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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. To do so, we create a new database on the number of judges per capita by collecting data from various public institutions and academic publications. We use these data as a country-level indicator to capture the structural evolution of ATJ from 1970 to 2019 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 has a sizable positive effect on economic growth. The substantial aggregate effect of ATJ on growth is independent of countries' legal origin, customary law, rule of law or level of democracy. However, we find evidence that the economic returns from ATJ are higher in poorer countries. In terms of mechanisms, our results suggest that ATJ promotes growth via higher government accountability and improved institutional quality.

"*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)]

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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. Numerous barriers, ranging from time and cost factors to various forms of discrimination, 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 has a sizable positive effect on economic growth. To conduct the empirical analysis, we assembled a new dataset from primary and secondary sources for a large sample of countries. Our measure of ATJ at the country level is the number of judges per capita. As the number of judges is a determinant of the supply of justice, increasing the number of judges per capita in a country means that more cases can be treated at a lower time and monetary cost, thus increasing the ATJ of the population. To further validate our measure, we provide evidence that changes in judges per capita correlates with changes over time in the access to civil justice indicator of the World Justice Project (2023) (see Fig. 3).

One major challenge of our analysis is the endogeneity of ATJ. Wealthier societies can afford more efficient justice systems and better ATJ. Similarly, more dynamic governments could hire more judges while putting in place policies that promote economic growth. To deal with this endogeneity problem, we use a dynamic panel specification, and estimate it with a difference GMM estimator as derived by Arellano and Bond (1991). Our main results show that ATJ has a significant and robust positive effect on economic growth. The benchmark estimates imply that a 1% increase in ATJ leads to a 0.4pp increase in the five-year growth rate of GDP per capita. Exploring potential heterogeneous effects, we find that the positive effect of ATJ on growth does not depend on countries' legal origin, customary law, rule of law or democracy levels. However, we find evidence that the less developed societies benefit more than rich societies from improving ATJ. In terms of mechanisms, our results suggest that the effect of ATJ on growth comes from higher government accountability and institutional quality. In particular, we show that ATJ negatively affects government consumption and public corruption, while positively affecting total factor productivity, 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 on quantitative measures of ATJ. We argue that the number of judges per capita is a valid measure for the historical evolution of ATJ at the country level. To construct our measure, we created a new database on the number of judges for a sample of 143 countries from 1953 to 2021 (unbalanced panel). In particular, we collected data from three types of sources: (i) international organizations, (ii) public national institutions, and (iii) academic publications. Using the specific temporal and geographical coverage of each source, we merge the data to create longer series on the number of judges for each country. Consistency of the final series was systematically checked to exclude potential spurious variations between two merged sources or within a source. We provide an empirical validation of our proxy, showing that countries with faster growth in their number of judges per capita also experienced faster growth in the more comprehensive (but shorter in the time dimension) access to civil justice score of the World Justice Project (2023) between 2010 and 2014. Compared to existing 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. This longer time dimension also allows us to produce a series of new stylized facts from our database that highlight substantial differences in regional trends, as well as in the levels of judges per capita across countries.

Second, we complement the existing literature on the effect of *de facto* 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 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 other country-specific studies showing the importance of court effectiveness for entrepreneurship, credit, agricultural, industrial activities, and productivity (Jappelli et al., 2005; Chemin, 2009a,b; Visaria, 2009; Chemin, 2012; Boehm and Oberfield, 2020; Chemin, 2020; 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 has more importance for

[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.

economic development than the legal family of the code. Feld and Voigt (2003), Voigt et al. (2015) show that *de facto* judicial independence is significantly correlated with economic growth, while *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 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 has 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 likely to satisfy the exclusion restriction.

The rest of the paper is organized as follows. Section 2 discusses first the definitions and measurements of ATJ, and then 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 tests first the validity of our ATJ proxy, and then 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

This section is divided into two parts. In Section 2.1, we introduce the concept of ATJ, its general definition, the dimensions captured by our proxy, and discuss the main limitations of our measure. Finally, in Section 2.2, we present the sources and methodology used to construct our dataset, as well as provide stylized facts on ATJ.

2.1. Definition 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 whom 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 their 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 afford the monetary cost of the procedure, to support the opportunity cost of the procedure, and to endure the emotional cost of the procedure. Overall, this multidimensionality makes ATJ difficult to quantify due to the need to collect, aggregate, and make weighting decisions for many variables.[2] 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.[3] 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, with data available only from 2015. Furthermore, ATJ is very persistent on such a narrow time interval, making it difficult to study potential effects on economic development.

To overcome the limitations of current indicators, we focus on a specific and structural definition of ATJ, namely the opportunity for the average citizens to have effective access to the judiciary. We argue that judges per capita is a good indicator for capturing structural changes in ATJ at the country-level. Compared to existing measures, this indicator has the advantage of being relatively easy to collect and comparable across countries and over time. This is because judges have a universal role in supplying justice by resolving disputes in court across all legal systems. As such, our measure primarily captures ATJ from the supply-side. Indeed, as a determinant of the supply of justice, an increase in the number of judges in the judiciary implies that more cases can be resolved with less time and monetary cost, everything else equals. We refer to this situation as an increase in ATJ.[4] In this sense, our indicator directly captures the availability and cost dimensions of ATJ considered above (components (ii) and (v) of the definition). ATJ can also be modified via the demand side. For instance, policies can encourage people to go to court, through legal capability or by addressing physical and social barriers (components (i), (iii), or (iv) of our ATJ definition). However, these demand-side policies have an ambiguous effect on ATJ. Indeed, depending on the capacity of the judicial system to treat additional demands, such policies can increase the number of resolved cases, but also, delays and cost. For example, encouraging people to go to court in an already overloaded justice system will result in increased delays and costs without resolving more cases, thus reducing ATJ.

[2] 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). See Barendrecht et al. (2006) for a complete discussion of ATJ and its dimensions.

[3] 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 recent legal needs surveys.

[4] This is equivalent to considering that ATJ increases when consumer surplus in the justice market is greater. To say so, we have considered a simple "market of justice" in the spirit of Buonanno and Galizzi (2014).

One remaining concern in the interpretation of our measure is therefore the extent to which supply and demand interactions are present. For example, if governments hire additional judges to meet the growing demand for justice in the country. Priest (1989) was among the first to highlight the possibility of a congestion equilibrium problem: when delay-reducing measures are implemented, more disputes proceed to court and court congestion increases back towards its initial level. However, testing the congestion equilibrium hypothesis in a panel of 36 European countries, Bielen et al. (2018b) find limited evidence of such interactions. In particular, they find a significant negative relationship between litigation rates and court backlogs, as measured by the number of pending cases per judge, but only in highly litigious countries. This suggests that the supply–demand interactions concerns are quite limited in the context of European countries, which represent a significant proportion of our sample. In Table 1, we also examine this issue and present empirical evidence supporting the supply shock interpretation of our indicator.

Our ATJ measure has some limitations. We do not have direct information on judges' domain of expertise, training/experience, the extent to which judges' behavior is affected by legal traditions, customary law, informal judicial institutions, alternative dispute resolution mechanisms, and the importance of lawyers. Although these factors are important for the relationship between judges per capita, ATJ and economic development, we explain below why each of these factors should not affect our main conclusions.

