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

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**Abstract**

This paper evaluates the importance of access to justice (ATJ) for economic growth. We have created 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 country-level indicator to capture the structural 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 initial levels 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 institutional quality.

**Keywords:** Access to Justice, Economic Growth, Institutions, Judges

**JEL Codes:** K00, K19, O11, O40, O43.

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"*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 barriers to reaching justice are numerous, ranging from time and cost factors to discrimination of all kinds; they prevent the accurate delivery of justice in civil, administrative, and criminal matters. This has a direct and powerful impact on people's everyday decisions in the most fundamental economic operations. As a result, such obstacles to justice generate substantial economic costs and slow down economic development in both developed and developing societies.[1] The judiciary, both as a source of judge-made law and as an enforcement institution, shapes the incentives of economic actors and is actively involved in enforcing contracts and protecting property rights, which are recognized as the cornerstones of a well-functioning market economy (North, 1990; Acemoglu et al., 2005). Although the existing literature has 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; Voigt et al., 2015), surprisingly little is known about the importance of a structural factor such as ATJ for economic development.

This paper fills this gap in the literature by showing that ATJ is an important determinant of economic growth. To conduct the empirical analysis, we have created a new database on the number of professional judges per 100,000 inhabitants by combining data from various public institutions. This measures the ability of for the average citizen to have access to the judiciary at the country level. Compared to available measures of ATJ in the literature, our measure has a much longer time dimension (1970-2014), which allows us to track its structural variations. To further validate our proxy, we show that our ATJ measure is robustly correlated with the more comprehensive, but much shorter in the time dimension, access to civil justice indicator of the World Justice Project (2020); at the same time, it is not robustly correlated with other dimensions of judicial performance at the country-level, such as the rule of law indicator of the World Bank (2018).

[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.

Using a dynamic panel setting and instrumental variables 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 the rule of law, implying that less developed societies are the ones that can gain the highest economic returns from improving the structural dimensions of ATJ. In terms of mechanisms, our results suggest that part of the effect of ATJ on growth comes from higher government accountability and institutional quality. In particular, we show that ATJ negatively affects the share of government consumption in GDP, public corruption, and the shadow economy, while positively affecting the protection of property rights and the regulation of the credit market. These results are consistent with the existing literature showing that institutions and their quality are one of the main determinants of economic development (Acemoglu et al., 2005).

Our paper makes three primary contributions. First, 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 to 2014. We draw on these data as a new structural indicator of ATJ, measuring 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 examine the effect of the structural changes 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 judicial independence or the rule of law). We also produce a series of new stylized facts from our database that highlight substantial 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, rather than relying solely on qualitative measures provided by legal expert 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]

Second, we complement the existing literature on the effect of judicial institutions on economic development by providing the first study that examines 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 promotes

[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.

entrepreneurship by increasing the geographic proximity and improving the speed of the judicial 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 created fewer jobs and fewer firms than unaffected cities. Our findings in an international setting are also complementary to country-specific studies showing 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 work 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 is more important for national economic development than the legal family of the code. Feld and Voigt (2003) and Voigt et al. (2015) show that *de facto* judicial independence is significantly correlated with economic growth while mere *de jure* judicial independence is not.

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 assess the impact of various *de facto* judicial changes on economic development. This strategy solves the omitted variable bias to a certain extent, but it does not address the reverse causality bias stemming from the high correlation between economic development and *de facto* judicial institutions. Taking advantage of the long panel dimension of our new database on ATJ, we treat endogeneity using a difference-GMM method as derived by Arellano and Bond (1991). This offers the advantage of controlling for country fixed effects, while allowing the use of lagged levels of endogenous variables as instruments. The key assumption for using lagged levels of endogenous variables as instruments and satisfying the exclusion restriction is the absence of serial correlation in the residuals. To mitigate this potential problem, we systematically provide a test for second-order autocorrelation of the residuals in our regressions, and we find no evidence of serial correlation in the residuals. Moreover, we use distant lags as instruments as they are more like likely to satisfy the exclusion restriction.

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 may affect growth. Section 5 presents our conclusions.

