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Judicial Capture

BY SULTAN MEHMOOD AND BAKHTAWAR ALI^{1*}

1 September 2023

We use data from Pakistan to establish a reciprocal exchange relationship between the judiciary and the government. We document large transfers in the form of expensive real estate from the federal government to the judiciary, and reciprocation in the form of pro-government rulings from the judiciary to the federal government. Our estimates indicate that the allocation of houses to judges increases pro-government rulings by 50% and reduces decisions on case merits by 40%. The allocation also incurs a cumulative cost of 0.03% of GDP to the government. However, it allows the government to expropriate additional land worth 0.2% of GDP in one year. The results suggest that such reciprocative exchanges within the state undermine the rule of law. (*JEL D72, D73, P16*)

Keywords: law, reciprocation, corruption, Pakistan, judges.

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“When plunder becomes a way of life in a society, over the course of time they create for themselves a legal system that authorizes it and a moral code that glorifies it.”

Justice Athar Minallah.²

I. Introduction

The judiciary traditionally acts as a check on governments (Madison, Hamilton and Jay, 1788; Montesquieu, 1748). Yet, in many countries, including consolidated democracies, judiciaries seem to be abdicating their crucial role as a check on executive power (Acemoglu et al., 2013). One reason for this abdication of responsibility may be judicial capture by the government. This can limit political accountability, retard economic growth, and foster the rise of autocracies (Persson et al., 1997; La Porta et al., 2003; Glaeser and Goldin, 2006; Voigt et al., 2015; Behrer et al., 2021).

Given the significant implications of the state's capture of judiciaries, it is crucial to ask two fundamental questions: How does the State manage to capture the judiciaries that are meant to keep executive power in check? What facilitates this judicial capture by the government? While it is a well-established fact that governments grant favors to state officials (Mian et al., 2010; Xu, 2018; Colonnelli et al., 2020), the mechanics of the sensitive relationship between the government and judiciary, which in principle are meant to be independent from each other, remain inadequately understood.

This paper presents quasi-experimental evidence of how granting favors to the judiciary by the government affects pro-government rulings and decision-quality. It does so by utilizing a unique natural experiment: the staggered allocation of real estate to judges in Pakistan. By combining case-level micro data on judicial decisions and real estate allocations in a generalized difference-in-differences empirical strategy, we are able to examine the complete transactional relationship between the judiciary and the government, including the transfer of real estate from the federal government to judges and the awarding of pro-government rulings from judges to the federal government. We find that allocation of houses to judges in Pakistan increases pro-government rulings and reduces decisions on merits of a case. The evidence suggests that judges

²Quoted from Justice Minallah’s court order barring the Federal government from acquiring additional land to allocate houses to judges.

reciprocate the federal government's allocation of real estate by rendering decisions that are more favorable towards the government and less based on case merits.

The study makes use of the real estate allocation system established by the Pakistani government under the "Prime Minister's Assistance Package". This initiative, which involves the distribution of high-end properties to senior officials within the Pakistani bureaucracy, is overseen by the federal government of Pakistan. Our analysis employs a generalized difference-in-differences research design, leveraging the staggered distribution of real estate to judges as a means of identifying variation. Under the parallel trends assumption, the generalized difference-in-differences design allows us to address several empirical issues that could otherwise hinder our ability to interpret the results as causal. To begin with, we can account for the possibility that the outcomes are influenced by time-invariant differences in pro-government rulings by judges. It is possible that some judges may be more likely to receive real estate based on their fixed characteristics such as their gender or the law school they went to, but by incorporating judge-fixed effects, we can rule out such concerns. Furthermore, we can dismiss the notion that our results are driven by court decisions that evolve over time in a common manner across judges in different periods. This is because certain political shocks, such as national elections, may have a specific effect on the rulings of all judges, ultimately influencing their judgments. However, by incorporating year fixed effects, we can account for these potential influences. Lastly, we can exclude the possibility that different cases appear in different courts at different times: certain courts may become more congested over time, leading to significant differences in their rulings from other district courts in a particular year. But by utilizing court-by-year fixed effects, we can eliminate these concerns.

Our main finding is when judges were allocated houses by the federal government, ruling in favor of the federal government increased. House allocation raised pro-government rulings by about 25 percentage points. This is equivalent to a 50% increase in government victories over the sample mean. Rulings in favor of the provincial or local government remain unaffected. As a point of comparison, this magnitude is roughly equivalent to the impact of replacing all judges in Pakistan with Presidential appointees instead of the current method of judicial selection by judge peers (Mehmood, 2022). We also find that rulings in favor of the government exclusively concerned cases involving the federal government. Since house allocation goes through the

federal government and must be “approved” by the Prime Minister, these are precisely the cases where we would expect incentives for reciprocation to be high. We highlight two additional results. We find that, on average, the real estate allocation decreased rulings on case merits. Our quantitative analysis of decision quality leverages legal experts’ evaluation of case quality: judgment texts are examined by a 5-member panel of attorneys to assess whether the decision is based on the cases’ legal merits.³ The results of this analysis suggest that the allocation of houses to judges also reduces the probability of cases being judged based on evidence or merits. Specifically, the house allocation reduces decisions on case merits by about 40%. These findings are consistent concerns raised by civil society in Pakistan that house allocation deteriorates judicial decision quality and the rule of law (Sattar, 2017). Lastly, we estimate the value of the additional land expropriation that occurs as a result of the “houses for judges” system. According to our computations, the system of house allocation results in a cumulative cost to the government of approximately 0.03% of GDP, based on the total market value of the allocated houses. However, the system also allows the government to expropriate additional land worth 0.2% of GDP each year. To put this amount into perspective, it is roughly equivalent to 10% of what the Pakistani government spent on education in 2019 (World Bank, 2019).

Our empirical strategy rests on the parallel trends assumption. One may worry about the plausibility of the parallel trends assumption in our setting. That is, it may be that judges who were allocated real estate may be on different pro-government rulings trends than those who were not. We speak to this concern in three ways. First, we estimate a fully dynamic version of the two-way fixed effect model and check for potential pretrends. Second, we explore the existence of differential trends prior to the house allocation by estimating fully dynamic versions of alternative estimators introduced in Borusyak, Jaravel, and Spiess (2021); Callaway and Sant’Anna (2021); and Sun and Abraham (2021). Results from all these estimators provide evidence consistent with parallel trends. This is also in line with anecdotal accounts suggesting that judges are generally unable to predict their house allocation status (The News, 2017). Third,

³In Common-Law jurisprudence, a ruling “on evidence or merits” means that the judicial decision is “based on evidence rather than technical or procedural grounds” (Pound, 1963). This dummy variable is constructed from a reading of the text of the judgment order by two independent teams of five attorneys each within a law firm (see Appendix A for more details). Anecdotal accounts from Pakistan also suggest that ruling on technicalities or legal lacunas is a “weapon of choice to rule unfairly” (Haq, 2018) and that judges use decisions on technicalities to “favour the state authorities” (Arshad, 2017)

to the extent that such trends are evolving at the court-by-year level, we are able to account for them by including court-by-year fixed effects. Last, anecdotal evidence further bolsters these quantitative findings, as it suggests that judges find it challenging to predict their house allocation status (The News, 2017). In all these settings, the evidence appears to not contradict the parallel trends assumption.

Finally, our empirical approach rests on the homogeneity of treatment effects across units and time to deliver consistent estimates. We speak to this identification challenge by showing that our results survive estimation by robust estimators that do not require the homogeneity assumption. These estimators shut down the 2x2 difference-in-differences comparisons between newly treated and already treated units to deliver consistent estimates (Borusyak, Jaravel, and Spiess, 2021; Callaway and Sant’Anna, 2021; Goodman-Bacon, 2021; Sun and Abraham, 2021). Across these robust estimators, the coefficient estimates on the impact of house allocation remain qualitatively and statistically similar. Goodman-Bacon decomposition of the estimates also indicates the “forbidden comparisons” of newly treated with already treated as controls do not drive our results (Goodman-Bacon, 2021).

This paper speaks to at least three strands of the literature. Principally, it relates to the literature on institutions and development, particularly the studies emphasizing the importance of checks and balances on executive power (North, 1990; La Porta et al., 1998; Shleifer and Vishney, 2002; Acemoglu et al., 2002; La Porta et al., 2003; Rodrik et al., 2004). We contribute to this literature by showing how reciprocation of favors by the judges may “increase expropriation risk” (Acemoglu et al., 2001, p. 1371) by increasing state victories in land expropriation cases against the citizenry. We hence provide empirical support for the theory and mechanisms behind many of these seminal studies. This paper is also related to recent studies that investigate the impact of the Presidential appointment of judges in Pakistan on the rule of law (Mehmood, 2022) and long-run economic development (Mehmood, 2023). The paper extends these works in two important dimensions (a) it provides evidence of a reciprocal relationship between the government and the judiciary, and (b) show how unlegislated and unwritten rules, unlike the Presidential appointment of judges in the previous work, may also undermine the independence of the judiciary.

Second, we speak to the literature on state capacity and public sector effectiveness (Besley and Persson 2010; Manacorda et al., 2011; Finan et al., 2017; Xu, 2018; Acemoglu et al., 2020; Bandiera et al., 2020; Callen et al., 2020; Colonnelli et al., 2020). We advance suggestive evidence of judicial capture by the government (Acemoglu et al., 2013)⁴ and document a reciprocative exchange relationship between the federal government and the judiciary. While past studies have focused on the reciprocal exchange of favors between politicians and donors (Colonnelli et al. 2020) and between regulators and special interest groups (Mian et al. 2010), we document the reciprocal transaction between the judiciary and the government. More importantly, however, the judiciary's de jure requirement for independence sets it apart from other government branches, since it must safeguard individual rights and serve as a check and balance to other branches of the State. Consequently, the exchange of favors with other state institutions becomes a significant concern. Last, we contribute to the literature on the legal origins of economic development (La Porta et al., 1998; Glaeser and Shleifer, 2002; La Porta et al., 2008; Anderson et al., 2018). Our contribution to this literature is two-fold. First, we show that the protections provided in common law for private economic interests are weakened when rules and norms allow for reciprocal exchange between the government and the judiciary. Second, we provide an estimate of the costs and benefits – to the government – of favor exchange to a key organ of the State, the judiciary.

The paper is structured as follows. Section II provides a brief overview of the background followed by Section III, which describes the data. In Section IV, we outline the empirical methodology employed in our analysis, and the results are presented in Section V. A final section concludes. A detailed description of the data and their sources, as well as variable definitions, and robustness analysis of our results, can be found in an online Appendix A. Additional figures and tables from the robustness analysis are reported in the online Appendix B.

II. Background

Context.—In many vulnerable democracies, powerful institutions such as the military and the judiciary wield their influence to establish institutionalized perks for their respective institutions. This is particularly prevalent in Pakistan, where both the military and the judiciary have set up

⁴Acemoglu et al, (2013) present a series of case studies showing that judicial capture by governments is common worldwide.

major housing schemes for their top cadres (Siddiqa, 2017). This study investigates one such housing scheme for judges. The existence of such transfers by the government is a closely guarded secret, with qualitative accounts reporting that even some judges learn of it only after assuming office.⁵ However, a high-profile media leak occurred when a prominent newspaper published the names of judges who had received real estate from the Prime Minister as part of the "Prime Minister's Assistance Package" (Express Tribune, 2010), prompting a public call for an inquiry into the house allocation system. The Prime Minister's Assistance Package was established in 1989, which created a government department responsible for allotting residential land to public servants. It was initially called the "FGE Housing Foundation", and was created as a department within the Ministry of Housing and Works. The department operated for a long-time without any legal cover from the legislature. It was only after repeated litigation by citizens that the President of Pakistan signed an executive order on July 12, 2019, giving it some official backing from the Government (Ministry of Housing and Works, 2022). Many commentators, including former judges, called the allocation of houses to judges "corruption". One newspaper ran the headline "daylight robbery", while the Islamabad High Court Chief Justice termed it "plunder", and a Supreme Court judge called it an "unlawful favor" (Justice Faez Isa quoted in Sattar, 2017). More fundamentally, doling out favors by the government to the judiciary is problematic because the judiciary is crucially different from other branches of government. One fundamental characteristic is its intended independence, which sets it apart from entities such as the bureaucracy or the legislature that at some level are answerable to the executive. Unlike the legislature, which is typically bound to the executive even in majoritarian systems, the judiciary operates with a degree of autonomy. This independence enables the judiciary to act as a check and balance on the other branches of government, ensuring the protection of individual rights and the interpretation and enforcement of laws. Given this unique position, the exchange of favors between the judiciary and other state branches becomes a matter of important consideration.

