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Montesquieu or the Federalists?**

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The impact of Presidential appointment of judges: Montesquieu or the Federalists?

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A central question in development economics is whether there are adequate checks and balances on the executive. This paper provides causal evidence on how increasing constraints on the executive –via removal of Presidential discretion in judicial appointments– promotes rule of law. The age structure of judges at the time of the reform and the mandatory retirement age law provides us with an exogenous source of variation in the termination of Presidential discretion in judicial appointments. Overall, the results indicate that Presidential appointment of judges deteriorates rule of law. (JEL D02, O17, K11, K40).

Keywords: President, judges, property rights, judicial subversion, expropriation risk.

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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*]

In 70% of countries worldwide, it is the President who appoints judges to the courts (CIA World Factbook, 2021). This makes it one of the most widespread institutions in the world. Proponents of Presidential appointment argue that this institution secures independence of the judiciary, especially when combined with life-time appointment of judges (Madison, Hamilton, and Jay, 1788). However, others like Montesquieu (1778), worry that Presidential appointment may result in judicial subversion by the government’s executive branch, undermining separation of powers and rule of law.¹

This paper contributes to this debate, by investigating the effect of removing Presidential discretion in judicial appointments on pro-government rulings and decision quality. In 2010, a sudden and unique institutional reform in Pakistan dramatically changed the judge selection system from Presidential appointment with life-time tenure (like Singapore or Brazil) to judicial commission-based selection, which involves appointment by peer judges (like Sweden or the UK). This natural experiment provides an opportunity to understand how removal of Presidential discretion in appointment of judges impacts judicial decision-making. We ask whether this switch from Presidential appointments to appointment by judge peers affected judicial independence 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 about 8500 cases from 1986 to 2019. Our measure of executive influence over the Judiciary is a judicial-dependence dummy variable “State Wins”, taking value 1 for “State

¹ This debate has also attracted considerable attention in economics. North (1990, p. 3) defined institutions as “humanly devised *constraints* that shape human interaction” and Acemoglu et al. (2001) in their influential study, cite this definition, to motivate the use of “constraints on the executive” as their central measure of institutions (see also discussion of importance of checks and balance as key for development in La Porta et al., 2004, Rodrik et al., 2004 and Acemoglu et al. 2020)

victories” and 0 for “State losses” in cases where the State is a party. Following the literature, we asked legal experts at a Law firm to code this variable (as in Djankov et al., 2003; La Porta et al., 2008). Coding was performed by two independent teams of legal experts, each supervised by a senior lawyer specializing in cases involving the government.

The Pakistani government is a party in a wide range of judicial cases, from tax disputes to blasphemy, suppression of political rights, and the constitutionality of Military Rule. However, the most common cases concern land expropriation by the government, totaling 40% of all petitions filed in the High Courts.² Indeed, in many developing countries, property is vulnerable to outright seizure by the government (Shleifer, and Vishny, 2002; Ali et al, 2014; Somin, 2015; Behr et al., 2021). According to the World Justice Project, about 60% of respondents in developing countries say that it is “unlikely” or “very unlikely” that homeowners will “be fairly compensated by the government” if the “government decides to expropriate their property”.

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 Pakistani Court presided over by judge peer appointees ordered the Karachi Development Authority to return 35,000 “public encroachments” to their owners (The News, 2017). Likewise, 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.

Figure 1 generalizes the anecdotal accounts of less-favorable rulings for the State following the 2010 reform to about 8500 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 over-time variation: there is a sharp fall in State Wins, precisely after Presidential discretion in judicial appointments is removed (Panel B). This, however, 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

²By 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).

and in 2010 the President's power to unilaterally terminate the legislature was also removed from the constitution. The overall fall in pro-government rulings following the selection reform could be explained by any of these changes. Similarly, a simple difference-in-differences estimate of the fraction of judges appointed by the judicial commission in each district court may not yield the causal effect of the reform, due to potential reassignment of judges across districts or strategic appointment of judges across districts. Indeed, Iyer and Mani (2012) show that the reassignment power of Indian politicians allows them to exert substantial control over bureaucrats. There is a legitimate concern here that an independent judge in Pakistan might be reassigned to a different district or that judicial-commission appointments might be made strategically.

We address these concerns 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. Both before and after the reform, Pakistani law made retirement at age 62 mandatory for judges.³ However, different High Court benches had different numbers of vacancies arising from mandatory retirements in the 2010 reform year. For instance, the property bench of district Rawalpindi had no vacancy arising from mandatory retirements in 2010, while the property bench of Peshawar district did. Simply put, our design compares pro-government rulings in district benches where judges turn 62 pre-reform and are replaced by the President (control group) with district benches where judges turn 62 post-reform and are replaced by judge peer appointees (treatment group). More precisely, our reduced-reform difference-in-differences framework compares pre- and post-reform government victories in district benches experiencing low versus high mandatory retirements in 2010 as determined by the age structure of judges in 2010. The district benches with a lower fraction of mandatory retirements in 2010 serve as a counterfactual to district benches with a higher fraction of mandatory retirements in 2010.

We find that Presidential appointment of judges substantially affects judicial decisions: a 10% rise in judges selected by the judge peers reduces State Wins by about 2 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 improvement in decision quality: peer appointees have lower case delays, are more likely to rule on case facts relative to legal lacunas and have higher ratings on due process followed than Presidential

³ Retirement of High Court judges at age 62 is also common in other large developing countries, for instance, India. This is likely to be due to combination of lower life expectancy and British colonial legacy (Siddique, 2013). Taking just the case of Pakistan and India where High Court judges must retire at age 62 implies that this exact institution affects at least 20% of the world population or about 1.5 billion people worldwide.

appointees. The reduced pro-government rulings also do not come at the expense of increased pandering to religious sentiments on the street or judicial subversion by large corporations. If anything, judges appointed by their peers rule in favor of the government *more* often when the State is opposing large corporations, which suggests that reduced pro-government rulings are also unlikely to result from corporate capture.

Two 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* the 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 uninformative of government victories. Second, we may be confounding the effect of the selection reform with differential prior trends among benches with high versus low mandatory retirements. However, we find no evidence of the district benches with more vacancies being on different trajectories before the reform. This is consistent with qualitative accounts indicating that the selection reform was unanticipated, framed by a secret parliamentary committee and implemented in a "hurry" to prevent potential sabotage (Almeida, 2018). Similar no effect of pre-treatment mandatory retirements and lack of evidence for differential pre-trends are found for other judicial outcomes proxying for decision quality.

We next explore the *likely* mechanism of type of judges driving the results. In particular, we ask what is peer appointments selecting for? We find that President-appointed and peer-appointed judges are similar along many characteristics such as age at appointment, ethnicity, religion and gender. That is, peer appointment is not selecting based on seniority or ethnicity. Nevertheless, judicial-commission appointees are about 35% less likely to have run for political office prior to their judicial appointments. This corroborates anecdotal accounts that the judicial commission chooses more meritocratic and bi-partisan judges relative to Presidential appointments (Zafar, 2012; Arshad, 2017; Alam, 2021). We also find not much evidence for spillover effects: peer-appointed judges do not impact the behavior of President-appointed judges, suggesting that incentives facing old-regime Presidential appointees remained unchanged post-reform.

Finally, we examine the heterogeneity by case-types and find that politically salient constitutional cases involving land expropriations and political-rights disputes with the State

are essentially driving our results. These are cases where government expropriation of valuable tangible resources (such as land) and intangible resources (such as political rights) is at stake. The expropriation of private property by the government opens the opportunity to conduct a back-of-the-envelope calculation to approximate 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 compute that the selection reform likely prevents land expropriations worth 0.14% of GDP, or USD 390 million yearly. In other words, our computations suggest 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 roughly equivalent to the entire federal government's expenditure on health care in 2019.

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. The results hold regardless of the President or Chief Justice in office. We also find no evidence for strategic case filing mechanism whereby government responded to the selection reform by strategically allocating cases, or that the Chief Justice differentially constituted judicial benches post-reform. This is consistent with *de jure* random allocation of cases, substantial sanctions for "forum shopping" i.e., litigants choosing a specific judge and the unanticipated implementation of the reform (Haq, 2018; Shah, 2020). The results of balance tests and no evidence for change in number of cases filed also corroborates this view. We conduct a number of additional sensitivity tests showing that the results are not confined to a particular district or a specific bench. The results are also robust to alternative coding of judicial outcomes, non-linear estimations, or different levels of aggregation and clustering.

This paper relates to several strands of literature. First, it speaks to the literature on institutions and development, particularly the studies emphasizing the importance of checks and balances on executive power (North, 1990; Acemoglu et al., 2001; Shleifer and Vishney, 2002; La Porta et al, 2004; Rodrik et al., 2004; Acemoglu et al., 2013). We contribute to this literature by showing how executive control over the judiciary and expropriation of private property by the government sharply reduces when one of the world's most ubiquitous institution, Presidential appointment of judges, is discontinued. We hence provide empirical support for the theory and mechanisms behind many of these seminal studies.

Second, the paper speaks to the vibrant literature on state effectiveness and bureaucracies in developing countries, especially in "fragile" or weakly institutionalized settings (Besley and Persson, 2009; Fujiwara and Wantchekon, 2013; Jia et al., 2015; Colonnelli et al., 2020; Acemoglu et al., 2020; Bandiera et al., 2020; Callen et al., 2020; Gulzar and Khan,

2021).⁴ 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 fostered in a country where democratic institutions are weak to begin with. Third, we contribute to the extensive cross-country literature on courts (Djankov et al., 2003; La Porta et al., 2004; Voigt, 2008; Palumba et al., 2013; Boehm, 2015; Bielen et al., 2018; Chemin, 2020). By drawing on variation across judicial benches subject to the same national institutions, we overcome many of the common identification issues arising in work exploring differences between countries. We, therefore, complement the pioneering studies of courts at the subnational level (Ponticelli and Alencar, 2016; Lambais and Sigstad, 2021; Behrer et al., 2021). However, by comparing judicial cases across judicial benches *within* a district –via district-by-year fixed effects– we are able to account for both time-invariant and time-varying determinants of judicial decision-making something hitherto not possible.

Last, our work is related to the literature on judge behavior. Most of this literature has focused on judge behavior in criminal cases (Lim, 2013; Chalfin and McCrary, 2017; Silveira, 2017; Cohen and Yang, 2019; Ash et al., 2021), 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), temperature (Heyes et al., 2019) and religion (Mehmood et al., 2021). 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 the background and institutional details. Section III describes the data, while Section IV outlines the empirical methodology. Section V presents and discusses the main results, and Section VI explores the mechanisms behind them. Section VII presents a heterogeneity analysis of cases, while in Section VIII, we rule out alternative explanations and detail a battery of robustness checks. Section IX concludes. Further information on data construction, variable descriptions, and additional robustness checks is in the Appendices.

⁴Other related works include Jones and Olken (2005), Lim (2013), Hessami (2018) and Ash and MacLeod (2019).

II. BACKGROUND

A. Contextual Details

Judicial Structure. —The judiciary in Pakistan has 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 High Courts. In these High Courts, an individual can file a case against 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). Last is the final appellate Court, the Supreme Court of Pakistan, located in the federal capital that hears 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 (Siddique, 2013).

High Courts and Specialized Benches. —The focus in this study is on the 16 High Court divisional benches that adjudicate over cases involving the government. If the government expropriates land or violates a political right, the High Court is the first, and in most cases the only platform offering remediation to individuals and firms. There are four provincial High Courts and one federal High Court in Islamabad. Each High Court has roughly 4 “divisional” benches, totaling 16 district or divisional High Courts in Pakistan. Figure C1 in Appendix C shows their locations and respective jurisdictions. Judges within these 16 divisional courts are allocated according to a roster of sittings that designates the judge’s specialty within the divisional court. In cases involving the State, the focus of our study, the judges serve on one of 4 specialized judicial benches within each divisional court: 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. Cases are randomly assigned within the specialized bench according to caseload and judge specialty, using a computerized case-flow management software. When a vacancy on a specific district bench arises, it is always filled by a judge with the same expertise or specialty. The specialty is determined a priori through consultation between the provincial chief justice and judge in question and is officially announced via the “roster of sittings” (Kureshi, 2020; Alam, 2021). From 1986 to 2019, about 70% of all cases filed in the High Courts were “constitutional petitions”, the majority of which involved government responding to land-expropriation claims from the citizenry.

Judge 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 in Pakistan. 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 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).⁵ Article 195, instituting security of tenure for High Court judges via mandatory retirement at age sixty-two, and Article 209, stipulating that judge can only be removed by filing a reference to their peers, was same as before the reform (Constitution of Pakistan, 2010). That is, the constitutional amendment only changed the procedure to select judges, the judges’ security of tenure, mandatory retirement law and powers resting with the judges remained unchanged.

Peer Appointment System. —The peer appointment system selects judges through majority voting. The judicial commission considers senior lawyers’ and lower court judges’ candidacy for High or Supreme Court judgeships based on their “reputation for impeccable integrity” (Alam, 2021). When a vacancy for a judge arises, the Chief Justice of Pakistan, who heads the judicial commission, convenes the commission to deliberate on potential candidates for “elevation to the bench”. Supreme Court judges are selected by a commission consisting 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). For the appointment of High Court judges, the focus of our study, additional members from the Court where the judge is to be appointed are included: this includes the provincial Chief Justice, the provincial Law Minister, the most senior judge of the High Court (after the provincial Chief Justice), and a lawyer nominated by the provincial bar council. In addition to this formal procedure laid out in the constitution, there are some informal norms that govern the operation of judicial commission. A key norm that

⁵The 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.

has been respected since the inception of peer appointment system is that for selection of High Court judges, the chief justice of the State where vacancy arises, recommends a list of potential candidates for the commission to vote on. This is based on the idea that the Chief Justice of the state is more likely to be privy to the institutional and contextual needs of the district High Court. The judicial commission can, in principle, reject the provincial chief justice's list of candidates although it is an informal norm to at least allow the vote for the suggested candidates considering the "stature" of the provincial chief justice (Alam, 2021).

The peer appointment system became operational in April 2010, when the Supreme and High Court judges began to be appointed by the judicial commission consisting of peer judges and senior lawyers, with no Presidential involvement. The appointment power of the executive was likely curtailed by this reform, as judges constitute the overwhelming majority of the commission. The 18th amendment also created a parliamentary committee consisting of four members from the government and four from the opposition. Nominations by the judicial commission had to be debated within the parliamentary committee, although it was never able to exercise its authority since the judicial commission could veto the parliamentary committee's objections without providing any explanation.⁶

B. *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 takeovers, thus shielding the country against "extra-constitutional" military takeovers (Kureshi, 2020).⁷ Pakistani politicians therefore hoped to take shelter behind "constitutional protections" from military take-overs by ensuring that judges were more independent, even though this reduced their own effective control over the judiciary. Moreover, it was hoped that reducing Presidential discretion over selection of judges would prevent abuses of power by future autocratic rulers

⁶ This was not in the original 18th amendment but was incorporated, 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).

⁷ Article 6 of the Constitution of Pakistan states military takeover is "abrogating the constitution" and "high treason punishable by death". Yet, courts have never been able to enforce this clause in the constitution and convict military rulers.

subverting the courts to imprison opposition politicians and violate fundamental rights (Sattar, 2012; Zafar, 2012).

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 system of Pakistan. Further discussion of the reform, the political context and the history of the Courts in Pakistan can be found in Appendix B.