# 2 Access to Justice: New Dataset and Stylized Facts

This section is divided into two parts. In Section 2.1, we discuss the concept of ATJ, explain how we measure it at the country-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 of the concept: (i) *Legal capability* – a citizen needs to know the law, legal procedures, and who to contact to file or defend a lawsuit; (ii) *Availability of justice* – a citizen needs to be able to rely on a sufficient and available supply of justice (infrastructure and personnel); (iii) *Physical barriers* – a citizen needs to have access to transportation to a court and to his or her legal advisors; (iv) *Social barriers* – a citizen needs to be free from fear of being discriminated against 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 of ATJ and its economic dimensions). Overall, this multidimensionality makes ATJ difficult to quantify due to the need to collect, aggregate, and make weighting decisions for many variables.[4] Moreover, although various datasets on ATJ are available at the national level, there is no such dataset that includes both a large cross-country aspect and a long time dimension.[5] One example is the World Justice Project's Rule of Law Index, which includes access to civil justice as one of its subcomponents. 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, namely the opportunity for citizens to have access to the judiciary. We argue that the number of judges per 100,000 inhabitants is a good indicator for capturing historical changes in the core elements of ATJ at the country-level. An increase in the number of judges relative to the population directly increases the available supply of justice (component ii) and general awareness of the law (component i). An increase in the density of judges may also coincide with the creation of new courts, which affects the accessibility (iii), and cost dimensions (v) of ATJ. Our ATJ measure has the advantage of being

[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 in each national survey is different, which limits the possibility of cross-country comparisons. See OECD and Open Society Foundations (2019) for a discussion of the most recent legal needs surveys.

widely available and comparable across countries and over time because judges have the universal role of supplying justice: they resolve disputes in court in all legal systems. Data on the number of judges we use are collected 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 can be measured to assess judicial performance: (i) *independence*, (ii) *accountability*, (iii) *efficiency*, (iv) *effectiveness*, and (v) *accessibility*. Given the central position of judges in the judicial decision-making process, one concern is that our proxy may reflect other judicial characteristics at the country-level, rather than just ATJ. We first empirically validate our measure by showing that there is a robust correlation between the density of judges and the accessibility dimension, while it is not 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 shows a highly significant and positive relationship between our measure of ATJ and the access to civil justice as measured by the World Justice Project for the year 2014.[6] Figure 1 depicts this relationship. Our measure 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 geographic 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 with 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 measure and two measures of overall judicial quality from the World Bank (Panel C - columns 1-8).

By focusing our analysis on the density of judges, we are essentially measuring access to a formal court rather than to 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 potential concern, as our measure may underestimate the level of ATJ in some countries. However, many alternative dispute resolution mechanisms, such as mediation, require court-backed enforcement and ultimately the intervention of the formal justice system (Voigt and Park, 2013). In the context of competition between informal and formal legal

[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).

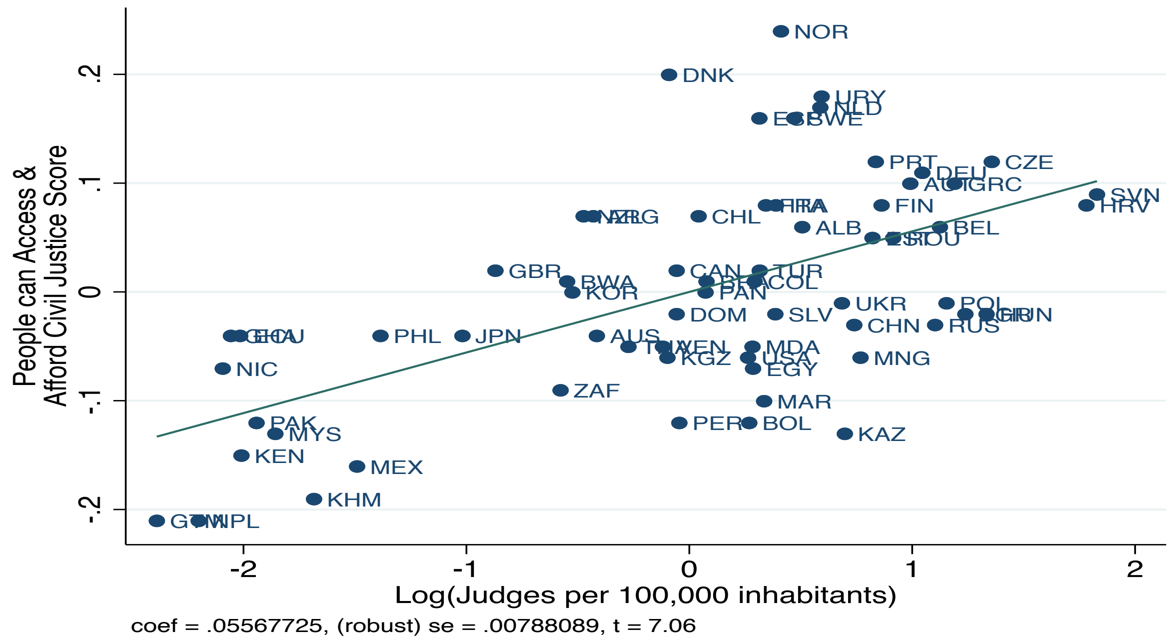
[7]We do not include a measure of judicial efficiency because data are not available for a sufficient number of countries. For a complete discussion of judicial efficiency and judicial effectiveness, see Marciano et al. (2019).

