JUDICIAL EFFICIENCY AND FIRM PRODUCTIVITY: EVIDENCE FROM A WORLD DATABASE OF JUDICIAL REFORMS

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Abstract—I assemble and classify a database of judicial reforms funded by foreign aid agencies as either comprehensive (targeting all characteristics of quality, speed, access) or limited reform. A triple difference is used to compare firms in countries with or without judicial reforms, before and after reforms, and in sectors more or less reliant on contract enforcement mechanisms, due to their need for relationship-specific investments. I find that externally financed comprehensive judicial reforms improve perceptions of judiciary efficiency (for all firms) and firm productivity (for sectors relying on relationship-specific investments) by 0.15 and 0.09 (22%) standard deviation, respectively.

I. Introduction

THE judiciary, by enforcing contracts and securing property rights, may foster investment and drive economic development (North, 1990; Djankov et al., 2003; Acemoglu & Robinson, 2013). As a result, judicial reforms are often seen as a core component of any development strategy. In this paper, I find that $5.4 billion has been spent by various foreign aid agencies on judicial reforms since 1996. (All monetary amounts are in U.S. dollars.) Yet there has been no rigorous impact evaluation of these costly interventions. Legal and academic scholars lament the lack of causal evidence for judicial reforms, or for the judiciary in general (Trebilcock & Daniels, 2009; Aboal, Noya, & Rius, 2014). Identifying the causal impact of judiciaries on economic development is extremely difficult due to endogeneity issues, such as reverse causality (rich countries can afford better judiciaries) or omitted variable bias (more fundamental unobserved factors may drive both improved judiciaries and economic development).

This is the first paper to systematically evaluate how judicial reforms, funded by foreign aid, affect firm productivity within countries. To identify the causal impact, I use a triple-difference analysis based on the implementation of judicial reforms in some countries and not others, and based on the varying dependence of some sectors on the judiciary for technological reasons. I assemble and codify a world database of 4,568 judicial projects implemented by 500 foreign aid agencies (governments, multilateral agencies, or private sector organizations) since 1996, a project made possible by the recent release of high-quality open data from the International Aid Transparency Initiative (IATI). This delivers a rich data set of projects that vary in budget size, targeted at improving the judiciary's quality, speed, or accessibility. For example, in Kenya, the $120 million World Bank's Judicial Performance Improvement Project (JPIP) provided support to train court officials and streamline procedures (quality), introduce performance management contracts designed to reduce the backlog of cases (speed), and build courts (access). I combine this data set of judicial reforms with the recent release of a second wave of World Bank Enterprise Surveys for 74 countries. These surveys are ideal for studying the impact of judicial reforms because they contain firm-level data on perceptions of judicial efficiency and firm productivity.

Comparing the evolution of firms in countries with or without judicial reforms, in a difference-in-differences framework, may not isolate the causal impact of judicial reforms if countries enacting such reforms are on a different time path from other countries. For example, judicial reforms may be implemented by reform-minded leaders who are likely implementing other reforms positively affecting firm productivity other than through these judicial reforms. A difference-in-differences framework may be capturing a general positive trend and not the effect of judicial reforms per se. To deliver causal estimates, a difference-in-differences framework must assume common time trends—treatment and control countries that would have been on the same trend had there been no reform.

To address this endogeneity issue, I use a well-established insight in economics: some sectors rely more on the judiciary than others because of the need for relationship-specific investments (Klein, Crawford, & Alchian, 1978; Nunn, 2007; Levchenko, 2007; Amirapu, forthcoming). Consider a buyer asking a seller to produce a customized good. Once this good is produced, the buyer can renegotiate prices down since there is no other buyer for this good. Anticipating this, the seller does not enter this relationship. No customized goods are produced, and the economy is trapped in producing generic goods to avoid this issue. One solution to this dilemma is to uphold contracts via third-party enforcement. Thus, sectors that require relationship-specific investments may be particularly dependent on well-functioning judiciaries.

Based on this insight, I then implement a triple-difference analysis and compare firms in countries with or without judicial reforms, before and after the reforms, and in sectors more or less affected by judiciaries. In essence, a triple difference performs two difference-in-differences: one for sectors less dependent on the judiciary and another for sectors more dependent on the judiciary. In this paper, I find that the first difference-in-differences is not significantly different from 0. This indicates that firms in sectors relying less on the judiciary are on a common time trend in all countries. Thus, one can reasonably assume that in the absence of judicial reforms, firms that rely more on the judiciary might also have been on a common time trend. The second difference-in-differences

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thus measures the effect of judicial reforms, not just a general positive trend. The remaining threats to the identification strategy are shocks happening concurrently with judicial reforms and affecting only firms involved in relationship-specific investments. To address this issue, I use the IATI data set to develop novel indicators of all other types of foreign aid projects occurring at the same time, disaggregated by main types (education, health, transport, energy, banking, support to industry, trade, debt, tax capacity, conflict, democratic institutions), to look at whether judicial reforms still have an effect on firms involved in relationship-specific investments once all of these other potential influences are taken into account.

In this paper, I find that judicial reforms funded by foreign aid agencies improve firm perceptions of the judiciary by 0.15 standard deviation for all firms. This translates into a 0.09 standard deviation increase in firm productivity in sectors relying on relationship-specific investments and a positive but insignificant effect on other firms. I find that these effects are driven mainly by comprehensive judicial reforms, defined as reforms that simultaneously target the three characteristics of a well-functioning judiciary (quality, speed, and access) and with a project budget over 5% of the judiciary's annual budget, for a given country—the average budget of World Bank and USAID judicial reforms, the two "largest and most influential rule of law actors" according to legal scholars (Humphreys, 2010, 20). In contrast, I find no effect of more limited reforms, defined as reforms that address only some of the three characteristics or with a small budget. The triple-differences results remain similar when controlling for the overall business climate, the amount of foreign aid received, and other foreign aid projects implemented at the same time. The results remain similar when using various measures of relationship-specific investments in the literature or when using various measures of firm productivity.

These results are important because they show a role for externally funded judicial reforms. In a theoretical context where the state of a judiciary is poor (i.e., difficult to access, slow, and biased), it is unclear whether judicial reforms, regardless of breadth, would work. Limited reforms that target only one characteristic may be even less effective: a reform that would increase access to an already slow and biased judiciary will not fundamentally alter the situation. Moreover, if the fundamental reasons for the poor state of the judiciary are extractive political institutions (Acemoglu & Robinson, 2013; Glaeser, Ponzetto, & Shleifer, 2016)—a small elite holds all the power and has no incentive to build an effective judiciary that would constrain their power—then there should be a low impetus for domestic judicial reforms and increased obstacles to hinder aid agencies from implementing judicial reforms. To investigate this, I assemble a data set of domestic judicial reforms implemented in between the two waves of World Bank Enterprise Surveys for the 74 countries in this study. In line with these predictions, I find no comprehensive domestic judicial reforms over the period of interest and no effect of limited domestic reforms. In contrast, the main finding of this paper is that externally financed comprehensive judicial reforms have an effect, even in countries with poorly functioning judiciaries or oligarchies.

These results are important because they confirm the fundamental importance of the judiciary in the process of economic development. Doubts remain on the benefits brought by the judiciary for various reasons since economic activity can be sustained by multilateral punishment strategies in social networks (Greif, 1993); people can always find alternative arrangements (Acemoglu & Johnson, 2005); informal institutions or social norms could equally foster cooperation and investment; and the judiciary can be subverted by powerful interests (Glaeser et al., 2016). In this paper, I show that the judiciary matters by using country-specific policies that implement institutional change, in line with Pande and Udry (2006). The triple difference also highlights a precise channel through which the judiciary matters: it encourages firms to undertake relationship-specific investments.

This study makes an important contribution to the empirical literature evaluating the impact of judiciaries on economic growth. Greiner and Pattanayak (2012), Aberra and Chemin (2017), and Sandefur and Siddiqi (2013) have used randomized experiments and offered access to the judiciary to a randomized set of individuals (in the form of legal representation in Greiner & Pattanayak, 2012, and Aberra & Chemin, 2017, and legal aid from paralegals in Sandefur & Siddiqi, 2013). Aberra and Chemin (2017) and Sandefur and Siddiqi (2013) find significant effects on effort, investment, and welfare, while Greiner and Pattanayak (2012) find no effect on the case win rate. One issue with these interventions is that if they were scaled up and access to the judiciary was given for free to all, court congestion could result, thereby deteriorating judicial efficiency. Thus, judicial reforms targeting only access, but not speed or quality, may have no effects on a global level. I directly test this by exploiting the richness and variation in the type of judicial reforms implemented. Some past reforms have targeted only one of the three characteristics of judiciaries (quality, speed, or access). I find that such reforms, which I call limited judicial reforms, have no effects on firm productivity at a national level. In contrast, I find that comprehensive judicial reforms—those that tackle quality, speed, and access at once and of significant size—drive the results of this paper.

