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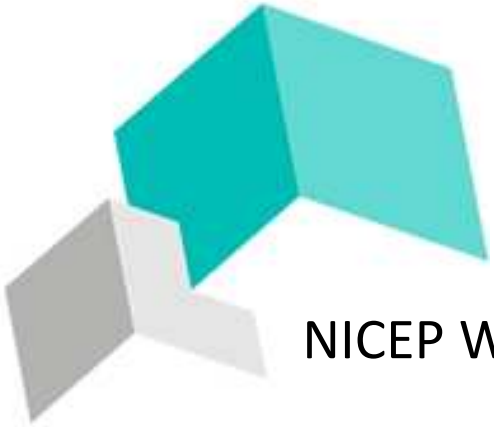

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Judicial selection and production efficiency: The role of campaign finance

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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), Porta et al. (1998). 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 is 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

1.1 State Supreme Court Selection

The judicial system in the United States is unique such that several state high courts and appellate court judges are elected. Overall 22 states undertake elections to fill up the bench of their judicial high courts (also referred to as state supreme courts). Figure (1) illustrates the geographic spread of states and how they are not concentrated within a specific geographic region. The remaining 38 states follow some form of variant of appointment. Either the judges are appointed by the governor, the state legislatures, or through a merit plan. Table (1) (reproduced here in the Appendix Figure (21)) from Kang and Shepherd (2015) provides details regarding the state procedures for selection of judges.

Among the states that do hold elections, the judge tenures differ. While in a majority of states the judges must go for retention election every 6 years, some states have tenures as long as 8 or 10 years. Most states go for judicial elections either for an open seat, i.e. elections without incumbents where the incumbent has retired or has died, or a general election in even years. A few states such as Pennsylvania (exclusively in odd-numbered years), Louisiana, and Wisconsin go for elections in odd-numbered years. The judges incumbents, and challengers raise funds for their election bid and use these funds to raise awareness about their ideological position. Figure (20) 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.

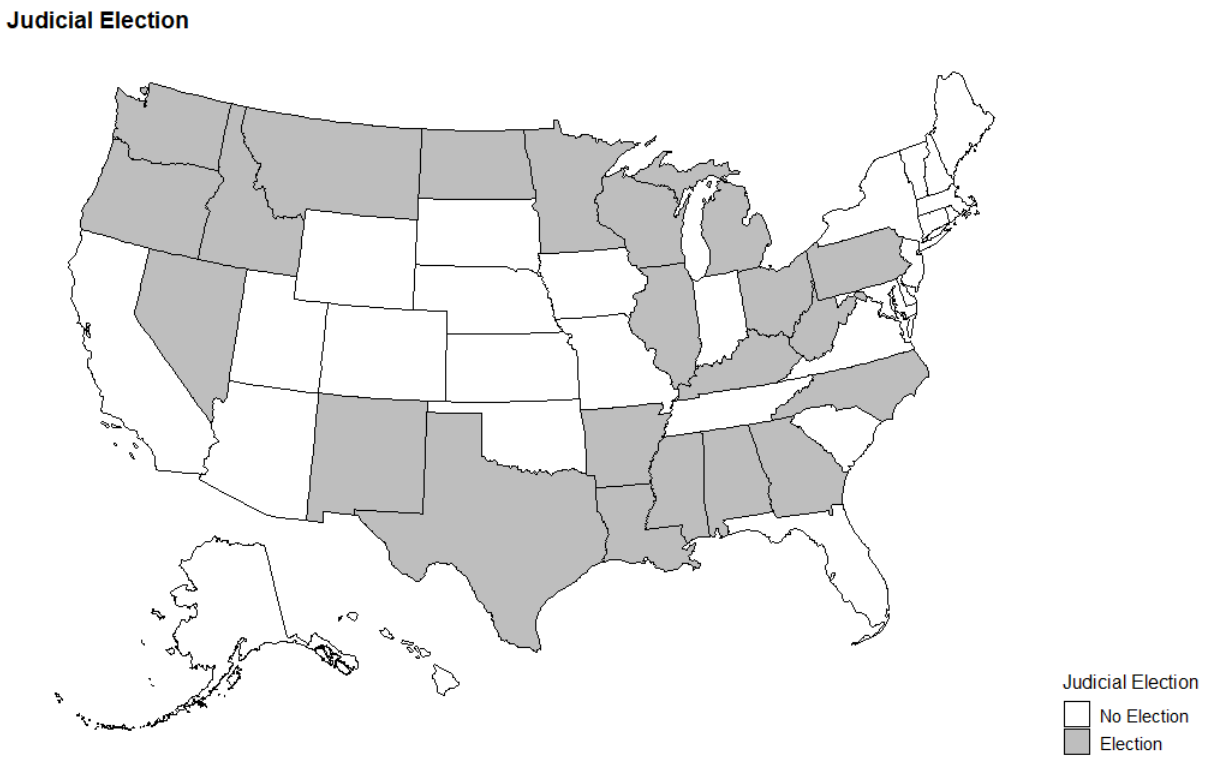


Figure 1: States with elections for the Supreme Courts in Grey. 22 states have judicial elections.

Some of the states had imposed bans on independent expenditures by corporations and/or unions. However, in January 2010, in the case of Citizens United vs. the FEC, the Supreme Court of the United States deemed such bans imposed as unconstitutional. Following this ruling, such political actors in these states were subjected to more relaxed funding constraints as now interest groups, corporations, and unions could spend unlimited sums on attacking or favoring a candidate without disclosures. In Figure (2), I focus on states with judicial elections that had imposed bans on independent expenditure, and which are considered as treated in our research design. States where such bans were placed in general are illustrated in Figure (23) in the online appendix. There is an additional difference in the electoral procedure for judiciary selection, such that some election states allow the party of the judge to be on the ballot, whereas others do not. The former are termed partisan election states and the latter the non-partisan election states. Legal scholars such as Kang and Shepherd (2015) have shown that this distinction is important in understanding how the political funding and influence operate in the judicial elections. Figure (19) in the Online Appendix provides information about the partisan and non-partisan states. Overall there are 11 states out of the 22 with judicial elections that have partisan elections, out of which 6 are treated. Among the states with non-partisan elections, 5 are treated.

2 Research Design and Data

2.1 Research Design

For simplicity consider two periods $t \in \{0, 1\}$ and two states $s \in \{T, C\}$ with multiple sectors of firms operating within each state. There is an intervention that affects state (and firms within these states) T, the treated states. The other state is the control state C which is not affected by the treatment. Define the across time and within state-sector differences by Δ_t, so that

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

The United States (US) offers a unique setting to study the effect of campaign finance

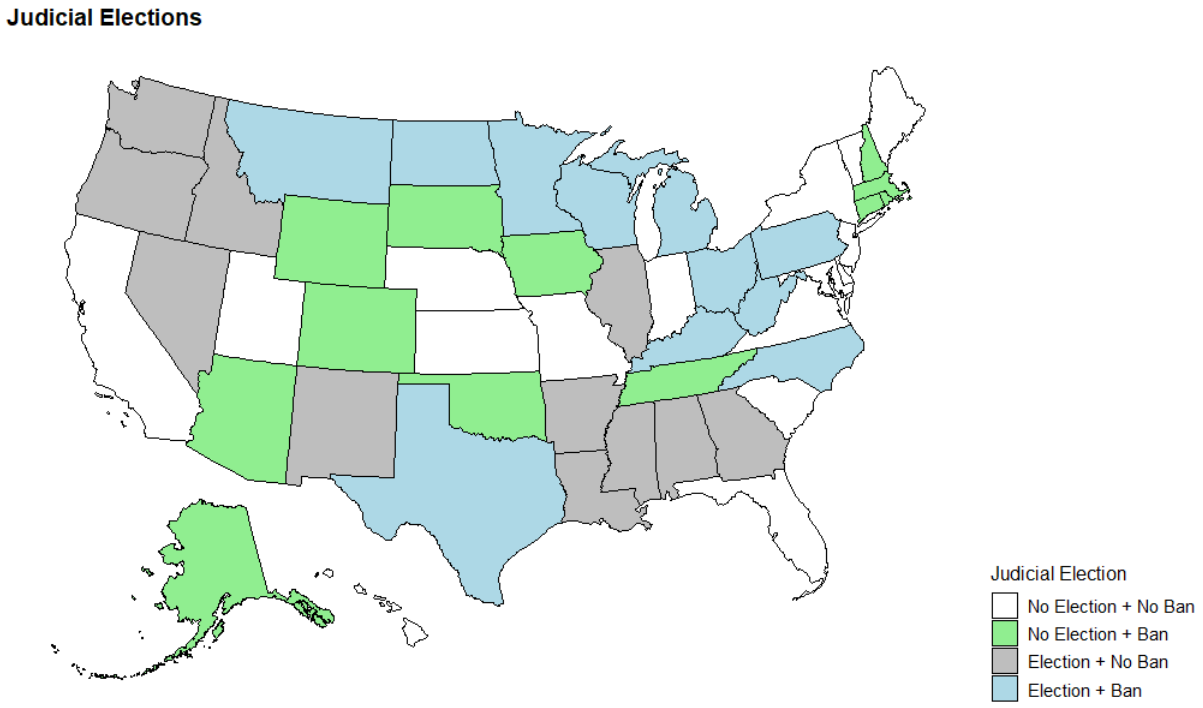


Figure 2: States that imposed some form of ban on independent expenditure are highlighted in blue and green. States with judicial elections, but no bans are in grey. States without judicial elections and no bans are in white.

on the judiciary and factor productivity. A total of 22 states in the US use elections to appoint judges to the highest state courts. The remaining 38 use some form of governor, or state-legislature appointment. Moreover, there were strict campaign finance restrictions imposed in around 23 states that banned independent expenditures from either the corporations or both corporations and unions. These restrictions were deemed unconstitutional following the 2010 Citizens United vs. FEC ruling by the Supreme Court of the US. This decision was unanticipated with a 5-4 split decision and led to larger sums of money flowing into the electoral races for different state and federal offices, particularly for states where bans were imposed initially.

I treat this intervention by the US Supreme Court and the states affected by this intervention as the treated states. The period following 2010, is considered as the post-period in our simple 2×2 DD research design. In this paper, I intend to focus on the effect of campaign finance laws on the judiciary, therefore I focus on states with judicial elections.

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, historic, or cultural characteristics. δ_t are the time fixed-effects that account 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,

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

In Table (2), 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 productivity-related measures such as the wage, labor productivity, and marignal revenue product of labor are also on average not statistically significant across treated and control groups. In the Appendix in Table (13) I provide evidence that

Table 1: 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				

the treatment assignment is uncorrelated on observables. This is further confirmed in the dynamic event-study specifications discussed along with the regression evidence.