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 |
| Geographic 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 the 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 5-8) as dependent variables to assess the (i) independence and the (ii) accountability dimensions. Both indicators are taken from Gutmann and Voigt (2018). Panel B uses an indicator of judicial effectiveness, defined as the 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. The data come from World Bank's Doing Business indicators and the World Justice Project's Rule of Law Index, respectively, both in 2014. Panel C uses proxies for the overall quality of justice, defined as the rule of law index (columns 1-4) and a composite index of the judicial quality (columns 5-8) as dependent variables. The indicators are taken from the World Bank's Governance Indicators and the World Bank's Doing Business in 2014 and 2018, respectively. Column 1 includes only log judges per 100,000 inhabitants as a regressor. Column 2 adds log GDP per capita and the share of government consumption in GDP. Column 3 adds the Polity2 score of democracy, years of schooling, legal origin, and geographic characteristics (absolute latitude, total land area, standard deviation of elevation, and average distance to coast or river). Finally, column 4 adds continent dummies. See Appendix A for more 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 the World Justice Project's Access to Civil Justice Index and the Density of Judges




Note: This figure plots the relationship between the log number of professional judges per 100,000 inhabitants (averaged over 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.

institutions, Aldashev et al. (2012) show that formal laws can act as an outside anchor or "magnet" that moves 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 only lead to an attenuation bias in 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 handling all types of cases (civil, criminal, and administrative). This is not a concern for our analysis for several reasons. First, judges often act as generalists: they decide all relevant of civil, criminal, and public law cases. Only in large courts and developed economies do judges tend to specialize in a particular area (e.g., marital matters for civil cases). Second, we cannot say a priori that judges in specific types of courts, dealing 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 discussed earlier, 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 simultaneously affect ATJ and economic growth (through higher court productivity) (Ramseyer, 2012; Bielen et al., 2018). We address this omitted variable problem by explicitly controlling for average 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

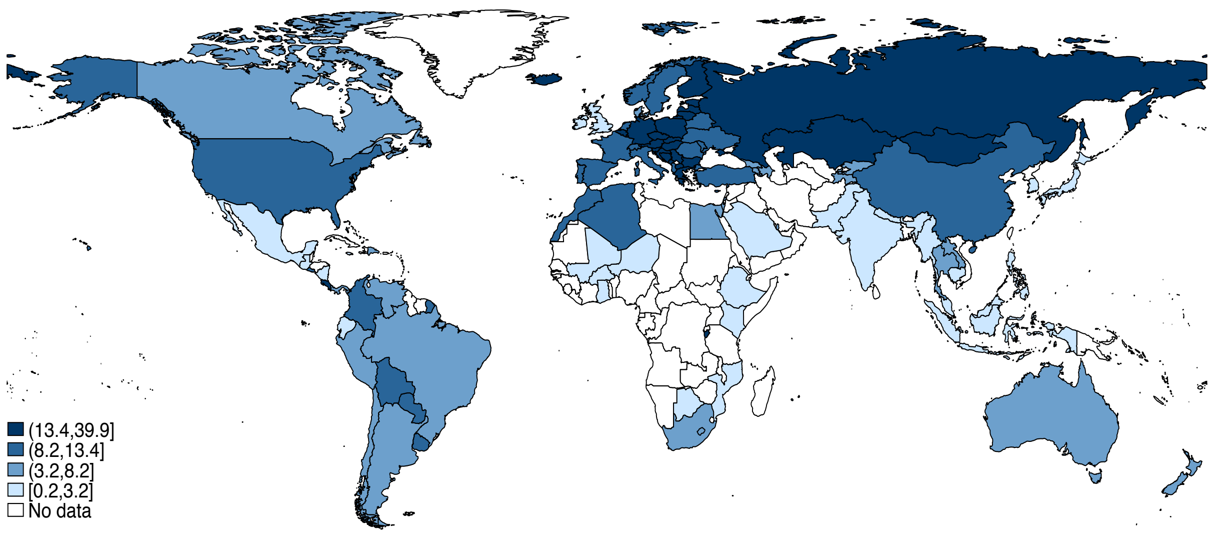
We create a new database on the density of judges by collecting data from several public institutions. First, we collect data from ministries of justice, supreme courts, and national statistical offices. We consider these sources to be the most reliable, as national institutions are often the first publishers of judicial data. As the second most reliable category of data sources, we consider international organizations that have long been recognized for their expertise in 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 into account 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 1970 to 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 gives an overview of our database by plotting the average density of judges over the entire period 1970-2014 by quartiles. We can see two things: (1) the 105 countries for which we were able 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 found in Montenegro, with 40 judges per 100,000 inhabitants, while the lowest density is found in Ethiopia, with only 0.25 judges per 100,000 inhabitants. Even within-continents, there is considerable variation. In Europe, for example, we find both countries in the top quartile (e.g., Germany or Serbia) and countries in the bottom quartile (e.g., the 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] Legal origin is one key variable that helps us understand

