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Judicial Independence and Development: Evidence from Pakistan

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This paper provides plausibly causal evidence that Presidential appointment of judges considerably impacts judicial independence and decision quality in Pakistan. We find that when the judge selection procedure changed from Presidential appointment to appointment by peer judges, rulings in favor of the government decreased significantly and the quality of judicial decisions improved. The age structure of judges at the time of the reform and the mandatory retirement age law provide us with an exogenous source of variation in the implementation of the reform. We test for and provide evidence against potential threats to identification and alternative explanations for our findings. The analysis of mechanisms reveals that our results are explained by rulings in politically salient cases and by "patronage" judges who hold political office prior to their appointments. According to our estimates, judicial appointment by peer judges prevents land expropriations worth 0.14 percent of GDP every year. *(*JEL *D02, O17, K40)*.

Keywords: President, judges, property rights, patronage.

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"There is no liberty if the power of judging is not separated from the legislative and executive power."

[Montesquieu (1748) in *l'Esprit des Lois*]

"A judiciary's job is to interpret the law not to challenge the administration."

[President Ziaul Haq (1982) in *Amnesty International Report*]

1. INTRODUCTION

In many countries, including the United States, Brazil, Singapore and South Africa, it is the President who appoints judges to the superior courts. This seems counterintuitive to the principle of the "separation of powers" (Montesquieu, 1748). Yet it is argued that the separation of powers is ensured by removing the power of dismissal from the President, for instance via the institution of "life-time appointment" or retirement only at a set mandatory age (Madison, Hamilton, and Jay 1788; Hayek 1960; Buchanan 1974; La Porta et al., 2004).

In this paper, we provide causal evidence that Presidential appointment of judges exerts considerable influence on judicial decision-making. We study a 2010 change in the judge selection procedure in Pakistan, from a system of Presidential appointment (like the US or Brazil) to a judicial commission-based selection (like Sweden or the UK). We ask whether this judicial-selection reform affected judicial outcomes and, if so, which mechanisms link Presidential appointment of judges to judicial decision-making?

To systematically examine the influence of this reform on judicial decision-making, we randomly sample the universe of cases in Pakistan's District High Courts and obtain information on 7500 cases from 1986 to 2016.[1] Our measure of executive influence over the Judiciary is a judicial-dependence dummy variable "State Wins", taking value 1 for "State

[1]More information on sampling is provided in the data section, with further details in the data-construction section in Appendix B.3.

victories" and 0 for "State losses" in cases where the State is a party. Following the literature, we asked a Law firm to code this variable (as in Djankov et al., 2003; La Porta et al., 2008).[2]

The Pakistani government is a party in a wide range of judicial cases, from simple tax disputes to blasphemy, political victimization of opposition politicians, suppression of fundamental rights, and the constitutionality of Military Rule. Nevertheless, about 40% of all petitions filed in High Courts against the State concern land expropriation and ownership disputes with the government.[3] When the government expropriates land, courts are generally the only recourse for citizens seeking to recover their property (La Porta et al., 2008). On November 29th, 2017, a Court presided over by judicial commission appointees ordered the Karachi Development Authority to return 35,000 "public encroachments" to their owners (The News, 2017). Similar instances of land expropriation by government have been reported elsewhere in India, Ghana, and China (BBC, 2013; Gadugah, 2017).[4]

Figure 1 generalizes the anecdotal accounts of less-favorable rulings for the State following the 2010 reform to about 7500 cases. Prior to the selection reform, around 50% of cases were decided in favor of the State, as opposed to about 40% thereafter (Panel A). These differences are both qualitatively and statistically significant. A similar pattern emerges when we consider yearly variation: there is a sharp fall in State Wins following the judicial selection reform in 2010 (Panel B).[5] However, this cannot be interpreted as conclusive evidence for a causal link between the change in judicial-selection procedure and judicial outcomes, as a number of other changes occurred around the selection-reform year. For instance, the transition from military to democratic rule took place in 2008. Likewise, a social movement in 2007 by Pakistani lawyers demanded President Musharraf's resignation and in 2010 the President's power to unilaterally terminate the legislature was removed from the constitution. The overall fall in the proportion of rulings in favor of the government following the selection reform could

[2]Law firms coded 1 if the State 'won' and 0 otherwise. Typically, when the State wins, the judgment text contains phrases such as "*Case against the State is dismissed*" and when the State loses, "*Petition against the State is accepted*". We later report the correlation coefficient between two independent codings of the State Wins variable (more details can be found in the data section and Appendix).

[3]By government we mean all levels of the administration with executive authority (i.e. local, provincial and federal government, and public agencies, e.g. the various land-development authorities in Pakistan).

[4]Such cases abound, the most recent (high-profile) example in India being on February 8th 2018, when a land-grab case was brought against government agency head Giriraj Singh (Times of India, 2018). Now pending trial, Mr. Singh is accused of facilitating the illegal "land grab" of a scheduled class villager in the State of Bihar.

[5]Identical pattern of a sharp trend break in 2010 reform year is observed if we instead plot government victories by district. These results are presented in Figure C1 of Appendix C.

be explained by any of these changes.[6] We address this concern by focusing on plausibly random cross-sectional variation in the implementation of the reform due to the age composition of near retirees in the reform year.[7] Both before and after the reform, Pakistani law made retirement at age 62 mandatory for judges. However, different High Court benches had different numbers of vacancies arising from mandatory retirements in the 2010 reform year.[8] For instance, the property bench of district Islamabad had no vacancy arising from mandatory retirements in 2010, while the property bench of Peshawar district did. Essentially, our difference-in-differences framework compares pre- and post-reform government victories in district benches experiencing low versus high mandatory retirements in 2010.

Figure 2 shows our identifying variation and key explanatory variables. The judicial commission appointments are almost perfectly correlated with mandatory retirements. This is consistent with the fact that 91% of judges in Pakistan serve out their full term and only retire on their 62nd birthday.[9] Assuming that judges reach their 62nd birthday randomly across district benches, we can address the concern that our results are confounded with strategic judicial appointments or transfer of judges across district benches. Together with the random allocation of cases across judges, and Pakistani jurisdiction laws preventing litigants from choosing the districts where they file cases, this allows us to estimate the causal effect of selection reform on judicial outcomes (Ponticelli and Alencar, 2016). We present evidence consistent with mandatory retirements providing plausibly exogenous vacancies in district benches via a balance test, showing that mandatory retirements are uncorrelated with a long list of observable case and district characteristics.

We find that Presidential appointment of judges substantially affects judicial decisions: a 10% rise in judges selected by the judicial commission reduces State Wins by about 2

[6] Nor can we simply compare the pro-government rulings of judges appointed by the President and those appointed by judge peers, because unobserved judge characteristics may be correlated with both selection procedure and judicial decisions. For instance, a corrupt judge may be simultaneously more likely to be a Presidential appointee and rule in favor of the government. Therefore, we risk confounding the effect of selection procedure with omitted judge characteristics.

[7] This is similar to the identification employed in Martinez-Bravo et al (2017), who insightfully document the impact of differential exposure of Suharto mayors across Indonesian districts on public good provision and quality of governance.

[8] Benches or "specialized benches" in a given district are groups of district High Court judges appointed to oversee cases pertaining to their expertise. There are four main specialized benches in each district: property bench, criminal bench, tax bench, and writ or human rights bench, each with an average 7 judges. Importantly, when a vacancy on a specific district bench arises, it is always filled by a judge with the same expertise or specialty.

[9] The remaining 9% of judges are either promoted to the Supreme Court (3%) or die in office (6%). The almost perfect correlation between new judicial appointments and mandatory retirements applies both before and after the reform.

percentage points. This is equivalent to a 4% reduction over the sample mean. We present evidence, consistent with qualitative accounts, that this reduction in State Wins reflects more 'correct' and higher quality judicial decisions made: peer appointees are more likely to rule on case evidence and better follow due process than presidential appointees.

Three key threats to identification could still hinder causal interpretation of the selection reform's impact on judicial decision-making. First, we might be picking up a pure appointment effect. If, for instance, new appointments have an independent effect on judge behavior, then we might be picking up the effect of new judicial appointments instead of the change in the judicial selection procedure. A falsification test, however, strongly suggests this is unlikely: we show that mandatory retirements before selection reform, i.e. in 2007, 2008 or 2009, have no influence on government victories. Mandatory retirements in those years were also determined by judges' age structure and led to new judicial appointments but by the President. In these instances, new Presidential appointments are unpredictive of government victories.[10]

Second, the district benches with more vacancies arising from mandatory retirements in 2010 might have been on different trajectories before the reform. Therefore, we may be confounding the effect of the selection reform with differential prior trends among district benches with high versus low mandatory retirements. However, we find no evidence of such differential trends, consistent with qualitative accounts indicating that the selection reform was unanticipated and framed by a secret parliamentary committee (Almeida, 2018). Third, peer-appointed and President-appointed judges might adjudicate over different types of cases. However, our evidence is consistent with random allocation of cases: a balance test shows that both types of appointees adjudicate over similar cases.[11]

We test for and reject alternative explanations for the finding that judge-selection reform changed judicial decision-making in Pakistan. We show that the effect of selection reform is not a President- or Chief Justice-specific effect. We also conduct a number of additional sensitivity tests showing that the results cannot be explained by strategic filing of cases or cases adjudicated in specific districts. The results are equally robust to non-linear estimations, alternative specifications, and different levels of clustering, including the wild bootstrap method due to Cameron et al. (2008) for small number of clusters.

[10] We also show that age and tenure of judges is uncorrelated with rulings in favor of the State: judges are equally likely to rule in favor of the government whatever their age or experience.
[11] Pakistani High Court cases are randomly assigned subject to a capacity or "workload" constraint.

We next explore the mechanisms, starting with the type of cases behind these results. These turn out to be politically salient *constitutional* cases involving land and human-rights disputes with the State. These are cases where government expropriation of valuable tangible resources (such as land) and intangible resources (such as political rights) is at stake. We carry out a placebo test to examine this political-influence mechanism using criminal cases, which also involve the State as Prosecutor. We find no effect of judge-selection reform on State Wins in run-of-the-mill criminal cases.[12]

While the economic value of decreased pro-government rulings in human rights cases is difficult to assess, a straightforward back-of-the-envelope calculation provides the value of total land expropriations avoided due to the selection reform. Based on judgment order valuations of expropriated property and our point estimates, we calculate that the selection reform prevents land expropriations worth 0.14% of GDP, or USD 390 million yearly. In other words, government would continue to expropriate additional USD 390 million-worth of land every year if all judges were still Presidential appointees. To put this amount into perspective, it is equivalent to roughly the entire federal government's expenditure on health care in 2016.

We also examine the type of judges driving the results. We find that President-appointed and peer-appointed judges are similar in many characteristics such as age, religion, gender, and experience. Nevertheless, judicial-commission appointees are about 35% less likely to have run for political office prior to their appointment. This is consistent with Presidential selection favoring more political or patronage judges. Our confidence, that the selection of different types of judges is key to understand our results, is increased when we find no evidence for spillover or incentive effects: peer-appointed judges do not impact the behavior of President-appointed judges.

This paper relates to several strands of literature. First, it speaks to the recent and vibrant literature on bureaucracies in developing countries, especially in weakly institutionalized settings (Acemoglu et al., 2020; Bandiera et al., 2020; Callen et al., 2020). [13] Our contribution lies in demonstrating how appointment by judge peers can increase judicial independence even in a weakly institutionalized setting. Very few studies have investigated the Judiciary in developing countries; our work provides insights into how judicial independence can be

[12]State Wins in this case is the conviction rate. Examination of a random sample of 100 criminal cases from our sample reveals that these are plausibly politically less salient, most cases involve bail pleas for theft and burglary (the categorization of the alleged crimes in these 100 cases is available on request). We, however, further disaggregate the criminal cases judged under Islamic vs Secular Law and find similar null effects of the reform.
[13]Other related works include Jones and Olken (2005), Lim (2013), Hessami (2018) and Ash and MacLeod (2019).

fostered in a country where democratic institutions are weak to begin with. Second, the paper speaks to the literature on patronage in bureaucracies: in British colonial civil service (Xu, 2018), among mayors and local bureaucrats in China (Jiang, 2018) and within public sector organizations in Brazil (Colonelli et al., 2020). We add to this literature by highlighting Presidential-appointment behind patronage in the Judiciary, a key organ of the State, and how removing Presidential appointment of judges may promote the rule of law. Third, we contribute to the extensive cross-country literature on courts (Djankov et al., 2003; La Porta el a., 2004; Voigt, 2008; Palumba et al., 2013; Boehm, 2015; Bielen et al., 2018; Chemin, 2020). By drawing on variation across districts subject to the same national institutions, we overcome many of the common identification issues arising in work exploring differences between countries.[14] Last, our work is also related to the literature on judge behavior. Most of this literature has focused on judge behavior in criminal cases (Chalfin and McCrary, 2017; Cohen and Yang, 2019), racial bias in criminal sentencing (Alesina and La Ferrara, 2014; Rehavi and Starr, 2014; Arnold et al., 2018), and extraneous factors affecting judge sentencing such as lunch breaks (Danziger et al., 2011), terrorism (Shayo and Zussman, 2011) and temperature (Heyes et al., 2019). We here reveal a political-selection mechanism: judge behavior in politically salient cases is affected by the way in which judges are selected.

The remainder of the paper is organized as follows. Section II provides background on the judicial structure of Pakistan and the selection reform. Section III presents the data, their sources, and descriptive statistics. Section IV describes the empirical methodology. Section V presents and discusses the main results, and Section VI explores the mechanisms behind them. Section VII rules out alternative explanations and details a battery of robustness checks. Section VIII concludes. Further information on data construction, variable descriptions, and additional robustness checks is in the Appendices.

2. BACKGROUND

2.1 *Judicial Structure in Pakistan*

The judicial system in Pakistan is a three-tier hierarchical structure. At the lowest level are the civil and session courts hearing civil and criminal cases respectively, whose rulings can be challenged in the District High Courts. In these High Courts an individual can file a case against

[14]Ponticelli and Alencar (2016) and Chemin (2018) are notable exceptions that exploit within-country variation to study the Courts.

the government in the form of a constitutional petition against the State. Cases with the State as respondent involve the federal government, provincial governments, local governments, government agencies or any organ of the State with executive authority (such as the office of the President or the Prime Minister).

From 1986 to 2016, about 70% of all cases filed in the High Courts were "constitutional petitions", and the majority of these involved government housing agencies responding to land-dispute claims from the public.[15] If the government expropriates land or violates a fundamental right, the High Court is the first, and in most cases the only, platform offering remediation to individuals and firms. There are 16 District High Courts in Pakistan; Figure 3 shows their locations and respective jurisdictions.[16] Judges serve on one of 4 specialized benches within each district: a property bench ruling on land or property disputes with the government, a tax bench for tax disputes, a writ bench for human rights petitions, and a criminal bench for criminal cases. When a vacancy on a specific district bench arises, it is always filled by a judge with the same expertise or specialty. Last is the final appellate Court, the Supreme Court of Pakistan, located in the federal capital. This typically hears criminal and constitutional appeals from the High Courts. The Supreme Court can have at most 16 judges, which greatly limits the number and scope of its cases; only a small fraction of cases ends up being heard by the Supreme Court (Haq, 2018).

2.2 *Judicial Selection Reform*

In April 2010, the ruling Pakistan People's Party tabled a constitutional amendment before the Pakistani Parliament that would dramatically change the process of judicial appointment. This Eighteenth Amendment to Pakistan's constitution was passed by Parliament on April 15th 2010 and signed into Law by the President on April 19th 2010, when it came into effect (Tavernise and Masood, 2010). It removed the following clause from the constitution:

"*The Chief Justice and each of other Judges of a High Court shall be appointed by the President in accordance with Article 175A*".

