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Can judiciaries constrain executive power? Evidence from judicial reforms ☆

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ABSTRACT

An accessible, swift and unbiased legal system may constrain the executive by limiting expropriation and the misuse of public office for private gain. I test this hypothesis by assembling a database of judicial reforms supported by foreign aid. To address the endogenous placement of these reforms, I implement a within-country identification strategy comparing groups more or less connected to the executive. I find that judicial reforms disproportionately benefit the powerless and discriminated groups of society. Their perception that the president will never ignore the courts and laws is lower at baseline by 11 percentage points compared to other more connected groups in society, and it differentially increases by 10 percentage points after a judicial reform, nearly closing the gap between groups.

The presence of constraints on the executive has been shown empirically to have a first-order causal effect on GDP, investment, access to credit and stock market capitalization (Acemoglu and Johnson, 2005). The argument is that, in countries where the government faces no constraints, politicians are free to steal, exploit, extract bribes without any fear of prosecution, which may stifle investment and economic activity. In contrast, when officials are constrained and punished swiftly in case of misuse of their public office for private gain, there is more security of property rights and thus more investment.

In this paper, I investigate whether judiciaries can act as such a constraint on the executive. The argument is that well-functioning courts make expropriating, stealing and bribing more difficult for government officials, and thus may increase the incentives to invest (North and Weingast, 1989). The challenge to prove this point is empirical: it is difficult to find a credible identification strategy for the causal effect of judiciaries on constraints on the executive that would address omitted variable biases and reverse causality concerns.

To address these issues, I use externally funded judicial reforms implemented by foreign aid agencies as a source of variation for judiciaries' quality. These judicial reforms are explicitly designed to improve the access, speed and quality of courts for all, and thus may add a constraint on the executive. I use the recently released open data International Aid Transparency Initiative (IATI) that collates data related to all foreign aid projects implemented by 500 organizations from 55 countries. To understand more about judicial reforms in particular, I gather documentation from official sources describing these reforms (available in the Online Appendix Acemoglu, 2005; Palumbo et al., 2013). I then classify them according to their targets which tend to be centered around the access, speed, and quality of judiciaries. I also collect data on their budgets to gauge the relative magnitude of these reforms. This process generates a list of 183 judicial reforms implemented in 36 African countries over the period of 2002 to 2015. I focus on Africa for three reasons. First, it is the only continent where the proposed identification strategy is feasible due to the unique availability of data on constraints on the executive as well as various groups' distance from the executive, as described below. Second, there are usually few constraints on executive power in African countries, especially from judiciaries which are sometimes subverted (Sanchez De La Sierra, 2018; Behrer et al., 2019; Widner, 1998; Gainer, 2017), making it a challenging test for the theoretical prediction that judicial reforms can build up constraints. Third, Africa is the largest recipient of foreign aid (35 percent according to that IATI dataset).

Judicial reforms may be placed endogenously. For example, treated countries may be growing faster, with more reform-minded leaders implementing other coincidental reforms. If that is the case, one could confound the effect of judicial reforms with the effect of these other factors.

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To address this endogeneity issue, I employ a within-country identification strategy by finding groups that could be more or less affected by judicial reforms. I use an insight uncovered by a recent empirical literature on judiciaries: some groups suffer from worse access and face discrimination in the courts - usually because of their lack of political connections to the executive that may have captured local judiciaries (Behrer et al., 2019; Sanchez De La Sierra, 2018; Alesina and La Ferrara, 2014; Abrams et al., 2012; Shayo and Zussman, 2011). The judicial reforms considered in this paper may have a disproportionate effect on these unconnected groups because these reforms are explicitly designed to improve the access, speed and quality of the judiciary for all (not just for groups connected to the executive).

A remaining threat to identification could be other policies or reforms implemented at exactly the same time and disproportionately benefiting unconnected groups. To account for this, I use the IATI data to compute indices of all other foreign aid initiatives to verify whether the effect of judicial reforms persist even after controlling for all these other initiatives. In fact, the IATI data allows me to develop new indicators of foreign aid projects supporting other well-known counter-powers to the executive: the media, human rights groups, anti-corruption organizations, the legislative body, communication technologies (such as the internet, which may foster social unrest and uprisings) and a transparent election process. This allows me to compare the relative importance of the judiciary versus these other counter-powers in checking executive power.

To implement this identification strategy, I develop new indicators of constraints on the executive. Constraints on the executive is notoriously difficult to measure and subject to interpretation since executive power varies from year to year according to variations in political circumstances and dynamics. The most commonly used measure in the existing literature is the Polity IV dataset, based on perception from experts who assign a score to each country (subject to "oscillation" or "fuzziness" according to their creators (Marshall et al. (2016), p. 7), i.e., low intercoder reliability). These perception surveys at the national level cannot be used for a within-country identification strategy.

Instead, I use microeconomic household surveys collected among 182,933 households living in 36 countries between 2002 and 2015 and which ask the same four following questions: "Does the President ignore the courts and laws of the country?"; "How often do officials who commit crimes go unpunished?"; "How many of the President/Prime Minister and Officials in his Office do you think are involved in corruption?"; "How many of the government officials and local government councilors do you think are involved in corruption?". These questions capture the essence of why the judiciary acts as a constraint on the executive. If the president can freely ignore the courts and laws, if officials who commit crimes go unpunished, and if the president or other government officials routinely engage in corruption, then the judicial branch of government does not serve one of its main functions as a credible check on power. I find that these variables are all strongly correlated with the Polity IV measure of constraints on the executive, and present three additional advantages: 1) they are collected on a combined sample of 182,933 individuals living in the countries of interest as opposed to a panel of experts, 2) they allow the possibility of within-country identification strategy, 3) they are important to verify whether these judicial reforms are not merely reforms in name, but actually place a constraint on the executive that is discernible by the local population and that may affect people's behavior and incentives to invest.

I then match these household surveys to a recent database of political connections of ethnic groups: the Ethnic Power Relations (EPR) dataset (Vogt et al., 2015). In this database, each ethnic group is classified according to the degree of access to executive power. Some ethnic groups are said to have a monopoly on power, to be dominant, to be senior partners, or to be junior partners. I call them the "Connected" groups. Other groups are classified as powerless, discriminated, or irrelevant. I call them the "Unconnected" groups. I merge each ethnic group in every round of the household surveys to the corresponding year in the EPR dataset, to get a measure of political connectedness to the executive.

I find that judicial reforms disproportionately benefit the powerless, irrelevant and discriminated groups of society. These groups trust more the courts after the judicial reforms. A concern with these results is that they are about perceptions of judicial quality, not actual judicial outcomes. I show that perceptions of judicial quality are in fact very strongly correlated with actual judicial outcomes, using other sources of data where both measures were available, one from rural Kenya (Aberra and Chemin, 2020) and one from Pakistan (Acemoglu et al., 2020). Perceptions matter. If citizens start perceiving that the courts might treat them fairly despite them being unconnected, that is an absolutely crucial first step. Of course, in the future these perceptions and expectations might turn out to be incorrect, but they have to be fundamental building blocks of an unbiased legal system.

The perception of constraints on the executive also change for the unconnected groups. The proportion of people from the unconnected groups who say the president will never ignore the courts and laws is 30 percent at baseline (11 percentage points less than in the more connected groups). This figure increases by 10 percentage points after a judicial reform, almost closing the gap in perceptions between unconnected and connected groups. The effect is large, a 0.2 standard deviation increase.

The effects of judicial reforms persist after controlling for other foreign aid projects related to: education, health, transport, energy, support to the private sector, support to the executive, emergency, banks, budget support, food, environment, tax, conflict, debt and trade. Moreover, the effects of judicial reforms persist after controlling for foreign aid projects that support other well-known counter-powers to the executive. The robustness of these results show that judiciaries occupies a central role in constraining governments.

The increased constraints on the executive translate into perceived economic benefits. The less politically connected report at baseline more problems of corruption with government officials and worse economic outcomes, but judicial reforms close the gap in perceptions across groups. This effect is achieved at the relatively small cost of 1 Billion dollars for these judicial reforms (950 Million to be precise), which represents a small fraction of total foreign aid (1.5 percent, according to the IATI dataset).

This paper is the first to use judicial reforms as a source of variation to show the effects that judiciaries have on constraining executive power. Porta et al. (2004) and Feld and Voigt (2003) provide measures of judicial independence and use cross-country regressions to show that judicial independence is good for the protection against state expropriation and for economic growth. I contribute to this literature by focusing on a potential source of judicial independence: judicial reforms that are financially supported by international organizations and explicitly designed to improve the access, speed, and quality of judiciaries. The spatial and temporal variation in the implementation of these reforms allows me to include country fixed effects in the econometric analysis, which has not been done in the existing literature. Moreover, I develop microeconomic measures of constraints on the executive which allows me to adopt a within-country identification strategy.

This paper also contributes to a more recent literature measuring the causal effect of judiciaries on economic development (von Lilienfeld-Toal et al., 2012; Lichand and Soares, 2014; Kondylis and Stein, 2017; Chemin, 2009; Chemin, 2009; Chemin, 2010; Chemin, 2020). This literature has focused on what Acemoglu and Johnson

(2005) call the "horizontal" effects of judiciaries, i.e., the ability of judiciaries to enhance cooperation among private agents by improving contract enforcement. In my paper, I focus on the "vertical" effect of judiciaries - the ability of judiciaries to protect private citizens against expropriation from predatory governments. This is important because citizens may find alternative arrangements to contracting imperfections (through trade in social networks, reputation, and repeat business) while there may be fewer recourses against powerful governments (the "vertical" effect).

The rest of the paper is organized as follows. Section 1 provides a conceptual framework for the role of externally financed judicial reforms in building constraints on the executive. Section 2 presents the empirical strategy and the data used to implement it. Section 3 presents the empirical methodology. Section 4 presents the results and Section 5 the robustness checks. Section 6 concludes.

1. Literature review and conceptual framework

1.1. Judiciaries as constraints on the executive

Constraints placed on the executive have been identified as a first-order determinant of economic development (North and Weingast, 1989; North, 1990; Acemoglu and Robinson, 2013). When elites face no constraints, they can freely expropriate the rest of the population and misuse public office for private gain. This decreases the incentives to invest and by the same token, economic development. In contrast, when elites are constrained and face sanctions for their actions, it is harder for them to expropriate or engage in bribery, which may reassure investors and unleash economic development.

There is convincing empirical evidence supporting the above proposition. Using the Polity IV dataset, and instrumental variables such as the mortality rate of settlers and initial population density in 1500, Acemoglu and Johnson (2005) have found large causal effects on economic development: if a country could somehow increase its constraint on the executive by 1 standard deviation (2 points on the 7-point scale of the Polity IV measure), then GDP per capita would increase by 2 standard deviations, moving a country from the middle to the top 5 percent of the distribution of income. Constraints on the executive dwarf other explanations of growth such as religion, latitude, macroeconomic indicators (inflation, government consumption, real exchange rate overvaluation) or legal formalism (i.e., procedural complexity which is instrumented by english legal origin).

Considering this evidence, the challenge is then to understand how these constraints can be built in the short- or medium-run. This would hold the key to economic development.

North and Weingast (1989) show through the history of England that an independent judiciary was a key factor in constraining the king. Before the Glorious revolution in 1688, the English king was free to ignore the courts and laws through two institutions: royal prerogatives and the Star chamber. The king could ignore the common law courts and issue new rules through prerogative courts which had the power to suspend laws for specific individuals. Moreover, the Star Chamber had the final say, and could reverse any judgments against the Crown. Thus, while the common law was often used against the king, he could simply alter the jurisdiction of a dispute by issuing proclamations (p.814). The situation changed soon after the King's defeat in the English civil war in 1640: the Star chamber was abolished in 1641. Moreover, after the Glorious revolution in 1688, the judiciary was made independent by The Act of Settlement (1701). The Act mandated that judges enjoy tenure during good behavior rather than at the pleasure of the Crown, thereby creating a check on both the government and Parliament. North and Weingast (1989) show that this was linked with an expansion in borrowing because lenders were more secure that the executive was committed to repaying loans. Using a daily event study, Klerman and Mahoney (2005) show that the Act of Settlement (1701) triggered economic growth: abnormal returns on stocks increased significantly around that time.

