

Least Contract Intensive			Most Contract Intensive		
Sr.No	NAICS	Industry	Sr.No	NAICS	Industry
1	3252	Resin, synthetic rubber, and artificial synthetic fibers and filaments manufacturing	1	3391	Medical equipment and supplies manufacturing
2	3311	Iron and steel mills and ferroalloy manufacturing	2	3333	Commercial and service industry machinery manufacturing, including digital camera manufacturing
3	3315	Foundries	3	3274, 3279	Lime, gypsum and other nonmetallic mineral product manufacturing
4	3115	Dairy product manufacturing	4	323	Printing and related support activities
5	3251	Basic chemical manufacturing	5	3271	Clay product and refractory manufacturing
6	3361	Motor vehicle manufacturing	6	3351	Electric lighting equipment manufacturing
7	3313	Alumina and aluminum production and processing	7	3327	Machine shops; turned product; and screw, nut, and bolt manufacturing
8	3117	Seafood product preparation and packaging	8	3371	Household and institutional furniture and kitchen cabinet manufacturing
9	3314	Nonferrous metal (except aluminum) production and processing	9	3332	Industrial machinery manufacturing
10	3222	Converted paper product manufacturing	10	3366	Ship and boat building

Table 18: Least and Most Contract Reliant Industries



(a) Density HHI inputs

(b) Density Gini inputs

Figure 12: Density of Contract intensity measures

	Mean	SD	Median	ρ_{gini}
Input HHI	0.136	0.107	0.096	-0.780
Levchenko (2007)	0.133	0.093		-0.742

Table 19: Summary Statistics of Input HHI

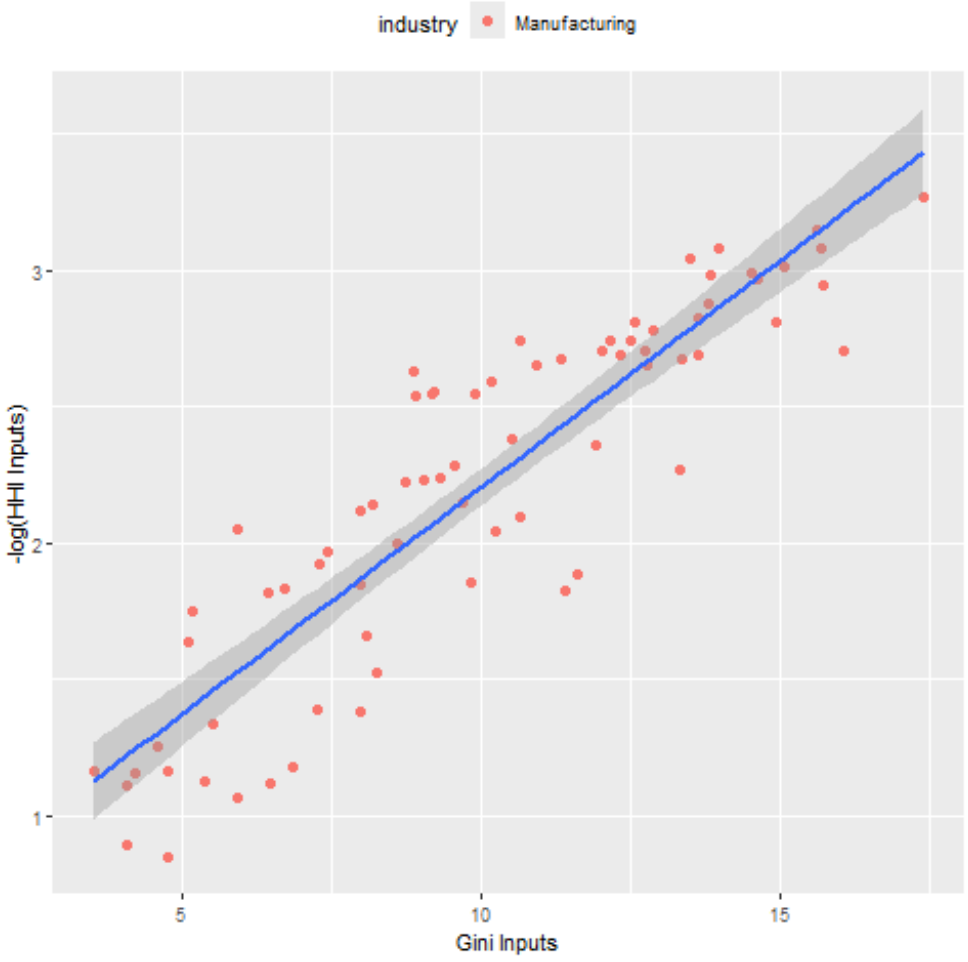


Figure 13: Relationship between Contract intensity Measures

Table 20: Heterogeneous Effect due to reliance on Contract Enforcement

Note: This table presents the estimation results of equation (7). Columns (1)-(4) show results with Labor Productivity in 1000 USD per worker, logarithm of Capital Expenditure, measured in USD 1000s, logarithm of Employment, and Wage measured in USD 1000 per worker as dependent variables. Variables *Elect* indicates states with judicial elections for state supreme court judges, *Ban* indicates the states that had imposed a ban on independent expenditures by unions or corporations, which were rendered unconstitutional by the Supreme Court in 2010. Variable *Contract* indicates sectors with high reliance on contract enforcement. All regressions include state, sector-by-year fixed effects, and state-specific linear time trends. Sector is defined at the 4-digit NAICS code level. Standard errors are clustered at the state level.

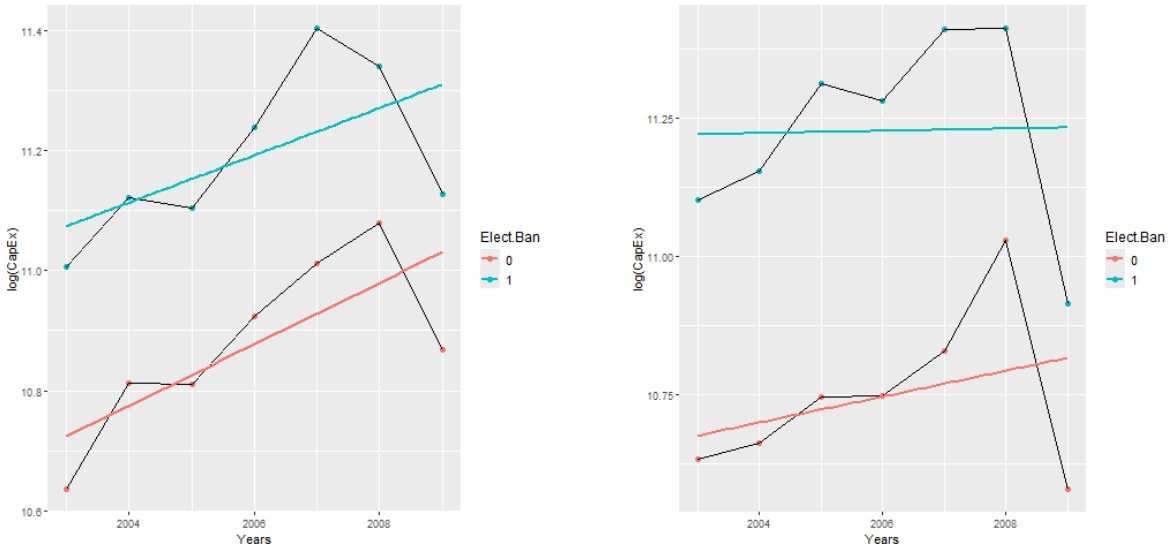
	(1)	(2)	(3)	(4)	(5)
	Lab Prod (USD/emp)	log(CapEx)	log(Emp)	Rev/Mat Cost	wage (USD/emp)
Ban × Post	3.04	0.01	0.01	0.00	0.35
	(15.65)	(0.05)	(0.03)	(0.04)	(0.58)
Elect × Ban × Post	1.86	0.05	0.04	0.04	-1.04
	(19.06)	(0.06)	(0.04)	(0.05)	(0.88)
Ban × Post × Contract	2.69	-0.04	-0.04	-0.01	-0.49
	(16.82)	(0.07)	(0.03)	(0.07)	(0.58)
Elect × Ban × Post × Contract	24.69	0.05	0.02	0.09	1.16
	(22.07)	(0.08)	(0.04)	(0.10)	(0.99)
State × Sector FE	Y	Y	Y	Y	Y
Sector × Year FE	Y	Y	Y	Y	Y
N	31,928	29,066	31,968	29,740	31,968
R-sq.	0.73	0.87	0.96	0.69	0.77

6nlStandard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

B.1 Pre Trends: Contract Reliant vs. Non-contract Reliant industries

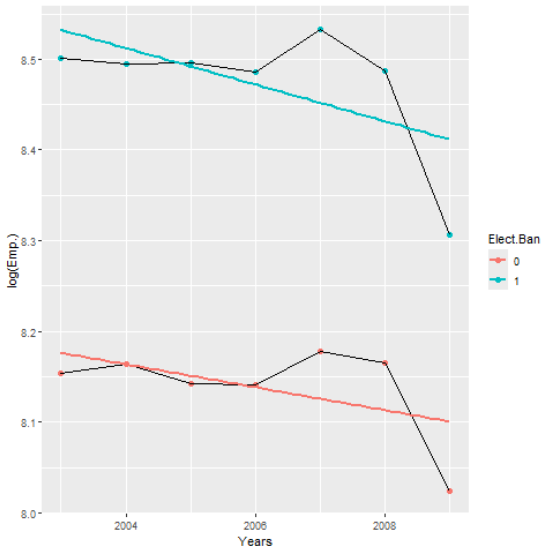
In this subsection, I document the pre-trends in capital expenditure, employment, and wages during the pre-period 2005-2009 for treated states with judicial elections across sectors with high contract reliance and low contract reliance.