The Procedure.—The precise method of assigning houses to judges remains veiled in secrecy. Our interviews with former judges and government officials indicate that Islamabad's

⁵ "Justice Jawwad Khawaja mentioned that soon after his elevation to the Court, the registrar sent him an application for a residential plot. Justice Khawaja inquired whether the granting of a plot was mentioned in the constitutional provisions determining the entitlement of judges. He was told it was not." (The News, 2017).

Capital Development Authority (CDA) allocates residential plots to judges through the Federal Ministry of Housing, upon written request from the Prime Minister to the federal ministry. The judge was required to apply for a house and to pay a non-refundable fee of PKR 500 or USD 2.10. If the Prime Minister approves the application, the judge is allotted a 4500-square-foot residential plot in a central location in Islamabad called the Judicial Colony. Importantly, however, the governmental allocation entitles the judges to full legal ownership of the property, to live in it, rent it or sell it (Abbasi, 2017). The colony of judges' houses in Islamabad is one of the most valuable pieces of residential real estate in the whole country. The average value of a house allocated is about USD 400,000 (Zameen, 2019), equivalent to roughly 80 times the High Court judges' monthly salary or about 7 times their annual salary..

III. Data

Our empirical analysis leverages data on judicial cases from case archives at the Registrar Offices of the High Courts. We randomly sample about 8600 cases from 1986-2019 for all High Courts in Pakistan (from the universe of cases decided in this period) conditional on the State being one of the parties. This is 0.2% of the total cases available at the High Courts. This is combined with the list of judges receiving real estate from a dataset obtained from the Public Accounts Committee of the Pakistani parliament.⁶ Table B1 presents the descriptive statistics of the variables used in the paper, while Figure B1 shows the distribution of pro-government rulings, our main outcome variable of interest.⁷ Out of the 446 judges included in our full sample, 224 of them were allocated real estate. The key outcome and explanatory variables are detailed below. Further explanations of the data and their sources, including a full description of the variables, can be found in Appendix A.

State Wins.—Our key outcome variable is State Wins. This is a case-level measure of government favoritism constructed from the text of the judgment order containing details on the

⁶ The opaque nature of transactions under the Prime Minister Assistance Package did not deter scrutiny of the allocation of real estate to superior court judges, following a 2010 media leak (Express Tribune, 2010). Our study supplements the leaked list with a more comprehensive roster obtained from the Public Accounts Committee of the Pakistani parliament, covering the 1990-2019 period.

⁷ Figure B2 illustrates the random sampling process of about 8,600 cases, which were selected from the entire population of district High Courts in Pakistan. These cases were sampled across all years from 1986 to 2019, with the sampled cases representing approximately 0.2% of the total cases decided during this period.

case. Following the literature (e.g. Djankov et al., 2003), we asked attorneys at a law firm to code this variable. The lawyers at the law firm coded the “State Wins” dummy variable as one if the State won the dispute and zero in case of government defeats in all cases where the State is one of the parties. The State here includes all organs of the state yielding executive power, such as local, provincial, and federal governments (in line with the conceptualization of the State as an executive organ in Montesquieu, 1748). The State tends to win roughly half of the cases on average. A graphical representation of this variable's distribution can be found in Figure B1 of Appendix B.

Merit.—To analyze the quality of judicial decisions, we use an additional outcome variable: Merit. This measure of decision quality 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). There are two reasons for constructing this variable. First, legal scholarship in Pakistan argues that ruling on technicalities is a “weapon of choice to rule unfairly” and that judges use decisions on technicalities to “favor the State authorities” (Aziz, 2001) and such rulings are “symptomatic of a biased decision” (Arshad, 2017). Therefore, we proxy the “correctness” or unbiasedness of a judicial decision by this dummy variable. Second, this variable is consistent with Common Law jurisprudence, which aspires toward rulings on merits, i.e. based on evidence and the spirit of the Law rather than legal technicalities (Pound, 1963, and Tidmarsh, 2009 discuss this in detail).

House Allotted.—The main explanatory variable in our study is House Allotted, which is a binary variable that turns on for a particular judge in the year when the judge receives a house. The variable is obtained from a list obtained from the Public Accounts Committee of the Pakistani parliament. This committee within the Pakistani legislature is responsible for examining public audits, summoning ministers and public officials to testify before the committee, and publishing their findings for the benefit of the public. We were able to match the complete list of 224 judges who received real estate in this roster among the 446 judges in case-level database.

Control Variables.—We use all available time-varying case characteristics as control variables in the regressions. The case characteristics are also obtained from judgment texts and include the following variables: number of pages of judgment order, presence of chief justice on

the bench, number of judges on the case, number of lawyers on the case, and dummies for the type of cases. Additional information on variable definitions, construction, and sources can be found in Appendix A.

IV. Empirical Methodology

Empirical Strategy.—In this paper, our main aim is to investigate the impact of real estate allocation on pro-government rulings. A simple correlation may be plagued by severe endogeneity concerns. Examples of such endogeneity concerns include reverse causality (e.g., judges who are already friendly towards the government may be more likely to receive real estate) and omitted variable bias (e.g., politically connected judges may be both more likely to receive houses and have higher pro-government rulings). To obtain estimates that can be more credibly interpreted as causal, we leverage the staggered allocation of real estate across Pakistani judges over time. Under parallel trends, the quasi-experimental variation generated by the staggered house allocation allows for estimation of impact of real estate on pro-government rulings. As a baseline specification, we estimate the following difference-in-differences model with judge, year and court-by-year fixed effects:

$$(1) \quad \Delta_{it} = \alpha_i + \delta_t + \lambda_{it} + \beta \times \text{Real Estate}_{it} + \mathbf{X}_{it} \times \gamma + \epsilon_{it}$$

where Δ_{it} denotes judicial decision for judge i in decision year t . α_i and δ_t are judge and year fixed effects, respectively. λ_{it} is the court-by-year fixed effect. \mathbf{X}_{it} is a vector of time-varying case-characteristics that we add as controls. This includes the type of case (e.g., constitutional, land, or criminal), the number of pages in the judge's order, the presence of the state chief justice in the judicial bench, and the number of judges and lawyers involved in the case. As baseline, we estimate equation (1) using ordinary least squares (OLS) at the case level regression and cluster standard errors at the judge-level.⁸

⁸ When using such designs, Chen et al. (2016) propose clustering standard errors at the decision-maker level, which is what we do as a baseline. The results, however, are robust to alternative clustering methods (Table B7 in

V. Results

Difference-in-Differences.—In this study, we examine whether judges who are allocated real estate by the federal government tend to issue rulings that favor the federal government. We estimate β for state victories using equation (1) and present these results in Table 1. In the first four columns of Table 1, we progressively estimate a more saturated specification for disputes involving the federal government. The results from Columns 1 to 4 indicate a significant and meaningful effect of house allocation on government victories in cases against the federal government, despite the relatively smaller sample size compared to cases involving local or state governments. Specifically, the house allocation increases pro-government rulings in cases involving the federal government by approximately 25 percentage points, a 50% increase compared to the sample mean. In Columns 5 and 6 of Table 1, we further estimate cases involving disputes with other government entities, specifically local and provincial governments. The coefficient estimates are statistically insignificant and close to zero, suggesting that rulings for the federal, not local or state governments, play a more pivotal role in explaining our results. Henceforth, our paper concentrates on cases that pertain to the federal government.

Event Study.—In order to partially test for parallel trends and study the dynamic effect of house allocation, we estimate an event-study version of the TWFE model with indicators for time to and from the actual house allocation. Specifically, we estimate the following specification:

$$(2) \quad Y_{it} = \alpha_j + \delta_t + \beta D_{itk} + \epsilon_{it}$$

where Y is government victories and D is a set of indicator variables that take the value one if the house allocation was k years away. The subscript " k " serves as a time index that represents the year of the judicial decision. It indicates the years from the house allocation to judges when the judicial decisions were observed. For example, when " k " is equal to $+2$, it corresponds to judicial decisions observed two years after the house allocation. α_j and δ_t are judge and year

Appendix B), including multi-way clustering at the judge-by-year level or the most conservative court-level clustering. We also applied wild-bootstrap with small sample correction, as suggested by Angrist and Pischke (2009) for cluster size less than 42. This approach, based on the method outlined by Cameron, Gelbach, and Miller (2008), allows us to make reliable inferences without relying on results from asymptotic theory. The results, shown in Figure B4 of Appendix B, maintain statistical significance at conventional levels, suggesting our findings are not driven by incorrect inference driven due to small sample bias.

fixed effects, respectively. When estimating the model, we treat one year before the house allocation as the omitted category and compare them to pro-government rulings in the other years.

As discussed in Sun and Abraham (2021), the fully dynamic version of the TWFE model in equation (2) estimated using OLS delivers consistent estimates but only under relatively strong assumptions regarding treatment effect homogeneity. In order to allow for heterogeneity in treatment effects across time and treated units, we also present the event study figures generated by a set of recently proposed estimators that are robust to treatment effect heterogeneity (De Borusyak, Jaravel, and Spiess 2021; Callaway and Sant’Anna 2021; Sun and Abraham 2021). Figure 1 presents this event-study figure. The results suggest that the estimates from all of the estimators are consistent with the parallel trends assumption. Specifically, regardless of the estimator employed, the coefficients on the years leading up to the house allocation are close to zero and do not show any noticeable pretrends. This aligns with anecdotal evidence, which indicates that judges typically face challenges in predicting their house allocation status (The News, 2017).

Interpretations.—Figure 1 reveals patterns regarding the influence of treatment effects. We note that the new, robust estimators all indicate lasting effects of house allocation. After houses are assigned, we observe a rise in state victories by around 20 percentage points, continuing for up to four years post-allocation. This trend towards pro-government rulings may represent judges’ gratitude or reciprocation towards the federal government. Figure 1 also dispels the possibility of two alternative mechanisms. In the first, judges anticipating receiving a house might try to pressure the government by ruling against it just before the allocation. This scenario is unlikely due to the absence of pre-trends and the unpredictability of house allocation outcomes (The News, 2017). The second scenario hypothesizes that judges may rule favorably towards the government in hopes of improving their chances of receiving a house, then cease such rulings once they’ve received their house. If this scenario were the main driver of our results, we would expect to see a post-allocation decrease in pro-government rulings. Instead, we see a surge in such rulings following house allocation. In summary, the lack of pre-trends, the observed increase in pro government rulings post-allocation, and the unpredictability of house allocation all support our interpretation that judges’ gratitude or reciprocation explain for our results.

Decision Quality.—To investigate whether real estate allocations negatively impacted decision quality, we utilize a measure of decision quality based on evaluations of judgment texts by attorneys in Pakistan. Our approximation of decision quality is an indicator variable called Merit, which switches on if the decision is “based on evidence rather than technical or procedural grounds” (Pound, 1963). We estimate equation (1) using this dependent variable, and the results are presented in Table 2. The first column in the table shows the findings from the simplest specification, which included only judge and year-fixed effects. The second column added time-varying case controls, and the third column further incorporated state-by-year fixed effects. In the fourth column, we replaced the state-by-year fixed effects with district court-by-year effects. Our results were consistent across all specifications, indicating that the allocation of real estate by the federal government to judges led to a decrease in decisions on merits. In particular, house allocation reduced decisions on merits by about 25 percentage points, a 40% decrease over the sample mean. Furthermore, we expand our analysis by evaluating the dynamic influence of house allocation on decisions based on their merits. To this end, we estimate equation (2) using merit decisions as the dependent variable across the four robust estimators. The results, presented in Figure 2, show a sharp and persistent fall in meritorious decisions following the house allocation, suggesting that the house allocation deteriorated the quality of judicial decision-making.