Extrapolating from the case of England, Porta et al. (2004) and Feld and Voigt (2003) provide measures of judicial independence and use cross-country regressions to estimate its impact empirically. They find that judicial independence helps protect against expropriation from the government and supports economic growth.

In the context of Africa studied in this paper, judiciaries have not been strong constraints on executives, if anything, the opposite is true, i.e., strong executives usually influence and subvert judiciaries. The most extreme case of influence is perhaps Uganda's leader Idi Amin killing of the Chief Justice in 1972. Widner (1998) describes more subtle ways through which judges can be influenced in Tanzania: judges live in communities, need to rent houses from powerful landlords, and are thus influenceable by powerful local elites. In the Democratic Republic of Congo, Sanchez De La Sierra (2018) describes how one ethnic group controls the state, fills state positions including the judiciary by nepotism, and has thus captured the judiciary.

One exception is Kenya. In 2017, the Supreme Court stunned observers by annulling elections won by the incumbent president. These observers went on to proclaim the birth of the rule of law in Africa.[1] The incumbent called the judges "thugs" and "crooks", promising to "fix the problem", alleged the judges were "paid by foreigners and other fools", but ultimately complied with the decision to rerun elections.[2] Interestingly, the World Bank had supported since 2012 a $120 Million judicial reform that built courts (access), designed a performance management system (speed), and trained judges (quality). Whether this judicial reform, by improving the access, speed and quality of the judiciary for all (including the opposition party), causally contributed to this episode which effectively added a new constraint on the executive is the topic of inquiry of this paper.

Of course, there could be many other constraints on the executive than the judiciary: the legislature (Poulsen and Varjao (2020, 2014); or the Parliament in England as in North and Weingast (1989)), a free press (Besley and Burgess, 2002), civil society and non-governmental organizations (Acemoglu and Robinson, 2019; Bjorkman and Svensson, 2009), free and fair elections (Ferraz and Finan, 2008; Olken, 2010). A contribution of my paper is to compare the effects of judiciaries to these other counter-powers by using the same dataset and developing measures of financial support for them.[3]

1.2. A framework for judicial reforms

The effect of judiciaries as constraints on the executive and the consequences for economic growth can be seen in a dynamic infinite-horizon model involving both citizen-producers and a self-interested ruler (see the theoretical model in Appendix A). A citizen produces a quantity of output that depends on own effort

[1] New York Times, "Kenya Supreme Court Nullifies Presidential Election", September 1, 2017, https://www.nytimes.com/2017/09/01/world/africa/kenya-election-kenyatta-odinga.html?_r=0.

[2] Financial times, "Kenya judges condemn political attacks on judiciary ahead of vote re-run", September 19, 2017, https://www.ft.com/content/242bc5d0-9d48-11e7-8cd4-932067fbf946.

[3] A more diffuse but nonetheless effective constraint on the executive can be the threat of revolution (Acemoglu and Robinson, 2006). Unfortunately, it is impossible to measure this in the IATI dataset since there is no foreign aid support to "revolutions" per se.

as well as the level of public goods in the economy (education, health, infrastructure, contract enforcement between private citizens, etc.) which is provided by the ruler. Once the effort level has been sunk and output has been produced, the ruler can grab some of the output (either through formal taxation or by outright stealing, expropriating, or asking for one or multiple bribes) for his own consumption as well as for the provision of public goods. If expropriation is excessive, for example if all the output is grabbed away, the citizen can decide to sue the ruler in court. A constraint is placed on the ruler by the judiciary: with a certain probability p and after a certain time T (the values of both parameters depend on the functioning of the courts), the ruler may lose the case and compensate the producer. In that case, the producer recovers an amount $p\beta^T$ of the original claim (β is the discount factor) less the legal fees of value l_p (proportional to the amount of the case, i.e., the value of the output grabbed away). In other words, the sign of the expression $p\beta^T - l_p$ critically determines whether the citizen sues or not.

The fundamental tension in this model resides in the conflicting effects of the judiciary on the ruler. A high-quality judiciary hurts the ruler since he can be sued in court. However, a high-quality judiciary also benefits the ruler since citizens recover a greater fraction of their output and invest more, thereby raising the revenues that can be gained from expropriating them (for either private consumption by the ruler or the financing of public goods that further benefit citizens). In Appendix A, I formally show that the balance of these two conflicting forces leads to an optimal level of functioning of the judiciary (called $(p\beta^T)^R$ in the model, where subscript R indicates the level chosen by the ruler). Importantly, the optimal level chosen by the ruler is less than the one that would maximize economic growth. The intuition is simple: the ruler prefers a lower-quality judiciary that increases its revenues, but by doing so, also leaves less for the worker, which depresses effort and investment.

In this context, externally-imposed judicial reforms would increase the overall level of economic development but they are not optimal for the point of view of the ruler. What this model illustrates is that judiciaries may be good for growth, but they would not emerge endogenously under a self-interested ruler.

1.3. Heterogeneous effects of judicial reforms on groups more or less politically connected

Judicial reforms may have different effects on groups more or less politically connected. This is an important point for the within-country identification strategy, which will be later used in this paper to address the endogenous placement of judicial reforms in a triple difference analysis.

This point is easily viewed in the theoretical model of Appendix A.A4. I show that if citizens had sufficient political power, they would optimally choose a level of judicial efficiency (called $(p\beta^T)^C$ where subscript C indicates the level chosen by the citizens) greater than the one chosen by the ruler (called $(p\beta^T)^R$, as explained above). The intuition is simple: citizens prefer to keep more for themselves. The politically connected citizens may choose $(p\beta^T)^C$ for themselves, but the unconnected citizens must operate with the level chosen by the ruler $(p\beta^T)^R$, less than $(p\beta^T)^C$. Judicial reforms that increase $p\beta^T$ to the same level across groups would benefit everyone, but more so the group starting with a lower $(p\beta^T)^R$, i.e., the unconnected groups.

There is empirical support for the basic premise of this theory, i.e., that the groups with more political connections have access to better functioning judciairies. Sanchez De La Sierra (2018) provide a randomized experiment proving this point. In their experiment, they introduce formal contracts in a situation where the rational strategy is for the buyer to renege on paying for a good provided by a seller.[4] The striking finding in their study is that formal contracts increase payment, but only when the seller is from a certain ethnic group which controls the state and fills state positions (including the judiciary) by nepotism, and has thus captured the judiciary. There is no increased payment from buyers if the seller is a from a different ethnic group. In other words, the level of protection experienced by ethnic groups not connected to the executive $(p\beta^T)^R$ is much lower than the one enjoyed by the ethnic group connected to the executive $(p\beta^T)^C$.

Generalizing outside of the context of the Republic Democratic of Congo, Behrer et al. (2019) show that over 80 percent of the population in some countries believe that most judges decide cases according to what the government or powerful private interests tell them to do, not what the law says. As a result, the weak are vulnerable to the strongs' taking and expropriating. According to the same paper, about 40–50 percent of respondents in most countries say it is unlikely that homeowners will be fairly compensated in case of expropriation by the government. Behrer et al. (2019) conclude that the justice system disproportionately fails the weak. Another strand of the literature which uses random assignment of cases to judges has found rigorous evidence of judicial bias against minorities and groups who face discrimination in society at large: African-Americans in the United States (Alesina and La Ferrara, 2014; Abrams et al., 2012), Arabs in Israel (Shayo and Zussman, 2011).

One should be cautious not to generalize based on very few examples, and an important goal of this paper will be to confirm this finding in a systematic way. But this literature seems to indicate that the less politically connected groups suffer from worse access and lower quality courts. In this case and for these individuals, the judicial branch of government cannot serve one of its main functions as a credible check on power.

In this paper, I will test whether judicial reforms that improve the access, speed and quality of courts for all, not just the connected groups, can change this.

1.4. Implications for judicial reforms

To summarize, the implications for judicial reforms are clear, and can be seen from the expression $p\beta^T - l_p$ (the fraction of output a citizen expects to recover in court). Any judicial reform that improves p and reduces T would increase the economy's output and citizen's welfare, and the effect is greater for the less politically connected (who start from a low $(p\beta^T)^R$) rather than for connected individuals (who start from a higher $(p\beta^T)^C$). Moreover, any judicial reform increasing access to the judiciary, i.e., decreasing the plaintiff's legal fees l_p, would achieve the same effects.

In fact, the nature of the expression $p\beta^T - l_p$ highlights an interesting implication for the optimal design of judicial reforms: only comprehensive reforms that simultaneously increase p and decrease T and l_p will have an effect. Since p and T are complements, any simultaneous change in p and T has a greater total effect than piecemeal reform affecting only one of the factors. To see this, consider the extreme case of a very slow judiciary, $T = \infty$. In this context, any increase in the quality of courts p has

[4] Buyers are sold soaps or cell phone credits, delivered now but that they can pay by phone after 2 days. The delay between delivery and payment offers an opportunity to defect for the buyer. The rational strategy for a buyer is to get the good and later renege on paying. Buyers in the treatment group must agree to sign a formal contract, while buyers in the control group first agree to sign a formal contract but are later told they can proceed without signing a contract.

no effect. Similarly, consider the extreme case of low quality of courts that systematically rule in favor of the expropriator, $p = 0$. In this context, any increase in speed also has no effect. In fact, even if both p and T are somehow increased, this may have no effect if l_p is so high that citizens cannot access the courts (since $p\beta^T - l_p$ is negative). In other words, judicial reforms must be comprehensive, simultaneously increasing p and decreasing T and l_p. Also, they must be of a large enough size to reverse the sign of $p\beta^T - l_p$ if it was negative to start with. In the extreme case of an inaccessible, slow and corrupt judiciary, limited reforms, i.e., reforms targeting only one characteristic of judiciaries (access, speed, or quality), will have no effects.

Overall, this model formalizes the idea of the impossibility for a ruler with no constraints to build a highly effective judiciary. No rational leader would willingly place a constraint on themselves, in the form of an optimally-functioning judiciary. This is because the judiciary reduces his share of output that can be obtained from the economy, despite the positive incentive effects on citizens' effort. A ruler will implement a judiciary that maximizes his utility, not the productive capacity of the economy, nor citizen's welfare. An outside intervention raising the quality of judiciaries may move the economy towards a situation of greater output and welfare, at the detriment of the ruler. Thus, judicial reforms may encounter significant resistance from the ruler (or the local elite) in their implementation.

I test these implications in the data by looking at the effect of externally-funded judicial reforms on the constraints faced by rulers. In the next section, I present the data on judicial reforms (and their coding into comprehensive versus limited reform), on the measures of constraints on the executive, and on the political connectedness of ethnic groups.

2. Data

2.1. Judicial reforms

To obtain information on externally funded judicial reforms for the 36 countries of the Afrobarometer dataset for the period 2002–2015, I use data from the International Aid Transparency Initiative (IATI). IATI collates all data related to foreign aid projects implemented by 500 organizations (donor governments, multilateral agencies, foundations, non-governmental organizations and private sector organizations) in 55 countries and within this database, I isolate all projects related to the judiciary.[5] For each project, the IATI data includes: the year and country of implementation, the implementing organization, a brief description and the budget. I complement the IATI dataset with the World Bank and USAID databases which publish their data in the same format but contain more detailed information. To understand more about the judicial reforms in question, I gather documents describing them from official sources (all documents available in the Online Appendix,[6] folder "Judicial Reforms"). This generates a list of 183 projects, as can be seen in "List Judicial Reforms.xlsx" in the Online Appendix.

I then codify the descriptions of these projects according to the three characteristics of efficient judiciaries highlighted in the theoretical section: access, speed and quality.[7] In keeping with the theoretical section that distinguishes between comprehensive and limited reforms, I also define a variable "Reform_Comprehensive". This variable takes on a value of 1 if, during a given time period, a country received reforms which simultaneously addressed the three characteristics (access, speed, and quality), AND exceeded 5 percent of the national judiciary's annual budget (see Appendix B for an explanation of how data of judiciaries' budgets was collected). I use a threshold of 5 percent since it is the average budget of both World Bank and USAID's judicial reforms and legal scholars recognize the World Bank and USAID as the "largest and most influential rule of law actors" (Humphreys, 2010, p.20). In the empirical section, I vary this threshold and show that results are not sensitive to the choice of this particular value. I also define a variable "Reform_Limited" as 1 if a country received a reforms with either a small budget or not targeting all three characteristics (quality, speed, or access).

