Access to Justice and Economic Development: Evidence from an International Panel Dataset

Arnaud Deseau Adam Levai ✉ Michèle Schmiegelow*

March 2022

Abstract

This paper evaluates the importance of access to justice (ATJ) for economic growth. We create a new database on the number of professional judges per 100,000 inhabitants by collecting data from various public institutions. We draw on these data as a macro-level proxy capturing the fundamental and historical evolution of ATJ from 1970 to 2014 for a wide range of developed and developing countries. Using an instrumental variable approach in a dynamic panel setting to deal with endogeneity, we show that ATJ is a positive and significant determinant of economic growth. The substantial aggregate effect of ATJ is inversely related to the initial level of income per capita, human capital, democracy, economic freedom, and the rule of law. These results hint at a stronger impact of ATJ on economic development in less developed societies. In terms of mechanisms, our results suggest that ATJ promotes growth via higher government accountability and improved quality of institutions.

Keywords: Access to Justice, Economic Growth, Institutions

JEL Codes: K00, O11, O43.

*Deseau: CEREC, Université Saint-Louis – Bruxelles, 38 Boulevard du Jardin Botanique, 1000 Bruxelles, Belgium and IRES/LIDAM, UCLouvain, College L. H. Dupriez, 3 Place Montesquieu, B-1348 Louvain-la-Neuve, Belgium (e-mail: arnaud.deseau@uclouvain.be); **Levai**: Corresponding author, IRES/LIDAM, UCLouvain, College L. H. Dupriez, 3 Place Montesquieu, B-1348 Louvain-la-Neuve, Belgium (e-mail: adam.levai@uclouvain.be); **Schmiegelow**: CRIDES, UCLouvain, College Thomas More, 2 Place Montesquieu, B-1348 Louvain-la-Neuve, Belgium (e-mail: michele.schmiegelow@uclouvain.be). We would like to thank Alexia Autenne, David de La Croix, Frédéric Docquier, Filip Dorssemont, Joseph Gomes, Alexandre Girard, Rafael La Porta, Hélène Latzer, William Parienté, Pierre Pecher, and Henrik Schmiegelow for their valuable comments and suggestions. We also thank the participants at seminars and workshops at University of Bonn, UCLouvain, Université Saint-Louis – Bruxelles, Erasmus University Rotterdam, KULeuven, Université Clermont-Auvergne, Université Paris-Nanterre, University of Warsaw and Spring Meeting of Young Economists for helpful comments and suggestions. We are grateful to: the Fonds Quetelet (Brussels) for granting us access to its archives, which include a cross-country collection of statistical yearbooks; the Ministries of Justice in Botswana, Belgium, Croatia, Estonia, Finland, Ireland, Latvia, Mali, Malta, New Zealand, Niger, Poland, Romania, Slovenia, South Korea, Sweden, Uruguay, Venezuela; and to the National Statistical Service of Australia, Armenia, Qatar, and United States for sharing their official data with us. This work was supported by the Fonds de la Recherche Scientifique - Projet de Recherches (FNRS-PDR), under the grant number "26036434." The usual disclaimers apply.

"*Promote peaceful and inclusive societies for sustainable development, provide access to justice for all and build effective, accountable and inclusive institutions at all levels*"

Sustainable Development Goal 16 - United Nations (2015)

1 Introduction

The last decade has seen a growing recognition of access to justice (ATJ) as one of the major challenges in the development of peaceful and prosperous societies for the coming years (United Nations, 2015; OECD and Open Society Foundations, 2016). A study conducted by the World Justice Project (2019) estimates that as much as 5.1 billion people – or approximately two-thirds of humanity – face issues related to ATJ. The obstacles to reaching justice are numerous, ranging from time and costs factors to discrimination of all kinds; they impede the accurate delivery of justice in civil, administrative, and criminal matters. This has direct and strong implications for peoples' everyday decisions in the most fundamental economic operations. Consequently, such obstacles to justice generate substantial economic costs and slows down economic development in both developed and developing societies.[1] The judiciary, both as a source of judge-made law and as a legal enforcement institution, shapes the incentives of economic actors and actively participates in the enforcement of contracts and the protection of property rights, which are recognized as the cornerstones of a well-functioning market economy (North, 1990; Acemoglu et al., 2005). Although existing contributions to the literature have stressed the importance of legal codes (La Porta et al., 2008) as well as effective or *de facto* judicial institutions for economic development (Djankov et al., 2003; Jappelli et al., 2005; Chemin, 2009a,b; Visaria, 2009; Chemin, 2012; Feld et al., 2015), surprisingly little is known about the structural importance of ATJ for economic development.

This paper fills that gap in the literature by showing that ATJ is an important determinant of economic growth. To do so, we construct a new database on the number of professional judges per 100,000 inhabitants by combining data from various public institutions. We use these data as a macro-level proxy capturing the fundamental and historical evolution of ATJ between 1970 and 2014 for a wide range of developed and developing countries. In a dynamic panel setting using an instrumental variable approach to deal with endogeneity, we show that ATJ is a positive and significant determinant of economic growth. The benchmark estimates imply that increasing ATJ by 1% increases the five-year growth rate of GDP per capita by 0.7pp. This substantial aggregate effect is inversely related to the initial level of GDP per capita, human capital, democracy, economic freedom, and rule of law, meaning that less developed societies are the ones that can gain the highest economic returns from improving ATJ. In terms of mech-

[1] A study by OECD and World Justice Project (2019) estimates that the costs generated by legal problems are between 0.5% to 3% of GDP in most countries.

anisms, our results suggest that part of the effect of ATJ on growth comes from higher government accountability and quality of institutions. In particular, we show that ATJ negatively affects the share of government consumption in GDP, public corruption, and the shadow economy while it positively affects the protection of property rights and credit market regulations. These results are consistent with the existing literature showing that institutions and their quality is one of the main determinants for economic development (Acemoglu et al., 2005).

Our paper makes three primary contributions. First, we complement the existing literature on the effect of judicial institutions on economic development by providing the first study looking at the effect of ATJ on economic growth in a large and long panel of countries. Most of the existing studies evaluating the impact of ATJ-related measures on economic development are country-specific. For example, Lichand and Soares (2014) find that the creation of new special civil courts in Brazil foster entrepreneurship by increasing the geographic proximity and improving the speed of the justice system. Espinosa et al. (2018) evaluate a reform that closed a quarter of French labor courts, and show that cities which experienced an increase in the distance to their associated labor courts were creating less jobs and less firms, compared to unaffected cities. Our findings in an international setting are also complementary to country-specific studies revealing the importance of court effectiveness for entrepreneurship, credit, agricultural, and industrial activities (Jappelli et al., 2005; Chemin, 2009a,b; Visaria, 2009; Chemin, 2012; Amirapu, 2021). Our research is also related to other papers explaining cross-country differences in GDP per capita with *de facto* judicial indicators. For instance, Berkowitz et al. (2003) find that the way a legal code is implemented matters more to national economic development than the legal family of the code. Feld and Voigt (2003) and Feld et al. (2015) show that *de facto* judicial independence is significantly correlated with economic growth while mere *de jure* judicial independence is not.

Second, we contribute to the existing literature by creating a new database on the number of professional judges per 100,000 inhabitants for a sample of 105 countries from 1970 through 2014. We draw on these data as a new structural indicator of ATJ, proxying its historical evolution at the country level. Compared to available measures of ATJ, our proxy has the great advantage of having a much longer time dimension, enabling us to capture and study the effect of long variations in ATJ. This is crucial for identification, as we exploit the within-country variation in ATJ to estimate its effect on economic development. We show that our new ATJ indicator is highly correlated with the more comprehensive (but shorter in the time dimension and using mostly qualitative data) access to civil justice score of the World Justice Project (2020); while it is not correlated with other dimensions of judicial performance at the country-level (such as the independence of the judiciary or the rule of law). We also produce a series of new stylized facts from our database, highlighting sizeable differences in regional trends as well as in the levels of the density of judges across countries.[2] The new dataset has the advantage of using only publicly available data sources with quantitative measures of ATJ, as opposed to relying only on qualitative measures made by legal experts opinions. The public data sources include various public institutions (national statistical offices, public archives, ministries of justice, and supreme courts), academic publications, and international organizations (UNOV, CEPEJ, UNODC, OAS, and Eurostat).[3]

Third, we advance the existing cross-country literature on the impact of *de facto* judicial institutions on economic development by addressing endogeneity issues in a more comprehensive way. Most of the studies rely solely on a fixed-effects estimator to evaluate the impact of various *de facto* judicial changes on economic development. This strategy solves omitted variable bias to a certain extent, but it does not address the reverse causality bias stemming from high correlation between economic development and *de facto* judicial institutions. Taking advantage of the long panel dimension of our new database, we use a difference-GMM approach (Arellano and Bond, 1991) which relies on first-differences and internal instrumentation to deal with endogeneity. As an additional piece of evidence, we use "politically motivated increase in the number of judgeships" from the V-Dem dataset (Coppedge et al., 2019) as an external instrument for ATJ, and we find similar results.

The rest of the paper is organized as follows. Section 2 discusses and tests the validity of our proxy for ATJ and describes the novel dataset on the number of judges along with stylized facts. Section 3 presents our identification strategy and discusses the main challenges for the empirical analysis. Section 4 presents our main results, robustness checks, heterogeneity, and the transmission channels through which ATJ can affect growth. Section 5 presents our conclusion.

2 Access to Justice: New Dataset and Stylized Facts

This section is organized in two parts. In Section 2.1, we discuss the concept of ATJ, explain how we measure it at a macro level and show that our proxy is both conceptually and empirically valid at that level. Then, in Section 2.2 we explain how we create our new ATJ dataset, and provide stylized facts on ATJ.

2.1 Theory and Measurement

ATJ is a broad and multidimensional concept. It is not surprising, therefore, that there is no commonly accepted definition of ATJ among legal scholars. Nevertheless, we can identify several core elements pertaining to the concept: (i) *Legal capability* – a citizen needs to know the law, legal procedures, and which person to contact in order

[2] For brevity, we will sometimes refer to "judges" instead of "professional judges".
[3] See Appendix Table B-1 for a detailed description of the sources used.

to introduce a lawsuit or defend oneself against one; (ii) *Availability of justice* – a citizen needs to be able to rely on a sufficient and available judicial supply (infrastructures and staff); (iii) *Physical barriers* – a citizen needs to have access to means of transport to a court of justice and his or her legal advisors; (iv) *Social barriers* – a citizen needs to be free of the fear of incurring any discrimination during the judicial process; (v) *Cost* – a citizen needs to be able to pay the monetary cost of the procedure, to support the opportunity cost of the procedure and to endure the emotional cost of the procedure (see Barendrecht et al. (2006), Botero and Ponce (2011) and OECD and Open Society Foundations (2019) for a complete discussion about ATJ and its economic dimensions). Overall, this multi-dimensionality makes ATJ difficult to quantify due to the need to collect, aggregate, and make weighting decisions for many variables.[4] Moreover, even though various datasets on ATJ are available at the national level, there are no such datasets including both a large cross-country aspect and a long time dimension.[5] One example is the Rule of Law Index from The World Justice Project, which has access to civil justice as one of its sub-components. Despite a rich cross-country dimension, the time dimension for ATJ is short in that case, as data are available only from 2015. In addition, ATJ is very persistent on such a narrow time interval, making it hard to study potential effects on economic development.

In this paper, we focus on a specific and structural indicator of ATJ, i.e. the opportunity for citizens to have access to the judiciary. We measure and argue that the number of judges per 100,000 inhabitants is a good proxy for capturing the fundamental and historical evolutions of ATJ at the country-level. A higher number of judges relative to population directly increases the available judicial supply (component ii) and general awareness of the law (component i). An increase in the density of judges can also coincide with the creation of new courts, influencing the accessibility (iii), and cost dimensions (v) of ATJ. Our ATJ proxy has the advantage of being widely available and comparable across countries and through time by the fact that judges have the universal role of supplying justice: they resolve disputes in a court of law across all legal systems. Data on the number of judges we use are gathered from official sources such as public institutions, international organizations, and academic publications (see Section 2.2 and Appendix B for more details).

Staats et al. (2005) lists five dimensions, including ATJ (the accessibility dimension), that one can measure to assess judiciary performances: (i) *independence*, (ii) *accountability*, (iii) *efficiency*, (iv) *effectiveness*, and (v) *accessibility*. Because of judges' central position in the judicial decision-making process, one concern is that our proxy might reflect other judicial characteristics at the country-level, rather than just ATJ. We first empirically validate our proxy showing that there is a robust correlation between the density of judges and the accessibility

[4]The OECD published a note mapping 14 distinct dimensions and more than 40 sub-dimensions for a comprehensive ATJ indicator (OECD and Open Society Foundations, 2019).

[5]One of the main reasons is that the methodology used for each national survey is different, limiting the possibility of cross-country comparisons. See OECD and Open Society Foundations (2019) for a discussion about the most recent legal needs surveys.

dimension, while not being correlated with the other dimensions of judicial performance. To do so, Table 1 looks at the cross-country correlation between the number of judges per 100,000 inhabitants and variables capturing the aforementioned dimensions of judicial performance identified by Staats et al. (2005). Panel B, column 5 reveals a highly significant and positive relationship between our proxy of ATJ and the access to civil justice as measured by the World Justice Project for the year 2014.[6] Figure 1 depicts that relationship. Our proxy explains as much as one-third of the cross-country variation in access to civil justice. In the subsequent columns, we control for potential confounders such as income per capita, government spending, political regime, human capital, legal origin or geographical characteristics. In each case, the correlation between our proxy and the access to civil justice as measured by The World Justice Project remains robust. At the same time, we find that our ATJ proxy is not robustly correlated across other dimensions of judicial performance. Following Staats et al. (2005) classification, we find no robust correlation between the density of judges and independence (Panel A - columns 1-4), accountability (Panel A - columns 5-8) or effectiveness (Panel B - columns 1-4) of the judiciary.[7] We also find no robust correlation between our proxy and two measures of the overall quality of the judiciary from the World Bank (Panel C - columns 1-8).

By focusing our analysis on the density of judges, we essentially measure access to a formal court of justice and not alternative or informal dispute resolution mechanisms. Informal justice and out-of-court settlements play an important role in some developing and developed countries (Galanter, 1981; Platteau, 2000). This is a possible concern as we could underestimate the level of ATJ with our proxy in some countries. However, many alternative dispute resolution mechanisms, such as mediation, require court-backed enforcement and ultimately the intervention of formal justice. In the context of competition between informal and formal legal institutions, Aldashev et al. (2012) show that formal laws can act as an outside anchor, or a "magnet," moving the custom in a favorable direction for the marginalized groups: if access to formal justice is improved, access to informal justice will improve as well. For our empirical results, this means that the presence of informal justice will lead only to an attenuation bias on the estimated effect of ATJ on economic growth, as we tend to underestimate the level of ATJ in a given country.

Due to data availability, we focus on judges in all types of courts (first instance, second instance, and supreme courts) and dealing with all types of cases (civil, criminal, and administrative). This is not a concern for our analysis for several reasons. Firstly, judges often act as generalists: they adjudicate all relevant cases of civil, criminal, and public law. Only in large courts and developed economies do judges tend to specialize in one specific domain (e.g., marital matters for civil cases). Secondly, we cannot say a priori that judges in specific types of courts, dealing

[6] The World Justice Project defines access to civil justice as: "the accessibility and affordability of civil courts, including whether people are aware of available remedies; can access and afford legal advice and representation; and can access the court system without incurring unreasonable fees, encountering unreasonable procedural hurdles, or experiencing physical or linguistic barriers" (World Justice Project, 2020).

