Presidential Control of the Judiciary via the Appointment Power: Evidence from Russia

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Abstract

In many countries, the president is involved in appointing judges. Does this lead to selection of friendly judges who then promote the president's interests? This question is explored here in the context of Russia, where judges are often said to favour the executive.

I gather data on 2000 court cases, and analyze them by exploiting changes in the appointment rules. I find clear evidence that judges selected by the president favour the government more than do their peers. In the process, the paper develops a new solution to the sample selection problem endemic to the analysis of court decisions.

Keywords: judiciary; political selection; institutions.

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1 Introduction

In most societies the president is directly involved in selecting judicial candidates. This paper investigates the impact of this on judicial decisions.

The paper makes three contributions. First, it provides robust empirical evidence on that judges selected with presidential involvement are more likely to favour the government. Second, it does so in the context of Russia – a trouble-ridden democracy where the government is often accused of interfering with law enforcement. Finally, the paper develops a new method for the empirical identification of the impact of appointment procedures on judicial decisions.

So far, the evidence on the impact of presidential involvement has been indirect, and the bulk of it comes from the United States, where democratic institutions are strong. Arguably, judicial restraint is more important where such institutions are weak, yet in such societies it is usually harder to collect the data which would allow to study this issue.

Russia is case in point. A major criticism laid against its democracy is the unchecked power of its executive government, headed by the country's president (Fish, 2005). Russia's courts have been described as "yet another tool of presidential power" (McFaul and Stoner-Weiss, 2008).

Although the Russian president has no direct control over the courts, he has the power to appoint judges. This study investigates whether he uses this to select judges who then favour the government in their decisions. I exploit a rare institutional change to construct a direct test of this hypothesis.

The focus here is on Russia's 'arbitrazh' courts which handle all economic disputes. These courts were created in 1991, during the country's rapid shift to a market economy and democratic regime, in a process overseen by the Supreme Soviet, without presidential involvement. Two years and 1200 judicial appointments later, the Supreme Soviet was disbanded and Russia's first democratic constitution put the president in charge of judicial appointments.

Today, these differently appointed judges work side by side. Do they behave in different ways? To answer this question, I collected data on judicial decisions in disputes involving taxes, regulation, property and contractual rights.

Comparing the decisions of the two types of judges is not straightforward because they are likely to handle different cases. For example, if a potential litigant believes the judge is biased against him, he is more likely to try to settle the case out of court. Identification of the appointment effect is greatly impeded by this problem of sample selection[1].

This is well known to the scholars who compare US judges appointed by presidents from different parties. Ashenfelter et al. (1995) summarize the early literature which failed to account for sample selection, and make a break with it by exploiting random assignment of judges to cases in certain trial courts. He finds no differences across judges[2].

More progress has been made by researchers analyzing judicial votes in the US Supreme Court and in the three member panels of US federal courts of appeal. Many of them do find significant partisan differences across judges (Revesz, 1997; Cross and Tiller, 1998; Sunstein et al., 2006; Epstein et al., 2007; Cox and Miles 2008). The US courts of appeal make an attractive setting for these tests because panels are randomly assigned to cases, and the votes of individual judges in the same case provide an additional source of variation. Still, the approach runs into difficulties if litigants can settle disputes after the case is allocated to judges, or if judges vote strategically. Even more importantly, this approach may not be available in other judicial systems.

By showing that, conditional on the same appointment scheme, different US presidents appoint different judges, the above papers give indirect evidence on the impact of the presidential involvement in judicial appointments. In contrast, this paper establishes this impact more directly by demonstrating the causal effect of a change in the formal appointment scheme which introduced the president into the process.

This paper also takes a different approach to solving the sample selection problem: it leverages data from two levels of judicial hierarchy. To do this, I coded information on 2000 cases that were heard in Russian courts of appeal between 1995 and 2002. Appellate courts were created in 1995, with judges selected with the president's help. However, in regional courts only about half of the judges were chosen by the president. My approach is to compare judicial decisions using two sources of variation in appointment – across judges within regional courts, and between regional and appellate courts. This identification technique is a version of the difference-in-differences method.

The main result is that there is indeed a significant appointment effect: judges appointed by the president side with the government more often than the rest. A firm litigating against a government agency under a judge appointed by the president finds its chances of winning reduced from about 55 to 50%. This confirms the conjecture that the president influences the courts through appointments.

Furthermore, the concerns about sample selection are well justified: differently appointed judges handle systematically different cases. These differences are so large that ignoring them would have led to the opposite conclusion about the attitude of presidential judges towards the government.

This paper complements existing work on selection of public officials. With a few exceptions (such as Prendergast, 2001), this literature has been heavily focused on voter selection (Osborne and Slivinsky, 1996; Besley and Coate, 1997) and comparison between elected and appointment officials (Besley and Coate, 2003; Besley and Payne, 2003; Guerriero, 2011; Lim, 2013). In contrast, this paper analyzes how appointed officials are selected.

I begin with a description of arbitrazh courts in section 2. The data are described in section 3. Section 4 develops empirical identification strategy. Section 5 contains the main empirical results, and section 6 considers their robustness and extensions. Section 7 concludes.

2 Russian arbitrazh courts, 1991 – 2002[3]

2.1 Structure and performance

In 1991, during its major political and economic transition, Russia created arbitrazh courts, a new judicial branch for economic disputes. The Supreme Soviet, then legislative branch of the government, set up 81 regional courts, drawing on the existing infrastructure of Gosarbitrazh, a branch of Soviet bureaucracy for resolution of economic disputes (Hendley, 1998)[4]. In 1995, 10 courts of appeal known as okrug courts were added.

Arbitrazh courts deal with disputes over contracts, property rights, taxes, regulatory decisions, and so on. Judges specialize in an area of law, and court chairmen use this to allocate cases, though this still leaves a lot of discretion with the chairmen (Aitkulov and Popelysheva, 2013; Despouy, 2009). Generally, on paper the system has enviable provisions for judicial independence (Kahn, 2008), including:

- The judges are appointed until retirement, and their salaries cannot be reduced.
- All courts are subject to the same federal rules and are funded from the federal budget. Thus they are formally separated from the regional authorities[5,6].
- Laws prevent litigants from strategically choosing their court[7].

Nevertheless, many disagree over how well these courts work in practice. Generalizing from high profile cases such as the government's tax evasion lawsuit against oil company Yukos and its CEO Mikhail Khodorkovsky, some believe that arbitrazh courts are ineffective and lack independence (McFaul, 2001; Black et al., 2000; BBC News, October 31, 2003). The alternative view, backed by surveys and case studies, is that despite this bad publicity firms rely on arbitrazh courts for routine disputes (Hendley, 2004; Frye, 2004). Indeed, several papers have established an empirical link between court performance and economic activity in Russia (Johnson et al., 2002a; Johnson et al., 2002b; Shvets, 2013).

A more detailed look at the evidence suggests that the performance of the arbitrazh courts depends on who is involved. First, when powerful government interests are present, this can affect judicial decisions (Lambert-Mogiliansky et al., 2007), and make firms reluctant to litigate (Frye, 2004).

