Judicial Selection and Production Efficiency: The Role of Campaign Finance

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Abstract

This paper studies the effect of campaign finance on judicial selection and production efficiency. Using the Supreme Court's surprise verdict in the Citizens United v. FEC case in 2010, which generates exogenous variation in campaign finance laws, I document that the removal of such bans led to a 33% ($ 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 populated with more business-friendly judges. State courts decide the majority of labor, contract, and administrative law disputes, and the State Supreme Court has the power to set legal precedents. Therefore, shifts in the judicial bench of the State Supreme Court affect the legal environment and the contracting choices of firms and labor. I document that labor productivity measured as value added per worker increased by 8% in treated states with judicial elections. For sectors more reliant on contract enforcement, labor productivity is higher in states with judicial elections. Overall, removing constraints on electoral finance improves competition in judicial elections, the judicial bench becomes more business-friendly, and improves production efficiency due to the alleviation of contract-enforcement frictions.

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

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Introduction

Formal contracts between trading parties reduce the role of trust, lower costs by promoting specialization, and allow parties to tide over uncertainty associated with production. Therefore, it is natural that strong legal institutions, particularly courts are a crucial determinant of economic and financial development, North et al. (1990), Acemoglu, Gallego and Robinson (2014), La Porta, Shleifer and Vishny (2002). The efficacy of the courts depends on the judges' ability to enforce the rule of law. Therefore, the quality of the courts and the judges depends on the judicial selection methods: election vis-a-vis appointment, and the ability of certain organized interests to affect judicial selection. For example, lobbies representing the interests of big business or unions may make campaign contributions to either the appointing authority such as the governor, or state legislators, or if the judges are elected then directly or indirectly to the judge running for election. In this paper, I ask whether and how judicial election and campaign finance affect the selection of judges, and evaluate its implications for contracting frictions and production efficiency.

Election, as opposed to appointment by the executive or the legislative branch, is a more democratic form of selection facilitating various interested parties to assert their preferences. For example, suppose that there is a bias in favor of labor unions in the state legislature. This may result in employment regulation that unfairly favors the workers, which puts a wedge between the observed and optimal factor choice and worsens production efficiency. Judicial elections may allow firms to get more business-friendly judges in the courts who may decide labor law disputes so that some of the bias due to the legislatures is mitigated and production efficiency improves. On the other hand, the uncertainty associated with electoral outcomes, coupled with the reliance of judicial candidates on special interest donors for campaign finance could lead to biased courts and subpar contract enforcement, which would then result in inefficient production. Whether the election of judges affects productivity positively or negatively is, therefore, an empirical question.

I answer this question in the context of the state Supreme Courts (or high courts) in

the United States.[1] This setting allows me to exploit the heterogeneity in judge selection procedures within the same country. I use the surprise Supreme Court ruling in the Citizens United v. Federal Election Commission (FEC) case in 2010 for exogenous variation in campaign finance laws. This 5-4 (5 judges out of 9 ruled in favor of Citizens United) split ruling rendered bans on independent expenditures in elections imposed by some states unconstitutional. As a result, around 23 states saw a lifting of bans from independent expenditures by corporations, unions, or both. Overall 22 states in the United States rely on judicial elections for the selection of judges to the high courts (State Supreme Courts) out of which 11 had instated such bans which got invalidated in 2010. Using a difference-in-differences research design where I consider the states where the bans on independent expenditure were lifted as the treated states and states without such bans that remain unaffected due to the Supreme Court ruling as control, I estimate the effect of campaign finance on judicial elections.

First, I document that lifting the bans increases the per-candidate average direct and independent expenditures by around $\$200,000$ in a judicial election. The pre-treatment average direct expenditure by a candidate is approximately $\$600,000$. Therefore, the average increase in the expenditure relative to the pre-treatment period is 33%. This finding is consistent with the interpretation that higher direct electoral expenditures are driven by a competitive response to higher independent expenditures due to the removal of restrictions. I use the categorization of campaign donations based on their source and document that a significant proportion of the increase in electoral expenditure is driven by monetary contributions from business interests and political parties. The lawyers and lobbyists, the biggest donor group for judicial candidates, do not spend more after the removal of bans. This is in line with the expectation that constraints on expenditure were not binding for this group. Interestingly, unions and other ideological groups also do not contribute more in response to the lifting of bans.

Second, I find that less restrictive campaign finance laws increase the competition in

[1]High Court and State Supreme Courts, both terms are used interchangeably to refer to the highest courts in the state judicial system.

electoral races. The vote margin, or the victory margin of winning candidates declines by about 20% following the removal of bans. On the extensive margin, the number of candidates per seat increases by 35%. Similarly, the incumbency advantage, i.e. the probability that an incumbent emerges as the victor in an electoral race declines by 20%, implying increased turnover of judges. Prior research has documented that the ideological leanings of the judges predict their decisions Bonica and Woodruff (2015), Windett, Harden and Hall (2015). I find that the average ideological leaning of the judges tends to be more business-friendly in states with judicial elections where the bans were removed. Moreover, this pattern of ideological leaning is reversed, in states without judicial elections where the bans were removed, i.e. the bench ideology leans more liberal (less conservative). The expectation that court decisions might lean in favor of certain parties affects whether and how a contractual dispute will be arbitrated in courts and therefore, contracting decisions among trading parties. For instance, Boehm and Oberfield (2020) shows that the quality of contract enforcement affects the firms' choice of production technology. Moreover, since the State Supreme Courts have the power to set precedents, they are effectively law-makers within the state (unless challenged by the US Supreme Court). The state courts are the arbiters of all contractual, labor, and administrative law disputes. Therefore, the expectation that decisions of the highest courts leaning in a particular ideological direction not only affects the parties directly involved in the dispute but also other parties operating within the state. I posit that the change in the ideological leanings of the highest courts affects the contracting choice of firms and labor, and ultimately the production efficiency and labor productivity.

I test whether less restrictive campaign finance laws affect production efficiency and factor productivity. I particularly focus on labor productivity, measured as the value added per worker at the state-sector level from the Annual Survey of Manufacturers. A sector is defined at the 4-digit NAICS level. I find that labor productivity is 5% higher for treated states relative to the control states. I further document that there are heterogeneous treatment effects of relaxing the campaign finance restrictions, depending on whether the state elects or appoints its judiciary. The labor productivity in USD value added per worker is

8% higher in states with judicial elections when the bans on independent electoral expenditures are removed, while there is no economic or statistically significant improvement in productivity in states where the judges are appointed. These results are robust to measuring labor productivity as USD value added per hour of labor and to the inclusion of sector-by-year fixed effects to account for time trends that affect the sectors differentially. The results are also robust to the inclusion of state-specific time trends to account for demand or industrial policy trends specific to each state. Thus, I provide evidence linking less restrictive campaign finance in judicial elections to higher factor productivity.

I posit that the increase in productivity could be due to improved production efficiency. To test for increased production efficiency, I test whether the production is more efficient in using material input. The revenue per unit raw materials cost increases by 22% in treated states with judicial elections. The value added per unit raw material cost also increases by 6% over the pre-treatment period in treated states relative to the control states. If production is becoming more efficient, there must be an increase in the marginal product of factors. This implies that firms will invest more in accumulating capital and employ more labor. I also test whether employment and capital expenditure growth rates are higher in treated states. I find a 8% increase in employment growth and an 11% increase in capital expenditure growth rate.

Alternatively, higher labor productivity could be due to a reduction in employment in treated states because firms find it easier to fire workers. I test whether the employment termination frictions have eased for the firms. Note that if the employment termination frictions have eased then employment growth rates should decline in the treated states with judicial elections. Rather, I find evidence that indicates an increase in employment and job creation rates. The ability of corporations to spend more on judicial elections could also result in weaker collective bargaining power of the unions. In this case, we may expect that the wage rates in treated states should be affected. I proxy for the average wage rate as the ratio of the total wage bill and the number of employees. I find that the growth rate in wages is 2% lower in treated states with judicial elections. However, this effect is

not robust to the inclusion of state-specific time trends. However, the increase in capital expenditure and employment growth are robust to state-specific time trends. Therefore, I conclude that the improvement in labor productivity and employment growth could not be solely driven by changes in the collective bargaining power of the workers due to the removal of campaign finance restrictions in states with judicial elections.

To examine the mechanism behind the improved production efficiency, I test whether the production efficiency is driven by sectors that are more reliant on contract enforcement for their production process. I use a measure of sector-specific supplier concentration, as in Levchenko (2007) to distinguish between sectors more and less reliant on contract enforcement. The rationale behind the measure is that production technology is institutionally dependent if the risk of expropriation by input suppliers is higher. This would be true if the product requires a complex mix of inputs from suppliers in different sectors. I proxy this input complexity as the inverse of the Herfindahl-Hirschman Index (HHI). The lower the HHI, the more complex the input product mix and the production is more reliant on contract enforcement with suppliers. I use the input-output matrix data from the Bureau of Economic Analysis that provides information on supplier relationships between various sectors. I find that labor productivity increases after the removal of bans on independent expenditure in judicial election states for sectors more reliant on contract enforcement, indicating a decline in contractual frictions. There is no effect on the productivity of contractually intensive sectors in the states without judicial elections. I also find evidence supporting the improvements in production efficiency for such sectors in the form of increased capital expenditure and employment growth rates, and higher revenue as a fraction of input costs.

Finally, I test whether sector-level improvements in productivity also hold at the plant level. I rely on the National Establishments Time Series (NETS) data and focus on the sample of standalone firms to overcome the imputation of revenue for firms with multiple establishments.[2] I show that the average productivity (revenue per employee) is higher

[2]The sales figures in the survey of establishments are reported at the firm level, and are then imputed using the industry classification for subsidiaries.

by 6% due to the lifting of bans in states with judicial elections. Although the average firm productivity is higher due to the changes in the legal institutions, there could be increased misallocation as some larger more established incumbents benefit more from this law change. This would be reflected in higher dispersion in productivity along the lines of Hsieh and Klenow (2009) and Sraer and Thesmar (2023). I find no evidence supporting increased misallocation because the dispersion of productivity is lower, however statistically insignificant for treated states with judicial elections.

In summary, this paper highlights a novel channel through which campaign finance affects productivity. Particularly, following the removal of bans on independent expenditures, the campaign expenditure and competition in judicial elections increased. The effect of such bans on the ideological composition of the judicial bench depends on whether the judges are elected in popular elections or appointed by the legislative (or the executive) branch. Productivity increases only in states with judicial elections, which also experience a shift in the ideology of the judicial bench. The labor productivity increase seems to be due to a reduction in contract enforcement frictions. I abstract from the welfare consequences of such a less restrictive campaign finance policy but emphasize that reducing constraints on political expenditures improves factor productivity, particularly in states where the selection procedure for judges is more democratic and less immune to capture by a minority coalition.