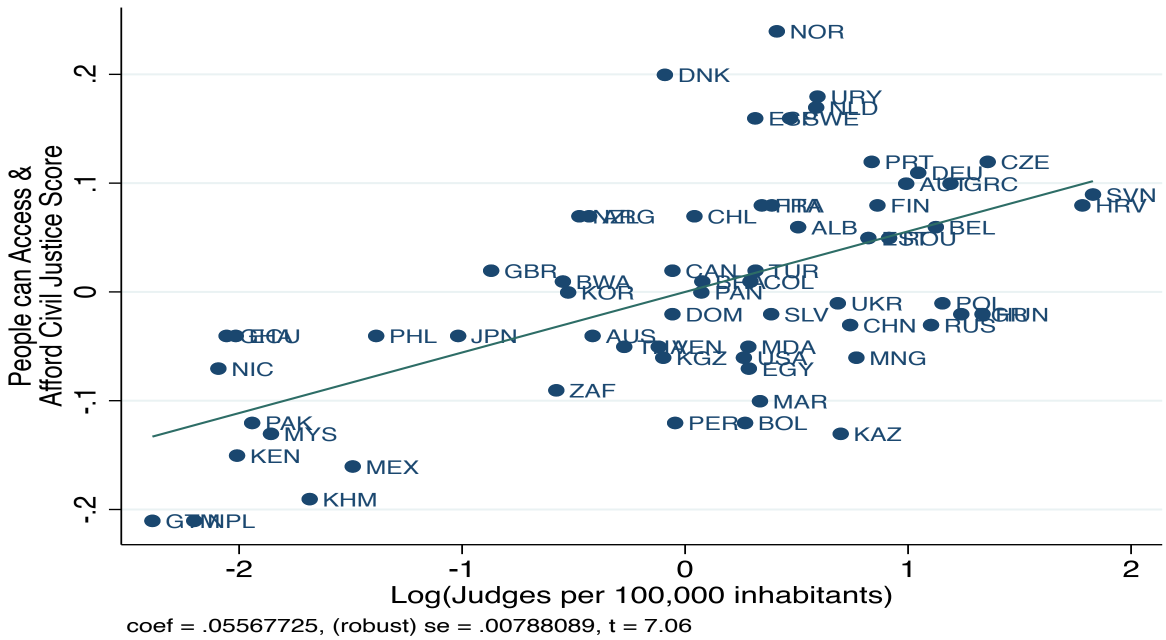
[7] We do not include a measure of judicial efficiency as there are no data available for a sufficient number of countries. For a complete discussion on judicial efficiency and judicial effectiveness, see Marciano et al. (2019).

Table 1: The Link between the Density of Judges and Judicial Performances

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
PANEL A	**Judicial Independence**				**Corruption Free Judiciary**			
$log(Judges\ per\ 100k\ inh.)$	0.036*	-0.045	0.005	0.002	0.082***	-0.027	0.026	0.040*
	(0.021)	(0.028)	(0.025)	(0.027)	(0.020)	(0.023)	(0.021)	(0.024)
$log(GDP_{pc})$		0.192***	0.105***	0.109***		0.247***	0.218***	0.213***
		(0.020)	(0.032)	(0.028)		(0.017)	(0.024)	(0.024)
$Gov.\ Cons.$		0.049	-1.108**	-1.149**		0.243	-0.594	-0.651
		(0.513)	(0.456)	(0.461)		(0.491)	(0.501)	(0.478)
$Polity2$			0.013***	0.021***			0.007	0.015***
			(0.005)	(0.006)			(0.004)	(0.005)
$Schooling$			-0.018	-0.014			-0.028***	-0.019**
			(0.012)	(0.011)			(0.009)	(0.009)
Observations	64	64	64	64	64	64	64	64
adj. R-sq	0.02	0.49	0.69	0.71	0.11	0.73	0.83	0.85
PANEL B	**Log(Time Enforcing Contract)**				**Access to Civil Justice**			
$log(Judges\ per\ 100k\ inh.)$	-0.039	0.056	0.137**	0.114	0.056***	0.034***	0.036**	0.038**
	(0.046)	(0.054)	(0.068)	(0.072)	(0.008)	(0.013)	(0.015)	(0.018)
$log(GDP_{pc})$		-0.100***	-0.025	-0.032		0.059***	0.037**	0.036**
		(0.037)	(0.062)	(0.066)		(0.011)	(0.014)	(0.015)
$Gov.\ Cons.$		-1.951**	-0.780	-0.500		-0.130	-0.190	-0.337
		(0.889)	(0.909)	(1.003)		(0.284)	(0.282)	(0.283)
$Polity2$			0.004	-0.004			0.005	0.006*
			(0.008)	(0.010)			(0.003)	(0.003)
$Schooling$			-0.052*	-0.052			-0.002	0.000
			(0.029)	(0.032)			(0.006)	(0.006)
Observations	83	83	83	83	64	64	64	64
adj. R-sq	-0.00	0.06	0.16	0.20	0.33	0.55	0.65	0.63
PANEL C	**Rule of Law (WB)**				**Quality of Judicial Processes**			
$log(Judges\ per\ 100k\ inh.)$	0.338***	-0.049	0.035	0.046	1.041***	0.128	0.048	0.206
	(0.076)	(0.087)	(0.097)	(0.080)	(0.247)	(0.339)	(0.379)	(0.382)
$log(GDP_{pc})$		0.781***	0.511***	0.561***		1.400***	0.352	0.352
		(0.083)	(0.089)	(0.090)		(0.336)	(0.519)	(0.515)
$Gov.\ Cons.$		1.011	-2.335*	-2.704**		10.495	7.146	3.004
		(1.625)	(1.257)	(1.271)		(7.318)	(8.430)	(8.680)
$Polity2$			0.040***	0.054***			0.018	0.079
			(0.013)	(0.017)			(0.087)	(0.114)
$Schooling$			-0.057	-0.040			0.250	0.252
			(0.040)	(0.037)			(0.225)	(0.226)
Observations	83	83	83	83	83	83	83	83
adj. R-sq	0.10	0.62	0.76	0.80	0.11	0.27	0.33	0.31
Legal origin controls	No	No	Yes	Yes	No	No	Yes	Yes
Geographical controls	No	No	Yes	Yes	No	No	Yes	Yes
Continent fixed effects	No	No	No	Yes	No	No	No	Yes

Note: This table presents OLS estimates of the relationship between the density of judges and different measures of judicial performance at the country-level as defined by Staats et al. (2005). In all panels, the density of judges is defined as log number of professional judges per one hundred thousand inhabitants. Panel A uses an indicator of judicial independence (columns 1-4) and corruption free judiciary (columns 4-8) as dependent variables to assess the (i) independence and the (ii) accountability dimensions. Both indicators are from Gutmann and Voigt (2018). Panel B uses an indicator of judicial effectiveness defined as log number of days required to enforce a contract (columns 1-4) and an indicator of access to civil justice (columns 5-8) as dependent variables to assess the (iv) effectiveness and the (v) accessibility dimension. Data come respectively from World Bank's Doing Business indicators and from The World Justice Project - Rule of Law Index both in year 2014. Panel C uses proxies of the overall quality of justice defined as the rule of law index (columns 1-4) and a composite index of judicial quality (columns 5-8) as dependent variables. Indicators come respectively from the World Bank's Governance Indicators and the World Bank's Doing Business in year 2014 and 2018. Column 1 only includes log judges per 100,000 inhabitants as regressor. Column 2 adds log GDP per capita and the share of government consumption in GDP. Columns 3 adds Polity2 score of democracy, years of schooling, legal origin, and geographical characteristics (absolute latitude, total land area, standard deviation of elevation, and mean distance to coast or river). Finally, column 4 adds continent dummies. See Appendix A for further information on the variables. Standard errors clustered at the country-level are in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Figure 1: Correlation between World Justice Project's Access to Civil Justice Index and the Density of Judges



Note: This figure plots the relationship between log number of professional judges per 100,000 inhabitants (averaged between 2000-2014) and the access to civil justice score of *The World Justice Project* (year 2014). This adjusted partial residual plot is based on the OLS regression in Table 1, Panel B, column 5.

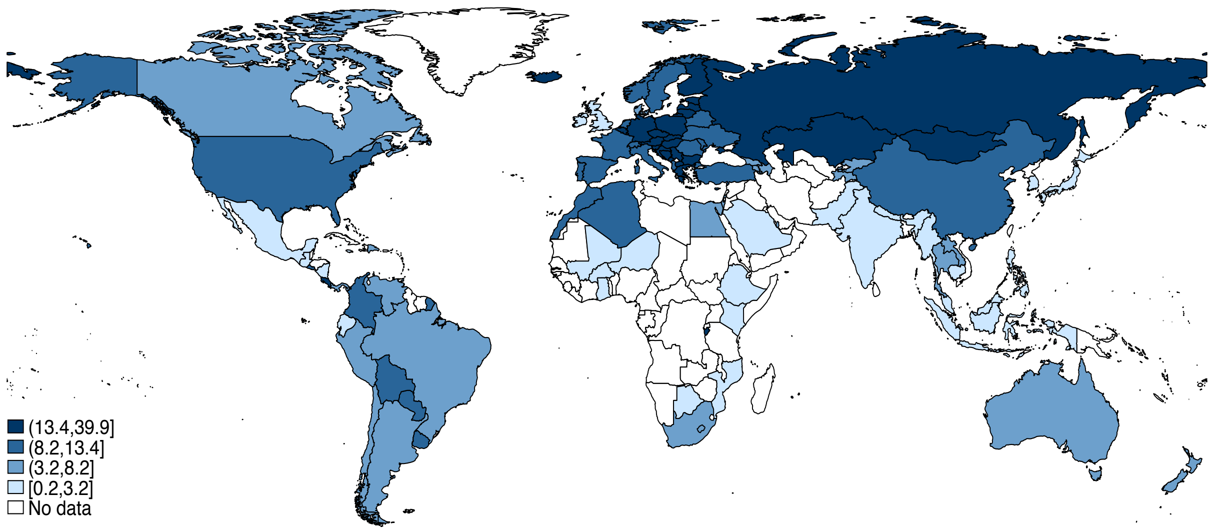
with certain types of cases, are not important for economic growth. However, we can argue that judges dealing with civil cases are potentially the most important for economic development, since civil cases represent the majority of all legal cases, and they are directly related to economic matters (Pleasence et al., 2013). As previously discussed, Table 1 shows that our proxy effectively captures access to civil justice as measured by the World Justice Project, which is reassuring.

In our empirical exercise, we focus on the quantity rather than the quality of judges as measured by judges' education and experience. This can be a problem as the quality of judges can impact simultaneously ATJ and affect economic growth (through higher court productivity) (Ramseyer, 2012; Bielen et al., 2018). We tackle this omitted variable problem by explicitly controlling for the mean years of schooling in our regressions, and by including country fixed effects to capture time-invariant country specificities in terms of judges' education or training.

2.2 Data and Stylized Facts

We create a new database on the density of judges by collecting data from various public institutions. First, we gather data from ministries of justice, supreme courts and national statistical offices. We consider these sources as the most reliable, given that national institutions are often the first issuers of judicial data. As the second most reliable data source category, we consider international organizations that are long recognized for their expertise on various judicial indicators such as the United Nations Office in Vienna (UNOV), the Commission Européenne pour l'Efficacité de la Justice (CEPEJ), the United Nations Office on Drugs and Crime (UNODC), the Organization of American States (OAS), and the European Statistical Office (Eurostat). As the third most reliable data source category, we use individual academic publications. We merge these three data source categories and take care of the potential spurious variation implied by pooling different sources together. As a result, we end up with an unbalanced panel of judges for 105 countries from year 1970 through 2014. In Appendix B, we provide a complete description of the merging process, sources and definitions (Appendix Table B-1), and descriptive statistics (Appendix Table B-2).

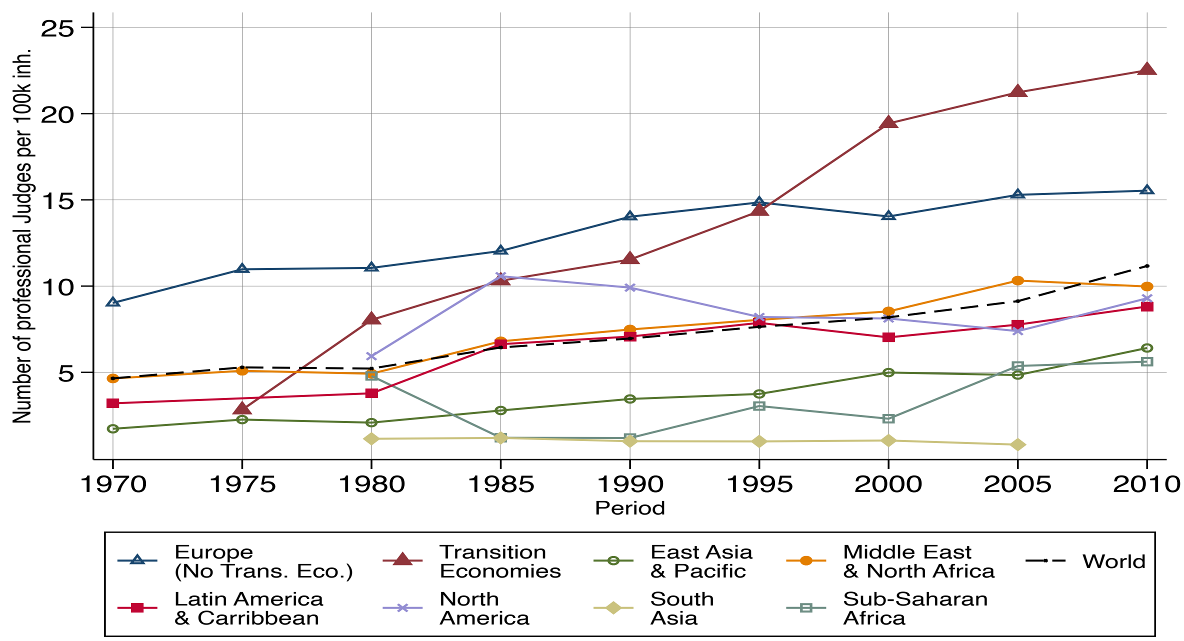
Figure 2: Average Density of Judges around the World between 1970-2014



Note: The map presents the country-level distribution of the number of professional judges per 100,000 inhabitants (averaged between 1970-2014) for 105 countries. Each color represents a quartile from the first (light blue) to the fourth (dark blue).

Figure 2 gives an overview of our database by plotting the average density of judges over the whole 1970-2014 period by quartiles. We can see two things: (1) the 105 countries for which we managed to collect at least one country-year observation, and (2) the high cross-country variation in the average number of judges per 100,000 inhabitants. The highest average density is reached in Montenegro with 40 judges per 100,000 inhabitants, while the lowest density is in Ethiopia with only 0.25 judges per 100,000 inhabitants. Even within-continents, we observe sizeable variations. In Europe, for example, we find both countries belonging to the top quartile (e.g., Germany or Serbia) and countries belonging to the bottom quartile (e.g., United Kingdom or Ireland). An interesting finding is that the top decile of the distribution is composed almost entirely of central and southeastern European countries.[8] Cross-country variations in the density of judges are driven by differences in factors such as GDP per capita, legal origin, culture, and ethnic composition of the population.[9] The legal origin is one key variable that helps us understand the systematic difference in the density of judges between two countries – for instance Germany (civil law) and the United Kingdom (common law) – that have similar income per capita, population size and level of democracy.

Figure 3: The Evolution of the Density of Judges across Regions between 1970-2014



Note: This figure plots trends of the number of professional judges per 100,000 inhabitants between 1970-2014 in 5-year averages. The graph covers 105 countries categorized into eight regions plus the World. Country classifications are identical to the World Bank with the exception of Middle East & North Africa region for which we include Cyprus and Turkey, and the additional group of Transition Economies that are composed of Albania, Armenia, Azerbaijan, Belarus, Bosnia and Herzegovina, Bulgaria, Croatia, Czech Republic, Estonia, Georgia, Hungary, Kazakhstan, Kyrgyz Republic, Latvia, Lithuania, Macedonia, Moldova, Montenegro, Poland, Romania, Russia, Serbia, Slovak Republic, Slovenia, Tajikistan, and Ukraine, following the IMF (2000) classification.

Figure 3 depicts the time variation (in five-year averages) of the density of judges across different groups of

[8] The top decile accounts for more than 21.8 judges per 100,000 inhabitants and is composed of the following countries: Bosnia and Herzegovina, Croatia, Czech Republic, Germany, Hungary, Luxembourg, Macedonia, Montenegro, Serbia, Slovak Republic, and Slovenia.

[9] See Appendix Table C-8 for more details on the determinants and correlates of the density of judges in our sample.

countries and the world average. Even though during a period of 45 years (from 1970 through 2014) the world population has increased, the number of judges has increased even more. The world average of the number of judges per 100,000 inhabitants has more than doubled, increasing from 4.65 to 11.17 during the analyzed period. This doubling was achieved at a quite stable growth rate of 2.2% per year on average and with clear patterns across different regions. We confirm the significant heterogeneity in the average levels that we saw in Figure 2, where Europe and Transition Economies have the highest values, whereas East Asia & Pacific, Sub-Saharan Africa as well as South Asia have the lowest levels. What is key for our identification strategy, is that we have significant differences in the growth rates of the density of judges, ranging from an threefold increase in East Asia & Pacific, to stagnation in South Asia.

Starting with Transition Economies, we document a remarkable doubling in the density of judges. Transition economies started from a density level close to the rest of European countries in the 1990 period, and reached the highest level among all the analyzed regions in the 2010 period with 22.51 judges per 100,000 inhabitants. The post-1990 doubling is mainly driven by two outcomes of the fall of the Soviet Bloc in 1991: (1) the new institutional framework and the transition to market economy created a demand for new courts and more judges;[10] (2) the population declined or stagnated in all Transition Economies since the 1990's. Considering only pre-1990 data should be treated with caution as the regional averages rely on less observations and countries compared to the post-1990 period.