Second, size matters. Small firms have a lower opinion of the court system than large firms, possibly due to their more limited ability to "faciliate" judicial process through payments and gifts – something that large firms do significantly more often (BEEPS, 2002).

The analysis below will touch upon both of these observations.

2.2 Appointment process

The history of appointments to arbitrazh courts has two distinct periods: before and after the 1993 constitution which granted the appointment power to the president (see figure 1).

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2.2.1 Between 1991 and 1993

The early 1990s were marked by major political turmoil and power struggles. On the one side was the Supreme Soviet, Russia's legislature largely inherited from the old Soviet system, and on the other Boris Yeltsin, the country's first democratically elected president. In late 1990, the Supreme Soviet set up the new Supreme Arbitrazh Court and appointed the ex-head of Gosabitrazh as its Chairman. In 1991, the Supreme Soviet created 81 new regional arbitrazh courts and they needed judges. In 56 regional courts, the law prescribed the following appointment procedure. The Chairman of the new Supreme Arbitrazh Court was to make a nomination for each post, first by himself, then with the help of so-called qualifying committees formed from (recently appointed) senior judges. The Supreme Soviet then approved these nominations, and made the final appointment (Law No. 1543–1, 1991, Law No. 3133-1, 1992).

Although the final appointments were recorded, there is virtually no evidence on how this process was implemented, and so it is not completely clear how big the *de facto* role of the Supreme Soviet in selecting judges was. We know that it approved over 1200 judges in the course of two years of extreme political turbulence, which makes it doubtful that the Supreme Soviet devoted much time to reviewing the candidates. Furthermore, anecdotal evidence suggests that a significant number of judges appointed during this period had been the members of the Soviet Gosarbitrazh who were moved to a roughly equivalent position in the new courts[8]. All of this suggests that the Supreme Soviet played little role in screening the candidates.

To the extent that it did do such screening, its agenda would have been very different from that of the president, with whom it was locked in a power struggle. Therefore, we will refer to judges appointed in these 56 courts in 1991-93 as 'non-presidential', in constrast with the judges appointed after the adoption of 1993 constitution.

In the remaining 25 regional courts, the procedure was different to reflect the relative autonomy of these regions from the central government. There, the law put regional authorities in charge of judicial selection. However, the process was not spelled out in the legislature, and this resulted in a variety of approaches, some rather *ad-hoc* (Trochev, 2006). Furthermore, no systematic record of these appointments exists[9]. Finally, it is believed that regional elites in these autonomous regions supported the president (Triesman, 1999), making this group of judges less than an ideal control group for the presidential appointees. For all these reasons, judges appointed in these 25 autonomous regions in 1991-93 are not included in this analysis.

2.2.2 After 1993

In 1993, Yeltsin disbanded the Supreme Soviet and pushed through a new Constitution which granted vast powers to the president (McFaul, 2001). One such power was that of final judicial appointments in all regional arbitrazh courts. By 2002, about a half of all regional arbitrazh court judges had been selected with the president's involvement. On their creation in 1995, okrug courts of appeal fell under the same rule. Those involved in this process report that the presidential office takes this power seriously and devotes substantial resources to gathering information on judicial candidates (Trochev, 2006).

This historical change has led to a situation where judges appointed by different methods work side by side in regional courts. In contrast, appellate courts only have judges selected by the president. This generates two sources of variation in judicial appointments: within regional courts and across the two tiers of courts.

3 Data

This section describes the data, which come in three parts: information on cases, firms, and judges. More details are in appendix A.

3.1 Cases

By reading the texts of decisions provided by Kodeks, a database of legal documents[10], I constructed characteristics of approximately 2000 court cases (1500 between firms and government, and 500 between two firms). They are summarized in table 1, and include identities and type of parties, type of dispute, courts and judges involved, and their decisions, and several other characteristics. The cases span the period from 1995 to 2002, and 52 regional courts[11].

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3.2 Firms

The disputes in the dataset involve a little over 2,500 firms. I group them into small, medium and large using the information on their name and legal status (e.g. solo proprietorship, joint stock company, etc.), and where possible supplement this with the data on employment from the Gnozis and the Alba databases of Russian medium and large enterprises. Table 1 provides the breakdown of cases by firm size[12].

For a subset of 300 cases, I was also able to get the information on the sector of the firm, from the Gnozis and the Alba databases (table 2).

3.3 Judges

Just over 800 judges are involved in the disputes in the dataset: 664 in regional courts and 142 in courts of appeal. Using the names of the judges listed in the decision, I matched them to the information on their appointment gathered from official statements of the Supreme Soviet and the President's Office[13,14,15].

Table 1 shows the decomposition of the sample by appointment and tier. We see that in the full sample, the government wins roughly half of the time against firms. Although selection into litigation prevents us from drawing strong conclusions, this number is at odds with the view that the system is so biased in the favour of the state that litigation is pointless (section 2.1). Yet, as we will see in section 5, behind this balanced win rate lies a significant difference in how often differently appointed judges rule in favour of the government.

4 Empirical identification

4.1 Sample selection problem

As a first pass at investigating the appointment effect we may be tempted to compare government victory rates for presidential and non-presidential judges in regional courts. They are 50% and 55% respectively, a statistically significant difference at the 5% level (table 1). This seems to suggest that the president's judges are *harsher* on the government – the opposite of our intuition.

However, if differently appointed judges handle systematically different disputes this conclusion does not follow. The presidential judges may rule in favour of the government less frequently either because they are harsher or because they attract different disputes (e.g. the disputes in which the government's case against the firm is relatively weaker) or both.

Indeed, table 1 shows that there are significant differences in the observable characteristics of cases handled by differently appointed judges. Presidential judges are more likely to hear disputes involving the government, particularly the federal government. They are less likely to handle contracts and property rights disputes and are more likely to work on regulatory issues; there are also some small differences in firm's sector (table 2). This corroborates the concern about sample selection, particularly if the observable features are related to key unobservable characteristics, which we will call "the strength of the government's case against the firm".

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Indeed the literature on litigation has long argued that there will be systematic differences in this strength across differently appointed judges. There are two main mechanisms at work: the first is self-selection by litigants, and the second is allocation of cases within courts.

First of these, self-selection, occurs because potential litigants choose whether to settle out of court based on the judicial decision they expect (Priest & Klein, 1984; Bebchuk, 1984; Waldfogel, 1995). If litigants know who will handle their case and that the appointment of a judge matters for the decisions, the strength of the government case in disputes that proceed to litigation will generally be different for differently appointed judges (see appendix B for a formal exposition). If the litigants have an option to settle out of court after the judge has been allocated to their case, self-selection is a concern even if cases are allocated to judges randomly within courts.

Second, consider non-random allocation of cases within courts. One possibility is that judges specialize in certain sectors or areas of law. Then if (a) the strength of government case is different across these areas of specialization and (b) differently appointed judges have different specializations, the comparison of government win rates across differently appointed judges will give a biased estimate of the appointment effect. Another possibility is strategic behaviour of court chairmen who allocate cases. For instance, if the chairmen want to help the government and they know that the presidential judges are more favourable towards it, they may be able to increase the share of government victories by allocating cases where the government's hand is weaker to such judges. Then, a comparison of government win rates across judges will understate the true appointment effect.