Balance Checks.—In the High Courts of Pakistan, the allocation of cases within a district court is carried out randomly by means of a computerized case management system. From a theoretical perspective, the non-random assignment of cases has ambiguous effects on the estimated coefficients. The non-random allocation of cases to judges who have received real estate versus those who have not could affect the way they rule on cases. This would depend on whether they are assigned cases where the government's position is strong or weak. If judges who have received real estate are assigned to cases where the government's position is strong, then any resulting pro-government rulings might be due to the strength of the government's position, rather than the fact of the house allocation itself. This could mean that we may be overestimating the impact of house allocation on state victories. In contrast, if house-allocated judges are assigned cases where the government's position is weak, then the high number of state victories we observe might actually underestimate the true effect of the house allocation

achieved if cases had been randomly assigned among judges. Our evidence from two balance tests, however, suggests both these possibilities are unlikely since consistent with de jure assignment rule at the High Court, cases appear to be randomly assigned within courts. We test for this de jure randomness of the case allocation within courts via two balance tests. In the first test, we estimated the baseline equation (1) reported in Column (3) of Table 1. However, we replaced the dependent variables with the available case characteristics, which are shown in Table 3. Second, we test for balance using the joint orthogonality balance test suggested by Bruhn and McKenzie, (2009). This balance test jointly estimates the case-characteristics with treatment variables and is shown to perform better in simulation exercises (Bruhn and McKenzie, 2009). The joint orthogonality balance test results are available in Table B2 of Appendix B. Results from both tests support the view that cases are randomly assigned within courts. Finally, it is worth noting that the inclusion of the court-by-year fixed effect in the main specification also precluded the comparison of cases across courts and over time, by allowing for within-court and within-year comparisons. Collectively, these findings suggest that our results are unlikely to be explained by non-random allocation of cases.

Costs and Benefits to the Government of House Allocation.—A back-of-the-envelope calculation allows us to ascertain the value of the land expropriated by the government that can be attributed to house allocation to judges. We have data on the market value of 57 properties expropriated by the government as a result of pro-government rulings in its favor. These property values were obtained from judgment texts and are assumed to be representative. Our strategy is similar to Mian and Khwaja (2005)'s computation of economy-wide costs of political connections using minimum and maximum bounds (i.e., most conservative to least conservative estimates). Using their methodology, we estimate that house allocation to judges allows the government to expropriate additional land valued in the range of 0.1% (most conservative) to 0.3% (least conservative) of GDP every year, where the yearly average value of land expropriated due to real estate allocation to judges is estimated at about 0.2 percent of GDP.⁹

⁹ In 45% of our 8600 sampled cases, the government successfully expropriated land. Since, we randomly sampled 0.2% of the total population of cases, the total successful land expropriations by the government would be about 600,000, basing computations on a survey indicating that 30% of land cases involve direct expropriation of private property, on average value of USD 53,700 for the 57 expropriated properties in our data, and coefficient estimate of 25%, we infer the amount of land expropriation due to house allocation as 0.1 to 0.3 percent of GDP every year. (for more details, see Table B3 in Appendix B).

Based on the number of houses allocated and the market value of houses in the Judicial Colony, our estimates indicate that the government cumulatively 0.03% of total GDP in 2019 on house allocation. This suggests that in a single year, through land expropriation alone, the government may be able to recover the total amount spent on house allocation. This may explain why the allocation of houses to judges has remained popular with the government and endured through transitions from left-wing to right-wing governments, as well as from military to ‘democratic’ rule.

VI. Conclusion

The relationship between the government and the judiciary is widely regarded as one of the most sensitive. This recognition is not new, as Montesquieu (1778) famously emphasized the importance of an independent judiciary, advocating for its separation from the executive branch in order to prevent abuses of government power and uphold the rule of law. Despite this, the writers of the US Constitution (Madison, Hamilton, and Jay, 1788) sought to regulate this relationship, exemplified by their decision to grant the executive branch the power of judicial appointment. Our paper delves into this special relationship to show how reciprocity or gratitude can sustain a two-way exchange relationship between the government and the judiciary. Our findings suggest that informal mechanisms can sustain a reciprocal relationship between the government and the judiciary, and indicate conditions under which the independence of the judiciary may be compromised. Specifically, we demonstrated that when the Pakistani central government allotted expensive real estate to judges, their rulings in favor of the government increased and the decisions on case merits decreased. The effects are exclusively observed in cases directly involving the federal government, that signs off the real estate allocation.

Research examining patronage has hitherto been unable to empirically establish the fundamental two-way – quid pro quo – transaction that is at the heart of patronage arrangements, especially between the judiciary and the government. We show that the protection of private property and political rights promised in common law is attenuated under norms that facilitate reciprocating exchange between the government and the judiciary. Even constitutional protections, such as security of tenure or a relatively independent appointment system, may not be enough to prevent government capture of the judiciary. This may severely curtail the

judiciary's ability to hold state officials to account, protect political rights and prevent abuse of government power.

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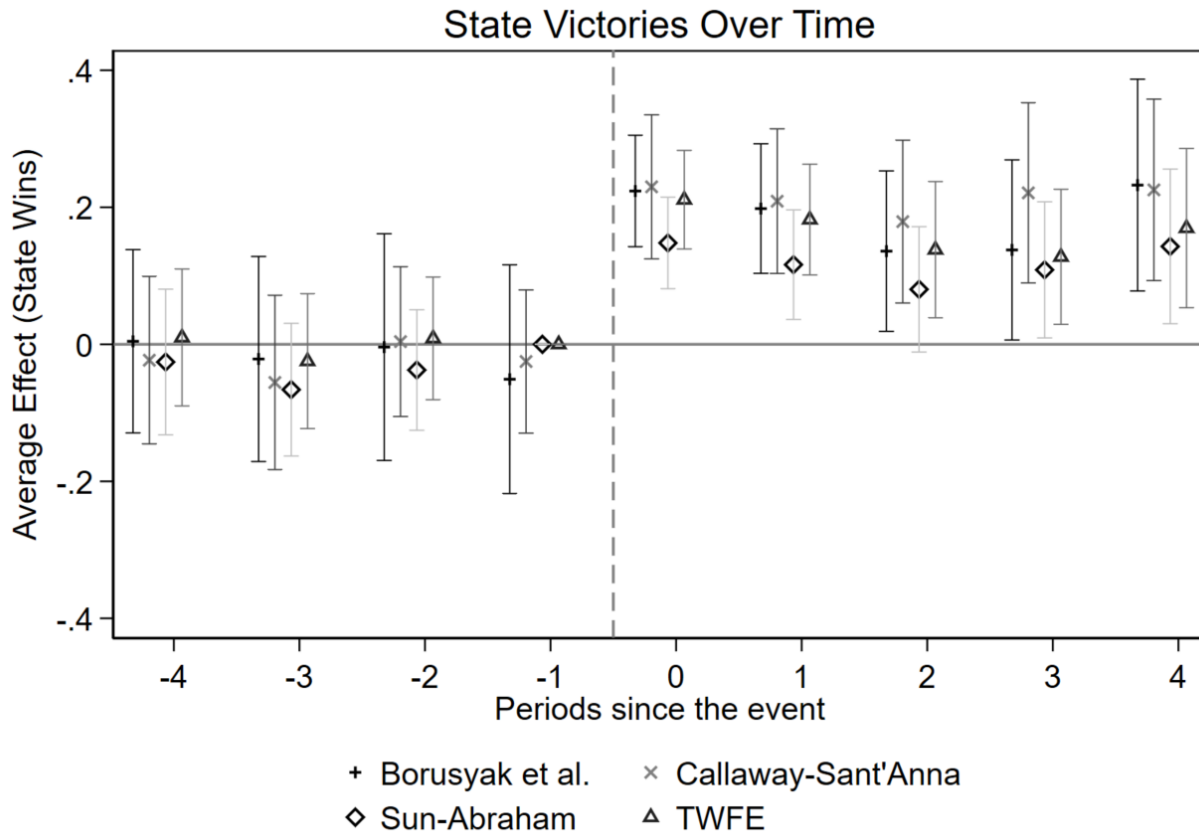
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Figures and Tables

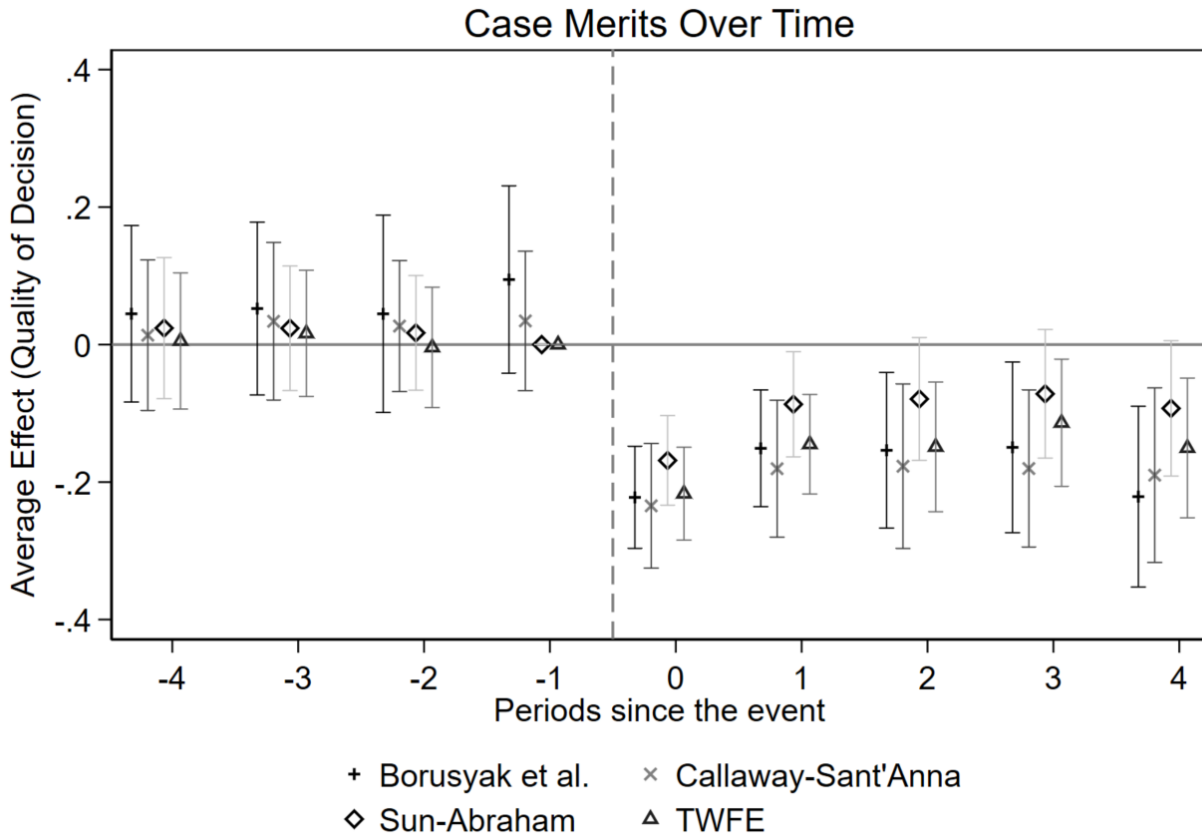
Figure 1: Time-Varying Impact on State Wins



Notes: This figure overlays the event-study plots constructed using four different estimators: a dynamic version of the TWFE model, equation (2), estimated using OLS (with triangle markers); Sun and Abraham (2021) (with diamond markers); Callaway and Sant'Anna (2021) (with cross markers); and Borusyak, Jaravel, and Spiess (2021) (with plus markers). The time variable is the year of decision and the treatment group variable is given by the judge allotted the house by the government. Omitted category is the year before the house allocation. The figure displays only four postperiods because the estimation of additional post periods would require employing already treated units as controls for the newly treated units. In the presence of heterogeneous dynamic treatment effects, such comparisons would bias the estimation and, therefore, they are shut down by all the newly introduced robust estimators. The maximum number of postperiods that can be estimated robustly, therefore, is four. We also use four preperiods because for the Borusyak, Jaravel, and Spiess (2021) estimator dramatically increases the standard errors after 4 preperiods (Borusyak, Jaravel, and Spiess 2021, p. 24). The bars represent 95 percent confidence intervals with standard errors clustered at the judge level.