This was replaced by:

"*There shall be a Judicial Commission of Pakistan, for appointment of Judges of the Supreme*

[15]The remaining 30% of cases were criminal appeals from the session court.

[16]Although, in theory there are four Provincial High Courts in Pakistan, in practice each of Pakistan's four provinces contains about four District or "Divisional" High Courts.

Court, High Courts and the Federal Shariat Court. The Commission by majority of its total-membership shall nominate for each vacancy of a Judge in the Supreme Court, a High Court or the Federal Shariat Court, as the case may be" (Constitution of Pakistan, 2010). [17]
The judicial commission consists of the "*Chief Justice of Supreme Court and 4 senior most judges, a former judge (nominated by the Chief Justice of Pakistan), federal law minister, and the attorney general of Pakistan, along with a senior advocate of Supreme Court nominated by the Pakistan Bar Council for two years.*" (Constitution of Pakistan, 2010; 2017).[18]

From April 2010, when the amendment was enacted, Supreme and High Court judges were appointed by a judicial commission (consisting of peer judges and senior lawyers), with no Presidential involvement. Many accounts suggest that the appointment power of the executive was severely curtailed by this reform, as judges constitute the overwhelming majority (6/9) of the commission (Zafar, 2012; Sattar, 2012).[19] We interpret this shift from the Presidential appointment of judges to their selection by a judicial commission as a *de jure* reduction in executive control over the judiciary, and evaluate its impact on judicial independence and decision quality.

2.3 *Political Economy of the Selection Reform*

Why would politicians willingly implement a judicial reform that entailed loss of political power over judicial appointments? According to many political observers, the judicial-selection reform introduced after a decade of military rule was intended to reduce the political power of the military. Pakistan's military leaders had long ruled as Presidents and used the Judiciary to obtain "constitutional indemnity" for their military coups. It was hoped that independent judges would uphold constitutional clauses barring military take-overs, thus shielding the country against "extra-constitutional" military takeovers.[20] Therefore, Pakistani

[17]Furthermore, Article 209 of the Constitution stipulates that judges can only be removed by filing a reference to their peers; this was left unchanged by the reform (Constitution of Pakistan, 2017).

[18]For the appointment of High Court judges, the case explored here, all of the above members plus the provincial Chief Justice, provincial Law Minister, the most senior judge of the provincial High Court, and a lawyer nominated for two years by the provincial Bar Council sit on the judicial commission.

[19]The 18th amendment also created a Parliamentary Committee consisting of four members from the treasury and four from the opposition. Nominations by the judicial commission have to be confirmed by this committee, although its effective power is limited since the Judicial Commission can overrule Parliamentary Committee objections. This was not in the original 18th amendment but was incorporated on December 20th 2010 as the 19th Amendment, which 1) increased the number of judges in the judicial commission (judges now have the "super majority" of 8/11 in the Judicial commission as opposed to 6/9 under the 18th amendment) and 2) stated that the Judicial Commission now also had the power to overrule Parliamentary Committees' objections to appointments (Constitution of Pakistan, 2010).

[20] Article 6 of the Constitution of Pakistan states military take-over is "high treason punishable by death".

politicians hoped to take shelter behind "constitutional protections" from military take-overs by ensuring that judges were more independent, even though this reduced their effective control over the judiciary (Siddique, 2013). Moreover, it was hoped that reducing Presidential discretion over selection of judges would prevent abuses of power by future military rulers using the courts to imprison opposition politicians and violate fundamental rights (Sattar, 2012; Zafar, 2012).[21]

The possibility of sabotage by Pakistan's politically powerful military also led to the reform being conceived and debated in complete secrecy. As one commentator observed "*It was debated and created in total secrecy by a small parliamentary committee*" (Almeida, 2018). This covertness meant that the selection reform came as a sudden and unanticipated shock to the judicial dynamics of Pakistan. Further discussion of the reform, the political context, the structure and the history of the Courts in Pakistan can be found in Appendix B.

3. DATA

Our empirical analysis uses data on judicial cases from the central repository of cases in Pakistan, used by Lawyers to prepare their cases. We randomly sampled 7500 cases from 1986-2016 for all 16 District High Courts (from the universe of all cases decided in this period) and matched the details from these cases with judge characteristics from judicial administrative data and district characteristics from the census records.[22] We successfully matched this information for 7439 cases out of the 7500.[23] Table 1 shows the descriptive statistics of the variables used in the study, and the key outcome and explanatory variables are detailed below. Further information on the variables, their sources, sampling, and data construction can be found in Appendices A and B.

Outcome Variables. — The key outcome variable is State Wins. This is a case-level measure of judicial independence constructed from the text of the judgment orders containing details of the case. Following the literature (e.g. Djankov et al., 2003 and La Porta et al., 2008), we asked

[21]The 18th Amendment also aimed to increase provincial autonomy and weaken the overall power of the President: for instance, it also took away the President's power to unilaterally dismiss Parliament.

[22]Further information on the sampling procedure and data construction can be found in Appendix B.3.

[23]The remaining 61 cases could not be matched because the poor image quality of the judgment order text prevented us from ascertaining the name of the judge. Moreover, a case can be decided by a group of judges as well as a "single-member bench" where one judge decides alone. The average number of judges on a case is 1.8 (as shown in Table 1, Panel A).

a Law firm to code this variable. Two independent teams coded the "State Wins" dummy variable as 1 if the State as a litigant won a dispute and zero otherwise.[24] The State here includes all organs of the state yielding executive power, such as local, provincial, and federal governments, the Office of the Prime Minister, the Office of the President, and governmental agencies (in line with the conceptualizations of the State as an executive organ in Montesquieu, 1748).

For the analysis of the quality or "correctness" of judicial decisions, we use two additional outcome variables: *Merit* and *Process Followed*, where the unit of observation is also at the case level. These two variables are also constructed from information in the judgment orders. Merit is a dummy variable that takes the value of 1 if the decision is "based on evidence or case merits" and zero if it is based on a technicality. There are two reasons for constructing this variable. First, legal scholarship in Pakistan argues that ruling on technicalities is a "weapon of choice to rule unfairly" and that judges use decisions on technicalities to "favor the State authorities" (Aziz, 2001) and such rulings are "symptomatic of a biased decision" (Arshad, 2017). Therefore, we proxy the "correctness" or unbiased nature of a judicial decision by this dummy variable. Second, this variable is consistent with Common Law jurisprudence, which aspires toward rulings on merits, i.e. based on evidence and the spirit of the Law rather than legal technicalities, as an ideal (Pound, 1963, and Tidmarsh, 2009 discuss this in detail). Process Followed is a discrete variable representing a rating for each judicial case. Specifically, the law firm was asked to rate on a scale of 1 to 5 the extent to which "all relevant jurisdictional, procedural, and evidential requirements were followed in reaching the judicial decision". This variable approximates the "correctness" of the due process followed in reaching the judicial decision.[25]

Main Explanatory Variables. — The key explanatory variable used in the analysis, $\frac{\text{Mandatory Retirements in 2010}}{\text{Total Judges}}$ X Post 2010 is the fraction of judges reaching their mandatory retirement age in 2010 in a given district bench interacted with a post-reform dummy. Data on retirements, total judges, and other judge characteristics come from judicial administrative

[24]We show that the results are robust to using data from either of the teams. Further details on the construction of the variables coded by the two teams can be found in Appendix B.3.
Typically, when the dummy for State Wins is 1, the judgment contains markers such as "case against the State is accepted" and when 0, markers such as "case against the State is dismissed". A textual analysis of cases containing these precise phrases confirms the main results.
[25]Two independent teams coded each of these outcome variables and the correlation coefficient between them appears in Table C1 in Appendix C. Both variables were chosen after extensive discussions with legal experts, lawyers and former judges in Pakistan.

records obtained from the Registrar Offices of the District High Courts and High Court Annual Reports submitted to the Ministry of Justice, Government of Pakistan. These two sources are also used to construct $\frac{\text{Appointments in 2010}}{\text{Total Judges}}$ X Post 2010. This is the fraction of new judicial commission appointments in each district bench for 2010, the reform year, interacted with a post-reform dummy. In 2010, about 12% of judges reached their mandatory retirement age and 10% of these vacancies were filled by peer-appointed judges (see Table 1, Panel B). Since there is almost perfect correlation between mandatory retirements and new judicial appointments (both before and after the reform), we report as baseline the results with mandatory retirements.[26] Importantly, when a vacancy on a specific district bench arises, it is always filled by a judge with the same expertise or specialty. This is reflected in Figure 2 where we observe mandatory retirements and new judicial commission appointments are almost perfectly correlated within district-benches. Moreover, the fraction of retirements is always larger than the fraction of judicial-commission appointments. This is because not all mandatory retirements are always accompanied by new appointments in the same year, although the correlation is close to 1.[27] This means that new appointments instrumented with mandatory retirements will give us the local average treatment effect for those district benches where a judicial-commission appointment and a mandatory retirement occurred in the same year.[28] Although, given the high correlation, the reduced form or *Intention-to-Treat* estimate is by construction close to 2SLS estimate.

Controls: Case, Judge, and District Characteristics. — We rely on a combination of judgment texts, judicial administrative data, bar association and census records to construct the case, judge, and district characteristics that we use as control variables. The case-characteristics data, like the outcome variables, are obtained from the judgment order texts. They include district where the case was heard, year when the case was filed, decision year, full name of the judge(s) adjudicating on the case, number of lawyers and judges, type of case, a dummy for land disputes with the government (land cases or "Eminent Domain" cases) and so on. Table C1 in Appendix C lists the means of the outcome variables, the case characteristics, and the

[26] The correlation coefficient between the two variables is 0.935 and F-statistic in first-stage is above 400. Therefore, it makes little difference between using mandatory retirements and new judicial appointments.

[27] 91% of judges in Pakistan serve out their full term and only retire on their 62nd birthday, while the remaining 9% either die in office (6%) or are promoted to the Supreme Court (3%).

[28] The compliers here would be those district benches where there are no strategic appointments or no transfers of judges across district benches and where vacancies arising from mandatory retirement were filled with judicial commission appointees in the same year.

corresponding correlation coefficients between these variables across the two teams of attorneys who coded them.[29] Judge characteristics are obtained from the judicial administrative records of the Registrar Offices of the High Courts of Pakistan and Annual Reports submitted to Ministry of Justice, Pakistan (Table 1, Panel C). This includes information on judges' gender, religion, promotion, and previous employment. Holding office in the Bar Association and running for political office prior to judicial appointment are ascertained from biographical information in the judicial administrative records, annual reports by District High Courts to the Ministry of Justice, and bar association records. District characteristics are obtained from population and agriculture censuses that we digitize. Combining the data from these sources gives us information on 7439 cases and 482 judges across 64 district High Court benches of Pakistan.

4. EMPIRICAL METHOD

We use district bench variation in mandatory retirements in reform year 2010, interacted with a post-reform dummy to estimate the effect of judicial selection reform on judicial outcomes at the case level. Our main specification is the following:

$$Y_{cjdbt} = \theta + \alpha \left(\frac{\text{Mandatory Retirements in 2010}}{\text{Total Judges}}_{db} \text{X Post 2010}_t \right)_{dbt} + \beta_{db} + \gamma_t + \boldsymbol{W}'_{cjdbt}\boldsymbol{\varphi} + \varepsilon_{cjdbt} \quad (1)$$

The subscripts *c*, *j*, *d, b* and *t* index cases, judges, districts, benches, and years respectively.[30] *Y* denotes State Wins and $\frac{\text{Mandatory Retirements in 2010}}{\text{Total Judges}}$ X Post 2010 is the fraction of judges on a given district bench reaching their mandatory retirement age in 2010, interacted with a post-reform dummy. This *Intention-To-Treat* effect can be interpreted as the effect of judicial commission appointments, since there is almost perfect correlation between mandatory

[29]The results are robust to using data from either team of coders; further information can be found in the discussion in Appendix B.3.

[30] *t* corresponds to the year of case decision. The use of decision year as opposed to case-filing year simplifies the interpretation of the coefficient estimates.

retirements and new judicial appointments.[31] β_{db} and γ_t are district-by-bench and year fixed effects respectively, and $\boldsymbol{W}'_{cdbt}$ is a vector of case and district controls as shown in Table 1.[32]

This empirical methodology is inspired by Martinez-Bravo et al. (2017). State Wins pre- and post-reform are compared in district benches with high versus low mandatory retirements in 2010. Since our identifying variation comes from 64 district-bench clusters (16 districts x 4 benches each), we cluster standard errors at the district-by-bench level. The 64 clusters exceed the rule of thumb of 42 clusters (given in Angrist and Pischke, 2008, p. 219) where inference by asymptotic theory may be considered valid. Nevertheless, the results are robust to clustering by wild bootstrap for small numbers of clusters (as suggested in Cameron et al., 2008) and clustering within each district bench separately pre- and post-reform (as suggested in Bertrand et al., 2004).

The main identifying assumption is that mandatory retirements across district benches are as good as randomly assigned, conditional on controls—that is, exogenous to underlying factors that could have affected judicial decisions. We find this assumption plausible for several reasons. First, the number of mandatory retirements on judges' 62nd birthday is determined by the age structure of near-retirees and is predetermined with judges' appointment. Second, anecdotal accounts suggest that the selection reform was unexpected and unrelated to specific district benches' dynamics (Zafar, 2012; Almeida, 2018). These two factors mean that the vacancies arising from mandatory retirements across district benches are likely to be uncorrelated with potential case and district outcomes.

The results section of the paper provides empirical evidence supporting our identification assumption. First, we show that mandatory retirements post-reform are uncorrelated with a large number of case and district characteristics. Second, we show that pre-reform mandatory retirements, also leading to new judicial appointments, but that were still being made by the President, have no effect on government victories. Finally, we provide evidence against differential pre-trends and show that State Wins in district benches that had

[31] In Table C2 of Appendix C we also present results with $\frac{\text{Appointments in 2010}}{\text{Total Judges}} \text{X Post 2010}$ estimated by OLS and 2SLS (where we instrument appointments with mandatory retirements). The results are similar given the strong correlation between mandatory retirements and new judicial appointments. The F-statistic for the first-stage is above 400 (as can be seen from Panel B of Table C2).

[32] The case controls include the number of Lawyers and Judges in the case, the presence of the Court Chief Justice on the bench, and the district characteristics (e.g. population), for a full list see Table 1, panels A and C. Note that we do not control for judge characteristics, as these may be correlated with the reform (this possibility is discussed in more detail in the Mechanisms section). However, case-type fixed effects are included in the list of controls, i.e. we consider case-type fixed effects (dummies for case types) as part of the case characteristics controls in $\boldsymbol{W}'_{cdbt}$.

more mandatory retirements are not on differential trajectories prior to the implementation of the selection reform. This strongly suggests that the pattern of judicial appointments due to mandatory retirements and judicial behavior did not change in the years leading to the selection reform, and it is consistent with the reform's unexpected introduction.

5. MAIN RESULTS

5.1. *Effect of judicial selection reform on State Wins*

Table 2 (column 1 and 2) presents the estimated effect of the judicial selection reform on State victories: there is strong and robust evidence of a substantial negative effect. The first column corresponds to the most basic specification, with only district bench and year fixed effects. The estimate implies that if 10% of the judges retired in 2010, State Wins would be about 1.5 percentage points lower post-reform. The second column adds all the available case and district characteristics (presented in Table 1) and estimates our main specification (equation 1). The estimates imply that if 10% of the judges retired in 2010, State Wins would fall by about 2 percentage points post-reform. This is equivalent to a 4% decrease over the sample mean.