4.2 Solution with appellate decisions

I address the sample selection problem by considering both appellate and regional court decisions in each case. Figure 2 shows the four groups for which the rate of government victories can be calculated in the dataset. α is the share of government victories in decisions of presidential judges in regional courts, and γ is this share in decisions of appellate judges in the same set of disputes. Similarly, β is the share of government successes in decisions of regional non-presidential judges, and δ is this share in the same cases at appeal.

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There are two ways to construct the difference-in-differences estimate of the appointment effect. First, note that $\alpha - \beta$ is the naive 'horizontal' estimate of the appointment effect which, as discussed above, is plagued by the sample selection problem. Now, $\gamma - \delta$ is the difference in government's success rates at appeal in cases that have come from differently appointed judges. Since there is no variation in the appointment of appellate court judges, this difference captures the underlying difference in the strength of the government's case in the two groups of disputes. This is therefore our estimate of the sample selection effect, and subtracting it from the 'naive' estimate $\alpha - \beta$ gives us the estimate of the true appointment effect:

$$(\alpha - \beta) - (\gamma - \delta)$$

Alternatively, observe that $\delta - \beta$ is the naive 'vertical' estimate of the appointment effect. It compares the decision of presidential judges at appeal to those of non-presidential judges in regional courts. This is for the same group of disputes, so it side-steps the selection problem. However, this estimate will be biased if there is a 'tier effect': a discrepancy in attitudes towards the government at different levels of the judicial hierarchy even if the judges are appointed in the same way.

Fortunately, tier effect can be estimated by $\gamma - \alpha$, the difference in government's success in decisions of the regional and appellate court judges in the same disputes when both are appointed by the same method (the president). The difference between the two gives us an unbiased estimate of the appointment effect:

$$(\delta - \beta) - (\gamma - \alpha)$$

This helps highlight the key assumption of the identification strategy: that the tier effect is the same whether the dispute was originally handled by non-presidential judges (counterfactual) or presidential judges (estimated)[16].

Although it is hard to assess the plausibility of this assumption without a full theoretical model of appellate decision making, we can think of two broad scenarios when it may be violated. First, if the decision of the appellate judge directly depends on how the regional court judge had been appointed: that is, in two identical cases, the appellate judge is more likely to favour the government if the regional judge had been appointed in a particular way. Although a theoretical possibility, this is not very plausible and is unlikely to be a major threat to identification.

Second, since we expect differently appointed judges to handle different disputes, the assumption may be violated if the tier effect varies with dispute characteristics. Though this cannot be tested directly, I will provide indirect evidence that it does not.

4.2.1 Judicial characteristics

The above analysis ignores numerous characteristics of judges that may affect their decisions. Failing to account for these might bias our results, but only if these characteristics are also correlated with the appointment. Although it is not easy to come up with such examples in this context (since the judges appointed by different methods adjudicate side by side, in the same courts, at the same time), one possible threat to identification is that non-presidential judges had been appointed earlier than their presidential counterparts. For example, if experience of a judge affects his attitude towards the government, the appointment variable may pick up the effects that are due to the *date* rather than *method* of appointment. I address this potential concern in the robustness checks (section 6) by explicitly controlling for the date of appointment.

5 Main empirical results

5.1 Graphical intuition

Figure 3 is a scatter plot showing the rate of government victories in regional courts (horizontal axis) and appellate courts (vertical axis). Each point corresponds to cases heard in a particular regional court, by type of judge: black filled circles for presidential judges, grey hollow circles for non-presidential judges. The sizes of circles are propotional to the number of observations for each regional court.

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The line of best fit for the presidential judges lies below or to the right of the line of best fit for the non-presidential judges: The difference in the intercepts is statistically significant at 10% level. This suggests a positive appointment effect — the presidential judges side with the government more frequently than do non-presidential judges in disputes that fare similarly at appeal. This visualization of our difference-in-differences estimate captures the main result of this paper.

5.2 Mean comparison tests

I present the first set of results in the form of mean comparison tables 3 through 6.

5.2.1 All firms versus government

Table 3 breaks down the sample of all 1500 cases between the government and firms in the way suggested by figure 2. Each cell reports the mean share of government victories for regional and appellate judges given the appointment of the regional judge (out of 100). The standard errors in brackets have been constructed allowing for a correlation between the decisions of appellate and regional court judges in the same case.

As discussed in section 4.2, there are two ways of thinking about the difference-in-differences estimate of the appointment effect. For the 'vertical' approach, we first compute the difference in government wins between a presidential appointee at appeal and non-presidential judge in the region ($\delta - \beta$ in figure 2), which is 5.3 percentage points here.

Then the tier effect ($\gamma - \alpha$) is calculated: it turns out to be small and insignificant. Subtracting the tier effect from ($\delta - \beta$) gives us the final estimate of the appointment effect. It is positive and significant at the 5% level: judges chosen by the president are more likely to side with the government than non-presidential judges in similar disputes. Quantitatively, a firm lowers its chances of winning against the government by 4.4 percentage points when it faces a presidential judge.

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An alternative way to view the estimate of the appointment effect is through a 'horizontal' comparison of government victories. First find the difference between government victories in the two appointment groups in regional court ($\alpha - \beta = -5.2$). This estimate is likely to be contaminated by sample selection, whose effect can be estimated by the difference in government victories in the same two groups of disputes in appellate court ($\gamma - \delta = -9.5$). By subtracting the selection effect from the naive estimate we find the final appointment effect: 4.4 percentage points.

Note that the selection effect is different from zero at the 1% significance level. This confirms the raison d'être of our empirical method: that differently appointed judges attract systematically different disputes.

Furthermore, the sign of the selection effect implies that presidential judges in regional courts get disputes in which the government's case is on average 10 percentage points *weaker*. This result is unsurprising if either the government or court chairman acting its on behalf leverage pro-government stance of presidential judges. While we cannot definitively say which is the true channel, additional evidence presented in Section 6.5 shows that court chairmen play an important role in facilitating the appointment effect, and so supports the idea of strategic case allocation by chairmen.

5.2.2 Appointment effect and firm size

Recall that according to surveys, smaller firms in Russia are less happy with the judicial process and less able to 'adjust' it in their favour (section 2.1). This suggests that small firms may suffer more from pro-government tendencies of presidential judges than their larger counterparts, who are better able to counteract unfavourable attitudes of judges with side payments.

To explore this, we break down our sample by firm size and compare sub-samples of disputes where the government is litigating against a large firm (table 4) and where it is litigating against a small firm (table 5).

Large firms do not escape the appointment effect: it is still positive, significant at 10% level and implies that the probability of winning against the government for a large firm drops by 8 percentage points (from 50 to 42%) when a non-presidential judge is replaced with a presidential appointee.

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The impact on small firms is similar but starker: the appointment effect is positive, significant at 1% level and implies a 12 percentage points drop in a small firm's probability of winning (from 39 to 27%) when a non-presidential judge is replaced with a presidential appointee. Although the point estimate for small firms is 1.5 times larger than that for large ones, the small sample size implies large standard errors and so the difference, although suggestive, is not statistically significant.

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The division of firms by size has an unavoidable element of arbitrariness. For robustness, we re-estimate the appointment effect for 'individual entrepreneurs', most of whom are street vendors (cigarette sellers, pastry stalls, etc.), and get very similar results to those using all of the small firms (table 6).

