Selecting Top Bureaucrats: Admission Exams and Performance in Brazil*

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January 23, 2023

Abstract

In the absence of strong incentives, public service delivery crucially depends on bureaucrat selection. Despite wide adoption by governments, it is unclear whether civil service examinations reliably select for job performance. We investigate this question focusing on state judges in Brazil. Exploring monthly data on judicial output and cross-court movement, we estimate that judges account for at least 23% of the observed variation in number of cases disposed. With novel data on admission examinations, we show that judges with higher grades perform better than lower-ranked peers. Our results suggest competitive examinations can be an effective way to screen candidates.

JEL Codes: D73, J45, M50

*We thank Ray Fisman, three anonymous referees, Ernesto Dal Bó, Alessandra Fenizia, Fred Finan, Guilherme Lambais, Guo Xu, Carlos Henrique Corseuil, Alexandre Samy, and audiences at SBE, Lacea-Lames, USP, UFJF, Berkeley Development Lunch, and SIOE for helpful comments.

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1 Introduction

Public employees play a key role in designing and delivering essential public services to development worldwide (Finan, Olken, & Pande, 2017). Recent studies have focused on the role that incentives and monitoring can play to improve the performance of government employees, particularly of frontline providers (Ashraf, Bandiera, & Jack, 2014; Khan, Khwaja, & Olken, 2019; Lavy, 2009). However, the impact of such tools is limited for the typical bureaucrat whose career is often characterized by tenure benefits and absence of performance pay (Bertrand, Burgess, Chawla, & Xu, 2020). In the face of such restrictions, especially to dismiss public employees, the issue of how to select bureaucrats becomes essential.

One widely used selection mechanism is competitive examinations. These may reduce corruption and patronage in hiring by political leaders (Colonnelli, Prem, & Teso, 2020; Brollo, Forquesato, & Gozzi, 2017; Weaver, 2021), but potentially at the expense of assessing candidates' soft and noncognitive skills (Hoffman & Tadelis, 2021; Hanna & Wang, 2017).[1] Further, it is an open empirical question, and a highly policy-relevant one, as to whether examinations reliably select the candidates who perform better on the job.

In this paper we study the role of exams in the selection of an important group of public sector employees in Brazil: state judges. Similar to the majority of civil servants in the country, judges are selected through highly competitive and mostly impersonal examinations, comprised of written and oral exams. Candidates are ranked based on their grades and top performers are offered jobs based on pre-specified number of available positions. Our estimates suggest that, within selected candidates, those ranking higher in exams are also high performers on the job as judges. In terms of magnitudes, we show that candidates that rank in the top quintile in their admission exam cohort dispose of approximately 20% more cases on a monthly basis than those in the bottom quintile.

The first step of our analysis is to estimate judge-level measures of performance. To do so, we leverage novel administrative data to construct a panel of judicial productivity at the

[1] These characteristics of public sector recruitment differ markedly from what is observed in the private sector, where managers and human resources officers have wide discretion in selecting employees and subjective assessments play an important role through interviews, for example (Hoffman, Kahn, & Li, 2018).

judge-court-month level, covering the universe of state judges working in Brazil from 2009 through 2015. Across the 76 months encompassed by our data, judges often work in several different courts. This high level of mobility allows us to estimate a two-way fixed-effects model akin to those in the labor literature decomposing wage variation between worker and firm fixed-effects. We separately estimate judge and court fixed-effects, and show that judges are important in explaining the observed variation in output: using bias-adjusted estimates, individuals' fixed effects account for at least 23% of the variation in the number of cases disposed.

We focus on the number of cases disposed for two reasons. First, timely delivery of judicial decisions is critical in developing countries. At the current pace of case disposition, it would take Brazilian courts three years to clear the backlog, assuming no additional cases were initiated (Conselho Nacional de Justiça, 2018). Ponticelli & Alencar (2016) show that judicial timeliness matters for important economic outcomes. They exploit differences in court congestion across Brazilian municipalities to show that a bankruptcy reform has larger effects on investment and financial access of firms located in districts with more efficient courts. Second, the speed with which judges dispose of cases is considered an important indicator of performance by the judicial branch in Brazil and is used, along with other considerations, to define promotions throughout the career of judges.

Yet, theory suggests that if the quantity of cases disposed is easily observable but quality is not, judges might divert efforts into the observable dimension of performance (e.g. speed), possibly to the detriment of quality (Holmstrom & Milgrom, 1991). One implication of that hypothesis is that fast judges might sacrifice important inputs in the "production process" of case disposition in order to increase output quantity. We leverage our detailed microdata to show that this is not the case for one important input for court decisions: the number of hearings held by judges. We re-estimate our two-way fixed-effects model using hearings as the dependent variable and show a strong, positive correlation between judges fixed-effects in both models. This shows that faster judges are not decreasing the number of hearings, one important input for high-quality case decision.[2]

[2]As we will discuss in Section 3, a natural measure of quality of case disposition would be the likelihood

Next, we examine whether judges highly ranked on the entrance exam actually perform better on the job. We collect novel data on admission exams for over 25% of all state judges working in Brazil during the period covered by our productivity dataset, including their final rankings and grades. Our results suggest a positive and strong correlation between the admission exam and on-the-job performances: we estimate that, within cohorts, being ranked in the top quintile of one's admission examination is correlated with a 0.2 s.d. increase in estimated FE when compared to the bottom quintile. This result is robust to different measures of performance both in exams and on the job; the results are also robust when excluding the top and bottom candidates in each cohort. We additionally use case-level data for one state to show that differential case characteristics do not explain our results: ranking on admission exams does not predict the composition of cases disposed of.

Taken as a whole, our results suggest that admission exams are able to rank candidates in a way consistent with their future performance on the job.[3][4] In order to make progress in understanding which dimensions of the exams are most relevant for future performance, we restrict our sample to a subset of judges for which we can break-down final grades in each of the recruitment phases and consider whether achievement in any of the specific exams is

of case reversals in higher courts. However, there is no such systematized data for Brazil in the time period we study. Another common measure of judicial quality, particularly in common law countries, are citations of judicial decisions (Landes, Lessig, & Solimine, 1998). These are not as common in civil law countries, such as Brazil, particularly in first instances courts such as the ones we study.

[3]The implications of sidelining any subjective assessments of candidates' qualities for job performance are not obvious. If knowledge about objective exam content is the crucial requirement to perform well, or if subjective traits that predict exam performance are also correlated with service delivery capacity, then objective recruitment strategies might be simultaneously effective and impartial. If certain subjective characteristics are very relevant to perform well on the job but hard to capture on objective admission examinations, nonetheless, these recruitment strategies are maintaining impartiality at the expense of accuracy.

[4]We recognize that admission exams can also have direct impact on the future performance of candidates. In section 5.4 we show that top ranked judges seem (weakly) more likely to be promoted and also stay longer in courts – but we are unable to assert whether these are direct effects of the admission performance since they are endogenously determined after the selection process.

particularly predictive of performance on the bench. Across different specifications, grades on the Judicial Decision Writing exam, where candidates are given a hypothetical case and asked to produce a decision, are the strongest predictors of performance. While these correlations should be interpreted with caution, they suggest that the use of impersonal examinations to screen candidates might be particularly efficient if focused on "practical" exams that mimic the situations faced by employees on the job.

Our paper makes contributions to three strands of literature. First, we add to the recent literature on the selection of workers in the public sector, mostly focused on the role of wages and career benefits in selection (Dal Bó, Finan, & Rossi, 2013; Deserranno, 2019; Ashraf, Bandiera, Davenport, & Lee, 2020). Callen, Gulzar, Hasanain, Khan, & Rezaee (2022) investigate the complementary between incentives and selection and find that health workers with better personality traits present higher performance and respond more to incentives. We study the role of impersonal admission examinations, aimed precisely at avoiding the kind of patronage documented in contexts as different as colonial governors in the British Empire (Xu, 2018) and public officials hired at the discretion of newly elected politicians in Brazil (Colonnelli, Prem, & Teso, 2020; Brollo, Forquesato, & Gozzi, 2017). In the context of courts in Pakistan, Mehmood (2022) documents that politically appointed judges are more likely to rule in favor of the government. However, the use of discretion when selecting officials need not lead to negative selection of providers. In an extreme example, Weaver (2021) shows that the selection of supervisors of community health workers by outright bribery leads to high quality workers being hired, since wealth and performance are strongly positively correlated.

To the best of our knowledge, our paper is the first to show that competitive examinations successfully select magistrates with higher case disposition rates and to quantify the importance of judges to court case disposition. We contribute to a nascent literature documenting that performance on impersonal admission exams, the dominant screening process for public sector employees in several countries, is informative about performance on the job.[5]

[5] Aman-Rana (2022) documents that public officials ranked at the top 10% of their admission cohorts in Punjab, Pakistan, also collect more taxes. Bertrand, Burgess, Chawla, & Xu (2020) documents a positive correlation between admission exam rankings and performance measured by 360 degree evaluations of IAS officers in India.

Our research also adds to efforts of measuring the role of bureaucrats in determining public sector performance (Finan, Olken, & Pande, 2017). While the relevance of front-line service providers like teachers (Chetty, Friedman, & Rockoff, 2014; Muralidharan & Sundararaman, 2011; Duflo, Dupas, & Kremer, 2015; Jacob, Rockoff, Taylor, Lindy, & Rosen, 2018) and community health workers (Deserranno, 2019; Ashraf, Bandiera, Davenport, & Lee, 2020; Weaver, 2021; Dal Bó, Finan, & Rossi, 2013) have been extensively discussed, the role of other decision-makers in the public sector bureaucracy has only recently garnered more attention. Our empirical strategy, exploring the movement of judges between courts to identify individual fixed-effects, is particularly related to the work of Best, Hjort, & Szakonyi (2019) on the role of procurement officers in Russia in explaining price dispersion in public purchases, and of Fenizia (2022) on how managers of Social Security offices in Italy explain variation in productivity.

Lastly, we contribute with new evidence about the determinants of judicial efficiency in the developing world. Research in Pakistan (Chemin, 2009), Senegal (Kondylis & Stein, 2021) and Mexico (Sadka, Seira, & Woodruff, 2018) has shown that judicial reforms aimed at simplifying procedures and speeding up the disposition of cases can be effective. Kondylis & Stein (2021), in particular, collect rich data at the case-level and show that higher speed in commercial courts in Senegal does not seem to affect the quality of decisions. The effects of judicial reforms more broadly also depend on the capacity of courts to deliver timely decisions, as shown in Ponticelli & Alencar (2016) and Rao (2021). In addition, reforms that limit the capacity of politicians to influence judges might strengthen the rule of law (Mehmood & Seror, 2022; Lambais & Sigstad, 2022). To the best of our knowledge, our paper is the first to perform a two-way fixed effects decomposition to quantify the importance of judges to court efficiency and, further, to show that competitive examinations successfully select the most efficient magistrates.

The remainder of this paper is organized as follows. Section 2 describes the structure of Brazilian courts and the admission process for judges. Section 3 presents the data used, provides descriptive statistics and explains how we obtain the sample used when estimating the two-way fixed-effects model. Section 4 describes our empirical model, identification and estimation procedures. The main results are presented in Section 5, while Section 6 concludes

and discusses avenues for future research.

2 Institutional Context

2.1 Brazilian Courts

The Brazilian Judiciary is comprised of five branches: State, Federal, Electoral, Labor and Military Courts. This paper uses data exclusively from State courts, which cover all cases that are not specifically under the competency of the other branches (that is, State courts have residual judicial competency). The majority of criminal and civil cases fall under the competence of State courts: in 2017, over 60% of all cases in the Judiciary were allocated to the first instance of these courts (Conselho Nacional de Justiça, 2018).

Each of the 27 Brazilian federative units (26 states plus the federal district) is responsible for establishing and organizing the state courts. Within each state, the main administrative units of state justice are the judicial districts (*comarcas*), which encompass one or more municipalities. Judicial districts are mainly divided in three administrative levels, related to the underlying demand for judicial services: *first level* districts are located in rural or less urbanized municipalities and contain a single court of general competency (i.e. it covers all types of cases); *second level* districts are located in municipalities with smaller cities and encompass specialized courts, often separate Civil and Criminal courts; while *third level* districts encompass the state capital and possibly other large cities, and include several specialized courts.

Court congestion is considered a serious impediment to the efficient application of justice in Brazil (Ponticelli & Alencar, 2016): at the state level, there were over 60 million cases allocated to courts in 2017. If no more cases entered the justice system and current levels of productivity were held constant, it would take almost three years to clear the backlog (Conselho Nacional de Justiça, 2018). While overall congestion is very high, there exists a large dispersion among judicial districts not fully explained by simple regional differences: Schiavon (2017) shows that the dispersion of several congestion and performance measures is larger within states than between states, highlighting the relevance of local determinants in explaining variation in performance.

The importance of timely decisions by courts and the challenges faced by the Brazilian Judiciary in that regard have not escaped the attention of policy-makers and legislators. For example, the 2004 Constitutional Amendment that created the National Justice Council, among several other sweeping changes to the organization of the Judiciary, also included specific language requiring that the promotion of judges take into account "objective criteria of productivity".[6] During the launch of the Open Justice System, in 2008, a Supreme Court Justice praised the tool as a way to "improve the management of justice and decrease the slowness of decisions".[7]

2.2 Selection of judges through competitive examinations

The broad rules for recruitment of judges are determined by Article 93 of the Brazilian Constitution. It states that all judges should be selected through public examinations (*Concursos Públicos*); since 2004, a Constitutional Amendment also institutes the requirement of three years of professional judicial experience. Judgeship admission exams are highly competitive (the ratio of candidates per position often exceeds 100), not only due to the prestige of the position but also likely because it is among the highest paid in the public sector.[8] Until 2009, federal law did not detail the content or structure of these examinations, which were left to the discretion of State courts. Since then, the structure of exams, including minimum content, qualification thresholds in each phase and weights for final ranking were harmonized.[9]

[6]Constitutional Amendment n.45, December 30th 2004.

[7]Available at https://www.estadao.com.br/noticias/geral,para-stf-criticas-ao-justica-aberta-sao-infundadas,195051. Accessed 08/10/2020.

[8]While the Constitution establishes that wages in the public sector should not surpass those of Supreme Court Justices, set at BRL 33,763 (approximately USD 8,500) per month until 2018, the vast majority of judges receive total compensation significantly higher than that due to fringe benefits not included in the above mentioned rule. In fact, in Table A1 we compare average nominal wages for judges and various other occupational categories between 2003 and 2019. We find that in 2019 judges' wages were significantly higher than federal government (257%), private sector (1502%), and other groups. The only comparable category is attorneys, with an average monthly wage of BRL 36,768 in 2019.

[9]National Justice Council Resolution 75 05/12/2009

In practice, nonetheless, the overall structure of these examinations was already rather similar across states. Upon deciding to hire new judges, courts publicly announce the beginning of a *Concurso* through a call for applications, informing how many positions are available and details about the timeline, content and structure of examinations. Potential candidates must enroll online and pay a fee[10] in order to be considered eligible for the position.

Most examinations are comprised of four phases: Multiple Choice, Written, Judicial Decision Writing and Oral Exams. The first phase is often a *Multiple Choice Exam* covering a wide range of topics: constitutional, civil, criminal, commercial, administrative and family law are among the themes covered. Like the other three phases, this exam is both qualifying, meaning that candidates with performance below a certain threshold are immediately eliminated, and classifying, since the grade received is a component of the weighted average that determines the final ranking of candidates. Those approved in the Multiple Choice phase are invited to take a *Written Examination* that encompasses the same topics mentioned before and also topics such as the sociology and philosophy of law, and ethics. The following phase is a *Judicial Decision Writing*, also called a "practical exam", where candidates are given a hypothetical case and asked to write a judicial decision. In most cases this phase includes two decisions, one in criminal and another in civil law. The last qualifying phase is the *Oral Exam*. Candidates are randomly assigned a topic from a pre-determined list 24 hours before their examination, and are then expected to answer questions from a committee composed of other judges and attorneys.