III. 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 8500 cases – conditional on the State being one of the parties– from 1986-2019 for 64 High Court benches (from the universe of all cases, with the government, decided in this period). The case-level data was successfully matched with judge and district bench characteristics from judicial administrative data for 8446 cases out of the 8500. The random sample is *conditional* on State being one of the defendants. Therefore, our sample only contains cases involving the State vs the Citizen. This includes criminal cases and more politically salient constitutional cases. Based on judgment texts coded by two independent teams of legal experts, government victories were ascertained at the case level. Cases may be decided by a group of judges i.e., a “double bench” (consisting of 2 judges), a “full bench” or “divisional bench” consisting of all judges within a division (about 7 judges), or by a “single-member bench” where one judge adjudicates alone. About 90% of the cases in the High Courts are decided by a single member bench, making case- and judge-level comparisons roughly equivalent (Arshad, 2017; Haq, 2018; Alam, 2021).⁸ The government wins the case about 50% of the time and there are about 7 judges in each of the 16 divisions shown in Figure C1. Table 1 shows the descriptive statistics of the variables used in the study. 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

⁸ Our empirical method also does not hinge on a one-to-one case-to-judge matching since we examine the impact of selection reform via comparing relative probabilities that case was adjudicated by a peer appointee.

the case. Following the literature (e.g. Djankov et al., 2003 and La Porta et al., 2008), we asked a Law firm to code this variable. We entered into contract with two independent teams at a prominent law firm in Lahore, with each team consisting of 5 legal experts (4 junior lawyers and a supervising senior attorney) coding the “State Wins” dummy variable as 1 if the State as a litigant won a dispute and 0 otherwise. 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). Figure 1 provides evolution of State Wins over time, while Figure C2 provides district-wide averages. In both instances, we observe a sharp fall in State Wins in the reform year of 2010. For the analysis of the quality of judicial decisions, we use four additional outcome variables: case delay, merit, correct decisions and process followed, where the unit of observation is also at the case level. These variables too are constructed from the information in the text of the judgment orders. Case Delay is calculated as the difference between the case decision and filing years. This variable is most straightforward to code since it only requires reading filing and decision years off the judgement text. Merit is a dummy variable that takes the value of 1 if the decision is “based on evidence or case merits” and 0 if it is based on a technicality. There are two reasons for constructing this variable. First, legal scholarship in Pakistan suggests that ruling on technicalities is a “weapon of choice to rule unfairly” and that judges use decisions on technicalities or legal lacunas 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). We also, however, cross check this result by actually asking legal experts to categorize judicial decisions as “correct” vs “incorrect”. Process followed is a discrete variable representing a quality rating on due process followed for each judicial case. Specifically, the legal experts were asked to rate on a scale of 1 to 5 the quality of the judgment order, taking into account the extent to which “all relevant jurisdictional, procedural, and evidential requirements” were accounted for. This variable approximates the “correctness” of the due process followed in reaching the judicial decision.⁹

⁹Two independent teams coded each of these outcome variables and the correlation coefficient between them appears in Table C1 of Appendix C. Data from Team 1 is used in the paper, but the very high correlations between the two codings implies using either data would give similar results. This checks out in Table C20 of Appendix C where we find the results to be essentially identical if we instead used Team 2’s coding.

Main Explanatory Variables. — A key explanatory variable used in the analysis, $\frac{\text{Mandatory Retirements in 2010}}{\text{Total Judges}} \times \text{Post 2010}$, 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 records obtained from the Registrar Offices of the High Courts. These two sources are also used to construct $\frac{\text{Appointments in 2010}}{\text{Total Judges}} \times \text{Post 2010}$. 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, 11.5% of judges reached their mandatory retirement age and 8% of these vacancies were filled by peer-appointed judges (as can be seen in Table 1, Panel B). Since there is very strong correlation between mandatory retirements and new judicial appointments (both before and after the reform), we report as baseline the results with mandatory retirements. Moreover, when a vacancy on a specific district bench arises, it is always filled by a judge with the same expertise or specialty, a pattern reflected in Figure 2 where we observe mandatory retirements and new judicial commission appointments are almost perfectly correlated within district benches.¹⁰ This implies 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.¹¹ Although, given the high correlation, the reduced form or *Intention-to-Treat* estimate is by construction close to the 2SLS estimate.

Controls: Case, Judge, and District Bench Characteristics. — We rely on a combination of judgment texts and judicial administrative data to construct the case, judge, and district-bench 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 or “Eminent Domain” cases) and so on.

¹⁰91% 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%). Moreover, the fraction of retirements is always larger than the fraction of judicial-commission appointments. This is because not all mandatory retirements in a judicial bench are accompanied by new appointments in the same year, although the correlation is very strong (0.9).

¹¹The 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.

Table C1 in Appendix C lists the means of the outcome variables, the case characteristics, and the corresponding correlation coefficients between these variables across the two teams of attorneys that coded them. Judge and bench characteristics are obtained from the judicial administrative records of the Registrar Offices of the High Courts of Pakistan (Table 1, Panel C and D). This includes information on judges' gender, ethnicity, religion, 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 and bar association records. Combining the data from these sources gives us information on 8446 cases and 511 judges across 64 district High Court benches of Pakistan.

IV. EMPIRICAL METHOD

A. First Specification

Our first empirical methodology uses 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. The corresponding specification is as follows:

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

The subscripts c, j, d, b and t index cases, judges, districts, benches, and years respectively. Y denotes State Wins and judicial outcomes proxying decision quality. $\frac{\text{Mandatory Retirements in 2010}}{\text{Total Judges}}$ 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 strong correlation between mandatory retirements and new judicial appointments. β_{dt} is district-by-year fixed effects, and \mathbf{W}'_{cjdabt} is a vector of case and district bench controls as shown in Table 1.

Since our identifying variation comes from 64 district benches (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 in specification (1) 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 in 2010 and is predetermined at the time of judges' appointment. Second, anecdotal accounts suggest that the selection reform was unexpected and unrelated to specific district benches' dynamics (Almeida, 2018). These two factors mean that the vacancies arising from mandatory retirements across district benches in 2010 are likely to be uncorrelated with determinants of judicial decision-making.

In the results section of the paper, we provide two additional pieces of evidence supporting this identification assumption. First, 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. Second, we provide evidence against differential pre-trends and show that judicial outcomes in district benches that had more mandatory retirements were not on differential trajectories prior to the implementation of the selection reform. There is, however, a sharp effect of mandatory retirements on judicial outcomes precisely at the time of the reform. This strongly suggests that the pattern of judicial appointments due to mandatory retirements and rulings did not change in the years leading to the selection reform, and it is consistent with the reform's unexpected introduction.

B. *Second Specification*

The first specification uses cross-sectional variation across district benches—before and after the reform—to provide plausibly exogenous variation in the implementation of the selection reform. There is, however, a concern that this neglects potentially useful variation in the implementation of the reform arising from differential peer appointments occurring *after* the 2010 reform year. Therefore, we propose a second specification that utilizes all available cross-district bench and over-time variation in the implementation of the reform to estimate the effect of judicial-selection reform on judicial outcomes. Specifically, we estimate the following Linear Probability Model by OLS and 2SLS:

$$Y_{cjdbt} = \theta + \alpha \left(\frac{\text{Cumulative Commission Appointed Judges}}{\text{Total Judges}} \right)_{dbt} + \beta_{at} + \mathbf{W}'_{cat} \boldsymbol{\varphi} + \varepsilon_{cjdbt} \quad (2)$$

The dependent variable, subscripts, controls are identical to specification (1). $\frac{\text{Cumulative Commission Appointed Judges}}{\text{Total Judges}}$ is the cumulative fraction of judges appointed by the judicial commission from 2010 to 2019. This variable is instrumented by the cumulative fraction of judges expected to reach their mandatory retirement age of 62 in each district bench from 2010 to 2019, as determined by the predicted trajectory of mandatory retirements in 2010 (Figure C3 in Appendix C shows how these variables evolve overtime).

C. Discussion of the Two Specifications

This section presented the two key specifications estimated in the paper. The first one exploits variation due to mandatory retirements in only the 2010 reform year. It, however, may be imprecisely estimated since it does not exploit post-2010 variation in the reform implementation. The second specification provides gains in precision by also utilizing variation in implementation of the reform post-2010. However, two comments are in order. First, the explanatory variable of interest in specification (2) necessarily follows an upward trend post-reform, raising the possibility of confounding the effect of the selection reform with district bench-specific over-time trends. In contrast, specification (1) relies only on cross-sectional variation arising due to mandatory retirements in 2010. As a result, the explanatory variable in equation (1) does not follow an upward trend post-reform by construction (as can be observed when comparing Figure 2 and Figure C3). Second, specification (1) allows for a more transparent examination of pre-trends by allowing us to examine if district benches that had more mandatory retirements in 2010 are on differential trajectories before the implementation of the reform. Importantly, however, the effects of selection reform are very similar across both specifications.

V. MAIN RESULTS

A. Effect of judicial selection reform on State Wins

Table 2 (Columns 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 specification with only district-by-year fixed effects.¹² The second column adds all the available case and district bench characteristics and estimates specification (1). The estimates imply that if 10% of the judges retired in 2010, State Wins would be about

¹² The availability of case-level micro data is particularly helpful here since it allows us to flexibly account for district court and time effects, that have not been possible in many other important works on courts (see e.g. Ponticelli and Alencar, 2016).

2 percentage points lower post-reform. This is equivalent to a 4% decrease over the sample mean. In Table C2 of Appendix C, instead of estimating the reduced form relationship, we estimate the effect of judicial-commission appointments in 2010 by OLS and 2SLS, where we instrument appointments with mandatory retirements. The results are similar: the 2SLS estimates imply that a 10% rise in judicial-commission appointments reduces State Wins by about 3 percentage points.

We also obtain very similar results when we estimate specification (2), which exploits post-2010 variation in implementation of the reform. These results are reported in Table 3. There is evidence of a large and statistically significant negative effect of the selection reform on State Wins. Panel A shows the OLS and IV (second-stage) results, while Panel B presents the corresponding first stages. The first column of panel A corresponds to the OLS specification, with district-by-year fixed effects. Column 2 of Panel A adds all the available case and district bench characteristics. In column 3, we instrument the fraction of commission-appointed judges by the fraction of predicted trajectory of retirements based on mandatory retirements in 2010. Column 4 adds the available case and district bench controls to this IV specification. The OLS estimates are smaller than the 2SLS estimates, suggestive of a possible downward bias in OLS estimates. This would occur if new judicial-commission appointments were made strategically in the most independent districts with relatively low State Wins. In contrast, if a judicial-commission judge is randomly assigned to a typical district bench, she is more likely to decrease State Wins than a judge assigned to an independent district bench, where most cases have low State Wins to begin with.

We should, however, be cautious in this interpretation, since the effect size of 2SLS estimates is only about 25% larger than OLS estimates, and new judicial appointments and mandatory retirements are very strongly correlated. The 2SLS estimates imply that a 10% rise in judges appointed by judge peers reduces State Wins by about 2.5 percentage points, a 5% decrease over the mean dependent variable. The first stages of the 2SLS estimates are reported in Panel B of Table 3. We find the instrument to be a strong predictor of the fraction of judges appointed by the judicial commission, with the F-statistic well above the critical value of 23 derived in Montiel, Olea and Pflueger (2013) for 10% potential bias, 5% significance, and clustered standard errors. Overall, the estimates presented in Table 2 and Table 3 paint a consistent picture that the selection reform reduced State Wins. A 10% increase in peer-appointed judges reduces State Wins by about 4-5% over the sample mean.

B. *Effect of judicial selection reform on decision quality*

We begin our investigation of decision quality by examining how the selection reform impacted case delay. Several influential studies argue that reduced delay in courts capture judicial efficiency and is associated with a reduction in court congestion (e.g. Djankov et al., 2003; Ponticelli and Alencar, 2016). Column 1 of Table 4 reports the results on impact of selection reform on case delay. The estimated coefficient implies that retirement by 10% of the judges in 2010 would reduce case delay by about 1 month post-reform. This is equivalent to about 3% reduction over the sample mean and suggests that selection reform may have increased court efficiency.

It may be reasoned, however, that shorter case delay following the reform reflects reduced deliberation on cases, implying poorer-quality judicial decisions. Nevertheless, three additional pieces of evidence contradict this conjecture. First are the results for cases decided “on merits”. In Common Law jurisprudence, rulings on merit imply that the judicial decision is “based on evidence rather than technical grounds” (Pound, 1963). We constructed this 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 legal experts, follows the argument by Pakistani legal scholars that ruling on technicalities is a “weapon of choice to rule unfairly” (Siddique, 2013) and “symptomatic of a biased decision” (Arshad, 2017). 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 decisions based on evidence: if 10% of judges retired from their respective district benches in 2010, merit decisions would increase by about 2 percentage points (Table 4, Column 2). This is equal to a 3% increase over the sample mean. These results are consistent with scholarship as well as our discussions with legal experts (senior lawyers and judges) in Pakistan who suggested that decision on “merits would approximate correct” or “unbiased” judicial decisions (Siddique, 2013; Alam, 2021). This checks out quantitatively when we directly code the judicial decisions along this dimension i.e., via a dummy that switches on for ‘correct’ judicial decisions. Column 3 of Table 4 reports these results. We find that a 10% increase in judges retiring in the reform year increases ‘correct’ decisions by 1.91 percentage points.

Finally, we ascertain whether the fall in State Wins also coincides with better observance of “due process”. Several legal scholars argue that a higher-quality judicial decision should not only be “unbiased” or “correct” but also needs to follow due process of law (see for instance seminal treatment of this issue in Dahl, 1957). To proxy for due legal process followed, we leverage legal experts’ ratings for each judicial case on observation of “relevant

jurisdictional, procedural, and evidential requirements”. This is a proxy 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 reaching the judicial decision. Estimating equation (1) with this dependent variable, indicates 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 on a 5-point scale (Table 4, Column 4). This is equivalent to about 1.3% increase in rating over the sample mean. Taken together, the results from Table 4 –of the selection reform reducing case delays, increasing following of due process of law, meritorious and correct decisions– all corroborates the view that peer-appointed judges issue higher-quality judicial decisions relative to rulings by Presidential appointees.

C. Threats to Identification

We now examine two key threats to identification that could undermine the causal interpretation of these estimates. First, we may be confounding the effect of selection reform with differences in behavior towards the State of old and new judges – a pure appointment effect. Table 2 (Columns 3, 4 and 5) presents evidence against this hypothesis by showing that pre-reform, mandatory retirements have 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 indicates that if there is a pure appointment effect, it is likely to be small and perhaps go in the opposite direction with new appointees more likely to issue pro-government rulings. Similar results are obtained for the decision quality variables with pre-treatment retirements uncorrelated with all four of our available decision quality outcomes: in Table C3 of Appendix C, we show that pre-reform mandatory retirements have no impact on case delay, due process followed, merit or correct decisions.

The findings of a significant effect of mandatory retirements in the selection reform year and no effect of pre-reform mandatory retirements are robust to different specifications (such as combining pre-treatment and treatment variables in a single regression (Table C4 in Appendix C). They are also insensitive to estimating the cumulative retirement specification, where we also find no effect of pre-reform retirements (Table C5 of Appendix C). Only the reduced relationship between post-reform cumulative retirements and State Wins is negative and statistically significant, while pre-reform cumulative retirements are uninformative of State Wins.

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 following different trajectories *before* the reform. Therefore, to systematically examine pre-trends, we estimate the following specification:

$$Y_{cjdbt} = \beta_0 + \sum_{s=1986}^{2019} \alpha_s \left(\frac{\text{Mandatory Retirements in 2010}_{db} \times \delta_s}{\text{Total Judges}_{db}} \right)_{dbt} + \beta_{dt} + W'_{cjdbt} \boldsymbol{\varphi} + \varepsilon_{cjdbt} \quad (3)$$

where Y is a given judicial outcome, 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 reform year of 2010. District-by-year fixed effects and controls are identical to the specifications (1) and (2). Equation (3) is conceptually 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 and investigate whether the selection reform had a persistent effect.