2.2 Data

I combine data from several sources. The data on political funding, judicial elections, judge ideology, and state-sector-level real outcomes come from different sources. I briefly describe the sources of data. The exact procedure for subsetting and combining the data are mentioned in the Online Appendix.

2.2.1 Political Funding Data

Election data is from the National institute on Money in State Politics (NIMSP, OpenSecrets.org). The data sample for the financing of state high court judges comprises years 2000-2022. There is data for some state races (including high court judges) going back up to 1989, however NIMSP started collecting data for all 50 states only since 2000.[3] The

[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

Table 2: 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

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

independent expenditure data has limited coverage. Available for 13 states with robust disclosure requirements from 2006-2022. Figure 26 in the online appendix provides an illustration of the lack of funding data availability before 2000, where it is clear that over 50% of the states going for high court judge elections had no funding data.

2.2.2 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 dataset contains the identity of the candidates, the votes received, the type of election, number of seats being contested.

Political scientists have designed several measures to arrive at the judicial positions 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 WHH score which combines elements of the common-space approach with judicial decisions Windett, Harden and Hall (2015). In this paper, I use the data from Wilhelm, Vining and Hughes (2023) which provides the replication of PAJID measures from 1979-2020. They also compile the CFScore from Bonica and Woodruff (2015) which is available until 2015.

2.2.3 Manufacturing Census Data

I rely on the manufacturing census data collected as part of the American Survey of Manufacturers compiled by the Census Bureau. I do not have access to the plant-level data which requires sworn status, I rely on the state-sector level aggregates on capital expenditure, shipments, wage-bill, capital expenditure, value-added for computations of labor productivity.

data for candidates running for federal office, and some local offices beginning in 2011-2012."

2.2.4 Other Data

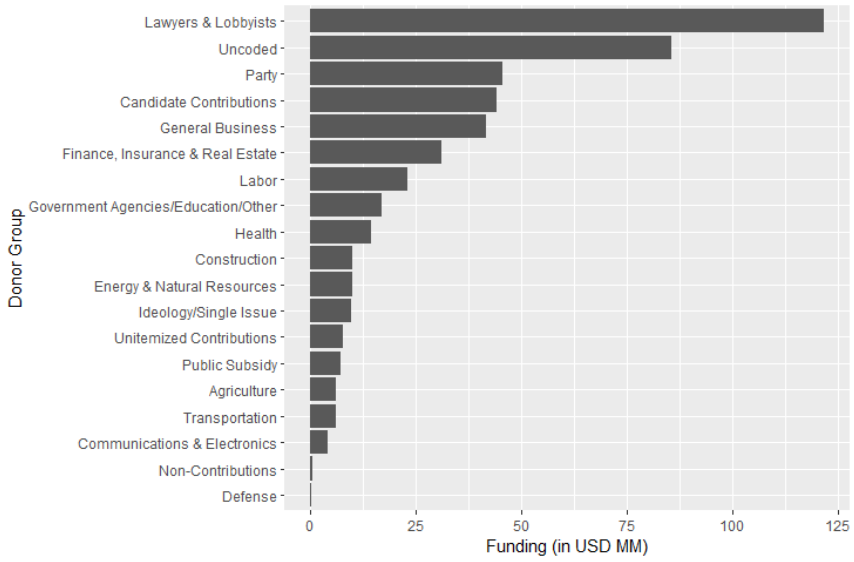
I also make use of other public sources of data such as state election commissions for gubernatorial election races, the input-output data from the Bureau of Economic Analysis to compile the measure of sector-level contract-reliance. As a robustness I also use the National Establishment Time Series (NETS) to provide firm-level evidence of resource misallocation due to campaign finance.

3 Judicial Elections and Competition

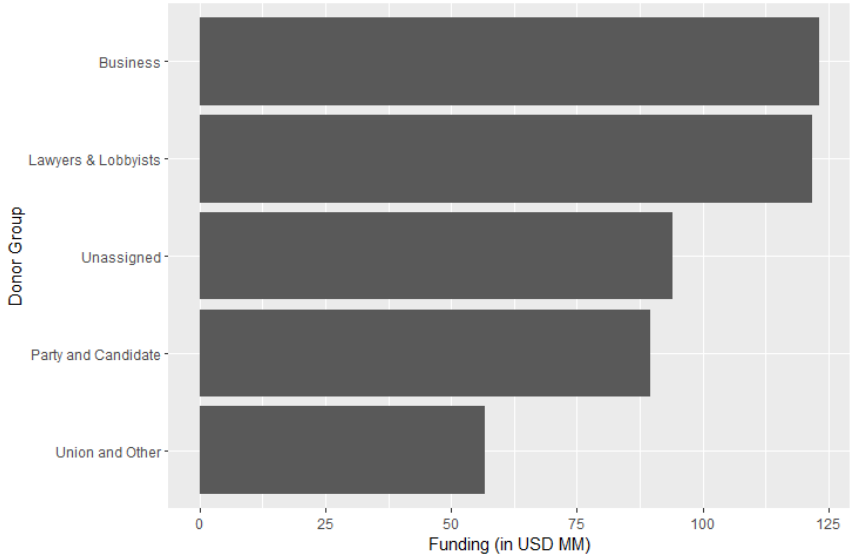
3.1 Judicial Funding: Key Facts

The funding in judicial elections is classified by NIMSP into several categories depending on the disclosure by the donors. As is clear, the single largest contributions come from the lawyers and lobbyists (NIMSP classification). A significant proportion of funding in the elections is somewhat opaque and hard to assign into a category. Such donations are assigned as unassigned. When I consolidate the donations from different business interests, such donations are the leading source of funding for judges competing in elections and amount to a total of $125 MM for elections from 2000-2021. Political parties have donated around $50 MM. The unions and ideological groups have donated roughly $50 MM. I hold the view that different groups compete for access with the judges. 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.

The bans were lifted from independent expenditures. Our hypothesis is that the relaxation of such funding constraints would lead to increased competition for funds in electoral races, and therefore a higher amount of average funding of a candidate in judicial elections. Note that the funding is generally quite skewed, and the average funding of a candidate is around $400,000. One would expect that if the removal of such bans increases competition for funding, then the total electoral funding should increase. This is what I see in Figure (4). The average funding in elections in states where the bans were removed (Treated) is higher after 2010. Note that I have not taken into account state-specific factors such as culture, natural endowments, state income, and traditional ideological position



(a) Funding Disaggregated



(b) Funding Aggregated

Figure 3: Funding of Judicial Candidates from different Donor Categories

that may influence the expenditures in judicial elections. In the following sections, I adopt the difference-in-differences framework to analyze the effect of the Supreme Court ruling on judicial competition for funds, electoral competition, and finally sector-level resource misallocation.

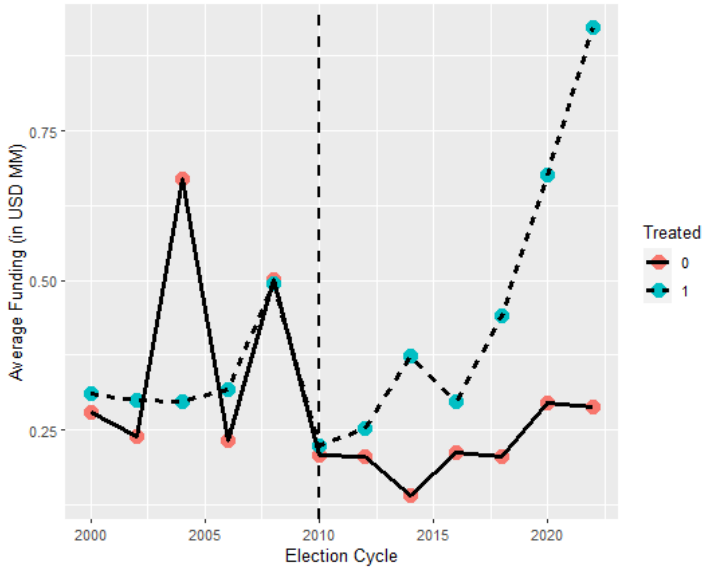


Figure 4: Average funding in USD for judicial elections in Treated vs. Control States

3.2 Judicial Funding: Effect due to Removal of Bans

in this section, I test whether the lifting of bans on independent expenditures increased funding of judicial candidates. I run the following regression specification.

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

in the regression specification $y_{ist} \in \{fund_{ist}, indexp_{ist}\}$ for candidate i, in state s and time t, $Treat_s = \mathbb{1}$(State imposed ban on independent expenditure), $Post = \mathbb{1}(t >= 2010)$. The ban removal increases the average funding of judges. $\{\delta_s, \lambda_t\}$ are state and time fixed-effects. I present the DD estimates for the average treatment effect on the funding and independent expenditures in Table (3). The average increase in funding due to the removal of bans is nearly \$200,000, and the increase is similar for independent expenditures. Controlling for time-invariant state characteristics such as their geography, ideological leanings,

etc. leads to an increase in the average treatment effect magnitude.

Table 3: Effect on Political Finance of Judges

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

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

Standard errors in parentheses

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

I can also examine the dynamic effects of the Supreme court ruling on the funding of representatives in elections. I estimate the following regression equation,

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

I set $\beta_0 = 0$. That is the comparisons in the change in funding across treated and control states take the 2010 cycle as the baseline. Inspecting Figure (5) I infer that the difference in funding in the years before 2010, is not significantly distinct from the difference in 2010. However, after the Supreme Court ruling, there seems to be an increase in the funding in treated states compared to the control states.

I see that the political funding of candidates increased on average following the lifting of bans on independent expenditures. It is interesting to understand which funding sources contribute to this rise. Table (4) shows that the increase in political funding is mainly driven by funding from businesses, and political parties (and unassigned groups). Interestingly, lawyers and lobbyists which is one of the main sources of funding for the judicial

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.

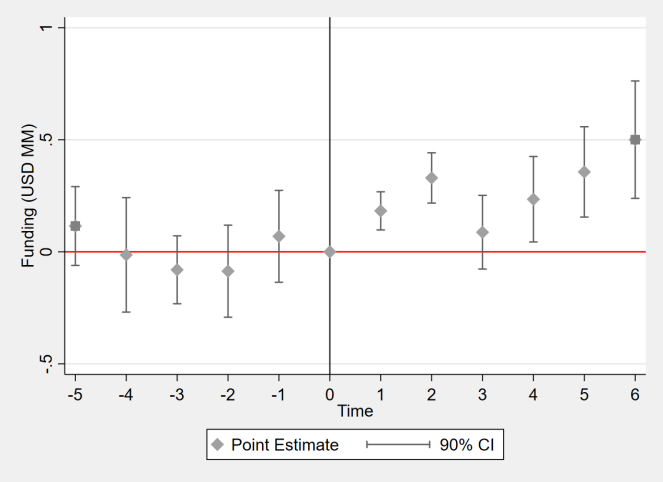


Figure 5: Event study plots. Event time is the 2009-2010 election cycle.