To illustrate the process which is outlined above, I present the full history of judicial reforms in Kenya between 2003 and 2015 (i.e., in between rounds of Afrobarometer data) in Table 1. The general layout of the table is organized as follows: Rows (2), (7), (10), (13) and (15) represent years when a round of Afrobarometer data was collected. The remaining rows show the specific judicial reform projects which were undertaken between rounds of data collection. Columns Qua., Sp., Acc., show the characteristics of each individual intervention, while columns "Comprehensive" and "Limited" are ratings for the entire time period between Afrobarometer rounds. To use a more concrete example, consider rows (2) to (7).

Row (2) shows that Afrobarometer data was collected in 2014 and that the period from 2011–2014 is one in which Kenya experienced comprehensive judicial reform. To understand why the reform was deemed comprehensive, we must look at the specific interventions (rows (3) to (6)) that were carried out during that period. Row (3) shows a reform supported by the United Nations that started in 2013 and whose description refers to a "Judiciary Transformation Framework". The document "Kenya/Kenya Judiciary Transformation Framework.pdf" (see Online Appendix, folder "Judicial Reforms") explains the four pillars of that reform: *"1) people-focused delivery of justice; 2) transformative leadership, organizational culture and professional staff; 3) adequate financial resources and physical infrastructure; 4) harnessing technology to facilitate speedier trials and enhance the efficiency and effectiveness of administrative processes."*[8] This reform is clearly designed to improve the access, speed, and quality of the judiciary so the variables reform_access, reform_speed, and reform_quality take on a

[5] I follow the exact same procedure outlined in Chemin (2020) but extend the database to 2002–2015 while Chemin (2020) only coded judicial reforms in between the baseline and endline years (which can vary according to the country) of the World Bank Enterprise Surveys.

[6] Temporarily hosted at: https://www.dropbox.com/sh/m5cszcvynpfcnu1/AAAXn2Y7FAwQbNtnmCdmrH2Fa?dl=0.

[7] To be more precise, I assign the value 1 to a variable "reform_access" if one of the objectives of the reform is increasing access to the judiciary. In practice, I look for keywords such as *access, legal services, for the poor, justice for all, J4Aorthe building of courts* (which reduces the distance to courts, thereby improving access). Similarly, I assign the value 1 to a variable "reform_speed" if one of the objectives of the reform is to increase the speed of courts and I look for keywords such as *effective, efficiency, fast, increase in cases disposed, reduction in pending cases, reduction in backlog.*Finally, I assign the value 1 to a variable "reform_quality" if one of the objectives of the reform is to improve the quality of the judiciary. I look for keywords such as *legal training for justice actors, legal education for judges, improvement of decision-making, capacity building, capability, accountability, integrity, independence, anti-corruption, governance, compliance with rule of law, fairness, improved service delivery, strengthening of the rule of law.*

[8] For example, "people-focused delivery of justice" means: *"ensuring awareness of and understanding of the law and procedures by litigants, simplifying court documents and procedures, enhancing the easy availability of information pertinent to litigants' cases, improving the affordability of the adjudication system; ensuring the cultural appropriateness of court procedures and processes, introducing friendly and non-intimidating courts, ensuring the timeliness in the processing of claims and enforcement of judicial decisions, increasing the number of mobile courts promoting, facilitating Alternative Dispute Resolution (ADR), and establishing a customer care desk at every court station*". These measures are designed to improve access for all individuals and firms alike.

Table 1
Judicial reforms in Kenya between 2003 and 2015.

Row	Year	Data	Comprehensive	Limited	Organization	Description	Budget	Qua.	Sp.	Acc.
1	2015				Ministry of Foreign Affairs - Netherlands	Promoting access to justice for the poor and marginalized	1.2			1
2	2014	Yes	1	0						
3	2013				United Nations Development Programme	This project supports the Judiciary in implementing the Judiciary Transformation Framework's principles and goals for transformation.	0.98	1	1	1
4	2012				Sweden	Support to Judiciary Transformation Framework in partnership with GIZ - (German Development Agency).	2.21	1	1	1
5	2012				World Bank	The objective of the Judicial Performance Improvement Project (JPIP) for Kenya is to improve the performance of the judiciary to provide its services in the project areas in a more effective and accountable manner.	120	1	1	1
6	2012				Canada (DFATD)	The goal of this project is to improve legal services for children and youth in East Africa, so that they have access to legal protection.	3.54	1		
7	2011	Yes	0	1						
8	2010				Ministry of Foreign Affairs, Finland	The purpose of the project has been to enable the marginalized communities living in Wajir district to access justice, enjoy their legal rights.	0.11	1		
9	2009				Canada (DFATD)	The program aims to improve access to justice in the programming countries, particularly for poor and marginalized people, including women.	2.48	1		
10	2008	Yes	1	0						
11	2006				Sweden	Support to the Governance Justice Law and Order Sector Reform Programme (GJLOS)	11.4	1	1	1
12	2006				Ministry of Foreign Affairs, Finland	Support to the Governance Justice Law and Order Sector Reform Programme (GJLOS)	11.3	1	1	1
13	2005	Yes	0	1					1	
14	2005				Ministry of Foreign Affairs - Netherlands	Transforming and strengthening the sector institutions for efficient, accountable and transparent administration of justice.	6.42	1	1	
15	2003	Yes	0	0						

Budget is in Million USD.

value of 1. Nonetheless, the budget is low: 0.98 Million USD compared to the Kenya's judiciary budget of 136 Million USD (in 2016). Considering the small budget size, one should not expect that such a project by itself would have large effects on outcomes such as constraints on the executive, propensity of the President to ignore the courts, or economic development. Row (4) shows that Sweden also provided support of 2.21 Million USD for that same reform, yet the overall budget is once again low.

Row (5) shows that the major project of this time period in Kenya was a World Bank program called the "Judicial Performance Improvement Project" (JPIP). JPIP started in 2012 with a planned duration of 6 years and a total budget of $120 million.[9] The explicit goal of the reform was to improve *"performance, accountability, access to justice and the expeditious delivery of judicial services."*Considering the keywords in the description of this reform, I codify it as intending to improve access, speed, and quality.

Row (6) shows another small (3.54 Million USD) project, funded by Canada, that started in 2012. Row (7) shows that an Afrobarometer dataset was collected in 2011.

Thus, between 2011 and 2014, two rounds of Afrobarometer data were collected, and rows (3) to (6) show that four judicial projects were implemented during that time. Together, these reforms tackled access, speed, and quality. Their combined average annual budget was 31 percent of the Kenya judiciary's annual budget (more than 5 percent, the threshold used in this paper) and this is why "Reform_Comprehensive" takes the value 1 in Row (2): taken as a whole, the period 2011 to 2014 is one in which a comprehensive reform was implemented.

Notice that the way "Reform_Comprehensive" is defined leaves little room for interpretation in the coding. Although there can be some disagreement in the coding of individual judicial reforms, "Reform_Comprehensive" is calculated as an aggregate of all reforms at the country level. For example in the case of Kenya, there can be some disagreement on whether the 2012 Canadian project (in Row (6)) should be coded as improving access because of its focus on specific sections of society. Yet overall, it is quite clear that Kenya received a comprehensive reform considering the budget size and breadth of the numerous projects which were undertaken from 2011–2014.

Row (1) is slightly different because it displays a reform which was implemented outside the timeframe for which Afrobarometer data is available but the rest of the table follows the same structure as described above. Rows (8) and (9) show two small projects in 2009 and 2010 which only targeted access. Thus, Kenya received a limited reform between the 2008 and 2011 rounds of the Afrobarometer dataset and "Reform_Limited" gets a value of 1 in Row (7).

Rows (11) and (12) show that Sweden and Finland supported a reform called the "Governance Justice Law and Order Sector Reform Programme (GJLOS)" in 2006.[10] This reform mentions access (improved access to the poor, marginalized and vulnerable), speed (improved service delivery), and quality (building capacity by training 3200 people, reducing corruption, renovation of 12 courts). The combined financial support during this period was 22 Million USD or 6 percent of the Kenyan judiciary's annual budget. This reform is thus coded as comprehensive, as shown in Row (10).

Row (14) shows a Dutch project with a budget of 6 Million USD (only 2.3 percent of the judiciary's annual budget) and not targeting access. This reform is thus coded as limited in Row (13).

[9] See the full description in Kenya/Kenya JPIP description.pdf

[10] See "Kenya/Kenya_GJLOS.pdf" in the Online Appendix for more details.

Prior to 2003, no reforms were implemented and the Afrobarometer data did not exist so no analysis can be run. Thus, both "Reform_Comprehensive" and "Reform_Limited" are equal to zero in row (15).

I repeat this exercise for the remaining 35 countries of the Afrobarometer dataset (see a short explanation for each country in Appendix E, the full list of reforms and all documents describing the reforms are in the Online Appendix). Overall, there are 13 country-periods with a comprehensive reform, 34 with limited reforms, and 77 with neither comprehensive nor limited reforms.

2.2. Data for constraints on executive

The goal in this paper is to relate these judicial reforms to constraints on the executive, in a within-country identification strategy (by comparing groups more or less affected by judicial reforms, depending on their political connectedness).

Since the identification strategy is within countries, the challenge is to find a micro-level measure of constraints on the executive. I use 6 rounds of the "Afrobarometer" dataset which are collected in 36 countries between 2002 and 2015 and have a combined total of 182,933 observations. I use four questions as measures of constraints on the executive. The first is:

> "How often does the President ignore the courts and laws of the country? (0 = Always, 1 = Often, 2 = Rarely, 3 = Never)"

This question captures the essence of North and Weingast (1989) and their description of how the English king could ignore the courts and laws by using royal prerogatives and the Star Chamber before the Civil war: "Rules the sovereign can readily revise differ significantly in their implications for performance from exactly the same rules when not subject to revision. The more likely it is that the sovereign will alter property rights for his or her own benefit, the lower the expected returns from investment and the lower in turn the incentive to invest. For economic growth to occur the sovereign or government must not merely establish the relevant set of rights, but must make a credible commitment to them." (p.803)

If the rule of law is respected, the president should never ignore the courts and laws. I thus define a dichotomous variable equal to 1 if people answer "Never" to the question above, 0 otherwise (grouping all other answers 0 = Always, 1 = Often, 2 = Rarely, in the 0 category). As will be shown later in the empirical section, results are similar when I use a continuous variable on the 0 to 3 scale.

To make sure the results are not coming from a single variable, I also use:

> "How often do officials who commit crimes go unpunished? 0 = Never, 1 = Rarely, 2 = Often, 3 = Always"

If officials can commit crimes and go unpunished, constraints on their behavior must be very weak. The hypothesis of this paper is that by improving the access, speed and quality of the legal system, judicial reforms will place a constraint on their behavior and affect this variable.

Finally, I use:

> "How many of the President/Prime Minister and Officials in his Office do you think are involved in corruption? (0 = None, 1 = Some of them, 2 = Most of them, 3 = All of them)."

and another similar question that refers to other government officials instead of the president. Once again, the hypothesis is that judicial reforms will strengthen constraints on the executive and curb corruption.