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) to construct our indicator. We believe that this does not affect the validity of our macro-level proxy for several reasons. First, judges often act as generalists: they decide all relevant 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). In Table 1 we investigate this question empirically and show that countries with higher growth rates in their number of judges per capita are also experiencing faster growth in their access to civil justice, as measured by the World Justice Project.

Legal origin is another potential source of heterogeneity among judges that is not directly captured by our indicator. Indeed, when delivering justice, judges may have different constraints on their productivity depending on the judicial system in which they operate. In particular, the common law systems are seen by legal scholars as giving less investigative power to the judge and more to the lawyers, compared to the civil law system. This should increase the productivity of civil law judges compared to common law judges. On the other hand, in the civil law system, judges are more constrained by statutes and legal codes, reducing their productivity compared to common law judges that have more discretion in interpreting and applying the law (Zweigert and Kötz, 1998; Schmiegelow and Schmiegelow, 2014). Therefore, it is uncertain whether there should be a differential effect of increasing judges per capita in civil law countries compared to common law countries. Consistent with this interpretation, column 4 of Table 5 shows that the positive effect of ATJ on economic growth does not significantly differ between legal origins.

Similarly, our indicator focuses 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 (through higher court productivity) and economic growth (Ramseyer, 2012; Bielen et al., 2018a). This would constitute an upward bias on the estimated effect of interest. Although our database does not provide information on changes in the quality of judges over time, we address this issue by explicitly controlling for the average years of schooling in our regressions. In the appendix, we also provide additional results showing the robustness of our benchmark results when controlling for higher levels of education, as measured by the percentage of the population with tertiary education (Table D-4). This aims to control for variations in the average quality of judges over time.

By focusing our analysis on the number of judges per capita, we are essentially measuring access to formal justice. However, in many developing countries, customary law still plays a dominant role in delivering justice to the average citizen. This is a potential concern as justice is often provided, in that case, by informal customary judges who are not included in our data.[5] In particular, this could lead to an upward bias in the estimated effect of ATJ on economic growth, as developing countries may replace informal judges with formal ones as they grow. In a robustness check, we show that our main results remain consistent even if we exclude from our sample countries where customary law is recognized as important (column 6 of Table 4).

Justice can also be accessed without explicitly going before a judge. This is the case in both developed and developing countries, where Alternative Dispute Resolution (ADR) mechanisms, such as negotiation and mediation, are available. These types of dispute resolution can be either formal or informal (Galanter, 1981). In the case they are informal, our indicator cannot capture that dimension, as it involves only private parties bargaining to reach a conflict resolution. However, it may be more prevalent in developing countries where market transactions based on contracts are relatively less widespread, and where formal justice is not always available. In such cases, the concerns for our main results are essentially the same as when discussing the role of informal (customary) judges. ADR can also be included in the formal justice system and used as an alternative to traditional court trials. This case is less problematic for the validity of our indicator for three main reasons. First, classic court trials directly involving judges remain the norm for the majority of dispute resolutions, especially when the cases are complex and of high monetary value, despite the increasing use of ADR (Schmiegelow and Schmiegelow, 2014). Second, ADR appears to be a complement rather than a substitute for classic trials led by judges (Voigt and Park, 2013). Third, some ADR mechanisms require court-backed enforcement and, ultimately, the intervention of the formal justice system, possibly involving a judge.

[5] In certain cases, customary judges may be integrated into the formal legal system. Although we do not have details regarding this matter, it is reasonable to assume that they are included in our data in such cases.

Our indicator focuses on judges, rather than lawyers, as the key legal position within the judicial system to capture ATJ. There are two main reasons for this choice. First, although lawyers can be seen, just as judges, as determinants of the supply of justice, the literature has shown clear evidence that an increase in the number of lawyers leads to a substantial increase in the demand for justice. Buonanno and Galizzi (2014) provides causal evidence of a positive relationship between the number of lawyers per capita and litigation rates in Italian provinces. Interestingly, they find no relationship between judges per capita and litigation rates. Mora-Sanguinetti and Garoupa (2015) also provide similar causal evidence linking lawyers and litigation rates in Spain. The size of the effect is particularly strong in this case. Increasing the number of lawyers per capita by 1% results in a 1.4% increase in litigation. This strong interaction between the supply and demand of justice in the case of lawyers makes the final effect on ATJ unclear. Second, previous studies have shown that the number of lawyers at the macroeconomic level is a proxy for rent-seeking behavior and is negatively associated with economic growth (Murphy et al., 1991; Ebeke et al., 2015).

2.2. New dataset on judges and stylized facts

We use data on judges per capita as a proxy for structural changes in ATJ for a country and over time. To this end, we have created a new database on the long evolution of the number of judges by country, based on primary and secondary sources. In particular, we have collected data from three types of sources: (i) international organizations (UNODC, UNOV and CEPEJ), (ii) public national institutions (ministries of justice, supreme courts and national statistical offices), and (iii) academic publications. Each of these sources is characterized by a specific temporal and geographical coverage. For instance, data published by a ministry of justice or a supreme court is limited to their country of origin. Data from international organizations provide information for several countries, but their temporal coverage is often limited. CEPEJ data, for example, covers only European countries, and has been available every two years since 2002.

Our contribution was to create longer data series by country, using the specific temporal and geographical coverage of each source. In practice, this means that we merged data to increase the temporal coverage of the original sources. Importantly, we systematically checked the consistency of the final series, dealing with possible spurious variations within a source and between two merged sources. Appendix B provides extensive details on the sources used and the construction of our database. We also illustrate our decision rules using the case of Spain in Figure B-1. Ultimately, our database on the number of judges is an unbalanced panel of 143 countries, covering the period from 1953 to 2021. To calculate our ATJ indicator, we divide the number of judges by the total population from the Penn World Tables (Feenstra et al., 2015) and take the five-year averages.

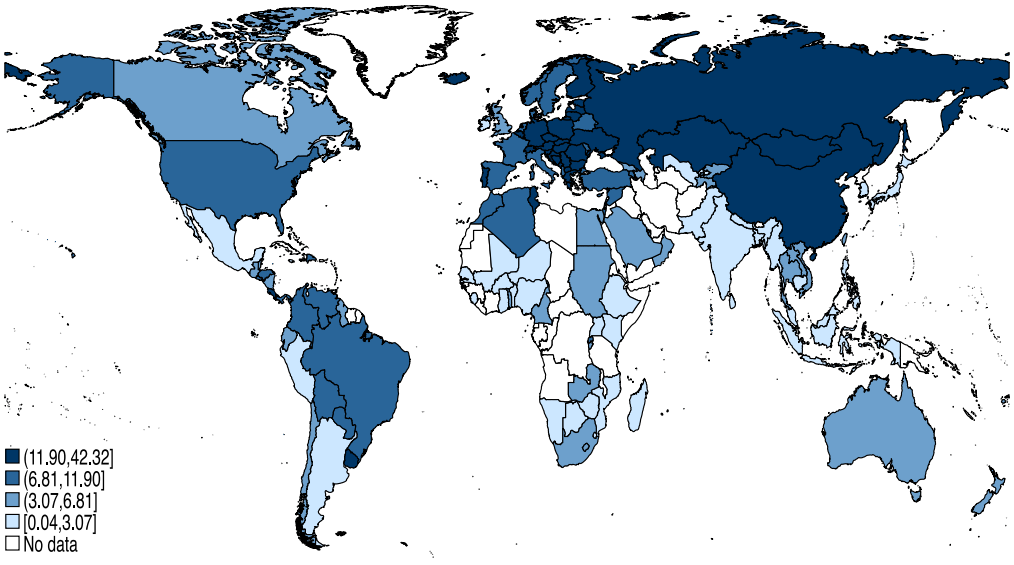

Fig. 1. Average Number of Judges per capita around the World between 1970–2019.
Note: The map shows the country-level distribution of the number of judges per 100,000 inhabitants (averaged between 1970–2019) for 143 countries. Each color represents a quartile from the first (light blue) to the fourth (dark blue). Table B-1 gives the average per country. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