Candidates approved in the *Oral Exam* are eligible to be in the final ranking that defines hiring. Other than the grades in each of the previous phases, the final score also includes the so called *Titles Exam* (*Exame de Títulos*), additional points for career and academic achievements, such as previous judgeship, professorship or advanced degree in Law, and publications in Law journals. Since 2009, the weights that define the final score are the following: 10% Multiple Choice, 30% Written Exam, 30% Judicial Decision Writing, 20% Oral Exam and 10% Title Exam. Candidates are ranked according to their final grades and the top performers are offered jobs

[10]Resolution 75 determines that the fee can be no greater than 1% of the gross monthly salary for the position, which amounts to about BRL 300, or USD 75.

according to the number of vacancies available.

It is worth briefly mentioning that these recruitment processes are considered transparent and free from undue influence of judges or politicians, unlike the hiring for other public sector positions which are heavily influenced by patronage practices (Colonnelli, Prem, & Teso, 2020; Brollo, Forquesato, & Gozzi, 2017; Barbosa & Ferreira, 2021). First, every step of the process is highly publicized: grades and lists of approved candidates in each phase are made public, as is the content of each exam. The composition of the committee writing exams and participating in the Oral tests is also made public at the beginning of the *Concurso*, and candidates can appeal for the exclusion of members (e.g. due to family ties of members to any candidate).[11] Second, any deviation from the stipulated rules regarding exams often leads candidates to sue and annul specific phases or even the entire recruitment process. In 2014, for example, candidates in the state of Para successfully sued to have their Oral exams annulled after being asked only three questions during the evaluation, while the call for applications determined four questions.[12] In that sense, the selection process of judges is believed to be broadly free from corruption and to reflect the performance of candidates.[13]

[11]Graders are blind to the identity of exam-takers in the Multiple Choice, Written and Judicial Decision Writing phases. In the Oral exam candidates present in front of a committee and therefore identities are known to graders.

[12]Available at: http://cnj.jus.br/noticias/cnj/61524-cnj-anula-prova-oral-de-concurso-para-ingresso-na-magistratura-do-tjpa. Accessed 08/10/2020.

[13]Exceptions do exist. In 2010 the Supreme Court ruled in favor of candidates asking for the annulment of a *Concurso* in the state of Minas Gerais, arguing that more candidates were accepted to the second phase of the process than initially announced. Two daughters of an appellate judge from that state were benefited (Available at: https://www1.folha.uol.com.br/fsp/poder/po2606201029.htm. Accessed at 08/10/2020)

2.3 Judges' careers and allocation of cases

Once hired, judges are considered "substitute judges" for a period of two years, a probational stage before gaining tenure protection[14]. After this period judges can only be dismissed if convicted of crimes or found guilty of administrative infractions. In practice, this is very rare: between 2005 and 2017, only 82 judges in the entire Judicial branch were punished by the National Justice Council, and 53 of those received "mandatory retirement", meaning they were excluded from judgeship but kept receiving salaries.[15]

As previously discussed, judicial districts are divided in three levels: first, second and third. This administrative division is directly linked to judges' careers. Substitute judges are often allocated to first level districts, where they work in general courts, dealing with all types of judicial cases. Promotion means being reallocated to a higher level district, which comes with wage increases. After achieving third level status, judges can be promoted to appellate courts, meaning they leave the first instance (and our database).

The allocation of magistrates to judicial districts is governed by the Constitution. One of the core principles considered is that of the *immovability* of judges, meaning that judges cannot be transferred from their assigned district without their consent[16]. The first placement of judges is determined as follows. They face a list of courts where they could work as substitutes and are invited to choose their preferred vacancy. Those with higher admission grades can choose vacancies first. The subsequent assignments of judges to different districts takes place through vertical or horizontal promotion. The vertical promotion occurs when judges move to an upper-level district, receive a wage increase, and advance in their careers. The horizontal promotion occurs when judges move to a same-level district, while

[14]There are no aggregate statistics on the share of judges dismissed in the probational stage, but conversations with members of the judiciary suggest these are extremely rare: very few judges nationwide are denied tenure.

[15]Available at: https://g1.globo.com/politica/noticia/cnj-puniu-82-juizes-no-brasil-desde-2005-53-deles-continuam-recebendo-salario.ghtml. Accessed on 08/10/2020.

[16]The principle is supposed to protect the public against the undue influence of politicians who might want to exclude a judge from judging a case in which they have an interest, for example, but it is also a clear benefit to judges who are only reassigned if they so decide.

their salary and career are unchanged. To be promoted, the judge must apply to fill a vacancy in a court. Then, a committee assesses whether the candidate meets the necessary conditions to be promoted and selects the best candidate considering alternately the criteria of merit or experience[17]. The promotion process must be completed within 40 days.

Besides the movement of judges between different districts, we also explore the movement of judges between courts within the same district in our empirical exercises. As described in Table 1, magistrates frequently work in more than one court per period. Most judges are officially assigned to a single court through the promotion process and work concurrently in other courts to cover vacations or leave for other magistrates, meeting the State Court's demand. This should make clear that in no way do we argue that the movement of judges between courts is quasi-random. The identification of judges' fixed-effect, therefore, does not rely on the exogenous allocation of judges to courts; our model allows for rich patterns of endogenous matching between judges and courts, and as discussed in detail below only rules out specific types of matches.

Finally, it is important to note that the distribution of cases among judges is as good as random. In judicial districts where there is only one court, cases will be randomly assigned to one judge in that court. For larger districts that encompass specialized courts, cases will be assigned to the proper court depending on their topics or, in the case where more than one relevant court exists, randomly assigned to one of the courts and a judge.[18] That should allay concerns that, within courts, different judges will have a distinct composition of cases, making it harder to interpret the number of cases disposed of.

[17]To be promoted, the candidate must fulfill the following conditions: two years of experience in the previous position, more years of experience in the position than 80% of judges who are in the same state and career stage, no disciplinary proceedings in the last twelve months, and non-existence of records withheld unjustifiably beyond the legal term. The second criterion can be relaxed if no candidates satisfy it.

[18]The method used to implement the random allocation varies from state to state. While we do not have case-level data to check the balance for the entire country, on Appendix C we present additional data for the state of São Paulo and show in Table A10 that judges' admission ranking is not predictive of the composition of cases they dispose of.

3 Data and descriptive statistics

3.1 Data sources

This paper uses three main data sources: information on monthly output of judges and courts provided by the Open Justice System, admission exams rankings collected from several different sources and administrative data on formal employment (*RAIS*).

All data on judicial performance come from the Open Justice System (*Sistema Justiça Aberta*), an online platform maintained by the National Justice Council (*Conselho Nacional de Justiça – CNJ*).[19] The Open Justice System provides monthly information, supplied by courts, on a range of quantitative outcomes at both the court and judge levels, including the number of cases disposed, hearings and intermediary decisions.

We construct a panel at the judge-court-month level: each observation is a vector of quantitative outcomes related to a judge working on a given court in a specific month. The dataset covers the universe of state judges working on first instance courts (i.e. excluding appeal level) from January 2009 through April 2015,[20] and we construct unique IDs using judges' full names to track the movement of magistrates between courts over time.

Our preferred measure of judges' performance is the number of *cases disposed on merits* in a given court and month. This refers to the number of cases for which the judge has issued a final decision based on the merits of the process, i.e., it excludes any cases terminated for procedural reasons or by a decision of one of the parts to withdraw. The decision to exclude cases decided for other reasons rather than on the merits of the process is an attempt to reduce the possible noise introduced by considering cases that are concluded for reasons unrelated to the judges' efforts.

[19] The National Justice Council was created in 2004, through a Constitutional Amendment, with the goals of improving the efficiency and transparency of the Brazilian judiciary. Among other tasks, the Council receives complaints from citizens against members of the judiciary, promotes tools to improve the efficient functioning of the courts and publishes data on judicial efficiency.

[20] The Open Justice System was extinguished in 2015, and replaced by a new system later that year. The new dataset, nonetheless, is not strictly comparable to the data we use.

Figure 1 presents preliminary evidence on the dispersion of judges' output. We plot the histogram of average monthly number of cases disposed at the judge level, across the entire panel. There is remarkable dispersion: judges on the 10th percentile of the distribution dispose of 11 cases on the merits on average, while judges on the 90th percentile dispose of 8 times as many. This dispersion reflects several forces, including potentially judges' efforts and capacity to make the court function efficiently, but also levels of demand in different courts.[21] We will attempt to disentangle these determinants with our empirical model.

The data on admission examinations (*Concursos*) was collected from a variety of sources. Results of *Concursos* are mandated to be public and are often published in PDF format either on the website of the State courts hiring or by the private institutions hired by the state to manage and implement the recruitment process. We scraped these documents and constructed a database of candidates' exam performance. We have collected data for 79 recruitment waves for the selection of Judges from 24 different states in the period 2000-2013. For all these examinations the final ranking of approved candidates is available; for a subset of them, we also collect the final grade and the individual grades in all phases of the exam.[22] We then match judges' grades with performance using full names and state of judgeship.[23] We are able to match over 2,800 judges observed in the productivity dataset to their admission examination performance, covering over 25% of all state judges working at some point between 2009 and 2015.

One additional data source used to recover information from judges' careers is administrative matched employer-employee data from *RAIS* (*Relação Anual de Informações Sociais*) for the period 1995-2017. We use unique individual identification numbers (*CPF – Cadastro de Pessoa Física*) to follow individuals over the years, and then match workers at RAIS to the judge

[21] Moreover, it is likely not driven by variation in backlogs across courts because the judicial system as a whole faces large excess demand in cases (Ponticelli & Alencar, 2016).

[22] Recent recruitment processes always include results for all the phases of the examinations. As we go back in time, nonetheless, the information available online becomes more scant. The minimal information we require to include an examination in the dataset is the nominal list of approved candidates and their final rankings.

[23] We benefit from the fact that Brazilians often hold several last names, which makes precise matches on names feasible.

productivity database using full names. We are able to uniquely match approximately 9,400 judges between the two datasets, or 80% of all judges observed in the productivity dataset in the period 2009-2015. We use RAIS data to obtain information on judges' gender, education, formal labor market experience, experience as judges and wages (prior to and during judgeship).

3.2 Sample Selection and Descriptive Statistics

The complete productivity dataset comprises close to one million observations at the judge-court-month level. Here we briefly describe the steps to obtain the sample used to estimate the two-way fixed effects model.

Despite the efforts by CNJ to assure quality of the performance data reported, there are clear instances of incorrect entries, such as hundreds of thousands of cases disposed by a single judge in a month. We therefore trim all performance measures at the 99th percentile.[24] We also observe a very high frequency of "mobility" in the raw data, as presented in Column (1) of Table 1: on average judges work in 11 different courts throughout the period. Yet, a large proportion of these judge-court matches is clearly transitory: for over half of the judge-court pairs the duration of the match is a single month.[25] In our baseline estimates, we drop any judge-court *spells* with a duration of less than three months. Our final baseline sample includes approximately 730,000 observations[26].

Table 1, Column (1) presents descriptive statistics for the full panel, while Column (2) refers to the sample used to estimate the two-way fixed-effects model[27]. There are 10,479 different judges and 9,048 courts in the estimating sample. Unlike other settings where there is limited mobility explored to estimate two-way fixed-effects models, that is clearly not a problem in our context: almost 80% of judges work in at least two different courts throughout the period, and only in about 10% of courts we observe a single judge in the entire period[28].

[24]For case disposition, the 99th percentile is 350 cases disposed by a judge in a single month.

[25]Informal conversations with judges suggest that it is common for judges work in different courts when colleagues are on vacation or sick leave.

[26]In Appendix A we present results using alternative sample definitions.

[27]Detailed descriptive statistics for the baseline sample are presented in Table A2.

[28]Using matched employer-employee data from Italy, Kline et al. (2020) report that in their largest connected

The first panel of Table 1 characterizes judges in the sample. While the panel covers a 76-month period, the median judge is observed working on any court in 56 months. Very few judges work in one single court throughout these five years: on average judges work in four different courts. While judges might work in more than one court on a given month, that is the exception rather than the rule: for over half of judge-month observations, magistrates are working in a single court. Once we drop short-lived judge-court matches, the average number of months for any match is over 16 months and the median 9 months, meaning that we have several repeated observations of output for each pair, reducing the noise inherent in a measure like the number of cases disposed.

Details about courts are presented in panel B of Table 1. While in any given month most courts are likely to be staffed by a single judge, their rotation means that, throughout the period, the average number of different judges working in a court is almost five, or one per year. We also present the breakdown of courts by category, according to the type of cases they hear. General courts, located in first level districts and handling all types of cases, comprise around 20% of the sample. The remaining courts are specialized on specific cases, such as Civil (22%), Criminal (16%), Small-stakes (18%) and Family Law (10%). As showed in Figure A1, courts dealing with different topics present systematic differences in the number of cases disposed on a monthly basis. This highlights why simple comparisons of performance between judges working in different courts might be misleading, and the need to condition on court fixed-effects when estimating judge-level performance.

Descriptive statistics on judicial performance are presented in Panel C of Table 1. The average number of case disposed on the merit per month is 40, but the distribution has a long right tail (maximum number is 350) and a non-negligible number of zeros: in 13% of judge-court-month observations the number of cases disposed was zero[29]. As discussed below, this motivates our main specification using the inverse hyperbolic sine of cases disposed as the main explanatory variable. The Table also shows that the average number of hearings is 35

set 21% of workers are movers.

[29]We interpret the number zero as an absence of cases solved by the judge in that court in a specific month, which is not not necessarily an indicator of lack of activity since judges take other decisions other than disposing of cases.

(median = 17).

The assessment of the predictive power of admission exams about judge performance relies on a smaller subsample of individuals matched between the two datasets. We present descriptive statistics for that matched sample in Column (3) of Table 1. We are able to match 2,881 judges in the productivity sample to their admission exam ranking, or 28% of judges observed in the estimation sample. Judges in the matched sample are observed for less months (45 vs. 50 months in the non-matched sample), work in more courts (5.9 vs. 4.3) and have slightly lower monthly output of cases disposed on the merit (36 vs. 40). It is important to note that candidates in the matched sample are not a random sample of the universe of judges[30]. In particular, Figure A2 highlights the difference in the share of judges we are able to match to recruitment exams by state. This should be taken into account when considering the external validity of our findings to the entire career of judges.

4 Empirical strategy and identification

4.1 Empirical Model

In order to estimate the permanent component of performance for judges, our main challenge is to separate the individual contribution of judges from the effects of courts they work in: courts in larger districts might have inherently more demand, or even within districts there might be systematic differences in length of cases between courts, so we cannot simply compare the performance of judges working in different courts. In order to do that, we borrow from the labor literature and estimate a two-way fixed effects model.

We model the number of cases disposed as follows. For a given judge j working in court c in month-year m, we model the inverse hyperbolic sine of the number of cases disposed as:

$$\operatorname{arsinh}(y)_{jcm} = \theta_j + \gamma_c + \alpha_{s(jc)} + \boldsymbol{X}'_{jcm}\boldsymbol{\beta} + \epsilon_{jcm} \tag{1}$$

[30] As we collected recruitment data from 2000 to 2013, our sample is composed of relatively "young judges": in 2013, judges had been on the bench for five years on average.

where θ_j refers to the permanent component of judge effect; γ_c refers to the permanent component of court effect; and X_{jcm} is a vector of time-varying controls. In our baseline specification X_{jcm} includes month-year indicators, the number of courts a Judge works on in a single month and the number of judges working in each single court.[31] Note that we also include an intercept for each connected set, α_s. As previously mentioned, the number of cases disposed is zero in approximately 13% of observations in our dataset. To deal with this, we use the inverse hyperbolic sine transformation (Bellemare & Wichman, 2020) of the number of cases disposed, which, unlike the log transformation, does not drop observations with zero cases disposed.

