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Essays on Judicial Independence and Development

Soutenue par

Sultan Mehmood

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Introduction

Independence of judiciary is considered a key ingredient for any modern democratic society. Yet, little is understood on how independence of the judiciary arises and what are its implications on judicial decision making and development, particularly in developing countries. In my dissertation, *Essays on Judicial Independence and Development*, I study shocks to independence of judiciary in Pakistan and examine its impact on judicial decision making and development.

The case of Pakistan is interesting to study independence of judiciary for several reasons. First, it is a large developing country with a population of 207 million people, 40 million living below the poverty line and where about 50% of the judicial cases take 5 to 10 years to be decided. Second, despite its economic underdevelopment and weak institutional structure, Pakistan seem to have a relatively independent judiciary when compared to countries with similar levels of development (e.g. Nigeria, Sudan or Vietnam). One indication of this is the fact that the last three heads of state in Pakistan were removed by the courts in cases pertaining to corruption of public officials. Third, the political instability and turnover in Pakistan, implies that the courts have undergone several shocks, upheavals and reforms that allow us to assess whether and how these natural experiments impacted the independence of the judiciary and, consequently, the economy.

In first chapter of my dissertation, I study how the institution of Presidential appointment impact judicial decision making and development. I show that a shift from presidential appointment of judges to appointment of judges by judicial commission consisting of peer judges reduces ruling in favour of the government and that this reduction is reflected in improved quality of judicial decisions as measured by reduced case delay and increased ruling on evidence of the case. Using mandatory retirement age as an instrument for new appointments allows me to estimate the causal effect of the reform. The analysis of the contents of the cases reveal that reduced rulings in favour of the government in politically salient government expropriation claims against the public is key in explaining these results. We also present evidence that the change in selection reform reduces distortions in the economy due to favouritism. In particular, we show that regions that had more judges appointed by the judicial commission experience an increase in investments in construction industry and a rise house prices.

In the second chapter of the dissertation, I study judicial independence from religious leaders. I document a substantial impact of religious leaders on judicial decision making in Pakistan. Utilizing a unique dataset on 13th century holy Muslims shrines across Pakistan, I show that districts where historically the shrine density was high, a military coup in 1999 induced a large decline in judicial independence and quality of judicial decisions. The evidence I present is consistent with the mechanism that military elite co-opted religious elite where increased political power of religious leaders allowed them to influence the courts. In particular, it is documented the impact of shrine density on the courts is only observed where local religious leaders gained political power and only in the cases involving disputes with the local government. I also show the judicial selection reform that changed the appointment procedure to select judges from presidential appointment to selection by a judicial commission consisting of peer judges mitigates the effect of historical shrine density on judicial outcomes.

In the final chapter of my dissertation, together with a fellow graduate student, Avner Seror, we study political economy of foreign aid and development in Pakistan. We present a theory that reconciles two views in the literature that foreign aid has a positive effect on economic growth but also funds patronage, corruption and favouritism. We build a theory that formalizes how foreign aid impacts growth, leader turnover and patronage. We show that when political institutions are weak, foreign aid is misallocated to home province of incumbent leader. Nevertheless, foreign aid may also increase the dynamic efficiency of public policies, making the effect of foreign aid on growth ambiguous. We present evidence consistent with the predictions of the model where foreign aid decreases leader turnover, increases misallocation while it has a positive and significant effect on economic growth. The identification strategy we propose allows us to provide causal evidence for the predictions of the model.

Chapter 1 - Judicial Independence and Development: Evidence from Pakistan

Job Market Paper

BY SULTAN MEHMOOD*

September 28th, 2019

To what extent does the Presidential appointment of judges impact judicial decision making? This paper provides causal evidence that the institution of Presidential appointment exerts considerable influence on judicial decision making in Pakistan. We find that a change in selection procedure of judges from Presidential appointment to appointment by a judicial commission (consisting of peer judges) significantly reduces rulings in favour of the government and that this reduction reflects an improvement in the quality of judicial decisions. Using mandatory retirement age as an instrument for new appointments allows us to estimate the causal effect of the reform. We test for, and provide evidence against, potential threats to identification and alternative explanations to our findings. The analysis of case content reveals that the results are explained by rulings in politically salient cases. We find evidence of selection effects mechanism: judges appointed by the President are more likely to have been politically active prior to their appointment. We also find a positive welfare effect of the reform, with lower expropriation risk leading to increased investment in the construction industry and higher house prices. (*JEL D02, O17, K40*).

Keywords: Presidential appointment, judicial distortions, judges, courts.

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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*]

I. Introduction

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

In this paper, we provide causal evidence that the institution of Presidential appointment exerts considerable influence on judicial decision-making. We consider a 2010 change in the selection procedure for judges in Pakistan, from a system of Presidential appointment similar to that in the United States or Brazil to a judicial commission-based selection procedure (appointment by peer judges) as in many European countries such as Sweden or the UK. We ask whether this judicial-

selection reform affected judicial outcomes and, if so, which mechanisms link the Presidential appointment of judges to judicial decision-making.

A number of anecdotal accounts suggest that the selection reform affected judicial decision-making in Pakistan. For instance, a bench where four out of the five judges were selected by the judicial commission ruled that the incumbent executive head, the Prime Minister Nawaz Sharif, be removed from office on account of his “undeclared assets” and “living beyond means” (Reuters, 2017).¹ The “disqualification” of the Prime Minister, a business tycoon, and the leader of a party with a two-thirds majority in Parliament, makes the judgement all the more salient. Similarly, in another judgement where all three judges were selected by the judicial commission, the Islamabad High Court, in a unanimous verdict, removed the Foreign Minister from office when it was adjudicated that he had “*deliberately and wilfully not disclosed his status as an employee of the foreign company, nor receiving of the salary per month*” while running for office.² This is in stark contrast to judgments involving individuals holding executive offices by Presidentially-appointed judges. For example, a bench of judges appointed by the President ruled that the “*President may, in the larger public interest perform all legislative measures which are in accordance with, or could have been made under the Constitution, including the power to amend it*”.³

¹The Court proceedings started in 2015 following the “Panama Papers” scandal, which consisted of 11.5 million leaked documents describing ownership and financial information for more than 200,000 offshore companies. These document leaks found firms linked to Prime Minister Nawaz Sharif and his family.

²The judgement concludes that it was “*obvious from the facts and circumstances in the instant case that the Respondent (Foreign Minister Khawaja Asif) had deliberately and wilfully not disclosed his status as an employee of the foreign company, nor receiving of the salary per month pursuant thereto ... working as an employee of the Company and receiving a substantial salary without being physically present, which is AED 50,000/- per month (USD 13, 600/- per month), were some benefits gained from non-disclosure. Disclosure would have led to giving the hefty salary paid by the Company for some advice sought telephonically by a foreign-based employer from the Foreign Minister of Pakistan. We have deeply pondered but could not persuade ourselves that this deliberate and wilful non-disclosure was an honest omission*”. (*The State vs. Usman Dar*, reported in *The News*, 2018).

³*Tikka Khan vs. The State*, 2008, PLD, p. 178.

Nevertheless, the more common cases with the government as a litigant in Pakistan concern land disputes with State agencies expropriating land or withholding payments (Gulf News, 2009).⁴ For instance, when the “*Grievance of plaintiff was that despite completion of project, authorities had withheld his payments*” the judge selected by the judicial commission ruled that the government had “*committed deliberate and wilful breach of the agreement*” and ordered the government to “*pay amount of balance outstanding of disputed running bill ... (and) pay damages*” (*Altaf Hussain vs. The State*, CLC, 2013, p. 284). This contrasts with the decisions in many such cases prior to the reform. For example, in a case where the bench consisted of Presidentially-appointed judges, a similar “petition was dismissed” on a technicality (*Khalid Mohsin vs. The State*, CLC, 2005, p. 745).

We argue that these examples are suggestive of a broader change in judicial decision-making in Pakistan following the selection reform. To systematically examine the influence of this reform on judicial decision making, we randomly sample the universe of cases in Pakistan district High Courts and obtain information on 7500 cases from 1986 to 2016.⁵ Our measure of executive influence over the judiciary is a judicial-dependence dummy variable “State Wins”, taking value 1 for “State victories” and 0 for “State losses” in a case when the State is one of the parties. Following the literature, we asked a Law firm to code this variable (as in Djankov et al., 2003; La Porta et al., 2008).⁶

⁴The government has been repeatedly accused and convicted of usurping private land through many notorious State agencies, chief among them the Lahore Development Authority (LDA), the Capital Development Authority (CDA) and the Karachi Development Authority (KDA).

⁵More information on sampling is provided in the data section, with further details in the data-construction section in Appendix B.3.

⁶Law firms coded 1 if the State ‘won’ and 0 otherwise. Typically, in cases that the State Wins, the judgement text contains phrases such as “*Case against the State is dismissed*” and when the State loses “*Petition against the State is accepted*”. We later report the correlation coefficient between two independent coding of the State Wins variable (more details can be found in the data section and Appendix).

Judicial cases involving the government as a party in Pakistan cover a wide range of disputes, from simple commercial disputes to blasphemy, the political victimization of opposition politicians, the suppression of fundamental rights and the constitutionality of Military Rule.⁷ Nevertheless, a substantial portion of the petitions (about 40% of all petitions filed in High Courts) involving the State as a litigant involve land expropriation and ownership disputes with the government.⁸ When the government expropriates land, courts are generally the only recourse left for citizens to recuperate their property (La Porta et al., 2008). On November 29th, 2017, a Court consisting of judges appointed by the judicial commission ordered the Karachi Development Authority to return 35,000 “public encroachments” to their owners (The News, 2017). Similar instances of land expropriation by government agencies have been reported elsewhere in India, Ghana and China (BBC, 2013; Gadugah, 2017).⁹

Figure 1 generalizes these anecdotal accounts of less-favourable rulings for the State following the 2010 reform to about 7500 cases. Prior to the reform around 50% of cases were decided in the favour of the State, as opposed to about 40% following the selection reform (Panel A). These differences are both qualitatively and statistically significant. A similar pattern emerges comparing cases decided by judges appointed by the President vis-à-vis those appointed by the judicial commission (Figure 1, Panel B).

⁷Petitions against the State include many important cases around the world. See, for example, the cases challenging the apartheid government in South Africa, *The State vs Nelson Mandela* (1963) and the bus segregation of African-Americans in the United States (*The State vs Martin Luther King*, 1956), or that invalidating Laws prohibiting interracial marriages (*Mildred Loving vs. The State*, 1963). The analysis of executive constraints and judicial dependence in this context has “*obvious value for securing ... political rights when the government is itself a litigant*” La Porta et al. (2004, p. 447).

⁸By 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).

⁹Many such cases abound, with the most recent (high-profile) example in India dating to only February 8th 2018, when the Police booked a land grab case against Giriraj Singh, who was heading a government agency (Times of India, 2018). Mr. Singh, who is pending trial, is accused of facilitating the illegal “land grab” of a scheduled class villager in the Indian State of Bihar.

Figure 1 cannot however be interpreted as conclusive evidence for a causal link between the change in the judicial-selection procedure and judicial outcomes, as a number of changes occurred around the selection-reform year of 2010. For instance, the transition from military to democratic rule was in 2008. Likewise, there was a social movement in 2007 by lawyers in Pakistan demanding a more independent judiciary. Similarly, in 2010 the President's power to unilaterally terminate the legislature was also removed from the constitution. The global fall in the fraction of rulings in favour of the government following the selection reform could be explained by any of these changes. We address this concern by focussing on the staggered implementation of the reform: new judicial appointments are staggered in district courts due to limited vacancies in the respective districts in a given year. That is, judges appointed by the President had to retire for the judges appointed by the judicial commission to replace them. For example, in 2016, 90% of judges in the district High Court of Peshawar were selected by the judicial commission, while the corresponding figure in the Sukkur High Court was only 40%.

However, a simple difference-in-differences estimate of the fraction of judges appointed by the judicial commission on State Wins may not yield the causal effect of the reform, due to the potential reassignment of judges across districts. Indeed, Iyer and Mani (2012) show that the reassignment power of Indian politicians allowed them to exert substantial control over bureaucrats.¹⁰ This is a plausible concern here, where an independent judge in Pakistan might be reassigned to a different district High Court. This reintroduces endogeneity concerns.

We hence propose an instrumental-variable strategy, and instrument the fraction of judges appointed by the judicial commission by the fraction of judges who attain the mandatory retirement

¹⁰ They show that even though Indian politicians cannot dismiss bureaucrats, the threat of reassignment to an alternative district allowed them to exert substantial control over them.

age of 62 following the selection reform (this age has been the same since 1969). Figure 2 shows that appointments by judicial commission are highly correlated with mandatory retirements, as 91% of judges in Pakistan serve out their full term and only retire on their 62nd birthday.¹¹ Under the assumption that judges reach their 62nd birthday randomly across district-years, we can address the concern that a more independent judge might be transferred to a different district High Court, or that district characteristics might be correlated with the staggered implementation of the selection reform. This, together with the random allocation of cases across judges and Pakistani jurisdiction laws preventing litigants from choosing the districts in which they file the case, allows us to estimate the causal effect of selection reform on judicial outcomes (Ponticelli and Alencar, 2016). We present evidence consistent with this identification assumption via a balance test, showing that the reform is not correlated with our observable case and district characteristics.

The Presidential appointment of judges substantially affects judicial decisions: a 10% rise in judges selected by the judicial commission reduces State Wins by about 4 percentage points. We present evidence that this reduction in State Wins reflects an improvement in the quality of judicial decisions, consistent with the anecdotal evidence (Haq, 2018; Arshad, 2017). First, judges appointed by the judicial commission are more efficient, with a 10% rise in judicial-commission judges reducing case delay by about two months. Second, judicial-commission judges are more likely to rule based on case merits or evidence than the specific technicalities of the law: a 10% increase in judges appointed by the judicial-commission increases merit rulings by 5.5 percentage points.¹² The results are robust, with the OLS and IV coefficients being statistically and qualitatively similar.

¹¹ The remaining 9% of judges are either promoted to the Supreme Court (3%) or die in office (6%).

¹² Rulings on merit imply that the judicial decision is “*based on evidence rather than technical or procedural grounds*” (Pound, 1963). Likewise, anecdotal accounts from Pakistan suggest that ruling on technicalities in Pakistan is a

There remain three key identification threats that can prevent the causal interpretation of the selection reform on judicial decision making. First, it might be the case that we are picking up a pure appointment effect. If, for instance, new appointment has an independent effect on judge behaviour then we would be picking up the effect of new appointments instead of the change in the judicial selection procedure. Second, the correlation may reflect a pure retirement effect. If it takes time for the judge-State relationship to develop, then we may pick up the impact of judge experience and other possibly unobservable judge characteristics related to his or her retirement. The use of judges' age as a control may not completely resolve this problem. Third, there may have been an unobserved shock long before the reform that is correlated with the implementation of the selection reform. We provide evidence against each of these possibilities. To examine if we are picking up a pure appointment or retirement effect, we conduct falsification tests. We show that new judge appointments and retirements have an effect on State Wins only after the reform, with no effect of pre-treatment appointments and pre-treatment retirements on judicial decision-making. Regarding a past unobserved shock, we present two pieces of evidence. We first show that there are no differential trends prior to the selection reform. Second, we demonstrate the robustness of the results via the logic of a regression discontinuity design: the results are similar when we limit the sample to cases just before and after the reform.¹³

We test for and reject alternative explanations to our finding that the judge-selection reform changed judicial decision-making in Pakistan. We show that the effect of selection reform is not

“weapon of choice to rule unfairly” (Haq, 2018) and that judges use decisions on technicalities to *“favour the state authorities”* (Arshad, 2017).

¹³We do so as cases just around the reform year are plausibly more similar to each other than those that are further away. The finding of similar coefficient estimates to those in the full sample suggests that unobserved differences in case characteristics are unlikely to be behind the results.

President- or Chief Justice-specific.¹⁴ The results are equally not driven by the 2008 transition from Military to Democratic Rule. We further provide evidence that the reform is not confounded by district specific trends. First, we find similar results when we control for district-specific linear trends in the baseline specification. Second, we conduct a permutation inference falsification test where we find no effect of the reform when we randomly scramble the districts. We also conduct a number of additional sensitivity tests showing that the results are robust to different levels of clustering, district-year aggregation, the strategic filing of cases and non-linear estimation.

We next consider the mechanisms, and first the type of cases behind these results. These turn out to be politically salient cases involving land and human-rights disputes with the State. We carry out a placebo test to examine this political-influence mechanism using criminal cases (these also involve the State, which acts as the Prosecutor). We find no effect of the judge-selection reform on State Wins in quotidian criminal cases.¹⁵

We also ask which type of judges are driving the results. We find that Presidential and judicial-commission judges are similar in terms of many characteristics such as age, tenure, gender and experience. Nevertheless, those appointed by the judicial commission are 35% less likely to have run for political office prior to their appointment. This is consistent with Presidential selection favouring more political judges (who rule in favour of the State more often, take longer to adjudicate and are less likely to rule on the merits of the case).¹⁶

¹⁴We can test this empirically since we have judges appointed by six different Presidents serving under five different Chief Justices in the sample.

¹⁵State Wins in this case is the conviction rate. An inspection of a random sample of 100 criminal cases from our sample reveal that these are plausibly politically less salient, as most cases involve bail pleas for theft and burglary (the categorization of the alleged crimes in these 100 cases is available on request).

¹⁶We discuss the link between these judicial outcomes (in context of a simple signalling framework) in the mechanism section.

We last link the selection reform to the recent housing boom in Pakistan. In the period since the reform, house prices in Pakistan more than doubled (Zameen, 2018). If, consistent with the anecdotal accounts, the judges appointed by the judicial commission reduced expropriation risk in the housing sector, we should observe increased investments in industries close to the housing sector where more judges were appointed by the judicial commission. Although, here we have data available at the provincial level and on investments in the construction industry, we find evidence suggestive of this. We observe that provinces that had more judges appointed by the judicial commission experienced an increase in investments in the construction industry relative to provinces that had more judges appointed by the President. A back-of-the-envelope calculation suggests that the replacement of all judges appointed by the President by judicial-commission judges increased construction-industry investment by about USD 600, 000. Consistent with this expropriation mechanism, we also find that the district High Courts that had more judges appointed by the judicial commission experienced an increase in house prices. A similar back-of-the-envelope calculation suggests that the selection reform increased house prices by 15%.

This paper relates to several strands of literature. We first contribute to the literature on selection of public officials (Hanssen, 1999; Guerriero, 2011; Shvets, 2016; Acemoglu et al., 2017). Most of this literature has focused on selection of politicians (Jones and Olken, 2005; Besley, 2005; Dal Bo et al., 2017; Hessami, 2018). The little literature on judge selection that exists has focused on selection via elections versus executive appointment (Lim, 2013; Besley and Payne, 2013), or via elections versus a judicial commission in the United States (Ash and MacLoed, 2019). Our key contribution here is to show the effect of selection via Presidential appointment versus judicial commission, and to document the causal effect of a change in judicial selection on judicial adjudication and economic outcomes in a developing country. We are, thus,

able to speak to a fundamental question in economics and political science that how judicial independence may arise in a country where democratic institutions are weak to begin with (Montesquieu, 1748; Hayek 1960; North and Weingast, 1989).

Second, we contribute to the large 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). As we appeal to variation across districts subject to the same national institutions, we overcome many of the common identification issues arising in work looking at differences between countries. Last, our work is also related to the literature on judge behaviour. Most recent work has focused on judge behaviour in criminal cases (Chalfin and McCrary, 2017; Cohen and Yang, 2019), the role of racial bias in criminal sentencing (Alesina and La Ferrara, 2014; Rehavi and Starr, 2014; Arnold et al., 2018), and extraneous factors affecting judge sentencing such as lunch breaks (Danziger et al., 2011), terrorism (Shayo and Zussman, 2011) and temperature (Heyes et al., 2019). We here reveal a political-selection mechanism: judge behaviour 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 on the judicial system of Pakistan and describes the specifics of the reform. Section III presents the data, their sources and descriptive statistics. Section IV describes the empirical methodology. Section V presents and discusses the main results, and Section VI provides evidence on the mechanisms. Section VII rules out alternative explanations and discusses a battery of robustness checks. Section VIII concludes. Further information on the data construction, variable descriptions and additional robustness checks is presented in the Appendices.

II. Background

A. The Judicial Structure in Pakistan

The judicial system in Pakistan is a three-tier hierarchical structure. At the lowest level are the civil and session courts hearing civil and criminal cases respectively. Decisions in these courts can be challenged in the district High Courts of Pakistan. Important for our study is the fact that in the High Court, an individual can file a case against the government. This takes the form of a constitutional petition against the State. Cases with the State as a responding party involve the federal government, provincial governments, local governments, government agencies or any organ of the State with executive authority (such as the office of the President or the Prime Minister).

From 1986 to 2016, about 70% of all cases filed in the High Courts were “constitutional petitions” in nature, and the majority of these involved government housing agencies responding to land-dispute claims with the public.¹⁷ If the government expropriates land or violates a fundamental right, the High Court is the first and in most cases the only platform for the remediation of individuals and firms. There are 16 district High Court benches in Pakistan.¹⁸ Figure 3 shows the location of the High Court benches and their respective jurisdictions. Last, there is the final appellate Court, the Supreme Court of Pakistan, located in the federal capital. This typically hears criminal and constitutional appeals from the High Courts. The Supreme Court can have at most 16 judges, which greatly limits the number and scope of the cases it can hear. As such, only a small fraction of cases end up being heard by the Supreme Court (Haq, 2018).

¹⁷The remaining 30% of the cases consists of criminal appeals from the session court.

¹⁸Although, in theory there are four provincial High Courts in Pakistan, in practice each of Pakistan’s four provinces contain about four district High Court benches.

B. The Judicial Selection Reform

In April 2010, the ruling Pakistan People’s Party tabled a constitutional amendment to Parliament that would dramatically change the process of judicial appointment in Pakistan.¹⁹ 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). With this amendment the following clause was removed 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; 2018).²⁰

The judicial commission consists of the *“Chief Justice of Supreme Court and 4 senior most judges, a former judge (nominated by the Chief Justice of Pakistan), federal law minister, and the attorney general of Pakistan, along with a senior advocate of Supreme Court nominated by the Pakistan Bar Council for two years.”* (Constitution of Pakistan, 2010; 2017).²¹

¹⁹More information on the political landscape at the time of the selection reform can be found in Appendix B.2.

²⁰Furthermore, Article 209 of the Constitution stipulates that judges can only be removed through filing a reference to their peers, which was unchanged by the reform (Constitution of Pakistan, 2017).

²¹For the appointment of High-Court judges as in the present case, all of the above members plus the provincial Chief Justice, provincial Law Minister, the most senior judge of the provincial High Court, and a lawyer nominated for two years by the provincial Bar Council also sit in the judicial commission.

After the amendment was enacted in April 2010, Supreme and High Court judges were appointed by a judicial commission (consisting of peer judges and senior lawyers) with no Presidential involvement.²² Many accounts suggest that the effective appointment power of the executive was severely curtailed by this reform, as judges form the overwhelming majority (6/9) in the commission (Ijaz, 2014; Iqbal, 2015).²³ We interpret this shift from the Presidential appointment of judges to their selection by a judicial commission as a *de jure* reduction in executive control over the judiciary, and evaluate its impact on judicial adjudication.²⁴

According to many political observers, the Eighteenth Amendment was introduced by the incoming democratic government following a decade of military rule to reduce the political authority of the President.²⁵ It was argued that the purpose of the change in selection procedure of judges was to prevent potential abuses of power by future military rulers who ruled as Presidents (Almeida, 2018). More information on the reform as well as the political landscape at the time of the reform, structure and history of courts in Pakistan can be found in Appendix B.

²²The Attorney General and the Law Minister are Lawyers and represent the executive branch of the government.

²³The 18th amendment also created a Parliamentary Committee consisting of four members from the treasury and four from the opposition. Nominations by the judicial commission have to be confirmed by this committee, although its effective power is limited since the Judicial Commission can overrule Parliamentary Committee objections. This was not in the original 18th amendment but was incorporated in December 20th, 2010, in the form of the 19th Amendment. This 19th amendment 1) increased the number of judges in the judicial commission (judges now had the overwhelming majority of 8/11 in the Judicial commission as opposed to 6/9 in the 18th amendment) and 2) stated that the Judicial Commission would now also have the power to overrule the Parliamentary Committees' objections to the appointments (Pakistan Constitutional Law, 2010).

²⁴ We do not argue that this new arrangement is completely immune from executive influence, just that the move to appointment by judicial commission reduced executive control over the judiciary relative to Presidential appointment.

²⁵The Eighteenth Amendment also aimed to increase provincial autonomy and weaken the power of the President globally: for instance, it also took away the power of the President to unilaterally dismiss Parliament (Almeida, 2018).

III. Data

Our empirical analysis uses data on judicial cases from the central repository of cases in Pakistan that are used by Lawyers to prepare their cases. We randomly sampled 7500 cases from 1986-2016 for all of the 16 district High Courts in Pakistan (from the universe of all cases decided in this period) and matched these to details on judge characteristics from judicial administrative data and district characteristics from the census records.²⁶ We successfully matched this information for 7439 cases out of the 7500.²⁷ Table 1 shows the descriptive statistics of the variables used in the analysis, and we below provide information on the key outcome and explanatory variables. Further information on the variables, their sources 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 that contains the information on the contents of 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. The Law firm was divided into two independent teams and asked to code the “State Wins” dummy variable as 1 if the State won in a dispute with the government as a party.²⁸ 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).

²⁶Further information on the sampling procedure and data construction can be found in Appendix B.3.

²⁷The remaining 61 cases could not be matched due to the poor image quality of the judgement order that did not permit the name of the judge to be ascertained from the judgement text.

²⁸We show that the results are robust to using data from either of the teams. Further details on the construction of the variables coded by the two teams can be found in Appendix B.3.

Typically, when the dummy for State Wins is 1, the judgement contains markers such as “case against the State is accepted” and when 0 markers such as “case against the State is dismissed”. A textual analysis of cases containing these precise phrases confirms the main results.

For the analysis of the quality of judicial decisions, we use two additional outcome variables: Case Delay and Merit, where the unit of observation is also at the case level. Both of these variables are again constructed from the information in the text of the judgement orders. Case Delay is calculated as the difference between the case decision and filing years. Merit is a dummy, also coded by attorneys at the Law firm, for the decision being “*based on evidence rather than technical or procedural grounds*” (Pound, 1963). This is based on Common Law jurisprudence, where cases decided on merit, i.e. based on evidence and the spirit of the Law rather than its technicalities, is an ideal to which Common-Law regimes aspire to (see e.g. Tidmarsh, 2009, for a discussion).²⁹ Furthermore, legal scholars in Pakistan argue that ruling on technicalities in Pakistan is a “*weapon of choice to rule unfairly*” (Aziz, 2001) and that judges use decisions on technicalities to “*favour the state authorities*” (Arshad, 2017).

Main Explanatory Variables. — The key explanatory variable used in the analysis varies by district-year and is called “Judicial Commission/Total Judges”. This is the fraction of judges appointed by the judicial commission in a given district-year. Data on the appointments and other judge characteristics comes from judicial administrative records obtained from the Registrar Offices of the district High Courts. Data on the total judges in each district High Court is obtained from the High Court Annual Reports submitted to the Ministry of Justice, Government of Pakistan. These two sources are also used to construct the instrumental variable, “Retiring at 62/Total Judges” also presented in the Panel C of Table 1. This variable, which also varies by district-year, is the the fraction of judges who reach the mandatory retirement age of 62 (in the post-reform

²⁹Two independent teams coded each of these outcome variables and the correlation coefficient between them appears in Table C.1 in Appendix C.

period).³⁰ Figure 2 plots each of these variables over time. We observe that appointments by judicial commission are highly correlated with mandatory retirements.³¹

Controls: Case, Judge and District Characteristics. — We rely on a combination of judgment texts, judicial administrative data, bar associations and census records to construct the case, judge and district characteristics that we use as control variables. The case-characteristics data, like the outcome variables, are obtained from the text of the judgment order. This includes case characteristics, such as the district where the case was heard, the year when the case was filed, decision year, the full name of the judge(s) adjudicating on the case, the number of lawyers and judges, the type of the case, a dummy for whether the case involved a land dispute with the government (land cases or “Eminent Domain” cases) and so on. Table C.1 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 who coded them.³² The data on judge characteristics is obtained from the judicial administrative records available at the Registrar Offices of the High Courts of Pakistan and provincial High Court websites (Table 1, panel B). This includes information on judges’ date of birth, appointment date and retirement, as well as information on their previous employment. Information on being an office holder in the Bar Association prior to the judicial appointment is obtained from a combination of biographical information in the judicial administrative data, annual reports of district High Courts submitted to Ministry of Justice and bar association records. Combining the data from these sources gives us information on 7439 cases and 482 judges across all of the 16 district High Courts in Pakistan.

³⁰We also construct the fraction of pre-reform appointments and pre-reform retirements using the same data sources.

³¹91% of judges in Pakistan serve out their full term and only retire on their 62nd birthday.

³²The results are robust to using data from either team of coders: further information can be found in the discussion in Appendix B.3.

IV. Empirical Method

We use cross-district and over-time variation in the implementation of the reform to estimate the effect of judicial-selection reform on judicial outcomes at the case level. The Linear Probability Model we estimate by OLS and 2SLS is as follows:

$$Y_{cjd t} = \theta + \alpha \left(\frac{\text{Commission Appointed Judges}}{\text{Total Judges}} \right)_{dt} + \beta_d + \gamma_t + \mathbf{W}'_{cdt} \boldsymbol{\varphi} + \varepsilon_{cdjt} \quad (1)$$

The subscripts c, j, d and t index cases, judges, district courts and years respectively. Y denotes the respective judicial outcome and $\frac{\text{Commission Appointed Judges}}{\text{Total Judges}}$ is the fraction of judges appointed by the judicial commission. As we run the regression at the case level, this variable can be interpreted as the probability that a case is adjudicated by a judicial-commission judge.³³ β_d and γ_t are district and year fixed effects respectively, and \mathbf{W}'_{cdt} is a vector of case and district controls as shown in Table 1.³⁴

OLS estimation of α in Equation (1) may not yield the causal impact of the selection reform, due to the potential reassignment of judges across districts. As shown by Iyer and Mani (2012) in India, the “transfer” powers of Indian politicians allowed them to exert substantial

³³As part of the robustness checks, we obtain similar results aggregating the data to the district-year level (i.e. to the level of variation of the main explanatory variable).

³⁴The case controls includes the number of Lawyers and Judges in the case, and the presence of the Court Chief Justice on the bench, and the district characteristics (e.g. population) in Table 1, panels A and C. Note that we do not control for judge characteristics, as these may be correlated with the reform (we will discuss this possibility in more detail in the mechanisms section). We, however, include case-type fixed effects in the list of controls. That is, we consider case-type fixed effects (dummies for case types) as part of the case characteristics controls in \mathbf{W}'_{cdt} .

control over bureaucrats. Independent judges in Pakistan may plausibly be reassigned to a different district High Court, rendering the reform-State Wins relationship endogenous.³⁵

We address this empirical challenge via an instrumental variable: we instrument the fraction of judicial-commission judges by the fraction of judges reaching the mandatory retirement age of 62 following the selection reform.³⁶ The first stage equation is as follows:

$$\left(\frac{\text{Commission Appointed Judges}}{\text{Total Judges}}\right)_{dt} = \theta + \pi \left(\frac{\text{Retiring at 62 Post Reform}}{\text{Total Judges}}\right)_{dt} + \beta_d + \gamma_t + \mathbf{W}'_{cdt}\boldsymbol{\varphi} + \varepsilon_{cdjt} \quad (2)$$

The IV estimate of α yields the causal effect of the reform under the orthogonal distribution of judges' retirement ages across districts and if the identification assumption of the difference-in-differences estimator that there are no systematic differences in the trends of the outcomes prior to the reform is satisfied. We provide evidence in support of both of these identification assumptions in the next section.

We cluster standard errors at the most conservative district level. We demonstrate the robustness of the results by clustering within each district separately before and after the reform (Bertrand et al., 2004) and clustering within each district-year combination i.e. the level of variation of the instrument (Abadie et al., 2017).³⁷

³⁵Although we should note that judges in Pakistan cannot be directly reassigned by politicians, as reassignment power lies with the Chief Justice of the Provincial High Court, not the Chief Minister as for Indian Civil Servants (Iyer and Mani, 2012). Nevertheless, based on anecdotal accounts, politicians may plausibly influence the Chief Justice to transfer a judge.

³⁶See Figure 2 for a graph showing the evolution of these variables over time.

³⁷We also demonstrate the robustness of the results by running an alternate specification where we regress the respective judicial outcome on the fraction of judges retiring at 62 interacted with the post reform dummy, and the fraction of judges retiring at 62, with and without controls.

V. Main Results

5.1. *The effect of the judicial-selection reform on State Wins*

Table 2 presents the estimated effect of the judicial-selection reform on State victories. There is strong and robust evidence of a substantial negative effect of selection reform on State victories. 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 most basic OLS specification with only district and year fixed effects. Column 2 adds all the available case and district characteristics (presented in Table 1, panels A and C). In column 3, we instrument the fraction of judicial-commission judges by the fraction of judges reaching the mandatory retirement age of 62 following the reform. In column 4, we add the available case and district controls to this IV specification. In the first stages of the two-stage least squares (2SLS) estimations in panel B the instrument is a strong predictor of the fraction of judges appointed by the judicial commission, with the F-statistic being above 100 in both specifications.

We find a negative and statistically-significant effect of judicial-commission appointments on State Wins in both OLS and 2SLS estimations. The size of the coefficients is similar across the OLS and the IV estimations. In the latter, a 10 percentage point rise in the judges appointed by the judicial commission reduces the probability of State Wins by about 3 to 4 percentage points (where the average State Wins before the reform was about 55%). There is thus a substantial effect of the selection reform on State Wins.

We also demonstrate the robustness of these results by estimating an alternate specification where we regress State Wins on the interaction of the fraction of judges reaching their mandatory retirement age and the post reform dummy, and the fraction of mandatory retirements. Table C.2 presents these results. We find across all specifications mandatory retirements influences State

Wins but only after the reform goes into effect. Interestingly, the coefficient point estimates suggest that the independent effect of mandatory retirements is positive across all specifications (although we cannot reject the possibility of a null effect).³⁸

5.2. Alternative Explanations and Discussion of Identification

We now investigate the key threats to identification that can invalidate the causal interpretation of these estimates. It first may be the case that our selection-reform estimate reflects a pure appointment effect, with new appointments affecting judge behaviour independently of the selection reform. In Table 3 (columns 1 and 2), we present evidence against this hypothesis by showing that pre-treatment appointments had no effect on rulings in favour of the government. In particular, the fraction of new appointments from 1986 (the first year for which we have data) up to the reform has no effect on State victories.

Second, we consider whether the estimated coefficient instead shows a pure retirement effect. This is possible if, for example, it takes time for the Judge-State relationship to develop, so that instead of the selection reform we pick up the impact of judge experience and other potentially unobservable judge characteristics related to his or her retirement. The use of mandatory retirement as an instrument here in fact exacerbates this problem, since we compare retirees to new appointees. We nevertheless find no evidence of a retirement effect as such in Table 3 (columns 3 and 4), which estimates the effect of the fraction of judges reaching their mandatory retirement age on State Wins in the pre-reform period. That is, we find no effect of the fraction of judges reaching their mandatory retirement age before the reform came into effect. If anything, the

³⁸We discuss this further in the following subsection.

coefficient estimate on pre-treatment retirements is positive.³⁹ As such, even if there is a retirement effect this seems to be relatively small and masked by the large effect of the selection reform. Likewise, we obtain similar results when we estimate an over-time reduced form (where we only see an effect of ‘retirement’ on State Wins after the reform goes into effect).⁴⁰

The third threat to the identification of the causal effect of the reform is that there are diverging trends prior to the reform. We thus estimate the baseline specification (Equation 1) for new appointments instrumented by mandatory retirement before and after the reform in three-year intervals. Figure 4 depicts the results by plotting the coefficients along with their 95% confidence intervals.⁴¹ There is no evidence of pre-trends.

A fourth identification threat is that the selection reform is correlated with case and district characteristics. This is possible if, for instance, the *de jure* random allocation of cases is not actually followed in Pakistan. We provide evidence in favour of the identification assumption that the selection reform is orthogonal to district and case characteristics by re-estimating the baseline equation (1) replacing State Wins with our case and district characteristics as the dependent variables. Table 4 presents this check for balance test. None of the case or district characteristics is correlated with the selection reform; we find similar results if we carry out the same test at the case or the judge level (see Table C.5 in Appendix C for these results). This is consistent with the random allocation of cases across judges, and district characteristics being uncorrelated with the selection reform.⁴²

³⁹We obtain similar results with controls for judge age and tenure, as judicial-commission and Presidential judges do not differ in these respects (as discussed in the following section).

⁴⁰Table C.3 in Appendix C presents this result. Appointments and retirement due to reaching age 62 pre-reform are constructed using the same method and data sources as those in the post-reform period. The trends in the pre-treatment variables is very similar to that shown post-treatment in Figure 2 (the pre-treatment plots of appointments and retirements are available on request).

⁴¹The table underlying Figure 4 appears in Table C.4 in the Appendix C.

⁴²We show the effect of the selection reform at the judge level in the next section.

Our confidence, in the causal interpretation of the results is increased when we find similar results when we limit the sample to cases just before and after the reform. In particular, we re-estimate the baseline specification in a one-year window around the reform by 2SLS. Table 5 presents these results. We find broadly similar results, despite the reduced statistical power (most likely due to the smaller sample): the selection reform reduces State Wins. This is reassuring as the cases around the reform year are plausibly quite similar as there is only little time for the government or litigants to respond to the reform.⁴³ This is also consistent with the random allocation of cases and the results from the balance test.⁴⁴

VI. Mechanisms

This section is organized as follows. We first describe the type of cases that are driving the results. Second, we present evidence consistent with the fall in State Wins following the reform reflecting better-quality judicial decisions. Third, we show that the type of judges driving the results is consistent with the selection effects or judge heterogeneity mechanism. Last, we discuss the welfare implications of the reform by linking it to the recent housing boom in Pakistan and consider how the type of cases driving our results are important in their interpretation.

6.1. Mechanisms: The type of cases driving the results

We begin our investigation of the mechanisms by discussing the type of cases driving the results. We find evidence that the judicial-selection reform affected politically salient cases. One

⁴³This is also consistent with anecdotal accounts that litigants are typically unaware of the age of the judge, and so cannot calculate their age to retirement and thus the reform exposure in the district (Arshad, 2017).

⁴⁴In the robustness section, we further present evidence that the district specific trends or strategic filing of cases are unlikely to explain the results.

key mechanism that we test is how the judicial selection affected cases involving land disputes with the State. Several anecdotal accounts suggest that the expropriation of private property by government housing agencies (such as by the Defence Housing Authority, the Lahore Development Authority, the Karachi Development Authority and the Capital Development Authority) was an important problem in Pakistan, and that rulings in these ownership or expropriation disputes with the government were heavily influenced by political considerations (Ijaz, 2014; Abbasi, 2017; Sattar, 2017). Some legal scholars in Pakistan go as far as to argue that land disputes involving the State are instances where the government is almost always 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). In columns 1 and 2 of Table 6, we present evidence that is consistent with this view: a 10% rise in judicial-commission judges reduces State Wins by about 5 percentage points in land disputes with the State.

We next examine how the selection reform affected human-rights cases that are also considered highly political in nature. These are constitutional cases that do not involve land disputes with the government. These cases are separately marked as “writ petitions” within the constitutional cases and involve the violation of fundamental rights such as freedom of movement or discrimination based on religion, gender and caste. For instance, in a typical case in the dataset an individual pleads that his fundamental right of freedom of movement within and outside Pakistan has been restricted by the government since he joined the opposition political party.⁴⁵ We find that the selection reform reduced State Wins in human-rights cases, as presented in columns

⁴⁵ *Khalid Langrov vs. The State*, PLD 2007.

3 and 4 of Table 6. A 10% increase in judicial-commission judges reduces State Wins in human-rights cases by 5.2 percentage points. These results suggest a political-influence channel, with less-favourable rulings for the government in politically salient cases by the judicial-commission judges.