[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-6 for more details on the determinants and correlates of the density of judges in our sample.

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




Note: The map shows 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).

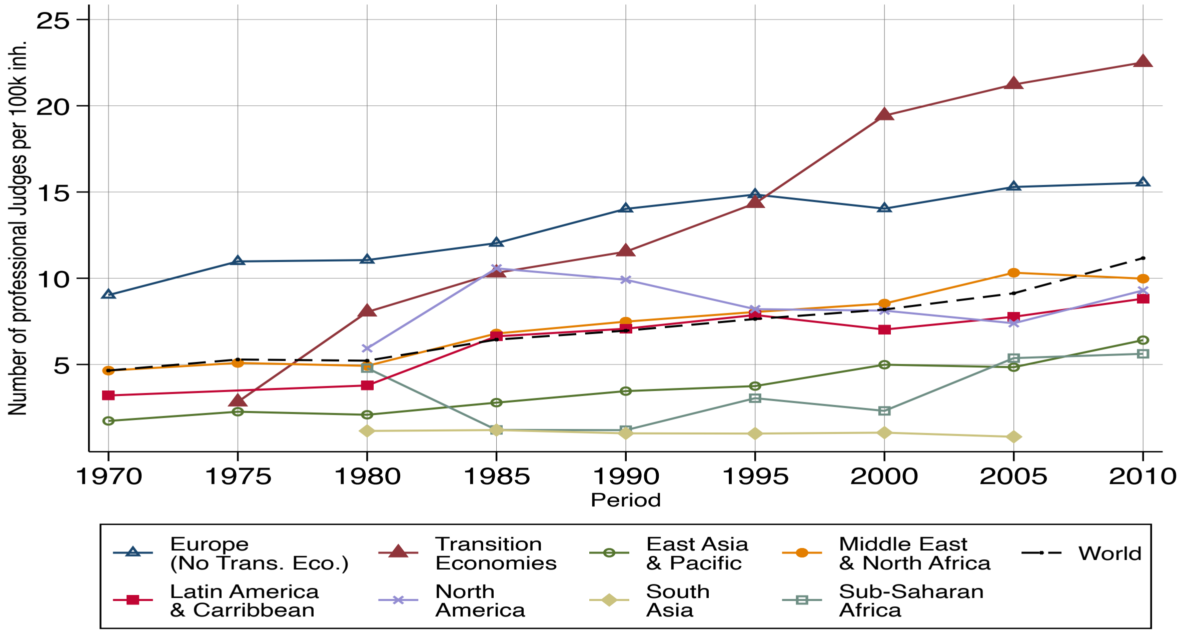
the systematic difference in the density of judges between two countries – for example, Germany (civil law) and the United Kingdom (common law) – that have similar per capita income, population sizes, and levels of democracy.