Fig. 1 gives an overview of our database by plotting the average number of judges per capita over the entire period 1970–2019 by quartiles. We can see two things: (1) the 143 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 is found in Montenegro, with 42 judges per 100,000 inhabitants, while the lowest average is found in Nigeria, with only 0.04 judges per 100,000 inhabitants. There is also considerable variation within continents. 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., Ireland). It is interesting to note that the top decile

of the distribution is composed almost entirely of Central and Southeastern European countries.[6] Cross-country variations in judges per capita are correlated with differences in factors such as GDP per capita, legal origin, culture, and ethnic composition of the population.[7] For instance, the legal origin variable helps us to understand why Germany (civil law) has significantly more judges than the United Kingdom (common law), despite having similar levels of per capita incomes, population, and democracy. Common law countries have an average of 5.5 judges per 100,000 inhabitants, compared to 12.5 for civil law countries.

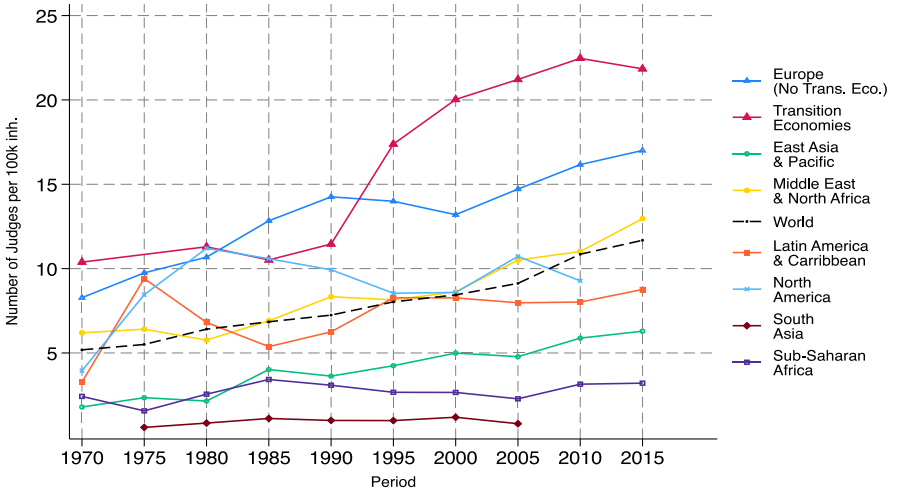

Fig. 2. The evolution of judges per capita across regions between 1970–2019.
Note: This figure shows trends in the number of judges per 100,000 inhabitants between 1970 and 2019, in five-year averages. The graph covers 105 countries, grouped into eight regions, plus the world average. The country classifications 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, Ukraine and Uzbekistan, according to the IMF (2000) classification.

Fig. 2 shows the time variation of the average number of judges per capita in different groups of countries and in the world. Although the world's population has increased over a 50-year period (from 1970 to 2019), the number of judges has increased even more. The world average number of judges per 100,000 inhabitants has more than doubled, rising from 5.19 to 11.93 during the analyzed period. The average annual growth rate of judges per capita worldwide was 1.9% over the period as a whole, with different patterns depending on the region. We confirm the significant heterogeneity in average levels that we saw in Fig. 1, 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. We highlight significant differences in the growth rates of the average number of judges per capita by region, ranging from a threefold increase in East Asia & Pacific, to stagnation in North America.

Starting with Transition Economies, we document a remarkable doubling of the average number of judges per capita.[8] Transition economies started from a level close to the rest of European countries before 1990, and reached the highest level among all the analyzed regions in the 2015 period, with an average of 21.84 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[9]; (2) the population has declined or stagnated in most Transition Economies since the 1990s.

Another dynamic region is East-Asia & Pacific, where the average number of judges per capita has more than tripled, increasing from 1.8 in 1970 to 6.29 in 2015. This is remarkable given that the region's average population has increased by 70% over the same period. Such high levels of investment in the justice sector have been made possible by strong and stable economic growth since the 1980s, especially in East-Asian countries.

Two regions grew at the same pace as the world average, roughly doubling their average level of judges per capita between the 1970 and 2015 period: Europe (excluding Transition Economies) and the Middle East & North Africa. European countries have the second-highest average level of judges per capita in 2015, while the Middle East & North Africa group closely followed the world average throughout the whole period.

[6] The top decile accounts for more than 20.72 judges per 100,000 inhabitants and is composed of the following countries: Austria, Bosnia and Herzegovina, Croatia, Czech Republic, Germany, Greece, Hungary, Luxembourg, Macedonia, Maldives, Montenegro, Russia, Serbia, Slovak Republic, and Slovenia.

[7] See Appendix Table D-13 for more details on the predictors of judges per capita in our sample.

[8] According to IMF (2000), we classify the following countries as Transition Economies: Albania, Armenia, Azerbaijan, Belarus, Bosnia and Herzegovina, Bulgaria, Croatia, Czech Republic, Estonia, Georgia, Hungary, Kazakhstan, Kyrgyz Republic, Latvia, Lithuania, Macedonia, Moldova, Montenegro, Poland, Romania, Russia, Serbia, Slovakia, Slovenia, Tajikistan, Ukraine, and Uzbekistan.

[9] 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).

In the four remaining groups of countries (Latin America and Caribbean, North America, South Asia, and Sub-Saharan Africa) the average number of judges per capita increased moderately or stagnated over the considered period. Growth in the average number of judges per capita was at around 30%, in the countries of Latin America and Caribbean, South Asia and Sub-Saharan Africa. This means that the number of judges per capita increased by less than 1% on average per year. We can note that the average number of judges in Sub-Saharan African countries rose by 32% between 1970 and 2015, despite a threefold increase in their average population over the same period.[10] The North American countries (Canada and the United States) are the only ones where the average number of judges per capita has stagnated. Indeed, between 1975 and 2000 (two years for which we observe both countries simultaneously in our data), the number of judges per capita only rose by 1.5%.

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 adapt it to our setting with ATJ as the variable of interest. Our estimation strategy uses five-year averages of all variables to smooth the short-run fluctuations and to deal with 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 $i = 1, \ldots, N$ indicates a country and $t = 1, \ldots, T$ 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 judges per 100,000 inhabitants; a vector of standard growth controls $\boldsymbol{X}_{i,t-1}$; a time fixed effect δ_t; a country fixed effect α_i; and an idiosyncratic error term $\varepsilon_{i,t}$. Our coefficient of interest is γ which captures the medium-run (five-year) effect of ATJ on economic growth.