The separate identification of judge and court fixed-effects in the model above, as shown by Abowd, Creecy, & Kramarz (2002) in the context of workers and firms, is only possible within connected sets – groups of individuals and organizations connected by movers, individuals who work in different organizations throughout the period. Formally, within each connected set g with C_g organizations and J_g individuals, we can identify at most $C_g + J_g - 2$ effects.

The vast majority of judges work in several courts during the period, and even in more than one court in the same month, meaning that connected sets *within states* are very large: in the majority of states the largest connected set comprises over 95% of judge-court-month observations, and in only one state it comprises less than 90%.[32] Within each state, we lose very few observations by restricting our sample to the largest connected sets, providing us with 27 connected sets in our estimating sample.

As previously discussed in Section 2.2, however, judges are selected to work in a specific state, and never work in courts of different states. That means each state is a separate connected set, and we cannot compare court or judge fixed effects across states. While that is not an impediment to our analysis of the predictive power of admission exams, since we only compare individuals in the same exam cohort (and therefore same connected set), adjustments

[31] Both the number of judges working in a court and the number of courts a judge works on are computed in the full sample, and not in the estimating sample. While we do not use the variation coming from short judge-court matches, our estimates take into account that, for any given month, judges might be "moonlighting" in other courts and thus have lower performance.

[32] In the small state of Sergipe (SE), the largest connected set comprises only 65% of observations.

are needed in order to perform the variance decomposition exercise.

We follow Best, Hjort, & Szakonyi (2019) in estimating the variance components with several connected sets. When estimating equation (1), we impose the additional restrictions that both court and judge fixed-effects have mean zero in each connected set. If we define $\tilde{\theta}_j$ and $\tilde{\gamma}_c$ to be the true judge and court fixed-effects, respectively, what we can identify in equation (1) are $\theta_j = \tilde{\theta}_j - \overline{\theta}_g$ and $\gamma_c = \tilde{\gamma}_c - \overline{\gamma}_g$, the deviations of the true effects from the connected set means. We can then write the variance of number of cases disposed as:

$$\begin{aligned} Var(arsinh(y)_{jcm}) =& Var(\theta_j) + Var(\gamma_c) + 2Cov(\theta_j, \gamma_c) + Var(\alpha_s) + \\ & Var(\boldsymbol{X}'_{jcm}\boldsymbol{\beta}) + 2Cov(\alpha_s, \boldsymbol{X}'_{jcm}\boldsymbol{\beta}) + \\ & 2Cov(\theta_j + \gamma_c, \alpha_s + \boldsymbol{X}'_{jcm}\boldsymbol{\beta}) + Var(\epsilon_{jcm}) \end{aligned} \tag{2}$$

Best, Hjort, & Szakonyi (2019) show that, since we can only estimate within connected sets variances, the estimates recovered are lower bounds of the total variance of both judges and courts fixed-effects. The total variance attributable jointly to judges and courts, nonetheless, can be recovered using the variance of the connected sets effects: $Var(\tilde{\theta}_j + \tilde{\gamma}_c) = Var(\theta_j + \gamma_c) + Var(\alpha_s)$.

4.2 Identification and estimation

As discussed in detail in Card, Heining, & Kline (2013), Card, Cardoso, & Kline (2016) and Card, Cardoso, Heining, & Kline (2017), identification in the two-way fixed-effects model does not require random allocation of workers (judges) across firms (courts). The structure of the model allows for rich patterns of sorting, including for judges that dispose of more cases to select into better courts, or for judges to specialize in certain courts where their output is higher. In other words, our identification assumption of exogenous mobility is that judges do not sort on the error term in Equation (1).

Here we focus on assessing whether two particular issues affect the identification of our model. First, we model judge and court fixed-effects as additive and linearly separable. If that is not the case and there exists a judge-court match effect (i.e. more productive judges are particularly efficient in productive courts), then our estimates of judge effect might be

biased. Figure 2, panel A, presents a heatmap where we break down residuals of our model by vingtiles of judge and court fixed effects, and graph the average residuals in each cell. To interpret these results, consider Figure 2, panel B, where we simulate a model in which there exists judge-court match effects, but we erroneously estimate a linearly separable mode. The residuals then are systematically large/small in cells with extreme fixed-effects, reflecting the incapacity of the model to capture the matching effect. Going back to panel A, the actual heatmap, we do not observe the same pronounced pattern as in the simulation, suggesting that even if match effects are real (our model seems to be unable to match the outcomes at the very top cell in terms of both judge and court fixed-effects), they are not large enough to severely affect our estimates.

The second issue we consider is whether judges are moving into courts systematically due to *trends in court productivity*. While the selection of judges into courts due to levels of productivity does not affect our estimates, the same is not true if judges can select into courts because they are improving/decreasing their performance. To consider whether that seems to be the case, we perform an event study that assesses how the number of cases disposed by judges evolves around the time judges make clear transitions between judicial districts (i.e. judges working in a given court for at least three months prior to transition and at least three months after).[33] Figure 3 reports the coefficients of the event-study, in which we consider the indicator for 6 months before the transition as the omitted category. Three things stand out from these results. First, productivity starts falling in the last two months before a judge moves: knowing they will change courts, they might put in less effort to dispose of more cases or transfer their cases to other magistrates. Second, the fall in performance persists for at least three to fourth months after the transition, but six months after there is no distinguishable effect on performance. Finally, and most important for the model, there seems to be no selection in trends: judges do not seem to be on a trend to be more or less productive, either

[33]It is much harder to create such event-study when judges start working in different courts in the same district, because they often do not clearly leave one court for another, but keep a "connection" to their old appointment. For that reason we restrict our analysis to clear changes of court when judges move from one district to another.

before or after the movement between judicial districts. These results suggest that selection on trends do not seem to be a threat to identification in this context.[34]

Consistent estimation of individual fixed-effects requires not only that the number of observations in a panel is large enough, but also that the number of periods in the panel grows to infinity. Since our dataset encompass around 70 months, finite sample bias will lead to excess dispersion in our estimates of both judge and court fixed-effects, inflating the estimated share of total variance explained (Best, Hjort, & Szakonyi, 2019; Silver, 2021). We deal with that issue by using a non-parametric, split-sample correction method that shrinks our variance estimates (Finkelstein, Gentzkow, & Williams, 2016)[35].

We randomly split our sample in two, stratifying at the judge-court level, so that we preserve the number of judge-court pairs in both samples. We then proceed to estimate the two-way fixed effect model separately in each sample and obtain separate judge and court fixed-effect estimates. While FEs are noisily estimated in each sample, the errors should be uncorrelated due to the random split. Formally, if in each sample $s = \{1, 2\}$ the estimated judge fixed effect can be written as $\hat{\theta}_{(j,s)} = \theta_j + e_{j,s}$, where θ_j is the true FE for individual j and $e_{j,s}$ the error term, with $Cov(e_{(j,1)}, e_{(j,2)}) = 0$, then it holds that $Cov(\hat{\theta}_{(j,1)}, \hat{\theta}_{(j,2)}) = Cov(\theta_j, \theta_j) = Var(\theta_j)$. That is, we can recover the true variance of FEs by separately estimating variances in the random samples and calculating their covariance.

[34]The Figure also shows confidence intervals growing in width after the transition. This happens because we only require judges to reappear in the sample post-transition three times.

[35]Kline, Saggio, & Sølvsten (2020) propose a leave-one-out estimator for the variance of fixed-effects in similar models and show their estimates differ substantially from "naive" estimators that do not take into account limited mobility bias. Their estimator was developed for a single connected set, nonetheless, while in our application we estimate variances from several connected sets.

5 Results

5.1 Judges' role in explaining variation in output

Before presenting the results decomposing the variance of total output, we present preliminary evidence that judge fixed-effects matter in explaining courts' output. Table 2, Columns (1) and (2), present goodness-of-fit measures when estimating Equation (1) excluding and including judge fixed-effects, respectively. The inclusion of judge fixed-effects increases the adjusted R-squared of the model by 8 p.p. and reduces the residual standard error (RSE) from 1.43 to 1.34. This is evidence that judges matter in explaining the variation in output observed across courts.

We present the results of formal variance decomposition in Table 3.[36] Column (1) presents the raw variance estimates, with no finite-sample corrections, while Column (2) presents corrected variance estimates using split-sample strategy, and Column (3) presents the share of total variance explained by each component using the split-sample estimates. The finite-sample corrected variance of judges' FE is very similar to the raw estimates, on the range of 0.74-0.80, suggesting that judges explain at least 23% of the total variance of output. To put that magnitude in context, it is significantly larger than the estimate of Fenizia (2022) on the share of social security offices' productivity in Italy explained by managers (9%), but very similar to those of Best, Hjort, & Szakonyi (2019) on the share of public procurement prices explained by procurement officers in Russia[37]. The estimates for share of total variance explained by courts fixed effects are close to 35%. Estimates for the variance explained by the sum of judge and court FEs range from 30 - 40%: since the sum of explained variance independently explained by judge and courts is much larger than that, it means the covariance of these fixed effects is large and negative, meaning that judges with higher FE are observed matched with courts of

[36] Due to the high dimensionality of fixed-effects, we cannot simply invert matrices to obtain OLS estimates. We then estimate the parameters using the *-lfe-* command in R, also used by Best, Hjort, & Szakonyi (2019).

[37] In Table A3, we present similar variance decompositions using alternative sample restrictions. The lower bound of total variance explained by judges ranges from 12% using the entire sample (including very short matches) to 29% using a minimum of 4-month spells.

low FE, and vice-versa.

While the previous estimates show that judges are important in explaining the quantity of cases disposed and provided individual measures of judge performance, one might worry that judges that dispose of more cases are prioritizing quantity over quality. If that is the case, judges with higher fixed-effects in our model might actually be those that cut back on the inputs necessary to arrive at "good decisions", hastening the process to increase their case disposition number. We test whether this is a plausible explanation in our context by investigating one important input for case decision: the number of hearings that judges hold each month. To assess if "high fixed-effect" judges are conducting systematically less hearings than their peers with lower fixed-effects, we follow Silver (2021) and re-estimate the two-way fixed-effects model using the number of hearings as dependent variable, thus obtaining a new fixed-effect estimate for each judge. If judges are severely cutting back on hearings in order to increase their case disposition, we might expect a weak or even negative correlation between the fixed-effects in both models. Figure 4 shows that this is not the case: fixed-effects from the two models are strongly positive correlated, suggesting that judges who dispose of more cases are also those that hold more hearings. While we are not able to assess whether the use of other inputs, including length or quality of hearings, this alleviates concerns that judges who dispose of more cases are systematically sacrificing on quality.

5.2 Entrance exams are predictive of performance

Results in the previous sections are strong evidence that the identity of judges matters for the timely delivery of justice. While we are unable to explain the reasons why some judges are more effective in disposing of cases than others, the fact that we observe such differences in judge output suggests that the screening of judges might be one tool in improving judicial efficiency. We now turn to the question of how candidates' performance in the admission exams is related to their performance on the job. In all the exercises that follow we use the sample for which we can match judges' admission exam performance.

We start by presenting "reduced-form" evidence that entrance exam ranks are correlated with the number of cases disposed on merits, once we control for court and month fixed-

effects. That is, in here we do not use estimated judges fixed-effects, but simply present OLS regressions of the form

$$\operatorname{arsinh}(y)_{jcm} = \boldsymbol{\beta}' \boldsymbol{ExamRankQuintile}_j + \gamma_c + \delta_{w(j)} + \boldsymbol{X}'_{\boldsymbol{jcm}}\boldsymbol{\theta} + \epsilon_{jcm} \qquad (3)$$

where $\operatorname{arsinh}(y)_{jcm}$ is the inverse hyperbolic sine (IHS) transformation of cases disposed, $\mathrm{ExamRankQuintile}_j$ are indicators for quintile of exam performance of judge j in their exam cohort and δ_w are indicators for each cohort of candidates, since we can only meaningfully compare ranking among candidates sitting the same examination[38]. Standard-errors are clustered at the judge-level.

Results are presented in Table 4, where the omitted category for exam quintile is the bottom 20%. Column (1) presents estimates for a regression that only includes cohort fixed-effects, while in Columns (2) and (3) we add Court and Month fixed-effects, respectively. Focusing on Column (3), the results suggest that, when compared to judges ranking in the bottom quintile of their cohorts, those in the top 20% dispose of approximately 21% more cases. The estimated effect is smaller but statistically significant and economically meaningful for judges with ranks in the second to fourth quintiles, and we can reject that the coefficient for the top 20% is identical to those on the second and third quintiles. In Column (4) we present a much more stringent exercise: we include court-by-month fixed effects, meaning that the only variation used comes from different judges working in the same court in the same month (hence the large drop in sample size, since observations for courts with a single judge in any given month are dropped). The estimated coefficients are slightly larger in absolute value, but broadly consistent with previous estimates suggesting that better ranking in entrance exams is correlated with higher case disposition on the job.

We now present results using the estimated judges' fixed-effects obtained in the previous section. Figure 5 presents preliminary evidence of the correlation between (residualized) ranks in admission exams and standardized FE.[39] The strong positive correlation between per-

[38] All our analyses consider performance conditional on being selected for the job, so rankings are computed within judges in each cohort and not across all applicants for the job.

[39] Since we only compare judges entering in the same cohort, we first regress each rank on cohort indicators

formance measures suggests that judges who perform well in the admission exams are also among the ones with highest FE in their cohorts.

In Table 5 we present this same evidence in regression form. We estimate simple OLS regressions at the judge-level, using measures of on-the-job performance (standardized fixed-effect) as dependent variables and quintiles of performance in the recruitment exam as the main explanatory variable. Column (1) presents results from an OLS regression including cohort fixed-effects. Consistent with the findings in the reduced form regression, our results suggest that being ranked in the top 20% in the admission exam is correlated with a 0.2 s.d. increase in judge's performance (estimated fixed-effects) in comparison to those in the bottom quintile. Those ranking in lower quintiles are also estimated to perform 0.1-0.15 s.d. higher when compared to those at the very bottom. In Column (2) we replace the quintile ranking in the admission exam with the standardized final grade used to construct ranking.[40] The coefficient on grade is significant and indicates that an increase of 1 standard deviation in final grade is correlated with a 0.07 s.d. increase in performance (measured by judges' fixed-effects). Taken together with the results from the reduced form model, this suggests that, among the candidates selected in the admissions exam, those that rank higher do perform better on the job than those ranking lower.

5.3 Results are robust to alternative specifications

We conduct several exercises to assess the robustness of our results. First, Table 6 presents regressions in which we drop top and bottom performers in each cohort, evaluating whether results are fully driven by the very best (or very worst) candidates. Column (1) reproduces our main specification, while the remaining Columns restrict the sample by dropping only the top 3 performers in each cohort (Column 2); the top 5% candidates in each cohort (Column 3); the bottom 5% candidates in each cohort (Column 4); and both the top and bottom 5% candidates in each cohort (Column 5). Estimates of the correlation between exam rank and FE are very stable, and we cannot reject that they are statistically indistinguishable from our

and use residuals to construct the binned scatter plot.

[40] We could not collect final grades for some of the cohorts, hence the smaller sample size in Column (2).

main specification.[41]

We also provide a series of robustness tests by considering different restrictions to the sample used to estimate judges' fixed-effects. While our baseline specification drops judge-court spells (i.e. consecutive match periods) with less than three months, on Table 7 we show that our results are similar if we make no sample restrictions and if we make several different restrictions, including dropping the first and last months we observe a judge-court pair (considering the large drop in case disposition we document on Figure 3).