The second most dynamic region is East-Asia & Pacific, where the density of judges has more than tripled, increasing from 1.73 in 1970 to 6.41 in the 2010 period. This is remarkable as the population has more than doubled during the same period in that region. Such a high investment in the justice sector was possible due to strong and stable economic growth since the 1980s, especially for East-Asian countries.

Four regions have grown in the pace of the world average, approximately doubling their density levels between the 1970 and 2010 period: Europe (excluding Transition Economies), Middle East & North Africa, Latin America & the Caribbean, and North America. European countries have the second highest level of density of judges in 2010, whereas Middle East & North Africa and Latin America & the Caribbean reached the world average levels during the considered period. As the North America country group is composed of only two countries (Canada and the United States), some variations are mechanical due to the unbalanced nature of our panel. Looking at the dates for which we observe both countries in North America (1980, 1995, and 2000), the level and trend are very close to the world average.

[10] In Russia, for example, after the fall of the USSR a constitutional court and commercial courts have been created (Hendley, 2007). Similarly, after declaring independence from Yugoslavia in 1991, Slovenia has created its new first instance courts and administrative courts (Dimitrova-Grajzl et al., 2012).

The remaining two regions (Sub-Saharan Africa and South Asia) have noted an overall stagnation. Sub-Saharan Africa has stagnated at around three judges per 100,000 inhabitants over the period 1980-2010. However, the pre-2000 trend relies on few countries due to data availability. Looking at the post-2000 period, we observe more countries, and we find that the density of judges has more than doubled, increasing from 2.31 to 5.62 judges per 100,000 inhabitants. As for South Asian countries (India, Nepal, and Pakistan), they have stagnated with the lowest density of judges with approximately one judge per 100,000 inhabitants. It is interesting to see the divergence between South Asia and East-Asia & Pacific, as the two regions had a similar density of judges in 1980. Even though both groups experienced similar population growth rates, East-Asian & Pacific countries managed to achieve a fivefold increase in the density of judges by 2005 compared to South Asian countries.

3 Empirical Strategy

Our goal is to empirically evaluate the impact of ATJ on economic development. We use a dynamic linear panel specification that is commonly used in the economic growth literature (Islam, 1995; Caselli et al., 1996; Barro, 2015; Acemoglu et al., 2019), and we adapt it to our setting with ATJ as the variable of interest. Our estimation strategy uses 5-year averages (nine time periods from 1970 through 2010) of all variables to smooth out the short-run fluctuations and to handle the annual gaps in the data. Furthermore, all our explanatory variables are lagged by one 5-year period as it takes time, particularly for ATJ, before the effect on economic activity materializes. Specifically, we focus on the following equation:

$$\ln\left(\frac{y_{i,t}}{y_{i,t-1}}\right) = \beta\ln(y_{i,t-1}) + \gamma\ln(ATJ_{i,t-1}) + \boldsymbol{\theta}'\boldsymbol{X}_{i,t-1} + \delta_t + (\alpha_i + \varepsilon_{i,t}) \tag{1}$$

where $i = 1, ..., N$ indicates a country and $t = 1, ..., T$ is a 5-year time period. Our left-hand side corresponds to a country's 5-year growth rate, with y denoting real GDP per capita. On the right-hand side, we have lagged GDP per capita y_{t-1}; our variable of interest $ATJ_{i,t-1}$ measured by the number of professional judges per 100,000 inhabitants; a vector of standard growth controls $\boldsymbol{X}_{i,t-1}$ such as schooling, investment, government expenditure, and political regime; a time fixed effect δ_t; a country fixed effect α_i; and an idiosyncratic error term $\varepsilon_{i,t}$. Our coefficient of interest is γ which captures the medium-run (5-year) effect of ATJ on economic growth.

Estimating equation (1) by OLS would produce inconsistent estimates due to endogeneity issues. First, ATJ and economic prosperity can be jointly affected by a third variable (e.g., government spending or political regime), leading to an omitted variable bias. Equation (1) treats part of the omitted variable problem by including country fixed effects which control for all time-invariant country characteristics affecting both economic growth and ATJ such as legal origin, culture, structural criminality, or geography. On top of country fixed effects, the vector $\boldsymbol{X}_{i,t-1}$ controls for time-varying growth determinants such as human capital, investment, government expenditures, and the level of democracy, further reducing the omitted variable bias. In addition, the inclusion of time fixed effects allows us to capture the world economy trends, global shocks, and the business cycle effects. Second, we face a reverse causality issue, as citizens with higher incomes have better ATJ. We mitigate this problem by lagging the ATJ variable in our specification. Another issue is the presence of a dynamic panel bias of order $1/T$, known as the Nickel bias (Nickell, 1981). In our case, this is a concern as we have a relatively short number of periods (T = 9).

To address the aforementioned biases, we estimate equation (1) using a two-step difference Generalized Method of Moments (GMM) (Arellano and Bond, 1991). This estimator differences away time-invariant country fixed effects, and relies on internal instrumentation to identify a plausible causal effect. Internal instruments are lagged levels of the endogenous variables used to instrument their current differences. The consistency of difference GMM crucially depends on valid moment conditions where instruments are not correlated with differenced error term, i.e. $\mathbb{E}\left[(\varepsilon_{i,t} - \varepsilon_{i,t-1})\, ATJ_{i,s}\right] = 0 \;\forall\; s \leq t-2$. This assumption imposes that levels of ATJ observed in the past decades are not correlated with current shocks on economic growth. As this assumption is a priori hard to evaluate, we adopt a conservative approach by treating as endogenous not only ATJ but also the lagged dependent variable and the other classical growth determinants.[11] In particular, we use distant lags (fifth to eighth) to instrument ATJ and other growth determinants, as they typically display time persistence and are more likely to violate difference GMM moment conditions. The maintained assumption is that levels of the endogenous variables observed 25 to 40 years ago are valid instruments. Throughout the paper, we also keep the same set of instruments for transparency and to avoid selecting "ad hoc" moment conditions for each specification.[12] More specifically, we focus on the following moment conditions:

$$\mathbb{E}\left[(\varepsilon_{i,t} - \varepsilon_{i,t-1})\left(\ln(y_{i,t-j}), \ln(ATJ_{i,t-k}), \boldsymbol{X}_{i,t-k}\right)\right] = 0 \quad \text{for all } 2 \leq j \leq 6 \text{ and } 5 \leq k \leq 8. \tag{2}$$

To evaluate the quality of our estimates, we systematically report p-values of the Hansen (1982) test and the Arellano-Bond test for AR2, two standard tests to detect instrumentation problems in GMM. The Hansen test is heteroskedasticity robust, and it evaluates the joint exogeneity of all the instruments. The Arellano-Bond test for AR2 evaluates the absence of second order autocorrelation of residuals. This is crucial as the presence of autocorrelation of residuals can yield to the invalidity of the set of instruments. Roodman (2009) shows that a usual

[11] Treating lag-dependent as endogenous is a standard choice in the growth literature using GMM estimations (Voitchovsky, 2005; Hauk and Wacziarg, 2009; Acemoglu et al., 2019).

[12] In Appendix Table C-3 we show that our results are robust to different moment condition choices.

source of weak instrumentation in the GMM framework comes from using "too many instruments."[13] Following his suggestions, we tackle this issue by keeping the instrument count well below the number of countries in our sample. We achieve this by collapsing the matrix of instruments in each specification.[14]

We use Arellano and Bond's (1991) difference GMM to estimate equation (1) as opposed to Blundell and Bond's (1998) system GMM, which is also widely used in empirical studies. System GMM requires an additional level equation in which the endogenous variables are instrumented with their past differences. Under the additional moment condition that country fixed effects are uncorrelated with lagged differences of endogenous variables, this estimator produces unbiased estimates. Hauk and Wacziarg (2009) and Roodman (2009) show that this additional moment condition is unlikely to hold in the context of growth regressions, like ours. One of the main concern is the correlation between country fixed effects and the speed of convergence for countries far from their steady-state position; this is typically the case for transition economies, of which our sample consist largely on.

As an additional source of evidence, we use a time-varying dummy variable indicating "politically motivated increase in the number of judgeships" from the V-Dem dataset (Coppedge et al., 2019) as an external instrument for ATJ. This represents plausibly exogenous variations in the density of judges, as those events are largely unanticipated and occur for political reasons. Moreover, the direct effect of the instrument on economic growth is rule out by the fact that we control for political regime changes in our regressions using the Polity2 score. Using that external instrument for ATJ, we find similar results with respect to our benchmark (see Table C-6). In Table C-7, we also provide evidence on the quality of our external instrument with Kleibergen-Paap F-stats and several falsification tests.

To further explore the impact of ATJ on economic growth, we look at its heterogeneity by augmenting equation (1):

$$\begin{gathered}\ln\left(\frac{y_{i,t}}{y_{i,t-1}}\right) = \beta\ln(y_{i,t-1}) + \gamma\ln(ATJ_{i,t-1}) + \boldsymbol{\theta}'\boldsymbol{X}_{i,t-1} + \\ \boldsymbol{\zeta}'\boldsymbol{\mu}_{i,t-1} + \boldsymbol{\eta}'\ln(ATJ_{i,t-1}) \times \boldsymbol{\mu}_{i,t-1} + \delta_t + (\alpha_i + \varepsilon_{i,t})\end{gathered} \tag{3}$$

where $\boldsymbol{\mu}_{i,t-1}$ is a vector of time-variant (e.g., level of democracy, the rule of law, or schooling) or time-invariant variables (e.g., the legal origin or geographical areas). The coefficient $\boldsymbol{\eta}'$ measures the effect of ATJ on economic growth at different levels of the variables contained in the $\boldsymbol{\mu}$ vector, meaning that we are looking at potential interaction effects with ATJ. When estimating equation (3), we instrument the new endogenous variables with the same lag structure as the other controls.

[13] In a difference GMM estimation, the number of instruments is quadratic in T.

[14] In the collapsed form the matrix of instruments contains one column per lag, instead of one column per lag and per time-period in the non-collapsed form. This allows us to significantly reduce the instrument count without losing information.

4 Results

The results are organized in three parts. In Section 4.1, we present our main results evaluating the impact of ATJ on economic growth, and we test its sensitivity to different sets of controls, placebos, and the exclusion of various sub-samples. In Section 4.2, we explore the heterogeneity of ATJ effect on growth across relevant macroeconomic dimensions. Section 4.3 investigates possible mechanisms through which the effect of ATJ on economic growth could materialize.

4.1 Main Results and Robustness Checks

In Table 2, we present our results based on the estimation of equation (1). We use 5-year averages in a panel of 83 countries (73 countries in the baseline specification) covering the 1970-2014 period. We start from a parsimonious specification in column 1 with OLS-FE where we include only our variable of interest and the lag dependent variable. In the next columns, we use a difference GMM estimation procedure to deal with endogeneity, and we gradually introduce control variables in order to avoid a potential problem of "bad controls" (Angrist and Pischke, 2008). In column 3, we report our main specification with standard growth controls: schooling, investment, government consumption, and political regime. In columns 4 to 6, we gradually include additional standard controls to help us further identify potential transmission channel candidates and to test for additional robustness.

Table 2 shows that ATJ has a positive and statistically significant impact on economic development. This finding is robust to the inclusion of standard growth controls. In particular, column 3 reveals that increasing ATJ by 1% increases five-year GDP per capita growth by 0.7pp (0.14pp annually). This is a substantial effect, revealing the importance of ATJ in explaining historical differences in growth rates. Here, we capture the overall effect of increasing ATJ on economic development which, as we argue in Section 4.3, encompasses the effect of ATJ on other growth determinants. By adding additional standard growth controls, the magnitude of the effect is indeed consistently reduced from 1.08pp in column 2 to 0.36pp in column 6. This decrease in the magnitude suggests that part of the effect is potentially channeled through other significant variables such as government consumption, fertility, and trade openness (in Section 4.3 we test whether they are indeed transmission channels). The magnitude of the effect is similar to other studies regressing *de facto* judicial indicators on economic growth. For example, Feld et al. (2015) find that a one standard deviation increase in *de facto* judicial independence implies 0.3pp faster annual economic growth, while a country that switches from a completely dependent judiciary to a completely independent one would grow 1.3pp faster. Moreover, Melcarne and Ramello (2016) find that every extra year of judicial delay in the disposition of private litigation lowers annual economic growth by 1pp.

Table 2: Effect of ATJ on Economic Growth

Estimation:	FE (1)	GMM (2)	GMM (3)	GMM (4)	GMM (5)	GMM (6)
$log(GDP_{pc})_{t-1}$	-0.488***	-0.294	-0.517***	-0.438***	-0.670***	-0.555***
	(0.073)	(0.177)	(0.168)	(0.155)	(0.217)	(0.204)
$log(ATJ)_{t-1}$	0.099*	1.077***	0.695***	0.494***	0.467***	0.365**
	(0.055)	(0.330)	(0.204)	(0.161)	(0.141)	(0.165)
$Schooling_{t-1}$			0.025	0.002	0.033	0.056
			(0.089)	(0.057)	(0.051)	(0.045)
$Investment_{t-1}$			-1.084	-0.855	0.008	-0.036
			(0.776)	(0.650)	(0.573)	(0.621)
$Gov.\ Cons._{t-1}$			-0.997**	-1.006**	-1.097*	-0.996**
			(0.424)	(0.481)	(0.554)	(0.415)
$Polity2_{t-1}$			-0.004	-0.007	-0.000	-0.007
			(0.015)	(0.012)	(0.016)	(0.013)
$log(Fertility)_{t-1}$				-0.543***	-0.371**	-0.379**
				(0.181)	(0.175)	(0.157)
$Openness_{t-1}$					0.908	0.833*
					(0.637)	(0.419)
$Inflation_{t-1}$						0.002
						(0.002)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
AR2	.	0.32	0.53	0.67	0.38	0.31
Hansen	.	0.89	0.75	0.45	0.61	0.53
Instruments	.	16	32	36	40	44
Countries	83	83	73	73	73	71
Observations	356	241	217	217	217	202

Note: This table presents estimates of the effect of ATJ on economic growth. Column 1 shows results using OLS-FE, while columns 2-6 present results using a two-step difference GMM estimator. In all specifications, we keep the same lag structure of instruments treating the lag dependent and other variables as endogenous. We use a collapsed matrix of instrument and report instrument count. The AR2 row reports the p-value for a test of no second order correlation in the residuals. The Hansen row reports the p-value for a test of joint exogeneity of the instruments. In all specifications, we control for country and time fixed effects. See Appendix A for further information on the variables. Windmeijer-corrected standard errors clustered at the country-level are in parentheses. * p$<$0.1, ** p$<$0.05, *** p$<$0.01.

In terms of post-estimations tests, in all specifications we do not reject the null hypothesis of no second order autocorrelation of the residuals (AR2 test) and of joint exogeneity of instruments (Hansen test). This is a clear and positive signal indicating the good quality of our instruments. Following Roodman's (2009) rule of thumb, we keep the number of instruments well below the number of countries in our sample by collapsing the matrix of instruments.

One can argue that the effect found for ATJ can be due to an increasing trend in upper-tail human capital, or can reflect the importance of public services, even after we control for government expenditure and education. To further address this concern, and to show the particular importance of the density of judges as a good macro-level proxy for ATJ, we conduct two placebo tests in Table 3. In Panel A, we replace our ATJ proxy with the number of physicians per 100,000 inhabitants, and in Panel B, with the number of public employees per 100,000 inhabitants. In both cases, after including the standard growth controls from column 2, we do not find any positive effect of the placebos on economic growth. Overall, this suggests that the effect of ATJ is not an artifact of a positive trend in top educated people, or in the employment size of the public sector.