To check the validity of these new measures, I compare them with the traditional measure of constraints on the executive used in the literature: the Polity IV data set. Fig. 1 below shows the correlation between the first question (i.e., the president never ignores the courts) and the Polity IV data, a score that ranges from one to seven, and where a higher score indicates greater constraints (the graph shows the average of both measures between 2002 and 2015). There is a clear positive relationship between both measures ($r^2 = 0.5$, significant at 5 percent), confirming that this variable from Afrobarometer is a valid measure of constraints on the executive. Similarly, the three other variables (likelihood of officials who commit crimes to go unpunished, corruption of presi-

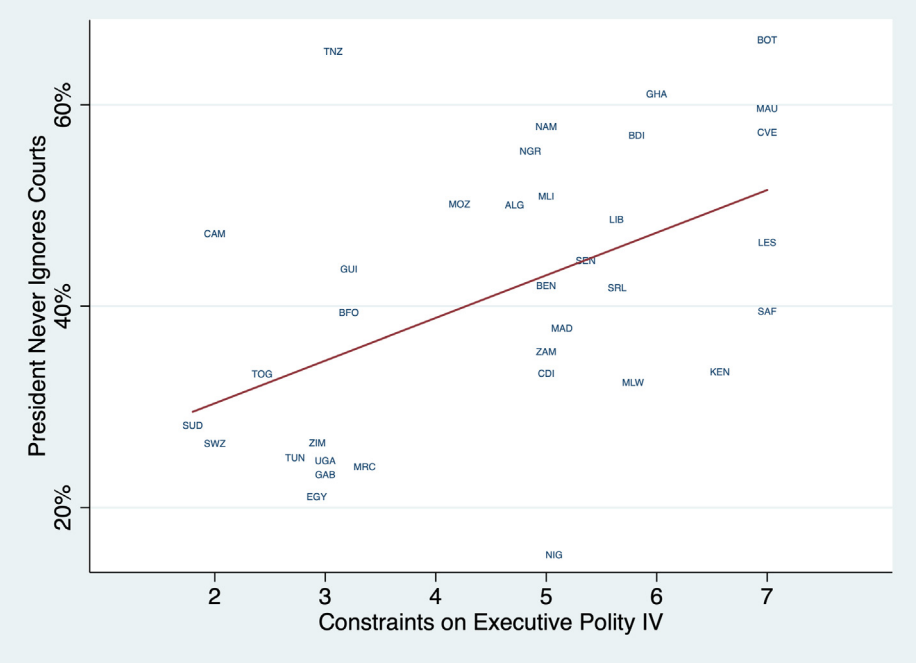

Fig. 1. Correlation between "President never ignores the courts" and "Constraints on the Executive".

dent, corruption of government officials) are strongly (negatively) correlated with the Polity IV measure (with correlations of 0.53, 0.36 and 0.36 respectively, significant at 5 percent).

One issue with the question "Does the president ignore the courts?" is that if courts are totally subservient to the executive, there may be no decisions against the executive, and therefore nothing to ignore. Thus, people might answer that the president is never ignoring the courts, while the president is in fact facing no real constraints. This possibility is ruled out by Fig. 1 above: there are no countries in which people say the president never ignores the courts and laws (which would correspond to y = 100 percent) and the president faces few constraints (which would correspond to x = 1). The only exception may be Tanzania where most people say the president never ignores the courts and laws (y = 60 percent) while the president faces few constraints according to Polity IV (x = 3). Outside of this exception, countries are mostly grouped around the line of best fit, such that both measures coincide well.

Another criticism of these variables from Afrobarometer may be that they are perceptions, however other approaches also use perceptions since constraints on the executive is difficult to measure accurately. For example, the Polity IV data is collected from the perception of panels of researchers who code constraints in each country. The organization collecting the Polity IV data notes that measuring constraints on the executive is a very difficult exercise: "of the 148 cases with no regime change compared across time, only 74 resulted in the assignment of identical POLITY codes" (Marshall et al. (2016), p.7). They further note that: "By far, the most common coding discrepancies involved assigning scale values to the executive constraints variables"; ""oscillation" or "fuzziness" can be explained by the fact that executive power often varies from year to year according to variations in political circumstances and dynamics. Gauging the exact degree of constraints on executive power is difficult at any single point in time" (Marshall et al. (2016), p. 7). By the law of large numbers, using a combined total of 182,933 African respondents in the Afrobarometer dataset may be a more appropriate gauge of constraints on the executive.

In fact, perceptions of constraints on the executive by actual respondents may be the relevant dimension to capture since it is a *de facto* measure of constraints. Judicial reforms may be just changes in name, an additional *de jure* not *de facto* constraint. It is important to verify whether these *de jure* constraints actually translate into *de facto* constraints, as experienced by individuals living in countries with judicial reforms. Ultimately, in the theoretical model, it is the perception of quality (p), speed (T) and access (l_p) that matters for the incentives to invest.

The fundamental advantage of the Afrobarometer dataset over the existing approach to measure constraints on the executive is that the data is at the individual level, allowing for a triple difference analysis within countries. For example, there is extremely detailed information on ethnic groups (round 5 alone has information on 533 different ethnic groups). This stands in contrast with other similar surveys which typically do not have data on ethnic groups. For example, the World Values Survey has data on just 5 broad ethnic groups, the Asian Barometer has data on ethnic groups only in the fourth round, and the Arab Barometer, Latinobarometer, and European Social Survey contain no such data at all. Moreover, none of these datasets asks the question used in this paper about the propensity of the president to ignore the courts and laws. Therefore, the nature of these datasets makes it possible to implement the identification strategy proposed in this paper - establishing the political connectedness of each group by merging this data with the EPR dataset described below. The identification strategy proposed in this paper is only applicable in Africa.

2.3. Data on political connectedness of ethnic groups

To measure the political connectedness of each ethnic group, I use the Ethnic Power Relations (EPR) dataset collected by Vogt et al. (2015). In each country, the data is collected from nearly one hundred national and regional experts who are asked to classify ethnic groups according to their degree of access to executive power. Depending on where political power is effectively exercised, this can be the presidency, the cabinet, senior members in the administration of democratic regimes; the army command in military dictatorships; or the ruling party leadership in one-party states. Each country coding is then reviewed by the EPR Management Committee and in region-specific workshops to ensure inter-coder reliability and global consistency in the implementation of the coding rules.

In this dataset, some ethnic groups are classified as either having a monopoly, being dominant, being senior partners or being junior partners.[11] I call these groups the "connected" groups.

Other groups are classified as powerless, discriminated, or irrelevant.[12] I call these groups the "unconnected" groups.

I merge each ethnic group in each round of the Afrobarometer dataset to the corresponding group in the correct year of the EPR data (since the political fortune of each group may vary over time). Both the Afrobarometer and EPR datasets have extremely specific data on ethnic groups. Appendix E describes how the ethnic groups were merged for each country in detail.

In some countries, the merge is simple and neatly produces two groups, the unconnected and the connected. For example, in the case of Kenya, data from EPR indicate that the Kikuyu-Meru-Embu tribe was the senior partner in 2002 while the Kalenjin-Masai-Turkana-Samburu; Kamba; Luhya; Luo; and Mijikenda were junior partners. These tribes are thus classified as the "Connected" group. The Kisii were powerless and the Somali irrelevant so they are classified as the "Unconnected" group.

In other countries, it is not possible to create these groups in such a straightforward manner. Consider the example of Benin: according to the EPR data, there is only one powerless group in 2005 (the "South/Central (Fon)") which then becomes a junior partner in 2008. All other groups are either senior or junior partners, hence the absence of an unconnected group after 2008. Instead of excluding Benin from the analysis, one solution would be to define connectedness based on the 2005 baseline data and identify the "South Central (Fon)" as a powerless group for the entire period. Since the case of Benin is not unique in presenting such a challenge, I also explain the corrections that can be done to include Burkina Faso, Cameroon, Ghana, Liberia, Malawi, Namibia, Niger, Senegal, Togo, Zambia, and Zimbabwe in Appendix E. The proportion of the sample that is "unconnected" is 9 percent according to the strict implementation of the identification strategy

[11] The exact definitions for these categories are: Monopoly: Elite members hold monopoly power in the executive to the exclusion of members of all other ethnic groups. Dominance: Elite members of the group hold dominant power in the executive but there is some limited inclusion of "token" members of other groups who however do not have real influence on decision making. Senior Partner: Representatives of the group participate as senior partners in a formal or informal power-sharing arrangement. Power sharing means any arrangement that divides executive power among leaders who claim to represent particular ethnic groups and who have real influence on political decision making. Junior Partner: Representatives participate as junior partners in government, measured by the number and importance of the positions controlled by group members.

[12] The exact definitions for these categories are: Powerless: Elite representatives hold no political power (or do not have influence on decision making) at the national level of executive power - although without being explicitly discriminated against. Discriminated: Group members are subjected to active, intentional, and targeted discrimination by the state, with the intent of excluding them from political power. Such active discrimination can be either formal or informal, but always refers to the domain of public politics (excluding discrimination in the socio-economic sphere).

(which excludes these countries) and 17 percent with this extended definition of connectedness. In the empirical analysis, I show results with the strict and extended definition of connectedness. They do not vary much.

3. Empirical strategy

3.1. Endogeneity of judicial reforms

Judicial reforms funded by foreign aid are clearly endogenously placed. Legal scholars argue that international organizations, such as the World Bank and USAID, work with "reform-minded and active leadership" who support the reforms (p.183, Humphreys (2010)). This creates an econometric issue: if this reform-minded leadership simultaneously implements other positive reforms, then the analysis may overestimate the impact of judicial reforms. The bias could also go the other way: if the World Bank and USAID implement judicial reforms where they are most needed, i.e., in poor countries with declining economies and political institutions, then the analysis would underestimate their effect.

A solution to isolate the impact of judicial reforms from these global trends is to undertake a within country analysis. If some groups within countries are more or less affected by judicial reforms, then one can compare: 1) the groups more or less affected by the reforms, 2) in countries with or without judicial reforms, 3) before and after these reforms. The intuition for the analysis is that the difference-in-differences for groups more affected by the reforms is purged from the country trend by differencing it with the other difference-in-differences for groups less affected by the reforms.

The advantage of this triple difference analysis is that the identification assumption is weaker than with a difference-in-differences analysis. A difference-in-differences requires the "common time trend" assumption, i.e., treated countries are on the same time trend as control countries had they not been treated (which is unlikely to be the case here since judicial reforms are endogenously placed). In contrast, the identification assumption in a triple difference is that, absent reforms, the difference-in-differences for groups more affected would be the same as the one for groups less affected, both of them equal to the country trend, a weaker assumption. In a triple difference analysis, the remaining threat to identification comes from other events implemented at exactly the same time as these judicial reforms and that would also disproportionately affect groups more affected by judicial reforms, a threat which I come back to later.

In this paper, I use the unconnected groups as the groups that would be more affected by judicial reforms, as explained in Section 1.3 and as defined in Section 2.3.

3.2. Empirical specification

To test the proposition that judicial reforms place a constraint on the executive, I estimate the following specification:

$$\begin{aligned}\text{ConstraintsExecutive}_{igjt} = {} & \beta_1 \text{JudicialReform}_{jt} + \\ & \beta_2 \text{JudicialReform}_{jt} \times \textit{Unconnected}_{gt} + \beta_3 \textit{Unconnected}_{gt} + \\ & \beta_4 \text{JudicialReformLongTerm}_{jt} + \beta_5 \text{JudicialReformLongTerm} \times \textit{Unconnected}_{gt} + \\ & \beta_6 \text{JudicialReformPre}_{jt} + \beta_7 \text{JudicialReformPre} \times \textit{Unconnected}_{gt} + \\ & + X_{igjt}\gamma + \alpha_j + \gamma_t + \varepsilon_{igjt} \qquad (1)\end{aligned}$$

where i is for individual i in ethnic group g in country j at time t. $\text{ConstraintsExecutive}_{igjt}$ is a measure of constraints on the executive, collected at the individual level. For example, the first measure is the propensity of the President to ignore the courts and laws of the country, according to individual i. It is a dichotomous variable equal to 1 if the president never ignores the courts and laws, 0 otherwise (i.e., if the president ignores the courts and laws always, often, or rarely).

$\text{JudicialReform}_{jt}$ is a dichotomous variable equal to 1 if country j experienced a comprehensive judicial reform in between waves $t-1$ and t of the Afrobarometer dataset, and 0 otherwise. This variable thus measures the short-run effects of reforms, the effect on the "switchers", the countries switching treatment status at time t.

To measure the long-term effects and test whether the effects of judicial reforms are sustained even after these reforms end, I define a variable $\text{JudicialReformLongTerm}_{jt}$ as a dichotomous variable equal to 1 if country j experienced a comprehensive judicial reform in between waves $t-2$ and $t-1$ of the Afrobarometer dataset, and 0 otherwise. This coefficient thus measures the effect of the judicial reform in the next period after it was implemented. In a robustness check presented in the Appendix, I also define a variable $\text{JudicialReformVeryLongTerm}_{jt}$ as a dichotomous variable always equal to 1 *forever* after country j experienced a comprehensive judicial reform in between waves $t-2$ and $t-1$ of the Afrobarometer dataset, and 0 otherwise. Thus, $\text{JudicialReform}_{jt}$ measures the instantaneous effects, and $\text{JudicialReformVeryLongTerm}_{jt}$ measures the effects in the very long-run.