Table 4: 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

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

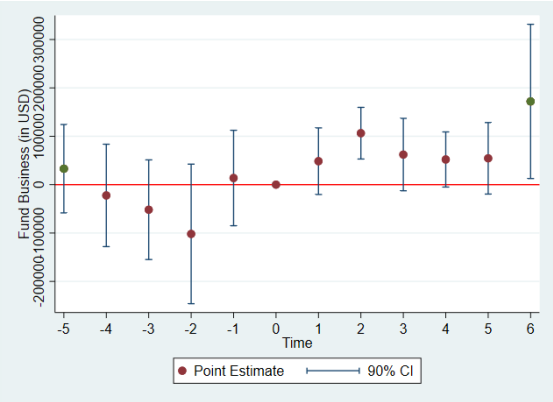
candidates are not affected by the Supreme Court ruling, as they were not constrained to begin with. Moreover, unions and other ideological groups do not seem to be affected by the Supreme court ruling either, at least in the states with judicial elections. Figure (6) provides further credible evidence regarding the change in funding from different sources. 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 increased political expenditure in judicial elections after the 2010 Supreme Court ruling.

3.3 Competitiveness of Election Races

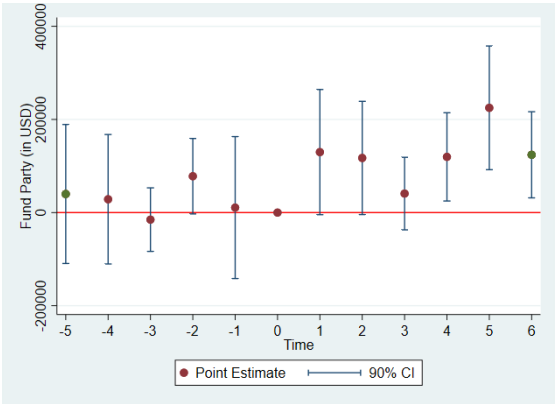
in this section, I document that the competitiveness of elections increases due to unrestricted campaign finance. I run the same regression specification as Equation (2) replacing y_{ist} with election level dependent variable in each election i in state s in election cycle t. The dependent variables that I focus on are the vote margin of the victors and the number of candidates competing in an election. 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 in our regression to account for such differences. The average treatment effect on the number of challengers in an election race is positive, i.e. as the limits on independent expenditure are relaxed the election races become more competitive due to the number of challengers in the race. In particular, if earlier there were 3 candidates competing for 2 judicial seats, after the removal of bans there are 2 candidates per seat.

The second piece of evidence, regarding the vote margin 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. The third piece of evidence, included in the appendix, deals with the competitive advantage of incumbents in judicial elections. I show that the probability that an incumbent wins the election declines by 20% following the Supreme Court ruling.

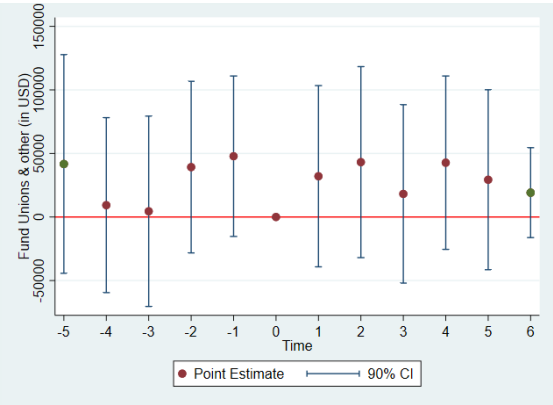
Note: This figure presents the estimation results of Equation (3). The dependent variable is the political expenditure by a judicial candidate, measured in USD MM from various sources such as Business groups in Panel (a), Party and self-funding from candidates in Panel (b), and funding from unions and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

Table 5: Electoral Competition

Note: This table presents the estimation results of Equation (2). The dependent variable in Columns (1) and (2) is the percentage difference in votes of the winner and the closest losing rival. Columns (3) and (4) 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) vote margin	(2) vote margin	(3) no. of cand.	(4) no. of cand.
Ban × Post	-0.16**	-0.15**	0.57**	0.57**
	(0.06)	(0.06)	(0.23)	(0.26)
Election Cycle FE	N	Y	N	Y
State FE	N	Y	N	Y
N	638	638	675	675
R-sq.	0.14	0.33	0.29	0.41

Standard errors in parentheses

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

3.4 Effect on Bench Composition

The increase in political finance also may have implications for judicial selection. In this section, I focus on the shift in ideology of the bench, and in particular the median ideology to show how unrestricted campaign finance may have implications for how the cases are viewed by the highest bench of the state judiciary. I focus on the Common Space ideology score from Bonica and Woodruff (2015). This score relies on actions taken by the judges, such as monetary donations to other political candidates to assign an ideology. This score captures the judge's ideological leaning and also highlights how the composition of the bench may start to change over time due to campaign finance. Bonica and Woodruff (2015) have already documented that ideology scores predict the votes of judges, and therefore, the ideological leaning of the judges affects how cases may be decided.

I estimate the regression specification in Equation (2) with the dependent variable being the median bench ideology, the mean bench ideology, and the standard deviation of the bench ideology (a measure of polarization). The ideology score lies in the range of $[0, 100]$ with a higher number associated with more right-leaning or business-friendly judges. The results are in Table (6) and I see that there is a shift in the ideology of the bench with the mean and polarization increasing for the states with ex-ante bans and judicial elections. An opposite pattern holds for states without judicial elections. A similar pattern holds in an event study design. The results are in the Figure (7).

I estimate the following regression equation,

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

To quantify the dynamic effect of the removal of bans on independent expenditure, I estimate the following event-study design equation where D_t is an indicator variable for year t, relative to the treatment year 2010.

$$y_{st} = \delta_s + \delta_t + \sum_{\tau=-5}^{T} \beta_\tau \cdot Ban_s \times D_{t-\tau} + \varepsilon_{st} \quad (5)$$

4 Main Result

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

Table 6: Effect on Ideology (Common Space CFscore)

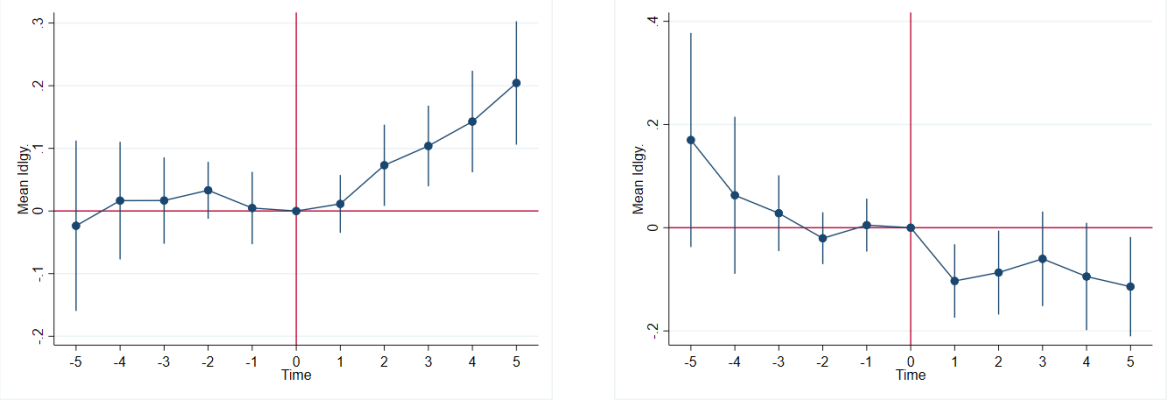
Note: This table presents the estimation results of equation (1). Columns (1)-(2) show results with median ideology (CFscore) of the judicial bench, which takes values in $(0, 100)$, with higher values indicating a more liberal ideology of a given state s in year t as the dependent variables. Columns (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 (5). The dependent variable is mean ideology (CFscore) of the judicial bench, which takes values in $(0, 100)$, with higher values indicating a more liberal 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 7: Effect on Mean Judicial Bench Ideology (CFscore) Bonica and Woodruff (2015)

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

$$y_{jst} = \beta_{eb} \cdot (\text{Elect}_s \times \text{Post}_t) + \beta_{bp} \cdot (\text{Ban}_s \times \text{Post}_t) + \beta_{ebp} \cdot (\text{Elect}_s \times \text{Ban}_s \times \text{Post}_t) \\ \delta_s + \delta_{jt} + \beta_{ST} \cdot (s \times t) + \varepsilon_{jst} \tag{6}$$

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 (7) 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 (8), 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 (7) for value-added per worker.

4.1 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 7: Effect on Labor Productivity (USD/emp)

Note: This table presents the estimation results of equation (6) The dependent variable is labor productivity, measured as value added in USD 1000s per worker. Variables *Elect* indicates states with judicial elections for state supreme court judges, *Ban* indicates the states that had imposed a ban on independent expenditures by unions or corporations, which were rendered unconstitutional by the Supreme Court in 2010. The specifications in each column vary depending on the inclusion of state-by-sector, and sector-by-year fixed effects. Sector is defined at the 4-digit NAICS code level. Standard errors are clustered at the state level.

	(1)	(2)	(3)	(4)	(5)
	Lab Prod (USD/emp)	Lab Prod (USD/emp)	Lab Prod (USD/emp)	Lab Prod (USD/emp)	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 8: Effect on Labor Productivity (USD/hr)

Note: This table presents the estimation results of equation (6) The dependent variable is labor productivity, measured in USD value added per hour. Variables *Elect* indicates states with judicial elections for state supreme court judges, *Ban* indicates the states that had imposed a ban on independent expenditures by unions or corporations, which were rendered unconstitutional by the Supreme Court in 2010. The specifications in each column vary depending on the inclusion of state-by-sector, and sector-by-year fixed effects. Sector is defined at the 4-digit NAICS code level. Standard errors are clustered at the state level.

	(1)	(2)	(3)	(4)	(5)
	Lab Prod (USD/hr)	Lab Prod (USD/hr)	Lab Prod (USD/hr)	Lab Prod (USD/hr)	Lab Prod (USD/hr)
Ban × Post	6.62**	-3.00			
	(3.15)	(5.21)			
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 (9), 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 (9) are robust to the inclusion of state-specific time trends.

Table 9: Effect on Capital Expenditure, Employment, and Wages

Note: This table presents the estimation results of equation (6) 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)	Val. Add/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

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

4.2 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 (6) 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 (8) 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 (9) shows that the results also hold for productivity measured as value added per hour of labor.

$$y_{jst} = \delta_{sj} + \delta_{jt} + \sum_{\tau=-5}^{T} \beta_\tau \cdot Ban_s \times D_{t-\tau} + \varepsilon_{jst} \tag{7}$$

Note: This figure presents the estimation results of equation (7). 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.