Figure 3 (and its corresponding table-form representation in Table C6 of Appendix C) presents the results from estimating equation (3). We find that district benches with a higher fraction of mandatory retirements in 2010 show no change in State Wins prior to 2010, although there is a sharp decrease in State Wins post-reform. The effect is likely persistent as indicated by qualitatively and statistically significant effect of the selection reform observed post-2014. Similar evidence is found when we estimate equation (3) using the available decision quality measures. Figure 4 present these results for case delay (Panel A), merit decisions (Panel B), correct decisions (Panel C), and due process followed (Panel D). We observe a sharp break post-reform and not much evidence of differential trends prior to the reform across all four of the outcomes. These results corroborate anecdotal accounts that the selection reform was an unanticipated shock to the judicial system of Pakistan (Almeida, 2018), and suggest that differential pre-trends are unlikely to explain our results.

VI. MECHANISMS

This section is organized into two brief subsections. The first one provides evidence consistent with the judge selection mechanism by documenting the judge characteristics correlated with the peer appointees. Second subsection provides lack of evidence for spillover or “peer effects” from judicial commission-appointed judges on Presidential appointees.

A. *What does peer appointment select for?*

First, we examine the key observed differences in the judges selected by judge peers relative to Presidential appointees. The evidence suggests selection effects –of judges with different characteristics selected under the two regimes– as the *likely* mechanism behind the impact of selection reform on government victories. In Table 5, we present correlates of peer appointees across a number of observed judge characteristics. We present here results at the judge level, although identical results are found if we instead ran case-level regressions (see Table C7 in Appendix C). Peer appointments are uncorrelated with judges’ gender, religion, previous employment, and ethnicity. Finding peer appointment uncorrelated with judge’s ethnicity is especially interesting, since it conflicts with concerns expressed in some quarters of Pakistan’s legal fraternity that peer appointment may select judges from specific ethnic groups i.e., from the politically powerful state of Punjab (Siddique, 2013). We find no evidence of this in the data. Ethnic discrimination in selection of judges is no greater under peer appointments for any major ethnic group in Pakistan.

Nevertheless, the two types of appointees differ in their observed political activity *prior* to their judicial appointments. Commission-appointed judges are about 35% less likely to have held political office in the Lawyers’ Bar Associations (Table 5, Column 10). As candidates for office in Pakistani Bar Associations often run on a political party platform, we consider this as a plausible proxy for political activity prior to judicial appointment. We also provide more evidence that judicial-commission appointees are significantly less likely to have run for political office in state or national elections before their judicial appointment.¹³ Specifically, we find that peer-appointed judges are also about 15% less likely to have run for elections in the provincial or national assembly prior to their judicial appointments (Column 11, Table 5). We interpret these results, in combination with anecdotal accounts that suggest that peer appointment system is likely to be more meritocratic and averse to selecting “political ideologues” (Zafar, 2012) or “partisans” (Arshad, 2017). Together, this suggests that selection of relatively apolitical judges by the judicial-commission is likely important in explaining our results.

B. *Do Peer Appointees have Spillover Effects on Presidential Appointees?*

In this subsection, we examine whether peer-appointed judges’ arrival induces spillover effects on Presidential appointees. To do this, we compare State Wins in district benches with more mandatory retirements in 2010 with those from district benches with fewer mandatory

¹³ Once appointed, judges are barred from running for political office until two years after their retirement.

retirements pre- and post-2010 *but only* for cases where all the judges are appointed by the President. Since these are the very places where peer-appointed judges are most likely to be appointed, given the strong correlation with new appointments, we can assess how Presidential appointees react to the arrival of judicial-commission appointees. Table 6 reports these results: estimates from Columns 1 to 4 paint a picture of no significant change in the behavior of old-regime judges with arrival of peer-appointed judges. These results suggest that peer effects are not likely to be large (if present at all) and that arrival of peer appointees is unlikely to have large impact on incentives facing old-regime Presidential appointees.

VII. Heterogeneity

This section is organized into three subsections. First, we discuss the heterogeneous effect of the reform by political saliency of the case. Second, we present results on a back-of-the-envelope calculation of the likely value of land expropriations avoided every year due to the institution of peer-appointment system. Last, we present evidence that peer appointment system does not have the unintended consequence of increasing pandering to religious sentiments on the street or facilitating corporate capture.

A. *Heterogeneity by Politically Salient Cases*

In this subsection we investigate the heterogenous effect of selection reform by political saliency of the cases. 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. In Columns 1 and 2 of Table 7 (Panel A) these results are reported: a 10% rise in judicial-commission appointees reduces State Wins by about 2.5 percentage points in constitutional disputes with the State. Likewise, in Columns 1 and 2 of Table 7 (Panel B), we disaggregate constitutional cases into those involving political rights and land expropriation by State: a 10% rise in judicial-commission appointees reduces State Wins by 2.3 percentage points in human rights cases and by 3.1 percentage points in cases involving land expropriation by the State. These results are also unlikely to be driven by differential trends prior to the reform, since limiting our sample to constitutional cases or their constituent land or political rights cases also reveals no evidence of pre-trends, although there is an unsurprising decrease in precision due to the smaller sample size as a result of the sample restriction (Figure C4 of Appendix C). Likewise, these results are robust to using the cumulative retirement specification (Table C8 of Appendix C).

This reduction in government victories in politically salient constitutional cases is corroborated by widespread qualitative accounts. For instance, expropriation of private

property by government or “outright takings” is considered a major problem in Pakistan and many developing countries (World Justice Project, 2020). Rulings in these ownership or expropriation disputes with the government are reported to be heavily influenced by political considerations (Abbasi, 2017; Sattar, 2017). Some legal scholars and lawyers 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*” (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). This is consistent with survey evidence from across the world with over 60% of respondents in developing countries fear expropriation of their private property by the State (World Justice Project, 2020). Similarly, human-rights or political 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 political rights, such as persecution of political opponents, limiting freedom of movement or discrimination based on gender, political affiliation, and caste. Typical examples include an opposition politician 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 (Naseer, 2018).

Further evidence of case-type heterogeneity by political saliency comes from a falsification test. As petty crime cases also involve the State (as prosecutor), but are politically less salient, we examine the impact of the selection reform on State Wins in criminal cases.¹⁴ These criminal cases mostly involve petty crime, vandalism, minor fraud, theft, and burglary, hence are low stakes politically. Results from Table 7 (Panel A, Columns 3 and 4) show that the selection reform has no significant 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. We interpret these results in light of the selection or judge heterogeneity mechanism, with peer-appointed – and relatively apolitical judges — making fewer rulings in the government’s favor in politically salient cases, while rulings in run-of-the-mill criminal cases are unaffected.

¹⁴ It should be noted that politically salient criminal cases, for instance those involving corruption of political opponents, are adjudicated in National Accountability Courts, whereas our analysis concern the High Courts that adjudicate over petty and violent crime cases in addition to the politically salient constitutional petitions against the government.

B. *Land Expropriations Prevented under the Selection Reform*

Our investigation of heterogeneous effects by type of cases 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 back-of-the-envelope approximation on the economic value derived from avoiding land expropriations due to peer-appointed judges. This is similar to the calculation by Mian and Khwaja (2005)'s computation of economy-wide costs of political connections using minimum and maximum bounds. Using property values taken from judgment orders, our point estimates on impact of selection reform, and total land cases decided in this period, we suggestively infer that the selection reform may have been able to prevent land expropriations worth 0.07 to 0.3 percent of GDP every year.¹⁵ This averages out 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 70% of Pakistan's federal budget for education or nearly the whole amount earmarked for health care in 2019.

C. *Heterogeneity by cases involving Islamic Law and Large Corporations*

We now present evidence that peer appointees are neither more likely to pander to religious sentiments on the street nor more amenable to corporate capture. First, we disaggregate criminal cases judged under Islamic Law from those judged under secular British common law. The Islamic cases judged under Islamic limits or "Hudood" law pertain to cases involving consumption of alcohol, blasphemy, adultery, homosexuality, fornication, and false accusation of fornication that are criminal offenses under Pakistan's Penal Code (PPC). The State Wins or convictions judged under Islamic law are reported in Panel B of Table 7 (Columns 4). The results are similar: peer-appointed judges are not significantly more likely to convict for violations of Islamic law than Presidential appointees.

Second, we investigate whether the reduced rulings in favor of the State due to the selection reform comes at the expense of increased rulings in favor of large corporations. Since

¹⁵ The calculation is made as follows: in 20% of our 8500 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 850,000. 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 2010-2019. We should, however, be cautious in interpreting these computations, since they rely on all Presidential appointees replaced by judge peers, while we use partial equilibrium estimates in these computations. For more details on these computations, see Table C9 in Appendix C.

about 15% of our sample involves the State opposing a firm rather than a citizen, we can test this hypothesis. Therefore, we examine the effect of selection reform in the subsample of cases involving small or medium enterprises as well as large firms. We consider a firm to be a large if it is associated with any of the 12 big business groups of Pakistan, “The Big 12” (Tech, 2020).¹⁶ Table 8 reports these results. Consistent with our main findings, we find that when peer appointments rise by about 10%, government victories against small and medium enterprises fall by about 2 percentage points (Columns 1 and 2). Nevertheless, in cases involving large corporations, the effect is reversed though marginally significant, possibly because of lower power due to the smaller number of large firms. Peer appointees are about 2.5 percentage points *more* likely to issue rulings in favor of the government when the State is opposing a large firm (Table 8, Column 4). This suggests an heterogenous effect of the selection reform: peer-appointees issue more pro-government rulings when State is opposing large corporations and are less likely to succumb to corporate capture. It should be noted, however, the evidence presented in the paper does not imply that peer appointment of judges is foolproof but rather judges selected by their peers are likely to issue better decisions across a suit of measurements, are no more amenable to corporate capture or pander to religious sentiments on the street than ones selected by the President.

VIII. ROBUSTNESS

A. *Robustness to Alternative Explanations*

This subsection 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 to competing explanations. These include the possibilities that our results are driven by an idiosyncratic President, a specific Chief Justice, or simply due to change in the type of cases adjudicated by the two types of judges.

President-Specific Effect.— 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, we can examine this claim empirically. That is, we can compare rulings by judges appointed by the judicial commission with rulings by judges

¹⁶ This includes all cases involving firms owned by large conglomerates. For instance, by Mian Muhammad Mansha’s Nishat Group, Byram Avari’s Avari Group, Razaq Dawood’s Descon Group, Mian Muhammad Latif’s Chenab Group, Mian Amir Mahmood’s Dunya News Group and Malik Riaz’s Bahria Town Group (see Tech, 2020 for the complete list).

appointed by different Presidents: we find no evidence that the effect of the selection reform is a President-specific effect (Table 9).

Chief Justice-Specific Effect.—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 (Arshad, 2017). As the Chief Justice of Pakistan is the head of the judicial commission, this alternative explanation is important to investigate. Since during our sample period seven different Chief Justices headed the judicial commission; we therefore test and reject the hypothesis that the results are driven by an idiosyncratic Chief Justice. These results are reported as Table C10 in Appendix C. No evidence that the selection reform is a Chief Justice-specific effect also suggests that the effect is unlikely to be confined to the tenure of Pakistan Peoples’ Party government that implemented the judicial selection reform. In fact, from Table C10, we can also observe that the selection reform is qualitatively and statistically significant at conventional levels *after* PPP is voted out of office in 2013 (Table C10, Column 5).

Strategic Case Filings. —Finally, an alternative mechanism might involve litigants responding to the selection reform by changing their case-filing behavior (Klein and Priest, 1984; Hubbard, 2013). We conduct two tests to investigate this possibility. First, we examine the differences in the type of cases decided in court. If strategic case-filings were driving the results, we would observe that cases adjudicated in district benches with more peer appointees to differ from cases adjudicated in district benches with less peer appointees. Results from Table 10 indicate this is highly unlikely where we observe that the selection reform is uncorrelated with a long list of case and district bench characteristics, suggesting that districts with more versus fewer mandatory retirements are adjudicating similar types of cases. This ‘balance check’ is robust to cumulative retirements specification: the selection reform is similarly uncorrelated with a long list of case and bench characteristics (Table C11 of Appendix C). These results also suggest that the Chief Justice endogenously constituting the bench or strategically assigning cases or specialties to judges is unlikely to explain the impact of selection reform on judicial decision-making. If the Chief Justice did strategically allocate cases, we would have observed the selection reform to be correlated with case characteristics. However, we find no evidence of this across both specifications. Second, we directly test for strategic case-filing behavior by investigating whether litigants change their filing behavior in response to the reform. We find that no evidence for total case-filing or case-filing in politically salient constitutional cases to be affected by the selection reform (Table C12 in Appendix C). We interpret these results in the light of recent evidence from Acemoglu et al. (2020) that

documents litigants in Pakistani courts to be unaware of reductions in case delay when they occur and that informing them about fall in delays increases their likelihood of using the formal courts. Together, these results suggest that selection reform did not induce a change in case-filing behavior.

B. Additional Sensitivity Tests

We carry out a number of additional robustness checks, results of which are presented from Tables C13 to Table C20 of Appendix C. First, we show that our results are not sensitive to excluding cases decided in federal and provincial capitals (Table C13). Courts in these capital judicial districts are called “principal benches” and some qualitative accounts suggest that the most politically salient cases are decided there (Haq, 2018). Nevertheless, our point estimates are very similar when we exclude cases decided in these political capitals. This supports the idea that the effect of reform is not geographically confined to cases decided in political capitals. Second, we show that our results are not driven by a specific judicial bench (Tables C14 and C15). 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 that we obtained access to. However, while going back in time allows us to gain precision, it may raise concerns regarding omitted variables. Therefore, we show that the results are robust to different starting years (Table C16). 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 or Logit (Tables C17 and C18, respectively). Fifth, we show in Table C19, that the results are robust to different levels of clustering: within each district bench separately before and after the reform (Bertrand et al., 2004), district-level clustering or using the wild bootstrap method for small number of clusters, as per Cameron et al. (2008).¹⁷ Finally, we show that essentially identical results are obtained across all our available judicial outcome variables when we use data coded by Team 2 (Table C20).

¹⁷The “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 C5 of Appendix C.

IX. CONCLUSION

The removal of presidential appointment of judges was described by a prominent legal historian and former judge S.M Zafar, as follows:

“Our judiciary or any other judiciary in the world should be independent to make correct decisions. We had provisions for this in the 1973 Constitution. Judges’ tenure was secure. But this strong judicial dispensation was suffering from a key ailment or a flaw of the induction of the judges into the judiciary. The previous procedure was very arbitrary, it was not only arbitrary, but it was noninstitutional, discretionary and as a result bad judges, sometimes political ideologues, sometimes crazies got into the judiciary. So, we thought that the judicial appointments be through a process, we institutionalized it. We took away the power, the discretionary power, the arbitrary power.”

He may have been right. This paper has shown that removing the power of judicial appointment from the President and transferring it to peer judges substantially reduces government victories and likely increases decision quality. The identification strategy we propose allows us to obtain plausibly causal effects. We present evidence against a number of threats to identification and alternative explanations for our findings. The evidence suggests that these results are likely driven by judge peers selecting more meritocratic and bipartisan judges relative to judicial selection by the President. Last, we conduct a back-of-the-envelope computation showing that the selection reform likely prevents land expropriations amounting to about 0.14% of GDP or USD 390 million every year.

Research examining the selection of public officials has largely focused on politicians. Our work focuses on the judiciary and advances the long-standing debate regarding Presidential appointment of judges. The results of this study highlight the potential to reform the judiciary in weakly institutionalized – low state capacity – settings and are consistent with recent evidence that tenure security for judges does not necessarily yield a “de facto” increase in judicial independence (Hayo and Voigt, 2019). Although many countries have instituted “security of tenure” with Presidential appointment of judges, as supported by The Federalists (Madison, Hamilton and Jay, 1788), our results suggest that this may not promote rule of law relative to other possible institutional arrangements. Some countries, therefore, have innovated with new institutional arrangements, such as peer appointment of judges. The evidence presented in this paper corroborates the view that the innovation of peer appointment of judges —as introduced in Sweden, Greece, and the UK— may be an improvement over Presidential appointment with security of tenure. More research on the judiciary, particularly on other

judicial selection mechanisms, will further clarify the counterfactual policy choices available to policymakers as they grapple with institutional reform in vulnerable democracies. This may also provide a deeper understanding of the conditions necessary for the establishment of rule of law in societies.