We also perform a randomization inference exercise to assess the robustness of our findings of the positive correlation between admission exam and job performance ranks. Within each cohort of judges, we randomly assign exam rankings, re-compute quintiles and then estimate the baseline model presented in Column (1) of Table 5. Figure A3 presents the histogram of these 1,000 simulated beta-coefficients for the top 20% performance indicator, and the solid line marks the true coefficient of 0.222. 95% of estimated coefficients are on the interval [-.118, 0.118], and none of the estimates is larger in magnitude than the true estimate. These results suggest that it is very unlikely that we would obtain a coefficient of this magnitude purely by chance.

While our main database does not allow us to observe the composition of cases disposed of, we use an alternative database at the case-level to show that, in the largest state in Brazil (São Paulo), the ranking in admission exams does not predict case composition and, furthermore, our key results on the correlation between ranking and on-the-job performance still hold. We provide details about this alternative database in Appendix C, and discuss the key findings here. We first group cases disposed of in large categories and show in Table A10 that judge admission rank is not correlated with the composition of cases they dispose of – as we should expect given that cases are randomly allocated across judges working in the same jurisdictions. We then proceed to estimate our two-way fixed-effects model using the

[41] We also show that results are very similar if we use the ranking of judges' FE instead of the FEs in Table A4. Additionally, we investigate whether our results are driven by less experienced judges. As presented in Column (2) of Appendix Table A5, we find no evidence of a heterogeneous effect of rank on performance according to judges' experience in the judicial system.

duration of cases as outcome variable and controlling for case type. On Table A11, we show that our main findings on the correlation between admission exam and judge FE also hold in this restricted sample for the state of São Paulo.

5.4 Why are entrance exams predictive of performance?

While we believe documenting that the overall ranking is informative about job performance is an important result, it does not shed light on exactly which dimension of the screening process is leading to this positive correlation. In this section we investigate to what extent this correlation is explained by exams *selecting* for higher-productivity judges and/or by other underlying causes, such as promotion incentives and experience.

First, we study what types of knowledge exams are selecting for. It is possible that the Titles Exam, which takes into account previous work and academic accomplishments, is the most predictive component of the overall performance. Or that the Oral Exam, in which there exists some degree of discretion by the selection committee, would be more informative. We attempt to provide evidence on that question by re-estimating the previous equation using grades in each exam phase as dependent variables and assessing which of those are more predictive about job performance. As previously discussed, we restrict our analysis to 20 admission cohorts and 619 judges for whom we observe six separate grades: Objective Exam, Written Exam, Civil and Criminal case decisions, Oral Exams and "Titles" Exam.

Results are reported in Columns (3) and (4) of Table 5. We first report in Column (3) the results of estimating the equivalent equation of Column (2) in the subsample for which we have detailed grade information. The result is very similar to that obtained in the full sample, suggesting that candidates with higher final grades also perform better on the job. We then estimate the model including standardized grades in each of the exams separately, and report results in Column (4). The only coefficient that is significant, and also the largest in magnitude, is that of grades on the Judicial Decision Writing on civil cases (the coefficient on criminal cases is less than half the size in magnitude and not statistically different from zero). This result is robust to other specifications of the estimation equation (Table A4), suggesting that the civil case admission is indeed the most predictive component of the admission exam.

Recall that, since the harmonization of admission exams in 2009, each of the Judicial Decision Writing exams has weight 15% for the final ranking, so grades in the civil case decision contribute less to the final selection than grades in the written exam (30% weight) or Oral exam (20%). Our results suggest, in contrast, that if the goal is to select candidates who will increase the speed of case disposition, exams should outweigh results in the civil case decision.

Second, we investigate whether entrance exam rankings are predictive of promotions along the judge's career. Ranking might impact careers directly as the best-ranked candidates have priority in choosing their placement and indirectly as they might affect judges' confidence, for example. We find weak evidence in favor of this channel. In Table A7 we document that ranking is not predictive of future promotion, except slightly for candidates in the top quintile. Judges in the highest grade quintile are 5 p.p. more likely to be promoted than judges in the bottom quintile, with a coefficient significant at the 10% level. This result is not surprising given the Brazilian judiciary institutional features around promotion. For instance, promotion vacancies open when a judge occupying a seat either retires or moves, but are filled following alternating criteria of merit or seniority. The definition of merit encompasses other outcomes besides the number of cases disposed of on merit. Moreover, judges must meet minimum experience requirements to be promoted.

Finally, we study whether court-specific experience could explain the reduced form results. It could be that higher-ranked judges also stay longer in courts, accumulate more experience, and as a consequence become more productive. We first show that exam ranking predicts court-specific experience. We collapse our dataset to the judge-court level and regress total time spent by the judge at that court on rankings, controls, and fixed effects. Table A8 reports that judges in high quintiles stay between 1.5 and 3 months longer in courts, an effect between 6 - 13% of the average time spent in courts. We also document that time spent in court indeed is correlated with higher disposal of cases. In Table A9, we show that, in a reduced form regression, being in a court for longer is correlated with higher output. That is true including when we exclude periods around the movement of judges, suggesting that this pattern is not only driven by the fall in productivity around movements, but is possibly a feature of judges "on-the-job" learning in an establishment.

6 Conclusion

This paper provides evidence that states can effectively design impersonal exams that are able to screen good candidates for top public service positions, even when recruitment practices are constrained by fears of political influence. We explore rich data on judges and courts in Brazil to show that judges are relevant in explaining the observed variation in output, and estimate judge-level measures of performance in case disposition – an important indicator in a judicial system with high levels of court congestion. We then link these measures to the performance of judges in admission exams, and show that within cohorts of hired judges those with higher grades also dispose of more cases. In particular, it seems that not all phases of the admission exams are equally likely to predict job performance: across different specifications, grades on the civil case exam is the only statistically significant predictor.

Our results have meaningful implications for policy makers. First, it adds to recent evidence that not only frontline providers matter for the delivery of public service: managers and other officials working across the state bureaucracy can have significant impact on service provision (Best, Hjort, & Szakonyi, 2019; Fenizia, 2022; Aman-Rana, 2022). Carefully designing systems that select and incentivize these individuals is therefore very important. Secondly, it is also relevant for the debate about rules and discretion in hiring (Hoffman, Kahn, & Li, 2018). We show that an admission process with little discretion by the selecting agency is able to rank individuals in a way that meaningfully predicts job performance. In particular, by breaking down exam performance into its components, we find evidence that an examination that approximates the kind of task faced by candidates on the job (the writing of sentences by judges) is especially predictive about their future performance.

Data limitations do not allow us to further explore three mechanisms we believe are relevant for future research. The first is what makes for an efficient judge. Judges do not work in isolation writing decisions, but, rather, manage complex organizations staffed by several workers and in close contact with other state actors (Pinheiro, 2003; Oliveira Gomes, 2014). A more efficient judge might be one that simply puts longer hours and more effort to increase case disposition, but might just as well be one that is able to put in place a well-oiled machine

where every staffer is pulling their weight and ensuring smooth handling of cases.[42] Management practices have shown to be very relevant in explaining productivity in both the private (Bloom & Van Reenen, 2007; Bloom, Eifert, Mahajan, McKenzie, & Roberts, 2013) and the public sector (Rasul & Rogger, 2018; Leaver, Lemos, & Dillenburg Scur, 2019; Bloom, Lemos, Sadun, & Reenen, 2015), so gaining better understanding of working practices in the judicial sector might shed light on the determinants of judge effectiveness.

Second, while we find a strong and robust positive correlation between grades in the admission test and performance, and consider this a relevant parameter for policy-makers designing screening processes, it is unclear exactly what is the force driving this correlation. One possibility is that exams are indeed effective in screening candidates with specific knowledge that is also useful for the tasks performed by a judge – the fact that grades in the civil case examination are the only ones with independent predictive power suggest this might be the case. Another possibility, however, is that competitiveness and difficulty of the exams screen candidates with high general ability and/or high motivation to be a judge, which implies that the congruence between test content and requirements of the job is less important. We think this is a relevant distinction, particularly in light of the theory and evidence that highlight the role of intrinsic motivation in driving workers' performance (Deserranno, 2019; Ashraf & Bandiera, 2018; Prendergast, 2008). A final possibility is that experience in court increases productivity, and that higher-ranked judges stay longer in courts because of features of the Brazilian judiciary. We investigated this channel to the extent our data allows, but leave it for future research to fully weigh all possible mechanisms.

Finally, given that we only observe judicial productivity for those who pass entrance examinations, we cannot make claims about the remaining pool of candidates. In particular, we cannot directly test if examinations are screening for the most productive candidates overall or not, or if exams should be made more selective or less. Future research with complete characteristics of candidates, their career paths and more examinations could evaluate the overall efficiency of the Brazilian selection mechanism into the public sector.

[42] Fenizia (2022) finds that the mechanism through which managers in social security offices are able to increase output per worker is by letting go of workers while maintaining total output stable.

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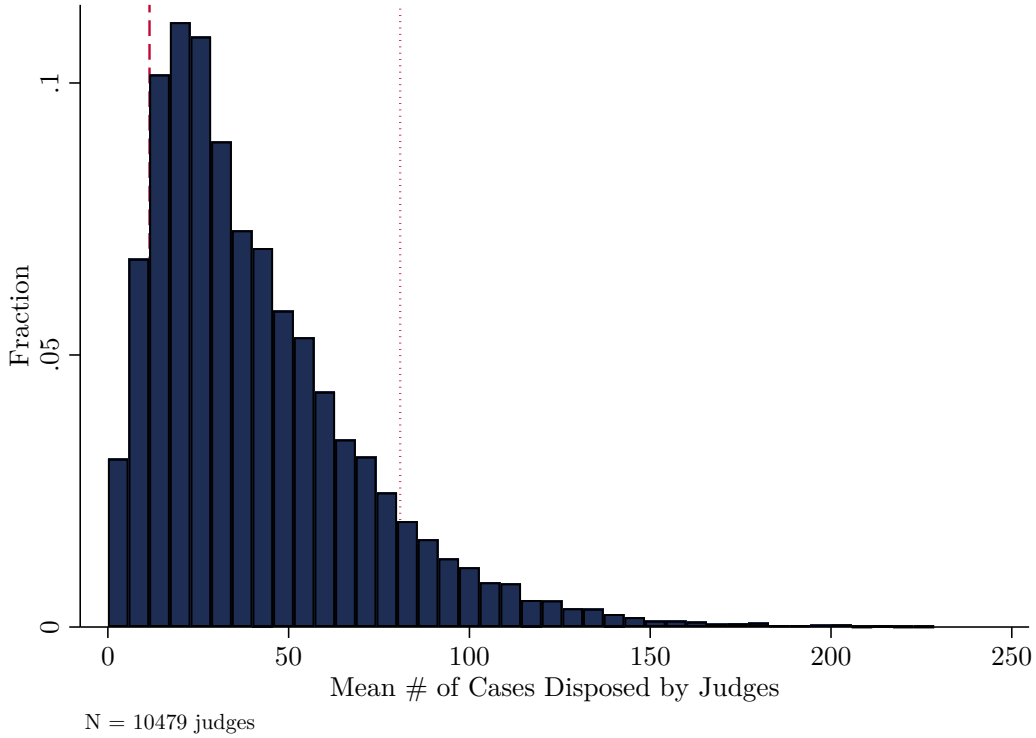
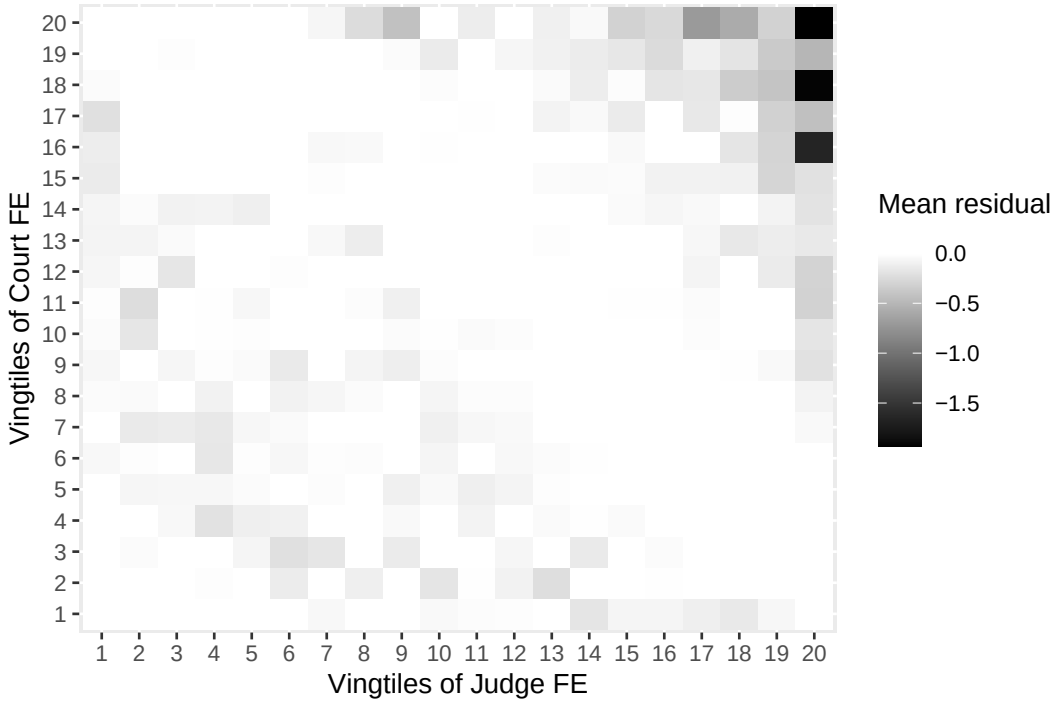
7 Figures and Tables

Figure 1: Histogram of mean number of cases disposed on the merits by judge.

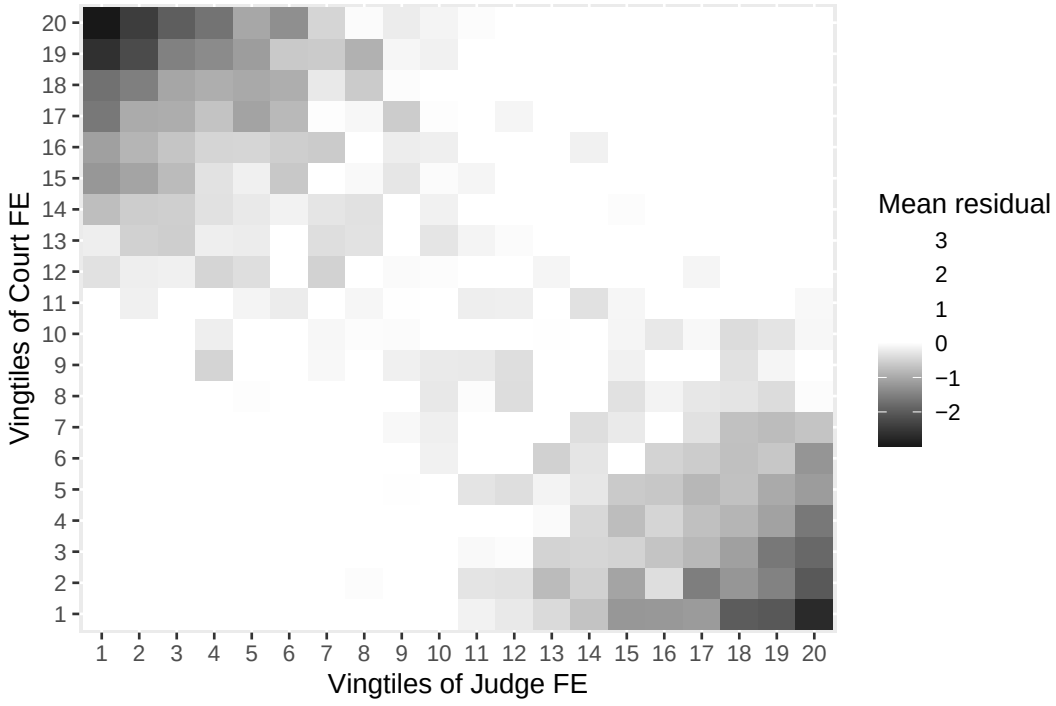


Note: The histogram presents the average monthly number of cases disposed by judges. Average number of cases is calculated in the sample used to estimate the two-way fixed-effects model, where outcome variables are trimmed at the top 1%, judge-court spells shorter than three months are dropped and only observations in the largest connected sets within each state are used. The dashed and dotted lines mark the 10th and 90th percentile of the distribution, respectively. The figure documents the vast dispersion in number of case dispositions across judges: those in the 90th percentile of the distribution dispose of eight times as many cases as those in the 10th percentile.