Figure 3 shows the time variation (in five-year averages) of the density of judges across different groups of countries and the world average. Although the world's population has increased over a period of 45 years (from 1970 to 2014), the number of judges has increased even more. The world average of the number of judges per 100,000 inhabitants has more than doubled, rising from 4.65 to 11.17 during the analyzed period. This doubling was achieved at a fairly 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 levels, while East Asia & Pacific, Sub-Saharan Africa as well as South Asia have the lowest levels. The key to our identification strategy, is that we have significant differences in the growth rates of the density of judges, ranging from a threefold increase in East Asia & Pacific, to stagnation in South Asia.

Starting with Transition Economies, we document a remarkable doubling of 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 a market economy created a demand for new courts and more judges;[10] (2) the

[10] In Russia, for example, a constitutional court and commercial courts were created after the fall of the USSR(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).

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




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

population has 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 are based on fewer 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 given that the region's population has more than doubled over the same period. 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 grew at the same pace as the world average, roughly doubling their density levels between the 1970 and 2010 period: Europe (excluding Transition Economies), the Middle East & North Africa, Latin America & the Caribbean, and North America. European countries have the second-highest level of density of judges in 2010, while the Middle East & North Africa and Latin America & the Caribbean reached the world average levels during the period. Since the North American group consists 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.

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 a 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 the 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 note the divergence between South Asia and East-Asia & Pacific, as the two regions had a similar density of judges in 1980. Although 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 five-year averages of all variables to smooth out the short-run fluctuations and to deal with the annual gaps in the data. Furthermore, all of our explanatory variables are lagged by one five-year period as it takes time, especially for ATJ, for the effect on economic activity to materialize. 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 five-year period. Our left-hand side corresponds to a country's five-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 $\delta_t$; a country fixed effect $\alpha_i$; and an idiosyncratic error term $\varepsilon_{i,t}$. Our coefficient of interest is $\gamma$ which captures the medium-run (five-year) effect of ATJ on economic growth.

Estimating equation (1) using OLS would produce inconsistent estimates due to endogeneity issues. First, ATJ and economic prosperity may be jointly affected by a third variable (e.g., government spending or political regime), leading to an omitted variable bias. Equation (1) addresses part of the omitted variable problem by including country fixed effects which control for all time-invariant country characteristics that affect 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 the problem of reverse causality, as citizens with higher income 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 potential concern as we have a relatively short number of periods (T = 9).

To address the aforementioned biases, we estimate equation (1) using the difference Generalized Method of Moments estimator (hereafter referred to as diff-GMM) derived by Arellano and Bond (1991). Diff-GMM uses a GMM estimation procedure, in which all variables in the regression are first-differenced and lagged levels of the variables are used to instrument the endogenous variables. This has the advantage of eliminating country fixed effects (by taking first-differences), while at the same time providing the possibility to control for other time-varying growth determinants, add time fixed effects, and include instruments for endogenous regressors.

The key assumption to satisfy for the exclusion restriction when using lagged levels of endogenous variables as instruments is the absence of serial correlation in the residuals. To address this potential issue, we follow the literature and systematically report the Arellano-Bond test for autocorrelation of the residuals (AR2 test).[11] We treat the lag-dependent variable as endogenous, using its second to sixth lagged levels as instruments.[12] On the other hand, we use more distant lags (fifth to eighth) to instrument ATJ and other growth determinants, as they are more likely to satisfy the exclusion restriction. The exclusion restriction is that ATJ levels observed 25 to 40 years ago are not correlated with current shocks affecting GDP per capita growth. Throughout the paper, we also keep the same set of instruments for transparency and to avoid selecting "ad hoc" moment conditions for each specification.[13]

To further evaluate the quality of our estimates, we follow the empirical literature and systematically report $p$-values of the Hansen (1982) test. The Hansen test is heteroskedasticity robust, and evaluates the joint exogeneity of all the instruments. Roodman (2009) shows that a common source of weak instrumentation in the GMM framework is the use of "too many instruments."[14] Following his suggestions, we address this issue by keeping the number of instruments well below the number of countries in our sample. We do this by collapsing the matrix of instruments

[11] In particular, we test for second order autocorrelation of the residuals, i.e. $\mathbb{E}\left[\varepsilon_{i,t}\varepsilon_{i,t-2}\right] = 0$, as we start instrumenting with the second lag of the dependent variable.

[12] Treating the 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).

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

[14] In a diff-GMM estimation, the number of instruments is quadratic in $T$.

in each specification.[15]

We use Arellano and Bond's (1991) diff-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 the country fixed effects are uncorrelated with the lagged differences of the 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 concerns 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 is largely composed.