Other attempts to measure the causal impact of the judiciary on economic activity, while taking into account general equilibrium effects, have been to use judicial reforms as natural experiments. Chemin (2009a, 2009b, 2010), Kondylis and Stein (2017), Lilienfeld-Toal, Mookherjee, and Visaria (2012), and Lichand and Soares (2014) have used judicial reforms implemented in India, Pakistan, Senegal, and Brazil, respectively, in a difference-in-differences framework and found positive effects on investment, access to credit, and growth. In this paper, I focus on a specific channel through which the judiciary matters: firms in sectors that rely more on relationship-specific investments should be more affected by contract enforcement mechanisms and, hence, by judicial

reforms. This insight allows me to use a triple-difference analysis. The advantage of a triple difference is that it controls for the underlying trend occurring in countries with judicial reforms by looking at sectors less reliant on the judiciary, and thus it relies on a weaker version of the common time-effects assumption. This paper also extends the existing literature by using a world database of judicial reforms and the wide diversity of 4,568 judicial projects implemented since 1996. Overall, this paper confirms the findings of previous work: the judiciary has large effects on economic activity, especially for sectors relying on relationship-specific investments.

An identification strategy based on relationship-specific investments has been used to infer the importance of judiciaries for international trade (Nunn, 2007; Levchenko, 2007). Both papers find that the quality of contract enforcement mechanisms is an important source of comparative advantage, more so than skill or capital intensity, thereby reshaping our understanding of the determinants of international trade. These two papers were cross-country, with no variation in the quality of the judiciary over time. This paper provides an alternative interpretation: I use judicial reforms as a shock to judiciaries, which provides time variation in the quality of the judiciary. This allows for the introduction of both country and time fixed effects. Amirapu (forthcoming) combines variation in court efficiency in India with the relationship-specific investments measure of Nunn (2007) and finds a large effect of the judiciary on firm productivity. I confirm and extend these findings by drawing from a source of variation in judicial efficiency: 4,568 judicial projects identified in the International Aid Transparency Initiative (IATI).

This paper is organized in the following way. Section II provides a conceptual framework for the role of judiciaries in the process of economic growth and, in particular, the emergence of relationship-specific investments. This conceptual framework also clarifies the theoretical predictions for the likely effects of comprehensive, limited, and domestic judicial reforms. Section III presents the empirical strategy. Section IV discusses the methodology. Section V presents the results. Section VI concludes.

II. Conceptual Framework

A. *The Problem: Relationship-Specific Investments*

A buyer enters a contract with a seller to produce a customized good that has no value outside the specific relationship. Once the customized good is produced, the buyer can renegotiate prices down since there is no other buyer for this good. Anticipating this, the seller does not invest because there are appropriable quasi-rents in the relationship: the value of the customized good to the buyer exceeds the value of the good to the next best user (Klein et al., 1978). Those with the bargaining power to do so will appropriate the quasi-rents and reduce the incentives for the other party to invest in the relationship. The fundamental issue is that the buyer has no credible commitment to respect the contract.

B. *A Solution: The Judiciary*

A solution to this problem is to uphold such relationship-specific contracts through efficient third-party enforcement facilitated by the judiciary. The seller can sue for the claim—the value of the good if the buyer breaks the contract and appropriates the good. The decision to sue depends on the three characteristics of the judiciary: the probability p of winning (quality), the time T after which the judgment is given (speed), and the legal fees to pay (access), equal to a fraction l_p of the claim (index p for plaintiff). Ideally, $p = 1$: the judge rules in favor of the seller since the buyer breaks the contract (high quality of judicial decision making), T is small (high speed), and l_p is low (the legal system is accessible). In practice, p may be less than 1 (it is possible that the courts will rule against the plaintiff if they are of low quality—for example, if judges can be influenced by the defendant), and the time and costs to get a verdict may be high. With a discount factor β, the net present value recovered after a trial is a fraction $p\beta^T$ of the claim. The seller sues if the net present value recovered is greater than the costs. This defines the suing condition:

$$p\beta^T \geq l_p. \tag{1}$$

If suing condition (1) holds, the seller sues, which might deter renegotiation in the first place as a portion of the output is guaranteed to be returned to the seller. As a result, the seller exerts effort, enters more of these relationship-specific investments, and increases output, as can be shown in a full-fledged model of effort provision under a judiciary in online appendix A. This notion shows that the judiciary is integral to solving the issue of lack of credible commitment.

This reasoning applies only if the suing condition (1) holds—the characteristics of the judiciary are such that the seller has incentives to sue. In fact, this condition does not hold in numerous countries (as shown by the map in figure A1 in the appendix using estimates from the Doing Business project), especially in poorer countries. There may be two explanations for this. First, it is costly to create and maintain a high-quality judiciary as the costs are more binding for poor countries, so they tend to have worse judiciaries. A second explanation centers on the identity of those holding political power, in line with Acemoglu and Robinson (2013). If buyers have the bargaining power to capture quasi-rents, they may also have the bargaining power to influence policies. Consider the case where buyers are powerful and collude with a small elite that controls a powerful government—an oligarchy. In this case, a dominant strategy for the elite is to promise a well-functioning judiciary ex ante and renege on the promise once the quasi-rents are produced so that buyers can capture them. Anticipating this, the sellers do not invest and no relationship-specific investments take place. The fundamental issue is that an oligarchy cannot credibly commit to build an effective judiciary. But if a broad cross-section of society, which represents interests of both buyers and sellers, is in power, there will be incentives to set up a well-functioning judiciary to benefit the entire population.

This reasoning would explain the existence of two groups of countries: one encompasses pluralistic governments, which set up highly functioning judiciaries (high $p\beta^T - l_p$) and experience growth and the other, oligarchic governments, which set up poor judiciaries to serve the interests of the elite (low $p\beta^T - l_p$) and as a result experience poor economic development.

The deficiencies of judiciaries (low $p\beta^T - l_p$) observable in numerous countries have important implications for the likely effects of judicial reforms.

C. Implications for Judicial Reforms

First, consider the case of a deficient judiciary with low incentives to sue: low $p\beta^T - l_p$. In the extreme case, an oligarchy reneges on its promise of a well-functioning judiciary to capture rents and sets $p = 0, T = \infty$, and l_p is high. In such a world, there are no incentives for the elite to change the situation, hence no impetus for domestic judicial reforms. In this context, there may be a role for externally financed judicial reforms. These reforms must be comprehensive, addressing all characteristics p (quality), T (speed), and l_p (access), at the same time, to have an effect since improving only one characteristic and leaving the others unchanged does not improve the incentives to sue. For example, increasing access to an otherwise slow and biased judiciary will not fundamentally alter the situation. Thus, limited reforms (i.e., reforms tackling only one characteristic p, or T, or l_p) have no effects. Moreover, the effect of the judicial reforms on p, T, and l_p must also be large enough to reverse the inequality $p\beta^T < l_p$; otherwise, they will have no effect. In the empirical section, I proxy the magnitude of a reform by its budget size. The overall prediction is that externally financed comprehensive judicial reforms, that is, addressing all three characteristics p, T, and l_p at the same time and of a large enough budget size, may have an effect.

Finally, in the case of a well-functioning judiciary (a rare occurrence according to figure A1) where suing condition (1) holds, a judicial reform that increases p and decreases T and l_p will unambiguously increase the seller's incentives to sue, further increasing effort and output (and lowering the buyer's incentives to ex post renegotiate).[1] This reasoning applies only to firms in sectors requiring relationship-specific investments. For other firms, the good is generic and prevents the buyer from renegotiating prices as the seller can easily turn to other potential buyers.

Overall, the predictions of the model are ambiguous and depend on the initial state of the judiciary characterized by the sign of the suing condition (1). In section III, I look at the effect of judicial reforms separately in countries with $p\beta^T < l_p$ and $p\beta^T \geq l_p$.

Moreover, there may be strong obstacles to the proper implementation of externally financed comprehesive judicial reforms. Oligarchies have incentives to undermine judicial reforms. Local elites may have ways to undo any efforts to raise quality (higher p), speed (lower T), and access (lower l_p). For example, giving judges a long tenure to guarantee their independence (thereby increasing p) may be pointless if politicians can find other ways to influence judges, such as killing chief justices in the famous case of Uganda's leader, Idi Amin, in 1972, or packing the courts to curb its constitutional review power as Perón and Menem did in Argentina (Acemoglu & Robinson, 2013). In the next section, I look at the effect of judicial reforms in more or less oligarchic countries. Overall, the effect of judicial reforms is an empirical question.

III. Empirical Strategy

A. Data on Firms

The World Bank Enterprise Surveys (henceforth known as the Enterprise surveys) standardized data set contains 124,939 firm-level surveys.[2] To make these surveys representative of an economy's private sector, the Enterprise surveys use a global methodology whereby firms are stratified by business sector, location, and firm size.

These surveys present several advantages for the analysis of judicial reforms. They include a question on the perception of the judicial quality: "Do you agree with the following statement: The court system is fair, impartial and uncorrupted?" On a scale of 1 to 4, where 1 is Strongly Disagree and 4 is Strongly Agree, the average answer is 2.3 (SD = 1) as can be seen in the descriptive statistics of table 1.