Table 4 presents the robustness of our benchmark specification to the removal of different groups of countries. In

Table 3: Placebo Tests

Panel A - Placebo	(1)	(2)	(3)	(4)	(5)
$log(GDP_{pc})_{t-1}$	-0.351*	-0.714***	-0.449***	-0.434***	-0.562***
	(0.197)	(0.195)	(0.146)	(0.155)	(0.170)
$log(Physicians)_{t-1}$	-0.958	0.053	-0.451*	-0.385	-0.342
	(0.592)	(0.204)	(0.234)	(0.237)	(0.250)
AR2	0.96	0.19	0.46	0.46	0.24
Hansen	0.10	0.01	0.20	0.39	0.30
Instruments	16	32	36	40	44
Countries	80	73	73	73	71
Observations	410	377	377	377	351
Panel B - Placebo	(1)	(2)	(3)	(4)	(5)
$log(GDP_{pc})_{t-1}$	-0.382**	-1.085***	-0.761**	-0.758***	-0.619***
	(0.166)	(0.294)	(0.303)	(0.273)	(0.157)
$log(Public\_emp)_{t-1}$	-0.905***	0.034	-0.129	-0.132	-0.048
	(0.214)	(0.208)	(0.120)	(0.102)	(0.092)
AR2	0.20	0.09	0.02	0.03	0.02
Hansen	0.01	0.04	0.10	0.17	0.55
Instruments	10	26	30	34	38
Countries	60	54	54	54	53
Observations	157	145	145	145	140

Note: This table presents estimates of the effect of log physicians per 100,000 inhabitants (Panel A) and log public employees per 100,000 inhabitants (Panel B) on economic growth. Columns 1-5 of both panels present results using two-step difference GMM estimator. In all specifications, we keep the same lag structure treating the lag dependent (instrumented with lags 2-6) and other variables (instrumented with lags 5-8) as endogenous. Control variables are included gradually from columns 2-5 and are the same as in Table 2. We use a collapsed matrix of instrument and report instrument count. The AR2 row reports the p-value for a test of no second order correlation in the residuals. The Hansen row reports the p-value for a test of joint exogeneity of the instruments. In all specifications, we control for country and time fixed effects. Windmeijer-corrected standard errors clustered at the country-level are in parentheses. * p$<$0.1, ** p$<$0.05, *** p$<$0.01.

column 1, we verify that our results are not driven by small or low population countries by removing countries with less than two million inhabitants in year 2000.[15] In columns 2 and 3, we check whether our results are influenced by countries with very low or very high values of ATJ by removing the bottom 10% (column 2) or the top 10% (column 3) of ATJ. The magnitude of the effect and the statistical significance of our variable of interest remains stable across the three columns. In column 4, we look at the potential influence of the unbalanced nature of our panel dataset by dropping countries with less than five periods per country for the ATJ variable. This is the most demanding robustness check that we perform, as we drop as much as 40% of the countries in our sample and 15% of the observations. Despite the large reduction in the sample size, the effect of ATJ on economic growth stays positive and significant at the 1% level, but the magnitude of the effect is reduced to 0.42pp.[16] Finally, in column 5 we check whether countries with highest variations of ATJ drive our benchmark results, since we exploit the within country variation in the identification strategy. Indeed, dropping the top 5% of countries with the highest standard deviation of ATJ decreases the magnitude of the effect from 0.7pp to 0.5pp, while remaining significant at the 1% level.

As a further piece of evidence, in Appendix Table C-6 we redo our main results from Table 2 using a plausible

[15]Countries below two million inhabitants for year 2000 in our sample are: Botswana, Cyprus, Estonia, Luxembourg, Mauritius, Qatar, Slovenia, and Trinidad and Tobago.

[16]It is worth noting that in column 4 we are rejecting the null hypothesis of no second order autocorrelation of the residuals, signaling the failure of difference GMM moment condition. This is not surprising as we are changing the data structure and reducing the sample size significantly.

Table 4: Effect of ATJ on Economic Growth - Robustness by Subsample

Sub-sample:	No Small Countries (1)	No Bottom 10% of ATJ (2)	No Top 10% of ATJ (3)	No Small # of Time Periods (4)	No High Variation of ATJ (5)
$log(GDPpc_{t-1})$	-0.524***	-0.571***	-0.446**	-0.685***	-0.641***
	(0.149)	(0.163)	(0.194)	(0.196)	(0.143)
$log(ATJ_{t-1})$	0.653***	0.569***	0.714***	0.421***	0.508***
	(0.200)	(0.202)	(0.217)	(0.154)	(0.173)
Controls	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes
AR2	0.39	0.88	0.57	0.06	0.43
Hansen	0.80	0.41	0.66	0.28	0.61
Instruments	32	32	32	32	32
Countries	65	65	65	44	69
Observations	198	201	196	185	202

Note: This table presents estimates of the effect of ATJ on economic growth. In particular, we test the sensitivity of our benchmark (column 3 of Table 2) to the removal of different group of countries. In column 1, we drop countries with less than 2 million inhabitants. Columns 2 and 3 remove respectively countries in the bottom and top decile of ATJ. In column 4, we drop countries with less than 5 period observations of ATJ. Column 5 removes the top 5% of countries with the highest standard deviation of ATJ. In all specifications, we keep the same lag structure of instruments treating the lag dependent and other variables as endogenous. We use a collapsed matrix of instrument and report instrument count. The AR2 row reports the p-value for a test of no second order correlation in the residuals. The Hansen row reports the p-value for a test of joint exogeneity of the instruments. In all specifications, we control for country and time fixed effects. Control variables are the same as in our benchmark specification: schooling, investment, government consumption, and political regime. See Appendix A for further information on the variables. Windmeijer-corrected standard errors clustered at the country-level are in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

external instrument for ATJ, while keeping the same internal instrumentation for other endogenous variables. Our external instrument for ATJ is a dummy variable capturing whether a country faced a politically motivated increase in the number of judgeships (Coppedge et al., 2019). Once controlling for the direct effect of political regime changes on economic growth, that variable represents plausibly exogenous variations in the number of judges and ATJ. Using that external instrument, we find similar results with respect to the benchmark. More importantly for endogeneity issues, in all cases we are still not rejecting the null of both the AR2 and Hansen tests. To further analyze the validity of our external instrument, we implement tests for the weakness of instruments in Appendix Table C-7 following Bazzi and Clemens (2013). In column 1, we replicate our most parsimonious specification, with Polity2 score as a control, in a 2SLS setting and find a strong effect of ATJ on economic growth. Crucially, we are able to reject the null hypotheses that the bias in the point estimates on the endogenous variables is greater than 30 percent of the OLS bias, indicating a good quality of our external instrument. In column 3, we perform a placebo test in which we lag our outcome and explanatory variables by two periods while keeping the ATJ variable in the same period; we find that ATJ does not impact past GDP growth, which provides evidence that our results are not spurious. Finally, in columns 5 and 6 we test if our instrument is correlated with the political regime which can be a sign of endogeneity. We do not find any correlation with Polity2 score, showing that our instrument is not a reflection of major political changes.

4.2 Heterogeneous Effects of ATJ on Economic Development

In Table 5, we test whether ATJ has a differential effect on economic growth depending on some key macroeconomic dimensions. In columns 1 to 4 of panel A, we look at the interaction effects of ATJ with the regions specified in Figure 3: Europe (excluding European Transition Economies), Transition Economies, MENA, and East-Asia and Pacific. These four regions are interesting for several reasons. European countries (excluding European Transition Economies) are characterized by a very high level of ATJ in the 1970-2010 period; Transition Economies have experienced a large increase in ATJ after the 1990s due to the political and institutional change resulting from the fall of USSR (Figure 3); MENA countries are mostly Muslim with Muslim law systems; East-Asia and Pacific countries comprise many high-growth economies (e.g., China, South Korea, and Japan) that have tripled ATJ despite high population growth over the same time period. Across all four regions, we find that ATJ is equally important for growth.[17] This also provides evidence that our results are not driven by specific geographic regions.

In columns 5 and 6, we include income level interaction terms. In contrast with interactions by regions, the effect of ATJ differs across income levels. Column 5 shows that the effect of ATJ on economic growth is significantly lower for advanced compared to non-advanced income countries.[18] Testing with a time-varying measure of economic development, column 6 confirms that the impact of ATJ on economic growth depends negatively on the initial level of GDP per capita. For example, column 6 predicts that increasing ATJ by 1% in Ethiopia raises five-year GDP per capita growth by 0.67pp; on the other hand, increasing ATJ by 1% in Belgium will have no effect on growth. Indeed, Figure 4 shows that there is no effect of increasing ATJ for countries with income levels higher than 36,680 2011US$ per capita, which corresponds to highly developed countries.[19] A direct and important policy implication is that the poorest countries are the ones that benefit the most from increasing ATJ, which is in line with other objectives promoted by the UN Sustainable Development Goals (SDGs).[20] This last result highlights, however, a limitation: our proxy is less likely to detect the effect of increasing ATJ where the basics of a well-functioning judicial system are already in place, which is a common feature in developed countries. However, we show in sub-section 4.3 that ATJ positively affects other determinants of economic growth such as the protection of property rights. Therefore, even in highly developed countries, ATJ can still have a positive effect on growth through other variables.

In column 7, we investigate the heterogeneous effect of ATJ with respect to human capital. In line with our

[17] In Appendix Table C-2 we test for all other regions specified in Figure 3 and we find no heterogeneous effects.

[18] The advanced income countries in our sample as classified by the IMF are Australia, Austria, Belgium, Canada, Cyprus, Czech Republic, Denmark, Finland, France, Germany, Greece, Iceland, Ireland, Israel, Italy, Japan, Luxembourg, Netherlands, New Zealand, Norway, Portugal, Singapore, Slovak Republic, Slovenia, South Korea, Spain, Sweden, Switzerland, United Kingdom, and the United States.

[19] In our sample for the most recent period (2010-2014), the highly developed countries are Australia, Austria, Bahrain, Belgium, Canada, Denmark, Finland, France, Germany, Iceland, Ireland, Luxembourg, Netherlands, Norway, Qatar, Singapore, Sweden, Switzerland, United Kingdom, and the United States.

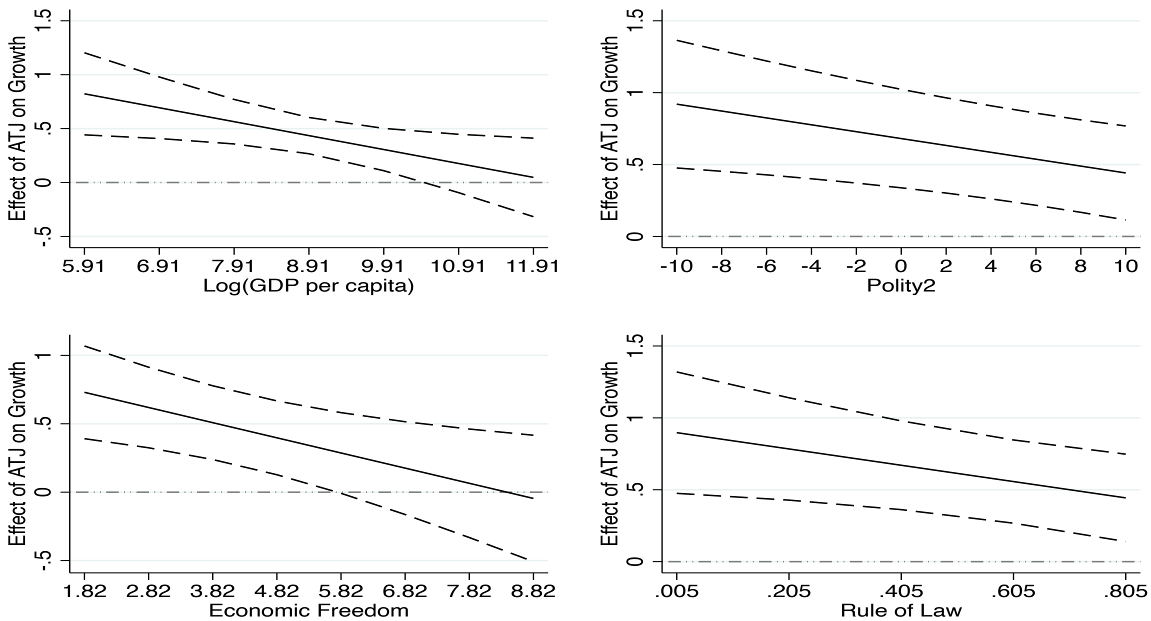
[20] As stated by the UN, ATJ is both an objective per se and a mean to achieve other SDGs such as "No Poverty" (SDG 1) or "Decent work and Economic growth" (SDG 8).

Table 5: Heterogeneous Effects of ATJ on Economic Growth

PANEL A	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$log(GDPpc_{t-1})$	-0.517***	-0.503**	-0.509***	-0.644***	-0.543***	-0.378**	-0.654***
	(0.132)	(0.206)	(0.168)	(0.172)	(0.153)	(0.167)	(0.165)
$log(ATJ_{t-1})$	0.571***	0.656***	0.621***	0.456**	0.555**	1.586**	0.847***
	(0.132)	(0.200)	(0.198)	(0.187)	(0.223)	(0.611)	(0.226)
$log(ATJ_{t-1}) \times EU\ No\,Trans.$	-0.461						
	(0.313)						
$log(ATJ_{t-1}) \times Trans.\ Econ.$		0.050					
		(0.210)					
$log(ATJ_{t-1}) \times MENA$			-0.408				
			(0.826)				
$log(ATJ_{t-1}) \times EAP$				0.150			
				(0.231)			
$log(ATJ_{t-1}) \times Adv.\ Income$					-0.478*		
					(0.273)		
$log(ATJ_{t-1}) \times log(GDPpc_{t-1})$						-0.129*	
						(0.067)	
$log(ATJ_{t-1}) \times Schooling_{t-1}$							-0.039*
							(0.021)
AR2	0.23	0.57	0.51	0.09	0.22	0.11	0.18
Hansen	0.55	0.82	0.66	0.66	0.47	0.26	0.44
Instruments	36	35	36	36	36	36	36
Countries	73	73	73	73	73	73	73
Observations	217	217	217	217	217	217	217
PANEL B	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$log(GDPpc_{t-1})$	-0.617***	-0.673***	-0.455***	-0.508***	-0.584***	-0.588***	-0.607***
	(0.182)	(0.175)	(0.137)	(0.151)	(0.199)	(0.178)	(0.173)
$log(ATJ_{t-1})$	0.681***	0.932***	0.727***	0.500**	0.574***	0.517***	0.900***
	(0.207)	(0.272)	(0.182)	(0.232)	(0.201)	(0.171)	(0.257)
$log(ATJ_{t-1}) \times Polity2_{t-1}$	-0.024**						
	(0.011)						
$log(ATJ_{t-1}) \times Eco.\ Freedom_{t-1}$		-0.111**					
		(0.051)					
$log(ATJ_{t-1}) \times LO_{UK}$			-0.682**				
			(0.305)				
$log(ATJ_{t-1}) \times LO_{FR}$				0.230			
				(0.220)			
$log(ATJ_{t-1}) \times LO_{GE}$					-0.192		
					(0.217)		
$log(ATJ)_{t-1} \times Indep.\ Judic_{t-1}$						-0.181**	
						(0.078)	
$log(ATJ_{t-1}) \times Rule\ of\ Law$							-0.567*
							(0.303)
AR2	0.13	0.28	0.77	0.63	0.26	0.14	0.14
Hansen	0.59	0.38	0.78	0.81	0.54	0.49	0.55
Instruments	36	40	36	36	36	40	40
Countries	73	67	73	73	73	70	73
Observations	217	193	217	217	217	204	217
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: This table presents estimates of the effect of ATJ on economic growth at different levels of some key macroeconomic variables. Columns 1-7 in Panel A and Panel B present results using two-step difference GMM estimator. In all specifications, we keep the same lag structure of instruments treating the lag dependent and other variables as endogenous. We use a collapsed matrix of instrument and report instrument count. The AR2 row reports the p-value for a test of no second order correlation in the residuals. The Hansen row reports the p-value for a test of joint exogeneity of the instruments. In all specifications, we control for country and time fixed effects. Control variables are the same as in our benchmark specification: schooling, investment, government consumption and political regime. See Appendix A for further information on the variables. Windmeijer-corrected standard errors clustered at the country-level are in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Figure 4: Marginal Effect of ATJ on Economic Growth



Note: This figure plots the marginal effect of ATJ on economic growth at different levels of log GDP per capita, democracy (Polity2 score), economic freedom, and rule of law. Values are calculated based on the results of Table 5. A 90 percent confidence interval is depicted with dashed lines.

results in columns 5 and 6 for GDP per capita, we see that highly educated societies are expected to benefit less from improving ATJ compared to low educated societies. This is not surprising considering the link between human capital and GDP per capita, as well as the link between education and legal capability, one of the ATJ components (see Section 2.1).

In panel B of Table 5, we start by investigating the effect of ATJ at different levels of political and economic freedom by using the Polity2 score of democracy (column 1) and the Economic Freedom of the World Index of the Fraser Institute (column 2). In both cases, we find that the effect of ATJ on economic growth decreases with the level of political or economic freedom. This is in line with expectations as both dimensions are correlated with many measures of the quality of the judicial system, such as judicial independence or the rule of law.[21] Furthermore, this reflects the fact that more democratic and more economically free countries already possess higher levels of ATJ. In Figure 4, we depict the marginal effect of ATJ on growth for different levels of democracy and economic freedom. There is a significant positive effect of ATJ on growth for all levels of political freedom but with different

[21] Table 1 provides evidence by showing that the level of democracy is positively correlated with judicial independence (panel A), absence of corruption in the judiciary (panel A), access to civil justice (panel B), and the rule of law (panel C).

magnitudes, as noted in Table 5. On the other hand, we do not find that the effect is significantly different from zero for high levels of economic freedom.