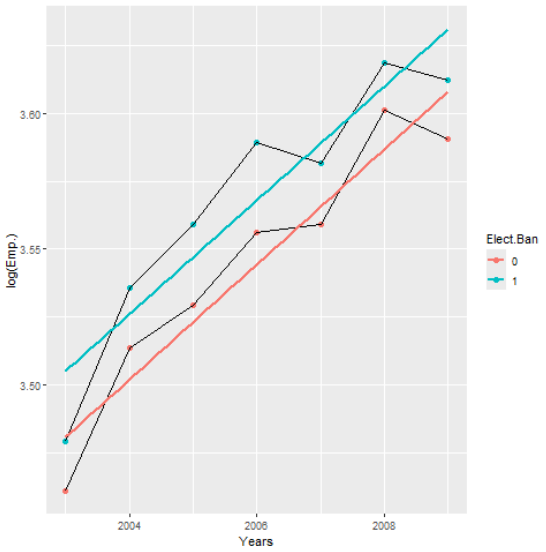
(a) Non-contract reliant Capex Pre-Trends

(b) Contract Reliant Capex Pre-Trends

Figure 14: Pre-Trends Capex

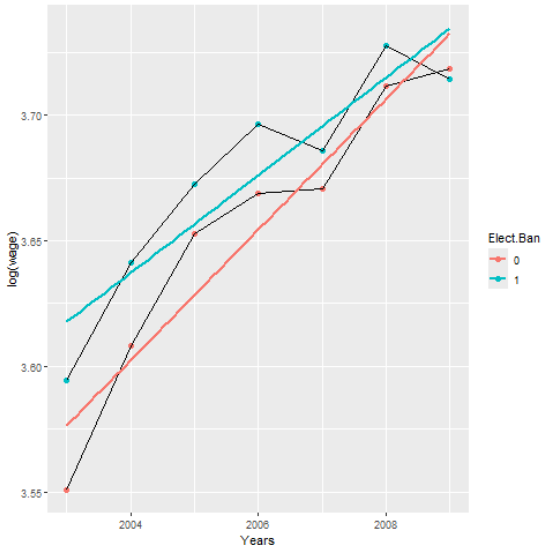


(a) Non-contract reliant Employment Pre-Trends

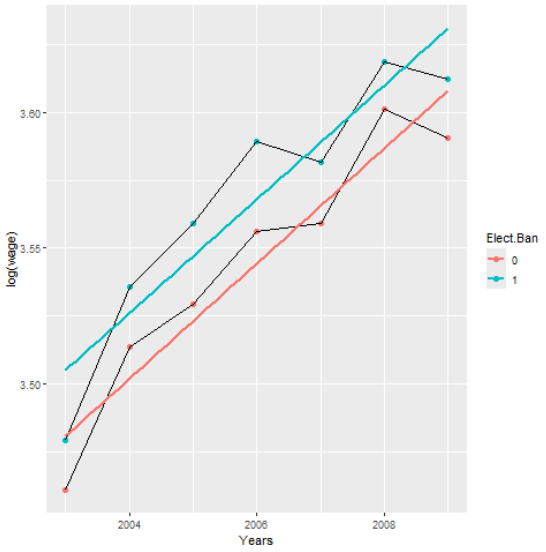


(b) Contract Reliant Employment Pre-Trends

Figure 15: Pre-Trends Employment



(a) Non-contract reliant Wage Pre-Trends



(b) Contract Reliant Wage Pre-Trends

Figure 16: Pre-Trends Wages

C Appendix: Independent and Direct Expenditures in Judicial Elections

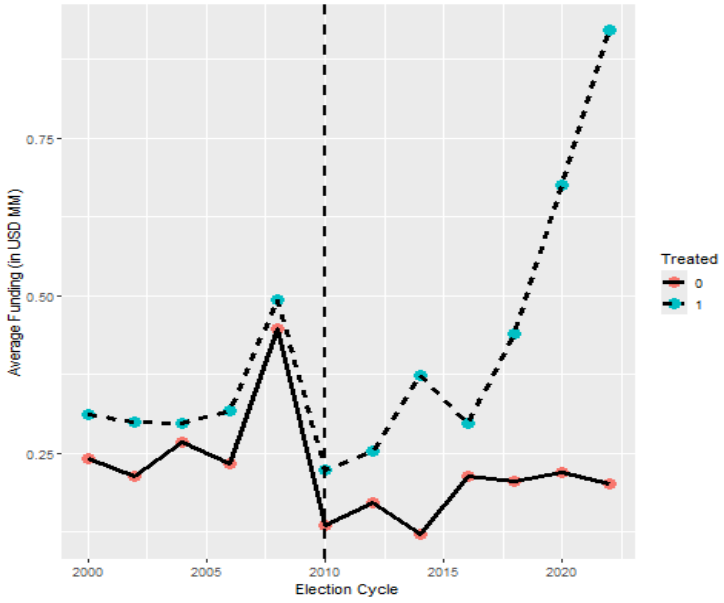


Figure 17: Average funding in USD for judicial elections in Treated vs. Control States (Excluding Illinois)

First, I present the direct evidence on independent expenditures in Table (21). As is clear, the majority of the increase in independent expenditure is from sources that could not be traced to a particular group such as businesses, parties, etc. This is in line with the fact that disclosures for independent expenditure are relatively more relaxed and it is difficult to trace the source of these expenditures.

C.0.1 Effect of Union vs. Corporate

Some states banned independent expenditures from corporations, while some states banned independent expenditures from both unions and corporations. Assuming **constant treatment effect** of unions (and similarly for corporations), I can test whether the corporation ban has a stronger effect vs. the union ban. Note that these tests are for the subset of states with judicial elections. In Table (22), the coefficient on $Corp.Ban \times Post$ captures the additional funding in states that imposed a corporation-only ban, relative to states

Table 21: Effect on Independent Spending (Categorized)

Note: This table presents the estimation results of Equation (2). The dependent variable is the independent expenditure on behalf of a candidate in the judicial elections, measured in USD MM. Different columns show results for estimation with the dependent variable as the political expenditure in an election by a candidate from a particular source, such as businesses, unions, or political parties. Column (3) shows the results for expenditure items that could not be assigned to a particular source. Variable *Ban* indicates the states that had imposed a ban on independent expenditures by unions or corporations, which were rendered unconstitutional by the Supreme Court in 2010. The specifications in each column vary depending on the inclusion of state, and year fixed effects. Standard errors are clustered at the state level.

	(1)	(2)	(3)	(4)	(5)
	Unassigned	Union and other	Ideology	Business	Party
Ban x Post	0.24***	0.01	0.17	0.10	-0.04***
	(0.04)	(0.01)	(0.12)	(0.10)	(0.01)
Election Cycle FE	Y	Y	Y	Y	Y
State FE	Y	Y	Y	Y	Y
Incumbency FE	Y	Y	Y	Y	Y
Observations	214	214	214	214	214
Rsq.	0.43	0.12	0.28	0.15	0.39
F	14.90	0.92	2.66	8.40	17.08

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

with no bans. Similarly, the coefficient on $CorpUnionBan \times Post$ captures the additional funding in states that imposed a ban on both, $corp + unions$. The difference between the two coefficients, allows us to compute the average treatment effect due to union bans.

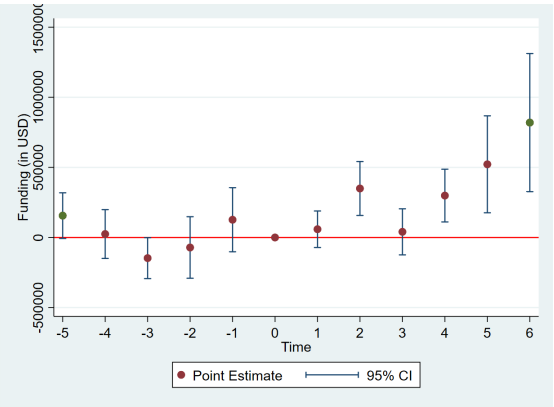
Table 22: Effect due to Corporations and Union bans

	(1)	(2)	(3)	(4)
	Fund (in MM)	Fund (in MM)	Ind. Exp. (in MM)	Ind. Exp. (in MM)
Ban x Post	0.22**		0.30**	
	(0.10)		(0.13)	
Corp ban (β_c)		0.07		0.22*
		(0.08)		(0.12)
Corp + Union Ban (β_{cu})		0.31**		0.31**
		(0.11)		(0.13)
F [H_0: $\beta_{cu} - \beta_c = 0$]		6.69		0.34
p-value		0.02		0.57
Election Cycle FE	Y	Y	Y	Y
State FE	Y	Y	Y	Y
Incumbency FE	Y	Y	Y	Y
Observations	1,227	1,227	251	251
Rsq.	0.27	0.27	0.36	0.36
F	3.85	3.74	6.82	13.15

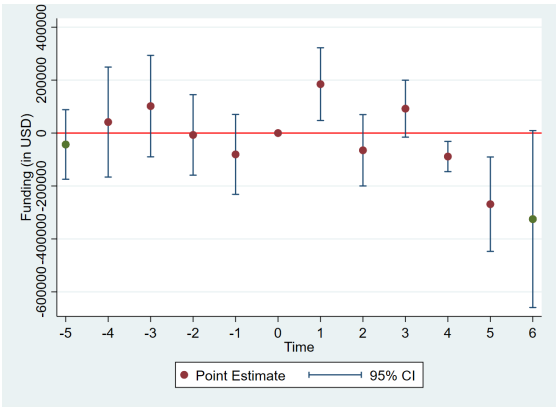
Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The candidate funding is not affected by the corporations-only ban being lifted. Moreover, the difference between the two coefficients is significant at the 10% confidence level. Therefore, this evidence points to higher funding of candidates in states where there is a higher increase in competition for electoral funding. These results are mainly driven by uncoded funding contributions. For business, or union spending this difference is insignificant. That is, only for uncoded funding I observe that the union + corporate ban has a higher funding effect than the corporate ban alone. On the other hand, when I perform a similar exercise for independent expenditures I find that the independent expenditures are higher for bans of both kinds (Corporations only and Unions and Corporations). The difference between the coefficients that captures the effect of the union ban being lifted, is not statistically significant at the 10% level (p-value = 0.68). This evidence points to higher independent expenditures mainly driven by states which imposed bans on corporations, and that corporations are more likely to spend through independent expenditures.