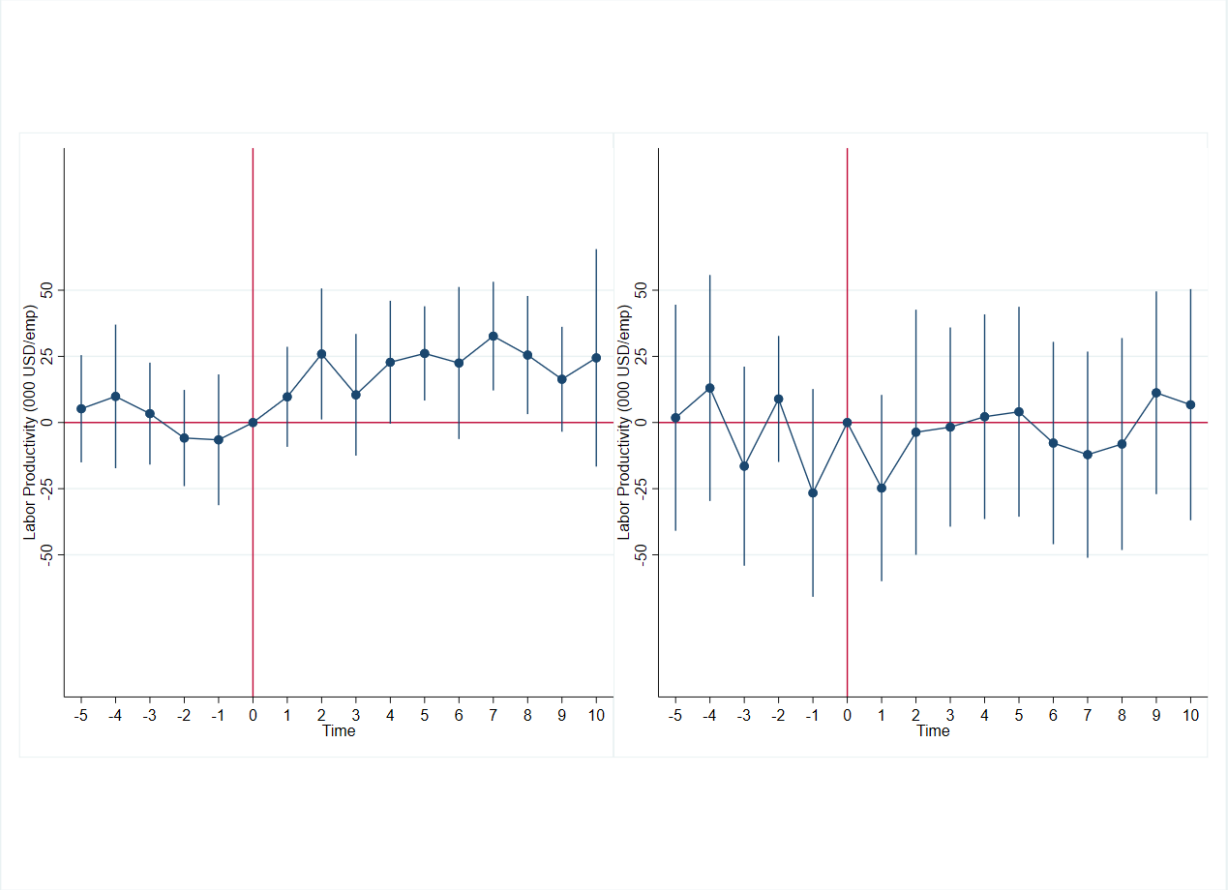


Figure 8: Effect on Labor Productivity in '000 USD /emp: States with Judicial Elections (Left), and without judicial elections (Right)

Note: This figure presents the estimation results of equation (7). 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.

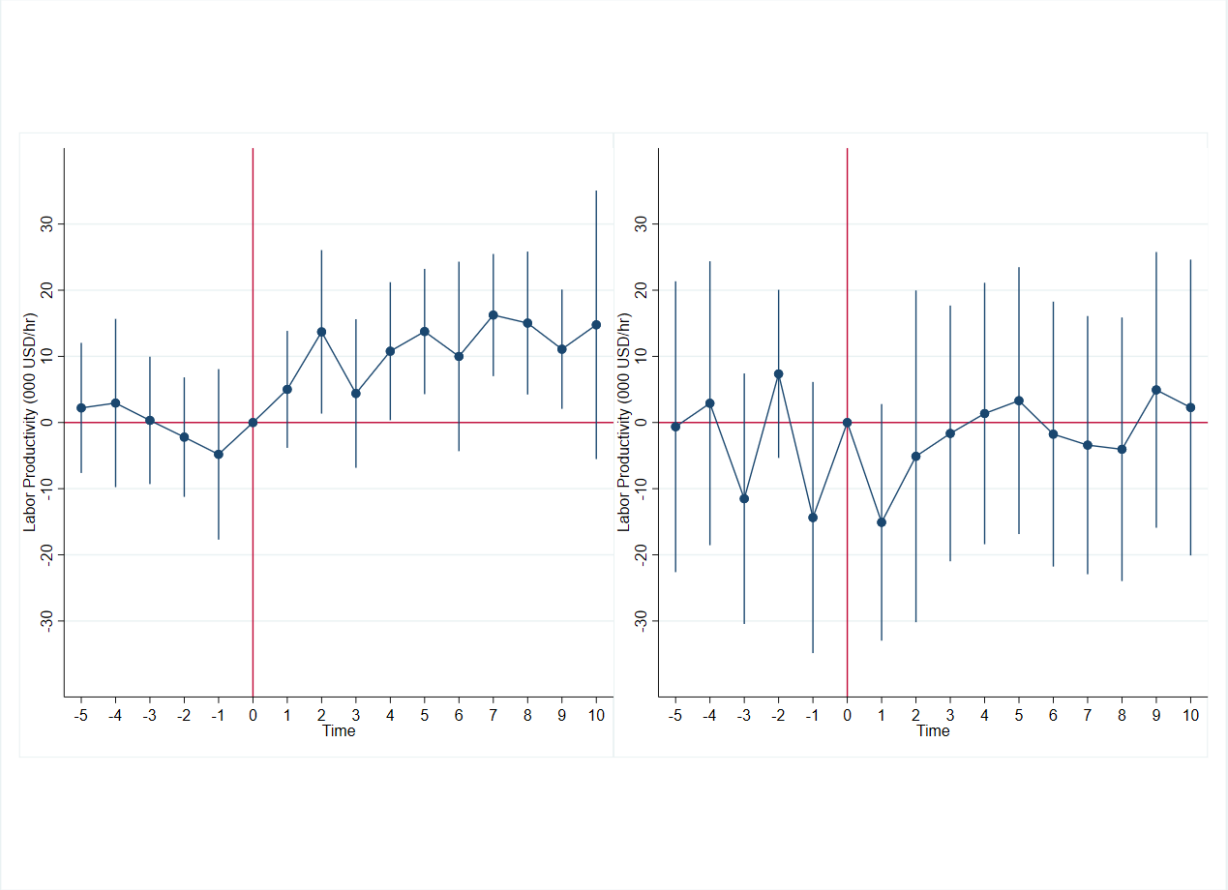


Figure 9: Effect on Labor Productivity in '000 USD /hr: States with Judicial Elections (Left), and without judicial elections (Right)

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). The measure is based on the input specificity and is derived from from the input-output matrices compiled by the Bureau of Economic Analysis (BEA). More details are in the Appendix.

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

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,

$$\begin{aligned} y_{jst} = {} & \beta_{bp} \cdot (Ban \times Post) + \beta_{ebp} \cdot (Elect \times Ban \times Post) \\ & + \beta_{bhp} \cdot (Ban \times Contract \times Post) + \beta_{ebhp} \cdot (Elect \times Ban \times Contract \times Post) \\ & + \mathcal{I}(Elect, Ban, Contract, Post) + \delta_s + \delta_{jt} + \varepsilon_{jst} \end{aligned} \tag{8}$$

where $\mathcal{I}(Elect, Ban, Contract, Post)$ includes all the 2 and 3 term interactions between the variables, except for those mentioned in Equation (8). 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 (10) 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 (11), I test whether the effect of a relaxed funding constraint has a heterogeneous effect on sectors.

Table 10: Heterogeneous Effect due to reliance on Contract Enforcement

Note: This table presents the estimation results of equation (8). 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 11: Treatment Effect Heterogeneity due to reliance on Contract Enforcement

Note: This table presents the estimation results of equation (8) 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

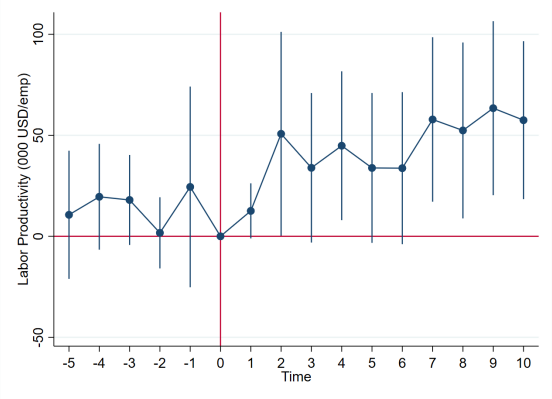
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table (11) 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 (10), estimates Equation (7) 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.

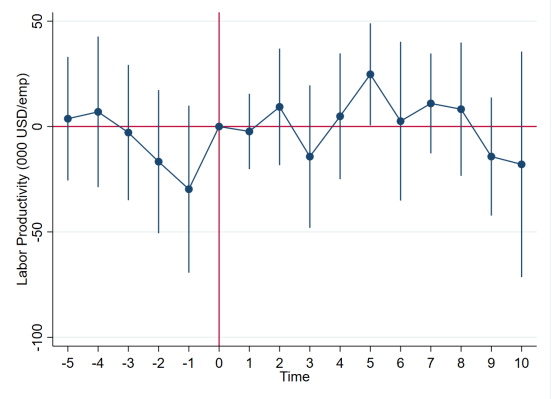
5.1 Effect on Firm Productivity

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 ≥ 10 and ≤ 1000 employees to avoid the effect on imputation on the measurement. I then compute the $log(MRPL)$ by equating the sales to the value-added as an approximation. 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 $log(MRPL)$ at the 4-digit NAICS-year level so as 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 so as to reduce noise in the estimation of the first and second moments. 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.

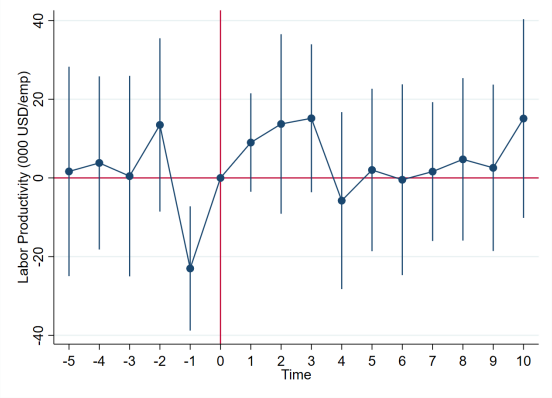
Note: This figure presents the estimation results of equation (7). The dependent variable is labor productivity, measured as USD value added per emp. Panel (a) shows results for states with judicial elections for state Supreme Court judges and Panel (b) for states that use some form of appointment. The 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.