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 for identifying the effect of increased political expenditure on various economic variables of interest. This paper studies an alternative channel through which the laws may be influenced and the implications for productivity and establishment entry. 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 only operative in states with judicial elections.

The paper also contributes to the literature that studies 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. I find that political interventions that increase political expenditure may increase electoral competition and increase factor productivity, particularly in states where the judiciary is elected. Moreover, the within-sector dispersion of productivity, a measure of factor misallocation does not increase along the lines of Hsieh and Klenow (2009), and Larrain and Stumpner (2017). I use a difference-in-differences

approach to quantify the misallocation as in Sraer and Thesmar (2023) and document that the improved productivity does not come at the expense of higher factor misallocation.

The paper is organized as follows. I begin with a discussion of the institutional background and research design and argue the plausible exogeneity of the treatment assignment. The following couple of sections document the effect on judicial elections and electoral expenditures of judicial candidates. I then present the main results on production efficiency, followed by the evidence on the heterogeneous effects on contract-reliant sectors and plausible mechanisms 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 become 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 fill up 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. Either the judges are appointed by the governor, the state legislatures, or through a merit plan. Table (28) in the Appendix provides more details regarding the selection procedures for judges across different states.

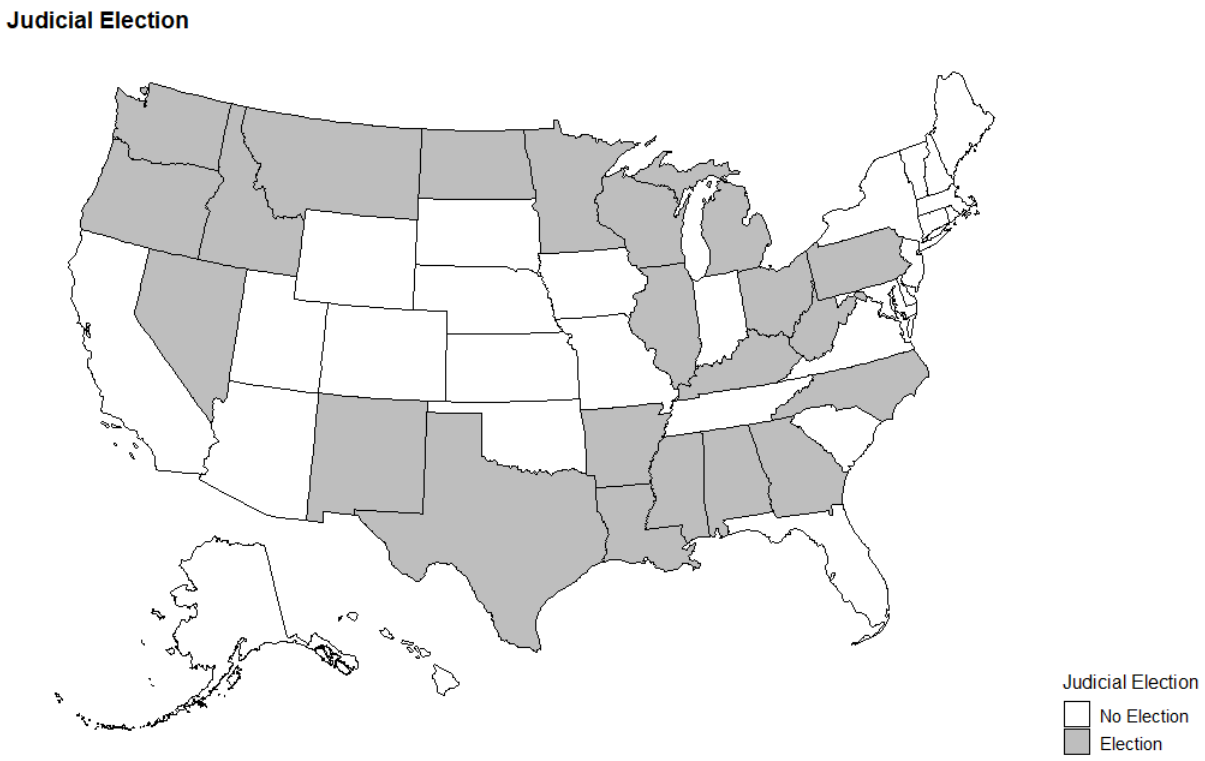


Figure 1: States with elections for the Supreme Courts in Grey. 22 states have judicial elections.

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, whereas 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 expenditure is referred to as the direct expenditure. Additionally, there are independent advertisements run on behalf of the candidate that 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 (19) 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 indirect expenditures, the disclosure laws for independent expenditures are quite lax. The anonymity of donors makes indirect expenditure the most preferred conduit for the flow of dark-money in elections. 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 insti-

tutional 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 Supreme Court ruling, these bans were lifted.

Judicial elections differ from legislative elections along 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, 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 (11) 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.

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

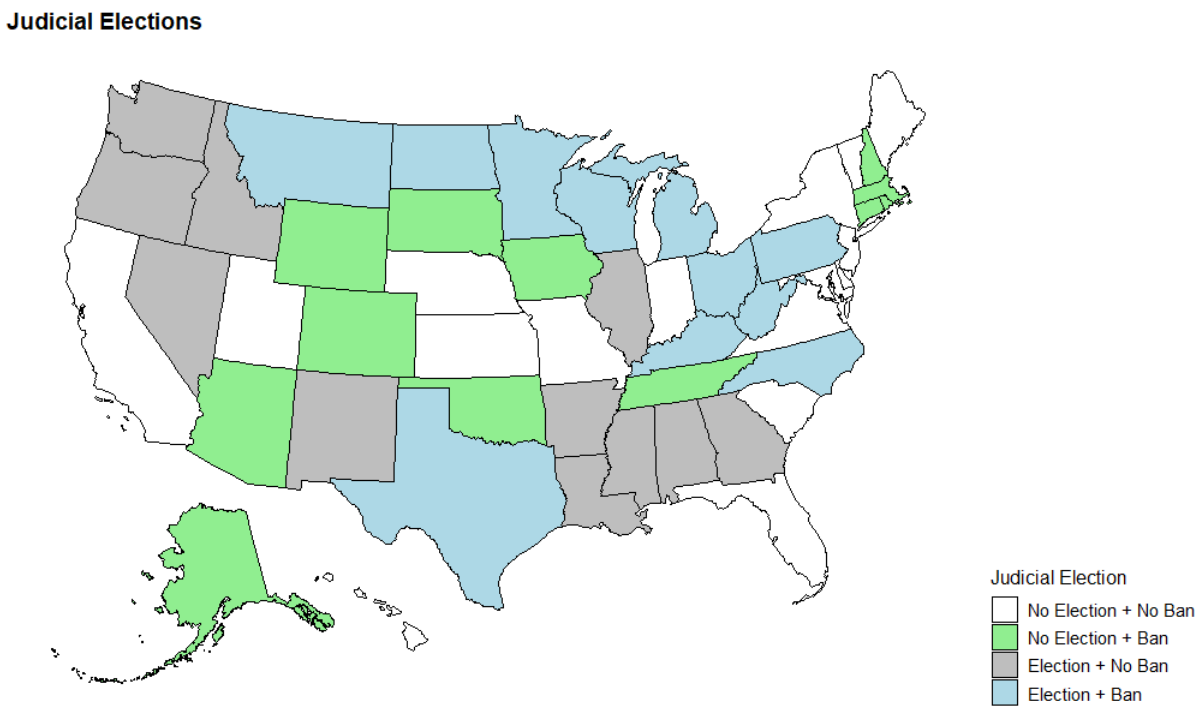


Figure 2: States that imposed some form of ban on independent expenditure are highlighted in blue and green. States with judicial elections, but no bans are in grey. States without judicial elections and no bans are in white.

data for all 50 states only since 2000.[3] Overall, we have data covering the direct expenditure of 907 judicial candidates for 23 years, with 1235 candidate-year observations. Figure 22 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.[4] Therefore, the judge election races are not as closely 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

[3]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."

[4]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

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

	Mean	SD	25th percentile	Median	75th percentile
Revenue (000 USD)	2,440,108.9	6,700,857.8	487,978	1,074,107	2,378,558
CapEx (000 USD)	67,778.2	187,605.3	9,313	25,788	64,468
Emp. (000)	4,002.5	5,611.3	1,184	2,252	4,394
Value Added (000 USD)	1,061,474.0	2,147,388.9	225,833.5	505,968.5	1,114,708
Lab Prod (000 USD/emp)	280.3	293.8	137.8	197.3	312.5
Rev/Mat Cost	2.1	0.7	1.7	2.0	2.4
wage (000 USD/emp)	41.9	13.5	33.2	39.9	48.1
Observations	33,620				

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 4000 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-difference 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,

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

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 ($Elect = 1$), and states without. The estimate of the conditional average treatment effect (CATE) is,

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

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 ($Elect \times Ban = 1$) 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 (19) 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. 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)	Lab Prod (USD/emp)	Lab Prod (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)
Elect × 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

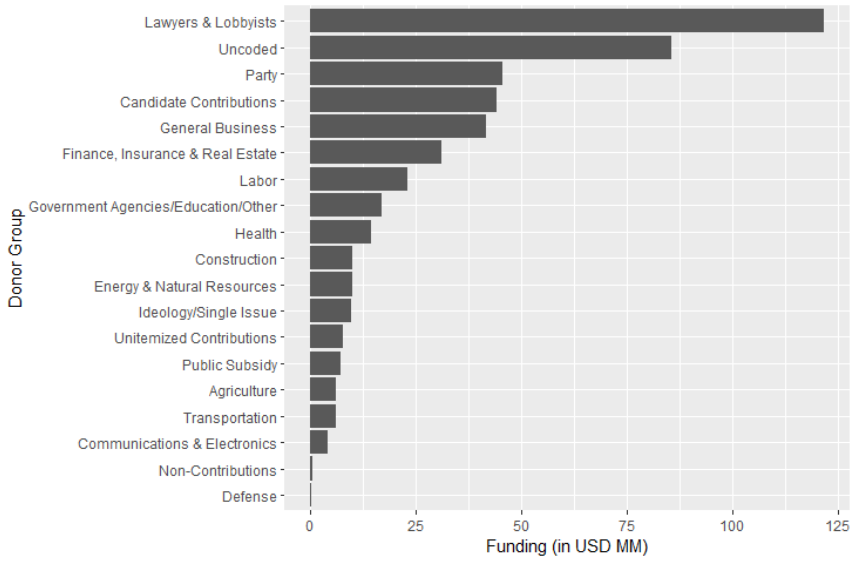
3.1 Political Expenditure in Judicial Elections

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.