The surveys also include measures of firm productivity. The main measure used in this paper is the value added per worker, that is, the difference between firm revenue and cost of intermediate inputs (raw materials and intermediate goods used in production, electricity, communications services, rental of land and buildings, equipment, furniture, fuel, transport for goods, water) divided by the total number of full-time employees, adjusted for temporary workers using PPP exchange rates.[3] The average value added per worker is

[1] To be more precise, within that second condition $p\beta^T \geq l_p$, for judicial reforms to have an effect, there must be some incentives for the buyer to ex-post renegotiate. Otherwise, judicial reforms have no effect since the judiciary is already perfect, the seller has incentives to sue, the buyer does not ex-post renegotiate and relationship-specific investments already have taken place. I abstract from this particular case since figure A2 in the appendix makes it clear that there are incentives to renegotiate ex post in almost all countries.

[2] The surveys are answered by business owners and top managers. Sometimes the survey respondent calls company accountants and human resource managers into the interview to answer questions in the sales and labor sections of the survey. Typically 1,200 to 1,800 interviews are conducted per country. The manufacturing and services sectors are the primary business sectors of interest. Formal (registered) companies with five or more employees are targeted for interview.

[3] This measure is quite noisy: the maximum value in the original data set is $47 billion per worker. I thus trim this measure at 1%. The maximum value with a trim at 1% is $3 million per worker, which is high. Yet the average value of this trimmed variable matches values in figure B1 in the appendix.

TABLE 1.—DESCRIPTIVE STATISTICS

	Mean	SD	N
Firm-level data			
Judicial efficiency	2.30	1.00	107,192
Value added per worker (thousand dollars)	80.98	212.07	99,623
Capital stock per worker (thousand dollars)	48.90	164.31	42,324
Proportion of skilled workers	0.70	0.31	59,168
Number employees	112.96	11,25.20	117,044
New firm	0.04	0.20	116,063
Judicial reforms			
Comprehensive judicial reform	0.22	0.41	117,320
Limited judicial reform	0.22	0.42	117,320
Relationship-specific investment			
Specific	0.40	0.49	54,439
Herfindhal Sweden	0.41	0.49	69,429
Input/output Sweden	0.41	0.49	73,743
Herfindhal Continent	0.59	0.49	105,966
Input/output Continent	0.53	0.50	105,962
Control variables			
Business climate	53.25	9.52	116,374
Foreign aid per capita	49.72	78.55	100,807

Judicial efficiency is agreement with the statement: "The court system is fair, impartial and uncorrupted" (1: Strongly Disagree, 4: Strongly Agree). "Comprehensive judicial reform" takes the value 1 when a reform addressing the three criteria of quality, speed, and access and with a budget above 5% of the judiciary budget was implemented in between the two rounds of a World Bank Enterprise Survey. "Specific" is a dichotomous variable equal to 1 if goods are specific, that is, it would take more than a month for the main customer to find the same good were the seller to shut down its operations. Each variable is defined in greater detail in appendix B.

$80,710. The Enterprise surveys also contain data on capital stock per worker, number of employees, and proportion of skilled workers.

Seventy-four countries have a baseline and end-line survey in the standardized data set which allows a before-and-after comparison. To check for common pretrends, I augment this data set with earlier waves of World Bank Enterprise Surveys that are not part of the standardized data set and are called "Pre" in the rest of the paper.[4] In total, 39 of 74 countries considered in this paper have an earlier wave of data.

The next step of the analysis is to systematically document all judicial reforms implemented between a baseline and end-line survey for each of these 74 countries.

B. Judicial Reforms

To identify externally funded judicial reforms, I use data from a new open-data initiative, the International Aid Transparency Initiative (IATI), which collates all data related to projects implemented by major donors around the world in a unified and high-quality format. Since 2010, 500 organizations (donor governments, multilateral agencies, foundations, nongovernmental organizations, and private sector organizations) from 55 countries have agreed to publish IATI data.

[4]Two issues have to be kept in mind when doing this. First, these earlier waves of data do not follow the global methodology. Hence, firms are not stratified by business sector, location, and firm size. According to the World Bank, "Surveys that are Non-Global do not yield results that are representative of the firm population in a country" (http://www.enterprisesurveys.org/~/media/GIAWB/EnterpriseSurveys/Documents/Misc/Indicator-Descriptions.pdf). Second, the variables are not standardized and may differ across data sets.

In this database, I identify all projects related to the judiciary. For each project, the IATI data include the year and country of implementation, the implementing organization, a description, and the budget. Within this data set, I identify all reforms that started precisely between the years of baseline and end-line Enterprise surveys for each country.

Despite the available information in the IATI data, the descriptions of judicial reforms are limited to a few sentences. To get additional information on the reforms and since legal scholars recognize the World Bank and USAID as the "largest and most influential rule of law actors" (Humphreys, 2010, 20), I complement the IATI data set by reviewing data on judicial reform projects directly from the World Bank and USAID databases. These two organizations publish their data in the same format as the IATI but with more detailed descriptions. I also complement these descriptions by systematically gathering official documents published by the donor describing the reforms in greater detail. The procedure is outlined in appendix C. To access the data, the online appendix contains the full list of reforms in "List Judicial Reforms.xls." This file contains the IATI data. For each project, the IATI data include the year and country of implementation, the implementing organization, a description, and the budget. The coding of the quality, speed, and access nature of the reforms comes from the reading of the extra documents also provided in the online appendix.

I then read and codify the description of all of these projects according to the three characteristics of efficient judiciaries highlighted in the theoretical section: quality (increase in p), speed (decrease in T), and access (decrease in L_p). To be more precise, 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, anticorruption, governance, compliance with rule of law, fairness, improved service delivery*, and *strengthening of the rule of law.* Similarly, I assign the value of 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*, and *reduction in backlog*. Finally, I assign the value 1 to a variable "reform_access" if one of the objectives of the reform is to increase access to the judiciary. In practice, I look for keywords such as *access, legal services, for the poor, justice for all, J4A*, and *the building of courts* (which reduces the distance to courts, thereby improving access).

As an example, I show the results for Kenya in table 2. (Appendix C provides an explanation for each of the 74 countries, and the online appendix contains the full list of reforms in "List Judicial Reforms.xls" with the IATI data. The online appendix also includes the extra documents used to code the quality, speed, and access nature of the reforms.)

Kenya had a baseline Enterprise survey collected in 2007 and an end-line in 2013 (and no earlier wave of data). In

TABLE 2.—JUDICIAL REFORMS IN KENYA BETWEEN 2007 AND 2013

Year	Sponsoring Organization	Description	Budget (millions of U.S. dollars)	Access	Speed	Quality
2009	Canada (DFATD)	The program aims to improve access to justice, particularly for poor and marginalized people, including women.	2.98	1		
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, and enjoy their legal rights.	0.11	1		
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
2012	Sweden	Support to Judiciary Transformation Framework in partnership with GIZ (German Development Agency).	2.21	1	1	1
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.	4.40	1		

2009, Canada implemented a project whose explicit goal was to "improve access to justice." As described above, I codify this project as intending to improve access. The budget of this reform was $3 million, 1.6% of Kenya's judiciary budget.[5] Similarly in 2010, Finland started a project to enable access to justice, which I also code as improving access.[6]

The major project at the time in Kenya was a World Bank program, Judicial Performance Improvement Project (JPIP), started in 2012 for a duration of six years with a total budget of $120 million.[7] The explicit goal of the reform was to improve "performance, accountability, access to justice and the expeditious delivery of judicial services." Overall, 230 activities are being implemented as part of JPIP: "JPIP has recorded some key achievements, including on infrastructure (construction of new courts under way or completed at 17 sites); performance management (performance contracts rolled out to court stations; and a new administrative data and case management system implemented); backlog reduction (almost 50,000 very old cases resolved); skills development (3100 people have been trained); strategic and administrative reform (launches of the Judiciary Strategic Plan; High Court Registry Operations Manual; and the Human Resources and Financial Management Policies and Procedures Manual)." Following the procedure above, I codify this reform as intending to improve quality, speed, and access.

In later reforms in Kenya, Sweden and Germany provided support for the judicial reform in the Judiciary Transformation Framework.[8] The four pillars of this reform are "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."[9] I codify this reform as intending to improve quality, speed, and access.

Overall, five projects were implemented in Kenya over the period between the baseline and end-line Enterprise surveys, and together they addressed the three characteristics of quality, speed, and access. The total value of these reforms was $129.7 million, or 12% of Kenya's annual judiciary budget.

I repeat the exercise for all other 74 countries between a baseline and end-line Enterprise survey (see a short explanation for each country in appendix C and the full list of reforms and all documents describing the reforms in the online appendix).