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In all subsamples, we continue to find strong selection effects of the same sign as before – the president's appointees handle disputes in which the government's case is significantly weaker. In none of the subsamples do we find a significant tier effect, which is consistent with the key assumption of our identification strategy.

5.2.3 Placebo test: Disputes between firms

We now look for a possible appointment effect in disputes between firms, where the government is not a party. If we find differences there as well, this would suggest that the differences between presidential and non-presidential judges are not just about their attitude towards the government. Table 7 shows the rates at which large firms (panel A) and small firms (panel B) lose to other firms. We find no appointment or selection effect in these disputes, corroborating our earlier finding and its interpretation.

⟨ ⟨ **COMP: Place Table 7 about here** ⟩ ⟩

6 Robustness and regressions

We now subject the main results to several robustness tests using regression estimates. To preview the results, the general message does not change, although the results are weaker in some of the more demanding specifications. In the overall sample, the appointment effect remains significant but now at the 10% level. A strong appointment effect also persists in disputes with small firms (significant at the 1% level), where robustness tests have minimal effect. Finally, although the appointment effect in disputes with large firms is now insignificant in most specifications, the point estimates are the same as those found in mean comparison tests.

To write down the estimation equation, denote the decision in case i by $d_i \in \{0, 1\}$, 1 if it is in favour of the government, and 0 otherwise. The appointment effect can then be estimated using the following model,

$$\text{prob}(d_i = 1) = f(\alpha_0 + \alpha_1 R + \alpha_2 P + \alpha_3 RP), \tag{1}$$

where $R = 1$ if the decision is made by a regional judge and $R = 0$ if by appellate judge; $P = 1$ if the judge who decided on this case in the regional court had been appointed by the president and $P = 0$ if not; RP is the interaction of these two dummies. We can estimate the effects of these variables using both linear and non-linear specifications for $f(.)$. In the linear model, which is presented first, the coefficients have the following interpretation:

- α_0 is the mean share of government victories in decisions of appellate judges in cases originally handled by regional judges not appointed by the president.
- α_1 is the naive 'vertical' estimate of the appointment effect which ignores the tier effect.

- α_2 is the selection effect.
- α_3 is the appointment effect.
- -($\alpha_1 + \alpha_3$) is the tier effect.
- $\alpha_2 + \alpha_3$ is the naive 'horizontal' estimate of the appointment effect which ignores sample selection problem.

6.1 Baseline regressions

Table 8 reports the results of estimating the baseline equation (1) using a linear probability model. The results (columns 1-4) are identical to those we obtained in mean comparison tests. Allowing for further correlation between decisions of the same regional judge increases standard errors slightly (columns 5–8). Qualitatively, this pushes the significance of the appointment effect in the overall sample down to the 10% level. The other qualitative results remain the same as before. In the remainder of the estimations, we will use these adjusted standard errors throughout.

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6.2 Confounding factors

The paper has argued that differently appointed judges will handle systematically different cases. Appellate court decisions allow me to correct for this by capturing all observable and unobservable differences in disputes across the two groups of judges that matter for judicial decision. If this works well, then the estimates of the appointment effect should not change if observable case characteristics are included into (1).

Table 9, columns 1 – 3 report the results of this exercise. The controls include:

- the type of case (contract, property rights, tax, regulation, or other);
- whether the government was the plaintiff in the original dispute;
- whether the government was absent at appellate court hearing;
- the number of months between regional and appellate court decisions;
- whether the dispute involved more than two parties.

The results are virtually the same as in the base line regressions in table 8. In disputes with large firms, the coefficient is a fraction smaller and this pushes it out of the significance region. In the subsample with small firms, there is still a significant appointment effect. The inclusion of case characteristics does not reduce the size of the selection effect, and this suggests that the unobservable characteristics that are important for the decision and selection (e.g. the strength of government's case) are mostly uncorrelated with observable case characteristics such as case type.

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The regressions consistently estimate the appointment effect as long as there are no judicial traits that are correlated with both the appointment procedure (but are not a consequence of it) and the propensity to favour the government. As discussed in section 4.4, the one potentially serious confounding factor is the timing of appointments: presidential judges were appointed in later years, and so have shorter tenure and are likely to be younger, less experienced and may have come from a different pool of candidates.

The limited data on judicial characteristics make it impossible to control for these traits. Instead, I exploit the nature of potential changes produced by the timing of appointment: age, experience and the pool of applicants are likely to change gradually over time. In contrast, the change in the appointment procedure was an abrupt one-off shift at the end of 1993. Thus one can separate the two effects from each other by including in the regression the date of appointment, a continuous variable which should control for all gradual changes associated with the timing of appointment. Since it may also pick up the effect of the appointment procedure, its inclusion should also make it harder to identify the appointment effect.

Columns 4, 5 and 6 in table 9 report the results of estimations which include the date of appointment. As expected the standard errors on the appointment effect rise. In the subsample with large firms, the appointment effect is similar in magnitude to that found earlier but it is no longer significant. In the overall sample, the appointment effect continues to be significant at the 10% level, and in the sample with small firms it continues to be significant at the 1% level. Note that the date of appointment is not significant in any of the regressions. Furthermore, the results do not change much when we also include the square of the appointment date to control for potential nonlinearities in the trend.[17]

I have also re-estimated equation (1) with observable judicial characteristics (gender and ethnic minority indicators). The estimates of the appointment effect do not change.

6.3 Non-linear estimations

To what extent do the regression results depend on the assumption of the linear probability model? To investigate this, (1) is re-estimated using probit and logit models, controlling for the key confounding factor, the date of appointment.

Table 10 reports marginal effects of these estimations for the entire sample, and the two sub-samples: large and small firms[18]. A comparison with the earlier linear estimates in columns 4-6 of table 9 shows that qualitative results are unchanged by shifting to non-linear models, and they are also quantitatively very similar.

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6.4 Fixed effects model

In all of the above estimations, *some* of the effect comes from comparing presidential judges in one region to non-presidential judges in another region. With the exception of autonomous regions before 1994, there is no variation in appointment procedures across regions, and the key institutional change occurs before the start of the sample. This means that standard concerns about difference-in-differences estimates that take advantage of regional variation do not apply here.

However, the one caveat is that the share of judges appointed by the two methods does vary from court to court. If this share is correlated with the court's attitude towards the government, this may bias the results[19].

To address this, I introduce court fixed effects into the linear regression (table 11). The identification now occurs by comparing differently appointed regional judges who adjudicate side by side in the same regional court. The model also includes year effects which help control for the large movements in aggregate economy during the sample period: Russia's 1998 economic crisis and subsequent recovery. Table 8 shows that our results remain virtually unchanged.

⟨ ⟨ **COMP: Place Table 11 about here** ⟩ ⟩

6.5 Court chairmen[20]

Like rank-and-file judges, court chairmen can also be divided into presidential and non-presidential appointees, who oversee 35% and 65% of cases in the sample respectively. Given that the chairmen are said to have a lot of power in Russia (Despouy, 2009) and are involved in appointment of judges, I now ask whether the presidential appointment effect particularly flourishes under the presidential chairmen. This might also provide an indirect indication on whether the large selection effects found in the data are due to strategic case allocation by some of the chairmen.