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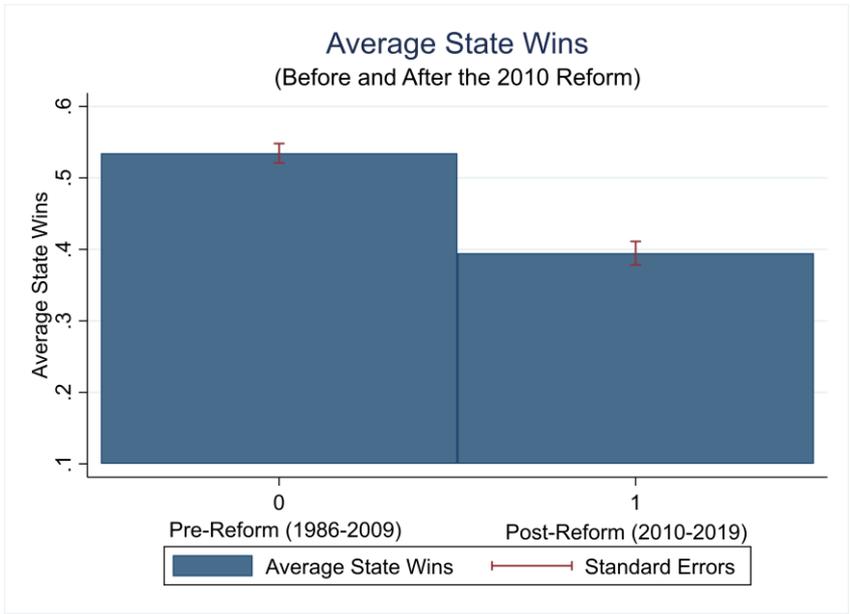
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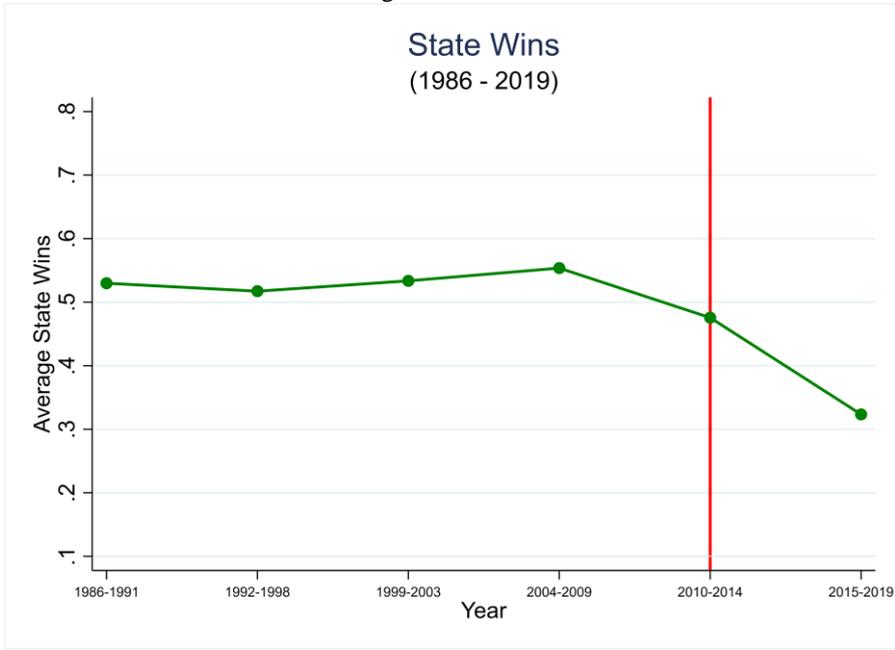
FIGURES AND TABLES

Figure 1: State Wins Pre- and Post-Reform

Panel A: Average State Wins Before and After the Reform

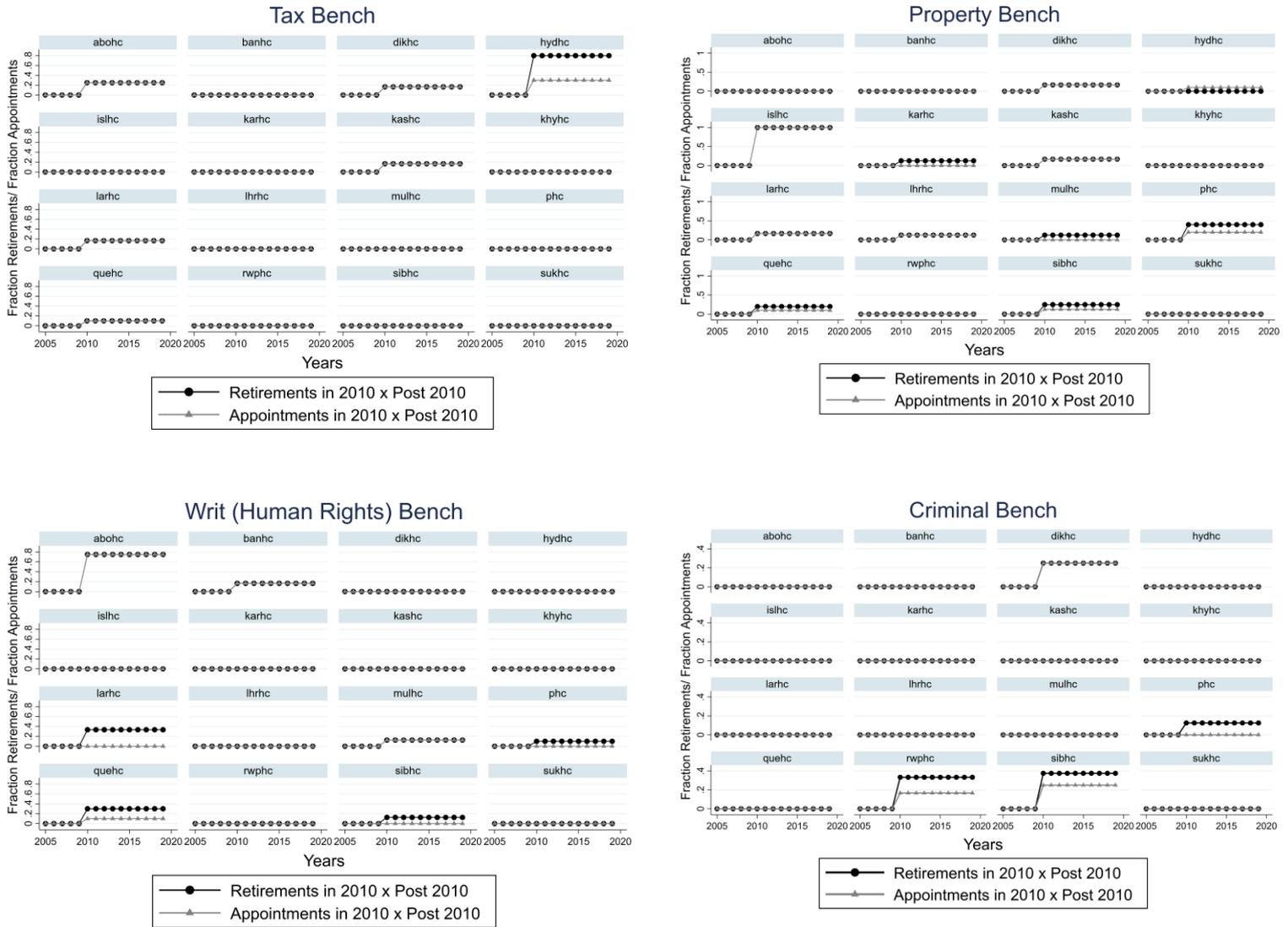


Panel B: Average State Wins over Time



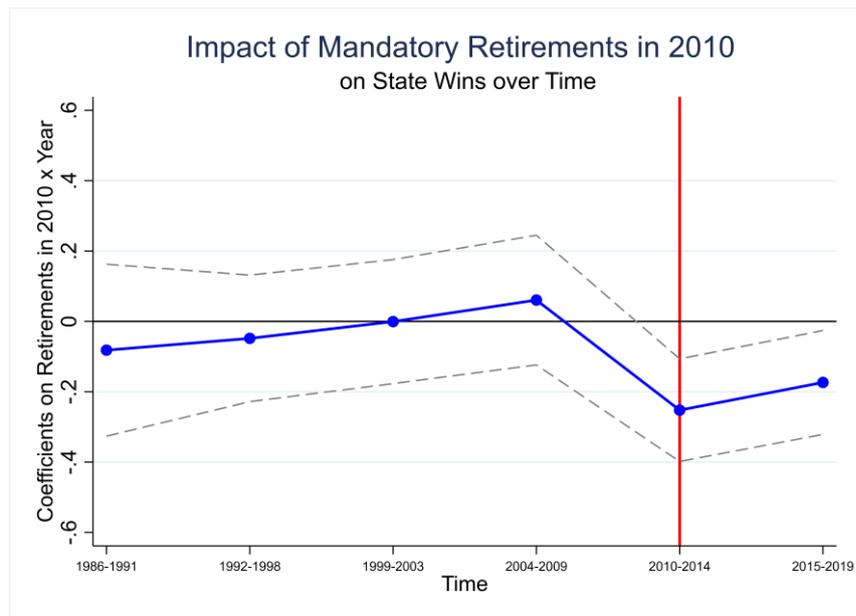
Note: The figure compares State Wins pre- and post-reform. In Panel A, the bar chart shows average State Wins in pre-reform and post-reform periods with 95% Confidence Intervals. In Panel B, we plot average State Wins over time. Vertical line in Panel B represents the 2010 reform year.

Figure 2: Mandatory Retirements and Judicial Commission Appointments



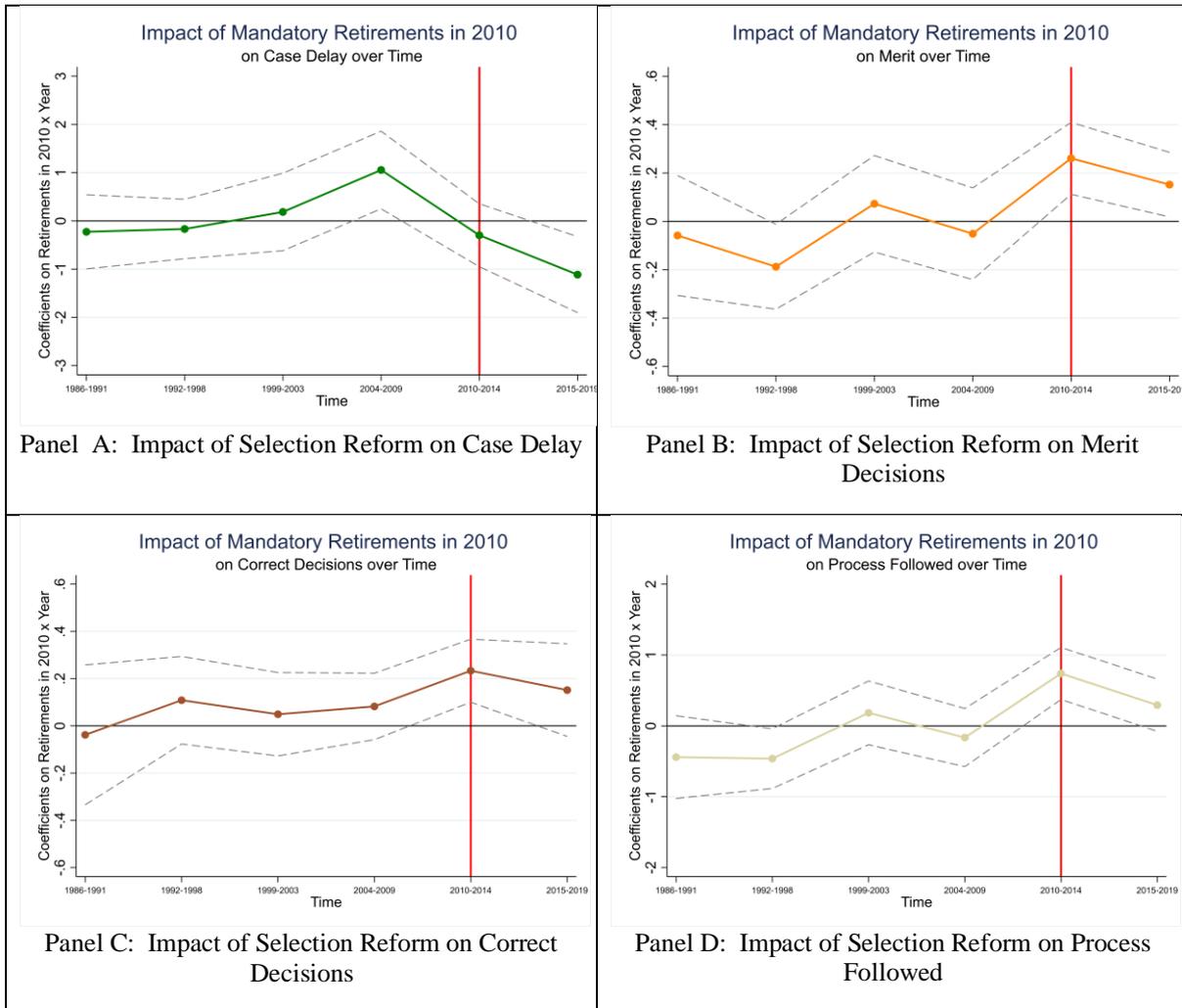
Note: The figure plots key explanatory variables, which vary at district-bench-year level. Each of the four panels shows a specialized judicial bench adjudicating cases involving tax, property, human rights, and crime, respectively. The dark line represents the fraction of judges reaching their mandatory retirement age of 62 in each district bench in 2010 interacted with the post-reform dummy. The light line represents the fraction of judges appointed by the judicial commission in each district bench in 2010 interacted with the post-reform time dummy. The correlation coefficient between these variables is 0.9. The regression-form representation of this figure with corresponding F-Statistics (first-stage) appears in Table C2 (Panel B) of Appendix C.

Figure 3: Impact of Mandatory Retirements in 2010 on State Wins over Time



Note: This figure presents the coefficients along with their 95% confidence intervals when we estimate equation (3). The table-form representation of this figure appears in Table C6 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 sharp fall in government victories post reform.

Figure 4: Impact of Mandatory Retirements in 2010 on Decision Quality



Note: This figure presents the coefficients along with their 95% confidence intervals when we estimate equation (3) using the four available measures of decision quality as dependent variables: Case Delay, Merit Decisions, Correct Decisions and Process Followed.