Figure 2: Residuals heatmap from two-way fixed-effects model



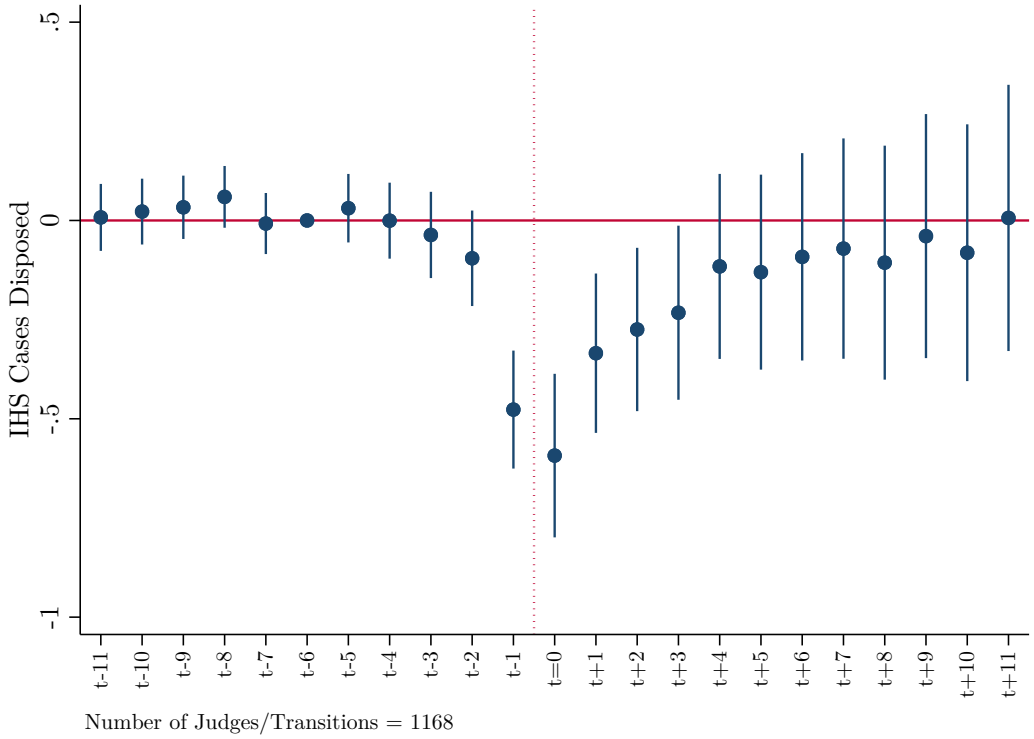
(a) Actual from data



(b) Simulated from misspecified model

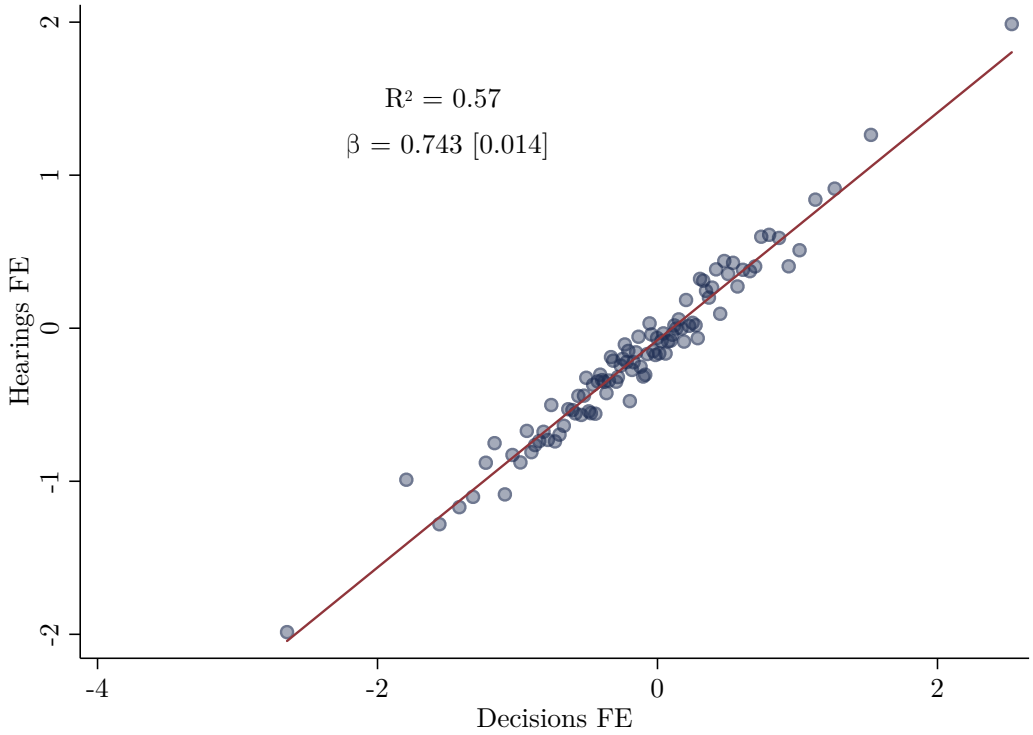
Note: These figures present heatmaps of average residuals from a two-way fixed-effects model. Panel A presents results from actual data estimated using equation (1). Panel B presents results from a simulated model with 10,000 worker-firm observations (200 workers in 50 firms) containing match effects between workers and firms, but estimated using a misspecified model in equation (1). Darker cells represent large negative residuals, while lighter cells represent large positive residuals. Judges and courts are binned into vingtiles of estimated fixed-effects.

Figure 3: Event-study around judicial district movement



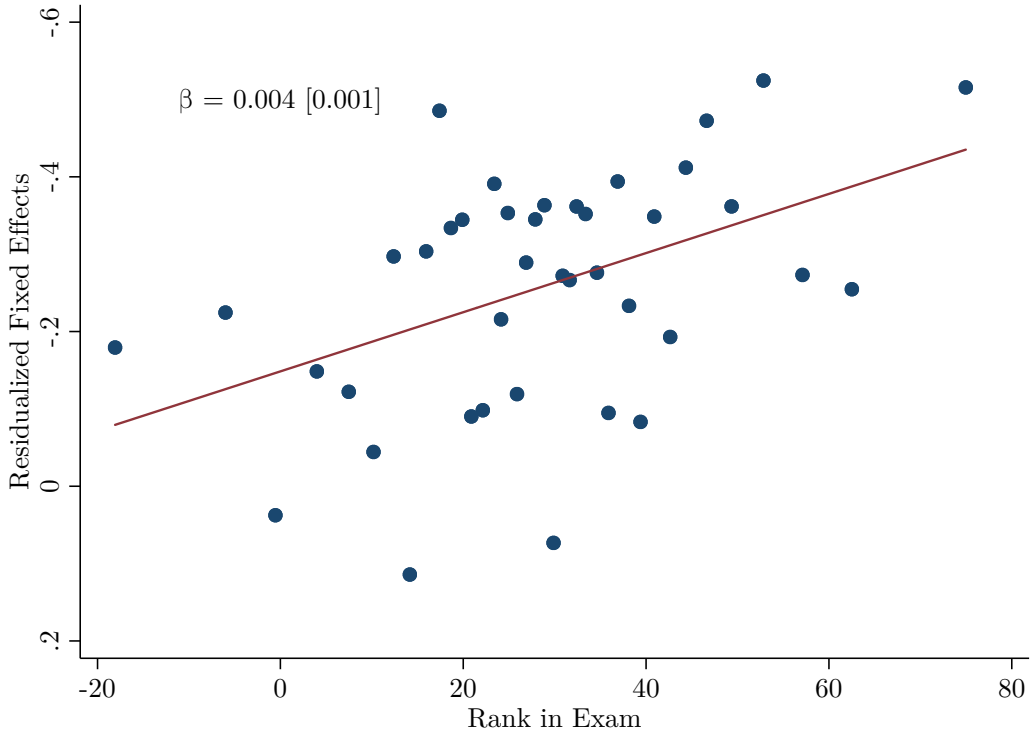
Note: This figure reports point estimates and 95% CI for coefficients on an event-study regression of the form $\text{arsinh}(y)_{jm} = \sum_t \beta_t \boldsymbol{RelativePeriod_t} + \gamma_j + \delta_m + \epsilon_{jm}$, where $\text{arsinh}(y)_{jm}$ is the IHS of cases disposed, γ_j and δ_m are judge and month fixed-effects, respectively, and β_t are the event-study coefficients. The omitted category is the indicator for six months before the movement. The sample is restricted only to clear moves across judicial districts, as detailed in Section 4.2. Standard errors are clustered at the transition-level.

Figure 4: Binscatter of hearings and case disposition fixed-effects



Note: This graph presents a binned scatter plot of residualized judge fixed-effects obtained by estimating equation (1) using hearings and case disposition separately. Residuals are obtained by regressing both FEs on connected set dummies. The R-squared and coefficients presented refer to a regression of hearing FE on case dispositions FE including connected set dummies.

Figure 5: Binscatter of residual ranks, conditioning on Concurso FE



Note: The graph presents a binned scatter plot of residualized rank in fixed-effects obtained by estimating Equation (1) and residualized ranks in admission exams, at the judge level. Residues are obtained by regressing each of the variables on *Concurso* fixed effects.

Table 1: Descriptive statistics

	Full Sample (1)	Estimation Sample (2)	Exam matched Sample (3)	Difference (2) - (3)
Panel A: Judges				
Share male judges	0.61	0.60	0.61	
Mean # courts by judge	10.52	4.28	5.87	***
Mean # months by judge	50.97	49.99	44.91	***
Mean # courts at judge-month level	1.70	1.39	1.55	***
Mean # judicial districts at judge-month level	3.72	2.28	3.29	***
Mean # months per judge-court pair	8.22	16.23	11.83	***
Panel B: Courts				
Mean # of judges by court	12.64	4.96	2.99	***
Mean # judges at court-month level	1.67	1.38	1.20	***
Share civil courts	0.22	0.22	0.23	***
Share general courts	0.20	0.20	0.24	***
Share small-stakes courts	0.18	0.18	0.16	
Share criminal courts	0.16	0.16	0.16	
Share family court	0.10	0.10	0.09	***
Share other courts	0.14	0.13	0.11	***
Panel C: Output measures				
Cases Disposed (on merit)	33.82	40.13	36.10	***
Total Hearings (presided or held)	29.32	34.88	35.85	***
Number of judges	11,462	10,479	2,881	
Number judges ever working in multiple courts	10,378	8,500	2,653	
Number of courts	9,540	9,048	5,667	
Number of courts with multiple judges	9,201	8,152	3,925	
Number of judge-court pairs	120,642	44,850	16,918	
Number of judge-court spells	273,074	77,799	24,089	
Number of connected sets	68	27	24	
Number of judge-court-month observations	991,324	727,784	200,212	

Note: This table reports descriptive statistics for key variables. Column (1) refers to the full original panel. Column (2) refers to the sample used to estimate the two-way fixed-effects model, where outcome variables are trimmed at the top 1%, judge-court spells shorter than three months are dropped and only observations in the largest connected sets within each state are used. Column (3) refers to the sample matched to admission exams, i.e., it only retains judge-court-month observations for which judges were matched to their admission exams ranking. This is the sample used in both the "reduced-form" exercises presented in Table 4 and the main results on the correlation between admission ranking and performance in Table 5. Significance results of a t-test comparing the means in Columns (2) and (3) are presented in the last Column (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$).

Table 2: Goodness of fit measures

	(1)	(2)	(3)
R-squared	0.379	0.464	0.619
Adjusted R-squared	0.371	0.45	0.593
Residual Standard Error (RSE)	1.434	1.341	1.153
Observations	727784	727784	727784
Judge FE	No	Yes	No
Judge-by-Court FE	No	No	Yes

Note: This table presents goodness-of-fit measures for several different models using the two-way fixed-effects estimation sample. Column (1) presents results from a model that does not include judge fixed-effects; Column (2) is our main specification from equation (1), including judge fixed-effects; while Column (3) includes judge-by-court fixed effects.

Table 3: Variance decomposition

	Raw Variance	Split Sample Variance	Split sample Var - % Total
Cases disposed (IHS)	3.27	3.27	1.00
Judge FE	0.80	0.74	0.23
Court FE	1.16	1.11	0.34
Connected Set FE	0.24	0.24	0.07
Judge+Court FE	1.23	1.00	0.31

Note: This table presents the variance decomposition exercise using estimates from the two-way fixed effects model in equation (1). Column (1) presents the variance estimates without adjustment, while Column (2) presents variance estimates corrected for finite-sample bias using the split-sample technique. Column (3) presents the finite-sample corrected variance estimates as a share of total variance.

Table 4: Reduced form regressions: output and admission exam performance

	(1)	(2)	(3)	(4)
Top quintile (β_1)	0.113**	0.214***	0.206***	0.340***
	(0.0545)	(0.0403)	(0.0402)	(0.0823)
4th quintile (β_2)	0.0920*	0.169***	0.164***	0.186**
	(0.0506)	(0.0383)	(0.0382)	(0.0801)
3rd quintile (β_3)	0.0854*	0.130***	0.125***	0.239***
	(0.0505)	(0.0374)	(0.0370)	(0.0803)
2nd quintile (β_4)	0.0918*	0.134***	0.133***	0.241***
	(0.0510)	(0.0377)	(0.0376)	(0.0791)
Observations	200,212	200,212	200,212	59,801
R-Squared	0.11	0.42	0.43	0.53
Concurso FE	Yes	Yes	Yes	Yes
Court FE	No	Yes	Yes	No
Month FE	No	No	Yes	No
Court-by-Month FE	No	No	No	Yes
$\beta_1 = \beta_2$	0.65	0.23	0.26	0.04
$\beta_1 = \beta_3$	0.55	0.02	0.02	0.16
$\beta_1 = \beta_4$	0.65	0.04	0.05	0.23

Note: This table reports results from estimating equation (3): $\mathtt{arsinh}(y)_{jcm} = \boldsymbol{\beta ExamRankQuintile_j} + \gamma_c + \delta_{w(j)} + \boldsymbol{X'_{jcm}\theta} + \epsilon_{jcm}$, where $\mathtt{arsinh}(y)_{jcm}$ is the inverse hyperbolic sine of cases disposed. All specifications include examination cohort (*Concurso*) fixed-effects. Columns (1) through (3) use the exam matched sample, observations used in the two-way fixed-effects models for which judge admission exams are available. Column (4) uses a subset of that sample that excludes all observations for which only one judge is working in any given court in a month. Standard-errors are clustered at the Judge level (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$).