Further evidence for this political-influence channel comes from a falsification test. As criminal cases also involve the State (as the prosecutor), but are politically less salient, we examine the impact of the selection reform on State Wins (or conviction rates) in criminal cases. These results appear in Table 7. We find no effect of the selection reform on State Wins in criminal cases, and the OLS point estimates are in fact positive. This suggests that judicial-commission judges do not rule against the government more than Presidential judges in politically less-salient criminal cases.⁴⁶

6.2. Do fewer State Wins reflect better-quality judicial decisions?

We now provide evidence that fewer State Wins following the selection reform reflects an improvement in the quality of judicial decisions. We first show in Panel A of Table 8 that the selection reform reduced case delay: a 10% increase in judicial-commission judges reduces case delay by about 0.15 years or 2 months. This shorter case delay is only found for land and human-rights cases with the State, with no effect for criminal cases. State Wins and Case Delay can be interpreted as separate outcome variables, where the former is a proxy for judicial independence while the latter is a measure of judicial efficiency. Nevertheless, there is good reason to believe that, in the current context, State Wins and Case Delay may be linked. Anecdotal accounts suggest

⁴⁶To verify that criminal cases in the High Courts are indeed politically low-stake cases, we inspected 100 criminal cases from our sample randomly to find they are indeed politically less salient: most cases involve bail pleas for theft and burglary (the categorization of the alleged crimes in these 100 cases are available on request).

that judges delay cases in order to favour the government (Sheikh, 2016; Ahmed, 2016). This becomes salient when government officials use the expropriated land for private benefit while the case is pending in the court, or judges do not rule over cases when the government position is particularly weak (Malik, 2018). The null effect of the selection reform on Case Delay in criminal cases is consistent with this interpretation.

It may be reasoned, however, that shorter case delay following the reform may reflect less deliberation in these cases, implying lower-quality judicial decisions. Nevertheless, we are confident that the fall in State Wins and Case Delay reflects better-quality judicial decisions due to the results for cases decided “on merit”. In Common Law jurisprudence, rulings on merit imply that the judicial decision is “*based on evidence rather than technical or procedural grounds*” (Pound, 1963). This is consistent with anecdotal accounts in Pakistan that ruling on technicalities is a “*weapon of choice to rule unfairly*” (Aziz, 2001) and that judges use decisions on technicalities to “*favour the state authorities*” in Pakistan (Arshad, 2017).

We consider how the selection reform affected meritorious decisions in panel B of Table 8. From the full sample estimates in column 1, we find that a 10% rise in judicial-commission judges increases merit decisions by about 5.5 percentage points. This rise is only seen for land-disputes and human-rights cases with the government, whereas reassuringly we find no effect for criminal cases.

6.3. Mechanisms: The type of judges driving the results

This subsection provides evidence consistent with the results being explained by judge-selection effects or heterogeneity mechanism. First, consistent with our main findings, Table 9 shows that judicial-commission judges are about 15% less likely to rule in favour of the State (at

the judge level): see columns 1 to 3. Nevertheless, controlling for all the available judge characteristics, we cannot reject the null hypothesis of no effect of the reform on State Wins. This is consistent with the judge heterogeneity mechanism, where the selection reform is correlated with judge characteristics.

We ask which judge characteristics distinguish judges appointed under the two selection procedures in Table 10. Presidential and judicial-commission judges are of similar gender, age and previous employment.⁴⁷ The key distinguishing feature here is that judicial-commission judges are about 35% less likely to have run for political office at the Lawyers' Bar Associations (see column 5 of Table 10). As being an office holder of a Bar Association in Pakistan requires running for election on a political party platform (i.e. on a party "ticket"), we consider this as a plausible proxy for political activity prior to appointment. We also provide direct evidence that the judicial-commission judges are significantly less likely to have run for political office in provincial or national elections before their formal appointment date: see column 6 of Table 10.⁴⁸ We find that judicial-commission judges are also about 16% less likely to have run for election as members of the provincial or national assembly prior to their appointment.⁴⁹

We can interpret these results through the lens of a simple signalling model. *A priori*, the President does not know the "type" of the judge. Once the judges run for elections, they reveal their type. Therefore, the President selects judges that are of a similar type or who share similar preferences as the President. Under the assumption that the President places a greater value on political loyalty than the judges who select the judges, and that judges place a greater value on

⁴⁷ We find similar results across three measures of "age": age at the time of the judgement, tenure at judgement (shown in Table 10) and age at appointment.

⁴⁸ Once appointed, judges are barred from running for political office until two years after their retirement.

⁴⁹ We present judge-level regressions here for two reasons. First, these show in a transparent manner the average differences between the judges appointed under the two selection procedures. Second, we can consider the results in columns 1 to 3 in comparison to the baseline results at the judge level. Similar results come from case-level regressions or mean comparisons (see, for instance, Table C.5 in Appendix C for an alternate illustration of the same results).

judicial competence relative to the President, the Presidential judges will be more pro-State, take longer and be less likely to rule on the merits of the case, consistent with what we find in this paper.⁵⁰

6.4. *The selection reform and the recent housing boom*

House prices have recently surged in Pakistan: in Figure C.1 house prices have risen from about Rs 2000 (USD 15) in 2009 to more than Rs 5000 (USD 35) per square foot in 2016 following the selection reform (Zameen, 2018). Is the selection reform related to this increase in house prices? We provide suggestive evidence that this is the case. As land disputes with the State involve disputes regarding residential land, we expect industries close to the housing sector to be disproportionately affected in regions where there are more judicial-commission judges.

Such a finding would be consistent with anecdotal accounts suggesting that the selection reform reduced expropriation risk (Sheikh, 2016; Abbasi, 2017). For instance, “*Many people, particularly overseas Pakistan can now purchase real estate given the judicial system has improved.*” (OFP Commission, 2018). Before the reform, a prominent minister in the government had noted, “*Land grabbing is a one of the major issues facing Pakistan. We have received a huge number of complaints regarding this ... courts take years to decide such cases*” (Gulf News, 2009). Shorter case delays and more merit decisions, particularly in cases involving land disputes with State housing agencies, is also consistent with the selection reform reducing expropriation risk.

We show that investment in an industry close to the housing sector is linked with the selection reform. In Table 11 (Panel A) we replace State Wins as the dependent variable in the baseline specification with “Investment in Construction Industry” (albeit at the provincial level, given data availability). The IV-point estimates imply that a 10% rise in judicial-commission

⁵⁰Note, however, that the evidence presented here is suggestive, as political activity prior to appointment is potentially correlated with unobservable judge characteristics. We should therefore interpret these results with caution.

judges increases construction-industry investment by about Rs 6 million (or USD 60, 000). A back-of-the-envelope calculation that assumes replacement of all Presidentially appointed judges by judicial-commission judges is thus estimated to increase construction-industry investment by about Rs 60 million (or USD 600, 000).⁵¹

We can similarly in Table 11 (Panel B) link the selection reform to house prices. An analogous, back-of-the-envelope calculation suggest that a 100% replacement of Presidentially appointed judges by those appointed by the judicial commission imply that the house prices (per square feet) would rise by Rs 176 (this is equivalent to a rise of about USD 1.20 per square feet). Putting this in perspective, average house price per square feet in Pakistan is Rs 1072 per square feet in our sample, with the reform reflecting a 15% rise in house prices. These results suggest that judicial independence has implications for development, especially when interpreted together with the vast literature that finds stronger property rights protection is key for development (see e.g. North and Thomas, 1973; Acemoglu, 2001; La Porta et al., 2008; Besley and Ghatak, 2010; De Janvry et al, 2015).

VII. Robustness

This section tests alternative explanations to the finding that the reform generated a change in judicial decision making in Pakistan. First, it might be the case that we are capturing a “President-specific effect”. For instance, the fall in State Wins post-reform may just reflect a correction from extremely high State Wins during the tenure of an idiosyncratic President (say, President General Musharraf). Since, we have judges appointed by six different Presidents in the

⁵¹We should note that this is an out-of-sample extrapolation and hence be interpreted cautiously.

sample period, we can examine this claim empirically. Table 12 presents the results: the reform reduced State victories irrespective of the President who had made the appointment.⁵²

Second, it may be argued that the change associated with the reform is a “Chief Justice-specific” effect. Anecdotal accounts suggest that some Chief Justices in Pakistan were particularly anti-government (Zafar, 2012). As the Chief Justice of Pakistan is the head of the judicial commission, this concern is particularly salient. During our sample period, five different Chief Justices headed the judicial commission, therefore we test and reject the hypothesis that the results are driven by an idiosyncratic Chief Justice in Table 13.⁵³

Third, the reform (and the consequent fall in State wins) may capture a move from dictatorship to democracy. There was military rule in Pakistan from 1999 to 2008, and the controls and identification strategy might not distinguish the effects of this democratic transition from that of judicial selection. Table 14 tests for this by considering only the sample from the democratic period (2009 to 2016), with results that are qualitatively and statistically similar to those in the full sample.⁵⁴

We carry out a number of additional robustness checks. We first show that the results are unlikely to be driven by district specific trends. In particular, we find the results are robust to adding district-specific linear trends to the baseline specification. Likewise, we also conduct a permutation inference falsification test where we find no effect of the reform if we randomly scramble the districts (Ernst, 2004). Second, we show that the results are robust to different levels

⁵²We here compare cases presided by judges who were appointed by different Presidents to those decided by judicial-commission judges (where the latter group of cases remain the same); similar results are found in alternate specifications with interaction terms for the respective Presidential tenure.

⁵³Table C.7 in Appendix C gives an alternative illustration of these results.

⁵⁴The similar results from the estimation of the baseline specifications in one-year windows around the reform are also consistent with this finding (see Table 5).

of clustering, where we cluster within each district separately before and after the reform and clustering within each district-year combination i.e. the level of variation of the instrument (Bertrand et al., 2004). Third, the results are robust to aggregating the data at the district-year level (the level of variation of the main explanatory variable and its corresponding instrument). Fourth, we present evidence against the hypothesis that the reform is confounded by selection of cases that go to trial (Klein and Priest, 1984; Hubbard, 2013). Although, as noted in the literature, one cannot completely rule out this possibility, but we show that total case filings as well as filings in politically salient cases are uncorrelated with the reform implementation. Last, our results remain unchanged when we estimate non-linear models such as Probit and Logit. These results can be found in the Appendix C.⁵⁵

VIII. Conclusion

This paper has shown that the institution of Presidential appointment exerts considerable influence over judicial decision-making in Pakistan. We demonstrate that the change in selection procedure of judges from Presidential appointment to appointment by a judicial commission significantly reduces State Wins and this reduction is suggestive of an improvement in the quality of judicial decisions. The identification strategy we propose allows us to obtain the causal effects of the reform. We present evidence against a number of threats to identification and alternative explanations to our finding that the selection reform reduced rulings in favour of the government.

These results are driven by politically salient cases involving land and human-rights disputes with the government, and by Presidentially appointed judges who are more likely to be politically

⁵⁵See Appendix C, Tables C.8 to C.13.

active prior to their appointments relative to judges appointed by the judicial commission. Last, we link the reform to reduction in expropriation risk in the housing sector where we provide suggestive evidence that the reform reduced distortions in the economy due to favouritism by increasing investment in the construction industry and by raising house prices.

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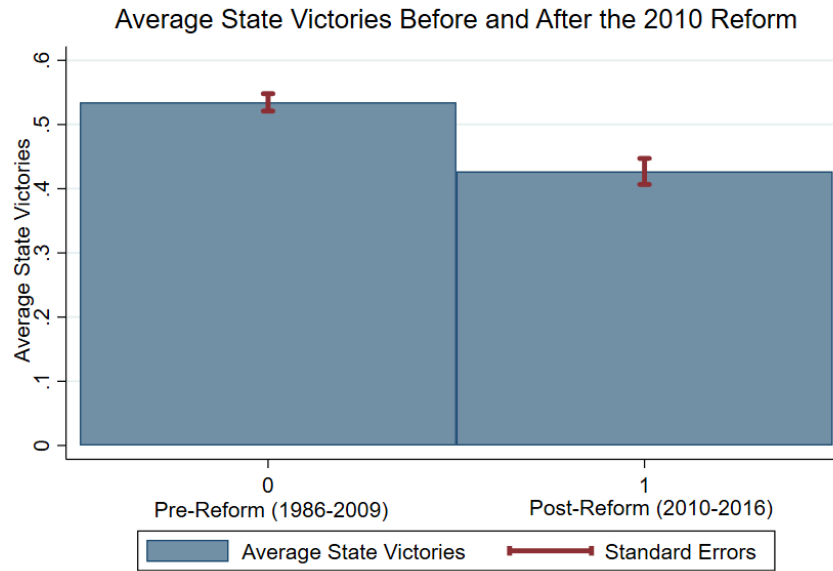
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Figures and Tables

Figure 1: State Wins Before and After the Reform

Panel A: Average State Victories Before and After the Reform



Panel B: Average State Victories for Cases Decided by Presidential and Judicial-Commission Appointed Judges

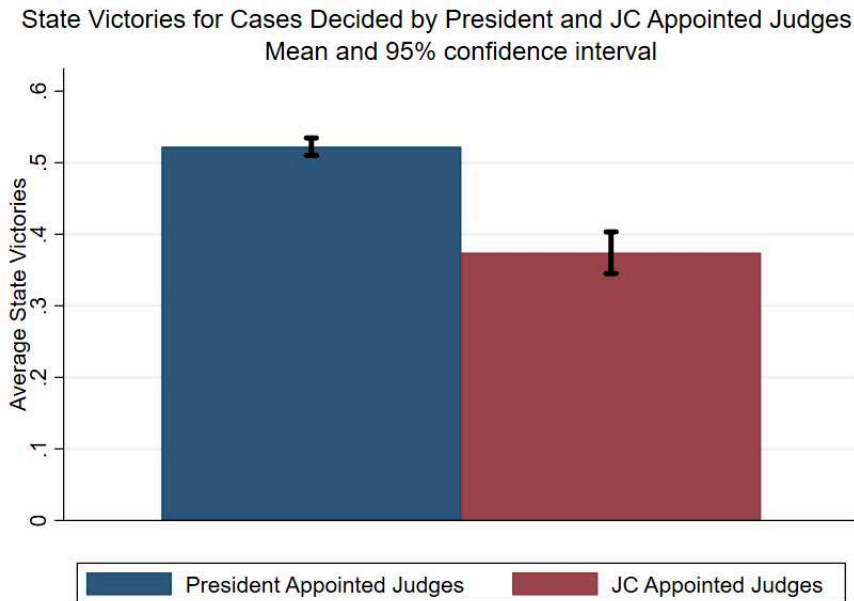
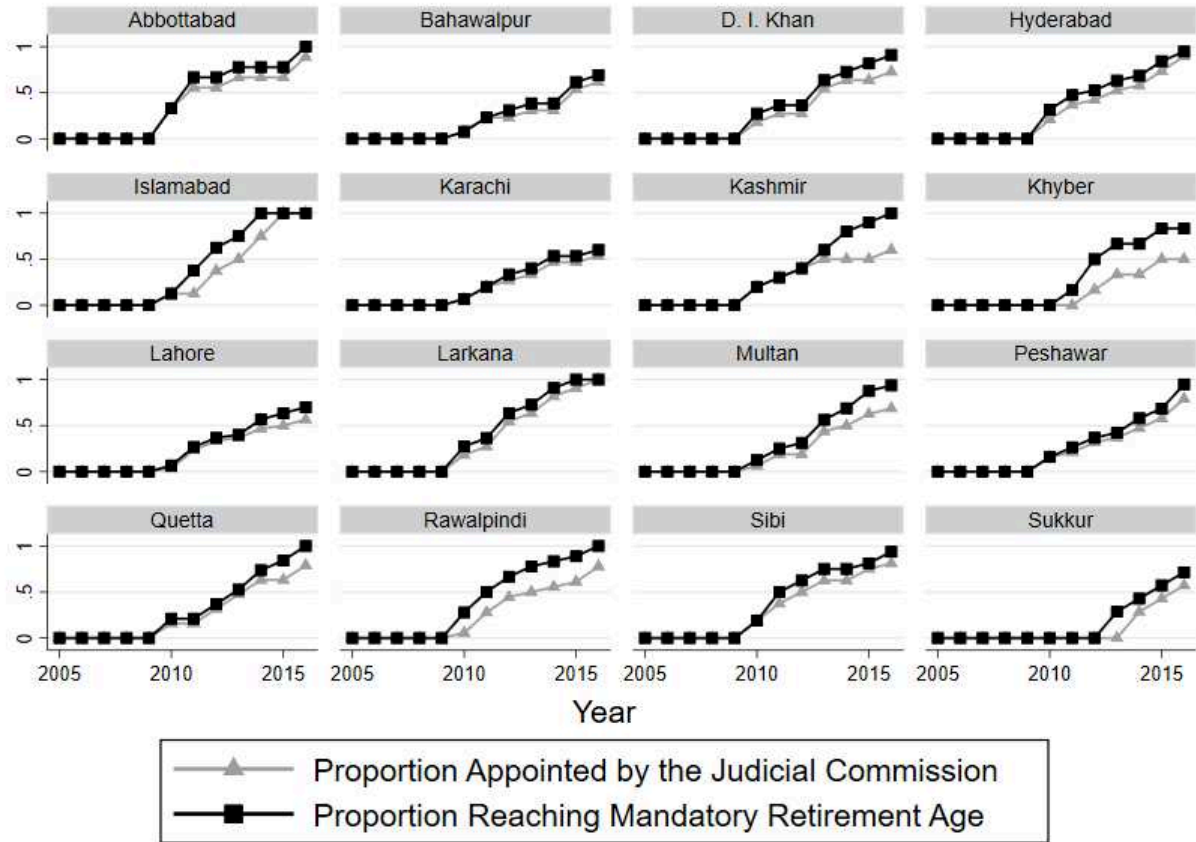
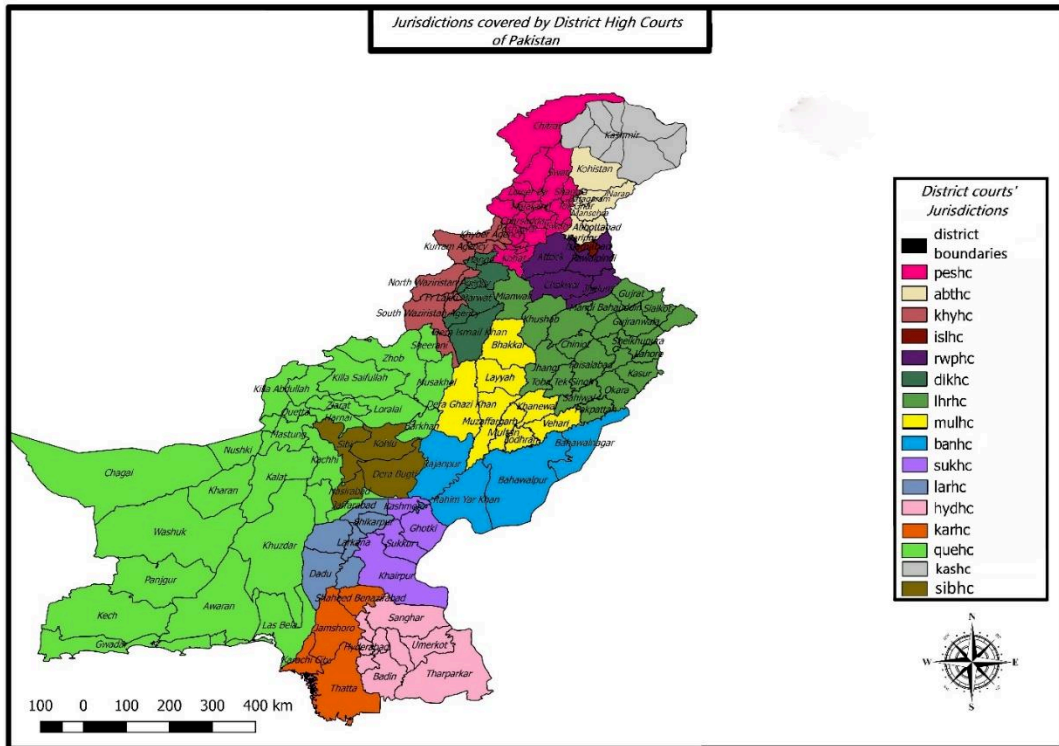


Figure 2: Appointments and Retirements Post-Reform



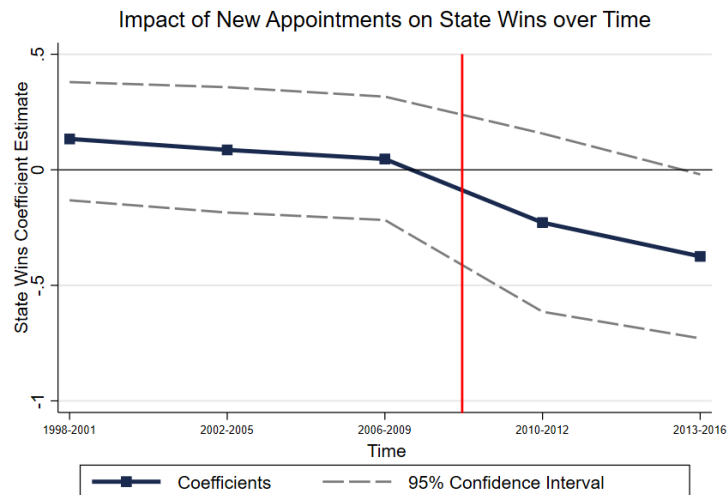
Note: The figure plots the fraction of judges appointed by the judicial commission (peer judges) in each district over time with mandatory retirements in the post-reform period. The retirements are highly correlated with new appointments. The regression-form representation of this figure (first-stage results) appears in Table 2 (Panel B) and the results of the permutation inference falsification test where these districts are randomly scrambled appears in Table C.9 in Appendix C.

Figure 3: Jurisdictions covered by District High Courts of Pakistan



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

Figure 4: The Impact of New Appointments on State Wins over Time



Note: This figure presents the coefficients (along with their 95% confidence intervals) in the regression of State Wins on the fraction of new appointments instrumented by the proportion of mandatory retirements for all cases in the respective three-year intervals. Pre-treatment appointments and retirements are not accompanied by judges selected by the judicial commission. The vertical line marks the timing of the judicial-selection reform. The table-form representation of the results of these estimations appears in Table C.4 in Appendix C.

Table 1: Descriptive Statistics

Variables	Observations	Mean	Std. Dev.	Min	Max
<i>Panel A: Case Characteristics (by cases)</i>					
State Wins	7,439	0.50	0.50	0	1
Case Delay	7,439	3.33	2.47	0	23
Merit	7,439	0.62	0.48	0	1
Year Filed	7,439	1999.69	9.53	1970	2016
Year Decision	7,439	2003.03	8.88	1986	2016
Constitutional Cases	7,439	0.72	0.44	0	1
<i>Land Cases</i>	7,439	0.41	0.49	0	1
<i>Human Rights Cases</i>	7,439	0.31	0.46	0	1
Criminal Cases	7,439	0.28	0.44	0	1
Pages of Judgement Order	7,439	8.88	7.71	1	81
Number of Lawyers	7,439	4.04	3.62	1	32
Number of Judges on a case	7,439	1.81	0.84	1	5
Chief Justice in Bench	7,439	0.06	0.24	0	1
<i>Panel B: Judge Characteristics (by judges)</i>					
Tenure at Decision	482	4.10	3.64	8.46	22
Gender	482	0.95	0.19	0	1
Promoted to SC	482	0.05	0.23	0	1
Former Judge	482	0.11	0.31	0	1
Fr. Office Holder Bar. Ass.	482	0.63	0.48	0	1
Ran for Political Office	482	0.19	0.39	0	1
Former Lawyer	482	0.89	0.31	0	1
After Reform Judge	482	0.14	0.34	0	1
<i>Panel C: Treatment Variables and District Characteristics (by district-year)</i>					
Commission Judges/Total	496	0.10	0.21	0	1
Retiring at 62/Total	496	0.12	0.26	0	1
Total Judges in district	496	14.16	5.84	6	30
Area (sq. km)	496	4321.81	3287.76	906	13297
Population	496	2150270	2428460	22454.11	1.14E+07
Density (per sq. km)	496	1094.32	1764.62	8.46	9023.83

Note: This table reports the summary statistics for the baseline sample of 7439 cases, 482 judges covering the 16 district High Courts in Pakistan over the 1986-2016 period.

Table 2: The 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>			
Commission Judges/Total Judges	-0.277** [0.112]	-0.318*** [0.105]	-0.311* [0.166]	-0.373*** [0.143]
District and Year Fixed Effects	Yes	Yes	Yes	Yes
District Controls	No	Yes	No	Yes
Case Controls	No	Yes	No	Yes
Observations	7,439	7,439	7,439	7,439
R-squared	0.044	0.052	0.044	0.052
Mean of dependent variable	0.50	0.50	0.50	0.50
Panel B: First-Stage Results				
		(3)	(4)	
	Commission Judges/Total Judges			
Retiring at 62/Total Judges		0.793*** [0.0779]	0.819*** [0.0719]	
District Fixed Effects		Yes	Yes	
Year Fixed Effects		Yes	Yes	
District Controls		No	Yes	
Case Controls		No	Yes	
Observations		7,439	7,439	
R-squared		0.971	0.979	
F-Statistic		103.62	129.66	

Robust standard errors appear in brackets (clustered at the district level). The dependent variable is State Wins, a dummy variable for the case being ruled in favour of the State. Commission/Total Judges is the fraction of judges appointed by the judicial commission (peer judges). In the IV regressions, this is instrumented by the fraction of judges reaching the mandatory retirement age of 62 in the post-reform period. The first-stage results corresponding to columns (3) and (4) appear in Panel B. The controls include all case and district characteristics in Table 1. The case controls also include case-type fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table 3: The Impact of Pre-Reform Appointments and Pre-Reform Retirements on State Wins

	OLS			
	(1)	(2)	(3)	(4)
	<i>State Wins</i>			
New Judges/Total Judges	-0.0390 [0.0755]	-0.0249 [0.0714]		
Retirements at 62/Total Judges			0.0705 [0.0560]	0.0635 [0.0537]
District and Year Fixed Effects	Yes	Yes	Yes	Yes
District Controls	No	Yes	No	Yes
Case Controls	No	Yes	No	Yes
Observations	7,439	7,439	7,439	7,439
R-squared	0.043	0.050	0.044	0.050
Mean of dependent variable	0.50	0.50	0.50	0.50

Robust standard errors appear in brackets (clustered at the district level). The dependent variable is State Wins, a dummy variable for the case being ruled in favour of the State. New Judges/Total Judges is the fraction of new judges appointed before the reform from 1986 (the first year of our data). Retirement at 62/Total Judges is the fraction of judges reaching the mandatory retirement age of 62 from 1986 onwards. Once all of the 1986 judges have been replaced this variable takes on the value of 1 for all ensuing years, analogous to the Commission Judges/Total Judges variable which is 1 once all Presidentially appointed judges have been replaced by judicial-commission judges in a given district. The controls include all of the case and district characteristics in Table 1. The case controls also include case-type fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table 4: The Impact of Selection Reform on Case and District Characteristics

	2SLS, 2 nd Stage							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Constituti onal Case	Crimin al Case	No. of Pages	CJ on Case	No. Lawyers on Case	No. Judges on Case	Population	Population Density
Commission/Total Judges	0.0139 [0.0196]	0.0107 [0.017]	0.482 [2.306]	-0.0328 [0.0546]	-2.777 [1.694]	0.159 [0.185]	374,495 [440,433]	-588.7 [403.5]
District Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,439	7,439	7,439	7,439	7,439	7,439	7,439	7,439
R-squared	0.092	0.091	0.246	0.017	0.055	0.072	0.995	0.995
Mean of dep. variable	0.722	0.278	8.887	0.064	4.042	1.815	3562527	2065.558

Robust standard errors appear in brackets (clustered at the district level). Commission/Total Judges is instrumented by the fraction of judges reaching the mandatory retirement age of 62 in the post-reform period. The controls include case and district characteristics in Table 1, excluding the dependent variable. The case controls include case-type fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table 5: The Impact of Selection Reform on State Wins in a one-year window around the reform

	2SLS, 2 nd Stage			
	(1)	(2)	(3)	(4)
	<i>State Wins (2009-2011)</i>			
Commission Judges/Total Judges	-0.676*** [0.101]	-0.735* [0.378]	-0.684 [0.404]	-0.679* [0.354]
			p-value = 0.11	
District and Year Fixed Effects	No	Yes	Yes	Yes
District Controls	No	No	Yes	Yes
Case Controls	No	No	No	Yes
Observations	887	887	887	887
R-squared	0.065	0.085	0.064	0.084
Mean of dependent variable	0.60	0.60	0.60	0.60

Robust standard errors appear in brackets (clustered at the district level). The dependent variable is State Wins, a dummy variable for the case being ruled in favour of the State. Commission/Total Judges is the fraction of judicial-commission judges. This is instrumented by the fraction of judges reaching the mandatory retirement age of 62 in the post-reform period. The controls include all case and district characteristics in Table 1. The case controls include case-type fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table 6: The Impact of Selection Reform on State Wins (by type of Constitutional Case)

	2SLS, 2 nd Stage			
	Land Cases		Human Rights Cases	
	<i>State Wins</i>			
Commission Judges/Total Judges	-0.453** [0.216]	-0.476** [0.205]	-0.363** [0.154]	-0.517*** [0.103]
District and Year Fixed Effects	Yes	Yes	Yes	Yes
District Controls	No	Yes	No	Yes
Case Controls	No	Yes	No	Yes
Observations	3,041	3,041	2,323	2,323
R-squared	0.083	0.084	0.047	0.050
Mean of dependent variable	0.47	0.47	0.46	0.46

Robust standard errors appear in brackets (clustered at the district level). The dependent variable is State Wins, a dummy variable for the case being ruled in favour of the State. Commission/Total Judges is the fraction of judges appointed by the judicial commission, which is instrumented by the proportion of judges reaching the mandatory retirement age of 62 in the post-reform period. The controls include all case and district characteristics in Table 1. The case controls also include case-type fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table 7: Placebo Test of the Political Influence Mechanism – The Impact of Selection Reform on Criminal Cases

	OLS		2SLS, 2 nd Stage	
	(1)	(2)	(3)	(4)
	<i>State Wins</i>			
Commission Judges/Total Judges	0.0195 [0.382]	0.0410 [0.370]	-0.227 [0.412]	-0.0720 [0.393]
District and Year Fixed Effects	Yes	Yes	Yes	Yes
District Controls	No	Yes	No	Yes
Case Controls	No	Yes	No	Yes
Observations	2,075	2,075	2,075	2,075
R-squared	0.071	0.079	0.071	0.079
Mean of dependent variable	0.58	0.58	0.58	0.58

Robust standard errors appear in brackets (clustered at the district level). The dependent variable is State Wins, a dummy for the case being ruled in favour of the State. Commission/Total Judges is the fraction of judicial-commission judges, instrumented by the proportion of judges reaching the mandatory retirement age of 62 in the post-reform period. The controls include all case and district characteristics in Table 1. The case controls also include case-type fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table 8: The Impact of the Reform on Decision Quality – Case Delay and Decisions on Merit

Panel A: Case Delay		2SLS			
		<i>Case Delay</i>			
		(1)	(2)	(3)	(4)
		Overall	Land	Human Rights	Criminal
Commission Judges/Total Judges		-1.571*** [0.470]	-2.697*** [0.711]	-1.306* [0.775]	0.237 [1.146]
District and Year Fixed Effects		Yes	Yes	Yes	Yes
District Controls		Yes	Yes	Yes	Yes
Case Controls		Yes	Yes	Yes	Yes
Observations		7,439	3,041	2,323	2,075
R-squared		0.085	0.145	0.135	0.081
Mean of dependent variable		3.33	3.33	3.28	3.40

Panel B: Decisions on Merit		2SLS			
		<i>Decisions on Merit</i>			
		(1)	(2)	(3)	(4)
		Overall	Land	Human Rights	Criminal
Commission Judges/Total Judges		0.558*** [0.182]	0.511** [0.234]	0.599*** [0.221]	0.0670 [0.347]
District and Year Fixed Effects		Yes	Yes	Yes	Yes
District Controls		Yes	Yes	Yes	Yes
Case Controls		Yes	Yes	Yes	Yes
Observations		7,439	3,041	2,323	2,075
R-squared		0.085	0.128	0.074	0.162
Mean of dependent variable		0.62	0.60	0.61	0.67

Robust standard errors appear in brackets (clustered at the district level). The dependent variable is Case Delay in Panel A, reflecting the difference between case decision year and filing year. For Panel B, the dependent variable is a dummy for the case being ruled on merits. Commission/Total Judges is the fraction of judicial-commission judges, instrumented by the proportion of judges reaching the mandatory retirement age of 62 in the post-reform period. All the coefficient estimates are calculated from Two-Stage Least Squares. The controls include all case and district characteristics in Table 1. The case controls also include case-type fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table 9: Selection Effects – The Impact of Selection Reform on State Wins at the Judge Level

	OLS			
	(1)	(2)	(3)	(4)
	<i>State Wins</i>			
JC Judge Dummy	-0.160*** [0.0253]	-0.161*** [0.0284]	-0.161*** [0.0287]	-0.00113 [0.0429]
District Controls	No	Yes	Yes	Yes
Case Controls	No	No	Yes	Yes
Judge Controls	No	No	No	Yes
Observations	482	482	482	482
R-squared	0.093	0.101	0.108	0.153
Mean of dependent variable	0.48	0.48	0.48	0.48

Robust standard errors appear in brackets (clustered at the judge level). The dependent variable is State Wins, a dummy for the case being ruled in favour of the State. JC Judge Dummy is dummy variable that takes the value of one if the judge is appointed by the judicial commission and zero if the judge is appointed by the President. The controls include all case and district characteristics in Table 1. The case controls also include case-type fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table 10: Judicial Commission Appointed Judges and Judge Characteristics

	OLS					
	(1)	(2)	(3)	(4)	(5)	(6)
	Gender	Former Judge	Former Lawyer	Age at Decision	Former Office Holder Bar Assoc.	Ran for Political Office
JC Judge Dummy	-0.0280 [0.0212]	-0.0368 [0.0364]	0.0368 [0.0364]	-0.557 [0.529]	-0.343*** [0.055]	-0.156*** [0.044]
Age Control	Yes	Yes	Yes	-	Yes	Yes
Case Controls	Yes	Yes	Yes	Yes	Yes	Yes
District Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	482	482	482	482	482	482
R-squared	0.024	0.010	0.010	0.091	0.195	0.050

Robust standard errors appear in brackets (clustered at the judge level). JC Judge is a dummy for the judge being appointed by the Judicial Commission. The case and district controls variable are identical to those in the baseline regression. The age control is tenure at decision (Similar results are obtained if we use age at decision or age at appointment or run case-level regressions) *** p<0.01, ** p<0.05, * p<0.1.

Table 11: The Impact of Selection Reform on Construction-Industry Investment and House Prices

Panel A: Province-Year Regression with Investment in Construction Industry				
	OLS		2SLS, 2 nd Stage	
	(1)	(2)	(3)	(4)
	<i>Construction Industry Investment</i>			
Commission Judges/Total Judges	13.39 [7.651]	16.11 [7.797]	64.07* [34.72]	59.43* [31.81]
Province and Year Fixed Effects	Yes	Yes	Yes	Yes
Province and Case Controls	No	Yes	No	Yes
Observations	124	124	124	124
R-squared	0.044	0.051	0.044	0.052
Mean of dependent variable	941.96	941.96	941.96	941.96
Panel B: District Year Regression with House Prices				
	OLS		2SLS, 2 nd Stage	
	(1)	(2)	(3)	(4)
	<i>House Price (per square foot)</i>			
Commission Judges/Total Judges	56.58 [586.6]	75.94 [533.9]	152.2* [863.7]	176.2** [709.2]
District and Year Fixed Effects	Yes	Yes	Yes	Yes
District and Case Controls	No	Yes	No	Yes
Observations	448	448	448	448
R-squared	0.092	0.093	0.092	0.093
Mean of dependent variable	1072.11	1072.11	1072.11	1072.11

Robust standard errors appear in brackets (clustered at the province level in Panel A and the district level in Panel B). The dependent variable is construction industry investment spending, denominated in millions of Pakistani Rupees in Panel A and house price per square foot, also denominated in Pakistani Rupees in Panel B. Commission/Total Judges is the fraction of judges appointed by the Judicial Commission. In the IV regressions this is instrumented by the proportion of judges reaching the mandatory retirement age of 62 in the post-reform period. The controls include all case and region characteristics in Table 1 and case-type fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table 12: The Impact of Selection Reform on State Wins (by appointing President)

	<i>State Wins</i>					
	2SLS, 2 nd Stage					
	Pres. Zardari (1)	Pres. Musharraf (2)	Pres. Tarar (3)	Pres. Leghari (4)	Pres. Khan (5)	Pres. Haq (6)
Commission/Total Judges	-0.498* (0.269)	-0.395** (0.154)	-0.349 (0.412)	-0.422** (0.189)	-0.605*** (0.189)	-0.494** (0.200)
District and Year FE Controls	Yes Yes	Yes Yes	Yes Yes	Yes Yes	Yes Yes	Yes Yes
Observations	1,861	2,608	1,049	2,224	1,834	2,249
R-squared	0.097	0.072	0.102	0.089	0.107	0.088
Mean Dep. Variable	0.43	0.47	0.38	0.45	0.44	0.46

Robust standard errors appear in brackets (clustered at the district level). The dependent variable, key explanatory variable, instrument and controls are identical to those in the baseline specification. The judicial outcomes of cases adjudicated by Judicial Commission judges are compared to those of the judges appointed by the last six Presidents prior to the selection reform. The sample size varies by Presidents as these were in power for different durations, so some Presidents had less time than others to appoint new judges when the vacancy to appoint a judge opened. For instance, President Rafiq Tarar was in office for only three years, and consequently appointed fewer judges and had fewer cases decided by his appointees. The case controls include case-type fixed effects. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 13: The Impact of Selection Reform on State Wins (by Chief Justice)

	<i>State Wins</i>				
	2SLS, 2 nd Stage				
	CJ Jamali (1)	CJ Khawaja (2)	CJ Mulk (3)	CJ Jillani (4)	CJ Chaudhary (5)
Commission/Total Judges X Chief Justice	-0.145 (0.106)	-0.326** (0.149)	-0.355** (0.124)	-0.283** (0.123)	-0.263* (0.155)
District and Year FE Controls	Yes Yes	Yes Yes	Yes Yes	Yes Yes	Yes Yes
Observations	7,439	7,439	7,439	7,439	7,439
R-squared	0.051	0.051	0.051	0.051	0.050
Mean of Dep. Variable	0.50	0.50	0.50	0.50	0.50

Robust standard errors appear in brackets (clustered at the district level). The instrument and controls are identical to those in the baseline specification. The fraction appointed by Judicial Commission is interacted with the time period when the respective Chief Justice was in office following the selection reform. The case controls include case-type fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table 14: The Impact of Selection Reform on State Wins in the Democratic Period (2009-2016)

	OLS		2SLS, 2 nd Stage	
	(1)	(2)	(3)	(4)
	<i>State Wins (2009-2016)</i>			
Commission Judges/Total Judges	-0.338* [0.183]	-0.276 [0.201]	-0.481** [0.207]	-0.412* [0.231]
District Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
District Controls	No	Yes	No	Yes
Case Controls	No	Yes	No	Yes
Observations	2,563	2,563	2,563	2,563
R-squared	0.096	0.100	0.095	0.100
Mean of Dep. Variable	0.46	0.46	0.46	0.46

Robust standard errors appear in brackets (clustered at the district level). Commission/Total Judges is the fraction of Judicial Commission judges. In the IV regressions this is instrumented by the proportion of judges reaching the mandatory retirement age of 62 in the post-reform period. The controls include all case and district characteristics in Table 1 and case-type fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

For Online Appendix to:

Judicial Independence and Development: Evidence from Pakistan
BY SULTAN MEHMOOD

Contents

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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 the judgement orders retrieved from an online portal that records universe of High Court cases in Pakistan (<https://www.pakistanlawsite.com/>). More information on this source and data construction can be found in Appendix B.3.

Case Lag = This variable is the difference between the case-decision and case-filing years. It is also retrieved from the texts of the judgements compiled from the same data source as the State Wins variable.

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

Judicial Commission / Total Judges = This variable is the fraction of judges selected under the new selection procedure. Information on new appointments is obtained from judicial administrative records obtained from the Registrar Offices of the High Courts. The data on total judges in each district High Court comes from High-Court Annual Reports submitted to the Ministry of Law, Justice and Human Rights, Government of Pakistan.

Retiring at 62 / Total Judges (instrument) = This variable is the fraction of judges reaching the mandatory retirement age of 62 (in the post-reform period). Information on judge retirements comes from judicial administrative records obtained from Registrar Offices of the High Courts. The data on total judges in each district high court comes from High-Court Annual Reports submitted to the Ministry of Law, Justice and Human Rights, Government of Pakistan.