Table 1: Impact on State Wins by the Type of Government

Figure 2: Time-Varying Impact on Meritorious Decisions



Notes: This figure overlays the event-study plots constructed using four different estimators: a dynamic version of the TWFE model, equation (2), estimated using OLS (with triangle markers); Sun and Abraham (2021) (with diamond markers); Callaway and Sant'Anna (2021) (with cross markers); and Borusyak, Jaravel, and Spiess (2021) (with plus markers). The time variable is the year of decision and the treatment group variable is given by the judge allotted the house by the government. Omitted category is the year before the house allocation. The figure displays only four postperiods because the estimation of additional post periods would require employing *already treated units* as controls for newly treated units. In the presence of heterogeneous dynamic treatment effects, such comparisons would bias the estimation and, therefore, they are shut down by all the newly introduced robust estimators. The maximum number of postperiods that can be estimated robustly is, therefore, also four. We also use four preperiods because for the Borusyak, Jaravel, and Spiess (2021) estimator dramatically increases the standard errors after 4 preperiods (Borusyak, Jaravel, and Spiess 2021, p. 24). The bars represent 95 percent confidence intervals with standard errors clustered at the judge level.

	(1)	(2)	(3)	(4)	(5)	(6)
	<i>State Victories</i>					
	<i>Disputes with the Federal Government</i>				<i>Disputes with Local Government</i>	<i>Disputes with State Government</i>
House Allotted	0.288*** [0.0924]	0.277*** [0.0890]	0.312*** [0.0949]	0.263*** [0.0807]	0.0940 [0.0791]	0.0780 [0.0657]
Judge and Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	Yes	Yes	Yes
State by Year FE	No	No	Yes	No	No	No
District by Year FE	No	No	No	Yes	Yes	Yes
Observations	857	857	857	857	1,675	1,815
R-squared	0.376	0.421	0.490	0.565	0.506	0.446
Mean of dependent Variable	0.520	0.520	0.520	0.520	0.487	0.472

Note: This table explores the effect of house allocation on government victories by type of government. Specifically, it presents estimates of coefficient β from equation (1) with state victories as the outcome variable. The outcome variable is state wins, a case-level dummy that takes the value of one if the state wins in the case and zero otherwise. Columns (1) to (4) estimate a progressively saturated model with varied fixed effects in cases pertaining to the federal government. Column (5) estimates equation (1) for cases involving the local government, and Column (6) for the state or provincial government. Controls include number of pages of judgment order, presence of the chief justice on the bench, number of judges on the case, number of lawyers on the case and dummies for type of cases. Unit of analysis in the case level for this table. All columns always include judge, year and district court-by-year fixed effects. Standard errors are reported in brackets and are clustered at the judge level. *** $p < 0.01$, ** $p < 0.05$

Table 2: Impact of House Allocation Case Merits - Vs Federal Government

	(1)	(2)	(3)	(4)
	<i>Case Merits</i>			
House Allotted	-0.308*** [0.0890]	-0.311*** [0.0876]	-0.331*** [0.0986]	-0.263*** [0.0842]
Judge and Year FE	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	Yes
State by Year FE	No	No	Yes	No
District by Year FE	No	No	No	Yes
Observations	857	857	857	857
R-squared	0.359	0.370	0.437	0.507
Mean	0.620	0.620	0.620	0.620

Note: This table explores the effect of house allocation on government victories and meritorious decisions. Specifically, it presents estimates of coefficient β from equation (1). Case merits is the dependent variable that takes the value of one if the “based on evidence rather than technical or procedural grounds”. House Allocated is a binary variable that turns on for a particular judge in the year when the judge receives the real estate. Controls include number of pages of judgment order, presence of chief justice on the bench, number of judges on the case, number of lawyers on the case and dummies for type of cases. The estimates from the baseline equation with judge, year and district court by year fixed effects are reported in Column 4 of the table. Standard errors are reported in brackets and are clustered at the judge level. The regressions are estimated on the sample presented in Column 3 of Table 1, specifically focusing on cases involving citizens and the federal government. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 3: Balance over Case Characteristics - Vs Federal Government

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Criminal Case	Constitutional Case	Land Case	Pages of Judgement Order	No. of Lawyers on Case	No. of Judges on Bench	Chief Justice on Bench
House Allotted	0.00479 (0.00473)	0.00368 (0.00363)	-0.0820* (0.0314)	-1.351 (0.688)	0.226 (0.173)	0.0855 (0.0956)	0.0552 (0.0451)
Year and Judge FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District by Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	857	857	857	857	857	857	857
R-squared	0.972	0.971	0.361	0.370	0.162	0.323	0.311
Mean	0.0677	0.935	0.540	8.537	4.102	1.758	0.0548

Note: This table explores the effect of house allocation on available observed case characteristics. Specifically, it presents estimates of coefficient β from equation (1) with the following case characteristics as dependent variables: number of pages of judgment order, presence of chief justice on the bench, number of judges on the case, number of lawyers on the case and dummies for type of cases. Unit of analysis is at the case level. Judge, year and district court-by-year fixed effects are always included in each column. Standard errors are reported in brackets and are clustered at the judge level. The regressions are estimated on the sample presented in Column 3 of Table 1, specifically focusing on cases involving citizens and the federal government. * *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

For Online Publication

Judicial Capture

By Sultan Mehmood and Bakhtawar Ali

Contents

A. Variable Definitions, Background and Robustness

B. Additional Figures and Tables

A. Variable Definitions, Background and Robustness

Appendix A1. Variable Definitions and Sources

State Wins = This is the average of government victories at the case level. This variable is coded at the case level by attorneys at a law firm, one for a State victory and zero for a State defeat. Coding of this variable was based on lawyers' reading of the judgment orders retrieved from an online portal that records the High Court cases in Pakistan (<https://www.pakistanlawsite.com/>) and High Court case archives at the Registrar Offices.

Merit = This variable is also coded at the case level by the attorneys, based on the text of the judgment orders. It was a dummy for the case being decided based on "*evidence rather than technical or procedural grounds*" (Pound, 1963).

House Allocation = This is a binary variable that turns on for a particular judge in the year when the judge receives a house. The variable is obtained from a list from the Public Accounts Committee of the Pakistani parliament. The PAC records contain the judges' full name, court and year of real estate (plot) allocation that is used to construct this variable.

Number of Lawyers = A count variable for the number of lawyers arguing the particular case, also derived from the text of the judgment order.

Number of Judges = A count variable for the number of judges adjudicating upon a particular case, also derived from the text of the judgment order.

Bench Chief Justice = A dummy variable for the Chief Justice adjudicating upon the case, as indicated in the text of the judgment order.

Number of Pages of Judgment Orders = A count variable for the number of pages of the judgment order for a particular case, also from the text of the judgment order.

Criminal Case = A dummy for criminal cases, as indicated in the text of the judgment order. A subset of these cases are called petty crime that involves vandalism, burglary, and theft.

Constitutional Case = A dummy for constitutional cases, as indicated in the text of the judgment order.

Land Case = This is a subset of the constitutional cases. Within this category, there exists a specific type of case which involves a dispute over land ownership or expropriation with the State, known as "Eminent Domain" cases. In these cases, the State is represented by various housing development agencies, such as the Defense Housing Authority, the Lahore Development

Authority (LDA), the Karachi Development Authority (KDA), the Peshawar Development Authority (PDA), and the Capital Development Authority (CDA), which are authorized to resolve disputes related to land ownership on behalf of the government.

Human-Rights Case = A subset of the constitutional cases. These cases, which are referred to as "writ petitions," are distinct from cases involving disputes over land ownership and instead involve non-land matters in which a fundamental right has been violated by the government.

Gender = A dummy for male judges. This is coded in two ways: 1) manually, the authors checking every judge name, and 2) automatically, the authors asking 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 judge). The data comes from a combination of biographical information contained in annual reports, Bar Council records and 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.

Appendix A2. Data Construction and Sources

We randomly sampled 0.2% of all the cases decided each year within this period from the universe of available cases from 1986 to 2019. The random sampling was specifically conditional on the State being one of the defendants in the case. As the years progressed, the number of cases decided gradually increased, likely attributed to the growing population and case filing. Consequently, our sample size also gradually expanded over time. This can be seen in Figure B2 of Appendix B, that illustrates this progression by presenting a plot comparing the

total number of sampled cases against the total number of cases. Therefore, the random sampling of implied that we obtained about 8,600 cases from the universe of High Court cases in Pakistan spanned the years 1986 to 2019. The information recorded in the judgment order texts was gathered by two teams, each comprising four lawyers who were supervised by a senior attorney. The data was collected from both the High Court Registry and the website for these cases. Our analysis focused on both the case characteristics and the outcome variables, both of which were derived from the judgment orders available online in the central repository of cases (<https://www.pakistanlawsite.com/>) that is commonly used by lawyers in Pakistan to prepare their cases. The website, which is password-protected and accessed with permission obtained via a Law firm, is also used as a "Central Library" for lawyers practicing within Pakistan's Common Law system where case precedent is of utmost importance. Furthermore, we supplemented this information with archives from the High Court's Registrar Offices.

Two independent teams of four lawyers each supervised by a senior Lawyer recorded the key information in the judgment texts. We ensured via a formal contract that both teams worked independently. Random field visits also reassured us that the contract was being enforced. Table C1 in Mehmood (2022) presents the means of the outcome variables and case characteristics coded by the two teams, as well as the correlation coefficient between them. There is a strong correlation between the coding of the two teams. For instance, the average State Wins from Team 1 is 0.50 and the correlation coefficient for State Wins between the two teams is 0.85. Unsurprisingly given the high correlation coefficients, similar results are obtained using the cases coded by Team 2. The two senior lawyers supervising the teams were experts in “constitutional law” with over 10 and 12 years of experience as attorneys practicing constitutional law, respectively. They were hence particularly suited to supervise coding of cases involving the State. The remaining 8 people who did the actual coding exercise were legal experts and had all passed “L.L.M” examination which is equivalent to 2 years master’s degree in law. This is the main requirement to practice law as an attorney in Pakistan. These coders were essentially junior attorneys starting practice as lawyers with the law firm. The teams were given no information on the research question, to ensure that those performing the coding exercise were unlikely to know whether a judge received the house or not.

Appendix A3. Pakistan's Court System

Background.—This section aims to provide an overview of the structure and background of the Courts in Pakistan. The Indian High Courts Act of 1861 granted the British Crown the authority to establish High Courts in the Indian colony, which were early versions of the current High Courts in India and Pakistan. After India and Pakistan gained independence from British colonial rule in 1947, several modifications were introduced to the legal institutions in both countries, while maintaining the overall institutional structure, including the Common Law jurisprudence and criminal procedures. One significant change was the increase in the mandatory retirement age from 60 to 62. In 1963, India raised the retirement age of High Court judges to 62, followed by Pakistan in 1969 (through constitutional amendments), and the mandatory retirement age for High Court judges has remained 62 in both countries since then. Furthermore, while the focus of study is the Pakistani High Court, Pakistan's judiciary operates under a three-tier hierarchical system. The lowermost Courts are the civil and session Courts, which handle civil and criminal cases, respectively, based on the domicile of the litigating parties. The High Courts can review decisions made by civil and session Courts. In cases of government land expropriation or violation of fundamental rights, the High Court is the primary, and often the only, recourse for individuals and firms seeking redressal. Cases are randomly assigned to judges within the High Court under a case management system. While there are only four provincial High Courts and one federal court High Court in Islamabad, each bench of the High Courts is dispersed across the four provinces in the form of 16 district High Court or “divisional benches”. Each district has four judicial benches, each comprising approximately seven judges, specializing in property, writ or human rights, tax, and criminal cases. Notably, Pakistani laws and capacity constraints limit the replacement of judicial vacancies at the judicial bench level. High Courts are particularly significant in Pakistan, as they offer a platform for individuals to file cases against the government, including constitutional petitions (writ petitions) or criminal appeals, with the State as a party. Constitutional cases that involve The State can be filed against the federal, provincial, or local governments, or any organ of the state that wields executive authority, such as the office of the Prime Minister. Finally, the Supreme Court of Pakistan serves as the final appellate Court, located in the federal capital of Islamabad. It typically hears appeals on "technical" grounds for criminal and constitutional cases in the High Courts. The Supreme

Court has few judges, which severely limits the number and scope of cases it can hear. Consequently, only a small proportion of cases end up being heard by the Supreme Court.