$\textit{Unconnected}_{gt}$ is the distance to the executive of ethnic group g at time t. It is a dichotomous variable equal to 1 if the ethnic group is powerless, discriminated, or irrelevant according to the EPR dataset; and 0 if it has a monopoly, is dominant, or is a senior or junior partner. Interacting $\textit{Unconnected}_{gt}$ with $\text{JudicialReform}_{kt}$ allows for a triple difference analysis. The main hypothesis tested in this paper is that β_2 is positive, i.e., judicial reforms increase the likelihood that the president will not ignore the courts and laws according to the unconnected groups. I also interact $\textit{Unconnected}_{gt}$ with $\text{JudicialReformLongTerm}_{jt}$ to look at the long-term effects.

To check for common pre-trends, I define $\text{JudicialReformPre}_{jt}$ as a dichotomous variable equal to 1 if the country experienced a judicial reform in between waves t and $t+1$ in country j. Thus, at time t when the outcome is measured, there has been no judicial reform yet. This coefficient measures the effect of the judicial reform before the reform has started. I also interact this variable with $\textit{Unconnected}_{gt}$ to form $\text{JudicialReformPre} \times \textit{Unconnected}_{gt}$, which measures the pre-trend for the unconnected groups. If β_5 is not significantly different from zero, this will indicate that unconnected groups in treated and control countries were on the same trend before judicial reforms were implemented.

α_j are country fixed effects, and γ_t are year fixed effects. InX_{igjt}, I control for basic demographics (gender, age, and education) interacted with $\text{JudicialReform}_{jt}$, $\text{JudicialReformLongTerm}_{jt}$ and $\text{JudicialReformPre}_{jt}$.

The remaining threats to identification in this triple difference analysis are shocks or policies implemented at exactly the same time as these judicial reforms, that may also disproportionately benefit unconnected ethnic groups (or hurt connected groups). To address this issue, I use the same IATI dataset to define indicators for spending on all other types of foreign aid projects: education, health, transport, energy, support to the private sector, support to the executive, emergency, banks, budget support, food, environment, tax, conflict, debt, trade, democracy, human rights, the media, anti-corruption organizations, communication, the parliament; as well as their interaction with $\textit{Unconnected}_{gt}$, for a total of 42 control variables in X_{igjt}.

Standard errors are robust, clustered at the level of countries.

4. Results

4.1. Effects on judicial quality

Before looking at constraints on the executive, I first verify that judicial reforms have an effect on perceptions of judicial quality. In Column (1) of Table 2, the dependent variable is the answer to the question: "How much do you trust Courts of law?" (0 = Not at all, 1 = Just a little, 2 = Somewhat, 3 = A lot). I find that the interaction term between judicial reforms and unconnected ethnic groups is positive and significant. Thus, judicial reforms improve trust in courts, especially for the ethnic groups that are not connected to the executive. The magnitude of this effect is large: the coefficient of the interaction term $\text{JudicialReform}_{jt} \times \textit{Unconnected}_{gt}$ is 0.29, compared to the average trust in courts of 1.7 on a scale of 0 to 3 (SD = 1). Another interesting finding is that the coefficient of the variable $\textit{Unconnected}_{gt}$ is negative (-0.13), indicating that in the absence of any judicial reforms, unconnected groups have a lower trust in courts. Judicial reforms change this by improving the trust in courts for these unconnected groups. In fact, the coefficient of $\text{JudicialReform}_{jt}$ is not significantly different from zero, which may be explained by the fact that connected groups already have access to courts that protect them. This is in line with Sanchez De La Sierra (2018) who finds that groups connected to the executive are able to use contracts to enforce payment since they can credibly threaten to litigate in case of default.

Concerning the long-term effects, the coefficient of $\text{JudicialReformLongTerm} \times \textit{Unconnected}_{gt}$ is not significant. Table C1 in the Appendix also shows no long-run effects with a step variable equal to 1 forever after the initial period of a judicial reform.

One explanation for the lack of long-term effects is given by the theoretical model: in the absence of external judicial reforms, the ruler has no incentives to provide a fully functioning-judiciary, because it constrains his actions. In the parlance of the model, $(p\beta^T)^R < (p\beta^T)^*$, which means that the state of the judiciary chosen by a ruler is always less than the one that maximizes the economy's output. Thus, when judicial reforms end, the system converges back to a suboptimal judiciary.

To further understand that judicial reforms may not have long-lasting effects, one may look at the JPIP reform in Kenya. This reform mostly consisted of: building courts, introduction of performance management, training of personnel, and administrative reform. The construction of new courts may be seen as long-lasting. Yet, buildings need maintenance, and courts may deteriorate if not properly maintained. In fact, most of the "new" courts in this reform in Kenya were in fact renovations of previously existing courts that fell in a state of disrepair. This illustrates the point that even physical buildings can depreciate if not well maintained by a ruler that does not value the judiciary, as per the model. The second component of the reform (performance management) also demands constant investment. There is no guarantee that once the donors leave, these performance contracts will be continued, since they require extra work (judges must sign contracts, and data must be analyzed) and judges may be averse to them. Absent the pressure from the World Bank, it is unclear whether these initiatives would continue. The third component (backlog reduction) may be entirely temporary. Financial support to resolve older cases may reduce the backlog, but the backlog may come back once these initiatives stop. Training of people may depreciate over time, some employees may leave (due to retirement), new employees without the training come in, such that it is unclear whether the effect of these trainings continue in the long-run. The administrative reforms, instead of being improved over time due to feedback from judges and court officials, may be abandoned once the donors leave.

Table 2
Effects of Judicial Reforms on Perceptions of Courts.

	(1) Trust Courts	(2) Corrupt Judges
Judicial Reform ∗ Unconnected	0.29***	−0.14**
	(0.07)	(0.06)
Judicial Reform	−0.14	−0.04
	(0.10)	(0.08)
Unconnected	−0.13*	0.06
	(0.07)	(0.06)
Judicial Reform ∗ Long Term ∗ Unconnected	0.16	−0.04
	(0.13)	(0.08)
Judicial Reform ∗ Long Term	−0.25*	−0.02
	(0.14)	(0.08)
Judicial Reform ∗ Pre ∗ Unconnected	−0.14	−0.12
	(0.09)	(0.08)
Judicial Reform ∗ Pre	−0.24	0.12
	(0.15)	(0.08)
Observations	117,022	106,768
Country FE	YES	YES
Year FE	YES	YES
Mean Dep. Var.	1.722	1.300
(SD)	1.045	0.867

Robust standard errors, clustered at the level of the country. *** Significant at 99 percent confidence-interval, ** Significant at 95 percent confidence-interval, * Significant at 90 percent. In Column (1), the dependent variable is answers to the question: "How much do you trust Courts of law?" (0 = Not at all, 1 = Just a little, 2 = Somewhat, 3 = A lot). The variable "Judicial Reform" is a dichotomous variable equal to 1 if the country experienced a comprehensive judicial reform in between waves $t-1$ and t of the Afrobarometer dataset, and 0 otherwise. The variable "Judicial Reform ∗ Long" as a dichotomous variable equal to 1 if country j experienced a comprehensive judicial reform in between waves $t-2$ and $t-1$ of the Afrobarometer dataset, and 0 otherwise. The variable "Unconnected" is a dichotomous variable equal to 1 if the ethnic group is powerless, discriminated, or irrelevant, 0 if the ethnic group has a monopoly, or is dominant, a senior or junior partner. The variable "Judicial Reform∗Unconnected" is the interaction between the two variables. The variable "Judicial Reform∗Long Term∗Unconnected" is the interaction between the two variables. The variable "Judicial Reform∗Pre" is a dichotomous variable equal to 1 if the country experienced a judicial reform in between waves t and $t+1$ in country j. The variable "Judicial Reform∗Pre∗Unconnected" is the interaction between "Judicial Reform∗Pre" and "Unconnected". All regressions include country and year fixed effects. In Column (2), the dependent variable is answers to the question: "How many of judges and magistrates do you think are involved in corruption?" (0 = None, 1 = Some of them, 2 = Most of them, 3 = All of them).

This is the conclusion I find in the data: I find no long-term effects of these reforms. This shows that supporting judiciaries is a constant, not a one-time, effort. The reason is that strong executives have incentives to undermine judiciaries and discontinue donors' effort.

To check for common pre-trends, one can look at the coefficients of $\text{JudicialReformPre}_{jt}$, a dichotomous variable equal to 1 if the country experienced a judicial reform in the next wave but not in the current one or any previous period, and its interaction with the unconnected groups $\text{JudicialReformPre} \times \textit{Unconnected}_{gt}$. As Column (1) shows, the coefficient of $\text{JudicialReformPre}_{jt}$, is not significantly different from zero, which indicates that in the periods just before judicial reforms were implemented, the treated and control countries were on a similar time trend. In particular, it is not the case that treated countries were already on a better trend before the judicial reforms. The coefficient of $\text{JudicialReformPre} \times \textit{Unconnected}_{gt}$ is not significantly different from zero, confirming that the unconnected groups were not on different trends before the reforms.

The results from Column (1) are confirmed by the second indicator relating to courts in the Afrobarometer dataset, shown in Column (2). The question here is: "How many of judges and magistrates do you think are involved in corruption? (0 = None, 1 = Some of them, 2 = Most of them, 3 = All of them) and judicial reforms are shown to lower this perception for unconnected

groups. Overall, Table 2 demonstrates that judicial reforms have a disproportionate effect on ethnic groups that are not connected to the executive, a fact consistent with the idea that they expand the reach of the rule of law to groups that had previously been excluded.

A concern with these results is that they are perceptions of judicial quality, not actual judicial outcomes. Unfortunately, there is no data in the Afrobarometer on actual experience in courts, except in round 6, which precludes a panel data analysis. Yet, I find in Appendix D that the two measures used above (trust in courts and perception of corruption of judges) are correlated with the experience of people who are actually using the courts.

In Appendix D, I use two other sources of data to show that perception are correlated with actual judicial outcomes, one from rural Kenya (Aberra and Chemin, 2020) and one from Pakistan (Acemoglu et al., 2020). These tests show that perceptions of courts matter because they are correlated with actual experience in courts and the actual use of courts. Thus perceptions matter. If citizens start perceiving that the courts might treat them fairly despite them being unconnected, that is an absolutely crucial first step. Of course, in the future these perceptions and expectations might turn out to be incorrect, but they have to be fundamental building blocks of an unbiased legal system.

Concluding this section, the effects found in Table 2 correspond in the theoretical model to an improvement in the quality $p\beta^T$ of the courts (where p is the probability of winning, β is the discount factor, and T is the time it takes to resolve cases, such that $p\beta^T$ is the fraction of the original claim that the producer can recover in case of expropriation by the ruler). An increase in $p\beta^T$ imposes a direct constraint on the ruler since it increases the compensation to be paid to the producer. We verify in the next section whether people perceive a greater constraint on the ruler after judicial reforms, especially for the unconnected groups that benefit the most from these reforms.

4.2. Effects on constraints on the executive

Before turning to regression results, I show the raw data for each of the 36 countries. In the case of Kenya, as explained in Table 1, there were comprehensive judicial reforms in the periods 2005–2008 and 2008–2011. This is represented by a grey shaded area in Fig. 2.

Fig. 2 shows the first variable measuring constraints on the executive, i.e., the proportion of the population that says that the president will never ignore the courts and laws. In 2003, the baseline period, approximately 20 percent of respondents consider this to be true. In the absence of any judicial reforms (in the period from 2003–2005) both the connected and unconnected groups follow a similar path (with connected and unconnected groups defined as explained in Section 2.3). This changes in the period 2005–2008: a comprehensive judicial reform is implemented, and the unconnected group follows a more positive evolution than the connected. The back of the envelope difference-in-differences in 2005–2008 yields a 7 percentage point increase for the unconnected group (i.e., (23–29) for the unconnected group - (19–32) for the connected group). This encapsulates the basic identification strategy followed in this paper: in the absence of judicial reforms, trends are similar, whereas after a judicial reform, the unconnected group evolves in a more positive manner.

The rest of the graph is also in accord with the hypothesis of the paper. In the period 2008–2011 without a judicial reform, the evolution for the unconnected group is worse than that for the connected group. This is consistent with a view that the effects of the judicial reform of the previous period are short-lived, and dissipate once funding is discontinued. This can actually be seen from the theoretical model: in the absence of any judicial reforms, the optimal level of efficiency of the judiciary is $(p\beta^T)^R$; thus, the judicial system reverts back to this level in the absence of external intervention.