(a) Direct expenditure increase in states with bans on Corporations and Unions relative to corporations only ban states



(b) Direct expenditures in corporations only ban states

Figure 18: Event study plots. Event time is the 2010-2011 election cycle.

C.1 Judicial Competition: Incumbency advantage

Using a linear probability model where the dependent variable is an indicator variable that takes a value of 1, if an incumbent emerges as a winner in a race with challengers, I test

whether relaxation of campaign finance laws led to a decline in the incumbency advantage and higher turnover for incumbents. The baseline is that in races where there is at least one challenger, an incumbent wins in 55% of the races. However, this advantage declined by 20 pp. after the Supreme Court ruling in states affected by the lifting of the bans.

C.2 Judicial Competition: Voter Turnout

There is an increase in electoral expenditure and judicial competition as more candidates enter the election race. However, do the higher expenditure and new challengers mobilize the voters? For example, if advertising expenditures create awareness among the voters then there should be an increase in the voter turnout. I use the voter eligible population at the state-level compiled by the US Elections Project[9]. The source is the Current Population Survey by the Census Bureau. I compute the voter turnout in judicial elections and test whether the higher expenditure translates into the mobilization of voters.

In Table (24), in Columns (1) and (2) I test whether higher expenditure and entry of candidates leads to voter mobilization. I show that voter turnout increases by 5 pp., a significant increase over the average turnout of 28% in judge elections. Next, I test whether the increase in turnout is especially prominent in states where voters have less information about the candidates such as non-partisan elections. I do this by regressing equation (2) interacting each variable with the indicator for partisan elections. The evidence is inconclusive as the voter turnout increase is observed in both, partisan and non-partisan states. Finally, it is natural to ask whether voter mobilization is associated with more competitive judicial elections. In Column (4), I estimate the equation (2) and interact all dependent variables with log(Voter Turnout). The coefficient of interest is the interaction between Ban $\times$ Post $\times$ log(Voter Turnout). Note that the regression does not have a causal interpretation, because voter turnout is determined simultaneously with the dependent variable. However, the interaction allows me to provide suggestive evidence that voter turnout is higher in competitive elections. I find that a 1% increase in voter mobilization is associated with a decrease in the vote margin of the winning candidate by 6 pp. Therefore,

[9](https://www.electproject.org/election-data/voter-turnout-data

Table 23: Incumbent's advantage

Note: This table presents the estimation results of Equation (2). The dependent variable in Columns (1) and (2) is an indicator function for an incumbent victory. The sample includes only elections where there was at least one incumbent in the election. Variable *Ban* indicates the states that had imposed a ban on independent expenditures by unions or corporations, which were rendered unconstitutional by the Supreme Court in 2010. The specifications in each column vary depending on the inclusion of state, and year fixed effects. Standard errors are clustered at the state level.

	(1) incumb. win	(2) incumb. win
Ban	-0.15	
	(0.13)	
Post	0.23**	
	(0.09)	
Ban × Post	-0.19*	-0.20**
	(0.10)	(0.10)
Constant	0.55***	0.33***
	(0.14)	(0.06)
Cycle FE	N	Y
State FE	N	Y
Elect. type FE	Y	Y
Observations	445	444
Rsq.	0.17	0.45
F	11.42	2.74

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

I find evidence that electoral expenditure directly affects voter mobilization, and voter mobilization is higher in more competitive judicial elections.

Table 24: Voter Turnout in Judicial Elections

Note: This table presents the estimation results of Equation (2). The dependent variable in Columns (1) and (2) is the voter turnout, calculated as total votes divided by the eligible voter population in the state. Column(3) tests whether the turnout differs across partisan and non-partisan states. Columns (4) tests if the vote margin is lower when voter turnout is higher. Variable *Ban* indicates the states that had imposed a ban on independent expenditures by unions or corporations, which were rendered unconstitutional by the Supreme Court in 2010. *Partisan* indicates a state where the political affiliation of the judge is on the ballot. The specifications in each column vary depending on the inclusion of state, and year fixed effects. Standard errors are clustered at the state level.

	(1)	(2)	(3)	(4)
	Voter Turnout	Voter Turnout	Voter Turnout	vote margin
# of Seats	0.30***	0.28***	0.33***	-0.14***
	(0.07)	(0.02)	(0.01)	(0.03)
Ban × Post	0.05*	0.05**	0.06	-0.15***
	(0.03)	(0.02)	(0.04)	(0.05)
Partisan			0.00	
			(.)	
Partisan × # of Seats			-0.08***	
			(0.02)	
Ban × Post × Partisan			-0.01	
			(0.04)	
Ban × Post × log(Voter Turnout)				-0.07***
				(0.02)
log(Voter Turnout)				0.04
				(0.03)
# of Seats × log(Voter Turnout)				-0.06***
				(0.02)
				(.)
Cycle FE	N	Y	Y	Y
State FE	N	Y	Y	Y
Elect. type FE	Y	Y	Y	Y
Observations	480	480	480	480
Rsq.	0.43	0.77	0.77	0.13

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

D Theoretical Framework

To highlight the key frictions, and margins through which the legal environment affects productivity, I present a model that is a modification of Hopenhayn (1992). There is a continuum of firms. Consider firm i in sector j in state s. Assume that the firms are operating in perfect competition, i.e. they are price takers. We consider a firm with two inputs, capital and labor to simplify the exposition. However, the setup can easily be generalized to incorporate materials and other inputs. The firms face some distortions in the form of a wedge $\tau^K_{isj} > 0$. As discussed earlier, in the main section of the paper this could be due to contract enforcement frictions and biased courts. Overall, such frictions prevent the firm from producing as in the friction-less competitive benchmark ($\tau^K_{ijs} = 0$).

$$y_{ijs} = z_j l^{\alpha}_{ijs} k^{\beta}_{ijs}$$

where $0 < \alpha + \beta < 1$ and $\{\alpha, \beta\} > 0$, i.e. decreasing returns to scale (This is crucial for non-zero profits). To highlight the effect of the distortions, we subsume the idiosyncrasies at the firm level into the distortion term. We assume that $\log(1 + \tau^K_{ijs}) \sim \mathcal{N}(\mu_{\tau sj}, \sigma^2_{\tau sj})$.[10] The firm chooses factors to maximize profit. The firms choosing to operate must pay a fixed cost c_{js}.

$$\Pi^*(\tau^K_{ijs}) = \max_{l_{ijs}, k_{ijs}} y_{ijs} - w_{js} l_{ijs} - r(1 + \tau^K_{ijs}) k_{ijs}$$

Optimization implies,

$$l^*_{ijs} = \begin{cases} \left[\left(\frac{\alpha z_j}{w_{sj}} \right)^{1-\beta} \left(\frac{(1-\alpha) z_j}{r(1+\tau^K_{isj})} \right)^{\beta(1-\alpha)} \right]^{\frac{1}{1-\alpha-\beta}} = \Lambda_{sj} \frac{1}{(1+\tau^K_{isj})^{\tilde{\alpha}}} & \Pi^*(\tau^K_{isj}) \geq c_{sj} \\ 0 & \Pi^*(\tau^K_{isj}) < c_{sj} \end{cases}$$

and

$$k^*_{ijs} = \begin{cases} \left(\frac{\alpha z_j}{w_{sj}} \right)^{\frac{\alpha}{1-\alpha-\beta}} \left(\frac{(1-\alpha) z_j}{r(1+\tau^K_{isj})} \right)^{\frac{1}{1-\beta}\left(1 + \frac{(1-\alpha)\alpha\beta}{1-\alpha-\beta}\right)} = \mathcal{K}_{sj} \frac{1}{(1+\tau^K_{isj})^{\tilde{\beta}}} & \Pi^*(\tau^K_{isj}) \geq c_{sj} \\ 0 & \Pi^*(\tau^K_{isj}) < c_{sj} \end{cases}$$

[10] Instead of tracking the distribution of $\frac{z_{isj}}{(1+\tau^K_{sj})}$, we consider firm-level distortions $(1 + \tau^K_{isj})$ and assume that all firms are hit by sector level productivity shock.

where $\tilde{\alpha}(\alpha,\beta) = \frac{\beta(1-\alpha)}{1-\alpha-\beta} > 0$ and $\tilde{\beta}(\alpha,\beta) = \frac{1}{1-\beta}\left(1+\frac{(1-\alpha)\alpha\beta}{1-\alpha-\beta}\right) > 0$. Note that, $\tilde{\alpha},\tilde{\beta}$ are increasing in their arguments, with

$$(\alpha\tilde{\alpha}+\beta\tilde{\beta}) = \tilde{\beta}-1>0 \quad , \quad 1+\tilde{\alpha}-\tilde{\beta} = \frac{-\alpha\beta}{1-\beta} < 0$$

.

Lemma 1. *There exists a threshold $\overline{\tau}(c_{sj}) > 0$ such that the firm enters the market if and only if $\tau^K_{ijs} \leq \overline{\tau}(c_{sj})$. $\overline{\tau}(c_{sj})$ is decreasing in c_{sj}.*

$$1+\overline{\tau}(c_{sj}) = \left(\frac{\Lambda^{\alpha}_{sj}\mathcal{K}^{\beta}_{sj}}{c_{sj}}\right)^{\tilde{\beta}-1}$$

Overall, a higher entry cost implies that only firms with sufficiently low distortion enter.

D.1 Entry, Employment, and Productivity

In this sub-section we discuss the predictions from the model setup.