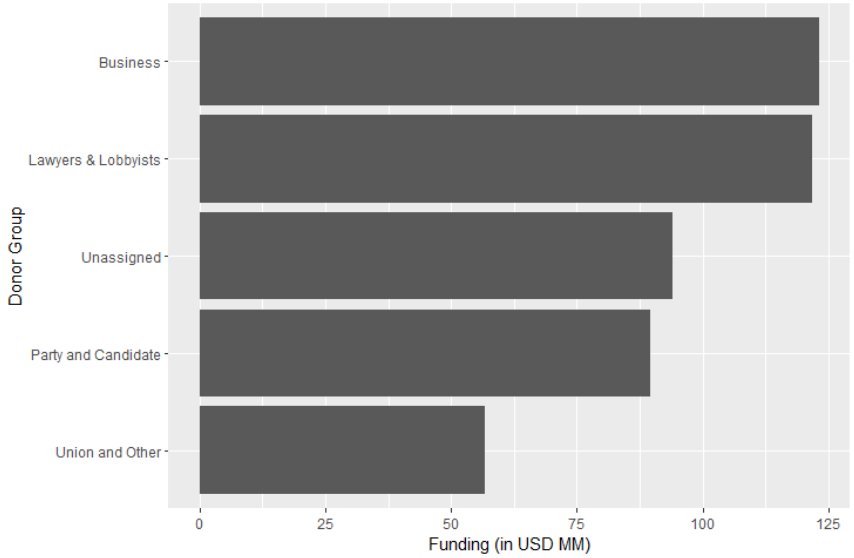
First, we consider the aggregate campaign contributions from different sources over the sample period, $2000 - 2021$. In Figure (3), Panel (a) I show the categories, as classified by NIMSP. In Panel (b), I combine these into the broad categories of Business, Lawyers, Party, and Unions. Small itemized contributions and donations where the source could not be attributed are classified as unassigned. 20% of the donations are unassigned.[5] 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. Note that the campaign contributions have a right-skewed distribution.

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

[5]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

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

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 (4) we compare the average direct expenditure 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 due to Removal of Bans

First, we must 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, we examine if there is a competitive response that

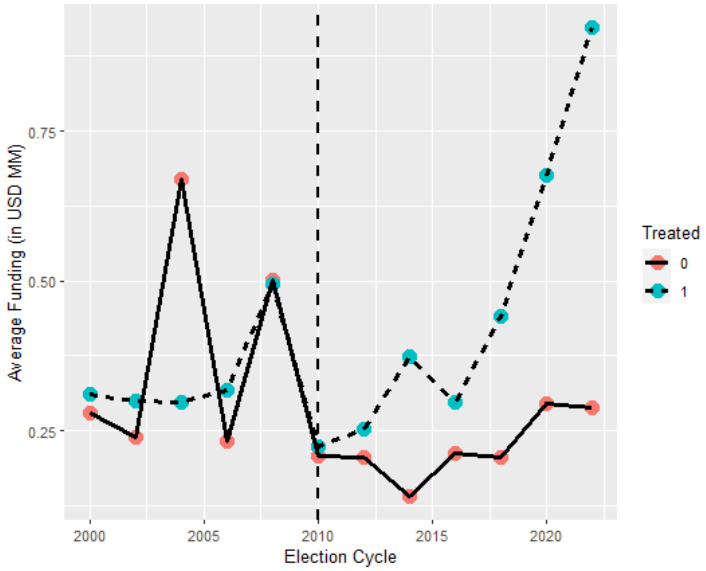


Figure 4: Average funding in USD for judicial elections in Treated vs. Control States

increases direct expenditure. To test for these hypotheses, I estimate regression equation.

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

in the regression specification $y_{ist} \in \{fund_{ist}, indexp_{ist}\}$ 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, we 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.[6] I estimate the following regression equation,

[6]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) Fund (in MM)	(2) Fund (in MM)	(3) Ind. Exp. (in MM)	(4) 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

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

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

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

In Table (6), we 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

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.

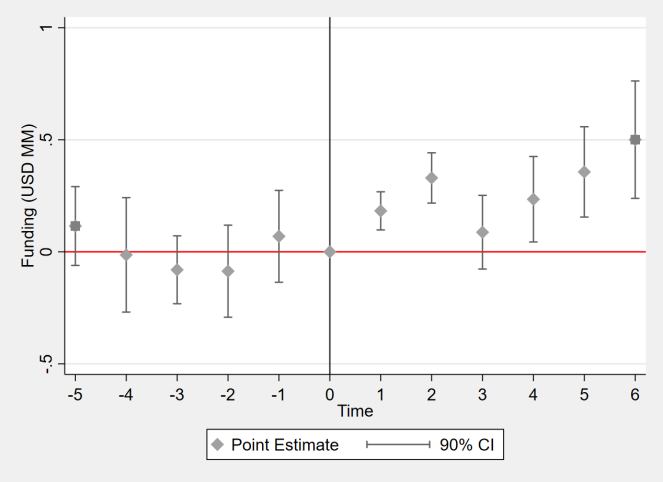


Figure 5: Event study plots. Event time is the 2009-2010 election cycle.

exerting influence, or due to the lack of financing. 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 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 Funding (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.

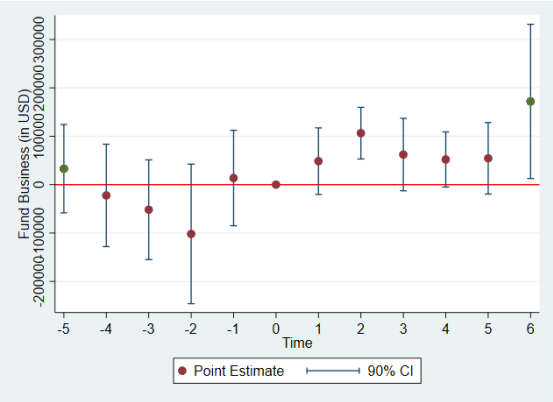
	(1)	(2)	(3)	(4)	(5)
	Business	Party and Cand.	Union and other	Lawyers and Lobbyists	Unassigned
Ban × Post	0.08*	0.08**	-0.00	-0.04	0.07**
	(0.05)	(0.03)	(0.02)	(0.03)	(0.03)
Election Cycle FE	Y	Y	Y	Y	Y
State FE	Y	Y	Y	Y	Y
N	1,070	1,070	1,070	1,070	1,070
R-sq.	0.21	0.16	0.17	0.21	0.32

Standard errors in parentheses

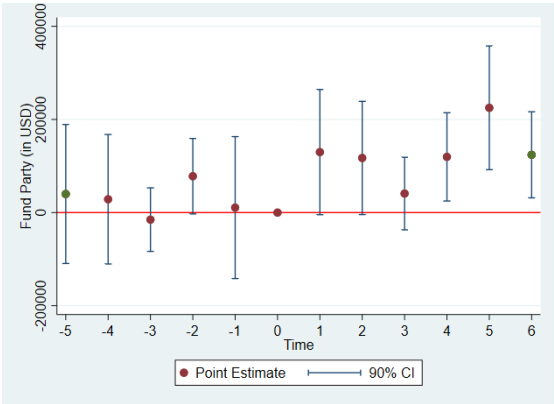
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Discussion Earlier in this section, we 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), we consider the average indirect and direct expenditure for candidates who are challengers, incumbents, or

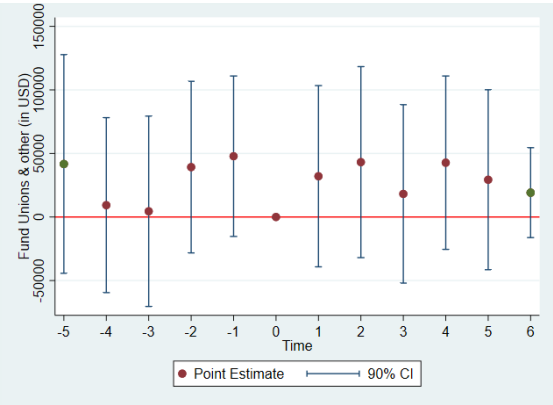
Note: This figure presents the estimation results of Equation (3). The dependent variable is the political expenditure by a judicial candidate, measured in USD MM from various sources such as Business groups in Panel (a), Party and self-funding from candidates in Panel (b), and funding from unions and 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 & Candidate



(c) Funding Unions & other

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

competing for an open seat. We 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, 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 akin to Osborne and Slivinski (1996). 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, we first test if removing constraints on indirect expenditure increased entry in judicial elections. Next, we 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 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), Columns (2) and (3) implies that there is a 15% reduction in the vote margin of the victors. Therefore, the electoral races have become more competitive after the relaxation of funding restrictions by the Supreme Court ruling. Figure (7) plots the average vote margin of victors in each election cycle. We can see a clear decline in vote margin in treated states relative to the control states after the 2010 Supreme Court decision. In Column(6), 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

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
no. of seats	2.28***	1.41**	-0.23***	-0.06	-0.15***
	(0.55)	(0.58)	(0.06)	(0.05)	(0.04)
Treat × Post	0.57**	0.57**	-0.16**	-0.15**	-0.24***
	(0.23)	(0.26)	(0.06)	(0.06)	(0.04)
Treat× Post × Partisan					0.20
					(0.12)
Cycle FE	N	Y	N	Y	Y
State FE	N	Y	N	Y	Y
Elect. type FE	Y	Y	Y	Y	Y
Observations	675	675	638	638	638
Rsq.	0.29	0.41	0.14	0.33	0.34
F	10.41	8.86	20.38	3.10	4.52

Standard errors in parentheses

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

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 bid also declines after 2010. In Table (24), (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.

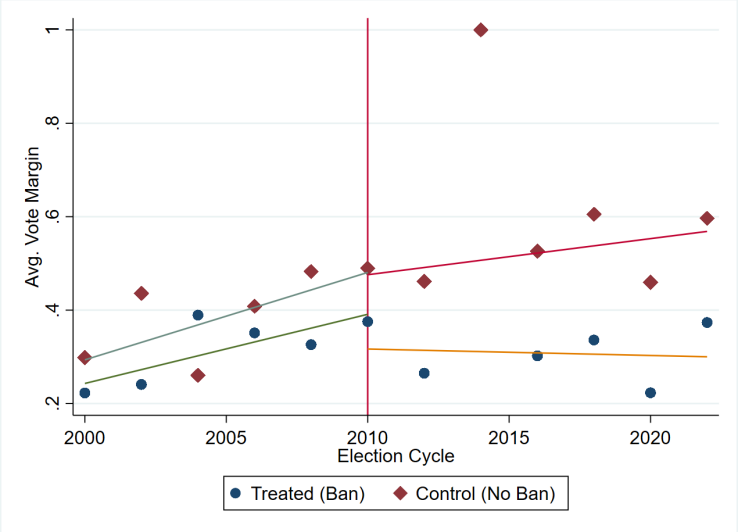


Figure 7: Vote Margin in Treated v. Control States

3.4 Effect on Bench Composition

The state supreme court decisions are made 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. However, how is the bench of the state supreme court affected? In this section, I provide evidence that the judicial bench composition 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, thus constructed predict the votes of judges, and therefore, the ideological leaning of the judges affects how cases may be decided.

$$y_{st} = \delta_s + \delta_t + \beta_{ep} \cdot (Elect \times Post) + \beta_{bp} \cdot (Ban \times Post) + \beta ebp \cdot (Elect \times Ban \times Post) + \varepsilon_{st} \quad (4)$$

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.