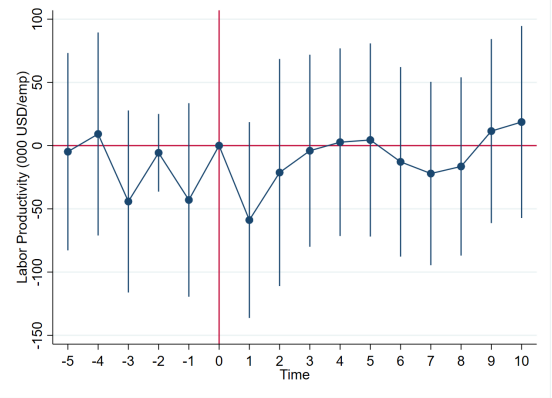
(a) Contract-intensive Sectors in States with Judicial Elections



(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 10: Effect on Labor Productivity in USD /emp

Table 12: Effect on Average Productivity and Dispersion

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

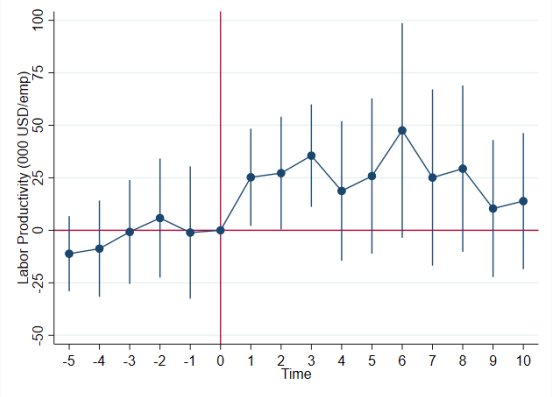
Standard errors in parentheses

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

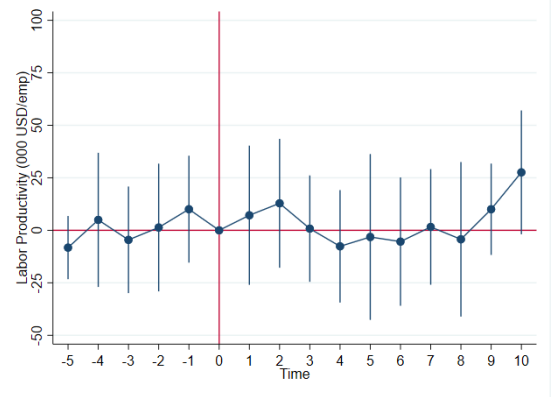
Table (12) illustrates how the average labor productivity is higher for the treated states, in line with the evidence presented earlier. Interestingly, it is plausible that the improvement in productivity comes along with higher dispersion in productivity, a measure of factor distortion as in Hsieh and Klenow (2009). Column (2) shows that the dispersion in productivity does not increase. Moreover, the increase in productivity is not only for larger firms as shown in Column (3), where the dependent variable is the covariance between labor productivity and revenue. Columns (2) and (3) show that the increased labor productivity is not at the expense of increased misallocation within the economy.

In Figure (11), I replicate the effect of higher labor productivity for sectors with higher contract reliance in states that had their bans invalidated by the Supreme Court ruling in 2010. Panel (a) confirms that there are no pre-treatment trends and that labor productivity is higher for sectors more reliant on contract enforcement for their production.

Note: This figure presents the estimation results of equation (7). 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) Sectors with High Contract Reliance



(b) Sectors with Low Contract Reliance

Figure 11: Effect on Average Labor Productivity in USD /worker and Dispersion within State-Sector

6 Conclusion

In this paper, I show that political finance may increase electoral competition for judges, and lead to a more ideologically diverse judicial bench, which could alleviate 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, i.e. the electoral races became more competitive. The incumbency advantage in judicial races also declined significantly. I further show that the increased political 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 in states with judicial elections. Finally, I provide some suggestive evidence that the higher labor productivity seems to be operating through the political expenditures in judicial elections and the ideological diversity of the bench and does not come at the expense of higher factor misallocation.

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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 13: 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

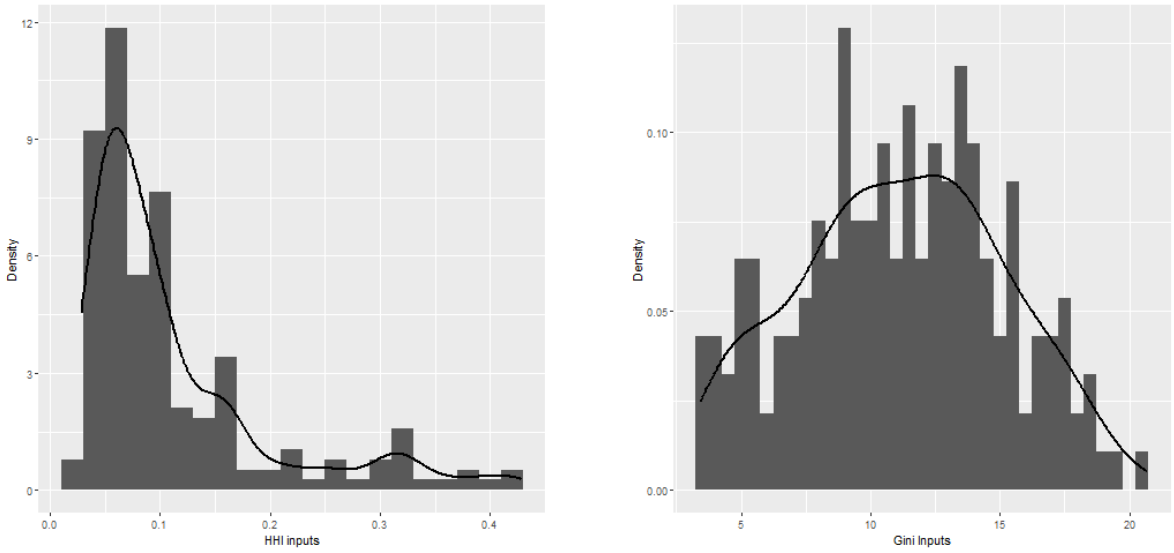
In this section, I show the results of the computations of the contract reliance measure described in the main text. I supplement the measure with another measure, the input Gini. All the results in the main text are robust to both measures. The *inputgini* is defined as,

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

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 14: Least and Most Contract Reliant Industries



(a) Density HHI inputs

(b) Density Gini inputs

Figure 12: Density of Contract intensity measures

	Mean	SD	Median	Min	Max	ρ_{gini}
Input HHI	0.136	0.107	0.096	0.038	0.623	-0.780
Levchenko (2007)	0.133	0.093		0.035	0.780	-0.742

Table 15: Summary Statistics of Input HHI

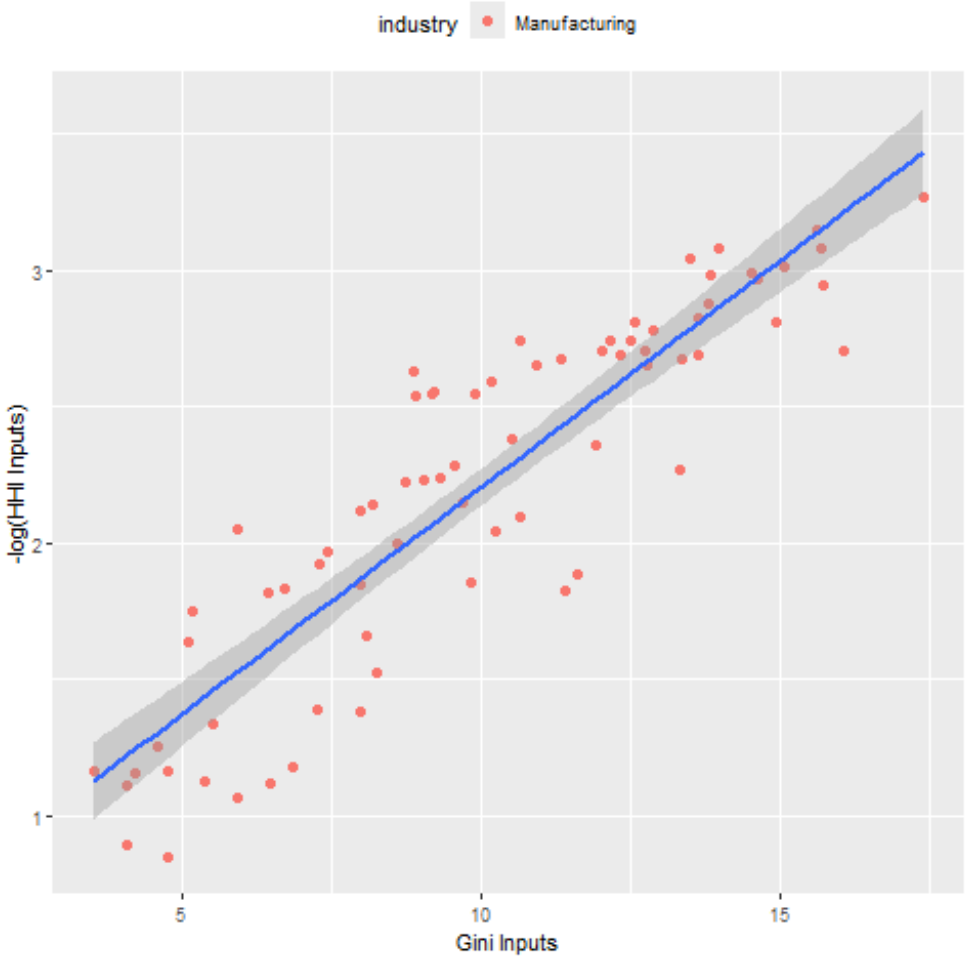
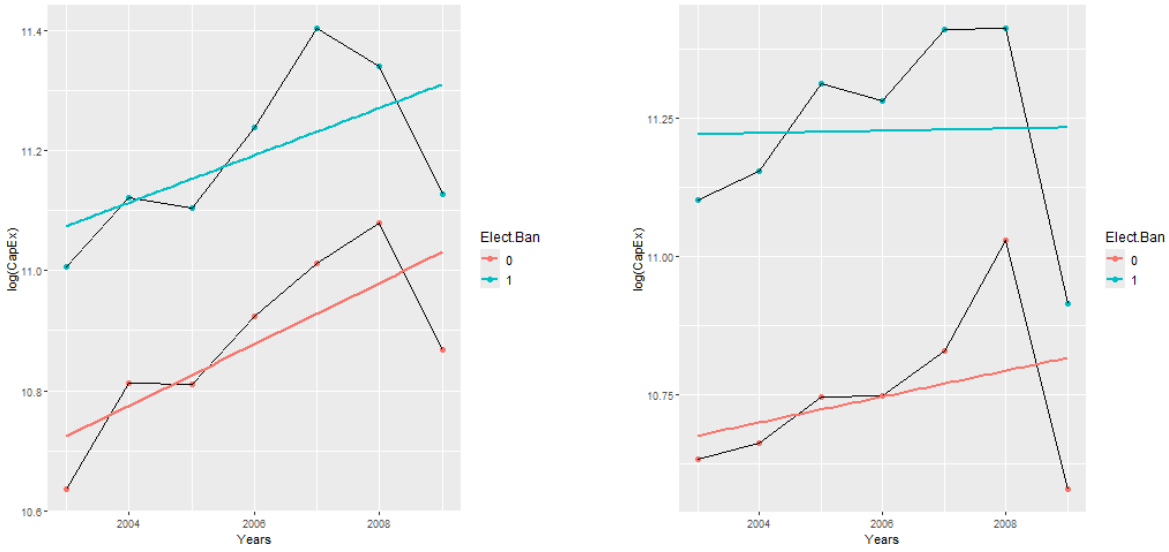