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

$$\begin{aligned} \ln\left(\frac{y_{i,t}}{y_{i,t-1}}\right) &= \beta \ln(y_{i,t-1}) + \gamma \ln(ATJ_{i,t-1}) + \boldsymbol{\theta}' \boldsymbol{X}_{i,t-1} + \\ &\boldsymbol{\zeta}' \boldsymbol{\mu}_{i,t-1} + \boldsymbol{\eta}' \ln(ATJ_{i,t-1}) \times \boldsymbol{\mu}_{i,t-1} + \delta_t + (\alpha_i + \varepsilon_{i,t}) \end{aligned} \tag{2}$$

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 geographic 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 (2), we instrument the new endogenous variables with the same lag structure as the other controls.

# 4 Results

The results are divided into three parts. In Section 4.1, we present our main results evaluating the impact of ATJ on economic growth, and we test their sensitivity to different sets of controls, placebos, and the exclusion of various subsamples. 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.

[15] 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 number of instruments without losing information.

## 4.1 Main Results and Robustness Checks

In Table 2, we present our results based on the estimation of equation (1). We use five-year averages in a panel of 83 countries (73 countries in the baseline specification) covering the 1970-2014 period. We start with a parsimonious specification in column 1 using OLS-FE including only our variable of interest and the lag dependent variable. In the next columns, we use a diff-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 further identify potential candidates for transmission channels and to test for additional robustness.

Table 2: Effect of ATJ on Economic Growth

| Estimation: | FE<br>(1) | GMM<br>(2) | GMM<br>(3) | GMM<br>(4) | GMM<br>(5) | GMM<br>(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 instruments and report the number of instruments. 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 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, that highlights the importance of structural elements 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 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 these are indeed transmission channels). The magnitude of the effect is similar to other studies regressing *de facto* judicial indicators on economic growth. For example, Voigt 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 moving from a fully dependent to a fully independent judiciary would grow 1.3pp faster. Moreover, Melcarne and Ramello (2016) find that each additional year of judicial delay in the resolution of private litigation lowers annual economic growth by 1pp.

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 the 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 may be due to an increasing trend in the upper-tail of human capital, or may reflect the importance of public services, even after controlling for government expenditure and education. To further address this concern, and to demonstrate the particular importance of the density of judges as a good country-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 find no 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 the number of highly educated people or in the size of public sector employment.

Table 4 presents the robustness of our benchmark specification to the removal of different groups of countries. In 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.[16] In columns 2 and 3, we check whether our results are influenced by countries with very low or very high ATJ values 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 remain stable across the three columns. In column 4, we examine the possible 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

[16]Countries with less than two million inhabitants in 2000 in our sample are: Botswana, Cyprus, Estonia, Luxembourg, Mauritius, Qatar, Slovenia, and Trinidad and Tobago.

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 the 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 instruments 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. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

demanding robustness check that we perform, as we drop up to 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 remains positive and significant at the 1% level, but the magnitude of the effect is reduced to 0.42pp.[17] Finally, in column 5 we check whether countries with the highest variation in ATJ drive our benchmark results, as 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.

## 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 changes resulting from the

[17] 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 the diff-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 instruments 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. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

fall of the USSR (Figure 3); MENA countries are mostly Muslim with Muslim legal systems; East-Asia and Pacific countries include many high-growth economies (e.g., China, South Korea, and Japan) that have tripled ATJ over the same period despite high population growth. Across all four regions, we find that ATJ is equally important for growth.[18] This also provides evidence that our results are not driven by specific geographic regions.

In columns 5 and 6, we include interaction terms by income level. In contrast to the interactions by region, 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.[19] Testing with a time-varying measure of economic development, column 6 confirms that the impact of ATJ on economic growth is negatively related to 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, a 1% increase in ATJ in Belgium has 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.[20] A direct and important policy implication is that the poorest countries stand to gain the most from increasing ATJ, which is in line with other objectives promoted by the UN Sustainable Development Goals (SDGs).[21] This last result, however, highlights a limitation: our

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

[19] 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, the Netherlands, New Zealand, Norway, Portugal, Singapore, Slovak Republic, Slovenia, South Korea, Spain, Sweden, Switzerland, the United Kingdom, and the United States.

[20] 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, the Netherlands, Norway, Qatar, Singapore, Sweden, Switzerland, the United Kingdom, and the United States.