Estimating Eq. (1) using OLS would produce inconsistent estimates for three main reasons. First, ATJ and economic prosperity may be jointly affected by a third variable, leading to omitted variable bias. Eq. (1) addresses part of the omitted variable problem by including country fixed effects α_i which control for all time-invariant country characteristics that might 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 factors that have been recognized as major determinants of economic growth, such as human capital, investment, government expenditures, the level of democracy, fertility, trade, and inflation. Among these factors, we believe that government spending and the level of democracy are a priori particularly important in reducing the omitted variable bias in our case. Indeed, as judges are ultimately civil servants, countries with higher government spending could simultaneously have more courts or judges (which would increase ATJ) and higher economic growth. Similarly, countries undergoing institutional or political change could see their number of judges evolve, while at the same time seeing their economic growth affected by these events. The other time-varying control variables (years of schooling, investment, fertility, trade, and inflation) can be considered more useful for the precision of our estimates, as they are well-established determinants of growth in the literature with no a priori direct link to ATJ. In addition, the inclusion of time fixed-effects allows us to capture the world economy trends, global shocks, and business cycle effects. The second empirical challenge we face is reverse causality, as more developed countries can afford a better ATJ. Our first approach to mitigate this problem in Eq. (1) is to lag our ATJ variable by one period. The third issue in estimating Eq. (1) with OLS is the presence of a dynamic panel bias of order $1/T$, known as the Nickell 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 three sources of biases mentioned above, we estimate Eq. (1) using the difference Generalized Method of Moments estimator (hereafter referred to as diff-GMM) derived by Arellano and Bond (1991). The advantage of diff-GMM over the fixed-effects estimator is its ability to further address reverse causality and omitted variable bias, by instrumenting endogenous regressors, while also correcting for the Nickell bias. In particular, the diff-GMM takes the first difference of regression Eq. (1), to eliminate country fixed effects, and uses lagged levels of endogenous variables as instruments.

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. Indeed, serial correlation in the residuals would open the possibility that some lagged levels of endogenous variables are correlated with the error term, thus violating the exclusion restriction. 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

[10] We have calculated the growth rate for Latin America and the Caribbean and South Asia based on the periods 1980–2015 and the 1980–2000, respectively. We did this because the average would otherwise be based on too few countries for the excluded years.

[11] In particular, we test for second-order autocorrelation of the residuals in differences, 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).

GDP per capita growth. Appendix C details the moment conditions that need to hold to achieve valid instrumentation of endogenous variables in our setting, and further discusses the issue of serial correlation in the residuals in the GMM context. 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 GMM 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 instruments. Roodman (2009) shows that a common source of weak instrumentation in the GMM framework is the use of "too many instruments", causing an overfitting bias. In line with Roodman (2009) suggestions, we restrict the number of instruments used in our specifications, following two strategies. First, we are parsimonious in the number of lags we take to instrument endogenous variables; we use only certain lags instead of taking all available lags. Second, we use a "collapsed" matrix of instruments, i.e. we reduce the instrument matrix from one column per time period and lag to one column per lag. This significantly reduces the number of instruments used, while retaining the same set of information available for the instrumentation (Roodman, 2009).

Despite our efforts, we cannot completely rule out the possibility that certain country-specific shocks in the past, particularly institutional ones, have affected both lagged levels of ATJ and long-term growth. This would constitute a violation of diff-GMM moment conditions, and consequently a failure of our identification strategy in removing the omitted variable bias. Consequently, we are cautious in interpreting our results as giving a strict causal relationship between ATJ and economic growth. One specific concern coming from Fig. 2 is that the end of the USSR, and the transition to a market economy, have triggered an increase in judges per capita and an increase in long-run growth for Post-Soviet countries. To shed light on this specific concern, we have carried out a series of robustness checks, which show that our main results are robust to the exclusion of Transition Economies from our sample (column 7 of Table 4) and that the effect of increasing ATJ on growth is not statistically different in Transition Economies (column 7 of Table 5).

We use Arellano and Bond's (1991) diff-GMM to estimate Eq. (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 Eq. (1):

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

where $\boldsymbol{\mu}_{i,t-1}$ is a vector of time-variant (e.g., level of democracy, rule of law, or schooling) or time-invariant variables (e.g., 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 Eq. (2), we instrument the new endogenous variables with the same lag structure as the other controls.

Finally, to explore possible transmission channels of the effect of ATJ on economic growth, we run regressions of the form:

$$T_{i,t} = \beta \ln(y_{i,t-1}) + \gamma \ln(ATJ_{i,t-1}) + \delta_t + (\alpha_i + \varepsilon_{i,t}) \tag{3}$$

where $T_{i,t}$ is the suspected transmission channel variable. We estimate Eq. (3) using diff-GMM, and keep the same lag specification as in our previous GMM estimations.

4. Results

The results are divided into four parts. In Section 4.1, we empirically validate our measure of ATJ. In Section 4.2, we present our main results evaluating the impact of ATJ on economic growth, and we test its sensitivity to different sets of controls, placebos, and the exclusion of various subsamples. In Section 4.3, we explore the heterogeneity of ATJ effect on growth across relevant macroeconomic dimensions. Finally, Section 4.4 investigates possible mechanisms through which the effect of ATJ on economic growth could materialize.

4.1. Empirical validation of the ATJ measure

The theoretical elements outlined in Section 2.1 suggest that judges per capita affect judicial performance through its efficiency and accessibility. The aim of Table 1 is to provide an empirical validation of our measure regarding these two specific dimensions of judicial performances. To do so, Panel A first examines the correlation between judges per capita and judicial efficiency. Using data from the World Bank (2020a), we measure judicial efficiency in two ways: (i) the number of days required to resolve a commercial dispute (columns 1 and 2) and (ii) the cost of resolving the dispute in percentage of the claim value (columns 3 and 4). In order to

[13] In Appendix, Table D-7 shows that our results are robust to different moment condition choices.

facilitate comparison with our main results, Panel A uses the same estimating equation, control variables, and target the same sample of countries as in our benchmark specification (column 3 of Table 2). Both columns 1 and 3 indicate that changes in judges per capita correlate negatively (but not significantly) with changes in the time or the monetary cost of resolving a commercial dispute within a country. In columns 2 and 4, we respectively add controls for the monetary cost of justice (attorney, court and enforcement fees) and the time cost of justice (time to fill, judge and enforce the case). Consistent with the supply shock interpretation of our measure, both columns now show that changes in judges per capita are negatively and significantly correlated with changes in the time or the monetary cost of resolving a dispute within a country. Table D-1 in appendix explores how judges per capita correlate with each of the sub-components of the time and cost dimension to resolve a dispute. Consistent with our interpretation, we find that an increase in judges per capita within a country is associated with a reduction of the time it takes to file a plaint to the court (column 2, Panel A) and of the fees that the parties pay to the court (column 4, Panel B).