Figure 13: Relationship between Contract intensity Measures

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

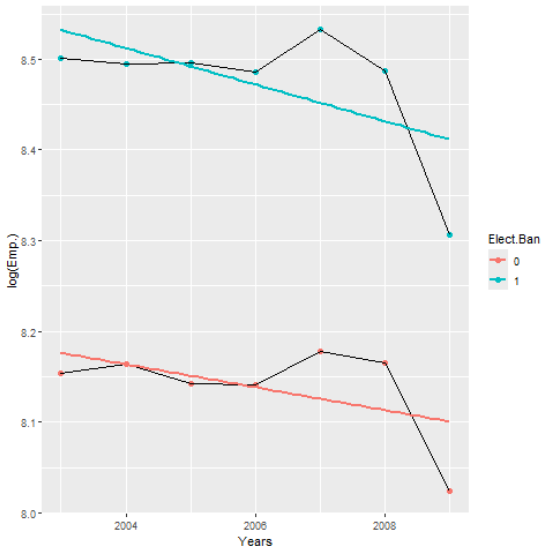
In this subsection, I document the pre-trends in capital expenditure, employment, and wages during the pre-period 2005-2009 for treated states with judicial elections across sectors with high contract reliance and low contract reliance.



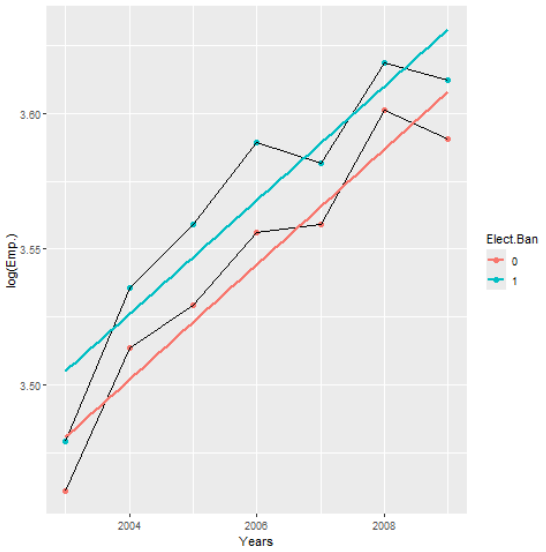
(a) Non-contract reliant Capex Pre-Trends

(b) Contract Reliant Capex Pre-Trends

Figure 14: Pre-Trends Capex

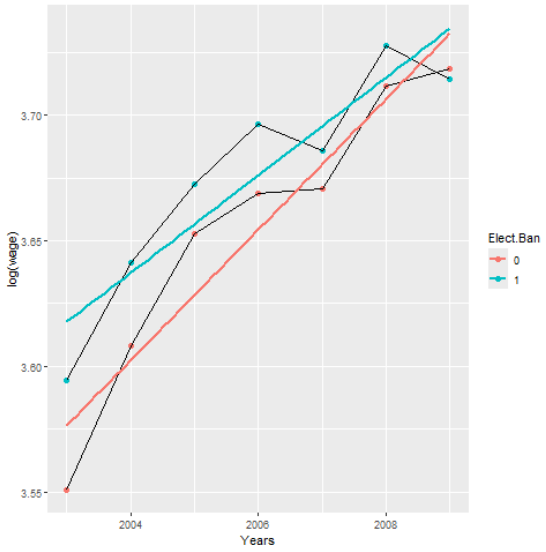


(a) Non-contract reliant Employment Pre-Trends

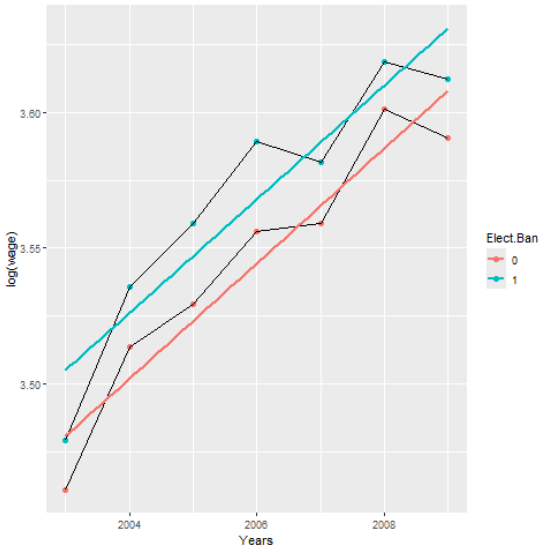


(b) Contract Reliant Employment Pre-Trends

Figure 15: Pre-Trends Employment



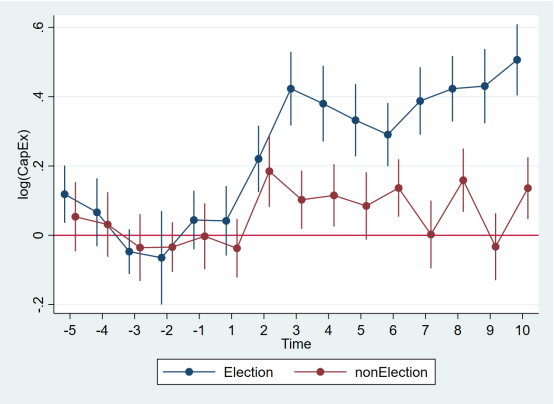
(a) Non-contract reliant Wage Pre-Trends



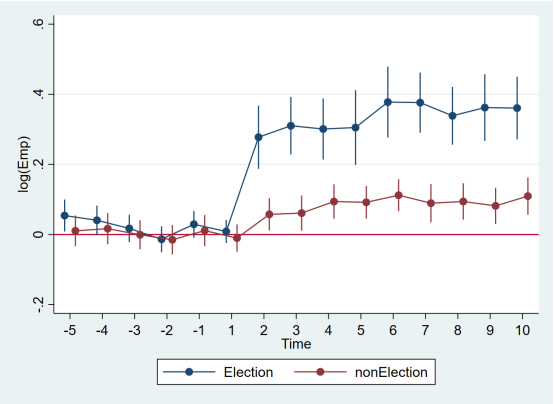
(b) Contract Reliant Wage Pre-Trends

Figure 16: Pre-Trends Wages

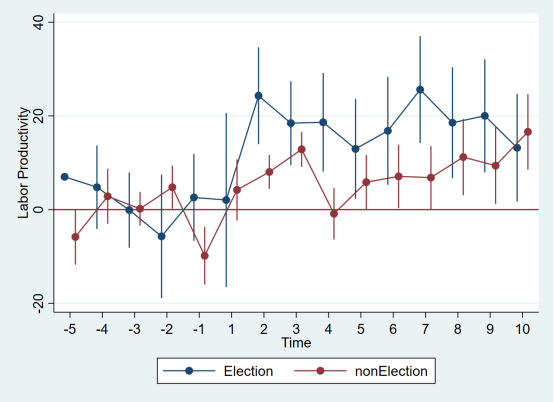
C Effect of Political Finance on High-Contract Reliant Sector



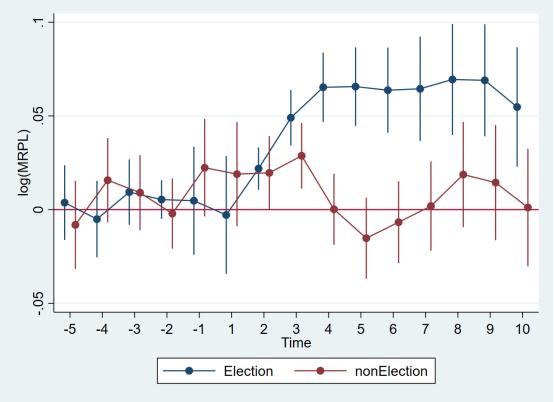
(a) Capital Expenditure



(b) Employment



(c) Labor Productivity



(d) $\log(MRPL)$

Figure 17: Event Study with event time 2010 (Time = 0) for industries with higher reliance on contract enforcement (input specificity).

D Appendix: Independent and Direct Expenditures in Judicial Elections

First, I present the direct evidence on independent expenditures in Table (16). 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 16: 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

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

D.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 (17), 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.

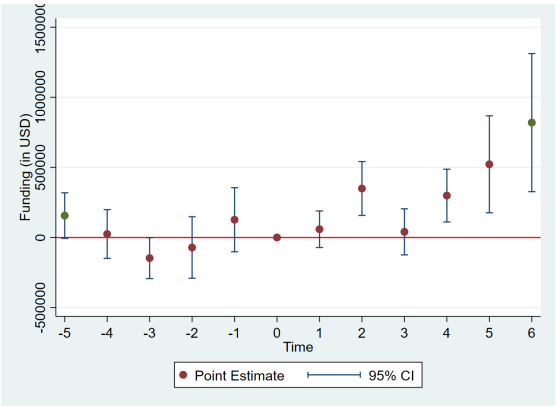
Table 17: Effect due to Corporations and Union bans

	(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

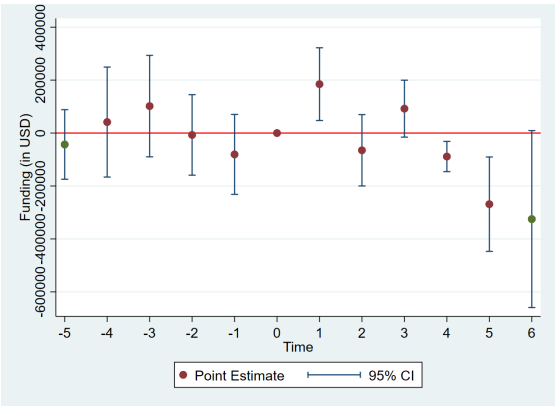
Standard errors in parentheses

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

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.