Table 10: Effect on Ideology (Common Space CFscore)

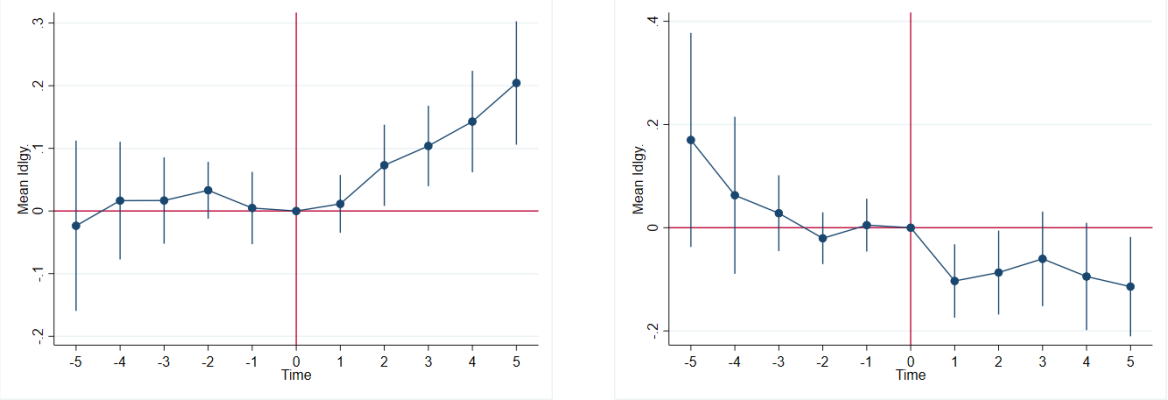
Note: This table presents the estimation results of equation (1). Columns (1)-(2) show results with median ideology (CFscore) of the judicial bench, which takes values in $(0, 100)$, with higher values indicating a more liberal ideology of a given state s in year t as the dependent variables. Columns (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 Idlgy.	(2) Mean Idlgy.	(3) SD Idlgy.	(4) SD Idlgy.
Ban × Post	-0.15	-0.25**	-0.04	-0.01
	(0.16)	(0.13)	(0.09)	(0.09)
Elect × 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)

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 as an intermediate good. The price of the final good is the numeraire. The final 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).[7] Such an assumption is reasonable if we assume that the supplier of the factor faces limited liability or the court system is biased against the final goods and imposes a higher cost as punishment. 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 $p \to 1$. In equilibrium, 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 is

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

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

4.1.1 Electoral Accountability

The distortion in input choice is due to biased courts. Production efficiency will improve if such distortion is lower. However, what is the role of elections and campaign finance in the 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 a single incumbent judge I, seeking re-election. The judge inherently wants to decide cases based on his ideological leanings, which increases distortion τ. This distortion affects the input choice. For now, we rely on our insight from the previous section and model the voter's preferences in a reduced form way so that they 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 also enters the race, the voters observe a noisy measure of the distortion

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

Lower value of alignment η 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 or where the ideal public preference is. However, he anticipates that the the voters do not observe η, but infer the level of rents from the observed distortion $\tilde{\tau}$ and vote. η and ν are independent and log-normally distributed.

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

We can solve the model by backward induction. At $t = 2$, 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 $t = 0$. With the updated belief, the probability that the incumbent wins conditional on choosing r_1 is

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

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, we test whether less restrictive campaign finance leads to higher productivity and employment. We 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 efficient production.

4.2 Productivity and Campaign finance

In this section, I test whether judicial elections along with the removal of bans on independent expenditures affect labor productivity. I measure labor productivity as the total

value added in USD per worker. I estimate the following regression specification,

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

where y_{jst} represents a quantity of interest for 4-digit NAICS sector j in state s at time t. δ_i for $i \in \{j, s, t\}$ are sector, state and time fixed effects. I also allow for state and sector-specific time trends. Table (11) illustrates the effect of the Supreme Court ruling invalidating bans placed on independent expenditures in elections on labor productivity. Column (1) shows that labor productivity increases by $\$13,000$ per worker in treated states. We control for state-sector specific factors such as suitability of geographic conditions by a more restrictive state-by-sector fixed effect, which also accounts for global time-invariant cross-sectional differences in productivity across different states and sectors. We control for sector-specific time trends by a sector-by-year fixed effect. Column (2) and (3) show the heterogeneous effect in judicial election and non-election states, and that the within sector productivity increases significantly in states with judicial elections after the removal of bans on independent expenditures. Column (4) shows that the results are robust to the inclusion of state-by-sector fixed effect to account for time-invariant factors that may lead to assortative matching between states and sectors, such as state constitution and geographic features. Column (5) illustrates that the effect on productivity is robust to the inclusion of state-specific linear time trends indicating that the effect on productivity is not picking up differential productivity trends across the states.

We verify that the labor productivity increase is not driven by more intensive utilization of the labor force. In Table (12), we show that the increase in productivity measured as value added per hour of labor also increases and the growth percentages are as documented in Table (11) for value-added per worker.

4.3 Effect on Investment and Employment

The increase in labor productivity may be due to the firms facing lower employee termination costs, due to decline in the collective bargaining power of workers. It could also be due

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) Lab Prod (USD/emp)	(2) Lab Prod (USD/emp)	(3) Lab Prod (USD/emp)	(4) Lab Prod (USD/emp)	(5) Lab Prod (USD/emp)
Ban × Post	11.08*	-7.27			
	(6.21)	(10.58)			
Elect × 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) Lab Prod (USD/hr)	(2) Lab Prod (USD/hr)	(3) Lab Prod (USD/hr)	(4) Lab Prod (USD/hr)	(5) Lab Prod (USD/hr)
Ban × Post	6.62**	-3.00			
	(3.15)	(5.21)			
Elect × 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

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

to higher factor productivity in the treated states. Moreover, if production efficiency due to better enforcement of contracts with suppliers is at play, then the revenue per unit cost of input materials should be higher. If the reduction of contractual frictions is the cause of increased productivity, 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. Moreover, if the bargaining power of the workers is lower, the wages should be lower. In Table (13), Columns (1) through (3) provide evidence for the higher productivity gains for sectors in states with judicial elections. Column (4) shows the estimation with the logarithm of average wage, i.e. the total wage bill divided by the number of employees, as the dependent variable. If the collective bargaining power of the workers is lower we would expect a decline in the wage rates. There is no economic or statistically significant decline in wages. All other results in Table (13) are robust to the inclusion of state-specific time trends.

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)
Elect × 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 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 ban estimated in Equation (5) is going to be biased if there are pre-period differential trends in labor productivity. In order to verify whether there are pre-period trends we estimate the following dynamic event-study specification where D_t is the indicator for the year relative to 2010, the year of the Supreme Court 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, and Panel (b) shows that there is no effect on labor productivity in states where judges to the high court are selected through legislative or executive appointment. Figure (10) shows that the results also hold for productivity measured as value added per hour of labor.

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

4.5 Establishment Entry and Reallocation

In this section, we test whether the changes in the campaign finance laws affect establishment entry, and exit. I employ a different dataset compiled by the Census Bureau to answer this question. I use the business statistics and dynamics data which provides me with establishment entry, exit, and job reallocation rates at the 2-digit NAICS level by states at an annual frequency. In Table (14) we document that there is 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 nor statistically significant effect on establishment exit rates. Table (15) illustrates the increase in job reallocation rates, which is mainly driven by job creation rates. Around 50% of this increase in reallocation is due to the continuing establishments and the remaining due to the new-entrant establishments and the ones that die off.

Note: This figure presents the estimation results of equation (6). 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. Sector is defined at the 4-digit NAICS code level. Standard errors are clustered at the state level.

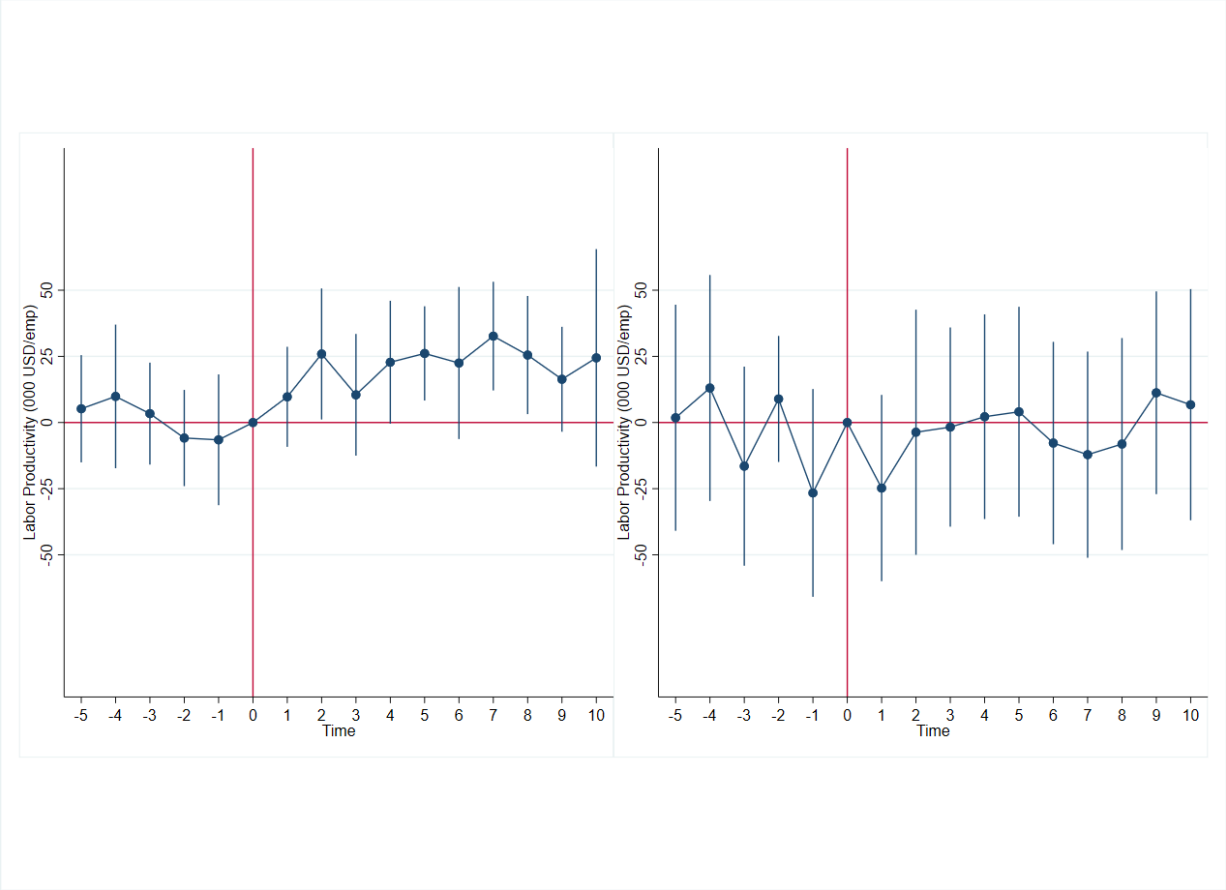


Figure 9: Effect on Labor Productivity in '000 USD /emp: States with Judicial Elections (Left), and without judicial elections (Right)

Note: This figure presents the estimation results of equation (6). 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.