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

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

Land Case = A subset of the constitutional cases. This is a dummy for the case involving a landownership or expropriation dispute with “*The State*”. These are “Eminent Domain” cases. The State here is the a housing development agency, which is authorized to resolve disputes with the public regarding land ownership on behalf of the government (e.g. Defense Housing Authority, the Lahore Development Authority (LDA), the Karachi Development Authority (KDA), the Peshawar Development Authority (PDA) and the Capital Development Authority (CDA)).

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

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

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

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

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

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

Gender = A dummy for male judges. This is coded in two ways: 1) Manually, where the author checks every judge name, and 2) Automatically, where the author asked Stata to read the string

starting with “Justice Miss” and “Justice Mrs.” as zero and the string starting with “Justice Mr.” as one. The two methods yielded an identical number of male and female justices.

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

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

Former Office Holder Bar Association = A dummy for the judge having been an office holder in the Lawyers’ Bar Association (before being appointed a High-Court justice). The data comes from a combination of biographical information contained in annual reports, Bar Council records and judicial administrative data.

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

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

Total Judges = A district-year count variable of the number of judges at a district High Court in a given year. The data comes from a combination of information contained in annual reports and judicial administrative data.

House Price = This variable is the price of house per square feet denominated in Pakistani Rupees. The data comes from www.zameen.com.

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

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

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

B. Data Appendix: Additional information and data collection

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

In this subsection we discuss 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 the 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 years in 1963 and Pakistan made the same change in 1969 (both as part of amendments to their respective constitutions). The mandatory retirement age of High Court judges has been 62 ever since (in both India and Pakistan).

Pakistan's judiciary is a three-tier hierarchical structure (see Figure C.2). The lowest Courts are the civil and session Courts, which hear civil and criminal cases respectively. These Courts are located in the provincial capitals and have jurisdictions dictated by the domicile of the litigating parties. The decisions in civil and session Courts can be challenged in Pakistan's High Courts. If the government expropriates land or violates a fundamental right, the High Court is the first (and in most cases) the only platform for individuals and firms for remediation. Although, in theory there are only four provincial High Courts in Pakistan, the benches of each are spread out over the four provinces (see Figure 3) in the form of 16 district High Court benches. Key for our paper is that cases can be filed against the government in the High Court in the form of a constitutional or criminal petition against the State. Constitutional cases involving *The State* are filed against the federal government, provincial governments and 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 end up being heard by the Supreme Court (Arshad, 2017).

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 Centre-Right Pakistan Muslim League Nawaz (PML-N, henceforth) led by Nawaz Sharif, and the Centre-Left Pakistan Peoples’ Party (PPP, henceforth) led by Benazir Bhutto. The 1990s was also a particularly volatile period in Pakistan’s history. For one, 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 bring “stability”, in what is now known as a “bloodless coup d’état of 1999.” General Musharraf consolidated his power and won a controversial referendum in 2002 that awarded him five years of Presidency and managed to cobble together a coalition government consisting of disgruntled ex-PPP and ex-PML-N lawmakers (Bose and Jalal, 2004).

With elections due in the January of 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 gain the largest share of the votes (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 against him. On 9th September 2008, the Pakistan Peoples Party's Chairman, the widower of Benazir Bhutto, Asif Ali Zardari, was sworn in as the 11th President of Pakistan. It was under this backdrop that the President Zardari and his party pushed for an amendment to the constitution that would dramatically change judicial selection in Pakistan.

B.3. Case Data Sources and Construction

The case characteristics as well as the outcome variables are based on the judgement orders available at the central repository of cases used by Lawyers in Pakistan to prepare their cases. This is available online at a law portal: (<https://www.pakistanlawsite.com/>). This website is the “Central Library” used by lawyers to prepare their cases (since Pakistan has a Common Law system, where case precedent is crucial), the central repository is also used by paralegals and students studying for their Law exams and contains the universe of (undigitized) cases in the High Courts from 1950 to 2016. Access is password-protected, where permission to use the website and cases is obtained through the Law firm. Typical examples of cases accessed are presented in Figures C.3 and C.4 in Appendix C (with the permission of the Law firm). As this library contains the universe of (undigitized) cases from 1950 to 2016, we had to choose a sample period given our budget and research question. We randomly sample all the available cases in every year from the universe of cases decided in that year from 1986 to 2016 inclusive. As the number of cases decided in a given year gradually rises over time, so does the fraction of sampled cases in our sample. Figure C.5 presents this information as plot of sampled cases and total available cases. There is a gradual rise in the total cases decided in Pakistan's High Courts from 1986 to 2016, which is reassuringly reflected in the randomly sampled cases.

Two teams of four paralegals supervised by a senior Lawyer each recorded the key information in the texts of the judgement order available at the website for these 7500 cases. Table C.1 presents the means of the outcome variables and case characteristics coded by the two teams, as well as correlation coefficient between them. There is a strong correlation between the coding of the two teams. For instance, the average State Wins figure from Team 1 is 0.50 and the correlation coefficient for State Wins between the two teams is 0.89. Since, there is some subjectivity in coding State Wins or Merit variable, we consider the robustness of our results across the two teams: we obtain similar results from the State Wins or Merit measure from either team. It is notable that the averages as well as the correlation coefficients are much more similar for variables that are plausibly more objective (e.g. for case delay, the correlation coefficient across the two teams is 0.99). This is reassuring. The discrepancies here most likely arise from minor coding errors. Throughout, the paper, for space reasons, we report the results from Team 1. Unsurprisingly given the high correlation coefficients, similar results are obtained from the dataset of Team 2 (the results from using variables from Team 2 are available on request).

C. Additional Tables and Figures

Table C.1: Outcome Variables and Case Characteristics

Variables	Team 1	Team 2	Difference	Correlation (ρ)
State Wins	0.50	0.56	-0.06	0.89
Case Delay	3.33	3.30	-0.03	0.99
Merit	0.62	0.67	0.05	0.88
Constitutional	0.72	0.70	-0.01	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	0.93
No. of Lawyers	4.04	4.09	-0.05	0.94
No. of Judges	1.81	1.83	-0.02	0.87
CJ in Bench	0.06	0.08	-0.02	0.83
Pg. of Judgement	8.88	8.71	0.03	0.97

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

Table C.2: Alternate Specification – Reduced Form with Post Treatment Interaction Term

	<i>State Wins</i>		
	(1)	(2)	(3)
OLS			
Retirements at 62/Total Judges X Post Reform	-0.233* [0.115]	-0.211* [0.120]	-0.296*** [0.0978]
Retirements at 62/Total Judges	0.0338 [0.0606]	0.0192 [0.0600]	0.0255 [0.0517]
District and Year Fixed Effects	Yes	Yes	Yes
District Controls	No	Yes	Yes
Case Controls	No	No	Yes
Observations	7,439	7,439	7,439
R-squared	0.044	0.050	0.052
Mean of dependent variable	0.50	0.50	0.50

Robust standard errors appear in brackets (clustered at the district level). The dependent variable is State Wins, a dummy for the case being ruled in favour of the State. Retirements at 62/Total Judges is the fraction of judges reaching the mandatory retirement age of 62 (pre and post reform). Post Reform is a dummy variable that switches on in 2010 i.e. when the selection reform goes into effect. The controls include all case and district characteristics in Table 1. The case controls also include case-type fixed effects. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table C.3: The impact of retirement in the pre- and post-reform periods (Reduced Form over time)

	OLS				
	1998-2001	2002-2005	2006-2009	2010-2013	2014-2016
	(1)	(2)	(3)	(4)	(5)
	<i>State Wins</i>				
Period1998_2001xRetired_1986	0.102 [0.0932]				
Period2002_2005xRetired_1986		0.0560 [0.0862]			
Period2006_2009xRetired_1986			0.0316 [0.0385]		
Period2010_2013xRetired_2010				-0.172 [0.159]	
Period2013_2016xRetired_2010					-0.334** [0.168]
District and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
District Controls	Yes	Yes	Yes	Yes	Yes
Case Controls	Yes	Yes	Yes	Yes	Yes
Observations	7,439	7,439	7,439	7,439	7,439
R-squared	0.050	0.050	0.050	0.050	0.051

Robust standard errors appear in brackets (clustered at district level). Retired_1986 is the fraction of judges reaching the mandatory retirement age since 1986, while Retired_2010 is the fraction of judges reaching mandatory retirement age since 2010. These variables are interacted with a dummy for the corresponding 3-year time periods. The case controls include case-type fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table C.4: The Impact of New Appointments on State Wins over Time

	2SLS				
	(1)	(2)	(3)	(4)	(5)
	<i>State Wins</i>				
Period 1998-2001 X Appoint since 1986	0.134 [0.278]				
Period 2002-2005 X Appoint since 1986		0.0862 [0.138]			
Period 2006-2009 X Appoint since 1986			0.0466 [0.0609]		
Period 2010-2013 X Appoint since 2010				-0.229 [0.197]	
Period 2013-2016 X Appoint since 2010					-0.375** [0.181]
District and Year FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Observations	7,439	7,439	7,439	7,439	7,439
R-squared	0.049	0.050	0.050	0.051	0.051

Robust standard errors appear in brackets (clustered at the district level). These are IV 2nd-stage results. The fraction of Judges appointed since 1986 is instrumented by the fraction of mandatory retirements post-1986. Likewise, the fraction of judges appointed post-2010 is instrumented by the fraction of retirements since 2010. The case controls include case-type fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table C.5: Balance Checks at the Case and Judge Level

Panel A: Case Level								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Constitutio nal Case	Criminal Case	No. of Pages	CJ on Case	No. Lawyers on Case	No. Judges on Case	Population	Population Density
After Reform Judge	0.004 (0.005)	0.003 (0.004)	0.211 (0.281)	-0.005 (0.016)	0.116 (0.252)	-0.035 (0.041)	-22,208* (12,497)	13.780 (8.765)
District and Year FE Controls	Yes Yes	Yes Yes	Yes Yes	Yes Yes	Yes Yes	Yes Yes	Yes Yes	Yes Yes
Observations	7,439	7,439	7,439	7,439	7,439	7,439	7,439	7,439
R-squared	0.099	0.992	0.246	0.017	0.057	0.072	0.995	0.995
Panel B: Judge Level								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Constitutio nal Case	Criminal Case	No. of Pages	CJ on Case	No. Lawyers on Case	No. Judges on Case	Population	Population Density
After Reform Judge	0.00482 [0.00352]	0.00291 [0.00353]	-1.692 [0.912]	-0.00675 [0.0148]	-0.296 [0.291]	-0.206 [0.511]	-150,535 [229,321]	68.982 164.34
Age Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Case & District Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	482	482	482	482	482	482	482	482
R-squared	0.090	0.091	0.301	0.026	0.080	0.145	0.219	0.037

Robust standard errors appear in brackets (clustered at the district level) in Panel A. Robust Standard errors are clustered at the judge level in Panel B (similar results are found if we use Newey-West standard errors). For Panel A, post-Reform Judge is a dummy for the case being adjudicated by a judicial-commission judge. For the judge-level regressions in Panel B, post-Reform Judge is a dummy for a judicial-commission judge. *** p<0.01, ** p<0.05, * p<0.1.

Table C.6: The Difference in Observed Characteristics of President and JC Appointed Judges

Variables	Pres. Judge	JC Judge	Difference	(p-value)
Gender	0.97	0.95	0.02	0.16
Former Lower Court Judge	0.11	0.10	0.01	0.70
Former Lawyer	0.89	0.90	-0.01	0.69
Age at Decision	53.43	54.18	-0.75	0.13
Former Office Holder Bar Asso.	0.70	0.27	0.43	0.00
Political Office Prior to Appoint.	0.23	0.08	0.15	0.00
Observations (judges)	347	135		

Note: The table lists average judge characteristics, their differences and the statistical significance for the differences between the judges appointed under the two selection procedures.

Table C.7: The Impact of Selection Reform on State Wins (by Chief Justice)

	2SLS				
	CJ Jamali (1)	CJ Khawaja (2)	CJ Mulk (3)	CJ Jillani (4)	CJ Chaudhary (5)
	<i>State Wins</i>				
CJ Jamali X Commission/Total Judges	-0.145 (0.106)				
CJ Khawaja X Commission/Total Judges		-0.326** (0.149)			
CJ Mulk X Commission/Total Judges			-0.355** (0.124)		
CJ Jilani X Commission/Total Judges				-0.283** (0.123)	
CJ Chaudhry X Commission/Total Judges					-0.263* (0.155)
District and Year FE Controls	Yes Yes	Yes Yes	Yes Yes	Yes Yes	Yes Yes
Observations	7,439	7,439	7,439	7,439	7,439
R-squared	0.050	0.051	0.051	0.051	0.051

Robust standard errors appear in brackets (clustered at the district level). The instrument and controls are identical to those in Table 2. The fraction appointed by Judicial Commission is interacted with the period when the respective Chief Justice was in office following the selection reform. The controls include case-type fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table C.8: The Effect of Reform on State Wins with District-Specific Trends

	<i>OLS</i>		IV, 2 nd Stage	
	(1)	(2)	(3)	(4)
	<i>State Wins</i>			
Commission Judges/Total Judges	-0.321** [0.130]	-0.292** [0.135]	-0.371** [0.167]	-0.344** [0.161]
District and Year Fixed Effects	Yes	Yes	Yes	Yes
District and Case Controls	No	Yes	No	Yes
District-Specific Trends	Yes	Yes	Yes	Yes
Observations	7,439	7,439	7,439	7,439
R-squared	0.048	0.055	0.048	0.055

Robust standard errors appear in brackets (clustered at the district level). The dependent variable is State Wins, a dummy for the case being ruled in favour of the State. Commission/Total Judges is the fraction of Judicial Commission judges. In the IV regressions, this is instrumented by the proportion of judges reaching the mandatory retirement age of 62 in the post-reform period. The controls include all the case and district characteristics in Table 1 and case-type fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table C.9: The Effect of Reform on State Wins with Randomly Scrambled Districts

	<i>OLS</i>		IV, 2 nd Stage	
	(1)	(2)	(3)	(4)
	<i>State Wins</i>			
Commission Judges/Total Judges (Scrambled)	-0.116 [0.158]	-0.126 [0.157]	-0.0280 [0.209]	-0.0450 [0.198]
District and Year Fixed Effects	Yes	Yes	Yes	Yes
District and Case Controls	No	Yes	No	Yes
Observations	7,439	7,439	7,439	7,439
R-squared	0.043	0.050	0.043	0.050

Robust standard errors appear in brackets (clustered at the district level). The dependent variable is State Wins, a dummy for the case being ruled in favour of the State. Commission/Total Judges (Scrambled) is the fraction of Judicial Commission judges where the 16 districts high courts are randomly scrambled. In the IV regressions, this is instrumented by the proportion of judges reaching the mandatory retirement age of 62 in the post-reform period where we use the randomly scrambled ordering of districts as used in the OLS estimation. The controls include all the case and district characteristics in Table 1 and case-type fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table C.10: The Impact of Selection Reform on State Wins at Different Levels of Clustering

	IV, 2 nd Stage			
	Before-After Clustering		District-Year Clustering	
	(1)	(2)	(3)	(4)
	<i>State Wins</i>			
Commission Judges/Total Judges	-0.311** [0.133]	-0.373*** [0.113]	-0.311*** [0.112]	-0.373*** [0.107]
District and Year Fixed Effects	Yes	Yes	Yes	Yes
District and Case Controls	No	Yes	No	Yes
Observations	7,439	7,439	7,439	7,439
R-squared	0.044	0.052	0.044	0.052
Mean of dependent variable	0.50	0.50	0.50	0.50

Robust standard errors appear in brackets. The first two columns cluster within each district separately before and after the 2010 reform. The subsequent two columns cluster within each district-year combination. The dependent variable is State Wins, a dummy for the case being ruled in favour of the State. Commission/Total Judges is the fraction of Judicial Commission judges. These are IV 2nd-stage results; the corresponding first-stage results can be found in Table 2 (Panel B). The controls include all the case and district characteristics in Table 1 and case-type fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table C.11: The Effect of Reform on State Wins on aggregated district-time panel

	OLS		IV, 2 nd Stage	
	(1)	(2)	(3)	(4)
		<i>State Wins</i>		
Commission Judges/Total Judges	-0.298*** [0.0899]	-0.331*** [0.111]	-0.357* [0.188]	-0.474*** [0.160]
District and Year Fixed Effects	Yes	Yes	Yes	Yes
District and Case Controls	Yes	Yes	Yes	Yes
Observations	496	496	496	496
R-squared	0.203	0.241	0.203	0.240
Mean of dependent variable	0.50	0.50	0.50	0.50

Robust standard errors appear in brackets (clustered at the district level). The dependent variable is State Wins aggregated at the district-time level. Commission/Total Judges is the fraction of judicial-commission judges. In the IV regressions, this is instrumented by the proportion of judges reaching the mandatory retirement age of 62 in the post-reform period. The controls include all the case and district characteristics in Table 1 and case-type fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table C.12: The Impact of Selection Reform on Case Filings

	2SLS, 2 nd Stage		
	(1)	(2)	(3)
	Total Filed	Constitutional Filed	Criminal Filed
Commission Judges/Total Judges	-1,665 [1,286]	-1,141 [926.8]	-524.1 [387.0]
District and Year Fixed Effects	Yes	Yes	Yes
District and Case Controls	Yes	Yes	Yes
Observations	448	448	448
R-squared	0.095	0.094	0.090
Mean of dependent variable	9557.09	6878.49	2678.59

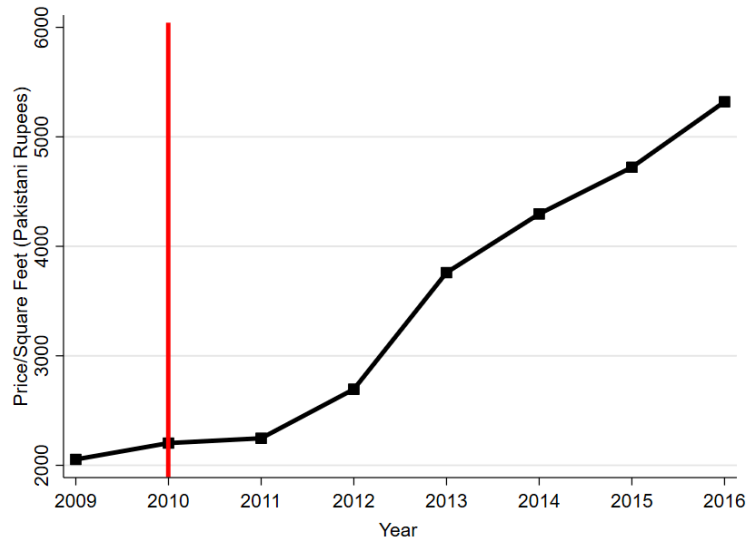
Robust standard errors appear in brackets (clustered at district-level). The dependent variable is total cases filed in the first column, total constitutional cases filed in the second column and total criminal cases filed in the third column. Commission/Total Judges is the fraction of Judicial Commission judges. This is instrumented by the fraction of judges reaching the mandatory retirement age of 62 in the post-reform period. The controls include all the case and district characteristics in Table 1 and case-type fixed effects. The regression is run at the district-year level, i.e. the level of variation of the dependent and main explanatory variables. *** p<0.01, ** p<0.05, * p<0.1.

Table C.13: 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>			
Commission Judges/Total Judges	-0.287** [0.116]	-0.329*** [0.109]	-0.283** [0.115]	-0.322*** [0.108]
District and Year Fixed Effects	Yes	Yes	Yes	Yes
District and Case Controls	No	Yes	No	Yes
Observations	7,439	7,439	7,439	7,439
R-squared (Pseudo)	0.032	0.038	0.050	0.047
Mean of dependent variable	0.50	0.50	0.50	0.50

Robust standard errors appear in brackets (clustered at the district level). The dependent variable is State Wins, a dummy for the case being ruled in favour of the State. Commission Judges/Total Judges is the fraction of Judicial Commission judges. The marginal effects from the corresponding Logit and Probit regressions are reported here. The controls include all the case and district characteristics in Table 1 and case-type fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Figure C.1: House Prices over Time



Note: The figure presents yearly residential property prices per square feet denominated in Pakistani Rupees. The data is obtained from an online portal of house prices across Pakistan (www.zameen.com). The vertical line indicates the 2010 judicial-selection reform.

Figure C.2: The Structure of the Judiciary

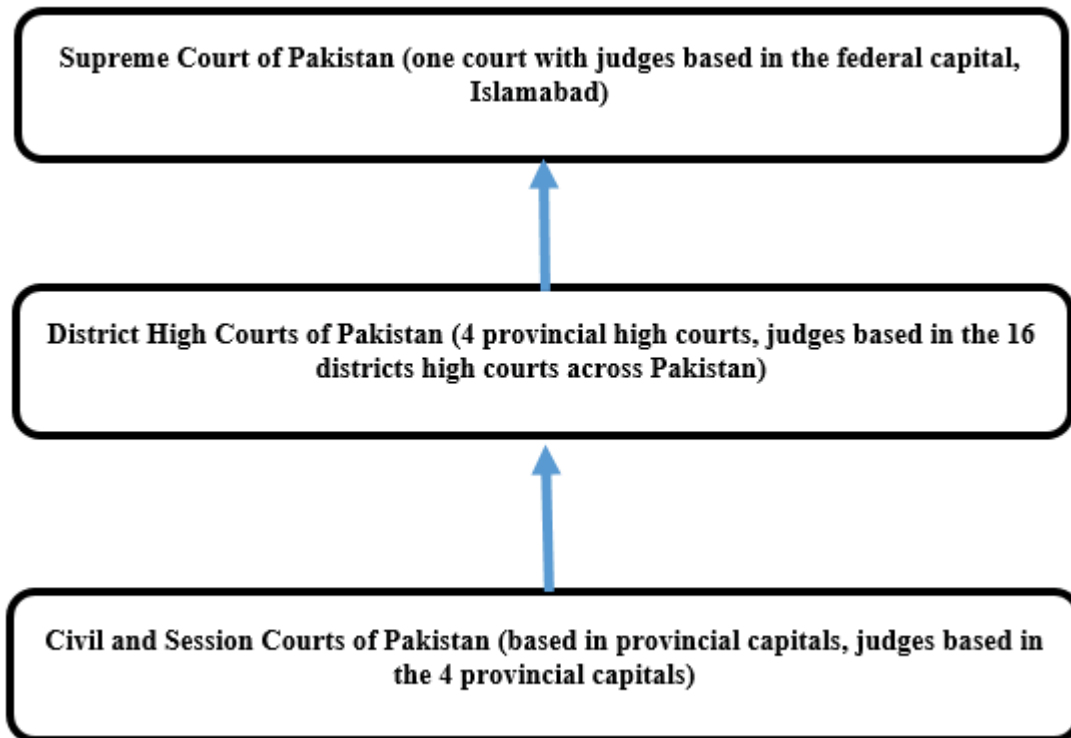


Figure C.3: An Example of a Land Case (case against the government dismissed)

2005 C L C 745

[Karachi]

Before Sabihuddin Ahmed and Khilji Arif Hussain, JJ

KHALID MOHSIN---Petitioner

versus

SECRETARY, MINISTRY OF DEFENCE, Government of Pakistan, Islamabad and 2 others---Respondents

Constitutional Petition No.59 of 1988, decided on 23rd November, 2004.

West Pakistan Land Revenue Act (XVII of 1967)---

---S. 45---Constitution of Pakistan (1973), Art.199---Constitutional petition---Maintainability---Disputed question of fact---Mutation, a document of title --Petitioner claimed to be owner of the land which had been mutated in favour of his predecessor-in-interest on the basis of allotment by Settlement authorities---Question for determination was whether the petitioner had acquired any title to the land so as to entitle him to the relief of possession or whether he had locus standi to seek declaration in respect of existence or otherwise of requisition---Validity---No allotment order by a competent officer in favour of the predecessor-in-interest of the petitioner was available on record---Petitioner had relied upon an order of Assistant Rehabilitation Mukhtiarakar directing that the land be mutated in the name of the predecessor-in-interest of the petitioner---Nothing was available to show that the predecessor-in-interest of the petitioner had applied for allotment of evacuee land against units available with him---Predecessor-in-interest of the petitioner was entitled to 159 units but was allotted land comprising of 453 units---Mutation entries did not confer title but could at the best be considered evidence of title which was rebuttable---Such disputed questions requiring detailed scrutiny of facts and production of evidence could not be undertaken in the proceedings under Art.199 of the Constitution-- Petition was dismissed in circumstances.

Hassan Akbar for Petitioner.

Nadeem Azhar Siddiqui, D.A.-G. and S. Tariq Ali, Federal Counsel for Respondents.

Ahmed Pirzada, Addl. A.-G.

Date of hearing: 29th August, 2004.

Figure C.4: An Example of a Land Case (Payment on land not made by government)

2009 C L C 1199

[Karachi]

Before Gulzar Ahmed and Malik Muhammad Aqil, JJ

MORRIS TANVIR---Petitioner

Versus

FEDERATION OF PAKISTAN through Secretary Ministry of Defence, Islamabad and 2 others-Respondents

Constitutional Petition No.D-2331 of 2006, decided on 16th July, 2009.

Transfer of Property Act (IV of 1882)---

---S. 54---Cantonment Land Administration Rules, 1937, Rr 15 & 16---Constitution of Pakistan (1973), Art 199---Constitutional petition---Amenity plot---Allotment---Plot in question was amenity plot reserved for school and petitioner contended that he purchased the same from a foreigner---Circumstance of petitioner was that authorities were not issuing lease in his favour---Validity---Sale of land for any such purpose without definite order of Central Government was prohibited under Rr 15 of Cantonment Land Administration Rules, 1937---Lease could be granted under R 16 of Cantonment Land Administration Rules, 1937, for maximum period of 30 years and maximum period of 90 years through public auction for building sites---Plots meant for municipal services and amenities could not be leased, sub-leased, sold or transferred to any person or to any one class of persons for the use and enjoyment rather it had to remain as government property for public purposes for unhindered free access, use and enjoyment of public at large---In no case plots meant for municipal services and amenities could be termed as building sites nor there could be any exceptional case or exceptional reasons for disposal of such land by private agreement or otherwise---There was no document of sale and purchase of plot in question between Defence Housing Authority and the foreigner in accordance with S 54 of Transfer of Property Act, 1882---Agreement to sell relied upon by petitioner was no agreement in the eyes of law as no authority had been shown nor any seemed to exist to sign the same on behalf of the foreigner---Transfer documents relied upon by petitioner were, therefore, of no consequence being themselves illegal and without authority of law and also seemed to High Court, without consideration---It seemed to High Court that fraud had been played by petitioner and officials of Defence Housing Authority for usurping and misappropriating valuable government property and there had to be an accountability of the same---High Court directed Federal Government to hold inquiry in the whole affair and to take action against all officials and persons found responsible in usurping and misappropriating valuable government property, and also to initiate criminal proceedings against such persons---High Court declined to issue any direction in favour of petitioner to authorities for issuance of lease deed---Petition was dismissed accordingly.

Ardeshr Covasjee and 10 others v. Karachi Building Control Authority (KMC), Karachi and 4 others 1999 SCMR 2883; Four Square Enterprises v. Karachi Building Control Authority PLD 2000 Kar. 161; Dr. Zahir Ansari and others v. Karachi Development Authority and others PLD 2000 Karachi 168 and Shafiqur Rehman and others v. Government of Sindh and others PLD 2006 Kar. 10 (ref.

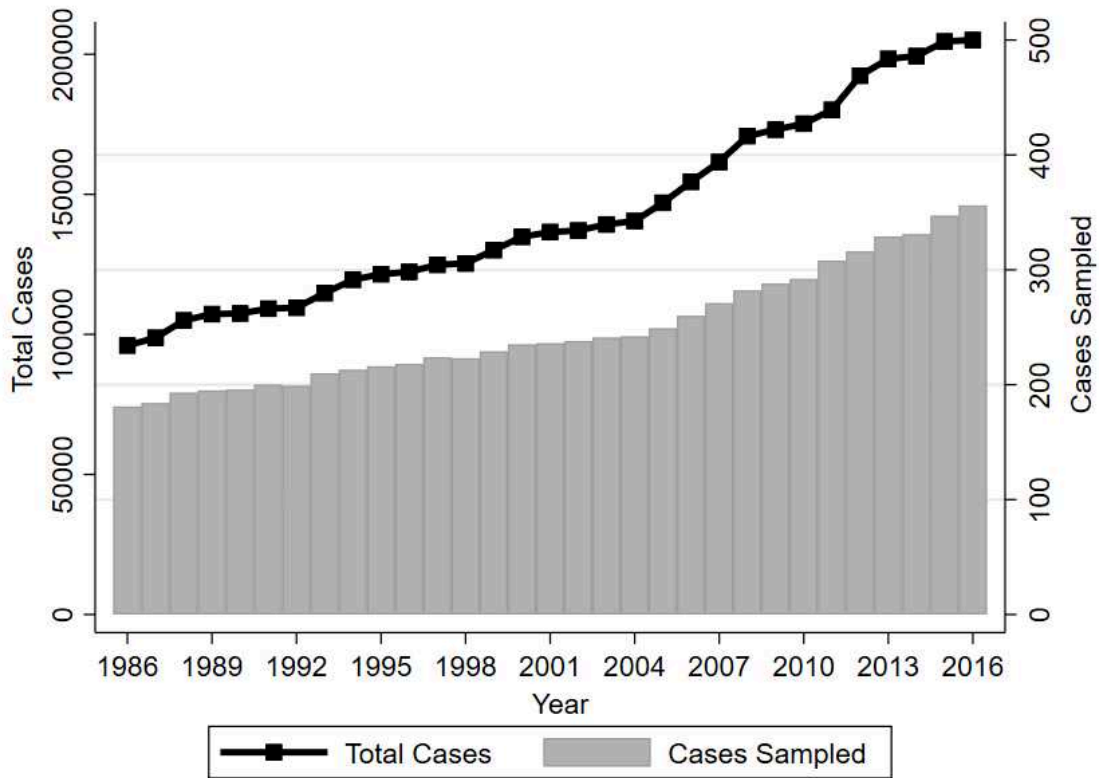
S.A. Jalib Chaudhry for Petitioner.

Ashiq Raza, Dy. A.-G. along with Hamid Niaz, Dy. Military Estate Officer for Respondents Nos.1 and 3.

Raja Sakandar Khan Yasar for Respondent No.2.

Date of hearing: 7th May, 2009.

Figure C.5: Total vs. Sampled Cases



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

Chapter 2 - The Dictator, the Imam and the Judge: Tracing the impact of religion on the courts

BY SULTAN MEHMOOD*

16th July 2019

How does religion impact the courts? In this paper, we document a substantial impact of religious leaders on judicial decision making in Pakistan. Utilizing a unique dataset on the holy Muslims shrines across Pakistan, we show that districts where historically the shrine density was high, a military coup in 1999 induced a large decline in judicial independence and quality of judicial decisions. We present evidence consistent with the mechanism that increased political power of religious leaders allowed them to influence the courts. In particular, we show the impact of shrine density on the courts is only observed where local religious leaders gained political power and only in the cases involving disputes with the local government. We also show a judicial selection reform that changed the appointment procedure to select judges from presidential appointment to selection by a judicial commission consisting of peer judges mitigates the effect of historical shrine density on judicial outcomes. (*JEL* D02, Z12, D72, K40, P37)

Keywords: religion, courts, institutions, Islam

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“The festivities here are Allah’s blessing, it cannot be found in tombs and palaces of kings...the message of this place is clear: for us to follow religion, follow it to the letter ...”.

Chief Justice Saqib Nisar at the Shrine of Data Ganj Bakhsh (Dawn, 2018)

I. Introduction

Religion, “the opiate of the masses” or the “soul of the soulless world” (Marx, 1844) is believed to influence economics, politics and society from time immemorial. The economics literature on religion provides wealth of evidence on how religion impacts development (Barro and McCleary, 2003; Kuran, 2011, Rubin, 2011, Cantoni et al., 2018), politics (Plateau, 2011; Chaney, 2013; Belloc et al., 2016; Bazzi et al., 2018) and social wellbeing (Clingsmith et al, 2009; Campante and Yanagizawa-Drott, 2015). Nevertheless, much less is understood about whether and how religion impacts formal institutions such as the judiciary. So, how does religion impact the judiciary? What are the mechanisms that link religion with judicial decision making?

In this paper, we answer these questions by combining a unique dataset on holy Muslim shrines spread across the districts of Pakistan with data on cases adjudicated in the district high courts. To measure judicial independence, we construct a judicial dependence variable called “State Wins”. This variable takes the value of 1 for “state victories” and 0 for “state losses” in a given case when the state is one of the parties.¹ Judicial cases involving the government as a party

¹For reasons that will become clear we examine how religious leaders impact judicial independence from the executive. Therefore, *the State* in this context includes the organs of the state yielding executive power such as public agencies, federal and local governments (in line with the conceptualizations of *The State* as an executive organ in Montesquieu, 1748). We ask a law firm to code this variable based on their reading of the texts of judgement orders. To reduce the inherent subjectivity in construction of some of these variables, we ask the law firm to divide in two independent teams to code the same cases. Table C.1 in appendix C presents correlation coefficients of the variables coded between the two teams of the coders. We obtain similar results for using either of the dataset.

in Pakistan cover a wide range of disputes, from business payment disputes to cases involving persecution of minorities, abuse of power, suppression of fundamental rights to the constitutionality of the military rules. Nevertheless, a substantial chunk of the petitions involving *The State* as a litigant in the high courts involved land expropriation disputes with the government (for instance, about 40% of all petitions filed in high courts involved land dispute with the government).

Using a military coup in 1999 as an exogenous shock to the local district high courts, we show that districts that had high historical shrine density experienced a large increase in State Wins.² We provide evidence consistent with the view that religious leaders associated with the shrines were able to influence the courts when they gained political office following the coup: the impact of shrine density on judicial outcomes is only experienced in those districts that implemented a local government system that mandated direct elections of mayors (*Nazims*) where religious leaders connected with the shrines gained political power. We verify these results by exploiting the 9/11 attacks in the US as an exogenous shock to implementation of the local government system in Pakistan that increases our confidence in the causal interpretation of the coefficients. This is because the 9/11 attacks and consequent War in Afghanistan instigated a ‘refugee crisis’ with about 2 million Afghan refugees moving to areas bordering Pakistan and Afghanistan (UNHCR, 2017). This reduced the probability that the local government reform was implemented in districts on the Afghan-Pakistan border.³

²The 1999 military coup is plausibly exogenous to judicial decision in the local district high courts. This is consistent with anecdotal accounts that argue that the coup was highly unanticipated and was result of a tussle between then Prime Minister Nawaz Sharif and military leadership (Bose and Jalal, 2004; Siddiqa, 2007).

³More details provided in section 5.2.

The increase in State Wins is only observed in cases involving land disputes with the government and human rights cases involving the State. This suggests that following the military coup, shrine leaders were able to use their political power to influence the courts and exert control over the population by expropriating land and violating fundamental rights such freedom of movement or right to an education. A placebo test provides a tighter link of this channel, since we observe no impact of high shrine density areas in criminal cases following the coup. This underscores the importance of cases involving land disputes and human rights cases with the government as key in explaining the results. This is consistent with anecdotal as well recent quantitative evidence that religious leaders reduced the provision of public goods (such as education) for their constituencies upon coming to power through the local government elections in 2000-2001 (Aziz, 2001; Mirza and Malik, 2018). We also document how a judicial selection reform that changed the appointment procedure to select judges from presidential appointment to selection by a judicial commission consisting of peer judges mitigated the effect of historical shrine density on judicial outcomes: the judges appointed by the judicial commission reduced the impact of historical shrine density on judicial outcomes.⁴

We demonstrate the robustness of the results by conducting a series of sensitivity checks. First, we show that the results are not driven by differential trends where we find no significant differential trends between low and high shrine density areas prior to the coup. Second, we present evidence that the increase in State Wins also implies a fall in the quality of the judicial decisions. Third, we show that the increase in State Wins following the coup is not a correction of low State Wins for high shrine density districts prior to the coup.⁵ Fourth, we present evidence that the

⁴We verify these results by using an instrumental variable strategy where we instrument fraction of judges appointed by the judicial commission by fraction of judges reaching the mandatory retirement age of 62 following the selection reform.

⁵That is, we show that the State Wins is not decreasing in shrine density prior to the coup.

results are not driven by a potentially confounding reform in 2004 that may have impacted judicial decision making. Fifth, we show that the results are insensitive to exclusion of potential outliers as well as the choice of the shrine dataset.⁶

The paper speaks to several strands of literature. First, the paper relates to growing literature on economics of religion. While most of the literature examines the impact of religion on economic (Clingingsmith et al, 2009; Campante and Yanagizawa-Drott, 2015; Malik and Mirza, 2018) or political outcomes (Platteau, 2008; Belloc et al., 2016; Rubin, 2017; Bazzi et al., 2018), the key contribution of the paper is to link religion with courts and by showing how subnational differences in judicial decision making is grounded in differential political power of the religious leaders. This echoes the themes in Platteau (2011), Chaney (2013) as well as in Rubin (2017) where a large part of the impact of (Islamic) religion on political and economic outcomes stems from the religious leaders' differential power over the course of history.

Second, we contribute to the literature on decentralization (Mas-Colell, 1980; Bardhan, 2002; Besley and Coate, 2003; Baum-Snow et al., 2017; Gulzar and Pasquale, 2017). While most of this literature has focused on the impact of decentralization in the delivery of the public goods, we contribute to this literature by showing how greater delegation of power to local politicians can have adverse consequences for the formal institution of dispute resolution, depending on the institutional structure and who is brought to power as a result of the decentralization.

Third, the paper relates to the theoretical literature on how informal institutions impacts formal institutions. On one hand, the “cultural channel” implies that religion impacts judiciary

⁶For instance, we show that the results obtain regardless of the choice of shrine dataset i.e. from British Colonial Gazettes or from Auqaf Department of the Ministry of Religious Affairs, Pakistan.

through providing greater legitimacy for the courts to synchronise their decisions with the executive: governments in higher shrine density enjoy greater legitimacy which in turn allows judges to rule in favour of the state more often (Rubin, 2017; Bisin, Verdier and Seror, 2018). On the other hand, greater religiosity impacts the courts through an “institutional channel” where the influence of religion stems from change in institutional structure such as a reform that varies the political power of religious leaders (Bazzi et al., 2018; Chaney, 2019). The evidence presented in this paper is consistent with the institutional view: the impact of religion on courts is mediated via changes in institutional structure of the local elections and appointment procedure of the judges.

The paper proceeds as follows. Section II provides the historical background on the shrines, courts and their relationship with the military coup. Section III presents the data and describes the sources for key variables used in the paper. Section IV presents the empirical methodology. Section V presents and discusses the main results. Section VI examines alternative explanations and verifies the results through conducting a series of robustness checks. Section VII concludes.

II. Background

This section is divided in two brief subsections that provides the background information and context of the study. We first discuss how shrines and courts are related, followed by a discussion on the relationship between shrines, courts and the military coup.⁷

2.1 Shrines and Courts

⁷Discussion on the structure and history of high courts is presented in the appendix B.1.

In this subsection, we discuss the background on the holy shrines and why these matter for judicial outcomes. Most historical sources suggest that holy Muslim shrines in South Asia were constructed around 12th and 13th centuries as tombs of Sufi saints. These shrines have continued to be places of worship and great reverence (Suvorova, 2004). Mughal emperors during the 16th century donated large sums of money and land to garner support from the local population as well as religious leaders associated with the shrine (Faruqui, 2012).

Historically, shrines and formal courts became linked when Indian subcontinent came under direct British rule in 1858. Under the British rule, the reward structure of religious leaders associated with the shrine was better systemized when British established formal property rights that allowed shrine resources to become subject to property law (Gilmartin, 1988). This is important since courts became directly involved in the matters of the shrines since the local district high courts would adjudicate upon shrine-controlled property. Ever since, courts have actively taken interest in matters associated with the shrine as well as the religious leaders associated with it.

The religious leaders associated with the shrines are key to understanding the importance and influence of the shrine. The focal person of each shrine is the *sajjada nashin* (literally, the wearer of the holy turban) who is believed to be a direct descendant of the Prophet Muhammad (S.A.W) of Islam. He acts as a “trustee” of all donations coming to the shrines and is responsible for holding traditional Sufi rituals at the shrine (Gilmartin, 1988). The power of these *sajjada nashin* or shrine trustees, derive from their devotees. It is widely believed that that the sacred genealogy of shrine trustees bestows on these religious leaders’ supernatural powers (Aziz, 2001). Therefore, local population seek shrine custodians’ attention for divine intercession to their problems. The allegiance of the devotees provides the shrine custodians a stable constituency of followers, a

potentially captive vote bank. The religious legitimacy is sustained through a relationship of master-disciple (*piri-mureedi*) with the local constituents.