Table 5: Main results – correlation between admission grades and performance

	(1)	(2)	(3)	(4)
Top quintile	0.222***			
	(0.0587)			
4th quintile	0.139**			
	(0.0597)			
3rd quintile	0.0979*			
	(0.0575)			
2nd quintile	0.151**			
	(0.0589)			
Final Grade (standardized)		0.0675***	0.0692*	
		(0.0224)	(0.0389)	
Objective Grade (standardized)				-0.0151
				(0.0417)
Written Exam Grade (standardized)				0.0141
				(0.0392)
Civil Essay Grade (standardized)				0.105**
				(0.0408)
Penal Essay Grade (standardized)				0.0362
				(0.0385)
Oral Grade (standardized)				0.0123
				(0.0422)
Titles Grade (standardized)				-0.0127
				(0.0429)
Observations	2880	2142	619	619
R-Squared	0.253	0.269	0.274	0.280
Number Admission Cohorts	78	65	20	20
Concurso Fixed-Effect	Yes	Yes	Yes	Yes

Note: This table reports results from estimating equations of the form: $\mathtt{JudgeFE}_j = \mathtt{ExamOutcome}_j'\beta + \delta_{w(j)} + \epsilon_j$, where $\delta_{w(j)}$ are admission cohorts (*Concurso*) fixed-effects and $\mathtt{ExamOutcome}_j$ are the the independent variables of interest in each model in Columns (1) though (4). The dependent variable is Judge FEs, standardized to have unitary standard deviation within exam cohorts. All grades are standardized to have unitary standard deviation within each admission cohort. Robust standard errors in parentheses (* p<0.1, ** p<0.05, *** p <0.01).

Table 6: Robustness – excluding top and bottom performers

	(1)	(2)	(3)	(4)	(5)
Top quintile	0.222***	0.282***	0.225***	0.238***	0.239***
	(0.0587)	(0.0679)	(0.0641)	(0.0668)	(0.0714)
4th quintile	0.139**	0.147**	0.139**	0.154**	0.152**
	(0.0597)	(0.0603)	(0.0599)	(0.0681)	(0.0682)
3rd quintile	0.0979*	0.0975*	0.0982*	0.114*	0.111*
	(0.0575)	(0.0576)	(0.0575)	(0.0658)	(0.0658)
2nd quintile	0.151**	0.149**	0.150**	0.165**	0.163**
	(0.0589)	(0.0588)	(0.0588)	(0.0670)	(0.0670)
Observations	2,880	2,646	2,733	2,655	2,508
R-Squared	0.253	0.255	0.255	0.251	0.253
Number Admission Cohorts	78	77	78	78	78
Drop Top 3	No	Yes	No	No	No
Drop Top 5%	No	No	Yes	No	Yes
Drop Bottom 5%	No	No	No	Yes	Yes

Note: This table reports results from estimating equations of the form: $\text{JudgeFE}_j = \boldsymbol{\beta ExamRankQuintile_j} + \delta_{w(j)} + \epsilon_j$, where $\delta_{w(j)}$ are admission cohorts (*Concurso*) fixed-effects. Column (1) reproduces the main result from the Table 5, while Columns (2) through (5) re-estimate the model in subsamples that exclude top and/or bottom contenders, as specified above. Robust standard errors in parentheses (* p<0.1, ** p<0.05, *** p <0.01).

Table 7: Robustness – alternative sample restrictions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Top quintile	0.222***	0.212***	0.182***	0.114*	0.171***	0.167***	0.0660
	(0.0587)	(0.0594)	(0.0591)	(0.0606)	(0.0585)	(0.0605)	(0.101)
4th quintile	0.139**	0.167***	0.105*	0.0778	0.134**	0.129**	0.0582
	(0.0597)	(0.0598)	(0.0597)	(0.0604)	(0.0591)	(0.0599)	(0.103)
3rd quintile	0.0979*	0.104*	0.0697	0.0482	0.0522	0.0873	0.152
	(0.0575)	(0.0579)	(0.0587)	(0.0594)	(0.0580)	(0.0588)	(0.101)
2nd quintile	0.151**	0.183***	0.122**	0.130**	0.116**	0.135**	-0.00374
	(0.0589)	(0.0603)	(0.0601)	(0.0607)	(0.0586)	(0.0598)	(0.0990)
Observations	2,880	2,880	2,782	2,726	2,880	2,783	1,006
R-Squared	0.253	0.381	0.228	0.282	0.284	0.252	0.168
Number Admission Cohorts	78	78	76	74	78	75	65
Concurso Fixed-Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample restriction	Baseline	Full sample	4-month spells	6-month total	Drop first 2 months	Drop first & last 2 months	Large courts

Note: This table reports results from estimating equations of the form: $\texttt{JudgeFE}_j = \boldsymbol{\beta ExamRankQuintile_j} + \delta_{w(j)} + \epsilon_j$, where $\delta_{w(j)}$ are admission cohorts (*Concurso*) fixed-effects. The dependent variable is Judge FEs, standardized to have unitary standard deviation within exam cohorts. Each Column refers to a different sample restriction used to estimate Judge FEs using the two-way fixed-effects model. Baseline refers to the sample used for our main results reported in Table 5, where we restrict the sample to judge-court spells (i.e. consecutive periods) of at least 3 months. In Column two we apply no restriction to the duration of judge-court matches, while in columm (3) we restrict to spells of at least 4 months and in Column (4) we only use judge-court pairs observer for at least six months (not necessarily consecutive). In Column (5) we drop the first two months of any judge-court match while in Column (6) we drop both the first and last two months. In Column (7) we keep only "large courts", defined as those court-month pairs when we see at least two judges working in the same court. Robust standard errors in parentheses (* p<0.1, ** p<0.05, *** p <0.01).

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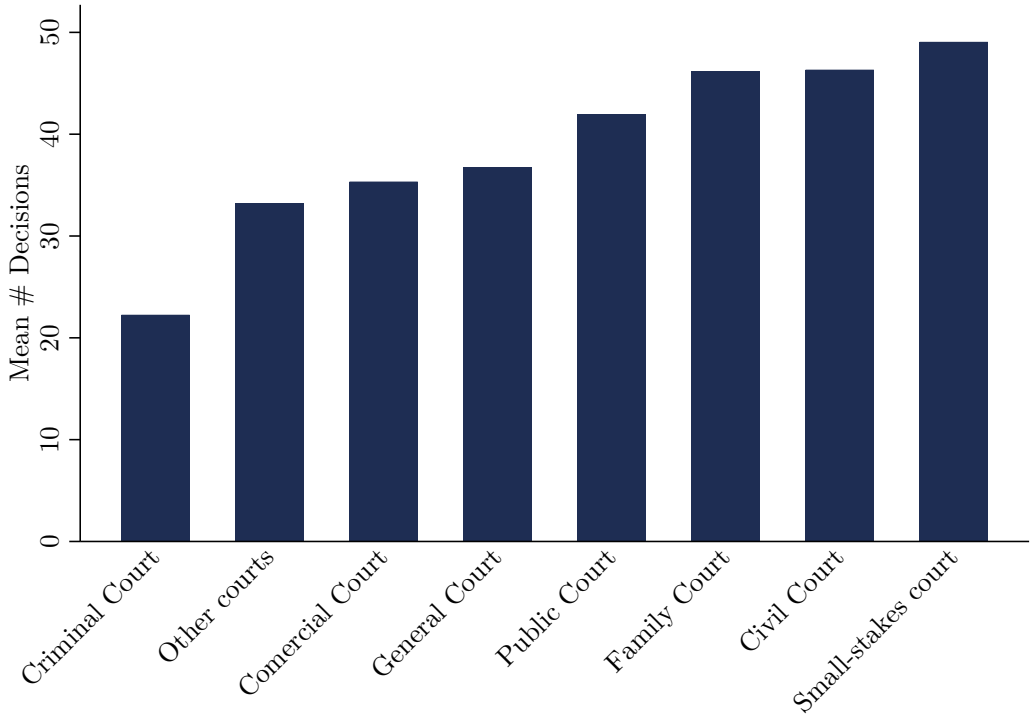
A Appendix figures and tables

Table A1: Nominal Monthly Wages in BRL for Judges and Other Occupation Categories

Year	Judge (1)	Public Federal (2)	Private (3)	Lawyer (4)	Attorney (5)	Defender (6)	Teacher (7)	Health (8)
2003	14022.05	2705.25	797.69	3739.90	13020.26	4861.35	832.61	1948.77
2004	14746.32	2927.70	865.33	3847.76	13302.02	5253.32	873.49	2139.42
2005	16626.96	3234.74	918.03	4026.98	14839.64	5733.74	957.81	2321.79
2006	20315.95	3753.44	975.94	4316.30	18774.73	7388.01	1055.26	2534.81
2007	20874.43	3767.66	1025.27	4389.33	20980.27	7985.20	1114.25	2742.50
2008	21866.22	4509.74	1104.17	4643.52	21682.48	9776.80	1229.63	2983.96
2009	22358.53	5135.16	1196.97	4896.28	21666.93	15589.55	1386.66	3276.08
2010	22820.27	4886.85	1268.28	4823.15	23035.83	16635.17	1478.91	2649.55
2011	22974.93	5946.65	1381.07	5192.09	23443.62	18492.54	1628.27	2901.54
2012	23218.68	6284.55	1517.16	5523.90	24208.90	18549.19	1918.28	3130.53
2013	24497.50	6237.62	1658.07	5890.48	25877.93	18942.78	2012.77	3347.08
2014	26504.97	6691.68	1776.56	6222.14	26462.71	20923.63	2288.11	3598.71
2015	30403.51	7422.67	1928.85	6723.52	30493.29	23818.92	2487.00	3898.43
2016	30767.15	7436.03	2098.78	7071.63	30415.53	24150.48	2702.13	4190.56
2017	30822.91	8357.96	2231.52	7346.37	30939.93	25297.52	2822.94	4385.94
2018	31508.46	8795.70	2273.77	7523.34	31352.18	26167.90	2938.03	4470.61
2019	35910.21	10034.71	2241.54	7479.57	36768.85	28696.86	3026.65	4396.10

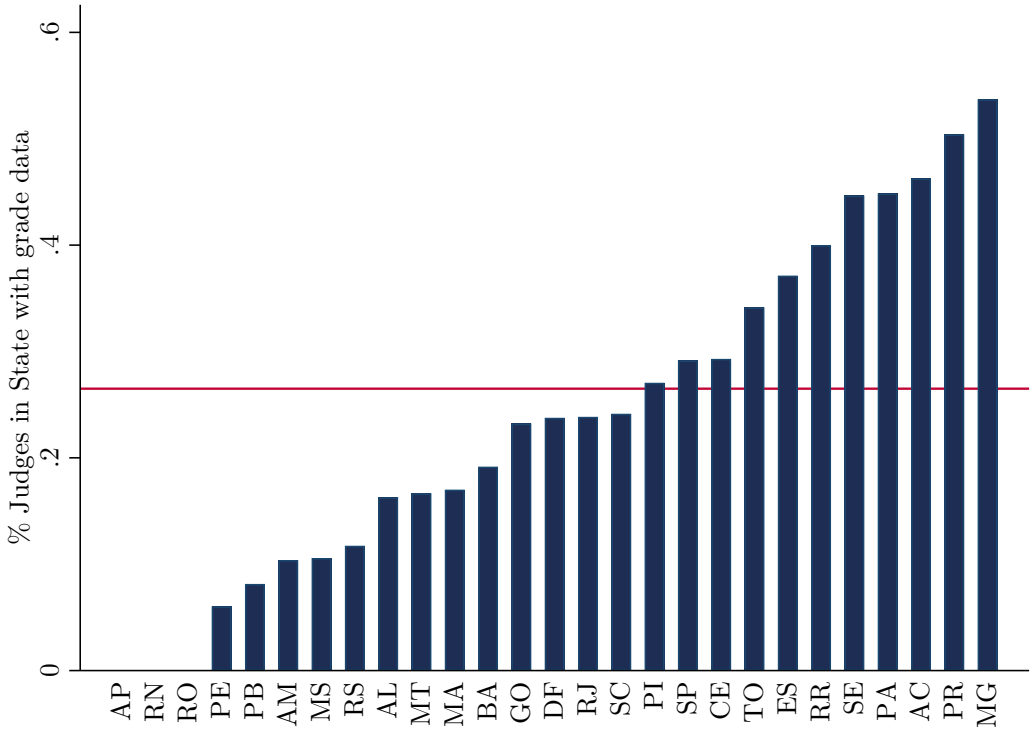
Note: This Table reports average nominal wages for judges and various other occupational categories for Brazil between 2003 and 2019 sourced from RAIS.

Figure A1: Average number of cases disposed, by type of court



Note: This figure presents the average monthly number of cases disposed by judges, in each type of court. Number of cases is calculated in the sample used to estimate the two-way fixed-effects model, where outcome variables are trimmed at the top 1%, judge-court spells shorter than three months are dropped and only observations in the largest connected sets within each state are used. The figure documents systematic differences in number of case disposition across courts: judges in criminal courts dispose of twenty cases, on average, every month, while judges in small-stakes courts dispose of almost 50 cases.

Figure A2: Share of judges matched by State



Note: This figure presents the share of judges in the estimation sample that are matched to their admission exam, by State. The red line marks the overall share of judges matched (28%).

Table A2: Detailed descriptive statistics in estimation sample

	Mean	SD	Median	N
Panel A - Judges				
Male	0.60	0.49	1	10,218
# Courts by Judge	4.28	3.56	3	10,479
Number of months Judge is observed	49.99	21.05	56	10,479
# of Courts at Judge-Month level	1.39	0.83	1	523,813
Number Municipalities Judge ever works in	2.28	1.69	2	10,479
Unique number of months per judge-court pair	16.23	17.15	9	44,850
Panel B - Courts				
# Judges by Court	4.96	3.60	4	9,048
# of Judges at Court-Month level	1.38	0.87	1	528,483
Civil Court	0.22	0.42	0	9,048
General Court	0.20	0.40	0	9,048
Small-stakes Court	0.18	0.39	0	9,048
Criminal Court	0.16	0.37	0	9,048
Family Court	0.10	0.30	0	9,048
Other Courts	0.13	0.34	0	9,048
Panel C - Output measures				
Cases Disposed (on merit)	40.13	50.09	22	727,784
Total Hearings (presided or held)	34.88	46.39	17	716,736

Note: This table reports descriptive statistics for key variables in the sample used to estimate the two-way fixed-effects model, where outcome variables are trimmed at the top 1%, judge-court spells shorter than three months are dropped and only observations in the largest connected sets within each state are used.

Table A3: Variance decomposition - alternative samples

	Baseline	Full sample	4-month spell	6-month total
Cases disposed (IHS)	3.27	3.65	3.13	3.54
Judge FE	0.23	0.12	0.29	0.23
Court FE	0.34	0.21	0.41	0.33
Connected Set FE	0.07	0.07	0.08	0.07
Judge+Court FE	0.31	0.20	0.38	0.30
Adju R-squared	0.45	0.41	0.46	0.43
Observations	727,835	988,160	650,998	795,669
Number Judges	10,479	11,273	10,000	10,092
Share movers	0.81	0.92	0.78	0.80

Note: This table presents the variance decomposition exercise using estimates from the two-way fixed effects model in equation (1). Column (1) presents our baseline sample restriction, while the following Columns consider alternative samples indicated in each Column. All variance estimates are obtained using the split-sample bias-correction method. Share of movers refers to the share of judges in each sample that were observed working in two or more courts throughout the period.