Table 1
The relationship between judges per capita, judicial efficiency, and access to justice.

	(1)	(2)	(3)	(4)
PANEL A: Dynamic Panel				
Outcome:	**log(Days to Resolve Dispute)**		**Cost to Resolve Dispute in %**	
$log(Judges_{pc})_{t-1}$	−0.106	−0.146*	−2.614	−3.564**
	(0.082)	(0.080)	(2.230)	(1.609)
Lagged Dep. Var.	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Judicial Fees	No	Yes	No	No
Time Cost of Justice	No	No	No	Yes
Countries	68	68	68	68
Observations	122	122	122	122
PANEL B: Cross-Section				
Outcome:	**Growth in Access to Justice Score**			
$Judges_{pc}\ Growth_{t-1}$	0.220**	0.213**	0.212**	0.215**
	(0.090)	(0.086)	(0.087)	(0.087)
GDP per capita Growth	Yes	Yes	Yes	Yes
Initial Level of ATJ	No	Yes	Yes	Yes
Initial Level of GDP p.c.	No	No	Yes	Yes
Continent FE	No	No	No	Yes
Observations	49	49	49	49

Note: This table presents OLS estimates of the relationship between judges per capita, judicial efficiency and access to justice at the country-level. Panel A uses the number of days to resolve a commercial dispute (columns 1 and 2) and the cost to resolve a commercial dispute as a percentage of the claim value (columns 3 and 4) as measures of judicial efficiency. These two variables come from the Doing Business Indicators of the (World Bank, 2020a). Panel B uses the growth rate of the access to civil justice score from the (World Justice Project, 2023) over the period 2015–2019 (columns 1–4) as a dependent variable to measure the accessibility dimension. Panel A runs dynamic panel specifications where the lagged dependent variable, our benchmark controls (see column 3 of Table 2 for the complete list of variables), and time and country fixed effects are included as regressors (columns 1 and 3). The subsequent columns of Panel A add average attorney fees, court costs and enforcement costs (column 2) or the number of days for filing and service, trial and judgment and enforcement (column 4) as controls. Panel B shows a cross-country relationship, with the growth of judges per capita over the period 2010–2014 as our variable of interest. In particular, column 1 controls for GDP per capita growth over the period 2015–2019. In subsequent columns (2–4), we add the level of access to civil justice score in 2015, GDP per capita in 2015 and continent fixed effects as controls. See Appendix A for more information on the variables. Standard errors clustered at the country-level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Compared to judicial efficiency indicators, the choice of variable for testing our proxy on its accessibility dimension is much more limited. The main alternative dataset is provided by the World Justice Project, which gives a score of access to civil justice in over one hundred countries since 2015.[14] However, due to the limited time coverage, we are unable to replicate our proxy test with the same dynamic panel setting as in our benchmark. Therefore, in Panel B we use a cross-sectional specification in which we regress the growth rate of the ATJ score between 2015 and 2019 on the growth rate of judges per capita between 2010 and 2014 (one lagged 5-year period). Column 1 shows a positive and significant correlation between lagged growth in judges per capita and growth in the ATJ score, conditional on GDP per capita growth. This correlation remains robust even after gradually including additional control variables and continent fixed effects in subsequent columns (columns 2–4). Fig. 3 illustrates the relationship found in column 4.

Judges per capita could also capture the evolution of other dimensions of judicial performance such as independence and accountability.[15] Reassuringly, Table D-2 shows that changes in judges per capita within a country are not significantly correlated with changes in judicial independence (columns 1–2), judicial accountability (columns 3–4), or the rule of law (columns 7–8), as measured by Coppedge et al. (2024).

[14] 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, 2023).

[15] Indeed, Staats et al. (2005) list 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*.

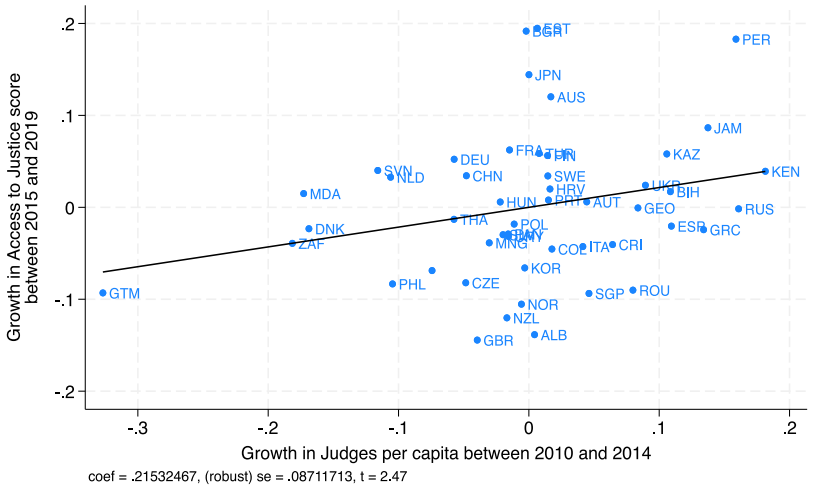

Fig. 3. Correlation between Growth in ATJ score and Growth in Judges per capita
Note: This figure plots the relationship between the growth in judges per capita over the period 2010–2014 and the growth in the access to civil justice score of the World Justice Project (2023) over the period 2015 and 2019. This adjusted partial residual plot is based on the OLS regression in Table 1, Panel B, column 4.

4.2. Main results and robustness checks

In Table 2, we present our results based on the estimation of Eq. (1). We use five-year averages in a panel of 110 countries (90 countries in the baseline specification) covering the 1970–2019 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 further deal with endogeneity, and we gradually introduce our control variables to avoid a potential problem of "bad controls" (Angrist and Pischke, 2008). In column 3, we report our main specification with standard growth controls: years of 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 enhance precision.

Table 2 shows that ATJ has a positive and statistically significant effect on economic development. This finding is robust to the inclusion of standard growth controls. In particular, column 3 reveals that a 1% increase in ATJ leads to a 0.4pp (0.08pp annually) increase in five-year GDP per capita growth. This is a substantial effect, that highlights the importance of ATJ in explaining historical differences in growth rates. Here, we capture the overall effect of increasing ATJ on economic development which, as we argue in Section 4.4, encompasses the effect of ATJ on other growth determinants. When additional standard growth controls are added in columns 4 to 6, the magnitude of the effect is stable and not statistically different from the benchmark. Table D-3 shows that our results remain robust when additional controls are incorporated.

The magnitude of the effect is similar to other studies that regress *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 resolving 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 positive signal indicating the good quality of our instruments. However, despite our efforts, we cannot completely rule out the possibility that certain country-specific shocks in the past, particularly institutional ones, have affected both lagged levels of ATJ and long-term growth. This would constitute a violation of diff-GMM moment conditions, and consequently a failure of our identification strategy in removing the omitted variable bias. Consequently, we are cautious in interpreting our results as giving a strict causal relationship between ATJ and economic growth.

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 judges per capita 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, we find no positive effect of the placebos on economic growth. Overall, this suggests that the relationship found between ATJ and economic growth 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

Table 2
Effect of ATJ on economic growth.