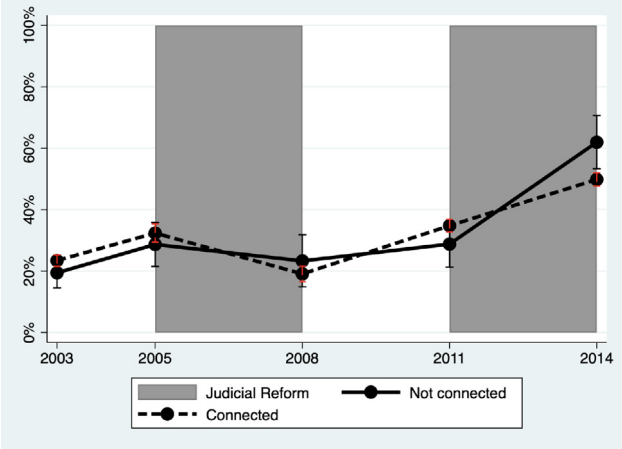

Fig. 2. Judicial Reforms and Constraints on the Executive - Kenya. *Note:* The variable on the y-axis is the proportion of the sample answering that the president never ignores the courts and laws (versus the president ignores the courts and laws always, often, or rarely).

In contrast, for the period 2011–2014 which corresponds to the start of the $120 Million JPIP project used as a running example in this paper, the difference-in-differences is + 18 percentage points (i.e., (62–29) for the unconnected group - (50–35) for the connected group).

Thus, for the case of Kenya, a visual inspection of the data from 2003–2014 lends support to the theory presented in Section 1. On a quantitative level, the estimated impact of reform is between 7 and 18 percentage points.

Appendix E repeats the same analysis for the 36 countries of the Afrobarometer, presenting the periods of judicial reform and the evolution of perceptions for the unconnected and connected groups over time. These graphs show that the theoretical predictions of this paper are borne out in 75 percent of the country-periods available in the dataset, i.e., judicial reforms improve peoples' perceptions that the president will never ignore the courts, especially among the unconnected ethnic groups.

Table 3 below presents the regression results. Out of simplicity, this table only shows the main coefficient of interest $\text{JudicialReform}_{jt} \times \textit{Unconnected}_{gt}$, however the results are exactly the same with long-term effects or with very long-term effects, and with or without control variables as shown in Table C1 in the Appendix.

In Column (1), the dependent variable is the dichotomous variable equal to 1 if the president never ignores the courts and laws, 0 otherwise. Column (1) shows that the coefficient of $\text{JudicialReform}_{jt} \times \textit{Unconnected}_{gt}$ is 10 percentage points and significant. This indicates that judicial reforms increase the unconnected group's perception that the president will never ignore the courts and laws by 10 percentage points. In other words, judicial reforms place a constraint on executive power, according to the unconnected. The effect of judicial reforms on connected groups (i.e., the coefficient of $\text{JudicialReform}_{jt}$) is much lower, 3 percentage points and not significant. In the theoretical model, this is explained by the fact that individuals connected to executive power have greater access to the courts and face less discrimination; thus judicial reforms have less of an effect on them.

Table 3
Effects of Judicial Reforms on Constraints on the Executive.

	(1) President Never Ignores Courts	(2) Officials Unpunished	(3) Corrupt President	(4) Corrupt Officials
Judicial Reform	0.10**	−0.40***	−0.20**	−0.19***
∗ Unconnected	(0.05)	(0.05)	(0.09)	(0.06)
Judicial Reform	0.03	0.11**	−0.09	−0.06
	(0.05)	(0.05)	(0.08)	(0.04)
Unconnected	−0.11**	0.07	0.23***	0.12*
	(0.04)	(0.04)	(0.08)	(0.06)
Judicial Reform	−0.05	0.05	0.20***	0.07
∗ Pre	(0.05)	(0.12)	(0.06)	(0.06)
Judicial Reform ∗ Pre	0.01	−0.09	−0.14	−0.11
∗ Unconnected	(0.06)	(0.20)	(0.10)	(0.07)
Observations	103,634	101,032	104,184	112,651
Country FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Mean Dep. Var.	0.412	1.604	1.227	1.381
(SD)	0.492	1.044	0.896	0.773

Robust standard errors, clustered at the level of the country. *** Significant at 99 percent confidence-interval, ** Significant at 95 percent confidence-interval, * Significant at 90 percent. In Column (1), the dependent variable the dependent variable is a dichotomous variable equal to 1 if the president never ignores the courts and laws, 0 otherwise (i.e., if the president ignores the courts and laws always, often, or rarely). The variable "Judicial Reform" is a dichotomous variable equal to 1 if the country experienced a comprehensive judicial reform in between waves $t-1$ and t of the Afrobarometer dataset, and 0 otherwise. The variable "Unconnected" is a dichotomous variable equal to 1 if the ethnic group is powerless, discriminated, or irrelevant, 0 if the ethnic group has a monopoly, or is dominant, a senior or junior partner. The variable "Judicial Reform∗Unconnected" is the interaction between the two variables. The variable "Judicial Reform∗Pre" is a dichotomous variable equal to 1 if the country experienced a judicial reform in between waves t and $t+1$ in country j. The variable "Judicial Reform∗Pre∗Unconnected" is the interaction between "Judicial Reform∗Pre" and "Unconnected". All regressions include country and year fixed effects. In Column (2), the dependent variable is answers to the question "In your opinion, how often do officials who commit crimes go unpunished?" (0 = Never, 1 = Rarely, 2 = Often, 3 = Always). In Column (3), the dependent variable is answers to the question "How many of the President/Prime Minister and Officials in his Office do you think are involved in corruption? (0 = None, 1 = Some of them, 2 = Most of them, 3 = All of them). In Column (4), the dependent variable is answers to the question: "How many of the government officials and local government councilors do you think are involved in corruption?" (0 = None, 1 = Some of them, 2 = Most of them, 3 = All of them).

An increase of 10 percentage points over a baseline level of 41 percent means a 24 percent change, or an effect size of 0.2 standard deviations.[13] It is a large effect since it roughly corresponds to a 2 point (or 1 standard deviation) increase on the seven-point Polity IV scale.[14] This effect is also equal in absolute value to the coefficient of $Unconnected_{gt}$ (-11 percentage points). This shows that unconnected groups have a worse perception of their president than connected groups at baseline and judicial reforms essentially close that gap, making unconnected groups similar to connected groups (at least for this variable). In other words, judicial reforms extend the same protection that connected groups have to a broader cross-section of society.

Finally, to check for common pre-trends, one can look at the coefficient of $\text{JudicialReformPre}_{jt}$, a dichotomous variable equal to 1 if the country experienced a judicial reform in the next wave but not in the current one or any previous period. Thus, it is equal to 1 in Kenya for 2003–2005 (but not for 2008–2011 since the country has experienced a reform in 2005–2008). As Column (1) shows, this coefficient is not significantly different from zero, which indicates that in the periods just before judicial reforms were implemented, the treated and control countries were on a similar time trend. The coefficient of $\text{JudicialReformPre} \times Unconnected_{gt}$ is also not significantly different from zero.

The rest of Table 3 shows results for the three other variables in the Afrobarometer dataset that relate to the notion of constraints on the executive. In Column (2), the dependent variable is: "In your opinion, how often do officials who commit crimes go unpunished?" (0 = Never, 1 = Rarely, 2 = Often, 3 = Always). The interaction term $\text{JudicialReform}_{jt} \times Unconnected_{gt}$ is negative, indicating that judicial reforms decrease the perception held by unconnected groups that officials who commit crimes go unpunished. In other words, officials are now constrained by better-functioning judiciaries.

Columns (3) and (4) present the two variables related to corruption of the president and government officials. The coefficients indicate that perceived corruption decrease during the periods when judicial reforms are implemented. Thus, corruption is curbed by these judicial reforms.

Looking at Column (2), the coefficient of $\text{JudicialReform}_{jt}$, which relate to the connected groups, is significant and of the opposite sign: judicial reforms have much less effect on the groups already connected to the executive. This reinforces the notion that judicial reforms expand the rule of law to new groups that were previously underserved. This is further confirmed by the coefficients associated with the explanatory variable $Unconnected_{gt}$, which are of the opposite sign than $\text{JudicialReform}_{jt} \times Unconnected_{gt}$: in the absence of judicial reforms, unconnected groups perceive more corruption by the executive, a perception that is attenuated by the implementation of reforms.

Overall, this table shows that judicial reforms increase constraints on the executive, making it harder for the president and government officials to escape punishment. Interestingly, the trends of the unconnected in the treated and control countries prior to these reforms were similar: the coefficients of $\text{JudicialReformPre} \times Unconnected_{gt}$ are not significant in all columns. These facts indicate that the statistical significance of the impact of the reforms is not an artifact of previous dynamics that would have continued regardless of the interventions. Rather, it is evidence of a break with past trends that was directly following the reforms.

4.3. Effects on economic conditions

The theoretical model shows that greater constraints on the executive will benefit the producer-citizens by reducing the expropriation risk they face. Table 4 below shows the effects of judicial

[13] Since an effect of 0.1 divided by the standard deviation (0.49) is equal to 0.2. The effect size is 0.23 standard deviations when considering the finer (0 to 3) scale for this dependent variable where 0 = Always, 1 = Often, 2 = Rarely, 3 = Never.

[14] As can be seen in Fig. 1: an increase of 10 percentage points in the propensity of the president to never ignore the courts and laws corresponds to a 2 point increase on the Polity IV scale.

Table 4
Effects of Judicial Reforms on Economic Outcomes.

	(1) Improvement in Economic Conditions	(2) Future Economic Conditions	(3) Satisfied with Democracy
Judicial Reform ∗ Unconnected	0.20**	0.16	0.17*
	(0.08)	(0.10)	(0.09)
Judicial Reform	0.01	−0.01	−0.09
	(0.14)	(0.18)	(0.07)
Unconnected	−0.18**	−0.19*	−0.14**
	(0.09)	(0.11)	(0.07)
Judicial Reform ∗ Pre	−0.05	−0.03	−0.22**
	(0.16)	(0.24)	(0.08)
Judicial Reform ∗ Pre ∗ Unconnected	−0.08	−0.08	0.03
	(0.12)	(0.20)	(0.09)
Observations	121,342	108,766	113,352
Country FE	YES	YES	YES
Year FE	YES	YES	YES
Mean Dep. Var.	2.860	3.401	2.478
(SD)	1.094	1.224	1.041

Robust standard errors, clustered at the level of the country. ∗∗∗ Significant at 99 percent confidence-interval, ∗∗ Significant at 95 percent confidence-interval, ∗ Significant at 90 percent. In Column (1), the dependent variable is the answer to the question "Looking back, how do you rate the economic conditions in this country compared to twelve months ago?" (1 = Much worse, 2 = Worse, 3 = Same, 4 = Better, 5 = Much better). In Column (2), the dependent variable is the answer to the question "Looking ahead, do you expect the economic condition in 12 months time to be better or worse?" (1 = Much worse, 2 = Worse, 3 = Same, 4 = Better, 5 = Much better). In Column (3), the dependent variable is the answer to the question "Overall, how satisfied are you with the way democracy works in the country?" (0 = the country is not a democracy, 1 = Not at all satisfied, 2 = Not very satisfied, 3 = Fairly satisfied, 4 = Very satisfied). The variable "Judicial Reform" is a dichotomous variable equal to 1 if the country experienced a comprehensive judicial reform in between waves $t-1$ and t of the Afrobarometer dataset, and 0 otherwise. The variable "Unconnected" is a dichotomous variable equal to 1 if the ethnic group is powerless, discriminated, or irrelevant, 0 if the ethnic group has a monopoly, or is dominant, a senior or junior partner. The variable "Judicial Reform∗Unconnected" is the interaction between the two variables. The variable "Judicial Reform∗Pre" is a dichotomous variable equal to 1 if the country experienced a judicial reform in between waves t and $t+1$ in country j. The variable "Judicial Reform∗Pre∗Unconnected" is the interaction between "Judicial Reform∗Pre" and "Unconnected". All regressions include country and year fixed effects.

reforms on economic outcomes. Column (1) uses the question: "Looking back, how do you rate the economic conditions in this country compared to twelve months ago? (1 = Much worse, 2 = Worse, 3 = Same, 4 = Better, 5 = Much better). Judicial reforms improve perceived economic conditions as is visible in Column (1), especially for groups unconnected to the executive. Groups that are connected to the executive benefit less since they already face better economic conditions than unconnected groups, as evidenced by the negative coefficient in front of $Unconnected_{gt}$.