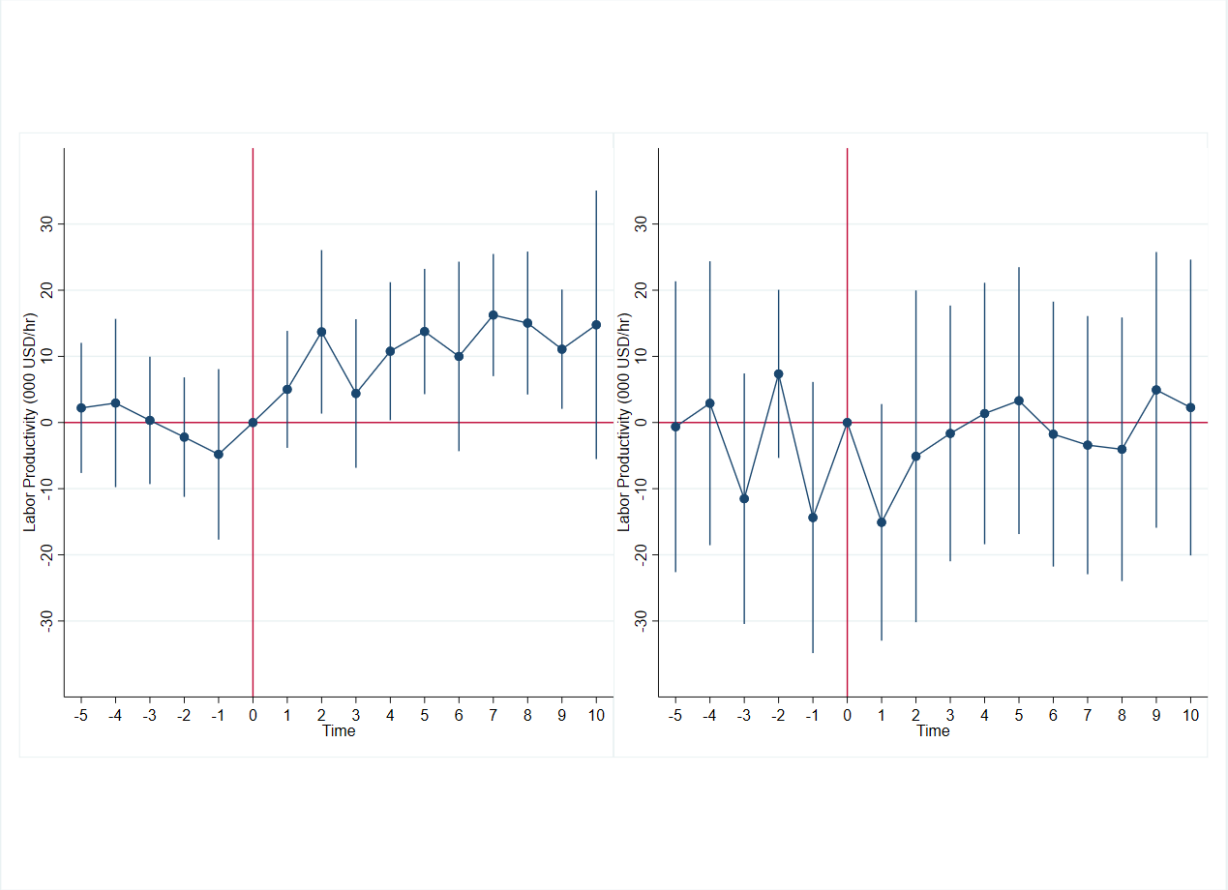


Figure 10: Effect on Labor Productivity in '000 USD /hr: States with Judicial Elections (Left), and without judicial elections (Right)

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
Elect × 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

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

Table 15: Job Reallocation Rates

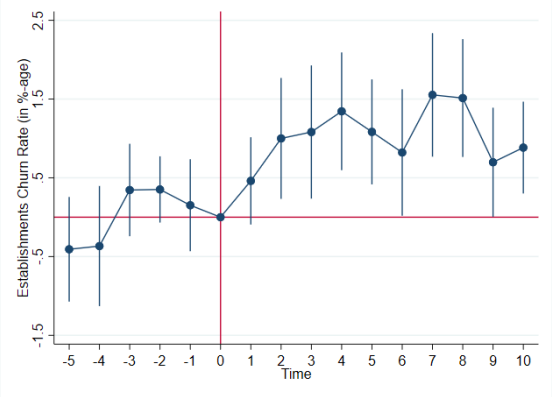
Note: This table presents the estimation results of equation (5). The dependent variables in Columns(1)-(3) are the job creation, destruction, and the sum of the creation and destruction rates, the reallocation rate. Columns (4) captures the job reallocation rate from continued establishments. All rates 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. 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)
	Creatn. Rate	Destructn. Rate	Reallocn. Rate	Reallocn. Rate (Contd. Estab.)
Elect × 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

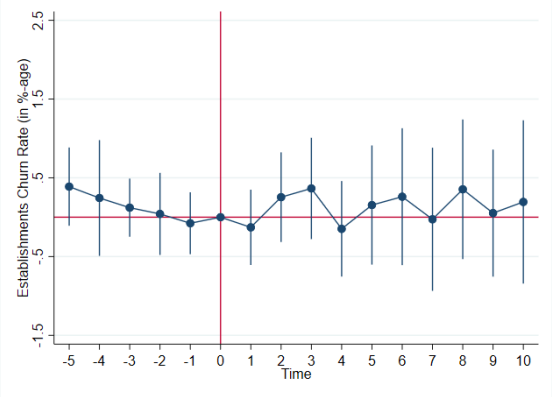
Standard errors in parentheses

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

Note: This figure presents the estimation results of equation (6). 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. 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 11: Dynamic effect on the Business Churn rate, sum of entry and exit rates of establishments.

5 Contract Reliance

I have documented that there has been an increase in sector-level labor productivity after the ruling in 2010. This effect is driven by sectors in states with judicial elections. So far, I have assumed that the treatment effect is homogeneous across sectors. However, one may expect that sectors more reliant on legal institutions for contract enforcement experience a more pronounced effect of more money in judicial politics. To create a measure of contract reliance, I rely on the methodology in Levchenko (2007) and Nunn (2005). The measure is based on the input specificity. The key idea is that if the product is complex, i.e. it requires inputs from several sectors then the firms operating in this sector are more exposed to incomplete contract risk with numerous suppliers. Therefore, such sectors are more institutionally reliant than others with fewer inputs and have worked out other means of solving the hold-up problem or are less susceptible to hold-up. I compute the measure from the input-output matrices compiled by the Bureau of Economic Analysis (BEA). More details 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. A negative sign is added so that a high *inputhhi* corresponds with an industry more reliant on contract enforcement (higher input specificity). Higher HHI of inputs corresponds to firms with lower contract reliance. Therefore, for the measure of contract reliance I use, $contint = -\log(inputhhi)$.

The regression specification,

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

where $\mathcal{I}(Elect, Ban, Contract, Post)$ includes all the 2 and 3 term interactions between the variables, except for those mentioned in Equation (7). The contract reliance variable is $Contract = \mathbb{1}(contint > \tau_{\{2/3\}})$ where $\tau_{\{2/3\}}$ denotes the 66-th percentile of contract

intensity . The coefficients on the variables, $\{Ban \times Post, Elect \times Ban \times Post, Ban \times Post \times Contract, Elect \times Ban \times Post \times Contract\}$ allow us to quantify the heterogeneous average treatment effects.

Table (16) provides the estimation results for the above specification. It is more important to compute the treatment effects of interest based on the above specification. First, I define the different quantities of interest and then later in Table (17), I test whether the effect of a relaxed funding constraint has a heterogeneous effect on sectors.

Table 16: 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

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

The average treatment effect of interest, The 2×2 differences in difference estimand for the effect on some real outcome of the relaxation of electoral funding restrictions due to the 2010 FEC ruling. I denote the incumbency advantage conditional on $\boldsymbol{X} = (Elect, Contract)$,

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

For example, the average treatment effect on industries that face higher reliance on institutions ($Contract = 1$) of the funding restriction getting relaxed for states without judicial elections is

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

Table 17: 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)
	Lab Prod (USD/emp)	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

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

Table (17) shows how the labor productivity increase is statistically and economically significant for sectors that are more reliant on contract enforcement. Moreover, this increase is only observed for states that hold judicial elections for high court judge selection. The corresponding effects on physical capital and employment growth rates are also higher for sectors more reliant on contract enforcement. No such heterogeneity in treatment effects is observed for wages indicating that the bargaining power of workers is likely not influencing the increase in labor productivity. Figure (12), estimates Equation (6) on two different sub-samples to highlight the dynamic effect of the change in campaign finance laws on the

productivity of contract-intensive and non-contract-intensive sectors. Panels (a) and (b) show the effect on states with judicial elections. Panel (b) confirms that the difference in labor productivity between treated and control states in the post-period is not significantly different than the pre-period difference. Panels (c) and (d) reaffirm our earlier observation that the effect on labor productivity is mainly driven by states with judicial elections.