The custodians of shrines are different from landed elite since they not only possess material wealth in the form of land but also “spiritual capital” (Iyer, 2016, p. 396). Therefore, some historians argue that shrine custodians can combine traditional instruments of landed elites such as coercion with voluntary compliance (Aziz, 2001). The persistence of religious power of these shrine leaders perpetuates through a permanent family seat (*gaddi or sajjada*).⁸

Anecdotal accounts suggest that the *gaddi nashin* have historically played a prominent role in politics during British rule as well as present day Pakistan (Gilmartin, 1988; Aziz, 2001). For instance, former Prime Minister Yousaf Raza Gilani and the current Foreign Minister Shah Mahmood Qureshi are descendants of shrine families and trustees of shrines in Pakistan. Likewise, these accounts also suggest that judges including several chief justices have visited these shrines and interact with the *sajjada nashin* (Khan, 2018).⁹ Indeed, recently the courts even formalized the role of religious leaders associated with the shrine by setting a legal precedent: “...with *sajjada nashin* rests the responsibility of the spiritual functions of guidance of the disciples and the performance of rituals...” (Case No. 542-L PLD, 2018).

2.2. Shrines, Courts and the Coup

How are the shrine density and military coup linked? The answer seems to lie in the decentralization reform by General Musharraf following the military coup. Musharraf “Devolution

⁸ In fact, shrines provide a safeguard against dilution of landed power through inheritance since the transfer of the *gaddi* or *sajjada* (religious seat) is through a “sacred genealogy” where seat is passed to the eldest son without traditional fragmentation of property due to inheritance (Malik and Mirza, 2018).

⁹See Figure C.1 and C.2 in the appendix for pictures of *gaddi nashins* performing traditional rituals as well as their pictures with multiple judges.

of Power Plan” introduced a local government system that allowed direct election of a mayor (*Nazim*) with substantial power in the distribution of public goods and allocation of district resources (Cheema et al., 2006).¹⁰

Anecdotal accounts suggest that the local government system institutionalized the “patron-client relationship between bureaucracy and local political elites” (Malik and Mirza, 2018, p. 19). We present evidence consistent with the view that judiciary was one such bureaucracy that could be influenced by shrine leaders who gained political power during the 2000-2001 local government elections. This is consistent with a long history of local and national politicians trying to “control the state apparatus” including the courts (Bose and Jalal, 2004; Martin, 2015).

III. Data

The shrine data is constructed from two key sources: British Colonial Gazettes and Auqaf Departments of provincial ministry of religious affairs.¹¹ The British colonial Gazettes provides data for all the shrines in Punjab and Sindh that allows us to cover all the judicial district high courts within the Sindh and Punjab province. The Pakistan governmental archives at the Provincial Auqaf Departments at the Ministry of Religious Affairs allows us to cover the remaining judicial

¹⁰While the local government did exist in the past, they “were practically inactive” since they had no power to allocate expenditures or raise taxes nor were they elected through direct elections (Cheema et al., 2006, p. 14).

¹¹British Colonial Gazettes were official bulletins of the British government that published public and legal notices aimed at the local population in British India. Under the section of “fair and festivals” they recorded the names of the shrines as well as the festivals taking place in the Punjab and Sindh province (see, Figure C.4 for example of the raw data and for further discussion of this data source, see data appendix B.3).

districts in the provinces of KPK and Baluchistan.¹² Therefore, combining these two shrine datasets allows us to measure the number of shrines in every judicial district in Pakistan.¹³

We obtain data on judicial cases from the central repository of cases that are used by lawyers to prepare their cases. We randomly sample 7500 cases from 1986-2016 for all the 16 districts high courts of Pakistan (from universe of all decided cases in this period) and match it with details on all shrines mentioned in British colonial archives and provincial Auqaf departments.¹⁴ Figure 1 presents the map of shrine density across the judicial districts of Pakistan, while Table 1 reports the descriptive statistics of the variables used in the study.¹⁵ Below we present descriptions of key outcome and the explanatory variables used in the analysis. Further details on the variables, data, their sources and compilation can be found in the appendices A and B.

Outcome variables. — The key outcome variable is State Wins. This is a case level measure of judicial independence we use in the paper. It is constructed based on the texts of judgment orders that contains the information on the contents of the case. Following the literature (e.g. Djankov et al., 2003 and La Porta et al., 2008), we asked a law firm to code these variables. In particular, the judicial dependence variable called “State Wins” is coded as 1 for state victories and 0 for state

¹²Since, British directly ruled Sindh and Punjab, their official gazettes did not record the shrines of districts outside their geographical boundaries. The data for Punjab and Sindh from colonial archives is compiled by historian Rinchan Ali Mirza, for details on the data see Mirza and Malik, 2018).

¹³We show as part of a robustness check that the results are similar if we use either dataset.

¹⁴ Details on the sampling procedure as well as further information on case level data collection is presented in data appendix B.2

¹⁵ In the baseline regressions, instead of the originally sampled 7500 cases we end up using 7,439 observations. This is because for few judgement orders the quality of texts does not allow to detect the name of the judge to match it with judge characteristics. Nevertheless, running the regression on 7500 observations without judge controls has no significant impact on qualitative and statistical significance of the results (more details in the appendix B.2).

losses, in all the cases that have the government as a party.¹⁶ This includes the organs of the state yielding executive power such as local government, federal and provincial governmental agencies (in line with the conceptualizations of The State as an executive organ in Montesquieu, 1748). In the analysis of the quality of judicial decisions, we use two additional outcome variables: Case Delay and Merit, where unit of observations is also at the case level. Both these variables are also constructed based on the information available in the texts of the judgement order. The former is calculated by taking the difference between the case decision year relative to the filing year. Merit is a measure of quality of the decision. This is a binary variable, also coded by the law firm, that switches on if the decision is “based on evidence rather than technical or procedural grounds” (Pound, 1963). This is based on common law jurisprudence, where cases decided on merits i.e. based on evidence and spirit of the law, rather than technicalities of law is an ideal that common law regimes aspire towards (see e.g. Tidmarsh, 2009 for a discussion).

Explanatory variables. — We use cross-district data on shrine density in 1911 from British colonial archives and Auqaf Department Archives at the Ministry of Religious affairs in Pakistan.¹⁷ We measure shrine density with shrines per 1000 people in the judicial district. Specifically, we sum all shrines present in the given judicial district in 1911 and normalize it by the population in the district. This allows us to obtain the historical “shrine density” measure at the (judicial) district level (see Figure 1).¹⁸ We also construct a dummy variable for military coup which switches on in 1999, the year when General Musharraf seized control of the government through a coup d’état.

¹⁶We verify the results by comparing results across two teams of coders within the law firm (see discussion in Appendix B.2 and Table C.1 for correlation coefficient across the coders).

¹⁷The Auqaf department records are taken from earliest available year (1950). This is combined with Colonial Gazette records from 1911. This aggregation allows to cover every district high court jurisdiction in Pakistan. We show that this aggregation is indeed justified, where the results are robust to using either dataset.

¹⁸Specifically: Shrine Density = $\frac{\text{Number of Shrines in the Judicial District}}{\text{Total Population in the Judicial District}} \times 1000$.

IV. Empirical Methodology

We use cross-district variation in shrine density and the exogenous shock of the military coup to the local district high courts to identify the effect of shrine density on judicial outcomes. The main specification is as follows:

$$Y_{cjd t} = \theta + \kappa \text{Military Coup } 1999_t \times \text{Shrine Density at } 1911_d + \delta_d + \gamma_t + \mathbf{W}'_{cjd t} \boldsymbol{\varphi} + \varepsilon_{cdjt} \quad (1)$$

Subscripts c , j , d and t indexes cases, judges, district and years, respectively. Y represents State Wins where the unit of analysis is at the case level.¹⁹ Military Coup is a dummy variable that switches in the post-coup period, while Shrine Density denotes historical shrines per 1000 people in a district. δ_d and γ_t are district and year fixed effects while \mathbf{W} are potential correlates of judicial outcomes, listed as case, judge and district controls presented in Table 1.

The interaction between Military Coup and Shrine Density is the main variable of interest. The coefficient on this interaction κ is the differences-in-differences estimator for the impact of shrine density on judicial outcomes (following the coup). The key identification assumption in equation (1) is that there are no differential trends for judicial outcomes among districts with different shrine densities prior to the coup (conditional on controls). We test for this by replacing the interaction between military coup and historical shrine density by a series of interactions between shrine density and dummies indicating various pre-coup and post-coup time periods. Likewise, to ensure that the results are not driven by a potentially confounding reform that may

¹⁹When we examine quality of judicial decisions, Y will represent Case Delay and Merit Decisions. The results are also robust to district-time aggregation i.e. at level of variation of the key interaction term.

have impacted the courts, we adjust the specification by interacting time period for which this reform was in effect (2004-2009) with the historical shrine density.²⁰

V. Results

5.1. *The Effect of the Shrines on State Wins*

Table 2 estimates equation (1) and reports the results on the impact of shrine density on State Wins. We observe that one standard deviation increase in shrine density (0.005) increases State Wins by about 5 percentage points (following the coup). In all specifications, we find a positive and statistically significant estimate of the coefficient on the interaction term between shrine density and post-coup dummy. The coefficients are similar without and with inclusion of large number of controls (listed in Table 1), implying that the military coup acts as a plausibly exogenous shock to the local district high courts.

Nevertheless, these results hinge on the main identification assumption of the differences-in-differences estimator, i.e. there are no differential pre-trends in state wins among districts with high and low shrine densities. Figure 2 visually represents the main results by plotting the coefficients on these interaction term along with their 90% confidence interval in two-year periods. We find no evidence of differential trends prior to the coup. The figure also documents how the magnitude of the effect evolved over time. Particularly, towards the end of sample period, the (point estimate) impact of shrine density following the military coup seems to be attenuating. In

²⁰This reform gave the Supreme Court power of judicial review over the Presidents' decision to dismiss the legislature. Although, this power rested only with the Supreme court not the district high courts we analyze here, however, one could reason that the increase in judicial power in the Supreme court could encourage lower courts to follow the Supreme Court (as argued in Chen et al., 2016), in manner correlated with factors correlated with historical shrine density. Therefore, we examine this possibility by including interaction of dummy when this reform was in effect with historical shrine density (although increasing the power of the courts in the post-coup period would most likely bias the estimated results downwards). More details on this will be provided when we conduct this robustness check in Section VI.

the discussion of mechanisms in subsection 5.5, we discuss how this attenuated effect may be explained by a judicial selection reform that changed the selection mechanism of judges from presidential appointment of judges to selection by a merit based judicial commission.

5.2. Mechanisms: Local Government Elections and Shrines

In this section, we present evidence consistent with the anecdotal accounts that suggest that local government elections increased the power of religious leaders associated with the shrine, institutionalizing possibly a patron-client relationship with the local judiciary (Aziz, 2011; Malik and Mirza, 2018). We cannot use the timing of local elections to explicitly test this channel. This is because local elections were held during the early days of a martial law regime, which according to many commentators were held “in a hurry” to give a “democratic cover” to the regime between January 2000 to September 2001 (Paracha, 2003). Given the lack of time variation and data constraints, we cannot use timing of the elections to examine this channel.

Nevertheless, there was “significant heterogeneity in the extent of implementation” (Cheema et al., 2006) of the local government system, where the occurrence of election did not always coincide with actual formation of local governments in 2000-2001. The speed and lack of administrative capacity for local government system to be implemented implied some districts could not have a functioning local government despite the elections held in these districts (Chellaney, 2002; Cheema et al., 2006). That is, election in a district, did not imply the local government system was enforced. By end of sample period, around 35% of the districts did not have a local government as a result of these elections (ECP, 2018). The local government formation process in the district, however, might be endogenous. This is because district (bureaucratic) capacity might be correlated with both differential implementation of the local government system and shrine density.

To mitigate this concern, we exploit the exogenous shock of 9/11 attacks in the United States and consequent US invasion of Afghanistan in October 2001 that instigated a ‘refugee crisis’ with a move of about 2 million Afghan refugees to Pakistan (Kronenfeld, 2008). By end of 2001, there were over 4 million Afghan refugees living in Pakistan where the UNHCR set camps in the areas along the Pakistan-Afghan border (UNHCR, 2017). This put additional stress on the limited state capacity and increased the relative probability that local governments would not form in the Pakistani districts conjoint with the Afghan border (Chellaney, 2002). We have enough variation to explore this channel since Afghanistan and Pakistan share a long land border of 2430 km that covers 25% of the total district high courts in the sample. This allows us to examine the differential impact of the shrine density on judicial outcomes for those districts that had a higher probability of a functioning local government i.e. where it was more likely for the local government system was enforced.²¹ The following equation is estimated:

$$Y_{cjd\text{t}} = \theta + \lambda \text{MilitaryCoup } 1999_t \times \text{ShrineDensity at } 1911_d \times \text{LG Enforced}_d + \kappa \text{MilitaryCoup } 1999_t \times \text{ShrineDensity at } 1911_d + \delta_d + \gamma_t + \mathbf{W}'_{cjd\text{t}}\boldsymbol{\varphi} + \varepsilon_{cdjt} \quad (2)$$

LG Enforced is a dummy variable that is switched on for the districts that had a higher probability to have a local government by end of 2016 (when our sample period ends), while it is turned off for districts touching the Afghan border. The coefficient of interest here is λ that measures how much shrine density affects judicial outcomes in districts where there was a higher probability that elections did lead to the formation of local government system. Table 3 presents these results. We observe the estimate of λ is qualitatively and statistically significant. One standard deviation

²¹ Specifically, LG Enforced is switched off for the following district high courts whose district jurisdictions share a border with Afghanistan: Quetta High Court, Khyber High Court, D. I. Khan High Court and Kashmir High Court (see Figure 1).

increase in shrine density increases State Wins by about 5 percentage points. The observed $\lambda > 0$ imply that in districts that had a higher probability for the local government to be formed, the impact on State Wins is greater relative to districts where there was lower probability for the local government to be formed. Moreover, we cannot reject the null effect of $\kappa = 0$. This implies that most of the impact of historical shrine density following the coup is observed only in areas where decentralization had higher probability to be enforced. This suggests that “cultural channel” linking religion with courts is relatively less important than the “institutional channel” of decentralization.²²

Our confidence in the institutional channel of shrine elites influencing the courts is strengthened when we examine the disaggregated effect of cases involving the State. That is, we divide State Wins into its component parts i.e. disputes where *The State* is the local, provincial and federal government, respectively. Table 4 presents these results. Although, we cannot statistically reject the null of homogeneous effect, the results are highly suggestive that the impact of shrine density following the coup stems from disputes with the local government.

5.3. Mechanisms: Types of cases driving the results

In this subsection, we examine the type of cases driving the results. In the first two columns of Table 5, we estimate equation (1) for cases involving land disputes with the government without and with the full set of controls, respectively. This is motivated by anecdotal accounts that suggest that the expropriation of private property by local government agencies (such as the Lahore Development Authority and Karachi Development Authority) is facilitated by the courts in

²²Nevertheless, it is possible that we do not have enough statistical power to disentangle the residual impact of the of shrines following the coup separate from the decentralization.

Pakistan (Sattar, 2017).²³ Furthermore, following the local elections, these agencies began to report directly to the (locally) elected district *Nazim* (mayor). The results from regressions on cases involving land disputes with the government imply that a 1 standard deviation increase in shrine density increases State Wins by about 8 percentage points.

Next, we consider how the military coup and shrine density impacted human right cases. We define human right cases as constitutional petitions that does not involve land disputes with the government. These cases are separately marked as “writ petitions” within the constitutional cases and involve cases pertaining to violation of fundamental rights such as freedom of movement or discrimination based on religion, gender and caste. For instance, in a typical case in the dataset, a woman pleads that she was “denied entry into medical school based on her gender” or that an individual pleads his fundamental right of freedom of movement within and outside Pakistan was restricted by the government.²⁴ We observe qualitatively and statistically meaningful impact in human rights cases: 1 standard deviation increases in shrine density increases State Wins by about 7 percentage points (Column 3 and 4, in Table 5). This is consistent with Aziz (2001, p 159) who notes that shrine elites violate the fundamental right to an education where “even the most superficial kind of public instruction ... might push some of his spiritual slaves out of their prison of superstition and unthinking obedience. Education is a plague which he does not want his flock to catch”. Likewise, Malik and Mirza (2018) documents that districts in Punjab with higher shrine density had lower spending on public education and literacy rates.

²³Examples of raw data based on the texts of judgement order of typical cases involving land disputes with the government can be found in the Appendix C (Figure C.5 and C.6).

²⁴ Examples of raw data for these cases i.e. texts of judgement orders can be found in the Figure C.7 and C.8 of Appendix C.

To further examine the political influence channel, we conduct a placebo test where we evaluate the impact of shrine density on State Wins for criminal cases, where *The State* acts as the prosecution (State Wins here can be interpreted as conviction rates). Table 6 presents these results. We find none of the coefficients are statistically significant. In fact, in most specifications, the coefficient estimates corresponding to the interaction terms of interest are negative. This more tightly links the political influence channel where the increase in State Wins is only observed in land and human rights disputes with the government and not in quotidian criminal cases.

5.4. Does the increase in State Wins imply a deterioration in the quality of judicial decisions?

Next, we show that the increase in State Wins following the coup reflects a deterioration in the quality of the judicial decisions. To do this, we replace the outcome variable, State Wins with Case Delay and estimate equation (1), where the latter is the difference between decision year and filing year. Table 7 (Panel A) presents these results by the type of cases. Overall, 1 standard deviation increase in shrine density implies an increase in case delay by about 0.2 years (or 2.5 months). Consistent with what was observed earlier, the results stem from cases involving land and HR disputes with the government, whereas we fail to reject the null effect of no increased delay in criminal cases.

State Wins and Case Delay can be interpreted as separate outcome variables where the former is a measure of judicial independence while the latter a measure of judicial efficiency. Nevertheless, there is good reason to believe that in the current context State Wins and Case Delay might be linked. Several anecdotal accounts suggest that judges delay cases as a strategy to favor governments (Zafar, 2012). This becomes particularly salient when government officials use the expropriated land for private benefit while the case is pending in the court or they do not rule over

cases when the government position is particularly weak (Arshad, 2018). The null effect in criminal cases for Case Delay is consistent with this channel.

It may be reasoned, however, that the increase in case delay following the coup may stem from a greater deliberation on the cases. This kind of increased case delay would represent an increase in the quality of the judicial decisions. Nevertheless, our confidence that the increase in State Wins and Case Delay following the coup implies a deterioration in the quality of the judicial decisions is strengthened when we examine cases decided ‘on merits’. In common law jurisprudence, the rulings on merits imply that the judicial decision is “based on evidence rather than technical or procedural grounds” (Pound, 1963). We use this as a measure of quality of the judicial decision. We examine the quality of the decisions, by examining how historical shrine density differentially impacted meritorious decisions following the coup. Table 7 (Panel B) reports these results by type of cases. The overall estimates imply that a standard deviation increase in shrine density decreases case quality by about 6 percentage points (Table 7, Panel B, Column, 1). The point estimates imply that the largest reduction in quality of decisions is observed in cases involving land disputes with the government, whereas there seems to be no change in the quality of decisions in criminal cases (the coefficient in criminal cases is positive, though statistically insignificant).

5.5. Mechanisms: Attenuation in the impact of Shrines through a Judicial Selection Reform

We proceed, the examination of mechanisms, by documenting how a 2010 judicial selection reform that changed the appointment procedure to select judges from presidential appointment to selection of judges by a judicial commission consisting of peer judges attenuated the impact of historical shrine density following the coup. The motivation behind examining this

is the large and statistically significant falls in State Wins and Case Delay and increase in Merit decisions following the 2010 judicial selection reform (see Figure 3).

We examine how the historical shrine density affect the impact of the selection reform on judicial outcomes. Since, there are limited vacancies for the judges in the district, the implementation of the reform was staggered across district-time. We exploit this feature to examine the extent to which the intensity of the reform differentially impacts state wins in high and low shrine density districts. The following equation is estimated:

$$Y_{cjd\text{t}} = \theta + \omega \text{ShrineDensity at 1911}_{d\text{t}} \times \frac{\text{Commission Appointed Judges}}{\text{Total Judges}}_{d\text{t}} + \zeta D1999_2007 \times \text{ShrineDensity at 1911}_{d\text{t}} + \Upsilon D2008_2010 \times \text{ShrineDensity at 1911}_{d\text{t}} + \eta D\text{Post}2010 \times \text{ShrineDensity at 1911}_{d\text{t}} + \delta_d + \gamma_t + \mathbf{W}'_{cjd\text{t}}\boldsymbol{\varphi} + \varepsilon_{cdjt} \quad (3)$$

All variables are similar to those defined in equation (1) with the following exceptions:

$\frac{\text{Commission Appointed Judges}}{\text{Total Judges}}$ is the fraction of judges appointed by the judicial commission in a given district-year. $D1999_2007$ and $D2008_2010$ are dummy variables that switch on during military and democratic rules, respectively, prior to the selection reform, whereas $D\text{Post}2010$ is a dummy that switches on in the post-reform period.

Since, there may be a differential effect of historical shrine density on judicial outcomes in military and democratic periods before and after the reform, we add several interaction terms: $D1999_2007 \times \text{ShrineDensity at 1911}$ captures the differential effect of shrine density on judicial outcomes during military rule before the reform, whereas $D2008_2010 \times \text{ShrineDensity at 1911}$ captures the differential impact of shrines on judicial

outcomes during the democratic period before the reform. Finally, $Post2010 \times ShrineDensity$ at 1911 captures the differential effect of shrine density on judicial outcomes in the post selection reform period. The coefficient of interest here is ω that measures how the judicial outcomes are differentially impacted by the judicial selection reform in high and low shrine density districts.

Table 8 (panel A) estimates equation (3) by least squares. We find empirical support for the conjecture that the judicial selection reform reduced the impact of historical shrine density on judicial outcomes. The judges appointed under the new selection procedure had the largest impact on judicial outcomes in higher shrine density districts where more judges appointed by the judicial commission reduces State Wins, Case Delay and increases decisions on Merit. For instance, 1 standard deviation increase in shrine density reduces State Wins by 1.3 percentage points at 10% judicial commission judges ($0.005 \times 10 \times 26.02$) relative to reduction of State Wins by 13 percentage points at 100% of judges selected by the judicial commission ($0.005 \times 100 \times 26.02$).

Nevertheless, least squares estimation of ω might be biased if the judicial commission appointments are made considering factors correlated with historical shrine density (such as historical non-religious political power of the district) and judicial outcomes. Although, we do add district fixed effects and host of controls to the specification but we verify the results by using an instrumental variable strategy where we instrument the fraction of judges appointed by the judicial commission by fraction of judges reaching the mandatory retirement age of 62 following the selection reform.²⁵ Under the assumption that judges reach their 62nd birthday randomly across

²⁵Figure C.9 in the Appendix C provides plots of the endogenous and instrumental variable used in the regressions. It shows that following the reform in 2010 exit of judges based on mandatory retirement age of 62 is highly correlated with the fraction of judges appointed by the judicial commission. We also find evidence consistent with this view that the instrument is plausibly exogenous since it is uncorrelated with any of the district or case characteristics controls used in the paper (results available on request).

district-time, this increases our confidence in the causal interpretation of ω . Table 8 (Panel B) reports these IV results. We find that the estimates from the instrumental variable regressions are qualitatively and statistically similar to the estimates obtained from least squares.

VI. Robustness

6.1. Are the estimates reflecting particularly high judicial dependence before the coup?

The positive and statistically significant coefficient estimate of κ in equation (1) does not necessarily reflect that following the coup, there is an increase in judicial dependence in high shrine density districts. It is possible that the increase in State Wins following the coup in high shrine density districts is a correction of particularly low State Wins in high shrine districts prior to the coup. This is possible if, for example, the military dictator restores a ‘disequilibrium’ by correcting the disproportionately low State Wins in high shrine districts prior to the coup. We examine this possibility by examining the average State Wins in high and low historical shrine density districts before and after the coup. If State Wins is decreasing in shrine density prior to the coup, while following the coup State Wins is constant for high and low shrine density districts, then the observed $\kappa > 0$ might indeed reflect the post-coup correction of particularly low State Wins prior to the coup in the high shrine density districts.

We examine this possibility. Figure 4 plots average State Wins and shrine densities both before and after the coup. We observe that State Wins is roughly constant prior to the coup, whereas average State Wins is increasing in shrine density following the coup.²⁶ This observation is robust

²⁶Similar results hold for Case Delay and Merit decisions (Figure C.10 in the Appendix C show the case delay is roughly constant prior to the coup and increases sharply following the coup). Likewise, Merit decisions is roughly constant prior to the reform, whereas it falls steeply following the coup (Figure C.11).

to both district-wide averages (left panel) as well as district-year averages (right panel).²⁷ This strengthens the case that following the coup, the increase in State Wins is not a pre-coup correction for particularly low State Wins.

6.2. Alternative Explanation

One key alternative explanation that might be driving the results is the 17th Amendment to the Constitution of Pakistan that was passed in the December 2003. This amendment included a package of reforms that gave a legislative cover to the military coup of 1999.²⁸ Nevertheless, this legislation had an important clause impacting the courts. As result of bargaining between the national politicians opposed to General Musharraf and the Musharraf regime, the presidential power to “dissolve national assemblies” was retained but was subjected to judicial review or a “veto” by the Supreme court (Nelson, 2010). This could be an alternative mechanism driving the results if local district high courts follow the precedents of higher State Wins set by the Supreme court following the coup (as in Chen et al., 2016). This is possible under scenarios where the Supreme Court justices want to signal compliance to the military regime and the lower courts follow suit.²⁹

We examine this alternative channel by estimating the following equation:

$$Y_{cjd\text{t}} = \theta + \kappa \text{MilitaryCoup } 1999_t \times \text{ShrineDensity at } 1911_d + \rho \text{ } 17 \text{ Amendment}_t \times \text{ShrineDensity at } 1911_d + \delta_d + \gamma_t + \mathbf{W}'_{cjd\text{t}}\boldsymbol{\varphi} + \varepsilon_{cdjt} \quad (4)$$

²⁷The district-year averages are a more relevant comparison since we exploit variation across district-years in the regression results.

²⁸This was required to preempt further litigation against General Musharraf since under Pakistani constitution, a military coup is “high treason punishable by death” (Constitution of Pakistan, 1973; 2018).

²⁹We do not have a common identifier for cases across the high and Supreme court to empirically examine this dependence. Nevertheless, if true, it would change the interpretation of mechanism explaining the link between shrine density and judicial outcomes.

All variables are similar to those defined above except the additional interaction term of shrine density with a dummy for the time period 17th Amendment remained active. This dummy switches on for the period that this law was in effect (2004-2009).³⁰ Table 9 (column 1 and 2) presents these results. We observe that there are no differential effects on State Wins over the baseline impact of shrines following the coup due to the 17th amendment.

6.3. Additional Sensitivity Checks

In this subsection we present two key robustness checks. First, we demonstrate the robustness of the results by showing that the results are similar when we exclude potential outlier districts. Second, we show that the results are insensitive to the choice of the shrine datasets.

From Figures 4, we observe Shrine Density is particularly high in some districts (for instance, Sukkur and Bahawalpur have 0.015 and 0.013 shrines per 1000 people relative to the average 0.005). It is possible that the positive relationship between shrine density and State Wins we observe post-coup is primarily driven by changes occurring in these ‘outlier’ districts. To examine this possibility, we estimate the following equation:

$$\begin{aligned}
 Y_{cjd} = & \mu + \kappa \text{MilitaryCoup } 1999_t \times \text{ShrineDensity at } 1911_d + \\
 & \theta \text{MilitaryCoup } 1999_t \times \text{ShrineDensity at } 1911_d \times \text{OutliersExcludedDummy}_d + \delta_d + \\
 & \gamma_t + \mathbf{W}'_{cjd} \boldsymbol{\varphi} + \varepsilon_{cjd}
 \end{aligned} \tag{5}$$

The equation above is similar except it has an additional interaction term where the key variable of interest $\text{MilitaryCoup } 1999_t \times \text{ShrineDensity at } 1911_d$ is interacted with the Outlier Excluded Dummy variable. The latter is a dummy variable that switches on for all districts

³⁰This law went into effect in January 2004 and was abolished in early 2010 after the democratic government passed the 18th Amendment to the Constitution of Pakistan that took away the power of the President to dismiss the parliament (making judicial review of the act redundant).

except for the ‘outlier’ districts of Bahawalpur and Sukkur. Column 3 and 4 of Table 9 presents these results, without and with the full set of controls, respectively. We find no differential effect of exclusion of outliers on judicial outcomes.³¹

Likewise, since we combine two datasets to obtain shrine density data across all district courts of Pakistan (i.e. from British Colonial Gazettes from Malik and Mirza, 2018 and Auqaf Department, Ministry of Religious Affairs), we examine if the results are dependent on the choice of the datasets. We do a similar exercise as the outlier exclusion test above where we construct a dummy variable that switches on when the data is from Colonial Gazettes and zero if it is from the Auqaf Department. The following equation is estimated:

$$\begin{aligned}
 Y_{cjd\text{t}} = & \zeta + \kappa \text{MilitaryCoups}_{1999_t} \times \text{ShrineDensity}_{1911_d} + \\
 & \varphi \text{MilitaryCoups}_{1999_t} \times \text{ShrineDensity}_{1911_d} \times \text{ColonialGazette}_d + \delta_d + \gamma_t + \\
 & \mathbf{W}'_{cjd\text{t}}\boldsymbol{\varphi} + \varepsilon_{cjd\text{t}}
 \end{aligned} \tag{6}$$

Table 9 (column 5 and 6) presents these results, where we observe no differential effect of being an observation from the colonial gazettes. Therefore, the estimates from Table 8 increases our confidence that the results are not driven by outliers nor the specificity of the chosen shrine dataset. We also examine the robustness of the results to the 17th amendment, exclusion of potential outliers and shrine dataset used for Case Delay and Merit Decisions variables, where we obtain similar results.³²

³¹ Note: $\theta = 0$ and $\kappa > 0$ implies no differential effect of these outlier districts since $dY/d\text{Shrines} = \kappa$. Likewise, we obtain similar results if we exclude the lowest shrine density districts.

³² See Table C.2 in the appendix C for these results.

VII. Concluding Remarks

In this paper, we estimate the impact of religion on the courts. Specifically, we show that the districts that had high historical shrine density, a military coup in 1999 induced a large decline in judicial independence and quality of judicial decisions. We present evidence consistent with the view that increased power of religious leaders following the coup is key in explaining the results: differential shrine density following the coup is only relevant for those districts that implemented a local government system where religious leaders associated with the shrines gained political office. We trace the type of cases driving the results as well as how a judicial selection reform that changed the appointment procedure to select judges mediated the impact of the shrines on the courts. The results are robust to a host of sensitivity tests and alternative explanations. Taken together, the results suggest that religion impacts the courts through changes in institutional structure.

VIII. Tables and Figures

Table 1: Descriptive Statistics of the variables used in the study

Panel A: Case Characteristics					
Variables	Observations	Mean	Std. Dev.	Min	Max
State Wins	7,439	0.50	0.50	0	1
Case Delay	7,439	3.33	2.47	0	23
Merit	7,439	0.62	0.48	0	1
Year Filed	7,439	1999.69	9.53	1970	2016
Year Decision	7,439	2003.03	8.88	1986	2016
Constitutional Cases	7,439	0.72	0.44	0	1
<i>Land Cases</i>	7,439	0.41	0.49	0	1
<i>Human Rights Cases</i>	7,439	0.31	0.46	0	1
Criminal Cases	7,439	0.28	0.44	0	1
Pages of Judgement Order	7,439	8.88	7.71	1	81
Number of Lawyers	7,439	4.04	3.62	1	32
Number of Judges on a case	7,439	1.81	0.84	1	5
Chief Justice in Bench	7,439	0.06	0.24	0	1
Panel B: Judge Characteristics					
Variables	Observations	Mean	Std. Dev.	Min	Max
Tenure at Decision	482	4.10	3.64	8.46	22
Gender	482	0.95	0.19	0	1
PM Assistance Package	482	0.33	0.47	0	1
Promoted to SC	482	0.05	0.23	0	1
Former Judge	482	0.11	0.31	0	1
For. Office Holder Bar. Asso.	482	0.63	0.48	0	1
Former Lawyer	482	0.89	0.31	0	1
After Reform Judge	482	0.14	0.34	0	1
Panel C: Treatment Variables and District Characteristics (by district-year)					
Variables	Observations	Mean	Std. Dev	Min	Max
No. of shrines per 1000 people	496	0.005	0.005	0	0.016
Military Coup	496	0.669	0.470	0	1
Commission Judges/Total	496	0.10	0.21	0	1
Retiring at 62/Total	496	0.12	0.26	0	1
Total Judges in district	496	14.16	5.84	6	30
Area (sq. km)	496	4321.81	3287.76	906	13297
Population	496	2150270	2428460	22454.11	1.14E+07
Density (per sq. km)	496	1094.32	1764.62	8.46	9023.83

Table 2: Impact on State Wins

VARIABLES	(1)	(2)	(3)	(4)
		<i>State Wins</i>		
Shrine Density 1911 X Coup 1999	9.318*** [2.859]	9.693** [3.550]	9.566** [3.450]	9.654** [3.398]
District Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
District Controls	No	Yes	Yes	Yes
Case Controls	No	No	Yes	Yes
Judge Controls	No	No	No	Yes
Observations	7,439	7,439	7,439	7,439
R-squared	0.045	0.045	0.052	0.055
Mean of dependent variable	0.50	0.50	0.50	0.50

Robust standard errors in brackets (clustered at district level)

*** p<0.01, ** p<0.05, * p<0.1

Table 3: Mechanism - Impact on Decentralized Districts

VARIABLES	(1)	(2)	(3)	(4)
		<i>State Wins</i>		
Shrine Density 1911 X Coup 1999 X LG Enforced District	10.98*** [2.863]	10.69** [4.005]	8.086* [4.412]	7.853* [4.456]
Shrine Density 1911 X Coup 1999	-1.603 [2.787]	-1.241 [5.448]	1.297 [5.825]	1.624 [5.916]
District Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
District Controls	No	Yes	Yes	Yes
Case Controls	No	No	Yes	Yes
Judge Controls	No	No	No	Yes
Observations	7,439	7,439	7,439	7,439
R-squared	0.045	0.045	0.052	0.055
Mean of dependent variable	0.50	0.50	0.50	0.50

Robust standard errors in brackets (clustered at district level)

*** p<0.01, ** p<0.05, * p<0.1

Table 4: Mechanism – Impact on State Wins (by the Type of Government)

VARIABLES	Disputes with Local Government	Disputes with Provincial Government	Disputes with Federal Government
	(1)	(2)	(3)
		<i>State Wins</i>	
Shrine Density 1911 X Coup 1999	15.10** [6.045]	10.96 [7.522]	-3.875 [14.97]
District Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
District Controls	Yes	Yes	Yes
Case Controls	Yes	Yes	Yes
Judge Controls	Yes	Yes	Yes
Observations	1,780	1,982	1,602
R-squared	0.068	0.101	0.077
Mean of dependent variable	0.47	0.48	0.45

Robust standard errors in brackets (clustered at district-level)

*** p<0.01, ** p<0.05, * p<0.1

Table 5: Impact on State Wins (by type of Constitutional Cases)

VARIABLES	Land Cases		Human Rights Cases	
	<i>State Wins</i>			
Shrine Density 1911 X Coup 1999	13.49*** [3.485]	17.31*** [4.999]	14.45*** [3.718]	13.72*** [4.243]
District Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
District Controls	No	Yes	No	Yes
Case Controls	No	Yes	No	Yes
Judge Controls	No	Yes	No	Yes
Observations	3,041	3,041	2,323	2,323
R-squared	0.082	0.088	0.051	0.057
Mean of dependent variable	0.47	0.47	0.46	0.46

Robust standard errors in brackets (clustered at district level)

*** p<0.01, ** p<0.05, * p<0.1

Table 6: Placebo on Mechanisms – Impact on Criminal Cases

VARIABLES	(1)	(2)	(3)	(4)
		<i>State Wins</i>		
Shrine Density 1911 X Coup 1999	-2.534 [5.340]	0.0267 [6.169]	-1.722 [5.662]	-1.828 [5.514]
District Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
District Controls	No	Yes	Yes	Yes
Case Controls	No	No	Yes	Yes
Judge Controls	No	No	No	Yes
Observations	2,075	2,075	2,075	2,075
R-squared	0.072	0.072	0.079	0.086
Mean of dependent variable	0.58	0.58	0.58	0.58

Robust standard errors in brackets (clustered at district level)

*** p<0.01, ** p<0.05, * p<0.1

Table 7: Impact on Quality – Case Delay and Decisions on Merit

Panel A: Case Delay				
VARIABLES	<i>Case Delay</i>			
	(1) Overall	(2) Land	(3) Human Rights	(4) Criminal
Shrine Density 1911 X Coup 1999	41.06** [15.47]	42.25* [23.21]	77.95*** [15.17]	20.26 [16.72]
District Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
District Controls	Yes	Yes	Yes	Yes
Case Controls	Yes	Yes	Yes	Yes
Judge Controls	Yes	Yes	Yes	Yes
Observations	7,439	3,041	2,323	2,075
R-squared	0.086	0.144	0.141	0.088
Mean of dependent variable	3.33	3.33	3.28	3.40
Panel B: Decisions on Merit				
VARIABLES	<i>Decisions on Merit</i>			
	(1) Overall	(2) Land	(3) Human Rights	(4) Criminal
Shrine Density 1911 X Coup 1999	-12.28*** [1.485]	-20.63*** [4.169]	-13.35** [4.655]	6.958 [7.809]
District Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
District Controls	Yes	Yes	Yes	Yes
Case Controls	Yes	Yes	Yes	Yes
Judge Controls	Yes	Yes	Yes	Yes
Observations	7,439	3,041	2,323	2,075
R-squared	0.086	0.134	0.078	0.164
Mean of dependent variable	0.62	0.60	0.61	0.67

Robust standard errors in brackets (clustered at district level)

*** p<0.01, ** p<0.05, * p<0.1

Table 8: Judicial Selection Reform and Shrines

Panel A: OLS Results			
VARIABLES	<i>Least Squares Estimation</i>		
	State Wins	Case Delay	Merit
Shrine Density 1911 X Commission Appointed/Total Judges	-26.02*** [5.738]	-122.3*** [33.45]	33.24*** [5.464]
D1999_2007 X Shrine Density 1911	6.960* [3.512]	49.08*** [13.13]	-7.769*** [1.042]
D2008_2010 X Shrine Density 1911	8.737 [6.433]	-16.83 [30.03]	-8.261 [4.839]
Dpost_2010 X Shrine Density 1911	21.79*** [5.952]	68.70*** [21.07]	-31.36*** [2.584]
District Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
District, Case, Judge Controls	Yes	Yes	Yes
Observations	7,439	7,439	7,439
R-squared	0.057	0.089	0.091
Mean of dependent variable	0.50	0.50	0.50
Panel B: Instrumental Variable Results			
VARIABLES	<i>Instrumental Variable, 2nd Stage</i>		
	State Wins	Case Delay	Merit
Shrine Density 1911 X Commission Appointed/Total Judges	-29.95*** [9.688]	-145.1*** [33.71]	34.42*** [7.691]
D1999_2007 X Shrine Density 1911	6.854** [3.296]	48.47*** [12.72]	-7.737*** [0.988]
D2008_2010 X Shrine Density 1911	8.549 [6.090]	-17.92 [29.15]	-8.204* [4.575]
Dpost_2010 X Shrine Density 1911	22.93*** [6.934]	75.29*** [20.71]	-31.70*** [2.751]
District Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
District, Case, Judge Controls	Yes	Yes	Yes
Observations	7,439	7,439	7,439
R-squared	0.057	0.089	0.091
Mean of dependent variable	0.50	0.50	0.50

Note: Shrine Density 1911 X Commission Appointed / Total is instrumented by Shrine Density 1911 X Fraction Reaching 62 following the reform. Robust standard errors in brackets (clustered at district level). *** p<0.01, ** p<0.05, * p<0.1

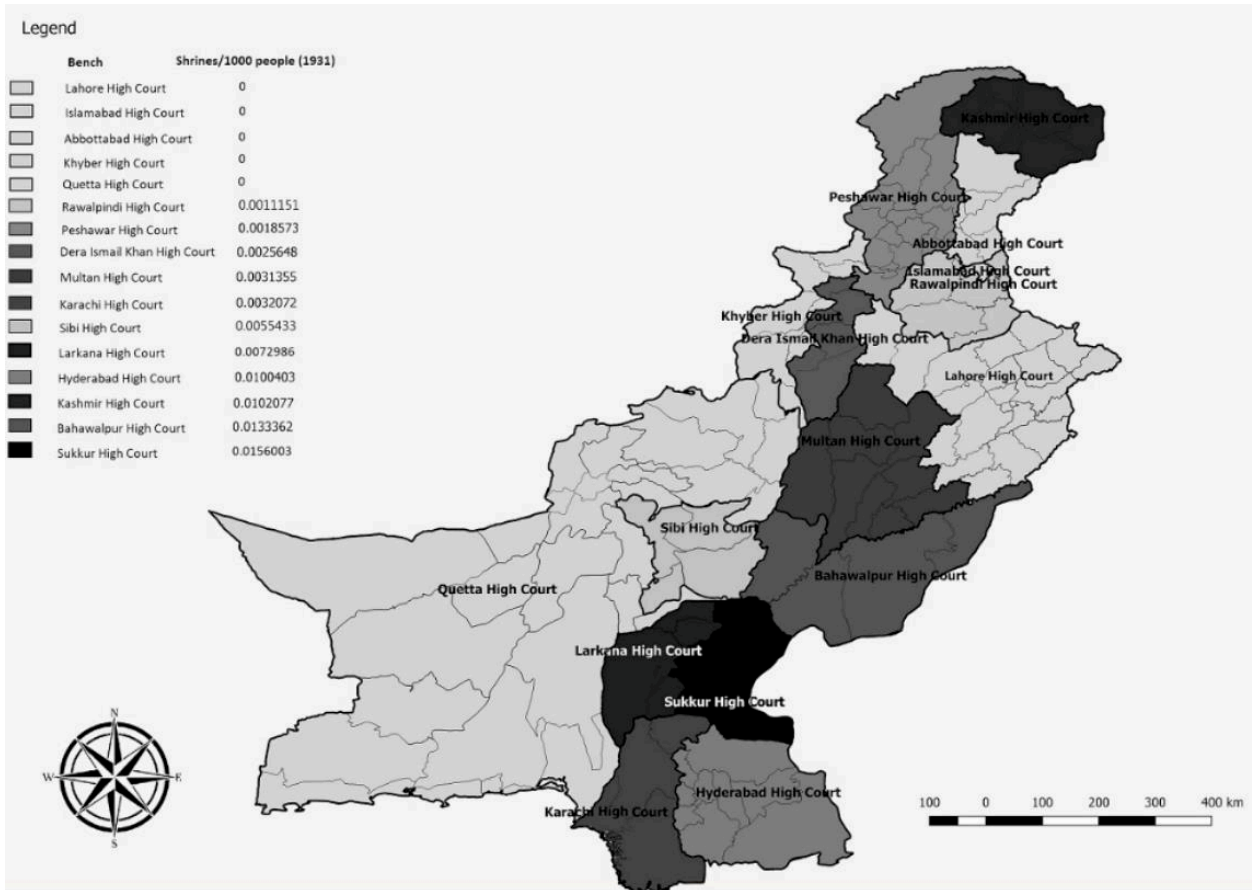
Table 9: Robustness - Alternative Reform, Outliers and Dataset

VARIABLES	17 th Amendment		Outliers Excluded <i>State Wins</i>		Colonial Gazette Data	
	(1)	(2)	(3)	(4)	(5)	(6)
Shrine Density 1911 X Coup 1999	9.160*** [2.931]	9.585** [3.345]	9.448** [3.319]	9.712** [3.654]	7.315** [3.204]	8.749** [3.764]
Shrine Density 1911 X 17 th Amendment	0.436 [2.998]	0.196 [2.742]				
Shrine Density 1911 X Outliers Excluded X Coup 1999			-0.749 [3.288]	-0.649 [3.536]		
Shrine Density 1911 X Colonial Gazette					2.075 [3.239]	0.898 [3.088]
District Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
District Controls	No	Yes	No	Yes	No	Yes
Case Controls	No	Yes	No	Yes	No	Yes
Judge Controls	No	Yes	No	Yes	No	Yes
Observations	7,439	7,439	7,439	7,439	7,439	7,439
R-squared	0.045	0.055	0.045	0.055	0.045	0.055
Mean of dependent variable	0.50	0.50	0.50	0.50	0.50	0.50

Robust standard errors in brackets (clustered at district level)

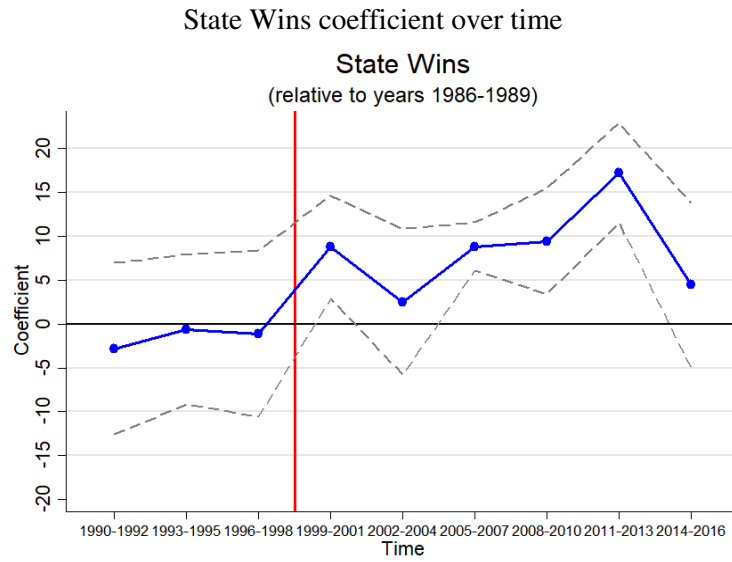
*** p<0.01, ** p<0.05, * p<0.1

Figure 1: Map of Shrine Density in Judicial Districts of Pakistan



Note: The shrine data covers all of Pakistan where shrine density is computed by total number of shrines in the jurisdiction divided by the population.