An important finding from this exercise is that the World Bank and USAID reform projects simultaneously target quality, speed, and access in all countries.[10] This is confirmed by World Bank and USAID documents outlining the overall objectives of their judicial reforms. In "Initiatives in Justice Reform 1992–2012" (World Bank, 2012), the World Bank describes its reforms as targeting quality (support for modern procedures for the selection, career development, and management of justice sector personnel and support justice mechanisms to hold public institutions accountable to the public), speed (project activities focus on court management, including strategic planning, financial management, data collection, and use of data for management decisions), and access

[5]There is no centralized database for the budget of judiciaries in the World Bank. One way to estimate it is to use figures from the OECD. The median budget of the judiciary in the OECD is 0.2% of a country's GDP (Palumbo et al., 2013). Kenya's GDP was $91 billion in 2007. According to this estimate, an upper bound for the Kenya's judiciary budget is $182 million. This is confirmed by the Kenyan official budget (available at https://www.internationalbudget.org/open-budget-survey), which states that the Kenya's judiciary budget was Ksh13 billion in 2016, approximately $130 million USD.

[6]One may worry about the relevance of this project for firm productivity since the Finland project targeted "marginalized communities living in Wajir district." I still code this reform as improving access since there may be some firms benefiting from the project within marginalized communities in Wajir district.

[7]See the full description in the online appendix in World Bank/Kenya JPIP description.pdf.

[8]See the full description in the online appendix in World Bank/Kenya Judiciary Transformation Framework.pdf

[9]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.

[10]There are two exceptions. In Nigeria, a World Bank project was solely a legal aid access intervention in one state (see the World Bank/Nigeria.pdf in the online appendix). In El Salvador, there was a small USAID project on mediation only (see USAID/El Salvador Tetra tech mediation.pdf). These two reforms are coded as limited reforms.

(project activities include analysis of the market for legal services, support for legal services and aid especially for the poor and vulnerable, and creation of mobile courts and services to support community-based or court-annexed alternative dispute resolution.

Regarding USAID, the overall objectives of their judicial projects are to "strengthen judicial independence and impartiality" (quality), "ensure an effective and equitable justice system" [*effective* implies speed] . . . by improving professional capacities and administrative and operational systems of actors and institutions; developing and implementing fair procedures; expanding access to justice [access]; and ensuring adequate oversight, advocacy, and accountability.[11] Thus, it is clear from this description that quality, speed, and access are all integral parts of a holistic approach when considering judicial reform projects.

Thus, I define a variable "reform_comprehensive" as 1 if a country received a reform addressing the three characteristics of quality, speed, and access, and with a budget above 5% of the national judiciary budget or a World Bank reform or a USAID reform (considering the magnitude and comprehensive approach taken by these two large, influential actors). I use a threshold of 5% since it is the average budget of both World Bank and USAID's judicial reforms. Similarly, I define a variable "reform_limited" as 1 if a country received a reform with either a small budget or not targeting all three characteristics (quality, speed, or access).

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 2010 Finland project should be coded as improving access because of its focus on some sections of society in one particular area of the country. Yet overall, it is quite clear that Kenya can be considered as having received a comprehensive reform in light of the budget size and breadth of numerous projects. In fact, the coding was cross-validated by two independent coders, with correlations in the coding of 93% and 86%.[12]

Out of the 74 countries with two waves of the Enterprise surveys, 22 have experienced such comprehensive reforms, 23 have had limited reforms, and 29 countries have had no reforms (see the complete list in the online appendix in List of Judicial reforms.xls). I also repeat the exercise for the 38 countries with an earlier wave of data. None of these countries had a comprehensive reform between a Pre wave and the baseline. I thus keep all these earlier waves of data to check for common pre-trends in the econometric analysis.

To identify domestic reforms, I use data from the Doing Business project, which collates all the reforms implemented by countries related to the topic of enforcing contracts (the exact dispute this paper focuses on: a buyer refuses to pay for a customized good).[13] I read and codified the description of all of these projects according to the same three characteristics (quality, speed, and access). The complete list is in appendix C.

C. Endogeneity of Judicial Reforms

Why are judicial reforms adopted in the first place? In the extreme case of an oligarchy described in the theoretical model, the judiciary is a constraint for the elite; the elite have no incentives to build an effective judiciary, and there is no impetus for domestic judicial reforms. In this world, it is hard to see why the elite would adopt externally financed judicial reforms.

One answer is critical junctures and reform-minded leadership. Kenya provides an illustration. In the 2007 general elections, the opposition was narrowly defeated and complained of rigged elections. The opposition did not file a case in court since it did not see the judiciary as capable of delivering a fair verdict and instead took the dispute to the streets (Gainer, 2015). The tension escalated into a grave ethnic conflict, killing more than a thousand and displacing up to 600,000. To avoid the repetition of such events, a new progressive constitution was voted in 2010, which proclaimed among other things that the judiciary shall be unbiased, fast, and accessible. The Kenyan embarked on the Judiciary Transformation Framework, supported financially by the World Bank with the JPIP project, as well as Sweden and Germany, as visible in table 2.

Interestingly, in the 2017 general elections, the same opposition was again defeated and complained of rigged elections. This time, they filed a case in court and won: the Supreme Court ordered a rerun of the elections, the first decision of its kind in Africa.[14] The incumbent called judges "crooks."[15] In line with the theoretical argument that the executive may have incentives to undermine the judiciary, the JPIP and the judiciary are now under attack. In 2018, the chief justice complained that the government reduced the judiciary's budget and refused to extend the JPIP project.[16] This illustrates that support for judicial reforms may be fleeting and dependent on the presence of reform-minded leaders. More generally than Kenya, legal scholars argue that the World Bank and USAID

[11] The official document, USAID/Department of State and USAID Strategic Plan.pdf, is in the online appendix.

[12] The disagreement comes from Malawi and Rwanda. These two countries did not receive a World Bank or USAID reform, yet as explained in greater detail in appendix C, the two coders uncovered official documentation on other reforms showing that the reforms received were in fact comprehensive. The inclusion of Malawi and Rwanda in "reform_comprehensive" = 1 does not affect the results, as shown in table C1. Other than these two countries, the coding was exactly the same.

[13] http://www.doingbusiness.org/Reforms/Overview/Topic/enforcing-contracts.

[14] Kimiko de Freytas-Tamura, "Kenya Supreme Court Nullifies Presidential Election," *New York Times*, 1, 2017.

[15] Tom Odula, "Kenya President Warns Judiciary after It Nullifies Election," Associated Press, 2, 2017.

[16] https://www.judiciary.go.ke/download/statement-on-the-state-of-the-judiciary-in-light-of-drastic-cuts-in-budgetary-allocations/.

work with "reform-minded and active" leaders who support the reforms (Humphreys, 2010, 183).[17]

The fact that aid-based judicial reforms may be responsive to reform-minded leadership poses an econometric issue: this reform-minded leadership may also be concurrently implementing other changes in the economy that may drive economic development. As such, a difference-in-differences analysis may overestimate the impact of judicial reforms. The bias could also go the other way: the World Bank and USAID may be implementing the reforms where they are needed the most: in poor countries with declining economies. To address these potential biases, I use within-country variation in the likely effects of these reforms, described in the following section.

D. Relationship-Specific Investment

I use a well-established insight in economics: some sectors rely on the judiciary more than others because of the need for relationship-specific investments (Levchenko, 2007; Nunn, 2007). To measure the sectoral need for relationship-specific investments, the literature uses measures of input complexity, the value of inputs (since more inputs require more contracts; Levchenko, 2007), or whether inputs are sold on an international organized exchange (Nunn, 2007).

Aside from replicating these measures in the Enterprise surveys, I develop a new measure from within these surveys based on the key point of the theory: the specificity of the good produced. If the good is generic, alternative buyers and sellers can easily be found for the good, and there is no possibility of renegotiation ex post. If the good is specific, there is a possibility of renegotiation ex post, and third-party contract enforcement is needed. The empirical difficulty is to provide a quantitative measure of a sector's technological propensity for dealing in specific versus generic goods.

To determine this, I use a specific question collected in a special type of the Enterprise surveys: the Management, Organisation and Innovation round. I use the survey from Germany since the goal is to measure the technological propensity of a sector to deal in specific versus generic goods under a near-perfect judiciary (Germany was ranked fourteenth in the World in 2016 by the Doing Business project). The question is: "If this establishment shut down its business, how long would it take your largest customers to find an alternative seller for its main product?" The possible answers are: "1: a day or less; 2: more than a day, less than a week; 3: more than a week, less than a month; 4: a month or more; 5: never (it would be impossible to replace)." If the answer is a day or less, the good is generic. The customer can easily find an alternative seller and there is no possibility of ex-post renegotiation. The value of the good outside of a buyer–seller relationship is close to the value inside the relationship, and, by definition, the good is not relationship specific (Klein et al., 1978).

In contrast, if the answer is a month or more (or even never), the good is specific. The customer would have trouble finding an alternative seller, which generates a possibility for ex-post renegotiation. In this case, the seller can renegotiate prices up, and the buyer agrees since he or she will have difficulty finding an alternative seller. Anticipating this, the buyer does not enter in the relationship and the seller does not invest unless efficient judiciaries enforce contracts. Both the buyer and the seller (despite holding the power in this particular setting) are dependent on well-functioning judiciaries.