[21] 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

Table 5: Heterogeneous Effects of ATJ on Economic Growth

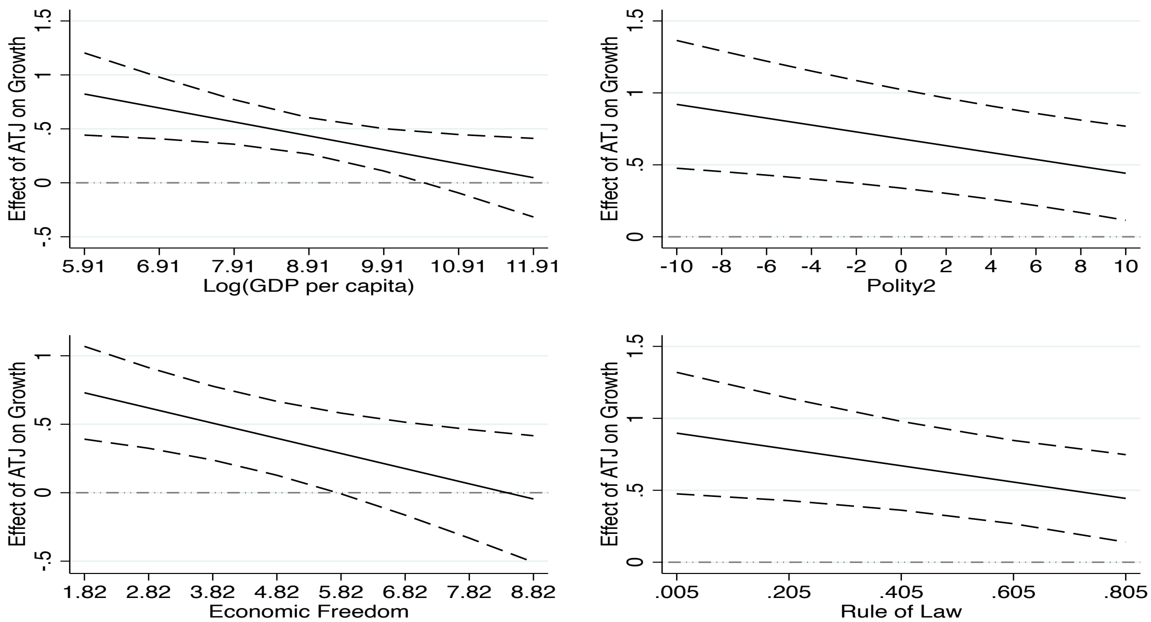
| PANEL A | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| $log(GDPpc_{t-1})$ | -0.517*** | -0.503** | -0.509*** | -0.644*** | -0.543*** | -0.378** | -0.654*** |
| | (0.132) | (0.206) | (0.168) | (0.172) | (0.153) | (0.167) | (0.165) |
| $log(ATJ_{t-1})$ | 0.571*** | 0.656*** | 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 the 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 instruments 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. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Economic growth" (SDG 8).

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.

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.

In column 7, we examine the heterogeneous effect of ATJ with respect to human capital. In line with our 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 given 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 first examine the effect of ATJ at different levels of political and economic freedom by using the Polity2 score of democracy (column 1) and the Fraser Institute's Economic Freedom of the World index (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.[22] This also reflects the fact that more democratic and economically free countries already have 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 magnitudes, as noted in Table 5. On the other hand, we find that the effect is not 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 the 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 find no significant difference in the effect for either French or German legal origin. Thus, in terms of heterogeneity, it is the fundamental differences between the common law and civil law legal systems that matter more than the less fundamental differences within the civil law legal system.

Lastly, we examine the differential effect of ATJ on growth depending on judicial independence (column 6) and judicial quality 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 an indication 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

[22] 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).

index and its main subcomponents.

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 instruments 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. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

We find that of the three classical growth control candidates from Table 2, only government consumption is a transmission channel through which ATJ affects GDP growth. Column 1 shows that ATJ has a negative effect on the share of government consumption in GDP.[23] We interpret this finding as a positive effect of ATJ on government accountability as it provides a way for citizens to challenge government policies; thus, one of the main transmission channels through which ATJ affects economic growth is the effectiveness or quality of institutions, a critical notion for achieving sustained economic growth (Acemoglu et al., 2005).