In Table C2 of Appendix C, instead of using fraction of mandatory retirements, we use fraction of judicial-commission appointments in 2010 interacted with the post-reform dummy. We obtain similar results when we use appointments in 2010 interacted with the post-reform dummy (column 1 and 2) or when we instrument appointments with mandatory retirements (column 3 and 4). A 10% increase in judicial-commission appointments reduces State Wins by about 1.5-2.5 percentage points (equivalent to a 3-5% decrease over the sample mean).

Ex-ante, however, reassignment of judges across district benches or strategic appointments of new judges could bias the OLS estimates of appointments on State Wins. For example, in a hypothetical scenario where new judicial-commission appointments occur in the most independent districts, the OLS estimates would underestimate the true effect of the selection reform. This is because a judge randomly assigned to a district bench is more likely to decrease State Wins than a judge assigned to an independent district bench where most cases are "fair" to begin with. Nevertheless, as we observe in Table C2 of Appendix C, using appointments relative to retirements makes little difference, since new judicial appointments and mandatory retirements are almost perfectly correlated. The F-statistic for the first stage is

above 400 and the correlation coefficient between the two variables is 0.935. If anything, the coefficient estimates with appointments are slightly larger.[33]

These results are also consistent with qualitative accounts suggesting that the selection reform affected judicial independence from the executive branch in Pakistan. For instance, a bench with four out of its five judges selected by the judicial commission ruled that the incumbent Prime Minister Nawaz Sharif be removed from office for "undeclared assets" and "living beyond means" (Reuters, 2017). The fact that the Prime Minister was a business tycoon and the leader of a party with a two-thirds majority in Parliament makes the judgment all the more salient politically. Similarly, in another judgment where all three judges were peer-appointed, the Islamabad High Court's bench unanimously ruled that the Foreign Minister be removed from office for having "deliberately and willfully not disclosed his status as an employee of the foreign company, nor receiving of the salary per month". This is in stark contrast to Presidential appointees' rulings involving individuals holding executive office. For example, a bench of President-appointed judges ruled that the "President may, in the larger public interest, perform all legislative measures which are in accordance with, or could have been made under the Constitution, including the power to amend it".

More often, however, cases with the government as a litigant concern land disputes with State agencies expropriating land and human rights abuses (Gulf News, 2009). For instance, in a case where the "Grievance of plaintiff was that despite being the owner of the house, the authorities had taken custody of the property", a peer-appointed judge ruled that the government had "committed deliberate and willful breach of the right to private property" and ordered the government to "return the property to the rightful owner and pay damages" (Altaf Hussain vs. The State, CLC, 2013, p. 284). This contrasts with decisions in many such cases prior to the reform. For example, in a case where the bench consisted of Presidential appointees, a similar "petition was dismissed" on a technicality (Khalid Mohsin vs. The State, CLC, 2005, p. 745). Overall, these qualitative accounts and the results presented in Table 2 and Table C2 paint a consistent picture of selection reform decreasing State Wins.

5.2. *Threats to Identification*

We now examine three key threats to identification that could undermine the causal interpretation of these estimates. First, our selection-reform estimate might reflect a pure

[33] We report the rather conservative estimates based on mandatory retirements as our baseline throughout the paper, therefore, our main specification (1) provides a lower bound effect of selection reform on State Wins.

appointment effect. It is possible, for instance, that we are confounding the effect of selection reform with differences in behavior towards the State between old and new judges. Table 2 (columns 3, 4 and 5) presents evidence against this hypothesis by showing that pre-reform mandatory retirement had no effect on rulings in favor of the government. That is, the age distribution of judges across district-benches in the years 2007, 2008 or 2009, when all new judicial appointments were still by the President, does not reduce State Wins post-reform. If anything, the coefficient estimates in all these instances are positive. This suggests that if there is a pure appointment effect, it is likely to be small and perhaps go in the opposite direction.[34] The findings of a negative effect of reform year mandatory retirements and no effect of pre-reform mandatory retirements on State Wins are robust to different specifications (such as combining all pre-treatment and treatment variables in a single regression). These results are reported in Table C3 of Appendix C.[35]

A second threat to identification might come from confounding the effect of the reform with diverging trends prior to the reform. District benches with more vacancies arising from mandatory retirements could already have been on different trajectories *before* the reform. Therefore, to systematically examine pre-trends, we estimate the following specification:

$$Y_{cjdbt} = \beta_0 + \sum_{s=2007}^{2016} \alpha_s \left(\frac{\text{Mandatory Retirements in 2010}_{\text{db}} \text{ X } \delta_s}{\text{Total Judges}_{\text{db}}}\right)_{dbt} + \beta_{db} + \gamma_t + \boldsymbol{W}'_{cjdbt}\boldsymbol{\varphi} + \varepsilon_{cjdbt} \quad (2)$$

where Y is State Wins, δ_s is a dummy variable that takes the value of one in the year s and $\frac{\text{Mandatory Retirements in 2010}}{\text{Total Judges}}$ is the fraction of judges reaching their mandatory retirement age of 62 in a given district bench in reform year 2010. Fixed effects and controls are identical to the main specification (1).

Equation (2) is essentially identical to the specification used to test for pre-trends in Martinez-Bravo (2017) and it allows us to transparently assess systematic differences in district benches prior to the selection reform. Figure 4 (and its corresponding table-form representation in Table C4 of Appendix C) present results from estimating equation (2). Reassuringly, we find that district benches with higher numbers of mandatory retirements in 2010 show no change in State Wins prior to 2010, although there is a permanent decrease in State Wins post-2010. This

[34]Although, we should be careful not to read too much into these positive point estimates given the statistical insignificance.
[35]Figure C2 in Appendix C we also show that age and experience of the judge are uncorrelated with government victories: judges of different ages and tenure display similar behavior toward the State.

is also consistent with anecdotal accounts that the selection reform was an unanticipated shock to the judicial system of Pakistan (Zafar, 2012; Almeida, 2018).

Finally, it might be that cases adjudicated in district benches with more mandatory retirements differ from cases adjudicated in district benches with less mandatory retirements, or that more mandatory retirements systematically occur in districts that are different to begin with. This is unlikely, since the mandatory retirements across district benches is determined by the age structure of near-retirees, which is plausibly randomly distributed at the time of the reform. However, in Table 3 we provide evidence supporting this argument: the fraction of mandatory retirements in 2010 interacted with the post-reform dummy is uncorrelated with a long list of case and district characteristics. This check for balance test strongly suggests that we are unlikely to be confounding the effect of the reform with differential case or district characteristics.

5.3. *Does lower State Wins imply more "correct" judicial decisions?*

To assess whether the fall in State Wins implies more "correct" judicial decisions, we constructed a variable that takes the value of 1 if the decision is "based on evidence or case merits" and 0 if it is ruled on a technicality. This variable, also coded by the law firm, follows the argument by Pakistani legal scholars that ruling on technicalities is a "weapon of choice to rule unfairly" (Aziz, 2001) and "symptomatic of a biased decision" (Arshad, 2017).[36] Column 2 of Table 4 reports the results of estimating equation (1) with Merit as the dependent variable. The estimates indicate that the selection reform increased "correct" rulings or decisions based on evidence. They imply that if 10% of judges retired from their respective district benches in 2010, Merit decisions would increase by about 2 percentage points post-reform (Table 4, column 2). This is a 3.2% increase over the sample mean.

To ascertain whether the fall in State Wins also coincides with better observance of "due process", we leverage legal experts' ratings for each judicial case on observation of "relevant jurisdictional, procedural, and evidential requirements". This proxies for the "correctness" of the legal process followed in reaching the judicial decision. A higher rating on Process Followed implies that higher jurisdictional, procedural, and evidential standards were met in the judicial decision. Estimating equation (1) with this dependent variable, we find

[36]Discussions with legal experts, senior lawyers and former judges in Pakistan motivated the use of the Merit variable, which these experts suggested would approximate "correct" or "unbiased" judicial decisions.

that if 10% of the judges retired from their respective district benches in 2010, the Process Followed rating would increase by about 0.04 points (Table 4, column 4). This is equivalent to a 1.2% increase over the sample mean. Taken together, the qualitative accounts and the results from Table 4 both indicate that the reduction in rulings in favor of the government under peer-appointed judges reflects more "correct" judicial decisions and better observance of due process.

6. MECHANISMS

This section is organized into three short subsections. First, we discuss the type of cases driving the results. Second, we document the type of judges driving the results and assess whether there are spillover effects from peer-appointed judges on President-appointed judges. Last, we describe our back-of-the-envelope calculation that computes the total value of land expropriations avoided every year due to the selection reform.

6.1. *Type of cases driving the results*

We begin our investigation of the mechanisms by discussing the type of cases driving the results. We find evidence that the judicial-selection reform particularly affected politically salient "constitutional cases". These are cases involving land expropriation and human rights abuses by the State. Columns 1 and 2 in Table 5 present these results: a 10% rise in judicial-commission appointees reduces State Wins by about 1.5-2 percentage points in constitutional disputes with the State.[37]

This reduction in government victories in constitutional cases is consistent with widespread qualitative accounts. For instance, expropriation of private property by government housing agencies (such as by the Peshawar Development Authority, the Lahore Development Authority, the Karachi Development Authority, and the Capital Development Authority) is considered a major problem in Pakistan. Rulings in these ownership or expropriation disputes with the government are reported to be heavily influenced by political considerations (Gulf News, 2009; Abbasi, 2017; Sattar, 2017).

Some legal scholars in Pakistan go as far as to argue that land disputes involving the State are instances where the government is almost always in the wrong. For instance, "*when you see (government) housing agency involved in a land case, you know that justice is dead*"

[37]For further disaggregation of constitutional cases into land disputes and human rights cases, see Table C5 in Appendix C (Panel A). Focusing on these subsamples naturally reduces statistical power. However, coefficient effect sizes and direction of effects is similar to the baseline combined effect.

(Sheikh, 2016) or "*these housing development authorities is a mafia that operates with the full support of the highest level of the government … some judges are part of it too.*" (Arshad, 2017).

Likewise, human-rights cases involving the State are also considered highly political in nature. These constitutional cases are separately marked as "writ petitions". These cases involve the violation of fundamental rights such as freedom of movement or discrimination based on gender, political affiliation, and caste.[38] Typical examples include an individual claiming that his fundamental right to freedom of movement within and outside Pakistan has been restricted by the government since he joined the opposition political party, or an opposition leader pleading his citizenship was cancelled a day before he was to lead a protest against the government.[39]

Further evidence for this political influence channel comes from a falsification test. As criminal cases also involve the State (as prosecutor), but are politically less salient, we examine the impact of the selection reform on State Wins (or conviction rates) in criminal cases. These criminal cases mostly involve petty crime, vandalism, minor fraud, theft, and burglary, hence are low-stake politically. Results from Table 5 (column 3 and 4) show that the selection reform has no effect on State Wins in criminal cases, and the point estimates are in fact positive. This suggests that judicial-commission appointees do not rule against the government more than Presidential appointees in politically less-salient criminal cases.[40] These results suggest a political influence channel, with peer-appointed judges making fewer rulings in the government's favor in politically salient cases.

[38]Worldwide, and throughout recent history, many important petitions have been brought against the State and involve violations of human rights. See, for example, the cases challenging the apartheid government in South Africa, *The State vs Nelson Mandela* (1963) and the bus segregation of African-Americans in the United States (*The State vs Martin Luther King*, 1956), or the case invalidating laws prohibiting interracial marriages (*Mildred Loving vs. The State*, 1963). The analysis of executive constraints and judicial dependence in this context has "*obvious value for securing … political rights when the government is itself a litigant*" La Porta et al. (2004, p. 447).

[39] *Khalid Langrov vs. The State*, PLD 2007 and Naseer (2018).

[40]To verify that criminal cases in the High Courts are indeed politically low-stake cases, we randomly examined 100 criminal cases from our sample and found them politically less salient: most cases involved petty crime such as bail pleas for theft, minor fraud, vandalism, and burglary. Nevertheless, we did distinguish criminal cases judged under "Islamic Law" i.e. judged under the Hudood Ordinance, from those under secular British common law. The results are similar: peer-appointed judges are no more likely to convict for violations of Islamic law than Presidential appointees. These results are presented in Table C5 (panel B) of Appendix C.

6.2. *Incentives or Selection?*

This subsection answers two questions. One, is there any evidence of change in the incentives of Presidential appointees with the arrival of peer-appointed judges? Two, are there any differences in the observed characteristics of judges selected under the two different selection regimes?

First, to examine whether peer-appointed judges' arrival induces spillover or peer effects on Presidential appointees, we rerun our main specification (equation 1) but limit our sample to the latter. That is, we compare State Wins in district benches with more mandatory retirements in 2010 with those from district benches with fewer mandatory retirements pre- and post-2010 *but only* for cases where the judges were President appointed. Since these are the very district benches where peer-appointed judges are most likely to be appointed, given the almost perfect correlation with new appointments, we can assess how President-appointees react to the arrival of judicial-commission appointees. Table 6 reports these results. Estimates from (column 1 to 4) paint a consistent picture of no significant change in the behavior of old-regime judges with arrival of peer-appointed judges. These results suggest that incentive or peer effects are not likely to be large (if present at all).[41]

If spillovers or peer effects are limited, then what explains the effect of selection reform on government victories? It turns out that selection effects or judge-heterogeneity is key to understand the effect of selection reform on government victories. We examine the selection mechanism in two steps. First, we demonstrate the robustness of our results with a judge-level regression, and show that consistent with our main specification, judicial-commission appointees are less likely to rule in favor of the State (see Table 7, Column 1 to 3).[42] However, we should be cautious in interpreting the coefficient estimates from this judge-level regression, which may confound selection procedure with other judge characteristics determining pro-government rulings. Results reported in column (4) of Table 7 confirm this conjecture: when we control for all the available judge characteristics, we cannot reject the null effect of the reform on State Wins.[43] This is consistent with the judge heterogeneity mechanism, where the reform selects judges with different characteristics.[44]

[41] It is of course possible that peer effects are there but are too small to detect due to lack of statistical power.

[42] In this instance, we aggregate all the 7439 cases at the judge level. Each of the 482 observations in these regressions pertain to a given judge.

[43] In Table C6 of Appendix C, we provide further iterations of judge controls to pin down exactly which judge characteristics are taking away the statistical significance of the main results.

[44] We present judge-level regressions here for two reasons. First, these show in a transparent manner the average differences between the judges appointed under the two selection procedures. Second, we can consider the results in columns 1 to 3, i.e. at the judge level, a comparison to the baseline specification we ran at the case level.

We next examine *which* judge characteristics distinguish judges appointed under the two selection procedures. Presidential and judicial-commission appointees are similar, for instance, in gender, religion, and previous employment (see Table 8). The key distinguishing feature between the two types of judges seems to be that commission-appointed judges are about 35% less likely to have held political office in the Lawyers' Bar Associations (Column 5 of Table 8). As candidates for office in Pakistani Bar Associations run on a political party platform (i.e. on a party "ticket"), we consider this as a plausible proxy for political activity prior to judicial appointment. We also provide direct evidence that judicial-commission appointees are significantly less likely to have run for political office in state or national elections before their judicial appointment.[45] Specifically, we find that peer-appointed judges are also about 15% less likely to have run for elections to the provincial or national assembly prior to their appointments (column 6, Table 8).[46] These results, combined with no evidence for peer effects, strongly suggest that selection of judges with different characteristics is key to understanding our results.[47]

We can interpret these results through the lens of a simple signaling model. A priori, the President does not know the "type" of the judge. Once the judges run for elections, they reveal their type. Therefore, the President selects the judges that are of a similar type or who share similar preferences as the President. Assuming that the President values political loyalty more highly than the peer judges who select judges, and that these judges appointing judges place a greater value on judicial competence relative to the President, the Presidential appointees will be more pro-State and less likely to rule on the evidence of the case or follow due process, as we find in this paper.