Establishment Entry:

Proposition 1. *The measure of establishments that enter the market are,*

$$N^*(\mu_{\tau sj},\sigma_{\tau sj},c_{sj}) = F_{\tau}(\tau^K_{ijs} < \overline{\tau}(c_{sj})) = \Phi\left(\frac{\log(1+\overline{\tau}(c_{sj}))-\mu_{\tau sj}}{\sigma^2_{\tau sj}}\right)$$

more establishments enter if entry cost c_{sj}, the average distortion $\mu_{\tau sj}$, or the variance of distortion $\sigma_{\tau sj}$ decline.

Therefore, entry increases because of the lower entry cost and if the average distortion or the dispersion of the distortive wedges is lower. More competitive elections imply an increase in σ^2, however, if the entry cost and the average distortion decline then entry should still increase.

Total Employment : Total state-sector-level employment is given by,

$$l^*_{sj} = \int_{\tau^K_{isj}<\overline{\tau}_{sj}(c_{sj})} l^*_{isj}dF_{\tau}(\tau^K_{ijs}) = \Lambda_{sj}\underbrace{\exp\left(-\tilde{\alpha}\mu_{\tau sj}+\frac{\tilde{\alpha}^2\sigma^2_{\tau sj}}{2}\right)}_{\text{average employment if } c_{sj}=0}\cdot\underbrace{\Phi\left(\tilde{\alpha}\sigma_{\tau sj}-\frac{\mu_{\tau sj}-\log(1+\overline{\tau}_{sj})}{\sigma_{\tau sj}}\right)}_{\text{entry effect}}$$

Proposition 2. *Average sector-level employment increases, if entry cost c_{sj}, or the average distortion $\mu_{\tau sj}$ declines, and the dispersion of distortion $\sigma_{\tau sj}$ increases.*

Average Productivity : The average productivity at the state-sector-level is,

$$APL_{sj} = \frac{\int_{\tau^K_{isj} < \overline{\tau}_{sj}(c_{sj})} \Lambda^{\alpha}_{sj} \mathcal{K}^{\beta}_{sj} (1+\tau^K_{isj})^{1-\tilde{\beta}} dF_{\tau}(\tau^K_{ijs})}{l^*_{sj}} \tag{8}$$

Proposition 3. *The average sector-level productivity*

$$APL_{sj} = \Lambda^{\alpha-1}_{sj} \mathcal{K}^{\beta} \underbrace{\exp\left((\tilde{\beta}-\tilde{\alpha}-1)\left(-\mu_{\tau sj} + (\tilde{\alpha}+\tilde{\beta}-1)\frac{\sigma^2_{\tau sj}}{2}\right)\right)}_{\text{average productivity with } c_{sj}=0} \underbrace{\frac{\Phi\left((\tilde{\beta}-1)\sigma_{\tau sj} - \frac{\mu_{\tau sj} - \log(1+\overline{\tau}_{sj})}{\sigma_{\tau sj}}\right)}{\Phi\left(\tilde{\alpha}\sigma_{\tau sj} - \frac{\mu_{\tau sj} - \log(1+\overline{\tau}_{sj})}{\sigma_{\tau sj}}\right)}}_{\text{entry effect}} \tag{9}$$

1. *Increases as the average distortion, $\mu_{\tau sj}$ declines.*

2. *Decreases if the entry cost c_{sj} declines.*

A decline in average distortion implies that more firms are less distorted, which then increases the average productivity. Interestingly this operates through two channels. First, the average productivity improves due to a decline in average distortion without the entry cost. Second is due to the entry effect which focuses on the average productivity of entrants. In this case, although complementary conditional (on entry) distribution (or tail function) of output improves, however, it does not increase as much as the complementary conditional distribution of employment. However, the overall effect without entry dominates and the average productivity improves.

Similarly, a reduction in the entry cost c_{sj} encourages more firms to enter. These marginal firms are ones with more distortion. Therefore, the average productivity decreases if entry costs are lowered.

Summary: We can summarize our predictions in Table 25. A reduction in average distortion due to the alleviation of contract enforcement friction will increase entry, employment,

and average productivity of a sector in a given state. I cannot rule out the other two effects working simultaneously, but an increase in employment, productivity, and entry makes the reduction in average distortion more plausible.

Table 25: Prediction on Entry, Employment, and Productivity

	Number of Firms (N^*_{sj})	Employment (l^*_{sj})	Average Productivity (APL_{sj})
Mean Distortion ($\mu_{\tau sj}$) $\downarrow$	$\uparrow$	$\uparrow$	$\uparrow$
Variance Distortion ($\sigma_{\tau sj}$) $\uparrow$	$\downarrow$	$\uparrow$	$\uparrow$
Entry Cost (c_{sj}) $\downarrow$	$\uparrow$	$\uparrow$	$\downarrow$

E Dynamic Effect on Productivity

I estimate the dynamic event-study regression (10) conditional on the selection procedure election v. non-election of state supreme court judges.

$$y_{jst} = \delta_{sj} + \delta_{jt} + \sum_{\tau=-5}^{T} \beta_\tau \cdot Ban_s \times D_{t-\tau} + \varepsilon_{jst} \tag{10}$$

The dependent variable is the value added per hour worked by production workers. As can be seen in the left Panel (A), the Citizens United ruling led to an increase in productivity in states with judicial elections. However, Panel (B) illustrates that there is no such productivity improvement in states without judicial elections.

Note: This figure presents the estimation results of equation (10). The dependent variable is labor productivity, measured as USD value added per hour. Panel (a) shows results for states with judicial elections for state Supreme Court judges and Panel (b) for states that use some form of appointment. The figures indicate the coefficients and 90% confidence intervals that illustrate the dynamic effects on labor productivity due to the lifting of the bans imposed on independent expenditures by unions or corporations, which were rendered unconstitutional by the Supreme Court in 2010. Event Time, 0 is the year 2010. All estimations include state and sector-by-year fixed effects. Sector is defined at the 4-digit NAICS code level. Standard errors are clustered at the state level.

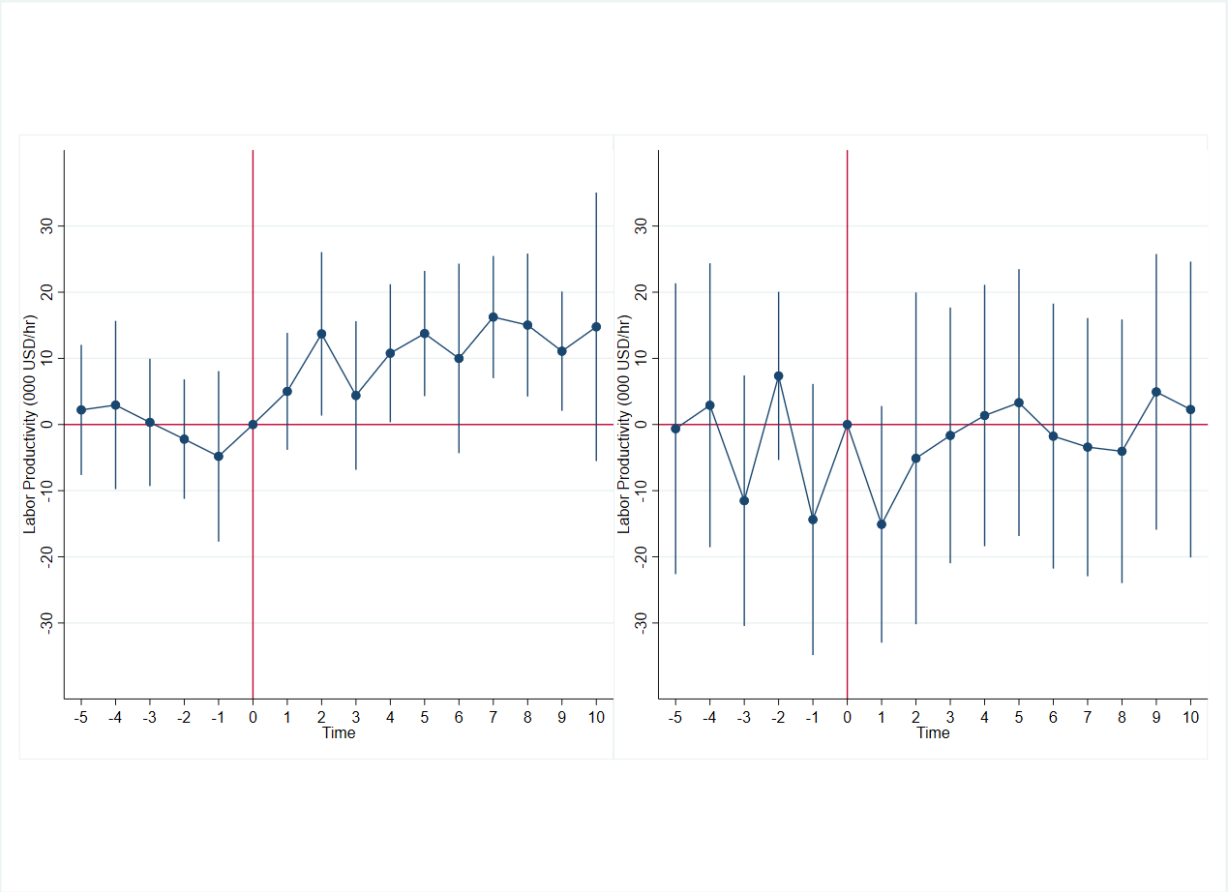


Figure 19: Effect on Labor Productivity in '000 USD /hr: States with Judicial Elections (Left), and without judicial elections (Right)

F Effect on Establishment Productivity and Misallocation

The sector-level aggregates of the real variables capture the average effect on productivity, which is less noisy than individual establishment-level estimates of productivity. To establish that the results are robust, I estimate the effects on productivity using establishment-level data from the National Establishments Time-Series (NETS) database. This database is compiled by Wall & Associates through Dun & Bradstreet survey data collection. The data provides the sales and number of employees for a representative sample at the establishment level. However, the NETS data suffers from a few limitations, such as imputation of sales and employment as documented in Barnatchez, Crane and Decker (2017). The authors also provide prescriptions to overcome some of these limitations. The establishment-level sample also allows us to test whether the productivity improvements are associated with factor misallocation captured by the variance of the marginal revenue product of labor. I follow Sraer and Thesmar (2023) to test if factor misallocation increases.