The complete list of goods with the answer 5 (never) and 1 (a day or less) is in table D1 in appendix D. Goods from firms that answered 5 are specific (e.g., actuators for plane seats, aluminum castings, locking systems for cars), and more generic when firms answered 1 (e.g., bakery products, beer, books). The complete distribution of answers is shown in figure 1a. This figure shows that firms can be split neatly into two groups of approximately equal size. In one group are firms answering 1, 2, or 3 (i.e., requiring less than a month to replace the good) and in the other group, those answering 4 or 5 (i.e., more than a month). I classify these two groups as firms dealing in generic and specific goods, respectively.

I then calculate the average answer of firms per sector, disaggregated at the four digit level of the ISIC rev3.1 classification (the industry code specification in the Enterprise surveys). The results are shown in figure 1b.

As evident from this graph, some sectors have all firms dealing in generic goods (i.e., answering 1, 2, or 3) and are thus classified as sectors dealing in generic goods. Other sectors have all firms dealing in specific goods (i.e., answering 4 or 5), and are thus classified as sectors dealing in specific goods. For the very few sectors where some firms answer 0 or 1 in the middle of the distribution, I split them according to the median of this index (median $= 0.36$) into sectors dealing in either generic or specific goods.

Overall, this procedure creates a dichotomous variable, called $Specific_j$, which defines two groups of sectors: those dealing in generic goods ($Specific_j = 0$) and those dealing in specific goods ($Specific_j = 1$). $Specific_j$ takes the value 1 for 40.2% of the sample, as shown in table 1. Firms in sectors dealing in specific goods where the opportunity for ex-post renegotiation is greater are more dependent on the judiciary and should be disproportionately affected by judicial reforms.

To ensure that the results do not depend on this particular measure used, I incorporate two completely different measures of reliance on contracts that focus on intermediate inputs rather than the downstream final good. The exact procedure and results are described in greater detail in section IV.

IV. Methodology

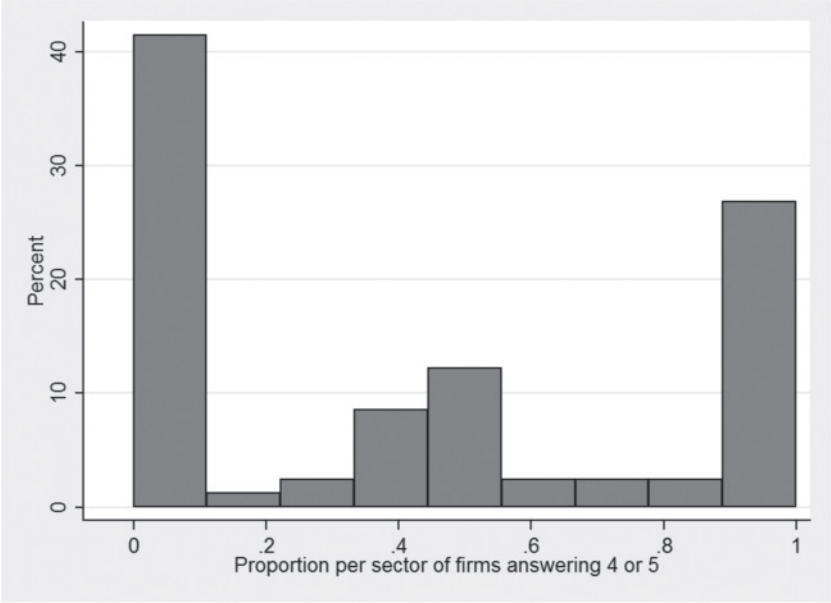
The empirical question is, Do firms become more productive after a judicial reform, especially in sectors dealing in

[17] Another example is Peru. In 1998, the World Bank pulled its support from a judicial project when President Fujimori reduced the independence of the judiciary, once again indicating that the placement of reforms responds to the local context (Trebilcock & Daniels, 2009).

FIGURE 1.—SPECIFIC AND GENERIC SECTORS

(a) If seller shut down, how long would it take to find alternative seller?

(b) Proportion per sector of firms answering 4 or 5



Panel a: 1: a day or less; 2: more than a day, less than a week; 3: more than a week, less than a month; 4: a month or more; 5: never (it would be impossible to replace). Panel b: For each sector (disaggregated at the four-digit level of the ISIC Rev3.1 classification), I calculate the proportion of firms answering 4 or 5. This graph shows the distribution of that proportion.

specific goods? To test this proposition, I estimate the following specification:

$$\begin{aligned}\text{Productivity}_{ijkt} = {} & \beta_1 \text{JudicialReform}_k \times Post_t + \beta_2 Post_t + \alpha_k + \\ & \beta_3 \text{JudicialReform}_k \times Post_t \times Specific_j \\ & + \beta_4 Post_t \times Specific_j + \alpha_k \times Specific_j + \\ & \beta_5 \text{JudicialReform}_k \times Pre_t + \beta_6 Pre_t + \\ & \beta_7 \text{JudicialReform}_k \times Pre_t \times Specific_j \\ & + \beta_8 Pre_t \times Specific_j + X_{kt}\gamma + X_{kt} \\ & \times Specific_j \delta + \theta_j + \varepsilon_{ijkt}, \end{aligned} \tag{2}$$

where i is for firm i in sector j in country k at time t. $\text{Productivity}_{ijkt}$ is firm productivity, measured as value added per worker. JudicialReform_k is a dichotomous variable equal

to 1 if country k experienced a comprehensive judicial reform in between baseline and end-line Enterprise surveys and 0 otherwise. $Post_t$ is a dichotomous variable equal to 1 if the observation is in the end-line survey and 0 otherwise. α_k are country fixed effects.

$Specific_j$ is the specificity of the good produced by sector j. Interacting $Specific_j$ with all variables of the model allows for a triple-difference analysis. The main hypothesis tested in this paper is that β_3 is positive: judicial reforms have positive effects on firm productivity in sectors depending on the judiciary. Judicial reforms may also have a positive effect on sectors where firms deal in generic sectors. If this is the case, then β_3 would represent a lower bound on the true effects of judicial reforms since the control group is also partially affected.

To check for common pre-trends, I also look at the earlier waves of data collected before the baseline and end-line surveys, called Pre. Pre_t is a dichotomous variable equal to 1 if the observation is in the earlier wave Pre, and 0 otherwise. I further interact Pre_t with JudicialReform_k, $\text{JudicialReform}_k \times Specific_j$, and $Specific_j$ to look at common pre-trends for firms in sectors dealing in specific goods in countries that later got a judicial reform. Since the omitted category is the baseline Enterprise survey, coefficients β_5, β_6, β_7, and β_8 must be interpreted as the difference between the Pre and baseline waves.

To address the issue of coincidental shocks that may disproprotionately affect firms in sectors with relationship-specific investments, I include the following control variables in X_{kt}: the general business climate,[18] total foreign aid per capita, and sector fixed effects θ_j disaggregated to the second level of ISIC Rev3.1. In the triple-difference analysis, I also interact these variables with $Specific_j$. Standard errors are robust, clustered at the level of countries.

V. Results

A. Perception of Judicial Efficiency

The Enterprise surveys include a question on the perception of the judiciary's quality. Each firm is asked on a four-point scale whether they agree with the following statement: "The court system is fair, impartial and uncorrupted" ($M = 2.3$, $SD = 1$). Figure 2a shows the results for sectors dealing in specific goods (i.e., more dependent on the judiciary), while figure 2b shows the results for sectors dealing in generic goods.

Clearly, perceptions of judicial efficiency improve in countries experiencing a judicial reform by almost 0.15 standard deviation for all firms regardless of the goods they deal in. This is to be expected, as judicial reforms improve the judiciary for all firms, not just some. Yet the theory predicts larger effects on firm productivity for sectors dealing in specific over generic goods.

B. Labor Productivity

Figure 2c shows the evolution of labor productivity (defined as value added per worker) for sectors dealing in specific goods, that is, more dependent on the judiciary. This panel shows a positive impact of judicial reforms: firms in countries with judicial reforms are less productive prior to reforms but catch up to firms in countries without reforms.

C. Common Time Trends

To identify this effect as causal, one has to assume common time trends: in the absence of judicial reforms, firm productivity in treated or control countries would have evolved the same way.

There are two ways to check this. First, one can look at the evolution over the same period of other firms (less dependent on the judiciary for technological reasons). Figure 2d shows the evolution of productivity for firms dealing in generic goods. The trends are rather similar. Considering the same trends in the generic sector, one can assume that the trends would also have been similar in the sectors dealing in specific goods had there been no judicial reforms. Yet panel c indicates a catch-up, which can be attributed to the judicial reforms.

Another approach to testing the common time effects assumption is to look at pre-trends. Figure 2e shows the period "pre," one wave before the start of the judicial reforms. This graph shows that firms in countries that later got judicial reforms were on quite a similar trend than were firms in other countries. If anything, the trend was slightly going in the other direction—firms in countries that got judicial reforms later were doing slightly worse. Figure 2c indicates a catch-up that cannot be attributed to the continuation of an ongoing catch-up trend.