Table A4: Robustness – correlation between admission grades and performance

	(1)	(2)	(3)	(4)
Top quintile	-4.619***			
	(1.081)			
4th quintile	-2.439**			
	(1.089)			
3rd quintile	-1.117			
	(1.072)			
2nd quintile	-1.980*			
	(1.088)			
Final Grade (standardized)		-1.451***	-1.783***	
		(0.350)	(0.629)	
Objective Grade (standardized)				-0.366
				(0.678)
Written Exam Grade (standardized)				-0.568
				(0.625)
Civil Essay Grade (standardized)				-1.311**
				(0.650)
Penal Essay Grade (standardized)				-0.845
				(0.656)
Oral Grade (standardized)				-0.626
				(0.667)
Titles Grade (standardized)				-0.227
				(0.756)
Observations	2881	2143	620	620
R-Squared	0.426	0.393	0.421	0.423
Number Admission Cohorts	79	66	21	21
Concurso Fixed-Effect	Yes	Yes	Yes	Yes

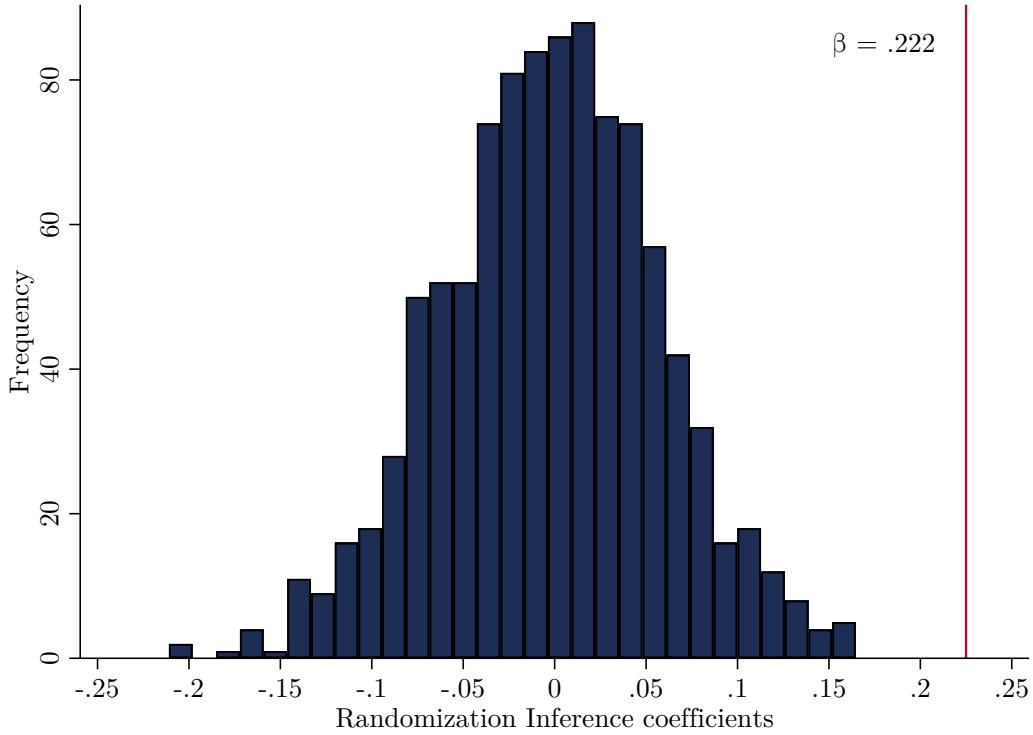
Note: This table reports results from estimating equations of the form: $\texttt{RankFE}_j = \texttt{ExamOutcome}_j'\beta + \delta_{w(j)} + \epsilon_j$, where $\delta_{w(j)}$ are admission cohorts (*Concurso*) fixed-effects and $\texttt{ExamOutcome}_j$ are the the independent variables of interest in each model in Columns (1) though (4). All grades are standardized to have unitary standard deviation within each admission cohort. Robust standard errors in parentheses (* p<0.1, ** p<0.05, *** p <0.01)

Table A5: Heterogeneity: entrance exam impact by experience as judge

	(1)	(2)
Top quintile	0.222***	0.306***
	(0.0587)	(0.0847)
4th quintile	0.139**	0.197**
	(0.0597)	(0.0879)
3rd quintile	0.0979*	0.112
	(0.0575)	(0.0842)
2nd quintile	0.151**	0.146*
	(0.0589)	(0.0851)
Above median experience		0.852***
		(0.293)
Top quintile*Above median experience		-0.158
		(0.118)
4th quintile*Above median experience		-0.110
		(0.120)
3rd quintile*Above median experience		-0.0266
		(0.115)
2nd quintile*Above median experience		0.00812
		(0.118)
Observations	2880	2880
R-Squared	0.253	0.254
Number Admission Cohorts	78	78
Concurso Fixed-Effect	Yes	Yes

Note: Column (1) reports results from estimating equations of the form: $\mathtt{JudgeFE}_j = \boldsymbol{\beta ExamRankQuintile}_j + \delta_{w(j)} + \epsilon_j$, where $\delta_{w(j)}$ are admission cohorts (*Concurso*) fixed-effects. Column (1) reproduces the main result from the Table 5. Column (2) reports results from estimating equations of the form: $\mathtt{JudgeFE}_j = \boldsymbol{\beta ExamRankQuintile}_j + \boldsymbol{\zeta AboveMedian}_j + \boldsymbol{\eta ExamRankQuintile}_j * \boldsymbol{AboveMedian}_j + \delta_{w(j)} + \epsilon_j$, where $\mathtt{AboveMedian}_j$ is a dummy that equals one if judge's experience is equal to or greater than the median experience and zero otherwise. Robust standard errors in parentheses (* p<0.1, ** p<0.05, *** p <0.01)

Figure A3: Histogram of simulated beta-coefficients



Note: The figure presents the histogram of 1,000 simulated coefficients for the top 20% indicator using our main specification, equivalent to the results presented in Column (1) of Table 5, where we randomly assign final admissions ranking within each cohort. The true coefficient is marked by the solid red line.

Table A6: Pairwise correlations between performance in admission exam phases

	.					
	Objective	Written	Civil case	Criminal case	Oral	Titles
Objective	1					
Written	0.0283	1				
Civil case	0.0127	0.0144	1			
Criminal case	0.0276	0.0886**	0.0604	1		
Oral	0.0352	0.103**	0.112***	0.0656	1	
Titles	-0.0403	0.0825**	0.122***	0.0265	0.154***	1

Note: This table reports pairwise correlations between residualized grades in each one of the six phases of admission examinations. Residues are obtained by regressing grades on admission exam fixed-effects so all grades are represented as deviations from exam average. Sample is restricted to exams with available grades for all exams (N = 619).

Table A7: Relationship between promotion and performance

	(1)	(2)	(3)
Top quintile	0.0458*		0.0451*
	(0.0260)		(0.0260)
4th quintile	0.00161		0.00114
	(0.0247)		(0.0247)
3rd quintile	-0.0145		-0.0149
	(0.0245)		(0.0246)
2nd quintile	-0.0105		-0.0110
	(0.0243)		(0.0244)
Judge FE		0.00427	0.00336
		(0.00793)	(0.00796)
Observations	2,880	2,880	2,880
R-Squared	0.178	0.176	0.178
Number Admission Cohorts	78	78	78
Dep Var Mean	0.313	0.313	0.313

Note: This table reports results from estimating equations of the form: $\texttt{Promotion}_j = \boldsymbol{\beta ExamRankQuintile_j} + \delta_{w(j)} + \epsilon_j$, where $\delta_{w(j)}$ are admission cohorts (*Concurso*) fixed-effects. The dependent variable is an indicator for whether the judge was promoted to a higher instance during the period covered by our data. Robust standard errors in parentheses (* p<0.1, ** p<0.05, *** p <0.01).

Table A8: Relationship between admission grades and time in court

	(1)	(2)	(3)	(4)
Top quintile	0.552	1.586***	1.807***	3.013***
	(0.758)	(0.481)	(0.476)	(0.843)
4th quintile	1.332*	1.877***	2.043***	3.553***
	(0.741)	(0.479)	(0.472)	(0.956)
3rd quintile	1.083	1.171***	1.249***	1.719**
	(0.747)	(0.446)	(0.441)	(0.840)
2nd quintile	0.571	0.383	0.464	1.688*
	(0.746)	(0.476)	(0.471)	(0.906)
Observations	200,212	200,212	200,212	59,801
R-Squared	0.23	0.73	0.74	0.67
Concurso FE	Yes	Yes	Yes	Yes
Court FE	No	Yes	Yes	No
Month FE	No	No	Yes	No
Court-by-Month FE	No	No	No	Yes
Observation level	Judge-Court-Month	Judge-Court-Month	Judge-Court-Month	Judge-Court-Month

Note: This table reports results from estimating equation of the form: $\texttt{experience}_{jc} = \boldsymbol{\beta ExamRankQuintile_j} + \gamma_c + \delta_{w(j)} + \boldsymbol{X'_{jcm}\theta} + \epsilon_{jcm}$, where $\texttt{experience}_{jc}$ is the time judge *j* spends in court *c*. All specifications include examination cohort (*Concurso*) fixed-effects. Columns (1) through (3) use the exam matched sample, observations used in the two-way fixed-effects models for which judge admission exams are available. Column (4) uses a subset of that sample that excludes all observations for which only one judge is working in any given court in a month. Standard-errors are clustered at the Judge level (* p < 0.1, ** p < 0.05, *** p < 0.01).

Table A9: Relationship between time in court and case disposition

	(1)	(2)	(3)	(4)	(5)
Months in court	0.0345***	0.0194***	0.0324***	0.0264***	0.0219***
	(0.00194)	(0.00158)	(0.00171)	(0.00591)	(0.00189)
Months in court (squared)	-0.000351***	-0.000248***	-0.000358***	-0.0000600	-0.000222***
	(0.0000380)	(0.0000295)	(0.0000307)	(0.000142)	(0.0000324)
Observations	200,212	200,212	200,212	59,801	166,008
R-Squared	0.13	0.42	0.44	0.54	0.46
Concurso FE	Yes	Yes	Yes	Yes	Yes
Court FE	No	Yes	Yes	No	Yes
Month FE	No	No	Yes	No	Yes
Court-by-Month FE	No	No	No	Yes	No
Observation level	Judge-Court-Month	Judge-Court-Month	Judge-Court-Month	Judge-Court-Month	Judge-Court-Month

Note: This table reports results from estimating equation of the form: $\texttt{arsinh}(\texttt{y})_{jcm} = \boldsymbol{\beta experience_j c} + \gamma_c + \delta_{w(j)} + \boldsymbol{X'_{jcm}\theta} + \epsilon_{jcm}$, where $\texttt{experience}_{jc}$ is the time judge *j* spends in court *c*. All specifications include examination cohort (*Concurso*) fixed-effects. Columns (1) through (3) use the exam matched sample, observations used in the two-way fixed-effects models for which judge admission exams are available. Column (4) uses a subset of that sample that excludes all observations for which only one judge is working in any given court in a month. Column (5) drops the first and last two months of all judge-court pairs. Standard-errors are clustered at the Judge level (* p < 0.1, ** p < 0.05, *** p < 0.01).

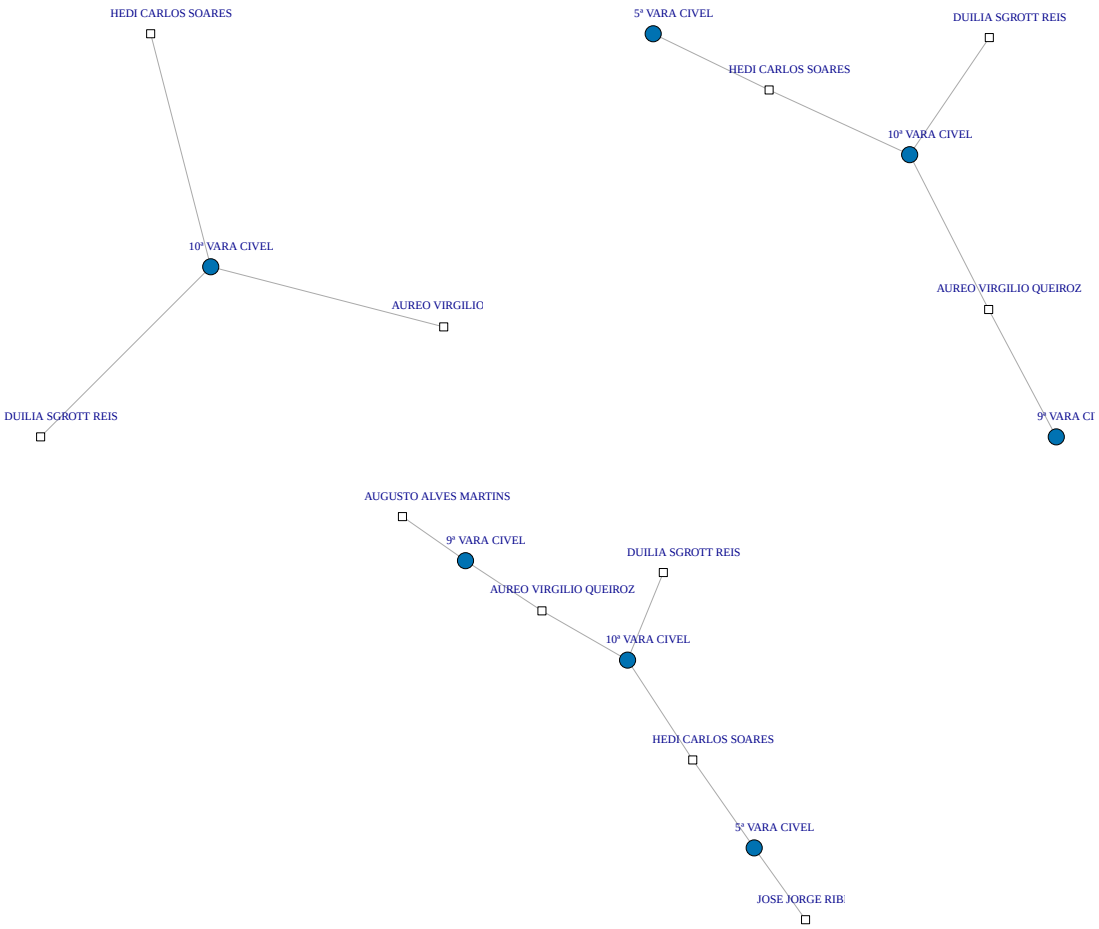
B Connected sets in the data

Connected sets are defined as as groups of organizations (courts) and individuals (judges) connected by "movers", workers who are observed in more than one organization. In our context, there are two sources of variation that allow us to construct connected sets. First, judges are often observed working in more than one court in the same month, allowing us to create connections even within a single period (month). Figure A4 below illustrates this fact. The top-right figure shows three judges observed working in the 10th Civil Court of Porto Velho, in the state of Rondônia, during the month of May 2013. As we can see in the top-right figure, two of these judges also worked in additional courts in that same month – in the 5th Civil Court and the 9th Civil court. These two courts, and all the judges working in them on that same month, are also part of the original connected set – the bottom figure shows that two additional judges were working in these courts in May, and our connected set has expanded.

This within month connection is only one source of variation used to build connected sets. Since we have a panel that covers 76 months, we can build all connections that happened at any point in that period. Figure A5 takes a broader view and presents all connections in the states of Rondônia and Amapá, two small states that allow for better visualization of the judge-court networks. The top two figures and the bottom-left one show all connections for the states of Rondônia in three periods: 2009, 2009-2010 and 2009-2011. Note that when only connections in 2009 are considered, connected sets are large but multiple: clusters of judges and courts are often not connected to other parts of the network. When we explore judges' movements across several years, on the other hand, the network becomes more densely connected: if we consider the entire 2009-2015 period, all judges and courts *within each state* belong to a single connected set, as shown in the bottom-right figure.[43] Since judges are hired to work in a specific state, nonetheless, the figure also shows that each state is a separate connected set: judges in the state of Rondônia, for example, are never observed matched to courts in Amapá, and vice-versa.

[43] All graphs represent connections in the sample used to estimate the two-way fixed-effects model, and therefore include a single connected-set by construction. As discussed above, nonetheless, the largest connected set within each state often includes over 95% of all observations.