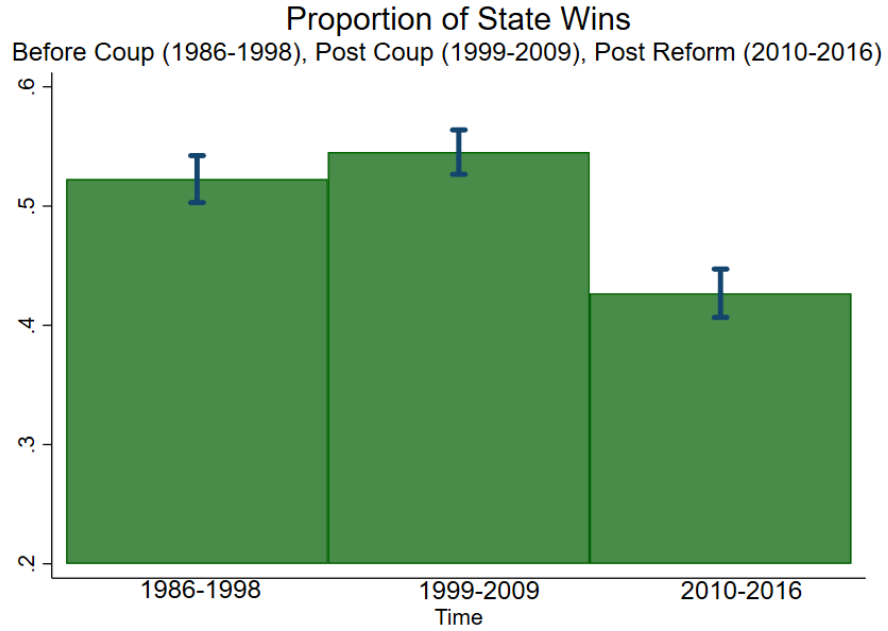
Figure 2: Time varying impact of military coup (90% CI)



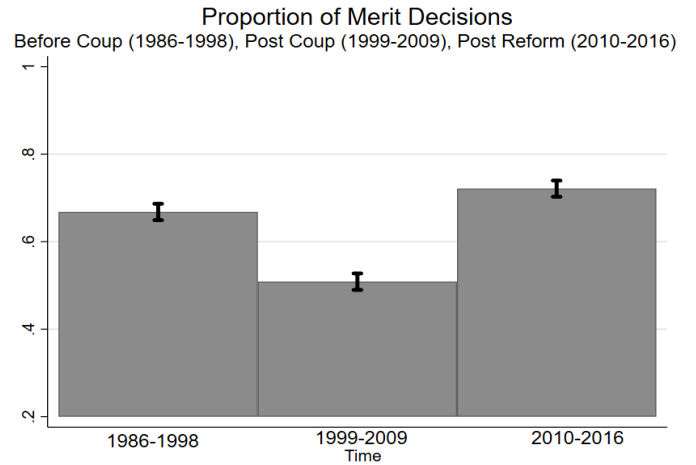
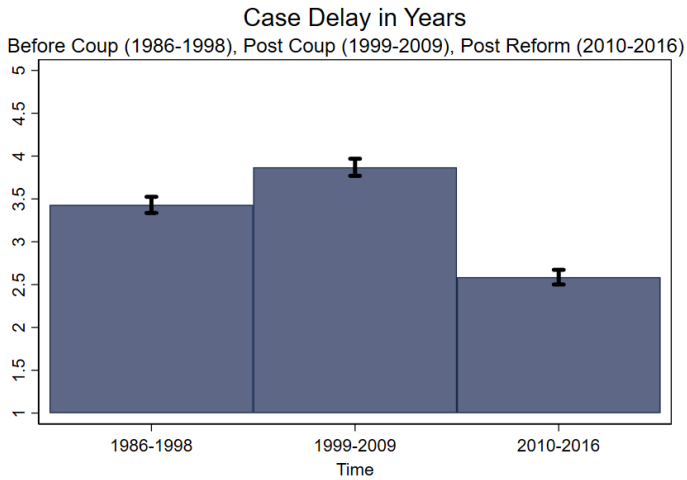
Note: The Figure presents coefficients and the corresponding 90% confidence intervals in the regressions of State Wins on 2-year interval dummies interacted with shrine density in the district together with case, judge and district controls as well as district and year fixed effects. Cross-sections between 1986 to 1989 are held as the comparison group. The vertical line marks that timing of the military coup that occurred in 1999.

Figure 3: Judicial Outcomes in three periods

Panel A: State Wins in three time periods



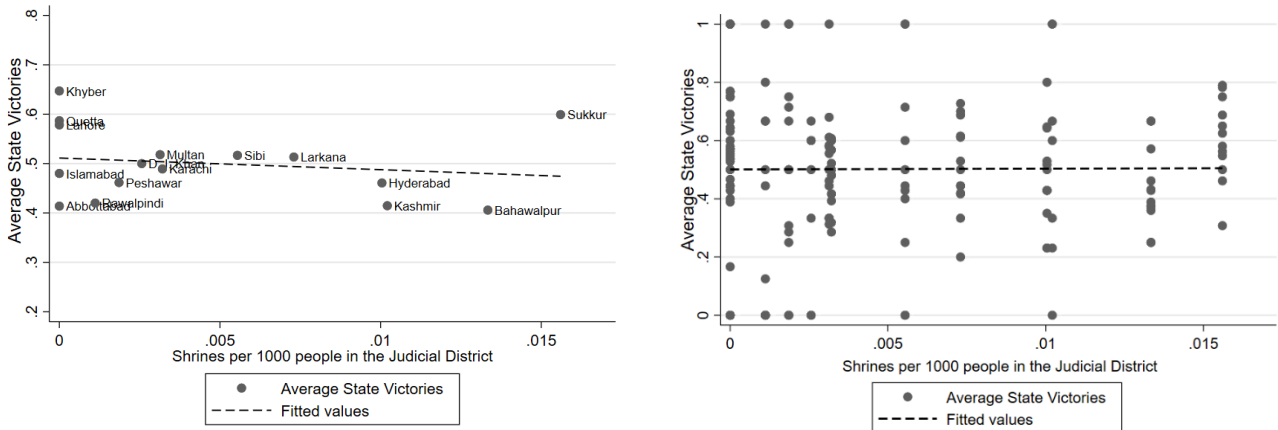
Panel B: Case Delay and Merit Decisions in three time periods



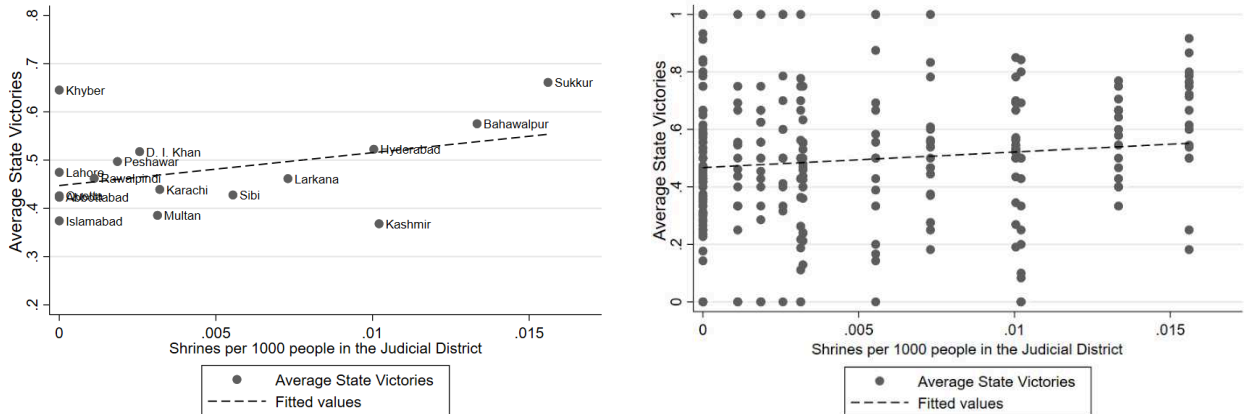
Note: We divide average State Wins, Case Delay and Merit Decisions in three time periods. One is during the democratic or pre-coup period (1986-1998), one is the post-coup and before reform period (1999-2009) and one following the judicial selection reform. Averages across the three time periods along with 95% confidence interval are presented.

Figure 4: State Wins by District Average

Panel A: Shrines and Average State Wins Before Coup (1986-1998)



Panel B: Shrines and Average State Wins After Coup (1999-2016)



Note: The figures on the left averages State Wins by the district regardless of the year, whereas, the figures on the right provides an average of each district for a given year. Similar plots for Case Delay and Merit Decisions can be found in Appendix C (Figure C.10 an C.11, for case delay and Merit, respectively).

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Online Appendix to:

The Dictator, the Imam and the Judge: Tracing the Impact of Religion on
the Courts

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 = Average State Victories in a district for a given year. The law firms coded this variable based on the following rubric: it takes the value of 1 in case of a “state victory and zero in case of a state loss”.

Shrine Density = This is number of shrines per 1000 people in British Colonial Gazettes of 1911 and number of Shrines in Auqaf Department records in 1952. The variable is constructed from the following simple operation:
$$\text{Shrine Density} = \frac{\text{Number of Shrines in the Judicial District}}{\text{Total Population in the Judicial District}} \times 1000.$$

Case Lag = It is the difference between case decision year and case filing year. This variable is also based on text of the judgement orders.

Merit Case = It is a dummy variable that takes the value of 1 if the case is decided on based on “evidence rather than technical or procedural grounds” (Pound, 1963). This is based on the assessments of the law firms based on reading the text of the judgement order.

Judicial Commission / Total Judges = It is the fraction of judges selected under the new selection procedure. Information on the new appointments is obtained from judicial administrative records obtained from Registrar Offices of the high courts. Data on total judges in each district high court is obtained from High Courts Annual Reports submitted to the Ministry of Law, Justice and Human Rights, Government of Pakistan.

Retiring at 62 / Total Judges (instrument) = It is the fraction of judges who reach the mandatory retirement age of 62 (in the post reform period). Information on judge retirements is obtained from judicial administrative records obtained from Registrar Offices of the high courts. Data on total judges in each district high court is obtained from High Courts Annual Reports submitted to the Ministry of Law, Justice and Human Rights, Government of Pakistan.

Constitutional Case = It is a dummy variable that takes the value of 1 if it is a constitutional case and zero otherwise. In the main specification is averaged across-district and over time. This is indicated on the text of the judgement order.

Land Case = It is a subset of constitutional cases, it is a dummy variable that takes the value of 1 if it is a case involving land ownership or expropriation dispute with “*The State*” and 0 otherwise. Often it is Ministry of Defense, housing authority or most commonly a “development” agency, which is authorized to resolve disputes regarding land ownership (Defense Ministry, Defense Housing Authority, Lahore Development Authority (LDA), Karachi Development Authority (KDA), Peshawar Development Authority (PDA), Capital Development Authority (CDA)).

Human Rights Case = It is a subset of constitutional cases, it is a dummy variable that takes the value of 1 if it does not involve a case involving land ownership or expropriation dispute with “*The State*” and 0 otherwise. These cases are marked as “writ petitions” in the text of judgment order.

Criminal Case = It is a dummy variable that takes the value of 1 if it is a criminal case and zero otherwise. In the main specification is averaged across-district and over time. This is indicated on the text of the judgement order.

Number of Lawyers = It is based on a count variable documenting the number of lawyers arguing in the particular case. This is also indicated on the text of the judgement order.

Number of Judges = It is based on a count variable documenting the number of judges adjudicating upon the particular case. This is also indicated on the text of the judgement order.

Bench Chief Justice = It is dummy variable that takes the value of 1 if the chief justice or senior most judge was adjudicating in the case and zero otherwise. In the main specification is averaged across-district and over time.

Number of Pages of Judgment Orders = It is a count variable documenting number of pages of the judgement order issues in the particular case. This is also indicated on the text of the judgement order.

Age at appointment = It is 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 = It is a dummy variable that takes the value of 1 if it is a male judge and 0 if it is a female judge. It is coded in two ways: 1) Manually, where the author checks every judge name, the dummy variable takes the value of 1 if it is male and zero if female. 2) Automatically, where the author asks Stata to read the string starting with “Justice Miss” and “Justice Mrs.” as zero and the string started by “Justice Mr.” as one. The two methods yield identical number of males and female justices.

PM Assistance Package = It is a dummy variable for the judge who received a (residential) plot as part of the PM Assistance Package and zero otherwise. This is obtained from the list of names available in Public Accounts Committee report “List of judges allotted plots since 1996”.

Promoted to SC = It is a dummy variable for the judge who was elevated to the supreme court bench and zero otherwise. This is obtained from judicial administrative records of the Supreme Court Registrar Office.

Former Lawyer = It is a dummy variable for the judge who was formerly a lawyer before being appointed as a justice of the high court. Data for this obtained through a combination of biographical information contained in annual reports, bar council records and judicial administrative data.

Former Office Holder Bar Association = It is a dummy variable for the judge who was formerly an office holder of the lawyers’ bar association (before being appointed as a justice of the high court). Data for this obtained through a combination of biographical information contained in annual reports, bar council records and judicial administrative data.

Former Judge = It is a dummy variable for the judge who was formerly a lower court (civil or session court) judge. Data for this obtained through a combination of biographical information contained in annual reports and judicial administrative data.

Total Judges = It is a district-time count variable that tells us the number of judges at a district high court in a given time period. Data for this obtained through a combination of information contained in annual reports and judicial administrative data.

Area = It is the area (in square kilometres) of the district where the high court is located. This is obtained from census of Pakistan.

Population = It is the population of the district where the high court is located. This is obtained from a linear interpolation of 1981, 1998 and 2017 census of Pakistan.

Density = It is the per square kilometre population density of the district where the high court is located (area/population). This is obtained from a linear interpolation of 1981, 1998 and 2017 census of Pakistan.

B. Data Appendix: Additional information and data collection

B.1 History and Structure of Courts in Pakistan

In this subsection we discuss background and structure of courts in Pakistan. The Indian High Courts Act of 1861 authorized the Crown to create the high courts in the Indian colony. These courts served as precursors to the modern-day high courts of 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 the overarching institutional structure such as the common law jurisprudence.

Pakistan's judiciary is composed of a three-tier hierarchical structure. The lowest courts are the civil and session courts where the civil courts hear civil cases and session courts adjudicate upon the criminal cases. These courts are located in the provincial capitals and have jurisdictions dictated by domicile of the litigating parties. Decisions in civil and session courts can be challenged in the high courts of Pakistan. If the government expropriates land or violates any fundamental right, the high court is the first (and in most cases) the only platform for the citizens and firms for remediation. Although, in theory there are only four provincial high courts in Pakistan, but the benches of each provincial high court are spread within the 4 provinces of Pakistan (see Figure 1). This is in the form of 16 district high court benches (about 4 district benches in each of the 4 provinces). Most important for our paper is the fact that in the high court, one can also file a case against the government. This takes the form of a constitutional petitions against The State or Criminal Petition against the State. Constitutional cases involving The State as a party involve cases filed against the federal government, provincial governments and 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. It typically hears appeals on “technical” ground for the criminal and constitutional cases from the high courts. The Supreme Court can have at most 16 judges which greatly limits the number and scope of cases it can hear. Therefore, only a small fraction of cases ends up being heard by the Supreme Court (Arshad, 2017).

B.2 Case Data Sources and Construction

The case characteristics is obtained from central repository of cases used by lawyers to prepare their cases. This is available online at Pakistan Law Site (<https://www.pakistanlawsite.com/>). This website is the “Central Library” used by lawyers to prepare their cases (since Pakistan is a common law system where case precedent is crucial) as well as paralegals and students studying for their law exams. Access for this is password protected where permission to use the website and cases is gained through a law firm. Two teams of paralegals supervised by a senior lawyer each record key information related to the cases in the texts of the judgement order available at the website. Table C.1 presents averages for case characteristics coded by the two teams as well as correlation coefficient between them.

Since, the Pakistan Law Site library contains the whole universe of (undigitized) cases decided from 1950 to 2017, we had to choose a sample period given our budget and research question. We randomly sample all the available cases for every year depending on the total universe of cases decided in that year from 1986 to 2016 inclusive. As number of cases decided in a year gradually rises, so does the fraction of sampled cases in our sample. Figure C.3 presents this information as plot of total cases sampled with total available cases.

B.3 Shrine Data Sources and Construction

The key source for the shrine data is the British Colonial Gazettes. The publication in the gazettes was a legal necessity that allowed documents to come into force and enter the public realm. Essentially, these were official legal and public bulletins of the British Government for its Indian Colony. Information on the shrines was published a regular section on “fairs and festivals”. This section contained the names and associated festivals of all shrines in the district. These shrines are counted for each district and forms the basis for the shrine dataset. Likewise, since British directly ruled in two of the provinces in present day Pakistan (Punjab and Sindh), this source only contains data on these provinces. Therefore, this Gazette data for shrines is complemented by data from Auqaf Department in the Ministry of Religious Affairs (Auqaf department is responsible for overseeing religious charities and donations within the ministry of religious affairs). Auqaf Department records all shrines with their location, which we use to construct the shrine dataset. Specifically, Auqaf department overseen by the provincial government is responsible for administration of “Waqf properties” (literally, devote indefinitely) that is an “inalienable charitable trust” (Bazzi et al, 2018, p. 1). These properties include shrines, mosques and other religious institutions that such as *Madrassas* (religious seminaries). Important thing to note is that the *Waqf* properties cannot be bought or sold where in the case of shrines, the shrine family can hold it infinitum. We combine both these data sources to obtain shrine density for every judicial district of Pakistan.³³

³³For further information on the shrine dataset, see Malik and Mirza (2018).

C. Additional Tables and Figures

Table C.1: Outcome Variables and Case Characteristics

Comparison of Team 1 and Team 2

Variables	Team 1	Team 2	Difference	Correlation (ρ)
State Wins	0.50	0.56	-0.06	0.89
Case Delay	3.33	3.30	-0.03	0.99
Merit	0.62	0.67	0.05	0.88
Constitutional	0.72	0.70	-0.01	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	0.93
# of Lawyers	4.04	4.09	-0.05	0.94
# of Judges	1.81	1.83	-0.02	0.87
CJ in Bench	0.06	0.08	-0.02	0.83
Pg. of Judgement	8.88	8.71	0.03	0.97

Note: The table compares the outcome variables and case characteristics for the two teams of coders for the same 7439 cases used in the analysis. Team 1 is the data used in the analysis. Means, their difference, and correlation coefficient between the two groups are presented.

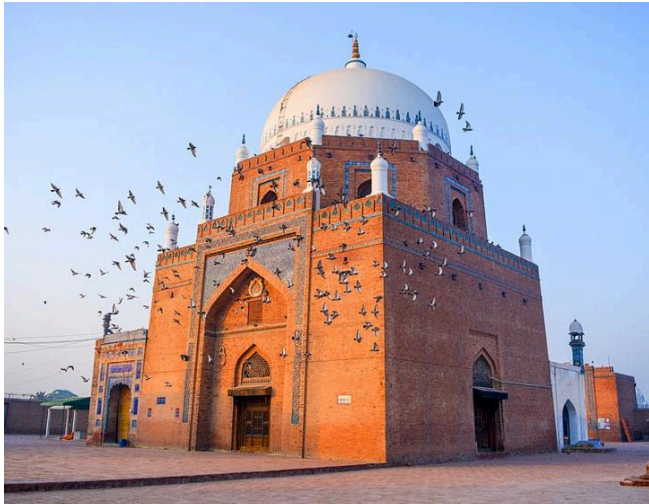
Table C.2: Case Lag and Merit - Robustness - Alternative Reform, Outliers and Dataset

VARIABLES	17 th Amendment		Outliers Excluded		Colonial Gazette Data	
	<i>Case Delay</i>	<i>Merit</i>	<i>Case Delay</i>	<i>Merit</i>	<i>Case Delay</i>	<i>Merit</i>
Shrine Density 1911 X Coup 1999	49.42** [20.49]	-11.49*** [2.593]	57.12*** [17.30]	-10.56*** [1.383]	73.93* [39.31]	-10.72** [4.510]
Shrine Density 1911 X 17 th Amendment	20.51 [32.46]	5.246* [2.951]				
Shrine Density 1911 X Outliers Excluded X Coup 1999			-1.496 [24.64]	5.646 [4.702]		
Shrine Density 1911 X Colonial Gazette					-17.67 [26.50]	1.173 [4.798]
District Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
District Controls	Yes	Yes	Yes	Yes	Yes	Yes
Case Controls	Yes	Yes	Yes	Yes	Yes	Yes
Judge Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,439	7,439	7,439	7,439	7,439	7,439
R-squared	0.082	0.080	0.082	0.080	0.082	0.079

Robust standard errors in brackets (clustered at district level)

*** p<0.01, ** p<0.05, * p<0.1

Figure C.1: The Shrine of Bahauddin Zakariya (left) with Trusty of the Shrine (right)



Note: The person in white turban “giving blessings” to the child on the right is a prominent shrine elite and current foreign minister of Pakistan.

Figure C.2: Chief Justices of Pakistan at Shrines with Religious Leaders

Panel A: Chief Justice of Pakistan *Saqib Nisar* at Data Darbar Shrine in Punjab (tenure of CJ from February 2010-January 2019) with shrine elites



Panel B: Former Chief Justice of Pakistan *Iftikhar Chaudhary* (tenure 2005-2013) at Shrine of Hazrat Sachal Sharif in Sindh with shrine elites



Figure C.3: Total vs Sampled Cases

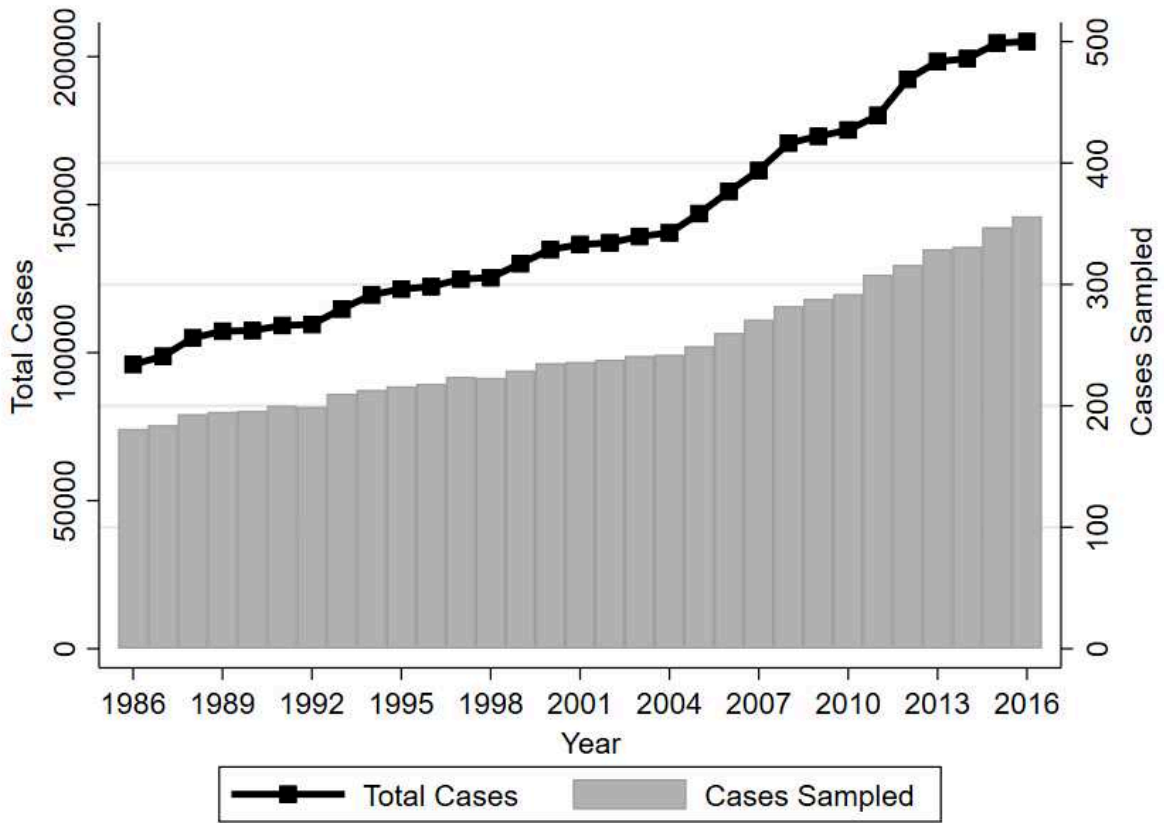


Figure C.4: British District Gazetteers



Figure C.5: Example of Land (Land Grab Case)

2005 C L C 745

[Karachi]

Before Sabihuddin Ahmed and Khilji Arif Hussain, JJ

KHALID MOHSIN---Petitioner

versus

SECRETARY, MINISTRY OF DEFENCE, Government of Pakistan, Islamabad and 2 others---Respondents

Constitutional Petition No.59 of 1988, decided on 23rd November, 2004.

West Pakistan Land Revenue Act (XVII of 1967)---

---S. 45---Constitution of Pakistan (1973), Art.199---Constitutional petition---Maintainability---Disputed question of fact---Mutation, a document of title ---Petitioner claimed to be owner of the land which had been mutated in favour of his predecessor-in-interest on the basis of allotment by Settlement authorities---Question for determination was whether the petitioner had acquired any title to the land so as to entitle him to the relief of possession or whether he had locus standi to seek declaration in respect of existence or otherwise of requisition---Validity---No allotment order by a competent officer in favour of the predecessor-in-interest of the petitioner was available on record---Petitioner had relied upon an order of Assistant Rehabilitation Mukhtiarkar directing that the land be mutated in the name of the predecessor-in-interest of the petitioner---Nothing was available to show that the predecessor-in-interest of the petitioner had applied for allotment of evacuee land against units available with him---Predecessor-in-interest of the petitioner was entitled to 159 units but was allotted land comprising of 453 units---Mutation entries did not confer title but could at the best be considered evidence of title which was rebuttable---Such disputed questions requiring detailed scrutiny of facts and production of evidence could not be undertaken in the proceedings under Art.199 of the Constitution-- Petition was dismissed in circumstances.

Hassan Akbar for Petitioner.

Nadeem Azhar Siddiqui, D.A.-G. and S. Tariq Ali, Federal Counsel for Respondents.

Ahmed Pirzada, Addl. A.-G.

Date of hearing: 29th August, 2004.

Figure C.6: Example of Land Case (Payment on land not made by government)

2009 C L C 1199

[Karachi]

Before Gulzar Ahmed and Malik Muhammad Aqil, JJ

MORRIS TANVIR---Petitioner

Versus

FEDERATION OF PAKISTAN through Secretary Ministry of Defence, Islamabad and 2 others-Respondents

Constitutional Petition No.D-2331 of 2006, decided on 16th July, 2009.

Transfer of Property Act (IV of 1882)---

---S. 54---Cantonment Land Administration Rules, 1937, Rr.15 & 16---Constitution of Pakistan (1973) Art 199---Constitutional petition---Amenity plot---Allotment---Plot in question was amenity plot reserved for school and petitioner contended that he purchased the same from a foreigner. Grievance of petitioner was that authorities were not issuing lease in his favour. Validity--- Sale of land for any such purpose without definite order of Central Government was prohibited under R.16 of Cantonment Land Administration Rules, 1937. Lease could be granted under R.16 of Cantonment Land Administration Rules, 1937, for maximum period of 30 years and maximum period of 90 years through public auction for building sites---Plots meant for municipal services and amenities could not be leased, sub-leased, sold or transferred to any person or to any one class of persons for the use and enjoyment rather it had to remain as government property for public purposes for unhindered free access, use and enjoyment of public at large---In no case plots meant for municipal services and amenities could be treated as building sites nor there could be any exceptional case or exceptional reasons for disposal of such land by private agreement or otherwise---There was no document of sale and purchase of plot in question between Defence Housing Authority and the foreigner in accordance with S.54 of Transfer of Property Act, 1882---Agreement to sell relied upon by petitioner was no agreement in the eyes of law as no authority had been shown nor any seemed to exist to sign the same on behalf of the foreigner---Transfer documents relied upon by petitioner were, therefore, of no consequence being themselves illegal and without authority of law and also seemed to High Court, without consideration---It seemed to High Court that fraud had been played by petitioner and officials of Defence Housing Authority for usurping and misappropriating valuable government property and there had to be an accountability of the same---High Court directed Federal Government to hold inquiry in the whole affair and to take action against all officials and persons found responsible in usurping and misappropriating valuable government property, and also to initiate criminal proceedings against such persons---High Court declined to issue any direction in favour of petitioner to authorities for issuance of lease deed---
Petition was dismissed accordingly.

Ardeskar Cowasjee and 10 others v. Karachi Building Control Authority (KMC), Karachi and 4 others 1999 SCMR 2883; Four Square Enterprises v. Karachi Building Control Authority PLD 2000 Kar. 161; Dr. Zahar Ansari and others v. Karachi Development Authority and others PLD 2000 Karachi 168 and Shafiqur Rehman and others v. Government of Sindh and others PLD 2006 Kar. 10 ref.

S.A. Jalib Chaudhry for Petitioner.

Ashiq Raza, Dy. A.-G. along with Hamid Niaz, Dy. Military Estate Officer for Respondents Nos.1 and 3.

Raja Sakandar Khan Yasar for Respondent No.2.

Date of hearing: 7th May, 2009.

Figure C.7: Example of HR case: Discrimination based on Gender

2005 Y L R 2063

[Quetta]

Before Raja Fayyaz Ahmed, C.J. and Akhtar Zaman Malghani, J

Miss MEHAK HASNAIN---Petitioner

Versus

SELECTION COMMITTEE and others---Respondents

C.P.No.159 of 2004, decided on 13th April, 2005.

Constitution of Pakistan (1973)---

---Art. 199---Constitutional petition---Educational Institution---Admission in 1st year M. B. B. S. Course---Petitioner submitted application form for admission in 1st year M. B. B. S. Course of Medical College concerned against one of reserved seats on District merit as well as for the seat reserved for doctors' children---Respondent whose father was working as Associate Professor in the college concerned and her mother as Doctor in Health Department, submitted three application forms for admission in 1st Year M. B. B. S. Course against Seats of District Merit, and categories "A" & "B" of Doctors' children sent---Respondent who could not succeed to get admission on District Merit, her forms were considered for categories "A" & "B" of doctors' children seat as in both categories she was at serial No.1 and granted her admission on seat allocated for category "A"---Respondent while submitting three application forms gave conflicting choices---In the form first in time, her first preference was on District Merit Seat and second preference was for category 'B'---Respondent having failed to get seat of District Merit, her second preference was for category "B", but Selection Committee, instead of granting admission to her on her choice which was category 'B', granted admission to her against category "A" which could not have been done by Selection Committee being contrary to object and option exercised by candidate and also admission policy---Choice of preferences once exercised by a candidate in his application form, would always, to all intents and purposes for admission on merits in the light of admission Policy, would be final---Selection Committee had acted arbitrarily and in excess of its authority by abusing its powers in granting admission to respondent in category 'A' instead of category 'B' which had resulted in depriving petitioner to get admission in 1st year M.B.B.S. of the college---Impugned decision of Selection Committee was illegal, void and without any lawful authority---Authorities were directed to grant admission to petitioner on the seat reserved for category 'A' in M.B., B. S. Course.

Syed Ayaz Zahoor for Petitioner.

Salahuddin Mengal, A.G. and Zahid Malik for Respondent No.3.

Date of hearing: 29th March, 2005.

Figure C.8: Freedom of Movement Limited

P L D 2007 Quetta 41

Before Amanullah Khan Yasinzai, C.J. and Akhtar Zaman Malghani, J

MIR KHALID LANGOV---Petitioner

Versus

SECRETARY, MINISTRY OF INTERIOR, GOVERNMENT OF PAKISTAN, ISLAMABAD---Respondent

Constitutional Petition No.479 of 2006, decided on 27th November, 2006.

Exit from Pakistan (Control) Ordinance (XLVI of 1981)---

---Ss. 2 & 3---Constitution of Pakistan (1973), Art.199---Constitutional petition---Placing name on Exit Control List---Petitioner had challenged in constitutional petition validity of notification whereby his name was placed on Exit Control List---No reasons, whatsoever had been mentioned in the said notification---Validity---Under subsection (3) of S.2 of Exit from Pakistan (Control) Ordinance, 1981, it was not necessary for Federal Government to specify grounds, if it would not be in the public interest, but as liberty of a citizen guaranteed by the Constitution was being curtailed, Federal Government was under legal obligation to have at least shown the basis of its intention of exercising power under subsection (3) of S.2 of Exit from Pakistan (Control) Ordinance, 1981---Federal Government on demand of court was legally bound to place on record the material which prompted it to place name on Exit Control List in order to satisfy conscience of the court and to show that such exercise was not colourable one---Contention of Standing Counsel of Federal Government regarding non-maintainability of constitutional petition without availing remedy of review available under S. of Exit from Pakistan (Control) Ordinance, 1981, was repelled, in view of the fact that reasons for placing name of petitioner on Exit Control List had neither been communicated to petitioner nor were disclosed in the impugned notification; which was necessary to enable the petitioner to avail remedy of review and in absence thereof, it was neither appropriate nor possible for petitioner to have availed such remedy of review---Constitutional petition, was maintainable, in circumstances---Impugned Notification whereby name of petitioner had been placed on Exit Control List, was declared to be bad in law, invalid and without lawful authority and same was struck down by the High Court.

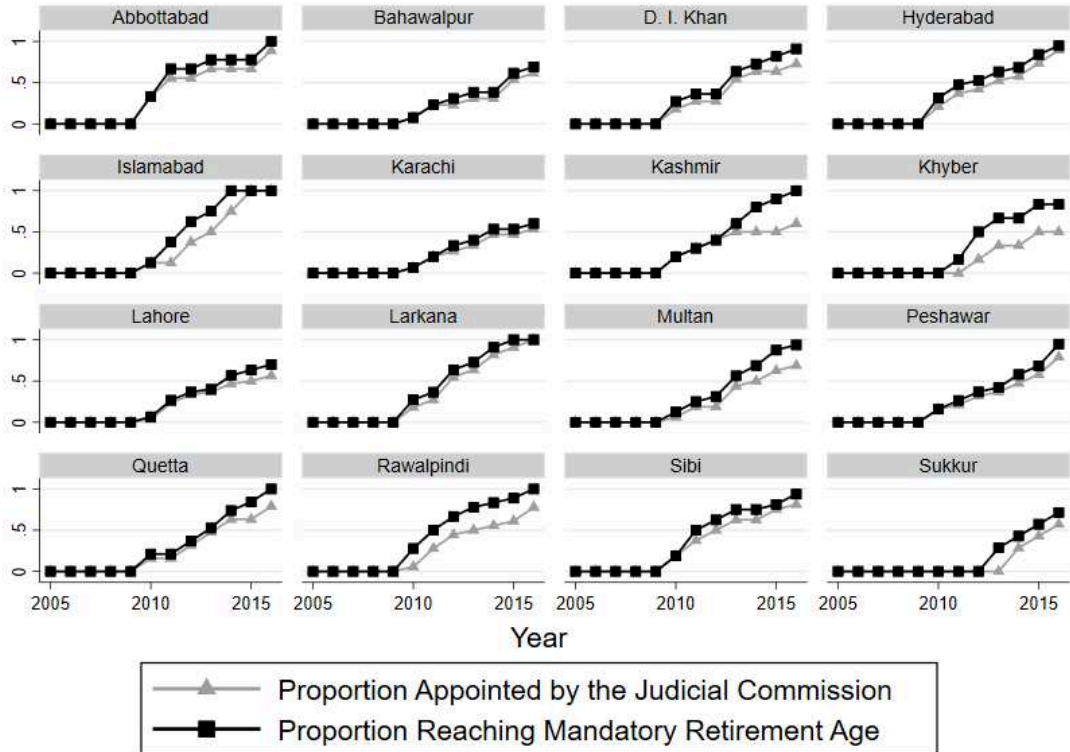
PLD 2003 Kar. 705; PLD 1997 Lah. 61; 2003 CLC 246 and PLD 1999 Lah. 459 ref.