Judicial Structure in Pakistan.—The judiciary in Pakistan is structured hierarchically into three tiers. At the base are the civil and session courts that handle civil and criminal cases respectively. These rulings can be contested in the high courts. In these high courts, individuals can file constitutional petitions against the state. Cases involving the state as the respondent include federal, provincial, and local governments. Our study centers on the high courts of Pakistan that preside over cases involving the government. In cases involving the State, the high court serves as the primary, and often the sole recourse for individuals seeking redress. Pakistan has one federal high court in Islamabad and four provincial high courts, each containing approximately four divisional or district high court benches. This equates to a total of 16 district high courts across the country. The allocation of judges to these courts is organized by a roster of sittings, generated through a computerized case management system, that assigns cases to each judge within the divisional district high court. On average there are about 30 judges per district high court. Within these courts, about 70 percent of all cases filed were “constitutional petitions”, majority of which involved the government responding to land expropriation claims from the citizenry and writ petitions or political rights cases involving abuse of power by the government. An example of a land expropriation case can be seen in the demolition of a residential complex to make way for a shopping mall, with concerned citizens petitioning courts to obtain compensation of their demolished property (The News, 2021). On the other hand, an example of a political rights case can be observed when a government opponent had his citizenship revoked for threatening to lead a protest against the government. He subsequently filed a constitutional petition, citing violation of his fundamental right to political participation (Naseer, 2019).

Case Assignment To Judges.—Cases in the High Courts of Pakistan are randomly assigned to judges subject to a workload constraint of a judge. First, a courthouse is determined based on territorial jurisdiction of the case. Then, the cases are randomly assigned through a computerized “case management system”. For instance, if there is just one judge adjudicating, that judge will be allocated the case, but if there are multiple judges, a random assignment

process fully determines the judge assignment process within the court. Moreover, the higher judiciary explicitly condemns and punishes attempts at “forum shopping,” where litigants select particular judges seeking a favorable outcome. Throughout the paper, we draw within court comparisons by including court by year fixed effects. This enables us to compare judgements of those judges who receive and those who do not receive real estate within the same court-year. The balance of cases across house allocated versus unallocated judges is also supported by evidence from a series of balance tests which are reported in Table 3 of main text and Table B2 of Appendix B.

Provincial versus Federal Judicial Colony.—Our research focuses on transfers between the executive branch and the judiciary. Specifically, we are investigating how the federal government exerts control over the state judiciary by examining the allocation of real estate in the federal capital's judicial colony, which is under federal jurisdiction and administered through the federal Ministry of housing. This makes studying the impact of houses allocated by the federal government in Islamabad's judicial colony a natural choice to help answer our research question. The lack of transparency in the rules governing house allocation to judges by state governments in other cities, coupled with data limitations, further narrows our focus to the judicial colony in Islamabad, where the Prime Minister's Office directly oversees the allocation of houses via the federal ministry of housing.

Appendix A4. Discussion and Robustness

Generalized Difference-in-differences.— Under parallel trends assumption, the generalized difference-in-differences model allows us to rule out various concerns that could otherwise impair our ability to interpret the results as causal. First, we can rule out that the results are driven by time-invariant differences in pro-government rulings. Specifically, one could worry that some judges may be more likely to get real estate allocated who may have different fixed characteristics such as gender or religion. By including judge-fixed effects, we can rule out such concerns. Second, we can rule out that our results are driven by state victories evolving over time in a way that is common across judges in different time periods. For example, political shocks such as national elections might affect all judges' rulings in a particular manner and subsequently influence their state victories. However, year fixed effects allow us to account

for such concerns. Last, we can exclude the possibility that different cases are presented in different districts over time. Some courts may become more congested over time, causing their rulings to differ significantly from other district courts in a given year. Court-by-year fixed effects allow us to eliminate such concerns. There may still be concerns about the plausibility of the parallel trends assumption in our setting, which implies that judges who received real estate may have been on a different pro-government ruling trajectory compared to those who did not receive real estate. To address this concern, we take two approaches. Firstly, we estimated a fully dynamic version of the two-way fixed effect model to check for any potential pretrends. Secondly, we utilize alternative and more robust estimators introduced by Borusyak, Jaravel, and Spiess (2021), Callaway and Sant'Anna (2021), and Sun and Abraham (2021) to explore the existence of differential trends prior to the house allocation. These strategies, implemented and reported in Figures 1 and 2, assuage concerns about violations of the parallel trends assumption in our setting.

Limitations of TWFE Models and Suggested Remedies.—Although estimation of TWFE OLS estimations are the workhorse models for differential timing research designs, they have been shown to deliver consistent estimates only under relatively strong assumptions about homogeneity of treatment effects (Borusyak, Jaravel, and Spiess 2021; Callaway and Sant'Anna 2021; Goodman-Bacon 2021; Sun and Abraham 2021). Specifically, as shown in Goodman-Bacon (2021), the treatment effect estimate obtained from a TWFE estimator is a weighted average of all possible 2×2 difference-in-differences comparisons between groups of units treated at different points in time. If treatment effects are homogeneous across treated groups and across time, the TWFE estimator is consistent for the ATT (average treatment effect on the treated). Conversely, if treatment effects are heterogeneous across groups or time, the TWFE estimator does not provide consistent estimates for the ATT. We address concerns about the reliability of TWFE estimator by replicating our results using the robust estimators introduced in Borusyak, Jaravel, and Spiess (2021); Callaway and Sant'Anna (2021); and Sun and Abraham (2021). By *shutting down* the 2×2 difference-in-differences comparisons between the newly treated and already treated units, the robust estimators deliver consistent estimates even in the presence of heterogeneous treatment effects across time or treated units.

SUTVA Violation.—In difference-in-differences, particularly those relying on treatment variation by geographic location, the stable unit treatment value assumption (SUTVA) is, either explicitly or implicitly, invoked. Controlling for geographical factors via court-by-year fixed effects assuages some of these concerns. However, the Stable Unit Treatment Value Assumption (SUTVA) can still be violated if there are heterogeneous treatment effects, meaning that the house allocation has different effects on different judges or if there are spillover effects meaning that house allocation to one judge affects ruling by other judges. We approach this issue in two ways. First, as noted in our event study (Figure 1), our main results hold for host of estimators that adjust for heterogeneous treatment effects (e.g. those due to Borusyak, Jaravel, and Spiess 2021; Callaway and Sant’Anna 2021; Sun and Abraham 2021). Second, we estimate the magnitude of the peer effects to assess the extent of spillovers, where SUTVA may be violated, where the house allocation to some judges may change the incentives and decisions of other judges. Specifically, we estimate the baseline Equation (1) by limiting our sample to the cases of judges who *did not* get the house allocation and analyze how they respond when their colleagues in the same bench receive real estate. Table B4 of Appendix B reports these results. We find that judges who serve in the same bench are no more likely to be impacted if their colleague in the same court receives a house. Estimate of baseline equation (1) is reported in Column 4 of Table B4. The estimate is statistically insignificant and has an effect size approximately 20 times smaller than what is documented in the corresponding main regression in Column 4 of Table 1. Since these judges are plausibly most likely to be impacted by their peers, the small magnitude and statistically weak relationship suggests SUTVA violation is not a major empirical concern in our analysis. This also indicates that rulings of house allocated judges on their non-house allocated colleagues is likely to be limited.

Robustness to Outliers.—To evaluate if outliers are driving our results, we conducted several sensitivity checks. First, we examine the distribution of judges based on the proportion of cases they decide in favor of the government, which is reported in the appendix as Figure B1. There appears to be substantial variation among judges in their propensity to rule in favor of the government. To ensure that our results are not driven by the most extreme judges, we excluded judges who had the top or bottom 10 and 5 percentiles of government victories. These results are reported in Table B5 of Appendix B. The results remain similar, indicating that our findings are

not solely driven by judges who exhibit the strongest or weakest slant towards the government. Additionally, we conducted a similar analysis by excluding cases from provincial and federal political capitals. Courts in these political capital courts are called “principal benches” and it may be the case that these political capitals may adjudicate upon particularly politically salient cases and in turn driving our results. Again, we found that our main conclusions hold even when we progressively drop cases in political capitals with replacement (Table B6 of Appendix B), suggesting our findings are not sensitive to the inclusion or exclusion of politically salient courts.

Additional Sensitivity Checks.—In order to further assess the sensitivity of our findings, we conducted several additional robustness tests. Firstly, we demonstrate in Table B7 of Appendix B that alternative clustering to compute standard errors, such as multiway clustering as recommended by Cameron et al. (2011), at either the judge-by-year or district court-by-year level, still yields statistically strong results. Similar results are obtained when employing small sample correction with court-level clustering, as suggested by Cameron et al. (2008). Figure B4 in the appendix illustrates these findings. Secondly, we show that our results remain statistically significant when we aggregate the outcome variable at the level of variation of the explanatory variable, i.e., when we run the regression at the judge year level (Table B8). Lastly, we show the impact of house allocation is similar before and after a major judicial reform in 2010 (Mehmood, 2022) that changed the procedure to appoint judges, suggesting the results are not driven by specific time period (Table B9). All of these sensitivity analyses further support the robustness of our results.

Impact of House Allocation by Case Type. — In Table B10, we report disaggregated results by case type which of which have similar effect on house allocation. In the High Courts, the government can be involved in constitutional cases or “writ petitions” (involving land and political rights cases) or criminal cases. We find that politically salient cases involving land expropriation and political rights expropriation have a statistically significant impact on government victories. Consistent with several qualitative accounts, we interpret these results as the Prime Minister allocating houses to judges so as to expropriate land from citizens and to persecute political opponents. We, however, do not find statistical evidence for politically less-salient petty crime cases when the State is the prosecution.

Heterogeneous Treatment Effects.—While TWFE regressions, such as Equations (1) and (2), are the standard models for differential timing research designs, their ability to deliver consistent estimates is contingent upon assumptions about treatment effect homogeneity (Borusyak, Jaravel, and Spiess 2021; Callaway and Sant’Anna 2021; Goodman-Bacon 2021; Sun and Abraham 2021). Specifically, as noted in Goodman-Bacon (2021), the TWFE estimator calculates treatment effects as weighted averages of all possible 2×2 difference-in-differences comparisons between groups of units treated at different times. If treatment effects are uniform across treated groups and time, the TWFE estimator delivers consistent estimates for the ATT. However, if treatment effects vary across groups or time, the TWFE estimator becomes unreliable for ATT estimation. As mentioned earlier, we address concerns about the reliability of the TWFE estimator, by replicating our results using robust estimators introduced in recent studies (Borusyak, Jaravel, and Spiess 2021; Callaway and Sant’Anna 2021; Sun and Abraham 2021) in Figure 1. By eliminating the 2×2 difference-in-differences comparisons between newly treated and already treated units, these robust estimators provide consistent estimates even in the presence of heterogeneous treatment effects across time or treated units. We highlight once, again our results, reported in Figures 1 and 2, for estimators that explicitly account for heterogeneous treatment effects, are essentially identical and strengthen the robustness of our findings. We further implement the Goodman-Bacon decomposition to assess the extent of the heterogeneous treatment effects. Goodman-Bacon (2021) has demonstrated that standard difference-in-differences estimates can be biased in cases where multiple treatments occur at different times, primarily due to earlier treatment units acting as controls for later treatment units. In our specific context, where judges acquire real estate in different years, we follow Goodman-Bacon (2021) and employ a Bacon decomposition of the difference-in-differences estimation to assess the extent of “forbidden comparisons” of judges obtaining houses earlier versus judges obtaining houses later. In Table B11 of Appendix B, we showcase the Bacon decomposition. The decomposition analysis involves comparisons between different treatment and control groups. The first row of Table B11 shows the coefficients estimated from comparing early house recipients with late house recipients as the control group (before they received treatment) along with their associated weights. The second row reports coefficients comparing late house recipients as the treatment group with early house recipients as the control group (the forbidden