Alternatively, these results could also be interpreted as the President appointing "patronage" judges to consolidate his grip on power and to extract resources such as land. This interpretation is consistent with historical evidence presented by Coulson (1964) and Platteau (2017). Using a series of case studies from many Muslim majority countries, including Pakistan, they argue that that Presidents in these countries consider land as a rival source of

Nevertheless, we obtain similar results when we instead conduct case-level regressions. These results are presented in Table C7 of Appendix C.

[45] Once appointed, judges are barred from running for political office until two years after their retirement.

[46] To transparently compare judge characteristics, we consider judge-level regressions as baseline, since case-level regressions will put more weight on the characteristics of judges who decide more cases. However, we obtain essentially identical results when we run case-level regressions, which are presented in Table C7 of Appendix C.

[47] However, we should still interpret this evidence with caution, as political activity prior to judicial appointment is potentially correlated with unobservable judge characteristics.

power, and appoint patronage judges to extract land from the population, thereby consolidating their grasp on power.

6.3. *Land Expropriations Prevented under the Selection Reform*

Our investigation of the type of cases driving the results highlighted reduced pro-government rulings in constitutional cases involving land expropriations and human rights disputes with the State. It is not straightforward to assess the economic value of the decrease in pro-government rulings in human rights abuse cases. However, it is possible to perform a straightforward back-of-the-envelope calculation to assess the economic value derived from avoiding land expropriations due to peer-appointed judges.[48] Using property values taken from judgment orders, our point estimates on impact of selection reform, and total land cases decided in this period, we infer that the selection reform was able to prevent land expropriations worth 0.07 to 0.3 percent of GDP every year.[49] This averages to about 0.14% of GDP or USD 390 million in land expropriations avoided every year through peer-appointed judges. To put this amount into perspective, it is equivalent to about 75% of Pakistan's federal budget for education or the whole amount earmarked for health care in 2016.

7. ROBUSTNESS

This section tests alternative explanations for the finding that the reform generated a change in judicial decision-making in Pakistan and investigates the robustness of our results. First, the documented effect of the reform may be a "President-specific effect". For instance, the fall in State Wins post-reform may simply reflect a correction from extremely high State Wins during the tenure of an idiosyncratic President in the pre-reform period (say, President General Musharraf). Since judges appointed by 5 different Presidents are included in the sample period, we can examine this claim empirically. That is, we can compare rulings by judges appointed by the judicial commission with rulings by judges appointed by different

[48] This back-of-the-envelope calculation is based on the market value of 57 properties expropriated by the Pakistani government. These are listed in judgment texts and are assumed to be representative. Our strategy is similar to Mian and Khwaja (2005)'s computation of economy-wide costs of political connections using minimum and maximum bounds.

[49] The calculation is made as follows: in 20% of our 7500 randomly-sampled cases, the government was successful in expropriating land, so since we randomly sampled 0.2% of the total population of cases, the total number of successful land expropriation cases is about 750,000. Basing computations on an average value of USD 51,280 for the 57 expropriated properties whose market values are listed in judgment texts, and assuming all judges are replaced by peer-appointed judges, then government victories fall by about 20 percentage points. We thus estimate the value of land expropriations prevented due to the selection reform as ranging from 0.07 to 0.3 percent of GDP during 2010-2016. For more details on these computations, see Table C8 in Appendix C.

Presidents. Results from Table 9 show that the effect of selection reform is not a President-specific effect.[50] It could also be argued that the change associated with the reform is a "Chief Justice-specific" effect: some Chief Justices in Pakistan are considered to be particularly anti-government. As the Chief Justice of Pakistan is the head of the judicial commission, this concern may be justified. Since during our sample period five different Chief Justices headed the judicial commission; we therefore test and reject the hypothesis that the results are driven by an idiosyncratic Chief Justice, as shown in Table C9 of Appendix C.

We carry out a number of additional robustness checks, results of which are presented in Tables C10 to C15 of Appendix C. We first show that the effect of the reform is unlikely to be confounded by selection of cases that go to trial (Klein and Priest, 1984; Hubbard, 2013). Although, as noted in the literature, this possibility cannot be entirely be ruled out, but we do show that both total case filings and case filings in politically salient cases are uncorrelated with the selection reform. This is consistent with qualitative accounts that the reform was unanticipated. Moreover, even when the reform was implemented, most litigants are unlikely to know about it due to information constraints, according to both qualitative accounts and recent work on Pakistani courts. For instance, Acemoglu et al., (2020) document that litigants were unaware of the reduced case delays in Pakistani courts and that truthful information on reduced delays increased the litigants' likelihood of using the courts.[51] This makes it unlikely that any case-filing response by litigants would be large if present at all (as also noted in Siddique, 2013). Second, we show that our results are not sensitive to excluding cases decided in federal and provincial capitals. These capital judicial districts are called "principal benches" and some qualitative accounts suggest that the most politically salient cases are decided there (Sattar, 2012). Nevertheless, our point estimates are very similar when we exclude cases decided in these political capitals. This also supports the idea that the effect of reform is not geographically confined to politically salient capital districts. Third, we show that the results are robust to different starting years. We chose to go back as far as 1986 in order to use all available data. However, while going back in time allows us to gain precision, it can raise concerns regarding omitted variables. Therefore, we show that the results are robust to different

[50]We here compare case rulings by judges appointed by different Presidents (different samples of cases) to rulings by judicial commission appointees (single group of cases); identical results are found in alternative specifications with interaction terms for the respective Presidential tenure.

[51]In particular, Acemoglu et al. (2020) document that providing truthful information about reduced delays in state courts in Pakistan leads to citizens reporting higher likelihood of using them and contributing more to the State in high-stake lab games. Likewise, Liaqat (2020) documents that even politicians are under-informed in Pakistan.

starting years. Fourth, our results are not sensitive to aggregation of variables to level of variation of the explanatory variable and non-linear models such as Probit and Logit.

Finally, we show that the results are robust to different levels of clustering: within each district bench separately before and after the reform (Bertrand et al., 2004) and using a wild bootstrap method for small number of clusters, as per Cameron et al. (2008).[52] The results of these robustness checks can be found in Appendix C.[53]

8. CONCLUSION

This paper has shown that Presidential appointment of judges exerts considerable influence over judicial decision-making in Pakistan. We demonstrate that the change in the judicial selection procedure from Presidential appointment to appointment by peer judges significantly reduced government victories, and that this reduction is strongly suggestive of improved quality of judicial decisions. The identification strategy we propose allows us to obtain plausibly causal effects of the reform. We present evidence against a number of threats to identification and alternative explanations for our finding that the selection reform reduced rulings in favor of the government. These results are driven by politically salient constitutional cases involving land and human-rights disputes with the government, and by Presidential appointees who participated in politics prior to their judicial appointments. Last, we calculate that the selection reform prevents land expropriations amounting to about 0.14% of GDP or USD 390 million every year.

Research examining the selection of public officials has, up to now, largely focused on politicians. Our work shifts the focus to the role of judges and contributes to the burgeoning scholarship on patronage in bureaucracies. We add to this literature by documenting how patronage in the Judiciary operates through Presidential appointment of judges. More research on the Judiciary in weakly institutionalized settings will further clarify the conditions necessary for the establishment of rule of law, thereby supporting institutional design efforts in vulnerable democracies.

[52]The "standard error" concept doesn't formally apply for small number of clusters when implementing wild bootstrap. Therefore, as suggested in Roodman et al. (2019), we compute p-values and confidence intervals instead. The results are still significant at 5% level and presented in Figure C3 of Appendix C.
[53]These results are presented Tables C10-Table C15 in Appendix C.

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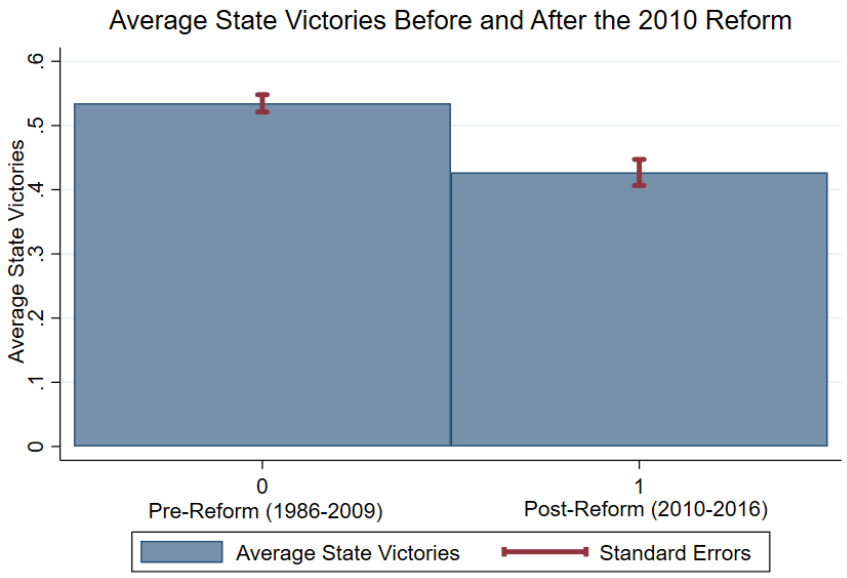
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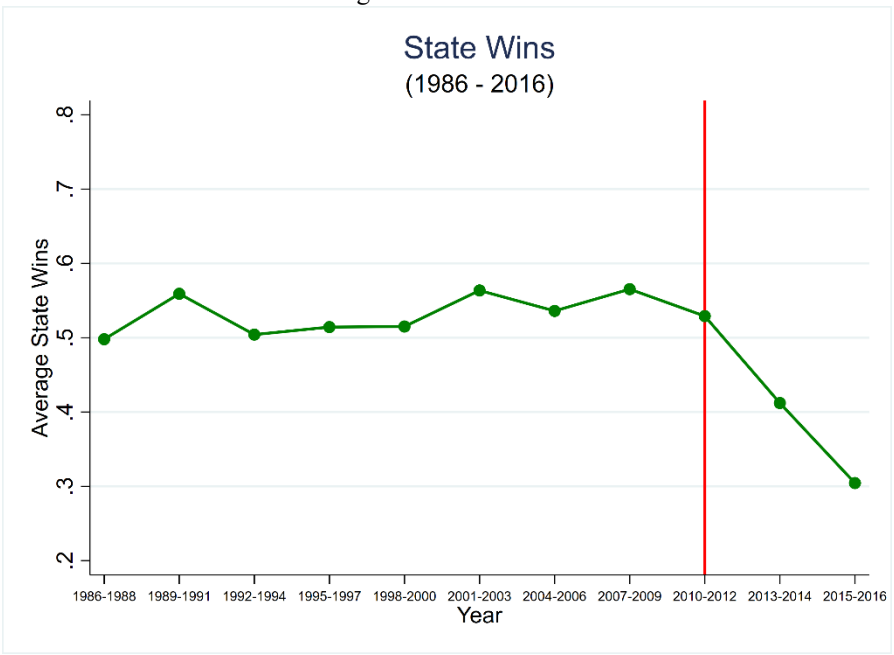
FIGURES AND TABLES