This graphical analysis pooled all countries experiencing judicial reforms together. In regressions, one can include country fixed effects to directly control for any country-specific factors.

D. Main Regressions

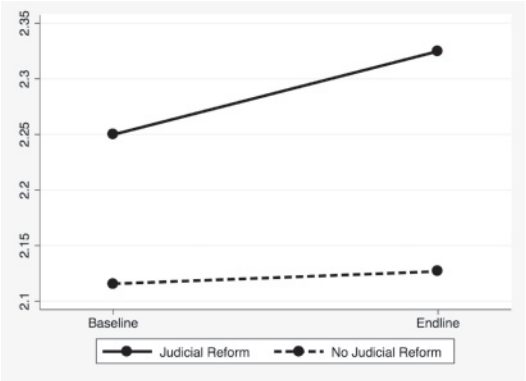
Table 3 confirms the graphical results: perceptions of judicial efficiency increase by 0.15 standard deviation for all firms since the coefficient of $JudicialReform_k \times Post_t$ is 0.15, while the coefficient of it $\text{JudicialReform}_k \times Post_t \times Specific_j$ is not significantly different from 0. Hence, all firms in sectors dealing in generic or specific goods have an improved perception of the judiciary.

[18] Measured by Doing Business, as the distance to frontier score that measures the gap between a particular economy's performance and the best practice of all the categories except enforcing a contract: starting a business, dealing with construction permits, getting electricity, registering property, getting credit, protecting minority investors, paying taxes, trading across borders, or resolving insolvency.

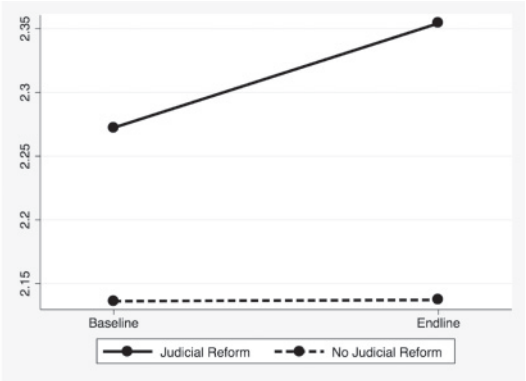
FIGURE 2.—EFFECT OF JUDICIAL REFORMS ON PERCEPTIONS OF JUDICIAL EFFICIENCY AND LABOR PRODUCTIVITY

Perception of judicial efficiency

(a) SPECIFIC sectors

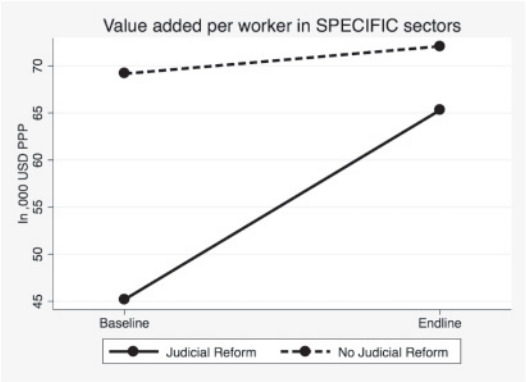


(b) GENERIC sectors

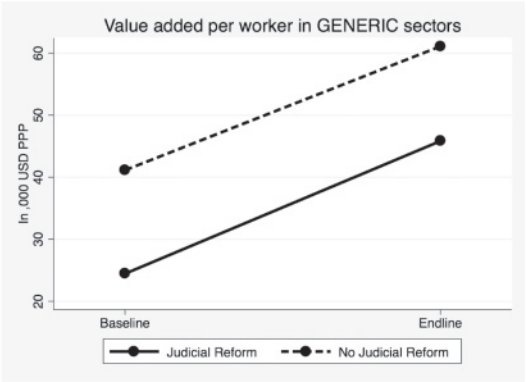


Labor productivity

(c) SPECIFIC sectors

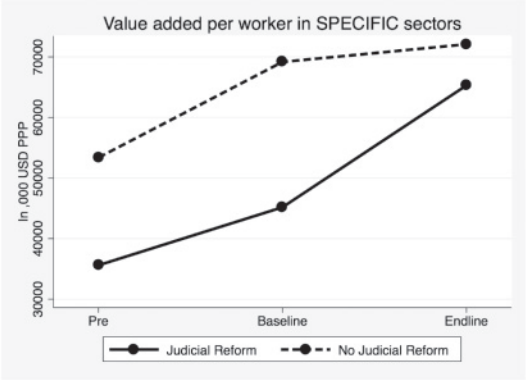


(d) GENERIC sectors



Common pre-trends

(e) SPECIFIC sectors



Perception of judicial efficiency is measured with, "The court system is fair, impartial and uncorrupted." Answers: 1: Strongly Disagree, 2: Tend to Disagree, 3: Tend to Agree, 4: Strongly Agree. Labor productivity is value added per worker, expressed in thousand U.S. dollars PPP. Judicial reform countries are countries in which at least one comprehensive judicial reform (targeting access, speed, and quality, and a budget above 5% of the annual judiciary's budget) was implemented in the period between two enterprise surveys. Specific = 1 is the group of sectors dealing in specific goods, Specific = 0 is the group of sectors dealing in generic goods (goods are specific if it would take more than a month for the main customer to find the same good were the seller to shut down its operations).

Column 2 of table 3 shows the results for firm productivity. The coefficient of interest—the triple-difference coefficient of $JudicialReform_k \times Post_t \times Specific_j$—\$19,400, significantly different from 0. This corresponds to an increase in 0.09 standard deviation of labor productivity (equal to \$214,000), or an increase of 22% (over the average firm productivity in the control group, countries not experiencing any judicial reforms, of \$86,000).

TABLE 3.—IMPACT OF JUDICIAL REFORMS ON FIRM PRODUCTIVITY

	(1) Judicial Efficiency	(2)	(3)	(4)	(5) Firm Productivity	(6)	(7)
Judicial reform × Post	0.15**	15.94	21.33	21.22	16.12	16.09	14.18
	(0.07)	(11.66)	(12.92)	(13.25)	(12.97)	(12.95)	(12.02)
Judicial reform × Post × Specific	−0.03	19.40**	19.92**	19.86**	20.33**	20.45**	17.94*
	(0.06)	(8.88)	(9.00)	(8.92)	(9.29)	(9.31)	(10.02)
Judicial reform × Pre	−0.07	−30.42	−34.64	−33.64	−36.49	−36.06	
	(0.28)	(33.31)	(40.07)	(32.59)	(43.99)	(43.92)	
Judicial reform × Pre × Specific	−0.15	10.75	13.72	13.61	15.83	15.27	
	(0.15)	(16.18)	(15.55)	(15.96)	(18.94)	(19.18)	
Limited judicial reform × Post			14.27	14.16	11.16	11.46	4.68
			(10.21)	(10.19)	(9.80)	(9.82)	(7.13)
Domestic judicial reform × Post				0.88	4.48	4.90	−3.02
				(8.36)	(9.72)	(9.90)	(8.21)
Business climate					−0.99	−0.94	0.26
					(1.00)	(0.99)	(0.63)
Foreign aid per capita						0.08	0.10**
						(0.05)	(0.04)
Observations	51,190	50,493	50,493	50,493	49,373	49,373	47,041
Country fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sector fixed effects							Yes
F-test	1,100	60.14	244.9	916.4	377.9	1832	2.766e+06

OLS regressions. Robust standard errors in parentheses, clustered at the country level. *** $p < 0.01$; ** $p < 0.05$; and * $p < 0.1$. The dependent variable in column 1 is the perception of judicial efficiency, as measured on a four-point scale as the tendency to agree with the statement: "The court system is fair, impartial and uncorrupted." The dependent variable in columns 2 to 7 is the value added per worker, expressed in thousand U.S. dollars PPP.

For the earlier waves of data, the coefficient of $JudicialReform_k \times Pre_t \times Specific_j$ is not significantly different from 0, which indicates that before the judicial reforms were implemented, firms in specific sectors were on common pre-trends. The coefficient is \$10,750, which can be interpreted as the difference between the "Pre" and the baseline wave. In other words, the difference between the baseline and the "Pre" wave was the opposite of this: −\$10,750. Thus, as visible from the graph, the situation for firms in specific sectors was slightly worsening in countries that later implemented judicial reforms. The overall finding from these earlier waves of data is that the effect of the judicial reforms is not the continuation of a positive, ongoing trend.

Table 3 provides an *F*-test for the joint significance of all variables interacted with $Specific_j$. The null hypothesis is whether the coefficients of $JudicialReform_k \times Post_t \times Specific_j$, $Post_t \times Specific_j$, and $\alpha_k \times Specific_j$ are equal to 0. The *F*-statistic is 60, indicating a rejection of the equality of the coefficients of the generic and specific sectors and the validity of the triple-difference approach.