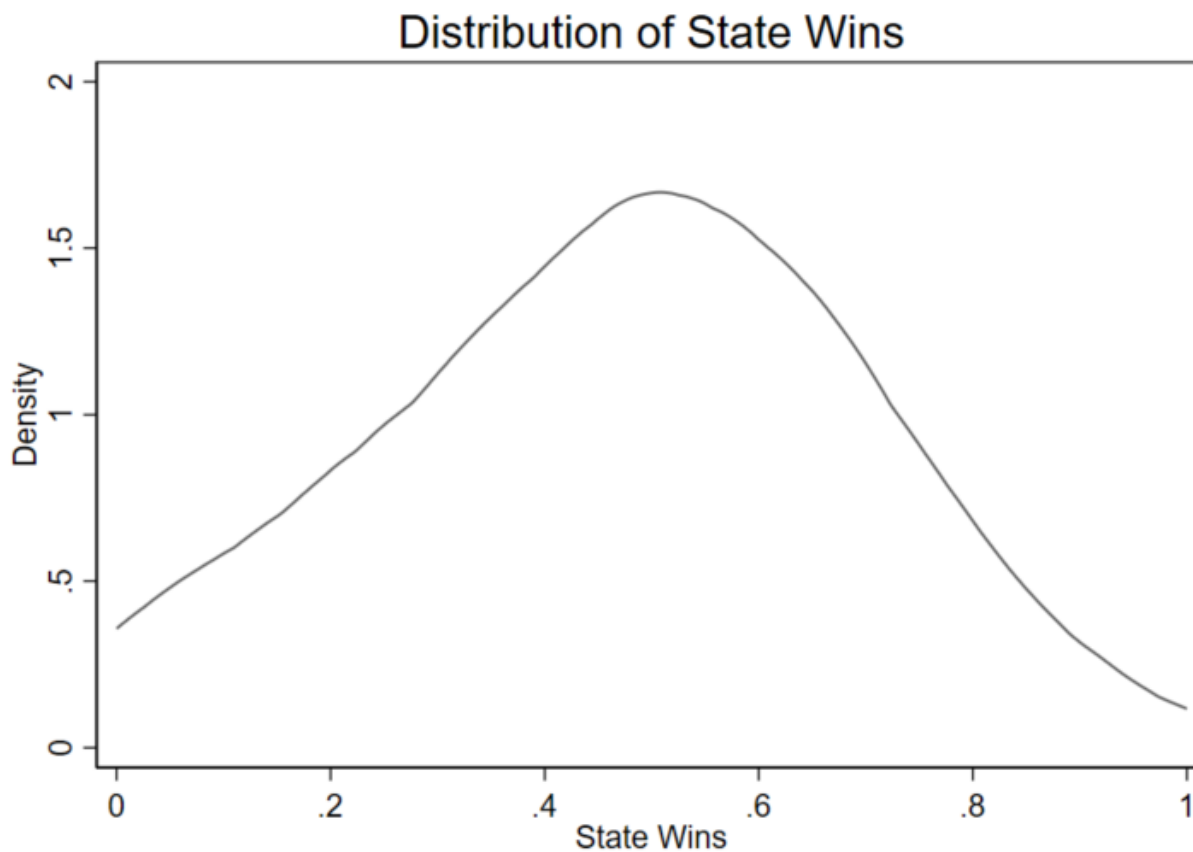
comparison) and provides this group's corresponding weight in the difference-in-differences estimation. The third row compares both early and late house recipients with units that never received a house (treated vs. untreated) and provides the associated weights. Our findings suggest that a minimal proportion of only 0.02% of the difference-in-differences estimates stem from the "the forbidden" comparisons. This can be attributed to the relatively short average tenure of judges in the dataset (around 6 years) and the replacement of judges over time, resulting in less weight given to already treated judges acting as the control group. However, it is noteworthy that the majority (88.5%) of the difference-in-differences estimates are solely derived from the comparisons between treated and untreated judges. Furthermore, the estimates for earlier versus late treatment closely align with the results from our baseline regression, highlighting the consistency of the estimates. This finding is consistent with similar effects observed when employing various recently developed estimators for assessing treatment effect heterogeneity.¹⁰

Robustness Summary.—Our findings withstand several robustness checks. Firstly, we observe evidence consistent with the random allocation of cases within courts, as demonstrated by two balance tests. The first test estimates the baseline equation by substituting the dependent variable with case characteristics. The second test follows a suggestion by Bruhn and McKenzie (2009) to conduct an orthogonality balance test that jointly estimates observed characteristics alongside the treatment variable. The results from both tests support the random assignment of cases within courts. Secondly, we find no evidence of house allocation impacting rulings by judges' peers. When a judge within a bench receives a house, the behavior of her colleagues remains unchanged, suggesting the Stable Unit Treatment Value Assumption (SUTVA) in difference-in-differences is unlikely to be violated. Thirdly, alternate clustering methods, such as multiway clustering as suggested by Cameron et al. (2011) or wild bootstrap clustering that take into account small number of clusters at the court level (Cameron et al., 2008), also provide statistically strong results. Finally, our findings remain consistent if we split the sample. All of these sensitivity analyses support the robustness of our results.

¹⁰ Similar robust results are also found for the merit decision variable when we conduct robustness analysis similar to those done for state victories variable. These robustness checks are reported in Table B12 of Appendix B.

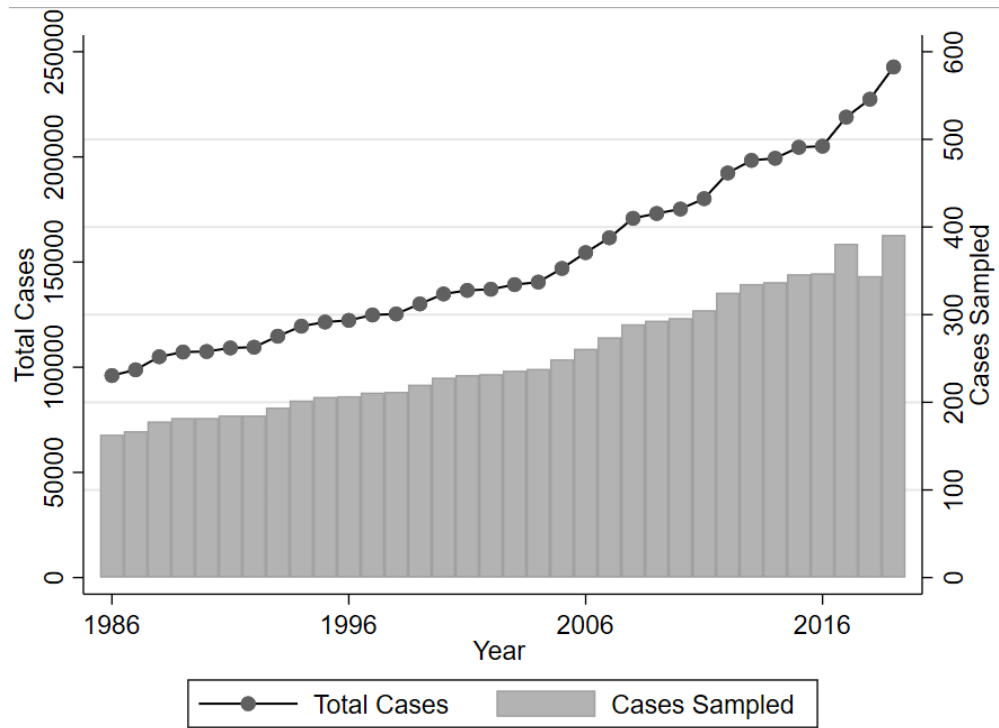
B. Additional Figures and Tables

Figure B1: Distribution of State Wins



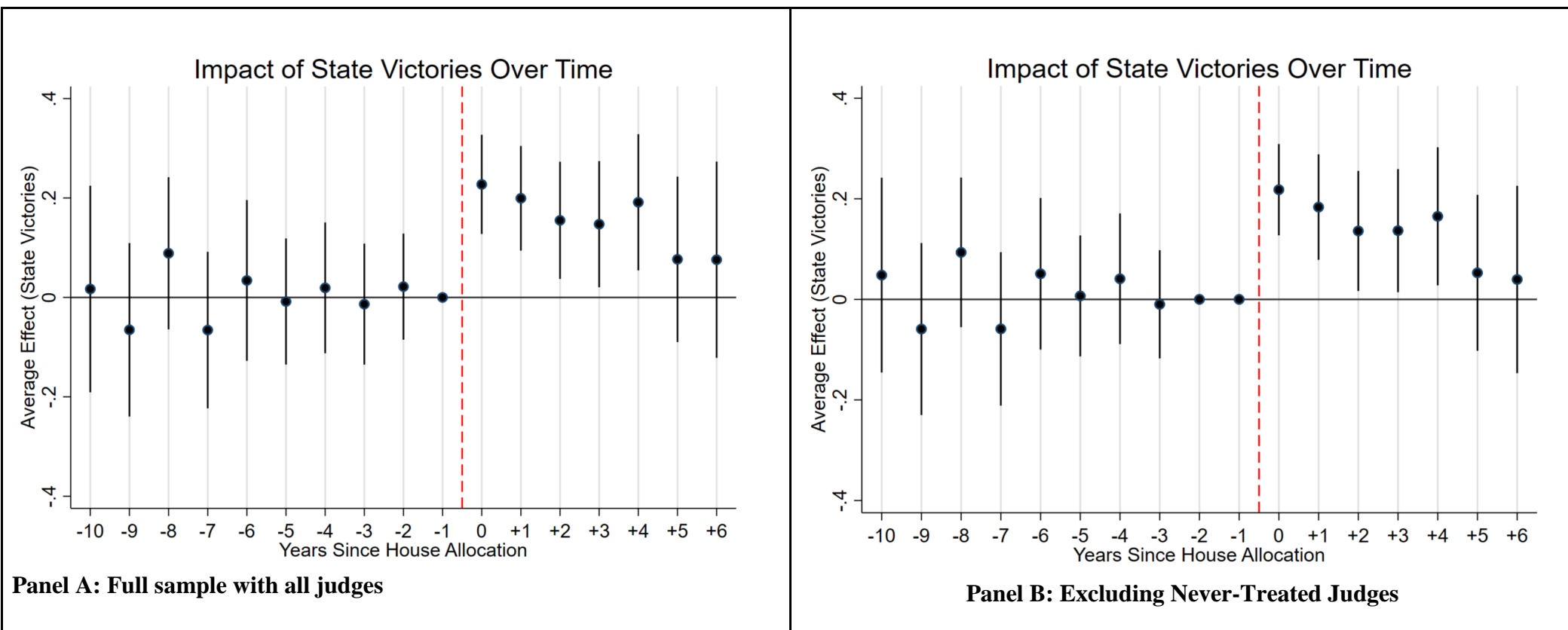
Notes: The figure displays the probability density estimate of the distribution of state victories of judges for the period 1986-2019. The bandwidth used for the estimation is 0.1. The variable State Wins is based on all the 446 judges in our sample. The mean and standard deviation of the distribution are 0.468 and 0.220, respectively.