Table 1: Descriptive Statistics

| Variables | Observations | Mean | Std. Dev. | Min | Max |
|--|--------------|-------|-----------|-----|-----|
| <i>Panel A: Case Characteristics (by case)</i> | | | | | |
| State Wins | 8,446 | 0.482 | 0.500 | 0 | 1 |
| Case Delay (years) | 8,446 | 3.354 | 2.238 | 0 | 29 |
| Merit | 8,446 | 0.627 | 0.484 | 0 | 1 |
| Process Followed | 8,446 | 3.314 | 1.496 | 1 | 5 |
| Constitutional Cases | 8,446 | 0.722 | 0.448 | 0 | 1 |
| <i>Land Cases</i> | 8,446 | 0.409 | 0.492 | 0 | 1 |
| <i>Human Rights Cases</i> | 8,446 | 0.314 | 0.464 | 0 | 1 |
| Criminal Cases | 8,446 | 0.280 | 0.449 | 0 | 1 |
| Pages of Judgment Order | 8,446 | 8.878 | 7.706 | 1 | 81 |
| Number of Lawyers | 8,446 | 4.124 | 1.807 | 2 | 32 |
| Number of Judges on a case | 8,446 | 1.809 | 0.839 | 1 | 5 |
| Chief Justice on Bench | 8,446 | 0.065 | 0.246 | 0 | 1 |
| <i>Panel B: Appointment and Retirement in the reform year (by district-bench)</i> | | | | | |
| Retirements in 2010/Total | 64 | 0.115 | 0.199 | 0 | 1 |
| Appointments in 2010/ Total | 64 | 0.080 | 0.168 | 0 | 1 |
| <i>Panel C: Judge Characteristics (by judges)</i> | | | | | |
| Tenure at Decision | 511 | 3.331 | 2.236 | 0 | 22 |
| Gender | 511 | 0.961 | 0.194 | 0 | 1 |
| Promoted to SC | 511 | 0.061 | 0.239 | 0 | 1 |
| Former Judge | 511 | 0.115 | 0.320 | 0 | 1 |
| Fr. Office-Holder Bar. Ass. | 511 | 0.569 | 0.496 | 0 | 1 |
| Ran for Political Office | 511 | 0.186 | 0.389 | 0 | 1 |
| Former Lawyer | 511 | 0.886 | 0.318 | 0 | 1 |
| Post-Reform Judge | 511 | 0.325 | 0.469 | 0 | 1 |
| Punjabi Ethnicity | 511 | 0.196 | 0.397 | 0 | 1 |
| Sindhi Ethnicity | 511 | 0.053 | 0.224 | 0 | 1 |
| Balochi Ethnicity | 511 | 0.063 | 0.243 | 0 | 1 |
| Pashtun Ethnicity | 511 | 0.143 | 0.350 | 0 | 1 |
| Other Ethnicities | 511 | 0.483 | 0.500 | 0 | 1 |
| <i>Panel D: Treatment Variables and Special Bench Characteristics (by bench-district-year)</i> | | | | | |
| Retirements in 2010 X Post 2010 | 1516 | 0.042 | 0.134 | 0 | 1 |
| Appointments in 2010 X Post 2010 | 1516 | 0.031 | 0.111 | 0 | 1 |
| Number of Judges on Bench | 1516 | 7.635 | 3.237 | 2 | 16 |
| Number of Criminal cases on Bench | 1516 | 1.566 | 4.716 | 0 | 42 |
| Number of Land cases on Bench | 1516 | 2.297 | 2.781 | 0 | 24 |
| Number of Human Right cases on Bench | 1516 | 1.749 | 2.229 | 0 | 16 |

Note: This table reports the summary statistics for the baseline sample of 8446 cases, 511 judges covering the 64 district benches over the 1986-2019 period.

Table 2: Impact of Selection Reform on State Wins

| | (1) | (2) | (3) | (4) | (5) |
|---------------------------------|-----------------------|-----------------------|--------------------|------------------|-------------------|
| | <i>State Wins</i> | | | | |
| Retirements in 2010 X Post 2010 | -0.237*** [0.0342] | -0.202*** [0.0398] | | | |
| Retirements in 2009 X Post 2010 | | | 0.0640 [0.0610] | | |
| Retirements in 2008 X Post 2010 | | | | 0.126 [0.112] | |
| Retirements in 2007 X Post 2010 | | | | | 0.104 [0.0985] |
| District-by-Year FE | Yes | Yes | Yes | Yes | Yes |
| Case and Bench Controls | No | Yes | Yes | Yes | Yes |
| Observations | 8,446 | 8,446 | 8,446 | 8,446 | 8,446 |
| R-squared | 0.135 | 0.142 | 0.141 | 0.141 | 0.141 |
| Mean of dependent variable | 0.482 | 0.482 | 0.482 | 0.482 | 0.482 |

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. The controls include all case and district bench 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 Cumulative Peer Appointments on State Wins

| Panel A: Ordinary Least Squares and 2 nd -Stage Least Squares Results | | | | |
|--|-----------------------|-----------------------|-----------------------------|-----------------------|
| | OLS | | 2SLS, 2 nd Stage | |
| | (1) | (2) | (3) | (4) |
| <i>State Wins</i> | | | | |
| Cumulative Peer Appointments from 2010-2019 | -0.224*** [0.0429] | -0.179*** [0.0446] | -0.276*** [0.0530] | -0.225*** [0.0606] |
| District-by-Year FE | Yes | Yes | Yes | Yes |
| Bench and Case Controls | No | Yes | No | Yes |
| Observations | 8,446 | 8,446 | 8,446 | 8,446 |
| R-squared | 0.137 | 0.143 | 0.136 | 0.143 |
| Mean of dependent variable | 0.482 | 0.482 | 0.482 | 0.482 |
| Panel B: First-Stage Results | | | | |
| | (3) | | (4) | |
| Cumulative Peer Appointments from 2010 | | | | |
| Cumulative Mandatory Retirements from 2010-2019 | | | 0.666*** [0.0613] | 0.652*** [0.0620] |
| District-by-Year FE | | | Yes | Yes |
| Case and Bench Controls | | | No | Yes |
| Observations | | | 8,446 | 8,446 |
| R-squared | | | 0.881 | 0.883 |
| F-Statistic (Montiel et al., 2013) | | | 118.270 | 110.679 |

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. Cumulative Peer Appointments from 2010-2019 is fraction of cumulative appointments in a given district bench from 2010 onwards. Cumulative Mandatory Retirements from 2010-2019 is fraction of mandatory retirements in a given district bench as predicted by age structure in 2010. These variables are plotted in Figure C3 of Appendix C. 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 bench characteristics shown in Table 1. The case controls also 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) |
|---------------------------------|---------------------|----------------------|--------------------------|-------------------------|
| | <i>Case Delay</i> | <i>Merit</i> | <i>Correct Decisions</i> | <i>Process Followed</i> |
| Retirements in 2010 X Post 2010 | -0.878** [0.387] | 0.215*** [0.0396] | 0.191*** [0.0515] | 0.425*** [0.127] |
| District-by-Year FE | Yes | Yes | Yes | Yes |
| Case and Bench Controls | Yes | Yes | Yes | Yes |
| Observations | 8,446 | 8,446 | 8,446 | 8,446 |
| R-squared | 0.218 | 0.141 | 0.098 | 0.080 |
| Mean of dependent variable | 3.354 | 0.627 | 0.469 | 3.314 |

Robust standard errors appear in brackets (clustered at district-bench level). In Column (1), the dependent variable is case delay i.e. the difference between filing and decision year. In Column (2) it is a dummy variable for the case being ruled on merits or evidence. In Column (3), the dependent variable is a dummy variable for the case being judged as correct by legal experts. In Column (4), the dependent variable is a rating from 1-5 on decision quality. 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 bench 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: Judicial Commission Appointees and Judge Characteristics at Judge Level

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) |
|----------------------------|---------------------|----------------------|----------------------|--------------------|---------------------|--------------------|----------------------|---------------------|--------------------|---------------------------------|--------------------------|
| | Gender | Muslim | Former Judge | Former Lawyer | Punjabi Ethnicity | Sindhi Ethnicity | Balochi Ethnicity | Pashtun Ethnicity | Other Ethnicity | Former Office Holder Bar Assoc. | Ran for Political Office |
| Post-Reform Judge | -0.0361 [0.0246] | -0.00636 [0.0142] | -0.00167 [0.0410] | 0.0148 [0.0403] | -0.0188 [0.0503] | 0.0316 [0.0318] | -0.00357 [0.0326] | -0.0433 [0.0440] | 0.0400 [0.0624] | -0.328*** [0.0582] | -0.164*** [0.0439] |
| Age Control | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Case Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| District Bench Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 511 | 511 | 511 | 511 | 511 | 511 | 511 | 511 | 511 | 511 | 511 |
| R-squared | 0.025 | 0.009 | 0.040 | 0.040 | 0.045 | 0.020 | 0.037 | 0.017 | 0.035 | 0.174 | 0.066 |
| Mean of dependent variable | 0.961 | 0.990 | 0.115 | 0.886 | 0.195 | 0.053 | 0.063 | 0.143 | 0.483 | 0.569 | 0.186 |

Robust standard errors appear in brackets (clustered at the judge level). The dependent variable is State Wins, judge-level average 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 Presidential appointees. The controls include judge level average of case and district bench characteristics shown in Table 1. Unit of observation in this table is an individual judge, for analogous case-level regressions see Table C7 in Appendix C.*** p<0.01, ** p<0.05, * p<0.1.

Table 6: Impact of Selection Reform on Presidential Appointees (Spillover Effects)

| | (1) | (2) | (3) | (4) |
|---------------------------------|---------------------|--------------------|--------------------|---------------------|
| | <i>State Wins</i> | | | |
| Retirements in 2010 X Post 2010 | -0.0718 [0.0594] | -0.121 [0.0774] | -0.102 [0.0788] | -0.0825 [0.0816] |
| District-by-Year FE | No | Yes | Yes | Yes |
| Bench Controls | No | No | Yes | Yes |
| Case Controls | No | No | No | Yes |
| Observations | 6,629 | 6,629 | 6,629 | 6,629 |
| R-squared | 0.0001 | 0.112 | 0.120 | 0.122 |
| Mean of dependent variable | 0.53 | 0.53 | 0.53 | 0.53 |

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 (serving both pre- and post-reform). *** p<0.01, ** p<0.05, * p<0.1.

Table 7: Impact of Selection Reform on State Wins (by type of Case)

| Panel A: Constitutional vs Criminal Cases | | | | |
|--|-----------------------|-----------------------|---------------------|-------------------|
| | Constitutional Cases | | Criminal Cases | |
| | (1) | (2) | (3) | (4) |
| | <i>State Wins</i> | | | |
| Retirements in 2010 X Post 2010 | -0.270*** [0.0304] | -0.258*** [0.0271] | 0.261 [1.365] | 2.635 [2.714] |
| District-by-Year FE | Yes | Yes | Yes | Yes |
| Case and Bench Controls | No | Yes | No | Yes |
| Observations | 6,094 | 6,094 | 2,368 | 2,368 |
| R-squared | 0.158 | 0.159 | 0.277 | 0.281 |
| Mean of dependent variable | 0.456 | 0.456 | 0.548 | 0.548 |
| Panel B: Constitutional and Criminal Cases Disaggregated | | | | |
| | Constitutional Cases | | Criminal Cases | |
| | (1) | (2) | (3) | (4) |
| | Human-Rights Cases | Land Cases | Non-Islamic Case | Islamic Case |
| | <i>State Wins</i> | | | |
| Retirements in 2010 X Post 2010 | -0.230*** [0.0460] | -0.307*** [0.0471] | 5.379 [3.387] | -1.895 [1.224] |
| District-by-Year FE | Yes | Yes | Yes | Yes |
| Case and Bench Controls | Yes | Yes | Yes | Yes |
| Observations | 3,428 | 2,650 | 2,143 | 225 |
| R-squared | 0.219 | 0.218 | 0.286 | 0.763 |
| Mean of dependent variable | 0.462 | 0.449 | 0.552 | 0.520 |

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. Panel A reports the disaggregated results into constitutional and criminal cases. The constitutional and criminal cases do not add to the 8446-case sample because 16 criminal cases are also marked as constitutional. Panel B shows further disaggregation of constitutional cases into human rights and land cases, and criminal cases into those judged under British common law and Islamic “Hudood Law”. Islamic cases includes cases pertaining to consumption of alcohol, blasphemy, adultery, homosexuality, fornication, and false accusation of fornication. The controls include all case and district bench characteristics shown in Table 1. The case controls also include case-type fixed effects. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 8: Small & Medium Firms and Large Corporations vs the State

| | Small and Medium Firms | | Large Corporations | |
|---------------------------------|------------------------|---------|--------------------|---------|
| | (1) | (2) | (3) | (4) |
| | <i>State Wins</i> | | | |
| Retirements in 2010 X Post 2010 | -0.248* | -0.203 | 0.189* | 0.237* |
| | [0.144] | [0.150] | [0.109] | [0.128] |
| District-by-Year FE | Yes | Yes | Yes | Yes |
| Case and Bench Controls | No | Yes | No | Yes |
| Observations | 864 | 864 | 435 | 435 |
| R-squared | 0.434 | 0.450 | 0.594 | 0.626 |
| Mean of dependent variable | 0.466 | 0.466 | 0.462 | 0.462 |

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. We consider subsample of cases where the State is opposing a firm. We distinguish between small and medium sized firms and large corporation where we classify a firm as large if it is associated with the “Big 12” business groups of Pakistan (Tech, 2020). The large corporations include all firms owned by Mian Muhammad Mansha’s Nishat Group, Byram Avari’s Avari Group, Razaq Dawood’s Descon Group, Mian Amir Mahmood’s Dunya News Group, Mian Muhammad Latif’s Chenab Group and Malik Riaz’s Bahria Town Group. Retirements in 2010 is fraction of mandatory retirements in a given district bench in reform year 2010. The controls include all case and district bench characteristics shown in Table 1. The case controls also include case-type fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

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

| | <i>State Wins</i> | | | | |
|--|------------------------|-----------------------|-----------------------|-----------------------|-----------------------|
| | Pres. Musharraf (1) | Pres. Tarar (2) | Pres. Leghari (3) | Pres. Khan (4) | Pres. Haq (5) |
| Retirements in 2010 X Post 2010 | -0.204*** (0.0504) | -0.542*** (0.0724) | -0.451*** (0.0608) | -0.454*** (0.0720) | -0.492*** (0.0703) |
| District-Bench and Year FE Controls | Yes Yes | Yes Yes | Yes Yes | Yes Yes | Yes Yes |
| Observations | 3,539 | 1,817 | 3,032 | 2,616 | 3,018 |
| R-squared | 0.199 | 0.267 | 0.253 | 0.281 | 0.244 |
| Mean Dep. Variable | 0.432 | 0.321 | 0.401 | 0.486 | 0.403 |

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 new judicial vacancies. The case controls also include case-type fixed effects. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 10: Impact of Selection Reform on Case and Bench 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 | No. Judges on Bench | No. Criminal cases on Bench | No. Land cases on Bench | No. Human Rights Cases on Bench |
| Retirements in 2010 X Post 2010 | -0.00303 (0.00644) | -0.00873 (0.00541) | -0.112 (0.580) | -0.00964 (0.0171) | -0.145 (0.179) | 0.0422 (0.0685) | -0.160 (0.175) | 2.164* (1.177) | 0.488 (0.683) | 0.750 (0.668) |
| 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 | 8,446 | 8,446 | 8,446 | 8,446 | 8,446 | 8,446 | 8,446 | 8,446 | 8,446 | 8,446 |
| R-squared | 0.991 | 0.991 | 0.292 | 0.110 | 0.068 | 0.138 | 0.962 | 0.763 | 0.663 | 0.662 |
| Mean of dependent variable | 0.722 | 0.280 | 8.877 | 0.065 | 4.123 | 1.809 | 8.025 | 2.77 | 3.476 | 2.586 |

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 bench characteristics outlined in Table 1 (except the dependent variable used in the respective column). The case controls include case-type fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

For Online Appendix to:

The impact of Presidential appointment of judges: Montesquieu or the Federalists?

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/>) and High Court Registrar Offices. More information on this source and data construction can be found in Appendix B.3.

Case Delay = This variable is calculated as the difference between the case decision and filing years. Both pieces of this information of filing and decision years are clearly marked at the top of every judgement order.

Merit Decisions = 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.

Correct Decisions = A dummy variable that switches on if the cases is rated as correct. 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 rating also comes from assessments by the legal experts at 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 dummy variable. Information to construct this variable is obtained from the judicial administrative records of the Registrar Offices of the High Courts.

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.