I create a sample of establishments following the recommendations in Barnatchez, Crane and Decker (2017). I focus on establishments with more than 10 employees and fewer than 1000 employees to avoid the effect of imputation in employment. I compute the average productivity measured as revenue per employee or labor productivity. The sales for branches of multi-establishment firms are imputed. To avoid the imputation of local sales, I restrict the sample to standalone firms without such imputation concerns. I discard the 1% tails of labor productivity at the 4-digit NAICS-year level to avoid the effect of outliers in our computations. This results in $152,198$ sector $\times$ state $\times$ year observations. Finally, I drop all the sector $\times$ state $\times$ year cells with fewer than 8 establishments to reduce the noise in the computation of the first and second moments.[11] I end up with $39,446$ sector, state, time (in years) observations. Following Sraer and Thesmar (2023), I focus on 3 dependent variables, $E[\log(Labor\ Productivity)]$ the average labor productivity, $V(\log(LaborProductivity))$ the

[11] The result is robust to restricting attention to more than 8 establishments per cell. In a separate subsection in this appendix, I replicate the results on average productivity without this restriction of a minimum number of establishments in a state, sector, or year cell, and the average treatment effect is robust and equal to USD $18,000$.

dispersion of productivity, and $C(\log(LaborProductivity, \log(Revenue))$ the covariance of productivity with size. The dispersion of labor productivity is indicative of factor misallocation within the economy.

Table 26: Effect on Average Productivity and Dispersion

Note: This table presents the estimation results of equation (5). The dependent variable in Column(1) is labor productivity, measured as revenue in USD 1000s per worker from the NETS data sample 1990-2021. Columns(2)-(3) are the mean and variance of labor productivity within state-sector-year cell, and Column(4) is the covariance of labor productivity and revenue within state-sector-year cell. Variables *Elect* indicates states with judicial elections for state supreme court judges, *Ban* indicates the states that had imposed a ban on independent expenditures by unions or corporations, which were rendered unconstitutional by the Supreme Court in 2010. The specifications in each column vary depending on the inclusion of state-by-sector, and sector-by-year fixed effects. Sector is defined at the 4-digit NAICS code level. Standard errors are clustered at the state level.

	(1)	(2)	(3)	(4)
	Labor Productivity (000 USD/emp)	E[log(Labor Productivity)]	V(log(Labor Productivity))	C(log(Labor Productivity),log(Revenue))
Ban × Post	-7.45	-0.00	-0.01	-0.02
	(7.66)	(0.02)	(0.03)	(0.04)
Election × Ban × Post	19.47**	0.02	0.04	0.09*
	(9.62)	(0.03)	(0.04)	(0.05)
State × Sector FE	Y	Y	Y	Y
Sector ×Year FE	Y	Y	Y	Y
N	39,446	39,446	39,446	39,446
R-sq.	0.84	0.89	0.72	0.70

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table (26) illustrates how the average labor productivity is higher for the states where bans on independent expenditure were invalidated following Citizens United. This evidence is in line with the evidence on average productivity presented earlier in the paper. Interestingly, it is plausible that the average productivity increase is associated with higher dispersion in productivity, a measure of factor misallocation along the lines of Hsieh and Klenow (2009). Column (3) shows that the dispersion in productivity does not increase. The increase in productivity is stronger for larger firms, as shown in Column (4), where the dependent variable is the covariance between labor productivity and revenue. Columns (3) and (4) show that the increased labor productivity is not at the expense of increased misallocation within the economy.

F.1 Robustness: Aggregation

In this section, we verify whether the productivity improvements, as documented in the main paper, are robust to the aggregation performed under the annual survey of manufacturers. We take the NETS sample and aggregate the revenue and employment at the state-sector-year level. The sector is defined at the 4-digit NAICS level. Column (1)-(3) in Table (27) and (28) confirm our findings from the annual survey of manufacturers. There is a 6% improvement in labor productivity measured as revenue per worker. Moreover, this improvement in productivity growth is robust to the inclusion of firm fixed effects. However, the effect declines, and therefore, some productivity improvement must come from the extensive margin, i.e., more productive firms entering these jurisdictions. In Section 4.4, we test for this hypothesis.

Table 27: Effect on Establishment Productivity

Note: This table presents the estimation results of equation (5). The dependent variable is labor productivity, measured as revenue in USD 1000s per worker from the NETS data sample 1990-2021. Variables *Elect* indicates states with judicial elections for state supreme court judges, *Ban* indicates the states that had imposed a ban on independent expenditures by unions or corporations, which were rendered unconstitutional by the Supreme Court in 2010. The specifications in each column vary depending on the inclusion of state-by-sector, and sector-by-year fixed effects. Sector is defined at the 4-digit NAICS code level. Standard errors are clustered at the state level.

	(1)	(2)	(3)	(4)
	Lab. Prod (000 USD/emp)	Lab. Prod (000 USD/emp)	Lab. Prod (000 USD/emp)	Lab. Prod (000 USD/emp)
Ban × Post	-2.61			-0.06
	(7.52)			(3.70)
Elect × Ban × Post	18.95*	16.35**	11.83*	4.05
	(9.84)	(6.87)	(6.19)	(5.69)
State FE	Y	Y	-	Y
State × Sector FE	N	N	Y	N
Sector ×Year FE	Y	Y	Y	Y
Estab. FE	N	N	N	Y
N	267,369	267,369	267,316	267,090
R-sq.	0.24	0.24	0.45	0.86

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 28: Effect on Establishment Productivity

Note: This table presents the estimation results of equation (5). The dependent variable is the logarithm of labor productivity, measured as revenue in USD 1000s per worker from the NETS data sample 1990-2021. Variables *Election* indicates states with judicial elections for state supreme court judges, *Ban* indicates the states that had imposed a ban on independent expenditures by unions or corporations, which were rendered unconstitutional by the Supreme Court in 2010. The specifications in each column vary depending on the inclusion of state-by-sector, and sector-by-year fixed effects. Sector is defined at the 4-digit NAICS code level. Standard errors are clustered at the state level.

	(1)	(2)	(3)	(4)
	log(Lab Prod.)	log(Lab Prod.)	log(Lab Prod.)	log(Lab Prod.)
Ban × Post	-0.00			
	(0.02)			
Election × Ban × Post	0.06**	0.06***	0.04***	0.02**
	(0.02)	(0.02)	(0.02)	(0.01)
State FE	Y	Y	-	Y
State × Sector FE	N	N	Y	N
Sector ×Year FE	Y	Y	Y	Y
Estab. FE	N	N	N	Y
N	267,369	267,369	267,316	267,090
R-sq.	0.32	0.32	0.49	0.84

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

G Unionization and Labor Productivity

To test whether the relative bargaining power of the unions is driving the results, I exploit the heterogeneity in state laws that directly affect unionization rates. Some states adopt right-to-work laws that free workers from the responsibility to pay union dues. Such a provision exacerbates the free-riding problem common with collective action, hurts unions' finances, and lowers unionization rates. This generally reduces the unions' bargaining power Fortin, Lemieux and Lloyd (2023).

I restrict attention to states with judicial elections. If the labor productivity improvements are due to the declining bargaining power of unions after the Citizens United ruling, then we would expect the productivity improvements to be more pronounced for states where unions were ex-ante more powerful, i.e. in states without right-to-work laws. However, in Table (29), we find that labor productivity (Column (3)) and employment growth (Column(4)) are higher for states with right-to-work laws. Therefore, the productivity improvements after the Citizens United ruling do not stem from the reduced bargaining power of unions relative to employers. It seems more plausible that the changes in legal environment reduced non-labor-related distortions, the firms responded by increasing production, hiring more workers, particularly in states where labor adjustment costs are lower, which translates to an increase in average labor productivity in states with right-to-work laws.

Table 29: Effect of Unionization in Judicial Election States

Note: This table presents the estimation results of equation (5) restricted to the sample of states with judicial elections. The dependent variable is labor productivity, measured as value added in USD 1000s per worker. Variables *Elect* indicates states with judicial elections for state supreme court judges, *Ban* indicates the states that had imposed a ban on independent expenditures by unions or corporations, which were rendered unconstitutional by the Supreme Court in 2010. $\mathbb{1}\{\text{Right To Work}\}$ indicates states with Right to Work laws and lower unionization rates. The specifications in each column includes state-by-sector, and sector-by-year fixed effects. Sector is defined at the 4-digit NAICS code level. Standard errors are clustered at the state level.

	(1)	(2)	(3)	(4)
	Labor Productivity (USD/emp)	Labor Productivity (USD/emp)	log(Emp)	log(Emp)
Ban × Post	14.34		0.02	
	(14.12)		(0.04)	
$\mathbb{1}\{\text{Right To Work}\}$ × Ban × Post	18.26	32.48**	0.05	0.06*
	(19.15)	(13.58)	(0.05)	(0.03)
State × Sector FE	Y	Y	Y	Y
Sector × Year FE	Y	Y	Y	Y
N	17,802	17,802	17,820	17,820
R-sq.	0.78	0.78	0.96	0.96

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

H Robustness

The Citizens United ruling affected all the states with bans at the same time. Therefore, the difference-in-differences estimates may be biased due to confounding events occurring around the event time 2010. The first among such concerns is the Global Financial Crisis of 2008-09. The second is the boost in production of Shale gas following the improved viability of hydraulic fracturing or fracking. I address both of these concerns using a battery of robustness checks and find that the labor productivity increase is not driven by these confounding events.