Figure B2: Total Cases vs Sampled Cases over Time



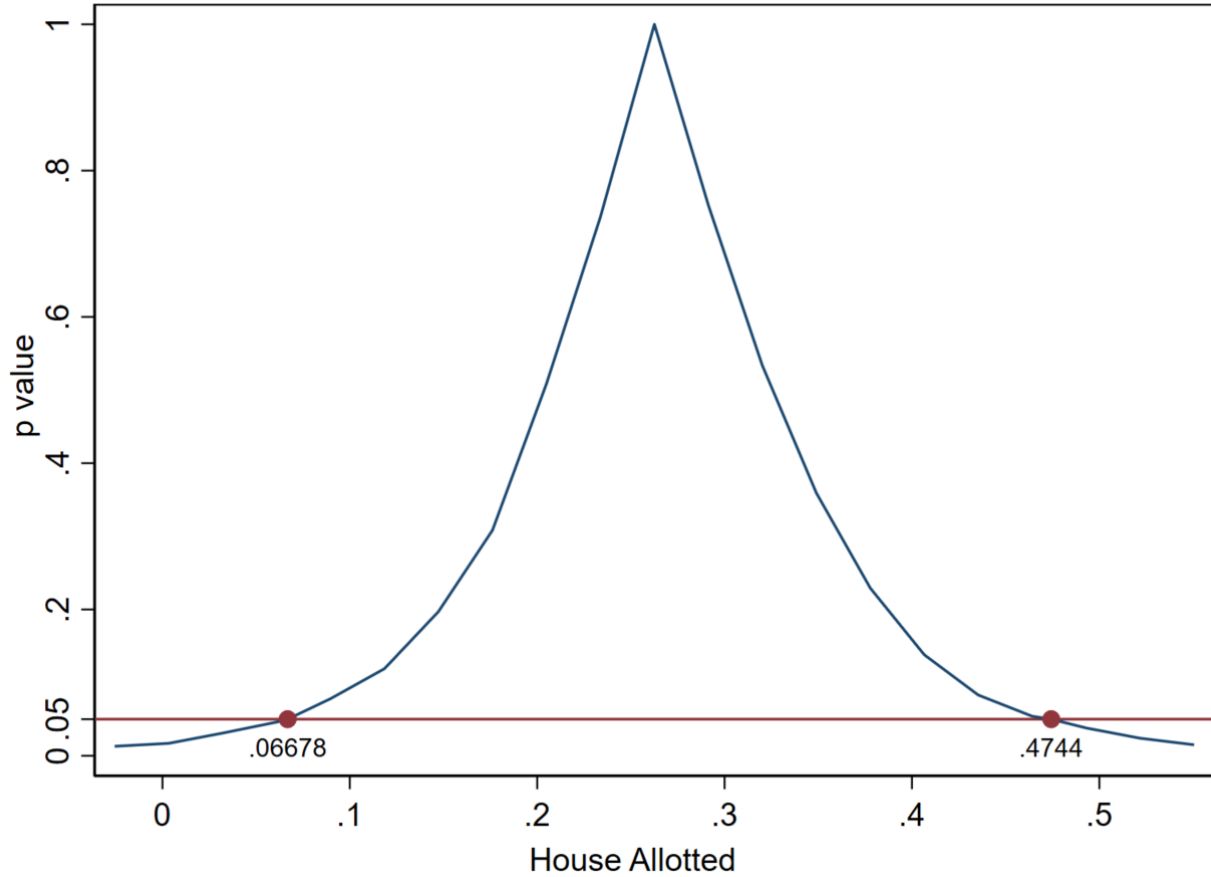
Note: These are 8600 randomly sampled cases for all years from 1986 to 2019 from the universe of district High Courts in Pakistan (about 0.2% of total cases decided in the period are sampled).

Figure B3: Impact of House Allocation over Time



Notes: This figure overlays the event-study plots constructed using OLS. That is, it displays OLS estimates of the dynamic versions of the TWFE model. The outcome variable is State Wins. The time variable is the year of decision and the treatment group variable is given by the judge receiving the house by the federal government. Panel A on the right includes all the judges in the dataset including never-treated individuals (judges who never received real estate). Panel B on the left excludes the never treated judges from the dataset (who ever recieved real estate), and estimates the TWFE model with “not-yet-treated” as the control group. The omitted category is the year before the house allocation in Panel A and in Panel B, since we require two omitted categories (to avoid multicollinearity), we have t-2 and t-1 as omitted categories. The bars represent 95 percent confidence intervals. Standard errors are clustered at the judge level.

Figure B4: Confidence Interval by Wild Bootstrap Clustering



Note: The figure displays the confidence interval for our coefficient of interest for the first specification (1) using wild bootstrap clustering, as per Cameron et al. (2008), which imposes a small cluster correction. Roodman et al. (2019)'s *bootest* in Stata 17 is used to construct this confidence interval.

Table B1.1: Descriptive Statistics - All Governments

Variables	Observations	Mean	Std. Dev.	Min	Max
Panel A: Outcome variables and case characteristics					
State Wins	8527	0.479	0.500	0	1
Merit	8527	0.625	0.484	0	1
Constitutional Cases	8527	0.723	0.447	0	1
Land Cases	8527	0.412	0.492	0	1
Human Rights Cases	8527	0.286	0.452	0	1
Criminal Cases	8527	0.279	0.448	0	1
Pages Judgment Order	8527	8.849	7.676	1	81
Chief Justice on Bench	8527	0.065	0.246	0	1
Number of Lawyers	8527	4.129	1.812	2	32
Number of Judges	8527	1.810	0.840	1	5
Panel B: Explanatory Variables of Interest					
House Allotted (Case level)	8527	0.209	0.407	0	1
House Allotted (Judge level)	446	0.502	0.501	0	1
Panel C: Judge Characteristics					
Gender	446	0.964	0.186	0	1
Muslim	446	0.991	0.094	0	1
Promoted to SC	446	0.056	0.230	0	1
Former Lower Court Judge	446	0.110	0.313	0	1
Fr. Office holder of Bar Ass.	446	0.573	0.495	0	1
Former Lawyer	446	0.890	0.313	0	1
Tenure of Judge	446	6.108	4.195	0	22

Note: The table reports the summary statistics for our sample of 8527 cases. The sample consists of rulings over cases from 1986-2019. 224 out of the 446 judges in our database were allotted real estate. The outcome and case characteristics averages are computed from averaging case-level outcomes. House Allotted is a dummy that takes the value of one when a judge receives the House. Panel A and B report outcomes, explanatory and case characteristics variables averaged at the case level. Panel C reports judge characteristics at the judge level. Mean, standard deviations, maximum and minimum values of the variables are reported in the table.

Table B1.2: Descriptive Statistics - Vs Federal Government

Variables	Observations	Mean	Std. Dev.	Min	Max
Panel A: Outcome variables and case characteristics					
State Wins	857	0.520	0.500	0	1
Merit	857	0.620	0.486	0	1
Constitutional Cases	857	0.935	0.247	0	1
Land Cases	857	0.540	0.499	0	1
Human Rights Cases	857	0.280	0.449	0	1
Criminal Cases	857	0.068	0.251	0	1
Pages Judgment Order	857	8.537	7.494	1	50
Chief Justice on Bench	857	0.055	0.228	0	1
Number of Lawyers	857	4.102	1.721	2	17
Number of Judges	857	1.758	0.883	1	5
Panel B: Explanatory Variables of Interest					
House Allotted (case level)	857	0.266	0.442	0	1
House Allotted (judge level)	223	0.57	0.496	0	1
Panel C: Judge Characteristics					
Gender	223	.951	.217	0	1
Muslim	223	.991	.094	0	1
Promoted to SC	223	.063	.243	0	1
Former Lower Court Judge	223	.09	.286	0	1
Fr. Office holder of Bar Ass.	223	.641	.481	0	1
Former Lawyer	223	.915	.28	0	1
Tenure of Judge	223	7.897	4.501	0	22

Note: The table reports the summary statistics for our sample of 857 case level observations. The sample consists of federal cases adjudicated by 223 judges ruling over cases from 1986-2019. 127 out of the 223 judges in our database were allotted real estate. The outcome and case characteristics averages are computed from averaging case level. House Allotted is a dummy which takes the value of one when a judge receives the House. Panel A and B reports outcomes, explanatory and case characteristics variables. Panel C reports judge characteristics, since they do not vary over-time we just report them at the judge level. The judge characteristics are absorbed in the regression with judge-fixed effects. Mean, standard deviations, maximum and minimum values of the variables are reported in the table.

Table B2: Joint Orthogonality Balance Test (Citizen Vs Federal Government)

	(1)	(2)	(3)
	<i>House Allotment</i>		
Criminal Case	0.252* [0.146]	0.164 [0.137]	0.301 [0.203]
Constitutional Case	0.220 [0.148]	0.130 [0.134]	0.202 [0.204]
Land Case	-0.0471* [0.0275]	-0.0453 [0.0279]	-0.0506* [0.0286]
Pages of Judgement Order	-0.00106 [0.00148]	-0.00158 [0.00164]	-0.00172 [0.00172]
No. of Lawyer on Case	0.00423 [0.00480]	0.00305 [0.00507]	0.00278 [0.00534]
No. of Judges on Bench	0.0105 [0.0114]	0.0120 [0.0124]	0.0182 [0.0145]
Chief Justice on Bench	0.0841 [0.0555]	0.0632 [0.0601]	0.0888 [0.0599]
Judge and Year FE	Yes	Yes	Yes
State by Year FE	No	Yes	No
District by Year FE	No	No	Yes
Observations	857	857	857
R-squared	0.822	0.851	0.859
p-values (Joint Significance)	0.222	0.321	0.0185
Mean	0.266	0.266	0.266
F Statistic (Joint Significance)	1.363	1.170	2.469

Note: This table reports the joint effect of case characteristics on house allocation. The equation includes judge, year and court-by-year fixed effects, as in the baseline equation (1). F statistics and corresponding p-values of the null of no relationship between all case characteristics available and included as controls and house allotted is displayed in the table. Standard errors are reported in brackets and are clustered at the judge level. The regressions are estimated on the sample presented in Column 3 of Table 1, specifically focusing on cases involving citizens and the federal government. * p<0.1, ** p<0.05, *** p<0.01.

Table B3: Land Expropriated due to House Allocation

Panel A: Land Expropriations Results			
	(1)	(2)	(3)
	Average Value	Minimum Value (Most Conservative)	Maximum Value
Total Land Expropriated (% of GDP)	1.024	0.543	1.449
Counterfactual: No House Allocation (% of GDP)	0.768	0.407	1.087
Land Expropriation Attributed to House Allocation (% of GDP)	0.256	0.136	0.362

Panel B: Land Expropriation Details

Year	Pakistan GDP (USD)	Total Land Expropriated (USD)	Total Land Expropriated (% of GDP)	Counterfactual: No House Allocation (% of GDP)	Land Expropriated Attributed to House Allocation (% of GDP)
1996-2000	332936000000	3888000000	1.168	0.876	0.292
2001-2005	478964000000	6941200000	1.449	1.087	0.362
2006-2010	805046000000	11020800000	1.369	1.027	0.342
2011-2015	1184106000000	6430800000	0.543	0.407	0.136
2016-2019	1176011000000	6969000000	0.593	0.444	0.148

Note: Panel A shows the results of the back-of-the-envelope calculations. The minimum value expropriated is for 2011, the maximum value is for the 2001-2005 group, while the average is the yearly average for our sample period of 1996-2019. Our computations suggest that house allocation to judges allowed the government to expropriate additional land worth 0.13 to 0.36 percent of GDP (average: 0.25 percent of GDP). The procedure for this calculation is as follows: Given the fact that in 45% of our 8500 sampled cases, the government was successful in expropriating land, and that we randomly sampled 0.2% of the total population of cases. Total land expropriation cases where the government was successful should be about 600,000, based on computations from a survey indicating that 30% of land cases involve direct expropriation of private property by the government, with an average value of USD 53,700 for the 57 expropriated properties in our sampled cases, and a coefficient estimate of 25%, we infer the amount of land expropriation due to house allocation as 0.13 to 0.36 percent of GDP every year. Panel B displays 4-averages to provide more information on the calculations. We do not estimate values before 1995 as the first allocation in the dataset used was in 1995.