Cumulative Retirements Since 2010-2019 = This variable is the cumulative fraction of judges expected to reach their mandatory retirement age of 62 in each district bench from 2010 to 2019, as determined by the predicted trajectory of mandatory retirements in 2010. Information to

construct this variable is obtained from the judicial administrative records of the Registrar Offices of the High Courts of Pakistan.

Cumulative Appointments Since 2010-2019 = This variable is the cumulative fraction of judges appointed by the judicial commission from 2010 to 2019. Information to construct this variable is obtained from the judicial administrative records of the Registrar Offices of the High Courts 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 political-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, false accusation of fornication, 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 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 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 Bar Council records, 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 from registrar office records.

Number of Judges on Bench = This is the number of judges in a given judicial bench of a given district court. This 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.

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); mandatory retirement at age 62 for High Court judges in both countries remained ever since. It is also 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' 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. Cases are randomly assigned across judges subject to judge capacity and specialization constraint. Although there are only four provincial High Courts and one federal court High Court in Islamabad, the benches of each are spread out over the four provinces in the form of 16 district High Court divisional units (Alam, 2021). Within each district, there are four judicial benches consisting of about 7 judges each. These are property bench, writ or human rights bench, tax bench and criminal bench. Importantly, Pakistani laws and capacity constraints only allow for judicial vacancy to be filled at the judicial bench level. For instance, property judge bench can only be replaced by the judge of same expertise. Moreover, in the High Courts studied are particularly important because it is here cases can be filed against the government. This takes 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.

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 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/>) and High Court Registrar Offices. 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 to prepare their cases. Access is password-protected, and permission to use the website and cases is obtained via a Law firm. This is combined with archives at the High Court’s Registrar Offices. In principle, these two data sources combine to provide the universe of High Court cases in Pakistan; we chose a sample period from 1986 to 2019 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 2019 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 C6 presents this information as a plot of sampled cases versus total available cases.

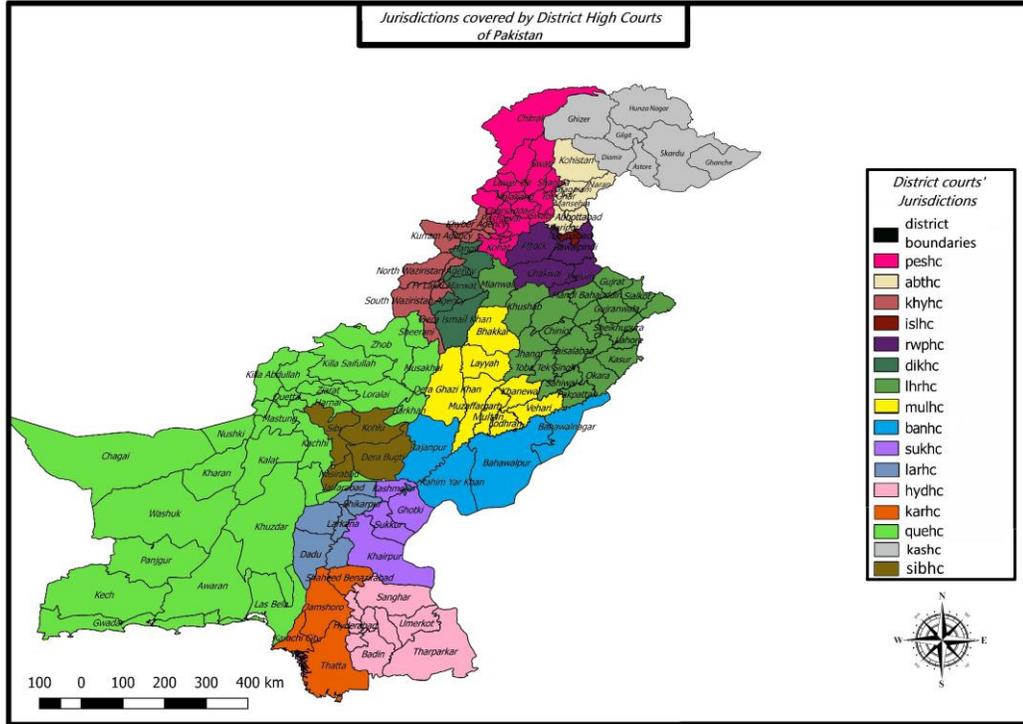
Two independent teams of four paralegals each supervised by a senior Lawyer recorded the key information in the judgment texts for the 8500 cases. We ensured via a formal contract that both teams worked independently. Random field visits also reassured us that the contract was being enforced. Table C1 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 0.85. Unsurprisingly given the high correlation coefficients, similar results are obtained using the cases coded by Team 2 (Table C20 reports for robustness of our main results using coding by Team 2). The two senior lawyers supervising the teams were experts in “constitutional law” with over 10 and 12 years of experience as attorneys practicing constitutional law, respectively. They were hence particularly suited to supervise coding of cases involving the State. The remaining 8 people who did the actual coding exercise were legal experts and had all passed “L.L.M” examination which is equivalent to 2 years master’s degree in law. This is the main requirement to practice law as an attorney in Pakistan. These coders were essentially junior attorneys starting practice as lawyers with the law

firm. The teams were given no information on the research question, to ensure that those performing the coding exercise did not know whether a judge was appointed by peers or the President as this might risk biasing the results. 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 (including Lahore) in Table C12 of Appendix C.

While legal experts' coding the variables may be an improvement over having the cases coded by research assistants without much understanding of legal context, there is unavoidable subjectivity in the coding of whether the government obtained a victory or whether the decision was on case merits. In contrast, the level of subjectivity is likely to be low for the case delay outcome variable. This is because it is computed simply based on an accurate reading of the judgment text (where case delay = case decision year – case filing year). Both these pieces of information are explicitly mentioned in the judgment order which we use to construct the case delay variable. This observation is reflected in Table C1 where the correlation coefficient between Teams 1 and 2 of more subjective outcome variables (State Wins, Merit, Process Followed) is in the range of 0.85-0.90, while the correlation coefficient for case delay between the two teams is almost exactly one.

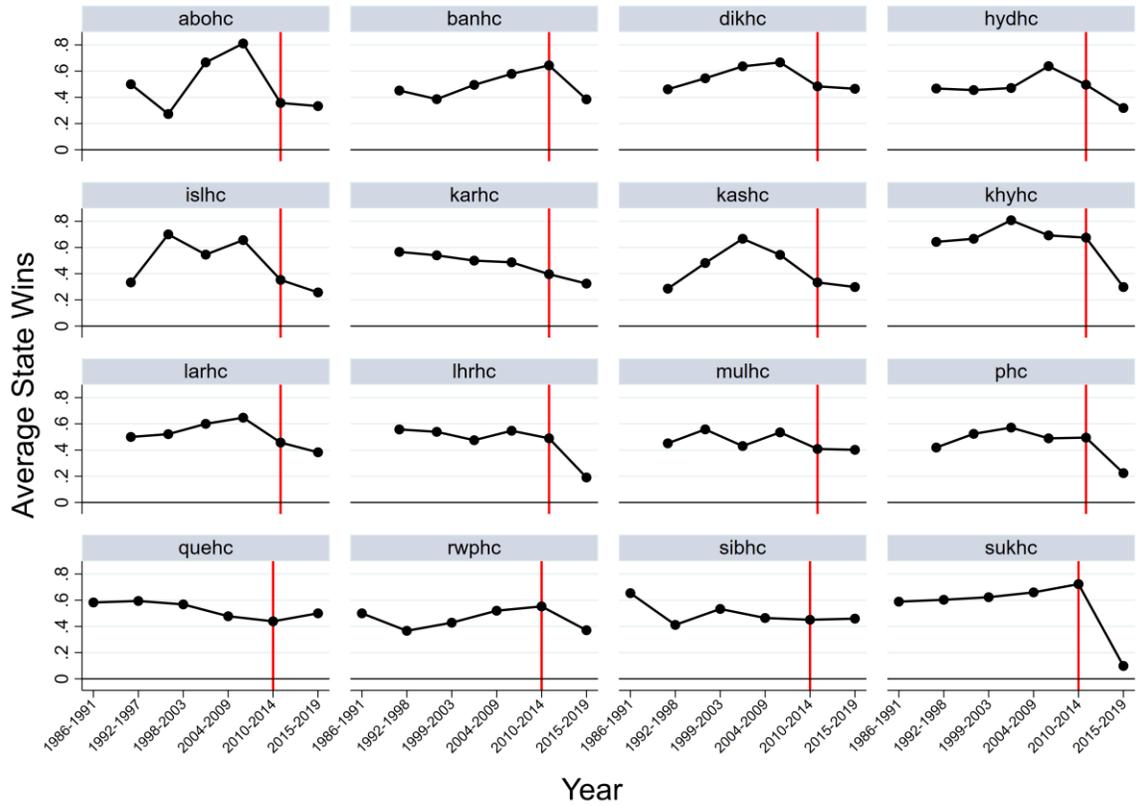
Appendix C. Additional Tables and Figures

Figure C1: Jurisdictions covered by District High Courts of Pakistan



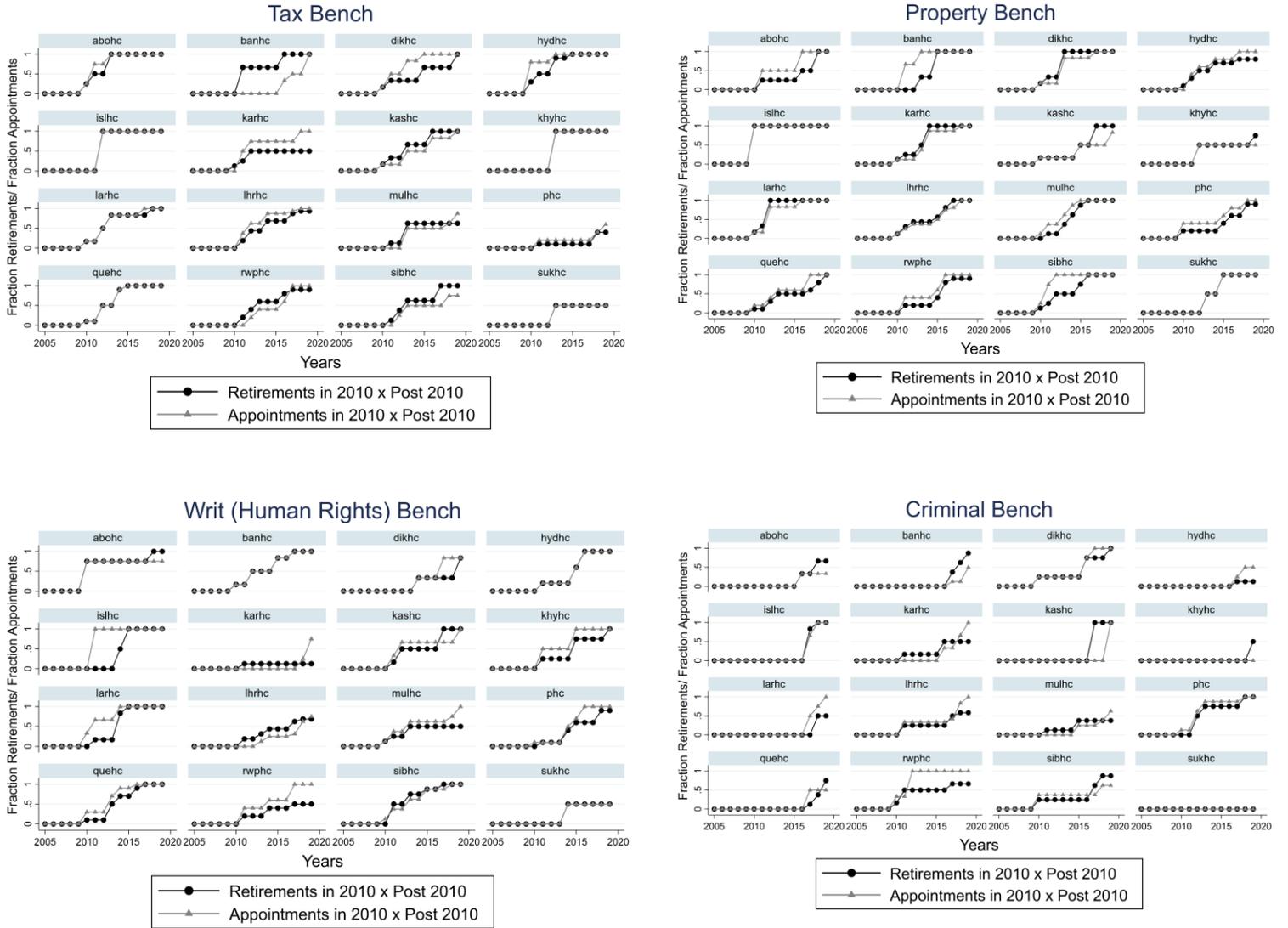
Note: peshc stands for Peshawar High Court, abthc for Abbottabad High Court, khyhc for Khyber High Court, islhc for Islamabad High Court and so forth.

Figure C2: State Wins over Time by District (16-panel graph)



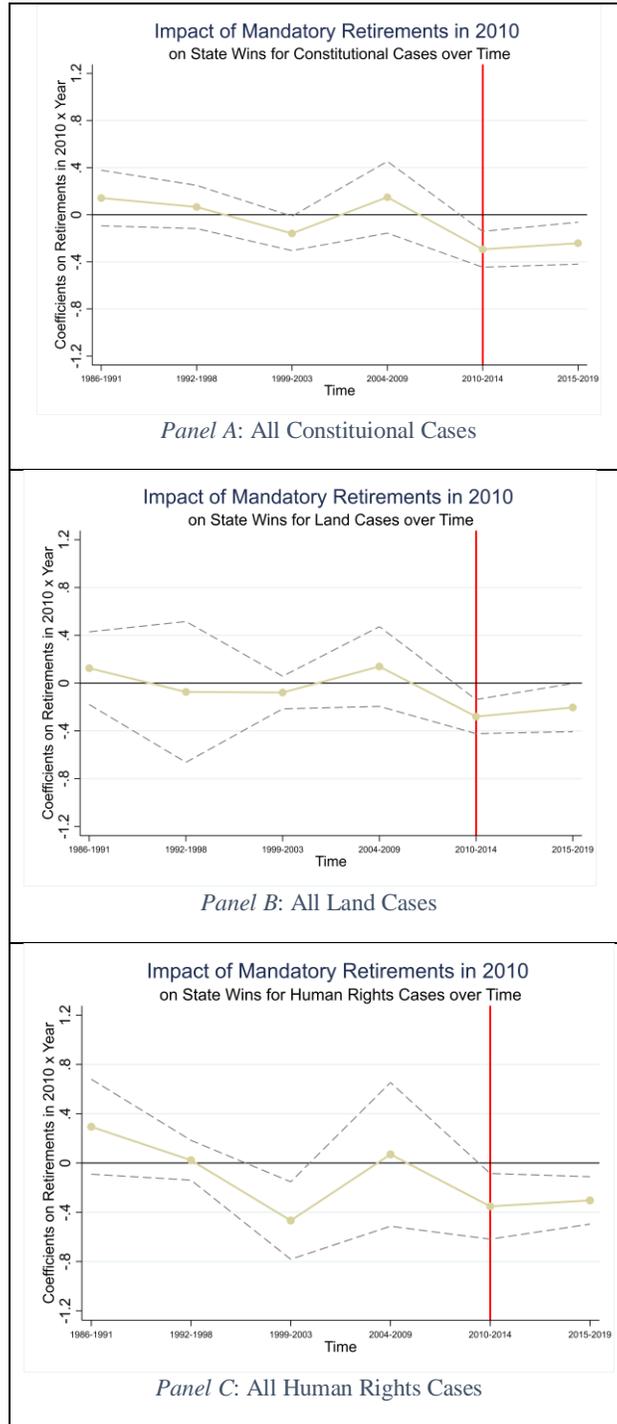
Note: The figure plots average State Wins over time for every district (or division). abohc is average State Wins over time across all benches in Abbottabad, islhc is average State Wins over time for all benches in Islamabad and so on. Vertical line represents the 2010 reform year.