H.1 Robustness: Global Financial Crisis

The post-period in the difference-in-differences research design is the period after 2010. However, in response to the Global Financial Crisis (GFC) of 2008, the US Treasury and Federal Reserve eased the monetary policy significantly starting in 2010. We control for year fixed effects which accounts for such year-specific aggregate policy shocks. However,

to the extent different states were affected heterogeneously during the financial crisis, the federal policies will have differential impact on the real outcomes in these states. To alleviate such concerns I construct a measure of crisis shock which measures the percentage decline in state output between 2007 and 2009. Then within the regression specification (5), I allow for the labor productivity in different states depending on this shock variable to evolve differentially over time. If the coefficient estimates of the effect on labor productivity remain statistically significant then the labor productivity differences across states with differential campaign finance restrictions are not due to the extent to which these states were negatively affected during the crisis or the federal fiscal and monetary policy response that followed.

In Table (30), in Columns(2)-(4) we control for state-specific linear time trends, the shock to state output during the crisis, and the state output in 2010 to allow for the labor productivity in states with different baseline output to evolve according to differential trends. The tests conclusively suggest that the pre-period differences in state output or the shock during the financial crisis are not responsible for the observed productivity improvements in states where the Citizens United ruling removed the bans on independent expenditure.

Table 30: State Output Differences and Labor Productivity

Note: This table presents the estimation results of equation (5) and displays the effect of treatment on states with judicial elections. The dependent variable is labor productivity, measured as value added in USD 1000s per worker. Variables *Elect* indicates states with judicial elections for state supreme court judges, *Ban* indicates the states that had imposed a ban on independent expenditures by unions or corporations, which were rendered unconstitutional by the Supreme Court in 2010. The specifications in each column includes state-by-sector, and sector-by-year fixed effects. Columns differ based on the state-specific time trend, the shock to state output during the global financial crisis, and state-specific trends depending on the state output in 2010. Sector is defined at the 4-digit NAICS code level. Standard errors are clustered at the state level.

	(1)	(2)	(3)	(4)
	Labor Productivity (USD/emp)	Labor Productivity (USD/emp)	Labor Productivity (USD/emp)	Labor Productivity (USD/emp)
Election × Ban × Post	19.67**	24.51**	19.19**	16.64**
	(8.98)	(10.79)	(9.09)	(7.88)
State × Sector FE	Y	Y	Y	Y
Sector × Year FE	Y	Y	Y	Y
State Time Trend	N	Y	N	N
GDP shock × Year	N	N	Y	N
GDP 2010 × Year	N	N	N	Y
N	36,324	36,324	36,324	36,324
R-sq.	0.75	0.75	0.75	0.75

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

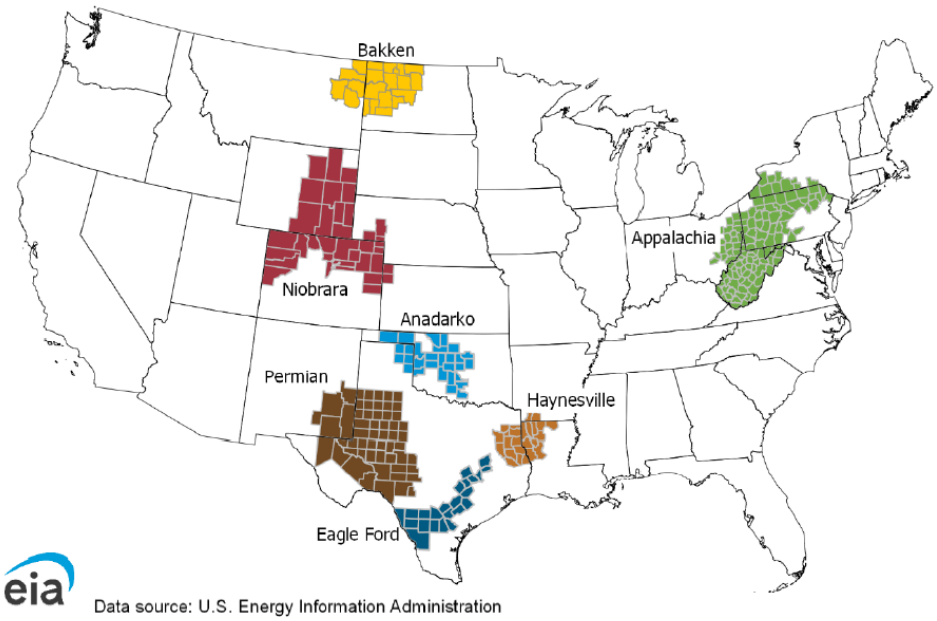


Figure 20: Shale Producing States. Data from Energy Information Administration.

H.2 Shale Boom

One possible identification concern is the concurrence of the Citizens United ruling with the boost in Shale gas production. Only certain states are endowed with Shale reserves. Interestingly, the top 5 states produce 70% of the shale output. Figure (20) illustrates the prominent Shale producing states.

I consider two robustness checks. First, I remove the top 5 and 10 Shale producing states. This list includes Texas, Pennsylvania, Louisiana, West Virginia, Ohio, New Mexico, Oklahoma, North Dakota, Arkansas, and Colorado. Note that among these states, Texas, Pennsylvania, Louisiana, West Virginia, Ohio, and North Dakota (6 out of 10) elect their state supreme court judges. Therefore, a mix of selection procedures across the states was retained in the sample. If the labor productivity results continue to hold on the restricted sample, then the labor productivity improvement is not exclusively driven by the Shale production boom. I test for this hypothesis in Columns (2) and (3) of Table (31). The estimate of the coefficient of interest declines slightly. However, it is still economically and statistically significant, indicating that the shale production boom is not the main driving

force behind productivity differences between states with and without bans on independent expenditure after the Citizens United ruling.

Second, I removed the NAICS code 324, which represents the Petroleum and Coal Products Manufacturing sector. The effect on labor productivity is robust even when we restrict the sample to sectors not directly related to the petroleum manufacturing sector (Column (4)). Therefore, the Shale boom that coincides with the Citizens United ruling does not seem to be the driving force behind the observed improvement in labor productivity in states where the campaign finance laws were made less restrictive.

Table 31: Shale Boom and Labor Productivity

Note: This table presents the estimation results of equation (5) and displays the effect of treatment on states with judicial elections. The dependent variable is labor productivity, measured as value added in USD 1000s per worker. Variables *Election* indicates states with judicial elections for state supreme court judges, *Ban* indicates the states that had imposed a ban on independent expenditures by unions or corporations, which were rendered unconstitutional by the Supreme Court in 2010. The specifications in each column includes state-by-sector, and sector-by-year fixed effects. Columns differ based on the exclusion of Top 5 and 10 Shale producing states and the NAICS sector 324 corresponding to the Petroleum and Coal Products Manufacturing. Sector is defined at the 4-digit NAICS code level. Standard errors are clustered at the state level.

	(1) Labor Productivity	(2) Labor Productivity	(3) Labor Productivity	(4) Labor Productivity
Election × Ban × Post	19.67**	17.76*	16.38*	21.12**
	(8.98)	(8.82)	(9.64)	(9.63)
State × Sector FE	Y	Y	Y	Y
Sector × Year FE	Y	Y	Y	Y
Remove Top 5 Shale Producers	N	Y	Y	N
Remove Top 10 Shale Producers	N	N	Y	N
Remove NAICS 324	N	N	N	Y
N	36,324	31,307	28,926	35,857
R-sq.	0.75	0.73	0.74	0.74

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

A Online Appendix

A.1 Judicial Selection

In Figure (21), I illustrate an advertisement made through indirect expenditures on behalf of Judge Protasiewicz in the Wisconsin State Supreme Court elections of 2023. The total expenditure in this election was USD 44 million, and it was the most expensive judicial election race till date.



Figure 21: Illustration of an advertisement for Judge Janet Protasiewicz (indirect Expenditure)

Table (32) are reproduced from Kang and Shepherd (2015) and illustrate the heterogeneity in election procedures across different states. In this paper, however, we focus on the dichotomy of election v. appointment. Therefore, I classify all states with merit plans, gubernatorial, and legislative appointment as homogeneous because the focus of this paper is on the effect of campaign finance laws, and when it comes to campaign finance the donations will generally be made not directly in the election of the judge but to some person (or group of people) responsible for appointment among other things.