When delivering justice, the judge can have different constraints depending on the judicial system. In particular, the common law system is seen by legal scholars as giving less investigative power to the judge and more to lawyers, compared to the civil law system (Zweigert and Kötz, 1998; Schmiegelow and Schmiegelow, 2014). Consequently, the productivity of the judge is more constrained in common law systems compared to civil law systems. In line with the insights from the comparative law literature, column 3 reveals that the effect of ATJ on economic growth is significantly lower in common law countries compared to civil law countries. Moreover, the total effect is not significantly different from zero for common law countries. This emphasizes the fact that our proxy is less able to capture the evolution of ATJ in common law countries, as we focus on judges and not lawyers. In columns 4 and 5, we decompose further the civil law legal system by distinguishing between French and German legal systems as classified by La Porta et al. (2008). We do not find any significant difference in the effect for both French and German legal origins. Therefore, what matters more in terms of heterogeneity are the fundamental differences between the common law and civil law legal systems, rather than differences within the civil law legal system, which are less fundamental.

Lastly, we investigate the differential effect of ATJ on growth depending on judicial independence (column 6) and quality of the judicial system as measured by the rule of law (column 7). In both cases, we find evidence that the effect of ATJ on growth is stronger in less developed judiciaries. It is worth noting that the effect of ATJ on growth is statistically different from zero for all levels of rule of law as shown in Figure 4. This finding is in line with our previous results: developed judiciaries already have higher levels of ATJ; therefore, increasing ATJ in these judiciaries yields a smaller return in terms of growth.

4.3 Transmission Channels

In Table 2, we find that the positive effect of ATJ on economic growth diminishes when we include classical growth controls. This could be evidence that ATJ exerts its influence on economic growth via significant growth determinants such as government consumption, fertility, and trade openness. In Table 6, we take each of these transmission channel candidates and use them as new outcome variables. Additionally, we include other suspected candidates as outcomes such as public corruption, the size of the shadow economy, and the economic freedom index and its main sub-components.

We find that among the three classical growth control candidates from Table 2, only government consumption is a transmission channel through which ATJ affects GDP growth. Column 1 reveals that ATJ has a negative effect on

Table 6: Effects of ATJ on Potential Mechanisms

Dependent var:	Government Cons.	log(Fertility)	Openness	Public Corruption	Shadow Economy	Economic Freedom	Legal Sys. & Prop. Rights	Regulation
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$log(GDPpc)_{t-1}$	0.118*	0.038	0.045	-0.013	-0.021	-0.051	0.309	-1.172
	(0.068)	(0.206)	(0.082)	(0.045)	(0.025)	(0.587)	(0.375)	(0.751)
$log(ATJ)_{t-1}$	-0.174***	-0.053	-0.154	-0.141***	-0.038**	2.672***	0.754**	1.926***
	(0.053)	(0.294)	(0.095)	(0.053)	(0.018)	(0.653)	(0.368)	(0.696)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
AR2	0.17	0.40	0.04	0.28	0.30	0.05	0.30	0.93
Hansen	0.70	0.00	0.48	0.90	0.14	0.66	0.53	0.43
Instruments	16	16	16	16	14	16	16	16
Countries	71	71	71	71	51	68	67	68
Observations	209	209	209	209	144	199	197	199

Note: This table presents estimates of the effect of ATJ on the different transmission channel candidates specified in the column labels. Columns 1-8 present results using two-step difference GMM estimator. In all specifications, we keep the same lag structure of instruments treating the lag dependent and other variables as endogenous. We use a collapsed matrix of instrument and report instrument count. The AR2 row reports the p-value for a test of no second order correlation in the residuals. The Hansen row reports the p-value for a test of joint exogeneity of the instruments. In all specifications, we control for country and time fixed effects. See Appendix A for further information on the variables. Windmeijer-corrected standard errors clustered at the country-level are in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

the share of government consumption in GDP.[22] We interpret this finding as a positive effect of ATJ on government accountability as it provides a way for citizens to challenge government policies; one of the main transmission channels through which ATJ affects economic growth is therefore the effectiveness or quality of institutions, a critical notion to achieve sustained growth (Acemoglu et al., 2005).

In column 4, we find a negative effect of ATJ on public corruption that further reinforces the interpretation of the government expenditure channel. Corruption, by reducing incentives to produce and invest, is long recognized as a hindrance to growth especially in developing countries (Mauro, 1995). By increasing the share of population that can potentially prosecute corruption affairs, ATJ lowers corruption and shapes economic agents' attitudes in favor of growth. Furthermore, that last finding is particularly interesting in regard to the link found in other studies between corruption and non-productive (military) government spending (Gupta et al., 2001; d'Agostino et al., 2016). These studies point out that public corruption, on top of exerting negative direct effects on growth, distorts the composition of public spending in favor of military spending. This confirms the negative impact of ATJ on government spending, which can be understood as a reduction in excessive military expenses.[23]

In columns 5-8, we include outcome variables that we think are also important for understanding the effect of ATJ on economic growth. We start with the size of the informal sector as a share of GDP in column 5. Many

[22] This confirms once more that ATJ, as measured by the density of judges, is not simply a reflection of government spending. Columns 6 to 8 of Panel B in Table 1 show that government expenditures are not correlated with ATJ as measured by the World Justice Project while the density of judges is. Columns 3 to 6 of Table 2 show that both government expenditure and the density of judges are robust determinants of economic growth.

[23] Column 2 of Appendix Table C-4 provides further evidence in that direction by showing that ATJ has a negative effect on military interference in the rule of law and politics as measured by the Fraser Institute.

developing countries face a situation known as legal dualism, - i.e., the coexistence of formal and informal judicial systems. Informal justice can be easily thought to go hand in hand with informal economic activities, as it provides a way to resolve disputes while minimizing the probability of detection by the government and possibly with lower costs and delay. In such a case, better ATJ can reduce the informal economic activities as it lowers the cost for firms to operate in the formal sector relative to the informal sector, everything else being equal. In line with this statement, column 4 confirms that the share of informal activities in GDP is negatively affected by ATJ.

Recalling the heterogeneous effects of ATJ on growth, Table 5 reveals that the effect of ATJ is decreasing for higher levels of economic freedom. In column 6 of Table 6, we find a positive significant effect for economic freedom. This confirms that part of the effect of ATJ on growth is channeled through an improvement of institutions under which economic activities are undertaken. The magnitude of the effect is sizable as our estimates suggest that by increasing ATJ by 20%, a country can pass from the median to the 75th percentile of economic freedom. However, a cautionary causal statement should be made here, as we are rejecting the null hypothesis of no second order autocorrelation of the residuals.

To better understand the economic freedom channel, we decompose the economic freedom index as much as possible to have a more precise view of the exact transmission channel.[24] Among the five Economic Freedom Index components, we find that ATJ affects the legal systems and property rights (column 7) and regulations (column 8). Going one step further, we decompose these two sub-indexes and we find that ATJ affects positively affects the protection of property rights (column 1 of Appendix Table C-4) and credit market regulations (column 1 of Appendix Table C-5), which are both recognized as important growth determinants in the literature (North, 1990; Levine, 2005). Our result on the positive effect of ATJ on credit market regulations is in line with the findings from Visaria (2009) and Chemin (2009a,b, 2012) who show that judicial efficiency has positive effects on access to credit markets, investment, and entrepreneurship.

5 Conclusion

As stated by the Sustainable Development Goal number 16 of the United Nations (2015) and reasserted by the World Justice Project (2019), societies, both developing and developed, are facing a considerable gap in the justice sector. This ATJ problem is key and needs to be addressed to have better functioning market economies. Whenever and wherever justice is denied, this has an economic cost that is sizeable and not yet fully measured and understood (OECD and World Justice Project, 2019).

[24] Economic freedom is an index comprised of five sub-indexes: (i) size of government, (ii) legal system and property rights, (iii) sound money, (iv) freedom to trade internationally, and (v) regulation.

In this paper, we create a new database on the number of professional judges per 100,000 inhabitants by collecting data from various public institutions. We use these data as a macro-level proxy capturing the fundamental and historical evolution of ATJ between 1970 and 2014 for a wide range of developed and developing countries. This unprecedented historical coverage enable us to study the plausible causal effect of ATJ on economic development, using instrumental variable approach in a dynamic panel setting. In a panel of 73 countries, we show that ATJ is a significant and positive determinant of GDP per capita growth. The results are robust to the exclusion of various sub-samples and to different falsification exercises. Our estimates imply that increasing ATJ by 1% increases the GDP per capita five-year growth rate by 0.7pp. That substantial overall effect of ATJ depends negatively on the initial level of GDP per capita, human capital, democracy, economic freedom, and the rule of law. This has important policy implications, as it means that the poorest countries are the ones who can have the highest economic returns from improving ATJ. In terms of mechanism, our findings suggest that ATJ increases growth via higher government accountability and better quality of institutions. In particular, we find that ATJ leads to a lower share of government consumption in GDP, less public corruption, a smaller shadow economy, better protection of property rights, and better credit market regulation.

Taken as a whole, our results indicate that ATJ is one of the key elements when trying to understand the impact of effective, or *de facto*, judicial institutions on economic development. Given the interconnection between UN Sustainable Development Goals (SDGs) and ATJ, improving ATJ might help achieve other objectives such as no Poverty (SGD 1), gender equality (SDG 5) or decent work and economic growth (SDG 8). Research looking at the total economic effect of ATJ is still in its infancy. Retracing the historical evolution of ATJ over a longer period, with a larger sample of countries and with new ATJ measures, is a fruitful area for future research.

References

Acemoglu, D., Johnson, S., and Robinson, J. A. (2005). Institutions as a Fundamental Cause of Long-run Growth. *Handbook of Economic Growth*, 1:385–472.

Acemoglu, D., Naidu, S., Restrepo, P., and Robinson, J. A. (2019). Democracy Does Cause Growth. *Journal of Political Economy*, 127(1):47–100.

Aldashev, G., Chaara, I., Platteau, J.-P., and Wahhaj, Z. (2012). Using the Law to Change the Custom. *Journal of Development Economics*, 97(2):182–200.

Amirapu, A. (2021). Justice Delayed is Growth Denied: The effect of Slow Courts on Relationship-specific Industries in India. *Economic Development and Cultural Change*, forthcoming.

Angrist, J. D. and Pischke, J.-S. (2008). *Mostly Harmless Econometrics: An Empiricist's Companion*. Princeton University Press.

Arellano, M. and Bond, S. (1991). Some Tests of Specification for Panel Data: Monte Carlo Evidence and an Application to Employment Equations. *The Review of Economic Studies*, 58(2):277–297.

Barendrecht, M., Mulder, J., and Giesen, I. (2006). How to Measure the Price and Quality of Access to Justice? Discussion Paper Series 2006-035, Dept. Law, Tilburg University.

Barro, R. J. (2015). Convergence and Modernisation. *The Economic Journal*, 125(585):911–942.

Bazzi, S. and Clemens, M. A. (2013). Blunt Instruments: Avoiding Common Pitfalls in Identifying the Causes of Economic Growth. *American Economic Journal: Macroeconomics*, 5(2):152–86.

Berkowitz, D., Pistor, K., and Richard, J.-F. (2003). Economic Development, Legality, and the Transplant Effect. *European Economic Review*, 47:165 – 195.

Bielen, S., Marneffe, W., Grajzl, P., and Dimitrova-Grajzl, V. (2018). The Duration of Judicial Deliberation: Evidence from Belgium. *Journal of Institutional and Theoretical Economics*, 174(2):303–333.

Blundell, R. and Bond, S. (1998). Initial Conditions and Moment Restrictions in Dynamic Panel Data Models. *Journal of Econometrics*, 87(1):115–143.

Botero, J. C. and Ponce, A. (2011). Measuring the Rule of Law. Working Paper Series no. 001, The World Justice Project, Washington DC.

Caselli, F., Esquivel, G., and Lefort, F. (1996). Reopening the Convergence Debate: A New Look at Cross-country Growth Empirics. *Journal of Economic Growth*, 1(3):363–389.

Chemin, M. (2009a). Do Judiciaries Matter for Development? Evidence from India. *Journal of Comparative Economics*, 37(2):230–250.

Chemin, M. (2009b). The Impact of the Judiciary on Entrepreneurship: Evaluation of Pakistan's "Access to Justice Programme". *Journal of Public Economics*, 93(1-2):114–125.

Chemin, M. (2012). Does Court Speed Shape Economic Activity? Evidence from a Court Reform in India. *The Journal of Law, Economics, and Organization*, 28(3):460–485.

Coppedge, M., Gerring, J., Knutsen, C. H., Lindberg, S. I., Teorell, J., Altman, D., Bernhard, M., Fish, M. S., Glynn, A., Hicken, A., et al. (2019). V-Dem Dataset v9. Varieties of Democracy (V-Dem) Project, University of Gothenburg, Sweden.

Dimitrova-Grajzl, V., Grajzl, P., Sustersic, J., and Zajc, K. (2012). Court Output, Judicial Staffing, and the Demand for Court Services: Evidence from Slovenian Courts of First Instance. *International Review of Law and Economics*, 32(1):19–29.

Djankov, S., La Porta, R., Lopez-de Silanes, F., and Shleifer, A. (2003). Courts. *The Quarterly Journal of Economics*, 118(2):453–517.

d'Agostino, G., Dunne, J. P., and Pieroni, L. (2016). Government Spending, Corruption and Economic Growth. *World Development*, 84:190–205.

Espinosa, R., Desrieux, C., and Ferracci, M. (2018). Labor Market and Access to Justice. *International Review of Law and Economics*, 54:1–16.

Feld, L., Gutmann, J., and Voigt, S. (2015). Economic Growth and Judicial Independence, a Dozen Years on: Cross-country Evidence using an Updated Set of Indicators. *European Journal of Political Economy*, 38:197–211.

Feld, L. and Voigt, S. (2003). Economic Growth and Judicial Independence: Cross-country Evidence using a New Set of Indicators. *European Journal of Political Economy*, 19(3):497–527.

Galanter, M. (1981). Justice in Many Rooms: Courts, Private Ordering, and Indigenous Law. *The Journal of Legal Pluralism and Unofficial Law*, 13(19):1–47.

Gupta, S., De Mello, L., and Sharan, R. (2001). Corruption and Military Spending. *European Journal of Political Economy*, 17(4):749–777.

Gutmann, J. and Voigt, S. (2018). The Rule of Law: Measurement and Deep Roots. *European Journal of Political Economy*, 54:68–82.

Hansen, L. P. (1982). Large Sample Properties of Generalized Method of Moments Estimators. *Econometrica: Journal of the Econometric Society*, 50(4):1029–1054.

Hauk, W. R. and Wacziarg, R. (2009). A Monte Carlo Study of Growth Regressions. *Journal of Economic Growth*, 14(2):103–147.

Hendley, K. (2007). Are Russian Judges Still Soviet? *Post-Soviet Affairs*, 23(3):240–274.

IMF (2000). Transition Economies: an IMF Perspective on Progress and Prospects. International Monetary Fund Washington, DC.

Islam, N. (1995). Growth Empirics: A Panel Data Approach. *The Quarterly Journal of Economics*, 110(4):1127–1170.

Jappelli, T., Pagano, M., and Bianco, M. (2005). Courts and Banks: Effects of Judicial Enforcement on Credit Markets. *Journal of Money, Credit and Banking*, 37(2):223–244.

La Porta, R., Lopez-de Silanes, F., and Shleifer, A. (2008). The Economic Consequences of Legal Origins. *Journal of Economic Literature*, 46(2):285–332.

Levine, R. (2005). Finance and Growth: Theory and Evidence. *Handbook of Economic Growth*, 1:865–934.

Lichand, G. and Soares, R. R. (2014). Access to justice and Entrepreneurship: Evidence from Brazil's Special Civil Tribunals. *The Journal of Law and Economics*, 57(2):459–499.

Marciano, A., Melcarne, A., and Ramello, G. B. (2019). The Economic Importance of Judicial Institutions, Their Performance and the Proper Way to Measure Them. *Journal of Institutional Economics*, 15(1):81–98.

Massenot, B. (2012). Why Do Some Countries Have So Many More Lawyers Than Others? Working Paper.

Mauro, P. (1995). Corruption and Growth. *The Quarterly Journal of Economics*, 110(3):681–712.

Melcarne, A. and Ramello, G. B. (2016). Justice Delayed, Growth Denied: Evidence from a Comparative Perspective. Unpublished manuscript.

Nickell, S. (1981). Biases in Dynamic Models with Fixed Effects. *Econometrica: Journal of the Econometric Society*, 49(6):1417–1426.