Column (2) confirms these findings with a question in the Afrobarometer related to economic conditions: "Looking ahead, do you expect the economic condition in 12 months time to be better or worse?" (1 = Much worse, 2 = Worse, 3 = Same, 4 = Better, 5 = Much better). Once again, judicial reforms improve the unconnected group's perception of future economic conditions, albeit not significantly so.

Overall, the evidence suggests that judicial reforms improve the functioning of courts which then discipline rulers into using their office less for private gain, and more for the common good. Another way to see this is Column (3), where the dependent variable is: "Overall, how satisfied are you with the way democracy works in the country?" (0 = the country is not a democracy, 1 = Not at all satisfied, 2 = Not very satisfied, 3 = Fairly satisfied, 4 = Very satisfied". Judicial reforms improve satisfaction with democracy, reinforcing the fact that unbiased rule of law is an important component of a working democracy.

5. Robustness checks

5.1. Effects of other reforms

Judicial reforms funded by foreign aid may come together with other projects that could also place constraints on the executive and disproportionately favor the unconnected groups. I address this concern by using the IATI dataset to develop new indicators that measure total spending on all other projects financed by foreign aid in a given country. Before turning to regression results, I describe the overall pattern of foreign aid spending when disaggregated by the nature of the project.

Fig. 3 shows total spending on the judiciary for all countries (the black line). In the IATI dataset, this is identified by code 15130 (Legal and judicial development: support to institutions, systems and procedures of the justice sector) and code 15134 (Judicial affairs: civil and criminal law courts and the judicial system). The full list of codes and description of all foreign aid projects is available in Table F1.

Fig. 3 shows that spending on the judiciary is very low until 2005 and increasing thereafter; however, this may simply reflect the fact that the IATI organization started in 2008 and did not impose its current standard for collecting data before that.[15] The interesting comparison is between the judiciary and all other projects after 2005.

Fig. 3 shows spending on all projects that support counterpowers to the executive (other than the judiciary) which may thus also be building constraints on the executive. The red line represents projects supporting a more vibrant civil society and more transparent elections.[16] Total spending on these "Democracy" projects has been increasing since 2005, and has actually overtaken spending on the judiciary since 2014. It is thus important to control for them in the regressions because their effects may be confounded with the effects of judicial reforms.

The other lines represent spending on: a free press, human rights, anti-corruption organizations, the parliament, and improvement in communication technologies (such as the internet, which may foster social unrest and uprisings). Fig. 3 shows that support to human rights and communication projects have overtaken spending on the judiciary in recent years, while other sectors such as support for anti-corruption organizations, parliament, and the media (close to zero and hidden by spending on parliament) received much less funding.

To control for the influence of these other projects, I include the variable $Democracy_{jt}$ in the main specification which is equal to the spending on democracy projects in country j in between waves $t-1$ and t of the Afrobarometer dataset. I also interact $Democracy_{jt}$ with $Unconnected_{gt}$ to capture the differential effect on unconnected groups. The results are displayed in Table 5. Column (1) replicates the main result of the paper and Column (2) adds the democracy projects. The important result is that the effect of judicial reforms on the unconnected groups survives the inclusion of this control variable. In fact, the following columns show that the main result of the paper remains significant when one controls for projects on human rights in Column (3), the media in Column

[15] The large spike in 2009 comes from several massive programs from the Asian Development Bank that are not part of the econometric analysis above since the analysis is restricted to Africa.

[16] Which correspond to code 15150: Democratic participation and civil society; code 15151: Electoral management bodies and processes, election observation, voters' education; code 15161: Electoral assistance and monitoring, voters' education).

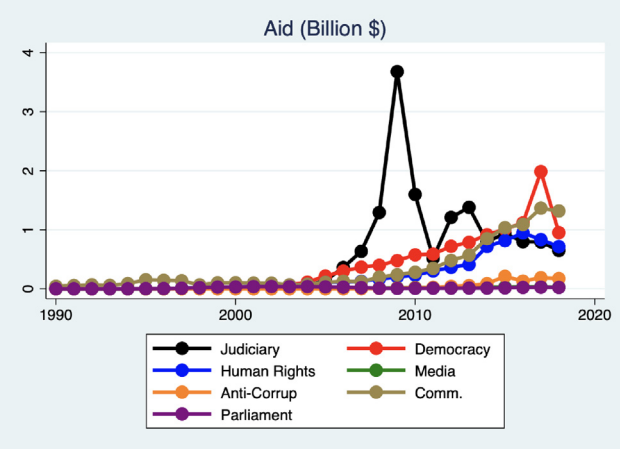

Fig. 3. Judicial Reforms and Other Reforms on Counter-Powers to the Executive.

(4), anti-corruption organizations in Column (5), communication in Column (6), or the parliament in Column (7). Table F3 in the appendix shows the same robustness check for all the other variables considered in this paper. Results remain similar throughout. Overall, the effect of judicial reforms remains robust to controlling for projects supporting other counter-powers to the executive.

I next consider other projects which are more traditionally the focus of foreign aid. Fig. 4 shows that spending on the judiciary is dwarfed by spending on: education, health, transport, energy, support to the private sector, support to the executive, emergency, or banks.

Table 6 controls for the influence of these other projects. Once again, the results show that judicial reforms still have a positive effect on unconnected groups when controlling for projects on education (Column (2)), health (Column (3)), transport (Column (4)), energy (Column (5)), support to the private sector (Column (6)), support to the executive (Column (7)), emergency (Column (8)), or banks (Column (9)). Table F4 in the appendix shows the same result for all the other outcomes considered in this paper.

Fig. F1 in Appendix F shows the remaining sectors with lower spending than on the judiciary (budget support, food, environment, tax, conflict, debt and trade; see the full list and description in Table F2). Table F5 in the appendix shows that the main result of this paper remains significant when one controls for spending on these other projects as well. Table F6 in the appendix shows the same result for the other outcomes considered in this paper.

Finally, Table F7 in Appendix F shows the results when controlling for the 6 variables representing counter-powers (democracy, human rights, the media, anti-corruption organizations, communication, the parliament), the 8 variables for projects that receive more spending than the judiciary (education, health, transport, energy, support to the private sector, support to the executive, emergency, banks) and the 7 variables with lower spending than the judiciary (budget support, food, environment, tax, conflict, debt, trade). Their interaction with $Unconnected_{gt}$ is also included. A total of 42 control variables are included in the regression and remarkably, the results are very similar. In fact, the main coefficient of interest $\text{JudicialReform}_{jt} \times Unconnected_{gt}$ is always of a slightly greater size (in absolute value) with these controls than without.

Overall, these results show that the main finding of this paper, i.e., judicial reforms build constraints on the executive according to the unconnected groups, is not driven by coincidental foreign aid projects.

5.2. Robustness check with other measures of unconnectedness

As explained in Section 2.3, using a strict measure of connectedness and unconnectedness led to several countries being excluded from the sample. Appendix E shows how the definition of unconnectedness can be extended to include more countries in the analysis, and Table 7 presents the regression results when employing this different measure. Column (1) replicates the main finding of the paper with no corrections brought to the unconnectedness variable. Column (2) excludes all problematic countries identified in Appendix E but includes Benin by defining the "South/Central (Fon)" as the unconnected group. The sample size is smaller since all problematic countries are excluded; yet the main result of the paper once again does not change.

In the next columns, I follow the extensions described in Appendix E, including Burkina Faso in Column (3), Cameroon in Column (4), Ghana in Column (5), Liberia in Column (6), Malawi in Column (7), Namibia in Column (8), Niger in Column (9), Senegal in Column (10), Togo in Column (11), Zambia in Column (12), Zimbabwe in Column (13), or all countries according to these extensions in Column (14). The main result of the paper remains similar in all columns. Table G1 in the appendix shows the same result for the other outcomes considered in this paper.

An issue with the findings is that the political connection of groups could change over time, which could affect the results. In practice, however, very few groups change from the connected to the unconnected groups. There are only 5 such shifts, out of 649 ethnic groups/round observations. In Table G2 in the Appendix, I restrict the sample to the other ethnic groups that do not change status over the period, and find very similar results.

In the appendix, I perform an additional robustness check by using a completely different measure of unconnectedness, not from the EPR dataset but taken directly from the Afrobarometer dataset. The dataset contains the question:

> "How often is [Respondent's Ethnic Group] treated unfairly by the government? 0 = Never, 1 = Sometimes, 2 = Often, 3 = Always"

This variable thus represents another potential measure of political connectedness, since groups less politically connected may also be treated more unfairly. As explained in greater detail in Appendix E, I calculate the average answer to this question for each ethnic group in a given country in a given year and use this as a measure of unconnectedness. Table G3 shows that the results are very similar using this completely different methodology.

5.3. Effects of limited judicial reforms

Table H1 in Appendix H looks at the effect of limited judicial reforms. Recall that the main coefficient of interest $\text{JudicialReform}_{jt}$ is a dichotomous variable equal to 1 if country j experienced a comprehensive judicial reform. I introduce a new variable called $LimitedJudicialReform_{jt}$ equal to 1 if country j experienced a limited judicial reform. Recall that judicial reforms are considered limited when they fail to target the three key criteria (access, speed and quality) or when they have combined funds that add up to less than 5 percent of the national judiciary's annual budget. The table shows that the coefficients of $LimitedJudicialReform_{jt}$ are not significantly different from zero. Thus, only comprehensive, not limited, judicial reforms build constraints on the executive. The basic intuition for these results is that increasing only one of the three criteria has little or no effect, e.g., increasing access to an otherwise corrupt judiciary serves no purpose. This has an important policy implication: judicial reforms

Table 5
Robustness Check with Other Reforms on Counter-Powers.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Judicial Reform	0.10**	0.09*	0.10**	0.10*	0.10**	0.11**	0.11**
∗ Unconnected	(0.05)	(0.05)	(0.04)	(0.05)	(0.05)	(0.04)	(0.05)
Judicial Reform	0.03	0.03	0.03	0.04	0.03	0.04	0.03
	(0.05)	(0.04)	(0.05)	(0.05)	(0.04)	(0.05)	(0.05)
Unconnected	−0.11**	−0.11**	−0.10**	−0.10**	−0.11**	−0.10*	−0.12**
	(0.04)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.04)
Judicial Reform ∗ Pre	−0.05	−0.05	−0.05	−0.05	−0.05	−0.04	−0.04
	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
Judicial Reform ∗ Pre	0.01	0.01	0.00	0.00	0.01	−0.01	0.01
∗ Unconnected	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)
Democracy		0.47					
∗ Unconnected		(1.10)					
Democracy		−0.35					
		(0.95)					
Human Rights			−0.15				
∗ Unconnected			(1.26)				
Human Rights			0.62				
			(1.48)				
Media				−10.05			
∗ Unconnected				(11.72)			
Media				17.79			
				(19.32)			
Corruption					6.19		
∗ Unconnected					(8.15)		
Corruption					−1.70		
					(14.12)		
Communication						−0.59	
∗ Unconnected						(1.71)	
Communication						−0.85	
						(1.27)	
Parliament							36.02***
∗ Unconnected							(9.96)
Parliament							−15.32
							(15.40)
Observations	103,634	103,634	103,634	103,634	103,634	103,634	103,634
Country FE	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES

Robust standard errors, clustered at the level of the country. *** Significant at 99 percent confidence-interval, ** Significant at 95 percent confidence-interval, * Significant at 90 percent. In all columns, the dependent variable is a dichotomous variable equal to 1 if the president never ignores the courts and laws, 0 otherwise (i.e., if the president ignores the courts and laws always, often, or rarely). The variable "Judicial Reform" is a dichotomous variable equal to 1 if the country experienced a comprehensive judicial reform in between waves $t-1$ and t of the Afrobarometer dataset, and 0 otherwise. The variable "Unconnected" is a dichotomous variable equal to 1 if the ethnic group is powerless, discriminated, or irrelevant, 0 if the ethnic group has a monopoly, or is dominant, a senior or junior partner. The variable "Judicial Reform∗Unconnected" is the interaction between the two variables. The variable "Judicial Reform∗Pre" is a dichotomous variable equal to 1 if the country experienced a judicial reform in between waves t and $t+1$ in country j. In Column (2), the variable "Democracy" equal to the spending on democracy projects in country j in between waves $t-1$ and t of the Afrobarometer dataset. The variable "Democracy∗Unconnected" is the interaction between the two variables. The next columns include spending on projects on human rights (Column (3)), the media (Column (4)), anti-corruption organizations (Column (5)), communication (Column (6)), or the parliament (Column (7)). All regressions include country and year fixed effects.