Table 32: State Supreme Court Judge Selection Procedures

State	**Selection Method**	**Details**	**Latest Change**	**Details of Latest Change**	**Tenure**	**Independent Expenditure Ban**	**Year Introduced**
Alabama	Partisan Election	Judges are elected through partisan elections.	1994	Nonpartisan to partisan election	6 years	No ban	-
Alaska	Merit Selection	Judicial Council nominates candidates; the governor appoints; judges face retention elections.	1970	Introduced merit selection	10 years	Ban	1996
Arizona	Merit Selection	Commission nominates candidates; the governor appoints; judges face retention elections.	1974	Introduced merit selection	6 years	Ban	1978
Arkansas	Nonpartisan Election	Judges are elected through nonpartisan elections.	2000	Partisan to nonpartisan election	8 years	No ban	-

California	Nonpartisan Election	Governor appoints; confirmation by Commission on Judicial Appointments; judges face retention elections.	1934	Introduced retention elections	12 years	No Ban	-
Colorado	Merit Selection	Judicial Nominating Commission nominates candidates; governor appoints; judges face retention elections.	1966	Introduced merit selection	10 years	Ban	2002
Connecticut	Appointment	Governor appoints; confirmed by General Assembly; judges serve eight-year terms.	1986	Increased term length	8 years	Ban	1987
Delaware	Appointment	Governor appoints; confirmed by State Senate; judges serve 12-year terms.	1897	Codified current appointment system	12 years	No ban	-

Florida	Merit Selection	Judicial Nominating Commission nominates candidates; governor appoints; judges face retention elections.	1976	Introduced merit selection	6 years	No Ban	-
Georgia	Nonpartisan Election	Judges are elected through nonpartisan elections.	1983	Partisan to nonpartisan election	6 years	No ban	-
Hawaii	Merit Selection	Judicial Selection Commission nominates; governor appoints; confirmed by State Senate.	1978	Introduced merit selection	10 years	No Ban	-
Idaho	Nonpartisan Election	Judges are elected through nonpartisan elections.	1934	Changed to nonpartisan elections	6 years	No ban	-
Illinois	Partisan Election	Judges are elected through partisan elections; subsequent retention elections.	1962	Introduced retention elections	10 years	No ban	-

Indiana	Merit Selection	Judicial Nominating Commission nominates; governor appoints; judges face retention elections.	1970	Introduced merit selection	10 years	No ban	-
Iowa	Merit Selection	Judicial Nominating Commission nominates; governor appoints; judges face retention elections.	1962	Introduced merit selection	8 years	Ban	1975
Kansas	Merit Selection (Supreme Court Nominating Commission)	Nominating Commission nominates; governor appoints; judges face retention elections.	1958	Introduced merit selection	6 years	No ban	-
Kentucky	Nonpartisan Election	Judges are elected through nonpartisan elections.	1975	Changed to nonpartisan elections	8 years	Ban	1974
Louisiana	Partisan Election	Judges are elected through partisan elections.	1974	Changed to partisan elections	10 years	No ban	-

Maine	Appointment	Governor appoints; confirmed by State Senate; judges serve seven-year terms.	1839	Established current appointment system	7 years	No ban	-
Maryland	Merit Selection	Judicial Nominating Commission nominates; governor appoints; confirmed by State Senate; judges face retention elections.	1970	Introduced merit selection	10 years	No ban	-
Massachusetts	Appointment	Governor appoints; confirmed by Governor's Council; judges serve until age 70.	1780	Established current appointment system	Until age 70	Ban	1975
Michigan	Nonpartisan Election	Judges are elected through nonpartisan elections.	1964	Partisan to nonpartisan election	8 years	Ban	1976
Minnesota	Nonpartisan Election	Judges are elected through nonpartisan elections.	1912	Changed to nonpartisan elections	6 years	Ban	1988

Mississippi	Nonpartisan Election	Judges are elected through nonpartisan elections.	1994	Partisan to nonpartisan election	8 years	No ban	-
Missouri	Merit Selection (Missouri Plan)	Appellate Judicial Commission nominates; governor appoints; judges face retention elections.	1940	Introduced merit selection (Missouri Plan)	12 years	No ban	-
Montana	Nonpartisan Election	Judges are elected through nonpartisan elections.	1935	Changed to nonpartisan elections	8 years	Ban	1912
Nebraska	Merit Selection	Judicial Nominating Commission nominates; governor appoints; judges face retention elections.	1962	Introduced merit selection	6 years	No ban	-
Nevada	Nonpartisan Election	Judges are elected through nonpartisan elections.	1976	Partisan to nonpartisan election	6 years	No ban	-

New Hampshire	Appointment	Governor appoints; confirmed by Executive Council; judges serve until age 70.	1784	Established current appointment system	Until age 70	Ban	1979
New Jersey	Appointment	Governor appoints; confirmed by State Senate; judges serve initial seven-year term; reappointed until age 70.	1947	Established current appointment system	7 years, renewable until age 70	No ban	-
New Mexico	Merit Selection	Judicial Nominating Commission nominates; governor appoints; judges face partisan retention elections.	1988	Introduced merit selection	8 years	No ban	-
New York	Appointment	Governor appoints; confirmed by State Senate; judges serve 14-year terms.	1977	Changed to gubernatorial appointment with confirmation	14 years	No ban	-

North Carolina	Partisan Election	Judges are elected through partisan elections.	2017	Nonpartisan to partisan election	8 years	Ban	1975
North Dakota	Nonpartisan Election	Judges are elected through nonpartisan elections.	1910	Changed to nonpartisan elections	10 years	Ban	1981
Ohio	Nonpartisan Election	Judges are elected through nonpartisan elections.	1911	Changed to nonpartisan elections	6 years	Ban	1995
Oklahoma	Merit Selection	Judicial Nominating Commission nominates; governor appoints; judges face retention elections.	1967	Introduced merit selection	6 years	Ban	1994
Oregon	Nonpartisan Election	Judges are elected through nonpartisan elections.	1931	Changed to nonpartisan elections	6 years	No ban	-
Pennsylvania	Partisan Election	Judges are elected through partisan elections; subsequent retention elections.	1968	Introduced retention elections	10 years	Ban	1937

Rhode Island	Merit Selection	Judicial Nominating Commission nominates; governor appoints; confirmed by General Assembly.	1994	Introduced merit selection	Lifetime	Ban	1998
South Carolina	Legislative Election	Judges are elected by the General Assembly.	1895	Codified current legislative election system	10 years	No ban	-
South Dakota	Merit Selection	Judicial Qualifications Commission nominates; governor appoints; judges face retention elections.	1980	Introduced merit selection	8 years	Ban	2007
Tennessee	Merit Selection	Judicial Nominating Commission nominates; governor appoints; judges face retention elections.	1971	Introduced merit selection	8 years	Ban	1972

Texas	Partisan Election	Judges are elected through partisan elections.	1876	Established current partisan election system	6 years	Ban	1987
Utah	Merit Selection	Judicial Nominating Commission nominates; governor appoints; confirmed by State Senate; judges face retention elections.	1985	Introduced merit selection	10 years	No Ban	-
Vermont	Merit Selection	Judicial Nominating Board nominates; governor appoints; confirmed by General Assembly.	1971	Introduced merit selection	6 years	No ban	-
Virginia	Legislative Election	Judges are elected by the General Assembly.	1779	Established current legislative election system	12 years	No ban	-

Washington	Nonpartisan Election	Judges are elected through nonpartisan elections.	1912	Changed to nonpartisan elections	6 years	No ban	-
West Virginia	Nonpartisan Election	Judges are elected through nonpartisan elections.	2015	Partisan to nonpartisan election	12 years	Ban	1908
Wisconsin	Nonpartisan Election	Judges are elected through nonpartisan elections.	1913	Changed to nonpartisan elections	10 years	Ban	1973
Wyoming	Merit Selection	Judicial Nominating Commission nominates; governor appoints; judges face retention elections.	1972	Introduced merit selection	8 years	Ban	1977

B Cases Appealed to the US Supreme Court

We evaluate the quality of decision-making at state courts by examining the cases that were appealed to the Supreme Court of the United States. There are a few key things to note. As shown in Figure (22), very few cases from the State Supreme Courts are appealed to the US Supreme Court. Second, among the states with elections, no case from the State Supreme Court was appealed from the treated states in the post-period. Therefore, in the following discussion, we evaluate the bias in decision-making at the lower courts.

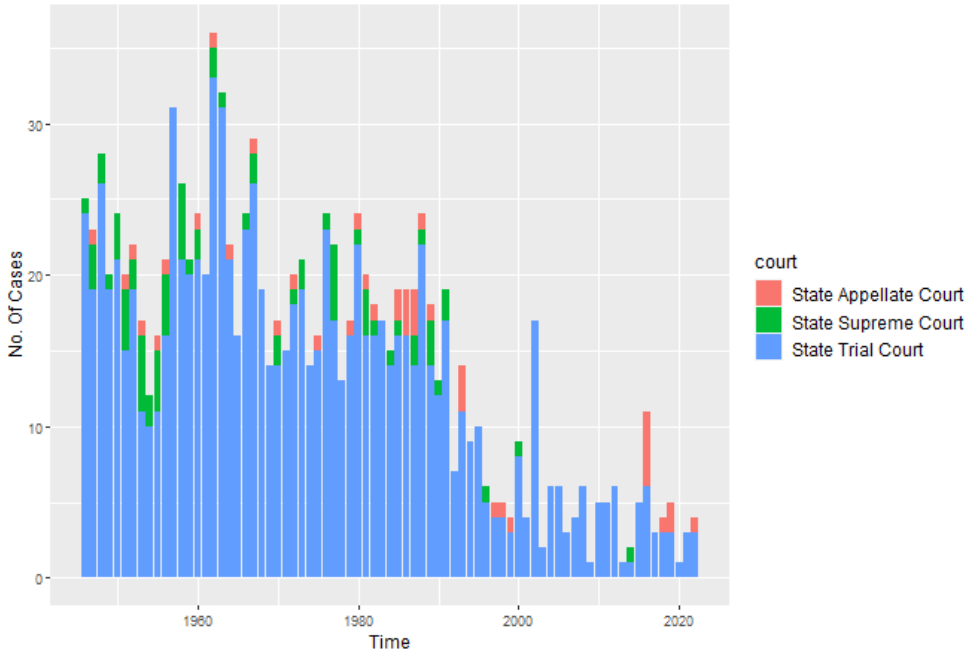
Table 33: Number of Cases argued in the US Supreme Court

Note: This table presents the estimation results of equation (5) The dependent variable is the logarithm of the number of cases appealed to the US Supreme Court after the decision in the state court system. Variables *Elect* indicates states with judicial elections for state supreme court judges, and *Ban* indicates the states that had imposed a ban on independent expenditures by unions or corporations, which were rendered unconstitutional by the Supreme Court in 2010. All regressions include state, year, case issue area fixed effects. Standard errors are clustered at the state level.