Figure C3: Cumulative Mandatory Retirements and Judicial Commission Appointments



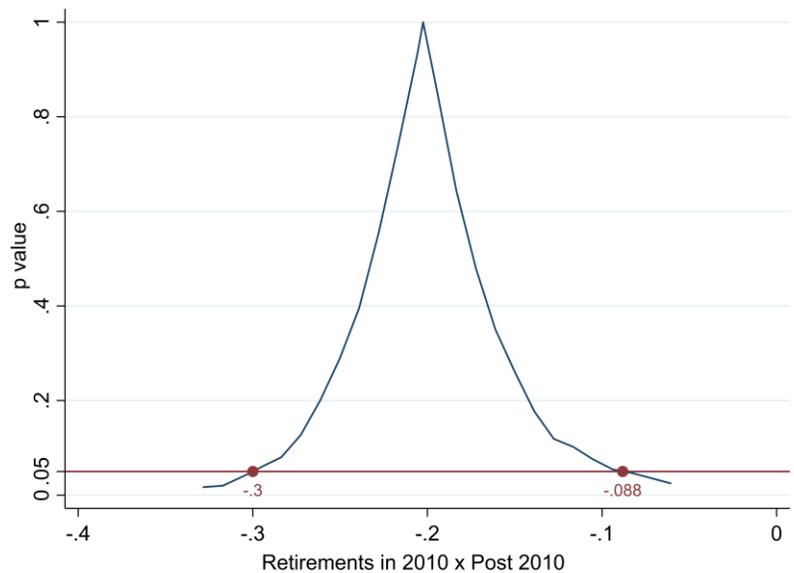
Note: The figure plots key explanatory variables used in specification (2), which vary at district-bench-year level. Each of the four panels shows a specialized bench adjudicating cases involving tax, property, human rights, and crime, respectively. The dark line represents the fraction of judges reaching their mandatory retirement age of 62 in each district bench from 2010 onwards. The predicted trajectory of retirements from 2010-2019 as determined by age structure of judges in 2010. The light line represents the fraction of judges appointed by the judicial commission (peer judges) in each district bench from 2010-2019.

Figure C4: Impact of Mandatory Retirements in 2010 on State Wins by Type of Cases



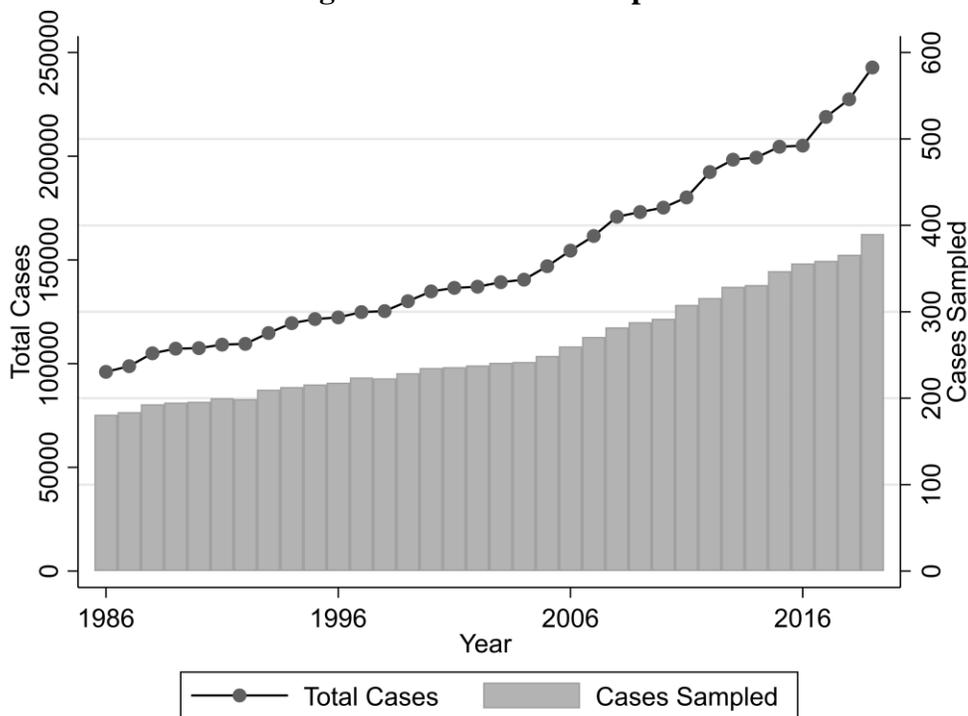
Note: This figure presents the coefficients along with their 95% confidence intervals when we estimate specification (3) on sample of all constitutional cases (Table 7, Panel A), and its constituent land and political rights cases (Table 7, Panel B).

Figure C5: Confidence Interval by Wild Bootstrap Clustering



Note: The figure displays confidence interval for our coefficient of interest for the first 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 15.1 is used to construct this confidence interval.

Figure C6: Total vs. Sampled Cases



Note: These are 8500 randomly sampled cases for all years from 1986 to 2019 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 | 0.48 | 0.56 | -0.08 | 0.85 |
| Merit | 0.63 | 0.67 | -0.05 | 0.91 |
| Case Delay | 3.35 | 3.33 | 0.02 | 0.99 |
| Process Followed | 3.31 | 3.22 | 0.09 | 0.88 |
| Correct Decisions | 0.47 | 0.44 | 0.02 | 0.95 |
| Constitutional | 0.72 | 0.70 | 0.02 | 0.95 |
| <i>Land Cases</i> | 0.41 | 0.38 | 0.03 | 0.94 |
| <i>HR Cases</i> | 0.31 | 0.33 | -0.02 | 0.96 |
| Criminal Cases | 0.28 | 0.29 | -0.01 | 0.98 |
| No. of Lawyers | 4.12 | 4.09 | 0.03 | 0.96 |
| No. of Judges | 1.81 | 1.84 | -0.03 | 0.95 |
| CJ in Bench | 0.07 | 0.08 | -0.02 | 0.87 |
| Pg. of Judgment | 8.88 | 8.70 | 0.18 | 0.95 |

Note: This table compares the outcome variables and case characteristics for the two teams of coders for the same 8446 cases used in the analysis. Team 1 data are used in the regressions (except Table C20 that reports that essentially identical results are found if we use data coded by Team 2). The table shows the two means, the difference, and the correlation coefficient between the two codings.

Table C2: Impact of Selection Reform on State Wins

| Panel A: Ordinary Least Squares and 2 nd -Stage Least Squares Results | | | | |
|--|-----------------------|----------------------------------|-----------------------------|-----------------------|
| | OLS | | 2SLS, 2 nd Stage | |
| | (1) | (2) | (3) | (4) |
| | <i>State Wins</i> | | | |
| Appointments in 2010 X Post 2010 | -0.289*** [0.0539] | -0.248*** [0.0546] | -0.363*** [0.0860] | -0.310*** [0.0817] |
| District-by-Year FE | Yes | Yes | Yes | Yes |
| Bench and Case Controls | No | Yes | No | Yes |
| Observations | 8,446 | 8,446 | 8,446 | 8,446 |
| R-squared | 0.135 | 0.142 | 0.135 | 0.142 |
| Mean of dependent variable | 0.482 | 0.482 | 0.482 | 0.482 |
| Panel B: First-Stage Results | | | | |
| | | (3) | (4) | |
| | | Appointments in 2010 X Post 2010 | | |
| Retirements in 2010 X Post 2010 | | 0.645*** [0.136] | 0.647*** [0.135] | |
| District-by-Year FE | | Yes | Yes | |
| Case and Bench Controls | | No | Yes | |
| Observations | | 8,446 | 8,446 | |
| R-squared | | 0.855 | 0.856 | |
| F-Statistic (Montiel et al., 2013) | | 22.572 | 22.835 | |

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 the rule of thumb of 10. The controls include all case and district bench 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 Mandatory Retirements on Decision Quality

| | (1) | (2) | (3) | (4) |
|---------------------------------|-------------------|---------------------|--------------------------|-------------------------|
| | <i>Case Delay</i> | <i>Merit</i> | <i>Correct Decisions</i> | <i>Process Followed</i> |
| Retirements in 2009 X Post 2010 | 0.0806 [0.152] | -0.0266 [0.0539] | -0.0185 [0.0538] | -0.0142 [0.163] |
| District-by-Year FE | Yes | Yes | Yes | Yes |
| Case and Bench Controls | Yes | Yes | Yes | Yes |
| Observations | 8,446 | 8,446 | 8,446 | 8,446 |
| R-squared | 0.216 | 0.138 | 0.096 | 0.080 |
| Mean of dependent variable | 3.354 | 0.627 | 0.468 | 3.314 |

Robust standard errors appear in brackets (clustered at district-bench level). In Column (1), the dependent variable is case delay i.e. the difference between filing and decision year. In Column (2) it is a dummy variable for the case being ruled on merits of the case. In Column (3), the dependent variable is a dummy variable for the case being judged as correct by legal experts. In Column (4), the dependent variable is a rating from 1-5 on process followed. Retirements in 2009 is the fraction of mandatory retirements in a given district bench in 2009. Post 2010 is a dummy for post-reform period. The controls include all case and district bench 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: Impact of Selection Reform on State Wins (Controlling for Pre-2010 Retirements)

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---------------------------------|-----------------------|-----------------------|-----------------------|-----------------------|-----------------------|-----------------------|
| | <i>State Wins</i> | | | | | |
| Retirements in 2010 X Post 2010 | -0.200*** [0.0416] | -0.204*** [0.0455] | -0.201*** [0.0465] | -0.204*** [0.0424] | -0.203*** [0.0460] | -0.210*** [0.0484] |
| Retirements in 2009 X Post 2009 | 0.0218 [0.0596] | | | -0.0168 [0.0607] | 0.0221 [0.0608] | -0.0172 [0.0608] |
| Retirements in 2008 X Post 2008 | | -0.0119 [0.0907] | -0.152 [0.259] | | -0.0202 [0.0938] | -0.229 [0.270] |
| Retirements in 2007 X Post 2007 | | | | 0.118 [0.0832] | | 0.132 [0.0886] |
| Retirements in 2008 X Post 2010 | | | 0.158 [0.293] | | | 0.183 [0.293] |
| District-by-Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Case and Bench 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 | 8,446 | 8,446 | 8,446 | 8,446 | 8,446 | 8,446 |
| R-squared | 0.142 | 0.142 | 0.142 | 0.143 | 0.142 | 0.143 |
| Mean of dependent variable | 0.482 | 0.482 | 0.482 | 0.482 | 0.482 | 0.482 |

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 bench characteristics shown in Table 1. The case controls also include case-type fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table C5: Impact of Post- and Pre-Reform Cumulative Retirements on State Wins

| | (2) | (3) | (4) | (5) |
|---|-----------------------|--------------------|-----------------------|------------------|
| Cumulative Retirements Since 2010-2019 | -0.224*** [0.0429] | | | |
| Cumulative Retirements from 2000 - 2009 | | 0.0772 [0.0670] | | |
| Cumulative Retirements from 1990 - 1999 | | | -0.000705 [0.0420] | |
| Cumulative Retirements from 1986-1989 | | | | 0.125 [0.158] |
| District-by-Year FE | Yes | Yes | Yes | Yes |
| Case and Bench Controls | Yes | Yes | Yes | Yes |
| Observations | 8,446 | 5,174 | 2,677 | 686 |
| R-squared | 0.137 | 0.108 | 0.127 | 0.125 |
| Mean of dependent variable | 0.482 | 0.534 | 0.521 | 0.511 |

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. Cumulative Mandatory Retirements Since 2010 is fraction of mandatory retirements in a given district bench from 2010-2019 as predicted by age structure in 2010. In Column 2, cumulative retirements are fraction of mandatory retirements in a given district bench from 2000-2009, as predicted by age structure in 2000 and so forth. The controls include all case and district bench characteristics shown in Table 1. The case controls also include case-type fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table C6: Test for Pre-Trends - Impact of Selection Reform on State Wins Over Time

| | (1) | (2) | (3) |
|---|-----------------------|-----------------------|-----------------------|
| | <i>State Wins</i> | | |
| Retirements in 2010 X Year 1986 to 1991 | -0.159 [0.149] | -0.0921 [0.130] | -0.0747 [0.126] |
| Retirements in 2010 X Year 1992 to 1998 | -0.181 [0.116] | -0.0955 [0.100] | -0.0621 [0.0953] |
| Retirements in 2010 X Year 1999 to 2003 | -0.0853 [0.0694] | 0.0130 [0.0853] | 0.00988 [0.0923] |
| Retirements in 2010 X Year 2004 to 2009 | -0.0283 [0.0958] | 0.0442 [0.0937] | 0.0511 [0.0953] |
| Retirements in 2010 X Year 2010 to 2014 | -0.287*** [0.0747] | -0.263*** [0.0764] | -0.252*** [0.0745] |
| Retirements in 2010 X Year 2015 to 2019 | -0.203*** -0.159 | -0.161** -0.0921 | -0.167** -0.0747 |
| District-by-Year FE | Yes | Yes | Yes |
| Case Controls | No | Yes | Yes |
| Bench Controls | No | No | Yes |
| Observations | 8,446 | 8,446 | 8,446 |
| R-squared | 0.136 | 0.140 | 0.142 |
| Mean of dependent variable | 0.482 | 0.482 | 0.482 |

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 time-period dummies as indicated in equation (3). The controls include all case and district bench 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 3. *** 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) | (7) | (8) | (9) | (10) | (11) |
|----------------------------|----------------------|----------------------|---------------------|--------------------|--------------------|-----------------------|---------------------|---------------------|--------------------|---------------------------------|--------------------------|
| | Gender | Muslim | Former Judge | Former Lawyer | Punjabi Ethnicity | Sindhi Ethnicity | Balochi Ethnicity | Pashtun Ethnicity | Other Ethnicity | Former Office Holder Bar Assoc. | Ran for Political Office |
| Post-Reform Judge | -0.0521* (0.0285) | -0.00232 (0.0105) | 0.00386 (0.0315) | 0.0106 (0.0301) | 0.0127 (0.0115) | -0.00270 (0.00610) | 0.00399 (0.0067) | -0.0004 (0.0097) | 0.0162 (0.0142) | -0.358*** (0.0505) | -0.140*** (0.0381) |
| Age Control | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Case Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Bench Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 8,446 | 8,446 | 8,446 | 8,446 | 8,446 | 8,446 | 8,446 | 8,446 | 8,446 | 8,446 | 8,446 |
| R-squared | 0.016 | 0.002 | 0.007 | 0.008 | 0.007 | 0.004 | 0.005 | 0.007 | 0.005 | 0.097 | 0.023 |
| Mean of dependent variable | 0.961 | 0.991 | 0.110 | 0.892 | 0.200 | 0.051 | 0.059 | 0.141 | 0.487 | 0.573 | 0.188 |

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 bench controls are identical to those in the baseline regression. *** p<0.01, ** p<0.05, * p<0.1.