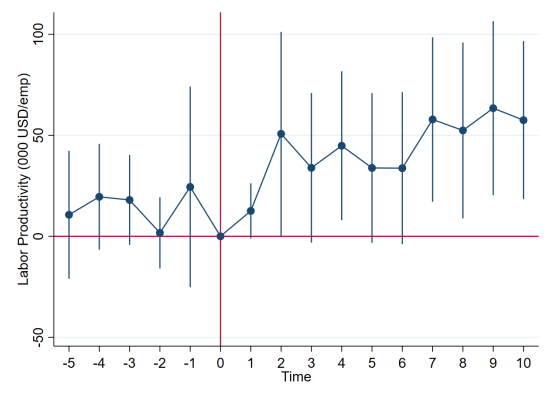
5.1 Effect on Firm Productivity

I use the National Establishments Time-Series (NETS) database 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. Following, Barnatchez, Crane and Decker (2017), I subset the data as follows. Focus on establishments with more than 10 employees and fewer than 1000 employees to avoid the effect of imputation on the measurement. I then compute the average productivity measured as revenue per employee ($MRPL$). Moreover, since sales figures are imputed for branches, to focus on local economic activity I subset the data for standalone firms. I discard the 1% tails of the $MRPL$ at the 4-digit NAICS-year level to avoid the effect of outliers in our computations. This gives us $152,198$ sector, state, time observations. Finally, I drop all the $sector \times state \times year$ cells with fewer than 8 establishments to reduce noise in the estimation of the first and second moments.[8] I end up with $39,446$ sector, state, time (in years) observations. Following Sraer and Thesmar (2023), I focus on 3 independent variables, $E[\log(MRPL)]$, $V(\log(MRPL))$, and $C(\log(MRPL), \log(y))$. The dispersion of $MRPL$ is indicative of misallocation within the economy.

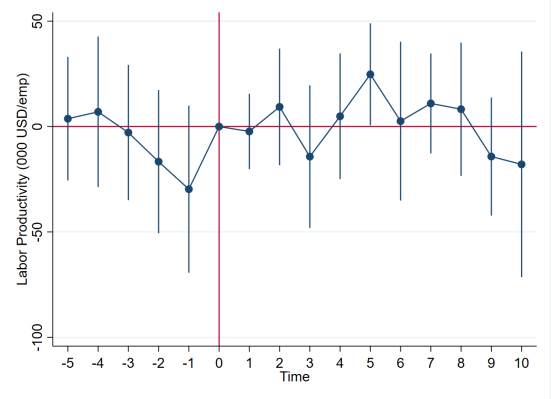
Table (18) illustrates how the average labor productivity is higher for the treated states, in line with the evidence presented earlier. Interestingly, it is plausible that the improvement in productivity comes along with higher dispersion in productivity, a measure of factor distortion as in Hsieh and Klenow (2009). Column (3) shows that the dispersion

[8]The result is robust to restricting attention to more than 8 establishments per cell. Later in the appendix, I replicate the results on average productivity without this restriction and the conditional average treatment effect remains robust around US $18,000$.

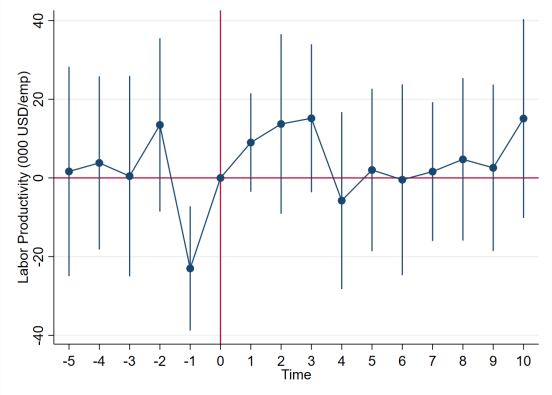
Note: This figure presents the estimation results of equation (6). The dependent variable is labor productivity, measured as USD value added per emp. Panel (a) shows results for states with judicial elections for state Supreme Court judges and Panel (b) for states that use some form of appointment. The 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.



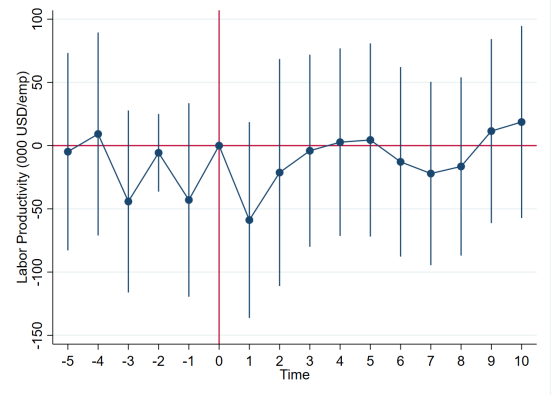
(a) Contract-intensive Sectors in States with Judicial Elections



(b) Non-contract-intensive Sectors in States with Judicial Elections



(c) Contract-intensive Sectors in States without Judicial Elections



(d) Non-contract-intensive Sectors in States without Judicial Elections

Figure 12: Effect on Labor Productivity in USD /emp

Table 18: 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)
	Lab. Prod (000 USD/emp)	E[log(Lab. Prod)]	V(log(Lab. Prod))	C(log(Lab. Prod),log(Rev))
Ban × Post	-7.45	-0.00	-0.01	-0.02
	(7.66)	(0.02)	(0.03)	(0.04)
Elect × 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

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

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.

6 Conclusion

In this paper, I show that political finance may increase electoral competition for judges, increase accountability, and improve judge performance, which could alleviate the contractual frictions faced by firms and increase factor productivity. Using the 2010 Supreme Court ruling that rendered bans imposed on independent expenditures by corporations and unions as unconstitutional. As a result, states that had imposed such bans experienced an increase in political financing. First, I document that the Supreme Court ruling led to an increase in political funding of judicial candidates. This increase was mainly driven by funding from businesses and political parties that may have been constrained due to the prior bans. Second, I document that the rise in competition for political finance is

also associated with increased competition in judicial races. 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. I further show that less restrictive campaign finance is associated with higher productivity of labor, particularly in states that have judicial elections. I then show the link between the removal of bans on political finance and higher labor distortion is particularly strong for sectors more reliant on contract enforcement and is operative in states with judicial elections. The state supreme court judges influence the legal environment in many ways, but in particular through the predictability and consistency in the enforcement of contract and administrative law. This paper provides evidence, that allowing firms to donate more freely in elections affects the selection of judges and alleviates the incomplete contract risk faced by firms which makes them more productive.

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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 19: 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

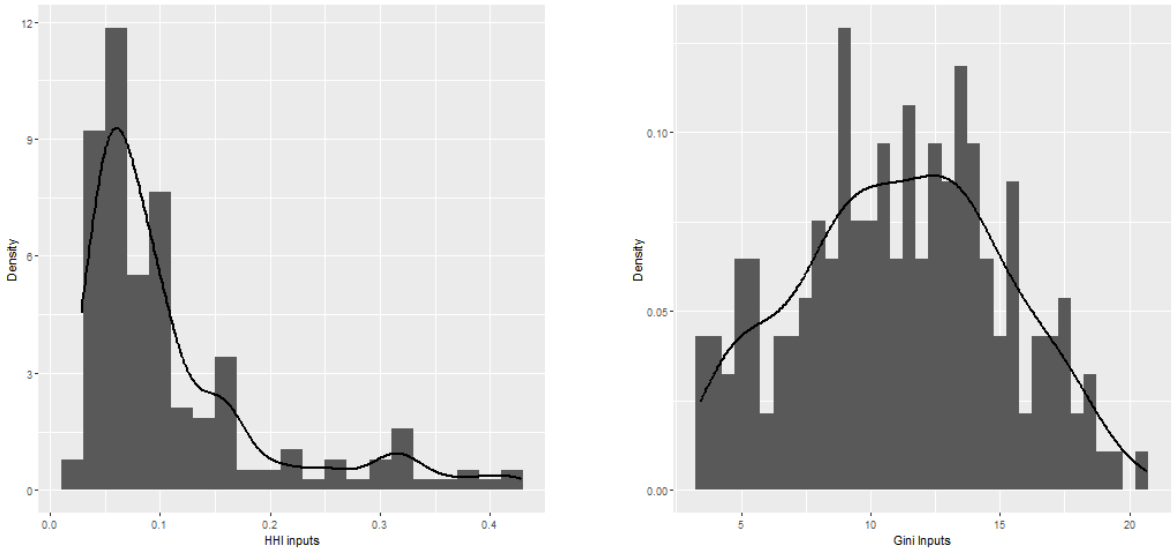
In this section, I show the results of the computations of the contract reliance measure described in the main text. I supplement the measure with another measure, the input Gini. All the results in the main text are robust to both measures. The *inputgini* is defined as,

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

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 20: Least and Most Contract Reliant Industries



(a) Density HHI inputs

(b) Density Gini inputs

Figure 13: 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 21: Summary Statistics of Input HHI