Figure 1: State Wins Pre- and Post-Reform

Panel A: Average State Victories Before and After the Reform

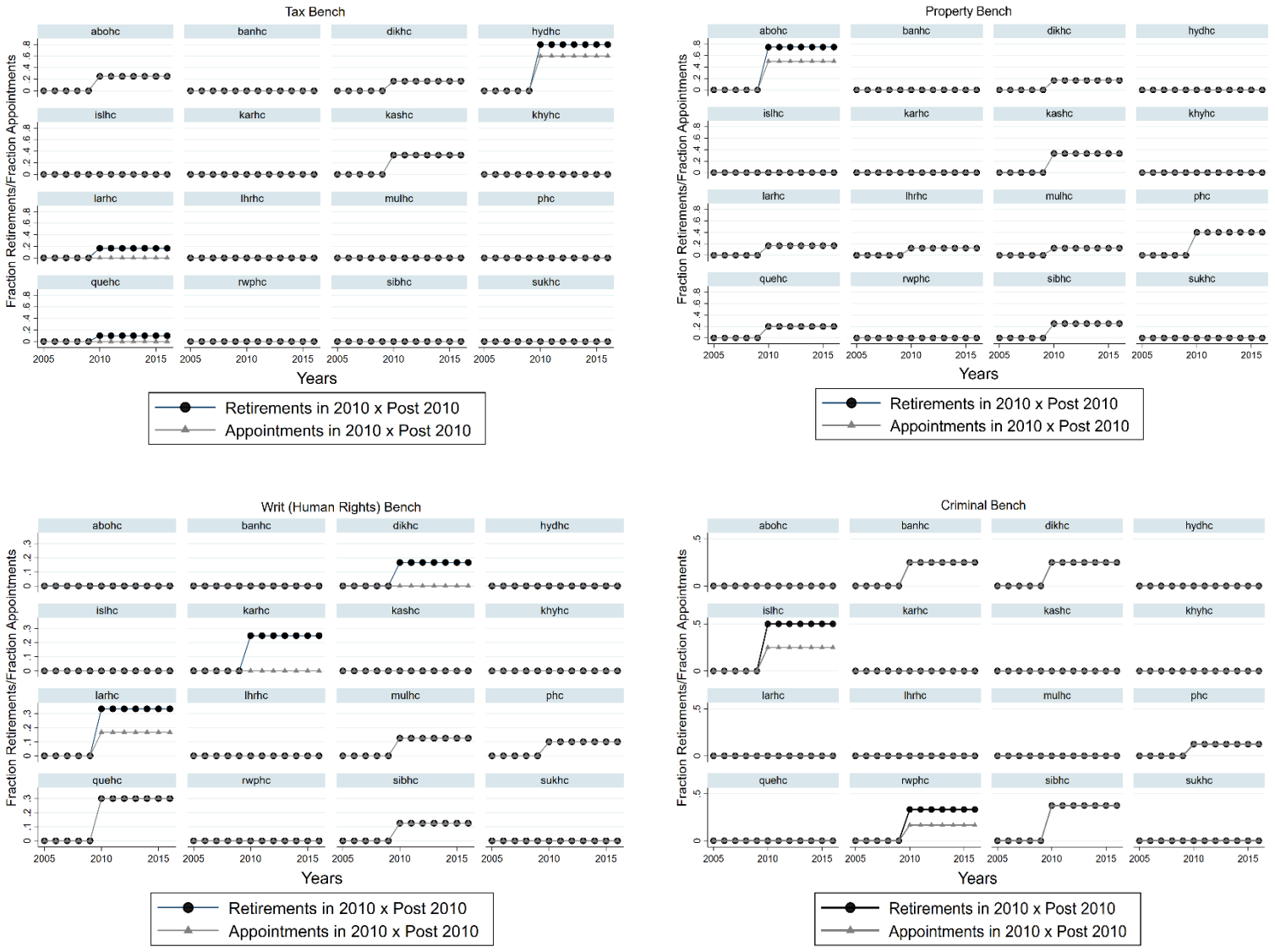


Panel B: Average State Victories over Time



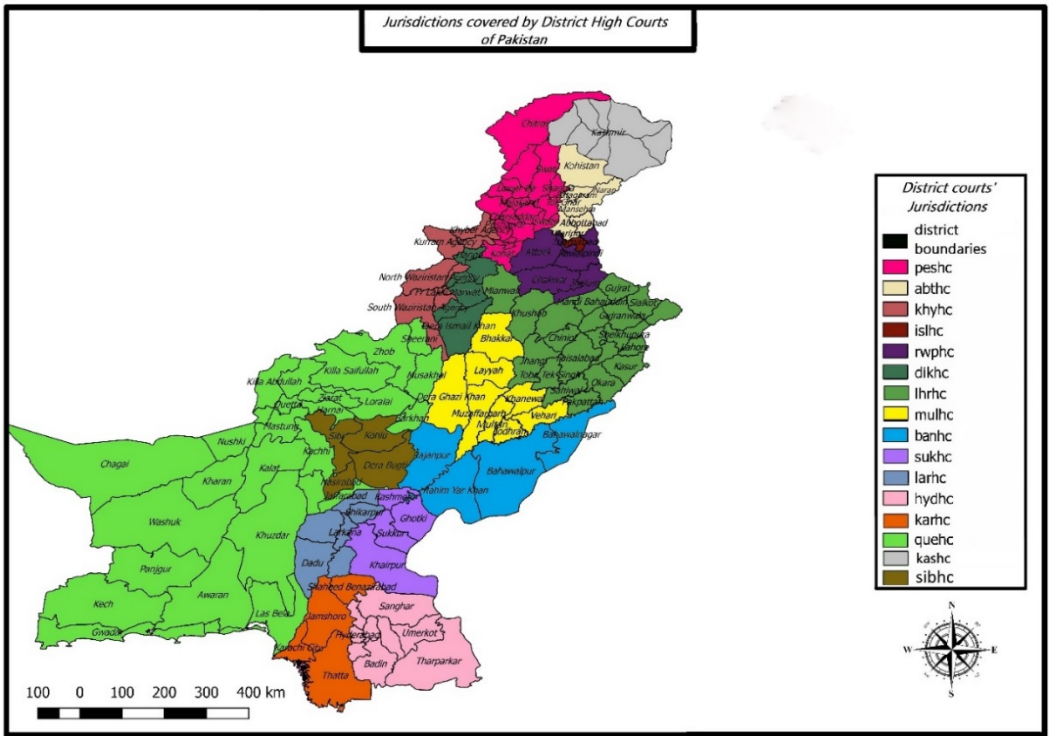
Note: The figure compares State Wins pre- and post-reform. In Panel A, the bar chart shows average State Wins in pre-reform and post-reform periods. In Panel B, we plot average State Wins over time. Vertical line represents the 2010 reform year. For district-wide averages over time, see Figure C1 in Appendix C.

Figure 2: Mandatory Retirements and Judicial Commission Appointments



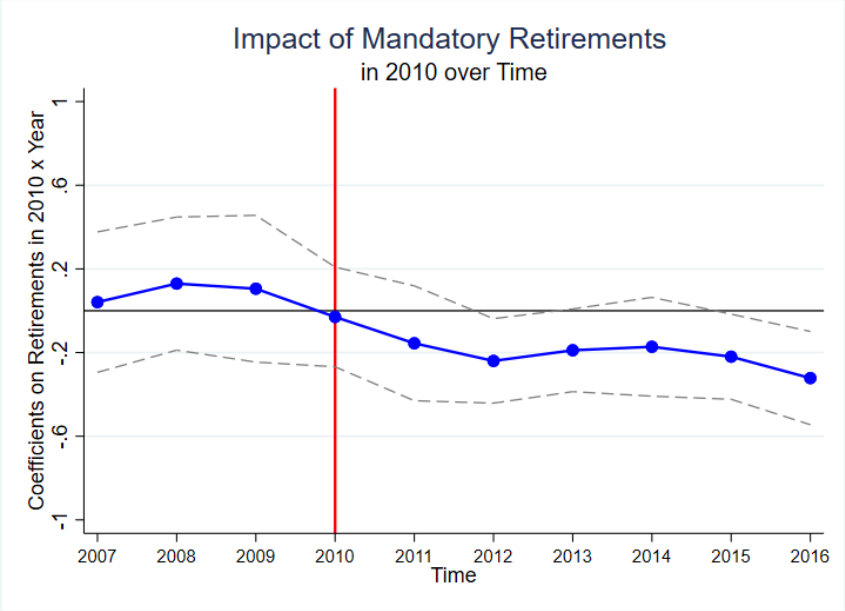
Note: The figure plots our key explanatory variables, which vary at district-bench-year level. Each of the four panels shows a specialized bench adjudicating cases involving tax, property, human rights, and crime, respectively. The dark line represents the fraction of judges reaching their mandatory retirement age of 62 in each district bench in 2010 interacted with the post-reform time dummy. The light line represents the fraction of judges appointed by the judicial commission (peer judges) in each district bench in 2010 interacted with the post-reform time dummy. The correlation coefficient between these variables is 0.935. The regression-form representation of this figure (first-stage) with corresponding F-Statistics appears in Table C2 (Panel B) of Appendix C.

Figure 3: Jurisdictions covered by District High Courts of Pakistan



Note: peshc stands for Peshawar High Court, abthc for Abbottabad High Court, khyhc for Khyber High Court, islhc for Islamabad High Court and so forth.

Figure 4: Impact of Mandatory Retirements in 2010 on State Wins over Time



Note: This figure presents the coefficients (along with their 90% confidence intervals) when we estimate equation (2). The table-form representation of this figure appears in Table C4 of Appendix C. District benches that had more mandatory retirements in 2010 display no change in State Wins prior to the reform year of 2010, while there is a permanent reduction of government victories post-2010 in district benches with more mandatory retirements in 2010.

Table 1: Descriptive Statistics

Variables	Observations	Mean	Std. Dev.	Min	Max
Panel A: Case Characteristics (by cases)					
State Wins	7,439	0.502	0.500	0	1
Merit	7,439	0.625	0.484	0	1
Process Followed	7,439	3.313	1.495	1	5
Constitutional Cases	7,439	0.722	0.448	0	1
Land Cases	7,439	0.408	0.491	0	1
Human Rights Cases	7,439	0.314	0.464	0	1
Criminal Cases	7,439	0.278	0.449	0	1
Pages of Judgment Order	7,439	8.887	7.713	1	81
Number of Lawyers	7,439	4.042	3.627	1	32
Number of Judges on a case	7,439	1.815	0.841	1	5
Chief Justice on Bench	7,439	0.065	0.246	0	1
Panel B: Treatment Variables (by district-bench)					
Retirements in 2010/Total	64	0.118	0.177	0	.8
Appointments in 2010/ Total	64	0. 091	0. 142	0	.6
Total Judges in district-bench	64	7.22	3.079	2	16
Panel C: Judge Characteristics (by judges)					
Tenure at Decision	482	3.090	2.221	0	22
Gender	482	0.964	0.184	0	1
Promoted to SC	482	0.060	0.238	0	1
Former Judge	482	0.112	0.315	0	1
Fr. Office-Holder Bar. Ass.	482	0.579	0.494	0	1
Ran for Political Office	482	0.193	0.395	0	1
Former Lawyer	482	0.888	0.315	0	1
Post-Reform Judge	482	0.280	0.450	0	1
Panel D: District Characteristics (by district-year)					
Area (sq. km)	496	4321.81	3287.76	906	13297
Population	496	2150270	2428460	22454.11	11361010.58
Population Density (per sq. km)	496	1094.32	1764.62	8.46	9023.83
Agriculture Production (tns)	496	455062.8	799778.9	0	4671500
Night Lights	496	8.64	6.18	2.86	28.22

Note: This table reports the summary statistics for the baseline sample of 7439 cases, 482 judges covering the 64 district benches over the 1986-2016 period.

Table 2: Impact of Selection Reform on State Wins

	(1)	(2)	(3)	(4)	(5)
	State Wins				
Retirements in 2010 X Post 2010	-0.149*	-0.202***			
	[0.0887]	[0.0740]			
Retirements in 2009 X Post 2010			0.0619		
			[0.0639]		
Retirements in 2008 X Post 2010				0.0680	
				[0.169]	
Retirements in 2007 X Post 2010					0.0275
					[0.118]
District Bench and Year FE	Yes	Yes	Yes	Yes	Yes
Case and District Controls	No	Yes	Yes	Yes	Yes
Observations	7,439	7,439	7,439	7,439	7,439
R-squared	0.055	0.064	0.063	0.063	0.063
Mean of dependent variable	0.50	0.50	0.50	0.50	0.50

Robust standard errors appear in brackets (clustered at district-bench level). The dependent variable is State Wins, a dummy variable for the case being ruled in favor of the State. Retirements in 2010 is the fraction of mandatory retirements in a given district bench in reform year 2010. Post 2010 is a dummy for post-reform period. See accompanying Table C2 for OLS and 2SLS estimates with Appointments instead of Retirements and Table C3 with pre-treatment Retirements in 2007, 2008 and 2009 as controls in a single equation. The controls include all case and district characteristics shown in Table 1. The case controls also include case-type fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table 3: Impact of Selection Reform on Case and District Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Constitutional Case	Criminal Case	No. Pages	CJ on Case	No. Lawyers on Case	No. Judges on Case	Population	Population Density	Agriculture Production	Night Lights
Retirements in 2010 X Post 2010	0.0251	-0.0673	-1.421	-0.047*	0.647	0.157	-1329735	-909.5	-1256030	0.505
	(0.0824)	(0.103)	(0.988)	(0.025)	(0.492)	(0.157)	(793,338)	(599.0)	(1130347)	(0.850)
District Bench Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,439	7,439	7,439	7,439	7,439	7,439	7,439	7,439	7,439	7,439
R-squared	0.382	0.222	0.254	0.027	0.064	0.081	0.950	0.954	0.707	0.768
Mean of dependent variable	0.722	0.279	8.887	0.065	4.042	1.815	3562527	2065.558	465266	8.113

Robust standard errors appear in brackets (clustered at the district-bench level). Retirements in 2010 is the fraction of mandatory retirements in a given district bench in reform year 2010. Post 2010 is a dummy for post-reform period. The controls include case and district characteristics outlined in Table 1. The case controls include case-type fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Selection Reform and Decision Quality

	(1)	(2)	(3)	(4)
	Merit		*Process Followed*	
Retirements in 2010 X Post 2010	0.146	0.215**	0.286	0.416*
	[0.104]	[0.101]	[0.198]	[0.217]
District Bench and Year FE	Yes	Yes	Yes	Yes
Case and District Controls	No	Yes	No	Yes
Observations	7,439	7,439	7,439	7,439
R-squared	0.087	0.094	0.023	0.027
Mean of dependent variable	0.625	0.625	3.313	3.313

Robust standard errors appear in brackets (clustered at district-bench level). The dependent variable in columns (1) and (2) is a dummy variable for the case being ruled on merits or evidence, in columns (3) and (4), dependent variable is a rating for following due process. Retirements in 2010 is the fraction of mandatory retirements in a given district bench in reform year 2010. Post 2010 is a dummy for post-reform period. The controls include all case and district characteristics shown in Table 1. The case controls also include case-type fixed effects. *** p<0.01, ** p<0.05, * p<0.1

Table 5: Impact of Selection Reform on State Wins (by type of Case)

	Constitutional Cases		Criminal Cases	
	(1)	(2)	(3)	(4)
	State Wins			
Retirements in 2010 X Post 2010	-0.149*	-0.207**	0.0273	0.0240
	[0.0790]	[0.0781]	[0.203]	[0.203]
District Bench and Year FE	Yes	Yes	Yes	Yes
Case and District Controls	No	Yes	No	Yes
Observations	5,362	5,362	2,077	2,077
R-squared	0.067	0.069	0.116	0.126
Mean of dependent variable	0.472	0.472	0.580	0.580

Robust standard errors appear in brackets (clustered at district-bench level). The dependent variable is State Wins, a dummy variable for the case being ruled in favor of the State. Retirements in 2010 is fraction of mandatory retirements in a given district bench in reform year 2010. Post 2010 is a dummy for post-reform period. Table C5 in Appendix C presents further disaggregation of these cases. The controls include all case and district characteristics shown in Table 1. The case controls also include case-type fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table 6: Impact of Selection Reform on Presidentially Appointed Judges (Peer Effects)

	(1)	(2)	(3)	(4)
	State Wins			
Retirements in 2010 X Post 2010	-0.0265	0.0147	-0.0544	-0.0532
	[0.0956]	[0.104]	[0.107]	[0.106]
District Bench and Year FE	No	Yes	Yes	Yes
District Controls	No	No	Yes	Yes
Case Controls	No	No	No	Yes
Observations	6,390	6,390	6,390	6,390
R-squared	0.001	0.042	0.044	0.052
Mean of dependent variable	0.52	0.52	0.52	0.52

Robust standard errors appear in brackets (clustered at district-bench level). The dependent variable is State Wins, a dummy variable for the case being ruled in favor of the State. Retirements in 2010 is fraction of mandatory retirements in a given district bench in reform year 2010. Post 2010 is a dummy for post-reform period. The case controls also include case-type fixed effects. In this table, we only consider the restricted sample of cases decided by presidential appointees. *** p<0.01, ** p<0.05, * p<0.1.

Table 7: Impact of Selection Reform on State Wins at the Judge Level

	State Wins			
	(1)	(2)	(3)	(4)
Post-Reform Judge	-0.160***	-0.162***	-0.162***	-0.0224
	[0.0253]	[0.0284]	[0.0288]	[0.0476]
District Controls	No	Yes	Yes	Yes
Case Controls	No	No	Yes	Yes
Judge Controls	No	No	No	Yes
Observations	482	482	482	482
R-squared	0.093	0.102	0.109	0.145
Mean of dependent variable	0.483	0.483	0.483	0.483

Robust standard errors appear in brackets (clustered at the judge level). The dependent variable is State Wins, an average over the judge of the dummy for the case being ruled in favor of the State. Post-Reform Judge is a dummy variable that takes the value of one if the judge is peer-appointed and zero if a presidential appointee. The controls include all case and district characteristics shown in Table 1. The case controls also include case-type fixed effects. The unit of observation in this regression is an individual judge. Table C6 reports results for different iterations of judge controls. *** p<0.01, ** p<0.05, * p<0.1.

Table 8: Judicial Commission Appointees and Judge Characteristics at Judge Level

	(1)	(2)	(3)	(4)	(5)	(6)
	Gender	Muslim	Former Judge	Former Lawyer	Former Office Holder Bar Assoc.	Ran for Political Office
Post-Reform Judge	-0.0233	0.00806	0.00807	-0.00807	-0.341***	-0.156***
	[0.0214]	[0.0137]	[0.0413]	[0.0413]	[0.0605]	[0.0458]
Age Control	Yes	Yes	Yes	Yes	Yes	Yes
Case Controls	Yes	Yes	Yes	Yes	Yes	Yes
District Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	482	482	482	482	482	482
R-squared	0.026	0.009	0.025	0.025	0.186	0.050
Mean of dependent variable	0.965	0.991	0.112	0.888	0.575	0.191

Robust standard errors appear in brackets (clustered at the judge level). The dependent variable is State Wins, an average over the judge of the dummy for the case being ruled in favor of the State. Post-Reform Judge is a dummy variable that takes the value of one if the judge is appointed by the judicial commission and zero if the judge is appointed by the President. The controls include all case and district characteristics shown in Table 1. The case controls also include case-type fixed effects. These regressions are conducted at judge-level to transparently compare judges of different characteristics. However, similar results are obtained if we instead run case-level regressions (see Table C7 of Appendix C). *** p<0.01, ** p<0.05, * p<0.1.