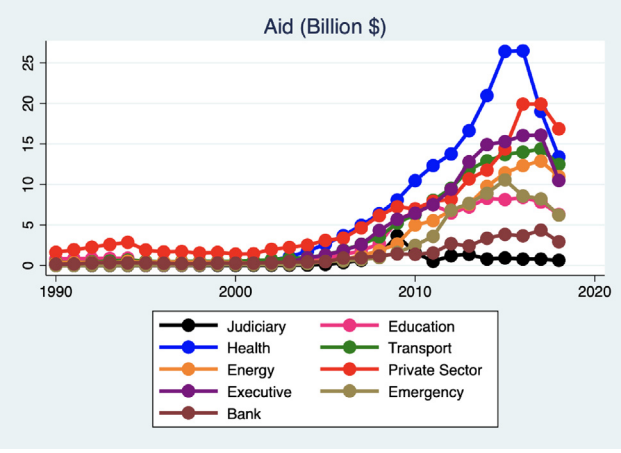

Fig. 4. Judicial reforms and other projects with greater aid spending.

must be comprehensive - tackling all factors at once and of a sufficient budget size - to have an effect.

5.4. Robustness check with other budget thresholds for comprehensive judicial reforms

Table 8 below shows the result of varying the threshold used to define comprehensive reforms. In the main analysis, I use 5 percent of the judiciary's annual budget since it is the average of both World Bank and USAID's judicial reforms. Column (1) of Table 8 replicates the main finding of the paper but in Columns (2) and (3), I introduce a new threshold of 3 and 7 percent respectively. Varying the size of the threshold to define a judicial reform as comprehensive makes little difference to the main result of the paper.

6. Conclusion

Judicial reforms that are externally supported by foreign aid agencies improve the quality of courts, especially according to the powerless and discriminated segments of society. In this sense,

Table 6
Robustness Check with Other Reforms.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Judicial Reform	0.10**	0.11**	0.10**	0.11*	0.10**	0.10**	0.10**	0.10**	0.11**
* Unconnected	(0.05)	(0.05)	(0.04)	(0.05)	(0.05)	(0.05)	(0.04)	(0.05)	(0.04)
Judicial Reform	0.03	0.03	0.03	0.03	0.01	0.03	0.03	0.03	0.04
	(0.05)	(0.05)	(0.05)	(0.05)	(0.04)	(0.05)	(0.05)	(0.05)	(0.05)
Unconnected	−0.11**	−0.10**	−0.11**	−0.10**	−0.14***	−0.11**	−0.10**	−0.11**	−0.14**
	(0.04)	(0.04)	(0.05)	(0.04)	(0.05)	(0.05)	(0.05)	(0.04)	(0.05)
Judicial Reform	−0.05	−0.05	−0.05	−0.04	−0.05	−0.05	−0.05	−0.05	−0.04
* Pre	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
Judicial Reform * Pre	0.01	0.00	0.01	0.00	0.04	0.01	0.01	0.01	−0.02
* Unconnected	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.05)
Education		−0.07							
* Unconnected		(0.09)							
Education		0.01							
		(0.17)							
Health			0.02						
* Unconnected			(0.08)						
Health			0.01						
			(0.09)						
Transport				−0.06					
* Unconnected				(0.22)					
Transport				−0.10					
				(0.13)					
Energy					0.66**				
* Unconnected					(0.27)				
Energy					−0.55**				
					(0.25)				
Private Sector						0.03			
* Unconnected						(0.03)			
Private Sector						−0.01			
						(0.04)			
Executive							−0.02		
* Unconnected							(0.22)		
Executive							−0.01		
							(0.19)		
Emergency								0.04	
* Unconnected								(0.15)	
Emergency								0.04	
								(0.11)	
Bank									2.79**
* Unconnected									(1.32)
Bank									0.28
									(2.26)
Observations	103,634	103,634	103,634	103,634	103,634	103,634	103,634	103,634	103,634
Country FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES

Robust standard errors, clustered at the level of the country. *** Significant at 99 percent confidence-interval, ** Significant at 95 percent confidence-interval, * Significant at 90 percent. In all columns, the dependent variable is a dichotomous variable equal to 1 if the president never ignores the courts and laws, 0 otherwise (i.e., if the president ignores the courts and laws always, often, or rarely). The table controls for projects on education (Column (2)), health (Column (3)), transport (Column (4)), energy (Column (5)), support to the private sector (Column (6)), support to the executive (Column (7)), emergency (Column (8)) or banks (Column (9)). All regressions include country and year fixed effects.

judicial reforms build inclusive institutions by extending the rule of law to previously underserved populations. These segments of society think it is less likely the president will ignore the courts, that officials who commit crimes will go unpunished, and that the president or other government officials will engage in corruption. In other words, previously powerless groups believe some constraints have been placed on the rulers' behavior. In line with a literature arguing that constraints on the executive is key to economic growth, I also find a more positive outlook on economic conditions for these groups.

Despite these positive results, I also show in this paper that judicial reforms are not currently a priority of foreign aid organizations. I find that total spending on such judicial reforms is dwarfed by the more traditional focus of foreign aid efforts like education, health, transport, energy, support to the private sector, support to the executive, emergency, and banks.

My paper thus calls for a global rethinking of foreign aid. One concern with supporting traditional sectors of aid, is that they create a moral hazard issue because self-interested rulers have no incentives to invest in them, knowing full well that they will be financed by foreign organizations. Thus, foreign aid may simply be displacing local spending, possibly into the hands of a corrupt elite. In contrast, there is no such issue for the judiciary - and more generally for counter-powers - since oligarchies have no incentive to finance them considering the constraints they place on their behavior.

Considering the first-order importance of constraints on the executive found in the literature (Acemoglu and Johnson, 2005), foreign aid may be used to finance such counter-powers. My paper finds rigorous evidence in favor of one of these counter-powers: the judiciary. An important avenue of future empirical research is to explore the effect of the other counter-powers identified in this paper (such as support to democracy, human rights, the media, anti-corruption organizations, communication such as the internet, the parliament) and the corresponding constraint on the executive that they may build. Rigorous evidence for these counter-powers may redesign priorities and build a new agenda for foreign aid.

Table 7
Robustness Check with Extended Definition of Unconnectedness.

	(1) Main Result	(2)	(3)	(4)	(5) Including:	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
		Benin	Burkina Faso	Cameroon	Ghana	Liberia	Malawi	Namibia	Niger	Senegal	Togo	Zambia	Zimbabwe	All
Judicial Reform	0.10**	0.13**	0.11**	0.11**	0.09	0.11**	0.13**	0.11*	0.11**	0.11**	0.09	0.08	0.10*	0.08*
∗ Unconnected	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.06)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
Judicial Reform	0.03	0.03	0.03	0.03	0.02	0.02	0.05	0.03	0.03	0.03	0.03	0.03	0.02	0.03
	(0.05)	(0.05)	(0.06)	(0.06)	(0.06)	(0.06)	(0.05)	(0.05)	(0.06)	(0.05)	(0.06)	(0.06)	(0.06)	(0.05)
Unconnected	−0.11**	−0.11**	−0.09*	−0.10**	−0.07	−0.09*	−0.09*	−0.09*	−0.10*	−0.09*	−0.07	−0.07	−0.08**	−0.05*
	(0.04)	(0.04)	(0.05)	(0.04)	(0.04)	(0.04)	(0.05)	(0.04)	(0.05)	(0.05)	(0.05)	(0.05)	(0.04)	(0.03)
Judicial Reform	−0.05	−0.05	−0.05	−0.05	−0.06	−0.05	−0.03	−0.04	−0.05	−0.07	−0.05	−0.02	−0.03	−0.04
∗ Pre	(0.05)	(0.04)	(0.04)	(0.04)	(0.05)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.06)	(0.05)	(0.05)
Judicial Reform	0.01	0.00	−0.02	−0.01	−0.03	−0.02	−0.01	−0.02	−0.01	0.01	−0.03	−0.05	−0.03	−0.04
∗ Pre∗ Unconnected	(0.06)	(0.00)	(0.07)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.07)	(0.06)	(0.07)	(0.07)	(0.06)	(0.04)
Observations	103,634	68,843	67,702	66,753	72,022	68,159	71,259	70,306	66,841	69,658	66,779	69,884	70,154	114,493
Country FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES

Robust standard errors, clustered at the level of the country. ∗∗∗ Significant at 99 percent confidence-interval, ∗∗ Significant at 95 percent confidence-interval, ∗ Significant at 90 percent. In all columns, the dependent variable is a dichotomous variable equal to 1 if the president never ignores the courts and laws, 0 otherwise (i.e., if the president ignores the courts and laws always, often, or rarely). The variable "Judicial Reform" is a dichotomous variable equal to 1 if the country experienced a comprehensive judicial reform in between waves $t-1$ and t of the Afrobarometer dataset, and 0 otherwise. The variable "Unconnected" is a dichotomous variable equal to 1 if the ethnic group is powerless, discriminated, or irrelevant, 0 if the ethnic group has a monopoly, or is dominant, a senior or junior partner. The variable "Judicial Reform∗Unconnected" is the interaction between the two variables. The variable "Judicial Reform∗Pre" is a dichotomous variable equal to 1 if the country experienced a judicial reform in between waves t and $t+1$ in country j. The variable "Judicial Reform ∗Pre∗Unconnected" is the interaction between "Judicial Reform ∗Pre" and "Unconnected". All regressions include country and year fixed effects.

Table 8
Robustness Check with Other Budget Size of Reforms.

	(1)	(2)	(3)
	Budget of Judicial Reform:		
	5 percent	3 percent	7 percent
Judicial Reform	0.10**	0.10*	0.10**
∗ Unconnected	(0.05)	(0.05)	(0.04)
Judicial Reform	0.03	0.05	0.03
	(0.05)	(0.05)	(0.05)
Unconnected	−0.11**	−0.10**	−0.11**
	(0.04)	(0.04)	(0.04)
Judicial Reform	−0.05	−0.04	−0.04
∗ Pre	(0.05)	(0.05)	(0.05)
Judicial Reform ∗ Pre	0.01	0.00	−0.00
∗ Unconnected	(0.06)	(0.05)	(0.06)
Observations	103,634	103,634	103,634
Country FE	YES	YES	YES
Year FE	YES	YES	YES

Robust standard errors, clustered at the level of the country. ∗∗∗ Significant at 99 percent confidence-interval, ∗∗ Significant at 95 percent confidence-interval, ∗ Significant at 90 percent. In all columns, the dependent variable is a dichotomous variable equal to 1 if the president never ignores the courts and laws, 0 otherwise (i.e., if the president ignores the courts and laws always, often, or rarely). In Column (1), the variable "Judicial Reform" is a dichotomous variable equal to 1 if the country experienced a comprehensive judicial reform (a reform with a budget above 5 percent of the national judiciary budget and addressing the three characteristics: quality, speed, and access) in between waves $t-1$ and t of the Afrobarometer dataset, and 0 otherwise. In Column (2), the threshold to define a judicial reform as comprehensive is 3 percent, and in Column (3), 7 percent. The variable "Unconnected" is a dichotomous variable equal to 1 if the ethnic group is powerless, discriminated, or irrelevant, 0 if the ethnic group has a monopoly, or is dominant, a senior or junior partner. The variable "Judicial Reform∗Unconnected" is the interaction between the two variables. The variable "Judicial Reform∗Pre" is a dichotomous variable equal to 1 if the country experienced a judicial reform in between waves t and $t+1$ in country j. All regressions include country and year fixed effects.

Appendix A. Supplementary material

Supplementary data associated with this article can be found, in the online version, at https://doi.org/10.1016/j.jpubeco.2021.104428.

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