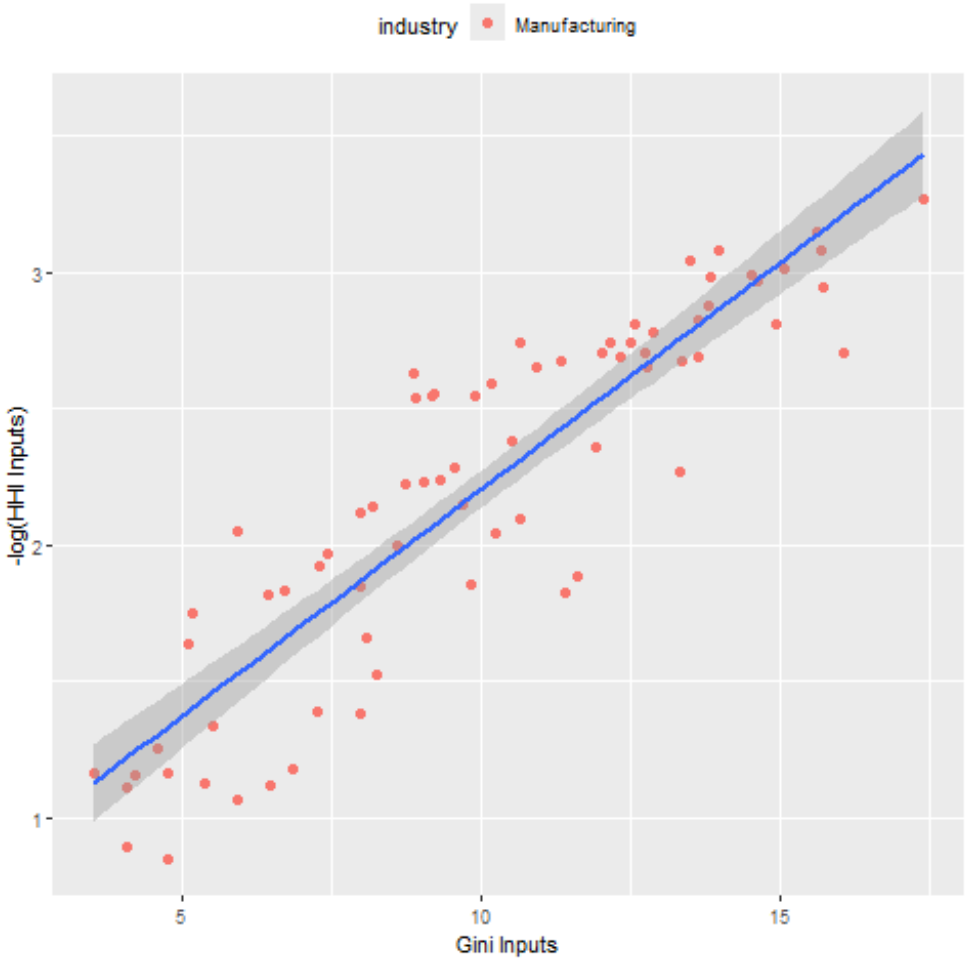
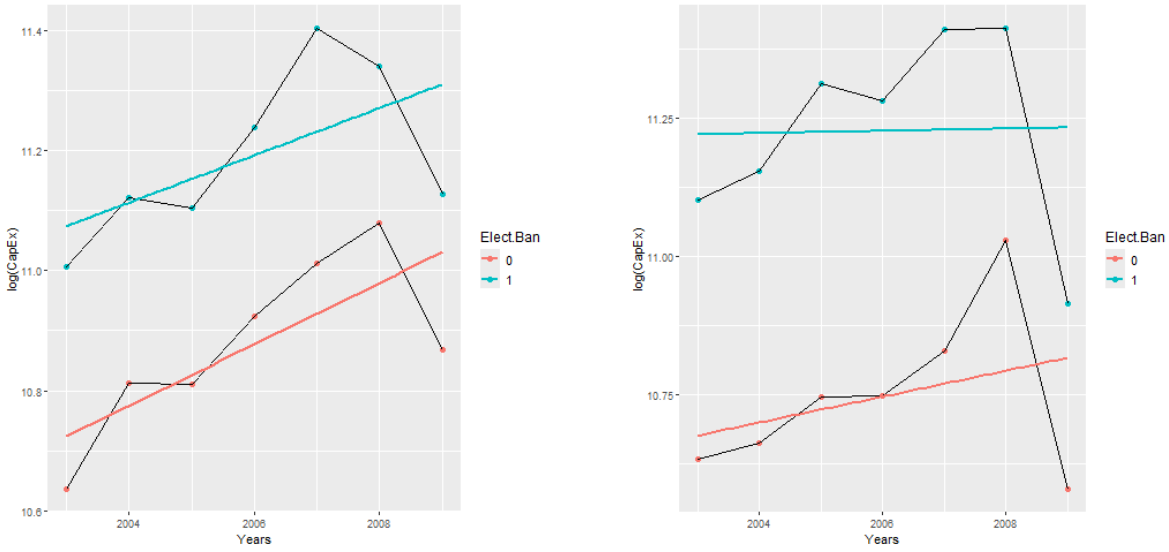


Figure 14: Relationship between Contract intensity Measures

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

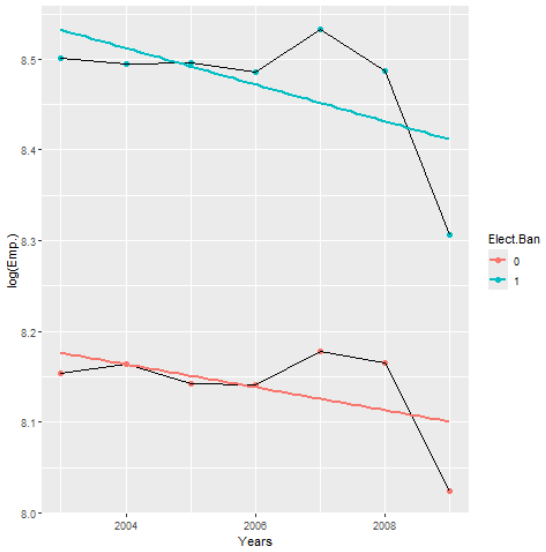
In this subsection, I document the pre-trends in capital expenditure, employment, and wages during the pre-period 2005-2009 for treated states with judicial elections across sectors with high contract reliance and low contract reliance.



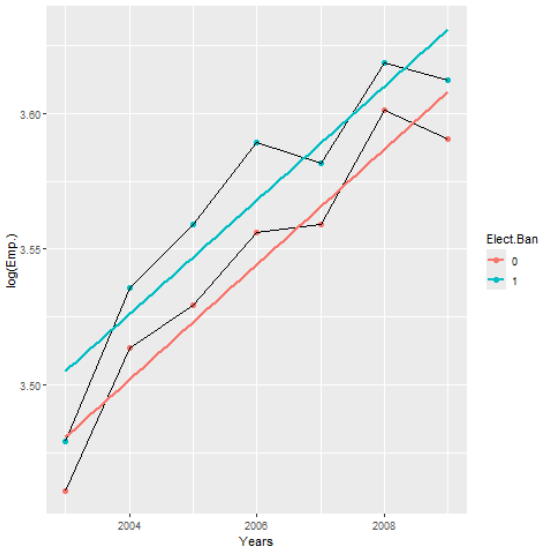
(a) Non-contract reliant Capex Pre-Trends

(b) Contract Reliant Capex Pre-Trends

Figure 15: Pre-Trends Capex

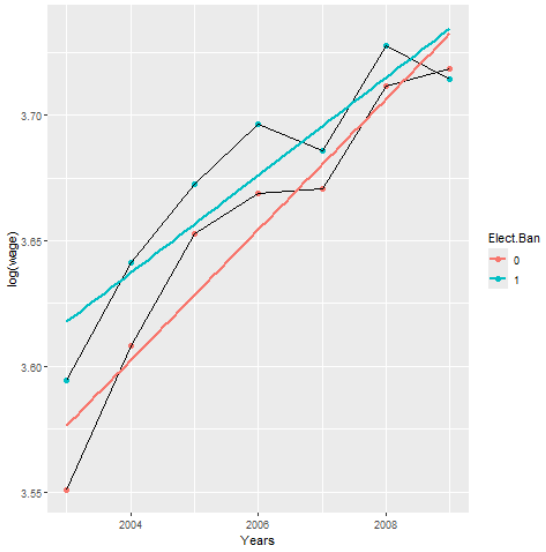


(a) Non-contract reliant Employment Pre-Trends

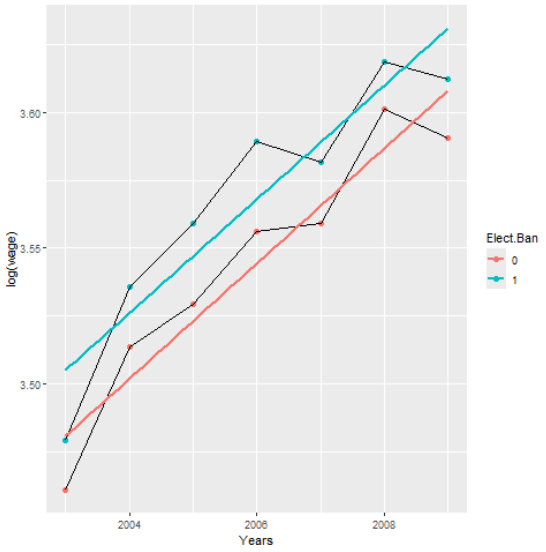


(b) Contract Reliant Employment Pre-Trends

Figure 16: Pre-Trends Employment



(a) Non-contract reliant Wage Pre-Trends



(b) Contract Reliant Wage Pre-Trends

Figure 17: Pre-Trends Wages

C Appendix: Independent and Direct Expenditures in Judicial Elections

First, I present the direct evidence on independent expenditures in Table (22). 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 Business, 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.

Table 22: 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) Unassigned	(2) Union and other	(3) Ideology	(4) Business	(5) Party and Cand.
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

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

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 check whether the corporation ban bites more vs. the union ban. Note that these tests are for the subset of states with judicial elections. In Table (23), the coefficient on $Corp.Ban \times Post$ captures the additional funding in states that imposed a corporation-only ban, relative to states 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.

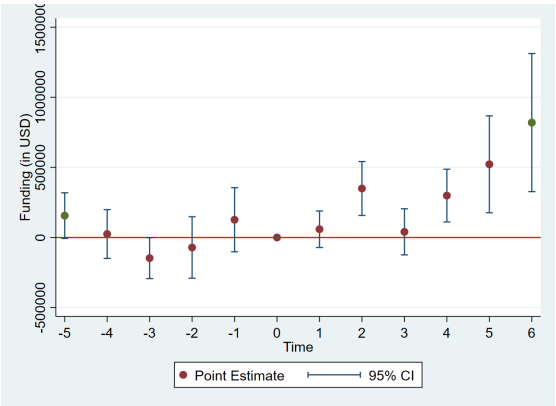
Table 23: Effect due to Corporations and Union bans

	(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

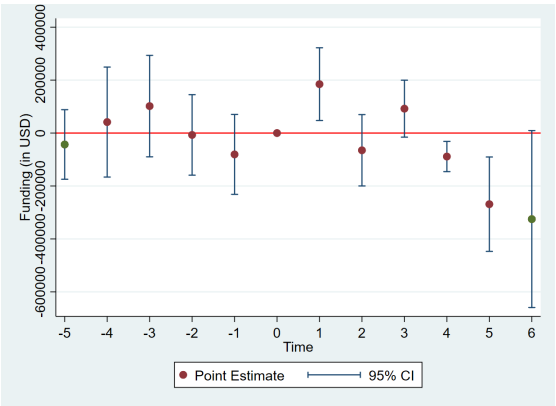
Standard errors in parentheses

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

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, the independent expenditures are higher for both treatments. The difference between the coefficients that captures the effect of the union ban being lifted, is not statistically significant at the 10% level. In fact, the p value is 0.68. This evidence points to higher independent expenditures mainly driven by states which imposed bans on corporations.



(a) Funding Unions Effect



(b) Funding Corporate Effect

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.

Table 24: 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

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

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 $\log(1 + \tau^K_{ijs}) \sim \mathcal{N}(\mu_{\tau sj}, \sigma^2_{\tau sj})$.[9] 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

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

[9] 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 Establishment Level Productivity

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 (26) and (27) 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 4.5, we test for this hypothesis.

Table 26: 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 27: 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 *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)
	log(Lab Prod.)	log(Lab Prod.)	log(Lab Prod.)	log(Lab Prod.)
Ban × Post	-0.00			
	(0.02)			
Elect × 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

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

A Online Appendix

A.1 Judicial Selection

In Figure (19), 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 19: Illustration of an advertisement for Judge Janet Protasiewicz (indirect Expenditure)

Table (28) 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 legistative 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 28: 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 (20), 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.