Outcome:	GDP_{pc} Growth					
Estimation:	FE	GMM	GMM	GMM	GMM	GMM
	(1)	(2)	(3)	(4)	(5)	(6)
$log(GDP_{pc})_{t-1}$	−0.384***	−0.417***	−0.602***	−0.514***	−0.518***	−0.442***
	(0.053)	(0.157)	(0.090)	(0.118)	(0.100)	(0.148)
$log(ATJ)_{t-1}$	0.128***	0.613**	0.415***	0.335***	0.342**	0.375**
	(0.044)	(0.236)	(0.102)	(0.113)	(0.144)	(0.154)
$Schooling_{t-1}$			−0.013	−0.054	−0.060	−0.029
			(0.070)	(0.041)	(0.038)	(0.041)
$Investment_{t-1}$			−1.438	−1.933**	−1.983*	−0.788
			(0.952)	(0.917)	(1.188)	(1.050)
$Gov.\ Cons._{t-1}$			−1.915***	−1.879**	−1.670**	−1.623**
			(0.628)	(0.833)	(0.738)	(0.783)
$Polity2_{t-1}$			0.020**	0.013	0.010	0.004
			(0.010)	(0.013)	(0.014)	(0.016)
$log(Fertility)_{t-1}$				−0.462**	−0.403**	−0.368*
				(0.220)	(0.201)	(0.218)
$Openness_{t-1}$					−0.268	−0.469
					(0.843)	(0.902)
$Inflation_{t-1}$						−0.002
						(0.002)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
AR2	.	0.30	0.44	0.29	0.28	0.56
Hansen	.	0.39	0.25	0.42	0.55	0.18
Instruments	.	17	33	37	41	45
Countries	110	110	90	88	88	84
Observations	552	392	343	335	335	327

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.

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 levels by removing the bottom 10% (column 2) or the top 10% (column 3) of ATJ. 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 demanding robustness check we perform, as we drop up to 30% of the countries in our sample and 10% of the observations. 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. To do so, we drop the 5% of countries with the highest ATJ standard deviation. In column 6, we test the sensitivity of our benchmark results to the exclusion of countries where customary law is important (whether as part of the formal or informal justice system). Finally, column 7 looks at the influence of Transition Economies on our benchmark results. In all cases, we find that the effect of ATJ stays highly significant. Despite variations in the point estimates, the results remain within the same orders of magnitude as in our benchmark (column 3 of Table 2).[17]

4.3. Heterogeneous effects of ATJ on economic development

In Table 5, we test whether ATJ has a differential effect on economic growth as a function of certain macroeconomic characteristics. In columns 1 and 2, we find evidence that the returns of ATJ on growth depend negatively on the initial level of development. In particular, column 1 shows that increasing ATJ in advanced income countries has a significantly smaller effect on growth compared to non-advanced income countries.[18] On the other hand, the effect of increasing ATJ in non-advanced income countries is large and in line with our benchmark. In column 2, we test the same relationship using initial levels of GDP per capita as

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

[17] Note that in columns 1 and 6 we are now rejecting the null hypothesis of no second order autocorrelation of the residuals, signaling the failure of the diff-GMM moment conditions in these two specific cases. Reassuringly, we reject the presence of third order autocorrelation in the residuals when we restrict the set of instruments to the third and further lags for these two same specifications. The regressions are available upon request.

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

Table 3
Placebo tests.

Outcome:	GDP_{pc} Growth				
Panel A - Placebo	(1)	(2)	(3)	(4)	(5)
$log(GDP_{pc})_{t-1}$	−0.560***	−0.550***	−0.488***	−0.364***	−0.420**
	(0.116)	(0.138)	(0.122)	(0.103)	(0.181)
$log(Physicians)_{t-1}$	−0.623**	−0.348*	−0.405*	−0.372**	−0.314*
	(0.310)	(0.192)	(0.233)	(0.183)	(0.180)
Controls	No	Yes	Yes	Yes	Yes
AR2	0.48	0.85	0.93	0.91	0.99
Hansen	0.12	0.07	0.16	0.47	0.25
Instruments	17	33	37	41	45
Countries	100	85	85	85	83
Observations	331	296	296	296	290
Outcome:	GDP_{pc} Growth				
Panel B - Placebo	(1)	(2)	(3)	(4)	(5)
$log(GDP_{pc})_{t-1}$	−0.428**	−0.708***	−0.380	−0.375*	−0.686***
	(0.161)	(0.217)	(0.252)	(0.191)	(0.213)
$log(Public\,Emp)_{t-1}$	−0.619***	−0.213*	−0.351***	−0.238	−0.017
	(0.173)	(0.110)	(0.099)	(0.149)	(0.091)
Controls	No	Yes	Yes	Yes	Yes
AR2	0.10	0.05	0.04	0.51	0.66
Hansen	0.01	0.03	0.21	0.21	0.59
Instruments	12	28	32	36	40
Countries	58	49	49	49	49
Observations	150	133	133	133	132

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 and other variables 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.

Table 4
Effect of ATJ on economic growth - robustness by subsample.

Outcome:	GDP_{pc} Growth						
Sub-sample:	No Small Countries (1)	No Bottom 10% of ATJ (2)	No Top 10% of ATJ (3)	No Small # Time Periods (4)	No High ATJ Variation (5)	No Customary (6)	No Transition (7)
$log(GDPpc_{t-1})$	−0.623***	−0.646***	−0.597***	−0.638***	−0.590***	−0.707***	−0.458
	(0.095)	(0.095)	(0.084)	(0.095)	(0.089)	(0.105)	(0.278)
$log(ATJ_{t-1})$	0.299***	0.366***	0.472***	0.303**	0.423***	0.339***	0.315**
	(0.105)	(0.100)	(0.142)	(0.116)	(0.089)	(0.091)	(0.155)
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
AR2	0.01	0.33	0.79	0.23	0.70	0.01	0.55
Hansen	0.29	0.21	0.11	0.21	0.40	0.38	0.48
Instruments	33	33	33	33	33	33	33
Countries	79	81	81	63	85	71	71
Observations	306	329	304	307	331	291	269

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 in year 2000. 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. Column 6 excludes countries where customary law plays a significant role in the judicial system. Column 7 removes countries classified as Transition Economies. In all specifications, we use a two-step difference GMM estimator and 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.

a time-varying measure of economic development, instead of a dummy variable as in column 1. In this case, we also find evidence of diminishing returns, as the interaction term is negative but not statistically significant at conventional levels. Fig. 4 presents this result, and shows that most countries have significant positive effects from increasing ATJ. In fact, only countries with income levels higher than 59,874 2011US$ per capita do not have a significant effect of increasing ATJ.[19] A direct and important policy implication is that most countries could benefit from an increase in ATJ, with possibly stronger economic returns for the poorest among them.

Table 5
Heterogeneous effects of ATJ on economic growth.