North, D. C. (1990). *Institutions, Institutional Change and Economic Performance*. Cambridge University Press.

OECD and Open Society Foundations (2016). Leveraging the SDG's for Inclusive Growth: Delivering Access to Justice for all. Policy Brief.

OECD and Open Society Foundations (2019). *Legal Needs Surveys and Access to Justice*. OECD Publishing, Paris.

OECD and World Justice Project (2019). Building a Business Case for Access to Justice. OECD White Paper.

Platteau, J.-P. (2000). *Institutions, Social Norms and Economic Development*. Academic Publishers & Routledge.

Pleasence, P., Balmer, N. J., and Sandefur, R. L. (2013). Paths to justice: A Past, Present and Future Roadmap. UCL Centre for Empirical Legal Studies.

Ramseyer, J. M. (2012). Talent matters: Judicial productivity and Speed in Japan. *International Review of Law and Economics*, 32(1):38–48.

Roodman, D. (2009). A Note on the Theme of Too Many Instruments. *Oxford Bulletin of Economics and statistics*, 71(1):135–158.

Schmiegelow, M. and Schmiegelow, H. (2014). *Institutional Competition between Common Law and Civil Law*. Springer.

Staats, J. L., Bowler, S., and Hiskey, J. T. (2005). Measuring Judicial Performance in Latin America. *Latin American Politics and Society*, 47(4):77–106.

United Nations (2015). Transforming our World: The 2030 Agenda for Sustainable Development. New York, United Nations.

Visaria, S. (2009). Legal Reform and Loan Repayment: The Microeconomic Impact of Debt Recovery Tribunals in India. *American Economic Journal: Applied Economics*, 1(3):59–81.

Voitchovsky, S. (2005). Does the Profile of Income Inequality Matter for Economic Growth? *Journal of Economic Growth*, 10(3):273–296.

World Justice Project (2019). Measuring the Justice Gap. World Justice Project, Washington, DC.

World Justice Project (2020). World Justice Project Rule of Law Index. World Justice Project, Washington, DC.

Zweigert, K. and Kötz, H. (1998). *Introduction to Comparative Law*. Third edition, Oxford and New York: Oxford University Press, Clarendon Press.

A Summary Statistics and Variable Definition

Table A-1: Variable Definitions and Sources

Variable	Description and Sources
$Judicial\ Independence$	Judicial independence index. The score ranges from 0 to 1 with higher values indicating higher levels of judicial independence. Source: Gutmann and Voigt (2018).
$log(ATJ)$	Log number of professional judges per 100,000 inhabitants. Source: Authors' calculations.
$log(GDP_{pc})$	Log expenditure-side real GDP at chained PPPs (in mil. 2011US) divided by total population (in mil.). Source: Feenstra et al. (2015).
$Gov.\ Cons.$	Share of government consumption in GDP. Source: Feenstra et al. (2015).
$Polity2$	Political regime score. The score ranges from -10 to +10 with higher values indicating more democratic regimes. Source: Marshall et al. (2016).
$Schooling$	Years of schooling. Source: Barro and Lee (2013).
LO_{UK}	Dummy = 1 for English Legal Origin. Source: La Porta et al. (2008).
LO_{FR}	Dummy = 1 for French Legal Origin. Source: La Porta et al. (2008).
LO_{GE}	Dummy = 1 for German Legal Origin. Source: La Porta et al. (2008).
LO_{SC}	Dummy = 1 for Scandinavian Legal Origin. Source: La Porta et al. (2008).
$Absolute\ Latitude$	Absolute value of the latitude of a country's approximate geodesic centroid. Source: CIA's *World Factbook*.
$Total\ Land\ Area$	The total land area of a country in millions of square kilometers. Source: World Bank (2018b).
$Std.\ Dev.\ Elevation$	The standard deviation of elevation across the grid cells within a country in km above sea level. Source: Nordhaus (2006).
$Mean\ Dist.\ Coast\ or\ River$	The distance, in thousands of km, from a GIS grid cell to the nearest ice-freecoastline or sea-navigable river, averaged across the grid cells of a country. Source: Gallup et al. (1999).
$Corruption\ Free\ Judiciary$	Corruption of judiciary index. The score ranges from 0 to 1 with higher values indicating less corrupt judicial systems. Source: Gutmann and Voigt (2018).
$log(Time\ Enforcing\ Contract)$	Log number of days required to enforce a contract. Source: World Bank (2018a).
$Access\ to\ Civil\ Justice$	Access to civil justice index. The score ranges from 0 to 1 with higher values indicating higher levels of access to civil justice. Source: World Justice Project (2020).
$Rule\ of\ Law\ (WB)$	Rule of law index. The score ranges from -2.5 to 2.5 with higher values indicating higher rule of law levels. Source: World Bank (2018c).
$Quality\ of\ Judicial\ Processes$	Quality of the judicial processes index. The score ranges from 0 to 18 with higher values indicating better quality of the judicial processes Source: World Bank (2018a).
$Investment$	Share of investment in GDP. Source: Feenstra et al. (2015).
$log(Fertility)$	Log total births per woman. Source: World Bank (2018b).
$Openness$	Share of exports and imports in GDP. Source: Feenstra et al. (2015).
$Inflation$	Consumer Price Index. Source: IMF (2019).

(continued on next page)

Variable	Description and Sources
$log(Physicians)$	Log of total number of physicians per 100,000 inhabitants (including generalist and specialist medical practitioners). Source: World Bank (2018b).
$log(Public\ Employees)$	Log of total number of public employees per 100,000 inhabitants. Source: ILO (2019).
$EU\ No\ Trans.$	Dummy = 1 for European economies (excluding transition economies). Source: IMF (2000).
$Trans.\ Econ.$	Dummy = 1 for Transition economies. Source: IMF (2000).
$MENA$	Dummy = 1 for Middle East and North Africa economies. Source: World Bank (2018b).
EAP	Dummy = 1 for East Asia and Pacific economies. Source: World Bank (2018b).
$Adv.\ Income$	Dummy = 1 for advanced income countries. Source: IMF (2019).
$Eco.\ Freedom$	Economic Freedom Index. The score ranges from 0 to 10 with higher values indicating higher economic freedom levels. Source: Gwartney et al. (2018).
$Indep.\ Judic.$	Dummy = 1 if there is an independent judiciary. Source: Dahlberg et al. (2018).
$Rule\ of\ Law$	Rule of law index. The score ranges from 0 to 1 with higher values indicating higher rule of law levels. Source: Coppedge et al. (2019).
$Public\ Corruption$	Public sector corruption index. The score ranges from 0 to 1 with higher values indicating higher levels of public corruption. Source: Coppedge et al. (2019).
$Shadow\ Economy$	Level of the shadow economy in % of GDP. Source: Dahlberg et al. (2018).
$Legal\ Sys.\ \&\ Prop.\ Rights$	Legal system and property rights index. The score ranges from 0 to 10 with higher values indicating better legal systems and higher protection of property rights. Source: Gwartney et al. (2018).
$Regulation$	Regulation index. The score ranges form 0 to 10 with higher values indicating better economic regulations. Source: Gwartney et al. (2018).
SSA	Dummy = 1 for Sub-Saharan Africa economies. Source: World Bank (2018b).
LAC	Dummy = 1 for Latin America and the Caribbean economies. Source: World Bank (2018b).
$North\ America$	Dummy = 1 for North America economies. Source: World Bank (2018b).
$South\ Asia$	Dummy = 1 for South Asia economies. Source: World Bank (2018b).
$Property\ Rights$	Property rights index. The score ranges from 0 to 10 with higher values indicating better protection of property rights. Source: Gwartney et al. (2018).
$Military\ Interference$	Military interference in rule of law and politics index. The score ranges from 0 to 10 with higher values indicating lower levels of interference. Source: Gwartney et al. (2018).
$Judi.\ Indep.\ (FI)$	Judicial independence index. The score ranges from 0 to 10 with higher values indicating higher levels of independence. Source: Gwartney et al. (2018).
$Impartial\ Courts$	Impartial courts index. The score ranges from 0 to 10 with higher values indicating higher levels of impartiality. Source: Gwartney et al. (2018).
$Legal\ System$	Integrity of the legal system index. The score ranges from 0 to 10 with higher values indicating higher integrity. Source: Gwartney et al. (2018).
$Contract\ Enforcement$	Legal enforcement of contracts index. The score ranges from 0 to 10 with higher values indicating better enforcement. Source: Gwartney et al. (2018).
$Regulatory\ Costs$	Regulatory costs of the sale of real property index. The score ranges from 0 to 10 with higher values indicating lower costs. Source: Gwartney et al. (2018).

(continued on next page)

Variable	Description and Sources
$Police\ Reliability$	Reliability of police index. The score ranges from 0 to 10 with higher values indicating higher reliability. Source: Gwartney et al. (2018).
$Business\ Cost\ of\ Crime$	Business costs of crime index. The score ranges from 0 to 10 with higher values indicating higher lower costs. Source: Gwartney et al. (2018).
$Credit\ Market\ Regulations$	Credit market regulations index. The score ranges from 0 to 10 with higher values indicating more efficient regulations. Source: Gwartney et al. (2018).
$Labor\ Market\ Regulations$	Labor market regulations index. The score ranges from 0 to 10 with higher values indicating more efficient regulations. Source: Gwartney et al. (2018).
$Business\ Regulations$	Business regulations index. The score ranges from 0 to 10 with higher values indicating more efficient regulations. Source: Gwartney et al. (2018).
$High\ Court\ Packing$	Dummy = 1 for limited, politically motivated increase in the number of judgeships on very important courts. Source: Coppedge et al. (2019).
$\%\ Tertiary\ Completed\ Educ.$	Percentage of complete tertiary schooling attained in population. Source: Barro and Lee (2013).
$\%\ Pop.\ Risk\ Malaria$	Percentage of population at risk of contracting malaria. Source: Gallup and Sachs (2001).
$Ethnic\ Fractionalization$	Ethnic fractionalization index. Source: Alesina et al. (2003).
$\%\ Euro.\ Descent\ Pop.$	Percentage of population of European descent. Source: Putterman and Weil (2010).
$\%Protestant$	Percentage of a country's population belonging to the Protestant religion. Source: La Porta et al. (1999).
$\%Catholic$	Percentage of a country's population belonging to the Roman Catholic religion. Source: La Porta et al. (1999).
$\%Muslim$	Percentage of a country's population belonging to the Muslim religion. Source: La Porta et al. (1999).

Note: This table provides a description and the sources of all the variables used in our paper. Variables are displayed by order of apparition in tables.

Table A-2: Summary Statistics

Variable	Mean	S.D.	Min.	Max.	Obs.
PANEL A					
$log(GDP_{pc})_{t-1}$	9.54	0.84	6.72	11.33	217
$log(ATJ)_{t-1}$	2.06	0.90	-0.38	3.94	217
$Schooling_{t-1}$	8.90	2.05	2.49	12.73	217
$Investment_{t-1}$	0.25	0.09	0.00	0.62	217
$Gov.\ Cons_{t-1}$	0.19	0.07	0.08	0.50	217
$Polity2_{t-1}$	6.80	5.18	-10.00	10.00	217
$log(Fertility)_{t-1}$	0.64	0.33	0.13	1.61	217
$Openness_{t-1}$	-0.05	0.13	-0.51	0.47	217
$Inflation_{t-1}$	63.99	27.67	0.01	102.50	207
$log(Physicians)_{t-1}$	-3.98	0.73	-7.33	-2.91	204
$log(Public\ Employees)_{t-1}$	8.90	0.56	7.05	10.43	123
$EU\ No\,Trans.$	0.50	0.50	0.00	1.00	217
$Trans.\ Econ.$	0.24	0.43	0.00	1.00	217
$MENA$	0.06	0.24	0.00	1.00	217
EAP	0.22	0.42	0.00	1.00	217
$Adv.\ Income$	0.49	0.50	0.00	1.00	217
$Eco.\ Freedom_{t-1}$	6.59	1.17	2.49	9.05	207
LO_{UK}	0.23	0.42	0.00	1.00	217
LO_{FR}	0.45	0.50	0.00	1.00	217
LO_{GE}	0.28	0.45	0.00	1.00	217
$Rule\ of\ Law_{t-1}$	0.77	0.26	0.06	1.00	217
$Indep.\ Judic._{t-1}$	0.65	0.46	0.00	1.00	204
$Gov.\ Cons._t$	0.18	0.06	0.08	0.50	217
$log(Fertility)_t$	0.60	0.30	0.13	1.49	217
$Openness_t$	-0.05	0.13	-0.54	0.47	217
$Shadow\ Economy_t$	0.27	0.13	0.08	0.71	144
$Eco.\ Freedom_t$	6.89	1.02	3.50	9.05	211
$Legal\ Sys.\ \&\ Prop.\ Rights_t$	6.23	1.31	2.40	8.86	208
$Regulation_t$	6.74	1.28	2.21	9.04	210
$Public\ Corruption_t$	0.28	0.26	0.00	0.91	217
PANEL B - Appendix Variables					
SSA	0.01	0.12	0.00	1.00	217
LAC	0.15	0.36	0.00	1.00	217
$North\ America$	0.03	0.16	0.00	1.00	217
$South\ Asia$	0.01	0.10	0.00	1.00	217
$Property\ Rights_t$	5.96	1.73	1.09	9.11	157
$Military\ Interference_t$	8.00	1.98	0.83	10.00	165
$Judicial\ Independence_t$	5.65	2.20	0.43	9.51	157
$Impartial\ Courts_t$	5.17	1.73	0.99	9.08	166
$Legal\ System_t$	7.16	1.98	1.67	10.00	162
$Contract\ Enforcement_t$	5.22	1.51	1.80	8.48	137
$Regulatory\ Costs_t$	7.85	1.48	3.30	9.94	137
$Police\ Reliability_t$	5.95	1.88	1.56	9.50	105
$Business\ Cost\ of\ Crime_t$	6.37	1.56	1.57	8.96	105
$Credit\ Market\ Regulation_t$	8.06	2.04	0.00	10.00	211
$Labor\ Market\ Regulation_t$	5.82	1.43	2.83	9.17	185
$Business\ Regulation_t$	6.51	1.15	2.72	9.50	157

Note: This table provides summary statistics for the main variables used in the core of our paper (Panel A) and in the appendix (Panel B).

B A New Database on the number of Professional Judges: Method and Sources

This paper provides a new database on the number of professional judges for a large sample of developed and developing countries from 1970 through 2014. To do so, we collect and merge data on the number of judges from three different official sources: (i) *public institutions* (ministries of justice, supreme courts, and national statistical offices), (ii) *international organizations* and (iii) *academic publications*. The vast majority, almost 70%, of our data are from the following international organizations: UNOV, CEPEJ, UNODC, OAS, and Eurostat. These organizations are specialized in collecting data around the world: some, as CEPEJ or OAS, are focused on a specific area (respectively Europe and the Americas) while others, as UNOV, provide data for all continents.

In the making of our database, we use the following criteria:

1. We include data considering the following preference order of data sources: (i) *public institutions*, (ii) *international organizations*, and (iii) *academic publications*.

2. If we have two competing data sources from the same group (e.g., within international organisations, CEPEJ, and UNODC) and covering the exact same period, we keep data from the source which is the most closely related to the national administration of justice (e.g., CEPEJ for European countries).

3. If we have two competing data sources from the same group and covering some common dates (e.g., UNODC from 1970 through 2004 and CEPEJ from 2004 through 2014), we merge the data keeping the source which is the most closely related to the national administration of justice for the overlapping dates (e.g., CEPEJ for European countries in 2004).

4. If we have two complementary data sources (e.g., UNODC from 1970 through 2000, and CEPEJ from 2002 through 2014), we merge the data to create a longer time series.

5. We do not merge datasets when suspiciously high changes are observed (an annual change larger than 50% at the juncture of the two datasets). This is important to minimize false variations that might happen due to changes in definition and counting methods between two datasets. When observed, we remove completely the dataset with the lowest quality.

Table B-1 details the sources, definitions, countries, and periods cover by each used source of data. Table B-2 provides summary statistics for the 105 countries for which we have managed to collect at least one data point.