When I re-estimate equation (1) separately for the two types of chairmen, the appointment effect is now entirely concentrated in courts where the chairman has been appointed by the president (table 12). This may be because the presidential chairmen help select judges that are particularly favourable towards the government, or because they encourage this stance among the presidential judges.

In both types of courts, the sign of the selection effect implies, as before, that the presidential judges get cases where the government's hand is weaker. Although the difference in selection effects is not statistically significant, the point estimate for the presidential chairmen is 1.5 times larger. Together with the finding above that the presidential chairmen facilitate the appointment effect, the evidence is suggestive of court chairmen using case allocation to help the government.

⟨ ⟨ **COMP: Place Table 12 about here** ⟩ ⟩

7 Concluding remarks

This study investigated whether powerful politicians could bridge the separation of powers by appointing friendly judges. It focussed on Russia, where relatively new democratic institutions struggle to keep the power of politicians in check, and the need for an independent judiciary is particularly pronounced.

It is difficult to gather data that can be analyzed for evidence of politicians interfering with the judiciary in any context. It is particularly hard to do in countries where such interference matters the most. A change in judicial appointment rules during Russia's chaotic transition from the Soviet state provided us with a window through which to gather the otherwise illusive evidence of the government's influence over the judiciary.

The findings show that the appointment process matters: the president selects judges who are significantly more favourable towards the government. On average, this helps the government increase its chances of winning from 50% to 55%: i.e. the appointment tips the scales in government's favour overall by changing the decision in 5% of cases, without being pivotal for the rest. Perhaps more of a concern is the size of appointment effect in cases with small firms, where getting a presidential judge shifts the probability of government winning from 62% to 74%.

Generally, the size of the estimated appointment effect will depend on the sample of cases considered, and so care must be taken when generalizing the results to other contexts. However, the approach developed in this paper of using appellate decisions as a solution to the sample selection problem can be applied to studying judicial attitudes in other settings. The key requirement is that the judicial characteristics that induce the differences in attitudes vary within at least one court tier. The method may be particularly useful for other countries with weak institutions where politicians might interfere with the judiciary and strategic case allocation may be allowed to flourish.

More generally, the paper contributes to the literature on design of institutions. This literature has mostly focussed on the incentives of public officials, while the evidence here demonstrates that the process of selection also has serious consequences. Further understanding selection as well as its interplay with incentives are important directions for future research.

Appendix A

Data sources

The data come from three sources. Case data were created by reading and coding the texts of appellate court decisions obtained from Kodeks, a company supplying legal information. Information on appointment of judges was collected from the documents of the Supreme Soviet and presidential offices, available in Konsultant+ legal information database and the president's office website. The data describing employment of firms participating in court cases were taken from their financial reports in the Gnozis and Alba databases[21].

Sample

Initially, a sample of 80 decisions was drawn for each of 81 regional courts, aiming at an equal number of disputes that involved a) only firms, b) a federal government agency, c) a regional government agency, and d) a local government agency. Within each category, the disputes were sampled randomly. Several small courts contained less than 80 decisions in Kodeks databank in total.

The final data set contains 2,028 decisions from 52 regional courts. The main filter in going from the initial to final sample was whether the court case included names of the judges who handled it – the information critical for us to establish how the judge was appointed. This first of all meant dropping 29 courts where the names of judges are virtually never reported[22]. I also dropped the cases from autonomous regions handled by a judge who was not in the database of all presidential appointees[23]. Of the cases in the final sample, 1,499 are between a firm and a government agency, and 529 are between two firms.

Variables

Characteristics of judges:

Ethnic minority is equal to 1 if the last name of the judge has a non-Russian ethnic origin, and zero otherwise. These include names of Kazakh, Tartar, Armenian, Georgian, Lithuanian, German, Polish, Finnish, Ukrainian, and other origins.

Date of appointment is the number of quarters between the appointment of the judge and December 31, 1991 (fractions allowed).

Case participants:

Government agencies include branches of and agencies reporting to federal, regional, and local executive governments in Russia.

Small firms include firms with less than 150 employees, and where the data on employment are not available, individual entrepreneurs, solo proprietorships, cooperatives, farms, and full liability partnerships.

Large firms include firms with more than 650 employees, and all publicly traded companies except those with less than 250 employees.

Firm's sector is the sector in which the disputing firm operates. This was gathered for all firms in approximately 300 firm v. government cases. They include

(a) all firms which could be matched to Gnozis and Alba databases, and have data on sector there. These are typically larger industrial firms.

(b) all firms that could be identified as a bank or a non-profit organization from their legal status in the decision texts.

Case characteristics:

More than two parties involved is equal to 1 if either on the side of the defendant, or the plaintiff there is more than one entity involved in the dispute.
Government absent from appellate hearing is equal to 1 if there was no government representative at appeal.

Appendix B

This formal framework demonstrates that if differently appointed judges have different attitudes towards the government, they will generally attract disputes with different underlying strength of government case. The premise of the model is that potential litigants diverge in their estimates of their chances of winning: overoptimistic forecasts then lead to litigation (Priest and Klein, 1984; Waldfogel, 1995; Yildiz, 2004).

The set up

Consider a population of disputes between two parties indexed by $i \in \{F, G\}$: a firm on the one side and the government on the other. M is the government's claim against the firm; for simplicity the same across disputes[24]. Disputes vary in the strength of the government's case for this claim which is denoted by s. The costs of litigation $C > 0$ are born by each litigant, and satisfy $2C < M$.

There are two judges, indexed by $P \in \{0, 1\}$ where 1 is the presidential judge, and 0 is the non-presidential judge. Each case is assigned to one of them, and this assignment is independent of s.[25]

In each case, a judge observes s and decides in favour of the government if $s > t_P$, where t_P a threshold which captures the attitude of judge P towards the government.

Disputing parties do not observe s, but each forms a noisy estimate denoted $\hat{s}_i$ before going to court. The parties know which judge will handle their case and t_P before they decide whether to litigate[26]. The timing is this:

1. The parties form expectations about s.
2. They learn whether $P = 1$ or $P = 0$ judge will handle their case.
3. The parties try to settle out-of-court. If out-of-court negotiations break down the case goes to court.

Selection into litigation

At the third stage, party i expects the government to win if $\hat{s}_i > t_P$. If both parties expect the government to win, the firm can make an offer to the government $(M + C)$ which exceeds the offer the government is willing to accept $(M - C)$. Hence the parties will settle the dispute out of court. Similar logic applies if both expect the government to lose.

In contrast, litigation will go ahead if both the government and the firm expect to win[27], i.e. if

$$\hat{s}_G > t_P, \text{ and } \hat{s}_F < t_P. \tag{B1}$$

Proposition 1 *The distribution of s in litigated disputes handled by a regional judge depends on his threshold.*

Proof. Denote the probability density of s in the population by $h(s)$. Assume that both parties draw their estimates of s from the same conditional distribution, denoted $F(\hat{s}|s)$ with a corresponding density $f(.)$.