Outcome:	GDP_{pc} Growth						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$log(ATJ_{t-1})$	0.455***	1.113*	0.381**	0.454***	0.385***	0.610**	0.469**
	(0.142)	(0.644)	(0.145)	(0.113)	(0.113)	(0.246)	(0.179)
$log(ATJ_{t-1}) \times Adv.\ Income$	−0.774*						
	(0.428)						
$log(ATJ_{t-1}) \times log(GDPpc_{t-1})$		−0.083					
		(0.065)					
$log(ATJ_{t-1}) \times Ruleof\ Law_{t-1}$			−0.037				
			(0.179)				
$log(ATJ_{t-1}) \times LO_{UK}$				−0.447			
				(0.322)			
$log(ATJ_{t-1}) \times CustomaryLaw$					0.272		
					(0.759)		
$log(ATJ_{t-1}) \times Polity2_{t-1}$						−0.026	
						(0.024)	
$log(ATJ_{t-1}) \times Trans.\ Econ.$							−0.063
							(0.202)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
AR2	0.48	0.43	0.48	0.47	0.50	0.64	0.39
Hansen	0.80	0.33	0.37	0.24	0.14	0.56	0.19
Instruments	37	39	41	37	37	37	37
Countries	90	90	90	90	90	90	90
Observations	343	343	343	343	343	343	343

Note: This table presents estimates of the effect of ATJ on economic growth at different levels of some key macroeconomic variables. Columns 1–7 presents interaction terms with the indicated variables. In all specifications, we use a two-step difference GMM estimator and 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.

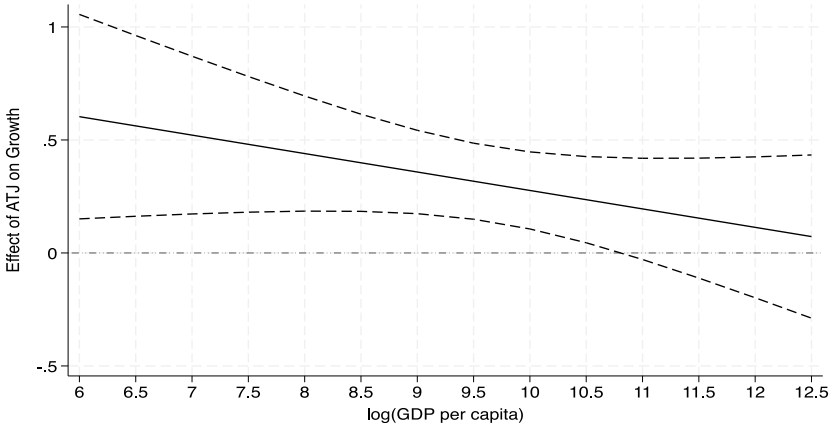

Fig. 4. Marginal Effect of ATJ on Economic Growth at different levels of log GDP per capita
Note: This figure plots the marginal effect of ATJ on economic growth at different levels of log GDP per capita (solid line). A 90 percent confidence interval is depicted with dashed lines. The figure is based on the GMM estimation in Table 5, column 2.

In columns 3 to 5, we test whether the effect of ATJ on growth differs according to certain important characteristics of the judicial system, such as rule of law, legal origin, or the importance of customary law. Each of these three variables could influence the effect of ATJ on growth for different reasons. First, the rule of law is a multidimensional concept that aims to capture a series of components

[19] In our benchmark specification, it corresponds to the following countries: Ireland, Luxembourg, Norway, Qatar, Singapore, Switzerland and the United States.

such as the security of people and property, the enforcement of contracts, controls on government and the fight against corruption. We could therefore expect the effect of ATJ on growth to depend on the overall level of development of the judicial system, as measured by the rule of law. Column 3 reveals a negative but statistically insignificant interaction term, showing that the effect of ATJ on growth does not depend on the initial level of rule of law. The legal origin of a country is another element that might affect the productivity of judges, and therefore the economic returns of ATJ. As the comparative law literature has established, on the one hand, common law judges have fewer investigative powers than their civil law counterparts, which reduces their productivity. On the other hand, common law judges enjoy greater freedom in interpreting and applying the law, which increases their productivity compared to civil law judges. Consistent with the insights from the comparative law literature, column 4 shows that the effect of ATJ on growth is not statistically different in common law countries compare to civil law countries, reflecting the theoretical ambiguity in the direction of the effect. This also means that our indicator is as capable of capturing the effects of ATJ in civil law countries as it is in common law countries, despite the differences in judges incentives in the two systems. Finally, some countries, mostly developing, are characterized by the importance of customary law in their judicial systems. This could affect the returns of ATJ in these countries, due to competition between customary and non-customary judges. The presence of informal customary judges, not included in our data, could also represent an omitted variable bias on the estimated effect of ATJ. Column 5 shows that the estimated effect of ATJ on growth is not statistically different in countries that rely on customary law, whether formal or informal. Reassuringly, the effect of ATJ on growth is stable compared to our benchmark.

Columns 6 and 7 examine whether the effect of ATJ on economic development depends on the initial level of democracy, or is specific to the radical institutional changes undergone by transition economies. In particular, since the 1990s and the end of the Soviet bloc, transition economies have experienced radical changes in their ATJ (see Fig. 2) and economic growth, as well as in their judicial system and economic institutions. This could have affected the productivity of judges, and the economic returns of ATJ. Changes in a country's level of democracy could potentially also have different returns. We find evidence that the economic returns of ATJ do not depend on radical institutional changes or on the initial level of democracy, since both interaction terms are not significant.[20]

Overall, our results suggest that the positive effect of ATJ on economic development is stable across judicial systems and institutional contexts. In one case, however, we find evidence that the overall economic returns from increasing ATJ could be greater in less developed economies (Table 5 column 1 and Fig. 4).

4.4. Transmission channels

In Table 2, we find that the positive effect of ATJ on economic growth tends to diminish when we include classical growth controls. This may indicate that ATJ exerts its influence on economic growth via significant growth determinants such as government consumption, investment, levels of democracy, and fertility. Table 6 presents our results based on the estimation of Eq. (3). In particular, we take each of the transmission channel candidates and use them as new outcome variables. Additionally, we include other suspected candidates as outcomes, such as public corruption, total factor productivity, the economic freedom index, and its main subcomponents.

We find that of the four classical growth control candidates from Table 2, only government consumption is a plausible transmission channel through which ATJ affects GDP per capita growth.[21] Column 1 shows that ATJ negatively affects the share of government consumption in GDP. We interpret this finding as a positive effect of ATJ on government accountability, as it allows citizens to effectively challenge government policies; thus, we believe that the main channel through which ATJ affects growth is the quality of institutions, an essential notion for achieving sustained economic growth (Acemoglu et al., 2005), which we explore in more detail in the following columns.

In column 4, we find a negative effect of ATJ on public corruption, confirming our interpretation of government accountability and institutional quality as transmission channels. By reducing incentives to produce and invest, corruption has long been recognized as an obstacle to growth, especially in developing countries (Mauro, 1995). Increasing ATJ enables more citizens to pursue against corruption cases. Increasing the number of judges may also enhance competition within the judicial system and reduce public corruption by increasing the likelihood of a decision being challenged by a judge. As a result, it reduces corruption and shapes the attitudes of economic agents in favor of growth. Moreover, this last result is particularly interesting in light of the link established in other studies between corruption and non-productive (military) government spending (Gupta et al., 2001; d'Agostino et al., 2016). These studies underline that public corruption, in addition to having direct negative effects on growth, distorts the composition of public spending in favor of non-productive (military) spending. This confirms the negative effect of ATJ on government spending, which can be understood as a reduction in excessive non-productive public spending.