D.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 18: 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

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

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 (19) and (20) 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 F, we test for this hypothesis.

Table 19: Effect on Establishment Productivity

Note: This table presents the estimation results of equation (6). 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 20: Effect on Establishment Productivity

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

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

F Establishment Entry and Reallocation

In this section, we test whether the changes in the campaign finance laws affect establishment entry, and exit. In Table (21) 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 (22) 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.

Table 21: Effect on Establishment Entry, Exit, and Employment

Note: This table presents the estimation results of equation (6). 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 22: Job Reallocation Rates

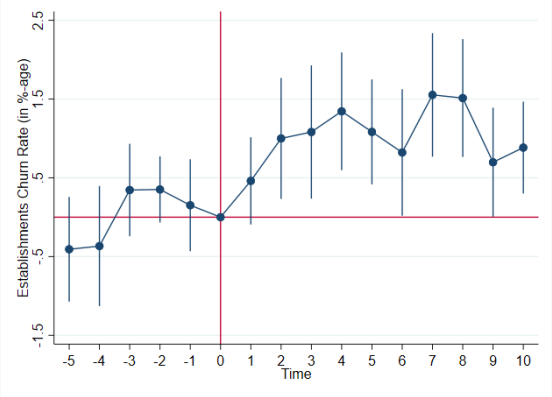
Note: This table presents the estimation results of equation (6). The dependent variables in Columns(1)-(3) are the job creation, destruction, and the sum of the creation and destruction rates, the reallocation rate. Columns (4) captures the job reallocation rate from continued establishments. All rates 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

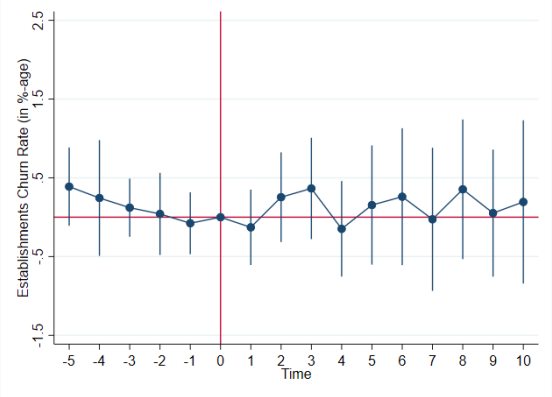
Standard errors in parentheses

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

Note: This figure presents the estimation results of equation (7). 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 19: Dynamic effect on the Business Churn rate, sum of entry and exit rates of establishments.

G 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 $\tau^K_{isj} > 0$. This could be due to contract enforcement frictions. For example, the supplier of capital goods or material inputs could raise the price of input over the market rate, or downgrade the supply quality. 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 $\log(1 + \tau^K_{ijs}) \sim \mathcal{N}(\mu_{\tau sj}, \sigma^2_{\tau sj})$.[4] 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

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

[4] 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.

G.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,

$$APL_{sj} = \frac{\int_{\tau^K_{isj} < \overline{\tau}_{sj}(c_{sj})} \Lambda^{\alpha}_{sj} \mathcal{K}^{\beta}_{sj} (1+\tau^K_{isj})^{1-\tilde{\beta}} dF_{\tau}(\tau^K_{ijs})}{l^*_{sj}} \tag{9}$$

Proposition 3. *The average sector-level productivity*

$$APL_{sj} = \Lambda^{\alpha-1}_{sj} \mathcal{K}^{\beta} \underbrace{\exp\left((\tilde{\beta}-\tilde{\alpha}-1)\left(-\mu_{\tau sj} + (\tilde{\alpha}+\tilde{\beta}-1)\frac{\sigma^2_{\tau sj}}{2}\right)\right)}_{\text{average productivity with } c_{sj}=0} \underbrace{\frac{\Phi\left((\tilde{\beta}-1)\sigma_{\tau sj} - \frac{\mu_{\tau sj} - \log(1+\overline{\tau}_{sj})}{\sigma_{\tau sj}}\right)}{\Phi\left(\tilde{\alpha}\sigma_{\tau sj} - \frac{\mu_{\tau sj} - \log(1+\overline{\tau}_{sj})}{\sigma_{\tau sj}}\right)}}_{\text{entry effect}} \tag{10}$$

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: It is only a reduction in average distortive wedges, $\mu_{\tau sj}$ that explains

1. Higher entry

2. Higher employment

3. Higher average productivity

Increase in $\sigma_{\tau sj}$ implies increase in employment and productivity, however it implies a decline in entry. A decrease in c_{sj} implies an increase in entry, employment, however, it implies a decline in average productivity.

A Online Appendix

A.1 Judicial Selection

In Figure (20), 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 20: Illustration of an advertisement for Judge Janet Protasiewicz (indirect Expenditure)

Table (21) and (22) 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.

METHODS OF SELECTION AND RETENTION FOR HIGHEST COURT BY STATE[48]

State	*Selection Method for Full Term*	*Retention Method*	*State*	*Selection Method for Full Term*	*Retention Method*
Alabama	P	P	Montana	N	N
Alaska	M	R	Nebraska	M	R
Arizona	M	R	Nevada	N	N
Arkansas	P	P	New Hampshire[49]	G	-
California	G	R	New Jersey[50]	G	G
Colorado	M	R	New Mexico	P	R
Connecticut[51]	LA	LA	New York	M	G
Delaware	M	G	North Carolina	P	P
Florida	M	R	North Dakota	N	N
Georgia	N	N	Ohio[52]	N	N
Hawaii	M	J	Oklahoma	M	R
Idaho	N	N	Oregon	N	N
Illinois	P	R	Pennsylvania	P	R
Indiana	M	R	Rhode Island[53]	M	-
Iowa	M	R	South Carolina	LE	LE
Kansas	M	R	South Dakota	M	R
Kentucky	N	N	Tennessee	M	N
Louisiana[54]	P	P	Texas	P	P

[48] The data in this table were collected from ROTTMAN ET AL., *supra* note 40, at 21 tbl.4, and the American Judicature Society, *supra* note 41. G = gubernatorial appointment or reappointment, P = partisan election or reelection, N = nonpartisan election or reelection, LA = legislative appointment or reappointment, LE = legislative election or reelection, M = merit plan, R = retention election, and J = reappointment by a judicial nominating commission.

[49] In New Hampshire, judges serve until age seventy. ROTTMAN ET AL., *supra* note 40, at 28 tbl.4.

[50] In New Jersey, after an initial gubernatorial reappointment, judges serve until age seventy. N.J. CONST. art. VI, § 6, ¶ 3.

[51] In Connecticut, the governor nominates, and the legislature appoints. ROTTMAN ET AL., *supra* note 40, at 21 tbl.4, 25 n.2.

[52] In Ohio, political parties nominate candidates to run in nonpartisan elections. Am. Judicature Soc'y, *supra* note 41.

[53] In Rhode Island, judges have life tenure. ROTTMAN ET AL., *supra* note 40, at 28 tbl.5.

[54] In Louisiana, candidates compete in a blanket primary with party labels on the ballot. The top two primary candidates go on to the general election. Am. Judicature Soc'y, *supra* note 41.

Figure 21: Judge Selection Procedures Kang and Shepherd (2015).

TABLE 1 CONTINUED

State	*Selection Method for Full Term*	*Retention Method*	*State*	*Selection Method for Full Term*	*Retention Method*
Maine	G	G	Utah	M	R
Maryland	M	R	Vermont	M	LE
Massachusetts[55]	M	-	Virginia	LE	LE
Michigan[56]	N	N	Washington	N	N
Minnesota	N	N	West Virginia	P	P
Mississippi	N	N	Wisconsin	N	N
Missouri	M	R	Wyoming	M	R

Figure 22: Judge Selection Procedures Kang and Shepherd (2015).

The exogenous variation in campaign finance is from the states that had imposed a ban on independent expenditures. I reproduce the figure from Klumpp, Mialon and Williams (2016), to highlight the states and the years in which these states imposed a ban on independent expenditures by corporations or unions, or both.

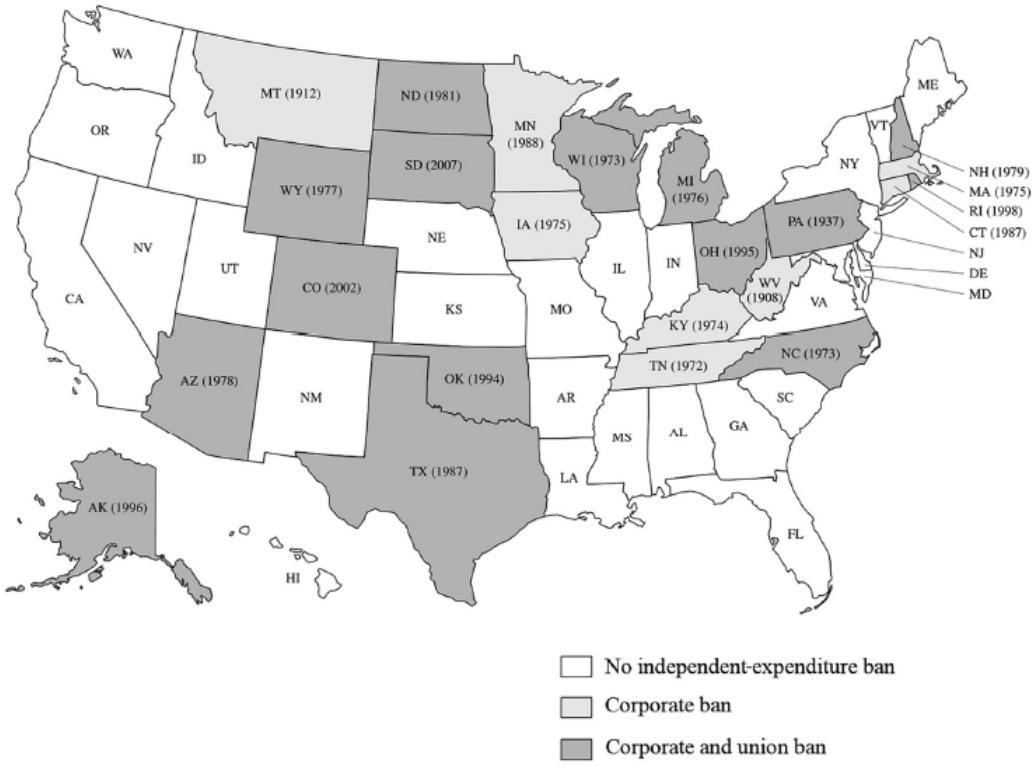


Figure 23: States that imposed bans on independent expenditure. Figure from Klumpp, Mialon and Williams (2016)

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 (24), 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.