E. *Effects of Comprehensive, Limited and Domestic Judicial Reforms*

The wide variation in the types of judicial reforms implemented can also be used to understand which judicial reforms work best to increase firm productivity. The index JudicialReform_k is defined for comprehensive judicial reforms (those reforms targeting quality, speed, and access) and a budget above 5% of a country's judiciary budget. Aside from these comprehensive reforms, 23 countries have experienced more limited judicial reforms—those not targeting all three characteristics of quality, speed, and access. I define $LimitedJudicialReform_k$ as a dichotomous variable equal to 1 if the country experienced a limited judicial reform and 0 otherwise. The results in column 5 of table 3 show that limited reforms have no effect on firm productivity.

This is in line with the model proposed in the previous section. Limited reforms may not be enough to reverse the inequality $p\beta^T < l_p$. In the extreme where $p = 0$, $T = \infty$, and l_p is high, addressing one constraint while leaving the others unaffected does not influence the decision for the seller to sue and the buyer to ex-post renegotiate. For example, increasing access to a slow and biased judiciary will achieve nothing. Only comprehensive reforms matter.

This framework can also be used to look at the impact of domestic judicial reforms. First, I find no comprehensive domestic-led reforms (those targeting quality, speed, and access) in the data (see table C2). This is in line with the proposed model that predicts low impetus for domestic judicial reforms, at least in oligarchies, because of the limited commitment issue. In the data, I find only limited domestic reforms. These limited reforms have less of an effect on firm productivity than comprehensive externally financed reforms, as can be seen in column 6 of table 3. This does not mean that comprehensive domestic reforms would have no effect—if only they existed.

F. *Robustness Checks on the Endogeneity of Judicial Reforms*

Judicial reforms may be enacted by reform-minded leaders who simultaneously implement other reforms that improve the business climate. This may suggest that the effect of judicial reforms detected in this paper may be due to these other policies. To address this issue, I include a business climate index as a control variable in column 7 of table 3. I use the "distance to frontier" score measured by the Doing Business project. This index measures the gap between a

particular economy's performance and the best practice on the following categories: starting a business, dealing with construction permits, getting electricity, registering property, getting credit, protecting minority investors, paying taxes, trading across borders, and resolving insolvency. From the calculation, I exclude the category "enforcing contract" since it is precisely the category studied in this paper. I call this variable $BusinessClimate_k$. Column 7 of table 3 shows that the effect of comprehensive reforms remains very similar when controlling for the general business climate. In other words, the effect detected in this paper is net of any coincidental improvements in the process of doing business on all categories mentioned above.

Judicial projects by the World Bank or USAID may come together with additional foreign aid for other projects. In column 8 of table 3, I control for the amount of foreign aid per capita received in the country at a given time and find that the effect of judicial reforms remains similar. Column 9 of table 3 also adds sector fixed effects and shows that the triple-difference coefficients are unaffected.[19]

In particular, some foreign aid projects may disproportionately affect sectors engaged in specific versus generic goods. To address this issue, I use the same IATI data set to systematically document the effect of all other foreign aid projects on firm productivity. For example, I first select all projects related to education: basic, secondary, and postsecondary.[20] I then match these data to the Enterprise surveys. All countries in the sample have received at least one education-related project over the period of interest. To develop a quantitative measure of the magnitude of such projects, I sum the budgets of all education-related projects in between the baseline and end line Enterprise surveys for each country in the data set and divide this total budget by the country's GDP. I first determine the correlation between the index $JudicialReform_k$ calculated above (i.e., a dichotomous variable equal to 1 if country k experienced a comprehensive judicial reform and 0 otherwise) and this measure of education-related projects. As shown in column 1 of table F1 in appendix F, there is a correlation between judicial reforms and education-related projects, indicating that both reforms come together. Therefore, the effect detected in this paper on firm productivity may be due to these coincidental education-related projects.

To verify that this is not the case, I add the education projects to the triple-difference analysis. In column 1 of table 4, I add education reforms, interacted with $Post_t$, and $Post_t \times Specific_j$. Importantly, the coefficient of interest associated with judicial reforms for the specific sector is still statistically significant, indicating that the impact of judicial reforms is not merely driven by coincidental educational reforms. Education reforms have no effect per se on firm productivity, probably because reforms of basic, secondary, and postsecondary education affect firm productivity with a delay, not instantaneously.

Table 4.—Impact of Judicial and Other Reforms on Firm Productivity

	(1)	(2)	(3)	(4)	(5)
Judicial reform × Post	14.29	12.34	14.69	13.16	14.14
	(12.55)	(12.86)	(11.94)	(12.84)	(12.00)
Judicial reform × Post	16.81*	18.18*	16.54*	18.58*	17.23*
× Specific	(10.13)	(10.24)	(9.84)	(10.09)	(9.81)
Education × Post	−0.63				
	(5.56)				
Education × Post	4.88				
× Specific	(4.70)				
Health × Post		3.00			
		(2.84)			
Health × Post × Specific		−0.69			
		(1.73)			
Transport × Post			1.25		
			(0.94)		
Transport × Post × Specific			−1.71		
			(1.04)		
Energy × Post				5.40	
				(10.23)	
Energy × Post × Specific				−4.14	
				(7.43)	
Banking × Post					13.23
					(29.99)
Banking × Post × Specific					20.96
					(49.08)
Observations	46,990	46,990	46,990	46,990	46,990
Country fixed effects	Yes	Yes	Yes	Yes	Yes
Sector fixed effects	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes

OLS regressions. Robust standard errors in parentheses, clustered at the country level. *** $p < 0.01$; ** $p < 0.05$; and * $p < 0.1$. The dependent variable is the value added per worker, expressed in thousand U.S. dollars PPP. Control variables are limited judicial reform interacted with Post, domestic judicial reforms interacted with Post, business climate, foreign aid per capita, sector-level fixed effects, further interacted with the specific index. In column 2, Education is the budget of education reforms over the period of interest in each country.

Importantly, the triple-difference coefficient remains significant if one controls for health sector reforms (health, basic health, population policies and programs and reproductive health, and water and sanitation) in column 2; transport sector reforms (road construction, rail, water, air transport) in column 3; energy (oil, gas, coal, nuclear, hydroelectric power plants, geothermal, solar, wind, ocean energy) in column 4; and banking (formal and informal, in the form of micro credit, savings, and credit cooperatives) in column 5.

In table F2 in the appendix, I also find that the triple-difference coefficient remains significant when external factors are controlled, such as trade and debt reforms, reforms addressing the other two pillars of prosperity, tax capacity, and conflict-related reforms, according to Besley and Persson (2011),[21] and support to other counterpowers to the executive (the parliament, the media, independent election bodies guaranteeing free and fair elections, and anticorruption organizations). Overall, this table shows that the effect of comprehensive judicial reforms is not driven by coincidental foreign aid projects.

[19] The earlier waves of data must be dropped for this test since the variable used to create the sector fixed effects is not the same in the standardized data set and the earlier waves of data.

[20] Available at http://datastore.iatistandard.org/query/, with codes 111 Education, 112 Basic education, 113 Secondary education, and 114 Postsecondary education.

[21] The three pillars of prosperity according to Besley and Persson (2011) are tax capacity, legal capacity, and an absence of internal conflict.

TABLE 5.—IMPACT OF JUDICIAL REFORMS ON FIRM PRODUCTIVITY

	(1)	(2)	(3)	(4)	(5)
Judicial reform × Post	14.18	−0.08	−7.37	−4.90	−0.52
	(12.02)	(14.61)	(14.28)	(17.97)	(15.32)
Judicial reform × Post × Specific	17.94*				
	(10.02)				
Judicial reform × Post × Herfindhal Sweden		29.20*			
		(16.46)			
Judicial reform × Post × Input/output Sweden			39.30**		
			(15.42)		
Judicial reform × Post × Herfindhal Continent				34.64*	
				(17.93)	
Judicial reform × Post × Input/output Continent					25.04
					(17.60)
Observations	47,041	57,155	60,844	82,007	82,007
Country fixed effects	Yes	Yes	Yes	Yes	Yes
Sector fixed effects	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes

OLS regressions. Robust standard errors in parentheses, clustered at the country level. $^{***}p < 0.01$; $^{**}p < 0.05$; and $^{*}p < 0.1$. The dependent variable in all columns is the value added per worker, expressed in thousand U.S. dollars PPP. In column 1, Specific is a dichotomous variable equal to 1 if goods are specific, that is, it would take more than a month for the main customer to find the same good were the seller to shut down its operations. In column 2, Herfindhal Sweden is 1 minus the Herfindahl index for input use, that is, 1 minus the sum of shares of each of the nine inputs (labor, raw materials and intermediate inputs, electricity, communications services, fuel, transport for inputs, water, rental of land/buildings, equipment, furniture) among total input value to the squared, estimated for each sector disaggregated to the fourth digit of the ISIC code in Sweden. Firms are classified in the generic or specific sectors if their Herfindhal index is above or below the median. In column 3, "Input/Output Sweden" is the ratio of the total value of inputs used divided by the value of output, disaggregated for sectors at the four-digit level of the ISIC code in Sweden. Firms are classified in the generic or specific sectors if their input/output ratio is above or below the median. "Herfindhal Continent" and "Input/Output Continent" are defined similarly but instead of using Sweden as the benchmark country, I use data from the two countries with the best judiciaries in the same continent.