For a given t_P, a dispute with the strength of government case s goes to court if (B1) is satisfied. This gives rise to the following conditional probability density of s among disputes that go to court

$$k(s|t_P) = \frac{h(s)[1 - F(t_P|s)]F(t_P|s)}{\int h(x)[1 - F(t_P|x)]F(t_P|x)dx}. \tag{B2}$$

Since $k(s|t_P)$ is a function of t_P, in general the two judges will end up with cases that have different distributions of s. ■

Notes

[1]In contrast to sample selection in other contexts, selection into litigation typically cannot be addressed by Heckman's two-stage procedure since the researcher usually knows nothing about the disputes that do not reach courts. There are however some notable exceptions (Sieg, 2000).

[2]Random allocation in some US courts has also been used to establish evidence of ideological disparities across judges that are not necessarily linked to their observable characteristics (Abrams et al. 2012, Anderson et al., 1999).

[3]After 2003, the system underwent a number of changes but these are beyond the scope of this paper, which analyzes data up to 2002.

[4]The Supreme Arbitrazh Court was established at the same time.

[5]The exception are appointment rules in 'autonomous' regions in early 1990s. We discuss this in subsection 2.2.1.

[6]Absence of cross-court variation in institutional arrangements also means that many standard concerns about natural experiment estimations do not apply to this paper.

[7]First, the plaintiff is required to sue in the court of the region where the *defendant* is officially registered. Second, decisions of each regional court can only be appealed to one specific okrug court.

[8]For example, see Ivanovo Arbitrazh Court (2015).

[9]Though to some extent this information can be reverse engineered from the other data assembled for this paper.

[10]www.kodeks.ru

[11]After the introduction of appellate courts in 1995, it took some time before they became widely used. As a result, the bulk of the cases in the sample is between 1998 and 2002.

[12]See Appendix A for definitions of firm sizes. Alternative definitions do not have much effect on the results.

[13]Not all regional courts publish names of judges in the texts of decisions, and so the sample had to be restricted to 52 out of the 81 regional courts (see appendix A).

[14]The official statements were taken from Konsultant+ database (http://www.consultant.ru) and the web site of the President's Office.

[15]2/3 of regional decisions in the sample are made by one judge, and 1/3 by a three judge panel. In the latter cases, I use the appointment of the panel's chair, to reflect the belief that the chair has a disproportionate influence on the decision. This view is shared by several lawyers and judges I interviewed. All appellate court decisions are made by a panel of three judges, all appointed by the president.

[16]This assumption is the equivalent of the common trends assumption usually made in the difference-in-differences identification.

[17]The sizes of coefficients remain the same, while some standard errors rise. As a result, in the overall sample, the appointment effect is no longer significant. It remains significant in the subsample with small firms at 1% level. Date of appointment squared is never significant.

[18]The marginal effects are estimated in the way that is meaningful given our setting: (1) The marginal effect of R is estimated at $P = 0$, $RP = 0$, and the average date of appointment of appellate and regional $P = 0$ judges. (2) The marginal effect of P is estimated at $R = 0$, $RP = 0$, and the average date of appointment of appellate judge. (3) The marginal effect of RP is estimated at $R = 1$, $P = 0$, and the average date of appointment of $P = 0$ judges. Finally, the marginal effect of the date of appointment is estimated at the median of the rest of the independent variables.

[19]One example of how such correlation may arise is this: if the president fails to appoint new judges and the court is understaffed, existing judges may express their discontent with the government in their judicial decisions. An estimate of the appointment effect using cross-regional variation would then overstate the true effect.

[20]I am grateful to an anonymous referee and the editor for suggesting this analysis and interpretation.

[21]These databases are described in detail in Bessonova et al., 2003.

[22]Out of the remaining 52 courts, 34 consistently report judge names, and 18 sporadically.

[23]As discussed in section 2, such judges must have been appointed in 1991-1993 potentially in an ad-hoc way that was characteristic of autonomous regions in those years.

[24] Thus, to break the symmetry, the government is always the plaintiff in the initial dispute. Changing this does not affect the results.

[25] This simplifying assumption is needed to keep the focus of the framework on litigant self-selection.

[26] The result is unchanged if we allow litigants to observe t_P with noise.

[27] In this case, the government expects to gain $M - C$ and the firm expects to lose C as a result of litigation. Since $2C < M$, the parties will not be able to agree on a settlement amount and the case will go to court.

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Figure cations

Table 1. Judges and cases[1]

Court tier	Regional		Appellate
Appointment	Presidential	Non-presidential	Presidential
	(1)	(2)	(3)
Number of cases	1019	1009	2028
Number of judges	340	324	142
Characteristics of judges			
Men	0.34*	0.26	0.30
Ethnic minority	0.21**	0.12	0.11
Date of appointment (quarters since Dec 31, 1991)	22.9**	2.9	14.6
Court chairman appointed by president	0.48**	0.23	1
Reversal rate	0.18	0.17	n/a
Cases involving the government			
Share in total	0.76**	0.71	0.74
In cases involving the government			
Share of government victories	0.50*	0.55	0.55
Characteristics of cases			
With federal government	0.54^{+}	0.50	0.52
With regional or local government	0.46^{+}	0.50	0.48
With small firm	0.29	0.25	0.27
With large firm	0.24	0.27	0.25
Contract	0.21^{+}	0.25	0.23
Tax	0.37	0.35	0.36
Regulation	0.25**	0.18	0.21
Property rights	0.07*	0.12	0.10
Government plaintiff	0.37	0.37	0.37
Government absent from appellate hearing	0.60	0.56	0.58
Months between regional & appellate decisions	4.3	4.2	4.3
More than two parties involved	0.08^{+}	0.06	0.07

[1]Unless otherwise stated, the numbers are expressed as shares of the relevant total

** significantly different from column (2) at 1% level, * 5% level, $^{+}$ 10% level

Table 2. Composition of cases by firm's sector (share)

	Regional judge appointment	
	Presidential	Non-presidential
	(1)	(2)
Banking	0.21	0.15
Food	0.16	0.18
Machinery	0.11	0.13
Wood & paper	0.06*	0.13
Fuel	0.11	0.09
Metals	0.11	0.06
Building materials	0.05	0.08
Misc industry	0.08	0.07
Non-profit	0.05	0.06
Textiles	0.05	0.02
Chemical	0.02	0.02
Total number of cases	133	163

** significantly different from column (2) at 1% level, * 5% level, $^{+}$ 10% level
All cases include government as one of the parties

Table 3. Firm v government: Share of government victories

Court tier	Regional court appointment		Difference
	Presidential	Non-presidential	
Regional	49.6 (1.8)	54.8 (1.9)	-5.2* (2.6)
Appellate	50.5 (1.8)	60.1 (1.8)	-9.5** (2.5)
Difference	0.9 (1.6)	5.3** (1.5)	4.4* (2.2)

Selection effect

Tier effect

Appointment effect

** significantly different from zero at 1% level, * 5% level, $^{+}$ 10% level
Standard errors are in parentheses

Table 4. Large firms v government: Share of government victories

Court tier	Regional court appointment		Difference
	Presidential	Non-presidential	
Regional	44.3 (3.7)	49.5 (3.6)	-5.2 (5.1)
Appellate	42.1 (3.7)	55.1 (3.6)	-13.1** (5.1)
Difference	-2.2 (3.5)	5.7* (2.8)	7.9^{+} (4.5)