Muhammad Wasey Tareen for Petitioner.

Mumtaz Yousaf, Standing Counsel for Respondent.

Date of hearing: 10th October, 2006.

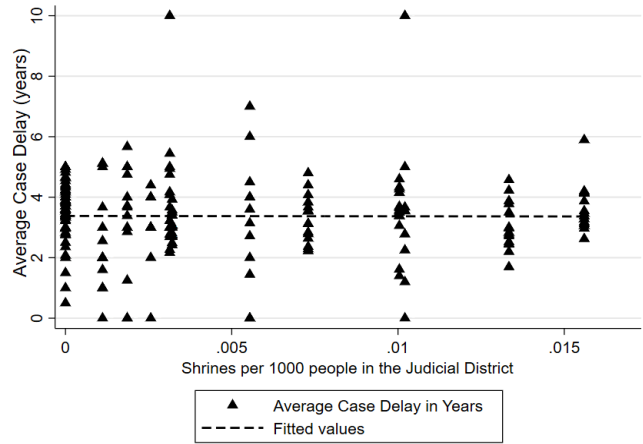
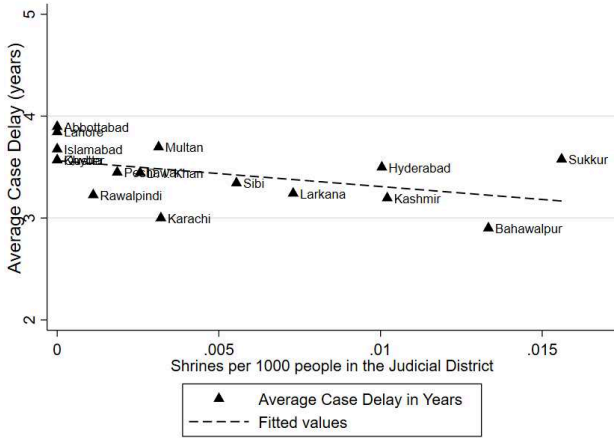
Figure C.9: District-Time Evolution of Judges Appointments and Retirements under the New selection mechanism



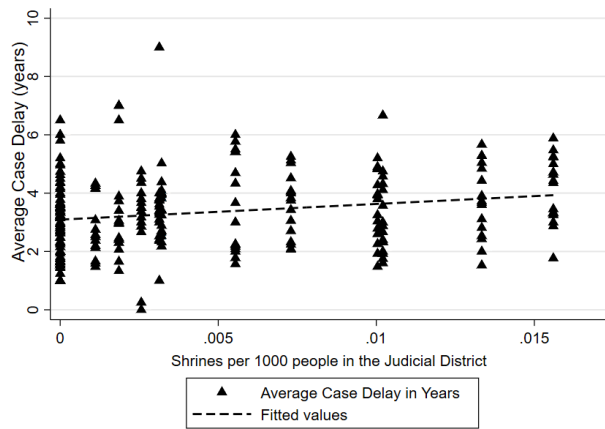
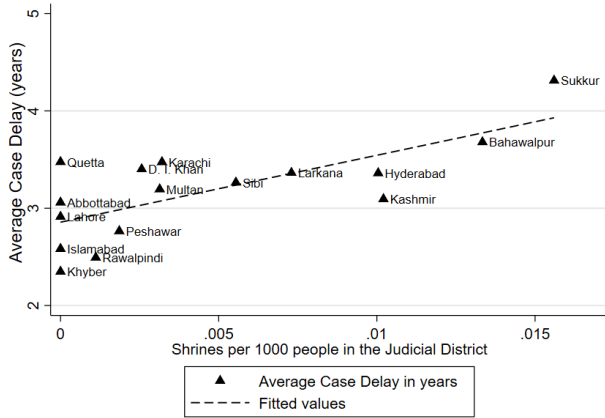
Note: The figure plots fraction of judges appointed by the judicial commission in each district over time along with mandatory retirements in the post-reform period. We notice that retirements are highly correlated with new appointments. Nevertheless, there is imperfect replacements where retirements do not always coincide with new appointments.

Figure C.10: Case Delay by District Average

Panel A: Shrines and Average Case Delay Before Coup (1986-1998)



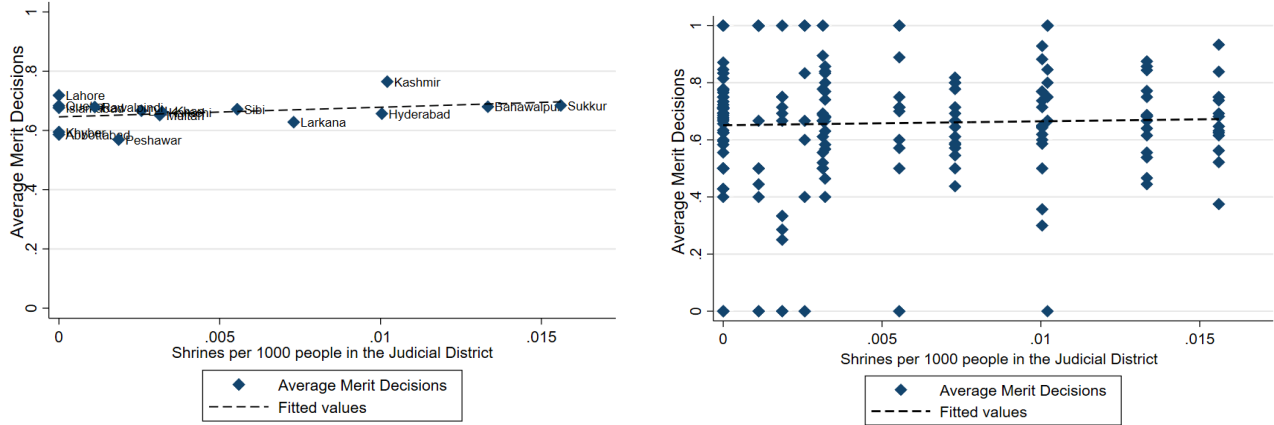
Panel B: Shrines and Average Case Delay After Coup (1999-2016)



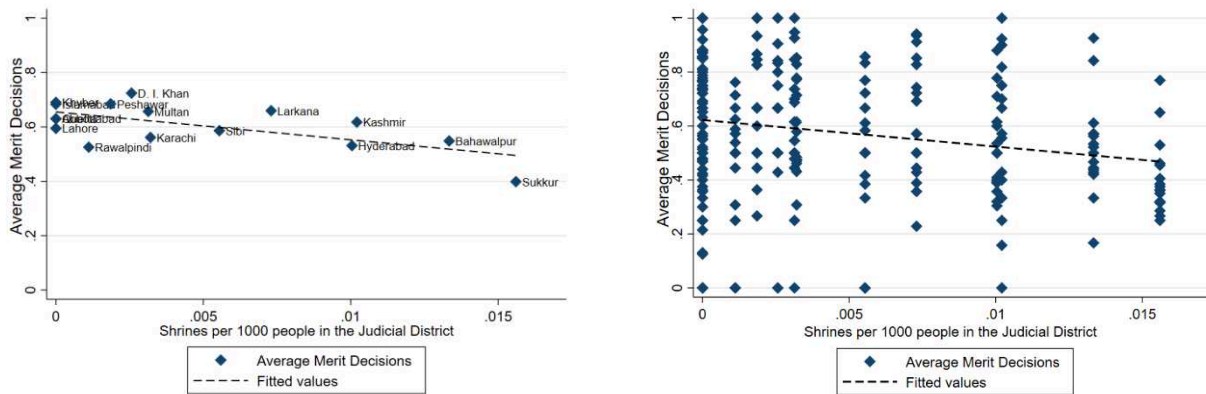
Note: The figures on the left averages Case Delay by the district regardless of the year, whereas, the figures on the right provides an average of each district for a given year.

Figure C.11: Merit Decisions by District Average

Panel A: Shrines and Average Merit Decisions Before Coup (1986-1998)



Panel B: Shrines and Average Merit Decisions After Coup (1999-2016)



Note: The figures on the left averages Merit decisions by the district regardless of the year, whereas, the figures on the right provides an average of each district for a given year.

Chapter 3: The Political Economy of Foreign Aid and Growth: Theory and Evidence*

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Abstract

In this paper, we demonstrate that political incentives are key to understanding the effectiveness of foreign aid. We present a theory that formalizes how foreign aid impacts growth, leader turnover and public employment. We show that when political institutions are weak, foreign aid is misallocated. Nevertheless, foreign aid may also increase the dynamic efficiency of public policies, making the overall effect on aid on growth ambiguous. We present evidence consistent with the predictions of the model where foreign aid decreases leader turnover, increases misallocation - by increasing public employment - while it has a positive and significant effect on economic growth. The identification strategy we propose allows us to provide causal evidence for the predictions of the model.

JEL: F35, D72, O1

Keywords: Foreign aid, Economic growth, Political Economy, Redistribution.

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1 Introduction

Foreign aid is one of the most important policy tools that rich countries use to fight global poverty. Most studies examining the effect of foreign aid on economic growth find positive effects (see for instance, [Burnside and Dollar \(2004\)](#); [Sachs \(2006\)](#); [Clemens et al. \(2011\)](#); [Werker, Ahmed and Cohen \(2009\)](#); [Arndt, Jones and Tarp \(2016\)](#); [Galiani et al. \(2017\)](#)).¹ Nevertheless, scholars have long recognised that it can also fund patronage, breed conflict, ethnic favoritism, corruption and consolidate autocratic regimes.²

In this paper, we reconcile these views in a theory that formalises the effect of foreign aid on political incentives and economic growth. When commitment problems plague policy-making, aid might be misallocated. Aid, however, may also increase the value of being in power. One consequence of this is that aid shocks may then increase the efficiency of public policies. This, in turn, may explain why the aid literature typically finds an overall positive effect on economic growth. Consistent with this view, we document that in Pakistan, there is (i) an overall positive effect of foreign aid on economic growth, while aid (ii) decreases political turnover and (iii) is disproportionately spent in the region of birth of the incumbent, (iv) while aid also increases public employment. These results suggest that aid shocks have an overall positive effect on growth, despite aid being used by the incumbents strategically, who increase their support by redistributing aid to their region of birth and by providing public employment.³

To provide causal evidence for the predictions of the model, we estimate the effect of US aid on public employment and economic growth in Pakistan using changes in aid legislations in the United States as an instrument for foreign aid flows coming into Pakistan. Indeed, we present evidence consistent with the excludability of the instrument, as US aid legislations are uncorrelated with US trade to Pakistan, US investments in Pakistan, non-US aid and potentially confounding outcomes in Pakistan. Our estimates imply that exogenous aid shocks exert positive impact on economic growth. We find that this positive effect of foreign aid on economic growth exists, while aid is at least partially misallocated, as it is used to fund public employment and is disproportionately spent

¹Notable exceptions that find null effect (positive but statistically insignificant coefficient estimates) include ([Easterly, Levine and Roodman \(2004\)](#) and [Rajan and Subramanian \(2008\)](#)).

²See, for instance, ([Ahmed \(2012\)](#); [Nunn and Qian \(2014\)](#); [Hodler and Raschky \(2014\)](#); [Dreher et al. \(2016\)](#), [Easterly and Pfutze \(2008\)](#); [Asongu \(2012\)](#); [Kono and Montinola \(2009\)](#))

³Public employment is widely regarded as a means of redistributing resources and building political support, see for instance [Alesina, Baqir and Easterly \(2000\)](#); [Alesina, Danninger and Rostagno \(2001\)](#); [Robinson and Verdier \(2013\)](#); [Robinson, Torvik and Verdier \(2006\)](#).

in the region of birth of the incumbent.

Our theoretical results show that when political institutions do not promote accountability politicians will not be able to commit to public policies. They will, therefore, offer public employment in their region of birth. By doing so, they credibly transfer resources to a fraction of the citizenry, and consolidate their power. Aid may be misallocated, when it is disproportionately spent in the incumbent's region of birth or used to fund public employment. These inefficiencies on how foreign aid is spent arise in our model, because politicians are unable to commit (as in [Acemoglu \(2003\)](#), [Robinson, Torvik and Verdier \(2006\)](#), [Besley and Coate \(1997\)](#), [Robinson and Verdier \(2013\)](#)). Crucially however, these misallocations - inherent to aid windfalls when political institutions do not promote accountability - are counterbalanced by another effect. Aid shocks also increase the incumbent's present value of winning upcoming elections and staying in power. This may make public policies more efficient. This, in turn, may lead to lower rent extraction and higher public investment. Hence, foreign aid may have an overall positive effect on economic growth.

In the context where the scope of informal employment is large, the inefficiencies implied by foreign aid on a less effective use of human resources are limited. Therefore, when resources are diverted from informal activities to the public sector, the negative effect of foreign aid on growth is low. This explains why foreign aid - despite increasing public employment - can lead to a substantial growth effect. We can reasonably expect this to be the case for many developing countries. Indeed, a large informal sector is typical for many developing countries ([International Labor Organization \(2018\)](#)). For instance, according to a recent 2017-2018 Labour Force Survey 71% of the total labour force operates in the informal sector in Pakistan ([Pakistan Bureau of Statistics \(2018\)](#)).

Testing our key theoretical results that foreign aid has a causal and positive impact on growth and public employment and a negative effect on political turnover however, faces an important empirical challenge: the decision to dispatch foreign aid is endogenous to the recipient country fundamentals. As is extensively noted in the literature, weak or in some cases strong growth in aid recipient countries determines donors' decision to dispatch aid. This makes the aid-growth relationship endogenous. Formal analysis of instrumental variables used thus far points towards important shortcomings ([Bazzi and Clemens \(2013\)](#)). This is because most instruments employed exploit aid variations within the recipient countries "from factors such as geography, population and income that naturally leave open many concerns about the exclusion restriction" ([Qian, 2015](#), p. 302). Therefore, in our identification strategy, instead of relying on an instrument based on aid recipient's characteristics, we utilise an aid instruments orthogonal to Pakistan's domestic fundamentals. First, we use geopolitical events such as Soviet Invasion of Afghanistan and 9/11 attacks

in the United States to identify the effects of aid on growth in Pakistan. Under the assumption, that these events are orthogonal to domestic fundamentals of Pakistan, we can estimate the causal effect of US foreign aid on output growth in Pakistan. Second, we examine the robustness of these results where we instrument foreign aid coming to Pakistan with broad changes in aid legislations in the United States. Under the assumption that changes in aid legislations in the US is determined by factors related to the US, not specificities of the Pakistani economy, we can plausibly interpret our results as causal. We provide evidence in favour of the exclusion restriction where we show that the instrument is uncorrelated with potential confounders such as US exports, US investments, non-US aid and potential confounding outcome variables in Pakistan.

Consistent with the predictions of the model, we find that aid shocks are positively associated with output growth. A typical US aid shock consisted of about 1% of Pakistans GDP and equalled about USD 400 million. This is about half of what the government spent on education and is a little more than what it spent on healthcare in 2015 (UNESCO, 2016; [World Health Organization \(2016\)](#)). The estimates imply that this aid shock is associated with an additional 1% GDP per capita growth. Formal analysis of variance decomposition indicates that about 25% of total variation in GDP per capita of Pakistan can be explained by these aid shocks. Similar results are found with high frequency monthly output series with aid shock associated with an additional 1% increase in output growth.

Likewise, we show that aid shocks increase public employment in Pakistan. In particular, we show public employment in Pakistan increases following these plausibility exogenous US aid shocks. Second, we show that these aid shocks are also associated with a decrease in political leader's turnover. The aid shock reduces the probability that the president will lose office in any given year by about 2.5%.⁴ Third, we find evidence that during time of aid windfalls, the birth region of the incumbent leader witnesses a disproportionate increase in output growth. Indeed, output growth in the birthplace of the incumbent leader is 2% higher during the aid windfalls.⁵

We provide evidence in favour of the key identification assumption that allows us to interpret the differential impact of foreign aid at the birthplace of incumbent leader as causal. That is, we find no evidence that there are differential trends prior to the aid shocks. First, we show that the average industrial production in the birth-province of the incumbent leader (the treatment) versus the average industrial production in the provinces where the leader is not in office (the control), follows common trends prior to the geopolitical aid shocks. Second, we conduct a placebo

⁴We obtain similar results if we use Prime Minister instead of the president losing political office in any given year

⁵This is consistent with the cross-country evidence presented by [Dreher et al. \(2016\)](#).

test where we show that one, two or three years before the region becomes the birth region of the incumbent head of state during the geopolitical aid shock does not induce a disproportionate increase in output growth.⁶

Finally, we provide evidence that the positive effect of aid on growth is robust to several alternative explanations. In addition to the foreign aid channel, there are four leading alternative channels. First, if the US aid is correlated with non-US global aid, then our estimates would be biased upwards, with the geopolitical aid capturing the effect of total global aid flows. Second, the US aid shocks may have accompanied a change in US trade policy where trade with the US, not foreign aid, could have caused the change in economic performance. Third, the US aid shocks could have accompanied lucrative investment deals with the United States. Therefore, it may be that US investment, not aid, that explains the improved growth following the aid shocks. Fourth, important geopolitical events such as the Soviet invasion of Afghanistan may have increased the external threat perception in Pakistan. This may have led to increased investment in defence spending causing growth, instead of the aid shocks. We examine each of these explanations. We test for the non-US aid, US trade, investments and defence spending explanations by examining how these variables changed during the geopolitical shocks. We discover that following the geopolitical shocks, these variables are virtually unresponsive.⁷ Therefore, when we control for them in the regressions, we observe no discernible effect on the qualitative as well as statistical significance of the results.⁸

This paper relates to several stands of literature.⁹ First, our paper relates to the literature on the political economy of aid, which demonstrate that foreign aid finances patronage and consolidates incumbent politicians (Ahmed (2012); Jablonski (2014)), erodes democratic institutions (Djankov, Montalvo and Reynal-Querol (2008)), breeds conflict (Nunn and Qian (2014)), causes moral hazard problems (Azam and Laffont (2003)) and deteriorates governance in recipient countries (Wright and Winters (2010)).¹⁰ Our model is robust enough to square the main issue raised

⁶We further show that many of the potentially confounding factors (such as US trade, FDI, non-US aid and military spending) when interacted with the head of state dummy are statistically insignificant, while interaction term of head of state incumbency with aid windfalls is significant.

⁷We find similar results across three non-US aid flow series: total non-US global aid, UK aid (Pakistan's largest donor after the United States) and European Union aid.

⁸Interestingly, the military spending as a proportion of public spending and defence component of GDP continue to follow a downward trend even in 'high external threat' periods.

⁹Here, we cannot do justice to the large aid-growth literature, for a recent review, see for instance Qian (2015).

¹⁰Bourguignon and Gunning (2016) provide a literature review on aid and governance.

by this literature, as we show that political incentives interact with the allocation of foreign aid when political institutions do not promote political accountability. Indeed, given that incumbent leaders are unable to commit to their future policies, they disproportionately transfer revenues to their region of birth, where they can credibly and cheaply increase their support. We show that this widely documented political strategy in developing countries has a limited effect on growth when the informal sector is large. Furthermore, we demonstrate that despite these misallocations, the dynamic efficiency of public policies is increased by the aid shocks. This may explain the significant effect of foreign aid on economic growth in developing countries, as we document for the case of Pakistan.

There are few theoretical studies on foreign aid, mainly focused on agency problems and the allocation of aid in the presence of multiple donors or recipient countries.¹¹ In this paper, we focus on the effectiveness of unconditional aid from a single donor to a single recipient country. We complement the existing theoretical literature by modelling the effect of foreign aid on the political equilibrium in the recipient country. In doing so, we adapt the theory of [Robinson, Torvik and Verdier \(2006\)](#) on the resource curse to the case of foreign aid.¹² In particular, these papers show that incumbents increase the scope of public employment in their own group (e.g. in their region of birth), because they cannot commit to public policies. Although, we simplify the determination of the political equilibrium, we extend [Robinson, Torvik and Verdier \(2006\)](#)'s model by assuming that incumbents can extract rents from the tax collection and we model the informal sector in the labour market. By doing so, we are able to demonstrate that foreign aid decreases rent extraction, while a large informal sector necessarily induces a positive relationship between foreign aid and economic growth.

Second, the paper speaks to the large cross-country literature on foreign aid and growth ([Burnside and Dollar \(2004\)](#); [Easterly, Levine and Roodman \(2004\)](#); [Clemens et al. \(2011\)](#); [Arndt, Jones and Tarp \(2016\)](#)). The paper complements this cross-country literature, especially the recent studies that try to identify the causal effect of aid on growth ([Rajan and Subramanian \(2008\)](#); [Dreher and Langlotz \(2017\)](#); [Galiani et al. \(2017\)](#); [Temple and Van de Sijpe \(2017\)](#)).¹³ We contribute

¹¹See for instance, [Adam and OConnell \(1999\)](#) and [Azam and Laffont \(2003\)](#). [Bourguignon and Platteau \(2017\)](#) provide a review of the theoretical literature on aid effectiveness, while they also present a framework that studies the trade-off between needs and governance in the allocation of aid.

¹²See also the related studies of [Robinson and Verdier \(2013\)](#) and [Robinson, Torvik and Verdier \(2017\)](#).

¹³In this strand of literature, our paper is close to recent work by [Galiani et al. \(2017\)](#). They use crossing of an 'arbitrary' World Bank's IDA income threshold (that leads to drops in foreign aid for the crossing countries) as an instrumental variable to document a positive impact of aid on growth. Nevertheless, [Dreher and Lohmann \(2015\)](#)

to this literature by estimating the impact of foreign aid on growth through relying on exogenous variation in foreign aid based on changes plausibly exogenous to the aid recipient characteristics.¹⁴ We also contribute to this literature by offering a national and subnational setting, thus, mitigating most of the common identification issues that arise in studies that exploit differences across countries.¹⁵ Third, our study complements the cross-country literature on politically motivated aid by providing an alternate politically motivated aid shock and introducing new identification strategies that rely on geopolitical events and changes in aid legislations in the donor country ([Kuziemko and Werker \(2006\)](#); [Faye and Niehaus \(2012\)](#); [Dreher, Eichenauer and Gehring \(2016\)](#)).

The remainder of the paper is organised as follows. In Section 2, we present a theory on the effect of foreign aid on redistribution and growth. Section 3 tests the predictions of the model, while Section 4 concludes. The mathematical proofs are relegated to the mathematical appendix.

2 The model

Public employment is widely regarded as a means of redistributing resources. For instance, [Alesina, Baqir and Easterly \(2000\)](#) show that politicians use public employment as a redistributive device in US cities. Similarly, [Alesina, Danninger and Rostagno \(2001\)](#) show for Italy that public employment allowed for transfer of resources from the North to the South. One major reason for the use of public employment in redistributive politics is that even though politicians cannot commit, they can credibly offer public jobs to their constituents.¹⁶ Indeed, commitment problems plague policy-making ([Acemoglu \(2003\)](#)), especially in developing countries.

Following [Robinson, Torvik and Verdier \(2006\)](#) and [Robinson and Verdier \(2013\)](#), our key premise is that the incumbent mitigates the commitment problem inherent to policy-making by offering public employment to members of his region of birth before the election, as he cares for

use the same IDA instrument at a more disaggregated level to document that once regional fixed effects are included, positive effect of aid on growth disappears. Therefore, unobservable aid recipient characteristics might be confounding the effect of aid on growth.

¹⁴[Nunn and Qian \(2014\)](#) too use exogenous variations in donor countries, i.e., agricultural production in the United States, to link foreign aid with conflict in a cross-country setting.

¹⁵For instance, [Easterly, Levine and Roodman \(2004\)](#) demonstrates that in a cross-country regression one can strategically select a sample of countries (and time periods) that can exhibit a positive, negative or null effect of foreign aid on development.

¹⁶[Alesina, Baqir and Easterly \(2000\)](#) suggest alternatively that public employments allow to transfer resources to the poor less visibly than tax-transfer schemes.

their welfare. In turn, these agents have a higher likelihood of voting for the incumbent, because they know that the ‘transfer’ they received is costly to reverse after the incumbent is reelected.¹⁷

In this model, we first set a theory of redistributive politics through public employment that adapts the study of [Robinson, Torvik and Verdier \(2006\)](#) on the resource curse to the case of foreign aid. Next, we study the effect of exogenous aid shocks on the size of the public sector across regions, and then on the income generated by the country.

Consider a two-period probabilistic voting model and a society with two regions labelled A and B . The incumbent originates from region A . For simplicity, the mass of voters is normalised to unity, and each region is of equal size $1/2$.

Voters have linear preferences in their own income:

$$V_t^i(j) = w_t^i(j) - \tau_t^i(j), \quad (2.1)$$

with $w_t^i(j)$ their wage and $\tau_t^i(j)$ a lump-sum tax paid by the voters from region $i \in \{A, B\}$ in period $t \in \{1, 2\}$, when the incumbent originates from region $j \in \{A, B\}$. We denote $g_t(j)$ the fraction of the labour force employed in the public sector in period t when the incumbent originates from region j .

For simplicity we assume that when an agent is not employed in the public sector, his wage is equal to his marginal productivity, which we denote $h > 0$. In contrast, we posit that the productivity in the public sector is lower than in the private sector. We normalise the productivity in the public sector to $h_p < h$. Therefore, when the incumbent offers public jobs to private workers, he necessarily reduces their productivity.

Finally, a fraction $z \in [0, 1]$ of the labour force is assumed to work in the informal sector. We can reasonably expect z to be large in the developing world. In Pakistan, for instance, according to a recent 2017-2018 Labour Force Survey 71% of the labour force operates in the informal sector ([Pakistan Bureau of Statistics \(2018\)](#)). This is typical for many developing countries ([International Labor Organization \(2018\)](#)).

The timing of the game is as follows:

1. In period 1, the incumbent offers public employments to a share $g(A)$ of the agents in region A and to a share $g(B)$ of the agents in region B . He also sets the transfers $\tau_1^A(A)$ and $\tau_1^B(A)$.

¹⁷Likewise, institutional changes mitigate commitment problems inherent to policy-making because they are not easily reversed. See for instance [Acemoglu and Robinson \(2000\)](#), [Bisin and Verdier \(2017\)](#), [Bisin et al. \(2018\)](#) and [Iyigun, Rubin and Seror \(2018\)](#).

2. At the end of period 1, the agents vote for the incumbent or for his challenger from region B .
3. At the beginning of period 2, the incumbent from region $j \in \{A, B\}$ sets the tax rates $\tau_2^A(j)$ and $\tau_2^B(j)$ and possibly offers new public employments.

We assume that the incumbent values both the rents he extracts from the citizenry and the welfare of the voters in his region of birth:

$$W_t^A = v(r_t^A) + \alpha V_t^A(A), \quad (2.2)$$

with $\alpha > 0$ his degree of altruism for the voters from region of birth A and $V_t^A(A)$ the preferences of the voters in region A (see (2.1)). The utility function $v(\cdot)$ is assumed increasing and concave in the level of rent extraction r_t^A .

The bias that the incumbent has for his region of birth in the specification above is backed by a large empirical literature on the allocation of public spending. For instance, [Hodler and Raschky \(2014\)](#) show for 38,427 subnational regions covering 126 countries that political leaders divert foreign aid to their birth regions when in power where the transfer of resources is starkest in weakly institutionalized countries. Similarly, [Lehne, Shapiro and Eynde \(2018\)](#) demonstrate that politicians allocate infrastructure contracts to members of their own network in India. [Luca et al. \(2018\)](#) also document similar ethnic favoritism, as nighttime lights are more intense in political leaders' ethnic homelands.

Furthermore, several anecdotal accounts point toward leaders in Pakistan favoring their birthplace regions, especially at times of aid windfalls. One example provided was the use of foreign aid to “give away” laptops, in the birthplace province of the incumbent leader ([Telegraph \(2012\)](#)). Nevertheless, there are a plethora of examples where several political observers and politicians from minority provinces have lamented the diversion of aid resources by incumbent leaders to their respective home constituencies. For instance, in the case of US aid: ‘Why there are so many US Aid tents found in Mardan and not anywhere else?’: Khan Hoti ([Dawn \(2015\)](#)). Likewise, a prominent senator representing a ethnic minority from Balochistan province noted for Chinese foreign aid: “It is, in fact, the China-Punjab (aid package) not China-Pakistan as stated officially, because it will mainly benefit Punjab and not the other provinces.”: Senator Achakzai (quoted in [Dawn \(2016\)](#)).

Credible policy:

A policy can be credible for two key reasons. Either it is ex post optimal for the incumbent, or the incumbent cannot easily renege on a policy he has already implemented. Public employment falls under the second category, as an incumbent cannot easily renege in period 2 on a job he offered in the public sector in the previous period. Offering public employment before the election is then a credible way of transferring resources in this model. Furthermore, the incumbent can make no credible transfer to the agent in region B , as he does not value their welfare. These agents then expect $\tau_t^B(A) = \tau > 0$ for any period $t \in \{1, 2\}$, with τ a parameter that relates to the fiscal capacity of the state. We assume that τ leaves the agents above their survival constraint. For simplicity, we also assume that the incumbent's degree of altruism α is low, so that he cannot credibly promise to the agents in his own region of birth a lower lump-sum tax, $\tau_t^A(A) = \tau > 0$.

Since the incumbent only values the welfare of the agents in his region of birth, he will only offer public jobs in his region of birth, $g(B) = 0$. Furthermore, as the incumbent faces no reelection in period 2, he will not offer more public jobs in the last period of the game. We denote u the minimum wage in the public sector in any period $t \in \{1, 2\}$.

Assumption 1 *The incumbent's degree of altruism α is sufficiently low, so that $\tau_t^A(A) = \tau$, while he necessarily sets the wage of public employees to its minimum $u > h$.*

This assumption is made for convenience, although it seems natural to assume that in a given period, the incumbent always puts a higher value on a marginal increase in his own revenue than on a marginal increase in the revenues of the agents in his region of birth.

Voting model:

As in standard probabilistic voting theory (e.g. [Persson and Tabellini \(2002\)](#)), each voter v has an ideological bias σ^v toward the incumbent. This ideological bias is uniformly distributed over the segment $[-\frac{1}{2\beta}, \frac{1}{2\beta}]$. The parameter $\beta > 0$ accounts for the density in the distribution of the voters' ideological biases. When β is large, then the voters are relatively homogeneous. The incumbent also experiences a popularity shock θ in any period, with θ uniformly distributed over the segment $[-\frac{1}{2\phi}, \frac{1}{2\phi}]$, with $\phi > 0$ a density parameter.

A voter v from group $i \in \{A, B\}$ elects the incumbent when:

$$V_t^i(A) + \sigma^v + \theta > V_t^i(B). \quad (2.3)$$

An agent that does not have a public employment has a utility $V_t^i(A) = h - \tau$ from choosing A . Similarly, since B can not credibly commit to any platform, $V_t^i(B) = h - \tau$. It follows that an agent that is not a public employee chooses the incumbent A when $\sigma^v + \theta > 0$. From simple computations, we deduce that the incumbent gets half of the votes in the pool of agents with no public employments. This is unsurprising; neither the incumbent nor his challenger makes credible promises to these voters, which are then indifferent between the two types of candidates. A public employee, however, knows that given that he has been offered a job by the incumbent from region A , he will keep his public employment in the next period, so he elects A when:

$$u - \tau + \sigma^v + \theta > h - \tau. \quad (2.4)$$

We deduce that the probability that the incumbent is reelected to be as follows:

$$P(g(A), w_2^A) = \frac{1}{2} + \frac{\phi g(A)}{2}(u - h). \quad (2.5)$$

The higher the share of public employees, the larger the probability that the incumbent is reelected, given that $u > h$ under assumption 1. Indeed, as long as the incumbent pays the public employees a higher wage than what is attainable in the private sector h - and creates public employment in his region of birth - he increases his probability of winning.

Predictions:

We denote $a_1 > 0$ the foreign aid that the incumbent receives in the first period, which we assume as exogenous with respect to the characteristics of the economy.¹⁸ The incumbent sets the rents (r_1^A, r_2^A) and the share of public employments $g(A)$ so as to maximise

$$R(r_1^A, r_2^A, g(A)) = v(r_1^A) + \alpha(g(A)u + (1 - g(A))h) + P(g(A))(v(r_2^A) + \alpha(g(A)u + (1 - g(A))h)), \quad (2.6)$$

given that

$$r_1^A + r_2^A + 2g(A)u \leq 2\tau + a_1. \quad (2.7)$$

The aid a_1 enters directly into the budget constraint of the incumbent, and therefore affects his incentive to offer public employment and to extract rents. The optimal policy solves the following

¹⁸In section 3, we back this by introducing an identification strategy and presenting evidence in support of this contention for the case of the US aid to Pakistan.

first-order conditions:

$$\begin{cases} v'(r_1) - P(g)v'(r_2) = 0 \\ (1 + P(g))\alpha(u - h) - 2v'(r_2)P(g) + \frac{dP(g)}{dg}[v(r_2) + \alpha(gu + (1 - g)h)] = 0, \end{cases} \quad (2.8)$$

with $r_2 = 2\tau + a_1 - 2ug - r_1$.

The first line of (2.8) relates to the optimal level of extraction in period 1. When setting the level of extraction in period 1, and for a given fraction of public employees, the incumbent trades-off the marginal value of capturing rents in the current period with that of capturing rents in the next period. The higher the probability of being reelected $P(g)$, then higher the marginal cost of capturing rents in the first period. Therefore, lower the level of extraction in period 1. We can then easily deduce from the first line of (2.8) a locus $r_1(g)$, which is decreasing with g (as $P(g)$ increases with g). In other words, the optimal level of extraction is a decreasing function of the share of public employments.

The second line of (2.8) gives the first-order condition with respect to the fraction of public employees in the region of birth of the incumbent g . First, when increasing g , the incumbent anticipates that he will increase his probability of winning, and hence the present value of the rents that he extracts in period 2. This effect is given by the last term in the LHS of the second line of (2.8), with $\frac{dP(g)}{dg} = \frac{\phi}{2}(u - h) > 0$. Second, the incumbent internalises that a marginal increase in g will positively affect the welfare of the agents in his region of birth, since $u > h$. This effect is given by the first term in the LHS of the second line of (2.8). Finally, the incumbent anticipates that an increase in the share of public employees in the first period also means that he will be more constrained when capturing rents in the next period. This is the second term in the LHS of the equation.

As for the first first-order condition, we can show that the second line of (2.8) gives a locus $g(r_1)$, which is decreasing in r_1 . Indeed, the higher the level of extraction in period 1, then the lower the incentive of the ruler to transfer resources to the citizenry by creating public jobs. This is because the present value of extracting resources in period 2 is low when the level of extraction in period 1 is high. Hence, the incumbent has less incentive to offer public employments in order to be reelected.

Given the previous analysis, the two loci that we deduced from the first-order conditions are decreasing functions, so they intersect only once. There is a unique equilibrium. We establish the following result.

Proposition 1 *When the aid shock a_1 increases, political turnover decreases, while public employment increases in the incumbent's region of birth.*

Proof. The proof is available in Appendix [A.1](#). ■

An increase in the level of aid induces an income effect that increases both the incentive of the incumbent to extract rents and to redistribute public jobs in his region of birth. Nevertheless, since rent extraction and redistribution of public jobs are substitutes, the net effect of aid on public employment is ambiguous. Indeed, as discussed previously, when the incumbent creates public employment, he also increases the marginal cost at capturing rents in the first period of the game. This is the key mechanism of this model: redistribution through public employment - by increasing the likelihood of winning the election - also disciplines the incumbent in period 1. Foreign aid then increases the dynamic efficiency of the public policy. As it turns out, the income effect induced by the aid flow increases the incentive of the incumbent to redistribute resources through public jobs, and this effect is not offset by a higher level of rent extraction.

The income generated by the country is

$$Y_1 = a_1 - r(a_1) + h(1 - (1 - z)\frac{g(a_1)}{2}) + \frac{g(a_1)}{2}h_P, \quad (2.9)$$

with h_P the productivity in the public sector. Indeed, the income generated by the country is equal to the sum of the production in the private sector $h(1 - (1 - z)\frac{g(a_1)}{2})$, in the public sector $\frac{g(a_1)}{2}h_P$ and the aid flow net of the rents extracted by the incumbent, $a_1 - r(a_1)$. We establish the following result.

Proposition 2 *The income generated by the country increases with the aid shock a_1 when the informal sector is sufficiently large.*

Proof. The proof is available in Appendix [A.2](#). ■

In the general case, the effect of the aid flow a_1 on the total income is ambiguous. Indeed, there are three effects to consider. First, the aid flow increases the fraction of public employees, as established in proposition [1](#). Therefore, a share of the workforce in the region of birth of the incumbent is driven out of the private sector. A fraction $(1 - z)\frac{g(a_1)}{2}$ of the agents in the region of birth of the incumbent then switches from formal occupations in the private sector to public jobs. This shifts their productivity from h to h_P , with h_P possibly below h . When $h_P < h$, the redistribution through public jobs leads to a negative effect on the aggregate income, as the productivity of the workforce decreases.

The second effect to consider is that redistribution through public employment decreases the scope of informality, as a fraction $z\frac{g(a_1)}{2}$ of the agents with informal occupations in the region of birth of the incumbent now have formal jobs in the public sector. This tends to increase the reported national income.

Finally, there is a third effect to consider, which is that the aid flow has a direct effect on income, and so does the extraction of rents. The aid flow net of rents $a_1 - r(a_1)$ tends to increase with the level of aid a_1 , which means that the aid flow is not completely dissipated in rents.

The aid flow has a positive net effect on the national income when the first effect above is dominated by the sum of the two others. More specifically, aid increases the national income when - despite the possible loss of productivity due to a shift of a fraction of the labour force from the private to the public sector - the scopes of informality and rent extraction are sufficiently diminished. Again, the net effect of aid on national income crucially relies on the following mechanism: redistribution through public employments - by increasing the benefit of staying in office - also increases the dynamic efficiency of the public policy. This political channel could explain why we observe a positive effect of foreign aid on economic growth. Aid unambiguously increases the national income when the share of informal workers is sufficiently large, because in that case the loss of productivity that is induced by the redistribution through less productive public jobs is limited.

2.1 Bringing the theory to the data:

In order to test the predictions of this theory, we rely on data on US aid to Pakistan for two major reasons. First, Pakistan is characterized by weak political institutions and a large informal sector. Second, our data as well as the identification strategy for US aid to Pakistan allows for causal inferences.¹⁹

Misallocation of aid in Pakistan:

First, Pakistan is a particularly interesting case study for this theory, as it is characterized by weak political institutions, a large informal sector, state resources subjected to elite capture and low level

¹⁹Another related reason for us focusing on United States foreign aid to Pakistan is data availability. We have a long output series both at national and subnational level. Additionally, for Pakistan, the data availability, for instance data on public employment, allows us to directly test for key prediction of the model. This is atypical for studies on foreign aid where data constraints, particularly for aid recipient countries is particularly stark.

of trust (see e.g. [Williams, Shahid and Martínez \(2016\)](#); [Cheema, Mehmood and Imran \(2016\)](#); [Acemoglu et al. \(2018\)](#)).²⁰ In Pakistan, the inefficiencies in the allocation of aid seem particularly important. Indeed, given the institutional background, foreign aid programs in Pakistan have come under strong criticism for their funding of patronage and lack of transparency ([Kopetchny \(2006\)](#)). As anecdotal evidence first, the use of British foreign aid money to give away laptops in birthplace of the incumbent leader (former prime minister Nawaz Sharif) ahead of elections caught attention of international media and was heavily criticised ([Telegraph \(2012\)](#)). Likewise, the use of Chinese aid to build infrastructure only around the birth-region of incumbent head of state led opposition leader in parliament to proclaim: “Nawaz is not the Prime Minister of Pakistan, he is the Prime Minister of Punjab (his home province)” Bilawal Bhutto quoted in [Dawn \(2016\)](#). In a well publicised World Bank’s Evaluation of Assistance Report by [Birdsall, Malik and Vaishnav \(2005\)](#), it was noted that although “growth rates were above the developing country average” during the aid program but “managing the economy and implementing reforms are not institutionalized nor embedded in a resilient and transparent system of government accountability or of adequate checks on abuse of power. ” Likewise, [Easterly \(2001\)](#) considered Pakistan a “puzzle” where he posed the question that “why in Pakistan the low human capital indicators ... social and institutional development did not prevent a respectable growth rate of 2.2 percent per capita over 1950-99?”.