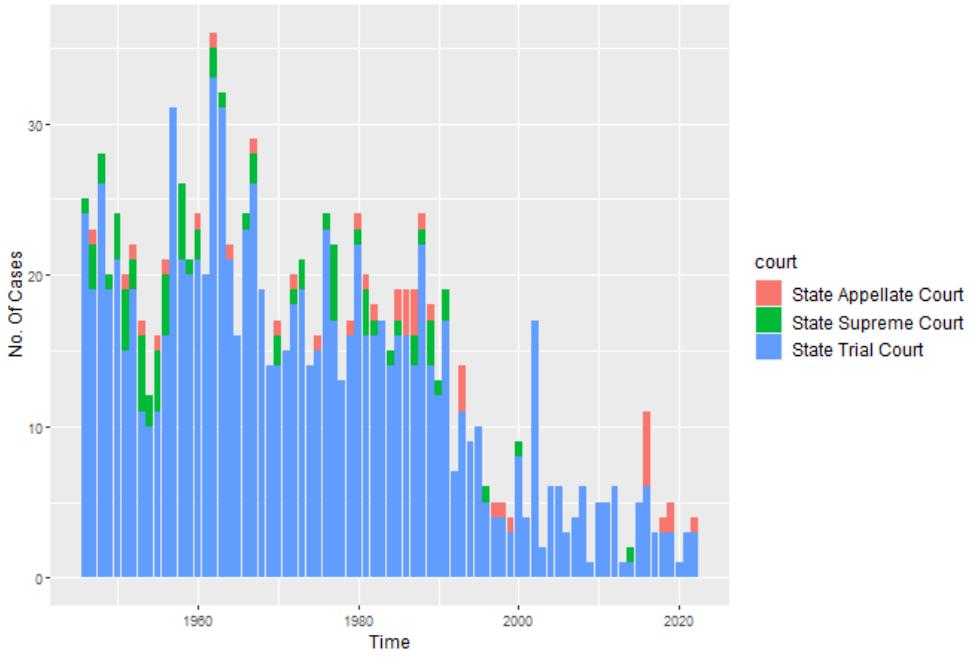
Table 23: Number of Cases argued in the US Supreme Court

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

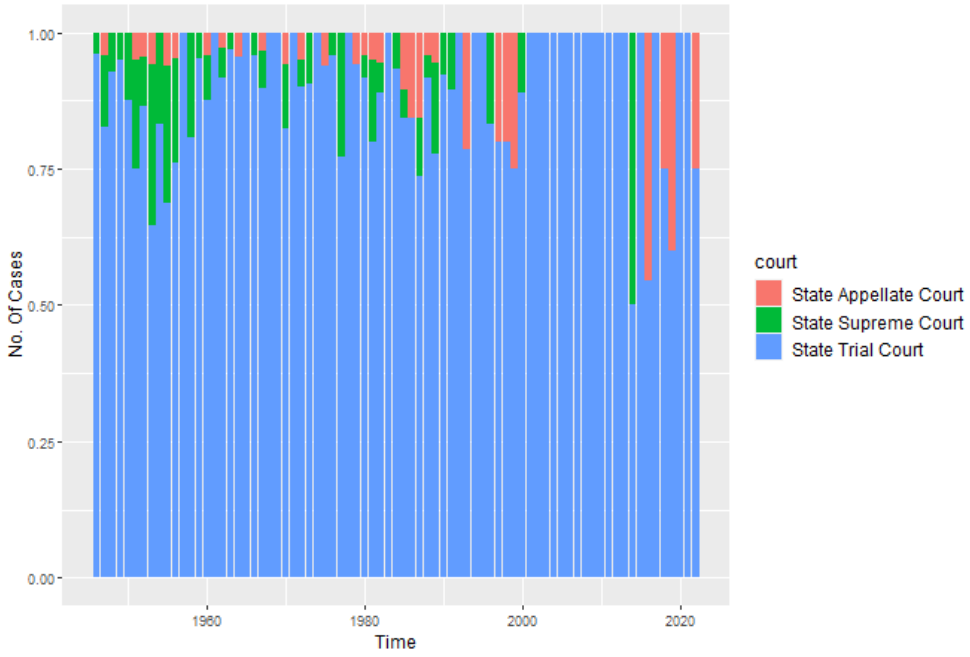
	(1)	(2)	(3)	(4)	(5)
	log(No. of Cases)	log(No. of Cases)	log(No. of Cases)	log(No. of Cases)	log(No. of Cases)
Ban × Post	0.03	0.01		0.11**	
	(0.05)	(0.04)		(0.04)	
Elect × Ban × Post	-0.04	-0.06	-0.06*	-0.19***	-0.08***
	(0.07)	(0.05)	(0.03)	(0.06)	(0.03)
State FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
Issue Area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

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

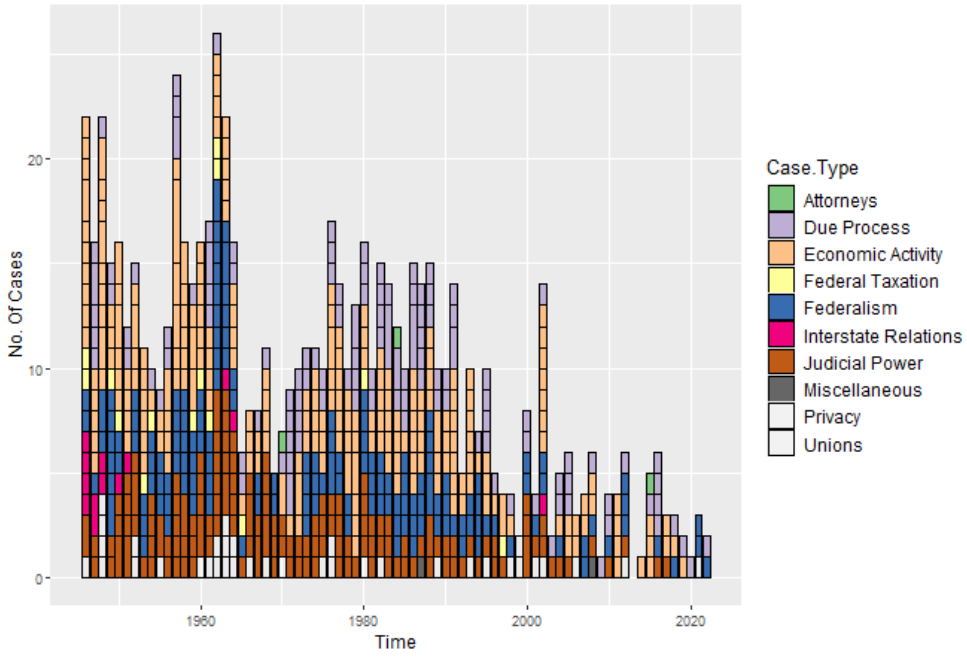


(a) No. of Cases appealed to the SCOTUS

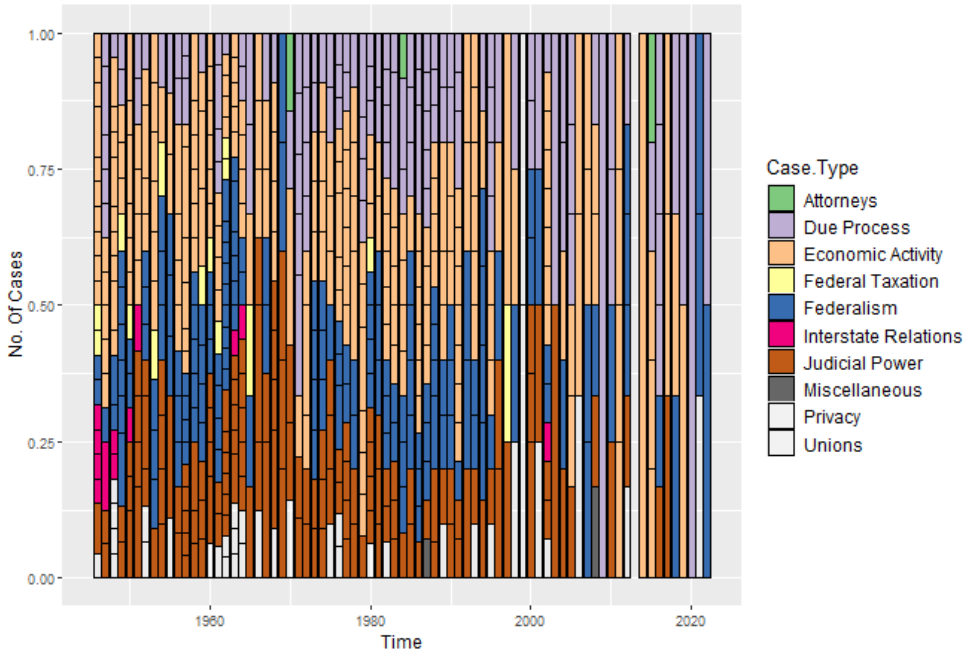


(b) Percentage Split of Cases appealed to the SCOTUS

Figure 24: Cases appealed to the SCOTUS



(a) No. of Cases appealed to the SCOTUS



(b) Percentage Split of Cases appealed to the SCOTUS

Figure 25: Cases appealed to the SCOTUS by Issue Area

Table 24: Case Decisions of Appeals in Supreme Court

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

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

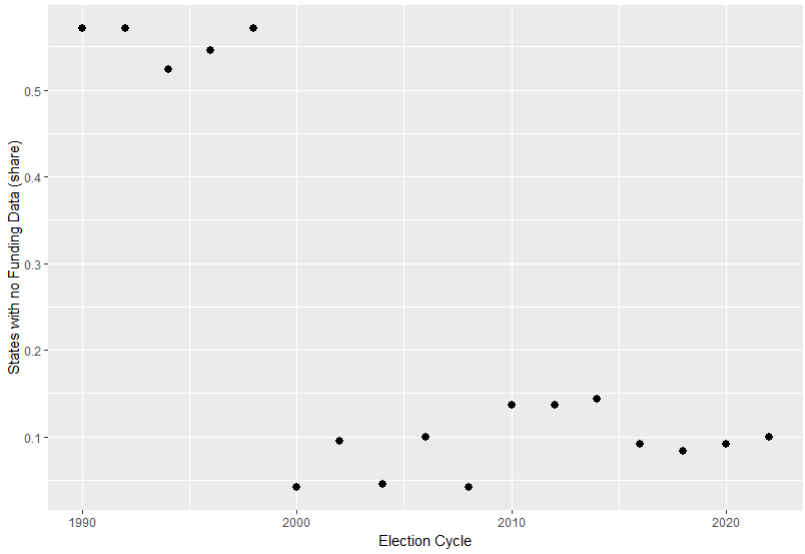


Figure 26: Judges Political Funding data coverage

B.1 Data Coverage

This section provides details about data coverage. As can be seen in Figure (26), 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.