G. *Robustness Checks on Measures of Relationship-Specific Investment*

In table 5, I use other indices of relationship-specific investments used in the literature to check whether the results still hold. Instead of using $Specific_j$, which measures the specificity of goods produced from the Germany "Innovation" round (column 1), I use an index of input complexity (1 minus the Herfindahl index of input use), which can be contrued as a measure of the complexity of the input mix and the reliance on contract enforcement mechanisms, according to Levchenko (2007) (see greater details in appendix G for the exact calculation). I compute this indicator using the Swedish Enterprise survey (Sweden is ranked ninth in the world, and there is no German World Bank Enterprise Survey in the standardized data set). The results are similar, indicating that firms requiring a more complex input mix (thereby relying more on contracts) greatly benefit from judicial reforms.

Results are also similar when using the ratio of inputs to output (firms using more inputs into their production rely more on contracts) in column 3, or when calculating the Herfindahl index and the input/output ratio not from Sweden but from the two countries per continent with the best judiciaries as benchmarks in columns 4 and 5. The results of columns 4 and 5 are similar to previous ones. In fact, they show a larger impact of judicial reforms. The remarkable finding of table 5 is that the results remain the same regardless of sample size (from 47,041 to 82,007), the sector of firms considered (manufacturing or all sectors), the benchmark country (Germany, Sweden, or the two countries with the best judiciaries within the continent), or the indicator of the relationship-specific investment used (specific versus generic output, input complexity, or input to output ratio).[22]

H. *Other Robustness checks*

The empirical framework developed in this paper can be used to answer important questions about the effects of judicial reforms in different contexts. In column 2 of table E1, I investigate whether comprehensive judicial reforms also work in the context of poorly functioning judiciaries. The theory predicts that if the suing condition (1) does not hold, $p\beta^T < l_p$, and there are no incentives to sue, then it is unclear whether comprehensive judicial reforms have any effect. To have an effect, reforms must be large enough in terms of their effect on p, T, and l_p to reverse this inequality. One can investigate this by looking at the effects of judicial reforms in countries where $p\beta^T < l_p$. In column 2 of table E1, I find that comprehensive judicial reforms work even in contexts with poorly functioning judiciaries.

In column 3, I investigate whether comprehensive, externally funded judicial reforms also work in oligarchies. In terms of the model, the extreme case of an oligarchy under a limited commitment issue implies that $p = 0$, $T = \infty$, and l_p is high. Here it is unclear whether judicial reforms have any effect. Moreover, the elite might be able to undermine the effects of externally funded judicial reforms by other means. To capture the oligarchic nature of a regime, I use data from Polity IV and find that large and comprehensive externally financed judicial reforms work, even in oligarchies.

In table H1 in the appendix, I show that the results remain similar when including country-specific trends, when considering the sum of comprehensive and limited reforms (instead of dichotomous variables), or when the budget threshold to be considered a comprehensive judicial reform is changed to 1%, 3%, or 7%.

I also adapt the randomization inference Fisher test to this particular setting of a triple difference. Young (2018) shows that the Fisher test is both exact (i.e., with a distribution that is known no matter what the sample size or the characteristics of errors) and intrinsically resilient to outliers. In this paper, I randomly permute the comprehensive and limited reforms at the level of countries. I then estimate the model. This generates a simulated average treatment effect, along with a simulated p-value. Doing this 1,000 times produces

[22] In appendix G, I present another robustness check using the measure developed by Nunn (2007). The issue with this measure is that it uses the Input-Output table of the U.S. Bureau of Economic Analysis, which uses the North American Industry Classification System (NAICS) industry classification, which is different from the International Standard Industrial Classification (ISIC), Rev 3.1 of the World Bank Enterprise Surveys. There is no exact correspondence between the two codes. In appendix G, I present a test using my own correspondence between the codes. The advantage of the test presented in my paper is that it uses only World Bank Enterprise Surveys that use the ISIC Rev3.1 code, with no issues of code correspondence. The matching of sectors between different data sets and different countries (Germany, Sweden, and other countries) is thus straightforward since all firms use the same code.

an empirical distribution of the *p*-values. The Fisher *p*-value is the number of times the observed *p*-value is lower than the simulated *p*-value. The Fisher *p*-value corresponding to the preferred specification in column 8 of table 3 is 0.09. This shows that the triple-difference coefficient is still significant with a Fisher test.

Finally, in table H2 of the appendix, I use other measures of firm output and profit to show that the results are not driven by the choice of a dependent variable. I also show that the effect is not driven by the accumulation of physical or human capital or by an increase in the number of workers. This points to the fact that the effect of institutions such as the judiciary is captured in the total factor productivity term, in line with Hall and Jones (1999).

VI. Conclusion

In this paper, I use a triple-difference analysis to compare firms in countries with or without judicial reforms, before and after the reforms, and in sectors relying more or less on judiciaries due to their need for relationship-specific investments. I assemble a world database of 4,568 judicial projects and codify each reform according to the characteristic targeted—quality, speed, or access of the judiciary—and budget size. For example, the World Bank Judicial Performance Improvement Project (JPIP) in Kenya trained court officials and streamlined procedures (quality), introduced performance management contracts designed to reduce the backlog of cases (speed), and built courts (for greater access). This greater quality, speed, and access of the courts may have increased the incentives to sue when confronted with a dispute, which could in turn have decreased the incentives to start a dispute in the first place. This may be especially important for firms engaged in relationship-specific investments that depend more on contract enforcement mechanisms.

In line with this argument, I find that judicial reforms significantly improve perceptions of judicial efficiency by 0.15 standard deviation for all firms, and firm productivity by 22% in sectors requiring more relationship-specific investments. The effect is positive but not significant for firms in sectors requiring fewer relationship-specific investments.

This paper contributes to the debate about the importance of the judiciary in the process of economic growth. On one hand, North (1990) argues that "the inability of societies to develop effective, low cost enforcement of contracts is the most important source of both historical stagnation and contemporary underdevelopment." On the other, it is easy to think of alternative arrangements to avoid the adverse effects of weak contracting institutions (Acemoglu & Johnson, 2005; Greif, 1993).

I provide support for both views articulated around the need for relationship-specific investments. If the output is generic, with many potential buyers, then a seller can circumvent the issue of ex-post renegotiation by turning to other buyers. In this case, the judiciary may matter less. I find empirical support for this in the data: judicial reforms improve firms' perceptions of the judiciary but have no significant effect on the productivity of firms dealing in generic goods.

In contrast, if the output is specific, with only one buyer, then a seller has no way to avoid the ex-post renegotiation by that one buyer. In such a case, the judiciary is more important. I find support for this in the data: judicial reforms have a large, positive effect on firms dealing in specific goods. In conclusion, the judiciary may be less important for generic goods, but it is crucial for the emergence of more specific and complex types of goods produced—the development of a modern economy.

Finding an effect of these judicial reforms was not a foregone conclusion. I show in this paper that in oligarchies, the elite in power have no incentives to implement domestic reforms; instead, they may have incentives to undermine externally financed judicial reforms to benefit themselves or their cronies. Judicial reforms leaving politics unchanged may achieve nothing. In this paper, I show that these judicial reforms have some effects, even in oligarchies.

This paper focused only on firm productivity; however, an important avenue for future research is to check whether these judicial reforms also have political effects by checking the power of the executive (La Porta et al., 2004), which could have further dramatic consequences on the economy. If the judiciary can be a check on the executive, this raises the question of why externally financed judicial reforms would be adopted in the first place. The JPIP may have been implemented following a critical juncture in Kenya (a new, progressive constitution due to grave ethnic conflicts), yet the judiciary is currently being undermined by the Kenyan executive. Despite the positive effects on the economy documented in this paper, there may be political obstacles to the implementation of judicial reforms.

This paper is important on both academic and policy levels. I find that $5.4 billion has been spent on 4,568 judicial projects implemented since 1996 by 500 agencies. To date, there has been no rigorous impact evaluation of these costly interventions. This paper is the first to systematically collect data on these judicial reforms. A practitioner involved in judicial reforms once said, "We know how to do a lot of things, but deep down we don't really know what we are doing" (Carothers, 2006, 5). This paper is the first to find large effects of these judicial reforms on firm productivity. In particular, an important implication for practitioners is that comprehensive reforms—those targeting all characteristics at once (quality, speed, and access) and with a budget size above 5% of a country's judiciary—affect firm productivity. In contrast, I find that limited reforms—those that do not target all characteristics at once or with a small budget—have no effects. The intuition is that increasing access to an otherwise slow and corrupt judiciary does not fundamentally affect outcomes. Therefore, comprehensive rather than limited judicial reforms should be favored.

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