Table B4: Impact of House Allocation on Judges' Peers (Citizen Vs Federal Government)

	(1)	(2)	(3)	(4)
	<i>State Wins</i>			
Fraction House Allotted in Bench	0.0957 [0.154]	0.0249 [0.116]	0.104 [0.162]	0.115 [0.102]
Judge and Year FE	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	Yes
State by Year FE	No	No	Yes	No
District by Year FE	No	No	No	Yes
Observations	284	284	284	284
R-squared	0.502	0.539	0.608	0.666
Mean	0.532	0.532	0.532	0.532

Note: This table reports the estimates on the effect of house allocation on non-house allocated judge peers. Specifically, it presents estimates of coefficient β from equation (1) with the state victory as the outcome variable. The explanatory variable is Fraction House Allotted in Bench. This is the proportion of judges in a bench within the court that receives real estate. A higher value denotes that a greater fraction of judges in the judge's bench are being allocated real estate. Controls include the number of pages of judgment order, presence of chief justice on the bench, number of judges on the case, number of lawyers on the case and dummies for type of cases. Standard errors are reported in brackets and are clustered at the level of variation of the treatment variable that is at the bench level. The regressions are estimated on the sample presented in Column 3 of Table 1, specifically focusing on cases involving citizens and the federal government. * $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table B5: Impact of House Allocation on State Victories - Dropping Extremes

	(1)	(2)	(3)	(4)
Panel A: 10% Dropped				
	<i>State Wins</i>			
House Allotment	0.298*** [0.0973]	0.286*** [0.0935]	0.332*** [0.0996]	0.265*** [0.0853]
Judge and Year FE	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	Yes
State by Year FE	No	No	Yes	No
District by Year FE	No	No	No	Yes
Observations	791	791	791	791
R-squared	0.343	0.392	0.465	0.550
Mean	0.513	0.513	0.513	0.513
Panel B: 5% Dropped				
	<i>State Wins</i>			
House Allotment	0.288*** [0.0924]	0.278*** [0.0891]	0.312*** [0.0953]	0.263*** [0.0808]
Judge and Year FE	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	Yes
State by Year FE	No	No	Yes	No
District by Year FE	No	No	No	Yes
Observations	852	852	852	852
R-squared	0.374	0.420	0.489	0.565
Mean	0.519	0.519	0.519	0.519

Note: This table explores the effect of house allocation on government victories by excluding extreme values. Panel A removes top 10 and bottom 10 percentile of state victories from the sample. Specifically, it presents estimates of coefficient β from equation (1) with the state victory variable as the dependent variable in this sample. House Allotment is a binary variable that turns on for a particular judge in the year when the judge receives the real estate. Controls include number of pages of judgment order, presence of chief justice on the bench, number of judges on the case, number of lawyers on the case and dummies for type of cases. The estimates from the baseline equation with judge, year and district court by year fixed effects are reported in Column 4. Panel B removes top and bottom 5 percentile of state victories from the sample. Standard errors are reported in brackets and are clustered at the judge level. The regressions are estimated on the sample presented in Column 3 of Table 1, specifically focusing on cases involving citizens and the federal government. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table B6: Impact of House Allocation on State Victories - Dropping political capitals

	(1)	(2)	(3)	(4)	(5)
	Without Lahore	Without Quetta	Without Peshawar	Without Karachi	Without Islamabad
	<i>State Wins</i>				
Home Allotment	0.229** [0.106]	0.257*** [0.0930]	0.282*** [0.0918]	0.254*** [0.0948]	0.296*** [0.0928]
Judge and Year FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
District by Year FE	Yes	Yes	Yes	Yes	Yes
Observations	742	794	797	829	778
R-squared	0.583	0.559	0.573	0.571	0.581
Mean	0.523	0.510	0.518	0.527	0.522

Note: This table explores the effect of house allocation on government victories. Specifically, it presents estimates of coefficient β from equation (1) with the state victory variable as the dependent variable. We progressively exclude cases in courts from political capitals of Lahore, Quetta, Peshawar, Karachi and Islamabad from the sample and estimate equation (1). House Allocated is a binary variable that turns on for a particular judge in the year when the judge receives the real estate. Controls include number of pages of judgment order, presence of chief justice on the bench, number of judges on the case, number of lawyers on the case and dummies for type of cases. The estimates from the baseline equation with judge, year and district court by year fixed effects are reported in all columns. Standard errors are reported in brackets and are clustered at the judge level. The regressions are estimated on the sample presented in Column 3 of Table 1, specifically focusing on cases involving citizens and the federal government. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table B7: Impact of House Allocation on State victories - Alternate Clustering

	(1)	(2)	(3)	(4)
	<i>State Wins</i>			
House Allotment	0.288***	0.277***	0.312***	0.263***
Standard Errors - Judge-level Clustering (baseline)	[0.0924]	[0.0890]	[0.0949]	[0.0807]
Standard Errors - Judge by Year Clustering	(0.0837)	(0.0775)	(0.0869)	(0.0662)
Standard Errors - Court by Year Clustering	{0.0454}	{0.0407}	{0.0531}	{0.0440}
Standard Errors - Court-level Clustering	«0.0612»	«0.0612»	«0.0677»	«0.0630»
Standard Errors - Birth-city Clustering	⟨0.0944⟩	⟨0.0847⟩	⟨0.0887⟩	⟨0.0782⟩
Judge and Year FE	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	Yes
State by Year FE	No	No	Yes	No
District by Year FE	No	No	No	Yes
Observations	857	857	857	857
R-squared	0.376	0.421	0.490	0.565
Mean	0.520	0.520	0.520	0.520

Note: This table explores the effect of house allocation on government victories. Specifically, it presents estimates of coefficient β from equation (1) with the state victory variable as the dependent variable. The standard errors are clustered at various levels: brackets represent the baseline clustering at the judge level (Row 1), parentheses represent clustering at the judge by year level (Row 2), curly braces represent clustering at the court by year level (Row 3), guillemet braces represent clustering at the court level (Row 4), and angle braces represent clustering at the City of Birth Level (Row 5). House Allocated is a binary variable that turns on for a particular judge in the year when the judge receives the real estate. Controls include number of pages of judgment order, presence of chief justice on the bench, number of judges on the case, number of lawyers on the case and dummies for type of cases. The estimates from the baseline equation with judge, year and district high court by year fixed effects are reported in Column (4) of the table. The regressions are estimated on the sample presented in Column 3 of Table 1, specifically focusing on cases involving citizens and the federal government. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table B8: Impact of House Allocation on State victories - Robustness at the Judge Year level

	(1)	(2)	(3)	(4)
	<i>State Wins</i>			
House Allotment	0.290*** [0.0947]	0.297*** [0.0961]	0.287*** [0.104]	0.284*** [0.0856]
Judge and Year FE	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	Yes
State by Year FE	No	No	Yes	No
District by Year FE	No	No	No	Yes
Observations	474	474	474	474
R-squared	0.479	0.536	0.613	0.692
Mean	0.559	0.559	0.559	0.559

Note: This table explores the effect of house allocation on government victories. Specifically, it presents estimates of coefficient β from equation (1) with the state victory variable as the dependent variable. The outcome variable is averaged at the judge-year level. House Allocated is a binary variable that turns on for a particular judge in the year when the judge receives the real estate. Controls include number of pages of judgment order, presence of chief justice on the bench, number of judges on the case, number of lawyers on the case and dummies for type of cases. The estimates from the baseline equation with judge, year and district court by year fixed effects are reported in Column 4 of the table. Standard errors are reported in brackets and are clustered at the judge level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table B9: House Allocation on State victories - Judicial Selection Reform of 2010

	(1)	(2)	(3)	(4)
<i>State Wins</i>				
Panel A: After judge selection reform of 2010				
House Allotted	0.246 [0.159]	0.315* [0.160]	0.491*** [0.185]	0.372** [0.160]
Controls	No	Yes	Yes	Yes
Judge and Year FE	Yes	Yes	Yes	Yes
State by Year FE	No	No	Yes	No
District by Year FE	No	No	No	Yes
Observations	304	304	304	304
R-squared	0.535	0.553	0.605	0.668
Mean	0.470	0.470	0.470	0.470
Panel B: Before Judge Selection Reform of 2010				
House Allotted	0.346*** [0.129]	0.294** [0.125]	0.278* [0.143]	0.279** [0.131]
Controls	No	Yes	Yes	Yes
Judge and Year FE	Yes	Yes	Yes	Yes
State by Year FE	No	No	Yes	Yes
District by Year FE	No	No	No	Yes
Observations	521	521	521	521
R-squared	0.336	0.400	0.462	0.555
Mean of dependent variable	0.547	0.547	0.547	0.547

Note: This table examines the impact of house allocation on government victories in five-year time periods. Specifically, it presents the estimates of coefficient β from equation (1) with state victories as the outcome variable. In Panel A, the sample is restricted to cases decided after 2010 i.e. those ruled following a major judge selection reform (Mehmood, 2022). In Panel B, the sample is restricted to cases before 2010 i.e. before the judge selection reform. House Allocated is a binary variable that turns on for a particular judge in the year when the judge receives real estate. In all columns, the following baseline case characteristics are included as controls: number of pages of judgment order, the presence of the chief justice on the bench, the number of judges on the case, the number of lawyers on the case, and dummies for type of cases. Standard errors are reported in brackets and are clustered at the judge level. The regressions are estimated on the sample presented in Column 3 of Table 1, specifically focusing on cases involving citizens and the federal government.*** p<0.01, ** p<0.05, * p<0.1.

Table B10: Impact on State Wins by Type of Case

	(1)	(2)	(3)
	Land Cases	Human Rights Cases	Petty Crime Cases
	<i>State Wins</i>		
House Allotted	0.126*** [0.0440]	0.0149** [0.0603]	0.0598 [0.159]
District and Year Fixed Effects	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Observations	3,443	2,339	787
R-squared	0.377	0.513	0.564
Mean	0.457	0.449	0.549

Note: This table explores the effect of house allocation on government victories by the type of government. Specifically, it presents estimates of coefficient β from equation (1) with state victories as the outcome variable. The outcome variable is state wins, a dummy that takes the value of one if the state wins in a case and zero otherwise. Column (1) covers cases involving land expropriation, Column (2) covers cases pertaining to expropriation of political rights. House Allocated is a binary variable that turns on for a particular judge in the year when the judge receives real estate. Controls include number of pages of judgment order, presence of chief justice on the bench, number of judges on the case, number of lawyers on the case and dummies for type of cases. Unit of analysis in the case level. All columns always include judge, year and district court-by-year fixed effects. Standard errors are reported in brackets and are clustered at the judge level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table B11: Goodman-Bacon Decomposition

	(1)	(2)
	Coefficient	Total Weight
Early treated vs. late treated	0.160	0.045
Late treated vs. early treated	0.229	0.003
Early and late treated vs. never treated	0.199	0.953

Note: This table reports the results from the difference-in-differences decomposition of Goodman-Bacon (2021). It re-estimates Table 1 column (3); The decomposition, in the first row, presents comparisons amongst early treated vs. late treated as the control (before they received treatment) along with the weight associated to this group. The second row presents a comparison amongst late treated vs. early treated as the control groups (the forbidden comparison), along with its weight. The third column presents comparisons of early or late treated vs units that never received treatment (treated vs. untreated) along with associated weights

Table B12: Robustness of Merit Decision Results - vs Federal Government

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Trimmed	Effect on Peers	Court by Year Clustering	No Lahore	No Quetta	No Peshawar	No Karachi	No Islamabad
<i>Case Merit</i>								
House Allotted	-0.189*** [0.0840]	-0.0473 [0.0794]	-0.321*** [0.108]	-0.271* [0.150]	-0.278** [0.126]	-0.358*** [0.126]	-0.328*** [0.126]	-0.352*** [0.118]
Judge & Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Year FE	No	No	No	Yes	Yes	Yes	Yes	Yes
District-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	381	300	414	349	365	366	389	373
R-squared	0.680	0.636	0.674	0.679	0.657	0.678	0.699	0.695
Mean	0.610	0.620	0.606	0.582	0.595	0.602	0.582	0.584

Note: This table examines the impact of house allocation on the merit of decisions. Specifically, it presents estimates of coefficient β from equation (1) with merit of decision as the outcome variable. Column (1) estimates the coefficient at the case level. Column (2) uses a restricted sample by dropping the top and bottom 10% of extreme decisions i.e. highest and lowest 10% of merit decisions are dropped in the regression. Column (3) estimates the effect of house allocation on judge peers' meritorious decisions. Column (4) reports estimates of the standard errors at court by year clustering. Columns (5)-(9) estimate the coefficients by dropping political capitals progressively. The outcome variable is merit decisions, and House Allocated is a binary variable that turns on for a particular judge in the year when the judge receives real estate. Controls include time-varying case characteristics that include number of pages of judgment order, presence of chief justice on the bench, number of judges on the case, number of lawyers on the case, and dummies for case type. Standard errors are reported in brackets. The regressions are estimated on the sample presented in Column 3 of Table 1, specifically focusing on cases involving citizens and the federal government. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.