Table 9: Impact of Selection Reform on State Wins (by appointing President)

	State Wins				
	Pres. Musharraf	Pres. Tarar	Pres. Leghari	Pres. Khan	Pres. Haq
	(1)	(2)	(3)	(4)	(5)
Retirements in 2010 X Post 2010	-0.318***	3.070	-0.312***	-0.195	-0.373***
	(0.0972)	(6.182)	(0.0967)	(0.146)	(0.108)
District-Bench and Year FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Observations	2,608	1,049	2,224	1,834	2,249
R-squared	0.104	0.163	0.129	0.144	0.120
Mean Dep. Variable	0.47	0.38	0.45	0.44	0.46

Robust standard errors appear in brackets (clustered at district-bench level). The dependent variable is State Wins, a dummy variable for the case being ruled in favor of the State. Retirements in 2010 is fraction of mandatory retirements in a given district bench in reform year 2010. Post 2010 is a dummy for post-reform period. The judicial outcomes on cases adjudicated by peer-appointed judges are compared to those of judges appointed by the last 5 Presidents prior to selection reform. The sample size varies according to Presidents' lengths of time in office, which gave them differing opportunities to fill vacancies by appointing new judges. For instance, President Rafiq Tarar was in office for only three years, and consequently appointed few judges; thus, fewer cases were decided by his appointees. The case controls also include case-type fixed effects. Table C9 in Appendix C presents similar results with different Chief Justices in office. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

For Online Appendix to:

Judicial Independence and Development: Evidence from Pakistan

BY SULTAN MEHMOOD

Contents

A. Variable Definitions and sources

B. Data Appendix: Additional information and data collection

C. Additional Figures and Tables

A. Variable Definitions and sources

State Wins = This is a case-level dummy variable for State victories. Law firm coded this variable as 1 for a State victory and 0 for a State loss based on reading the judgement orders retrieved from an online portal that records High Court cases in Pakistan (https://www.pakistanlawsite.com/). More information on this source and data construction can be found in Appendix B.3.

Merit Case = A dummy variable for the case being decided based on "*evidence rather than technical or procedural grounds*" (Pound, 1963). This comes from the assessments by the Law firm based on their reading of the text of the judgment order.

Process Followed = This is a discrete variable that rates from 1 to 5 the extent to which "all relevant jurisdictional, procedural, and evidential requirements were followed in reaching the judicial decision". A higher rating implies higher jurisdictional, procedural, and evidential standards are followed while making the judicial decision. This also comes from assessments by the Law firm based on their reading of the text of the judgment order.

Retirements in 2010 x Post 2010/Total Judges = This variable is the fraction of judges reaching their mandatory retirement age of 62 in 2010. This variable is interacted with a post-2010 reform year dummy variable. Information to construct this variable is obtained from the judicial administrative records of the Registrar Offices of the High Courts and Annual Reports submitted to the Ministry of Law, Justice and Human Rights, Government of Pakistan.

Appointments in 2010 x Post 2010/Total Judges = This variable is the fraction of judges appointed by the judicial commission in 2010. This variable is interacted with a post-2010 dummy variable. Information on new appointments is obtained from the judicial administrative records of the Registrar Offices of the High Courts. Information to construct this variable is obtained from the judicial administrative records of the Registrar Offices of the High Courts and Annual Reports submitted to the Ministry of Law, Justice and Human Rights, Government of Pakistan.

Criminal Case = A dummy for criminal cases. This is indicated in the text of the judgement order.

Constitutional Case = A dummy for constitutional cases. This is indicated in the text of the judgement order.

Land Case = A subset of the constitutional cases. This is a dummy for the case involving a landownership or expropriation dispute with "*The State*". These are "Eminent Domain" cases. The State here is the housing development agency authorized to resolve disputes with the public regarding land ownership on behalf of the government (the Defense Housing Authority, the Lahore

Development Authority (LDA), the Karachi Development Authority (KDA), the Peshawar Development Authority (PDA), and the Capital Development Authority (CDA)).

Human-Rights Case = A subset of the constitutional cases. This is a dummy variable for the case involving a human-rights dispute with "*The State*". These cases are marked as "writ petitions" in the text of the judgment order and are non-land cases against the government involving violation of a fundamental right.

Islamic Law Case = A subset of criminal cases that involve Islamic Law violations, i.e. all cases that were judged under the "Hudood Ordinance". These cases involve consumption of alcohol, adultery, fornication, homosexuality, and blasphemy.

Number of Lawyers = A count variable for the number of lawyers arguing the case. This is also indicated in the text of the judgment order.

Number of Judges = A count variable for the number of judges adjudicating upon the case. This is also indicated in the text of the judgment order.

Bench Chief Justice = A dummy variable for the Chief Justice adjudicating in the case. This is also indicated in the text of the judgment order.

Number of Pages of Judgment Orders = A count variable for the number of pages in the judgment order. This is also indicated in the text of the judgement order.

Age at appointment = The difference between date of birth and age at appointment. This data is obtained from Judicial Administrative Data Records at the High Court Registrar Offices.

Gender = A dummy for male judges. This is coded in two ways: 1) Manually, where the author checks every judge name, and 2) Automatically, where the author asked Stata to read the string starting with "Justice Miss" and "Justice Mrs." as zero and the string starting with "Justice Mr." as one. The two methods yielded an identical number of male and female justices.

Promoted to SC = A dummy for the judge being elevated to the Supreme Court. This comes from the judicial administrative records of the Supreme Court Registrar Office.

Former Lawyer = A dummy for the judge having been a Lawyer before being appointed as a High-Court justice. The data comes from a combination of biographical information contained in annual reports, Bar Council records, and judicial administrative data.

Former Office Holder Bar Association = A dummy for the judge having been an office holder in the Lawyers' Bar Association (before being appointed a High-Court justice). The data comes

from a combination of biographical information contained in annual reports, Bar Council records, and judicial administrative data.

Ran for Political Office = A dummy for the judge having run for State or national elections prior to appointment. The data comes from the Election Commission of Pakistan matched with judicial administrative data.

Former Judge = A dummy for the judge having formerly been a lower (civil or session) court judge. The data comes from a combination of biographical information contained in annual reports and judicial administrative data.

Total Judges in District Bench = A district-bench-year count variable of the number of judges on a district bench in a given year. This variable is constructed from information contained in annual reports submitted to Ministry of Justice, Pakistan and registrar office records.

Night-time Lights = This variable is a logarithm of visible light bands that range from 0-63, with higher values representing higher night-time intensity. The data is obtained from the United States' National Oceanic and Atmospheric Administration database.

Agricultural Production = Total production of agriculture goods (in tons) in the district where the High Court is located. This comes from Pakistan agriculture censuses.

Area = The area (in square kilometers) of the district where the High Court is located. This is obtained from Pakistan census data.

Population = The population of the district where the High Court is located. This is obtained from a linear interpolation of 1981, 1998, and 2017 Pakistan census data.

Population Density = The per square kilometer population density of the district where the High Court is located (area/population). This comes from a linear interpolation of 1981, 1998, and 2017 Pakistan census data.

B. Data Appendix: Additional information and data collection

Appendix B.1. *The History and Structure of Courts in Pakistan*

This subsection presents the background and structure of the Courts in Pakistan. The Indian High Courts Act of 1861 authorized the Crown to create High Courts in the Indian colony. These Courts served as precursors to the modern-day High Courts in both India and Pakistan. With the independence of India and Pakistan from British colonial rule in 1947, gradual changes were made in the legal institutions in both countries, but both retained their overarching institutional structure, such as Common Law jurisprudence. One change that is relevant here is the raising of the mandatory retirement age from 60 to 62. India raised the retirement age of High Court judges to 62 in 1963 and Pakistan made the same change in 1969 (both under constitutional amendments); 62 has remained the mandatory retirement age of High Court judges in both countries ever since. It is worth noting that this change occurred long before the selection reform of 2010.

Pakistan's judiciary is a three-tier hierarchical structure. The lowest Courts are the civil and session Courts, which hear civil and criminal cases respectively. These Courts are located in the provincial capitals, and their jurisdictions are dictated by the domicile of the litigating parties. Decisions by civil and session Courts can be challenged in the High Courts. If the government expropriates land or violates a fundamental right, the High Court is the first, and in most cases the only, remediation platform for individuals and firms. Although there are only four provincial High Courts in Pakistan, the benches of each are spread out over the four provinces (see Figure 3) in the form of 16 district High Court divisional units. Key for our paper is that cases can be filed against the government in the High Court in the form of a constitutional or criminal petition with the State as a party. Constitutional cases involving *The State* are filed against the federal government, provincial governments, local governments, or any organ of the state that yields executive authority (such as the office of the Prime Minister). Finally, there is the final appellate Court, the Supreme Court of Pakistan, located in the federal capital of Islamabad. This typically hears appeals on "technical" grounds for the criminal and constitutional cases in the High Courts. The Supreme Court can have at most 16 judges, which greatly limits the number and scope of cases it can hear. Only a small fraction of cases therefore ends up being heard by the Supreme Court (Arshad, 2017).

Appendix B.2. *The Political landscape at the time of the selection reform*

Since the 1990s, Pakistan has largely been dominated by two political parties: the Center-Right Pakistan Muslim League Nawaz (PML-N, henceforth) led by Nawaz Sharif, and the Center-Left Pakistan Peoples' Party (PPP, henceforth) led by Benazir Bhutto. The 1990s was also a particularly volatile period in Pakistan's history. First, no government was able to complete its five-year electoral term. Second, there were eight changes of Prime Minister and five changes of President over this period, rotating between the PML-N and the PPP. It was in this time of political uncertainty that the then army chief, General Pervez Musharraf, stepped in and seized power to ensure "stability", in what is now known as the "coup d'état of 1999." General Musharraf consolidated his power and won a controversial referendum in 2002 that awarded him five years of Presidency; he managed to cobble together a coalition government consisting of ex-PPP and ex-PML-N lawmakers (Bose and Jalal, 2004).

With elections due in January 2008 and Musharraf leading the polls, the sudden assassination of Benazir Bhutto on December 27th, 2007 drastically changed Pakistan's political landscape. The PPP managed to obtain the majority (Perlez and Gall, 2008), with many analysts attributing this result to a "sympathy wave" sweeping across the country as a direct consequence of the assassination (Basu, 2008). General Musharraf's political allies obtained less than 10% of the vote, and Musharraf resigned as President on 8th September 2008, once the impeachment proceedings were due to start. On 9th September 2008, the PPP Chairman, the widower of Benazir Bhutto, Asif Ali Zardari, was sworn in as the 11th President of Pakistan. It was against this backdrop that President Zardari gave a small parliamentary committee party the authority to frame an amendment to the constitution that would dramatically change judicial selection in Pakistan. The idea was that an independent judiciary might reduce the power of the military and provide a safeguard against future "unconstitutional" military takeovers (Zafar, 2012; Almeida, 2018).

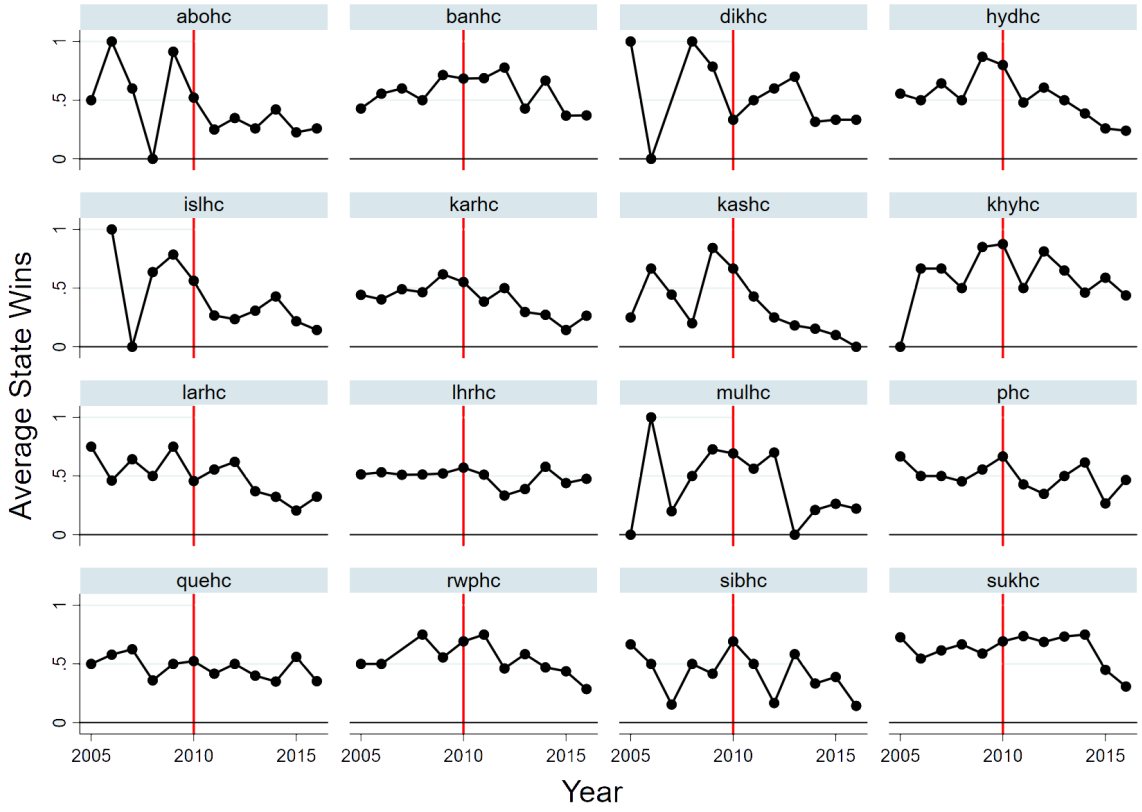
Appendix B.3. *Case Data Sources and Construction*

Both the case characteristics and the outcome variables are based on judgment orders available online from the central repository of cases used by Lawyers in Pakistan to prepare their cases: (https://www.pakistanlawsite.com/). This website is the "Central Library" for lawyers preparing cases (Pakistan has a Common Law system, where case precedent is crucial), and is also used by lawyers, paralegals, and students studying for their Law exams. Access is password-protected, and permission to use the website and cases is obtained through a Law firm. This library contains the universe of High Court cases from 1950 to 2016; we chose a sample period from 1986 to 2016 inclusive, given our budget and research question. We randomly sample 0.2% of all the available cases in every year from the universe of cases decided in that year from 1986 to 2016 inclusive. The random sample is conditional on State being one of the defendants. The number of cases decided in a given year gradually rises over time, most likely due to rising population, and this is reflected in a gradual increase in our sample. Figure C4 presents this information as a plot of sampled cases and total available cases.

Two teams of four paralegals supervised by a senior Lawyer each recorded the key information in the judgment order texts available from the website for these 7439 cases. Table C1 presents the means of the outcome variables and case characteristics coded by the two teams, as well as the correlation coefficient between them. There is a strong correlation between the coding of the two teams. For instance, the average State Wins from Team 1 is 0.50 and the correlation coefficient for State Wins between the two teams is 0.89. Since there is some subjectivity in coding the State Wins or Merit variables, we consider the robustness of our results across the two teams: we obtain similar results, measuring either State Wins or Merit, from both teams. Throughout, the paper, for space reasons, we report the results from Team 1. Unsurprisingly given the high correlation coefficients, similar results are obtained from the dataset of Team 2. The law firm was given no information on the research question, to ensure that the paralegals performing the coding did not know whether a judge was appointed by peers or the President. Nevertheless, it is possible that since the law firm is based in Lahore, some paralegals might have had information about some judges in the Lahore district, including how they were appointed. We therefore show that the results are robust to excluding cases adjudicated in political capitals in Table C11 of Appendix C.