In column 4, we find a negative effect of ATJ on public corruption, which further supports the government expenditure channel interpretation. Corruption, by reducing incentives to produce and invest, has long been 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, this last finding is particularly interesting in light of 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, in addition to having 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.[24]

[23] 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.

[24] Column 2 of Appendix Table C-4 provides further evidence in this direction by showing that ATJ has a negative effect on military interference

In columns 5-8, we include outcome variables that we consider 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 developing countries face a situation known as legal dualism, or the coexistence of formal and informal judicial systems. It is easy to think that informal justice goes hand in hand with informal economic activity, as it provides a means of resolving disputes while minimizing the likelihood of detection by the government and possibly with less cost and delay. In such a case, better ATJ may reduce the informal economic activities by lowering the cost for firms to operate in the formal sector relative to the informal sector, all else being equal. In line with this explanation, 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 shows that the effect of ATJ decreases with 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 in the institutions under which economic activity takes place. The magnitude of the effect is substantial, as our estimates suggest that a 20% increase in ATJ can move a country from the median to the 75th percentile of economic freedom. However, we should be cautious about causality, as we reject the null hypothesis that there is no second-order autocorrelation in the residuals.

To better understand the economic freedom channel, we decompose the Economic Freedom Index as much as possible to get a more precise view of the exact transmission channel.[25] Among the five components of the Economic Freedom Index, 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 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 regulation is in line with the findings of Visaria (2009) and Chemin (2009a,b, 2012), which show that judicial efficiency has positive effects on access to credit markets, investment, and entrepreneurship.

# 5 Conclusion

As stated in 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 in order to have better functioning market economies. Whenever

in the rule of law and politics as measured by the Fraser Institute.

[25] Economic freedom is an index consisting 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.

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).

In this paper, we have created 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 country-level proxy that captures the fundamental and historical evolution of ATJ between 1970 and 2014 for a wide range of developed and developing countries. This unprecedented historical coverage allows us to study the plausible causal effect of ATJ on economic development, using an 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 subsamples and to different falsification exercises. Our estimates imply that a 1% increase in ATJ increases the five-year GDP per capita growth rate by 0.7pp. This substantial overall effect of ATJ is negatively related to 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 the structural elements of ATJ. In terms of mechanism, our findings suggest that ATJ increases growth via higher government accountability and better institutional quality. 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 regulation of credit markets.

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 the 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.

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# 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 (2018). |
| $Quality\ of\ Judicial\ Processes$ | 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 to 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 comes from the following international organizations: UNOV, CEPEJ, UNODC, OAS, and Eurostat. These organizations specialize in collecting data around the world: some, like CEPEJ or OAS, focus on a specific area (Europe and the Americas, respectively) while others, like UNOV, provide data for all continents.

We use the following criteria to create our database:

1. We include data considering the following order of preference 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 exactly the 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 to 2004 and CEPEJ from 2004 to 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 to 2000, and CEPEJ from 2002 to 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 greater 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. If observed, we completely remove the dataset with the lowest quality.

Table B-1 details the sources, definitions, countries, and periods covered by each data source used. Table B-2 provides summary statistics for the 105 countries for which we were able 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 | **the 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 | **the 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 periods. From column 1 to 7, we include an interaction term between ATJ and the corresponding five-year 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 instruments 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 geographic 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 instruments 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 diffenent sets of lags specified in the corresponding lines for $GDP_{pc}$ and $ATJ$ (including other endogenous variables). We use a collapsed matrix of instruments 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 instruments 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 instruments 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: 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 into the factors that explain 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 five-year 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, $\delta_t$ 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 variation in the density of judges, and are widely used in the macroeconomic literature. We find that a very robust predictor of the density of judges is the legal origin. In particular, we find that civil law countries with 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 have more than double the density of judges compared to common law countries. Within the civil law legal family, we find that German legal origin countries have a significantly higher density of judges than French legal origin countries, as we reject the null hypothesis of equality of the three civil law legal origins coefficients in columns 3 and 6. These findings are consistent with the comparative law literature, which describes common law as an adversarial dispute resolution system in which the judge has less investigative power 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 fewer judges and more lawyers than civil law.[a] Differences within the civil law family can be explained by peculiarities 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 indicating the percentage of population of European descent. Outside of Europe, this variable may 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, as a robustness check, columns 4-6 replicate the first three columns considering only data from the 1990-2014 period. 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.

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