Table B-1: Number of Judges: Sources, Definitions and Coverage

Source	Definition	Sample used
PANEL A - Public Institutions		
Ministries of Justice and Supreme Courts	Data for: Botswana, Belgium, Croatia, Estonia, Finland, Ireland, Latvia, Mali, Malta, New Zealand, Niger, Poland, Romania, Slovenia, South Korea, Sweden, Uruguay, and Venezuela.	18 countries, 1970-2014
National Statistical Offices	Data for: Australia, Armenia, Qatar, and the United States.	4 countries, 1980-2014
PANEL B - International Organizations		
UNOV (United Nations Office at Vienna - Crime Prevention and Criminal Justice Branch)	Professional judges or magistrates may be understood to mean both full-time and part-time officials authorized to hear civil, criminal and other cases, including in appeal courts, and make dispositions in a court of law.	67 countries, 1973-2014 (online database and reports)
CEPEJ (European Commission for the Efficiency of Justice)	Total number of professional judges in all types of courts. Professional judges are "those who have been trained and who are paid as such" and whose main function is to work as a judge and not as a prosecutor; the fact of working full-time or part-time has no consequence for their status. It does not include the court clerks that exist in some member states.	34 European countries 2002-2014 (2002-2009 reports and 2010-2014 online database)
UNODC (United Nations Office on Drugs and Crime)	Professional Judges or Magistrates means both full-time and part-time officials authorized to hear civil, criminal and other cases, including in appeal courts, and to make dispositions in a court of law. Includes also authorized associate judges and magistrates.	24 countries, 2003-2015 (online database and reports)
OAS (Organization of American States)	Professional judges or magistrates understood as both full-time and part-time officials authorized to hear civil, criminal and other cases, including in appeal courts, and to make dispositions in a court of law. Including authorized associate judges and magistrates.	17 countries, 2003-2014 (online database and reports)
Eurostat (European Statistical Office)	Both full-time and part-time officials authorized to hear criminal and civil cases, including in appeal courts, and to make dispositions in a court of law. Authorized associate judges and magistrates are included.	7 European countries, 2002-2013 (online database and reports)
PANEL C - Publications		
Albers, Pim (2003)	Data for: Armenia, Botswana, Cambodia, Ghana, Mexico, Mozambique, Pakistan, Papua New Guinea, Peru, and Trinidad and Tobago.	10 countries, 2000
Pistor et al. (1999)	Data for: China, France, Germany, Japan, India, South Korea, and Malaysia.	7 countries, 1970-1995
Calleros-Alarcón (2008)	Data for: Bolivia, Brazil, Paraguay, and Venezuela.	4 countries, 1993-2001
Schmiegelow and Schmiegelow (2014)	Data for: Cambodia, Laos, and Tajikistan.	3 countries, 1995-2011
Contini (2000)	Data for: Austria and Spain.	2 countries, 1980-1990
Dakolias (1999)	Data for Ecuador.	1 country, 1998
Kühn (2011)	Data for Poland.	1 country, 1981
Turner (2009)	Data for Serbia.	1 country, 2002
IMF (2012)	Data for Burundi.	1 country, 2005-2010

Note: This table provides the source, definition and country-time data coverage we use for the number of judges. Sources are displayed according to the number of countries they provide within each category.

Table B-2: Number of judges per 100.000 inhabitants - 105 countries, 1970-2014

Country	Mean	S.D.	Min.	Max.	Country	Mean	S.D.	Min.	Max.
Albania	11.29	2.04	8.55	13.07	**Kyrgyz Republic**	6.21	0.67	5.31	6.82
Algeria	8.72	3.53	3.89	12.32	Laos	5.92	.	5.92	5.92
Argentina	3.6	1.87	1.44	4.75	**Latvia**	16.5	8.08	6.81	25.71
Armenia	6.7	0.8	5.95	7.54	**Lithuania**	17.55	7.46	6.36	25.19
Australia	4.83	0.19	4.63	5	**Luxembourg**	34.65	6.01	25.87	39.21
Austria	19.6	0.97	18.22	20.92	**Macedonia**	28.63	4.57	20.79	31.52
Azerbaijan	4.62	1.79	2.49	6.48	**Malaysia**	1.31	0.37	0.83	1.67
Bahamas	9.16	1.49	8.11	10.21	Mali	2.58	.	2.58	2.58
Bahrain	11.44	2.27	9.83	13.05	**Malta**	5.97	2.76	2.69	10.07
Barbados	8.2	0.42	7.9	8.5	**Mauritius**	4.68	1.15	3.46	5.75
Belarus	8.85	2.1	5.75	10.32	**Mexico**	2.15	1.88	0.83	4.31
Belgium	19.66	2.65	16.31	22.81	**Moldova**	8.41	2.28	5.21	10.79
Bolivia	8.85	1.35	7.4	10.05	**Mongolia**	15.77	0.7	14.96	16.24
Bosnia and Herzegovina	21.75	4.25	18.25	26.47	**Montenegro**	39.9	1.04	39.26	41.11
Botswana	2.82	1.22	2.1	4.22	Morocco	10.22	.	10.22	10.22
Brazil	7.46	0.99	6.13	8.35	Mozambique	0.97	.	0.97	0.97
Bulgaria	14.24	10.49	2.82	30.41	**Myanmar**	2.72	0.23	2.46	2.86
Burkina Faso	1.81	0.12	1.73	1.9	**Nepal**	1	0.19	0.81	1.2
Burundi	14.41	6.77	6.62	18.88	**Netherlands**	9.96	3.74	5.77	14.45
Cambodia	1.11	0.71	0.47	1.86	**New Zealand**	3.76	0.87	2.17	4.81
Canada	5.3	3.1	0.68	7.39	Nicaragua	0.94	0.34	0.7	1.19
Chile	5.12	3.19	2.51	10.54	Niger	1.84	0.14	1.75	1.94
China	8.58	5.92	2.56	16.38	**Norway**	9.07	1.96	6.67	11.11
Colombia	9.93	0.37	9.4	10.34	Pakistan	1.05	.	1.05	1.05
Costa Rica	14.28	7.04	4.98	25.48	**Panama**	7.51	0.83	6.04	8.42
Croatia	36.98	9.57	22.2	44.51	Paraguay	10.39	0.16	10.27	10.51
Cyprus	9.06	2.94	5.67	12.88	**Peru**	4.73	2.13	2.99	7.58
Czech Republic	28.4	0.96	27.3	29.06	**Philippines**	2.17	0.47	1.73	2.85
Denmark	6.68	0.2	6.45	6.83	**Poland**	17.25	6.62	8.63	25.86
Dominican Republic	6.92	0.09	6.86	6.99	**Portugal**	12.07	4.88	5.33	18.67
Ecuador	0.97	0	0.97	0.97	**Qatar**	7.39	1.9	5.37	9.62
Egypt	7.11	3.7	4.5	9.73	**Romania**	13.36	6.1	5.15	20.44
El Salvador	10.75	0.08	10.69	10.8	**Russia**	18.42	7.33	7.54	22.87
Estonia	15.5	2.01	12.01	16.83	**Saudi Arabia**	3.17	0.17	3.05	3.3
Ethiopia	0.25	0.08	0.16	0.3	**Serbia**	34.54	2.56	32.31	37.34
Finland	16.42	1.52	13.7	18.07	**Singapore**	1.82	0.56	1.11	2.59
France	9.5	1.67	6.55	10.6	**Slovak Republic**	22.53	2.57	18.74	25.24
Georgia	6	0.95	4.39	6.71	**Slovenia**	38.96	10.87	25.72	51.58
Germany	22.13	3.15	18.07	26.25	**South Africa**	3.67	0.55	2.96	4.12
Ghana	0.93	.	0.93	0.93	**South Korea**	2.75	1.45	1.28	5.39
Greece	17.18	7.39	9.8	30.32	**Spain**	8.55	2.04	5.03	10.83
Guatemala	0.67	0.02	0.66	0.69	**Sweden**	11.48	0.73	10.75	12.35
Hungary	22.4	5.99	12.68	28.64	**Switzerland**	13.17	2.53	10.63	15.36
Iceland	16.26	1.14	15.33	17.74	Tajikistan	3.19	2.36	0.47	4.63
India	1.06	0.08	1	1.15	**Thailand**	4.23	1.71	2.24	6.35
Indonesia	1.62	0.03	1.6	1.64	**Trinidad and Tobago**	6.68	0.94	5.6	7.34
Ireland	2.59	0.48	2.08	3.17	**Turkey**	9.89	0.7	9.19	11.02
Israel	7.07	1.04	5.72	8.32	**Ukraine**	11.84	4.88	4.73	17.33
Italy	11.86	1.38	10.59	14.58	**United Kingdom**	2.73	0.93	1.58	3.55
Jamaica	2.57	1.01	1.07	3.25	**United States**	10.09	0.68	9.29	11.19
Japan	2.42	0.2	2.26	2.86	**Uruguay**	13.19	0.97	11.45	14.4
Kazakhstan	14.73	1.26	13.28	15.58	**Venezuela**	6.5	0.36	6.1	6.8
Kenya	1	0.24	0.83	1.17					

Note: This table provides summary statistics on the density of judges for the 105 countries for which we have at least one observation during the 1970-2014 period. In bold, we indicate the 83 countries that have sufficient time variations in the density of judges to be included in our analysis (see column 1 of Table 2). Countries with standard deviation = (.) have one observation during the 1970-2014 period.

C Additional Analysis

Table C-1: Effect of ATJ on Economic Growth - Robustness by Periods

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$log(GDP_{pc})_{t-1}$	-0.696***	-0.705***	-0.620***	-0.589***	-0.532***	-0.502***	-0.494***	-0.590***
	(0.122)	(0.123)	(0.141)	(0.163)	(0.186)	(0.153)	(0.175)	(0.164)
$log(ATJ)_{t-1}$	0.384***	0.399***	0.631***	0.579***	0.682***	0.648***	0.722***	0.607***
	(0.133)	(0.123)	(0.187)	(0.217)	(0.219)	(0.187)	(0.205)	(0.188)
$log(ATJ_{t-1}) \times period2$	0.296							
	(0.534)							
$log(ATJ_{t-1}) \times period3$		0.315						
		(0.217)						
$log(ATJ_{t-1}) \times period4$			-0.250					
			(0.232)					
$log(ATJ_{t-1}) \times period5$				-0.005				
				(0.070)				
$log(ATJ_{t-1}) \times period6$					0.014			
					(0.055)			
$log(ATJ_{t-1}) \times period7$						0.051		
						(0.048)		
$log(ATJ_{t-1}) \times period8$							0.005	
							(0.036)	
$log(ATJ_{t-1}) \times post90s$								-0.028
								(0.171)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
AR2	0.09	0.23	0.51	0.33	0.52	0.62	0.60	0.33
Hansen	0.13	0.36	0.76	0.40	0.70	0.82	0.64	0.38
Instruments	35	35	34	33	32	32	32	33
Countries	73	73	73	73	73	73	73	73
Observations	217	217	217	217	217	217	217	217

Note: This table presents estimates of the effect of ATJ on economic growth. In particular, we test the sensitivity of our benchmark (column 3 of Table 2) to particular time periods. From column 1 to 7, we include an interaction term between ATJ and the corresponding 5-year time period dummy. In column 8, we include an interaction term between ATJ and post 1990 periods. In all specifications, we keep the same lag structure of instruments treating the lag dependent and other variables as endogenous. We use a collapsed matrix of instrument and report instrument count. The AR2 row reports the p-value for a test of no second order correlation in the residuals. The Hansen row reports the p-value for a test of joint exogeneity of the instruments. In all specifications, we control for country and time fixed effects. Control variables are the same as in our benchmark specification: schooling, investment, government consumption and political regime. See Appendix A for further information on the variables. Windmeijer-corrected standard errors clustered at the country-level are in parentheses. * p$<$0.1, ** p$<$0.05, *** p$<$0.01.

Table C-2: Effect of ATJ on Economic Growth - Additional Regional Interactions

	(1)	(2)	(3)	(4)
$L.log(GDP_{pc})$	-0.517***	-0.540***	-0.519***	-0.485***
	(0.163)	(0.144)	(0.175)	(0.170)
$L.log(ATJ)$	0.692***	0.570***	0.702***	0.759***
	(0.202)	(0.151)	(0.217)	(0.227)
$L.log(ATJ) \times SSA$	-0.072			
	(0.616)			
$L.log(ATJ) \times LAC$		-0.302		
		(0.286)		
$L.log(ATJ) \times N.\ America$			-0.468	
			(0.702)	
$L.log(ATJ) \times South\ Asia$				-1.455
				(2.944)
Controls	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
AR2	0.52	0.15	0.50	0.64
Hansen	0.74	0.51	0.79	0.76
Instruments	33	36	34	32
Countries	73	73	73	73
Observations	217	217	217	217

Note: This table presents estimates of the effect of ATJ on economic growth. In particular, we test the sensitivity of our benchmark (column 3 of Table 2) to particular geographical areas. From column 1 to 4, we include an interaction term between ATJ and the corresponding region. In all specifications, we keep the same lag structure of instruments treating the lag dependent and other variables as endogenous. We use a collapsed matrix of instrument and report instrument count. The AR2 row reports the p-value for a test of no second order correlation in the residuals. The Hansen row reports the p-value for a test of joint exogeneity of the instruments. In all specifications, we control for country and time fixed effects. Control variables are the same as in our benchmark specification: schooling, investment, government consumption and political regime. See Appendix A for further information on the variables. Windmeijer-corrected standard errors clustered at the country-level are in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table C-3: Effect of ATJ on Economic Growth - Alternative Moment Conditions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$log(GDP_{pc})_{t-1}$	-0.519***	-0.523***	-0.533***	-0.524***	-0.535***	-0.462***	-0.603***	-0.513***
	(0.134)	(0.153)	(0.156)	(0.171)	(0.173)	(0.147)	(0.093)	(0.173)
$log(ATJ)_{t-1}$	0.624***	0.591***	0.630***	0.704***	0.634***	0.631***	0.526***	0.631***
	(0.153)	(0.153)	(0.148)	(0.194)	(0.154)	(0.220)	(0.106)	(0.143)
$Schooling_{t-1}$	-0.005	0.001	-0.000	0.027	-0.001	-0.012	0.062	-0.023
	(0.050)	(0.059)	(0.060)	(0.086)	(0.078)	(0.076)	(0.042)	(0.063)
$Investment_{t-1}$	-1.002	-0.885	-1.031	-1.090	-1.025	-0.544	-0.671	-0.848
	(0.617)	(0.746)	(0.742)	(0.780)	(0.935)	(0.853)	(0.548)	(0.809)
$Gov.\ Cons._{t-1}$	-1.133***	-1.255**	-1.123***	-0.970**	-1.105***	-0.951**	-0.975***	-1.207**
	(0.354)	(0.572)	(0.359)	(0.418)	(0.387)	(0.464)	(0.262)	(0.541)
$Polity2_{t-1}$	0.006	0.001	0.006	-0.004	0.006	0.005	0.005	-0.002
	(0.007)	(0.009)	(0.008)	(0.015)	(0.007)	(0.010)	(0.008)	(0.010)
$First\_log(GDP_{pc})_{t-1}$	2	2	3	3	4	4	5	5
$Last\_log(GDP_{pc})_{t-1}$	5	8	5	6	6	8	6	8
$First\_log(ATJ)_{t-1}$	3	4	3	5	3	3	3	4
$Last\_log(ATJ)_{t-1}$	6	6	6	8	6	5	8	6
AR2	0.38	0.38	0.38	0.54	0.41	0.52	0.19	0.53
Hansen	0.60	0.36	0.53	0.71	0.54	0.44	0.58	0.39
Instruments	31	29	30	31	30	27	39	26
Countries	73	73	73	73	73	73	73	73
Observations	217	217	217	217	217	217	217	217

Note: This table presents estimates of the effect of ATJ on economic growth. In particular, we test the sensitivity of our benchmark (column 3 of Table 2) to alternative lag structure for instruments. From column 1 to 8, we use diffrenent sets of lags specified in the corresponding lines for GDP_{pc} and ATJ (including other endogenous variables). We use a collapsed matrix of instrument and report instrument count. The AR2 row reports the p-value for a test of no second order correlation in the residuals. The Hansen row reports the p-value for a test of joint exogeneity of the instruments. In all specifications, we control for country and time fixed effects. See Appendix A for further information on the variables. Windmeijer-corrected standard errors clustered at the country-level are in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table C-4: Effects of ATJ on Potential Mechanisms - Decomposing the Legal System and Property Rights Indicator

Dependent var:	Property Rights (1)	Military Interfer-ence (2)	Judi. Indep. (FI) (3)	Impartial Courts (4)	Legal System (5)	Contract Enforce-ment (6)	Regulatory Costs (7)	Police Reliabil-ity (8)	Business Cost of Crime (9)
$log(GDPpc)_{t-1}$	0.438	0.000	1.588	2.044**	-0.549	-0.501	1.309	0.593	0.968
	(0.628)	(0.517)	(1.272)	(0.786)	(0.894)	(0.682)	(0.963)	(0.507)	(0.628)
$log(ATJ)_{t-1}$	2.375***	1.965***	-0.585	-0.049	-0.147	-0.333	0.382	-0.442	0.994
	(0.790)	(0.649)	(2.082)	(0.854)	(1.404)	(0.583)	(1.141)	(2.142)	(2.558)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
AR2	0.96	0.93	0.37	0.52	0.10	.	.	.	.
Hansen	0.16	0.69	0.16	0.11	0.35	0.34	0.79	0.80	0.52
Instruments	12	12	12	12	12	11	11	10	10
Countries	65	65	65	65	63	64	64	60	60
Observations	122	132	122	132	125	99	99	60	60