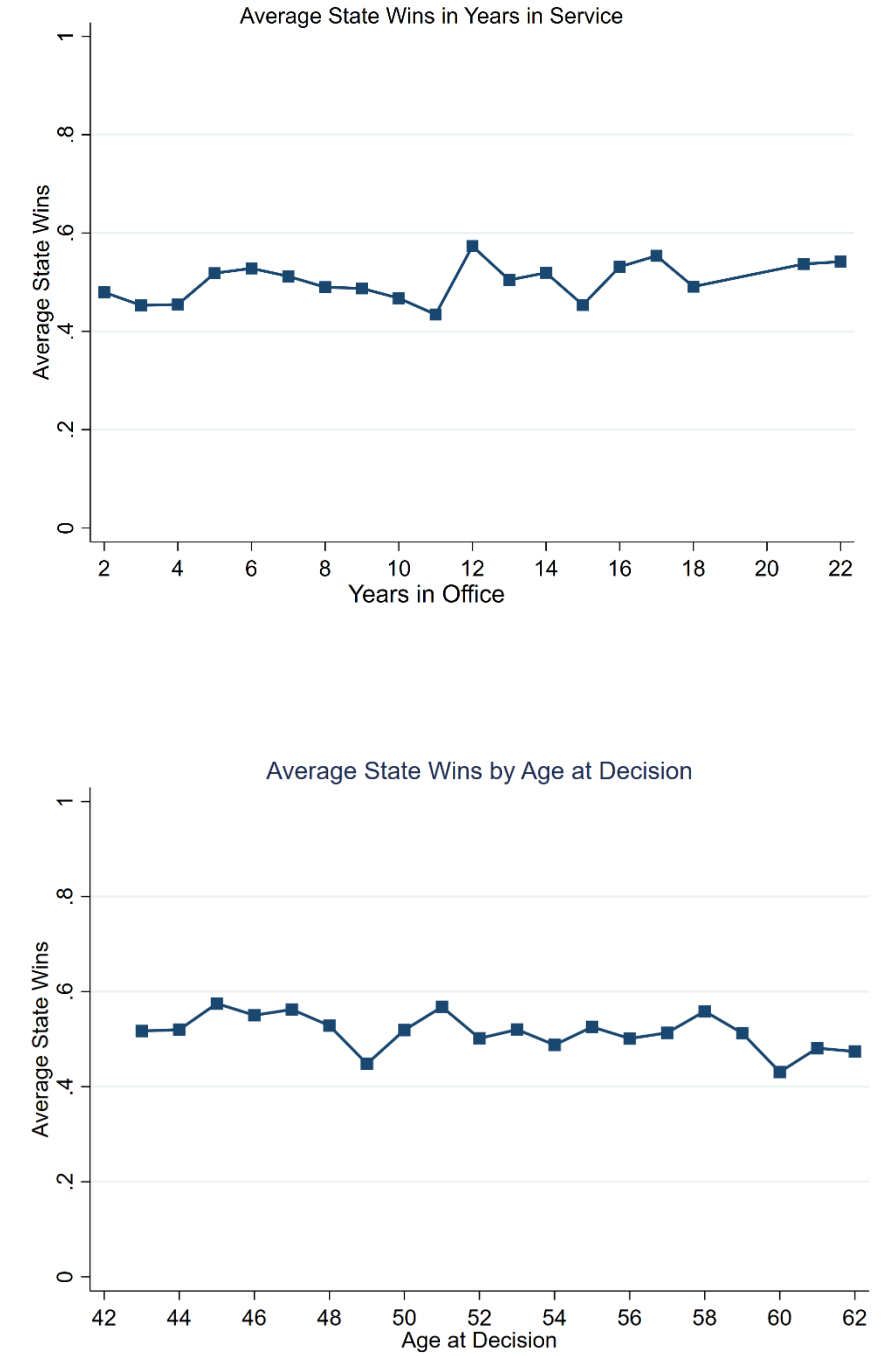
Appendix C. Additional Tables and Figures

Figure C1: State Wins over Time by District (16-panel graph)



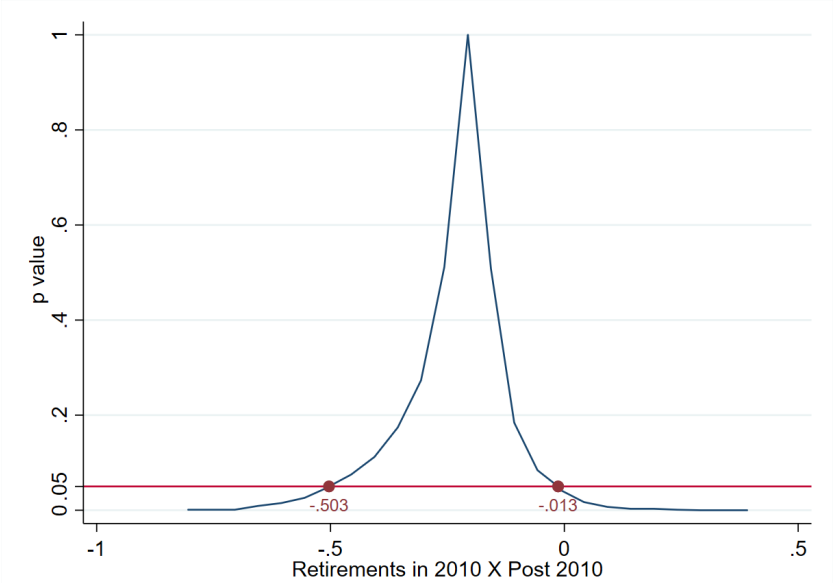
Note: The figure plots average State Wins over time for every district. abohc is average State Wins over time across all benches in Abbottabad, islhc is average State Wins over time for all benches in Islamabad and so on. Vertical line represents the 2010 reform year.

Figure C2: Average State Wins by Years in Office and Age at Decision



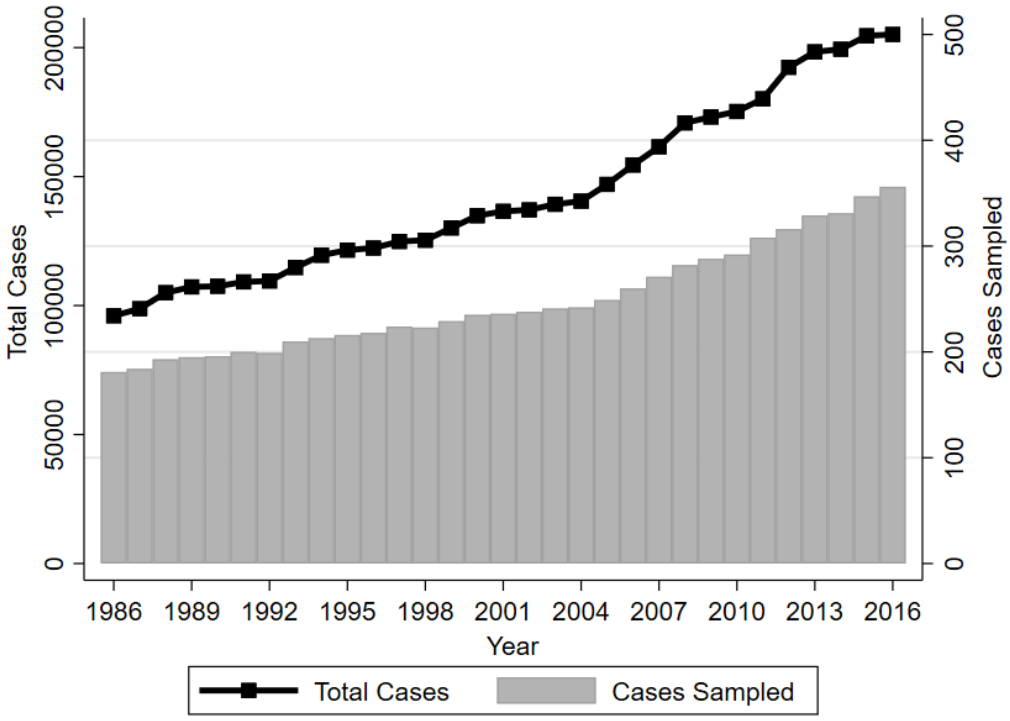
Note: The Figures above plot the average State Wins by years in office (upper panel) and age at decision (lower panel). For instance, the upper panel shows that judges with 15 years of experience averaged 45% State Wins. Likewise, the lower panel shows that all 42-year-old judges averaged about 50% State Wins. Importantly, however, we observe no sharp jump in State Wins for judges nearing retirement.

Figure C3: Confidence Interval by Wild Bootstrap Clustering



Note: The figure displays confidence interval for our main coefficient of interest for the baseline specification (1) using wild bootstrap clustering, as per Cameron et al. (2008) (that imposes a small cluster correction). Roodman et al. (2019)'s *bootest* in Stata is used to construct this confidence interval.

Figure C4: Total vs. Sampled Cases



Note: These are 7500 randomly sampled cases for all years from 1986 to 2016 from the universe of district High Courts in Pakistan (0.2% of total cases decided in the period are sampled).

Table C1: Outcome Variables and Case Characteristics

Variables	Team 1	Team 2	Difference	Correlation (ρ)
State Wins	0.50	0.56	-0.06	0.89
Merit	0.62	0.67	0.05	0.88
Process Followed	3.31	3.22	0.09	0.85
Constitutional	0.72	0.70	-0.01	0.95
Land Cases	0.41	0.38	0.03	0.94
HR Cases	0.31	0.33	0.02	0.96
Criminal Cases	0.28	0.29	-0.01	0.93
No. of Lawyers	4.04	4.09	-0.05	0.94
No. of Judges	1.81	1.83	-0.02	0.87
CJ in Bench	0.06	0.08	-0.02	0.83
Pg. of Judgment	8.88	8.71	0.03	0.97

Note: This table compares the outcome variables and case characteristics for the two teams of coders for the same 7439 cases as used in the analysis. Team 1 data are used in the regressions. The table shows the two means, the difference, and the correlation coefficient between them.

Table C2: Impact of Selection Reform on State Wins

Panel A: Ordinary Least Squares and 2nd-Stage Least Squares Results				
	OLS		2SLS, 2nd Stage	
	(1)	(2)	(3)	(4)
	State Wins			
Appointments in 2010 X Post 2010	-0.169	-0.211**	-0.191*	-0.257***
	[0.104]	[0.0946]	[0.110]	[0.0926]
	p-value=0.10			
District-Bench and Year FE	Yes	Yes	Yes	Yes
District and Case Controls	No	Yes	No	Yes
Observations	7,439	7,439	7,439	7,439
R-squared	0.055	0.064	0.055	0.064
Mean of dependent variable	0.503	0.503	0.503	0.503

Panel B: First-Stage Results		
	(3)	(4)
	Appointments in 2010 X Post 2010	
Retirements in 2010 X Post 2010	0.779***	0.786***
	[0.0375]	[0.0372]
District-Bench and Year FE	Yes	Yes
Case and District Controls	No	Yes
Observations	7,439	7,439
R-squared	0.962	0.963
F-Statistic (Montiel et al., 2013)	432.120	447.586

Robust standard errors appear in brackets (clustered at district-bench level). The dependent variable is State Wins, a dummy variable for the case being ruled in favor of the State. Appointments in 2010 is fraction of mandatory retirements in a given district bench in reform year 2010. Post 2010 is a dummy for post-reform period. In the IV regressions, we instrument Appointments in 2010 X Post 2010 with Retirements in 2010 X Post 2010. The first-stage results corresponding to columns (3) and (4) appear in Panel B. The F-Statistics on the first stage results are well above both the rule of thumb of 10 and the threshold of 23 derived by Montiel, Olea and Pflueger (2013) for 10% potential bias, 5% significance, and clustered standard errors. The controls include all case and district characteristics shown in Table 1. The case controls also include case-type fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table C3: Impact of Selection Reform on State Wins (Controlling for Pre-2010 Retirements)

	(1)	(2)	(3)	(4)	(5)	(6)
	State Wins					
Retirements in 2010 X Post 2010	-0.213***	-0.203***	-0.205***	-0.213***	-0.214***	-0.215***
	[0.0705]	[0.0731]	[0.0723]	[0.0709]	[0.0690]	[0.0689]
Retirements in 2009 X Post 2009	0.0533			0.0463	0.0532	0.0482
	[0.0629]			[0.0599]	[0.0619]	[0.0585]
Retirements in 2008 X Post 2008		0.0996	0.173		0.0991	0.168
		[0.148]	[0.393]		[0.150]	[0.409]
Retirements in 2007 X Post 2007				0.0315		0.0226
				[0.168]		[0.177]
Retirements in 2008 X Post 2010			-0.0866			-0.0879
			[0.420]			[0.417]
District-Bench and Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Case and District Controls	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Treatment Retirement (2009)	Yes	No	No	Yes	Yes	Yes
Transition to Democracy (2008)	No	Yes	Yes	No	Yes	Yes
Pre-Treatment Retirement (2007)	No	No	No	Yes	No	Yes
Observations	7,439	7,439	7,439	7,439	7,439	7,439
R-squared	0.064	0.064	0.064	0.064	0.064	0.064
Mean of dependent variable	0.503	0.503	0.503	0.503	0.503	0.503

Robust standard errors appear in brackets (clustered at district-bench level). The dependent variable is State Wins, a dummy variable for the case being ruled in favor of the State. Retirements in 2010 is fraction of mandatory retirements in a given district bench in reform year 2010. Retirements in 2007, 2008, 2009 are the fraction of mandatory retirements in a given district bench in 2007, 2008, 2009, respectively. Column (3) examines the robustness of our results to retirements in the transition to democracy year in 2008. It controls for fraction of mandatory retirements in 2008 (democratic transition year) interacted with post 2008 and 2010 dummies, respectively. Column (6) controls for pre-treatment retirements in 2007, 2008, and 2009 and corresponding interactions. The controls include all case and district characteristics shown in Table 1. The case controls also include case-type fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table C4: Test for Pre-Trends - Impact of Selection Reform on State Wins Over Time

	(1)	(2)	(3)
		State Wins	
Retirements in 2010 X Year 2007	0.141	0.0963	0.0422
	[0.178]	[0.193]	[0.205]
Retirements in 2010 X Year 2008	0.186	0.184	0.115
	[0.179]	[0.179]	[0.195]
Retirements in 2010 X Year 2009	0.163	0.160	0.105
	[0.216]	[0.215]	[0.214]
Retirements in 2010 X Year 2010	0.0396	0.0326	-0.0275
	[0.169]	[0.164]	[0.144]
Retirements in 2010 X Year 2011	-0.0866	-0.0938	-0.155
	[0.161]	[0.160]	[0.168]
Retirements in 2010 X Year 2012	-0.177	-0.175	-0.237*
	[0.138]	[0.135]	[0.123]
Retirements in 2010 X Year 2013	-0.140	-0.127	-0.188
	[0.130]	[0.128]	[0.121]
Retirements in 2010 X Year 2014	-0.106	-0.0962	-0.173
	[0.147]	[0.146]	[0.144]
Retirements in 2010 X Year 2015	-0.159	-0.146	-0.219*
	[0.145]	[0.140]	[0.124]
Retirements in 2010 X Year 2016	-0.258**	-0.258**	-0.322**
	0.141	0.0963	0.0422
District-Bench and Year FE	Yes	Yes	Yes
Case Controls	No	Yes	Yes
District Controls	No	No	Yes
Observations	7,439	7,439	7,439
R-squared	0.056	0.063	0.064
Mean of dependent variable	0.50	0.50	0.50

Robust standard errors appear in brackets (clustered at district-bench level). The dependent variable is State Wins, a dummy variable for the case being ruled in favor of the State. Retirements in 2010 is fraction of mandatory retirements in a given district bench in reform year 2010. This is interacted with year dummies as indicated in equation (2). The controls include all case and district characteristics shown in Table 1. The case controls also include case-type fixed effects. Estimates from column (3) with controls are presented in graphical form in Figure 4. *** p<0.01, ** p<0.05, * p<0.1

Table C5: Impact of Selection Reform on State Wins (by type of Constitutional and Criminal Case)

Panel A: Constitutional Cases				
	Land Cases		Human-Rights Cases	
	State Wins			
Retirements in 2010 X Post 2010	-0.0517	-0.124	-0.0767	-0.149
	[0.131]	[0.107]	[0.127]	[0.107]
District-Bench and Year FE	Yes	Yes	Yes	Yes
Case and District Controls	No	Yes	No	Yes
Observations	3,041	3,041	3,028	3,028
R-squared	0.099	0.103	0.100	0.104
Mean of dependent variable	0.48	0.48	0.50	0.50

Panel B: Criminal Cases				
	Non-Islamic Criminal Cases		Islamic Criminal Cases	
	State Wins (Convictions)			
Retirements in 2010 X Post 2010	0.0407	0.0240	0.231	0.397
	[0.154]	[0.153]	[0.778]	[0.718]
District-Bench and Year FE	Yes	Yes	Yes	Yes
Case and District Controls	No	Yes	No	Yes
Observations	1,849	1,849	226	226
R-squared	0.126	0.137	0.315	0.368
Mean of dependent variable	0.587	0.587	0.527	0.527

Robust standard errors appear in brackets (clustered at district-bench level). The dependent variable is State Wins, a dummy variable for the case being ruled in favor of the State. Retirements in 2010 is fraction of mandatory retirements in a given district bench in reform year 2010. Post 2010 is a dummy for post-reform period. The controls include all case and district characteristics shown in Table 1. The case controls also include case-type fixed effects. The Islamic cases are all cases judged under Islamic limits or "Hudood" Law. This includes cases pertaining to consumption of alcohol, blasphemy, adultery, homosexuality, and fornication. *** p<0.01, ** p<0.05, * p<0.1.