Figure A4: Construction of connected sets in the data (Rondônia – May 2013)



Note: These graphs present a selected network of judges (white squares) and courts (blue dots) in the state of Rondônia. Connections between dots and squares represent judges being observed working in a court in the month of May 2013. Starting from the top-left and moving clockwise, the graph expands the connected set by adding courts and judges observed paired in that month. All graphs use data from the sample used to estimate the two-way fixed-effects model.

Figure A5: Visualizing connected sets in the data



Note: These graphs present the networks of judges (white squares) and courts (blue dots) for the states of Rondônia and Amapá. Connections between dots and squares represent judges being observed working in a court in the referred period. The top-left figure presents connections observed in the state of Rondônia in 2009; the top-right includes connections observed in 2009 and 2010, while the bottom left presents connections in the period 2009-2011. The bottom right figure presents the universe of connections observed in in the entire panel for the states of Rondônia and Amapá. It highlights that there are no connections across states, since judges from one state are never observed working in a different state. All graphs use data from the sample used to estimate the two-way fixed-effects model, and therefore within each state all observations are connected by construction.

C São Paulo case-level data

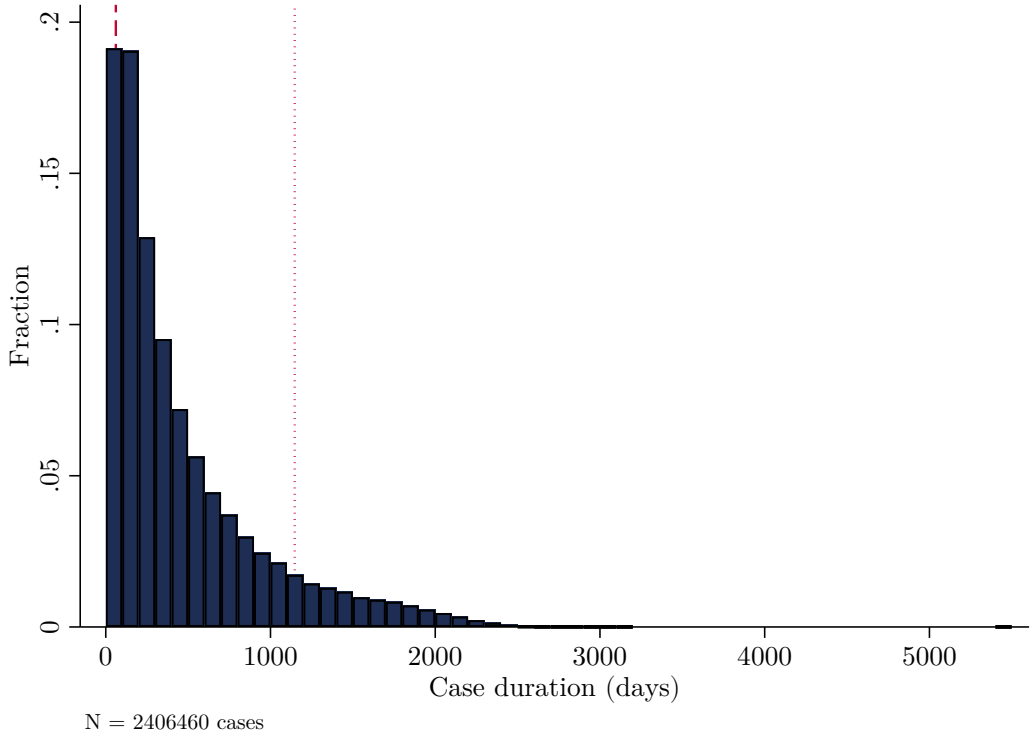
In this appendix we describe additional data on case-level duration from the State of São Paulo. We use data from the "decision docket" (*Banco de Julgados*) for the São Paulo courts between 2009 and 2015 (i.e. a similar period to our main dataset from Open Justice for the entire country).[44] The decision docket contains case-level data on all *decisions on the merit* by judges in the state, including the name of Judges, their court, case number and a series of case characteristics. As in our main analysis, we merge judges' entrance rankings to this new dataset using judges' full names. We explore two main case-level traits in this appendix: the duration of cases and the type of case.

We define the duration of cases as the time between the last distribution of a case to a judge and the disposition of the case. In less than one percent of cases we see previous distribution of a case, meaning that a case might have first been assigned to a different judge and then re-assigned to the judge that decides the case. To focus on the time between distribution and decision for the deciding judge, our duration measure takes into account the last distribution of a case. We also use the case type to assess whether judges' ranking in the entrance exam are predictive about the composition of the cases they dispose of. There are more than 170 case types in our data, but the top three types – common civil procedure, small value civil procedure and debt enforcement – represent more than 60% of total cases.

In Figure A6 we present the distribution of case duration in the data. The median case is decided in about 10 months (300 days) but with wide dispersion: the 10th percentile is 2 months and the 90th percentile over 3 years (1,100 days).

[44]We thank Alexandre Samy and IPEAJUS for providing us with access to the data on case-level duration from the State of São Paulo

Figure A6: Histogram of duration of cases in days



Note: The histogram presents the duration of cases in days, considering only cases disposed on merit in the state of São Paulo. The dashed and dotted lines mark the 10th and 90th percentile of the distribution, respectively.

In Table A10 we assess whether the ranking of judges in the entrance exam is predictive of the case composition they dispose of. In each Column, we regress the share of cases of each type observed in a judge-court pair on the judges' quintile in the entrance exam. Across the seven main categories of cases, judges' entrance exam performance is not predictive of the composition of cases they face: all coefficients are very small in magnitude and not statistically different from zero. This is what we should expect given the institutional context, since cases are randomly assigned to judges, but these results should allay concerns that differences in levels of case disposition and/or speed of judges is driven by the composition of cases they face.

Table A10: Case composition and entrance exam performance

	(1) Common Civil	(2) Small causes	(3) Debt enforcement	(4) Extrajudicial enforcement	(5) Sentence enforcement	(6) Criminal	(7) Embargoes	(8) Other cases
Top quintile	0.00391	0.00223	-0.00625	0.000710	-0.000340	-0.00427	0.00332	0.000696
	(0.00670)	(0.00281)	(0.00365)	(0.00232)	(0.00216)	(0.00475)	(0.00342)	(0.00721)
4th quintile	-0.00234	-0.000328	-0.00199	0.000951	0.000476	-0.000289	0.00173	0.00179
	(0.00541)	(0.00303)	(0.00349)	(0.00214)	(0.00195)	(0.00503)	(0.00261)	(0.00689)
3rd quintile	-0.00233	-0.000893	-0.000870	-0.00256	-0.000949	0.00665	-0.00108	0.00203
	(0.00550)	(0.00257)	(0.00341)	(0.00223)	(0.00207)	(0.00475)	(0.00206)	(0.00736)
2nd quintile	-0.00197	0.00132	0.00134	-0.00171	-0.00245	0.0000754	-0.00123	0.00462
	(0.00542)	(0.00256)	(0.00355)	(0.00222)	(0.00179)	(0.00450)	(0.00221)	(0.00679)
Observations	37,469	37,469	37,469	37,469	37,469	37,469	37,469	37,469
Dep var mean	0.18	0.18	0.09	0.04	0.03	0.11	0.02	0.35
R-Squared	0.56	0.92	0.80	0.42	0.36	0.52	0.29	0.53

Note: This table reports results of regressions using the share of each case type decided by judges. Observations are the judge-by-month level and all regressions include court, month and entrance exam cohort fixed effects. The sample only includes judges in the State of São Paulo, and the composition of cases is constructed using case-level data from the "Banco de Julgados" database. Standard errors clustered at the judge-level are presented in parentheses (* p<0.1, ** p<0.05, *** p <0.01).

We also replicate our main results on the correlation between measures of judges' output and performance in the entrance exam using the new dataset for São Paulo. We re-estimate the two-way fixed-effects model using the case-level data. There are two key-differences with our baseline model. First, since we have data at the case-level, we can control for case type when estimating the AKM. While we document that judges' exam ranking is not predictive of case composition, this is an additional check of whether our results are driven by differential nature of cases. Second, instead of using the number of cases as a proxy for output we can use the duration of cases.

We present our main results in Table A11. One key caveat in all these analyses is that since we only have data for the state of São Paulo our sample is much smaller: we only observe 540 judges vs. 2,880 for the main sample. In the first Column, we present the correlation between the FE estimated using our baseline model (with Open Justice data and number of disposed cases as an outcome) and the model using case-level data in SP. We document a positive and statistically significant correlation between the estimated FE in both models: an increase in 1 s.d. in the baseline model is correlated with a 0.1 s.d. increase in the case-level model.[45] In Column (2) we replicate the results for our baseline model in our restricted sample for São Paulo: the coefficients are slightly larger than our main sample and not always as precisely estimated, but still suggest that judges ranking higher in the entrance exam dispose of more cases. In Column (3) we present the main results using the new case-level data. Using a distinct outcome variable measuring case duration and including case-type controls, we also

[45]Since a small duration is a proxy for better performance, in this table we present results using the negative of judges' FE in the duration model, to obtain positive coefficients.

document a positive correlation between exam performance and judges' FE: those in the top quintile have FEs that are 0.4 s.d. larger than those at the bottom quintile. The effects are again noisier than in our main estimates but magnitudes are similar overall.

Table A11: Robustness - Case duration and entrance exam performance

	(1) Judge FE (duration)	(2) Judge FE (output)	(3) Judge FE (duration)
Judge FE (output)	0.102**		
	(0.0416)		
Top quintile		0.230	0.406***
		(0.140)	(0.137)
4th quintile		0.227	0.149
		(0.150)	(0.140)
3rd quintile		0.260*	0.208*
		(0.144)	(0.121)
2nd quintile		0.229	0.180
		(0.146)	(0.128)
Observations	540	540	540
R-Squared	0.12	0.18	0.13

Note: This table reports results of regressions using the estimated Judge FE using case-level duration data as dependent variable in Columns (1) and (3) and the baseline Judge FE using number of case disposition in Column (2). The sample only includes judges in the State of São Paulo. Robust standard-errors are presented in parentheses (* p<0.1, ** p<0.05, *** p <0.01).

D Correlates of judges' and courts' fixed-effects

In this section we briefly describe whether estimated fixed-effects of courts and judges are systematically correlated with observable characteristics. We start by presenting results for courts' FEs in Table A12. The first panel shows that courts' have higher fixed effects when located in judicial districts outside the state capital, with larger populations and higher urbanization rates. Conditional on time and judges' fixed-effects, this suggests that the number of cases disposed is particularly high in poorer, large urban districts outside the largest urban center of states. There are several possible explanations for that finding. If relative demand for judicial services is higher in these poorer areas, relative to supply, courts in those areas might present higher case disposition, possibly to the detriment of decision quality. It is also possible that the composition of cases in these areas is different, and the higher number of cases in poorer areas reflects the fact that cases are easier to dispose. All those factors might co-exist, and will be picked up by courts' fixed-effects in our model. The results in Table A12 also shed light on how fixed-effects differ by the nature of cases assigned to each court. Similarly to what we observed in the simple descriptive statistics of Figure A1, criminal courts and those dealing with other topics such as commercial law (pooled with "others" here) have a particularly low level of case disposition when compared to general courts.

We now turn to describe how judges' fixed-effects correlate with observable characteristics. Here we rely on the sample matched to RAIS, the employer-employee database of formal workers, in order to construct judges' work history and obtain individual traits such as gender and age. Results are presented in Table A13. In Column (1) we present results for all judges that are matched to RAIS, and in Column (2) we restrict to judges that are observed at least once working outside of the judiciary, in order to include wages prior to judgeship as a correlate. All estimates include connected-sets (State) fixed-effects. Results in Column (1) suggest that individual traits explain very little of the estimated effects: gender, education and experience, both in general and in the judiciary, are not significant predictors of judge fixed-effects. Age is correlated with the estimated effect, with a positive and concave relationship: older judges dispose of more cases, but the effect is diminishing in age. These results, however, are not very robust: when we restrict the sample to those observed working outside the judiciary since 1995, we no longer observe age as a significant predictor, but overall experience does seem positively correlated with case disposition. The coefficient on (log) average yearly wage received before joining the judiciary, which we interpret as potential earnings outside of judgeship, is small in magnitude and not statistically different from zero.

Table A12: Correlation between courts' fixed-effects and courts' characteristics

	(1)
Judicial district characteristics	
State Capital	-0.233**
	(0.104)
Log population (2010)	0.139***
	(0.0292)
Log GDP per capita (2016)	-0.0329
	(0.0529)
Share urban households (2010)	-0.0598
	(0.164)
Second level	0.182**
	(0.0710)
Third level	-0.0246
	(0.0962)
Special level	-0.138
	(0.179)
Type of courts	
Criminal court	-1.073***
	(0.0892)
Civil court	-0.416***
	(0.0827)
Family court	-0.486***
	(0.0957)
Small-stakes court	-0.261***
	(0.0766)
Other courts	-0.551***
	(0.0976)
Observations	2,881
R-Squared	0.148
Number Connected Sets	24
CS fixed-effect?	Yes

Note: This table reports regressions using the estimated courts' FE (standardized to have unit standard deviation within connected sets) as a dependent variable. State capital is a dummy variable indicating whether the judicial district where the court is located is a state's capital; Log population is from the 2010 Census and Log GDP per capita is from the 2016 national accounts published by the Brazilian Institute of Geography and Statistics (IBGE). The omitted category for court type is "general courts" (i.e. courts that cover all types of cases). Robust standard errors in parentheses (* p<0.1, ** p<0.05, *** p <0.01).

Table A13: Correlation between judges' fixed-effects and individual characteristics

	(1)	(2)	(3)	(4)
Male	0.0777*	0.0630	0.0935**	0.0777*
	(0.0413)	(0.0573)	(0.0402)	(0.0413)
Age in 2015	-0.00896	-0.0100	-0.0361	-0.00896
	(0.0422)	(0.0577)	(0.0446)	(0.0422)
Age (squared)	0.000159	0.0000702	0.000507	0.000159
	(0.000506)	(0.000663)	(0.000529)	(0.000506)
Graduate degree	0.0105	0.0323	-0.0231	0.0105
	(0.0973)	(0.115)	(0.0951)	(0.0973)
Formal labor experience in 2015	0.0243	0.0637	0.0214	0.0243
	(0.0227)	(0.0469)	(0.0223)	(0.0227)
Formal experience (squared)	-0.000948	-0.00220	-0.000951	-0.000948
	(0.000853)	(0.00171)	(0.000838)	(0.000853)
Formal judicial experience in 2015	0.0455**	0.0578**	0.00690	0.0455**
	(0.0201)	(0.0289)	(0.0217)	(0.0201)
Judicial experience (squared)	-0.00186**	-0.00229	-0.000312	-0.00186**
	(0.000911)	(0.00148)	(0.000958)	(0.000911)
Formal experience outside judicial sector	-0.0859		-0.0504	-0.0859
	(0.0651)		(0.0635)	(0.0651)
Log average wage before judiciary (2017 prices)		0.0201		
		(0.0285)		
Top quintile			0.182***	
			(0.0624)	
4th quintile			0.141**	
			(0.0648)	
3rd quintile			0.0900	
			(0.0606)	
2nd quintile			0.192***	
			(0.0638)	
Observations	2,471	1,246	2,471	2,471
R-Squared	0.156	0.158	0.232	0.156
Number Connected Sets	22	22	22	22
CS fixed-effect?	Yes	Yes	Yes	Yes

Note: This table reports regressions using the estimated judges' FE (standardized to have unit standard deviation within connected sets) as a dependent variable. Independent variables are obtained from matching judges in the performance dataset to RAIS, a matched employer-employee administrative dataset. Data from RAIS covers the period 1995-2017, so measures of experience in the formal sector and in the judiciary in 2015 are capped at 20 years. Robust standard errors in parentheses (* p<0.1, ** p<0.05, *** p <0.01)