The theory above partly answers to this puzzle, as we show that despite aid being spent disproportionately to increase public employment in the region of birth of the incumbent, it can still have a positive effect on growth. Our point is that incumbents use the foreign aid to redistribute resources in their region of birth, as it allows them to alleviate the commitment problems inherent in policy-making. This result is established in proposition 1. Nevertheless, we demonstrate in proposition 2 that as incumbents consolidate their power - through redistribution - they also increase the present value of better public policies. This, we argue, is particularly true in the context of the developing world, where the effect of redistribution through public employment on the reported income is limited. When the size z of the informal sector is sufficiently large for instance - as it is the case for Pakistan - the effect of a larger public sector on the reported national income should be limited. Hence, after receiving aid revenues, incumbents may also increase their investments in public goods, and lower the rents they extract from the citizenry. This is precisely what we find in

²⁰For instance, [Khwaja and Mian \(2005\)](#) estimates the costs from corruption due to borrowing from public banks by politically connected firms in Pakistan to be in the range of 0.3 to 1.9% of GDP every year. Likewise, [Khwaja and Mian \(2008\)](#) show how political connections and political volatility in Pakistan also weakens the financial system and access to finance in the credit markets.

the empirical section below.

Pakistan and the US aid:

Pakistan is also an interesting case study, as our data and identification strategy we propose allows to provide causal evidence for the predictions of the model. Indeed, many scholars and policy makers have noted the highly variable and potentially exogenous United States foreign aid to Pakistan and its dependence on strategic factors related to US not Pakistan needs (Kopetchny (2006); Fair (2009); Haqqani (2013)). Analysis of foreign aid data confirm these observations where the aid series is highly variable and shows dramatic shifts following major geopolitical events. For instance, Pakistan was receiving about USD 500 million per year following the 9/11 attacks, whereas virtually no US aid was flowing to Pakistan in the 1990s (see Figure 1).

Historically, the United States foreign aid to Pakistan has flowed with varying intensity since Pakistan's independence from the British colonial rule in 1947. The first recorded assistance package to Pakistan was approved by congress in 1953 and involved a transfer of USD 75 million. The stated aim of this aid package was to cement strategic ties with Pakistan, following India's policy of "non-alignment" with the United States on several key geopolitical issues (Department of State Bulletin, 1953).²¹ The second major assistance package was received following the Soviet Invasion of Afghanistan in the December of 1979. To curb the 'communist threat' in the region, the United States government began to lobby congress to resume foreign aid to Pakistan. This resulted in the United States providing an assistance package of US\$ 3.2 billion in the September of 1981 (Gwertzman (1981)).²² Finally, the third, major aid package for Pakistan, was following the 9/11 attacks and to garner Pakistan's support in the global war on terror. This assistance package involved a transfer of "\$1.5 billion in annual non-military aid payments to Pakistan" (Epstein and Kronstadt (2012), 2013, p. 2).

This strategic nature of US aid was also noted in an US Congressional Oversight Hearing by the head of United Nations Assistance Mission on Pakistan:

The variation in assistance (to Pakistan) appears to have little to do with Pakistan's objective needs; rather Washington's changing policy priorities towards the country at different points in time and efforts to achieve U.S. objectives towards the country" (Fair (2009)).

²¹The United States mission in Islamabad noted "it is in the security interest of United States to extend assistance to Pakistan for a friendly government of an important and strategic country" (Department of State Bulletin, 1953).

²² An earlier assistance package of US\$ 400 million was rejected, dubbed condescendingly as peanuts by President General Zia (Bose and Jalal (2004)).

Likewise, quantitative analysis of the determinants of United States foreign aid to Pakistan shows that US aid is uncorrelated with “official development objectives or the recipient need” (Anwar and Michaelowa (2006), p. 195).

What seems to be highly correlated with United States foreign aid to Pakistan are major geopolitical events. For instance, on March 5, 1970, the Nuclear Non-Proliferation Treaty (NPT) was signed between the US, the UK and the Soviet Union (Shaker (1980)). This reduced the US incentive to give aid to Pakistan (given Pakistan’s nuclear ambitions). Similarly, in December of 1979 Soviet Russia invaded Afghanistan. This, amidst the Cold War, increased US incentive to give aid to Pakistan. Our interpretation is that these geopolitical events exogenously vary foreign aid coming into Pakistan, while they do not directly affect the domestic fundamentals of Pakistan, which might be correlated with growth dynamics. From Figure 1, we observe that both aid allocations as well as actual US aid disbursements peak following the geopolitical shocks of Soviet Invasion of Afghanistan and 9/11 attacks.

In addition to the geopolitical events, foreign aid flowing to Pakistan is also correlated with changes in aid legislations in the United States. As can also be seen in Figure 1, when aid legislations in the United States were expansive, this too resulted in inflows of US foreign aid to Pakistan. Under the assumption that the changes in United States aid legislations occur due to changes in domestic and internal policies of United States, this provides another source of exogenous variation to estimate the causal effect of foreign aid on growth and redistribution in Pakistan. Similar results using geopolitical events and changes in aid legislations in the United States as instruments for foreign aid to Pakistan strengthens the case that the results we obtain are causal. For a list of all changes in US aid legislations i.e. changes to the Foreign Assistance Act of 1961 from 1970 to 2015, see Table 1.²³

3 Empirical evidence

3.1 Data Sources and Main Variables

We combine publicly available and archival data sources to construct a national time series and a province-level panel for output series from 1972 to 2015.²⁴ The data sources include US Green-

²³For description of the content of each change in aid legislation, see Table C.1 in Appendix C.1.2.

²⁴We start from year 1972 primarily because in 1971 East Pakistan split into Bangladesh i.e. Pakistan was not the same country before and after 1971. Although, we find similar results when we take full available sample of

book of Aid, IMF historical archives, Central Bank of Pakistan archives, World Development Indicators of the World Bank, Penn World Tables, Polity IV and ICRG institutional risk indices, UNESCO Institute of Statistics (2018); Arby (2008), Pakistan Bureau of Statistics (2018) and Global Terrorism Database (2018).

Outcome variables.—The main outcome variables are: annual national GDP per capita, annual province-level panel for industrial production and a monthly series for industrial production. These variables are converted to natural logarithms to simplify the interpretation and to remain in sync with much of the aid-growth literature.²⁵ The GDP figures are in constant per capita terms and obtained from the Central Bank of Pakistan.²⁶ The monthly industrial production data are obtained from the IMF historical archives. The province-level industrial production series is retrieved from a study commissioned by the Central Bank of Pakistan (Arby (2008)).

Foreign aid variables.—We employ four foreign aid regressors as explanatory variables. First, is the “Economic Support Fund” (ESF). An unearmarked budget support fund deposited by the US government to the Ministry of Finance in Pakistan. The stated aim of this “*strategic*” component of foreign aid is to “*advance U.S. foreign policy interests*” (State Department Briefings, 2010, p. 39).²⁷ As can be seen in Figure 1 (Panel A), the stated aim of this foreign aid matches well with observed behaviour of US policy makers, in light of changing world events.

Second, we generate a month-specific measure of foreign aid that is constructed based on important geopolitical event dates that changed the US government’s incentive to give aid to Pakistan. This dummy variable, also shown in Figure 1 (Panel A), takes the value 1 for positive aid shock months such as the Soviet Invasion and 9/11 attacks, and zero otherwise. Third, we construct a US Aid Legislation dummy variable. This variable switches on when the United States Congress makes the US aid policy expansive (and switches off when it makes it restrictive). Shaded areas in Figure 1 (Panel B) represent episodes of expansive foreign aid regimes in the United States.²⁸

1960-2015.

²⁵Juselius, Miller and Tarp (2014) discusses other advantages of the log transformation of aid variable such as mitigating non-normality, non-linearity and explosive roots.

²⁶The results are robust if we consider the GDP series from the Penn World Tables 9.0 or the World Development Indicators.

²⁷We find similar results when we use standard measure of aid flows employed in much of literature, as defined by the Development Assistance Committee (DAC), instead of the ESF. In fact, we find similar results across all the four aid regressors.

²⁸The coding of aid legislation as expansive is rather straightforward since it involves formalization of a assistance package as opposed to restrictive legislation that put explicit conditions on aid (e.g. no aid to human rights violators as in Morgan Amendment or no aid to countries trying to acquire a nuclear weapon as in Symington and Pressler

Fourth, we use the US Official Development Assistance (ODA) aid flows measure used in much of the literature. The Economic Support Fund allocations as well as foreign aid flows are also converted to natural logarithms and their overtime evolution can be seen in Figure 1.

Additional variables: We use several additional variables in the analysis. This includes non-US aid, military aid, defence spending in Pakistan, US trade and investments. These variables are obtained from Ministry of Finance, the World Development Indicators and the Central Bank of Pakistan. The series for composite Polity IV index is obtained from database of [Marshall, Gurr and Jagers \(2016\)](#) and “ICRG” country risk indicators and policy indices are obtained from Political Risk Services Group. Data on terrorism is obtained from the [Global Terrorism Database \(2018\)](#). The public expenditures on education series is obtained from UNESCO’s Institute for Statistics, public employment data is obtained from [Pakistan Bureau of Statistics \(2018\)](#) the consumption data is obtained from the Central Bank of Pakistan, province level controls such as the provincial defence production, agricultural value added, value of construction projects is obtained from [Arby \(2008\)](#).²⁹

3.2 The Effect of Foreign Aid on Growth

Before estimating the impact of foreign aid on economic growth and a differential effect of aid at the birth region of incumbent leader we note that output growth in Pakistan experiences a change in trend around time of geopolitical events. A formal test confirms this. As can be seen from Figure 2, GDP per capita and its cyclical component through the HP filter, respond heavily following major geopolitical events. A similar relationship is observed with the monthly industrial production series. Is this change in trend the result of the causal impact of foreign aid on output growth? In this section, we present evidence consistent with the view that US foreign aid shocks have a causal impact on output growth in Pakistan. Our baseline method is the Narrative Vector Autogression (due to [Romer and Romer \(2010\)](#)). This method allows us to assess the dynamic impact of a economic shock on aggregate outcomes. We also show that the results are robust to using an alternate methodology.

We begin the analysis by estimating the bi-variate Narrative Vector Autoregression (NVAR, henceforth) model. A parsimonious two variable NVAR is estimated as the baseline, since it pro-

Amendments). See Table 1 for the list of all the legislative changes to the Foreign Assistance Act and Table C.1 in the Appendix C.1.2 for the descriptions of these changes in aid legislations.

²⁹For more details on the sources and explanations of the variables see the variable description section in the online Appendix B.1.

vides greater transparency, especially when making causal claims (as in [Romer and Romer \(2010\)](#)). To show the robustness of the results, we also estimate the NVAR with additional variables. The salient feature of this methodology allows one to completely circumvent the ordering restrictions which are needed to make causal claims in traditional VARs and allows us to trace the dynamic overtime impact of a unit shock on aggregate output in a transparent manner ([Ramey \(2011\)](#); [Monnet \(2014\)](#)).

The Impulse Response Functions (IRFs) trace the short and medium-term impact of aid shocks on the GDP per capita. We estimate standard errors with asymptotic theory as well as through bootstrapping ([Stock and Watson \(2001\)](#)). We report in this section the more conservative standard errors derived from asymptotic theory since their behaviour is well known ([Ramey \(2011\)](#)). In line with much of the literature, we present 1 standard deviation confidence bands as the baseline (as in [Blanchard and Perotti \(2002\)](#); [Romer and Romer \(2010\)](#); [Monnet \(2014\)](#)).³⁰

The NVAR model is represented as follows:

$$A(L)\vec{Y} = C + \vec{\varepsilon}, \quad (3.1)$$

where $A(L)$ is a lag polynomial equivalent to $A_1L^1 + A_2L^2 + A_3L^3 + \dots + A_pL^p$, while \vec{Y} , $\vec{\varepsilon}$ and C are $n \times 1$ vectors, with \vec{Y} representing a vector of ‘endogenous’ variables, $\vec{\varepsilon}$ the vector of disturbance terms and C the vector of intercept terms, respectively.³¹ From this, a Structural Vector Autoregression (SVAR) is estimated where structure is imposed through the elements of the covariance matrix:

$$u_t = Be_t. \quad (3.2)$$

Nevertheless, since we only consider aid shocks that are plausibly exogenous, the standard SVAR equation (3.2) boils down to [Romer and Romer \(2010\)](#)’s NVAR equation:³²

$$u_t = e_t, \quad (3.3)$$

³⁰Later, we show that the results are robust when we consider 2 and even 3 standard deviation confidence bands.

³¹Note that $Y_t \in \vec{Y}$ and in our case, $Y_t = [\text{GDPpc}_t \text{ ESF}_t]^T$ for annual series and $Y_t = [\text{Industrial Production}_t \text{ Aid Dummy}_t]^T$ for monthly series.

³²This is done by separating the shock variable into endogenous and exogenous component and plugging it into the output equation: $\Delta \text{Aid}_t = \sum_{i=1}^I b_i^t \varepsilon_i^t + \sum_{j=1}^J \omega_j^j \Rightarrow \Delta_t = \alpha + \beta \sum_{j=1}^J \omega_j^j + v_t$, where $v_t = \sum_{i=1}^I (1 + \beta b_i^t) \varepsilon_i^t$, which is then reformulated in a VAR framework and used to calculate the impulse response plots (see [Romer and Romer \(2010\)](#) for more details).

with ε_t is an element of $\vec{\varepsilon}$ that is, the structural errors and reduced form errors are identical. This means we are directly estimating (3.1) and do not require the contentious ordering restrictions used in VAR models to gain identification (Blanchard and Perotti (2002)).

Figure 3 presents the estimated effect of foreign aid on GDP per capita, across the four aid regressors.³³ The results yield evidence of a substantial positive link between foreign aid and output growth across all measures of foreign aid. 5 years following the USD 400 million foreign aid shock, GDP per capita is about 1% higher relative to the counterfactual of no such aid shock. In fact, formal analysis of variance decomposition indicates that at least 25% of total variation in GDP per capita in Pakistan can be explained by these aid shocks.³⁴

Next, we document the impact of geopolitical aid shocks and changes in aid legislations on industrial production. Since, we have industrial production data at a monthly frequency, we can better match it with geopolitical event dates and dates of changes in aid legislations.³⁵ Figure 4 presents these results. We note, once again, that 5 years following the aid shock (60 months), industrial production is about 1% higher relative to the counterfactual of no such aid shock.

The estimated effect of aid on growth is robust to several alternative mechanisms that one might expect can change following the geopolitical events. For instance, the results are similar when we add non-US World Aid, US trade, FDI and Pakistan's defence spending as control variables (see Figure 5).

A placebo test also confirms that these results are not driven by some mechanical statistical correlation of the aid shocks with the real sector. In Figure 6 we shows that there is no effect of a unit shock of Industrial Production on geopolitical or aid legislation dummy. Consistent with this, the results of variance decomposition also showed that less than 1% of variation in US aid can be explained by the movements in output growth. We take this, as evidence against reverse causality flowing from output growth to the aid shocks, as well as to conclude that the impact of geopolitical aid shocks on the real sector is not a statistical artefact.³⁶

³³We present the reduced form estimates of the NVAR when the IV results are presented.

³⁴The results of variance decomposition are presented in C.1 in the online appendix.

³⁵ Furthermore, Monnet and Puy (2016) argue that industrial production is a better tracker of real activity and output growth since it is recorded based on real time economic activity as opposed to ex-post linear interpolations as in GDP per capita and other national account measures.

³⁶We perform additional Placebo tests e.g. assessing the impact of aid shocks on world output (to see if the results are coming from some global trends) and to assess the impact of aid shocks on Indian output (where we chose India given common colonial history of Pakistan and similar institutions to examine regional effects) to find no effect of US foreign aid to Pakistan on global or Indian output growth. Results are available on request.

Next, we present the robustness of these results by instrumental variable regressions. Specifically, we instrument, the US foreign aid flows with geopolitical event and aid legislation dummies, and estimate the reduced form NVAR with OLS and 2SLS, respectively:

$$y_t = \beta_0 + L.USAid_t + Trend + SeasonalDummies + L.X_t + \varepsilon_t, \quad (3.4)$$

where, y_t is GDP per capita, US Aid is the US Official Development Assistance (ODA) aid flows and X_t is a matrix of available control variables used in the analysis.³⁷ Lag length is decided as in the NVAR based on information criteria that gives the optimal statistical fit relative to complexity.³⁸ Table 2 presents the results. The results yield strong evidence that foreign aid exerts a qualitatively and statistically significant impact on output growth. Considering the aid legislation instrument, a 1% increase in aid flows is associated with about a 0.15 percentage point increase in GDP per capita growth the following year.³⁹ Plotting the overtime impact of foreign aid with additional lags shows that this effect is persistent (see Figure 7).

Nevertheless, if the expansive US aid legislations signal global investors that the United States government favours Pakistan beyond its foreign aid policy, it might signal global investors that the investment climate in Pakistan is about to improve in expectation due to being in good books of the United States, then we might be picking up the impact of this favorable investment climate instead of the actual impact of US aid. Although, we cannot completely rule out this mechanism, but we conduct a placebo test that undermines this hypothesis. Since, there were several aid legislations could not pass both houses of Congress, so we use expansive US aid legislations that were tabled yet could not be enacted. Table 3 presents these results. The results show that instrumenting US foreign aid with failed expansive aid legislations has no impact on output growth in Pakistan.⁴⁰

Finally, we provide evidence in favor of the exclusion restriction by conducting a check for balance for all available potential confounders of US aid. Table 4 presents these results. We note

³⁷We obtain similar results when we use Economic Support Fund (ESF) aid allocations instead of US ODA aid flows as a measure of foreign aid.

³⁸Nevertheless, the results are not sensitive to the lag length ordering. In Figure 7, we plot coefficient estimates on foreign aid with varying lag lengths and conclude that the impact of foreign aid is robust and persists.

³⁹This magnitude of the impact of foreign aid is consistent with recent reviews where a “one percentage-point increase in Aid/GDP is typically followed an annual average real GDP per capita growth of 0.10.2 percentage points” (Clemens et al. (2011), p. 609). Likewise, the downward bias of OLS estimate is consistent with recent work by Galiani et al. (2017) Nevertheless, we should interpret the magnitudes here with caution as they do not capture the overall dynamic effect of aid over the 5 years as in the VAR results.

⁴⁰Later, we also show that investment risk and other political risk indices in Pakistan do not change following the geopolitical aid shocks.

that detrended US FDI, trade, non-US aid, Pakistan’s military spending is statistically similar with geopolitical dummy switched on and off but US aid flows, US aid allocations and GDP per capita are not. Similar results are found with aid legislation dummy switched on and off.⁴¹

3.3 The Allocation of Foreign Aid

This subsection is divided into two parts. First, consistent with the predictions of the model, we present evidence that aid shocks leads to a higher public employment. Second, we show how foreign aid shocks deferentially impact birth province of incumbent leaders.

A key theoretical prediction of the model is that foreign aid increases public employment. Indeed, in proposition 1, we demonstrate that incumbents create public jobs as it is a credible transfer, which allows them to increase the likelihood of staying in office. We test for this by examining the impact of foreign aid on employment in the public sector. This is consistent with anecdotal accounts from Pakistan where politicians use public employment as a tool to dole out political favours (see for instance, [Aziz et al. \(2014\)](#)). Figure 8 (Panel A) present these results. We find that the USD 400 million foreign aid shock increases public employment by about 2% relative to the counterfactual of no such aid shock. Likewise, consistent with the second prediction in proposition 1, we find that the aid shock decreases incumbent’s turnover. We construct leader turnover variable identical to the one employed by [Chaney \(2013\)](#) i.e. a dummy equal to 1 if the incumbent head of state at the start of the year t is no longer in office at the end of that year. In particular, the aid shock reduces the probability that the president will lose office in any given year by about 2.5%. (see Figure 8, Panel B).

Next, we show that the gains from foreign aid are heterogeneously spread at the national level. To test the prediction of the model that gains from foreign aid are differentially distributed according to the birthplace of incumbent leader, we extend [Nunn and Qian \(2014\)](#) and [Temple and Van de Sijpe \(2017\)](#) intuition of interact time-varying variable related to the donor with a time and space varying variable related to the recipient, but at a subnational level. Specifically, we estimate the following equation:

$$y_{pt} = \beta(\text{GeographicalAid}_t \times \text{HeadofState}_{pt}) + \rho \text{HeadofStateBirth}_{pt} + \mathbf{X}'_{pt} \cdot \theta + \alpha_p + \gamma_t + t \cdot \phi_p + \varepsilon_{pt}, \quad (3.5)$$

where y_{pt} is the industrial production in province p at year t , α_p and γ_t are province and year fixed

⁴¹ Available on request.

effects, respectively. As different provinces can have different development trajectories, for example due to different provincial policy choices, we control for these differential trends by adding province-specific trend, $t.\phi_p$, to equation (3.5).

Geopolitical aid shock is the geopolitical shock dummy variable used in the time series analysis. The $HeadofStateBirth_{pt}$ is a province and time varying variable that takes the value of 1 for the birthplace province, in the years that the head of state is in office (and zero otherwise). Furthermore, we add the available province level controls (province agricultural output, construction and housing value added) to the list of covariates (X_{pt}). The interaction between the head of states birthplace and the geopolitical aid shock dummy is the main variable of interest. The coefficient on this interaction (β) gives us the estimate of the differential effect of the geopolitical aid shock if the incumbent head of state was born in that province.

Table 5 presents these results. The results imply that output growth during the geopolitical aid shocks is about 2 percentage points higher in the birthplace provinces of the incumbent leaders relative to those that were not in power during the aid windfalls. In absence of geocoded US aid data to Pakistan, we present two pieces of evidence that suggest that incumbent leaders divert aid resources to their respective birth provinces. First, we present anecdotal accounts. Second, we rule out several channels that might confound the results. Many anecdotal accounts point towards leaders favoring their birthplace regions, especially at times of aid windfalls in Pakistan. One example that was discussed with the use of foreign aid to “give away” laptops, in the birthplace province of the incumbent leader (Telegraph (2012)). Similarly, several political observers and politicians have lamented the diversion of aid resources by incumbent leaders to their respective home constituencies.⁴²

In addition to the anecdotal accounts, we rule out several alternate channels that might result in the confounding of the impact of aid and diversion of resources to incumbent head of states birthplace during the aid windfalls. We add additional controls and their interactions to equation (3.5) and examine how the coefficient estimate on the interaction of incumbent’s birthplace and aid change. Table 6 presents these results. We note that results become more precise and that only the US aid shocks result in a disproportionate increase in output growth in the birthplace of incumbent head of states and that adding non-US foreign aid, US exports, US FDI and other potential

⁴²“Why there are so many US Aid tents found in Mardan and not anywhere else in the province?”: Khan Hoti. or “It is, in fact, the China-Punjab (aid package) not China-Pakistan as stated officially, because it will mainly benefit Punjab and not the other provinces.”: Senator Achakzai. Or “Nawaz is not the Prime Minister of Pakistan, he is the Prime Minister of Punjab (his home province)”: Member Parliament, Bilawal Bhutto, quoted in Dawn (2016)

confounders has no bearing on the results. We also present evidence that there are no differential trends prior to the aid shock. In particular, we show that average industrial production in the birth-province of the incumbent leader (the treatment) versus the average industrial production in the provinces where the leader is not in office (the control), follows common trends prior to the geopolitical aid shocks (see, Figure 9). Finally, in Table 7, we present the results of a placebo test where we show that one, two or three years before the region becomes birth region of the incumbent head of state during the geopolitical aid shock does not induce a disproportionate increase in output growth in Pakistan.

3.4 Robustness Checks

In this section we conduct additional robustness checks and sensitivity analysis of the results. First, we show that the results are robust to additional alternative explanations linking US aid to Pakistan's output growth. Second, we conduct econometric stability checks on the results.

There are additional alternative channels that might link US foreign aid to output growth in Pakistan. For one, even if the US economic assistance does not directly impact defense spending, it might still increase defense production in Pakistan, if for example, the US economic support funds were diverted to the military. Second, higher terrorism levels in Pakistan may draw more US aid, then the higher consequent GDP per capita growth might not be due to increased aid per se, but its conflict reducing effect. Third, geopolitical aid shocks might give rise to a new political equilibrium which might in turn impact investment and political risk in Pakistan.⁴³ The placebo test that attempted to link failed aid legislations in the United States to output growth in Pakistan, undermines this hypothesis to some extent, but it might not completely capture the change in investment and political climate following successful changes in aid legislation and the geopolitical shocks.

Although, it is impossible to completely rule out each of these channels, but we present evidence against them. Figure 10 presents the results. From Panel A, we observe that foreign aid exerts negligible impact on defence component of GDP in Pakistan. This implies that the defence component of GDP is not impacted by foreign aid shocks. Furthermore, controlling for terrorism, ICRG investor and Polity IV institutional index has no bearing on the results: foreign aid is still positively associated with output growth in Pakistan (Figure 10, Panel B, C and D).⁴⁴

⁴³Depending on whether geopolitical events increased or decreased domestic risk in Pakistan, it can lead to an improvement or decline in economic performance.

⁴⁴In fact, none of the political and investment risk indices in Pakistan seem to change following the geopolitical

Next, we conduct some key NVAR diagnostic tests that have been discussed in the literature (Romer and Romer (2010); Lütkepohl and Schlaak (2018); Monnet (2014)). First, we show that the results are not dependent on the chosen ordering restrictions that crucially determine standard VAR results (see for example, Fernández-Villaverde et al. (2007)’s critique of VARs on exactly this point). From Panel A of Figure 11, we observe that varying the ordering restriction of the NVAR has no impact on the qualitative as well as statistical significance of the results. Second, we show the results are statistically strong i.e. they are robust even when we consider two or even three standard deviation bands.⁴⁵ Panel B of Figure 11 presents these results. We observe especially from year 2 to 5, the IRF estimates are even within three standard deviation bands.

Finally, we show that results are robust to computing standard errors through bootstrapping instead of the asymptotic theory. Kilian (1998) and Pool, De Haan and Jacobs (2015) have suggested using bootstrapped confidence intervals especially when working with small samples. Although, we have continued to report more conservative standard errors, based on asymptotic theory, but in Figure 11 (Panel C), we also compute standard errors by bootstrapping. We note that the results remain unchanged.

4 Conclusion

In this paper, we have demonstrated that although foreign aid is partially misallocated, it may still have a positive - and significant - effect on economic growth in developing countries. By doing so, our theory links two strands of literature that grew apart in the recent decades: one demonstrating the positive effect of foreign aid on growth, the other emphasising the distortionary effects of foreign aid on political incentives.

Pakistan provides a particularly interesting context to empirically examine the predictions of the model, as political corruption is widespread, informal sector is large, while data on US aid, identification strategy and subnational output series allows us to provide causal evidence. We demonstrated that the effects of aid on redistribution and economic growth are positive and significant. An aid shock of about USD 400 million leads to an additional 1% increase in GDP per capita

events or changes aid legislations in the United States. This is why controlling for them in the regressions has no bearing on the main results.

⁴⁵Although, most of the empirical VAR literature uses one confidence interval bands (e.g. see Blanchard and Perotti (2002); Monnet (2014)). Nevertheless, to demonstrate the statistical power of the relationship between US aid shocks and output growth, we also report here the impulse responses with 95% and 99% confidence intervals.

growth and 2% increase in public employment. Moreover, the impact of aid is disproportionately larger in the region of birth of the incumbent.

This study furthers the debate on the use of foreign aid in the developing world, foremost by suggesting that aid - even when misallocated - increases incumbents' benefit of winning the upcoming elections, making public policies more efficient. It also raises the important - and understudied issue - of the interaction between aid, development and institution building in the developing world. In particular, when aid allows to increase the scope of commitment to future policies and development through redistribution, it may also prevent developing countries from building more inclusive political institutions.

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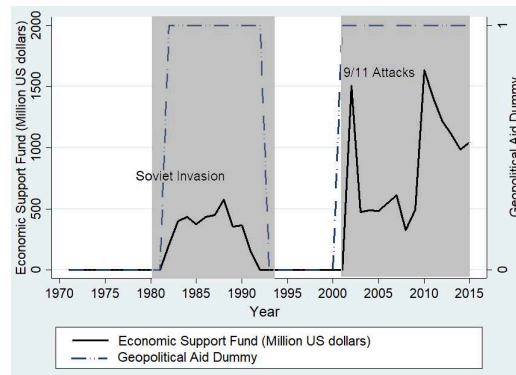
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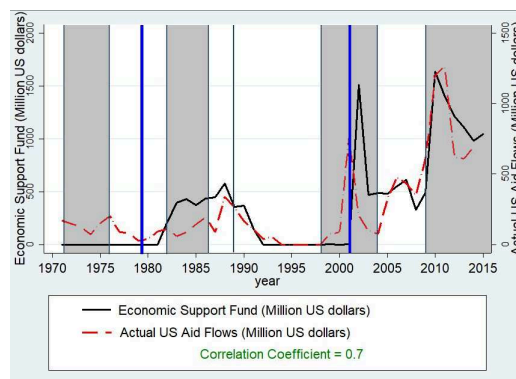
Figures

Figure 1: United States Foreign Aid to Pakistan (1971-2015)

(a) *Economic Support Fund (ESF) allocations and Geopolitical Events*



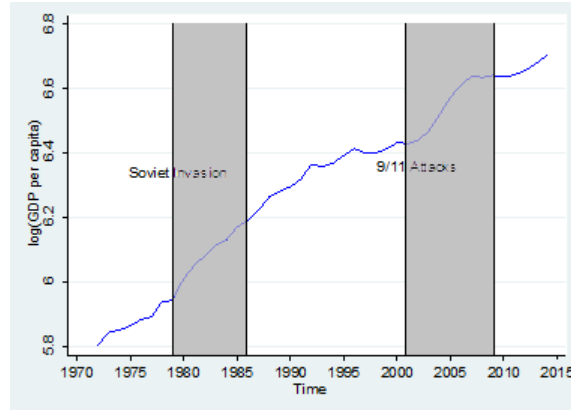
(b) *ESF aid, ODA foreign aid flows, Geopolitical Events, Changes in Aid Legislations*



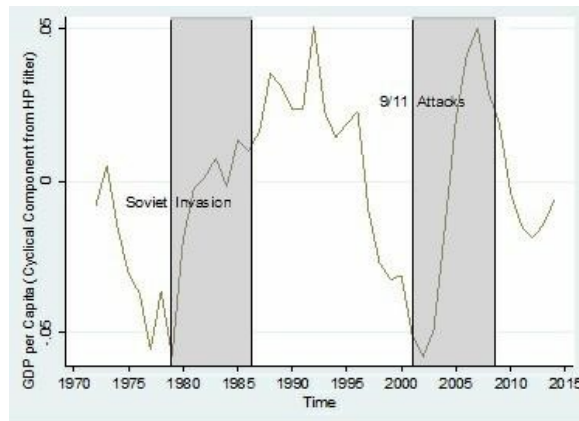
Note: The figure shows evolution of US foreign aid flows and economic support fund allocations over time. Thick vertical lines mark the geopolitical shocks of Soviet Invasion and 9/11 attacks. The thin vertical lines mark all the amendments to US Foreign Assistance Act. This includes Sparkman Amendment, Symington Amendment, Morgan Amendment, Symington Waiver, Pressler Amendment, Brownback Amendment and Kerry-Lugar Bill. Shaded areas represent periods of expansive aid legislations (Sparkman, Symington Waiver, Brownback, Kerry-Lugar Bill) in Panel B and geopolitical shocks of Soviet Invasion and 9/11 attacks in Panel A. See Table 1 and Table A in the appendix for more details.

Figure 2: Output series and Geopolitical Events

(a) *Aggregate GDP per capita series*



(b) *Cyclical Component of GDP per capita series through HP filter*



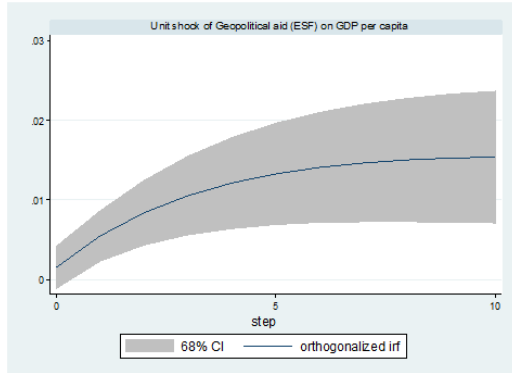
(c) *Aggregate Industrial Production Series*



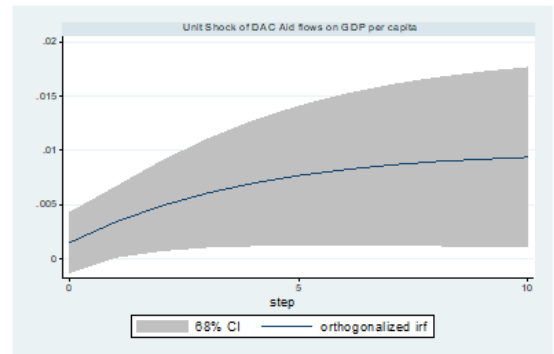
Note: The figures present evolution of output series from 1970 to 2015. Shaded areas represent time of geopolitical aid shocks following the Soviet Invasion of Afghanistan and 9/11 attacks.

Figure 3: Impact of Foreign Aid on GDP per capita (annual)

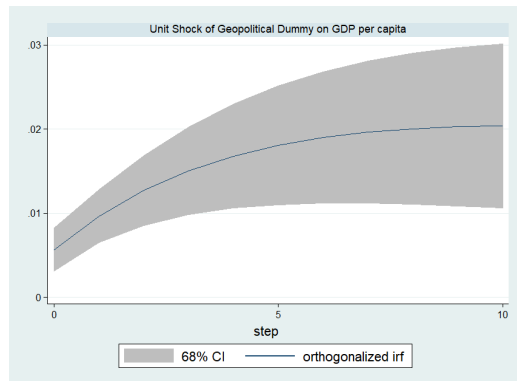
(a) *ESF Aid allocation shock on GDP per capita*



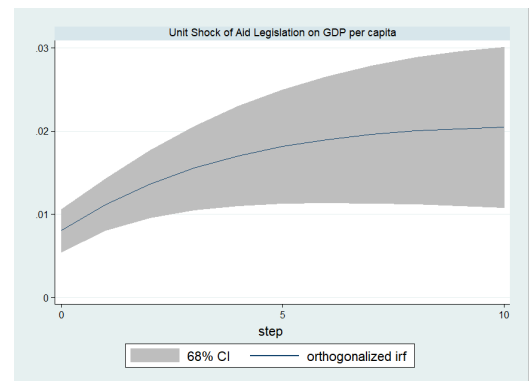
(b) *Foreign Aid flows shock on GDP per capita*



(c) *Geopolitical Aid Dummy on GDP per capita (reduced form)*



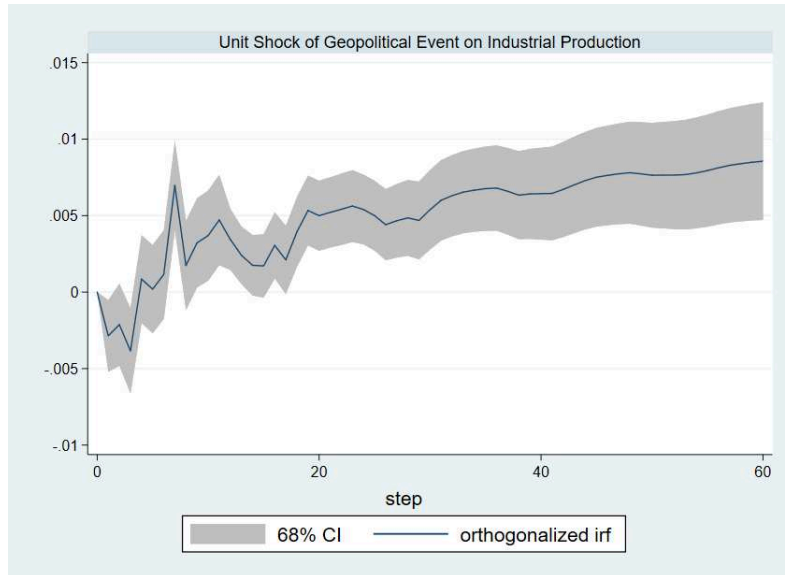
(d) *Aid Legislation Dummy on GDP per capita (reduced form)*



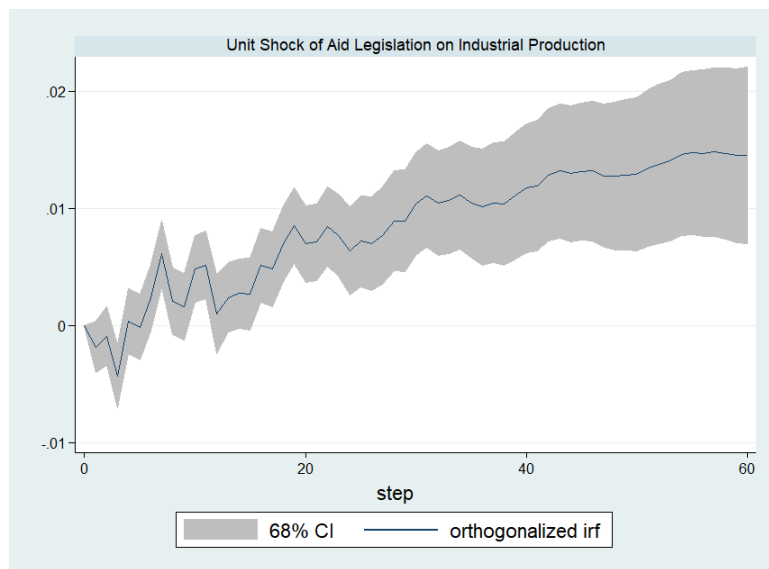
Note: The figures present the plots of impulse response functions from the baseline Narrative Vector Autoregression where the respective aid variable is shocked on GDP per capita series. The lag length selection is made according to information criteria.

Figure 4: Impact of Foreign Aid on Industrial Production (monthly)

(a) *Impact of Geopolitical Shock Dummy on Industrial Production*



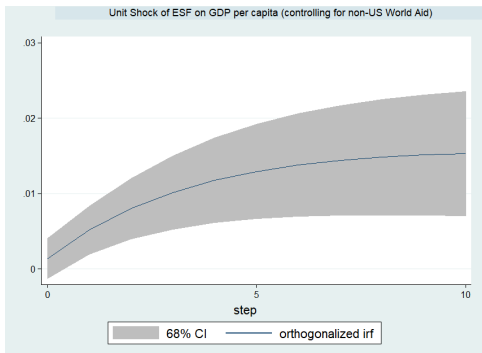
(b) *Impact of Aid Legislation Dummy on Industrial Production*



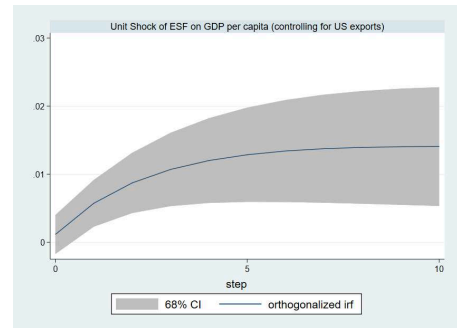
Note: The figures present the plots of impulse response functions from the baseline Narrative Vector Autoregression where the respective aid variable is shocked on monthly industrial production series.