Note: This table presents estimates of the effect of ATJ on the different transmission channel candidates specified in the column labels. Columns 1-9 present results using two-step difference GMM estimator. In all specifications, we keep the same lag structure of instruments treating the lag dependent and other variables as endogenous. We use a collapsed matrix of instrument and report instrument count. The AR2 row reports the p-value for a test of no second order correlation in the residuals. The Hansen row reports the p-value for a test of joint exogeneity of the instruments. In all specifications, we control for country and time fixed effects. See Appendix A for further information on the variables. Windmeijer-corrected standard errors clustered at the country-level are in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table C-5: Effects of ATJ on Potential Mechanisms - Decomposing the Regulation Indicator

Dependent var:	Credit Market Regulation (1)	Labor Market Regulation (2)	Business Regulation (3)
$log(GDPpc)_{t-1}$	-3.684**	0.059	-0.218
	(1.402)	(0.865)	(0.735)
$log(ATJ)_{t-1}$	5.675***	0.227	0.594
	(1.159)	(1.324)	(0.854)
Country FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
AR2	0.50	0.25	0.64
Hansen	0.43	0.09	0.14
Instruments	16	16	12
Countries	68	65	65
Observations	201	162	123

Note: This table presents estimates of the effect of ATJ on the different transmission channel candidates specified in the column labels. Columns 1-3 present results using two-step difference GMM estimator. In all specifications, we keep the same lag structure of instruments treating the lag dependent and other variables as endogenous. We use a collapsed matrix of instrument and report instrument count. The AR2 row reports the p-value for a test of no second order correlation in the residuals. The Hansen row reports the p-value for a test of joint exogeneity of the instruments. In all specifications, we control for country and time fixed effects. See Appendix A for further information on the variables. Windmeijer-corrected standard errors clustered at the country-level are in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table C-6: Effect of ATJ on Economic Growth - with External Instrument for ATJ

Estimation:	FE (1a)	GMM (1b)	GMM (2)	GMM (3)	GMM (4)	GMM (5)
$log(GDP_{pc})_{t-1}$	-0.488***	-0.258	-0.494***	-0.476***	-0.702***	-0.568**
	(0.072)	(0.186)	(0.167)	(0.147)	(0.224)	(0.223)
$log(ATJ)_{t-1}$	0.099**	1.183***	0.735***	0.482***	0.477***	0.392**
	(0.044)	(0.327)	(0.190)	(0.159)	(0.156)	(0.153)
$Schooling_{t-1}$			-0.006	0.026	0.042	0.064
			(0.097)	(0.056)	(0.062)	(0.051)
$Investment_{t-1}$			-1.219	-1.065	-0.160	-0.116
			(0.837)	(0.782)	(0.774)	(0.658)
$Gov.\ Cons._{t-1}$			-0.951*	-1.105*	-1.221**	-1.085**
			(0.488)	(0.562)	(0.611)	(0.418)
$Polity2_{t-1}$			-0.007	-0.004	0.002	-0.007
			(0.014)	(0.011)	(0.015)	(0.014)
$log(Fertility)_{t-1}$				-0.539**	-0.340*	-0.347**
				(0.207)	(0.194)	(0.164)
$Openness_{t-1}$					0.881	0.859*
					(0.725)	(0.473)
$Inflation_{t-1}$						0.002
						(0.002)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
AR2		0.26	0.60	0.44	0.25	0.31
Hansen		0.85	0.74	0.43	0.55	0.38
Instruments		12	28	32	36	40
Countries	83	83	73	73	73	71
Observations	356	241	217	217	217	202

Note: This table presents estimates of the effect of ATJ on economic growth. In particular, we redo Table 2 using "politically motivated increase in the number of judges" (Coppedge et al., 2019) as an external instrument for ATJ. The instrument is a dummy variable indicating if the judiciary of a country experienced "massive, politically motivated increase in the number of judgeships across the entire judiciary" or "limited, politically motivated increase in the number of judgeships on very important courts." Column 1a presents results using fixed effects. Columns 1b-5 present results using two-step difference GMM estimator. In all specifications, we keep the same lag structure of instruments treating the lag dependent and other variables as endogenous. We use a collapsed matrix of instrument and report instrument count. The AR2 row reports the p-value for a test of no second order correlation in the residuals. The Hansen row reports the p-value for a test of joint exogeneity of the instruments. In all specifications, we control for country and time fixed effects. See Appendix A for further information on the variables. Windmeijer-corrected standard errors clustered at the country-level are in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table C-7: External Instrument Validity

			Placebo			
Dependent var:	$D.growth_t$	$D.growth_t$	$D.growth_{t-2}$	$D.growth_{t-2}$	$D.polity2_t$	$D.polity2_t$
Estimation:	2SLS	OLS	2SLS	OLS	OLS	OLS
	(1)	(2)	(3)	(4)	(5)	(6)
$D.ln\_ATJ_{t-1}$	1.094***		-0.101			
	(0.185)		(0.238)			
$D.log(GDP_{pc})_{t-1}$	-0.318**	-0.696***				0.108
	(0.121)	(0.048)				(0.629)
$D.log(GDP_{pc})_{t-3}$			-1.085***	-0.744***		
			(0.093)	(0.071)		
$D.Polity2_{t-1}$	-0.012	-0.001				
	(0.019)	(0.004)				
$D.Polity2_{t-3}$			-0.001	-0.002		
			(0.011)	(0.002)		
$High\ Court\ Packing_{t-1}$		0.490***		-0.020	-0.104	-0.154
		(0.024)		(0.066)	(0.120)	(0.280)
First stage						
$High\ Court\ Packing_{t-1}$	0.272***		0.215***			
	(0.041)		(0.048)			
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
KP LM p-val	0.05		0.58			
KP F-stat	6.52		1.30			
rel. OLS bias>30	0.0027		0.9664			
Countries	78	78	78	78	79	79
Observations	229	229	179	320	230	230

Note: This table test the validity of the external instrument presented and used in Table C-6. In column 1, we use a standard 2SLS approach and instrument ATJ with "politically motivated increase in the number of judges" (Coppedge et al., 2019). The instrument is a dummy variable indicating if the judiciary of a country experienced "massive, politically motivated increase in the number of judgeships across the entire judiciary" or "limited, politically motivated increase in the number of judgeships on very important courts." Column 2 displays the reduced form relationship estimated by OLS. In columns 3 and 4 we make a placebo test by lagging the dependent and lag-dependent variable by two periods while keeping the ATJ variable in the same $t-1$ period. Columns 5 and 6 test if our instrument is correlated with the political regime variable. The KP LM row reports the p-value for a test of underidentification of the structural equation. The KP F-stat row reports the heteroskedasticity-robust first-stage F-statistics. Below, we report the p-value of a test that the bias in the point estimates on the endogenous variables is greater than 30 percent of the OLS bias following Bazzi and Clemens (2013). Considering a 30% maximal IV relative bias means that the critical value for the F-stat is around 4.5. In all specifications, we control for country and time fixed effects. See Appendix A for further information on the variables. Standard errors clustered at the country-level are in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table C-8: Determinants and Correlates of the Density of Judges

Period covered:	1970-2014 (1)	1970-2014 (2)	1970-2014 (3)	1990-2014 (4)	1990-2014 (5)	1990-2014 (6)
$log(GDP_{pc})$	0.523***	0.439***	0.201**	0.521***	0.451***	0.185
	(0.082)	(0.081)	(0.099)	(0.078)	(0.081)	(0.111)
$Gov.\ Cons.$	2.793***	2.777***	0.862	3.341***	3.348***	0.949
	(0.949)	(1.000)	(0.993)	(1.019)	(1.121)	(1.105)
LO_{FR}	0.879***	0.902***	0.635***	0.905***	0.905***	0.598***
	(0.157)	(0.157)	(0.216)	(0.151)	(0.157)	(0.223)
LO_{GE}	1.064***	1.104***	1.017***	1.169***	1.205***	1.070***
	(0.258)	(0.287)	(0.263)	(0.241)	(0.279)	(0.272)
LO_{SC}	0.671***	0.657**	0.673*	0.614***	0.582**	0.674*
	(0.232)	(0.252)	(0.378)	(0.213)	(0.241)	(0.354)
$Polity2$		0.023**	0.012		0.016	-0.004
		(0.011)	(0.015)		(0.013)	(0.019)
$\%\ Tertiary\ Completed\ Educ.$		-0.002	-0.001		0.000	-0.000
		(0.014)	(0.012)		(0.014)	(0.012)
$Absolute\ Latitude$			-0.002			-0.001
			(0.007)			(0.007)
$\%\ Pop.\ Risk\ Malaria$			-0.238			-0.406
			(0.447)			(0.407)
$Ethnic\ Fractionalization$			0.275			0.212
			(0.356)			(0.378)
$\%\ Euro.\ Descent\ Pop.$			0.011***			0.011***
			(0.003)			(0.003)
$\%Protestant$			-0.003			-0.005
			(0.004)			(0.004)
$\%Catholic$			-0.001			-0.001
			(0.002)			(0.003)
$\%Muslim$			0.004			0.003
			(0.003)			(0.003)
Constant	-4.498***	-3.810***	-1.704*	-4.467***	-3.882***	-1.221
	(0.753)	(0.784)	(0.925)	(0.770)	(0.784)	(1.108)
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
adj. R-sq	0.47	0.47	0.58	0.47	0.47	0.57
Countries (N)	102	91	89	102	91	89
Observations (NT)	459	416	409	359	321	314

Note: The aim of this table is to provide insights on the factors explaining the cross-country differences in the density of judges as observed in Figure 2. The presented results are based on an OLS estimation of: $\ln(Density.Judges_{i,t}) = \alpha + \beta_1 \ln(y_{i,t}) + \beta_2\ g_{i,t} + \boldsymbol{\zeta'} \boldsymbol{L}_i + \boldsymbol{\gamma'} \boldsymbol{X}_{i,t} + \delta_t + \varepsilon_{i,t}$ where $i = 1, \ldots N$ indicates a country and $t = 1, \ldots T$ is a 5-year time period. The dependent variable $Density.Judges_{i,t}$ is the number of judges per 100.000 inhabitants, y denotes GDP per capita, g stands for the share of government consumption in GDP, $\boldsymbol{L}$ is a vector of legal origin dummies, $\boldsymbol{X}$ is a vector of other explanatory variables, δ_t is a time fixed effect and $\varepsilon_{i,t}$ is an error term. In particular, the vector $\boldsymbol{X}$ contains, depending on the specification, the Polity2 score of democracy, the share of population having completed tertiary education, absolute latitude, percentage of population at risk of contracting malaria, ethnic fractionalization, percentage of population of European descent, and share of population of a given religion. All the mentioned variables are good candidates for explaining the cross-country variations of the density of judges, and are widely used in the macroeconomic literature. We uncover that a very robust predictor of the density of judges is the legal origin. In particular, we find that civil law countries of either French, German, or Scandinavian legal origin, have significantly higher density of judges compared to common law countries. Based on our preferred specification in column 3, French legal origin countries have on average 63% more judges per capita, and German legal origin countries more than double their density of judges compared to common law countries. Within the civil law legal family, we find that German legal origin countries have significantly higher density of judges compared to French legal origin countries, as we reject the null hypothesis of equality of the three civil law legal origins coefficients in column 3 and 6. These findings are consistent with the comparative law literature describing common law as an adversarial dispute resolution system, where the judge has less investigative powers than lawyers, while civil law countries dispute resolution system is inquisitorial, giving more power to the judge (Zweigert and Kötz, 1998). Common law legal system requires therefore less judges and more lawyers than civil law.[a] Differences inside the civil law family can be explained by specificities of the German legal origin in terms of judicial organisation (Schmiegelow and Schmiegelow, 2014). Finally, we confirm the fact that European countries have a significantly higher density of judges (Figure 3) as we find a positive and highly significant coefficient for the variable giving the percentage of population of European descent. Outside Europe, this variable might reflect the link between colonization patterns and current institutions (Acemoglu et al., 2005). Given the fact that many new countries gained independence after the 1990 period, columns 4-6 replicate the first three columns considering only data from the 1990-2014 period as a robustness check. Consistent with our previous findings, we confirm the highly significant and sizeable effect found for the three civil law legal origins. Standard errors are clustered at country-level. * p<0.1, ** p<0.05, *** p<0.01.

[a]Explaining the cross-country differences in the number of lawyers, Massenot (2012) finds that common law and French civil law countries have more lawyers than German and Scandinavian legal origins countries.

Appendix references (not cited in the main text)

Albers, Pim (2003). Evaluating Judicial Systems: A Balance between Variety and Generalisation. European Commission for the Efficiency of Justice, Strasbourg.

Alesina, A., Devleeschauwer, A., Easterly, W., Kurlat, S., and Wacziarg, R. (2003). Fractionalization. *Journal of Economic Growth*, 8(2):155–194.

Barro, R. J. and Lee, J. W. (2013). A New Data Set of Educational Attainment in the World, 1950–2010. *Journal of Development Economics*, 104:184–198.

Calleros-Alarcón, J. C. (2008). *The Unfinished Transition to Democracy in Latin America*. Routledge.

CEPEJ. European Judicial Systems Efficiency and Quality of Justice. European Commission for the Efficiency of Justice, Strasbourg.

Contini, F. (2000). European Database on Judicial Systems. *European Research Network on Judicial Systems*.

Dahlberg, S., Holmberg, S., Rothstein, B., Alvarado Pachon, N., and Svensson, R. (2018). The Quality of Government Dataset. The Quality of Government Institute, University of Gothenburg, Sweden.

Dakolias, M. (1999). *Court Performance Around the World: A Comparative Perspective*. The World Bank, Washington, D.C.

Eurostat. Eurostat Database, Luxembourg.

Feenstra, R. C., Inklaar, R., and Timmer, M. P. (2015). The Next Generation of the Penn World Table. *American Economic Review*, 105(10):3150–82.

Gallup, J. L. and Sachs, J. D. (2001). The Economic Burden of Malaria. *The American Journal of Tropical Medicine and Hygiene*, 64(1-2):85–96.

Gallup, J. L., Sachs, J. D., and Mellinger, A. D. (1999). Geography and Economic Development. *International Regional Science Review*, 22(2):179–232.

Gwartney, J., Lawson, R., Hall, J., and Murphy, R. (2018). Economic Freedom of the World: 2018 Annual Report. Fraser Institute, Vancouver, Canada.

ILO (2019). ILOSTAT Database. International Labour Organization, Genève, Switzerland.

IMF (2012). Burundi: Poverty Reduction Strategy Paper II. IMF Country Report n°12/224.

IMF (2019). International Financial Statistics. International Monetary Fund, Washington, D.C.

Kühn, Z. (2011). *The Judiciary in Central and Eastern Europe: Mechanical Jurisprudence in Transformation?* Brill.

La Porta, R., Lopez-de Silanes, F., Shleifer, A., and Vishny, R. (1999). The Quality of Government. *The Journal of Law, Economics, and Organization*, 15(1):222–279.

Marshall, M. G., Gurr, T. R., and Jaggers, K. (2016). Polity IV project: Political Regime Characteristics and Transitions, 1800–2014. The Polity IV Dataset. Center for Systemic Peace, Vienna (USA).

Nordhaus, W. D. (2006). Geography and macroeconomics: New data and new findings. *Proceedings of the National Academy of Sciences*, 103(10):3510–3517.

OAS. Organization of American States Database, Washington, D.C.

Pistor, K., Wellons, P. A., and Sachs, J. (1999). *The Role of Law and Legal Institutions in Asian Economic Development, 1960-1995*. Oxford University Press, USA.

Putterman, L. and Weil, D. N. (2010). Post-1500 Population Flows and the Long-Run Determinants of Economic Growth and Inequality. *The Quarterly Journal of Economics*, 125(4):1627–1682.

Turner, B. (2009). *The Statesman's Yearbook 2010: The Politics, Cultures and Economies of the World*. Springer.

United Nations Office at Vienna - Crime Prevention and Criminal Justice Branch. United Nations Surveys of Crime Trends and Operations of Criminal Justice Systems Series, Waves 1-10, 1970-2006. Inter-university Consortium for Political and Social Research [distributor], 2010-07-22. https://doi.org/10.3886/ICPSR26462.v1.

UNODC. United Nations Office on Drugs and Crime Database, Vienna.

World Bank (2018a). Doing Business Database. The World Bank, Washington, D.C.

World Bank (2018b). World Development Indicators Database. The World Bank, Washington, D.C.

World Bank (2018c). Worldwide Governance Indicators. The World Bank, Washington, D.C.