Several previous contributions highlighted the positive impact of judicial efficiency on productivity. Boehm and Oberfield (2020) show that slow contract enforcement pushes Indian manufacturing firms towards suboptimal organizational forms. Their results suggest that the distortions associated with slow enforcing courts are sizable and negatively affect productivity at the macroeconomic level. Chemin (2020) finds that externally financed comprehensive judicial reforms, aimed at improving quality, speed or ATJ, improve firm productivity in sectors requiring more relationship-specific investments. In line with previous evidence and our interpretation on institutional quality, column 5 shows that ATJ is positively correlated with total factor productivity, suggesting

[20] In appendix, we show further evidence that our main results are not influenced by a specific time period (Table D-5) or region (Table D-6).

[21] Table D-8 shows that investment and years of schooling are not found to be transmission channels.

Table 6
Effects of ATJ on potential mechanisms.

Outcome:	Government Cons.	Polity2	log(Fertility)	Public Corruption	log(TFP)
	(1)	(2)	(3)	(4)	(5)
$log(GDPpc)_{t-1}$	0.003	0.043	0.053	−0.065	0.140
	(0.068)	(1.663)	(0.146)	(0.055)	(0.197)
$log(ATJ)_{t-1}$	−0.169***	3.061	0.180	−0.127**	0.380**
	(0.058)	(2.428)	(0.144)	(0.057)	(0.188)
Country FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes
AR2	0.13	0.06	0.17	0.27	0.56
Hansen	0.24	0.41	0.00	0.82	0.04
Instruments	17	17	17	17	17
Countries	90	90	88	90	83
Observations	344	344	336	344	332
Outcome:	Economic Freedom	(Small) Government Size	Legal System & Property Rights	Sound Money	(Low) Regulations
	(6)	(6a)	(6b)	(6c)	(6d)
$log(GDPpc)_{t-1}$	1.099*	0.344	0.857*	2.275	0.424
	(0.600)	(0.419)	(0.444)	(1.513)	(0.569)
$log(ATJ)_{t-1}$	2.293***	0.880*	1.430***	6.850***	2.451***
	(0.478)	(0.460)	(0.479)	(1.005)	(0.715)
Country FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes
AR2	0.04	0.02	0.26	0.03	0.08
Hansen	0.79	0.93	0.78	0.91	0.66
Instruments	18	18	18	18	18
Countries	87	87	89	87	87
Observations	395	398	405	395	395

Note: This table presents estimates of the effect of ATJ on the different transmission channel candidates. Columns 1–6 present results on government consumption, democracy levels, fertility, public corruption, TFP, and economic freedom accordingly. Column 6a–6d decompose economic freedom index into its main subcomponents: government size, legal system and property rights, sound money, and regulation. In all columns, we use a two-step difference GMM estimator and 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

that aggregate productivity is another channel through which ATJ promotes economic growth.

In columns 6 to 6d, we further explore the link between ATJ and institutional quality. To measure the overall quality of institutions for economic growth, we use the Fraser Institute's Index of Economic Freedom. Column 6 shows a significant positive effect of ATJ on economic freedom at the macroeconomic level. The magnitude of the effect is substantial, since our estimates suggest that a 50% increase in ATJ results in a 1.15 increase in the economic freedom score (standard deviation is 1).[22]

To better understand the economic freedom channel, in the subsequent columns (6a–6d), we decompose the economic freedom index of column 6 into its main components. Among the five components of the Economic Freedom Index, we find that ATJ reduces the size of the government in the economy (column 6a), increases the quality of the legal system and the protection of property rights (column 6b), increases monetary availability and stability (column 6c), and decreases regulations that limit the freedom of exchange in credit, labor, and product markets (column 6d).[23] In Appendix D, we go a step further and decompose these four components into their respective sub-components, to get a more precise view of the exact transmission channel. We are able to establish three main transmission channels. First, we confirm that ATJ has a positive effect on government accountability, as it affects negatively the share of government consumption relative to total consumption (column 1 of Table D-9). This is in line with our previous result regarding government expenditure in column 1. Second, in the sub-components of the "legal system and property rights" score, we find a strong and positive effect of ATJ on the protection of property rights (column 3 of Table D-10)). This is consistent with the literature showing that economic institutions, and in particular well-defined and protected property rights, are the fundamental cause of differences in economic development (North, 1990; Acemoglu et al., 2005). Finally, we find a negative effect of ATJ on inflation (Table D-11) and on the control of the state over the credit market (Table D-12). In particular, countries that increase ATJ have a higher proportion of deposits held in private banks, less government intervention on the interest rate, less extreme inflation episodes, and less inflation on average. This is consistent with studies showing the positive effects of judicial efficiency on credit, and suggests that credit is another potential channel through which ATJ affects growth (Jappelli et al., 2005; Chemin, 2009a; Visaria, 2009).

[22] However, we should be cautious about causality here, as we reject the null hypothesis that there is no second-order autocorrelation in the residuals. This is also the case for some subcomponents of the index.

[23] The only dimension for which we do not find evidence of a significant effect of ATJ is the freedom to trade internationally.

5. Conclusion

As stated in the Sustainable Development Goal (SDG) 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 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 investigate the importance of ATJ for economic development. To do so, we use the number of judges per capita as a measure of how effectively the population can have ATJ in a country and through time. Combining data from international organizations, public (national) institutions and academic publications, we created a new database giving the evolution of the number of judges for a large cross-section of countries and period of time. This unprecedented historical coverage allows us to study the effect of ATJ on economic development in a more comprehensive way than previous studies, using an instrumental variable approach and a dynamic panel setting to deal with endogeneity.

Our benchmark results, based on a panel of 90 countries, show that ATJ has a sizable positive effect on 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 0.4pp. This substantial overall effect of ATJ does not depend on countries' legal origin, customary law, rule of law or level of democracy. However, we find evidence that the effect of ATJ on growth is smaller for richer countries. 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, higher total factor productivity, 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 *de facto* judicial institutions on economic development over the long-run. Our work implies that priority should be given to improving judicial efficiency in poorer countries, where increasing ATJ could yield higher economic returns. This could help reach the ATJ objective (SDG 16) of the UN SDGs, while also contributing to other goals, such as reducing poverty (SDG 1), promoting gender equality (SDG 5), and fostering economic growth (SDG 8). Research looking at the total economic effect of ATJ is still in its infancy. Our study focuses on the number of judges as a measure of ATJ, but the number of courts, their geographical distribution, and the concentration of judges within courts are other key elements worth exploring. In general, we believe that retracing the historical evolution of ATJ over a longer period, at a sub-national level and with new ATJ measures, are fruitful areas for future research.

Declaration of competing interest

The authors declare the following financial interests/personal relationships which may be considered as potential competing interests: Adam Levai reports financial support was provided by Le Fonds de la Recherche Scientifique - FNRS. Arnaud Deseau reports financial support was provided by French National Research Agency - ANR. If there are other authors, they declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.euroecorev.2024.104947.

Data availability

Data and code available at https://www.openicpsr.org/openicpsr/project/210401/version/V1/view.

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