Figure 5: Robustness to Alternative Explanations

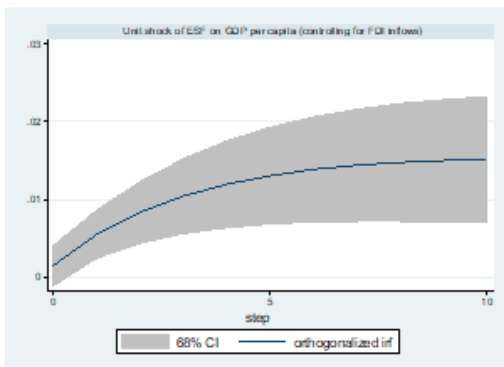
(a) *Impact of Foreign Aid on GDP per capita (controlling for non-US World Aid)*



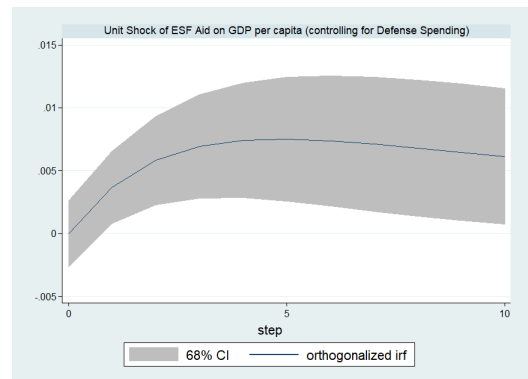
(b) *Impact of Foreign Aid on GDP per capita (controlling for US Trade)*



(c) *Impact of Foreign Aid on GDP per capita (controlling for US FDI)*



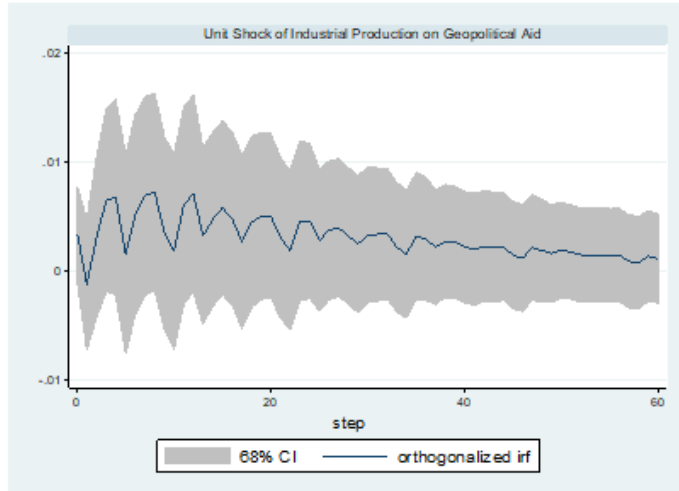
(d) *Impact of Foreign Aid on GDP per capita (controlling for Defense Spending)*



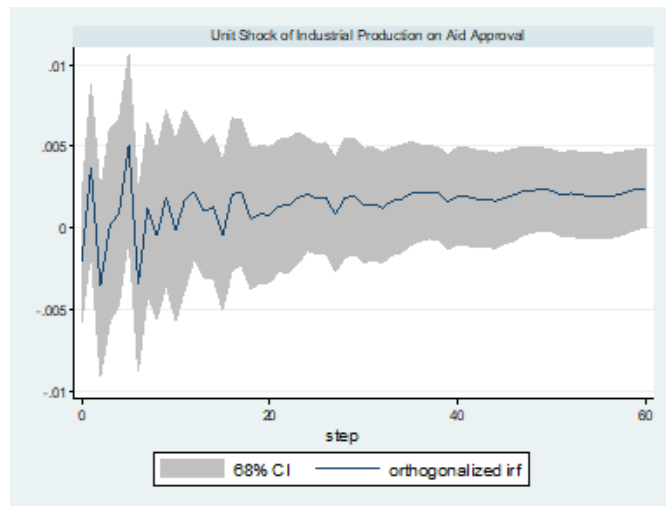
Note: The figures present the plots of impulse response functions where additional controls are added to the baseline Narrative Vector Autoregression. These includes non-US world aid, US exports, US FDI and Pakistan's defense spending.

Figure 6: Placebo Test Reverse Causality

(a) *Impact of Industrial Production on Geopolitical Aid*

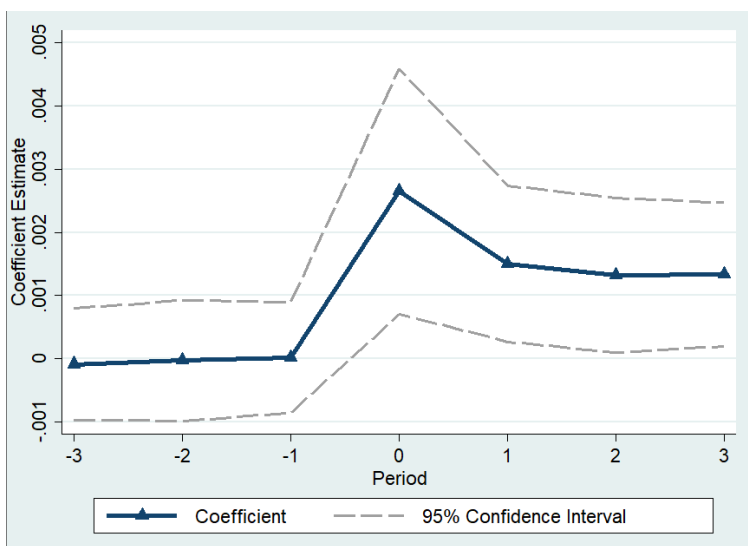


(b) *Impact of Industrial Production on Aid Legislation*



Note: The figures present the plots of impulse response functions where instead of the aid variable being shocked on the output series, the reverse is done: output series is shocked on the aid variable.

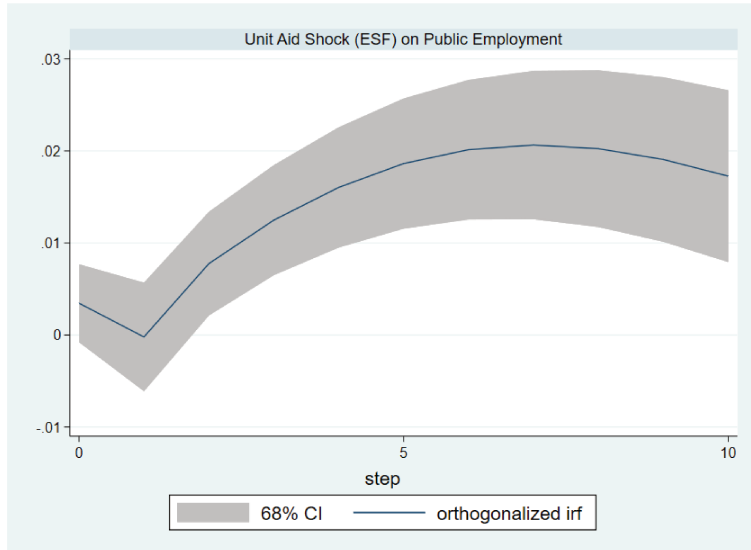
Figure 7: Plot of Coefficient Estimates in the IV regressions



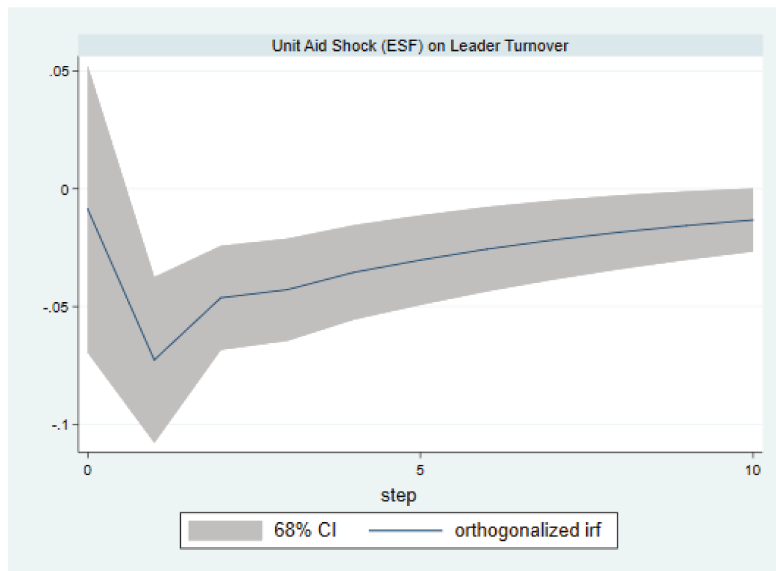
Note: The graph plots coefficients of US foreign aid flows instrumented by aid legislation dummy with leads and lags. Period 0 shows coefficient on the contemporaneous impact of aid, period 1 is one year lagged aid and so forth.

Figure 8: Effect of Foreign Aid on Public Employment and Political turnover

(a) *Impact of Foreign Aid on Public Employment*

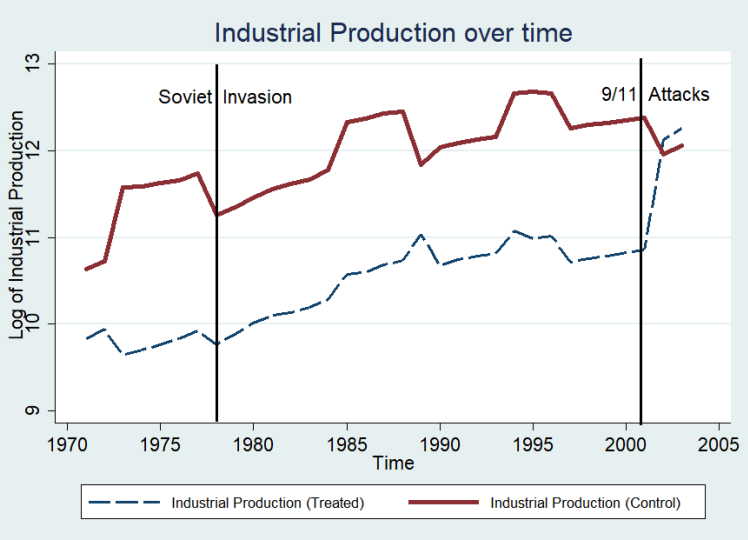


(b) *Impact of Foreign Aid on Political Turnover*



Note: The figures present the plots of impulse response functions where aid variable is shocked on public employment (Panel A) and a dummy variable that switches on when the incumbent president was no longer in office (Panel B).

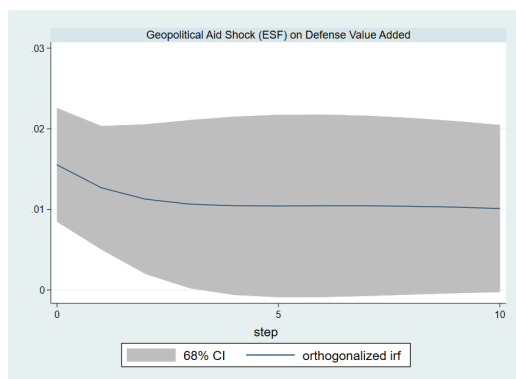
Figure 9: Industrial Production with Head of State Dummy taking the value of one (treatment) versus Head of State Dummy taking the value of zero (control).



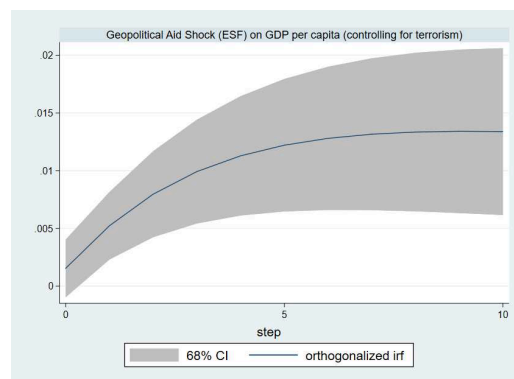
Note: The figure shows that average industrial production in the birth-province of the incumbent leader (the treatment) versus the average industrial production in the provinces where the leader is not in office (the control), follows common trends prior to the geopolitical aid shocks. Province level panel data is only available till 2004 so we are forced to terminate the analysis in the year 2004.

Figure 10: Additional Alternative Explanations and Controls

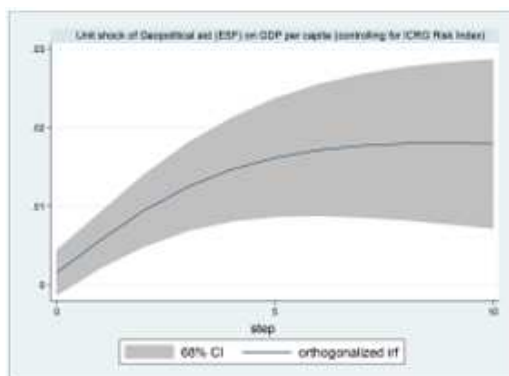
(a) *Impact of Foreign Aid on Defense Component of GDP per capita*



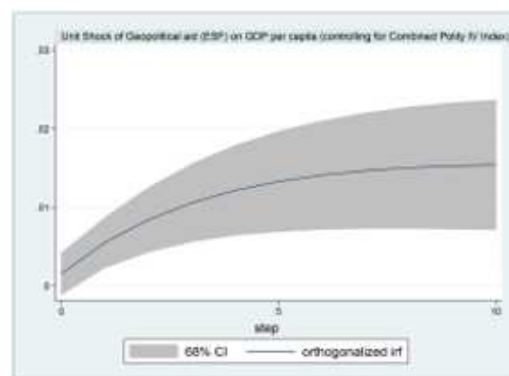
(b) *Impact of Foreign Aid on GDP per capita (controlling for terrorism)*



(c) *Impact of Foreign Aid on GDP per capita (controlling for ICRG Risk Index)*



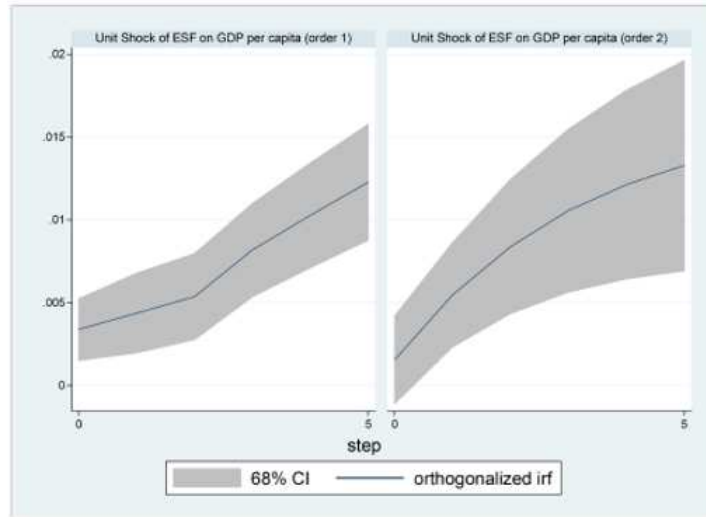
(d) *Impact of Foreign Aid on GDP per capita (controlling for Combined Polity IV Index)*



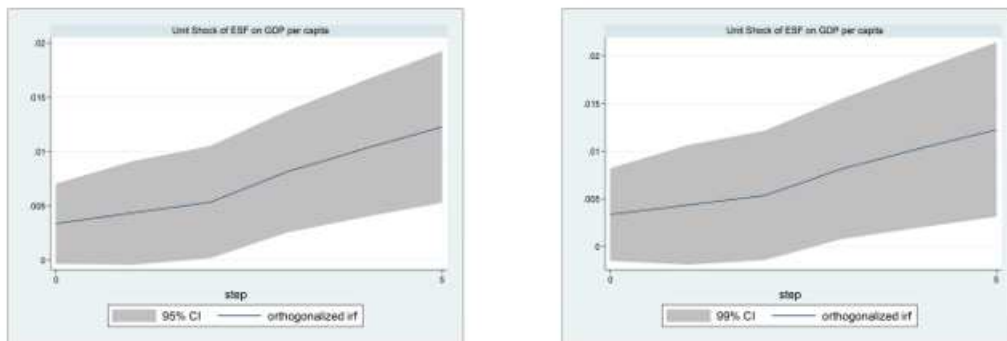
Note: The figure plots the impulse response functions with additional controls, especially controls for various political and investment ‘risk’ factors.

Figure 11: NVAR Diagnostics

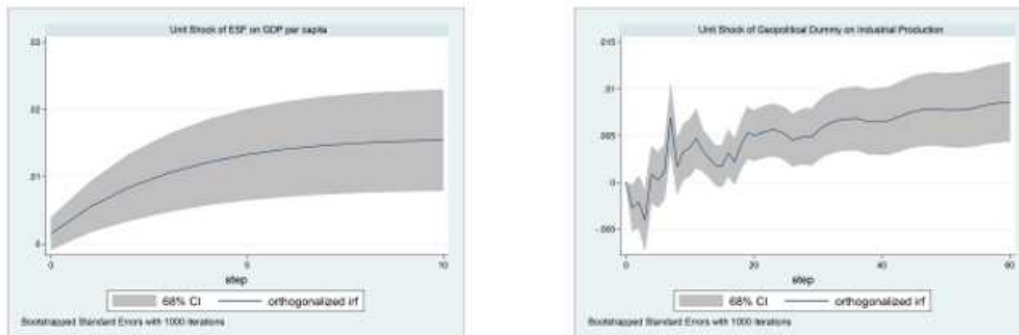
(a) Robustness to Ordering Restrictions



(b) Robustness to 2 and 3 Standard Deviation Bands



(c) Robustness to Bootstrapped Standard Errors



Tables

Table 1: Changes to Foreign Assistance Act of 1961 from 1970 to 2015

Name of Legislation	Year Tabled	Year Enacted	Passed	Aid Expansive
Sparkman Amendment	1973	1973	Yes	Yes
Symington Amendment	1976	1976	Yes	No
Humphrey Bill	1978	-	No	Yes
Morgan Amendment	1978	1979	Yes	No
Symington Waiver	1982	1982	Yes	Yes
Pressler Amendment	1985	1986	Yes	No
Fascell-Hamilton Bill	1991	-	No	Yes
Clinton Bill	1994	-	No	Yes
Brownback Amendment	1999	1999	Yes	Yes
Kerry-Lugar Bill	2009	2010	Yes	Yes

Note: The aid legislation dummy switches on in the year of enactment. Failed aid legislation switches on in year the legislation is tabled. Geopolitical Aid shock dummy for Soviet Invasion switches on from 1980 to 1985 for annual data, and 12/1979 to 07/1985 for monthly data. Likewise, Geopolitical Aid shock dummy for 9/11 attacks switches on from 2002 for annual data and 09/2001 for monthly data until the end of dataset. See Table C.1 in the appendix C.1.2 for further details on these changes in aid legislations.

Table 2: Instrumental Variable Results

<i>Log GDP per capita</i>	<i>Geopolitical Aid Instrument</i>		<i>Aid Legislation Instrument</i>		
	OLS	IV, 2 nd Stage	IV, 2 nd Stage	IV, 2 nd Stage	IV, 2 nd Stage
	(1)	(2)	(3)	(4)	(5)
L. Log Foreign Aid	0.00053 (0.00039)	0.0047* (0.0027)	0.0029* (0.0016)	0.0014** (0.00067)	0.0015** (0.00062)
Constant	0.0711 (0.0765)	-0.0348 (0.114)	0.262* (0.140)	0.0476 (0.0827)	0.228* (0.133)
Controls	No	No	Yes	No	Yes
Observations	43	43	43	43	43
R-squared	0.995	0.987	0.993	0.995	0.995

Note: Robust standard errors in parentheses. In column (2) and (3) US aid flows is instrumented by geopolitical aid shock dummy. In column (4) and (5) aid flows is instrumented by aid related legislative changes dummy. Lagged GDP per capita as well as controls added in line with the NVAR specification. One-year lag is chosen as per Bayesian and Akaike Information Criteria. Thus, estimates from this table can also be interpreted as a reduced form NVAR.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 3: Placebo Test - Impact of Failed Aid Legislations

	<i>Logarithm of GDP per capita</i>		
	<i>IV, 2nd Stage</i>	<i>IV, 2nd Stage</i>	<i>IV, 2nd Stage</i>
	(1)	(2)	(3)
L. Log Foreign Aid	0.0159 (0.0387)	-0.000953 (0.00267)	-0.00107 (0.00417)
Constant	6.032*** (0.654)	0.108 (0.0964)	0.167 (0.164)
Controls	No	No	Yes
Observations	43	43	43
R-squared	0.998	0.994	0.994

Note: In all columns, US aid flows is instrumented by failed aid legislation dummy variable that switches on when aid legislation was tabled but failed to pass (See Table 1 and Table A in the appendix). In column (1), we present simple bi-variate regression of GDP per capita and the instrumented foreign aid variable (without lagged GDP). In column (2) and (3), we estimate the underlying NVAR reduced form in the main results, without and with controls, respectively (instrumented by failed aid legislation). Robust standard errors are presented in the parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 4: A Check for the Exclusion Restriction - A Comparison of Means

Variables	No Aid Shock	Aid Shock	Difference	(p-value)
US Exports	-0.05	0.06	-0.12	0.29
US FDI	-0.10	0.08	-0.19	0.81
UK Aid	-0.00	0.00	-0.00	0.95
EU Aid	-0.06	-0.09	0.03	0.83
Military Spending	0.02	-0.01	0.03	0.44
US Aid Disbursements	-2.32	2.24	-4.56	0.00
US ESF Aid	-0.72	0.67	-1.39	0.00
Pakistan GDP	0.59	0.63	-0.04	0.00

Note: All variables (in logs) are regressed on a linear time trend to obtain their residuals, whose means are compared when Geopolitical shock dummy switched off and on, respectively. Identical results are found for aid legislation dummy switched off and on.

Table 5: Differences-in-Differences Results

	Log of the Industrial Production			
	(1)	(2)	(3)	(4)
Aid Shocks X	0.422*	0.0323**	0.0191**	0.0210**
Birth Place of HofS	(0.169)	(0.00712)	(0.00575)	(0.00646)
Head of State	-1.550**	0.0222	0.0657	0.0315
	(0.372)	(0.0271)	(0.0467)	(0.0244)
Time Fixed Effects	Yes	Yes	Yes	Yes
Province Fixed Effects	No	Yes	Yes	Yes
Province Specific Trends	No	No	Yes	Yes
Controls	No	Yes	No	Yes
Observations	140	140	140	140
R-squared	0.649	0.998	0.998	0.998

Note: Robust standard errors clustered at province level in the parenthesis. Aid Shock is a dummy variable that takes the value of 1 in geopolitical aid shock years for all provinces. Birth Place of HoS is a time and province varying dummy variable that takes the value of 1 for the province where the head of state (Prime Minister or President) was born and during her/his term in office and zero otherwise.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 6: Alternative Explanations, Additional Controls and Interactions

	Log of the Industrial Production		
	(1)	(2)	(3)
Aid Shocks X Birth Place of HoS	0.0210** (0.00646)	0.0211** (0.00644)	0.0263*** (0.00348)
Head of State	0.0315 (0.0244)	0.0315 (0.0254)	-0.524 (0.578)
EU Aid X Birth Place of HoS			0.0465 (0.0393)
UK Aid X Birth Place of HoS			0.0709 (0.0604)
US FDI X Birth Place of HoS			0.00659 (0.00426)
US Exports X Birth Place of HoS			-0.0328 (0.0331)
Defense Spending X Birth Place of HoS			-0.0199 (0.0773)
Year and Province Fixed Effects	Yes	Yes	Yes
Province Specific Trends	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Additional Table 4 Controls	No	Yes	Yes
Interaction Terms	No	No	Yes
Observations	140	140	140
R-squared	0.998	0.998	0.999

Note: Robust standard errors clustered at province level in the parenthesis. Aid Shock is a dummy variable that takes the value of 1 in geopolitical aid shock years for all provinces. Birth Place of HoS is a time and province varying dummy variable that takes the value of 1 for the province where the head of state (Prime Minister or President) was born and during her/his term in office and zero otherwise.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 7: Placebo Test

	Log of the Industrial Production			
	(1)	(2)	(3)	(4)
Head of State at t x Aid	0.0210** (0.00646)			
Head of State at t-1 x Aid		0.00736 (0.0118)		
Head of State at t-2 x Aid			-0.000743 (0.0122)	
Head of State at t-3 x Aid				-0.00742 (0.0125)
Head of State	0.0315 (0.0244)	0.0269 (0.0209)	0.0239 (0.0179)	0.0210 (0.0166)
Time Fixed Effects	Yes	Yes	Yes	Yes
Province Fixed Effects	Yes	Yes	Yes	Yes
Province Specific Trends	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Constant	-39.08 (143.9)	30.22 (311.4)	24.56 (288.2)	-5.335 (93.53)
Observations	140	136	132	128
R-squared	0.998	0.998	0.999	0.999

Robust standard errors in parentheses (clustered at province level).

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

For Online Publication: Online Appendix

Mathematical Appendix

A.1 Proof of Proposition 1

From (2.6) and (2.8), we deduce that

$$\frac{\partial^2 R}{\partial r_1^2} = v''(r_1) + P(g)v''(r_2) < 0, \quad (\text{A.1})$$

and by differentiating the first line of (2.8) with respect to a_1 , we deduce that:

$$\frac{\partial r_1}{\partial a_1} = \frac{P(g)v''(r_2)}{v''(r_1) + P(g)v''(r_2)} < 1. \quad (\text{A.2})$$

Also, by differentiating the second line of (2.8) with respect to a_1 and r_1 , we find that:

$$\frac{\partial g}{\partial a_1} = -\frac{\frac{\partial^2 R}{\partial a_1 \partial g}}{\frac{\partial^2 R}{\partial g^2}}, \quad (\text{A.3})$$

with $\frac{\partial^2 R}{\partial g^2} < 0$, so

$$\frac{\partial g}{\partial a_1} = -\frac{-2v''(r_2)P(g) + \frac{dP(g)}{dg}v'(r_2)}{\frac{\partial^2 R}{\partial g^2}} \quad (\text{A.4})$$

and

$$\frac{\partial g}{\partial r_1} = -\frac{\partial g}{\partial a_1}. \quad (\text{A.5})$$

As

$$\frac{dg}{da_1} = \frac{\partial g}{\partial a_1} + \frac{\partial g}{\partial r_1} \frac{\partial r_1}{\partial a_1}, \quad (\text{A.6})$$

We deduce from (A.2) and (A.5) that

$$\frac{dg}{da_1} > \frac{\partial g}{\partial a_1} + \frac{\partial g}{\partial r_1} = 0. \quad (\text{A.7})$$

This concludes the proof of proposition 1.

A.2 Proof of Proposition 2

By differentiating Y_1 with respect to a_1 , we find:

$$\frac{dY_1}{da_1} = 1 - \frac{dr}{da_1} - \frac{1}{2}((1-z)h - h_P) \frac{dg}{da_1}. \quad (\text{A.1})$$

$\frac{dg}{da_1} > 0$ from proposition (1).

$$\frac{d}{dz} \frac{dY_1}{da_1} Y_1 = \frac{h}{2} \frac{dg}{da_1} > 0, \quad (\text{A.2})$$

so $\frac{dY_1}{da_1}$ increases with z .

Furthermore,

$$1 - \frac{dr}{da_1} = 1 - \frac{\partial r}{\partial a_1} - \frac{\partial r}{\partial g} \frac{\partial g}{\partial a_1}, \quad (\text{A.3})$$

with $\frac{\partial r}{\partial a_1} < 1$, see the proof of proposition (1). As $\frac{\partial r}{\partial g} \frac{\partial g}{\partial a_1} < 0$, we deduce that

$$1 - \frac{dr}{da_1} > 0. \quad (\text{A.4})$$

This proves that aid tends to increase the national income by reducing the scope of rent extraction. Furthermore, it also demonstrates that for $z = 1$, $\frac{dY_1}{da_1} > 0$ necessarily holds, so there exists a $\bar{z} < 1$ such that if $z > \bar{z}$, $\frac{dY_1}{da_1} > 0$. This concludes the proof of proposition 2.

B.1 Variable Definitions and sources

Real GDP per capita: logarithm of GDP per capita deflated by GDP deflator from Central Bank of Pakistan.

Industrial Production (monthly) = logarithm of Industrial Production Index from Central Bank of Pakistan (SBP).

Head of State Birth = This variable takes the value of 1 if: a) the head of state is born in that province where head of state (president or the prime minister) b) the head of state is in office

Terrorism = Logarithm of annual terrorism deaths as coded by Global Terrorism Database rubric. The data is obtained from the [Global Terrorism Database \(2018\)](#).

ESF: logarithm of Economic Support Fund from US Green Book of Aid. The section 202 of the Foreign Assistance Act of 1971 outlines the motivation for creation of budget based Economic Support Fund through Congressional legislation as follows: The Congress recognizes that, under special economic, political, or security conditions, the national interests of the United States may require economic support for countries in amounts which could not be justified solely under chapter 1 of part I. In such cases, the President is authorized to furnish assistance to countries (under the ESF), on such terms and conditions as he may determine, in order to promote national interest of the United States. (FAA, 1971). The Economic Support Fund (ESF) promotes the economic and political foreign policy interests of the United States by providing assistance to allies (FAA, 1961).

Aid Legislation = dummy variable that takes value 1 when an expansive aid legislation is enacted zero otherwise. For more details see Table 1.

Geopolitical Shock: dummy variable that takes value 1 in positive aid shock years such as Soviet Invasion till withdrawal, 9/11 till the end of War on Terror and zero otherwise.

US Aid Flows: logarithm of bilateral aid flows from United States also referred to Official Development Assistance (ODA) in the literature, from World Development Indicators of the World Bank. Constructed by Development Assistance Committee (DAC) criteria. US Exports = logarithm of exports (in millions) to United States from Handbook of Statistics State Bank of Pakistan (various issues)

Combined Polity IV Index: This is composite Polity IV index computed in Gur et al. (2015), using the component AUTOC score and DEMOC score. The resultant combined polity scale

ranges from +10 being strongly democratic to -10 being strongly autocratic.

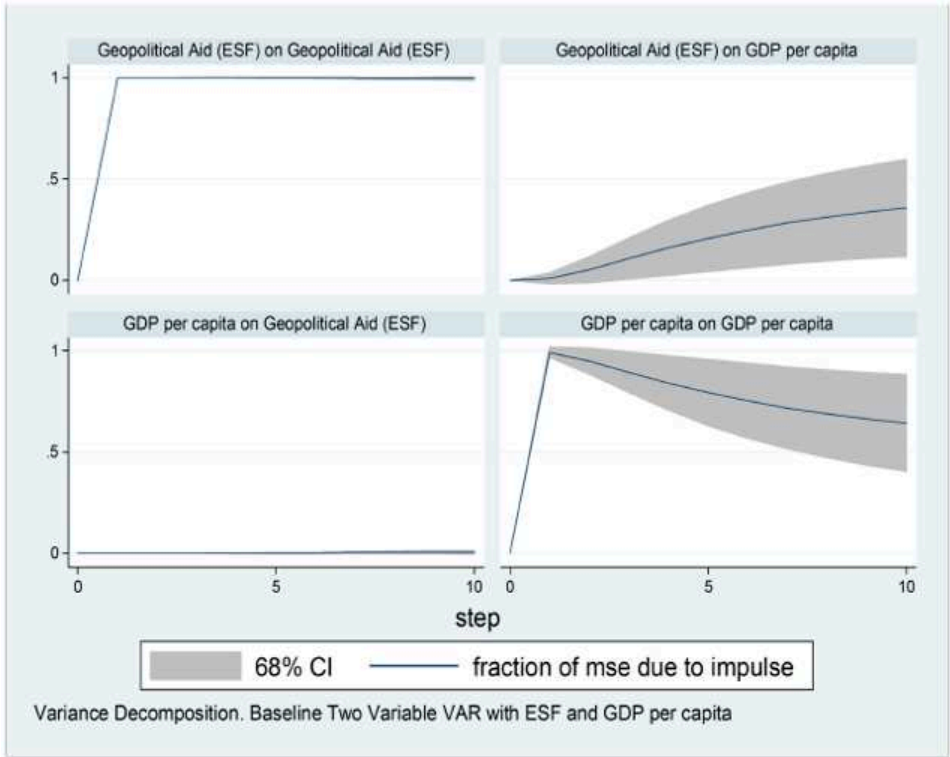
ICRG Risk Index: It is a composite risk rating of 12 risk country risk factors constructed by Political Risk Services Group (PRSG): Government Stability, Socioeconomic Conditions, Investment Profile, Internal Conflict, External Conflict, Corruption, Military in Politics, Religious Tensions, Law and Order, Ethnic Tensions, Democratic Accountability and Bureaucracy Quality Defense: logarithm of defense value added i.e. defense contribution to GDP, retrieved from [Arby \(2008\)](#).

Public Education Expenditures: logarithm of total general (local, regional and central) government expenditure on education (current, capital, and transfers). It includes expenditure funded by transfers from international sources to government. It refers to the current operating expenditures in education, including wages and salaries and excluding capital investments in buildings and equipment. Retrieved from UNESCO Institute of Statistics (UNESCO, 2016).

C.1 Additional Tables and Figures

C.1.1 Figures

Figure C.1: Variance Decomposition of baseline NVAR



C.1.2 Tables

Table C.1: Description of Changes to the Foreign Assistance Act

Name of Legislation	Description
Sparkman Amendment	Increases overall aid budget of the United States. Makes special appropriations for strategic partners (Egypt, Israel and Pakistan).
Symington Amendment	Sanctions against all countries that attempt to acquire a nuclear weapon.
Humphrey Bill	This bill attempted to increase the focus of foreign assistance on development, streamline bilateral and multilateral US aid.
Morgan Amendment	This bill curtailed foreign aid to all countries that were involved in gross human right violations.
Symington Waiver	This legislation declared that Symington amendment induced aid restrictions did not apply to strategic allies of United States.
Pressler Amendment	This legislation mandated that all US aid to its strategic partners is to be halted, unless the US President certified that the country did not possess a nuclear device.
Fascell-Hamilton Bill	The bill attempted to reduce certification requirements and micromanagement of foreign aid.
Clinton Bill	The bill attempted to increase aid for democracies and bring aid allocation under one umbrella.
Brownback Amendment	The legislation gave the US President, the executive authority to waive aid sanctions on Pakistan and India.
Kerry-Lugar Bill	Assistance Package for Pakistans support in War on Terror.

Note: All the above amendments to the Foreign Assistance Act were enacted except for Humphrey, Fascell-Hamilton and Clinton Bill, which failed to pass both houses of Congress.

Table C.2: Province-specific dummy that switches on when head of state is in power

Year	Prime Minister	President
1970-1971	5	1
1971-1973	5	2
1973-1977	2	1
1978-1985	-	5
1985-1988	2	1
1988-1990	2	3
1990-1993	1	3
1993-1996	2	1
1997-1999	1	1
1999-2002	1	2
2002-2004	4	2

Note: 1 represents Punjab, 2 is Sindh, 2 is Khyber Pakhtunkhwa (previously) NWFP, 4 is Baluchistan and 5 is birth outside Pakistan e.g. British India. The dummy switches on when either the Prime Minister or the President from the province is in office. The entry for Prime Minister for 1978-1985 is missing due to banning of political parties in the martial law regime of General Zia-ul-Haq.

Conclusion and Limitations

In this dissertation, we studied judicial independence and development. In the first chapter we examined how the institution of Presidential appointment exerts considerable influence over judicial decision-making in Pakistan. In particular, we demonstrate that the change in selection procedure of judges from Presidential appointment to appointment by a judicial commission significantly reduces State Wins and this results in better quality judicial decisions. The identification strategy, relying on mandatory retirement age, allows us to obtain the causal effects of the reform. We also connect the reform to reduction in expropriation risk in the housing sector where we provide suggestive evidence that the reform reduced distortions in the housing market. Nevertheless, there are some limitations to this study. For instance, data availability implies that we have limited information on development outcomes at the district level to directly examine the impact of the selection reform on development. Likewise, not enough time has elapsed for all the judges appointed by the President to be replaced by those appointed by the judicial commission. The sample terminates in 2016 when about 75% of the judges appointed by the President are replaced by judicial-commission judges, therefore, we are unable to estimate the long-run impact of the reform. Going forward as more data becomes available and 100% of Presidential judges are replaced by judicial-commission judges, we can better ascertain the long-run effects of the reform.

In the second chapter of the dissertation, we studied independence of judiciary from religious authorities instead of independence from the executive. Specifically, we show that districts where historically the shrine density was high, a military coup in 1999 induced a large decline in judicial independence and quality of judicial decisions. The evidence we present is consistent with the mechanism that increased political power of religious leaders associated with the shrines allowed them to influence the courts. We also connect this with the previous chapter by showing that the judicial selection reform reduces the effect of religious leaders on judicial outcomes. There are, however, several limitations to this study. First, in this chapter we only provide indirect evidence on how shrine leaders impact judicial outcomes. That is, we observe the impact of shrine density only in decentralized districts (only in districts where local elections took place) and in cases involving disputes with the local government (and not with the federal or provincial government). One promising extension may be to study the fraction of shrine elites elected before and after the coup. This is feasible, since election data gives us information on the

name of the winner that includes the title of the shrine elites (“Makhdoom”). This gives us information on whether the elected leaders are shrine elites or not. A natural question, corroborating the mechanism presented here arises: Do districts where more shrines elite became mayors have more rulings in favour of the State? Likewise, further analysis on the type of cases driving the results may reveal important insights on the mechanisms. We observe the effect of religious leaders on judicial decisions comes from more rulings in favour of the local government in land expropriation cases. This gives rise to question: Why would shrine elites expropriate more land than non-shrine political elites? This question may be answered collecting more information on the shrine elites. For instance, the shrine elites may be poorer than landed political elite to begin with, implying a “wealth effect” incentivising the shrine elites to expropriate more land. Therefore, shrine elite may have a larger incentive to expropriate more private property than the non-shrine political elite. Likewise, shrine elites might not face the same re-election incentives that non-shrine elites face. This may be due to the revered status of shrine elites that allows them to have a ‘captive vote bank’. Going forward, I hope to study these possibilities and disentangle the channels.

The third chapter of the dissertation studies the political economy of foreign aid and development. A theory of foreign aid is presented that shows how foreign aid has positive effect on economic growth but also funds patronage and favouritism. We present causal evidence consistent with the predictions of the model by focusing on the case of US foreign aid to Pakistan. In particular, we show that foreign aid is misallocated when political institutions are weak. Foreign aid, however, may also increase the dynamic efficiency of public policies, making the effect of foreign aid on growth ambiguous. We show the later effect dominates since foreign aid not only increases misallocation but also increases dynamic efficiency of public policies by reducing leader turnover. There are, however, some important limitations to this study. As is well recognized in the literature, the data from aid recipient countries is often patchy. Most of analysis is based on yearly time series data, presenting evidence against alternative explanations and identification strategy we propose might not be able to disentangle general time-series trends from the impact of foreign aid on development. Similarly, many US aid shocks to Pakistan are highly correlated with geopolitical shocks (9/11 attacks, Soviet invasion of Afghanistan). Nevertheless, these geopolitical shocks may have direct impact on geopolitical equilibrium in the aid recipient country. We present some evidence against this possibility; however, it is still possible that the effect of foreign aid may be confounded by the direct effects of geopolitical shocks.

RÉSUMÉ

L'indépendance du système judiciaire est considérée comme un élément clé de chaque société démocratique moderne. Dans ma thèse *Essais sur l'indépendance judiciaire et le développement*, je me concentre sur les chocs sur l'indépendance du pouvoir judiciaire au Pakistan et, notamment, j'examine son impact sur la prise des décisions des juges et, en gros, sur le développement du pouvoir judiciaire. Dans le premier chapitre de ma thèse *Judicial Independence and Development: Evidence from Pakistan*, je montre comment l'institution de la nomination présidentielle a un impact sur l'indépendance et le développement judiciaire. Dans le deuxième chapitre *Dictateur, Imam et Juge : retraçant l'impact de la religion sur les tribunaux*, je documente un impact considérable des chefs religieux sur l'indépendance judiciaire au Pakistan. Le troisième chapitre *L'économie politique de l'aide étrangère* présente une nouvelle stratégie d'identification et une nouvelle approche théorique sur l'économie politique de l'aide étrangère et du développement.

MOTS CLÉS

Indépendance judiciaire, institutions, tribunaux, développement

ABSTRACT

Independence of judiciary is considered a key ingredient for any modern democratic society. In my dissertation, *Essays on Judicial Independence and Development*, I study shocks to the independence of judiciary in Pakistan and examine its impact on judicial decision making and development. In the first chapter, *Judicial Independence and Development: Evidence from Pakistan*, I show how the institution of presidential appointment impact judicial independence and development. In the second chapter, *The Dictator, Imam and the Judge: Tracing the Impact of Religion on the Courts*, I document how religious leaders impact judicial independence in Pakistan. In the third chapter, *The Political Economy of Foreign Aid*, a new identification strategy and theory is presented on the political economy of foreign aid and development.

KEYWORDS

Judicial independence, institutions, courts, development