Table C8: Impact of Selection Reform on State Wins (by type of Case) – Cumulative Retirements

| Panel A: Constitutional vs Criminal Cases | | | | |
|--|-----------------------|-----------------------|-------------------|--------------------|
| | Constitutional Cases | | Criminal Cases | |
| | (1) | (2) | (3) | (4) |
| | <i>State Wins</i> | | | |
| Cumulative Retirements Since 2010 | -0.260*** [0.0535] | -0.244*** [0.0551] | -0.337 [0.298] | -0.271 [0.317] |
| District-by-Year FE | Yes | Yes | Yes | Yes |
| Case and Bench Controls | No | Yes | No | Yes |
| Observations | 6,094 | 6,094 | 2,368 | 2,368 |
| R-squared | 0.158 | 0.159 | 0.277 | 0.281 |
| Mean of dependent variable | 0.456 | 0.456 | 0.548 | 0.548 |
| Panel B: Constitutional and Criminal Cases Disaggregated | | | | |
| | Constitutional Cases | | Criminal Cases | |
| | (1) | (2) | (3) | (4) |
| | Human-Rights Cases | Land Cases | Non-Islamic Case | Islamic Case |
| | <i>State Wins</i> | | | |
| Cumulative Retirements Since 2010 | -0.261*** [0.0655] | -0.259*** [0.0746] | -0.374 [0.337] | -0.0529 [0.551] |
| District-by-Year FE | Yes | Yes | Yes | Yes |
| Case and Bench Controls | Yes | Yes | Yes | Yes |
| Observations | 3,428 | 2,650 | 2,143 | 225 |
| R-squared | 0.220 | 0.217 | 0.285 | 0.763 |
| Mean of dependent variable | 0.462 | 0.449 | 0.552 | 0.520 |

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. Cumulative Mandatory Retirements Since 2010 is fraction of mandatory retirements in a given district bench from 2010-2019 as predicted by age structure in 2010. Panel A show that our results disaggregated into constitutional and criminal cases. The constitutional and criminal cases do not add to the 8446-case sample because 16 criminal cases were also marked as constitutional. Panel B shows further disaggregation of constitutional cases into human rights and land cases. The criminal cases are disaggregated into those judged under Islamic limits law and those judged under common law. The controls include all case and district bench characteristics shown in Table 1. The case controls also include case-type fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table C9: Savings in Land Expropriations Avoided Due to Selection Reform

| Panel A: Land Expropriations Results | | | |
|---|---------------|---|--|
| | (1) | (2) | (3) |
| | Average Value | Minimum Value (<i>Most Conservative</i>) | Maximum Value (<i>Least Conservative</i>) |
| Total Land Expropriated (% of GDP) | 0.828 | 0.333 | 1.693 |
| Counterfactual: No Selection Reform (% of GDP) | 0.930 | 0.446 | 1.693 |
| Land Expropriations Avoided Due to Selection Reform (% of GDP) | 0.102 | 0.067 (2015) | 0.282 (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 | 1.441079947 | 0 |
| 2008 | 170077814106 | 2880000000 | 1.693342553 | 1.693342553 | 0 |
| 2009 | 168152775283 | 1700000000 | 1.010985395 | 1.011985395 | 0 |
| 2010 (max) | 177165635077 | 2494000000 | 1.407722214 | 1.689897545 | 0.282175332 |
| 2011 | 213587413184 | 851000000 | 0.398431718 | 0.480711789 | 0.080128007 |
| 2012 | 224383620830 | 2320000000 | 1.033943561 | 1.241868048 | 0.207924486 |
| 2013 | 231218567179 | 1400000000 | 0.605487707 | 0.727007199 | 0.121519492 |
| 2014 | 244360888751 | 1150000000 | 0.470615411 | 0.565620954 | 0.094005544 |
| 2015 (min) | 270556131701 | 900000000 | 0.332648162 | 0.399208387 | 0.067260225 |
| 2016 | 278654637738 | 1679000000 | 0.602537971 | 0.723455717 | 0.121917746 |
| 2017 | 304567253219 | 1680000000 | 0.551602309 | 0.662412918 | 0.110526871 |
| 2018 | 314567541558 | 1690000000 | 0.537245512 | 0.645843597 | 0.107598084 |
| 2019 | 278221906023 | 1904000000 | 0.684345826 | 0.821952374 | 0.137606548 |

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: 0.135 percent of GDP) from its implementation onward. The procedure for this calculation is as follows: given that in 20% of our 8500 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 850,000. 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 2010-2019. Panel B presents yearly figures showing the minimum and maximum value of total land expropriations, counterfactual land expropriations, and expropriations avoided.

Table C10: The Impact of Selection Reform on State Wins (by Chief Justice)

| | <i>State Wins</i> | | | | | | |
|------------------------------------|---------------------|---------------------|---------------------|----------------------|---------------------|---------------------|---------------------|
| | CJ Khosa (1) | CJ Nisar (2) | CJ Jamali (3) | CJ Khawaja (4) | CJ Mulk (5) | CJ Jillani (6) | CJ Chaudry (7) |
| Retirements in 2010 X Post 2010 | -0.115* (0.0605) | -0.137* (0.0693) | -0.135* (0.0689) | -0.130** (0.0605) | -0.136* (0.0702) | -0.0903 (0.0699) | -0.0793 (0.0795) |
| District-by-Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 7,019 | 7,284 | 7,236 | 7,083 | 6,944 | 6,865 | 6,781 |
| R-squared | 0.134 | 0.142 | 0.141 | 0.128 | 0.122 | 0.124 | 0.124 |
| Mean Dep. Var. | 0.517 | 0.507 | 0.506 | 0.514 | 0.518 | 0.524 | 0.525 |
| Tenure of CJ | 2019-2019 | 2016-2019 | 2015-2016 | 2015-2015 | 2014-2015 | 2013-2014 | 2007-2013 |

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 bench 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 Case and Bench Characteristics – Cumulative Retirements

| | (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 | No. Judges on Bench | No. Criminal cases on Bench | No. Land cases on Bench | No. Human Rights Cases on Bench |
| Cumulative Retirements Since 2010 | -0.000643 (0.00430) | -0.0156*** (0.00502) | 0.347 (0.504) | -0.0185 (0.0177) | 0.113 (0.158) | -0.0152 (0.0561) | 0.770* (0.444) | 2.164* (1.177) | 0.488 (0.683) | 0.750 (0.668) |
| District-by-Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 8,446 | 8,446 | 8,446 | 8,446 | 8,446 | 8,446 | 8,446 | 8,446 | 8,446 | 8,446 |
| R-squared | 0.991 | 0.991 | 0.292 | 0.110 | 0.068 | 0.138 | 0.910 | 0.763 | 0.663 | 0.662 |
| Mean of dependent variable | 0.722 | 0.280 | 8.877 | 0.065 | 4.123 | 1.809 | 8.025 | 2.77 | 3.476 | 2.586 |

Robust standard errors appear in brackets (clustered at the district-bench level). Cumulative Mandatory Retirements Since 2010 is fraction of mandatory retirements in a given district bench from 2010-2019 as predicted by age structure in 2010. The controls include case and bench characteristics outlined in Table 1 (except the dependent variable used in the respective column). The case controls include case-type fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table C12: The Impact of Selection Reform on Case Filings

| | (1) | (2) | (3) |
|---------------------------------|-------------------|----------------------|-------------------|
| | Total Filed | Constitutional Filed | Criminal Filed |
| Retirements in 2010 X Post 2010 | -7,659 [6,253] | -5,666 [4,453] | -2,023 [1,805] |
| District and Year FE | Yes | Yes | Yes |
| District and Case Controls | Yes | Yes | Yes |
| Observations | 491 | 491 | 491 |
| R-squared | 0.955 | 0.955 | 0.950 |
| Mean of dependent variable | 9935.959 | 7153.159 | 2784.768 |

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. The filing variable is only available at district-year level so we *cannot* add district-by-year fixed effects (we instead add district and year FEs separately). Retirements in 2010 is fraction of mandatory retirements in a given district bench in reform year 2010. The controls include all case and district characteristics available. The case controls also include case-type fixed effects. *** p<0.01, ** p<0.05, * p<0.1

Table C13: Impact of Selection Reform on State Wins (excluding cases from Political Capitals)

| | <i>Excluding Lahore</i> | <i>Excluding Karachi</i> | <i>Excluding Quetta</i> | <i>Excluding Peshawar</i> | <i>Excluding Islamabad</i> |
|---------------------------------|-----------------------------|------------------------------|-----------------------------|-------------------------------|--------------------------------|
| | <i>State Wins</i> | | | | |
| Retirements in 2010 X Post 2010 | -0.207*** [0.0401] | -0.198*** [0.0397] | -0.218*** [0.0383] | -0.235*** [0.0359] | -0.199*** [0.0499] |
| District-by-Year FE | Yes | Yes | Yes | Yes | Yes |
| Bench and Case Controls | Yes | Yes | Yes | Yes | Yes |
| Observations | 7,363 | 6,945 | 7,913 | 8,295 | 8,388 |
| R-squared | 0.154 | 0.172 | 0.152 | 0.148 | 0.144 |
| Mean of dependent variable | 0.479 | 0.483 | 0.474 | 0.481 | 0.482 |

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 bench 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 C14: Are the results robust to excluding particular benches

| | Excluding Criminal Bench | Excluding Property Bench | Excluding Tax Bench | Excluding Writ Bench |
|---------------------------------|--------------------------------|--------------------------------|------------------------|-------------------------|
| <i>State Wins</i> | | | | |
| Retirements in 2010 X Post 2010 | -0.264*** [0.0279] | -0.207*** [0.0657] | -0.185*** [0.0557] | -0.144** [0.0684] |
| District-by-Year FE | Yes | Yes | Yes | Yes |
| District and Case Controls | Yes | Yes | Yes | Yes |
| Observations | 6,194 | 6,823 | 6,266 | 6,487 |
| R-squared | 0.165 | 0.171 | 0.165 | 0.176 |
| Mean of dependent variable | 0.435 | 0.485 | 0.495 | 0.483 |

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 bench 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 C15: Is a particular bench driving the results?

| | Criminal Bench | Property Bench | Tax Bench | Writ Bench |
|---------------------------------|-------------------|---------------------|-----------------------|-------------------|
| <i>State Wins</i> | | | | |
| Retirements in 2010 X Post 2010 | 0.363 [0.550] | -0.358** [0.138] | -0.249*** [0.0673] | -0.161 [0.153] |
| District-by-Year FE | Yes | Yes | Yes | Yes |
| District and Case Controls | Yes | Yes | Yes | Yes |
| Observations | 2,396 | 1,767 | 2,324 | 2,103 |
| R-squared | 0.101 | 0.060 | 0.091 | 0.079 |
| Mean of dependent variable | 0.547 | 0.456 | 0.437 | 0.468 |

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 bench characteristics shown in Table 1. The case controls also include case-type fixed effects. Every column considers cases for the mentioned specialized bench. *** p<0.01, ** p<0.05, * p<0.1.

Table C16: Impact of Selection Reform on State Wins with different starting years

| | State Wins | | | |
|---------------------------------|-----------------------|-----------------------|-----------------------|-----------------------|
| | (1) | (2) | (3) | (4) |
| | 1990-2019 | 1995-2019 | 2000-2019 | 2005-2019 |
| Retirements in 2010 X Post 2010 | -0.210*** [0.0385] | -0.225*** [0.0367] | -0.242*** [0.0345] | -0.249*** [0.0353] |
| District-by-Year FE | Yes | Yes | Yes | Yes |
| District and Case Controls | Yes | Yes | Yes | Yes |
| Observations | 7,900 | 6,954 | 5,900 | 4,755 |
| R-squared | 0.152 | 0.158 | 0.163 | 0.167 |
| Mean of dependent variable | 0.476 | 0.470 | 0.460 | 0.438 |

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 bench 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 C17: The Effect of Reform on State Wins on aggregated district-bench-year panel

| | OLS | | IV, 2 nd Stage | |
|----------------------------------|-----------------------|-----------------------|---------------------------|-----------------------|
| | (1) | (2) | (3) | (4) |
| | State Wins | | | |
| Appointments in 2010 X Post 2010 | -0.264*** [0.0821] | -0.223*** [0.0775] | -0.284*** [0.0703] | -0.243*** [0.0725] |
| District-by-Year FE | Yes | Yes | Yes | Yes |
| District and Case Controls | No | Yes | No | Yes |
| Observations | 1,529 | 1,529 | 1,529 | 1,529 |
| R-squared | 0.415 | 0.440 | 0.415 | 0.440 |
| Mean of dependent variable | 0.494 | 0.494 | 0.494 | 0.494 |

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 bench 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 in 2010 X Post 2010. This is instrumented with Retirements in 2010 X Post 2010. *** p<0.01, ** p<0.05, * p<0.1.

Table C18: The Impact of Selection Reform on State Wins – Non-Linear Models

| | Logit Marginal Effects | | Probit Marginal Effects | |
|---------------------------------|------------------------|----------------------|-------------------------|----------------------|
| | (1) | (2) | (3) | (4) |
| <i>State Wins</i> | | | | |
| Retirements in 2010 X Post 2010 | -0.299*** [0.047] | -0.258*** [0.051] | -0.280*** [0.041] | -0.241*** [0.045] |
| District-by-Year FE | Yes | Yes | Yes | Yes |
| District and Case Controls | No | Yes | No | Yes |
| Observations | 8,167 | 8,167 | 8,167 | 8,167 |
| R-squared (Pseudo) | 0.083 | 0.089 | 0.083 | 0.088 |
| Log-likelihood | -5186.08 | -5155.77 | -5186.89 | -5165.20 |
| Mean of dependent variable | 0.490 | 0.490 | 0.490 | 0.490 |

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 bench characteristics in Table 1 and case-type fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table C19: The Impact of Selection Reform on State Wins at Different Levels of Clustering

| | Before-After Clustering | Before- After Clustering | District- Level Clustering | Bootstrap Clustering |
|---------------------------------|----------------------------|--------------------------------|----------------------------------|-------------------------|
| | District- Bench | District | | |
| | (1) | (2) | (3) | (4) |
| <i>State Wins</i> | | | | |
| Retirements in 2010 X Post 2010 | -0.202*** [0.0659] | -0.202** [0.0967] | -0.202*** [0.0412] | -0.191*** [0.0571] |
| District-by-Year FE | Yes | Yes | Yes | Yes |
| District and Case Controls | Yes | Yes | Yes | Yes |
| Observations | 8,446 | 8,446 | 8,446 | 8,446 |
| R-squared | 0.142 | 0.142 | 0.142 | 0.142 |
| Mean of dependent variable | 0.482 | 0.482 | 0.482 | 0.482 |

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 et al. (2008), see Figure C5 for these results. The controls include all the case and district bench characteristics in Table 1 and case-type fixed effects. *** p<0.01, ** p<0.05, * p<0.1

Table C20: Robustness to using data coded by Team 2

| | (1) | (2) | (3) | (4) | (5) |
|---------------------------------|-----------------------|---------------------|----------------------|--------------------------|-------------------------|
| | <i>State Wins</i> | <i>Case Delay</i> | <i>Merit</i> | <i>Correct Decisions</i> | <i>Process Followed</i> |
| Retirements in 2010 X Post 2010 | -0.151*** [0.0437] | -0.907** [0.376] | 0.185*** [0.0326] | 0.181*** [0.0515] | 0.322** [0.126] |
| District-by-Year FE | Yes | Yes | Yes | Yes | Yes |
| Case and Bench Controls | Yes | Yes | Yes | Yes | Yes |
| Observations | 8,446 | 8,446 | 8,446 | 8,446 | 8,446 |
| R-squared | 0.117 | 0.217 | 0.132 | 0.094 | 0.073 |
| Mean of dependent variable | 0.563 | 3.336 | 0.672 | 0.445 | 3.222 |

Robust standard errors appear in brackets (clustered at district-bench level). In Column 1, the dependent variable is State Wins, a dummy variable for the case being ruled in favor of the State. In Column (2), the dependent variable is case delay i.e. the difference between filing and decision year. In Column (3) it is a dummy variable for the case being ruled on case merits. In Column (4), the dependent variable is a dummy variable for the case being judged as correct by legal experts. In Column (5), the dependent variable is a rating from 1-5 on due process followed. 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 bench characteristics shown in Table 1. The case controls also include case-type fixed effects. The variables coded by Team 2 are used in this Table. *** p<0.01, ** p<0.05, * p<0.1

Disclosure Statement

The author declares that I have no relevant or material financial interests that relate to the research described in this paper.

Thank you.

Sultan Mehmood

A handwritten signature in black ink, appearing to read 'Sultan Mehmood', written in a cursive style.