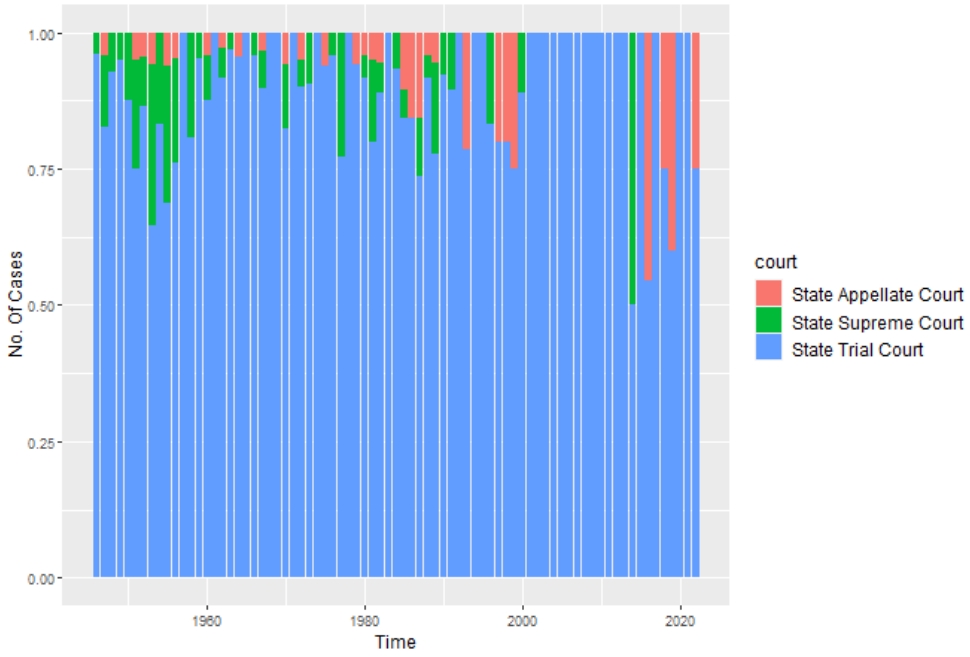
	(1)	(2)	(3)	(4)	(5)
	log(No. of Cases)	log(No. of Cases)	log(No. of Cases)	log(No. of Cases)	log(No. of Cases)
Ban × Post	0.03	0.01		0.11**	
	(0.05)	(0.04)		(0.04)	
Elect × Ban × Post	-0.04	-0.06	-0.06*	-0.19***	-0.08***
	(0.07)	(0.05)	(0.03)	(0.06)	(0.03)
State FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
Issue Area FE	Y	Y	Y	Y	Y
Excl. Criminal Cases	N	Y	Y	Y	Y
Excl. Civil and First Amendmt.	N	N	N	Y	Y
N	1,710	1,204	1,204	770	770
R-sq.	0.17	0.14	0.14	0.18	0.18

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

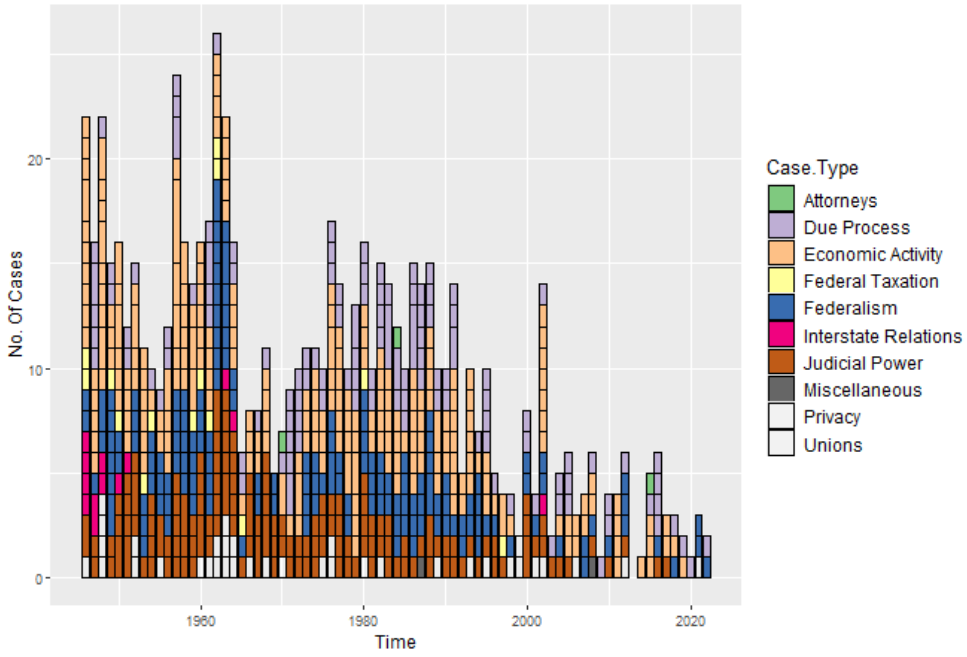


(a) No. of Cases appealed to the SCOTUS

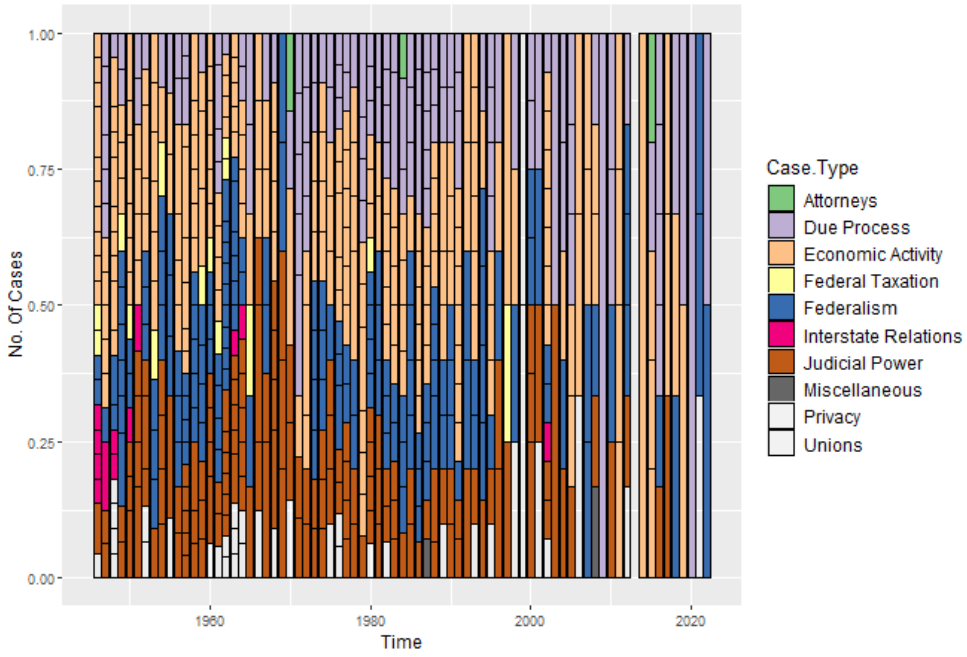


(b) Percentage Split of Cases appealed to the SCOTUS

Figure 22: Cases appealed to the SCOTUS



(a) No. of Cases appealed to the SCOTUS



(b) Percentage Split of Cases appealed to the SCOTUS

Figure 23: Cases appealed to the SCOTUS by Issue Area

Table 34: Case Decisions of Appeals in Supreme Court

Note: This table presents the estimation results of equation (5). The dependent variable in Columns (1)-(2) is the indicator function whether the Petitioner won the appeal, i.e. the decision of the lower court is reversed. In Columns (3)-(4), the dependent variable is the indicator function for the petitioner winning, and the court issuing a conservative decision. Variables *Election* indicates states with judicial elections for state supreme court judges, and *Ban* indicates the states that had imposed a ban on independent expenditures by unions or corporations, which were rendered unconstitutional by the Supreme Court in 2010. All regressions include state, year, case issue area fixed effects.

	(1)	(2)	(3)	(4)
	Petitioner Won	Petitioner Won	Cons. Decsn.	Cons. Decsn.
Ban × Post	0.23		0.17	
	(0.27)		(0.30)	
Election × Ban × Post	-0.25	-0.02	0.44	0.60***
	(0.33)	(0.20)	(0.35)	(0.21)
State FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Issue Area FE	Y	Y	Y	Y
Excl. Criminal, Civil and First Amendmt.	Y	Y	Y	Y
N	746	746	746	746
R-sq.	0.27	0.27	0.24	0.23

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

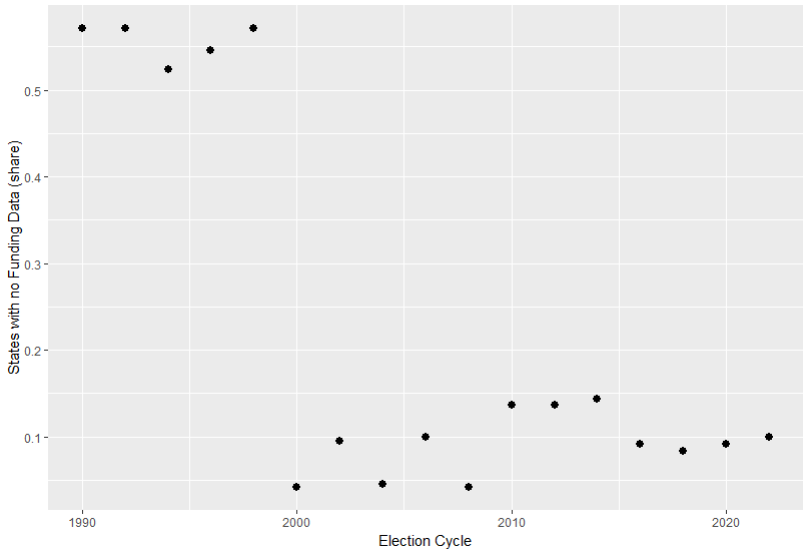


Figure 24: Judges Political Funding data coverage

B.1 Data Coverage

This section provides details about data coverage. As can be seen in Figure (24), the coverage of the direct expenditure from NIMSP is scant for the pre-2000 period. Therefore, for direct expenditures we focus on the period starting in 2000.