Table C6: Impact of Selection Reform on State Wins at Judge Level

	(1)	(2)	(3)	(4)	(5)
	State Wins				
Post Reform Judge	-0.158***	-0.161***	-0.160***	-0.135***	-0.0205
	[0.0251]	[0.0282]	[0.0286]	[0.0325]	[0.0476]
District Controls	No	Yes	Yes	Yes	Yes
Case Controls	No	No	Yes	Yes	Yes
Some Judge Controls	No	No	No	Yes	-
All Judge Controls	No	No	No	No	Yes
Observations	482	482	482	482	482
R-squared	0.092	0.100	0.108	0.118	0.143
Mean of dependent variable	0.483	0.483	0.483	0.483	0.483

Robust standard errors appear in brackets (clustered at the judge level). The dependent variable is State Wins, average State Wins for a case ruled in favor of the State at the judge level. Post Reform Judge is a dummy variable that takes the value of one if the judge is appointed by the judicial commission and zero if the judge is appointed by the President. The controls include all case and district characteristics shown in Table 1. The case controls also include case-type fixed effects. Some controls include judge level controls except "Former Office Holder Bar Association" and "Ran for Political Office" prior to judicial appointments. *** p<0.01, ** p<0.05, * p<0.1.

Table C7: Judicial Commission Appointees and Judge Characteristics at Case Level

	(1)	(2)	(3)	(4)	(5)	(6)
	Gender	Muslim	Former Judge	Former Lawyer	Former Office Holder Bar Assoc.	Ran for Political Office
Post Reform Judge	-0.0747*	0.00191	-0.0137	0.0137	-0.366***	-0.139***
	[0.0410]	[0.0133]	[0.0332]	[0.0332]	[0.0605]	[0.0482]
Age Control	Yes	Yes	Yes	Yes	Yes	Yes
Case Controls	Yes	Yes	Yes	Yes	Yes	Yes
District Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,439	7,439	7,439	7,439	7,439	7,439
R-squared	0.023	0.004	0.009	0.009	0.083	0.017
Mean of dep. variable	0.960	0.991	0.115	0.88	0.631	0.226

Robust standard errors appear in brackets (clustered at the judge level). Post Reform Judge is a dummy for a case decided by a judge appointed by the judicial commission. The case and district controls are identical to those in the baseline regression. The age control is tenure at decision (Similar results are obtained if we use age at decision or age at appointment). *** p<0.01, ** p<0.05, * p<0.1.

Table C8: Savings in Land Expropriations Avoided Due to Selection Reform

Panel A: Land Expropriations Results

	(1) Average Value	(2) Minimum Value	(3) Maximum Value
Total Land Expropriated (% of GDP)	0.899	0.332	1.693
Counterfactual: No Selection Reform (% of GDP)	0.998	0.399	1.692
Land Expropriations Avoided Due to Selection Reform (% of GDP)	0.140	0.067 (2015)	0.284 (2010)

Panel B: Land Expropriation Details

Year	Pakistan GDP (USD)	Total Land Expropriated (USD)	Total Land Expropriated (% GDP)	Counterfactual: No Selection Reform (% GDP)	Non-expropriated land under Selection Reform (% GDP)
2007	152385716312	2196000000	1.441079947	1.44107995	0
2008	170077814106	2880000000	1.693342553	1.69334255	0
2009	168152775283	1700000000	1.010985395	1.0109854	0
2010 (max)	177165635077	2494000000	1.407722214	1.692082101	0.284359887
2011	213587413184	851000000	0.398431718	0.478914925	0.080483207
2012	224383620830	2320000000	1.033943561	1.242800161	0.208856599
2013	231218567179	1400000000	0.605487707	0.727796224	0.122308517
2014	244360888751	1150000000	0.470615411	0.565679724	0.095064313
2015 (min)	270556131701	900000000	0.332648162	0.399843091	0.067194929
2016	278654637738	1679000000	0.602537971	0.724250641	0.12171267

Note: Panel A shows the results of the back-of-the-envelope calculations based on Mian and Khwaja (2005)'s computation of economy-wide costs of political connections using minimum and maximum bounds in Pakistan. The minimum value of avoided land expropriations is realized in 2015 (0.067% of GDP), the maximum in 2010 (0.284% of GDP), while the average value of land expropriations avoided is about 0.140% of GDP every year. That is, the computations indicate that the selection reform prevented land expropriations to the tune of 0.07 to 0.3 percent of GDP (average: 0.14 percent of GDP) from its implementation onward. The procedure for this calculation is as follows: given that in 20% of our 7500 randomly sampled cases, the government was successful in expropriating land, and that we randomly sampled 0.2% of the total population of cases, total state victories in land expropriation cases are calculated at 750,000. Basing computations on an average value of USD 51,280 for the 57 expropriated properties whose market values are listed in judgment texts, and assuming all judges are replaced by peer-appointed judges, state victories should fall by about 20 percentage points. We thus estimate that value of avoided land expropriations to be about 0.07 to 0.3 percent of GDP from 2010-2016. Panel B presents yearly figures showing the minimum and maximum value of total land expropriations, counterfactual land expropriations, and expropriations avoided.

Table C9: The Impact of Selection Reform on State Wins (by Chief Justice)

	(1)	(2)	(3)
		State Wins	
CJ Jamali X Retirements in 2010 X Post 2010	-0.284***	-0.281***	-0.338**
	(0.101)	[0.102]	[0.130]
CJ Khawaja X Retirements in 2010 X Post 2010	-0.156	-0.143	-0.212**
	(0.117)	[0.112]	[0.0983]
CJ Mulk X Retirements in 2010 X Post 2010	-0.166	-0.151	-0.204*
	(0.129)	[0.126]	[0.117]
CJ Jillani X Retirements in 2010 X Post 2010	-0.202	-0.198	-0.253**
	(0.132)	[0.129]	[0.114]
CJ Chaudhry X Retirements in 2010 X Post 2010	-0.0362	-0.0413	-0.0954
	[0.161]	[0.160]	[0.168]
District-Bench and Year FE	Yes	Yes	Yes
Case Controls	No	Yes	Yes
District Controls	No	No	Yes
Observations	7,439	7,439	7,439
R-squared	0.056	0.063	0.064
Mean of dependent variable	0.50	0.50	0.50

Robust standard errors appear in brackets (clustered at district-bench level). The dependent variable is State Wins, a dummy variable for the case being ruled in favor of the State. Retirements in 2010 is fraction of mandatory retirements in a given district bench in reform year 2010. Post 2010 is a dummy for post-reform period. Retirements in 2010 X Post 2010 is interacted with periods when different Chief Justices headed the Judicial Commission. The controls include all case and district characteristics shown in Table 1. The case controls also include case-type fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table C10: The Impact of Selection Reform on Case Filings

	(1)	(2)	(3)
	Total Filed	Constitutional Filed	Criminal Filed
Retirements in 2010 X Post 2010	-6,447	-4,778	-1,669
	[6,425]	[4,669]	[1,765]
District-Bench and Year FE	Yes	Yes	Yes
District and Case Controls	Yes	Yes	Yes
Observations	449	449	449
R-squared	0.957	0.957	0.951
Mean of dependent variable	9561.178	6881.19	2679.89

Robust standard errors appear in brackets. The dependent variable is total cases filed in the first column, total constitutional cases filed in the second column, and total criminal cases filed in the third column. Retirements in 2010 is fraction of mandatory retirements in a given district bench in reform year 2010. Post 2010 is a dummy for post-reform period. The controls include all case and district characteristics shown in Table 1. The case controls also include case-type fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table C11: Impact of Selection Reform on State Wins (excluding cases from Capitals)

	Excluding Lahore	*Excluding Karachi*	*Excluding Quetta*	*Excluding Peshawar*	*Excluding Islamabad*
	State Wins				
Retirements in 2010 X Post 2010	-0.172**	-0.269***	-0.199***	-0.223***	-0.222***
	[0.0738]	[0.0808]	[0.0741]	[0.0786]	[0.0758]
District-Bench and Year FE	Yes	Yes	Yes	Yes	Yes
District and Case Controls	Yes	Yes	Yes	Yes	Yes
Observations	5,928	6,488	6,867	7,204	7,294
R-squared	0.077	0.065	0.066	0.061	0.061
Mean of dependent variable	0.496	0.510	0.499	0.503	0.505

Robust standard errors appear in brackets (clustered at district-bench level). The dependent variable is State Wins, a dummy variable for the case being ruled in favor of the State. Retirements in 2010 is fraction of mandatory retirements in a given district bench in reform year 2010. Post 2010 is a dummy for post-reform period. The controls include all case and district characteristics shown in Table 1. The case controls also include case-type fixed effects. Every column excludes cases one-by-one for the mentioned political capital. *** p<0.01, ** p<0.05, * p<0.1.

Table C12: Impact of Selection Reform on State Wins with different starting years

	State Wins			
	(1)	(2)	(3)	(4)
	1990-2016	1995-2016	2000-2016	2005-2016
Retirements in 2010 X Post 2010	-0.207**	-0.208**	-0.233**	-0.264***
	[0.0780]	[0.0847]	[0.0912]	[0.0984]
District-Bench and Year FE	Yes	Yes	Yes	Yes
District and Case Controls	Yes	Yes	Yes	Yes
Observations	6,772	5,827	4,775	3,630
R-squared	0.071	0.074	0.082	0.097
Mean of dependent variable	0.501	0.498	0.492	0.471

Robust standard errors appear in brackets (clustered at district-bench level). The dependent variable is State Wins, a dummy variable for the case being ruled in favor of the State. Retirements in 2010 is fraction of mandatory retirements in a given district bench in reform year 2010. Post 2010 is a dummy for post-reform period. The controls include all case and district characteristics shown in Table 1. The case controls also include case-type fixed effects. Column (1) excludes from our sample cases decided from 1986-1989, column (2) excludes all cases decided from 1986-1994, column (3) cases decided from 1986-1999 and so on. *** p<0.01, ** p<0.05, * p<0.1.

Table C13: The Effect of Reform on State Wins on aggregated district-bench-time panel

	OLS		IV, 2nd Stage	
	(1)	(2)	(3)	(4)
	State Wins			
Appointments in 2010 X Post 2010	-0.208*	-0.210**	-0.219**	-0.206**
	[0.105]	[0.0869]	[0.103]	[0.0843]
District-Bench and Year FE	Yes	Yes	Yes	Yes
District and Case Controls	No	Yes	No	Yes
Observations	1,291	1,291	1,291	1,291
R-squared	0.173	0.197	0.173	0.197
Mean of dependent variable	0.503	0.503	0.503	0.503

Robust standard errors appear in brackets (clustered at district-bench level). The dependent variable is State Wins, averaged by district-bench-year. Retirements in 2010 is fraction of mandatory retirements in a given district bench in reform year 2010. Post 2010 is a dummy for post-reform period. The controls include all case and district characteristics shown in Table 1. The case controls also include case-type fixed effects. The dependent variable is aggregated at district-bench-year level, the level of variation of Appointments x 2010 X Post 2010. This is instrumented with Retirements in 2010 X Post 2010. *** p<0.01, ** p<0.05, * p<0.1.

Table C14: The Impact of Selection Reform on State Wins – Non-Linear Models

	Logit Marginal Effects		Probit Marginal Effects	
	(1)	(2)	(3)	(4)
	State Wins			
Retirements in 2010 X Post 2010	-0.151	-0. 205***	-0.152*	-0. 204***
	[0.092]	[0.0755]	[0.091]	[0.075]
	p-value=0.101			
District-Bench and Year FE	Yes	Yes	Yes	Yes
District and Case Controls	No	Yes	No	Yes
Observations	7,437	7,437	7,437	7,437
R-squared (Pseudo)	0.041	0.047	0.041	0.048
Log-likelihood	-4942.42	-4908.88	-4942.15	-4908.68
Mean of dependent variable	0.502	0.502	0.502	0.502

Robust standard errors appear in brackets (clustered at the district-bench level). The dependent variable is State Wins, a dummy for the case being ruled in favor of the State. Retirements in 2010 is fraction of mandatory retirements in a given district bench in reform year 2010. Post 2010 is a dummy for post-reform period. The marginal effects from the corresponding Logit and Probit regressions are reported. The controls include all the case and district characteristics in Table 1 and case-type fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table C15: The Impact of Selection Reform on State Wins at Different Levels of Clustering

	Before-After Clustering District-Bench	Before-After Clustering District	District-Level Clustering	Bootstrap Clustering
	(1)	(2)	(3)	(4)
	State Wins			
Retirements in 2010 X Post 2010	-0.202***	-0.202**	-0.202*	-0.202***
	[0.0575]	[0.0792]	[0.104]	[0.0750]
District-Bench and Year FE	Yes	Yes	Yes	Yes
District and Case Controls	Yes	Yes	Yes	Yes
Observations	7,439	7,439	7,439	7,439
R-squared	0.064	0.064	0.064	0.064
Mean of dependent variable	0.503	0.503	0.503	0.503

Robust standard errors appear in brackets. The first and second column clusters within each district bench and district separately before and after the 2010 reform, respectively. The third column clusters at the district level. The fourth column uses bootstrap clustering as per Ng et al. (2013). For wild bootstrap clustering that imposes a small cluster correction a la Cameron at al. (2008), see Figure C3. The controls include all the case and district characteristics in Table 1 and case-type fixed effects. *** p<0.01, ** p<0.05, * p<0.1