** significantly different from zero at 1% level, * 5% level, $^{+}$ 10% level
Standard errors are in parentheses

Table 5. Small firms v government: Share of government victories

Court tier	Regional court appointment		Difference
	Presidential	Non-presidential	
Regional	59.5 (3.3)	61.1 (3.6)	-1.7 (4.9)
Appellate	57.7 (3.3)	71.7 (3.4)	-14.0** (4.8)
Difference	-1.8 (2.8)	10.6** (2.9)	12.3** (4.0)

** significantly different from zero at 1% level, * 5% level, $^{+}$ 10% level
Standard errors are in parentheses

Table 6. Entrepreneurs v government: Share of government victories

Court tier	Regional court appointment		Difference
	Presidential	Non-presidential	
Regional	60.9 (3.7)	60.8 (4.4)	0.01 (5.7)
Appellate	60.3 (3.7)	72 (4.0)	-11.7* (5.5)
Difference	-0.6 (3.0)	11.2** (3.6)	11.8** (4.6)

** significantly different from zero at 1% level, * 5% level, $^{+}$ 10% level
Standard errors are in parentheses

Table 7. Placebo tests

Court tier	A. Large firm defeats against other firms (%)			B. Small firm defeats against other firms (%)		
	Regional court appointment		Difference	Regional court appointment		Difference
	Presidential	Non-presidential		Presidential	Non-presidential	
Regional	42.4 (3.7)	44.7 (3.5)	-2.3 (5.1)	59.5 (5.4)	61.6 (4.6)	-2.1 (7.0)
Appellate	42.9 (3.7)	41.3 (3.4)	1.7 (5.1)	52.4 (5.5)	56.3 (4.7)	-3.9 (7.2)
Difference	0.6 (2.7)	-3.4 (2.9)	-4.0 (4.0)	-7.1 (4.7)	-5.3 (3.6)	1.8 (5.8)

** significantly different from zero at 1% level, * 5% level, + 10% level

Table 8. Base line regressions

	(1) Gov v All	(2) Gov v Large	(3) Gov v Small	(4) Gov v Ent	(5) Gov v All	(6) Gov v Large	(7) Gov v Small	(8) Gov v Ent
	Dependent variable: 1 if government wins, 0 otherwise							
Selection effect (α_2)	-0.10**	-0.13*	-0.14**	-0.12*	-0.10**	-0.13*	-0.14**	-0.12+
	(0.03)	(0.05)	(0.05)	(0.06)	(0.03)	(0.06)	(0.05)	(0.06)
Appointment effect (α_3)	0.04*	0.08+	0.12**	0.12*	0.04+	0.08+	0.12**	0.12*
	(0.02)	(0.05)	(0.04)	(0.05)	(0.02)	(0.05)	(0.04)	(0.05)
Naive 'vertical' estimate (α_1)	-0.05**	-0.06*	-0.11**	-0.11**	-0.05**	-0.06*	-0.11**	-0.11**
	(0.02)	(0.03)	(0.03)	(0.04)	(0.02)	(0.03)	(0.03)	(0.04)
Constant (α_0)	0.60**	0.55**	0.72**	0.72**	0.60**	0.55**	0.72**	0.72**
	(0.02)	(0.04)	(0.03)	(0.04)	(0.02)	(0.04)	(0.04)	(0.05)
Tier effect ($-\alpha_1-\alpha_3$)	0.01	-0.02	-0.02	-0.01	0.01	-0.02	-0.02	-0.01
F test statistic	(0.32)	(0.40)	(0.42)	(0.03)	(0.28)	(0.37)	(0.42)	(0.03)
Number of decisions	2998	754	804	608	2988	752	802	608
Number of cases	1499	377	402	304	1494	376	401	304
Standard error clustering	Dispute level				Regional judge level			

**, * and +: significant at 1%, 5% and 10% level respectively
Standard errors are in parentheses

Table 9. Controlling for potentially confounding factors

	(1) Gov v All	(2) Gov v Large	(3) Gov v Small	(4) Gov v All	(5) Gov v Large	(6) Gov v Small
	Dependent variable: 1 if government wins, 0 otherwise					
Selection effect (α_2)	-0.10**	-0.16**	-0.13*	-0.10**	-0.13*	-0.14**
	(0.03)	(0.06)	(0.05)	(0.03)	(0.06)	(0.05)
Appointment effect (α_3)	0.05+	0.07	0.13**	0.06+	0.07	0.17**
	(0.02)	(0.05)	(0.04)	(0.04)	(0.07)	(0.06)
Naive 'vertical' estimate (α_1)	-0.05**	-0.06*	-0.11**	-0.06**	-0.06	-0.13**
	(0.02)	(0.03)	(0.03)	(0.02)	(0.04)	(0.04)
Property rights[1]	-0.01	-0.13	-0.25+			
	(0.05)	(0.08)	(0.14)			
Tax	-0.11**	-0.12	-0.12+			
	(0.03)	(0.07)	(0.07)			
Regulation	-0.02	0.06	-0.14+			
	(0.04)	(0.08)	(0.08)			
Other	-0.03	-0.14	-0.09			
	(0.05)	(0.09)	(0.09)			
Government plaintiff	-0.03	0.01	-0.04			
	(0.03)	(0.05)	(0.06)			
Government absent	0.03	-0.02	0.03			
	(0.03)	(0.05)	(0.05)			
Months between two decisions	0.01*	0.02*	0.01			
	(0.01)	(0.01)	(0.01)			
More than two parties involved	0.03	0.03	0.14			
	(0.05)	(0.08)	(0.12)			
Date of appointment				-0.00	0.00	-0.00
(quarters since December 31, 1991)				(0.00)	(0.00)	(0.00)
Constant (α_0)	0.58**	0.53**	0.76**	0.61**	0.55**	0.75**
	(0.04)	(0.08)	(0.09)	(0.03)	(0.05)	(0.05)
Tier effect ($-\alpha_1-\alpha_3$)	0.01	-0.02	-0.02	0.00	-0.02	-0.05
F test statistic	(0.21)	(0.21)	(0.41)	(0.00)	(0.15)	(1.40)
Number of cases	1488	375	398	1481	372	398

**, * and +: significant at 1%, 5% and 10% level respectively

Standard errors are in parentheses. They are clustered at regional judge level

[1] Omitted category of dispute is contract

Table 10. Non-linear regressions - marginal effects

	(1)	(2)	(3)	(4)	(5)	(6)
	Gov v All	Gov v Large	Gov v Small	Gov v All	Gov v Large	Gov v Small
	Probit			Logit		
	Dependent variable: 1 if government wins, 0 otherwise					
Selection effect	-0.10**	-0.13*	-0.14**	-0.10**	-0.13*	-0.14**
	(0.03)	(0.06)	(0.05)	(0.03)	(0.06)	(0.05)
Appointment effect	0.06+	0.07	0.18**	0.06+	0.07	0.18**
	(0.04)	(0.07)	(0.06)	(0.04)	(0.07)	(0.06)
Naive 'vertical' estimate	-0.06**	-0.06	-0.13**	-0.06**	-0.05	-0.13**
	(0.02)	(0.04)	(0.04)	(0.02)	(0.04)	(0.04)
Date of appointment	-0.00	0.00	-0.00	-0.00	0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Number of cases	1481	372	398	1481	372	398