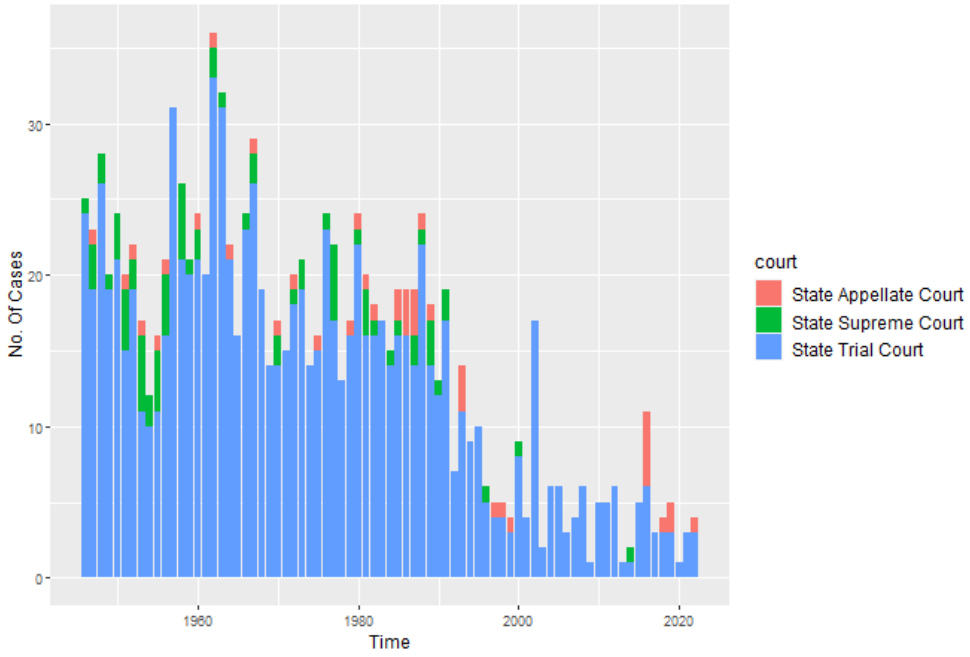
Table 29: Number of Cases argued in the US Supreme Court

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.

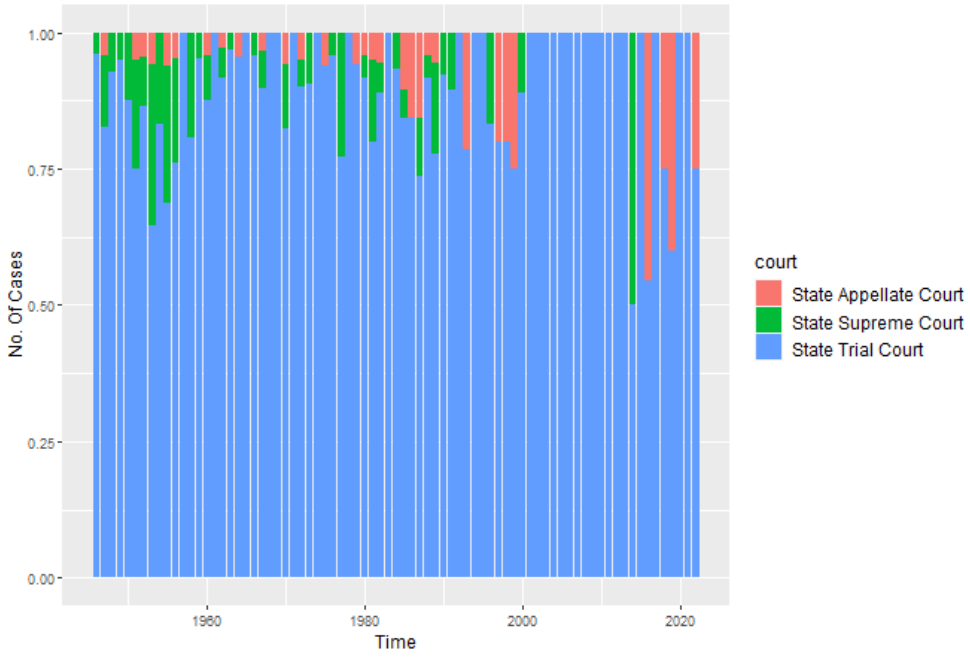
	(1)	(2)	(3)	(4)	(5)
	log(No. of Cases)	log(No. of Cases)	log(No. of Cases)	log(No. of Cases)	log(No. of Cases)
Ban × Post	0.03	0.01		0.11**	
	(0.05)	(0.04)		(0.04)	
Elect × Ban × Post	-0.04	-0.06	-0.06*	-0.19***	-0.08***
	(0.07)	(0.05)	(0.03)	(0.06)	(0.03)
State FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
Issue Area 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

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

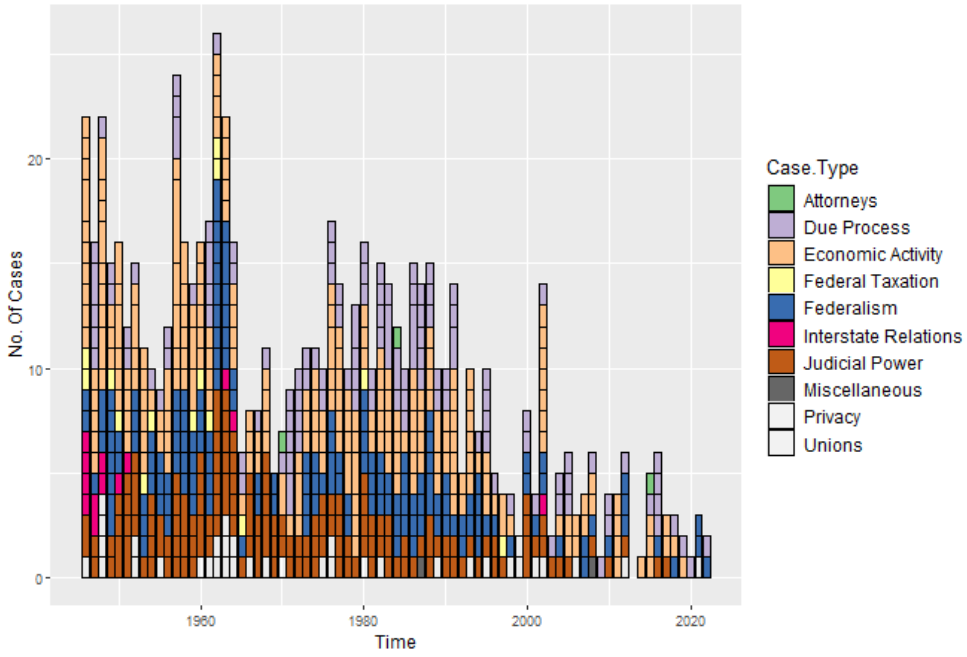


(a) No. of Cases appealed to the SCOTUS

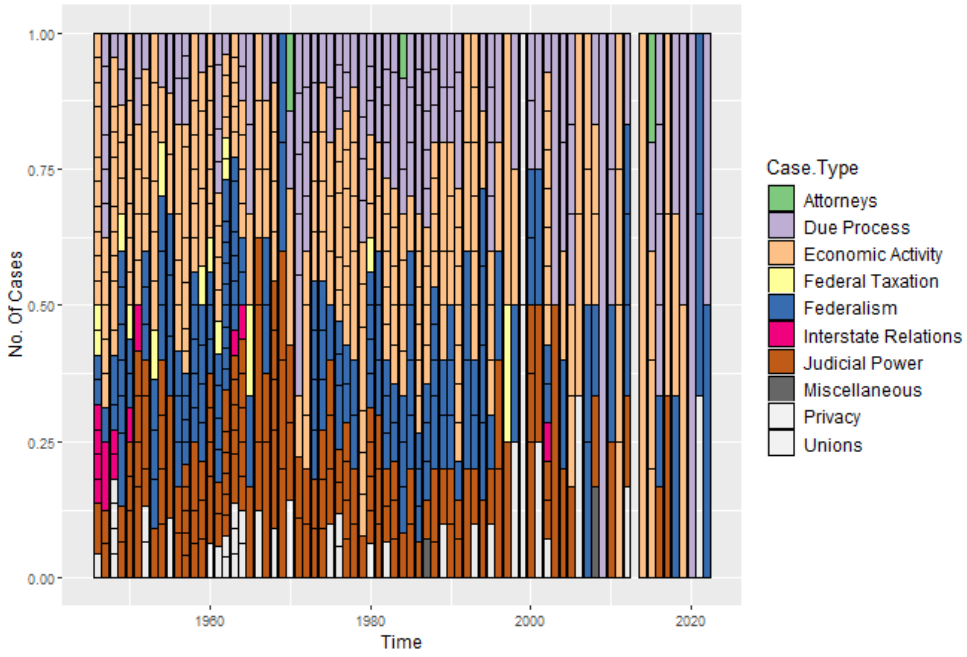


(b) Percentage Split of Cases appealed to the SCOTUS

Figure 20: Cases appealed to the SCOTUS



(a) No. of Cases appealed to the SCOTUS



(b) Percentage Split of Cases appealed to the SCOTUS

Figure 21: Cases appealed to the SCOTUS by Issue Area

Table 30: 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 *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.

	(1)	(2)	(3)	(4)
	Petitioner Won	Petitioner Won	Cons. Decsn.	Cons. Decsn.
Ban × Post	0.23		0.17	
	(0.27)		(0.30)	
Elect × 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

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

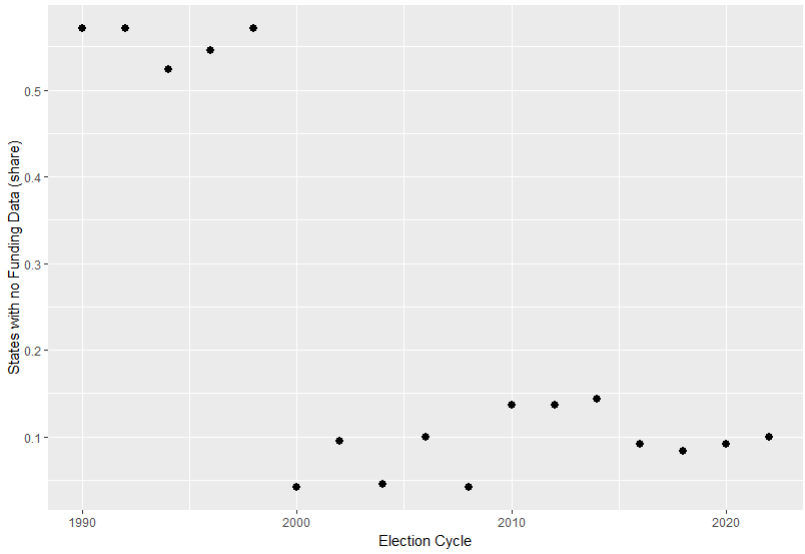


Figure 22: Judges Political Funding data coverage

B.1 Data Coverage

This section provides details about data coverage. As can be seen in Figure (22), 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.