**, * and +: significant at 1%, 5% and 10% level respectively

Standard errors are in parentheses. They are clustered at regional judge level

See paper for how marginal effects were calculated

Table 11. Fixed effects estimations

	(1)	(2)	(3)	(4)
	Gov v All	Gov v large	Gov v small	Gov v ent
	Dependent variable: 1 if government wins, 0 otherwise			
Selection effect (α_2)	-0.08*	-0.12*	-0.10$^+$	-0.08
	(0.03)	(0.06)	(0.06)	(0.06)
Appointment effect (α_3)	0.05$^+$	0.07	0.13**	0.12*
	(0.02)	(0.05)	(0.04)	(0.05)
Naive 'vertical' estimate (α_1)	-0.05**	-0.06$^+$	-0.11**	-0.11**
	(0.02)	(0.03)	(0.03)	(0.04)
Constant (α_0)	0.68**	0.61**	0.77**	0.74**
	(0.08)	(0.19)	(0.11)	(0.12)
Tier effect ($-\alpha_1-\alpha_3$)	0.01	0.00	-0.02	-0.01
F test statistic	(0.21)	(0.20)	(0.39)	(0.03)
Number of cases	1490	375	400	303

Regressions include regional court fixed effects and year effects. Omitted are Chelyabinsk court and year 2000

**, * and +: significant at 1%, 5% and 10% level

Standard errors are in parentheses. They are clustered at regional judge level

Table 12. Court chairmen and appointment effect

	(1) Gov v All	(2) Gov v Large	(3) Gov v Small
Dependent variable: 1 if government wins, 0 otherwise			
Presidential chairman in regional court			
Selection effect (α_2)	-0.13**	-0.14	-0.13
	(0.05)	(0.91)	(0.09)
Appointment effect (α_3)	0.13**	0.14+	0.15*
	(0.05)	(0.08)	(0.08)
Tier effect ($-\alpha_1$-α_3)	-0.05+	-0.05	-0.06
(F test statistic)	(3.43)	(0.68)	(2.1)
Number of decisions	1092	250	330
Number of cases	546	125	165
Non-presidential chairman in regional court			
Selection effect (α_2)	-0.08*	-0.16*	-0.12+
	(0.04)	(0.07)	(0.07)
Appointment effect (α_3)	-0.02	0.04	0.08
	(0.03)	(0.05)	(0.05)
Tier effect ($-\alpha_1$-α_3)	0.06**	0.01	0.03
(F test statistic)	(9.86)	(0.05)	(0.59)
Number of decisions	1888	500	470
Number of cases	944	250	235

Standard errors are in parentheses. They are clustered at regional judge level

**, * and +: significant at 1%, 5% and 10% level respectively

Figure 1. Judicial appointments in arbitrazh courts

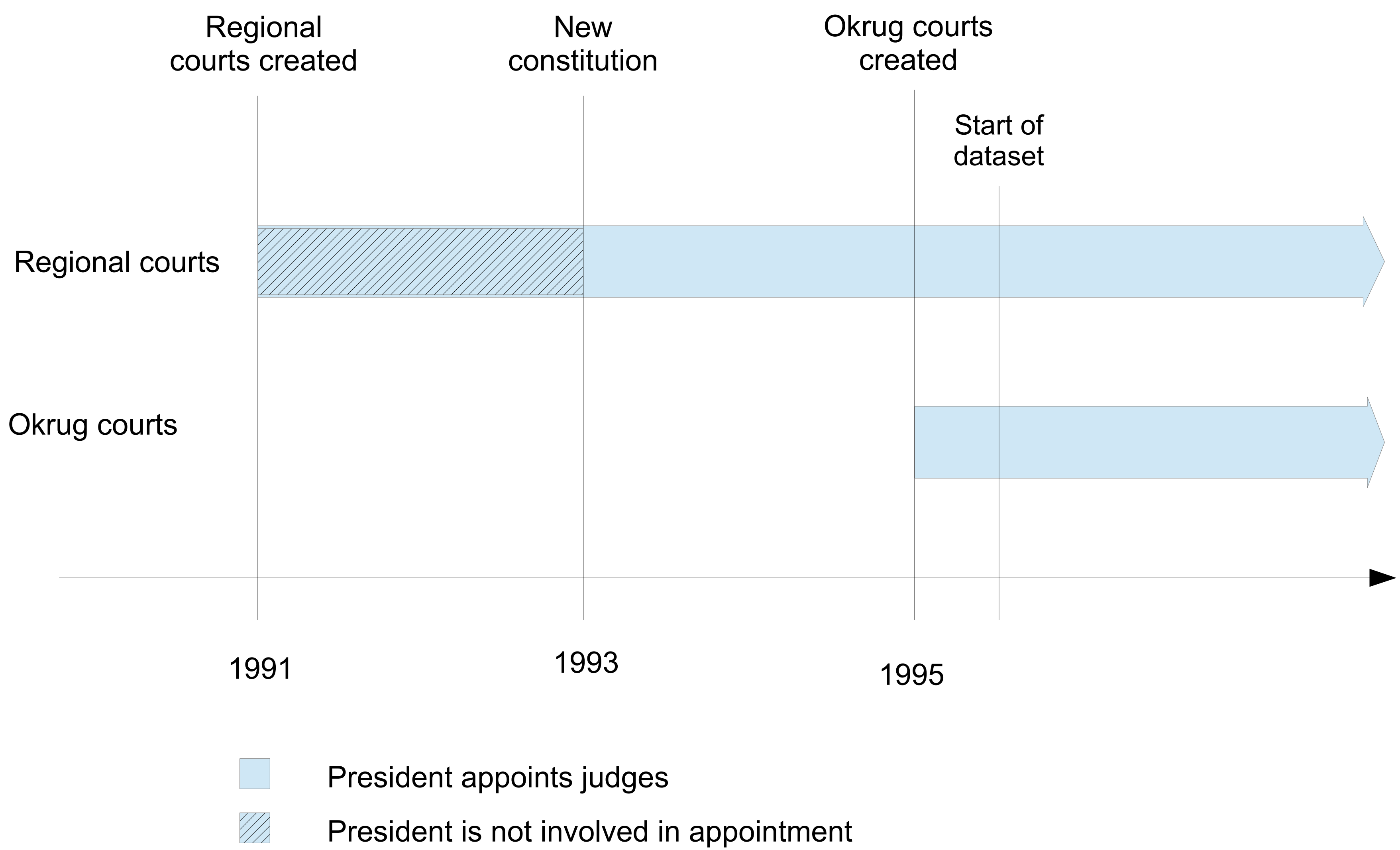

Figure 2. Difference-in-differences

Share of government's victories

	Regional court appointment		
Court tier	Presidential	Non-presidential	
Regional	α	β	α−β
Appellate	γ	δ	γ−δ
	γ−α	δ−β	α−β−(γ−δ)

Selection effect

Tier effect

Appointment effect

Figure 3. Share of government victories, by regional court